Nudging Parents out the Door: The Impacts of Parental Encouragement on School Choice and Test Scores*

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This study evaluates a large-scale SMS outreach program to engage caregivers of students in private primary schools in Kenya. Using a two-stage randomization design, we tested two types of weekly SMS messages: growth-mindset encouragement and personalized performance information. We find two main effects: First, outreach improved test scores by 0.07 standard deviations, with particularly strong gains among initially lower-performing students. This improvement generates 12 learning-adjusted years of schooling per US$100 spent—making it highly cost-effective relative to other education interventions. Second, outreach increased student exit rates by 4.7-5.0 percentage points, with effects concentrated among higher-achieving students (5.7 to 6.6 percentage points). We develop a theoretical model of vertically differentiated schools where parental engagement affects both learning production and school choice. The model shows that when parents update their understanding of education production through engagement programs, they become more sensitive to perceived school quality differences. This increased sensitivity can lead lower-quality schools to forgo implementing engagement programs—even when costless—as enhanced parental discernment accelerates student exits. Our findings suggest a role for third-party provision of parent engagement programs in competitive education markets.

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1 Introduction

The education of children is shaped by both home and school environments (Coleman et al., 1966; Desforges and Abouchaar, 2003). To maximize student outcomes, schools frequently attempt to activate parents to play a larger educational role, as research has shown that even simple forms of outreach can meaningfully change parental behavior (Kraft and Rogers, 2015; Mayer et al., 2019; Bettinger et al., 2020; Avvisati et al., 2014). However, schools face a strategic challenge in encouraging parental engagement: parents make decisions not only about their involvement at home, but also about where to send their children to school. This dual role of parents—as both educational inputs and consumers of education—creates a dilemma for schools operating in competitive environments. While increased parental engagement may improve student outcomes through greater involvement in activities like homework help, it may also lead parents to become more discerning about school quality. When parents become more invested in their children's education, they may be more likely to transfer their children to schools they perceive as higher quality (e.g. Attanasio, Boneva and Rauh, 2022). This potential trade-off between improving student outcomes and retaining enrollment poses particular challenges for schools facing competitive pressures.

To examine how schools' efforts to engage parents interact with competitive pressures, we conducted a randomized evaluation of a program that delivered weekly SMS messages to caregivers in Bridge Kenya schools, a chain of relatively low-cost private primary schools. This setting is particularly well-suited for studying this interaction, as Bridge Kenya operates in a highly competitive educational marketplace, where it competes not only with other low-cost private schools but also with free public schools.

The study implements a two-stage randomization design to evaluate different approaches to parent outreach while measuring potential spillover effects. The program delivered two types of content through SMS: some caregivers received general messages designed to encourage parents to adopt a growth mindset—a view that intelligence can be improved with effort, as opposed to being fixed (Dweck, 2006)—while others received personalized information on their child's academic performance, including individual and class scores on midterm and endterm exams, as well as practice questions. Classrooms were randomly assigned to receive growth mindset messages, receive personalized performance information, or serve as control classrooms. Within treatment classrooms, a subset of caregivers was randomly assigned to receive no direct messages. This design allows us to examine both the relative effectiveness of different message content and potential spillover effects on students whose parents did not receive messages directly.

Our results reveal both promising learning gains and a challenging trade-off for schools.

Among those who remain in Bridge, outreach is associated with meaningful improvements in student performance, with students in treatment classrooms scoring 0.07 standard deviations above those in control classrooms. This effect size aligns with broader evidence on parent engagement programs: Figure 1 displays a forest plot of all experimental studies of Information and Communication Technology (ICT)-based outreach (including provision of information, reminders, or suggestions for educational content), showing an overall effect of 0.11 standard deviations.[1] The content of messages appears to matter less than the act of outreach itself, though effects are largest and most robust for students whose parents received growth-mindset encouragement. Personalized performance information shows no direct positive effect and is associated with slightly smaller test score gains (0.08 standard deviations), but these differences are not statistically significant. We examine whether performance feedback might erode encouragement benefits by anchoring parental beliefs about their child's ability to improve, but find limited evidence for such concerns.

The program is highly cost-effective. Despite modest absolute effects, the low cost of SMS delivery means the program produces 12 learning-adjusted years of schooling (LAYS) per US$100 (Angrist et al., 2020; Akyeampong et al., 2023). However, from the school's perspective, these learning gains come at a significant cost: increased student exit. Students in outreach classrooms were between 4.7 and 5.0 percentage points more likely to leave Bridge Kenya by the end of the study, as measured both through administrative test score records and enrollment data. This effect is particularly pronounced among higher-achieving students, with those above median baseline scores being 5.7 to 6.6 percentage points more likely to exit. Importantly, in the Kenyan context, where net primary school enrollment was 91.1 percent in 2016 (Ministry of Education, 2016), exit from Bridge likely indicates transfer to another school rather than dropping out of education entirely.[2]

The substantial exit effects raise important concerns about how to interpret the program's impact on learning. While we find positive test score effects, these are only observed for students who remain enrolled in Bridge schools. However, several patterns in the data suggest that our estimated effects likely represent a lower bound on the program's true impact on learning. First, exit effects are concentrated among higher-achieving students, suggesting

[1] We believe this sample likely includes most evaluations of SMS parental encouragement on child test scores relative to a comparison group where the treatment group only received educational interventions through the phone. Several studies that compare alternative messaging strategies without a control group that received no SMS encouragement are not included such as Asher, Scherer and Kim (2022) and Cortes et al. (2021). We also exclude Macarena Santana and Claro (2019) because the treatment was accompanied by changes in classroom instruction.

[2] Enrollment is likely higher among those families who have previously demonstrated a willingness to pay tuition at private schools. Another study of Bridge Kenya found universal school participation of students who left Bridge Kenya schools before the end of primary school (Gray-Lobe et al., 2021).

negative selection bias in our test score sample. Second, among students who remain enrolled, the program's effects are larger for initially lower-performing students. Together, these patterns indicate that the outreach program may be even more effective at improving learning than our estimates suggest.

A simple model illustrates how parent outreach can generate this tension between improved performance and increased exit. In the model, which builds on Shaked and Sutton (1982), schools are perceived by parents to be vertically differentiated in terms of quality. Parents make decisions about both their effort at home and school choice based on their mental model of education production. When schools encourage parents to update their mental model to recognize how their effort affects their child's education, they may inadvertently make parents more sensitive to perceived quality differences between schools. We show that when schools cannot adjust prices after encouraging parents, lower-quality schools face a disincentive to provide encouragement, even when it is costless. This result suggests there may be a role for third-party (e.g., government) provision of low-cost parental engagement strategies.

To our knowledge, this is the first study to connect the literature on low-cost efforts to encourage parents to be more active in their children's education and that on school choice. A growing experimental literature shows that relatively light-touch, school-initiated interventions, such as invitations to structured parent meetings, can increase parental involvement and improve student outcomes, particularly in disadvantaged settings (Avvisati et al., 2014; Bergman, 2021; Kraft and Rogers, 2015). Dizon-Ross (2019) also shows that providing parents with information about their child's academic ability can shift beliefs, educational investments, and enrollment decisions, with parents increasing enrollment for higher-achieving children and decreasing it for lower-achieving children. However, this literature has largely abstracted from the competitive environment in which many schools operate. Our study indicates that interventions that shape parents' beliefs about how learning is produced can interact with school choice in important ways. When parents update their understanding of how their own effort affects achievement, they may also become more responsive to perceived differences across schools.

Furthermore, this study suggests that competition within education systems can create disincentives to provide even low-cost information that changes parents' mental models. Incentives to attract students in a competitive market (e.g., tuition fees or capitation grants) may affect schools' incentives to share information with parents that emphasizes their agency and encourages them to take action in their child's education. Third-party outreach (e.g., public provision) may address the under-provision of parental encouragement.[3]

[3] These results may explain why schools appear to under-invest in parental outreach in some cases (Kraft

Additionally, we provide evidence from the first large-scale experimental evaluation of a *parent*-facing growth mindset program on child academic outcomes. Smaller-scale studies have found positive impacts of related programs on early-grade reading skills (Andersen and Nielsen, 2016; Rowe and Leech, 2019).[4] While our results may be consistent with the view that growth mindset messaging to parents can cost-effectively improve child academic outcomes at a large scale, we cannot rule out other mechanisms besides parental mindset.

The remainder of this paper is organized as follows: Section 2 provides information on the context of the study, its design, and the data used. Section 3 describes the empirical framework for the analysis. Section 4 describes the results. Section 5 outlines our theoretical model, and section 6 concludes.

2 Context, Study Design & Data

2.1 Context

This study was conducted in private primary schools administered by Bridge Kenya ('Bridge' hereafter), a subsidiary of NewGlobe. NewGlobe operates private schools in India, Kenya, Nigeria, Liberia, and Uganda. NewGlobe also provides materials and training for public school systems in Nigeria and Rwanda. Bridge Kenya has been operating schools since 2009. In 2018, Bridge Kenya operated 250 schools.

Bridge's schools serve a predominantly low-income population and are typically located in urban and peri-urban informal settlements and smaller towns and communities. Tuition at Bridge varies by location and grade but tended to be around US$ 100 per year in 2018, which is similar to many other private schools (Oketch et al., 2010; Heyneman and Stern, 2014; Zuilkowski et al., 2017; Gray-Lobe et al., 2021).

A distinguishing feature of Bridge's model is its extensive use of modern management practices to tightly control quality. Teachers use centrally developed lesson plans that are delivered to teachers using a digital tablet. The lesson plans and software for the tablets follow the national curriculum, are developed centrally by a team in the United States and in Kenya, and then synced to the teacher's tablet remotely. Students are tested seven times a year in each subject to measure student progress using centrally developed standardized assessments. Teachers are required to meet regularly with parents. Meetings typically focus on providing information on student performance. In a prior experimental evaluation, Gray-

and Rogers, 2015).

[4]Experimental evidence on programs attempting to scale up *child*-facing growth mindset programs is mixed (Yeager et al., 2019; Foliano et al., 2019; Ganimian, 2020), but we are not aware of any studies that have attempted to scale up parent-facing programs.

Lobe et al. (2021) find that attending Bridge schools has a large positive impact on test scores, non-academic cognitive skills, and parental engagement.

Parental engagement is high in Bridge schools. Bridge, like other private schools, attracts a self-selected group of parents who are willing to pay more for a higher-quality education.[5] In survey data (to be described in more detail below) among parents in the study, over sixty percent of NewGlobe parents reported talking to their child daily about learning (Figure 3). Over eighty-five percent reported talking to their child about learning at least weekly. Seventy percent reported helping their child with homework at least weekly. Over sixty percent reported meeting with their child's teacher at least monthly. Around sixty percent report knowing their child's class rank.

Survey results indicate there may be some gaps in parental engagement. Over 20 percent of parents report helping their child with homework only annually or less often. Less than 40 percent know their child's most recent test score results. Comparing pupils' baseline percentile rank to parent-reported rank, we find that parents tend to view their child's performance more positively than reality (Figure 4), similar to Dizon-Ross (2019) and Bettinger et al. (2020).

Overall research agenda. The present evaluation is one of several such experimental evaluations of pedagogical programs done in collaboration with NewGlobe's Learning Innovation team. The Learning Innovation unit works to identify ways to improve learning in schools using NewGlobe's materials and test whether variations in materials are sufficiently effective to be implemented at a large scale. The authors have worked with this unit to conduct pedagogical evaluations in schools (van der Haar et al., 2023; Gray-Lobe et al., 2023*a*,*b*; Dam et al., 2023). In addition, NewGlobe has worked with other researchers (Schueler and Rodriguez-Segura, 2020; Romero, Chen and Magari, 2022). Esposito Acosta and Sautmann (2022) to evaluate a similar SMS outreach program aimed at promoting home reading using adaptive experimental methods. They do not report impacts on exit or test scores.

2.2 Experimental Design & Data

This study was conducted in grade 3 and grade 6 classrooms in 202 Bridge Kenya schools. These represented the universe of Bridge's schools for which baseline data were available.

