Saving for a lean season: Evidence from a randomized experiment and high-frequency data *

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Abstract

Can commitment-saving (CS) ahead of a lean season alter consumption downfalls among the ultra-poor? We collected 36 rounds of bi-weekly household panel data over two-years in Bangladesh and conducted a savings experiment in the second year by randomly allocating commitment-saving (CS) products with either temporary savings subsidy with "premium", or prevailing "market" interest rate. Premium group doubles the formal savings, resulted in increased food and non-food expenditure by 8.6-12.6% during the lean season, with no lasting post-lean season impact. Market group shows no discernable impacts. Our results suggest that, while imperfect, a better-designed savings product could potentially mitigate seasonal deprivation.

Keywords: consumption seasonality, micro-savings, poverty traps, randomized experiment, Bangladesh

JEL Classification: G21, I32, O16, P46

1 Introduction

A large part of the global poverty is intensely concentrated in the rural areas – as two-thirds of the world's extreme poor reside in rural settings Kharas et al. (2020). Being predominantly agriculture-dependent, the rural economy suffers from seasonal income variations with disturbing consistency, known as the lean or "hungry" season in the literature (Fink et al., 2020). Lean seasons are low activity periods in crop production coupled with negligible economic diversification in rural areas — limiting income and earning possibilities — triggering a seasonal famine with severe food insecurity. Routinely enduring such conditions can have devastating consequences on economically marginalized families, especially adolescents and young children, adversely hurting their brain development, cognitive capacity, physical growth, education, and future income possibilities — pushing them further into poverty trap.

Poor typically use various *ex-post* survival strategies such as the advance sale of labor (Berg and Emran, 2020), borrowing from the informal loan providers with exorbitant interest rate (Khandker et al., 2012), and seasonally migrating to nearby urban centers (Bryan et al., 2014). Surprisingly, there exists little or no *ex-ante* measure to tackle seasonal consumption downfalls. Why do rural poor fail to smooth consumption against a recurrent and seemingly anticipated income seasonality? The lack of a precautionary self-coping mechanism is puzzling, given the rural poor can often accumulate extra savings in the form of stored grain and livestock for unanticipated adverse shocks, despite limited labor, credit, and insurance markets (Rosenzweig and Wolpin, 1993; Fafchamps et al., 1998; Park, 2006; Lee and Sawada, 2010). In this study, we focus on secured formal savings, as *ex-ante* measures, in tackling seasonal consumption downfall and explore whether commitment saving (CS) products can enable consumption smoothing for the extreme poor.

We set up this study in northern Bangladesh, an area that regularly experiences a well-documented seasonal famine before the *Aman* paddy pre-harvesting period, locally termed as *Monga* (Pitt and Khandker, 2002; Khandker, 2012; Khandker and Mahmud, 2012; Bryan et al., 2014; Shonchoy and Kurosaki, 2014; Berg and Emran, 2020). The *Monga* driven food insecurity is particularly acute between mid-September to the end of October (Mobarak and Reimão, 2020), suffered mainly by the landless rural households (Paxson, 1992, 1993; Jacoby and Skoufias, 1998; Dercon and Krishnan, 2000; Fink et al., 2020). We sampled 180 un-banked ultra-poor (and near ultra-poor) households from flood-prone river basin areas with no or limited access to agricultural land. These households depend on agricultural and non-agricultural wage employment for living and regularly face seasonal variations in income and consumption.

We address our research question using a unique data-collection exercise along with a household-level randomized controlled trial (RCT). Our study has three key features, which are: (1) high-frequency (twice a month), multiple-round panel surveys on the sample households; (2) tracking the households 18 times a year (from April to January) for two years: 2018/2019 and 2019/2020 (a total of 36 rounds); and (3) implementing an

RCT with one control and two treatment arms, where treated households are offered CS products with either temporary savings subsidy with "premium" (50%),[1] or prevailing "market" interest rates (approximately 8% per year), through a local Micro-Finance Institute (MFI). These savings products were introduced in June 2019, about four months before the target acute *Monga* period from mid-September, with an upper saving deposit limit of 4,000 BDT (approximately 48 USD).[2]

Our MFI partner executed these zero-cost saving products using door-to-door deposit collectors who visited households twice a month. These visits work as savings reminders while facilitating deposit collections. One of our interventions, the market interest-based CS product mimics formal and secured "labeled" time deposit accounts with a conventional interest rate. It allows households to save for a future event while overcoming the social pressure of sharing savings among family members and social networks. Also, it helps to deal with behavioral constraints, such as self-control, present-bias, and inattention to saving. The rationale for the premium account is to provide the ultra-poor with a strong incentive for targeted savings ahead of *Monga* with a subsidy offer. Hence the premium account product can be considered as a conditional cash transfer (CCT) design where the "conditionality" is the precautionary savings, and the "cash-transfer" part is the promised 50% return as a subsidy. Our CS products allowed households to withdraw savings at any time. But households are eligible to receive the promised return only if they retained the accumulated deposit amount in the account until the onset of *Monga* in mid-September.

Our first year's (pre-intervention period of 2018) high-frequency data show that 73% of the sample households were extremely poor, living on less than the 1.90 USD poverty threshold.[3]. The poverty headcount ratio substantially varies seasonally from 30% in the post-harvesting period of *Boro* paddy to 84% during the *Monga*, thereby demonstrating a robust seasonal dimension of poverty. Administrative data from our MFI partner show that the take-up rates of the savings accounts were quite high, 86% and 78% for the premium and market interest rate products, respectively.[4] Almost no household voluntarily withdrew their savings before the target date of mid-September of 2019, and thus, the majority of the households (about 98.5%) received the pre-specified interest return. We documented that, on average, premium account holders made almost double more deposit amount compared to the market interest group. However, none of the groups fully maximized the savings return offers. The average savings amount, conditional on take-up, was 677 BDT (8.15 USD) and 385 BDT (4.63 USD) for premium- and market-interest products, respectively, much below the ceiling deposit amount offered in the RCT,

[1]Not annually

[2]The regional poverty line in Bangladesh is 70.50 BDT per-day-per-person and consumption gap in the *Monga* period is estimated to be 20BDT in the pre-intervention year of 2018. Together, it requires about 3600 BDT for a family of 4 in the acute *Monga* period of 1.5 months. We designed a 4000 BDT upper savings cap based on these statistics in our RCT design. The conversion rate used in this paper is 1 USD $\approx$ 83 BDT, as of June 2019.

[3]2011 Purchasing Power Parity [PPP] equivalent

[4]Based on any positive amount deposited in the offered account.

demonstrating the limited savings capacity of these ultra-poor households. [5]

Our estimations show that the CS product with premium interest rate contributes in preventing consumption downfall during the acute *Monga* period. Compared to the control group with no intervention, premium account holders increased their consumption of food and non-food items, respectively, about 9-13% against the drop of 10-23% during *Monga*. We also observe significant improvements in calorie intake, especially through increased protein expenditures, among premium interest households in the Monga period. However, we do not see any differential impact of the CS product among the households with typical market interest-bearing accounts compared to control. This demonstrates that the prevailing market-return-based product is not particularly useful for the ultra-poor to encourage savings for a lean season, even when the behavioral constraints are addressed. Participants in this group made less formal savings, and the resulting deposit returns did not help with consumption recovery compared to the control group. These estimates are robust to alternative specifications.

We then examine the factors behind the observed changes in consumption dynamics among premium account holders. We focus on the following possible behavioral changes in the second year which may induce increased consumption during *Monga*: (1) intertemporal reallocation of consumption between *Monga*, and post-*Monga* periods, (2) higher income and remittance flows, (3) greater inter-household informal transfers, and (4) relocation of savings places.

First, while a reduction in consumption is expected, we do not observe any significant change in consumption pattern among premium account holders relative to control households in the pre-*Monga* period. This indicates that the source of savings is not the intertemporal reallocation of consumption. Second, there exist no significant changes in income or remittance throughout the period. This is in contrast to the theory and recent evidence that the higher interest rate increases the incentive to earn today to enjoy interest-driven consumption in the future (Callen et al., 2019). Third, we find no evidence of changes in inter-household informal transactions throughout the period. This finding indirectly supports the view that spillover from the treated groups is of little concern in our causal inference. Finally, we document significant changes in savings location from home or other financial institutions to our experimental account — immediately after the intervention initiation. Thus, premium account holders benefited from the conditional high-interest return by changing the savings location. However, households did not shift all of their savings due to high transaction costs, lack of trustworthiness of this new product introduced by our MFI partner,[6] and difficulty in timely withdrawal of deposit amounts when needed.[7] Overall, we find that the premium CS account helps ultra-poor

[5]In the endline survey, about 59% of respondents mentioned that they do not have sufficient money to save. See Appendix Table 3.

[6]About 28% of the premium account holders mentioned lack of trust as the reason for not fully utilizing the account.

[7]Clients need to inform our MFI partner about their withdrawal decision during the bi-weekly deposit collection activity to receive the amount in the next home visit. In case of emergency, clients need to

households mitigate the severity of seasonal consumption poverty through an increase in formal savings coupled with the subsidy reward.

This study contributes to several strands of literature. First, we shed light on the seasonal dimension of poverty in low- and middle-income countries (LMICs) using unique high-frequency multiple round panel surveys. Household surveys measuring the living standards of LMICs are generally implemented annually, thus masking consumption volatility across the year. Other studies use panel surveys with limited frequency (e.g., two or three times a year (Dercon and Krishnan, 2000; Basu and Wong, 2015)) or exploit the cross-sectional variation of interview timing across households, showing consumption seasonality for different households (Paxson, 1993; Khandker, 2012; Fink et al., 2020). Only a few studies use a similar approach to ours: Chaudhuri and Paxson (2002) on International Crops Research Institute for the Semi-arid Tropics (ICRISAT) villages in India and a series of papers by the Townsend Thai Monthly Survey (Townsend et al., 1997). These studies mainly target farm households that are relatively better off. To the best of our knowledge, no study has collected intensive high-frequency information from landless or near-landless extremely poor households.[8] Building on detailed high-frequency surveys to trace consumption dynamics among year-round ultra-poor, this study extends our understanding of the severity and pervasive nature of seasonal poverty.[9]

Second, this study speaks to the new stream of literature on combating seasonal deprivation during the hungry season, a common feature observed in large parts of Africa and Asia. Existing policies typically address extreme poverty using safety net support through cash or in-kind transfers, but policymakers rarely design timely benefits for the lean seasons. Moreover, existing blanket safety-net supports are costly and prone to mistargeting (Mobarak and Reimão, 2020). One encouraging intervention is the guaranteed rural employment scheme, a cash-for-work program implemented in India in 2006 to address seasonal poverty. Studies report positive impacts of such a scheme on improving consumption and other welfare indicators (Das and Singh, 2013; Dasgupta, 2017; Deininger and Liu, 2013). However, a rigorous RCT evaluation of a similar program executed in Malawi does not find any benefits on food security (Beegle et al., 2017). Moreover, such social protection programs are challenging to design, and expensive to execute since they require forced job creation under limited infrastructural settings — with the right balance in wage offer to allow self-selection for the needy. Another promising intervention is a subsidy program encouraging seasonal migration that documents a considerable gain

visit the MFI office, located at the district town, and complete the paperworks for withdrawal.

