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School attendance information or conditional cash transfer? Evidence from a randomized field experiment in rural Bangladesh

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Abstract

Low school attendance remains an important challenge in resource-poor settings with cash and information constraints. We compare conditional cash transfer (CCT) treatments with framing variations (gain and loss) against attendance information treatment as interventions to address these constraints in a unified framework. Our randomized evaluation shows CCT treatments increase attendance by 11 percentage points, about half of which is attributable to attendance information. These treatments improve girls' academic aspirations and reduce early marriage. Daily CCT set at a quarter of local child wage maximizes attendance impact. We highlight the importance of low-cost information technology to boost attendance sustainably and cost-effectively. [100 words]

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Keywords: Attendance Information, Conditional Cash Transfers, Cost-Effectiveness, Secondary Education, Gender, Rural Bangladesh

JEL: D91, H75, I25, O15

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1 Introduction

Low school attendance remains a chronic problem in many countries, despite the massive gain in primary and secondary education enrollment around the world over the past decades. The global net secondary school attendance rate is estimated to be 61 percent on average, and the rate is only 31 percent for students belonging to the lowest wealth quintile UNICEF (2016). Even in a rich country such as the United States, one in six students was reported to be chronically absent—missing at least 15 days of school in 2015-16 (US Department of Education, 2022). Why does school attendance remain low given that secondary education, particularly at the lower level, is free in many countries? Past research has indicated that cash constraints could trigger absenteeism due to out-of-pocket educational expenses and the opportunity cost of schooling, particularly where children are an important source of labor for households' businesses, agricultural activities, and domestic work (Ito and Shonchoy, 2020). Parents may also face information constraints due to inattention and infrequent communication with schools. This constraint is particularly relevant to rural illiterate parents who lack appreciation of the school's operation, absenteeism issues, and importance of regular communication with schools or children. In this study, we relax the cash and information constraints in a unified setting under a randomized field experiment to examine the relative importance of the two constraints in sustainably improving secondary school attendance in a developing country

Existing literature primarily focuses on the cash constraint and opportunity cost problem of households that are typically addressed with conditional cash transfers (CCTs) intervention for school attendance.[1] This type of cash transfer conditionality has been a hugely popular policy instrument—spreading over 60 countries (World Bank, 2018a) with governments engaging as high as 1.2 percent of their GDP on CCT programs (e.g., the Bobo Desarrollo Humano program in Ecuador). Although existing evidence suggests CCTs can be effective in increasing attendance, three important caveats remain. First, CCT programs are costly to implement, infeasible, or often unsustainable in resource-constrained settings, particularly in low- and lower-middle-income countries. Nevertheless, existing literature has paid little attention to the program and implementation costs of CCTs. Second, as we elaborate subsequently, CCT programs typically have an implicit transmission of attendance information to households. However, existing literature

[1]CCTs are designed to encourage schooling and other socially desirable behavior and to suppress intergenerational transmission of poverty by breaking the vicious cycle of low human capital investment and deprivation. CCT programs have various aims and impacts, such as increasing school enrollment (Attanasio et al., 2010); increasing the demand for food (Attanasio and Lechene, 2010); empowering women (Almås et al., 2018); improving health, reducing neonatal and infant mortality (Barber and Gertler, 2010); child marriage (Buchmann et al., 2018); and even deforestation (Jayachandran et al., 2017). Such programs are inspired by the iconic and widely successful 1990s' CCT program in Mexico called "Progresa", which was subsequently renamed as "Oportunidades" and "Prospera" (Parker and Todd, 2017). Educational conditions imposed in CCTs typically include school enrollment and attendance on 80-85 percent of school days (Field and Ambrus, 2008).

has paid little attention to the role of such attendance information, thereby attributing all CCT effects to the relaxation of cash constraints instead of the relaxation of the information constraint. This is an important issue because we may be able to boost attendance without spending substantial resources if information alone can significantly increase attendance. Third, CCT programs are traditionally "gain" framed, by enabling beneficiaries to gain money by performing a desired behavior. However, recent behavioral literature underscores the importance of loss framing in generating higher responsiveness (Fryer Jr et al., 2022). This can be incorporated in the CCT design at no extra cost by adopting "loss" framing such that beneficiaries lose money if they deviate from the required behavior. Hence, the primary research question of this study is: Can school attendance be improved sustainably and cost-effectively through interventions that exploit high-frequency information against typical or alternative framing of CCT transfer?

To answer this question, we conduct an RCT over two years involving 800 secondary school students between grades 6 and 9 in rural Bangladesh. We randomly assign sample students into one of the following four treatment arms—(i) "SMS", in which households receive weekly text messages containing daily school attendance information accompanied with voice calls so that illiterate parents can also understand the information; (ii) "Gain", in which households receive voice and text messages similar to the SMS treatment arm with cash-gain information, typically done in conventional gain framing CCT, where the gain is determined by the number of days children are present in school; (iii) "Loss", in which households receive voice and text messages similar to the SMS treatment arm, plus information on cash transfers under a novel loss-framing CCT, where the loss is determined by the number of days children are absent from school; and (iv) "Control", in which households receive neither voice and text messages nor cash. This randomized design enables us to rigorously compare the efficacy of information and CCT interventions in a unified setting.

We take advantage of low-cost information technologies—voice calls and text messages—to provide parents with information on children's weekly school attendance and updated cash transfer balance. We thus aim to reduce the potential information gap between parents and children while reinforcing the CCT framing in the relevant treatment arms. We also vary the daily transfer amount in the CCT interventions (i.e., Gain and Loss) to explore the relevance of the intensive margin of cash transfers. Our study features unique daily school attendance information (123,500 person-day records), collected from different sources at different times of the day over two years, allowing us to cross-check the validity of the official administrative records. Further, we collect data on friendship networks and household characteristics to account for possible spillover effects and to uncover important impact heterogeneity and potential mechanisms.

We find that the SMS and CCT interventions increase attendance rate by about 5 and 11 percentage points, respectively, from the mean attendance rate of 53 percent in the Control group. Loss-framed CCT improves attendance by the greatest margin, although the impact is not statistically different from the conventional gain-framed CCT. Our estimates highlight that about half of the CCT impact is attributable to information. These findings remain valid across a battery of sensitivity analyses, alternative attendance measures, and econometric specifications. The estimates are robust even after controlling for peer networks.

Further, we find significant gendered effects of our interventions. Girls who have received the SMS or CCT intervention tend to have greater educational aspirations, higher attendance, and a reduction in early marriage. Both treatments generate lasting impacts on girls' post-intervention school attendance and CCT and SMS impacts converge in the year following the intervention period. We also show that girls' parents in the SMS treatment invest significantly more in educational resources. These findings suggest that the information embedded in the CCT and SMS treatments may be a key driver of the post-intervention impacts. These are critical findings for girls' individual and societal well-being, given that higher educational attainment is known to influence the labor market and (intergenerational) health outcomes positively in the long-term (Asadullah, 2006; Currie and Moretti, 2003). We do not find any treatment impact on anthropometric measures (BMI and height), child labor, learning outcomes, parental aspirations, and study hours.[2]

Finally we conduct back-of-the-envelope calculations to compare the cost-effectiveness of CCT and SMS interventions. We argue that both can be cost-effective for boosting attendance in a policy-relevant setting. Nevertheless, their relative cost-effectiveness would depend on how the expenses of cash-reward and schooling information are accounted for, depending on the prevailing policies and available infrastructure for cash transfer and attendance data collection. Exploiting the variation in the daily CCT amount, we further find evidence of a positive and diminishing marginal impact of the daily transfer amount on attendance, indicating the importance of the intensive margins and the potential presence of gains in cost-effectiveness from calibrating the daily transfer amount adequately. Our calibration exercise suggests that a policymaker who wishes to maximize attendance may consider the most cost-effective CCT of approximately 20–22 Bangladeshi taka per day (0.22–0.24 USD), or roughly a quarter of child daily wages in the region. Nevertheless, given that simple information treatment has lasting post-intervention effects, the benefits of high-frequency attendance information may potentially outweigh those of CCT in the long run. These findings would help policymakers, particularly those in developing countries, formulate education interventions to raise school attendance cost-effectively.

[2]Learning outcomes are measured using a mathematics test executed by our implementation partner both at the baseline and the endline.

Literature and Contribution

This paper relates to a large body of literature on CCTs. CCTs have generally been found to be successful at promoting school enrollment across all grades around the developing world (Glewwe and Muralidharan, 2016; Murnane and Ganimian, 2014; Dubois et al., 2012; Glewwe and Kassouf, 2012; Attanasio et al., 2010; Behrman et al., 2009; Fiszbein and Schady, 2009; Schady et al., 2008; Todd and Wolpin, 2006; Shultz, 2004; Khandker et al., 2003). However, most existing studies examine the attendance and enrollment impacts of CCT programs but not the features that would improve their cost-effectiveness. The lack of adequate attention to cost-effectiveness is surprising, particularly given that these programs are typically implemented in developing countries where resources for cash transfers are limited and state capacity to administer such program is weak.

An emerging literature sheds light on the design features of CCT programs, with a particular focus on targeting subgroups with the largest human capital gaps, while selecting appropriate conditions and size of cash transfer (Barrera-Osorio et al., 2011; Field and Ambrus, 2008; de Janvery and Sadoulet, 2006). Moreover, a few studies indicate that monitoring and enforcing the conditionality is essential when targeting school attendance as a policy goal (Baird et al., 2011; De Brauw and Hoddinott, 2011; Martinelli and Parker, 2003). This could be particularly relevant in agrarian areas, where parents may be myopic and value fieldwork or early marriage over education. Therefore, how the conditions in CCT programs are derived and enforced require more attention. We offer a new design angle—loss framing—to increase the cost-effectiveness of the CCT programs. A related but separate design question is whether the information communicated, often implicitly in CCT interventions, could by itself help improve attendance and other education-related outcomes.

Our intervention has several distinctive features. First, we study whether high-frequency (weekly) attendance information to parents increases children's schooling, particularly in an environment where a low-cost solution is essential. Since an SMS or a voice call is inexpensive (costs 0.05 and 0.07 US cents for each text and per minute of call, respectively) and a vast majority of households in the region own mobile phones (predominantly budget feature phones), regular information transmission can bring children to school cost-effectively. We, therefore, contribute to the recent literature on the effects of reducing the parent-child information gap through parent-teacher meetings in France (Avvisati et al., 2013), bi-weekly calls, texts, or emails about children's missed assignments in the US (Bergman, 2021), and weekly text SMS about attendance, grade, and classroom behavior in Chile (Berlinski et al., 2021).[3] Our study differs from these in

[3]Prior literature focused predominantly on developed countries and found that reducing parent-child information gaps through text messages or voice calls tends to improve learning outcomes (Bergman and Chan, 2021; Barrera-Osorio et al., 2020; Bergman et al., 2020; Gallego et al., 2020; Dizon-Ross, 2019; De Walque and Valente, 2018; Rogers and Feller, 2018; Castleman and Page, 2015; Kraft and Rogers, 2015;

terms of the culture, sophistication of widely available technologies, and average household income and educational levels, among others. Further, in addition to the text-based information, we also provide voice calls since many parents in our sample are illiterate. Hence, our study underscores the potential of a low-cost technology (high-frequency information over phone) to raise school attendance in developing countries.

Second, our field experiment enables us to compare the cost-effectiveness of CCTs and SMS in a unified setting. This contrasts with most CCT studies, which ignore the role of information in CCT programs. Even though typical CCT programs do not explicitly or regularly give attendance information, households can often infer their children's attendance from the CCT amounts they receive, possibly with a significant time lag. If this information plays a central role in bringing children to school, the transfer amount would not matter much. On the other hand, if households primarily respond to CCT intervention due to the cash incentives, providing attendance information will not be useful. To determine the relative importance of attendance information in CCTs, we include the SMS group in our study design, which receives attendance information without cash transfers. Our results suggest that both cash and information elements contribute to the impact of CCTs on attendance, and about half of the overall CCT treatment effect can be attributed to the information component.

Third, we examine the persistence of the SMS and CCT effects after the intervention period. Hence, this study relates to the growing body of literature on the longer-term effects of CCTs, documenting the improvement in educational and employment outcomes (see Molina-Millan et al. (2016) for a review). However, there has been little discussion on whether such long-term effects could potentially be driven by the permanent behavioral change induced by information or by the persistent effect of increased attendance due to CCTs. Our results indicate that the former is a likely possibility, because the effects of SMS persist and attendance rates in the CCT and SMS treatment groups converge to about three percentage points above the control group mean—measured one year after the intervention. Furthermore, we find evidence that the persistence of SMS treatment on attendance is not attributable to the expectations of future cash transfers.

Fourth, we experimentally explore the relevance of loss framing to CCT. The use of loss framing is inspired by the widely documented psychological trait of loss aversion, which describes the phenomenon that people tend to react to losses more strongly than gains of the same amount (Kahneman et al., 1990; Kahneman and Tversky, 1979).[4]. Since loss-framed CCTs can be implemented without adding costs to conventional gain-

Kraft and Dougherty, 2013; Bursztyn and Coffman, 2012).

[4]Based on an often-cited figure, pain from a loss is believed to be twice as large as the pleasure from a gain of the same magnitude, even though the external validity of this figure is debatable (Chapman et al., 2018; Fehr-Duda and Epper, 2012). Loss framing has been used to boost incentives in various contexts such as productivity of manufacturing workers in China (Hossain and List, 2012), credit card use in Israel (Ganzach and Karsahi, 1995), and nutritional choice in the US (List and Samek, 2015).

framed CCTs, we can potentially make CCTs more impactful by adopting loss framing without increasing the transfer amount. Despite its general appeal, there are only a limited number of applications of loss framing to education policy.[5] In particular, loss aversion has been applied to incentivize teachers and students in the US, with mixed evidence on the effectiveness of loss framing (Fryer Jr et al., 2022; Levitt et al., 2016). To the best of our knowledge, this is the first study applying loss framing to CCTs to boost attendance in a developing country context.

Fifth, aside from the innovative loss framing, our CCT design further distinguishes itself from the existing ones. While most CCT programs impose a threshold on attendance (i.e., only one transfer level per month or school term), our cash transfer design is strictly linear in attendance (i.e., the transfer amount is proportional to the number of school days attended). Thus, our conditions for cash transfer incentivize households to send children to school on *every* intervention day, which eliminates the possibility of threshold effects where students stop attending school once they meet the minimum number of days or once they miss too many days of cash incentive.

Sixth, we vary the transfer amount to understand the relevance of intensive margins in the CCT programs. As far as we know, Filmer and Schady (2011) is the only study that rigorously explores the relevance of intensive margins with two transfer levels. They find that a larger transfer does not raise attendance beyond the level attained by a modest transfer. Using three transfer levels, our study elucidates how the marginal effects on attendance vary with the daily transfer amount. This is an important exercise that contributes to our subsequent cost-effectiveness calculations. In particular, the current literature provides little insight, if any, into how to do such calibration to have a cost-effective policy. As with other incentive programs, a CCT program with a given transfer amount does not change the behavior of always-takers (i.e., households that would send children to school regardless of the availability of CCTs) and never-takers (i.e., households that would not send children to school even in the presence of CCTs). Obviously, never-takers may become compliers (i.e., households that would send children to school if and only if CCTs are available) when the reward for school attendance is increased. Hence, it is important to calibrate the most cost-effective amount of cash transfer to strike a balance between the increased attendance from compliers and leakage of resources towards always-takers at the intensive margin.

Seventh, we collect detailed attendance information during our intervention period from several sources at different times of the day. Specifically, in addition to the official morning attendance record maintained by the school teachers, we collect attendance information from hired class representatives in the afternoon and from our enumerators during unannounced random school visits. With these data, we can address the poten-

[5] A few studies applied other psychological tools (e.g., the appeal of financial versus non-financial rewards) to design education incentives (Koch et al., 2015; Lavecchia et al., 2012; Jabbar, 2011).

tial concerns that teachers may intentionally mark absent students as present, since the morning attendance record is utilized for the CCTs and SMS interventions.[6] Even if teachers keep correct attendance records, students may immediately return home once their morning attendance is taken, which would lead to overestimation of attendance effects. It has indeed been reported that many students in Bangladesh leave school after the lunch break (Tuhin, 2018; The Daily Star, 2015). Hence, morning attendance alone would not be sufficient to capture the granular attendance behavior, which is typically ignored in the existing CCT literature. As elaborated subsequently, using both morning and afternoon attendance records, we find that both CCTs and SMS effectively bring children to school while keeping them there during regular school operating hours.

Finally, our paper contributes to the literature on CCT impact on several downstream outcomes such as child labor (Cepaluni et al., 2022; Peruffo and Ferreira, 2017; Del Carpio et al., 2016; Duryea and Morrison, 2004), early marriage (Millán et al., 2020; Barham et al., 2018; Cahyadi et al., 2018), educational aspirations (Hartarto et al., 2021; Contreras Suarez and Cameron, 2020; García et al., 2019), health outcomes (Shei et al., 2014; Glassman et al., 2013; Gaarder et al., 2010; Leroy et al., 2009), learning outcomes (McEwan, 2015; Fiszbein and Schady, 2009), and parental investment in education (Majid, 2018; Aber et al., 2016; Sinha and Yoong, 2009).

Altogether, this study assesses the efficacy of alternative interventions such as high-frequency information or loss-framing or variation of the daily CCT in boosting attendance among lower-secondary school students in rural Bangladesh. The research investigation offers a new set of insights that are valuable for policymakers to design interventions to increase school attendance cost-effectively in a low income setting. The rest of the paper is organized as follows. Section 2 describes the field experiment, and Section 3 describes the data. The effects of CCTs and SMSes on daily attendance during the intervention period as well as persistence of the treatment effects after the intervention are presented in Section 4. We elucidate some important treatment heterogeneity by gender in Section 5. We analyze the cost-effectiveness of our interventions in Section 6 and conclude in Section 7.

2 Experimental Setting and Design

Our field experiment was executed from 2017 to 2018 in Gaibandha, a rural district in northern Bangladesh. The district is predominantly agricultural with 71 percent of the

[6]Teachers could plausibly fake attendance for various reasons. For example, personal relationship between students and teachers could trigger fake attendance for poor students out of sympathy. Another example is corruption. Teachers may demand kickback of cash transfers from absent students, or students may pay teachers to fake attendance.

working population working in the agricultural sector (World Bank, 2020). Gaibandha is also a disaster-prone district that suffers from regular flooding and riverbank erosion. The poverty rate in Gaibandha is estimated at 46.7 percent, far exceeding the national average of 24.3 percent (Bangladesh Bureau of Statistics and World Food Programme, 2020). Moreover, the district's adult literacy rate is only 38 percent, which is substantially lower than the national average of 51 percent, placing Gaibandha at the 57th position out of the 64 districts in the country. The school attendance rate in the district is also low and exhibits a significant gender gap: 56.5 percent for boys and 49.0 percent for girls in 2011 (Bangladesh Bureau of Statistics, 2013).[7] Given this background, it is imperative that we understand school attendance behavior and devise effective interventions to bring children, especially girls, to school.