SMS outreach program. Caregivers in the SMS outreach interventions either received growth mindset messages, personalized messages, or both. The *growth* messages aimed to

[5]Surveys suggest that parents of children enrolled in low-fee schools view them as having higher quality instruction (Zuilkowski et al., 2017) than alternative public schools.

use growth mindset theory to encourage parental involvement, emphasizing the value of hard work, the malleability of intelligence, the importance of attendance, and the value of homework (ex. "Intelligence and achievement can be improved through hard work. Keep encouraging your child to work hard on their studies."). All parents received the same message regardless of grade. Additional examples of messages are given in Figure 5. On average, growth messages contained 118 characters.[6] [7] The *personalized* messages shared information on the student's scores on midterm or endterm exams, the class average on the same exam, and one practice problem covering content from the exam (ex. "[First name] [Last name]'s score was [% Score] on their social studies endterm exam. The class average was [Class % Score]. Ask [Pupil name] to tell you where solar power comes from. (answer: the sun)"). Practice problems at the end of each message were pulled from a practice exam corresponding to the student's grade level. Personalized messages contained 207 characters on average.

Caregivers assigned to either the growth or personalized arms received 27 total messages. Those receiving both types received 54 messages. No messages were sent to households in the pure control or the within-classroom control groups.

Messages were delivered to the primary (caregiver) phone number on file for the student. Upon enrollment, parents/caregivers of Bridge students are required to list at least one contact phone number. Caregivers pay tuition through mobile money platforms such as M-Pesa that link mobile phone SIM cards with mobile money accounts. Bridge and caregivers, therefore, have strong incentives to maintain up-to-date mobile phone contact information.

Assignment. Assignment to message conditions was randomized at two levels: classroom and individual. Figure 2 describes assignment and criteria for inclusion in the study.

Classrooms were randomly assigned to either SMS outreach or pure control conditions. A classroom represents a grade-school cell. In the experimental sample, each school contains a single classroom for each grade. Classroom assignment was stratified by terciles of classroom-level lesson completion and class size, an indicator for whether the school's primary-school-leaving exam pass rate was above the median, and a variable indicating the urbanicity of the location (urban, peri-urban, or rural). Stratification was conducted separately for grades 3 and 6 based on these characteristics, but a single random number was drawn for each *school*. As a result, the assignment of grade 3 and 6 to the pure control condition is highly correlated

[6] This estimate is from the first term of the program, for which message text is available.

[7] In other settings, parental mindset has been found to be correlated with parental engagement as well as child mindset and academic outcomes (Gunderson et al., 2013; Haimovitz and Dweck, 2016; Muenks et al., 2015; Song, Barger and Bub, 2022). How children are praised for their behavior is thought to be one way through which children's beliefs are formed about the malleability of traits (Mueller and Dweck, 1998).

within schools, but not perfectly collinear.[8] Randomization strata containing fewer than two schools were assigned deterministically to the outreach condition.

In the second stage, students enrolled in SMS outreach classrooms were randomly assigned to receive one of the four outreach conditions described above.

This second-level assignment was not stratified by classroom so the fraction of students receiving each treatment assignment varies across classrooms and schools.

Caregivers in the within-class control group did not receive messages directly. Differences in outcomes for this group and the pure control group identify the impact of information spillovers from parents of classmates who did receive the messages directly.

Timeline. The SMS intervention operated over three terms from mid-August 2018 (the start of the 2018 third term) to July 2019 (the end of the 2019 second term). Messages were sent every Friday so that caregivers would have the weekend to discuss and act on the information.

Data. Data were provided by Bridge. Data include midterm and endterm exams for each of the three academic terms. Tests were developed centrally by Bridge. Subjects include Kiswahili, English, math, science, social studies, and religious studies. For both Kiswahili and English, test scores are available for specific components, including language and composition, writing, and reading scores. Midterm and endterm tests were graded by their classroom teachers. The number of correct responses was transmitted by the school's head-teacher to a central database via the digital tablet.

We construct a baseline test score index to have a single scalar measure of initial performance, formed by the average of standardized midterm and endterm scores in the term before the study. This score is then use to classify students as above or below median within their classrooms.

Enrollment data come from administrative files indicating the withdrawal date (if any) of all students in the analysis sample. For students who re-enroll, there are multiple withdrawal events. To create a unique withdrawal date corresponding to end-of-program test score attrition, we use the last withdrawal date between August 1, 2018, and August 1, 2019.

Test score outcomes are midterm and endterm tests observed in the three terms after the start of the study. Our preferred specifications pool subject tests because the program was

[8] Still, some schools contain one classroom that received messages, while another was assigned to the pure control group. This fact could lead to contamination of the pure control group if the impact of messages spills over across grades. The main results of this study are similar when we exclude schools in which one grade was assigned to the outreach condition and another was assigned to the pure control condition.

not designed to improve performance in a particular academic subject.[9] We report estimates separately by term, motivated principally by the fact that selective attrition concerns weigh on estimates in later terms.

Bridge conducted two phone call surveys with caregivers regarding engagement in their child's education. A baseline survey in September 2018 collected information on caregiver engagement from a randomly selected sub-sample of caregivers enrolled in Bridge schools. The survey accepted non-parent guardians as respondents. Over 90 percent of respondents were either mothers or fathers. A total of 367 interviews were conducted. Out of these, 217 of the pupils met the inclusion criteria described below. Bridge conducted a second survey at the end of the program in September 2019. This survey included 374 interviews. Out of these, 158 meet the inclusion criteria described below.

Inclusion criteria. The analysis sample is restricted to those students with at least one baseline score. Missing baseline test scores may indicate that students had already left Bridge at the time of the baseline, or that they were not yet enrolled at baseline, but enrolled after the start of term 3 in August 2018 (the start of the SMS outreach program). This restriction is meant to focus the analysis on those students who were active at the start of the SMS outreach program. A consequence of this restriction is that it leaves out students who were enrolled in early August 2018 when the term 2 endterm exams were administered (our baseline measures) but who were not present on any of the days of these exams. The data, however, lacks the level of granularity to distinguish this group from the ones above who were not yet enrolled during this baseline.[10]

The penultimate row of Figure 2 shows the number of classrooms and students assigned to each condition, including classrooms with degenerate risk of assignment to the outreach condition. This represents the sample that will be used in analyses at the individual level. The final row illustrates analogous figures restricting to those classrooms with non-degenerate risk of outreach assignment, representing the sample that will be used in analyses of the effect of outreach relative to the pure control condition.

We relax the inclusion criteria when evaluating impacts on parent survey responses to maximize the sample. The results are similar when restricted to those with at least one baseline test score, although the standard errors are much larger.

[9] For third-grade students, we exclude the 2019 term 1 midterm results for composition, math, and science because these assessments have unusually low follow-up rates. Only five percent of students in pure control classrooms have these assessments compared to around 30 percent in classrooms receiving messages.

[10] Students without baseline test scores are more likely to lack test score data in periods after the start of the program, suggesting that missing baseline data are associated with long-term absence and dropout.

Sample description and covariate balance. Bridge schools are dispersed throughout Kenya, with 40.6 percent in urban or peri-urban areas Table 1.[11] The average Bridge Kenya school had been in operation for around 5 years, and students in our analysis sample had been enrolled in those schools for around 3 years.

Students assigned to different message conditions are similar in terms of baseline test scores. Table 1 compares baseline test scores in all message conditions, including the baseline test score index, as well as individual subject test scores. Estimates come from specifications that pool grade 3 and grade 6 students together, mirroring the primary specification in this study. Baseline data also include the date on which the student enrolled at Bridge originally, allowing us to compare students in terms of the number of years that they have been enrolled at Bridge. For most outreach arms, this characteristic is similar across arms. However, students assigned the growth mindset message condition were enrolled at Bridge for 0.3 fewer years on average. The main results are robust to controlling for the years enrolled at Bridge.

3 Empirical Framework

Our primary empirical specification estimates the impact of the outreach program itself on student turnover and test scores. We are interested in estimating the impact of different experimental conditions on an outcome (retention or test scores) Y_{ij}, where i indexes students-parent dyads and j indexes classrooms. In our primary results, we estimate the following linear model of Y_{ij}:

$$Y_{ij} = \alpha + \beta D_j + \epsilon_{ij} \tag{1}$$

where $D_j \in \{0, 1\}$ is an indicator for assignment of classrooms to receive outreach and ϵ_{ij} is a disturbance term. The causal estimand β reflects the effect of a classroom adopting parental engagement program that sends parents different messages. We estimate Equation 1 using ordinary least squares (OLS).[12] Identification comes from the first-level randomization of schools to the outreach condition.

To estimate the impacts of message content, we estimate the following linear model

$$Y_{ij} = \lambda + \sum_m \delta_m Z_{mi} + \eta_{ij} \tag{2}$$

[11] Rural/urban/peri-urban classifications are created by Bridge for administrative purposes. It is not based on objective criteria. In more densely populated regions (such as Nairobi or Kiambu) peri-urban schools may be classified as ‘rural’. In less densely populated regions, schools in small towns may be classified as ‘urban’.

[12] For reasons of concision, this model abstracts away from the need for controls for randomization strata.

where $m \in M$ indexes the message types – personalized message, growth mindset, personalized + growth mindset, and within class control groups, $Z_{mi} \in \{0, 1\}$ is an indicator for assignment to each group m (The omitted category is the within-classroom control group). Identification of the δ_m parameters comes from the second-level random assignment of students to message content.

Spillover effects on classmates whose parents did not receive messages are estimated by the effect of being assigned to the within-class control group inside an outreach classroom relative to students in pure control classrooms.

Standard errors are clustered at the school level. Although assignment varies at the classroom level, this is due to differences in stratification. A single random number was used for each school. Because the present study is a cluster-randomized trial with small strata (i.e., less than 5 randomization units in at least some strata), there is an argument that standard errors should be clustered at the strata level (Chaisemartin and Ramirez-Cuellar, 2020). However, because the strata vary within schools, doing so would ignore correlation in treatment assignment between classrooms.

Motivated by prior literature, we test for heterogeneous impacts of messages across students with different baseline test scores. Parental engagement is often correlated with higher levels of academic achievement.[13] Parent interventions have been found to have unequal effects on children with different demographic characteristics and test scores. Most studies of the impact of information provision include some discussion of distributional impacts.[14] The provision of feedback on pupil performance may have heterogeneous effects depending on the information that is being provided (e.g. Dizon-Ross, 2019). Furthermore, messages promoting a growth mindset may be more impactful for lower-performing pupils for whom persistent underperformance may reinforce a fixed mindset. An influential meta-analysis of evaluations of child-facing growth mindset interventions emphasized the importance of distributional impacts (Burnette et al., 2022).

To examine impacts on firm profitability, we evaluate the effect of outreach on aggregate classroom enrollment, including those students who do not meet the aforementioned inclusion criteria. Under an assumption that most expensive inputs are fixed, including teacher

[13] See Wilder (2014) and Patall, Cooper and Robinson (2008) for recent meta-analyses.

[14] In most cases, larger impacts of information provision are found for students expected to have less desirable academic outcomes: Berlinski et al. (2016) finds larger impacts on grade attainment and test scores for those at higher risk of grade retention or dropout; Doss et al. (2019) show that programs that tailor information to a child's development level can improve the effectiveness of information; Hastings and Weinstein (2008) show that providing information about school quality is most important for socio-economically disadvantaged households; Bergman and Chan (2021) find larger impacts of parent messaging for students with below-median GPA. One notable exception is Dizon-Ross (2019) which found that providing information about performance for low-achieving students in Malawi led parents to withdraw their children from schools.

compensation, maintenance of facilities, and central office costs of NewGlobe, profits should scale approximately linearly with enrollment, at least for smaller changes in enrollment which would not require changes in staffing. We also assume that prices are largely fixed. Impacts on aggregate classroom enrollment can diverge from the estimates of school exit among the sample that meet inclusion criteria because the schools may be able to replace exiting students. The impact of encouragement on enrollment overall depends on several factors. First, the program itself may attract families who value outreach itself. Second, improvements in average test scores may attract families who either are attracted to higher value-added of the school environment, or who see peer performance as a proxy for school effectiveness. Finally, if classroom enrollment was previously capacity-constrained, then schools may be able to substitute for exiters without any change in demand, although we believe capacity constraints likely played an insignificant role at baseline.[15]

4 Results

4.1 Effect on student turnover

We report estimates of the effect of outreach on several measures of student turnover. Our preferred measures use observation of students in administrative test score records maintained by Bridge as evidence of current enrollment. Students may miss a test due to temporary absence, record-keeping errors, or other idiosyncratic reasons, however, repeatedly missing tests likely reflects a student's exit from Bridge. Our preferred measure of enrollment at the end of the study is an indicator for whether the student sat for any of tests in terms 1 and 2 of 2019 (the final two-thirds of the study). We also consider an indicator of whether the student sat for any test in the final term of the study.

