Are the Effects of Informational Interventions Driven by Salience?*

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Abstract

Informational interventions have been shown to significantly change behavior across a variety of settings. Is that because those lead subjects to *frictionlessly* update beliefs? Or, alternatively, is it to a large extent because those increase the *salience* of the decision they target? We study this question in the context of communication with school parents. In a large-scale field experiment with ninth-graders 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 highlight the importance of attending to their child's behavior, but *no child-specific information.* While, compared to a pure control group, communication has large impacts on attendance, test scores and grade promotion rates, *most of its effects are driven by salience*: outcomes in this group improve by 89-126% of those in the information group. Our results suggest that alternative interventions that manipulate attention can presumably generate larger impacts and qualify the interpretation of previous findings, with direct implications for the design and welfare analysis of informational interventions across a range of domains.

Keywords: Information; Salience; Inattention; Welfare

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1 Introduction

Informational interventions across a wide array of domains have been show to successfully affect fundamental economic decisions. Information about employees' productivity makes turnover of low-productivity staff more likely and increases overall productivity (Rockoff et al., 2012), information about labor market returns to education increases educational attainment (Jensen, 2010), information 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 energy-efficiency increases consumers' demand for LED lightbulbs (Allcott and Taubinsky, 2015), and information about husbands' support for female labor market participation increases investments in labor market skills (Bursztyn et al., 2018), among many other applications.

Presumably, such informational interventions work because agents rationally update beliefs when provided with information, and, endowed with more accurate beliefs, they make decisions more closely aligned to optimality, generally leading to higher welfare.[1] Having said that, when agents receive information, frictionless belief updating is not the only thing that may occur: the decision-making domain targeted by the informational intervention becomes *top-of-mind* (Golman and Loewenstein, 2018). In face of limited attention (Gabaix, 2019), if that makes the benefits of some actions more salient, agents might undertake those actions to a greater extent, *regardless* of their fundamental beliefs about returns. This paper provides first-hand evidence for this mechanism in the real world.[2]

Distinguishing the two mechanisms matters: if salience effects are large, alternative interventions that manipulate attention could presumably change behavior to a greater extent. What is more, in that case, there is no guarantee that behavior change triggered by the informational intervention actually leads to welfare gains – a point conceptually made in Loewenstein et al. (2014), intimately connected to the ambiguity of the welfare effects of nudges pointed out by Benkert and Netzer (2018). To see why, consider the following example: when Nina's parents get a message informing them that she missed school yesterday, they learn at no cost that her behavior was (potentially) not in line with their expectations. However, the message might also trigger other mechanisms; e.g. make them feel guilty or disappointed from being perceived by the school as disengaged from their daughter's school life, leading them to *over-monitor* relative to the counterfactual situation in which they had acquired the information themselves, or make them focus too narrowly on absences, leading them to *under-monitor* other inputs to children's human

[1] Exceptions include models of information avoidance (Golman et al., 2017) and strategic interactions when agents' inability to devise contingent plans due to imperfect information generates higher welfare. Our claim that informational interventions might not necessarily increase welfare applies even outside of those models.

[2] Gabaix 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.

capital production function relative to that counterfactual situation.[3,4]

Are such salience effects *quantitatively important* when it comes to informational interventions? Studying this question in a realistic setting is hard. Outside the lab, how could one approximate the ideal experiment that contrasts subjects' behavior when they acquire information through their own means as opposed to when they receive information from another source, holding everything else constant? To do that, we resort to a mechanism experiment (Ludwig et al., 2011): 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 a moral hazard problem between parents and children: as children grow older, their goals may drift increasingly apart from those of their future-oriented parents; 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 weekly fill in a platform with information about their students' behavior (attendance, tardiness and homework completion) over the course of 18 weeks. Taking advantage of a partnership with an EdTech startup[6], 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 less than 3 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

[3]For a formal treatment of guilt (for instance, from not "living up to expectations"), see Battigalli and Dufwenberg (2007). More generally, for sub-optimal information acquisition out of social image concerns, see **?**. On cognitive spillovers from being targeted by interventions that encourage active choice, see Altmann et al. (2019).

[4]Nina's parents may also realize that attendance is an important dimension of their daughter's behavior to which they should attend moving forward, as in "learning through noticing" (Hanna et al., 2014). If such mechanism is at play, salience might actually magnify the welfare effects of information alone since, given limited attention, individuals may fail to learn from dimensions they do not notice. Ultimately, whether that is the case depends on the underlying decision mechanism, as in Benkert and Netzer (2018).

[5]To 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.

[6]Movva (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.

class every day"), while others receive no message at all (the control group). While the salience message potentially conveys additional information – e.g. about social expectations –, presumably information *does the same*.[7] Last, because we anticipate that parents' or peer interactions may generate large spillovers, we randomly vary the intensity of each group at the school level – 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, 2016), we find that weekly communication has large impacts on attendance (2.1 percentage points, or about 5 additional classes a year), math GPA and standardized test scores (0.09 standard deviation) and grade promotion rates (3.2 percentage points). 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 with no child-specific information improve outcomes by 89-126% of those in the information group. In line with the behavioral mechanism, parents update beliefs in response to the intervention in ways *inconsistent* with purely rational belief updating. In the information group, accuracy 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 two months*.[8] Moreover, while information about attendance, tardiness and homework completion increases accuracy about children's math GPA, salience messages increase accuracy to an even greater extent – consistent with learning through noticing (Hanna et al., 2014). Interestingly, the symmetry between information and salience holds even when it comes to heterogeneous responses. For instance, within children that are never late, information increases significantly the likelihood of grade promotion compared to students in the same class who in the control group, while for those who are systematically late, it actually significantly. decreases it. That pattern would be perfectly consistent with

[7] What is more, alternatives are imperfect substitutes: e.g. offering parents lower-cost opportunities to request information via SMS would not really capture the effects of being passively targeted by information; moreover, denying information to those who request it would entail deception. In turn, contrasting the salience and the information treatment arms approximately separates the effects of *being targeted* by information from those of *holding accurate information*.

[8] Unlike most papers studying the effects of informational interventions, we document that information does not make subjects' beliefs more accurate in at least one dimension (attendance). We claim such difference is likely driven by a caveat in the typical belief elicitation procedure, which cannot distinguish belief updating from anchoring – a real concern once salience is brought under the spotlight. Our finding highlights the importance of eliciting beliefs *in a different a unit* from that at which information is conveyed. Concretely, in our experiment, parents in the information group received messages with their child's absences over the course of the 3 previous weeks, but reported beliefs (at both baseline and end line about) about their child's absences *over the course of the previous quarter*. This is in contrast to what is typically done in the literature; e.g. Dizon-Ross (2019) states "Mean 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).

frictionless rational updating: parents update their beliefs about their child's ability or effort, in each case, and then increase or decrease their investments in response, just like in Dizon-Ross (2019). Having said that, salience messages induce the *exact same pattern*, consistent with "learning through noticing" (**?**). Last, differences in accuracy across groups do *not* map into differences in children's school outcomes: communication leads to positive effects across the entire spectrum of baseline beliefs. In particular, even parents who were accurate already – precisely those who had *acquired information themselves* – change behavior when targeted by information, and information has *no additional effect* on those who were inaccurate at baseline.

Is 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).

Spillover effects from communication are substantial: within-classroom control students experience almost as large average effects on attendance and GPA, and statistically identical effects on standardized test scores and promotion rates.[9] For this reason – 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, induced by requiring them to weekly fill-in a platform with information about their students.[10] To investigate whether such requirement may drive our results, we deliver an SMS nudge program to a different sub-sample, reaching parents directly without involving teachers at all.[11] Nudges share weekly suggestions of activities for parents to do with their children. Using differences-in-differences, we find effects of the exact same magnitude to those of communication on standardized test scores (0.09 standard deviation). Using the first quarter as the reference period, different trends across sub-samples only become significant after the program was introduced, dismissing concerns with differential pre-trends due to different baseline char-

[9]We take advantage of our saturated design to rule out that spillovers from information drive the effects of salience messages: we find that the effects of salience messages are *increasing* in the share of the classroom assigned to that group instead of the information group.

[10]There are no other differences across the treatment and pure control groups: (i) sub-samples are balanced across a rage of observable characteristics, (ii) students in pure control schools were enrolled through the same process as those in treatment schools, and (iii) 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.

[11]Students 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.

acteristics. 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.

An important concern is whether salience effects are only relevant in the short-run. If parents infer poor performance from salience messages, and if such inference is systematically biased, then parents may realize this over time and stop reacting to communication. To test whether that is the case, we look again at heterogeneous treatment effects with respect to parents' baseline beliefs. We *do not* find that parents in the salience group become systematically more biased about their children's behavior.[12] What is more, we find that, at least within the 6-month horizon of our study, not only it is *not* the case that the effects of salience messages die out, they even *increase over time*, as the gap between the salience group and the pure control group increases between the third and the fourth quarters both with respect to attendance and GPA.

These results suggest that the effects of informational interventions could be obtained at lower cost – and presumably magnified – by interventions that manipulate attention, raising the salience of the decision domain they target.[13] Most importantly, they 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). Such interventions are likely to make the decisions they target top-of-mind, potentially inducing *over-reaction* – even when they also change beliefs in the correct direction (potentially thanks to salience effects; see Hanna et al., 2014). Ultimately, whether they improve welfare is an open question. In line with recent qualifications of the welfare effects of nudges in Benkert and Netzer (2018), whether informational interventions are welfare-improving or not (in the absence of belief-dependent preferences, see Caplin and Leahy, 2004) depends ultimately on the underlying model for how the intervention affects decision-making. While previous literature posits that the welfare 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*.

Just like parents who receive information about their child's effort from the school react to the salience of monitoring benefits, employers are likely to react to the salience of firing low-performing employees in face of information about their performance, husbands are likely to react to the salience of conforming to expectations in face of information about others' support for female labor force participation, and consumers are likely to react to the salience of energy efficiency in face of information about the benefits of fluorescent

[12]Moreover, if parents systematically inferred poor performance from salience messages and increased monitoring in response, the ratio between the effects of salience and information should be higher for high-performing students than for low-performing ones (whose parents should infer the same from either information or salience messages). However, we do not find evidence that such ratio varies across students' profiles.

[13]In a companion paper (Bettinger et al., 2019), we find that the effects of nudges increase with the frequency of communication. The maximal effect size from randomly assigning frequency, time of delivery (on or off work hours, always at the same time or alternating) and interactivity yields 4-fold the effects of information on math standardized test scores.

lightbulbs.[14] Our findings from a setting where imperfect information is pervasively targeted by policy-makers as a "low-hanging fruit" for cost-effective impacts highlight that caution should be exercised when considering the design and the welfare implications of such interventions, across a multiplicity of decision domains. Given the sheer magnitude of salience effects that we document, without an accurate understanding of the underlying decision process in each case – in particular, the extent to which decisions are tied to beliefs, and affected by additional biases and constraints – there is no guarantee that such interventions are the best available instrument, or even that they are socially desirable.

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.[15] 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.[16]

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 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 servce 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

[14] Allcott and Taubinsky (2015) acknowledges this: "It 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).

[15] 2015 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.

[16] The 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 representive assessment of about 29 million students across 72 participating countries.

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.[17] 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 by summarizing the conceptual framework for why being targeted by an informational intervention might induce different responses relative to the counterfactual of acquiring information oneself, in subsection 3.1. Next, informed by that framework, subsection 3.2 describes the experimental design to isolate the salience effects 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. Last, subsection 3.5 describes the equations we estimate, our treatment of standard errors, and pre-registration.

[17] Less than 30% of Brazilian households own landlines, while 93.4% of them own mobile phones, according to the 2015 National Household Survey (PNAD). While mobile phone penetration is high in Brazil, 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).

3.1 Conceptual Framework

Why might being targeted by an informational intervention induce differential responses relative to the counterfactual of acquiring information oneself? Several papers (Caplin and Leahy, 2004; Golman and Loewenstein, 2018; Koszegi, 2006) model utility as a function not only of material consequences, but also of beliefs about those. In Golman and Loewenstein (2018), utility also depends on the attention paid to such beliefs. As such, seeking or avoiding information does not result merely of the comparison between expected costs of making decisions under inaccurate beliefs and those of acquiring information, but also accounts for the expected utility derived directly from beliefs (and the expected attention reallocation triggered by them, in the latter).

An immediate implication of that model is that an uninformed parent might be optimally so – what already casts doubt on the welfare properties of informational interventions. A less straightforward but logical implication from Golman and Loewenstein (2018) is that parents who actively decide to acquire information might experience a very different distribution of cognitive states than those passively targeted by the same piece of information. The reason is that communication is likely to trigger (using the language of their model) different subjective probabilities for the true answers of each activated question, and different attention weights to each of those questions.

Concretely, a set of parents decides to actively seek information about their child's effort in school not merely motivated by the expected material benefits of optimizing monitoring based on more accurate beliefs being higher than information acquisition costs, but also motivated by the expected utility of updating (or even confirming) their prior beliefs about their children's school performance, and/or by the anticipated attention reallocation triggered by such beliefs (e.g. away from child's bad behavior at home). It is unlikely that being passively targeted with the same piece of information could replicate that exact same distribution of cognitive states. Conversely, it is likely that receiving information without having actively searched for it might trigger disutility from unintentionally having beliefs updated (or perhaps confirmed), and disproportionately reallocate attention to the dimension reported on – as in Altmann et al. (2019), which documents cognitive spillovers from being encouraged to choose actively (rather than passively) in a lab experiment.

In face of a different distribution of cognitive states, it is unlikely that the two sets of parents (those who acquire information themselves and those informed thanks to the intervention) end up deciding the same way, due to differences in attention weights or even more generally, as a reflection of belief-dependent utility – as in Battigalli and Dufwenberg (2007), in which guilt from not "living up to expectations" of the other party affects equilibrium behavior in strategic interactions (such as those involving moral hazard between parents and children), or in Koszegi (2006), in which self-image concerns induce sub-optimal task choice and information acquisition. What is more, even if decisions do not change across the two sets of parents, the welfare ranking of those informed and uninformed within each set might still be very different.

Outside the context of the model, other constraints (such as attentional biases) might imply that information is sub-optimally acquired. In that case, it might be that the different distribution of cognitive states triggered by informational interventions actually improves on decision-making. Having said that, just by chance will salience effects bundled in informational interventions be the optimal instrument to address those constraints – alternative interventions specifically designed to address the latter are likely to be superior.

3.2 Experimental Design

How could one approximate the ideal experiment that contrasts subjects' behavior when they acquire information through their own means as opposed to when they receive information from another source, holding everything else constant? The challenge in comparing those informed thanks to the experiment to those that were already accurate is that, as the previous subsection emphasizes, information seeking is not randomly assigned outside the lab. As such, the proposed comparison would confound the effects of any differences in underlying characteristics across the two sets of parents.

Instead, what we do is 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). This alternative comparison approximates the ideal experiment by isolating the mechanism of interest, along the lines of (Ludwig et al., 2011).

Concretely, in the experiment – across 287 schools in São Paulo, Brazil, encompassing 19,300 ninth graders –, math teachers have to weekly fill-in a platform with information about their students' behavior (attendance, tardiness and homework completion). Within each classroom, we randomly assign parents to different messages, shared by the platform over text messages (SMS): some parents receive child-specific information (e.g.: "Nina missed less than 3 classes over the last 3 weeks"), some receive a salience message, emphasizing the importance of attending to that dimension (e.g.: "It is important that Nina attends class every day"), while others receive no message at all (the control group). Comparing *information* and *salience* students to *control* students allows separating the effects of lower monitoring costs from those of higher salience of monitoring.

Framing salience messages in this way might raise concerns, in that claiming a dimension is important might change preferences or beliefs above and beyond raising that dimension to the 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 imperfectly approximate those salience effects. For instance, the message "you can learn about your children's attendance at the school" is presumably not surprising at all and would be unlikely to draw attention comparably to the informational intervention. Alternatively, a message offering parents the opportunity to get information if they reply indeed raises the salience but will induce at least some to request information, making it unfeasible to decompose its salience effects (since deception is not allowed).[18]

One concern is that parents may already have (to a reasonable extent) information about their child behavior; the key piece of information missing may be how to place their child relatively to his or her classmates (Rogers and Feller, 2016).[19] To tackle that issue, for a different set of schools we frame information on child behavior relative to the median of their classroom.[20] [21]

0. **Control:** No messages.

