# Lowering Barriers to Remote Education: Experimental Impacts on Parental Responses and Learning*

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**Abstract**

We conduct a randomized controlled trial with households of secondary school students in Bangladesh to investigate how parents adjust their educational investments in response to three interventions: an informational campaign about an educational phone application, an internet data subsidy, and one-on-one phone learning support. We find that these light-touch interventions can trigger changes in parental educational investments. These behavioral responses, in turn, can increase learning. Only when the information is provided alongside the subsidy is there a marginal increase in the phone app's use, all concentrated among richer households. This highlights the existence of multiple barriers to adoption. Regular information does lead parents to increase private tutoring investment on the intensive and extensive margin, especially among richer households. When parental economic investment in education increases, parental time investment tends to decrease. We find suggestive evidence that the information intervention—mediated through increases in tutoring—leads to an increase in math achievement, concentrated among wealthier households.

**JEL Classification:** C93, I21, I24, J13, O15
**Keywords**: Human capital, parental investments, educational technology, educational inequality

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# 1 Introduction

Parental investments are an important determinant of children's skills and human capital (Becker and Tomes, 1976; Cunha et al., 2006; Todd and Wolpin, 2007; Francesconi and Heckman, 2016). In addition to selecting schools and children's formal learning environments, parents provide important supplemental educational inputs that complement children's formal schooling. These fall into two broad categories: time investments—dedicating time helping their children with homework or engaging with them in educational activities—and economic investments—paying for tutoring or other after-school activities (Bray, 1999). Parents face barriers preventing them from optimizing along these two investment channels. These barriers may include limited knowledge of educational investment options, low perceived returns to these investments (Attanasio et al., 2020), limited availability of different resources and their costs, and parents' own resource constraints (Dahl and Lochner, 2012). Educational policies attempt to relieve these constraints with interventions such as information provision and nudges, conditional cash transfer (CCT) programs, or personalized tutoring and teaching at the right level (TaRL) programs.[1]

Understanding parental behavioral responses to education policies is critical to accurately assess their net impact on children's human capital development (Das et al., 2013) and understanding potential distributional impacts. Because households are often the intermediaries between education policies and children's learning, failing to account for these behavioral responses makes it impossible to determine, for example, whether a particular intervention was ineffective or whether parents re-optimized in response to the intervention, changing household inputs and undoing the intended effect. Additionally, inequality in parental time, skills, and money can generate disparities in parents' educational investments in their children, in turn exacerbating educational inequality (Blanden et al., 2022). Hence, understanding how parental responses vary across households with different socioeconomic backgrounds also provides insight into the drivers of the distributional impacts of reducing barriers to education.

We conducted a randomized controlled trial with 7,313 households with secondary school students in Bangladesh to investigate how parents adjust their investments in response to three interventions that relieve different barriers to education: one-on-one phone learning support, informational phone messages about a novel educational phone app, and an internet data subsidy alongside the phone messages. We evaluate their impact on student achievement and examine the extent to which they are inequality-enhancing or reducing. We collect detailed intermediate investment outcomes to look into the black box of educational investment responses.

This study took place during the 2020–2021 COVID-19 school closures, and this context makes it particularly suitable for investigating the impact of these three educational policies on parental responses and educational inequality. Given that parental investment decisions are especially relevant in settings where access to quality schooling—or any schooling at all—is limited or disrupted, parents become the primary decision-makers regarding their children's educational inputs in our setting. This allows us to more easily detect the impact of their behavioral responses and isolate their contribution to human capital development from other inputs such as formal schooling.

Stark differences in access to educational resources suggest that parents' responses will depend on their socioeconomic status. Our sample is not nationally representative of families with secondary school children given that the nature of the interventions required that we include only households with access to a smartphone. We address this restriction by building our sample from three different sources, including a

[1] See Ganimian and Murnane (2016) and Glewwe and Muralidharan (2016) for two recent reviews on interventions in developing countries aimed at reducing barriers to education and improving educational outcomes.

random-digit-dialing sample (RDD) and a database of low-income recipients of a government school stipend program. In this setting of virtually no access to formal schooling, potential differences in parents' ability and resources to support remote learning and compensate for the lost school-based inputs may deepen educational inequality (Fredriksson et al., 2016; Blanden et al., 2022; Agostinelli et al., 2022), which could have long-run implications (Fuchs-Schündeln et al., 2022).

By including households facing a broad range of constraints, we can investigate how socioeconomic status is associated with parents' investments and responses to reduced barriers to remote education. Limited resources and additional barriers among poorer households may shape parents' initial investments, and restrict their ability to respond to and benefit from reduced barriers to education. Descriptive insights show that indeed, higher socioeconomic households invested more time and money in their children's education one year after the school closures. Providing information about learning resources, decreasing the economic costs of accessing them, and offering one-to-one support could yield larger impacts on low-SES students if the associated constraints are more binding for them. Conversely, children from poorer households could see smaller impacts if they or their parents lack the resources or bandwidth to benefit from these interventions or if additional constraints are present.[2]

We delivered interventions for 4–8 weeks from February to April 2021 to the phone number we reached during the baseline survey, nearly always that of the child's parent. We conducted two rounds of follow-up surveys by phone to measure the impact of these interventions on parent and student educational investment, while the interventions were ongoing (March 2021) and approximately one month after they concluded (June 2021).[3] In the first part of the paper, we investigate the impacts of the interventions on parental responses while the interventions were ongoing. In the second part of the paper, we examine the impacts on student math achievement one to two months after the interventions ended.

Our first set of findings indicates that the interventions caused changes in the use of specific learning resources. Only when the app information is provided alongside the subsidy we observe a marginal increase in the app's use. This increase is concentrated among richer households. Providing the app information alone does not seem sufficient to change its use. These two findings suggest that others barriers may be present. The app information reduces the use of tech-dependent learning resources, whereas teacher support decreases the use of non-tech-dependent learning resources. The subsidy alongside the information doesn't change the extensive- or intensive-margin use of learning resources.

Our second set of findings shows that the interventions significantly affect parental educational investments. The app information—alone, and accompanied by the subsidy—increases the likelihood of using a private tutor, whereas the teacher support decreases this likelihood. The data subsidy combined with the app information intervention attenuates the parental responses to the app information alone. We also observe that, when parental economic investment in tutoring increases, parental time investment tends to decrease.

When looking at student match achievement, we observe suggestive impacts of interventions on student math knowledge. The app information increases student math achievement by 0.11 SD. This increase is concentrated among richer households (with 0.205 vs. 0.001 SD effects for above-median income compared to below median income households). In contrast, we find that the app information alongside the subsidy,

[2]In a similar vein, List et al. (2021) show that simple informational policies are not enough to change parental beliefs about the effectiveness of parental investments and that more intensive programs combining information and home visits and feedback are needed to increase parental investments and reduce socioeconomic gaps in children's achievement.

[3]We pre-registered our primary empirical specification and key outcomes at https://www.socialscienceregistry.org/trials/6191.

and the teacher support have no effect on student achievement. By contrasting the impacts on parental responses and on student learning, we conclude that tutoring increases seem to be the cause of the student achievement improvements, not the app's use.

We contribute to three strands of literature. First, our results speak to the literature on parental investments and involvement in their children's education. Research on parental effort and time investment exploits exogenous sources of variation in schooling inputs to assess parenting behavioral responses in terms of time investment at home, finding that it is a substitute for school resources in India and Zambia (Das et al., 2013) and Romania (Pop-Eleches and Urquiola, 2013), whereas it has been found to be a complement (Gelber and Isen, 2013) or substitute (Houtenville and Conway, 2008) in different contexts in the United States. Our paper experimentally examines parental educational investment responses to three prevalent remote educational interventions—an informational campaign, a reduction in the economic costs of access, and one-on-one teacher support—in a setting where school inputs are minimally influencing students learning. By using a unified framework that clarifies hypotheses and key channels, as well as by collecting detailed data on parental time and economic investments and choices of learning resources, we shed light on parental behavioral responses to relieving different barriers to education in a systematic way.

Second, we contribute to the literature investigating the effectiveness of interventions aimed at improving educational outcomes during school disruptions caused by natural disasters and emergencies (Andrabi et al., 2021; Bandiera et al., 2020), including the COVID-19 pandemic. Some of this work has explored the channels through which school closures affect learning (Agostinelli et al., 2022) and how closures may create inequalities (Bacher-Hicks et al., 2021; Singh et al., 2022), while others focus on investigating the experimental impacts of interventions designed to promote student engagement and learning during school closures (Angrist et al., 2022; Carlana and La Ferrara, 2021; Lichand et al., 2022; Hassan et al., 2021; Schueler and Rodriguez-Segura, 2021). More broadly, we contribute to the literature on educational technology. Relatively low-tech solutions such as SMS and phone calls (Angrist et al., 2022) and in-school TV-based lessons (Navarro-Sola, 2021; Johnston and Ksoll, 2017; Beg et al., 2019) have shown promise, as well as personalized adaptive computer-assisted learning (CAL) programs (Muralidharan et al., 2019).[4] Given that parents are a crucial intermediary between interventions and students, we contribute to the literature by focusing on understanding parental decisions and responses to several interventions aimed at relieving different constraints to accessing remote education.

Third, our paper provides insights into the literature linking the role of parental investments and constraints to achievement gaps and educational inequality, extensively reviewed in Blanden et al. (2022). Households from lower socioeconomic backgrounds face greater time and monetary constraints. Information frictions, which are greater for poorer families (Dizon-Ross, 2019), can widen differences in parental investments in children's human capital (Caucutt et al., 2017). Schooling disruptions affect inequality by increasing the relative importance of parental investments in their children's education, and higher-SES parents may be better able to adjust their investments to ameliorate the impact of such shocks (Blanden et al., 2022), although most evidence to date is from wealthier contexts (Andrew et al., 2020; Del Bono et al., 2021; Bansak and Starr, 2021; Bacher-Hicks et al., 2021). Our study suggests that heterogeneous constraints among parents from different socioeconomic backgrounds mean that some policies aiming to reduce educational barriers can, in fact, worsen educational inequality.

The rest of the paper proceeds as follows. Section B presents the stylized framework outlining the impact of educational policies when taking household educational investment decisions into account. Section

[4]See Caballero Montoya et al. (2021) for a thorough review of the literature on distance education.

3 presents descriptive insights about parental investments in children's education. Section 2 describes the experimental design, including the context of education in Bangladesh during the COVID-19 school closures, the sample selection and study timeline, and the interventions randomization and attrition. It also presents descriptive statistics, balance tests, and the empirical specification. Section 4 describes the impacts of the interventions on parental re-optimization responses with respect to economic and time educational investment and learning resource usage overall and heterogeneously by socioeconomic status. Section 5 explores the persistent effects on student learning. Section 6 discusses and interprets the results, and Section 7 concludes.

# 2 Experimental Design

## 2.1 Context: Education in Bangladesh during COVID-19

The first known cases of COVID-19 were reported in Bangladesh on March 7, 2020. Bangladesh initiated a general holiday on March 18, 2020, closing schools and all non-essential businesses and closing most public transport. The government canceled the national grade 5 and grade 8 exams in late August. In October, the government issued assignments and evaluation guidelines for secondary-level students and announced that students would be automatically promoted to the next grade based on these evaluated assignments (Alamgir, 2020). In January 2021, the government announced plans to reopen schools in February, and it issued and distributed new books to students for the 2021 academic year. The government withdrew this decision as COVID-19 cases rose, and it did not re-open schools until September 2021. Appendix Figure A.1 outlines key events in Bangladesh affecting children's education alongside the study timeline.

During the school closures, the government's main priority was to minimize the disruption of learning as much as possible. The Ministry of Education and Aspire to Innovate (a2i) collaborated to use a combination of mass media broadcasting and an online platform to remotely deliver educational content from the school curriculum. The government began broadcasting daily television lessons for secondary-level students on March 29, which was later expanded to all levels. The secondary broadcasts consisted of 10 videos daily—two grade-specific 20-minute daily lessons for students in grades 6 through 10—and these lessons were also posted on a YouTube channel. Weekly broadcast schedules were disseminated widely: schools asked teachers to share schedules with households and encouraged them to watch, and schedules were also posted online and broadcast over radio. However, Sangsad TV was broadcast via satellite, so non-subscribing households, as well as those without televisions, were not able to access materials. Additionally, the Sangsad TV channel stopped broadcasting secondary lessons in anticipation of an early 2021 school reopening, and so it only telecast lessons for grades 1 through 5 during the intervention period. The pool of videos posted on YouTube, however, remained available.

Non-governmental organizations also offered educational resources and initiatives to aid remote learning during school closures. One such resource was Robi 10-Minute School, a free website platform with an accompanying mobile application that provided free videos and adaptive quizzes aligned with national curriculum standards. More than 1.5 million students accessed its materials daily in 2020 (Axiata Group Berhad, 2020).

## 2.2 Sample Selection

Because the interventions are useful only to those who have access to the requisite technology, our baseline phone sample consists of 7,576 respondents that have (a) at least one child in grades 6–10 (grades 7–11 in

January 2021) and (b) have at least one smartphone in the house. While mobile phone penetration in Bangladesh is fairly high, smartphone ownership is substantially lower, meaning that our study sample is not nationally representative. Estimates in 2022 put individual-level smartphone ownership at 41% (Okeleke, 2021), although rates of access are likely higher given that device sharing is common in Bangladesh (Ahmed et al., 2017).

To maximize our ability to reach families from varying socioeconomic backgrounds, our sampling frame drew from three sources: (1) a random-digit-dialing (RDD) sample of 30,000 numbers from the most popular telecommunications company in Bangladesh; (2) the database of recipients of the Secondary School Stipend (SSS) Programme, who tend to be from lower-income households; and (3) a database of users registered on a government-created online learning platform that preceded the COVID-19 pandemic. While the RDD sample aims, by design, to be nationally representative of the smartphone ownership population, the secondary school stipend sample includes a higher share of lower-income households, and the last sample includes households potentially more inclined to use educational technologies during school closures. We first screened numbers by sending a test SMS message and removing any numbers for which the message was not delivered. Overall, 7,576 respondents completed a baseline survey, about 19% of numbers attempted, or 29% of those who answered the phone (see Appendix Table A.1 for more detail). Respondents are distributed broadly across the country (see Appendix Figure A.2). We randomized all baseline respondents into treatment, but we further restrict our sample to the 97% (7,313 households) who agreed to be recontacted for follow-up surveys.

## 2.3 Study timeline and data collection

We recruited and conducted a baseline survey with households by phone in September–October 2020. We targeted the caregivers of children in grades 6–10 in the household, with a nearly even split between female and male caregivers at 49% and 50%, respectively.[5] For those who met the screening criteria, we asked about demographics, family socioeconomic status, current student educational activity, parent expectations, and aspirations for their children's schooling.

We launched the three sets of interventions shortly after completing the baseline survey. We delivered informational interventions weekly for eight weeks, beginning February 24. On March 1, we distributed the initial invitation for the data subsidy, which would last for one month. We launched the teacher intervention simultaneously with the informational interventions, which lasted four weeks for each student.

The sampling frame for our first follow-up survey (Round 1), conducted while the interventions were ongoing, comprised all 7,313 baseline households that agreed to be recontacted.[6] We again targeted parents, conducting 43% of surveys with mothers, 39% with fathers, and 17% with another family member, usually a child's older sibling.[7] We surveyed 3,775 households, 55.8% of those contacted, representing 3,881 children.

Our second follow-up survey (Round 2) took place approximately 4 to 8 weeks after the interventions concluded to measure the persistence of treatment effects. The potential sampling frame again included the 7,313 baseline households that agreed to be recontacted, from which we conducted a random subsample due to budget constraints.[8] We also randomized the order in which we contacted households.

[5] The remaining 65 respondents were generally the student or another family member.

[6] Because of timing and budget constraints, we randomized the order in which we contacted respondents and ultimately attempted 95% of the sample.

[7] Respondent relationship to the student was missing for 1.5% of the sample.

[8] We included all households that were in any of the following categories (1) were not in Round 1 sampling frame; (2) assigned to data subsidy; (3) assigned to teacher subsidy. We randomly selected a subset of respondents in other treatment

During this wave, we also separately interviewed children to measure their engagement and aspirations and also to assess their learning.[9] Secondary school teachers created a bank of mathematics test questions aligned with the grade-specific national curriculum, since mathematics is included in the high-stakes SSC exams and is taught in all secondary grade levels and curriculum tracks.[10] The questions were designed to be asked orally and answered via multiple choice, and we piloted and revised them prior to implementation. Each student answered eight questions: a grade-specific set of four math questions at their 2020 grade level or lower, and then four additional questions at slightly lower or slightly higher grade levels, based on their performance on the initial four questions. We repeated questions across questionnaires when possible, generating a bank of 19 questions.[11] We completed child interviews in 86.9% of households who completed the endline survey.

## 2.4 Interventions

We test the impact of three interventions designed to reduce different constraints to parental educational investment:

**Treatment 1: Information about an internet learning platform.** Households received twice-weekly reminders about a free internet-based learning platform, Robi 10-Minute School for eight weeks.[12] This resource had a webpage containing videos and adaptive quizzing aligned with the national curriculum, as well as a companion smartphone app.

**Treatment 2: Internet data subsidy.** Households received an SMS message informing them that they would receive a 10GB data package with 30-day validity, allowing them to opt-out if they did not wish to participate. We coordinated with a large mobile provider to activate the package. This data could be used however the recipient wished. The value of this subsidy averaged 366 taka ($4.40 USD), which roughly equals the average per-student weekly expenditure on private tutoring (conditional on receipt) of 386 taka ($4.63 USD). We roughly estimate that the package would be sufficient for 15–20 hours of video per month.[13]

**Treatment 3: Teacher support.** Treated students were matched with a partner teacher from a pool of 71 teachers recruited for the study. Each recruited teacher provided a weekly, 30-minute individual phone check-in with seven assigned students for four weeks. During these meetings, teachers typically discussed students' current learning activities and plans for the week, reviewed completed work and answered student questions, and provided reviews or delivered lessons on specific topics. Teachers received a modest honorarium to cover their time and associated phone charges.

Considering that the teacher support intervention is conducted entirely remotely and provided by teachers previously unknown to students and their families, take up of this treatment is relatively high. Slightly more than half of all invited households (54%) have a child that participates in the teacher meetings. Conditional on enrolling, students attended an average of 3.1 out of 4 meetings, with 61% of enrolled students joining all four teacher sessions.

arms for this wave, specifically a 25% sample of those assigned to receive "general information" only, and a 65–75% sample of those assigned to other information treatments that were not cross-randomized with the data or teacher interventions.

[9] In households with multiple children, we randomly selected one child to complete the assessment.

[10] We designed and implemented a similar instrument in the Bangla subject, but because the content is not necessarily cumulative, it is difficult to differentiate student abilities across a range of grade-specific questions. Appendix C describes these challenges in more detail.

[11] A twentieth question was excluded because of an error in its wording.

[12] Sample message: "Hello! Robi 10-minute school has free video lessons and quizzes to help your student keep learning! (shortened link). Text 1 if you will help your child visit the site!" Messages were delivered by SMS or voice recording (IVR).

[13] Calculation based on a "standard" resolution video (480p) using 480–660MB/hour (Hindy, 2022).

