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

Emily A. Beam†
Priya Mukherjee‡
Laia Navarro-Sola§

October 27, 2023

Abstract

*This study was supported by the World Bank's Strategic Impact Evaluation Fund (SIEF) and the Stiftelsen Carl Mannerfelts fund and benefited in particular from support from Alaka Holla and Erin Kelley. We thank Junnatul (Ani) Ferdosh, Md. Afzal Hossain Sarwar, Avijit Saha, Rafiqul Islam Sujon, Sakhwat Hosain, and 71 a2i partner-teachers for their cooperation to execute the research. We thank Sathia Chakrapani and Sohini Chaparala for outstanding project management and support. We also thank Tanzila Tabassum, Sukriti Ahmed, Zaima Promy, Pavarin Bhandtivej, Ishmail Baako, and Samina Hossain for critical research support, as well as the team of enumerators and supervisors who diligently collected data in spite of the challenges of the lockdown. This paper has benefited greatly from the comments and suggestions of Sebastián Gallegos, Alejandro Ganimian, Seema Jayachandran, David Schönholzer, and seminar and conference participants at Stockholm University, ASWEDE conference, AEFP EdDev Workshop, Midwest International Economic Development Conference, Advances with Field Experiments conference, CEPR Annual Symposium in Labour Economics, Universidad Autonoma de Madrid, Universitat Autonoma de Barcelona, Universitat Pompeu Fabra, University of Essex, EIEF Rome, Lund University, and University of Bergen.

The Human Subjects Committee for Innovations for Poverty Action provided oversight for this project, "Bangladesh COVID-19 Remote Learning Technologies," protocol #15594. This study was pre-registered at the AEA RCT Registry as "Take-up, Use, and Effectiveness of Remote Learning Technologies," #AEARCTR-0006191 (Beam et al., 2021)

†University of Vermont, emily.beam@uvm.edu

‡U. Wisconsin-Madison, priya.mukherjee@wisc.edu

§IIES, Stockholm University, laia.navarrosola@iies.su.se

We conduct a randomized controlled trial with households in Bangladesh during the COVID-19 school closures to investigate how parents adjust their educational investments in response to three interventions: edtech information about a specific learning app, an internet data package, and one-on-one phone learning support. We find that these light-touch interventions trigger important changes in parental educational investments. Specifically, parents increase tutoring investment as a result of edtech info, which appears to drive an increase in math learning. We document differential behavioral responses—and outcomes–by socioeconomic status, highlighting the potential for education interventions to affect inequality.

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

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—helping their children with homework or engaging with them in educational activities—and economic investments— such as paying for tutoring or other after-school activities (Bray, 1999). However, parents face barriers to optimizing along these two investment channels, including 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). Relieving these barriers is particularly important for families with fewer resources, for whom educational investment may also have the highest returns, and for contexts with educational emergencies where standard schooling may be disrupted, such as the COVID-19 school closures.

Understanding parental behavioral responses to these sorts of education policies is critical to accurately assess their net impact on children's human capital development (Das et al., 2013) and anticipate 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 it worked but parents re-optimized in response to the intervention, changing household inputs and offsetting 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 short-run interventions that relieve different barriers to education: one-on-one phone learning support, informational phone messages about a phone-based educational technology (edtech), and an internet data package alongside the phone messages. We evaluate their impact on household responses by collecting detailed information on parental investment outcomes for poorer and wealthier families. We also collect phone data on student achievement and examine the extent to which these interventions are inequality-enhancing or reducing.

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. While this element differs from a traditional educational setting, it 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. 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).

We include households facing a broad range of constraints to 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. We present descriptive insights showing that indeed, higher socioeconomic households invested more time and money in their children's education one year after the school closures than lower socioeconomic status households. 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.[1]

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 a follow-up survey by phone to measure the impact of these interventions on parent and student educational investment while the interventions were ongoing (March 2021). We conducted a second follow-up approximately one month after they concluded (June 2021) to measure student learning.[2] We first describe the nature of parental economic and financial investments and explore how it correlates with socioeconomic status. We then investigate the impacts of the interventions on parental investments while the interventions were ongoing. Lastly, we examine the impacts on student math achievement one to two months after the interventions ended.

We first demonstrate that although schools had remained closed for nearly one year, students remained consistently engaged in learning activities. Parents reported an average of 6.6 hours per week helping their child with schoolwork, and 64% of households reported using private tutoring in the past month. Relatively few households use tech-based resources to support learning, especially in poorer households.

[1] In a similar vein, List et al. (2021) show that in the U.S. 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.

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

While there is a positive relationship between time and financial parental investments among wealthier households, there is no relationship at all among poorer households, suggesting that financial constraints to investment (via tutoring) could be binding.

Our first set of experimental findings indicates that the interventions shifted investments in specific learning resources on the extensive margin. Providing the edtech information along with the data package increases the reported use of the edtech tool, and teacher support reduces the use of non-tech learning resources. Two additional results, that providing the edtech information alone does not change its use, and that the data package-induced change in the edtech tool use is concentrated among richer households, suggest that other barriers beyond information may be important.

Our second set of findings shows that the interventions significantly affect parental educational investments. The edtech information—alone or accompanied by the data package—increases private tutoring use, whereas the teacher support decreases it. The data package combined with edtech information attenuates the parental responses to the edtech information alone. We also observe that parents trade off their own time investments with economic investments: when parents increase tutoring expenditures, their own time investment tends to decrease.

We also observe suggestive impacts of interventions on student math knowledge. The edtech 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 edtech information alongside the data package 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 use of the edtech tool.

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 parental behavioral responses in terms of time investment at home, finding that it substitutes 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 U.S. Our paper experimentally examines parental educational investment responses to three prevalent remote educational interventions in a setting where school inputs are minimally influencing students' learning. By collecting detailed data on both parental time and economic investments and choices of learning resources, we show that households' investment responses differ depending on the intervention received—even if they all aim to reduce barriers to access—and that a joint re-optimization of educational inputs shifts investments above and beyond the educational input targeted by the particular intervention. While the specific magnitudes of our estimates are context-specific, particularly in light of the contemporaneous COVID-19 pandemic, the broader finding that these investment responses lead to sizeable and heterogeneous impacts on student outcomes are unlikely to be unique to this setting.

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).[3] Given that parents are a crucial intermediary between interventions and students, we contribute to the literature by demonstrating that simply providing resources is not enough because parents may reallocate their own investments.

Third, our paper provides insights into the literature linking the role of parental

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

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 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 a description of the interventions, randomization, and attrition. It also presents descriptive statistics, balance tests, and the empirical specification. Section 3 then presents descriptive insights about parental investments in children's education. Section 4 describes the impacts of the interventions on parent re-optimization responses with respect to economic and time educational investment and learning resource usage overall. Section 5 explores the persistent effects on student learning. Section 6 discusses potential mechanisms and channels that may explain the findings on parental investment responses. 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.[4] 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).

[4]The Bangladesh academic calendar follows the calendar year, beginning in January and ending in December.

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, of which 7,313 agreed to be recontacted in follow-up waves. While mobile phone penetration in Bangladesh is fairly high, smartphone ownership is substantially lower, meaning that our study sample is not nationally representative of families with secondary school children. 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).

Because we include only households with access to a smartphone, our sample is not nationally representative of families with secondary school children. We attempt to include a wide range of socioeconomic backgrounds despite this restriction by building our sample 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. The baseline survey included questions on 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 package, which would last for one month. We launched the teacher support intervention simultaneously with the informational interventions, which lasted four weeks for each student.

We measure impacts on resource use and parental investments in the first follow-up survey (Round 1), conducted while the interventions were ongoing. The sampling frame comprised all 7,313 baseline households that agreed to be recontacted.[5] 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. We surveyed 3,775 households, 55.8% of those contacted, representing 3,881 children.

We measure persistent impacts of the interventions in a second follow-up survey (Round 2), which took place approximately 4 to 8 weeks after the interventions concluded. 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. We also randomized the order in which we contacted households.

During this wave, we also separately interviewed children to measure their engagement and aspirations and also to assess their learning. In households with multiple children, we randomly selected one child to complete the assessment. 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

[5] We attempted to follow-up with a randomly selected 95% subsample due to timing constraints.

SSC exams and is taught in all secondary grade levels and curriculum tracks.[6] 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. 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 educational technology (edtech) tool. Households received twice-weekly reminders about a free internet-based learning platform, Robi 10-Minute School, for eight weeks.[7] 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 package. Households received an SMS message informing them that they would receive a free 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 free package 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.[8]

Treatment 3: Teacher support. Treated students were matched with a part-

[6] 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.

[7] 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).

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

ner 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.

Each treatment allows us to empirically test the net effect of reducing a different barrier to accessing remote education. Treatment 1 increases the salience of returns to an edtech learning resource, specifically the promoted edtech tool. Treatment 2 reduces the economic cost of accessing internet learning activities by providing a free data package. And Treatment 3 provides personalized teacher support to students, effectively reducing the cost of educational inputs external to the household.

In addition to these three treatments, we also measure 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 edtech information. 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 delivering 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.

Table 1: Distribution of treatment arms

N=7,576	Information			
	None	General Info	Edtech Tool Info	General Info + Edtech Tool Info
No Data Package	**25%** 1,894	**18.75%** 1,423	**12.5%** 947	**12.5%** 947
Data package		**6.25%** 471	**12.5%** 947	**12.5%** 947
Teacher support		∼44% within maize 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.

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 (edtech information), which we cross-randomized with the general information treatment. We further cross-randomized Treatment 2 (data package) 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.[9] A diagram of all treatment combinations is in Appendix A.3.

During randomization, we stratified along four baseline dimensions: household income (five categories), sample source, child gender (whether households had male

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

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 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.[10]

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

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

Table 2: Balance tests by pooled treatment assignment

	(1) All	(2) Control	(3) Edtech info	(4) Data + Edtech info.	(5) Teacher	(6) Joint tests, all, p-val
Household 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.

