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A 'SMART BUY' FOR ALL? UNEQUAL AND UNINTENDED CONSEQUENCES OF A MESSAGING PROGRAM FOR CHILD EDUCATION

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Abstract:

Through a large-scale household-randomized trial, we document divergent and unintended effects of a SMS-nudge parenting intervention in Ghana. For parents with some exposure to formal schooling, the program supported parental education engagement, child school participation and social-emotional skills. Conversely, for parents with no schooling, the program backfired, exacerbating educational inequality. Messages also lowered parental self-efficacy, educational aspirations, and the perceived importance of regular school attendance among parents with no schooling. As light-touch, low-tech strategies integrate into education systems to combat the global learning crisis, these findings caution against potential unintended and distributional consequences, particularly in rural, low-literacy contexts.

JEL Codes: I24, I25, O15

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Pennsylvania, University of Newcastle (Australia), Universitat Rovira i Virgili, Autonomous University of Barcelona, Jacobs Foundation and European Economic Association. This research project's protocol was reviewed and approved by the Institutional Review Board (IRB) at Innovation for Poverty Action 2020 with approval ID No. 15573. A preanalysis plan was registered at the American Economic Association RCT registry - AEARCTR-0006118.

1. Introduction

A global priority is to improve school participation and learning equitably at low cost and at scale. These goals are especially pressing in Sub-Saharan Africa, the region with the largest out-of-school population worldwide (98 million children), and where 90% of children are unable to read and understand simple text by age 10 (UNESCO, 2022). Girls, children from lower socioeconomic status households, and from rural areas are at highest risk of poor educational outcomes.

Messaging strategies to parents that address cognitive, informational, and behavioral barriers to educational engagement can improve child schooling and learning in middle- and high-income countries (Bergman, 2019; Lichand & Christen, 2023). In view of this evidence, the Global Education Evidence Advisory Panel (Akyeampong et al., 2023) identified information or nudge-based programs delivered though low-tech tools such as SMS as 'smart-buys' for improving education in low- and middle-income countries (LMICs). Efforts to scale-up these approaches are currently ubiquitous (Asanov et al., 2023).

While promising, the evidence on messaging programs to raise child education through improved parent engagement in LMICs – and in Sub-Saharan Africa particularly – includes several unknowns: (i) effectiveness in low-literacy settings; (ii) distributional effects, e.g. by child socioeconomic status or gender; (iii) mechanisms; (iv) use of administrative data to evaluate effectiveness, rather than self-reports. The latter tend to overstate school participation data, potentially leading to underestimation of program impacts (Baird & Özler, 2012; Evans & Mendez Acosta, 2021). Other open questions relate to: (v) program design, including framing of messages, optimal duration, and persistence of effects after programs end. To quantify the extent of these limitations, we conducted a literature review, which revelead that only *two* of fifteen studies conducted in LMICS were set in a predominantly rural setting with low literacy rates. Yet, these did not address the other limitations we identified (see Appendix 1 for a review).

To bridge these gaps, we conducted a household-randomized trial of a messaging intervention aimed at supporting parent educational engagement and child schooling through SMS-text nudges in a rural, low-literacy setting. With a sample of 2,600 primary caregivers (henceforth 'parents') and around 5,000 school-age children across five regions of northern Ghana, we combine rich surveys at the parent and child level with administrative records on school participation to uncover average and distributional program effects and potential mechanisms.

We focus on parent educational engagement as a vast literature has highlighted its key role to children's education (Cunha & Heckman, 2007; Islam, 2017). In Ghana, as in many LMICs, parent educational engagement is generally limited (Kamei et al., 2020), and teacher and policy-makers perceive it as a main hindrance to child educational outcomes (Wolf, 2020). Cognitive, behavioral, and informational barriers, which amplify resource or time limitations, can explain such low levels (Lavecchia et al., 2016).

The program we test addressed three such barriers for parents: low salience of educational investments when poverty decreases cognitive bandwidth; limited knowledge on how to support children's education; and gender bias. These barriers can be very high in low-resource contexts such as the one of northern Ghana in which this study takes place, where half of the population lives in poverty (Ghana Statistical Service, 2018), many children are first-generation learners, and gender gaps in education are large (UNICEF, 2023). Parents were sent messages aimed at empowering them through information, reminders, and suggestions of practical, non-academic activities to engage with their children's education. Messages also targeted fostering social-emotional skills, with themes such as positive discipline, growth mindset, and warm communication at home. Based on previous evidence (Bergman, 2019), messages would operate through raising the salience of education in parents' minds, and by providing practical and timely tools to effectively engage with children's education. The latter might be particularly helpful for parents that had limited educational exposure. To address gender gaps, we experimentally tested differential message framings, distinguishing between a 'standard' program and messages aimed at raising the salience of girls' education ('gender boost').

Our main finding relates to the large and diverging effectiveness of messages by parental schooling, a key prespecified variable for heterogeneity, in a way that unintentionally increased educational inequalities for parents with no formal schooling and their children. This result stands in contrast to the absence of average treatment effects and any heterogeneity indicated by other pre-specified variables or additional variables identified through causal forest methodology among a rich set of covariates (Athey & Wager, 2019).

Intent-to-treat (ITT) estimates show that SMS-nudges promoted parent engagement and child school participation but *only* for parents that had some formal schooling. By contrast, for parents without any formal education, nudges *backfired*, decreasing parent engagement and child school attendance. Results are consistent across parent- and child-reports, as well as administrative data on school participation. The program also led to increases in social-emotional skills for children whose parents had some schooling. These contrasting effects appear to be driven by parental human capital itself rather than other factors that education may proxy (e.g., household wealth; socio-demographics; baseline measures of traditional gender norms; baseline educational investments; and distance from educational facilities). Further, our data suggest that such heterogeneity cannot be completely explained by differential receipt, use, or satisfaction with the program.

In terms of mechanisms, nudges had additional unintended effects for parents with no formal schooling: they decreased their self-efficacy by around 10% SD; they lowered their educational aspirations and their attitudes towards the importance of regular school attendance for their children, which explain may higher absenteim for children in treatment with unschooled parents; and they increased their pro-boy bias. By contrast, parents with some schooling decreased their pro-boy bias, increased their positive attitudes towards school attendance, but also became more distressed, perhaps due to an increased recognition of the importance of their engagement. Drawing on qualitative insights from participants, receiving nudges was often interpreted as a *signal* that they were not supporting their children well enough. We speculate that for parents with no schooling, the program might have made their own education and resources limitations more salient, without removing other barriers, leading to overall disengagement.

Finally, as more than 40% of parents reported asking family members or neighbors to read the messages for them (regardless of schooling status), we find suggestive evidence of 'social spillover' effects (as defined in Carter et al., 2021) for school enrolment. Positive effects of being treated for schooled parents were reinforced in areas where a higher proportion of schooled parents were randomly assigned to the program. Similarly, negative effects were magnified for non-schooled parents in areas where more neighboring parents with no schooling were assigned to the program.

Given these findings, we infer that parents' motivation and capability to enact the information offered by the nudges—a necessary condition for messaging to work (Akyeampong et al., 2023)—were in this context inextricably linked to a base level of parental human capital. Without it, messages backfired. We note that parents with some education are one-third of our sample, and half of them had at most a primary education. These low levels of human capital are consistent with the average adult educational levels in many other rural areas of sub-Saharan Africa and South Asia (Aker et al., 2023), warning that similar backfiring effects of educational messages may occur in other low-literacy settings[1]. From a policy perspective, our findings suggest that message-based behavioral programs work through a complex array of parental capabilities, pressures, expectations, and needs, which are often neglected or overlooked. Understanding and factoring-in heterogenous parental responses in program design or scale-up is key to avoid harmful consequences, as well as to offer more nuanced guidance to policy-makers. This is especially true in light of current global efforts to scale light-touch, low-tech educational solutions such as the one we tested (Asanov et al., 2023).

The results make several contributions to the literature. We add to a burgeoning literature evaluating parenting programs to support learning that leverage digital technology in LMICs (Appendix 1) by combining for the first time surveys and administrative data to offer evidence on messaging parents in rural, *low-literacy* settings; focusing on distributional effects; and investigating mechanisms through rich survey and qualitative data. We also contribute to the literature on nudge-based interventions, as we experimentally varied the framings of the messages, the length of program exposure to assess effectiveness in relation to treatment duration and the persistence of effects after the program concluded. These are open questions about optimizing policy applications of nudges in education and beyond (Brandon et al., 2017; DellaVigna & Linos, 2022; Keller & Szakál, 2023). No study we reviewed in Appendix 1 experimentally varied program framing or length, and only one (Amaral et al., 2024) examined persistence of impacts beyond program end. We do not find differences by whether messages were sent for 12 or 24 weeks on endline outcomes. Further, program effects on engagement—in both directions—arose

[1] Adult educational levels in our sample are also consistent with the latest nationally-representative data for the Ghanaian regions in which this sample was based.

early, as measured by a midline survey after the 12-week program ended, and persisted at endline, around five and two months after the 12- and 24-week programs ended, respectively. Finally, we do not find variation by program framing (standard vs 'gender-boost'), potentially indicating that the effects primarily stemmed from exposure to the SMS-nudges rather than the manner in which the content was presented.

The paper proceeds as follows. Section 2 presents the program and the study design. Section 3 introduces the data, and Section 4 presents the pre-registered main results. Section 5 presents potential mechanisms, exploratory extensions, and program costs. Section 6 discusses the findings and concludes.

2. Program Theory and Intervention

Parent involvement in education in LMICs is often limited, despite parents placing significant value on education as a crucial path out of poverty and maintaining high aspirations for their children's education (Favara, 2017). Thus, why do parents in LMICs not participate more actively in their children's education? Cognitive, behavioral, and informational barriers provide potential explanations (Lavecchia et al., 2016). First, education is an investment with long-term returns and high present costs including monetary and time investments. This intertemporal trade-off is larger for parents from lower socioeconomic backgrounds, whose cognitive bandwidth may be lower due to poverty (Bergman, 2019; Mani et al., 2013), or in rural settings, where schools are on average of lower quality and the returns to educational investments may be perceived as lower as compared with urban areas (Angrist et al., 2022; Cooke et al., 2016). Second, behavioral barriers may be especially high in areas where a large share of children are first-generation learners (Ogando Portela & Atherton, 2020). Parents often receive little guidance from schools on how to support children's education (Balarin & Cueto, 2007). Practical, applicable suggestions provided through messages can guide parents and increase their self-efficacy in such engagement. Finally, parents might perceive lower returns to education for girls, or be influenced by biased social norms and beliefs regarding girls' education[2], resulting in reduced investments in their daughter's academic pursuits.

To address these barriers, we co-designed the EDU+ program with Movva Technologies (an ed-tech start-up) on the model of Ready4K! (Cortes et al., 2019; York et al., 2019). This model aims to break down the complexity of parenting into small steps by providing parents with chunks of actionable information over an extended period. Nudges were structured around sequences of four messages sent over two weeks to the child's parent that owned a mobile phone, or if both owned the phone, the child's primary caregiver. Sequences included a motivating fact, a suggested activity, an interactive message, and a growth message, the latter aiming to promote lasting behavioral change (Appendix 2, Table A2.1).

According to our theory of change, the program would alliviate parents' limited cognitive bandwidth and lack of guidance on involvement in their children's education by enhancing the visibility of educational investments and providing information, along with practical suggestions for incorporating educational activities into daily life. Examples include setting dedicated time for homework and engaging in conversations with children about their future educational plans. The nudges also focused on nurturing social-emotional skills through messages on growth mindset and fostering warm communication at home (Table A2.2 in Appendix 2 for all contents). To address gender biases in parents' perceptions around the importance of girls' education, half of the treatment sample was randomly assigned to receive messages with similar content but framed to increase the salience of girls' education for their future ('gender boost'). We note that our evaluation

[2] For instance, in our baseline data more than half of the parents agreeing with the statement "It is important that sons have more education than daughters".

was carried out during the COVID-19 pandemic, which likely exacerbated behavioral barriers faced by parents. Lost incomes and remittances increased economic stress, potentially lowering further the salience of educational investments (Alvi et al., 2021). Our first message was set to alert parents about the re-opening of schools after 10-months closures. Such an extended period out of school might have made parents' concerns about school quality and returns to educational investments even more pronounced, especially for parents of lower socioeconomic strata (which might have been more affected by the crisis), and for girls. In this context, the intervention had even a larger potential to support parents' educational engagement by addressing some of the barriers to their involvement.

In terms of mechanisms, we hypothesized that the program would foster parent engagement and child schooling through multiple pathways at both the parent and child levels. First, by providing parents with simple, practical tips for getting closer to their children's school lives, we anticipated an increase in parental self-efficacy in supporting child education (Jones & Prinz, 2005). Second, the reminders were expected to elevate parental attitudes and enhance monitoring of school attendance (Berlinski et al., 2022). Third, nudges may affect parent stress, but in which direction is unclear. On the one hand, receiving actionable guidance for engagement may lower stress. On the other, messages may increase stress by reminding parents about their key role in promoting child education without removing resource or time constraints. Fourth, specific content aimed at offering encouragement and support for children's educational and career aspirations was included, anticipating an increase in educational aspirations for their children—a potential catalyst for increased educational investments (Eble & Escueta, 2021). Finally, the program was anticipated to influence child time use patterns by explicitly encouraging parents to shift their children's time from chores and labor to educational activities.

All contents were aligned with the Ministry of Education's campaigns and were adapted to the context through discussions with local NGOs. A phone-based pilot with a separate sample prior to implementation start was conducted to assess content understanding and acceptability, and implementation (e.g., preferred time to receive messages). Messages were sent in plain English, Ghana's *lingua franca* and medium of instruction from Grade 4 onwards[3].

3 Methods

3.1 Data sources, sample, and timeline

We rely on four data sources. First, we conducted a phone-based parent enrolment survey in December 2020 to recruit our sample. Participants were sampled across the Northern, North East, Savannah, Upper East, and Upper West regions. The sampling frame consisted of two previously completed studies, 'Communications for Development' (C4D, 2012-16) (Fink et al., 2018) and 'Graduating the Ultra-Poor' (GUP, 2011-13) (Banerjee et al., 2015)[4]. This strategy was preferred to random sampling through phone surveys, as the latter tends to oversample older and better-educated households *vis-a-vis* the general adult population in Sub-Saharan Africa (Brubaker et al., 2021).

Households were eligible to be included in the evaluation if there was an adult and at least one school-aged child (aged 5-17 years) in the household, and if they consented to

[3] At the national level, 96% of the Ghanaian population that can read and write does so in English, compared to 54% of the population that is literate in any local language (Ghana Statistical Service, 2022). A key consideration at the design stage was using text vs audio messages. We offer more insight on this choice in the discussion section.