Each treatment allows us to empirically test the total effect of reducing a different barrier to accessing education as presented in the stylized framework in section B, as well as investigating the potential disaggregated effects and behavioral responses shown in the associated comparative statics. Treatment 1 increases the salience of perceived returns to a tech-based learning resource available to households, $\partial\tilde{g}/\partial E$ toward the true returns, $\partial g/\partial E$. Treatment 2 reduces the economic cost of accessing internet learning activities by providing discounted data packages, effectively reducing $dH/dc^e$ as in equation 5. Treatment 3 provides personalized teacher support to students, effectively reducing the cost of external teaching and generating changes similar to the ones caused by $dH/dc^p$ in equation 2.

In addition to these three treatments, we also test the impact of "general information": we delivered information and reminders about daily TV lessons broadcast on the government satellite channel, Sangsad TV, in a similar format and frequency as the reminders about the learning app. Because the government ceased broadcasts of regular lessons during the study period, we exclude this intervention from the main discussion.

We estimate the cost per participant of the information-only treatments (Treatment 1) as \$2.77 USD per household, which is driven mainly by fixed costs to set up the initial interventions. The total cost per participant of the messages themselves was approximately \$0.79 USD over the two months. The costs of the data package and teacher support were roughly equivalent, at \$4.40 USD and \$4.48 USD, respectively, on top of the information costs.

## 2.5 Randomization

We randomized at the household (individual-phone) level among the set of 7,576 baseline respondents. Table 1 illustrates the distribution of treatment assignments. We randomly selected half of the sample to receive Treatment 1 (information and reminders about adaptive learning), which we cross-randomized with the general information treatment. We further cross-randomized Treatment 2 (data subsidy) only among those who already received some information treatment, leaving 25% of the sample to form the pure control group. Treatment 3 (teacher support) was randomized among those who received the general information treatment only.[14] The extended list of all treatment combinations is in Appendix C.3.

During randomization, we stratified along four baseline dimensions: household income (five categories), sample source, child gender (whether households had male only, female only, or both male and female children in grades 6–10), and whether the household had access to at least one smartphone with an *active* internet connection.

## 2.6 Descriptive statistics and balance tests

Column 1 of Table 2 shows the distribution of household characteristics, reported at the child level, for the entire baseline sample. Among our sample, households average 1.9 children, or 1.3 who were in grades 6–10 during the 2020 academic year. Roughly two-thirds have access to satellite or cable television, meaning that they would have the technology necessary to access lessons on the government-run television channel. Nearly all respondents were parents, with the distribution between mothers and fathers nearly exactly equal.

Parental education levels vary substantially, and mothers have less education on average than fathers. Specifically, 35% of mothers and 26% of fathers have completed only primary school, 18% of mothers and

[14] We initially planned to assign 25% to the teacher information treatment, but due to incomplete take-up, we expanded the share to 44%.

Table 1: Distribution of treatment arms

| | Information | | | |
|---|---|---|---|---|
| **N=7,576** | None | General Info | Learning App Info | General Info + App Info |
| **No Free Data** | **25%** 1,894 | **18.75%** 1,423 | **12.5%** 947 | **12.5%** 947 |
| **Free Data** | | **6.25%** 471 | **12.5%** 947 | **12.5%** 947 |
| **Teacher support** | ~44% within cells | | | |

**Notes:** This table shows the complete distribution of the treatment arms and the cross-randomizations, with the share of the total and the number of participants receiving each treatment combination in each cell.

17% of fathers have completed secondary school, and 18% of mothers and 25% of fathers have completed some post-secondary education.

Reflecting far lower rates of labor force participation among mothers, average mothers' income in the past 30 days is 4,864 taka ($58 USD). Income among fathers averages 51,555 taka ($619 USD), which is highly skewed relative to the median of 8,000 taka ($96 USD) per month.[15]

Parents report that their secondary school children completed school activities an average of 5.4 days per week in the month after the school closures began, which remains the same on average at the end of 2020, at 5.7 days per week.

More than half of students (59%) received private tutoring during the closures. While common globally, private supplemental tutoring is especially common in both South and East Asia (Bray, 1999; Bray and Lykins, 2012). In Bangladesh, an estimated 68% of secondary school students receive tutoring (Nath, 2011), which is higher than the baseline rate but comparable to the 64% of students in our sample receiving tutoring as of March 2021. Despite concerns about the economic hardship imposed by COVID-19 pushing youth into the workforce, just 3% of youth in grades 6–10 worked for pay in the past 30 days at baseline. These patterns of high rates of educational engagement despite the ongoing school closures are consistent with studies that focus on less advantaged populations (Beam and Mukherjee, 2021).

Our sample is generally well-balanced along these pre-specified baseline covariates. Among the set of tested covariates, we only reject the null hypothesis of equal means across treatment arms in the case of mother's income. When testing whether these covariates jointly predict treatment assignment relative to the control group using seemingly unrelated regressions, however, we do reject equal covariate means between the application information arm and the control group at the 10% level.

## 2.7 Attrition

We reach 69% of households that we attempted to contact in the Round 1 survey, and treatment assignment does not predict the likelihood of recontact (Appendix Table A.2). Additionally, baseline characteristics among those who received the phone app information, internet data and phone app information, or teacher support are indistinguishable from the control group (Appendix Table A.3).

[15] Income is winsorized at the 99th percentile.

Table 2: Balance tests by pooled treatment assignment

| | (1) All | (2) Control | (3) App info | (4) Data + App info. | (5) Teacher | (6) Joint tests, all, p-val |
|---|---|---|---|---|---|---|
| HH size | 1.92 | 1.91 | 1.96 | 1.90 | 1.92 | 0.845 |
| | (0.99) | (0.99) | (1.00) | (1.00) | (1.02) | |
| Num. secondary children | 1.30 | 1.27 | 1.32** | 1.29 | 1.30 | 0.469 |
| | (0.53) | (0.50) | (0.55) | (0.53) | (0.59) | |
| Has cable/satellite TV | 0.65 | 0.65 | 0.63 | 0.65 | 0.66 | 0.260 |
| | (0.48) | (0.48) | (0.48) | (0.48) | (0.47) | |
| Mother present | 0.49 | 0.50 | 0.48 | 0.51 | 0.49 | 0.790 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Father present | 0.50 | 0.49 | 0.51 | 0.48 | 0.51 | 0.740 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Mother primary | 0.35 | 0.36 | 0.33* | 0.35 | 0.35 | 0.434 |
| | (0.48) | (0.48) | (0.47) | (0.48) | (0.48) | |
| Mother secondary | 0.18 | 0.17 | 0.18 | 0.19* | 0.18 | 0.395 |
| | (0.39) | (0.38) | (0.39) | (0.39) | (0.39) | |
| Mother post-secondary | 0.18 | 0.18 | 0.16 | 0.18 | 0.17 | 0.516 |
| | (0.38) | (0.39) | (0.37) | (0.38) | (0.38) | |
| Father primary | 0.26 | 0.25 | 0.26 | 0.27 | 0.25 | 0.768 |
| | (0.44) | (0.43) | (0.44) | (0.44) | (0.43) | |
| Father secondary | 0.17 | 0.16 | 0.16 | 0.17 | 0.19 | 0.359 |
| | (0.37) | (0.37) | (0.37) | (0.37) | (0.39) | |
| Father post-secondary | 0.25 | 0.25 | 0.26 | 0.25 | 0.24 | 0.726 |
| | (0.44) | (0.44) | (0.44) | (0.43) | (0.43) | |
| Mother income | 4864 | 4550 | 4492 | 5921* | 3394 | 0.000*** |
| | (25390) | (24830) | (23506) | (28666) | (21705) | |
| Father income | 51555 | 51415 | 52910 | 51328 | 50834 | 0.726 |
| | (134271) | (134679) | (138072) | (132713) | (130614) | |
| School days/week, curr. | 5.70 | 5.76 | 5.67 | 5.71 | 5.64 | 0.917 |
| | (2.23) | (2.17) | (2.26) | (2.21) | (2.29) | |
| School days/week, Apr. 20 | 5.37 | 5.38 | 5.37 | 5.37 | 5.43 | 0.923 |
| | (2.16) | (2.18) | (2.14) | (2.16) | (2.12) | |
| Has private tutor | 0.59 | 0.58 | 0.60 | 0.59 | 0.60 | 0.818 |
| | (0.49) | (0.49) | (0.49) | (0.49) | (0.49) | |
| Working for pay | 0.03 | 0.03 | 0.03 | 0.03 | 0.02 | 0.622 |
| | (0.17) | (0.18) | (0.17) | (0.16) | (0.15) | |
| Number of students | 8771 | 2175 | 2219 | 2189 | 954 | |
| Number of households | 7576 | 1894 | 1891 | 1897 | 828 | |
| Joint test, p-val | | | 0.079* | 0.612 | 0.465 | |

**Notes**: Sample includes all randomized baseline respondents at the child level. Stars in columns 3–5 indicate statistically significant differences relative to the control group (column 2). Column 6 reports p-values based on F-tests of the joint significance of all eight treatment indicators, excluding respondents with missing values. P-values in the bottom row are from seemingly unrelated regressions that predict treatment assignment relative to the control group, with missing variable flags included. Standard errors are clustered at the household level. Stratification-cell fixed effects are included in all regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent significance levels.

In Round 2, we survey 65% of households that we attempt to reach. We attempted learning assessments with only one child per household, such that we completed assessments with a child in 82% of households that completed the Round 2 survey. We do find evidence that treatment assignment is associated with the likelihood of recontact and learning assessment completion (Appendix Table A.2). While response rates for those assigned to receive application info, data and application info, or teacher support are statistically indistinguishable from control group rates, we note that those who received general information have slightly lower response rates relative to the control group, and those who received teacher support and a data subsidy have higher response rates. We therefore reject a null hypothesis of equal response rates across all treatment arms at the 10% level ($p = 0.061$) for Round 2, and at the 1% level ($p = 0.001$) for the learning assessments. In terms of respondent characteristics among Round 2 and learning assessment respondents, we do not reject equal distribution of baseline characteristics between each of our main treatment arms and the control group (Appendix Tables A.4 and A.5).

## 2.8 Empirical specification

We estimate intention-to-treat effects, reflecting the causal impact of assignment to each treatment arm on our outcomes of interest. Because some households have more than one child in grades 6–10, we estimate our models at the child level and cluster our standard errors at the household level to reflect the household-level randomization (Abadie et al., 2017).

We estimate equations of the following general form:[16]

$$\begin{aligned} y_{hc} = \alpha &+ \beta_1 GenInfo_h + \beta_2 AppInfo_h + \beta_3 AppInfo * GenInfo_h + \\ &\beta_4 Data_h * AppInfo_h + \beta_5 Data_h * GenInfo_h + \beta_6 Data_h * AppInfo_h * GenInfo_h + \\ &\beta_7 Teacher_h * GenInfo_h + \beta_8 Teacher_h * Data_h * GenInfo_h + X'_{hc}\gamma + f_s + g_w + h_j + \epsilon_{hc} \end{aligned}$$

where $y_{hc}$ is our outcome variable of interest measured at the *household-child* level. $GenInfo_h$ is equal to 1 if household $h$ receives general information about the government TV channel, and $AppInfo_h$ is a binary variable equal to 1 if household $h$ receives information about the adaptive learning platform (Treatment 1). $Data_h$ is assignment to the data subsidy treatment (Treatment 2), and $Teacher_h$ is assignment to the teacher support intervention (Treatment 3). For conciseness, our regression tables present estimates of the main coefficients of interest on $AppInfo_h$ ($\widehat{\beta_2}$), $Data_h * AppInfo_h$ ($\widehat{\beta_4}$), and $Teacher_h * GenInfo_h$ ($\widehat{\beta_7}$), as well as of $GenInfo_h$ ($\widehat{\beta_1}$) to also aid the interpretation of the impacts of the teacher intervention. Note that $Data_h * AppInfo_h$ and $Teacher_h * GenInfo_h$ are both interaction terms, such that $\widehat{\beta_4}$ and $\widehat{\beta_7}$ reflect impacts relative to receiving the corresponding information treatment only. Tables A.6 through A.15 show all seven treatment and treatment interaction coefficients.

We also include a vector of pre-specified household- and child-level covariates, $X$, as well as stratification-cell fixed effects ($f_s$), survey-week fixed effects ($g_w$), and enumerator fixed effects ($h_j$).[17]

[16] The specification we choose as preferred differs slightly from the PAP specification, $y_{hc} = \alpha + \beta_1 GenInfo_h + \beta_2 AppInfo_h + \beta_3 Data_h + \beta_4 Teacher_h + \beta_5 GenInfo_h * AppInfo_h + \beta_6 GenInfo_h * Data_h + \beta_7 AppInfo_h * Data_h + \beta_8 * GenInfo_h * AppInfo_h * Data_h + X'_{hc}\gamma + f_s + g_w + h_j + \epsilon_{hc}$. The reasons are twofold. First, the data discount treatment, $Data_h$, was never provided without an information treatment. Hence, the term $Data_h$ drops from the regression when adding the three interacted terms with $Data_h$ due to multicollinearity. We therefore omit it. Second, teacher support, $Teacher_h$, is always provided with the information treatment. Hence $Teacher_h \equiv Teacher_h * Info_h$. For ease of interpretation, we relabel it accordingly.

[17] Following our pre-analysis plan, we also estimate a set of models in which we use lasso regression to select relevant covariates (Urminsky et al., 2016), selecting a penalty parameter that minimizes the 10-fold cross-validated mean squared error. Following Jones et al. (2019), we use the set of selected covariates that predict the dependent variables, as the treatment variables are

The outcome variables of interest are parent-reported measures of financial investment, time investment, and student use of technology- and non-technology-dependent learning resources (measured in Rounds 1 and 2) and student learning (measured in Round 2). These variables are a subset of those registered outcomes in our pre-analysis plan, and Appendix A presents results for the full set of pre-specified outcomes.

In domains for which we have multiple indicators, we also generate an index based on a simple average of the component outcomes normalized to the control-group mean and standard deviation, following Kling et al. (2007).[18] For individual outcomes, we also adjust for multiple hypothesis testing within each domain by reporting sharpened q-values (Anderson, 2008) alongside the p-values for our key estimated treatment coefficients of interest: $\widehat{\beta_2}$, $\widehat{\beta_4}$, and $\widehat{\beta_7}$. Our secondary analysis includes a discussion of individual coefficients to better understand what factors drive these main results. In that discussion, we report unadjusted p-values only, reflecting the more exploratory nature of the analysis.

## 3 Descriptive insights on parents' educational inputs

To understand the nature of parents' educational investments and their relationship with available household resources among resource-constrained households in Bangladesh, this section documents parental time and economic investments in children's education across three dimensions: type of learning resources used, economic investment in terms of reported tutoring expenditures, and time investment in terms of reported weekly hours helping their children. We split the sample between students from wealthier and poorer households by dividing at the median of the first principal component across a series of socioeconomic status measures.[19] Because we collected investment information while the interventions were ongoing, we examine data only from the control group to avoid confounding the descriptive evidence with treatment effects.

This evidence generates four key insights: (1) roughly one year into school closures, nearly all students continued to regularly pursue educational activities; (2) rates of tech-based resource use are low relative to non-tech-based resources regardless of household characteristics; (3) the difference in parental inputs between wealthier and poorer households is greatest for more costly, tech-based resources; (4) economic and time investments are positively correlated among wealthier households, while the economic investments of poorer households remain consistently lower and are not correlated with their time inputs.

Figure 1 reports the average use of tech-dependent and non-tech-dependent resources disaggregated by socioeconomic status, showing differential use of tech-dependent learning resources across both groups. This hints at the presence of differential constraints to accessing them, indicating that this setting could be fertile ground for educational interventions that reduce specific barriers such as intervention costs. Specifically, non-tech-dependent resources are widely used in all groups in a quite homogeneous way, with 93–95% of students using textbooks and 60–64% meeting with an in-person teacher or tutor. This data was collected in March 2021, a year after school closures began in Bangladesh, and it is remarkable that the vast majority of interviewed students are still engaged with learning resources in some way.

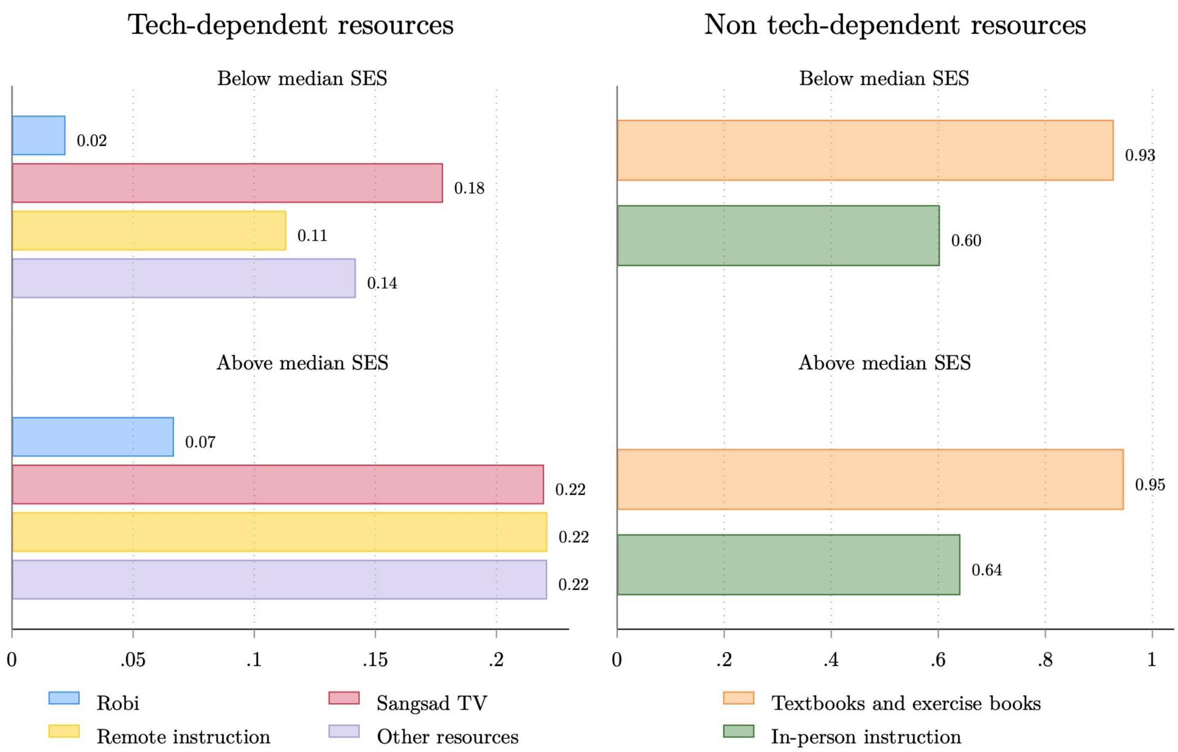
On the other hand, the use of tech-dependent resources is more restricted across all groups, showing that

random in expectation. These results are reported in Appendix Tables A.16, A.17, and A.18, demonstrating modest increases in precision.

[18] In the case of respondents with one or more missing outcome variables, we generate an index by averaging the remaining outcomes for which we have data.

[19] Following our pre-analysis plan, we take the first principal component of the following household SES measures collected at baseline: home ownership, whether members have a bank account, household asset ownership (20 items), fuel and water sources (binary indicators for each type), electricity, number of rooms for sleeping, latrine type (binary indicators for each type), and whether there is a separate kitchen.

