Conditional Cash Transfer, Loss Framing, and SMS Nudges: Evidence from a Randomized Field Experiment in Bangladesh*

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Abstract

Conditional cash transfers (CCTs) have become one of the most common policy interventions to increase school attendance, but the cost-effectiveness of such interventions has not attracted the attention it deserves. Hence, in addition to a standard CCT implementation, our rich unique dataset on daily attendance allows us to experimentally study two potential ways to improve the cost-effectiveness of school attendance interventions: (i) SMS information nudges and (ii) loss framing in CCTs. The former provides school attendance information to parents and the latter exploits the endowment effect. Consistent with the existing literature, CCT intervention significantly increases school attendance. Though the difference between gain

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and loss framing is not statistically significant, the point estimate of the Loss treatment is consistently higher than that of the Gain treatment.The SMS treatment has a modest impact on school attendance but the overall cost of treatment is low. We also find diminishing marginal impact of cash transfer amount on attendance, indicating that the intensive margin matters. Thus, both loss framing and SMS nudges can be considered as alternative cost-effective approaches to promote attendance in schools in developing and less developed economies where resources are typically limited.

Keywords: conditional cash transfers, loss aversion, peer effect, information treatment, Bangladesh

JEL: D91, H75, I21, I28, O22

1 Introduction

Conditional cash transfers (CCTs) have gained popularity since the inception of Progresa in 1997 in Mexico.[1] CCT programs have been implemented with various aims such as increasing demand for food (Attanasio and Lechene, 2010), empowering women (Almås et al., 2018), improving health, and reducing neonatal and infant mortality (Barber and Gertler, 2010), child marriage (Buchmann et al., 2017), and even deforestation (Jayachandran et al., 2017). As of 2014, they had spread to about 70 countries (Lindert, 2014) with governments engaging as high as 1.2 percent of their GDP on CCT programs (for example, the Bobo Desarrollo Humano program in Ecuador). While the details of the implementation vary substantially across programs, CCT interventions typically aim at promoting investment in human capital in the form of education, health, and/or nutrition. These programs require beneficiary households to fulfill certain conditions, such as regular school attendance of school-age children or regular visits to health clinics, to receive cash transfers. Even though the specific aims of the programs vary, one of the main objectives of these programs has been to suppress intergenerational transmission of poverty by breaking the vicious cycle of low human capital investment, through the use of cash incentive to promote desirable behavior.

Studies have shown that the Progresa program and its successors led to a significant positive impact on school enrollment for all grades (Behrman et al., 2009; Dubois et al., 2012; Shultz, 2004; Todd and Wolpin, 2006). With the success of Progresa, CCT has become a popular policy tool and has been implemented in Latin America and other developing regions over the last two decades. CCT programs have thus promoted school enrollment in Brazil (Glewwe and Kassouf, 2012), Bangladesh (Khandker et al., 2003), Ecuador (Schady et al., 2008), and Colombia (Attanasio et al., 2010), among others, even though the short-term impact on student learning as measured by test scores tends to be small and insignificant at best.[2]

[1] Progresa was later renamed to Oportunidades in 2001 and Prospera in 2014 before being terminated in 2019. Parker and Todd (2017) provide a review of Progresa/Oportunidades.

[2] See Fiszbein and Schady (2009) for a review. Glewwe and Muralidharan (2016) and Murnane and Ganimian (2014) provide some discussion of CCT interventions in comparison to various other education interventions.

Our study relates to a large body of literature on CCTs. Some programs that are CCT on paper may look more like unconditional cash transfers (UCTs) when there is poor monitoring and enforcement of conditions.[3] We focus on CCTs instead of unconditional cash transfers (UCTs) because the conditionality of CCTs may be important in incentivizing parents to send their children to school. Existing studies indeed indicate that conditionality is essential when raising school attendance is an important policy goal (Baird et al., 2011; Martinelli and Parker, 2003; De Brauw and Hoddinott, 2011). This could be particularly relevant in agricultural areas, where parents may be myopic and value field work or early marriage over education. How the conditions are written and enforced is, therefore, an important consideration in our study. In particular, we design our field experiment with the objective of improving school attendance in a cost-effective way. Our study thus has the potential to generate important policy implications on the design of CCTs in developing countries.

Our intervention has a number of distinct features compared to other CCT studies that have looked to improve school attendance. Unlike existing studies, this experiment features (i) nudges through Short Message Service (SMS) and (ii) CCT with loss framing to potentially improve the cost-effectiveness of interventions. These features may be useful, because the former involves no cash transfer and the latter can exploit the widely documented psychological trait of loss aversion—losses loom larger than gains as Kahneman and Tversky (1979) describe it—at no additional program cost. Despite its widespread application, the existing CCT literature provides only limited insights into the features of cost-effective interventions to increase school attendance. This is because most existing studies examine the impact of CCT programs and not the features that would improve their cost-effectiveness. The lack of adequate attention to cost-effectiveness is surprising, particularly given that these programs are typically implemented in developing countries where resources for cash transfers are limited. If the Loss treatment and SMS information can generate the same desired effect at less or no additional cost as compared to the conventional gain framing, policy-makers can adopt loss framing and information nudges to increase secondary school atten-

[3]See, for example, Baird et al. (2011) for an experimental study that compares CCTs and UCTs on schooling and other outcomes such as early pregnancy.

dance. Our study thus contributes to a small body of literature on efficient design of CCTs, such as de Janvery and Sadoulet (2006), Filmer and Schady (2011), and García and Saavedra (2017), by exploring the following avenues to increase CCT's cost-effectiveness.

First, we study whether simple SMS nudges on school attendance increase the likelihood of children going to school. Since sending SMS is inexpensive and a vast majority of households in the region owns mobile phones, information transmission can be a cost-effective way to bring children to school from the perspective of a policy-maker. Our study shows that this is indeed the case, even though the impact of SMS on attendance is modest compared to the CCT treatment arms. This indicates that SMS nudges can be an important policy tool in places where resources for policy interventions are limited. We also experimentally explore the relevance of loss framing, which attempts to exploit loss aversion, to CCTs. Since implementing a CCT under a loss framing, instead of the conventional gain framing, can be done virtually at no additional cost, we can potentially make CCTs more impactful for a given amount of transfer. We find that this is possible, even though the additional impact arising from the use of loss framing is small and statistically insignificant.

Second, we vary the amount of transfers to understand the relevance of intensive margin in the treatment, which has largely been unexplored in the literature. The current literature provides little insight into how to do such calibration to have a cost-effective policy. To our knowledge, Filmer and Schady (2011) is the only study that rigorously explores the relevance of intensive margin, but even they have only two levels of transfers while we have three levels. As with other incentive programs, a CCT program with a given transfer amount does not change the behavior of always takers (i.e., households that would send children to school regardless of the presence of the CCT program) and never takers (i.e., households that would not send children to school regardless of the presence of the CCT program). Obviously, never takers may become compliers (i.e., households that would send children to school if and only if some cash transfers are given when the children attend school) when the reward for school attendance is increased. Hence, it is important to calibrate the amount of cash transfers to strike the balance between the increased attendance from compliers and leakage of resources towards always takers in the intensive margin. We provide evidence of diminishing marginal impact of transfer. That is, at a very low level of current transfer, the marginal effect is small. When the current level of transfer is increased, the marginal impact on attendance also goes up. A further increase in the transfer amount leads to smaller increase in attendance, indicating that the marginal impact tends to diminish. Thus, we find that there are diminishing marginal returns to transfer size.

Finally, we collect daily attendance during our intervention period, as opposed to aggregate attendance rate in a year or school semester. This enables us to investigate the seasonality of the treatment impact across different months or during the lean and harvesting periods. This consideration is important since a sizable fraction of students in our sample belong to agricultural households. We also collect information on social network for each study participant to understand the peer effect in attendance. Taken together, this study offers a new set of insights that could assist policy-makers to design cost-effective interventions to increase school attendance.

We have a number of interesting findings from our analysis. First, morning attendance for students who received a CCT treatment with a loss framing significantly increased by 11.2 percentage points relative to the students in the control group. This impact is higher than that for the conventional CCT treatment under a gain framing, but the difference is statistically insignificant. Second, sending information to parents through SMS also increased attendance by 4.7 percentage points relative to the control arm, which is statistically significant. The SMS treatment arm is found to be more cost-effective than the CCT treatment arms in bringing children to school.

Third, variation in the amount of cash transfer across different phases allows us to test the impact on attendance in the intensive margin. We find that the initial 10 Bangladeshi taka/day (about 0.12 USD/day) is too little to have any perceptible impact on attendance. The subsequent increase to 20 taka/day promotes attendance significantly. However, the marginal impact from a further increase of 20 taka/day to 30 taka/day, though positive and statistically significant, was not as large. Fourth, our analysis reveals that the impact of the CCT treatment with a loss framing is high for agricultural households during the harvest season. This result is particularly apparent in the latter half of 2018, when the transfer amount was increased to 30 taka/day for some participants. Thus, a moderate amount of cash transfer appears to make a large impact near the harvest season when school attendance drastically declines due to rising income opportunities in the agricultural sector. However, the impact of the same amount of cash transfer is not as large during the lean period, potentially because the opportunity cost of sending children to school may be very small. Fifth, we find evidence of peer effect in attendance across all treatment arms. Interestingly, even when we control for such peer interactions, the point estimates for treatment assignment are similar, indicating that the peer effects are approximately uniform across different treatment arms and the estimated treatment impacts mentioned above are valid net of peer effects. Finally, we find suggestive evidence that the CCT treatment with a loss framing helps older girls in higher grades stay in school and delays incidence of child marriage.

The rest of the paper is organized as follows. In Section 2, we review existing studies on loss aversion and information treatment, in particular those with policy applications and discuss the relevance and contribution of our study to this literature. We then provide the details of the design of our field experiment in Section 3. Section 4 describes the data sources and measurement of school attendance and provides balance checks across different treatment arms at baseline. Section 5 discusses the specification of econometric model used in our empirical analysis. Section 6 provides our main empirical results on the impact of our treatments on school attendance. Section 7 presents the impact on other outcomes such as post-intervention school enrollment, child marriage, child labor, test scores, and spending patterns. Section 8 performs a cost-effective analysis for the different treatment arms, and Section 9 concludes.

2 Relevance and Contributions to the Related Literature

This study explores two possible avenues of increasing the cost-effectiveness of CCTs—loss framing and SMS nudges. As such, in addition to a large body of the

CCT literature discussed above, this paper relates to the studies on loss aversion and information treatment, and particularly those with policy applications. In this section, we discuss the relevance and contributions of our intervention to these studies.

The use of loss framing in this study was inspired by the literature on loss aversion, which describes the phenomenon that people tend to react more to losses than to gains of the same amount (Kahneman and Tversky, 1979; Kahneman et al., 1990). Based on an often cited figure, pain from a loss is twice as large as the pleasure from a gain of the same magnitude (Todd and Wolpin, 1991; Tversky and Kahneman, 1992), even though the external validity of this figure is debatable (Chapman et al., 2018; Fehr-Duda and Epper, 2012).

Loss aversion has been increasingly used to explain seemingly irrational behaviors in a wide range of situations, ranging from the supply of cab services in New York City (Camerer et al., 1997; Crawford and Meng, 2011) and measuring productivity of workers in a Chinese high-tech manufacturing facility (Hossain and List, 2012) to credit card use in Israel (Ganzach and Karsahi, 1995) and nutritional choice of American kids (List and Samek, 2015). However, the available evidence on the effectiveness of loss framing relative to gain framing has been mixed, and our result that loss framing has a small but desirable effect adds to this literature. We also offer some explanation as to why the loss framing has only a small effect.

Despite its general appeal, there are only a limited number of applications of loss framing to education policy. For example, Fryer Jr et al. (2012) found moderate impact on mathematics scores of students through loss framed incentives for teachers. Levitt et al. (2016) observed that student performance was better when rewards were framed in terms of losses than gains, even though the difference was not statistically significant. Since loss framing does not add costs to the conventional gain framing, one could potentially exploit it to increase the cost-effectiveness of a given intervention.

Our intervention also studies the impact of sending attendance information through SMS. Mobile phones have become widespread in the developing and less developed world over the recent years (Howard and Mazaheri, 2009), and this has, on average, led to better educational outcomes (Valk et al., 2010), and employment levels (Klonner and Nolen, 2008). Some studies have exploited this increased access to network coverage and used information transmission as a means to promote attendance at health promotion centres (Chen et al., 2008), and enhance productivity (Fafchamps and Minten, 2012; Abraham, 2006) in the developing world.[4] In our intervention, to disentangle the effect of information provision from loss framing, we also include the SMS treatment arm to identify the impact of conveying attendance information to parents. Since sending SMS is inexpensive, information transmission might be a cost-effective way of getting children to attend school. Hence, this study also contributes to the strand of applied behavioral economics literature that uses the traits of human psychology to design education policies (Jabbar, 2011; Lavecchia et al., 2012; Koch et al., 2015).

Our study is distinct from existing studies in a number of ways. First, contrary to Progresa and most other CCT programs, our conditions for eligibility of cash transfer are linear. This means that cash transfer amount is proportional to the number of school days attended during the intervention period, eliminating the possibility of any threshold effect. As a result, our CCT intervention gives an incentive to *every* household to send children to school on *every* intervention day. In contrast, when there is only one level of transfer per month or school term, the intervention does not give any incentive to those households which have already passed the threshold or which cannot reach the threshold because they have already missed too many days. Our set-up also makes person-day level analysis both straightforward and meaningful. Second, we provide incentives to the household, instead of the teacher or the student. Though there have been studies that have examined the effects of parental involvement on children's educational outcomes in France (Avvisati et al., 2013), we provide direct monetary incentives to the household and conduct the study in a developing country. Third, the timing of the disbursement of cash transfer is delayed where the monetary benefits are distributed to the households at the end of every phase. This allows us to replicate a real-life setting, where daily transfers are impractical. Though SMSes would help reinforce the role of the current transfer amount as the reference amount, one must note that the disbursement at the end of each phase might fail to generate the intended endowment effect for people in the Loss treatment arm. Finally, we

[4]See Aker and Mbiti (2010) for a comprehensive literature review on the impact of mobile phones on economic development in Africa.

collect data on study participants' social network, which enables us to identify peer effect in attendance.

3 Design of the Field Experiment

The field experiment was conducted in the district of Gaibandha in northern Bangladesh. Gaibandha is a relatively poor district with 48 percent of the population living below the poverty line in 2016, compared to the national average of 32 percent (World Bank, 2016). It is prone to serious flooding with heavy rainfall from June to August as well as occasional draughts. Given this background, it is not surprising that the Gaibandha district performs poorly in average educational attainment. The primary and secondary education completion rates among adults in the region are 24 percent and 11 percent in 2016, which are below the national averages of 33 percent and 13 percent respectively (World Bank, 2016). Therefore, it is imperative that we understand the school attendance behavior and devise effective policies to bring children to school in places like Gaibandha.

Also, Gaibandha is a predominantly agricultural district, where 71 percent of the working population is in agriculture, a figure well above the national average of 47 percent (World Bank, 2016). Hence, our study area offers an ideal setting to test whether CCT and SMS interventions are effective in bringing children to school from agrarian households across different agricultural cycles. This consideration is important as we may be able to achieve a higher overall attendance by setting the transfer amounts in accordance to the varying opportunity cost of sending children to school.