Since most countries in the developing world, including Bangladesh, have achieved universal primary education—envisaged in the Millennium Development Goals (MDGs)—education policies have gradually shifted their foci to quality education and enrollment rates at the secondary and higher levels over the past decade, as is evident from the Sustainable Development Goals, a successor of MDGs. In Bangladesh and many other developing countries, secondary education is of critical importance today, not only because secondary education would be a key enabler for the youth to take advantage of new job opportunities being created but also because it may help reduce the incidence of child labor (Yildirim et al., 2015), early marriage, and pregnancy (Cohen, 2014; Raj et al., 2014).

Against this backdrop, we designed an intervention targeted at students between grades 6 and 9 from three lower secondary schools.[8] To recruit students, we first obtained school headmasters' consent to participate in this study and then obtained the academic calendars and student roster of target grades from these schools. Due to logistical constraints and feasibility of the intervention, we restricted our student sample from the roster to those with a valid (i.e., currently active) mobile phone number in the household and residing in one of the three catchment unions.[9] Note that the valid phone number requirement is not so restrictive because around 90 percent of the parents had a functional phone in the initial roster. Further, when we saw that more than one eligible

[7] According to the World Development Indicators, the gross enrollment ratio in secondary school was 69.2 percent in 2017 (75.2 percent for females and 64.3 percent for males) (World Bank, 2021).

[8] In Bangladesh, lower secondary education can be subdivided into junior secondary (grades 6–8) and secondary (grades 9–10). The selection of schools was made as follows: Enumerators visited ten lower secondary schools in Gaibandha to collect some basic school level information such as number of students and reasons for school dropout. School sizes range from 305 to 995 students in total and schools' headmasters stated economic crisis as one of the main reasons for school dropouts, followed by early marriage and poor parental awareness. The research team subsequently selected three schools to recruit based on target sample size, and capturing a moderate mix of school sizes (437 to 870 students). It should also be noted that there were some nationwide educational programs such as the Female Secondary Stipend and Assistance Program (Khandker et al., 2021) during our intervention period. However, there is no reason to believe that these programs would have a differential impact across our treatment arms.

[9] Unions are the lowest administrative unit and consist of wards.

student was listed from the same household, we randomly kept only one of them on the roster for the RCT. We then drew a random sample from the restricted roster, stratified by the student's gender, school, and grade. We thus recruited a total of 400 students from grades 6 and 7 at the start of 2017 ("old cohort") and an additional 400 students from grades 8 and 9 at the start of 2018 ("new cohort").[10] We selected these grades to target students at risk of missing or dropping out of school, for example, due to child labor or early marriage. All parents and students consented to participate in the study, when they were approached. The distribution of the final sample by grade, gender, and cohort is reported in Table A1 of Appendix D.[11]

We conducted a detailed baseline survey for each household from each cohort immediately after the sample recruitment was complete. After that, each study participant was randomly assigned to one of the four treatment arms—Gain, Loss, SMS, and Control, which will be detailed below. The treatment assignment remained the same throughout the study for a given participant. After the randomization, we announced the treatment assignment and started our intervention, which was conducted in four phases: two phases in 2017 (2017-I and 2017-II) and another two in 2018 (2018-I and 2018-II). Therefore, the intervention lasted for two years for the old cohort and one year for the new cohort. The number of intervention days N, on which we counted school attendance for our SMS and CCT interventions, was predetermined in each phase. It was 60 days in 2017-I and 50 days in the other three phases. We reduced the intervention days after 2017-I to cope with administrative delays in finalizing the student rosters and unanticipated school closures due to floods and teacher strikes (The Daily Star, 2018; FAO, 2017). When unexpected delays or school closures occurred, we adjusted the intervention period adequately. This was feasible because we did not fix the specific start or end date at the beginning of the school year. Instead, a few days prior to the start of each phase, all households except for those in the control group were informed of the start date and the number of intervention days in the phase.[12]

One of the primary research objectives is to examine the efficacy of CCT treatments with gain and loss framing and SMS. Therefore, including the pure control arm, we have the following four treatment arms:

Gain: Households receive CCTs with gain framing. That is, households are told that

[10]In Bangladesh, school years coincide with calendar years for our target grades. The staggered recruitment design was adopted primarily due to funding constraints. As we obtained more funding, we expanded the target grades.

[11]There exist some irregularities in the 2018 recruitment process due to human errors such as spelling mistakes. First, one student in the old cohort was mistakenly re-listed in the new cohort roster and was dropped from the new sample. Second, there were ten households with more than one participating child. Our main findings remain robust after dropping these ten households from all estimations.

[12]In one school, the intervention days for 2017-II had to be reduced by five days because of unanticipated last-minute school closures (classes from our target lower grades were utilized as exam venues for higher grades at the end of the school year without any prior notice). These days were treated as attended for cash transfer payment, but they were removed from the analysis.

they have an initial balance of 0 taka in a given phase, will receive T taka for each day the student attends school, and may gain up to NT taka in a given phase. Households receive weekly information on school attendance and updates on the cash transfer balance through SMS and scripted voice calls.

Loss: Households receive CCTs with loss framing. That is, households are told that they have an initial balance of NT taka in a given phase, but will lose T taka for each day that the student misses school, and may lose up to NT taka in a given phase. Households receive weekly information on school attendance and updates on the cash transfer balance through SMS and scripted voice calls.

SMS: Households receive information on school attendance through weekly SMS and scripted voice calls.

Control: Households receive neither SMSes or voice calls nor CCTs.

As the above shows, both CCT groups—Gain and Loss—received the same daily transfer amount of T taka and the maximum possible transfer amount was NT in a given phase. Further, because the cash was disbursed after the end of each phase, the only difference between the Gain and Loss groups lies in the framing. That is, for a given attendance record in a given phase, households in the Gain and Loss groups received the same amount of transfer at the same time. Note that neither the students, parents, headmasters nor the teachers were informed of the existence of different gain/loss framings, even though study participants were informed that they have a 75 percent chance of receiving SMS and voice calls or a 50 percent chance of receiving cash transfers when we conducted the baseline survey.

The daily cash transfer amount varied across phases. We set T to be 10 taka (≈ 0.12 USD) in 2017-I and 2017-II. This amount roughly corresponds to the average hourly wage for children aged 5-17 in Gaibandha (Islam et al., 2009).[13] In 2018-I, T was increased to 20 taka per day. In 2018-II, we introduced a 'High' [H] CCT subtreatment, which raised the transfer amount to 30 taka per day. Half of the households in each of the Gain and Loss groups was randomly allocated to the H-subtreatment. The remaining half was assigned to the 'Low' [L] subtreatment and continued to receive 20 taka per day.

[13] Islam et al. (2009) surveyed 1,157 child laborers in Gaibandha and reported grouped and top-coded data on their monthly salary and working hour per day. We use the multimodal generalized beta estimator (MGBE)—which allow us to calculate the mean (and other statistics) robustly from grouped and top-coded data—to estimate the average salary among those who were reporting positive salary and the average hours worked, which are respectively 1,035 taka per month and 8.9 hours per day. Assuming 20 workdays, we obtain a back-of-envelope estimate of the district nominal wage rate of 5.81(=1,035/8.9/20) taka per hour in 2009. Inflating this figure by the ratio of Consumer Price Index in the World Development Indicators between 2009 and 2017, we have 10.1 taka per hour. The results are similar when we alternatively use the robust Pareto midpoint estimator (RPME). See von Hippel et al. (2016) for the details of the MGBE and RPME.

The subtreatment assignment was made and announced after 2018-I and before 2018-II. The cash to be transferred to CCT households was calculated based on the school days attended during the intervention days and disbursed at the end of each phase. Figure 1 summarizes the timeline of the study.

One of the key features of our interventions is the framing of the weekly SMS sent to the parents. To ensure that the three treatment groups receiving SMS are comparable, we ensured that Gain, Loss, and SMS groups receive the same message, except for certain details specific to each group. Namely, we sent the following text messages at the start of each phase:

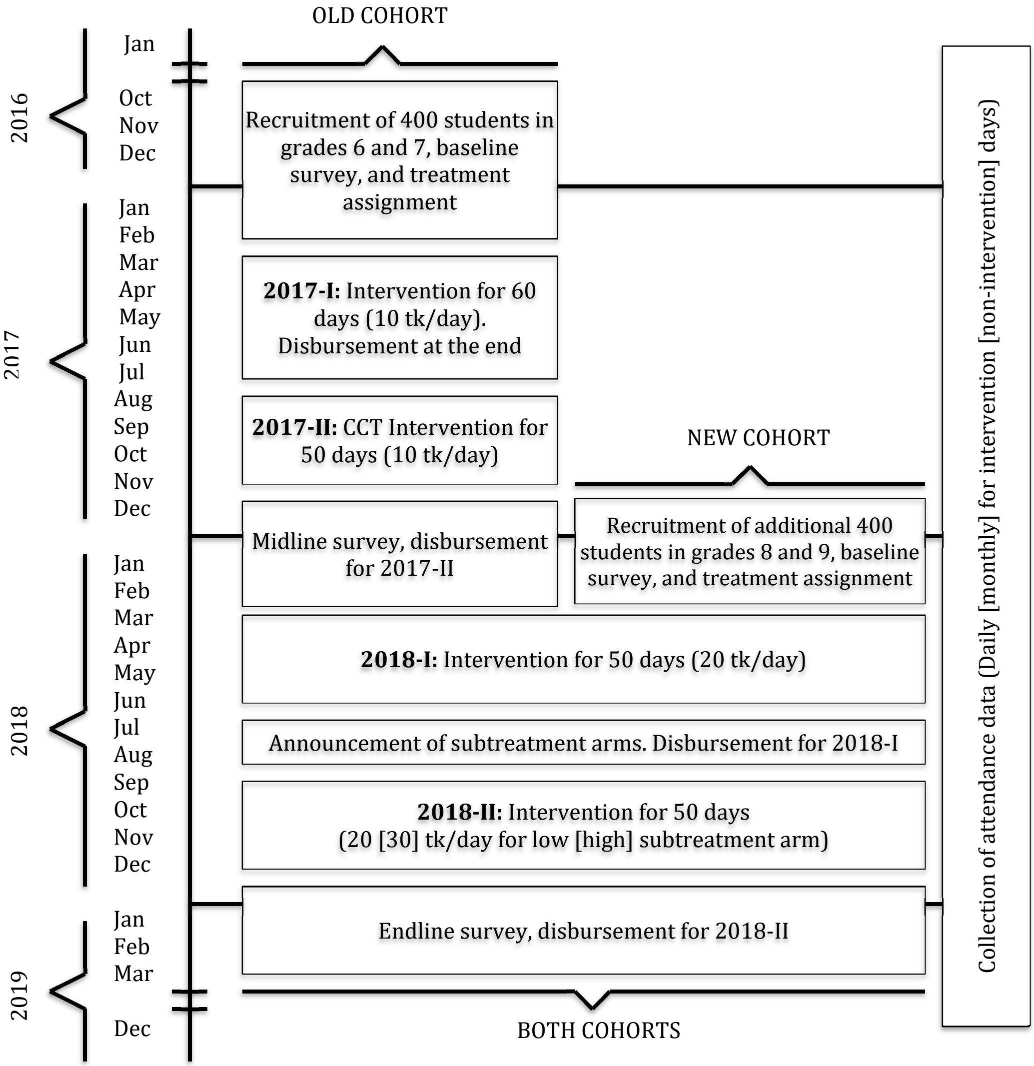
Gain: We are pleased to inform you that you will gain cash transfers and will receive weekly SMS for your child's school attendance. Your current cash transfer balance is 0 taka. You will gain T taka for each school day that your child is recorded present during the study period. The attendance recording period will start from *PhaseStart* and last for N school days so that you may receive possibly up to TN taka for school attendance. Payment of any cash transfer balance will be made after *Disbursement*.

Loss: We are pleased to inform you that you have been awarded a cash transfer balance of TN taka and will receive weekly SMS for your child's school attendance. Your current cash transfer balance is TN taka. You will lose T taka for each school day that your child is recorded absent during the study period. The attendance recording period will start from *PhaseStart* and last for N school days so that you may lose up to TN taka for school absence. Payment of any cash transfer balance will be made after *Disbursement*.

SMS: We are pleased to inform you that you will receive weekly SMS for your child's school attendance. The attendance recording period will start from *PhaseStart* and last for N school days.

In the text messages above, *PhaseStart* refers to the date when the intervention begins and *Disbursement* refers to the approximate timing of disbursement, which occurred shortly after N intervention days, or after a midline or endline survey discussed in Section 3. The key difference between the Gain and Loss treatments lies in the framing of how the cash balances change with attendance and absence respectively. The balance starts from zero in the Gain treatment and increases as the child attends school up to NT taka. On the other hand, the balance for the Loss group starts from NT taka and decreases as the child misses school up to zero taka. For the purpose of (i) providing information on attendance and (ii) reinforcing the CCT framing to make the changes in balance salient to households, the following weekly SMSes were sent during the intervention period:

Figure 1. Timeline of the Study



Note: Daily attendance data are collected during the intervention days. Monthly attendance data are collected from the study schools between 2016 and 2019 outside the intervention days, which include (i) pre-intervention days in 2016 for the old cohort (grade 7 in 2017) and 2017 for the new cohort (grades 8 or 9 in 2018), (ii) non-intervention days in January, February, June, and December, 2017 and in May, June, July, and December, 2018, and (iii) post-intervention days in 2019. The daily transfer amount in parentheses in each intervention phase is applicable only to the Gain and Loss treatment groups.

Gain: Last week, your child has attended D_a school days and missed D_m school days. You have gained TD_a taka for D_a school days attended. Your current cash transfer balance has increased to B taka.

Loss: Last week, your child has attended D_a school days and missed D_m school days. You have lost TD_m taka for D_m school days missed. Your current cash transfer balance has decreased to B taka.

SMS: Last week, your child has attended D_a school days and missed D_m school days.

In the text messages above, D_a and D_m refer to the days the child attended and missed school, respectively, over the last reporting week. B is the updated cumulative balance at the end of the week. The implementation of weekly SMS went on smoothly for the major part of the intervention though there were some lapses in early 2018-I. A subsequent audit revealed that some texts were not sent in certain weeks, and there were also some errors in weekly attendance and transfer amount information in SMSes sent in that phase. Nevertheless, these error rates were minimal (around 2 and 4 percent, respectively), and did not systematically differ across the three treatment groups. Therefore, our impact estimates are unlikely to be significantly affected by these lapses and, if anything, likely to be slightly attenuated. Further discussions are provided in Appendix A.

As noted above, cash transfers were disbursed at the end of each phase, and disbursement was made by visiting the households. We decided to disburse cash in this way for three reasons. First and foremost, disbursing the transfer once or twice a year would mimic the real-world application of CCTs, regardless of the use of gain or loss framing. Second, transferring cash daily or weekly to households is prohibitively costly. The administrative and transaction cost of trips will become substantial, relative to the transfer amount, when transfers are made frequently (see related discussions in section 6). Transferring the CCT amount using a bank account is not practical in our context as a sizable proportion of households is unbanked.[14] Third, it is infeasible and arguably unethical to give the full amount to the Loss group (i.e., NT taka) at the beginning of the phase and then ask them to return a portion of the amount for each school-day missed. Finally, by making the timing of the disbursement the same, we can exclusively focus on the effect of framing, which was regularly reinforced by the weekly SMS. It should be noted that just showing rewards (rather than letting participants take the rewards home or spend

[14] According to the Global Findex Database, around 50 percent of the rural population aged 15 or older in Bangladesh reported to have a single or joint account at a bank or other financial institution (World Bank, 2018b). Until recently, the payments of educational CCT programs in Bangladesh were transferred through to headmasters, who distributed them to students. This saves the transaction cost for individual students (and households) but creates a higher risk of leakage, where the headmaster misappropriates the transfers. To tackle the leakage, the stipend in secondary schools has recently been digitized in Bangladesh (New Age Bangladesh, 2022).

them) is common in experiments that seek to create an endowment effect and exploit loss aversion (Fryer Jr et al., 2022; Kahneman et al., 1990).

3 Data

Data Sources

Accurate school attendance information is critical for making our interventions relevant and analysis meaningful. Therefore, we used three different sources to obtain attendance data. Our first and primary data source for attendance is the official record of student presence, taken by school teachers at the beginning of each morning class/session. We digitized this official attendance record from the school administration book for each intervention day. In addition, we also collected official monthly attendance records for study participants from 2016 (pre-intervention) to 2019 (post-intervention), including the non-intervention days in 2017 and 2018. Pre-intervention attendance data (for the year 2016 [2017] for the old [new] cohort) enables us to control for unobservable individual-specific time-invariant effects. In contrast, post-intervention attendance data for the year 2019 allows us to capture the potential persistence effect of our intervention.[15]

The second source of attendance is daily afternoon attendance, which was independently collected by class representatives. Afternoon attendance data allows us to examine whether each student continued schooling since morning on a given day. The third source of attendance data is unannounced random school visits, which took place about eight times each year. Since the visits were made by field officers who have no personal relationship with the students, they are least susceptible to arbitrary manipulation. Attendance data collected from multiple sources taken at different points in a day thus enables us to cross-validate attendance records and capture granular attendance behavior, including coming to school late and leaving school early.[16] Such granular school attendance behavior is not captured in the existing literature to the best of our knowledge.

Our raw data suggest that concerns about misreporting or partial attendance are limited. As shown in Table I, there is a strong positive correlation between morning and afternoon attendance from 123,500 person-day records. For nearly 90 percent of the person-day data, morning attendance matches with afternoon attendance. The off-diagonal elements in Table I indicate that the odds of students leaving school early before

[15] Because of the limited budget for data entry and data availability in 2016, we chose to collect monthly, instead of daily attendance data for non-intervention days. For old cohorts who were in grade 6 in 2017, their pre-intervention attendance record in 2016 was unavailable as they were in a primary school. The post-intervention attendance records in 2019 are missing for 210 students, out of which 92 are from the old cohort and 118 from the new, because of school transfers and dropouts.

[16] Participants were told at the baseline that daily attendance information would be collected in the morning and afternoon as well as through random visits. Participants were not informed of which attendance information is used for CCTs.

afternoon attendance are higher than students coming later after morning attendance, which is consistent with our casual field observations.