[4] First, we sampled participants in C4D, a cluster-randomized trial in 12 districts to increase health preventive behaviors. The program did not change study outcomes (Fink et al. 2018). Since we could not reach many of the households in this sample due to high rates of changes in phone numbers, we then relied on the GUP sample to obtain our desired sample size. GUP investigated the effects of an anti-poverty program on ultra-poor households (Banerjee et al., 2015). These two samples turned out to be highly different (Appendix 4). GUP captures extremely deprived households, while C4D is more reflective of average living standards in northern Ghana, as compared with the representative national data (available upon request). GUP caregivers were more likely to be males (79% vs 14% in C4D), older (47 years vs 40 years) and less schooled (27% vs 40%). At baseline, they also had higher pro-boy bias, and lower estimated returns to education. Given these differences, we control for the original sample in our estimates and investigate whether the program worked differently based on the origin sample as a robustness check. We note that out of all households we reached, only 10 declined participating in the program.

participate in the study. Randomization to receiving any version of the program occurred after consent. We enrolled 2,628 households. Parents assigned to treatment were notified that they soon would receive biweekly SMS messages focusing on their children's education. Using a roster collected during the call, we randomly sampled two focal children in each household: one younger child (5-9 years) and one older child (10-17 years). The final sample of children was 4,675. The enrolment calls also collected information on educational engagement during school closures, pro-boy bias, and household socioeconomic characteristics.

Implementation for most households started in early January 2021, a week before schools re-opened after pandemic-related closures. For 12% of parents, messages started a few weeks later, due to connectivity challenges that delayed enrolment[5]. These families received three messages a week for the first few weeks to "catch up" with the rest of the treatment sample. Implementation ended at the same time for all (at either 12 or 24 weeks from onset, depending on randomized assignment to the 'short' or 'long' intervention groups).

Second, we conducted midline and endline in-person surveys with parents and the two focal children at their homes in April-June and August-September 2021, respectively. In this paper, we report on endline outcomes only. We assessed parent- and child-reports of educational engagement, and children's school enrolment and attendance (Section 3). Surveys also included modules on child and parent well-being, and household sociodemographics, and for children, direct skill assessments and time use. Attrition was very low. Of the 2,628 households interviewed at baseline, 88 (3.4%) were not interviewed at endline. There was no differential attrition by treatment status nor by the interaction

[5] Households for which the implementation was delayed had slightly fewer members, were less likely to have a male head, had slightly less educated caregivers, and more likely to live in the remote North East and Upper East regions. Given these differences, we control for this delay in implementation start in all our models.

between treatment and parent schooling, or other household socio-demographics (Appendix 3). This finding, combined with the low attrition overall, decrease concerns of attrition bias. Yet to lessen such concerns even more, our analytic sample focuses on households present at all rounds of data collection.

Third, we collected administrative data on school participation at children's schools. These are considered accurate records of school participation, especially compared with self-reports which are likely to overstate enrolment and attendance (Baird & Özler, 2012). In early 2023, we received approval from the Ghana Educational Service to collect school enrolment and attendance data for the 2021 and 2022 school years. We were able to track 91% of children that were present at the endline household survey and for which we had school names (N=4,565). Yet, for 2% of these children, we were unable to verify enrolment and/or attendance due to permanent school closures, principals' refusal, or lack of attendance data at the school. On its own, being assigned to any nudges did not predict a higher probability of loss-to-follow-up, neither did the interaction between treatment and household characteristics (Appendix 3). Thus, the administrative sample does not appear to be selected.

Fourth, for a sub-sample of 30 treated caregivers, we conducted in-depth interviews and focus groups to understand experiences with the program. We report on these findings in the discussion section.

2.2 Experimental Design and Empirical Strategy

Parents in the treatment groups were randomly assigned through household-level randomization to receive biweekly messages, while the control group did not receive any messages. This design allows for the identification of causal effects. This paper focuses on heterogeneity by parent education of ITT parameters, by estimating Equation 1 below through OLS:

$Y_{i,\ h,\ ,t}\ = \beta_0\ + \beta_1 Nudge_h + \beta_2 Nudge_h * Schooling_{j,\ h} + \beta_3 Schooling_{j,\ h} + \beta_4 GUP_h + \beta_5\ Late_h\ + \eta_r\ + e_{i,\ h,,t}$ (Eq. 1),

Where $Y_{h,t}$ is the outcome variable related to child i (based on parent-, child-, and school-reports) in household h at time t. $Nudge_h$ =1 if the household was randomly assigned to any treatment. $Schooling_{j,h}$ is a binary variable equal to one if child's parent j has ever attended any formal education. GUP_h = 1 if the household was sampled GUP sample, 0 = C4D sample; η_r, a vector of region fixed effects; $Late_h$ = 1 if implementation started later for household h (see details in Section 5.3); $e_{i,h,t}$: individual error term. Standard errors are clustered at the household-level.

While our main focus is on heterogeneity by parent schooling, in line with our pre-analysis plan, we also report ITT parameters and heterogeneity by other pre-specified axes (parental schooling and sex, and child age group and sex) in Appendices. Similarly, we report the effects of experimentally varying the length of a household's exposure to the nudges (12 and 24 weeks) and their framing (EDU+ or Gender boost), as per our original plans.

3.3 Baseline Equivalence and Implementation Fidelity

Appendix 4 highlights that the two main arms—control and any treatment—were broadly statistically equivalent across a broad range of baseline characteristics[6]. There were small imbalances with regards to the treatment slightly more likely to reside in the North-East region and to be sampled from GUP, less likely to own a TV, and more likely to have low educational aspirations for boys. On average, parents were 43.0 years old, households had an average of 10 members and 3 school-age children. Forty-one percent of sampled parents were male, and only 35% had ever attended school. Sampled children were, on average,

[6] Balance is ensured also when considering the five treatment arms (available upon request).

10.3 years old, and were equally split in the two target age groups (5-9 years and 10-17 years).

SMS records from our implementing partner show 89% message delivery rates. Due to inactive phone numbers, less than 2% of the treatment parents never received messages (n=37). No SMS were sent to the control group. Additional issues on program implementation and use are discussed in Section 5.1.

3.4 Measures

We pre-specified three types of outcomes: *primary* (directly targeted by the program), *secondary* (child skills), and *mechanisms*. For all continuous outcomes, we regressed out interviewer effects and standardized outcomes over the control group (M = 0 and SD = 1).

3.4.1 Primary Outcomes

These include: (1) parent engagement, (2) children's school enrolment, and (3) school attendance. All three outcomes are reported by parents (with regards to each focal child within the household), and the focal children. For the school records on enrolment and attendance, these relate to the second trimester of 2021, consistently with the endline survey reports.

Adapted from the Family Care Indicators (Kariger et al., 2012), for *parent engagement*, parents and children reported whether the parent or another adult in the household did any stimulating or educational activity with each focal child in the previous three days. All elicited activities were age- and culturally-appropriate: e.g., playing, discussing family or community traditions, asking about school or homework, cooking or going to the market together. Parents were also asked if they met teachers or participated in any school activity in the previous term for each focal child. Endorsed activities were summed to create a score. Correlations between parent educational engagement scales between midline and endline are moderate (parent-reported rho=0.26, $p<0.001$; child-reported rho=0.21, $p<0.001$), but in line with previous research (Berlinski et al., 2022). Parent- and child-reports correlation is also moderate (rho=0.27, $p<0.001$). The top row in Appendix Figure A5 highlights that it is more likely that children report no engagement activity than their parents: 19% of children report no activity, compared with 2% of parents. Endline engagement among parents with no schooling is more limited as compared with parents with some schooling.

School enrolment is a binary variable =1 if the parent, child, or school ledgers indicate each focal child is currently enrolled. Survey-based reports by parents and children indicate enrolment rates of around 90%, in contrast to 70% based on school records. Self-reported *attendance* is measured as the number of days the child went to school in the previous five-day school week. In school ledgers, attendance is measured as the proportion of days attended by the child out of the total days in which the school was open during a trimester. In line with previous evidence (Baird & Özler, 2012a; Evans & Mendez Acosta, 2021), self-reports tend to overestimate attendance: parents and children report attending 90% and 82% of days in the previous week, while the proportion of days attended over the trimester based on school records is 75%.

The tetrachoric correlation between parent- and child-reports on enrolment was 0.78 ($p<0.001$), but children's reports had much higher correlation with school records (rho = 0.70, $p<0.001$) than parents' (rho = 0.38, $p<0.001$). The correlations between data sources related to attendance records were much lower (rho, child- and parent-reports = 0.70; rho, child-reports and school-records = 0.15; rho, parent-reports and school-records = 0.07, all $p<0.001$). This highlights that child reports may be more accurate than parents, at least for enrolment. Given discrepancies between different reports, and potentially different biases (e.g. social desirability bias for parents, recall issues for children, inaccurate reports by teachers in school ledgers), we assess treatment effects across all data sources.

3.4.2 Secondary Outcomes

Secondary outcomes include children's academic and social-emotional skills. *Academic skills* covered similar academic sub-skills for all age groups (e.g., reading comprehension or oral vocabulary for literacy; addition and subtraction for math), but were aligned with age-appropriate curricular knowledge. Items included in the assessments were sourced from validated scales (e.g. Early grade reading assessment, Early grade math assessment, and so on). To create summary scores, the total correct number of answers on each subtask was calculated within each age-group. Sub-tasks were then aggregated through maximum likelihood factor scores and standardized over the control.

Social-emotional skills were measured through the International Social-emotional Learning Assessment (ISELA; D'Sa & Krupar, 2021). ISELA was developed to measure primary school-aged children's social-emotional competencies in low-resource contexts. We focused on two subscales: conflict resolution (children's prosocial conflict resolution skills within a hypothetical scenario) and relationships (how much children draw on relational support from parents and other community members in challenging times).

Appendix Figure A5, bottom row, presents descriptive statistics of raw secondary outcomes at endline by parental schooling. Overall, academic skills were low, with large floor effects. The average child only responded correctly to 14% and 35% of the literacy and numeracy questions. Social-emotional skills were higher on average, as well as skills among children of parents with some education.

3.3.3 Parent- and Child-level Mechanisms

We examine potential mechanisms through which the program may change parental behaviors. We measure parent: self-efficacy, psychological distress, proboy bias, and attitudes towards school attendance. These scales were aggregated through maximum likelihood factor scores and standardized over the control mean and SD after removing interviewer effects. We also measure parent educational aspirations and expectations for each focal child, by asking which grade they would aspire (or expect) their child to complete. At the child level, we assess child-reported time use in different activities. Details on all measures are reported in Appendix 5. All measures were pre-specified except attitudes towards attendance.

4. Results

4.1 Effects on Primary Outcomes

Our main result relates to the program's large and diverging effects based on parent formal schooling. This contrasts with the lack of average treatment effects and the limited and generally inconsistent heterogeneity by both prespecified (child gender, age group, or parent gender; Appendix 6) and non-pre-specified variables identified among a rich set through causal forest methodology[7] (Appendix 7). Likewise, the program had no differential effects by either framing or duration (Appendix 8).

Table 1 reports heterogenous treatment effects on primary outcomes by parental schooling using parent- and child-reports, and school records. For children, we focus on reports of children aged 10-17, assuming these are more reliable than reports by younger children[8]. For all results, we present unadjusted p-values and p-values adjusted for

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[7] The main exceptions are: a decrease in attendance as reported by children, which is not reflected in the school records (Table A6.1); decrease in enrolment for boys in treated households, based on school records (Table A6.2); decrease in enrolment based on parent and school reports for older children; decrease in attendance for younger children based on child records (Table A6.3); positive effects on enrolment for parents with baseline higher expected educational returns (Table A7.1).

[8] Robustness estimates using the full sample of children offer similar results, but noisier (available upon request).

multiple hypothesis testing following the Romano-Wolf (RW) method (Clarke et al., 2020). We take the latter as our main reference.

Being randomly assigned to any nudge type *decreased* parent educational engagement for parents that did not have any formal schooling, but *promoted* engagement for parents with some schooling, based on both parent- and child-reports (Panel A). The program led to similarly diverging results by parental education for school participation, which are remarkably consistent across all three data sources[9]. For attendance, we find diverging effects by parent schooling for child-reports and school records: nudges decreased attendance for children of parents with no schooling by 3 percentage points (p.p.) across both sources, while the coefficient of the interaction between treatment and parental schooling is 4.3 p.p. based on administrative data. For enrolment, effects go in similar directions but are mostly not significant, possibly due to high enrolment rates. Figure 1 provides a graphical representation by plotting the linear prediction of the treatment effects by parent formal schooling exposure for the three data sources. The figure shows a decrease of around 0.14SD in engagement as reported by parents for parents with no education, which translates into 0.42 fewer engagement activities reported in the past three days by the caregiver. Similarly, for attendance, we observe consistent negative effects for non-schooled parents.

[TABLE 1]

[FIGURE 1]

4.2 Effects on Secondary Outcomes

[9] This result is robust by running a triple interaction between any treatment, parental schooling and whether the household was sampled from GUP. GUP households were much more deprived than C4D and with lower levels of literacy (27% vs 40%). We find that any treatment interacted with GUP is always negative and significant, while the triple interaction is always positive, meaning that even in a sample characterized by extreme levels of deprivations like GUP, parents with some formal schooling can benefit from this program (available upon request).

As in the case of the primary outcomes, we find no average treatment effects nor heterogeneity by child gender and age, or by parent gender (available upon request). Again, however, we document heterogeneity by parent schooling, as reported in Table and Figure 2. We recall that literacy, numeracy, and social-emotional skills are age-standardized over the control. Thus, effects represent children's performance relative to same-age peers in the control group. The coefficient for the interaction between being assigned to receive any nudges and parent schooling is positive across all three outcomes, but only statistically significant in the case of social-emotional skills. Nudges increased social-emotional skills for children of parents with some schooling. This measure captured a range of prosocial and problem-solving skills, as well as the extent to which children draw on relationships for support and their ability to manage conflict. Thus, impacts are consistent with the program's emphasis on getting parents closer to their children.

[TABLE 2]

[FIGURE 2]

4.3 Robustness: Is Parental Schooling A Proxy for Something Else?

Parent education is likely correlated with other drivers of educational engagement and child schooling, such as other proxies of household socioeconomic status, parents' access to information, their norms and beliefs, and preferences for educational investments. Also, it could proxy their remoteness. Indeed, Appendix 8 shows that households are quite different across most of these measures based on parents' formal schooling. Thus, we ask whether parent schooling is itself driving the divergent impacts we observe, or whether such heterogeneity is driven by other characteristics that parent schooling might be proxying.