Timeline of study

Since a majority of countries in the developing world, including Bangladesh, has achieved universal primary education envisaged in the Millennium Development Goals, education policies have shifted their foci to the enrollment rates at the secondary and higher levels. Against this backdrop, our intervention focuses on secondary school students in grades 6–9 enrolled in one of our three study schools[5] and residing in one of the three catchment unions in Gaibandha.[6]

There were other educational programs that were also implemented in the country during our intervention period, for example, the school-feeding program during 2017-2020 (UN, 2020), and the Female Secondary School Attendance Project that aimed to increase girls' enrollment and retention in secondary schools through stipends and tuition waivers since the early 1990s (Sosale et al., 2019). However, there is no particular reason to believe that the students in our study sample were differentially affected by any of these interventions.

Our intervention was carried out in academic years 2017 and 2018, where academic years coincide with calendar years. In each of these two academic years, there are two phases of intervention, each of which has a pre-determined number of intervention days—60 days in Phase 1, 2017, and 50 days in all other phases.[7] Thus, not all school days were part of our intervention days. When there were some unexpected school closures, we made adjustments by pushing the end of the phase backward.[8] That is, we change the days that were planned to be non-intervention days before the start of the phase into intervention days. The transfer amount given to households was calculated based only on the attendance of the children during the actual intervention days after adjustments were made.

We recruited 400 students at the start of each of academic years 2017 and 2018. We refer to those who were recruited in 2017 and 2018 as 'old' and 'new' cohorts, respectively. To recruit students, we first obtained a roster of students who were enrolled in the target grades, which were grades 6 and 7 in 2017 and grades 8 and 9 in 2018. We then drew a random sample stratified by the student's gender, school, and grade using the roster, while restricting each household to have a valid mobile

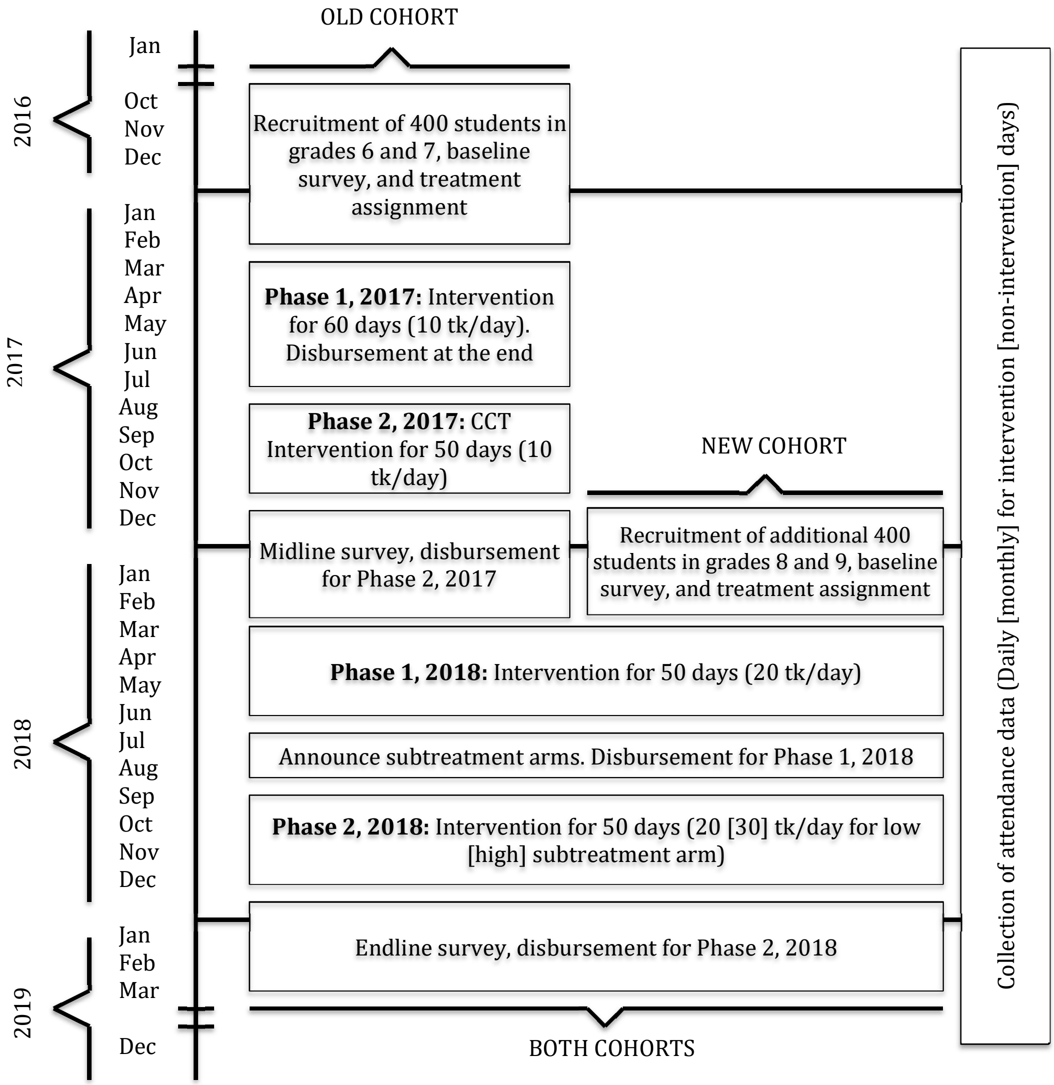
[5]In Bangladesh, secondary education corresponds to grades 6–10.

[6]Unions are the second smallest administrative unit after villages, but before subdistricts, districts, and divisions.

[7]We chose to reduce the number of intervention days after Phase 1, 2017 to be able to cope with the following two unanticipated operational issues: (i) long delay in obtaining the finalized list of enrolled students and (ii) unexpected school closures due to, for example, floods and teacher strikes.

[8]In one of the three study schools, the intervention days for Phase 2 of 2017 had to be cut by five days because of a sustained school closure. For the purpose of cash transfer, these days were treated as attended but they were removed from the analysis.

Figure 1: Timeline of the study



Note: Daily attendance data are collected during the intervention days. Monthly attendance data are collected from the study schools between 2016 and 2019 outside the intervention days, which include (i) pre-intervention days in 2016 for the old cohort (grade 7 in 2017) and in 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 arms.

phone number and no more than one participating student.[9] All but two study participants from the old cohort were promoted to the next grade in 2018. The timeline of our study and the distribution of our final sample by grade, gender, and cohort, are given in Figure 1 and Table A1 of Appendix D respectively.[10]

Treatment Assignment

In each of the two cohorts, half of the students were assigned to a treatment arm to receive cash transfers of T taka per intervention day attended, where T is fixed for a given household in a given phase and year. Among those who received CCTs, half belonged to the gain framing and the other half to the loss framing. The framing was reinforced by weekly SMS, which provides attendance information and the balance, or the amount of cash that is to be transferred to the household at the end of the intervention phase. Since SMS may have an impact on attendance independent of cash transfers, we created an SMS treatment, to which half of those who did not receive CCTs were assigned. Hence, 100 students from each cohort were randomly assigned to one of the following four treatment arms:

Gain: Households in this treatment arm receive conditional cash transfers with gain framing. That is, households receive T taka for each day the student attends school. The balance starts from zero for this treatment group and the parents receive information on attendance and cash balance through SMS on a weekly basis.

Loss: Households in this treatment arm receive conditional cash transfers with loss framing. That is, households lose T taka for each day the student is absent from school. The balance starts from the maximum possible transfer

[9]In the initial roster of all eligible households, 84 percent and 97 percent of the parents in the old and new cohort respectively had a valid phone number.

[10]There were two irregularities in our recruitment process, which happened because of the short timeline (roster only finalized on the first day of school), variation in the spelling of the names, and inaccurate information in the roster, the last of which was corrected subsequently during the baseline survey. First, one student in the old cohort was mistakenly listed in the roster of the new cohort, and was dropped upon realization. Second, there were 10 households with more than one participating child. However, our main findings do not change much if we drop these 10 households.

amount in a given phase. The parents receive information on the child's attendance and cash balance through SMS on a weekly basis.

SMS: Households in this treatment arm receive weekly SMS on the school attendance of their child but no cash transfer.

Control: Households in this treatment arm receive neither cash transfer nor SMS.

The transfer amount T was 10 taka for the two phases in 2017. In Phase 1 of 2018, it was increased to 20 taka. In Phase 2 of 2018, we introduced "High" [H] subtreatment for each of the Gain and Loss treatment arms, which raised the transfer amount to 30 taka per day. In each of Gain and Loss treatment arms, half of the households were randomly allocated to the "High" [H] subtreatment and the remaining half to the "Low" [L] subtreatment, who continued to receive 20 taka per day.

The cash transfers were made at the end of each phase for both the Gain and Loss treatment arms. The timing of disbursement and the amount of transfer are the same for a given attendance record in a given phase. It should be noted that the households in the Loss treatment arm were *not* endowed with cash at the beginning of the phase and that there was a time lag between school attendance and cash transfers. Because transferring cash daily can be administratively too complicated and costly and taking money away from households is difficult, the way we operationalized the loss framing is practical.

The most important difference between the Gain and Loss treatment arms is the way balance changes. That is, the balance starts from zero in the Gain treatment and it goes up as the child attends school. On the other hand, for the Loss treatment arm the balance starts from the maximum amount that can be possibly earned in a given phase, or T taka times the number of intervention days in the phase, and it goes down as the child misses school. To reinforce the effect of framing and make the changes in balance conspicuous to the households, we sent weekly SMS to the Gain and Loss treatment groups. We additionally created an SMS treatment arm to examine the pure effect of providing attendance information as noted earlier. The SMS messages sent to the Gain, Loss, and SMS households are as follows:

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

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

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

In the above messages, T refers to the daily transfer amount in each phase while D_a and D_m refer to the days the child attended and missed school, respectively, over the last week, and B is the balance at the end of the week.

Because disbursement occurs only once at the end of each intervention phase, the gains and losses exist only in the account balance stated in the SMSes sent during the intervention phase. Unfortunately, some issues were discovered in the SMSes sent during the early part of Phase 1, 2018, upon the realization of which we conducted an audit. The inaccuracy of information on attendance, transfer amount, and current balance can undermine the impact of our intervention and would create attenuation bias if the errors are random. The error rates from the audit were found to be very small and not significantly different across the treatment arms. Therefore, our impact estimates are unlikely to be significantly affected by the errors in SMSes and, if anything, they are likely to be slightly attenuated. Further discussion on this issue is provided in Appendix A.

4 Data

Data Sources

Our main outcome of interest is school attendance. Because of this and because the amount of cash transfer depends on daily attendance, its accurate measurement is critical for our intervention. Therefore, we collect attendance data for intervention days from three different data sources. The first data source is the morning attendance record, which is the official attendance record maintained by the class teacher before the class begins each morning. This is our primary source of attendance data and the cash transfer amount is computed based on the morning attendance.

Since the students and teachers tend to live near the school, it is plausible that teachers know at least some students' households personally. Hence, they might mark some absent students as present out of sympathy, if they believe that the cash transfer would benefit the students. Also, there is a potential concern for corruption in which students pay the teacher to buy attendance. Therefore, to check if morning attendance is seriously undermined by these issues, we also use a second and third source of attendance record, which are much less likely to suffer from these problems.

The second source of attendance is afternoon attendance, which is collected independently and daily in the afternoon by the class representative of each section engaged by us.[11] Since the afternoon attendance does not relate to cash transfer, there is a minimum concern that it is affected by the issues described above. Further, even if the morning records are completely accurate, the afternoon record is still useful. It is possible that students show up in school in the morning since the cash transfer amount is tied to their morning attendance. Hence, they may leave soon after their morning attendance is taken, if they come to school just to mark attendance. Another reason for not relying solely on the morning attendance is that many students leave school after the lunch break and hence morning attendance captures partial attendance of the entire school day (Star, 2015; Tuhin, 2018). Clearly, our intervention would be of no practical purpose if any of these occurs. Hence, afternoon attendance allows us to see whether each student continued schooling after tiffin or the mid-day lunch break.

The third source of attendance data comes from unannounced random visits to schools. There were about eight visits a year to cross-validate our morning and afternoon attendance records. Since the visits were made by field officers who have no personal relationship with the students, they are least susceptible to arbitrary manipulation.

[11] A section is essentially a class in which students study together. In each grade in each school, there are typically up to two sections for lower grades.

In addition to the above-mentioned daily attendance data, we also collected official monthly attendance records, which cover years between 2016 and 2019.[12] To understand the pre-intervention attendance trends across different treatment arms, we collected aggregate monthly attendance records from study schools in the pre-intervention year (2016 for the old cohort, and 2017 for the new cohort). Since a large fraction of students from the old cohort were in grade 6 in 2017 and thus were in a primary school in 2016, we do not have their pre-intervention attendance records.

We are also interested in seeing whether the effects of our intervention are fleeting or persistent. Thus, we collect monthly attendance data for the year 2019. However, because students may transfer to a different school or drop out of the school, we do not have information on the attendance records for 210 students in 2019 (92 students from the old cohort, and 118 students from the new cohort). We also collected monthly attendance data for the non-intervention school days in the years 2017 (old cohort only) and 2018 (both cohorts).

In addition to the attendance records, we also gather household-level and individual-level data through surveys. We administered a baseline survey to the households of students participating in the study before the treatment assignment was announced. After the intervention, an endline survey was administered to them except for the 16 households that could not be reached possibly because of migration. For the old cohort, we additionally conducted a midline survey between Phase 2, 2017 and Phase 1, 2018 (see Figure 1). The surveys collected information on a host of variables, such as consumption and assets of the household and age, sex, education, and employment of each household member.

We also carried out disbursement surveys for the Gain and Loss treatment arms at the end of every phase during the household visits for cash disbursement. The disbursement surveys were integrated into midline survey in Phase 2, 2017 and endline survey in Phase 2, 2018. These surveys asked households how they plan to utilize the cash received from the study for different purposes such as education and purchase of luxury goods. Additionally, in the midline and endline surveys, the households were asked to provide retrospective information on how

[12]Monthly data, instead of daily data, were collected because of the limited data availability in 2016 and limited budget for data collection.

the cash was utilized on various household expenditures. Finally, the disbursement survey also contained questions on the understanding of the CCT intervention, the recollection of the amount that they were supposed to receive, and whether they kept a record of the SMS that was sent to them at the end of every week. All questions were asked before the money was actually disbursed.

Summary Statistics and Balance Checks

As mentioned earlier, the cash transfer given to a household at the end of each phase is based on the official morning attendance record. However, due to concerns arising from possible misreporting of morning attendance data or students leaving school after morning attendance has been taken, we additionally have data from afternoon attendance and random visits. Our raw data suggest that these concerns are unlikely to be important and that the morning attendance record is a reliable measure of school attendance. As shown in Table 1, there is a strong positive correlation between morning and afternoon attendance records from over 123 thousand person-day records. For nearly 90 percent of the valid person-day records, morning attendance matches afternoon attendance. The off-diagonal elements in Table 1 indicate that the odds of students leaving school early before afternoon attendance are higher than those of students coming later after morning attendance.

Moreover, the attendance records from unannounced random visits also have a high correlation between morning and afternoon attendance. Based on the 8,876 person-day observations with all the three attendance records, the correlation is the highest at 0.87 between morning and random visit records. This is to be expected because the random visit records are likely to capture the attendance of some early leavers (Table 1). The correlation between afternoon and random visit records is 0.79, which is higher than the correlation of 0.76 between morning and afternoon records. Again, this ranking is to be expected because random visit records are likely to capture attendance of some latecomers.

