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TELEMENTORING AND HOMESCHOOLING DURING SCHOOL CLOSURES: A RANDOMISED EXPERIMENT IN RURAL BANGLADESH*

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Using a randomised experiment in 200 Bangladeshi villages, we evaluate the impact of an over-the-phone learning support intervention (telementoring) among primary school children and their mothers during Covid-19 school closures. Post-intervention, treated children scored 35% higher on a standardised test, and the homeschooling involvement of treated mothers increased by 22 minutes per day (26%). We also found that the intervention forestalled treated children's learning losses. When we returned to the participants one year later, after schools briefly reopened, we found that the treatment effects had persisted. Academically weaker children benefited the most from the intervention that only cost USD20 per child.

Educational disruptions in low- and middle-income countries are ubiquitous. Natural and human-induced events often damage educational infrastructure and limit school operations, creating significant barriers to the learning of children worldwide. For instance, the 2010 floods in Pakistan affected one-fifth of the country's population, damaging and shutting down schools for months (Eble *et al.*, 2021). Recurring natural disasters in Bangladesh, such as floods and cyclones, force schools to shut down every year (*Daily Star*, 2014). In Syria, 40% of schools have been severely damaged and about 2.5 million children have been out of school since the conflict began in 2011 (UNICEF, 2021). In West Africa, the Ebola outbreak disrupted the schooling of about 5 million children for nine consecutive months (World Bank, 2015). Moreover, frequent political unrest and protests, such as *hartals*, in India force schools to operate for nearly one month shorter than the minimum requirement to cover the yearly syllabus (*New Indian Express*, 2019).

These pre-existing problems were exacerbated by the Covid-19 pandemic when about 1.5 billion students worldwide were affected by partial or full closures of schools (UNESCO, 2021a). School closures in many countries lasted for over a full school year and about one-third of students, primarily in low- and lower-middle-income countries, were unable to study remotely due to the lack of digital connectivity, devices and effective learning support at home (Azevedo *et al.*, 2021;

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Bacher-Hicks *et al.*, 2021; Larsen *et al.*, 2021; Parolin and Lee, 2021; UNESCO, 2021b). As many children in developing countries are first-generation learners, their parents usually do not have the ability or confidence to support their learning at home (Banerjee and Duflo, 2006; Hanushek and Woessmann, 2015; Glewwe and Muralidharan, 2016; Agostinelli *et al.*, 2022). Thus, the pandemic has disproportionately worsened the learning of these children and led to calls for better leverages on low-cost and widely accessible technologies, such as mobile phones, to improve educators' engagement with these children and their parents (Muralidharan and Singh, 2021).

This paper evaluates the impact of a multifaceted educational intervention that relied on basic feature mobile phones for treatment delivery. To help with the learning of rural children at home during Covid-19 school closures, we engaged public university students in Bangladesh as volunteers to provide learning support to primary school children and their mothers through phone calls and text messages. Children received weekly tutoring (30 minutes per session) on mathematics and English—two core subjects that Bangladeshi students struggle with the most—and mothers received homeschooling mentoring over the phone (telementoring hereinafter), which was not otherwise available to them.[1] Support for mothers involved structured guidance through weekly phone calls and text messages to facilitate and improve homeschooling. An over-the-phone intervention in Bangladesh was the most suitable option during this period because about 95% of rural households have access to at least one basic phone, while only 33% have internet access (UNICEF, 2019). Bangladesh also had one of the longest and most restrictive school shutdowns in the world, which lasted for 18 consecutive months.[2]

The design of this multifaceted educational intervention is informed by theoretical and empirical literature on the determinants of out-of-school educational production and human capital investment. The literature on educational production highlights the importance of out-of-school educational inputs, such as private tutoring and parental time, in influencing children's achievement (Hanushek, 1979; Todd and Wolpin, 2003). The intervention involves one-on-one direct tutoring of children, which is recognised as an effective method to boost learning (Nickow *et al.*, 2024). The intervention also aims to augment the effectiveness of parental homeschooling time via several pathways, which can lead to improved learning for children. First, the mentoring of mothers focuses on equipping mothers with skills and resources to effectively homeschool. It addresses constraints that mothers may face, such as gaps in subject knowledge or teaching techniques, and offers solutions to homeschooling challenges. All of these are expected to improve the quality of homeschooling time. Second, the mentors potentially serve as role models for mothers. Witnessing the success and methods used by trained mentors may induce a behavioural change in the mothers, motivating them to replicate these effective teaching methods. Third, the one-on-one nature of mentoring provides a unique opportunity for customisation, allowing advice to be tailored to a mother's strengths and weaknesses. This may help increase their confidence in teaching, which in turn lowers the psychological cost of homeschooling and increases the time spent on homeschooling. The literature on human capital investment highlights the important

[1] Single and Muller (1999) define telementoring as electronic communications (primarily over the phone) between a mentor and a mentee with a goal to develop and grow the skills and knowledge of the mentee.

[2] Schools were initially shutdown on 17 March 2020, and then partially reopened on 12 September 2021. Schools were again closed on 21 January 2022, and then fully reopened in mid-March 2022. In total, there are 37 million primary school children in Bangladesh, a country of 165 million people (Alamgir, 2022). Given poor digital connectivity in rural areas, the government used public broadcasting (via television and radio) for asynchronous lessons targeted towards school students (UNICEF, 2020). However, it was largely problematic because over half of rural households do not own a television and only 3% of rural households listen to the radio (UNICEF, 2019).

roles that parental beliefs and expectations play in their investment in their children's learning (Attanasio *et al.*, 2020). Mentoring can potentially reshape a mother's expectations and subjective beliefs about the returns to homeschooling and the effectiveness of homeschooling. The altered expectations may promote a more positive attitude and induce mothers to invest more time in homeschooling and set higher goals for their children's academic and emotional growth.

We evaluate this intervention using a randomised controlled trial implemented in 200 Bangladeshi villages. In the treatment group (419 households), mother–child dyads received weekly telementoring, while those in the control group (419 households) did not receive any support. Note that the control group did not have access to alternative learning opportunities, as online/over-the-phone teachings were unavailable and access to television and radio was very limited in rural areas. The intervention ran for 13 weeks in late 2020 when all schools were closed. One month after the intervention ended (in January 2021), we conducted standardised learning assessments among children and surveys among mothers to evaluate the immediate impact. We then returned to the participants one year later (in December 2021)—when schools briefly reopened—and conducted a second round of standardised learning assessments and surveys to evaluate the medium-term impact. All learning assessments and surveys were conducted face-to-face when social distancing rules were relaxed by the government.

We find several important results. One month after the intervention ended (first endline), treated children scored 0.66 SD or 52% higher in English literacy and 0.56 SD or 33% higher in numeracy relative to children in the control group. The positive impacts persisted one year after the intervention ended: 0.30 SD (19%) higher in English and 0.44 SD (20%) higher in numeracy. To put this in more tangible terms, treated children provided, on average, one additional correct answer in both English literacy and numeracy at each endline. We also find positive spillovers on two other core subjects taught in Bangladeshi schools, Bangla and general knowledge, which were not targeted by the intervention. At the first endline, treated children scored 0.62 SD (37%) higher in Bangla literacy and 0.50 SD (22%) higher in general knowledge relative to the untreated children. In practical terms, this means that the treated children provided, on average, half an additional correct answer in both Bangla literacy and general knowledge. What is particularly noteworthy is that the spillover impacts also persisted. Treated children continued to score higher in Bangla (21 SD or 10%) and general knowledge (0.23 SD or 13%) one year after the intervention ended. Further analysis suggests our intervention effectively forestalled learning loss among children in the treatment group and the treatment effect arises from a significant drop in learning over time in the control group. We also find considerable differences in impacts between academically weaker and stronger children at the first endline, where the treatment effect is larger for children who were academically weaker at baseline. However, this heterogeneity disappears after one year. In other respects, such as gender and socioeconomic background of children, we do not observe any heterogeneity in treatment effects.

