Journal of Public Economics 186 (2020) 104185

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Providing performance information in education: An experimental evaluation in Colombia☆

Felipe Barrera-Osorio [a,*], Kathryn Gonzalez [b], Francisco Lagos [c], David J. Deming [d]

[a] Harvard Graduate School of Education (HGSE), United States of America
[b] HGSE, United States of America
[c] University of Maryland, College Park, United States of America
[d] HGSE and Harvard Kennedy School of Government, United States of America

ARTICLE INFO

Article history:
Received 19 April 2019
Received in revised form 27 March 2020
Accepted 28 March 2020
Available online 20 May 2020

JEL classification:
I21
I24
I28
O10

Keywords:
Education
Randomization
Information
Parents' beliefs
Quality of education
Colombia

ABSTRACT

We conducted a randomized controlled trial of an information intervention to evaluate the effects of providing information to families on their children's reading and math achievement in a mid-size city in Colombia. Most families are poorly informed about their children's performance, but our information intervention closes the gap between beliefs and performance and induces some behavioral response among parents in the treatment group. We find positive impacts on student achievement of 0.09 SD to 0.10 SD in the first two semesters after treatment, followed by fadeout in year two. This overall pattern is driven by large gains—around 0.28 SD—and then similarly complete fadeout for students with low baseline test scores.

1. Introduction

In the human capital model, individuals invest in education if the present value of benefits exceeds costs (Becker, 1962). As such, information about the quality of education and student performance is important, since the benefits of education depend on the actual acquisition of skills in the classroom. Nonetheless, evidence suggests that parents have limited information or are misinformed about school quality, their children's academic performance, and the returns of education (Nguyen, 2008; Jensen, 2010; Loyalka et al., 2013). This lack of information may lead to suboptimal educational investment by households (Houtenville and Conway, 2008; Avvisati et al., 2014; Bergman, 2015; Berlinski et al., 2016; Dizon-Ross, 2019). Providing performance information to parents may cause them to update their beliefs, which could lead to changes in parents' investment of time and financial resources in their children's education, and ultimately to increases in student achievement.

In this paper, we study the impact of providing families with standardized information about their child's own performance in school in a mid-size city in Colombia. In association with a local foundation (The Luker Foundation) and the Secretary of Education, we collected baseline data on the Early Grade Reading and Early Grade Math Assessments (EGRA and EGMA) of children in grades four through six in 31 public schools in the city. We visited the households of students in the sample to collect household socioeconomic information as well as information on parents' beliefs about the performance of their children on EGRA and EGMA. We randomly assigned some treatment families to receive standardized information about the actual performance of their children at the end of the household interview.

☆ This project was done in partnership with the Luker Foundation, Manizales, Colombia. The commitment and professionalism of Santiago Isaza, Maria Camila Arango and Angela Trujillo, from Luker, made this project possible. We are grateful as well with the support of the Secretary of Education of the city, and all participant public schools. We are indebted to Alejandro Ganimian, Karen Mapp and Richard Murnane for valuable suggestions, as well to participants in seminars at Harvard and NEUDC and APPAM conferences. We received financial support from the Weiss Family Program Fund for Research in Development Economics; from the Dean's Venture Fund Award at HGSE and from the Luker Foundation. The study was approved by Harvard's IRB (IRB14-2842). The trial was registered at the American Economic Association archive, AEARCTR-0002192.

* Corresponding author.
E-mail addresses: felipe_barrera-osorio@gse.harvard.edu (F. Barrera-Osorio), kgonzalez01@g.harvard.edu (K. Gonzalez), flagosm@umd.edu (F. Lagos), david_deming@harvard.edu (D.J. Deming).

https://doi.org/10.1016/j.jpubeco.2020.104185
0047-2727/

However, the provision of information alone may not affect behavior if parents do not have knowledge about specific actions to take in response to the information. Research shows that providing parents with information regarding their children's academic performance can impact parent investment behavior and improve students' academic performance (Berlinski et al., 2016; Dizon-Ross, 2019). At the same time, recent research also shows that the provision of information can most effectively induce optimal decision-making if individuals receive actionable advice regarding how to use the information, in addition to the information itself (Cortes et al., 2018; Doss et al., 2018; Kraft and Rogers, 2015). The personalized information about student performance was therefore bundled with additional information providing suggestions about how parents could engage with their children's education. We presented these families with a menu of options that they might consider in light of the information. These options ranged from asking their children about school every day to encouraging children to read and write more often. We also encouraged treatment families to strengthen their relationship with the school by interacting with their children's teachers more often.

Our study makes two contributions to the literature. First, we contribute to the limited literature that examines the impacts of providing information about student performance – bundled with actionable information about how parents can engage with their children's education – on subsequent academic achievement. While several studies have examined the impact of information provision on student learning and achievement outcomes, many focus on providing information on the quality of schools (Andrabi et al., 2017; Banerjee et al., 2010), target specific student behaviors such as attendance (Rogers and Feller, 2018), or provide information to parents on a range of student outcomes (Berlinski et al., 2016). A smaller stream of literature examines the impact of providing information to parents about their children's academic performance (Bergman, 2015; Dizon-Ross, 2019; Doss et al., 2018). Second, we collect longitudinal information on student learning outcomes, allowing us to examine the dynamics of information provision. We consider the impacts of the provision of information in both the short-term (e.g., after two months) and longer-term (e.g., after more than one year), allowing us to examine the timing of when the impacts of information provision on student learning outcomes emerge and fade out. In doing so, we provide insight into the scalability of information interventions by demonstrating whether impacts persist over time in the absence of continued intervention.

Our results show an initial pattern of small and significant short run effects following the receipt of information on student performance and the menu of options to support parent investment (0.09 SD to 0.10 SD) and then zero effects in the subsequent follow-up waves. This suggests that parents respond to information by increasing their effort, but this initial response decays unless new reports are available to them. This pattern is consistent with effects that follow a pattern of action and backsliding observed in other interventions (for instance, Gallagher, 2014 in the case of insurance, and Allcott and Rogers, 2014 in the case of electricity bills). We find increases in the number of parent-teacher meetings and an update in parents' beliefs. We present some evidence suggesting that this last effect is the main channel of impact. In contrast, we do not find any effect on parent investment within the household. One explanation for the lack of effects on these “internal” investment mechanisms is that all measures that proxy these variables were quite high at baseline, according to parents' self-reported answers.

The second important finding of the paper is that the results are larger for students with low baseline scores (of the order of 0.28 SD, at the peak of effects). This is consistent with these students and families having less accurate information about performance, or alternatively an increase in parent-student information frictions in these households (Bergman, 2015). Still, the same pattern in the dynamics of effects is detected for this population, with a backsliding to the baseline score. We also randomly provided some teachers in the 31 schools with information about their students' academic performance. Like the intervention with parents, we encouraged teachers to engage with the families to talk about these results. However, we find no impact on student achievement of providing teachers with performance information.

In the next section, we present related literature; in Section 3 we present the description of the experiment; Section 4 discusses the data and sample. Section 5 presents the analytical plan. Section 6 shows the main results and Section 7 closes with conclusions.

2. Related literature

Parental investment in education, namely the financial resources and time that is devoted by parents to support, monitor, or induce more effort in their children, has been identified as one of the main determinants of students' educational outcomes (Avvisati et al., 2014; Houtenville and Conway, 2008; Todd and Wolpin, 2007). Investment decisions in education critically depend on the information that is available to parents (Jensen, 2010), such that information failures may result in suboptimal investments, especially among low-income families (Dizon-Ross, 2019). Recent interventions in developed and developing countries that provide information to parents have not only demonstrated positive effects on enrollment decisions and several student outcomes, including attainment and achievement in standardized tests, but have also been shown to be cost-effective (Ganimian and Murnane, 2016; Kremer et al., 2013).

These information interventions can be broadly divided in four types, depending on what type of information is provided to parents: information about the returns to schooling (Jensen, 2010; Nguyen, 2008); information about the quality of educational institutions (Andrabi et al., 2017; Banerjee et al., 2010; Hastings and Weinstein, 2008); information about parenting strategies (Mayer et al., 2015; York et al., 2018); and, information about the ability, behavior, and/or academic progress of their children (Bergman, 2015; Berlinski et al., 2016; Dizon-Ross, 2019; Rogers and Feller, 2018). We focus here primarily on this fourth type of intervention.

In the process of making educational investment decisions, parents face at least two sources of information asymmetry. First, school staff and students themselves have information that would allow parents to make adequate educational investment decisions. But this information is not completely or timely disclosed to them due to misaligned incentives or strategic behavior from schools (Berlinski et al., 2016) or their children (Bergman, 2015). Schools may not have incentives to reveal, for example, how well a student is performing in comparison to their peers in the same school or in the city, or how to better support the student's academic progress. Second, students may be inclined to strategically disclose (hide) positive (negative) information to their parents. Students may be incentivized to provide information to their parents about the subject areas in which they are doing well but not the subject areas in which they need the most support. For example, students may be more likely to share information about performance on a test when they perform relatively well. This type of information asymmetry between children and parents in turn prevents parents from optimally targeting investment resources (Dizon-Ross, 2019). Interventions that provide information to parents about students' ability, behavior, or academic progress not only aim to close this information gap (Berlinski et al., 2016), but also to correct parental misbeliefs about students abilities or behavior (Dizon-Ross, 2019; Rogers and Feller, 2018) and to reduce information frictions between parents and their children's academic progress (Bergman, 2015), all of which affect educational investments.