Besides attendance measurement, the random assignment of students to the four different treatment arms is also critical. Since the random assignment was made by the research team, there was no concern for arbitrary manipulation.

Table 1: Morning and afternoon attendance on valid school days

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

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

Nonetheless, the assignment can be unbalanced by chance. Therefore, we performed a balance check for 16 household characteristics including parental education, household size, possession of assets, children's height and weight, and baseline test scores. This exercise was done separately for the old and new cohorts. No significant difference across the four treatment arms was found in any of the 16 variables for the old cohort by a pairwise t-test of equality of means. For the new cohort, the proportion of households with an agricultural land and that with a television or radio at home for the SMS treatment arm was significantly larger than that for the Gain treatment arm. The joint test of equality of means for the old and new cohort reported in Tables A2 and A3 in Appendix D respectively also show that there is no significant difference in observable characteristics across the four treatment arms, except for the ownership of agricultural land and possession of television/radio for the new cohort.

5 Empirical Specification

Our baseline specification is:

$$\mathrm{Y_{it}} = \alpha + \beta_1 \mathrm{Gain}_i + \beta_2 \mathrm{Loss}_i + \beta_3 \mathrm{SMS}_i + \gamma X_i + \theta Z_i + \phi D_t + \epsilon_{it}, \tag{1}$$

where $\mathrm{Y_{it}}$ is an attendance outcome indicator that takes a value of one if individual i is present in school in day t, and zero otherwise. As attendance outcome indicators, we analyze morning attendance, afternoon attendance, attendance at the time of random visits, and "morning & afternoon" attendance, the last of which requires that the individual is present both in the morning and afternoon. Gain_i, Loss_i, and SMS_i are indicator variables that take a value of one if the individual i belongs to the Gain, Loss, and SMS treatment groups, respectively, and zero otherwise. The main coefficients of interest are β_1, β_2 and β_3.

Since the ownership of agricultural land and possession of television/radio at the baseline were not balanced across treatment arms, we additionally include these variables in X_i in equation (1). We also incorporate the fixed effects terms at the levels of cohort-school-grade combination (Z_i) and calendar date (D_t) to control for any unobserved heterogeneity at these levels. The errors term ϵ_{it} is clustered at the individual level.

Besides the above specification, we also consider a difference-in-differences (DiD) specification with individual-level fixed effects using monthly aggregate data before the start of intervention and during intervention (including monthly attendance for non-intervention days). The individual-level fixed-effects specification takes the following form:

$$\begin{aligned} \mathrm{Y}_{it} \;=\; & \alpha + \beta_1 \mathrm{Gain}_i \times \mathrm{TreatmentYear}_{it} + \beta_2 \mathrm{Loss}_i \times \mathrm{TreatmentYear}_{it} \\ & + \beta_3 \mathrm{SMS}_i \times \mathrm{TreatmentYear}_{it} + \phi_i + \gamma_t + \epsilon_{it} \end{aligned} \tag{2}$$

where the reference category is monthly attendance for the year 2016 [2017] for the old [new] cohort. Note that the old cohort students include only those who were in grade 7 as of 2017 because pre-intervention attendance records are unavailable for the old-cohort students who were in grade 6 as of 2017. The treatment year indicator $\text{TreatmentYear}_{it}$ takes a value of one in both 2017 and 2018 for the old cohort and only in 2018 for the new cohort, and a value of zero otherwise. We denote the individual- and year-month-specific fixed effects by ϕ_i and γ_t, respectively. The error term ϵ_{it} is clustered at the individual level. The specification above has the advantage of being able to control for all time-invariant individual characteristics that affect attendance. Because the proportion of intervention days among all school days varies across different months, we also consider a specification in which the interaction terms in eq. (2) further multiplied by the fraction of intervention days among all school days in the given calendar month are included in the regression.

6 Results

Main Findings

As reported in column (1) of Table 2, the Gain treatment increases morning attendance in school by about 11 percentage points, based on the regression estimates in eq. (1). The impact of the Loss treatment is slightly higher, but the difference between the Gain and Loss treatment is not statistically significant. The SMS treatment arm, on the other hand, increased attendance by 4.7 percentage points. The estimates are similar even when alternative measures of attendance such as afternoon attendance (column (2)), morning & afternoon attendance (column (3)), and attendance upon random visit (column (4)) are used. This is to be expected since the morning attendance records taken by the class teacher closely match with other attendance records.

The similarity of the results across columns is nevertheless reassuring. If students came to school only to mark attendance for the cash transfer and left immediately after the morning attendance was taken, the CCT would have been of no practical value for education. However, Table 2 does not show any evidence with such a possibility since the estimated impacts of the CCT treatment arms (i.e., Gain and Loss treatment arms) on afternoon attendance in such a scenario would have been weaker than those on morning attendance. To further strengthen our case, we also use afternoon attendance as the outcome variable for the sub-sample of person-day records where the child was present in the morning. As shown in Table A4 in Appendix D, the Gain, Loss, and SMS treatments all had positive and significant effects on afternoon attendance conditional on morning attendance. While we included the unbalanced covariates and cohort-school-grade and day fixed effects in Table 2 to remove confounding from these variables and increase the precision of the estimates, we also consider the pure experimental design by removing X_i and the fixed effects from eq. (1) and adjusting for multiple hypothesis testing. Doing this does not change our results much qualitatively or quantitatively except that the statistical significance of SMS treatment is weakened as shown in Table A5 in Appendix D.

Robustness Checks

Our results may be potentially driven by differential attrition rates across different treatment arms. Hence, we re-estimate the impact of treatment on attendance by dropping those discontinued students who left the study at any point during the two year period, which include students who dropped out of school or transferred to a different school.[13] As reported in Tables A6 and A7 in Appendix D, the point estimates become higher for all the treatment arms—Gain, Loss, and SMS, and no significant difference exists in the attrition rates across the four treatment arms respectively. Using an alternative definition of attrition, where an individual is identified as missing if the endline survey was not administered to the individual, does not change the results qualitatively or quantitatively.[14]

The main findings we presented above also remain unchanged when we use various alternative specifications. We check the pre-intervention attendance data to see if our point estimates are driven by differences in attendance rates at the baseline. Table A8 in Appendix D shows that the attendance rates were balanced across all treatment arms in the pre-intervention period. It should be reiterated that the sample used for this analysis comprises of only grade 7 students in 2017

[13] There were 79 such discontinued students—44 and 35 from the old and new cohorts, respectively. Discontinuation is likely to be an inaccurate measure because it is generally difficult to distinguish between long-term absence and dropout. Nevertheless, there is also no significant difference in the attrition in the endline survey across different treatment arms.

[14] Using the alternative definition of attrition based on administration of endline survey, there were 16 missing students—8 from the old cohort and 8 from the new cohort.

Table 2: Treatment effect for all students: baseline specification

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain	0.107***	0.120***	0.123***	0.092***
	(0.025)	(0.026)	(0.025)	(0.026)
Loss	0.112***	0.129***	0.130***	0.128***
	(0.024)	(0.025)	(0.024)	(0.025)
SMS	0.047**	0.055**	0.056**	0.069***
	(0.024)	(0.024)	(0.024)	(0.026)
P(Gain=Loss)	0.859	0.749	0.785	0.159
P(Gain=SMS)	0.027	0.020	0.015	0.380
P(Loss=SMS)	0.014	0.006	0.005	0.020
Observations	123,500	123,500	123,500	8,869
R-squared	0.064	0.078	0.076	0.041
Control mean	0.534	0.481	0.449	0.605

Note: "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Afternoon" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. "Random visit" takes a value of 1 if the child was present in school on the day of the random visit, and 0 otherwise. The Control treatment arm 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 cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors are clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

(from the old cohort), and grade 8 and grade 9 students in 2018 (from the new cohort), since the grade 6 students in 2017 were in a primary school in the preceding year.

Table 3 performs a DiD analysis employing monthly attendance data for old-cohort students who were in grade 7 in 2017 and all new-cohort students. The odd-numbered columns use standard DiD specifications while the even-numbered columns control for the intensity of treatment within a month using the fraction of intervention days in the total number of valid school days. Columns (1) and (2) use data between 2016 and 2018, whereas columns (3) and (4) additionally use data for 2019 and estimate the impact of treatment assignment in the post-intervention period to conduct a test of persistence of treatment. This test is of interest, because our treatments may let the households realize the benefits of attending school and change their behavior even after the intervention period is over. If this is the case, our treatments can generate a long-term impact.

Table 3 shows that the addition of 2019 in the analysis does not change the estimated impacts of our treatments much during the intervention years. Further, as column (3) of Table 3 shows, the impacts of the Loss and SMS treatments appear to be persistent. However, the results are only marginally significant. Further, once we control for the treatment intensity, the treatment effects are no longer significant. Hence, we have limited evidence of persistence in school attendance beyond the intervention period.

Finally, we cluster errors at the grade, school, school-grade, and section level to account for the possibility of spillovers, and our results are robust to these alternative specifications. (Appendix D Table A9).

Heterogeneity Analysis

Restricting all the coefficient estimates to be identical across the entire sample masks various types of impact heterogeneity that may exist. For example, the baseline specification in eq (1) does not capture the variation in treatment effects at the intensive margin. Also, the treatment effects could significantly vary across different months of the year, and they could also differ based on pre-intervention attendance, gender of the child, education level of parents, distance from school,

Table 3: Individual fixed effects specification

	2016–2018		2016–2019	
Dependent variable: Monthly attendance rate	(1)	(2)	(3)	(4)
Gain × TreatmentYear	0.106***		0.110***	
	(0.029)		(0.027)	
Loss × TreatmentYear	0.124***		0.131***	
	(0.029)		(0.028)	
SMS × TreatmentYear	0.027		0.030	
	(0.026)		(0.025)	
Gain × TreatmentYear × TrIntensity		0.128***		0.134***
		(0.023)		(0.023)
Loss × TreatmentYear × TrIntensity		0.134***		0.141***
		(0.024)		(0.023)
SMS × TreatmentYear × TrIntensity		0.005		0.007
		(0.022)		(0.022)
Gain × 2019			0.048	0.026
			(0.031)	(0.025)
Loss × 2019			0.058*	0.024
			(0.034)	(0.026)
SMS × 2019			0.053*	0.035
			(0.031)	(0.025)
Observations	14,178	14,178	20,304	20,304
R-squared	0.490	0.493	0.456	0.458

Note: Columns (1) and (3) are based on standard DiD specifications. Columns (2) and (4) control for the intensity of treatment within a month using the fraction of intervention days. The outcome variable in both the specifications is monthly attendance rate. Monthly attendance rates for each student are calculated by dividing the total number of days present by the valid number of school days in a given month. The Control treatment arm is the reference category in all regressions. The above specifications control for household and year-month fixed effects. TreatmentYear is an indicator function that takes a value of 1 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 clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

and occupation and economic status of the household. The impact estimates could also be different based on how well the households were able to recollect the transfer amount and whether they kept records of weekly SMS. We therefore explore the impact heterogeneity in these dimensions.

We first analyze the effect of treatment assignment at the intensive margin through a non-experimental design by allowing the effect of Loss and Gain treatments to depend on the quantity of transfer. Table 4 shows that the initial 10 taka/day had no significant impact on attendance. However, the additional 10 taka/day transfer had significantly improved attendance. The effects were further magnified when the transfer amount was increased to 30 taka/day, but the incremental gain from 20 taka/day to 30 taka/day in attendance was smaller than the incremental gain from 10 taka/day to 20 taka/day. This suggests that the marginal impact of transfer is diminishing. The same conclusion is reached when we alternatively use a model that is quadratic in the transfer amount (See Appendix C).

We also observe consistent results when we perform heterogeneity analysis by phase. Among all phases, the attendance impact of the CCT treatments is the highest in phase 2 of 2018, when the daily transfer was raised to 30 taka/day for the "High" subtreatment households in each CCT treatment arm (Table A10 in Appendix D). Further, even though the difference is statistically insignificant, the attendance impact of CCT treatments is higher for the households in the "High" [H] subtreatment than that for the "Low" [L] subtreatment in phase 2 of 2018 (Table A11 in Appendix D). Taken together, our results indicate that the intensive margin of cash transfer matters.

Second, the seasonality of the treatment effect is of interest. Figure 2 shows the attendance rates across different treatment arms for the years 2017 and 2018. It suggests the presence of seasonality in attendance and treatment effects. In particular, the CCT treatment arms have a greater impact on attendance during the planting and harvesting periods for two of the major crops, *aus* and *aman* in the region. The planting and harvesting times are, respectively, May–June and July-August for *aus* and August–September and November–December for *aman*.

The seasonality effect is particularly interesting when we perform a heterogeneity analysis by occupation. Figure 3 shows the differential impact between agricultural and non-agricultural households for each treatment arm during the

Table 4: Treatment effect by amount: non-linear specification

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain (10tk)	-0.014	-0.011	-0.007	-0.023
	(0.033)	(0.035)	(0.035)	(0.035)
Gain (20tk)	0.090***	0.092***	0.093***	0.052*
	0.030	0.030	0.030	0.030
Gain (30tk)	0.133***	0.150***	0.151***	0.057
	0.039	0.039	0.039	0.045
Loss (10tk)	-0.009	0.004	0.000	0.012
	(0.031)	(0.033)	(0.033)	(0.033)
Loss (20tk)	0.089***	0.093***	0.096***	0.075**
	0.030	0.030	0.030	0.029
Loss (30tk)	0.154***	0.170***	0.172***	0.142***
	0.038	0.038	0.038	0.042
SMS	0.048**	0.056**	0.056**	0.069***
	0.024	0.024	0.024	0.026
Observations	123,500	123,500	123,500	8,869
R-squared	0.067	0.082	0.080	0.043
Control mean	0.534	0.481	0.449	0.605

Note: The above estimates are from a non-linear specification. "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Afternoon" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. "Random visit" takes a value of 1 if the child was present in school on the day of the random visit, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

intervention months of 2017 and 2018, respectively.[15] The point estimates for 2017 are small in absolute value and noisy because both the transfer amount and sample size are small. However, the corresponding estimates for 2018 exhibit a clearer pattern. The difference in attendance rate between agricultural and non-agricultural households for the Loss treatment arm is positive and increases during the months of August and September, which are also the harvesting time for *aus* and planting time for *aman* respectively. The month of August also marked the beginning of the second phase of 2018 when the "High" [H] and "Low" [L] sub-treatment arms were introduced. The greater impact of the Loss treatment on agricultural households relative to non-agricultural households can be attributed to at least two possible reasons. First, the increased transfer amount for the "High" [H] subtreatment perhaps enabled agricultural households to cover the opportunity cost of sending the child to school during the harvest season. Second, there may be interactions between loss aversion and transfer amount. That is, the difference between Gain and Loss treatment arms would be small when the transfer amount is small. However, when the transfer amount is large, the effect of loss aversion may become more obvious. Since boys are usually more prone to child labor in agriculture, we plot Figure 2 and Figure 3 restricting the sample to male students only (Appendix D Table A3 and Appendix D Table A4 respectively). We observe similar attendance rates.

