Implicit and Explicit Commitment in Credit and Saving Contracts: A Field Experiment*

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Abstract

We conduct a field experiment with microfinance clients in Pakistan, where we offer a commitment contract inspired by the rotating structure of a ROSCA. Additional treatments test *ex ante* demand for soft commitment (in the form of reminders, either to respondents or to their families), hard commitment (in the form of a penalty for missing an instalment), and flexibility (an option to postpone an instalment). We find substantial demand for both credit and savings contracts, in ways that imply that respondents value the implicit commitment required by such contracts. However, we find no demand for additional contractual features; indeed, demand for credit is substantially reduced by offering additional flexibility (unless paired with personal reminders), and by hard commitment (particularly when paired with reminders to the respondent's family). This shows that demand for commitment depends on whether commitment features are implicit or explicit, and that the particular form of commitment features is likely to matter substantially for microfinance demand.

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1 Introduction

Commitment problems – whether due 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 as impediments to the repayment of loans (Casaburi and Macchiavello, 2019; Dupas and Robinson, 2013b; Duflo, Kremer, and Robinson, 2011). So too are problems of inattention, which have been identified as an alternative psychological factor behind saving behavior (Karlan, McConnell, Mullainathan, and Zinman, 2016). Indeed, several key 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 an emphasis on group lending – all typical of many 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 and default options – relying either on psychological or economic motivations – have proven effective in encouraging savings and reducing loan defaults (Ashraf, Karlan, and Yin, 2006; Dupas and Robinson, 2013b; Stango and Zinman, 2014; Karlan et al., 2016; Brune, Giné, Goldberg, and Yang, 2016; Somville and Vandewalle, 2018).

The evidence on commitment and inattention problems in financial decisions comes from two largely distinct streams of research, which often treat saving and borrowing as two separate behavioral realms, both conceptually and practically. However, when individuals struggle to hold savings over time and wish to incur lumpy expenditures, saving and borrowing may be substitutes (Afzal, d'Adda, Fafchamps, Quinn, and Said, 2018), and individuals may 'borrow to save' (Rutherford, 2000; Morduch, 2010; Collins, Morduch, Rutherford, and Ruthven, 2009; Armendáriz and Morduch, 2010; Kast and Pomeranz, 2018). Little work has been done to explore the implications of this idea for design of financial instruments using behavioral insights to address commitment and inattention issues.

In this paper, we present evidence from a large field experiment 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 – that is, a regular instalment schedule – but differ in the timing of the lumpsum disbursement. Contracts also vary in their rate of return, i.e., in whether the lumpsum is larger, smaller or equal to the sum of the instalments. We term these variations – that is, random variations in the timing and the size of the

lumpsum payment – as '*contractual variations*'.

We then augment this standard product with a set of '*contractual add-ons*', designed to isolate each of the main barriers to saving and timely debt repayment identified in the literature: self-commitment issues, inattention, and intra-household dynamics. We address each barrier through the major tools found to be effective by studies on saving and borrowing. In two ways, we vary the level of commitment built into the contract. On one hand, we increase commitment through the introduction of a penalty for missing an instalment (e.g., John (2020)); on the other hand, we decrease commitment by allowing higher flexibility in the repayment schedule through the possibility of deferring one instalment (e.g., Field et al. (2013)). We vary the salience of the repayment schedule by sending reminders to the clients. Finally, we vary the extent to which household members can influence clients' repayment efforts by targeting reminders to family members.

Our rich experimental design allows us to address a series of questions. First, we can exploit *within-subject* variations in the timing and size of the lumpsum payment to test directly how demand for commitment varies with its price and on whether it is implicitly embodied in a saving or credit contract. We develop a stylised model of lumpsum accumulation, from which we derive testable hypotheses, tailored to our experimental design, that guide our empirical analysis. Combining the administrative data on take-up with survey data on participants' characteristics – particularly on self-reported ability to hold on to cash and make payments on time, and on household pressures to share income – we examine correlates of heterogeneity in contract take-up.

Our results show substantial demand for credit contracts and, to a lower extent, saving contracts. Take-up rates are on average about 30% for our credit contracts, and about 8% for saving contracts. This is in line with demand for similar commitment saving products (Cole, Sampson, and Zia, 2011), although somewhat lower than the average take-up rate of financial products with commitment features found in the literature (Karlan, Ratan, and Zinman, 2014). When offered both a credit and a saving contract, 46 percent of the participants who take up at least one offer 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. The version of the model that best accounts for the combined evidence is one in which subjects face substantial costs of holding cash and have occasional and sometimes unforeseen need for a lumpsum. This creates a demand for commitment contracts, either in the form of credit or savings, which however does not manifest itself all the time. This explains the observed variation in take-up across cycles by the same subject. Further, unanticipated financial needs favour take-up of the credit contract, while only non-urgent needs foster demand for commitment savings contracts. A decision-maker who can hold onto cash would deal with this situation by accumulating precautionary savings – and would not take up some of our less appealing products. But this is not true for our subjects, many of whom face substantial impediments to accumulating on their own and thus are willing to take otherwise-unappealing

contracts when the need arises.

