Impact of Flexible Microcredit on Repayment and Food Consumption: Experimental Evidence from Rural Bangladesh [#]

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Abstract:

The mismatch between credit repayments and income seasonality implies a challenge for microfinance institutions (MFIs) working in developing countries. For instance in northern Bangladesh, income and consumption downfalls during the lean season after the transplanting of major paddy crops are a serious threat to the household economy. Poor landless agricultural wage laborers suffer the most due to this seasonality as they face difficulty to smooth their consumption. In designing microcredit products, MFIs do not usually provide any flexibility or seasonal adjustment during the lean season, however. This is mainly because MFIs are afraid of the possibility that such flexibility might break the repayment discipline of borrowers, resulting in higher default rates. We thus conducted a randomized controlled trial in 2011-12 in northern Bangladesh to test empirically whether flexible microcredit leads to an increase in repayment problems for MFIs and whether it can increase and stabilize consumption of borrower households. Our results suggest no statistically discernible difference among the treatment arms in case of default, overdue amount, or repayment frequency. This is in favor of flexible design of microcredit. On the other hand, we find no positive impact of the repayment flexibility on food consumption, either. This could be due to the possibility that the main problem for the ultrapoor is consumption smoothing between the lean and non-lean seasons, the insignificant difference in income changes across the credit schemes studied, or the treated households' perception of the transient nature of the intervention.

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1. Introduction

Given the current global move to fight poverty and hunger, it is important to understand the seasonal dimension of the poverty and hunger nexus, which affects the poor of developing countries regularly and repeatedly. Agriculture-dependent rural poverty can be linked to such distinct crop-cycle-based seasonality, and it becomes more severe when coupled with adverse seasonal climatic conditions that could lead to poor-quality harvests or outright crop failure (Chambers et al. 1981). Moreover, inadequate access to formal credit and insurance products further traps people in chronic and inter-generational poverty—poverty that is very difficult to tackle through the use of general public policy measures and social safety net approaches.

For example, in Bangladesh, the term "seasonality" is associated with a seasonal food deprivation phenomenon known locally as *monga*; it is mostly common in northern Bangladesh (Khandker and Mahmud 2012). Rural life in Bangladesh revolves around the agricultural cycle, which is characterized by three crop seasons that are in turn based on three categories of rice: *aus* (April to August), *aman* (July/August to November/December; traditionally the most important paddy crop), and *boro* (December/January to April). As a consequence of this cycle, two major seasonal deficits occur: one from late September to early November, and the other from late March to early May. With the widespread expansion of *boro* cultivation in recent years, the incidence of the lean period in March–May has significantly declined. However, the lean season in September–November that follows the transplantation of the *aman* crop still affects most parts of the country, and especially the northwest part of Bangladesh (Khandker and Mahmud 2012). Almost no alternative agricultural activity takes place in that period, and the nonagricultural sector cannot sufficiently absorb the seasonally unemployed labor.

During *monga*, drastic drops in employment-led income constitute the major reason behind reduced food consumption; this has been well documented in the literature (e.g., Rahman and Hossain 1995). Such a lack of income and alternative means for earnings limit the purchasing power of the people, and this situation cannot be mitigated with the minuscule amounts of assets and savings that poor households typically carry. Anecdotal evidence suggests that, on average, the number of meals consumed is significantly reduced during *monga*, and that the families of young and elderly members suffer the most. The absence of a functional credit market obstructs households from smoothing their consumption (Pitt and Khandker 2002). As a result, many individuals borrow from landlords or informal money lenders—both of which tend to charge very high interest rates—and they subsequently fall into a debt trap.

Given this status quo, various coping strategies have emerged among the *monga*-affected people of northern Bangladesh. Other than borrowing from informal sources that charge high interest rates, coping strategies common among them include advance sales of labor (Khandker and Mahmud 2012), the purchase of household essentials on credit, skipping meals during the lean season (Berg and Emran 2011), and seasonal migration (Shonchoy 2011). Of these coping strategies, temporary seasonal migration to urban areas appears to be a relatively practical and rational strategy, as individuals can move from rural areas to nearby urban areas or cities for a short period of time, in an attempt to earn a livelihood during the lean season. However, such a migration strategy is not suitable for everyone, due to family constraints (especially among households with female heads or disabled heads that may not be able to migrate during the lean season); additionally, credit and financing constraints, a lack of networking, and asymmetric information problems limit individuals' ability to migrate (Bryan et al. 2012).

One recent policy development in developing countries has been the emergence of microfinance institutions (MFIs) that focus on poverty alleviation. It is argued that, given access to even small amounts of credit, entrepreneurs from poor households will find opportunities to engage in viable income-generating activities (IGA)—many of which will be secondary to their primary occupations—and thus ameliorate poverty on their own. According to the Microcredit Summit Campaign, as of December 2007, MFIs had 154,825,825 clients; of these, more than 100 million were women. In 2006, Mohammad Yunus and the Grameen Bank were awarded the Nobel Prize for Peace, for their contributions to poverty reduction, especially in Bangladesh. However, among academics, there is thus far no consensus on the impact of microcredit on income improvement and poverty reduction (Banerjee et al. 2009). On one hand, various studies on the impact of microcredit in developing countries have found evidence of consumption-smoothing, asset-building (Pitt and Khandker 1998), and poverty reduction (Khandker 2005). Conversely, using the same dataset of Pitt and Khandker (1998), Morduch (1999) found that the average impact of microfinance is "nonexistent."

A major drawback of the microcredit framework is its rigid loan repayment rules (Karlan and Mullainathan 2007). Nearly all loan contracts are fixed in their repayment schedules, which involve equal weekly payments, along with a high interest rate. However, MFIs work with poor rural people who most often have uncertain and infrequent incomes, and these circumstances make it very difficult for them to maintain such rigid weekly loan repayments. Especially during the lean period—when there are no jobs available in the rural agricultural sector—it can be very difficult for the poor to generate income, let alone comply with a loan repayment scheme; indeed, to say that rigid weekly repayments during the time of seasonal hardship exacerbates their misery is an understatement. It was found that during *monga*, households take extreme measures—like selling productive assets (Khandker and Mahmud 2012) or borrowing from loan sharks who charge extraordinarily high interest rates—in order to maintain a clean record of repayment and be assured access to future microcredit loans from MFIs.

Using primary data from rural households in Bangladesh, Shonchoy (2009, 2011) shows that during the lean season, access to microcredit does not increase the income levels of individuals, compared to those with no access to credit, ceteris paribus. Additionally, Shonchoy (2009, 2011) at the time of survey found no MFI that operates any well-targeted microfinance program solely dedicated to tackling seasonality issues such as *monga*. Given that seasonality in northern Bangladesh is historically well known, it is particularly puzzling to find that no leading microcredit product—save for PRIME intervention by PKSF[1]—has been designed to mitigate the effects of seasonality by providing some form of moratorium of loan repayment during *monga*.

The mismatch between credit repayments and income can create serious distortions that, for some people, deepen the debt trap, especially if they take extreme measures to repay loans on a weekly basis during the lean period. In this study, we examine whether these distortions are inevitable. If MFIs could allow some flexibility in the microcredit repayment schedules in periods of uncertain income during lean periods, this may improve the livelihood of the poor, provide them with greater flexibility and mobility, and in turn improve their capacity to repay the loan. Currently, MFIs are reluctant to relax their loan repayment rules; it seems that they fear that allowing people a moratorium on a weekly repayment scheme during the lean period may adversely affect their debt repayment discipline. It is possible that that borrowers, if they are given seasonal adjustment in repayment, could become behaviorally accustomed to making lower or no repayments when those payments are nonetheless required, ultimately leading to lower recovery rates or even higher default rates.

Given this trade-off, it appears that an appropriate way of addressing these issues is the introduction of a field experiment that features a randomized controlled trial (RCT). A large number of RCT studies have been undertaken in microfinance-related research; such research covers a wide range of subjects, including the impact of microfinance (Banerjee et al. 2009), weekly versus monthly repayment (Field and Pande 2008), group versus individual liability (Giné and Karlan 2011), random variations in meeting frequency (Feigenberg et al. 2011), and variance in a loan's term structure (Field et al. 2012), to name a few.

Despite this potential, rigorous evaluation of the impact of such flexibility in microcredit design is lacking in the literature. Among the few existing studies, Shoji (2010) evaluates the effectiveness of Bangladeshi microfinance in introducing a contingent repayment system, beginning in 2002; this system allowed for the rescheduling of savings and installments for

[1] PRIME (Programmed Initiatives for Monga Eradication) was introduced in 2006 by PKSF (Palli Karma–Sahayak Foundation), a microcredit wholesaler and umbrella organization in Bangladesh. Under the PRIME scheme, individual nongovernment organizations (NGOs) receive credit facilities that have "flexible" terms—under which those NGOs are free to negotiate the credit amount, repayment schedule, and frequency of meetings with the beneficiary, and impose completely different sets of schemes with various borrowing groups. While this is ideal for beneficiaries to some extent, it is not easy to evaluate flexibility in terms that improve the accessibility of beneficiaries to microfinance, performance in IGA, or the livelihoods of their families.

affected members during times of natural disaster. Using evidence pertaining to flooding in 2004 and based on an instrumental variable approach, Shoji found that rescheduling played the role of a safety net by substantially decreasing the probability that borrowers would skip meals in response to negative shocks; the effect was even more pronounced on the landless and women. Furthermore, if we restrict our attention to studies in the context of *monga*-related seasonal deprivation in northern Bangladesh, we find there to be a similar dearth of qualitative research. Khandker and Mahmud (2012) analyze the correlates of seasonal deprivation while focusing on social protection programs and microcredit, using nonexperimental data. In India, the neighboring country of Bangladesh, Czura et al. (2011) examine the impact of repayment flexibility by undertaking a randomized experiment with dairy farmers; they show that repayment flexibility contributed to consumption-smoothing and also enhanced demand for credit. With the exception of this study by Czura et al. (2011), we are unaware of any rigorous study on the impact of repayment flexibility in South Asia based on an RCT design.