We also use administrative data on withdrawal dates to measure retention. These records may overstate retention if they are not updated promptly for students, especially if students do not inform the administration of their intent to leave (i.e, "ghosting"). Using these data, we evaluate impacts on an indicator for whether a student was enrolled in the 2019 school year. Most school changes occur at the start of the new academic year. We use the date January 15, 2019 as the cut-off for withdrawals instead of January 3 (the opening date of the 2019 academic year) because, again, withdrawals may not be recorded promptly. We also evaluate the impact of outreach on the number of days that student was enrolled in Bridge during the study. Finally, we estimate a Cox proportional hazard model of exit using the

[15] We observe only a handful of classrooms with more than 40 students, and most have fewer than 15. While the research team is unaware of any formal cap on classroom size, previous statements from Jay Kimmelman, a founder of NewGlobe indicate that this cap would be around 60 (Rangan and Lee, 2010).

recorded withdrawal date to measure survival time. We estimate two models, one over the period from the start of the program to the beginning of the 2019 school year and another for hazard during the entire study period.

Turnover in Bridge schools is high. Depending on how we measure continuous enrollment, between 67.7 and 77.0 percent of children remained in Bridge through the study (bottom row, columns 1 and 3 of Table 2). Only 86.3 were recorded as still enrolled as of the start of the 2019 academic year. These results are consistent with other studies of Bridge: Two-thirds of the students in Gray-Lobe et al. (2021) who were enrolled at Bridge in 2015 had left by the end of 2017.

Outreach generated significant increases in exit rates during the study. Overall, students in outreach classrooms were 5 percentage points more likely to leave Bridge using our preferred measure (Column 1 Table 2), a 6.4 percent effect relative to the pure control mean. For students who were above the median in terms of initial academic performance, the effect is an 8.4 percentage point reduction. For below-median students, outreach is associated with a 1.8 percentage point reduction in Bridge enrollment, and the difference between above and below-median students is statistically significant at the 10 percent level. Using other measures of turnover, we find similar results. In all cases, estimates are consistent with a reduction in Bridge enrollment which is larger for above median students. The impacts on above-median students are statistically significant for all measures at least the 10 percent level.

The effect of outreach on turnover is similar regardless of message content. Table 3 reports estimates of Equation 2 using the same set of turnover measures. All message types are associated with higher student turnover.

Outreach effects on student turnover appear to spill over onto students whose parents did not directly receive messages. The effect of being assigned to the within-class control group relative to the pure control is a 6.1 percentage point reduction in the likelihood that a student has any test score in Terms 1 and 2. Although we cannot reject the hypothesis that the effects of all arms are equal, it is noteworthy that point estimates of the effect of being assigned to the within-class control group relative to a pure control classroom are larger than those for the groups receiving messages directly. The effects on turnover of above-median students are especially large and statistically significant for the within-class control group.

4.2 Effects on test scores

Interpretation of test score effects is complicated by the results above regarding exit from Bridge because test scores are only observed for students conditional on their continued enrollment at Bridge. We report estimates of differential attrition in Appendix Tables A3, A4, and A5, corresponding to the effect of outreach itself, the impact of different message types, and the effect by term. Overall, we find that outreach is associated with 6.5 percentage points lower follow-up in the second term of the study, and 7.9 percentage points lower follow-up in the third term (Appendix Table A3).[16]

Outreach is associated with higher test scores on average. Students in outreach classrooms scored 0.070 standard deviations higher in the first term (column 1 of Table 4), and 0.064 standard deviations overall (column 7).

Estimated effects are larger for below-median students. The effect on below-median students in the first term is 0.104 standard deviations (column 2), and 0.092 overall (column 8). This may reflect heterogeneity in the impacts of outreach. However, it also may reflect a smaller negative selection effect due to attrition.

The evidence of negative selective attrition in test scores indicates that test score effects may underestimate the effect of outreach on test scores. Appendix Table A7 reports results using inverse probability weighting (IPW). Estimates using IPW are larger than those without, but only by approximately 0.04 standard deviation.

As above, message content itself appears to matter little. All message arms are associated with positive test score effects. Growth mindset messages generate a 0.08 standard deviation gain in test scores in the first term relative to the pure control group. The effects of messages including personalized information are smaller and statistically insignificant. However, it is not possible to reject the hypothesis that the effects of all arms are equal.

Students in outreach classrooms whose parents do not directly receive messages appear to benefit as well, indicating the presence of spillover effects. Assignment to the within-class control group is associated with a 0.089 standard deviation test score gain in the first term. While somewhat surprising, this result is consistent with Bettinger et al. (2020).

Outreach appears to have smaller impacts at the bottom of the test score distribution. Figure 6 reports quantile regression estimates separately by term and in aggregate. Overall, the estimates suggest the intervention shifted the distribution of test scores fairly evenly. There is some evidence of larger, more statistically significant effects among above-median points in the distribution, but we do not see clear evidence of distributional effects.

[16]We note that these differences are larger than those observed in the withdrawal records. To test the hypothesis that withdrawal records can explain the attrition differential, we estimate the impact of outreach on an imputed follow-up variable indicating whether the student's withdrawal date precedes the assessment date. We can reject the null hypothesis that the observed follow-up differential equals the (smaller) imputed follow-up differential at less than the 1 percent level. This could reflect error in the withdrawal records (e.g., due to lagged recording) or that the attrition difference is due to other factors, such as student avoidance of tests.

4.3 Effects of messages on caregiver engagement and knowledge

Overall, outreach did not generate detectable changes in the behavior, attitudes, or information of parents. Panel A Table 7 reports results from estimates of Equation 1 on responses to the phone call survey with parents. Parents reported whether they know their child's marks and rank. The survey asked parents how often they talk to their child about learning, help their child with homework, and talk to their child's teacher. We transform the categorical responses into dichotomous outcomes by choosing the frequency closest to dividing the responses in half. No estimate is statistically distinguishable from zero at a conventional level.

Similarly, we find little evidence of impacts of specific messages (Panel B). It is perhaps noteworthy that parents in the within-class control group are 26.4 percentage points less likely to report that they know their child's test scores. This could indicate that these households felt that they had less information than other parents. However, given the high levels of performance feedback in Bridge Kenya schools, the fact that we do not see a comparable effect for parents in the growth message condition (who also did not receive performance information), and we do not see a comparable effect on parents' knowledge of student class rank, we are inclined toward the view that this estimate reflects sampling variation and Type M error (Gelman and Carlin, 2014).

4.4 Cost-effectiveness & impacts on firm profit

We briefly discuss the cost-effectiveness of the outreach program. We start by discussing the cost-effectiveness of the policy within the conventional framework in the education impact evaluation literature. Because we are unable to say whether students who exited benefited or not, we focus the discussion on the estimated average effect for those who remained. We then briefly discuss the potential impacts on a profit-maximizing firm, for which exit represents a loss in revenue.

Results above are consistent with SMS outreach being a highly cost-effective educational intervention in this specific context. Classrooms assigned to receive outreach perform 0.070 standard deviations higher than pure control classrooms in the first term, or 0.0875 learning adjusted years of schooling (Angrist et al., 2020).[17] The marginal cost of messages per pupil

[17] We consider the cost-effectiveness of the program in the first term for two reasons: 1) first term results appear to be less compromised by selective attrition; and 2) this study provides some empirical support that the marginal impact of the program may diminish over time, suggesting that an efficient program might run over a shorter time. Results from Bettinger et al. (2020) also suggest effects of text-based parental engagement campaigns may diminish over time. This analysis is similar when considering aggregate impacts and aggregate costs.

is US$ 0.031.[18] While NewGlobe is unable to provide an exact estimate of the amount of managerial time required to implement the program, we conservatively estimate these costs to be around $2,307 in the first term.[19] The total per-pupil cost of the program, assuming conservatively that the program's impact is only for 3,134 pupils in the analysis sample, is, therefore, 2307.031/3,134=US$0.736, which means outreach produced almost 12 LAYS per US$ 100.[20] This is large relative to most education evaluations included in the 2023 recommendations of the Global Education Evidence Advisory Panel (Akyeampong et al., 2023).

These calculations are highly sensitive to consideration of other costs that, in this study's setting, are sunk. We evaluated this program in a setting where contact data was already available. Collecting contact information at a large scale could be challenging for some organizations and may not be excludable from the costs of the program. The possibility of spillover effects as evidenced by the within-class control group, suggests that such contact data need not be comprehensive. However, extrapolation from this setting to a program where fewer students within the classroom receive direct messages requires assumptions about the relationship between the concentration of direct messaging and the spillover effect. Data on academic performance may also represent an additional cost of the program.[21]

The results suggest that such programs could be especially effective in the public sector, given larger scale and public finance impacts of student exit. Average fixed costs of managing an outreach program may be even lower given the larger scale of operation. Furthermore, any exit effects (from families choosing private schools) could lead to savings or higher per-pupil expenditure.

Costs of the program are larger from the perspective of NewGlobe. We estimate that classrooms running the program have approximately 0.5 fewer students,[22] which in Bridge schools represents a cost of around US$8,000 in lost revenue. Setting aside potential non-financial motivations of the social enterprise, the program represented a loss in profitability.

[18]The cost of a message is $ 0.0046. Nine messages were sent each term. Three-quarters of students in outreach classrooms received messages.

[19]This comes from assuming one week of full-time work from an instructional designer earning $ 70,000 annually and two weeks of full-time work from an entry-level staff member earning $ 25,000 annually.

[20]Using the entire 3,134 included sample implicitly assumes that the average treatment effect for retained students is equal to that for exiters. If we assume that test scores of exiters are unaffected, cost per retained student retained through the study period would be about to one dollar, and the LAYS per 100 US$ would be around 8.75.

[21]While there is little support for the view that personalized information is critical to text-based parental outreach programs, further evidence is needed to confirm this.

[22]Appendix Table A1 reports the impacts of outreach on total enrollment (including students who do not meet the evaluation's inclusion design).

4.5 Sub-group analysis

In this section, we examine effects separately for sub-groups, focusing on the impact of outreach itself for conciseness. We report impacts on student turnover and test scores. For turnover we focus on our preferred measure of retention: having at least one test score in the 2019 academic year. For test scores, we report impacts on 2018, term 1 scores, the scores that appear to be least affected by selective attrition.

Impacts on turnover are broadly similar for all sub-groups (Panel A Table 6). The impact is slightly larger for grade 3 students (5.7 percentage point decrease in retention) than grade 6 (4.0 percentage point decrease), as is the interaction with initial performance. Impacts are also larger for rural schools (7.8 percentage points) than for urban schools (3.4 percentage points). Impacts for boys are much larger (7.5 percentage points) than for girls (1.8 percentage points). Importantly, it is possible to reject the null hypothesis for top students at at least the 10 percent level for all sub-groups.

Impacts on test scores are also broadly similar for all sub-groups. Main effects, however, are statistically insignificant for all except for girls (0.091 standard deviations). We estimate a negative interaction with above median in all cases.

5 A Model of Vertical Differentiation and Parental Engagement

To understand how competitive pressures affect schools' incentives to engage parents, we develop a model of vertical differentiation in the spirit of Shaked and Sutton (1982). Our model incorporates parental beliefs about education production and schools' ability to influence these beliefs. The key insight is that when schools influence parents' production function, they may inadvertently make parents more sensitive to school quality differences - potentially leading to student exits from schools perceived to be lower quality.

5.1 Model Setup

Consider a market with two schools indexed by $i \in \{1, 2\}$ that differ in quality, where school quality is denoted by s_i. Without loss of generality, we assume $0 < s_1 < s_2$, so that school 2 is the higher-quality institution.

