

# When Nudge Comes to Shove: Demand for Commitment in Microfinance Contracts\*

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December 2018

## Abstract

We conduct a field experiment to test the demand for flexibility and for soft and hard commitment among clients of a microfinance institution. We offer a commitment contract inspired by the rotating structure of a ROSCA. Additional treatments test *ex ante* demand for soft commitment (e.g., reminders), hard commitment (e.g., penalty for missing an instalment), and flexibility (e.g., option to postpone an instalment). Our design is unique in the literature for allowing us to test — using the same respondent population — how demand for behavioral features differs between loan and savings contracts. We find substantial demand for both credit and saving contracts but no demand for any of the additional contract features, in isolation or in combination, in spite of their effectiveness in improving repayment. In particular, demand for savings is insensitive to behavioral features. Individuals offered loans actively dislike commitment and flexibility, unless the latter is combined with reminders. These findings complement a literature showing that commitment devices induce financial discipline, but suggest that many commitment devices used in practice may be seen as overly restrictive *ex ante*, even for a population with a demonstrated demand for commitment products.

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\*We are grateful to RCons for their help collecting the survey data and to NRSP for their invaluable assistance in running the experiment. Funding for the experiment and data collection was provided by the International Growth Centre and by the Economic and Social Research Council (UK). We thank Mahreen Mahmud and Muhammed Meki, both for excellent field assistance and for valuable comments. We thank participants at the 2018 CSAE Annual Conference.

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## 1 Commitment problems and commitment devices

Commitment problems, due either to intra-personal factors such as time-inconsistent preferences, or to inter-personal ones like the inability to resist demands from others, are often cited as important barriers to saving and impediments to the repayment of loans (Casaburi and Macchiavello, 2018; Dupas and Robinson, 2013b; Duflo, Kremer, and Robinson, 2011). Features of formal and informal financial products testify to the importance of commitment issues in financial decisions. The high frequency of instalments, the rigidity of the repayment schedules and group lending, all typical of microfinance contracts, are believed to provide financial discipline and commitment devices to borrowers (Field and Pande, 2008; Field, Pande, Papp, and Rigol, 2013). Similar elements characterize rotating credit and savings associations, one of the oldest and most prevalent informal financial product in the developing world (Gugerty, 2007). Consistent with this, commitment devices, relying either on psychological or economic motivations, have proven effective in encouraging savings and reducing loan defaults (Ashraf, Karlan, and Yin, 2006; Brune, Giné, Goldberg, and Yang, 2016; Dupas and Robinson, 2013b; Karlan, McConnell, Mullainathan, and Zinman, 2016; Stango and Zinman, 2014).

The evidence on commitment problems in financial decisions comes from two largely distinct streams of research, which treat saving and borrowing as if they were two separate behavioral realms, both conceptually and practically. However, when individuals struggle to hold on to savings over time and wish to incur lumpy expenditures, saving and borrowing may be substitutes (Afzal, d'Adda, Fafchamps, Quinn, and Said, 2017). While the idea that individuals may "borrow to save" is not new (Rutherford, 2000; Morduch, 2010; Collins, Morduch, Rutherford, and Ruthven, 2009; Armendáriz and Morduch, 2010),

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its implications for models of individual financial decision-making and for the design and evaluation of financial instruments have not been explored.

In this paper we fill this gap by presenting evidence on two large field experiments in Pakistan, conducted with clients of a prominent microfinance institution. Participants are offered financial commitment products that differ along several dimensions. Some take the form of a standard credit contract, with a lumpsum disbursed at the outset followed by a sequence of regular instalments to be repaid. Others take the form of a commitment saving contract, with a sequence of regular instalments followed by a lumpsum disbursed at the end. Both contracts offer the same commitment device, but differ in the timing of the lumpsum disbursement. In addition, in one of the field experiments we augment this standard product with a set of commitment devices or with an option for flexible payments. We select commitment devices that are representative of the major tools tested in the literature on savings and borrowing: we offer soft commitment in the form of reminders to the clients or to his or her peers; and hard commitment in the form of a penalty for missing an instalment. We introduce higher flexibility by allowing clients to defer one instalment. Finally, the contracts vary in the interest rate they charge.

This design, by offering credit, saving, and various commitment features to the same sample of individuals, allows us to test how demand for commitment varies depending on whether it is embodied in a saving or credit contract, and on whether it is increased by soft or hard commitment devices or decreased by flexibility. Crucially, we can test whether demand for the same commitment features, which have been added by previous studies either to saving or to credit contracts only, varies when these features are part of a credit or to a saving product. Since we offer multiple cycles of the product to the same subjects,

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and randomly vary contract terms in each cycle, we can test how demand changes not only between subjects in the same sample, but also within subjects. These tests can help further our understanding of the behavioural foundations of microfinance (Bauer, Chytilová, and Morduch, 2012), and is important for guiding the future development of next-generation microfinance products incorporating behavioural features.

We find substantial demand for both saving and credit contracts, although take-up of the former is lower than that of the latter. When offered both a credit and a saving contract, 46 percent of the participants who take up at least one offer actually accept both types of contracts. We also find that some subjects take up savings contracts with a negative return, while others do not take credit contracts with an interest subsidy. We interpret these results as demand for commitment: saving and credit are substitutes for some individuals, as both represent means to finance expenditure by committing to periodic instalments. However, we find no demand for the additional features of the product — like flexibility and reminders. In particular, take-up of the saving product is insensitive to the presence of any additional feature. Instead, clients offered credit contracts appear to actively dislike the additional commitment features and find no value in reminders, with one exception being the combination of flexibility and personal reminders. Finally, we find no meaningful effect of our microfinance product on either business growth or household welfare — whether the product is offered in its basic form, or with additional features included.

Together, we see our results as making three distinct contributions. First, by presenting the same set of clients with both debt and credit products, we show that, in developing countries, many microfinance clients ‘borrow to save’. In previous pilot work, we showed this pattern of behaviour for small product sizes with daily repayments (Afzal, d’Adda,

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Fafchamps, Quinn, and Said, 2017). This paper substantially extends that result, by showing that the same general behaviour holds for a product with much larger payments, over a longer period — in particular, a product with payment sizes and repayment periods similar to those of standard financial products on the market. To the best of our knowledge, this is the first paper that randomly offers the same client pool both credit and savings products of a size comparable to standard microfinance products — and, therefore, the first paper to confirm that, for such products, many of the same clients will accept both credit and savings products. This complements recent work by Kast and Pomeranz (2018) showing that, for many microfinance clients, provision of savings accounts reduces levels of debt, and supports the already cited, slightly older, literature on borrowing to save among microfinance clients (Morduch, 2010; Collins, Morduch, Rutherford, and Ruthven, 2009; Armendáriz and Morduch, 2010).<sup>1</sup>

Second, this is the first time that demand for multiple types of commitment devices has been tested within the same field experiment. This is important, because it allows us to quantify the relative effect of different kinds of nudge, in the context of a common underlying contract and a common sample. Further, since we cross-cut behavioural features with both savings and credit products, we are uniquely positioned to test their relevance for both savings and credit products. Our results on demand for commitment are consistent the existing microfinance and microsavings literature. In particular, the decrease in take up of our product, when augmented with the additional commitment devices, and the high sensitivity of take-up rates to contractual terms mirror the wide range of take-up figures found in the literature, especially for credit contracts (Karlan, Morduch, and Mullainathan,

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<sup>1</sup> The paper also relates to a recent literature on formalisation of informal savings products (Dupas and Robinson, 2013b,a).

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2010). It also conforms with the mixed evidence on demand for commitment (see for instance [Ashraf, Karlan, and Yin \(2006\)](#)) and on its sensitivity to cost ([Laibson, 2015](#)).

This result — that interest in credit and commitment saving contracts is high, but there is no demand for flexibility or for additional commitment devices, either soft or hard — complements a recent literature documenting the hidden welfare cost of nudges. Recent empirical research shows that, while nudges can encourage intended behaviour, they also increase avoidance behaviour ([Allcott and Kessler, 2015](#); [Damgaard and Gravert, 2018](#)). We speculate that many kinds of commitment devices, including the rather ostentatious features that we add in this experiment, are viewed by respondents as patronising and infantilising, rather than supportive or helpful. What is intended as a gentle nudge may be resented as an aggressive shove, particularly in the credit domain.<sup>2</sup>

Finally, we join a growing set of papers in microfinance by measuring the impact of our financial product on a wide range of household and business outcomes. Consistent with previous studies in the literature, we do not find transformative effects of microfinance on either business outcomes or household material welfare ([Meager, forthcoming](#); [Angelucci, Karlan, and Zinman, 2015](#); [Attanasio, Augsburg, De Haas, Fitzsimons, and Harmgart, 2015](#); [Augsburg, De Haas, Harmgart, and Meghir, 2015](#); [Banerjee, Duflo, Glennerster, and Kinnan, 2015](#); [Crépon, Devoto, Duflo, and Parienté, 2015](#); [Tarozi, Desai, and Johnson, 2015](#); [Karlan and Zinman, 2011](#)).

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<sup>2</sup> More generally, experimental studies on ‘avoiding the ask’ and ‘moral wiggle room’ demonstrate how individuals avoid information or requests that make them feel morally obliged to act in a certain way, when such actions are costly ([Andreoni, Rao, and Trachtman, 2017](#); [d’Adda, Gao, Golman, and Tavoni, 2018](#); [Dana, Weber, and Kuang, 2007](#)). A related phenomenon, ‘control aversion’, is also found to cause incentives and regulations to backfire, when they are perceived as controlling ([Falk and Kosfeld, 2006](#); [Fehr and List, 2004](#)).

Our paper proceeds as follows. In Section 2, we present the experimental design and the different treatments, together with implementation details. We analyse demand for the product in Section 3, test the effect of access to the product in Section 5, and conclude in Section 6.

## 2 Experimental design

### 2.1 The basic contract

The financial product offered in the experiment is inspired by the repayment structure of rotating savings and credit associations: ‘ROSCAs’. Such associations have many different names in different parts of the world; in Pakistan, they are generally known as ‘committees’. In a committee, a group of individuals come together with the goal of facilitating saving. They agree to meet at regular intervals — for example, each week — for a set number of meetings at which they each make a fixed monetary contribution, the amount of which is agreed at the beginning of the contract. At each meeting, the contributions of all members are put into a common pool, which is then allocated to a group member. Participants take turns receiving the content of the pot, until everyone has received the pot once, at which time the contract ends.<sup>3</sup>

The contract we offer in our experiment has the same general profile of payments: fixed instalments at regular intervals over a set number of periods, plus one lumpsum payment mimicking receiving the pot. But the contract does not require the formation of a group —

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<sup>3</sup> The order in which members receive the pot can be determined by fixed order, or determined randomly, or assigned by bidding; this depends on the individual committee.

and thus sidesteps the selection and enforcement issues inherent to the formation of saving committees. Instead, the contract is designed as an individual financial product offered by our partner institution, the National Rural Support Programme (NRSP), a microfinance institution with extensive experience offering credit to women across Pakistan. The timing of the lumpsum disbursement is known to participants at the time of take-up.