We thus initiated RCT experiments in northern Bangladesh in early 2011. The aim of this study is to elucidate the mismatch between seasonality and the terms of microcredit, and to understand the impact of seasonality-adjusted microcredit. In our RCT design, our counterpart NGO first formed typical microfinance groups from randomly chosen villages. Borrowers were then provided with credit and began making weekly repayments after a short, two-week grace period. For a random subsample of these borrower groups, the repayment schedule was relaxed in two ways during the designated *monga* period. Under the first treatment, the borrower was temporarily given a moratorium, while under the second flexibility treatment, the repayment scheme was changed into a monthly repayment.

We surveyed 1,440 households belonging to the borrower groups both before (baseline) and after one year of intervention (endline). We also executed a short *monga* survey during the time of *monga* in 2011, to understand the severity of the seasonal conditions. Making use of both survey and experimental methods, we empirically analyze the impact of the flexibility schemes on repayment and consumption. As a preview of the results, we find no statistically discernible difference among the treatment arms in case of default, overdue amount, or repayment frequency, while we find no positive impact of the repayment flexibility on food consumption, either. We believe that our study contributes a new insight on the consequences of flexible microcredit that is both geographically and seasonally adjusted to help the vulnerable and lean season-affected poor cope better with periods of hardship.

The rest of the paper is organized as follows. Section 2 describes our RCT design and field surveys. Section 3 investigates the impact of the repayment flexibility on repayment behavior of borrowers, while Section 4 investigates its impact on consumption of borrower households. Section 5 concludes the chapter.

2. Experimental Design for Flexible Microcredit Trials

2.1 RCT Strategy

(1) Inflexible Microcredit as the Control

A typical Grameen-style microcredit scheme proceeds as follows (Armendariz and Morduch 2010). Persons eligible for microcredit first form a group wherein its members are expected to help each other in times of difficulty. Not all members can borrow immediately. It is usually the case that only some of them are offered credit after all members have saved a small amount of money on a regular basis; the rest of them are given credit after the first borrowers successfully repay several installments and all members have continued to save the same small amount on a regular basis. Weekly repayments begin without a long grace period. With typical Grameen-type microcredit, the first lent amount is small, and it is to be repaid in 50 weekly installments within a 12-month period.

Several rationales have been offered for this rigidly designed repayment schedule (Armendariz and Morduch 2010). The success of frequent repayment in minimizing default and delay could be attributed to the early warning mechanism, the lender's capture of information vis-à-vis the income flow of the borrower, and the borrower's commitment to save regularly. Repayment in group meetings in front of others also drives regular repayment by those borrowers who would like to maintain their reputation within the village.

Probably on account of these mechanisms, classic Grameen-type microcredit has been successful in maintaining high repayment rates.[2] However, attending weekly meetings regularly puts a high burden on the borrowers in terms of the opportunity costs of their time. Relaxing several of the classic Grameen-type features is thus being demanded from borrowers. Academic research has responded to this request, to identify the key element that was the most critically important in guaranteeing high repayment rates. For example, using a field experiment approach, Giné and Karlan (2011) evaluate the impact of removing group liability in the Philippines; they find there was no adverse impact on repayment, as long as public and frequent repayment systems were maintained. On the other hand, recent studies comparing weekly versus monthly installments and based on RCT designs show mixed results: in India, Field and Pande (2008) show no differences between microfinance schemes with weekly and monthly repayment frequencies, as long as repayments were made in public meetings, while in Indonesia, Feigenberg et al. (2011) find that repayment performance was better when repayments were collected weekly rather than monthly.

Given this background, we adopted the following borrowing and repayment scheme as the

[2] See Kurosaki and Khan (2012) for an exceptional case where an MFI suffered from high default rates, despite adopting a Grameen-type credit scheme. In their case, due to weak enforcement of the contingent renewal rule, strategic default prevailed among borrowers.

control. Borrowers obtain credit of BDT 3,000[3] and begin repayment after a short, two-week grace period. Repayments are made in 45 installments, each of which is BDT 75, implying a gross interest payment of BDT 360 that is spread throughout the borrowing period of approximately one year. Each of the weekly installments is to be repaid by the borrower at a weekly meeting. The borrower is obliged to attend the weekly meeting, even during the *monga* period. This design of a traditional or inflexible microcredit scheme is denoted as the "Control."

(2) Flexible Microcredit as the Treatment

During the *monga* period, microcredit borrowers may face difficulties in preparing the money needed for regular repayment. To facilitate the demand for repayment flexibility within this context, the treatment relaxes the repayment schedule in two ways during the *monga* period, which for this purpose is designated as September 20–December 20.

Under the first treatment, "Flexible 1," a moratorium is temporarily applied to repayments during the designated *monga* period. During that moratorium, households within the Flexible 1 groups do not pay any installment. After the *monga* period, the borrowers begin to pay BDT 100 per week, so that their total repayment amount and repayment period would be identical to those of the Control group.

As a variant of the first treatment, one-third of those treated with Flexible 1 are also given income generation activities (IGA) support. We refer to this treatment as "Flexible 1 + IGA." Under IGA support, instead of providing cash, we provide microcredit borrowers with a productive asset of their choice, within the credit amount, along with advice for utilizing the asset; no further subsidy is provided.

Under the second flexibility treatment, the repayment schedule is changed to feature two monthly installments of BDT 300 each during the designated *monga* period. After the *monga* period, borrowers resume paying BDT 75 per week, so that their total repayment amount and repayment period would be the same as those of the Control group. We refer to this treatment as "Flexible 2."

(3) Randomization of Treatment Arms

To preclude unequal treatment among members within a group, we randomized the four treatment statuses at the borrower-group level. Since our counterpart NGO usually forms one group in one village, our randomization took place at the village level.

Of the list of 90 villages that were under potential treatment by the counterpart NGO, we randomly selected 12 villages for "Control," 24 for "Flexible 1," 12 for "Flexible 1 + IGA," and

[3] BDT 100 is equivalent to approximately JPY 99 or USD 1.22. BDT 3,000 therefore equals approximately USD 37.

24 for “Flexible 2.” In the randomization, we stratified the villages based on their distance from the closest bus station and the location type of the village (see the next subsection).

The reason for the larger number of villages under “Flexible 1” and “Flexible 2” than under “Flexible 1 + IGA” and “Control” was that our initial design had another experiment dimension, distinguished by the timing of when the borrower groups would be delivered the information that the repayment schedule would be relaxed. The intention was to create exogenous variation in the information structure, as implemented by Karlan and Zinman (2009) in the context of consumer credit in South Africa. However, due to delays in group formation and loan disbursement, the exact timing of the announcement became similar across all groupings. Therefore, in analyzing the impact of our experiment, we eventually merged the two types of treatments (previously “surprise” and “preannounced flexibility”).

In each village, our counterpart NGO formed a borrower group known as *samity*, which comprised 20 members who satisfied the NGO’s microcredit criteria and had voiced an interest in receiving microcredit. The member names were then recorded in the *samity* formation book by the loan officers. In the book, each *samity* member was assigned a number in ascending order; the members who happened to hold numbers 1–15 were to be offered credit, while those holding numbers 16–20 were kept in the group as observers. This randomization implies the following sample distribution: there are 72 sample villages and 1,440 sample households, one-sixth or one-third of which falls into one of the four treatment arm categories; three-fourths of the sample households (1,080 households) were actual borrowers of microcredit.

2.2 Implementation of Surveys and RCT Interventions

(1) Counterpart NGO and Study Area

Our counterpart NGO is Gono Unnayan Kendra (GUK), which operates in the greater Gaibandha area, comprising five districts in northern Bangladesh: Gaibandha, Kurigram, Rangpur, Lalmonirhat, and Nilphamari. It has offices in all 32 *upazillas* (subdistricts) in Gaibandha district and five offices in the Kurigram district. Prior to this study, GUK had had limited experience in running traditional microfinance; on the other hand, it had already been a promoter of flexible microfinance in combination with its reportedly successful “asset transfer” program, which was financed by international donors. However, since its asset transfer program contains a large subsidy component, it is not clear how much of its success vis-à-vis outreach to the ultrapoor can be attributed to the flexibility in their repayment design *per se*. For instance, under one of GUK’s programs, ultrapoor beneficiaries were provided with a livestock animal and required to return the offspring or an equivalent monetary value. This design also implies a much longer grace period than traditional microcredit.

In the study area, poverty is concentrated in so-called *char* areas. *Char* literally means "river island," and it is an area of land regularly formed from river bed sediment that has been eroded by the major rivers of Bangladesh. People living on *char* islands tend to be poorer and more vulnerable to various types of natural disasters (Khandeker and Mahmud 2012). For this reason, in our experiments, we distinguished the *char*, river basin, and inland areas where our target group—i.e., the poor and vulnerable—live. More concretely, in the randomization, we stratified villages based on the distance from the closest bus station, and on the village location types (*char*, river basin, or inland). The distribution of our final sample villages is shown in Table 1. Forty-five of the 72 sample villages (62.5% of the sample) were in Gaibandha district; the rest (37.5%) were in the Kurigram district. Eighteen of the 72 sample villages (25.0% of the sample) were in *char* areas, 42 villages (58.3%) were in inland areas, and the remaining 12 villages (16.7%) were in river basin areas.

(2) Schedule of Surveys and Experiments in the Field

Figure 1 shows the timeline of our surveys and experiments. In the first half of 2011, we visited Gaibandha and GUK to undertake preparatory investigations and make logistical arrangements. Following our agreement with GUK regarding the research design, village-level randomization was implemented, followed by the formation of *samity*. The benchmark survey (Panel 1) of 1,440 households was executed in July–August 2011; it captured detailed information on the household roster; education; health, including the weights of the children; occupation; assets; income; migration experiences; agricultural production; nonagricultural enterprises; saving; credit; debt; *monga* coping; and the like.