Besides whether the household is agricultural, our CCT treatments would generally be more impactful for households that are at the margin of sending the child to school or not. In this paper, we examine several dimensions that may be related to whether the household is close to the margin. First, cash transfers might be able to incentivize parents in the Gain and Loss treatment arm, whose pre-intervention attendance rates were low, to send their kids to school. As shown in Appendix D Table A12, for the Gain treatment group, the treatment effects are higher for students whose pre-intervention rate was above median, while for the Loss treatment group, the treatment effects are marginally higher for students whose pre-intervention attendance rate was below median. Though these differences are not statistically significant, loss framing appears to be more effective for

[15] Households where at least one member is self-employed (primary or secondary occupation) in agriculture, forestry or aquaculture are defined as agricultural households.

students whose baseline attendance is low. Second, we study the impact heterogeneity by gender. If the parents in our sample value the education of the girl child less, the impact of our treatment assignment might differ considerably between boys and girls. Though the control mean for female students in our sample is higher than their male counterparts, the point estimates for female students are consistently larger than male students across all treatment arms (Appendix D Table A13). However, none of these differences are statistically significant as shown in the middle panel. Third, education is a human capital that is often intergenerational with less educated parents often having offsprings who are poorly educated, thus giving rise to a vicious cycle of poverty (Oreopoulos et al., 2006; Coneus and Sprietsma, 2009; De Haan, 2011). We look at treatment heterogeneity by education levels of both the parents of the study participants. While there is no significant difference in impact estimates across education levels of parents for each of the treatment arms, the point estimates for the Gain treatment arm are increasing in magnitude as the education level of the father increases. Though there is no clear pattern for the education level of the mother, children in households where the father is more educated (completed secondary or tertiary education) are more likely to attend school. Fourth, we analyze the heterogenous treatment effect by the distance from school. Though the difference in the treatment impacts between households whose distance from school is below and above the median (7.84 kilometers) is statistically insignificant for each treatment arm, all treatments are observed to have a greater impact on attendance for the above-median households (Appendix D Table A15). Fifth, we also study the differential impact of the intervention across different quartiles of predicted consumption per capita, which serves as a proxy for household economic status (See Appendix B for a description of this measure). While the impact of the Loss treatment arm is stable across different quartiles, the point estimates for the Gain treatment are the highest for the richest quartile, even though there is no significant difference in the effect of each treatment across the four quartiles according to an F-test for equality of means (Appendix D Table A16). Thus, while there are some indications of potential presence of impact heterogeneity, our results are not driven by the impact heterogeneity due to pre-intervention attendance rates, gender of the child, education level of household head, distance to school or consumption level.

Finally, the success of such CCT interventions is likely to depend on the understanding of the participants. Before the disbursement of cash at the end of each intervention phase, CCT households were asked in the disbursement survey to state the final balance to be transferred to them and with due permission, their phones were checked to see whether they had retained the weekly SMS. About four-fifth of the respondents claimed that they remember the actual cash balance, and nearly all of them stated the correct balance. Around 90 percent of the respondents said that they had not deleted the SMS on their phones prior to disbursement. We use these pieces of information to see whether remembering the CCT transfer amount and retaining the SMS are correlated with attendance. We find that both of them are positively correlated with morning attendance. In particular, remembering the CCT transfer amount is positively and statistically significantly associated with morning attendance, even after controlling for the household fixed-effects (Appendix D Table A20). While we cannot make a causal inference here, our results indeed suggest that understanding of the intervention is important.

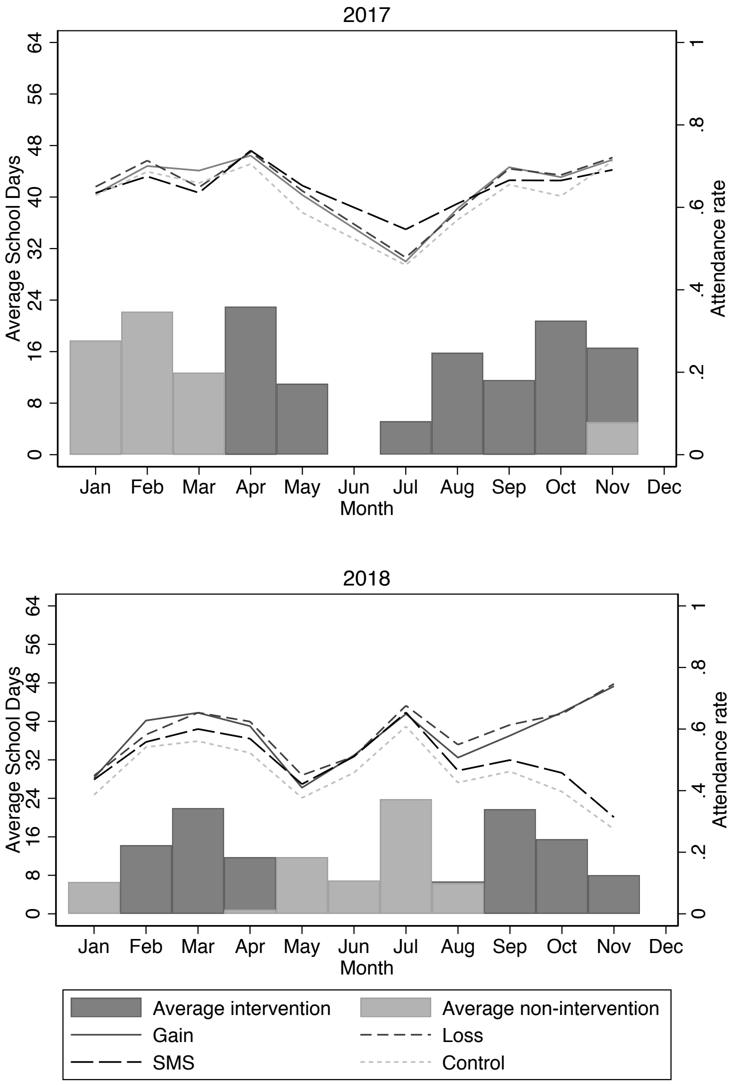
Peer Effect in Attendance

We have not accounted for the possibility of spillover in our analysis so far. However, the anecdotal evidence gathered through informal interactions with some study participants indicates that students tend to make a collective—rather than individual—decision with their friends to attend or skip school. Therefore, it is important to account for potential presence of significant spillover effect arising from peer interactions.

If the peer effect on attendance is positive and unilateral from the treatment groups to the control group, the estimates presented so far would understate the true impact of our interventions. On the other hand, if the peer effect is similar across all treatment arms, our estimates would reflect the true treatment impact net of spillovers. The latter possibility is plausible since treatment assignment is random and thus the impact of our intervention on peers should be similar across all treatment arms.

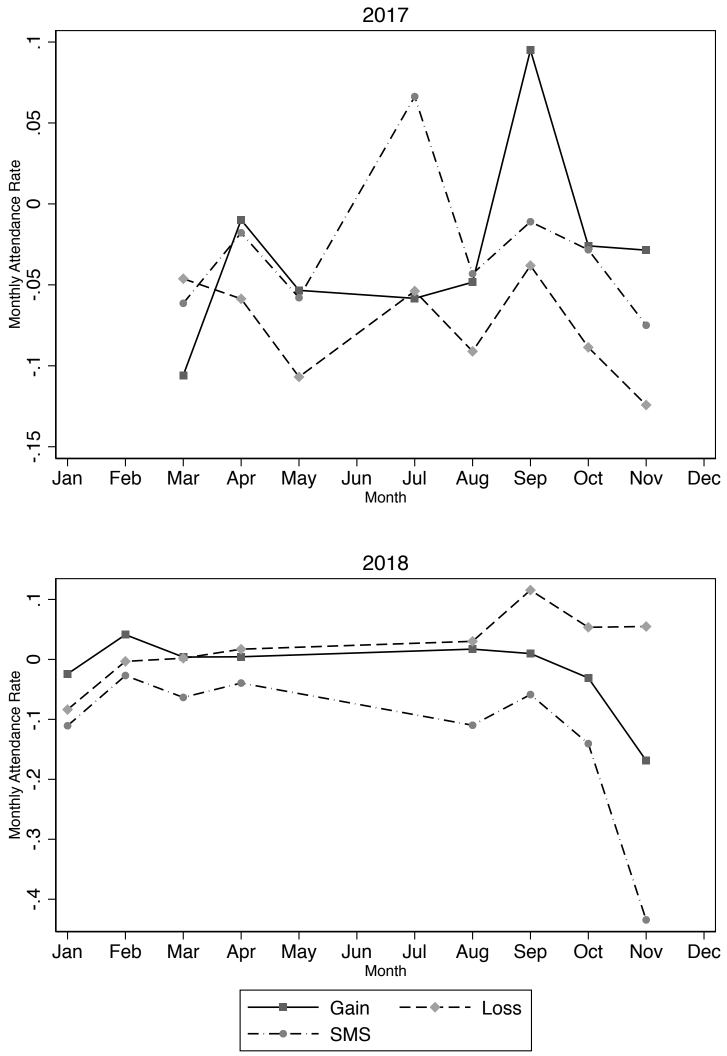
To understand the peer effects, we collected social network data for each study

Figure 2: Monthly attendance rates in 2017 (top) and 2018 (bottom)



Note: In each figure above, the number of intervention days (black) and non-intervention days (grey) averaged over all students are given in the bar chart (left axis) and the average monthly morning attendance rates for the different treatment arms using aggregate monthly data, including both intervention and non-intervention days, are given in line graphs (right axis). The non-intervention months in the year 2017 were January, February, June, and December, and in 2018 they were May, June, July, and December. There was no school day in the month of June in 2017. December is omitted from the graph since students go to school only for the final examination.

Figure 3: Treatment heterogeneity by occupation for 2017 (top) and 2018 (bottom)



Note: In each figure above, the differences in treatment impacts between agricultural and non-agricultural households are given. To this end, monthly attendance rates, which are based on both intervention and non-intervention days, are regressed on the treatment assignment indicators separately for agricultural and non-agricultural households and the differences in point estimates between agricultural and non-agricultural households for each treatment arm (relative to the control arm) are plotted. The non-intervention months in the year 2017 were January, February, June, and December, and in 2018 they were May, June, July, and December. There was no school day in the month of June in 2017. December is omitted from the graph since students go to school only for the final examination.

participant. Specifically, we asked each study participant to report the names of his/her five best friends from the same school and grade in each of the survey rounds. Thus, the social network data would have friends who were in the same school and grade but might not necessarily be part of our intervention. All survey respondents gave exactly five names.[16]

The names of the reported friends were matched to those of the study participants within each school-grade combination by engaging research assistants who are proficient in Bengali. The match was not perfect because of name variations, 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 there in each treatment arm. We denote the proportion of the five friends who are in the Gain treatment arm at the baseline by GainProp and use a similar notation 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 school-grade combination and assume that he 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 = 0.00, respectively. Note that the sum of these proportions does not necessarily add up to unity, because there may be some friends who could not be matched due to name variations or because they were not part of our sample.

Using these data, we test the hypothesis that having a higher proportion of friends in the CCT or SMS treatment arms generates a positive spillover effect on attendance. Specifically, we adopt the following specification using the data for both cohorts.

$$\begin{aligned} \mathrm{Y_{it}} &= \alpha_0 + \alpha_1 \mathrm{Gain}_i + \alpha_2 \mathrm{Loss}_i + \alpha_3 \mathrm{SMS_i} + \alpha_4 \mathrm{GainProp}_i \\ &\quad + \alpha_5 \mathrm{LossProp}_i + \alpha_6 \mathrm{SMSProp}_i + \alpha_7 \mathrm{ControlProp}_i + \epsilon_{it} \end{aligned} \tag{3}$$

As reported in Table 5, we find evidence of significant peer effect. If the proportion of friends in the Gain treatment group goes up by one unit, attendance of the

[16]We chose to collect data in this way instead of attempting to collect complete social network data because our budget was limited and because the peer effect is likely to come primarily from best friends.

individual increases by 11.5 percentage points (column (2) of Table 5). There is a significant peer effect of having friends in the Loss treatment and SMS treatment arm as well, and the estimates are significant at the one percent level. However, the treatment effects are similar to the baseline specification (column (1) of Table 2)

While these point estimates may appear large, the average spillover effects implied by these figures are of plausible magnitude. Using the point estimates in column (2) of Table 2, the spillover effect evaluated at the sample average (i.e., use sample averages of GainProp, LossProp, SMSProp, and ControlProp) is 1.6 percentage points relative to the case when all friends are non-participants. Given that about 53 percent of students are study participants in the participating sections based on our roster, the overall impact on the section-level attendance can be estimated at $5.1(= 1.6 + ((10.7 + 11.2 + 4.7) \times 0.53/4))$ percentage points using the estimates in Column (1) of Table 2. This holds true under the assumption that the spillover effects for non-participating and participating students in the participating sections (i.e., sections with students participating in this study) are the same and that there is no direct program effect for the control and non-participating students.

To verify the validity of this estimate, we compare this figure to the results of a section-level analysis using monthly attendance data from years 2016 to 2018 (Table A22 in Appendix D), which include both participating and non-participating sections. Specifically, we regress the section-level monthly attendance rate on the indicator variable for the intervention months for participating sections weighted by the number of valid school days. The point estimate of the coefficient on the intervention month is positive and statistically significant in the specification without the fixed effects for each of calendar month, calendar year, grade, and school. Further, the point estimate is close to and statistically indistinguishable from 5.1 percentage points regardless of the inclusion of fixed effects. Notice that the point estimates from the section-level data would underestimate the overall impact of our intervention to the extent that there are spillover effects from participating sections to non-participating sections. Subject to this caveat, these crude calculations suggest that our intervention increased the attendance rate at the section level by around 5 percentage points out of which about a third could be attributed to the spillover effects.

Table 5: Impact of social network on attendance

Dependent variable	Morning Attendance		Afternoon Attendance	
	(1)	(2)	(3)	(4)
Gain	0.101***	0.106***	0.116***	0.120***
	(0.004)	(0.004)	(0.004)	(0.004)
Loss	0.105***	0.109***	0.123***	0.126***
	(0.004)	(0.004)	(0.004)	(0.004)
SMS	0.034***	0.044***	0.042***	0.052***
	(0.004)	(0.004)	(0.004)	(0.004)
GainProp	0.119***	0.115***	0.182***	0.150***
	(0.011)	(0.011)	(0.011)	(0.011)
LossProp	0.076***	0.051***	0.061***	0.034***
	(0.011)	(0.011)	(0.011)	(0.011)
SMSProp	0.101***	0.099***	0.145***	0.128***
	(0.012)	(0.012)	(0.012)	(0.012)
ControlProp	-0.042***	-0.084**	-0.063***	-0.104***
	(0.012)	(0.012)	(0.012)	(0.012)
P(Gain = Loss)	0.324	0.397	0.098	0.108
P(Gain = SMS)	0.000	0.000	0.000	0.000
P(Loss = SMS)	0.000	0.000	0.000	0.000
Observations	123,500	123,500	123,500	123,500
R-squared	0.010	0.060	0.015	0.072
Cohort-School-Grade FE	No	Yes	No	Yes
Day FE	No	Yes	No	Yes

Note: “Morning Attendance” takes a value of 1 if the child was present in school according to morning attendance on a given day, and 0 otherwise. “Afternoon Attendance” takes a value of 1 if the child was present in school according to afternoon attendance on a given day, and 0 otherwise. The Control treatment arm is the reference category in all regressions. GainProp denotes proportion of friends in the Gain treatment arm at the baseline. LossProp, SMSProp, and ControlProp are similarly defined for the Loss, SMS, and Control groups. The p-values for the test of equality of means between two different treatment arms are given in the middle panel. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

7 Impact of the Intervention on Other Outcomes

Our rich dataset enables us to study the impact of the intervention on various other outcomes of interest. In this paper, we focus on the following outcomes that may be closely related to attendance: school enrollment, child labor, child marriage, test scores, and spending patterns. Though the impact of our intervention on attendance was short-lived and did not last beyond the intervention period, it can potentially increase the enrollment rates in school. We analyze the enrollment rates in 2019 to understand whether our intervention helped keep students in school for longer. Further, two important reasons why children may drop out of school are child labor and child marriage. These two outcomes may also be affected by our CCT intervention, because CCT can lower the opportunity cost of attending school, particularly for boys belonging to agricultural households (as we had seen in Appendix D Figure A4). Similarly, the incidence of child marriage for girls, particularly those in higher grades, may be affected, because the lower opportunity cost of education would reduce the need for households to marry off girls early (Field and Ambrus, 2008; Amin et al., 2016). For boys, incidence of child marriage is low; no boy was married at the baseline and only one was married at the endline. Hence, we focus on child marriage for girls.

