Reducing parent-school information gaps and improving education outcomes: Evidence from high frequency text messaging in Chile

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Schools around the world routinely collect high frequency data on student outcomes like absenteeism, grades, and student conduct, all strong predictors of grade repetition and school dropout. Yet parents rarely have access to these data in real time. We test whether a program of sending these data to parents using high frequency text messaging improves education outcomes in a sample of 1,500 students in eight elementary schools in a low-income region of Chile. After four months, treated students had significantly higher math grades, improved attendance, a lower prevalence of bad behaviors, and were less likely to fail the grade at the end of the year. We find some evidence of positive spillovers from having more students (randomly) treated in the same classroom. Treatment narrowed parent-school information gaps and treated parents reported a higher willingness to pay for continuing the messaging program at follow up. Our results suggest that poor communication between parents and schools may be an important barrier to improving educational attainment. Using low-cost technology to deliver existing data on student grades, attendance and behavior at higher frequency could significantly raise human capital attainment down the line.

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1. Introduction

Grade retention and early dropout are two of the biggest challenges facing education systems in middle-income countries today. In Latin America, only 46% of students graduate from secondary school on time and only 53% of young people aged 20 to 24 complete their high school education (UNFPA and ECLAC 2011). These outcomes contribute to persistent education gaps between families at different points in the income distribution.

Researchers have identified absenteeism, poor conduct, and failing grades as important early warning signals for grade repetition and dropout in later years (Manacorda, 2012; Wedenoja 2016). While schools around the world regularly record these student outcomes, families may not have immediate access to this information. At best, schools communicate these data to parents using infrequent "report cards" that may often never reach home. Could the data inputs already generated by schools be put to better use by parents and families? If families have better access to information about these important predictors of grade retention and dropout, will school outcomes improve, and if so, how?

In this paper, we ask whether improving the frequency and quality of communication between parents and schools can help families improve school outcomes. We conduct a randomized experiment in a sample of low-income Chilean families to evaluate the effects of digitizing existing school records on attendance, grades, and behavior and communicating this information to parents each week through cellphone text messages (SMS messages). The program was called *Papas al Dia*, or, "*Parents up to date*". Our experimental sample includes almost 1,500 children enrolled in grades 4 through 8 in eight schools in a metropolitan area in Chile. We collected administrative and survey data at baseline and at a five month follow up to assess the impacts of *Papas al Dia*.

Our intervention has several distinguishing features. First, the information treatment continues for one and a half years, and we observe many outcomes at monthly level throughout the period. In this version of the paper, we focus on outcomes measured at the end of the first year of the experiment (after five months of treatment), but we will eventually be able to observe outcomes after the second year of treatment ends, to analyze persistence dynamics. It is important to be able to measure short, medium, and longer run outcomes because parents may take time to learn how to use the SMS messages, they may become fatigued by the program, or they may develop new habits that make the program obsolete. Few school intervention experiments are able to study the persistent effects of interventions.

Second, by design of the experiment, we can examine spillover effects within classrooms. These are important to understand and quantify for reasons of scaling up an intervention. Third, we ask parents about how much they value the technology, after some exposure. We can measure, using self-reported willingness to pay, whether parents who are treated value the frequent communication with schools differently than control parents, and whether this willingness to pay is sensitive to a student's place in the baseline distribution of characteristics.

We start by showing fairly sizeable gaps in parent information about student grades and school reports of actual grades using baseline survey and administrative data. About one in four parents was unable to report correct information about their child's grades at baseline. Similar information gaps have been found in settings as diverse as the USA (Bergman 2016) and Malawi (Dizon-Ross 2016). As one might expect, students with lower actual performance in school have parents who misreport grades at higher rates. Narrowing these gaps is the initial target of our SMS treatment.

The SMS treatment had immediate impacts on grades and on grade progression. After the first four months of treatment (at the end of the first school year), average math grades and cumulative math GPA rise by 0.08 standard deviations for SMS treatment students relative to control students.[2] The probability of earning a passing grade in math increases by 2.8 percentage points (relative to a mean of 90%). Exposure to the SMS treatment increases the chances of attending school for more than 85% of the time by 6.6 percentage points. The 85% cutoff is one of the necessary conditions for grade progression. The share of students reported to have extremely bad behavior (e.g. bullying, or physical/verbal violence) falls by a substantial 1.25 percentage points, or 20% relative to the mean rate of bad behavior. And exposure to treatment increases the probability that a student passes the grade at the end of the year by 2.9 percentage points, virtually eliminating grade failure among marginal students. Across the board, the impacts of the treatment are positive and persist to the end of the first year of treatment. Because only around 65% of SMS messages sent were actually received these intent to treat estimates underestimate the impact of high frequency communication between parents and schools, although are reasonable estimates of the impacts of a scaled up version of the program.

In addition to randomizing the SMS treatment assignment at the individual level, we randomized the share of kids treated at the classroom level. Our hypothesis was that classroom-level spillovers could be important for several reasons. Parents might share information and affect social norms about how much they need to be involved in helping or monitoring kids in school. Or, student peer effects could be

[2] The effects on math grades are somewhat smaller than grade effects of other types of interventions in the literature. For example: Bettinger (2012) generates an improvement of 0.15 standard deviations on math test scores with a $15 incentive in grades 3 through 6 in Ohio; Kremer, Miguel and Thornton (2009) generate a 0.13 standard deviation impact on test scores by offering expensive scholarships for two years of high school among Grade 6 girls in Kenya; while Bergman (2016), uses a similar program to ours but with much more expensive ways of communicating with parents in low income US schools, and generates a 0.19 standard deviation increase in test scores.

important, for example: the utility of skipping school may depend on how many of your friends also skip school. To test for spillovers, classes were randomized into having a high (75%) or low (25%) share of consenters treated. We find evidence of positive classroom level spillovers for grade and attendance outcomes, and for the probability of passing the grade, but not for behavior outcomes.

Papás al Día is a relatively low-touch intervention –we did not teach parents how to interpret or use the information we provided. We wanted to understand which students were most affected by the frequent contact with schools via text message. In particular, because dropout is more likely later on in high school, we wanted to know whether our intervention had large impacts on those students most at risk for dropping out. To make some headway on this, we predict the probability of dropping out in the next year using administrative data on grades and attendance. We examine heterogeneity in treatment effect size across the distribution of these predicted probabilities of dropout. We show that grades, grade progression, and behavior outcomes improve the most for those in the middle of the predicted probability of dropout distribution. Relative to the control group, the weakest treated students see little improvement in outcomes while the strongest students have no room to improve. Only students with elevated, but not extreme, risk of dropping out respond to the SMS program by changing their behaviors.

To gain further insight into how *Papas al Dia* worked, we combine our rich administrative data collected from each school with before and after survey data from parents and students. We show that the frequency of contact matters: the positive effects on school outcomes were larger when treated parents were sent more total messages. Importantly, we show that the SMS treatment shrunk information gaps between parents and schools, with parent reports closer to administrative data on grades after four months. Parents with the largest information gaps at baseline "correct" the most, relative to control group parents, although with a relatively small sample, this effect is imprecisely estimated. In ongoing work, we continue to explore how parental involvement at home, and at school, changed in the wake of the treatment, using parent and student survey data at baseline and follow up. Finally, all parents report a willingness to pay (WTP) for the SMS program.[3] We show that treated parents have more inelastic preferences: more of them are WTP at every (randomized) price after the end of the first year of treatment. We interpret this as evidence that parents learned about their value of the program during this first year, and so were more willing to continue paying for the service after five months.

We make three contributions to the literature on how to improve human capital outcomes. First, we show that while parents do not have the same information about their kids as schools do, these information gaps

[3] This result echoes Burztyn and Coffman (2013), who show that Brazilian parents report being willing to pay for receiving regular updates on their child's absenteeism.

can be reduced, and grade, attendance and behavior outcomes improved, with a simple and ongoing transfer of relevant data to parents. Our results connect with a growing economics literature that identifies a lack of correct and timely information as one of the critical constraints on good decision-making.[4] Having the right information at the right time is especially important for children, who must rely on adult caregivers to act as their agents before they are developmentally capable of making good choices about human capital or health investments. *Papas al Dia* is one simple intervention that can help parents be more effective principals, with potentially long term positive impacts on educational attainment for children from low-income backgrounds. Our results also suggest that once parents understand the value of such a program, they may be willing to pay for at least part of the costs of improving parent-school communication.

Second, over the last few decades, while efforts to improve school access around the world have been successful, improving school quality has been more elusive. Digitizing existing data that is already collected by schools around the world on an ongoing basis, and providing these data to parents at high frequency, may be an important and relatively inexpensive way for improving grades and attendance outcomes. Closing information gaps between parents and schools may be a feasible tool for improving the performance of existing school inputs, leading to higher quality outcomes.

Third, relative to other types of parenting programs, our intervention is relatively low cost and would likely be more sustainable and amenable to scale up in developing country settings outside of Chile. Recent successful parenting programs have targeted parenting skills, and require more contact time between parents and schools (e.g. Avvisati, Gurgand, Guyon and Maurin 2014, Banerji, Berry and Shotland 2014). Consequently, these programs tend to be costly, and difficult to scale. Our work is more closely related to Bergman (2016) who evaluates a program of improved parent-school communication in low-income communities in Los Angeles. Like Bergman's work, *Papas al Dia* leverages existing data collected by schools. But in contrast, our SMS program is implemented under conditions that would most likely prevail in a scaled-up version of the program, using a lower cost means of communicating with parents – automated SMS messages rather than personalized emails and phone calls.

Our paper starts with a description of the experimental setting and we document the extent of parent-school information gaps at baseline. Section 3 describes the design of our experiment and section 4

[4] See, for example, Nguyen (2008), Jensen (2010), Oreopolous and Dunn (2013), Dinkelman and Martinez A. (2014) for evidence on how education outcomes improve after parents or students are informed about the returns to, or costs of, educational investments. Bettinger et al (2012) and Fryer (forthcoming) are examples in which information alone was insufficient for improving educational attainment. Dizon-Ross (2016) studies a once-off information intervention with parents in Malawi, showing that the intervention improves what parents know about their children and causes family educational investments to adjust to match newly revealed abilities of each child.

describes the data we collect and use in our analysis. Section 5 discusses our estimation strategy and analyses the internal validity of our experiment. Sections 6 and 7 present main results and explore some of the mechanisms for the impacts we find, before section 8 concludes.

2. Experimental setting: School performance, dropout risk and parent-school information gaps

In Latin America, 37% of adolescents (15 to 19 years) drop out of school without completing Grade 12. About half of these dropouts leave early, before completing primary school (first 8 grades), although in many places, a significant share of dropout happens sometime in Grade 9, the first year of high school. Dropout is also significantly concentrated among the lower income quintiles in these middle income countries. In Chile, only 65% of students in the lowest income quintile complete high school.

Figure 1 uses the administrative data at baseline for the universe of students enrolled in Chilean schools to plot grade repetition rates by grade level in 2013. Figure 2 plots the dropout rate between 2013 and 2014, by grade level in 2013. While grade repetition and dropout are clearly outcomes of concern in lower grades, the figures show that these rates increase substantially in high school. In particular, the transition from 8th grade to high school appears to be a point at which students are at high risk of repeating a grade, or leaving, the school system. In our experiment, we focus on students in the last four grades of primary school, Grades 4 through 8. We target information at parents during the years when attendance and grades start to matter, but before the risk of grade repetition or dropout are elevated.

Researchers have shown that attendance and grades in school are key factors affecting the risk of grade repetition, and dropout, at higher grades (e.g. Manacorda 2012, Wedenoja 2016). In Chile, grade progression is largely a function of meeting minimum attendance and GPA requirements.[5] As a result, there are strong correlations between attendance and subject grades, and the outcomes of grade repetition, and dropout. We show this correlation in Appendix Table 1, using administrative data on all of the students enrolled in our experimental schools in the year before the intervention. Even conditional on age and gender controls, and grade level and school fixed effects, lower attendance and lower grades are associated with a higher risk of failing the grade and a higher risk of dropping out of school.[6]

[5] Twelve years of schooling are mandatory in Chile: eight of primary school and four of secondary school. In most grades, students must attend for at least 85% of school days in a school year, and must meet grade requirements. A passing grade in any subject is 4.0. Grades range from 1 to 7 in units of 0.1. Students pass a grade if they pass all subjects; if they fail one subject and maintain an average grade of 4.5 over the remaining subjects; or if they fail two subjects and maintain an average grade above 5.0 in the remaining subjects.