In the first three weeks of September 2011, microcredit in the amount of BDT 3,000 was issued to each of three-fourths of our sample households. Our initial plan was to issue the microcredit earlier. However, due to the holy month of Ramazan and the subsequent festival of Eid-ul-Fitr, the disbursement was delayed. As a result, those households who were given flexible microcredit entered the designated *monga* period before the due date of their first repayment installment. Nevertheless, GUK was able to collect monthly installments (Flexible 2) and larger weekly installments in the post-*monga* period (Flexible 1), without experiencing serious delays or nonrepayment problems. Another small deviation from our initial design was that in several villages, the number of *samity* members who were issued credit was not exactly 15 (i.e., three-fourths of the *samity* members). As the deviation cancelled out each other, the initial design of giving credit to three-fourths of the sample households was achieved.[4]

After the RCT experiments began, two more surveys were executed: the first *monga* survey (Panel 2) in November 2011, and the follow-up survey (Panel 3) in July–August 2012. Panel 1

[4] More precisely, the distribution was as follows: 12 borrowers = one village, 13 borrowers = two villages, 14 borrowers = eight villages, 15 borrowers = 47 villages, 16 borrowers = 13 villages, and 17 borrowers = one village.

(the benchmark survey) and Panel 3 were based on the long questionnaire, which covers all aspects of the household economy; Panel 2, meanwhile, was based on the short questionnaire, which focused on how the household was coping with ongoing *monga* difficulties. Panel 1 was meant to capture the state of affairs *before* our interventions, Panel 2 describes the household economy *during* our interventions, and Panel 3 was designed to collect information *after* our RCT experiments. In Panels 1 and 2, 1,440 households were surveyed. In Panel 3, 1,422 of the initial 1,440 households were resurveyed, implying an attrition rate of 1.25%.

In addition to these surveys, administrative data for all borrowers (i.e., 1,080 borrowers) were obtained from GUK. This dataset provides us with detailed and precise information on repayment behavior.

The distribution of our final sample households is shown in Table 1. Data for the full set of 1,440 household observations surveyed in Panel 1 are utilized as the benchmark information. Data for the subset of 1,080 borrowers are utilized in Section 3 in which the impact of flexibility on repayment behavior is investigated. Data for the subset of 1,422 Panel-3 households are utilized in Section 4 in which the impact of flexibility on food consumption is investigated.

2.3 Validity of Randomization

As our randomization was implemented properly, we expect to observe no systematic difference in pre-intervention characteristics at the village level across the various treatment arms. To test this expectation, we estimated the following village-level regression model, using the benchmark survey data:

$$X_v = b_0 + b_1 D_{1v} + b_2 D_{2v} + b_3 D_{3v} + u_v, \qquad (1)$$

where X_v is a pre-intervention variable for village v, D_{jv} is a dummy variable for treatment j (j = 1, 2, 3; i.e., Flexible 1, Flexible 1 + IGA, and Flexible 2, respectively), and u_v is a zero mean error term. If the null hypothesis that $b_1 = b_2 = b_3 = 0$ is not rejected, the balance test is passed.

Similarly, we expect to observe no systematic difference in pre-intervention characteristics at the household level across the four treatment arms, either.[5] To test this, we estimated the following household-level regression model, using the benchmark survey data:

$$X_h = b_0 + b_1 D_{1h} + b_2 D_{2h} + b_3 D_{3h} + b_4 D_{4h} + u_h, \qquad (2)$$

[5] It might be possible for a difference to occur at the household level across treatment arms, as treatments had been randomized at the village level. For example, Czura et al. (2011) state that "Differences in client characteristics are due to the fact that randomization occurred at the group level and groups form according to socioeconomic characteristics" (p.10).

where X_h is a pre-intervention variable for household h, D_{jh} (j = 1, 2, 3) is a dummy variable indicating that household h was provided with flexible microcredit under treatment arm j (j = 1, 2, 3; i.e., Flexible 1, Flexible 1 + IGA, and Flexible 2, respectively), D_{4h} is a dummy for nonborrower households, and u_h is a zero mean error term. If the null hypothesis that $b_1 = b_2 = b_3 = 0$ is not rejected, the balance test is passed. If there was no selection bias in assigning borrower vs. nonborrower households within each *samity*, we expect b_4 to be zero as well. Because the randomization had been implemented at the village level and sample households were drawn using the village as the primary sampling unit, we used robust standard errors for b's clustered at the village level, in order to test the null hypotheses using equation (2).

Appendix Table 1 shows the results for village-level variables. At the village level, the distance from the closest bus station to the village, the dummy for a *char* village, and the dummy for an inland village were perfectly orthogonal to the treatment, confirming our randomization strategy. For all six variables that represent village-level public facilities (bazar, college, Hindu temple, town, bus stand,[6] and railway station), the null hypothesis that $b_1 = b_2 = b_3 = 0$ was not rejected at the 5% level. In this sense, the balance test at the village level was passed, suggesting that our randomization strategy at the village level had been implemented properly. Nevertheless, the null hypothesis was rejected at the 10% level for the case of Hindu temples, and the individual coefficient on D_{2v} was significant at the 5% level for the case of distance to the nearest town. As we had randomized the treatment status, we assessed them as having occurred by chance. As will be shown in Section 4, these nonrandom components do not affect our impact analysis; see the results of the robustness check, undertaken by controlling for these benchmark village-level variables.

Appendix Table 2 shows the regression results for household-level variables using four variables characterizing the household head, six variables characterizing household members, five variables characterizing land holdings, and five variables characterizing liquid asset ownership. All of these variables were compiled from the benchmark survey data.[7] Of the 20 variables analyzed in Appendix Table 2, in only one case (i.e., the ratio of adults in the household roster) was the null hypothesis that $b_1 = b_2 = b_3 = 0$ rejected at the 5% level. If we individually assess the significance of b_1, b_2, and b_3, again, only one of them (i.e., b_3 for the ratio of adults in the household roster) was statistically significant at the 5% level. We can therefore safely conclude that these rejections occurred by chance and that randomization had been properly implemented. As will be shown in Section 4, the nonrandom components at the

[6] The "bus stand" here refers to any bus stand, while the "bus station" used in our randomization strata refers to a larger bus stand where medium- and long- distance bus services are available.

[7] To be more precise, due to data entry problems, we used Panel 3 data for the household demography variables (age was adjusted by one year), supplemented by Panel 1 data for the 22 attrition households. For land and assets, we used Panel 1 data.

household level do not affect our impact analysis (see the robustness check undertaken by controlling for these benchmark household-level variables).

2.4 Summary of the Experimental and Survey Design

This section explained the experimental design of our RCT in northern Bangladesh, which had been undertaken to examine the impact of flexible microcredit that targets the ultrapoor. After describing our experimental design, this section also compared the means of sample villages' and households' characteristics across the various treatment arms. It was found that most of the observable characteristics prior to our intervention were very similar across the treatment arms, indicating that randomization had been implemented properly. Means of the benchmark survey data also showed that our sample households owned very few liquid assets (such as household appliances or livestock) and managed very small land holdings. These findings indicate that our sample households belong to the poorest section of rural Bangladesh.

3. Impact of Flexibility on Repayment Behavior

In this section, we examine repayment behavior to test whether seasonal adjustment in microcredit affects the default rate and repayment delays. Through this examination, we assess the general claims by the NGOs vis-à-vis a moratorium during *monga*.

3.1 Extent of Default and Absence in Weekly Meetings

(1) Definition and Summary Statistics of Empirical Variables

We compiled two sets of empirical variables that characterize the extent of repayment problems. Table 2 shows definitions and summary statistics of these variables.

The first set of empirical variables is based on the information on a borrower's payment due at the end of a loan cycle. The first variable, *default,* is defined as a dummy variable taking the value of 1 if the overdue amount was positive, and 0 otherwise. On average, 25% of borrowers had a positive overdue amount at the end of the loan cycle. The second variable, *due_amount,* is a continuous variable for the absolute amount of delinquency. Its mean was BDT 155. We can convert this number into a relative number by dividing it by the total due amount (BDT 75 times 45 equals BDT 3,375). On average, the overdue amount was equivalent to 4.6% of their required, accumulated amount due at the end of the loan cycle. Therefore, although the incidence of default was frequent, the overdue amount was small in both absolute and relative terms on average.

The second set of empirical variables is based on the information on the number of weekly meetings missed by borrowers. MFIs typically impose a strict loan collection regime, where each borrower must pay weekly loan installments of equal amounts. However, in our experimental design, we instructed GUK not to impose any strict loan repayment discipline. Instead, we instructed GUK to conduct household visits each week, hold weekly meetings, and inform each borrower of the cumulative amount due. This was done, in particular, to observe the loan collection pattern and behavior of loan repayment among borrowers. GUK also accepted advance payments and thus gave extra credit for scheduled weekly repayment dues; GUK refers to this as the "balance carried forward." The first variable, *num_miss1*, is defined as the number of total missed weeks with the "balance carried forward" option. In this definition, "missed weeks" considers only those cases where the borrowers did not pay at all[8] and had not earned any credit toward one or more missed weeks of payments. On average, borrowers missed payments by 5.4 weeks under this definition. Similarly, when we calculate the gross missed weeks without the "balance carried forward" option, borrowers missed payments by 6.4 weeks on average. The average ratio of total missed weeks to the total loan collection weeks (variable *num_ratio*) was 0.17, or 17%. Therefore, although the overdue amount was small on average, borrowers missed meetings quite frequently at the average rate of one in six.

As discussed in Section 2, our experimental design used as randomizations strata three distinct geographical properties: the *char*, river basin, and inland areas. Across three regions, borrowers in *char* areas had more difficulty in repayment than those in other two areas if we focus on two variables defined on the overdue amount at the end of a loan cycle (*default*, and *due_amount*). On the other hand, borrowers in river-basin areas had more difficulty in repayment than those in other two areas if we focus on three variables defined on missed weeks of repayment (*num_miss1*, *num_miss2*, and *num_ratio*). As *char* households typically face greater difficulties in ensuring a regular flow of income, and they recurrently suffer on account of seasonal adversity, we expected that *char* households had more difficulty in regular repayment than other households. This expectation was met only regarding the overdue amount.