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 mothers' 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 edtech information arm and the control group at the 10% level.

As noted earlier, we do not expect that our sample will be nationally representative of the population of households with secondary-age children. Appendix Table A.3 compares key demographic characteristics of baseline sample with households from the 2019 Multiple Indicator Cluster Survey that have a child enrolled in grades 6–10. We see that households that have below-median socioeconomic status are most comparable to the general population, with roughly equal rates of parents with post-secondary education. However, the share of parents that did not complete primary is still lower among the poorer baseline sample, at 34% and 40% for mothers and fathers, respectively, versus 43% and 50% in the overall population.

2.7 Attrition

When collecting resource usage and parental investments outcomes in the Round 1 survey, 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 edtech information, information and data package, or teacher support are indistinguishable from the control group (Appendix Table A.4).

The response rate in Round 2, when we collect learning outcomes, is 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 edtech information, information and data package, 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 package 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.5 and A.6).

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. We examine impacts across the entire sample and investigate treatment heterogeneity by socioeconomic status, which we pre-specified in our analysis plan. 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:

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

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 $EdtechInfo_h$ is a binary variable equal to 1 if household h receives edtech information (Treatment 1). $Data_h$ is assignment to the data package 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 $EdtechInfo_h$ ($\widehat{\beta}_2$), $Data_h * EdtechInfo_h$ ($\widehat{\beta}_4$), 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 $Data_h * EdtechInfo_h$ 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.7 through A.16 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).[11]

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).[12] 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 $\widehat{\beta}_7$.

3 Descriptive insights on parents' educational inputs

To understand the nature of parents' educational investments and their relationship with available household resources in our sample, this section documents parental time and economic investments in children's education across three dimen-

[11]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 random in expectation. These results are reported in Appendix Tables A.17, A.18, and A.19, demonstrating modest increases in precision.

[12]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.

sions: 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.[13] 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.

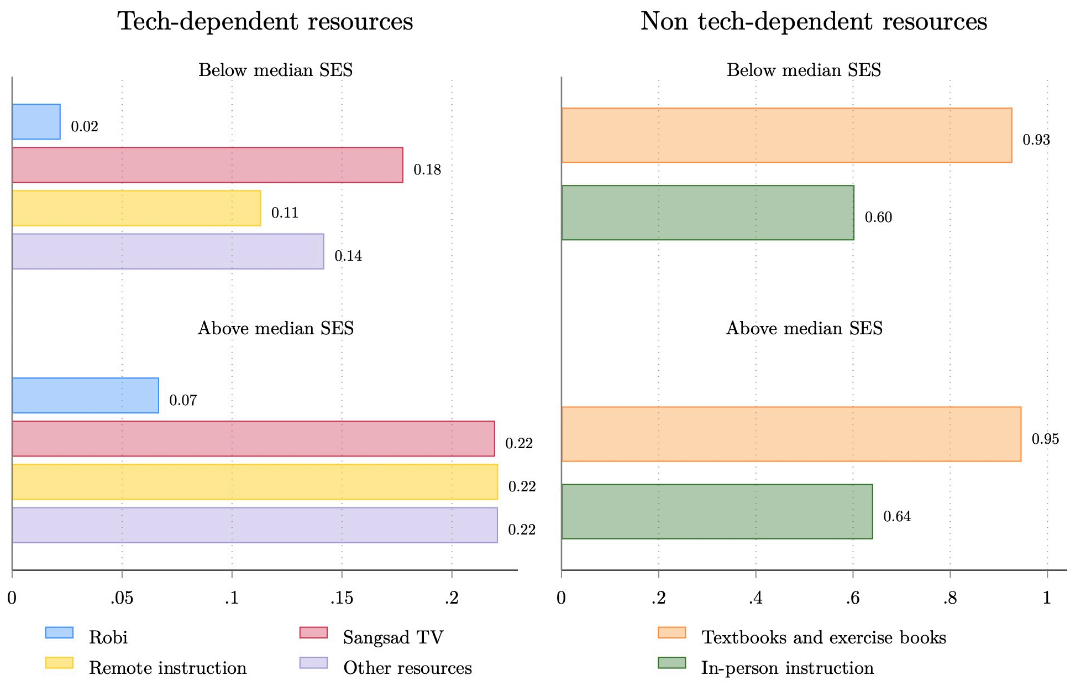
Although schools had remained closed for nearly one year, rates of engagement are high, with 89% of children doing school activities at least weekly and 78% of students studying or doing schoolwork at least 5 days on a typical week. Parents average 6.6 hours per week helping their children (unconditional), or an average of 9.5 hours among the 69% who ever help.

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. Specifically, non-tech-dependent resources are widely used in both groups in a homogeneous way, with 93–95% of students using textbooks and 60–64% meeting with an in-person teacher or tutor ever in the past month.

On the other hand, the use of tech-dependent resources is more restricted across all groups, showing that there is a substantial margin for increasing investment. The most popular resources are used by at most 22% of individuals in wealthier

[13]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).

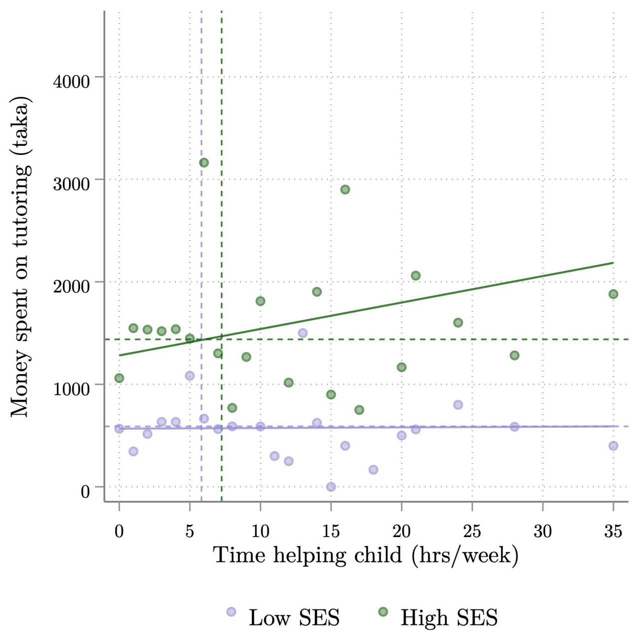
households. The use of other resources is lower, with government-televised lessons on Sangsad TV being used by 18–22% of students, remote teachers and classes by 11–22%, and the edtech tool we target by only 2–7%. Additionally, the use of tech-dependent resources differs significantly by socioeconomic status, with higher adoption rates across the board for students from wealthier households.[14] This hints at 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

[14]Although the sample eligibility criteria required that all surveyed households have access to a smartphone, at baseline, 47% of below-median SES households and 58% of above-median households had an active data plan on their phone

socioeconomic status. It is theoretically ambiguous whether parents from wealthier households will invest more or less time helping their children compared to poorer 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 on whether time and monetary investments are perceived substitutes or complements in the human capital production function.

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. Each data point plots the mean amount of money spent on tutoring for each (discrete) value of hours helping the child per week. It contains data only for the control group collected during Round 1 (March 2021).

In our (control) sample, 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.[15] Although wealthier families are more likely to hire private tutors (68%), it is of note that 59% of children in poorer households also use this service, indicating that there is extensive use of private tutoring among all sectors of the population. 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. Additionally, parents dedicate on average 6.5 hours 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 richer and poorer 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 reflect differences in the number of tutoring hours used or 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 from the U.S. 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

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

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. Given that, we disaggregate the impacts of the experimental interventions relieving some of these potential barriers by socio-economic status.

4 Impacts on educational investments

In this section, we measure the impacts of the three remote educational interventions—providing edtech information, supplementing the edtech 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 take-up of the intended educational service.

In particular, we find that providing edtech information does not increase the edtech 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 edtech's usage only increases when the information treatment is accompanied by a data package—suggesting that economic costs could be a 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 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 the edtech information does not increase the edtech tool's use, but it decreases the likelihood of using tech-based learning resources. Modest increases in reported edtech tool usage occur *only* when the free data package accompanies the edtech 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 that 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 edtech information alone does not affect the edtech tool's use, and we can reject at the 5% level even modest changes in usage [95% CI: -0.026, 0.012]. Columns 2 and 3 report impact on a tech-based and non-tech-based learning resource index, which are equally weighted index of binary indicators for whether the student used each of 5 tech-based learning resources or 3 non-tech-based learning resources, respectively, standardized to the control group. Appendix Table A.10 shows the impacts on the elements of each index. 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, 0.04). Appendix Tables A.7 and A.8 report the impacts on extensive and intensive margin use of specific learning resources, respectively, broadly showing effects in the same direction on both margins.

