Boosting Study Habits with High-Frequency Information: A Field Experiment to Aid Disadvantaged Students*

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Abstract

We investigate whether increased parental engagement can enhance students' self-study habits and routine educational participation—disrupted by pandemic-led prolonged school closure. Using a field experiment in rural Bangladesh, we evaluate a high-frequency parent-focused information intervention, delivered via text and voice messages. Our intervention increased parental awareness, children's self-study hours, and narrowed study-hour inequalities—particularly for resource-poor households. Shapley-value decomposition indicates 5–20 percent of reduced inequality stems from the direct treatment effect. Greater parental involvement and increased tutoring investments drive these gains, highlighting scalable, low-cost, and parent-focused nudging can bolster learning continuity during education crisis and recovery phase in low-income settings.

Keywords: High-frequency information, Study hours, Post-pandemic recovery
JEL: D91, H75, I24, I25, O15

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1 Introduction

A widely cited principle in educational research is the importance of "time on task" in influencing student achievement (Asadullah et al., 2021; Hattie, 2008). The amount of time students actively engage in learning—encompassing quality instructional time and structured home study hours, including self-study and homework—can positively influence their academic trajectories (UNESCO, 2016; World Bank, 2016). However, extended school closures during the COVID-19 pandemic introduced unprecedented challenges to students. This was particularly evident in low-income countries, where technologies to compensate for the loss of traditional face-to-face education did not reach those who needed them the most. Consequently, many students missed critical school instructional hours, which disrupted routine self-study activities (Baird et al., 2020; Amin et al., 2021; Booth et al., 2021; Grewenig et al., 2021).

Bangladesh had faced pandemic-related school closures for 1.5 years since March 2020. During this period, the Ministry of Education adopted a multi-modal strategy to deliver educational programs to students through both online (e.g., YouTube and Facebook) and offline (e.g., television programs) platforms (Biswas et al., 2020; Sarwar et al., 2020).[1] However, the actual use of these resources was highly limited, potentially due to the lack of information, motivation, or supervision (Asadullah et al., 2020). Studies conducted during the pandemic find increased time spent on household chores (Baird et al., 2020) and a considerable reduction in study hours (71 percent decrease in daily study hours for girls compared to pre-pandemic levels) (Amin et al., 2021). These concerning findings are linked with greater learning loss, lower grade progression, and potentially lifetime income consequences (Donnelly and Patrinos, 2021).

How can we help students enhance their self-study habits, particularly for disadvantaged students facing widening educational inequalities? This question is particularly salient in the post-pandemic recovery period, as schools reopened and resumed regular academic activities. One promising approach is to position parents at the forefront of academic oversight. Evidence from a randomized controlled trial (RCT) in rural Bangladesh Fujii et al. (2023) shows that high-frequency information (HFI) intervention, centered on school attendance, significantly increased students' academic engagement in the years before the pandemic (2017–19). The program sent weekly SMS and automated voice calls to parents of at-risk secondary students, prompting greater parental involvement. Inspired

[1]Live Television Programs were hosted by Sangsad TV through national TV broadcasting, where academic content relevant to each grade was telecast every weekday at pre-announced time slots. Konnect.com is an additional government-hosted online platform offering video and PDF resources. Appendix Figure A2 illustrates some of these educational resources. There were also attempts to host academic and co-curricular classes for students and live sessions for parents and teachers on different platforms, and students were encouraged to stay in constant communication with their teachers to address study-related queries. These various initiatives, mainly through the use of digital platforms as well as offline sources, were designed to mitigate learning loss during extended periods of school closure (Datta, 2022; Khan et al., 2021; UNICEF, 2020).

by these findings, we designed a novel HFI intervention that covers four key information domains: offline TV education programs, online educational resources, teachers' contact details, and the long-term benefits of continuing education.

We conducted this study among 1,200 secondary school students in Gaibandha—a rural district in northern Bangladesh—during three key phases of pandemic-led education crisis: the school shutdown (closure phase), the partial (soft) school re-starting (reopening phase), and the resumption of regular academic activities (recovery phase). At the baseline, our data show that 60 percent of the sampled students lived in households with television, yet only 39 percent regularly watched educational telecasts. Meanwhile, although all the households had mobile phones, just 33 percent had internet access on those phones,[2] and only a tiny fraction of students had ever viewed educational content online. We find that students spent relatively few hours on self-study, a finding consistent with Amin et al. (2021). During the baseline, we further found that parents were largely unaware of existing educational content across various media platforms. Only 33 percent of students' parents knew about TV-based education programs, and just 9 percent were aware of online platforms offering similar resources. These low levels of awareness are concerning, given that parents serve as a principal source of study-related support for students—a large portion of the students surveyed in baseline reported receiving some help and guidance from their parents over the past year.

With regular schooling being interrupted and limited student-teacher interactions, parents shoulder a greater responsibility for their children's education. Although parental involvement is high, many parents have limited formal education or access to adequate information, making it difficult for them to provide meaningful guidance or supervision. This issue is particularly pressing in Gaibandha, our study region, where adult literacy stands at about 67 percent (BBS, 2022). In such contexts, prolonged school interruptions can widen existing educational inequalities, since students whose parents have limited formal education may not have the knowledge for providing guidance and supervision required at home. Although these parents cannot substitute for teachers, they can provide meaningful learning support by guiding children toward appropriate resources and monitoring their study routines, if equipped with the necessary information. It is therefore essential to ensure that parents are aware of publicly available educational programs and resources to support, guide, and supervise their children effectively.

To address these challenges, we implemented a high-frequency information intervention aimed at encouraging parents to actively monitor and support their children's study habits. The intervention was evaluated through a randomized controlled trial (RCT), in which half of the student-parent pairs were assigned to receive the high-frequency infor-

[2]This internet connectivity is almost entirely accessed through Telco-based pay-per-use mobile internet, and broadband-based internet connectivity (or WIFI connection) is almost non-existent in rural Bangladesh.

mation program (treatment group), while the other half followed their usual routines (*status quo*) without additional support (control group). The intervention primarily aimed to close informational gaps as a mechanism to increase parents' awareness of available educational resources and foster greater parental support—thereby boosting self-study habits.[3]

Our intent-to-treat (ITT) estimate, using the recovery phase follow-up survey, shows that students in the treated households spend 0.53 hours more on self-study-related activities compared to the control mean of 3.53 hours per day (15.01 percent increase), which is highly statistically significant, showing better educational efforts in the treated households. This impact is observed consistently across all sub-samples: boys only, girls only, households with and without access to digital technology, and children of more and less educated parents. However, we observe statistically significant impact heterogeneity for households without access to technology (TV or internet). Specifically, compared to households with such access, households without access to technology experienced a substantially greater increase in study hours (p-value = 0.004). Treated students without [with] access to technology studied 0.84 [0.39] hours more per day on average than their counterparts in the control group. These findings suggest that the intervention may be particularly effective in contexts where alternative sources of educational content or information are limited, helping to reduce the educational inequality faced by disadvantaged students. Furthermore, while the estimated effect on study hours for girls is larger than that for boys, the difference is not statistically significant (p-value = 0.110).

We further examine mobility in average daily study hours, where upward mobility and downward mobility, respectively, refer to an increase and decrease in a student's relative position in the study hour distribution compared with the control group. We find that the intervention led to a significant improvement with a 13.53 percentage point improvement in upward mobility (p-value = 0.000) and a 13.36 percentage point reduction in downward mobility (p-value = 0.000), and the results remain similar even when we examine the changes in quantiles beyond different thresholds. These effects were particularly strong for females, households without technology access, and households where no parent had attended school, highlighting the potential of such interventions to address structural inequalities in education access.

Our main mechanism, as hypothesized, is greater parental awareness on educational resources. We find that HFI intervention significantly increased parental awareness by 23 percentage points (p-value = 0.000). This impact remains robust in different sub-samples: boys only, girls only, households with and without access to digital technology, and children of more and less educated parents. Interestingly, we find that households lacking

[3]Our primary outcome variables of interest are study hours and viewership of educational programs as registered in the pre-analysis plan. Another primary outcome related to teachers and instructional practices is beyond the scope of this current paper and is reserved for a separate, dedicated analysis.

access to television or the internet exhibit substantially greater increases in awareness compared to those with such access (p-value = 0.000). Additionally, parents in the treatment group were significantly more likely to guide their children towards both online and offline educational programs for study support, an effect observed consistently across the entire sample and all sub-samples, with no evidence of heterogeneity. We also document a statistically significant increase in resource allocation for private tutoring (7 percentage points more than the base of 76, p-value = 0.000), particularly benefiting treated girls and households with at least one parent who had attended school. Nevertheless, we find no significant heterogeneity across other observable attributes. To underscore the importance of awareness, we perform a simple mediation analysis and find that about 37 percent of the total treatment effect on study hours could be attributed to the awareness channel.

To assess how the treatment effect contributed to the observed reduction in study-hour inequality, we employ a regression-based decomposition using the Shapley-value approach. To our knowledge, this is the first study to conduct a regression-based inequality decomposition analysis in an experimental setting. This method enables us to decompose changes in the Gini coefficient of study hours—a measure of inequality that captures how study time is distributed among students—into components attributable to the treatment effect and broader time trends. Our results indicate that, depending on the chosen inequality metric, 5–20 percent of the decline in study-hour inequality can be directly attributed to the intervention. Similar patterns were observed when applying alternative inequality measures, reinforcing the robustness of these findings.

The intervention implemented in this study is automated, leverages readily available low-cost mobile technology, and draws on publicly accessible educational resources. In our study region, 91 percent of households own mobile phones, and sending an SMS or making a one-minute voice call costs roughly \$0.01, enabling high compliance rates (i.e., high receipt of text messages and voice calls). This approach offers a cost-effective solution. Our intervention is the first study aimed at boosting the study habits of students in the post-pandemic recovery phase and helping them return to their pre-pandemic educational trajectories. The intervention is rapidly scalable and can be adopted in other low- and middle-income countries in Asia and Africa with similar infrastructure.[4]

A reasonable concern in our study is that estimates are based on parents' reported study hours, which may be influenced by experimenter demand effects. However, our parent reported study-hour data collected across different survey rounds show clear seasonal fluctuations tied to exam cycles and parallel patterns across treatment and control

[4]Although mobile phone penetration is high, most households own only basic feature phones, particularly in rural areas; SMS and voice calls still work effectively on these devices. The number of smartphone users in Bangladesh is expected to grow; however, as of 2020, only 41 percent of mobile phone users had smartphones (Bhuiyan, Mohsin, 2021). Similar trends can be found in other developing and less developed countries worldwide (Cheney, Catherine, 2018; Silver and Johnson, 2018).

groups, which is inconsistent with uniform inflation, driven by demand effects. Second, both treatment and control households show similar seasonal shifts (students study more during exam months and less in non-exam periods), suggesting that reporting reflects genuine behavioral changes rather than strategic misreporting. A second concern is whether our high-frequency messaging inadvertently increased children's exposure to online entertainment or gaming platforms by encouraging the use of digital resources. This is unlikely in our context. Internet access in rural Gaibandha is almost entirely based on pay-as-you-go mobile data plans from telecom operators, rather than on broadband or Wi-Fi connections. The marginal cost of mobile data is high relative to household income, and households in our sample are socioeconomically marginalized with minimal discretionary spending capacity.

In our study, we did not detect any discernible impact of study hours on learning outcomes, though this may reflect limitations of the available learning measures.[5] Following COVID-19 disruptions, Bangladesh's education authorities shortened the secondary school syllabus in 2022 and shifted from subject-specific examinations to a more flexible "promoted/not promoted" system, reducing our ability to assess subject-level performance changes. In our own data, the brief, unincentivized ten-item mathematics test—originally intended for baseline balance checks—shows no statistically significant treatment effects. These null results may reflect the short and low-stakes nature of the test, the slow emergence of learning gains (Brisson et al., 2017), or the genuine absence of short-term effects of increased study hours on learning performance. Nonetheless, fostering better study habits during the post-pandemic recovery period remains a critical first step toward improving self-study behavior and possible long-term learning outcomes.

We contribute to three strands of the literature. First, we add to the growing body of work documenting the educational consequences of prolonged school closures during the COVID-19 pandemic. Extended periods of school closure during the pandemic impeded consistent study routines, leading to significant learning losses (Baird et al., 2020; Amin et al., 2021; Booth et al., 2021; Grewenig et al., 2021) and difficulty in re-establishing routine educational activities when schools reopened (Betthäuser et al., 2023). Existing strands of literature have focused on learning losses and the resulting economic impacts (Engzell et al., 2021; Hanushek and Woessmann, 2020; Makino et al., 2021), while other studies have aimed to find ways to alleviate learning loss through remedial online or mobile lessons and telementoring (Angrist et al., 2020; Clark et al., 2021; Hassan et al., 2024). Our research departs from these studies and looks at reducing pervasive information gaps about publicly available remedial educational contents that are already in place, and thereby improve study habits in the post-pandemic recovery period when schools were beginning to operate but not at full capacity. Thus, we offer an affordable way of exploiting existing resources to help children from disadvantaged backgrounds to

[5]Registered as a secondary outcome in the pre-analysis plan.

reestablish previous study routines and potentially recuperate from learning losses.

Second, our study informs targeted policy design and advances understanding of how to reduce educational disparities, especially in a post-pandemic recovery context. The pandemic not only disrupted study habits but also amplified pre-existing disparities in educational outcomes between students from marginalized households and those from better socioeconomic backgrounds. Children from wealthier families often had greater access to digital technologies, private tutoring, and conducive learning environments, enabling them to better adapt to remote learning (Di Pietro et al., 2020). In contrast, students from disadvantaged households faced significant barriers, including lack of internet access, fewer parental resources for academic support, and increased economic pressures that diverted attention away from education (OECD, 2021). Addressing such inequalities has become a critical focus in educational policy and research. Previous interventions targeting marginalized students have primarily focused on expanding access to educational resources and reinforcing parental engagement to help overcome these challenges (Alon, 2007; Castleman and Page, 2015; Fack and Grenet, 2015; Lai et al., 2015). However, the heterogeneity of outcomes based on gender, access to technology, and parental education remains underexplored. Our study directly addresses this gap by analyzing the differential effects of information nudges on these dimensions. For instance, we assess whether access to a television or the internet mediates the impact of our interventions or whether parental education levels influence the efficacy of these nudges.

Finally, our paper contributes to the literature on closing parental information gaps regarding their children's education through various channels, including parent-teacher meetings, emails, phone calls, and text messages (Avvisati et al., 2014; Barrera-Osorio et al., 2020a,b; Bergman, 2021; Bergman and Chan, 2021a; Berlinski et al., 2021; De Walque and Valente, 2018; Dizon-Ross, 2019a; Kraft and Dougherty, 2013; Kraft and Rogers, 2015; Rogers and Feller, 2018a; York et al., 2019). Previous studies in this area largely consist of interventions such as sending parents individualized updates on students' behavior, attendance, and test scores, or providing information on the benefits of education, and have mainly been carried out in developed countries. Our study differs from the existing literature in terms of culture, sophistication of widely available technologies, and average income and educational levels of the household, among others. Thus, our paper adds to a relatively thin evidence base on the importance of parents' role in their children's education in an economically marginalized and challenging rural setting in South Asia.

The rest of the paper is organized as follows. Section 2 describes the context and the study design, including a discourse on data, power calculations, randomization balance checks, and HFI compliance rates. Section 3 presents the main identification strategy, Section 4 explains the microfoundation and Section 5 presents the results. Section 6 performs a decomposition analysis and Section 7 concludes.