(2) Seasonality

One important aspect of this loan repayment analysis is understanding the impact of seasonality on total collection and weekly repayment. To examine any pattern of seasonality, Figure 2 plots monthly loan collection and missed weeks information.

Most of the underpayments occurred in the off-harvest periods (e.g., September–October and March–April). This reflects the income-smoothing problem faced by borrowers during these months. However, the drop in the repayment ratio during these months was not very large in magnitude. In contrast, months of December–January and May–June were associated with higher repayment on average. In December, overpayment was recorded on average. This seasonality pattern was found in all three regions of *char*, inland, and river basin.

[8] Any partial payment would not result in missed weeks.

To understand the discipline framework imposed by the MFIs, seasonality in the number of weekly meetings missed is informative. As shown in Figure 2, borrowers tended to miss more weekly payments as they reached the end of the loan cycle, compared to the beginning of the loan collection period. One interesting observation to note is that the ratio of missed weeks to the total monthly due weeks was lower in November–December and in May, which could be attributed to the paddy harvest cycle, as previously observed. An almost similar pattern and trend are observed for all three regions.

3.2 Impact of Flexibility on Default and Absence in Weekly Meetings

(1) Econometric Model

Since our treatment assignment was distributed randomly (see Section 2), to empirically complement our discussion of the repayment analysis, we could simply use ordinary least squares (OLS) regressions to evaluate the impact of various treatments on a number of outcomes. More precisely, we estimated:

$$Y_h = b_0 + b_1 D_{1h} + b_2 D_{2h} + b_3 D_{3h} + u_h, \tag{3}$$

where Y_h is the outcome variable for household h, D_{jh} (j = 1, 2, 3) is a dummy variable indicating that household h was provided flexible microcredit under treatment arm j (j = 1, 2, 3; i.e., Flexible 1, Flexible 1 + IGA, and Flexible 2, respectively), and u_h is a zero mean error term. Equation (3) was applied to all borrowers in the sample so that the number of observations was 1,080. Because the randomization was implemented at the village level and sample households were drawn using the village as the primary sampling unit, we used robust standard errors for b's cluster at the village level, in order to test the null hypothesis.

The coefficient b_0 indicates the repayment behavior of control borrowers who were under the traditional, inflexible microcredit scheme. If the null hypothesis $b_1 = b_2 = b_3 = 0$ is rejected, we will investigate which flexibility scheme was more effective than others by comparing the three parameters of b_1, b_2, and b_3. If the null is not rejected, the coefficient b_0 indicates the repayment behavior of all borrowers on average. Therefore, in the tables that show regression results, the estimate for the intercept is presented in the first row, which is readily interpreted as the estimate for the overall mean if all coefficients on the dummy variables are zero, for the sake of convenience.

(2) Regression Results

Table 3 shows the regression results using each of the five variables associated with repayment behavior as the dependent variable. We would like to start this analysis by highlighting the indicator variable *default*, a dummy variable equal to 1 if a borrower's due payment at the end of the loan cycle is positive, and 0 otherwise. As shown in column (1) of Table 3, the Control group (i.e., borrowers under a traditional, rigid weekly repayment scheme) had a relatively lower average rate of default than did the three groups that belonged to other flexible repayment schemes. However, the difference was not statistically significant at the 5% level. The variable *default* was higher by 15.3% among Flexible 2 borrowers but the difference was only marginally significant (at the 10% level). The null hypothesis that $b_1 = b_2 = b_3 = 0$ was not rejected at the 10% level, indicating that the flexibility in our RCT did not result in higher default rate.

Regarding the households' repayment behavior, this indicator variable did not take into account the degree of such loan repayment defaults. To understand better the delinquency amount of various microcredit groups, we need to look at the amount of default, rather than the indicator variable. Columns (2) of Table 3 show the regression results when the dependent variable is *due_amount* (absolute level of delinquency). The result indicates that the "Flexible 1 + IGA" treatment arm had a higher absolute amount of delinquency due payment than the other groups. As discussed, unlike other groups, the Flexible 1 + IGA group received the IGA asset of their choice within the loan amount, plus IGA-related training, in place of a cash credit. The main reason for such a design is to capture the popular criticism of microcredit—namely, that the credit received by borrowers is largely used in consumption-smoothing, rather than in acquiring assets or undertaking IGAs (e.g., Armendariz and Morduch 2010). However, practitioners of microcredit usually allow borrowers to smooth consumption, as long as the required weekly amount due is paid on time. In our experimental design, we wanted to test the impact of a restricted consumption-smoothing option with credit, by introducing the "IGA group with loan repayment" pattern, which is similar to that of the "Flexible 1" groups (i.e., complete moratorium during *monga*). Possibly due to the illiquid nature of this treatment arm, we observed a greater rise in delinquency amount in the "Flexible 1 + IGA" group, as the assets they received did not generate enough revenue to allow regular repayments. However, the difference was not statistically significant. The null hypothesis that $b_1 = b_2 = b_3 = 0$ was not rejected at the 10% level, either.

The conclusion from the first two columns of Table 3 is thus clear. As far as the delinquency is concerned, Flexible 2 or Flexible 1 + IGA borrowers slightly tended to have larger delinquency, but the difference was statistically insignificant.

To understand the repayment discipline and commitment behavior of various groups, equation (3) was re-estimated using indicators related with the total number of missed weeks as the dependent variable. Columns (3) and (4) of Table 3 corresponding to the number of missed weeks with or without the "balance carried forward" option (*num_miss1* and *num_miss2*) show the results regarding the absolute number. Both columns show that on average, a greater number of weeks were missed among the Control group (a traditional, rigid weekly repayment scheme) than those borrowers with flexible repayment. This seems to suggest that flexibility result in better discipline, which is the opposite to MFIs' fear.

However, these figures do not represent the true state of the loan discipline pattern, as the Control group had more weekly dues (i.e., 45 weeks of repayment obligations) than the other groups (e.g., "Flexible 1" had 36 weeks of repayment obligation). Thus column (5) of Table 3 shows the regression results when the dependent variable was the missed weeks as a percentage of total due weeks (*num_ratio*). It appears that "Flexible 1 + IGA" borrowers had a relatively larger ratio of missed weeks compared to the other groups; this is consistent with previous observations using *due_amount* or *od_ratio* as the dependent variable. However, unlike the case for *due_amount* or *od_ratio*, the difference is absolutely small and statistically insignificant. Again the null hypothesis that $b_1 = b_2 = b_3 = 0$ was not rejected at the 10% level.

We found that neither the seasonality nor the spatial heterogeneity (*char*, river-basin, and inland) affected the regression results reported in Table 3. The rejection of the null hypothesis that $b_1 = b_2 = b_3 = 0$ was found robust to other specifications that allow for the seasonality or the spatial heterogeneity.[9]

(3) Subjective Evaluation of Flexible Microcredit by Borrowers

To understand borrowers' reactions to the current repayment flexibility experiment, and their feedback with respect to it, we executed a satisfaction survey that followed the work of Devoto et al. (2012), who asked existing clients whether they had any complaints, problems, or difficulties with the assigned treatment schedule of repayment. The survey was conducted as a part of the first *monga* survey (Panel 2) in November 2011. In the current study, if the borrower responded negatively, then we categorized such an answer as "not satisfied" in the satisfaction index, and 0 otherwise.

The regression result based on equation (3) is presented in Table 4. It clearly shows that borrowers under the Flexible 1 repayment scheme (complete moratorium of repayment during *monga*) were more likely to report positively than the typical microcredit repayment scheme (regular weekly repayment). Among the treatment arms, Flexible 1 had a higher level of satisfaction than the other groups; this finding is consistent with our hypothesis. Our conjecture is that because of this satisfaction, borrowers maintained their discipline in repayment under flexible schemes.

[9] The robustness check results are not reported here, but are available upon request.

3.3 Summary and Discussion

In this section, we empirically analyzed the repayment behavior among borrowers with access to various microcredit products assigned to them under the RCT-based field experimental framework. Using an RCT-based field experiment in northern Bangladesh, we randomly assigned seasonality-adjusted flexible microcredit and traditional rigid microcredit to various borrowing groups. Our results suggest there are no statistically discernible differences among the treatment arms in terms of default or overdue amounts, and these findings thus support the provision of a flexible microcredit design.

As mentioned in the introduction, our main motivation in introducing seasonality-adjusted flexible microcredit was to verify the rationales of the MFIs working in northern Bangladesh in not providing flexibility in loan repayment during *monga*. The reluctance of MFIs in providing flexibility or seasonal adjustments during *monga* is mainly due to their worry that the flexibility might break the borrowers' loan collection discipline so that it might increase the rate of loan default. When we introduced this experimental design, GUK, our counterpart NGO, strongly argued that the loan default rate would increase significantly in the moratorium group (Flexible 1): they thought that it would hamper loan discipline and also affect their financial behavior vis-à-vis the making of regular installment payments. Some GUK executives also said that the loan borrowers from the moratorium group might "run away" with the money. Our regression results convincingly show that this worry is baseless. Unlike the claims of MFIs in Bangladesh, we saw no statistically significant differences among the treatment arms in terms of seasonality-adjusted flexible microcredit. With the treatment arm featuring a complete moratorium of weekly repayment during *monga* (high-risk credit) and monthly repayment during *monga* (low-risk credit), we found that borrowers did not show any statistically significant pattern of delinquency or lower frequency repayment that was in line with the claims of the MFIs of i) discipline problems or ii) repayment problems. It appears that even when imposing a high level of credit risk (Flexible 1) on our counterpart MFI, GUK did not face a level of delinquency that was statistically different from the delinquency amount seen among traditional groups (the delinquency rates were 3.77% and 3.75% of the total due amount in the cases of traditional and Flexible 1 borrowing, respectively). In other words, even after allowing a moratorium during *monga*, we found that our counterpart NGO managed to regain more than 95% of its targeted amount of credit with interest, and so this can be considered a successful business microfinance model.

4. Impact of Flexibility on Household Consumption

In this section, we examine whether seasonal adjustment in microcredit affects the food consumption level of borrower households. Through this examination, we assess the welfare impact of moratorium or less frequent repayment meetings during *monga*.