[8] For example, the Townsend Thai Monthly Survey includes only 5% of agricultural wage laborers in their sample (Kinnan and Townsend, 2012). One rare exception is theCollins et al. (2009) paper that covers the financial diaries of the ultra-poor households in various LMICs. While their book is quite informative, it does not provide any statistical analysis to help us understand the causal mechanisms underlying seasonal poverty.

[9] In addition to a large number of observations within a year, the advantage of the short-term recall survey is that it collects precise information relative to traditional long-recall surveys. For example, using the Vietnam Household Living Standards Survey 2006, Sawada et al. (2019) show that there is a systematic bias arising from the aggregation of categorized expenditures through long recall surveys.

in consumption Bryan et al. (2014). However, such programs are often unsuitable for females and elderly household members. Furthermore, those who migrated temporarily suffer from other disutilities resulting from pollution and horrible living conditions in informal urban settlement areas (Lagakos et al., 2018).

Financial interventions, for example, credit access, could also play a role. Unfortunately, the existing rigid micro-credit design is not particularly accommodative in addressing seasonality due to strict regular repayment obligations irrespective of the lean season (Shonchoy, 2015). Improving the existing micro-credit design by introducing a seasonality-adjusted repayment scheme shows favorable impacts in reducing reliance on loan sharks and improvement in consumption during the *Monga* Shonchoy and Kurosaki (2014). Related to this approach, a lean season-specific loan program in Zambia, intended to tackle liquidity constraints for farmers, finds a reduction in the desperate coping strategy of selling labor for low-wage in the hungry season (Fink et al., 2020). However, no study has explored the possibility of supporting precautionary savings ahead of the lean season for consumption smoothing. Hence our study sheds light on this unexplored avenue as a promising tool to address seasonal consumption downfall, which is universal, less restrictive, rapidly scalable using existing MFI networks and can be utilized even by ultra-poor households. Our intervetnion and resulted impact could potentially offer a low-cost solution to the exisitng evidence-base to reduce seasonal deprivation. Without the account opening, operation and administrative cost, our 3.5$ savings subsidy in the premium account increased food and non-food expenditure by 8.4 and 13.2% which is comparable to Bryan et al. (2014) 11.50$ migration subsidy that increased food and non-food consumption consumption by 8.5 and 12%, respectively.

Third, this study also adds to the growing literature on helping rural poor save in developing countries. Rigorous evaluation of various savings products, particularly commitment savings accounts, demonstrate promising impacts on achieving savings goals, resulting in improvements in fertilizer adoption for farmers (Ashraf et al., 2006), investment in preventive and emergency health (Dupas and Robinson, 2013a), educational outcomes (Jack and Habyarimana, 2018), business investments and income (Dupas and Robinson, 2013b; Callen et al., 2019), and greater decision-making power within households by women (Ashraf et al., 2010). Another related stream of literature is encouraging savings by inducing positive financial behavior. De Mel et al. (2013) finds evidence that deposit collection service adds to the salience of savings. Schaner (2018) tested a temporary savings subsidy intervention and documented a persistent long-term impact on improving savings habits. We add to this literature by encoureging savings in the CS products in addressing seasonal poverty and hunger.[10]

The remainder of this study is organized as follows. Section 2 explains the study's setting, sampling framework, and experimental design. Section 3 presents and examines

[10]Unfortunately, we could not measure the habit formation and impact persistence of our temporary savings encouragement design on the households in the post-intervention period. This is due to the onset of the COVID-19 pandemic in Bangladesh in early 2020. At that time, our MFI partner discontinued the door-to-door deposit collection initiative.

the summary statistics of the sample. Section 4 explains our estimation strategy for the impact of our intervention on consumption dynamics and discusses the estimation results of the benchmark model. Section 5 conducts a detailed analysis to identify the mechanisms of the observed changes. Finally, Section 6 presents the conclusions of this study.

2 Study Setting

2.1 Background of study area

Rice is the single most important crop in Bangladesh. Hence the seasonality of rural areas is strongly connected to the country's rice-growing cycles, namely *Boro* (January to April/May), *Aus* (May to July/August), and *Aman* (September to December). Of these, the *Aman* paddy, the monsoon rain-fed rice crop in autumn, is the most popular crop in terms of the area harvested, followed by the *Boro* paddy, which is cultivated during the dry season using irrigation.[11] *Aus* plays a limited role in terms of both the area harvested and total production. This agricultural cycle creates a lean season from September to October every year. During this period, rural employment opportunities and rice availability become limited.[12]

Food shortages during the lean season are particularly pronounced in northern Bangladesh, including the Greater Rangpur region— which is the focus of this study—where the economy is less developed and diversified. Rangpur was among the worst hit districts in the Great Bengal Famine of 1942-44 and was the epicenter of the 1974 famine in Bangladesh. The agricultural sector in this region relies heavily on paddy production, with a limited focus on labor-intensive high-value crops, such as vegetables. Furthermore, industrialization in this area lagged behind the national average, with off-farm income sources mostly limited to brickfields and construction works and rickshaw/van pulling. Temporal migration to nearby urban locations is possible but is mainly limited to capable male laborers who are not credit-constraint and can afford the migration cost or has the social network (Bryan et al., 2014). Notably, the poverty headcount ratio of Rangpur in 2016 is 47.2%, unusually high compared with the national average of 24.3%.

Many landless ultra-poor households in river basins and islands in Rangpur experience seasonal starvation during *Monga*. The exact dates of the beginning and end of *Monga* cannot be clearly defined. Based on our pre-intervention data, we consider the acute *Monga* period from mid-September to the end of October, which is consistent with (Mobarak and Reimão, 2020). Since many ultra-poor have minuscule household and pro-

[11]The use of modern inputs, such as the improved variety of seeds, is most common in *Boro* rice because of the complementarity between modern technologies (i.e., irrigation and improved seeds) and the shorter maturity of improved varieties compared to the traditional varieties. Therefore, the total rice production is highest for *Boro*, followed by *Aman*.

[12]Although there is a second lean season before *Boro* in March to May, this is less severe due to the expansion of *Boro* cultivation.

ductive assets, intensive sales of their labor (including advance sale) during *Monga* is one of the limited options they have to avoid starvation, along with borrowing from the loan-sharks. In addition, river-basin dwellers frequently suffer the loss of economic activity, possessions, and earnings owing to seasonal flash floods and subsequent river-bank erosions (Zug, 2006; Takahashi et al., 2017).

2.2 Data

Our sample consists of 180 landless or near-landless rural households in the Gaibandha district of the Rangpur region in Bangladesh. We first purposively selected six villages considering environmental diversity, of which three are on river islands (called "Chars"), and the other three are on the river basins. Using asset-based wealth-ranking assessment with the help of village leaders, we classified the surveyed households in the village census list into ultra-poor (UP) and moderately poor (MP) households. We use these abbreviations to indicate the poverty-assessment-based classification and distinguish it from the consumption-based classification obtained from the survey data that reflects the severity of poverty in each period. We then randomly selected 20 and 10 UP and MP households, respectively, from each village.

The baseline survey for the sample households commenced in April 2018, and short follow-up surveys continued mostly twice per month after that (Figure 1). Appendix Table A1 presents the number of observations for each month, showing that most household surveys were concentrated from April to December, creating (unbalanced) panel data; we extended the surveys to January in the following year if the number of surveys was less than 18.[13] We repeated these surveys twice in 2018-2019 (year 1) and 2019-2020 (year 2), which allowed the comparison of seasonal consumption patterns before and after experimental interventions.

The baseline survey in April 2018 provided detailed information on households' roster, food and non-food expenditures, durable and nondurable assets, savings, and debts. This survey also collected year-long retrospective information on income-generating activities. We then traced seasonal changes, such as expenditure and experiences of socioeconomic shocks, and local informal transfers in the high-frequency follow-up surveys. We did not collect income data during the first year to reduce the response burden and possible reporting errors due to lengthy questionnaires. The expenditure information contains retrospective data for a week prior to the surveys and covers most of the household's primary food and non-food items.[14] To better capture the welfare dynamics of the sample households, we began collecting seasonal data on income-generating activities and remittances in the second year (intervention year).

[13]We skipped household surveys at the time of Eid holidays and during the occurrence of flood due to enumerators and respondents unavailability.

[14]Non-food consumption here includes items such as cigarettes, clothing, cosmetics, soap, fuel, transport and communications, education and medical expenditures, and social expenses, such as on ceremonies and other religious events.

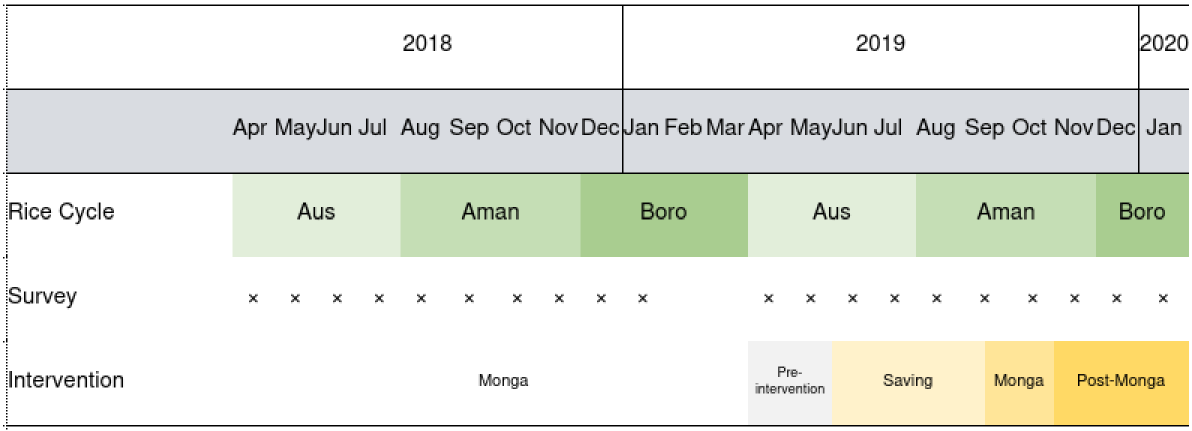

Figure 1: Time Table of Survey and Experiment

2.3 Experimental design

We introduced CS products to a subset of sample households in collaboration with a local MFI, the Gaibandha Rural Development Foundation (GRDF).[15] This intervention was announced in May 2019, and households had opportunities to save twice a month from June to early September through the MFI hired deposit collectors who provided door-to-door service. Our CS scheme offers a free account without opening, maintenance, and withdrawal fees. Households can withdraw money at any time; however, a commitment component is imposed on them: interest (or the subsidy reward) is not paid if they break the commitment to save until the beginning of the set *Monga* period.

