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Are the Effects of Informational Interventions Driven by Salience?*

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May 24, 2020

Abstract

Informational interventions have been shown to significantly change behavior across a variety of settings. Is that because they lead subjects to merely update beliefs in the right direction? Or, alternatively, is it to a large extent because they increase the *salience* of the decision they target, affecting behavior *even in the absence* of inputs for belief updating? We study this question in the context of an informational intervention with school parents in Brazil. 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, and *to a greater extent* among students with lower attendance at baseline. Our results suggest that alternative interventions that manipulate attention can generate larger impacts at lower costs, and have implications for the design of informational interventions across a range of domains.

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 Having said that, it is unclear whether those interventions work because of the specific information they convey, tailored to the circumstances of the recipient. Alternatively, it could be that those work mainly because such messages make particular issues *top-of-mind*, i.e. they induce recipients to refocus their attention on the salient issue that is addressed by the informational intervention. If that is the case, then *salience interventions* 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.³

Whether such mechanism is *quantitatively important* matters. After all, salience interventions have the advantage of demanding less or no information and, thus, requiring less knowledge by policy-makers. What is more, if refocusing attention is the key mechanism, salience interventions are bound to 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), 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 with school parents. Second, nudges also allow for additional features to manipulate attention, such as interactivity.⁴

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

¹<http://www.bbc.com/news/world-latin-america-17484532>

²Information about children’s school performance increases the likelihood of enrollment for those with high tests scores and decreases it for those with low test scores (Dizon-Ross, 2019), information about labor market returns to education increases educational attainment (Jensen, 2010), information about employees’ productivity makes turnover of low-productivity staff more likely (Rockoff et al., 2012), and information about husbands’ support for female labor market participation increases investments in labor market skills (Bursztyn et al., 2018), among many other applications.

³Gabaix et al. (2006) documents that receiving news directs subjects’ attention within a lab experiment; Ambuehl et al. (2017) shows that certain types of financial education messages – those that emphasize abstract benefits rather than specifying concrete actions – affect behavior without affecting financial knowledge in an online experiment.

⁴For instance, nudges might ask parents what they aspire to their children’s future. Having parents engage with such questions might be very effective in capturing their attention and changing their behavior. In a companion paper (Bettinger et al., 2019), we evaluate the impacts of different features of nudges to parents – frequency, time of delivery, consistency, and interactivity – on their children’s educational outcomes.

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 held fixed*. To do that, we 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 parent’s beliefs and behavior *above and beyond* those that operate through the salience mechanism (if any).

Communication between schools and parents is a great setting to study this question for the following reasons. First, because of a clear moral hazard problem between parents and children: as children grow older, their goals may drift increasingly apart from those of their future-oriented parents; moreover, it becomes progressively harder for parents to observe children’s effort at school (Cunha and Heckman, 2007; Heckman and Mosso, 2014).⁵ Second, because there are objective dimensions of children’s effort (such as attendance) on which we can report or to which we can direct parents’ attention. Third, because of administrative data on school outcomes (such as standardized test scores), which we can use to track the impacts of the experiment above and beyond surveying parents about their beliefs and behavior.

Concretely, in the experiment – across 287 schools in São Paulo, Brazil, encompassing 19,300 ninth graders –, math teachers provide weekly information about their students’ behavior (attendance, punctuality and homework completion) through a platform over the course of 18 weeks. Taking advantage of a partnership with an EdTech startup⁶, we randomly assign parents to different messages within each classroom, shared by the platform over text messages (SMS). Some parents receive child-specific information (e.g.: “Nina missed between 3 and 5 math classes over the last 3 weeks”), some receive a salience message, emphasizing the importance of paying attention to that dimension (e.g.: “It is important that Nina attends every math class”), while others receive no message at all (the control group). While the salience message potentially conveys additional information (although not child-specific; e.g. on social expectations about parenting), the message with child-specific information presumably *does the same*.⁷ Last, because we anticipate that parents’ or peer interactions may generate large spillovers, we randomize treatment assignment at two levels: across schools and within classrooms, including a pure control group, which we use as counterfactual in most of our analyses.

In line with previous findings (Bergman, 2017; Berlinski et al., 2016; Rogers and Feller,

⁵To that effect, poor parents in Brazil prefer conditional cash transfers that mandate school attendance – such that parents get notified when students miss over 15% of classes – to unconditional ones (Bursztyn and Coffman, 2012). Consistent with the moral hazard mechanism, such preference disappears when schools systematically share information about their children’s attendance.

⁶Movva (<http://movva.tech>) delivers nudges to engage parents in their children’s education across Brazil and Ivory Coast (as of 2019). One of the authors (Guilherme) is Movva’s co-founder and chairman.

⁷See section 3.1 for a detailed discussion.

2016), we find that the informational intervention has large impacts on attendance (2.1 percentage points, or about 5 additional classes a year; 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). We find that treated parents ask their children systematically more about school, incentivize studying to a greater extent, and have higher aspirations about their children’s making it to college. Children in treated households report engaging in academic and reading activities to a greater extent.

Strikingly, *most of the effects of information are driven by salience*: messages without child-specific information improve outcomes by 89-126% relative to those in the information group. What is more, we show that the effects of salience are *even higher* than those of information among students with lower attendance at baseline – precisely those whose parents would benefit the most from information.

Could it be that salience effects are large because such messages lead parents to *incorrectly infer child-specific information* from them? If salience messages induced parents to think that their children are missing more classes than they actually are, those messages might indeed lead them to monitor to a greater extent than child-specific information. We *do not*, however, find that parents in the salience group become systematically more pessimistic about their children’s behavior. Moreover, if parents inferred poor performance from salience messages, then salience effects would be identical to those of information among children with low attendance at baseline – since, in that case, inference from salience or child-specific information would be identical. In contrast, we find just the opposite: salience effects on educational outcomes are *actually greater* than those of child-specific information *precisely for children who were absent often* (those below the median 1st-quarter attendance), and *identical for those who were rarely absent* (above the median).

In line with the behavioral mechanism, parents update beliefs in response to the intervention in ways *inconsistent* with belief updating in the absence of attentional constraints. In the information group, parents’ accuracy about their children’s attendance actually *decreases*, as parents *anchor on the small numbers* for weekly absences we share over text messages when asked about about the *total number of absences over the last quarter*. What is more, differences in accuracy across groups do *not* map into differences in children’s school outcomes: information leads to positive effects across the entire spectrum of baseline beliefs; in particular, even parents who were *already accurate* change behavior when targeted by information, and information has *no additional effect* on those who were inaccurate at baseline.

Are salience effects only relevant in the short-run? Our results suggest *the opposite*: at least within the 4-month horizon of our study, not only it is *not* the case that the effects of salience messages die out, but also they even *increase over time*: the gap between the salience group and the pure control group increases between the third and the fourth quarters both with respect to math attendance and GPA.

Is child-specific information really unnecessary, or did the experiment convey too coarse information to produce additional effects? To test whether finer-grain information matters, for a sub-sample of the information group we framed child-specific information *in relative terms to the median behavior of their peers* (e.g.: “Nina missed less than 3 classes over the last 3 weeks, while most of her colleagues missed between 3 and 5”). Similar to [Rogers and Feller \(2016\)](#), the effect sizes of relative information are larger, but it is still the case that the effects of salience are at least 68% of those of information. While more frequent or finer-grain information could promote larger effect sizes, our information intervention matches the typical structure of school-parents communication campaigns in developing countries (e.g.: [Berlinski et al., 2016](#), which also finds a 0.09 effect size of an SMS information program on students’ standardized test scores).

Since we rely on the pure control group as a counterfactual, could it be that our results are driven by differences in teacher behavior, induced by requiring them to weekly fill in a platform with information about their students? To investigate whether such requirement may drive our results, we deliver an SMS nudge program to a different sub-sample, reaching parents directly with weekly suggestions of activities for parents to do with their children – without any involvement from teachers.⁸ Comparing students whose parents receive nudges to those in the pure control group, before and after the start of the intervention, we estimate effect sizes on math GPA identical to those of information (0.09 standard deviation). Using the first quarter as the reference period, there are no differences in attendance or GPA across sub-samples before the onset of the program, dismissing concerns with differential pre-trends. All in all, results suggest that our findings do not stem from differential teacher behavior in treatment schools and can be generalized beyond this setting.

Taking advantage of an additional experiment in a companion paper ([Bettinger et al., 2019](#)), we find that salience effects significantly *increase with the frequency of nudging*. The effect size of 3 nudges per week on math GPA is nearly 2-fold that of child-specific information. What is more, nudges – which are not topic-specific – improve outcomes across *both* math and Portuguese classes, while the effects of math-specific information or salience messages are mostly confined to math attendance and grades. Both patterns are consistent with *attentional constraints*.

These results suggest that the effects of informational interventions can be obtained at lower cost – and even magnified – by interventions that manipulate attention, raising the salience of the decision they target. 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

⁸Students in this sub-sample are not statistically identical at baseline to those of our main sample. The reason is that the Education Secretariat required us to work in a different region whenever the communication platform was not made available to principals.

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](#)).⁹ 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, 2017](#); [Dizon-Ross, 2019](#); [Jensen, 2010](#)). While previous literature posits that the effects of information can be non-trivial when it directs 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 is *quantitatively important*.

2 Education in Brazil and São Paulo State

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.¹⁰ To that point, the eight Latin-American countries that took the 2015 PISA exam scored at the very bottom of the 65 participating countries, and were outscored even by those with much lower per capita income. Brazilian 15 year-old students scored 121 points below the OECD average in math, what is equivalent to a *two-year lag* in math skills.¹¹

Education in Brazil is supervised by government offices across municipal, state and federal levels. Municipalities are responsible for early childhood education and primary schools, State governments are responsible for middle schools and high schools, and the federal government is responsible for college education (besides a few special secondary education programs ran by federal institutes) and for regulating private educational institutions at all levels.

São Paulo is the wealthiest and most populous Brazilian State, and its education 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

⁹In [Bursztyn et al. \(2019\)](#), where the largest reduction in default rates is caused by messages highlighting the consequences of default for future access to credit, authors point out that they “(...) cannot directly assess the extent to which this treatment provides new information as opposed to *bringing customers’ attention to something they already knew*” (p. 1644, emphasis added). In [Allcott and Taubinsky \(2015\)](#), authors recognize that “[i]t is thus not unreasonable to assume that (...) the conditional average treatment effect on willingness-to-pay from our information treatments equal the average marginal bias from imperfect information and inattention” (p. 2503, emphasis added).

¹⁰2015 National Household Survey (PNAD), Brazilian Institute for Geography and Statistics (IBGE). Primary school enrollment is mandatory in the country for children between ages 6 and 14.

¹¹The Programme for International Student Assessment (PISA) is an ongoing triennial survey that assesses the extent to which 15 year-old students approaching the end of middle-school have acquired key knowledge and skills that are essential for productive engagement in modern societies. Around 540,000 students took the assessment in 2015, a representative assessment of about 29 million students across 72 participating countries.

ninth graders, 63% of which served by schools directly administered by the State authority. Despite being a relatively wealthy State – accruing to 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 service students from disadvantaged backgrounds. In our sample, over 50% of households earn less than 3 minimum wages (about 900 USD as of September, 2017), within the income range of slum dwellers in the State capital. As such, public school students tend to perform particularly poorly: in 2015, São Paulo State’s public middle-school students scored 4.7 out of 10 in the National Index for the Development of Basic Education (Ideb) – which averages math and language standardized test scores, penalizing that average by grade repetition rates –, falling short of its already extremely modest target (5.0) for middle-school students in the State.

Poor educational outcomes emerge as a combination of poor infrastructure, low teacher value-added, and low family engagement in students’ school life. Across OECD countries, 20% of students 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%. Strikingly, according to the 2015 National Survey of Students’ Health, about 1 in every 4 parents do not know whether their child skipped classes, about 1 in every 3 parents do not systematically ask their child about problems in school, and about 1 in every 2 parents do not regularly ask about homework. Consistent with those statistics, in focus groups, public school teachers often cite low family engagement as the leading cause of students’ poor school performance.

Engaging parents in this setting is hard: the leading technology for communication between schools and parents are still handwritten notes sent through students themselves, who may not face the right incentives to ensure the message gets through. 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.¹² 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 school behavior. Teachers keep daily records on paper, but typically only upload such information into centralized school systems at the end of the school year.

3 Empirical strategy

This section introduces our empirical strategy. We start with a conceptual framework in subsection 3.1 to ground the design of our mechanism experiment. Next, informed by that framework, subsection 3.2 describes the experimental design to isolate the salience effects

¹²Less 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, that of internet and smartphone apps is not: about 55% of active lines are not systematically connected to the internet (Regional Study Center to Information Society Development, CETIC).

of informational interventions. We describe the platform that teachers fill in weekly in subsection 3.3, followed by a summary of the outcomes we draw upon, from administrative sources to survey data, in subsection 3.4. Subsection 3.5 describes the equations we estimate, our treatment of standard errors, and how we deal with multiple comparisons. Last, subsection A.5 highlights the elements of the analyses that deviate from what had been specified in the pre-analysis plan.

3.1 Conceptual Framework

The ideal experiment to study this question would compare parents who receive information to other parents whose beliefs about their children’s behavior are manipulated *while their attention is held fixed*. Such experiment is 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 parent’s beliefs and behavior *above and beyond those that operate through the salience mechanism* (if any).

Framing salience messages in this way might raise concerns, in that claiming that a dimension 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 two-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. Second, alternative framings would only imperfectly approximate those salience effects. For instance, the message “you can learn about your children’s attendance by asking at their school” is presumably not surprising at all and would be unlikely to draw attention comparably to the informational intervention. In fact, as Bursztyn et al. (2019) document, simple reminders might be unable to approximate the effects of informational interventions. Alternatively, while a message offering parents the opportunity to receive attendance information over text message conditional on their reply is indeed likely to make attendance salient, it would induce at least some to actually reply, making it unfeasible to unpack its salience effects without resorting to deception.¹³

¹³Evidently, as there is selection in who would take up the information offer, one could not merely restrict the sample to those who do not reply.

3.2 Experimental Design

All details of the experimental design and a pre-analysis plan were pre-registered at the AEA RCT Registry on June 26, 2016 (AEARCTR-0001379).¹⁴

3.2.1 Within-classroom randomization

Concretely, in the experiment – across 287 schools in São Paulo, Brazil, encompassing 19,300 ninth graders –, math teachers had to fill in a platform weekly with information about their students’ behavior (attendance, tardiness and homework completion). 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, while 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 unpacking the effects of information into those of salience, as discussed in the previous subsection.¹⁵

Some details about the specifics of content are in order. First, we restricted communication to student’s behavior 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. Second, the school behavior reported on in the (relative) information messages alternated weekly, rotating across three dimensions of children’s effort: 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 the previous subsection. For the full script of messages sent for each treatment arm, see Supplementary Appendix B.

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 confound-

¹⁴See Supplementary Appendix A for the pre-analysis plan in full. A preliminary version of this paper was circulated under the title “What Is It About Communicating With Parents?”.

¹⁵We survey parents at baseline about their beliefs about their child’s attendance and grades, to investigate heterogeneous treatment effects of information and salience messages with respect to baseline beliefs.

ing treatment effects with potential differences in teachers’ compliance across conditions.¹⁶ 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.

3.2.2 Identification concerns and two-level randomization

Having said that, there are a number of potential concerns with making inference about salience effects only through the within-classroom randomization design explained above – from peer effects to spillovers across treatment conditions to teacher effects –, all of which would lead one to under-estimate differences across the information and salience groups. We discuss how we deal with each of those concerns in detail below. Broadly speaking, our design randomizes at two levels. First, between schools, randomly assigned to different sub-samples – each associated with a specific saturation of treatment arms.^{17,18} Second, within classrooms at each school, randomizing students to treatment arms en suite with the saturation assignment at the school level.¹⁹ Importantly, other than in the pure control group (see below), neither schools nor teachers were aware of differences in assignment between schools, nor of child-specific assignments within their school.

A first-order concern with the within-classroom experimental design is the possibility that peer effects, contamination across parents or teacher effects may bias downwards any differences across the information and salience groups. To deal with that concern, we assign a set of schools to a *pure control* group, in which all students are assigned to the control group. Students in pure control schools were enrolled through the same process as those in treatment schools.²⁰

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 parents 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 some information about their own child’s school behavior *thanks to the information*

¹⁶Teachers 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.

¹⁷One set of treated schools could not be randomly assigned; see discussion on nudges below.

¹⁸Whenever there are multiple ninth-grade classrooms in a given school, we include all of them in the experiment.

¹⁹We stratified randomization in two steps. In the first step, we stratified the assignment of schools to sub-samples based on three school-level variables: their average math scores in the first quarter (in the Education Secretariat’s internal assessment), their average absenteeism rate, and their share of parents enrolled to participate in the study. In the second step, we stratified the assignment of students to treatment arms within classroom based on their first quarter math scores (in the Education Secretariat’s internal quarterly assessment). Not all students take this test (which is not mandatory). For students with missing values, we predict their score based on a simple linear regression using all baseline covariates, stratifying assignment based on predicted scores.

²⁰What is more, principals of all schools, even in the pure control group, are allowed to use the platform to send monthly communication to parents about school events; see next subsection.

treatment. While this is a more convoluted story, in order to rule it out we assign a set of schools to a different sub-sample, in which students were assigned to either salience messages or to the control groups. Since within those schools no parents received child-specific information, this sub-sample allows estimating the effects of salience messages in the absence of the information treatment.