4.1 Data on Household Food Consumption

For the impact analysis regarding consumption, we use microdata collected in the resurvey (Panel 3, July–August 2012) of the 1,440 households that were covered in the benchmark survey. We were able to resurvey 1,422 households, implying an attrition rate of 1.25%. Although this rate is low, we need to be attentive to the possibility of attrition bias, if the attrition happened in a nonrandom manner. In the third panel of Table 1, we show the distribution of resurveyed households across various treatment arms. As shown in the table, attrition occurred irrespective of treatment status, with no concentration within a particular treatment type. In Appendix Table 3, we formally show that the attrition was statistically independent of treatment status; therefore, we conclude that the resurvey data can be used in the impact evaluation, without concerns vis-à-vis attrition bias.

Table 5 describes seven qualitative measures of food consumption,[10] which we will analyze in this section. During *monga* 2011,[11] many households were not able to have three stomach-full meals each day. The average number of *num_mong1* was 2.1 meals per day; this became as low as 1.7 meals a day, if we specifically focus on the worst days during *monga* (variable named *num_mong2*). A dummy variable that takes the value of 1 if the household could afford two or three meals per day, even during the worst period, is used as a measure of food safety (denoted as *safe_mong* in the table). Using this measure, 68% of the households were food-secure during *monga* 2011. As another measure of food security, we will analyze a dummy variable for meat consumption within a month during *monga* 2011 (denoted as *meat_mong*), indicating that 76% of sample households were able to eat some meat.[12]

As shown in the last panel of Table 5, food consumption situations recovered substantially after *monga*. The average number of stomach-full meals in a day during the normal, non-*monga* time in 2012 (*num_norm1*) was 2.9 meals a day; that number was slightly reduced to 2.1 meals a day, if we specifically focus on the worst days during the same period (*num_norm2*). Using *safe_norm*, a dummy variable that takes the value of 1 if the household could afford two or three meals per day, even during the worst period, 89% of the households were food-secure during normal non-*monga* times in 2012. Although not shown in the table, almost all sample

[10] Quantitative information on household consumption—such as total expenditure, including the imputed value of self-produced foods—is not available in our dataset.

[11] Information on food consumption during *monga* 2011 was collected in the Panel 3 survey, which covered the entire *monga* period; this information, therefore, is not the same as that on food consumption, which was collected during *monga* 2011—i.e., in the Panel 2 survey in November 2011. The results reported in this paper remain qualitatively the same, if we use the Panel 2 survey data instead.

[12] In the questionnaire, we also asked about fish consumption. The absolute majority of sample households were able to eat fish in a month, even during *monga*. Given this lack of variation, we use meat as a measure of protein security.

households were able to consume some meat in a month; therefore, for the impact analysis of food consumption during this period, we will use only *num_norm2* and *safe_norm* as dependent variables.

Similar to the case for the repayment behavior, food consumption variables too are systematically correlated with geographical categories: *char*, inland, and river basin. Inland households had the highest mean for all of the six variables. This is as expected as households living in inland areas away from rivers have better access to food markets than households living in *char* areas or areas close to rivers. Against our prior expectation, *char* households had higher means for five of the six variables than river-basin households, although the difference was small.

4.2 Impact of Flexibility on Household Food Consumption

(1) Econometric Model

Because the intervention was randomly assigned (see Section 2), we simply regressed the Panel 3 outcomes on the dummy variables for various treatments, to evaluate the impact. More precisely, we estimated:

$$Y_h = b_0 + b_1 D_{1h} + b_2 D_{2h} + b_3 D_{3h} + b_4 D_{4h} + u_h, \tag{4}$$

where Y_h is a post-intervention outcome variable for household h, D_{jh} (j = 1, 2, 3) is a dummy variable indicating that household h was provided with flexible microcredit under treatment arm j (j = 1, 2, 3; i.e., Flexible 1, Flexible 1 + IGA, and Flexible 2, respectively), D_{4h} is a dummy for nonborrower households, and u_h is a zero mean error term. If the null hypothesis that $b_1 = b_2 = b_3 = 0$ is not rejected, it is indicated that the flexibility within our RCT had no impact. If this null hypothesis is not rejected while another null hypothesis that $b_4 = 0$ is rejected, it is indicated that microcredit provision had an impact, regardless of flexibility. If the null hypothesis that $b_1 = b_2 = b_3 = 0$ is rejected, we will investigate which flexibility scheme was more effective than others by comparing the three parameters of b_1, b_2, and b_3. Because the randomization was implemented at the village level and sample households were drawn using the village as a primary sampling unit, we use robust standard errors for b's clustered at the village level, in order to test the null hypotheses.

Although randomization is likely to result in the treatment and control households being similar across all variables in expectation, within any particular sample, there can be small baseline differences (see Appendix Tables 1-2). To address this issue, we added to equation (4) a control for baseline variables that were associated with significant differences across treatment arms. We will report on this as a robustness check. Other specifications using changes in outcomes between Panels 3 and 1 as dependent variables are left for future research.

As other robustness checks, we estimated two further models. In the first one, the last term in equation (4), b_4D_{4h}, was allowed to have various slopes, depending on the village-level treatment type. If there existed spillover effects from borrowers to nonborrower households within a *samity*, and the spillover effects were systematically different, depending on the treatment arm assigned to the *samity*, nonborrower households could be heterogeneous across the village-level treatment arms. The extended model can accommodate this possibility. Second, we dropped the last term in equation (4), b_4D_{4h}, and estimated the contracted model using only data on borrower households.

(2) Expected Signs of Parameter Estimates

To examine the impact of repayment flexibility on food consumption, we estimated equation (4) using each of the six variables listed in Table 5 (*num_mong1*, *num_mong2*, *safe_mong*, *meat_mong*, *num_norm2*, and *safe_norm*) as dependent variables. As stated previously, the variable *num_norm1* in Table 5 was not analyzed, due to a lack of variation therein.

Theoretically speaking, the impact of repayment flexibility on food consumption is indirect. The flexibility does not directly affect the ways in which households choose consumption. On the other hand, it indirectly affects consumption through income, price, and credit constraint effects.

We begin the discussion of the likely sign of b_4. We expect it to be negative, i.e., we expect that the provision of microcredit increases food consumption. The first channel is the income effect. If microcredit enhances permanent household income by allowing households to allocate resources more efficiently, the resulting increase in income should be reflected in higher levels of food consumption. This route should apply to each of the six dependent variables. The second channel is the price effect. If microcredit enhances the productivity of self-employment businesses and there is imperfection in labor markets, the shadow price of family labor should increase, which is in turn likely to lead to the allocation of more household resources to food (as the major input to human capital). However, it is also possible that an increase in shadow wage could work in the *opposite* direction regarding food consumption demand. The net impact can be either positive or negative theoretically, but in either case, the absolute value of the net impact is not likely to be large. The third channel is the credit constraint effect. By definition, the provision of microcredit to a household enhances its ability to smooth resource allocation across time. Since *monga* suffering is anticipated by households, it is possible that reducing food consumption during *monga* is a symptom of a binding liquidity constraint. If this is the case, we expect b_4 to be more negative when the dependent variables are food consumption during *monga* than during the normal time following *monga*.

If the flexibility arrangements examined in our experiments have similar magnitudes of income, price, and credit effects, we expect each of b_1, b_2, and b_3 to be zero. Alternatively, if Flexible 1 + IGA makes it more likely for borrower households to engage in self-employment businesses that yield immediate gains, the income and price effects are likely to be larger for this treatment than for others. If this is the case, we expect b_2 to be positive and larger than each of b_1 and b_3. Regarding the liquidity effect, we expect Flexible 1 and Flexible 1 + IGA to have additional gains over Flexible 2, and Flexible 2 to have additional gains over Control. This is because the repayment moratorium gives households greater freedom to allocate money across 60 days of *monga* than can the inflexible, traditional microcredit scheme; similarly, monthly repayments give households more freedom to allocate money across 30 days in a month during *monga* than can traditional microcredit. If this is the case, we expect $b_1 = b_2 > b_3 > 0$.

(3) Regression Results

The results regarding the impact of our RCT on food consumption are reported in Table 6. Regarding food consumption during *monga* 2011 (columns (1)–(4), Table 6), the null hypothesis that $b_1 = b_2 = b_3 = 0$ was not rejected at the 10% level for all four consumption variables. This indicates that the flexibility in our RCT had no impact on household-level food consumption behavior during *monga* 2011. Looking at individual parameters, in the equation for *meat_mong* (dummy for meat protein safety), parameter b_2 (the impact of Flexible 1 + IGA) is negative and statistically significant at the 5% level. The estimated parameter suggests that such borrowers were 19 percentage points less likely to have meat in a month (versus the sample average of 76%); this marks a significant welfare loss. One explanation could be the enhanced need for money to undertake IGA activities that do not yield immediate income gains, combined with limited consumption-smoothing abilities. The combination of these factors implies that those borrowers under Flexible 1 + IGA were likely to have spent more on IGA business at the expense of meat consumption.[13] However, testing of this explanation will be left for future research.

Parameter b_4 was estimated with a negative sign (as expected) in three of the four equations, but its absolute value was small; it was also statistically insignificant in all four equations. Therefore, contrary to our expectations, none of our microcredit provisions had any impact on food consumption during *monga*. As this parameter is estimated imprecisely, the null

[13] Note that credit was given just before the lean season. As a result, if the borrower wanted to use the credit for business investment, it was more likely that her household would reduce consumption (or, at least, not increase it) and try to divert as much money as possible to the business (Banerjee and Duflo 2011, p.171). We could then expect that once the business would start to earn revenue, the household might increase its consumption.

hypothesis $b_1 = b_2 = b_3 = b_4 = 0$ is not rejected, even at the 20% level.