Subjects are offered the opportunity to take up a contract in each of three product cycles, typically with different contract terms. At the beginning of a cycle, each participant is offered an individual contract with known terms. If they accept the contract, payment starts the following Monday (Week 1). Participants pay a weekly instalment of size  $M$  in  $N - 1$  of the  $N$  weeks, and receive a lump-sum payment of size  $L$  in the remaining week. A missed payment is considered a default and results in cancellation of the contract. In case of default, the participant has to return any payment owed to NRSP as soon as possible and, at the latest, before the beginning of the following cycle.<sup>4</sup> Within this basic design we experimentally vary the contracts offered along several dimensions: the number of weeks  $N$ ; the size of each instalment  $M$ ; the week in which the lumpsum payment is made (either Week 1 or Week  $N$ ); and the amount of the lumpsum payment  $L$ .

Recent literature has emphasized the value of replicating similar experiments in different variations and across different contexts; this is valuable for understanding the generalizability of results, and for understanding whether results are sensitive to specific aspects of design (see, for example, [Banerjee, Karlan, and Zinman \(2015\)](#)). With this principle in mind, we implemented our experiment in two distinct phases. These phases used differ-

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<sup>4</sup> If the money owed is not returned by the end of Week  $N$ , the participant is not offered any contract in the following cycle. In case NRSP owes money to the participant at the time of default, the money already paid in is returned to the subject at the end of the cycle — that is, after Week  $N$ .



ent sampling frames (one focusing on microenterprises; the other focusing on households), with contractual terms adapted to the respective respondent population.

In the first phase, we restricted participation to female NRSP clients — past and current — whose household owns a business. For this group, we set  $N = 6$  and  $M = PKR 1000$  and we let the lumpsum payment take three possible values:  $PKR 5000$ ,  $PKR 4500$  or  $PKR 5500$ . Since participants pay  $N - 1 = 5$  instalments of  $PKR 1000$  each, a lumpsum of  $PKR 5000$  simply returns the five installments to the subject. A lumpsum of  $PKR 4500$  is equivalent to deducting 10% from the lumpsum, while a lumpsum of 5500 means adding 10% to the sum of installments received. Table 1 illustrates the payment schedule for a basic contract with a lump sum payment on Week 1 and an interest of -10%.

< Table 1 here. >

Since there are three possible lumpsum values and two possible disbursement weeks, there is a total of six possible contracts. Three of these contracts have a lumpsum paid in Week 1: they are a form of commitment credit contract. Three have a lumpsum paid in Week  $N$ : they are a form of commitment savings contract. Note that some credit contracts charge a negative interest: credit is subsidized. Similarly, some saving contracts yield a negative interest: subjects pay to save. This latter feature seeks to mimic the fact that savings instruments made available to the poor often yield a negative return, either because of fees and charges (e.g., Dupas and Robinson (2013a)), or because of inflation. More generally, the variation in total remuneration allows us to understand subjects' willingness to pay for such products.

In the second phase, we drew our sample from past and current female NRSP clients, whether or not their household owned a business. Following guidance from local partners, we decided for this broader sample to use more payments, with smaller amounts: specifically, we set  $N = 8$  and  $M = PKR 500$ . In these sessions, the lumpsum takes three values:  $PKR 3500$ ,  $3200$ , or  $3800$ . As in the first phase, the middle value is equivalent to setting a zero interest rate, as in a standard ROSCA contract. The other two values are equivalent to adding or subtracting 8.6% to the total installments paid by the participant — slightly less than the 10% used in the first phase.

## 2.2 Behavioural features: Flexibility and reminders

The experimental literature on microfinance has examined how contractual flexibility affects repayment. With some exceptions (e.g. [John \(2018\)](#)), there has been almost no research on the demand for either flexibility or hard commitment among clients of MFIs in developing countries. With the aim of filling this gap, we introduced three additional treatments to the second experimental phase — one treatment to reduce flexibility, one to increase it and one treatment to remind respondents to repay. These treatments are added to the basic contract of some subjects; other subjects receive just the basic contract described in the previous sub-section. Our main research focus is on the *demand* for reminders and for contractual rigidity/flexibility. Consequently, these treatments are always introduced to subjects before take-up. In other words, when a subject decides to accept a contract offer, she knows all the details of the contract that is being offered.

### 2.2.1 Reminders

Reminders are the most common form of soft commitment studied in the behavioral literature on savings. Their purpose is to help participants follow their regular schedule of payments. In our experiment, we send reminders one day before an instalment is due. Reminders are transmitted through phone messages. In ‘respondent reminder’ treatment, the reminder is sent directly to the participant; in the ‘peer reminder’ treatment, the reminder is sent to a family member of the participant. Subjects are told that the financial product offered to them includes reminders. For instance, if a subject is assigned to a respondent reminder treatment in the first product cycle, she is told that she will receive a reminder before each installment is due.<sup>5</sup> This is different from other experiments that have externally introduced reminders and observed how these reminders affect payment patterns (see, for example, [Karlán, McConnell, Mullainathan, and Zinman \(2016\)](#) who introduce reminders via letters and text messages). Here we investigate whether subjects are more willing to accept a financial contract that includes reminders.

### 2.2.2 Commitment features

Our commitment arm involves either adding a cancellation fee (we term this the ‘Sunk’ treatment), or adding additional contractual flexibility (we term this the ‘Flex’ treatment).

The Sunk treatment adds a cancellation fee of *PKR* 500 for defaulting on a contract. This

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<sup>5</sup> The experimental protocol stipulates that subjects are told: ‘To help you commit to a regular schedule of payments, we will call you on the day before an instalment is due. . . This call will be directed to you personally, on a phone number that you will provide to us if you take up the product.’ For peer reminders, the text is: ‘To help you commit to a regular schedule of payments, we will call a member of your family on the day before an instalment is due.’

penalty is added to the total amount owed by the participant to the bank. If subjects demand harder commitment contracts, we expect more take-up in this treatment. How this penalty operates depends on whether the contract is a credit contract (i.e., lumpsum paid in Week 1) or a savings contract (lumpsum paid in Week  $N$ ). To recall, in case of default in a savings contract, our implementing partner NRSP returns to the subject in Week  $N$  all the installments paid before defaulting. For instance, if a subject has paid three instalments totalling  $PKR$  1500 then defaults, this subject receives  $PKR$  1500 in Week  $N$  in the standard savings contract, but only  $PKR$  1500 minus the cancellation fee, that is,  $PKR$  1000 in the Sunk treatment. This is equivalent to making the first instalment ‘sunk’ (e.g., [John \(2018\)](#)). In case of default in a credit contract, the remainder of the debt becomes immediately due. For instance, if a subject had repaid  $PKR$  1500 on a  $PKR$  3500 loan granted in Week 1 but stops paying in week 5, the unpaid portion of the loan becomes due in that week, i.e.,  $PKR$  2000. In the Sunk treatment, the cancellation fee of  $PKR$  500 is added to this amount.

In the Flex treatment, in contrast, more repayment flexibility is added to the contract. In this treatment, we give the participant the flexibility of delaying *one* installment by *one* week only.<sup>6</sup> To illustrate, the subject may decide not to pay the installment  $PKR$  500 in Week 3. In this case, the subject will have to pay the regular instalment of  $PKR$  500 in Week 4 plus the delayed instalment of  $PKR$  500 from Week 3 – i.e., a total of  $PKR$  1000 in Week 4. Other instalments remain unchanged. Note that the subject in the Flex treatment decides when to use the option to delay an instalment. It can be applied to any instalment between the first instalment and the last — or to none at all. All other rules regulating default continue

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<sup>6</sup> Subjects are told that “We understand that it is not always possible to pay installments every week. Therefore, over the course of eight weeks, we will allow you on one occasion only to delay a payment by one week.”

to apply.<sup>7</sup>

### 2.3 Implementation

The first phase was conducted from 25 August 2014 to 1 March 2015 in two districts of Pakistan Punjab: Bhakkar and Chakwal. The endline survey was completed by 30 March 2015. The second phase was implemented from October 2015 to May 2016 in four districts of Punjab: Jhelum, Rawalpindi, Khushab and Mandi Bahuddin. The endline survey was carried out after Ramadan, in July-August 2016.<sup>8</sup>

Participants are drawn from clients of microfinance products offered by NRSP. All subjects are women. In the first phase, participation is restricted to past and current clients whose household own at least one business. In the second phase, this restriction does not apply. The implementation of the experiment was carried out by NRSP field staff. Table 2 shows the districts, offices and sample size that was included in the two phases of the experiment.

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<sup>7</sup> In the Sunk and Flex treatments, we experimented with an additional level of randomization aimed at better identifying demand for harder or softer commitment. The intention was to ask some subjects to draw a wildcard after having accepted or rejected the contract offered. Depending on the wildcard they drew, subjects would be offered an alternative contract, and asked whether they would prefer this alternative to the contract they just accepted or rejected. Once the lumpsum week and interest cards were drawn by the participant and the take-up decision has been recorded, the participant would be asked to draw another card that would offer a chance of a revision in contract terms. If a contract revision was offered, the participant would have the option to choose between her original offer and the revised offer.

Out of 12 possible cards, half do not offer any change in the contract. In the Sunk treatment, the remaining cards offer to waive the cancellation fee in return for a reduction in the lump sum payment, which ranges, depending on the card, from PKR 100 to PKR 500. In the Flex treatment, the cards remove the flexibility of missing a payment in return for an increment in the lumpsum payment ranging between PKR 0 and PKR 250, depending on the card. In practice, this part of the experimental protocol proved too complicated and time consuming for NRSP staff to implement, and it had to be abandoned.

<sup>8</sup> Funding for the first phase was provided by the International Growth Centre (IGC); funding for the second phase was provided by the Economic Social Research Council (ESRC).

< Table 2 here. >

In Table 3, we summarize the experimental design, and report the share of participants assigned to each treatment. In Phase 1, we used a simple treatment/control division (with 50% of our sample in each). In Phase 2, we assigned 25% of participants to the control group; the remaining 75% were then assigned in a  $3 \times 3$  factorial design, covering all combinations of (i) sunk, flex or no commitment feature, and (ii) respondent reminders, peer reminders and no reminders. Subjects were told whether they are in a Sunk, Flex or Reminder treatment before drawing the cards determining contract terms.<sup>9</sup> The combination of treatment dimensions results in a total of six different generalized ROSCA contracts in the first phase (that is, the six combinations of interest rate and time of lumpsum payment), and 54 different generalized ROSCA contracts in the second phase (the six combinations from the first phase interacted with the nine treatment cells). We do not have sufficient statistical power to study the effect of each possible combination of treatments separately, but this was never the intention of the experimental design: rather, in the empirical analysis, we use the orthogonality of treatment assignment to examine different treatment dimensions separately.