2 Research Setting

2.1 Background

Bangladesh experienced one of the world's longest COVID-19 school closures, lasting about 18 months until reopening in September 2021 (UNICEF et al., 2021). Even after reopening, schools operated on a highly restricted schedule (Mahmud, 2021), with most grades attending in-person classes only one day per week throughout late 2021 and a considerable part of 2022 (The Daily Star, 2021a). Students themselves reported that such limited in-person instruction offered "*class... once a week, which had no benefit*" and led to declining motivation and difficulty maintaining study routines (The Financial Express, 2022). Assessment systems were also heavily modified: students were promoted using abbreviated syllabi, reduced-subject terminal examinations (such as the Secondary School Certificate, SSC), and widespread automatic grade promotion (otherwise known as "auto-promotion") during 2020–2021 (The Daily Star, 2021b).

Learning recovery was further disrupted in 2022 by major floods that forced school closures in affected districts and delayed the SSC examinations (Al Jazeera, 2022), by teacher protests and exam-center shutdowns, and by extended religious holidays that reduced instructional time (The Daily Star, 2022). Terminal exams such as SSC examinations continued to be administered on abridged content in 2022, with fewer subjects and reduced marks per subject (Bonik Barta, 2022). For younger students, traditional annual examinations were largely suspended, and most primary and secondary students were promoted based on internal assessments or general promotion policies rather than standardized year-end tests (Dhaka Tribune, 2021; The Business Standard, 2021). In practice, many students received a simple "promoted" status without sitting formal exams, unless a school independently organized a limited assessment. These overlapping disruptions meant that, even after school reopening, students continued to face significant instability in schooling and persistent barriers to reestablishing regular study habits.

Our intervention took place in a relatively poor agricultural district, Gaibandha, located in northern Bangladesh. Gaibandha is a rural district where educational attainment has been historically low. It ranked 57^{th} in the literacy rate among the 64 districts in the country, where adult literacy stands at about 67 percent (BBS, 2022). Similarly, 48 percent of the population was below the poverty line in Gaibandha, which is much higher than the national average of 32 percent (World Bank, 2020). Agriculture is the primary occupation for around 71 percent of the working population in this district. Since the opportunity cost of attending school might be relatively high for children from agricultural households, the district of Gaibandha presents a setting where children may be highly susceptible to dropping out of school due to repeated school interruptions.

2.2 Experimental Design

The trial is approved by SMU-IRB (IRB-21-064-A059(421)) and FIU-IRB (IRB-21-0164) and was pre-registered in the AEA RCT registry AEARCTR-0007674. We document and discuss all deviations from the pre-registered analysis plan in detail in Appendix A.5.

We first collected a detailed student roster from the four participating secondary educational institutes in our study area after obtaining consent from the school headmasters (see Appendix A.1 for details on school selection). We randomly kept only one child per household in the roster in order to maintain analytical tractability and to avoid the possibility of within-household spillovers and confusion about the messages sent to the households. Enumerators subsequently visited each household to obtain parental consent following our pandemic-related survey protocol. Students whose households lacked a valid mobile phone were excluded from the sampling frame. From this refined roster, we selected 1,200 students enrolled in grades 6–10, stratifying by school, grade, and gender.

Once consent was secured, we completed baseline data collection and then randomly assigned students to one of two experimental arms: (i) a treatment group whose households received weekly informational SMS messages (masked to be read in Bangla) and automated voice calls (henceforth the High-Frequency Information, or HFI, arm); and (ii) a control group that received neither SMS nor voice calls. The content delivered to households in the HFI arm varied weekly and covered four categories of information:

(I1) Schedules for viewing grade-appropriate educational television programs.

(I2) Instructions for accessing educational content on digital platforms such as YouTube.

(I3) Contact details for English and Mathematics teachers at the class and subject level.

(I4) General messages highlighting the long-term benefits of continued schooling.

The intervention was designed to increase parental awareness of televised and online educational resources, strengthen communication channels between teachers and households, and reinforce the perceived value of ongoing education. Messages were partially customized: for (I1) and (I2), content varied by the student's grade; for (I3), by grade-section; and for (I4), all treatment households received the same message.[6] As described in Appendix A.2, the minimum detectable effect size for our RCT design is 0.12 standard deviations (with significance level 0.10 and power 0.80), indicating adequate sensitivity to detect moderately sized impacts. We carefully structured our information intervention to ensure that no information was withheld from the control group, and instead, we aim to reinforce only publicly available information for the HFI treatment arm. Table A1 in

[6] As part of the intervention, we also administer an incentivised short SMS quiz to all study participants at the end of each month, which participants have to send their answers within a day of receiving the quiz questions. However, the response and compliance rate of these quizzes were extremely low to infer anything meaningful, hence we refrained from using this data for our analysis.

Appendix A.3 provides examples of the SMS and voice calls sent to households in the information treatment arm.

The HFI intervention started in November 2021 and continued until December 2022, spanning the period of school closures due to the Omicron surge, as well as the gradual reopening and eventual return to regular schooling. A midline survey (during the intervention continuation) was conducted in June–August 2022, followed by an endline survey in January–February 2023 (immediately after the intervention was completed) and a phone-based follow-up survey (8 months after the intervention) in August-September 2023. All surveys collected our main outcome of interest (study hours) and collected additional household-level information, including the availability of digital technology, educational resources, and parental awareness. Baseline and Endline surveys also administered a short math assessment. Since the focus of our study is improving the self-study activities, particularly in the recovery phase, most of our analysis is based on the follow-up survey, along with the learning assessment conducted at the endline survey. Figure A1 in Appendix A.3 illustrates the timeline of our study.

2.3 Treatment Compliance

The treatment compliance was not perfect in our study. First, we had some technical limitations in the delivery of SMS and voice calls. That is, the SMS we attempted to send was not always successfully delivered, and the voice calls we made were not always picked up. Nevertheless, the rates of successful SMS and voice call deliveries were very high, as Table 1 shows. Almost all households in the treatment group received an SMS in most of the 52 weeks of intervention, and voice calls were received by 79 percent of the treatment households on average.[7]

Second, the rate of successful delivery only indicates the proportion of households that received SMS or voice calls, but it does not reflect the proportion of households that read the SMS or listened to the voice calls. To address this issue, we consider a self-reported compliance indicator that takes the value of one if the respondent in the follow-up reported that the respondent's household member had received any SMS/voice call on education content from our partner organization in the past year and zero otherwise. We instrument this variable with the treatment assignment indicator to obtain the treatment effect on the treated. It is worth noting that there was no evidence of treatment leakage, as those households that reportedly received the SMS or voice call were all in the treatment group.[8]

[7]The imperfect compliance could have occurred for a variety of reasons, such as invalid phone numbers (because the phone number had changed since the time of recruitment, for example) or the phone being switched off (for an extended time, in the case of SMS). As Table 1 shows, there were notable drops in compliance rates in Weeks 10, 20, 23, and 24 for SMS and Weeks 3, 4, and 12 for voice calls. These were due to implementation issues, such as system-wide failure in mobile network connectivity.

[8]Sending both SMS and voice calls together is associated with a strong and statistically significant increase in study hours, while sending only SMS also shows a marginally higher positive and significant

2.4 Measurement of the key outcomes

2.4.1 Measurement of Study Hours

Our primary outcome of interest is the number of hours children spend on self-study, as reported by parents, following the approach used in Stevenson et al. (1986); Booth et al. (2021); Grewenig et al. (2021). Our reliance on parent-reported study hours aligns directly with the intervention's target audience—parents—and provides the most consistent and comprehensive data across all survey rounds. Parent reports were collected at baseline, endline, and the follow-up survey, enabling a richer measurement timeline and greater variation for capturing changes in children's study behavior over the course of the intervention.[9]

At baseline (August 2021), parents reported relatively low study hours, consistent with the reduced academic pressure following the mid-year examinations. By the endline (November 2022), they reported substantially higher study hours, reflecting the increased academic intensity preceding high-stakes grade progression final exams. This temporal pattern is consistent with expected seasonal fluctuations in educational engagement. The follow-up survey (September 2023) collected parent-reported study hours over a full year, allowing us to examine longer-run patterns in children's study behavior after the intervention had concluded. This recall measure offers additional insight into sustained educational engagement, which is central to our analysis. Table 2 summarizes the parent-reported study hours across the three survey rounds.

If parents in treated households perceived an expectation to report higher study hours, this could bias our results upward. However, two features of the data potentially mitigate this concern. First, we observe substantial intra-household temporal variation in reported study hours that aligns closely with exam preparation cycles: students study more during exam months and less in non-exam periods. If demand effects were driving the responses, we would expect inflated and relatively uniform reporting across the calendar rather than systematic fluctuations tied to academic schedules. Second, treatment and control households exhibit parallel shifts during exam-sensitive periods, suggesting that reporting behavior reflects genuine changes rather than strategic overstatement.

One limitation we encounter is the inability to objectively measure the duration of educational television and online media exposure. Such measurement would require technological infrastructure—such as account-based subscription data or device-specific

effect. Voice calls alone do not have a meaningful impact on study hours.

[9]Students also self-reported their study hours at baseline and endline. The correlations between student-reported and parent-reported study hours are high at 0.521 and 0.439 for the baseline and endline, respectively. However, these self-reports exhibited inconsistencies and were less sensitive to expected seasonal variation in academic activity. Moreover, because students may have incentives to under- or over-report their study time, their responses are more prone to misreporting. Given that the intervention was designed to influence parental awareness and behavior and that parent reports were collected across all three rounds, we use parent-reported hours as our primary outcome.

tracking—that was not available or feasible in our research setting. Even if these data were accessible, it would remain difficult to determine which household members actually engaged with the content. Moreover, participants could access educational programs through neighbors' televisions or utilize online resources via devices belonging to other family members, friends, or neighbors, further complicating the establishment of precise exposure metrics. This limitation underscores the broader methodological challenge of accurately capturing media consumption in contexts where shared access to technology is common.

2.4.2 Measuring Mobility

While the level of study hours is our primary interest, the gap in study behavior among students is also of interest. Even if our intervention increases the study hours, the implications of our intervention for inequality will depend on whose study hours have increased by our intervention. Since our intervention intends to help disadvantaged students study harder by addressing the information gap, we expect to see higher upward mobility and lower downward mobility relative to the distribution of the study hours for the control group. Thus, to measure the mobility of students in the distribution of study hours, we first find the quantiles of study hours for each individual in the control-group distribution at the time of observation.

To this end, we sort the control observations from the lowest to the highest regarding the average number of study hours per day.[10] Without the loss of generality, we can let $y_1^C \leq y_2^C \leq \cdots y_{N_C}^C$, where y_i^C is the ith smallest observation of daily study hours in the control group and N_C is the number of control observations. If y_i has no ties, we assign the value of quantile $q_i = (i - 1/2)/N$. When y_i has $j(> 1)$ ties (i.e., there are j observations of y_i in the control group), we take the average of $(i - 1/2)/N_C, (i + 1 - 1/2)/N_C, \cdots, (i + j - 1 - 1/2)/N_C$ and assign $q_i = (i + j/2 - 1)/N_C$ for the quantile of y_i.

Next, we map each treatment observation to the quantile in the control distribution. If a given treatment observation y^T has a corresponding observation i in the control group such that $y^T = y_i^C$, we map this treatment observation to q_i. If a treatment observation is smaller than y_1^C [greater than $y_{N_c}^C$], we assign a quantile value of $q = 0$ [$q = 1$]. Finally, if a given treatment observation y^C satisfies $y_i^C < y^T < y_{i+1}^C$ for some i in the control observation, we linearly interpolate and assign $(i - 1/2)/N + (y^T - y_i^C)/(y_{i+1}^C - y_i^C)$ as the quantile value for this observation. Based on this process, we assign a quantile in the control distribution to each y_i^g for treatment group $g = \{C, T\}$ and individual i.

Therefore, for each individual in control and treatment groups, we have the quantile of study hours for each of the baseline and follow-up surveys, which we denote by q_{it}, where

[10] We ask questions about the daily study hours for typical weekdays and weekends. We give a weight of 5/7 and 2/7 to the former and latter, respectively, to obtain the daily average study hours.

$t = 0$ and $t = 1$ respectively represent the baseline and comparison period, where the latter of which will be the follow-up. We define upward mobility and downward mobility as follows

$$UM_i^0 = \mathbf{1}_{(q_{i1} > q_{i0})}, \quad DM_i^0 = \mathbf{1}_{(q_{i1} < q_{i0})}, \tag{1}$$

Because we have ties and measurement errors, we may be interested in large enough changes. Therefore, we also consider the following upward and downward mobility measures, where $a > 0$ is a given constant:

$$UM_i^a = \mathbf{1}_{(q_{i1} > q_{i0}+a)}, \quad DM_i^a = \mathbf{1}_{(q_{i1} < q_{i0}-a)}, \tag{2}$$

We use UM^a and DM^a for $a \geq 0$ as mobility measures for our purpose.

2.5 Randomization Balance

The control and treatment groups should have the same expectation for both observable and unobservable characteristics due to the randomization. To check for the potential violation of randomization, we conduct a t-test of equality of means for several individual and household-level observable baseline characteristics such as study hours, gender of the student, education level of household head, availability of television or internet at home, number of household members, number of male members, number of female members, and ownership of household assets such as agricultural land, radio, television, bank account, NGO account, and bicycle, and baseline test score. As Table 3 shows, nearly all the observable characteristics and, more importantly, study hours are similar between the control and treatment groups. A notable exception is bicycle ownership, which is controlled for in our empirical analysis as detailed in Section 3.

The overall sample reflects a socioeconomically modest population. Slightly more than half of the participating children are female (57%), and parents report that children study an average of 2.29 hours per day at the baseline. Households are medium-sized, averaging 4.53 members with a near-equal distribution of males and females. Educational attainment among household heads is low: over one-third have no formal schooling, and the remaining two-thirds are split between primary and secondary education levels. Asset ownership patterns suggest modest living standards—although nearly all households own some agricultural land, access to consumer durables and financial services is mixed, with 60% owning a television, 23% having a bank account, and 45% maintaining an NGO/MFI-linked account; about 70% report having either a television or internet at home. Children's baseline academic performance is moderate, with an average math score of 5.22, measured using a math test questionnaire developmed by the research team (reported in Table A5 of Appendix A.3.

It is important to note that, due to a prior study (Fujii et al., 2023) conducted in

the same area, approximately 12% of participants in our sample also participated in that earlier intervention. However, our randomization checks confirm that participation in the previous study is evenly distributed across treatment and control groups in the current experiment, indicating that earlier exposure is unlikely to bias our estimates.

3 Empirical Strategy

3.1 Intention-to-treat (ITT) effect

We use a simple intention-to-treat analysis to test the impact of our intervention on our main outcome of interest—study hours. We denote the outcome of interest for student i at time t by y_{it}, where $t = 0$ and $t = 1$ respectively denote the baseline and endline/follow-up surveys. As we are most interested in the persistent effect of the treatment, we primarily use the follow-up survey for $t = 1$, but we also consider the endline survey for $t = 1$ as an alternative for some estimates.