A different concern relates to the effects of information itself. Parents may already have reasonable information about their child behavior. If that is the case, students whose parents are targeted by the information or salience treatments might look similar not because informational effects are driven by salience, but rather because informational effects are minor to start with. While having a pure control group allows testing for the effects of information separately, we also follow [Rogers and Feller \(2016\)](#) by including an additional treatment group with more informative messages, in which parents are targeted by messages that place their child relatively to his or her classmates (framing information on child behavior relative to the *median* of their classroom).²¹ The platform automatically computed the classroom median behavior once the teacher submitted information on all their students, each week.

- i. **Control:** No messages
- ii. **Child-specific information:** Messages with child-specific information about attendance, tardiness and homework completion
- iii. **Salience:** Messages highlighting the importance of school attendance, punctuality and homework completion
- iv. **Relative information:** Messages with child-specific information about attendance, tardiness and homework completion framed *relatively* to their classroom’s median behavior

Sample messages by treatment arm		
Child-specific information	Salience	Relative Information
In the past 3 weeks, Eric missed less than 3 math classes.	For good school outcomes, it is important that Guilherme does not miss math classes without good reason.	In the past 3 weeks, Nina missed less than 3 math classes. In her class, most of the students did not miss any math class.

While relying on the pure control group as a counterfactual can help rule out spillovers and estimate informational effects, it also brings about additional concerns. Teachers in the pure control group schools did not have to fill in the platform weekly (in order to avoid

²¹[Rogers and Feller \(2016\)](#) sends parents information about their child’s attendance relative to their classroom’s *modal behavior*. Using child-level information as the comparison group, findings point out that relative information has significantly higher impacts on students’ educational outcomes in US schools. We use median behavior (e.g.: “most students in Nina’s class missed less than 3 classes in the previous 3 weeks”) instead since it is presumably much easier to understand; the mode of the distribution had to be explained graphically in [Rogers and Feller \(2016\)](#), a feature that technically cannot be combined with text messages.

deception and poor compliance in filling in the platform once teachers eventually realized that information was not being delivered to parents). But having to input information into the platform could have induced teachers in treated schools to change behavior (e.g. due to perceptions of increased parent or school monitoring). If that is the case, one would over-estimate informational effects when using the pure control group as comparison. To deal with this concern, we include an additional sub-sample of schools in which parents are assigned only to either *nudges* or to a control group.²² Nudges are delivered to parents also once a week, but do not require any inputs by teachers. Their content draws inspiration in READY4K (York et al., 2017), sharing weekly suggestions of activities for parents to do with their children, also over SMS. This sub-sample allows estimating salience effects without teacher effects driven by the platform requirements and in the absence of the information treatment.

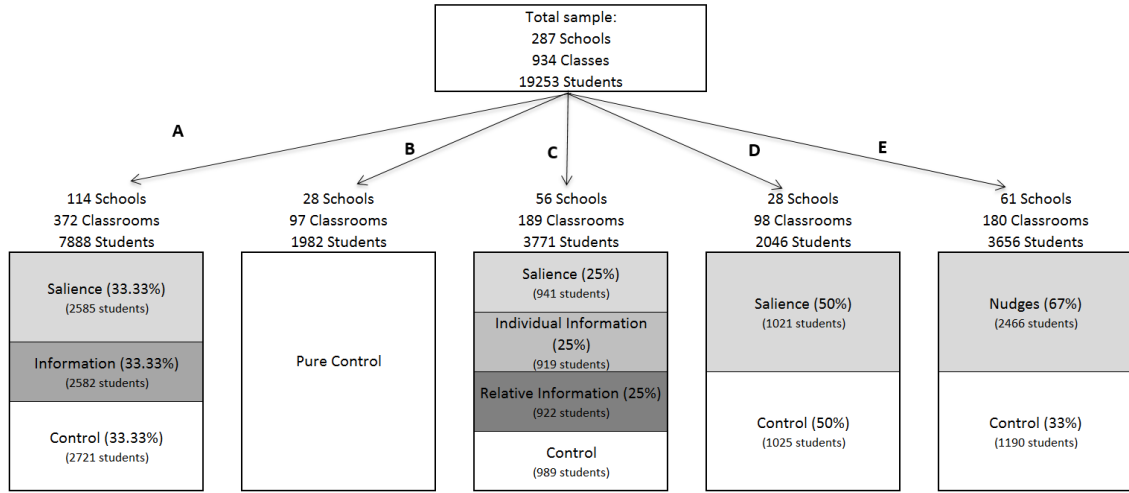
- v. **Nudges:** Messages suggesting non-curricular activities for the parent to engage with the child

To really shut down the possibility of teacher effects, those schools were not offered 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 next subsection), since in some schools math teachers also handled this activity (delegated by principals). That decision, however, had a caveat: the Education Secretariat required us to work on a different region of the State whenever the communication platform was not made available to principals. As such, in that sub-sample, students are not statistically identical to those in our other sub-samples because, in that region, students 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 strategy estimates the causal effects of nudges as long as student outcomes in different sub-samples would have not have changed differentially over time in the absence of the interventions. We discuss this identification strategy in detail in subsection 4.8.

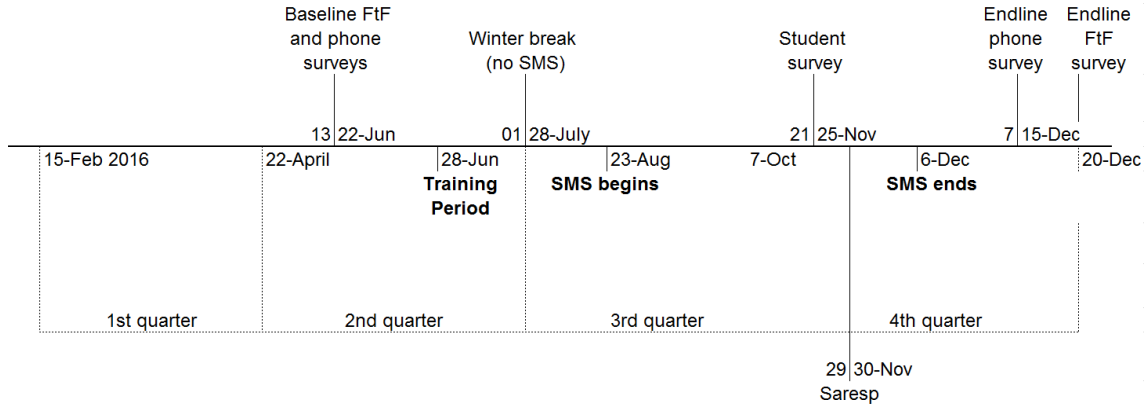
The figure below summarizes our two-level randomization design. Sub-samples A through C allow estimating the effects of information and unpacking the its effects into those of salience; sub-sample D allows estimating the effect of salience without potential spillovers from information; and sub-sample E allows estimating the extent of teacher effects.²³

²²The saturation within those schools assigned 2/3 of students to nudges and 1/3 to the control group, reflecting power calculations that had the subjects assigned to nudges involved in multiple comparisons.

²³The 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 timeline of the experiment was as follows. The school year in Brazil runs from February to December and is divided in 4 quarters, with a winter break in July. Parents were surveyed at baseline, in mid-June, during the 2nd quarter. All those who opted-in had to fill basic characteristics of the child and the caregiver, followed by a much more detailed phone survey. Teachers were trained to fill in the platform only after that, by the end of June. Parents started receiving messages only after the winter break, after 3 weeks since teachers started filling the platform (given that messages in the information group always describe children’s behavior over the past 3 weeks). Communication lasted until the first week of December, when final exams take place. Standardized tests took place immediately before, at the very end of November. Last, parents were surveyed at end line immediately after the end of SMS communication, over the second week of December.



3.3 Teacher platform

We created an online data entry platform specifically for this study, designed in a simple and intuitive way such that schools could easily manage it.²⁴ As discussed in the previous

²⁴60% 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

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’s behavior on that dimension over the past three weeks.²⁵ The system requires teachers to fill in information for all students.

Scale by student behavior’s dimension		
Attendance	Punctuality	Homework 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

Every week, data entry was only validated if the teacher filled in information for *every* student in the classroom. Teachers were reminded to fill in the platform every week via SMS. The relevant scale for that particular week was always visible in the platform, to minimize measurement error. Teachers who failed to fill in their data in any given week received an SMS alert pointing out that they had not filled in the platform that week, and encouraging them to do so in the 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 a certain threshold. As a result, average compliance was high – about 70% weekly, very similar across all sub-samples where teachers had to enter student-level data in the platform.

In order to collect cell phone numbers and baseline data for parents in the control group as well, we offered both treatment and control schools access to the platform for sharing notifications about school events (limited to no more than two messages per month). Once an event was scheduled in the platform (using the principal’s credentials), the system would send SMS notifications to parents one week prior and one day prior to the event. As discussed in the previous subsection, that functionality was only not made available in the pure control and nudge sub-samples.

3.4 Data and outcomes

We rely on four different data sources for the outcomes of interest. First, face-to-face survey data. At baseline (June), we surveyed all parents who opted-in to participate in the program about basic characteristics of themselves and their child. This dataset comprises 15,597 observations, giving us access to a wide range of controls: caregiver’s family tie with the student, and their (and their child’s) gender, age, race, and income bracket, and educational achievement. At end line (by December), we surveyed students face-to-face about their perceptions about their parents’ engagement in their school life, their time

very little data, and could be accessed by principals and teachers from any computer or smartphone, even outside of the school.

²⁵Students have around six math classes per week.

use, values and aspirations, and elicited their social and emotional skills. This dataset comprises 9,539 observations, reflecting the fact that take-up of this final survey it was optional (although strongly encouraged by schools).

Second, phone surveys (collected via automated calls²⁶). At both baseline and end line, we surveyed parents to elicit their parenting practices, beliefs about their child’s behavior, and their demand for information. Response rates of phone surveys were X% at baseline and Y% at end line. 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.

Third, administrative data from São Paulo State’s Education Secretariat: quarterly data on students’ attendance and grades (from teacher-specific exams) in math and Portuguese classes, grade repetition by the end of the academic year, and 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), across all sub-samples. Attendance is recorded in percentage points (0–100 interval). Teachers assign numeric integer grades ranging from 0 to 10, 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 teachers’ assessment of their students’ behavior entered into the online platform, alternating weekly across attendance, punctuality and homework completion – except for the pure control and the nudge sub-samples.²⁷ This dataset allows us to compute average attendance for 12,641 of the participating students, average punctuality for 12,208 of the participating students, and average homework completion for 12,025 of the participating students.

3.5 Estimation

To estimate the effects of the informational intervention and to unpack its salience effects, we estimate the following equation:

$$Y_{sci} = \alpha + \beta_1 salience_{sci} + \beta_2 info_{sci} + \beta_3 control_{s=treated,ci} + \sum_{k=1}^K \gamma_k X_{scik} + \theta_s + \varepsilon_{sci} \quad (1)$$

where Y_{sci} denotes the outcome of interest for student i in classroom c of school s ; $salience_{sci} = 1$ for students whose parents were assigned to salience messages, and 0 otherwise; $info_{sci} = 1$ for students whose parents were assigned to child-specific messages, and 0 otherwise; $control_{s=treated,ci} = 1$ for the control group within treatment schools,

²⁶Interactive voice response units (IVR).

²⁷Teachers’ average compliance rates across sub-samples and treatment arms was about 66%; see Supplementary Figure 3.

and 0 otherwise (with the omitted indicator variable for pure control schools, the reference sub-sample); X_{sck} is a matrix of student’s covariates, including students’ gender, age and race, their attendance and GPA prior to the intervention, and their parents’ gender, age, race, income and education; θ_s is a randomization stratum fixed-effect; and ε_{sci} is the error term.

We start estimating this equation only within sub-samples A and B (see subsection 3.2.2). Coefficients are estimated using Ordinary Least Squares (OLS), and we cluster standard-errors at the classroom level. Our statistic of interest is $\frac{\hat{\beta}_1}{\hat{\beta}_2}$. In particular, we are interested in the hypothesis $\beta_1 = \beta_2$.

In robustness checks, we estimate this equation also including sub-sample D, and augment this specification with an indicator variable for students whose parents were assigned to relative information messages when we include sub-sample C.²⁸ When we use the online platforms’ data – naturally unavailable in pure control schools –, the omitted category is instead the within-classroom control group.

Last, we also estimate heterogeneous treatment effects by parents’ baseline beliefs and accuracy, and by students’ behavior entered by teachers into the platform. In those cases, we are interested in comparing heterogeneous treatment effects of the salience and child-specific information treatment arms.

Following our pre-analysis plan, educational outcomes are analyzed individually. In contrast, when it comes to measures of parental engagement and 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 educational outcomes

This section starts by describing participants’ characteristics in subsection 4.1, including a discussion about selection into the sample and balance across treatment conditions. Next, subsection 4.2 describes the distribution of parental beliefs about children’s attendance and grades at baseline. This is followed by manipulation checks in subsection 4.3: analyses of the extent to which teachers fill in the platform weekly and statistical tests of whether subjects acknowledge receiving the messages differentially across groups – minimal conditions for the interventions to have any effects on the participants. Subsection 4.4 then presents our main results for the effects of information and salience messages on educational outcomes. Subsection 4.5 follows by investigating whether salience effects die out over time. Last, we assess the robustness of our results to different concerns: subsection 4.6 investigates whether results are driven by the interaction of the two treatments, subsection 4.7 explores whether conclusions change when we allow for more informative content (namely, *relative information* messages), and subsection 4.8 documents the extent to which results are driven by teacher effects by taking advantage of *nudges* (which do not require any

²⁸ As afore-mentioned, we estimate a different equation to obtain differences-in-differences estimates when include sub-sample E, discussed in subsection 4.8.

inputs by teachers).

4.1 Descriptive statistics, selection and balance

We start by looking at the whether parental engagement is correlated with children’s school outcomes at baseline. While we have no direct elicitation of parenting activities at baseline, we can proxy parental engagement with parents’ willingness to sign up to participate in the educational program that we evaluate. The program had no participation costs – it just required providing consent and a phone number to receive messages, which caregivers provided directly in parent-teacher meeting towards the end of the 2nd quarter, or indirectly by filling in a form that students took home (in the cases when parents were absent from the school meeting). Hence, opt-in status is presumably a good proxy for whether parents attended the school meeting, or at least expressed interest in receiving messages from the school otherwise.

Figure 1 displays a positive correlation between the classroom-level share of parents who opted-in to participate in the program, and 1st quarter math grades. While that correlation is certainly not causal – higher educational achievement could be driven by other underlying characteristics of parents in classrooms for which those are more engaged –, it resonates with the idea that low monitoring combined with a moral hazard problem between parents and their children might in fact drive lower school effort and worse educational outcomes.

[Figure 1]

Over 66% of the 23,398 parents invited to participate signed up for the program. Table 1 analyzes selection in opt-in. For parents who did not sign up, we only have student characteristics available from administrative records – gender, age, math and Portuguese baseline attendance and grades, and Bolsa Família’s beneficiary status .

[Table 1]

Table 1 shows that parents who signed up for the program (those more engaged at baseline, according to our interpretation) 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.²⁹

Treatment assignment is randomized conditional on enrollment. Table 2 presents subsample means for students’ and parents’ baseline characteristics by treatment arm, along with p-values of ANOVA tests of the null hypothesis that means are equal across all subsamples for each variable.

[Table 2]

²⁹Table D.5 in the Supplementary Appendix re-weights observations by their inverse probability of opt-in, showing our results to be robust even in the presence of heterogeneous treatment effects.

Panel A displays baseline characteristics for participating students. They average 14.7 years old, 48% of them are girls, and 40% are brown or black. Average math and Portuguese grades are around 6 (from 0 to 10, with a passing grade of 5). Panel B shows parents' baseline characteristics. 76% of caregivers enrolled are mothers, on average at their early 40s. 69% of them have not attended school beyond middle school: that means that, for 2/3 of our sample, children have been in school at least as far as their parents did. What is more, 59% of families earn less than 3 minimum wages (about USD 750 at the time), which is the range of low socioeconomic status in the State of São Paulo. The sample is balanced across treatment arms: out of 17 variables, for only one (age) differences are statistically significant at the 10% level – consistent with what is expected to happen just by chance. Even in that case, the difference across groups is minimal.

4.2 Baseline beliefs

Next, we turn to parents' beliefs. If all parents were perfectly accurate at baseline, then there would be little room for an informational intervention to change their behavior to start with. Beliefs about attendance and grades were elicited through our baseline phone survey (non-incentivized). To make it simpler for parents to answer in the absence of enumerators, the belief elicitation procedure was coarse, requesting parents to pick the most appropriate brackets in each case.

Specifically, when it comes to attendance, parents were asked to provide their best estimate of how many times their child had missed math classes over a typical three-week period. Their answers were then compared to administrative records on students' attendance over the first quarter.³⁰ Parents had to choose one out of four brackets over the phone survey (no absences; 1 to 2 absences; 3 to 5 absences; or more than 5 absences).³¹ Parents were also asked to give their best estimate of their child's 1st quarter math grade. Again, parents had to choose one out of four categories (below average; adequate; good; or very good). In the Brazilian school system, GPA ranges from 0 to 10, with 5 as the passing grade.³²

Figure 2 showcases the distribution of parents' beliefs at baseline, contrasted with children's actual attendance and grades. Panel A overlays the distributions of parents' answers and administrative records, while Panel B documents the gap between the two – whereby positive values indicate over-optimistic parents (those who believe their children are less absent than they actually are, or that they have higher grades than they actually have).