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

2. **Salience:** Messages with statements raising awareness about school attendance, punctuality and assignment completion.

3. **Relative information:** Messages with child-specifc information about attendance, tardiness and homework completion framed *relatively* to classroom's median behavior

Communication is delivered through weekly text messages (SMS) over the course of the second half of the school year. Since administrative outcomes – school attendance, test scores and grade promotion – are only available at low-frequency, we hold the assignment fixed over the course of the experiment. Content alternates across three dimensions of children's effort that the online platform requires teachers to fill-in— attendance, tardiness and assignment completion. We included those dimensions because teachers already measure them weekly (even if on paper), because the Secretariat thought it was important to inform parents about all of them (rather than just about attendance), and because we thought it would be less likely that teachers' usage of the platform would die out over time if they had to alternate across behaviors rather than just replicate the same records they already do on paper every week over the course of four months. We restrict communication to student's behavior in math classes. The reason is that while standardized tests only cover math and language, the Education Secretariat believes that math teachers keep more accurate records and would have an easier time using the online platform than Portuguese teachers.

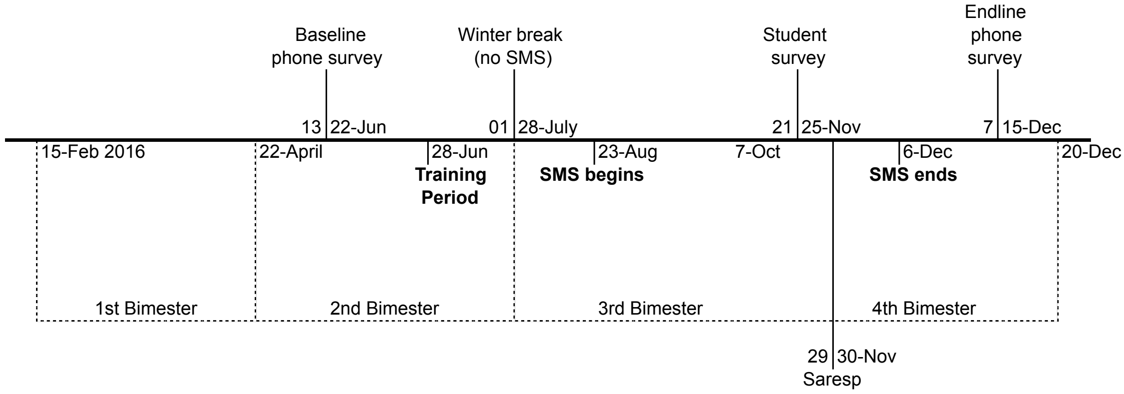
[18]Evidently, as there is selection in who would take up the information offer, one could not merely restrict the sample to those who do not reply.

[19]Rogers and Feller (2016) convey information relative to the classroom modal behavior, using child-level information as a placebo, across US schools.

[20]We thought that the median behavior (e.g.: "most students in Nina's class missed less than 3 classes in the previous 3 weeks") was much easier for parents to understand than the mode, which was graphically conveyed through letters in Rogers and Feller (2016).

[21]We also survey parents at baseline about their best guess for their child's attendance, so as to investigate heterogeneity of treatment effects by the accuracy of parents' beliefs.

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, on XXXX] Teachers began to fill the platform on the week of June 24$^{\text{th}}$. Parents were exposed to the program during six months of the academic year, until the first week of December (when final exams take place. [Parents were surveyed at end line on XXXX. Standardized test scores took place on YYYYY.]



Parents of all treatment arms only receive the text message if the teacher fills in the platform that week. This is true even for the salience group, in order to avoid confounding treatment effects with teachers' non-compliance. After teachers have filled the platform until Sunday of each week, parents receive the message on the following Tuesday, according to their treatment status, as showed in the table below. The content of the messages is simple and clear, and messages across treatment arms were designed to match number of characters as close as possible.

Salience	**Information**	**Relative Information**
For a good school performance, it is important that Guilherme 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, Nina missed less than 3 classes. In his class, most of the students didn't miss any class.

For salience messages, we change the wording of the messages only slightly every cycle, so as to prevent triggering spam-avoiding behavior by parents. For the full script of messages sent for each treatment arm, see Appendix A.

Teachers and schools are not aware of their assignment, nor of parents' assignments. For the *relative information* arm, the platform computes the class median once the teacher submits all students' information every week. As for the *salience* arm, although teacher will fill in child-level information every week, parents will only receive general information aimed at raising salience about that dimension of children's effort.

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 school events per month). Once an event is registered through principal's login, the system sends two SMS notifications to parents, respectively one week and one day prior to the event.

Because we worry about the possibility that peer effects, contamination across parents or teacher effects may bias downwards any differences across groups, [we randomize at two levels, saturating the intensity of treatments across schools]. In a subset of schools, students were assigned to either the salience or the control groups – but not to the child-specific information group. This group allows ruling out concerns with interactions between the information and the salience groups, since it allows estimating the effects of salience in the absence of the former. In a final subset of schools – the *pure control* group –, all students were assigned to the control group.

We randomly selected these schools using a stratified assignment to ensure that students in this group were statistically identical with respect to baseline characteristics to students assigned to the interventions. Teachers in the pure control group schools do not weekly fill in the platform. The reason is two-fold. First, to avoid deception, and, second, to avoid poor compliance in filling in the platform once teachers eventually realized that information was not being delivered to parents. There are no other differences across the treatment and pure control groups: students in pure control schools were enrolled through the same process as those in treatment schools, and 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.

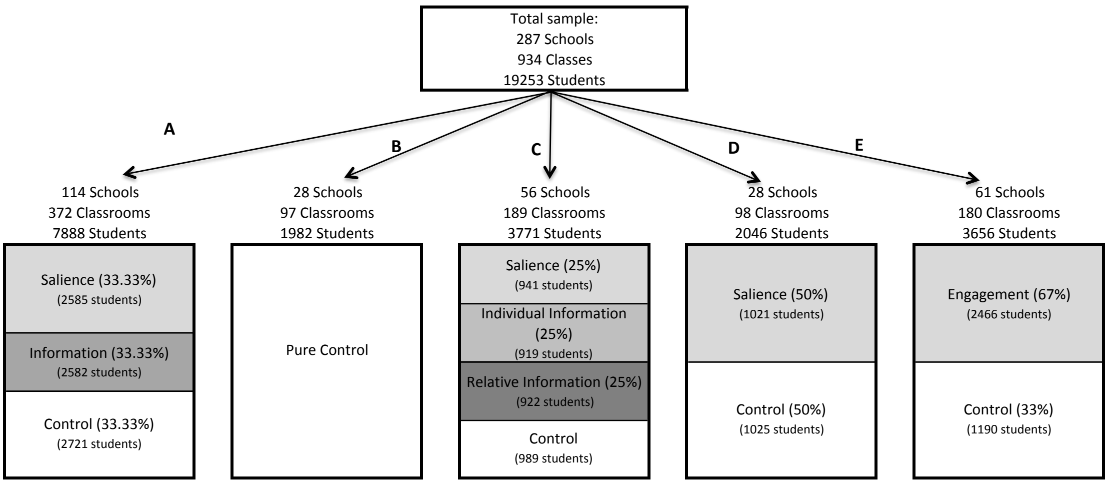
While relying on the pure control group as a counterfactual can rule out spillovers, it also brings about potential concerns with teacher effects, since entering information in the platform may have induced teachers to change behavior (e.g. perceptions of increased parent or school monitoring, salience of the activities). To deal with this concern, we include an additional subset of schools for which we deliver a nudge program instead, reaching parents directly, without informational requirements or the need to involve teachers at all. Such program is inspired by READY4K (York et al., 2017), sharing weekly suggestions of activities for parents to do with their children, over SMS. This intervention is also randomized within classrooms within this subsample.

4. **Nudge:** Messages with suggestions of non-curricular activities to do with their children

There are two relevant features of this subset of schools. First, they were not offered the possibility of sending monthly communication to parents, since in some schools math teachers also handled this activity (delegated by principals) and we want to preclude any teacher effects. Second, their students are not statistically identical at baseline to those in our other subsamples. The reason is that the Education Secretariat required us to work in a different region of the State whenever the communication platform was not made available to principals, where students were relatively low-performing at baseline.

We tackle this issue by taking advantage of the fact that our program was ran only during the second half of the school year, comparing the evolution of the different sub-samples, before and after the program was introduced. The differences-in-differences strategy estimates the causal effects of the nudge program as long as student outcomes in different sub-samples would have evolved identically in the absence of the program. We discuss this strategy in detail in subsection 5.

Randomization is performed in two steps. First, with the one exception just discussed, schools are randomly assigned to each of the five different subsamples, determining the treatment arms made available at each school.[22] Second, students are randomized within-class to each treatment arm:



To summarize, subsamples A through C allow separating the effects of information and salience; subsample B allows a counterfactual for estimating spillovers; subsample D allows estimating the effect of salience without spillovers from information; and subsample E allows pinning down the extent of teacher effects.

We randomize assignment in two steps. In the first step, we stratify the assignment of schools to subsamples based on three variables: school average first quarter math scores in the Education Secretariat's internal quarterly assessment, school average truancy rate, and share of parents enrolled in our program. In the second step, we stratify the assignment of students to groups within class based on the first quarter math scores in the Education Secretariat's internal quarterly assessment.[23]

The design choice for subsamples A through D reflects power calculations accounting for the hypothesis of interest. In the case of subsample E, the sample reflects the demands of the Education Secretariat.

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

[23]Not all students take this test (which is not mandatory). For students with no scores, we predict their scores based on a simple linear regression using all baseline covariates, and then stratify based on predicted scores.

3.3 Teacher platform

We created a web-platform specifically for this project and designed it in a simple and intuitive way such that schools could easily manage it.[24] 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.[25] The system requires teachers to fill in information for all students.

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

Each week teachers receive a text message, reminding them of the information that they should fill in that week. Teachers who miss a week receive an alert, stating that they did not fill in the platform that week and encouraging them to do so in the following week. Principals receive motivational messages, encouraging them to engage teachers in the program, as well as alerts in case the usage by teachers in the school is low.

There is perfect compliance with the randomization protocols, since our implementing partner (MGov Brasil) had full control over both enrollment files (all data had to be entered by teachers into its system prior to the start of the experiment, and assignment was conditional on enrollment) and the platform's outputs and messages ultimately sent to parents and guardians.

3.4 Outcomes

In order to enroll in the program, parents had to provide informed consent through a registration form, in which they listed their cell phone number, their relationship with the student, gender, age, race, income bracket, education, and information on their other children's genders and ages.

Through our online platform, we have weekly records of teachers' inputs about their students, alternating weekly across attendance, tardiness and assignment completion.

In what comes to parents, we surveyed those enrolled through automated phone surveys (Interactive Voice Response, IVR) at baseline and endline to collect self-reported parenting practices, parents' beliefs about their children, as well as parents' demand for information.

[24] 60% of Brazilian schools have access to internet, although typically only with very limited bandwidth – typically below 4 mbps, shared across staff and all student computers, if any. The web-platform is very low-bandwidth, and can be accessed by principals and teachers from any computer or smartphone, even outside of school.

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

The baseline survey was conducted on the week of June 13$^{\text{th}}$ and the endline survey took on the weeks of December 5$^{\text{th}}$ and 12$^{\text{th}}$.

We also conducted a face-to-face survey with enrolled students at the end of the intervention (December), through which we collected data on parents' participation, student's activities, values and aspirations, as well as students' social and emotional skills.

The São Paulo Education Secretariat provided quarterly data on student attendance, grades and retention status in 2016. According to official guidelines, all teachers assign numeric integer grades ranging from 0 to 10, with a passing grade set at 5 points for all disciplines. Attendance is recorded in percentage points (0–100 interval). Finally, we draw upon data from SARESP (System of School Performance Evaluation of the State of São Paulo), the Education Secretariat's yearly standardized test, applied across all State schools.[26]

[DESCRIBE SAMPLE SIZES FOR EACH OF THE SURVEY INSTRUMENTS]

In what comes to beliefs, parents were asked at baseline to give 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, scaled for three weeks. 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).[27] Parents were also asked to give their best estimate of their child's performance in math classes. Again, parents had to choose one out of 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. Parents' answers were compared to administrative records for the first quarter: below average was determined as a GPA below 5, adequate as 5-6; good as 7-8 and very good as 9-10.

Figure 1 illustrates the distribution of parents' beliefs at baseline and children's actual outcomes. Panel A overlays the distributions of parents' answers and administrative records, while Panel B documents the gap between the two, such that positives values indicate optimistic parents – for attendance, those who believe their children are less absent than they actually are, for GPA, that their kids are doing better than they actually are.

[Figure 1]

Overall, parents are optimistic about their children attendance: similar to Bergman (2017), most parents think that their child misses fewer classes than they actually do. Interestingly, however, the same is not true for GPA: the sample is about evenly distributed across optimistic, accurate and pessimistic parents. We take advantage of that variation when teasing out the mechanisms behind the effects of communication.

Finally, we repeat the same exercise at the end line survey with parents, asking them about attendance and grades over the last quarter. In particular, we are interested in

[26] All students in 1$^{\text{st}}$, 3$^{\text{rd}}$, 5$^{\text{th}}$, 7$^{\text{th}}$, and 9$^{\text{th}}$ grades of primary school and the 3rd (final) year of high school are tested on their knowledge of math and Portuguese.

[27] See Appendix B for the full script.

whether communication affects accuracy at end line. Note the important change with respect to how we ask about attendance at end line – for the whole quarter, rather than for the last three weeks. The reason why we ask about it in this way is because by that time students were supposed to have been handed in their final scorecards. If communication increases the likelihood that parents learn about the content of the scorecards, then the right metric to track would be their knowledge about their child's overall absences, rather than the scaled version for the last three weeks (for all the details, see 5). [HERE MENTION INFORMATION VS. SALIENCE]

3.5 Estimation

Estimation, regression equations, standard errors and pre-registration.

4 Results

This section starts by presenting manipulation tests in subsection 4.1, followed by descriptive statistics and randomization checks in subsection 4.2. Subsection 4.3 presents our main results. Next, subsection 4.4 presents findings for more demanding counterfactuals to salience effects: relative information and extreme messages, both of which are more likely to make parents update their beliefs. In face of our null result, we discuss power calculations in subsection **??** to tackle the issue of whether our design would have allowed detecting meaningful differences between salience and information effects. Last, we assess the robustness of our results to different concerns: in subsection 4.5 we investigate whether the lack of difference between salience and information is driven by the interaction of the two treatments; in subsection 4.6 we document the extent to which effects may be driven by differential behavior of teachers required to fill-in the platform; and in subsection 4.7 we investigate whether treatment effects are short-lived.

4.1 Manipulation tests

To begin with, 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 output measures. Figure 2 displays statistics for platform usage and receipt of text messages.

[Figure 2]

Over the course of the 18 weeks, 66% of teachers inputted students' information through the platform on 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 the Appendix, we assess the robustness of our results to dropping observations from schools with the highest platform usage for the salience-only subsample (D), so as to equalize usage rates across treatment arms.

At the endline 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 cell phone number for their kids at enrollment, this result is not just a mechanical artifact of sharing parents and children sharing the same cell phone, but rather hints at some form of communication between parents and children being triggered by the text messages.

4.2 Sample and balance tests

Table 1 presents averages for students' and parents' baseline characteristics by treatment arm, along with p-values of a joint test for the null hypothesis of whether averages are equal across groups. Panel A displays baseline characteristics for students. Around 50% are girls, 40% are brown or black, and the average age is 14.7, within the range of the appropriate age for the ninth grade. Panel B shows parents' characteristics. 76% of those enrolled are mothers, at their early 40s; strikingly, 69% have educational achievement no greater than middle school, what means that, for 2/3 of our sample, children have progressing in school at least as far as their parents did. Together with the figure of 59% of families earning monthly less then 3 minimum wages, the table illustrates the low socioeconomic status of parents in our sample and the challenges associated with the most straightforward interventions, such as advising parents to work together with their children in homework assignments.