Let A denote student achievement (or academic performance). Parents make decisions based on beliefs about how achievement is produced, and schools can influence these beliefs through low-cost encouragement. We assume encouragement costs are negligible, motivated by the low marginal cost of providing SMS messages in our empirical setting.

Initially, parents believe achievement is determined solely by school quality:

$$A^B = s_i$$

where A^B represents achievement under parents' beliefs. The true achievement production function is:

$$A^U = \alpha s_i + \beta e$$

where A^U is true student achievement, $\alpha > 1$ are the returns to school quality, $e \in \{0, 1\}$ is binary parental effort, and $\beta > 0$ are the returns to parental effort.

In our model, parental effort at home is allowed to reduce schools' costs of educating students. This could occur through multiple channels: parental effort may accelerate learning (reducing the need for differentiated instruction), reduce behavioral problems (lowering classroom management costs), or generally decrease the resources needed to achieve given educational outcomes.

5.2 Parents' Decision Problem

Parents are heterogeneous in their willingness to pay for achievement, represented by preference parameter θ. This parameter is distributed uniformly on $[\underline{\theta}, \bar{\theta}]$, where $\bar{\theta} > 2\underline{\theta}$. This heterogeneity may reflect both variation in the marginal value of income and tastes for education, following Tirole (1988). High-valuation households may thus be a mixture of wealthier households and those more willing to invest in education at any income level.

A parent with preference θ chooses school i to maximize

$$U = \theta E[A] - p_i$$

where $E[A]$ is their expected child achievement given beliefs, and p_i is the tuition price for school i.

Without encouragement, parents expect $A = s_i$ and choose school 1 if: $\theta s_1 - p_1 \geq \theta s_2 - p_2$. This defines a cutoff $\theta^* = (p_2 - p_1)/\Delta s$, where $\Delta s = s_2 - s_1$. Parents with $\theta < \theta^*$ choose school 1. With encouragement, parents learn the true production function. When parents are influenced to understand the true production function, this enrollment threshold becomes $\theta^* = \dfrac{p_2 - p_1}{\alpha \Delta s} < \dfrac{p_2 - p_1}{\Delta s}$.

This shift in the threshold demonstrates how encouragement can reduce enrollment in lower-quality schools by making parents more sensitive to quality differences. The magnitude of this effect increases with α, the true sensitivity of outcomes to school quality.

5.3 Schools' Decision Problem

Schools must decide whether to encourage parents, recognizing that this decision affects both parental effort school choice. We assume that parental effort at home reduces schools' marginal costs. For example, it may accelerate learning, reducing the need for differentiated instruction, or it may reduce behavioral problems, reducing the effort needed to manage classrooms. The marginal cost of educating a child without parental effort is given by $\bar{c}$. The marginal cost with parental effort is given by $\underline{c}$ where $\underline{c} < \bar{c}$. The marginal costs savings to the school from parental effort is given by $\Delta c = \bar{c} - \underline{c}$.

We assume that the cost of encouragement itself is negligible, motivated by the low marginal cost of SMS messages in our empirical setting. This assumption allows us to focus on strategic disincentives beyond direct costs of provision.

5.4 Equilibrium

We now characterize the equilibrium outcomes under a fixed pricing regime; schools cannot adjust their prices after their encouragement decision.This price rigidity reflects the fact that most schools must continually recruit new students and may face reputational costs from price adjustments that appear opportunistic.

We make two technical assumptions following the textbook treatment in Tirole (1988)

Assumption 1. $\bar{\theta} > 2\underline{\theta}$

Assumption 2. $\bar{c} + \frac{\bar{\theta}-2\underline{\theta}}{3}(s_2 - s_1) \leq \underline{\theta} s_1$

Assumption 1 ensures that there's enough variation in preferences for demand for both schools in equilibrium. Assumption 2 ensures that the market is "covered", so no family decides not to enroll in one of the schools.

Proposition 1. *Under fixed prices:*

1. *The high-quality school always provides encouragement*

2. *The low-quality school provides encouragement if and only if:*

$$\frac{(p_1 - \bar{c})(p_2 - p_1)(\alpha - 1)}{p_2 - p_1 - \underline{\theta}\alpha\Delta s} < \Delta c$$

The intuition behind Propositions 1 reveals important asymmetries between schools. The high-quality school (s_2) benefits unambiguously from encouragement: parents who update their beliefs about education production will value quality more highly ($\alpha > 1$), and no student who initially chose the high-quality school will switch to the lower-quality option. Combined with the cost savings from increased parental effort (Δc), this makes encouragement strictly optimal for school 2. However, even with negligible encouragement costs, the lower-quality school may optimally choose not to encourage parents. When $\alpha = 1$, i.e., parents' beliefs about the value of school quality is correct, the lower-quality school's decision is identical to that of the higher-quality school. However, lower-quality schools may face steeper costs in terms of lost profits whenever $\alpha > 1$.

5.5 Model Implications

This model provides a parsimonious interpretation of the empirical results reported in this paper. If valuations of educational output correlate with academic performance, the model naturally explains why exit effects concentrate among higher-achieving students.

If Δc, the cost savings from encouraging children, is sufficiently low, there will be no equilibrium in which school 1 encourages parents. This suggests a potential market failure in the provision of parent encouragement, particularly among schools serving lower-achieving students who might benefit most from increased parental engagement. Government agencies or non-profit organizations might be better positioned to provide encouragement because they can reach parents across multiple schools without facing the same competitive pressures as individual schools. Moreover, these organizations can internalize the broader social benefits of parent engagement that may not be captured in individual schools' decision-making. This institutional solution might help address the systematic underprovision of parent engagement initiatives, particularly in more competitive educational markets.

The central theoretical insight that schools may face a disincentive to provide encouragement can be generated with other assumptions about firm conduct. In markets with more competition, short-term capacity constraints can mean that firms receive a markup on the marginal student. A market with horizontal differentiation (and uncertainty about match effects) can also generate markups.

6 Conclusion

This study evaluated a large-scale, randomized trial that provided SMS outreach for one year to caregivers of grade 3 and grade 6 students enrolled at Bridge Kenya's private schools. Outreach reduced retention, especially among top-performing students. Outreach also increased test scores, especially for lower-performing students suggesting that these students may benefit from parental outreach via SMS.

One interpretation of the negative effect on student retention is that encouragement led parents to seek out new schools. Many Kenyan parents, including those who are willing to pay for private school, see school choice as an important way in which they can actively promote their child's education. Caregivers in Kenya are closely involved in their children's education by raising resources to pay for school fees and materials (Oketch and Rolleston, 2007).[23] Encouraging parents to appreciate the malleability of their child's education and their agency in shaping that education may, therefore, be construed as a call to choose another, perhaps more expensive, educational option.

A limitation of this study is that we lack data on where students who left Bridge enrolled. Based on the context and the nature of the intervention it is natural to speculate that parents left to seek out schools that were perceived to be appropriate for their advanced students. Although impact evaluations indicate that Bridge schools are highly effective at improving test scores, their facilities are more spare and teachers are less educated than is common in Kenya (Gray-Lobe et al., 2021). Bridge schools are typically constructed of wood and iron sheets (Education International, 2016; Gray-Lobe et al., 2021). In most markets, Bridge competes with more expensive schools that attract higher-performing students from, on average, wealthier families. Therefore, it seems plausible that high-performing parents took the growth message mindset program as encouragement to seek out a school where their child would be challenged.

Our study is limited in its ability to identify a particular mechanism driving either the exit or test score effects. We discuss a few candidates and evidence for and against each. A satisfying explanation should explain how outreach may have benefitted households directly, the robust negative impact on student retention on households, and the large positive impacts on their test scores.

Our preferred interpretation is given by the theoretical model above as it provides a parsimonious explanation for the empirical effects. We only need to assume that families with higher valuations of education (or lower marginal valuations of the money paid for tuition) are those with higher-achieving students. In this case, families of higher-performing students were persuaded by the message of the importance of providing their children with a high-quality academic environment to seek out schools that they perceived to do more to advance their education. Meanwhile, parents with lower valuations whose children tend to

[23] Recent policy efforts seek to capitalize on parental engagement by explicitly laying out roles for caregivers as supporters of their children's education (KICD, 2019). However, these efforts have faced challenges, as many caregivers in Kenya view education as the responsibility of teachers, not parents (Spernes, 2011; Muigai, 2018). Many parents lack time to be engaged, and they may lack the skills to be effective: Among adults aged 25 to 35, 50 percent have completed only a primary school education and 26 percent are unable to read in any language (Kenya National Bureau of Statistics, 2016).

have lower test scores increased their effort at home in response to encouragement. While encouragement may have led these households to appreciate the role of school quality more than they did before, this change was not large enough to induce them to choose an entirely different school.

Although the parental mindset mechanism appears to be consistent with the results, we also cannot reject the hypothesis that the growth messages had the same effect as the other arms. As in many growth mindset interventions, we are unable to distinguish the effects of changes in mindset from broader encouragement effects (Sisk et al., 2018; Macnamara and Burgoyne, 2022). Other studies have shown that similar encouragement to parents can promote more active parental engagement, even without any explicit connection to growth mindset (Bettinger et al., 2020). In any case, the study suggests that growth mindset theory at least offers useful language with which to encourage parents.

The disclosure of performance information may also be driving the effect, either by providing parents with encouragement or information, and potentially putting competitive pressures on the school. Many other studies have found sharing personalized information can change parental behavior and affect academic outcomes (Doss et al., 2019; Bursztyn and Coffman, 2017; Berlinski et al., 2016). Disclosing information about the relative performance of schools to parents has been found to affect both school choice and school quality (Hastings and Weinstein, 2008; Andrabi, Das and Khwaja, 2017). In this interpretation, the test score impacts might arise through competitive pressures because the sharing of performance information unilaterally disclosed information about the school's performance. If caregivers attribute peer performance with value-added (Abdulkadiroğlu et al., 2020), a parent may prefer for their child to be lower in their school's distribution, believing that top students are those who cannot benefit more. It seems unlikely that the positive spillovers could arise directly from transmission of the personalized performance information, but the messages also shared distributional information that might have been useful more broadly. If sharing this information increased competitive pressure on schools, it might have induced greater effort by local staff explaining the aggregate test score impacts. The absence of any clear impact on reported parental behavior (aside from school choice) supports this interpretation.

Other interpretations, including mixtures of the two mechanisms above, could also explain the results. One possible explanation for the increase in student exit is that parents responded to perceived inequities in the distribution of outreach within schools. Caregivers in within-classroom control groups may have been aware that other households were receiving SMS messages, potentially leading to concerns about unequal access to information or support. However, while this mechanism cannot be ruled out based on the available data, it appears unlikely to be a primary driver. Bridge did not report receiving widespread complaints or requests to join the program. Moreover, this explanation does not readily account for the observed heterogeneity in exit across the distribution of students.