The results regarding the impact of our RCT on food consumption during normal, non-*monga* times in 2012 are reported in columns (5)–(6), Table 6. When the number of minimum stomach-full meals per day during these normal times (*num_norm2*) was used as the dependent variable, all coefficients on the four dummy variables were small in terms of absolute values, and the null hypothesis that $b_1 = b_2 = b_3 = b_4 = 0$ was not rejected at the 20% level. On the other hand, when the same variable was transformed as a dummy for food safety during normal times (*safe_norm*), b_3, the impact of Flexible 2, was negative and statistically significant at the 5% level. The estimated parameter suggests that such borrowers were 9 percentage points less likely to be food-secure (versus the sample average of 89%). This too marks a welfare loss associated with flexible microcredit, which remains a puzzle. When the dependent variable is *safe_norm*, the point estimate for b_4 is –0.082 and statistically significant at the 5% level. The estimated parameter indicates that nonborrowers were 8 percentage points more likely to be food-insecure (versus the sample average of 89%). This evidence supports the favorable impact of credit provision in enhancing consumption.

The results reported in Table 6 were robustly found from other specifications.[14] We tried (i) extending equation (4) with benchmark village and household attributes as additional explanatory variables, (ii) extending the last term in equation (4), $b_4 D_{4h}$, to have different slopes depending on the treatment arms, (iii) re-estimating equation (4) without the last term, while using only borrower households, and (iv) using the limited dependent variable models, considering the truncation or integer nature of the dependent variables. The robustness check results from extension (i) are reported in Table 7. From the nine village-level variables analyzed in the balance check in Section 2, the distance to the nearest town and the distance to a Hindu temple were included as village-level controls, since these two variables were associated with a marginal failure of the balance check. Similarly, from the 20 household-level variables analyzed in Section 2, the household size, average age of members, ratio of adults, and the number of chickens and ducks owned were included as household-level controls, since these four variables were associated with a marginal failure of the balance check. The addition of these six controls did not alter the coefficients and test results regarding the four parameters of interest: b_1, b_2, b_3, and b_4. One small change was that the negative coefficient of b_4 in the *meat_mong* equation became marginally significant, suggesting that credit enhances the meat consumption of borrower households during *monga*.

4.3 Summary and Discussion

This section empirically assessed whether a flexible repayment design for microcredit

[14] The robustness check results are not reported here, but are available upon request.

could enhance food consumption among the ultrapoor. We used a cross-sectional dataset collected in 2012, after an RCT was implemented in 2011–12 in northern Bangladesh. We found repayment flexibility to have no positive impact on food consumption. On the other hand, all microcredit borrowers tended to have more secure food consumption than nonborrowers, although the difference was marginal.

Our finding of the difficulty in identifying pinpointing a positive impact of microcredit on food consumption is consistent with the literature on microcredit in Bangladesh (e.g., Roodman and Morduch 2009). Our finding that repayment flexibility had no positive impact on consumption may appear inconsistent with the finding of Czura et al. (2009), who show that flexible repayment schedules resulted in smoother consumption, and with the finding by Shoji (2010), who shows that rescheduling substantially decreased the probability of meal-skipping among borrowers. However, our finding is not inconsistent with that of Czura et al. (2009) because they also show a lack of difference in consumption *levels* between borrowers under standard and flexible repayment schedules. Our finding is not inconsistent with that of Shoji (2009) either, since what he analyzed was emergency cases involving flooding, when rescheduling would have immediately affected food consumption, whereas our experiment was conducted under normal conditions.

In the context of the current study, we could suggest several possible explanations for the insignificance of the flexibility impact. First, if the main route through which the provision of microcredit enhances consumption is the reduction of liquidity constraints, our finding is consistent with the view that the main problem for the ultrapoor is consumption-smoothing between the *monga* and non-*monga* seasons, as they were already able to smooth consumption within each season in the absence of microcredit. If this is the case—and both income and price effects are negligible—there should be no difference across microcredit types, but nonborrowers' consumption should be smaller than that of borrowers. Our empirical results broadly support this pattern. The unexpected negative coefficient of the impact of a repayment moratorium with IGA support in the regression for meat consumption during *monga* is consistent with this view as well: the borrowers under this scheme experienced difficulty in smoothing resources between the future and the current *monga* period, and they were compelled to spend more on their IGA. Second, the insignificance of the repayment flexibility impact could be due to the insignificant difference in income changes across the four credit schemes studied. This was likely when the borrowed money was invested in a business that did not generate immediate income gains. Third, the overall insignificance of regressions while using food consumption could be due to the treated households' perception of the transient nature of the intervention. If the borrower households perceived the change brought by microcredit—be it an income, price, or liquidity effect—as a one-time phenomenon, they did not realize that their permanent income (in terms of both level and variability) and credit-access positions improved. If this is the case, rational households may not adjust their consumption.

5. Conclusion

In this paper, we empirically examine whether flexible microcredit leads to an increase in repayment problems for MFIs and whether it can increase and stabilize consumption of borrower households. The empirical analysis is based on data collected through a randomized controlled trial in 2011-12 in northern Bangladesh. Our results suggest no statistically discernible difference among the treatment arms in case of default, overdue amount, or repayment frequency. This is in favor of flexible design of microcredit. On the other hand, we find no positive impact of the repayment flexibility on food consumption, either, while all microcredit borrowers tended to have more secure food consumption than nonborrowers. This could be due to the possibility that the main problem for the ultrapoor is consumption smoothing between the lean and non-lean seasons, the insignificant difference in income changes across the four credit schemes studied, or the treated households' perception of the transient nature of the intervention. The findings of this study will help MFIs optimize their credit schemes; they could also help other interested parties, including governmental institutions, advocate a relaxation of microcredit rules, or search for alternative policy instruments.

We encountered various issues while preparing this paper, and they need to be addressed in future research. For example, many of the coefficients had the expected signs, but they were not statistically significant. Adding greater variation through the implementation of additional rounds of surveys could enhance the precision of the estimations. We also need to investigate more carefully whether the findings observed herein are robust, by adding more control and geographical interaction; this will increase the statistical power of our estimations. Additionally, to observe behavioral changes in food consumption, and other household decisions, we need to make use of a dataset featuring a longer time horizon.

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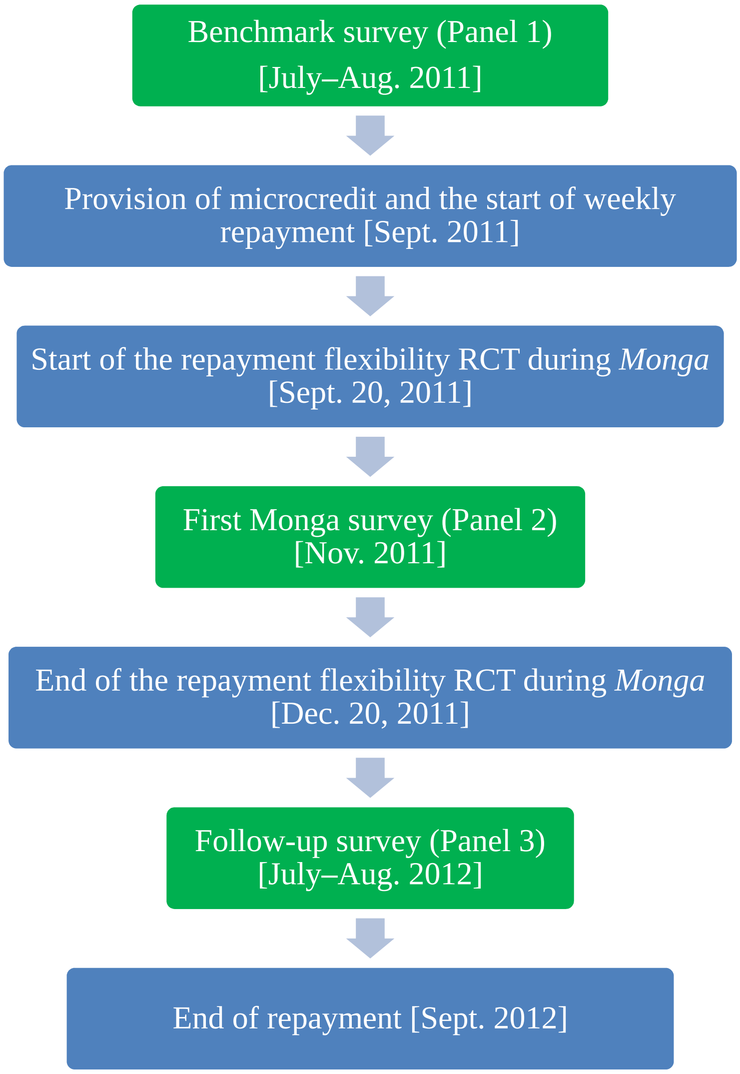
Figure 1: Timeline of Interventions and Surveys



Source: Prepared by the authors. The blue panels show events regarding interventions, and the green panels show events regarding surveys.

