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 low-income countries due to cash and information constraints. Using a randomized field experiment, we compare the effectiveness of conditional cash transfer (CCT) with framing variations (gain vs loss) against high-frequency attendance information in a unified setting. CCT increases secondary school attendance by 12.4 percentage points, of which two fifths is attributable to the information component. These treatments further improve girls' academic aspirations and delay early marriage. Daily CCT set around 30% of child wage maximizes the contemporaneous attendance impact. Conversely, weekly attendance information via inexpensive mobile technology boosts attendance sustainably and cost-effectively. [100 words]

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, especially at the secondary school level, remains a chronic problem in many developing countries. The net secondary school attendance rate for students belonging to the lowest wealth quintile is only 46%, and much lower than the global average of 65% (UNICEF, 2022). Why does secondary school attendance remain low given that the return to education is considerably high, at 18.7% in low-income countries (Psacharopoulos and Patrinos, 2018)? Cash constraints could trigger absenteeism due to out-of-pocket educational expenses along with the high opportunity cost of schooling in resource-poor settings, whereby children are an essential source of labor for households' businesses, agricultural activities, and domestic work (Ito and Shonchoy, 2020). Absenteeism may also be driven by parental inattention and information constraints, possibly due to infrequent communication with children and schools (Escueta et al., 2020). In this study, we experimentally relax the cash and information constraints to examine their relative importance for school attendance. Specifically, we evaluate the effectiveness of providing conditional cash transfers (CCTs) with framing variations (gain vs loss) and high-frequency attendance information to parents in a unified setting.

Inspired by the earlier success of the pioneering *Progresa* program in Mexico, CCT has been a hugely popular policy instrument to ease cash constraints so as to promote schooling and other socially desirable behaviors such as regular visits to health facilities (Fiszbein et al., 2009; Gertler, 2004; Glewwe and Muralidharan, 2016). However, CCT programs are costly to implement or often financially unsustainable for low- and lower-middle-income countries. Moreover, CCT may operate through both the relaxation of cash constraints as well as the indirect information from the conditionality of cash transfers (Bursztyn and Coffman, 2012). Yet, existing literature has paid little attention to the implicit transfer of information in CCT programs for schooling, typically attributing all CCT effects to the relaxation of cash constraints rather than information constraints. If information constraints are relevant, then providing information to parents may boost children's school attendance inexpensively. Furthermore, CCT programs are traditionally "gain" framed, by enabling beneficiaries to gain rewards by performing a desired behavior. However, recent behavioral literature underscores the importance of "loss" framing, where beneficiaries lose money if they deviate from the required behavior, in generating higher responsiveness (Fryer Jr et al., 2022). Whether a "loss" framed CCT tied to school absenteeism can generate higher responsiveness for the same cost, remains an open question. 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 gain or loss framing of CCT transfer?

To answer our research question, we conduct a randomized controlled trial (RCT) over two years involving about 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 and voice messages containing existing information on their children's school attendance and absence; (ii) "Gain", in which households receive weekly text and voice messages and cash transfers in a gain-framed CCT program; (iii) "Loss", in which households receive weekly text and voice messages and cash transfers in a loss-framed CCT program; and (iv) "Control", in which households receive neither messages nor cash. The text and voice messages in Gain and Loss treatment arms contain the same attendance and absence information as the SMS treatment arm plus the cash transfer balance information to reinforce the framing. This randomized design enables us to rigorously compare the efficacy and cost-effectiveness of the transfers of information and cash, within the same low-income context. Our study further exploits extensive daily school attendance information (110,800 person-day records), collected from various sources–teachers, class representatives, and random visits–at different times of the day over two years. This allows us to cross-check the validity of the official administrative records and capture the possibility of partial attendance in a day.

We find that CCT (with Gain and Loss treatments combined) and SMS interventions increase attendance by 12.4 and 5.4 percentage points, respectively, from the mean of 57% attendance in the control group. These estimates indicate that about two fifths of the CCT impact is attributable to information. The Loss-framed CCT improves attendance by the greatest margin (13.3 percentage points), although the impact is not statistically different from the Gain-framed one (11.5 percentage points). We also use an achievement of 80% attendance–a common policy target (Fiszbein et al., 2009)–as an alternative outcome. Gain [Loss] treatment increases the likelihood of achieving 80% attendance by 24.4 [28.6] percentage points from the control mean of 13.4%, although the difference between the Gain and Loss estimates is not statistically significant. Conversely, SMS treatment increases the likelihood of achieving 80% attendance by 7.1 percentage points.

The results that both CCT and SMS treatments boost secondary school attendance, and that Loss framing generates the highest impacts, albeit not statistically significantly different from Gain framing, are robust to a battery of additional sensitivity analyses. These include using alternative attendance measures (e.g., morning, afternoon, random visits), accounting for the presence of spillover effects across students (i.e., we control for the proportion of classmates among the 5 closest in each treatment arm), and using alternative econometric specifications (e.g., pure experimental design, difference-in-differences, double-selection lasso regression). Although the size of our cash transfer (around 3.8% of per capita household pretransfer consumption) falls into the lower end of the generosity for CCT programs found elsewhere, our estimates are comparable to those from past literature.[1] The effects of our SMS treatment also compare favorably to those of the emerging literature on information interventions in developing countries.[2]

We further document notable gendered heterogeneity in the treatment effects of our interventions. Girls who received the SMS or CCT intervention achieved higher attendance, had greater educational aspirations, and were less likely to be married early. We also find that girls' parents in the SMS treatment invested significantly more in educational resources although we do not find any treatment impact on mathematics test scores or child labor. Interestingly, both treatments generate lasting impacts on girls' post-intervention school attendance, and we notice a convergence in the CCT and SMS

[1] Fiszbein et al. (2009) document the size of transfers as ranging from no more than 4% of mean household consumption in Honduras, Cambodia, and Pakistan to 20% in Mexico. Meanwhile, García and Saavedra (2017) report the CCT effects on attendance as 4.6 percentage points in Honduras, 1.7 to 7.65 percentage points in Mexico, and more than 27.8 percentage points in Cambodia.

[2] The impact of our information treatment falls into the upper end of estimates from literature: 0.9 percentage points in Chile (Berlinski et al., 2022), 2.1 percentage points in Brazil (Bettinger et al., 2021), and 4.5-6 percentage points in Mozambique (De Walque and Valente, 2023).

impacts in the year following the intervention. These results 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 individual and societal well-being, since 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).

Finally we conduct back-of-the-envelope calculations to compare the cost-effectiveness of CCT and SMS interventions within our low-income setting. We argue that both can be cost-effective for boosting attendance in a policy-relevant setting. Exploiting the variations in the intensive margin of CCT (i.e., the daily transfer amount), we further find evidence of a positive and diminishing marginal impact of the daily transfer amount on attendance. This finding highlights the importance of the intensive margin and the potential gains in cost-effectiveness from calibrating the daily transfer amount adequately. Our calibration exercise suggests that policymakers who wish to maximize attendance may consider the most cost-effective CCT of approximately 24-27 Bangladeshi taka per day (0.22-0.25 USD), or about 30% of child daily wages in the region.

Nevertheless, given that the information treatment has lasting post-intervention effects, the benefits of using high-frequency and low-cost information technology to improve attendance may potentially outweigh those of CCT in the long run. Moreover, a simple information treatment that uses existing attendance records would be easily scalable and politically more palatable than CCTs that use taxpayers' money and that may be more prone to mistargeting and leakages (Banerjee et al., 2020). Given the increasing use of ICT and ongoing efforts to monitor school attendance digitally, sharing such information may become feasible in Bangladesh and other developing countries in the near future. These findings would help policymakers, particularly those in low-income settings, formulate education interventions to raise school attendance cost-effectively.

Literature and Contribution. One of the primary contributions of this study is to examine the relative importance of cash and information in boosting school attendance cost-effectively. While there is an extensive literature documenting the success of CCTs in boosting school attendance and enrolment (Attanasio et al., 2010; Ganimian and Murnane, 2016; Glewwe and Muralidharan, 2016; Molina-Millan et al., 2016; Shultz, 2004), most studies ignore the role of the attendance information implicitly transferred to the parents through CCTs although recent evidence suggests that parents value such information. Using a lab-based choice experiment, Bursztyn and Coffman (2012) find that Brazilian parents' preference for CCTs (over unconditional cash transfers) significantly lowers once information on school absenteeism is explicitly provided to them through text messages. However, that study only offers monitoring with cash transfer and thus, cannot gauge how monitoring alone could induce attendance relative to CCT.

The relative importance between cash and information is still under-explored. To the best of our knowledge, De Walque and Valente (2023) is the only study to have done so, and they find that both cash and information promote the school attendance of primary school girls in Mozambique, with the effect of information being as large as 75% of that of CCT. Besides apparent differences between that study and ours in terms of gender focus (girls vs. both genders) and geography (East Africa vs. South Asia), we target a crucial age group in our low-income setting: secondary school students, who tend to face higher opportunity cost of schooling due to child labor or early marriage (Cepaluni et al., 2022; Corno and Voena, 2023). We thus, also explore the effects of our intervention on other outcomes such as educational aspirations, parental investments, child labor, and early marriage. Moreover, in De Walque and Valente (2023), girls convey a report card on attendance from and back to school every week. Thus, girls may be motivated to go to school just to get the report card, which potentially creates an effect independent of the attendance information itself. In contrast, we provide the attendance information directly to parents using low-cost text messages and voice calls, to ensure that the information is effectively conveyed to literate and illiterate parents.

Our study also contributes to the emerging literature that abstracts from cash transfers, and provides information to parents about student attendance and other measures of student effort using text messages in developing countries. Berlinski et al. (2022) and Bettinger et al. (2021) find that such information is effective at boosting attendance in

Chile and Brazil, respectively.[3] However, both of these studies focus on urban middle- to high-income settings, while our study focuses on a rural low-income setting. Moreover, in contrast to prior studies that provide information on attendance to parents in developing countries (Berlinski et al., 2022; Bettinger et al., 2021; De Walque and Valente, 2023), our study follows up on school attendance up to a year after the end of the intervention. This lets us confirm that information–rather than cash transfer–can potentially drive persistent behavioral changes. This also suggests that recent findings of positive longer-term effects of CCTs on educational outcomes (Molina-Millan et al., 2016) could potentially be partly driven by the information component of CCTs.

Our study also contributes to the CCT literature by introducing two design features aimed at raising the cost-effectiveness of CCTs.[4] First, we introduce loss framing in CCT, as inspired by the widely documented psychological trait of loss aversion, a phenomenon where people react more strongly to losses than gains of the same amount (Kahneman et al., 1990; Kahneman and Tversky, 1979). Loss-framed CCTs may thus generate greater impact at no additional cost compared to gain-framed CCTs. Despite this appeal, loss-framing has rarely been applied to education policy, and our paper contributes to this strand of literature.[5] To reinforce the framing, our CCT design further adopts novel linear incentives, in which the transfer amount is proportionate to the number of days attended, as opposed to imposing an attendance threshold per school term. Thus, our conditions for cash transfer incentivize households to send children to school on *every* intervention day, eliminating the possibility of threshold effects where students stop attending school once they meet the minimum number of days or miss too many days of cash incentive.[6]

[3]Other related literature on reducing parent-child information gaps tend to be more focused on developed countries (Bergman, 2021; Bergman and Chan, 2021) or target outcomes other than attendance (Avvisati et al., 2013; Barrera-Osorio et al., 2020).

[4]There are only a handful of studies on design features that could improve the cost-effectiveness of CCTs such as payment schedule (Barrera-Osorio et al., 2011) and targeting and conditionality (de Janvery and Sadoulet, 2006).

[5]Loss-framing has been applied to incentivize teachers' and students' performance in the United States (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.

[6]The importance of such threshold effects is reported by Duflo et al. (2012) in the context of teacher incentives. Whether linear or threshold incentives are desirable depends on how we weigh marginal attendance at different attendance levels. In a separate ongoing project, we examine the cost-effectiveness of linear and threshold incentives under different weights placed on marginal attendance.

Second, we vary the intensive margin of CCTs, that is, the amount paid for a given attendance record. Contrary to the existing evidence (Baird et al., 2011; Filmer and Schady, 2011), we find that the intensive margin matters for school attendance, with the marginal impact diminishing with respect to daily transfer amount.[7] This important finding helps us calibrate the most cost-effective amount of cash transfer.

Our work complements the above studies by providing rigorous empirical evidence on the relative effectiveness of information compared to CCT in boosting secondary school attendance in rural Bangladesh. By additionally analyzing framing variations, nonlinearities in CCT effects, gendered impact heterogeneity, and post-intervention influences this research investigation offers a new set of valuable insights for policymakers to design cost-effective interventions to increase school attendance in resource-poor settings.

The rest of the paper is organized as follows. Section 2 describes the field experiment, and Section 3 describes the data. The effects of CCT and SMS treatments on daily attendance during the intervention period and sensitivity analyzes are presented in Section 4. We elucidate some important treatment heterogeneity by gender and document persistence of the treatment effects after the intervention in Section 5. Section 6 analyzes the cost-effectiveness of our interventions, and Section 7 concludes.

2 Experimental Setting and Design

Our field experiment was conducted in Gaibandha, a rural district in north Bangladesh. The district is predominantly agricultural with 71% of the working population engaged in the agricultural sector (World Bank, 2020). The poverty rate of Gaibandha is 46.7%, far exceeding the national average of 24.3% (Bangladesh Bureau of Statistics and World Food Programme, 2020). Gaibandha also lags behind in education. The adult literacy rate in the district is 38%, one of the lowest in the country. School attendance rate is also low at 56.5% for boys and 49.0% for girls (Bangladesh Bureau of Statistics, 2013).

Against this backdrop, we designed an intervention targeted at students between

[7] Our findings align with recent evidence that the intensive margin of CCTs matter in the context of migration in Indonesia (Bryan et al., 2023).

Table 1. Sample Size by Cohort, Grade, Gender in Three Participating Schools

Cohort (Intervention Years)		Grade 6	7	8	9	Total
Old	Male	157	40	—	—	197
(2017 & 2018)	Female	163	40	—	—	203
	Total	320	80	—	—	400
New	Male	—	—	105	100	205
(2018)	Female	—	—	101	93	194
	Total	—	—	206	193	399

grades 6 and 9 from three lower secondary schools (see Appendix A for discussion of school selection). To recruit students, we first obtained school headmasters' consent to participate in this study and then obtained the student roster of the target grades from these schools. We randomly kept only one child per household in the roster for the recruitment. We then drew a random sample of students stratified by gender, school, and grade from this list, excluding students from a household without a valid mobile phone (i.e., currently active) or outside three catchment unions due to logistical constraints.[8] A total of 400 students of grades 6-7 at the beginning of 2017 ("old cohort") and an additional 399 students of grades 8-9 at the start of 2018 ("new cohort") were recruited for this RCT.[9] The distribution of the final sample by grade, gender, cohort, and intervention years is reported in Table 1.