We also find significant increases in mothers' daily time spent on their children for homeschooling—an average of 22 minutes per day in the first and 14 minutes per day in the second endline—and activities regarding playing and storytelling. Importantly, increased daily time input neither crowded out mothers' involvement in income-generating activities nor had any negative implications on their mental well-being and leisure. On parenting perceptions, we find that negative parenting (such as frequent punishments and coercive interaction) decreased, self-reported parenting skills increased, confidence in homeschooling increased, and aspirations about children's educational attainment increased significantly following the intervention. Later,

using a Marlowe–Crowne Social Desirability Scale, we address potential social desirability bias concerns pertaining to these subjective outcomes.

Although important findings on their own, positive impacts on homeschooling involvement and parenting also contribute to our understanding of the underlying mechanisms why children's learning outcomes were positively affected and persisted. Beyond these channels, we investigate several other potential mechanisms for forestalling learning losses using a survey conducted during the second endline. First, mothers in the treatment group reported that children's fathers also began homeschooling, and children themselves began spending more time on their homework from school. However, self-assisted studying, starting new private tuition, increased tutoring input by existing tutors, etc., were not affected by the treatment and, hence, are unlikely to be potential channels. Second, as the second endline was conducted immediately after schools briefly reopened, we also surveyed teachers about students' school-related activities. According to teachers, treated children appeared more attentive during classroom teachings, but their interest and time commitment to classwork, playing after school, and the ability to catch up and recover from missed schoolwork were similar to those in the control group. Thus, fathers' involvement in learning support and children's improved study habits appear to have played important roles.

The impacts on learning outcomes are remarkable. The persistent effects are likely the result of an interplay of various factors: not just the brief learning support during the health crisis, but also the continued engagement of parents in their children's education, children's own effort, parenting quality and perceptions, as well as the aspirations generated by interactions with the mentors. All these elements may collectively contribute to the transformative and lasting benefits observed in vulnerable children.

Our intervention provides a unique intersection between volunteer-driven educational interventions and parental involvement in education, particularly for mothers with limited educational backgrounds. Nickow *et al.* (2024) indicate that randomised interventions focusing on 'parent tutoring' are not common in the literature, but they are highly effective, showing a pooled programme effect of 0.23 SD on standardised test scores. Drawing on the maternal literacy programmes explored by Banerji *et al.* (2017), as well as the parental involvement studies by Avvisati *et al.* (2014), Islam (2019) and Koepp *et al.* (2022), our paper innovates by combining these two streams of research. We demonstrate that volunteer-delivered learning support via basic mobile phones can both counteract learning losses during school closures and effectively involve mothers, even those with limited education, in their children's learning.

Our study also contributes to the recent literature on the effectiveness of distance learning and mentoring interventions on students' learning outcomes during the Covid-19 pandemic. For instance, Angrist *et al.* (2022) show that weekly phone calls and test messages from an non-governmental organisation (NGO) to parents of primary school-aged children in Botswana, over five weeks, improved the learning outcomes of children by 0.12 SD. In Brazil, nudges through text messages significantly improved standardised test scores of high-school students (Lichand and Christen, 2021). Crawfurd *et al.* (2023) find that 15-minute weekly tutoring calls with children from their school teachers in Sierra Leone, over 16 weeks, increased educational engagement by parents (0.31 SD) and children (0.34 SD), but did not affect test scores. Wang *et al.* (2023) find that weekly 70–90 minutes of audio lessons accessed via interactive voice response by primary school-aged children in rural Bangladesh, over 15 weeks, improved their numeracy and literacy scores by 0.60 SD. In the context of developed countries, Carlana and La Ferrara (2021) find that tutoring programmes via videoconferencing in Italy led to a 0.21 SD improvement in middle school children's learning outcomes. Similarly, Hardt *et al.* (2022) find that the use of remote peer

mentoring had positive effects on students' motivation, study behaviour and exam registration at a German university.[3]

Our key contribution, relative to these existing studies, is that we show volunteer-delivered learning support via basic mobile phones can be particularly effective in addressing learning losses in poor environments. As more than a quarter of the adult population volunteers their time in many countries, including Bangladesh, they provide a large reserve of manpower in delivering low-cost services to communities in need (Islam *et al.*, 2018). As a result, our intervention only costs USD20 per mother–child dyad, making it low-cost and policy relevant. However, this per-unit cost would significantly increase if the programme were scaled up by building a fully independent infrastructure for implementation and oversight. This is because the programme leveraged the unpaid involvements of two primary investigators and volunteers during implementation. More broadly, our findings also indicate that telementoring can be a potential remedy for learning disruptions caused by other shocks, such as conflict, political unrest, natural disasters, teacher strikes and teacher absenteeism, which many developing countries frequently encounter (Banerjee and Duflo, 2006; Chaudhury *et al.*, 2006; Islam, 2019). A further novelty of our study is that we demonstrate both immediate and one-year impacts of an intervention that was implemented and evaluated amid the pandemic. Importantly, all learning assessments and data collection were conducted in person, as opposed to remote surveys or assessments conducted in most aforementioned studies, which allowed us to test a much broader range of skills.

Our study also sits within the broader literature on after-school tutoring, remedying education and targeted instructions (Banerjee *et al.*, 2007; Duflo *et al.*, 2020; Eble *et al.*, 2021). In-person tutoring, with or without fees, has been found to be highly effective in improving learning outcomes (Carr and Wang, 2018; Islam and Ruthbah, 2020; Nickow *et al.*, 2024). Specifically, one-on-one or small group tutoring is particularly beneficial for students that struggle (Ander *et al.*, 2016). The reason is that it allows the educator to target instruction and teach at the right level (Banerjee *et al.*, 2007). Existing studies have also shown that delivering targeted instructions through technology can be highly effective for learning (Banerjee *et al.*, 2007; Muralidharan *et al.*, 2019; Escueta *et al.*, 2020). Aker *et al.* (2012) assessed the effects of an adult education programme in Niger that incorporated mobile phones. They discovered that integrating mobile phones as teaching tools led to higher writing and math scores compared to a conventional adult education approach. However, in-person or distant tutoring that requires computing facilities and internet access is often unavailable to children in low-income countries, particularly in rural

[3] Our study closely aligns with Angrist *et al.* (2022) and Crawfurd *et al.* (2023) in terms of treatment delivery and context. However, there are several programme features that are different from Angrist *et al.* (2022) and Crawfurd *et al.* (2023). First, our primary focus was on mentoring and guiding mothers to enhance homeschooling quality and engagement (akin to the CHAMP model in Banerji *et al.* (2017), but relatively shorter and remote), whereas other studies emphasised directly tutoring children. We opted for a mentoring approach to ensure that children's at-home learning would continue even after our intervention concluded. Providing direct tutoring to the children might have posed a risk, potentially leading to a cessation of home-based learning once the intervention ended. Second, we sent weekly text messages to parents to encourage them to enhance homeschooling quality. Third, for treatment delivery, we recruited highly motivated university students as volunteering mentors, while the aforementioned studies employed NGO employees and school teachers. Fourth, our mentors supported only two mother–child pairs—a ratio significantly smaller than in the aforementioned studies. Fifth, in terms of direct dosage, our intervention spanned 13 weeks with a direct dosage of 6.5 hours, in contrast to the 3 and 4 hours in Angrist *et al.* (2022) and Crawfurd *et al.* (2023), respectively. Moreover, we also examined impacts beyond the short term, specifically one year after the intervention ended, in contrast to the more immediate results observed 2–4 months post-intervention in the other studies. Finally, while all three studies are set in developing countries, our context saw school closures lasting 20 months, compared to 6 and 7 months in Botswana and Sierra Leone, respectively.

contexts. Our findings, thus, demonstrate that phone-based distant support can mitigate such instruction delivery challenges.