To test this hypothesis, we re-run Equation 1 on the main outcomes by adding interactions between treatment assignment and a range of measures that may be plausibly leading to divergence in treatment effects through their correlation with parental schooling. In Table 3, Column 1 presents the baseline model estimated through Equation 1. In column 2, we add interactions of treatment assignment with variables proxying household socioeconomic conditions and access to information. The latter are operationalized as whether the household is food insecure, owns a TV, and household size. Column 3 adds interactions of treatment with variables related to parent baseline pro-boy bias and whether decisions in the household are taken by the mother alone, by both parents, or by other household members (the omitted category is whether decisions are taken by the father only). We take baseline gender bias and decision-making as proxies for adherence to traditional norms, which parental schooling may indirectly capture. Column 4 adds further interactions of treatment with baseline educational investments. These include whether during school closures, parents: had contacts with their children's teachers, instructed children to engage in remote learning activities, and hired a private tutor. These variables should be capturing household preferences for schooling investments, which can vary by parent education. Finally, column 5 adds an interaction between treatment with distance from children's school in kilometers, as a proxy for educational access and household remoteness. Again, we hypothesize that parent education may proxy such dimensions, with most remote parents being also more likely to display lower levels of human capital.

If any of the interactions between treatment assignment and these variables is driving program impact heterogeneity, the interaction coefficient between treatment and parent schooling would decrease in both magnitude and significance. Table 3 presents the results of this analysis for parent-reported engagement (Panel A), and enrolment and attendance based on school records (Panels B and C). The main take-away from this analysis is that parent schooling remains the key driver of treatment heterogeneity. For both engagement and attendance, interactions between treatment and schooling remained remarkably stable across specifications, including in the fully saturated model of column 5. This highlights an independent role for parents' formal schooling in moderating the effects of the intervention on parental engagement, including when potential correlates of parent schooling have been controlled for. Further, for all three outcomes, the negative effects of being assigned to treatment for parents with *no* schooling becomes three- and four-times larger when the model is augmented with all the interactions. Taken together with the lack of heterogeneity by other variables, these results suggest that parent schooling is the most critical factor driving the unequal effects of the program. This finding is especially remarkable considering that, on average, parents with some schooling have low education levels: half of them had at most completed primary school. We infer that schooling might have been especially important in relation to the way the messages were received, interpreted, and acted upon by parents. We investigate some of these factors in the next section.

[Table 3 here]

5. Mechanisms

5.1 Program fruition by parent education

Variation in fruition and engagement with the program by parental schooling may help explaining the diverging effects we document. Parents with no schooling may reside in more remote communities, whereby connectivity challenges may limit SMS receipt. Further, if they are more likely to be unable to read, they may ignore the messages altogether, or ask someone else to read contents to them. In this case, the person that reads the SMS may add her own interpretation to the meaning of the message, resulting in non-schooled parents more likely to receive a filtered version of the original contents. Also, the mere act of asking someone else to read the messages for them may lead to feelings of inadequacy or shame, increasing the salience of their lack of formal education. Additionally, there may be differences by parental schooling status in the ability to remember, apply, or like the SMS.

These explanations are explored in Table 4. Except for the delivery rates by broker reports, all measures are self-reported by the parents, and were only collected for treatment parents. We start by noting that delivery rates based on broker reports were generally high at around 89%, yet with a small difference based on parental schooling. Parental self-reports related to receiving the messages were, overall, lower than actual delivery rates, again with some differences by parental schooling. Receiving the messages is, of course, different from reading and using them. Unsurprisingly, the largest difference between the two groups of parents relates to their ability to read the messages. Among all parents that reported receiving the messages, 15% with no formal schooling mentioned being able to read the message by themselves, compared to 41% of parents with some schooling. 42% percent of parents often resorted to other members of the household or the community to read the messages, with no differences by schooling. However, parents recalled contents – as measured by a dummy in which they needed to state some of the contents of the messages -- at similar rates, with parents with no schooling slightly more likely to report correctly contents. Similarly, there are no differences in the self-reported rates of applying the contents by parent education.

Overall, these differences highlight that differential program engagement and fruition by parental schooling do not fully explain our main finding. Given the widespread inability to read the messages by themselves, even among parents that went to formal school, in the hindsight, audio messages might have been more effective in this setting, an issue that we will come back to in the discussions.

[Table 4]

5.1.2 Extension: Social spillovers

As many parents had resorted to other members of the household or community to read the SMS for them -- implying that a large portion of treatment participants likely did discuss the messages with someone else -- we examine "social interaction" spillovers (as defined by Angelucci and Di Maro (2015) and Carter et al., 2021) for treated parents within communities[10]. This extension, originally not included in our pre-analysis plan, is important to understanding more fully the societal impacts of messaging programs beyond individual households. We examine spillover effects across treatment participants, focusing on the effects of being "close" to another treatment group household. This measure is defined by having a higher proportion of treated households in the specific 2-km grid in which the household is located. Given the heterogeneity in treatment effects by parent education, we anticipate that the proportion of treated and schooled parents will be another predictor of diverging impacts across communities. The technical details on how this proportion is defined is included in Appendix 9. In this Appendix, we also show that there is high spatial variation in the proportion of treatment and schooled parents across communities, as the treatment was randomized at the household, and not at the community level (Figure A9.1). This allow us to examine social spillover effects arising from being close to other treatment households.

Figure A9.2 plots the results from our baseline model of equation 1 augmented by the terciles in the proportions of treated, and treated and schooled households. Estimates also control for the number of treated and treated *and* schooled households in each grid. We focus on parent-reported engagement and school records of enrolment and attendance.

[10] While many studies examining spillover effects focus on effects on the control group (e.g., Bobonis & Finan, 2009), we cannot pursue this avenue as we did not collect data on implementation for the control. Given the high rates of treated parents asking others to read the SMS, we cannot exclude that this other type of spillover might be at play – potentially damping estimated treatment effects. In this case, our estimates are likely lower bounds of true program effects.

First, by including the proportion of treated households within each grid, the diverging effects of treatment remain unchanged. Also, being surrounded by more treated *and* schooled parents increases the positive effects on enrolment for treatment parents with some schooling. On the other hand, living in communities with a greater proportion of treated households decreased further engagement and enrolment, once we controlled for the proportion and number of treated *and* schooled families in the grid. We do not find evidence of social spillovers for attendance. This suggests that treated parents may discuss contents with peers, leading to more pronounced program effects in both directions.

5.2 Impacts on hypothesized mechanisms

The program's diverging effects might also depend on differential shifts in beliefs and behaviours adopted by the parents based on their formal schooling exposure. Figure 4 reports plotted effect sizes by parental education on pre-specified parental beliefs and behaviors identified as drivers of educational investments, while Appendix Table A10.1 presents point estimates. The program had some major unintended effects for parents with no formal schooling: first, it decreased their self-efficacy by around 10%SD (RW s.e. <0.1). Second, it lowered their educational aspirations. If we take aspirations as a proxy for demand of education (Eble & Escueta, 2021), this is another clue that the intervention backfired. Third, nudges decreased parental positive attitudes towards school attendance, which may explain higher absenteim for children in treatment with unschooled parents. Fourth, it increased their pro-boy bias. By contrast, parents with some schooling lowered their pro-boy bias, raised their attitudes towards school attendance, but also had increased distress levels – likely due to the higher awareness about the importance of their involvement. With regards to child-level pathways, we did not find any change in child time use overall or by parental education[11] (Appendix Table A10.2).

[Figure 4 here]

5.3 Differences by nudge framing and length of exposure

We do not find differences in average impacts by program framing (standard EDU+ vs. gender boost) or length (12-week vs 24-week exposure) (available upon request). Thus, consistent with the rest of the paper, we focus on whether differential program type or length amplifies or mitigates the contrasting effects of nudges by parent education. There are reasons to think that different framings or program length may work differently by parent education. Compared with parents with some schooling, parents with no education may be more reluctant to follow the suggestions of the gender boost framing due to higher baseline pro-boy bias (Appendix 8), which could result in greater backfire effects compared with parents with no schooling receiving the standard program. Concerning length of exposure, parents with no schooling may need more time to change their behaviors as compared with parents with some schooling, and thus a longer program exposure might be relatively more beneficial to them.

In contrast to our expectations, the two program framings worked similarly: for parents with no schooling, the standard and gender boost programs led to decreases in the outcomes, while for parents with some schooling, both framings improved those (Table A11.1). Similarly, there were no differential effects based on program length and parent schooling (Table A11.2). This is an important finding, as a shorter (and presumably cheaper) program seems to be as effective on engagement and school participation as a longer one, if parents have some schooling exposure. Further, as the endline survey was

[11] One limitation of our mechanism analysis is that we did not formally test changes in parental time use on educational activities versus work, although our measure of involvement already should partially capture parental time use on child investments.

conducted between August and September 2021, we note the diverging effects of the short (long) treatment on engagement are persistent after around six (two) months after implementation end. With regards to school-based attendance, however, only the long treatment is persistent.

5.4 Costs

We estimate program costs following the International Rescue Committee and World Bank Group (2019) guidelines, which were designed to allow cross-country comparability of costs across evaluations. Estimated costs[12]—including time devoted to content development and piloting, monitoring, SMS system set-up and roll-out, and overheads—averaged over 18 weeks were 7.56 USD (2021) per child. If the program were to be implemented again, it would only include the cost of sending the SMS. These data highlight the limited costs associated with messaging programs. However, in our context, cost-effectiveness critically depended on appropriate targeting. Future programs may consider tailoring contents based on participant characteristics, including education.

6. Discussion and Conclusions

As interest grows globally in the use of messaging to parents to improve children's educational outcomes, our results confirm that short, light-touch SMS-based interventions *can* change parent behaviors and beliefs. However, our results also caution that attention to subgroups, particularly families of first-generation learners, must be considered. In our sample, impacts varied widely by parent formal schooling exposure. For parents with some schooling, the program worked in the expected direction, supporting parental engagement, and child schooling and social-emotional skills, consistent with our theory of change. Effect sizes on child outcomes are similar in size to what is usually found in the messaging literature in education (Angrist et al., 2022; Ome & Menendez, 2022). By contrast,

[12] Receiving the SMS is free in Ghana.

for parents with no formal schooling, messaging had negative effects on engagement and child school attendance. For the latter group of parents, nudges led to undesired cognitive processes such as decreases in self-efficacy and educational aspirations, or increases in pro-boy bias.

Qualitative data collected with a sub-sample of parents through in-depth interviews and focus groups shed additional light on our findings. First, consistent with the negative effects on parental self-efficacy, parents discussed that the messages raised feelings of inadequacy in supporting their children's schooling. Nudges have been interpreted by many parents as a *signal* of their limited capability in supporting their children's education, and possibly may have increased the salience of their low literacy or limited resources to engage with their children's education. For example, in an interview, one parent stated: "*What the messages were telling us was different from what I have been doing, and I believe I received the messages because they knew I was not doing enough.*" Similar mechanisms have been highlighted in 'social identity' and 'stereotype threat' literatures, whereby disadvantaged groups may perform worse in assessments when they are reminded of their identity (Akerlof & Kranton, 2002).

Second, in line with the increase in distress for the whole sample, qualitative data revealed a dichotomy of emotions in response to the messages: parents reported contrasting feelings motivation to support their children but also increased stress by being more aware about the importance of their engagement for children's education. For example, one parent said: "*It made me feel like I have not been supporting them well. This is because I was not practicing some of the content in the message…it rather motivated and encourage me.*" Similar increases in male parents' stress in response to receiving SMS-nudges have been found in a recent study assessing a digital parenting program in El Salvador (Amaral et al., 2024). For male parents, stress lead to disengagement from parenting, similar to our case. The qualitative findings stress the key and complementary role of adequate engagement with parents throughout program implementation (e.g. through regular group meetings) to adequately convey why they are part of the intervention, and to eventually resolve some of their doubts, as in Ome and Menendez (2022). In our case, we explained to parents what the intervention involved during the enrolment call, but probably this was not enough. Unfortunately, given COVID-related restrictions to gatherings, additional engagement activities were challenging.

Third, parents discussed perceiving this intervention in light of many other programs that have been implemented in their communities; parents were hesitant to believe that the messages and the researchers had their best interest at heart and felt that other programs they had experienced previously were short-lived with no follow through; they assumed this intervention would come and go like the others and thus had doubts about it. This issue may be particularly relevant in our case as both samples for this evaluation were originally part of previous intervention studies. Effects were particularly negative for the GUP sample, which was previosly part of a discontinued large cash and asset transfer evaluation. This insight further underscores the need to engage communities to raise the trustworthiness of the program, and also raises additional research questions related to program credibility based on sender's identity.

Another potential mechanism through which the program may have signaled parents' low educational / social identities and increased the salience of a lack of education was through not being able to read the SMS by themselves. Sixteen percent of parents with no schooling reported that they were able to read the messages as compared to 41% of parents with some schooling. It is possible that parents that asked for help discussed the content with the reader, distorting their interpretation of the content or fostering backlash. The spillover analysis highlights this as a potential channel, as the largest negative effects of the program were for parents living in areas where many peers also received the messages. This evidence highlights that in settings characterized by low literacy rates, audio messages may be most effective. This was a choice that we considered in the design phase, but ultimately decided to employ text-messages based on multiple considerations. First, we operated in the uncertain context of the pandemic, whereby contents may have needed to be changed rapidly (e.g. in the event of further school closures). Audio messages are less nimble than text-messages as their production requires hiring local translators, validating content invariance across multiple languages, and recording messages. All of this would have been challenging due to pandemic-related movement restrictions. Second, a study in Cote d'Ivoire that compared the two modalities showed equal effectiveness but lower delivery rates of audio messages, as delivery of the latter depends on answering the phone at the time of automatic call, while SMS can be read anytime (Wolf & Lichand, 2023). In urban Uganda, audio messages had limited take-up, due to dependency on having a data plan and connectivity issues (Pouezevara & King, 2014). Third, audio messages are more costly, hampering chances of future scale-up if proven effective: in the Cote d'Ivoire study, they cost 2.7 times more than the SMS (Lichand and Wolf, 2020). Fourth, data from the C4D sample that we had available at the design stage suggested that 66% of households had a member that at least completed primary, which reassured us about the capability of at least one household member to read text messages. Unfortunately, schooling rates turned out to be much lower in the GUP sample, for which we did not have this information at the time. Future studies may reconsider the choice of employing messages in low-literacy settings.

Importantly, our results were similar whether the program was implemented for a shorter versus longer duration, and impacts persisted up to 6 months after the short treatment ended. From a program-design perspective, this is an important finding, as nudges are generally believed to be effective in changing behaviors in the short-term, but less is known about their effectiveness after programs end (Brandon et al., 2017; Hertwig & Grüne-Yanoff, 2017). Further, high frequency information interventions are usually more effective (Berlinski et al., 2022; Rogers & Feller, 2018), but in our case biweekly messages were sufficient to change behaviors in the mid-term.