Theoretically, once information is available to parents, they may update both their amount of effort and how it is allocated. Recent evidence confirms this hypothesis: providing information on students' ability, behavior, or academic progress not only reduces absenteeism (Berlinski et al., 2016; Rogers and Feller, 2018) and the prevalence of disruptive behaviors (Berlinski et al., 2016), but also improves educational achievement, as measured by test scores and graduation rates (Bergman, 2015; Berlinski et al., 2016; Dizon-Ross, 2019).

Finally, while recent studies in this area have asked about longer-run effects of information provision, evidence is still scarce. One exception is Allcott and Rogers (2014), who examine the effect of information in the context of an energy conservation initiative. The authors find an '*action and backsliding*' pattern of impacts: individuals respond to information in the short term, but the initial response decays in the absence of continued information provision. This pattern of action and backsliding has been observed in other studies of information provision (Gallagher, 2014 in the case of insurance). However, there is little evidence regarding this phenomenon in the context of information interventions that provide information to parents about students' academic progress.

3. Context, experimental design and timing

3.1. Context

The city of Manizales, capital of the Department of Caldas in central Colombia, is a mid-size Colombian city, with population close to half a million, and approximately 55,000 students in public basic education in grades 1 to 11. The local authorities deem education as a priority. In addition, the city has a very active civic society that is also very engaged in education policy. Authorities are interested in tackling the perceived low quality of education, as shown in national and international standardized tests. The public-school system in Manizales includes 57 schools. In the present study we focus on 31 schools serving grades four, five, and six.

We worked in close collaboration with the Luker Foundation and the Secretary of Education of the city. The Luker Foundation was created in 1994 "as a private, non-profit organization, by the interest of a Manizales company to transform its city into a better place to live and educate children and young people" (see more information at https://fundacionluker.org.co/en/home/#). The Secretary of Education is the highest authority in education in the city. The Secretary, Luker and the research team contacted the schools and requested permission to conduct the experiment. After we had received permission to conduct the experiment, we obtained consent from families of the students. We contracted with a survey company to carry out home visits and data collection.

3.2. Treatment

Our study combined efforts to provide information to parents with a family engagement intervention, using an experimental design. Students in grades four and five were randomly assigned to one of two treatment groups or a control group. Randomization was done at the student level after blocking by grade. In the first treatment condition (Individual student performance; Treatment Group 1), we conducted a home visit (described below) and provided families with a one-page report card that showed their child's performance, as well as the child's position relative to the average performance of students in the same grade and school. The information provided a ranking of the student's performance relative to the school and was presented in a way that was highly salient to all families (e.g., showing the students and school performance on the same number line). In the second treatment condition (Individual student performance in the city; Treatment Group 2), during the home visit parents received a one-page report card that showed their child's performance, as well as their relative position to the average performance of students in the same grade in the entire city. Finally, no information was provided to the control group. Examples of the individual information report card provided to families in each of the two treatment conditions are presented in Figs. 1 and 2.

Although both treatments received individual information, we anticipated that impacts of the two treatments could differ. The key difference between the two treatment groups was the information parents received about students' relative performance (student performance relative to the school vs. student performance relative to the city). In the first treatment condition, we expected that parents would interpret information about their children's performance relative to other students in the same school as indicative of whether students were performing on par with their peers. In the second treatment condition, we hypothesized that parents might interpret information on their children's performance relative to students across the city not only as a measure of student achievement, but also as a signal of school quality. We hypothesized that this could affect parents how parents respond to the information.

Additionally, the report card provided to the two treatment groups included a list of suggestions for parents to engage with their children's education in both reading and mathematics. These included the following: recommendations on how parents can discuss their children's progress in school, and recommendations on how parents can incorporate math and literacy activities into the everyday routine. The list of recommendations we provided to parents is presented in Fig. 3.

We chose to bundle the personalized information about student performance with recommendations for parent actions for two reasons. First, the provision of information alone may not be the most effective way to improve student performance. Recent studies suggest that the provision of information is particularly effective at changing behaviors if individuals receive information about specific actions to take in response to the receipt of information (e.g., Cortes et al., 2018; Kraft and Rogers, 2015). In our context, parents may not know how to effectively respond to new information that their children are performing better or worse than believed. Parents may not know the production function of test scores and may not know how to increase investments to support their children's performance in specific subject areas. Results of evaluating a pilot version of the intervention provided further evidence for the importance of bundling the information with suggested actions. The pilot intervention only provided information about student performance and did not provide suggestions for recommended parent actions. After the completion of the pilot intervention both the foundation and several stakeholders (including schools and parents) suggested the need for the intervention to include clear guidance for parents for how to use this information.

3.3. Timing

The experimental design was divided in three phases: the pilot intervention, the full intervention, and a follow-up phase without additional intervention. In the first phase (the pilot intervention), we provided report cards on school and student performance to parents of students in grades four and five. In the second phase (the full intervention), we provided a new round of information to parents that was modified based on our findings from the pilot intervention, and additionally included a list of suggestions for parents on how to support their children. The effects on students' outcomes presented here come from the second phase of the experiment (the full intervention). In the third phase (the follow-up phase without additional intervention), we collected follow-up information on student performance. We did not provide additional information to families during this phase.[1] The components of each phase of the experiment are described in detail below.

The first phase of the study (the pilot intervention) began in 2014 and included 3026 students in grades four and five. The purpose of the pilot intervention was to test study procedures and refine the intervention design. In April 2014, the Secretary of

[1] During the third phase of the intervention we also incorporated an additional family-engagement component, focused on providing information to the teachers of students already in the experiment. This intervention led to null results. We do not report the results of that experiment here, but details of the intervention and results are provided in the Appendix A.

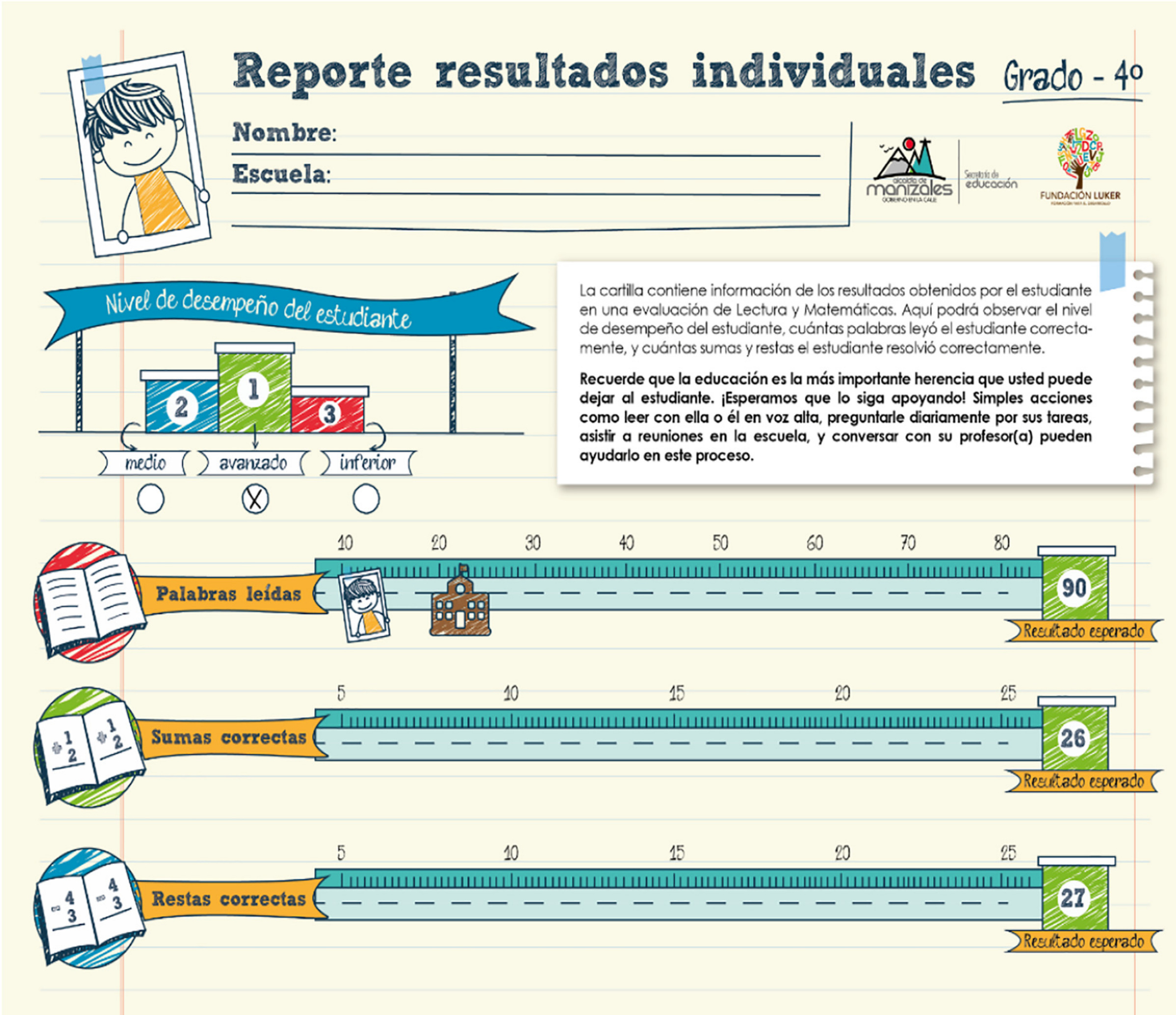

Fig. 1. Individual information report card in the full intervention (Treatment 1). *Note:* Example report card for students in Grade 4 at the time of the home visit. The report card indicates the individual student's performance and school performance on the reading, sums, and subtractions EGRA/EGMA subtests. The student's score and the school average score, relative to the maximum possible score, are shown on the number line.