We denote the unbalanced covariate at the baseline (ownership of bicycle) by X_{i0} and the strata-specific fixed effect by $\mu_{s(i)}$, where $s(\cdot)$ is a mapping from individual to the stratum that the individual belongs to. T_i denotes the treatment assignment indicator, which takes unity if individual i is assigned to the treatment group and zero otherwise. Using these notations, our primary specification is as follows:

$$y_{i1} = \alpha_0 + \alpha_1 T_i + \alpha_2 \mathrm{X}_{i0} + \mu_{s(i)} + \epsilon_{i1}, \tag{3}$$

where α_1 is the coefficient of primary interest. We report standard errors clustered at the strata level. We additionally report multiple hypothesis testing by presenting Anderson's sharpened q-values that adjust for the false discovery rate. We use the same econometric specification to study the impact of our intervention on upward and downward mobility and other outcomes. Additionally, we use an ANCOVA specification to control for the baseline measure of the outcome variables of interest. The results are qualitatively and quantitatively similar and are available upon request.[11]

Because our interest is not only in the level of the study hours, but also in whether our intervention can narrow the gap between disadvantaged students and the rest, we examine the impact heterogeneity of our intervention. Specifically, we study the impact heterogeneity along the following three important dimensions—gender (boys vs girls),

[11] We also examine the impact of the intervention on reported study hours at the endline, but we do not observe any statistically significant effects. This pattern is plausibly explained by two factors. First, the endline recall period overlapped with grade-progression examinations, during which students in both treatment and control groups were studying intensively and in similar ways. Second, households may have needed more time to fully internalize the intervention, especially since the endline survey was conducted very close to the completion of the program. These supplementary estimates are available upon request.

accessibility to digital technology (households with access to television or internet versus households with access to neither), and the education level of parents (parents who attended school versus parents who were never enrolled in school).

3.2 Treatment effect on the treated (TOT)

As the treatment compliance was not perfect, we also estimate a variant of eq. (3) by instrumental variables regression with T_i replaced with the endogenous compliance indicator. This indicator takes one if the respondent in the follow-up reported that the respondent's household member had received SMS or voice calls, and zero otherwise, and it is instrumented by T_i. Here α_1 represents the treatment effect on the treated (TOT).

We also acknowledge that there may be potential spillovers from the treatment to the control group, given that the information we send by SMS can be easily shared with the control group. While this is a possibility, we argue that the scope of such spillovers is likely to be limited during our intervention, as the level of interactions among parents is generally limited. If anything, our estimates would represent the lower bound of the actual treatment effect.

3.3 Differential Attrition Between Treatment Arms

Our impact estimates could be biased if the attrition rates systematically differ between the information treatment and control arms. While 62 household students discontinued the study by the time of the endline survey, a total of 122 student households attrited at the time of follow-up. This attrition could have occurred due to school dropout or migration. We test for systematic attrition between the two experimental arms using eq. (3), and find that the attrition rates are statistically indistinguishable between the two arms (Table A2 in Appendix A.3).

4 Microfoundational Framework

We present a simple microfoundation to interpret how high-frequency information (HFI) delivered to parents can affect children's self-study behavior. The framework is intentionally stylized and focuses on *study time* as the primary outcome; we do not model a time-based budget constraint. Instead, we treat study hours as an input generated by the interaction between parental engagement and children's effort.

In each period $t = 1, \ldots, T$, parent i chooses an engagement level $e_{it} \geq 0$ (e.g., monitoring, reminders, guidance toward learning resources, or arranging tutoring) at a private cost. Children respond by choosing study hours $s_{it} \geq 0$. We assume a reduced-

form study response function

$$s_{it} = \phi(e_{it}, A_i, \eta_i), \qquad \frac{\partial \phi}{\partial e} > 0, \tag{4}$$

where A_i captures baseline access to educational technology (television or internet) and η_i captures child-specific traits (e.g., ability or intrinsic motivation).

A central friction is that parents may underestimate the returns to engagement or face high activation costs due to limited awareness of available educational resources. This is consistent with evidence that parent–child information frictions can depress human capital investments (Bergman, 2021; Dizon-Ross, 2019b). HFI can relax this constraint by (i) increasing the perceived returns to engagement (e.g., salience of study and the value of continued schooling), (ii) lowering engagement costs by making costly information acquisition cheaper and providing actionable guidance (e.g., schedules and access instructions), and/or (iii) increasing the productivity of engagement in generating study hours. Related evidence shows that low-cost high-frequency messaging to parents can meaningfully affect student behavior and outcomes (Bergman and Chan, 2021b; Rogers and Feller, 2018b; Berlinski et al., 2021; Fujii et al., 2023).

This framework yields three testable predictions. First, HFI increases parental engagement and thereby raises children's study hours, i.e., $\partial s_{it}/\partial T_i > 0$. Second, HFI increases parental awareness and guidance toward educational inputs, which serve as mechanisms through which engagement rises. Third, the effect is larger when baseline information constraints bind more tightly—in particular among households without access to television or the internet—because the marginal value of new information and the marginal product of parental engagement are higher in these settings. The full micro-foundational framework, including equilibrium conditions and comparative statics, is presented in Appendix A.4. We empirically evaluate these predictions in the next Section 5.

5 Results

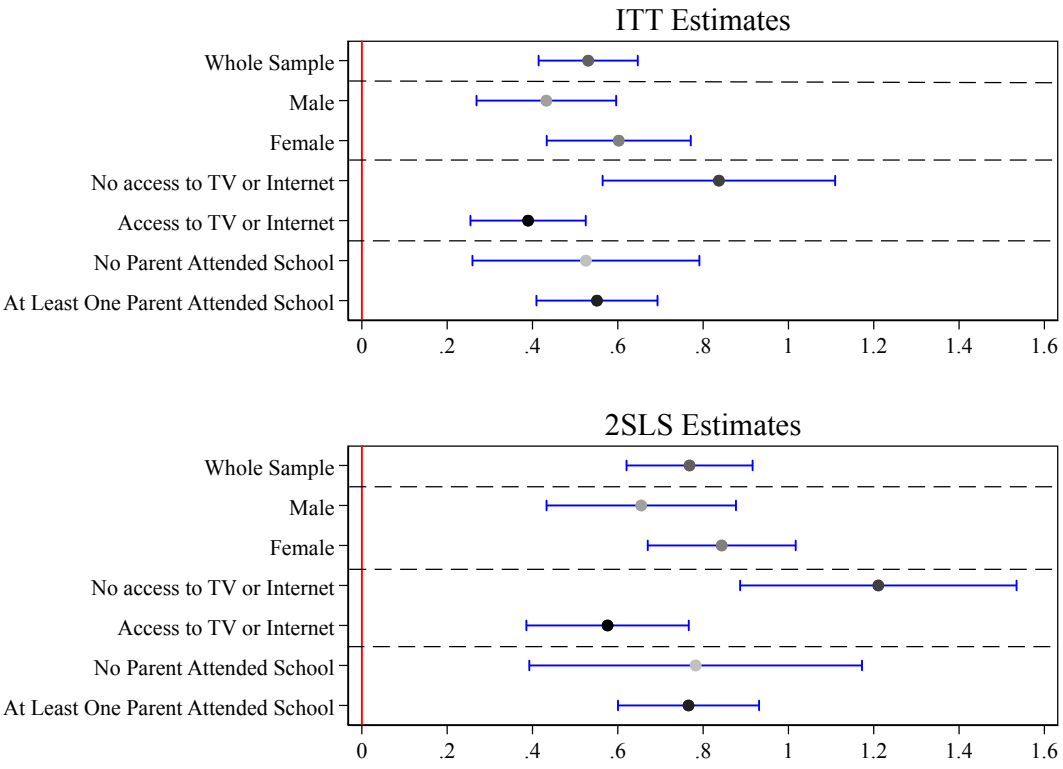
5.1 Study Hours

5.1.1 Level of Study Hours

The intervention resulted in a significant increase in study hours among the treated group, with the effects being observed consistently across all sub-samples, as reported in Figure 1 (and estimates are reported in Table 4). While the mean study hours for students in the control arm were 3.53 hours per day, students in the treatment group on average studied 0.53 hours more in the follow-up survey, and this difference is statistically significant at the 1 percent level (reported in the upper panel of Column (1) in Table 4). This impact pattern has been consistently observed across subgroups: gender (columns (2)-(3) of the

upper panel), households with and without access to digital technology (columns (4)-(5) of the upper panel), and children of more and less educated parents columns (6)-(7) of the upper panel).

Figure 1: Impact of Treatment on Study Hours



Note: The above estimates are derived from the follow-up survey conducted with 1,077 parents. The outcome variable in the above figure is a continuous variable measuring daily study hours in the past year. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. The 95% confidence intervals are based on the standard errors clustered at the school-grade-gender level.

We further look at the impact of our intervention on the treated (reported in the lower panel of Table 4). To estimate the treatment-on-the-treated (TOT) effects, we asked the parents in the follow-up survey whether any member in the household received SMS or voice calls on education content from our partner organization in the past year. Though self-reported, all households in the treatment arm reported receiving the intervention, while none in the control arm reported the same, indicating perfect compliance. Leveraging this compliance, we employ a two-stage least squares (2SLS) regression, instrumenting compliance with treatment assignment. The 2SLS estimates reveal significant increases in study hours across the entire sample as well as in various subgroups. These TOT estimates are larger than the intention-to-treat (ITT) effects, as expected, since they reflect the impact of the intervention on the subset of households that complied with the treatment. To further check the robustness of our findings, we also pooled baseline,

endline, and follow-up surveys together to estimate the average treatment effect, which also shows a similar impact (reported in Table A3 in Appendix A.3).

Consistent with these main findings, Table A4 in Appendix A.3 shows that higher exposure to the intervention—measured through successful receipt of SMS or voice calls—also translates into meaningful improvements in children's study hours. Households that successfully received the weekly SMS or voice-call reminders reported substantially more study time for their children over the past year. The estimates indicate that higher SMS or voice-call delivery success (as the proportion of weeks in which a household successfully received the treatment) positively increases daily study hours, mirroring the magnitude of the ITT effects (reported in columns (4)-(8)). These results are robust across specifications and remain strongly statistically significant. Overall, the evidence suggests that the degree of actual message delivery, and thus the intensity of exposure to the information nudge, plays an important role in reinforcing the intervention's positive impact on study behavior.

It is also worth noting that our intervention appears to be gender-neutral. While the estimated effect on study hours is somewhat larger for girls than for boys, the difference is not statistically significant (p-value = 0.110). We also find no significant difference in treatment impacts by parental schooling. These findings suggest that closing gaps in study behavior stems more from differences in technology access than from factors like gender or parental educational background. Consequently, this has important implications for how study hours may evolve—an issue we now turn to.

However, we observe statistically significant impact heterogeneity only in households with access to technology. Specifically, households without access to television or the internet experience substantially greater increases in study hours compared to households with such access (p-value = 0.004)—treated students without access to technology studied 0.84 hours more per day on average, whereas the ones without access to such technology studied 0.39 hours more per day compared to their counterparts in the control group. These findings suggest that our intervention is particularly effective when students have limited access to educational information or content. Heterogeneity analysis is not reported in the current manuscript but is available upon request.

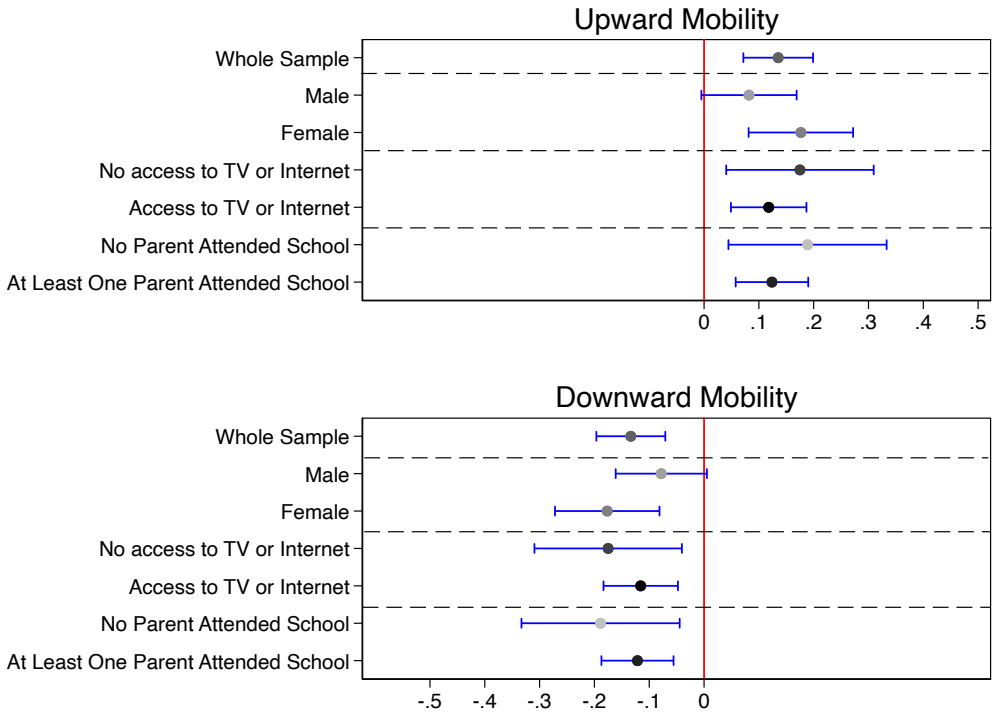
5.1.2 Mobility in Study Hours

We start the mobility analysis following the definition introduced in Section 2.4.2: UM^0 (upward mobility) and DM^0 (downward mobility). We document that our intervention led to a significant upward mobility in study hours for the treated sample, alongside a marked reduction in downward mobility (reported in Figure 2). More specifically, we observe a significant 13.53 percentage point improvement in study behavior in upward mobility (p-value = 0.000) and a 13.4 percentage point reduction in downward mobility

(p-value = 0.000) for students in treated households (reported in column (1) of both upper and lower panels of Table 5). These effects were particularly strong for females (reported in column (3)), households without access to television or the internet (column (4)), and households where no parent had attended school (column (6)), highlighting the potential of such interventions to address structural disparities in education access.

Since Figure 2 does not consider the changes in quantiles, we also vary the value of "a" and re-run similar regressions, dropping the observations that are within the share "a" among the top [bottom] of the control distribution at the baseline. We drop these observations because UM^a [DM^a] is equal to zero for such observations by definition. As shown in Figure A3 in the Appendix A.3, we see that the treatment significantly increases upper mobility and decreases downward mobility for a wide range of values of "a".

Figure 2: Impact of Treatment on Mobility



Note: The above estimates are derived from the baseline and follow-up survey conducted with 1,072 parents. The outcome variables are mobility measures based on study hours at the baseline and follow-up. "Upward mobility" ["Downward mobility"] are indicator variables that take a value of 1 if the student moved up [down] in the control distribution of average daily study hours between the baseline and follow-up surveys, as defined in eq. (1). The specifications control for unbalanced covariates at the baseline—ownership of bicycle—and school-grade-gender fixed effects. The 95-percent confidence intervals are based on the standard errors clustered at the school-grade-gender level.

Heterogeneity analyses reveal that the impacts on both upward and downward mobility are larger in absolute value for traditionally more disadvantaged groups of students, such as females (relative to males), those without access to technology (relative to those

with access), and children with parents who did not attend school (relative to children with parents who attended school), even though the difference is statistically insignificant.[12] Hence, our information nudges can compensate for the challenges that disadvantaged students face and offer a low-cost and scalable solution to bridge the gaps in study behavior and possibly learning in resource-poor settings.