Second, we use *between-subject* variation in contractual add-ons to provide direct evidence of how demand responds to variations in commitment and flexibility, assistance in keeping track, and exposure to intrahousehold pressure. Crucially, we test whether demand for add-ons (Bryan, Karlan, and Nelson, 2010) varies when added to a credit or a saving product. These tests help our understanding of the behavioral foundations of microfinance (Bauer, Chytilová, and Morduch, 2012). We find no demand for contractual add-ons – such as flexibility and reminders. This is particularly in evidence for credit contracts, where take-up falls when explicit commitment features are imposed. The negative effect is particularly strong when hard commitment is coupled with family reminders, while it is interestingly absent when flexibility is combined with individual reminders. In the saving domain, take-up does not increase with contract add-ons, but we cannot reject that it is the same as without the added features. These findings echo the results of the analysis of heterogeneity in take-up of the standard contract, i.e., without add-ons: they suggest the empirical relevance of inattention issues and of intrahousehold pressures to share in determining demand for commitment in the credit domain. These results also indicate that explicit additional hard or soft commitment is not valued by clients, and so is additional flexibility without increased salience of the payment schedule. The commitment that is implicit in a credit or savings contract seems sufficient for the needs of our subject population.

Together, we see our results as making several distinct contributions. 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' – or more precisely to accumulate a lumpsum – thereby demonstrating a demand for implicit commitment. In previous pilot work, we showed this pattern of behavior for small financial products with daily repayments (Afzal et al., 2018). This paper substantially extends that result in several key ways. First, in the earlier paper, we focused on daily repayment contracts only – a contract quite unlike standard microfinance products offered on the market – and we were left to speculate as to the generalisability of our result. To the best of our knowledge, our current experiment is the first to randomly offer 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.[1] Second, the earlier experiment did not involve any cross-cutting contractual add-ons. Third, the earlier work said almost nothing about correlates of take-up, whereas the current paper uses recent machine learning techniques to provide a rich characterisation of heterogeneity patterns. Finally,

[1] This complements recent work by Kast and Pomeranz (2018) showing that, for many microfinance clients, provision of savings accounts reduces levels of debt. It also supports the literature already cited on borrowing to save among microfinance clients (Morduch, 2010; Collins et al., 2009; Armendáriz and Morduch, 2010). The paper also relates to a recent literature on formalisation of informal savings products (Dupas and Robinson, 2013a,b). Similarly, Brune and Kerwin (2019) find a positive effect of deferred income streams designed as lumpsum payments.

our earlier work did not involve any follow-up interviews – and was therefore unable to provide any estimates of the consequences of being offered this kind of rotating product. This shortcoming is corrected here.

More generally, this is the first time that demand for multiple types of commitment devices have been tested within the same microfinance field experiment. Augmenting the basic contract with the add-ons allows us to explore the interaction between different barriers to saving and loan repayment, such as self-commitment issues and inattention or intrahousehold pressures (Karlan et al., 2016). Our results are consistent with the existing microfinance and microsaving literature. In particular, the decrease in take up of our product, when augmented with explicit 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, 2010). It also conforms with the mixed evidence on demand for commitment (Ashraf et al., 2006; Allcott and Kessler, 2015; Damgaard and Gravert, 2018; Allcott, Kim, Taubinsky, and Zinman, 2020) and on its sensitivity to cost (Laibson, 2015).

The contrast between the high demand for implicit commitment (built into the repayment structure of the financial product we offer) and the low demand for explicit commitment (embodied by the add-ons) complements existing evidence on individuals' demand for commitment devices. Such evidence is mixed, with some authors finding high demand for commitment and welfare-improving impacts of commitment contracts (Kaur, Kremer, and Mullainathan, 2015; Schilbach, 2019; Augsburg, Caeyers, Giunti, and Malde, 2018), while others showing the opposite (Bai, Handel, Miguel, and Rao, 2017).[2] Our results suggest that demand for commitment is not just driven by the level of commitment offered, but also by whether commitment is implicit or explicit. This distinction is consistent with the observed tendency of private institutions to shroud commitment mechanisms (Laibson, 2018) and to exploit individuals' partial naivete in the contracts they offer (DellaVigna and Malmendier, 2004). Shrouded paternalism is motivated by the tendency of naive consumers to demand sub-optimal levels of commitment (John, 2020; Bai et al., 2017), which drives firms to make commitment features implicit.