< Table 3 here. >

We assigned participants to these various treatments by stratified randomization.<sup>10</sup> We

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<sup>9</sup> This means that individuals who reject the contract before cards are drawn do so knowing whether the contract would include a Sunk or Flex component or a Reminder component.

<sup>10</sup> We stratify by: (1) dividing participants into blocks based on their answers to questions on 'running a business', 'whether the respondent makes the final decision on spending' and 'whether the respondent would use a loan for investment'; then (2) sorting within blocks by household income; then (3) assigning different treatments to those individuals with a similar income in the same block.

fielded baseline and endline surveys on both treated and control subjects. In the second phase of sessions we also conducted phone surveys to gather higher frequency information about short list of indicators, with the view of more precisely estimating any potential impact of treatment on business outcomes.

In practice, assignment to treatment was implemented by inviting participants to draw a card at random at the beginning of a cycle in order to determine the interest charge (i.e., zero, negative, or positive) and the week of the lumpsum payment (Week 1 or Week  $N$ ). The card is drawn in Week 0, at which time subjects are asked whether they take the contract or not. If they do, they are invited to come back a week later to start the contract proper. Inserting a week between the acceptance of the contract and any initial payment is done both for logistical reasons, and to minimize the effect of pure present bias on take-up.

Tables [A1](#) and [A2](#) describe main characteristics of the sample in the two phases. Monthly household consumption averages PKR 25,000 (\$250) in Phase 1 and PKR 20,000 (\$200) in Phase 2. A large proportion (60%) of the sample in Phase 1 is self-employed but this proportion is much smaller in Phase 2. On average, respondents in the two samples have low levels of decision making power in the household and report finding it difficult to save. Tables [A1](#) and [A2](#) also report  $p$ -values for randomisation balance across treatments. This is done by regressing each variable on the assigned treatment status in a saturated specification. We also test for randomisation balance across contract terms, using a similar saturated specification that regresses each variable on randomly assigned interest rate and week of payment. We find strong balance across treatment status and contract terms in

Phase 1. We find 4 variables to be unbalanced at the 90% confidence level.<sup>11</sup>

### 3 Demand for the product

Our empirical analysis — both in this section and in the next section — follows two Pre-Analysis Plans.<sup>12</sup> Here we combine the findings from both phases of sessions and summarize the main results. We start by documenting the effect of the various treatments on product take-up.

#### 3.1 Average take-up rates

We start by documenting average take-up frequencies for the six combinations of interest charge and lumpsum week offered in the two phases of experimental sessions. To do this, we estimate only among treated respondents (that is, we omit respondents in the control groups, because they were not offered the contracts). Take-up frequencies are obtained by estimating a linear probability model of the form:

$$a_{it} = \sum_{w=1}^2 \sum_{r=1}^3 \beta_{wr} \cdot T_{it}^w \cdot T_{it}^r + u_{it}, \quad (1)$$

where  $a_{it} = 1$  if individual  $i$  accepts the contract in cycle  $t$  and 0 otherwise. Variables  $T_{it}^w$  and  $T_{it}^r$  are dummies equal to 1 if individual  $i$  in cycle  $t$  is offered a contract with payment

<sup>11</sup> As a robustness check, we re-run the main estimations with these four variables as additional controls but results are unaffected

<sup>12</sup> We filed separate Pre-Analysis Plans for the first and second phases of the experiment. For Phase 1, we filed a Pre-Analysis Plan on 10 May 2015; this is available at <https://www.socialscienceregistry.org/trials/684>. This pre-analysis plan was followed in the report submitted to the donor agency (IGC) in March 2016. For Phase 2, we filed a Pre-Analysis Plan on 15 January 2017; this is available at <https://www.socialscienceregistry.org/trials/1916>. This PAP is a straightforward description of the treatment-by-treatment presentation of results. It was followed in a presentation to academics and policymakers in Lahore in April 2017.



in week 1 or  $N$  and with a negative, zero or positive interest added to the lumpsum.<sup>13</sup> Standard errors are clustered at the individual level.

< Table 4 here. >

Results for all subjects from both experimental phases appear in the top panel of Table 4; in the bottom panel, we exclude ‘automatic refusers’ — that is, respondents who refused the contract before learning the contractual terms.<sup>14</sup>

Focusing on the top panel, we first note that, in both phases, take-up is positive for all six contracts.<sup>15</sup> Take-up responds to contract terms: demand for the product is higher when payment is in week 1 instead of in week  $N$ ; and demand is higher when the lumpsum is larger. These differences are all statistically significant at the 1% level. Comparing the

<sup>13</sup> In phase 2 some subjects said they were not interested in any contract and consequently refused to select a card to determine  $T_{it}^w$  and  $T_{it}^r$ . There are two ways we could have captured this in the estimation. We could have assigned treatment dummies at random to each of these observations and estimated the take-up model (1) as such. Alternatively, we could recognize these subjects refused all six possible contracts – and thus account for six take-up observations. This is the approach we take in this paper. However, simply adding six observations for each refusal would give too much weight to these individuals, we instead give each of them a weight of 1/6. This approach is equivalent, in expectation terms, to picking  $T_{it}^w$  and  $T_{it}^r$  for these subjects with a probability 1/6. But it yields a better balanced sample that does not require ex post randomization and incorporates all the available information in a consistent manner.

<sup>14</sup> The proportion of automatic refusers in each cycle of each phase is reported in Table A3. Across all three cycles, automatic refusers account from one third of phase 1 observations, and two fifths of phase 2 observations. In phase 1, the proportion of automatic refusers increases slowly across cycles; in phase 2, the proportion of automatic refusers is twice as high in the first cycle than in the other two. 58% and 29% of subjects are never automatic refusers in phase 1 and 2, respectively. Automatic refusers tend not to refuse in all product cycles: the proportion of those who always refuse automatically is 25% and 20% in phase 1 and 2, respectively.

<sup>15</sup> The figures for Phase 2 refer to all subjects, regardless of the additional contractual features. If we only consider subjects who were offered the basic contract — that is, without reminders, Sunk or Flex treatments, we obtain qualitatively similar results, both with and without automatic refusers, although average take-up is slightly higher in the basic treatment.

impact of interest rate on take-up between credit than saving contracts, we see a larger sensitivity of demand to the size of the lumpsum for the former, consistent with the existing evidence (Karlan, Morduch, and Mullainathan, 2010).

We observe behaviors consistent with a pure demand for saving commitment. For instance, 2.7% of participants in phase 1 and 4.1% in phase 2 take up saving commitment contracts with a fee — i.e., for which the lumpsum is less than total installments  $M \cdot (N - 1)$ . In both cases, participants could in principle accumulate the instalments themselves and end up with more money.<sup>16</sup> Other behaviors are consistent with a difficulty to save independently: a large proportion of subjects, 53% and 62.8% in phase 1 and 2 respectively, refrain from taking a subsidized credit contract.<sup>17</sup> In both cases, subjects could have taken the loan, paid back the instalments, and be left with the subsidy. The only reason for not doing so is having to hold onto the funds to pay the instalments. All these results are consistent with earlier findings obtained by Afzal, d’Adda, Fafchamps, Quinn, and Said (2017) using a similar contract design but a much shorter contract duration.

It is possible to construct standard models of saving and borrowing — that is, models in which respondents do not have a preference for commitment savings — in which participants accept a saving contract with a fee, or refuse subsidized credit. However, the implied discount rates for such a model are so extreme as to be implausible. To illustrate, consider the week 1 frequencies, i.e., the take-up of loans. The change in take-up rates over experimental interest rates implies that 8.2% of phase 1 subjects have annual compound discount

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<sup>16</sup> In the first phase, these contracts offered a payment of PKR 4500 in week 6 after five payments of PKR 1000; while in the second phase they paid PKR 3200 in week 8 after 7 installments of PKR 500.

<sup>17</sup> Under these contracts, subjects in would receive PKR 5500 in week 1 in exchange for PKR 5000 in 5 installments of PKR 1000 phase 1; and PKR 3800 in week 1 in exchange for PKR 3500 in seven instalments in phase 2.

rates higher than 128%, and 53% of them have *negative* discount rates higher than 50%, with remaining subjects lying in between these two extremes. Similar patterns are observed in phase 2.

This is not all. If we turn to take-up of savings contracts, we get completely different implied discount rates if we believe our subjects follow a standard model of borrowing and saving. For instance, the fact that 89% of phase 1 subjects turn down an opportunity to save at an annual compound interest of 128% (10% over 6 weeks) would imply that these subjects have a discount rate higher than 128% – a number that is much larger than the 8.2% of subjects who take a loan at that same interest rate. A discrepancy of the same magnitude is found in phase 2. The same contradiction arises for those subjects exhibiting negative discount rates: 2.7% (4.1%) of phase 1 (2) subjects appear to have a negative discount rate higher than 60%, compared to 53% (62.8%) based on loans.

The results excluding automatic refusers are similar in terms of sensitivity to contract terms and in terms of discrepancies in the implied distribution of discount rates across saving and credit contracts. It is reasonable to assume that automatic refusers include all the subjects who are uninterested in our contracts because of the implied transaction costs. It follows that the discrepancies in discount rate distribution observed across the saving and credit frames are not an artefact of transaction costs.

Even more damaging for a standard model with stable time preferences is the fact that the same subjects often accept — or reject — both a saving and a credit contract with comparable terms. This is summarized in Table 5. In the first panel of the table, we consider individuals who take a loan charging a positive interest — implying impatience — as well

as a savings contract with a zero or negative return — implying a strong desire to postpone consumption. After dropping automatic refusers (for whom contract terms were not drawn), we have 107 individuals in phase 1 and 350 in phase 2 who were offered each type of contract at least once during one of the three product cycles. In both cases, the majority of subjects rejected both contracts — in line with the generally low take-up of low payout contracts documented in Table 4. Of those who take at least one loan contracts with zero or positive interest, 20 to 24% also take the savings contract. Similarly, of those who take at least one saving contract with a negative return, 67 to 68% also take a loan contract with a zero or positive interest. This shows that most individuals who take up a negative return savings contract also take a credit contract — suggesting that credit, for them, serves the same lumpsum accumulation role as credit.

< Table 5 here. >

In the second panel of Table 5 , we consider individuals who accept either of the two costly contracts discussed in Panel 1, while at the same time refusing a credit contract with a negative interest — i.e., a contract paying *PKR* 5500 (Phase 1) or 3800 (Phase 2) in week 1. We have already argued that refusing such a contract violates the standard model — except for the existence of transaction costs. We have 101 individuals in phase 1 and 399 individuals in phase 2 who are offered both types of contracts, i.e., a negative interest credit contract and a contract (credit or saving) with a low payout. As noted earlier, the majority of individuals who are offered the negative interest credit take it. There is, however, a non-negligible fraction of the subjects who refuse this contract. Of those, all take the lower payout contract. Similarly, among those who take the low payout credit or savings contract,

two thirds refuse the attractive credit contract. In spite of the different sample sizes, the proportions are almost identical in the two subject populations.