1. Study Design and Data

1.1. *Experimental Design*

1.1.1. *Telementoring*

We partnered with a research NGO, Global Development and Research Initiative (GDRI), to implement and evaluate our telementoring intervention using a randomised controlled trial (RCT) in rural Bangladesh. Our sample consists of 838 mother–child dyads distributed across 200 villages in five subdistricts of the Khulna Division (map in Figure A1, Online Appendix A). Our unit of randomisation was at the individual level. We recruited student volunteers from various local public universities as mentors to provide learning support to primary school children (grades 1–3) and homeschooling support to their mothers every week for 13 consecutive weeks (from early September to late December 2020). During the intervention period, each mentor called a mother once a week at a pre-determined time and day to provide educational support over the phone. Each session, which lasted roughly 30 minutes, had seven brief steps:

1. Greetings and preparation. The mentor interacts with the child and mother (2 minutes).
2. Setting time commitment goals for the current week's homeschooling. The mentor advises the mother about items for her time diary to reach goals (2 minutes).
3. Previous week's homeschooling challenges and understanding weaknesses, such as identifying difficult problems/questions in textbooks. In this step, the mentor interacts with both the child and mother (4 minutes).
4. Solving problems identified in Step 3 with both the child and mother and then asking the child to solve similar problems (12 minutes).
5. Theme-based discussions (based on the text messages discussed below) with the mother, while the child continues solving problems from Step 4 (5 minutes).
6. Assigning homework based on the current week's problems and advising the mother about how to help with the homework (3 minutes).
7. Setting date, time and agenda for next week, and saying goodbye (2 minutes).

Through GDRI, treated mothers were also provided with printed solutions to textbook problems and a study plan (i.e., which textbook chapters are to be covered in which week) of the telementoring programme at the beginning of the intervention.[4] Printed solutions played an important role in this intervention. Various problems, particularly in math, require the visualisation of step-by-step solutions to fully grasp the solving technique. The printed solutions enabled mentors to walk through the steps over the phone while children and mothers followed along, thereby enhancing their understanding of the exercise. The children may also rely on printed solutions for self-assisted studying. In addition, there were ten different weekly themes for text messages and discussions in Step 5. These theme-based text messages were sent to mothers (composed in *Bangla*) weekly, in weeks 3–12. Themes include positive parenting, gender equality in education, thinking positively about children's future, the importance of following a routine, etc. The objective was to encourage mothers to act on the themes and facilitate more interaction with

[4] For instance, the study plan for grade 2 was on chapters/units 1–10 in the English and mathematics textbooks.

children. Table B1 (Online Appendix B) lists these themes and provides a brief overview of the text messages sent. Each text message was sent twice, once before and once after each session.

Mentors only provided support on two core subjects—mathematics and English—which Bangladeshi students struggle with the most. The tutoring component of the intervention (Steps 3–4) mimics the status quo of private tuition in Bangladesh—tutors help children with problems/topics that they struggle with. Thus, tutoring involved solving and explaining problems in children's existing textbooks—problems that mothers could not solve or explain to children in the previous week—as *no* new curriculum or contents were developed for this study. Mentors, however, did not keep records of the problems covered during sessions. Qualitative feedback from mentors suggests it was rare for phone calls to end early. Figure B1 (Online Appendix B) shows pictures from the intervention.

1.1.2. *Recruitment of mentors*

In July 2020, we announced a call for volunteer mentors on various universities' official Facebook pages. Initially, 267 university students signed up as prospective mentors. We conducted an introductory training followed by three additional training seminars on education and development in the context of Bangladesh. Training sessions were conducted via videoconferencing on four different days. Two co-authors of this study, Hashibul Hassan and Asad Islam, conducted the training. Eventually, 219 volunteers were recruited as mentors, as the remaining 48 volunteers could not be contacted. Mentors were also given relevant books and solution manuals (in digital format), a 13-week plan outlining the weekly themes, and mentoring guidelines adapted from the guidelines of the Government Teacher's Training College, Bangladesh.[5] A small team from GDRI did support Hashibul Hassan and Asad Islam in the implementation, but their role was limited to distributing printed copies of textbook solutions to treatment households and conducting a rapid survey on the mentors only. Table A1 (Online Appendix A) summarises the characteristics of the recruited mentors. On average, they were 22 years old and studied social sciences in their undergraduate degrees. Half of them were female and over three-quarter of them had tutoring experience.

1.1.3. *Sampling and randomisation*

Our local partner, GDRI, has a survey dataset from a previous research project from 2018/19 that includes contact information on 6,503 households from 223 villages in the Khulna Division. We used households from this existing survey for our randomisation because it was not feasible for the NGO to collect mobile phone numbers from new households at the onset of the pandemic. From this list, we randomly selected 1,500 households that met our eligibility criteria: children were enrolled in grades 1–3 at any public primary school and households had at least one mobile phone. We were successful in contacting and inviting mothers from 1,042 households, as the remaining 458 phone numbers were found to be either *switched off* or *invalid*. At the end of the invitation call, we also conducted a rapid survey to check if they still met the eligibility criteria. Only 838 continued to meet the eligibility criteria based on the rapid survey. We randomly assigned half of 838 households (419) to the treatment arm—those who received weekly telementoring—and the remaining half (419) to the control arm—no telementoring was provided. At the first endline, we were able to conduct standardised assessments and surveys on 814 households (attrition of 3%). At the second endline, this number further dropped to 796 households (attrition of 5%). We

[5] The mentoring guidelines describe child development stages, ideas for better interactive telephone sessions, time management tips and the 'dos and don'ts' for running mentoring sessions.

have low attrition possibly because the NGO is known to and trusted by households (through past research activities) and is well-regarded in this region. Reasons for attrition are outlined in Figure A2 (Online Appendix A). Section 1.3 discusses attrition and conducts various checks to address it.

To ensure data quality, enumerators that measured outcomes were kept blind to the treatment status. Also, there were no overlaps between the enumerators of this project and those from the 2019 survey. First, we cross-checked the names of enumerators from this study with those from the 2019 household survey. Second, enumerators at GDRI work on a contractual basis, making overlaps across different data collection periods unlikely. Therefore, enumerators from the current study should not be known to the participating mothers and children.

1.1.4. *Mentor–mentee assigning*

Each mentor was randomly assigned to two primary school children in the same grade and their mothers (mentees). We allocated 419 mentees to 210 mentors. The remaining 9 mentors were kept as a reserve. During the first two weeks, 22 mentees in the treatment arm dropped out due to problems with mobile phone availability, family issues, etc. Moreover, 13 mentors left in the first two weeks due to personal reasons, leaving us with 397 mentees and 206 mentors in the treatment group.[6] Therefore, we re-organised the mentor–mentee matches after the second week by randomly re-assigning mentees whose mentor left to mentors whose mentee(s) left. From the third week onwards, none of the remaining mentees or mentors dropped out.

1.2. *Data*

1.2.1. *Learning outcomes*

Learning outcomes were measured using a standardised one-on-one assessment test: word translation, fill-in-the-blanks, additions, etc. The exact questions asked are given in Table B2 and Table B3 (Online Appendix B). All test questions were created by closely following existing textbooks developed by the National Curriculum and Textbook Board, Bangladesh.[7] Therefore, the difficulty level of assessments was analogous to that of problems/questions in the textbooks, and the tutoring component of the intervention directly maps into our main learning outcomes. During the assessment, assessors verbally asked questions to children and recorded their answers on a tablet computer. We intentionally did not include any questions that could be partially correct in order to reduce assessment biases. For example, if the assessor asked, 'What is the sum of 6 and 0?', then they recorded the answer as correct if the answer was 6 and incorrect if otherwise. There were four segments in the test: English (6 questions, 30 points), numeracy (5 questions, 30 points), Bangla (4 questions, 20 points) and general knowledge (4 questions, 20 points). We consider English and numeracy as the main learning outcomes, as they were directly targeted by the intervention.