Education of Manizales (SEM) and the Luker Foundation (LF), using the Early Grade Reading Assessment (EGRA) and Early Grade Math Assessment (EGMA), collected language and math test scores of students in grades four and five. We then visited the households of students in grade four and five in October 2014. In December 2014, at the end of the academic year, we administered a new round of EGRA and EGMA tests to all students in our sample. We see this first year (April 2014–December 2014) as a pilot of the treatment and operation, which we subsequently modified in the full intervention.[2] In this sense, the pilot was necessary to test the format of information provision (the report card format); the content of information provision (individuals versus school average); and the actual operation of visiting families and collecting information.

The second phase of the study (the full intervention) began in 2015. In June 2015, SEM and LF again collected language and math test scores for all the students in grades four, five and six. This included the 3026 students who were in grades five or six in 2015 (grades four or five in 2014), who participated in the pilot intervention. Therefore, the sample for the full intervention included students who participated in the pilot intervention. There was no re-randomization at the start of the full intervention, and students remained in the same treatment conditions in the pilot intervention and full intervention. In addition, SEM and LF collected language and math scores of 1345 students in grade four in 2015 (grade three in 2014). These students did not participate in the pilot intervention and were added to the study and randomized during the full intervention. We conducted a second home visit in October 2015 where we visited the households of students in grades four, five, and six in 2015 (grades three, four, and five in 2014). In December 2015, at the end of the academic year, we administered a new round of EGRA and EGMA tests to all students in our sample.

Follow up data occurred in 2016–2017; in this phase, we did not provide any additional information to households or conduct additional home visits. In June 2016 and December 2016, we administered new rounds of EGRA and EGMA tests to all students in our sample who were in grades three or four in 2014. In June 2017, we administered new rounds of EGRA and EGMA tests to all students in our sample who were in grade three in 2014. Timing of all data collection and study procedures are presented in Table 1 (see Fig. 4 and Appendix Table A1 for a more detailed description of the timing of the intervention and data collection).

[2] Based on the results of the pilot intervention, we modified the second treatment condition. In the pilot intervention, we provided information about school average performance relative to other schools in the city. In the full intervention, we instead provided information about student performance relative to other students in the city.

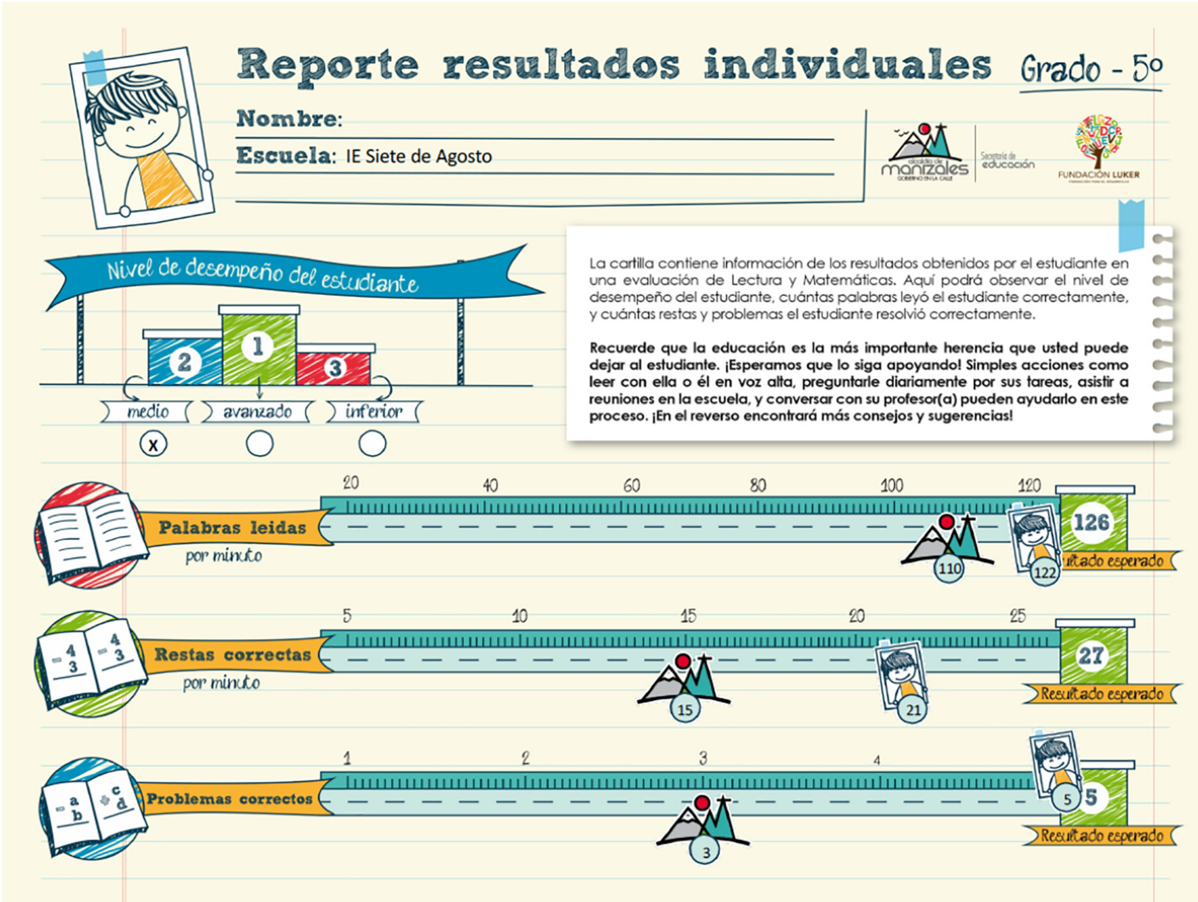

Fig. 2. Individual information report card in the full intervention (Treatment 2). *Note:* Example report card for students in Grade 5 at the time of the home visit. The report card indicates the individual student's performance and city performance on the reading, sums, and problems EGRA/EGMA subtests. The student's score and the city average score, relative to the maximum possible score, are shown on the number line.

3.4. Household visits

We visited parents and guardians at their homes at two time points, October 2014 and October 2015. Parents and guardians were contacted by phone before the visit. In that phone call, after confirming that the house corresponded with the house of the student and who the guardian of that student was, the enumerators briefly explained the purpose of the project, asked whether it was possible to schedule a home visit, and confirmed the home address. Enumerators called each household until the student's guardian was reached. While most visits were previously scheduled, when phone numbers were not available or not functioning, enumerators directly visited the home. In most cases, visits were rescheduled when the guardian was not present at the moment of the visit. Among the difficulties encountered by enumerators in their fieldwork, address accuracy, neighborhood safety, and presence of the guardian at the moment of the visit — even with scheduled and rescheduled visits — were the most common.

The visits were divided into three sections. First, the agent explained the objective of the study and provided the consent materials. Once the parent read the consent form, asked questions about the study and her or his participation, and signed the consent form, the visit continued. Second, the agent administered a questionnaire to the parent or guardian. One important piece of this questionnaire was to ask parents to state their beliefs for both student and school performance, using the same report card format that the treatment groups received, but without any information. We asked them to point to a place on a number line that represented where they thought their child and school were relative to the range of possible scores on the assessment. After the administration of the questionnaire concluded, the agent gave the appropriate report card to each treatment group and explained its meaning. For the control group, the visit concluded with the administration of the questionnaire (i.e., no information was provided).

We conducted the first home visit in October 2014, with students in grades four and five during 2014. During these home visits, families in the treatment group received information regarding their children's performance on the April 2014 EGRA and EGMA tests. Due to budget limitations, the first visit was conducted with a subsample of the full 3026 students and their families who were randomly assigned to one of the two treatment arms or to the control group. Specifically, we attempted to visit 2100 households: 1700 of the 2016 treated households, and 400 of the 1010 households assigned to the control condition. Of these 2100 households, 1077 received a home visit.

We conducted a second home visit in October 2015, with students in grades four, five and six in 2015 (grades three, four, and five in 2014). During these home visits, families in the treatment group received information regarding their children's performance on the June 2015 EGRA and EGMA tests, along with the suggestions for how to engage in their children's education. For students in grades five and six during 2015 (grades four and five in 2014), this was the second round of home visits. For students in grade four in 2015 (grade three in 2014), this was their first home visit. The full sample of 4371 students and their families who were randomly assigned to one of the two treatment arms or the control group were assigned to receive this home visit. Of these 4371 households, 2950 received home visit.