Figure 1: Use of learning resources




**Notes:** This figure reports the average use of tech-dependent and non-tech-dependent resources, disaggregated by socioeconomic status. It contains data only for the control group collected during Round 1 (March 2021).

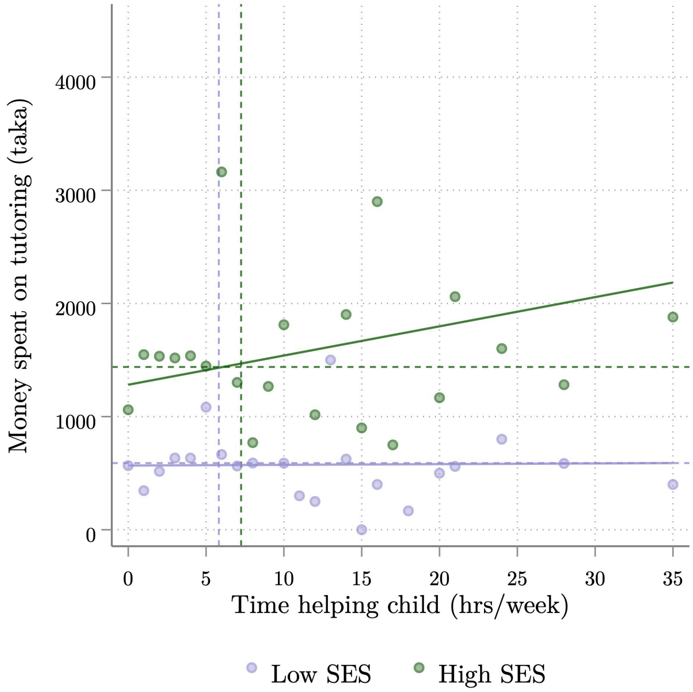
there is a substantial margin for increasing investment. The most popular resources are used by at most 22% of individuals in wealthier households. The use of other resources is lower, with government televised lessons on Sangsad TV being used by 18–22% of students, remote teachers and classes by 11–22%, and the learning app we target by only 2–7%. Additionally, the use of tech-dependent resources differs significantly by socioeconomic status, with higher adoption rates across the board for students from wealthier households. This indicates the potential presence of barriers to education such as economic constraints or social norms that disproportionately affect students from poorer households.

Figure 2 shows the relationship between parental economic investment (money spent on tutoring) and time investment (time helping the child), disaggregated by socioeconomic status. Parents from all socio-economic backgrounds invest significant resources to support their children's learning. Private tutoring is very prevalent, with 64% of households reporting using it in the past month, dedicating on average 1028 taka ($12.46) per month.[20] Although wealthier families are more likely to hire private tutors (68%), it is of note that 59% of children in low-SES households also use this service, indicating that there is extensive use of private tutoring among all sectors of the population.[21] Additionally, parents dedicate on average 6.5 hours

[20]This and all other conversions are based on 1 USD = 83.28 Bangladeshi taka, the average exchange rate between April–June 2021 (OANDA, 2021).

[21]This widespread use of private tutoring is consistent with Alam and Zhu (2021), who report that 68–81% of secondary students in Bangladesh used private tutoring, based on various household survey estimates.

Figure 2: Relationship between parental time and economic investment




**Notes:** This figure reports the relationship between parental economic investment (money spent on tutoring) and time investment (time helping the child), disaggregated by socioeconomic status. It contains data only for the control group collected during Round 1 (March 2021).

a week to support their children with learning activities. Although parents of higher socioeconomic status tend to slightly spend more time per week helping their children with schoolwork (7.25 hours/week and 5.82 hours/week among high-SES and low-SES households, respectively), the primary difference between both groups is in terms of the money spent on tutoring: Poorer households spend on average 589 taka ($7.07) per month, whereas wealthier households spend 1439 taka ($17.28) per month. This difference could be generated either by the number of tutoring hours used or by the price per hour paid by each group.

The figure also shows that the relationship between economic and time investment is positive for wealthier households and zero for poorer households. Hence, wealthier parents spending more money on tutoring tend to also spend more hours per week helping their children with educational activities. This finding is in line with descriptive evidence showing that more educated parents tend to spend more time on childcare and especially on education-oriented activities (Kalil et al., 2016; Ramey and Ramey, 2010; Bansak and Starr, 2021) and larger monetary investments (Corak, 2013; Kornrich and Furstenberg, 2013; Schneider et al., 2018). This could indicate that parents perceive both investments as complements in the human capital production function, or that they have an overall preference for educational investments. However, this observed positive relationship does not appear for poorer families, which may indicate that, being more constrained along several dimensions, they may not have the flexibility to adjust their investments as wealthier households can, leading to sub-optimal investment decisions.

Overall, Figure 1 and Figure 2 highlight that there are significant differences between the use of tech- and non-tech-learning resources and that wealthier and poorer households experience different trade-offs between time and economic investments. One reason for this difference could be that poorer households face more constraints than wealthier households. In the next sections, we describe and present the results of a randomized controlled trial aimed at relieving some of these potential barriers.

# 4 Impacts on parents' educational investments

In this section, we measure the impacts of the three remote educational interventions—providing information about a learning app, supplementing the learning app information with a data package, and providing teacher support in addition to generic information—on the use of tech- and non-tech-dependent learning resources and on parental economic and time investment responses. The key finding is that the interventions trigger changes in the usage of learning resources and in parental time and economic educational investments—regardless of whether they increase the take-up of the intended educational service.

In particular, we find that providing information about the app does not increase the app's use, but it decreases the likelihood of using tech-based learning resources. It also significantly impacts parental economic investment by increasing private tutoring expenditures, while marginally decreasing the time parents help their children with educational activities. The app's usage only increases when the information treatment is accompanied by a data subsidy—suggesting that economic costs could be relevant barrier to the take-up of tech-based learning resources—and parental investment responds less than with the information-only treatment. The teacher support intervention only causes parents to substitute away from the use of non-tech learning resources, but it does not affect their use of tech-dependent learning resources nor their other educational investment choices overall.

## 4.1 Impacts on the usage of learning resources

This section examines the effects of the intervention on students' usage of different learning resources—reported by parents—, broadly classified into tech-dependent and non-tech-dependent learning resources, based on the delivery medium. We find that providing information about the learning app does not increase the app's use, but it decreases the likelihood of using tech-based learning resources. Modest increases in reported learning app usage occur *only* when a free phone data package accompanies the app information. This impact is concentrated on richer households. This suggests that budget constraints could be relevant to the take-up of novel tech-based learning resources, but there are also other important barriers to take-up. The teacher support intervention only causes parents to substitute away from the use of non-tech learning resources.

Panel A of Table 3, Column 1, shows that the provision of information alone does not affect the app's use, and we can reject at the 5% level even modest changes in usage [95% CI: -0.025, 0.011]. Although the use of the learning resource targeted by the messages does not change, the information causes a substitution away from using tech-learning resources, with a net 0.051-SD decrease in the overall index. This result is statistically significant at the 5% level. We see no detectable aggregate change in the use of non-tech-based learning resources (95% CI: –0.06, 0.03).[22]

The data subsidy and the app information increase the use of the remote learning platform by 1.8 percentage points, significant at the unadjusted 10% level (Table 3, Column 1). This is a sizeable 36%

[22] Appendix Tables A.6 and A.7 show the impacts on extensive and intensive margin use of specific learning resources, respectively.

Table 3: Impact of outreach on learning resources

| | (1) | (2) | (3) |
|---|---|---|---|
| **Panel A. All** | | | |
| | App platform | Tech index | Non-tech index |
| App info. | -0.007 | -0.051** | -0.013 |
| | (0.009) | (0.024) | (0.024) |
| | [1.00] | [0.148] | [1.00] |
| Data + App info. | 0.018* | -0.006 | 0.007 |
| | (0.011) | (0.026) | (0.023) |
| | [0.254] | [1.00] | [1.00] |
| Teacher support | -0.012 | 0.007 | -0.102*** |
| | (0.011) | (0.030) | (0.028) |
| | [0.681] | [1.00] | [0.003***] |
| DV mean, control | 0.05 | -0.00 | -0.00 |
| Observations | 5715 | 5715 | 5715 |
| **Panel B. Low-SES Households** | | | |
| | App platform | Tech index | Non-tech index |
| App info. | -0.002 | -0.043 | -0.008 |
| | (0.010) | (0.029) | (0.035) |
| | [1.000] | [0.654] | [1.000] |
| Data + App info. | -0.000 | -0.044 | 0.016 |
| | (0.009) | (0.029) | (0.034) |
| | [1.000] | [0.654] | [1.000] |
| Teacher support | -0.006 | -0.010 | -0.106*** |
| | (0.012) | (0.040) | (0.039) |
| | [1.000] | [1.000] | [0.064*] |
| DV mean, control | 0.02 | -0.10 | -0.03 |
| Observations | 2787 | 2787 | 2787 |
| **Panel C. High-SES Households** | | | |
| | App platform | Tech index | Non-tech index |
| App info. | -0.010 | -0.071* | -0.011 |
| | (0.016) | (0.037) | (0.035) |
| | [0.763] | [0.187] | [0.903] |
| Data + App info. | 0.041** | 0.044 | 0.006 |
| | (0.019) | (0.041) | (0.032) |
| | [0.187] | [0.519] | [0.903] |
| Teacher support | -0.023 | 0.009 | -0.092** |
| | (0.017) | (0.043) | (0.039) |
| | [0.366] | [0.903] | [0.187] |
| DV mean, control | 0.07 | 0.09 | 0.02 |
| Observations | 2928 | 2928 | 2928 |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, the interaction between the general and adaptive information treatments $AppInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * AppInfo_h$, the interaction between teacher, data, and general information treatment $Teacher_h * Data_h * GenInfo_h$, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

increase compared to the 5% usage rate in the control group. With respect to the substitutability between learning resources after individuals received the free data package, there are no statistically significant changes in the use of any other tech-dependent or non-tech learning resources.

There are no detectable impacts of providing one-to-one phone teacher support for 30 minutes each week on the extensive-margin use of tech-related learning resources (Column 2). On the other hand, we do see changes in usage of non-technological learning resources, with a 0.10-SD decrease in the non-tech-dependent learning resource index (Column 3), which is economically and statistically significant at the unadjusted 1% level and has an MHT-adjusted $q$-value of 0.003.

Panels B and C show that, on average, children from both wealthier and poorer backgrounds do not differentially change the use of the app or tech- and non-tech learning resources in response to the interventions. An exception is that wealthier households seem to respond to the subsidy by increasing the app usage by 4.1 percentage points (58%) from a baseline of 7%, significant only at 5% unadjusted levels. In contrast, the subsidy does not impact the behavior and choices of poorer households, which suggests that the economic costs and knowledge about learning resources are not the only barriers to using phone learning apps that low-SES families face. Parents from both high and low socioeconomic backgrounds do not differentially change their resource use in response to the teacher support, with a 0.092-SD and 0.106-SD decrease of the non-tech resource use, respectively, and no changes otherwise.

Overall, that we observe any increase in the use of the app is remarkable in light of its light-touch nature. First, the free data package was delivered to *a* mobile phone in the household, but it could have been the case that it was not available to the student for regular learning use. Second, the data package was delivered in an *unconditional way*, i.e., individuals received the internet top-up with simply a message explaining the award, without additional checks on how the data was being spent. Thus, nothing prevented students (or parents) from using the data package for non-academic activities like navigating the web, using it for business purposes, or calling family or friends. Data use may be especially likely to be undirected when parents are unable to easily monitor their children's use (Gallego et al., 2020).[23]

Given that the learning resource usage is parent-reported, it instead could be the case that the information increased the salience of the app, leading parents to report increased use due to desirability or other reasons, or it could be that students told parents they were using the recommended app while they spent their time (and internet data) accessing other learning resources or distractions. Social desirability bias seems an unlikely explanation for the observed effects given that we would also expect an increase in reported app usage with only the provision of information. Social desirability bias would also not support the documented reduction in parental time spent with children as a result of app information. Additionally, we detect no persistent effects of the information interventions on parental investment decisions after the interventions concluded, when parents could presumably still feel pressured to report higher usage.[24]

## 4.2 Impacts on parental educational investments

This section reports significant impacts of the interventions on parental time investment, and parental economic investment, measured primarily through private tutoring expenditures. We find that the information and weekly reminders about the app—alone, and accompanied by the subsidy—led parents to reduce

[23]Appendix Table A.8 shows treatment impacts on self-reported estimated data consumption. The data and app information combination leads to an estimated 1 GB increase in monthly use, which is not statistically significant ($p = 0.382$). However, we interpret this with great caution because measurement error is likely to be high.

[24]These results are shown in Appendix Tables A.9 and A.10.

self-reported time investment in their children's learning, and increase their financial investments in private tutoring, both on the extensive and intensive margin. Supplementing the learning app information with the data package generates attenuates parental time and economic re-optimization responses of the app information alone. The teacher support does not seem to affect parents' other educational investment decisions. The results also show that parental time investments and parental tutoring investments tend to move in opposite directions.

Table 4: Impact of outreach on parental investment

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A. All** | | | | |
| | Hours parent helped | Private tutoring | Money on tutoring | Money on other education |
| App info. | -0.646** | 0.045** | 197.309** | -31.178*** |
| | (0.328) | (0.021) | (81.368) | (11.194) |
| | [0.082*] | [0.082*] | [0.074*] | [0.069*] |
| Data + App info. | -0.347 | 0.050** | 66.722 | -14.409 |
| | (0.346) | (0.021) | (74.831) | (11.606) |
| | [0.312] | [0.074*] | [0.331] | [0.226] |
| Teacher support | 0.227 | -0.052** | 27.459 | 4.202 |
| | (0.393) | (0.025) | (93.884) | (14.645) |
| | [0.511] | [0.082*] | [0.582] | [0.582] |
| DV mean, control | 6.57 | 0.64 | 1027.82 | 138.56 |
| Observations | 5359 | 5688 | 5359 | 5065 |
| **Panel B. Low-SES Households** | | | | |
| | Hours parent helped | Private tutoring | Money on tutoring | Money on other education |
| App info. | -1.333*** | 0.016 | 107.278 | -36.110*** |
| | (0.420) | (0.032) | (75.137) | (11.173) |
| | [0.010**] | [0.450] | [0.152] | [0.010**] |
| Data + App info. | -0.177 | 0.072** | 180.483** | -25.173** |
| | (0.482) | (0.032) | (76.757) | (11.975) |
| | [0.481] | [0.063*] | [0.063*] | [0.076*] |
| Teacher support | 0.332 | -0.068* | -5.103 | -12.670 |
| | (0.537) | (0.036) | (76.203) | (15.088) |
| | [0.425] | [0.090*] | [0.652] | [0.358] |
| DV mean, control | 5.82 | 0.59 | 589.29 | 80.12 |
| Observations | 2613 | 2772 | 2643 | 2458 |
| **Panel C. High-SES Households** | | | | |
| | Hours parent helped | Private tutoring | Money on tutoring | Money on other education |
| App info. | -0.201 | 0.063** | 265.518* | -26.719 |
| | (0.499) | (0.029) | (143.429) | (19.213) |
| | [1.000] | [0.628] | [0.628] | [1.000] |
| Data + App info. | -0.543 | 0.029 | -35.527 | 3.684 |
| | (0.502) | (0.029) | (132.385) | (20.473) |
| | [1.000] | [1.000] | [1.000] | [1.000] |
| Teacher support | 0.082 | -0.032 | 29.609 | 15.189 |
| | (0.602) | (0.035) | (173.089) | (25.194) |
| | [1.000] | [1.000] | [1.000] | [1.000] |
| DV mean, control | 7.25 | 0.68 | 1438.55 | 190.40 |
| Observations | 2746 | 2916 | 2716 | 2606 |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, the interaction between the general and adaptive information treatments $AppInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * AppInfo_h$, the interaction between teacher, data, and general information treatment $Teacher_h * Data_h * GenInfo_h$, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 shows the likelihood of using private tutoring increased by 4.5 percentage points, which is a 7% increase over a baseline of 64% and statistically significant at the 5% unadjusted level (Panel A, Column 2), with a MHT-adjusted q-value of 0.082. On average, parents also spent 197 BDT ($2.37 USD) more in a week, a 19% increase in tutoring expenditures with respect to the baseline of 1028 BDT ($12.34 USD). This increase is significant at the 5% unadjusted level and 10% adjusted level. However, about 16% of this increase may have been offset by moving away from money spent on other educational resources, which decreases by 31 BDT ($0.37, a 22% decline). In terms of parental time investment, parents' weekly hours spent helping their children study decreased by 9.8% from a baseline of weekly 6.6 hours (Panel A, Column 1).

The impacts of the information combined with the data subsidy show a similar pattern than with the provision of information alone—reduction of parental time investment and increase in tutoring investment—with on average smaller and more noisy estimates. The only significantly estimated impact is a 5 percentage point increase in the likelihood of using private tutoring, significant at the 5% unadjusted and 10% MHT adjusted levels. This increase is very similar to the one caused by the app information alone. If anything, this suggests that the data subsidy causes a mitigation of the parental investment re-optimization responses from the information provision only.

Phone teacher support reduces the likelihood that students are receiving private tutoring by 5.2 percentage points (a 8.1% decrease), which is significant at the unadjusted 5% and MHT-adjusted 10% level. We do not, however, see significant change in parents' time spent helping children or in expenditures on tutoring or other educational expenses.

In terms of heterogeneity analysis, Panels B and C show that the increase in economic educational investment is greatest for high-SES parents, for whom the reduction in time investments is less pronounced. App information increases the likelihood that parents of high-SES students use a private tutor by 6.3 percentage points relative to the high-SES control group, significant at the 5% level, and they increase monthly tutoring expenditures by 266 taka ($3.19), significant at 10% level. In contrast, parents of low-SES households have modest increases in extensive and intensive margin tutoring investment in response to app information, although none of them are significant at conventional levels, and they additionally substitute away from other educational investments, reducing expenses by 36 taka ($0.43). In addition, lower-SES parents reduce hours spent helping by 1.3 hours relative to a control-group mean of 5.8 hours per week, statistically significant at the 1% level, while the reduction among higher-SES parents is only 0.2 hours per week, which is not statistically significant ($p = 0.313$).

In contrast, the subsidy supplementing the app information mitigates parental economic investments from wealthier families, with decreases in the private tutoring expenditures that essentially cancel the increased investment caused by the app information alone, with the additional effect being significant at the 10% level.

# 5 Persistent effects on student learning

This section provides suggestive evidence that the parental behavioral responses to educational interventions may have increased students' observed achievement. The learning app information provision and reminders increase student achievement in mathematics, with the increase concentrated among richer households. However, receiving the subsidy alongside the app information leads to null effects on learning. The phone teacher support does not improve math achievement either. By contrasting the results on parental responses to the learning results, we conclude that tutoring increases seem to be the cause of the student achievement increase, not the app's use.