[Table 1]

Column (6) shows the p-values for the F-tests of joint equality of averages for each variable across the four treatment arms. The sample is balanced: across 17 variables, only for age differences are statistically significant at the 10% level – which is consistent with chance, and even in that case it is fair to say the difference is a precisely estimated zero.

66% of the almost 30,000 parents invited to participate enrolled in the program. Table 2 analyzes selection in opt-in. For parents who did not enroll, we only have student characteristics available from administrative records – gender, age, math and Portuguese baseline attendance and grades, and status of participation in *Bolsa Familia*, Brazil's flagship conditional cash transfer. If there are systematic differences across those enrolled and those who are not in what comes to those characteristics, then one might be concerned about whether our results would generalize if the intervention was scaled-up to the whole school system.

[Table 2]

According to Table 2, parents who joined the program were less likely to benefit from the conditional cash transfer, and their children had statistically higher attendance and grades compared to those of parents who did not enroll in the program. Since assignment is randomized conditional on enrollment, selection does not bias our results. Having said that, one might still worry about generalizability. To that point, since any educational intervention that requires parents' consent is expected to have imperfect compliance, the relevant parameter should be the average treatment effect on the treated, which is captured by our estimates. Moreover, even if one were interested in the average treatment effect on the absence of selection, we can still re-weight observations by the inverse opt-in probability to gauge the extent to which results would change due to heterogeneous treatment effects (See Table C.5 in the Appendix).

4.3 Main results

To decompose the effects of communication into those of lower monitoring costs and those of higher salience of monitoring benefits, we estimate the following equation:

$$\begin{aligned} Y_{sci} = \alpha + \beta_1 Salience_{sci} + \beta_2 Info_{sci} + \beta_3 Control_{s=treated,ci} \\ + \sum_{k=1}^{K} \gamma_k X_{scik} + \theta_s + \varepsilon_{sci} \end{aligned} \tag{1}$$

where Y_{sci} denotes the outcome of interest for student i in classroom c of school s ; $Control_{s=treated,ci} = 1$ for the control group within treatment schools, and 0 otherwise – pure control schools stand for the reference category, the omitted indicator variable –; X_{scik} is a matrix of student's covariates, including students' gender, age and race, their attendance and GPA prior to the intervention, and their parents' or guardians' gender, age, race, income and education; θ_s is a randomization stratum fixed-effect; and ε_{sci} is a zero-mean error term. We cluster standard-errors at the classroom level. The share of the effects of *information* that could be accounted for by *salience* effects is computed from the ratio $\frac{\hat{\beta}_1}{\hat{\beta}_2}$.

Table 3 shows the results for fourth-quarter's attendance in Math classes, math GPA, promotion status, and math scores in SARESP, São Paulo State standardized test.[28]

[Table 3]

First, focusing on the estimates for the effects of *information*, even though average attendance on the control group is already quite high – in particular, because *Bolsa Familia*'s

[28]Only students with non-missing values for all outcomes and control variables are included in the analysis. Descriptive statistics and balance tests are shown in Tables C.1, C.2, (C.3) and (C.4) in the Appendix.

conditionality requires attendance 85% or higher – it is still the case that communication increases it by 2.1 percentage points, equivalent to attending five additional classes in the academic year. Information increases Math GPA by 0.071 standard deviation, similar to what has been found elsewhere (Berlinski et al., 2016). Counter to the worry that math tests might be graded differentially by the teacher herself, effect sizes are about the same (0.107 standard deviation) when it comes to standardized test scores, graded centrally by external officers. We also find a significant and sizeable positive effect of information on the likelihood of being promoted to high school – a 2.6 percentage-point effect size, even though the control mean is above 90% (partly because it is quite expensive for the State to fail students).

Second, and most strikingly, comparing those estimates to those of awareness messages, we find that *salience* can account for most of the effects of information: the ratio of coefficients is never lower than 89%, and salience point estimates are sometimes larger – up to 126% of information effects. Information and salience coefficients are never statistically different at the 10% significance level, and, even considering the lower bound of 90% confidence intervals for the ratio between salience and information effects, the former would never account for less than 60% of the effects of information.

Going beyond averages, Figure 3 displays the fourth-quarter distribution of math attendance, math GPA and math standardized test scores for the different groups.

[Figure 3]

Panels A through C show that the effects of information and salience percolate to the whole distribution of students, but are especially visible in Panel C for students around the median of the pure control distribution, whose test scores are more pronouncedly 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.

Exploring parents' and students' endline survey data, we find that treated parents ask their children systematically more about schools, 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. We summarize those results in Appendix D, in Tables D.1 through D.4. Results inform the theory of change of the program, whereby communication positively affects parents' behavior and aspirations, then students' behavior, and finally students' attendance, grades, test scores and promotion rates.

We also consider heterogeneous treatment effects by gender within the theory of change's framework: boys experienced larger treatment effects from the interventions, with higher impacts on attendance, GPA, promotion rates and standardized test scores (Table D.5). Consistently, male students parents' behavior and aspirations are affected to a significantly greater extent, as well as boys' behavior (Tables D.6 through D.8). [29]

Are gender differences driven by differential parental responses to treatments, or by baseline differences in performance across boys and girls that generate ceiling effects for the latter? To answer that question, we match boys and girls by their baseline characteristics, and re-estimate the regressions above controlling for that propensity score matching.

[FORTHCOMING]

Last, Table C.5 in the Appendix shows results for re-weighting observations by the inverse probability of opting-in the program. Treatment effects are very similar to those showed in table 3 – if anything, slightly larger –, and all conclusions from the main analyses remain unchanged.

[STATISTICAL POWER = 2.8 S.D. FORTHCOMING – BE PRECISE ABOUT WHAT WE CAN DETECT]

4.4 Can more informative messages do better?

Is information really unnecessary, or did the experiment convey too coarse information to produce additional effects? This subsection considers more demanding counterfactuals for salience effects. First, to test whether finer-grain information matters, for a sub-sample of the information treatment group we communicate children's metrics in relative terms to the median behavior of their peers.

Table 4 shows results using the same specification in equation 1, but adding an indicator variable for the relative information treatment. In this table, all ratios and cross-coefficients tests refer to differences between salience and relative information point estimates.

[Table 4]

Similar to Rogers and Feller (2016), relative information effect sizes are larger for some outcomes – notably, for standardized test scores, even though point estimates are actually lower for promotion rates. Nevertheless, it is still the case that awareness messages amount to at least 68% of the effects of information.

While we cannot rule out that even finer-grain information might promote larger effect sizes, our information intervention provides an appropriate counterfactual as it resembles 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).

Second, we estimate heterogeneous treatment effects by the share of weeks in which teachers' tried to communicate extreme messages – filling in no stars, what is equivalent to missing most classes or assignments over the previous three weeks –, since those would make it more likely that parents would update their beliefs.

[29] For Appendix D, all regressions were estimated using a smaller sub-sample, which excludes observations with missing values for any of the outcomes within the theory of change. Table C.2 shows balance tests for this sub-sample.

[FORTHCOMING]

[STATISTICAL POWER = 2.8 S.D. FORTHCOMING – BE PRECISE ABOUT WHAT WE CAN DETECT]

4.5 Are effects driven by interactions between the two treatments?

Does a combination of spillovers across parents, peer effects and teacher effects – all coming from the information treatment – affect those receiving salience messages? Since the main counterfactual we rely on is *not* within-class control group students, this is a relevant concern. If that is the case, treatment effects should be lower within the sub-sample of schools in which there was no information treatment.

To test this hypothesis, we investigate wether salience effects are smaller in sub-sample D, for which only salience messages – and no information – were delivered. We estimate the following model:

$$\begin{aligned} Y_{sci} = \alpha + \beta_1 Salience_{sci} + \beta_2 Info_{sci} + \beta_3 Control_{s=treated,ci} \\ + \beta_4 Salience_{sci} \times \varphi_{s \in D} + \varphi_{s \in D} + \sum_{k=1}^{K} \gamma_k X_{scik} + \theta_s + \varepsilon_{sci} \end{aligned} \quad (2)$$

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

If it is the case that the effect of salience is lower on the absence of information, we would expect $\beta_4 < 0$. Table 5 presents the results.

[Table 5]

It is not the case that salience effects are lower on the absence of information; conversely, its effect are even larger within those schools. Once we correct for the fact that, in sub-sample D, the frequency of teachers who weekly filled-in the platform was slightly higher than that of other sub-samples, salience effects are no longer statistically larger within sub-sample D, but it is still the case that they are nowhere lower on the absence of information. [30]

[30] Differences in frequency are very relevant since, even in the salience group, parents only receive text messages on weeks in which teachers fill-in the platform. To test if higher SMS frequency drives higher salience effects in sub-sample D, we build a new sample in which we equalize the frequency teachers filled-in the platform across sub-samples. To maximize the number of observations we keep in the analysis, we do so by dropping all observations from 7 sub-sample D classrooms for which teachers had filled-in the platform all the 18 weeks, and from 27 classrooms from the sub-sample C (25% salience, 25% info, 25% relative info, 25% control) where average frequency was lower. In this new sample, the average number of times the teacher filled-in the platform is equal across all sub-samples. We then replicate our main results as well as the above analyses for interactions between treatments. Results, shown in Tables H.1 and H.2, are very similar to those of the main text.

4.6 Are effects driven by differential teacher behavior?

Table 6 shows results for the within-class control group as a counterfactual. Spillover effects from communication are substantial: within-classroom control students experience almost as large effects on attendance and GPA, and statistically identical effects on standardized test scores and promotion rates.

[Table 6]

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. There are no other differences across the treatment and pure control groups: (i) sub-samples are balanced across a rage of observable characteristics, (ii) students in pure control schools were enrolled through the same process as those in treatment schools, and (iii) 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. Despite all commonalities, requiring teachers to weekly fill-in a platform with information about their students may have made them feel they were being monitored, and changed their behavior. For inference about the mechanisms behind communicating with parents to be generalizable beyond our setting, it is crucial to understand the extent to which impacts would remain when parents are nudged directly, on the absence of a platform for teachers.

To answer that question, for a sub-sample of those enrolled we deliver a nudge program instead, reaching parents directly, without informational requirements or the need to involve teachers at all. Such program shares weekly suggestions of activities for parents to do with their children, over SMS. The program is based on READY4K! (York et al., 2017); see Section 7 for more details.

The main challenge of using that sub-sample is that its students 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 communication platform was not made available to principals, and students were relatively low-performing at baseline in this 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, comparing the evolution of the different sub-samples, before and after the program was introduced. The differences-in-differences strategy estimates the causal effects of the nudge program as long as student outcomes in different sub-samples would have evolved identically on the absence of the program. While the identification assumption cannot be tested, we can test whether the different sub-samples were evolving differentially within the first half of the school year, even before the onset of the program. Results are as follows.

We estimate the following model:

$$Y_{scit} = \alpha + \theta_t Post_t + \theta_j Engagement_{sci} + \beta Engagement_{sci} \times Post_t + \varepsilon_{scit} \qquad (3)$$

where Y_{scit} denotes the outcome of interest for student i in classroom c at school s on quarter t; $Post_t = 1$ if $t \geq 3$, and 0 otherwise. Pure control schools stand for the reference category (omitted indicator variable); and $\epsilon_{i,c,t}$ stands for robust standard errors.

Figure 4 displays the quarterly evolution of math attendance and GPA for the pure control group and the engagement treatment. Visibly, students in sub-sample E had significantly worse performance at baseline.

[Figure 4]

Outcomes were moving in parallel for the two groups before the intervention (during the first two quarters); during the last two quarters, however, outcomes for the engagement treatment start trending upward, reversing a declining trend for attendance within pure control schools and fully catching up in grades already by the third quarter.

Figure 5 shows quarterly coefficients for the differences-in-differences estimate of model 3, using the first quarter as the reference period.

[Figure 5]

Panels A and B showcase no statistically significant difference across groups before the onset of the program. For attendance, this difference becomes significant and increases to a 2.3 p.p. and 2.4 p.p. respectively on the second and third quarters. For GPA, the difference becomes significant on the third quarter (of the order of 0.14 s.d.), and marginally insignificant on the last quarter even though engagement's effect size (0.09 s.d.) is the same we find for the main sample (although less precisely estimated from a much smaller sample).

Last, Table 7 compares the nudge program to the salience-only sample (D), contrasting experimental results for the former with differences-in-differences estimates for the latter. Since both samples were receiving one message a week and no information, the only difference between them are potential teacher effects.[31]

[Table 7]

Comparing point estimates across Panels A and B, we can rule out that platform-induced teacher behavior explains more than 1/3 of treatment effects. All in all, results suggest that the bulk of our findings do not stem from teacher effects in treatment schools and can be generalized beyond this setting.

[31] Another difference is that in pure control schools principals could send up to two monthly communications to parents about school events. If those were relevant for treatment effects, we would overestimate teacher effects from the comparison.

4.7 Are effects short-lived?

A final concern is whether the effects of salience messages are short-lived. Effects could die out over time if parents infer poor performance from salience messages, but gradually realize they were misled. If that is the case, then salience messages should make parents more pessimistic about their children's school performance. Moreover, under the biased inference hypothesis, the ratio between the effects of salience and information messages should close to one for low-performing students (since both treatments would affects parents' beliefs the same way), but greater than one for high-performing students (since awareness messages would tend to increase monitoring relative to reassuring information messages).

To test whether that is the case, we first look at heterogeneous treatment effects by parents' baseline accuracy. Table 8 presents the results.

[Table 8]

We do not find evidence that salience messages make parents systematically more pessimistic than information messages. Coefficients of both salience and information treatments on parent's beliefs are the same across all pessimistic, accurate and optimistic parents.[32] Moreover, salience makes parents who were pessimistic at baseline significantly more accurate (hence, less pessimistic) at endline in what comes to their children's Math GPA.

Next, Table 9 presents results for heterogeneous treatment effects by students' baseline performance, splitting the sample between below- and above-median students, according to first quarter's Math GPA. We rely on baseline performance rather than teachers' inputs to the platform because student performance after the onset of the program is endogenous to treatment status.

[Table 9]

The ratio between salience and information treatment effects is higher for above-median students in what comes to promotion rates and standardized test scores, in line with the prediction from the biased inference hypothesis, but the opposite is true in what comes to attendance and GPA.[33] Altogether, results do not support the idea that the salience treatment works by making parents systematically more pessimistic about their children's performance.

Last, we look at the dynamics of treatment effects on attendance and GPA, taking advantage of the fact that we have access to quarterly data for administrative outcomes. Figure 6 present the results.

[Figure 6]

[32]The negative treatment effects on accuracy about attendance are linked to the mismatch between the time span at which we conveyed information about attendance ("over the last 3 weeks") and that for which we could verify attendance at endline (over the last quarter); see Section 6.

[33]Table E.4 on the Appendix shows similar results for heterogeneous effect by students' baseline performance, but considering students' baseline GPA instead of attendance.

We find that the gap between salience and the pure control group increases over time both with respect to attendance and GPA. At least within the 6-month length of our study, not only it is not the case that the effects of salience messages die out; they even increase over time.

5 Beliefs

[EXPLAIN HOW THE LITERATURE ELICITS BELIEFS, AND HOW WE DO IT INSTEAD – WHAT THE BEST PRACTICES SHOULD BE]

The richness of our data allows us to say more about mechanisms. Parents in our sample have mixed beliefs: in what comes to GPA, the sample is about equally distributed across optimistic, pessimistic and accurate parents. This provides an opportunity to test whether beliefs are indeed the mediating mechanism for the effects of communicating with parents, as Bergman (2017) claims.