Figure 2: Seasonality of Repayment Behavior

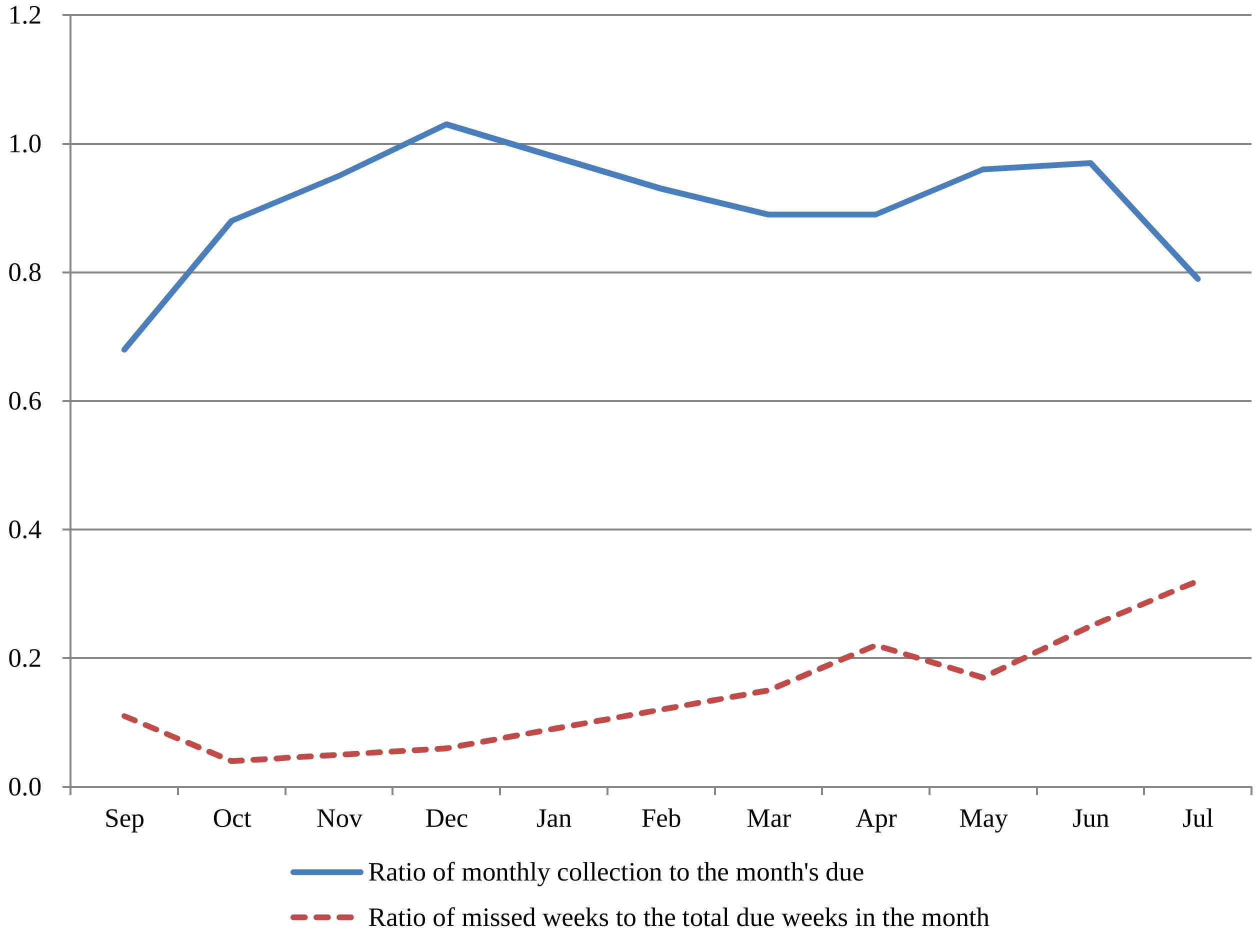


Table 1: Distribution of Sample Villages and Households by Treatment Type, Northern Bangladesh, 2011-12

	Treatment Allocation at the Village Level				Total
	Control	Flexible 1	Flexible 1 + IGA	Flexible 2	
Number of villages	12	24	12	24	72
By district					
Gaibandha District	9	16	8	12	45
Kurigram District	3	8	4	12	27
By location type					
Char	3	6	3	6	18
Inland	7	14	7	14	42
River-basin	2	4	2	4	12
Number of households in the benchmark survey, 2011 (Panel 1)					
Borrower	184	360	179	357	1,080
Nonborrower	56	120	61	123	360
Total	240	480	240	480	1,440
Number of households in the resurvey, 2012 (Panel 3)					
Borrower	183	357	175	354	1,069
Nonborrower	56	117	61	119	353
Total	239	474	236	473	1,422

Source: Compiled by the authors.

Table 2: Definitions and Summary Statistics of Variables Related with Repayment Behavior, Northern Bangladesh, 2011-12

Variable	Definition	N	Mean	Std. Dev.	Min	Max
Overdue at the end of a loan cycle						
default	Dummy for default (1 if the due amount is positive at the end of a loan cycle)	1,080	0.250	0.430	0	1
due_amount	Due amount at the end of a loan cycle (in BDT)	1,080	154.6	400.6	0	2,485
Number of weekly meetings missed						
num_miss1	Total number of missed weeks (balance carried forward)	1,080	5.380	5.980	0	41
num_miss2	Total number of missed weeks (without balance carried forward)	1,080	6.430	5.340	0	30
num_ratio	Ratio of total missed weeks to the total due weeks	1,080	0.170	0.190	0	1

Note: Mean and standard deviations are simple ones, without weighting.
Source: Compiled by the authors using the administrative information for borrowers.

Table 3: Impact of Flexible Microcredit on Repayment Behavior

	Overdue at the end of a loan cycle		Number of weekly meetings missed		
	default	*due_amount*	*num_miss1*	*num_miss2*	*num_ratio*
	(1)	(2)	(3)	(4)	(5)
Intercept	0.133**	126.7	7.256***	7.189***	0.173***
	[0.062]	[93.2]	[1.949]	[1.319]	[0.047]
D 1 (dummy for Flexible 1)	0.108	–0.511	–2.567	–1.139	–0.018
	[0.084]	[100.3]	[2.099]	[1.500]	[0.053]
D 2 (dummy for Flex. 1+IGA)	0.178	132.4	–2.150	–1.150	0.032
	[0.113]	[163.3]	[2.15]	[1.578]	[0.066]
D 3 (dummy for Flexible 2)	0.153*	18.1	–1.975	–0.561	–0.005
	[0.091]	[107.7]	[2.142]	[1.588]	[0.054]
F -stat. for zero slopes of all dummies	1.31	0.32	0.51	0.27	0.82
Number of observations	1,080	1,080	1,080	1,080	1,080

Notes: Robust standard errors clustered at the village level are shown in brackets. Significant at the 10% (*), 5% (**), and 1% (***).
Source: Estimated by the authors using the microdata described in the

Table 4: Regression Result for Satisfaction Survey

	Dependent variable: dummy for satisfaction
Intercept	0.456***
	[0.104]
D 1 (dummy for Flexible 1)	0.303**
	[0.124]
D 2 (dummy for Flex. 1+IGA)	0.106
	[0.163]
D 3 (dummy for Flexible 2)	0.206
	[0.128]
R^2	0.050
Adjusted R^2	0.047

Notes: The number of observations is 1,080. Robust standard errors clustered at the village level are shown in brackets. Significant at the 10% (*), 5% (**), and 1% (***).

Source: Compiled by the authors using the benchmark survey data.

Table 5: Definitions and Summary Statistics of Variables Related with Food Consumption, Northern Bangladesh, 2011-12

Variable	Definition	N	Mean	Std. Dev.	Min	Max
Food consumption during *monga* 2011						
num_mong1	Number of stomach-full meals in a day during Monga 2011	1,414	2.114	0.411	1	3
num_mong2	Number of minimum stomach-full meals a day during Monga 2011	1,412	1.693	0.498	1	3
safe_mong	Dummy for food safety during Monga 2011 (defined as *num_mong2* = 2 or 3)	1,412	0.676	0.468	0	1
meat_mong	Dummy for having meat within a month during Monga 2011	1,414	0.756	0.430	0	1
Food consumption during normal times in 2012						
num_norm1	Number of stomach-full meals in a day during normal time in 2012	1,416	2.859	0.362	1	3
num_norm2	Number of minimum stomach-full meals a day during normal time in 2012	1,415	2.127	0.586	1	3
safe_norm	Dummy for food safety during normal time in 2012 (defined as *num_norm2* = 2 or 3)	1,415	0.885	0.319	0	1

Note: Mean and standard deviations are simple ones, without weighting. The question of "Number of (minimum) stomach-full meals in a day" was asked of the respondents who had reported a typical number, and so that the answer took an integer value of either 1, 2, or 3.
Source: Compiled by the authors using the 2012 resurvey data (Panel 3).

Table 6: Impact of Flexible Microcredit on Food Consumption

	Food consumption during *monga* 2011				Food consumption during normal times in 2012	
	num_mong1	*num_mong2*	*safe_mong*	*meat_mong*	*num_norm2*	*safe_norm*
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	2.083***	1.713***	0.702***	0.834***	2.149***	0.939***
	[0.038]	[0.074]	[0.075]	[0.040]	[0.049]	[0.028]
D 1 (dummy for Flexible 1)	0.027	0.044	0.046	-0.051	-0.037	-0.054
	[0.063]	[0.085]	[0.087]	[0.060]	[0.073]	[0.039]
D 2 (dummy for Flex. 1+IGA)	0.124	-0.006	-0.023	-0.193**	0.092	0.021
	[0.082]	[0.109]	[0.100]	[0.086]	[0.080]	[0.034]
D 3 (dummy for Flexible 2)	0.013	-0.074	-0.089	-0.091	-0.062	-0.092**
	[0.051]	[0.090]	[0.087]	[0.057]	[0.077]	[0.041]
D 4 (dummy for non-borrower)	0.023	-0.044	-0.050	-0.077	-0.035	-0.082**
	[0.047]	[0.076]	[0.075]	[0.047]	[0.062]	[0.034]
R^2	0.008	0.008	0.012	0.014	0.006	0.016
F -stat. for zero slopes of all dummies	0.73	1.02	1.46	1.47	0.95	4.66***
F -stat. for zero slopes of *D* 1, *D* 2, and *D* 3	0.81	1.02	1.58	1.96	1.22	4.20***
Number of observations	1,414	1,412	1,412	1,414	1,415	1,415

Notes: Robust standard errors clustered at the village level are shown in brackets. Significant at the 10% (*), 5% (**), and 1% (***).
Source: Estimated by the authors using the microdata described in the text.