5.2 Plausible Mechanisms

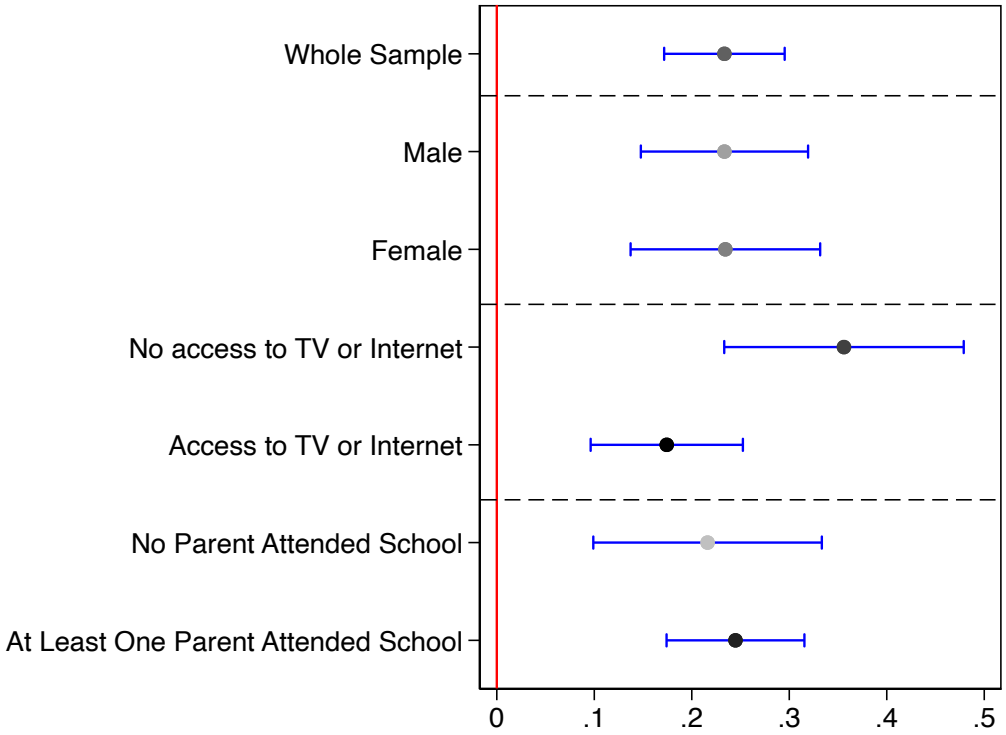
Let us now explore some of the potential mechanisms through which the intervention may have impacted our primary outcome of interest—study hours. In particular, we examine parental awareness of different education programs, as reported in Figure 3. Given that the SMS and voice call interventions were primarily directed to mobile phones owned by parents, increasing parental awareness was a critical prerequisite for influencing changes in children's study hours (estimations reported in Table 6). The findings indicate a significant increase in awareness of educational programs among treated households, encompassing all samples and sub-samples: boys, girls, households with and without access to digital technology, and children of both more and less educated parents. Further impact heterogeneity across sub-samples reveals that the difference in parental awareness is mostly statistically insignificant. However, one notable exception is access to technology. That is, households lacking access to television and the internet exhibit substantially greater increases in awareness compared to those with such access (p-value = 0.013).

Similarly, we examine which education-related resources—namely offline or online digital platforms, school teachers, or private tutoring assistants—that treated parents encouraged their children to use (reported in Figure 4 and in Table 7). We document that treated parents are significantly more likely to guide their children to both online and offline education programs for study-related assistance, an effect observed consistently across the overall sample and all sub-samples, with no significant heterogeneity. This suggests that the intervention effectively shifted parental behavior towards leveraging educational resources, irrespective of individual or household characteristics. However, we document no statistically discernible treatment impact on parental guidance-seeking initiatives towards school teachers across any group, indicating that traditional channels of academic support may remain underutilized, perhaps due to limited access during school closures or entrenched barriers in teacher-student engagement. Together with the high compliance rate (Section 2.3), these estimates show that our intervention has created the intended awareness impacts as hypothesized.

For household investment in complementary educational expenditure, our intervention had a significant positive effect, captured by the dummy variable for private tutoring support, detected for the overall sample (reported in the bottom panel of Table 7, column

[12]Heterogeneity analyses are not reported in the current manuscript but are available upon request.

Figure 3: Impact of Treatment on Awareness About Education Programs



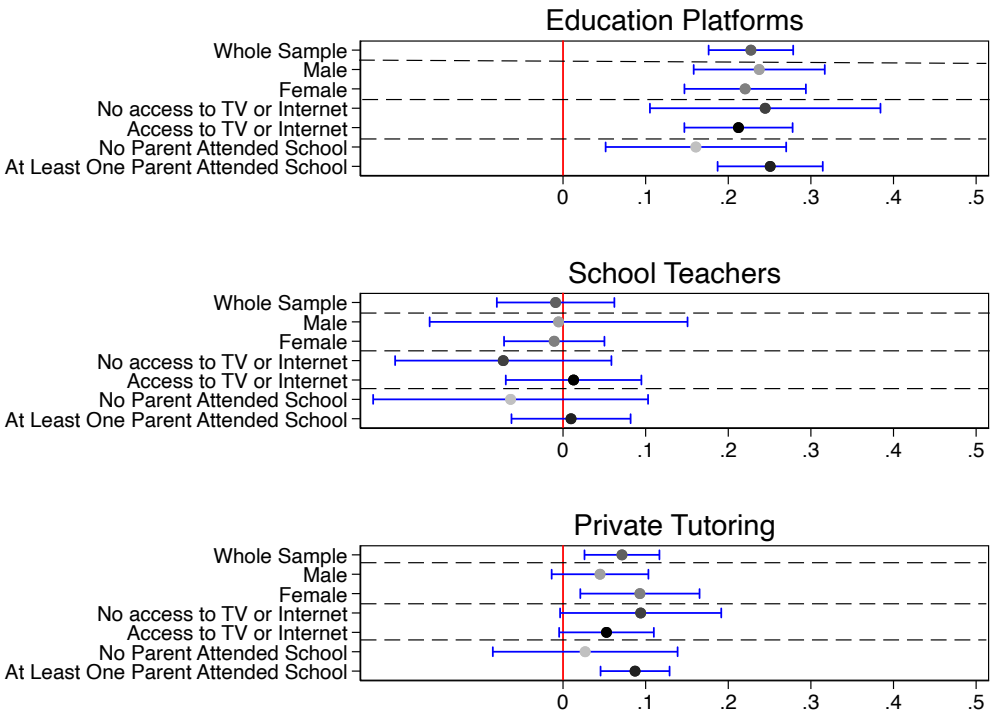
Note: The above estimates are derived from the follow-up survey conducted with 1,077 parents. The outcome variable in the above figure is awareness of education programs on online or offline platforms. It takes unity if the parent is aware of education programs on offline platforms such as Sangsad TV or online programs such as YouTube, Facebook, Konnect, or WhatsApp, and 0 otherwise. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. The 95-percent confidence intervals are based on the standard errors clustered at the school-grade-gender level.

(1)). While we observe significant heterogeneity across genders (female benefited more) and parental schooling (kids with educated parented had higher impacts), we found that the information nudge tends to have a larger impact (in terms of point estimates) on disadvantaged children as their parents are more likely to direct them to obtain help from private tutors for their study, suggesting that some of the underprivileged children in the treatment group are benefiting from private tutoring support, who wouldn't otherwise benefit in the absence of the information nudge. Overall, our results underscore the importance of tailoring educational policies to enhance access to formal and informal learning opportunities while addressing structural barriers limiting the effectiveness of school-based support systems.

5.3 Mediation Analysis

To underscore the importance of awareness, we perform a simple mediation analysis on the awareness of education programs (termed as "awareness") as an additional regressor

Figure 4: Mechanism: Sources of Help Directed by Parents

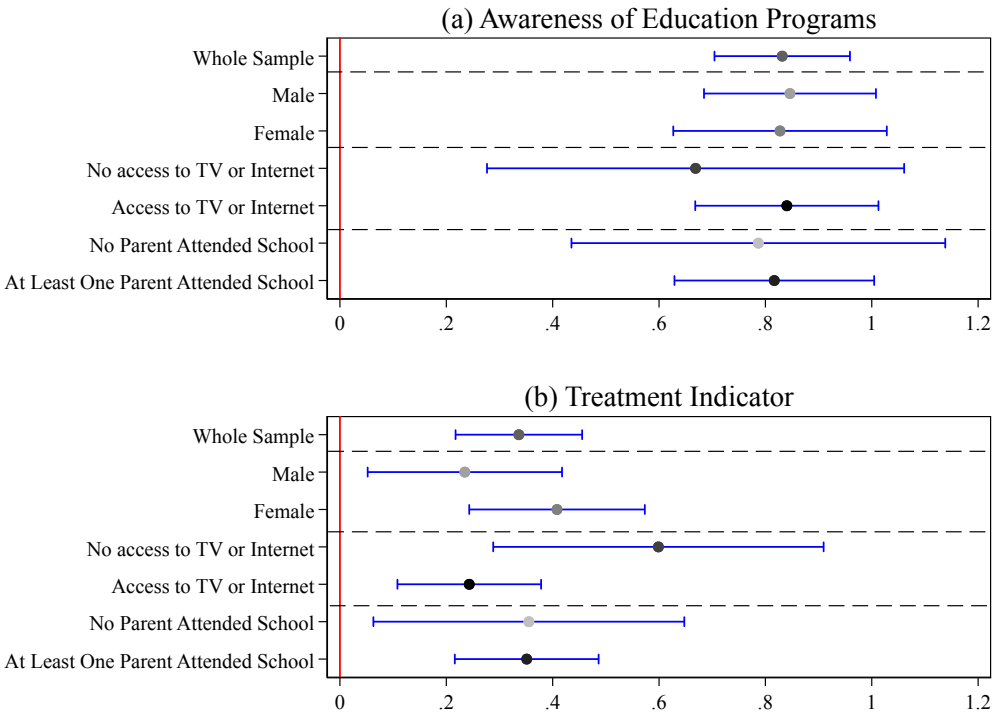


Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variables in the above panel take unity if the parents guide their children to the mentioned source for study-related help in the past year and zero otherwise. The specifications control for unbalanced covariates at the baseline—ownership of bicycle—and school-grade-gender fixed effects. The 95-percent confidence intervals are based on the standard errors clustered at the school-grade-gender level.

in eq. 3. Figure 5 (also in Table 8) shows that the coefficient of "awareness" is highly significant both statistically and economically, regardless of the choice of the sample, and the coefficient on the treatment indicator is generally smaller than those reported in Figure 1. To understand the order of magnitude of the importance of awareness, let us take the estimates for the whole sample. The coefficients for the whole sample in Figures 3 and 5(a) are 0.233 and 0.832, respectively, which suggest that treatment increased the study hours by $0.194 (= 0.233 \times 0.832)$ hours due to increased awareness.

Correspondingly, the coefficient on the treatment indicator for the whole sample drops from 0.531 in Figure 1 to 0.336 in Figure 5(b). Thus, about 37 percent (=0.194/0.531) of the total treatment effect on study hours could be attributed to the treatment effect through the awareness channel, and the rest could be attributed to the direct treatment effect. In Section 6, we use a similar specification but extended to two periods with heterogeneity with respect to technology access to quantify the effect of treatment on inequality using a regression-based decomposition approach.

Figure 5: Mediation Analysis



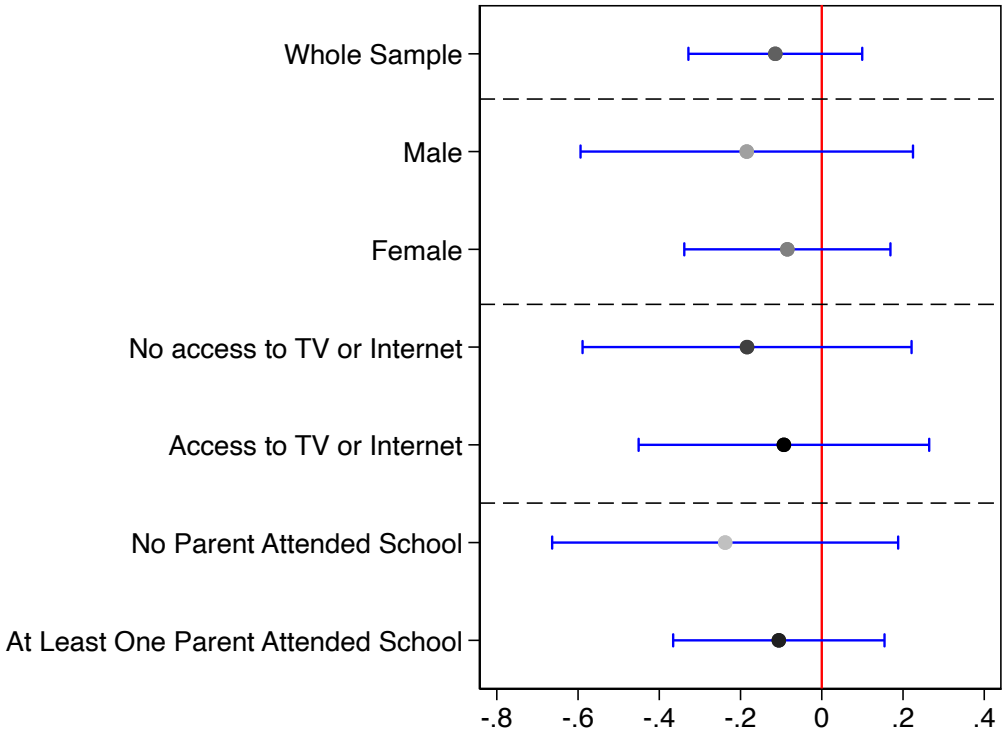
Note: The above figures the point estimates and 95-percent confidence intervals derived from the follow-up survey of 1,077 parents. Figures 5(a) and (b) plot the coefficients and confidence intervals on the indicators for the awareness of education programs and treatment, respectively, in the mediation regression analysis. The outcome variable in the above figures is the average daily study hours in the past year as reported by parents in the follow-up survey. The specifications control for the unbalanced covariate at the baseline—ownership of bicycle—and school-grade-gender fixed effects. The confidence intervals are based on the standard errors clustered at the school-grade-gender level.

5.4 Secondary Impact: Learning Outcomes

Our intervention, which significantly increased study hours, holds promise for improving learning outcomes. Lacking formal academic assessment due to Bangladesh's simplified post-pandemic school promotion and shortened syllabus, we administered a short mathematics test comprising 10 questions that combined foundational and some advanced math knowledge, at the baseline and endline (see Table A5 to check the math test questions). Using eq. (3), we estimated the impact of our intervention on this mathematics test as our measure for learning outcome. Our estimations show no statistically significant effect on endline test scores, whether we use the overall sample or any sub-sample (see Figure 6).

This null effect of the information nudges on test scores may be driven by several factors. First, following COVID-19 disruptions, Bangladesh's education authorities implemented substantial changes to the secondary education system. In 2022, the syllabus was shortened, and formal subject-specific examinations were replaced by a more flexible "promoted/not promoted" system. This shift reduced both the coverage and the dis-

Figure 6: Impact of Treatment on Test Scores



Note: The above estimates are derived from the endline survey conducted with 1,137 students. The outcome variable is the test score at the endline. The specifications control for test score at the baseline, unbalanced covariate at the baseline—ownership of bicycle—and school-grade-gender fixed effects. The 95-percent confidence intervals are based on the standard errors clustered at the school-grade-gender level.

criminatory power of test-based assessments, limiting our ability to capture meaningful variation in academic performance.

Second, the mathematics test administered in our study—a brief, ten-item instrument designed primarily for baseline balance checks—was unincentivized, low stakes, and not aligned with the revised school syllabus or the subject topics/areas students prioritizing during exam preparation. By the time of the endline survey, students were preparing for their year-end assessments, which followed the official curriculum. The items on our math test were not part of that curriculum and, therefore, were unlikely to be top-of-the-mind for students. As a result, performance on this test may not reflect the kinds of knowledge or skills that students were actively studying, which further reduces its sensitivity to treatment-induced learning gains.

These considerations help explain why we detect no statistically significant treatment effects on test performance. Such null results could arise because the test was too short or too weakly aligned with students' actual learning activities, because learning gains emerge more slowly and may require sustained exposure to improved study habits (Brisson et al., 2017), or because there genuinely were no short-term impacts of increased study hours on

measured learning. Nonetheless, cultivating better study habits during the post-pandemic recovery period remains an essential foundation for improving self-directed learning, and such behavioral improvements may translate into more meaningful academic gains as students have more time to internalize and apply what they study.