[6] These data are from the Ministry of Education and cover the universe of enrolled students in Chile. We extract data for all students enrolled in our experimental schools in 2013.

A starting point for our analysis is the idea that parents do not have good information about what their children are doing at school. Gaps in information between parents and schools (or, "misbeliefs"), have been identified in settings as diverse as the US (Bergman 2016) and Malawi (Dizon-Ross 2016). In general, all parents tend to over-report child performance in school, and parents with less education systematically have worse information about their child's performance is like in school. In the Chilean context, there are similar types of information gaps between parents and schools that vary with the student's actual grade.

In Figures 3 and 4, we plot parent reports of child grades from our baseline survey of experimental parents against school-level data on actual child grades. Figure 3 plots the parent's report of the child's grade at the end of 2013 (y-axis) against the child's actual grade in 2013 (x-axis), and includes the 45 degree line. Figure 4 plots the share of parents who misreport the child's grade against the child's actual grade at baseline. We define a misreported grade as one that deviates more than 0.5 points (above or below) from the actual grade. We cannot tell whether parents purposely misreport grades (e.g. because of some type of social desirability bias), or whether they reporting their actual, but inaccurate, beliefs about grades.

Figure 3 shows how parents make both positive and negative mistakes in reporting their child's end of year grade. However, there is a larger mass of points that lie above the 45 degree line, implying that parents shade upwards. Data points also become more dispersed around the line as the child's grade falls, suggesting that parents are making larger mistakes with weaker performers. Figure 4 shows this in another way: kids who have lower grades at baseline are also more likely to have parents who do not know what their grades are at baseline. This pattern shows up among parents who respond to our survey at baseline (the solid line in Figure 4) and in the entire sample (the broken line in Figure 4), if we make the strong assumption that all parents who do not respond to our survey would have misreported their child's grade.[7] In our experiment, we aim to narrow, or close, information gaps between parents and schools using high frequency, low cost methods of contact, and affect education outcomes for children.

3. Experimental design: *Papás al Día*

In early 2014, we worked with education leaders in two deprived municipalities of Santiago de Chile to recruit schools to join our study, *Papás al Día,* or *Parents up-to-date*. Eight school principals consented to work with the program.

[7] The gap between the two graphs in Figure 4 indicates the extent of survey non-response among parents. It is reassuring that the non-response rate is fairly constant by student grades at baseline.

i. Intervention

The experiment offered each participating parent the chance to receive high frequency information about their selected child via text message (SMS messages).[8] The specific information covered attendance, behavior and mathematics test scores of their child. In addition to the information SMS messages, parents of both treatment and control groups received general SMS messages about school meetings, holidays and other general school matters throughout the year.

Once the intervention began, treated parents received weekly messages on attendance, and bimonthly messages on behavior and recent math test scores. For attendance information, we told parents how many days out of the last week (usually five days) the child was in school. For behavior information, we provided parents with the number of positive, neutral and negative behaviors teachers' recorded in the class notebook over the prior month. For math scores, we provided the three most recent test scores of the child, the average of these scores, and the class average score for the same tests. Hence, parents learned information about their own child, as well as how their child performed relative to the class mean. Our research team collected data for attendance, grade, and behavior from school administrative records and entered these data into our digital platform.[9] The platform automated message sending each week. Appendix 1 provides a script of each type of message sent to parents.

ii. Sample, randomization and timeline

In the first part of the 2014 school year, during a series of school meetings, we invited parents of all students (2,720 students) in grades 4 to 8 (85 classes) of the eight participating schools to join the experiment. Over 50 percent of parents (1,447 students) signed consent. Consent rates by grade level were roughly similar (Appendix Table A2.1). Younger students, those new to the school, and those with better baseline attendance and grades were somewhat more likely to consent (Appendix Table A.2).

We allocated students to the SMS treatment in two steps. First, we stratified by school-grade level and randomly allocated classes (sections) to receive a high or low share of SMS treatment. In classrooms allocated to the high share of SMS treatment (HIGH), 75% of consented students in the class were treated. In classrooms allocated to the low share of SMS treatment (LOW), 25% of consented students in the class were treated. Within each class, we randomized consenting students into treatment or control status, according to the HIGH/LOW shares allocated in the first step randomization.

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[8] Siblings were not targeted in this experiment.

[9] Behavior data were difficult to collect. In Chile, each class has a notebook in which teachers can make comments about particularly good or bad behaviors of specific students. For example, "Samuel concentrated well in reading", or "Taryn hit her friend during math class". We developed a system for categorizing behavior "notes" as positive, negative, or very bad, and then implemented these definitions in all classes.

The randomization resulted in 37 classes (with 634 students) being assigned to a higher share of treatment and 48 classes (813 students) to a lower share of treatment. After the individual level randomization, 710 students (448 in high share classes and 222 in low share classes) were assigned to receive the SMS treatment and 737 students (146 in high share classes and 591 in low share classes) were controls.

Table A2.1 in Appendix 2 shows the timeline for the intervention. The Chilean school year runs from March to December, with two weeks of winter vacation in July. A first welcome message was sent to all participants (consenting parents in treatment and control groups) in 7 out of 8 schools on May 23, 2014. Attendance SMS messages started on June 13th 2014; behavior SMS messages around July 9th 2014; and math test score SMS messages around July 14th 2014. The 8th school was incorporated into the experiment slightly later. The implementation milestones for this school were as follows: July 28th 2014 (welcome message), August 1st 2014 (first attendance message), August 12th 2014 (first behavior message), and August 11th 2014 (first math grade message). Because winter vacations are taken in July, differential timing of the start of the intervention for the 8th school is of little consequence. Our analysis takes this differential start time into account where necessary.[10]

The intervention continued for a second year. From April 2015 to December 2015, we continued to send SMS messages to treated parents in a retained sample of students. The retained sample included all participating students in our original sample who were enrolled in grades 5 through 8 in our schools. Students who left our sample were those who graduated from grade 8 and continued to grade 9 in the same or other schools, those who repeated grade 4, or those who left the school entirely. For now, we focus our attention on outcomes recorded up to the end of 2014.

iii. Implementation

All SMS messages were sent as planned. However, not all SMS sent were delivered or received. Several factors contributed to message failure. A message was more likely to fail if the network was very busy, if there was some technical problem with the network, if parents had turned off their phones or if they

[10] A few months into the intervention, in late August 2014, we also distributed a training DVD to a subset of treated parents with specific guidance about how to use the school-provided data. We worked with educational psychologists at Arizona State University to adapt DVD materials from their successful parenting interventions delivered to low-income schools in the US (Lim, Stormshak and Dishion, 2005). The video is available from the authors upon request and will be online soon. Stratifying by school-grade level we randomly allocated classes to receive or not receive the informational DVDs – hence, DVD treatment is orthogonal to the HIGH/LOW share treatment. We find no additional impacts of the DVD randomization. This is likely related to very low compliance rates. Of the 382 students randomized to receive the DVD, we effectively gave the DVD to 375 guardians (98% of the randomized sample). Sixty five percent of the parents received the DVD on the first attempt and the remainder at a second attempt. We verified delivery and take-up of the treatment through a phone survey that reached 76 percent of the guardians. Among these, 87 percent confirmed that they had received the DVD. Forty percent of these guardians reported watching the DVD in the five days after receiving. At most, therefore, take up was 153 parents.

changed their numbers during the experiment. To maximize the chances of SMS receipt, we changed the dates of message delivery from Friday to Monday in August 2014, early on in the intervention. We also re-contacted all consenting parents in March 2015 to verify and/or update their cellphone numbers, to minimize the chance of message failure due to new phone numbers.

Figure 5 shows the successful delivery rate for treatment and control SMSs during the first year of the intervention, from July 2014 to December 2014. Different lines represent different types of messages sent. Message receipt rates were initially high for the earliest messages sent, and dropped off after the first month of the intervention. During this time, we learned more about the technical reasons for non-receipt, and changed the day of delivery of treatment messages to Mondays. From August 2014 onwards, the rate of successful SMS delivery settles down to between 60 and 70 percent. There are no large differences between rates of SMS receipt across treatment message types. Our intent to treat (ITT) estimates will therefore be lower bounds on the impact of receiving the SMS messages, given this incomplete compliance. However, since message failure would be a feature of any policy that scales this intervention up to the whole school system, the lower bound ITT is the effect we want to estimate to compute cost effectiveness.

Technical reasons affecting whether an SMS is successfully delivered or not (e.g. network overload at certain times of the day/week) are unlikely to be correlated with family-level unobservables that also affect child outcomes. The main reason we might worry that message failure is correlated with family-level unobservable characteristics is if some types of parents change cell numbers frequently. For example, if parents with low attachment to the labor market have unstable incomes, and cannot afford to maintain cell contracts, or need to switch numbers to avoid creditors, they will be more likely to not receive SMS messages from our project. Children in these families may also have worse school outcomes. To check this possibility, we regressed the monthly share of successful SMS messages (total received/total sent) of each type on baseline grades and attendance, month and section fixed effects. Students with higher baseline grades or attendance behaviors are no more (or less) likely to receive SMS messages that were sent (see Appendix Table A.3). Nonetheless, where we use total numbers of SMS messages as the treatment, we always measure the total number sent, rather than the share received, to avoid concerns of selective receipt of SMS messages.

4. Data

i. Administrative data

Our analysis takes advantage of rich administrative data. Table 1 summarizes our data and rates of non-missing data for the sample of participating students. Column 1 shows summary statistics for the full experimental sample and column 3 shows statistics for the sample excluding those enrolled in Grade 8 in 2014. We collect basic demographic data (age, gender) and school performance data (e.g. end of year grades, annual attendance rates, and repetition outcomes) on 92.8% our students in December 2013, the year before our intervention. The baseline data exist for all students enrolled in our sample schools in 2013, and for about half of the students who joined the school in 2014. The remaining missing data are for other new students joining the schools in 2014. We assign class-level mean attendance and grades to those with missing baseline data. In all regressions that control for 2013 values of attendance and/or grades, we use these imputed values and include an indicator variable denoting that the attendance/grade baseline data are imputed.

Outcomes data (attendance, behavior, grades) are available at monthly level in 2014. We also have end of year data on average attendance through the year and average grades. Because of the way we collected attendance data, we have and will eventually use daily attendance data.[11] We also collected school data on the grades attained for each subject (not just math) at the end of each year. In 2014, these administrative data exist for 99.3% of the sample.

ii. Survey data

The intervention had the potential to affect the information parents have about their children as well as the behavior of children and parents. To assess these changes we applied baseline, midline (end of 2014) and endline (end of 2015) surveys to consenting parents and children. In this paper, we restrict our analysis to 2014 data and refer to these as follow up data. We applied the baseline (follow-up) survey to 93.3% (82.9%) and 72.6% (53.6%) of students and parents respectively.

In each survey, parents and children self-reported recent grades and recent absences from school, and parental involvement in school. To capture the degree of parental involvement in school and at home, and to create measures of child effort in school, we asked children and parents a series of questions that covered study habits, academic efficiency, misbehavior in class, parental support, parental supervision, parental school involvement and parental positive reinforcement. These questions, listed in Appendix 3, were randomly mixed into the student and parents' survey instruments.[13] Students and parents could give

[11] In Chile, attendance is taken only once during the day, in comparison to US schools where attendance is marked in each class.

[13] These methods are typical in education psychology research. The survey items were drawn from: The University of Chicago Consortium on Chicago School Research, the Manual for the Patterns of Adaptive Learning Scales

categorical answers of the type "strongly agree," "agree," etc. to each statement. We aggregated student and parent answers into scales (indices) using a maximum likelihood (ML) principal components estimator where only one latent factor was retained to describe all responses to the same category of questions. The ML models were estimated on the control sample only and the results applied to the full sample. After the prediction was computed to produce each scale, we standardized them using the mean and standard deviation of the control group. A unit of the resulting index can therefore be interpreted as a standard deviation unit.

Follow up surveys asked parents about their willingness to pay for the SMS. We asked : "*It is possible that next year your daughter's/son's school can send you regular text messages with information about their school performance (attendance, grades, and behavior) four times a month. However, there might not be enough funds to provide this service free of charge. Thinking about how valuable this service would be for you, please tell us whether you will be willing to pay $V pesos a month to receive four text messages a month, from April to December.*" Parents were randomly assigned a value $V of (low) $500, (medium) $1000 and (high) $1500 price (where $ is Chilean pesos per month, and where $1,000 is about USD1.50).