We randomly selected 72 households (48 UP and 24 MP) to receive a premium CS account with a temporary 50% interest return (Treatment 1: T1)[16], while 54 households (36 UP and 18 MP) are assigned to receive the market interest rate, that is, approximately 8% per annum interest reward (Treatment 2: T2). The remaining 54 households (36 UP and 18 MP) served as an experimental control group (Control: C) that received no intervention. Randomization was stratified by poverty categories and village level. The maximum saving amount for the CS account was set at 4,000 BDT. In execution, the deposit collectors of GRDF visited eligible households twice a month to collect savings independently from the household survey team organized by our research collaborator MOMODa Foundation.

We differentiate the interest rate to examine whether access to CS schemes alone is sufficient or whether additional incentives are required for people to save for anticipated seasonal deprivation during *Monga*. While the assignment of each treatment arm was not publicly disclosed, and the deposit collector visited each household individually, an obvious threat to our identification is the existence of a spillover effect, where benefits from the extra savings interest are shared through local inter-household transactions. If

[15] GRDF is a licensed MFI to collect savings in local areas.

[16] For simplicity, 50% interest is rewarded regardless of the duration of savings.

this is the case, the treatment effect may be attenuated. Our field visit revealed that most sample households knew others' treatment status, creating room for such informal transactions. We discuss this possibility in more detail later.

3 Descriptive Statistics

3.1 Baseline household characteristics

Table 1: Baseline Characteristics

Variable	(1) Total Mean/SE	(2) MP Mean/SE	(3) UP Mean/SE	t-test Difference (2)-(3)
Household size	4.011	4.333	3.850	0.483**
	(0.104)	(0.185)	(0.123)	
Head is male (dummy)	0.928	0.983	0.900	0.083**
	(0.019)	(0.017)	(0.028)	
Head's age (years)	43.544	44.667	42.983	1.683
	(0.919)	(1.435)	(1.178)	
Head's education (years)	1.900	2.533	1.583	0.950*
	(0.242)	(0.512)	(0.254)	
Temporary worker: Agriculture, Construction etc (dummy)	0.767	0.750	0.775	-0.025
	(0.032)	(0.056)	(0.038)	
Transportaion worker (dummy)	0.061	0.083	0.050	0.033
	(0.018)	(0.036)	(0.020)	
Trader (dummy)	0.050	0.067	0.042	0.025
	(0.016)	(0.032)	(0.018)	
Own any agricultural land (=1)	0.272	0.667	0.075	0.592***
	(0.033)	(0.061)	(0.024)	
Land size (hectare)	0.387	0.968	0.096	0.871***
	(0.050)	(0.101)	(0.032)	
Number: cattle	0.811	1.267	0.583	0.683***
	(0.071)	(0.109)	(0.085)	
Number: goat	0.522	0.833	0.367	0.467***
	(0.080)	(0.172)	(0.080)	
Value of other productive asset (BDT)	1475.811	1297.000	1565.217	-268.217
	(515.065)	(290.071)	(759.946)	
Daily per capita income (BDT)	59.067	61.816	57.693	4.123
	(2.962)	(5.900)	(3.335)	
of which (%)				
Wage income	0.801	0.760	0.821	-0.061
	(0.026)	(0.048)	(0.030)	
Crop income	0.000	0.011	-0.005	0.015
	(0.007)	(0.005)	(0.010)	
Livestock income	0.001	0.004	0.000	0.004
	(0.001)	(0.004)	(0.000)	
Non-farm self-employment	0.136	0.159	0.124	0.035
	(0.026)	(0.045)	(0.032)	
Other income	0.062	0.066	0.059	0.007
	(0.009)	(0.018)	(0.011)	
Observations	180	60	120	

Notes: The value displayed for t-tests are the differences in the means across the groups. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Daily per capita income and its composition reflect the data one year prior to the baseline survey.

Table 1 presents the baseline characteristics of the 180 sample households separately for the UP and MP households as of April 2018. Each household has an average of four members and is headed by a male. The average age of the household head is 44 years, with less than two years of formal education. The main occupation is temporary work, such as agricultural wage employment or construction work, followed by transportation (e.g., rickshaw pulling), occasional fishing, and seasonal petty trading. Most households, especially UP households, do not have sufficient productive assets, including agricultural land or livestock.[17] Approximately 80% of the household income comes from wage earnings. The contributions of crops and livestock to total household income are negligible.

Columns (2) and (3) confirm that the asset-based wealth-ranking exercise is mostly successful. Regarding the household head's education, area of owned land, and the number of cattle and goats, MP households are significantly better than UP households. The value of productive assets, including fishing nets, is slightly higher for UP households, but the difference is not statistically significant.

3.2 Consumption and poverty dynamics in the base year

Regardless of the initial asset-based poverty ranking, the vast majority of the sample households are extremely poor. Figure 2 shows the daily real per capita household expenditure in the pre-intervention year, from April 2018 to January 2019, decomposed into food and non-food expenditures, using the monthly consumer price index (CPI) as a deflator.[18] The horizontal dotted line represents the international poverty line of 1.90 USD (2011 PPP) converted into the local currency.

Expectedly, our sample households' average consumption level is far below the poverty line in most months, except for June and August. June comes just after the *Boro* harvest, with greater employment and earning opportunities coupled with an overall reduction in the staple prices. Note that the month-long fasting for Muslims, *Ramadan* lasted from May 16 to June 14 in 2018, and Muslims celebrated the *Eidul-Fitr* festival after the end of the fasting period.[19] Although those who are fasting are not allowed to take any food or drink from sunrise to sunset; food consumption typically happens before dawn and after sunset during *Ramadan*; moreover, because of the festive season, many spend considerably on luxury items, such as clothing, to prepare for *Eidul-Fitr*. Meanwhile, August corresponds to another significant religious event, *Eidul-Adha*, when people sacrifice animals, typically cows, for the completion of pilgrimages performed by Muslims worldwide at *Mecca*.

Except for these months, the sample households suffer from chronic poverty regardless of whether they are categorized as UP and MP. Table 2 presents the Foster-Greer-

[17]Agricultural land owned by our sample households is of low quality and not suitable for intensive crop cultivation. Thus, it is often left idle or remains sub-merged most of the year due to river-bank erosion.

[18]The monthly CPI is obtained from https://www.theglobaleconomy.com/Bangladesh/cpi/

[19]The timing of the *Eidul-Fitr* festival, explained shortly, is not fixed regarding their dates as they follow the lunar calendar.

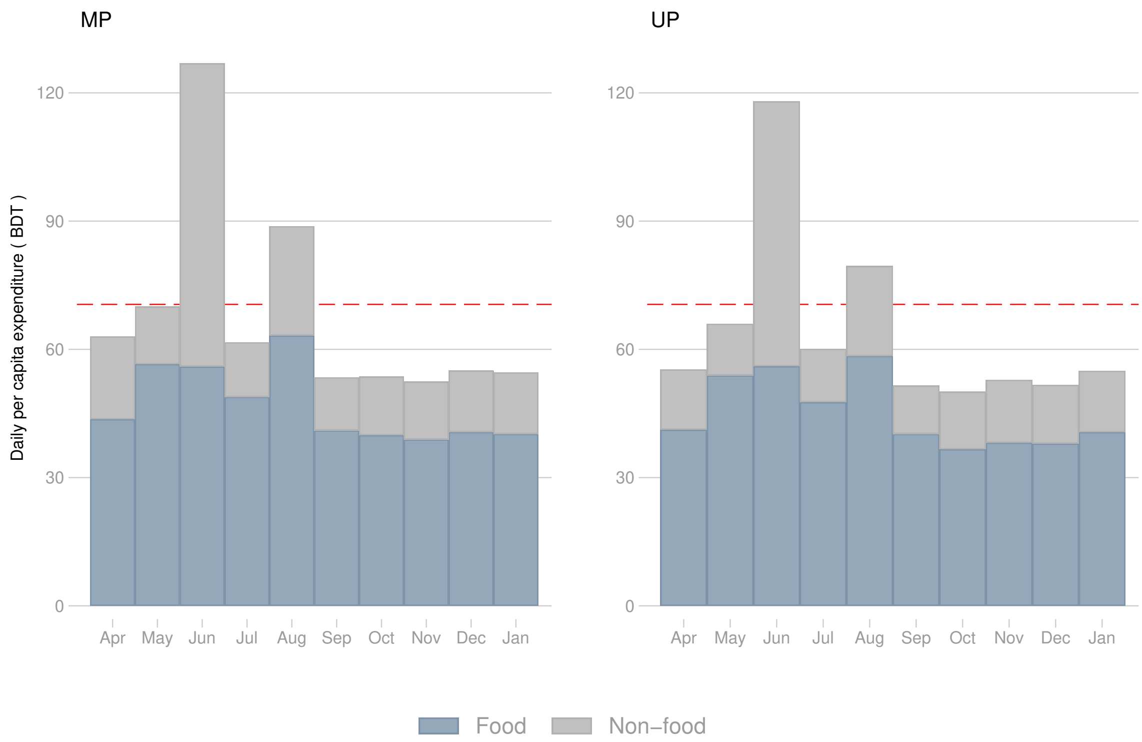

Figure 2: Food and Non-food Expenditure (daily per capita BDT) in the Base Year

Thorbecke poverty indexes, coefficients of variation, and generalized entropy (GE) indexes for real per capita consumption in each month.[20] On average, 73 % of households are poor based on the international poverty line, with the average number of survey rounds when the household remains below the poverty line being 13 (out of 18 pre-intervention survey rounds). The fall in the consumption level is extremely severe during *Monga*, with the poverty headcount ratio jumping to 84% and 85% in September and October, respectively. Both the poverty gap and severity are the highest, with a relatively lower coefficient of variation in October, indicating that many sample households suffer starvation equally and deeply during this month.