[Figure 2]

³⁰We asked the question about 3 weeks prior to frame it consistently with the information they would (potentially) receive over text messages. Since attendance data from the Education Secretariat comprises the entire span of the 1st quarter, we scale it down proportionately to generate its three-week equivalent.

³¹See Supplementary Appendix C for the full script.

³²To compare those with administrative records, we define *below average* as a GPA below 5, *adequate* as 5-6; *good* as 7-8, and *very good* as 9-10.

On average, parents are over-optimistic about their children’s attendance. Similar to [Bergman \(2017\)](#), 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. Interestingly, however, the same is not true for GPA: the sample is about evenly distributed across over-optimistic, accurate and over-pessimistic parents. We take advantage of that variation for documenting heterogenous treatment effects of information and salience messages.

4.3 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, we start by looking at these manipulation checks. [Figure 3](#) displays statistics for platform usage and receipt of text messages across treatment arms.

[Figure 3]

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 H](#), 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.

4.4 What are the effects of child-specific information and salience messages?

We estimate the effects of the information and salience messages at the end of the school year on participants’ attendance in math classes, math GPA, grade promotion status, and

math scores in Saresp (São Paulo State’ standardized test).³³ Table 3 presents the results.

[Table 3]

Results are as follows. First, information significantly improves educational outcomes relative to the pure control group. Average attendance within the pure control group is already quite high: participants attend 87.5% of math classes even without school messages. Nevertheless, information significantly increases it by 2.1 percentage points – equivalent to five additional classes over the course of the academic year. Information also increases math GPA by 0.071 standard deviation, an effect size similar to that found in the literature (Berlinski et al., 2016). While math GPA is computed from tests graded by the teacher herself, information significantly increases standardized test scores (graded centrally, rather than by school personnel) by an even higher margin (0.107 standard deviation). This is a large effect size, as if treated students were 1 quarter ahead in school in terms of math knowledge.³⁴ We also find a significant and sizeable effect of information on the probability of advancing to high school – 2.6 percentage points, a nearly 1/3 reduction in the pure control grade repetition rate.³⁵

Second, we find that *salience* accounts for the lion’s share of the effects of information: not only do salience messages also significantly affect all educational outcomes, but also the ratio of information and salience coefficients is never lower than 89%. What is more, and salience effect sizes are sometimes larger – up to 126% of information effects. Information and salience coefficients are never statistically different at the 10% significance level.³⁶

Figure 4 displays the 4th quarter distribution of math attendance, math GPA and math standardized test scores for the different treatment arms.

[Figure 4]

Panels A through C show that the effects of information and salience messages spread across the whole distribution of each variable. That feature is especially visible in Panel C: relative to the pure control distribution of test scores, those of information and salience are equally shifted to the right. For attendance and standardized test scores, Kolmogorov-Smirnov tests significantly reject the hypothesis that salience and pure control distributions are the same. Across all outcomes, the test fails to reject differences between information and salience distributions at conventional significance levels.

³³The analyses restrict attention to the students who are still in school by the end of the year and who have taken the exam, in each case. Supplementary Appendix D compiles balance tests for each data source that we draw upon, documenting that differential attrition is not a concern for our estimates.

³⁴Such statement is based on comparing the average performance in SARESP of ninth graders and that of high-school freshmen, and dividing the difference in averages by 4 to compute expected quarterly learning.

³⁵The State cost of grade repetition per student at the 9th grade was estimated then at R\$ 4,000 (about USD 1,230 at the time).

³⁶Supplementary Appendix E analyzes heterogeneous treatment effects by child’s gender. Effects are primarily driven by caregivers of boys. Since such heterogeneous responses are not driven by differences in the effects of information and salience messages, we refer the interested reader to that section.

Are the effects of information and salience heterogeneous according to students' effort at baseline? Intuitively, one would expect those with lower attendance at baseline to benefit the most from an informational intervention that discloses such behavior to parents. Table 4 replicates the analyses separately for students below- and above-median attendance at baseline.

[Table 4]

Strikingly, salience effects are actually *higher* than those of information among below-median attendance students (statistically significant at the 5% level for GPA and grade promotion). The effect of salience on math GPA is almost two-fold that of information (nearly 0.12 s.d.), and nearly 50% higher for grade promotion. At the same time, their effects are no different among students above median baseline attendance. That result showcases that not only can salience messages in fact have larger effects than informational interventions, but also that that might happen precisely among the subset of participants who would presumably benefit the most from belief updating in the absence of attentional constraints.³⁷

Why is it that information and salience improve student's educational achievement? To understand the behavior change behind improved educational outcomes, we can take advantage of end line survey data on parents' and students' behavior and time use. When it comes to parents' behavior, students were asked 12 questions about how often (never, almost never, sometimes, almost always, or always) their parents engage in different activities. Based on those, we create 3 summary measures of of parental behavior (standardizing each variable and averaging across them within summary measures): *academic activities* (comprising help with homework, help with organizing school materials, participation in school meetings, and conversations with teachers); *incentives* (comprising words of incentives to attend school, to be on time, to study, and to read); *talk* (comprising conversations about homework, about grades, about the day at school, and about classes). We analyze *parent's aspirations* separately, an indicator variable equal to 1 if the student states at end line that their parents believe s/he would go to college, and 0 otherwise. Table 5 presents treatment effects on those measures parent's behavior and aspirations.

[Table 5]

When it comes to 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 each of the following activities: studying at home on weekdays; studying at home on weekends; studying at home the day before an exam; reading a book; reading the newspaper; reading

³⁷Why are salience effects larger among low-attendance students? One possibility is that information about absenteeism is not very surprising in this group – and hence, less able to capture parents' attention relative to salience messages. However, we cannot rule out that other mechanisms are also at play. For instance, it could be that negative information leads parents to corporally punish children, and that this partially backfires – in line with the literature on its negative effects on learning outcomes [Gershoff \(2002\)](#). We unfortunately have no data on corporal punishment in the context of this study.

magazines; watching TV; browsing the internet or on social media; and helping with house chores. Based on those, we create 3 summary measures of students' behavior (standardizing each variable and averaging across them within summary measures): *academic activities* (the first three items); *reading activities* (the next three) and *other activities* (the last three ones). Table 6 presents treatment effects on those measures of students' behavior.

[Table 6]

We find that both information and salience messages lead parents to ask their children significantly more about school and to incentivize studying to a greater extent. Moreover, both significantly induce higher aspirations about their children's making it to college. Children in treated households report engaging in academic and reading activities to a greater extent.

Spillovers from treatment effects are very large. Relative to the pure control group, students whose parents were assigned not to receive message within classrooms where other parents do improve often as much or only slightly less than those assigned to information or salience messages. This is true both when it comes to educational outcomes and to the behavioral responses underlying those changes. Effects on parental behavior (reported by students) are especially surprising. While the increase in parents' *incentives* within the control within classroom (relative to the pure control group) is about 1/3 than of salience messages, and that on aspirations, 50% lower, those are still statistically significant. What is more, the increase in parent's help with academic activities and conversations about school are nearly identical to those among treated parents. This suggests that spillovers are not merely driven by peer effects: parents indeed change behavior even when they are not targeted by systematic school communication.

While participating schools (other than in the pure control group and in the schools assigned to nudges) could send up to 2 text messages per month about school events (to all participating parents in that school), only about 25% of them use this functionality at all during the 18 weeks, so we find it unlikely that such low-intensity and scarce communication would drive spillover effects. More likely, parents react to either (or a combination of) their children's attempt at engaging to a greater extent, interactions with other parents, or interactions with the math teacher.

4.5 Are salience effects short-lived?

Are the effects of salience messages short-lived? One could hypothesize that parents initially react to salience messages because they infer that they must be receiving a message from the school due to their children's high absenteeism or low grades. Parents would, however, eventually realize they were misled, a stop reacting to salience messages. This subsection tests this hypothesis.

To study this question, we analyze the dynamics of treatment effects, taking advantage of the fact that we have access to quarterly data on math attendance and GPA from the

Education Secretariat administrative records. Figure 5 displays how each outcome varied over the course of the school year, across the information, salience and pure control groups.

[Figure 5]

Panel A displays data on attendance, which shows a clear decreasing pattern over the course of the school year, starting above 91% and ending below 89% for all groups – but less so among the treated. Differences between groups become significant only in the 3rd quarter, and persist into the 4th quarter. What is more, differences between the salience and pure control groups *increase* over time: the effect size of salience increases from 1.3 to 2.1 percentage points.

Panel B displays data on grades, which conversely increase over the second half of the school year – but more so among the treated. As in the case of attendance, differences between the salience and pure control groups *increase* over time: the effect size of salience increases from 0.092 to 0.104 standard deviations between quarters.

The fact that salience effects increase over time is not merely a mechanical effect: data on attendance and grades is not cumulative, and those are computed only for the sample of students who do not drop out. In sum, at least within the 4-months length of our study, not only it is not the case that the effects of salience messages die out; they even increase over time.

Although we show that salience effects are not short-lived, it is still interesting to ask if parents systematically treat salience messages as evidence that their children’s behavior must be poor. Before looking directly at beliefs (which we do in section 5), we can take advantage of results on heterogeneous treatment effects in the previous subsection to gain some insight into this question. If parents were indeed systematically inferring bad news from salience messages, then the effects of salience and information messages should be equal for low-attendance students (since, in those case, parents would conclude exactly the same). For high-attendance students, however, their effects should be very different. Table 4, however, shows that both claims are falsified by the data: salience effects on math GPA and grade promotion are higher among students with low attendance at baseline, and the effects of information and salience messages are statistically identical for students with high attendance at baseline.

Section 5 turns directly to beliefs to investigate whether salience messages make parents more pessimistic about their children’s behavior in school.

4.6 Are effects driven by interactions between information and salience?

As discussed in section 3.2, 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 parents 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 some information about their own child’s school behavior *thanks to*

the *information treatment*. To test this hypothesis, this subsection investigates whether salience effects are smaller within the schools where students were assigned to either the salience treatment or control – but not to the child-specific information treatment.

Concretely, we estimate the following equation:

$$Y_{sci} = \alpha + \beta_1 salience_{sci} + \beta_2 info_{sci} + \beta_3 control_{s=treated,ci} + \beta_4 salience_{sci} \times 1\{s \in D\} + \varphi 1\{s \in D\} + \sum_{k=1}^K \gamma_k X_{scik} + \theta_s + \varepsilon_{sci} \quad (2)$$

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 $\beta_4 \leq 0$.

Following equation 2, Table 7 shows the results of the regression that also incorporates the sub-sample of schools where child-specific information treatment was *not* made available, augmenting equation 1 with an indicator variable equal to 1 for children within those schools, and 0 otherwise, and its interaction with the salience treatment indicator.

[Table 7]

We find overwhelming evidence that salience effects are *not* driven by interactions with the information treatment. The coefficient for the interaction is not negative for any of the educational outcomes of interest, and it is even positive and statistically significant for math GPA: for math grades, salience effects are 70% *higher* within schools where no parent received child-specific information.

One caveat is that the frequency at which teachers within sub-sample D filled in the platform weekly was slightly higher than that of other sub-samples (statistically significant at the 1% level; see Supplementary Appendix D). Implementing a bounding procedure to deal with selection, salience effects are no longer statistically larger within those schools, but it is still the case the effects of salience effects are not lower in the absence of information.³⁸ Hence, we conclude that the lack of differences between the effect sizes of information and salience messages is not driven by interaction between the two treatments.

4.7 Are 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 children’s behavior in school top-of-mind? Following Rogers and Feller (2016), while it might 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

³⁸We equalize the frequency at which teachers filled in the platform across sub-samples by dropping observations while respecting the cluster-structure of the data; see Supplementary Appendix H.

effects, studying the effects on educational outcomes of framing child-specific information *in relative terms* to their classroom median behavior that week.

Table 8 shows the results of the regression that also incorporates the sub-sample of schools where the *relative information* treatment was made available, augmenting equation 1 with an indicator variable equal to 1 for children whose parents received child-specific information framed relatively to the classroom median, and 0 otherwise.

[Table 8]

Similar to Rogers and Feller (2016), relative information effect sizes are larger for math attendance and grades, and notably for standardized test scores (an effect size of 0.141 standard deviations, about 50% larger than that of information itself). Having said that, its effect size on grade promotion rates is actually lower – less than 60% – than that of child-specific information alone. While richer content might promote larger effect sizes on educational outcomes, our information intervention provides an appropriate counterfactual as it 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).

Most importantly, when it comes to the comparison between salience and relative information, the only instances for which their effects is statistically different is exactly grade promotion, for which it is the effect of salience that is statistically higher (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.

4.8 Are the effects of salience messages driven by teacher effects?

As discussed in subsection 4.4, spillover effects within the classroom are substantial: control 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 have to rely on the pure control group as a counterfactual, an important concern is whether our results are driven by differences in teacher behavior, as requiring them to fill in a platform weekly with information about their students may have made them feel they were being monitored.

Teacher effects would be consistent both with large within-classroom spillovers and with salience effects being proportional to the frequency at which teachers fill in the platform (documented in the last subsection). While having teachers provide information on their students at high frequency is a fundamental ingredient of informational interventions in developing countries (where real-time information systems are lacking), in the context of our study such effects make the comparison between information and salience difficult. For this reason, in this subsection we study the effects of a different program that did not require any inputs by teachers.

As introduced in section 3.2.2, for a sub-sample of participants we deliver nudges over weekly text messages, reaching parents directly without informational requirements or the

need to involve teachers at all. Such program (Eduq+, powered by EdTech Movva) shares weekly suggestions of activities for parents to do with their children – none of them 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 conflicts back in the day. Nudges are structured around sequences in a format inspired by READY4K! (York et al., 2017); see Supplementary Appendix B for examples of messages.

The main challenge of using that sub-sample, as also discussed in section 3.2.2, is that students within those schools were not statistically identical at baseline to those of our main sample. The reason is that 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 that cannot be controlled for. To deal with this concern, we take advantage of the fact that our program was ran only 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.

The differences-in-differences estimator identifies the causal effects of the nudge program under the assumption that student outcomes in nudge schools and in pure control schools *would not have changed differentially* in the absence of the program. 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:

$$Y_{scit} = \alpha + \beta nudge_{sci} \times Post_t + \theta Post_t + \gamma nudge_{sci} + \varepsilon_{scit} \quad (3)$$

where Y_{scit} denotes the outcome of interest for student i in classroom c at school s on quarter t ; $nudge_{sci} = 1$ for schools within sub-sample E, and 0 for pure control schools; $Post_t = 1$ if $t \geq 3$, and 0 otherwise; and ε_{scit} stands for the error term. We are interested in testing $\beta = 0$.

Figure 6 displays the quarterly evolution of math attendance and GPA within the nudge and pure control groups (sub-samples B and D).

[Figure 6]

When it comes to attendance, Panel A shows that there were no differences in baseline across groups. As before, we see a downward trend over the course of the school year – but much less so within schools targeted by nudges, which end up with 2.2 percentage points higher attendance in math classes at the last quarter. Turning to math grades,

differences in baseline GPA are apparent in Panel B; having said that, outcomes were moving in parallel for the two groups before the onset of the program. During the last two quarters, outcomes for nudge schools start trending upward, fully catching up in math grades with pure control schools already by the third quarter – a differential improvement of 0.143 standard deviations relative to baseline. This is a huge effect size, equivalent to 1.5 quarters ahead in school – way above what one would expect merely driven by mean reversion. What is more, no mean reversion is observed at the 2nd quarter.

Figure 7 displays non-parametric estimates of the effects of nudges on math attendance and GPA relative to the control group, with the first quarter as the reference period.

[Figure 7]

Panels A and B document that no statistically significant difference between the nudge and pure control groups arose before the start of the program. When it comes to math attendance, this difference becomes significant and increases to a 2.1 p.p. on the 3rd quarter and 2.2 p.p. in the 4th quarter. For math GPA, the difference becomes significant on the third quarter (0.143 s.d.), and marginally insignificant on the last quarter even though its effect size (0.09 s.d.) is exactly the same that we find for the main sample (although less precisely estimated given its much smaller sample size).

All in all, results for nudges support the claim that the effects of salience messages are *not* driven by teacher effects.

5 Beliefs

While parents’ and children’s behavior reacts just as much to salience messages as it does to child-specific information, that could arise out of (for simplicity) two polar scenarios for parents’ beliefs. At one extreme, perhaps salience messages induce parents to *acquire information themselves*, updating beliefs just as much as they would do in response to child-specific information. At the other extreme, perhaps only child-specific information makes parents more accurate, but both information and salience messages change parents’ behavior – *regardless* of underlying beliefs. Understanding which is the case matters: the two instances might have very different welfare implications. To do that, this section analyzes treatment effects on parents’ end line beliefs, as well as heterogeneous treatment effects based on parents’ baseline accuracy. By taking advantage of heterogeneity in the content entered into the platform by teachers, we can also test how parents react differentially to different messages (as in [Dizon-Ross, 2019](#)) and, most importantly, whether salience messages generate similar patterns.