Table 5 presents two alternative measures of student achievement at endline, two months after the interventions concluded. Column 1 reports the "unadjusted score" created by summing student scores across the set of four questions asked of all students of the same grade level, normalizing to the control-group mean for each grade level. Column 2 shows impacts on predicted latent ability based on a two-parameter

Table 5: Impact of outreach on student learning (math), endline

| | (1) | (2) |
|---|---|---|
| **Panel A. All** | | |
| | Unadjusted score | IRT, 2pl |
| App info. | 0.105* | 0.107* |
| | (0.060) | (0.057) |
| | [1.000] | [1.000] |
| Data + App info. | 0.006 | -0.009 |
| | (0.050) | (0.050) |
| | [1.000] | [1.000] |
| Teacher support | 0.023 | -0.019 |
| | (0.060) | (0.057) |
| | [1.000] | [1.000] |
| DV mean, control | 0.01 | 0.00 |
| Observations | 3433 | 3433 |
| **Panel B. Low-SES Households** | | |
| | Unadjusted score | IRT, 2pl |
| App info. | 0.001 | 0.040 |
| | (0.096) | (0.092) |
| | [1.000] | [1.000] |
| Data + App info. | 0.035 | 0.039 |
| | (0.076) | (0.073) |
| | [1.000] | [1.000] |
| Teacher support | 0.065 | 0.069 |
| | (0.098) | (0.089) |
| | [1.000] | [1.000] |
| DV mean, control | -0.15 | -0.21 |
| Observations | 1561 | 1561 |
| **Panel C. High-SES Households** | | |
| | Unadjusted score | IRT, 2pl |
| App info. | 0.205** | 0.178** |
| | (0.080) | (0.074) |
| | [0.053*] | [0.053*] |
| Data + App info. | -0.023 | -0.042 |
| | (0.071) | (0.073) |
| | [0.815] | [0.734] |
| Teacher support | -0.003 | -0.099 |
| | (0.077) | (0.079) |
| | [0.931] | [0.389] |
| DV mean, control | 0.15 | 0.17 |
| Observations | 1862 | 1862 |

**Notes:** Sample includes all Round 2 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, the interaction between the general and adaptive information treatments $AppInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * AppInfo_h$, the interaction between teacher, data, and general information treatment $Teacher_h * Data_h * GenInfo_h$, plus flags for missing values. Anderson $q$-values reported in brackets. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

item response model among the full set of mathematics inventory items, normalized by the control-group mean (not grade-specific). Because the assessment was conducted by phone, we limited students to eight items from the 19-question battery in order to minimize the burden on respondents. Consequently, these IRT-based results should be interpreted with some caution, although they align closely in magnitude and significance with the unadjusted scores based on the grade-specific base questions in Column 1 in nearly all cases.[25] One exception is that impacts of teacher support on latent ability in Column 2 are qualitatively the same but roughly half that of those measured in the smaller set of four base questions.[26] Although the math achievement measures have shortcomings that limit their use as hard evidence on the effectiveness of a particular intervention in improving achievement, they are useful outcomes to assess whether the parental educational investment responses to different interventions can have persistent impacts on a key outcome of interest.

Panel A of Table 5 shows a 0.11-s.d. increase in mathematics scores among those who received the app information. Reflecting the reduced sample size due to endline attrition and that we administered the test to one randomly-selected child per household, these effects are at most statistically significant at the unadjusted 10% level.

Receiving the free data package alongside the learning app information effectively negates the learning gains from the app information alone by reducing achievement by 0.10–0.11 s.d., resulting in a summed impact of zero. The results in Section 4 show that a reduction in data costs slightly increases the reported app usage. However, we cannot distinguish whether the lack of a detectable change in human capital is a result of low overall dosage of the app, or because the technology was not effective in the short term.

The bottom row of Table 5 suggests that the remote teacher support treatment does not significantly increase student mathematics achievement relative to the control group, neither on the grade-specific set of four "base" nor on the estimated latent ability.[27] This is perhaps not surprising, given the light-touch nature of the teaching support. Successful teaching support interventions rely on more intensive treatments.

Panels B and C show that the estimated positive impacts occur entirely among high-SES students. We estimate a 0.18–0.21 standard-deviation increase in the math knowledge of wealthier students, both measures are statistically significant at 5% unadjusted levels at at 10% MHT-adjusted levels. We find no evidence of impacts of the app information among low-SES students, with the 95% CI intervals of [–0.19, 0.19] using the base scores, and [–0.14, 0.22] using the 2-parameter IRT model. The concentration of math achievement gains only among high-SES students suggests that the information treatment may be exacerbating existing educational inequalities, and that additional constraints may exist for low-SES households. There are no detectable learning impacts of the subsidy for wealthier or poorer households, nor of the teacher phone support.

Combined with the evidence that the app information increases parental educational investment via

[25] Stone (1992) finds that estimates of ability using two-parameter logistic models for test lengths of at least 10 are precise and stable using simulated data, although extreme levels of ability were biased toward zero with all tested combinations of relatively short tests (10–30 items) and relatively small samples (250–1000). Sahin and Anil (2017) use test results from university students and finds that lengths of 10 perform well conditional on a sample size of at least 750. In line with this previous work, Crawfurd et al. (2021) estimate student ability measured through a phone survey using a two-parameter model with 11–12 questions per respondent.

[26] Appendix C includes a summary of descriptive statistics showing that questions have positive discrimination and capture a range of ability levels. We also observe that both the unadjusted four-question scores and latent measures are strongly correlated with student baseline ability, which we measure based on students' reported PEC math scores, the high-stakes exam students take after grade 5 (Figure A.3). In this self-reported question, students indicate whether they received an A+ (80–100), A (70–79), A- (60–69), B (50–59), C (40–49), or D (33-39).

[27] The difference in teacher effects between Columns 1 and Columns 2 reflects the fact that Column 1 uses only the grade-specific set of four base questions, whereas the other column draws from the full set of questions students answered to construct the latent ability parameters.

tutoring, and that both the tutoring and learning effects are concentrated among students from higher-SES families, we conclude that the increase in math knowledge from the app informational campaign goes through parental re-optimization of their investments—increasing private tutoring—, not through the app effectiveness.

# 6 Discussion

To guide the interpretation and discussion of the results, as well as to identify potential channels through which they may operate, it is helpful to put the three interventions— one-on-one teaching support, information about a novel technology, and a data subsidy—into a unified framework where parental educational investments and decisions are important inputs. This section identifies the key elements of the setting, and highlights important trade-offs and dynamics that can occur in this context when attempting to decrease barriers to remote education through different policies. Appendix B includes a stylized model that formalizes some of the discussions below.[28]

In a context without formal schooling, human capital and learning can be fostered by households themselves—when they invest time supporting their children—, by private tutors or support teachers, or by students in an autonomous way through a variety of learning resources. Some of these resources are well-known, widely used, and require little investment and infrastructure (i.e., textbooks and exercise books). Another set of learning resources, educational technologies (edtech) have emerged in recent years as an alternative or complement to traditional pedagogical methods. They have been implemented both in formal schooling environments and as a means to provide education remotely. The latter is likely to be most important for learners from more disadvantaged backgrounds, who may lack access to an adequate learning environment and quality instruction (Lai et al., 2012; Caballero Montoya et al., 2021) or have higher rates of education disruption. However, edtech learning resources are usually costly and can have stricter barriers to access.

A commonly used policy to increase the usage of new educational technologies is to provide information about it.[29] Information about a new technology may cause households to adjust their perceived returns about the new technology *and* about other personalized learning options such as external teaching. The information can signal the value of this new platform, leading individuals to revise their beliefs about its marginal returns. However, the information on new technologies can more broadly shift the beliefs of the value of similar features from other educational inputs, making them also revise their perceived returns to investment (upwards or downwards). The information could also shift parents' beliefs related to broader educational investments by increasing their salience. Based on this new signal, households may update their educational investments accordingly, investing in the combination of learning options they can afford that will give the highest perceived returns. This implies that an informational policy aimed at decreasing the

[28]In the model, human capital is provided through internal teaching—by parents themselves—or through external teaching via schools and tutors. First, we characterize the optimal household decision rules in this environment. Then, we include a novel form of acquiring human capital through the use of education technology that is costly and about which the perceived and actual returns may not be equal.

[29]A series of papers highlight the utility of behavioral nudges to encourage continuous investment among students enrolled in MOOC courses, in which student drop-out rates are high (see Yeomans and Reich (2017); Martinez (2014); Patterson (2018); Baker et al. (2016) among others). In Uruguay, e-messages and nudges targeting different behavioral biases boosted parental investment in early childhood development (Bloomfield et al., 2022). In the United States, SMS reminders to do home literacy activities for parents increased early literacy of preschoolers (York et al., 2018) and kindergarteners only when messages were personalized (Doss et al., 2019). Similar reminders provided to parents of children of Head Start centers (Hurwitz et al., 2015) and parents of primary school children over the summer break (Kraft and Monti-Nussbaum, 2017) had also positive effects.

barriers to edtech may generate behavioral responses on monetary and time investments from parents and a re-optimization of the learning resource portfolio above and beyond direct changes in edtech usage.

The absence of an information-induced increase in usage of the targeted learning platform suggests that information was not the only binding barrier to the app's use. This hypothesis is further supported by an increase in learning app take-up *only* when information about the app is accompanied by reduced data costs, showing that app info indeed led parents to revise upwards their perceived marginal returns of the learning app (equation 3), but they only increased investment in the novel remote learning resource when the data package relieved additional binding barriers to usage. Additionally, the data-induced change in app usage generated an extensive-margin increase in the use of tech-dependent options, rather than being offset through substitution with other tech-dependent learning resources, further suggesting that the data package reduced barriers to access rather than affecting parents' perceptions of the app's effectiveness relative to other tech options. However, low overall reported app take-up even after receiving information and data support suggests that other barriers are still present or that parents may still ascribe relatively low marginal returns to the app. Similarly, the lack of detectable learning impacts generated by the data package could reflect low take-up and usage of the app, so we cannot rule out that the app itself may be effective, if used. More specific information or content may be needed to update parents' beliefs about its returns, or parents may correctly attribute a low return to the resource, either because it provides intrinsically low value or because a lack of computer literacy or the accompanying distractions of internet use limit its benefit (Beuermann et al., 2015; Piper et al., 2016; Cristia et al., 2017; Malamud et al., 2019).

That the phone messages and reminders about a novel learning app caused substitution away from tech-learning resources and substantial parental investment responses is consistent with the assumption in section B.2 that they provided new information about the returns of other personalized learning options or increased the salience of certain types of investments. The presence of additional constraints to using the targeted resource induced parents to respond to the information or salience by adjusting investments in other dimensions, causing a move away from other learning resources and parental time investments and an increase in private tutoring. There are three potential reasons for the increase in tutoring. First, information about the learning app highlighted learning interaction via quizzes—an adaptive learning feature—which could have triggered increased investment among the personalized learning resource most familiar and available to families: tutoring. Second, although the app is cheaper than tutoring, parents may feel they have more control over children's time usage through tutoring (Gallego et al., 2020). Lastly, they may simply believe that tutoring has a higher return to investment. In turn, it appears that the increase in tutoring generated learning gains, which are concentrated among students from higher SES backgrounds.

One interpretation of the impacts of the learning app info on tutoring and app usage (with and without the supplemental data package) is that there is an underlying economic trade-off between tutoring and learning app usage. Although using the learning app is less costly than tutoring, this economic trade-off could be a potential explanation if tutoring has a higher perceived return to investment. An alternative explanation speaks to the allocation of resources within the household and to the potential existence of different "investment buckets" within the household (Duflo and Udry, 2004). In normal circumstances, parents may invest in their children's education primarily through tutoring expenses, and phone internet data is only used by parents. In this case, they may respond to changes in education information or costs by reallocating resources within their children's investment options set. However, when a data package is provided explicitly for educational purposes, they allow their child to use it for the novel learning resource. Under this framework of mental accounting of household resources, it could also be that parents with a set budget are only willing to make investment changes toward the educational investment with the highest perceived returns (tutoring). However, if a novel learning resource with unknown returns is provided at no cost, they may be more likely to experiment and try it out even if it has a low perceived, but uncertain, return.

The results also show that parents adjust their economic investment relatively more than their time investment. This could indicate the existence of differential perceived marginal returns between time and economic investments or of differential parental investment elasticities, with time spent with their children being more inelastic in the short-run than economic investment in tutoring. The heterogeneity results support the latter argument by showing that increased tutoring effects are concentrated among wealthier families. Because the intervention duration was relatively short, it is plausible that more time is needed for stronger behavioral responses. Re-optimizing time and economic investments is costly, and it may be differentially so for low-SES households. Anticipating a return to pre-intervention levels of internet data and one-to-one teaching support, households may have internalized the temporality of the changes and favored their baseline investment choices. This argument would suggest that interventions longer than a couple of months could generate larger and more persistent changes in household investments.

Lastly, the impact of the teacher support intervention on learning appears to be driven directly by the increase in external teaching hours, given high take-up of the intervention and very limited parental responses in terms of monetary and time investments (equation 2). These relatively small behavioral responses could reflect parents' understanding that the support is temporary. The few observed changes in household educational investments suggest that parents perceived a weekly call from a teacher as a substitute of non-tech learning resources for consultation purposes and solving questions, decreasing the likelihood of meeting with a teacher in person in an economically and statistically meaningful way—potentially reducing unsafe behavior in the context of a pandemic—and marginally decreasing the use of textbooks, the most widespread consultation material. An alternative explanation for the lack of parental responses is that they do not see the support as valuable but, in that case, we would expect that perception to be accompanied by low treatment take-up and no changes in financial investment.

An alternative channel through which the interventions may affect human capital is through impacting student effort and engagement, either directly or indirectly as a result of the observed changes in parental investments. Although the stylized model abstracts from this feature, student effort may change in response to educational policies or changes in parental investments, impacting in turn the equilibrium parameters (Albornoz et al., 2018). We explore the impacts on the time students spend studying and their self-reported engagement and aspirations, finding that student effort and aspirations are high and unaffected by the interventions (Appendix Table A.11).

In the absence of any interventions, nearly all students devote substantial time to studying and schoolwork despite the ongoing school closures. In March 2021, parents report that students spent an average of 5.7 days per week doing school work, amounting to an average of 19 hours per week. We do not find evidence that any information, data subsidies, or teacher interventions affect student effort along these dimensions (Appendix Table A.12). It could be that time use is not a margin that could be easily influenced if other barriers to student time investment are also present. It could also be that students were already dedicating their optimal amount of time to learning, in which case we would see no effects when more information or support is added.

# 7 Conclusions

We conduct a field experiment among households across Bangladesh during the COVID-19 school closures to measure the impact of three educational interventions aimed at reducing different barriers to parental education investment. Our results show that offering an educational service when other barriers to take-up are present may lead parents to reoptimize their educational investments, even when they don't adopt the promoted service, and this response can have lasting effects on achievement. When interventions do increase the take-up, parental responses are less prominent, and learning changes can be attributed more directly to the educational service promoted. The disparate impacts of these interventions between poorer and wealthier households indicate that some policies aimed at reducing barriers to accessing remote education may exacerbate educational inequalities.

We find that a light-touch informational campaign promoting a remote learning app does not increase the app's usage, but it instead triggers parental behavioral responses, with significant increases in economic investment in tutoring and decreases in parental time investment in helping their children with educational activities. We provide evidence supporting the explanation that the information acted as a signal about returns to certain learning services or as a salience nudge, but that other barriers to take-up were present. This caused a re-optimization of the other parental educational investments, especially among wealthier households, while not changing the promoted service take-up. Relieving additional constraints by combining informational messages with a free internet data package does increase reported usage of the learning app and limits the need for parental resource reallocation.

We also observe persistent increases in student math achievement only for the app information campaign, while these learning effects disappear when the information intervention is combined with the data package. We interpret these results as evidence that the positive impacts of the informational campaign are partly driven by the increased parental investments in the form of tutoring expenditures. The learning gains indirectly generated through regular information provision are concentrated among wealthier households, which are likely the ones with the capacity to adjust their investments in response to the new information.

As a contrasting intervention, we find that individualized teacher support by phone also promotes math learning, and minimal parental behavioral responses in that treatment group suggest that these effects are not driven by household re-optimization. These effects are stronger for lower-income students, who are more likely to be resource constrained. Teacher support improves learning only among students from households with below-median socioeconomic status, contrasting with the indirect positive effects of the informational intervention concentrated among high-SES students.

Our results highlight the importance of parent behavioral responses as a driver of policy impacts, and they show that these decisions affect the distributional impacts of educational policies. While the specific trade-offs and constraints parents face may be context-specific, the underlying insight—that parental investment responses, far from second-order, generate measurable and heterogeneous impacts on student outcomes—is important across a range of settings (Das et al., 2013). Additionally, the observed parental behavioral responses to our interventions suggest that parents value personalized support, such as that provided through teacher support or private tutoring, which in turn have detectable effects on student learning. While phone data appears to be a constraint to the take-up and use of remote learning technologies, its unconstrained provision does not generate measurable learning gains, suggesting the need to complement it with guidance and personalized learning support to reap the benefits of educational technologies.

From a policy perspective, these results demonstrate the potential of remotely delivered interventions to affect parental educational investments and promote student learning. That we find learning gains through personalized inputs—tutoring and one-on-one teacher support, in this context—is in line with existing literature on teaching at the right level (Banerjee et al., 2007, 2016; Muralidharan et al., 2019). The extent to which policymakers consider both the role of parents' re-optimization responses and the potential constraints to intervention take-up when designing educational policies will be important factors in determining the effectiveness of these policies and their implications for inequality.