Table I. Morning and Afternoon Attendance on Intervention Days

	Afternoon Present	Afternoon Absent	Total
Morning Present	52.10	7.35	59.45
Morning Absent	3.07	37.47	40.54
Total	55.17	44.82	100.0

Note: Based on 123,500 person-day observations with 799 unique individuals in the study and 239 unique calendar days. The correlation coefficient for the morning and afternoon attendance is 0.79. Note that the number of unique calendar days is larger than the total number of intervention days in Figure 1 because of the differences in school calendars and unexpected closures in some schools.

Furthermore, the attendance records from unannounced random visits also have a high correlation between morning and afternoon attendance. Based on the 8,876 person-day observations with all the three attendance records, the correlation is the highest at 0.87 between morning and random visit records, followed by the correlation of 0.79 between afternoon and random visit records. The correlation between morning and afternoon records is 0.76, the lowest among these three correlations. This is expected since the random visit records are likely to capture the attendance of some latecomers and early leavers.

Besides detailed attendance data discussed above, we also gather household- and individual-level data through baseline, midline, and endline surveys. For both old and new cohorts, a baseline survey was conducted before the treatment assignment was announced. The endline survey was conducted at the end of the intervention.[17] In addition, we administered a midline survey to the old cohort between the 2017-II and 2018-I phases (see Figure 1). These surveys collected information on a host of variables including demographic and socio-economic characteristics of parents and children such as age, education, and household assets. These surveys also asked each student participant to name five of their closest classmates and included a 15-minute mathematics test covering basic arithmetic and geometry knowledge based on the local curriculum. Both the students and their parents were also asked about academic aspirations in terms of the highest grade that they would like the students to achieve.

In Tables A2 and A3 in Appendix D, we report 16 key observable characteristics—such as parental education, household size, possession of assets, children's height and weight, and baseline test scores—for the old and new cohorts, respectively, disaggregated by the treatment assignment. Since the random treatment assignment was implemented by the research team, there was no concern for arbitrary manipulation. Nonetheless, it

[17] 16 households could not be reached for the endline survey due to migration.

is possible that some covariates may be unbalanced across different treatment arms by chance. To address this concern, we conducted a pairwise t-test of equality of means for each of the 16 variables separately for the old and new cohorts. While the proportion of households with agricultural land and with a television or radio at home for the SMS group was significantly higher than that for the Gain group in the new cohort, there was no significant difference in all other comparisons. Further, the null hypothesis of the joint orthogonality test for each covariate could not be rejected at conventional significance levels, except for the tests involving these two covariates. Given that there is a potential concern that the unbalanced covariates will drive our results, we control for the two covariates in all our regression analyses.

In addition to the baseline, midline, and endline survey data discussed above, we also carried out short disbursement surveys for the Gain and Loss groups at the end of every phase during the household visits and before the cash disbursement. These disbursement surveys were integrated into the midline survey in 2017-II and the endline survey in 2018-II. For other phases, these surveys were conducted as a standalone survey. The disbursement surveys contained questions on the understanding of the CCT intervention, the recollection of the amount they were supposed to receive, and whether they kept a record of the last SMS sent to them. Once the survey was done, enumerators proceeded to disburse the cash and asked households how they planned to utilize the cash they received from the study (such as for education purposes or household consumption). In the endline survey, households were also asked to tell retrospectively how the disbursed cash was actually spent.

4 Did the Intervention Improve School Attendance?

Main Empirical Model

In this section, we discuss the impacts of our intervention on school attendance. Our baseline econometric specification is as follows:

$$\mathrm{Y}_{ict} = \beta_0 + \beta_1 \mathrm{Gain}_{ic} + \beta_2 \mathrm{Loss}_{ic} + \beta_3 \mathrm{SMS}_{ic} + \theta' X_{ic} + u_c + v_t + \epsilon_{ict}, \tag{1}$$

where Y_{ict} is an attendance indicator that takes unity if individual i from class c is present in school on date t, and zero otherwise, where a "class" is determined by the combination of cohort, school, and grade. Our primary outcome of interest is morning attendance. However, we also analyze afternoon attendance and "morning and afternoon attendance", the latter of which takes unity if and only if the student was present both in the morning and afternoon on a given day, and zero otherwise. We also use attendance at unannounced random visits as an outcome of interest. Our primary coefficients of interest are β_1, β_2, and β_3. Since the ownership of agricultural land and possession of television or radio at the baseline are unbalanced across treatment arms for the new cohort, we additionally include these variables as X_{ic} in model (1). However, the omission of these covariates does not change the results qualitatively. We also include class-specific and date-specific fixed-effects, denoted by u_c and v_t, to control for any unobserved heterogeneity across classes and different calendar dates, respectively.[18] While students in a class may be in different sections, we regard a class to be an important unit because students from different sections in the same class may take or might have previously taken lessons together. We also add an idiosyncratic error term ϵ_{ict}, which is clustered at the individual level to allow for correlation over time for a given individual.[19]

Main Results

Table II reports the effects of the intervention on daily attendance using different measures of attendance: morning attendance in Column (1), afternoon attendance in Column (2), morning and afternoon attendance in Column (3), and attendance taken during random visits in Column (4). The Gain and Loss treatments increase school attendance by 9.2 to 12.3 and by 11.2 to 13.1 percentage points, respectively. The SMS treatment increases attendance by 4.8 to 6.9 percentage points. All effects are statistically significant at conventional level.

The estimates in Table II are quantitatively similar across different measures of attendance, which is reassuring and mitigates concerns about students leaving school right after morning attendance has been taken. Indeed, if this were true, then the estimated impacts of the CCT treatments on afternoon attendance would have been weaker than those on morning attendance. We do not find such evidence in Table II; if anything, the effects on afternoon attendance are slightly stronger than those on morning attendance, although the differences are statistically insignificant.[20] While the impact of the Loss treatment is always higher than the Gain treatment, the differences are not statistically significant. There are a few possible explanations for the lack of significant differences.

First, participants may not have understood the framing. In the disbursement survey, households in the Gain and Loss treatment groups were asked questions to test their understanding of framing before the cash was disbursed. Nearly all respondents incorrectly answered framing-related questions in 2017, while 81.07 percent correctly answered these

[18]We include date-specific effects, because the dates on which the school is open can be different across schools and grades. However, we alternatively run a regression of attendance rate during the intervention days (i.e., average of Y_{ict} over t). The estimated attendance impacts remain similar to those reported in Table II below (and are available upon request).

[19]Clustering standard errors at five alternative levels—school, grade, school-grade, school-grade-cohort, and section—yields results similar to those reported below (and are available upon request).

[20]We also found that the intervention increased afternoon attendance conditional on students being present in the morning, which suggests that the intervention not only boosted attendance but also motivated students to stay in school until the afternoon.

Table II. The Effects of CCTs and SMSes on Daily Attendance During the Intervention

Dependent variable	Morning	Afternoon	Morning & Afternoon	Random visit
	(1)	(2)	(3)	(4)
Gain	0.107	0.120	0.123	0.092
	(0.025)	(0.026)	(0.025)	(0.026)
Loss	0.112	0.129	0.131	0.128
	(0.024)	(0.025)	(0.024)	(0.025)
SMS	0.048	0.055	0.056	0.069
	(0.024)	(0.024)	(0.024)	(0.026)
P(Gain=Loss)	0.859	0.749	0.785	0.159
P(Gain=SMS)	0.027	0.020	0.015	0.380
P(Loss=SMS)	0.014	0.006	0.005	0.020
Observations	123,500	123,500	123,500	8,869
R^2	0.064	0.078	0.076	0.041
Control mean	0.534	0.481	0.449	0.605

Note: (1) "Morning" takes unity if the child was present in school in the morning, and zero otherwise. (2) "Afternoon" takes unity if the child was present in school in the afternoon, and zero otherwise. (3) "Morning & Afternoon" takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. (4) "Random visit" takes unity if the child was present in school on the day of random visit, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between two different treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

questions in 2018.[21] Nevertheless, the lack of understanding is unlikely to be driving our results. As reported in Table A4 in Appendix D, even when each of the four phases is analyzed separately, no statistically significant difference was found between the Gain and Loss framing CCT groups.

Second, as described in Section 2, the cash was disbursed at the end of each phase. This means that the present value of the cash amount can be small, particularly at the early stage of the phase. At a high discount rate environment, the delay in cash disbursement can also narrow the difference between the Gain and Loss framed CCT treatment effects. When we analyze each phase by the first and second half, we find that the impact of CCT intervention tends to be stronger in the second half. Further, the difference in treatment effects between Gain and Loss CCT groups is larger in the second half of each phase, though statistically insignificant (Table A5 in Appendix D).

[21] Two questions are asked to parents: "Suppose that (Child Name) is present [absent] in school tomorrow / at the next school day, does your cash transfer balance increase, decrease or stay the same?" The correct answers are supposed to be "increase" ["same"] for the Gain group and "same" ["decrease"] for the Loss group.

Third, the weekly SMS balances and delayed disbursement may have generated limited endowment effects. In particular, individuals may not pay much attention to changes in their cash transfer balance, and thus not completely factor those into their decisions—until they are close to receiving the actual cash. For instance, Berson et al. (2021) find that the attention of the lowest paid workers is cyclical and increases prior to payday. Mani et al. (2013) argue that thinking about everyday financial demands creates a cognitive load which taps the "mental bandwidth" of the poor, but not of the rich. It is thus possible that households (in our relatively poor setting) pay lower attention in the earlier part of a phase. Therefore, the findings from Table A5 in Appendix D may also be consistent with potential behavioral inattention. From Table A6 in Appendix D, we further find that the treatment effects tend to be lower and statistically insignificant across most treatments for those in the lowest quartile of baseline consumption, indicating that the very poorest may pay even less attention to cash transfer balances, although the differences across quartiles are not statistically significantly different. Moreover, the differences between the Gain and Loss groups tend to be higher in the second part of a phase, albeit not statistically significant. These results suggest that increasing the frequency of information and payment could potentially help promote greater attention. With the spread of smartphones combined with digitized attendance management and mobile banking, future research may explore how the provision of more frequent updates and payment may impact attendance in Bangladesh, and possibly elsewhere at a similar level of development.[22]

Finally, loss aversion may simply be inoperative in our setting. We performed heterogeneity analysis by loss aversion measured by a coin toss experiment, adapted from Fehr and Goette (2007) in the baseline survey and found no significant differences in the treatment effects by loss aversion, indicating that loss aversion may not be operative at a detectable level. This may be because the loss in our context is merely a paper loss (Imas, 2016) and, as a result, we do not observe a significant impact of loss framing. Further, recent literature in psychology argues that "losses loom larger than gains" may apply only for hefty losses. This point is relevant, because the stake involved in the attendance decision in a given day is small (i.e., only 10–30 taka of gain or loss).[23] It is also possible that the concept of loss aversion has been overhyped due to publication bias, where only those articles that confirm loss aversion are published and cited and the rest remain ignored or unpublished (Yechiam, 2019; Gal and Rucker, 2018).

Regardless of the lack of significant difference between Gain and Loss, the main source

[22]Smartphone adoption in Bangladesh was 40 percent in 2019, but this is projected to grow to 69 percent in 2025 (Stryjak and Pedros, 2020).

[23]To partially counter this effect and the third point on delayed disbursement discussed above, future research may consider explaining the long-term implications of attendance/absences. For example, participants could be given the projected cash transfer amount at the end of the phase when the attendance rate over the previous week continues for the rest of the phase, which would be one exciting area of research.

of impact for CCT treatments is the cash transfer and not the framing. Indeed, there is some evidence that those parents who remember their cash transfer balance and keep their SMSes are more likely to have sent their children to school. In our sample, 77 percent of the respondents in the CCT treatment groups claim that they remember the actual cash balance, out of which 95 percent indeed remembered the balance correctly in the disbursement surveys.[24] Seven in ten respondents said that they had not deleted the last SMS on their phones. In Table A7 in Appendix D, we report the regression estimates of the students' morning phase attendances rate (i.e., the proportion of intervention days attended in a given phase) in the Gain and Loss groups on the indicators for remembering the cash balance and for keeping the last SMS, among others. The regression results show that school attendance is strongly and positively correlated with these indicators. We also find no association between the attendance rate and the loss framing (relative to the gain framing) at conventional levels of significance. While remembering the transfer amount and keeping SMS may be endogenous, we have no evidence that the CCT framing has significantly affected attendance during the intervention days.

Sensitivity and Heterogeneity Analyses

We perform a battery of sensitivity analyses, detailed in Appendix B. First, we employ a pure experimental design strategy and adjust for multiple hypothesis testing (Table A8), showing the robustness of our main estimates. Second, we consider a difference-in-differences (DiD) specification that uses monthly attendance rate as the outcome (captured from official school records before and during the intervention period), and control for individual fixed effects (Table A9). Third, we test for differential attrition rates and find no significant difference across the four treatment arms (Table A10). We also re-run the analyses excluding the students who discontinued their schooling during the intervention (Table A11). Fourth, we account for peer effects by controlling for the baseline proportions of each student's five closest classmates in each of the Gain, Loss, SMS, and Control groups (Table A12). The results and inferences from all sensitivity analyses generally align with the findings from Table II.

We also conduct various heterogeneity analyses but find no statistically significant differences by pre-intervention attendance rate, distance from school, education of parents, and socioeconomic status of households.

Across all sensitivity and heterogeneity analyses discussed in Appendix B, the impact of the loss-framed CCT is consistently higher than that of the gain-framed CCT, although the difference between the two CCT groups is small. Notably, gender of our study participant shows significant impact heterogeneity across treatments, which we thoroughly explore in Section 5.

[24]The survey questions were asked prior to disbursement of the cash transfer balance.

CCT vs SMS Treatments

Given that CCTs seem to matter much more than framing across all specifications, we henceforth combine the Gain and Loss treatments into a single CCT treatment arm to simplify the comparison between CCT and SMS interventions. Column (1) of Table III reports the same regression as Column (1) of Table II with the Gain and Loss groups merged into "CCT" treatment group. As the comparisons of these tables show, the resulting point estimate for the CCT group is similar to those for the Gain and Loss groups estimated separately. This is true in other specifications.[25]

To put our estimates into a broader context, we compare our estimates against what has been reported in the literature. The estimated attendance impact of our CCT intervention lies in the range of zero to 32 percentage points as reported in previous studies (García and Saavedra, 2017). The attendance impact of SMS is also comparable to the range of 0.9 to 11.3 percentage points found in studies using SMSes as a tool to reduce parent-child information gaps (Bergman and Chan, 2021; Berlinski et al., 2021).

As Table III shows, CCT consistently exceeds the SMS treatment effects, suggesting that economic incentives help in increasing school attendance. However, SMS treatment also increases attendance, showing attendance information has an independent impact. By comparing the impact estimates for CCT and SMS treatments in a unified setting, we can see that around half of the CCT impact could be attributed to the information effect. Our finding also suggests that the attendance information, often provided implicitly to parents in actual CCT programs, is likely to have played a role in the previous CCT studies, even though the importance of such information may depend on various factors such as the local culture, the way cash is disbursed, frequency and amount of transfer given to students, among others.

We also explore the impacts of CCT and SMS on various downstream outcomes such as student and parental educational aspirations, parental investment in education, early marriage, child labor, health, study hours, and learning outcomes. The impacts on these downstream outcomes are mixed. Both CCT and SMS treatment groups result in increased educational aspiration for students. We also observe an increase in parental investment on child's education for the SMS treatment group. On the other hand, we do not find any significant impact on parental aspirations. Thus, while parental educational aspirations may be *sticky* in nature, inexpensive information on school attendance seems to have desired impacts on educational investment. Both CCT and SMS treatments reduce the incidence of early marriage, but there is no discernible impact of either treatment on prevalence of child labor. We also do not see any impact of our interventions on child health, children's study hours, or learning outcomes. For some of these downstream outcomes, we find significant gender differences in the impacts of our interventions. We

[25] All subsequent analyses are also conducted with separate indicators for the Gain and Loss treatment groups. The results are available upon request.

elaborate on our downstream outcome measures and gender disaggregated estimates in the next section.

As discussed above, our interventions have no discernible impacts on many of these outcomes. Given that our interventions are not designed to directly address these downstream outcomes, these findings are not surprising. Nevertheless, our finding of null CCT effect on learning is broadly consistent with the existing literature (McEwan, 2015; Fiszbein and Schady, 2009). Yet, one might argue that the null impact of our interventions on learning outcomes is an important concern, because boosting attendance is of no use if it doesn't help students learn. While such a concern is valid, there are at least three reasons why our results do not allow us to conclude that school attendance may not help improve academic outcomes. First, our learning outcome measure is the score of a short mathematics test, and students are not incentivised to do well in the test. Therefore, our outcome is a noisy measure of learning outcome. Second, it is possible that attendance can improve the learning outcomes for subjects other than mathematics, but we are unable to observe this due to the lack of data. Finally, our observational time horizon may be too short to find discernible attendance impacts on learning. Since our interventions lead to a persistent increase in attendance—possibly through higher educational investment, better academic aspirations, and lower incidence of early marriage for girls as elaborated in the next section—our interventions may lead to better learning outcomes in the long run. This is an essential consideration since higher educational attainment may positively influence long-term labor market and (intergenerational) health outcomes (Asadullah, 2006; Currie and Moretti, 2003).

Table III. The Gendered Effects of CCTs and SMSes on Attendance

	During Intervention			Intervention + Non-Intervention			Post-Intervention		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)	All (7)	Boys (8)	Girls (9)
CCT	0.110	0.081	0.130	0.115	0.086	0.134	0.031	0.006	0.052
	(0.020)	(0.030)	(0.028)	(0.019)	(0.029)	(0.026)	(0.014)	(0.022)	(0.018)
SMS	0.048	0.003	0.075	0.047	0.003	0.076	0.036	0.009	0.054
	(0.024)	(0.035)	(0.032)	(0.023)	(0.034)	(0.030)	(0.016)	(0.025)	(0.021)
P(CCT=SMS)	0.007	0.025	0.049	0.002	0.012	0.029	0.691	0.901	0.897
Observations	123,500	61,680	61,820	9,387	4,634	4,735	8,678	4,369	4,309
R^2	0.064	0.065	0.083	0.095	0.101	0.125	0.419	0.376	0.495
Control Mean	0.534	0.514	0.555	0.531	0.518	0.545	0.263	0.274	0.251

Note: The dependent variable in Columns (1)–(3) is daily morning attendance during our intervention period in 2017 and 2018. The dependent variables in Columns (4)–(6) are monthly morning attendance rates during our intervention and non-intervention periods in 2017 and 2018 (the number of days attended in a month divided by the total number of valid school days in the month). The dependent variables in Columns (7)–(9) are monthly morning attendance rates in 2019 (the number of school days attended in a month divided by the total number of valid school days in the month). The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arms are given in the middle panel. The above specifications control for the date fixed effects in Columns (1)–(3) and year-month fixed effects in Columns (4)–(9). They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television and class fixed effects. Standard errors (in parentheses) are clustered at the individual level.