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Tables and Figures

Table 1: Sample description and covariate balance

	School-level assignment			Student-level (message content) assigment				
	Pure control mean	Outreach	Number of students	Within-class control mean	Growth message	Personalized message	Personalized& growth message	Number of students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline test score index	-0.016	0.058	3,515	0.018	0.001	-0.039	-0.006	2,183
		(0.066)			(0.049)	(0.050)	(0.049)	
Math score	-0.032	0.046	3,388	0.001	-0.010	-0.039	-0.033	2,112
		(0.073)			(0.061)	(0.062)	(0.062)	
Language score	-0.019	0.060	3,371	0.026	0.008	-0.034	0.017	2,045
		(0.066)			(0.051)	(0.053)	(0.051)	
Science score	-0.038	0.064	1,305	0.056	0.023	0.036	0.010	697
		(0.126)			(0.103)	(0.101)	(0.103)	
Social studies score	0.066	0.045	1,320	0.047	0.032	-0.010	-0.019	697
		(0.098)			(0.101)	(0.096)	(0.099)	
Age	10.812	-0.035	3,109	10.529	-0.031	-0.046	-0.059	2,172
		(0.059)			(0.064)	(0.068)	(0.069)	
Female	0.462	0.009	3,134	0.476	0.001	-0.005	0.000	2,183
		(0.018)			(0.030)	(0.030)	(0.030)	
Years enrolled	2.710	-0.168*	3,515	2.479	0.225**	0.275***	0.249***	2,183
		(0.102)			(0.102)	(0.103)	(0.102)	
Years since academy established	5.105	-0.171	3,515	4.889	0.062	0.122*	-0.012	2,183
		(0.173)			(0.073)	(0.074)	(0.071)	
Urban location	0.406	0.016	3,515	0.403	0.091***	0.058*	0.034	2,183
		(0.012)			(0.030)	(0.030)	(0.030)	
Pupil-teacher ratio	18.694	-0.394	3,502	18.667	0.030	-0.018	-0.033	2,170
		(0.702)			(0.473)	(0.485)	(0.469)	
Overall KCPE pass rate	0.724	-0.013	3,454	0.665	-0.022*	-0.024**	-0.021*	2,183
		(0.023)			(0.012)	(0.012)	(0.012)	
Joint F-test (p-value)		0.582	3,035		0.488	0.467	0.869	2,159

Notes: This table describes the sample and compares characteristics of pupils assigned to each message condition. The first three columns correspond to the first-level assignment of classrooms to the outreach condition. Column 1 gives the pure control mean. Column 2 gives the estimated difference between the outreach and pure control groups. The joint F-test p-value tests the hypothesis that there is no difference between the message type indicated in the column header and the pure control group. The joint test p-values in the bottom row exclude science and social studies scores due to low coverage. Columns 4-8 correspond to the second-level assignment of students to message content. The within-classroom control mean is given in column 4, and the difference between each message form the within-classroom control is given in Columns 5-7. The p-value from a joint test of the independence between all message groups and all covariates is 0.170. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 2: Effect of outreach on student turnover

	Ordinary least squares								Proportional hazard regression			
	Has any term 1 & 2 scores		Has any term 2 scores		Enrolled at start of 2019		Days enrolled		Exit by start of 2019		Exit by end of study	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outreach classroom	-0.050**	-0.018	-0.055**	-0.023	-0.047**	-0.019	-7.661	-0.606	0.312**	0.106	0.139	0.062
	(0.020)	(0.028)	(0.026)	(0.032)	(0.018)	(0.025)	(4.740)	(6.671)	(0.127)	(0.160)	(0.087)	(0.106)
Outreach × above median		-0.066*		-0.065		-0.057**		-14.619*		0.492**		0.176
		(0.037)		(0.045)		(0.028)		(8.442)		(0.209)		(0.160)
Above median	-0.013	0.033	-0.016	0.030	-0.012	0.028	-2.094	8.195	0.074	-0.302	0.030	-0.098
	(0.019)	(0.033)	(0.024)	(0.043)	(0.015)	(0.026)	(4.700)	(7.872)	(0.102)	(0.200)	(0.089)	(0.157)
Observations	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134
Outreach effect on top students = 0 (p-value)		0.001		0.020		0.000		0.012		0.000		0.072
Pure control mean	0.770	0.770	0.677	0.677	0.863	0.863	336.279	336.279	0.137	0.137	0.297	0.297

Notes: This table reports estimates of the outreach program on measures of school exit. Odd columns report the average effect of outreach. All specifications include controls for the grade, baseline test score index, and the probability of assignment to the outreach condition for the classroom's randomization strata. Even columns report estimates from a model that interacts outreach assignment with an indicator for whether the student was in the bottom or top half of the initial test score distribution. The first two columns report impacts on an indicator for whether the student has any test score record in the final two terms of the study. Columns 3 and 4 use an indicator for whether the child has any test scores in the final term. Columns 5 and 6 use an indicator for whether the recorded withdrawal date for the student was before the assessment date in terms 1 or 2. Columns 7 and 8 use an outcome measuring the number of days that the student was recorded as enrolled during the period from the start to the end of the study. Columns 9 and 10 estimate a Cox proportional hazard model using information on the recorded withdrawal date. Errors are clustered at the school level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 3: Effects of outreach on student turnover by message type

	Ordinary least squares								Proportional hazard regression			
	Has any term 1 & 2 scores		Has any term 2 scores		Enrolled at start of 2019		Days enrolled		Exit by start of 2019		Exit by end of study	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Growth message	-0.031	0.006	-0.045	0.006	-0.014	0.032	-1.241	10.113	0.090	-0.234	0.054	-0.116
	(0.026)	(0.037)	(0.032)	(0.040)	(0.021)	(0.028)	(5.967)	(8.292)	(0.158)	(0.211)	(0.113)	(0.146)
Personalized message	-0.060**	-0.047	-0.053*	-0.047	-0.061***	-0.049	-11.458*	-9.574	0.404***	0.283	0.185*	0.169
	(0.025)	(0.037)	(0.032)	(0.041)	(0.022)	(0.033)	(5.801)	(8.465)	(0.142)	(0.186)	(0.103)	(0.131)
Personalized & growth message	-0.048*	-0.032	-0.055*	-0.029	-0.047*	-0.044	-5.856	-5.194	0.306*	0.241	0.115	0.161
	(0.028)	(0.040)	(0.032)	(0.042)	(0.026)	(0.038)	(6.479)	(9.414)	(0.168)	(0.211)	(0.110)	(0.138)
Within-class control	-0.061**	0.001	-0.066**	-0.024	-0.065***	-0.015	-12.095**	2.102	0.419***	0.070	0.202*	0.020
	(0.023)	(0.031)	(0.031)	(0.039)	(0.022)	(0.030)	(6.059)	(8.062)	(0.144)	(0.187)	(0.108)	(0.139)
Growth × above		-0.075		-0.107*		-0.096***		-23.753**		0.739***		0.378*
		(0.048)		(0.058)		(0.036)		(10.666)		(0.277)		(0.206)
Personalized × above		-0.027		-0.013		-0.026		-4.019		0.307		0.037
		(0.049)		(0.057)		(0.041)		(11.390)		(0.268)		(0.203)
Personalized & growth × above		-0.033		-0.053		-0.005		-1.268		0.164		-0.116
		(0.052)		(0.059)		(0.043)		(12.211)		(0.291)		(0.221)
Within control × above		-0.128***		-0.087		-0.101**		-29.201**		0.775***		0.395**
		(0.048)		(0.058)		(0.040)		(11.317)		(0.260)		(0.201)
Above median	-0.013	0.034	-0.016	0.030	-0.012	0.028	-1.989	8.284	0.069	-0.307	0.029	-0.100
	(0.019)	(0.033)	(0.024)	(0.043)	(0.015)	(0.026)	(4.708)	(7.874)	(0.103)	(0.199)	(0.089)	(0.157)
Observations	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134	3,134
P-values												
Effect on top students = 0												
-Growth		0.034		0.028		0.018		0.073		0.015		0.104
-Personalized		0.031		0.178		0.006		0.082		0.004		0.198
-Growth + Personalized		0.071		0.072		0.087		0.436		0.081		0.798
-Within-class control		0.001		0.018		0.000		0.002		0.000		0.009
All main effects = 0	0.056	0.526	0.294	0.572	0.002	0.030	0.092	0.158	0.001	0.055	0.228	0.195
All top student effects = 0		0.012		0.147		0.003		0.027		0.001		0.055
All interactions = 0		0.077		0.278		0.032		0.017		0.012		0.028
Within-class control mean	0.770	0.770	0.677	0.677	0.863	0.863	336.279	336.279	0.137	0.137	0.297	0.297

Notes: This table reports estimates of the effect of assignment to different message types on student turnover relative to the pure control group. Details of specification and dependent variable construction are as in Table 2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 4: Effects on test scores by term

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outreach classroom	0.070*	0.104**	0.075	0.100	0.047	0.066	0.064*	0.092**
	(0.042)	(0.051)	(0.055)	(0.064)	(0.052)	(0.062)	(0.037)	(0.045)
Outreach classroom × above		-0.068*		-0.048		-0.039		-0.055*
		(0.035)		(0.041)		(0.044)		(0.033)
Above median	0.097***	0.145***	0.165***	0.198***	0.242***	0.268***	0.162***	0.200***
	(0.035)	(0.040)	(0.042)	(0.047)	(0.040)	(0.045)	(0.030)	(0.035)
Number tests	38,930	38,930	28,825	28,825	30,246	30,246	98,001	98,001
Number students	2,870	2,870	2,270	2,270	2,119	2,119	2,959	2,959
Top-student effect = 0		0.361		0.331		0.587		0.301
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of the messaging program on student test scores relative to the pure control group. Each column represents a separate specification. Specifications include multiple test scores for each student. All specifications include controls for the baseline test score index interacted with test score fixed effects as well as a linear control for the probability of treatment in the classroom's randomization strata. Errors are clustered at the school level. Each ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 5: Effects of outreach on test scores by message type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth message	0.080*	0.120**	0.096	0.148**	0.061	0.095	0.079**	0.122**
	(0.042)	(0.052)	(0.059)	(0.074)	(0.058)	(0.074)	(0.040)	(0.050)
Personalized message	0.055	0.065	0.049	0.055	0.036	0.074	0.048	0.066
	(0.051)	(0.061)	(0.058)	(0.077)	(0.057)	(0.075)	(0.043)	(0.057)
Personalized & growth message	0.056	0.072	0.078	0.115	0.038	0.070	0.057	0.084*
	(0.043)	(0.055)	(0.061)	(0.072)	(0.058)	(0.071)	(0.040)	(0.049)
Within-class control	0.089*	0.156**	0.076	0.076	0.051	0.025	0.073*	0.093*
	(0.049)	(0.073)	(0.060)	(0.068)	(0.061)	(0.065)	(0.041)	(0.050)
Growth × above		-0.083**		-0.105		-0.068		-0.086*
		(0.041)		(0.065)		(0.067)		(0.046)
Personalized × above		-0.021		-0.011		-0.073		-0.036
		(0.050)		(0.066)		(0.068)		(0.051)
Personalized & growth × above		-0.030		-0.073		-0.064		-0.054
		(0.046)		(0.062)		(0.058)		(0.042)
Within control × above		-0.138*		0.002		0.057		-0.039
		(0.071)		(0.059)		(0.063)		(0.050)
Above median	0.097***	0.145***	0.166***	0.198***	0.243***	0.267***	0.163***	0.200***
	(0.035)	(0.040)	(0.042)	(0.047)	(0.040)	(0.045)	(0.030)	(0.035)
Number tests	38,930	38,930	28,825	28,825	30,246	30,246	98,001	98,001
Number students	2,870	2,870	2,270	2,270	2,119	2,119	2,959	2,959
P-values								
Effect on top students=0								
-Growth		0.349		0.480		0.641		0.381
-Personalized		0.394		0.432		0.985		0.475
-Growth & personal		0.311		0.525		0.915		0.466
-Within-class control		0.675		0.233		0.264		0.249
All main effects = 0	0.292	0.110	0.456	0.294	0.883	0.552	0.320	0.180
All top-student effects = 0		0.801		0.834		0.715		0.846
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of assignment to each message condition relative to the pure control. Details of specification and dependent variable construction are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 6: Effects on student turnover and test scores sub-groups

	Grade 3		Grade 6		Urban		Rural		Girls		Boys	
Panel A: Student turnover effects												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outreach classroom	-0.057**	-0.030	-0.040	-0.008	-0.034	0.007	-0.078**	-0.056	-0.018	0.024	-0.075***	-0.050
	(0.023)	(0.033)	(0.032)	(0.043)	(0.026)	(0.034)	(0.032)	(0.047)	(0.029)	(0.044)	(0.025)	(0.036)
Outreach × above median		-0.057		-0.067		-0.085**		-0.046		-0.086		-0.053
		(0.041)		(0.060)		(0.041)		(0.062)		(0.054)		(0.048)
Above median	-0.041*	0.001	0.043	0.086	-0.008	0.053	-0.019	0.013	-0.025	0.035	-0.003	0.035
	(0.022)	(0.038)	(0.038)	(0.054)	(0.023)	(0.034)	(0.033)	(0.057)	(0.026)	(0.046)	(0.028)	(0.048)
Observations	1950	1950	1184	1184	1747	1747	1387	1387	1475	1475	1659	1659
Outreach effect on top students = 0 (p-value)		0.003		0.098		0.014		0.018		0.083		0.002
Pure control mean	0.795	0.795	0.735	0.735	0.779	0.779	0.759	0.759	0.740	0.740	0.795	0.795
Panel B: Test score impacts												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outreach classroom	0.066	0.103	0.080	0.103	0.071	0.098	0.076	0.120	0.091**	0.110**	0.050	0.096*
	(0.056)	(0.067)	(0.060)	(0.068)	(0.052)	(0.066)	(0.062)	(0.075)	(0.045)	(0.052)	(0.043)	(0.057)
Outreach classroom × above		-0.073		-0.048		-0.055		-0.088*		-0.040		-0.093*
		(0.045)		(0.051)		(0.046)		(0.053)		(0.038)		(0.049)
Above median	0.072**	0.127***	0.138	0.167**	0.147***	0.186***	0.031	0.093*	0.068*	0.096**	0.124***	0.190***
	(0.036)	(0.044)	(0.084)	(0.082)	(0.045)	(0.054)	(0.047)	(0.054)	(0.035)	(0.042)	(0.045)	(0.051)
Number tests	24617	24617	14313	14313	22184	22184	16746	16746	18390	18390	20540	20540
Number students	1806	1806	1064	1064	1630	1630	1240	1240	1365	1365	1505	1505
Top-student effect = 0		0.579		0.366		0.339		0.592		0.135		0.931