To test whether this is the case, we start by analyzing treatment effects on parents' beliefs. If beliefs are a key mediating factor for our results, then communication should make parents more accurate, and effects should be concentrated on optimistic parents, who presumably under-monitor their children within the moral hazard framework.

For eliciting beliefs, parents were asked to provide their best estimate of how many times their child had missed school during three weeks prior to the baseline phone survey – to match the frequency at which we report attendance –, choosing the bracket that most closely matched their prior beliefs (no absence; 1 to 2 absences; 3 to 5 absences; or more than 5 absences; see Appendix B). Accuracy is computed by approximating absences over the first-quarter to expected absences over a three-week period. Parents with guesses in the right bracket were considered accurate, those with guesses in a higher (lower) bracket were considered pessimistic (optimistic).[34]

We start by analyzing whether communication made parents more accurate about children's school behavior. Table 8 presents heterogeneous treatment effects on endline accuracy by splitting the sample according to parents' baseline accuracy with respect to their child's Math GPA.

[Table 8]

Results in Panel A suggest communication made parents *less accurate* with respect to Math attendance, across all baseline accuracy categories; significantly so for the effects of information on pessimistic parents. Such negative effects on accuracy are probably 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). Conversely, when it comes to Math GPA – for which we never shared information over text messages –, Panel B shows that communication seems

[34] Results are robust to different definitions of accuracy.

to *increase* accuracy amongst parents who were not accurate at baseline; significantly so for the effects of salience on pessimistic parents, further corroboration for the attention mechanism. Since we have shown both treatments to positively affect student outcomes to the same extent, the fact that they affect parent's beliefs in opposite ways is suggestive that the latter is not a key mediating factor for treatment effects.

Moving forward, we analyze whether effects are driven by optimistic parents at baseline. Table 12 presents heterogeneous treatment effects on students' outcomes by splitting the sample according to parents' baseline accuracy with respect to their child's Math GPA.

[Table 12]

All coefficients are less precisely estimated due to both smaller sample size due to high non-response rates in phone surveys and splitting the sample according to baseline accuracy. Results show that children of both pessimistic and optimistic parents experience positive effects of communication; in fact, even accurate parents experience positive and significant impacts on promotion rates.

Last, if effects were fundamentally driven by changes in monitoring effort in response to updated beliefs, the only pattern consistent with this framework would be a an *increase* in monitoring amongst optimistic parents accompanied by a *decrease* in monitoring amongst pessimist parents, as the former presumably under-monitored students at baseline, whereas the latter presumably over-monitored. Table 13 presents heterogeneous treatment effects on parent's behavior by splitting the sample according to parents' baseline accuracy with respect to their child's Math GPA.

[Table 13]

Once again, coefficients are less precisely estimated due to both smaller sample size due to high non-response rates in phone surveys and splitting the sample according to baseline accuracy. Results show positive effects of communication on parental involvement in academic activities, on incentivizing school activities and on talking to the child. Both pessimistic and optimistic parents report higher engagement, and even accurate parents change behavior.

[NEXT, WE LOOK AT WHETHER EVEN ACCURATE PARENTS WERE AFFECTED BY INFO, AND WHETHER INFO HAS ADDITIONAL EFFECTS ON THE INACCURATE:]

$$Y_{sci} = \alpha + \beta_1 Salience_{sci} + \beta_2^1 Info_{sci} \times Accurate_{sci} + \beta_2^2 Info_{sci} + \beta_3 Control_{s=treated,ci} + \delta Accurate_{sci} + \sum_{k=1}^{K} \gamma_k X_{scik} + \theta_s + \varepsilon_{sci} \tag{4}$$

where Y_{sci} denotes the outcome of interest for student i in classroom c at school s; $Accurate_{sci} = 1$ if caregiver of student i was accurate at baseline with respect to attendance;

we also analyze an alternative version with baseline accuracy with respect to GPA. Pure control schools stand for the reference category (omitted indicator variable); and $\epsilon_{i,c,t}$ stands for robust standard errors.

[FORTHCOMING]

Altogether, our findings suggest that parent's beliefs do not play a central role in the behavioral change leading to better school performance. Rather, parents' *engagement* seems to be the key mediating factor.

6 Concluding Remarks

We find that weekly communication has large impacts on attendance (2.1 percentage points), test scores (0.09 standard deviation) and promotion rates (3.2 percentage points). Sharing information has no or small additional effects: salience improves outcomes by 89%-126% of the effects of information.

Effects are consistent with inattention: they are larger for parents who are most inaccurate at baseline, most inattentive, and positive even for those with lower willingness to receive information. Consistent with the mechanism, effects of communication are larger for higher-frequency and alternating delivery times. Having said that, delivery on or off work hours did not significantly impact outcomes, and interactivity led to puzzling lower impacts.

Beliefs. When we elicit posterior beliefs after informational interventions, we tend to set ourselves up for success. But science requires precisely the opposite.

Different from Bergman (2017) and Dizon-Ross (2019), we found that beliefs are not central to behavior change: communication leads to positive effects even when accuracy responds differently to different treatments. Moreover, positive effects extend beyond optimistic parents; even parents accurate at baseline change behavior and see better school performance at endline. Altogether, our findings suggest that parent's beliefs do not play a central role in the behavioral change leading to better school performance. Rather, parents' *engagement* seems to be the key mediating factor.

As for all informational interventions, understanding the drivers of the effects of informational interventions with school parents matters for three reasons. First, in developing countries, real-time information systems are often absent, making informational interventions expensive.[35] If salience explains most of the effects of communication, similar effects could be achieved at much lower costs, as interventions to capture attention do not require such information systems. Second, if salience is the key driver of the effects of communication, potentially, the effects of communication could be *much larger*. While informational

[35] Vitória da Conquista, a municipality in a poor Brazilian State, spent over USD 700,000 in 2012 placing microchips in students' uniforms, hoping to cut truancy by informing parents immediately when students missed classes. Read more: http://www.bbc.com/news/world-latin-america-17484532

interventions are constrained by the frequency at which information is available, nudging can be implemented at much higher frequency, and allows for additional features for manipulating attention, such as interactivity.[36] Third, even though higher investments in education might always seem welfare-improving, if salience at least partially drives parents' behavior change one has to ask what is the underlying mechanism behind that change. If parents' *over-monitor* relative to what they would have done had they acquired the information themselves, the intervention is likely to *decrease* overall welfare (unless in combination with other optimization frictions).

This is the first paper to test the hypothesis that behavioral mechanisms may explain why communicating with parents works, decomposing its effects into lower monitoring costs and higher salience of monitoring benefits. 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 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.

A growing education literature suggests parents can affect students' educational behaviors and success when they receive proper information. Bergman (2017) finds that sending parents SMS when their child was missing assignments resulted in significant gains in tests scores, GPA, and measures of student engagement. Kraft and Dougherty (2013) show that frequent teacher-to-parent phone calls increased student engagement as measured by homework completion, in-class behavior, and in-class participation during a summer school program. Bergman and Chan (2017) report a decrease in course failures and absenteeism as a result of alerting parents through SMS about their child's missed assignments, grades and class absences. Berlinski et al. (2016) show that students of treated parents in Chile– who received information on absenteeism, grades, and student behavior–had significantly higher math grades, attendance, better behaviors, and a lower probability of failing the grade at the end of the year.

Informational interventions are mainly based on the hypothesis that there is a moral hazard problem between parents and children, with high monitoring costs (Cunha and Heckman, 2007; Heckman and Mosso, 2014). To that effect, Bursztyn and Coffman (2012) show that 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. Consistent with the moral hazard mechanism, such preference disappears when schools systematically share information about their children's attendance.

[36] It is also worth noting that certain pieces of information may not be as effective in raising perceived returns to monitoring as nudges. Consider a parent who thinks their kid is missing more classes than s/he actually is; information may induce him or her to monitor even less.

Alternatively, effects could be driven by behavioral biases – as we show in this paper. Behavioral interventions had already been shown to systematically improve students' outcomes. York et al. (2017) find that a SMS program affected the extent to which parents engaged in home literacy activities with their children, as well as parental involvement at school, which translated into student learning gains in some areas of early literacy. Castleman and Page (2015) report positive effects of a texting program for recent high-school graduates, designed to incentivize college enrollment. Rogers and Feller (2016) show preliminary evidence that sending messages to increase parents' salience about good school behavior was effective at increasing attendance. The contribution of this paper is to show that information and nudge programs share a common denominator: their effects are driven by getting parenting to the top of mind amongst inattentive parents.

As we argue in this paper, the distinction matters for two reasons. First, providing timely and accurate information about children's behavior requires integrated systems and customized communication, which can be quite costly, particularly in developing countries, where real-time information systems are usually not available; conversely, simply nudging to raise salience does not require any such systems in place. Second, and most importantly, if salience is the key driver of the effects of communication, the effects of communication could be much larger. In fact, we show that combining different features of SMS communication allows for potentially much larger effect sizes on students' attendance and GPA. Without the need to invest in real-time information systems, nudging can deliver larger effect sizes at lower costs.

In this vein, our study contributes to a rich literature that investigates cost-effective alternatives to improving educational outcomes in developing countries. As Ludger et al. (2015) and others have shown, students in developing countries learn much less than students of the same age, or in the same grade, learn in OECD countries. Researchers and policy-makers in these regions have been searching for evidence on how to increase enrollment and attendance at scale, and on how to simultaneously improve quality of human capital formation (Glewwe and Muralidharan, 2015). While different approaches have been explored– 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) –, few have managed to improve student outcomes cost-effectively, through easily scalable interventions.

Lastly, our study also contributes to the still scarce literature on behavioral educational interventions. A growing number of studies studies interventions to tackle parents' inertia and affect parents' routine behavior, (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, so far Education has received comparatively less attention (Lavecchia et al., 2014). Given that investments in children's human capital are crucially about inter-temporal decisions – typically plagued by all sorts of behavioral biases –, the is huge potential for behavioral interventions to improve educational investments . Lavecchia et al. (2014) reviews the recent and growing literature of interventions designed to overcome behavioral barriers in education.

In what comes to welfare, there are two potential caveats to what otherwise seem to be very positive impacts of nudging. First, we do not know what gets "displaced" from parents' attention when parenting becomes top of mind. If higher attention to children's education leads parents to attend less to children's health, for instance, it would not even be clear that investments in children's human capital experience a net increase from a nudge program. Second, parents have to pay the costs of decentralized monitoring costs whenever the State decides to nudge rather than share centralized information, and the sum of those costs may turn out to be higher under decentralized monitoring. Although we cannot completely dismiss those concerns, it is not hard to believe a story in which not much gets displaced by those nudges, since poor parents are at home with children typically only after work, in the evening, when they are quite cognitively depleted. Moreover, the potential increase in monitoring costs going from centralized to decentralized monitoring is presumably small relative to the increase in individual returns to higher parental engagement, especially given the high wage premium of schooling in developing countries like Brazil.

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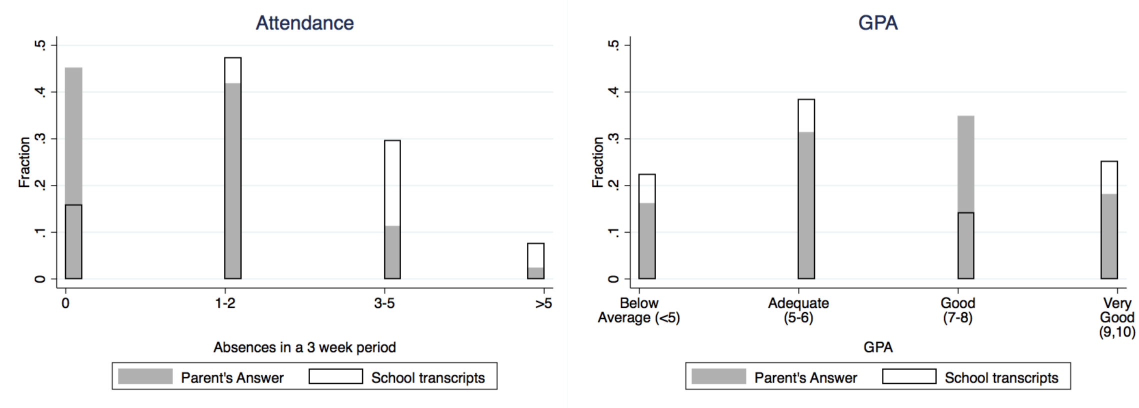
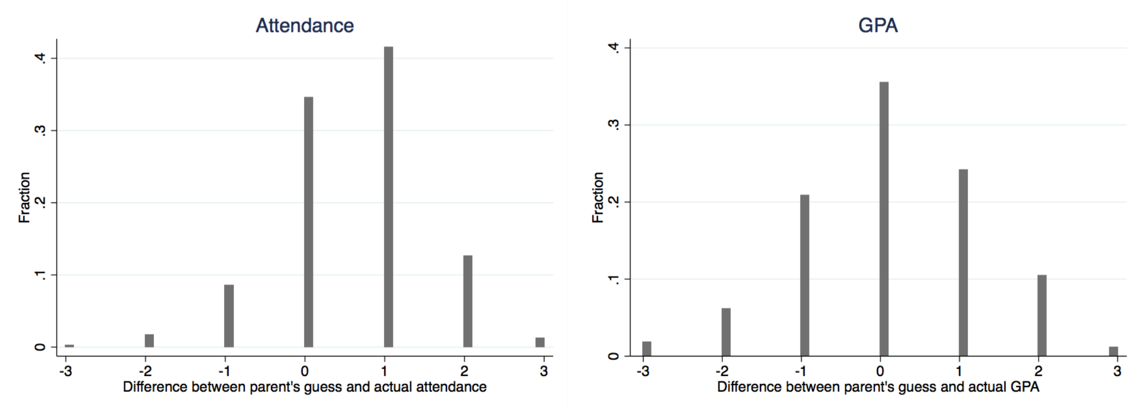
Figures

Figure 1: Parents' accuracy wrt their child's baseline attendance and GPA

Panel A: Parents' answers versus students' baseline performance



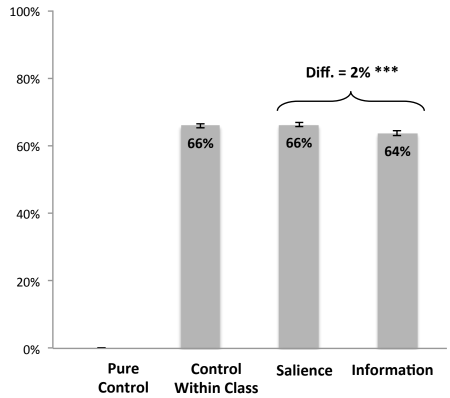
Panel B: Difference parents' answer and baseline performance



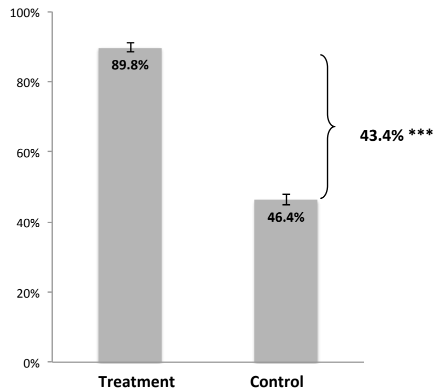
Note: Parents were asked at baseline to give their best estimate on how many times their child misses math classes on a period of three weeks, as well as on their performance in math classes. Data was then crossed with administrative records. Four categories were available for parents' answers on attendance (missed 0; 1-2; 3-5; more than 5). Administrative data register data on attendance on a quarterly basis (period of $\sim$ 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 2: Manipulation Tests

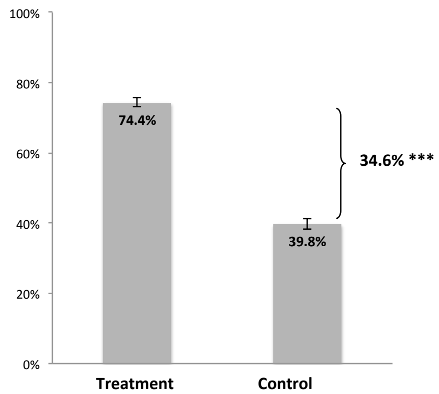
Panel A: Average number of times teachers filled the platform
by treatment status during the 18 week period



Panel B: Did parents acknowledge receipt of text messages?