Taken as a whole, the evidence is impossible to reconcile with a standard model of borrowing and saving in which the distribution of discount rates is stable, reasonable, and identical for borrowing and lending. The fact that take-up is higher for credit contracts with a low or negative interest suggests that subjects prefer more money at an earlier date. But many are also willing to take less advantageous contracts, while others are unwilling to accept even the most generous contract. As shown by [Afzal, d'Adda, Fafchamps, Quinn, and Said \(2017\)](#), this behavior is consistent with a model in which individuals have a preference for lumpsum accumulation but are unable to save without contractual commitment. When their demand for lumpsum accumulation is high enough, they are more likely to accept a commitment contract, provided the effort to save is not too high, i.e., provided that the interest charge is low enough and the timing of lumpsum disbursement early enough. When the need for lumpsum accumulation is low, subjects refuse any contract, even those with a credit subsidy.

### **3.2 Demand for behavioural features**

We now turn to the reminder and commitment treatments. We have seen that the behavior of many participants is consistent with a demand for lumpsum accumulation and an inability to save at home, which leads them to accept contracts that commit them to the payment of a sequence of regular installments. Since this behavior indicates a demand for commitment contracts, we are interested in finding out whether these same participants

are also interested in contracts with additional commitment features, such as receiving reminders or facing a cancellation fee. The literature has shown that such features reduce the probability of default. What is less clear is whether subjects have an ex ante demand for these additional commitment features. At the same time, we wonder whether subjects might benefit from the option to postpone an installment to deal with an emergency. If this is the case, we expect a higher take-up when this option is included in the contract. Further, we are able to test whether these contractual features are valued differently when the participant has to pay to save up for to a lumpsum amount in a savings product or when they have to pay down to repay a lumpsum provided under a standard credit framework.

Table 6 presents the result from this investigation. The top panel of the table presents average take-up for each of the nine treatment combinations included in the experiment. This average is estimated using an OLS regression of take-up on dummies for the nine combinations of reminder and commitment treatments, as well as dummies for the six combinations of payment week and interest rate. Standard errors are clustered at the individual level and observations from all three product cycles are combined.<sup>18</sup> To ascertain whether differences between treatments are significant, we report in the second panel of Table 6 pairwise significance tests for various treatment comparisons.

< Table 6 here. >

The table shows that, contrary to what one might expect, clients do not value additional commitment features. Indeed of the eight variations on the 'basic, no reminders' product,

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<sup>18</sup> Automatic refusers are included in the analysis, setting  $a_{it} = 0$ ; they had been informed of the reminder and commitment treatment before deciding whether to take up the contract.

demand is lower in seven cases; in two of these cases ('sunk, no reminders', and 'sunk, reminder to peers'), there is a demand reduction of more than 25% (i.e. 5 percentage points), and is significant: see Panel 2A. This shows that clients actively dislike the additional penalty implied by the 'sunk' case.

Panels 2B and 2C show respectively the marginal effects of flexibility and of reminders. Relative to a basic contract, the addition of flexibility increases demand only when coupled with the option of reminders to the respondent (Panel 2B). On their own, reminders reduce take-up, especially when they are sent to peers. Adding reminders to the respondent has a significant effect on demand only when coupled with the added contractual flexibility (Panel 2C). This seems to suggest that reminders are more valued *ex ante* when the contract is more flexible — perhaps because subjects feel that the lower level of commitment needs to be compensated by reminders. This interpretation finds some additional support in the fact that reminders have no effect on take-up in the Sunk treatment. When reminders are sent to peers, the positive effect on take-up in the Flex treatment is smaller in magnitude and no longer significant.

In Tables 7 and 8, we exploit the fact that our experimental design randomly assigned subjects to be offered either credit contracts or savings contracts; this allows us to test how demand for behavioural features differs between loan contracts and savings contracts. The results are stark: all of the heterogeneity in demand for behavioural features is driven by respondents in the credit domain. Table 7 provides average take-up rates for credit products. Take-up of credit products falls significantly in both the Flex and Sunk treatments compared to the basic treatment with no reminders. Cancellation fees significantly reduce take-up, particularly when combined with reminders sent to peers. Take-up of flexible

credit contracts where reminders are provided to the participant is significantly higher compared to a basic contract with no reminders.

Table 8 provides a similar analysis for savings product. Take-up is unaffected by additional behavioral features in the savings domain: we cannot reject a null hypothesis that we have equal demand across all contract types ( $p = 0.321$ , Panel 1). These results imply that commitment devices such as penalties or reminders have no value when making a commitment to a regular savings schedule, but they are relevant when people have a lumpsum amount they need to repay.

< Table 7 here. >

< Table 8 here. >

Based on these findings, it appears that the repayment structure built into the savings contract offers the right amount of commitment, and that individuals do not demand additional behavioral nudges. Perhaps subjects anticipatively dislike the stress associated with reminders and with the risk of incurring a cancellation fee, or they fail to recognize ex ante that these features will help them stick to their commitment.<sup>19</sup> Subjects, used to the structure of ROSCA contracts (on which our basic contract is tailored), may find the additional contractual stipulations pointless, and hence off-putting.

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<sup>19</sup> The latter interpretation is consistent with subjects being partially sophisticated, that is, sophisticated enough to realize they need a commitment device, but not so sophisticated that they realize how much they need commitment devices (e.g., [DellaVigna and Malmendier \(2006\)](#)).



### 3.3 Product demand and commitment needs

To add further context to these results, we now provide a brief descriptive analysis of how participant characteristics correlate with product take-up. Tables 9 and 10 provide average characteristics of participants in Phase 1 and Phase 2 who never accept the product offered and those who accept it at least once. Tables A4 - A7 in the Appendix provide a similar analysis for each of the four behavioral features - Flex, Sunk, reminders to respondent and to peers.

Existing NRSP borrowers are significantly more likely to take-up the product in both Phase 1 and 2. Demand may be motivated by familiarity with the organization or because the lumpsum payment can help in repayment of existing loans. For Phase 2, it is also possible that additional commitment features help keeping track of when instalments are due. Findings indicate the latter is likely true in our sample – respondents with existing NRSP loans display higher demand for commitment but show no preference for flexible features that reduce commitment (Tables A4 - A7). Demand is lower among participants with informal, and probably more flexible, loans that need to be repaid to family and friends. We find that flexible features are valued by participants who are household heads – with greater responsibility over household financial circumstances, and among self employed women with irregular income (Table A4). As seen in Afzal, d’Adda, Fafchamps, Quinn, and Said (2017), households with irregular income have a greater demand for a lumpsum, perhaps because income from other sources is not readily available for lumpy purchases.

< Table 9 here. >

< **Table 10** here. >

In Phase 1, demand is uncorrelated with household consumption or respondent literacy level. In Phase 2, where we offer a product with additional behavioural features, the correlation between take-up on average and education level is negative; as is the one between take-up and performance in numeracy and working memory tests. In addition, demand is higher among women who find it hard to save in Phase 2 – on their own or because they are under pressure to share resources with the rest of the household, and among women who report difficulties in keeping track of their finances. We ask a number of questions to measure financial discipline in Phase 2. Specifically, participants with poor financial discipline show a marked preference for the stricter commitment device in the Sunk treatment and for reminders provided to peers to keep track of the payment schedule. Reminders have an added effect – they can help credibly inform household members of the participants' financial commitments. It is interesting to note from Tables [A6](#) and [A7](#), that reminders are preferred by women who report finding it difficult to save because of pressure to share from others and by women who think it is appropriate for women to make small consumption and investment decisions on their own. From these results, it appears that behavioural features may help reduce cognitive load associated with making timely payments, impose financial discipline and help reduce some of the pressures women face in their household over sharing their resources.

## 4 Contract features and repayment

How do contract features affect repayment? For phase 1 subjects, the only variations are on the week at which the lumpsum is paid out — either week 1 or week 6 — and on the amount of the lumpsum — either PKR 4500, 5000 or 5500. Contracts for which the lumpsum is paid upfront operate like credit while contracts paid at the end operate like commitment savings.

We show in Table 11 the frequency of repayment difficulties encountered by NRSP staff on each of the contracts taken up by respondents.<sup>20</sup> The first panel focuses on basic contracts with no reminders, which are common to both phases. It is immediately apparent that contract performance is less satisfactory in commitment saving contracts. In phase 1, repayment difficulties are on average 21 percentage point more likely in such contracts, conditional on take-up which, as we have seen earlier, is itself much lower than for credit contracts. In phase 2 the difference is smaller — 17.3% instead of 14.5% — but it is nonetheless present.

< Table 11 here. >

Why this is the case is probably the combination of two intermingled factors. First, subjects who renege on a commitment saving contract only face mild penalties: their paid-in instalments are kept until the end of the product cycle in week 6, at which point they are returned. In phase 1, there is no Sunk installment that subjects lose by renegeing. Given

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<sup>20</sup> These frequencies are calculated using a simple regression of a repayment difficulty dummy on treatment interaction regressors. The repayment difficulty indicator is equal to 1 if there is at least one late payment. Table 11 presents the resulting average frequencies for each of the contract terms.

this, subjects essentially have the option to walk away from the contract, and this is what 26% of them do on average. Second, default in credit contracts is much lower because NRSP collection effort are much stronger. The logic is simple: the subject has already received the lumpsum, so renegeing is individually optimal for the borrower and thus has to be disincentivized by a concerted debt recovery effort. NRSP is also probably worried that allowing subjects to default on our contracts could have a negative reputation effect that would trickle-down on their main credit activity. Collection efforts may also be more effective because both NRSP and its clients are more familiar with managing loans than savings contracts. For these reasons, default in credit contracts is lower. While these findings are not particularly surprising, they nonetheless bring to light the inherent difficulty of getting a third party to enforce a commitment savings contract, as opposed to a credit contract. This simple dichotomy may go a long way in explaining the predominance of credit contracts in microfinance, in spite of the fact that an important purpose of microfinance is to enable households to save.

The second panel of Table 11 shows the effect of the Flex, commitment and reminder treatments, conditional on take-up. We see that reminders in general reduce the incidence of late payments, without much of a difference between reminders to peers or to the respondent — except in the Flex treatment where reminders to peers are associated with fewer payment delays. If we combined these findings with lower take-up in reminder treatments, it is unclear whether reminders reduce late payments through an incentive effect, or by selecting out borrowers who anticipate more repayment problems. We also note that there is more late payment in the Flex contract, which is anticipated by design. But the incidence of late payment is well below the authorized one installment out of eight: without reminders, Flex clients are late on one payment only 20% of the time, and the average number of late

payments is 0.8. This number falls drastically with reminders. In contrast, late repayment is much less frequent in sunk contracts, especially if combined with reminders. Since sunk contracts also have the lowest take-up, this suggests that subjects may have a demand for commitment, but not one that over-penalizes them for default.