6 Decomposition analysis

The preceding discussion indicates that our intervention had a positive impact on study hours both in the absolute and relative sense. We have also observed some changes in the behavior. While these results are encouraging, it is unclear how much the treatment effect contributes to the reduction in the inequality in study hours. To address this question, we propose a regression-based decomposition of changes in inequality based on the Shapely-value approach.

This decomposition analysis builds on the conceptual framework introduced in Appendix A.4. In the model, high-frequency informational nudges affect study hours through changes in parental engagement, with potentially heterogeneous impacts depending on baseline household characteristics such as access to educational technology. This structure implies that the same intervention can simultaneously raise average study time and alter its distribution by disproportionately affecting households facing larger initial information constraints.

The fundamental idea behind the proposed approach is somewhat similar to the Oaxaca-Blinder decomposition after Oaxaca (1973) and Blinder (1973). It is a regression-based decomposition approach that has been widely used to describe the wage gaps across different demographic groups, among others. As with the Oaxaca-Blinder decomposition, we consider the marginal contribution of a particular variable by letting it change while fixing all other variables constant. However, unlike the Oaxaca-Blinder literature, we decompose an overall inequality measure rather than a specific gap.

There are also regression-based studies that attempt to decompose inequality by essentially computing a series of marginal contributions, but they tend to impose an arbitrary sequence of variables that are allowed to change (Juhn et al., 1993; Yun, 2006). To address this issue, we adopt the Shapley-value approach, in which the marginal contribution of a given variable is computed over all the possible sequences of change (Okamoto, 2011; Shorrocks, 2013). An early empirical study based on this approach includes Wan (2004), who analyzed inequality in China.

The current study differs from most of the existing literature at least in three ways. First, we are considering the *changes* in inequality instead of the level of inequality. When the level of inequality is decomposed, the outcome—such as income and consumption—is written as a sum of different sources, which could be the product between a covariate and its regression coefficient. Then, inequality is decomposed by sources. The current

approach is different because the coefficient is allowed to vary over time, such that we can decompose the change in inequality into the structural component (due to the change in coefficient) and the distributional component (due to the changes in the distribution of covariates). Given that we have the structural and distributional components, the current study is somewhat similar to Fujii (2018), but Fujii (2018) is focused on the ethnic gap, rather than inequality.[13] Second, we explicitly include potentially heterogeneous treatment effect and mediation effect terms. To our knowledge, this is the first study to conduct a regression-based inequality decomposition analysis in an experimental setting. Finally, unlike most studies, we use study hours as an outcome of interest. While average hours of study is surely of interest, inequality measures are also of interest as we would be interested in removing the gap between better and worse students.

We denote the average parent-reported study hours and indicator for having access to TV or internet ("technology access") by y_{it} and A_{it}, respectively, for individual i at time $t \in \{0, 1\}$. We let X_{it}^j be the j-th covariate, which may include mediators. In our empirical analysis, we only include the awareness of either online or offline educational programs.[14] We denote the indicator for the treatment group by T_i. We then consider the following regression model:

$$y_{it} = \alpha_i + \sum_{a=0}^{1} \mathbf{1}_{(A_{i0}=a)} \cdot \left(\beta_a \mathbf{1}_{(t=1)} + \gamma_a T_i \mathbf{1}_{(t=1)}\right) + \sum_{j=1}^{J} \sum_{\tau=0}^{1} \delta_\tau^j X_{i\tau}^j \mathbf{1}_{(t=\tau)} + \varepsilon_{it}, \quad (5)$$

where α_i, and ε_{it} represent the individual fixed-effect and idiosyncratic error terms, and β_a and γ_a capture the time fixed effect and treatment effect, which are allowed to be heterogeneous with respect to the baseline technology access A_{i0}. We run an ordinary least squares (OLS) regression of eq. (5) to obtain the estimates of α_i, β_a, γ_a, δ_t^j for each i, a, j, and t, which are denoted with a hat ($\hat{}$), and the regression residual $\hat{\varepsilon}_{it}$. Now let us define $\hat{S}_{it}^a \equiv \hat{\beta}_a \mathbf{1}_{(t=1)} \mathbf{1}_{(A_{i0}=a)}$ and $\hat{\Gamma}_{it}^a \equiv \hat{\gamma}_{it}^a \mathbf{1}_{(t=1)} \mathbf{1}_{(A_{i0}=a)}$ for $a \in \{0, 1\}$, which can be interpreted as the estimated time-fixed and treatment effects, respectively, for individual i at time t whose status of access to technology at the baseline is a. Note that $\hat{S}_{it}^0$ and $\hat{\Gamma}_{it}^0$ [$\hat{S}_{it}^1$ and $\hat{\Gamma}_{it}^1$] are defined to be zero for individuals that have [do not have] access to technology at the baseline (i.e., $A_{i0} = 1$ [$A_{i0} = 0$]). With these notations, we obtain the following identity:

[13] Another important difference is that Fujii (2018) uses an integration-based approach rather than the Shapley value for decomposition. The integration-based decomposition has some merits (Fujii, 2017) and can be potentially applied to the current context as well. However, because our treatment does not change continuously over time, we chose to use the Shapley decomposition, as it does not require differentiability with respect to time.

[14] Hours or minutes of educational program viewership were difficult to capture objectively, and self-reported viewership information is unreliable and noisy.

$$
\begin{aligned}
y_{it} &= \hat{\alpha}_i + \sum_{a=0}^{1} \mathbf{1}_{(A_{i0}=a)} \cdot \left(\hat{\beta}_a \mathbf{1}_{(t=1)} + \hat{\gamma}_a T_i \mathbf{1}_{(t=1)} \right) + \sum_{j=1}^{J} \hat{\delta}_t^j X_{it}^j + \hat{\varepsilon}_{it} \\
&= \hat{\alpha}_i + \hat{S}_{it}^0 + \hat{S}_{it}^1 + \hat{\Gamma}_{it}^0 + \hat{\Gamma}_{it}^1 + \sum_{j=1}^{J} \hat{\delta}_t^j X_{it}^j + \hat{\varepsilon}_{it}. \qquad (6)
\end{aligned}
$$

In this equation, there is a total of $2J + 5$ variables that have subscript t, including $\hat{S}_{it}^0$, $\hat{S}_{it}^1$, $\hat{\Gamma}_{it}^0$, $\hat{\Gamma}_{it}^1$, $\hat{\varepsilon}_{it}$, $\hat{\delta}_t^j$, and X_{it}^j. Note that we have y_{i0} [y_{i1}] when the subscript t for each of these variables is equal to zero [one]. It is also possible to consider $\tilde{y}_i^B$, where some of the variables subscripts t are one and others zero, where B is a $(2J+5)$-digit binary index that represents the combinations of the t-subscripts for these $2J + 5$ variables. Table 9 shows four examples. In Row (a) [(d)], all subscripts are zero [one]. Hence, $\tilde{y}_i^B$ is equal to y_{i0} [y_{i1}]. In Row (b) [(c)], the subscript for $\hat{\delta}_t$ is changed from 0 to 1 [1 to 0] in Row (a) [(d)]. Hence, the marginal contribution of $\hat{\delta}_t$ to the change in y_{it} (i.e., $y_{i1} - y_{i0}$) when all other variables' subscript t is equal to zero [one] can be written as $\tilde{y}_i^{(b)} - \tilde{y}_i^{(a)} = \Delta\hat{\delta} X_{i0}$ [$\tilde{y}_i^{(d)} - \tilde{y}_i^{(c)} = \Delta\hat{\delta} X_{i1}$], where $y^{(m)}$ for $m \in \{a, b, c, d\}$ is the value of B in Row (m) and $\Delta\hat{\delta} \equiv \hat{\delta}_1 - \hat{\delta}_0$ is the change in $\hat{\beta}_t$.

While we considered the decomposition of the change in y_{it} here, we can also consider a similar decomposition for the change in the inequality index $I_t \equiv I(\{y_{it}\})$, where $I(\cdot)$ gives an inequality index from the distribution of y_{it} such as the Gini index, general entropy measure, and percentile ratios. As y_{it} can be zero, we only consider measures that allow for the presence of zeros. Therefore, we primarily consider the Gini index ("Gini") as it is a widely used measure of inequality. We also consider the general entropy measure with parameter value of two ("GE2"). We choose the parameter value of two to allow for zero or even negative value in the argument. In addition, we also use the the interquartile ratio ("IQR") and interdecile ratio ("IDR"), which are both percentile ratios. The former [latter] takes the ratio of the 75th [90th] percentile to 25th [10th] percentile. The method discussed below is generally applicable to other well-defined inequality measures.

The inequality measures we adopt have different properties. Gini coefficient is bounded between zero and one (in the absence of negative outcomes) and has a straightforward graphical interpretation that it is twice the area between the Lorenz curve and 45-degree line. GE2 is known to be a monotonic transformation of the coefficient of variation and sensitive to the presence of large extreme values. IQR and IDR are robust to the presence of outliers. However, as our method is based on the predictions from linear regression, IDR does not work as well as IQR since the linear models do not predict the tails of the distribution very well. Also, the decomposition would not work (well) when the denominator of IQR or IDR has a value of (near) zero.[15]

[15]This is not an issue for the main analysis using the follow-up survey. However, this is an issue when

To define the Shapley decomposition in our context, we define $\tilde{I}^B \equiv I(\{\tilde{y}_i^B\})$.[16] Now, we consider the marginal effects of each variable by sequentially changing the subscript t and then average the marginal effects across all of the $(2J+5)!$ possible sequences. However, because there are only 2^{2J+5} possible combinations of the subscripts, the same marginal effects appears multiple times in this calculation.

To advance the argument, we define $B_{b,l,n}$ be the binary expression where we insert $n \in \{0,1\}$ into a $(2J+4)$-digit binary number b before the lth digit from the left. Here, b can be interpreted as a summary index that represents the values of subscript t for all variables except for the lth variable, for which we are trying to find the marginal contribution. Then, the marginal contribution of the lth variable can be written as: $\Delta_b^l \equiv \tilde{I}^{B_{b,l,1}} - \tilde{I}^{B_{b,l,0}}$. Now, let $q(b)$ be the number of digits that take unity in the binary form. Then, there are $q(b)!(2J+4-q(b))!$ sequences along which Δ_b^l is the relevant marginal effect for the lth variable. Therefore, by taking the average over all sequences, we have the marginal contribution of lth variable as follows:

$$
\begin{aligned}
\Delta^l &= \frac{1}{(2J+5)!} \sum_{b=0}^{2^{2J+4}-1} q(b)!(2J+4-q(b))!\Delta_b^l \\
&= \frac{1}{(2J+5)!} \sum_{B=0}^{2^{2J+5}-1} (q(B)-1)!(2J+4-(q(B)-1))! \cdot \tilde{I}^B \cdot \mathbf{1}_{d(B,l)=1} \\
&\quad -q(B)!(2J+4-q(B))! \cdot \tilde{I}^B \cdot \mathbf{1}_{d(B,l)=0} \\
&= \sum_{B=0}^{2^{2J+5}-1} \left[\frac{\mathbf{1}_{d(B,l)=1}}{{}_{2J+4}\mathrm{C}_{q(B)-1}} - \frac{\mathbf{1}_{d(B,l)=0}}{{}_{2J+4}\mathrm{C}_{q(B)}} \right] \cdot \frac{\tilde{I}^B}{2J+5}, \qquad (7)
\end{aligned}
$$

where ${}_n\mathrm{C}_k = n!/k!(n-k)!$ is the n-choose-k binomial coefficient and $d(B,l)$ is the lth digit of B in the binary expression. Because we have $\sum_{l=1}^{2J+5} \Delta_b^l = \Delta_I (\equiv I_1 - I_0)$ for each b by construction, we have $\sum_{l=1}^{2J+5} \Delta^l = \Delta_I$. Eq. (7) is convenient as each decomposed component Δ^l can be expressed as a weighted sum of the inequality measure for different values of B.

Table 10 shows the decomposition results for the change in inequality between the baseline and follow-up.[17] Rows (a) and (b) give the initial and terminal inequality and Row (c) gives the difference between these rows. The decomposition exercise proposed

we use the endline survey for $t = 1$.

[16] Note that $\tilde{y}_i^B$ can be negative or more than 24 hours for some i and B. Our inequality measures can be defined in the presence of negative values of $\tilde{y}$. Nevertheless, as a robustness check, we define $\check{y} \equiv \min(24, \max(\tilde{y}_{it}, 0))$—a doubly-censored version of $\tilde{y}_{it}$ to be between zero and 24—and use $\check{I}^B \equiv I(\{\check{y}_i^B\})$ for the decomposition analysis. The results remain the same for IQR and IDR and very similar for Gini and GE2.

[17] The change in inequality between the baseline and endline is generally small and mixed. Gini and IDR increased by 0.0084 and 0.500, respectively, whereas GE2 and IQR decreased by 0.0164 and 0.6667, respectively.

above allows us to account for this difference with different components. Rows (d), (e), (g), (h), (j), (k), and (m) provide different components from the decomposition results, and the percentages in the parentheses represent the share of each decomposition component in the total observed change in Row (c). While the share can be negative or above 100 percent, the sum of $\Delta^1, \cdots, \Delta^7$ gives Δ_I (and thus the shares add up to 100 percent). Table 10 also provides the aggregate components due to time effect (Row (f)), direct treatment effect (Row (i)), and awareness (Row (l)).

The table shows that the inequality in average daily study hours in our sample declined between the baseline and follow-up surveys. Note thought that this decline may partially reflect the change in the recall span between the baseline (one month) and follow-up (one year). As the idiosyncratic effect component is most uninformative, we would like this component to be small. This is the case for Gini, GE2, and IQR, as Row (m) shows. However, for IDR, it is large. As discussed above, this is not surprising as the linear regression does not predict the tails of distribution very well.

With this caveat in mind, let us look at the remaining rows. As Rows (d)–(f) show, much of the decline in inequality is driven by the time effect, particularly for households with technology access. Nevertheless, the treatment also played a role. About 8.50 percent of the decline in Gini can be attributed to the direct effect of treatment (Row (i)). While the share of this component varies across different inequality measures, the total direct treatment effect appears to have contributed to 5-20 percent of the decline in inequality.

The total direct treatment effect is arguably an underestimate of the overall treatment effect because the former does not account for the changes in awareness that might be affected by the treatment and contributed to the study hours. For example, if we believe that the distributional effect of awareness can be attributed to the treatment, 10.93(=8.50+2.43) percent of the decline in Gini could be attributed to the treatment. The results are qualitatively similar for GE2 and IQR as well. However, a similar calculation leads to -0.78(=5.58-6.36) percent for IDR, but because the proportion of idiosyncratic effect for IDR is much larger than other inequality measures (Row (j)), this number would not be as reliable as other inequality measures. Taken together, our treatment appears to have reduced the inequality in study hours in the middle of the distribution but did not affect the inequality between the tails.

7 Conclusion

This study provides the first experimental evidence on whether high-frequency information (HFI) nudges delivered to parents can rebuild students' self-study habits in the post-pandemic recovery period in a low-income, resource-constrained setting. Leveraging a large-scale randomized controlled trial conducted across three phases of the COVID-19

education crisis in rural Bangladesh, we show that providing parents with timely and structured information about publicly available educational resources leads to meaningful improvements in students' study behaviors. Students in treated households studied, on average, 0.53 additional hours per day—a 15 percent increase relative to the control mean—with consistent effects across gender, parental education, and digital access. The intervention was particularly effective for households without access to television or the internet, suggesting that information constraints bind most tightly for disadvantaged students and that light-touch nudges can serve as an equalizing force. We further demonstrate improvements in upward mobility within the study-hour distribution, reduced study-hour inequality, and substantial gains in parental awareness about educational resources. Approximately one-third of the total impact on study hours can be explained by increases in parental awareness, underscoring the centrality of parents as agents in shaping children's behavioral responses.