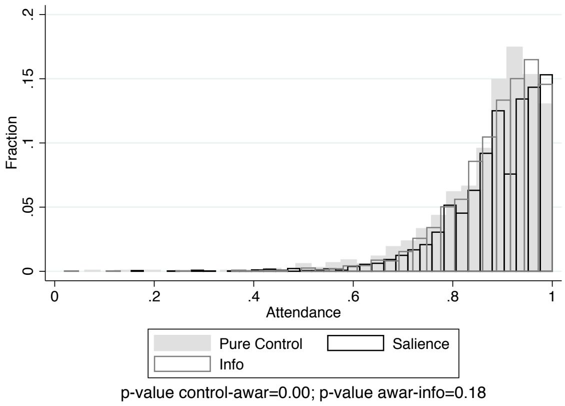
Panel C: Did students know their parents
were receiving text messages?



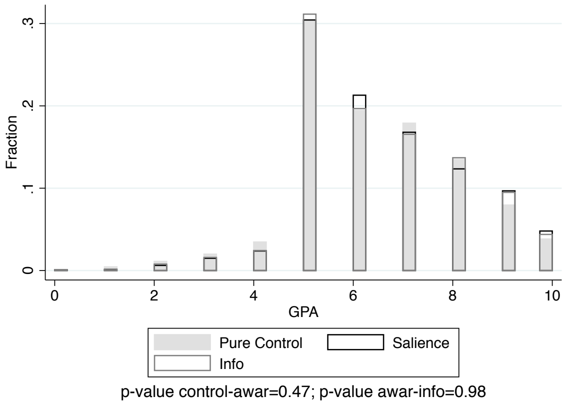
Note: 90% confidence interval. The difference between categories was estimated through a simple regression including fixed effect for strata, and standard errors were clustered at the classroom level. Significance levels are denoted by * if $p<0.1$, ** if $p<0.05$ and *** if $p<0.01$. In Panel A, data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

Figure 3: Distribution Effects

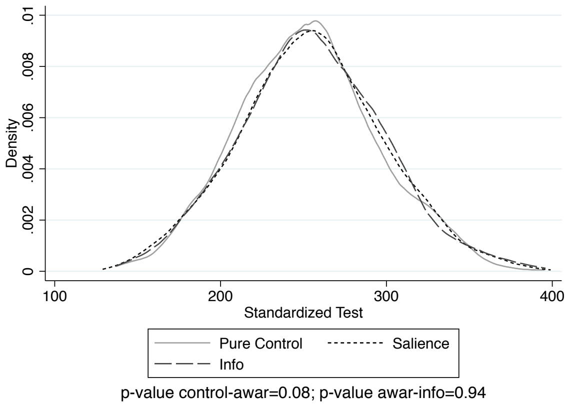
Panel A: Attendance



Panel B: GPA



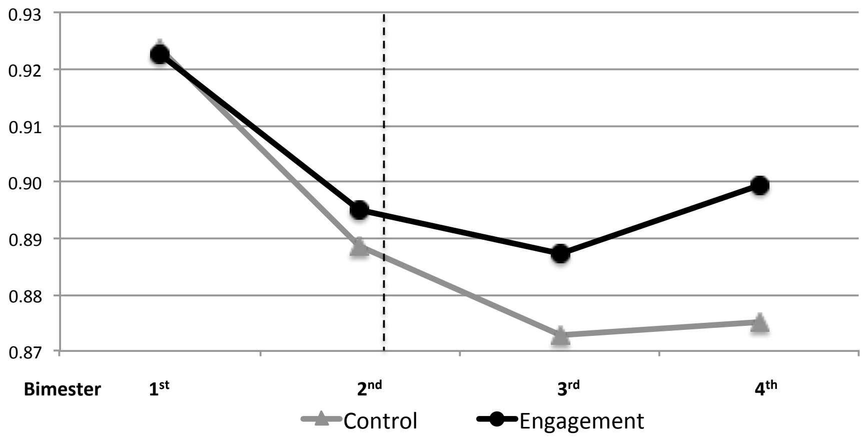
Panel C: Standardized test



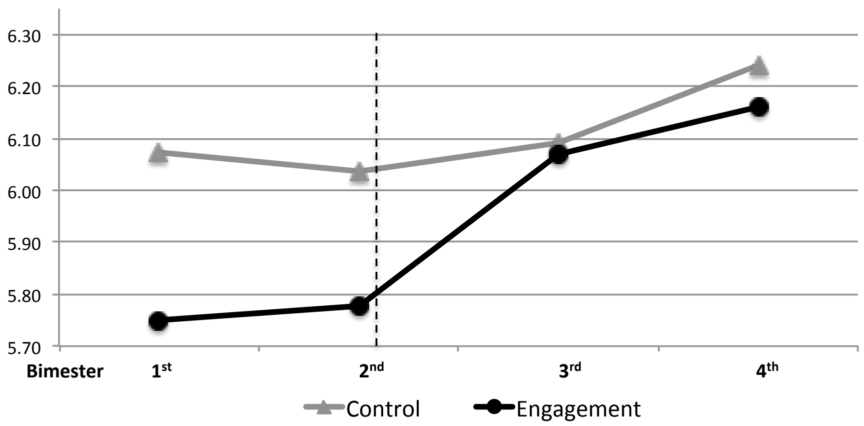
Note: Panels A, B and C show the effect across the distribution of students' attendance, GPA and standardized test for each treatment arm. Data was extracted from administrative records. Attendance is recorded in percentage points (0-1 interval). The GPA has a 10 point scale, where 5 is the passing grade. The standardized test (Saresp) has a 400 scale, where zero is the minimum score. No controls were included. A Kolmogorov-Smirnov equality-of-distributions test was performed to test equality of distribution between the "salience" and "control" groups; and the 'info" and "salience" groups. P-values reports result of the test.

Figure 4: Theory-based nudging program - effect by quarter

Panel A: Attendance



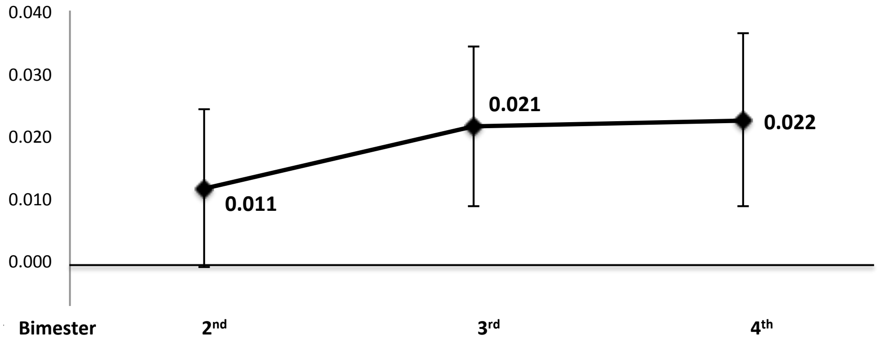
Panel B: GPA



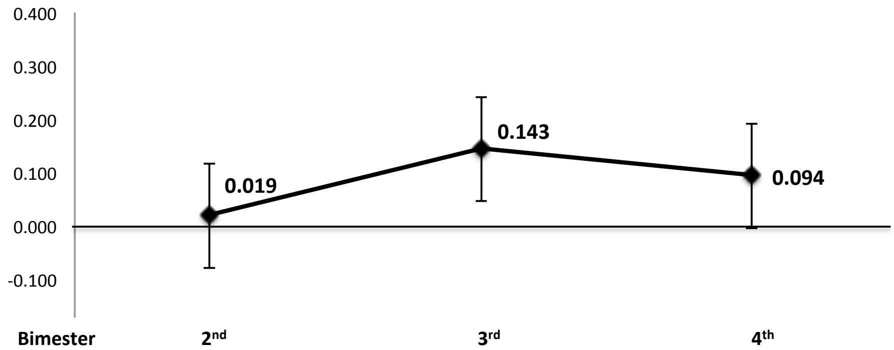
Note: Panels A and B show the raw data for attendance and GPA pre- and post-intervention, for treatment (engagement) and control groups of the theory-based nudging program. Attendance is recorded in percentage points (0-1 interval). The GPA has a 10 point scale, where 5 is the passing grade. The intervention 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 forth 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 5: Differences-in-differences coefficient of the theory-based nudging program by quarter

Panel A: Attendance (p.p.)



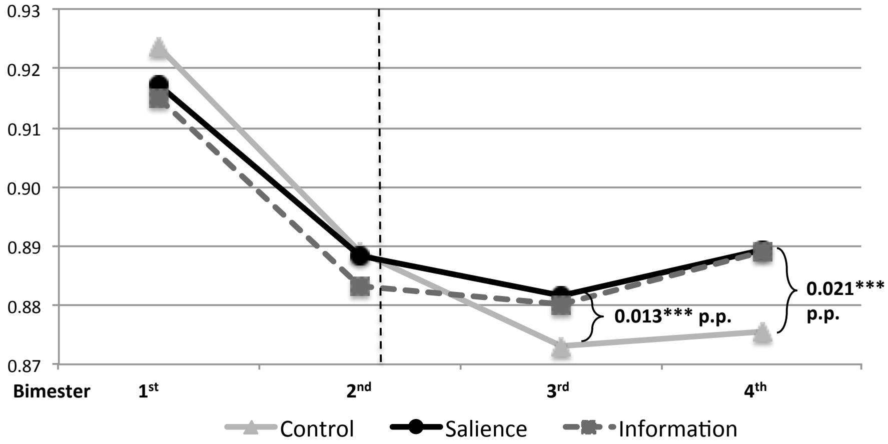
Panel A: GPA (std.)



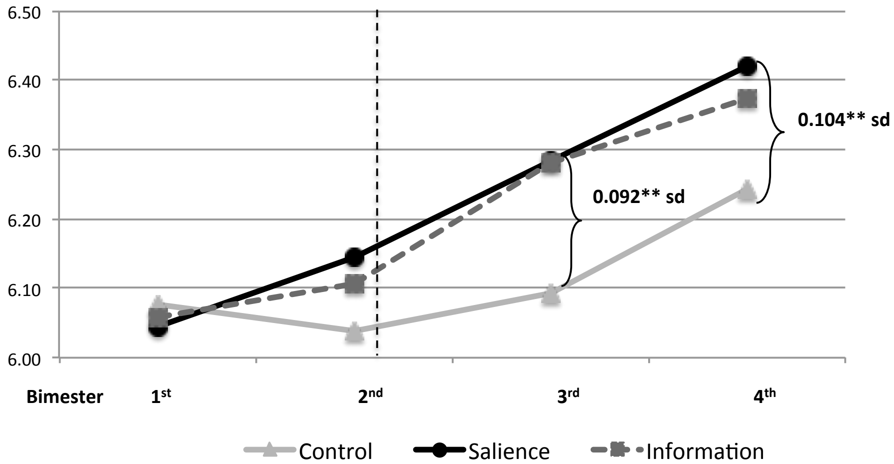
Note: Panels A and B show the differences-in-differences estimates from equation 3 for the theory-based nudging program by quarter, where the first quarter if the reference group. GPA was normalized relative to the distribution of the comparison group (pure control), such that the mean and standard deviation 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 it's interaction with a time dummy. Attendance and GPA are available for each of the forth 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 6: Are effects short-lived? Effect of the intervention over time

Panel A: Effect on attendance over time



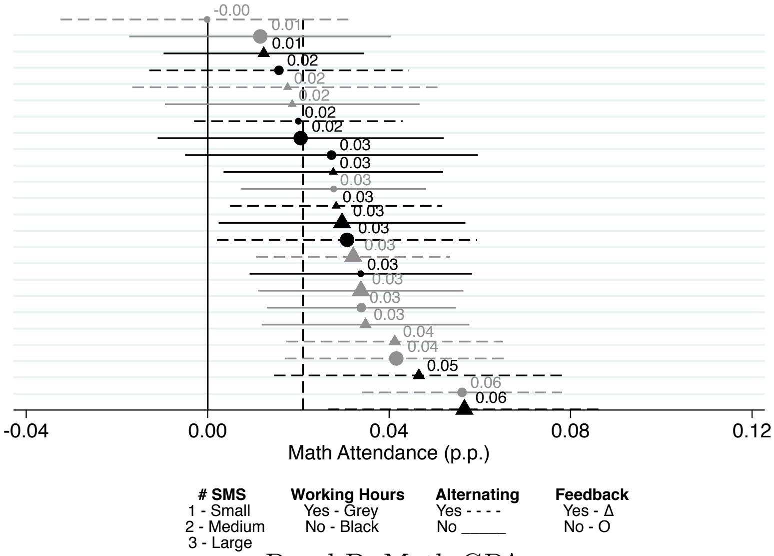
Panel B: Effect on GPA over time



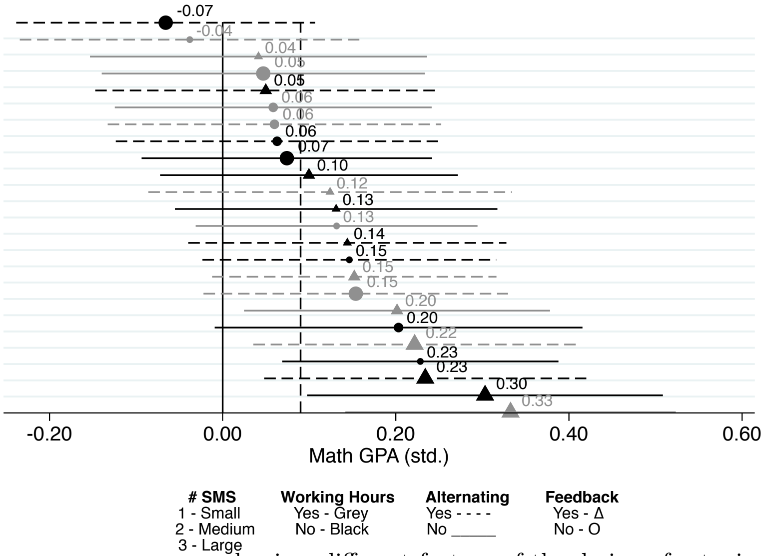
Note: Panels A and B show the raw data for attendance and GPA pre- and post-intervention, for treatment and control groups. Attendance is recorded in percentage points (0-1 interval). The GPA has a 10 point scale, where 5 is the passing grade. The intervention started at the beginning of the third quarter and lasted until the end of the fourth quarter. Attendance and GPA are available for each of the forth 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 7: All combinations of features of a nudge program targeted at capturing parents' attention

Panel A: Math attendance



Panel B: Math GPA



Note: The nudging program cross-randomizes different feature of the design of a typical SMS campaign. The program assess the impacts of alternative campaign parameters: (i) frequency (0, 1, 2 or 3 times a week), (ii) time of the day (afternoon or evening), (iii) consistency (constant or varying time of delivery), and (iv) interactivity (in the form of a feedback flow that asks whether parents complied with the suggested activity). The combination of each treatment generates 24 cells. Panels A and B show the differences-in-differences estimates of each combination cell on students' attendance and GPA (e.g. effect of receiving 3 SMS per week, during the afternoon, alternating time and with feedback). Each horizontal line of the graph represents one cell. 90% confidence interval with standard errors clustered at the classroom level are showed. The size of the markers indicates the number of SMS received (1, 2 or 3); the color of the marker and error bar indicates if the message was sent during work hours (grey) or evening (black); the error bar line style indicates if the time was alternated (dashed line) or not (continuing line); and the shape of the marker indicates if feedback was sent (yes for triangle and no for circle). Attendance is showed in percentage points (0-1 interval), and GPA is showed 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.