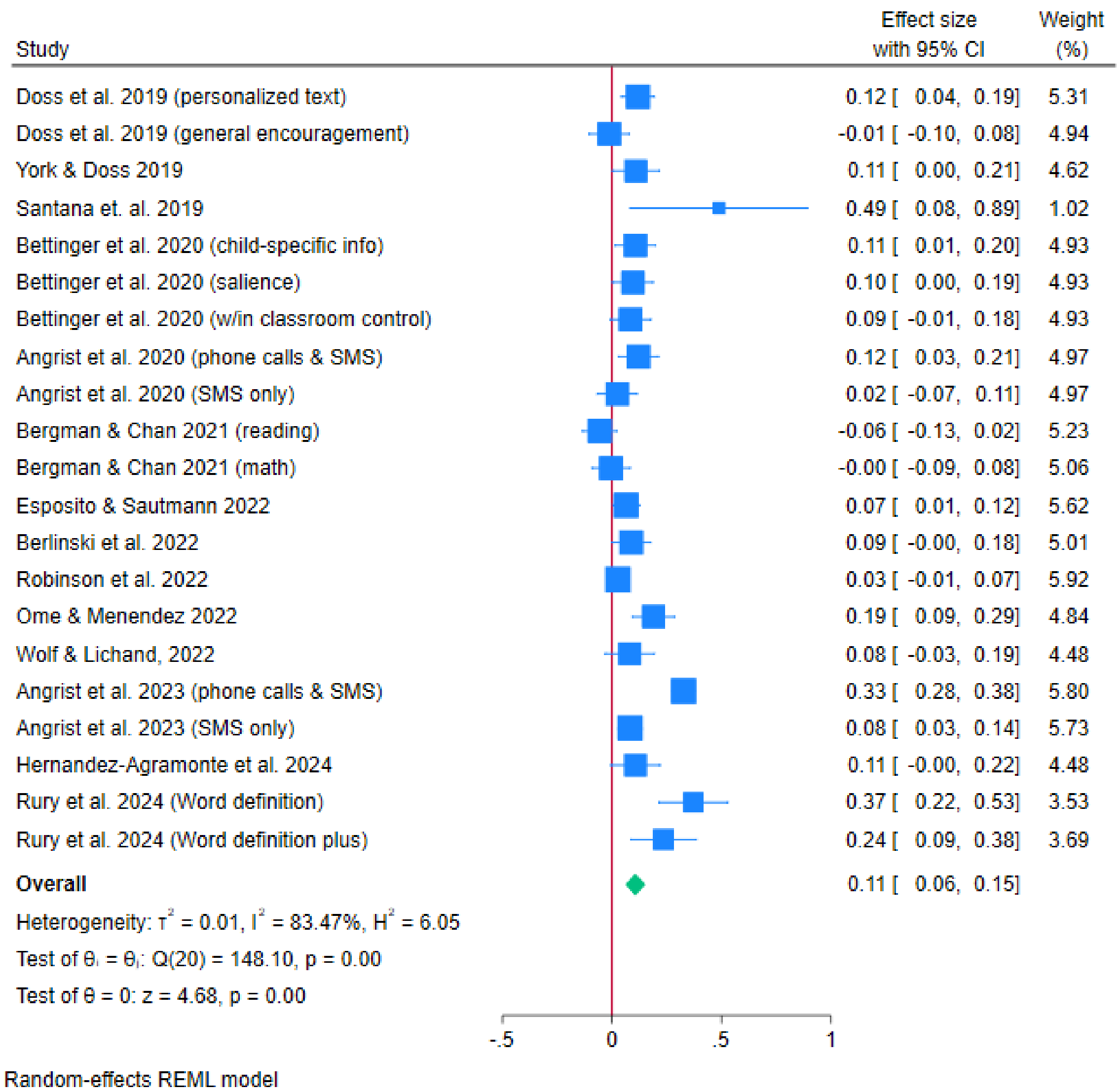
Notes: This table reports the effect of the messaging program on student turnover and test scores in the first term (Term 3, 2018). The dependent variable in Panel A is an indicator for whether the student as any test score in terms 1 and 2 of the 2019 academic year. Other details of the specification are as in Table 2. The dependent variable in Panel B is test scores. Other details of the specification are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 7: Effects of messages on caregiver behavior

	Talks to child about learning daily	Helps with homework weekly	Talks to teacher monthly	Knows child's test scores	Parent reported score	Knows child's class rank	Parent reported rank (percentile)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Effect of outreach							
Outreach classroom	0.083	0.064	-0.055	-0.042	0.009	-0.062	0.024
	(0.084)	(0.081)	(0.083)	(0.079)	(0.026)	(0.086)	(0.061)
Panel B: Effect of outreach by message type							
Growth message	0.160	0.013	-0.049	0.029	0.042	-0.129	0.097
	(0.109)	(0.106)	(0.125)	(0.122)	(0.032)	(0.120)	(0.068)
Personalized message	-0.006	0.136	-0.004	-0.125	0.023	-0.110	0.024
	(0.111)	(0.096)	(0.108)	(0.109)	(0.035)	(0.123)	(0.078)
Personalized & Growth message	0.082	0.104	-0.054	0.120	-0.011	0.044	-0.026
	(0.117)	(0.100)	(0.115)	(0.113)	(0.027)	(0.113)	(0.074)
Within-class control	0.112	-0.034	-0.132	-0.264***	-0.020	-0.071	0.016
	(0.123)	(0.131)	(0.126)	(0.107)	(0.056)	(0.117)	(0.074)
All messages = 0 (p-value)	0.327	0.428	0.509	0.596	0.730	0.475	0.697
Number of observations	158	156	156	156	75	151	68
Mean of dependent variable (pure control)	0.653	0.667	0.662	0.500	0.611	0.608	0.544

Notes: This table reports the effects of messaging interventions on caregiver-reported behaviors. Each column reports the results from a separate specification that pools all test scores observed for each student over the course of the intervention. Standard errors, clustered at the school level, are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Figure 1: Forest plot of experimental interventions using mobile phones to encourage parents



Notes: All studies used experimental designs to estimate the impact of an ICT-enabled parent outreach condition. An attempt was made to include the primary effect, if one was noted in the abstract or introduction of the paper. Separate estimates are included for different treatment arms for studies that do not report a single impact on outreach. Avvisati et al. (2014) and Bergman and Chan (2021) only report separate estimates by subject, and each are included separately. Averaging the estimates or randomly selecting one has a negligible effect on the aggregate estimate. Angrist et al. (2023) finds that phone calls from teachers have an especially large impact, and may not be comparable to the other interventions in the list. Dropping this estimate reduces the aggregate estimate by around 0.01 standard deviations. Esposito Acosta and Sautmann (2022) reports the 95 percent highest posterior density intervals instead of standard errors. We derive the standard error heuristically as the average absolute difference between the top and bottom of the interval and the point estimate divided by 1.96.