Our inequality decomposition using a regression-based Shapley-value approach—a novel application of this method within an RCT setting—shows that 5–20 percent of the reduction in study-hour inequality can be attributed to the intervention itself. This highlights the potential of low-cost information strategies not only to raise average effort levels but also to narrow persistent gaps between advantaged and disadvantaged learners. Given the widespread disruptions caused by prolonged school closures and the heightened risk of cumulative learning loss, such scalable parental-support models can play a critical role in stabilizing educational trajectories for vulnerable students.

At the same time, our results indicate no detectable short-term impacts on learning outcomes, a finding that warrants cautious interpretation. The pandemic-era shift in Bangladesh's assessment system, the brevity and low-stakes nature of our mathematics test, and the misalignment between the test items and the content emphasized in school-level exams likely limited our ability to measure academic gains. Consistent with prior research, learning improvements may require more extended periods of sustained effort before becoming detectable or may rely on complementary instructional support unavailable during the study period. Yet fostering strong and consistent study habits during this critical transition remains essential to subsequent learning gains, particularly for students at risk of long-term educational deficits.

A few important policy implications emerge from our findings. First, simply making educational content publicly available is unlikely to benefit disadvantaged children unless parents are aware of—and understand how to utilize—such resources. While seemingly obvious, our results underscore how binding informational constraints can be, and how essential it is to integrate awareness-building into any education-support program. Second, behavioral responses to information nudges often take time to materialize. In our case, we observed meaningful increases in study hours only in the follow-up period, while neither study hours nor test scores improved significantly at the endline—highlighting

the gradual nature of habit formation and the persistence of the status quo in household learning routines. Third, even disadvantaged parents actively seek private tutoring for their children when possible, indicating that publicly available educational content—even when known—may not be sufficient on its own. As personalized learning technologies expand alongside the spread of smartphones and computers, making adaptive, high-quality learning tools freely available or affordably priced could substantially enhance the effectiveness of such interventions. Doing so may further reduce inequality in study effort and help level the educational playing field across socioeconomic groups.

Taken together, our findings offer three broad insights. First, informational barriers faced by parents—especially in low-connectivity, low-literacy settings—are substantial and consequential for children's educational engagement. Second, simple, inexpensive, and easily scalable communication tools such as SMS and automated voice calls can effectively overcome these barriers, even in communities with minimal digital infrastructure. Third, such interventions can meaningfully contribute to reducing structural inequalities in educational engagement by disproportionately benefiting students with fewer resources at home.

As education systems worldwide continue to grapple with the lingering aftermath of the COVID-19 crisis, identifying cost-effective, scalable strategies to restore learning routines remains a policy priority. In contexts like rural Bangladesh, where mobile phone penetration is high, but internet connectivity is limited, high-frequency information delivered through basic communication channels represents a practical and actionable policy lever. Future research may explore how such nudges can be complemented with structured academic support, personalized digital learning tools, or teacher-led reinforcement to translate improved study habits into measurable learning gains.

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Table 1: Compliance Rate of Intervention

Week	SMS	Voice Call	**Week**	SMS	Voice Call	**Week**	SMS	Voice Call
1	100.00	65.83	**19**	100.00	89.33	**37**	100.00	83.50
2	100.00	75.17	**20**	77.50	87.33	**38**	99.83	82.67
3	100.00	22.50	**21**	100.00	88.50	**39**	100.00	83.17
4	100.00	20.33	**22**	100.00	87.50	**40**	99.83	86.67
5	100.00	91.00	**23**	89.33	63.00	**41**	99.83	86.00
6	100.00	88.67	**24**	89.33	76.67	**42**	99.83	84.83
7	99.83	99.67	**25**	100.00	85.50	**43**	99.83	84.17
8	100.00	99.83	**26**	100.00	84.33	**44**	99.83	83.33
9	98.50	82.67	**27**	100.00	77.33	**45**	99.83	82.17
10	63.50	56.17	**28**	100.00	75.00	**46**	99.83	76.83
11	100.00	70.33	**29**	100.00	75.83	**47**	99.83	82.67
12	96.83	16.67	**30**	99.83	83.17	**48**	99.83	81.33
13	100.00	87.00	**31**	100.00	83.50	**49**	99.83	83.50
14	100.00	72.83	**32**	100.00	83.67	**50**	99.83	83.33
15	97.83	87.33	**33**	100.00	84.00	**51**	99.83	64.50
16	95.50	87.50	**34**	100.00	82.83	**52**	99.83	81.33
17	100.00	87.33	**35**	100.00	82.17			
18	97.00	88.50	**36**	100.00	82.33			

Note: The above table provides the average compliance rate of the intervention in the treatment group—SMS and voice calls received—by week.

Table 2: Summary of Self-Reported Study Hours by Parents

Respondent	Recall Span	Reference period	Mean	(s.d.)
Baseline (conducted in Sep 2021)				
Parent	One month	Aug 2021	2.29	(1.27)
Endline (conducted in Jan 2023)				
Parent	One month	Nov-Dec 2022	5.37	(2.78)
Follow-up (conducted in Sep 2023)				
Parent	One year	Sep 2022–Aug 2023	3.79	(1.35)

Note: The above table shows the average study hours, as reported by parents at the baseline, endline, and follow-up surveys. The baseline survey began in September 2021, and respondents were asked about study hours in August. The endline survey was conducted in January 2023, and respondents were asked about study hours in November 2022. The follow-up survey was conducted in September of 2023, and parents were asked about study hours in the past year.

Table 3: Baseline Summary Statistics and Balance Check

	Control (1)	Treatment (2)	Overall (3)	Orthogonality† (4)
Participating child is female	0.568	0.568	0.568	1.000
Parent-reported daily study hours	2.254	2.324	2.289	0.340
Number of household members	4.547	4.513	4.530	0.643
Number of male household members	2.188	2.137	2.162	0.365
Number of female household members	2.358	2.377	2.368	0.757
Household head has no schooling	0.350	0.375	0.363	0.368
Household head has primary education	0.297	0.298	0.298	0.950
Household head has secondary education	0.353	0.327	0.340	0.330
Household has agricultural land	0.990	0.993	0.992	0.526
Household has radio	0.020	0.017	0.018	0.667
Household has television	0.605	0.603	0.604	0.953
Household has bank account	0.235	0.227	0.231	0.732
Household has NGO/MFI account	0.467	0.438	0.452	0.325
Household has television/internet at home	0.703	0.690	0.697	0.616
Household has bicycle	0.537	0.475	0.506	0.033
Math score at baseline (out of 10)	5.228	5.205	5.217	0.853
Any child participated in Fujii et al. (2023)	0.118	0.128	0.123	0.599
Observations	600	600	1,200	1,200

Note: "Any child participated in Fujii et al. (2023)" takes a value of 1 if any child from a household participated in another study that the research team conducted during 2017-2018, and 0 otherwise. Column (1) shows the mean value of the variables for the control arm, and Column (2) shows the mean value of the variables for the information treatment arm. Ownership of assets (agricultural land, radio, television, bank account, NGO/MFI account, access to television/internet, bicycle) is a binary variable that takes a value of 1 if the household owns the asset, and 0 otherwise. "Math score at baseline" shows the raw score in a mathematics test, comprising of ten questions, administered by the research team at baseline. Column (3) shows the mean value for each variable. Column (4) shows the p-value for joint orthogonality.

Table 4: Impact of Treatment on Study Hours: ITT and 2SLS Estimates

Dependent variable	Daily Study Hours in Past One Year						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ITT Estimates							
Treated	0.531***	0.433***	0.602***	0.837***	0.390***	0.525***	0.551***
	(0.057)	(0.076)	(0.079)	(0.133)	(0.066)	(0.129)	(0.069)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
2SLS Estimates							
Treated	0.765***	0.649***	0.842***	1.175 ***	0.573***	0.862***	0.763***
	(0.110)	(0.180)	(0.138)	(0.193)	(0.134)	(0.231)	(0.123)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Strata Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student observations	1,077	471	606	313	764	272	805
Control Mean	3.525	3.562	3.497	3.267	3.631	3.418	3.560

Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variable in the above figure is a continuous variable measuring daily study hours in the past one year. For the 2SLS estimates, we instrument for compliance, that is, whether any member in the treated household received SMS or voice call, with treatment assignment. The specifications control for unbalanced covariate at the baseline—ownership of bicycle—and school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "Treated" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels respectively.

Table 5: Impact of Treatment on Mobility

Dependent variable	Upward Mobility						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.135***	0.082*	0.177***	0.175**	0.118***	0.189**	0.124***
	(0.031)	(0.040)	(0.044)	(0.066)	(0.034)	(0.070)	(0.032)
	[0.002]	[0.015]	[0.003]	[0.007]	[0.003]	[0.007]	[0.003]
Control Mean	0.462	0.478	0.450	0.400	0.488	0.408	0.480
Dependent variable	Downward Mobility						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	-0.134***	-0.078*	-0.177***	-0.175**	-0.116***	-0.189 **	-0.121***
	(0.031)	(0.039)	(0.044)	(0.066)	(0.033)	(0.070)	(0.032)
	[0.002]	[0.015]	[0.003]	[0.007]	[0.003]	[0.007]	[0.003]
Strata Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.536	0.517	0.550	0.600	0.509	0.592	0.518
Student observations	1,072	469	603	313	759	269	803

Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variable in the above figure is a mobility measure based on study hours at the baseline and follow-up. "Upward mobility" ["Downward mobility"] is an indicator variable that takes a value of 1 if the student moved up [down] in the control distribution of average daily study hours between the baseline and follow-up surveys. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "Treated" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 6: Impact of Treatment on Awareness on Educational Resources

Dependent variable	Awareness About Education Resources						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.233***	0.233***	0.234***	0.356***	0.174***	0.216***	0.245***
	(0.030)	(0.040)	(0.045)	(0.060)	(0.038)	(0.057)	(0.035)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Strata Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student observations	1,077	471	606	313	764	272	805
Control Mean	0.382	0.392	0.373	0.271	0.427	0.344	0.394

Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variable takes unity if the parent is aware of education resources on offline platforms such as Sangsad TV or online programs such as YouTube, Facebook, Konnect, or WhatsApp, and 0 otherwise. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "Treated" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent lev,els respectively.

Table 7: Mechanisms: Sources of Help Directed by Parents

Dependent variable	Education Resources (Both offline and online platforms)						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.227***	0.237***	0.220***	0.245***	0.212***	0.161***	0.251***
	(0.025)	(0.037)	(0.034)	(0.068)	(0.032)	(0.053)	(0.031)
	[0.001]	[0.001]	[0.001]	[0.003]	[0.001]	[0.009]	[0.001]
Control Mean	0.130	0.138	0.123	0.097	0.143	0.137	0.127
Dependent variable	School Teachers						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	-0.009	-0.005	-0.011	-0.072	0.013	-0.063	0.010
	(0.035)	(0.073)	(0.028)	(0.064)	(0.040)	(0.081)	(0.035)
	[0.469]	[0.560]	[0.469]	[0.208]	[0.469]	[0.307]	[0.469]
Control Mean	0.707	0.728	0.690	0.781	0.676	0.725	0.701
Dependent variable	Private Tutoring						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.071***	0.045	0.093**	0.094*	0.053*	0.027	0.087***
	(0.022)	(0.027)	(0.034)	(0.048)	(0.028)	(0.054)	(0.020)
	[0.006]	[0.117]	[0.019]	[0.063]	[0.070]	[0.456]	[0.001]
Control Mean	0.761	0.789	0.740	0.716	0.780	0.786	0.753
Student observations	1,077	471	606	313	764	272	805

Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variables in the above panels take unity if the parents guide their children to the mentioned sources for study-related help in the past year, and zero otherwise. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "Treated" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 8: Mediation Analysis

Dependent variable	Study Hours						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.336***	0.235**	0.408***	0.599***	0.243***	0.355**	0.351***
	(0.058)	(0.085)	(0.077)	(0.152)	(0.066)	(0.142)	(0.066)
	[0.001]	[0.003]	[0.001]	[0.001]	[0.001]	[0.003]	[0.001]
Awareness	0.832***	0.846***	0.828***	0.669***	0.840***	0.787***	0.817***
	(0.062)	(0.075)	(0.094)	(0.191)	(0.084)	(0.142)	(0.092)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Strata Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student observations	1,077	471	606	313	764	272	805
Control Mean	3.525	3.562	3.497	3.267	3.631	3.418	3.560

Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variable in the table above is the average daily study hours over the past year, as reported by parents in the follow-up survey. "Awareness" variable takes unity if the parent is aware of education resources on offline platforms such as Sangsad TV or online programs such as YouTube, Facebook, Konnect, or WhatsApp, and 0 otherwise. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "Treated" and "Awareness" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 9: Examples of B and their interpretations

			Digit for...							
	Value of B	$q(B)$	$\hat{S}^0_{it}$	$\hat{S}^1_{it}$	$\hat{\Gamma}^0_{it}$	$\hat{\Gamma}^1_{it}$	$\hat{\varepsilon}_{it}$	$\hat{\delta}^1_t\hat{\delta}^2_t\cdots\hat{\delta}^J_t$	$X^1_{it}\cdots X^J_{it}$	$\tilde{y}^B_i$
(a)	0	0	0	0	0	0	0	$00\cdots0$	$0\cdots0$	y_{i0}
(b)	$2^{J+1}-1$	1	0	0	0	0	0	$10\cdots0$	$0\cdots0$	$y_{i0}+\Delta\hat{\delta}^1X^1_{i0}$
(c)	$2^{2J+5}-2^{J+1}-1$	$2J+4$	1	1	1	1	1	$01\cdots1$	$1\cdots1$	$y_{i1}-\Delta\hat{\delta}^1X_{i1}$
(d)	$2^{2J+5}-1$	$2J+5$	1	1	1	1	1	$11\cdots1$	$1\cdots1$	y_{i1}

Table 10: Decomposition results

Inequality index		Gini	GE2	IQR	IDR
(a)	Initial inequality (I_0)	0.2867	0.1546	3.0000	4.0000
(b)	Terminal inequality (I_1)	0.1996	0.0628	1.7368	2.7857
(c)	Change in inequality (Δ_I)	-0.0871	-0.0918	-1.2632	-1.2143
(d)	Time effect for HH	-0.0196	-0.0172	-0.3008	-0.1570
	without TV/inet access (Δ^1)	(22.52%)	(18.71%)	(23.81%)	(12.93%)
(e)	Time effect for HH	-0.0581	-0.0485	-0.3880	-1.5464
	with TV/inet access (Δ^2)	(66.67%)	(52.84%)	(30.72%)	(127.35%)
(f)	Direct treatment effect on	-0.0031	-0.0037	-0.1683	0.0055
	HH without TV/inet access (Δ^3)	(3.52%)	(3.98%)	(13.33%)	(-0.46%)
(g)	Direct treatment effect on	-0.0043	-0.0048	-0.0917	-0.0733
	HH with TV/inet access (Δ^4)	(4.98%)	(5.28%)	(7.26%)	(6.04%)
(h)	Structural effect	-0.0084	-0.0098	-0.2293	0.0512
	of awareness (Δ^5)	(9.63%)	(10.73%)	(18.15%)	(-4.22%)
(i)	Distributional effect	-0.0021	-0.0026	-0.0895	0.0772
	of awareness (Δ^6)	(2.43%)	(2.84%)	(7.08%)	(-6.36%)
(j)	Idiosyncratic	0.0085	-0.0052	0.0045	0.4285
	effect (Δ^7)	(-9.75%)	(5.63%)	(-0.35%)	(-35.29%)
(k)	Total time effect	-0.0777	-0.0657	-0.6888	-1.7034
	($\Delta^1 + \Delta^2$)	(89.19%)	(71.56%)	(54.53%)	(140.28%)
(l)	Total direct treatment	-0.0074	-0.0085	-0.2601	-0.0678
	effect ($\Delta^3 + \Delta^4$)	(8.50%)	(9.25%)	(20.59%)	(5.58%)
(m)	Total awareness effect	-0.0105	-0.0125	-0.3188	0.1284
	effect ($\Delta^5 + \Delta^6$)	(12.06%)	(13.57%)	(25.24%)	(-10.58%)

TV/internet access refers to the access to either TV or the internet at home. The percentages in the parentheses show the size of the inequality component relative to the change (Δ_I). IQR and IDR, respectively, stand for interquartile ratio (p75/p25) and interdecile ratio (p90/p10).