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# A For Online Publication: Appendix Figures and Tables

Figure A.1: Project timeline

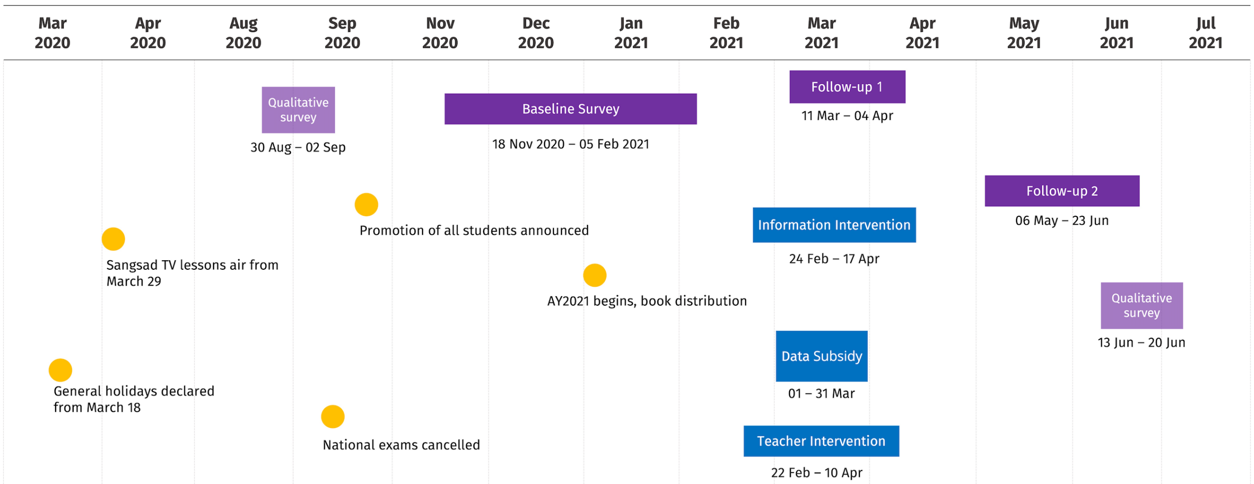


Figure A.2: Distribution of respondents

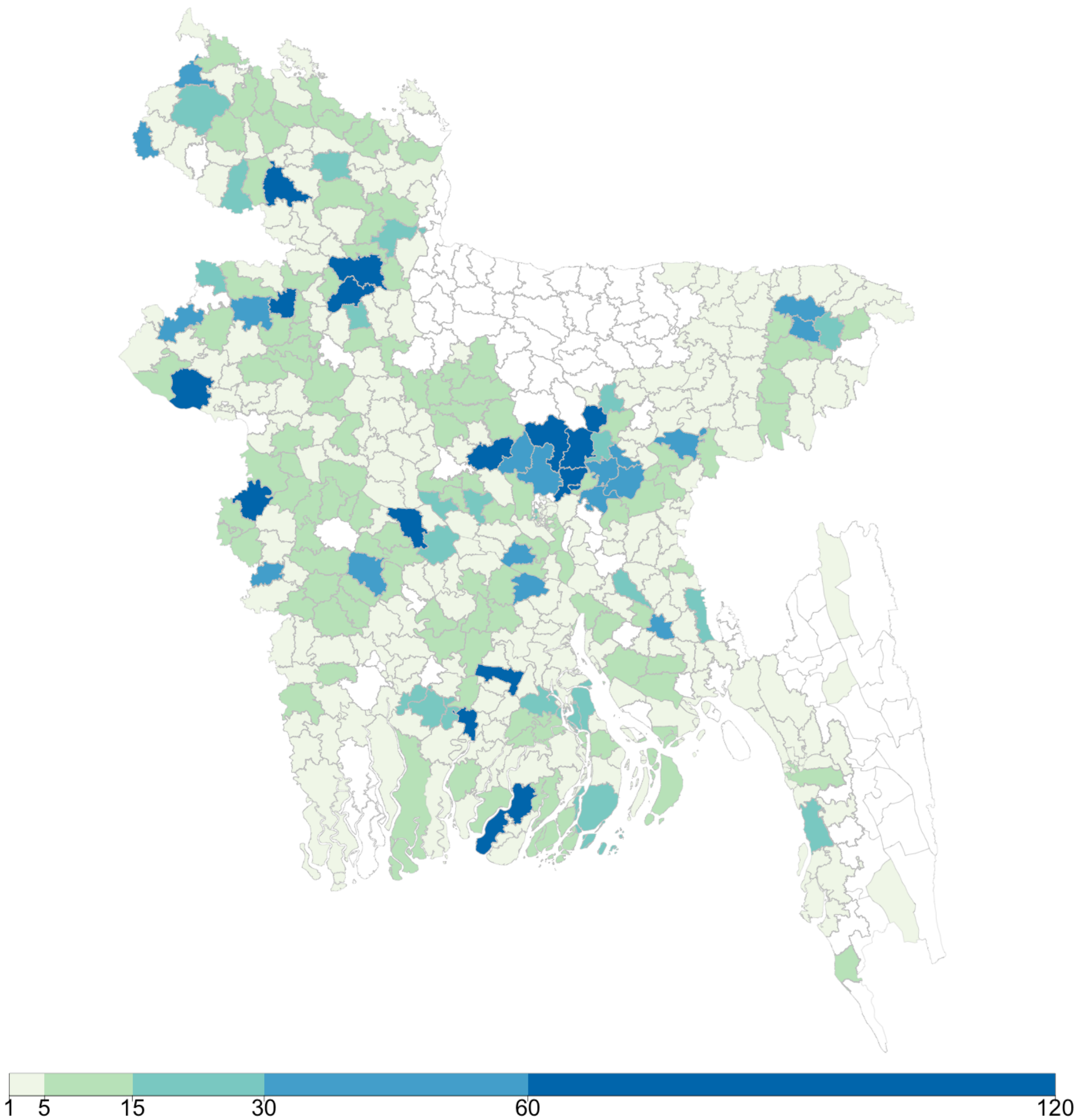


Figure A.3: Distribution of endline math scores by self-reported Grade 5 exam scores

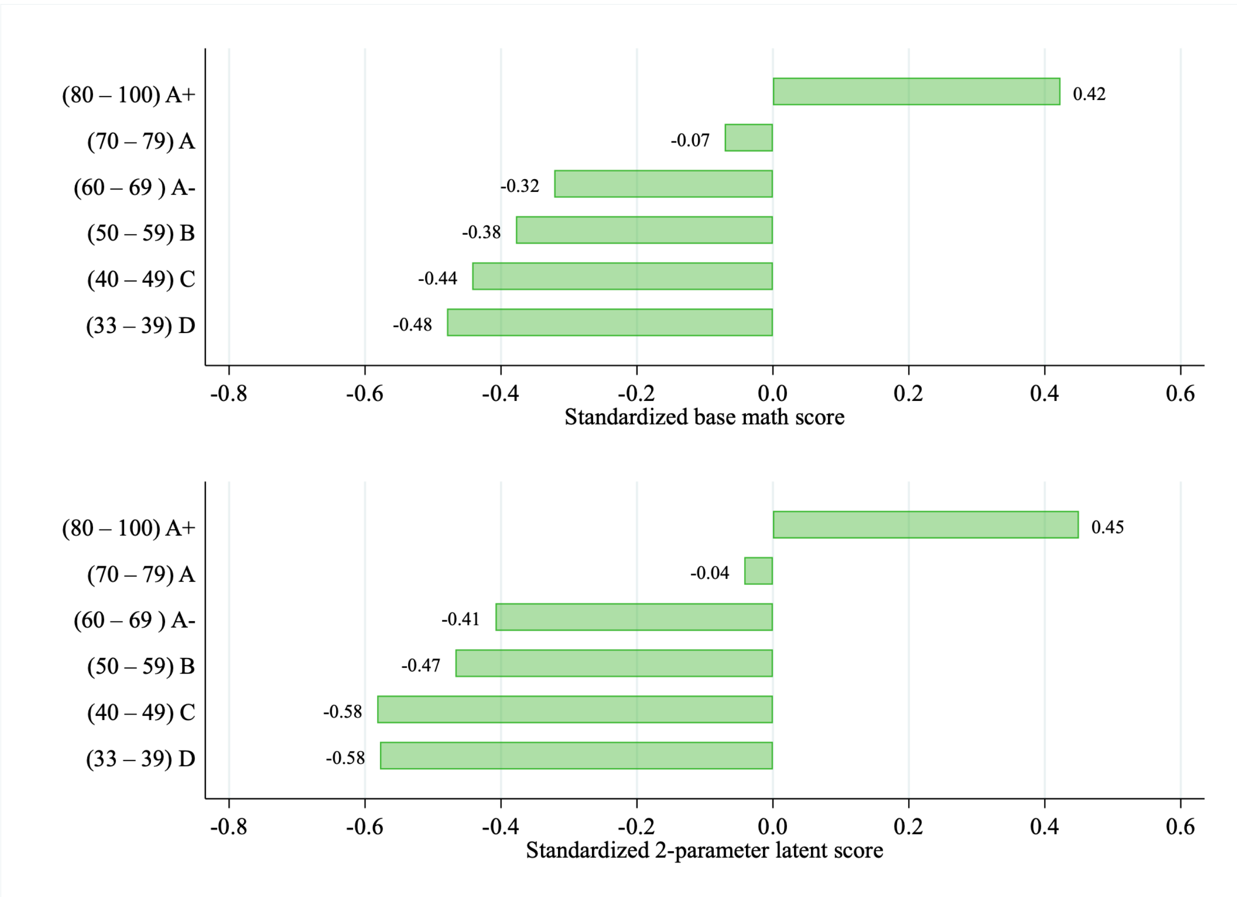


Table A.1: Study eligibility by data source

| | Konnect | | SSS Sample | | RDD Sample | | Total | |
|---|---|---|---|---|---|---|---|---|
| | N | % | N | % | N | % | N | % |
| Attempted | 14678 | | 12569 | | 11720 | | 38967 | |
| Answered | 10563 | 72% | 8573 | 68% | 6772 | 58% | 25908 | 66% |
| Children in grades 6-10 | 5681 | 54% | 5528 | 64% | 2163 | 32% | 13372 | 52% |
| Smartphones in household | 3962 | 70% | 3152 | 57% | 1321 | 61% | 8435 | 63% |
| Eligible and consented | 3653 | 92% | 2983 | 95% | 1240 | 94% | 7876 | 93% |
| Completed Baseline | 3506 | 96% | 2896 | 97% | 1174 | 95% | 7576 | 96% |
| Baseline / Attempted | | 24% | | 23% | | 10% | | 19% |

Table A.2: Response rates by treatment assignment

| | (1) | (2) | (3) |
|---|---|---|---|
| | Round 1 | Round 2 | R2 Learning assessment |
| General info. only | 0.018 | -0.070** | -0.061** |
| | (0.020) | (0.032) | (0.027) |
| App info. | 0.015 | -0.027 | -0.033* |
| | (0.019) | (0.023) | (0.020) |
| Data + General info. | -0.010 | -0.065* | -0.055* |
| | (0.031) | (0.033) | (0.028) |
| Data + App info. | 0.022 | -0.012 | 0.019 |
| | (0.018) | (0.020) | (0.017) |
| Data + General info. + App info. | -0.027 | 0.056 | 0.044 |
| | (0.039) | (0.041) | (0.035) |
| Teacher support | -0.013 | -0.032 | 0.008 |
| | (0.022) | (0.023) | (0.020) |
| Teacher support + Data | -0.074 | 0.132** | 0.118** |
| | (0.054) | (0.053) | (0.047) |
| Observations | 8397 | 6981 | 6981 |
| Response rate, control | 0.68 | 0.67 | 0.51 |
| P-val, joint significance | 0.1254 | 0.1446 | 0.0049 |

**Notes:** Child-level data includes all respondents contacted at Round 1 and Round 2 surveys, respectively. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey week fixed effects. Household-level controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Balance tests by pooled treatment assignment, Round 1 respondents only

| | (1) All | (2) Control | (3) App info | (4) Data + App info. | (5) Teacher | (6) Joint tests, all, p-val |
|---|---|---|---|---|---|---|
| HH size | 1.89 | 1.88 | 1.96 | 1.85 | 1.90 | 0.730 |
| | (0.97) | (0.95) | (0.98) | (0.96) | (1.01) | |
| Num. secondary children | 1.27 | 1.24 | 1.32*** | 1.24 | 1.29 | 0.082* |
| | (0.51) | (0.45) | (0.56) | (0.48) | (0.61) | |
| Has cable/satellite TV | 0.65 | 0.66 | 0.63* | 0.65 | 0.67 | 0.007*** |
| | (0.48) | (0.47) | (0.48) | (0.48) | (0.47) | |
| Mother present | 0.49 | 0.50 | 0.49 | 0.51 | 0.49 | 0.631 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Father present | 0.50 | 0.50 | 0.50 | 0.48 | 0.51 | 0.603 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Mother primary | 0.35 | 0.36 | 0.34 | 0.34 | 0.35 | 0.023** |
| | (0.48) | (0.48) | (0.47) | (0.47) | (0.48) | |
| Mother secondary | 0.19 | 0.18 | 0.19 | 0.20 | 0.17 | 0.933 |
| | (0.39) | (0.39) | (0.39) | (0.40) | (0.38) | |
| Mother post-secondary | 0.19 | 0.19 | 0.17 | 0.19 | 0.19 | 0.552 |
| | (0.39) | (0.40) | (0.38) | (0.39) | (0.40) | |
| Father primary | 0.25 | 0.25 | 0.26 | 0.26 | 0.25 | 0.803 |
| | (0.44) | (0.43) | (0.44) | (0.44) | (0.43) | |
| Father secondary | 0.18 | 0.17 | 0.17 | 0.18 | 0.20 | 0.316 |
| | (0.38) | (0.38) | (0.37) | (0.38) | (0.40) | |
| Father post-secondary | 0.26 | 0.26 | 0.25 | 0.25 | 0.23 | 0.265 |
| | (0.44) | (0.44) | (0.43) | (0.44) | (0.42) | |
| Mother income | 5069 | 5034 | 3910 | 6331 | 3668 | 0.011** |
| | (25644) | (26069) | (20698) | (29703) | (22706) | |
| Father income | 53751 | 52451 | 53614 | 54721 | 51200 | 0.783 |
| | (137335) | (134796) | (137993) | (140122) | (124421) | |
| School days/week, curr. | 5.75 | 5.80 | 5.74 | 5.72 | 5.68 | 0.935 |
| | (2.20) | (2.13) | (2.23) | (2.22) | (2.31) | |
| School days/week, Apr. 20 | 5.43 | 5.47 | 5.46 | 5.39 | 5.45 | 0.914 |
| | (2.13) | (2.11) | (2.07) | (2.19) | (2.14) | |
| Has private tutor | 0.60 | 0.59 | 0.60 | 0.59 | 0.61 | 0.981 |
| | (0.49) | (0.49) | (0.49) | (0.49) | (0.49) | |
| Working for pay | 0.03 | 0.03 | 0.03 | 0.03 | 0.02 | 0.660 |
| | (0.17) | (0.17) | (0.18) | (0.17) | (0.15) | |
| Number of students | 5736 | 1411 | 1477 | 1448 | 587 | |
| Number of households | 5021 | 1249 | 1258 | 1284 | 514 | |
| Joint test, p-val | | | 0.482 | 0.955 | 0.728 | |

**Notes**: Sample includes all randomized baseline respondents at the child level. Stars in columns 3–5 indicate statistically significant differences relative to the control group (column 2). Column 6 reports p-values based on F-tests of the joint significance of all eight treatment indicators, excluding respondents with missing values. P-values in the bottom row are from seemingly unrelated regressions that predict treatment assignment relative to the control group, with missing variable flags included. Standard errors are clustered at the household level. Stratification-cell fixed effects are included in all regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent significance levels.

Table A.4: Balance tests by pooled treatment assignment, Round 2 respondents only

| | (1) All | (2) Control | (3) App info | (4) Data + App info. | (5) Teacher | (6) Joint tests, all, p-val |
|---|---|---|---|---|---|---|
| HH size | 1.89 | 1.93 | 1.89 | 1.84*** | 1.88 | 0.241 |
| | (0.97) | (1.00) | (0.91) | (0.94) | (1.02) | |
| Num. secondary children | 1.27 | 1.27 | 1.29 | 1.26 | 1.28 | 0.597 |
| | (0.50) | (0.47) | (0.49) | (0.47) | (0.60) | |
| Has cable/satellite TV | 0.67 | 0.67 | 0.67 | 0.66 | 0.66 | 0.612 |
| | (0.47) | (0.47) | (0.47) | (0.47) | (0.47) | |
| Mother present | 0.51 | 0.51 | 0.49 | 0.51 | 0.52 | 0.851 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Father present | 0.49 | 0.48 | 0.51 | 0.48 | 0.47 | 0.855 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Mother primary | 0.34 | 0.36 | 0.32 | 0.33 | 0.32 | 0.416 |
| | (0.47) | (0.48) | (0.47) | (0.47) | (0.47) | |
| Mother secondary | 0.19 | 0.19 | 0.19 | 0.20 | 0.19 | 0.588 |
| | (0.39) | (0.39) | (0.39) | (0.40) | (0.39) | |
| Mother post-secondary | 0.21 | 0.21 | 0.22 | 0.21 | 0.21 | 0.912 |
| | (0.41) | (0.41) | (0.41) | (0.41) | (0.41) | |
| Father primary | 0.24 | 0.24 | 0.24 | 0.25 | 0.22 | 0.981 |
| | (0.43) | (0.43) | (0.43) | (0.43) | (0.42) | |
| Father secondary | 0.17 | 0.17 | 0.15 | 0.19 | 0.17 | 0.060* |
| | (0.38) | (0.37) | (0.36) | (0.39) | (0.38) | |
| Father post-secondary | 0.28 | 0.27 | 0.31 | 0.26 | 0.27 | 0.664 |
| | (0.45) | (0.45) | (0.46) | (0.44) | (0.44) | |
| Mother income | 5100 | 4650 | 5812 | 5059 | 3545 | 0.045** |
| | (25007) | (24178) | (26786) | (24173) | (21876) | |
| Father income | 50855 | 50545 | 51780 | 48752 | 50812 | 0.115 |
| | (130451) | (129612) | (132957) | (128139) | (126238) | |
| School days/week, curr. | 5.78 | 5.86 | 5.76 | 5.74 | 5.72 | 0.265 |
| | (2.19) | (2.09) | (2.24) | (2.20) | (2.26) | |
| School days/week, Apr. 20 | 5.49 | 5.50 | 5.56 | 5.45 | 5.57 | 0.852 |
| | (2.11) | (2.10) | (2.06) | (2.16) | (2.08) | |
| Has private tutor | 0.61 | 0.60 | 0.62 | 0.62 | 0.63 | 0.866 |
| | (0.49) | (0.49) | (0.49) | (0.49) | (0.48) | |
| Working for pay | 0.03 | 0.03 | 0.03 | 0.02 | 0.03 | 0.980 |
| | (0.16) | (0.17) | (0.17) | (0.15) | (0.16) | |
| Number of students | 3881 | 1161 | 728 | 1170 | 492 | |
| Number of households | 3375 | 1009 | 628 | 1024 | 433 | |
| Joint test, p-val | | | 0.642 | 0.197 | 0.673 | |

**Notes**: Sample includes all randomized baseline respondents at the child level. Stars in columns 3–5 indicate statistically significant differences relative to the control group (column 2). Column 6 reports p-values based on F-tests of the joint significance of all eight treatment indicators, excluding respondents with missing values. P-values in the bottom row are from seemingly unrelated regressions that predict treatment assignment relative to the control group, with missing variable flags included. Standard errors are clustered at the household level. Stratification-cell fixed effects are included in all regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent significance levels.