Although a mounting body of evidence indicates that SMS-nudges to parents and children are a "smart buy" for improving educational outcomes, our findings point to the fact that the reality is much more nuanced. Our study is one of the first to test this type of program in a rural, low-literacy, African setting and suggests that careful consideration of the context of parents' and children's lives is needed to ensure programs are tailored in ways that ultimately support parent investments, parent-child relationships, and children's education. Indeed, such unintended effects are not new in the behavioural literature targeting parents in LMICs: in Cote d'Ivoire, messages backfired when they were sent to teachers *and* parents (Lichand & Wolf, 2020), while in rural Nicaragua parents with low education decreased their investments in children if nudges were also sent to local leaders (Barrera-Osorio et al., 2020). Given this mounting level of evidence, it is key to have deeper understanding of parent identities, experiences, and capabilities across diverse contexts to truly make programs a good option for all families and a scalable tool for education systems worldwide. Our results suggest that investing more in building human capital among parents—whether through adult literacy programs, social protection and health, knowledge and skill development, or parenting skills training—should be considered alongside such light-touch SMS programs to optimize their effectiveness in rural, low-literacy contexts.

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Tables

Table 1. Treatment effects by parental schooling on primary outcomes

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent	**Child**	**Parent**	**Child**	**School**	**Parent**	**Child**	**School**
Any nudge	-0.129**	-0.106*	-0.022	-0.028	-0.040	-0.011	-0.032*	-0.030**
	[-0.235 - -0.024]	[-0.228 - 0.015]	[-0.051 - 0.006]	[-0.064 - 0.008]	[-0.088 - 0.008]	[-0.027 - 0.005]	[-0.066 - 0.003]	[-0.060 - -0.000]
Any nudge * some schooling	0.235***	0.191*	0.032	0.052*	0.047	0.009	0.004	0.043*
	[0.059 - 0.412]	[-0.033 - 0.415]	[-0.007 - 0.070]	[-0.003 - 0.106]	[-0.030 - 0.125]	[-0.013 - 0.031]	[-0.055 - 0.063]	[-0.005 - 0.091]
Caregiver ever went to school	-0.075	-0.092	0.009	-0.010	-0.016	0.007	-0.000	-0.010
	[-0.234 - 0.084]	[-0.297 - 0.112]	[-0.025 - 0.042]	[-0.060 - 0.039]	[-0.086 - 0.054]	[-0.013 - 0.026]	[-0.051 - 0.051]	[-0.052 - 0.033]
Observations	4,664	2,099	4,716	2,934	4,137	4,350	2,622	2,923
Mean control no education	0.0217	-0.0307	0.924	0.900	0.719	0.904	0.852	0.736
Unadj. pvalue Nudge	0.0164	0.0856	0.119	0.127	0.105	0.175	0.0724	0.0473
Unadj. pvalue Nudge*School	0.00900	0.0952	0.105	0.0638	0.231	0.434	0.891	0.0823
RW pvalue Nudge	0.00300	0.0310	0.123	0.123	0.123	0.0140	0.0430	0.0280

RW pvalue								
Nudge*School	0.00200	0.0310	0.123	0.0579	0.123	0.0430	0.905	0.0430

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Any nudge is a binary variable equal to 1 if the household was assigned to any program. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed. Standard errors are clustered at the caregiver level. RW p-values were adjusted for multiple testing by group of outcomes using the Romano–Wolf* (Clarke et al., 2020) *step-down method with 1,000 iterations and standard errors clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. The scale was aggregated through a maximum likelihood factor score that included all the items administered in the parent- and child-modules, which was then netted of interviewer effects and standardized over the control. Enrolment is a binary variable =1 if the child is currently enrolled in school. Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, conditional on their report of being enrolled in school) and the proportion of days in which the child was present over total days the school was open (school records). Children's reports are for children aged 10-17 years.*

Table 2. Heterogeneity in effects on child skills by parental schooling

	(1) Literacy	(2) Numeracy	(3) Social-emotional
Any nudge	-0.001	-0.008	-0.023
	[-0.085 - 0.083]	[-0.099 - 0.082]	[-0.116 - 0.070]
Any nudge * some schooling	0.083	0.058	0.179**
	[-0.055 - 0.221]	[-0.106 - 0.221]	[0.021 - 0.336]
Caregiver ever went to school	-0.015	0.038	-0.049
	[-0.138 - 0.109]	[-0.110 - 0.186]	[-0.191 - 0.094]
Observations	4,614	4,629	4,614
Mean control no education	0.00369	-0.0140	0.0142
Unadj. pvalue Nudge	0.977	0.858	0.625
Unadj. pvalue Nudge*School	0.241	0.490	0.0260
RW pvalue Nudge	0.960	0.955	0.834
RW pvalue Nudge*School	0.335	0.756	0.0110

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. RW p-values were adjusted for multiple testing using the Romano–Wolf (2005, 2016) step-down method with 1,000 iterations and standard errors clustered at the caregiver level. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. Parental schooling is a binary variable =1 if the main caregiver has some formal education. Literacy, numeracy and social-emotional skills are measured through a single factor score through maximum likelihood estimation. Outcomes are netted of enumerators effects and then standardised by age and control means.*

Table 3. Inclusion of interactions related to socioeconomic status, norms, and educational investments and access

	(1)	(2)	(3)	(4)	(5)
	Panel A. Parental engagement (parent-reported)				
Any nudge	-0.129**	-0.372***	-0.402**	-0.447**	-0.357*
	[0.0539]	[0.1405]	[0.1802]	[0.1826]	[0.1822]
Any nudge * Some schooling	0.235***	0.225**	0.239***	0.223**	0.185**
	[0.0900]	[0.0899]	[0.0909]	[0.0927]	[0.0942]
Observations	4,664	4,658	4,658	4,654	4,066
Interaction nudge * SES proxy	No	Yes	Yes	Yes	Yes
Interaction nudge * decision-making/norms	No	No	Yes	Yes	Yes
Interaction nudge * Educational investments	No	No	No	Yes	Yes
Interaction nudge * Distance from school	No	No	No	No	Yes
	Panel B. Enrolment (school records)				
Any nudge	-0.030**	-0.097***	-0.109**	-0.106**	-0.105**
	[0.0152]	[0.0348]	[0.0436]	[0.0466]	[0.0472]
Any nudge * Some schooling	0.043*	0.049*	0.053**	0.048*	0.053**
	[0.0247]	[0.0250]	[0.0253]	[0.0251]	[0.0254]
Observations	2,923	2,920	2,920	2,920	2,913
Interaction nudge * SES proxy	No	Yes	Yes	Yes	Yes
Interaction nudge * decision-making/norms	No	No	Yes	Yes	Yes
Interaction nudge * Educational investments	No	No	No	Yes	Yes
Interaction nudge * Distance from school	No	No	No	No	Yes
	Panel C. Attendance (school records)				
Any nudge	-0.040	-0.107*	-0.138**	-0.136*	-0.117
	[0.0246]	[0.0568]	[0.0694]	[0.0737]	[0.0727]
Any nudge * Some schooling	0.047	0.053	0.052	0.049	0.039
	[0.0397]	[0.0401]	[0.0406]	[0.0400]	[0.0396]
Observations	4,137	4,132	4,132	4,128	4,105
R-squared	0.0567	0.0611	0.0635	0.0662	0.0981
Interaction nudge * SES proxy	No	Yes	Yes	Yes	Yes
Interaction nudge * decision-making/norms	No	No	Yes	Yes	Yes
Interaction nudge * Educational investments	No	No	No	Yes	Yes
Interaction nudge * Distance from school	No	No	No	No	Yes

*Notes: Standard errors clustered at the caregiver level in brackets, *** p<0.01, ** p<0.05, * p<0.1 Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementations start was delayed for the household. Parental schooling is a binary variable =1 if the main caregiver has some formal education. Children's reports are for children aged 10-17 years. Engagement measures the number of activities parents or other household members they undertook to support child education. The scale was aggregated through* *a maximum likelihood factor score that included all the items administered. This was netted of interviewer effects and then standardized over the control. Enrolment is a binary variable =1 if the child is enrolled in school. Attendance measures the proportion of days of school attended in which the child was present over total days the school was open. Column 2 includes a model with the following variables and their interactions with any treatment: a binary variable if the household owns a TV, household food insecurity as measured through FAO FiES scale, and household size. Model 3 adds the following variables and interactions: baseline proboy bias, and whether decisions in the household are taken by the mother alone, by both parents, or by other household members (the omitted category is whether decisions are taken by the father only). All these variables were measured at baseline. Column 4 includes proxies for educational investments at baseline and distance from school. We interact treatment with the following variables measuring whether during school closures, parents: had contacts with their children's teachers, instructed children to engage in remote learning activities, hired a private tutor, and distance from school.*

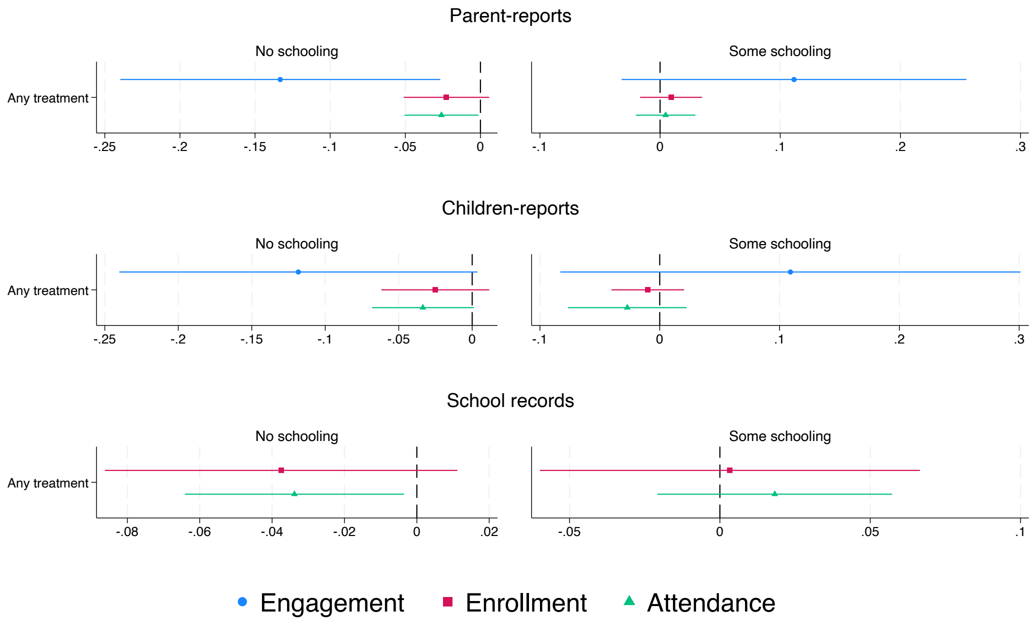
Table 4. Fruition of program, by parent schooling

	N	No schooling Mean/SE	N	Some schooling Mean/SE	Diff. (1)-(2)
Delivery rates (broker reports)	1313	87.997	706	89.595	-1.598*
		[0.574]		[0.721]	
Reeived SMS (parent report)	1250	0.451	672	0.525	-0.074***
		[0.014]		[0.019]	
Parent was able to read (midline)	972	0.154	517	0.412	-0.258***
		[0.012]		[0.022]	
Someone read contents for parents (midline)	1313	0.410	706	0.421	-0.011
		[0.014]		[0.019]	
Parent remember contents (midline)	508	0.719	277	0.657	0.061*
		[0.020]		[0.029]	
Parent applied contents	551	0.670	351	0.704	-0.034
		[0.020]		[0.024]	
Parent still applies contents	555	0.640	348	0.658	-0.018
		[0.020]		[0.025]	

Notes: this table presents differences in means by parental schooling in implementation and program fruition indicators. Except for broker reports of delivery rates, all other indicators are self-reported by treatment parents. Unless stated otherwise, indicators refer to the endline. There were some errors with skip patterns in the administration of this module at both midline and endline, which is why the number of observations varies depending on each indicator.

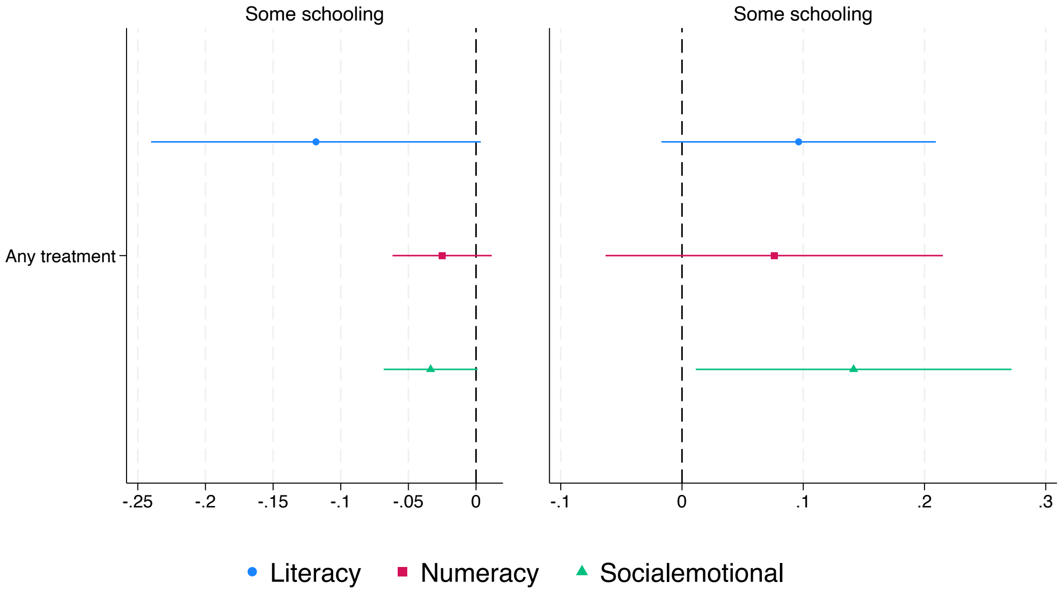
Figures

Figure 1. Plotted effect sizes on primary outcomes, by parent schooling



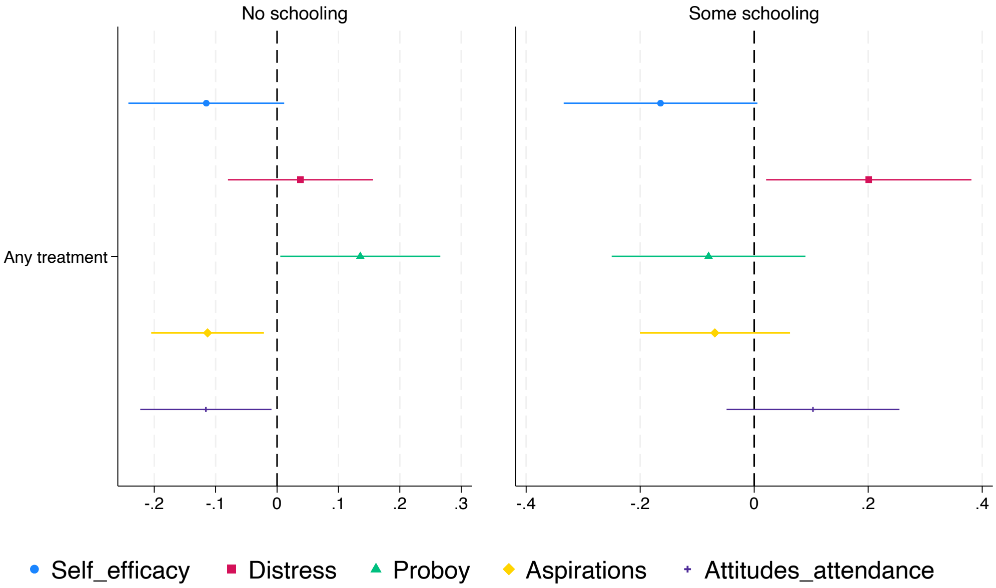
Notes: plotted effect sizes of endline treatment effects with 95% confidence intervals that are not adjusted for multiple hypothesis testing. Coefficients were estimated by regressing each outcome on treatment assignment, and controlling for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. For children's reports, we restricted the sample to children aged 10-17 to be consistent with the results reported in Table 2. Standard errors are clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child development. The scale was aggregated through a maximum likelihood factor score. This was netted of enumerator effects and then standardized over the control. Enrolment is a binary variable =1 if the child is enrolled in school, based on parent- and child-self-reports or administrative records. Attendance measures the proportion of days in the previous week in which parents and children reported for the child to have attended school, and the proportion of days attended by the child over the total days in which the school was open (school records).