Table 2: Monthly Poverty and Inequality in the Base Year

	(1) % Poverty (P0)	(2) Poverty Gap (P1)	(3) Poverty Severity (P2)	(4) Coefficient of Variation	(5) Theil's L GE(0)	(6) Theil GE(1)
Apr	0.839	0.257	0.098	0.532	0.071	0.081
May	0.689	0.150	0.045	0.354	0.071	0.076
Jun	0.304	0.066	0.019	0.632	0.174	0.182
Jul	0.775	0.203	0.067	0.322	0.053	0.055
Aug	0.526	0.146	0.051	0.611	0.152	0.169
Sep	0.835	0.308	0.133	0.416	0.067	0.073
Oct	0.846	0.312	0.135	0.396	0.078	0.087
Nov	0.847	0.306	0.129	0.561	0.083	0.093
Dec	0.808	0.302	0.130	0.395	0.106	0.117
Jan	0.800	0.291	0.123	0.426	0.087	0.104
Overall	0.729	0.231	0.091	0.464	0.115	0.131
Decomposition by month						
within					0.095	0.109
between					0.020	0.022
Decomposition by UP or MP						
within					0.115	0.131
between					0.0001	0.0001

The consumption inequalities across households, measured by GE indexes, are also relatively low during *Monga* and high in June and August when the mean consumption level is inflated. To understand the factors accounting for the consumption inequality, we show the contributions of within- and between-group inequality to overall inequality at the bottom part of table 2 separately by month and initial asset-based poverty assessment. Nearly one-fourth of the overall inequality is accounted for by differences in consumption levels across months, and the rest are those across households within each month. However, the different consumption levels found by the initial asset-based poverty category explain little of the overall inequality.[21]

These results indicate that sampled households remain poor almost all the months and show large seasonal fluctuations in consumption levels, with exaggerated poverty and food

[20] The GE indexes are a measure of inequality in a population. The parameter in parentheses reflects the relative weight assigned to differences between welfare levels at different places in the distribution, where the smaller parameter is sensitive to the lower end of the distribution. GE(0) is also known as Theil's L index or the mean log deviation, whereas GE(1) is known as Theil's index.

[21] We see a similar trend for calorie intake, which can more directly represent the severity of food starvation and be a deflation-free measure.

deprivation in the *Monga* period.[22] This may be somewhat puzzling because households can anticipate periodic food shortages. Supporting these un-banked households with formal financial intermediaries, such as CS products, could mitigate the severity of seasonal poverty — an important question that we address in the next section.

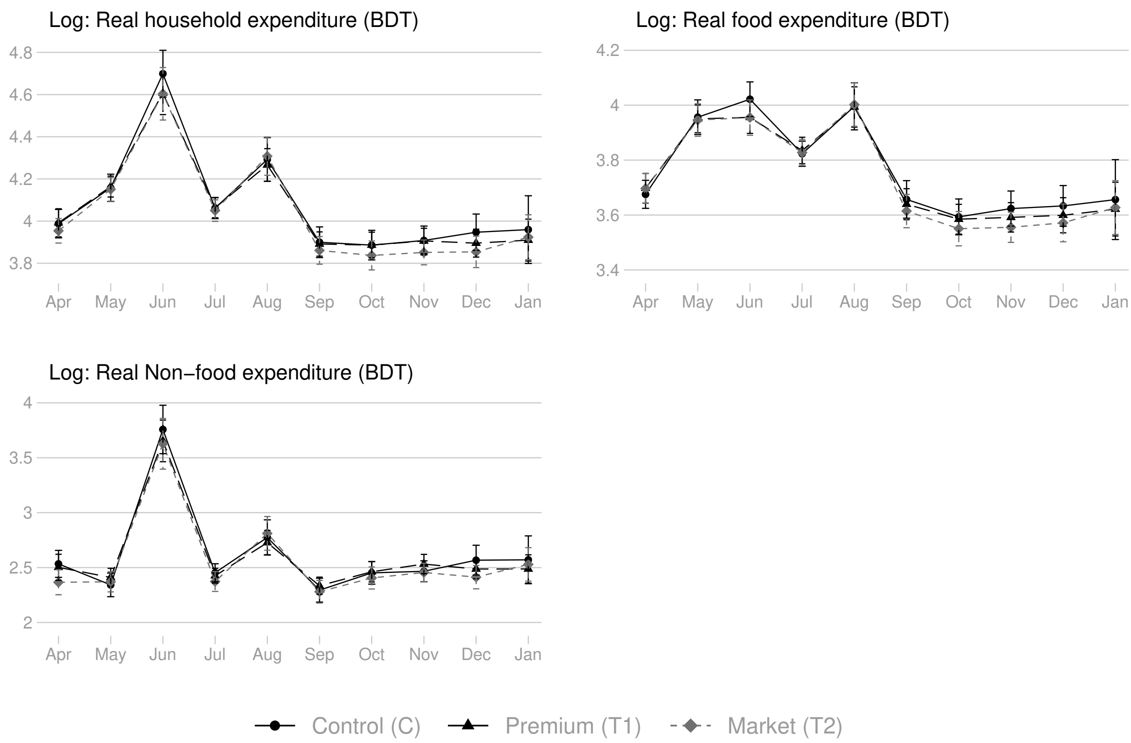

Figure 3: Outcome Balance across Treatment in the Base Year

Figure 3 checks whether the consumption patterns in year 1, i.e., the pre-intervention period, differ by treatment status. The overall similarity in consumption dynamics between the MP and UP households prompts the pooling of their data for better readability. Overall, we find no apparent differences in consumption patterns by treatment status during the base year. Appendix Table A2 for balance check confirms that our randomization is largely successful; the number of t-tests showing statistical differences is less than 5% with no joint significant difference in the basic characteristics and the average outcomes in the base year, as reflected in the F-statistics.

[22] This pattern persists if we use the adult equivalent scale, suggested by Waida et al. (2017).

3.3 Formal savings in the second year

Table 3 shows the amount of savings in our experimental savings account by treatment status for each month from June to September 2019. The control group is excluded because it does not save in our experimental account throughout the study period. Take-up rates among treatment groups are high: 86% and 78% of eligible households in T1 (premium account) and T2 (market interest), respectively. All eligible households started saving in June, even though they sometimes stopped accumulating additional deposits. Only two households withdrew money before the *Monga* period, and the rest received the full savings amount with interest between mid-September to early October.

Table 3: Savings by Treatment Status

	Mean	S.D	p25	p50	p75	Max
Premium (T1, N=72)						
Deposit (dummy)	0.86	0.35	1.00	1.00	1.00	1.00
Deposit in June (BDT)	139.31	98.13	100.00	145.00	160.00	560.00
Deposit in July (BDT)	152.22	178.69	50.00	115.00	200.00	1110.00
Deposit in August (BDT)	126.25	148.88	0.00	100.00	165.00	600.00
Deposit in September (BDT)	165.56	291.76	0.00	85.00	200.00	1800.00
Total deposit (BDT)	583.33	599.54	200.00	450.00	755.00	3810.00
Market (T2, N=54)						
Deposit (dummy)	0.78	0.42	1.00	1.00	1.00	1.00
Deposit in June (BDT)	88.89	60.86	40.00	100.00	130.00	210.00
Deposit in July (BDT)	94.63	87.69	0.00	85.00	170.00	300.00
Deposit in August (BDT)	66.85	72.78	0.00	50.00	100.00	320.00
Deposit in September (BDT)	49.44	62.30	0.00	20.00	100.00	200.00
Total deposit (BDT)	299.81	248.03	60.00	300.00	500.00	810.00

Note: p25, p50, and p75 correspond to 25%, 50%, and 75% percentile, respectively.

Despite the high account utilization rates and a good understanding of the accounts benefit features, our sample households did not fully exploit the opportunity to save up to the upper limit of 4,000 BDT. The average savings amounts for the T1 and T2 households are only 583 and 300 BDT, respectively. Conditional on take-up, the average savings in the T1 and T2 groups are 677 and 385 BDT, respectively. [23] The low savings may be partly explained by the low-income levels of the sample households, since about two-third of the respondents mentioned insufficient income as the main reason for this limited savings in the endline survey.

3.4 Consumption and income in the intervention year

There are several noteworthy findings on income and consumption seasonality in Figure 4. First, we can notice that the average local earning was highest in May, which decreased

[23]While T1 households are more likely to save, surprisingly, they could not maximize the benefits. It is rational for them to keep the maximum amount in the experimental account by borrowing even from moneylenders, who may charge extraordinarily high but generally less than 50% interest per month (Khandker and Mahmud, 2012).

during the *Monga* period. Local earning seasonality correlates with consumption seasonality depicted in the bottom right sub-figure. Second, the relatively flat income flow is presumably caused by the wage income-dependent households who do not produce rice. In addition, household income shifts towards seasonal migration-driven remittances from September to December, when their local employment opportunities are relatively limited. This relatively flat income flow is consistent with observations in Thailand, where the income of rural non-farm households does not necessarily respond much to the agricultural cycle (Chaudhuri and Paxson, 2002).[24] Third, households are often in deficit and have insufficient income to save, regardless of treatment status. This observation — that total consumption is consistently larger than total income — is puzzling but in line with the findings of Meghir et al. (2022) in northern Bangladesh. Lastly, the difference in consumption dynamics across treatments is visually less clear; however, household expenditure in the *Monga* period appears more stable for T1 than for C and T2.