[6] Mentoring was only given to child–mother dyads in the treatment arm while those in the control arm did not receive mentoring; thus, dropping out occurred in the treatment arm only.

[7] Due to school closures and exam cancellations in Bangladesh for over two years, we could not use school administrative data. Instead, we designed our tests to mirror primary school exams, which are closely following the textbooks (but not copied directly). To compare questions, visit http://www.nctb.gov.bd/ to access English textbooks from grades 1–3.

1.2.2. *Mothers' involvement outcomes*

Each mother's time engagement in their child's learning and leisure activities is measured using two survey questions answered by the mother. One is about average daily time spent on homeschooling (based on their time diary); the other is about average daily time spent (in minutes) on leisure activities, such as storytelling and playing.

1.2.3. *Parenting perceptions outcomes*

We have four measures for parenting: (i) *negative parenting avoidance*, which is the sum of five dummy variables, such as avoiding the use of abusive words and beating. A higher score on *negative parenting avoidance* means a more favourable outcome; (ii) *parenting ability*, which is the sum of 11 items, each answered on a 5-point Likert scale, assesses the perception of the mother in her parenting role; (iii) *future aspirations* about children's education, which is a categorical variable where a higher value corresponds to higher aspirations; and, (iv) *homeschooling confidence*, which is the sum of three 10-point scales regarding the mother's confidence in teaching at home.

1.2.4. *Baseline*

We also have baseline measures of learning (only literacy and numeracy) and parental involvement in education from the 2019 survey. The remaining outcomes were only measured at endlines. We also use household characteristics sourced from the 2019 survey as our baseline controls.

All learning assessments and surveys were conducted face-to-face. We convert all outcomes into standardised indices following Kling *et al.* (2007), outcomes of control groups have mean 0 and SD 1.

1.3. *Sample Characteristics, Balance and Attrition*

Table 1 reports our baseline sample characteristics by treatment and control status, where children are about 7.5 years old and 50% are female, and parents have about 6 years of education and earn BDT11,500 (USD135) per month. Their homeschooling time involvement was approximately 135 minutes per day. Also, about 60% of children had private tutors. Importantly, these characteristics are balanced across the two arms (joint F-test $p = 0.60$).

Since we had multiple sampling stages, we conduct three different comparisons of household characteristics and present these tables in Online Appendix A: (i) among 6,503 households from the 2019 survey, 5,003 unselected versus 1,500 randomly selected (Table A2; joint F-test $p = 0.25$); (ii) among 1,500 randomly selected households, 662 that were excluded for various reasons versus 838 that participated (Table A3; joint F-test $p = 0.13$); (iii) among 1,042 contacted households, 204 that did not participate versus 838 that participated (Table A4; joint F-test $p = 0.44$). When compared individually (12 tests per table), we find differences in baseline numeracy and literacy and father's education. However, overall, the characteristics of samples are largely similar, as suggested by the joint tests.

In Table A5 (Online Appendix A), we also compare household characteristics of those who dropped out after intervention began (22) versus those who remained (397) and find that characteristics neither individually nor jointly explain dropping out (joint F-test $p = 0.84$).

Finally, attritions across the two arms are statistically similar (T-test $p > 0.10$). In Table A6, Online Appendix A, we regress the attrition dummy on treatment, baseline covariates and their interactions, and find that treatment status does not explain attrition at either endline. There

Table 1. *Baseline Sample Characteristics and Balance Checks.*

Variables	(1) Treatment $N = 419$	(2) Control $N = 419$	(3) Difference $N = 838$	(4) T-test p-value
Child age in years	7.39 (0.02)	7.40 (0.02)	0.01 (0.03)	0.83
Child gender (Boy = 1)	0.49 (0.02)	0.49 (0.02)	0.00 (0.04)	0.96
Father's education in years	6.01 (0.21)	6.01 (0.21)	0.06 (0.31)	0.84
Mother's education in years	6.98 (0.16)	6.73 (0.17)	0.21 (0.24)	0.37
Family's monthly income (in BDT)	11,409 (279)	11,342 (226)	31.31 (380.20)	0.93
Number of sibling(s) under 15	0.64 (0.03)	0.63 (0.03)	0.00 (0.05)	0.95
Religion (Islam = 1)	0.77 (0.02)	0.78 (0.02)	−0.01 (0.03)	0.69
Homestead land size (in decimal)	8.40 (0.48)	9.03 (0.54)	−0.74 (0.73)	0.31
English literacy score of children (out of 30)	16.12 (0.19)	16.24 (0.20)	−0.07 (0.27)	0.80
Numeracy score of children (out of 20)	14.78 (0.14)	14.75 (0.15)	−0.01 (0.21)	0.97
Negative parenting (dummy variable for coercive interaction)	0.37 (0.03)	0.39 (0.03)	0.01 (0.04)	0.79
Homeschooling time (in daily hours)	2.31 (0.05)	2.27 (0.05)	0.01 (0.07)	0.90
Parenting abilities or skill (15-item scale)	4.33 (0.02)	4.31 (0.02)	−0.00 (0.03)	0.88
Television in the household	0.35 (0.02)	0.34 (0.02)	0.02 (0.03)	0.59
Private tutor	0.62 (0.02)	0.58 (0.02)	0.03 (0.04)	0.34
Joint F-test p-value on individual/household characteristics		1.00		

Notes: This table reports the background characteristics of children included in the baseline sample. All variables are self-explanatory. The p-value reported in the last column is obtained by regressing the variables on the treatment dummy with grade and union council fixed effects. Robust standard errors in parentheses.

is also no evidence of differential attrition by baseline characteristics (all joint p-values on interactions > 0.10). We also summarise the frequency of attrition at both endlines in Table A7, Online Appendix A, which shows that 93% of households never dropped out. Given the absence of differential attrition, we do not conduct attrition-bounds analyses.

1.4. *Empirical Strategy*

To investigate the impact of telementoring, we estimate the following ordinary least squares (OLS) regression:

$$\boldsymbol{Y}_{ijk} = \boldsymbol{\alpha} + \boldsymbol{\beta}\boldsymbol{T}_{ijk} + \Gamma'\boldsymbol{X}_{ijk} + \boldsymbol{g}_j + \boldsymbol{c}_k + \boldsymbol{\varepsilon}_{ijk},$$

where Y_{ijk} is an outcome of mentee i with the child being in grade j, living in union council k, measured at the endline; T is an indicator for the treatment; $\boldsymbol{X}$ is a vector of controls that includes the child's gender, age, birth order, baseline English literacy, baseline numeracy and access to private tuition, as well as the number of children under 15 in the household, parental education, household income and religion. g and c are grade and union council fixed effects, respectively.[8] Given the high participation in mentoring sessions (94.6% of the treated mentees participated in at least one session), intent-to-treat effects would be similar to treatment-on-treated effects. We only report intent-to-treat estimates in this paper.

Since we consider a range of outcomes, we correct for multiple hypotheses testing using Westfall and Young (1993) adjustment. The adjustment accounts for correlations across outcomes

[8] Not all villages include both treatment and control households. As a result, we use union council fixed effects—the smallest rural administrative unit in Bangladesh, where each union council consists of nine villages. All control variables were taken from the 2019 survey and the rapid over-the-phone survey. We included all variables that we thought would capture any individual and household characteristics of participants. Later, in Section 2.1, we check the robustness of our results by selecting controls using the post-double selection LASSO method (Belloni *et al.*, 2014).