5. Estimation strategy and experimental validity

i. Analysis of main impacts

Our analysis proceeds in two stages. In the first stage, we estimate the main effects of individual-level assignment to the SMS treatment and investigate the extent of spillovers in the classroom. We also identify who is marginal for the intervention, or, whose behavior is most affected by the treatment. In the second stage, we try to understand some of the mechanisms through which the intervention affected specific outcomes. We look at whether the number of SMS messages mattered for impacts, whether effects wear off over time, whether the treatment closed parent-school information gaps, and whether treated parents report a differential willingness to pay for continuing the program at the end of the first year of the experiment. In ongoing work, we will explore how the survey measures of parental involvement shift after treatment, and investigate the longer run effects of the treatment after the second year of the intervention.

We estimate two types of regressions to identify the main impacts of our information treatment on student outcomes. First, we estimate an individual-level regression among consenters to identify the total impact

(PALS) developed by the University of Michigan, and scales on positive parenting developed by the Prevention Group at Arizona State University.

of being exposed to the SMS treatment. Second, we estimate an individual-level regression among consenters to separate out the direct effect of treatment from any spillover effects on treated individuals within the classroom. We exploit the classroom level randomization of HIGH/LOW shares of treated students to identify these spillover effects among treated students.

Total effect of SMS treatment: To identify the impact of being allocated to the SMS treatment group, we estimate regressions in the form of equation (1) for outcomes y_{icjgt} of student *i* in classroom *c* of school *j* and in grade *g*, where *t* denotes either the month or year of observation (for monthly or annual data):

$$(1)\ \ y_{icjgt} = \beta_0 + \beta_1 SMS_{icjg} + \gamma_c + \pi_t + \bar{y}_{icjg,2013} + \varepsilon_{icjgt}$$

We use three types of grade, attendance, and behavior outcomes, and two types of "attrition" outcomes. For grades, we use the average math grade at the end of 2014, the cumulative grade in math (or math GPA) by month, and an indicator for whether the math grade at the end of the year was above 4.0, the official grade cutoff for passing the math class. For attendance, we use the share of school days attended per month, the cumulative number of days attended each month, and an indicator for whether attendance over the school year was above the 85% official threshold for passing the grade. For behavior, we use the share of total notes reported by the teacher that were positive, negative, and extremely negative. Extremely negative notes include bad behavior like bullying, physical and verbal violence at school. Finally, we look at outcomes that capture whether a student passed the grade at the end of 2014, and whether they left the school by the end of 2014. This last outcome captures both school switching and dropout.

SMS_{icjg} is an indicator for whether a child was randomized into receiving the SMS treatment and is constant over time, γ_c is a classroom level fixed effect (where the classroom is defined by the 2014 class), π_t are time fixed effects that are included where the data are recorded monthly. $\bar{y}_{icjg,2013}$ is a measure of the outcome variable at baseline, where it exists. This last variable is included to absorb residual variation in the outcome variable because grades and attendance behavior are correlated over time for an individual and are also difficult outcomes to shift.

β_1 gives us the impact of being assigned to treatment in the first year of the intervention: it captures both the direct effects of being assigned to treatment as well as spillovers from others in the classroom being treated. Because we include classroom level fixed effect (γ_c), β_1 is identified off of differences in individual-level treatment status within a classroom.

Decomposing direct and indirect (spillover) effects of SMS treatment: If there are peer effects in the classroom, we might expect our treatment to affect the outcomes of other children, independent of their treatment status. For example, the value of skipping school may fall, when friends no longer play truant. Or, if one's friends are working harder to improve grades, own effort involved in improving grades may change (be higher, or lower). To separate out the direct effect of being treated from the spillovers associated with others in the same classroom being treated, we estimate the following interacted specification, again restricting the data to 2014 observations[14]:

$$(2)\ \ y_{icjgt} = \alpha_0 + \alpha_1 SMS_{icjg} + \alpha_2 HIGH_{cjg} + \alpha_3 SMS_{icjg} * HIGH_{cjg} + \bar{y}_{icjg,2013} + \eta_c + \rho_t + \omega_{icjgt}$$

where the subscripts and all prior variables are as before, η_c is a section fixed effect and ρ_t a set of time fixed effects (when monthly data are used), and $HIGH_{cgj}$ is an indicator for whether the classroom was randomized into being a high or low share treated classroom.[15] Because we had no experimental classroom with zero treated students, we identify the differential effect of the spillovers by comparing high and low share treated classrooms. α_0 captures the spillover effect of being a non-treated student in a low share classroom while α_2 is the differential spillover associated with being a non-treated student in a high share classroom. α_1 captures the total effect of being a treated student in a low share classroom and $\alpha_2 + \alpha_3$ captures the total differential effect of being a treated student in a high share classroom (i.e. the spillover to treated students). If we assume that the spillover effect is linear in the share of students treated, then α_3 is the differential spillover of being a treated student in a high share treated classroom relative to a low share classroom. In other words, it is the extra value of being in the text messaging program, given that so many more of your classmates are also in the program. Such spillovers could be important, especially if such parent-school communication programs are scaled up to cover all enrolled students (rather than just a randomly selected treatment group).

Notice that we cannot estimate α_2 with section fixed effects included in the specification. Hence, we will not be able to capture the spillover to non-treated students, nor the total spillover to treated students. We can estimate the *differential* spillover to treated students in high share treated classrooms (α_3). We can also relate the parameters in (2) to those in (1), which is helpful for interpreting results. From (1), β_1 is the

[14] We restrict the data to 2014 for two reasons. First, classes potentially re-sort from 2014 to 2015. Changing class composition therefore changes the nature of the spillover in the second year of the intervention. Second, we hypothesize that if spillovers do not show up in the first year of treatment they are unlikely to be important in the second year. Our analysis of the second year of data is ongoing.

[15] We tested an alternative interacted specification that included only school-grade fixed effects and a control for class size in place of classroom fixed effects. We find very similar estimates. However, since the individual level treatment is stratified on classroom, the classroom fixed effects specification is our preferred specification.

average effect of being assigned to treatment across treated individuals in high and low share treated classrooms, including all spillovers. That is:

$$\text{(3)} \quad \beta_1 = ShareHIGH*(\alpha_1+\alpha_3) + (1\text{-}ShareHIGH)*\alpha_1 = ShareHIGH*(\alpha_3) + \alpha_1$$

where *ShareHIGH* is the share of all students in the experiment who are in $HIGH_{cgj} = 1$ classes. Equation (3) states formally that β_1 is the (weighted) average effect of the treatment among students who are in high and low share treated classrooms.

ii. Experimental validity: Balance at baseline and attrition

Table 2 presents our baseline balance tests for the two specifications (1) and (2) above. We look for balance in the administrative data at baseline and in responses to key parent and student baseline survey questions. The table shows total observations with non-missing data (column 1), the mean of the control group outcome (column 2), and the *p* value on the coefficient on SMS_{icjg} (column 3) estimated using our main specification in equation (1). The last two columns provide *p* values associated with coefficients on SMS_{icjg} and $SMS_{icjg}*HIGH_{cjg}$ estimated using the spillover specification in equation (2).

Our sample is 46% female, and almost 20% are students new to the school in 2014. The median age in the sample is 13 years, and students range in age from 9 to 18. About 5% of students in the 2014 sample are repeating a grade. Among parents who completed baseline surveys, only 68% have completed high school. This variable is constructed using the highest level of completed education among all listed guardians in the household (mom, dad, or other guardian, who is often a grandmother). The experimental sample is balanced at baseline (see *p* values in column 3) across administrative and survey data for all but one variable, the survey measure of parent-reported family support. Balance is similar using the expanded specification that allows for classroom-level spillovers (see columns 4 and 5), with only two variables not balanced at baseline.

In Table 3, we show the availability of baseline and follow up administrative and survey data for our experimental sample. We also show whether data are differentially available by individual treatment status, SMS_{icgj} using an OLS specification in Panel A, and a logit specification in Panel B. All data are available at the same rates for treatment and control groups, except for administrative data at the end of the year in 2014. While we have administrative data collected weekly from schools for all of our continuing treatment and control students, the end-of-year data collected on student outcomes seem less likely to be available among the treated students (column 2). This significant difference is driven by 12 students from our experimental sample. We are working to track down these students and fill in their end-of-year data on grade repetition, and school leaving. Overall, attrition on most of our main administrative and survey outcomes is balanced across treatment and control students.

iii. Treatment compliance

As noted above, technical reasons drive much of the incomplete compliance with treatment. Using the same structure as equation (1), we estimate differences in the total number of all SMS messages sent (or received) across treatment and control groups in Table 4, Panel A column (1) (Panel B, column (1) for message receipt). In the remaining columns of Panel A, we examine differences in the number of each type of SMS message sent (or received, Panel B) by treatment assignment. Each regression includes a full set of section fixed effects.

By the end of 2014 and over a span of five months, an average of 27 SMS messages had been sent to each parent, and a total of 18 messages had been received. These numbers line up with Figure 3, where we show that between 60 and 70% of sent SMS messages are successfully received by the end of the year. Most of the messages were about attendance (18 total), with equal numbers of behavior and grade messages (just over four of each type) sent by the end of the year. Between seven and eight general messages were sent to parents in treatment and control groups, and around five of these were received. For almost all outcomes, those randomized into receiving the SMS treatment were sent and actually received significantly more SMS messages than those in the control group. For attendance, behavior and grade outcomes, the average number of messages sent/received is the same as the point estimate on the SMS indicator. Treatment messages were only sent to, and received by, parents assigned to treatment. In contrast, the control group received general messages at largely the same rate as those in the treatment group (the coefficient on SMS_{icgj} is negative but small for the outcome of number of general SMS messages received (Table 4, column 5, Panel B)).

6. Results

i. Main effects of individual-level treatment

Table 5 presents the main results from estimating equation (1) on our experimental sample. The first three columns present math grade outcomes at the end of the year (column 1), cumulatively by month (column 2) and an indicator for whether the math grade was a passing grade above 4.0 (column 3). Columns 4-6 focus on attendance outcomes: monthly attendance (column 4), cumulative days attended (column 5), and an indicator for whether attendance was above the 85% cutoff required for the student to pass the grade (column 6). Columns 7-9 capture behavior outcomes: the share of total behavior notes that were positive (column 7), negative (column 8), and extremely negative (column 9). The final two columns present grade repetition outcomes (column 10) and an indicator for whether the student moved schools (column 11). In each regression, we include section fixed effects for 85 sections. Where outcomes are measured monthly, we also include month dummies. In columns 1, 2, 4 and 5, we include baseline controls for grades or attendance in 2013 (although estimates are not sensitive to this inclusion). All standard errors are robust and clustered at the level of the section.

Across the board, our treatment had positive impacts on school outcomes. Exposure to the SMS treatment increases math grades by 0.072 points or 0.088 standard deviations. The effects are evident at the end of the school year, and also show up in the monthly GPA measure. This positive impact on math grades lifts 2.8 percentage points more students over the 4.0 cutoff for passing the subject. Attendance results are muted, although exposure to treatment lifts a sizeable fraction of students over the 85% cutoff relevant for passing the grade.

The treatment has a small positive, but insignificant, impact on the occurrence of positive behaviors among students. However, it significantly reduced the prevalence of extremely bad behaviors. Exposure to the SMS messages reduced the share of extremely bad behavior notes in class (column 9) by 1.25 percentage points, or 18 percent. Finally, while treatment did not induce more kids to change schools – if anything, it reduced school transitions – it significantly reduced grade repetition. In the full sample, treated students had a 2.9 percentage point increase in the probability of passing the grade by the end of 2014; this is about a 3% increase relative to the control mean. In numbers, this represents an additional 20 students who are prevented from repeating the grade.

ii. Classroom-level spillovers

Table 6 presents the results of estimating the spillover equation (2) for the same set of outcomes in Table 5. We present the estimates of α_1 and of α_3 from that equation, as well as the sum of the two coefficients. Recall that α_1 is the direct effect of the SMS treatment among kids in low share treated classrooms ($HIGH_{cgj}=0$), while $\alpha_1+\alpha_3$ is the effect of the SMS treatment among kids in high share treated classrooms ($HIGH_{cgj}=1$).

In all cases, the main effect of being assigned to treatment in a low share classroom is positive; bad behavior improves and the probability of moving schools also falls. And in almost all cases, the *differential* effect of being assigned to treatment in a high share classroom is positive, and larger than the main effect of the treatment in low share classrooms. Examining the second row of coefficients, we see that grades are higher, students are more likely meet the 4.0 grade cutoff, and meet the 85% attendance cutoff for passing, and are more likely to pass if they are treated in high share treated classrooms. Because we split the sample to estimate this spillover effect among the treated students, individual coefficients are not always statistically significant. However, on adding up the effect of the treatment among treated students in high share classrooms (row 3 of the table), we see that the probability of meeting the grade cutoff for passing increases by 4.8 percentage points; the probability for meeting the attendance cutoff for passing increases by 11.9 percentage points, and the probability of passing the grade at all increases by 4.9 percentage points.