5 Gendered Impacts and Persistence

It is well-known that girls tend to receive lower human capital investments compared to boys in poorer countries. There have thus been many interventions targeted at girls in existing literature and these past works suggest that targeted CCTs enhance longer-term schooling outcomes of girls while information interventions improve their learning outcomes (Khandker et al., 2021; Armand et al., 2020; Hahn et al., 2018; Shamsuddin, 2015; Ullah, 2013). Even though our intervention does not specifically target girls, there may be important heterogeneous treatment effects by gender. In particular, females tend to be more receptive and less sticky in their attitudes than boys (Lee, 2005; Keenan et al., 1999; Keenan and Shaw, 1997; Johnson and MacDonnell, 1974; Stein, 1969). It is therefore possible that girls would respond more to our intervention compared to boys. In what follows, we discuss the effects of our interventions on post-intervention school attendance as well as various downstream outcomes such as students' and parents' academic aspirations, child labor and marriage, parental investment and spending on education, and learning outcomes, disaggregated by gender.

Gender Differences in Attendance

We begin by disaggregating Column (1) of Table III by gender. The estimated impacts of our interventions on attendance are reported in Columns (2) and (3) for boys and girls, respectively. To see the impacts both during and after the intervention period in a comparable way, we use monthly—instead of daily—morning attendance rate as the dependent variable. Columns (4)–(6) redo the same analysis as Columns (1)–(3) using the monthly attendance rate, or the proportion of school days attended in a month, including both intervention and non-intervention days. In Columns (7)–(9), we report the regressions of monthly attendance rates in 2019, after our interventions had already ended.[26]

There are three observations to make from Table III. First, by comparing Columns (1)–(3) against Columns (4)–(6), we see that the use of monthly data does not change the results much. Second, as noted above, the attendance impacts of CCTs and SMSes for girls are larger than those for boys during the intervention period. Third, it can be seen from the comparison of Columns (7)–(9) against Columns (4)–(6) that the effects of CCTs and SMSes attenuate but still persist beyond the intervention period—except for the attendance impact of SMS for boys, which was insignificant and close to zero even during the intervention period. In Table A13 in Appendix D, we divide the 2019 sample

[26]The attrition rate in 2019 was around three times greater than that during the intervention period. Reassuringly, the 2019 attrition rates did not differ across the four treatment arms. We also re-estimate the model dropping the students who discontinued their studies in 2019 or earlier. This analysis yields qualitatively similar results and are available upon request.

into the first and second half and re-run the analysis, which reveals that the effects of both CCTs and SMS on girls' attendance are very strong in the six months immediately after the intervention. The effects remain positive and statistically significant, albeit weaker, in the second half of 2019. It is also worth noting that the effects of CCTs and SMS converge after the intervention period. These results indicate that information embedded in the CCT and SMS treatments, rather than the cash incentives, may be a key driver of the persistence of the treatment effects for girls (but not for boys). We, therefore, explore the plausible reasons behind the gender difference in the post-intervention effects.

Gender Differences in Aspirations

Academic aspiration is one plausible channel through which the attendance impact of our interventions persists beyond the treatment period. For example, weekly SMS information may have raised parents' attention to education, particularly for girls. This, in turn, may have increased parental aspiration for girls' education relative to boys. Similarly, our interventions may have raised aspiration for girls relative to boys, possibly through increased attendance during the intervention period. To test these possibilities, we use students' and parents' academic aspirations collected in the baseline and endline surveys, which are measured by the highest grades that the students want to achieve and that the parents want their children to achieve, respectively.

We construct the outcome variable in two different ways in this regard: (i) a continuous outcome that measures the change in the years of schooling that the student wants to achieve or the parent wants the student to achieve between the baseline and endline surveys and (ii) a discrete outcome that takes unity if the change in the continuous outcome is positive (i.e., academic aspiration has increased) and zero otherwise. For parental aspirations, we restrict the sample to households in which the same respondent answered the educational aspiration question in the baseline and endline surveys.[27]

We then estimate a model similar to eq. (1), except that the dependent variable is replaced by the outcome variables defined above and without the date fixed effects. Column (1) of Table IV indicates that both CCT and SMS interventions increased the academic aspirations of students, but the increase is statistically insignificant for the CCT treatment arm. We further see that the increase comes only from girls with CCT and SMS interventions. That is, both CCT and SMS treatments significantly increase girls' educational aspirations as shown in Column (3). On the other hand, there is no significant impact on boys as shown in Column (2). Using the discrete outcome, we find that the proportions of girls that increased their academic aspirations during our intervention were 20 and 12 percentage points higher for CCT and SMS treatment arms

[27]Because of this and missing responses, the indicator of higher academic aspirations was observed only for 721 students and 475 parents. There were no statistically significant differences across treatment arms in the prevalence of missing values in this indicator, either for students or parents.

respectively than the girls in the Control group, and the former figure is statistically significant.[28] In contrast, parental aspirations were more *sticky* in nature and were not impacted by our interventions (Table A14).

Table IV. The Effects of CCTs and SMSes on Changes in Students' Aspirations

	Continuous Outcome			Discrete Outcome		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.540	-0.271	1.473	0.080	-0.031	0.200
	(0.347)	(0.439)	(0.375)	(0.052)	(0.066)	(0.047)
SMS	0.612	0.185	1.244	0.095	0.082	0.122
	(0.314)	(0.403)	(0.486)	(0.059)	(0.063)	(0.082)
P(CCT=SMS)	0.792	0.395	0.668	0.640	0.050	0.355
Observations	721	364	357	721	364	357
R^2	0.061	0.080	0.085	0.058	0.097	0.069
Control mean	0.580	0.956	0.176	0.352	0.385	0.318

Note: The dependent variable ("Continuous Outcome") in Columns (1)–(3) is the change between the baseline and endline surveys in the number of years of schooling that the participating student aspires to achieve. The dependent variable ("Discrete Outcome") in Columns (4)–(6) is an indicator that takes unity when the continuous outcome used in Columns (1)–(3) is positive and zero otherwise. Completed years of schooling for BA/BSc/BSS/Fazil, MA/MSc/MA/MSS/Kamil, and PhD are treated to be 15, 17, and 22 years, respectively. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arm are given in the middle panel. The above specifications control for class fixed effects and unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the class level.

Parental Expectations of Future Cash Transfers

It is also possible that the attendance impact persisted for girls because parents expected to receive cash transfers in the future. Such expectation is plausible, given that Bangladesh has gender-targeted conditional cash transfer programs for secondary school students (Xu et al., 2022). To delve into this potential mechanism, we asked all households in the endline survey about their expectations of receiving cash transfers in the future. Note that we neither planned to resume cash transfers nor did we announce our plans when they were surveyed. Nevertheless, approximately 74 percent of adult respondents expected to receive cash transfers in the future.

[28]Sensitivity analyses conducted with separate indicators for the Gain and Loss groups yielded similar results and inferences, except that the effect of SMSes was marginally significant for girls at the 10 percent level.

We use a model similar to eq. (1), except that the dependent variable is an indicator for the parental expectation of future cash transfers, which takes unity when the parents believe that they are very likely or somewhat likely to receive cash transfers in the next two years, and zero otherwise. Table V shows that the CCT treatment significantly raises the expectation to receive cash transfer in the future, regardless of the gender of the child. While the SMS treatment also increases the expectation of future cash transfers, the increase is minor and statistically insignificant. Furthermore, there is no significant gender difference in the impact of our interventions on parental expectations of future cash transfers. The impact for boys is greater than that for girls, if anything. Taken together, expectation of future cash transfers is unlikely to be the main driver of persistent attendance impact for girls.

Table V. The Effects of CCTs and SMSes on Expectations of Future Cash Transfers

	All (1)	Boys (2)	Girls (3)
CCT	0.175	0.197	0.147
	(0.052)	(0.067)	(0.059)
SMS	0.052	0.069	0.020
	(0.032)	(0.057)	(0.051)
P(CCT=SMS)	0.037	0.074	0.048
Observations	783	392	391
R^2	0.051	0.094	0.071
Control mean	0.639	0.663	0.615

Note: The dependent variable is a binary outcome that takes unity if the adult respondent said that he/she is highly or somewhat likely to receive conditional cash transfers for school attendance in the next two years, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arms are given in the middle panel. The above specifications control for class fixed effects and unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the class level.

Gender Differences in Child Labor and Early Marriage

A decrease in child labor and delay in early marriage are other potential channels through which the attendance impact of our interventions persist. Once children start working or get married, they may permanently stop schooling. Therefore, some children who would have dropped out of school without our interventions may have continued schooling. In this subsection, we analyze the impact of our interventions on child labor for both boys and girls. We also analyze early marriage only for girls, because early marriage is nearly negligible for boys (no boy was married at the baseline and only one at the endline).

To capture the change in child labor, we construct an indicator variable that takes unity if the student is engaged in an economically gainful activity within the last seven days of the endline survey but not in the baseline survey, and zero otherwise.[29] We also construct the early marriage indicator in a similar manner.[30] We exclude the participants from the analyses if the status of child labor or marriage is missing in either the baseline or endline survey. Using these definitions, about 2 percent of girls and 10 percent of boys were engaged in an economically gainful activity at the endline but not at the baseline. About 4 percent of the girls became married between the baseline and endline.

We run regressions of child labor and marriage indicators using specifications similar to Tables V. From Columns (1)–(3) of Table VI, there is no statistically significant evidence that our interventions impacted child labor. Column (4) of Table VI, however, suggests that both the CCT and SMS treatments reduced the incidence of early marriage for girls. A sub-sample analysis of early marriage for girls by grades in Table A15 in Appendix D suggests that the effects of the CCT and SMS treatments may have been driven by grade-9 girls, who are around 15 years old.[31] Overall, we have no evidence that the persistence of attendance impact for girls is attributable to a reduction in child labor. However, it may be attributable to a reduction in early marriage, which is a notable finding.

It is well-known that gender norms often limit girls from attending secondary school in poorer settings (Millán et al., 2020; Barham et al., 2018; Cahyadi et al., 2018). Indeed, parents in poor countries often prioritize domestic duties at the cost of schooling and are more willing to marry off girls earlier. Conversely, there is qualitative evidence that girls who attend schools regularly tend to have parents who actively encouraged their educational and career aspirations and communicate openly with the parents (Satyanarayana et al., 2018). If information on school attendance—as provided by our intervention—can increase girls' educational aspirations, then parents may feel a lesser need to marry girls off for the sake of preserving their purity and securing their future as a bride. In the next subsection, we indeed find evidence that SMS treatment increased parental investment in children's education, especially for girls.

[29] A student is considered to be engaged in an economically gainful activity if the student's primary or secondary activity over the past week was employment in agriculture, forestry, or aquaculture; employment in a wage/salaried position; other self-employment in production, business, and services; or performance of domestic duties. Because of the reference period, our surveys do not capture the impact of the intervention on temporary agricultural seasonal labor during the harvest and planting seasons. While seasonal child labor is an important deterrent to education, it may not necessarily affect school attendance permanently. Since seasonal child labor is also relevant to school absenteeism in rural Bangladesh, it will be helpful to explore the interventions that address absenteeism due to seasonal child labor in future research investigations.

[30] One of the female participants in the new cohort was separated both at the baseline and endline. For our purpose, she was not considered married during our intervention period.

[31] We found no effects of the intervention on child labor when we break down the analysis by grades. Sensitivity analyses that adopt a DiD estimation with household fixed effects yielded qualitatively similar results.

Table VI. The Effects of CCTs and SMSes on Child Labor and Early Marriage

Dependent variable	Child Labor			Early Marriage
	All	Boys	Girls	Girls
	(1)	(2)	(3)	(4)
CCT	-0.012	0.016	-0.039	-0.079
	(0.018)	(0.021)	(0.033)	(0.034)
SMS	-0.029	0.006	-0.057	-0.090
	(0.018)	(0.017)	(0.040)	(0.037)
P(CCT=SMS)	0.317	0.701	0.438	0.710
Observations	754	380	374	391
R^2	0.022	0.022	0.049	0.047
Control Mean	0.059	0.021	0.097	0.134

Note: "Child Labor" includes primary or secondary occupation of the child being wage/salaried employment, self-employment in agriculture, forestry, and aquaculture, other self-engagement (including family business) in production, business, and services, or domestic duties. "Child Labor" takes unity if the child was engaged in any of the above activities at the endline and not at the baseline, and zero otherwise. "Early Marriage" takes unity if the child was married at the endline and unmarried at the baseline, and zero otherwise. There was one girl child from the new cohort who was separated at the baseline and remained so at the endline. We assumed her marriage status as "unmarried". The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arms are given in the middle panel. Standard errors (in parentheses) are clustered at the class level.

Gender Difference in Parental Investment in Education, Spending Patterns, and Test Scores

Next, we look at whether our intervention increased parental investment in the education of participating children, and whether there was any gender difference in such investment.[32] To study the impact on parental investment, we define the outcome variable in two ways: (i) the logarithmic difference in real education expenditure between the baseline and endline with nominal expenditures adjusted for inflation by the CPI, and (ii) a discrete outcome that takes unity if the endline expenditure was higher than the baseline expenditure, and zero otherwise.[33] While the CCT intervention does not appear

[32]Education expenditure includes (i) admission, tuition, and exam fees; (ii) books, uniform, name-tag, pencil, and other equipment expenditure; (iii) transportation and tiffin costs; and (iv) private tuition costs in the last 12 months.

[33]There are 46 households that report zero expenditure on every expenditure component at the baseline or endline. These households have been dropped from the analysis; there is no systematic difference across treatment arms in the prevalence of households dropped. For the remaining households, we include an indicator variable that takes one if the household reports zero expenditure on at least one item, and zero

to have any impact, SMS treatment increases parental investment in child's education, especially for girls (Table A16 in Appendix D). Higher parental investment in education could therefore be a potential driver of an improvement in girls' educational aspirations, lower incidence of early marriage, and persistent attendance impact.

We also attempt to understand how households in the CCT treatment arms utilize the cash transferred to them. If the household spends the additional money on unnecessary luxury items or sin goods such as alcohol and cigarettes, the intervention will not provide any additional benefit to the child beyond school attendance, higher aspirations and lower incidence of early marriage. Thus, it is important to examine whether the cash provided by the CCT intervention benefited the child in the form of increased parental educational investments. During the disbursement at the end of the first three phases, households were asked how they plan to spend the transferred cash in terms of the proportions spent on each of health, education, luxury, savings, and other expenses. During the endline survey, households were asked to report the actual proportion of cash spent on these items. While one cannot completely rule out the possibility of social desirability bias, the reported actual spending on education captured from the endline survey was high, about 92 percent of the total cash transferred to the CCT treatment arms. The share of the actual spending on education for girls was larger than that for boys by about one percentage point, but the difference is statistically insignificant. It is also worth noting that the actual spending share on education was considerably higher than the intended share of 72 percent in the disbursement survey in 2018-I. The higher proportion spent at the endline could be due to the fact that the endline survey was conducted between the end of the current school year and the beginning of the next school year, so that new school supplies and enrollment costs were needed. Indeed, we find similar patterns in the reported planned expenditure in 2017: households reported that they intend to spend 54 [86] percent of the cash transfers on education in 2017-I [2017-II]. Hence, based on responses from the household surveys, the extra cash that the household received from the CCT intervention was, most likely, spent on education expenditures for children.

We also examine the impact of our intervention on learning outcomes, measured by a short mathematics test administered at the baseline and endline. We normalize the test scores for each individual by subtracting the control mean and dividing by the control standard deviation for each class. We then apply two-stage least squares (2SLS) estimation with the following second-stage equation to estimate the impact of school attendance on endline test scores.

otherwise in the set of regressors. Omitting this indicator variable from the model does not alter the result much.

$$
\begin{aligned}
\text{EndlineScore}_{ic} &= \theta_0 + \theta_1 \text{AttendanceRate}_{ic} + \theta_2 \text{BaselineScore}_{ic} \\
&\quad + \theta_3 \text{Z}_{ic} + u_c + \eta_{ict}.
\end{aligned} \tag{2}
$$

The vector of covariates Z_{ic} includes child-level characteristics such as education levels of the father and mother, gender, weight, and height of the student, and unbalanced covariates at baseline. Since $\text{AttendanceRate}_{ic}$ is endogenous, we instrument it by indicator variables for the CCT and SMS groups. As Table A17 shows, attendance has no statistically discernible impact on the endline test score either for boys or girls. This finding is broadly consistent with existing literature, suggesting that interventions to incentivize school attendance typically have no or only little impact on learning outcomes (McEwan, 2015; Fiszbein and Schady, 2009). Nevertheless, one must exercise caution when interpreting the results as we examined test scores only for mathematics and not for other subjects. It is also plausible that despite the transferred cash being reportedly spent mostly on education, students who were induced to attend school by our interventions may not have received sufficient amount of complementary investment to improve test scores in a short period of time. For example, the quality of education in schools may have reduced due to the high student-teacher ratio—induced by the higher attendance by students through our intervention. As a result, learning aids in the form of private tutoring from teachers or informal coaching from siblings or parents would be critical for students to academically excel, particularly in subjects like mathematics that are deemed challenging without external help for many learners. Hence, our learning impact estimates are not at odds with the possibility that our interventions may improve the learning of non-mathematical subjects or even mathematics in the long run.

Taken together, the results discussed in this section indicate that CCT and SMS improved girls' school attendance persistently, measured during the intervention period and one year after the end of the intervention. This could plausibly be because our interventions increased parental investment in girls' education, raised girls' academic aspirations, and decreased their incidence of early marriage. Even though our interventions do not improve mathematics test scores in the short run, they may lead to positive long-run labor market and health outcomes (Asadullah, 2006; Currie and Moretti, 2003). Our results also suggest that attendance information rather than cash transfers may be an important driver of the persistent attendance impacts on girls. The information element embedded across CCT and SMS treatments plausibly led to the realization of the importance of education by parents, as shown by an increased investment in education expenditure. It may also have spurred girls to pursue higher academic achievements, thereby reinforcing the habit of attending school even after the end of the intervention. Given that CCTs involve costly payment transfers whereas SMSes only rely on a low-cost technology, our findings trigger the question of whether SMS intervention is more cost-effective than CCTs in bringing children to school, a question which we address in the next section.