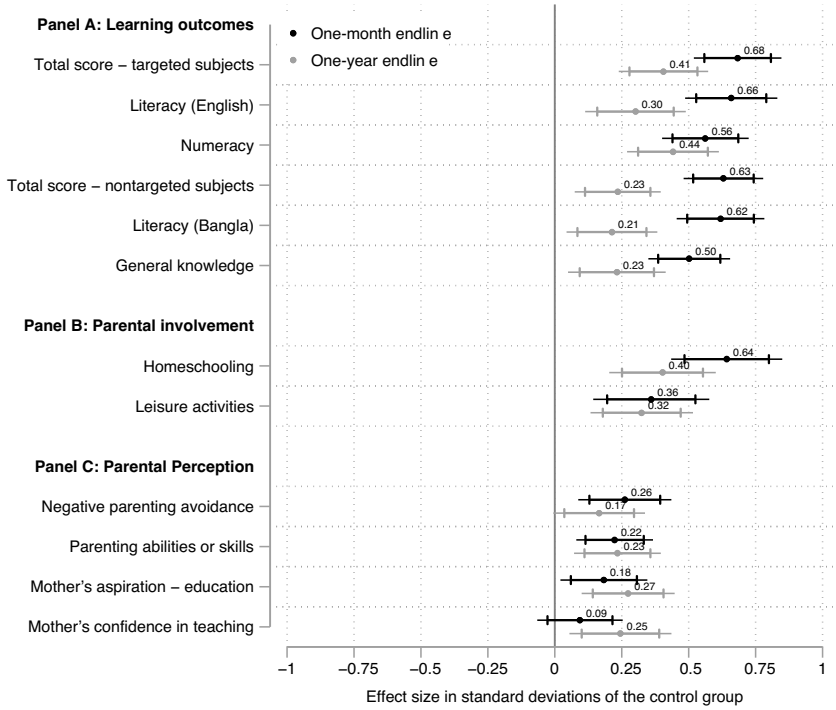

Fig. 1. *Treatment Effects on Standardised Indices.*
Notes: All outcomes are standardised indices with the control group having a mean of 0 and SD of 1. Therefore, this figure shows where the mean of the treatment groups lies in the distribution of the control group in SD units, with 95 and 99 confidence intervals. All coefficients were estimated using OLS, while controlling for child's gender, age, birth order, baseline literacy score, baseline numeracy score, access to private tuition, parents' education, household income, religion and the number of children in the household. Specifications also include children's grade and union council fixed effects and robust standard errors.

using sample bootstrapping with 5,000 repetitions. Moreover, we also compute randomisation inference (RI) p-values by reshuffling the treatment status 5,000 times following Young (2019). Our results are largely robust to using both adjustments.

2. Results

2.1. *Learning Outcomes of Children*

2.1.1. *Impacts on learning*

We plot the estimated treatment effects using standardised indices in Figure 1 (Panel A), with 99% and 95% confidence intervals, where results in black correspond to estimates from the one-month endline and those in grey correspond to estimates from the one-year endline. We find significant improvements in both aggregate and disaggregated test scores of targeted subjects (all $p < 0.01$). Specifically, we find that the intervention led to an improvement in scores of targeted subjects by 0.68 SD one month after the intervention ended and by 0.40 SD one year later. Mentors did not keep records of the exact problems covered during sessions; however, informal discussions

with mentors indicated that math questions were generally discussed more often than English questions. This trend is reflected in the treatment effects seen at the one-year endline, where the impact on math scores was greater than on English scores.

Children were also assessed on Bangla and general knowledge. We find positive and significant spillovers on both Bangla (0.62 SD and 0.21 SD at the two endlines) and general knowledge (0.50 SD and 0.23 SD at the two endlines), suggesting our intervention had broader impacts and benefited children through channels outside direct tutoring. However, the largest impact at the first endline was in English literacy (0.66 SD), and that at the second endline was in numeracy (0.44 SD), implying that children benefited the most in subjects targeted by the intervention.

We also report absolute proficiencies in numerical operations in Figure A3 (Online Appendix A) and Table A8 (Online Appendix A), which show large and robust effects in absolute numeracy in all domains among the treated.[9] Next, using the Bangladeshi public schools' passing score (40 or above) for 'foundational numerical skills', we also assessed treatment effects on passing this threshold in Table A9 (Online Appendix A). Treated children had a 21 percentage point higher likelihood of passing at the first endline ($p < 0.01$), decreasing to 6 percentage points by the second ($p < 0.05$).[10] Finally, our pandemic-adjusted assessment involved verbally asking questions to children, for which they either got full points or none. We define 'mastery' as consistently answering correctly across both endlines. Our results showed significant improvements in 'mastering' topics in the treatment group: 67% mastered addition (against 43% in control, $p < 0.01$), 50% mastered subtraction (against 28% in control, $p < 0.01$) and 69% mastered multiplication/division (against 49% in control, $p < 0.01$).

We also report treatment effect estimates using raw test scores (Panel A, Table 2). We find that the treatment improved the overall test score of treated children by 17.7 points (between 0 and 100) or 35% higher than children in the control group in the first endline. One year later, the treatment effect persisted as children in the treatment arm scored 8.7 points (or 16%) higher than children in the control arm ($p < 0.01$). Disaggregated by subject, we find that English literacy improved by 5.6 points (52%) and numeracy by 5.4 points (33%). Moreover, Bangla literacy improved by 3.9 points (37%) and general knowledge by 2.8 points (22%) among the treated in the first endline. One year later, test-score improvements of treated children in all four subjects were smaller, but remained statistically higher than those of the untreated children. Panel A of Figure 2 also shows the test-score distributions of the treated are considerably to the right of the test-score distributions of the control group, implying large benefits.[11] In a robustness check detailed in Table A12 (Online Appendix A), we varied the inclusion of covariates, baseline scores and fixed effects, employing methods like post-double selection LASSO (Belloni *et al.*, 2014). Consistently across specifications, our results remained both substantial and robust.

[9] Because of the pandemic, enumerators assessed the children through tablets. Therefore, it is possible that some enumerators are friendlier than others, which could influence how comfortable children felt during assessments. We check the robustness of results reported in Table 2 by adding enumerator fixed effects to the existing set of controls. The fixed effects are applicable because each enumerator collected data from both the treatment and control groups. The results are presented in Table A10 (Online Appendix A). It is evident from this table that our findings regarding children's learning remain robust throughout.

[10] For robustness, we introduced a median score benchmark. With this, treated children exceeded the threshold by 16–34 percentage points across both endlines.

[11] Using our baseline, a typical year of learning among these children appears to be around 1.5 points (or 9%) higher than the previous year's test score. See Table A11 (Online Appendix A). Comparatively, the impact of our intervention at the one-month endline is over triple this benchmark, and the impact at the one-year endline is roughly double. However, this result should be interpreted with caution because learning gains at baseline were, not solely from formal schooling, but also from home education. By our endlines, students had entered formal schooling, complicating comparisons of treatment effects to a typical year of learning.