For almost all outcomes, being treated along with a larger share of children in your class raises the “return” to treatment; grade and attendance impacts are higher, and the chances of passing the grade are higher. The one outcome which does not follow this pattern is the share of extremely bad behaviors recorded in the classroom. In column 9, we see that individual level randomization to treatment significantly and substantially reduces bad behaviors among treated students in low share treated classrooms. However, the estimate of the interaction term is large, significant, and negative. This means that in high share treated classrooms, the spillovers coming from having other kids in your class that are also treated completely negate the direct impact of the treatment on your reduction in bad behavior.

iii. Identifying marginal students

Because *Papás al Día* is a relatively low-touch intervention, it is important to understand which students were most affected by the frequent contact with schools via text message. In particular, because dropout is likely to manifest only later on in high school, we want to know whether our intervention has large impacts on those students most at risk for dropping out. That is, is the intervention self-targeting?

To make some headway on this, we generated a predicted probability of dropout for our experimental sample in two steps. First, we regressed an indicator of dropout (did the student drop out of school by 2014) on 2013 grades, attendance, the interaction of grades and attendance, age, gender, and school fixed effects. We use administrative data on all students enrolled in our experimental schools in 2013 to estimate this model. Then, we apply these estimated coefficients to our sample of students in the intervention to create a predicted probability. We standardize this predicted probability so that a one point change in the variable is a one standard deviation shift in the predicted probability of dropping out.

In Figure 6, we plot four of our main outcome variables against the (standardized) predicted probability of dropout from the above procedure. The outcomes are (clockwise, from top left panel) average math grade, an indicator for above 85% attendance, an indicator for passing the grade, and the share of really bad behavior notes received, all measured at the end of 2014 and residualized for section fixed effects.[16] The solid line is the locally smoothed regression line for the treatment group, and the control group is shown with the dotted line.

As we might expect, endline grades, attendance, and probability of passing the grade fall with the value of the baseline predicted probability of dropout (x-axis). The graphs show that based on observables at baseline, kids who have a higher probability of dropping out end up with worse end-of-year outcomes. Interestingly, the prevalence of really bad behavior in school is highest for kids with medium values of the predicted probability of dropping out of school; kids with high probability of dropping out and low probability of dropping out have the lowest rates of bad behaviors reported at the end of 2014. This may be because kids with the highest risk of dropping out (extreme right on the x-axis) are also attending school much less often, and so have fewer opportunities to exhibit bad behaviors in class.

Our intervention had the largest impacts on grades, bad behaviors, and passing the grade, for students in the middle of the distribution of predicted probability of dropping out. We can see this by observing the gap between the treatment and control lines in each graph. This gap illustrates the differential effect of our treatment on each outcome for different values of the baseline predicted probability of dropout. Effects on attendance seem smaller, but more uniformly distributed across students throughout the predicted probability of dropout distribution. Part of this may be because improving attendance can be done with lower effort than improving other outcomes.

For grades and passing the grade, students who have an elevated predicted probability of dropping out experience a larger treatment effect. Most dramatically, students in the same (middling) range of the distribution of predicted dropout experience very large reductions in bad behaviors in school. Crucially, though, *Papás al Día* had little impact on students with the lowest predicted probabilities of dropping out, and little impact on those with the highest predicted probabilities of dropping out. With such a light touch intervention, we can still see positive gains among students with elevated, but not highest risk, of dropping out.

7. Exploring mechanisms

In this section, we explore how the information treatment worked to improve outcomes over time. We examine whether the number of SMS messages was important for generating positive impacts, whether there is evidence of the intervention wearing off over time, whether parent-school information gaps

[16] That is, we regress each outcome on section fixed effects, predict the residual from this regression, and use these residuals to create each graph.

shrink, and whether treatment and control parents reveal different willingness to pay for continuing the program. For all of the following results, we focus on the total effect of the treatment, i.e. on specifications like (1), which average the effects of the treatment across students in both high and low share treated classrooms.

i. Specific and frequent information affects a broad range of behaviors

The volume (number) of SMS messages seemed to be important for explaining the positive impacts of *Papás al Día*. We estimate regressions of the form in (1), but instead of using the treatment indicator, we use the number of actual SMS messages sent to the parent by the end of 2014. The variation in total number of messages sent depended partly on when the school was entered into the treatment, and partly on how often there was updated information (e.g. on recent math grades) received from the schools.[17] Since we do not use the number of messages actually received as the treatment, these estimates are still intent to treat estimates.

Table 7 presents the results. Treated students whose parents were sent more total SMS messages have somewhat higher grades, a higher chance of meeting the attendance cutoff for passing, show lower prevalence of very bad behavior, and are more likely to pass the grade and stay in the same school. Unsurprisingly, messages are not only connected to targeted behaviors, but also impact other behaviors. For example, the more grade messages are sent, the more attendance improves, bad behavior declines, and the chances of passing the grade increase. More attendance messages improve attendance, and also increase math grades, reduce negative behaviors and increase the chances of passing at the end of the year. And, the more behavior SMS messages are sent, the larger the positive impacts on grades, attendance, and pass rates. These results are reassuring. They show an absence of crowding out: sending attendance SMS messages does not crowd out effort in improving grades, but rather contributes to improvements in both areas.

ii. Grade effects persist but shrink over time

Table 8 shows that assignment to treatment wears off a little over time. In this table, we estimate specifications of the type in equation (1), but use monthly math grades as the outcome, for each of the months of September, October, November and December 2014. Not all students have math tests every month, so sample size varies across columns.

[17] School math test schedules were not standardized across schools, and so having a vacation day or different classroom schedule for testing would have affected how much information we had to distribute to parents in any given week.

The effect of treatment assignment on math grades is strong and positive in the first complete month of treatment: grades are a significant 0.126 points higher among treated students relative to controls, or about 0.1 of a standard deviation. In subsequent months, the grade impacts fall, and lose some significance. Part of this could be because effort costs of constant grade improvements increase with higher grades. The impacts of the SMS program may be muted by these "ratchet" effects.

iii. Specific and frequent information narrowed parent-school information gaps about grades

Figures 3 and 4 showed the prevalence of parent misinformation/misreporting of student grades at baseline. About one in four parents were unable to report their child's end of year school grade for 2013 within 0.5 points of the actual grade. Focusing on the balanced sample of parents who respond to both baseline and follow up surveys (N=412), we ask: does assignment to receiving SMS messages reduce the information gap between parents and schools at follow up? We estimate regressions of the following type:

$$(4)\ InfoGap_{icjg,2014} = \delta_0 + \delta_1 SMS_{icgj} + \delta_2 InfoGap_{icgj,2013} + \delta_3 SMS_{icgj} {*} InfoGap_{icgj},_{2013} + \lambda_{cgj} + \varepsilon_{icgj}$$

where $InfoGap_{icgj}$ is the linear difference between parent and school grade reported in period t, the absolute gap of this difference, or an indicator for whether the parent report is further than 0.5 points from the administrative grade data reported by the school. The grade reported is final end of year grade, the average over all subjects, including math.

Table 9 shows that the SMS program improved parent-school communication about student grades by follow-up. Parents with the largest information gaps at baseline continue to report grades that differ from the school administrative data (columns 1 through 4), and to misreport at higher rates (columns 5 and 6). However, our treatment reduces the size of the reporting gap, measured as the difference between parent and school reports, or the absolute difference in reports. The probability of misreporting also declines among treated parents, relative to parents in the control group. Because our sample of parent follow-up survey respondents is relatively small, these information gap reductions are not always precisely estimated, but coefficients are large and negative for all outcomes. Imprecisely estimated negative coefficients on the interaction term ($SMS_{icgj} {*} InfoGap_{icgj},_{2013}$) provide further suggestive evidence that parents for whom information gaps were largest at baseline benefited the most from the new information provided by *Papás al Día.* Overall, these results show that treated parents had more accurate information about their child's grades at follow up. Future work will investigate impacts on parent reports of student attendance.

iv. Parents valued the information provided

In our follow up survey, we asked both treatment and control parents to tell us whether they would be willing to pay for an SMS service that provided them with four monthly messages from schools about their child's performance and behavior in school. We randomized the price at which parents were given the take it or leave it offer: a high price of 1,500CLP (Chilean pesos, or 2.2 USD) per month, a medium price of 1,000 CLP (or 1.5 USD per month), or a low price of 500CLP (0.74 USD) per month. Table 10 uses this randomization and the survey responses from parents to estimate demand curves for the full sample (column 1), the control group (column 2), the treatment group (column 3), and the pooled sample of treatment and control groups (columns 4 and 5). In the final two columns, we allow each experimental group to have a different response to the randomized price by including price assignment by treatment assignment interaction terms.

Overall, the demand curve for a service like *Papás al Día* slopes downwards. Column (1) shows that as the price moves from low to medium, the share of parents willing to pay falls by 18 percentage points, and falls a further 23 percentage points when the price increases from medium to high. These patterns resemble what happens in the control group (column 2). Among treated parents, demand falls by 24 percentage points when the price rises from low to medium, and falls by 19 percentage points when the price rises to its highest level; these coefficients are not statistically different from each other.

Next, we combine the treatment and control groups in column (4) and estimate the demand equation without controlling for section fixed effects. We do this because only half of the parent sample responded to the follow up survey questionnaire, so our sample size is relatively small. Column (4) shows that at the highest price, treatment parents are more likely to say they are willing to pay for the continued service relative to control parents. When parents have some experience with using the service, they demand more of the good at every price. Once we include section fixed effects, the interaction terms are no longer significant (column 5).

Of course, as we noted in the section discussing marginal students, it is likely that not all families would experience the same "return" to the SMS program. For example, the value of such a service may be relatively low for parents who have high performing children. In Table 10 column 6, we present results where we restrict to the sample of parents whose children score below 6.5 at baseline. These are children who are not at the top end of the grade distribution. In this subsample, the SMS treatment increases parent willingness to pay at all prices, relative to the control group (coefficients for willingness to pay are 0.18 and 0.02 for high and medium prices respectively), with the highest differential impact on demand at the highest randomized price. Comparing treatment with control parents in this subsample implies that the elasticity of demand for *Papás al Día* is 40% lower among treated parents than control parents.

8. Conclusions

In this paper, we present a simple, and effective, intervention that uses existing data regularly collected by schools to improve parent information about their children' outcomes on a high frequency basis. We show that sending weekly SMS messages with attendance information and bimonthly SMS messages with behavior and math grade outcomes decreases the gap between what parents know about their children, and what schools report. Effects on school behaviors and outcomes are evident after four months of treatment. Providing parents with this information resulted in higher math grades, better school attendance, lower probabilities of extremely bad behaviors and higher probabilities of grade progression. Effects are larger among individuals with a medium level of baseline risk of dropping out of school. We use experimental variation to test the existence of spillovers, and find that program effectiveness is higher (spillovers are positive) when a larger share of parents receive the SMS messages. In ongoing work, we analyze the effects of the program over a longer time period.

Overall, our results show that a low-cost, low-touch, feasibly scalable intervention can have an important impact on students' behavior, with potentially large gains in long run human capital attainment. Relative to other types of parenting programs, our intervention is relatively low cost and would likely be more sustainable and amenable to scale up in developing country settings outside of Chile. Moreover, we demonstrate that effective use of a technology that improves parent-school communication can improve outcomes, thereby improving the returns to existing school inputs.