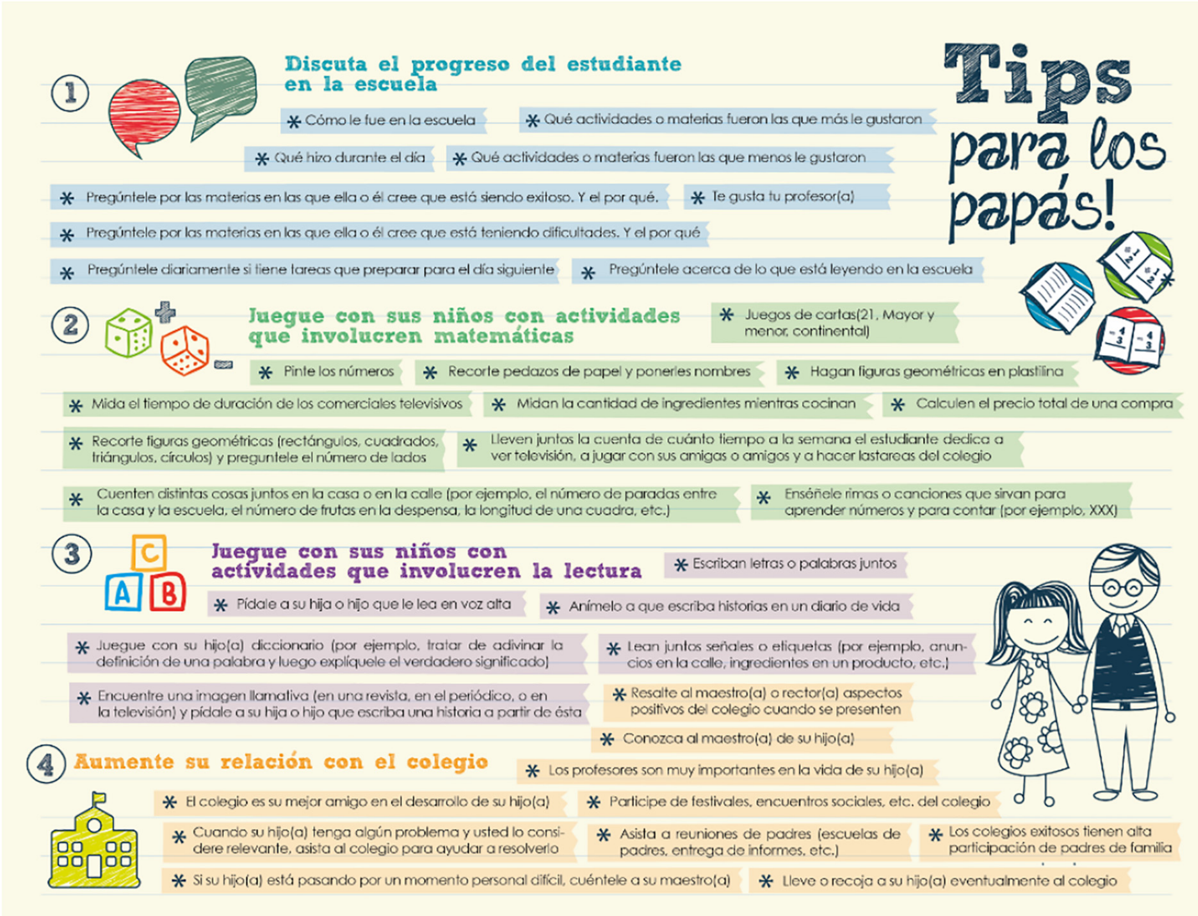

Fig. 3. Recommendations for parents in the full intervention. *Note:* Card provided to parents with suggestions about how to engage in their children's education. This card was provided to parents in the first and second treatment conditions.

Table 1
Timing of intervention with percent of students with outcome information in each wave.

	Total	Baseline data collection for Groups A & B	Pilot study for Groups A & B, Baseline data collection for Group C		Full intervention			
		April 2014	Dec 2014	June 2015	Dec 2015	June 2016	Dec 2016	June 2017
Intervention Group A (Grade 5 in 2014)	1606 (100%)	1602 (100%)	1422 (89%)	1356 (84%)	990 (62%)	–	–	–
Intervention Group B (Grade 4 in 2014)	1420 (100%)	1420 (100%)	1282 (90%)	1237 (87%)	954 (67%)	1213 (85%)	1171 (82%)	–
Intervention Group C (Grade 3 in 2014)	1345 (100%)	–	–	1345 (100%)	1043 (78%)	1203 (89%)	1165 (87%)	1036 (77%)

Note: Table includes the number and percent of students with math or reading data at each of the time points of the study. April 2014 represents the baseline data collection for students in Group A (grade 5 in 2014) and students in Group B (grade 4 in 2014). June 2015 represents the baseline data collection for students in Group C (grade 3 in 2014/grade 4 in 2015).

4. Data and sample

The analysis sample includes two cohorts of students: 3026 students who entered the study in grades four and five in April 2014; and 1345 students who entered the study in grade four in June 2015. On average, children in the sample were 9.9 years old and in fourth grade at the beginning of the 2014 school year; 46% of students were female. There are no statistically significant differences between the treatment and control group on demographic characteristics (including age, gender, and grade) as well as baseline test scores collected prior to intervention except for one marginally significant difference in baseline reading between the first treatment group and the control group (see Appendix Table A2 for details).

The primary outcomes of interest are scores from the EGRA and EGMA tests. Both the EGRA and EGMA were administered at baseline and in all follow-up waves. Student reading performance was measured using the EGRA, and student scores were based on the number of words correctly read. Student math performance was measured using the EGMA, and student scores were based on the number of subtraction problems solved correctly.[3] For our main analyses, we created a composite measure of student

[3] The EGMA assessment also includes two additional components: sums and problems. However, these outcomes were not collected for all grades across all outcome waves. We also observed moderate ceiling effects for both of these measures (see Appendices Figs. A1 and A2). Therefore, we focus on subtractions as our measure of mathematics).

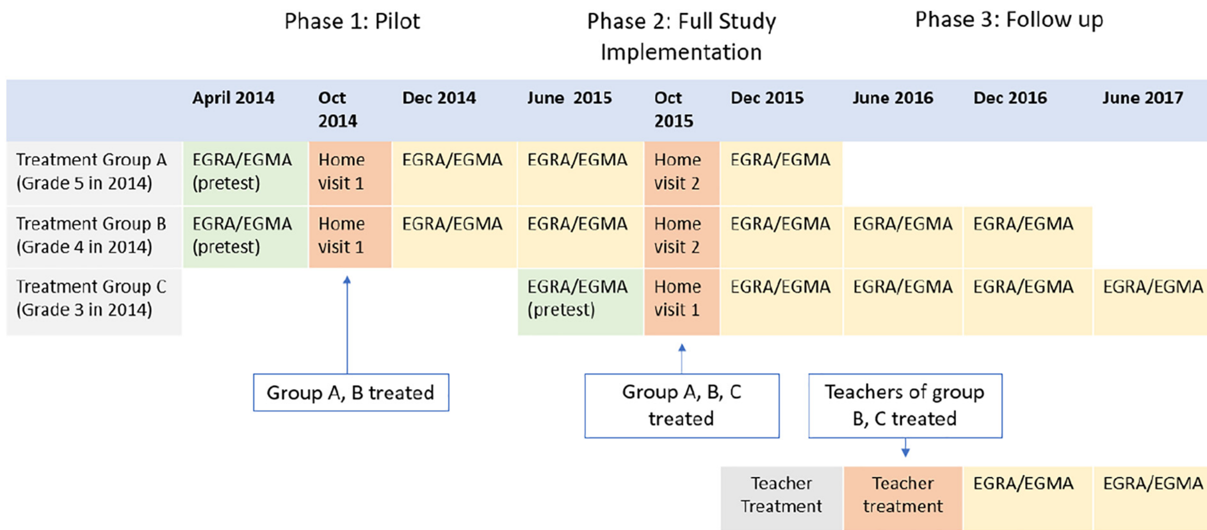

Fig. 4. Overview of intervention timing.

achievement by combining standardized scores across the math and reading assessments (see Table A3 for the correlations between student performance over time and between subjects). We chose to combine the math and reading assessments because the patterns of impacts are similar, and present impacts separately by subject in Appendix A.

The percent of students with outcome information differed across waves, ranging between 68[4] percent and 87 percent. However, we do not see any evidence of differential test-taking based on treatment status in any of the follow-up waves. We also do not find evidence that baseline test scores are related to the interaction between missing follow-up test score and treatment status, across all follow-up waves (see Appendices Tables A4, A5, and A6 for details).

4.1. Parental beliefs

At each home visit, prior to providing treatment households with information, we elicited parents' beliefs about their children's performance on the EGRA and EGMA assessments. We asked parents about the total number of words they expected their children to read correctly on the EGRA assessment, and the total number of subtraction problems they expected their children to solve correctly on the EGMA assessment. We also asked parents their beliefs about the average number of words read and subtractions problems solved correctly among other children attending their child's school and among other children in the city. We asked parents in both the treatment and control conditions about their beliefs regarding the average performance in their child's school in order to elicit comparable information across the treatment and control conditions. Parents were provided with a blank version of the intervention report card (without marks for individual scores) when asked for their beliefs about student scores. Initial parent beliefs were generally balanced across the treatment and control conditions; however, households in the first treatment condition had lower beliefs regarding math performance as compared to the control group (see Appendix Table A2 for details).

We also examine the extent to which parents' beliefs regarding their children's performance on the math and reading assessments accurately reflected their children's actual performance. We asked parents to state the scores they expected their children to receive on initial EGRA and EGMA assessments and find large differences between students' raw scores and their parents' beliefs. These results are reported in Table 2. On average, parents underestimated their children's reading performance by 11.1 points (a difference of approximately 0.5 SD). However, we see a very different pattern for math. On average, parents overestimated their children's performance on the subtractions assessment by 5.2 points (a difference of nearly 1 SD).

We also find some evidence that parents' accuracy of beliefs regarding baseline performance is related to parental education. Parents with higher levels of education had more accurate beliefs regarding students' reading performance at baseline after controlling for other household characteristics, although there were no differences in the accuracy of parents' beliefs based on education for math performance (see Table 2).

Additionally, we find some evidence that the accuracy of parents' baseline beliefs is related to student baseline performance. Parents' beliefs were more accurate when students had higher baseline math performance and were in schools with higher baseline math performance. However, the gap between parent beliefs and student performance was

Table 2
Difference between parent beliefs and performance at baseline (actual performance – beliefs).