The result – that the appeal of credit and commitment saving contracts is high, but there is no demand for formal flexibility, added penalty for cancellation, reminders, or intra-household pressure – also complements a recent literature documenting the hidden welfare cost of nudges. Recent empirical research shows that, while nudges can encourage intended behavior, they also

[2] The evidence on the impact of flexibility on repayment quality in microfinance contracts is similarly mixed, with recent contributions showing that flexibility is valued, leads to improved business outcomes, and does not increase default (Battaglia, Gulesci, and Madestam, 2019; Barboni and Agarwal, 2018), and other papers providing evidence to contrary (Field and Pande, 2008; Czura, 2015).

increase avoidance behavior (Allcott and Kessler, 2015; Damgaard and Gravert, 2018).[3] 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.

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 microcredit on either business outcomes or household material welfare (Meager, 2018b; 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; Tarozzi, Desai, and Johnson, 2015; Karlan and Zinman, 2011; Liu and Roth, 2019). We also contribute to a smaller and more recent literature showing similarly limited effects of microsaving (Dupas, Karlan, Robinson, and Ubfal, 2018; De Mel, McIntosh, Sheth, and Woodruff, 2018; Castellanos, Jiménez-Hernández, Mahajan, and Seira, 2019).

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 comes 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. The order in which members receive the pot is either determined randomly or assigned by bidding, depending on committee rules.

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 – and thus sidesteps the

[3] In a related literature, 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). Another relevant phenomenon, 'control aversion', causes incentives and regulations to backfire when they are perceived as overbearing (Falk and Kosfeld, 2006; Fehr and List, 2004).

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; we randomly draw different contractual variations in each cycle. 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 week (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, by the end of Week N. If not, the participant is not offered any contract in the following cycle. In case the participant has a positive balance with NRSP at the time of default, this balance is returned to the subject at the end of the cycle – that is, after Week N. 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 providing a breadth of contexts, for understanding the generalizability of results, and for understanding whether results are sensitive to specific aspects of design (see, for example, Dupas et al. (2018), Karlan et al. (2016) and Banerjee, Karlan, and Zinman (2015)). With this principle in mind, we implemented our experiment in two distinct phases. These phases used different 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 instalments to the subject. A lumpsum of 4500 is equivalent to deducting 10% from the lumpsum, while a lumpsum of 5500 means adding 10% to the sum of instalments received. Table 1 illustrates the payment schedule for a basic contract with a lumpsum payment on Week 1 and a net balance of -10%.

Since there are three possible lumpsum values and two possible disbursement weeks, there are six possible contractual variations. 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 provide a positive net balance: credit is subsidized. Similarly, some saving contracts yield a negative net balance: 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 instalments paid by the participant.

2.2 Contractual add-ons: Flexibility and reminders

Beside traditional explanations relying on credit constraints, transaction costs and lack of financial literacy, the recent behavioral literature on saving and borrowing by the poor has focused on three main explanations for undersaving and take-up of microfinance products: commitment issues, inattention and intra-household dynamics. Existing evidence on these barriers typically tests one of these factors within the same design – or at most two – or relies on the analysis of heterogeneous treatment effects to assess their empirical relevance (Dupas and Robinson, 2013a; Karlan et al., 2016). Moreover, the existing evidence focuses either on saving or borrowing behavior. We instead augment our basic contracts with add-ons aimed at isolating each of these factors.

Reminders: Reminders are a common tool studied in the behavioral literature on savings. Their purpose is to increase the salience of saving goals or payment obligations and of the benefits from meeting them, and through this to help participants follow a regular schedule of payments. In our experiment, we send reminders one day before an instalment is due. Reminders are transmitted through phone calls. In the 'respondent reminder' treatment, the message is sent directly to the participant; in the 'family reminder' treatment, the message 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 instalment is due.[4] This is different from other experiments that have externally introduced reminders and observed how these reminders affect payment patterns (see, for example, Karlan et al. (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.

Respondent reminders and family reminders are aimed at isolating the impact of two different sources of saving or repayment issues: inattention and intra-household pressures. Inability to meet financial obligations may derive from their lack of salience: in such instances, personal reminders increase commitment attainment (Karlan et al., 2016). The influence of household members on financial discipline can be positive or negative: peer pressure and demands from household members to share available resources may limit individual ability to save or repay a loan (Squires, 2018; Jakiela and Ozier, 2015; Ashraf, 2009), but household members may also act as 'saving monitors', to help the respondent to stick to the savings commitment (Breza and Chandrasekhar, 2019). By comparing the impact of reminders on take-up and repayment when sent to the respondent or to a household member, we are able to assess the direction and relative weight of these constraints.