We start by analyzing treatment effects on parents’ beliefs. If beliefs are a key mediator of parents’ behavior change, then information and salience should make parents more accurate. At the end line phone survey, parents were asked to provide their best estimate of how many times their child had missed school over the 3rd quarter, choosing the bracket that most closely matched their beliefs (no absence; 1 to 2 absences; 3 to 5 absences; 6

to 8 absences; or more than 8 absences; see Supplementary Appendix C).³⁹ Parents with guesses in the right bracket were considered accurate. Similarly, parents were asked to give their best estimate of their child’s 3rd-quarter math grade. Again, parents had to choose one out of four categories (below average; adequate; good; or very good). To compare those with administrative records, we define *below average* as a GPA below 5, *adequate* as 5-6; *good* as 7-8, and *very good* as 9-10.

Table 9 presents treatment effects on end line accuracy, focusing on the probability of mistakes in columns 1 and 2 (for attendance and grades, respectively) and directly on parents’ reported beliefs at end line in columns 3 and 4 (for attendance and grades, respectively).

[Table 9]

Unlike most papers about the effects of informational interventions, we document that messages tailored to the specific circumstances of the recipient did *not* make subjects’ beliefs more accurate with respect to the dimension reported on. Information significantly *increased the likelihood of mistakes* with respect to math attendance (5.5 percentage points more likely to choose the wrong bracket for 3rd-quarter absences; column 1). Column 3 shows that parents in the information treatment reported, on average, significantly less absences than those in the pure control group. In contrast, salience messages did not significantly affect end line beliefs or accuracy with respect to math attendance.

We claim that this pattern is consistent with attentional effects. In our experiment, parents in the information group received messages with their child’s absences over the course of the 3 previous weeks, but had to report beliefs about their child’s absences *over the course of the previous quarter* – a (much) higher figure for most students. This is in contrast to what is typically done in the literature, which elicits beliefs over the same frequency at which information was conveyed. As an example, [Dizon-Ross \(2019\)](#) states that “[m]ean beliefs about academic performance were measured by asking parents about the same performance metrics that were later delivered in the intervention report cards (...) We used the same measure later used in the intervention so that any gaps between believed and true performance represent belief inaccuracies, not differences between measures” (p. 13). Such elicitation procedure cannot distinguish belief updating from *anchoring* – a real concern once salience is brought into the spotlight. Our finding highlights the importance of eliciting beliefs *in a different a unit* from that at which information is conveyed.

When it comes to accuracy about 3rd-quarter math GPA, Table 9 documents that information increases parents’ accuracy. Even though we never directly conveyed information on grades, information on attendance, punctuality and homework completion presumably correlates with academic achievement. Having said that, salience messages increase accuracy to an *even greater extent*: parents assigned to this treatment were 8.2 percentage points less likely to choose the wrong bracket, compared to 6.9 percentage points less in

³⁹We focused on the 3rd quarter because 4th quarter report cards had still not been made available to parents at the time of the survey.

the information group (column 2). This is driven by parents becoming less optimistic in both arms, but especially so within those receiving salience messages (column 4).

All in all, the patterns we document are consistent with inattention and “learning through noticing” (Hanna et al., 2014). Interestingly, the fact that salience makes parents more accurate about grades but not about attendance suggests parents might treat the former as a sufficient statistic for their children’s school behavior.

Next, we turn to heterogeneous treatment effects by parents’ baseline accuracy. Regardless of the specifics of the belief elicitation procedure at end line, if belief updating is the key mechanism behind parents’ behavior change, then information should have larger effects among parents that were not already accurate at baseline. We estimate the following equation:

$$Y_{sci} = \alpha + \beta_1 salience_{sci} + \beta_2 info_{sci} + \beta_3 info_{sci} \times inaccurate_{sci} + \beta_4 Control_{s=treated,ci} + \delta inaccurate_{sci} + \sum_{k=1}^K \gamma_k X_{scik} + \theta_s + \varepsilon_{sci} \quad (4)$$

where $inaccurate_{sci} = 1$ for parents who were *not* accurate at baseline, and 0 otherwise. We are interested in testing $\beta_2 = 0$ and $\beta_2 \leq \beta_3$.

Table 10 presents the results for treatment effects on educational outcomes. Following equation 4, the indicator of the child-specific information treatment arm is interacted with an indicator of whether the parent was *inaccurate* at baseline (with respect to 1st-quarter math attendance in columns 1-4, and to 1st-quarter math GPA in columns 5-8). As such, the coefficient for child-specific information captures treatment effects for parents already accurate at baseline.

[Table 10]

Results are estimated with much less precision because the sample is restricted to those who answer the baseline survey (used the compute accuracy) – about 1/3 of the sample size in Table 3. Regardless, not only does information systematically affects parents *already accurate* at baseline, but also its effect sizes are often *larger* among accurate parents than among those initially inaccurate. When it comes to accuracy about 1st-quarter math attendance, child-specific information actually *decreased* attendance among those whose parents were inaccurate at baseline, while it increased attendance otherwise (column 1). When it comes to accuracy about 1st-quarter math grades, child-specific information significantly increased 4th-quarter math attendance, GPA and standardized tests scores *only* among those who were already accurate at baseline (columns 5-7). Even though child-specific information significantly also increases grade promotion among parents inaccurate at baseline, its effect size is *nearly 3-fold* among parents already accurate at baseline (col-

umn 8). Such patterns are *inconsistent* with effects being driven by belief updating.⁴⁰

Last, we analyze heterogeneity in treatment effects on educational outcomes with respect to the content entered into the platform by teachers. We interact the indicators for the information and salience groups with the average score entered for each dimension (attendance / punctuality / homework completion) separately. In the main paper, we restrict attention to punctuality for the sake of illustrating the main patterns; effects are qualitatively identical across the different dimensions. Weekly scores range from 1 to 4 (see subsection 3.3). For punctuality, an average score of 4 indicates a child that was never late, whereas an average of 1 indicates a child late to at least 5 classes every three-week cycle. Because teachers did not fill in any content for pure control schools, the estimates are relative to the control group within classroom (we include classroom fixed-effects). Despite large spillovers, which make treatment effects hard to detect, we are interested in the interaction coefficients – and, especially, in whether those differ across information and salience messages. Table 11 presents the results.

[Table 11]

As one would expect, being more punctual correlates with better educational outcomes across the board. Also in line with the literature, the coefficient of the interaction of information with average content reported is positive (significantly so for grade promotion), which means that parents double down on students who are already doing well. The unexpected pattern in Table 11 is the fact that the interaction of *salience* with average content is *also positive* (significantly so for attendance and grades). This is yet another piece of evidence consistent with “learning through noticing” (Hanna et al., 2014).

Together, results point out that parents collect information themselves when primed about the importance of attendance, punctuality and homework completion, even in the absence of direct inputs for belief updating. Having said that, the fact that they become more accurate about their children’s grades at end line seems second-order to the effects of the intervention on educational outcomes, as effects are actually larger for those already accurate at baseline. This suggests parents end up more accurate but change behavior irrespective of underlying beliefs. Hence, children’s educational outcomes improve essentially due to salience effects.

6 Attentional constraints

The evidence on the previous section already overwhelmingly supports attentional constraints as the main driver of the large effects of our interventions on educational outcomes. In this section, we look at more direct evidence that effects are driven by inattention.

⁴⁰Supplementary Appendix F analyzes heterogeneous treatment effects by parents’ willingness to receive information at baseline. The interventions significantly improve children’s educational outcomes *even among parents with low willingness to receive information*, consistent with the inattention mechanism.

First, we study the extent to which subject-specific nudges have effects confined to that subject. Due to working memory limitations and heuristics such as *associativeness* (Kahneman, 2011), the fact that child-specific information was restricted to school behavior *within math classes* suggests its effects should be lower within educational outcomes for Portuguese. In contrast, nudges should affect math and Portuguese attendance and grades to the same extent – since those messages were not designed to be subject-specific.⁴¹ Table 12 showcases the results for parents’ end line accuracy (with respect to Portuguese 3rd-quarter attendance and grades, columns 1-2) and for students’ 4th-quarter Portuguese attendance, GPA and standardized test scores (columns 3-5).⁴²

[Table 12]

In Table 12, while treatment effects are still positive, those are statistically insignificant across most outcomes. In particular, the effect sizes of both information and salience messages on Portuguese standardized tests scores is *less than half* their direct effects on math standardized test scores.

Next, Figure 8 documents that effects of the nudge program using a differences-in-differences estimator, as in subsection 4.8, for both math and Portuguese 4th-quarter attendance (Panel A) and GPA (Panel B).

[Figure 8]

Restricting attention to the left column of both panels (which portrays the results of 1 nudge per week), Figure 8 shows that nudges have significant effects on Portuguese attendance, which increases by 1.4 percentage points – 2-fold the effect size of information or salience messages designed to be math-specific. With one message per week, nudges also have no significant effects on Portuguese GPA (but see below).

Second, we study the extent to which salience effects increase with the frequency of messages to parents. Under attentional constraints, effects sizes should increase with frequency if more messages are effective in raising that domain more often to the top of the mind. To do that, we take advantage of the research design in Bettinger et al. (2019). In this companion paper, we randomize the number of nudges parents receive each week. Parents assigned to receive 1 message per week receive 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

⁴¹It 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 nudges would still provide the appropriate benchmark for salience effects when content is not domain specific.

⁴²As 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.

parents to do it regularly – not just that week.⁴³

Figure 8 displays effect sizes of 1, 2 and 3 nudges per week on students’ math and Portuguese 4th-quarter attendance (Panel A) and GPA (Panel B) using a differences-in-differences estimator. It documents that effect sizes substantially increase with the frequency of nudges, for both math and Portuguese. With 3 nudges per week, math GPA increases by 0.176 standard deviations, almost 2-fold the effect of 1 message per week. What is more, at that frequency, the effect of Portuguese GPA (0.147 standard deviation) also becomes significant (at the 5% level).

Together, results not only demonstrate that attentional constraints are the key driver behind why child-specific information improves educational outcomes, but also corroborate the claim that nudges can magnify the effects of informational interventions by drawing on additional features to manipulate subjects’s attention.

7 Concluding Remarks

We show that while an intervention that informs parents about their children’s school behavior has large impacts on educational outcomes, an alternative intervention that tries to draw parents’ attention to those behaviors *without providing any inputs for belief-updating* improves those outcomes by just as much – and to an even greater extent among students with low attendance at baseline. We show that salience effects are not short-lived, and that they are not driven by interactions with information or by teacher effects.

Our findings are consistent with the effects of child-specific messages being driven by salience. Parents update beliefs in ways inconsistent with Bayesian updating in the absence of attentional constraints, as salience messages improve accuracy by at least as much as information. Even though parents seem to acquire information themselves, they still change behavior irrespective of underlying beliefs; in particular, information improves educational outcomes especially among parents that were already accurate at baseline.

We provide direct evidence that interventions that manipulate attention can induce larger effects at lower cost. The effects of more frequent messaging (evaluated by [Bettinger et al., 2019](#)) are up to 2-fold that of weekly messages with child-specific information. What is more, nudges (especially at higher frequency) have significant impacts across math and Portuguese, while the effects math-specific messages are mostly confined to that subject.

Our study builds on different recent experimental evaluations of school communication program, as well as on a growing body of evidence that suggests parents play a crucial role in shaping their children’s behavior and school performance ([Barnard, 2004](#); [Houtenville and Conway, 2008](#); [Nye et al., 2006](#)). Differences in parental inputs are viewed as an important cause of intergenerational inequality ([Becker and Tomes, 1979](#)), and family socio-economic status is a key factor behind variation in children’s educational achievement ([Woessmann](#)

⁴³From 2 to 3 messages per week, there are already some signs of saturation effects. 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.

and Hanushek, 2011). While poor and rich families differ across many dimensions, few seem as easy to address as their differential monitoring of children’s school performance. To that point, a growing education literature suggests parents can affect students’ educational behaviors and success when they receive proper information (e.g. Bergman, 2017; Bergman and Chan, 2017; 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., 2017). Our results qualify those findings, suggesting most of their effects could actually be driven by salience.⁴⁴

This paper also contributes to a booming literature that investigates cost-effective interventions to improve educational outcomes in developing countries.⁴⁵ 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. Our results point out that messages designed to capture parents attention can be a powerful cost-effective instrument to improve educational outcomes.

Our findings suggest that the effects of informational interventions can be obtained at lower cost – and even magnified – by interventions that manipulate attention, raising the salience of the decision they target. Presumably, this insight extends way beyond education. Of course, not all informational interventions can be replicated simply with messages that try to make the decision top-of-mind. For instance, 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, and 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. Even in those cases, though, the intervention effects could be at least partly driven by salience, to the extent that messages that do not address educational beliefs (in the former) or the moral implications of default (in the latter) might fail to capture subjects’ attention to the same extent. In any case, those studies suggest that manipulating attention is not trivial, and designing effective interventions requires careful piloting and evaluation.⁴⁶

⁴⁴Our study also contributes to the still scarce literature on behavioral educational interventions. Recent papers study interventions that tackle parents’ inertia and routine, (Avvisati et al., 2013; Banerji et al., 2013; Benhassine et al., 2015; Harackiewicz et al., 2002; Kraft and Rogers, 2015), including text messages, email reminders, and letters targeted at parents and students (Castleman and Page, 2015; Hoxby et al., 2013; Jensen, 2010). While the field of behavioral economics has been successfully applied to many areas, Education has received comparatively less attention so far (Lavecchia et al., 2014).

⁴⁵As Ludger et al. (2015) and others document, students in developing countries learn much less than students of the same age or grade in OECD countries. In those countries, the focus has recently shifted from merely increasing school enrollment to improving learning (Glewwe and Muralidharan, 2015).

⁴⁶Additional research is needed to understand how attention reallocation affects the underlying decision

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process and, ultimately, welfare in each case. When information drives parents to monitor children more closely, improving educational outcomes, it seems hard to make the case that this would not improve welfare even if not driven by underlying beliefs. In other domains, however, there might be no guarantee that behavior change triggered by informational interventions actually leads to welfare gains – 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 \(2018\)](#).

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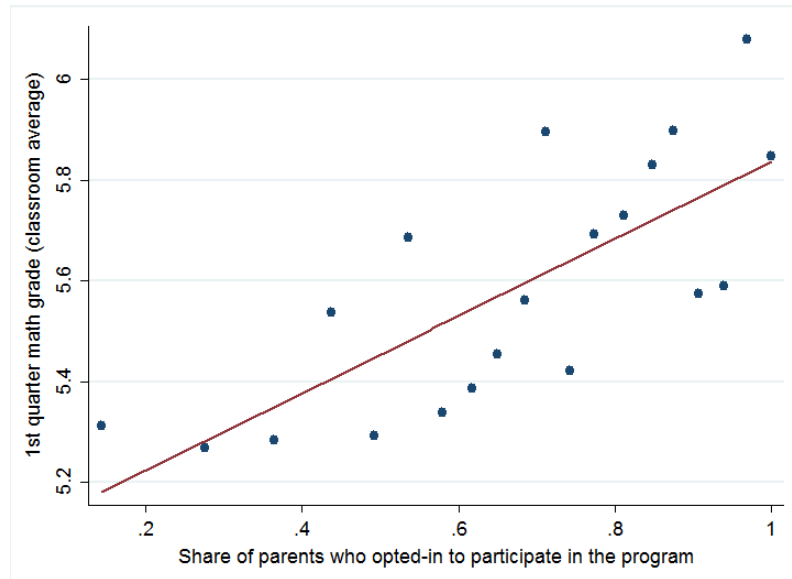
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Figures

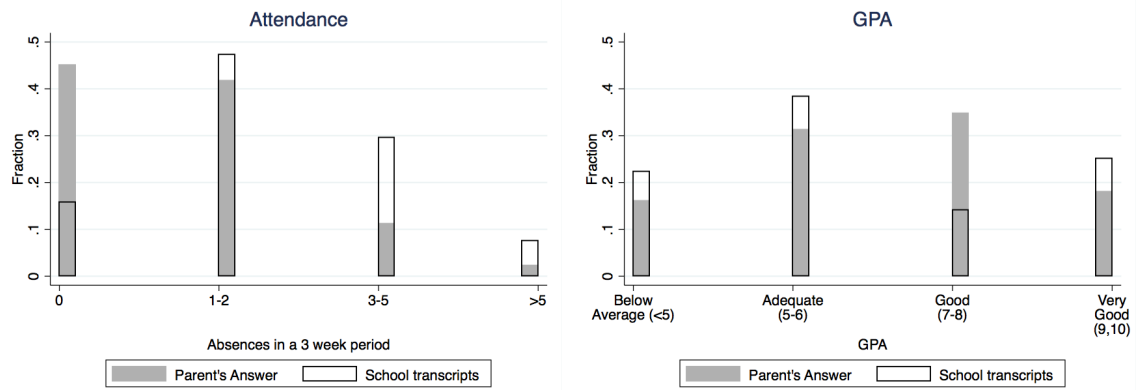
Figure 1: Correlation between classroom-level parental engagement and grades



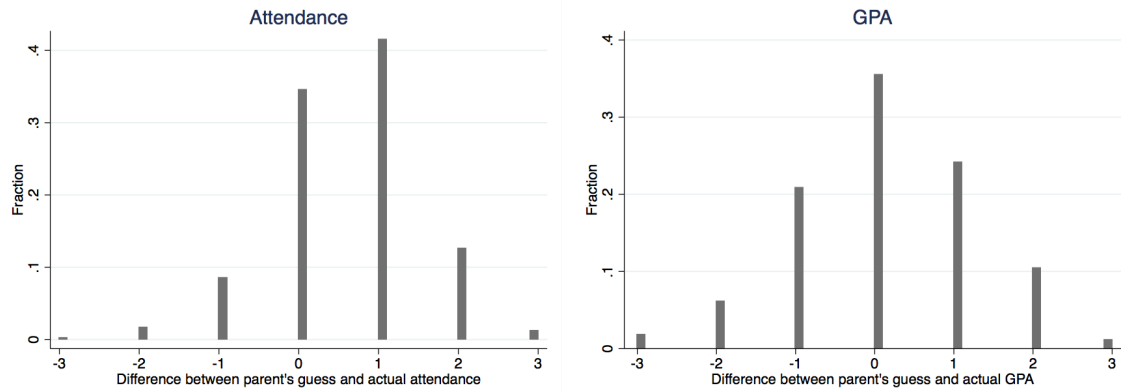
Note: Binned scatterplot of classrooms' average math grade in the 1st quarter against the share of parents in each classroom that signed up to participate in the Education Secretariat program, which we take as a proxy for parental engagement at baseline.