	Mean	SD	N	Gap (mean)	Abs(Gap) (mean)	N
Number of words read	91.91	20.09	4362	11.08	19.45	2008
Number of correct subtractions	12.75	5.29	4190	−5.21	7.43	1988

Relationship between accuracy of beliefs and parent education

	Abs(Gap): Reading		Abs(Gap): Subs	
Mother's years of education	−0.587***	−0.355***	0.006	−0.002
	(0.130)	(0.138)	(0.035)	(0.038)
Observations	1862	1835	1843	1816
Dependent variable mean	19.360	19.354	7.416	7.386
Includes child and parent controls	No	Yes	No	Yes

Baseline performance based on administration of EGRA and EGMA assessments in April 2014 among students who were in grades 4 or 5 in 2014, and assessment in June 2015 among students who were in grade 3 in 2014. Parent beliefs based on home visit conducted in October 2014 among students who were in grades 4 or 5 in 2014, and in October 2015 among students who were in grade 3 in 2014. The gap between parent beliefs and performance is calculated by the difference between parent beliefs regarding student performance (e.g. number of words read, correct subtractions, etc.) and student performance at baseline (e.g. number of words read, correct subtractions, etc.). Child and parent controls include child age and gender, mother's occupational status, and household income (less than one minimum salary (MS), one MS, between one and two MS, at least two MS). Standard error in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[4] The percent of students with outcome information was substantially lower in December 2015 (69%) relative to the later follow-up waves (77% to 87%). This is due in part to teacher strikes that occurred during this school year that led to a shortened school year and also because of delays in the visits to schools. In Table A5 we show that students with information do not differ in baseline performance from students without information.

smaller when students were in schools with *lower* baseline reading performance. We also find some evidence that parents with higher reported involvement in their children's education at the initial home visit also more accurate beliefs about students' reading performance at baseline (see Appendix Table A7).

4.2. Parental behaviors

At each home visit, we also asked parents about their behaviors around investing in the children's education. Specifically, we asked parents to state the number of days per week, on average, they engaged in the following activities with their child: asking about school, helping with studying, reading to him or her, helping with homework, and asking about grades (One day per week/Two days per week/Three days per week/Four days per week/Five days per week). We also collected information about parents' relationship with the school, including how frequently parents attended parents' and guardians' meetings, school activities, and meetings with teachers (Never/Almost never/Occasionally/Almost always/Always).

We also collected information about parents' satisfaction with their children's school. We asked parents about their satisfaction regarding the overall instructional quality of the school, school order and discipline, school infrastructure, whether schools kept parents and guardians informed about the school, teacher quality and preparation, and teachers' willingness to listen to parents and guardians (Very unsatisfied/Unsatisfied/Neither satisfied nor unsatisfied/Satisfied/Very satisfied).

We collected information on the characteristics of participating families during the initial home visits. In most cases, the respondent was the mother (73%). In other cases the respondent was the father (9%) or another relative or non-relative (17%). On average, the responding parent or guardian was approximately 39 years old. In nearly 90% of households the father was reported as working for pay, while the mother reported working for pay in just under 50% of households. Both mothers and fathers had approximately eight years of education, and household income was one minimum salary or less in most households (see Appendix Table A2 for details).

Parents also reported relatively high involvement in their children's education at the initial home visit prior to the intervention, and most of these measures demonstrated substantial ceiling effects. On average, parents reported asking about school, helping with homework, and asking about grades nearly five days per week. Parents reported helping their children study approximately four days per week and reported reading to their children approximately three days per week. There are a few significant differences in household characteristics or parent investment behavior prior to intervention between the treatment and control group. Parents of children in the treatments group reported asking about school and helping their children with studying more frequently, on average, than parents in the control group (differences of approximately 0.07 days and 0.15 days, respectively) but were not more likely to report reading with their children, helping with homework, or asking about grades. Parents in the treatment conditions were also more likely report that they always attended meetings with teachers (a difference of about five percentage points, respectively; see Appendix Table A2 for details).

5. Analytic models

For our main analysis, we estimate a series of intent-to-treat (ITT) empirical models which provide a set of causal estimates of the effect of providing parents with information on their children's academic performance bundled with suggestions for how parents can engage with their children's education on children's subsequent achievement. The ITT estimates measure the effects of being assigned to one of the treatment conditions, in which parents were assigned to receive a home visit during which they received a report containing information on student performance as well as the suggestions for how to engage in their children's education. It is plausible that impacts may have differed based on whether parents received information on student performance relative to their peers in the same school (Treatment 1) or to other students in the city (Treatment 2). Therefore, for our main analysis we examine the impacts of the treatment conditions separately. Specifically, we estimate models of the following form:

$$Y_{ij} = \alpha + \beta_1 Treatment1_i + \beta_2 Treatment2_i + \gamma X_i + \epsilon_{ij} \qquad (1)$$

The variable Y_{ij} represents the test score for student i in school j. This is regressed on the variables $Treatment1_i$and $Treatment2_i$ which are indicators for whether the household of student i was assigned to Treatment Group 1 or Treatment Group 2, respectively. We also include a series of child baseline covariates, X_i, which include student age, gender, baseline math test scores and baseline reading test scores, and grade in 2014.[5] We estimate a model for test scores collected in each of the follow-up waves between December 2015 and June 2017.[6] Given that the randomization was done at the individual level, we did not cluster the standard errors by any higher level (e.g., schools).

To examine the extent to which there are heterogeneous impacts based on students' baseline achievement, we estimate models of the following form that include an interaction between treatment status and baseline achievement:

$$Y_{ij} = \alpha + \beta Treatment_i + \delta Treatment_i * LowAchievement_i + +\rho LowAchievement_i + \gamma X_i + \epsilon_{ij} \qquad (2)$$

The variable $Treatment_i$ is an indicator for whether student i was assigned to Treatment Group 1 or Treatment Group 2, and $LowAchievement_i$ is an indicator for whether student i scored low on either the baseline math or reading assessments (i.e., below the 25th percentile of students in the sample in the same grade).[7] We estimate analogous models to examine whether impacts differ based on whether parents' initial beliefs of student ability were above or below parents' beliefs regarding average school performance.

We use dummy variable adjustment to account for missing baseline covariates. In cases where students were missing baseline math and/or reading scores, we set the missing values to the overall mean. For each subject, we created indicator variables set to one if the baseline score was missing and zero otherwise. These indicator variables were included in all analyses.

6. Results

6.1. Impacts of information receipt on parental beliefs and behaviors

We first examine whether parents' beliefs about their children's math and reading performance were affected by the receipt of information one year after receiving the initial report card during the pilot intervention. As noted above, home visits were conducted in October 2014 and October 2015. In the first home visit, we asked parents about their beliefs regarding their children's performance on the math and reading assessments immediately prior to parents' receipt of the report card information. In the second home visit, as in the first home visit, we also elicited parents' beliefs about their children's performance on the math and reading assessments. We therefore collected information

[5] Results of models that only control for grade and do not control for other child baseline covariates are similar. All models control for grade since the randomization was done at the individual level after blocking by grade.

[6] In our main analyses, we focus on the impacts of information receipt on student performance on the follow-up waves from the full intervention (December 2015 through June 2017). We present results on student performance from the pilot intervention (December 2014 and June 2015) in the Appendix A.

[7] For the distributions of baseline scores and the 25th percentile cutoff for reading and subtractions, see Figs. A5 and A6.

Table 3
Impact of pilot intervention on parent beliefs.

	Post-pilot intervention beliefs		Gap between post-pilot intervention beliefs and June 2015 performance (absolute value)	
Number of words read				
Pilot Treatment 1	1.096		−3.504***	
(Individual information)	(1.296)		(1.174)	
Pilot Treatment 2	3.155**		−3.520***	
(School information)	(1.303)		(1.180)	
Pilot Treatment 1/2		4.009***		−3.553***
		(1.290)		(1.168)
Pilot Treatment 1/2*Low student reading performance at baseline		−7.973***		0.092
		(2.648)		(2.399)
Observations	1962	1957	1962	1957
Number of correct subtractions				
Pilot Treatment 1	−0.571**		−1.127***	
(Individual information)	(0.289)		(0.269)	
Pilot Treatment 2	0.192		−0.669**	
(School information)	(0.291)		(0.271)	
Pilot Treatment 1/2		−0.060		−0.779***
		(0.299)		(0.277)
Pilot Treatment 1/2*Low student math performance at baseline		−0.771		−0.239
		(0.600)		(0.555)
Observations	1953	1852	1953	1852

Note: Standard errors in parentheses. Low baseline performance indicates that student was below the 25th percentile on math and/or reading at baseline. Baseline performance based on administration of EGRA and EGMA assessments in April 2014 among students who were in grades 4 or 5 in 2014, and assessment in June 2015 among students who were in grade 3 in 2014. All models include controls for age, gender, grade, and baseline math and reading scores. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

about parents' beliefs immediately prior to and one year after their receipt of the report card information in the pilot intervention.

In Table 3, we confirm that there is evidence that the beliefs of parents in the second home visit among treated household differed from those of parents in control households.[8] We see that on average, parents' beliefs regarding their children's reading performance were higher for households assigned to the second treatment condition relative to households assigned to the control group. Point estimates suggest that receipt of information during the pilot may also have led parents in the first treatment condition to increase their beliefs regarding their children's reading performance, but the magnitude of the effect is smaller and not statistically significant. Furthermore, we also find that the extent to which the treatment shifted parents' reading beliefs differed based on students' baseline performance. While parents shifted their beliefs regarding reading upwards for students who scored at or above the 25th percentile in reading, parents shifted their beliefs downward if their children were below the 25th percentile. Moreover, we also find that the distance between parent beliefs in the second home visit and their children's actual reading performance in the prior follow-up wave (June 2015) was smaller in both treatment groups relative to the control group. This impact did not differ based on students' baseline reading performance.