Test score is another important outcome to look at both from policy and research perspectives, because increased school attendance may or may not lead to better learning outcomes. There might be serious supply side constraints such as high student-teacher ratio in schools or lack of requisite home learning aids in the form of private tutoring and sibling or parental support especially for first generation learners, which our study would not have addressed. Finally, it is important to understand how the households utilize the extra cash received for the child's attendance in school. If the households spend the additional money on unnecessary luxury items or sin goods such as alcohol and cigarettes, the intervention will not provide any additional benefits to the child beyond school attendance. Thus, it is important to examine whether the cash provided by the CCT intervention benefited the child.

Enrollment Rate

Dropout rates generally tend to increase as students progress to higher grades. We exploit the monthly attendance data in 2019 to analyze whether our intervention had any positive impact on enrollment rates in the post-intervention period, either because households realized the benefits of attending school or they expected to receive more cash in the future.[17] Using the pure experimental design, we find that the CCT treatment did not have any significant impact on the enrollment rates of the study participants. (Appendix D Table A23). However, conducting a sub-sample analysis across different grades and genders, we find that the SMS treatment arm had a positive impact on the enrollment rates of girls in higher grades, but the results are weakly significant.

Child Labor and Child Marriage

We use the following DiD specification to study the impact of the intervention on incidence of child labor and early marriage:

$$\begin{aligned} \mathrm{Y}_{it} &= \alpha + \gamma_0 \mathrm{Endline}_t + \beta_1(\mathrm{Gain}_i \times \mathrm{Endline}_t) + \beta_2(\mathrm{Loss}_i \times \mathrm{Endline}_t) \\ &\quad + \beta_3(\mathrm{SMS}_i \times \mathrm{Endline}_t) + \theta Z_i + \epsilon_{it}, \qquad (4) \end{aligned}$$

where Y_{it} is the indicator variable for child labor (i.e., whether the child is engaged in a gainful activity[18]) or child marriage (i.e., whether the child is married). That is, Y_{it} takes a value of one if the child is working or married, and zero otherwise. $\mathrm{Endline}_t$ is an indicator variable for the endline survey, and Z_i is the household fixed effects. β_1, β_2 and β_3 are the coefficients of interest.

As reported in columns (1)–(3) of Table 6, we find that the intervention does not have much impact on incidence of child labor. This is true even when we break down the analysis by grades (not reported). However, because the definition of

[17] Students who are not enrolled could either have dropped out of school or taken transfer to a different school.

[18] The student's primary or secondary activity over the past week was employment in agriculture, forestry, aquaculture, employment in a wage/salaried position, other self-employment in production, business and service or performance of domestic duties.

labor is referenced to the week preceding the survey, we are unable to capture the impact of the intervention on seasonal labor during the harvest or planting season. Hence, we cannot exclude the possibility that the seasonality in attendance impact observed in Figure 2 is driven by the reduction in seasonal child labor.

Column (4) of Table 6 provides the results for early marriage for girls. As the column shows, the Loss treatment reduces incidence of early marriage by 10.4 percentage points for girls, which is marginally significant. We further conduct a sub-sample analysis of female early marriage by grades and the impact of treatments are negative and large for grade 9 both in absolute value and relative to lower grades (Table A24 in Appendix D). In particular, the estimated reduction in female early marriage for grade 9 students due to the Loss treatment is 31.1 percentage points, which is significant both statistically and economically.

Test Scores

We also administered a mathematics test for the study participants at the baseline and endline. The questions examined their competencies in basic arithmetic and geometry.[19] Higher attendance rates in school for the Gain and Loss treatment arms might have resulted in better learning outcomes. To test for that possibility, we estimate a value added model that includes the baseline test score, the child's anthropometric measures and several household level controls, and where attendance rate of the individual is instrumented for by treatment assignment. Both baseline and endline test scores are normalized relative to control mean and standard deviation for every cohort-school-grade combination.

$$\text{Score}_{it} = \alpha_0 + \alpha_1 \text{AttendanceRate}_i + \alpha_2 \text{Score}_{i0} + \alpha_3 \text{X}_i + \epsilon_{it}, \tag{5}$$

where AttendanceRate_i is the attendance rate of individual i during our intervention years, Score_{it} is the mathematics test score for individual i at time t, where

[19]Since the endline survey was conducted after our intervention was over and some children were no longer in school, the missing rate for the mathematics test at the endline was as high as 10 percent, but the attrition rates were not significantly different across the four treatment arms. There was no missing data for the mathematics test at the baseline.

Table 6: Impact of intervention on child labor and child marriage

Dependent variable	Child labor			Child Marriage
	(1)	(2)	(3)	(4)
Endline	0.043	-0.011	0.097**	0.134***
	(0.028)	(0.034)	(0.044)	(0.049)
Gain × Endline	-0.010	0.042	-0.063	-0.051
	(0.038)	(0.047)	(0.060)	(0.064)
Loss × Endline	-0.002	0.031	-0.036	-0.104*
	(0.036)	(0.044)	(0.056)	(0.058)
SMS × Endline	-0.0371	-0.000	-0.075	-0.103*
	(0.036)	(0.048)	(0.053)	(0.059)
Observations	1,508	760	748	782
R-squared	0.515	0.491	0.543	0.523
Household FE	Yes	Yes	Yes	Yes
Gender	All	Male	Female	Female

Note: The dependent variable in the first three columns is "Child labor" while in the last column the outcome variable is "Child Marriage". "Child labor" takes a value of 1 if primary or secondary occupation of the child 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. "Child Marriage" takes a value of 1 if the child is married, and 0 otherwise. There was one girl child from the new cohort who was separated at baseline and remained so at endline. We assumed her marriage status as "unmarried". The Control treatment arm is the reference category in all regressions. The above specifications control for household level fixed effects. Standard errors are clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

$t = 0$ corresponds to the baseline. The vector of covariates X_i includes child-level characteristics such as education level of the parents, gender, weight and height of the child, and unbalanced covariates at baseline. Since $\mathrm{AttendanceRate}_i$ is endogenous, we instrument it by an indicator variable for the treatment assignment. Table A25 in Appendix D shows that attendance rate has no statistically discernible impact on test scores. This finding is consistent with existing studies that tend to find that interventions to incentivize school attendance have no or only little impact on learning outcomes (McEwan, 2015; Fiszbein and Schady, 2009).

However, one must exercise caution when interpreting the results. We only examine the test score for mathematics and not other subjects. Further, as recent studies on the long-term impact of CCTs (e.g., Barham et al. (2018), Cahyadi et al. (2018), and Millán et al. (2020)) suggest, exposure to CCTs can positively influence labor market outcomes through higher educational attainment and changes in reproductive health outcomes for girls (see also Millán et al. (2019) for a review). Hence, it may be the case that higher attendance—and delayed marriage for girls—would improve the learning in subjects other than mathematics or open the door to higher educational attainment in the long run, even though the short-run impact on mathematics test scores is negligible.

Spending Patterns

During the baseline survey, households in the Gain and Loss treatment arms were asked questions on how they intend to spend the CCT amount on health, education, luxury, savings, and other expenses. Additionally, during disbursement at the end of each phase, they were asked to provide retrospective information on the actual spending in each of the above categories. While one cannot completely rule out the possibility of social desirability bias, reported spending on education from the endline survey was as high as 90 percent and 92 percent of the total cash transferred to the Gain and Loss treatment arms, respectively. This was considerably higher than the intended percentage quoted in the disbursement survey at the end of Phase 1, 2018, which were 75 percent and 69 percent, respectively. In contrast, the households in the Gain and Loss treatment arms reported that they intend to spend, respectively, 15 percent and 20 percent of the CCT amount on luxury expenses, but their actual spending was only 1 percent. Hence, the extra cash that the household received from the CCT intervention was mostly used to finance the education expenditures of the child.

8 Cost-effectiveness of CCT and SMS Treatments

The estimated impact of the SMS treatment was smaller than that of the Gain and Loss treatments and the difference is statistically significant (Table 2). However, the SMS treatment is much less expensive than CCT treatments from the policy-maker's perspective as the former does not involve cash transfers. Therefore, it is not obvious whether the SMS treatment is more or less cost-effective than the CCT treatments. Furthermore, the impact of the SMS treatment on attendance may potentially persist beyond the intervention period at least as strongly as the CCT treatments, even though this result is inconclusive (Table 3 columns (3) and (4)). Hence, SMS treatment may be a particularly attractive policy option in a country with very tight budget constraint. The SMS treatment may also be more politically palpable than CCT treatments since it merely provides information and the implementation cost is minimal. Hence, we consider the cost-effectiveness of our intervention over the two years.

The average transfer cost per student for the Gain CCT treatment was 1755.00 taka where as the same for the Loss CCT treatment was 1773.70 taka. The average non-transfer cost per student for the CCT treatment, which includes the costs of communication and transportation, was 188.65 taka and the corresponding figure for the SMS treatment was 134.40 taka (only communication costs). Using these figures and the impact estimates in column (1) of Table 2, we can estimate the total program cost per percentage point increase in attendance per student at 181.65 (=(1,755.00+188.65)/10.7), 175.21(=(1,773.70+188.65)/11.2), and 28.60(=134.40/4.7) taka for the Gain, Loss, and SMS treatments, respectively. Alternatively, since cash transfers do not change the total amount of resources in the population of interest, we could also omit the transfer cost in the calculation of the cost-effectiveness measure. In this case, the non-transfer program cost per percentage point increase in attendance per student is 17.63 (=188.65/10.7), 16.84 (=188.65/11.2), and 28.60 (=134.40/4.7) taka for the Gain, Loss, and SMS treatments, respectively. While it is also common to use the latter measure or its reciprocal (e.g., García and Saavedra (2017)) as a cost-effectiveness measure, we argue that the former measure tends to be more relevant for policy-makers who tend to face binding resource constraints. Put differently, for policy-makers who only have a modest amount of resources to increase attendance, SMS treatment can be a good option.

It should be noted that the analysis above took the transfer amount as given. However, because there is a diminishing marginal impact of transfer, it is possible to increase the cost-effectiveness of the CCT interventions by changing the daily transfer amount. As detailed in Appendix C, our estimates from a quadratic model in the transfer amount suggest that the most cost-effective amount of transfer turns out to be roughly around 20 taka per student per intervention day, even though the amount increases with the potential attendance probability in the absence of CCT program. However, note that this quadratic model does not differentiate between Gain and Loss treatment arms and treats them as one CCT treatment arm. This is admissible since the difference between Gain and Loss CCT is not statistically significant, and moreover, we are interested to study the impact of CCT at the intensive margin.

While it is difficult to convincingly determine how large the net benefits of our treatments are, it is still useful to understand the order of magnitude of the long-run benefits of our interventions. The results in Tables 3 and A23 in Appendix D indicate that the impacts on attendance and enrollment persist, even though the estimates are not always significant. To provide a lower bound of the benefits in terms of increase in wages, we take 4.6 percent as a conservative recent estimate of the Mincerean rate of return for secondary education (Rahman et al., 2019) and assume that the increase in the enrollment rate reported in column (1) of Table A23 lasts for a year.[20] Put differently, those who were enrolled in school as a result of the treatment are assumed to study one additional year in comparison to the counterfactual situation without the treatment. Based on these assumptions, both the Gain and Loss treatments should lead to an increase in the logarithmic wage by 0.15 ($= 4.6 \times 0.032$) log points whereas for the SMS treatment, it should lead to an increase by 0.33 ($= 4.6 \times 0.072$) log points.

[20]Ito and Shonchoy (2020) report a higher estimate of 6.6 percent.

9 Conclusion

We have shown that the conventional Gain treatment increases morning attendance for secondary school students in Bangladesh by about 11 percentage points net of peer effects. The impact of the Loss treatment is higher than the conventional Gain treatment, even though the difference is not statistically significant. The SMS treatment also has a positive and significant impact on morning attendance by five percentage points. The results are similar when alternative measures of attendance are used. The estimated impacts of our treatments compare favorably to the mean impact of 5.75 percentage points derived from 22 evaluations of the impact of CCTs on secondary-school attendance reported in García and Saavedra (2017). Even though both the details of the program implementation and the program impacts are highly heterogeneous, our study appears to indicate the presence of 'low hanging fruits' to promote secondary-school attendance in our study area and possibly elsewhere in Bangladesh.

A heterogeneity analysis based on occupation reveals that the loss framing seems to be most effective for agricultural households when the transfer amount is sufficiently large, and is thus able to decrease the opportunity cost of attending school during the harvest season. We also find that the CCT treatment effects are higher for girls and for children in households who live further away from school, thus benefiting individuals at the margin. Analyzing the data on social network indicates significant peer effect in attendance but the effect sizes remain the same once such peer interactions are controlled for. Finally, though our intervention does not have any perceptible impact on learning outcomes, the Loss and SMS treatments delay incidence of marriage for older girls in our sample. This result is further strengthened by the fact that the older girls in these treatment arms are more likely to stay enrolled in school even in the year following the intervention.

The ineffectiveness of the Loss treatment to achieve significant improvement in attendance over and above the conventional Gain treatment is possibly due to delayed rewards for households in the Loss treatment arm. Not giving them cash at the beginning of each phase would have failed to generate the desired endowment effect. Thus, future experimental studies on loss aversion can endow households with cash at the beginning of the intervention to generate an endowment effect.

However, within the current framework, we do not find evidence of any adverse impact of the loss design on households. Also, the SMS treatment is more cost-effective than CCT treatment in terms of overall program costs while if we look at non-transfer program costs, loss framing is marginally more cost-effective than the conventional gain framing. Therefore, both loss framing and SMS nudges can be considered as alternative cost-effective approaches to promote attendance in schools, especially in the developing world where resources for policy interventions are typically limited.

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Appendix

A Documentation of Error Rates in SMS

SMSes were sent to households in the CCT and SMS treatment arms in Bengali both in the form of text messages and voice on a weekly basis, and this process took place in the week following intervention school days and lasted from Monday to Wednesday. This was done manually in 2017. However, there were operational difficulties such as the departure of a key operational personnel and nationwide teacher strikes which led to irregular working hours in schools. This resulted in some lapses in the first few weeks of Phase 1, 2018. The lapses include the replacement of text SMSes with informal phone calls until February and the omission of voice SMS until March. We also discovered that some of the SMSes sent to the study participants contained errors.

Once these issues were discovered, we conducted an audit to assess the prevalence of errors by checking the SMS against the attendance records in one of the subsequent weeks in Phase 1, 2018. Based on this audit exercise, the error rates in weekly attendance information, weekly transfer amount, and current balance were estimated at 3.9 percent, 4.2 percent, and 5.9 percent, respectively. We found no significant difference in the error rates across different treatment arms. This, in turn, suggests that the estimated effects of our treatments, particularly SMS treatment, may have been attenuated because of the errors in the SMS information.