Table 2. *Treatment Effects on Non-Standardised Outcomes.*

Endlines	Outcomes	(1) Control means	(2) Treatment effects (only Fes)	(3) Treatment effects (controls and Fes)	(4) FWER *p*-value	(5) RI *p*-value
One-month	*Panel A1: Learning outcomes*					
	Targeted subjects, aggregate score [60 points]	27.00 (0.79)	11.37*** (1.11)	11.01*** (1.02)	0.00	0.00
	Literacy (English) [30 points]	10.76 (0.42)	5.84*** (0.62)	5.59*** (0.57)	0.00	0.00
	Numeracy [30 points]	16.24 (0.48)	5.53*** (0.63)	5.42*** (0.60)	0.00	0.00
	Non-targeted subjects, aggregate score [40 points]	23.11 (0.51)	6.89*** (0.65)	6.69*** (0.61)	0.00	0.00
	Literacy (Bangla) [20 points]	10.52 (0.31)	3.99*** (0.42)	3.87*** (0.40)	0.00	0.00
	General knowledge [20 points]	12.59 (0.28)	2.91*** (0.34)	2.82*** (0.33)	0.00	0.00
	Panel B1: Parental involvement					
	Homeschooling (in minutes/day)	84.41 (1.68)	21.95*** (2.77)	21.81*** (2.73)	0.00	0.00
	Leisure activities (in minutes/day)	79.15 (1.65)	12.20*** (2.82)	12.05*** (2.81)	0.00	0.00
	Panel C1: Parenting perception					
	Negative parenting [0 to 5 scale]	4.69 (0.05)	0.28*** (0.07)	0.28*** (0.07)	0.00	0.00
	Parenting abilities or skills [11 to 55 scale]	48.70 (0.32)	1.58*** (0.38)	1.47*** (0.36)	0.00	0.00
	Mother's aspiration—education [1 to 7 scale]	4.87 (0.07)	0.27*** (0.09)	0.25*** (0.08)	0.01	0.00
	Mother's confidence in teaching [0 to 30 scale]	21.41 (0.34)	0.82* (0.48)	0.65 (0.43)	0.13	0.13
One-year	*Panel A2: Learning outcomes*					
	Targeted subjects, aggregate score [60 points]	30.56 (0.68)	6.11*** (1.02)	6.04*** (0.96)	0.00	0.00
	Literacy (English) [30 points]	13.24 (0.42)	2.59*** (0.64)	2.52*** (0.61)	0.00	0.00
	Numeracy [30 points]	17.32 (0.40)	3.52*** (0.55)	3.52*** (0.53)	0.00	0.00
	Non-targeted subjects, aggregate score [40 points]	23.15 (0.51)	2.67*** (0.73)	2.63*** (0.70)	0.00	0.00
	Literacy (Bangla) [20 points]	12.27 (0.30)	1.30*** (0.40)	1.26*** (0.39)	0.00	0.00
	General knowledge [20 points]	10.88 (0.30)	1.38*** (0.43)	1.38*** (0.42)	0.00	0.00
	Panel B2: Parental involvement					
	Homeschooling (in minutes/day)	50.99 (1.72)	14.15*** (2.70)	13.80*** (2.64)	0.00	0.00
	Leisure activities (in minutes/day)	55.54 (1.78)	11.07*** (2.68)	11.55*** (2.64)	0.00	0.00
	Panel C2: Parenting perception					
	Negative parenting [0 to 5 scale]	4.78 (0.06)	0.20*** (0.08)	0.19** (0.08)	0.01	0.01
	Parenting abilities or skills [11 to 55 scale]	47.59 (0.41)	1.99*** (0.53)	1.92*** (0.52)	0.00	0.00
	Mother's aspiration—education [1 to 7 scale]	4.40 (0.07)	0.36*** (0.10)	0.36*** (0.09)	0.00	0.00
	Mother's confidence in teaching [0 to 30 scale]	9.90 (0.41)	2.00*** (0.64)	2.00*** (0.60)	0.00	0.00

Notes: Treatment effects were estimated using OLS, with the usual set of controls and fixed effects mentioned in Section 1.4. Robust standard errors are in parentheses. Columns 3 and 4 report the Westfall–Young FWER adjusted *p*-values and randomisation inference (RI) *p*-values, both computed using 5,000 replications. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Next, we present the comparison of distributions of overall test scores as a percentile-to-percentile mapping of the two distributions in Panel B of Figure 2. One month after the intervention ended, the 30th percentile of the treatment group distribution corresponds approximately to the 60th percentile of the control group distribution. The effect of telementoring intervention is thus equivalent to moving a child from the 30th percentile of the control group to the 60th percentile. One year later, the impact is equivalent to moving a child from the 30th percentile of the control group to roughly the 40th percentile.

Due to pandemic-related constraints, a full baseline survey was unfeasible. Instead, we used 2019 data on children's numeracy (e.g., counting) and literacy (e.g., reciting alphabet) skills as a baseline measure. In this regard, only one question each in English and mathematics (questions 1 and 7, respectively, as shown in Table B2 in Online Appendix B) from the endline assessments can be vertically linked to the baseline assessment. Using these vertically linked questions, we present the treatment effects in Table A13 in Online Appendix A. The results remain robust

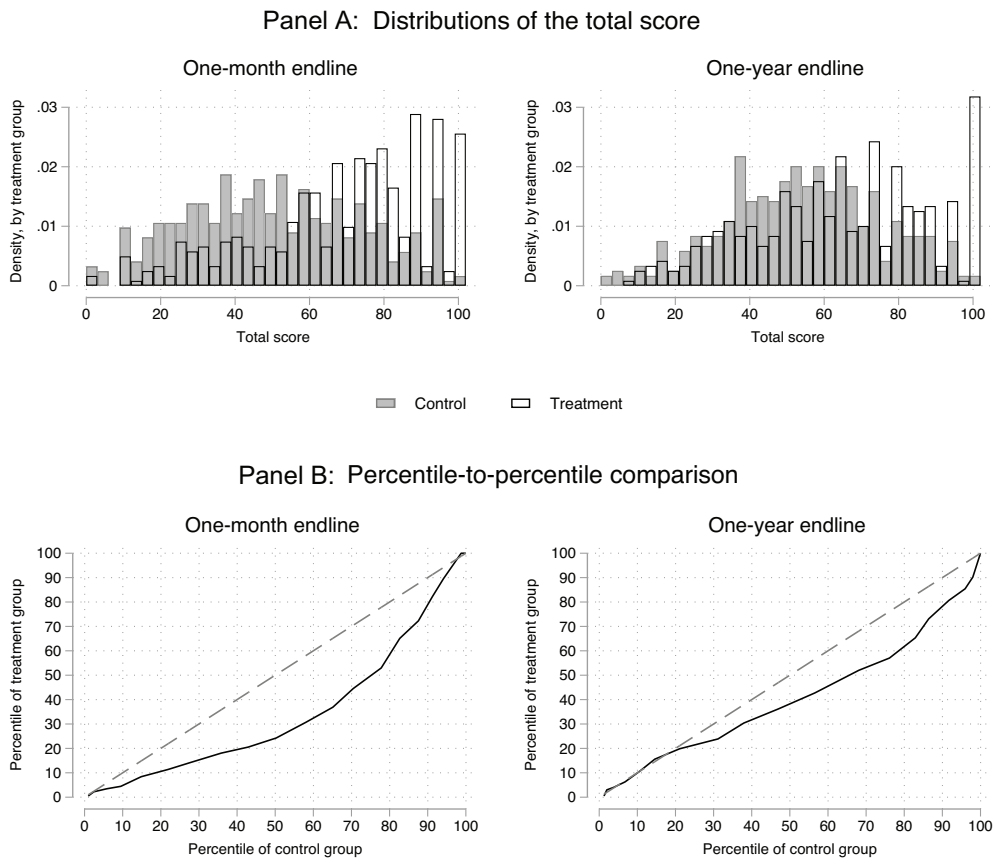

Fig. 2. *Distributions of the Total Test Score and Percentile-to-Percentile Comparisons.*
Notes: This figure shows our standardised test-score distributions (Panel A) and percentile-to-percentile plots (Panel B) by treatment arms. The maximum test score students could get was 100 points. The 45-degree line indicates a zone where there is no difference in percentile distribution between treatment and control groups.

when relying on these questions. Our results also remain robust to excluding English translation questions that could involve elements of recall (see Table A14 in Online Appendix A), suggesting that the treatment effects are not likely driven by recall.

Are these impacts due to learning progress or due to preventing pandemic-induced learning losses? In Figure A4 (Online Appendix A), we plot test scores of English literacy and numeracy at different data collection points. It shows that test scores in the treatment arm remained fairly stable over time, while in the control arm, test scores dropped significantly after the Covid-19 pandemic, implying a large loss in learning in the absence of alternative learning opportunities.