A Appendix [For Online Publication Only]

A.1 Selection of Schools

Bangladesh's secular school education system consists of two tiers of schools: primary (grades 1-5), and secondary consisting of lower secondary (grades 6–8), and upper secondary (grades 9–10). Higher secondary schooling (grades 11-12) is known as college, followed by tertiary education. For the purpose of our study, we selected secular secondary schools following multiple stages of a recruitment process. First, enumerators communicated with headmasters of secondary schools in Gaibandha Sadar Upazila (subdistrict) of Gaibandha district to compile basic school-level information, including total enrollment, number of teachers, highest grade offered, and grade repetition and dropout rates. Based on this information, we selected four schools that met two key criteria: a sufficiently large student population to support the study design and logistical feasibility for data collection during COVID-19. Key demographic and schooling characteristics of students in our sample closely align with those observed in rural Gaibandha and rural Bangladesh more broadly, suggesting that our findings are likely to be externally relevant within similar rural contexts.

A.2 Power Calculation

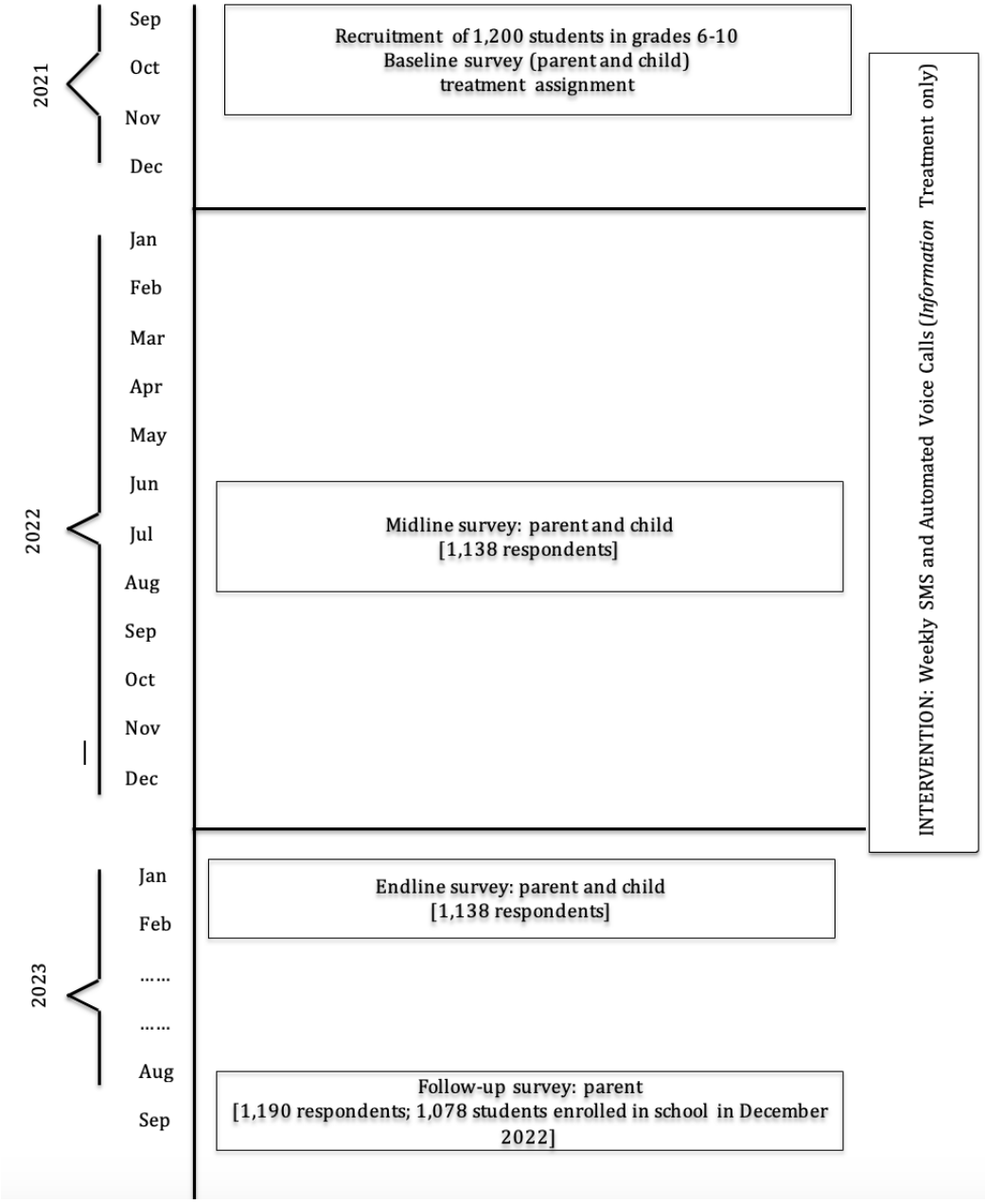
Since our intervention is highly unlikely to decrease study hours, we conducted a one-sided test. We denote the significance level and the probability of type-II error by α and β, respectively. Let P and N respectively denote the proportion of treated individuals and the sample size. Then, because our study design was based on individual-level randomization, we applied the following standard formula under normal approximation to calculate the minimum detectable effect size (MDES)—expressed in multiples of the population standard deviation of the outcome of interest:

$$\text{MDES} = \frac{z_{1-\alpha} + z_{1-\beta}}{\sqrt{NP(1-P)}}, \tag{A1}$$

where z_p is the z-critical value for probability p for standard normal distribution. Since we recruited a total of 1,200 students and divided them equally into the information treatment and control arms, we set $N = 1,200$ and $P = 0.5$. Further, letting $\alpha = 0.1$, $1-\beta = 0.80$, we have MDES $\simeq 0.12$ standard deviation. Therefore, if we find statistically significant impact, it will likely be economically significant as well.

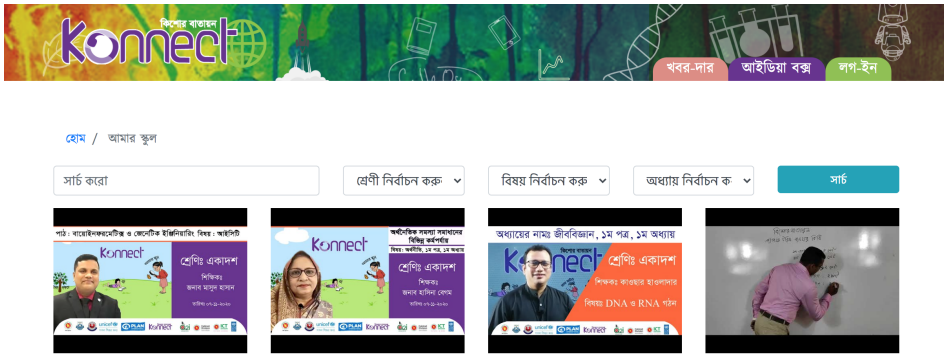
A.3 Additional Tables and Figures

Figure A1: Timeline of the Study



Note: The academic year coincides with the calendar year for grades 6-9.

Figure A2: Examples of Online and Offline Educational Content



Panel A: Konnect



গণপ্রজাতন্ত্রী বাংলাদেশ সরকার
মাধ্যমিক ও উচ্চ শিক্ষা অধিদপ্তর
বাংলাদেশ, ঢাকা
www.dshe.gov.bd

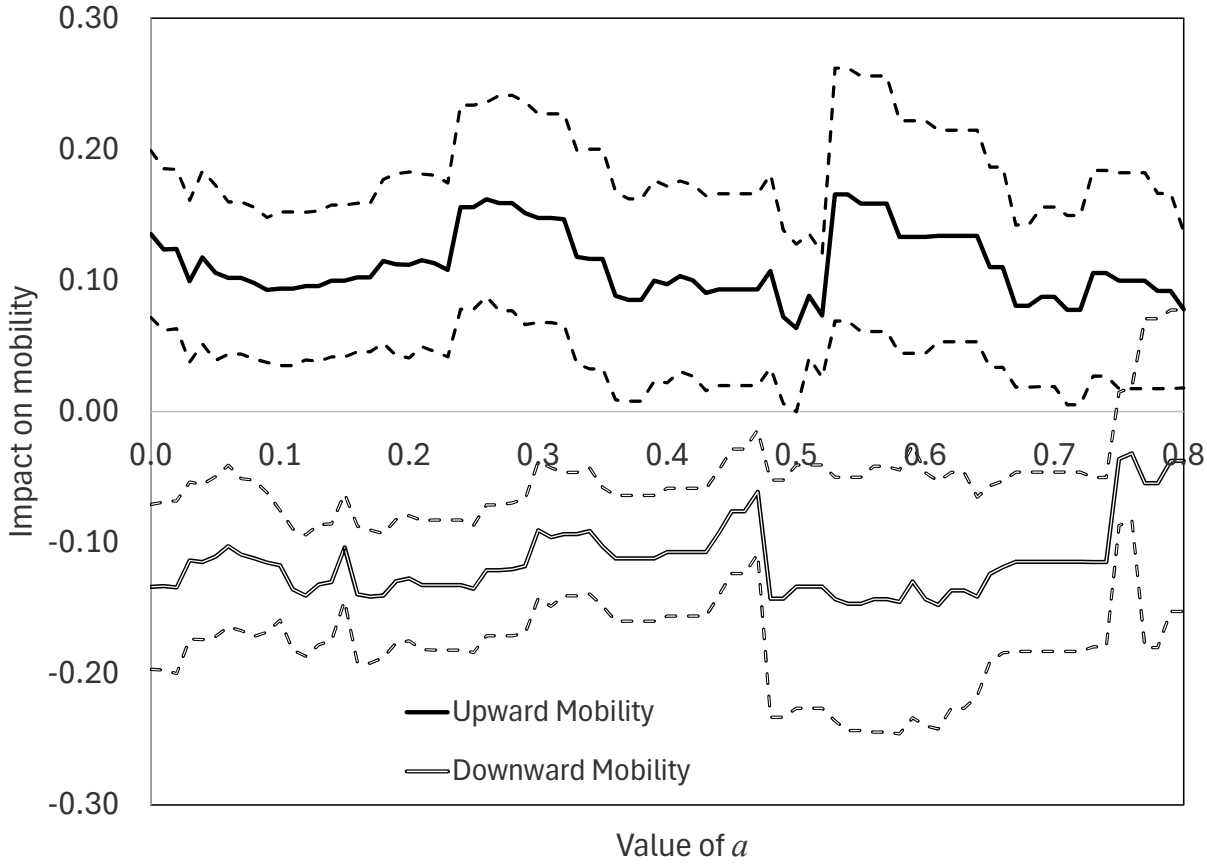
ক্লাস রুটিন
'আমার ঘরে আমার স্কুল'

তারিখ	শ্রেণি	১ম পিরিয়ড	২য় পিরিয়ড
১৯ এপ্রিল-২০২০	জাতীয় সঙ্গীত ও করোনা সচেতনতা (সকাল ১১.০০-১১.০৫)		
	ষষ্ঠ (সকাল ১১.০৫-১১.৪৫)	বিজ্ঞান	বাংলা
	সপ্তম (সকাল ১১.৪৫-দুপুর ১২.২৫)	গণিত	বিজ্ঞান
	অষ্টম (দুপুর ১২.২৫-১.০৫)	বাংলা	বিজ্ঞান
	নবম (দুপুর ১.০৫- ১.৪৫)	গণিত	পদার্থ বিজ্ঞান
	দশম (দুপুর ১.৪৫- ২.২৫)	ইংরেজি	রসায়ন বিজ্ঞান
২০ এপ্রিল-২০২০	জাতীয় সঙ্গীত ও করোনা সচেতনতা (সকাল ১১.০০-১১.০৫)		
	ষষ্ঠ (সকাল ১১.০৫-১১.৪৫)	গণিত	বাংলাদেশ ও বিশ্বপরিচয়
	সপ্তম (সকাল ১১.৪৫-দুপুর ১২.২৫)	ইংরেজি	গণিত
	অষ্টম (দুপুর ১২.২৫-১.০৫)	বিজ্ঞান	বাংলাদেশ ও বিশ্বপরিচয়
	নবম (দুপুর ১.০৫- ১.৪৫)	জীব বিজ্ঞান	ইংরেজি
	দশম (দুপুর ১.৪৫- ২.২৫)	গণিত	ভূগোল
২১ এপ্রিল-২০২০	জাতীয় সঙ্গীত ও করোনা সচেতনতা (সকাল ১১.০০-১১.০৫)		
	ষষ্ঠ (সকাল ১১.০৫-১১.৪৫)	বাংলা	গণিত
	সপ্তম (সকাল ১১.৪৫-দুপুর ১২.২৫)	গণিত	বাংলাদেশ ও বিশ্বপরিচয়
	অষ্টম (দুপুর ১২.২৫-১.০৫)	বাংলা	গণিত
	নবম (দুপুর ১.০৫- ১.৪৫)	গণিত	ইংরেজি
	দশম (দুপুর ১.৪৫- ২.২৫)	ইংরেজি	রসায়ন

Panel B: Sangsad TV Schedule

Note: Panel A is an illustration of the home page for the Konnect website (http://konnect.edu.bd/my-school) and Panel B shows the subject-grade specific schedule of educational programs on Sangsad TV for April, 2020, as published by the Ministry of Education in Bangladesh.

Figure A3: Treatment effect on upward and downward mobility in daily study hours for different values of a



Note: The number of observations varies with the value of a, because the observations that are within the top [bottom] $100a$ percentile in the control distribution of average daily study hours at the baseline are dropped from the analysis. The upward and downward mobility measures are defined in eq. (2). The unbalanced covariate at the baseline–ownership of bicycle–and school-grade-gender fixed-effects are controlled for. The dashed lines represent 95-percent confidence interval based on robust standard errors clustered at the school-grade-gender level.

Table A1: Examples of High Frequency Information

Sl	Message
1	Greetings from XXXXXXX! You may consult your English grammar/Math teacher <insert teacher name> over a message/voice call at <insert phone number> if you have any English grammar/Math-related concerns. Keep studying!
2	Greetings from XXXXXXX! You can watch education programs for <insert grade of student> on Sangsad TV on <insert day of the week> at <insert time (in HHMM format)> every week. Keep studying!
3	Greetings from XXXXXXX! Research finds that one additional year of schooling improves wages by 7 percent. Keep studying!
4	Greetings from XXXXXXX! You can access educational content on <insert educational platform> at <insert hyperlink>. Keep studying!