To adequately address the above-mentioned issues, we introduced an automated process of sending SMSes from Phase 2, 2018, which increased the reliability of the information in the SMS. As Table 2 shows, the impact of the SMS treatment in Phase 2, 2018 is highly statistically significant and larger than previous phases. These results are indeed consistent with our conjecture that the improved reliability of the SMS information in Phase 2, 2018 increased the effect of the SMS treatment.

B Measuring Consumption

Instead of using a full consumption module that takes a long time to complete, we chose to collect consumption expenditures for a small number of consumption items that have high predictive power for total consumption because of the limited budget for data collection. To determine the consumption items to collect, we used the consumption data from the skills training program, which was also performed in the Gaibandha district. The following items were found to have a high predictive power of the total consumption in a linear regression ($R^2 = 0.929$): rice, chicken, fish, okra, onion, and cigarettes consumed in the past seven days, and energy, clothes and footwear, soap or washing product, hair cut and other personal services, and cosmetic articles consumed in the past one year. We collected these consumption items in the baseline survey and used the regression coefficients and household size to derive the predicted annual household consumption per capita, which is used as a measure of standards of living.

C Further Discussion on Cost-Effective Analysis

In this section, we consider the choice of cost-effective intensive margin. Because the difference in the impact of Gain and Loss treatments on the attendance is similar, we use the following model that is quadratic in the transfer amount τ_{it} to focus on the intensive margin of the CCT transfer.

$$\mathrm{Y_{it}} = f_0\mathrm{CCT}_i + f_1\tau_{it} + f_2\tau_{it}^2 + \beta\mathrm{SMS}_i + \alpha + \gamma X_i + \theta Z_i + \phi D_t + \epsilon_{it}, \tag{A1}$$

where $\mathrm{CCT}_i = \mathrm{Gain}_i + \mathrm{Loss}_i$ is an indicator that individual i is either in the Gain or Loss treatment. The transfer amount τ_{it} is the transfer individual i receives, which is zero if $\mathrm{CCT}_i = 0$ and 10, 20, or 30 otherwise depending on the phase and subtreatment. We denote the expected attendance in the absence of any intervention for individual i in day t by $A_{it} \equiv \alpha + \gamma X_i + \theta Z_i + \phi D_t$ and interpret $f(\tau) \equiv f_0 + f_1\tau + f_2\tau^2$ as the attendance impact of a CCT intervention with a transfer of τ taka/day. Based on the regression estimates from eq. (A1), we predict $f(\tau)$ (full regression results available upon request). Figure A1 shows the graph of the predicted value of $f(\tau)$ and its 95 percent confidence bounds, which clearly shows the diminishing marginal impact. As the figure indicates, the transfer amount has to be slightly above 10 taka/day to have a significant impact on attendance. The figure also indicates that the marginal effect becomes zero around 36 taka/day. Note, however, that τ takes values between 10 taka/day and 30 taka/day and thus the estimates outside this range may not be very reliable in our data. The regression based on eq. (A1) also allows us to predict A_{it}. Even though it is not bound to be on the unit interval, 99.8 percent of observations are within the unit interval. The mean and median of A_{it} are both around 0.53, which is very close to the control mean reported in column (1) of Table 2. The dotted line in Figure A2 represents the kernel density estimate of A_{it}.

Let us now find the most cost effective amount of transfer τ^* as a function of A. Notice that the non-transfer cost of the program does not vary with τ. Since there are 155(=(60+50)/2+50+50) intervention days across the two cohorts, the non-transfer cost for each student is $C = 188.65/155 = 1.22$ taka/day. Now, notice that the attendance 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 $(A + f(\tau))\tau + C$. The attendance impact per program cost is therefore maximized when the following expression is maximized:

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

Taking the first order condition and rearranging the terms, we see that τ^* is implicitly given by the following expression:

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

The solid line in Figure A2 shows the most cost-effective transfer amount τ^* as a function of A. While the analysis above ignores spillover effects, most of the arguments above will hold so long as the spillover effects are uniform across individuals, which is likely to be the case. However, the estimated value of A may be biased upwards since the students in the regression analysis are all affected by spillover effects.

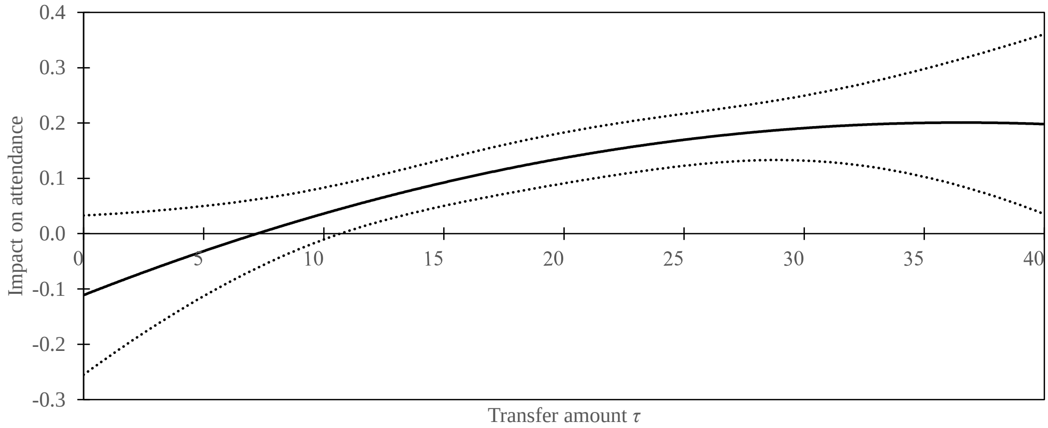


Figure A1: The estimated attendance impact of CCT with daily transfer τ.

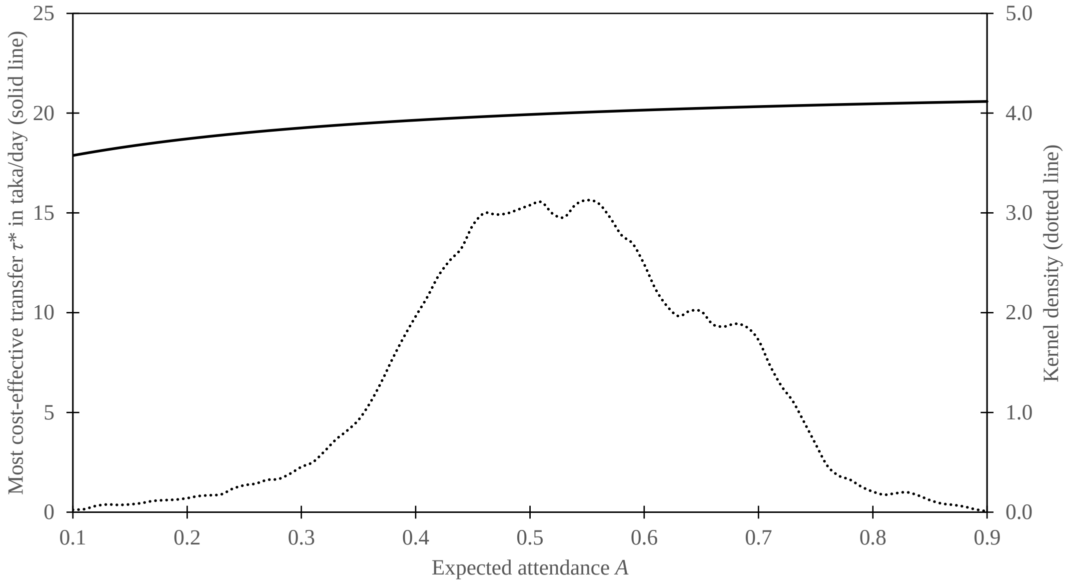


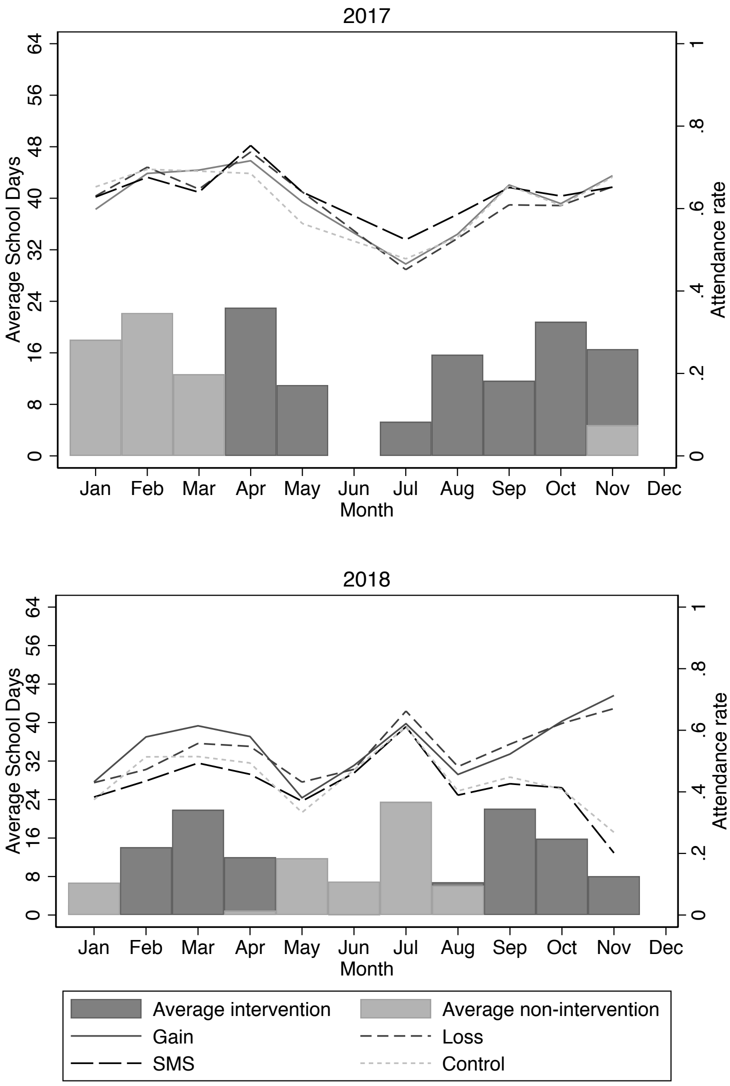
Figure A2: The most cost-effective transfer τ^* (solid line) and the kernel density estimate of the expected attendance A in the absence of intervention (dotted line) as a function of A.

D Supplementary Tables and Figures

Figure A3 shows the attendance rates for boys in our study for both the years 2017 and 2018. Figure A4 shows the treatment heterogeneity for boys in agricultural and non-agricultural households in 2017 and 2018.

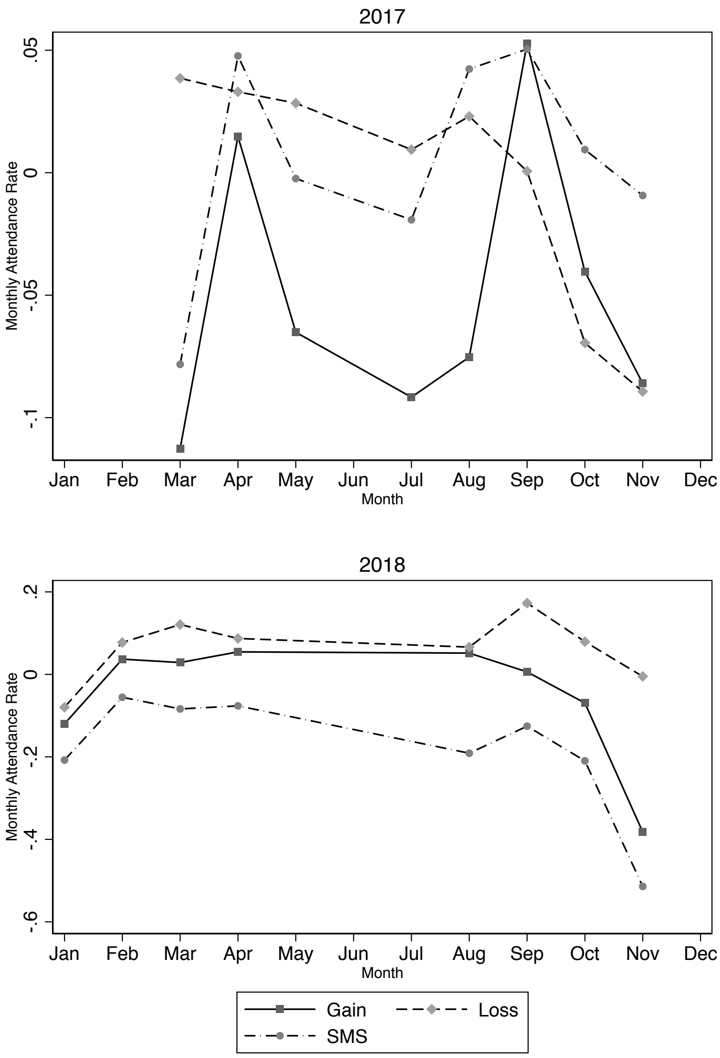
Table A1 shows the sample size by cohort, grade and gender. Tables A2 and A3 perform balance checks for the old and new cohort, respectively. Table A4 checks afternoon attendance for the subsample of person-day records where the child was present in the morning. Table A5 shows the impact estimates from the pure experimental design, without controlling for fixed effects and inclusion of unbalanced covariates at the baseline. It additionally shows the p-values from multiple hypothesis testing using Westfall-Young correction. Table A6 shows the impact estimates for students who were part of the intervention throughout the entire two year duration. Table A7 checks for whether there is differential attrition across the four treatment arms. Table A8 shows the pre-intervention attendance trends for some old-cohort students (grade 6 in 2016) and all new-cohort students. Table A9 performs a robustness check by clustering errors at the individual, school, grade, school-grade, and section levels. Table A10 shows the impacts of the interventions by phase. Table A11 shows the differential impact on “High” [H] and “Low” [L] CCT subtreatments in the second phase of 2018. Table A12 looks at treatment heterogeneity by pre-intervention attendance trends. Table A13 studies the differential impact between boys and girls in the sample. Table A14 looks at impact heterogeneity across education levels of the head of household. Table A15 analyzes the heterogenous impact of treatment based on the distance from home to school. Table A16 looks at whether the impact estimates vary across different levels of household consumption per capita. Table A20 checks for whether remembering CCT and retaining SMS have any positive impact on attendance. Table A22 looks at spillover across sections with participating and non-participating students. Table A23 identifies the impact of our treatment assignment on the post-intervention school enrollment rate in 2019. Table A24 performs a heterogeneity analysis by grade on the incidence of early marriage for females. Finally, Table A25 analyzes the impact of the intervention on the mathematics test scores of students.

Figure A3: Monthly attendance rates for boys in 2017 (top) and 2018 (bottom)



Note: The above graphs look at the attendance rates for only boys in 2017 and 2018. In each figure above, the number of intervention days (black) and non-intervention days (grey) averaged over all students are given in the bar chart (left axis) and the average monthly morning attendance rates for the different treatment arms using aggregate monthly data, including both intervention and non-intervention days, are given in line graphs (right axis). The non-intervention months in the year 2017 were January, February, June, and December, and in 2018 they were May, June, July, and December. There was no school day in the month of June in 2017. December is omitted from the graph since students go to school only for the final examination.