2.1.2. *Discussion*

Our effects in SD units are larger than studies conducted during the pandemic. However, if we interpret effect sizes as the number of additional correct answers given (see Table A15 in Online Appendix A), we find that treated children, on average, provided one additional correct

answer in both English and mathematics at both endlines. In the untargeted subjects—Bangla and general knowledge—children provided approximately half an additional correct answer per subject at both endlines. We believe presenting the raw effect sizes alongside the SD units offers a more tangible perspective, particularly given the substantial magnitude of the effects in SD units.

We acknowledge that the impacts are fairly large, and it might be due to a variety of factors. First, our intervention placed an important emphasis on mentoring mothers—the primary caregivers of children in this context. This feature is unique and differs from the approach taken by Angrist *et al.* (2022) and Crawfurd *et al.* (2023)—two similar studies focusing on low-income countries. See Table A16 (Online Appendix A) for more programme comparisons. Second, our one-to-one tutoring allowed mentors to provide feedback and support at the children's and mothers' learning levels, which can be particularly effective for students who fall behind and parents with limited homeschooling knowledge (Banerjee *et al.*, 2007). The mentor-to-mentee ratio was low in our study (2 : 1), which likely facilitated higher-quality interactions and reduced psychological strain on the mentors. Third, participants in the control group did not have access to any alternative learning opportunities, as online, over-the-phone, private tuition, or televised teachings were either unavailable or limited in rural areas. In contrast, students in Crawfurd *et al.* (2023) had access to radio-based lessons. Fourth, it could be due to 'role model effects', as public university students in Bangladesh are considered intelligent (because of the highly competitive nature of public university entrance exams) and role models for many, possibly prompting children and mothers to put higher effort into homeschooling. This aspect differentiates our study, as the aforementioned studies predominantly employed NGO employees and school teachers for tutoring. Fifth, mothers were already allocating approximately 135 minutes daily to homeschooling, indicating a strong value placed on their children's education. Sixth, in contrast to the aforementioned studies, we sent weekly text messages to mothers aimed at enhancing homeschooling engagement, improving parenting behaviours and bolstering motivation. Finally, our intervention ran for 13 weeks (with a direct dosage of 6.5 hours), which was relatively longer than other comparable studies—almost double the direct dosage provided in aforementioned studies. It should also be noted that the larger effects observed may be partly attributed to the different assessment data used. We conducted our own pandemic-adjusted assessment as schools were closed for around two years and exams were cancelled, so administrative data from schools were not available.

2.1.3. *Heterogeneity*

We examine heterogeneity in learning gains by baseline test scores, children's gender, mothers' education, household income and mothers' homeschooling involvement at baseline. We find that academically weaker children benefited the most from our intervention one month after it ended ($p < 0.05$). We report these estimates in Table A17 (Online Appendix A). However, this heterogeneity faded after a year. We also do not observe heterogeneity by the remaining characteristics at either endlines, including mothers' homeschooling involvement at baseline (Table A18 in Online Appendix A). We also explore heterogeneity in learning outcomes based on mentors' characteristics in Table A19 (Online Appendix A). Since no mentors were assigned to the control group, our focus is solely on the treatment group. Notably, we observe a correlation between learning gains and mentors' cognitive flexibility and mental health conditions at the one-month endline. At the one-year endline data, children mentored by female volunteers obtained slightly lower scores. Similarly, children mentored by volunteers with prior paid tutoring experience at

the primary level also showed poorer test performance. Otherwise, we observe no correlations based on other mentor characteristics such as age, education or prior tutoring experience.

2.2. *Parenting Outcomes*

2.2.1. *Mothers' involvement*

Treatment effects on mothers' involvement are reported in Panel B of Figure 1 and Panels B1–B2 in Table 2. One month after the intervention ended, we find significant increases in homeschooling engagement (0.64 SD) and leisure activities, such as playing and storytelling (0.16 SD). These impacts translate to 22 minutes (26%) more per day for homeschooling and 12 minutes (16%) more for leisure activities than mothers in the control group (both $p < 0.01$). These effects also persisted one year after the intervention ended: daily homeschooling by 14 minutes (0.40 SD) and leisure activity engagement by 12 minutes (0.32 SD). However, we do not find these impacts to vary by children's gender, baseline test score, mothers' education level or household income (see Panels A1–A2 in Table A20, Online Appendix A). Muted heterogeneity by gender is not surprising as one of the weekly themes during mentoring was gender equality in education, which could have encouraged mothers to put equal input on girls and boys.

A potential concern regarding increased parental involvement, especially during a pandemic, is the crowding out of leisure and employment time, which could have detrimental effects on mothers' mental well-being, leisure and income. Since daily involvement increased by 25–34 minutes per day on average, we do not believe it had a substantial negative impact on income-generating activities. In fact, only 7.6% of mothers in our sample engaged in income-generating work (while the remaining 92.4% are homemakers) and our treatment had no negative impact on their household income at either endlines (Panel A, Table 3). In terms of mental health, we measure depressive symptoms of mothers at both endlines using the 20-item CES-D scale (Radloff, 1977). Panel A in Table 3 also shows that our intervention did not deteriorate mothers' mental well-being. Moreover, we do not find any negative impact on mothers' self-reported sleep, suggesting mothers did not accommodate additional time for children by sacrificing sleep (Panel A, Table 3). Therefore, mothers were possibly spending their discretionary time on children, as social/outdoor activities were restricted during the pandemic.

2.2.2. *Parenting perceptions*

Weekly themes during telementoring included topics such as avoiding negative parenting, staying positive about children's education, thinking of oneself as a teacher and maintaining a routine for homeschooling. These themes were sent as text messages and discussed with mothers in Step 5 of each session. Therefore, we pre-registered that our intervention was expected to have positive impacts on several parenting perceptions that are related to the weekly themes. We report these estimates in Figure 1 (Panel C) and Table 2 (Panels C1 and C2). We find that our intervention was successful at: (i) reducing the prevalence of negative parenting by 0.26 SD (22%) at the first and 0.17 SD (16%) at the second endline; (ii) increasing self-reported parenting ability by 0.22 SD (3%) at the first and 0.23 SD (4%) at the second endline; (iii) increasing future aspirations about children's education by 0.18 SD (5%) at the first and 0.27 SD (8%) at the second endline; and, (iv) increasing self-reported confidence in homeschooling by 0.25 SD (20%) at the second endline only, with no significant impact at the first endline. Analogous to heterogeneity results for parental involvement, we again do not observe heterogeneity by gender or baseline test score at either endlines. However, for parenting ability, we find that relatively poorer households and

Table 3. *Potential Side Effects and Mechanisms.*

Intermediate outcomes	(1) Control means	(2) Treatment effects	(3) FWER *p*-values	(4) RI *p*-values
Panel A: Potential side effects, reported by mothers at both endline surveys				
Monthly household income at 1-month	9,990.49 (261.15)	549.23 (393.1)	0.96	0.27
CES-D-20 score ($0 \leq Score \leq 60$) at 1-month	8.63 (0.56)	−0.80 (0.79)	0.96	0.55
Depressed (=1 if true) at 1-month	0.16 (0.02)	−0.02 (0.02)	0.96	0.61
Monthly household income at 1-year	11,344.75 (322.56)	262.2 (870.5)	0.96	0.88
CES-D-20 score ($0 \leq Score \leq 60$) at 1-year	11.05 (0.55)	0.98 (0.80)	0.95	0.21
Depressed (=1 if true) at 1-year	0.23 (0.02)	0.04 (0.03)	0.95	0.22
Daily sleep and nap time (in hours) at 1-year	7.45 (0.05)	0.07 (0.07)	0.96	0.30
Panel B: Potential channels at home, reported by mothers at one-year endline survey				
Father's homeschooling time (in minutes/day)	32.39 (2.19)	9.67*** (3.31)	0.11	0.00
Self-induced study time (in minutes/day)	83.39 (2.42)	3.29 (3.43)	0.96	0.33
Other family member's homeschooling time (5-point scale)	3.44 (0.13)	−0.09 (0.23)	0.96	0.68
Private tutor's tutoring time (in minutes/day)	100.22 (3.73)	3.60 (5.35)	0.96	0.49
Time on homework by children (5-point scale)	3.66 (0.04)	0.16*** (0.06)	0.19	0.01
Started new private tuition recently (=1 if yes)	0.20 (0.03)	−0.03 (0.03)	0.96	0.38
Panel C: Potential channels at school, reported by teachers at one-year endline survey				
Time playing after school (in hours/week)	3.30 (0.04)	−0.04 (0.05)	0.96	0.44
Catching up with study (5-point scale)	3.15 (0.04)	0.05 (0.05)	0.96	0.28
Recovering quickly (4-point scale)	2.41 (0.04)	0.09 (0.06)	0.82	0.10
Time spent on classwork (5-point scale)	2.72 (0.04)	0.09 (0.06)	0.91	0.15
Interest in class activities (5-point scale)	3.18 (0.04)	0.08 (0.06)	0.91	0.17
Attention during class (5-point scale)	3.15 (0.04)	0.09* (0.06)	0.79	0.09