Table A2: Impact of Treatment on Attrition

Dependent variable	Attrited	
	Endline	Follow-up
	(1)	(2)
Treated	-0.013	-0.021
	(0.014)	(0.017)
Student observations	1,200	1,200
R^2	0.042	0.028
Control Mean	0.058	0.112

Note: "Attrited" takes unity if the child discontinued from our study at the endline or follow-up, and 0 otherwise. 62 students discontinued at the endline and 122 students discontinued at follow-up. The specifications control for unbalanced covariate at the baseline—ownership of bicycle—and school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A3: Impact of Treatment on Study Hours (Pooled Estimation): ITT and 2SLS Estimates

Dependent variable	Study Hours						
	All	Male	Female	No Access to TV or Internet	Has Access to TV or Internet	No Parent Attended School	At Least One Parent Attended School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ITT Estimates							
Treated	0.193***	0.123	0.246***	0.24**	0.15***	0.067	0.221***
	(0.071)	(0.096)	(0.079)	(0.123)	(0.076)	(0.154)	(0.079)
	[0.000]	[0.1]	[0.000]	[0.001]	[0.001]	[0.1]	[0.000]
2SLS Estimates							
Treated	0.293***	0.223	0.345***	0.38***	0.229**	0.261	0.305***
	(0.097)	(0.149)	(0.125)	(0.168)	(0.119)	(0.218)	(0.108)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Survey Round Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student observations	3162	1452	1778	1002	2351	871	2482
Control Mean	3.67	3.74	3.612	3.615	3.69	3.49	3.73

Note: The above estimates are derived from pooling the baseline, endline, and follow-up survey of our participating households. The outcome variable in the above figure is a continuous variable measuring study hours reported by parents. For the 2SLS estimates, we instrument for compliance, that is, whether any member in the treated household received SMS or voice calls, with treatment assignment. The specifications control for unbalanced covariate at the baseline—ownership of bicycle—and school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "Treated" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A4: Compliance and Study Hours

Dependent variable	Self-Reported Compliance			Daily Study Hours in Past One Year				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SMS Success	0.739***		0.755***	0.580***		0.567***		0.643***
	(0.022)		(0.091)	(0.086)		(0.087)		(0.283)
	[0.001]		[0.001]	[0.001]		[0.001]		[0.009]
Voice Call Success		0.811***	-0.020		0.725***		0.612***	-0.095
		(0.028)	(0.110)		(0.108)		(0.101)	(0.331)
		[0.001]	[0.207]		[0.001]		[0.001]	[0.207]
Regression	OLS	OLS	OLS	2SLS	2SLS	OLS	OLS	OLS
Strata Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student observations	1,077	1,077	1,077	1,077	1,077	1,077	1,077	1,077
Control Mean	0.000	0.000	0.000	3.525	3.525	3.525	3.525	3.525

Note: The above estimates are derived from the follow-up survey of 1,077 parents. The outcome variable in Columns (1)-(3) is the self-reported measure of whether households received SMS or voice calls in the past year and takes unity if yes, and 0 otherwise. The outcome variable in Columns (4)-(8) is the average daily study hours in the past year as reported by parents in the follow-up survey. "SMS Success" is the proportion of weeks in which a household successfully received SMS, and "Voice Call Success" is the proportion of weeks in which a household successfully received voice calls. In Columns (4)-(5), "SMS Success" and "Voice Call Success" are instrumented by treatment assignment. The specifications control for unbalanced covariates at the baseline — ownership of a bicycle — and for school-grade-gender fixed effects. Robust standard errors are clustered at the school-grade-gender level. Anderson's sharpened q-values from the test of "SMS Success" and "Voice Call Success" coefficient being equal to zero in all of the columns are reported in square brackets. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A5: Math Test Items

I. Price of one mango is 80 taka. What is the price of two mangoes?	☐1. 40 ☐2. 80 ☐3. 100 ☐4. 160 ☐5. 250
II. Calculate: $(0.15 \times 3) + 1$	☐1. 0.45 ☐2. 0.46 ☐3. 0.55 ☐4. 0.60 ☐5. 1.45
III. Calculate: 4/0.2	☐1. 2 ☐2. 4 ☐3. 10 ☐4. 20 ☐5. 80
IV. Calculate: $0.01 \times 0.2 \times 3$	☐1. 6 ☐2. 0.6 ☐3. 0.06 ☐4. 0.006 ☐5. 0.0006
V. If $(x/8) + 10 = 17$, what is the value of x?	☐1. 1 ☐2. 9 ☐3. 32 ☐4. 56 ☐5. 80
VI. How many seconds are there in 5 days and 5 hours?	☐1. 125 ☐2. 3600 ☐3. 7200 ☐4. 86400 ☐5. 450000
VII. What is the largest number of students among whom 84 eggs and 120 breads can be distributed in equal amount?	☐1. 4 ☐2. 6 ☐3. 12 ☐4. 24 ☐5. 48
VIII. Sandip's scores are 75 in 2 subjects and 95 in the remaining 8 subjects. What is the average score across all subjects?	☐1. 79 ☐2. 82 ☐3. 85 ☐4. 88 ☐5. 91
IX. The sum of Sakib's age and Sajid's age is 66 years. Sakib is 6 years older than Sajid. What is Sakib's age?	☐1. 27 ☐2. 30 ☐3. 33 ☐4. 36 ☐5. 39
X. Mr. Nogen has kept $\frac{4}{17}$ of his property for himself and $\frac{3}{17}$ for his wife. The rest is equally distributed among his 5 sons. Each son receives 1,234 taka. How much is the property Mr. Nogen kept for himself worth?	☐1. 617 ☐2. 1234 ☐3. 1851 ☐4. 2468 ☐5. 20978

A.4 A Simple Dynamic Model of Parental Engagement and Study Time

This appendix presents a stylized dynamic model that formalizes the micro-foundational framework in Section 4 and in Figure A4.

A.4.1 Setup

Time is discrete, $t = 1, \ldots, T$. Parent i chooses engagement $e_{it} \geq 0$ each period. Higher engagement increases children's study hours but is privately costly. Children's study hours are given by a reduced-form study response function

$$s_{it} = \phi(e_{it}; A_i, \eta_i), \qquad \phi_e(\cdot) > 0, \;\; \phi_{ee}(\cdot) \leq 0, \tag{A2}$$

where $A_i \in \{0, 1\}$ indicates baseline access to educational technology (TV or internet) an η_i captures child-specific heterogeneity.

Parents derive benefits from study time through perceived learning/human capital accumulation. For tractability, assume per-period benefits are $B(s_{it})$ with $B'(\cdot) > 0$, $B''(\cdot) \leq 0$, and engagement costs are $C(e_{it})$ with $C'(\cdot) > 0$, $C''(\cdot) \geq 0$. Parent i solves:

$$\max_{\{e_{it}\}_{t=1}^{T}} \sum_{t=1}^{T} \beta^{t-1} \Big(B(s_{it}) - C(e_{it}) \Big), \qquad \text{s.t. } s_{it} = \phi(e_{it}; A_i, \eta_i), \tag{A3}$$

where $\beta \in (0, 1]$.

A.4.2 First-order conditions

Substituting (A2) into (A3), the problem is separable by t. An interior optimum $e^*_{it} > 0$ satisfies, for each t,

$$B'(s^*_{it}) \cdot \phi_e(e^*_{it}; A_i, \eta_i) = C'(e^*_{it}). \tag{A4}$$

Equation (A4) equates the marginal benefit of engagement (via the induced increase in study hours) to the marginal cost.

A.4.3 Modeling the informational nudge

Let $T_i \in \{0, 1\}$ denote treatment assignment. The HFI nudge can be represented as shifting (i) perceived returns, (ii) engagement costs, and/or (iii) the productivity of engagement.

(i) Returns (salience/beliefs). Suppose treatment increases perceived returns by scaling benefits: $B_T(s) = (1 + \lambda_i)B(s)$ with $\lambda_i \geq 0$ and $\lambda_i = \lambda_0 + \lambda_1(1 - A_i)$ potentially larger when $A_i = 0$ (more constrained settings). In particular, when households lack

access to educational technology ($A_i = 0$), parental engagement plays a more central role in shaping study behavior, implying a higher marginal effect of engagement on study time (λ_i). This reflects fewer substitutes for parental input rather than a mechanical effect of technology access. Then the FOC becomes

$$(1 + \lambda_i)\, B'(s^*_{it}) \cdot \phi_e(e^*_{it}; A_i, \eta_i) = C'(e^*_{it}). \tag{A5}$$

A higher λ_i increases the LHS, implying higher optimal engagement and thus higher study hours.

(ii) Engagement costs (actionable guidance). Alternatively, let treatment reduce the marginal cost of engagement by making costly information acquisition cheaper: $C_T(e) = C(e) - \kappa_i e$ with $\kappa_i \geq 0$. Then

$$B'(s^*_{it}) \cdot \phi_e(e^*_{it}; A_i, \eta_i) = C'(e^*_{it}) - \kappa_i, \tag{A6}$$

which again implies higher e^*_{it} relative to control.

(iii) Productivity of engagement. Finally, treatment may raise the productivity of engagement by shifting ϕ:

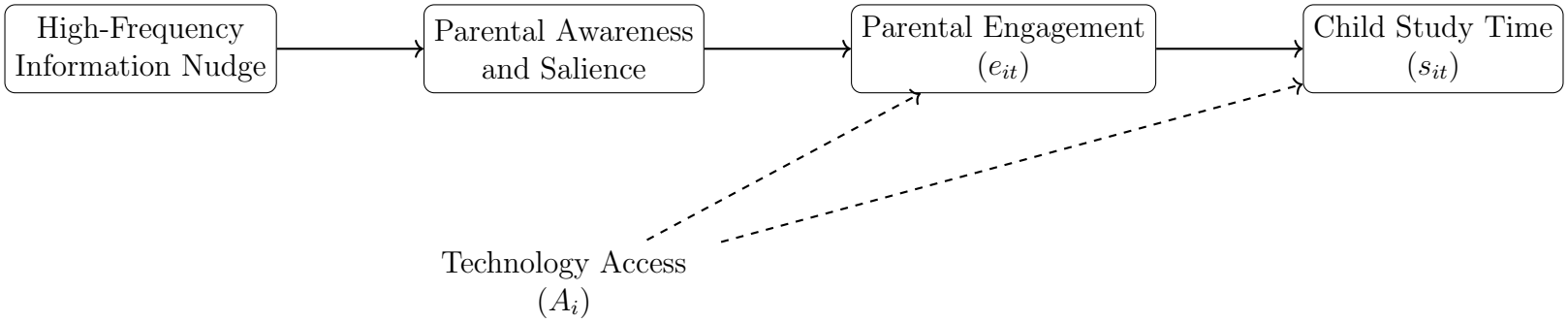
$$s_{it} = \phi(e_{it}; A_i, \eta_i) + \pi_i T_i, \tag{A7}$$

or by increasing ϕ_e (e.g., engagement becomes more effective when parents are better informed). In either case, optimal engagement rises under standard curvature conditions.

A.4.4 Heterogeneity by technology access

The model rationalizes larger effects when $A_i = 0$ (no TV/internet) if informational constraints bind more tightly. This can be captured by: (a) larger salience shift $\lambda(A_i)$ when $A_i = 0$, (b) larger cost reduction $\kappa(A_i)$ when $A_i = 0$, and/or (c) higher marginal product $\phi_e(\cdot; A_i, \eta_i)$ when households have fewer substitute learning inputs. These mechanisms imply that e^*_{it} and hence s^*_{it} increase more in low-access households.

Figure A4: Conceptual Framework: Parental Information, Engagement, and Study Time



A.5 Pre-Registration

This study was pre-registered in the AEA RCT Registry (AEARCTR-0007674). The initial registration date was May 16, 2021, and sample recruitment began in July 2021, while baseline was initiated in September 2021. The pre-analysis plan (PAP) document was uploaded on June 2, 2022, prior to the midline, endline, and follow-up surveys. Below, we describe the different sections of the pre-registration and note any deviation.

Experimental Design. In our experimental design, we implemented one high-frequency treatment (regular and repetitive information nudges) and a control, as originally planned and documented in PAP (1200 subjects, divided equally between treatment and control). In addition to information nudging related to televised programs, we also include information on online education programs, teachers' contact numbers, and educational trivia (such as the benefits of education), rotating across the weekly nudges, as explicitly described in PAP.

Timeline and Surveys We completed the project within the set timeline (Start date: 13th June, 2021, and End date: 30th June, 2023). We hoped that endline data collection would be sufficient to assess the self-study impact of the intervention, along with administrative exam performance data for learning impact (Junior School Certificate Examination (JSC) for grade 8 and Secondary School Certificate (SSC) for grade 10 in April–May 2023). However, severe disruptions to the national education system limit our ability to use these administrative exam outcomes reliably, effectively eliminating standardized academic assessments as a secondary outcome during the study period.

Moreover, we realized that the endline was administered too close to the intervention's completion (due to the grant completion deadline), overlapping with grade-progression examinations, during which students were preparing intensively, reducing the likelihood of observing meaningful differences in study hours between treatment and control. As a consequence, immediately after completion of the endline and grant closure due diligence, we submitted a protocol modification to the SMU-IRB in March 2023 (IRB-21-064-A059(421)) and received approval to conduct a follow-up phone survey with parents in 2023. This telephonic survey, conducted approximately eight months after the intervention completion, collected parent-reported data on study hours, education-related activities, and SMS receipt (but not learning outcomes due to the remote survey), which we used in our main analysis. The content and structure of this phone survey closely mirrored the original parent questionnaires.

Key Dependent Variables (Primary): In the pre-analysis plan (PAP), we specified three primary outcomes: study hours (which is the central variable used in the current study), along with children's viewership of educational programs and teacher involvement. During the endline, we recognized limitations in our ability to measure children's viewership of educational TV or online content. Because no account-based tracking

systems exist in our setting, we lack an objective record of TV or online viewership. Even if such data were available, accurately identifying which household member viewed the content would remain challenging, given the common practice of sharing televisions and mobile devices among family members, friends, and neighbors. Upon further review, we also recognized that classifying teacher involvement as a primary outcome was not appropriate. Rather, teacher involvement is better understood as a mechanism through which the intervention may affect students' study behavior, and we therefore treat it as such in the present analysis.

Key Dependent Variables (Secondary): We registered academic performance—measured using school tests and terminal examination administrative records—as a secondary outcome. However, frequent school disruptions in 2022 (see Section 2.1) substantially limited the feasibility of using these data. In the post–COVID period, Bangladesh's Ministry of Education implemented a simplified syllabus alongside a flexible "promoted/not promoted" grading policy, which effectively suspended standardized academic assessments for our study cohort. As a result, administrative exam data lacked consistency and comparability over time and could not be used as a secondary dependent variable in the analysis.

Additionally, we had planned to administer an incentivised monthly SMS quiz to all study participants, requiring students to submit their answers within one day of receiving the questions. However, response and compliance rates for these quizzes were extremely low, making it impossible to draw meaningful inferences. For this reason, we refrained from using the SMS quiz data in our analysis. Our other two secondary outcomes, Child Labor and Marital Status, have not been used and are left for a separate study.

Analysis - Primary Analyses. Following the pre-registered baseline specification, we conducted intent-to-treat (ITT) regression analysis to measure the impact of the interventions on the main outcome of interest, "study hours." However, for the subsample analysis, to show impact heterogeneity, we took three variables (gender, parental education, and technology) out of the 9 listed in the PAP to make a focused analysis and discussion. We did not use most of the further analysis section of PAP, as pre-registered, for the robustness checks, except for the Measuring Compliance Adjusted Treatment Effects (treatment-on-the-treated estimates).

Analysis - Exploratory. Our study incorporates additional mechanism tests, polled estimates, and exploratory analyses of how our intervention affects mobility and inequality in study hours, which were not pre-registered.

These deviations reflect the reality of conducting research during an unprecedented period of educational disruption. We believe that the adjustments made—particularly the follow-up telephonic survey—allow us to better understand how low-cost behavioral interventions may influence study behaviors and reduce educational inequality, even in the absence of observable short-term learning gains.