Figure 2. Plotted effect sizes on secondary outcomes, by parent schooling



Notes: estimated effect size of treatment effects with 95% confidence intervals that are not adjusted for multiple hypothesis testing. Coefficients were estimated by regressing each outcome on treatment assignment, and controlling for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. Parental schooling is a binary variable =1 if the main caregiver has some formal education. Literacy, numeracy and social-emotional skills were measured through a single factor score through maximum likelihood estimation. Factors were then netted of enumerator effects and then standardized by age and control means.

Figure 3. plotted effects size on parental mechanisms



Notes: estimated effect size of treatment effects with 95% confidence intervals that are not adjusted for multiple hypothesis testing. Coefficients were estimated by regressing each outcome on treatment assignment, and controlling for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. Parental schooling is a binary variable =1 if the main caregiver has some formal education. Parental self-efficacy for education was measured through an eight-item scale using a Likert scale for each item (Bandura et al., 2001). Parental psychological distress was measured through the Kessler Psychological Distress Scale (Kessler et al, 2002), a 10-item questionnaire. Pro-boy bias was measured through the "Gender norms and attitudes scale" (Waszak et al, 2001). Parents also reported their educational aspirations for both focal children, as assessed in the Young Lives survey (Favara 2019). These are measured through a four-category measure, ranging from 1 (Senior High School or less) to 4 (graduate). Parents' attitudes towards school attendance were measured through a eight-item scale asking questions like "When my child misses school, they miss valuable instruction" (Dalzien and Hentorne, 2005). All these variables, except for aspirations, were first netted out of interviewer effects, then aggregated through ML factor scores, and, finally, standardized over the control mean and SD.

Appendix

Appendix 1. Literature review of SMS-based programs in low- and middle-income countries

This literature review highlights that a only *two* of fifteen studies conducted in LMICS were set in a predominantly rural setting with low literacy rates, but did not address the other limitations we identified (see Appendix 1 for a detailed review). Two studies examined heterogeneity in effects by household socioeconomic backgrounds, and three did so by child or parent gender. Further, only a few studies investigated mechanisms—a key gap in understanding of how educational behavioral programs impact underlying *processes* underpinning educational outcomes (Weijers et al., 2021). No study we reviewed experimentally varied program length, and only one (Amaral et al., 2024) examined persistence of impacts after six months from program end. All other studies examined effects immediately after implementation end. Few studies, none in Africa, relied on administrative data.

Study	**Study design; Program design and implementation**	**Context & parent literacy**	**Results**	**Heterogeneity assessed?**	**Mechanisms?**	**Persistence?**
Ajzenman et al., 2022	RCT SMS delivered through government's official app to increase preschool attendance (13 weeks) Children 4 -5 years	Uruguay Parent education not reported	Null effects for increasing school attendance and cognitive skills	Significant effect for schools with socioeconomic status (SES) below the highest quintile	Improve cognitive skills through the increase in school attendance through nudges to parents	Not assessed

Amaral et al., (2024)	RCT Three whatsapp messages on stress management and positive parenting (nine weeks) Children 0-8 years	El Salvador, COVID-pandemic; All parents have at least basic education	Negative impact on parent mental health	Treatment increased stress and violent punishment for male caregivers and poor households, but not for female caregivers. Family structure and poverty moderated effects	Effects driven by increase in stress. No effects on caregiver impulsiveness, the quantity of caregiver–child interactions, caregiver perpetration of abuse and attitudes toward violent parenting, or children's behaviors	Not assessed
Angrist et al., 2022	RCT SMS or SMS+calls implemented for a sample of 4,500 families of primary school children	Botswana (Covid school closures) with 29% sample rural. 29% parents had more than secondary school	Increase learning but small effects. Combined SMS + calls more effective	No	Parents more engaged in child education and increased self-efficacy	Effects evaluated two months after intervention end
Angrist et al., 2023	RCT to test at scale the approach tried in Angrist et al (2020) SMS or SMS+calls implemented during 8-16 weeks (depending on country)	India, Kenya, Nepal, Philippines, and Uganda during COVID-19 crisis Parent literacy rates not reported	Large effects of phone-based tutorials on learning, with average effects of 0.30-0.35SD	Calls particularly effective if caregiver had lower levels of education (primary education	The program caused a net increase in the share and frequency of caregivers undertaking educational activities with their child	Not assessed

				or less). No differences by child gender and baseline learning		
Barrera et al., 2020	RCT Personalized daily messages to parents of preschoolers (0-6 years) delivered over 10 months	Nicaragua, four rural municipalities Low educational levels (mothers: 3,26 years; fathers: 3.0 years)	No effects on children's skills	When local leaders were exposed to messages, backfire effects for kids with parents with low education	The program improved self-reported parent engagement	Not assessed
Beam et al., 2022	RCT Three treatments: 1. Phone learning support; 2. Informational biweekly SMS about a new educational app; 3. Internet data subsidy with SMS. 4-8 weeks implementation Secondary school students (grades 6–10)	Bangladesh (Covid school closures). A third of parents with less than primary school	Information on the app increases moderately learning (acts as a nudge)	Learning gains larger among richer households.	Combining SMS with a data package increased app usage without increasing spending on tutors.	Increases in student math achievement only for the app information campaign two months later.

Berlinski et al., 2022	RCT Weekly and monthly SMS to parents about attendance, grades, and behavior. A subset of parents received SMS on parent engagement. 2 academic years Children last 5 grades of primary	Chile, low-income urban schools in Chile. 53% of mothers have completed high school	Improved math grades and attendance, with particularly large impacts on at-risk students, and positive spillovers within classrooms.	Larger impacts on at-risk students	Reduced information gaps about student attendance, grades and classroom behavior. Students perceived that they received significantly more family Support. Greater parent engagement for sub-sample that received engagement SMS	Not assessed
Bettinger et al., 2019	RCT EDUQ+ (focus on parent engagement) implemented for 18 weeks Ninth grade children (N=19,300)	São Paulo, Brazil Around 30% of main caregivers with high school	Increase in school attendance, short-term increases in test scores.	Not assessed	Salience SMS increases effort, but not information treatment. 45% increase in accuracy of between beliefs and absences reported by teachers.	Not assessed
Crawfurd et al., 2023	RCT Three treatments: 1. SMS; 2. SMS + weekly tutoring calls (private teachers); 3. SMS+ tutoring calls from government teachers	Sierra Leone, COVID-pandemic 64% of sample in Freetown, remaining in rural districts	Tutoring calls increased engagement in educational activities but had no effect on test scores	No differences by child gender or school type	No increases in engagement in educational activities	Ten weeks after intervention end

	Children aged 7-17 years enrolled in school	47% household heads completed secondary school.				
Dinarte Diaz et al., (2023)	Virtually delivered 10-week parenting intervention in Jamaica for parents with children ages 2-6 years. 30 SMS messages, data-free app with weekly content, and opportunity to join weekly, one-hour virtual group parenting sessions.	Jamaica. On average parents have completed 14 years of primary school	Changes in caregiver disciplining behaviors, with a 0.12 SD reduction in violence against children. Treatment children also experience fewer emotional problems (0.17 SD).	Some evidence that impacts are larger with those with higher endorsement of violence against children at baseline; no differences by gender or income]	Caregiver knowledge (0.52 SD) and attitudes around violence (0.2 SD)	Nine months later, also find reductions in caregiver depression (0.12 SD), anxiety (0.16 SD), and parental stress (0.16 SD) for treatment caregivers.
Lichand and Christen, (2023)	RCT EDUQ+ to increase parent engagement and remote learning among middle and high school students (10-17 years). Implemented for 6 months	Brazil (Covid school closures) Parental literacy not reported	Increase in portuguese scores and decrease in drop-out rates	Increased learning for high-achievers and girls at baseline, but decreased drop-out for kids most at risk	Explored different framing of nudges (motivation and peer pressure) No differences if SMS sent to parents or children	Not assessed
Hernandez-Agramonte et al., (2022)	RCT	Costa Rica (Covid school closures)	SMS improved child cognitive skills and parent engagement	Not available	Parents increased the activities they did with their children and also	Not assessed

	SMS to support parent engagement. 15-week implementation Preschool kids (3-5 years)	Different areas of the country (both rural and urban) 53% parents completed high school. Parents self-enrolled in the program			complemented remote learning with other activities. No effects on parental self-efficacy	
Ome & Menendez (2022)	School-based RCT Grade1 and 2 students Parents received three SMS weekly comprising a short story for children to read with their families, and a question about the story for nine months. Parents also attended monthly meetings to encourage reading	Two districts in Zambia 70% parents can read	Positive effects on early reading (0.2-0.3SD)	No differences by child gender or baseline reading skills, greater effects if the child's caregiver can read on oral reading, and higher impact on grade 3 students	Children were spending more time reading at home on their own or with family members. Monthly meetings may have encouraged more caregiver involvement with their children's education	Not assessed
Pouezevara and King (2014)	SMS with audio instructions for early literacy	Uganda (Wakiso district of urban Kampala, parents signed up to the intervention) 33% parents with no education, but 80% mentioned they could read and write Luganda well	Learning gains for basic literacy were comparable across the audio SMS arm and a printed content arm	Not assessed	Not assessed	Not assessed

Wolf and Lichand (2023) and Lichand and Wolf (2020)	RCT with SMS nudges to parents, teachers, or both, implemented for 9 months over one school year. Second and fourth grade students.	Cote d'Ivoire, rural areas 41% parents never attended school	Small but statistically insignificant impacts on learning outcomes in parent-only arm; increases in child labor	Program improved learning for baseline low-achievers of parents-only arm; negative impacts on learning for girls in teachers-only arm	Audio vs SMS but results were similar	Not assessed

Appendix 2. Program Sequences and Contents

Table A2.1. Example of sequence structures, EDU+ (top) and 'gender boost' (bottom)

Week 1, message 1 (motivating fact): Your children have a lot to say! Sitting down to talk as a family strengthens your relationship and creates a space for children to be guided.
Week 1, message 2 (activity): Be AVAILABLE for your children: start by telling them how your day went, what made you happy or sad, then go on and ask them how their day was.
Week 2, message 1 (interactivity): Ever stop to think about how many times a day you help your child? Reflect on this and try to increase this number!
Week 2, message 2 (growth message): Get close to children, talk to them often. It's your duty to assure them of the STUDY and the AUTONOMY they need for a successful future.

Week 1, message 1 (motivating fact): Your daughters have a lot to say! Sitting down to talk as a family strengthens your relationship and creates a space for children to be guided.
Week 1, message 2 (activity): Be AVAILABLE for your daughters: start by telling them how your day went, what made you happy or sad, then go on and ask them how their day was.
Week 2, message 1 (interactivity): Ever stop to think about how many times a day you help your daughter? Reflect on this and try to increase this number!
Week 2, message 2 (growth message): Get close to your son and daughter, talk to them often. It's your duty to assure them of the STUDY and the AUTONOMY they need for a successful future.

Table A2.2 List of program sequences and contents

Sequence	Theme	Edu+	Gender Boost
1	Introduction	Encouragement to enroll children back in school	Encouragement to enroll daughters and sons back in school.
2	Back to school and parent involvement	Hygiene measures to ensure children attend school safely	Hygiene measures to ensure daughters attend school safely
3	Education, aspirations and future	Encouragement to support children's education and dreams	Encouragement to support daughters' education and dreams.
4	Parenting	Strengthen the relationship with children by being available and talking to them	Strengthen the relationship with daughters by being available and talking to them
5	Education, aspirations and future	Help children organize their work timetable	Help daughters organize their work timetable
6	Parenting	Encourage to stop beating children and solve problems together	Encourage to stop beating daughters and solve problems together
7	Back to school and parent involvement	Show more interest in children's school life	Show more interest in daughters' school life
8	Other social and emotional skills	Organize and divide the household responsibilities with your children	Organize and divide the household responsibilities with your sons and daughters
9	Parenting	Encouragement to play and spend time with children	Encouragement to play and spend time with sons and daughters
10	Other social and emotional skills	Encourage children to challenge themselves and learn from their mistakes	Encourage daughters to challenge themselves and learn from their mistakes
11	Other social and emotional skills	Teach children to not give up and be persistent with their goals	Teach daughters to not give up and be persistent with their goals
12	Other social and emotional skills	Help children grow their confidence	Help daughters love their body and qualities
13	Parenting	Strengthen the relationship with children by learning from each other	Strengthen the relationship with daughters by learning from each other
14	Farewell	Encouragement to keep up with the activities to ensure children's success	Encouragement to keep up with the activities to ensure daughters' success

Appendix 3. Attrition

Only 88 households were lost-to-follow-up in the endline survey (3.35% of initial sample). Columns 1 and 2 of Table A3.1 show that neither treatment assignment, nor treatment interacted with household characteristics predict household tracking between baseline and endline (2020-2021). Columns 3 and 4 focus on tracking during the administrative data collection occurred in 2023, whereby 91% of children that were assessed at endline were tracked two years later. Again, treatment assignment did not predict tracking, while only the number of school-children in the household interacted with treatment assignment predicted tracking after two years from the endline.