We also find evidence that parents' beliefs regarding their children's math performance were affected by the receipt of information in the pilot intervention. We see some evidence that the pilot intervention caused parents in the first treatment condition to adjust their beliefs about students' subtractions performance downwards. This effect was not observed for parents in the second treatment condition. However, we find that the distance between parent beliefs and student performance in the June 2015 follow-up waves was smaller for among households in both treatment conditions relative to the control group. As with the reading assessment, we see little evidence that the increase in accuracy differed for students with relatively high or low baseline performance.

Table 4 presents effects of the program on parents' educational investment behavior.[9] Parents in the first treatment group were 6.6 percentage points more likely to report consistently (always) attending meeting with teachers relative to the control group; the impact was similar in magnitude for parents in the second treatment group (8.1 percentage points). We find little evidence that these impacts differed based on students' baseline performance. However, we do not observe effects on other aspects of parents' relationship with the school. The treatment also had only limited impacts on other parent behaviors regarding families' internal investment in education, including the number of days per week parents reported engaging in the following activities: asking about school, helping with studying, reading with their child, helping with homework, and asking about grades. We find some evidence of positive impacts of the second treatment condition on an index of these outcomes, but do not observe this effect for the first treatment condition (see Appendix Table A8 for details).[10] Treatment impacts on both parent beliefs and behaviors were generally similar across the two treatment conditions. Although the second treatment condition in the pilot intervention provided information about school (rather than student) performance, it is possible that parents interpreted the information about school performance as indicative of the (average) performance of their children.

We also examine whether the treatment impacted parents' satisfaction with their children's school. We find some evidence that the treatment impacted parents' satisfaction with the school. Parents in both treatment groups reported being less satisfied with school discipline and order relative to control parents. Additionally, parents in the first treatment group were less satisfied with how schools kept parents and guardians informed about the school relative to control parents (see Appendix Table A9 for details).

6.2. Impacts of information receipt on student performance

Table 5 presents effects of information on math and reading scores across all follow-up waves in the full intervention (December 2015 to June 2017).[11] We find evidence of positive impacts in the December 2015 follow-up wave for both the first treatment condition (0.10 SD) and second treatment condition (0.09 SD). We also find some evidence of positive impacts on student performance in the June 2016 follow-up wave, although the estimated impact is only marginally significant for the second treatment condition (0.13 SD). The December 2015 and June 2016 results represent impacts roughly 2 months and 8 months, respectively, after households received information as part of the full intervention. However, these impacts do not persist through the final two follow-up waves of the study (December 2016 and June 2017). The December 2016 and June 2017 results represent impacts roughly 14 and 20 months, respectively, after information receipt. Taken together, these results suggest that the provision of information had positive impacts on student performance in the short term, likely through impacts

[8] We note that the results presented in Table 3 represent the impact of the pilot intervention on parent beliefs. Therefore, results represent the impacts of receiving information, rather the impact of receiving information bundled with suggestions about how parents could engage with their children's education. We also confirmed that these results are robust to including controls for parents' baseline beliefs.

[9] As in Table 3, we note that the results presented in Table 4 represent the impact of the pilot intervention, which provided information only rather than information in combination with suggestions for how parents could engage with their children's education.

[10] We confirmed that these results are robust to including controls for parents' baseline behaviors.

[11] Although we do not present the impacts of the pilot intervention on student performance in Table 5, results the first year are reported in Appendix Table A10. Impacts of the pilot intervention on math and reading performance are mixed in sign and not statistically significant. These results suggest the importance of bundling information on student performance with specific suggestions for how parents can engage with their children's education.

Table 4
Impact of pilot intervention on parents' relationship with the school.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Guardians' meetings		Parents' school		School activities		Meetings with teachers	
Pilot Treatment 1	−0.021		−0.016		0.022		0.066***	
	(0.018)		(0.028)		(0.026)		(0.025)	
Pilot Treatment 2	0.016		0.024		0.007		0.081***	
	(0.018)		(0.028)		(0.026)		(0.025)	
Pilot Treatment 1/2		−0.015		−0.005		0.008		0.090***
		(0.021)		(0.032)		(0.030)		(0.029)
Pilot Treatment 1/2* Low baseline math and/or reading performance		0.027		0.029		0.040		−0.042
		(0.032)		(0.050)		(0.048)		(0.045)
Observations	1970	1863	1970	1863	1970	1863	1970	1863
Control mean	0.89		0.55		0.66		0.68	

Note: Standard errors in parentheses. All outcomes are binary indicators for whether parents reported "Always" when asked how frequently they participated in each activity. Other possible responses included "Almost always", "Occasionally," "Almost never," and "Never." All models include controls for age, gender, grade, and baseline math and reading scores. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

on parent beliefs and behaviors (although we cannot test this empirically). However, in the absence of continued information provision, these impacts were not sustained, possibly because parental investment returned to pre-intervention levels.[12] When we consider effects on math and reading scores separately, we find that the patterns of impacts are similar across the two subjects (see Appendix Table A11).

Given the levels of missing data across the follow-up waves, we also replicate our results using inverse probability weighting (IPW). In this specification, we reweight the treatment and control groups with outcome information in each phase such that the two groups are comparable on observed baseline characteristics (age, gender, grade, and baseline test scores). We also bound the effect of a combined indicator for assignment to the first or second treatment condition using Lee bounds and present results in the bottom panel of Table 5. We tighten the Lee bounds estimates by grade since the randomization was conducted at the individual level after blocking by grade. Specifically, we include indicators for grade at baseline (i.e., grade three, four, or five). The results are substantively unchanged from our main results but lack precision.

It is important to note that the estimates presented for each of the follow-up waves presented in Table 5 represent the average impact across three cohorts of students who differed in the timing and duration of treatment. First, different grade cohorts exited and entered the study at different points. For example, students who were in grade five in 2014 were not followed after December 2015 and students who were in grade four in 2014 were not followed after December 2016. Similarly, students who were in grade three in 2014 did not enter the study until June 2015. Furthermore, as described above, home visits occurred in October 2014 (pilot) and October 2015 (full intervention). As a result, students in the treatment group who were in grades four and five in 2014 may have received up to two home visits. The impact in the December 2015 follow-up wave therefore represents the combined effect of receiving information during the pilot and full intervention home visits over the course of approximately one year. In contrast, the first home visit for students who were in grade three in 2014 occurred in October 2015; therefore, the impact in the December 2015 follow-up wave represented the effect of the intervention roughly two months after the start of the intervention. If the information provided to households in the pilot home visit impacted students' outcomes (for example, if there was a cumulative effect of receiving information in both home visits), we would expect to see differences in the trajectories of impacts across the first cohort of students who entered the study in 2014 (students in grades 4 and 5 in 2014) and the second cohort of students who entered the study in 2015 (students in grade 4 in 2015). Therefore, the overall impact in each follow-up wave could mask heterogeneity by cohort.

To examine whether this is the case, we estimate our main specification separately for students in grades four and five in 2014, and students in grade three in 2014 (grade four in 2015). As shown in Table 6 and Fig. 5, the patterns of results across the follow-up waves are generally similar across the two cohorts. Among students who were in grades four and five in 2014, impacts in December 2015 shortly after the second home visit are positive and statistically significant for both treatment

[12] An alternative explanation for the small impacts in later waves is potential contamination, if treated parents shared information with control parents. However, we believe it is unlikely that information spillovers are driving the observed results, since the full intervention provided personalized information about individual student (rather than school) performance.

Table 5
Impact of information intervention on composite test score outcomes.

	(1)	(2)	(3)	(4)
	Dec 2015	June 2016	Dec 2016	June 2017
Main specification				
Treatment 1	0.096**	0.086	−0.054	−0.051
(Individual information + performance vs. school)	(0.043)	(0.077)	(0.082)	(0.118)
Treatment 2	0.085**	0.131*	−0.101	0.011
(Individual information + performance vs. city)	(0.043)	(0.077)	(0.082)	(0.119)
Observations	2984	2416	2336	1036
p-Value of F-test that T1 = T2	0.795	0.562	0.570	0.601
Inverse probability weighting (IPW)[a]				
Treatment 1	0.106**	0.090	−0.052	−0.073
	(0.043)	(0.077)	(0.080)	(0.119)
Treatment 2	0.098**	0.148*	−0.089	−0.022
	(0.044)	(0.077)	(0.083)	(0.119)
Observations	2984	2416	2336	1036
Lee bounds				
Treatment 1 or Treatment 2				
Lower	0.058	0.047	−0.112	−0.061
	(0.113)	(0.103)	(0.122)	(0.149)
Upper	0.093	0.171	−0.103	0.006
	(0.077)	(0.137)	(0.109)	(0.169)

Note: Standard errors in parentheses. Composite math and reading score calculated by the following: Reading and math (subtractions) scores were standardized within grade with respect to the control group at baseline. The sum of the standardized reading and math scores were calculated for each wave. Composite scores for each follow-up wave were standardized again within each grade with respect to the composite score for the control group at baseline. All models include controls for age, gender, grade, and baseline math and reading scores. First column includes students who were enrolled in grades 3, 4 or 5 in 2014. Second and third columns include students who were enrolled in grades 3 or 4 in 2014. Fourth column includes students who were enrolled in grade 3 in 2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.[a] For the IPW approach, predicted probabilities of students' assignment to treatment 1 and treatment 2 were estimated based on a multinomial logistic regression predicting treatment status as a function of baseline covariates (age, gender, grade and baseline test scores).

Table 6
Impact of information intervention on composite test score outcomes, separately by cohort.