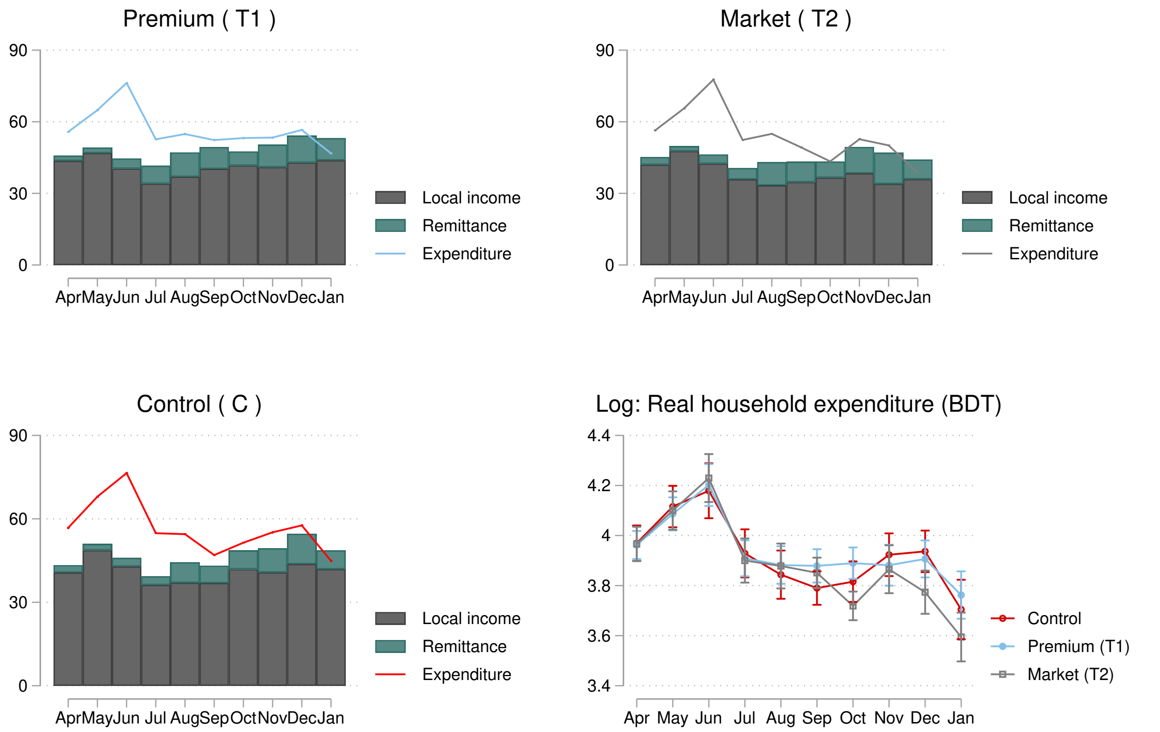

Figure 4: Real per Capita Income and Expenditure in 2019/2020

[24] A significant drop in local income in July is partly attributable to sudden flash floods in this month.

4 Estimation

4.1 Empirical framework

We conduct ANCOVA Intention-to-treat (ITT) estimations on our outcome variable of interest as follows:

$$\begin{aligned} y_{ijt} =& \beta_0 + \beta_1 Premium_i + \beta_2 Market_i + \beta_3 Monga_t + \beta_4 (Premium_i \times Monga_t) \\ &+ \beta_5 (Market_i \times Monga_t) + \beta_6 y_{ijt_0} + \epsilon_{ijt}, \end{aligned} \tag{1}$$

where y_{ijt} is the outcome variable of interest at the household i in village j at interview date t in year 2019/2020. $Premium_i$ and $Market_i$ are dummy variables equal to one if the household is in the T1 (premium interest) and T2 (market interest) groups, respectively. The reference category is a household in the control group. $Monga_t$ is a dummy equal to one if the observation date t is in the *Monga* season (from mid-September to October). y_{ijt_0} is the outcome observed in the pre-intervention year 2017/2018. Since no sample households are surveyed on the exact same date, we use one year-lagged average monthly consumption at t, following (McKenzie, 2012). Finally, ϵ_{ijt} is the error term clustered at the household level and βs are parameters to be estimated.

β_3 represents the average consumption of the control group during *Monga* conditional on y_{ijt_0}, while the coefficients β_4 and β_5 measure the degree of improved consumption due to our interventions on T1 and T2 groups, respectively. We estimate the above equation with and without household fixed effects. These two estimates should essentially be the same if we do not include any control variables except for the main variables of interest (i.e., *Monga* and treatment dummies) because the constant term and each treatment dummy absorb the average household-level differences across treatment groups. With some controls, such as one-year lagged outcomes, results can differ. Without household fixed effects, the coefficients of the interaction term capture the differential consumption level across the treated groups during *Monga*, controlling for covariates. With fixed effects, the coefficients of the interaction term capture the differential consumption level across treatment groups relative to each household-specific mean. In other words, the latter represents how the deviation of household consumption of the *Monga* seasons from normal periods differs across the treated groups. This is of interest to understand the degree of recovery in the hungry season relative to each household mean; moreover, household fixed effects can control for any time-invariant confounders, including baseline imbalances.

The primary outcomes of interest are (1) per-day total expenditure, (2) per-day food expenditure, and (3) per-day non-food expenditure, transformed into real per-capita scale with log transformation.

To obtain deeper insight into the underlying behavioral changes induced by our experiment, we further disaggregate food and non-food expenditure into major consumption items. To reduce the probability of detecting false positives with multiple outcome variables, we categorize major consumption items by (1) staple foods (rice, wheat, maize, etc.), (2) protein (egg, fish, meat, etc.), (3) temptation goods (cigarette, chewed tobacco, betel leaf, etc.), (4) personal care (clothes, haircut, soaps, cosmetic items, etc.), (5) education and medical care, and (6) social activities (festivals, ceremonies, etc.). All variables are expressed in per capita real daily expenditure (BDT). Because some variables contain zero values in some survey rounds, except for staple foods, we use the level variables without log transformation for these secondary outcomes to avoid missing observations.[25]

4.2 Effects on primary outcomes

Table 4 presents the estimation results for the primary outcomes. Columns (1) to (3) present the results of a parsimonious model for reference, where only *Monga* and treatment dummies are used as regressors. Columns (4) to (9) show our main estimation results based on Equation (1), where Columns (7) to (9) report the estimates additionally with the household fixed effects. The number of observations drops by 60 in columns (4) to (9) due to missing data on one year-lagged outcomes for some households [26].

Table 4: Effects of Commitment Savings on Consumption

	(1) Total	(2) Food	(3) Non-food	(4) Total	(5) Food	(6) Non-food	(7) Total	(8) Food	(9) Non-food
Premium	0.000	0.024	-0.064	0.010	0.029	-0.058			
	(0.049)	(0.050)	(0.054)	(0.033)	(0.028)	(0.047)			
Market	-0.031	-0.031	-0.046	-0.011	-0.017	-0.028			
	(0.049)	(0.051)	(0.054)	(0.032)	(0.028)	(0.047)			
Monga	-0.167***	-0.118***	-0.256***	-0.048	0.006	-0.201***	-0.123***	-0.101***	-0.237***
	(0.028)	(0.026)	(0.049)	(0.031)	(0.029)	(0.051)	(0.029)	(0.026)	(0.050)
Premium × Monga	0.090**	0.084**	0.132**	0.076**	0.079**	0.118*	0.086**	0.086**	0.126**
	(0.035)	(0.033)	(0.060)	(0.037)	(0.035)	(0.062)	(0.036)	(0.033)	(0.061)
Market × Monga	-0.003	0.012	-0.001	-0.009	0.015	-0.014	-0.006	0.012	-0.009
	(0.038)	(0.036)	(0.065)	(0.039)	(0.037)	(0.067)	(0.038)	(0.036)	(0.067)
Lagged: log total per capita expenditure				0.483***			0.195***		
				(0.034)			(0.030)		
Lagged:log per capita food expenditure					0.636***			0.099***	
					(0.039)			(0.038)	
Lagged:log per capita non-food expenditure						0.227***			0.113***
						(0.023)			(0.022)
Control mean	3.931	3.557	2.644	3.936	3.560	2.655	3.936	3.560	2.655
Household FE	No	No	No	No	No	No	Yes	Yes	Yes
β_3+β_4=0	0.000	0.108	0.001	0.223	0.000	0.024	0.085	0.474	0.002
Obs	3240	3240	3240	3180	3180	3180	3180	3180	3180

Notes: Clustered standard errors at the household level are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% significance level.

Our estimates reported in columns (7) to (8), our most preferred regressions, indicate that *Monga* time food and non-food consumption decreases by about 10 and 24 percent, respectively, with an overall decrease in total expenditure by 12%. These estimates are similar as reported in columns (1)-(3) in the parsimonious estimates. We see that households with the T1 (premium interest) account significantly improved their food and non-food consumption during *Monga* period, relative to that of the control group, by about 9% and 13%, respectively. These impact estimates of the *Monga* time food and non-food expenditure improvement in the premium account holders are consistent with

[25] Alternatively, one can use the inverse hyperbolic sine transformation. While we see minor differences between those results and those presented here (significant impacts on social expenditure does not hold), the main results remain similar.

[26] Mostly missing information for January 2019.

other specifications reported in Table 4, approximately 8-9% and 13%, respectively, with and without fixed effects.

Although the F-test (on β_3+β_4=0) based on the parsimonious model rejects the null hypothesis that T1 households fully recover their total expenditure downfall during the *Monga*, we noticed statistical evidence of expenditure smoothing on the food items, as evident in column (2) and (8). Our simple back-of-envelope calculation reveals that these figures are not unrealistic. Given that the monthly household expenditure of control households in *Monga* is approximately 5,937 BDT, premium account holding households increase food expenses by 474 BDT (8%) on average. Although the average interest income gain is only 291 BDT (583 $\times$ 0.5), T1 households who saved in CS account also receive their principal amount back; hence, they received an additional 874 BDT in total at the beginning of *Monga*. It indicates that immediately after the unlocked savings plus subsidy reward was given back to these households, it mostly went for supporting expenditure, with a very high marginal propensity to consume, as found in Bryan et al. (2014).

However, T2 (market interest) households did not benefit from our intervention, as our CS account with a typical interest rate did not encourage families to do more precautionary savings ahead of *Monga*. These results are robust to different specifications. Together, they indicate that secure saving access or commitment saving addressing behavioral constraints are not sufficient and greater incentives to save are necessary to mitigate the seasonal poverty of ultra-poor households, at least temporarily.[27]

4.3 Effects on secondary outcomes

Table 5 shows the estimation results with (Columns 6-10) and without (Columns 1-5) household fixed effects for the secondary outcome variable of interests. The increased expenditure of the T1 group in *Monga* appears to be driven by improved food intake, especially in the form of protein, as well as increased social involvement. Households in the T1 group can mitigate consumption downfall during *Monga*, with roughly 15% increase in per capita protein expenditure relative to control households.

This reallocation pattern is similar to the findings of a savings experiment in Nepal (Prina, 2015). Given the dominant share of food in overall consumption, at more than 70%, the most salient treatment effect may be that T1 households shift to consuming more nutritious foods in the acute hungry season with money received from the savings account. In fact, our data confirm that daily per capita calorie intake increased by approximately 100 kcal in the T1 group during the *Monga* period relative to the control households (where the control mean is about 1,900 kcal). There is no significant increase in the other non-food items, including temptation goods. We detect no significant impact for the T2 group on these secondary outcomes as found before.

[27] According to Schilbach (2019), sophisticated hyperbolic discounters make a commitment even if it is costly. If this is the case, the negligible impact on market interest groups would mean our experiments involve many naive people. We leave the direct test of this hypothesis to future studies.