## 5 Impact on business and household outcomes

We now turn to the estimation of the impact of treatment on business and household outcomes. The main focus of this analysis is the comparison of control participants (who were not invited to take-up any of our commitment contracts) with treated participants (who were). We use the following ANCOVA specification:

$$y_{i1} = \beta_0 + \beta_1 \cdot T_i + \beta_2 \cdot y_{i0} + \phi_s + \eta_d + \varepsilon_i, \quad (2)$$

where  $y_{i1}$  denotes an outcome variable of interest measured at endline 1,  $y_{i0}$  is the baseline value of  $y_{i1}$ ,  $\phi_s$  are strata dummies, and  $\eta_d$  are district fixed effects. We cluster errors at the household level.

The variable  $T_i$  takes two interpretations, depending on the specification. First, we denote  $T_i$  as assignment to treatment; in that case, we estimate equation 2 using OLS, and interpret  $\hat{\beta}_1$  as the ITT. Second, we denote  $T_i$  as take-up. This takes four possible values, which depend on whether the subjects takes up the contract in 0, 1, 2 or 3 cycles. In this case, we calculate average take-up at the individual level, and instrument this using assignment to treatment and to contractual terms; we then interpret  $\hat{\beta}_1$  as providing the LATE, normal-

## 5. IMPACT ON BUSINESS AND HOUSEHOLD OUTCOMES

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ized for a case where a respondent takes up in all three product cycles.<sup>21</sup>

Outcomes are divided into two broad categories: business outcomes and household finance and consumption. In Table 12, we report business outcomes; we collect ITT and LATE estimates for both phase 1 and phase 2 samples. In principle, our MFI partner lends for business purposes: it is therefore of primary interest whether our commitment saving contract is able to improve business investment and performance.

< Table 12 here. >

We find almost no significant effect on business and household outcomes of having been offered our treatment; this is consistent with a growing body of evidence on the effects of microfinance (see, for example, Meager (forthcoming) and Meager (2018)). In the phase 1 sample, 60% of respondents have a business. Among these subjects, we find generally positive point estimates on business performance, as measured by investment, sales, or profit. But these point estimates are in general not statistically significant. Two of the ITT coefficients are above the 10% significance level, but only one of the LATE coefficients is

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<sup>21</sup> Specifically, to instrument average take-up, we proceed as follows. First, for each cycle  $s$ , we estimate the predicted take-up of individual  $i$  based on the different types of treatments  $i$  was exposed to in that cycle – i.e., payment week, negative or positive interest, reminders, and Flex or Sunk treatment. This is achieved using the same regression that was used in generating Table 6 for the six combinations of payment week and interest rate — except that it is estimated separately for each cycle. This generates a predicted take-up for each product cycle. The sum over all three cycles is then used as instrument for  $T_i$  when estimating. For automatic refusers, we do not have a specific payment week or interest rate on which to base our prediction – since these subjects refused the contract before cards were drawn. To circumvent this issue, we ascribe to each of these observations the average predicted take-up associated with their commitment and reminder treatment, assuming an average interest rate and payment week. In practice this is achieved, as before, by generating six observations for each refuser, one for each combination of payment week and interest rate, and ascribing a weight of 1/6 to each of these observation when estimating the predicting equation.

## 5. IMPACT ON BUSINESS AND HOUSEHOLD OUTCOMES

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significant, and it is for another dependent variable. In contrast, among the phase 2 sample, estimated treatment effects are small in magnitude and never significant. This may be because a much smaller proportion (12.5%) of these households have a business at baseline.

Results for household material outcomes are presented in Table 13. We find no significant effect on household consumption or household income (the latter being measured only in the phase 2 sample). In the phase 1 sample, we find a large and significant LATE coefficient on total household assets and total individual assets. This encouraging result is, however, negated in the phase 2 sample where we find a large but negative LATE effect on total household assets.

< Table 13 here. >

The bottom part of Table 13 relates to household finances. We see that 75% of control subjects in the phase 1 sample save in a 'committee'. The proportion is smaller in phase 2: 16.6%. We find a positive and significant LATE effect on participation in a committee, but given that the corresponding ITT coefficient is essentially 0, it is unclear how much faith to put in this result. We also find a positive LATE for participation in a committee among phase 2 respondents, but the effect is not statistically significant. The last row of Table 13 reports results for the total debt of the respondent. Our commitment saving product should have helped participants reduce their stock of debt. We find little evidence of this. Among phase 1 subjects, ITT and LATE coefficients are positive but not significant, while among phase 2 subjects the ITT is negative and significant but the LATE coefficient is not.

Finally, we measure the impact on a short list of indicators using higher frequency information from phone surveys conducted at the end of each experiment wave. Table 14 summarizes the results for business and household outcomes. We find generally insignificant effects. There are no significant effect on the likelihood of running a business, the number of businesses or on the value of capital invested in the business in the last one month. Treated participants have higher consumption and lower debt but this difference is never significant.

< Table 14 here. >

## 6 Conclusions

Recent years have produced a wealth of exciting research on commitment problems, including empirical work on the demand for commitment devices in developing countries. In such work, it is often assumed that, if people are aware of their commitment problems, they will welcome the opportunity to take commitment devices. But this need not be the case: depending upon how they are designed, commitment devices can either serve to be supportive and helpful, or can serve instead to patronise and infantilise. Many of us, for example, welcome the implicit commitment in a Pay-As-You-Earn taxation system, or appreciate that consumption taxes are deducted at the point of sale rather than at the end of the year — yet most of us would likely be appalled if the government were to offer us monthly reminders to pay our taxes, or to offer harsher penalties for non-compliance. This basic fact has been long understood in the design of many commitment devices; groups like ‘Alcoholics Anonymous’ or ‘Self-Management and Recovery Training’ seek to provide commitment while at the same time maintaining strong philosophies of respect and mutual



support.

For this reason, the optimal design of commitment features remains a very open question for empirical research. This paper makes progress on that issue, by testing the role of commitment in microfinance. We have done this in two distinct ways. First, we test directly whether the rotating structure of a committee can be implemented as an individual commitment-saving product. In previous pilot work, we established this fact for small product sizes with daily repayments (Afzal, d’Adda, Fafchamps, Quinn, and Said, 2017); in this paper, we show that the same structure can be used for a product with much larger payments, over a longer period. Further, we find substantial demand for such a product. Specifically, we find that many microfinance clients ‘borrow to save’ (Collins, Morduch, Rutherford, and Ruthven, 2009; Armendáriz and Morduch, 2010; Bauer, Chytilová, and Morduch, 2012; Kast and Pomeranz, 2018; Afzal, d’Adda, Fafchamps, Quinn, and Said, 2017). But we also find that take-up is much higher for credit contracts than for commitment savings contract. In addition, we find a significantly higher incidence of repayment difficulties with commitment savings contract, and a lower willingness of MFI staff to enforce such contracts.

Second, we then add additional ‘behavioural’ features — in the form of reminders (both for respondents and for respondents’ peers), and in variation of repayment flexibility. Our design allows to compare how demand for these features and their impact varies between the saving and credit domain. Our findings show that all these contract features are not valued by clients, on the contrary, they appear to be actively disliked. The only exception is when we combine flexibility with reminders directed at the respondent. All the variation in demand of the behavioral features is driven from the credit domain. Our results suggest

that hard commitment features and reminders may be preferred by clients who have difficulty in imposing financial discipline or those who feel pressure to share resources with others.

These results have important policy implications for thinking about the future design of microfinance products. Our results imply, first, that microfinance institutions should *not* be seeking to build additional commitment features into their products — not because their clients do not have a demand for commitment devices, but because that demand is already met through the regular repayment schedule required by most microfinance products. Second, our findings imply that the same set of contract features may have very different impact on demand and financial discipline in the credit and in the saving domain.

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# Tables

Table 1: An illustrative contract structure

	WEEK 0	WEEK 1	WEEK 2	WEEK 3	WEEK 4	WEEK 5	WEEK 6
Participant pays			1000	1000	1000	1000	1000
Bank pays		4500					

*This table shows a payment schedule for a basic contract with lump-sum in Week 1 and interest of  $-10\%$ .*

**Table 2: Sample structure across phases and locations**

	DISTRICT	OFFICES	RESPONDENTS
<i>Phase 1</i>	Bhakkar	3	418
	Chakwal	5	372
<i>Total</i>		8	790
<i>Phase 2</i>	Khushab	5	725
	Mandi Bahauddin	4	674
	Jhelum	6	296
	Rawalpindi	2	721
<i>Total</i>		17	2416

*This table shows the breakdown of our 3206 respondents, between Phase 1 (790 respondents) and Phase 2 (2416 respondents).*

Table 3: **Structure of treatments**

<u>Phase 1</u>		
<b>Basic treatment (1/2)</b> n = 394		
<b>Control group (1/2)</b> n = 396		
<u>Phase 2</u>		
<b>Basic treatment with no reminders (1/12)</b> (n = 197)	<b>Basic treatment with respondent reminders (1/12)</b> (n = 204)	<b>Basic treatment with peer reminders (1/12)</b> (n = 199)
<b>Sunk treatment with no reminders (1/12)</b> (n = 201)	<b>Sunk treatment with respondent reminders (1/12)</b> (n = 202)	<b>Sunk treatment with peer reminders (1/12)</b> (n = 207)
<b>Flex treatment with no reminders (1/12)</b> (n = 202)	<b>Flex treatment with respondent reminders (1/12)</b> (n = 204)	<b>Flex treatment with peer reminders (1/12)</b> (n = 198)
<b>Control group (1/4)</b> n = 602		

*This table shows the structure of treatments: a simple treatment/control division in Phase 1, and a 3 × 3 factorial design with controls in Phase 2. In each case, the fractions (1/2, 1/4 and 1/12) show the proportion of the respondents in the phase who were intended for assignment; in each case 'n' refers to the actual number assigned.*



Table 4: Average take-up by contract terms

**ALL SUBJECTS**

		<i>Lumpsum amount</i>		
		4500	5000	5500
<b>Phase 1</b>	<i>Lumpsum paid in</i>			
	Week 1	8.2%	30.2%	47.0%
	Week 6	2.7%	4.3%	11.0%
		<i>Lumpsum amount</i>		
		3200	3500	3800
<b>Phase 2</b>	<i>Lumpsum paid in</i>			
	Week 1	11.0%	26.0%	37.2%
	Week 8	4.1%	8.9%	11.3%