	(1)	(2)	(3)	(4)
	Dec 2015	June 2016	Dec 2016	June 2017
Grade 4 & 5 in 2014 (Cohort 1)				
Treatment 1	0.121**	0.117	−0.042	–
	(0.059)	(0.130)	(0.135)	
Treatment 2	0.117**	0.075	−0.079	
	(0.059)	(0.129)	(0.134)	
Observations	1941	1213	1171	
p-Value of F-test, T1 = T2	0.944	0.746	0.782	
Grades	4,5	4	4	
Grade 3 in 2014 (Cohort 2)				
Treatment 1	0.064	0.076	−0.052	−0.051
	(0.058)	(0.082)	(0.093)	(0.118)
Treatment 2	0.028	0.156*	−0.135	0.011
	(0.058)	(0.083)	(0.094)	(0.119)
Observations	1043	1203	1165	1036
p-Value of F-test, T1 = T2	0.534	0.331	0.380	0.601

Note: Standard errors in parentheses. Composite math and reading score calculated by the following: Reading and math (subtractions) scores were standardized within grade with respect to the control group at baseline. The sum of the standardized reading and math scores were calculated for each wave. Composite scores for each follow-up wave were standardized again within each grade with respect to the composite score for the control group at baseline. All models include controls for age, gender, grade, and baseline math and reading scores. First column includes students who were enrolled in grades 3, 4 or 5 in 2014. Second and third columns include students who were enrolled in grades 3 or 4 in 2014. Fourth column includes students who were enrolled in grade 3 in 2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

conditions (0.12 SD). Point estimates are similarly positive in June 2016, but less precisely estimated. However, impacts are negative and not statistically significant in the December 2016 follow-up wave. Turning to the second cohort, although we observe no impact of either treatment condition in December 2015, we observe a positive, marginally significant impact of the second treatment condition in June 2016 (0.16 SD). The estimated impact of the first treatment condition is similarly positive, but smaller in magnitude and not statistically significant. As with the first cohort, estimated impacts are generally negative and not statistically significant in the remaining follow-up waves (December 2016 and June 2017). As the study did not include additional intervention in either 2016 or 2017, the pattern of impacts for both cohorts suggests that the short-term impacts of providing information on students'

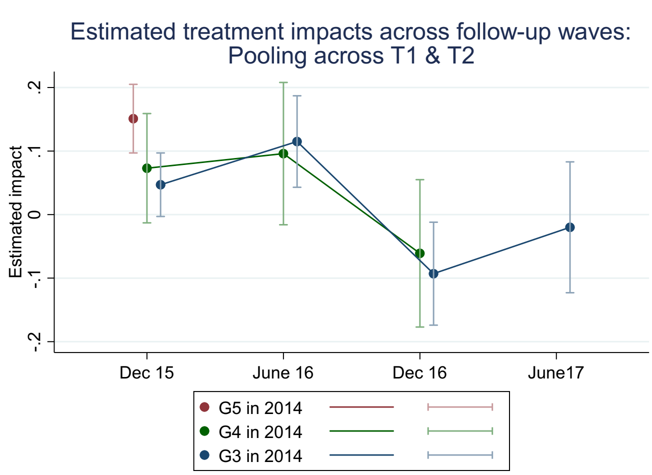

Fig. 5. Estimated treatment impact across each follow-up wave, by grade. *Note:* Points indicate treatment impact estimates; bars indicate standard errors. Each result is from a regression of composite test scores on a single indicator for treatment 1 or treatment 2, controlling for age, gender, and baseline test scores. Models are estimated separately for each grade and follow-up wave.

academic performance fade out over time in the absence of continued intervention ("backsliding").

The cost of this intervention is relatively low. The Luker foundation provided the numbers to calculate the direct cost of implementation. The household visits cost (approximately) US$20,000 (at current prices and using the average exchange rate for pesos to U.S. dollars); the application of EGRA and EGMA per grade (at schools) cost US$3200. For two rounds of household visits (assuming zero inflation), the cost was US $40,000; for the application of the tests to four grades over four years, the cost was US$51,200. In total, the direct cost of the intervention for the four years was US$91,200; the cost of the operation per year was US$20,000 + US$12,800 = US$32,800.

Based on a yearly cost of US$32,800 and a total of 4371 students, we calculated an annual cost per child of US$7.50. Based on our main results, we assumed an average treatment effect of 0.085 SD in the first follow-up wave of the study and an effect of 0.131 SD in the second follow-up wave of the study. Using these average costs and benefits per student, we calculated a benefit-cost ratio of 1.133 SD per US$100 in the first follow-up wave and 1.747 SD per US$100 in the second follow-up wave. We then benchmarked our results against information provided by J-PAL on the cost effectiveness of education interventions from 27 studies examining the impacts of these programs on on student test scores (J-PAL, 2014). This comparison indicates that the cost-effectiveness of our intervention was comparable with teacher monitoring (Duflo et al., 2012) and other relatively high-investment interventions (e.g., Abeberese et al., 2014; Banerjee et al., 2007; Duflo et al., 2015; Kremer et al., 2009). However, our intervention was less cost-effective than a prior study providing information on the returns to education in Madagascar (Nguyen, 2008).

6.3. Heterogeneity by baseline student performance

Next, we explored whether treatment impacts differ based on students' baseline math and reading performance. As described above, there is some evidence that the extent to which the provision of information affected parents' beliefs about their children's math and reading performance differed based on baseline student performance. Therefore, it is possible that impacts on student outcomes similarly differed based on baseline performance. Our results presented in the top panel of Table 7 suggest that the positive impacts observed in the December 2015 and June 2016 follow-up waves were driven by positive impacts among the lowest-performing students. Impacts are positive and statistically significant in the first two follow-up waves for students who scored below the 25th percentile on either the baseline math or reading assessment, with effects ranging from 0.20 SD to 0.28 SD. However, as with the main impacts, these results do not persist through the December 2016 and June 2017 follow-up waves.[13]

6.4. Heterogeneity by baseline parental behaviors

We next examine whether treatment impacts on students' math and reading performance differ based on parents' initial behaviors around investing in the children's education prior to the receipt of information. As noted above, parents reported overall high levels of investment at baseline. However, we might expect impacts to be larger in households where parents reported less investment in their children's education at baseline and more readily impacted by the combination of personalized student performance information and actionable suggestions. We then also examine whether treatment impacts differ based on an index of parents' reported levels of investment, including information on parents' investment behavior in the home and relationship with their child's school, during the initial home visit. Our results presented in the bottom of Table 7 suggest evidence that impacts were larger when

[13] We also examine the sensitivity of this result to other ways of classifying students as relatively high performing and low performing at baseline. For the full results, see Appendix Tables A18 through A20.

Table 7
Impact of information intervention on composite test score outcomes, by baseline performance and parent beliefs.

	(1) Dec 2015	(2) June 2016	(3) Dec 2016	(4) June 2017
Impacts based on baseline performance[a]				
Treatment 1/2	−0.013	0.010	−0.081	0.001
	(0.049)	(0.089)	(0.094)	(0.135)
Treatment 1/2*Low baseline performance	0.216***	0.268**	−0.011	−0.051
	(0.077)	(0.136)	(0.144)	(0.208)
Low baseline performance	−0.300***	−0.503***	−0.316**	−0.049
	(0.072)	(0.127)	(0.134)	(0.201)
Impact on high-performing students	−0.013	0.010	−0.081	0.001
Impact on low-performing students	0.203***	0.278***	−0.092	−0.050
Observations	2884	2365	2290	1036
Impacts based on baseline parent behaviors[b]				
Treatment 1/2	0.066	0.259***	−0.041	0.034
	(0.055)	(0.088)	(0.095)	(0.119)
Treatment 1/2*Baseline parent behavior index	−0.061	−0.154*	−0.220**	−0.002
	(0.055)	(0.085)	(0.090)	(0.116)
Baseline parent behavior index	0.026	0.065	0.156**	−0.030
	(0.047)	(0.072)	(0.076)	(0.094)
Observations	1520	1380	1328	765

Note: Standard errors in parentheses. Composite math and reading score calculated by the following: Reading and math (subtractions) scores were standardized within grade with respect to the control group. The sum of the standardized reading and math scores were calculated, and standardized again within each grade with respect to the control group, to form the composite score. All models include controls for age, gender, and baseline math and reading scores. First column includes students who were enrolled in grades 3, 4 or 5 in 2014. Second and third columns include students who were enrolled in grades 3 or 4 in 2014. Fourth column includes students who were enrolled in grade 3 in 2014. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] Low baseline performance indicates that student was below the 25th percentile on math and/or reading at baseline. Omitted category is students who scored at or above the 25th percentile on both math and reading at baseline. Excludes students missing baseline math or reading scores.

[b] The baseline parent behavior index was calculated as follows: First, we created binary indicators based on whether parents reported that they engaged in the following five activities five days per week: asking about school, helping with studying, reading to him or her, helping with homework, and asking about grades (One day per week/Two days per week/Three days per week/Four days per week/Five days per week). Next, we created binary indicators based on whether parents reported that they always attended the following four activities: attended parents' school meetings, guardians' meetings, school activities, and meetings with teachers. We then standardized the mean of these nine binary variables with respect to the overall sample.

initial levels of parental investment were lower. Impacts in both the June 2016 and December 2016 follow-up waves were lower when parents had lower levels of investment in their children's education at baseline. Impacts are consistent in direction across the other follow-up waves, although mixed in magnitude and precision. This is consistent with our hypothesis that the provision of information about student performance is most effective at improving student achievement if this information is paired with information that changes parent behaviors.