6 Cost-Effectiveness of CCT and SMS Interventions

Back-of-the-envelope Calculations

Let us now compare the cost-effectiveness of the CCT and SMS interventions. We can make a fair comparison since both interventions were implemented in a unified setting. From Column (1) of Table III, the estimated impact of the SMS treatment on attendance is 4.8 percentage points while that of the CCT treatment is 11.0 percentage points, suggesting that CCT is around twice as powerful as SMS. However, SMS is less expensive than CCT in terms of the program implementation cost, because the former does not involve cash transfers. Therefore, it is not apparent which intervention is more cost-effective. Hence, we perform a back-of-the-envelope calculation to gauge the cost-effectiveness of our interventions over the two-year period, denoted by λ, as measured by the increased attendance in percentage point per program cost in thousand taka.

We perform the cost-effectiveness calculations under each of the following three different cost scenarios: (1) the actual program costs for our interventions; (2) policy costs without digital support; and (3) policy costs with digital support. Under scenarios (2) and (3), we assume that collecting attendance data generates no additional program cost, since teachers collect attendance data as part of their duties. That is, we do not give additional remuneration for collecting daily attendance data, and hence the cost of collecting schooling information is not a part of the program cost. In scenario (3), we further assume that there is adequate digital infrastructure such that data can be collected automatically, say, through biometric finger scanners and cash transfers can be made through digital financial services (DFS), such as mobile banking. DFS enables households to receive transfers directly in the mobile phone, making it unnecessary to conduct physical visits to households for the disbursement of cash transfers. Scenario (3) is the most optimistic scenario, in which good digital infrastructure enables efficient data collection and cash transfers. Arguably, it is an overly optimistic scenario, given the current ground reality in Bangladesh. Nevertheless, it is not an implausible scenario in a foreseeable future. We discuss detailed breakdowns of the cost components included under the three cost scenarios in Appendix C, and present the cost figures used in our analysis and the resulting cost-effectiveness measures for SMS and CCT interventions under each scenario in Table A18 in Appendix D.

Here, we summarize the main takeaways from the cost-effectiveness analysis. First, the resulting cost-effective measure λ, expressed in percentage point increase in attendance rate per thousand taka, is 2.70, 5.85, and 47.52 [2.84, 3.37, and 5.15] under Scenarios

(1), (2), and (3), respectively, for the SMS [CCT] intervention. Thus, as we move from Scenario (1) to Scenario (3), λ becomes larger. This is because the cost assumptions become more favorable for implementing the CCT and SMS interventions. Second, SMS treatment is more cost-effective in Scenarios (2) and (3), whereas the difference in the cost-effectiveness measure between the CCT and SMS interventions is negligible in Scenario (1). Therefore, with adequate supporting policy and digital infrastructure, simple information provided through SMS would become a more cost-effective way of boosting secondary school attendance than CCTs. This result is not surprising, because the direct cost of cash transferred to households, which is counted towards program costs in our analysis, cannot be reduced by policies or technologies.

Obviously, one could argue that the direct cost of cash transfers should be excluded from the program cost, because cash transfers would not change the surplus in society. We therefore report an alternative cost-effective measure $\tilde{\lambda}$ that excludes the direct cost of cash transfers from cost calculations in Table A18. However, we argue that $\tilde{\lambda}$ is not a policy-relevant measure for at least three reasons. First, government officials are often interested in cost-effectiveness with respect to the financial resources spent by the government. Second, even if the government officials take a position more like a social planner and care about the social surplus, the cash to be transferred must be raised from somewhere. If the cash comes from distortionary taxes, there will be efficiency loss in general. Third, the non-transfer program cost per student is unlikely to increase in proportion to the daily transfer amount per student in a CCT program. This suggests that we can potentially always improve the cost-effectiveness of a CCT program by raising the transfer amount so long as there are students who are not attending school, if cash transfers are excluded from the cost-effectiveness calculations. To see this point, suppose that the non-transfer program cost per student does not change with the daily transfer amount. Then, the daily transfer amount should be large enough so that everyone would come to school every day. Despite these issues, the SMS treatment remains more cost-effective than CCT under Scenario (3), even when using $\tilde{\lambda}$.

It should be reiterated here that our cost-effectiveness measure focuses on attendance during the intervention days. As we have seen, the magnitudes of the effects of CCTs and SMSes on post-intervention attendance are similar. Hence, our cost-effectiveness measure would favor SMS over CCT intervention once attendance over a longer time horizon is taken into account or infrastructure for efficient attendance data collection is established. Another noteworthy point is that the amount of resources needed for the SMS and CCT interventions may differ by the order of magnitude. Therefore, when the amount of resources available to the government is limited, SMS intervention would be more attractive. In addition, our cost-effectiveness calculations do not consider impacts on outcomes other than attendance, such as early marriage and academic aspirations. Depending on how these factors are taken into consideration, both CCTs and SMSes can be viable alternatives to boost attendance.

Cost-effective CCTs

In the preceding discussion, we ignored the variations in the daily cash transfer amount across different phases. The daily cash transfer amounts given to households in the CCT treatment arms varied between 10 and 30 taka as described in Section 2. We use this variation to find the cost-effective daily transfer amount. To this end, we first estimate a model somewhat similar to eq. (1) but the indicators for the Gain and Loss groups are replaced with different indicators for three daily transfer amounts—10 taka, 20 taka, and 30 taka—to capture the effects of different CCT amounts. As shown in Table VII, the initial 10 taka per day in 2017 had no statistically significant impact on attendance. However, an additional 10 taka per day transfer significantly improved attendance. The effects are further magnified when the transfer amount is increased to 30 taka per day, but the incremental gain in attendance from 20 to 30 taka per day is smaller than that from 10 to 20 taka per day. This suggests that the intensive margin of CCT matters and that the impact of transfer at the intensive margin is diminishing.[34]

Since there is a diminishing marginal impact of transfer, it is possible to increase the cost-effectiveness of the CCT interventions by adequately calibrating the daily transfer amount. We thus attempt to address the choice of cost-effective transfer amount using the following model that is quadratic in the transfer amount τ_{ict}, and allows us to identify the effects at the extensive margin (i.e., whether the household receives CCTs) and intensive margin (i.e., how much the household receives for each day of attendance) of the CCT interventions separately.

$$\mathrm{Y}_{ict} = \underbrace{f_0 \mathrm{CCT}_{ic} + f_1 \tau_{ict} + f_2 \tau_{ict}^2}_{\text{Attendance impact of CCT transfer}} + g\mathrm{SMS}_{ic} + \underbrace{\beta_0 + \gamma X_{ic} + u_c + v_t}_{\text{Exp. att. without intervention}} + \epsilon_{ict}, \tag{3}$$

where Y_{ict} is a daily morning attendance indicator that takes unity if individual i from class c is present in school on date t, and zero otherwise. CCT_{ic} is an indicator that takes unity if individual i belongs to the CCT treatment arm, and zero otherwise. The daily transfer amount τ_{ict} satisfies $\tau_{ict} = 0$ if $\mathrm{CCT}_{ict} = 0$. Otherwise, τ_{ict} is equal to 10, 20, or 30, depending on the phase and subtreatment assignment. SMS_{ic}, X_{ic}, u_c, and v_t remain the same as in eq. (1). The error term ϵ_{ict} is clustered at the individual level.

[34] Given that households received 10 taka/day in 2017-I and 2017-II, 20 taka/day in 2018-I, and 20 or 30 taka/day in 2018-II, the results could be driven by the combination of phase and amount effects. Therefore, we also perform sensitivity analyses using attendance data from 2018-II only, when households were randomized into receiving either 20 or 30 taka. Table A19 shows that the attendance impacts of CCT treatments are always higher for the households in the "High" [H] subtreatment (30 taka/day) than for the "Low" [L] subtreatment (20 taka/day) in 2018-II. Even though the differences between low and high subtreatment effects are statistically insignificant, our results overall underscore the potential importance of intensive margin of cash transfer.

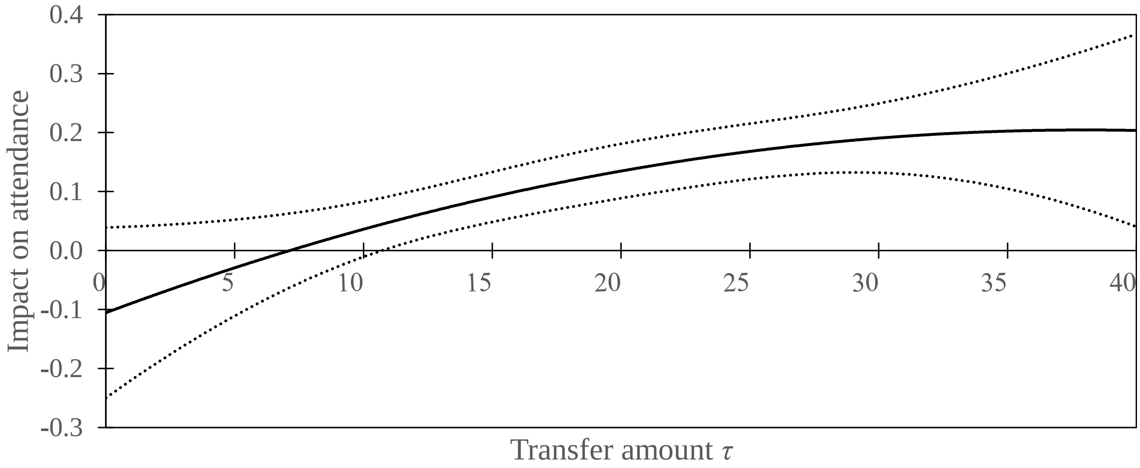
Table VII. Non-Linear Treatment Effects in CCTs

Dependent variable	Morning Daily (1)	Afternoon Daily (2)	Morning & Afternoon (3)	Random Visit (4)
CCT [10tk]	-0.012	-0.004	-0.003	-0.005
	(0.027)	(0.029)	(0.029)	(0.029)
CCT [20tk]	0.089	0.092	0.095	0.064
	(0.026)	(0.025)	(0.025)	(0.025)
CCT [30tk]	0.143	0.160	0.161	0.099
	(0.031)	(0.031)	(0.031)	(0.035)
SMS	0.048	0.056	0.056	0.069
	(0.024)	(0.024)	(0.024)	(0.026)
P(CCT [10tk] = CCT [20tk])	0.000	0.000	0.000	0.015
P(CCT [10tk] = CCT [30tk])	0.000	0.000	0.000	0.005
P(CCT [20tk] = CCT [30tk])	0.042	0.013	0.013	0.265
P(CCT [10tk] = SMS)	0.193	0.204	0.201	0.115
P(CCT [20tk] = SMS)	0.344	0.406	0.383	0.905
P(CCT [30tk] = SMS)	0.041	0.027	0.025	0.552
Observations	123,500	123,500	123,500	8,869
R^2	0.067	0.082	0.080	0.042
Control mean	0.534	0.481	0.449	0.605

Note: The above estimates are from a non-linear specification. "Morning" takes unity if the child was present in school in the morning, and zero otherwise. "Afternoon" takes unity if the child was present in school in the afternoon, and zero otherwise. "Morning and Afternoon" takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. "Random visit" takes unity if the child was present in school on the day of random visit, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the test of equality of means between different treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

We denote the control attendance, or the expected attendance in the absence of any intervention for individual i on date t, by $A_{ict} \equiv \beta_0 + \gamma X_{ic} + u_c + v_t$ and interpret $f(\tau) \equiv f_0 + f_1\tau + f_2\tau^2$ as the attendance impact of a CCT intervention with a daily transfer of τ taka. Based on the regression estimates from eq. (3), we predict $f(\tau)$. Figure 2 shows the graph of the predicted value of $f(\tau)$ and its 95 percent confidence bounds, which clearly show diminishing marginal impact. As the figure indicates, the transfer amount has to slightly exceed 10 taka per day to have a statistically significant impact on attendance. This is consistent with the findings from Table VII. Figure 2 also indicates that the marginal effect becomes zero around 38 taka per day. This amount

Figure 2. The Estimated Attendance Impact of CCT with Daily Transfer τ



Note: The figure plots the estimated impact of CCT with daily transfer τ on daily morning attendance, $f(\tau)$, from a regression of eq. (3).

should be taken with a grain of salt, because it is outside the range of daily transfers between 10 and 30 taka in our intervention.

The regression based on eq. (3) also allows us to predict the control attendance A_{ict}. Even though the predicted value is not bound to be on the unit interval, 99.9 percent of observations are within the unit interval. The mean and median of A_{ict} are both around 0.53, which is extremely similar to the control mean reported in Column (1) of Table II. The dotted line in Figure 3 represents the kernel density estimate of A_{ict}.

When the government resources to increase school attendance are limited, policymakers may focus on maximizing the bang for the buck. We, therefore, derive the most cost-effective daily transfer amount τ^* that maximizes our cost-effectiveness measure λ. We assume that the policy-relevant non-transfer program cost is τ. Using the figures reported in Table A18, we obtain a combined cost of $C = 0.380$ thousand taka per student for communication and disbursement.[35] We then express τ^* as a function of C and A. Notice that the expected attendance rate in the presence of the CCT program is given by $A + f(\tau)$. Therefore, the expected daily transfer cost is $(A + f(\tau))\tau$ per student and the expected total program cost is $(A + f(\tau))\tau D + C$, where $D = 155$ represents the average number of intervention days across the two cohorts.[36] The attendance impact per thousand taka is given by the following expression:

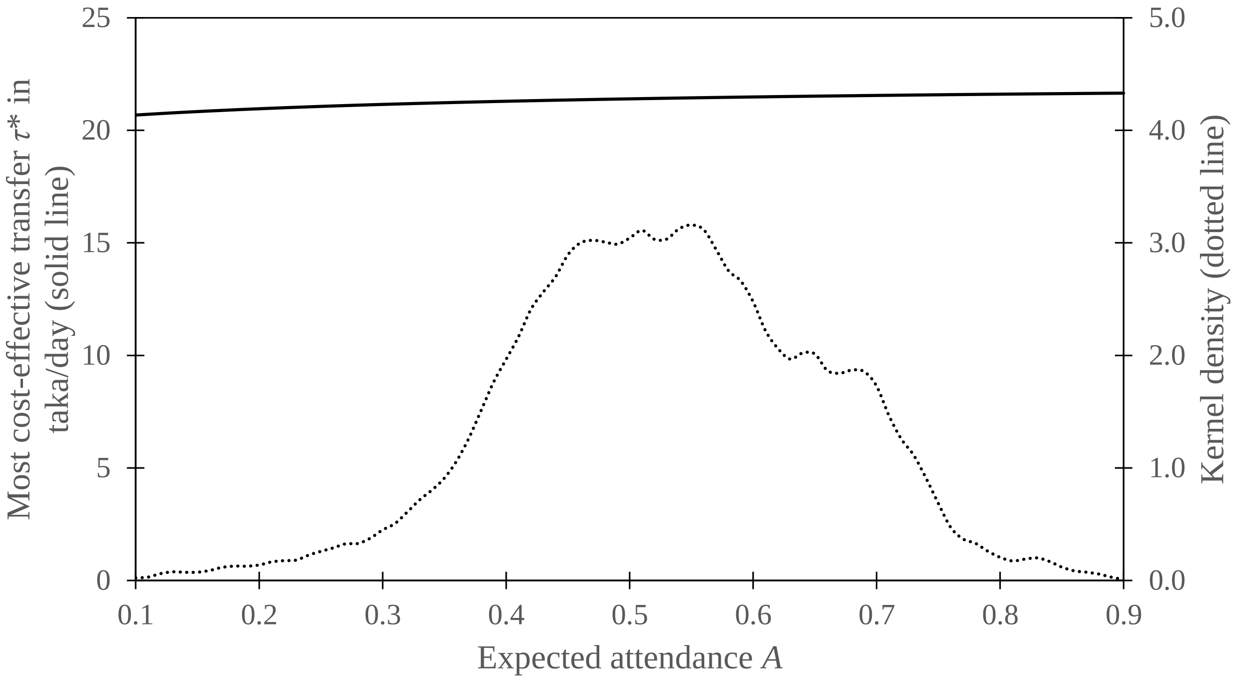
$$\lambda(\tau) = \frac{f(\tau)D}{(A + f(\tau))\tau D + C}$$

Taking the first order condition with respect to τ and rearranging the terms, we see

[35]Specifically, we add the cost components (A), (G), (H), and (I) in Table A18 and arrive at $380 (= 101 + 81 + 135 + 63)$ taka per student.

[36]Since there are $60 + 50 \times 3 = 210$ intervention days for the old cohort and $50 \times 2 = 100$ days for the new cohort, the average is $D = (210 + 100)/2 = 155$ days.

Figure 3. Most Cost-Effective Transfer τ^* as a Function of Expected Attendance



Note: The solid line depicts the most cost-effective transfer, τ^*, as a function of control attendance, $A_{ict} \equiv \beta_0 + \gamma X_{ic} + u_c + v_t$. The dotted line depicts the kernel density estimate of control attendance A_{ict}.

that τ^* is implicitly given by the following expression:

$$f'(\tau^*)(A\tau^*D + C) - f(\tau^*)(A + f(\tau^*))D = 0.$$

The solid line in Figure 3 shows the most cost-effective transfer amount τ^* as a function of A.[37] The estimates from the quadratic specification suggest that the most cost-effective amount of transfer is around 21 taka per student per intervention day, regardless of the expected attendance A.

Because the most cost-effective amount of transfer derived in this way depends on the functional form specification, we repeat the same analysis under alternative functional forms in Appendix C. Regardless of the functional form used, the most cost-effective daily cash transfer amount falls between 20 and 24 taka per day, which is roughly a quarter of child daily wages in the region.[38] Hence, with only a fraction of the daily wage for child labor, daily attendance can be cost-effectively increased. This is an important consideration, given the potential high returns to education in the form of better employment prospects and income (Ito and Shonchoy, 2020; Asadullah, 2006).

[37] While the analysis above ignores spillover effects, most of the arguments above will hold so long as the spillover effects are uniform across individuals, which is likely to be the case. However, the estimated value of A may be biased upwards since the students in the regression analysis are all affected by spillover effects.

[38] Using the average wage rate of 10.1 taka per hour and average daily work hours of 8.64 hours in footnote 13, the average daily wage is about $87.3(= 10.1 \times 8.64)$taka per day

7 Conclusion

This paper addresses the problem of low secondary attendance by separating cash and information constraints with three potentially cost-effective interventions in a unified framework: (i) weekly attendance information through SMS text and voice calls to parents, (ii) conventional gain-framed CCT plus weekly SMS, and (iii) a novel loss-framed CCT plus weekly SMS. With a per-student per-day cost of about 11.5 (=1,775/155) BDT, or 0.137 USD, the SMS treatment increases school attendance by 4.8 percentage points. In comparison, the CCT treatments increase school attendance by around 11 percentage points, with a cost of 25.0 (=3,870/155) BDT, or 0.298 USD, per student per day. These results are robust across a battery of sensitivity analyses. The estimated attendance impact of our treatment compares favorably to the mean impact of 5.75 percentage points derived from 22 evaluations of the impact of CCTs on secondary-school attendance reported in García and Saavedra (2017). Even though the program implementation details and the program impacts are highly heterogeneous across studies, our research indicates the potential presence of a low-hanging fruit to promote secondary-school attendance in our study area and possibly elsewhere in Bangladesh, the rest of the developing world, and in resource-poor settings.