Table A3.1 Predictors of tracking in survey and administrative data

	(1)	(2)	(3)	(4)
	Household tracked at endline survey		Child tracked with administrative data	
Any treatment	-0.002	0.026	-0.006	-0.031
	[0.004]	[0.020]	[0.010]	[0.060]
Parent male		-0.015		0.018
		[0.018]		[0.028]
Nudge * Parent male		0.013		-0.007
		[0.020]		[0.031]
Parent age		0.000		0.000
		[0.000]		[0.001]
Nudge * Parent age		-0.001		-0.001
		[0.000]		[0.001]
Parent schooling		-0.003		-0.029
		[0.008]		[0.021]
Treatment * Schooling		0.012		0.032
		[0.009]		[0.023]
Parent is HH head		-0.001		-0.018
		[0.010]		[0.025]
Nudge * Parent is head		0.004		0.024
		[0.011]		[0.027]
# children		0.002		-0.003
		[0.003]		[0.007]
Nudge * # children		-0.002		0.014*
		[0.003]		[0.008]
Household size		-0.000		0.001
		[0.001]		[0.002]
Nudge * Household size		0.001		-0.001
		[0.001]		[0.002]
Northern	-0.000	0.006	-0.029***	-0.000
	[0.000]	[0.004]	[0.010]	[0.027]
Savannah	-0.000	0.002	-0.396***	-0.342***

	[0.000]	[0.003]	[0.032]	[0.069]
Upper East	-0.016***	-0.014	-0.030**	-0.032
	[0.005]	[0.012]	[0.012]	[0.036]
Upper West	-0.021***	-0.003	-0.044***	-0.033
	[0.007]	[0.009]	[0.013]	[0.038]
Nudge * Northern		-0.009*		-0.018
		[0.005]		[0.030]
Nudge * Savannah		-0.000		-0.069
		[0.003]		[0.078]
Nudge * Upper East		-0.007		0.042
		[0.015]		[0.040]
Nudge * Upper West		-0.028**		0.027
		[0.013]		[0.042]
GUP sample		0.016		0.008
		[0.012]		[0.032]
Nudge * GUP sample		-0.023		0.012
		[0.014]		[0.035]
SMS later		0.007*		-0.003
		[0.004]		[0.013]
Constant	1.002***	0.976***	0.959***	0.938***
	[0.003]	[0.016]	[0.012]	[0.054]
Observations	2,558	2,555	4,654	4,648
R-squared	0.011	0.018	0.085	0.092

*Notes: Robust standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1. Household tracked =1 if the household is present between baseline and endline (December 2020 and August/September 2021). Child tracked =1 if a child interviewed at endline has been tracked in the administrative data collection (January-February 2023).*

Appendix 4. Balance in baseline household characteristics

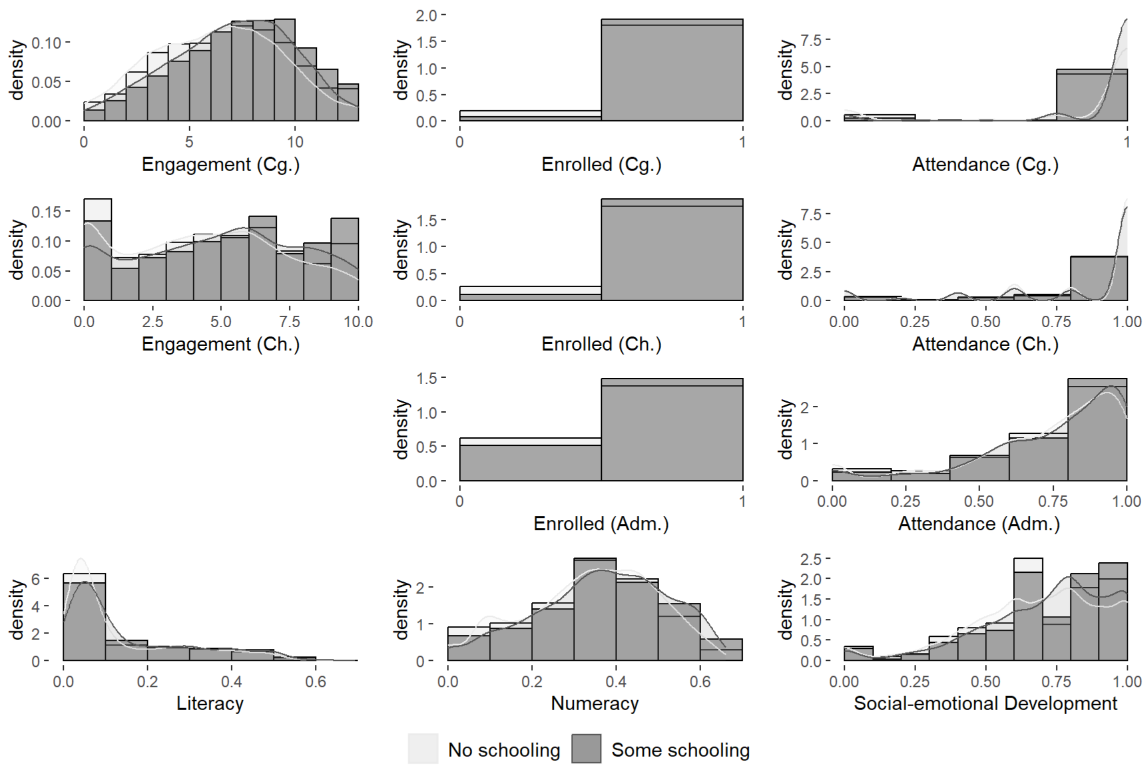
	Control		Any treatment		
	N	Mean/SE	N	Mean/SE	T-test of differenc e
Caregiver is head	517	0.482	2019	0.475	0.007
		[0.022]		[0.011]	
Caregiver is male	517	0.424	2019	0.411	0.013
		[0.022]		[0.011]	
Caregiver age	517	42.768	2016	43.083	-0.315
		[0.537]		[0.266]	
Caregiver schooling	517	0.342	2019	0.350	-0.007
		[0.021]		[0.011]	
Owns TV	517	0.491	2016	0.418	0.074***
		[0.022]		[0.011]	
Household size	517	9.756	2019	9.760	-0.004
		[0.237]		[0.113]	
School-age children, #	517	3.093	2019	3.085	0.008
		[0.070]		[0.035]	
Household school engagement	511	0.036	1996	-0.018	0.054
		[0.041]		[0.020]	
Number of books in household	509	7.409	2002	4.953	2.456
		[2.947]		[0.511]	
Expected returns from JHS	438	5.641	1666	5.667	-0.027
		[0.046]		[0.022]	
Expected returns from SHS	471	6.283	1799	6.318	-0.034
		[0.041]		[0.019]	
Expected returns from University	493	7.402	1875	7.383	0.019
		[0.036]		[0.018]	
Caregiver pro-boy bias	476	-0.004	1858	0.005	-0.009
		[0.041]		[0.022]	
All children doing remote learning activities	517	0.381	2019	0.392	-0.011
		[0.021]		[0.011]	
Caregiver low educational aspirations (girls)	403	0.201	1579	0.198	0.003
		[0.020]		[0.010]	
Caregiver low educational aspirations (boys)	444	0.106	1672	0.144	-0.038**
		[0.015]		[0.009]	
North East	517	0.103	2018	0.135	-0.033**
		[0.013]		[0.008]	
Northern	517	0.435	2018	0.409	0.026
		[0.022]		[0.011]	
Savannah	517	0.058	2018	0.055	0.003
		[0.010]		[0.005]	
Upper East	517	0.217	2018	0.220	-0.003
		[0.018]		[0.009]	
Upper West	517	0.188	2018	0.181	0.007
		[0.017]		[0.009]	

Christian	489	0.198	1926	0.199	-0.001
		[0.018]		[0.009]	
Muslim	489	0.691	1926	0.677	0.015
		[0.021]		[0.011]	
Traditional religions	489	0.092	1926	0.101	-0.009
		[0.013]		[0.007]	
GUP sample	517	0.406	2019	0.425	-0.019
		[0.022]		[0.011]	

Appendix 5. Measures

This appendix presents descriptive statistics of key study outcomes in Figure A5 and then discusses the different measures of mechanisms included in the analysis.

Figure A5. Descriptive statistics of raw primary and secondary outcomes, endline



Notes: descriptive statistics of raw primary and secondary outcomes at endline, by treatment assignment. Nudge is a binary variable =1 if the household was assigned to any program. Caregiver engagement (parent-report, cg, and child-report, ch.) are counts variables that measure the number of activities parents or other household members undertook to support child development. Enrolment is a binary variable =1 if the child is enrolled in school, based on parent- and child-self-reports or administrative records. Attendance measures the number of days in the previous week in which parents and children reported the child attended school, and the proportion of days attended by the child over the total days in which the school was open (school records). Literacy, numeracy, and social-emotional development are measured as percent correct. Variables related to children are for 10-17 only.

Measures of mechanisms

As hypothesized mechanisms to explain program effectiveness, we measure:

Parent self-efficacy for child education was measured through an eight-item scale using a Likert scale for each item (Bandura, 2001). Examples include: "How much can you do to make your children see school as valuable?" and "How much can you do to help your children get good grades in school?".

Parent psychological distress was assessed through the Kessler Psychological Distress Scale (Kessler et al., 2002), a 10-item questionnaire used globally to measure general psychological distress based on questions about anxiety and depressive symptoms. Each item is scored from zero (none of the time) to four (all the time). Items are added to create a total score, with higher scores indicating higher psychological distress and a higher likelihood of a mental health disorder.

Pro-boy bias was measured through the "Gender Norms and Attitudes Scale" (Waszak et al., 2001), which assesses gender-egalitarian beliefs and norms. The scale assesses whether parents agree or disagree with a series of statements about gender equity and maintaining the rights and privileges of men versus women (14 items). Example include: "Daughters should be sent to school only if they are not needed to help at home," and "Daughters should have just the same chance to work outside the homes as sons".

Parents also reported their *educational aspirations and expectations* for both focal children, as assessed in Young Lives (Favara, 2017). Both were measured through a single question: 'What is the highest level of education that you *wish* [child] to achieve?' if no barriers existed for aspirations, and 'What is the highest level of education that you *expect* [child] to achieve?' for expectations. Both variables were coded as follows: 1= to Senior High School (SHS) or less; 2=Higher Vocational Training (e.g., College of Education, Agriculture, Nursing, etc) or a University Diploma; 3=bachelor's degree; 4=graduate level.

Regarding school attendance, we first measured parents' *attitudes towards school attendance* through an eight-item scale adapted from Dalziel and Henthorne (2005). Parents reported on how much they agree with a set of statements on a scale from 1-4 (e.g., "When my child misses school, they miss valuable instruction"). In addition, we measured child self-reported attendance rates (days missed in the past week) and collected school administrative records on attendance rates in the 2021 and 2022 school years.

Child time use was measured through questions administered to children aged 10-17 years, asking them about the hours spent in the previous day (or in a normal day of the week, if the previous day was over the weekend or a holiday) in different activities, including: time spent in sleep, at school, studying, doing house-chores or caring for other household members, and at work in the family farm or business. This module was administered by using pebbles, to facilitate recall, and was based on the Young Lives survey.

Appendix 6. Average treatment effects on primary outcomes, and heterogeneity by child gender, age group, and parent gender

A6.1. Average treatment effects

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent-reports	**Child-reports**	**Parent-reports**	**Child-reports**	**School records**	**Parent-reports**	**Child-reports**	**School records**
Any nudge	-0.046	-0.039	-0.011	-0.010	-0.023	-0.008	-0.030**	-0.015
	[-0.132 - 0.040]	[-0.143 - 0.064]	[-0.032 - 0.009]	[-0.038 - 0.018]	[-0.062 - 0.015]	[-0.020 - 0.004]	[-0.058 - -0.002]	[-0.038 - 0.009]
Observations	4,668	2,099	4,720	2,934	4,137	4,350	2,622	2,923
Mean control no education	0.0217	-0.0307	0.924	0.900	0.719	0.904	0.852	0.736
Unadj. pvalue Nudge	0.290	0.459	0.284	0.483	0.236	0.211	0.0373	0.234
RW pvalue Nudge	0.266	0.294	0.256	0.328	0.256	0.0539	0.00699	0.116

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Any nudge is a binary variable equal to 1 if the household was assigned to any program. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. RW p-values were adjusted for multiple testing using the Romano–Wolf* (Clarke et al., 2020) *step-down method with 1,000 iterations and standard errors clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. The scale was aggregated through a maximum likelihood factor score that included all the items administered in the parent- and child-modules, which was then netted of interviewer effects and standardized over the control. Enrolment is a binary variable =1 if the child is currently enrolled in school (parent- and child-reports) and by trimester (school records). Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, if they report enrolment) and the proportion of days in which the child was present over total days the school was open (school records). Children's reports are for children aged 10-17 years.*

A6.2 Heterogeneity by child gender

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent-reports	Child-reports	Parent-reports	Child-reports	School records	Parent-reports	Child-reports	School records
Any nudge	0.001	-0.016	-0.018	-0.011	0.034	-0.006	-0.009	0.005
	[-0.120 - 0.123]	[-0.165 - 0.133]	[-0.045 - 0.009]	[-0.048 - 0.026]	[-0.023 - 0.090]	[-0.021 - 0.009]	[-0.050 - 0.032]	[-0.031 - 0.041]
Any nudge * Child male	-0.086	-0.041	0.011	0.001	-0.100***	-0.004	-0.037	-0.034
	[-0.229 - 0.058]	[-0.239 - 0.157]	[-0.023 - 0.046]	[-0.049 - 0.051]	[-0.166 - - 0.034]	[-0.023 - 0.015]	[-0.088 - 0.013]	[-0.079 - 0.010]
Child is male	0.011	-0.017	-0.014	-0.019	0.053*	-0.007	0.010	0.013
	[-0.116 - 0.138]	[-0.194 - 0.160]	[-0.044 - 0.017]	[-0.063 - 0.025]	[-0.006 - 0.111]	[-0.023 - 0.009]	[-0.033 - 0.053]	[-0.026 - 0.052]
Observations	4,668	2,099	4,720	2,934	4,137	4,350	2,622	2,923
Mean control female	-0.0156	-0.0498	0.943	0.926	0.703	0.925	0.852	0.727
Unadj. pvalue Nudge	0.985	0.835	0.186	0.562	0.241	0.450	0.669	0.797
Unadj. pvalue Nudge*Male	0.242	0.685	0.522	0.980	0.00315	0.701	0.149	0.129
RW pvalue Nudge	0.981	0.946	0.254	0.721	0.328	0.229	0.885	0.912
RW pvalue Nudge	0.981	0.946	0.254	0.721	0.328	0.229	0.885	0.912
RW pvalue Nudge*Male	0.284	0.905	0.721	0.971	0.00200	0.955	0.172	0.163

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Any nudge is a binary variable equal to 1 if the household was assigned to any program. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. RW p-values were adjusted for multiple testing using the Romano–Wolf* (Clarke et al., 2020) *step-down method with 1,000 iterations and standard errors clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. The scale was aggregated through a* *maximum likelihood factor score that included all the items administered in the parent- and child-modules, which was then netted of interviewer effects and standardized over the control. Enrolment is a binary variable =1 if the child is currently enrolled in school (parent- and child-reports) and by trimester (school records). Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, if they report enrolment) and the proportion of days in which the child was present over total days the school was open (school records). Children's reports are for children aged 10-17 years.*