The information and data package increase the use of the edtech resource by 1.8 percentage points, significant at the unadjusted 10% level (Table 3, Column 1). This is a sizeable 36% increase compared to the 5% usage rate in the control group. With respect to the substitutability between learning resources after individuals received the 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 signifi-

Table 3: Impact of outreach on learning resources

	(1)	(2)	(3)
	Panel A. All		
	Edtech tool	Tech index	Non-tech index
Edtech info.	-0.007	-0.051	-0.013
	(0.009)	(0.024)	(0.024)
	[1.00]	[0.148]	[1.00]
Data + Edtech 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		
	Edtech tool	Tech index	Non-tech index
Edtech info.	-0.002	-0.043	-0.008
	(0.010)	(0.029)	(0.035)
	[1.000]	[0.654]	[1.000]
Data + Edtech 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		
	Edtech tool	Tech index	Non-tech index
Edtech info.	-0.010	-0.071	-0.011
	(0.016)	(0.037)	(0.035)
	[0.763]	[0.187]	[0.903]
Data + Edtech 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: Edtech tool equals 1 if student used the targeted ed tech tool in the past 30 days. The tech-index and non-tech indices are an equally weighted index of binary indicators for whether the student used each of 5 tech-based learning resources or 3 non-tech-based learning resources, respectively, standardized to the control group. 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 edtech information treatments $EdtechInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * EdtechInfo_h$, 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.

cant 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 edtech tool or other tech- and non-tech learning resources in response to the interventions. An exception is that wealthier households seem to respond to the data package by increasing the edtech tool usage by 4.1 percentage points (58%) from a baseline of 7%, significant only at 5% unadjusted levels. In contrast, the data package 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 edtech tools 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 edtech tool is remarkable in light of the light-touch nature of the interventions. 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).[16]

Given that the learning resource usage is parent-reported, it instead could be the case that the information increased the salience of the edtech tool, 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 edtech tool while they spent their time (and internet data) accessing other learning resources or distractions. Social

[16]Appendix Table A.9 shows treatment impacts on self-reported estimated data consumption. The information and data package 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.

desirability bias seems an unlikely explanation for the observed effects given that we would also expect an increase in reported edtech tool usage with only the provision of information and that it would not support the documented reduction in parental time spent with children as a result of edtech 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.[17]

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 edtech tool—alone, and accompanied by the data package—led parents to reduce time investment in their children's learning, and increase their financial investments in private tutoring, both on the extensive and intensive margin. Supplementing the edtech information with the data package attenuates parental time and economic re-optimization responses of the edtech 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 shows the likelihood of using private tutoring increased by 4.5 percentage points after receiving the edtech tool information, 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 statistically significant at the 5% unadjusted level and 10% adjusted level. In our study, the median of mothers' and father's pooled income over the past 30 days was 10,000 BDT, or approximately 2,222 BDT per week. This increase is therefore a non-trivial shift in household spending. However, about 16% of this increase may have been offset by moving away from money spent on other educational resources, which

[17] These results are shown in Appendix Tables A.10 and A.11.

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
Edtech 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 + Edtech 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
Edtech 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 + Edtech 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
Edtech 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 + Edtech 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 edtech information treatments $EdtechInfo_h * GenInfo_h$, the interaction between data and both information treatments $Data_h * GenInfo_h * EdtechInfo_h$, 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.

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 package 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 edtech information alone. If anything, this suggests that the data package 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 statistically 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.

To examine heterogeneous impacts by socioeconomic status, Panels B and C show that the increase in economic educational investment is greatest for wealthier parents, for whom the reduction in time investments is less pronounced. Edtech 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 edtech information, although none of them are significant at conventional levels, and they additionally substitute away from other educational investments, reducing expenses by 36 taka ($0.43). In addition, poorer parents reduce hours spent helping by 1.3 hours relative to a control-group mean of 5.8 hours per week, statistically significant at the 1% level, while the reduction among higher-SES parents is only 0.2 hours per week, which is not statistically significant ($p = 0.313$).

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

5 Persistent effects on student learning

This section provides evidence that the parental behavioral responses to educational interventions may have increased students' achievement. The edtech information increase student achievement in mathematics, with the increase concentrated among richer households. However, receiving the data package alongside the edtech 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.

These results should be interpreted with some caution for two reasons. First, unlike the initial follow-up round, there is evidence of differential attrition among those who completed the learning assessment. However, the higher attrition rate only appears in the general information treatment arm, which is not part of our key hypothesis tests. Reassuringly, Appendix Tables A.20 and A.21 show that treatment impacts on resource usage and parental investments are similar in magnitude and statistical significance when restricting to the learning assessment sample. Second, because assessments were conducted by phone, we were limited in the depth and breadth of questions that could be asked.

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 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

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

	(1)	(2)
Panel A. All		
	Standardized score	IRT, 2pl
Edtech info.	0.105	0.107
	(0.060)	(0.057)
	[1.000]	[1.000]
Data + Edtech 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		
	Standardized score	IRT, 2pl
Edtech info.	0.001	0.040
	(0.096)	(0.092)
	[1.000]	[1.000]
Data + Edtech 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		
	Standardized score	IRT, 2pl
Edtech info.	0.205	0.178
	(0.080)	(0.074)
	[0.053]	[0.053]
Data + Edtech 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: Standardized score includes sum of scores on 4 math questions, normalized to the grade-specific control group. IRT adjusted score shows predicted latent ability from full set of math questions, normalized to control group mean (not grade-specific). 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 treatments, plus flags for missing values. Anderson q-values reported in brackets. Robust standard errors are shown in parentheses and clustered at the household level.

magnitude and significance with the unadjusted scores based on the grade-specific base questions in Column 1 in nearly all cases.[18] 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.[19]

Panel A of Table 5 shows a 0.11-s.d. increase in mathematics scores among those who received the edtech 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 information and data package together effectively negates the learning gains from the edtech 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 reported edtech tool usage. However, we cannot distinguish whether the lack of a detectable change in human capital is a result of low overall dosage of the edtech tool 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.[20] This is perhaps not surprising given the light-touch nature of the teaching support, as successful teaching support interventions rely on more

[18]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.

[19]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.4). 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).

[20]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.

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 edtech information among low-SES students, with the 95% CI intervals of [–0.21, 0.21] using the base scores, and [–0.16, 0.24] using the 2-parameter IRT model. The concentration of math achievement gains only among high-SES students suggests that the edtech 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 data package for wealthier or poorer households, nor of the teacher phone support.

Combined with the evidence that the edtech information increases parental educational investment via 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 edtech information goes through parental re-optimization of their investments—increasing private tutoring—, not through the edtech tool effectiveness.

6 Discussion

This section discusses the potential mechanisms by which the three interventions may influence parental economic and time investment and, in turn, affect children's learning and human capital development. Appendix B includes a stylized model that formalizes some of the discussions below.[21] Children's learning can be fostered by teachers and private tutors, by household members through time helping or supporting educational activities, and by the learning resources that students use.

Edtech learning resources serve as a complement or alternative to traditional pedagogical methods and resources, and they have been implemented both in formal

[21]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.

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 can have non-negligible economic costs, either for the product itself or for necessary devices or technical capabilities to reliably use it (e.g., through internet connection or cable TV subscription). Other barriers to access, such as the need for technological literacy, may also make their adoption among underprivileged groups difficult.

Common interventions to address these barriers to edtech take-up include providing information and reducing costs.[22] Information can signal the value of this new platform, leading individuals to revise their beliefs about its marginal returns. However, it may have broader impacts. Information on new technologies may shift beliefs about the value of features highlighted by that technology. For example, providing information about an edtech tool that promotes adaptive learning methods may lead households to value adaptive learning methods generally. Additionally, edtech could make educational investment more salient, leading parents to increase their educational investments. Hence, information about an edtech tool may have both direct impacts on tool usage and indirect impacts on general education investment. Reducing edtech costs via subsidies may increase usage, and the net impact will depend on the perceived returns to the technology as well whether the data package is directly tied to usage.

Our findings are consistent with this hypothesis that edtech information provision has indirect impacts on educational investment. Information about the edtech tool did not change its use but caused substitution away from tech-learning resources

[22]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.

and substantial parental investment shifts, with a substantial increase in tutoring. One potential reason for this increase is that information about the edtech tool 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. Under the presence of additional constraints, households may not adopt the educational technology even if they perceive a high return on investment, only using the 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. Another possibility is that, although the edtech tool is cheaper than tutoring, parents may feel they have more control over children's time usage through tutoring (Gallego et al., 2020), or they may have uncertainty on how to use these novel resources effectively. Lastly, they may simply believe that tutoring has a higher return to investment. Indeed, it appears that the app-info-induced increase in tutoring did generate learning gains, which are concentrated among students from wealthier backgrounds.

Although significant shifts in other investments occurred, there was no information-induced increase in usage of the targeted edtech tool. This indicates that other barriers to the app's use existed beyond information, with economic constraints being an obvious one. The increase in edtech tool take-up *only* when information about the edtech tool is accompanied by reduced data costs supports this hypothesis, showing that parents only increased investment in the novel remote learning resource when they received the data package with information. The data-induced change in edtech tool usage also generated an extensive-margin increase in the use of tech-dependent options, rather than being offset through substitution. However, even after receiving the data support, reported edtech tool take-up is low, suggesting that other barriers are still present or that parents may still ascribe relatively low marginal returns to the edtech tool. 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).

Regarding the related impacts of the edtech information on tutoring and edtech tool usage (with or without the data support), one potential explanation is that there is an underlying economic trade-off between these two learning resources. Although using the edtech tool is less costly than tutoring, this economic trade-off could be important 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" (Duflo and Udry, 2004). At baseline, 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.

That the edtech information increases tutoring and also learning, particularly among wealthier households, suggests that tutoring itself is driving the learning gains. This is further supported by the absence of changes in other educational inputs. Another possibility is that the information affected student engagement and motivation, either directly or indirectly, as a result of the observed changes in parental investments. We measure 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.12).

The absence of learning impacts from the remote teacher support intervention is consistent with Crawfurd et al. (2023), who find that a similar intervention Sierra Leone increases engagement but not learning.[23] These results could indicate that this method of supporting student learning is not effective. In our case, teacher support covered a range of topics, not necessarily mathematics, so any learning gains may be more dispersed. An alternative hypothesis is that parents could have temporarily reallocated their investments as a result of the support, leading to no net change in educational investments. Despite the short nature of the teacher support intervention, we see suggestive evidence of this through the decrease in private tutoring in Table 4.

7 Conclusions

We conducted a field experiment among households across Bangladesh during the COVID-19 school closures to document parental investments and measure the impact of three short-run educational interventions aimed at reducing different barriers to parental education investment. We find that rates of student and parental engagement are high despite ongoing school closures and that the main difference in parental investments between wealthier and poorer families is through financial expenditures rather than time.