Our study has several important implications. First, the Loss-framed CCT has the highest impact across all three interventions, even though its impact is not statistically different from the conventional Gain-framed CCT. It is unclear exactly why the loss framing has, at best, limited effects. We argue that the delay in cash disbursement and delivery of SMS from the realization of school absence and insensitivity of respondents to small losses are among the plausible reasons. We also find that students from households who saved their SMS and remembered their cash balances had better attendance records than those who did not. These findings suggest that cash is of fundamental importance in CCT programs and framing may be of secondary importance, at best.

Second, sending SMS and voice calls can be a cost-effective and rapidly scalable intervention to boost school attendance. In our study, about half of the attendance impact of the CCT interventions can be attributed to the information provided by SMS. This underscores the importance of information provision typically implicit in existing CCT programs. Using our unified setting for SMS and CCT interventions, we find that SMS is more cost effective than CCT interventions, even when we ignore the cash transfer cost, provided that there is infrastructure for collecting attendance data digitally. Depending on the time horizon, resource constraints, and policy objectives beyond school attendance, both SMS and CCT interventions can be viable policy instruments to boost school attendance.

Third, both CCT and SMS treatments show lasting effects on girls: those who received either CCT or SMS interventions were significantly more likely to attend school during the post-intervention year. Further, the post-intervention impacts of CCT and SMS interventions tend to converge. This suggests that the weekly attendance information in our interventions had induced behavioral changes that contributed to increased schooling for girls. This is plausible because the regular attendance information draws households' attention to education and possibly creates a sense of being monitored. Our analysis indicates that increased academic aspirations, higher parental investment in education, and decreased early marriage due to our interventions may have contributed to this sustained attendance impacts for girls.

Finally, our cost-effectiveness analysis reveals diminishing marginal impacts of the CCT transfer amount on attendance. Using the quadratic specification, we derive the most cost-effective daily transfer amount that maximizes attendance per program cost. The cost-effective transfer amount is particularly important for countries that have limited resources and need to maximize the bang for the buck. Our analysis indicates that the most cost-effective transfer is approximately one quarter of a child's daily wages in Bangladesh.

Overall, the current study highlights the positive and persistent attendance impacts of CCT and SMS interventions. Both interventions can be viable policy options, though the actual policy choice should depend on the time horizon for decision making, resource constraints, data collection infrastructure, and how non-attendance outcomes are evaluated. We also find that one can improve the cost-effectiveness of CCT interventions by adequately calibrating the daily transfer amount as the attendance impacts are non-linear. These are especially relevant considerations for the developing world, where resources for policy interventions are typically limited. Our study provides the first set of insights into how policymakers could make a positive impact on school attendance and other outcomes cost-effectively. More research would be needed to shed further light on designing cost-effective policies, possibly with at-scale interventions. With a rapid expansion of affordable digital education technologies and mobile financial services, there will be ample opportunities to conduct future research—harnessing new technologies to improve education in developing countries with low secondary school attendance rates.

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[For Online Publication]

Appendix

A Documentation of Error Rates in SMS

The households in the CCT and SMS treatment arms received the previous school week's attendance information every week through both text messages and voice calls. The information was sent out manually in the first three phases of the intervention—2017-I, 2017-II, and 2018-I, and then the process was automated in 2018-II. There were unfortunately a few implementation issues in the first three phases. First, the implementing partner failed to keep records of SMSes sent in 2017. Nevertheless, given that around 70 percent of respondents stated that they kept the last SMS in the disbursement surveys, it is likely that most SMSes were received by the target households in 2017. Second, in the first five weeks of 2018-I, weekly voice calls were made, but text messages were not sent, because of the operational difficulties due to the absence of a key personnel and nationwide teacher strikes, which led to irregular working hours in schools.[39] Third, we also discovered that some of the SMSes sent to the study participants in 2018-I contained errors. Once these issues were discovered, we immediately conducted an audit to assess the prevalence of errors by checking the SMS against the attendance records in the subsequent weeks of 2018-I. Based on this audit exercise, the error rates in attendance information and CCT amounts were estimated at around 2 and 4 percent, respectively. We found no significant difference in the error rates in attendance information across Gain, Loss, and SMS groups. We also found no significant differences in the error rates in CCT amounts between the Gain and Loss groups.[40] Since the error rates are very small and seem random, they are unlikely to affect our results.

Despite these issues, our results still identify the intention-to-treat effects of our interventions. If anything, we conjecture that the partial omission of weekly SMSes and incorrect attendance information, which can be seen as a noise, would attenuate the estimated effects of our interventions (see also footnote 39). On the other hand, our experience also underscores the operational challenges to ensure that SMSes are accurate and delivered on time. Similar challenges were also noted in the literature (Berlinski et al., 2021; Bryan et al., 2019; Bauchet et al., 2015; Banerjee et al., 2007). Partly due

[39] SMSes were sent from the sixth week of intervention. There were 21 to 23 intervention days, depending on the class, in the first five weeks of 2018-I (out of 50 intervention days). Sensitivity analyses using daily morning attendance and the main empirical model in eq. (1) conducted separately for the first five weeks and the last seven weeks of 2018-I yielded similar results to those reported in Column (1) of Table II, albeit with somewhat smaller estimates for the first five weeks.

[40] Approximately a quarter of SMSes sent did not have a specific phone number in the backed-up SMS log, possibly due to non-delivery issues. Hence, the analyses were done on the three-quarters of SMSes that had a specific phone number attached.

to the issues described above and partly due to better funding availability, we automated the process of sending SMSes in 2018-II. This increased the reliability of the information in the SMS. As Table A4 shows, the impact of the SMS treatment in 2018-II is highly significant and larger than previous phases, in terms of magnitude. These results are also consistent with our conjecture that the estimated effects in the first three phases may have been attenuated.

B Sensitivity and Heterogeneity Analyses

In this Appendix, we conduct a variety of sensitivity analyses to verify the robustness of our main results. We also examine impact heterogeneity by different characteristics such as pre-intervention attendance, distance from school and parental education and socioeconomic status of the household as measured by predicted consumption.

Pure Experimental Design

We first consider the pure experimental design by removing the unbalanced controls X_{ic} and the fixed effects u_c from model (1) and also present the p-values from the Westfall-Young correction for multiple hypothesis testing. As Table A8 shows, our results are very similar to those in Table II even though the SMS treatment effect appears to be marginally weaker.

Difference-in-differences Estimation

We next consider a DiD specification with individual-level fixed effects using monthly attendance data from official school records before the start of the intervention and during the intervention (including monthly attendance for non-intervention days). This specification has the advantage of being able to control for all time-invariant individual characteristics that affect attendance.

The DiD specification uses monthly data and takes the following form:

$$\begin{aligned} \mathrm{Y}_{it} &= \alpha_0 + \alpha_1 \mathrm{Gain}_i \times \mathrm{TreatmentYear}_{it} + \alpha_2 \mathrm{Loss}_i \times \mathrm{TreatmentYear}_{it} \\ &\quad + \alpha_3 \mathrm{SMS}_i \times \mathrm{TreatmentYear}_{it} + u_i + v_t + \epsilon_{it}, \end{aligned} \tag{A1}$$

where Y_{it} is the monthly attendance rate of individual i at time period t (i.e., proportion of attended school days among all school days in a given month), which is defined by the year-month combination. The reference category is monthly attendance rate for the year 2016 [2017] for the old [new] cohort. The treatment year indicator $\mathrm{TreatmentYear}_{it}$ takes unity in both 2017 and 2018 [only 2017] for the old [new] cohort and zero otherwise. Note that the old cohort includes only those who were in grade 7 as of 2017, because pre-intervention attendance records are unavailable for the old cohort students who were in grade 6 as of 2017 (and thus in primary school in 2016). We denote the individual- and year-month-specific fixed effects by u_i and v_t, respectively. The error term ϵ_{it} is clustered at the individual level.

The regression results based on eq. (A1) are reported in Table A9. From Column (1), the results are qualitatively similar to those in Table II. Because the proportion of intervention days among all school days varies across different months, we also consider a specification in which the interaction terms in eq. (A1) are further multiplied by the fraction of intervention days among all school days in the given calendar month, denoted by TrIntensity_{it}. Column (2) shows that the effects of CCT treatments are similar while the effect of SMS treatment is lower and close to zero. Finally, we also include monthly attendance rates for 2019 in the analysis. As shown in Columns (3) and (4), the addition of 2019 in the analysis does not change the estimated impacts of our treatments on monthly attendance rates by much during the intervention years. In addition, Column (3) also indicates that the impacts of the Loss and SMS treatments are persistent even though the point estimates are only marginally significant.

Attrition Rates

There were 79 students who discontinued from our study in either 2017 or 2018, out of which 44 and 35 are from the old and new cohorts, respectively. Discontinuation may occur due to school dropout, transfer, or possibly other reasons. Therefore, our results are potentially affected by differential discontinuation rates across different treatment arms. As Table A10 shows, there were no significant differences in the discontinuation rates across the four treatment arms.

Since students who discontinued their schooling do not attend our study schools, they are treated to be absent from school after discontinuation in the main text. Nevertheless, we are unable to exclude the possibility that some of them may be attending a different school. To partly address this issue, we re-estimate the impact of our interventions on attendance without the records for the discontinued students using the baseline specification in eq. (1). As Table A11 shows, the point estimates become slightly higher for the Gain, Loss, and SMS treatment arms.[41]

[41] Discontinuation is likely to be an inaccurate measure because it is generally difficult to distinguish between long-term absence and dropout. We therefore also used an alternative definition of attrition, where an individual is identified as missing if the endline survey was not administered to the individual. Based on this definition, there were 16 missing students, 8 from the old cohort and 8 from the new cohort. Using this alternative definition does not change the results qualitatively.

Accounting for Peer Effects

Anecdotal evidence gathered through informal interactions with some study participants suggests that they tend to make a collective, rather than individual, decision with their friends to attend or skip school. Therefore, it is important to account for the potential presence of spillover effect arising from peer interactions. If the peer effect on attendance is positive and unilateral from the treatment groups to the control group, then the estimates presented so far would understate the true impact of our interventions. On the other hand, if the peer effect is similar across all treatment arms, the estimates would reflect the true treatment impact net of spillovers. The latter possibility is more plausible since treatment assignment is random and thus the impact of our intervention on peers can be expected to be similar across all treatment arms.

To understand the potential relevance of the peer effects, we collected social network data at the baseline—the names of the student participants' five best friends from the same class regardless of whether the friends are participating in our study. All survey respondents gave exactly five names.[42] The names of the reported friends were matched to those of the study participants within each class by engaging research assistants who are proficient in Bengali. The match was imperfect because of variations in the spelling of names, even though we have no reason to believe that the errors in matching differ across different treatment arms.

After matching was completed, we computed the proportion of friends who were in each treatment arm. We denote the proportion of the five best friends who are in the Gain treatment arm by GainProp and use similar notations for other treatment arms. For example, suppose that the names of four out of five best friends for a given study participant were matched within the same class and assume that he/she has two, one, one, and zero friends from the Gain, Loss, SMS, and Control treatment arms, respectively. Then, we have: GainProp = 0.4, LossProp = 0.2, SMSProp = 0.2, and ControlProp = 0.0, respectively. Note that the sum of these proportions is not necessarily equal to one, because there may be some friends who could not be matched due to the variations in the spelling of their names or because they were not part of our sample.

Using these data, we test the hypothesis that having a higher proportion of friends in the CCT or SMS treatment arms generates a positive spillover effect on attendance. Specifically, we adopt the baseline specification in eq. (1) using the data for both cohorts and additionally controlling for the proportion of friends in each treatment arm. Table A12 reports the effects of CCTs and SMSes on daily morning attendance, controlling for the proportion of friends in different treatment arms. The effects of being in the Gain, Loss, and SMS treatment arms are very similar to those reported in Column

[42] Because of the large number of students involved and limited budget available to us, it was infeasible to collect complete social network data. We therefore focus on best friends, because the peer effect is likely to be most relevant for best friends.

(1) of Table II. Nevertheless, we still find some evidence of significant peer effects on attendance. In particular, we find significant peer effect in attendance for the Gain and SMS treatment arms. In a separate ongoing project, we examine the relevance of our intervention to the social networks.

Heterogeneity Analyses

Restricting all the coefficient estimates to be identical across the entire sample masks various types of impact heterogeneity that may exist. For example, the baseline specification in eq. (1) does not capture the variation in treatment effects by pre-intervention attendance, distance from school, education of fathers and mothers, and socioeconomic status of the household (as measured by predicted household consumption).[43] Therefore, we use the baseline specification in eq. (1) and conduct sub-sample analyses along these observable attributes. We found no statistically significant difference across these dimensions (the detailed results are available upon request).

C Cost-Effectiveness Analysis

Summary of Back-of-the-envelope Calculations

We consider the following three cost scenarios: (1) actual program costs, (2) policy costs without digital support, and (3) policy costs with digital support. For each of the three cost scenarios, we examine the following program costs relevant to both the SMS and CCT treatments: (A) cost of communication which includes the cost of sending weekly SMSes to CCT and SMS participants; (B) cost of collection of attendance data from schools; (C) cost of digitization of attendance data; (D) compensation to teachers for collecting morning attendance data; and (E) compensation to senior students for collecting afternoon attendance data. In addition, we also consider the following cost components for the CCT interventions: (F) actual disbursed cash amount; (G) transportation costs for cash disbursement; (H) enumerator compensation for cash disbursement; (I) research assistant compensation for cash disbursement; and (J) processing costs, which include accountant salary and bank charges for cash disbursement.

The cost components (B), (D), and (E) are included only in Scenario (1), since attendance data are collected as part of teachers' duties. Further, the cost component (C) is not included in Scenario (3), because the data are already digitized at the time of data entry by teachers. Cost components (F), (G), (H), (I), and (J) are only relevant for the CCT treatments. In Scenario (3), cost components (G), (H), and (I) are irrelevant,

[43] To save the cost of household survey, we chose to have a short consumption survey and predicted the household consumption using the coefficient estimates from a separate dataset collected in the same region.

because money is transferred through mobile banking. Because of this, the processing cost in Scenario (3) is higher than those in Scenarios (1) and (2). The cost figures and the cost-effectiveness measures used in this study are given in Table A18.

In our back-of-envelope calculations of the cost-effectiveness measure λ, we assume that the attendance impact of our interventions is unaffected by the scenarios. While this assumption would be reasonable given the design of our interventions, we can potentially improve it with the help of technology. For example, we may be able to increase payment frequency so that the average time lag between the attendance decision and cash transfer is shorter and thus the average present value of the cash transfer is larger. With mobile baking, payment may be made monthly or even more frequently. This can improve λ in Scenario (3), since high-frequency payment is unlikely to add much cost.

Robustness Check on Cost-Effective Amount

We conduct robustness checks on the most cost-effective amount using the alternative specifications. First, we drop CCT_{ic} from eq. (3) and replace it with a cubic term of the daily transfer amount τ. This specification enables more flexibility in τ than the quadratic form, but assumes that the transfer has no direct impact in the extensive margin. As Figure A1 shows, the marginal impact of the cash transfer on attendance tends to diminish beyond 10 taka per day and we observe the maximum impact at around 31 taka per day. As before, we note that τ takes values between 10 taka/day and 30 taka/day in our intervention. Hence, the results should be interpreted with caution. Figure A2 shows that the most cost-effective CCT amount ranges between 22 and 24 taka per student per intervention day.

Second, since information is also embedded within the CCT treatment arm, we consider a specification where the control group is dropped from the sample. That is, we estimate eq. (3) using a subsample of participants in the CCT and SMS treatment arms only and without the SMS treatment arm indicator, SMS_{ic}. In this specification, $f(\tau)$ could be interpreted as the pure effect of cash transfers conditional on households receiving SMS. Figure A3 demonstrates that we still have evidence of diminishing marginal impact of the transfer amount on attendance with a maximum impact at around 32 taka/day. Figure A4 shows that the cost-effective amount of transfer is roughly around 24 taka per student per intervention day, regardless of the expected attendance A. Taken together, our results presented in Section 6 remain similar even when we conduct the cost-effective analysis under alternative assumptions.

D Supplementary Figures and Tables

Table A1. Sample Size by Cohort, Grade, Gender

		Grade				
Cohort		6	7	8	9	Total
Old	Male	157	40	—	—	197
	Female	163	40	—	—	203
	Total	320	80	—	—	400
New	Male	—	—	105	100	205
	Female	—	—	101	93	194
	Total	—	—	206	193	399

Note: One student in the old cohort was mistakenly re-listed in the roster for the new cohort, and was dropped from the new-cohort sample.

Table A2. Summary Statistics and Balance Check for Old Cohort

Variable	Gain (1)	Loss (2)	SMS (3)	Control (4)	Overall (5)	Orthogonality† (6)
Household head (male) has at least primary education[a]	0.420	0.450	0.410	0.440	0.430	0.939
Household head (male) has at least secondary education[a]	0.080	0.100	0.080	0.070	0.083	0.891
Spouse (female) has at least primary education[a]	0.420	0.400	0.380	0.420	0.405	0.929
Spouse (female) has at least secondary education[a]	0.030	0.060	0.030	0.080	0.050	0.287
Household size	4.840	4.680	4.860	4.780	4.790	0.733
Male members in household	2.440	2.350	2.470	2.410	2.418	0.852
Female members in household	2.400	2.330	2.390	2.370	2.372	0.964
Owns residential land	0.940	0.980	0.990	0.980	0.973	0.138
Owns agricultural land	0.290	0.250	0.340	0.230	0.278	0.319
Has television or radio	0.350	0.410	0.450	0.480	0.423	0.276
Has a bicycle	0.310	0.400	0.340	0.370	0.355	0.582
Has a tube well	0.950	0.940	0.950	0.970	0.952	0.791
Has an electric fan	0.710	0.690	0.730	0.770	0.725	0.627
Weight of the child	142.037	139.370	143.332	142.443	141.796	0.218
Height of the child	55.920	54.870	56.430	56.080	55.825	0.218
Standardized test score	0.000	0.101	0.115	0.000	0.054	0.780
Observations	100	100	100	100	400	0.895

Note: Ownership of assets (agricultural land, radio/television, bicycle, tube well, electric fan) is a binary variable that takes unity if the household owns the asset, and zero otherwise. The weight of the child is measured in kilograms, and the height of the child is measured in centimeters. Test scores are normalized relative to control mean and standard deviation. Column (5) shows the mean value for each variable. Column (6) shows the p-value for joint orthogonality.