**EXCLUDING AUTOMATIC REFUSERS**

		<i>Lumpsum amount</i>		
		4500	5000	5500
<b>Phase 1</b>	<i>Lumpsum paid in</i>			
	Week 1	12.8%	43.1%	64.2%
	Week 6	4.3%	6.8%	17.6%
		<i>Lumpsum amount</i>		
		3200	3500	3800
<b>Phase 2</b>	<i>Lumpsum paid in</i>			
	Week 1	20.0%	41.9%	57.0%
	Week 8	7.2%	14.6%	19.3%

*This table shows the average take-up rates by contractual terms (lumpsum value and timing). Weekly instalments were PKR 1000 in Phase 1 and PKR 500 in Phase 2. 'Automatic refusers' refers to respondents who declined the contract even before knowing the contractual terms on offer.*

Table 5: **Proportion of individuals who contradict a standard model**

	Phase 1	Phase 2
<i>Subjects were offered at least one loan charging a zero positive interest AND a savings contract with a zero or negative return:</i>		
took neither	76	225
took the loan	30	102
took the saving contract	7	47
took both	6	24
<i>Total:</i>	107	350
conditional on loan, take saving	20%	24%
conditional on saving, take loan	86%	51%
<i>Subject was offered at least one loan charging a negative interest AND at least one loan charging a zero or positive interest OR a savings contract with a zero or negative return:</i>		
took both	6	39
took the subsidized loan	89	315
took the loan or saving contract	18	123
took neither	0	0
<i>Total:</i>	101	399
conditional on taking a loan or saving contract, refused subsidized loan	67%	68%
conditional on refusing a subsidized loan, took a loan or saving contract	100%	100%

*This table reports the proportion of individuals whose take-up behaviour would contract a standard model, using two descriptive tests. Automatic refusers are omitted from these calculations.*

Table 6: Average take-up by contract terms

**Panel 1: Average take-up in each treatment combination**

	Flex	Basic	Sunk
No reminders	16.0%	20.1%	14.5%
Reminder to self	22.2%	16.9%	18.5%
Reminder to peers	16.2%	16.6%	14.6%

Joint equality test (*p*-value): 0.087\*

**Panel 2: Pairwise take-up comparisons**

**A. Difference from basic contract with no reminders**

	Flex	Basic	Sunk
No reminders	-4.1%	<i>reference</i>	-5.6%**
Reminder to self	2.1%	-3.2%	-1.6%
Reminder to peers	-3.9%	-3.5%	-5.5%**

**B. Difference from basic contract**

	Flex	Basic	Sunk
No reminders	-4.1%	<i>reference</i>	-5.6%**
Reminder to self	5.3%**	<i>reference</i>	1.6%
Reminder to peers	0.4%	<i>reference</i>	-2.0%

**C. Difference from no reminder contract**

	Flex	Basic	Sunk
No reminders	<i>reference</i>	<i>reference</i>	<i>reference</i>
Reminder to self	6.2%**	-3.2%	4.0%
Reminder to peers	0.2%	-3.5%	0.1%

All the calculations in this Table are based on an OLS regression of take-up on all interactions between reminder and commitment treatments. Interaction terms for payment week and interest rate are included as controls. Standard errors clustered at the household level. We use '\*' to denote confidence at the 90% level. For Panel 2A, *p*-values for pairwise tests come from OLS coefficient estimates. For Panels 2B and 2C, *p*-values come from the relevant pairwise coefficient tests.

Table 7: Average take-up by contract terms: Credit contracts

**Panel 1: Average take-up in each treatment combination**

	Flex	Basic	Sunk
No reminders	22.2%	31.2%	23.1%
Reminder to self	33.5%	25.8%	26.5%
Reminder to peers	25.7%	25.9%	18.7%

Joint equality test (*p*-value): 0.011\*\*

**Panel 2: Pairwise take-up comparisons**

**A. Difference from basic contract with no reminders**

	Flex	Basic	Sunk
No reminders	-8.9%**	<i>reference</i>	-8.1%*
Reminder to self	2.3%	-5.4%	-4.7%
Reminder to peers	-5.5%	-5.3%	-12.5%***

**B. Difference from basic contract**

	Flex	Basic	Sunk
No reminders	-8.9%**	<i>reference</i>	-8.1%*
Reminder to self	7.7%*	<i>reference</i>	0.7%
Reminder to peers	-0.2%	<i>reference</i>	-7.2%*

**C. Difference from no reminder contract**

	Flex	Basic	Sunk
No reminders	<i>reference</i>	<i>reference</i>	<i>reference</i>
Reminder to self	11.2%***	-5.4%	3.4%
Reminder to peers	3.4%	-5.3%	-4.4%

All the calculations in this Table are based on an OLS regression of take-up on all interactions between reminder and commitment treatments. Interaction terms for payment week and interest rate are included as controls. Standard errors clustered at the household level. We use '\*' to denote confidence at the 90% level. For Panel 2A, *p*-values for pairwise tests come from OLS coefficient estimates. For Panels 2B and 2C, *p*-values come from the relevant pairwise coefficient tests.

Table 8: Average take-up by contract terms: Savings contracts

**Panel 1: Average take-up in each treatment combination**

	Flex	Basic	Sunk
No reminders	9.4%	8.2%	5.4%
Reminder to self	9.9%	7.4%	10.1%
Reminder to peers	6.1%	6.8%	9.9%

*Joint equality test (p-value): 0.321*

**Panel 2: Pairwise take-up comparisons**

**A. Difference from basic contract with no reminders**

	Flex	Basic	Sunk
No reminders	1.2%	<i>reference</i>	-2.9%
Reminder to self	1.7%	-0.8%	1.9%
Reminder to peers	-2.1%	-1.4%	1.7%

**B. Difference from basic contract**

	Flex	Basic	Sunk
No reminders	1.2%	<i>reference</i>	-2.9%
Reminder to self	2.5%	<i>reference</i>	2.7%
Reminder to peers	-0.7%	<i>reference</i>	3.1%

**C. Difference from no reminder contract**

	Flex	Basic	Sunk
No reminders	<i>reference</i>	<i>reference</i>	<i>reference</i>
Reminder to self	0.5%	-0.8%	4.7%
Reminder to peers	-3.3%	-1.4%	4.5%

All the calculations in this Table are based on an OLS regression of take-up on all interactions between reminder and commitment treatments. Interaction terms for payment week and interest rate are included as controls. Standard errors clustered at the household level. We use '\*' to denote confidence at the 90% level. For Panel 2A, p-values for pairwise tests come from OLS coefficient estimates. For Panels 2B and 2C, p-values come from the relevant pairwise coefficient tests.

Table 9: Correlate means of product take-up behaviour - Phase 1

	Never accepted	Accepted	Equality ( <i>p</i> )
Age (years)	37.58	37.09	0.655
Dummy: Married	0.76	0.87	0.009***
Dummy: Education to class 5	0.20	0.21	0.856
Dummy: Education to matric	0.17	0.19	0.659
Dummy: Literate	0.59	0.54	0.377
Household size	5.71	5.98	0.226
Monthly household consumption	24116.58	25575.56	0.261
Dummy: Household head	0.12	0.13	0.809
Dummy: Spouse of household head	0.65	0.71	0.224
Dummy: Self-employed	0.58	0.74	0.001***
Dummy: Has experience in a savings committee	0.51	0.61	0.065*
Dummy: Currently in a savings committee	0.32	0.38	0.251
Dummy: Currently owes family or friends	0.15	0.27	0.011**
Dummy: Currently owes NRSP	0.13	0.25	0.005***
Dummy: Currently owes an MFI	0.10	0.15	0.165
Dummy: Has a bank account	0.15	0.17	0.527
Number of minutes to walk to NRSP	28.31	28.34	0.983
Dummy: Correct on math question	0.50	0.50	1.000
Digit span test score	4.65	4.59	0.671
Dummy: Usually makes final decision on spending	0.80	0.82	0.706
Dummy: Faces pressure to share	0.59	0.58	0.766

Dummy: Finds it hard to save	0.64	0.55	0.097*
Patience measure (higher is more patient)	3.88	3.67	0.261
Patience measure in future frame	3.83	3.75	0.653
Risk aversion measure (higher is more risk-tolerant)	5.49	5.61	0.717

*All the calculations in this Table are based on an OLS regression of respondent characteristic on product take-up. Standard errors clustered at the household level. We use ‘\*’, to denote confidence at the 90% level. Equality test refer to coefficient equality across columns (1) and (2).*

Table 10: Correlate means of product take-up behaviour - Phase 2

	Never accepted (1)	Accepted (2)	Equality test ( <i>p</i> ) (3)
Age (years)	39.03	39.28	0.638
Dummy: Married	0.82	0.80	0.220
Dummy: Education to class 5	0.18	0.17	0.917
Dummy: Education to matric	0.20	0.13	0.000***
Dummy: Literate	0.53	0.46	0.002***
Household size	5.79	5.99	0.048**
Household income last week	2631.00	2747.37	0.354
Monthly household consumption	17845.87	18994.08	0.013**
Dummy: Household head	0.15	0.20	0.017**
Dummy: Spouse of household head	0.69	0.67	0.390
Dummy: Has a wage job	0.12	0.11	0.738
Dummy: Self-employed	0.12	0.15	0.089*
Dummy: Has experience in a savings committee	0.28	0.27	0.722
Dummy: Currently in a savings committee	0.15	0.20	0.024**
Dummy: Currently owes family or friends	0.13	0.10	0.074*
Dummy: Currently owes NRSP	0.30	0.42	0.000***
Dummy: Currently owes an MFI	0.02	0.04	0.160
Dummy: Has a bank account	0.07	0.05	0.081*
Number of minutes to walk to NRSP	35.36	34.30	0.311
Dummy: Correct on math question	0.56	0.47	0.001***



Digit span test score	4.53	4.40	0.025**
Dummy: Usually makes final decision on spending	0.72	0.67	0.059*
Dummy: Faces pressure to share	0.77	0.83	0.002***
Dummy: Finds it hard to save	0.72	0.80	0.000***
Dummy: Good at keeping track of time	0.71	0.67	0.122
Dummy: Follows a tight routine	0.51	0.48	0.350
Dummy: Others remind of appointments	0.48	0.45	0.333
Dummy: Acts early to avoid forgetting	0.50	0.51	0.743
Dummy: Good at keeping track of finances	0.62	0.57	0.065*
Dummy: Follows a strict schedule on finances	0.63	0.58	0.050*
Dummy: Others remind of financial obligations	0.44	0.43	0.520
Dummy: Acts early to avoid forgetting finances	0.48	0.45	0.335
Dummy: Keeps cash earmarked	0.55	0.52	0.207
Dummy: Would immediately spend 100 rupees if found	0.27	0.29	0.302
Patience measure (higher is more patient)	5.58	5.78	0.083*
Patience measure in future frame	5.57	5.82	0.034**
Risk aversion measure (higher is more risk-tolerant)	0.24	0.27	0.020**
Appropriate for a woman to buy a scarf	0.30	0.35	0.036**
Appropriate for a woman to invest in her business	0.25	0.29	0.081*