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 take-up, we find that parental responses are less prominent. 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 an edtech tool does not increase the edtech tool's usage, but it instead triggers parental behavioral responses, with significant increases in economic investment in tutoring and

[23] Unlike Crawfurd et al. (2023), we do not see changes in student engagement or effort (Appendix Table A.12).

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 edtech tool and limits the need for parental resource reallocation.

We also observe persistent increases in student math achievement only for the edtech 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 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 does not promote math learning, and minimal parental behavioral responses in that treatment group suggest that these effects are not driven by household re-optimization.

Because the interventions' 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 poorer 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. In a similar vein, the lack of detectable learning impacts generated by the data package could reflect low take-up and usage of the edtech tool, so we cannot rule out that the edtech tool itself may be effective, if used.

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. The specific trade-offs and constraints parents face may be context-specific, particularly as this study takes place during the COVID-19 pandemic, when the closure of schools massively reshaped educational investments. However, the underlying insight—that parental investment responses, far from second-order, generate measurable and heterogeneous impacts on student outcomes—is unlikely to be tied to this context and 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 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 edtech tools, 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 tutoring 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.

References

Abadie, A., S. Athey, G. Imbens, and J. Wooldridge (2017). When Should You Adjust Standard Errors for Clustering? Working Paper w24003, National Bureau of Economic Research, Cambridge, MA.

Agostinelli, F., M. Doepke, G. Sorrenti, and F. Zilibotti (2022, February). When the great equalizer shuts down: Schools, peers, and parents in pandemic times. *Journal of Public Economics 206*, 104574.

Ahmed, S. I., M. R. Haque, J. Chen, and N. Dell (2017). Digital privacy challenges with shared mobile phone use in Bangladesh. *Proceedings of the ACM on Human-Computer Interaction 1*(CSCW), 1–20.

Alam, M. B. and Z. Zhu (2021, January). Private tutoring in Bangladesh: Evolution, expansion, and policy responses. *International Journal of Comparative Education and Development 24*(1), 20–36.

Alamgir, M. (2020). Assignments for class 6-9 students to start on Nov 1. https://www.thedailystar.net/frontpage/news/assignments-class-6-9-students-start-nov-1-1983217.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American Statistical Association 108*(484), 1481–1495.

Andrabi, T., B. Daniels, and J. Das (2021). Human Capital Accumulation and Disasters: Evidence from the Pakistan Earthquake of 2005. *The Journal of Human Resources*, 0520–10887R1.

Andrew, A., S. Cattan, M. Costa Dias, C. Farquharson, L. Kraftman, S. Krutikova, A. Phimister, and A. Sevilla (2020). Inequalities in children's experiences of home learning during the COVID-19 lockdown in England. *Fiscal Studies 41*(3), 653–683.

Angrist, N., P. Bergman, and M. Matsheng (2022, June). Experimental evidence on learning using low-tech when school is out. *Nature Human Behaviour*, 1–10.

Attanasio, O., T. Boneva, and C. Rauh (2020, September). Parental Beliefs about Returns to Different Types of Investments in School Children. *The Journal of Human Resources*, 0719.

Axiata Group Berhad (2020). Integrated Annual Report 2020. Annual Report.

Bacher-Hicks, A., J. Goodman, and C. Mulhern (2021). Inequality in household adaptation to schooling shocks: Covid-induced online learning engagement in real time. *Journal of Public Economics 193*.

Baker, R., B. Evans, and T. Dee (2016). A Randomized Experiment Testing the Efficacy of a Scheduling Nudge in a Massive Open Online Course (MOOC). *AERA Open 2*(4).

Bandiera, O., N. Buehren, M. Goldstein, I. Rasul, and A. Smurra (2020). Do School Closures During an Epidemic have Persistent Effects? Evidence from Sierra Leone in the Time of Ebola. Working Paper.

Banerjee, A., R. Banerji, J. Berry, E. Duflo, H. Kannan, S. Mukherji, M. Shotland, and M. Walton (2016). Mainstreaming an effective intervention: Evidence from randomized evaluations of "Teaching at the Right Level" in India. Technical report, National Bureau of Economic Research.

Banerjee, A. V., S. Cole, E. Duflo, and L. Linden (2007). Remedying education: Evidence from two randomized experiments in India. *The Quarterly Journal of Economics 122*(3), 1235–1264.

Bansak, C. and M. Starr (2021). COVID-19 shocks to education supply: How 200,000 US households dealt with the sudden shift to distance learning. *Review of Economics of the Household 19*(1), 63–90.

Beam, E. and P. Mukherjee (2021). Gendered Impacts of the Pandemic: Evidence from Vulnerable Households in Bangladesh. Working Paper.

Becker, G. S. (1965). A Theory of the Allocation of Time. *The Economic Journal 75*(299), 493–517.

Becker, G. S. and N. Tomes (1976). Child Endowments and the Quantity and Quality of Children. *Journal of Political Economy 84*(4), S143–S162.

Beg, S. A., A. M. Lucas, W. Halim, and U. Saif (2019). Beyond the Basics: Improving Post-Primary Content Delivery Through Classroom Technology. *National Bureau of Economic Research* (Working Paper 25704).

Beuermann, D. W., J. Cristia, S. Cueto, O. Malamud, and Y. Cruz-Aguayo (2015). One laptop per child at home: Short-term impacts from a randomized experiment in Peru. *American Economic Journal: Applied Economics 7*(2), 53–80.

Blanden, J., M. Doepke, and J. Stuhler (2022). Educational inequality. Technical report, National Bureau of Economic Research.

Bloomfield, J., A. Balsa, and A. Cid (2022). Using behavioral insights in early childhood interventions: The effects of Crianza Positiva e-messaging program on parental investment. *Review of Economics of the Household*, 1–36.

Boneva, T. and C. Rauh (2018). Parental beliefs about returns to educational investments—the later the better? *Journal of the European Economic Association 16*(6), 1669–1711.

Bray, M. and C. Lykins (2012, May). *Shadow Education: Private Supplementary Tutoring and Its Implications for Policy Makers in Asia.* Asian Development Bank.

Bray, T. M. (1999). *The Shadow Education System: Private Tutoring and Its Implications for Planners.* UNESCO International Institute for Educational Planning.

Caballero Montoya, E., D. Mtambo, K. Nzomo, M. Shotland, and J. Thunde (2021). Distance Learning. What have we learned? Technical report, IDinsight.

Carlana, M. and E. La Ferrara (2021). Apart but Connected: Online Tutoring and Student Outcomes during the COVID-19 Pandemic. *SSRN Electronic Journal*.

Caucutt, E. M., L. Lochner, and Y. Park (2017). Correlation, consumption, confusion, or constraints: Why do poor children perform so poorly? *The Scandinavian Journal of Economics 119*(1), 102–147.

Corak, M. (2013, September). Income Inequality, Equality of Opportunity, and Intergenerational Mobility. *Journal of Economic Perspectives 27*(3), 79–102.

Crawfurd, L., D. K. Evans, S. Hares, and J. Sandefur (2021). *Teaching and Testing by Phone in a Pandemic*. Center for Global Development.

Crawfurd, L., D. K. Evans, S. Hares, and J. Sandefur (2023, September). Live tutoring calls did not improve learning during the COVID-19 pandemic in Sierra Leone. *Journal of Development Economics 164*, 103114.

Cristia, J., P. Ibarrarán, S. Cueto, A. Santiago, and E. Severín (2017). Technology and child development: Evidence from the one laptop per child program. *American Economic Journal: Applied Economics 9*(3), 295–320.

Cunha, F., J. J. Heckman, L. Lochner, and D. V. Masterov (2006). Chapter 12 Interpreting the Evidence on Life Cycle Skill Formation. In E. Hanushek and F. Welch (Eds.), *Handbook of the Economics of Education*, Volume 1, pp. 697–812. Elsevier.

Dahl, G. B. and L. Lochner (2012). The impact of family income on child achievement: Evidence from the earned income tax credit. *American Economic Review 102*(5), 1927–56.

Das, J., S. Dercon, J. Habyarimana, P. Krishnan, K. Muralidharan, and V. Sundararaman (2013). School Inputs, Household Substitution, and Test Scores. *American Economic Journal: Applied Economics 5*(2), 29–57.

Del Bono, E., L. Fumagalli, A. Holford, and B. Rabe (2021). Coping with school closures: Changes in home-schooling during COVID-19. *Institute for Economic and Social Research Report*.

Dizon-Ross, R. (2019). Parents' Beliefs about Their Children's Academic Ability: Implications for Educational Investments. *American Economic Review 109*(8), 2728–2765.

Doss, C., E. M. Fahle, S. Loeb, and B. N. York (2019). More than just a nudge supporting kindergarten parents with differentiated and personalized text messages. *The Journal of Human Resources 54*(3), 567–603.

Duflo, E. and C. R. Udry (2004). Intrahousehold resource allocation in Cote d'Ivoire: Social norms, separate accounts and consumption choices.

Francesconi, M. and J. J. Heckman (2016). Child Development and Parental Investment: Introduction. *The Economic Journal 126*(596), F1–F27.

Fredriksson, P., B. Ockert, and H. Oosterbeek (2016, October). Parental Responses to Public Investments in Children: Evidence from a Maximum Class Size Rule. *The Journal of Human Resources 51*(4), 832–868.

Fuchs-Schündeln, N., D. Krueger, A. Ludwig, and I. Popova (2022). The long-term distributional and welfare effects of covid-19 school closures. *The Economic Journal 132*(645), 1647–1683.

Gallego, F. A., O. Malamud, and C. Pop-Eleches (2020). Parental monitoring and children's internet use: The role of information, control, and cues. *Journal of Public Economics 188*, 104208.

Gelber, A. and A. Isen (2013). Children's schooling and parents' behavior: Evidence from the Head Start Impact Study. *Journal of Public Economics 101*, 25–38.