Study participants were informed at the time of recruitment that they have a 75% chance of receiving SMS and voice calls, and a 50% chance of receiving cash transfers. No further detail was provided on the interventions at that stage. We conducted a detailed baseline household survey for each cohort immediately after the sample recruitment was complete. Subsequently, each study participant was randomly assigned to one of the four treatment arms:

[8] Unions are the lowest administrative unit and consist of wards. Approximately 90% of the parents had a valid phone number.

[9] 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. Some irregularities occurred during the recruitment of the new cohort due to human errors such as spelling errors. First, one student in the old cohort was mistakenly re-listed in the new cohort roster and was dropped from the new-cohort sample. Second, there were ten households with more than one participating child. Dropping these ten households does not alter our main findings.

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

Gain: In addition to the SMS treatment, households receive gain-framed CCTs.

Loss: In addition to the SMS treatment, households receive loss-framed CCTs.

Control: Households receive neither school attendance information nor CCTs.

After randomization, we announced the treatment assignment–which remained the same throughout the study for a given participant–and then implemented our intervention in the following four phases: two phases in 2017 (2017-I and 2017-II) and another two in 2018 (2018-I and 2018-II). Therefore, the RCT continued for four phases (two years) for the old cohort and two phases (one year) for the new cohort. The predetermined number of intervention days N, on which we counted school attendance for our SMS and CCT interventions, was $N = 60$ in 2017-I and $N = 50$ in the other three phases. We reduced N after 2017-I to cope with administrative delays in finalizing the student rosters and unanticipated school closures due to floods and teacher strikes (FAO, 2017; The Daily Star, 2018). When unexpected delays or school closures occurred, we pushed back the end date of the intervention phase. 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 those in the control group were informed of the start date and the number of intervention days in the phase.[10]

The daily cash transfer amount T varied across phases. We set T to be 10 taka (≈ 0.09 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).[11] In 2018-I, T was increased

[10]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.

[11]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 (von Hippel et al., 2016)–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

to 20 taka. In 2018-II, we introduced a 'High' [H] CCT subtreatment, which raised T to 30 taka. Half of the households in each of the Gain and Loss groups were randomly assigned to the H-subtreatment, whereas the remaining half were assigned to the 'Low' [L] subtreatment with T remaining at 20 taka. The subtreatment assignment was made and announced after 2018-I and before 2018-II. The cash to be transferred to the CCT households was calculated based on the school days attended during the intervention days and disbursed at the end of each phase by visiting the CCT households. Figure A1 in Appendix C summarizes the study timeline.

We disbursed cash once after each phase for the following reasons.[12] First, the administrative and transaction cost of disbursement trips is relatively high, which prohibits more frequent payments (see Section 6). Transferring the CCT amount using a bank account is also not possible in our context as a sizable proportion of households is unbanked.[13] Second, it would be impractical and arguably unethical to give the full phase-specific CCT amount to the Loss group (i.e., the full amount the household would receive if the child never misses a valid school day in a given phase) at the beginning of the phase and then take a portion of the amount away from them at the end of the phase for each missed school day. Third, by making the timing of the disbursement the same, we can exclusively focus on the effect of Gain vs Loss framing, which was regularly reinforced by the weekly messages. It should be noted that just showing rewards (rather than letting participants take the rewards) is common in experiments that seek to create an endowment effect and exploit loss aversion (Fryer Jr et al., 2022; Kahneman et al., 1990).

One of the key features of our interventions is the framing of weekly text and voice messages sent to parents. These messages are the same across the three treatment groups

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).

[12] While disbursement on a monthly (Benhassine et al., 2015; De Brauw et al., 2015) or quarterly basis (Akresh et al., 2013) is not uncommon, others make disbursements much less frequently (Barrera-Osorio and Filmer, 2016; Filmer and Schady, 2011). Our phase-wise disbursement mimics the real-life educational stipend transfer in Bangladesh, which distributes cash once after each quarter.

[13] According to the Global Findex Database, around 50% of the rural population aged 15 or older in Bangladesh is unbanked (World Bank, 2018). Until recently, the payments of educational stipend in Bangladesh were transferred through headmasters, who distributed them to students. This saves the transaction cost for individual students (and households) but creates a higher risk of leakage. To address the leakage, the stipend in secondary schools has recently been digitized (New Age Bangladesh, 2022).

except for certain details specific to each group. At the start of each phase, we sent the following messages to the three groups:

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.

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. The 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. The payment of any cash transfer balance will be made after *Disbursement*.

In the text messages above, *PhaseStart* refers to the date when the intervention begins and *Disbursement* refers to the approximate timing of the 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.

During each intervention phase, the three treatment groups received the following text and voice messages about attendance: "Last week, your child attended D_a school days and missed D_m school days," where D_a and D_m are the number of school days in which the child was present and absent, respectively, during the last reporting week. To make the change in balance salient to households and to reinforce the CCT framing, we additionally gave the following message to the Gain [Loss] treatment group: "You have gained TD_a [lost TD_m] taka for D_a [D_m] school days attended [missed]. Your current cash transfer balance has increased [decreased] to B taka," where TD_a, TD_m, B are the total amount gained (for the Gain group), the total amount lost (for the Loss group), and the updated cumulative balance at the end of the week. The implementation of weekly messages went smoothly except for early 2018-I, when some lapses were found in subsequent audits. During this period, some texts were not sent in certain weeks and some contained errors in weekly attendance and transfer amount information. However, these error rates were minimal (around 2-4%), 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 B.

3 Data

3.1 Attendance

The main outcome of interest is whether a child was present in school on a given day. As self-reported attendance is often subject to overreporting (Baird and Özler, 2012), we rely on three different data sources. The first and primary source is the official record of student presence, taken by school teachers at the beginning of each morning session. We digitized this official attendance record for each intervention day from the school administration book. The second data source is daily afternoon attendance, which was independently collected by a randomly chosen student representative from each school-grade-section. This enables us to address the potential concern that teachers may intentionally mark absent students as present (e.g., out of sympathy for the poorer students), as the morning attendance record is used for the CCTs and SMS interventions.[14] Afternoon attendance data also allows us to examine whether each student stayed in school after morning attendance was taken. The third data source is unannounced random school visits by field officers, which took place around eight times each year. The three sources of attendance records obtained at different points in a day, collectively enable us to cross-validate attendance records and capture granular attendance behavior, including coming to school late and leaving school early. To our knowledge, this study is the first to examine such granular school attendance behavior.

We have complete attendance records for 720 out of 799 students and focus on these 720 "continuing" students when examining attendance outcomes. In particular, 79 students (44 from the old cohort and 35 from the new cohort) discontinued their studies in our study schools during the intervention period. Nevertheless, there is no statistically or economically significant differences across treatment arms in the probability of discontinuation (see Footnote 17). This is true whether we examine the two cohorts together or separately. Thus, potential concern for attrition bias is minimal. We note that the discontinued students may have completely dropped out of the school system or transferred to another school. It is nevertheless valid to say that the discontinued students are no longer attending our study schools. In sensitivity analyses, we include the discontinued students and treat them as absent from the onset of discontinuation. Our estimates and inferences are robust to the inclusion of discontinued students.[15]

The raw data suggest that concerns about misreporting or partial attendance mentioned above are limited. From Table 2, there is a strong positive correlation between morning and afternoon attendance from 110,800 person-day records for continuing students. For almost 90% of the person-day data, morning attendance matches with af-

[14] 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 would be used for CCTs.

[15] We also used an alternative definition of attrition, where an individual is identified as missing if the endline survey could not be administered for that individual due to migration. Based on this definition, there were 16 missing students, 8 from the old cohort and 8 from the new cohort. Our results do not meaningfully change by using this alternative definition.

ternoon attendance, and the correlation between morning and afternoon records is 0.77. The off-diagonal elements in Table 2 indicate that the chances of students leaving school early (before afternoon attendance) are higher than the odds of students coming late (after morning attendance). Attendance records from unannounced random visits are also highly correlated with morning and afternoon attendance. Based on 8,460 person-day observations for continuing students with all three attendance records, the correlation is the highest at 0.87 between morning and random visit records, followed by a correlation of 0.79 between afternoon and random visit records, and a correlation of 0.76 between morning and afternoon records. The larger correlations with random visit records are plausible since the random visits are likely to capture some latecomers and early leavers.

Table 2. Morning and Afternoon Attendance on Intervention Days [Continuing Students]

	Afternoon Present	Afternoon Absent
Morning Present	56.83	7.67
Morning Absent	3.24	32.25

Note: Based on 110,800 person-day observations with 720 unique students and 239 unique calendar days. The number of unique calendar days is higher than the total number of intervention days in Figure A1 due to differences in school calendars.

In addition to the three sources of daily attendance data, we also collected official monthly attendance records of study participants for pre-intervention, non-intervention, and post-intervention days in 2016 [2017], 2017-18 [2018], and 2019, respectively, for the old [new] cohort. Pre-intervention attendance data enable 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 persistent effect of our intervention.[16]

3.2 Survey

We also collect data at the household and individual levels through baseline, midline, and endline surveys. For both old and new cohorts, a baseline survey was conducted before

[16]We collected monthly rather than daily data for pre-intervention, non-intervention, and post-intervention days due to budget limitations. The pre-intervention attendance records for old-cohort students who were in grade 6 in 2017 was unavailable as they were in a primary school in 2016. Post-intervention attendance records for 2019 are missing for 218 students due to school transfers and dropouts. There are no statistical differences in the probability of missing across treatment arms.

the treatment assignment was announced. The endline survey was conducted at the end of the intervention, except for 16 households that had migrated (see Footnote 15). In addition, we administered a midline survey to the old cohort between the 2017-II and 2018-I phases (see Figure A1). 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. The surveys also asked each student participant to name five closest classmates and included a short 15-minute mathematics test covering basic arithmetic and geometry knowledge based on the local curriculum. Moreover, both the students and their parents were asked about academic aspirations in terms of the highest grade that they would like the students to achieve.

We additionally 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 was disbursed. These disbursement surveys were integrated into the midline survey in 2017-II and the endline survey in 2018-II. For the other two 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 households were supposed to receive, and whether the parents 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 spend the amount received from the study (such as for education purposes or household consumption). In the endline survey, CCT households were also surveyed on how the previously disbursed cash transfers were spent.

3.3 Balance Check

Table 3 reports 16 key socio-demographic and economic characteristics for the old and new cohorts, disaggregated by the treatment assignment. We conduct a pairwise t-test of equality of means for each of the 16 variables and 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, all other variables were balanced at baseline. 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.[17] We control for the two unbalanced covariates in our main regression analyses but note that the results are similar when the unbalanced covariates are omitted.

4 Did the Intervention Improve School Attendance?

4.1 Main Empirical Model

Our baseline econometric specification is as follows:

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

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; a "class" is determined by the combination of cohort, school, and grade.[18]

Our primary coefficients of interest are β_1, β_2, and β_3, which reflect the intent-to-treat (ITT) effects of the Gain, Loss, and SMS treatments, respectively. Our main attendance outcome is morning attendance. However, we also analyze afternoon attendance and "morning and afternoon attendance", the latter of which takes unity if the student was present both in the morning and afternoon on a given day, and zero otherwise. We further use attendance at unannounced random visits as an outcome of interest. X_{ic} capture the unbalanced covariates: ownership of agricultural land and possession of television or radio. We also include strata (cohort-school-grade-gender) and date fixed-effects, denoted by u_d and v_t, to control for any unobserved heterogeneity across strata and different calendar dates, respectively. We also add an idiosyncratic error term ϵ_{ict}, which is clustered at the

[17] These findings remain unchanged even when we focus on continuing students. Appendix C reports versions of Tables 1 and 3 only for continuing students in Tables A1 and A2. We also run a regression of discontinuation on the treatment assignment, and found no statistically or economically significant differences in the probability of discontinuation across treatment arms (Table A3).

[18] While students in a class may be in different sections, we regard a class as an important unit because students from different sections may take or might have previously taken lessons together.

Table 3. Summary Statistics and Balance Check

	Old Cohort					
Variable	Gain (1)	Loss (2)	SMS (3)	Control (4)	Overall (5)	Orthogonality (6)
Participating child is female	0.510	0.520	0.500	0.500	0.507	0.991
Male HH at least pri. educ.	0.420	0.450	0.410	0.440	0.430	0.939
Male HH at least sec. educ.	0.080	0.100	0.080	0.070	0.083	0.891
Female spouse at least pri. educ.	0.420	0.400	0.380	0.420	0.405	0.929
Female spouse at least sec. educ.	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
Height of the child	142.037	139.370	143.332	142.443	141.796	0.218
Weight 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
	New Cohort					
Participating child is female	0.470	0.490	0.490	0.495	0.486	0.986
Male HH has at least pri. educ.	0.430	0.400	0.480	0.424	0.434	0.713
Male HH at least sec. educ.	0.070	0.100	0.140	0.121	0.108	0.424
Female spouse at least pri. educ.	0.390	0.460	0.480	0.556	0.471	0.136
Female spouse at least sec. educ.	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
Height of the child	148.565	149.809	146.152	146.037	147.645	0.258
Weight of the child	41.340	40.010	41.630	41.717	41.173	0.318
Standardized test score	-0.200	-0.087	-0.257	0.000	-0.136	0.232
Observations	100	100	100	99	399	0.032

Note: Male HH and female spouse refer to the household head and his spouse when the household is headed by a male. For about 7.6% of female-headed households, female spouse and male HH, represent the household head and her spouse, respectively. These variables take unity if the household head or spouse has at least primary or secondary education, and zero otherwise. The summary statistics for spouses are calculated over those households with the relevant household member. Ownership of assets (agricultural land, radio/television, bicycle, tube well) is a binary variable that takes unity if the household owns the asset, and zero otherwise. The weight and height of the child are measured in kilograms and centimeters, respectively. Test scores are normalized relative to the control mean and standard deviation. Column (5) shows the mean values for each variable. Column (6) shows the *p*-value for joint orthogonality.

individual level to allow for correlation in attendance over time for a given individual.[19]

[19]Clustering standard errors at five alternative levels–school, grade, school-grade, school-grade-cohort,

4.2 Main Results

Table 4 reports the effects of the intervention on daily attendance of continuing students 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 10.5 to 13.3 and by 13.3 to 15.3 percentage points, respectively. On the other hand, the SMS treatment increases attendance by 5.4 to 7.7 percentage points. All effects are statistically significant at conventional levels. As the estimates in Table 4 are quantitatively similar across different measures of attendance, we have no evidence that students are leaving school right after morning attendance is taken.[20] Running regressions disaggregated by phase show consistent results with all three treatments having positive and mostly significant impacts on attendance (Table A4).