Figure 2: Parents' accuracy wrt their child's baseline attendance and GPA

Panel A: Parents' answers versus students' baseline performance



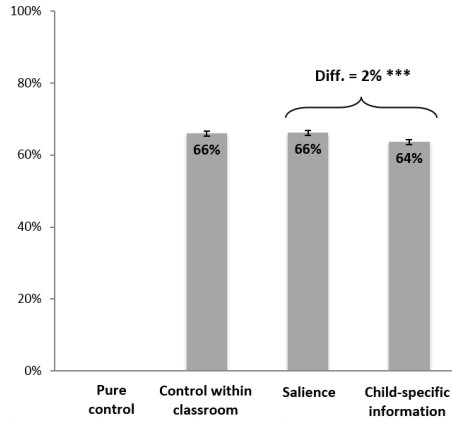
Panel B: Difference parents' answer and baseline performance



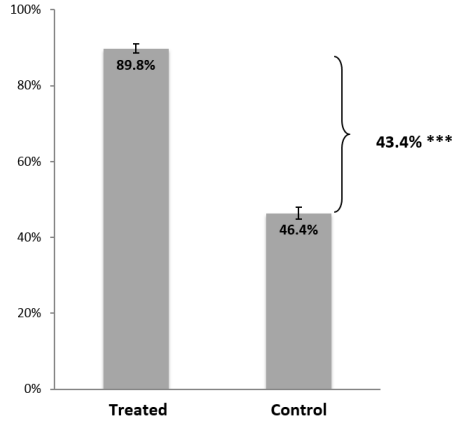
Note: Parents were asked at baseline to give their best estimate on how many times their child misses math classes on a period of three weeks, as well as on their performance in math classes. Data was then crossed with administrative records. Four categories were available for parents' answers on attendance (missed 0; 1-2; 3-5; more than 5). Administrative data register data on attendance on a quarterly basis (period of ~ 9 weeks) and was divided by 3 to validate parents' answers. Four categories were available for parents' answers on performance (below average; adequate; good; very good). The GPA has a 10 point scale, where 5 is the passing grade. Parents' answers below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10. Panel A shows parents' answers and school transcripts. Panel B shows the difference between parents' answers and students' performance. Note that the value zero indicates parents were accurate, positive values indicate they were pessimist and negative values indicate they were optimistic.

Figure 3: Manipulation Tests

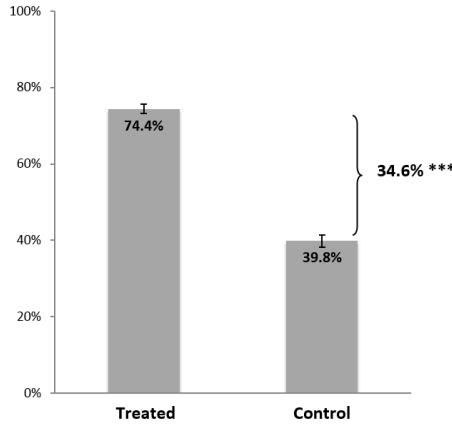
Panel A: Share of weeks teachers filled in the platform by treatment status



Panel B: Did parents acknowledge receiving text messages?



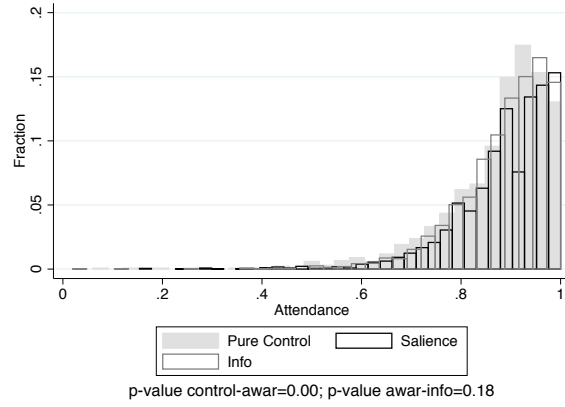
Panel C: Did students know their parents were receiving text messages?



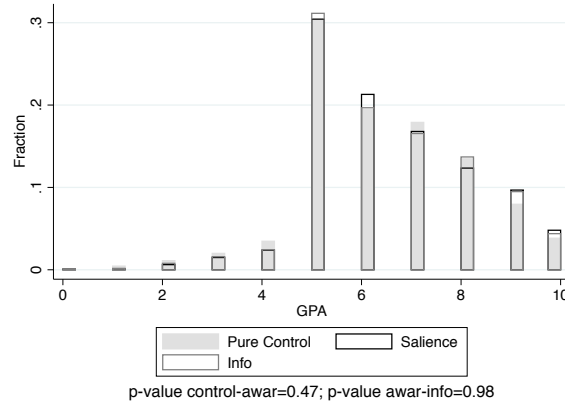
Note: 90% confidence interval. The difference between categories was estimated through a simple regression including fixed effect for strata, and standard errors were clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. In Panel A, data from teachers' platform were used, while Panel B and C used data from parents and students endline survey, respectively.

Figure 4: Distributional Effects

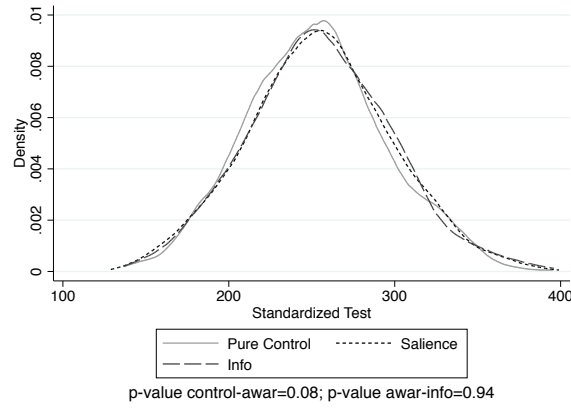
Panel A: Attendance



Panel B: GPA

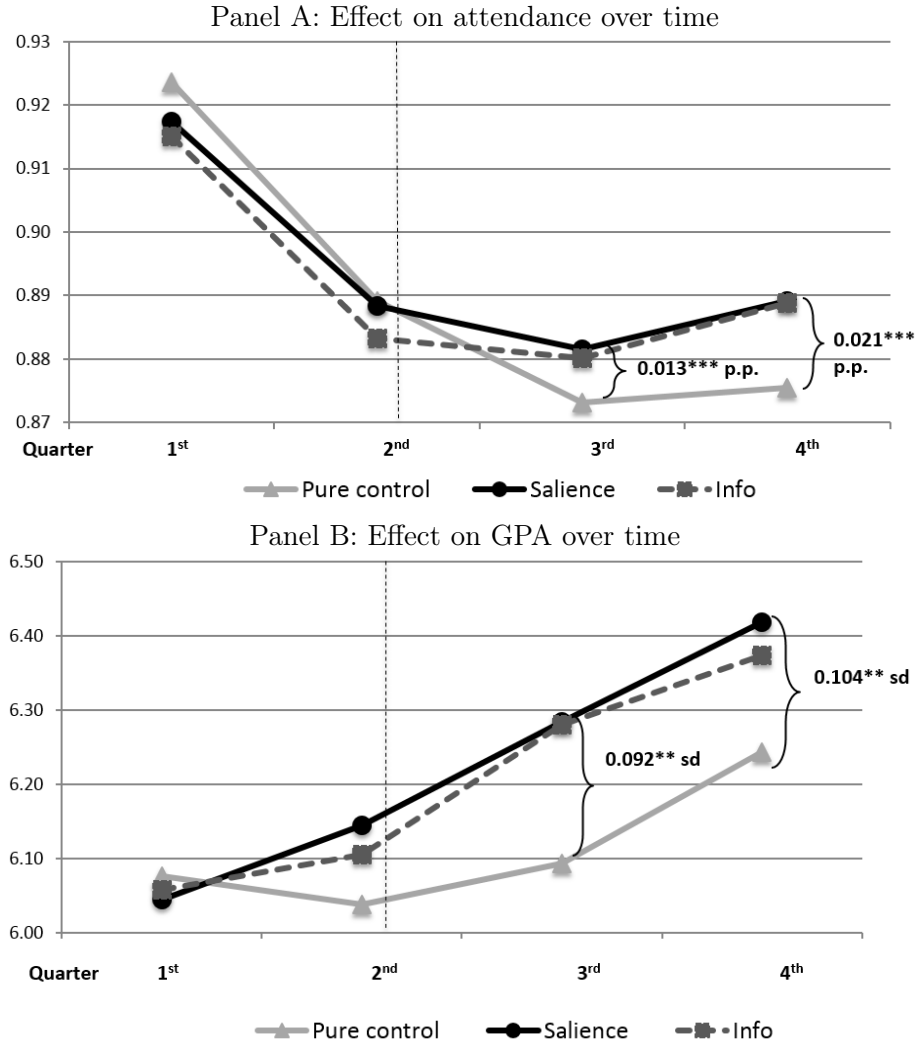


Panel C: Standardized test scores



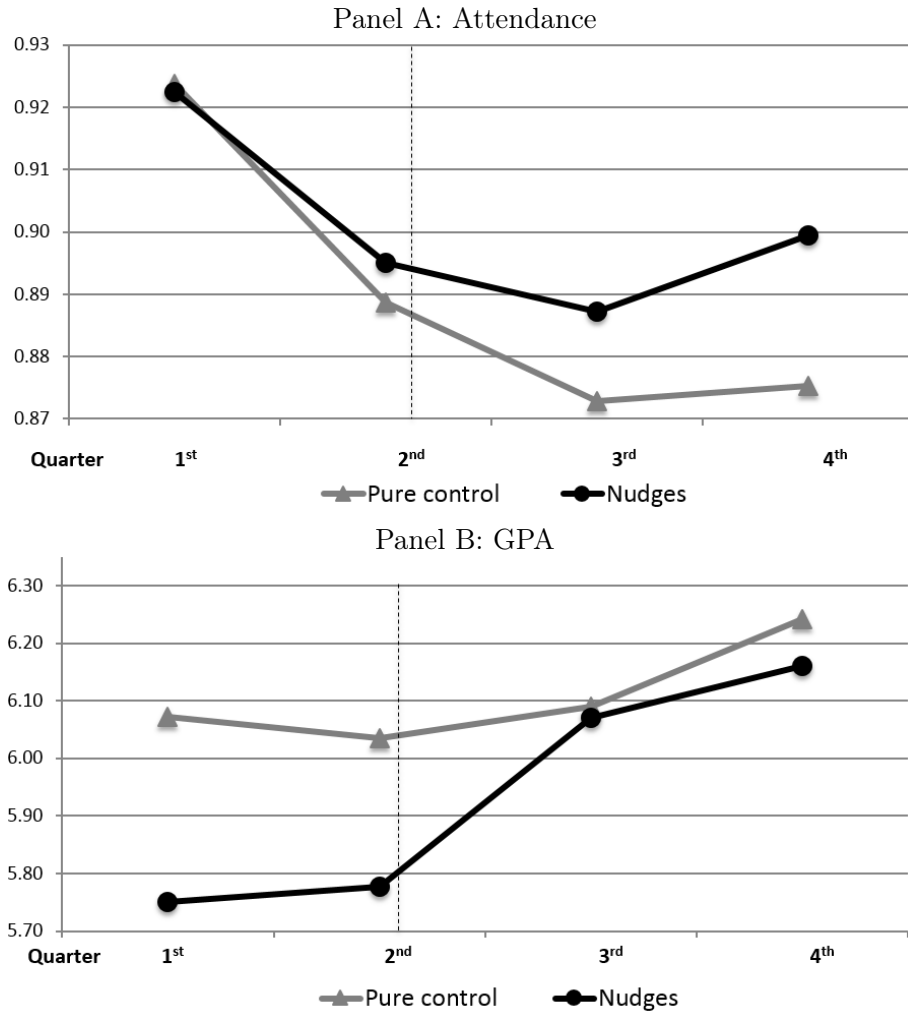
Notes: Panels A, B and C show the effect across the distribution of students' math attendance, GPA and standardized test scores for each treatment arm. Data from administrative records. Attendance is recorded in percentage points (0-1 interval). GPA is recorded in a 10-point scale, where 5 is the passing grade. 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.

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



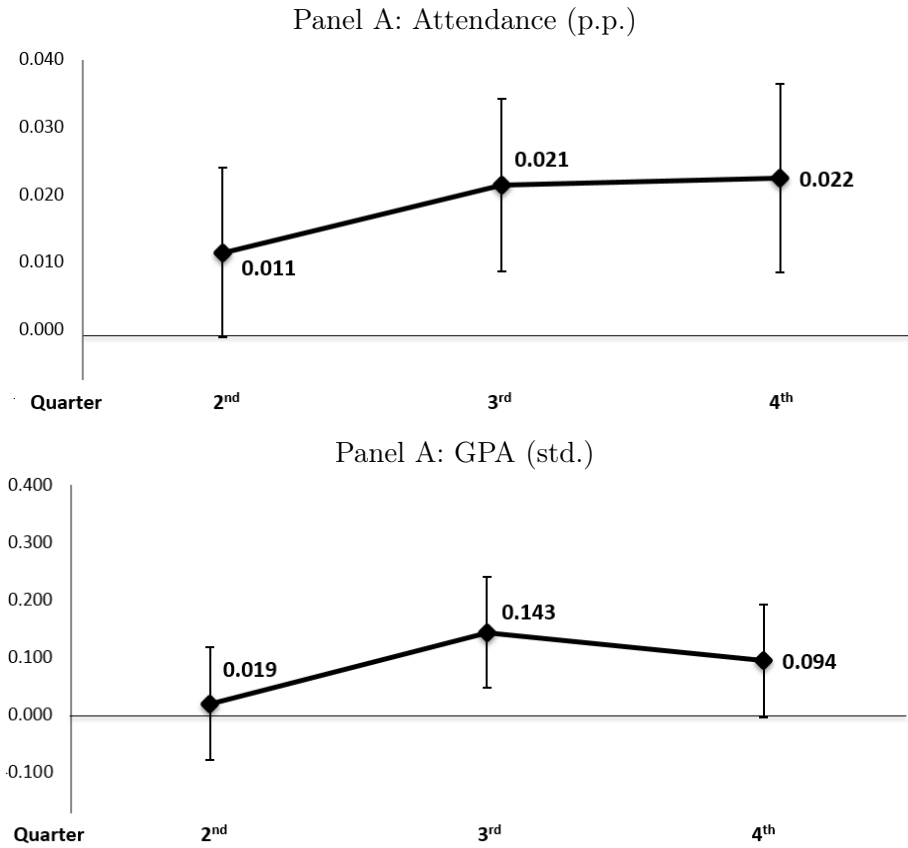
Note: Panels A and B show the raw data for attendance and GPA pre- and post-intervention, for treatment and control groups. Attendance is recorded in percentage points (0-1 interval). The GPA has a 10 point scale, where 5 is the passing grade. The intervention started at the beginning of the third quarter and lasted until the end of the fourth quarter. Attendance and GPA are available for each of the fourth quarter, as part of students' transcripts, while promotion rate and standardized test are only available at the end of the school year. The coefficients on the graph show the difference between the salience and pure control group from a model estimated with student controls, strata fixed effect and standard errors clustered at the classroom level, as specified by equation 1. Coefficients for GPA are in standard deviation, where GPA was 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. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Figure 6: Nudges - effect by quarter