Table A.5: Balance tests by pooled treatment assignment, learning assessment respondents only

| | (1) All | (2) Control | (3) App info | (4) Data + App info. | (5) Teacher | (6) Joint tests, all, p-val |
|---|---|---|---|---|---|---|
| HH size | 1.79 | 1.83 | 1.80 | 1.73*** | 1.77 | 0.101 |
| | (0.91) | (0.95) | (0.89) | (0.87) | (0.90) | |
| Num. secondary children | 1.15 | 1.15 | 1.17 | 1.14 | 1.14 | 0.465 |
| | (0.38) | (0.37) | (0.39) | (0.36) | (0.40) | |
| Has cable/satellite TV | 0.67 | 0.67 | 0.68 | 0.66 | 0.66 | 0.529 |
| | (0.47) | (0.47) | (0.47) | (0.47) | (0.47) | |
| Mother present | 0.51 | 0.51 | 0.49 | 0.50 | 0.53 | 0.970 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Father present | 0.49 | 0.48 | 0.50 | 0.49 | 0.47 | 0.965 |
| | (0.50) | (0.50) | (0.50) | (0.50) | (0.50) | |
| Mother primary | 0.34 | 0.36 | 0.32 | 0.33 | 0.32 | 0.487 |
| | (0.47) | (0.48) | (0.47) | (0.47) | (0.47) | |
| Mother secondary | 0.19 | 0.19 | 0.19 | 0.20 | 0.18 | 0.670 |
| | (0.39) | (0.39) | (0.39) | (0.40) | (0.38) | |
| Mother post-secondary | 0.21 | 0.21 | 0.22 | 0.21 | 0.21 | 0.869 |
| | (0.41) | (0.41) | (0.41) | (0.41) | (0.41) | |
| Father primary | 0.24 | 0.24 | 0.24 | 0.24 | 0.23 | 0.970 |
| | (0.43) | (0.43) | (0.42) | (0.43) | (0.42) | |
| Father secondary | 0.17 | 0.16 | 0.15 | 0.19* | 0.18 | 0.080* |
| | (0.38) | (0.37) | (0.36) | (0.39) | (0.38) | |
| Father post-secondary | 0.28 | 0.27 | 0.32 | 0.27 | 0.25 | 0.319 |
| | (0.45) | (0.45) | (0.46) | (0.44) | (0.43) | |
| Mother income | 4934 | 4515 | 5070 | 5335 | 3021 | 0.060* |
| | (24460) | (23778) | (24156) | (25386) | (19522) | |
| Father income | 49471 | 49286 | 47700 | 49500 | 48891 | 0.308 |
| | (128879) | (128434) | (124462) | (130278) | (126192) | |
| School days/week, curr. | 5.80 | 5.91 | 5.74* | 5.77 | 5.71 | 0.097* |
| | (2.16) | (2.02) | (2.26) | (2.18) | (2.26) | |
| School days/week, Apr. 20 | 5.51 | 5.51 | 5.60 | 5.45 | 5.55 | 0.906 |
| | (2.10) | (2.10) | (2.06) | (2.14) | (2.08) | |
| Has private tutor | 0.62 | 0.61 | 0.63 | 0.62 | 0.62 | 0.934 |
| | (0.49) | (0.49) | (0.48) | (0.49) | (0.48) | |
| Working for pay | 0.03 | 0.03 | 0.03 | 0.02 | 0.03 | 0.936 |
| | (0.17) | (0.17) | (0.17) | (0.15) | (0.18) | |
| Number of students | 3434 | 1031 | 638 | 1039 | 442 | |
| Number of households | 3218 | 970 | 597 | 976 | 418 | |
| Joint test, p-val | | | 0.423 | 0.084* | 0.456 | |

**Notes**: Sample includes all randomized baseline respondents at the child level. Stars in columns 3–5 indicate statistically significant differences relative to the control group (column 2). Column 6 reports p-values based on F-tests of the joint significance of all eight treatment indicators, excluding respondents with missing values. P-values in the bottom row are from seemingly unrelated regressions that predict treatment assignment relative to the control group, with missing variable flags included. Standard errors are clustered at the household level. Stratification-cell fixed effects are included in all regressions. ***, **, and * indicate significance at the 1, 5, and 10 percent significance levels.

Table A.6: Impact of outreach on learning resources

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A. Tech-dependent learning resources** | | | | | | |
| | Sangsad TV | Video lessons | App platform | Teacher remotely | Remote classes | Index |
| General info. only | 0.034* | 0.016 | -0.001 | 0.004 | 0.003 | 0.034 |
| | (0.020) | (0.022) | (0.010) | (0.016) | (0.013) | (0.026) |
| App info. | -0.048*** | -0.027 | -0.007 | -0.008 | -0.012 | -0.051** |
| | (0.017) | (0.020) | (0.009) | (0.014) | (0.012) | (0.024) |
| General + App info. | 0.052** | 0.019 | 0.000 | 0.013 | 0.035** | 0.055* |
| | (0.021) | (0.023) | (0.010) | (0.016) | (0.015) | (0.029) |
| Data + General info. | -0.002 | 0.102*** | -0.012 | -0.019 | -0.018 | 0.006 |
| | (0.030) | (0.036) | (0.015) | (0.024) | (0.020) | (0.041) |
| Data + App info. | 0.016 | 0.002 | 0.025** | 0.004 | 0.026* | 0.045 |
| | (0.019) | (0.022) | (0.012) | (0.016) | (0.014) | (0.029) |
| Data + General info. + App info. | -0.000 | -0.075 | -0.008 | 0.013 | -0.020 | -0.035 |
| | (0.042) | (0.049) | (0.022) | (0.034) | (0.029) | (0.059) |
| Teacher support | -0.007 | 0.006 | -0.012 | 0.007 | 0.006 | 0.007 |
| | (0.022) | (0.023) | (0.011) | (0.017) | (0.015) | (0.030) |
| Teacher support + Data | -0.029 | -0.123** | 0.025 | 0.014 | 0.007 | -0.062 |
| | (0.050) | (0.060) | (0.026) | (0.042) | (0.033) | (0.067) |
| DV mean, control | 0.20 | 0.25 | 0.05 | 0.12 | 0.08 | -0.00 |
| Observations | 5715 | 5715 | 5715 | 5715 | 5715 | 5715 |
| **Panel B. Non tech-dependent learning resources** | | | | | | |
| | Textbooks | Exercise books | Teacher in-person | Index | | |
| General info. only | -0.002 | 0.011 | 0.007 | -0.012 | | |
| | (0.011) | (0.020) | (0.022) | (0.024) | | |
| App info. | -0.011 | 0.004 | 0.009 | -0.013 | | |
| | (0.011) | (0.019) | (0.021) | (0.024) | | |
| General + App info. | 0.014 | -0.033 | -0.044* | -0.021 | | |
| | (0.013) | (0.022) | (0.024) | (0.028) | | |
| Data + General info. | -0.006 | 0.049 | 0.008 | 0.037 | | |
| | (0.019) | (0.030) | (0.033) | (0.044) | | |
| Data + App info. | 0.014 | 0.010 | 0.012 | 0.020 | | |
| | (0.013) | (0.021) | (0.024) | (0.027) | | |
| Data + General info. + App info. | -0.006 | -0.032 | 0.007 | -0.036 | | |
| | (0.026) | (0.043) | (0.047) | (0.059) | | |
| Teacher support | -0.030** | -0.016 | -0.075*** | -0.102*** | | |
| | (0.015) | (0.022) | (0.025) | (0.028) | | |
| Teacher support + Data | 0.037 | -0.031 | 0.112** | 0.060 | | |
| | (0.029) | (0.049) | (0.056) | (0.062) | | |
| DV mean, control | 0.94 | 0.32 | 0.62 | -0.00 | | |
| Observations | 5715 | 5715 | 5715 | 5715 | | |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: INTENSIVE-MARGIN IMPACT OF OUTREACH ON LEARNING RESOURCES

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A. Tech-dependent learning resources** | | | | | | |
| | Sangsad TV | Video lessons | App platform | Teacher remotely | Remote classes | Index |
| General info. only | 2.335 | -9.701 | 0.836 | -6.758 | -6.925 | -0.006 |
| | (6.513) | (10.194) | (3.272) | (4.687) | (5.955) | (0.026) |
| App info. | -10.498** | -24.027*** | -2.804 | -5.420 | -5.467 | -0.039 |
| | (5.354) | (8.954) | (2.861) | (4.796) | (5.949) | (0.025) |
| General + App info. | 12.891 | 2.210 | -1.249 | 4.402 | 3.055 | 0.013 |
| | (9.506) | (9.109) | (2.471) | (4.918) | (6.244) | (0.027) |
| Data + General info. | 12.373 | 23.110 | -2.428 | -6.011 | -8.155 | 0.010 |
| | (12.375) | (16.802) | (3.666) | (7.631) | (9.766) | (0.046) |
| Data + App info. | 0.638 | 13.927 | 7.135** | 7.467 | -0.540 | 0.022 |
| | (5.758) | (10.516) | (3.279) | (6.166) | (5.792) | (0.027) |
| Data + General info. + App info. | -23.036 | -11.515 | 1.605 | 10.678 | 5.108 | 0.011 |
| | (17.353) | (22.665) | (5.649) | (12.603) | (12.838) | (0.061) |
| Teacher support | -6.148 | -8.121 | -2.470 | -1.958 | 2.441 | -0.013 |
| | (5.482) | (11.214) | (2.434) | (5.533) | (7.712) | (0.024) |
| Teacher support + Data | -27.919* | -32.332 | 6.703 | 8.541 | 4.479 | -0.055 |
| | (15.813) | (28.037) | (7.579) | (12.702) | (15.188) | (0.062) |
| DV mean, control | 36.70 | 74.73 | 6.84 | 18.62 | 25.78 | 0.00 |
| Observations | 5409 | 5321 | 5628 | 5507 | 5621 | 5715 |
| **Panel B. Non tech-dependent learning resources** | | | | | | |
| | Textbooks | Exercise books | Teacher in-person | Index | | |
| General info. only | -0.891 | -3.458 | 18.127 | 0.002 | | |
| | (39.671) | (14.088) | (22.876) | (0.025) | | |
| App info. | 14.196 | -13.187 | 22.819 | 0.021 | | |
| | (37.339) | (12.430) | (20.161) | (0.034) | | |
| General + App info. | -39.280 | -4.319 | -57.000** | -0.066* | | |
| | (41.695) | (13.568) | (22.197) | (0.037) | | |
| Data + General info. | 68.355 | 26.370 | 40.630 | 0.158* | | |
| | (63.724) | (22.990) | (37.503) | (0.095) | | |
| Data + App info. | 5.001 | 10.696 | -15.617 | -0.017 | | |
| | (41.235) | (14.042) | (21.970) | (0.038) | | |
| Data + General info. + App info. | -15.203 | -31.346 | -11.761 | -0.118 | | |
| | (87.285) | (31.032) | (49.343) | (0.103) | | |
| Teacher support | 2.107 | 1.489 | -23.345 | -0.021 | | |
| | (41.227) | (17.581) | (21.859) | (0.026) | | |
| Teacher support + Data | -202.544** | -30.867 | 70.014 | -0.138 | | |
| | (96.803) | (34.050) | (59.369) | (0.100) | | |
| DV mean, control | 996.71 | 117.18 | 284.49 | 0.01 | | |
| Observations | 5226 | 5142 | 5312 | 5715 | | |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Impact of outreach on phone and data use

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A. Main Effects** | | | | |
| | Smartphone use | Pre-paid data use | Pre-paid GB used | Spent on phone/internet (taka) |
| General info. | 0.009 | 0.004 | 1.617 | 0.615 |
| | (0.024) | (0.021) | (2.000) | (13.898) |
| App info. | -0.060*** | -0.039** | 0.081 | -31.178*** |
| | (0.021) | (0.018) | (0.905) | (11.194) |
| General + App info. | 0.052 | 0.018 | -2.014 | 17.107 |
| | (0.035) | (0.030) | (2.224) | (19.136) |
| Data + General info. | 0.096** | 0.047 | 1.186 | 7.572 |
| | (0.041) | (0.036) | (3.148) | (21.616) |
| Data + App info. | 0.033 | -0.001 | 1.174 | 16.769 |
| | (0.025) | (0.021) | (1.344) | (12.213) |
| Data + General info. + App info. | -0.119** | -0.014 | -1.308 | -10.420 |
| | (0.055) | (0.047) | (3.350) | (28.878) |
| Teacher support | 0.001 | 0.013 | -1.480 | 3.587 |
| | (0.030) | (0.026) | (2.079) | (17.251) |
| Teacher support + Data | -0.057 | -0.023 | -1.022 | -22.419 |
| | (0.066) | (0.057) | (3.496) | (35.112) |
| DV mean, control | 0.34 | 0.20 | 2.03 | 138.56 |
| Observations | 5715 | 5715 | 5321 | 5065 |
| **Panel B. Persistence Effects** | | | | |
| | Smartphone use | Pre-paid data use | Pre-paid GB used | |
| General info. | -0.029 | -0.035 | -1.276 | |
| | (0.038) | (0.032) | (0.869) | |
| App info. | -0.024 | -0.019 | -0.886 | |
| | (0.026) | (0.022) | (0.833) | |
| General + App info. | 0.025 | 0.009 | 2.011 | |
| | (0.050) | (0.042) | (1.345) | |
| Data + General info. | 0.060 | 0.044 | 1.074 | |
| | (0.050) | (0.044) | (0.822) | |
| Data + App info. | -0.003 | -0.009 | 0.894 | |
| | (0.028) | (0.024) | (0.823) | |
| Data + General info. + App info. | -0.036 | 0.026 | -0.641 | |
| | (0.064) | (0.057) | (1.570) | |
| Teacher support | -0.005 | -0.002 | 1.185 | |
| | (0.042) | (0.035) | (1.014) | |
| Teacher support + Data | -0.061 | -0.027 | -1.667 | |
| | (0.072) | (0.061) | (1.399) | |
| DV mean, control | 0.29 | 0.19 | 2.35 | |
| Observations | 4326 | 4326 | 4039 | |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Persistence of the impact of outreach on learning resources

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Panel A. Tech-dependent learning resources** | | | | | |
| | Sangsad TV | Video lessons | App platform | Teacher remotely | Remote classes | Index |
| General info. only | 0.012 | -0.034 | -0.001 | -0.016 | -0.026 | -0.025 |
| | (0.029) | (0.033) | (0.019) | (0.025) | (0.021) | (0.052) |
| App info. | -0.023 | -0.019 | 0.007 | -0.008 | 0.003 | -0.010 |
| | (0.019) | (0.023) | (0.013) | (0.017) | (0.016) | (0.031) |
| General + App info. | 0.023 | -0.015 | -0.010 | 0.011 | 0.016 | 0.006 |
| | (0.023) | (0.028) | (0.016) | (0.022) | (0.020) | (0.039) |
| Data + General info. | 0.038 | 0.026 | 0.012 | 0.031 | 0.006 | 0.058 |
| | (0.032) | (0.034) | (0.018) | (0.028) | (0.022) | (0.046) |
| Data + App info. | 0.012 | 0.004 | -0.015 | 0.005 | -0.001 | -0.011 |
| | (0.020) | (0.025) | (0.014) | (0.020) | (0.017) | (0.032) |
| Data + General info. + App info. | -0.020 | 0.005 | 0.007 | -0.038 | -0.027 | -0.011 |
| | (0.044) | (0.050) | (0.027) | (0.040) | (0.032) | (0.067) |
| Teacher support | 0.017 | -0.036 | -0.003 | -0.024 | -0.001 | -0.018 |
| | (0.021) | (0.023) | (0.012) | (0.018) | (0.015) | (0.032) |
| Teacher support + Data | -0.085* | -0.015 | -0.036 | -0.011 | -0.013 | -0.121* |
| | (0.047) | (0.056) | (0.024) | (0.043) | (0.034) | (0.070) |
| DV mean, control | 0.13 | 0.22 | 0.05 | 0.12 | 0.08 | -0.00 |
| Observations | 4326 | 4326 | 4326 | 4326 | 4326 | 4326 |
| Joint test: Information (p-val) | 0.568 | 0.418 | 0.936 | 0.876 | 0.276 | 0.963 |
| Joint test: Data (p-val) | 0.240 | 0.644 | 0.432 | 0.800 | 0.794 | 0.388 |
| Joint test: Teacher (p-val) | 0.191 | 0.172 | 0.182 | 0.272 | 0.894 | 0.072 |
| | **Panel B. Non tech-dependent learning resources** | | | | | |
| | Textbooks | Exercise books | Teacher in-person | Index (4) | | |
| General info. only | 0.009 | -0.009 | -0.011 | 0.014 | | |
| | (0.017) | (0.031) | (0.040) | (0.052) | | |
| App info. | -0.005 | -0.029 | -0.013 | -0.031 | | |
| | (0.013) | (0.023) | (0.028) | (0.031) | | |
| General + App info. | -0.009 | 0.032 | -0.022 | -0.022 | | |
| | (0.018) | (0.029) | (0.034) | (0.037) | | |
| Data + General info. | 0.001 | 0.030 | -0.009 | -0.004 | | |
| | (0.019) | (0.035) | (0.038) | (0.039) | | |
| Data + App info. | 0.009 | 0.060** | 0.032 | 0.062* | | |
| | (0.015) | (0.026) | (0.031) | (0.034) | | |
| Data + General info. + App info. | 0.005 | -0.052 | 0.021 | 0.051 | | |
| | (0.029) | (0.050) | (0.058) | (0.063) | | |
| Teacher support | 0.006 | 0.010 | -0.025 | -0.020 | | |
| | (0.012) | (0.024) | (0.028) | (0.029) | | |
| Teacher support + Data | -0.012 | -0.027 | 0.050 | 0.047 | | |
| | (0.030) | (0.053) | (0.063) | (0.068) | | |
| DV mean, control | 0.95 | 0.41 | 0.48 | 0.00 | | |
| Observations | 4326 | 4326 | 4326 | 4326 | | |
| Joint test: Information (p-val) | 0.692 | 0.631 | 0.642 | 0.269 | | |
| Joint test: Data (p-val) | 0.842 | 0.087 | 0.447 | 0.015 | | |
| Joint test: Teacher (p-val) | 0.861 | 0.856 | 0.608 | 0.722 | | |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey week fixed effects. Individual controls are those listed in Table 2, the interaction between the general and adaptive information treatments $AppInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * AppInfo_h$, the interaction between teacher, data, and general information treatment $Teacher_h * Data_h * GenInfo_h$, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Persistence of the impact of outreach on parental investment

| | (1) | (2) |
|---|---|---|
| **Panel A. Time investment** | | |
| | Days reminded student | Hours parent helped |
| General info. only | 0.242 | -0.133 |
| | (0.230) | (0.513) |
| App info. | 0.101 | -0.103 |
| | (0.167) | (0.328) |
| General + App info. | -0.166 | 0.186 |
| | (0.209) | (0.410) |
| Data + General info. | -0.142 | 0.091 |
| | (0.219) | (0.520) |
| Data + App info. | -0.020 | -0.124 |
| | (0.184) | (0.359) |
| Data + General info. + App info. | 0.243 | -0.234 |
| | (0.346) | (0.733) |
| Teacher support | 0.032 | 0.370 |
| | (0.167) | (0.358) |
| Teacher support + Data | 0.266 | -0.589 |
| | (0.359) | (0.784) |
| DV mean, control | 4.13 | 4.56 |
| Observations | 4236 | 4185 |
| Joint test: Information (p-val) | 0.629 | 0.966 |
| Joint test: Data (p-val) | 0.939 | 0.849 |
| Joint test: Teacher (p-val) | 0.635 | 0.556 |
| **Panel B. Economic investment** | | |
| | Private tutoring | Money on tutoring |
| General info. only | -0.008 | -233.401** |
| | (0.039) | (94.449) |
| App info. | 0.002 | -13.237 |
| | (0.029) | (92.434) |
| General + App info. | -0.020 | -26.262 |
| | (0.036) | (113.400) |
| Data + General info. | 0.059 | 109.488 |
| | (0.039) | (120.667) |
| Data + App info. | 0.033 | 92.810 |
| | (0.032) | (102.542) |
| Data + General info. + App info. | -0.076 | -44.036 |
| | (0.060) | (188.803) |
| Teacher support | -0.042 | 11.117 |
| | (0.029) | (90.160) |
| Teacher support + Data | 0.015 | -44.225 |
| | (0.066) | (191.201) |
| DV mean, control | 0.48 | 743.05 |
| Observations | 4299 | 4256 |
| Joint test: Information (p-val) | 0.926 | 0.097 |
| Joint test: Data (p-val) | 0.239 | 0.342 |
| Joint test: Teacher (p-val) | 0.309 | 0.973 |