Table 4 further shows that the Loss treatment's impact is the largest but not significantly larger than the Gain treatment's impact. There are at least three possible explanations for this lack of significant difference. First, delayed cash disbursement might have failed to generate the expected endowment effect. Since the cash was disbursed only at the end of each phase, individuals may have paid little attention to the cash balance until they were close to the disbursement date, a possibility that is consistent with cyclical attention of lowest paid workers (Berson et al., 2021). Second, it is plausible that the present value of the cash may be too low to make any difference at the beginning of the phase. Consistent with these possibilities, we find that the impact of CCT intervention tends to be stronger in the second half of each phase relative to the first half (Table A5). Nevertheless, the impact difference between Gain and Loss treatments remains statistically indistinguishable. Third, loss aversion may be simply 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. This may be because the loss in our

and section–yields results similar to those reported below (available upon request).

[20] We also found that the intervention increased afternoon attendance conditional on students being present in the morning (available upon request).

Table 4. The Effects of CCTs and SMS on Daily Attendance During the Intervention [Continuing Students]

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain	0.115***	0.130***	0.133***	0.105***
	(0.022)	(0.023)	(0.023)	(0.025)
Loss	0.133***	0.150***	0.153***	0.138***
	(0.022)	(0.023)	(0.023)	(0.025)
SMS	0.054**	0.062***	0.062***	0.077***
	(0.022)	(0.022)	(0.022)	(0.025)
P(Gain = Loss)	0.437	0.384	0.411	0.182
P(Gain = SMS)	0.006	0.003	0.003	0.246
P(Loss = SMS)	0.000	0.000	0.000	0.013
Observations	110,800	110,800	110,800	8,460
R^2	0.072	0.096	0.093	0.059
Control Mean	0.570	0.513	0.480	0.604

Note: "'Morning", "Afternoon", "Morning & Afternoon", and "Random Visit", take unity if the child was marked present in, respectively, the morning, afternoon, in both the morning and afternoon, 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 strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

context is merely a paper loss (Imas, 2016). It is, however, also possible that the concept of loss aversion is overhyped due to publication bias, where only articles that confirm loss aversion are published and cited (Gal and Rucker, 2018; Yechiam, 2019).[21]

It is also worth noting that 77% of the respondents in the CCT treatment groups claimed that they remembered the actual cash balance, of which 95% remembered the balance correctly in the disbursement surveys. Seven in ten respondents also kept their last SMS from us on their phones. The regression estimates of the students' phase-based morning attendances rate (i.e., number of days attended as a proportion of intervention days in a given phase) in the Gain and Loss groups, on the indicators for remembering the cash balance and for keeping the last SMS, show that those parents who remember

[21]Loss aversion could be also inoperative if participants did not understand the framing. However, this seems unlikely because as high as 81.96% of respondents in the both CCT groups demonstrated their correct understanding of the framing in their response to questions about how their cash transfer balance changes according to a given record of presence and absence in 2018.

their cash transfer balance and keep their SMS are more likely to have sent their children to school (Table A6). However, we do not find any association between attendance rate and loss framing (relative to gain framing) at conventional levels of significance. While remembering the transfer amount and keeping SMS may be endogenous, the discussion above indicates that the main source of impact for CCT treatments is the cash transfers and not the framing.

4.3 Sensitivity and Heterogeneity Analyses

Target-based Attendance Impact. Our CCT design does not stipulate any minimum attendance percentage for receiving CCT and ties cash conditionality to daily attendance, thereby eliminating the possibility of threshold effect (see Footnote 6). Although the Bangladesh Female Secondary School Stipend and Assistance Program (FSSAP) aims to achieve attendance for 75% of school days, most CCT programs target 80-85% attendance (Fiszbein and Schady, 2009). Thus, we use an alternative measure of attendance that takes unity if a child attended 80% of valid school days during the intervention, and zero otherwise (Table A7). While Gain treatment increases the likelihood of achieving the target by 20.5 to 24.4 percentage points, the impact of Loss treatment tend to be marginally higher but not statistically different. Conversely, SMS treatment increases the probability of achieving the target of 80% attendance by 3.1 to 13.7 percentage points.

Alternative Specifications. We perform a battery of sensitivity analyses using alternative specifications. First, we include discontinued students, assuming that the discontinued students were absent from school from the onset of discontinuation (Table A8). Second, we employ a pure experimental design strategy and adjust for multiple hypothesis testing (Table A9). Third, we consider a difference-in-differences 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 A10). Finally, we use a double-selection lasso linear regression model (Table A11). The results to all four alternative specifications are quantitatively similar to the main results in Table 4.

Accounting for Peer Effects. We further account for peer effects by controlling for the baseline proportions of the classmates in each of the Gain, Loss, SMS, and Control groups among the five closest classmates (Table A12). The attendance impact of the CCT and treatments remain quantitatively similar to those presented above.

Impact Heterogeneity. We find no statistically significant differences by pre-intervention attendance rate, distance from school, education of parents, and socioeconomic status of households (the results are available upon request). However, the gender of our study participant notably shows significant impact heterogeneity across treatments, which we thoroughly explore in the next section.

4.4 CCT vs. SMS Treatments

As CCT seems to matter much more than Gain/Loss framing across all specifications, we henceforth combine the Gain and Loss treatments into a single CCT treatment arm to focus on the comparison between CCT and SMS interventions. Column (1) of Table 5 reports the same regression as Column (1) of Table 4, with the Gain and Loss groups merged into a "CCT" treatment group. From Column (1) of Table 5, we find that CCT increases daily attendance by 12.4 percentage points while SMS increases daily attendance by 5.4 percentage points. These estimates are comparable to those from Column (1) of Table 4, and suggest that SMS increases attendance by around two fifths of the CCT impacts in our preferred specification.

To put our results into a broader perspective, we compare our estimates against those reported in prior literature. We first point out that the size of our cash transfer of around 3.8% of per capita household pretransfer consumption, falls within the lower end of the generosity of other CCT programs, which range from no more than 4% to 20% of mean household consumption (Fiszbein et al., 2009). Nevertheless, our attendance impacts lie within the range of 1.7 to 27.8 percentage points reported in previous studies (García and Saavedra, 2017). The attendance impact of SMS is also comparable to the range of 0.9 to 6 percentage points found in studies that provide information on attendance to parents in developing countries (Berlinski et al., 2022; Bettinger et al., 2021; De Walque and Valente, 2023). See also Footnotes 1 and 2 for a discussion. Finally, our findings that the impacts of information range from two fifths to three quarters of the impacts of CCT align with De Walque and Valente (2023) who find that information impacts are up to 75% of the impacts of CCT for primary school girls in Mozambique.

Our findings suggest that attendance information–often implicitly provided to parents in CCT programs–potentially plays a role in reinforcing CCT impacts. In the next section, we focus on impact heterogeneity by gender and show that both CCT and SMS treatments had lasting impacts on girls in the year following the intervention. Interestingly, we find that the effects CCT and SMS treatments converge one year post-intervention, with the impacts of CCTs being reduced to similar magnitudes to those of SMS, suggesting that the information embedded in the CCTs may matter. We further analyze other outcomes such as educational aspirations and investments, child labor and early marriage, and learning outcomes.

5 Gender, Persistence, and Other Outcomes

It is well known that girls tend to receive less human capital investments than boys in poor countries (Hanushek, 2008; Ho, 2019; Jayachandran, 2015). Past studies on interventions targeted at girls suggest that CCT and information can enhance long-term schooling and learning outcomes for girls (Armand et al., 2020; Hahn et al., 2018; Khandker et al., 2021; Shamsuddin, 2015). Although our intervention does not specifically target girls, there may be important heterogeneous treatment effects by gender. In particular, evidence from social psychology suggest that females tend to be more receptive and more amenable in their attitudes than boys (Johnson and MacDonnell, 1974; Lee, 2005; Stein, 1969). Therefore, it is 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 and explain the plausible mechanisms such as students' and parents' academic aspirations and parental investment in education, disaggregated by gender.

Table 5. The Gendered Effects of CCT and SMS treatments on Attendance [Continuing Students]

	During Intervention			Intervention + Non-Intervention			Post-Intervention		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CCT	0.124***	0.105***	0.141***	0.127***	0.107***	0.144***	0.032**	0.004	0.060***
	(0.019)	(0.028)	(0.025)	(0.018)	(0.026)	(0.024)	(0.016)	(0.024)	(0.020)
SMS	0.054**	0.022	0.079***	0.055***	0.021	0.082***	0.030*	0.001	0.059**
	(0.022)	(0.034)	(0.027)	(0.020)	(0.032)	(0.025)	(0.018)	(0.028)	(0.023)
P(CCT=SMS)	0.000	0.010	0.005	0.000	0.004	0.003	0.908	0.898	0.991
P(CCT^{Boys}=CCT^{Girls})		0.332			0.299			0.073	
P(SMS^{Boys}=SMS^{Girls})		0.177			0.131			0.104	
Observations	110,800	54,465	56,335	8,439	4,114	4,325	7,750	3,824	3,926
R^2	0.072	0.063	0.080	0.124	0.106	0.134	0.397	0.345	0.455
Control Mean	0.570	0.550	0.590	0.557	0.542	0.573	0.291	0.299	0.282

Note: The dependent variable in Columns (1)-(3), (4)-(6), and (7)-(9) are, respectively, daily morning attendance during our intervention period in 2017 and 2018, 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), and 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 above specifications control for strata fixed effects and 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

5.1 Gender Differences in Attendance

We begin by disaggregating Column (1) of Table 5 by gender. The estimated impacts of our interventions on attendance are reported in Columns (2) and (3) for boys and girls, respectively. Columns (4)-(6) use the monthly–instead of daily—attendance rate in 2017-2018, calculated as 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.[22]

We make three observations from Table 5. First, by comparing Columns (1)-(3) against Columns (4)-(6), we confirm that the results are very similar, irrespective of whether we use daily or monthly data. Second, the attendance impacts of the CCT and SMS treatments for girls are larger than those for boys during the intervention period, although the difference between the two genders is insignificant for each treatment. Third, from the comparison of Columns (7)-(9) against Columns (4)-(6), the effects of the CCT and SMS treatments attenuate but still persist beyond the intervention period for girls but not for boys. We also divide the 2019 sample into the first and second halves and re-run the analyses to examine whether post-intervention effects may gradually diminish. This set of analyses reveals that the effects of CCTs and SMS on girls' attendance are very strong in the six months immediately after the intervention (Table A13). The effects remain positive and statistically significant, albeit weaker, in the second half of 2019.

The results suggest that girls tend to be more responsive than boys, especially post-intervention. Specifically, male students are never quite responsive to SMS treatment–both during and after the intervention–and they also stop responding to the CCT treatment right after the intervention. Moreover, the effects of CCT and SMS on girls converge in 2019, with the post-intervention effect of CCT becoming close to that of SMS. These results suggest that the 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. We explore some plausible mechanisms behind the gender differences below.

[22] Sensitivity analyses (available upon request) dropping students with missing attendance records in 2019 (see Footnote 16) yielded qualitatively similar results and inferences to those reported below.

5.2 Gender Differences in Aspirations

Academic aspiration is one plausible channel through which the attendance impact persists. For instance, our interventions may have raised aspirations for girls, possibly through increased attendance during the intervention period. The weekly SMS information may have also raised parents' attention to education, particularly for girls. To test these possibilities, we collected information on the highest grades that students want to achieve themselves and the highest grade that parents want their children to achieve, both in the baseline and endline surveys. We then define the changes in the students' and parents' aspirations by: (i) a continuous outcome in the desired number of years of schooling 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.[23]

We 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 6 indicates that both CCT and SMS interventions increased the academic aspirations of students, although the effects are statistically insignificant for most estimates. As Columns (2) and (3) show, the increase comes only from girls but not boys. In particular, both CCT and SMS treatments significantly increase girls' educational aspirations. 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 respectively than the girls in the Control group, and the former figure is statistically significant. In contrast, parents' aspirations were more sticky in nature and were not impacted by our interventions (Table A14).

5.3 Child Labor and Early Marriage

In poor countries, secondary school students are often at-risk of missing school due to child labor (Cepaluni et al., 2022). Gender norms also often limit girls from attending

[23] For parental aspirations, we restrict the sample to those households in which the same respondent answered the educational aspiration question in the baseline and endline surveys. Because of this restriction and missing responses, the data on academic aspirations are available only for 721 students and 475 parents. There were no statistically significant differences across treatment arms in the prevalence of missing values for either students' or parents' academic aspirations.

Table 6. The Effects of CCT and SMS treatments on Changes in Students' Aspirations

	Continuous Outcome			Discrete Outcome		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.579	-0.271	1.473***	0.083	-0.031	0.200***
	(0.339)	(0.439)	(0.375)	(0.049)	(0.066)	(0.047)
SMS	0.690*	0.185	1.244**	0.100*	0.082	0.122
	(0.333)	(0.403)	(0.486)	(0.050)	(0.063)	(0.082)
P(CCT=SMS)	0.756	0.395	0.668	0.716	0.050	0.355
P(CCT^{Boys}=CCT^{Girls})		0.002			0.003	
P(SMS^{Boys}=SMS^{Girls})		0.079			0.682	
Observations	721	364	357	721	364	357
R^2	0.074	0.080	0.085	0.075	0.097	0.069
Control mean	0.580	0.956	0.176	0.352	0.385	0.318

Note: The dependent variables in Columns (1)-(3) and in Columns (4)-(6) are, respectively, the change between the baseline and endline surveys in the number of years of schooling that the participating student aspires to achieve, and an indicator that takes unity when the continuous outcome 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 above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

secondary school (Corno and Voena, 2023; Field and Ambrus, 2008; Millán et al., 2020), and boys' education may be prioritized at the expense of girls' (Björkman-Nyqvist, 2013). Parents in such countries often value girls' domestic duties more than their schooling and marry them off early. Here, we analyze the impact of our interventions on child labor for both boys and girls and early marriage for girls.[24]

To capture the change in child labor, we construct an indicator that takes unity if the student was engaged in an economically gainful activity within the seven days prior to the endline survey but not so in the baseline survey, and zero otherwise. We construct the early marriage indicator in a similar manner. We exclude the handful of participants whose child labor or marriage information is missing in either survey from the analyses. Using these definitions, about 2% of girls and 10% of boys were engaged in

[24]Early marriage for boys is negligible; no boy was married at the baseline and only one at the endline.

an economically gainful activity at the endline but not at the baseline. About 4% of the girls married between the baseline and endline. Columns (1)-(3) of Table 7 show that our interventions did not have a significant impact on child labor.[25] Column (4) of Table 7, however, suggests that both the CCT and SMS treatments reduced the incidence of early marriage for girls. A subsample analysis of early marriage for girls by grades suggests that the effects of the CCT and SMS treatments on delaying marriage may have been driven predominantly by grade-9 girls, who were around 15 years old at the time of the intervention (Table A15). The evidence above suggests that the attendance and school continuation for girls may be partially attributable to a reduction in early marriage.