Commitment features: Our commitment arm involves either adding a cancellation fee (we term this the 'sunk' treatment), or allowing for 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 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). In case of default in a savings contract, our implementing partner NRSP returns to the subject in Week N all the instalments paid before defaulting. For instance, if a subject has paid three instalments totalling *PKR* 1500 and 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 (2020)). 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

[4] 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 family 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.' Staff were instructed that, for reminder calls to respondents in the 'family reminder' treatment, it was not permissible to leave reminder messages with any other person who might answer the call.

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* instalment by *one* week only.[5] To illustrate, the subject may decide not to pay the instalment *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 to apply.

The design of the 'sunk' and 'flex' treatments draws from existing studies introducing hard commitment features to saving products (John, 2020) or adding flexibility to the rigid repayment schedule typical of microfinance products (Field et al., 2013; Czura, 2015; Barboni and Agarwal, 2018; Battaglia et al., 2019).[6] We combine these features in an unique experimental design and test their impact on take-up and default both in the saving and credit domains.

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.

Participants are drawn from clients of microfinance products offered by NRSP. We choose this sample both for the purpose of academic insight and the purpose of potential external policy validity. Our experiment seeks to understand the demand for microcredit, and the demand for contractual add-ons; for both purposes, the ideal sample comprises microfinance clients: precisely the kind of people with whom microfinance institutions will engage. 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.

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

[5] Subjects are told that 'We understand that it is not always possible to pay instalments every week. Therefore, over the course of eight weeks, we will allow you on one occasion only to delay a payment by one week.'

[6] Our 'flex' treatment is closest to the flexibility option in Barboni and Agarwal (2018), with the key difference that customers have to pay in full the instalment that they decide to skip with the following instalment, rather than spreading out the outstanding balance over the remaining loan instalments.

in each). In Phase 2, we assigned 25% of participants to the control group; the remaining 75% were then assigned in a 3×3 factorial design, covering all combinations of (i) sunk, flex or no commitment feature, and (ii) respondent reminders, family reminders and no reminders. As showed in that table, Phase 2 respondents faced a single form of treatment throughout the experiment (that is, we introduce *between-subjects* exposure to either the basic contract or one of the contractual add-ons), and this was known in advance of the wave 1 take-up decision. Respondents then faced random wave-to-wave variation in contractual terms (that is, the timing of the lumpsum and the contractual balance varies by design *within subjects*); this was known in advance of the take-up decision for each wave. We explain the specific details of randomization in Online Appendix A.

Tables A15 and A16 describe main characteristics of the sample in the two phases. Monthly household consumption averages *PKR* 25,000 (at the time, equivalent to about $250) in Phase 1 and *PKR* 20,000 (approximately $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 report finding it difficult to save. Tables A15 and A16 also report p-values for randomization balance across treatments. This is done by regressing each variable on the assigned treatment status in a saturated specification. We also test for randomization 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 four variables to be unbalanced at the 90% confidence level in Phase 2. In Appendix Figures A5 and A6, we use a post-double LASSO estimation and find that our main estimation results are unaffected if we include the set of baseline variables as controls.

3 Demand and implicit commitment

We now analyse take-up patterns; we consider both average behaviour, and also exploit the panel dimension of our experiment to analyse within-respondent take-up patterns.[7]

3.1 Conceptual framework

To provide a theoretical frame to the empirical analysis, we present three stylised benchmark scenarios: (1) when subjects can hold onto cash; (2) when they cannot hold onto cash but have no need for bunching their expenditures into a lumpsum; and (3) when they cannot hold onto cash and have

[7] Our empirical analysis – both of product demand and consequences of adopting – follows two Pre-Analysis Plans. Our pre-analysis plan for Phase 1 (filed on 10 May 2015) is available at https://www.socialscienceregistry.org/trials/684, and the extensive implementation of that analysis is available at http://www.simonrquinn.com/research/Microfinance_PreAnalysis_Phase1.pdf; our pre-analysis plan for Phase 2 (filed on 15 January 2017) is available at https://www.socialscienceregistry.org/trials/1916, with extensive implementation available at http://www.simonrquinn.com/research/Microfinance_PreAnalysis_Phase2.pdf.

a demand for lumpsum accumulation. We then draw on the insights gained from this framework to guide our empirical analysis.