Notes: Treatment effects on the intermediate outcomes (all self-explanatory) were estimated using OLS, with the usual set of controls and fixed effects mentioned in Section 1.4. Robust standard errors are in parentheses. For outcomes other than depression, a higher value corresponds to more favourable outcomes. Columns 3 and 4 report the Westfall–Young FWER adjusted *p*-values and randomisation inference (RI) *p*-values, both computed using 5,000 replications. $* p < 0.10$, $*** p < 0.01$.

low-educated mothers experienced increases in parenting ability at the second endline (Panels B1–B2 in Table A20, Online Appendix A).

2.2.3. *Discussion*

While the control group mothers engaged in an average of 135 minutes of daily homeschooling at baseline (due to data unavailability we cannot definitively say what activities comprised), time spent on homeschooling dropped significantly over time, likely due to pandemic-related factors. See Figure A5 (Online Appendix A). Against this backdrop, our intervention, not only boosted the quantity of homeschooling time by 25–27%, but is also likely to have enhanced the quality of mother–child interactions. These effects were beyond the 22 minutes of direct programme engagement and potentially transformative. The decline in negative parenting and improvement in parenting skills also suggest a broader impact on the quality and effectiveness of homeschooling during the pandemic.

2.2.4. *Social desirability bias*

Parenting outcomes are based on survey responses and might be susceptible to social desirability bias (SDB). We address potential SDB concerns pertaining to self-reported outcomes following Dhar *et al.* (2022) using a 13-item Marlowe–Crowne scale that records a respondent's too-good-to-be-true traits (a higher SDB score corresponds to a higher chance of giving socially desirable

responses), we perform a heterogeneity analysis. Our results hold even among mothers that have a lower tendency to give socially desirable responses. Though these results, reported in Table A21 (Online Appendix A), support our conclusion, they cannot entirely rule out experimenter demand effect concerns.

2.3. *Potential Mediators for Forestalling Learning Losses*

Parenting can be an important channel through which children's learning can be affected. Existing literature suggests that higher parental investment can affect the cognitive development and human capital accumulation of children with many positive economic consequences later in life (Cunha and Heckman, 2007; Francesconi and Heckman, 2016; Doepke *et al.*, 2019; Attanasio *et al.*, 2020). Therefore, significant improvements in homeschooling-leisure involvements and parenting perceptions might have contributed to the learning gains of treated children. However, there might still be various other potential channels—such as learning activities at home or school—that were also affected by the treatment and complemented parenting input, which as a result affected the learning outcomes of treated children. To explore such potential channels, we surveyed both mothers and school teachers at the second endline (after schools briefly reopened for 3.5 months) and present these results in Panels B–C of Table 3.

At home (Panel B), we find that fathers also started homeschooling their children, roughly by 10 minutes per day ($p < 0.01$). Treated children also spend relatively more time, by 4%, on their homework than the untreated children ($p < 0.01$). However, self-assisted studying other than homework, beginning new private tuition, increased support by existing tutors, and homeschooling support from older siblings or grandparents were not affected by telementoring and, thus, are unlikely to be possible channels (all $p > 0.10$). At school (Panel C), according to teachers, treated children appeared marginally more attentive during classroom teachings ($p = 0.09$); however, children's interest in and time spent on classwork, after-school play activities with peers, and the ability to catch up and recover from missed schoolwork were not affected by the treatment (all $p > 0.10$).[12]

To decompose the treatment effect, we also conducted a mediation analysis following Heckman and Pinto (2015) that considered parents' and children's contributions as mediators. After one year, about 56% of the treatment effect on learning can be jointly explained by these mediators, with observed maternal channels being the most important factors. For instance, time involvement by mothers, such as that for homeschooling and leisure activities, accounts for approximately 11% of the treatment effect, while other observed parenting channels, including negative parenting (around 6%), parenting skills (approximately 6%), aspirations (about 16%) and confidence in teaching (around 2%), collectively explain roughly 30% of the treatment effect on learning. Thus, 41% of the treatment effect can be attributed to changes in parental behaviour, while 15% can be explained by changes in children's behaviour. This finding aligns with Nickow *et al.* (2024), underscoring the importance of tutors' inputs or skills as crucial factors in the success of tutoring programmes (in our context, both mothers and mentors). A detailed description of the analysis and the results are available in Figure A6 (Online Appendix A).

[12] In Hassan *et al.* (2023), we explored an additional, un(pre)registered outcome related to children's mental health, assessed via the Strengths and Difficulties Questionnaire (SDQ) (Goodman and Goodman, 2009); although symptoms related to conduct problems and hyperactivity/inattention among children in the treatment group decreased at the one-month endline, it disappeared at the one-year endline, allowing us to rule out mental health of children as a channel explaining the persistent effects.

In all, it is difficult to pin down a specific channel that explains the persistent treatment effect of our multifaceted intervention. However, improved homeschooling input, parenting and children's own study habits appear to be possible indirect channels. Increases in time spent on homework and class attentiveness also suggest that the intervention might have changed children's study habits by an extensive margin.

3. Conclusion

This study finds that telementoring in low-resource settings had positive impacts on the learning outcomes of children and homeschooling during Covid-19 school closures. These positive impacts persisted one year after the intervention ended. The intervention was low cost, costing less than USD20 per child–mother dyad (see the breakdown in Table B4 in Online Appendix B). Importantly, our benefit-to-cost ratio is relatively higher than that of the vast majority of interventions on education in developing countries (Kremer *et al.*, 2013). However, this per-unit cost would likely increase if the programme were scaled up by building a fully independent infrastructure for implementation and oversight.

Our findings have both immediate and long-term policy implications. Telementoring can support low-performing students that frequently fall behind by teaching them at the right level. More importantly, it can supplement education in a world where hybrid formats of teaching and learning are being discussed to address the pre-existing learning gap and pandemic-induced learning loss. Volunteer-delivered in-person tutoring already exists in many developing countries, e.g., JAAGO foundation or BRAC in Bangladesh and Pratham in India. Such existing infrastructure and human resources can also be utilised and scaled up to provide over-the-phone education support in poor environments where school closures due to conflict, political unrest, teacher absenteeism and natural disasters are ubiquitous and often unavoidable. Due to supply constraints, in addition to volunteers from universities, community-based volunteer teachers that are more readily available could be recruited and trained to offer such support. Our study revealed that prior tutoring experience did not affect children's learning differently, emphasising the potential flexibility in choosing tutors. As we consider the scalability of these programmes, we encourage that future research be conducted through independent implementing bodies. This would provide insights into the impact of trainer identity on the success of the programme and allow for a comprehensive assessment of associated costs.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package.

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