Table 7. The Effects of CCT and SMS treatments on Child Labor and Early Marriage

Dependent variable	Child Labor			Early Marriage
	All	Boys	Girls	Girls
	(1)	(2)	(3)	(4)
CCT	-0.011	0.016	-0.039	-0.079**
	(0.020)	(0.021)	(0.033)	(0.034)
SMS	-0.025	0.006	-0.057	-0.090**
	(0.023)	(0.017)	(0.040)	(0.037)
P(CCT=SMS)	0.389	0.701	0.438	0.710
P(CCT^{Boys}=CCT^{Girls})		0.176		
P(SMS^{Boys}=SMS^{Girls})		0.137		
Observations	754	380	374	391
R^2	0.039	0.022	0.049	0.047
Control Mean	0.059	0.021	0.097	0.134

Note: "Child Labor" takes unity if the child's primary or secondary occupation is wage/salaried employment; self-employment in agriculture, forestry, and aquaculture; other self-engagement (including family business) in production, business, and services; or domestic duties over the last seven days at the endline but not so 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. The Control group is the reference category in all regressions. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

[25] We also found no effects of the intervention on child labor when we break down the analysis by grades. There is, however, one caveat in these results. Our surveys do not capture the impact of the interventions on temporary agricultural seasonal labor during harvest and planting seasons. In a separate ongoing project, we find that our CCT interventions can potentially help maintain the level of attendance during harvest season (Fujii et al., 2023).

5.4 Parental Investment in Education

The findings above suggest that attendance information can raise girls' academic aspirations and reduce their early marriage. There is also qualitative evidence that girls who attend schools regularly tend to have parents who actively encouraged their educational and career aspirations (Satyanarayana et al., 2018). Hence, the attendance information our intervention provides can increase investment in girls both through their effects on parents and children. To shed light on this issue, we turn our analysis on household investment in education. 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.[26] Our findings show that the SMS treatment has increased parental investment in child's education, especially for girls (Table A16). Interestingly, we also find suggestive evidence that SMS treatment increased the level of trust that girls have in the family (Table A17). The above results are consistent with the possibility that parents have become more engaged in and willing to spend more on their daughters' education thanks to the information provided. This in turn may have boosted the daughter's trust and educational aspirations, and resulted in higher post-intervention attendance.

We note that while the CCT intervention does not appear to have significantly raised parental investment in a child's education (Table A16), the cash transferred to them appears to have been spent on the child's education. In the first three phases [endline survey], households in the CCT groups were asked what proportion of the transferred cash they intend to spend [actually spent] on education, among other expenses items. Households reported that they intend to spend 54% [86%] of the cash transfers on education in 2017-I [2017-II]. Education share in the planned [actual] spending from the

[26]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. There are 46 households that report zero expenditure on every expenditure component at the baseline or endline. These households were 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 unity if the household reports zero expenditure on at least one item, and zero otherwise in the set of regressors. Omitting this indicator variable from the model does not alter the result much.

2018-I [2018-II] disbursement surveys, was 72% [92%] of the total cash transferred. The higher reported proportions in 2017-II and 2018-II could be because these surveys were conducted between the end of the current school year and the beginning of the following school year, so that new school supplies and enrollment costs had to be borne. The gender differences in intended and actual spending were statistically insignificant.

5.5 Parental Expectations of Future Cash Transfers

The attendance impact may have persisted for female and not male students because girls' parents expected to receive cash transfers in the future while parents of boys did not. Such an expectation is plausible given that Bangladesh has gender-targeted conditional cash transfer programs for secondary school students (Xu et al., 2022).[27] 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% of adult respondents expected to receive cash transfers in the future. While CCT treatment significantly raises the expectation to receive cash transfer in the future, there is no significant gender difference in the impact of our interventions on parental expectations of future cash transfers (Table A18). Hence, expectation of future cash transfers is unlikely to be the main driver of persistent attendance impact for girls.

5.6 Learning Outcomes

We now examine the impact of our intervention on learning outcomes, measured by a short mathematics test administered by our implementation partner at the baseline and endline. The test score is normalized by subtracting the control mean and dividing by the control standard deviation for each class. We then use eq. (1) to estimate the impact of school attendance on endline test score, but include the baseline test scores in the set of regressors to form a value-added model. We find no statistically discernible impact of

[27] Some nationwide educational programs such as the FSSAP (Khandker et al., 2021) existed during our intervention period. Our treatment assignment was made independent of these, and we have no evidence that their presence significantly altered our results.

our interventions on learning outcome, either for boys or girls or both genders combined (Table A19). Our finding of null CCT effect on learning is broadly consistent with the existing literature (Fiszbein and Schady, 2009; McEwan, 2015).[28]

Whereas one may be concerned about the lack of immediate effects on mathematics test scores, 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 based on a score of a short mathematics test, and students are not incentivized to do well in the test. Therefore, our assessment 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. Given that our interventions led to an increase in attendance, it may lead to better learning outcomes in the long run.

Altogether, the results discussed in this section indicate that CCT and SMS improved girls' school attendance during the intervention period and even one year after the end of the intervention. This could be because our interventions raise female students' academic aspirations and receipt of education investment from parents and decrease their chance of getting married early. Even though our interventions do not improve mathematics test scores in the short run, they may have positive long run consequences since higher educational attainment has been found to improve labor market and intergenerational health outcomes (Asadullah, 2006; Currie and Moretti, 2003). Our results also suggest that attendance information rather than cash transfers is an important driver of the persistent attendance impact 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

[28] We also do not see any significant impact on other outcomes that we examined including study hours and child health (measured in BMI and height). Results are available upon request.

payment transfers whereas SMS is a low-cost technology, our findings trigger the question of whether SMS intervention is more cost-effective than CCTs in bringing children to school both during and after the intervention. We explore the former in the next section, while bearing in mind the evidence above, which suggests that low-cost information–rather than cash–may matter for girls post-intervention.

6 Cost-Effectiveness of CCT and SMS Interventions

6.1 Back-of-the-envelope Calculations

We now compare the cost-effectiveness of the CCT and SMS interventions given our unified setting. From Column (1) of Table 5, the estimated impact of the SMS treatment on contemporanous attendance is 5.4 percentage points while that of the CCT treatment is 12.4 percentage points, suggesting that the latter is more than twice as impactful as the former. However, the SMS treatment is less expensive than the CCT treatment to implement. Therefore, it is not apparent which intervention is more cost effective. We perform a back-of-the-envelope calculation to gauge the cost-effectiveness of our interventions over the two-year period, λ, as measured by the increased attendance in percentage point per program cost in thousand taka. Note that the gender difference in the treatment impacts on attendance during the intervention period is statistically insignificant (see Columns (2)-(3) of Table 5), despite the notable patterns on post-intervention attendance and other outcomes discussed in the previous section. For ease of presentation, we use the estimates for the entire sample for the purpose of cost-effectiveness calculations.

We perform the calculations based on the following three cost scenarios: (1) the actual program costs for our interventions; (2) policy costs without digital support; and (3) policy costs with digital support. Scenario (1) is based on our implementation, which include some cost components–such as payments to class representatives–that are unlikely to exist when our interventions are scaled up. Scenario (2) is the main policy-relevant cost scenario for the current discussion, which is applicable given the current ground reality in Bangladesh. Scenario (3) is an optimistic scenario that is applicable in the presence of digital infrastructure that enables automatic attendance data collection and electronic transfer of money. Further details are provided in Appendix C.

Here, we summarize the main takeaways from the cost-effectiveness analysis. First, the resulting λ under Scenarios (1), (2), and (3) is 3.01, 11.42, and 48.21 [2.95, 3.84, and 5.36], respectively, for the SMS [CCT] intervention. Thus, as we move from Scenario (1) to Scenario (3), λ becomes larger for both the SMS and CCT interventions. Second, the SMS treatment is substantially 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 digital infrastructure–as envisioned under Digital Bangladesh, simple attendance information provided through SMS could become a more cost-effective way to boost secondary school attendance than CCTs.[29]

It should be reiterated that our cost-effectiveness measure focuses on attendance during the intervention days. As we have seen, the magnitudes of the effects of the CCT and SMS treatments 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 and the CCT transfer amount would need to adjust yearly with the local cost of living and wage. Therefore, when the amount of resources available to the government is limited, SMS intervention may 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 CCT and SMS treatments can be viable alternatives to boost attendance.

[29] 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 A20. The SMS treatment remains more cost-effective than CCT under Scenarios (2) and (3), even when using $\tilde{\lambda}$. Nevertheless, we posit that $\tilde{\lambda}$ may not be a suitable a policy-relevant measure as (i) government officials are often interested in the actual financial resources spent, (ii) distortionary taxes may still need to be imposed to finance the cash transfers, and (iii) by excluding the cash from the cost calculations, one may improve cost-effectiveness simply by giving an arbitrarily large transfer to achieve perfect attendance, given that non-transfer program cost per student is unlikely to increase in proportion to the daily transfer.

6.2 Cost-effective CCTs

We now exploit variations in the daily CCT to find the most cost-effective transfer amount during the intervention period. 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 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, 20, and 30 taka–to capture the effects of different CCT amounts. As shown in Table 8, 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.

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. Using the following model that identifies the effect separately at the extensive (i.e., whether the household receives CCTs) and intensive (i.e., how much the household receives for daily attendance) margins, we derive the most cost-effective transfer amount:

$$\mathrm{Y}_{ict} = \underbrace{f_0 \mathrm{CCT}_{ic} + f_1 \tau_{ict} + f_2 \tau_{ict}^2}_{\text{Attendance impact of CCT transfer}} + g\mathrm{SMS}_{ic} + \underbrace{\beta_0 + \gamma X_{ic} + u_c + v_t}_{\text{Expected status-quo attendance}} + \epsilon_{ict}, \qquad (2)$$

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.

We denote the control attendance, or the expected status-quo 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

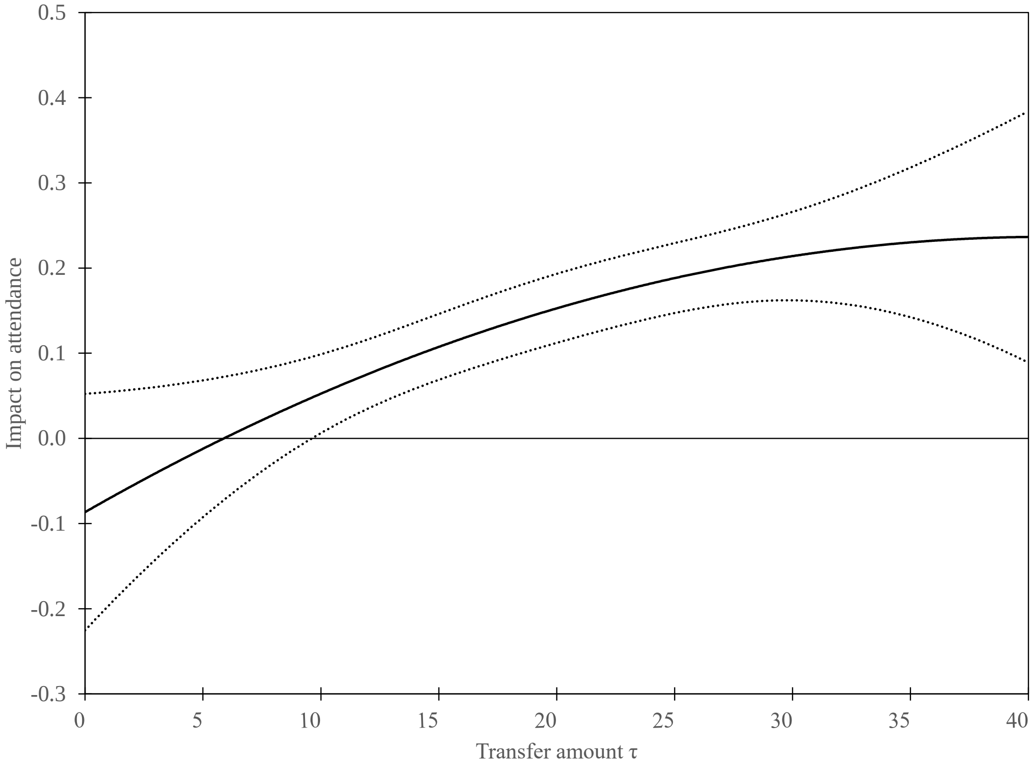
Table 8. Non-Linear Treatment Effects in CCTs [Continuing Students]

Dependent variable	Morning Daily (1)	Afternoon Daily (2)	Morning & Afternoon (3)	Random Visit (4)
CCT [10tk]	-0.005	0.005	0.005	-0.002
	(0.025)	(0.026)	(0.027)	(0.027)
CCT [20tk]	0.096***	0.098***	0.102***	0.065***
	(0.021)	(0.021)	(0.021)	(0.024)
CCT [30tk]	0.159***	0.176***	0.179***	0.097***
	(0.027)	(0.027)	(0.028)	(0.034)
SMS	0.053**	0.061***	0.062***	0.077***
	(0.021)	(0.022)	(0.022)	(0.025)
P(CCT [10tk] = CCT [20tk])	0.000	0.000	0.000	0.013
P(CCT [10tk] = CCT [30tk])	0.000	0.000	0.000	0.006
P(CCT [20tk] = CCT [30tk])	0.007	0.002	0.002	0.303
P(CCT [10tk] = SMS)	0.140	0.173	0.177	0.080
P(CCT [20tk] = SMS)	0.253	0.324	0.299	0.778
P(CCT [30tk] = SMS)	0.009	0.006	0.005	0.681
Observations	110,800	110,800	110,800	8,460
R^2	0.076	0.100	0.097	0.060
Control Mean	0.570	0.513	0.480	0.604

Note: The above estimates are from a non-linear specification. "'Morning", "Afternoon", "Morning & Afternoon", and "Random Visit", take unity if the child was marked present in, respectively, the morning, afternoon, in both the morning and afternoon, 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 strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

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. (2), we predict $f(\tau)$. Figure 1 shows the graph of the predicted value of $f(\tau)$ and its 95% 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 8. Figure 1 also indicates that the marginal effect becomes zero around 40 taka per day, although this amount 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.