Tables

Table 1: Descriptive statistics and balance

	Means				Diff=0 p-value	Sample Size
	Pure Control	Control Within Class	Salience	Information		
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 (max 10)	6.18	6.19	6.13	6.13	0.36	15437
Math GPA (max 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 (1MW $\sim$ $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: 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 2: Selection in opt-in

	Mean		Diff.	Sample Size
	No	Yes		
Female	0.45	0.50	0.05***	23372
			[0.01]	
Age	14.92	14.73	-0.19***	23398
			[0.01]	
Portuguese GPA (max 10)	5.39	6.16	0.77***	22687
			[0.03]	
Math GPA (max 10)	5.09	5.94	0.84***	22691
			[0.03]	
Portuguese attendance	0.88	0.91	0.04***	22850
			[0.00]	
Math attendance	0.87	0.91	0.04***	22753
			[0.00]	
Cash transfer beneficiary	0.19	0.16	-0.03***	23029
			[0.01]	

Note: Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01. Because parents who did not opt-in to the program didn't answer the baseline survey, we only have limited information on them, coming from administrative records (students' gender, age, GPA, attendance and if the family receives cash transfer). We run a simple regression, where each of the characteristics in the horizontal line served as dependent variable, and a dummy indicating if parents opted-in served as the independent variable.

Table 3: School transcripts and standardized tests

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.021***	0.090***	0.032***	0.095**
	[0.006]	[0.032]	[0.012]	[0.047]
Information	0.021***	0.071**	0.026**	0.107**
	[0.006]	[0.032]	[0.012]	[0.047]
Control Within	0.018***	0.070**	0.030**	0.085*
	[0.006]	[0.031]	[0.012]	[0.047]
Control Mean	0.875	0.000	0.938	-0.000
P-value diff. [Info] -[Salience]	0.896	0.221	0.219	0.596
Sample Size	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. Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

Table 4: Salience vs. relative information

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.021***	0.090***	0.032***	0.095**
	[0.006]	[0.032]	[0.012]	[0.047]
Individual Info	0.021***	0.069**	0.029**	0.097**
	[0.006]	[0.032]	[0.012]	[0.047]
Relative Info	0.022***	0.078*	0.017	0.141**
	[0.007]	[0.041]	[0.014]	[0.058]
Control Within	0.018***	0.070**	0.030**	0.085*
	[0.006]	[0.031]	[0.012]	[0.047]
Control Mean	0.875	0.000	0.938	-0.000
P-value diff. [Rel. Info] -[Salience]	0.770	0.690	0.086	0.252
Sample Size	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. Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

Table 5: Interactions with information?

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.017***	0.070**	0.027**	0.101**
	[0.006]	[0.033]	[0.012]	[0.048]
Information	0.021***	0.070**	0.026**	0.108**
	[0.006]	[0.032]	[0.012]	[0.047]
Salience Only	0.001	0.049*	0.004	0.015
	[0.004]	[0.029]	[0.009]	[0.042]
Control Within	0.014**	0.062*	0.026**	0.094**
	[0.006]	[0.033]	[0.012]	[0.047]
Sample Size	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. Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

Table 6: School transcripts and test score - no pure control

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.005**	0.030*	0.003	0.000
	[0.003]	[0.017]	[0.006]	[0.025]
Information	0.005*	0.023	-0.000	0.016
	[0.003]	[0.019]	[0.005]	[0.028]
Control Mean	0.887	0.000	0.966	-0.000
P-value diff. [Info] -[Salience]	0.898	0.713	0.561	0.581
Sample Size	11217	11217	11217	11217
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. Controls in the treated schools are the reference group. The pure control group was excluded from this analysis. 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 7: A parallel salience intervention

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Panel A				
Salience	0.033***	0.138***	0.045***	0.118*
	[0.007]	[0.040]	[0.011]	[0.060]
Sample Size	3180	3180	3180	3180
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes
Panel B				
Engagement	0.020**	0.096		
	[0.009]	[0.060]		
Sample Size	7338	7338		

Notes: 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. In Panel A, treatment effect was estimated from equation 1 for the subsample D (50% salience + 50% control) and standard error are clustered at the classroom level. Panel B shows differences-in-differences estimates from equation 3 for the parallel salience intervention, where the first quarter is the reference group and the fourth quarter is the final period. Only the group of parents who received one text message per week were included in the analysis of Panel B, and standard error are clustered at the classroom level. Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

Table 8: Heterogeneity by parents' baseline beliefs wrt their child's GPA - parents' endline accuracy

	Pessimistic Parents (30.7%)		Accurate parents (36.9%)		Optimistic parents (32.4%)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Accuracy Math Attendance	Accuracy Math GPA	Accuracy Math Attendance	Accuracy Math GPA	Accuracy Math Attendance	Accuracy Math GPA
Salience	-0.04	0.14*	-0.03	0.02	-0.05	0.08
	[0.07]	[0.08]	[0.07]	[0.07]	[0.06]	[0.06]
Information	-0.12*	0.10	-0.04	-0.00	-0.06	0.04
	[0.07]	[0.08]	[0.07]	[0.07]	[0.06]	[0.06]
Control Within	-0.01	0.03	-0.01	0.00	-0.02	0.02
	[0.07]	[0.08]	[0.07]	[0.07]	[0.06]	[0.06]
Control Mean	0.30	0.25	0.29	0.33	0.21	0.21
P-value diff. [Info] -[Salience]	0.13	0.50	0.72	0.61	1.00	0.53
Sample Size	480	480	576	576	506	506
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes

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 $\sim$ 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 9: Heterogeneity by students' baseline attendance

	Low-performing (≤ Median - 54.6%)				High-performing (>Median - 45.4%)			
	(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.)
Salience	0.023***	0.116***	0.045***	0.107**	0.018***	0.070*	0.016	0.092
	[0.008]	[0.039]	[0.016]	[0.053]	[0.006]	[0.037]	[0.010]	[0.058]
Information	0.023***	0.066*	0.032**	0.095*	0.019***	0.095***	0.018*	0.135**
	[0.008]	[0.040]	[0.016]	[0.052]	[0.006]	[0.037]	[0.009]	[0.059]
Control Within	0.018**	0.076*	0.041**	0.108**	0.017***	0.077**	0.015	0.069
	[0.008]	[0.040]	[0.016]	[0.052]	[0.006]	[0.036]	[0.010]	[0.058]
Control Mean	0.85	-0.23	0.92	-0.12	0.90	0.25	0.96	0.13
P-value diff. [Info] -[Salience]	0.95	0.01	0.05	0.70	0.54	0.26	0.65	0.16
Sample Size	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 10: Heterogeneity by parents' attention: time to answer the first question of baseline survey

	Low Attention (>Median - 47.5%)				High Attention (≤ Median - 52.5%)			
	(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.)
Salience	0.037***	0.183***	0.049**	0.098	0.023**	0.080	0.035*	0.075
	[0.010]	[0.056]	[0.020]	[0.078]	[0.009]	[0.053]	[0.018]	[0.073]
Information	0.031***	0.133**	0.037*	0.186**	0.025***	0.097*	0.046***	0.057
	[0.010]	[0.057]	[0.020]	[0.079]	[0.009]	[0.053]	[0.017]	[0.070]
Control Within	0.026**	0.122**	0.028	0.069	0.030***	0.077	0.041**	0.002
	[0.010]	[0.056]	[0.020]	[0.079]	[0.009]	[0.052]	[0.017]	[0.069]
Control Mean	0.86	0.01	0.93	-0.00	0.86	-0.01	0.93	0.00
P-value diff. [Info] -[Salience]	0.26	0.17	0.27	0.07	0.55	0.64	0.26	0.71
Sample Size	2073	2073	2073	2073	2295	2295	2295	2295
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. Response times are used by cognitive psychologists as a measure of cognitive control, see Mani et al. (2013). Parents with above-median average response times are treated as inattentive (low attention), while parents with below-median average response (or equal) are treated as attentive (high attention). 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 11: 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***	0.12**	0.03*	0.08	0.02	0.10**
	[0.01]	[0.05]	[0.02]	[0.07]	[0.04]	[0.04]
Information	0.03***	0.09*	0.04**	0.16**	-0.03	0.02
	[0.01]	[0.05]	[0.02]	[0.07]	[0.04]	[0.04]
Control Within	0.03***	0.08	0.03*	0.03	0.06	0.03
	[0.01]	[0.05]	[0.02]	[0.07]	[0.04]	[0.04]
Control Mean	0.86	-0.06	0.93	-0.05	0.21	0.23
P-value diff. [Info] -[Salience]	0.57	0.42	0.56	0.10	0.13	0.04
Sample Size	2578	2578	2578	2578	1071	1071
	High willingness to receive information (WTR) (36.7%)					
Salience	0.04***	0.18***	0.07***	0.14	-0.15**	0.02
	[0.01]	[0.07]	[0.02]	[0.10]	[0.07]	[0.08]
Information	0.04***	0.15**	0.07***	0.07	-0.16**	0.04
	[0.01]	[0.07]	[0.02]	[0.10]	[0.07]	[0.08]
Control Within	0.03**	0.15**	0.05**	0.08	-0.12*	-0.01
	[0.01]	[0.07]	[0.02]	[0.10]	[0.07]	[0.08]
Control Mean	0.86	0.04	0.91	0.07	0.36	0.33
P-value diff. [Info] -[Salience]	0.89	0.46	0.70	0.24	0.67	0.75
Sample Size	1317	1317	1317	1317	620	620
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: 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.

Table 12: Heterogeneity by parents' baseline beliefs wrt their child's GPA - transcripts and test scores

	Pessimistic Parents (29.4%)				Accurate parents (36.2%)				Optimistic parents (34.5%)			
	(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.)	(9) Math Attendance (p.p.)	(10) Math GPA (std.)	(11) Promotion Rate (p.p.)	(12) Math Standardized Test (std.)
Salience	0.05***	0.09	0.02*	0.14	0.01	0.08	0.03	0.06	0.03**	0.14**	0.05*	0.03
	[0.01]	[0.07]	[0.01]	[0.10]	[0.01]	[0.07]	[0.02]	[0.10]	[0.01]	[0.06]	[0.03]	[0.11]
Information	0.05***	0.09	0.02	0.14	0.01	0.10	0.04*	0.14	0.02*	0.10	0.05*	0.01
	[0.01]	[0.07]	[0.01]	[0.10]	[0.01]	[0.07]	[0.02]	[0.10]	[0.01]	[0.07]	[0.03]	[0.11]
Control Within	0.04***	0.09	0.02	0.04	0.01	0.11	0.02	0.12	0.02*	0.10	0.04	-0.02
	[0.01]	[0.07]	[0.01]	[0.10]	[0.01]	[0.07]	[0.02]	[0.10]	[0.01]	[0.07]	[0.03]	[0.11]
Control Mean	0.85	0.50	0.97	0.21	0.88	0.01	0.93	0.01	0.84	-0.43	0.91	-0.18
P-value diff. [Info] -[Salience]	0.62	0.91	0.08	0.93	0.99	0.73	0.44	0.19	0.42	0.37	0.87	0.73
Sample Size	1026	1026	1026	1026	1263	1263	1263	1263	1203	1203	1203	1203
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	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 $p<0.01$. 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.

Table 13: Heterogeneity by parents' baseline beliefs wrt their child's GPA - parents' behavior

	Pessimistic Parents (29.4%)			Accurate parents (36.2%)			Optimistic parents (34.5%)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Academic Activities	Incentives	Talk	Academic	Incentives Academic	Talk	Academic Academic	Incentives	Talk
Salience	0.22*	0.06	0.10	-0.01	0.07	-0.04	-0.03	0.07	0.21*
	[0.13]	[0.13]	[0.12]	[0.11]	[0.10]	[0.10]	[0.11]	[0.12]	[0.11]
Information	0.20	0.03	0.05	0.19*	0.10	0.10	-0.10	0.14	0.15
	[0.12]	[0.13]	[0.12]	[0.11]	[0.10]	[0.10]	[0.12]	[0.12]	[0.12]
Control Within	0.31**	-0.00	0.09	0.12	-0.05	0.12	-0.14	-0.04	0.11
	[0.12]	[0.13]	[0.12]	[0.11]	[0.10]	[0.10]	[0.11]	[0.13]	[0.11]
Control Mean	-0.17	-0.05	0.00	0.01	-0.02	0.05	0.12	-0.04	-0.01
P-value diff. [Info] -[Salience]	0.84	0.74	0.54	0.01	0.68	0.07	0.39	0.45	0.40
Sample Size	865	863	860	1050	1048	1045	995	994	988
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: outcome variables 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 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. Students were asked at the endline survey about their parent's behavior, where they had to state how often their parents engage in certain activities (never, almost never, sometimes, almost always). Out of the 12 questions, factor analysis was used to create 3 variables of parents 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 14: Heterogeneous effects by features of SMS communication - attendance

	(1) Math Attendance (p.p.)	(2) Portuguese Attendance (p.p.)
Frequency		
1 SMS per week	0.020**	0.014*
	[0.008]	[0.008]
2 SMS per week	0.034***	0.028***
	[0.008]	[0.008]
3 SMS per week	0.032***	0.029***
	[0.008]	[0.008]
P-value diff.	0.08	0.02
SMS Delivery Time		
Work hours	0.030***	0.022***
	[0.008]	[0.008]
Off-work hours	0.028***	0.025***
	[0.008]	[0.008]
P-value diff.	0.72	0.54
Consistency of delivery time		
Varying	0.031***	0.028***
	[0.008]	[0.008]
Constant	0.027***	0.020**
	[0.008]	[0.008]
P-value diff.	0.42	0.09
Interactivity		
Interactivity	0.027***	0.019**
	[0.008]	[0.008]
Passive	0.031***	0.029***
	[0.008]	[0.008]
P-value diff.	0.37	0.05
Sample Size	10308	10308
Randomization strata FE	No	No

Note: The nudging program cross-randomizes different feature of the design of a typical SMS campaign. The program assess the impacts of alternative campaign parameters: (i) frequency (0, 1, 2 or 3 times a week), (ii) time of the day (afternoon or evening), (iii) consistency (constant or varying time of delivery), and (iv) interactivity (in the form of a feedback flow that asks whether parents complied with the suggested activity). The table shows the differences-in-differences estimates from equation 3 for the theory-based nudging program, where the first quarter if the reference group, and the fourth quarter is the end period. Standard errors are clustered at the classroom level. A dummy variable for the control group within class was also included in the model, as well as it's interaction with a time dummy. Attendance and GPA are available for each of the forth 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). Attendance is measured in percentage points (0-1 interval). Significance levels are denoted by * if $p<0.1$, ** if $p<0.05$ and *** if $p<0.01$.

Table 15: Heterogeneous effects by features of SMS communication - GPA

	(1) Math GPA (std.)	(2) Portuguese GPA (std.)
Frequency		
1 SMS per week	0.095	0.042
	[0.060]	[0.067]
2 SMS per week	0.118**	0.067
	[0.060]	[0.068]
3 SMS per week	0.176***	0.147**
	[0.062]	[0.068]
P-value diff.	0.08	0.02
SMS Delivery Time		
Work hours	0.150**	0.074
	[0.058]	[0.067]
Off-work hours	0.110*	0.097
	[0.056]	[0.064]
P-value diff.	0.34	0.50
Consistency of delivery time		
Varying	0.139**	0.074
	[0.056]	[0.065]
Constant	0.121**	0.096
	[0.059]	[0.068]
P-value diff.	0.68	0.59
Interactivity		
Interactivity	0.090	0.047
	[0.056]	[0.065]
Passive	0.170***	0.123*
	[0.057]	[0.067]
P-value diff.	0.04	0.04
Sample Size	10308	10308
Randomization strata FE	No	No

Note: The nudging program cross-randomizes different feature of the design of a typical SMS campaign. The program assess the impacts of alternative campaign parameters: (i) frequency (0, 1, 2 or 3 times a week), (ii) time of the day (afternoon or evening), (iii) consistency (constant or varying time of delivery), and (iv) interactivity (in the form of a feedback flow that asks whether parents complied with the suggested activity). The table shows the differences-in-differences estimates from equation 3 for the theory-based nudging program, where the first quarter if the reference group, and the fourth quarter is the end period. Standard errors are clustered at the classroom level. A dummy variable for the control group within class was also included in the model, as well as it's interaction with a time dummy. Attendance and GPA are available for each of the forth 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). GPA is showed in standard deviation, where GPA was 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 $p<0.01$.