Figure 2: Consort diagram

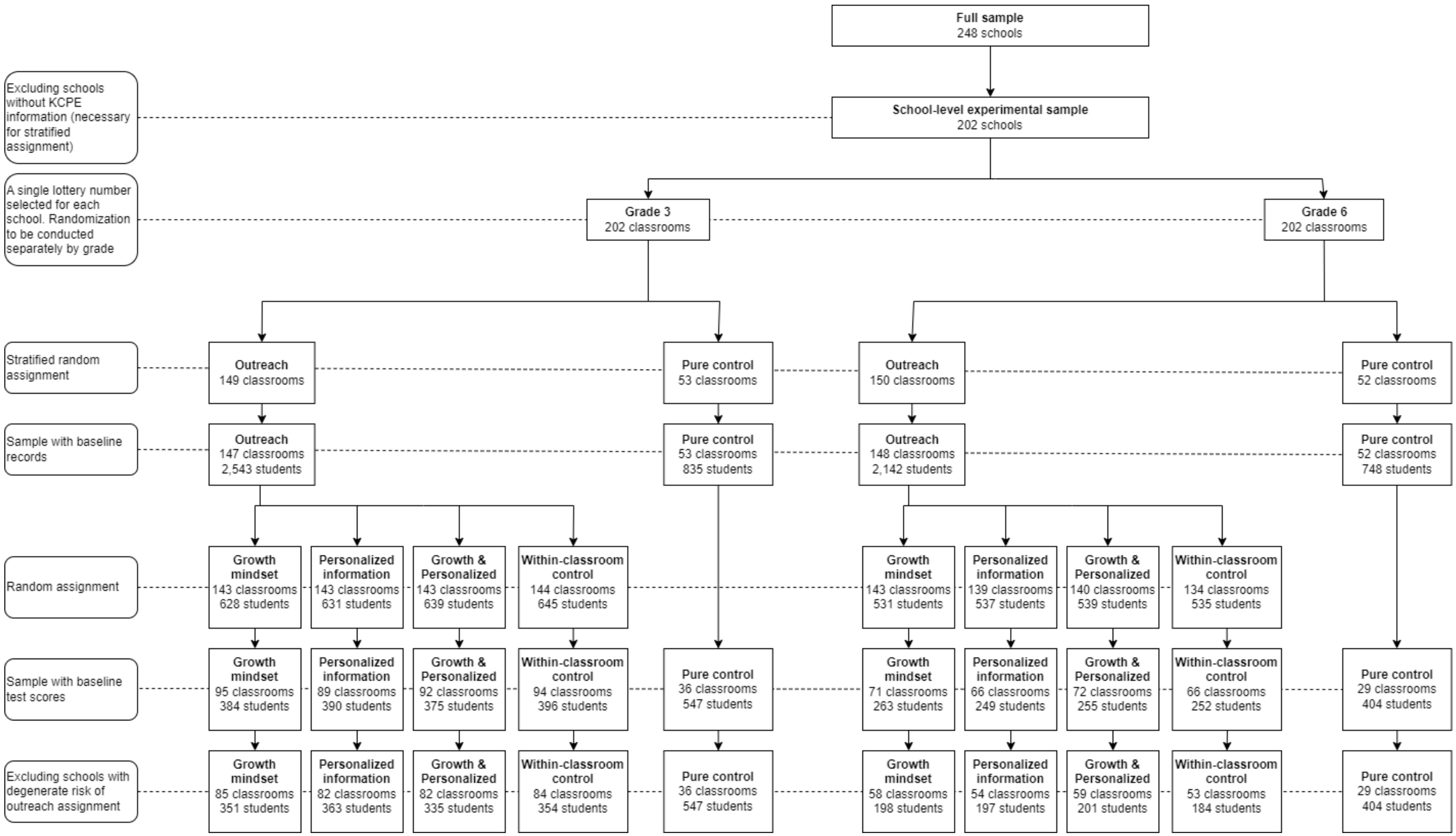
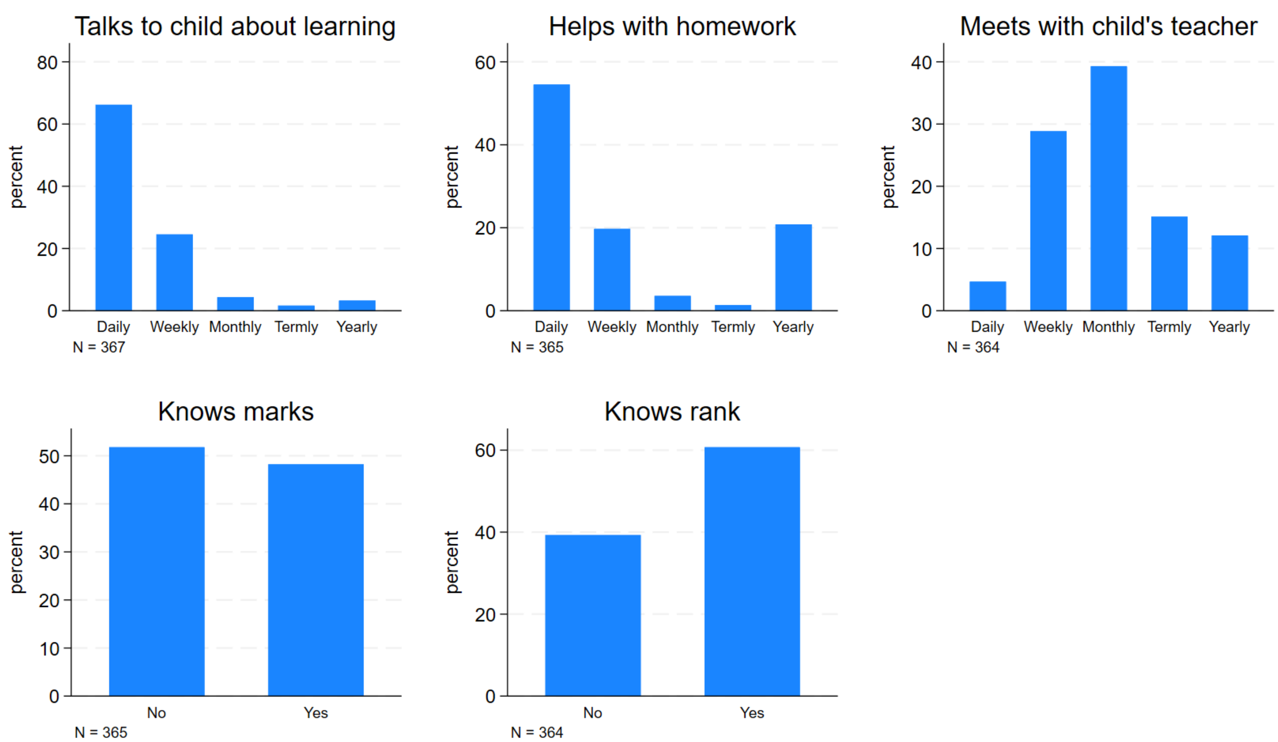
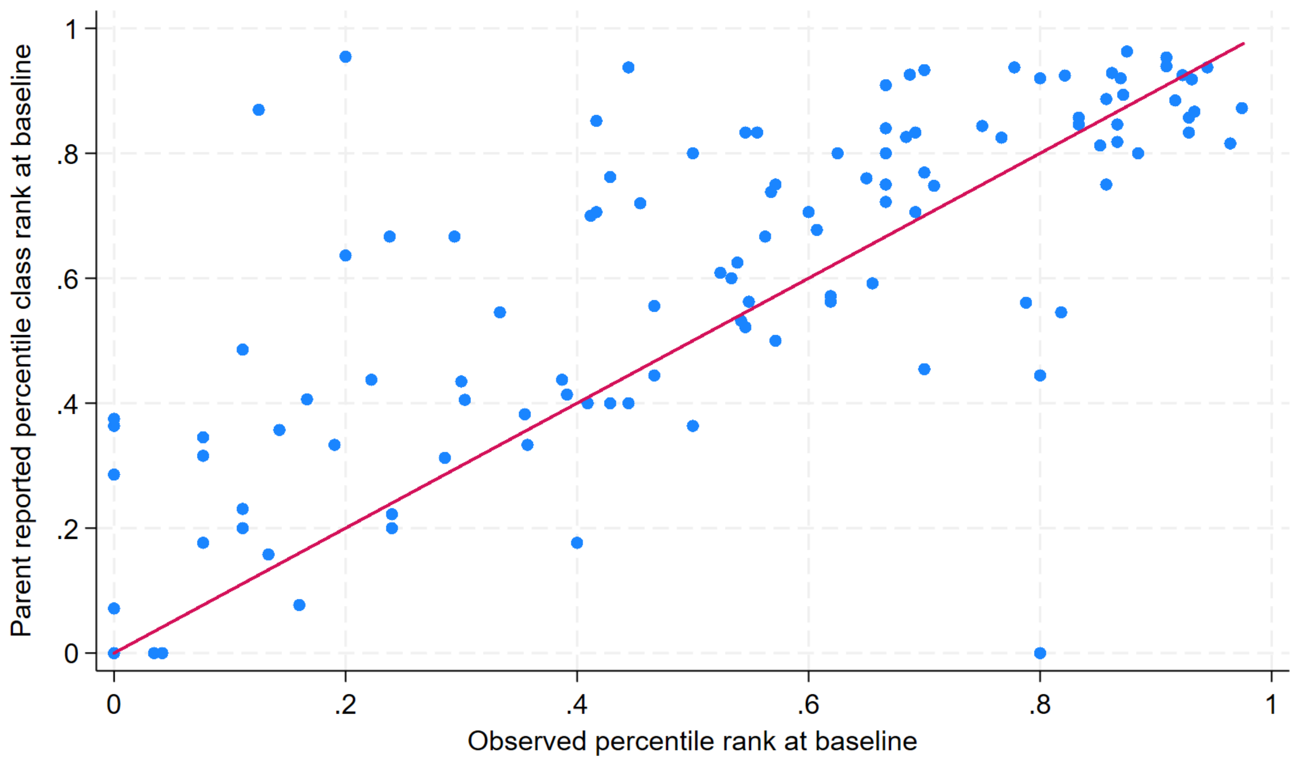


Figure 3: Effects on parent-reported educational engagement



Notes: This figure illustrates parent-reported educational engagement from a baseline survey with a sub-sample of N=367 parents.

Figure 4: Relationship between parent-reported percentile rank and observed percentile rank



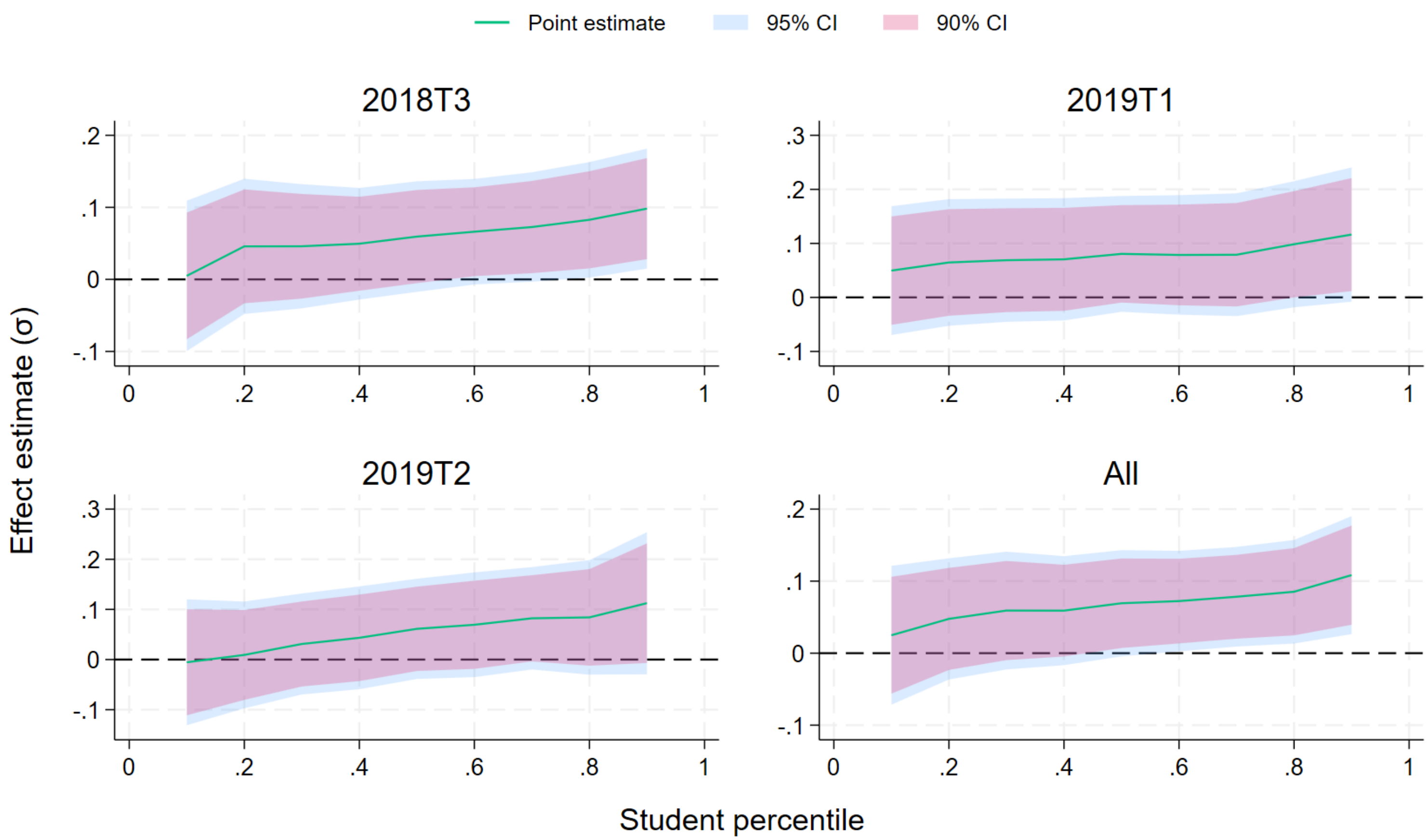
Notes: This figure illustrates the relationship between parental beliefs and actual test scores. The child's actual percentile rank is estimated using the baseline test score index, described in the text. Parent beliefs are their stated belief about the child's rank within the classroom. The line represents the 45-degree line describing the relationship if the baseline index accurately forecasts parental beliefs.

Figure 5

Growth Mindset messages (Class 3 and 6)	**Personalized messages (Class 3)**	**Personalized messages (Class 6)**
Intelligence and achievement can be improved through hard work. Keep encouraging your child to work hard on their studies.	{First name} {Last name}'s score was {% Score} on their Social Studies end term exam. The class average was {Class % Score}. Ask {Pupil name} to tell you where solar power comes from. (answer: the sun)	{First name} {Last name}'s score was {% Score} on their maths end term exam. The class average was {Class % Score}. Ask {Pupil name} to convert 4.5 litres to decilitres. (answer: 45 decilitres)
Homework provides your child with opportunities to practise material. Encourage them to complete homework each night.	{First name} {Last name}'s score was {% Score} on their maths end term exam. The class average was {Class % Score}. Ask {Pupil name} to write the number in words 4546. (answer: four thousand five hundred forty six)	{First name} {Last name}'s score was {% Score} on their social studies end term exam. The class average was {Class % Score}. Ask {Pupil name} to identify the capital of Burundi {answer: Bujumbura}.
Attendance is a major part of school success. You can help your pupil succeed by ensuring that they attend school on time each day.	{First name} {Last name}'s score was {% Score} on their language end term exam. The class average was {Class % Score}. Ask {Pupil name} to fill in the sentence. People should cross the road at the ___. (answer: zebra crossing)	{First name} {Last name}'s score was {% Score} on their social studies midterm exam. The class average was {Class % Score}. Ask {Pupil name} what kind of objects absorb but don't reflect light. (answer: opaque)

Notes: Figure shows examples of messages sent in each message treatment arm.