Table 5: Effects on Consumption by Categories: ANCOVA

	(1) Staple	(2) Protein	(3) Temptation	(4) Beutification	(5) School/Health	(6) Social	(7) Staple	(8) Protein	(9) Temptation	(10) Beutification
Premium	0.093	0.402	-0.189	-0.570	-0.430	-0.281	0.000	0.000	0.000	0.000
	(0.542)	(0.631)	(0.198)	(0.742)	(0.388)	(0.236)	(.)	(.)	(.)	(.)
Market	-0.530	-0.274	-0.370**	-1.043	0.954	-0.271	0.000	0.000	0.000	0.000
	(0.545)	(0.624)	(0.187)	(0.754)	(0.688)	(0.260)	(.)	(.)	(.)	(.)
Monga	0.428	-2.872***	0.289	-3.334***	1.471	-1.092***	-0.128	-3.843***	0.188	-3.409***
	(0.366)	(0.565)	(0.181)	(0.692)	(1.656)	(0.220)	(0.366)	(0.561)	(0.172)	(0.691)
Premium × Monga	0.611	1.981**	-0.239	1.026	-1.349	0.512*	0.749*	2.039**	-0.281	1.026
	(0.471)	(0.784)	(0.227)	(0.880)	(1.689)	(0.273)	(0.444)	(0.798)	(0.213)	(0.875)
Market × Monga	0.083	0.157	-0.169	0.682	-2.999	0.158	0.091	0.184	-0.226	0.685
	(0.469)	(0.819)	(0.249)	(0.921)	(1.829)	(0.267)	(0.437)	(0.851)	(0.245)	(0.919)
Control mean	13.986	11.885	3.229	6.119	3.438	0.705				
Household FE	No	No	No	No	No	No	Yes	Yes	Yes	Yes
Obs	3180	3180	3180	3180	3180	3180	3180	3180	3180	3180

Notes: Clustered standard errors at the household level are in parentheses. A-year-lagged outcome is controlled. ***, **, and * indicate significance at the 1%, 5%

4.4 Alternative specification

We now conduct a robustness check of our benchmark results using an alternative model: the difference-in-differences (DID) method. While ANCOVA is generally preferred for noisy and relatively less autocorrelated outcomes (McKenzie, 2012),[28] we can increase the statistical power by including multiple baseline observations of year 1, taking advantage of the high-frequency surveys. In addition, Figure 3 shows parallel consumption patterns across treatments in the absence of intervention in the base year, which justifies the use of the DID model of the following form:

$$\begin{aligned} c_{ijt} =& \alpha_0 + \alpha_1 Premium_i + \alpha_2 Market_i + \alpha_3 Monga_t + \alpha_4 Post_t + \alpha_5 (Monga_t \times Post_t) \\ &+ \alpha_6 (Premium_i \times Monga_t) + \alpha_7 (Market_i \times Monga_t) + \alpha_8 (Post_t \times Premium_i) \\ &+ \alpha_9 (Post_t \times Market_i) + \alpha_{10} (Premium_t \times Monga_i \times Post_t) + \alpha_{11} (Market_i \times Monga_t \times Post_t) \\ &+ \epsilon_{ijt}, \end{aligned} \tag{2}$$

where $Post$ is a second-year dummy, and α_{10} and α_{11} are the parameters of interest. α_1 and α_2 are absorbed into household fixed effects for the fixed estimation model.

Table 6 presents the DID estimation results with (Columns 4-6) and without (Columns 1-3) household fixed effects. The early findings from the ANCOVA-ITTmodel are supported in this robustness check. As before, we find a positive and significant increase in food and non-food consumption of T1 (premium interest) households during *Monga* in the intervention (second) year, with no impact on T2 (market interest) households. The estimated magnitude is mostly similar to that in the ANCOVA-ITT estimation. The DID result also supports our causal inference that the increased consumption during *Monga* for the T1 households is attributable to our intervention; we find no significant difference in consumption across the treated groups during *Monga* in year 1, supporting the parallel trend assumption, reflected in the coefficients of interaction between the treatment and *Monga* dummies.

[28] Our log per capita household expenditure exhibits an autocorrelation of 0.35.

Table 6: Effects on Consumption:DID

	(1) Total	(2) Food	(3) Non-food	(4) Total	(5) Food	(6) Non-food
Premium	-0.017	-0.014	-0.007			
	(0.054)	(0.051)	(0.066)			
Market	-0.036	-0.024	-0.056			
	(0.056)	(0.053)	(0.070)			
Monga	-0.240***	-0.210***	-0.223***	-0.246***	-0.214***	-0.235***
	(0.015)	(0.015)	(0.032)	(0.013)	(0.011)	(0.032)
Premium × Monga	0.018	0.011	0.003	0.018	0.012	0.002
	(0.021)	(0.019)	(0.044)	(0.019)	(0.016)	(0.043)
Market × Monga	-0.003	-0.012	0.017	0.001	-0.009	0.024
	(0.025)	(0.021)	(0.054)	(0.021)	(0.017)	(0.051)
Premium × Monga × 2nd year	0.073*	0.073**	0.129*	0.073*	0.072**	0.130*
	(0.038)	(0.036)	(0.070)	(0.038)	(0.035)	(0.070)
Market × Monga × 2nd year	-0.001	0.024	-0.018	-0.004	0.022	-0.025
	(0.040)	(0.040)	(0.078)	(0.039)	(0.038)	(0.078)
Control mean	3.988	3.675	2.533			
Household FE	No	No	No	Yes	Yes	Yes
Obs	6479	6479	6479	6479	6479	6479

Notes: Clustered standard errors at the household level are in parentheses. We also control interaction of treatment status and post intervention dummies, as well as *Monga* and post intervention dummies. ***, **, and * indicate significance at the 1%, 5%, and 10% significance level.

5 Intermediary Channels

Previous estimation results consistently show increased consumption of the premium interest (T1) group relative to the control (C) during the lean season, *Monga*. Here, we consider the intermediary channels behind these observed impacts. We first characterize the potential pathways with reference to the above empirical findings and then test each possible path using regression analysis.

5.1 Conceptual framework

Following conventional practice, we assume that poor households in our sample are risk-averse with a concave utility function; consequently, they prefer consumption smoothing across periods. However, perfect consumption smoothing for all expenditure categories is impossible, hence households face seasonal consumption fluctuations (Figure 2).[29]

Although identifying the exact binding constraints is difficult in this setting, one possible candidate is an institutional constraint: the poor do not have adequate access to formal and safe savings instruments. Such savings instruments help poor to avoid demand to share savings among family members and social networks. Also such products help tackle behavarioal contrainstraint to saving (such as inattention and temptation). Our CS account intervention improves the accessibility of financial instruments for the T1 and T2 households. The high uptake rate of CS accounts by T1 and T2 households is indicative of the existence of institutional constraints in the status quo (Table 3). Nonetheless, relaxing institutional constraints *per se* have no meaningful impact on T2 households. Instead, we consistently see a positive effect only for T1 households where savings have

[29]As Abdullah (1989) precisely point out, a constant level of food energy consumption may not necessarily represent an optimal pattern of intake if energy expenditure patterns vary from season to season. Even when this is the case, a poor household may try to smooth out consumption fluctuations over time; however, this does not seem to be the case in our sample.

been encouraged with a generous subsidy reward (Tables 4-6).

Another possibility relates to preferences: the conventional savings scheme is not sufficiently attractive because either the discount rate of poor households is much higher than the market interest rate, or households are myopic and have time-inconsistent preferences. While we cannot directly test them, our intervention exogenously changes the interest rate T1 households face. This may affect intertemporal decision making without directly changing their preferences and income-generating capacities.[30]

When the interest rate temporarily increases by 50%, T1 households can react to it by (1) reducing current and increasing savings for future consumption, (2) increasing current labor earnings (by exerting more efforts) inside and outside of the local areas, (3) increasing or decreasing inter-household transactions, including informal borrowing and lending, and (4) shifting from savings at home or other institutions into the experimental savings account.

The first is obvious because this is a textbook example of the intertemporal substitution of consumption (with no substantial income effects). We may also observe this shift if our commitment devices help overcome self-control problems.[31] The second one can also be based on a neoclassical explanation of intertemporal labor substitution and has recently been discussed and empirically validated by Callen et al. (2019). In a nutshell, the increased interest rate induces more labor efforts to enjoy interest-driven consumption in the future. The theoretical consequences of the third one, which includes inter-household resource reallocation, are less clear. T1 households may increase (informal) borrowing as long as the savings interest rate exceeds the borrowing interest rate. Meanwhile, the increased interest rate may induce to relocate more money to own savings instead of informal transfer to others, leading to less reliance on informal risk-sharing networks over time. In this case, informal borrowing and lending, if any, may be crowded-out. The fourth one occurs because the opportunity cost of ”mattress” or informal insured savings increases. Alternatively, other households may be willing to ask the T1 households to save on their behalf to capture a part of the benefits from our intervention. If such interhousehold transactions occur between T1 and other sample households, it can be a threat to our causal inference as it is likely to violate the Stable Treatment Unit Value Assumption (SUTVA). Next, we empirically test each of these potential channels.

[30]Carvalho et al. (2016) discuss the possibility that a time discount factor may vary with changes in the accessibility of a savings account. We cannot test this because of a lack of appropriate data.

[31]We tried to elicit time preferences and whether a household is present-biased following the standard experimental protocol. However, we could not reliably measure these parameters because of the very low literacy rates among our sample households; therefore, we decided not to use them in our analysis. If these preferences and behavioral parameters are time-invariant, they should be absorbed in household fixed effects. Therefore, we infer the plausibility of our treatment impact based on actual behaviors rather than the experimentally elicited-preference approach.

5.2 Effects on intertemporal consumption

To identify dynamic behavioral responses, we re-classify the observation period in the second year depending on the stage of our intervention as follows: (1) Pre-intervention (April to May); (2) Savings intervention (June to mid-September); (3) *Monga* (mid-September to October); and (4) Post-*Monga* (November to January next year). We then run regressions with the same outcome and control variables using equation (1) with and without fixed effects. The reference group is now the pre-intervention consumption of the control households. Because the reference period changed from the non-*Monga* period, the magnitude of the coefficients on *Monga* can also change accordingly.