**Notes:** Sample includes all Round 2 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, the interaction between the general and adaptive information treatments $AppInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * AppInfo_h$, the interaction between teacher, data, and general information treatment $Teacher_h * Data_h * GenInfo_h$, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Impact of outreach on student engagement and motivation, endline

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Student engagement index | Hope post-secondary | Attending in-person classes | Index (7) |
| General info. | 0.121 | -0.014 | -0.005 | 0.009 |
| | (0.088) | (0.028) | (0.007) | (0.049) |
| App info. | 0.063 | -0.019 | 0.008 | 0.028 |
| | (0.057) | (0.021) | (0.007) | (0.036) |
| General + App info. | -0.243** | 0.021 | -0.001 | -0.065 |
| | (0.113) | (0.038) | (0.011) | (0.068) |
| Data + General info. | -0.134 | -0.005 | 0.008 | -0.017 |
| | (0.116) | (0.039) | (0.012) | (0.070) |
| Data + App info. | -0.071 | 0.004 | -0.010 | -0.054 |
| | (0.063) | (0.023) | (0.008) | (0.039) |
| Data + General info. + App info. | 0.268* | 0.003 | 0.001 | 0.092 |
| | (0.146) | (0.051) | (0.016) | (0.090) |
| Teacher support | -0.129 | 0.007 | 0.001 | -0.030 |
| | (0.096) | (0.032) | (0.008) | (0.056) |
| Teacher support + Data | 0.280* | -0.013 | -0.016 | 0.011 |
| | (0.154) | (0.056) | (0.014) | (0.091) |
| DV mean, control | -0.01 | 0.89 | 0.01 | -0.00 |
| Observations | 3397 | 3297 | 3442 | 3442 |

**Notes:** Sample includes all Round 2 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. Anderson q-values for joint hypothesis tests not reported because all equal 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Impact of outreach on student time investment, midline

| | (1) | (2) |
|---|---|---|
| | Days/week schoolwork | Hrs/week schoolwork |
| General info. | 0.062 | -0.463 |
| | (0.093) | (0.706) |
| App info. | 0.031 | -0.138 |
| | (0.089) | (0.670) |
| General + App info. | -0.199 | -0.283 |
| | (0.137) | (1.027) |
| Data + General info. | 0.009 | 0.880 |
| | (0.141) | (1.153) |
| Data + App info. | -0.083 | -0.093 |
| | (0.105) | (0.754) |
| Data + General info. + App info. | 0.204 | -0.252 |
| | (0.199) | (1.565) |
| Teacher support | -0.010 | 0.139 |
| | (0.116) | (0.889) |
| Teacher support + Data | 0.069 | -3.672** |
| | (0.214) | (1.797) |
| DV mean, control | 5.65 | 19.03 |
| Observations | 5619 | 5168 |

**Notes:** Sample includes all midline" survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. Anderson q-values for joint hypothesis tests not reported because all equal 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Impact of outreach on student time investment, endline

| | (1) | (2) |
|---|---|---|
| | Days/week schoolwork | Hrs/week schoolwork |
| General info. | 0.024 | -1.433 |
| | (0.181) | (0.956) |
| App info. | -0.059 | -0.670 |
| | (0.125) | (0.662) |
| General + App info. | -0.030 | 1.657 |
| | (0.239) | (1.247) |
| Data + General info. | -0.064 | 1.106 |
| | (0.259) | (1.273) |
| Data + App info. | 0.022 | 0.423 |
| | (0.139) | (0.728) |
| Data + General info. + App info. | 0.098 | -1.533 |
| | (0.327) | (1.647) |
| Teacher support | -0.013 | 1.159 |
| | (0.199) | (1.037) |
| Teacher support + Data | -0.170 | -1.340 |
| | (0.346) | (1.773) |
| DV mean, control | 5.46 | 15.35 |
| Observations | 4245 | 4194 |

**Notes:** Sample includes all endline" survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. Anderson q-values for joint hypothesis tests not reported because all equal 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Impact of outreach on student learning (math), endline

| | (1) | (2) | (3) |
|---|---|---|---|
| | Unadjusted score | IRT, 1pl | IRT, 2pl |
| General info. | -0.188** | -0.121 | -0.095 |
| | (0.087) | (0.087) | (0.085) |
| App info. | 0.105* | 0.121** | 0.107* |
| | (0.060) | (0.057) | (0.057) |
| General + App info. | 0.079 | 0.000 | 0.008 |
| | (0.117) | (0.115) | (0.113) |
| Data + General info. | 0.145 | 0.121 | 0.113 |
| | (0.119) | (0.117) | (0.115) |
| Data + App info. | -0.098 | -0.107* | -0.115* |
| | (0.066) | (0.064) | (0.063) |
| Data + General info. + App info. | 0.014 | 0.024 | 0.022 |
| | (0.153) | (0.151) | (0.148) |
| Teacher support | 0.211** | 0.112 | 0.076 |
| | (0.097) | (0.095) | (0.094) |
| Teacher support + Data | -0.269* | -0.286* | -0.222 |
| | (0.161) | (0.157) | (0.154) |
| DV mean, control | 0.01 | -0.00 | 0.00 |
| Observations | 3433 | 3433 | 3433 |

**Notes:** Sample includes all Round 2 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table A.15: Impact of outreach on parental investment

| | Panel A. Time investment | | Panel B. Economic investment | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Days reminded student | Hours parent helped | Private tutoring | Money on tutoring | Money on other education | | |
| General info. only | 0.080 | -0.293 | -0.003 | -65.836 | 0.615 | | |
| | (0.146) | (0.375) | (0.024) | (80.130) | (13.898) | | |
| App info. | 0.066 | -0.646** | 0.045** | 197.309** | -31.178*** | | |
| | (0.136) | (0.328) | (0.021) | (81.368) | (11.194) | | |
| General + App info. | -0.028 | 0.704* | -0.069*** | -154.392* | 17.723 | | |
| | (0.152) | (0.396) | (0.025) | (90.686) | (13.008) | | |
| Data + General info. | 0.066 | 0.317 | 0.090*** | 73.805 | 8.187 | | |
| | (0.219) | (0.616) | (0.034) | (112.631) | (19.431) | | |
| Data + App info. | -0.022 | 0.300 | 0.005 | -130.587 | 16.769 | | |
| | (0.155) | (0.375) | (0.024) | (91.513) | (12.213) | | |
| Data + General info. + App info. | -0.033 | -0.217 | -0.069 | 0.475 | -11.035 | | |
| | (0.310) | (0.841) | (0.049) | (166.814) | (27.266) | | |
| Teacher support | 0.193 | 0.227 | -0.052** | 27.459 | 4.202 | | |
| | (0.156) | (0.393) | (0.025) | (93.884) | (14.645) | | |
| Teacher support + Data | -0.059 | -1.818* | 0.001 | 161.329 | -23.035 | | |
| | (0.368) | (1.008) | (0.059) | (204.667) | (33.796) | | |
| DV mean, control | 4.40 | 6.57 | 0.64 | 1027.82 | 138.56 | | |
| Observations | 5600 | 5359 | 5688 | 5359 | 5065 | | |
| Joint test: Information (p-val) | 0.939 | 0.187 | 0.040 | 0.036 | 0.028 | | |
| Joint test: Data (p-val) | 0.998 | 0.257 | 0.023 | 0.307 | 0.510 | | |
| Joint test: Teacher (p-val) | 0.429 | 0.194 | 0.075 | 0.561 | 0.793 | | |
| **Panel C. Secondary time investment outcomes: Activities parents helped students** | | | | | | | |
| | Explain concepts | Help with assignments | Watch videos/TV | Find resources | Encourage student | Supervise student | Activities index |
| General info. only | 0.001 | 0.007 | 0.004 | -0.018 | -0.039 | -0.009 | -0.017 |
| | (0.023) | (0.020) | (0.017) | (0.022) | (0.024) | (0.025) | (0.035) |
| App info. | 0.023 | 0.011 | -0.034** | 0.003 | -0.031 | -0.031 | -0.024 |
| | (0.021) | (0.018) | (0.015) | (0.020) | (0.022) | (0.022) | (0.031) |
| General + App info. | -0.025 | -0.015 | 0.013 | -0.020 | 0.011 | 0.036 | -0.000 |
| | (0.025) | (0.020) | (0.017) | (0.023) | (0.026) | (0.027) | (0.036) |
| Data + General info. | -0.066** | -0.031 | -0.033 | -0.052* | -0.053 | -0.022 | -0.099** |
| | (0.032) | (0.029) | (0.024) | (0.031) | (0.035) | (0.035) | (0.046) |
| Data + App info. | -0.041* | -0.024 | 0.014 | -0.001 | -0.018 | 0.033 | -0.014 |
| | (0.024) | (0.020) | (0.017) | (0.023) | (0.025) | (0.026) | (0.036) |
| Data + General info. + App info. | 0.133*** | 0.070* | 0.050 | 0.074 | 0.101** | 0.018 | 0.173** |
| | (0.047) | (0.041) | (0.035) | (0.045) | (0.050) | (0.051) | (0.069) |
| Teacher support | 0.001 | -0.006 | 0.014 | -0.008 | -0.019 | 0.023 | 0.004 |
| | (0.024) | (0.019) | (0.018) | (0.022) | (0.026) | (0.025) | (0.034) |
| Teacher support + Data | -0.034 | -0.021 | -0.070* | 0.037 | -0.002 | -0.071 | -0.070 |
| | (0.056) | (0.046) | (0.036) | (0.054) | (0.060) | (0.062) | (0.081) |
| DV mean, control | 0.27 | 0.17 | 0.12 | 0.33 | 0.58 | 0.63 | -0.00 |
| Observations | 4908 | 4908 | 4908 | 4908 | 4908 | 4908 | 4908 |

**Notes:** Sample includes all Round 1 survey respondents. All regressions include stratification-cell fixed effects, enumerator fixed effects, and survey-week fixed effects. Individual controls are those listed in Table 2, plus flags for missing values. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Impact of outreach on learning resources, lasso

| | (1) | (2) | (3) |
|---|---|---|---|
| | App platform | Tech index | Non-tech index |
| General info. | -0.002 | 0.066*** | -0.001 |
| | (0.010) | (0.025) | (0.026) |
| App info. | -0.011 | -0.036 | 0.001 |
| | (0.008) | (0.022) | (0.025) |
| | [0.288] | [0.125] | [0.604] |
| Data + App info. | 0.023** | 0.042 | -0.002 |
| | (0.010) | (0.027) | (0.028) |
| | [0.073*] | [0.125] | [0.604] |
| Teacher support | -0.015 | -0.052* | -0.093*** |
| | (0.010) | (0.030) | (0.033) |
| | [0.390] | [0.265] | [0.016**] |
| DV mean, control | 0.05 | -0.00 | -0.00 |
| Observations | 5715 | 5715 | 5715 |

**Notes:** Sample includes all Round 1 survey respondents. Includes covariates chosen from among all baseline variables and stratification cell fixed effects, along with their pair-wise interactions, using a lasso procedure with a penalty parameter that minimizes the 10-fold cross-validated mean squared error. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Impact of outreach on parental investment, lasso

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Hours parent helped | Private tutoring | Money on tutoring | Money on other education |
| General info. | 0.075 | -0.014 | -48.987 | 8.055 |
| | (0.385) | (0.022) | (68.405) | (13.017) |
| App info. | -0.505 | 0.022 | 161.172** | -24.381** |
| | (0.335) | (0.020) | (66.624) | (10.193) |
| | [0.199] | [0.199] | [0.098] | [0.082] |
| Data + App info. | 0.331 | 0.008 | -92.694 | 19.474* |
| | (0.382) | (0.023) | (76.273) | (11.013) |
| | [0.294] | [0.826] | [0.294] | [0.199] |
| Teacher support | -0.132 | -0.027 | 8.540 | 2.255 |
| | (0.471) | (0.027) | (88.028) | (16.039) |
| | [0.831] | [0.199] | [0.638] | [0.830] |
| DV mean, control | 6.57 | 0.64 | 1027.82 | 138.56 |
| Observations | 5359 | 5688 | 5359 | 5065 |

**Notes:** Sample includes all Round 1 survey respondents. Includes covariates chosen from among all baseline variables and stratification cell fixed effects, along with their pair-wise interactions, using a lasso procedure with a penalty parameter that minimizes the 10-fold cross-validated mean squared error. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Impact of outreach on student learning (math), endline, lasso

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Unadjusted score | IRT, 2pl | Unadjusted score, easy | Unadjusted score, hard |
| General info. | -0.211** | -0.105 | -0.153* | -0.075 |
| | (0.083) | (0.085) | (0.089) | (0.082) |
| App info. | 0.129** | 0.138** | 0.105* | 0.136** |
| | (0.056) | (0.056) | (0.056) | (0.061) |
| | [0.048**] | [0.048**] | [0.048**] | [0.048] |
| Data + App info. | -0.123** | -0.108* | -0.111* | -0.090 |
| | (0.061) | (0.063) | (0.063) | (0.069) |
| | [0.057*] | [0.048**] | [0.048**] | [0.098*] |
| Teacher support | 0.185** | 0.095 | 0.133 | 0.085 |
| | (0.092) | (0.094) | (0.099) | (0.092) |
| | [0.048**] | [0.270] | [0.103] | [0.161] |
| DV mean, control | 0.01 | 0.00 | 0.00 | -0.00 |
| Observations | 3434 | 3434 | 3434 | 3430 |

**Notes:** Sample includes all Round 2 survey respondents. Columns 3 and 4 include share of easy and hard questions answered correctly, respectively, for which items are divided at the median difficulty based on a 2-parameter logistic model. Includes covariates chosen from among all baseline variables and stratification cell fixed effects, along with their pair-wise interactions, using a lasso procedure with a penalty parameter that minimizes the 10-fold cross-validated mean squared error. Robust standard errors are shown in parentheses and clustered at the household level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# B For Online Publication: Conceptual framework

This section presents a stylized model of the effects of decreasing specific barriers to education when parental educational investments are key inputs. It highlights that a policy targeting a single, specific barrier to education may affect other parts of the decision-making problem through changes in the adoption of different learning options and in household time and economic investments, with unclear net impacts on human capital development.

This framework draws upon the literature related to household production and time allocation theory (Becker, 1965; Becker and Tomes, 1976), and it is similar to the one outlined in Todd and Wolpin (2003). We include parental inputs as key contributors to the human capital production function, separating parental time investment (Houtenville and Conway, 2008) from parental monetary investments. We allow for heterogeneity in parental human capital and resources.[30] We first define a set-up in which human capital is provided through internal teaching—by parents themselves—or through external teaching via schools and tutors,[31] and we characterize the optimal household decision rules in this environment. We then expand this framework to include a novel form of acquiring human capital through the use of education technology that is costly and about which the perceived and actual returns may not be equal. We use this stylized framework to demonstrate the potential net and disaggregated impacts of three policies targeting human capital development: a cost reduction in external teaching costs, information about a novel technology, and reduced costs of using the technology.

Parents maximize utility derived from the child's long-term human capital, $H$, and the household's present consumption of other goods, $C$, subject to a human capital production function, a budget constraint, and a time constraint. Assume the utility function is additively separable, increasing, and concave in human capital and in other consumption goods, so that $U(H, C) = u(H) + v(C)$. Households are heterogeneous in parental human capital, $\theta$.

Households invest in human capital via parental time spent on education, $i^t$, and money for educational activities, $i^m$. Parents distribute available total household time, $T$, between labor market supply and teaching their children, ($i^t$), which could include direct instruction as well as supervising or helping them with their homework. Parents' opportunity cost of teaching is represented by their labor market wage, $w \cdot \theta$, which is increasing in parental human capital $\theta$. The household budget constraint is $C = w \cdot \theta \cdot (T - i^t) - i^m$, where the price of other consumption goods is the numeraire. Hence, higher-skilled parents receive higher incomes and have a higher opportunity cost of time investment. The effectiveness of time investment in teaching also depends on parental human capital, with $i^{t\prime}(\theta) > 0$, so that high-skilled parents are more effective in helping their children.

## B.1 Human capital investment through teaching

Human capital is determined by an education production function that relates parental investments and household wealth. All human capital production occurs through effective teaching hours $S$, which are a function of the effective units of time investment, $i^t(\theta)$ (internal teaching), and the amount of external

[30]Parental beliefs about children's ability and the returns to investment also shape investment decisions (Boneva and Rauh, 2018; Dizon-Ross, 2019; Attanasio et al., 2020; Celhay and Gallegos, 2022). However, because our experimental design does not enable us to differentiate between true and perceived ability or true and perceived returns to their own investments, we abstract from this source of heterogeneity in the model and the analysis.

[31]The relationship between parental investment and formal schooling has been the focus of most literature on household-school input trade-offs (Todd and Wolpin, 2003; Houtenville and Conway, 2008; Pop-Eleches and Urquiola, 2013; Das et al., 2013).

teaching hours, $P$, so that $S = f(i^t(\theta), P)$ and $H = g(S)$. $g(\cdot)$ and $f(\cdot)$ are increasing and concave in their arguments. The cost of outside-household teaching hours is $c^p$, and we assume there is a single external teaching price. Then, parental monetary investment is $i^m = P \cdot c^p$.[32] Note that we are effectively assuming that student effort and motivation do not interact with any of the potential teaching inputs. Households choose the level of monetary investment—through the amount of external teaching they choose— and time investment that maximize their utility, subject to the budget constraint and (expected) human capital production functions:

$$\begin{aligned}
&\underset{P,i^t}{max} \quad u(H) + v(C) && (1)\\
&\text{s.t.}\\
&H = g(S)\\
&S = f(i^t(\theta), P)\\
&C = w \cdot \theta \cdot (T - i^t) - P \cdot c^p
\end{aligned}$$

For ease of interpretation of the impacts and behavioral responses, we assume households make their investment decisions in two steps: first, they decide the amount of external teaching $P$, and then they choose their time investment, taking $i^m$ as an additional input. Then, the optimization problem yields the following household decision rules: $i^m = \varphi(c^p, \theta)$ and $i^t = \nu(i^m, \theta)$.

It is theoretically ambiguous whether households with higher-skilled parents (high $\theta$) will invest more or less time helping their children compared to lower-skilled parents, given that the opportunity cost of their time is higher and that it is less costly for high-skilled parents to generate an effective unit of time investment. Hence, the absolute number of hours invested by wealthier parents, as well as their relative weight compared to their monetary investment, will depend on this trade-off as well as whether time and monetary investments are perceived substitutes or complements in the human capital production function (i.e., the sign of $\frac{\partial i^t}{\partial i^m}$).[33]

*Reduction in cost of teaching hours.* We first consider the total effect of decreasing the cost of external teaching hours $c^p$ on human capital *not* holding other inputs constant:

$$\frac{dH}{dc^p} = \frac{\partial g(S)}{\partial S} \cdot \left[ \overbrace{\underbrace{\frac{\partial S}{\partial P} \cdot \frac{\partial P}{\partial c^p}}_{\text{Direct effect}} + \underbrace{\frac{\partial S}{\partial P} \cdot \frac{\partial P}{\partial i^m} \cdot \frac{\partial i^m}{\partial c^p}}_{\substack{\text{Behavioral response}\\ \text{monetary investment}}}}^{\substack{\text{External teaching}\\ \text{impacts}}} + \overbrace{\underbrace{\frac{\partial S}{\partial i^t} \cdot \frac{\partial i^t}{\partial i^m} \cdot \frac{\partial i^m}{\partial c^p}}_{\substack{\text{Behavioral response}\\ \text{time investment}}}}^{\substack{\text{Internal teaching}\\ \text{impacts}}} \right] \tag{2}$$

The first term is the direct effect of decreasing the costs of external teaching on human capital, which is weakly positive ($\frac{\partial g}{\partial S} \cdot \frac{\partial S}{\partial P} \cdot \frac{\partial P}{\partial c^p} \leqslant 0$ and $\partial c^p < 0$). The second and third terms capture the impacts of the parental behavioral responses on human capital through changes in monetary and time investments, and the signs of both terms are ambiguous. The overall sign of monetary investments depends on $\frac{\partial i^m}{\partial c^p} = P + \frac{\partial P}{\partial c^p} \cdot c^p$, which depends on the elasticity of demand for $P$ with respect to $c^p$, thus reflecting the net income and

[32] We assume universal access to textbooks and exercise books at home, so the outside option for human capital generation is to use them without additional guidance ("other resources"), which generates a baseline human capital of $\alpha$. Without loss of generality, we assume that these resources alone do not produce any human capital, $\alpha = 0$.