Table 7: Impact of Flexible Microcredit on Food Consumption (Robustness check with baseline controls)

	Food consumption during *monga* 2011				Food consumption during normal times in 2012	
	num_mong1	*num_mong2*	*safe_mong*	*meat_mong*	*num_norm2*	*safe_norm*
Baseline village characteristics						
Mondir (Hindu temple)	0.080	0.135**	0.135*	0.046	0.009	0.025
	[0.097]	[0.064]	[0.073]	[0.050]	[0.075]	[0.035]
Town	-0.119	-0.045	-0.046	-0.010	0.054	0.030
	[0.090]	[0.087]	[0.092]	[0.076]	[0.104]	[0.034]
Baseline household characteristics						
Household size (number of members)	0.033***	0.019	0.020*	0.022**	0.058***	0.027***
	[0.012]	[0.012]	[0.011]	[0.009]	[0.013]	[0.007]
Average age of household members	-0.002	0.000	0.000	-0.001	-0.003	0.000
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.001]
Ratio of adults (age 15+)	0.247***	-0.014	0.016	0.088	0.220**	0.059
	[0.086]	[0.068]	[0.061]	[0.057]	[0.089]	[0.045]
No. of chickens and ducks owned	0.002	-0.001	-0.001	-0.002	0.000	-0.003
	[0.003]	[0.003]	[0.003]	[0.003]	[0.004]	[0.002]
Treatment status						
$D1$ (dummy for Flexible 1)	0.011	0.044	0.047	-0.051	-0.036	-0.051
	[0.064]	[0.092]	[0.094]	[0.060]	[0.074]	[0.037]
$D2$ (dummy for Flex. 1+IGA)	0.102	0.005	-0.013	-0.192**	0.098	0.027
	[0.078]	[0.117]	[0.108]	[0.087]	[0.081]	[0.033]
$D3$ (dummy for Flexible 2)	0.013	-0.046	-0.059	-0.080	-0.050	-0.080**
	[0.058]	[0.099]	[0.096]	[0.057]	[0.080]	[0.040]
$D4$ (dummy for non-borrower)	0.013	-0.029	-0.036	-0.074*	-0.026	-0.075**
	[0.049]	[0.082]	[0.081]	[0.044]	[0.061]	[0.032]
Intercept	1.852***	1.605***	0.569***	0.732***	1.859***	0.793***
	[0.110]	[0.124]	[0.122]	[0.081]	[0.128]	[0.061]
R^2	0.028	0.018	0.023	0.020	0.025	0.032
F-stat. for zero slopes of all explan. variables	1.46	1.29	1.50	1.41	2.79***	3.05***
F-stat. for zero slopes of $D1$, $D2$, $D3$, and $D4$	0.62	0.64	0.93	1.44	0.92	4.48***
F-stat. for zero slopes of $D1$, $D2$, and $D3$	0.65	0.55	0.87	1.85	1.21	4.22***
Number of observations	1,414	1,412	1,412	1,414	1,415	1,415

Notes: Robust standard errors clustered at the village level are shown in brackets. Significant at the 10% (*), 5% (**), and 1% (***).
Source: Estimated by the authors using the microdata described in the text.

Appendix Table 1: Balance Test at the Village Level

	A. Dependent variable: Location (strata used in randomization)			B. Dependent variable: Minutes of travel to the nearest facility					
	Distance from the closest bus station (km)	Dummy for a *char* village	Dummy for an inland village	Bazar	College	Mondir (Hindu temple)	Town	Bus stand	Railway station
Intercept	32.167***	0.250*	0.583***	7.917**	27.083***	29.583***	34.167***	29.583***	61.667***
	[9.695]	[0.129]	[0.146]	[3.711]	[6.764]	[8.100]	[7.666]	[6.764]	[16.069]
$D1$ (dummy for Flexible 1)	12.458	0.000	0.000	1.042	3.75	3.125	8.958	26.292	18.750
	[12.431]	[0.158]	[0.179]	[4.536]	[7.833]	[9.846]	[8.661]	[21.057]	[19.620]
$D2$ (dummy for Flex. 1+IGA)	10.333	0.000	0.000	5.417	13.333*	25.000*	25.833**	24.583*	25.417
	[14.172]	[0.182]	[0.207]	[5.382]	[7.904]	[13.530]	[11.848]	[12.809]	[22.523]
$D3$ (dummy for Flexible 2)	15.25	0.000	0.000	-0.417	1.875	16.458*	13.333	10.417	26.458
	[12.426]	[0.158]	[0.179]	[4.495]	[8.071]	[8.869]	[8.463]	[8.054]	[20.090]
R^2	0.020	0.000	0.000	0.026	0.047	0.109	0.109	0.028	0.028
F-stat. for zero slopes of all dummies	0.537	0.000	0.000	0.556	1.731	2.556*	1.807	1.549	0.649

Note: The number of observations is 72. Robust standard errors are shown in brackets. Significant at the 10% (*), 5% (**), and 1% (***). Dependent variables for B are measured in minutes if public transportation is used and the value of zero is assigned when the facility exists in the village.

Source: Estimated by the authors using the benchmark survey data.

Appendix Table 2: Balance Test at the Household Level

	A. Dep. variable: Characteristics of the head				B. Dependent variable: Charactesitics of household members					
	Age	Dummy for female	Dummy for literacy	Years of schooling	Household size	Average age	Female ratio	Ratio of adults (age 15+)	Literacy rate of adult males	Literacy rate of adult females
Intercept	38.592***	0.228***	0.245***	1.603***	3.745***	26.152***	0.552***	0.695***	0.277***	0.234***
	[1.223]	[0.064]	[0.041]	[0.253]	[0.206]	[1.158]	[0.018]	[0.022]	[0.040]	[0.041]
D 1 (dummy for Flexible 1)	-0.423	-0.037	-0.025	-0.189	0.350	-1.146	-0.016	-0.042	0.008	-0.017
	[1.362]	[0.081]	[0.048]	[0.298]	[0.255]	[1.350]	[0.024]	[0.026]	[0.051]	[0.046]
D 2 (dummy for Flex. 1+IGA)	-0.738	-0.100	-0.010	-0.285	0.345	-1.902	-0.029	-0.030	0.023	0.080
	[1.390]	[0.072]	[0.073]	[0.373]	[0.259]	[1.405]	[0.023]	[0.026]	[0.062]	[0.063]
D 3 (dummy for Flexible 2)	-0.007	-0.055	-0.026	-0.127	0.398	-1.792	-0.030	-0.069***	0.017	0.052
	[1.341]	[0.079]	[0.049]	[0.308]	[0.246]	[1.255]	[0.021]	[0.026]	[0.049]	[0.048]
D 4 (dummy for non-borrower)	-0.676	-0.037	-0.047	-0.265	0.047	0.444	-0.012	-0.003	0.016	-0.001
	[1.320]	[0.064]	[0.045]	[0.281]	[0.204]	[1.269]	[0.020]	[0.026]	[0.046]	[0.045]
R^2	0.001	0.005	0.001	0.001	0.012	0.011	0.002	0.016	0.000	0.008
F -stat. for zero slopes of all dummies	0.25	0.76	0.42	0.28	2.58**	2.81**	0.62	4.55***	0.06	1.80
F -stat. for zero slopes of *D* 1, *D* 2, and *D* 3	0.27	0.86	0.12	0.22	0.94	0.85	0.78	2.85**	0.06	2.21
Number of observations	1,440	1,440	1,440	1,440	1,440	1,437	1,440	1,440	1,252	1,428

	C: Dependent variable: Landholdings					D: Dependent variable: Liquid asset				
	Dummy for owning the house land	Dummy for owning farm land	Size of operational farmland for *aus*	Size of operational farmland for *aman*	Size of operational farmland for *boro*	Total value of household assets	Dummy for owning livestock animals	Number of cows and bulls owned	Number of goats and sheep owned	Number of chickens and ducks owned
Intercept	0.310***	0.054**	0.554**	2.201***	2.370***	2755.2***	0.668***	0.364***	0.475***	3.060***
	[0.092]	[0.022]	[0.269]	[0.734]	[0.781]	[329.7]	[0.073]	[0.089]	[0.120]	[0.715]
D 1 (dummy for Flexible 1)	0.146	-0.032	1.312	0.163	1.261	458.100	0.034	0.164	0.083	0.210
	[0.113]	[0.023]	[0.840]	[1.163]	[1.239]	[519.4]	[0.088]	[0.137]	[0.161]	[0.841]
D 2 (dummy for Flex. 1+IGA)	0.104	-0.021	0.189	0.117	-1.046	895.5*	-0.087	0.172	0.005	-0.697
	[0.127]	[0.029]	[0.557]	[1.546]	[1.033]	[499.9]	[0.100]	[0.228]	[0.163]	[0.845]
D 3 (dummy for Flexible 2)	0.094	-0.015	0.011	-1.111	-0.742	466.500	-0.013	0.031	0.093	-0.085
	[0.120]	[0.029]	[0.367]	[0.842]	[0.914]	[382.2]	[0.083]	[0.115]	[0.148]	[0.813]
D 4 (dummy for non-borrower)	0.087	-0.013	0.557	0.110	0.347	444.600	-0.063	0.100	-0.003	-1.085
	[0.089]	[0.022]	[0.585]	[1.162]	[1.031]	[315.9]	[0.074]	[0.101]	[0.125]	[0.667]
R^2	0.008	0.003	0.005	0.003	0.007	0.003	0.008	0.005	0.002	0.011
F -stat. for zero slopes of all dummies	0.46	0.89	0.66	0.87	1.44	0.86	1.60	0.56	0.36	4.47***
F -stat. for zero slopes of *D* 1, *D* 2, and *D* 3	0.56	0.84	0.87	1.06	1.55	1.09	0.69	0.63	0.23	0.73
Number of observations	1,440	1,440	1,440	1,440	1,440	1,440	1,440	1,440	1,440	1,440

Notes: Robust standard errors clustered at the village level are shown in squared brackets. Significant at the 10% (*), 5% (**), and 1% (***) levels.
Source: Estimated by the authors, using the microdata described in the text.

Appendix Table 3: Attrition and Treatment Status

	Dependent variable: dummy for attrition
Intercept	0.00543
	[0.00526]
D 1 (dummy for Flexible 1)	0.00290
	[0.00800]
D 2 (dummy for Flex. 1+IGA)	0.01691
	[0.01739]
D 3 (dummy for Flexible 2)	0.00297
	[0.00803]
D 4 (dummy for non-borrower)	0.01401
	[0.00958]
R^2	0.003
F -stat. for zero slopes of all dummies	0.81

Notes: The number of observations is 1,440. Robust standard errors clustered at the village level are shown in brackets. None of the coefficients on dummy variables is significant at the 20% level.

Source: Compiled by the authors using the benchmark survey data.