Figure A4: Treatment heterogeneity by occupation for boys in 2017 (top) and 2018 (bottom)



Note: The above graphs look at the impact heterogeneity by occupation for only boys in 2017 and 2018. In each figure above, the difference in treatment impacts between agricultural and non-agricultural households are given. To this end, monthly attendance rates, which are based on both intervention and non-intervention days, are regressed on the treatment assignment indicators separately for agricultural and non-agricultural households and the differences in point estimates between agricultural and non-agricultural households for each treatment arm (relative to the control arm) are plotted. The non-intervention months in the year 2017 were January, February, June, and December, and in 2018 they were May, June, July, and December. There was no school day in the month of June in 2017. December is omitted from the graph since students go to school only for the final examination.

Table A1: Sample size by cohort, grade, gender

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

Note: Two male students from the old cohort repeated grade 6 in 2018.

Table A2: Summary statistics and balance check for old cohort

	Gain (1)	Loss (2)	SMS (3)	Control (4)	Overall (5)	Orthogonality† (6)
Father has at least primary education	0.420	0.450	0.410	0.440	0.430	0.939
Mother has at least primary education	0.420	0.400	0.380	0.420	0.405	0.928
Father has at least secondary education	0.080	0.100	0.080	0.070	0.083	0.900
Mother has at least secondary education	0.030	0.060	0.030	0.080	0.050	0.323
Household size	4.840	4.680	4.860	4.780	4.790	0.740
Male members in household	2.440	2.350	2.470	2.410	2.418	0.862
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	0.289
Owns agricultural land	0.290	0.250	0.340	0.230	0.278	0.331
Has television or radio	0.350	0.410	0.450	0.480	0.423	0.267
Has a bicycle	0.310	0.400	0.340	0.370	0.355	0.580
Has a tube well	0.950	0.940	0.950	0.970	0.952	0.738
Has an electric fan	0.710	0.690	0.730	0.770	0.725	0.613
Weight of the child	35.510	35.830	36.520	36.090	35.987	0.790
Height of the child	55.920	54.870	56.430	56.080	55.825	0.347
Standardized test score	0.000	0.101	0.115	0.000	0.054	0.780
Observations	100	100	100	100	400	0.895

Note: The first four rows are dummy variables that take a value of 1 if the male household head has at least primary and secondary education and the female household head has at least primary and secondary education, and 0 otherwise, respectively. Ownership of assets (agricultural land, radio/television, bicycle, tube well, electric fan) is a binary variable that takes a value of 1 if the household owns the asset, and 0 otherwise. The weight of the child is measured in kilograms, and the height of the child is measured in inches. Test scores are normalized relative to control mean and standard deviation. Column (5) shows the mean value for each variable. Column (6) shows the p-value for joint orthogonality.

Table A3: Summary statistics and balance check for new cohort

	Gain (1)	Loss (2)	SMS (3)	Control (4)	Overall (5)	Orthogonality† (6)
Father has at least primary education	0.430	0.400	0.480	0.424	0.434	0.715
Mother has at least primary education	0.390	0.460	0.480	0.556	0.471	0.132
Father has at least secondary education	0.070	0.100	0.140	0.121	0.108	0.380
Mother has at least secondary education	0.040	0.060	0.080	0.061	0.060	0.684
Household size	4.710	4.520	4.650	4.737	4.654	0.472
Male members in household	2.580	2.320	2.370	2.566	2.459	0.170
Female members in household	2.120	2.200	2.280	2.172	2.193	0.712
Owns residential land	0.990	0.980	0.990	0.980	0.985	0.878
Owns agricultural land	0.220	0.320	0.380	0.253	0.293	0.064
Has television or radio	0.350	0.430	0.500	0.535	0.454	0.040
Has a bicycle	0.550	0.520	0.490	0.596	0.539	0.483
Has a tube well	0.960	0.990	0.970	0.980	0.975	0.501
Has an electric fan	0.810	0.840	0.860	0.879	0.847	0.584
Weight of the child	41.340	40.010	41.630	41.717	41.173	0.249
Height of the child	58.490	58.980	57.540	57.495	58.128	0.198
Standardized test score	-0.192	-0.079	-0.250	0.000	-0.136	0.243
Observations	100	100	100	99	399	0.032

Note: There are 399 students from the new cohort since one student from the old cohort was mistakenly listed in the roster of new cohort, and was dropped upon realization. The first four rows are dummy variables that take a value of 1 if the male household head has at least primary and secondary education and the female household head has at least primary and secondary education, and 0 otherwise, respectively. Ownership of assets (agricultural land, radio/television, bicycle, tube well, electric fan) is a binary variable that takes a value of 1 if the household owns the asset, and 0 otherwise. The weight of the child is measured in kilograms, and the height of the child is measured in inches. Test scores are normalized relative to control mean and standard deviation. Column (5) shows the mean value for each variable. Column (6) shows the p-value for joint orthogonality.

Table A4: Does our intervention induce students to leave school early?

Dependent variable	Afternoon attendance	
	(1)	(2)
Gain	0.053***	0.046***
	(0.012)	(0.011)
Loss	0.056***	0.050***
	(0.012)	(0.010)
SMS	0.025*	0.028**
	(0.013)	(0.011)
P(Gain = Loss)	0.766	0.649
P(Gain = SMS)	0.017	0.090
P(Loss = SMS)	0.008	0.030
Observations	73,423	73,423
R-squared	0.005	0.105
Control Mean	0.841	0.841
Cohort-School-Grade FE	No	Yes
Day FE	No	Yes
Control Variables	No	Yes

Note: The above observations are for valid school days in which the child was present in school according to the morning attendance record. "Afternoon attendance" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. The Control treatment arm 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 control variables are ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A5: Treatment effect for all students: experimental design

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

Note: "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Afternoon" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. "Random visit" takes a value of 1 if the child was present in school on the day of the random visit, and 0 otherwise. The Control treatment arm 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 clustered at the individual level are given in parentheses. The *p*-values for Westfall-Young correction for multiple hypothesis testing are given in square brackets below the standard errors. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A6: Treatment effect for continued students: baseline specification

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain	0.117***	0.134***	0.136***	0.108***
	(0.022)	(0.024)	(0.024)	(0.026)
Loss	0.138***	0.157***	0.159***	0.146***
	(0.021)	(0.023)	(0.023)	(0.025)
SMS	0.060***	0.071***	0.071***	0.085***
	(0.022)	(0.023)	(0.023)	(0.026)
P(Gain = Loss)	0.379	0.342	0.377	0.133
P(Gain = SMS)	0.014	0.012	0.009	0.377
P(Loss = SMS)	0.001	0.000	0.000	0.015
Observations	110,800	110,800	110,800	8,460
R-squared	0.063	0.077	0.077	0.043
Control Mean	0.570	0.513	0.480	0.604

Note: Discontinued students are the ones who left the study at any point during the two year intervention period. There were 79 such students - 44 from the old cohort and 35 from the new cohort. This analysis drops such students. "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Afternoon" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. "Random visit" takes a value of 1 if the child was present in school on the day of the random visit, and 0 otherwise. The Control treatment arm 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 cohort-school-grade and day-fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A7: Does discontinuity matter?

Dependent variable	Discontinued		
	Old Cohort	New Cohort	Both cohorts
	(1)	(2)	(3)
Gain	-0.029	-0.030	-0.028
	(0.039)	(0.038)	(0.027)
Loss	0.005	-0.003	0.002
	(0.043)	(0.041)	(0.030)
SMS	-0.030	0.004	-0.013
	(0.042)	(0.041)	(0.030)
P(Gain = Loss)	0.427	0.479	0.285
P(Gain = SMS)	0.967	0.384	0.585
P(Loss = SMS)	0.428	0.861	0.624
Observations	400	399	799
R-squared	0.167	0.077	0.124
Control Mean	0.11	0.09	0.10

Note: Discontinued students are the ones who left the study at any point during the two year intervention period. There were 79 such students - 44 from the old cohort and 35 from the new cohort. "Discontinued" is a dummy variable if the individual left the study at any point in time, and 0 otherwise. The Control treatment arm 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 cohort-school-grade fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A8: Pre-intervention analysis for new cohort

Dependent variable	Days Attended		Attendance Rate	
	(1)	(2)	(3)	(4)
Gain	-0.365	-0.366	-0.019	-0.019
	(0.467)	(0.467)	(0.025)	(0.025)
Loss	0.285	0.285	0.014	0.014
	(0.485)	(0.486)	(0.026)	(0.026)
SMS	0.168	0.168	0.008	0.008
	(0.471)	(0.471)	(0.025)	(0.025)
P(Gain = Loss)	0.148	0.148	0.158	0.158
P(Gain = SMS)	0.216	0.216	0.243	0.243
P(Loss = SMS)	0.794	0.794	0.784	0.784
Observations	4,676	4,676	4,676	4,676
R-squared	0.041	0.299	0.036	0.112
Control Mean	10.595	10.595	0.556	0.556
Cohort-School-Grade FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes

Note: Daily attendance data is not available for non-intervention days. The outcome variables are derived from monthly attendance data for the year 2016 [2017] for the old [new] cohort. Grade 6 students in 2017 from the old cohort are dropped from the analysis since they went to a different primary institution in the preceding year, and we do not have their attendance data. "Days Attended" is the total number of days attended by a child in a given month. "Attendance Rate" for each student is calculated by dividing total number of days present in a month by total number of valid school days. The Control treatment arm 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. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A9: Treatment effect: clustering errors at different levels

Dependent variable	Morning Attendance				
	(1)	(2)	(3)	(4)	(5)
Gain	0.107***	0.107*	0.107**	0.107*	0.104***
	(0.025)	(0.027)	(0.022)	(0.025)	(0.022)
Loss	0.112***	0.112**	0.112**	0.112***	0.102***
	(0.024)	(0.019)	(0.031)	(0.027)	(0.006)
SMS	0.048**	0.048	0.048**	0.048**	0.050**
	(0.024)	(0.020)	(0.014)	(0.021)	(0.018)
P(Gain = Loss)	0.859	0.648	0.667	0.796	0.937
P(Gain = SMS)	0.027	0.222	0.154	0.074	0.000
P(Loss = SMS)	0.014	0.129	0.213	0.051	0.024
Observations	123,500	123,500	123,500	123,500	107,750
R-squared	0.064	0.064	0.064	0.064	0.055
Control mean	0.534	0.534	0.534	0.534	0.534
Cluster Type	Individual	School	Grade	School-Grade	Section

Note: The number of observations when errors are clustered at the section level differs from other specifications since information on section was missing for 128 students. "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Afternoon" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. "Random visit" takes a value of 1 if the child was present in school on the day of the random visit, and 0 otherwise. The Control treatment arm 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 cohort-school-grade and day-fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Cluster type refers to the level at which standard errors are clustered. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A10: Treatment effect by phase

Dependent variable	Morning				Morning & Afternoon			
	2017-I	2017-II	2018-I	2018-II	2017-I	2017-II	2018-I	2018-II
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Gain	0.029	0.031	0.101***	0.208***	0.041	0.058	0.096***	0.240***
	(0.034)	(0.035)	(0.029)	(0.029)	(0.038)	(0.037)	(0.028)	(0.029)
Loss	0.030	0.039	0.091***	0.226***	0.055	0.057	0.091***	0.259***
	(0.031)	(0.033)	(0.029)	(0.030)	(0.035)	(0.035)	(0.029)	(0.028)
SMS	0.047	0.034	0.046	0.058**	0.066***	0.045	0.046	0.067**
	(0.033)	(0.034)	(0.030)	(0.028)	(0.037)	(0.036)	(0.029)	(0.026)
P(Gain = Loss)	0.969	0.823	0.733	0.556	0.720	0.984	0.881	0.550
P(Gain = SMS)	0.611	0.925	0.073	0.000	0.527	0.739	0.099	0.000
P(Loss = SMS)	0.613	0.897	0.152	0.000	0.760	0.749	0.141	0.000
Observations	24,000	19,600	39,950	39,950	24,000	19,600	39,950	39,950
R-squared	0.066	0.062	0.050	0.077	0.092	0.103	0.048	0.094
Control Mean	0.612	0.661	0.545	0.415	0.476	0.577	0.497	0.323

Note: Each of the columns indicates the point estimates for the different phases. The first four columns are for "Morning" attendance and the last four columns are for "Morning & Afternoon" attendance. "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. The Control treatment arm 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 cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A11: Treatment effect across subtreatment arms

Dependent variable	Morning (1)	Afternoon (2)	Morning & Afternoon (3)	Random Visit (4)
Gain High	0.218***	0.248***	0.249***	0.132***
	(0.037)	(0.037)	(0.037)	(0.047)
Gain Low	0.198***	0.224***	0.231***	0.133***
	(0.037)	(0.038)	(0.037)	(0.044)
Loss High	0.237***	0.266***	0.269***	0.211***
	(0.037)	(0.037)	(0.037)	(0.047)
Loss Low	0.216***	0.247***	0.249***	0.148***
	(0.038)	(0.037)	(0.036)	(0.045)
SMS	0.058**	0.069**	0.067**	0.078**
	(0.028)	(0.027)	(0.026)	(0.038)
P(Gain High = Gain Low)	0.657	0.598	0.698	0.973
P(Loss High = Loss Low)	0.648	0.660	0.643	0.220
P(Gain High = Loss High)	0.665	0.683	0.645	0.129
P(Gain Low = Loss Low)	0.685	0.619	0.697	0.770
Observations	39,950	39,950	39,950	2,463
R-squared	0.077	0.100	0.094	0.041
Control Mean	0.534	0.481	0.449	0.605

Note: The above results hold true only for phase 2 in 2018. "Morning" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. "Afternoon" takes a value of 1 if the child was present in school in the afternoon, and 0 otherwise. "Morning and Afternoon" takes a value of 1 if the child was marked present in both the morning and afternoon attendance record, and 0 otherwise. "Random visit" takes a value of 1 if the child was present in school on the day of the random visit, and 0 otherwise. The Control treatment arm 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 cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A12: Impact heterogeneity by pre-intervention attendance rate

Dependent variable	Morning Attendance	
	Below median	Above median
	(1)	(2)
Gain	0.071**	0.097***
	(0.032)	(0.030)
Loss	0.086***	0.084***
	(0.031)	(0.028)
SMS	-0.008	0.043
	(0.028)	(0.027)
$P(\text{Gain}_1 = \text{Gain}_2) = 0.549$		
$P(\text{Loss}_1 = \text{Loss}_2) = 0.965$		
$P(\text{SMS}_1 = \text{SMS}_2) = 0.178$		
Observations	46,340	46,125
Control Mean	0.534	0.695

Note: Column (1) shows the point estimates for students whose pre-intervention attendance was below median while Column (2) is for students whose pre-intervention attendance was above median. This analysis drops students who were in grade 6 in 2017 since they went to a different primary institution in the preceding year. "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The *p*-values for the joint test of equality of the coefficients for the below-median and above-median students using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade fixed effects only. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A13: Impact heterogeneity by gender

Dependent variable	Morning Attendance	
	Male	Female
	(1)	(2)
Gain	0.082**	0.123***
	(0.035)	(0.035)
Loss	0.081**	0.136***
	(0.038)	(0.030)
SMS	0.003	0.074**
	(0.035)	(0.032)
$P(\text{Gain}_{\text{male}} = \text{Gain}_{\text{female}}) = 0.397$		
$P(\text{Loss}_{\text{male}} = \text{Loss}_{\text{female}}) = 0.247$		
$P(\text{SMS}_{\text{male}} = \text{SMS}_{\text{female}}) = 0.133$		
Observations	61,680	61,820
Control Mean	0.514	0.555