A Appendix – SMS Text Messages

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, lateness or assignment completion, as shown in the table below. Teachers filled information regarding student behavior on each dimension considering the past three weeks.

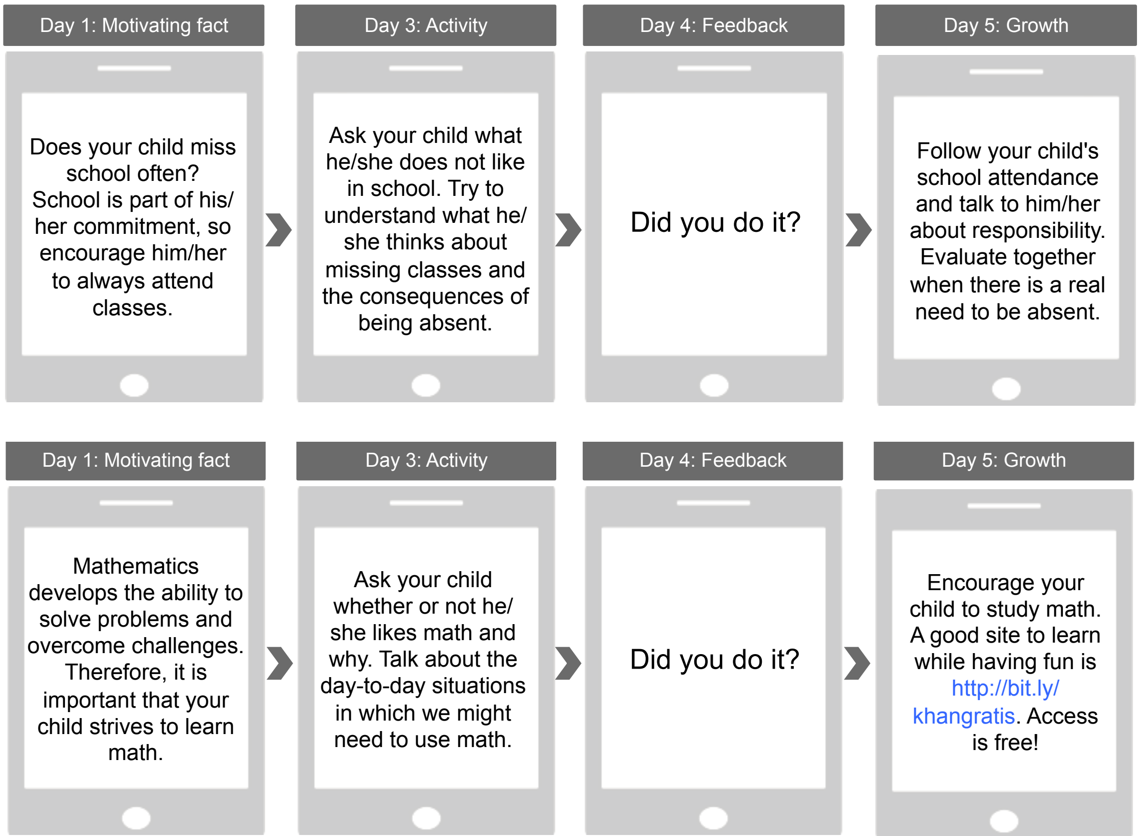
Attendance	Tardiness	Assignment Completion
1. Missed more than 5 classes	**1.** Was late for more than 5 classes	**1.** Did not complete any of the assignments
2. Missed 3 to 5 classes	**2.** Was late 3 to 5 classes	**2.** Completed less than half of the assignments
3. Missed less than 3 classes	**3.**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

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

The figure below shows two examples of the sms sequence sent to parents assigned to the nudge program (described in sections 4.6 and ??). 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 4.6 [37].



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

B Appendix – Survey Instruments

B.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!"

B.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!"

B.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**	**Someti mes**	**Almost always or always**
a.	Help you with homework or schoolwork.	1	2	3	4
b.	Ask if you did you homework or schoolwork	1	2	3	4
c.	Help you to organize the school material, such as books, notebooks and backpack.	1	2	3	4
d.	Incentivize you to not miss school.	1	2	3	4
e.	Incentivize you to not be late for school.	1	2	3	4
f.	Ask you about your grades in tests, activities and classes.	1	2	3	4
g.	Incentivize you to study.	1	2	3	4
h.	Incentivize you to read.	1	2	3	4
i.	Ask you about your day in school.	1	2	3	4
j.	Ask you about your classes.	1	2	3	4
k.	Go to school parent meetings.	1	2	3	4
l.	Talk to your teachers.	1	2	3	4

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

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

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

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

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

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

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

9. If it were **only up to you,** up to which level you would study?	
a. I would have already dropped out of school	1
b. Until finishing the 9º 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 9º 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 9º grade.	1
b. I will finish the 9º 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

C Appendix – Balance and attrition tests

In this section, we present balance and attrition tests. Table C.1 shows descriptive statistics and balance test for the main sample used in the analysis (e.g. Tables 3, 4, 5). Table C.2 presents descriptive statistics and balance test for the theory of change sample. Next, Tables C.3 and C.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 2), in Table C.5 we show results for school transcripts and test scores re-weighting observations by the inverse probability of opting into the program. Finally, Table C.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 C.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 $\sim$ \$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 C.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 $\sim$ \$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 C.3: Selective attrition - survey completion

	(1) Baseline Survey - Parents	(2) Endline Survey - Parents	(3) Endline Survey - Students
Salience	-0.016	0.022	0.016
	[0.020]	[0.024]	[0.016]
Information	-0.008	0.039	0.013
	[0.021]	[0.024]	[0.016]
Control Within Class	-0.006	0.045*	0.020
	[0.020]	[0.023]	[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 C.4: Marginal probability of completing the survey

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

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 C.5: School transcripts and standardized tests - 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***	0.100***	0.038***	0.096**
	[0.006]	[0.032]	[0.013]	[0.046]
Information	0.022***	0.077**	0.031**	0.105**
	[0.007]	[0.032]	[0.013]	[0.046]
Control Within	0.019***	0.081**	0.036***	0.087*
	[0.007]	[0.032]	[0.013]	[0.046]
Control Mean	0.875	0.000	0.938	-0.000
P-value diff. [Info] -[Salience]	0.854	0.141	0.162	0.680
Sample Size	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 C.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 $\sim$ \$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.

D Appendix – Theory of change

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 D.1 shows results for school transcripts and test score; Tables D.2 and D.3 present results for parent's behavior and aspirations, respectively; and Table D.4 describes results for student's behavior. Next, Tables D.5, D.6, D.7, and D.8 show heterogeneous results for boys and gilrs, following the same order: school transcripts and test score, parent's behavior and aspirations, and student's behavior.

Table D.1: School transcripts and test score

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.016***	0.072**	0.030**	0.075
	[0.006]	[0.034]	[0.012]	[0.053]
Information	0.017***	0.058*	0.026**	0.091*
	[0.006]	[0.034]	[0.012]	[0.053]
Control Within	0.016***	0.054*	0.030**	0.068*
	[0.006]	[0.034]	[0.012]	[0.053]
Control Mean	0.889	0.000	0.945	0.000
P-value diff. [Info] -[Salience]	0.634	0.420	0.477	0.510
Sample Size	9539	9539	9539	9539
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes

Note: 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 D.2: Parents' behavior

	(1) Academic activities	(2) Incentives	(3) Talk
Salience	0.064	0.096**	0.122***
	[0.050]	[0.041]	[0.043]
Information	0.092*	0.075*	0.147***
	[0.051]	[0.042]	[0.044]
Control Within	0.073*	0.033*	0.111***
	[0.050]	[0.042]	[0.043]
Control Mean	-0.000	0.000	-0.000
P-value diff. [Info] -[Salience]	0.263	0.382	0.374
Sample Size	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 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 D.3: Parents' aspirations

	(1) Parents' Aspirations College
Salience	0.095***
	[0.036]
Information	0.092**
	[0.036]
Control Within	0.064**
	[0.037]
Control Mean	-0.000
P-value diff. [Info] -[Salience]	0.891
Sample Size	9539
Randomization strata FE	Yes
Student controls	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 D.4: Students' behavior

	(1) Academic activities	(2) Reading activities	(3) Other activities
Salience	0.123**	0.113*	-0.110**
	[0.050]	[0.060]	[0.052]
Information	0.151***	0.116*	-0.108**
	[0.051]	[0.065]	[0.054]
Control Within	0.130***	0.127*	-0.089**
	[0.050]	[0.063]	[0.052]
Control Mean	0.000	-0.000	0.000
P-value diff. [Info] -[Salience]	0.344	0.946	0.933
Sample Size	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 D.5: 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***	0.13***	0.04**	0.10*	0.01*	0.02	0.01	0.04	-0.01	-0.12**	-0.03*	-0.06
	[0.01]	[0.04]	[0.02]	[0.06]	[0.01]	[0.04]	[0.01]	[0.06]	[0.01]	[0.05]	[0.02]	[0.06]
Information	0.02***	0.12***	0.04**	0.13**	0.01**	0.00	0.01	0.05	-0.01	-0.12**	-0.03*	-0.07
	[0.01]	[0.04]	[0.02]	[0.06]	[0.01]	[0.04]	[0.01]	[0.06]	[0.01]	[0.05]	[0.02]	[0.07]
Control Within	0.02***	0.12***	0.04**	0.10	0.01*	-0.01	0.02*	0.04	-0.01	-0.13***	-0.03*	-0.06
	[0.01]	[0.04]	[0.02]	[0.06]	[0.01]	[0.04]	[0.01]	[0.06]	[0.01]	[0.05]	[0.02]	[0.07]
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 D.6: 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**	0.07	0.14***	0.00	0.11*	0.11*	-0.12*	0.04	-0.03
	[0.06]	[0.06]	[0.05]	[0.06]	[0.06]	[0.06]	[0.07]	[0.08]	[0.07]
Information	0.13**	0.05	0.17***	0.05	0.09	0.12**	-0.08	0.03	-0.04
	[0.06]	[0.06]	[0.05]	[0.07]	[0.06]	[0.06]	[0.08]	[0.08]	[0.07]
Control Within	0.16***	0.06	0.15***	-0.01	0.00	0.07	-0.17**	-0.06	-0.08
	[0.06]	[0.06]	[0.05]	[0.07]	[0.06]	[0.06]	[0.07]	[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 D.7: Parents' aspirations - boys and girls

	Boys	Girls	Diff. (Girls)-(Boys)
	(1) Parents' Aspirations College	(2) Parents' Aspirations College	Parents' Aspirations College
Salience	0.12**	0.08	-0.04
	[0.06]	[0.05]	[0.08]
Information	0.10*	0.09*	-0.02
	[0.06]	[0.05]	[0.08]
Control Within	0.10*	0.03	-0.07
	[0.06]	[0.05]	[0.08]
Control Mean	-0.09	0.09	
P-value diff. [Info] -[Salience]	0.76	0.79	
Sample Size	4654	4885	
Randomization strata FE	Yes	Yes	
Student controls	Yes	Yes	

Note: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 D.8: 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***	0.17**	-0.09	0.06	0.06	-0.13**	-0.13*	-0.11	-0.04
	[0.06]	[0.07]	[0.06]	[0.07]	[0.07]	[0.07]	[0.07]	[0.08]	[0.08]
Information	0.18***	0.15**	-0.13*	0.12*	0.08	-0.09	-0.06	-0.07	0.04
	[0.05]	[0.07]	[0.07]	[0.07]	[0.08]	[0.07]	[0.07]	[0.08]	[0.08]
Control Within	0.19***	0.17**	-0.07	0.08	0.09	-0.10	-0.11	-0.07	-0.04
	[0.05]	[0.07]	[0.06]	[0.07]	[0.08]	[0.07]	[0.07]	[0.08]	[0.08]
Control Mean	-0.14	-0.07	-0.18	0.14	0.08	0.18			
P-value diff. [Info] -[Salience]	0.81	0.73	0.38	0.13	0.65	0.26			
Sample Size	4654	4654	4654	4885	4885	4885			
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes			
Student controls	Yes	Yes	Yes	Yes	Yes	Yes			

Note: 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).

E Appendix – Mechanisms

In this section, we present extra tables on the mechanisms to complement the analysis of section 5. In section 5, Tables 8, 12, and 13 describe heterogeneity analysis by parents' baseline beliefs with respect to their child's GPA. Results are showed for parent's endline accuracy, students' transcripts and test scores, and parents' behavior. In this section, we replicate these results for parents baseline beliefs with respect to their child's attendance, instead of GPA, as showed by Tables E.5, E.2, and Table E.3. Moreover, Table 9 of section 5 shows heterogeneous analysis by students' baseline attendance, and in this section we show a similar analysis, but for students' baseline GPA instead of attendance (Table E.4). Finally, Table E.5 replicates the heterogeneous analysis by parents' endline accuracy showed in section 5 (Table 8) for parents' accuracy on students' attendance, instead of GPA.

Table E.1: Heterogeneity by parents' baseline beliefs wrt their child's attendance - parents' endline accuracy

	Pessimistic Parents (10.2%)		Accurate parents (35.9%)		Optimistic parents (53.8%)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Accuracy Math Attendance	Accuracy Math GPA	Accuracy Math Attendance	Accuracy Math GPA	Accuracy Math Attendance	Accuracy Math GPA
Salience	-0.08	-0.12	-0.02	0.09	-0.03	0.08
	[0.12]	[0.13]	[0.07]	[0.06]	[0.05]	[0.05]
Information	-0.08	-0.14	-0.07	0.05	-0.04	0.02
	[0.12]	[0.14]	[0.06]	[0.06]	[0.05]	[0.05]
Control Within	-0.09	-0.27**	0.04	0.02	-0.01	0.05
	[0.13]	[0.12]	[0.06]	[0.06]	[0.05]	[0.05]
Control Mean	0.29	0.38	0.30	0.24	0.22	0.24
P-value diff. [Info] -[Salience]	1.00	0.88	0.35	0.42	0.67	0.20
Sample Size	171	171	600	600	898	898
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 to give their best estimate of how many times their child misses school in a period of three weeks. 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 attendance (missed 0, 1-2; 3-5; more than 5). Administrative data register data on attendance on a quarterly basis (period of $\sim$ 9 weeks). Administrative data was divided by 3 to validate parents' answers. 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 E.2: Heterogeneity by parents' baseline beliefs wrt their child's attendance - transcripts and test score

	Pessimistic Parents (10.4%)				Accurate parents (35.3%)				Optimistic parents (54.2%)			
	(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.)	(9) Math Attendance (p.p.)	(10) Math GPA (std.)	(11) Promotion Rate (p.p.)	(12) Math Standardized Test (std.)
Salience	0.02	0.21*	0.03	0.11	0.02**	0.06	0.06**	0.11	0.04***	0.14**	0.03	0.08
	[0.02]	[0.11]	[0.03]	[0.15]	[0.01]	[0.07]	[0.02]	[0.10]	[0.01]	[0.06]	[0.02]	[0.08]
Information	0.03	0.16	0.02	0.16	0.02*	0.02	0.06***	0.12	0.03***	0.16***	0.03	0.08
	[0.02]	[0.12]	[0.03]	[0.15]	[0.01]	[0.07]	[0.02]	[0.10]	[0.01]	[0.06]	[0.02]	[0.07]
Control Within	0.00	0.18	0.01	-0.02	0.03**	0.05	0.06**	0.06	0.03***	0.11**	0.02	0.04
	[0.02]	[0.11]	[0.03]	[0.15]	[0.01]	[0.07]	[0.02]	[0.10]	[0.01]	[0.06]	[0.02]	[0.07]
Control Mean	0.89	-0.01	0.96	0.00	0.87	0.07	0.92	0.02	0.84	-0.05	0.94	-0.01
P-value diff. [Info] -[Salience]	0.40	0.64	0.64	0.63	0.82	0.40	0.54	0.78	0.27	0.70	0.80	0.96
Sample Size	399	399	399	399	1350	1350	1350	1350	2073	2073	2073	2073
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	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 to give their best estimate of how many times their child misses school in a period of three weeks. 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 attendance (missed 0, 1-2; 3-5; more than 5). Administrative data register data on attendance on a quarterly basis (period of $\sim$ 9 weeks). Administrative data was divided by 3 to validate parents' answers.