Figure 1. The Estimated Attendance Impact of CCT with Daily Transfer τ



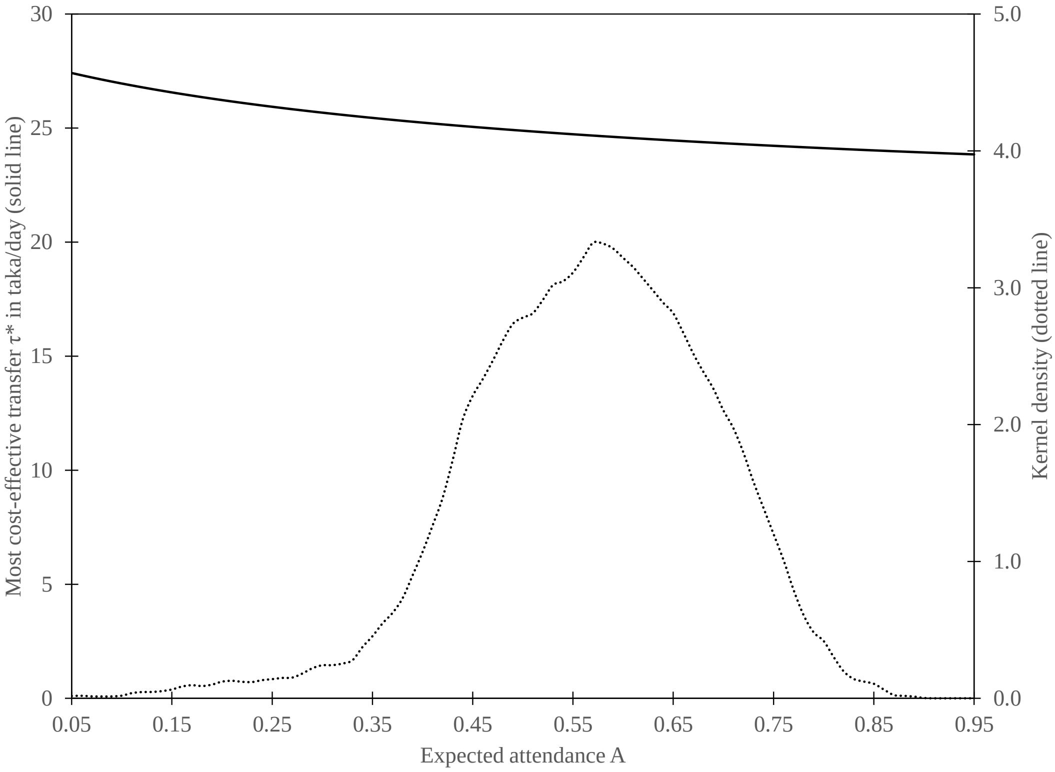
Note: The figure plots the estimated impact of CCT with daily transfer τ on daily morning attendance, $f(\tau)$, from a regression of eq. (2).

The regression based on eq. (2) also allows us to predict the control attendance A_{ict}. Even though the predicted value is not bound to be on the unit interval, over 99.9% 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 4. The dotted line in Figure 2 represents the kernel density estimate of A_{ict}.

When 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 C. Using the figures reported in Table A20, we obtain a combined cost of $C = 1,304$ thousand taka per student for communication, disbursement, and processing in Scenario (2).[30] We then express τ^* as

[30] Specifically, we add the total cost minus actual disbursed cash in Table A20 to arrive at $1,304 (= 3,227 - 1,923)$ taka per student.

Figure 2. 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}.

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.[31] 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 that τ^* is implicitly given by the following expression:

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

[31] 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.

The solid line in Figure 2 shows the most cost-effective transfer amount τ^* as a function of A. The estimates suggest that the most cost-effective amount of transfer is around 24-27 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 two alternative functional forms (Figures A2-A5). Regardless of the functional form used, the most cost-effective daily cash transfer amount still falls between 24 and 27 taka per day, or roughly 30% of child daily wages in the region.[32] Hence, with only a fraction of the daily wage for child labor, daily attendance can be cost-effectively increased. This is an important finding, given the potential high returns to education in the form of better employment prospects and income (Asadullah, 2006; Ito and Shonchoy, 2020).

7 Conclusion

This paper addresses the problem of low secondary-school attendance by relaxing cash and information constraints with three potentially cost-effective interventions in a unified framework: (i) weekly attendance information through SMS and voice calls to parents, (ii) gain-framed CCT plus weekly SMS, and (iii) a novel loss-framed CCT plus weekly SMS. We find that the loss treatment generates the highest impacts but that these impacts are not statistically significantly different from those of the gain treatment, suggesting that the cash matters more than the framing. Moreover, we find that SMS treatment improves school attendance by two fifths of the CCT impacts. Back-of-the envelope calculations reveal that CCT and SMS were equally cost-effective during the intervention period so that both cash or information could be viable ways to boost attendance.

We also find interesting gender differences in the post-intervention impacts. Specifically, whereas boys do not seem to respond to the treatments once the interventions ended, girls who benefited from the CCT or SMS treatments display higher attendance rates in the year following the intervention. Furthermore, the effects of CCT and SMS

[32] Using the average wage rate of 10.1 taka per hour and average daily work hours of 8.9 hours in Footnote 11, the average daily wage is about $89.9 (= 10.1 \times 8.9)$ taka per day

treatments converge during the post-intervention period, suggesting that information potentially has lasting effects on girls, possibly by improving their educational aspirations. We further find that treated girls–especially those from the higher grades–were less likely to be married by the endline survey compared to girls in the control group. This is an important consideration for long term well-being given that female education attainment may be an important determinant of labor and health outcomes.

Our research indicates the potential presence of a low-hanging fruit to promote secondary-school attendance in resource-poor settings, either through the calibration of the CCT amount to maximize the bang for the buck or through simple information. Specifically, we find that a daily CCT of around 30% of child wage would maximize the contemporaneous attendance impact. Nevertheless, given the rapid expansion of affordable digital technologies, the provision of simple information could provide a highly scalable and cost-effective way of boosting attendance. In particular, with a policy-relevant cost (i.e., Scenario (2) in Table A20), simple information provision can be around three times more cost effective at boosting attendance than cash. These are especially relevant considerations for the developing world, where resources for policy interventions are limited. Our study thus provides policymakers with a valuable set of insights to cost-effectively improve secondary school attendance and other outcomes.

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Online Appendix

A Selection of Schools

In Bangladesh, lower secondary education can be divided 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 the number of students in junior secondary and secondary grades, the total number of teachers, the highest grade taught in the school and the grade repetition and dropout rates. School sizes ranged from 305 to 995 students in total. The research team subsequently selected the three study schools based on target sample size while ensuring a moderate mix of school sizes (437 to 870 students).

B 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 and the process was automated in the last phase. There were unfortunately a few implementation issues in the first three phases. First, the implementing partner failed to keep records of text messages sent in 2017. Nevertheless, given that around 70% of respondents stated that they kept the last SMS in the disbursement surveys, it is likely that most text messages 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 operational difficulties due to the absence of a key personnel and nationwide teacher strikes, which led to irregular working hours in schools.[A1] Third, we also discovered that some of the text messages sent to the study participants in 2018-I contained errors.

[A1]Text messages 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 4, albeit with somewhat smaller estimates for the first five weeks.

Once these issues were discovered, we immediately conducted an audit to assess the prevalence of errors by checking the text messages against the attendance records in the subsequent weeks of 2018-I. The error rates in attendance information and CCT amounts were estimated at around 2% and 4%, respectively. We found no significant difference in the error rates in the 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.[A2] Since the error rates are very small, they are unlikely to systematically 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 text and voice messages and incorrect attendance information, which can be seen as a noise, would attenuate the estimated effects of our interventions (see also footnote A1). On the other hand, our experience also underscores the operational challenges to ensure that SMS messages are accurate and delivered on time. Similar challenges were also noted in published literature (Banerjee et al., 2007; Bauchet et al., 2015; Berlinski et al., 2022). Partly due to the issues described above and partly due to better funding availability, we automated the process of sending text and voice messages 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 that in 2018-I. These results are also consistent with our conjecture that the estimated effects in the first three phases may have been attenuated.

C Supplementary Tables and Figures

In this section, we present the supplementary tables and figures discussed in the main text: the sample characteristics when we drop discontinued students, a variety of sensitivity analyses, and additional gender-differentiated results. We also provide details of the cost-effectiveness analysis.

[A2] Approximately a quarter of text and voice messages 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 messages that had a specific phone number attached.

C.1 Sample Characteristics without Discontinued Students

Tables A1 and A2 are the same tables as Table 1 and Table 3, respectively, except that we focus on the continuing students. Table A3 gives the results of the regression of the binary indicator variable for discontinuation in 2017 or 2018. The table shows that there is no significant difference in the probability of discontinuation across treatment arms.

C.2 Impact by Phase and CCT Recollection

Table A4 disaggregates Table 4 by phase. The attendance impacts of CCT treatments are the largest in 2018-II when the households received 20 or 30 taka per day attended, suggesting the relevance of the intensive margin in CCT programs. Table A5 is the same as Table 4 except that we split the sample into the first and second halves of the phase. As discussed in the main text, we find that all treatments have a larger impact in the second part of each phase, and this is particularly so for the CCT treatments. To see the association between attendance and recollection of CCT, we regress morning attendance rate in each phase on the indicators for remembering the CCT amount that they are supposed to receive and keeping the last SMS sent to the participant. As Table A6 shows, these two indicators are positively associated with the morning attendance rate.

C.3 Sensitivity Analyses

Target attendance. Table A7 uses an alternative outcome for attendance that takes unity if a child has attended 80% of valid school days during the intervention, and zero otherwise. We see strong positive impact for the CCT and SMS treatment arms, especially for morning attendance and attendance during random visits.

Including discontinued students. Tables A8 presents the same results as those in Table 4, except that we include all students–including discontinued students–in the analysis. Discontinued students are assumed to have been absent from school from the start of discontinuation based on the participating schools' records. The point estimates in

Table A8 are not statistically different from the corresponding estimates in Table 4 and estimated treatment effects remain statistically significant at conventional levels.

Pure experimental design. We first consider the pure experimental design by removing the unbalanced controls X_{ic} and the fixed effects u_c from eq. (1) and also present the p-values from the Westfall-Young correction for multiple hypothesis testing. As Table A9 shows, our results are very similar to those in Table 4 even though the SMS treatment effect appears to be marginally weaker.

Difference-in-differences. We next consider a difference-in-differences 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, which takes the form of eq. (A1) below, has the advantage of being able to control for all time-invariant individual characteristics that affect attendance.

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

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 the monthly attendance rate for the year 2016 [2017] for the old [new] cohort. The treatment year indicator $\mathrm{TreatmentYr}_{it}$ takes unity in both 2017 and 2018 [only 2018] for the old [new] cohort and zero otherwise.[A3] We denote the individual- and year-month-specific fixed effects by u_i and v_t, respectively, in eq. (A1). The error term ϵ_{it} is clustered at the individual level.

The regression results based on eq. (A1) are reported in Table A10. The results in Column (1) are quantitatively similar to those in Table 4. Because the proportion of

[A3] Note that the old cohort includes only those who were in grade 7 as of 2017. Because those old-cohort students who were in grade 6 as of 2017 were in primary school in 2016, their pre-intervention attendance records are unavailable.

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 the CCT treatments depend on the treatment intensity, whereas the effects of the SMS treatment are lower and closer 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 significantly change the estimated impacts of our treatments on monthly attendance rates during the intervention years. In addition, Column (3) also indicates that the impacts of our treatments are persistent.

Lasso. We further use a double-selection lasso linear regression, where we select covariates from a list of potential controls such as education level of parents, number of male and female members in the household, religion of household head, and ownership of household assets at baseline (Table A11). The results from lasso estimation are qualitatively similar to the pure experimental design and baseline specification.

Spillover effects. Given our individual-level randomization, participating students in the same classroom can be assigned to different treatment arms. Anecdotal evidence gathered through informal interactions with some study participants suggests that some may make collective, rather than individual, attendance decisions. Therefore, it is important to account for the potential spillover 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. Conversely, if individuals in the control arm are discouraged due to exclusion from the intervention, we might overstate the impact of our interventions on school attendance. A comparison of educational aspirations between the baseline and endline surveys reveals a small increase the educational aspirations of students in the control group, albeit a smaller increase compared to those in the treatment groups. Thus, we believe that discouragement effects are unlikely but that there may potentially be some positive spillovers.

To understand the potential relevance of the peer effects, we collected social network data at the baseline–the names of the student participants' five closest friends from the same class regardless of whether the friends are participating in our study.[A4] Our social network data is likely to capture important spillover effects not only through the interactions between students but also through potential communications among parents to some extent, since parents' network is likely to be closely related to child's friendship network. 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 study.

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 the attendance of students in other groups. 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 CCT and SMS treatments on daily morning attendance, controlling for the proportion of friends in different treatment arms.

[A4] Because of the large number of students involved and limited budget available to us, it was infeasible to collect complete social network data. All survey respondents gave exactly five names. Existing literature on peer influence and education outcomes has considered such small social networks (Calvó-Armengol et al., 2009; Lavy and Sand, 2019).

Even though Table A12 suggests the presence of significant peer effects on attendance, the estimated treatment are very similar to those reported in Column (1) of Table 4. This result is not surprising, because the spillover effects are expected to be similar across different treatment groups due to the random assignment. The results in Table 4 also suggest that the spillover effects on the control group are likely positive and thus the estimates in Table 4 represent lower bounds of the effects of our interventions, if anything.

C.4 Additional Gender-Differentiated Results

Table A13 disaggregates the treatment effects in 2019 by the first and second halves of the year and the two genders. The table shows that the treatment effects on girls are stronger than those on boys. The treatment effects are also stronger in the first half than the second half. Table A14 presents the regression of changes in parental aspirations about the participating child's education level. As with Table 6, we consider continuous and discrete outcomes. All coefficients are statistically insignificant and small. Therefore, there is no evidence that our treatments have raised parental aspirations.