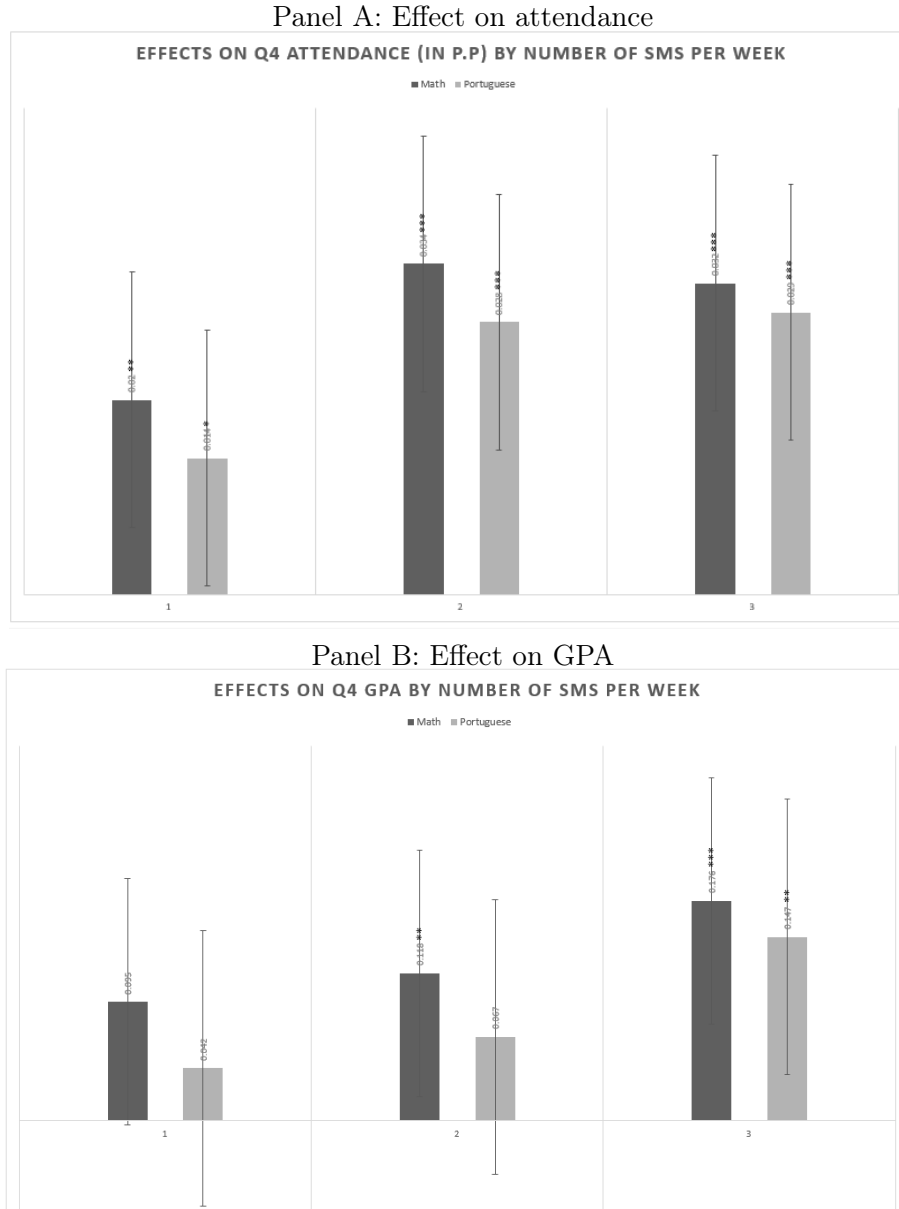
Note: Panels A and B show the raw data for attendance and GPA pre- and post-intervention, for treatment (engagement) and control groups of the theory-based nudging program. Attendance is recorded in percentage points (0-1 interval). The GPA has a 10 point scale, where 5 is the passing grade. The intervention started at the beginning of the third quarter and lasted until the end of the fourth quarter. Attendance and GPA are available for each of the fourth quarter, as part of students' transcripts, allowing us to estimate a differences-in-differences model. Promotion rate and standardized test, however, are only available at the end of the school year (post-intervention).

Figure 7: Differences-in-differences coefficient of nudges, by quarter



Note: Panels A and B show the differences-in-differences estimates from equation 2 for the nudge program by quarter, where the first quarter is the reference period. GPA was 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. 90% confidence interval with standard errors clustered at the classroom level are showed. A dummy variable for the control group within class was also included in the model, as well as its interaction with a time dummy. Attendance and GPA are available for each quarter, as part of students' transcripts, allowing us to estimate a differences-in-differences model. Promotion rate and standardized test, however, are only available at the end of the school year (post-intervention).

Figure 8: Effect sizes as a function of the number of nudges per week



Note: Based on [Bettinger et al. \(2019\)](#)'s results. Panels A and B show the raw data for attendance and GPA pre- and post-intervention, for treatment and control groups. Attendance is recorded in percentage points (0-1 interval). The GPA has a 10 point scale, where 5 is the passing grade. The intervention started at the beginning of the third quarter and lasted until the end of the fourth quarter. Attendance and GPA are available for each of the fourth quarter, as part of students' transcripts, while promotion rate and standardized test are only available at the end of the school year. The coefficients on the graph show the difference between the salience and pure control group from a model estimated with student controls, strata fixed effect and standard errors clustered at the classroom level, as specified by equation 1. Coefficients for GPA are in standard deviation, where GPA was 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. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Tables

Table 1: Selection at opt-in

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

Notes: Because parents who did not opt-in to the program did not take up the baseline survey, we only have limited on them from the Education Secretariat's administrative records: students' gender and age, their first quarter attendance and GPA, and whether the family is a Bolsa-Família (Brazil's flagship conditional cash transfer) recipient. * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table 2: Descriptive statistics and balance

	Sub-sample means				ANOVA test	Number of observations
	Pure Control	Control within classroom	Salience	Child-specific information	p-value	
Panel A: Student characteristics						
Female	0.48	0.50	0.51	0.51	0.14	15589
Age	14.71	14.72	14.71	14.75	0.03	15595
Brown	0.34	0.35	0.34	0.35	0.48	15592
Black	0.06	0.05	0.06	0.06	0.45	15592
Portuguese GPA (0-10)	6.18	6.19	6.13	6.13	0.36	15437
Math GPA (0-10)	5.94	5.99	5.92	5.90	0.25	15453
Portuguese attendance	0.91	0.92	0.92	0.91	0.68	15480
Math attendance	0.91	0.91	0.91	0.91	0.30	15440
Panel B: Adult responsible for student						
Mother	0.78	0.76	0.76	0.76	0.28	15597
Age	40.43	40.25	40.34	40.42	0.86	15461
Brown	0.34	0.34	0.34	0.34	0.65	15593
Black	0.07	0.06	0.07	0.07	0.80	15593
Middle school incomplete	0.32	0.30	0.31	0.31	0.66	15591
Middle school complete	0.30	0.26	0.28	0.27	0.17	15591
High School	0.31	0.33	0.30	0.31	0.13	15591
Earns less than 1 MW (~ \$250)	0.17	0.18	0.17	0.18	0.63	15593
Earns between 1 - 3 MW	0.42	0.45	0.45	0.46	0.41	15593

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. Data on students' gender, age, GPA and attendance from administrative records, and data on students' race and on caregivers' characteristics from the face-to-face baseline survey within those who opted-in to participate in program.

Table 3: Effects on attendance, grades and grade promotion

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Child-specific information	0.021*** [0.006]	0.071** [0.032]	0.026** [0.012]	0.107** [0.047]
Salience	0.021*** [0.006]	0.090*** [0.032]	0.032*** [0.012]	0.095** [0.047]
Control within classroom	0.018*** [0.006]	0.070** [0.031]	0.030** [0.012]	0.085* [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	12577	12577	12577	12577
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: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table 4: Heterogeneous treatment effects (by students' attendance at baseline)

	Below-median attendance (1st quarter)				Above-median attendance (1st quarter)			
	(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.)
Child-specific information	0.023*** [0.008]	0.066* [0.040]	0.032** [0.016]	0.095* [0.052]	0.019*** [0.006]	0.095*** [0.037]	0.018* [0.009]	0.135** [0.059]
Saliency	0.023*** [0.008]	0.116*** [0.039]	0.045*** [0.016]	0.107** [0.053]	0.018*** [0.006]	0.070* [0.037]	0.016 [0.010]	0.092 [0.058]
Control within classroom	0.018** [0.008]	0.076* [0.040]	0.041** [0.016]	0.108** [0.052]	0.017*** [0.006]	0.077** [0.036]	0.015 [0.010]	0.069 [0.058]
Control mean	0.85	-0.23	0.92	-0.12	0.90	0.25	0.96	0.13
p-value diff. [Info] -[Saliency]	0.95	0.01	0.05	0.70	0.54	0.26	0.65	0.16
Observations	6862	6862	6862	6862	5715	5715	5715	5715
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: A GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Students with baseline attendance below or equal to the class median were determined as low-performing, and students with baseline attendance above the median were determined as high-performing for the purposes of this analysis.

Table 5: Effects on parents' behavior and aspirations

	(1) Academic activities	(2) Incentives	(3) Talk	(4) Aspirations
Child-specific information	0.092* [0.051]	0.075* [0.042]	0.147*** [0.044]	0.092** [0.036]
Salience	0.064 [0.050]	0.096** [0.041]	0.122*** [0.043]	0.095*** [0.036]
Control within classroom	0.073* [0.050]	0.033* [0.042]	0.111*** [0.043]	0.064** [0.037]
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: 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. At the endline survey, students 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). At end line, students were also asked if their parents believed they would go to college and a dummy variable for *parent's aspirations* was created, which assumes value one if parents do believe the student will go to college and zero otherwise.

Table 6: Effects on students' time use

	(1) Academic activities	(2) Reading activities	(3) Other activities
Child-specific information	0.151*** [0.051]	0.116* [0.065]	-0.108** [0.054]
Salience	0.123** [0.050]	0.113* [0.060]	-0.110** [0.052]
Control within classroom	0.130*** [0.050]	0.127* [0.063]	-0.089** [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: 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. At the endline survey, students were requested to answer how many hours per day (0, 15 minutes, 30 minutes, 1 hours, 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).

Table 7: 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.)
Child-specific information	0.021*** [0.006]	0.070** [0.032]	0.026** [0.012]	0.108** [0.047]
Salience	0.017*** [0.006]	0.070** [0.033]	0.027** [0.012]	0.101** [0.048]
Salience x No-information sub-sample	0.001 [0.004]	0.049* [0.029]	0.004 [0.009]	0.015 [0.042]
Control within classroom	0.014** [0.006]	0.062* [0.033]	0.026** [0.012]	0.094** [0.047]
Observations	12577	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table 8: 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*** [0.006]	0.069** [0.032]	0.029** [0.012]	0.097** [0.047]
Relative information	0.022*** [0.007]	0.078* [0.041]	0.017 [0.014]	0.141** [0.058]
Salience	0.021*** [0.006]	0.090*** [0.032]	0.032*** [0.012]	0.095** [0.047]
Control within classroom	0.018*** [0.006]	0.070** [0.031]	0.030** [0.012]	0.085* [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: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table 9: Accuracy of parents' end line beliefs about students attendance and grades

	Mistakes (end line)		Beliefs (end line)	
	absences (1)	grades (2)	absences (3)	grades (4)
Child-specific information	0.0545*** [0.0195]	-0.0688** [0.0300]	-0.113* [0.0594]	-0.159 [0.105]
Saliency	0.0258 [0.0202]	-0.0821*** [0.0296]	0.0728 [0.0588]	-0.236** [0.104]
3rd-quarter math absences			0.169*** [0.0138]	
3rd-quarter math GPA				0.317*** [0.0217]
Observations	4,279	3,303	2,098	1,605
Strata fixed-effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R-squared	0.030	0.055	0.031	0.066

Note: Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Parents were asked at baseline to give their best estimate of their child performance in math classes. Data was then crossed with administrative records and parents who estimated exactly right were determined as accurate, those who estimated below were determined optimistic and those who estimated above were determined pessimist. Four categories were available for parents' answers on performance (below average; adequate; good; very good). Administrative data register data on attendance and GPA on a quarterly basis (period of ~ 9 weeks). The GPA has a 10 point scale, where 5 is the passing grade. Parents' answers below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10. Parents were also asked at endline to give their best estimate of how many times their child missed school and what was their final math GPA in the past quarter. Five categories were available for parents' answers on attendance (missed 0, 1-2; 3-5; 6-8; more than 8) and parents answers for GPA were absolute values from 1-10. Data was then crossed with administrative records and a dummy variable were created, where parents who estimated right received value 1 and those who estimated wrong received value 0.

Table 10: Heterogeneity by parents' baseline beliefs

	Accuracy wrt baseline attendance				Accuracy wrt baseline GPA			
	Math attendance (1)	Math GPA (2)	Math test score (3)	Grade promotion (4)	Math attendance (5)	Math GPA (6)	Math test score (7)	Grade promotion (8)
Child-specific information	0.0186** [0.00939]	0.123 [0.131]	0.0504 [0.0572]	0.0276 [0.0173]	0.0272*** [0.00920]	0.220* [0.131]	0.105* [0.0580]	0.0415** [0.0180]
Child-specific information x inaccurate	-0.0107*** [0.00409]	-0.0634 [0.0587]	-0.0211 [0.0264]	0.00366 [0.00761]	-0.000729 [0.00430]	0.0139 [0.0619]	0.0203 [0.0281]	0.0174** [0.00877]
Inaccurate	0.00691 [0.00786]	0.0643 [0.111]	0.0654 [0.0490]	0.000705 [0.0148]	-0.00171 [0.00794]	-0.120 [0.113]	-0.0281 [0.0522]	-0.0199 [0.0156]
Salience	0.0232*** [0.00795]	0.187* [0.107]	0.109** [0.0457]	0.0380** [0.0150]	0.0265*** [0.00822]	0.172 [0.108]	0.104** [0.0478]	0.0348** [0.0151]
Observations	4,295	4,211	4,128	4,288	3,910	3,835	3,763	3,903
Strata fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.203	0.431	0.584	0.123	0.208	0.433	0.582	0.119

Note: A GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. Parents were asked at baseline to give their best estimate of their child performance in math classes. Data was then crossed with administrative records and parents who estimated exactly right were determined as accurate, those who estimated below were determined optimistic and those who estimated above were determined pessimist. Four categories were available for parents' answers on performance (below average; adequate; good; very good). Administrative data register data on attendance and GPA on a quarterly basis (period of ~ 9 weeks). The GPA has a 10 point scale, where 5 is the passing grade. Parents' answers below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10. * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table 11: Effects by average content entered by teachers into the platform (tardiness)

	Math attendance (1)	Math GPA (2)	Reported punctuality Math standardized test score (3)	Grade promotion (4)
Child-specific information	-0.0264 [0.0310]	-0.380 [0.274]	-0.203 [0.147]	-0.130** [0.0519]
Child-specific information x average reported	0.00725 [0.00802]	0.111 [0.0732]	0.0556 [0.0395]	0.0338** [0.0133]
Salience	-0.0433* [0.0250]	0.0191 [0.239]	-0.0447 [0.129]	-0.0931* [0.0527]
Salience x average reported	0.0117* [0.00650]	0.00199 [0.0639]	0.00914 [0.0344]	0.0252* [0.0136]
Average reported	0.0718*** [0.00659]	1.363*** [0.0777]	0.675*** [0.0482]	0.0962*** [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: Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Parents were asked at baseline to give their best estimate of their child performance in math classes. Data was then crossed with administrative records and parents who estimated exactly right were determined as accurate, those who estimated below were determined optimistic and those who estimated above were determined pessimist. Four categories were available for parents' answers on performance (below average; adequate; good; very good). Administrative data register data on attendance and GPA on a quarterly basis (period of ~ 9 weeks). The GPA has a 10 point scale, where 5 is the passing grade. Parents' answers below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10. Parents were also asked at endline to give their best estimate of how many times their child missed school and what was their final math GPA in the past quarter. Five categories were available for parents' answers on attendance (missed 0, 1-2; 3-5; 6-8; more than 8) and parents answers for GPA were absolute values from 1-10. Data was then crossed with administrative records and a dummy variable were created, where parents who estimated right received value 1 and those who estimated wrong received value 0.

Table 12: Attention: Spillovers within student

	Parent's accuracy		Educational outcomes		
	(1)	(2)	(3)	(4)	(5)
	Accuracy Portuguese Attendance (p.p.)	Accuracy Portuguese GPA (p.p.)	Portuguese Attendance (p.p.)	Portuguese GPA (std.)	Portuguese Standardized Test (std.)
Child-specific information	0.027 [0.029]	0.051* [0.031]	0.007 [0.005]	0.053 [0.036]	0.047 [0.043]
Salience	0.009 [0.029]	-0.005 [0.031]	0.007 [0.005]	0.066* [0.036]	0.032 [0.043]
Control within classroom	0.035 [0.029]	-0.021 [0.031]	0.004 [0.005]	0.054 [0.035]	0.026 [0.043]
Observations	3069	3069	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes

Note: GPA and standardize test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. For each outcome of interest, the following model was estimated: $Y_{i,c,s} = \alpha + \beta_1 \text{Salience}_{i,c,s} + \beta_2 \text{Info}_{i,c,s} + \beta_3 \text{Control}_{i,c=\text{treated},s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \epsilon_{i,c,s}$, where $Y_{i,c,s}$ denotes the outcome of interest for student i in classroom c of stratum s ; pure control schools stand for the reference category (omitted indicator variable); Control assumes value 1 for the control group in treatment schools and 0 otherwise; $X_{k,i,c,s}$ is a matrix of student's covariates; θ_s is a randomization stratum FE and $\epsilon_{i,c,s}$ is an error term, clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Parents were asked at endline to give their best estimate of how many times their child missed school and what was their final Portuguese GPA in the past quarter. Five categories were available for parents' answers on attendance (missed 0, 1-2; 3-5; 6-8; more than 8) and parents answers for GPA were absolute values from 1-10. Data was then crossed with administrative records and a dummy variable were created, where parents who estimated right received value 1 and those who estimated wrong received value 0.