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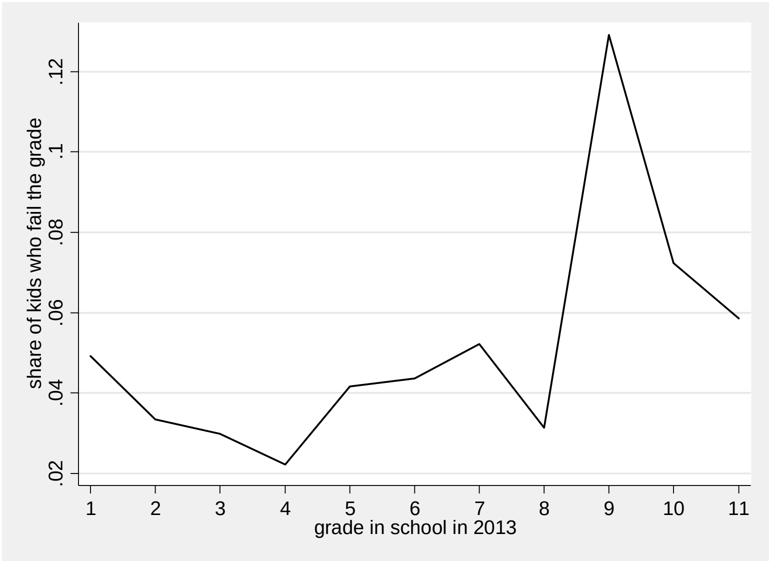
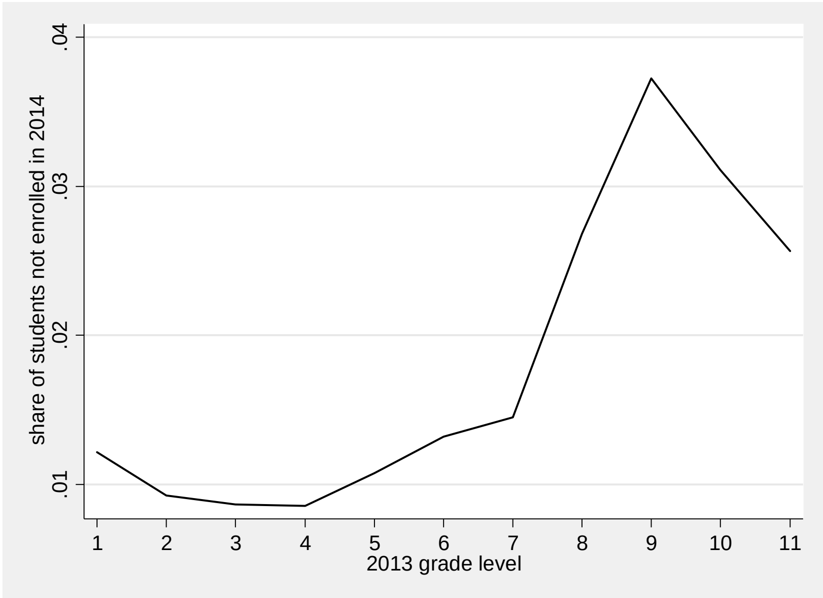
Figure 1: Share of children enrolled who fail the grade, Grades 1-11



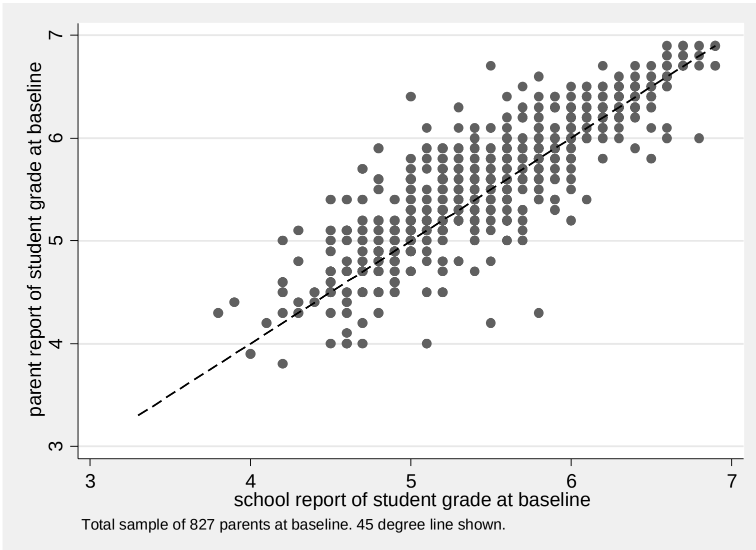
Notes: Administrative data from Chilean MINEDUC for universe of students enrolled in 2013

Figure 2: Share of students who drop out between 2013 and 2014, Grades 1-11



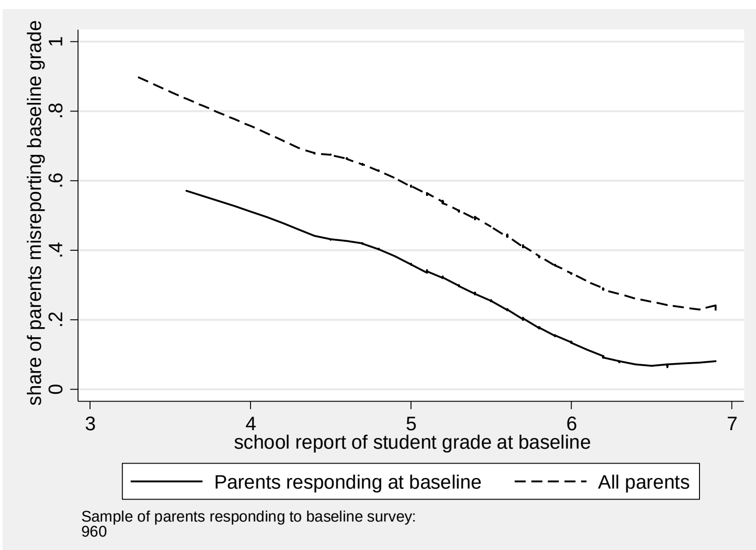
Notes: Administrative data from Chilean MINEDUC for university of students enrolled in 2014. Dropout here is defined as “student is not found in administrative records in 2014”.

Figure 3: Correlation between school grade and parent report of school grade at baseline



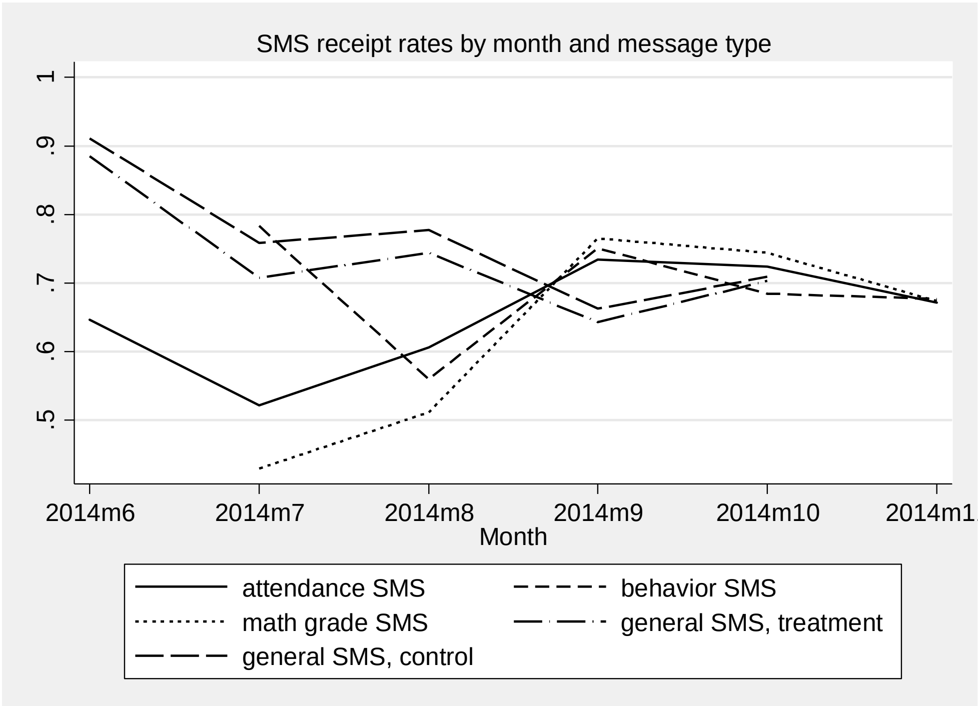
Notes: X-axis shows student grades at baseline collected from administrative data at the end of 2013. Y axis shows parent report of student grade collected from baseline parent surveys. 45 degree line also shown.

Figure 4: Share of parents misreporting grades at baseline by actual grades at baseline



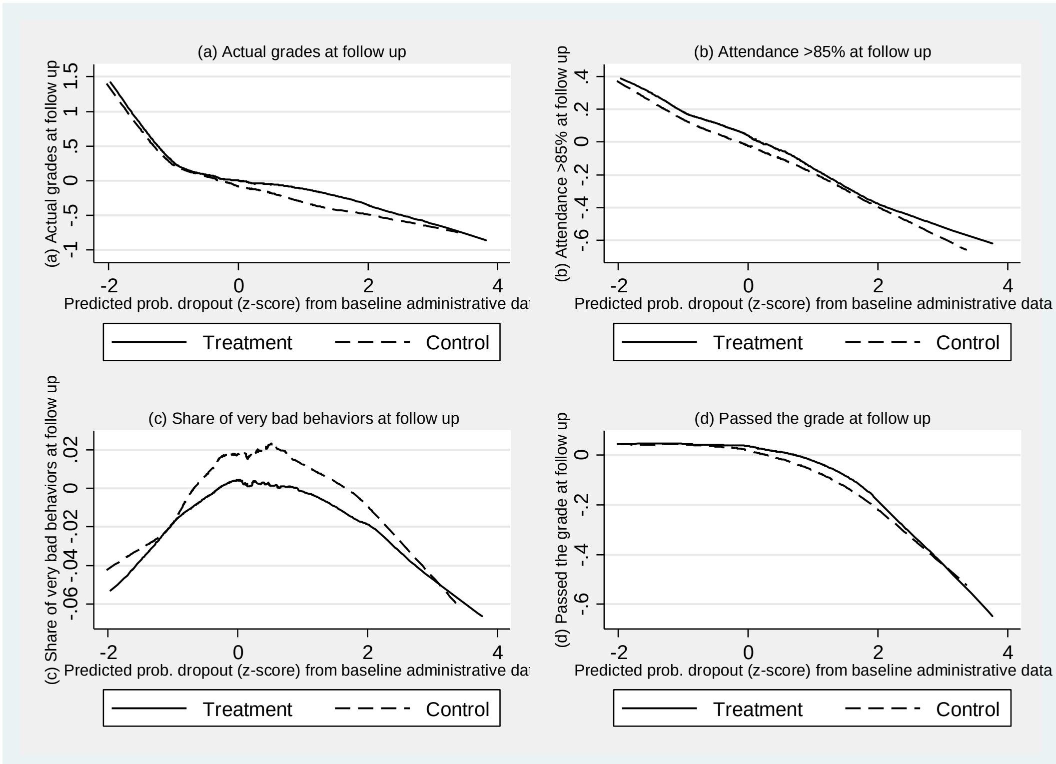
Notes: X-axis shows student grades at baseline collected from administrative data at the end of 2013. Y –axis shows the (lowess-smoothed) share of parents misreporting student grade in 2013, collected from baseline parent surveys. We define a misreported grade for grades that are outside of 0.5 points of the actual grade. Grades range from 3 to 7 in units of 0.1. Solid line uses data from parents who respond to our survey N=960. Broken line includes all parents. We assign parents who do not respond to our baseline survey a 1, imputing that they misreport the grade.

Figure 5: Share of SMS messages sent that are received, by month and message type



Notes: Share of SMS's sent that were actually received by treatment and control groups by month of the intervention. Receipt rates are presented for each type of information message sent to the treatment group, and for the general message sent to the treatment and control groups. Successful receipt of SMS messages stabilizes around 60-70% by September 2014.

Figure 6: Heterogeneous treatment effects with respect to predicted probability of dropout at baseline



Notes: Clockwise from top left, figures show end of year average math grade, over 85% attendance for the school year, whether the student passed the grade, and the share of very bad behaviors at follow up. All outcomes are measured at follow up (December 2014) for the sample of treatment (solid line) and control (broken line) students in the experiment. Outcomes are first residualized for section fixed effects, and plotted on the y-axis against the student's (standardized) predicted probability of dropout at baseline. Predicted probabilities of dropout are measures of student vulnerability created using baseline data from administrative records from all Chilean students enrolled in our experimental schools in 2013. We predict the probabilities of dropout among grades 4-8 using these data based on attendance, school grades, gender, age and school fixed effects, then we apply the coefficients to our experimental sample to generate a predicted probability of dropout based on observable characteristics at baseline.