Table A15 disaggregates column (4) of Table 7 by grade. This indicates that the impact of our interventions on girls' early marriage comes primarily from grade-9 students. Table A16 presents the treatment effects on changes in parental investment in education. Continuous outcome refers to the changes in the logarithmic education expenditure between baseline and endline adjusted for inflation, and discrete outcome takes unity when the continuous outcome is positive. Table A16 shows that there is a notable increase in educational investment in girls for the SMS treatment arm, but the gender differences in parental investment in education are statistically insignificant.

In Table A17, we use the students' trust in their family on a four-point Likert scale (not trust at all, not trust very much, somewhat trust, or completely trust) as an outcome of interest. The "Continuous Outcome" uses the change this trust measure between the baseline and endline as an outcome. The "Discrete Outcome" takes unity if the continuous outcome is non-negative (i.e., the trust does not decrease) and zero otherwise. We use non-negative changes, rather than positive changes, in the continuous outcome cases to define discrete outcome, because an overwhelming majority of participants completely trust their family at the baseline, which means that their trust level cannot increase from the baseline. The table shows that the point estimates are all positive. Most notably, when the discrete outcome is used, the SMS treatment gives a positive and significant impact. This result suggests that our treatment is unlikely to have lead to undesirable forms of parenting such as coercion.

Table A18 reports regressions of the indicator for parental expectation of future cash transfers, which takes unity when the parents believe that they are somewhat or very likely to receive cash transfers in the next two years, and zero otherwise, using a model similar to eq. (1). The table shows that the CCT treatment significantly raises the expectation to receive cash transfer in the future, regardless of the gender of the child. The SMS treatment also increases the expectation but only insignificantly. Importantly, there is no significant gender difference in the impact of our interventions on parental expectations of future cash transfers. Hence, expectation of future cash transfers is unlikely to be the main driver of persistent attendance impact for girls.

Table A19 presents the estimates of value-added regression model of mathematics test score and shows that our treatments have no significant effect on the test scores. This result should be interpreted with caution as our mathematical test was very short due to the logistical constraint and thus its score is a noisy measure of true mathematical ability. It is also possible that our interventions may have improved the treated students' performance in non-mathematical subjects or even mathematics after a long enough exposure to treatment, although we cannot test these due to lack of data.

C.5 Details of the Cost-Effective Analysis

We consider the following three cost scenarios: (1) actual program costs, (2) policy costs without digital support, and (3) policy costs with digital support. Scenario (1) is based on our own implementation, which includes some features that are unlikely to be implemented when the program is scaled up (such as data collection by class representatives). Scenarios (2) and (3) are the policy-relevant scenarios. Under these scenarios, we assume that collecting attendance data generates no additional program cost, since teachers collect attendance data as part of their duties. Scenario (2) is our cost scenario for scaling up our intervention taking the current situation in Bangladesh as given. Under this scenario, the program costs includes the costs for digitizing attendance records and manually disbursing the transfers. 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, but it is not implausible in the foreseeable future (see also footnote 13). Under the vision of Digital Bangladesh, there has been a major push to increase the usage of digital technology across the country. While the proportion of population aged 25 and above has increased access to a bank account or mobile banking (37% in 2011 to 55% in 2021) (World Bank, 2023), there are ongoing efforts in the country to monitor school attendance through mobile phones. These trends are not exclusive to Bangladesh as many other developing countries such as Pakistan and Cambodia are also tapping into digital technology to monitor student or teacher attendance (Khan, 2019; Ministry of Education Youth and Sport, 2019).

For each of these three cost scenarios, we examine the following cost components: (A) communication; (B) collection of attendance data; (C) digitalization of attendance data; (D) payments to teachers; (E) payment to senior students; (F) actual disbursed cash; (G) travel for disbursement; (H) enumerator wage for disbursement; (I) RA wage for disbursement; and (J) processing. Further details of each component are given in the table note of Table A20. The cost components (B), (D), and (E) are included only in Scenario (1), since attendance data can be collected as part of teachers' duties when the program is scaled up. 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 also zero, because money is transferred through mobile banking. Because of this, the processing cost in Scenario (3) is slightly higher than those in Scenarios (1) and (2).

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 may not add much cost.

Using the cost components and scenarios discussed above, we conducted the cost-effective analysis presented in Section 6. Here, we conduct some robustness checks on the most cost-effective amount using the alternative specifications. First, we drop CCT_{ic} from eq. (2) 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 A2 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 A3 shows that the most cost-effective CCT amount is around 25-26 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. (2) 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 A4 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 A5 shows that the cost-effective amount of transfer is around 25-27 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.

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Table A1. Sample Size by Cohort, Grade, Gender in the Three Participating Schools [Continuing Students]

Cohort (Intervention Years)		Grade 6	7	8	9	Total
Old	Male	136	38	—	—	174
(2017 & 2018)	Female	144	38	—	—	182
	Total	280	76	—	—	356
New	Male	—	—	98	95	193
(2018)	Female	—	—	83	88	171
	Total	—	—	181	183	364

Table A2. Summary Statistics and Balance Check [Continuing Students]

	Old Cohort					
	Gain	Loss	SMS	Control	Overall	Orthogonality
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Participating child is female	0.505	0.540	0.506	0.494	0.511	0.937
Male HH at least pri. educ.	0.440	0.460	0.416	0.427	0.435	0.945
Male HH has at least sec. educ.	0.077	0.103	0.090	0.067	0.084	0.841
Female spouse at least pri. educ.	0.440	0.379	0.404	0.393	0.404	0.865
Female spouse at least sec. educ.	0.033	0.057	0.034	0.079	0.051	0.449
Household size	4.769	4.655	4.854	4.742	4.756	0.754
Male members in household	2.418	2.299	2.449	2.382	2.388	0.746
Female members in household	2.352	2.356	2.404	2.360	2.368	0.984
Owns residential land	0.945	0.977	0.989	0.978	0.972	0.321
Owns agricultural land	0.297	0.253	0.348	0.202	0.275	0.159
Has television or radio	0.341	0.425	0.449	0.472	0.421	0.303
Has a bicycle	0.319	0.391	0.337	0.382	0.357	0.705
Has a tube well	0.945	0.943	0.944	0.966	0.949	0.873
Height of the child	142.352	139.000	143.724	142.383	141.883	0.149
Weight of the child	35.374	36.000	36.393	36.191	35.986	0.790
Standardized test score	-0.071	0.045	0.198	-0.007	0.041	0.346
Observations	91	87	89	89	356	0.761
	New Cohort					
Participating child is female	0.473	0.516	0.522	0.500	0.503	0.912
Male HH at least pri. educ.	0.430	0.396	0.511	0.411	0.437	0.409
Male HH at least sec. educ.	0.075	0.088	0.156	0.122	0.110	0.304
Female spouse at least pri. educ.	0.398	0.473	0.500	0.567	0.484	0.148
Female spouse at least sec. educ.	0.043	0.066	0.078	0.056	0.060	0.789
Household size	4.688	4.516	4.622	4.744	4.643	0.586
Male members in household	2.548	2.286	2.311	2.567	2.429	0.126
Female members in household	1.129	2.231	2.311	2.178	2.212	0.586
Owns residential land	0.989	0.978	0.989	0.978	0.984	0.874
Owns agricultural land	0.215	0.341	0.356	0.244	0.288	0.091
Has television or radio	0.366	0.440	0.533	0.544	0.470	0.049
Has a bicycle	0.570	0.495	0.522	0.600	0.547	0.487
Has a tube well	0.957	0.989	0.967	0.989	0.975	0.399
Height of the child	148.412	149.413	145.316	145.994	147.299	0.269
Weight of the child	41.129	39.736	41.967	41.533	41.088	0.209
Standardized test score	-0.177	-0.079	-0.226	0.024	-0.115	0.313
Observations	93	91	90	90	364	0.027

Note: Male HH and female spouse refer to the household head and his spouse when the household is headed by a male. For about 7.6% of female-headed households, female spouse and male HH, represent the household head and her spouse, respectively. These variables take unity if the household head or spouse has at least primary or secondary education, and zero otherwise. The summary statistics for spouses are calculated over those households with the relevant household member. Ownership of assets (agricultural land, radio/television, bicycle, tube well) is a binary variable that takes unity if the household owns the asset, and zero otherwise. The weight and height of the child are measured in kilograms and centimeters, respectively. Test scores are normalized relative to the control mean and standard deviation. Column (5) shows the mean values for each variable. Column (6) shows the p-value for joint orthogonality.

Table A3. Effects of CCT and SMS treatments on Discontinuation

Dependent variable	Discontinued		
	Old Cohort	New Cohort	Both cohorts
	(1)	(2)	(3)
Gain	-0.025	-0.022	-0.023
	(0.030)	(0.030)	(0.021)
Loss	0.010	-0.007	0.003
	(0.027)	(0.051)	(0.027)
SMS	-0.027	0.005	-0.011
	(0.034)	(0.034)	(0.025)
P(Gain = Loss)	0.239	0.720	0.325
P(Gain = SMS)	0.816	0.515	0.595
P(Loss = SMS)	0.250	0.651	0.522
Observations	400	399	799
R^2	0.175	0.101	0.138
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 strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A4. Effects of CCT and SMS treatments by Phase [Continuing Students]

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	0.055	0.065**	0.084***	0.199***	0.071*	0.087**	0.081***	0.238***
	(0.034)	(0.032)	(0.025)	(0.027)	(0.038)	(0.034)	(0.024)	(0.028)
Loss	0.057*	0.069**	0.095***	0.247***	0.087**	0.086**	0.094***	0.281***
	(0.032)	(0.032)	(0.025)	(0.027)	(0.037)	(0.034)	(0.025)	(0.027)
SMS	0.083**	0.073**	0.030	0.042*	0.102***	0.081**	0.031	0.054**
	(0.033)	(0.032)	(0.025)	(0.025)	(0.038)	(0.033)	(0.025)	(0.025)
P(Gain = Loss)	0.947	0.924	0.678	0.090	0.674	0.968	0.589	0.142
P(Gain = SMS)	0.430	0.823	0.031	0.000	0.420	0.854	0.049	0.000
P(Loss = SMS)	0.456	0.902	0.011	0.000	0.692	0.890	0.014	0.000
Observations	21,360	17,440	36,000	36,000	21,360	17,440	36,000	36,000
R^2	0.070	0.060	0.060	0.095	0.099	0.106	0.064	0.116
Control Mean	0.609	0.673	0.605	0.461	0.468	0.593	0.553	0.359

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 & 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 strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A5. Treatment Effects by the First and Second Halves of the Phase [Continuing Students]

	Morning		Afternoon		Morning & Afternoon		Random Visit	
	First	Second	First	Second	First	Second	First	Second
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gain	0.092***	0.138***	0.105***	0.154***	0.107***	0.158***	0.095***	0.118***
	(0.022)	(0.023)	(0.023)	(0.024)	(0.023)	(0.024)	(0.027)	(0.029)
Loss	0.102***	0.164***	0.123***	0.177***	0.124***	0.181***	0.122***	0.157***
	(0.023)	(0.022)	(0.023)	(0.024)	(0.023)	(0.024)	(0.027)	(0.030)
SMS	0.045**	0.062***	0.052**	0.071***	0.052**	0.073***	0.060**	0.097***
	(0.022)	(0.022)	(0.023)	(0.023)	(0.023)	(0.023)	(0.027)	(0.030)
P(Gain=Loss)	0.675	0.282	0.427	0.371	0.509	0.350	0.298	0.172
P(Gain=SMS)	0.038	0.001	0.026	0.001	0.021	0.001	0.197	0.466
P(Loss=SMS)	0.014	0.000	0.002	0.000	0.003	0.000	0.020	0.039
Observations	55,364	55,436	55,364	55,436	55,364	55,436	4,582	3,878
R^2	0.081	0.068	0.098	0.099	0.095	0.097	0.070	0.061
Control Mean	0.580	0.560	0.514	0.513	0.483	0.477	0.612	0.595

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 strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A6. Associations between Attendance and CCT Recollection Prior to Disbursement [Continuing Students]

Dependent variable	Phase Morning Attendance Rate			
	(1)	(2)	(3)	(4)
Remembers CCT	0.143***	0.115***	0.115***	0.055**
	(0.021)	(0.024)	(0.024)	(0.024)
Kept SMS		0.048**	0.048**	0.012
		(0.021)	(0.021)	(0.018)
Loss			0.015	
			(0.023)	
Observations	1,074	1,074	1,074	1,074
R^2	0.063	0.068	0.069	0.756
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. Standard errors (in parentheses) are clustered at the individual level. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A7. The Effects of CCTs and SMS on Achieving 80% Attendance [Continuing Students]

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain	0.244***	0.239***	0.205***	0.224***
	(0.044)	(0.040)	(0.037)	(0.048)
Loss	0.286***	0.232***	0.220***	0.266***
	(0.046)	(0.041)	(0.039)	(0.050)
SMS	0.071*	0.038	0.035	0.137***
	(0.040)	(0.035)	(0.031)	(0.048)
P(Gain=Loss)	0.410	0.885	0.735	0.419
P(Gain=SMS)	0.000	0.000	0.000	0.085
P(Loss=SMS)	0.000	0.000	0.000	0.012
Observations	720	720	720	720
R^2	0.118	0.155	0.133	0.106
Control mean	0.134	0.095	0.061	0.235

Note: (1) "Morning", "Afternoon", "Morning & Afternoon", and "Random Visit", take unity if the child was present in school at least 80% of the valid relevant school days in, respectively, the mornings, afternoons, both mornings and afternoons, and during random visits, 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 strata and date fixed effects. They also control for unbalanced covariates at the baseline-ownership of agricultural land and radio/television. Robust standard errors (in parentheses) are used. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A8. The Effects of CCTs and SMS treatments on Daily Attendance During the Intervention for All Students

Dependent variable	Morning	Afternoon	Morning & Afternoon	Random visit
	(1)	(2)	(3)	(4)
Gain	0.103***	0.114***	0.117***	0.088***
	(0.025)	(0.025)	(0.025)	(0.025)
Loss	0.108***	0.123***	0.126***	0.121***
	(0.024)	(0.024)	(0.024)	(0.025)
SMS	0.042*	0.047**	0.049**	0.061**
	(0.024)	(0.024)	(0.024)	(0.025)
P(Gain=Loss)	0.833	0.725	0.754	0.195
P(Gain=SMS)	0.020	0.011	0.009	0.265
P(Loss=SMS)	0.010	0.003	0.003	0.016
Observations	123,500	123,500	123,500	8,869
R^2	0.074	0.096	0.092	0.057
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 strata and date fixed effects. They also control for unbalanced covariates at the baseline-ownership of agricultural land and radio/television. Robust standard errors (in parentheses) are used. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A9. Treatment Effect for All Students: Pure Experimental Design [Continuing Students]