[33] Del Boca et al. (2014) discuss the trade-off between parental investments in detail.

substitution effects as a result of the cost reduction. The change in human capital due to parental time investments depends on both $\frac{\partial i^m}{\partial c^p}$ as well as $\frac{\partial i^t}{\partial i^m}$; that is, whether parental time and monetary investments are complements ($\frac{\partial^2 g}{\partial i^t \partial i^m} > 0$) or substitutes ($\frac{\partial^2 g}{\partial i^t \partial i^m} < 0$).

The existence of parental behavioral responses to policy changes and the interdependence of parental investment inputs through the household decision rules means that a policy that reduces barriers to external teaching, hence reducing the cost, may affect both the optimal amount of external teaching and the optimal amount of parental effort. Although both internal and external teaching generates learning gains, if one human capital production technology is more effective than the other, the re-optimization responses across parental investments may lead to null impacts or even an overall *decrease* in human capital.

## B.2 Human capital investment through educational technology

As section B.2 of the stylized framework highlights, providing information about the learning app could signal the value of the highlighted platform—a novel resource they may not know about—causing parents to revise upward its perceived marginal return or signal the value of similar features from other educational inputs. The information could also shift parents' beliefs related to general educational investments by increasing their salience, which could in turn trigger investment re-optimization on other inputs as well.

In recent years, educational technologies (edtech) have emerged as an alternative or complement to traditional external and internal teaching supports. They have been implemented both in formal schooling environments and as a means to provide education remotely. The latter is likely to be most important for learners from more disadvantaged backgrounds, who may lack access to an adequate learning environment and quality instruction (Lai et al., 2012; Caballero Montoya et al., 2021) or have higher rates of education disruption.

To understand the impacts of policies targeting these two different learning options—teaching and edtech—and how they interact with parental investment responses, we expand the conceptual framework to include educational technology as a second, costly, channel of human capital development. We generally specify the technology pricing schedule of the number of (effective) hours spent on the edtech platform, $E$, as $c^e = \rho(E)$. The cost $c^e$ may be non-linear in the hours of use and can reflect a purchase price, regular subscription fees, or internet data costs associated with its use. Then, economic investment in the edtech platform is $i^e = h(E, c^E)$.

Human capital can now be produced through two different channels, the teaching channel and the edtech channel, $H = g(S, E)$.[34] Parents are uncertain about the value of the edtech platform because it is new. We characterize this uncertainty by differentiating between the *actual* returns of the learning options, $g(\cdot)$, and their *perceived* returns, $\widetilde{g}(\cdot)$ (Boneva and Rauh, 2018). Given the novelty of the edtech platform, at baseline we assume that its perceived returns are very low compared to its actual returns. Formally,

$$\frac{\partial g(S,E)}{\partial E} > \frac{\partial \widetilde{g}(S,E)}{\partial E} \approx 0 \tag{3}$$

Parents now choose the optimal allocation of their time and monetary investment, with the latter being split between external teaching investment and edtech: $C = w \cdot \theta(T - i^t) - [i^m + i^e]$. It is important to note that their choice of investment inputs will result in a different combination of three human capital production

[34]The hours spent on the edtech platform $E$ directly enter the human capital production function instead of contributing to $S$ to differentiate inputs that require teaching support (private tutoring, formal schooling, parental help) with an input that students can independently use.

technologies: internal teaching, external teaching, and edtech.

$$
\begin{aligned}
&\max_{P, i^t, E} \quad u(H) + v(C) \\
&\quad \text{s.t.} \\
&\quad H = \widetilde{g}(S, E) \\
&\quad S = f(i^t(\theta), P) \\
&\quad C = w \cdot \theta \cdot (T - i^t) - P \cdot c^p - h(E, \rho(E))
\end{aligned}
\tag{4}
$$

Then, the new household decision rules are: $\widetilde{i^m} = \widetilde{\varphi}(\widetilde{i^e}, c^p, \theta)$, $\widetilde{i^e} = \widetilde{\xi}(\widetilde{i^m}, c^e, \theta)$, and $\widetilde{i^t} = \widetilde{\nu}(\widetilde{i^m}, \widetilde{i^e}, \theta)$. Note that the optimal investments $\widetilde{i^t}$, $\widetilde{i^m}$, and $\widetilde{i^e}$ are optimally chosen based on the perceived returns of the technologies $\widetilde{g}(\cdot)$, not on the actual returns.

*Information provision.* A commonly used policy to increase the usage of new educational technologies is to provide information about it.[35] Information about a new technology may cause households to adjust their perceived returns about the new technology *and* about other personalized learning options such as external teaching. The information can signal the value of this new platform, leading individuals to revise their beliefs about its marginal returns upward, hence increasing $\partial\widetilde{g}/\partial E$ toward $\partial g/\partial E$. However, the information on new technologies can more broadly shift the beliefs of the returns to personalized teaching resources as well, making them also revise (upwards or downwards) the returns to teaching, so that $\partial\widetilde{g}/\partial S \lesseqgtr \partial g/\partial S$. Based on this new signal, households may update their educational investments accordingly, investing in the combination of learning options they can afford that will give the highest perceived returns. This implies that, similar to equation (2), an informational policy aimed at decreasing the barriers to edtech may generate behavioral responses on monetary and time investments from parents and a re-optimization of the learning resource portfolio above and beyond direct changes in edtech usage.

*Reducing edtech costs.* Besides awareness about the new technology, another barrier to edtech usage can be its cost. Under the presence of economic constraints, households may not adopt the educational technology even if they perceive a high return on investment. In this case, households can only use part of the signal of the informational message about the importance of personalized learning options. Then, households may still re-optimize their investments and subsequent learning option choices *without* actually adopting the new technology. One educational policy aimed at reducing budget constraints to accessing education is to decrease the cost of edtech. The total effect of lowering $c^e$ on human capital is:

$$
\frac{dH}{dc^e} = \overbrace{\frac{\partial g(S,E)}{\partial E} \cdot \Bigg[ \underbrace{\frac{\partial E}{\partial c^e}}_{\text{Direct effect}} + \underbrace{\frac{\partial E}{\partial \widetilde{i^e}} \cdot \frac{\partial \widetilde{i^e}}{\partial c^e}}_{\substack{\text{Edtech} \\ \text{investment response}}} \Bigg]}^{\text{Edtech human capital impacts}}
\tag{5}
$$

[35] A series of papers highlight the utility of behavioral nudges to encourage continuous investment among students enrolled in MOOC courses, in which student drop-out rates are high (see Yeomans and Reich (2017); Martinez (2014); Patterson (2018); Baker et al. (2016) among others). In Uruguay, e-messages and nudges targeting different behavioral biases boosted parental investment in early childhood development (Bloomfield et al., 2022). In the United States, SMS reminders to do home literacy activities for parents increased early literacy of preschoolers (York et al., 2018) and kindergarteners only when messages were personalized (Doss et al., 2019). Similar reminders provided to parents of children of Head Start centers (Hurwitz et al., 2015) and parents of primary school children over the summer break (Kraft and Monti-Nussbaum, 2017) had also positive effects.

$$
+ \overbrace{\frac{\partial g(S,E)}{\partial S} \cdot \left[ \underbrace{\frac{\partial S}{\partial P} \cdot \frac{\partial P}{\partial \widetilde{i^m}} \cdot \frac{\partial \widetilde{i^m}}{\partial \widetilde{i^e}} \cdot \frac{\partial \widetilde{i^e}}{\partial c^e}}_{\substack{\text{Behavioral response} \\ \text{monetary investment}}} + \underbrace{\frac{\partial S}{\partial \widetilde{i^t}} \cdot \frac{\partial \widetilde{i^t}}{\partial \widetilde{i^e}} \cdot \frac{\partial \widetilde{i^e}}{\partial c^e}}_{\substack{\text{Behavioral response} \\ \text{time investment}}} \right]}^{\text{Teaching impacts}}
$$

As before, a change in policy aimed only at decreasing the barriers to educational technologies may affect the adoption of teaching options through investment re-optimization responses from the household's side. Note that although the human capital impacts will be realized through the *actual* learning technology $g(\cdot)$, households' decisions will be based on their beliefs about the learning technology, $\widetilde{g}(\cdot)$, so an informational intervention will not change the partial derivatives, but will change the optimal investments $\widetilde{i^m}$, $\widetilde{i^t}$ and $\widetilde{i^e}$ through the household decision rules.

# C For Online Publication: Additional methodological details

## C.1 Item response theory

We measure student learning based on a phone-based assessment with students conducted at endline. Partner teachers assisted in creating a bank of math and Bangla test questions aligned with the grade-specific national curriculum that could be asked orally and answered via multiple choice. Each student completed a grade-specific set of four questions per subject set at their 2020 grade level or lower. Based on their performance on these questions, they were then asked four more questions at a slightly lower or slightly higher grade levels. We repeated questions across questionnaires. For example, a math question deemed as ”grade 7” would be asked for students who were in grade 7 as their ”at grade level” questionnaire, asked to students in grade 8 as 'below one level,” asked to students in grade nine as ”below two levels” and to grade 6 as ”above one level”'

We estimate a two-parameter logistic model separately by subject.

## C.2 Distribution of answers

With the exception of grade 8 students, very few students answer all or no question correctly in math. Similarly, very few students answer all questions or no questions correctly in Bangla. Overall, 8.9% of the sample is at an endpoint in math, and 6.5% of the sample is at an endpoint in Bangla.

Table C.19: Distribution of test scores, by grade

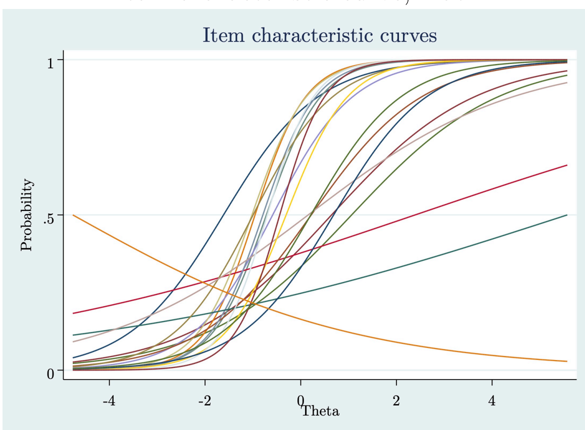
| | **Math** | | **Bangla** | |
|---|---|---|---|---|
| | Zero correct | All correct | Zero correct | All correct |
| Grade 6 | 1.9% | 6.1% | 0.6% | 0.3% |
| Grade 7 | 4.6% | 6.0% | 3.0% | 1.8% |
| Grade 8 | 3.6% | 14.0% | 2.4% | 0.0% |
| Grade 9 | 4.0% | 2.6% | 3.8% | 5.3% |
| Grade 10 | 3.5% | 0.0% | 1.5% | 8.6% |
| All | 3.7% | 5.2% | 2.5% | 4.0% |

### C.2.1 Math

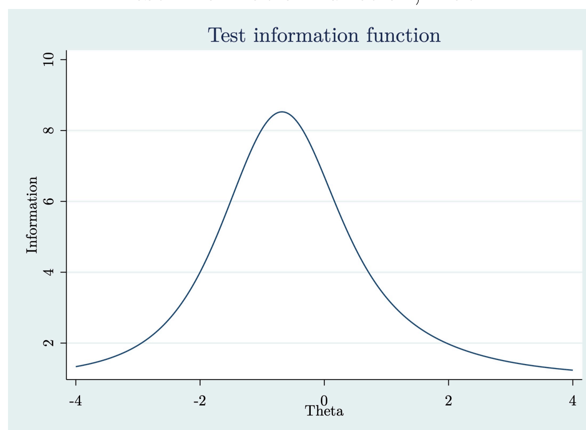
In general, we find that each item has positive discrimination, with well-behaved item characteristic curves:

**2-parameter logistic**

Item characteristic curve, math

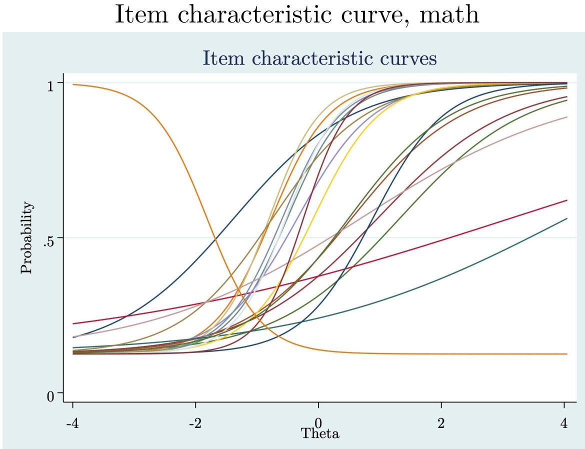



Test information function, math

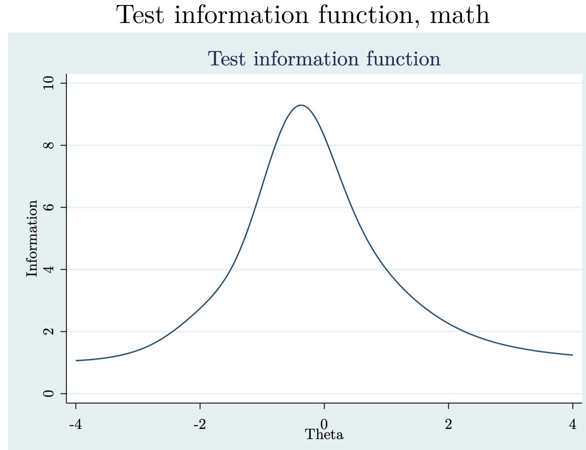



**3-parameter logistic**

Item characteristic curve, math




Test information function, math

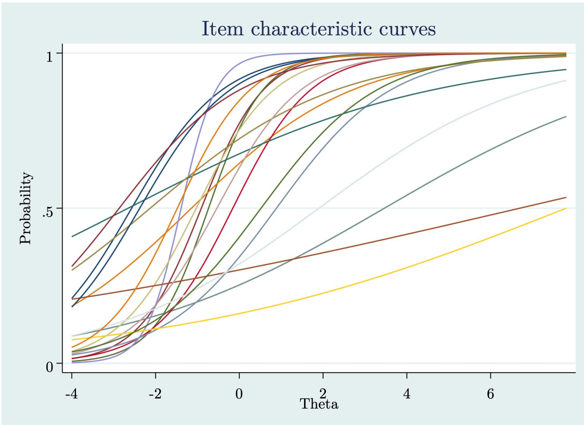


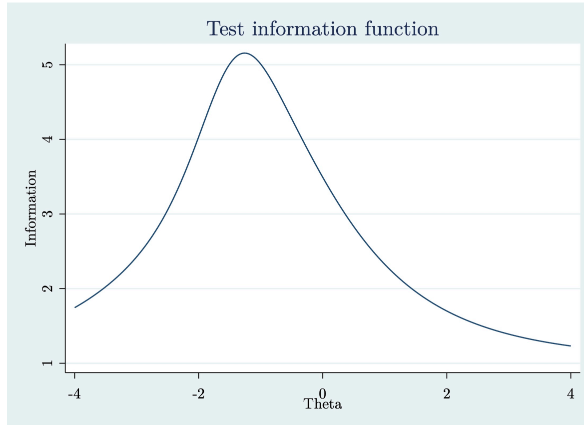

### C.2.2 Bangla

The following curves show that the Bangla results are very noisy. Becuase elements of the curriculum are fully cumulative, we do not expect that a grade 7 would excel at grade 5 questions. We exclude two questions in order to achieve convergence (question 16 and question 76), and we see that the results with the two-parameter model are very different from the three-parameter model results. For these reasons reason, we include this subject from our analysis.

**2-parameter logisitic**

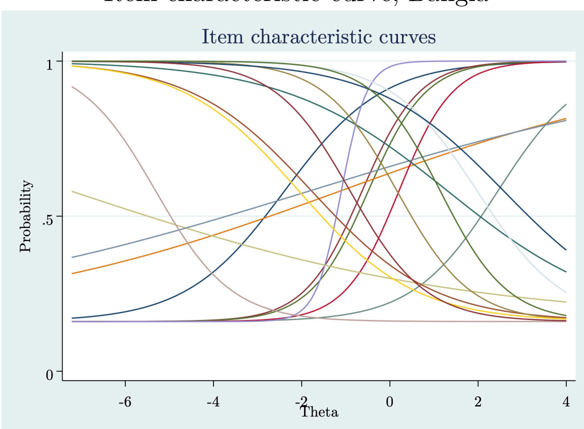
| Item characteristic curve, Bangla | Test information function, Bangla |
|---|---|

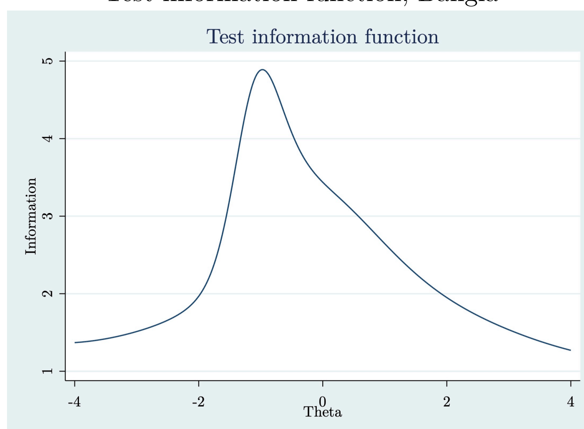



**3-parameter logistic**

| Item characteristic curve, Bangla | Test information function, Bangla |
|---|---|




## C.3 Extended list of treatments (12 arms)

Figure C.4 shows the full distribution of treatment arms, which reflects the following set of individual treatment arms:

0 Control

1a TV

1b TV + internet

2a Adaptive learning

2b Adaptive learning + TV

2c Adaptive learning + TV + internet

3a Data subsidy + TV + internet

3b Data subsidy + Adaptive learning

3c Data subsidy + Adaptive learning + TV

3d Data subsidy + Adaptive learning + TV + internet

4a Teacher support + TV

4b Teacher support + TV + internet

4c Teacher support + Data subsidy + TV + internet

Figure C.4: Assignment to individual treatment arms