Table 1:Response Rates and Administrative Data

	[1]	[2]	[3]	[4]
	Whole sample		*Excluding Grade 8s at baseline*	
	Total sought	Found (%)	Total sought	Found (%)
Consent	1,447	100%	1,124	100%
Administrative Data				
Student outcomes				
2013	1,343	92.8%	1,041	93%
2014	1,437	99.3%	1,117	99%
Parent attendance, school meetings				
2015	1,008	69.7%	952	85%
Survey Data				
Student surveys				
Baseline 2014	1,336	92.3%	1,028	91%
End of 2014	1,286	88.9%	993	88%
Parent surveys				
Baseline 2014	1,050	72.6%	817	73%
End of 2014	776	53.6%	629	56%

Note: Column [2] presents the percent of consented individuals with non-missing data. Column [4] presents the % of consented individuals enrolled in Grades 4-7 at baseline (excluding grade 8's) who have non-missing data. Administrative data is considered available for a student if an individual has data on grades, attendance, and pass/fail/exited school status at the end of the year. We impute baseline values for those with missing attendance, grades, or end of year outcome (pass/fail) data in 2013.

Table 2: Balance of Baseline characteristics - whole sample

	[1]	[2]	[3]	[4]	[5]
				p values	
	N	Control Mean	SMS	SMS	SMS X High
Administrative Data					
Female	1,447	0.460	0.836	0.742	0.747
Age	1,447	13.07	0.852	0.680	0.711
New student in 2014	1,351	0.189	0.878	0.874	0.631
Final grade in 2013	1,352	5.497	0.407	0.605	0.851
Attendance rate in 2013	1,379	0.888	0.526	0.696	0.859
Passed grade end 2013	1,447	0.951	0.992	0.694	0.560
Missing 2013 grades/attendance/passing data	1,447	0.0719	0.596	0.677	0.888
Survey Data					
Parents: completed high school (0/1)	1,031	0.681	0.907	0.967	0.832
Student Scales (selected)^^					
Study habits	1,242	0	0.820	0.335	0.298
Family support	1,199	0	0.428	0.134	0.151
Parent School involvement	1,196	0	0.766	0.091	0.036
Parent Scales (selected)^^					
Study habits	939	0	0.655	0.707	0.962
Family support	988	0	0.0436	0.111	0.737
Parent School involvement	953	0	0.711	0.318	0.363
Math score on student survey test, share correct	1,336	0.407	0.842	0.475	0.406
Parent attends meetings, student report (0/1)	1,298	0.771	0.237	0.533	0.674
Parent attends meetings, parent report (0/1)	1,050	0.758	0.725	0.839	0.925
Ave. parental involvement score, student report	1,320	0.940	0.436	0.258	0.347
Ave. parental involvement score, parent report	1,028	0.974	0.110	0.072	0.161

Column [1] shows the number of observations with non-missing data, column [2] the mean value of each baseline characteristic in the control group. Column [3] reports the p-value on the SMS coefficient in a regression using each baseline characterstic as the dependent variable. Columns [4] and [5] report p-values for coefficients on the SMS coefficient and on the interaction coefficient (SMSxHigh treatment share classroom) for regressions using each baseline characteristic as the dependent variable. All regressions include controls for class fixed-effects (strata). Robust standard errors are clustered at the section level. *Difference between parent report of baseline grade/attendance and student report of baseline grade/attendance. ^^Scales variables are summary measures of parent and student survey responses to categories of questions.

Table 3: Attrition - Whole sample

	[1]	[2]	[5]	[6]	[8]	[9]
	Administrative Data Exists		Student Survey Data Exists		Parent Survey Data Exists	
	2013	2014	2013	2014	2013	2014
Target N	1,447	1,447	1,447	1,447	1,447	1,447
Match/response rate	0.93	0.89	0.92	0.88	0.72	0.53
SMS	-0.011	-0.0141**	-0.007	0.015	0.008	0.031
	(0.020)	(0.00558)	(0.017)	(0.020)	(0.026)	(0.028)
R-squared	0.18	0.064	0.12	0.11	0.16	0.13

Outcomes are indicators for whether an individual has administrative, student survey, or parent survey data for the relevant year. Panel shows coefficients from OLS regressions of outcomes on individual SMS treatment indicator. Each specification controls for section fixed effects, robust standard errors are shown.

Table 4: Compliance with treatment by end of 2014

	[1]	[2]	[3]	[4]	[5]
			Whole sample		
Type of message	All treatments	Attendance	Behavior	Grades	General
			Panel A: Cumulative number of SMS messages sent		
Ave. Num. SMS sent to relevant group^^	27.14	18.00	4.570	4.569	7.635
Std. Dev. of SMS sent	4.405	2.911	0.753	0.759	1.565
SMS Treatment Assignment	27.34***	18.13***	4.601***	4.602***	-0.00600
	(0.333)	(0.217)	(0.0592)	(0.0593)	(0.0582)
N	1,447	1,447	1,447	1,447	1,447
			Panel B: Cumulative number of SMS messages received		
Ave. Num. SMS received by relevant group^^	18.12	12.08	3.128	2.915	5.135
Std. Dev. of SMS received	9.311	6.254	1.750	1.640	2.705
SMS Treatment Assignment	18.15***	12.09***	3.137***	2.923***	-0.273*
	(0.480)	(0.319)	(0.101)	(0.0821)	(0.154)
N	1,447	1,447	1,447	1,447	1,447

Each column and panel shows output from a regression of the cumulative number of SMSs of each type sent/received by a parent in the study by the end of 2014. General SMS messages were sent to all treatment and control individuals. Ave. number of SMS received by relevant group captures number of treatment SMS messages received by treatment group and number of control (General SMS) messages received by all individuals. Each regression includes section fixed effects and robust standard errors are clustered at the section level. The sample in columns 6-10 consists of students enrolled in grades 4, 5, 6 or 7 in 2014. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Intent To Treat (ITT) Effects on Grades, Attendance, Behavior, Passing the grade and Moving schools

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Type of outcome	*Grades in math*			*Attendance*			*Behavior*			*Passing / Moving*	
	Ave. Grade end of year [3 to 7]	Cumul. Grade by month [3 to 7]	Math grade >4.0 [0/1]	Monthly Attendance rate	Cumulative days attended	Cumul. attendance >85% [0/1]	Share of Positive Notes	Share of Negative Notes	Share Extremely Negative Notes	Passed the grade [0/1]	Moved schools [0/1]
Control Mean	5.094	5.043	0.898	0.811	67.68	0.647	0.170	0.515	0.0663	0.939	0.0313
Control Std. Dev.	0.816	0.830	0.302	0.224	24.23	0.478	0.326	0.448	0.183	0.240	0.174
SMS treatment (β_1)	0.0728*	0.0771*	0.0280*	0.000390	0.367	0.0666**	0.0104	0.00942	-0.0125**	0.0291**	-0.0120
	(0.0423)	(0.0397)	(0.0143)	(0.0107)	(0.709)	(0.0256)	(0.0167)	(0.0221)	(0.00528)	(0.0136)	(0.00916)
N observations	1,439	6,737	6,737	6,780	6,784	6,784	6,784	6,784	6,784	1,435	1,435
Unique observations	1439	1439	1439	1446	1447	1447	1447	1447	1447	1435	1435
Lagged Dep. Var.?	X	X		X	X						

Columns report the intent-to-treat (ITT) estimate and standard error (in parenthesis) of individual-level program assigment on outcomes measured by the end of 2014. All columns include section fixed effects and standard errors are calculated allowing for clustering at the section level. Outcomes in columns [1], [10] and [11] are measured once at the end of the year. Outcomes in all other columns are monthly measures; these regressions additionally include month fixed effects. Regressions in columns [1], [2], [4], and [5] control for the baseline value of the dependent variable. If baseline values are missing we impute using the mean and flag these observations with an imputed dummy in the regression. In Columns [7], [8] and [9], the outcomes are the share of total notes (positive, negative, or extremely negative) reported on each student by the teacher during the month. See data description for more details of these variables. Sample size varies due to missing values. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Intent To Treat (ITT) Effects on Grades, Attendance, Behavior, Passing the Grade, and Moving Schools: Separating out direct and spillover effects

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Type of outcome		*Grades in math*			*Attendance*			*Behavior*		*Passing grade / Moving*	
	Ave. Grade end of year [3 to 7]	Cumul. Grade by month [3 to 7]	Math grade >4.0 [0/1]	Monthly Attendance rate	Cumulative days attended	Cumulative attendance >85% [0/1]	Share of Positive Notes	Share of Negative Notes	Share Extremely Negative Notes	Passed the grade [0/1]	Moved schools [0/1]
SMS treatment	0.0498	0.0754	0.0142	-0.00673	0.0398	0.0305	0.00815	0.0115	-0.0208***	0.0155	-0.0125
α_1	(0.0545)	(0.0537)	(0.0186)	(0.0152)	(1.059)	(0.0353)	(0.0240)	(0.0315)	(0.00757)	(0.0161)	(0.0110)
SMS*HIGH	0.0562	0.00404	0.0338	0.0175	0.804	0.0887*	0.00546	-0.00515	0.0204**	0.0329	0.00133
α_3	(0.0862)	(0.0800)	(0.0284)	(0.0207)	(1.331)	(0.0487)	(0.0321)	(0.0427)	(0.00968)	(0.0283)	(0.0192)
SMS + SMS*HIGH	0.106	0.0795	0.048**	0.011	0.844	0.119***	0.0136	0.00636	-0.000335	0.049**	-0.0112
$\alpha_1+\alpha_3$	(0.066)	(0.059)	(0.022)	(0.014)	(0.808)	(0.034)	(0.021)	(0.029)	(0.006)	(0.023)	(0.016)
N observations	1,439	6,737	6,737	6,780	6,784	6,784	6,784	6,784	6,784	1,435	1,435
Unique observations	1439	1439	1439	1446	1447	1447	1447	1447	1447	1,435	1,435
Lagged Dep. Var. ?	X	X		X	X						

Columns report the intent-to-treat (ITT) estimate and standard error (in parenthesis) of individual-level program assigment on outcomes measured by the end of 2014, and on the interaction of individual-level treatment and class-level treatment (ShareHIGH=1 if 75% of experimental sample were assigned to treatment). All columns include section fixed effects and standard errors are calculated allowing for clustering at the section level. Outcomes in columns [1], [10] and [11] are measured once at the end of the year. Outcomes in all other columns are monthly measures; these regressions additionally include month fixed effects. Regressions in columns [1], [2], [4], and [5] control for the baseline value of the dependent variable. If baseline values are missing we impute using the mean and flag these observations with an imputed dummy in the regression. In Columns [7], [8] and [9], the outcomes are the share of total notes (positive, negative, or extremely negative) reported on each student by the teacher during the month. See data description for more details of these variables. Sample size varies due to missing values. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effects of number of SMSs sent on school outcomes: ITT (OLS) results

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
Type of outcome		*Grades in Math*			*Attendance*			*Behavior*		*Repetition/Passing/Moving*	
	Average Grade end of year [3 to 7]	Cumulative Grade by month [3 to 7]	Math grade >4.0, "PASS" [0/1]	Monthly Attendance rate	Cumulative days attended	Cumulative attendance >85%, "VOUCHER" [0/1]	Share of Positive Notes	Share of Negative Notes	Share Extremely Negative Notes	Passed the grade [0/1]	Moved schools [0/1]
Treatment is											
Num. SMS sent	0.002	0.003*	0.001	0.083**	0.001	0.003***	0.000	0.001	-0.001**	0.002***	-0.001***
	(0.001)	(0.002)	(0.001)	(0.034)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)
Mean of treatment	27.21	21.78	21.78	21.72	21.75	21.72	21.72	21.72	21.72	27.12	27.12
Num. Grades SMS sent	0.013	0.017*	0.006	0.499**	0.004	0.021***	0.003	0.007	-0.003*	0.010***	-0.007***
	(0.009)	(0.010)	(0.004)	(0.206)	(0.003)	(0.007)	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)
Mean of treatment	4.582	6,737	6,737	6,784	6,780	6,784	6,784	6,784	6,784	1,435	1,435
Num. Attendance SMS sent	0.003	0.004*	0.002*	0.125**	0.001	0.005***	0.001	0.002	-0.001**	0.003***	-0.002***
	(0.002)	(0.002)	(0.001)	(0.050)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Mean of treatment	18.04	14.62	14.62	14.58	14.59	14.58	14.58	14.58	14.58	17.99	17.99
Num. Behavior SMS sent	0.013	0.017*	0.006	0.493**	0.004	0.021***	0.003	0.007	-0.003*	0.010***	-0.007***
	(0.009)	(0.010)	(0.004)	(0.203)	(0.003)	(0.007)	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)
Mean of treatment	4.582	3.583	3.583	3.573	3.577	3.573	3.573	3.573	3.573	4.567	4.567
N observations											
Unique observations	1439	1439	1439	1447	1446	1447	1447	1447	1447	1435	1435
Lagged Dependent Variable	X	X		X	X						