A Pre-analysis plan [Supplementary Appendix]

Our pre-analysis registered at the AEA RCT Registry is presented in full in subsections A.1 through A.4. Subsection A.5 highlights the elements of the analyses that deviate from what had been specified in that pre-analysis plan.

A.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.

A.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 relative 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 relative 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 A.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 A.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 A.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.

A.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.

A.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 (through 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 A.4: Timeline & Milestones

A.5 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 three 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, sub-sample E (nudges) 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.

Third, 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.

B Content of text messages [Supplementary Appendix]

As described in section 3.3, 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.

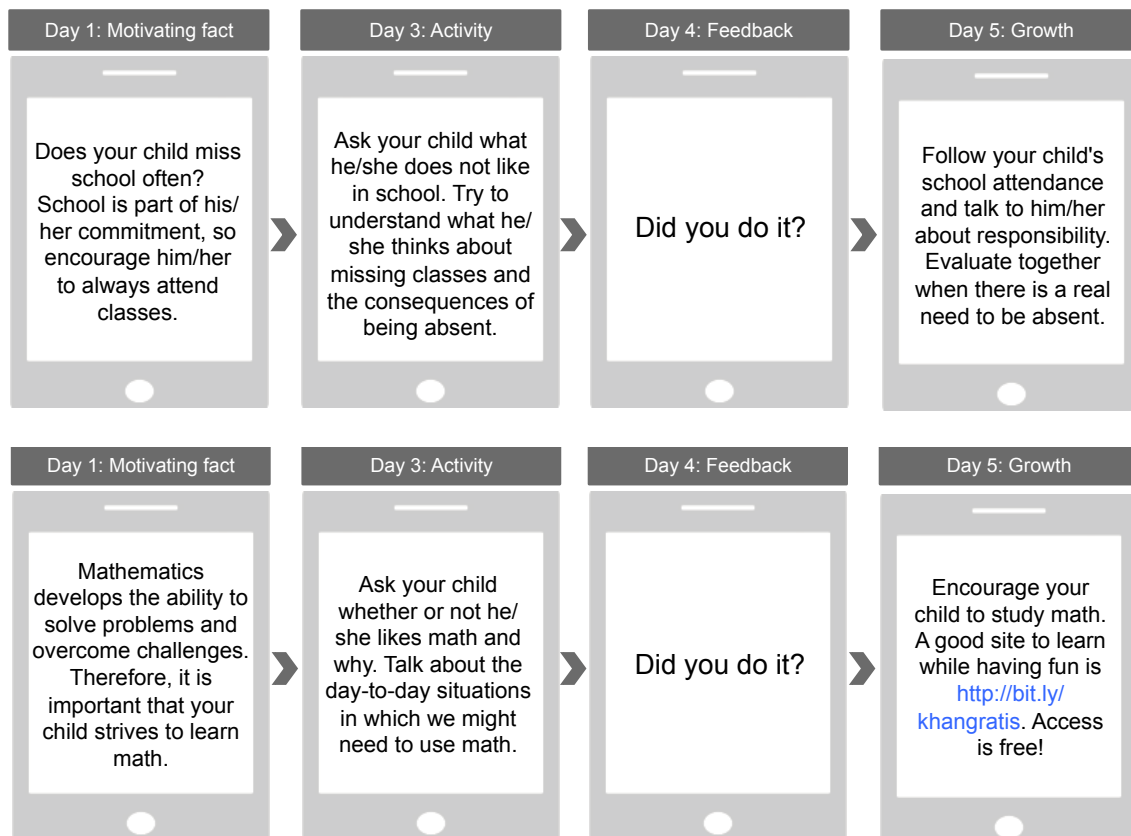
Attendance	Tardiness	Assignment Completion
1. Missed more than 5 classes	1. Was late for more than 5 classes	1. Did not complete any of the assignments
2. Missed 3 to 5 classes	2. Was late 3 to 5 classes	2. Completed less than half of the assignments
3. Missed less than 3 classes	3. Was late for less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

Week	Individual Info.	Relative Info.	Salience
Week 1	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	If missing a class, <i>@name</i> can miss important parts of the content taught, which could impair <i>his/her</i> performance at school.
Week 2	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	When students are late for class, they can impair the progress of the group and disturb their peers' concentration. It is important that <i>@name</i> arrives on time for classes.
Week 3	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	It is important for <i>@name</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	Learning requires constant participation. It is important that <i>@name</i> is always present in class.
Week 5	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	For a good learning experience, it is important that <i>@name</i> is always punctual, so <i>he/she</i> doesn't miss important content taught in class.
Week 6	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	<i>@Name</i> could fall behind if <i>he/she</i> does not turn in the homework, because the teacher may not be able to help <i>him/her</i> with <i>his/her</i> specific difficulties.
Week 7	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	Participate in <i>@name's</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	It is important that <i>@name</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	If <i>@name</i> does not turn in homework assignments, it may hurt <i>his/her</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	If <i>he/she</i> misses classes, <i>@name</i> may miss important parts of the content, impairing <i>his/her</i> school performance.
Week 11	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	Arriving late impairs the progress of the class and the concentration of <i>@name's</i> peers. It's important <i>@name</i> is punctual.
Week 12	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	It is important for <i>@name</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	Learning requires constant participation, so it's important that <i>@name</i> is always present in class.
Week 14	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	For good learning, it is essential that <i>@name</i> is always punctual so <i>he/she</i> does not miss important content taught in class.
Week 15	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	The teacher might not be able to help <i>@name</i> in <i>his/her</i> specific challenges if <i>he/she</i> does not turn in <i>his/her</i> homework.
Week 16	According to the information recorded by the teacher in the system, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	Engage in <i>@name's</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	It is important that <i>@name</i> 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, <i>@name @info</i> in the past 3 weeks.	In the past 3 weeks, <i>@name @info</i> . In <i>his/her</i> class, most of the students <i>@info_class</i> .	If <i>@name</i> does not turn in the school assignments, it may be detrimental to <i>his/her</i> 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 nudge program (described in section 4.8). 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 4.8 ⁴⁷.



⁴⁷The 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.

C Survey instruments [Supplementary Appendix]

C.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!"

C.2 Endline 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 3rd 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 3rd 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 3rd 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 3rd 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!"

C.3 Endline 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	Sometimes	Almost always or always
a. Help you with homework or schoolwork.	1	2	3	4
b. Ask if you did your 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	Somewhat 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 than 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

D Balance and attrition tests [Supplementary Appendix]

In this section, we present balance and attrition tests. Table D.1 shows descriptive statistics and balance test for the main sample used in the analysis (e.g. Tables 3, 8, 7). Table D.2 presents descriptive statistics and balance test for the theory of change sample. Next, Tables D.3 and D.4 contain a selective attrition analysis for completing the surveys by treatment status and by baseline characteristics, respectively. Because parents who opted into the program had different characteristics from those who did not opt in (as we showed in Table 1), in Table D.5 we show results for school transcripts and test scores re-weighting observations by the inverse probability of opting into the program. Finally, Table D.6 describes statistics and balance for the theory-based nudging program for the parents receiving one message per week, which is the sample sample used to run the differences-in-differences analysis described in section 4.

Table D.1: Descriptive statistics and balance - school transcripts and test score sample

	Means				Diff=0 p-value	Sample Size
	Pure Control	Control Within Class	Salience	Info		
Student characteristics						
Female	0.47	0.50	0.51	0.52	0.03	12577
Age	14.69	14.67	14.67	14.71	0.03	12577
Brown	0.36	0.35	0.34	0.35	0.14	12577
Black	0.06	0.05	0.06	0.06	0.79	12577
Portuguese GPA (max 10)	6.39	6.31	6.27	6.28	0.69	12577
Math GPA (max 10)	6.10	6.11	6.05	6.06	0.57	12577
Portuguese attendance	0.92	0.92	0.92	0.92	0.50	12577
Math attendance	0.92	0.92	0.92	0.91	0.39	12577
Adult responsible for student						
Mother	0.77	0.75	0.76	0.76	0.45	12577
Age	40.39	40.28	40.34	40.57	0.68	12577
Brown	0.36	0.34	0.34	0.34	0.15	12577
Black	0.07	0.06	0.07	0.07	0.71	12577
Middle school incomplete	0.32	0.30	0.30	0.31	0.32	12577
Middle school complete	0.28	0.26	0.28	0.28	0.48	12577
High School	0.34	0.33	0.32	0.31	0.19	12577
Earns less than 1 MW (1MW ~ \$250)	0.17	0.17	0.17	0.17	0.80	12577
Earns between 1 - 3 MW	0.44	0.46	0.46	0.47	0.80	12577

Note: Means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. 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 D.2: Descriptive statistics and balance - theory of change sample

	Means				Diff=0 p-value	Sample Size
	Pure Control	Control Within Class	Salience	Info		
Student characteristics						
Female	0.50	0.50	0.52	0.52	0.18	9539
Age	14.65	14.65	14.66	14.68	0.24	9539
Brown	0.36	0.35	0.33	0.34	0.33	9539
Black	0.05	0.05	0.05	0.05	0.68	9539
Portuguese GPA (max 10)	6.51	6.45	6.39	6.39	0.51	9539
Math GPA (max 10)	6.21	6.22	6.20	6.17	0.87	9539
Portuguese attendance	0.93	0.93	0.93	0.93	0.30	9539
Math attendance	0.93	0.92	0.92	0.92	0.45	9539
Adult responsible for student						
Mother	0.78	0.75	0.76	0.76	0.43	9539
Age	40.62	40.39	40.34	40.74	0.64	9539
Brown	0.35	0.34	0.34	0.33	0.27	9539
Black	0.07	0.06	0.07	0.07	0.67	9539
Middle school incomplete	0.31	0.29	0.29	0.28	0.44	9539
Middle school complete	0.28	0.26	0.27	0.28	0.37	9539
High School	0.33	0.34	0.32	0.33	0.42	9539
Earns less than 1 MW (1MW ~ \$250)	0.16	0.16	0.16	0.16	0.86	9539
Earns between 1 - 3 MW	0.44	0.47	0.46	0.47	0.92	9539

Note: Means net of randomization strata fixed effects. P-values calculated using randomization strata fixed effects and standard errors clustered at the classroom level. 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 D.3: Selective attrition - survey completion

	(1) Baseline Survey - Parents	(2) Endline Survey - Parents	(3) Endline Survey - Students
Salience	-0.016 [0.020]	0.022 [0.024]	0.016 [0.016]
Information	-0.008 [0.021]	0.039 [0.024]	0.013 [0.016]
Control Within Class	-0.006 [0.020]	0.045* [0.023]	0.020 [0.016]
P-value Salience=Info=Control Within	0.828	0.412	0.694
Sample Size	4862	4653	15597
Randomization strata FE	Yes	Yes	Yes

Note: pure control is the omitted group. Parental survey was considered completed if at least 11 questions were answered, and student survey was considered completed if at least 75% of the questions were answered. We run a simple regression where a dummy indicating if parents completed the survey served as the outcome variable and treatment status served as independent variables. Randomization stratum fixed effects were also included. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table D.4: Marginal probability of completing the survey

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

Note: Parental survey was considered completed if at least 11 questions were answered, and student survey was considered completed if at least 75% of the questions were answered. We run a simple regression, where each of the characteristics in the horizontal line served as independent variable, and a dummy indicating if parents completed the survey served as dependent variable. A different regression was estimated for each characteristic. Randomization stratum fixed effects were also included. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table D.5: School transcripts and standardized tests - re-weighting by the probability of opting-in the program

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.022*** [0.006]	0.100*** [0.032]	0.038*** [0.013]	0.096** [0.046]
Information	0.022*** [0.007]	0.077** [0.032]	0.031** [0.013]	0.105** [0.046]
Control Within	0.019*** [0.007]	0.081** [0.032]	0.036*** [0.013]	0.087* [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	12550	12550	12550	12550
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Inverse probability weighting was used to weight estimates by the probability of opting-in the program based on observables.

Table D.6: A parallel salience intervention: balance

	Means			Diff=0 p-value	Sample Size
	Pure Control	Control Within Class	Engagement		
Panel A: Student characteristics					
Female	0.47	0.51	0.50	0.23	3058
Age	14.68	14.66	14.69	0.68	3058
Brown	0.36	0.32	0.31	0.05	3058
Black	0.06	0.05	0.05	0.53	3058
Portuguese GPA (max 10)	6.37	5.99	5.99	0.00	3019
Math GPA (max 10)	6.07	5.79	5.75	0.00	3021
Portuguese attendance	0.93	0.92	0.93	0.91	3037
Math attendance	0.92	0.92	0.92	0.88	2975
Panel B: Adult responsible for student					
Mother	0.78	0.76	0.74	0.14	3058
Age	40.38	40.77	40.47	0.51	3008
Black	0.07	0.07	0.07	0.88	3058
Middle school incomplete	0.31	0.28	0.26	0.06	3058
Middle school complete	0.30	0.25	0.25	0.03	3058
High School	0.33	0.32	0.33	0.85	3058
Earns less than 1 MW (1MW ~ \$250)	0.16	0.15	0.13	0.10	3058
Earns between 1 - 3 MW	0.43	0.46	0.47	0.11	3058

Note: P-values computed from robust standard. Engagement treatment includes only parents who received one text message per week. 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.

E Heterogeneous treatment effects by gender [Supplementary Appendix]

This section presents tables for the theory of change analysis, as well as the heterogeneous effects for boys and girls, both described in section 4. We also explain in more details the variables used in the analysis. The theory of change analysis uses data from students' endline survey, where students answered questions about their parent's behavior and aspirations, as well as their own behavior. A common sample of 9539 students was used to investigate results on parent's behavior and aspirations, student's behavior, and school transcripts and test score.

At the endline survey, students were asked to state how often their parents engage in certain activities (never, almost never, sometimes, almost 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). Students were also asked if their parents believed they would go to college and a dummy variable for *parent's aspirations* was created, which assumes value one if parents do believe the student will go to college and zero otherwise.

Finally, students were requested to answer how many hours per day (0, 15 minutes, 30 minutes, 1 hours, 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. We used factor analysis 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).

All the 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. Results were estimated according to equation 1.

Table E.1 shows results for school transcripts and test score; Table 5 presents results for parent's behavior and aspirations; and Table 6 describes results for student's behavior. Next, Tables E.2, E.3, E.4, and E.5 show heterogeneous results for boys and girls, following the same order: school transcripts and test score, parent's behavior and aspirations, and student's behavior.

Table E.1: School transcripts and test scores

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.016*** [0.006]	0.072** [0.034]	0.030** [0.012]	0.075 [0.053]
Information	0.017*** [0.006]	0.058* [0.034]	0.026** [0.012]	0.091* [0.053]
Control Within	0.016*** [0.006]	0.054* [0.034]	0.030** [0.012]	0.068* [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: GPA and standardized test 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table E.2: School transcripts and test score - boys and 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*** [0.01]	0.13*** [0.04]	0.04** [0.02]	0.10* [0.06]	0.01* [0.01]	0.02 [0.04]	0.01 [0.01]	0.04 [0.06]	-0.01 [0.01]	-0.12** [0.05]	-0.03* [0.02]	-0.06 [0.06]
Information	0.02*** [0.01]	0.12*** [0.04]	0.04** [0.02]	0.13** [0.06]	0.01** [0.01]	0.00 [0.04]	0.01 [0.01]	0.05 [0.06]	-0.01 [0.01]	-0.12** [0.05]	-0.03* [0.02]	-0.07 [0.07]
Control Within	0.02*** [0.01]	0.12*** [0.04]	0.04** [0.02]	0.10 [0.06]	0.01* [0.01]	-0.01 [0.04]	0.02* [0.01]	0.04 [0.06]	-0.01 [0.01]	-0.13*** [0.05]	-0.03* [0.02]	-0.06 [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: GPA and standardized test 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$.

Table E.3: Parents' behavior - boys and 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** [0.06]	0.07 [0.06]	0.14*** [0.05]	0.00 [0.06]	0.11* [0.06]	0.11* [0.06]	-0.12* [0.07]	0.04 [0.08]	-0.03 [0.07]
Information	0.13** [0.06]	0.05 [0.06]	0.17*** [0.05]	0.05 [0.07]	0.09 [0.06]	0.12** [0.06]	-0.08 [0.08]	0.03 [0.08]	-0.04 [0.07]
Control Within	0.16*** [0.06]	0.06 [0.06]	0.15*** [0.05]	-0.01 [0.07]	0.00 [0.06]	0.07 [0.06]	-0.17** [0.07]	-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: 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. At the endline survey, students were asked to state how often their parents engage in certain activities (never, almost never, sometimes, almost 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).

Table E.4: Parents' aspirations - boys and girls

	Boys	Girls	Diff. (Girls)-(Boys)
	(1) Parents' Aspirations College	(2) Parents' Aspirations College	Parents' Aspirations College
Salience	0.12** [0.06]	0.08 [0.05]	-0.04 [0.08]
Information	0.10* [0.06]	0.09* [0.05]	-0.02 [0.08]
Control Within	0.10* [0.06]	0.03 [0.05]	-0.07 [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: The dependent variable was 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. At the endline survey, students were asked if their parents believed they would go to college and a dummy variable for *parent's aspirations* was created, which assumes value one if parents do believe the student will go to college and zero otherwise.