[a]: For male headed households, the first two rows are indicator variables that take unity if the household head has at least primary / secondary education, and zero otherwise, and the next two rows are the indicator variables that take unity if the spouse of the household head has at least primary / secondary education, and zero otherwise, conditional on him/her living in the same household. For about 7 percent of households in the old cohort that are headed by female, we instead use the male spouse's education for the first two rows and female household head's education for the next two rows.

Table A3. Summary Statistics and Balance Check for New Cohort

	Gain (1)	Loss (2)	SMS (3)	Control (4)	Overall (5)	Orthogonality[†] (6)
Household head (male) has at least primary education[a]	0.430	0.400	0.480	0.424	0.434	0.713
Household head (male) has at least secondary education[a]	0.070	0.100	0.140	0.121	0.108	0.424
Spouse (female) has at least primary education[a]	0.390	0.460	0.480	0.556	0.471	0.136
Spouse (female) has at least secondary education[a]	0.040	0.060	0.080	0.061	0.060	0.704
Household size	4.710	4.520	4.650	4.737	4.654	0.561
Male members in household	2.580	2.320	2.370	2.566	2.459	0.168
Female members in household	2.120	2.200	2.280	2.172	2.193	0.668
Owns residential land	0.990	0.980	0.990	0.980	0.985	0.877
Owns agricultural land	0.220	0.320	0.380	0.253	0.293	0.061
Has television or radio	0.350	0.430	0.500	0.535	0.454	0.044
Has a bicycle	0.550	0.520	0.490	0.596	0.539	0.486
Has a tube well	0.960	0.990	0.970	0.980	0.975	0.567
Has an electric fan	0.810	0.840	0.860	0.879	0.847	0.575
Weight of the child	41.340	40.010	41.630	41.717	41.173	0.318
Height of the child	148.565	149.809	146.152	146.037	147.645	0.258
Standardized test score	-0.200	-0.087	-0.257	0.000	-0.136	0.232
Observations	100	100	100	99	399	0.032

Note: Ownership of assets (agricultural land, radio/television, bicycle, tube well, electric fan) is a binary variable that takes unity if the household owns the asset, and zero otherwise. The weight of the child is measured in kilograms, and the height of the child is measured in centimeters. Test scores are normalized relative to control mean and standard deviation. Column (5) shows the mean value for each variable. Column (6) shows the *p*-value for joint orthogonality.

[a]: For male headed households, the first two rows are indicator variables that take unity if the household head has at least primary / secondary education, and zero otherwise, and the next two rows are the indicator variables that take unity if the spouse of the household head has at least primary / secondary education, and zero otherwise, conditional on him/her living in the same household. For about 8 percent of households in the new cohort that are headed by female, we instead use the male spouse's education for the first two rows and female household head's education for the next two rows.

Table A4. Effects of CCTs and SMSes by Phase

Dependent variable	Morning				Morning & Afternoon			
	2017-I	2017-II	2018-I	2018-II	2017-I	2017-II	2018-I	2018-II
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gain	0.029	0.031	0.101	0.208	0.041	0.058	0.096	0.240
	(0.034)	(0.035)	(0.029)	(0.029)	(0.038)	(0.037)	(0.028)	(0.029)
Loss	0.030	0.039	0.091	0.226	0.055	0.057	0.091	0.259
	(0.031)	(0.033)	(0.029)	(0.030)	(0.035)	(0.035)	(0.029)	(0.028)
SMS	0.047	0.034	0.046	0.058	0.066	0.045	0.046	0.067
	(0.033)	(0.034)	(0.030)	(0.028)	(0.037)	(0.036)	(0.029)	(0.026)
P(Gain = Loss)	0.969	0.823	0.733	0.556	0.720	0.984	0.883	0.550
P(Gain = SMS)	0.611	0.925	0.073	0.000	0.527	0.739	0.100	0.000
P(Loss = SMS)	0.613	0.897	0.152	0.000	0.760	0.749	0.141	0.000
Observations	24,000	19,600	39,950	39,950	24,000	19,600	39,950	39,950
R^2	0.066	0.062	0.050	0.077	0.092	0.103	0.048	0.094
Control Mean	0.612	0.661	0.545	0.415	0.476	0.577	0.497	0.323

Note: Each column indicates the point estimates of the ITT effect in different phases. The first four columns are for "Morning" attendance and the last four columns are for "Morning & Afternoon" attendance. "Morning" takes unity if the child was present in school in the morning, and zero otherwise. "Morning and Afternoon" takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the test of equality of means between two different treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

Table A5. Are the Impact Estimates Higher when Closer to Disbursement?

	Morning		Afternoon		Morning & Afternoon		Random Visit	
	First (1)	Second (2)	First (3)	Second (4)	First (5)	Second (6)	First (7)	Second (8)
Gain	0.087	0.128	0.099	0.142	0.100	0.146	0.080	0.106
	(0.025)	(0.026)	(0.026)	(0.027)	(0.026)	(0.026)	(0.026)	(0.028)
Loss	0.085	0.140	0.107	0.151	0.105	0.156	0.113	0.145
	(0.025)	(0.025)	(0.024)	(0.026)	(0.025)	(0.025)	(0.027)	(0.029)
SMS	0.041	0.055	0.048	0.063	0.047	0.065	0.051	0.090
	(0.024)	(0.025)	(0.025)	(0.026)	(0.025)	(0.025)	(0.028)	(0.030)
P(Gain=Loss)	0.945	0.683	0.759	0.747	0.850	0.732	0.219	0.183
P(Gain=SMS)	0.088	0.009	0.068	0.006	0.055	0.005	0.315	0.586
P(Loss=SMS)	0097	0.002	0.027	0.002	0.030	0.001	0.023	0.064
Observations	61,710	61,790	61,710	61,790	61,710	61,790	4,806	4,063
R^2	0.071	0.060	0.078	0.082	0.075	0.082	0.043	0.044
Control Mean	0.544	0.525	0.482	0.481	0.453	0.445	0.614	0.594

Note: The dependent variable is daily attendance during our intervention period. Each phase is broken down into two parts—first and second. Since 2017-I phase has 60 intervention days, each part consists of 30 days. Since 2017-II, 2018-I, and 2018-II phases all have 50 intervention days, each part in these phases consists of 25 intervention days. The Control group is the reference category in all regressions. The *p*-values for the tests of equality of means between two different treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

Table A6. Impact Heterogeneity by Consumption Level and Time to Disbursement

Dependent variable	Morning Attendance			
	Lowest quartile consumption		Top three quartiles consumption	
	First Half (1)	Second Half (2)	First Half (3)	Second Half (4)
Gain	0.062	0.061	0.093	0.148
	(0.050)	(0.052)	(0.028)	(0.028)
Loss	0.071	0.103	0.091	0.154
	(0.049)	(0.049)	(0.028)	(0.028)
SMS	0.045	0.054	0.042	0.056
	(0.054)	(0.055)	(0.027)	(0.028)
P(Gain=Loss)	0.859	0.434	0.955	0.850
P(Gain=SMS)	0.756	0.896	0.086	0.003
P(Loss=SMS)	0.629	0.387	0.100	0.002
Observations	15,454	15,476	46,256	46,314
R-squared	0.074	0.074	0.075	0.064
Control Mean	0.496	0.486	0.557	0.535

Note: The dependent variable is daily morning attendance during our intervention period. Each phase is broken down into two parts—first and second. Since 2017-I phase has 60 intervention days, each part consists of 30 days. Since 2017-II, 2018-I, and 2018-II phases all have 50 intervention days, each part in these phases consists of 25 intervention days. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between two different treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

Table A7. Associations between Attendance and CCT Recollection prior to Disbursement

Dependent variable	Phase Morning Attendance Rate			
	(1)	(2)	(3)	(4)
Remembers CCT	0.182	0.145	0.145	0.069
	(0.022)	(0.024)	(0.024)	(0.023)
Kept SMS		0.062	0.062	0.021
		(0.022)	(0.022)	(0.018)
Loss			-0.001	
			(0.023)	
Observations	1,137	1,137	1,137	1,137
R^2	0.083	0.090	0.090	0.775
Household FE	No	No	No	Yes
Phase FE	Yes	Yes	Yes	Yes

Note: The sample used in the above regressions is the set of households that belong to the Gain and Loss treatment arms. "Morning Attendance Rate" is the ratio of the number of intervention days present in the morning in a phase over the total number of intervention days in a given phase. "Remember CCT" takes unity if the interviewee (often the head of the household) remembers the amount due, and zero otherwise. "Kept SMS" takes unity if the interviewee stated that they kept the last SMS, and zero otherwise. "Loss" is a indicator variable that takes unity if the child belongs to the Loss treatment group, and zero otherwise. Households belonging to the Gain treatment arm form the reference category. All the specifications control for phase fixed effects. Standard errors (in parentheses) are clustered at the individual level.

Table A8. Treatment Effect for All Students: Pure Experimental Design

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random visit (4)
Gain	0.100	0.115	0.118	0.085
	(0.025)	(0.026)	(0.026)	(0.026)
	[0.000]	[0.000]	[0.000]	[0.001]
Loss	0.105	0.123	0.125	0.121
	(0.025)	(0.026)	(0.026)	(0.026)
	[0.000]	[0.000]	[0.000]	[0.000]
SMS	0.035	0.043	0.044	0.063
	(0.025)	(0.025)	(0.025)	(0.026)
	[0.145]	[0.093]	[0.093]	[0.024]
P(Gain = Loss)	0.861	0.785	0.826	0.178
P(Gain = SMS)	0.017	0.011	0.008	0.394
P(Loss = SMS)	0.010	0.005	0.004	0.025
Observations	123,500	123,500	123,500	8,869
R^2	0.008	0.011	0.011	0.009
Control mean	0.534	0.481	0.449	0.605

Note: "Morning" takes unity if the child was present in school in the morning, and zero otherwise. "Afternoon" takes unity if the child was present in school in the afternoon, and zero otherwise. "Morning and Afternoon" takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. "Random visit" takes unity if the child was present in school on the day of random visit, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the test of equality of means between two different treatment arms are given in the middle panel. Standard errors (in parentheses) are clustered at the individual level. The p-values for Westfall-Young correction for multiple hypothesis testing are given in square brackets below the standard errors.

Table A9. Difference-in-Differences with Individual Fixed Effects

	2016–2018		2016–2019	
Dependent variable: Monthly attendance rate	(1)	(2)	(3)	(4)
Gain × TreatmentYear	0.106		0.110	
	(0.029)		(0.027)	
Loss × TreatmentYear	0.125		0.131	
	(0.029)		(0.028)	
SMS × TreatmentYear	0.027		0.030	
	(0.026)		(0.025)	
Gain × TreatmentYear × TrIntensity		0.128		0.134
		(0.023)		(0.023)
Loss × TreatmentYear × TrIntensity		0.134		0.141
		(0.024)		(0.023)
SMS × TreatmentYear × TrIntensity		0.005		0.007
		(0.022)		(0.022)
Gain × 2019			0.047	0.025
			(0.031)	(0.025)
Loss × 2019			0.057	0.023
			(0.034)	(0.026)
SMS × 2019			0.052	0.034
			(0.031)	(0.026)
Observations	14,176	14,176	20,286	20,286
R^2	0.491	0.494	0.456	0.458

Note: Columns (1) and (3) are based on standard DiD specifications. Columns (2) and (4) control for the intensity of treatment within a month using the fraction of intervention days. The outcome variable in both specifications is monthly attendance rate (the total number of days present divided by the total number of days that schools were open in a given month). The Control group is the reference category in all regressions. The above specifications control for the household and year-month fixed effects. TreatmentYear is an indicator function that takes unity if the individual belongs to the old cohort and the attendance data is for the year 2017/2018, or the individual belongs to the new cohort and the attendance data is for the year 2018. TrIntensity denotes the fraction of intervention days in the number of school days in a given month. Standard errors (in parentheses) are clustered at the individual level.

Table A10. Does Discontinuity Matter?

Dependent variable	Discontinued		
	Old Cohort	New Cohort	Both cohorts
	(1)	(2)	(3)
Gain	-0.029	-0.030	-0.028
	(0.032)	(0.030)	(0.022)
Loss	0.005	-0.003	0.002
	(0.033)	(0.052)	(0.029)
SMS	-0.030	0.004	-0.013
	(0.037)	(0.034)	(0.025)
P(Gain = Loss)	0.237	0.556	0.245
P(Gain = SMS)	0.826	0.416	0.466
P(Loss = SMS)	0.241	0.786	0.479
Observations	400	399	799
R^2	0.167	0.077	0.124
Control Mean	0.110	0.091	0.101

Note: The dependent variable "Discontinued" is a indicator variable that takes unity if the individual left the study at any point during the two year intervention period, and zero otherwise. There were 79 such students—44 from the old cohort and 35 from the new cohort. The Control group is the reference category in all regressions. The above specifications control for class fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. The p-values for the test of equality of means between two different treatment arms are given in the middle panel. Standard errors (in parentheses) are clustered at the class level.

Table A11. Treatment Effect for Continued Students

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain	0.117	0.134	0.136	0.108
	(0.022)	(0.024)	(0.024)	(0.026)
Loss	0.138	0.157	0.159	0.146
	(0.021)	(0.023)	(0.023)	(0.025)
SMS	0.060	0.071	0.071	0.085
	(0.022)	(0.023)	(0.023)	(0.026)
P(Gain = Loss)	0.378	0.341	0.376	0.133
P(Gain = SMS)	0.014	0.012	0.010	0.377
P(Loss = SMS)	0.001	0.000	0.000	0.015
Observations	110,800	110,800	110,800	8,460
R^2	0.063	0.077	0.077	0.043
Control Mean	0.570	0.513	0.480	0.604

Note: Discontinued students are those who left the study at any point during the two year intervention period. There were 79 such students—44 from the old cohort and 35 from the new cohort. This analysis drops such students. “Morning” takes unity if the child was present in school in the morning, and zero otherwise. “Afternoon” takes unity if the child was present in school in the afternoon, and zero otherwise. “Morning and Afternoon” takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. “Random visit” takes unity if the child was present in school on the day of random visit, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the test of equality of means between two different treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

Table A12. Accounting for Peer Effects

Dependent variable	Morning	Afternoon	Morning & Afternoon	Random Visit
Gain	0.113	0.129	0.131	0.098
	(0.025)	(0.026)	(0.026)	(0.026)
Loss	0.114	0.132	0.133	0.129
	(0.024)	(0.025)	(0.024)	(0.025)
SMS	0.045	0.054	0.054	0.071
	(0.024)	(0.024)	(0.024)	(0.026)
GainProp	0.156	0.214	0.204	0.145
	(0.059)	(0.061)	(0.061)	(0.059)
LossProp	0.031	0.027	0.017	0.039
	(0.067)	(0.068)	(0.067)	(0.067)
SMSProp	0.224	0.278	0.266	0.180
	(0.064)	(0.066)	(0.066)	(0.065)
ControlProp	0.035	0.067	0.063	0.057
	(0.062)	(0.063)	(0.063)	(0.064)
P(Gain = Loss)	0.981	0.918	0.948	0.232
P(Gain = SMS)	0.012	0.006	0.005	0.294
P(Loss = SMS)	0.009	0.003	0.003	0.022
Observations	123,500	123,500	123,500	8,869
R^2	0.064	0.078	0.076	0.040
Control Mean	0.534	0.481	0.449	0.605

Note: "Morning" takes unity if the child was present in school in the morning, and zero otherwise. "Afternoon" takes unity if the child was present in school in the afternoon, and zero otherwise. "Morning and Afternoon" takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. "Random visit" takes unity if the child was present in school on the day of random visit, and zero otherwise. The Control group is the reference category. GainProp denotes the proportion of best friends in the Gain treatment arm at the baseline. LossProp, SMSProp, and ControlProp are similarly defined for the Loss, SMS, and Control groups. The p-values for the test of equality of means between two different treatment arms are given in the middle panel. Standard errors (in parentheses) are clustered at the individual level. The above specification controls for the class and date fixed effects.

Table A13. The Effects of CCTs and SMSes in the First and Second Half of 2019

Dependent variable	Monthly Attendance Rates					
	First half			Second half		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.072	0.022	0.121	0.013	-0.025	0.050
	(0.023)	(0.033)	(0.029)	(0.022)	(0.031)	(0.030)
SMS	0.068	0.012	0.101	0.039	0.003	0.073
	(0.027)	(0.039)	(0.033)	(0.024)	(0.036)	(0.030)
P(CCT = SMS)	0.839	0.770	0.468	0.163	0.347	0.314
Observations	2,356	1,192	1,164	3,031	1,530	1,501
R^2	0.119	0.117	0.189	0.213	0.222	0.236
Control Mean	0.388	0.360	0.421	0.392	0.406	0.376

Note: Columns (1)–(3) ("First half") estimate the effect of the intervention on morning attendance in the months of January to April in 2019. Columns (4)–(6) ("Second half") estimate the effect of the intervention on morning attendance in the months of June to November in 2019. The months of May and December have been dropped from the analysis since there were very few school days in these months due to term break or examination. The Control group is the reference category in all specifications. The p-values for the test of equality of means between the CCT and SMS treatment arms are given in the middle panel. The above specifications control for class fixed effects, month fixed effects, and unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

Table A14. The Effects of CCTs and SMSes on Changes in Parents' Aspirations

	Continuous Outcome			Discrete Outcome		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.109	-0.003	0.195	-0.071	-0.098	-0.052
	(0.266)	(0.262)	(0.433)	(0.073)	(0.090)	(0.093)
SMS	0.237	0.267	0.197	0.029	0.099	-0.047
	(0.293)	(0.272)	(0.492)	(0.075)	(0.083)	(0.109)
P(CCT=SMS)	0.593	0.428	0.995	0.242	0.008	0.960
Observations	475	227	248	475	227	248
R^2	0.047	0.081	0.047	0.066	0.137	0.046
Control mean	0.028	0.000	0.055	0.454	0.396	0.509

Note: The dependent variable ("Continuous Outcome") in Columns (1)–(3) is the change between the baseline and endline surveys in the number of years of schooling that the responding parent expect the participating student to achieve. The dependent variable ("Discrete Outcome") in Columns (4)–(6) is an indicator that takes unity when the continuous outcome used in Columns (1)–(3) is positive and zero otherwise. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arms are given in the middle panel. The above specifications control for class fixed effects and unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the class level.