Columns report the intent-to-treat (ITT) estimate and standard error (in parenthesis) of number of SMSs of each type sent on outcomes measured by the end of 2014. All columns include section fixed effects and standard errors are calculated allowing for clustering at the section level. Outcomes in columns [1], [10] and [11] are measured once at the end of the year. Outcomes in all other columns are monthly measures; these regressions additionally include month fixed effects. Regressions in columns [1], [2], [4], and [5] control for the baseline value of the dependent variable. If baseline values are missing we impute using the mean and flag these observations with an imputed dummy in the regression. In Columns [7], [8] and [9], the outcomes are the share of total notes (positive, negative, or extremely negative) reported on each student by the teacher during the month. See data description for more details of these variables. Sample size varies due to missing values. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Time-varying effects of the treatment assignment math grades

	[1]	[2]	[3]	[4]
Monthly math grade in	*September*	*October*	*November*	*December*
SMS treatment	0.126**	0.0890*	0.0779*	0.0686
	(0.0536)	(0.0488)	(0.0439)	(0.0428)
N	1,299	1,326	1,330	1,330
R2	0.436	0.460	0.462	0.467
Control mean	5.057	5.117	5.166	5.147
Control s.d.	1.082	1.007	0.888	0.862

Outcomes are math grades measured in September, October, November and December (marginal math grades in that month, not cumulative math GPA). Columns report the intent-to-treat (ITT) estimate and standard error (in parenthesis) of being assigned to the SMS treatment (SMS). All columns include section fixed effects and baseline overall GPA measured in 2013. Robust standard errors are clustered at the section level. Sample size varies due to missing values of the outcome or (student grade>class mean) variables. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9: Effects of treatment on parent misinformation about grades

	[1]	[2]	[3]	[4]	[5]	[6]
Information gap measure:	Parent report - School report, end of year grade		|Parent report - School report|, end of year grade		Parent misreports end of year grade (0/1)	
Baseline information gap~	0.447***	0.511***	0.299***	0.360**	0.156**	0.220**
	(0.118)	(0.152)	(0.113)	(0.147)	(0.0770)	(0.0961)
SMS Treatment indicator	-0.127*	-0.123*	-0.101*	-0.0916	-0.0876	-0.0743
	(0.0667)	(0.0671)	(0.0582)	(0.0608)	(0.0583)	(0.0606)
SMS*Baseline information gap~		-0.166		-0.161		-0.146
		(0.253)		(0.229)		(0.153)
N observations	412	412	412	412	412	412
R2	0.362	0.362	0.309	0.310	0.269	0.271
Mean of control	0.402	0.402	0.492	0.492	0.525	0.525
S.d. of control	0.610	0.610	0.539	0.539	0.501	0.501
p value of the sum of SMS vars		0.257		0.248		0.136

Each column shows output from a regression of the relevant measure of parent information gaps about student grades on a treatment indicator and various controls. Outcomes are: the exact and absolute difference between a parent's report of child grade and the school reported grade, and an indicator for whether the parent gets the grade wrong by more than 0.5 points. Each misinformation gap measure is captured at follow up (the outcome) and baseline (a control in every regression). The baseline measure is interacted with the SMS treatment indicator and included as a control in even-numbered columns. Each regression includes section fixed effects and robust standard errors are clustered at the section level. Sample consists of parents who responded to baseline and follow up surveys. Responses are balanced across groups. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Effects of treatment on parental willingness to pay (WTP) for Papas al Dia

	[1]	[2]	[3]	[4]	[5]	[6]
Outcome: Parent is willing to pay (0/1) for continued SMS program	Full sample	Control	Treatment	Full sample, no section FE	Full sample, section FE	Full sample, section FE, baseline grades<6.5
Randomized price: High price	-0.227***	-0.277***	-0.197***	-0.304***	-0.268***	-0.309***
	(0.048)	(0.076)	(0.066)	(0.063)	(0.068)	(0.066)
Randomized price: Medium price	-0.182***	-0.156**	-0.245***	-0.195***	-0.156**	-0.193***
	(0.048)	(0.076)	(0.071)	(0.064)	(0.068)	(0.073)
Randomized price: High price*SMS				0.136*	0.097	0.188**
				(0.080)	(0.088)	(0.092)
Randomized price: Medium price*SMS				-0.014	-0.049	0.029
				(0.086)	(0.089)	(0.091)
Constant	0.702***	0.697***	0.730***	0.719***	0.694***	0.721***
	(0.027)	(0.044)	(0.038)	(0.045)	(0.043)	(0.046)
N	734	368	366	734	734	632
Y mean	0.569	0.552	0.587	0.569	0.569	0.584

Outcome is a dummy for whether the parent/guardian reports being willing to pay for continued SMS service (4 SMS mesages per month from the school) after the end of the year. Sample includes all parents who returned a parent survey at follow up, in December 2014. Outcomes are measured at the end of 2014. Columns report the intent-to-treat (ITT) estimate and standard error (in parenthesis) of being assigned a particular randomized priced (1,500CLP, 1,000 CLP or 500CLP, the omitted category), and interactions of these randomized prices with the randomized SMS treatment. All columns except column (4) include section fixed effects. Column (6) restricts to the sample of parents whose children scored below 6.5 on baseline grades. Robust standard errors are clustered at the section level. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table A.1

	Failed the grade end of 2013	*Dropped out by 2014*
Attendance rate 2013	-0.040***	-0.004
	(0.010)	(0.003)
End of year grade 2013	-0.789***	-0.078*
	(0.133)	(0.035)
Attendance*Grade interaction	0.007***	0.0008
	(0.002)	(0.000)
Age	-0.009*	0.0169***
	(0.004)	(0.004)
Female	-0.006	-0.002
	(0.004)	(0.005)
N	2,740	2,740
R2	0.34	0.044
Mean of outcome	0.059	0.011

Regression uses MINEDUC data for our experimental school sample, kids enrolled in grades 4-8 in 2013. Grade of enrollment and school fixed effects included in regression. Robust standard errors, clustered at school level. Mean repetition rate = 0.059. Partial R2 on attendance, grades, interaction: R=0.32

Table A.2: Baseline correlates of consent

	Consented to be part of the intervention	
	[1]	[2]
Age in 2014	-0.028	-0.023
	(2.66)**	(2.17)*
Female	-0.019	-0.017
	(0.980)	(0.930)
Grade 5 in 2014	-0.054	
	(1.640)	
Grade 6 in 2014	0.032	
	(0.840)	
Grade 7 in 2014	0.043	
	(0.960)	
Grade 8 in 2014	0.089	
	(1.700)	
Grade, end of year 2013	0.054	0.068
	(2.75)**	(3.34)**
Grade missing	0.120	0.154
	(1.190)	(1.460)
Attendance share in 2013	0.619	0.484
	(6.06)**	(4.45)**
Attendance missing	-0.155	-0.128
	(2.37)*	(1.910)
Pass in 2013	-0.151	-0.153
	(2.96)**	(2.95)**
Pass outcome missing	-0.481	-0.551
	(4.92)**	(5.47)**
New Student in 2014	0.173	0.194
	(5.80)**	(5.38)**
N	2,720	2,720
R-squared	0.050	0.140
Mean of outcome	0.530	0.530
Including Section Fixed effects	No	Yes

Table shows coefficients from OLS regressions of an indicator for *"Did the individual and their parents consent to being part of the intervention at baseline"* on baseline characteristics. Characteristics are measured in December 2013 or June 2014. Regressions in column [2] include controls for section fixed-effects (strata), and the omitted grade is Grade 4. Robust standard errors are clustered at the section level. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Are baseline grades and attendance correlated with share of SMS messages received?

	[1]	[2]	[3]	[4]	[5]
		Sample of treated students			*Full sample*
Type of message successfully delivered:	Attendance	Behavior	Grades	General	General
Grades in 2013	0.0262	0.0317	0.0218	0.0144	0.00379
	(0.019)	(0.021)	(0.021)	(0.024)	(0.016)
Attendance share in 2013	-0.108	-0.129	-0.126	-0.217	-0.067
	(0.135)	(0.154)	(0.155)	(0.180)	(0.116)
N	1,931	1,928	1,929	1,284	2,627
Mean of outcome	0.712	0.706	0.729	0.674	0.681

Outcomes are the share of all SMS messages of a certain type that were sent and received in a given month. Months include September through end of November 2014. Each specification controls for month and section fixed effects, robust standard errors are shown.

Appendix 1: Experimental Protocol for *Papas al Dia*

This appendix describes the experimental protocol for *Papas al Dia*. It sets out the timeline for the overall project, the sampling frame and recruitment, randomization and data collection.

1. *Timeline*

The school year coincides with the calendar year in Chile. Schools open in March, and end in December. Winter vacation occurs in July, summer vacation is from December through the end of February.

<u>Table A.1: Timeline</u>

Timeline of main study activities	**Timeline of school year**
Year 1	
Jan-April 2014: School recruitment	March 2014: School year starts
May 2014: Parent recruitment	
May-middle June 2014: Baseline survey for consented participants administered in schools (to students) and sent home with students (for parents). Parent surveys collected from schools. May 23, 2014 – Welcome message to 7 schools	June 17-end June 2014: World Cup soccer, vacation days
June 2014: consented students randomized into treatment and control groups	
Mid-June-December 2014: SMS information treatments sent to parents each week. Control messages: general messages sent occasionally to all participating parents. Treatment messages: weekly attendance SMS (started June 13^{th} 2014), grade SMS (started July 14^{th} 2014) and behavior SMS (started July 9^{th} 2014) every two weeks, general messages sent occasionally. Welcome message to 8^{th} school (July 28^{th} - 2014). First messages sent for attendance (August 1^{st} 2014), behavior (August 12^{th} 2014) and math grades (August 11^{th} 2014)	Early to middle July 2014: Winter vacation period
End September 2014: DVD treatment administered to all parents in treatment group	Middle July 2014 – early December 2014: second semester
End November 2014: First follow up survey to participant parents and students	

Year 2	
Jan-March 2015: update classroom rosters and recheck parent contact (cellphone) details	March 2015: Start of first semester of school year
March-November 2015: continue sending messages to parents of treated students who remain in sample schools	July 2015: Winter vacation
WHEN in November 2015: final follow up survey to participant parents and students	

2. *Sampling frame*

We worked with a particular municipality in Chile and sampled all of the public and public-private schools in the municipality. The principal consented to work with our program in 7 schools. We added an 8th school to be sure we had enough sample for the intervention. All students enrolled in grades 4 through 8 in 2014 in each of these schools were included in the study. This sampling created 85 separate grade-class combinations. We invited parents of all children in these grades to participate in the project.

3. *Consent*

We introduced parents to *Papas al Dia* at school meetings located at school premises in May and June 2014, and collected consent forms at these meetings. Since school meetings were not always well attended, we also sent project introduction materials and consent forms home with students and followed up by phone to get verbal and written consent. 53% of parents consented to participate and consent rates were very similar across grade levels.

Table A.2: Consent rates by grade

Grade level	**N kids enrolled in March 2014**	**N parents consented**	**Share consented**
Grade 4	469	269	.57
Grade 5	501	247	.49
Grade 6	550	297	.54
Grade 7	593	311	.52
Grade 8	607	323	.53
Total	2,720	1,447	.53

4. *Experimental and non-experimental groups, stratification and randomization*

We randomized in two stages. Taking the 85 grade-school classes, we first stratified by school-grade level and randomized classes within each grade to be treated at a high or low share. That is, each class within a grade received an assignment from this set: ShareTreated={High=0.75, Low=0.25}. In the second step, we randomized consented participants within each class into one of two groups: SMSTreated=1 or SMSControl=0. Treatment status at the individual level followed the student as long as they remained in the same school.

After the first two and half months of the intervention, we randomized all classes in each grade-school strata into DVDTreated=1 or DVDTreated=0. DVDs were then delivered to all students within DVDTreated=1 classrooms, regardless of their SMS Treatment status.

Our sample and treatment groups break down in Table 3:

Table A.3: Distribution of sample into treatment and control groups

Groups	**High treatment share classes (ShareTreated=0.75): N Classes=37**	**Low treatment share classes (ShareTreated=0.25): N Classes=48**
SMSTreatment	N=488 (N=253 also got DVD, 235 did not)	N=222 (N=126 also got DVD, 93 did not)
SMSControl	N=146 (N=77 also got DVD, 69 did not)	N=591 (N=344 also got DVD, 247 did not)
Non-participants: parents who did not consent to participate	N=538	N=735

Our randomization algorithm involved 10,000 re-randomizations and we chose the randomization seed that produced the maximum of the minimum *p* values used in the joint test of balance of baseline variables across treatment groups.