Hassan, H., A. Islam, A. Siddique, and L. C. Wang (2021). Telementoring and home-schooling during school closures: A randomized experiment in rural Bangladesh.

Hindy, J. (2022, May). How much data does YouTube actually use? https://www.androidauthority.com/how-much-data-does-youtube-use-964560/.

Houtenville, A. J. and K. S. Conway (2008). Parental Effort, School Resources, and Student Achievement. *The Journal of Human Resources 43*(2), 437–453.

Hurwitz, L. B., A. R. Lauricella, A. Hanson, A. Raden, and E. Wartella (2015). Supporting Head Start parents: Impact of a text message intervention on parent–child activity engagement. *Early Child Development and Care 185*(9), 1373–1389.

Johnston, J. and C. Ksoll (2017). Effectiveness of Interactive Satellite-Transmitted Instruction: Experimental Evidence from Ghanaian Primary Schools. *Center for Education Policy Analysis Working Paper No. 17-08*.

Jones, D., D. Molitor, and J. Reif (2019, November). What do Workplace Wellness Programs do? Evidence from the Illinois Workplace Wellness Study*. *The Quarterly Journal of Economics 134*(4), 1747–1791.

Kalil, A., K. M. Ziol-Guest, R. M. Ryan, and A. J. Markowitz (2016, July). Changes in Income-Based Gaps in Parent Activities With Young Children From 1988 to 2012. *AERA Open 2*(3), 2332858416653732.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental Analysis of Neighborhood Effects. *Econometrica 75*(1), 83–119.

Kornrich, S. and F. Furstenberg (2013). Investing in Children: Changes in Parental Spending on Children, 1972—2007. *Demography 50*(1), 1–23.

Kraft, M. A. and M. Monti-Nussbaum (2017). Can Schools Enable Parents to Prevent Summer Learning Loss? A Text-Messaging Field Experiment to Promote Literacy Skills. *The ANNALS of the American Academy of Political and Social Science 674*(1), 85–112.

Lai, F., L. Zhang, Q. Qu, and X. Hu (2012). Does Computer-Assisted Learning Improve Learning Outcomes? Evidence from a Randomized Experiment in Public Schools in Rural Minority Areas in Qinghai, China.

Lichand, G., C. A. Doria, O. Leal-Neto, and J. P. C. Fernandes (2022). The impacts of remote learning in secondary education during the pandemic in brazil. *Nature Human Behaviour*, 1–8.

List, J. A., J. Pernaudet, and D. Suskind (2021, October). It All Starts with Beliefs: Addressing the Roots of Educational Inequities by Shifting Parental Beliefs.

Malamud, O., S. Cueto, J. Cristia, and D. W. Beuermann (2019, May). Do children benefit from internet access? Experimental evidence from Peru. *Journal of Development Economics 138*, 41–56.

Martinez, I. (2014). Never Put Off Till Tomorrow?

Muralidharan, K., A. Singh, and A. J. Ganimian (2019). Disrupting education? Experimental evidence on technology-aided instruction in India. *American Economic Review 109*(4), 1426–60.

Nath, S. R. (2011, August). Private tutoring. https://www.thedailystar.net/news-detail-199463.

Navarro-Sola, L. (2021). Secondary schools with televised lessons: The labor market returns of the Mexican telesecundaria.

OANDA (2021). Historical exchange rates. Accessed September 20, 2021.

Okeleke, K. (2021). Achieving mobile-enabled digital inclusion in Bangladesh.

Patterson, R. W. (2018). Can behavioral tools improve online student outcomes? Experimental evidence from a massive open online course. *Journal of Economic Behavior & Organization 153*, 293–321.

Piper, B., S. S. Zuilkowski, D. Kwayumba, and C. Strigel (2016, July). Does technology improve reading outcomes? Comparing the effectiveness and cost-effectiveness of ICT interventions for early grade reading in Kenya. *International Journal of Educational Development 49*, 204–214.

Pop-Eleches, C. and M. Urquiola (2013). Going to a Better School: Effects and Behavioral Responses. *American Economic Review 103*(4), 1289–1324.

Ramey, G. and V. A. Ramey (2010, March). The Rug Rat Race. Technical report.

Sahin, A. and D. Anil (2017). The Effects of Test Length and Sample Size on Item Parameters in Item Response Theory.

Schneider, D., O. P. Hastings, and J. LaBriola (2018, June). Income Inequality and Class Divides in Parental Investments. *American Sociological Review 83*(3), 475–507.

Schueler, B. E. and D. Rodriguez-Segura (2021). A Cautionary Tale of Tutoring Hard-to-Reach Students in Kenya.

Singh, A., M. Romero, and K. Muralidharan (2022). COVID-19 Learning Loss and Recovery: Panel Data Evidence from India. Technical Report 22/122.

Stone, C. A. (1992, March). Recovery of Marginal Maximum Likelihood Estimates in the Two-Parameter Logistic Response Model: An Evaluation of MULTILOG. *Applied Psychological Measurement 16*(1), 1–16.

Todd, P. E. and K. I. Wolpin (2003). On the Specification and Estimation of the Production Function for Cognitive Achievement. *The Economic Journal 113*(485), F3–F33.

Todd, P. E. and K. I. Wolpin (2007). The Production of Cognitive Achievement in Children: Home, School, and Racial Test Score Gaps. *Journal of Human Capital 1*(1), 91–136.

Urminsky, O., C. Hansen, and V. Chernozhukov (2016). Using double-lasso regression for principled variable selection. *Available at SSRN 2733374*.

Yeomans, M. and J. Reich (2017). Planning prompts increase and forecast course completion in massive open online courses. In *Proceedings of the Seventh International Learning Analytics & Knowledge Conference*, Vancouver British Columbia Canada, pp. 464–473. ACM.

York, B. N., S. Loeb, and C. Doss (2018). One Step at a Time: The Effects of an Early Literacy Text Messaging Program for Parents of Preschoolers. *The Journal of Human Resources*, 0517.