Table E.5: Students' behavior - boys and 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*** [0.06]	0.17** [0.07]	-0.09 [0.06]	0.06 [0.07]	0.06 [0.07]	-0.13** [0.07]	-0.13* [0.07]	-0.11 [0.08]	-0.04 [0.08]
Information	0.18*** [0.05]	0.15** [0.07]	-0.13* [0.07]	0.12* [0.07]	0.08 [0.08]	-0.09 [0.07]	-0.06 [0.07]	-0.07 [0.08]	0.04 [0.08]
Control Within	0.19*** [0.05]	0.17** [0.07]	-0.07 [0.06]	0.08 [0.07]	0.09 [0.08]	-0.10 [0.07]	-0.11 [0.07]	-0.07 [0.08]	-0.04 [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: Dependent 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. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. At the endline survey, students were requested to answer how many hours per day (0, 15 minutes, 30 minutes, 1 hours, 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).

F Heterogeneous treatment effects by willingness to receive information [Supplementary Appendix]

Willingness to receive information was also measured at the baseline survey. Parents were asked at baseline about their interest in receiving information about their child's school attendance, given the following options: no interest, some interest, or great interest (see Supplementary Appendix C). 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.

Table F.1 presents heterogeneous treatment effects by splitting the sample according to this indicator variable. The lower sample size reflects the fact that we can only use parents who answer our baseline phone survey in this table.

Table F.1: Heterogeneity 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*** [0.01]	0.12** [0.05]	0.03* [0.02]	0.08 [0.07]	0.02 [0.04]	0.10** [0.04]
Information	0.03*** [0.01]	0.09* [0.05]	0.04** [0.02]	0.16** [0.07]	-0.03 [0.04]	0.02 [0.04]
Control Within	0.03*** [0.01]	0.08 [0.05]	0.03* [0.02]	0.03 [0.07]	0.06 [0.04]	0.03 [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*** [0.01]	0.18*** [0.07]	0.07*** [0.02]	0.14 [0.10]	-0.15** [0.07]	0.02 [0.08]
Information	0.04*** [0.01]	0.15** [0.07]	0.07*** [0.02]	0.07 [0.10]	-0.16** [0.07]	0.04 [0.08]
Control Within	0.03** [0.01]	0.15** [0.07]	0.05** [0.02]	0.08 [0.10]	-0.12* [0.07]	-0.01 [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: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Parents were asked at baseline about their interest in receiving information about their child's attendance and they had three options: i. no interest, ii. some interest, iii. a lot of interest. Parents who answered i. or ii. were defined as having a low WTR and parents who answered iii. were defined as having a high WTR. Parents were asked at endline to give their best estimate of how many times their child missed school and what was their child final math GPA in the past quarter. Data was then crossed with administrative records and a dummy variable was created, where parents who estimated right received value 1 and those who estimated wrong received value 0.

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.⁴⁸ 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 information effects, highlighting the complementary nature between attention and decentralized information acquisition by parents.

⁴⁸The 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.

G Treatment effects on the online platform’s outcomes [Supplementary Appendix]

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 weeks⁴⁹. 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:

$$Y_{i,c,s} = \alpha + \beta_1 \text{Salience}_{i,c,s} + \beta_2 \text{Info}_{i,c,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$$

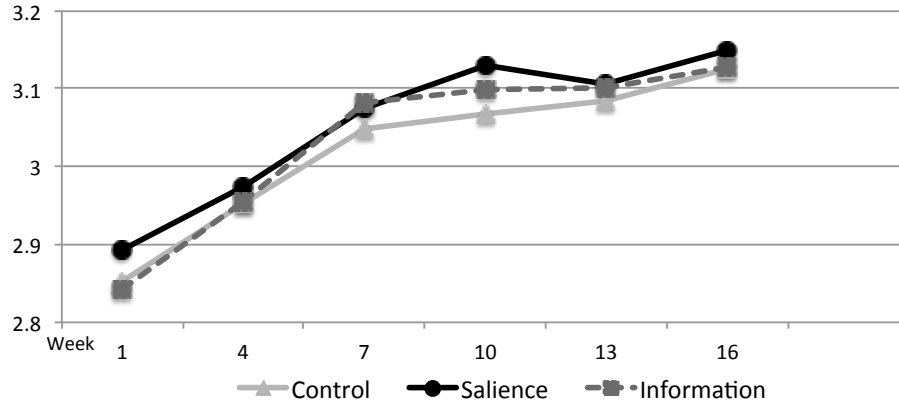
where $Y_{i,c,s}$ denotes the weekly score of each dimension for student i in classroom c of stratum s , the within-class control stand for the reference category (omitted indicator variable), $X_{k,i,c,s}$ is a matrix of student’s characteristics, θ_s are randomization stratum FE, and $\varepsilon_{i,c,s}$ is an error term, clustered at the classroom level. Results are presented in Table G.1, where Panel A show data for attendance, Panel B for punctuality and Panel C for assignment completion. Note that teachers from the pure control schools did not fill the platform and the control group in the graph represents control students in the treated classrooms.

We start by plotting coefficients week-by-week in Figure G.1. As behaviors rotate weekly, we can plot coefficients in 3-week intervals.

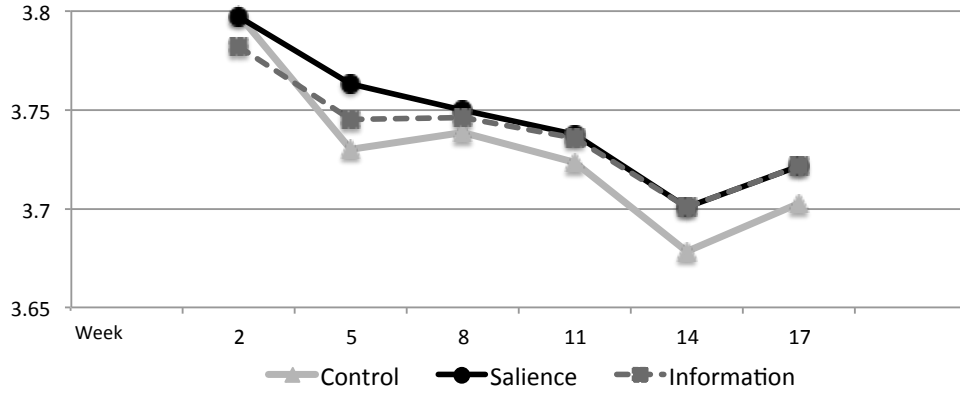
⁴⁹Students are scheduled to have 6 Math classes per week.

Figure G.1: Weekly effect on platform scores

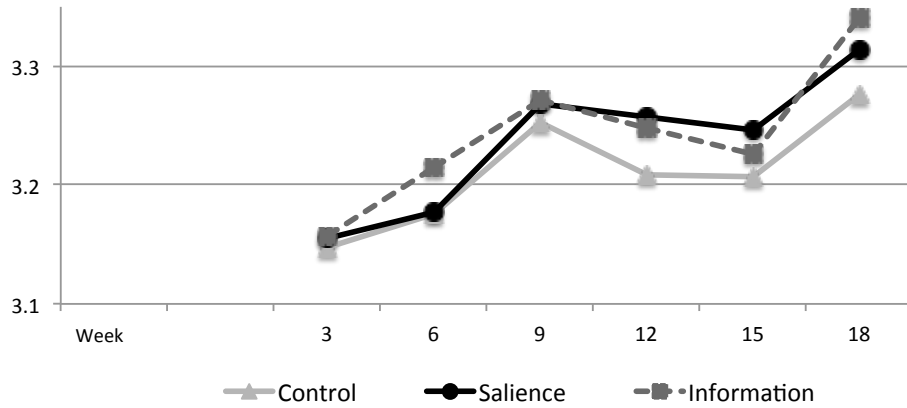
Panel A: Weekly effect on attendance



Panel B: Weekly effect on punctuality



Panel C: Weekly effect on homework completion



Note: For each outcome and each week, the following equation was estimated: $Y_{i,c,s} = \alpha + \beta_1 \text{Salience}_{i,c,s} + \beta_2 \text{Info}_{i,c,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \epsilon_{i,c,s}$, where $Y_{i,c,s}$ denotes the weekly score 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 covariates, θ_s are randomization stratum FE, and $\epsilon_{i,c,s}$ is an error term, clustered at the classroom level. Each week, teachers evaluated students using a 4 point scale, where 1 was the minimum and 4 was the maximum. For this analysis, we reversed coded scores for tardiness, to investigate the effect on punctuality.

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 pool all weeks by averaging scores of each dimension over the course of the 18 weeks, and display the regression results in Table G.1. Scores were normalized relative to the distribution of the comparison group, to yield effect sizes in standard deviations. Coefficients are interpreted as average treatment effects over this period.

Table G.1: Results on platform scores - average of all weeks

	(1) Attendance (std.)	(2) Punctuality (std.)	(3) Homework Completion (std.)
Salience	0.046** [0.022]	0.028 [0.020]	0.027 [0.019]
Information	0.025 [0.026]	0.022 [0.022]	0.044** [0.022]
Control Mean	3.043	3.729	3.237
P-value diff. [Info] -[Salience]	0.427	0.822	0.436
Sample Size	11529	11529	11529
Randomization strata FE	Yes	Yes	Yes
Student controls	Yes	Yes	Yes

Note: The platform scores of each dimension—attendance, tardiness and assignment completion—were averaged for each student and then normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. For each score, the following equation was estimated: $Y_{i,c,s} = \alpha + \beta_1 \text{Salience}_{i,c,s} + \beta_2 \text{Info}_{i,c,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$, where $Y_{i,c,s}$ denotes the averaged score 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 covariates, θ_s are randomization stratum FE, $\varepsilon_{i,c,s}$ is an error term, clustered at the classroom level, and $\% \text{Salience} = \beta_1 / \beta_2$.

Table G.1 confirms the visual patterns across the three panels in Figure G.1: there is a positive treatment effect of both salience and information messages on students' behavior relative to those assigned to the control group. These effects are not, however, always estimated with statistical precision. Salience increases attendance scores by 0.046 standard deviation (significant at the 5% level) and information increases homework completion by 0.044 standard deviation (significant at the 5% level); in all other cells, effect sizes are between 0.022 and 0.028 standard deviations, and not statistically significant. Importantly, in none of the cases are the effects of information statistically different from those of salience messages, at a 10% significance level.

Last, we analyze heterogeneity in treatment effects with respect to attendance and

homework completion scores entered into the platform by teachers (Table G.1, in the main text, presents the results for tardiness).

Table G.2: Effects by average content entered by teachers into the platform (attendance)

	Math attendance (1)	Math GPA (2)	Reported attendance Math standardized test score (3)	Grade promotion (4)
Child-specific information	0.00465 [0.0163]	-0.00219 [0.165]	0.0135 [0.0844]	-0.0335 [0.0296]
Child-specific information x average reported	-0.00125 [0.00492]	0.0142 [0.0529]	-0.000167 [0.0274]	0.0102 [0.00884]
Salience	-0.0193 [0.0172]	0.179 [0.160]	0.0468 [0.0850]	-0.0103 [0.0294]
Salience x average reported	0.00603 [0.00511]	-0.0544 [0.0508]	-0.0199 [0.0269]	0.00347 [0.00858]
Average reported	0.0966*** [0.00420]	1.129*** [0.0471]	0.563*** [0.0244]	0.0721*** [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: Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Parents were asked at baseline to give their best estimate of their child performance in math classes. Data was then crossed with administrative records and parents who estimated exactly right were determined as accurate, those who estimated below were determined optimistic and those who estimated above were determined pessimist. Four categories were available for parents' answers on performance (below average; adequate; good; very good). Administrative data register data on attendance and GPA on a quarterly basis (period of ~ 9 weeks). The GPA has a 10 point scale, where 5 is the passing grade. Parents' answers below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10. Parents were also asked at endline to give their best estimate of how many times their child missed school and what was their final math GPA in the past quarter. Five categories were available for parents' answers on attendance (missed 0, 1-2; 3-5; 6-8; more than 8) and parents answers for GPA were absolute values from 1-10. Data was then crossed with administrative records and a dummy variable were created, where parents who estimated right received value 1 and those who estimated wrong received value 0.

Table G.3: Effects by average content entered by teachers into the platform (homework completion)

	Math attendance (1)	Math GPA (2)	Reported homework completion Math standardized test score (3)	Grade promotion (4)
Child-specific information	-0.0231 [0.0175]	-0.115 [0.147]	-0.0740 [0.0802]	-0.0462 [0.0338]
Child-specific information x average reported	0.00650 [0.00491]	0.0315 [0.0450]	0.0178 [0.0244]	0.0123 [0.00936]
Salience	-0.0341* [0.0177]	-0.00518 [0.142]	-0.0340 [0.0770]	-0.0206 [0.0352]
Salience x average reported	0.0103** [0.00493]	0.00474 [0.0432]	0.00686 [0.0236]	0.00591 [0.00986]
Average reported	0.0528*** [0.00393]	1.687*** [0.0425]	0.847*** [0.0230]	0.0967*** [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: Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. Parents were asked at baseline to give their best estimate of their child performance in math classes. Data was then crossed with administrative records and parents who estimated exactly right were determined as accurate, those who estimated below were determined optimistic and those who estimated above were determined pessimist. Four categories were available for parents' answers on performance (below average; adequate; good; very good). Administrative data register data on attendance and GPA on a quarterly basis (period of ~ 9 weeks). The GPA has a 10 point scale, where 5 is the passing grade. Parents' answers below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10. Parents were also asked at endline to give their best estimate of how many times their child missed school and what was their final math GPA in the past quarter. Five categories were available for parents' answers on attendance (missed 0, 1-2; 3-5; 6-8; more than 8) and parents answers for GPA were absolute values from 1-10. Data was then crossed with administrative records and a dummy variable were created, where parents who estimated right received value 1 and those who estimated wrong received value 0.

As one would expect, attending classes more often and handing in homework more frequently correlates with better educational outcomes across the board. Also in line with the literature, the effects of information are negative when the message bears bad news, and positive otherwise: the coefficient of the interaction of information with average content reported is always positive when it comes to homework completion, and either 0 or positive when it comes to attendance (although none of those coefficients are statistically significant). As in the case of tardiness, we find that the effects of salience messages follow exactly the same pattern as those of information, once again suggesting that parents collect information themselves when primed about the importance of attendance, punctuality and homework completion, even in the absence of direct inputs for belief updating.

H Selective non-response and bounding treatment effects [Supplementary Appendix]

As shown in Figure H.1, 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 our 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 7). Results are showed in tables H.1 and H.2.

Figure H.1: Average number of times teachers filled the platform by sub-sample during the 18 week period

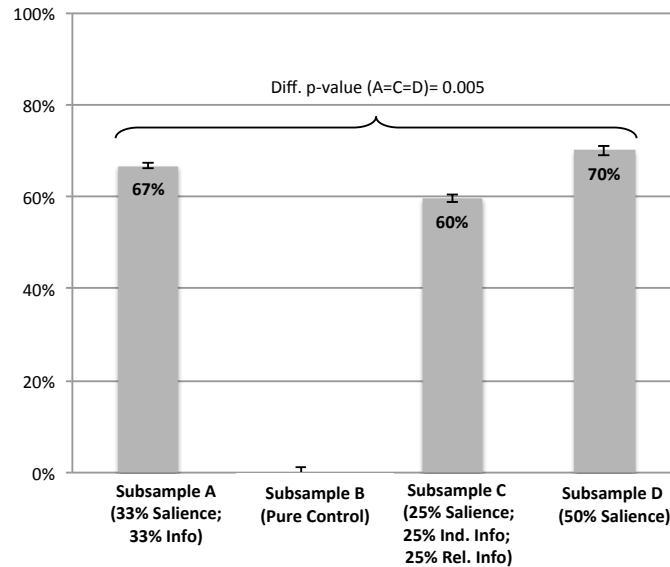


Table H.1: Robustness school transcript and test score - equalizing SMS received by sub-sample

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.019*** [0.006]	0.085*** [0.032]	0.030*** [0.011]	0.108** [0.045]
Information	0.019*** [0.006]	0.070** [0.032]	0.026** [0.011]	0.110** [0.046]
Control Within	0.016*** [0.006]	0.072** [0.031]	0.028** [0.011]	0.102** [0.045]
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: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. 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 were low (teachers filled 3 times or less the platform) where also excluded.

Table H.2: Interactions with 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.)
Salience	0.016** [0.006]	0.068** [0.033]	0.027** [0.011]	0.110** [0.047]
Information	0.019*** [0.006]	0.070** [0.032]	0.026** [0.011]	0.110** [0.046]
Salience Only	0.002 [0.004]	0.030 [0.029]	0.002 [0.009]	-0.004 [0.044]
Control Within	0.013** [0.006]	0.062* [0.032]	0.026** [0.011]	0.103** [0.046]
Sample Size	11951	11951	11951	11951
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: GPA and standardized test were normalized relative to the distribution of the comparison group, such that the mean and standard deviation of the comparison group is zero and one, respectively. Standard error clustered at the classroom level. Significance levels are denoted by * if $p < 0.1$, ** if $p < 0.05$ and *** if $p < 0.01$. 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 were low (teachers filled 3 times or less the platform) where also excluded.