Table A15. The Effects of CCTs and SMSes on Child Marriage by Grade

Dependent variable	Child Marriage		
	Grade 7	Grade 8	Grade 9
	(1)	(2)	(3)
CCT	-0.062	-0.001	-0.230
	(0.032)	(0.026)	(0.075)
SMS	-0.045	-0.068	-0.232
	(0.081)	(0.058)	(0.054)
P(CCT=SMS)	0.801	0.331	0.944
Observations	158	141	92
R^2	0.035	0.031	0.177
Control Mean	0.083	0.105	0.261

Note: "Child Marriage" takes unity if the child was unmarried at the baseline and married at the endline, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arms are given in the middle panel. The above specifications control for class fixed effects and unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the class level.

Table A16. The Effects of CCTs and SMSes on Parental Investment in Education

	Continuous Outcome			Discrete Outcome		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.048	0.105	-0.004	0.010	0.021	0.009
	(0.063)	(0.061)	(0.093)	(0.039)	(0.050)	(0.039)
SMS	0.132	0.111	0.175	0.071	0.054	0.105
	(0.059)	(0.069)	(0.076)	(0.044)	(0.064)	(0.056)
P(CCT=SMS)	0.219	0.947	0.121	0.220	0.657	0.049
Observations	737	368	369	737	368	369
R^2	0.145	0.165	0.149	0.082	0.116	0.084
Control mean	0.211	0.267	0.157	0.646	0.700	0.593

Note: The dependent variable in Columns (1)–(3) is the change between the baseline and endline surveys in the amount of money spent on the education of the participating student adjusted for inflation. The dependent variable in Columns (4)–(6) is an indicator that takes unity when the continuous outcome used in Columns (1)–(3) is positive and zero otherwise. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between the CCT and SMS treatment arms are given in the middle panel. The above specifications control for class fixed effects and unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the class level.

Table A17. The Effects of Attendance on Mathematics Test Score

Dependent variable	Endline Score					
	All		Girls		Boys	
	(1)	(2)	(3)	(4)	(5)	(6)
Attendance Rate	-0.426	-0.586	-0.112	-0.417	-0.702	-0.951
	(0.426)	(0.475)	(0.957)	(0.810)	(1.244)	(1.214)
Baseline score	0.091	0.084	0.155	0.145	0.035	0.036
	(0.024)	(0.024)	(0.032)	(0.026)	(0.049)	(0.056)
Child is male		0.105				
		(0.116)				
Household head (male) has		0.063		0.065		0.057
primary education[a]		(0.035)		(0.094)		(0.062)
Household head (male) has		0.113		-0.183		0.448
secondary education[a]		(0.224)		(0.317)		(0.225)
Spouse (female) has		0.021		0.166		-0.089
primary education[a]		(0.086)		(0.101)		(0.099)
Spouse (female) has		0.254		0.416		0.100
secondary education[a]		(0.166)		(0.345)		(0.138)
Owns agricultural land		-0.107		-0.184		-0.055
		(0.089)		(0.125)		(0.113)
Owns radio or television		-0.143		-0.198		-0.105
		(0.045)		(0.044)		(0.123)
Weight of child		-0.001		-0.007		0.004
		(0.006)		(0.008)		(0.006)
Height of child		-0.002		-0.000		-0.007
		(0.012)		(0.012)		(0.014)
Kleibergen-Paap F Stat	16.12	19.15	13.76	14.47	5.67	5.86
Hansen J p-value	0.463	0.537	0.184	0.396	0.112	0.058
Observations	718	718	354	354	364	364
Control Variables	No	Yes	No	Yes	No	Yes

Note: The mathematics test were administered to 718 students at the time of the endline survey, when schools were closed. We could not administer the test to the remaining 81 students, because they were not at home when the research team visited the household to conduct the survey. Both baseline and endline test scores are normalized relative to control mean and standard deviation for every class combination. The above estimates are obtained from a two-stage least squares regression where the attendance rate during the intervention period is instrumented by the CCT and SMS treatment indicators. The Control group is the reference category in all regressions. Columns (1), (3), and (5) regress endline test score on baseline test score and treatment assignments. Columns (2), (4) and (6) add the class fixed effects and several additional individual and household characteristics as control variables. The Kleibergen-Paap Wald F stat for weak identification and the Hansen J statistic p-value for over identification are given in the middle panel. Robust standard errors are given in parentheses.

[a]: For male headed households, the household head (male) and spouse (female) refer to the household head's and spouse's education, respectively, conditional on the spouse living in the same household. For a small proportion of female headed households, we instead use the education of the male spouse and female household head, respectively.

Table A18. Cost Calculations under Different Scenarios

Scenario	Scenario (1)		Scenario (2)		Scenario (3)	
Cost components	Actual program cost		Policy cost (no digital supp.)		Policy cost (digital supp.)	
(taka per student)	SMS	CCT	SMS	CCT	SMS	CCT
(A) Communication	101	101	101	101	101	101
(B) Collection of attendance data	900	900	0	0	0	0
(C) Digitization of attendance Data	720	720	720	720	0	0
(D) Payment to teachers	48	48	0	0	0	0
(E) Payment to senior students	6	6	0	0	0	0
(F) Actual disbursed cash	0	1,764	0	1,764	0	1,764
(G) Travel (disbursement)	0	81	0	81	0	0
(H) Enumerator wage (disbursement)	0	135	0	135	0	0
(I) RA wage (disbursement)	0	63	0	63	0	0
(J) Processing	0	52	0	52	0	272
Total Cost	1,775	3,870	821	3,265	101	2,137
Effect Size	4.8	11.0	4.8	11.0	4.8	11.0
Cost-Effectiveness Measure (λ)	2.70	2.84	5.85	3.37	47.52	5.15
Alternative Cost-Effectiveness Measure ($\tilde{\lambda}$)	2.70	5.22	5.85	7.33	47.52	29.49

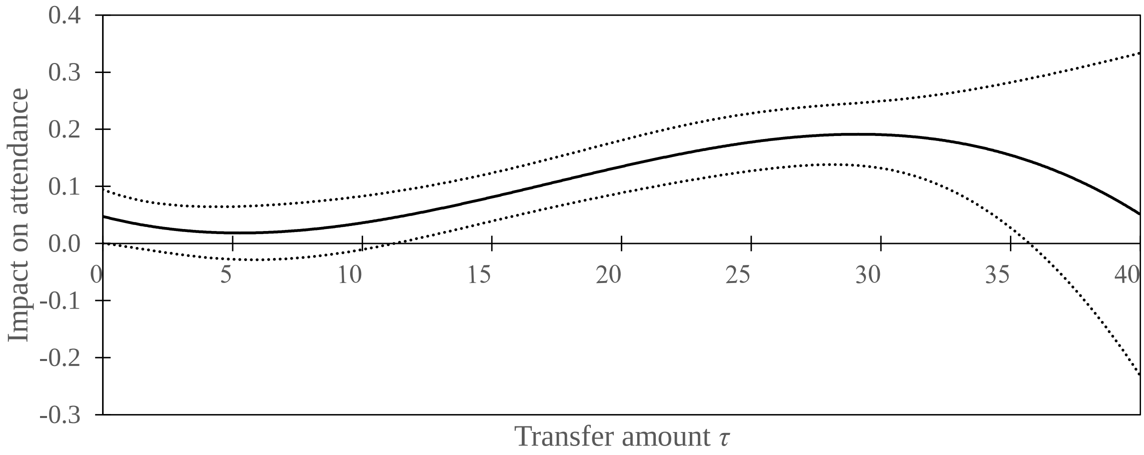
Note: "(A) Communication cost" includes the cost of sending weekly SMSes to CCT and SMS households. "(B) Collection of attendance data" covers the compensation paid to enumerators for visiting schools to collect attendance data, including their daily wage and transportation costs. "(C) Digitization of attendance Data" includes the compensation paid to enumerators for digitizing daily attendance data. "(D) Payment to teachers" is the compensation given to teachers for collecting daily morning attendance data. "(E) Payment to senior students" is the compensation given to senior students for collecting daily afternoon attendance data. "(F) Actual disbursed cash" is the actual amount of cash disbursed to CCT treatment arms over the two-year intervention period. "(G) Travel (disbursement)" includes the total compensation given to enumerators for making household visits for cash disbursement, and includes lunch and transportation costs for enumerators. "(H) Enumerator wage (disbursement)" is the total compensation given to enumerators for cash disbursement. Since we had two research assistants in our study, "(I) RA wage (disbursement)" is the compensation given to two research assistants for cash disbursement. "(J) Processing cost" is the compensation given to the accountant for calculating the cash balance of CCT treatment arms and also includes bank charges for transferring cash through mobile banking, if applicable. Effect size, taken from Column (1) of III, is expressed as percentage point increase in the attendance morning rate. Cost-effectiveness measure (λ) is the percentage point increase in attendance per program cost in thousand taka during the intervention period. λ is calculated by dividing Effect Size by Total Cost expressed in thousand taka. For example, λ for SMS under Scenario (1) is calculated as $4.8/1.775 = 2.70$. Alternative cost-effectiveness measure $\tilde{\lambda}$ is the same as λ, except that cost component (F) is excluded from total cost calculation.

Table A19. The Effects of CCTs at the Intensive Margin in 2018-II

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
CCT High	0.227	0.257	0.259	0.170
	(0.030)	(0.029)	(0.029)	(0.037)
CCT Low	0.204	0.232	0.237	0.141
	(0.030)	(0.029)	(0.029)	(0.037)
SMS	0.058	0.069	0.067	0.078
	(0.028)	(0.027)	(0.026)	(0.038)
P(CCT High = CCT Low)	0.473	0.442	0.495	0.419
P(CCT High = SMS)	0.000	0.000	0.000	0.015
P(CCT Low = SMS)	0.000	0.000	0.000	0.089
Observations	39,950	39,950	39,950	2,463
R^2	0.074	0.097	0.091	0.039
Control Mean	0.534	0.481	0.449	0.605

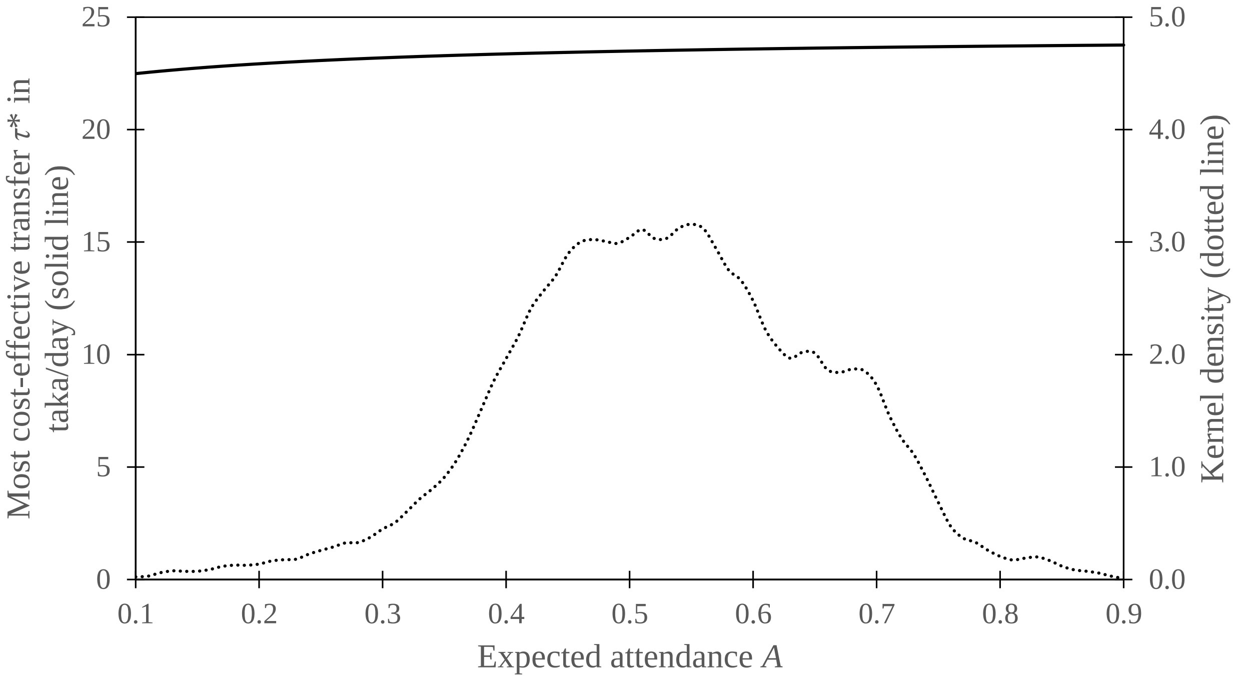
Note: “Morning” takes unity if the child was present in school in the morning, and zero otherwise. “Afternoon” takes unity if the child was present in school in the afternoon, and zero otherwise. “Morning and Afternoon” takes unity if the child was marked present both in the morning and afternoon attendance records, and zero otherwise. “Random visit” takes unity if the child was present in school on the day of random visit, and zero otherwise. The Control group is the reference category in all regressions. The p-values for the test of equality of means between two different (sub)treatment arms are given in the middle panel. The above specifications control for class and date fixed effects. They also control for unbalanced covariates at the baseline—ownership of agricultural land and radio/television. Standard errors (in parentheses) are clustered at the individual level.

Figure A1. The Estimated Attendance Impact of CCT using a Cubic Specification



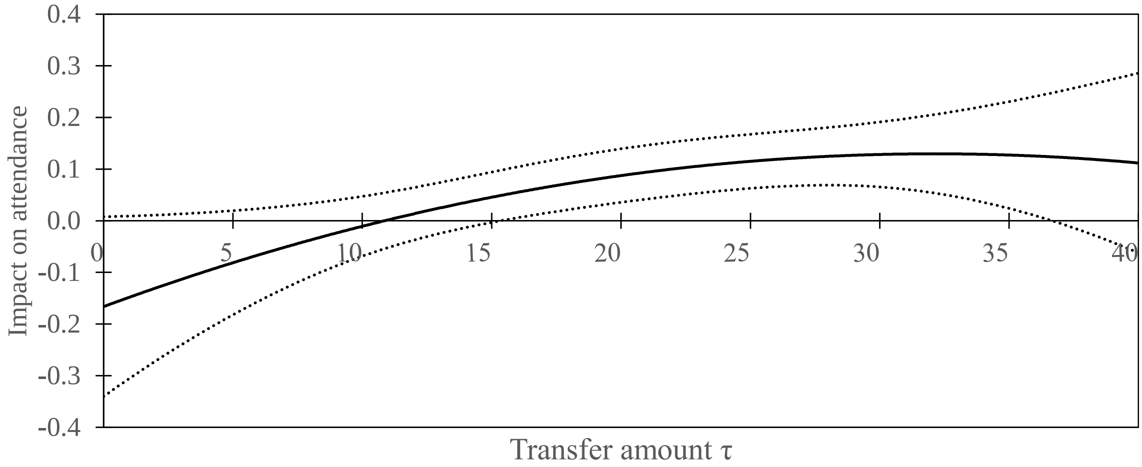
Note: The figure plots the estimated impact of CCT with daily transfer τ on daily morning attendance, $f(\tau)$, from a regression of eq. (3) with cubic—instead of quadratic—specification.

Figure A2. Most Cost-Effective Transfer using a Cubic Specification



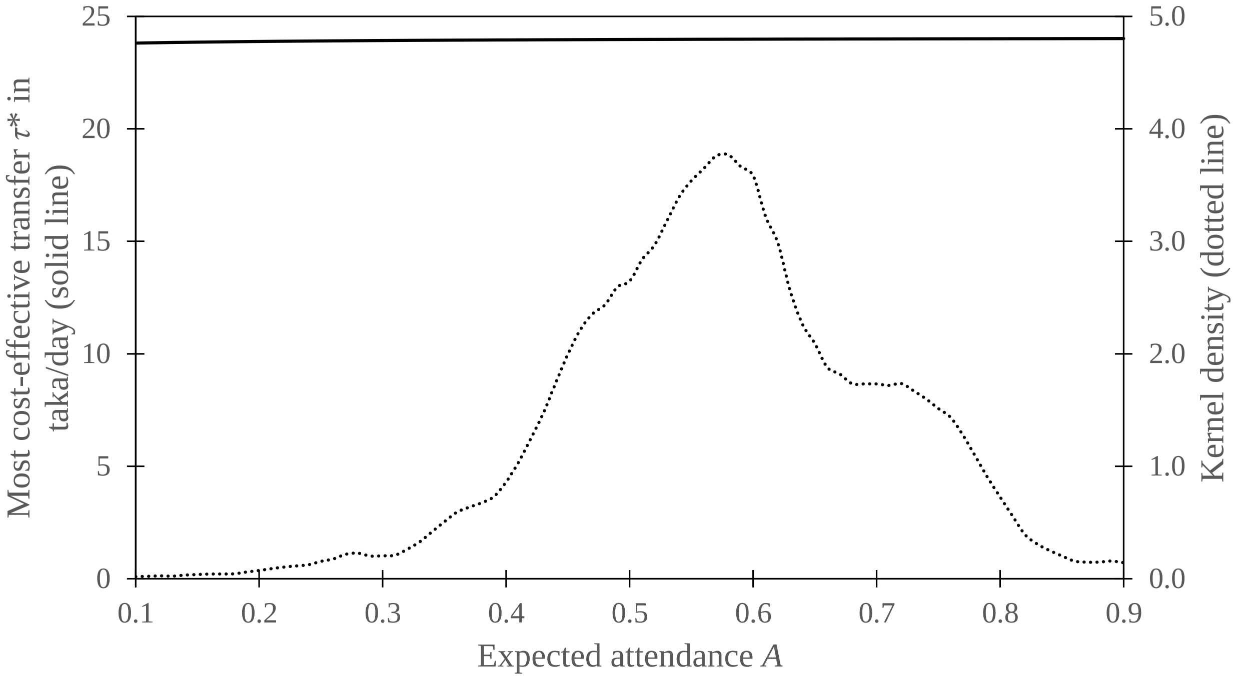
Note: The solid line depicts the most cost effective transfer, τ^*, as a function of control attendance, $A_{ict} \equiv \beta_0 + \gamma X_{ic} + u_c + v_t$, using a cubic specification. The dotted line depicts the kernel density estimate of control attendance A_{ict}.

Figure A3. The Estimated Attendance Impact of CCT: Treated Students' Subsample



Note: The figure plots the estimated impact of CCT with daily transfer τ on daily morning attendance, $f(\tau)$, from a regression of eq. (3) on the subsample of treated students in the CCT and SMS treatment arms only.

Figure A4. Most Cost-Effective Transfer: Treated Students' Subsample



Note: The solid line depicts the most cost effective transfer, τ^*, as a function of control attendance, $A_{ict} \equiv \beta_0 + \gamma X_{ic} + u_c + v_t$, based on a regression on the subsample of treated students in the CCT and SMS treatment arms only. The dotted line depicts the kernel density estimate of control attendance A_{ict}.