6.5. Heterogeneity by baseline parental beliefs

Next, we examine whether treatment impacts on students' math and reading performance differ based on the accuracy of parents' initial beliefs prior to the receipt of information. First, we examine whether impacts differ based on the accuracy of parents' initial beliefs regarding their children's performance, i.e., the gap between parents' beliefs and student performance at baseline. We find some evidence that impacts on reading in the December 2015 and June 2016 follow-up waves were larger when the distance between parents' beliefs and their children's performance at baseline was larger. However, we do not see the same pattern of impacts on math performance (see Appendix Table A14). We also see little evidence that impacts on student performance differ based on whether parents over-predict (rather than under-predict) their children's performance at baseline (see Appendix Table A15). This suggests that correcting parents' inaccurate beliefs about their children's reading performance may have helped parents more effectively invest in their children's education regardless of whether parents over- or under-predicted their children's initial performance.

We also examine whether treatment impacts on students' performance differ based on whether parents believed their children were high- or low-performing at baseline. Parents' beliefs regarding the raw number of correct reading or subtractions may not be the most salient measure of parents' beliefs regarding their children's academic achievement. We therefore examine whether impacts differ based on parents' perception of their children's performance relative to the performance of their peers. In the initial home visit, parents were asked about their beliefs both about the average performance of students in their child's school on the math and reading assessments, in addition to the performance of their own child. Whether parents placed their child above or below the school average is another potential indicator of parents' perception of their child's performance. Therefore, we examine whether impacts vary based on whether the parents' beliefs regarding their child's math and/or reading score was below parents' beliefs regarding the school average. Although point estimates generally suggest that impacts were higher if parents had higher beliefs about student relative performance, we cannot reject that impacts across the groups are the same across all follow-up waves (see Appendix Table A16).

7. Conclusion

Parents have limited and, sometimes, erroneous information about the academic performance of their children. As such, information failures may induce misallocation of resources and suboptimal investment in education—either within the household (e.g., time, financial resources) or in the relationship with the school. In this intervention we aimed to solve the problem of information by providing results in an early assessment on literacy and math. We hypothesized that the provision of information alone – without providing parents with information about specific actions to take in response to this information – may fail to impact parent behavior or student performance. Therefore, we bundled the provision of information about student performance with specific suggestions about how parents could engage with their children's education.

We show that parents do have erroneous information about the academic performance of their children. Upon receiving information in the pilot intervention, parents update their assessment of the performance of their children and meet more frequently with the teachers. Also, we show that the provision of information leads to some improvements in the academic performance of children, especially students with low scores at baseline. We demonstrate that families can react to information, and that the provision of information may be one lever to increase learning. However, these effects are short-lived. After some time, the control group catches up with the group, producing a dynamic of "action" (in the short run) and backsliding to the (control) mean. This is consistent with models in which the parents cannot permanently modify their behavior (or change a "stock" or permanent variable); parents can temporarily modify their behavior in the short run but actions quickly return to a "business as usual" mode. However, the question of whether impacts can be sustained through more frequent provision of information remained unanswered in the literature. We also cannot rule out other explanations. For example, it is possible that the value of the information provided to parents declines over time as children's academic performance, and the areas in which they need the most support, changes over time. Although we cannot disentangle these potential explanations, our findings provide initial evidence to suggest that impacts of information may not persist without continued intervention.

Considering the results of the present study, the Secretary of Education in Manizales and the Luker Foundation have begun implementing a scaled-up version of the intervention managed by the Foundation.

Building on the present findings that information was most effective at improving the math and reading performance of lower-performing students, the scaled-up intervention will target lower-performing students. As the present study also finds that impacts of information provision fade out in the absence of new information, the expansion of the program will also deliver information (including recommendation actions how parents can engage with their children's education) on a more frequent, ongoing basis.

The program will use even a more cost-effective approach to deliver the high-frequency messages by providing information via cell phone text messages rather than in-person home visits. As it is, the cost-effectiveness of the intervention is relatively low (low cost, high impact). Per year, the cost was approximately $32,800, and the impact on low-achievers at baseline ranging from 0.20 SD to 0.28 SD. With a strategy of using cell phone messages, the cost of visiting households is zero. Future research will test whether the results of the present study generalize to a scaled-up version of the intervention implemented by the local authorities.

Declaration of competing interest

We have no relevant or material financial interests that relate to the research described in this paper. We don't have any conflict of interests.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2020.104185.

References

Abeberese, A.B., Kumler, T.J., Linden, L.L., 2014. Improving reading skills by encouraging children to read in school: a randomized evaluation of the Sa Aklat Sisikat reading program in the Philippines. J. Hum. Resour. 49 (3), 611–633.

Allcott, H., Rogers, T., 2014. The short-run and long-run effects of behavioral interventions: experimental evidence from energy conservation. Am. Econ. Rev. 104 (10), 3003–3037.

Andrabi, T., Das, J., Khwaja, A.I., 2017. Report cards: the impact of providing school and child test scores on educational markets. Am. Econ. Rev. 107 (6), 1535–1563.

Banerjee, A.V., Cole, S., Duflo, E., Linden, L., 2007. Remedying education: evidence from two randomized experiments in India. Q. J. Econ. 122 (3), 1235–1264.

Avvisati, F., Gurgand, M., Guyon, N., Maurin, E., 2014. Getting parents involved: a field experiment in deprived schools. Review of Economic Studies 81 (1), 57–83.

Banerjee, A.V., Banerji, R., Duflo, E., Glennerster, R., Khemani, S., 2010. Pitfalls of participatory programs: evidence from a randomized evaluation in education in India. Am. Econ. J. Econ. Pol. 2 (1), 1–30.

Becker, G.S., 1962. Investment in human capital: a theoretical analysis. Journal of Political Economy 70 (5, Part 2), 9–49.

Bergman, P., 2015. Parent-Child Information Frictions and Human Capital Investment: Evidence From a Field Experiment. Columbia University, Mimeo http://www.columbia.edu/~psb2101/BergmanSubmission.pdf.

Berlinski, S., Busso, M., Dinkelman, T., Martinez, C., 2016. Reducing Parent-School Information Gaps and Improving Education Outcomes: Evidence From High Frequency Text Messaging in Chile. Unpublished Manuscript.

Cortes, K.E., Fricke, H., Loeb, S., Song, D.S., 2018. Too Little or Too Much? Actionable Advice in an Early-Childhood Text Messaging Experiment (No. w24827). Research, National Bureau of Economic.

Dizon-Ross, R, 2019. Parents' beliefs about their children's academic ability: Implications for educational investments. American Economic Review. 109 (8), 2728–2765.

Doss, C.J., Fahle, E.M., Loeb, S., York, B.N., 2018. More Than Just a Nudge: Supporting Kindergarten Parents With Differentiated and Personalized Text-Messages (No. w24450). Research, National Bureau of Economic.

Duflo, E., Hanna, R., Ryan, S.P., 2012. Incentives work: getting teachers to come to school. Am. Econ. Rev. 102 (4), 1241–1278.

Duflo, E., Dupas, P., Kremer, M., 2015. School governance, teacher incentives, and pupil–teacher ratios: experimental evidence from Kenyan primary schools. J. Public Econ. 123, 92–110.

Gallagher, J., 2014. Learning about an infrequent event: evidence from flood insurance take-up in the United States. Am. Econ. J. Appl. Econ. 6 (3), 206–233.

Ganimian, A.J., Murnane, R.J., 2016. Improving education in developing countries: lessons from rigorous impact evaluations. Rev. Educ. Res. 86 (3), 719–755.

Hastings, J.S., Weinstein, J.M., 2008. Information, school choice, and academic achievement: evidence from two experiments. Q. J. Econ. 123 (4), 1373–1414.

Houtenville, A.J., Conway, K.S., 2008. Parental effort, school resources, and student achievement. J. Hum. Resour. 43 (2), 437–453.

Jensen, R., 2010. The (perceived) returns to education and the demand for schooling. Q. J. Econ. 125 (2), 515–548.

J-PAL, 2014. Test Scores – Full Workbook. Jameel Abdul Poverty Action Lab. Available at. https://www.povertyactionlab.org/research-resources/cost-effectiveness.

Kraft, M.A., Rogers, T., 2015. The underutilized potential of teacher-to-parent communication: evidence from a field experiment. Econ. Educ. Rev. 47, 49–63.

Kremer, M., Miguel, E., Thornton, R., 2009. Incentives to learn. Rev. Econ. Stat. 91 (3), 437–456.

Kremer, M., Brannen, C., Glennerster, R., 2013. The challenge of education and learning in the developing world. Science 340 (6130), 297–300.

Loyalka, P., Liu, C., Song, Y., Yi, H., Huang, X., Wei, J., Zhang, L., Shi, Y., Chu, J., Rozelle, S., 2013. Can information and counseling help students from poor rural areas go to high school? Evidence from China. J. Comp. Econ. 41, 1012–1025.

Mayer, S.E., Kalil, A., Oreopoulos, P., Gallegos, S., 2015. Using Behavioral Insights to Increase Parental Engagement: The Parents and Children Together (PACT) Intervention (No. w21602). National Bureau of Economic Research.

Nguyen, T., 2008. Information, Role Models and Perceived Returns to Education: Experimental Evidence From Madagascar. Unpublished manuscript.

Rogers, T., Feller, A., 2018. Reducing student absences at scale by targeting parents' misbeliefs. Nat. Hum. Behav. 2 (5), 335.

Todd, P.E., Wolpin, K.I., 2007. The production of cognitive achievement in children: home, school, and racial test score gaps. J. Hum. Cap. 1 (1), 91–136.

York, B.N., Loeb, S., Doss, C., 2018. One step at a time: the effects of an early literacy text messaging program for parents of preschoolers. J. Hum. Resour. 0517–8756R.