A For Online Publication: Appendix Figures and Tables

Figure A.1: Project timeline

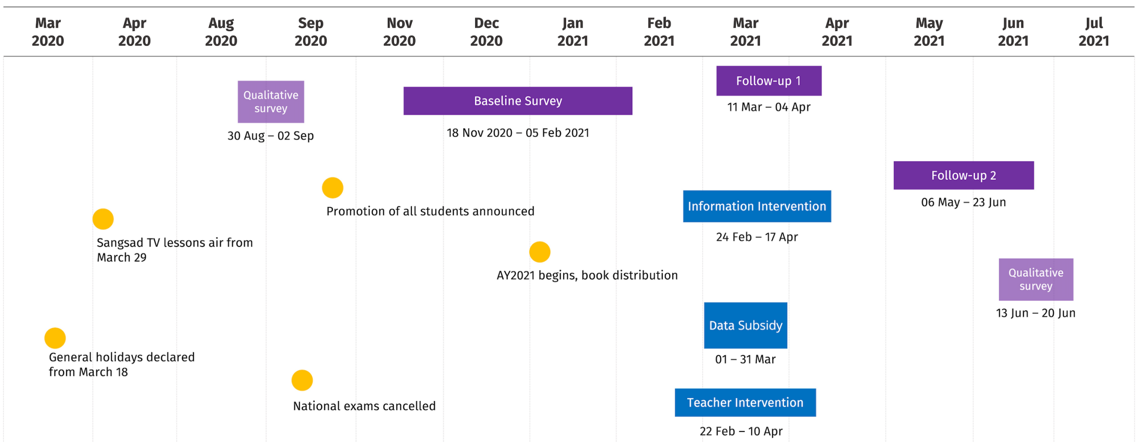


Figure A.2: Distribution of respondents

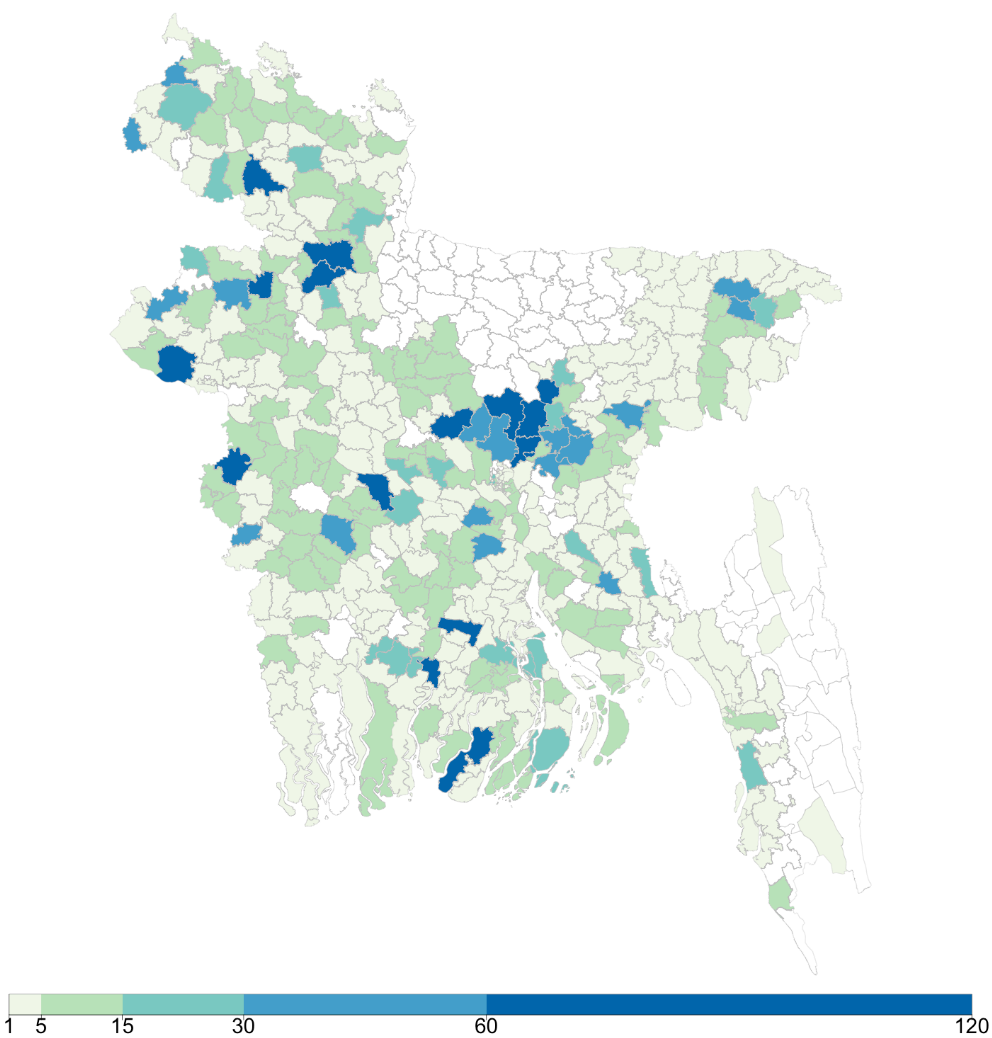


Figure A.3: Assignment to individual treatment arms

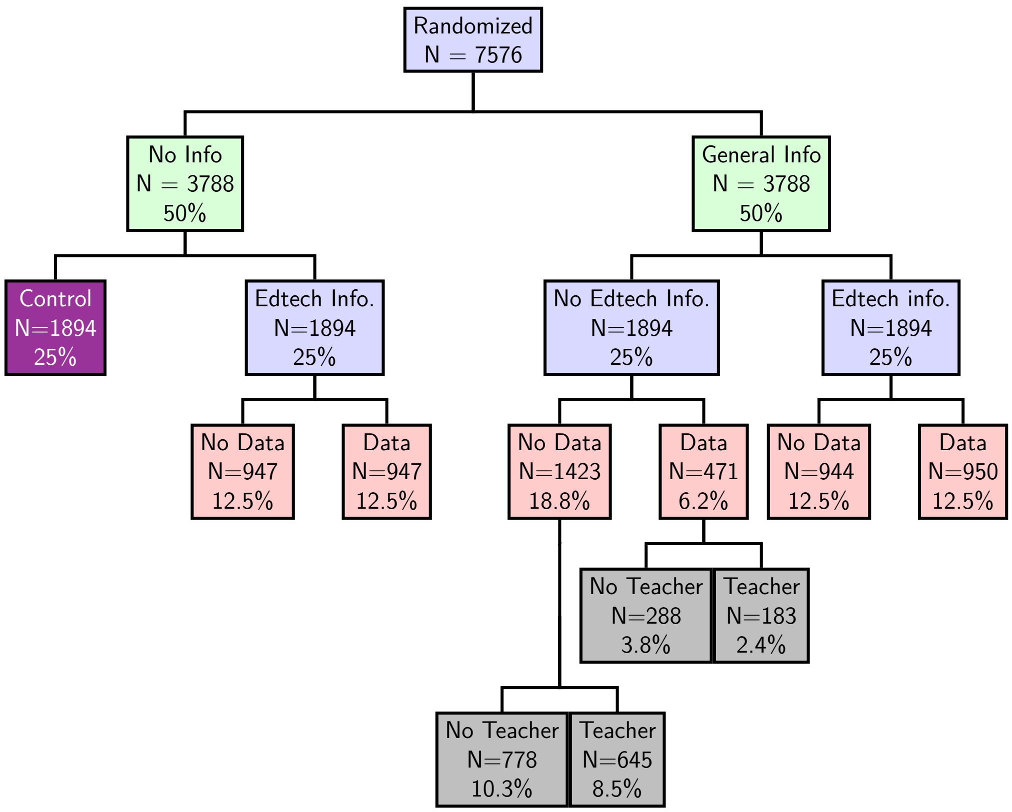


Figure A.3 shows the full distribution of treatment arms. “General info” combines those who received information about the TV program only and those who received information about the TV program and the corresponding YouTube channel.

Figure A.4: Distribution of endline math scores by self-reported Grade 5 exam scores

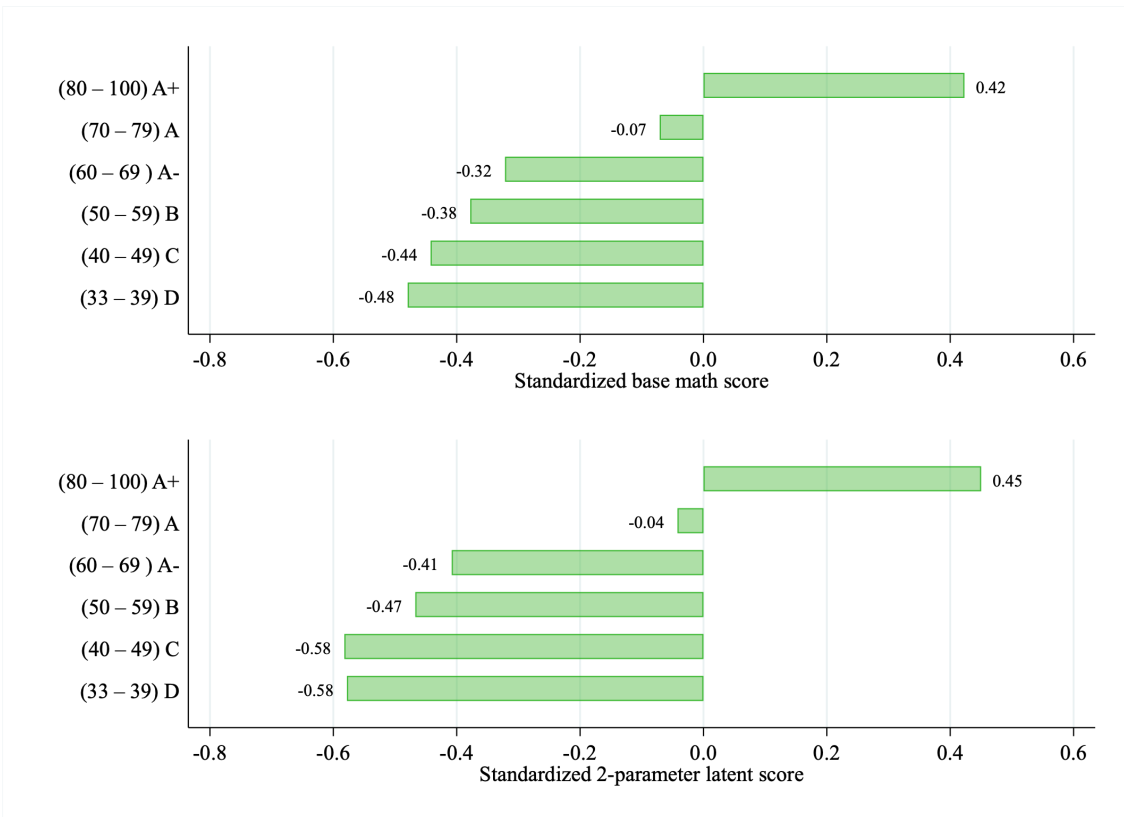


Table A.1: Study eligibility by data source

	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)
Edtech 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 + Edtech info.	0.022	-0.012	0.019
	(0.018)	(0.020)	(0.017)
Data + General info. + Edtech 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.

Table A.3: Characteristics of households with children in grades 6–10, baseline and MICS-2019 samples

Variable	All	Baseline Low SES	High SES	MICS-2019 Mean
Children age 5–17	1.93	1.95	1.91	1.87
Rooms for sleeping	2.68	2.33	3.00	2.39
Flush toilet	0.58	0.20	0.93	0.51
Has mobile	1.00	1.00	1.00	0.97
Less primary, mother	0.23	0.34	0.13	0.43
Primary graduate, mother	0.38	0.44	0.32	0.43
Secondary graduate, mother	0.20	0.15	0.25	0.07
Post-secondary graduate, mother	0.19	0.08	0.31	0.06
Less primary, father	0.27	0.40	0.15	0.50
Primary graduate, father	0.27	0.33	0.22	0.30
Secondary graduate, father	0.18	0.15	0.21	0.08
Post-secondary graduate, father	0.27	0.12	0.43	0.12
		7576		20120

Notes: Baseline sample includes all househods randomized into treatment. MICS-2019 sample includes all households with children enrolled in grades 6–10 to ensure comparability with the baseline sample.

Table A.4: Balance tests by pooled treatment assignment, Round 1 respondents only

	(1) All	(2) Control	(3) Edtechinfo	(4) Data + Edtech 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.

Table A.5: Balance tests by pooled treatment assignment, Round 2 respondents only

	(1) All	(2) Control	(3) Edtech info	(4) Data + Edtech 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.

Table A.6: Balance tests by pooled treatment assignment, learning assessment respondents only

	(1) All	(2) Control	(3) Edtech info	(4) Data + Edtech 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.

Table A.7: Impact of outreach on learning resources

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Tech-dependent learning resources						
	Sangsad TV	Video lessons	Edtech tool	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)
Edtech 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 + Edtech 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 + Edtech 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. + Edtech 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)		
Edtech info.	-0.011	0.004	0.009	-0.013		
	(0.011)	(0.019)	(0.021)	(0.024)		
General + Edtech 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 + Edtech info.	0.014	0.010	0.012	0.020		
	(0.013)	(0.021)	(0.024)	(0.027)		
Data + General info. + Edtech 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: Indices are equally weighted averages of the previous columns, standardized to the control group. 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.