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random visit (4)
Gain	0.112***	0.130***	0.133***	0.102***
	(0.023)	(0.024)	(0.024)	(0.026)
	[0.000]	[0.000]	[0.000]	[0.001]
Loss	0.132***	0.153***	0.154***	0.140***
	(0.022)	(0.024)	(0.024)	(0.026)
	[0.000]	[0.000]	[0.000]	[0.000]
SMS	0.056**	0.066***	0.066***	0.082***
	(0.022)	(0.023)	(0.023)	(0.026)
	[0.145]	[0.093]	[0.093]	[0.024]
P(Gain = Loss)	0.403	0.376	0.412	0.140
P(Gain = SMS)	0.015	0.011	0.008	0.447
P(Loss = SMS)	0.001	0.000	0.000	0.022
Observations	110,800	110,800	110,800	8,460
R^2	0.012	0.015	0.015	0.012
Control Mean	0.570	0.513	0.480	0.604

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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A10. Difference-in-Differences with Individual Fixed Effects [Continuing Students]

Dependent variable:	2016-2018		2016-2019	
Monthly attendance rate	(1)	(2)	(3)	(4)
Gain × TreatmentYr	0.126***		0.125***	
	(0.026)		(0.025)	
Loss × TreatmentYr	0.151***		0.152***	
	(0.027)		(0.026)	
SMS × TreatmentYr	0.054**		0.052**	
	(0.023)		(0.023)	
Gain × TreatmentYr × TrIntensity		0.121***		0.129***
		(0.021)		(0.020)
Loss × TreatmentYr × TrIntensity		0.136***		0.146***
		(0.020)		(0.020)
SMS × TreatmentYr × TrIntensity		0.007		0.013
		(0.018)		(0.018)
Gain × 2019			0.067**	0.030
			(0.031)	(0.025)
Loss × 2019			0.079**	0.029
			(0.033)	(0.026)
SMS × 2019			0.068**	0.036
			(0.031)	(0.025)
Observations	13,214	13,214	19,229	19,229
R^2	0.446	0.446	0.433	0.433

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. TreatmentYr 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A11. The Effects of CCT and SMS treatments on Daily Attendance During the Intervention (Double Lasso Estimates) [Continuing Students]

Dependent variable	Morning	Afternoon	Morning & Afternoon	Random Visit
	(1)	(2)	(3)	(4)
Gain	0.115***	0.130***	0.133***	0.105***
	(0.022)	(0.023)	(0.023)	(0.025)
Loss	0.133***	0.150***	0.153***	0.138***
	(0.022)	(0.023)	(0.023)	(0.025)
SMS	0.054**	0.062***	0.062***	0.077***
	(0.021)	(0.022)	(0.022)	(0.025)
P(Gain = Loss)	0.432	0.385	0.410	0.181
P(Gain = SMS)	0.006	0.003	0.003	0.248
P(Loss = SMS)	0.000	0.000	0.000	0.012
Observations	110,800	110,800	110,800	8,460
R^2	0.072	0.096	0.093	0.059
Control Mean	0.570	0.513	0.480	0.604

Note: The above estimates are from a lasso regression. (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 strata and date fixed effects. They also control for unbalanced covariates at the baseline-ownership of agricultural land and radio/television. Additional controls include education levels of male and female household heads, number of male and female members in the household, ownership of residential land, bicycle, tubewell, electric fan, dummy for religion, dummy for whether the participating child received scholarship, and remittances. Standard errors (in parentheses) are clustered at the individual level. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A12. Accounting for Peer Effects [Continuing Students]

Dependent variable	Morning	Afternoon	Morning & Afternoon	Random Visit
Gain	0.119***	0.134***	0.137***	0.109***
	(0.022)	(0.023)	(0.023)	(0.025)
Loss	0.133***	0.150***	0.152***	0.139***
	(0.022)	(0.023)	(0.023)	(0.025)
SMS	0.054**	0.061***	0.062***	0.077***
	(0.022)	(0.022)	(0.023)	(0.025)
GainProp	0.088	0.098*	0.097*	0.080
	(0.054)	(0.056)	(0.058)	(0.061)
LossProp	0.001	0.005	-0.010	0.057
	(0.060)	(0.061)	(0.062)	(0.067)
SMSProp	0.115**	0.131**	0.128**	0.119*
	(0.054)	(0.056)	(0.058)	(0.063)
ControlProp	-0.009	0.004	0.002	0.026
	(0.056)	(0.057)	(0.059)	(0.065)
P(Gain = Loss)	0.542	0.486	0.514	0.225
P(Gain = SMS)	0.004	0.002	0.002	0.188
P(Loss = SMS)	0.000	0.000	0.000	0.011
Observations	110,800	110,800	110,800	8,460
R^2	0.074	0.097	0.095	0.061
Control Mean	0.570	0.513	0.480	0.604

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. The above specifications control for strata 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. The above specification controls for the class and date fixed effects. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A13. The Effects of CCT and SMS treatments in the First and Second Half of 2019 [Continuing Students]

Dependent variable	Monthly Attendance Rates					
	First half			Second half		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)
CCT	0.053***	0.023	0.082***	0.014	-0.012	0.040*
	(0.017)	(0.027)	(0.022)	(0.017)	(0.025)	(0.022)
SMS	0.033	0.005	0.063**	0.028	-0.002	0.058**
	(0.021)	(0.032)	(0.027)	(0.019)	(0.028)	(0.024)
P(CCT = SMS)	0.273	0.517	0.384	0.356	0.680	0.372
P(CCT^{Boys}=CCT^{Girls})		0.089			0.113	
P(SMS^{Boys}=SMS^{Girls})		0.171			0.105	
Observations	3,600	1,775	1,825	4,150	2,049	2,101
R^2	0.377	0.278	0.446	0.440	0.422	0.464
Control Mean	0.290	0.275	0.306	0.291	0.320	0.262

Note: Columns (1)-(3) ("First half") estimate the effect of the intervention on morning attendance in the months of January to May in 2019. Columns (4)-(6) ("Second half") estimate the effect of the intervention on morning attendance in the months of June to December in 2019. The Control group is the reference category in all specifications. The p-values for the tests of equality of means between boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A14. The Effects of CCT and SMS treatments on Changes in Parents' Aspirations

	Continuous Outcome			Discrete Outcome		
	All (1)	Boys (2)	Girls (3)	All (4)	Boys (5)	Girls (6)
CCT	0.098	-0.003	0.195	-0.076	-0.098	-0.052
	(0.257)	(0.262)	(0.433)	(0.064)	(0.090)	(0.093)
SMS	0.230	0.267	0.197	0.024	0.099	-0.047
	(0.274)	(0.272)	(0.492)	(0.067)	(0.083)	(0.109)
P(CCT=SMS)	0.567	0.428	0.995	0.128	0.008	0.960
P(CCT^{Boys}=CCT^{Girls})		0.630			0.681	
P(SMS^{Boys}=SMS^{Girls})		0.882			0.263	
Observations	475	227	248	475	227	248
R^2	0.056	0.081	0.047	0.081	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 boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A15. The Effects of CCT and SMS treatments on Child Marriage by Grade for Girls

Dependent variable	Child Marriage		
	Grade 7	Grade 8	Grade 9
	(1)	(2)	(3)
CCT	-0.062	0.000	-0.235*
	(0.031)	(0.029)	(0.077)
SMS	-0.044	-0.053	-0.241**
	(0.082)	(0.058)	(0.054)
P(CCT=SMS)	0.799	0.423	0.875
Observations	158	141	92
R^2	0.036	0.052	0.187
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 boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A16. The Effects of CCT and SMS treatments on Parental Investment in Education

	Continuous Outcome			Discrete Outcome		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.050	0.105	-0.004	0.016	0.021	0.009
	(0.055)	(0.061)	(0.093)	(0.031)	(0.050)	(0.039)
SMS	0.142**	0.111	0.175**	0.086*	0.054	0.115*
	(0.051)	(0.069)	(0.076)	(0.041)	(0.064)	(0.053)
P(CCT=SMS)	0.187	0.947	0.121	0.127	0.657	0.049
P(CCT^{Boys}=CCT^{Girls})		0.391			0.888	
P(SMS^{Boys}=SMS^{Girls})		0.662			0.543	
Observations	737	368	369	737	368	369
R^2	0.164	0.165	0.149	0.107	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 logarithmic 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 boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A17. The Effects of CCT and SMS treatments on Students' Trust in Family

	Continuous Outcome			Discrete Outcome		
	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)
CCT	0.032	0.030	0.036	-0.035	-0.009	-0.061*
	(0.064)	(0.067)	(0.109)	(0.021)	(0.033)	(0.032)
SMS	0.077	0.035	0.118	-0.017	0.009	-0.045
	(0.077)	(0.063)	(0.134)	(0.024)	(0.027)	(0.044)
P(CCT=SMS)	0.410	0.939	0.113	0.391	0.527	0.332
P(CCT^{Boys}=CCT^{Girls})		0.958			0.280	
P(SMS^{Boys}=SMS^{Girls})		0.535			0.302	
Observations	724	367	357	724	367	357
R^2	0.091	0.075	0.100	0.046	0.050	0.049
Control mean	0.000	0.054	-0.059	0.938	0.967	0.906

Note: The dependent variable ("Continuous Outcome") in Columns (1)-(3) is the change between the endline and baseline surveys in the participating children's trust in their family on a four-point Likert scale. 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 non-negative and zero otherwise. The Control group is the reference category in all regressions. The p-values for the tests of equality of means between boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A18. The Effects of CCT and SMS treatments on Expectations of Future Cash Transfers

	All (1)	Boys (2)	Girls (3)
CCT	0.172***	0.197**	0.147**
	(0.043)	(0.067)	(0.059)
SMS	0.046	0.069	0.020
	(0.036)	(0.057)	(0.051)
P(CCT=SMS)	0.006	0.074	0.048
,P(CCT^{Boys}=CCT^{Girls})		0.514	
P(SMS^{Boys}=SMS^{Girls})		0.595	
Observations	783	392	391
R^2	0.087	0.095	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 boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A19. The Effects of CCT and SMS treatments on Mathematics Test Score

Dependent variable	Endline Test Score		
	All	Boys	Girls
	(1)	(2)	(3)
CCT	-0.038	-0.073	0.003
	(0.085)	(0.124)	(0.122)
SMS	0.039	0.006	0.058
	(0.106)	(0.138)	(0.164)
Baseline test score	0.077**	-0.003	0.164**
	(0.034)	(0.037)	(0.046)
P(CCT=SMS)	0.325	0.443	0.653
P(CCT^{Boys}=CCT^{Girls})		0.688	
P(SMS^{Boys}=SMS^{Girls})		0.792	
Observations	718	364	354
R^2	0.057	0.042	0.071

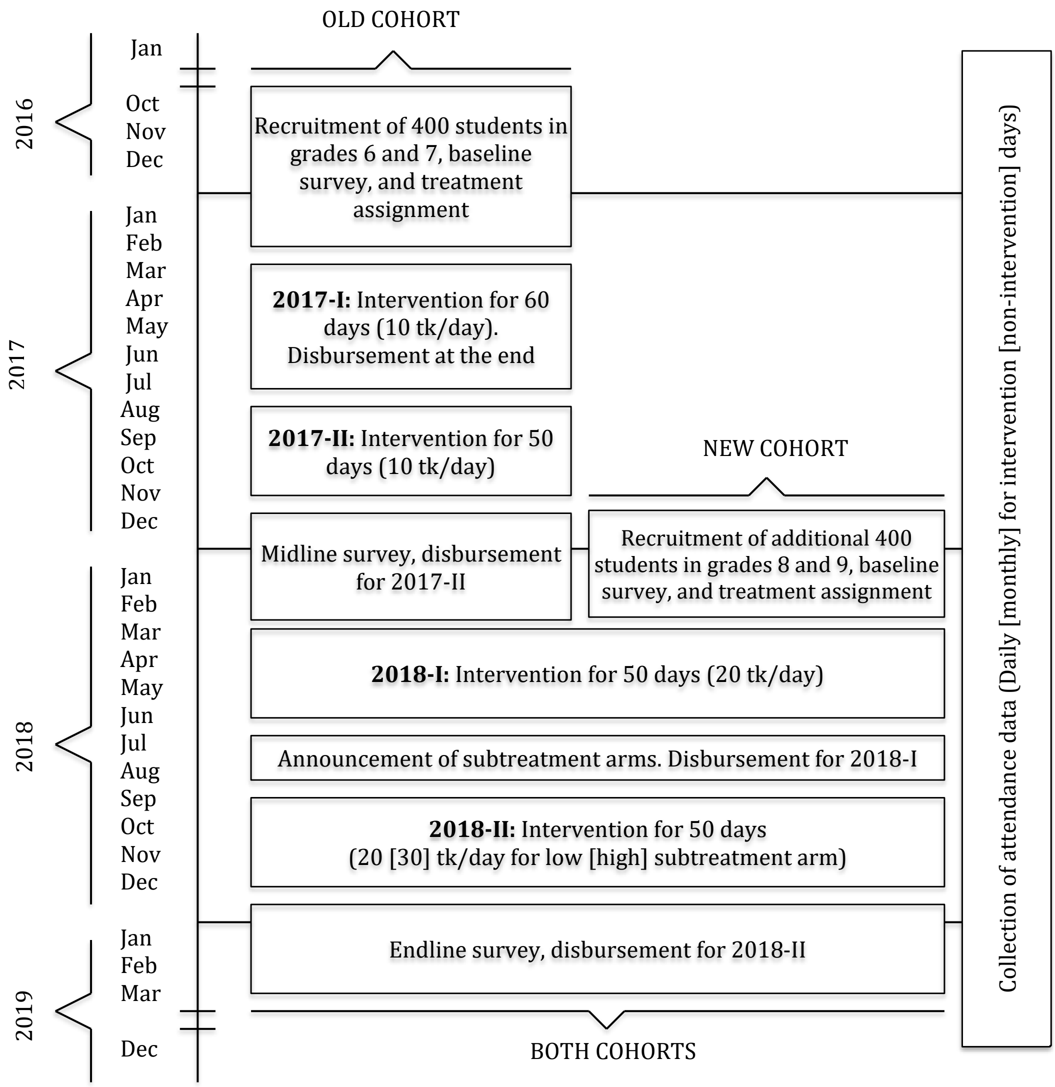
Note: The mathematics test were administered to 718 students at the time of the endline survey, when schools were closed. We could not administer the test to the remaining 81 students, because they were not at home when the research team visited the household to conduct the survey. Both baseline and endline test scores are normalized relative to control mean and standard deviation for every class combination. The Control group is the reference category in all regressions. The *p*-values for the tests of equality of means between boys and girls for each of the CCT and SMS treatment arms are given in the middle panel. The above specifications control for strata 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. ***, **, and * indicate statistical significance at 1, 5, and 10 percent levels, respectively.