Note: Column (1) shows the point estimates for male students while Column (2) does the same for female students. "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The p-values for the test of equality of the coefficients for male and female students using seemingly unrelated regressions are given in the middle panel. The above specifications control for the cohort-school-grade and day fixed effects. They also control for the unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A14: Impact heterogeneity by education level of parents

Dependent variable	Morning Attendance							
	Father has completed				Mother has completed			
	No education (1)	Primary education (2)	Secondary education (3)	Tertiary education (4)	No education (5)	Primary education (6)	Secondary education (7)	Tertiary education (8)
Gain	0.054	0.125**	0.143**	0.214***	0.049	0.144***	0.157***	0.148**
	(0.040)	(0.041)	(0.057)	(0.059)	(0.041)	(0.039)	(0.049)	(0.069)
Loss	0.084**	0.116**	0.179***	0.136**	0.088	0.161***	0.079	0.079
	(0.041)	(0.050)	(0.055)	(0.060)	(0.038)	(0.045)	(0.052)	(0.055)
SMS	0.071*	0.035	0.048	0.071	0.041	0.092**	0.039	0.010
	(0.040)	(0.053)	(0.056)	(0.066)	(0.039)	(0.043)	(0.051)	(0.069)
$P(\text{Gain}_{\text{father,no}} = \text{Gain}_{\text{father,prim}} = \text{Gain}_{\text{father,sec}} = \text{Gain}_{\text{father,ter}}) = 0.142$					$P(\text{Gain}_{\text{mother,no}} = \text{Gain}_{\text{mother,prim}} = \text{Gain}_{\text{mother,sec}} = \text{Gain}_{\text{mother,ter}}) = 0.248$			
$P(\text{Loss}_{\text{father,no}} = \text{Loss}_{\text{father,prim}} = \text{Loss}_{\text{father,sec}} = \text{Loss}_{\text{father,ter}}) = 0.567$					$P(\text{Loss}_{\text{mother,no}} = \text{Loss}_{\text{mother,prim}} = \text{Loss}_{\text{mother,sec}} = \text{Loss}_{\text{mother,ter}}) = 0.521$			
$P(\text{SMS}_{\text{father,no}} = \text{SMS}_{\text{father,prim}} = \text{SMS}_{\text{father,sec}} = \text{SMS}_{\text{father,ter}}) = 0.946$					$P(\text{SMS}_{\text{mother,no}} = \text{SMS}_{\text{mother,prim}} = \text{SMS}_{\text{mother,sec}} = \text{SMS}_{\text{mother,ter}}) = 0.710$			
Observations	46,365	33,155	17,940	10,585	49,500	34,310	24,465	6,275
Control Mean	0.517	0.547	0.566	0.565	0.532	0.516	0.576	0.615

Note: "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The p-values for the joint test of equality of the coefficients for using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A15: Impact heterogeneity by distance from school

Dependent variable	Morning Attendance	
	Below median	Above median
	(1)	(2)
Gain	0.082**	0.127***
	(0.033)	(0.037)
Loss	0.088***	0.137***
	(0.032)	(0.035)
SMS	0.046	0.059*
	(0.036)	(0.033)
$P(\text{Gain}_1 = \text{Gain}_2) = 0.358$		
$P(\text{Loss}_1 = \text{Loss}_2) = 0.297$		
$P(\text{SMS}_1 = \text{SMS}_2) = 0.786$		
Observations	61,400	61,085
Control Mean	0.540	0.529

Note: Column (1) shows the point estimates for households whose distance to school is below the median distance while Column (2) is for households whose distance to school is above the median distance. "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The *p*-values for the test of equality of the coefficients for above-median and below-median distance households using seemingly unrelated regressions are given in the middle panel. The above specifications control for the cohort-school-grade and day fixed effects. They also control for the unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A16: Impact heterogeneity by consumption level

Dependent variable	Morning Attendance			
	1st Quartile (1)	2nd Quartile (2)	3rd Quartile (3)	4th Quartile (4)
Gain	0.069	0.120**	0.088*	0.150***
	(0.049)	(0.048)	(0.046)	(0.047)
Loss	0.087**	0.127***	0.130***	0.091**
	(0.047)	(0.047)	(0.045)	(0.045)
SMS	0.051	0.035	0.074	0.023
	(0.053)	(0.050)	(0.046)	(0.046)
$\mathrm{P}(\mathrm{Gain}_1 = \mathrm{Gain}_2 = \mathrm{Gain}_3 = \mathrm{Gain}_4) = 0.647$				
$\mathrm{P}(\mathrm{Loss}_1 = \mathrm{Loss}_2 = \mathrm{Loss}_3 = \mathrm{Loss}_4) = 0.868$				
$\mathrm{P}(\mathrm{SMS}_1 = \mathrm{SMS}_2 = \mathrm{SMS}_3 = \mathrm{SMS}_4) = 0.879$				
Observations	24,980	26,270	27,495	29,005
Control Mean	0.491	0.517	0.590	0.533

Note: Each of the columns represents different consumption quartiles with quartile 1 comprising of households with the least consumption per capita and quartile 4 comprising of households with the highest consumption per capita. “Morning Attendance” takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The *p*-values for the joint test of equality of the coefficients for four consumption quartiles using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A17: Impact heterogeneity by risk and loss preferences

Dependent variable	Morning Attendance			
	Loss Averse Risk Averse (1)	Loss Averse Risk Neutral (2)	Loss Neutral Risk Averse (3)	Loss Neutral Risk Neutral (4)
Gain	0.117**	0.058	0.127***	0.097*
	(0.061)	(0.076)	(0.043)	(0.050)
Loss	0.070	-0.082	0.160***	0.135***
	(0.060)	(0.076)	(0.038)	(0.050)
SMS	0.106**	-0.036	0.059	-0.008
	(0.052)	(0.076)	(0.044)	(0.046)
	$P(Gain_1 = Gain_2 = Gain_3 = Gain_4) = 0.874$			
	$P(Loss_1 = Loss_2 = Loss_3 = Loss_4) = 0.034$			
	$P(SMS_1 = SMS_2 = SMS_3 = SMS_4) = 0.274$			
Observations	19,485	9,705	37,090	29,680
Control Mean	0.539	0.551	0.572	0.531

Note: Each of the columns represents different combinations of loss and risk preferences. "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The p-values for the joint test of equality of the coefficients for four categories using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A18: Impact heterogeneity by risk preferences

Dependent variable	Morning Attendance	
	Risk Averse	Risk Neutral
	(1)	(2)
Gain	0.111***	0.110***
	(0.036)	(0.041)
Loss	0.118***	0.109**
	(0.034)	(0.042)
SMS	0.076**	0.008
	(0.035)	(0.039)
$P(\text{Gain}_1 = \text{Gain}_2) = 0.991$		
$P(\text{Loss}_1 = \text{Loss}_2) = 0.865$		
$P(\text{SMS}_1 = \text{SMS}_2) = 0.202$		
Observations	57,205	40,200
Control Mean	0.561	0.533

Note: Each of the columns represents different risk preferences. "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The *p*-values for the joint test of equality of the coefficients for four categories using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A19: Impact heterogeneity by loss preferences

Dependent variable	Morning Attendance	
	Loss Averse	Loss Neutral
	(1)	(2)
Gain	0.071	0.119***
	(0.044)	(0.030)
Loss	0.025	0.152***
	(0.043)	(0.028)
SMS	0.034	0.045
	(0.040)	(0.030)
$P(\text{Gain}_1 = \text{Gain}_2) = 0.365$		
$P(\text{Loss}_1 = \text{Loss}_2) = 0.013$		
$P(\text{SMS}_1 = \text{SMS}_2) = 0.814$		
Observations	37,690	83,350
Control Mean	0.535	0.536

Note: Each of the columns represents different loss preferences. "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The *p*-values for the joint test of equality of the coefficients for four categories using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A20: Regression of CCT recollection and keeping SMS on morning attendance using disbursement surveys

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

Note: The sample used in the above regressions is the set of households that belong to the gain and loss treatment groups. "Morning Attendance Rate" is the ratio of total number of days present in the morning over total valid number of school days in a given phase. "Remember CCT" takes a value of 1 if the interviewee (typically the head of the household) remembers the amount due, and 0 otherwise. "Kept SMS" takes a value of 1 if the records of the weekly SMS were found in the phone, and 0 otherwise. "Loss" is a dummy variable that takes a value of 1 if the child belongs to the Loss treatment group, and 0 otherwise. Households belonging to the Gain treatment arm form the reference category. All the specifications control for phase fixed effects. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A21: Does the timing of payment matter by loss aversion?

Dependent variable	Morning Attendance							
	Loss Averse		Loss Neutral		Loss Averse		Loss Neutral	
	Phase 1 Part 1 (1)	Phase 1 Part 2 (2)	Phase 1 Part 1 (3)	Phase 1 Part 2 (4)	Phase 2 Part 1 (5)	Phase 2 Part 2 (6)	Phase 2 Part 1 (7)	Phase 2 Part 2 (8)
Gain	0.133**	0.110*	0.084**	0.094**	0.129*	0.277***	0.118***	0.297***
	(0.065)	(0.059)	(0.041)	(0.037)	(0.068)	(0.062)	(0.041)	(0.040)
Loss	0.053	0.061	0.046	0.109***	0.113*	0.241***	0.160***	0.285***
	(0.065)	(0.066)	(0.044)	(0.039)	(0.068)	(0.064)	(0.043)	(0.042)
SMS	0.083	0.092	-0.003	0.018	0.074	0.130**	0.041	0.032
	(0.061)	0.056)	(0.042)	(0.039)	(0.062)	(0.055)	(0.041)	(0.038)
	P($Gain_1$ = $Gain_2$) = 0.613		P($Gain_1$ = $Gain_2$) = 0.722		P($Gain_1$ = $Gain_2$) = 0.012		P($Gain_1$ = $Gain_2$) = 0.000	
	P($Loss_1$ = $Loss_2$) = 0.873		P($Loss_1$ = $Loss_2$) = 0.036		P($Loss_1$ = $Loss_2$) = 0.018		P($Loss_1$ = $Loss_2$) = 0.000	
	P(SMS_1 = SMS_2) = 0.827		P(SMS_1 = SMS_2) = 0.493		P(SMS_1 = SMS_2) = 0.311		P(SMS_1 = SMS_2) = 0.776	
Observations	3,016	6,234	7,203	14,947	5,257	3,993	12,605	9,545
Control Mean	0.514	0.540	0.564	0.586	0.438	0.387	0.478	0.412

Note: "Morning Attendance" takes a value of 1 if the child was present in school in the morning, and 0 otherwise. The Control treatment arm is the reference category in all regressions. The *p*-values for the joint test of equality of the coefficients using seemingly unrelated regressions are given in the middle panel. The above specifications control for cohort-school-grade and day fixed effects. They also control for unbalanced covariates at baseline - ownership of agricultural land and radio/television. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A22: Spillover across sections

Dependent variable	Present Rate	
	(1)	(2)
Intervention Section X Month	0.033*	0.030
	(0.019)	(0.019)
Observations	13,234	13,234
R-squared	0.008	0.380
Grade FE	No	Yes
School FE	No	Yes
Month FE	No	Yes
Year FE	No	Yes

Note: The above table looks at section level aggregate attendance data for each year month combination. The dependent variable is "Present Rate" which is derived as the ratio of total number of days present to total valid school days in a given year and month. "Intervention Section X Month" is an interaction term that takes a value of 1 if there was at least one study participant in a given section and there were intervention days in that particular month, and 0 otherwise. Column (2) controls for grade, school, month, and year fixed effects with 2016 being the reference year for year fixed effects. Standard errors clustered at the section-year-month level are given in parentheses. Both the columns use total school days in a given month as frequency weights. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A23: Impact of intervention on post-intervention enrollment rate

Dependent variable	Full Sample	Male Enrollment Rate			Female Enrollment Rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gain	0.032	0.059	-0.050	0.042	0.063	-0.048	0.157
	(0.045)	(0.099)	(0.104)	(0.115)	(0.094)	(0.121)	(0.134)
Loss	0.032	0.104	-0.095	-0.077	0.007	0.088	0.198
	(0.045)	(0.093)	(0.116)	(0.121)	(0.101)	(0.110)	(0.130)
SMS	0.072	0.140	-0.171	-0.061	0.092	0.214*	0.210*
	(0.044)	(0.091)	(0.109)	(0.123)	(0.094)	(0.111)	(0.121)
P(Gain = Loss)	1.000	0.617	0.699	0.310	0.545	0.245	0.736
P(Gain = SMS)	0.353	0.364	0.271	0.386	0.730	0.028	0.632
P(Loss = SMS)	0.353	0.666	0.529	0.895	0.356	0.236	0.908
Observations	799	155	143	102	163	141	93
Control Mean	0.704	0.725	0.750	0.792	0.737	0.579	0.652
R-squared	0.003	0.017	0.019	0.013	0.009	0.038	0.044
Grade in 2018	All	7	8	9	7	8	9

Note: The dependent variable is "enrolled in 2019". The first column looks at the impact of the intervention on the full sample in 2019. Columns (2)-(4) look at the impact on boys in 2019 and columns (5)-(7) do the same for girls. We use the grades of students in 2018 for the sub-sample analysis since information on grade is missing in 2019 for the non-enrolled students. The Control treatment arm is the reference category in all regressions. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A24: Incidence of child marriage for girls: heterogeneity analysis by grade

Dependent variable	Female Child Marriage		
	(1)	(2)	(3)
Endline	0.083	0.105	0.261*
	(0.066)	(0.072)	(0.133)
Gain × Endline	-0.037	0.020	-0.166
	(0.081)	(0.110)	(0.162)
Loss × Endline	-0.083	-0.010	-0.311**
	(0.066)	(0.097)	(0.150)
SMS × Endline	-0.033	-0.105	-0.225
	(0.083)	(0.100)	(0.142)
Observations	316	282	184
R-squared	0.521	0.523	0.569
Grade in 2018	7	8	9

Note: The dependent variable is "Female Child Marriage"."Female Child Marriage" takes a value of 1 if the child is married, and 0 otherwise. There was one girl child from the new cohort who was separated at baseline and remained so at endline. We assumed her marriage status as "unmarried". The Control treatment arm is the reference category in all regressions. The above specifications control for household level fixed effects. Standard errors clustered at the individual level are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.

Table A25: Impact of intervention on mathematics test score

Dependent variable	Endline score	
	(1)	(2)
Attendance Rate	-0.294	-0.484
	(0.814)	(0.782)
Baseline score	0.082**	0.079**
	(0.037)	(0.038)
Child is male		0.125
		(0.098)
Father has primary education		0.061
		(0.087)
Father has secondary education		0.102
		(0.159)
Mother has primary education		0.055
		(0.087)
Mother has secondary education		0.248
		(0.173)
Owns agricultural land		-0.090
		(0.083)
Owns radio or television		-0.159**
		(0.077)
Weight of child		0.001
		(0.006)
Height of child		-0.001
		(0.009)
Observations	718	718
Control Variables	No	Yes

Note: The mathematics test could be administered for 718 students at endline since the endline survey was conducted when schools were closed and the remaining 81 students 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 cohort-school-grade combination. The above estimates are obtained from instrumental variable regression where attendance rate is instrumented for by treatment assignment. The Control treatment arm is the reference category in all regressions. Column (1) regresses endline test score on baseline test score and treatment assignments. Column (2) adds household characteristics as control variables. Robust standard errors are given in parentheses. ***, **, and * denote statistical significance at 1, 5, and 10 percent levels, respectively.