Figure 6: Quantile effects



Appendices

Appendix A Tables and figures

Table A1: Effects of message content on aggregate classroom enrollment

	(1)	(2)	(3)
Outreach classroom	0.004	-0.504*	-0.524*
	(0.681)	(0.281)	(0.305)
Number classrooms	356	351	353
Number of schools	192	191	192
Term	2018T3	2019T1	2019T2

Notes: This table reports estimates of the effect of outreach on aggregate enrollment in the school, including those students who were not enrolled before the study and therefore do not meet inclusion criteria. Specification includes controls for the baseline enrollment level (measured by the total number of students in the baseline test score file, including those with missing test scores) and a linear control for the probability of assignment to the outreach condition within the classroom's randomization strata. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A2: Effects of message content on student turnover

	Ordinary least squares								Proportional hazard regression			
	Has any term 1 & 2 scores		Has any term 2 scores		Enrolled at start of 2019		Days enrolled		Exit by start of 2019		Exit by end of study	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Growth message	0.027	0.023	0.024	0.033	0.035**	0.025	7.110	2.659	-0.315**	-0.231	-0.121	-0.048
	(0.022)	(0.031)	(0.024)	(0.034)	(0.016)	(0.021)	(4.551)	(6.086)	(0.147)	(0.214)	(0.106)	(0.149)
Personalized message	-0.002	-0.021	0.004	-0.012	-0.005	-0.032	-2.215	-11.208*	0.042	0.277	0.046	0.215
	(0.023)	(0.032)	(0.024)	(0.034)	(0.017)	(0.024)	(4.800)	(6.581)	(0.136)	(0.191)	(0.104)	(0.142)
Personalized & growth message	-0.007	-0.029	-0.000	-0.002	0.010	-0.030	3.180	-8.786	-0.087	0.248	-0.054	0.219
	(0.023)	(0.032)	(0.025)	(0.034)	(0.017)	(0.024)	(4.697)	(6.495)	(0.140)	(0.191)	(0.106)	(0.142)
Growth × above		0.009		-0.020		0.020		9.532		-0.167		-0.156
		(0.045)		(0.048)		(0.033)		(9.131)		(0.295)		(0.212)
Personalized × above		0.041		0.033		0.057*		18.955**		-0.484*		-0.365*
		(0.046)		(0.049)		(0.035)		(9.614)		(0.273)		(0.209)
Personalized & growth × above		0.047		0.003		0.083**		24.917***		-0.720**		-0.611***
		(0.046)		(0.049)		(0.034)		(9.387)		(0.283)		(0.215)
Above median	-0.016	-0.040	-0.020	-0.024	-0.020	-0.060**	-2.597	-15.785**	0.151	0.492**	0.009	0.285*
	(0.020)	(0.034)	(0.022)	(0.037)	(0.015)	(0.026)	(4.145)	(7.212)	(0.129)	(0.208)	(0.095)	(0.156)
Observations	3139	3139	3139	3139	3139	3139	3139	3139	3139	3139	3139	3139
P-values												
Effect on top students = 0												
-Growth		0.316		0.698		0.062		0.073		0.050		0.178
-Personalized		0.540		0.553		0.324		0.269		0.291		0.329
-Growth + Personalized		0.582		0.971		0.030		0.017		0.024		0.015
All main effects = 0	0.421	0.345	0.724	0.575	0.061	0.033	0.192	0.094	0.079	0.046	0.425	0.122
All top student effects = 0		0.421		0.724		0.061		0.192		0.079		0.425
All interactions = 0		0.664		0.757		0.063		0.044		0.054		0.028
Within-class control mean	0.716	0.716	0.617	0.617	0.866	0.866	347.793	347.793	0.136	0.136	0.233	0.233

Notes: This table reports estimates of the effect of varying message content within classrooms receiving outreach relative to the within-classroom control. Standard errors are clustered at the student level. These specifications include students in schools that were deterministically assigned to the outreach condition because they were in small randomization strata. Controls are as in Table 2. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A3: Effects of outreach on test score follow-up

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outreach classroom	0.008	0.026	-0.065**	-0.032	-0.080***	-0.056*	-0.045*	-0.020
	(0.040)	(0.039)	(0.029)	(0.036)	(0.026)	(0.033)	(0.024)	(0.026)
Above median	-0.007	0.020	-0.010	0.039	-0.014	0.020	-0.010	0.026
	(0.020)	(0.023)	(0.023)	(0.035)	(0.024)	(0.042)	(0.017)	(0.027)
Outreach classroom × above		-0.038**		-0.070*		-0.048		-0.051*
		(0.017)		(0.036)		(0.042)		(0.027)
Number tests	50144	50144	44294	44294	50144	50144	144582	144582
Number students	3134	3134	3134	3134	3134	3134	3134	3134
Top-student effects = 0 (p-value)		0.775		0.002		0.003		0.014
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of the messaging program on test score follow-up relative to the pure control group. Specification details are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A4: Effects of outreach on test score follow-up by message type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth message	0.015	0.044	-0.041	-0.001	-0.069**	-0.034	-0.031	0.003
	(0.041)	(0.042)	(0.031)	(0.040)	(0.030)	(0.039)	(0.025)	(0.030)
Personalized message	-0.002	-0.011	-0.081**	-0.065	-0.093***	-0.092**	-0.058**	-0.056*
	(0.041)	(0.042)	(0.036)	(0.043)	(0.031)	(0.041)	(0.027)	(0.032)
Personalized & growth message	0.006	0.022	-0.069*	-0.050	-0.068**	-0.058	-0.043	-0.028
	(0.042)	(0.044)	(0.036)	(0.046)	(0.033)	(0.042)	(0.029)	(0.034)
Within-class control	0.010	0.049	-0.070**	-0.010	-0.087***	-0.042	-0.048*	-0.001
	(0.041)	(0.040)	(0.030)	(0.039)	(0.031)	(0.040)	(0.025)	(0.028)
Above median	-0.007	0.020	-0.010	0.039	-0.014	0.020	-0.010	0.026
	(0.020)	(0.023)	(0.023)	(0.035)	(0.024)	(0.042)	(0.017)	(0.027)
Growth × above		-0.060*		-0.085*		-0.075		-0.073*
		(0.033)		(0.048)		(0.056)		(0.038)
Personalized × above		0.019		-0.032		-0.004		-0.005
		(0.031)		(0.048)		(0.056)		(0.037)
Personalized & growth × above		-0.033		-0.040		-0.021		-0.031
		(0.031)		(0.050)		(0.054)		(0.038)
Within control × above		-0.079***		-0.124**		-0.094*		-0.098***
		(0.030)		(0.049)		(0.052)		(0.035)
Number tests	50144	50144	44294	44294	50144	50144	144582	144582
Number students	3134	3134	3134	3134	3134	3134	3134	3134
P-values								
Effect on top students=0								
-Growth		0.728		0.026		0.012		0.036
-Personalized		0.863		0.029		0.024		0.081
-Growth & personal		0.811		0.036		0.069		0.101
-Within-classroom control		0.513		0.001		0.001		0.003
All main effects=0	0.908	0.145	0.152	0.311	0.030	0.229	0.267	0.187
All top-student effects=0		0.683		0.019		0.022		0.056
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of the message content on test score follow-up, relative to the pure control group. Specification details are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A5: Effects of message content on test score follow-up

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth message	0.006	-0.002	0.029	0.010	0.018	0.008	0.017	0.005
	(0.018)	(0.025)	(0.027)	(0.037)	(0.027)	(0.038)	(0.019)	(0.026)
Personalized message	-0.011	-0.059**	-0.010	-0.055	-0.006	-0.050	-0.009	-0.055**
	(0.019)	(0.027)	(0.027)	(0.038)	(0.027)	(0.038)	(0.019)	(0.027)
Personalized & growth message	-0.003	-0.025	0.000	-0.040	0.019	-0.017	0.005	-0.027
	(0.019)	(0.026)	(0.027)	(0.037)	(0.028)	(0.038)	(0.020)	(0.027)
Above median	-0.016	-0.056**	-0.022	-0.076*	-0.022	-0.067	-0.020	-0.066**
	(0.017)	(0.029)	(0.024)	(0.041)	(0.024)	(0.041)	(0.017)	(0.030)
Growth × above		0.016		0.039		0.020		0.024
		(0.036)		(0.054)		(0.055)		(0.038)
Personalized × above		0.098***		0.092*		0.090*		0.093**
		(0.037)		(0.055)		(0.054)		(0.039)
Personalized & growth × above		0.044		0.083		0.073		0.066*
		(0.038)		(0.055)		(0.055)		(0.040)
Number tests	34928	34928	30719	30719	34928	34928	100575	100575
Number students	2183	2183	2183	2183	2183	2183	2183	2183
P-values								
Effect on top students=0								
-Growth		0.596		0.221		0.486		0.299
-Personalized		0.139		0.356		0.305		0.167
-Growth & personal		0.476		0.275		0.153		0.175
All main effects = 0	0.821	0.099	0.502	0.251	0.731	0.437	0.578	0.110
All top-student effects = 0		0.514		0.611		0.536		0.479
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of message content on test score follow-up relative to the within-classroom control. Standard errors are clustered at the student level. These specifications include students in schools that were deterministically assigned to the outreach condition because they were in small randomization strata. Controls are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A6: Effects on test scores by term

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Growth message	-0.007	-0.035	0.021	0.073	0.013	0.074	0.008	0.030
	(0.028)	(0.046)	(0.037)	(0.054)	(0.039)	(0.056)	(0.026)	(0.038)
Personalized message	-0.032	-0.090*	-0.027	-0.021	-0.019	0.046	-0.026	-0.029
	(0.029)	(0.047)	(0.038)	(0.055)	(0.039)	(0.057)	(0.026)	(0.039)
Personalized & growth message	-0.033	-0.084*	-0.000	0.036	-0.018	0.040	-0.018	-0.011
	(0.028)	(0.046)	(0.037)	(0.054)	(0.038)	(0.055)	(0.025)	(0.037)
Growth × above		0.059		-0.108		-0.127*		-0.046
		(0.056)		(0.074)		(0.077)		(0.051)
Personalized × above		0.118**		-0.016		-0.132*		0.003
		(0.058)		(0.075)		(0.078)		(0.053)
Personalized & growth × above		0.105*		-0.075		-0.119		-0.015
		(0.055)		(0.074)		(0.076)		(0.050)
Above median	0.118***	0.048	0.159***	0.209***	0.245***	0.338***	0.170***	0.184***
	(0.026)	(0.044)	(0.036)	(0.058)	(0.035)	(0.058)	(0.025)	(0.040)
Number tests	27254	27254	19433	19433	20277	20277	66964	66964
Number students	2017	2017	1553	1553	1450	1450	2070	2070
-Growth		0.462		0.480		0.317		0.624
-Personalized		0.421		0.475		0.104		0.473
-Growth & personal		0.488		0.442		0.132		0.444
All main effects = 0	0.534	0.164	0.628	0.351	0.817	0.613	0.527	0.525
All top-student effects = 0		0.840		0.861		0.367		0.869
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of message content on student test scores relative to the within-classroom control group. Standard errors are clustered at the student level. These specifications include students in schools that were deterministically assigned to the outreach condition because they were in small randomization strata. Controls are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A7: Effects on test scores by term using IPW correction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outreach classroom	0.074*	0.106**	0.077	0.102	0.046	0.068	0.065*	0.092**
	(0.042)	(0.051)	(0.054)	(0.063)	(0.052)	(0.062)	(0.037)	(0.045)
Outreach classroom × above		-0.068*		-0.052		-0.046		-0.055*
		(0.035)		(0.040)		(0.044)		(0.033)
Above median	0.094***	0.142***	0.168***	0.204***	0.247***	0.279***	0.170***	0.208***
	(0.034)	(0.040)	(0.040)	(0.044)	(0.040)	(0.045)	(0.030)	(0.035)
Number tests	38930	38930	28825	28825	30246	30246	98001	98001
Number students	2870	2870	2270	2270	2119	2119	2959	2959
P-values								
Main effects = 0	0.082	0.040	0.160	0.107	0.377	0.272	0.081	0.042
Top-student effects = 0		0.319		0.340		0.655		0.302
Term	2018T3	2018T3	2019T1	2019T1	2019T2	2019T2	All	All

Notes: This table reports the effect of the messaging program on student test scores using inverse probability weights (IPW) to correct for potentially selective attrition. Weights are calculated separately for each assessment by estimating a logit model analogous to the specification used to estimate the test score impacts. All other specification details are as in Table 4. ***, **, and * indicate significance at the 1%, 5%, and 10% levels.