Table A.8: Intensive-margin impact of outreach on learning resources

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Tech-dependent learning resources						
	Sangsad TV	Video lessons	Edtech tool	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)
Edtech 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 + Edtech 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 + Edtech 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. + Edtech 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)
Edtech info.	14.196	-13.187	22.819	0.021
	(37.339)	(12.430)	(20.161)	(0.034)
General + Edtech 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 + Edtech info.	5.001	10.696	-15.617	-0.017
	(41.235)	(14.042)	(21.970)	(0.038)
Data + General info. + Edtech 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: Indices are equally weighted averages of the previous columns, standardized to the control group. 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.

Table A.9: 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)
Edtech info.	-0.060	-0.039	0.081	-31.178
	(0.021)	(0.018)	(0.905)	(11.194)
General + Edtech 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 + Edtech info.	0.033	-0.001	1.174	16.769
	(0.025)	(0.021)	(1.344)	(12.213)
Data + General info. + Edtech 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)	
Edtech info.	-0.024	-0.019	-0.886	
	(0.026)	(0.022)	(0.833)	
General + Edtech 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 + Edtech info.	-0.003	-0.009	0.894	
	(0.028)	(0.024)	(0.823)	
Data + General info. + Edtech 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.

Table A.10: 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	Edtech tool	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)
Edtech 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 + Edtech 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 + Edtech 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. + Edtech 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)		
Edtech info.	-0.005	-0.029	-0.013	-0.031		
	(0.013)	(0.023)	(0.028)	(0.031)		
General + Edtech 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 + Edtech info.	0.009	0.060	0.032	0.062		
	(0.015)	(0.026)	(0.031)	(0.034)		
Data + General info. + Edtech 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: Indices are equally weighted averages of the previous columns, standardized to the control group. 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.

Table A.11: Persistence of the impact of outreach on parental investment

	(1)	(2)
Panel A. Time investment		
	Days reminded student	Hours parent helped
General info.	0.242	-0.133
	(0.230)	(0.513)
Edtech info.	0.101	-0.103
	(0.167)	(0.328)
General + Edtech info.	-0.166	0.186
	(0.209)	(0.410)
Data + General info.	-0.142	0.091
	(0.219)	(0.520)
Data + Edtech info.	-0.020	-0.124
	(0.184)	(0.359)
Data + General info. + Edtech 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.	-0.008	-233.401
	(0.039)	(94.449)
Edtech info.	0.002	-13.237
	(0.029)	(92.434)
General + Edtech info.	-0.020	-26.262
	(0.036)	(113.400)
Data + General info.	0.059	109.488
	(0.039)	(120.667)
Data + Edtech info.	0.033	92.810
	(0.032)	(102.542)
Data + General info. + Edtech 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: All expenses reported in taka. 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.

Table A.12: 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)
Edtech info.	0.063	-0.019	0.008	0.028
	(0.057)	(0.021)	(0.007)	(0.036)
General + Edtech info.	-0.059	-0.012	0.003	-0.028
	(0.059)	(0.020)	(0.007)	(0.040)
Data + General info.	-0.013	-0.019	0.004	-0.008
	(0.085)	(0.030)	(0.011)	(0.056)
Data + Edtech info.	-0.008	-0.016	-0.001	-0.027
	(0.048)	(0.017)	(0.005)	(0.030)
Data + General info. + Edtech info.	0.084	0.036	-0.003	0.055
	(0.123)	(0.043)	(0.015)	(0.081)
Teacher support	-0.008	-0.006	-0.004	-0.021
	(0.056)	(0.020)	(0.006)	(0.036)
Teacher support + Data	0.158	0.000	-0.011	0.002
	(0.132)	(0.049)	(0.013)	(0.079)
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, 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.

Table A.13: 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)
Edtech info.	0.031	-0.138
	(0.089)	(0.670)
General + Edtech info.	-0.106	-0.884
	(0.084)	(0.642)
Data + General info.	0.071	0.416
	(0.128)	(1.061)
Data + Edtech info.	-0.052	-0.231
	(0.089)	(0.654)
Data + General info. + Edtech info.	0.111	0.349
	(0.196)	(1.544)
Teacher support	0.052	-0.325
	(0.101)	(0.772)
Teacher support + Data	0.007	-3.208
	(0.207)	(1.736)
DV mean, control	5.65	19.03
Observations	5619	5168

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.

Table A.14: 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)
Edtech info.	-0.059	-0.670
	(0.125)	(0.662)
General + Edtech info.	-0.065	-0.445
	(0.125)	(0.654)
Data + General info.	-0.040	-0.327
	(0.205)	(0.986)
Data + Edtech info.	-0.038	-0.246
	(0.104)	(0.580)
Data + General info. + Edtech info.	0.133	0.570
	(0.284)	(1.454)
Teacher support	0.011	-0.273
	(0.121)	(0.657)
Teacher support + Data	-0.194	0.093
	(0.307)	(1.583)
DV mean, control	5.46	15.35
Observations	4245	4194

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.

Table A.15: Impact of outreach on student learning (math), endline

	(1)	(2)
	Unadjusted score	IRT, 2pl
General info.	-0.188	-0.095
	(0.087)	(0.085)
Edtech info.	0.105	0.107
	(0.060)	(0.057)
General + Edtech info.	-0.005	0.019
	(0.062)	(0.062)
Data + General info.	-0.043	0.017
	(0.091)	(0.087)
Data + Edtech info.	0.006	-0.009
	(0.050)	(0.050)
Data + General info. + Edtech info.	0.098	0.011
	(0.130)	(0.128)
Teacher support	0.023	-0.019
	(0.060)	(0.057)
Teacher support + Data	-0.081	-0.127
	(0.140)	(0.134)
DV mean, control	0.01	0.00
Observations	3433	3433

Notes: Standardized score includes sum of scores on 4 math questions, normalized to the grade-specific control group. IRT adjusted score shows predicted latent ability from full set of math questions, normalized to control group mean (not grade-specific). 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.

Table A.16: 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.	0.080	-0.293	-0.003	-65.836	0.615		
	(0.146)	(0.375)	(0.024)	(80.130)	(13.898)		
Edtech info.	0.066	-0.646	0.045	197.309	-31.178		
	(0.136)	(0.328)	(0.021)	(81.368)	(11.194)		
General +Edtech 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 + Edtech info.	-0.022	0.300	0.005	-130.587	16.769		
	(0.155)	(0.375)	(0.024)	(91.513)	(12.213)		
Data + General info. + Edtech 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.	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)
Edtech 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 + Edtech 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 + Edtech 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. + Edtech 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: All expenses reported in taka. 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.

Table A.17: Impact of outreach on learning resources, lasso

	(1)	(2)	(3)
	Edtech tool	Tech index	Non-tech index
Edtech info.	-0.011	-0.036	0.001
	(0.008)	(0.022)	(0.025)
	[0.288]	[0.125]	[0.604]
Data + Edtech 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: Edtech tool is a binary indicator for whether student used targeted ed tech tool in the past month. The tech-index and non-tech indices are an equally weighted index of binary indicators for whether the student used each of 5 tech-based learning resources or 3 non-tech-based learning resources, respectively, standardized to the control group. 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.

Table A.18: Impact of outreach on parental investment, lasso

	(1)	(2)	(3)	(4)
	Hours parent helped	Private tutoring	Money on tutoring	Money on other education
Edtech 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 + Edtech 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: All expenses reported in taka. 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.

Table A.19: IMPACT OF OUTREACH ON STUDENT LEARNING (MATH), ENDLINE, LASSO

	(1)	(2)
	Unadjusted score	IRT, 2pl
Edtech info.	0.129	0.138
	(0.056)	(0.056)
	[0.048]	[0.048]
Data + Edtech info.	-0.123	-0.108
	(0.061)	(0.063)
	[0.057]	[0.048]
Teacher support	0.185	0.095
	(0.092)	(0.094)
	[0.048]	[0.270]
DV mean, control	0.01	0.00
Observations	3434	3434

Notes: Standardized score includes sum of scores on 4 math questions, normalized to the grade-specific control group. IRT adjusted score shows predicted latent ability from full set of math questions, normalized to control group mean (not grade-specific). Sample includes all Round 2 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.

Table A.20: Impact of outreach on learning resources

	(1)	(2)	(3)
Panel A. All			
	App platform	Tech index	Non-tech index
Edtech info.	-0.015	-0.072	-0.012
	(0.015)	(0.038)	(0.034)
Data + Edtech info.	0.014	0.005	0.012
	(0.014)	(0.034)	(0.028)
Teacher support	-0.022	-0.017	-0.107
	(0.013)	(0.035)	(0.031)
DV mean, control	0.05	0.03	0.03
Observations	2696	2696	2696
Panel B. Low-SES Households			
	Edtech tool	Tech index	Non-tech index
Edtech info.	-0.005	-0.088	0.028
	(0.016)	(0.045)	(0.047)
Data + Edtech info.	0.010	0.009	0.081
	(0.013)	(0.041)	(0.038)
Teacher support	-0.016	-0.028	-0.105
	(0.009)	(0.039)	(0.043)
DV mean, control	0.02	-0.11	-0.03
Observations	1332	1332	1332
Panel C. High-SES Households			
	App platform	Tech index	Non-tech index
Edtech info.	-0.017	-0.049	-0.038
	(0.027)	(0.061)	(0.050)
Data + Edtech info.	0.032	0.007	-0.045
	(0.027)	(0.056)	(0.044)
Teacher support	-0.027	-0.000	-0.104
	(0.026)	(0.061)	(0.049)
DV mean, control	0.08	0.17	0.09
Observations	1355	1355	1355

Notes: Edtech tool is a binary indicator for whether student used targeted ed tech tool in the past 30 days. The tech-index and non-tech indices are an equally weighted index of binary indicators for whether the student used each of 5 tech-based learning resources or 3 non-tech-based learning resources, respectively, standardized to the control group. Sample includes all Round 1 survey respondents who also completed the R2 survey and learning assessment. 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.