A6.3 Heterogeneity by child age group

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent-reports	Child-reports	Parent-reports	Child-reports	School records	Parent-reports	Child-reports	School records
Any nudge	-0.003	-0.060	0.009	-0.002	0.013	-0.006	-0.046**	-0.011
	[-0.137 - 0.131]	[-0.194 - 0.074]	[-0.023 - 0.042]	[-0.039 - 0.034]	[-0.046 - 0.073]	[-0.024 - 0.013]	[-0.085 - - 0.007]	[-0.049 - 0.027]
Any nudge * Child 10-17y	-0.070	0.011	-0.033*	-0.007	-0.058*	-0.003	0.012	-0.005
	[-0.226 - 0.086]	[-0.146 - 0.167]	[-0.071 - 0.006]	[-0.050 - 0.035]	[-0.125 - 0.009]	[-0.024 - 0.017]	[-0.031 - 0.055]	[-0.049 - 0.039]
age_group	0.288***	-0.139*	-0.002	-0.016	0.047	0.000	0.005	0.017
	[0.150 - 0.426]	[-0.279 - 0.001]	[-0.037 - 0.032]	[-0.054 - 0.022]	[-0.012 - 0.105]	[-0.018 - 0.018]	[-0.032 - 0.041]	[-0.021 - 0.054]
Observations	4,668	3,749	4,720	4,609	4,137	4,350	4,125	2,923
Mean control younger	-0.186	0.0728	0.926	0.903	0.687	0.912	0.847	0.719
Unadj. pvalue Nudge	0.963	0.379	0.566	0.898	0.662	0.551	0.0215	0.565
Unadj. pvalue Nudge*Older	0.379	0.895	0.0956	0.733	0.0893	0.748	0.575	0.831
RW pvalue Nudge	0.982	0.518	0.712	0.712	0.712	0.874	0.00999	0.874
RW pvalue Nudge*Older	0.518	0.982	0.0839	0.000999	0.0839	0.654	0.874	0.874

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Any nudge is a binary variable equal to 1 if the household was assigned to any program. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. RW p-values were adjusted for multiple testing using the Romano–Wolf* (Clarke et al., 2020) *step-down method with 1,000 iterations and standard errors clustered at the* *caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. The scale was aggregated through a maximum likelihood factor score that included all the items administered in the parent- and child-modules, which was then netted of interviewer effects and standardized over the control. Enrolment is a binary variable =1 if the child is currently enrolled in school (parent- and child-reports) and by trimester (school records). Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, if they report enrolment) and the proportion of days in which the child was present over total days the school was open (school records).*

A6.4 Heterogeneity by parent gender

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent-reports	Child-reports	Parent-reports	Child-reports	School records	Parent-reports	Child-reports	School records
Any nudge	-0.008	-0.032	0.003	-0.001	-0.013	-0.005	-0.036**	-0.025*
	[-0.116 - 0.100]	[-0.157 - 0.094]	[-0.022 - 0.027]	[-0.028 - 0.026]	[-0.063 - 0.037]	[-0.019 - 0.008]	[-0.072 - -0.001]	[-0.054 - 0.004]
Any nudge * Caregiver male	-0.095	-0.018	-0.032	-0.024	-0.023	-0.006	0.018	0.023
	[-0.268 - 0.078]	[-0.233 - 0.197]	[-0.076 - 0.011]	[-0.087 - 0.040]	[-0.100 - 0.054]	[-0.031 - 0.020]	[-0.039 - 0.076]	[-0.026 - 0.071]
Caregiver is male	0.014	-0.099	0.051**	0.024	0.051	0.003	-0.027	-0.055**
	[-0.158 - 0.185]	[-0.302 - 0.105]	[0.007 - 0.095]	[-0.040 - 0.087]	[-0.025 - 0.128]	[-0.020 - 0.026]	[-0.082 - 0.029]	[-0.102 - -0.009]
Observations	4,668	2,099	4,720	2,934	4,137	4,350	2,622	2,923
Mean control female	0.0396	0.0152	0.944	0.943	0.728	0.435	0.0424	0.0891
Unadj. pvalue Nudge	0.889	0.621	0.823	0.945	0.608	0.658	0.532	0.356
Unadj. pvalue Nudge*Male	0.283	0.867	0.148	0.470	0.564	0.779	0.0230	0.0819
RW pvalue Nudge	0.969	0.864	0.931	0.931	0.872	0.137	0.659	0.489
RW pvalue Nudge*Male	0.384	0.969	0.214	0.819	0.872	0.931	0.860	0.767

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Any nudge is a binary variable equal to 1 if the household was assigned to any program. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. RW p-values were adjusted for multiple testing using the Romano–Wolf* (Clarke et al., 2020) *step-down method with 1,000 iterations and standard errors clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. The scale was aggregated through a maximum likelihood factor score that included all the items administered in the parent- and child-modules, which was then netted of interviewer effects and standardized over the control. Enrolment is a binary variable =1 if the child is currently enrolled in school (parent- and child-reports) and by trimester (school records). Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, if they report enrolment) and the proportion of days in which the child was present over total days the school was open (school records). Children's reports are for children aged 10-17 years.*

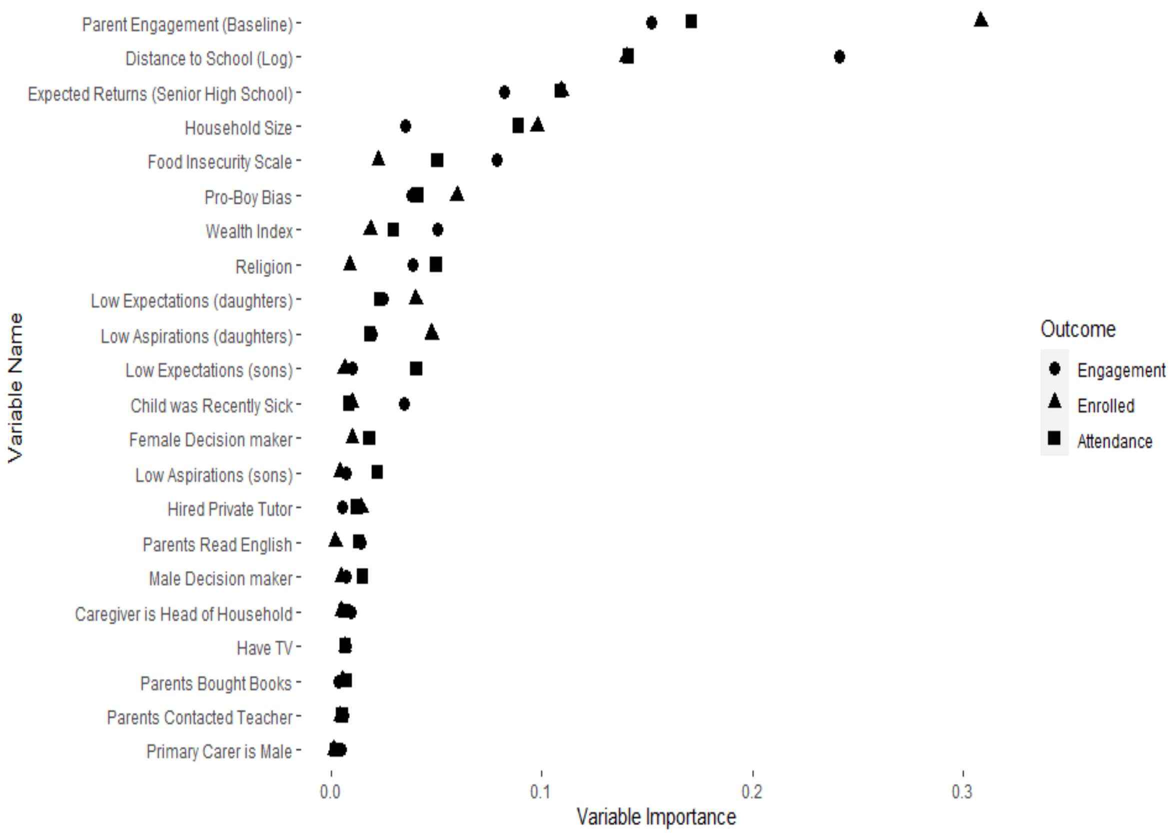
Appendix 7. Results from causal forest models

As part of our heterogeneity analysis, we explored whether other non-pre-specified variables may drive variation in treatment effects. We thus employ a data-driven approach through causal forest methodology (Athey & Wager, 2019).

We first fed the causal forest algorithm with a wide set of *baseline* characteristics: parent engagement score, expected returns to completing senior high school (as a proxy of perceived educational returns), wealth index, pro-boy bias, household size, head's religion, if households have a TV, low baseline educational aspirations and expectations for daughters and sons, whether the child was recently sick, if the household's decision maker was a woman or a man (compared with joint decision-making), if the parent had schooling, if the parents read English, whether parents hired a private tutor or contacted teachers during school closures, whether children's primary carer was a man or the head of the household, and food insecurity. We also included the log distance to school by calculating the distance between the household and children' s schools.

Figure A7.1 presents ranks variable importance for endline caregiver-reported engagement; school enrolment and attendance (based on school records). We consistently found that parent engagement at baseline, the distance to the school (logged), and expected returns to senior high school have the highest predictive power for our outcomes. Results are similar if we use caregiver- and child-reported school participation variables, and child-reported engagement. Thus, we focus on these variables, and add the interaction of these with being randomly assigned to any nudge.

Figure A7.1 Average variable importance based on causal forest



Notes: Variable importance for parent engagement (parent-reported) and enrolment and attendance (school records), using a generalized random forest framework (N=2,000). The variable importance plot provides a simple weighted sum of how many times a feature was split at each depth in the forest.

Table A7.1 present the results of the regressions measuring the effect of treatment on parent-reported engagement, and enrolment and attendance as reported by the administrative data, with the same controls as in our original specification, and adding interactions with the variables that most drove heterogeneity as identified by the previous step. The main result from this analysis is that any of the interactions between these variables with the treatment is significant. The only exception is a positive interaction between treatment and perceived returns to education for enrolment. Based on this analysis and the lack of heterogeneity by our pre-specified axes, we conclude that parent education is the main variable driving divergence in our results.

Table A7.1: Heterogeneity analysis Causal Forest

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Engagement			**Enrolment**			**Attendance**	
Any nudge	-0.079*	-0.038	0.062	-0.023	-0.021	-0.363**	-0.015	-0.016	0.039
	[-0.166 - 0.007]	[-0.122 - 0.047]	[-0.554 - 0.679]	[-0.060 - 0.014]	[-0.060 - 0.018]	[-0.667 - -0.058]	[-0.039 - 0.009]	[-0.040 - 0.008]	[-0.137 - 0.215]
Distance from schools	-0.074***			-0.069***			0.011*		
	[-0.123 - -0.025]			[-0.089 - -0.048]			[-0.002 - 0.023]		
Nudge*Distance	0.030			0.015			-0.002		
	[-0.025 - 0.084]			[-0.009 - 0.038]			[-0.017 - 0.012]		
Baseline engagement		0.169***			0.011			0.027**	
		[0.088 - 0.250]			[-0.030 - 0.052]			[0.004 - 0.049]	
Nudge*Baseline engagement		0.017			0.007			-0.004	
		[-0.074 - 0.109]			[-0.038 - 0.053]			[-0.030 - 0.022]	
Baseline SHS returns			0.015			-0.033			-0.010
			[-0.068 - 0.097]			[-0.076 - 0.010]			[-0.034 - 0.014]
Nudge*Baseline returns			-0.013			0.055**			-0.007
			[-0.111 - 0.085]			[0.007 - 0.103]			[-0.035 - 0.020]
Observations	4,074	4,615	4,170	4,113	4,087	3,698	2,916	2,888	2,626

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Any nudge is a binary variable equal to 1 if the household was assigned to any program. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. The scale was aggregated through a maximum likelihood factor score that included all the items administered, which was then netted of interviewer effects and standardized over the control. Enrolment is a binary variable =1 if the child was enrolled (school records). Attendance measures the proportion of days of school attended over total days the school was open (school records). Distance from schools measure the log-distance of the household from the child's school, based on GPS coordinates. Baseline engagement measures the number of engagement activities done at baseline. Baseline returns measures parental baseline log-returns by completing Secondary High School (SHS).*

Appendix 8. Descriptive statistics by parent schooling

	No schooling		Some schooling		T-test, Difference in Means
	N	Mean/SE	N	Mean/SE	(1)-(2)
Caregiver is household head	1655	0.531	885	0.375	0.156***
		[0.012]		[0.016]	
Caregiver male	1655	0.447	885	0.351	0.095***
		[0.012]		[0.016]	
Caregiver age	1653	44.335	884	40.590	3.744***
		[0.299]		[0.381]	
HH owns TV	1652	0.388	885	0.515	-0.127***
		[0.012]		[0.017]	
Household size	1655	10.102	885	9.108	0.994***
		[0.125]		[0.174]	
Baseline proboy bias	1655	7.680	885	6.718	0.962***
		[0.093]		[0.111]	
Caregiver is decision-maker	1655	2.011	885	1.968	0.043
		[0.024]		[0.033]	
Bought books during school closures	1654	0.426	885	0.638	-0.212***
		[0.012]		[0.016]	
Contacted teacher during school closures	1654	0.336	885	0.415	-0.079***
		[0.012]		[0.017]	
Made children listened to remote instruction	1655	0.224	885	0.341	-0.118***
		[0.010]		[0.016]	
Got private tutor during school closures	1655	0.082	885	0.185	-0.103***
		[0.007]		[0.013]	
GUP sample	1655	0.469	885	0.330	0.140***
		[0.012]		[0.016]	
Received SMS later	1655	0.127	885	0.118	0.010
		[0.008]		[0.011]	
Christian	1592	0.175	826	0.245	-0.069***
		[0.010]		[0.015]	
Muslim	1592	0.716	826	0.609	0.107***
		[0.011]		[0.017]	
Household distance from children's schools	1519	6.831	814	7.554	-0.723
		[0.498]		[0.715]	

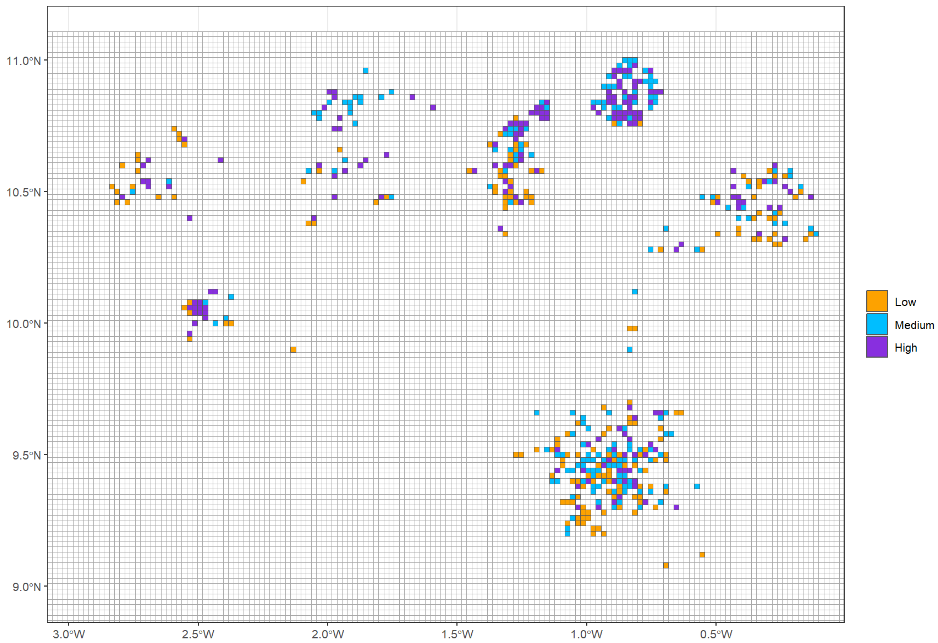
*Notes: *** p<0.01; **p<0.05; * p<0.1. This table presents differences in means in variables by caregiver education. Most variables were collected at baseline, except for household distance from* *school, which is calculated based on administrative data collected in 2023 and household location in 2021.*

Appendix 9. Spillovers

To construct the spillover measures, household survey data including GPS coordinates of households were layed over a map of Ghana, containing grid cells. With the package *rnaturalearth* in R, household geolocations were converted into an sf object, and created the grid (each cell of 2km or 0.02 degrees). For each cell of the grid, a new variable related to the proportion of treated households over total survey households in that specific grid. The same was done for the proportion of treated *and* schooled households within the grid. These proportions were then converted into terciles. We also created two additional variables related to the number of treated households, and the number of treated and schooled households in each raster to use as additional controls in the model (as some rasters may only have one or a few households, while others may have more).