*All the calculations in this Table are based on an OLS regression of respondent characteristic on product take-up. Standard errors clustered at the household level. We use ‘\*’, to denote confidence at the 90% level. Equality test refer to coefficient equality across columns (1) and (2).*

Table 11: Frequency of repayment problems by contract terms

	Phase 1	Phase 2
<b>1. Credit contract (lumpsum paid in week 1)</b>		
Lumpsum is low	6.7%	15.8%
Lumpsum is average	4.5%	16.4%
Lumpsum is high	5.2%	12.9%
<b>2. Commitment saving contract (lumpsum paid in last week)</b>		
Lumpsum is low	20.0%	14.6%
Lumpsum is average	37.5%	18.9%
Lumpsum is high	21.1%	17.2%
<b><u>Flex contract</u></b>		
No reminders	n.a.	20.0%
Reminder to self	n.a.	17.6%
Reminder to peers	n.a.	13.6%
<b><u>Basic contract</u></b>		
No reminders	n.a.	15.1%
Reminder to self	n.a.	11.4%
Reminder to peers	n.a.	11.1%
<b><u>Sunk contract</u></b>		
No reminders	n.a.	6.9%
Reminder to self	n.a.	5.4%
Reminder to peers	n.a.	5.7%

*This table shows how the proportion of repayment problems varies with contractual terms. In each case, we report coefficients from a linear probability model in which the dependent variable is 1 if there is at least one late payment. (After four late payments, the contract is considered to be in default.) We drop observations where the respondent did not accept the contract; that is, our reported frequencies are conditional on take-up.*

Table 12: Summary of ITT and LATE estimates of business outcomes

	Phase 1		Phase 2	
	Control mean	ITT	Control mean	ITT
Runs a business	0.606	0.016 (0.019)	0.125	-0.009 (0.013)
Number of businesses	1.116	0.087* (0.047)	0.156	-0.002 (0.017)
Value of capital invested in business	7803	610 (607)	2023	-301 (371)
Value of monthly sales	8184	709 (519)	1237	-42 (188)
Value of monthly expenses	6228	152 (452)	502	-42 (82)
Monthly profit (sales - expenses)	1871	737** (353)	665	25 (112)
Monthly profit (self-reported)	2933	518 (329)	869	-23 (130)
Observations		789	789	1991

*This table reports regression estimates of equation 2. We report standard errors under each coefficient in parentheses. All values are in Pakistani rupees. Monthly self-reported profits include the imputed values of business goods consumed. Confidence: \*  $\leftrightarrow$   $p < 0.1$ ; \*\*  $\leftrightarrow$   $p < 0.05$ ; \*\*\*  $\leftrightarrow$   $p < 0.01$ .*

Table 13: Summary of ITT and LATE estimates of household material outcomes

	Phase 1		Phase 2	
	Control mean	ITT	Control mean	ITT
Household monthly consumption	24706	599 (810)	18814	355 (582)
Household monthly income	n.a.		21974	-165 (681)
Value of household assets	46041	3106 (3990)	40821	-3446 (2818)
Value of subject's assets	23151	3007 (2470)	n.a.	
Participates in a committee	0.750	-0.002 (0.046)	0.166	0.005 (0.018)
Total debt of respondent	13300	1987* (1030)	11587	-1670* (901)
Observations		789		1991

*This table reports regression estimates of equation 2. We report standard errors under each coefficient in parentheses. All values are in Pakistani rupees. Confidence: \*  $\leftrightarrow p < 0.1$ ; \*\*  $\leftrightarrow p < 0.05$ ; \*\*\*  $\leftrightarrow p < 0.01$ .*

Table 14: Summary of ITT and LATE estimates of mobile phone data: Phase 2 experiment

	Control mean	ITT	LATE	Observations
<i>Business/employment outcomes:</i>				
Runs a business	0.118	-0.001 (0.005)	-0.006 (0.040)	9115
Number of businesses	0.144	0.002 (0.007)	-0.027 (0.056)	9115
Value of capital invested in business	26.379	5.141 (3.871)	-4.017 (26.322)	9115
Has a wage job	0.124	0.001 (0.005)	0.005 (0.042)	9115
<i>Household material outcomes:</i>				
Value of household assets	18633	-19.876 (791.485)	-246.266 (6460.983)	9115
Household monthly consumption	6737	166.658 (275.633)	-115.492 (1436.094)	9115
Total respondent debt	4147.403	-196.318 (253.633)	-559.797 (2441.105)	9115

*This table reports regression estimates of equation 2. We report standard errors under each coefficient in parentheses. All values are in Pakistani rupees. Confidence: \*  $\leftrightarrow p < 0.1$ ; \*\*  $\leftrightarrow p < 0.05$ ; \*\*\*  $\leftrightarrow p < 0.01$ .*

# Appendix

Table A1: Description of the sample - Phase 1

	<b>N</b>	<b>Mean</b>	<b>Treatment balance (p)</b>	<b>Terms balance (p)</b>
<b>Dummy: participates in a committee</b>	790	0.7	0.176	0.957
<b>Total amount owed by individual (PKR)</b>	790	17695.1	0.281	0.345
<b>Total household consumption in the last month (PKR)</b>	780	25581.9	0.454	0.945
<b>Total value of assets owned by household (PKR)</b>	790	47662.6	0.052	0.357
<b>Dummy: runs a business</b>	790	0.6	0.783	0.341
<b>Number of businesses owned by respondent or household</b>	790	0.9	0.186	0.663
<b>Value of total capital invested in business(es) (PKR)</b>	790	9633.6	0.554	0.310
<b>Total monthly sales of the business (PKR)</b>	790	9602.9	0.591	0.827
<b>Total monthly expense of the business (PKR)</b>	790	6688.4	0.393	0.768
<b>Total monthly profits(1) of the business (PKR)</b>	790	2834.2	0.789	0.234
<b>Total monthly profits(2) of the business (PKR)</b>	789	4029.3	0.785	0.339
<b>Dummy: finds it hard to save</b>	790	0.6	0.144	0.297
<b>Index: opinions taken into account in household decisions</b>	790	-0.0	0.928	0.768
<b>Index: needs to ask permission for making decisions</b>	790	0.0	0.078	0.671
<b>Dummy: faces pressure to share cash on hand</b>	790	0.6	0.523	0.099
<b>Age (years)</b>	790	38.0	0.212	0.157
<b>Dummy: is currently married</b>	790	0.8	0.567	0.774
<b>Number or years of education</b>	790	4.7	0.098	0.220
<b>Dummy: can read and write</b>	790	0.5	0.151	0.717
<b>Number of children</b>	790	3.4	0.096	0.338
<b>Dummy: is the household head</b>	790	0.1	0.937	0.601

*This table provides basic summary statistics for sample characteristics. For each variable, 'Treatment balance' reports a p-value from a joint test of the null hypothesis that the variance is balanced across the treatment status and 'Terms balance' reports a p-value from a joint test of the null hypothesis that the variance is balanced across the contract terms (interest and week of lumpsum payment)*

Table A2: Description of the sample - Phase 2

	<b>N</b>	<b>Mean</b>	<b>Treatment balance (p)</b>	<b>Terms balance (p)</b>
<b>Dummy: participates in a committee</b>	2416	0.2	0.842	0.644
<b>Total amount owed by individual (PKR)</b>	2406	12061.2	0.851	0.060
<b>Total household consumption last month (PKR)</b>	2416	19312.2	0.143	0.169
<b>Total household monthly income (PKR)</b>	2407	19958.2	0.720	0.710
<b>Total value of assets owned by household (PKR)</b>	2416	35546.4	0.713	0.469
<b>Dummy: runs a business</b>	2416	0.1	0.785	0.964
<b>Number of businesses owned by respondent or household</b>	2416	0.2	0.907	0.994
<b>Total capital invested in business(es)</b>	2416	2182.0	0.881	0.318
<b>Total monthly sales of the business (PKR)</b>	2416	1218.8	0.730	0.978
<b>Total monthly expense of the business (PKR)</b>	2416	551.2	0.980	0.991
<b>Total monthly profit(1) of the business (PKR)</b>	2416	617.2	0.256	0.701
<b>Total monthly profit(2) of the business (PKR)</b>	2416	787.3	0.930	0.679
<b>Dummy: finds it hard to save</b>	2416	0.7	0.159	0.244
<b>Index: opinions taken into account in household decisions</b>	2416	0.0	0.042	0.101
<b>Dummy: faces pressure to share cash on hand</b>	2416	0.8	0.003	0.000
<b>Index: needs to ask permission for making decisions</b>	2416	0.0	0.005	0.003
<b>Age (years)</b>	2416	39.1	0.473	0.667
<b>Dummy: is currently married</b>	2416	0.8	0.398	0.346
<b>Number of years of education</b>	2416	4.3	0.098	0.000
<b>Dummy: can read and write</b>	2416	0.5	0.250	0.000
<b>Number of children</b>	2416	3.5	0.704	0.481
<b>Dummy: is the household head</b>	2416	0.2	0.357	0.177

*This Table provides basic summary statistics for sample characteristics. For each variable, 'Treatment balance' reports a p-value from a joint test of the null hypothesis that the variance is balanced across the treatment status and 'Terms*



*balance' reports a p-value from a joint test of the null hypothesis that the variance is balanced across the contract terms (interest and week of lumpsum payment)*

**Table A3: Proportion of treated who refused the product offered before the contract offer cards were drawn**

	Phase 1	Phase 2
Percent of refusers in:		
Cycle 1	26.1%	63.0%
Cycle 2	33.5%	27.7%
Cycle 3	40.1%	29.6%
All cycles	33.3%	40.1%
Percent of subjects refusing:		
Never	58.6%	29.4%
Once	7.6%	40.6%
Twice	9.1%	10.5%
Three times	24.6%	19.6%
Number of treated subjects	394	1814

*This table shows the proportion of the treated who refused the product offered before the cards determining the contract terms were drawn.*