Table A.21: 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
Edtech info.	-0.612	0.031	325.004	-16.776
	(0.506)	(0.032)	(149.505)	(18.960)
Data + Edtech info.	-0.220	0.043	76.415	-8.471
	(0.452)	(0.026)	(108.039)	(15.662)
Teacher support	0.846	-0.080	65.696	-0.325
	(0.514)	(0.032)	(135.805)	(18.387)
DV mean, control	6.69	0.68	1163.64	146.31
Observations	2556	2686	2525	2402
Panel B. Low-SES Households				
	Hours parent helped	Private tutoring	Money on tutoring	Money on other education
Edtech info.	-1.147	0.024	97.393	-31.532
	(0.606)	(0.051)	(136.688)	(18.062)
Data + Edtech info.	-0.029	0.075	229.099	-18.885
	(0.640)	(0.040)	(113.287)	(17.350)
Teacher support	0.916	-0.111	-63.691	-10.829
	(0.675)	(0.049)	(105.280)	(22.728)
DV mean, control	5.98	0.62	649.43	77.76
Observations	1260	1325	1265	1180
Panel C. High-SES Households				
	Hours parent helped	Private tutoring	Money on tutoring	Money on other education
Edtech info.	-0.021	0.015	520.628	7.893
	(0.822)	(0.042)	(266.086)	(33.090)
Data + Edtech info.	-0.183	0.007	-90.108	6.157
	(0.694)	(0.038)	(198.014)	(28.470)
Teacher support	0.738	-0.057	46.118	-6.091
	(0.847)	(0.045)	(261.923)	(31.126)
DV mean, control	7.35	0.74	1664.52	211.66
Observations	1285	1352	1252	1214

Notes: All expenses reported in taka. Sample includes all Round 1 survey respondents who also completed the R2 survey and learning assessment. 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.

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. 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.

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, θ.

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 θ. 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 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 $i^m = P \cdot c^p$.[24] Note that we are effectively assuming that student effort

[24] 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, which generates a baseline

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:

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

For ease of interpretation, 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 $i^t = \nu(i^m, \theta)$.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 $\frac{\partial i^t}{\partial i^m}$).

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:

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

The first term is the direct effect of decreasing the costs of external teaching on human capital, which is weakly positive. 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 the elasticity of demand for P with respect to c^p, thus reflecting the net income and substitution effects as a result of

human capital of α. Without loss of generality, we assume that these resources alone do not produce any human capital, $\alpha = 0$.

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 ($\frac{\partial^2 g}{\partial i^t \partial i^m} < 0$).

B.2 Human capital investment through educational technology

We now 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, $H = g(S, E)$.[25] 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 technologies: internal teaching, external teaching, and edtech.

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

[25] 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.

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. Information about a new technology 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 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. 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:

$$\frac{dH}{dc^e} = \overbrace{\frac{\partial g(S,E)}{\partial E} \cdot \left[\underbrace{\frac{\partial E}{\partial c^e}}_{\text{Direct effect}} + \underbrace{\frac{\partial E}{\partial \widetilde{i^e}} \cdot \frac{\partial \widetilde{i^e}}{\partial c^e}}_{\substack{\text{Edtech} \\ \text{investment response}}} \right]}^{\text{Edtech human capital impacts}} \tag{5}$$
$$+ \overbrace{\frac{\partial g(S,E)}{\partial S} \cdot \left[\underbrace{\frac{\partial S}{\partial P} \cdot \frac{\partial P}{\partial \widetilde{i^m}} \cdot \frac{\partial \widetilde{i^m}}{\partial \widetilde{i^e}} \cdot \frac{\partial \widetilde{i^e}}{\partial c^e}}_{\substack{\text{Behavioral response} \\ \text{monetary investment}}} + \underbrace{\frac{\partial S}{\partial \widetilde{i^t}} \cdot \frac{\partial \widetilde{i^t}}{\partial \widetilde{i^e}} \cdot \frac{\partial \widetilde{i^e}}{\partial c^e}}_{\substack{\text{Behavioral response} \\ \text{time investment}}} \right]}^{\text{Teaching impacts}}$$

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.22: Distribution of test scores, by grade

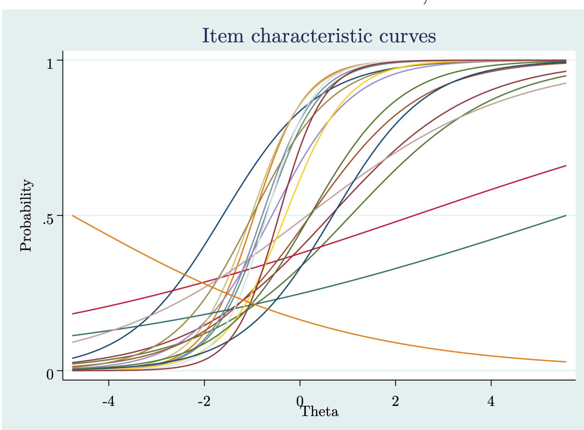
	Math		**Bangla**	
	Zero correct	All correct	Zero correct	All correct
Grade 6	1.9%	6.1%	0.6%	0.3%
Grade 7	4.6%	6.0%	3.0%	1.8%
Grade 8	3.6%	14.0%	2.4%	0.0%
Grade 9	4.0%	2.6%	3.8%	5.3%
Grade 10	3.5%	0.0%	1.5%	8.6%
All	3.7%	5.2%	2.5%	4.0%

C.2.1 Math

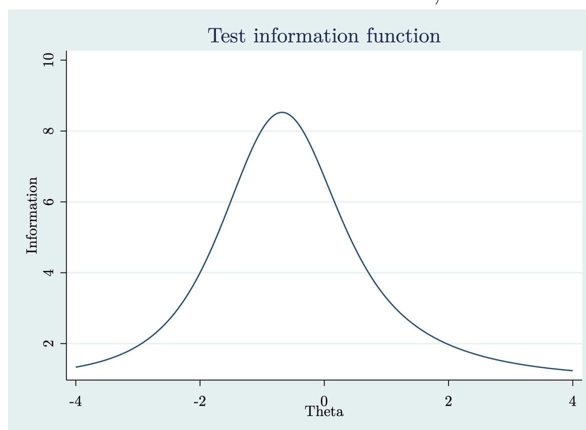
In general, we find that each item has positive discrimination, with well-behaved item characteristic curves:

2-parameter logistic

Item characteristic curve, math

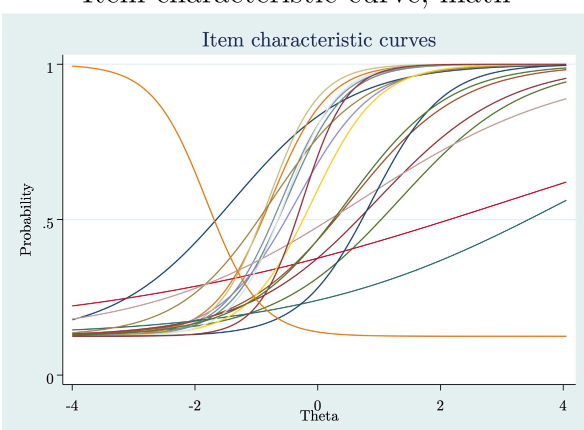


Test information function, math

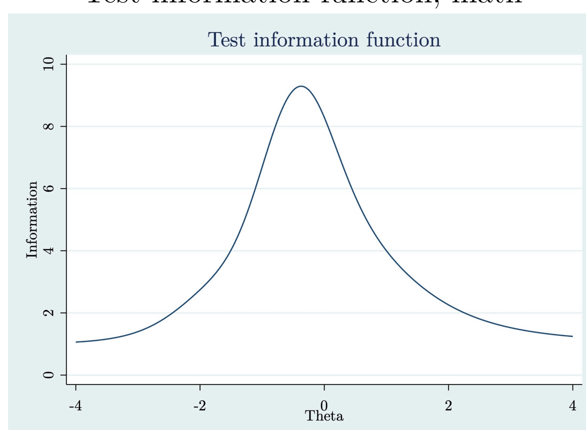


3-parameter logistic

Item characteristic curve, math



Test information function, math

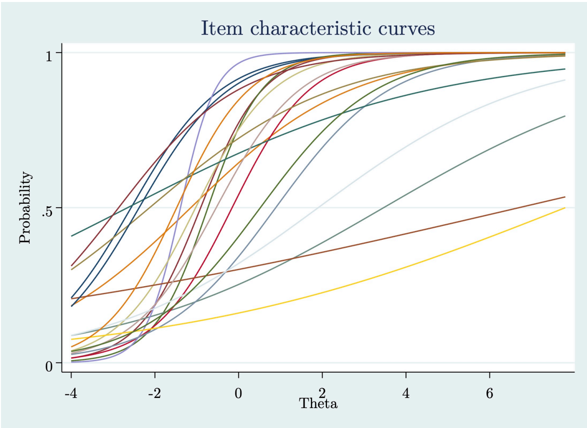


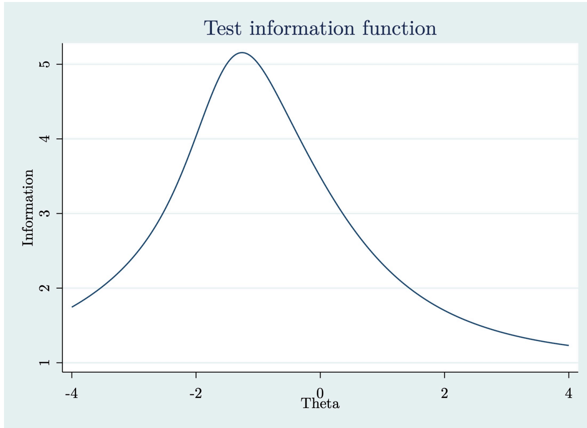
C.2.2 Bangla

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 exclude 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