5. *Data collection*

i. *Parent and student surveys*

We administered surveys to all participating parents and all children in all grades. Surveys were administered at baseline, at midline (end of year 1) and endline (end of year 2). Child surveys were administered in class; parent surveys were administered at the first parent meeting or sent home with children and incentivized to be returned to the school. Baseline parent surveys collected information on what parents knew about their child's attendance (questions were for a specific child in our sample), grades and behavior; their level of involvement with the school and the child; demographics and economic characteristics; and any concerns they had with schooling. Baseline child surveys collected demographics, self-reported engagement in schooling, engagement of parents, and information on their peer networks within the classroom. We also tested them on a few age-appropriate simple math problems.

Follow up survey data collected a similar set of variables and included some questions specifically about the intervention. For example, we asked questions about whether parents had ever received any information from schools via SMS, and we asked parents how much they were willing to pay (WTP) to continue receiving SMS messages from school. We randomly assigned one out of three WTP amounts to this question for each parent.

ii. *Administrative data*

Through the life of the project, we collected administrative data from each school, weekly. Project teams collected information on attendance, grades, and behaviors for all children in all grades by photographing attendance and behavior notebooks, and collected school records on all recent math tests. We digitized these data, and uploaded to a platform that turned the information into SMS messages for the treatment groups.

For students who left our sample schools during or at the end of the first year, we collected their aggregate data on attendance and grades (subject-specific GPA) from the municipality records. This allowed us to fill in missing data for attritors.

Over the long run, we plan to use administrative data on SIMCE scores and MINEDUC data to track achievement on standardized tests, school switching, grade progression/failure, high school completion and dropout.

iii. Data on parent attendance at school meetings

For 2015, we collected school records of parent attendance at school meetings for as many schools as possible (not all schools kept records on parent attendance at these meetings). We digitized this information and matched it to our sample using parent reports of the name and Chilean identity number of their child.

Appendix 2: Treatment Protocol for *Papas al Dia*

This document describes the two treatments in the *Papas al Día* intervention and challenges encountered in implementing the treatment in the field.

1. *Information treatment: Weekly SMS messages*

We sent all participants, including controls, a welcome text message to introduce *Papas al Día* and let them know they might expect further free messages from their child's school. In Chile, receiving an SMS message is free. The child was mentioned by name. This message helped identify valid cell numbers for caregivers. We used the failure rate from these welcome messages to follow up and correct cell phone numbers for undelivered messages.

The format for messages sent between June-December 2014 is given in Table 1 below. Messages were populated with relevant child-specific data from school records collected on all enrolled children by our research team. The relevant school contact visited their assigned school once per week to digitally enter the administrative data to our platform. The school surveyors had no knowledge of the randomization design of the program. Once the data were entered, a team leader assigned information to treated students and automated the SMS message sending according to the above timeline.

2. *Challenges to implementation*

i. Identifying the primary caretaker as the treated unit

One of the issues arising in the field was how to define the primary caregiver. While one parent is usually identified as the main person responsible for the child at the school, it is often the case that a different person attends parent-teacher meetings, e.g. a grandmother or aunt. In our initial consent request, we asked consent from the person *responsible* for the child's schooling, and allowed the consenter to give permission for a different adult to receive the regular SMS messages about this child.

ii. Tracking cell phone numbers

Tracking cellphone numbers was important for being able to deliver the SMS messages. We updated cellphone registers for participating parents WHEN and HOW?

iii. Minimizing failed SMS messages

Conditional on having the correct cellphone number for a participating parent, an SMS message may not have been successfully delivered because (1) the cell phone was switched off for too long (messages are resent three times before being discarded (2) the cell phone could not receive any more messages (3) some other reason related to the inability of the network to get the information through. We discovered that a large share of messages were not being received when they were sent on Friday night. In August 2014, we switched to sending the attendance messages on Mondays. After this change, the delivery success rate stabilized around 60% for all messages.

Table A2.1: SMS Message texts

Message type	Frequency	English text	Spanish text
1. Attendance (Treatment group only)	One SMS per week to EVERY parent/guardian in treatment group, sent EVERY FRIDAY, 19:30.	"[Caregiver]: school records indicate that {ChildName} attended {AttendDays} of{ValidDays} school days this week. Papás al Día"	"[Apoderado]: los registros del colegio indican que {Alumno} asistio {dias de asistencia} de {dias validos} dias de clase de esta semana. Además fue enviado x veces a inspectorial. Papás al Día"
2. Behavioral misconduct – monthly (Treatment group only)	One SMS per month to EVERY parent/guardian in the treatment group, first TUESDAY of EVERY MONTH, 19:30	"[Caregiver]: school records indicate that last month, {Name} had {NumPos} positive behaviors and {NumNeg} negative behaviors in class. Papás al Día"	"[Apoderado]: los registros del colegio el ultimo mes indican que {Alumno} tuvo {NumPos} anotaciones positivas y {NumNeg} negativas en el colegio. Papás al Día"
3. Grades for recent math tests – at most monthly (Treatment group only)	One SMS per month to EVERY parent/guardian in the treatment group, first TUESDAY of each month at 19:30	"[Caregiver], according to school records, {Name} scored {TestScore} on their last math test. The class average was {ClassScore}. Papás al Día"	"{Apoderado}: segun los registros del colegio, {Alumno} obtuvo un {N} en su última prueba de matematicas. El promedio del curso fue {N}. Papás al Día."
4. Messages to encourage continuation (Treatment and Control)	One SMS per month, to EVERY PARENT/guardian in ALL GROUPS, first MONDAY of the month , 19:30	"[Caregiver], The next {Meeting Type} at school {SchoolName} is {DATE} at the (hour). Do not miss it! Papás al Día"	"{Apoderado} el próximo {fecha} a las {hora} se realizará {actividad} en el colegio [nombre del colegio] ¡No te lo pierdas!. Papás al Día "

Appendix 3 Table A3.1: Student Scale – Baseline

Scale	Variable	Loadings
Study habits	I always study for the exams	0.6338
Eigenvalue: 2.298	I spend free time doing homework and study	0.5307
Cronbach's Alpha: 0.772	I try to do well my school work even though I do not find interesting	0.4966
	If I must study I do not spent time with friends	0.4514
	I always know the homework that I must present	0.5793
	I organize well my time to do my school work	0.7322
	I can organize school tasks and spent time with friends and family	0.5412
Academic efficiency	I am sure that I can dominate all the school subjects	0.7015
Eigenvalue: 2.364	I am sure that I can understand the hardest things	0.7815
Cronbach's Alpha: 0.811	I can do almost all the work or I give up	0.5624
	Even though subjects are hard I can learn	0.7070
	I can do the hardest homework if I try	0.6668
Bad behaviour	Sometimes I bother teachers during classes	0.6450
Eigenvalue: 2.031	Sometimes I have problems with my teachers	0.6021
Cronbach's Alpha: 0.770	Sometime I behave for some way that bother teachers	0.6869
	Sometimes I do not follow instructions during classes	0.5506
	Sometimes I misbehave during classes	0.6909
Family support	My parents or guardian checked that I really made my homework	0.5014
Eigenvalue: 2.350	My parents or guardians motivated me to work hard at school	0.5251
Cronbach's Alpha: 0.784	My parents or guardians supported me in activities outside school	0.6116
	My parents or guardian heard me when I needed to talk with them	0.5593
	My parents or guardians showed that they were proud of me	0.7741
	My parents or guardians helped me to take decisions	0.7329
Family suppervision	I went alone to school*	0.7768
Eigenvalue: 1.492	My parents or guardians checked the behaviour and attendance book	0.1898
Cronbach's Alpha: 0.566	I returned to home alone*	0.7621
	I stayed alone at home without adult supervision *	0.3583
	I left home without letting know my parents where I went or with who I was*	0.3064
	I allowed that my parents or guardian spoke with my school friends	0.0492
	I went to school and did not enter or left home saying I will not assist*	0.1548
	I signed in school but I left before class' end*	0.1541
Parent School Involvement	My parents or guardians met with school's director	0.5772
Eigenvalue: 1.816	My parents or guardians met with school teachers	0.5198
Cronbach's Alpha: 0.670	My parents or guardian contacted the director through e-mail	0.6642
	My parents or guardians contacted teacher trough e-mail	0.6525
	My parents or guardian went to school meetings	0.0915
	My parents or guardians went to school events	0.4003
	My parents or guardians volunteered at school	0.4204
Positive reinforcement	My parents or guardian thanked me for helping with housework	0.5823
Eigenvalue: 3.552	My parents or guardians told me they have fun with me	0.7200
Cronbach's Alpha: 0.870	My parents or guardians congratulated me for my effort	0.8190
	My parents or guardians told me that I have outstanding qualities	0.5754
	My parents or guardian told me that they were proud of me	0.7833
	My parents or guardians congratulated me for having done well or having improve	0.7419
	My parents or guardians encouraged me when I was doing something hard	0.7269

Appendix 3 Table A3.2: Guardian Scale – Baseline

Scale	Variable	Loading
Study habits	My son always studies for the exams	0.6740
Eigenvalue: 3.071	My son spends free time doing homework and study	0.6133
Cronbach's Alpha: 0.834	My son tries to do well school work even though he do not find interesting	0.6429
	If my son must study he does not spent time with friends	0.4388
	My son always knows the homework that he must present	0.6156
	My son organizes well my time to do my school work	0.8477
	My son can organize school tasks and spent time with friends and family	0.7328
Academic efficiency	I am sure that my son can dominate all the school subjects	0.7616
Eigenvalue: 2.724	I am sure that my son can understand the hardest things	0.8180
Cronbach's Alpha: 0.843	My son can do almost all the work or he gives up	0.4733
	Even though subjects are hard my son can learn	0.7508
	My son can do the hardest homework if he tries	0.8290
Bad behaviour	Sometimes my son bothers teachers during classes	0.7342
Eigenvalue: 2.525	Sometimes my son has problems with his teachers	0.6967
Cronbach's Alpha: 0.831	Sometime my son behaves a way that bother teachers	0.7671
	Sometimes my son does not follow instructions during classes	0.5759
	Sometimes my son misbehaves during classes	0.7621
Family support	I checked that my son really made his homework	0.5539
Eigenvalue: 2.124	I motivated my son to work hard at school	0.5074
Cronbach's Alpha: 0.749	I supported my son in activities outside school	0.5046
	I heard my son when he needed to talk with him	0.5317
	I showed that I was proud of my son	0.7094
	I helped my son to take decisions	0.7202
Family suppervision	My son went alone to school*	0.7384
Eigenvalue: 1.612	I checked the behaviour and attendance book	0.2250
Cronbach's Alpha: 0.566	My son returned to home alone*	0.8535
	My son stayed alone at home without adult supervision *	0.4091
	My son left home without letting me know where he went or with who he was*	0.2451
	My son allowed that I speak with his school friends	0.1722
	My son went to school and did not enter or left home saying he will not assist*	0.1393
	My son signed in school but he left before class' end*	0.1050
Parent School Involvement	I met with school's director	0.6206
Eigenvalue: 1.756	I met with school teachers	0.4878
Cronbach's Alpha: 0.642	I contacted the director through e-mail	0.6642
	I contacted teacher trough e-mail	0.6799
	I went to school meetings	-0.0668
	I went to school events	0.3182
	I volunteered at school	0.3512
Positive reinforcement	I thanked my son for helping with housework	0.4599
Eigenvalue: 3.054	I told my son I have fun with him	0.5863
Cronbach's Alpha: 0.838	I congratulated my son for his effort	0.7820
	I told my son that he has outstanding qualities	0.6507
	I told my son that I was proud of me	0.7473
	I congratulated my son for having done well or having improved	0.6830
	I encouraged my son when he was doing something hard	0.6622

Notes: Students and parents could give categorical answers to each question, of the type "strongly agree," "agree," etc.. We aggregated student and parent answers into scales (indices) using a maximum likelihood (ML) principal components estimator where only one latent factor was retained to describe all responses to the same category of questions. These models were estimated on the control sample only and the results applied to the full sample. Column 1 in Tables A3.1 and A3.2 present the eigenvalue of each latent factor and Column 3 shows the loading associated with each variable. The tables also indicate the Cronbach's alpha reliability coefficient that each scale had in our sample. After the prediction was computed to produce each scale, we standardized them using the mean and standard deviation of the control group. A unit of the index can therefore be interpreted as a standard deviation unit.