Table E.3: Heterogeneity by parents' baseline beliefs wrt their child's attendance - parents' behavior

	Pessimistic Parents (10.4%)			Accurate parents (35.3%)			Optimistic parents (54.2%)		
	(1) Academic Activities	(2) Incentives	(3) Talk	(4) Academic	(5) Incentives Academic	(6) Talk	(7) Academic Academic	(8) Incentives	(9) Talk
Salience	0.16	0.10	0.12	0.06	-0.00	0.17	0.05	0.03	0.03
	[0.20]	[0.21]	[0.19]	[0.11]	[0.11]	[0.11]	[0.10]	[0.10]	[0.09]
Information	0.27	0.03	0.08	0.15	0.18*	0.19*	0.06	-0.00	0.06
	[0.21]	[0.22]	[0.20]	[0.11]	[0.10]	[0.11]	[0.10]	[0.10]	[0.09]
Control Within	0.42**	-0.14	0.36*	0.16	-0.00	0.17*	-0.01	-0.07	0.01
	[0.20]	[0.23]	[0.20]	[0.11]	[0.10]	[0.10]	[0.10]	[0.10]	[0.09]
Control Mean	-0.12	-0.08	0.00	0.02	-0.01	-0.00	-0.00	-0.04	0.04
P-value diff. [Info] -[Salience]	0.47	0.68	0.79	0.25	0.03	0.78	0.90	0.65	0.65
Sample Size	329	324	329	1137	1140	1139	1700	1699	1687
Randomization strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	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 to give their best estimate of how many times their child misses school in a period of three weeks. 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 attendance (missed 0, 1-2; 3-5; more than 5). Administrative data register data on attendance on a quarterly basis (period of $\sim$ 9 weeks). Administrative data was divided by 3 to validate parents' answers. Students were asked at the endline survey about their parent's behavior, where they had to state how often their parents engage in certain activities (never, almost never, sometimes, almost always). Out of the 12 questions, factor analysis was used to create 3 variables of parents 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: Heterogeneity by student's baseline GPA

	Low-performing (≤ Median - 54.7%)				High-performing (>Median - 45.3%)			
	(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.)
Salience	0.016**	0.119***	0.050***	0.107**	0.026***	0.069*	0.008	0.090
	[0.007]	[0.040]	[0.019]	[0.050]	[0.007]	[0.038]	[0.007]	[0.057]
Information	0.020***	0.096**	0.040**	0.119**	0.023***	0.049	0.011	0.091
	[0.007]	[0.040]	[0.019]	[0.051]	[0.007]	[0.038]	[0.007]	[0.057]
Control Within	0.016**	0.100**	0.044**	0.086*	0.020***	0.044	0.011*	0.087
	[0.007]	[0.039]	[0.019]	[0.050]	[0.007]	[0.038]	[0.007]	[0.057]
Control Mean	0.86	-0.57	0.90	-0.36	0.89	0.65	0.98	0.42
P-value diff. [Info] -[Salience]	0.28	0.24	0.18	0.68	0.28	0.36	0.38	0.96
Sample Size	6879	6879	6879	6879	5698	5698	5698	5698
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 GPA below or equal to the class median were determined as low-performing, and students with baseline GPA above the median were determined as high-performing for the purposes of this analysis.

Table E.5: Heterogeneity by parents' baseline accuracy wrt attendance

	Pessimistic Parents (10.2%)		Accurate parents (35.9%)		Optimistic parents (53.8%)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Accuracy Math Attendance	Accuracy Math GPA	Accuracy Math Attendance	Accuracy Math GPA	Accuracy Math Attendance	Accuracy Math GPA
Salience	-0.08	-0.12	-0.02	0.09	-0.03	0.08
	[0.12]	[0.13]	[0.07]	[0.06]	[0.05]	[0.05]
Information	-0.08	-0.14	-0.07	0.05	-0.04	0.02
	[0.12]	[0.14]	[0.06]	[0.06]	[0.05]	[0.05]
Control Within	-0.09	-0.27**	0.04	0.02	-0.01	0.05
	[0.13]	[0.12]	[0.06]	[0.06]	[0.05]	[0.05]
Control Mean	0.29	0.38	0.30	0.24	0.22	0.24
P-value diff. [Info] -[Salience]	1.00	0.88	0.35	0.42	0.67	0.20
Sample Size	171	171	600	600	898	898
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 to give their best estimate of how many times their child misses school in a period of three weeks. Data was then crossed with administrative records and parents who estimated exactly right were determined as more accurate and those who estimated wrong were determined as less accurate. 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. 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.

F Appendix – Results platform scores

As described in section 3, a web-platform was created specifically to this project. Math teachers from treatment schools were oriented to fill in the platform every week with that week's dimension of students' behavior: attendance, lateness or assignment completion, for a duration of 18 weeks. Teachers filled information regarding student behavior on each dimension considering the past three weeks[38]. The system required teacher to fill in information for all students. In this section we investigate the effect of the program on the platform scores.

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 lateness, to investigate the effect on punctuality. For each week, we estimated the following model:

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

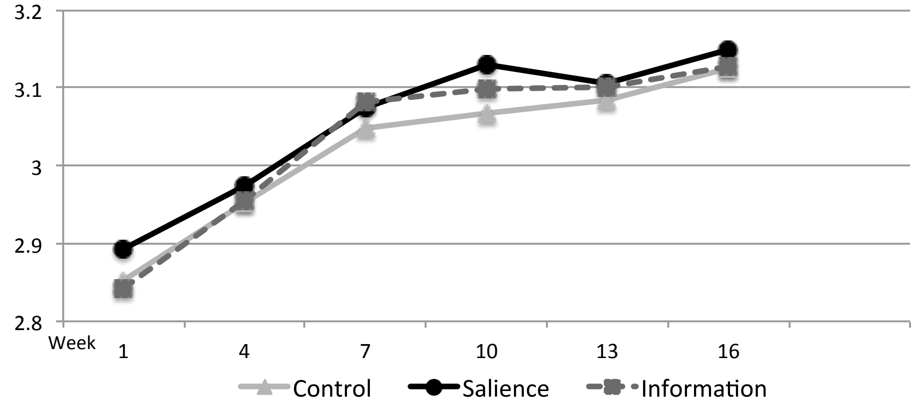
where $Y_{i,c,s}$ denotes the weekly score of each dimension for student i in classroom c of stratum s, the within-class control stand for the reference category (omitted indicator variable), $X_{k,i,c,s}$ is a matrix of student's covariates, θ_s are randomization stratum FE, and $\epsilon_{i,c,s}$ is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

Next, the platform scores of each dimension–attendance, lateness and assignment completion– were averaged and we estimated the same model for the averaged score of each dimension, as showed in Table F.1. The scores 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.

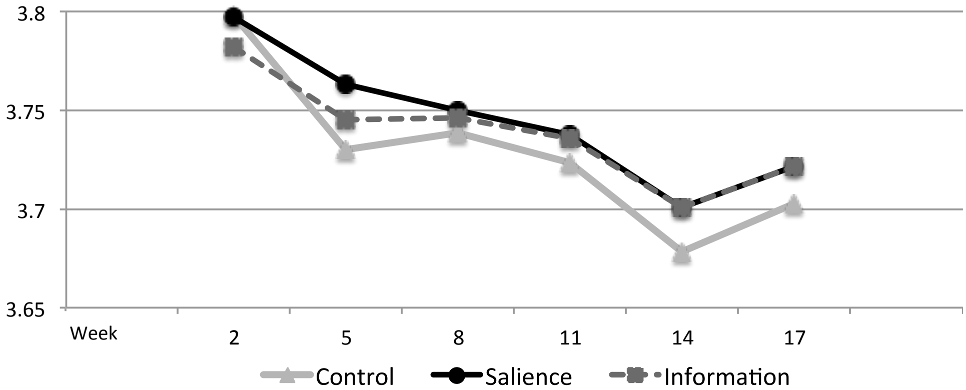
[38] Students have around 6 class of Mathematics per week.

Figure F.1: Weekly effect on platform scores

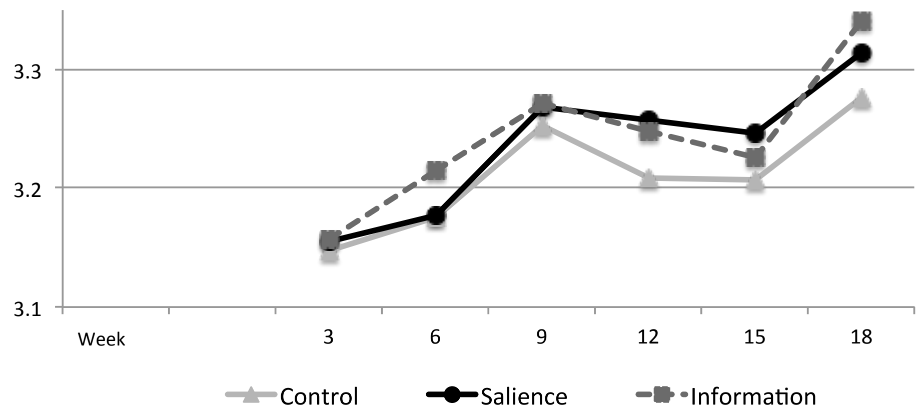
Panel A: Weekly effect on attendance



Panel B: Weekly effect on punctuality



Panel C: Weekly effect on assignment completion



Note: For each outcome and each week, the following equation was estimated: $Y_{i,c,s} = \alpha + \beta_1 Salience_{i,c,s} + \beta_2 Info_{i,c,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$, where $Y_{i,c,s}$ denotes the weekly score 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 lateness, to investigate the effect on punctuality.

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

	(1) Attendance (std.)	(2) On Time (std.)	(3) Assignment Completion (std.)
Salience	0.046**	0.028	0.027
	[0.022]	[0.020]	[0.019]
Information	0.025	0.022	0.044**
	[0.026]	[0.022]	[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, lateness 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: $Y_{i,c,s} = \alpha + \beta_1 Salience_{i,c,s} + \beta_2 Info_{i,c,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$, where $Y_{i,c,s}$ denotes the 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, $\epsilon_{i,c,s}$ is an error term, clustered at the classroom level, and $\%Salience = \beta_1/\beta_2$.

G Appendix – Spillover

This section presents results on spillover within classroom (peers) and within students (discipline), by comparing the control group of treated classrooms with the pure control group. For each outcome of interest, we estimate the the same model estimated on section 4 (equation 1)[39] but we now show in the table results for the control group of the treated classrooms (and we omit coefficients from the treatment groups).

Table G.1 shows results for the spillover within classroom on students' transcripts and test score, and Table G.2 present results for spillover within student on students' transcripts and test score and parents' endline accuracy.

Table G.1: Spillover within classroom

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Control Within Class	0.018***	0.070**	0.030**	0.085*
	[0.006]	[0.031]	[0.012]	[0.047]
Sample Size	12577	12577	12577	12577
Randomization strata FE	Yes	Yes	Yes	Yes
Student controls	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: $Y_{i,c,s} = \alpha + \beta_1 Salience_{i,c,s} + \beta_2 Info_{i,c,s} + \beta_3 Control_{i,c=treated,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$, 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. Only coefficients for the control group is displayed in the table (β_3), coefficients for salience and information were omitted ($\beta_1 and \beta_2$). Significance levels are denoted by * if p<0.1, ** if p<0.05 and *** if p<0.01.

[39] $Y_{i,c,s} = \alpha + \beta_1 Salience_{i,c,s} + \beta_2 Info_{i,c,s} + \beta_3 Control_{i,c=treated,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$

Table G.2: Spillover within student

	School transcript and test score			Parent's accuracy	
	(1) Portuguese Attendance (p.p.)	(2) Portuguese GPA (std.)	(3) Portuguese Standardized Test (std.)	(4) Accuracy Portuguese Attendance (p.p.)	(5) Accuracy Portuguese GPA (p.p.)
Salience	0.007	0.066*	0.032	0.009	-0.005
	[0.005]	[0.036]	[0.043]	[0.029]	[0.031]
Information	0.007	0.053	0.047	0.027	0.051*
	[0.005]	[0.036]	[0.043]	[0.029]	[0.031]
Control Within	0.004	0.054	0.026	0.035	-0.021
	[0.005]	[0.035]	[0.043]	[0.029]	[0.031]
Sample Size	12577	12577	12577	3069	3069
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: $Y_{i,c,s} = \alpha + \beta_1 Salience_{i,c,s} + \beta_2 Info_{i,c,s} + \beta_3 Control_{i,c=treated,s} + \sum \gamma_k X_{k,i,c,s} + \theta_s + \varepsilon_{i,c,s}$, 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.

H Appendix – Robustness: equalizing the number of times teacher filled the platform by subsample

As showed in Figure H.1, the number of times the teacher filled the platform over the 18 weeks was not equal across the different subsamples. To test if this difference might be somehow affecting the results, we analyze a separate sample, where we equalize the number of times teachers fill the platform by subsample. We do so by eliminating 7 classrooms from the salience only sample, where teachers had filled the platform all the 18 weeks; and 27 classrooms from the subsample containing all treatments (25% salience, 25% ind. info; 25% relative info, 25% control) where teacher participation was low (teachers filled 3 times or less the platform). In this new sample, the average number of times the teacher fill the platform is equal for all subsamples. 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 5). Results are showed in tables H.1 and H.2.

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

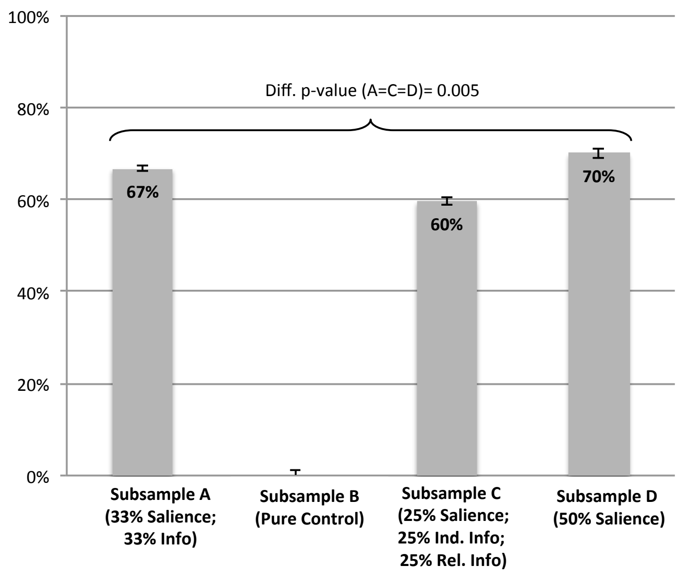


Table H.1: Robustness school transcript and test score - equalizing SMS received by subsample

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.019***	0.085***	0.030***	0.108**
	[0.006]	[0.032]	[0.011]	[0.045]
Information	0.019***	0.070**	0.026**	0.110**
	[0.006]	[0.032]	[0.011]	[0.046]
Control Within	0.016***	0.072**	0.028**	0.102**
	[0.006]	[0.031]	[0.011]	[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 subsample 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 subsample

	(1) Math Attendance (p.p.)	(2) Math GPA (std.)	(3) Promotion Rate (p.p.)	(4) Math Standardized Test (std.)
Salience	0.016**	0.068**	0.027**	0.110**
	[0.006]	[0.033]	[0.011]	[0.047]
Information	0.019***	0.070**	0.026**	0.110**
	[0.006]	[0.032]	[0.011]	[0.046]
Salience Only	0.002	0.030	0.002	-0.004
	[0.004]	[0.029]	[0.009]	[0.044]
Control Within	0.013**	0.062*	0.026**	0.103**
	[0.006]	[0.032]	[0.011]	[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 subsample 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.