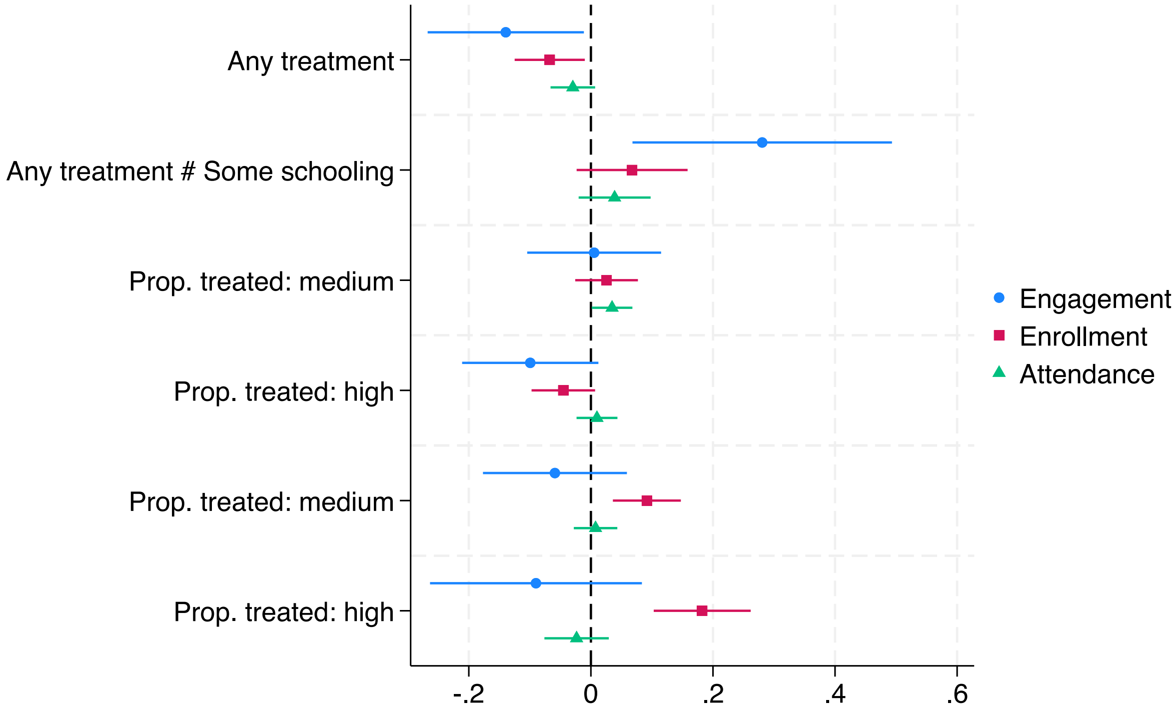
As it is evident from Figure A9.1, variation in the proportion of treated and schooled parents across grids is high. This is because the randomization was household-level, so the distribution of treated and treated and schooled households across grids was aleatory.

Figure A9.1 Proportion of treated and schooled parent households across grids



Notes: these figures show the distribution of treatment and treatment and schooled households across the grids in which households are located. Each grid (2x2 km or 0.2 degrees) is assigned a tercile measuring the proportion of households within each category compared to the whole distribution of proportions.

Figure A9.2 Plotted treatment effects of augmented model



Notes: This figure plots coefficients from our baseline model, augmented by the terciles of proportion of treated households within each 2-km grid, and the proportion of treatment and schooled households within the grid. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, a binary variable =1 if implementation start was delayed, number of treated household within each 2-km grid, and number of treated and schooled households in each grid. Standard errors are clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education (parent-reported). The scale was aggregated through a maximum likelihood factor score that included all the items administered, which was then netted of interviewer effects and standardized over the control. Enrolment and attendance measures rely on school records. Enrolment is a binary variable =1 if the child is currently enrolled in school. Attendance measures the proportion of days in which the child was present over total days the school was open (school records).

Appendix 10. Treatment effects on parent and child mechanisms

Table A10.1 Effects on parent-level mechanisms

	(1) Self-efficacy	(2) Pro-boy bias	(3) Distress	(4) Aspirations	(5) Attitudes on attendance
Any nudge	-0.106*	0.145**	0.055	-0.105**	-0.279**
	[-0.231 - 0.020]	[0.017 - 0.273]	[-0.065 - 0.175]	[-0.196 - - 0.014]	[-0.536 - -0.022]
Any nudge * some schooling	-0.051	-0.188*	0.135	0.026	0.529**
	[-0.258 - 0.155]	[-0.398 - 0.023]	[-0.081 - 0.350]	[-0.131 - 0.184]	[0.089 - 0.969]
Caregiver ever went to school	0.234**	-0.074	-0.300***	0.077	-0.281
	[0.048 - 0.420]	[-0.262 - 0.113]	[-0.494 - - 0.106]	[-0.064 - 0.219]	[-0.674 - 0.112]
Observations	2,443	2,420	2,255	4,472	4,711
Mean control no education	-0.0863	0.0135	0.0944	2.747	23.23
Unadj. pvalue Nudge	0.0986	0.0266	0.366	0.0238	0.0331
Unadj. pvalue Nudge*Schooling	0.627	0.0801	0.221	0.743	0.0184
RW pvalue Nudge	0.0729	0.0150	0.449	0.00500	0.00599
RW pvalue Nudge*Schooling	0.507	0.0689	0.220	0.653	0.00500

*Notes: Standard errors clustered at the caregiver level in brackets, *** p<0.01, ** p<0.05, * p<0.1 Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementations start was delayed for the household. Parental self-efficacy for education was measured through an eight-item scale using a Likert scale for each item (Bandura et al., 2001). Parental psychological distress was measured through the Kessler Psychological Distress Scale (Kessler et al, 2002), a 10-item questionnaire. Pro-boy bias was measured through the "Gender norms and attitudes scale" (Waszak et al, 2001). Parents also reported their educational aspirations for both focal children, as assessed in the Young Lives survey (Favara 2019). These are measured through a four-category measure, ranging from 1 (Senior High School or less) to 4 (graduate). Parents' attitudes towards school attendance were measured through a eight-item scale asking questions like "When my child misses school, they miss valuable instruction" (Dalzien and Hentorne, 2005). All these variables, except for aspirations, were first netted out of interviewer effects, then aggregated through ML factor scores, and, finally, standardized over the control mean and SD.*

Table A10.2 Effects on child-level time use

	(1) School	(2) Study	(3) Housework	(4) Work	(5) Leisure
Any nudge	0.163	-0.063	-0.172	-0.087	-0.047
	[-0.162 - 0.488]	[-0.227 - 0.100]	[-0.423 - 0.079]	[-0.341 - 0.166]	[-0.214 - 0.120]
Any nudge * some schooling	-0.181	-0.019	0.051	0.183	-0.051
	[-0.757 - 0.394]	[-0.263 - 0.226]	[-0.338 - 0.439]	[-0.211 - 0.578]	[-0.334 - 0.232]
Caregiver ever went to school	0.170	0.148	-0.183	-0.484***	0.245*
	[-0.347 - 0.687]	[-0.070 - 0.366]	[-0.529 - 0.164]	[-0.836 - - 0.133]	[-0.008 - 0.499]
Observations	2,163	2,166	2,006	2,039	2,155
Mean control no education	5.863	1.618	3.411	2.274	4.347
Unadj. pvalue Nudge	0.324	0.448	0.180	0.499	0.584
Unadj. pvalue Nudge*Schooling	0.536	0.880	0.798	0.362	0.724
RW pvalue Nudge	0.749	0.749	0.749	0.749	0.905
RW pvalue Nudge*Schooling	0.905	0.905	0.905	0.905	0.907

*Notes: Standard errors clustered at the caregiver level in brackets, *** p<0.01, ** p<0.05, * p<0.1 Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementations start was delayed for the household. Child time use was measured through questions administered to children aged 10-17 years, asking them about the hours spent in the previous day (or in a normal day of the week, if the previous day was over the weekend or a holiday) in different activities, including: time spent at school, studying, doing housework or caring for other household members, at work in the family farm or business; and in leisure activities.*

Appendix 11. Heterogeneity by treatment type and duration, and parent schooling

Table A11.1 Effects by treatment framing type and parent education

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent	Child	Parent	Child	School	Parent	Child	School
EDU+	-0.137**	-0.128*	-0.019	-0.048**	-0.043	-0.028**	-0.022	-0.037**
	[-0.252 - -0.021]	[-0.264 - 0.008]	[-0.050 - 0.012]	[-0.089 - -0.008]	[-0.096 - 0.011]	[-0.055 - -0.000]	[-0.061 - 0.017]	[-0.071 - -0.003]
Gender boost	-0.126**	-0.085	-0.027*	-0.007	-0.037	-0.025*	-0.042**	-0.024
	[-0.244 - -0.007]	[-0.218 - 0.048]	[-0.058 - 0.005]	[-0.047 - 0.033]	[-0.090 - 0.016]	[-0.052 - 0.002]	[-0.082 - -0.003]	[-0.057 - 0.009]
EDU+*Some schooling	0.240**	0.213*	0.042**	0.075**	0.046	0.049**	-0.016	0.034
	[0.047 - 0.432]	[-0.028 - 0.453]	[0.001 - 0.083]	[0.015 - 0.135]	[-0.039 - 0.131]	[0.011 - 0.086]	[-0.083 - 0.050]	[-0.020 - 0.089]
Gender*Some schooling	0.247**	0.168	0.022	0.027	0.049	0.016	0.022	0.051*
	[0.052 - 0.443]	[-0.076 - 0.412]	[-0.021 - 0.066]	[-0.031 - 0.086]	[-0.037 - 0.134]	[-0.024 - 0.055]	[-0.045 - 0.088]	[-0.002 - 0.104]
Observations	4,668	2,099	4,720	2,934	4,137	4,720	2,625	2,923
EDU+=Gender boost, pvalue	0.83	0.46	0.58	0.02	0.81	0.43	0.28	0.47
EDU*Schooling+=Gender*Schooling, pvalue	0.92	0.63	0.29	0.05	0.94	0.1	0.21	0.4

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. EDU+ relates to the standard program framing, while Gender boost relates to the framing emphasizing gender parity in education. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. Enrolment is a binary variable =1 if the child is currently enrolled in school (parent- and child-reports) and by trimester (school records). Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, if they report enrolment) and the proportion of days in which the child was present over total days the school was open (school records). Children's reports are for children aged 10-17 years. Parent schooling =1 if the parent has ever been in formal education.*

Table A11.2 Effects by treatment exposure duration and parent education

	Panel A: Engagement		Panel B: Enrolment			Panel C: Attendance		
	Parent	**Child**	**Parent**	**Child**	**School**	**Parent**	**Child**	**School**
Short (12 weeks)	-0.146**	-0.094	-0.030*	-0.032	-0.047*	-0.034**	-0.034*	-0.026
	[-0.264 - -0.027]	[-0.228 - 0.041]	[-0.062 - 0.001]	[-0.072 - 0.009]	[-0.100 - 0.006]	[-0.062 - -0.007]	[-0.073 - 0.006]	[-0.060 - 0.009]
Long (24 weeks)	-0.118**	-0.118*	-0.016	-0.025	-0.033	-0.019	-0.030	-0.035**
	[-0.233 - -0.002]	[-0.252 - 0.016]	[-0.047 - 0.016]	[-0.065 - 0.015]	[-0.086 - 0.021]	[-0.046 - 0.008]	[-0.069 - 0.008]	[-0.067 - -0.002]
Short * Some schooling	0.220**	0.119	0.030	0.063**	0.072*	0.041**	-0.006	0.030
	[0.026 - 0.414]	[-0.126 - 0.364]	[-0.013 - 0.073]	[0.004 - 0.122]	[-0.012 - 0.156]	[0.002 - 0.080]	[-0.072 - 0.061]	[-0.023 - 0.084]
Long * Some schooling	0.269***	0.264**	0.034	0.040	0.023	0.023	0.012	0.056**
	[0.075 - 0.463]	[0.025 - 0.504]	[-0.008 - 0.075]	[-0.019 - 0.100]	[-0.065 - 0.110]	[-0.015 - 0.061]	[-0.055 - 0.078]	[0.002 - 0.110]
Observations	4,668	2,099	4,720	2,934	4,137	4,720	2,625	2,923
Short = Long, p-value	0.58	0.68	0.28	0.69	0.53	0.95	0.87	0.36
Short*Schooling = Long*Schooling, p-value	0.55	0.12	0.85	0.35	0.18	0.25	0.57	0.18

*Notes: Confidence intervals in brackets. *** p<0.01; **p<0.05; * p<0.1 based on unadjusted p-values. Short and Long are two binary variables =1 for 12- and 24-week treatment exposure, respectively. Estimates control for region fixed effects, a binary variable =1 for whether the household was drawn from the GUP Survey, and a binary variable =1 if implementation start was delayed for the household. Standard errors are clustered at the caregiver level. Engagement measures the number of activities parents or other household members undertook to support child education. Enrolment is a binary* *variable =1 if the child is currently enrolled in school (parent- and child-reports) and by trimester (school records). Attendance measures the proportion of days of school attended in the previous week (parent- and child- reports, if they report enrolment) and the proportion of days in which the child was present over total days the school was open (school records). Children's reports are for children aged 10-17 years. Parent schooling =1 if the parent has ever been in formal education*

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