Table 7: Effects on Consumption Dynamics: ANCOVA

	(1) Total	(2) Food	(3) Non-food	(4) Total	(5) Food	(6) Non-food
Premium	-0.021	0.005	-0.077	0.000	0.000	0.000
	(0.050)	(0.042)	(0.078)	(.)	(.)	(.)
Market	-0.000	-0.002	-0.009	0.000	0.000	0.000
	(0.054)	(0.048)	(0.083)	(.)	(.)	(.)
Savings	-0.209***	-0.123***	-0.231***	-0.123***	-0.064**	-0.146**
	(0.036)	(0.028)	(0.062)	(0.033)	(0.027)	(0.059)
Monga	-0.151***	-0.031	-0.388***	-0.218***	-0.172***	-0.395***
	(0.048)	(0.047)	(0.067)	(0.041)	(0.040)	(0.065)
Post-monga	-0.076	0.030	-0.315***	-0.118**	-0.085*	-0.290***
	(0.062)	(0.057)	(0.091)	(0.056)	(0.047)	(0.089)
Premium × Savings	0.066	0.038	0.082	0.052	0.026	0.071
	(0.043)	(0.034)	(0.076)	(0.041)	(0.033)	(0.075)
Premium × Monga	0.107*	0.103*	0.137	0.108**	0.098**	0.140*
	(0.056)	(0.055)	(0.083)	(0.051)	(0.050)	(0.081)
Premium × Post-monga	0.013	0.028	-0.042	0.000	0.002	-0.051
	(0.081)	(0.073)	(0.118)	(0.073)	(0.062)	(0.113)
Market × Savings	0.043	0.014	0.092	0.042	0.003	0.096
	(0.046)	(0.038)	(0.078)	(0.043)	(0.035)	(0.076)
Market × Monga	-0.019	-0.000	-0.032	-0.020	-0.019	-0.024
	(0.063)	(0.061)	(0.092)	(0.057)	(0.054)	(0.087)
Market × Post-monga	-0.088	-0.066	-0.174	-0.099	-0.101	-0.175
	(0.091)	(0.078)	(0.134)	(0.082)	(0.066)	(0.128)
Lagged: log total per capita expenditure	0.505***			0.169***		
	(0.034)			(0.026)		
Lagged:log per capita food expenditure		0.651***			0.011	
		(0.038)			(0.035)	
Lagged:log per capita non-food expenditure			0.244***			0.092***
			(0.026)			(0.021)
Control mean	4.043	3.633	2.831			
Household FE	No	No	No	Yes	Yes	Yes
Obs	3180	3180	3180	3180	3180	3180

Notes: Clustered standard errors at the household level are in parentheses. A-year-lagged outcome, period specific dummies, and treatment status dummies are also controlled. ***, **, and * indicate significance at the 1%, 5%, and 10% significance level.

The estimates reported in Table 7, Columns (1)-(3) are without fixed effects, and Columns (4) -(6) are with household fixed effects. All these estimates show positive and significant impact of having T1 account on food and non-food consumption for the households during *Monga*. However, these positive impacts do not persist in the post-*Monga* period as households may consume most of the savings and the temporary interest reward immediately after they receive it at the end of September or early October.

It is also noteworthy that treatment households do not sacrifice consumption during the savings intervention phase (from June to mid-September), as reflected in the statistically insignificant coefficients during the intervention period for the T1 households. This finding is consistent with the recent evidence from Sri Lanka, where savings have (substantial) impacts without any foregone consumption (Callen et al., 2019). Furthermore, it shows that, in our context, the source of the increased consumption during *Monga* is not the intertemporal reallocation of consumption. Finally, it may also indirectly suggest that overcoming self-control problems, if any, will not be a driver of the observed impacts, although we cannot prove this formally without reliable time-preference parameters.[32]

5.3 Effects on income generation

Since there is no substantial intertemporal substitution in consumption, the next question is how T1 (and T2) households can finance their savings. Following Callen et al. (2019), we examine whether our savings intervention alters households' leisure-consumption choices.

As we do not have detailed data on labor allocation, we focus on total labor income, which is divided into local earnings and remittances recieved from family members. These measures are expressed on a daily per capita basis (BDT). We use the same control variables as in Table 7, except for the lagged outcome. We cannot include this variable because no base-year data are available on this outcome variable of interest. Table 8 presents the estimation results for incomes. Because we only control for period and treatment dummies, estimations with and without fixed effects yield the same results. Thus, we only show the results without fixed effects.

Unlike Callen et al. (2019), where treated households increase labor hours and earnings as the interest rate increases, we find no significant impact on either local earnings or remittances received at any time before, during, or after *Monga*. The lack of significant changes in income may support the view that our sample households face various binding constraints in which they have little choice to increase labor hours or have temporary migration based remittance income suppot — due to low nutrition intake, credit constraints to move to a city, or limited employment opportunities and job networks surrounding them.

5.4 Effects on inter-household transactions

Next, we examine inter-household transactions. To address this, we explore the ITT effects on informal transfers based on the estimation model in Table 7. While we cannot exactly identify the transaction partner due to IRB restrictions, our data allow us to differentiate within- and outside-villagers' informal transactions.

We focus on two main outcomes: i) the total transfer received and ii) the total transfers given to others. The former can be further divided into following two parts: a) transfer received within the village, and (b) transfer received from outside the village. All variables are expressed on a daily per capita basis (BDT). Two issues are notable. First, gift exchanges are uncommon in our data. Thus, most transfers (approximately 99%) take the form of informal borrowing and lending. Second, the vast majority of borrowing from

[32]We also perform heterogeneity analysis, focusing on initial poverty-assessment categories (UP versus MP). We find no evidence of differential impacts for poorer households, presumably because our intervention is not as substantial as Balboni et al. (2022) did in their study.

Table 8: Effects on Income

	(1) Total	(2) Local	(3) Remittance
Premium × Savings	1.518	-1.335	2.852
	(2.650)	(3.125)	(2.397)
Premium × Monga	0.824	0.389	0.436
	(4.017)	(4.142)	(3.581)
Premium × Post-monga	0.746	-0.458	1.204
	(4.160)	(4.779)	(3.053)
Market × Savings	-0.409	-2.030	1.621
	(2.572)	(3.284)	(2.642)
Market × Monga	-4.005	-4.406	0.401
	(3.591)	(4.000)	(4.018)
Market × Post-monga	-4.685	-6.674	1.988
	(3.864)	(4.883)	(3.887)
Control mean	47.140	44.684	2.456
Household FE	No	No	No
Obs	3240	3240	3240

Notes: Clustered standard errors at the household level are in parentheses. Period specific dummies and treatment status dummies are also controlled. ***, **, and * indicate significance at the 1%, 5%, and 10% significance level.

outside the village is from retail shops — where households buy items on credit and pay back later with extra interest. Other personal lending from outside the village is rare.

Examining inter-household transaction trends can shed light on possible spillover effects. If borrowing increases during the savings intervention period and the total amount given increases during the withdrawal period among the T1 (premium interest rate) group, then this may be a signal of spillover effects, as those in the control or T2 groups may ask the T1 households to save on their behalf and share the benefit of premium interest.

Table 9: Effects on Informal Transfers: ANCOVA

	(1) Total Received	(2) Local Recieved	(3) Outside Recieved	(4) Total Given	(5) Total Received	(6) Local Recieved	(7) Outside Recieved
Premium × Savings	-12.421	-0.414	-11.609	0.276	-12.517	-0.418	-11.683
	(9.784)	(0.549)	(9.795)	(0.427)	(9.903)	(0.552)	(9.899)
Premium × Monga	-9.692	-1.433*	-7.860	0.199	-9.849	-1.438*	-7.986
	(15.921)	(0.814)	(15.797)	(1.021)	(16.078)	(0.815)	(15.936)
Premium × Post-monga	0.437	0.608	0.081	-2.266	0.241	0.607	-0.070
	(28.958)	(0.827)	(28.947)	(1.981)	(28.584)	(0.815)	(28.563)
Market × Savings	-5.065	0.220	-4.963	0.036	-5.098	0.261	-5.021
	(10.113)	(0.576)	(10.141)	(0.467)	(10.155)	(0.581)	(10.178)
Market × Monga	-14.840	0.071	-14.487	-0.123	-14.877	0.093	-14.531
	(12.487)	(1.281)	(12.468)	(0.940)	(12.539)	(1.298)	(12.510)
Market × Post-monga	-16.975	-0.072	-16.532	-1.559	-16.813	-0.047	-16.360
	(24.203)	(0.643)	(24.195)	(2.050)	(23.984)	(0.638)	(23.966)
Control mean	26.475	0.613	25.862	0.668			
Household FE	No	No	No	No	Yes	Yes	Yes
Obs	3180	3180	3180	3180	3180	3180	3180

Notes: Clustered standard errors at the household level are in parentheses. A-year-lagged outcome, treatment status dummies, and period dummies are also inclu
and * indicate significance at the 1%, 5%, and 10% significance level.

Table 9 (without fixed effects in Columns (1)-(4) and with fixed effects in Columns (5)-(8)) shows that spillover effects may not be a major concern in our study. We find that the total transfers (both received and given) for T1 households are not statistically significantly different from those of control households throughout the study periods. If anything, we find that T1 households' borrowing within villagers decreases relative to the control households during *Monga*. This is presumably because T1 households can self-finance due to the returns from the premium account. This result contrasts with that of Flory (2018) in Malawi, but is consistent with a field experiment by Kast and Pomeranz

(2014) in Chile; the latter find that giving access to a free formal savings account reduces reliance on informal borrowing while improving the level of consumption smoothing when there is an economic shock to income.

In the follow-up survey, we observe that T1 households are generally reluctant to give saving opportunities to other villagers. This is because they believe that the opportunity is theirs, not their neighbors, and that they may be able to exploit these opportunities later on. We also asked T1 households why they did not increase borrowing from others for savings when the interest of the latter was larger. Most answered that they did not want to be in debt. This direct and indirect evidence suggests that spillover effects may not be an issue in our study.

5.5 Effects on savings

Finally, we examine whether the savings locations change. Although we did not collect data on the amount of savings outside our experimental account in the repeated household surveys, we did so in the follow-up survey in October 2020 to better understand the induced behavioral change. This involves a relatively long-term recall with a significant risk of errors. Therefore, the results should be interpreted with caution. We ask each sample household about the total savings at home and at other financial institutions for each surveyed month in 2019/2020.[33] To examine the changes in the savings amount over time, we estimate the following model:

$$\begin{aligned} ds_{ijm} = &\gamma_0 + \sum_{m=6}^{13} \gamma_{1m} Month_m + \sum_{m=6}^{13} \gamma_{2m} (Month_m \times Premium_i) + \\ &\sum_{m=6}^{13} \gamma_{3m} (Month_m \times Market_i) + \gamma_4 Premium_i + \gamma_5 Market_i + \xi_{ijt}, \end{aligned} \tag{3}$$

where ds_{ijm} is the change in the total savings amount excluding our experimental account for household i in village j in month m; $Month_m$ is a set of month dummies; and γ_{2m} and γ_{3m} are the parameters of interest. The reference group is the control group in May.