Table A4: Correlate means of Flex product take-up behaviour

	Never accepted (1)	Accepted (2)	Equality test ( <i>p</i> ) (3)
Age (years)	38.75	40.06	0.167
Dummy: Married	0.86	0.82	0.244
Dummy: Education to class 5	0.19	0.17	0.670
Dummy: Education to matric	0.22	0.16	0.080*
Dummy: Literate	0.55	0.46	0.048**
Household size	5.87	5.88	0.953
Household income last week	2602.21	2765.23	0.452
Monthly household consumption	17423.52	19560.07	0.007***
Dummy: Household head	0.14	0.20	0.072*
Dummy: Spouse of household head	0.71	0.70	0.668
Dummy: Has a wage job	0.11	0.12	0.687
Dummy: Self-employed	0.12	0.17	0.082*
Dummy: Has experience in a savings committee	0.28	0.32	0.265
Dummy: Currently in a savings committee	0.15	0.25	0.007***
Dummy: Currently owes family or friends	0.12	0.10	0.367
Dummy: Currently owes NRSP	0.31	0.37	0.177
Dummy: Currently owes an MFI	0.02	0.04	0.318
Dummy: Has a bank account	0.06	0.05	0.508
Number of minutes to walk to NRSP	36.25	31.70	0.009***
Dummy: Correct on math question	0.58	0.46	0.008***

Digit span test score	4.55	4.52	0.763
Dummy: Usually makes final decision on spending	0.71	0.66	0.266
Dummy: Faces pressure to share	0.77	0.81	0.192
Dummy: Finds it hard to save	0.71	0.81	0.009***
Dummy: Good at keeping track of time	0.72	0.69	0.421
Dummy: Follows a tight routine	0.51	0.54	0.497
Dummy: Others remind of appointments	0.49	0.45	0.423
Dummy: Acts early to avoid forgetting	0.50	0.52	0.542
Dummy: Good at keeping track of finances	0.62	0.59	0.478
Dummy: Follows a strict schedule on finances	0.62	0.59	0.408
Dummy: Others remind of financial obligations	0.42	0.44	0.703
Dummy: Acts early to avoid forgetting finances	0.45	0.44	0.809
Dummy: Keeps cash earmarked	0.56	0.52	0.357
Dummy: Would immediately spend 100 rupees if found	0.27	0.25	0.620
Patience measure (higher is more patient)	5.51	5.69	0.385
Patience measure in future frame	5.53	5.73	0.356
Risk aversion measure (higher is more risk-tolerant)	0.25	0.26	0.322
Appropriate for a woman to buy a scarf	0.32	0.31	0.716
Appropriate for a woman to invest in her business	0.25	0.26	0.828

*All the calculations in this Table are based on an OLS regression of respondent characteristic on product take-up. Standard errors clustered at the household level. We use ‘\*’, to denote confidence at the 90% level. Equality test refer to coefficient equality across columns (1) and (2).*

Table A5: Correlate means of Sunk product take-up behaviour

	Never accepted (1)	Accepted (2)	Equality test ( <i>p</i> ) (3)
Age (years)	39.22	38.03	0.210
Dummy: Married	0.79	0.78	0.751
Dummy: Education to class 5	0.17	0.20	0.380
Dummy: Education to matric	0.20	0.12	0.011**
Dummy: Literate	0.54	0.49	0.257
Household size	5.69	6.24	0.003***
Household income last week	2601.85	2874.16	0.229
Monthly household consumption	17892.22	19440.63	0.064*
Dummy: Household head	0.16	0.18	0.508
Dummy: Spouse of household head	0.66	0.64	0.613
Dummy: Has a wage job	0.13	0.15	0.705
Dummy: Self-employed	0.11	0.13	0.381
Dummy: Has experience in a savings committee	0.27	0.27	0.976
Dummy: Currently in a savings committee	0.15	0.19	0.303
Dummy: Currently owes family or friends	0.12	0.12	0.850
Dummy: Currently owes NRSP	0.31	0.45	0.001***
Dummy: Currently owes an MFI	0.02	0.03	0.313
Dummy: Has a bank account	0.06	0.06	0.762
Number of minutes to walk to NRSP	34.57	37.23	0.185
Dummy: Correct on math question	0.56	0.46	0.022**

Digit span test score	4.55	4.39	0.137
Dummy: Usually makes final decision on spending	0.72	0.68	0.330
Dummy: Faces pressure to share	0.78	0.82	0.253
Dummy: Finds it hard to save	0.75	0.76	0.921
Dummy: Good at keeping track of time	0.68	0.65	0.460
Dummy: Follows a tight routine	0.50	0.42	0.050*
Dummy: Others remind of appointments	0.47	0.45	0.608
Dummy: Acts early to avoid forgetting	0.50	0.48	0.630
Dummy: Good at keeping track of finances	0.63	0.55	0.082*
Dummy: Follows a strict schedule on finances	0.62	0.53	0.042**
Dummy: Others remind of financial obligations	0.46	0.43	0.416
Dummy: Acts early to avoid forgetting finances	0.50	0.43	0.111
Dummy: Keeps cash earmarked	0.53	0.49	0.303
Dummy: Would immediately spend 100 rupees if found	0.28	0.30	0.664
Patience measure (higher is more patient)	5.53	5.76	0.269
Patience measure in future frame	5.46	5.80	0.110
Risk aversion measure (higher is more risk-tolerant)	0.24	0.26	0.292
Appropriate for a woman to buy a scarf	0.29	0.34	0.302
Appropriate for a woman to invest in her business	0.25	0.27	0.703

*All the calculations in this Table are based on an OLS regression of respondent characteristic on product take-up. Standard errors clustered at the household level. We use ‘\*’, to denote confidence at the 90% level. Equality test refer to coefficient equality across columns (1) and (2).*

Table A6: Correlate means of respondent reminders take-up behaviour

	Never accepted (1)	Accepted (2)	Equality test ( <i>p</i> ) (3)
Age (years)	39.18	39.61	0.636
Dummy: Married	0.84	0.79	0.147
Dummy: Education to class 5	0.16	0.15	0.748
Dummy: Education to matric	0.17	0.15	0.473
Dummy: Literate	0.51	0.45	0.147
Household size	5.88	6.08	0.244
Household income last week	2617.63	2471.26	0.473
Monthly household consumption	18436.14	18615.26	0.816
Dummy: Household head	0.14	0.20	0.068*
Dummy: Spouse of household head	0.69	0.66	0.477
Dummy: Has a wage job	0.11	0.13	0.465
Dummy: Self-employed	0.12	0.13	0.667
Dummy: Has experience in a savings committee	0.25	0.27	0.575
Dummy: Currently in a savings committee	0.15	0.21	0.086*
Dummy: Currently owes family or friends	0.14	0.10	0.215
Dummy: Currently owes NRSP	0.29	0.43	0.001***
Dummy: Currently owes an MFI	0.03	0.04	0.469
Dummy: Has a bank account	0.06	0.04	0.376
Number of minutes to walk to NRSP	36.40	32.23	0.020**
Dummy: Correct on math question	0.53	0.49	0.382

Digit span test score	4.58	4.39	0.075*
Dummy: Usually makes final decision on spending	0.70	0.64	0.127
Dummy: Faces pressure to share	0.77	0.81	0.265
Dummy: Finds it hard to save	0.75	0.74	0.904
Dummy: Good at keeping track of time	0.67	0.65	0.757
Dummy: Follows a tight routine	0.48	0.44	0.431
Dummy: Others remind of appointments	0.46	0.49	0.496
Dummy: Acts early to avoid forgetting	0.47	0.50	0.445
Dummy: Good at keeping track of finances	0.59	0.57	0.585
Dummy: Follows a strict schedule on finances	0.58	0.56	0.557
Dummy: Others remind of financial obligations	0.44	0.44	0.902
Dummy: Acts early to avoid forgetting finances	0.47	0.47	0.910
Dummy: Keeps cash earmarked	0.55	0.52	0.603
Dummy: Would immediately spend 100 rupees if found	0.30	0.28	0.647
Patience measure (higher is more patient)	5.72	5.71	0.961
Patience measure in future frame	5.58	5.75	0.398
Risk aversion measure (higher is more risk-tolerant)	0.24	0.27	0.115
Appropriate for a woman to buy a scarf	0.32	0.38	0.098*
Appropriate for a woman to invest in her business	0.28	0.32	0.276

*All the calculations in this Table are based on an OLS regression of respondent characteristic on product take-up. Standard errors clustered at the household level. We use ‘\*’, to denote confidence at the 90% level. Equality test refer to coefficient equality across columns (1) and (2).*



Table A7: Correlate means of respondent reminders take-up behaviour

	Never accepted (1)	Accepted (2)	Equality test ( <i>p</i> ) (3)
Age (years)	38.79	39.13	0.728
Dummy: Married	0.81	0.80	0.964
Dummy: Education to class 5	0.18	0.14	0.168
Dummy: Education to matric	0.21	0.13	0.009***
Dummy: Literate	0.54	0.41	0.005***
Household size	5.71	6.07	0.058*
Household income last week	2578.89	3188.73	0.007***
Monthly household consumption	17611.44	19023.10	0.080*
Dummy: Household head	0.16	0.20	0.366
Dummy: Spouse of household head	0.68	0.67	0.742
Dummy: Has a wage job	0.11	0.13	0.649
Dummy: Self-employed	0.11	0.16	0.114
Dummy: Has experience in a savings committee	0.29	0.23	0.131
Dummy: Currently in a savings committee	0.16	0.17	0.822
Dummy: Currently owes family or friends	0.11	0.10	0.792
Dummy: Currently owes NRSP	0.31	0.43	0.005***
Dummy: Currently owes an MFI	0.02	0.03	0.581
Dummy: Has a bank account	0.06	0.06	0.881
Number of minutes to walk to NRSP	35.07	34.80	0.883
Dummy: Correct on math question	0.55	0.45	0.032**

Digit span test score	4.52	4.38	0.165
Dummy: Usually makes final decision on spending	0.74	0.68	0.138
Dummy: Faces pressure to share	0.74	0.84	0.004***
Dummy: Finds it hard to save	0.71	0.83	0.001***
Dummy: Good at keeping track of time	0.71	0.65	0.180
Dummy: Follows a tight routine	0.51	0.46	0.212
Dummy: Others remind of appointments	0.46	0.42	0.402
Dummy: Acts early to avoid forgetting	0.52	0.47	0.335
Dummy: Good at keeping track of finances	0.61	0.53	0.068*
Dummy: Follows a strict schedule on finances	0.64	0.55	0.048**
Dummy: Others remind of financial obligations	0.43	0.42	0.789
Dummy: Acts early to avoid forgetting finances	0.48	0.45	0.375
Dummy: Keeps cash earmarked	0.57	0.52	0.261
Dummy: Would immediately spend 100 rupees if found	0.25	0.34	0.032**
Patience measure (higher is more patient)	5.51	5.99	0.020**
Patience measure in future frame	5.59	5.97	0.070*
Risk aversion measure (higher is more risk-tolerant)	0.25	0.28	0.092*
Appropriate for a woman to buy a scarf	0.29	0.33	0.408
Appropriate for a woman to invest in her business	0.22	0.30	0.054*

*All the calculations in this Table are based on an OLS regression of respondent characteristic on product take-up. Standard errors clustered at the household level. We use ‘\*’, to denote confidence at the 90% level. Equality test refer to coefficient equality across columns (1) and (2).*