Table A20. Cost Calculations under Different Scenarios [Continuing Students]

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	112	112	112	112	112	112
(B) Collection of attendance data	900	900	0	0	0	0
(C) Digitization of attendance Data	720	720	361	361	0	0
(D) Payment to teachers	53	53	0	0	0	0
(E) Payment to senior students	7	7	0	0	0	0
(F) Actual disbursed cash	0	1,923	0	1,923	0	1,923
(G) Travel (disbursement)	0	81	0	77	0	0
(H) Enumerator wage (disbursement)	0	134	0	373	0	0
(I) RA wage (disbursement)	0	63	0	174	0	0
(J) Processing	0	207	0	207	0	279
Total Cost	1,792	4,200	473	3,227	112	2,314
Effect Size	5.4	12.4	5.4	12.4	5.4	12.4
Cost-Effectiveness Measure (λ)	3.01	2.95	11.42	3.84	48.21	5.36
Alternative Cost-Effectiveness Measure ($\tilde{\lambda}$)	3.01	5.45	11.42	9.51	48.21	31.71

Note: "(A) Communication cost" includes the cost of sending weekly text and voice messages 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 5, 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 $5.4/1.792 = 3.01$. Alternative cost-effectiveness measure $\tilde{\lambda}$ is the same as λ, except that cost component (F) is excluded from total cost calculation.

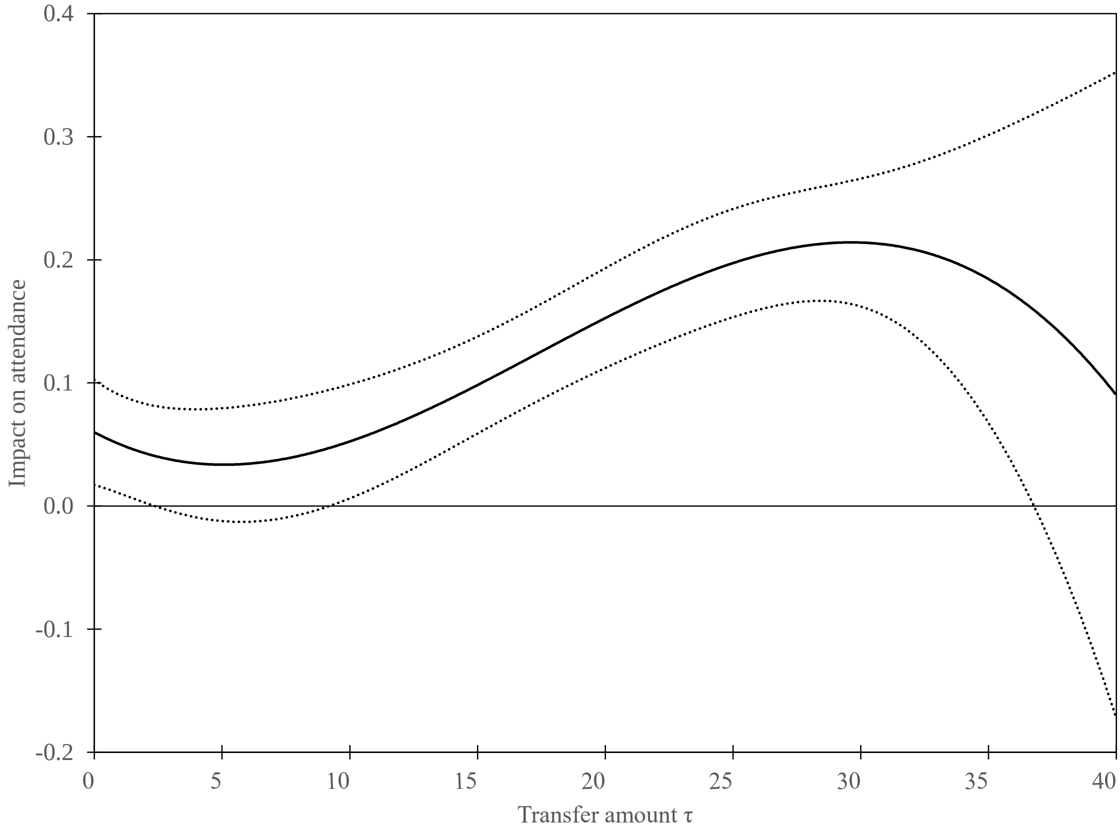
Figure A1. Timeline of the Study



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,

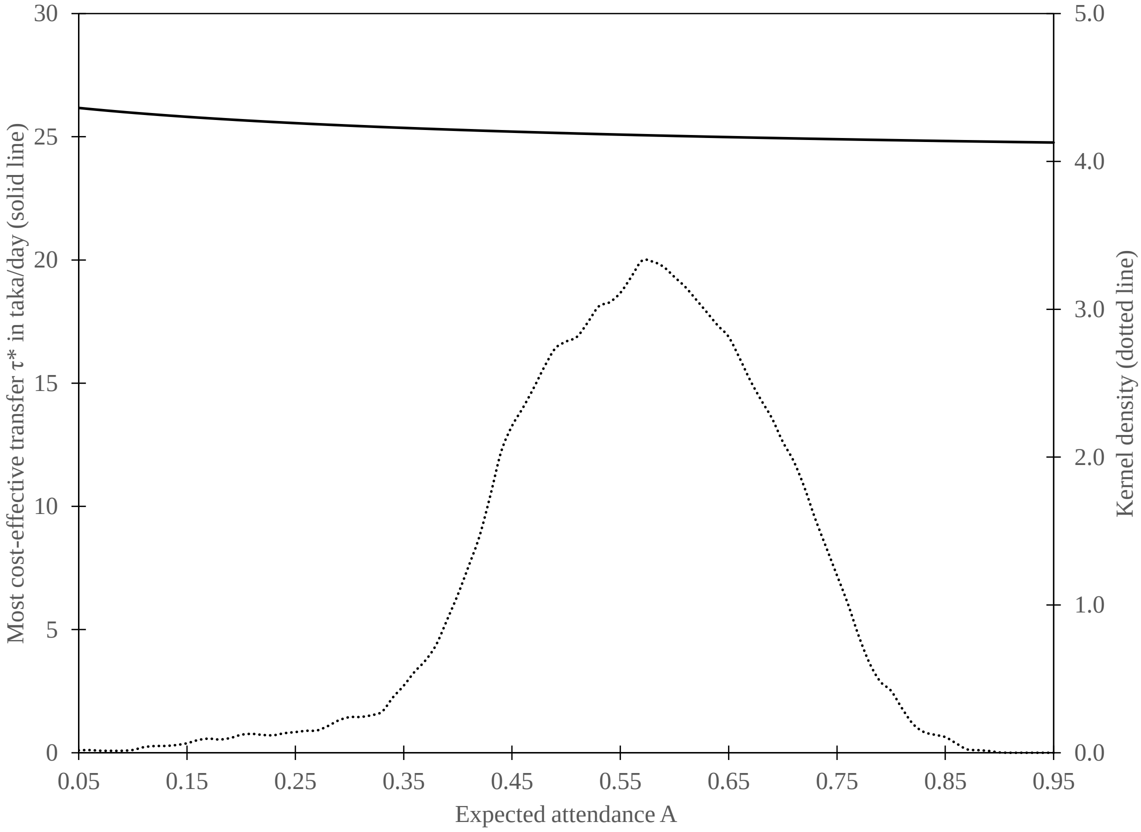
and (iii) post-intervention days in 2019. The daily transfer amount in parentheses in each intervention phase is applicable only to the Gain and Loss treatment groups.

Figure A2. The Estimated Attendance Impact of CCT using a Cubic Specification



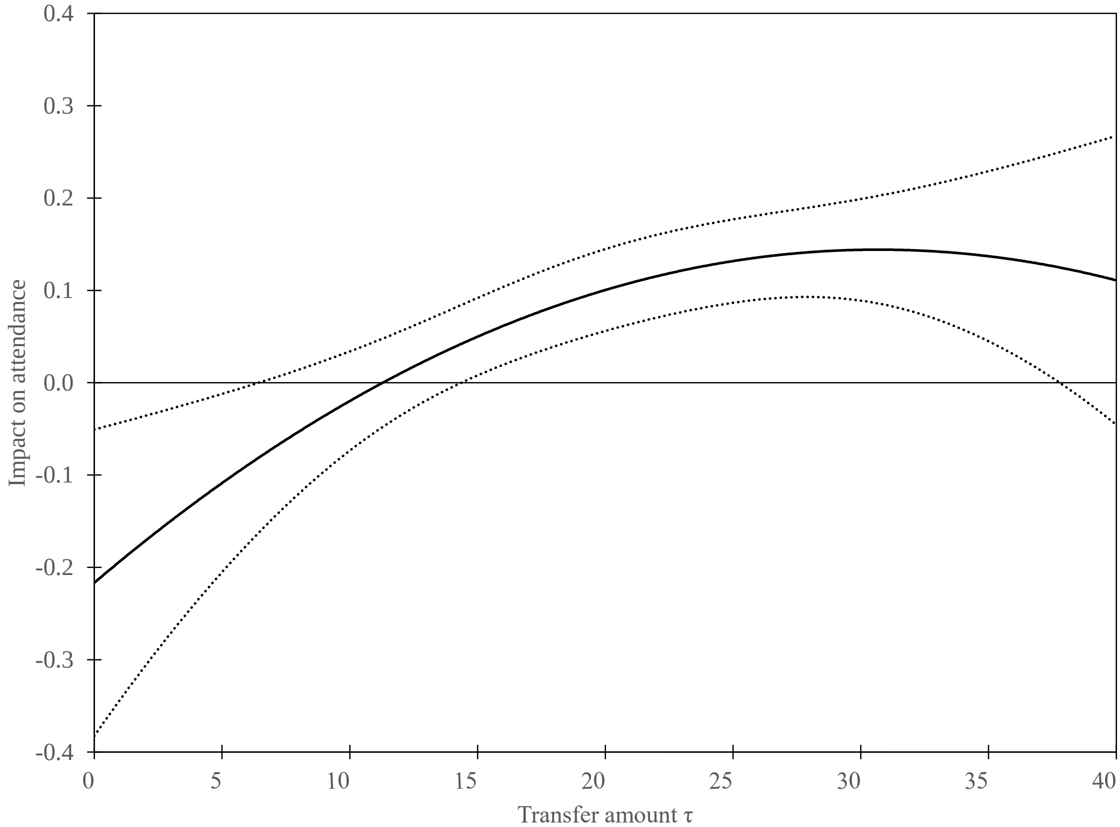
Note: The figure plots the estimated impact of CCT with daily transfer τ on daily morning attendance, $f(\tau)$, from a regression of eq. (2) with cubic–instead of quadratic–specification.

Figure A3. Most Cost-Effective Transfer using a Cubic Specification



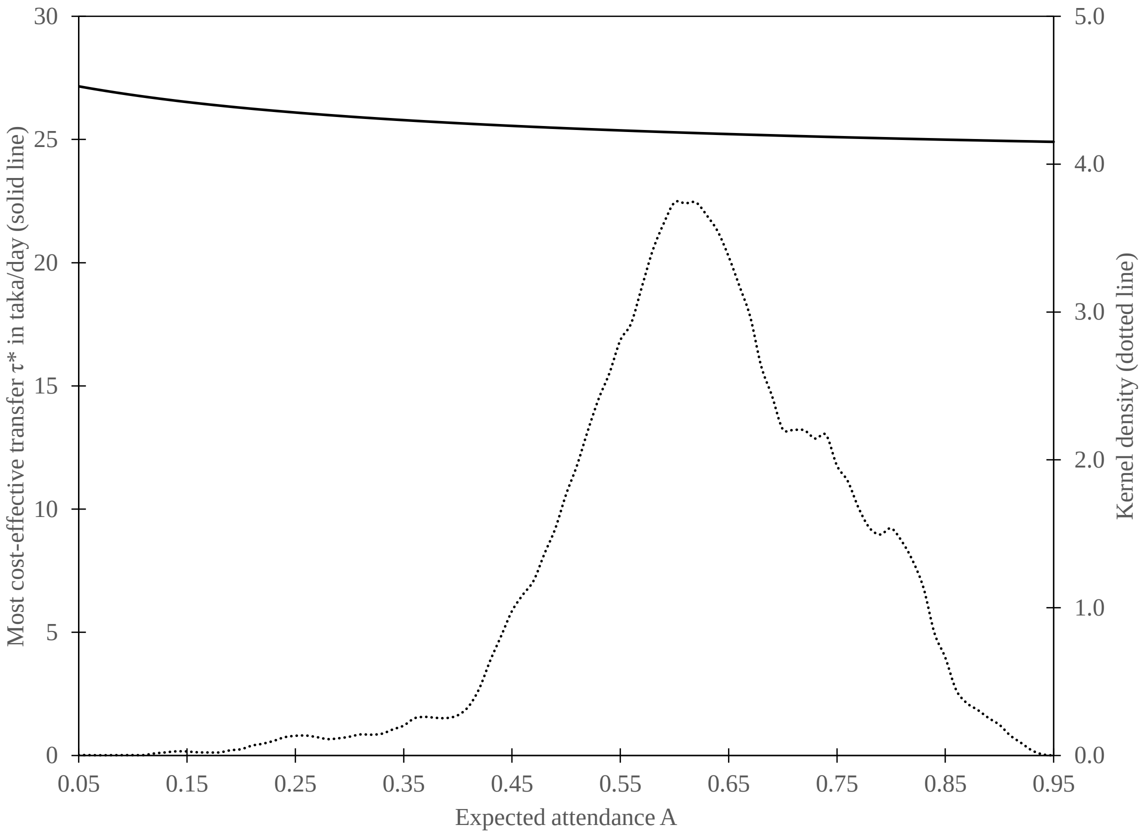
Note: The solid line depicts the most cost effective transfer, τ^*, as a function of control attendance, $A_{ict} \equiv \beta_0 + \gamma X_{ic} + u_c + v_t$, using a cubic specification. The dotted line depicts the kernel density estimate of control attendance A_{ict}.

Figure A4. 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. (2) on the subsample of treated students in the CCT and SMS treatment arms only.

Figure A5. 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}.