Figure 5 presents visual evidence of the intention-to-treat effect for the premium (T1) and market (T2) interest households with 90% confidence intervals. We see an immediate and statistically significant decrease in savings for T1 households in June when our experimental savings account opens. It appears that these treated households effectively shift the savings location from traditional informal saving instruments to more formal secured ones to take advantage of the reward subsidy. However, we do not observe any significant effect thereafter. Moreover, as shown in Panel B of Appendix Table 3, T1 households kept some of their savings at home and do not fully save all their money in our experimental account, even though the upper limit of savings of 4,000 BDT has not been reached.

[33] The descriptive statistics of savings is presented in Appendix Table 5.

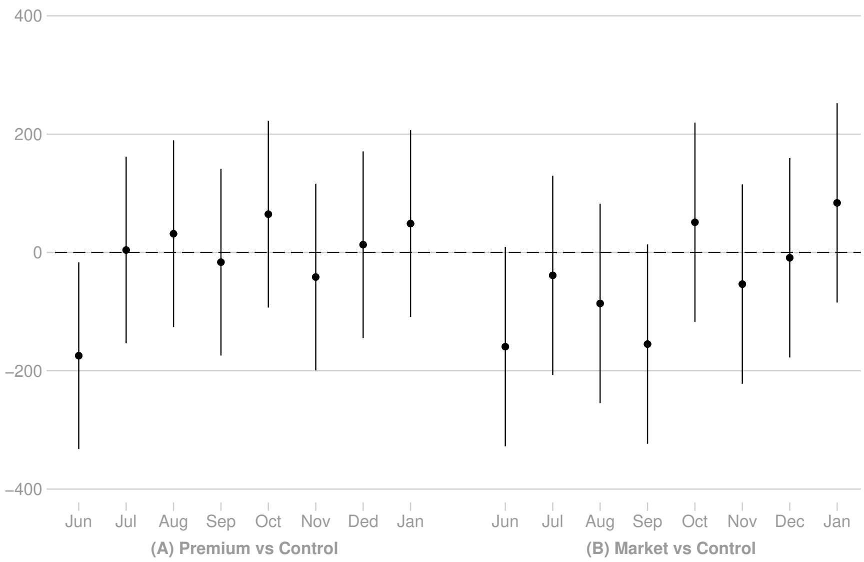

Figure 5: Effects on Savings at Other Places

In the follow-up survey in October 2020, we asked households the primary reasons why they did not save until the upper limit (Panel A of Appendix Table 5) 59% of the T1 group answered not having sufficient money, 28% answered that did not sufficiently trust out partner MFI, and 19% answered that the money could not be withdrawn in a timely manner (because MFI's deposit collector visited households only twice a month). The fact that the poor would value flexibility and liquidity rather than commitment devices are in line with theory proposed by Amador et al. (2006) and empirical findings by Dupas and Robinson (2013a).

In that follow-up survey, no households in the T1 group complained that the interest rate was not attractive, whereas 55% of the T2 group claimed this. Although T2 households could receive the market interest rate, they felt that it was relatively low compared to the premium rate offered to the T1 group.

Together, these results suggest that part of the increased savings in our experimental account is financed by the reallocation of savings within a household across locations. Descriptive evidence also suggests that capacity constraints, the trustworthiness of the savings institution, inflexible and higher trasnaction cost of savings withdrawl partly explain the low intensity of utilization of CS acocunts.

6 Conclusion

Seasonal poverty and hunger are widespread in many parts of the rural developing world owing to a lack of appropriate data. Official statistics tend to measure poverty based on annual estimates of food and non-food expenditure, concealing the seasonal dimension of extreme poverty. It is still unclear that is the most effective way to tackling seasonal poverty.

We explore the severity of seasonal poverty among the ultra-poor using unique bi-weekly data collected over two years in northern Bangladesh. We show that most sampled households suffer year-round extreme poverty and become even poorer during the agricultural lean season. This happens even though households can anticipate income and consumption seasonality in the lean season.

We then study the effect of time-locked commitment savings, targeted for the lean season, with and without encouragement for savings subsidy through the premium interest rate. We find households that offered temporary premium interest rate improved food and non-food consumption expenditure by approximately 9 and 13%, respectively, when food starvation is acute.

From a policy perspective, our findings indicate that providing a commitment saving scheme with a savings subsidy can potentially improve consumption of the ultra-poor, at least temporarily, during the hungry season. Our intervention does not require large initial funds relative to other cash and in-kind transfer programs. It can also be easily scalable using the existing grass-root organizations such as NGOs, and MFIs.

Barring administrative expenses, our experiment costs about 3.5 USD for the premium CS households, given the average savings amount. To put this into perspective, this 3.5 USD savings subsidy increased food and non-food expenditure by 9 and 13% compared to Bryan et al. (2014) 11.50 USD migration subsidy program that improved food consumption by 8.5% and non-food consumption by 12% [34]. However, the administrative cost for account opening, maintenance, and door-to-door savings collection can be sizable when scaled up. However, given the recent development of mobile money, CS accounts can potentially be operated under the Digital Financial Service platform with savings reminders through SMS and Interactive voice response — which show promising results in Ghana (Riley and Shonchoy, 2022).

As Khandker and Samad (2016) and others note, perhaps there is no single silver bullet for the ultra-poor against seasonal hunger. Instead, a comprehensive policy may be necessary to remove the underlying root causes of extreme poverty effectively — to help the poor cope with seasonal hunger by enhancing their income-generating capacity through, for example, enhanced spatial job connections or skill improvements (Bryan et al., 2014; Shonchoy et al., 2018; Balboni et al., 2022). Although this may appear as a policy that applies to all, small pushes may only work to elevate consumption without releasing people from poverty traps. In this regard, the efficacy of BRAC's "big push" approach with proper risk management with a design feature for addressing the seasonal dimension of poverty may provide an interesting scope for future research. In addition, considering that the ultra-poor consist of elderly-, disabled-, and female-headed households who may not be capable of actively working in local areas or through out-migration, the relevance of well-targeted cash transfers will also require rigorous research in the future.

[34]See Table III of the Bryan et al. (2014)

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Table A1: Sample Size at Each Month

Month	First year 2018-2019	Second year 2019-2020
April	361	361
May	360	361
June	293	339
July	417	199
August	346	360
September	363	320
October	324	400
November	379	298
December	276	422
January	120	180

Table A2: Balance Check of the Base Year Variables

Variable	(1) Control Mean/SE	(2) Premium Mean/SE	(3) Market Mean/SE	(1)-(2)	T-test P-value (1)-(3)	(
Household size	4.019	3.986	4.037	0.895	0.945	0
	(0.180)	(0.165)	(0.200)			
Head is male (dummy)	0.907	0.917	0.963	0.857	0.245	0
	(0.040)	(0.033)	(0.026)			
Head's age (years)	43.185	42.097	45.833	0.605	0.272	0
	(1.510)	(1.420)	(1.863)			
Head's education (years)	2.037	2.278	1.259	0.703	0.195	0
	(0.490)	(0.404)	(0.341)			
Temporary worker: Agriculture, Construction etc (dummy)	0.741	0.764	0.796	0.768	0.498	0
	(0.060)	(0.050)	(0.055)			
Transportation worker (dummy)	0.074	0.056	0.056	0.676	0.699	1
	(0.036)	(0.027)	(0.031)			
Trader (dummy)	0.074	0.042	0.037	0.436	0.406	0
	(0.036)	(0.024)	(0.026)			
Own any agricultural land (=1)	0.315	0.278	0.222	0.655	0.282	0
	(0.064)	(0.053)	(0.057)			
Land size (hectare)	0.444	0.392	0.323	0.674	0.365	0
	(0.099)	(0.076)	(0.089)			
Number: cattle	0.759	0.681	1.037	0.623	0.155	0.
	(0.127)	(0.101)	(0.147)			
Number: goat	0.741	0.389	0.481	0.076*	0.223	0
	(0.161)	(0.120)	(0.137)			
Value of other productive asset (BDT)	964.000	2477.778	651.667	0.309	0.295	0
	(276.800)	(1264.387)	(107.199)			
Daily income (BDT)	53.767	62.291	60.070	0.256	0.398	0
	(5.629)	(4.894)	(4.848)			
Wage income	0.775	0.812	0.813	0.557	0.578	0
	(0.049)	(0.040)	(0.046)			
Crop income	0.004	0.010	-0.015	0.335	0.421	0
	(0.003)	(0.005)	(0.023)			
Livestock income	0.004	0.000	0.000	0.250	0.340	0
	(0.004)	(0.000)	(0.000)			
Non-farm self-employment	0.145	0.107	0.165	0.495	0.795	0
	(0.046)	(0.035)	(0.058)			
Other income	0.072	0.072	0.038	0.993	0.091*	0
	(0.019)	(0.018)	(0.005)			
Log real per capita expenditure in base year	4.004	4.004	3.970	0.994	0.521	0
	(0.040)	(0.032)	(0.034)			
Log real per capita food expenditure in base year	3.663	3.676	3.636	0.796	0.595	0
	(0.038)	(0.033)	(0.034)			
Log real per capita non-food expenditure in base year	2.621	2.596	2.571	0.672	0.421	0
	(0.048)	(0.035)	(0.038)			
N	54	72	54			
F-test of joint significance (F-stat)				0.726	1.144	
F-test, number of observations				126	108	

Notes: The value displayed for t-tests are p-values. The value displayed for F-tests are the F-statistics. All missing values in balance variables are treated as zero.***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A3: Savings amount at home and reasons not for using the experimental account

	Premium	Market	Control
Panel A: Reasons for not savings (%)			
Don't have sufficient money to save	58.9	31.3	
Don't trust GRDF	27.9	21.6	
Cannot withdraw immediately	19.1	11.8	
Want to save in better institutions	0.0	0.0	
Want to keep money at home	0.1	0.8	
Interest rate is not attractive	0.0	54.9	
Strong pressure from peers to share interest	0.0	0.0	
Other	0.0	0.0	
Panel B: Savings amount in April 2019 - January 2020			
April	559.6	541.8	617.1
May	524.6	539.4	541.0
June	374.7	437.5	524.5
July	320.0	372.5	424.4
August	396.9	364.3	428.6
September	449.3	310.8	456.1
October	521.4	402.0	422.4
November	577.2	478.4	478.6
December	629.4	541.2	476.5
January	624.3	603.7	381.4
Obs	68	51	49

Notes: Multiple answers are allowed.