Scenario 1: Subjects can hold onto cash: We first discuss the case where subjects are able to save in a liquid asset (*i.e.* cash). When people can hold cash on their own, a simple arbitrage argument implies that taking up a saving contract with a negative return can never be optimal.[8] For take-up of a saving contract to be rational in this case, the subject must face meaningful economic costs of holding onto cash – for instance, because of self-commitment issues. The same reasoning applies to savings contracts with a zero return: subjects able to save on their own can mimic the contract without incurring the time cost of visiting the MFI to pay each installment. It follows that take-up of saving contracts with a zero or negative return represent a lower bound on the demand for commitment: they are the least appealing commitment contracts.

By a similar arbitrage argument, credit contracts in which the lumpsum exceeds the value of the installments should always be accepted by subjects who can hold onto cash.[9] This kind of loan may nonetheless be rejected by subjects who have difficulties holding cash – for example, subjects who are sophisticated about their self-commitment problems but for whom the credit contract is not a sufficiently strong external commitment. Take-up of these loans therefore represent an upper bound on the demand for commitment: they are the most attractive commitment contract. In particular, such loans will not be taken by subjects who cannot hold onto cash but have no regular income to service the debt or no need for lumpsum accumulation. We also note that subjects who can hold onto cash but refuse subsidized credit only because of transaction costs should refuse all other contracts as well since, by construction, these contracts are less advantageous – they either do not include the subsidy, pay the lumpsum later, or both.

Scenario 2: Subjects cannot hold onto cash: We now examine predicted take-up in the stylised case of a subject who *cannot* hold cash and for whom the contracts we offer are the *only* available way of moving funds across periods. To do this, we use the standard framework of expected utility with exponential discounting and weekly discount parameter β.[10] We denote weekly income as y, and assume that y is drawn from a stationary distribution. This framework implies the following

[8] This is because the decision-maker can exactly replicate the cash flow pattern of this contract by saving all the installments and spending the lumpsum at the end – and be left with a positive net balance. For instance, instead of taking a savings contract with a payout of *PKR* 4500 in exchange for five installments of *PKR* 1000, the subject could simply set aside the installments each week and end up with *PKR* 5000 – a strategy that dominates the contract.

[9] That is, a subject who can hold onto cash can use the loan to pay the installments and keep the difference. For instance, the subject could take an upfront payment of *PKR* 5500, repay the five installments of *PKR* 1000, and be left with *PKR* 500. Hence take-up is the optimal decision, provided the cost of visiting the MFI to pay the installments is small enough relative to the surplus.

[10] As before, N is the duration of the contract, L is the lumpsum, and M is the installment.

utility-maximising behaviour:

1. Take a credit contract if and only if:

$$\sum_{t=1}^{N} \beta^t \cdot \mathbb{E}\left[U(y)\right] \leq \beta \cdot \mathbb{E}\left[U(y+L)\right] + \sum_{t=2}^{N} \beta^t \cdot \mathbb{E}\left[U(y-M)\right]. \tag{1}$$

2. Take a savings contract if and only if:

$$\sum_{t=1}^{N} \beta^t \cdot \mathbb{E}\left[U(y)\right] \leq \sum_{t=1}^{N-1} \beta^t \cdot \mathbb{E}\left[U(y-M)\right] + \beta^N \cdot \mathbb{E}\left[U(y+L)\right]. \tag{2}$$

Figure 1 shows the predictions of the model for linear utility.[11] In Panel A and Panel B of that figure we graph the indifference curves implied by equations 1 and 2 for Phase 1 and Phase 2, respectively. In each case, the horizontal axis shows the variation in β, and the vertical axis shows the payout ratio of L over $(N-1)\cdot M$ – that is, what the client receives divided by what she paid in total.[12] We use a log-log space for clarity. We show on each figure the three $L/[(N-1)\cdot M]$ values used in the experiment: 1.1, 1, and 0.9 for Phase 1; and 1.086, 1 and 0.914 for Phase 2. The graph shows that, for all $\beta < 1$, there exists values of $L/[(N-1)\cdot M]$ at which a client accepts a loan but not a saving contract. We see that in Phase 1, clients with $\beta > 0.9695$ take a saving contract with a 1.1 payout ratio (the 1.1 line is above their indifference curve) as well as loans with $L/[(N-1)\cdot M] = 1$ or higher. But they do not take loans with $L/[(N-1)\cdot M] = 0.9$ (the 0.9 line is below their indifference curve). Clients with $\beta < 0.965$ take all loans, but they do not take up any of the savings contracts. Clients with β in between only take the loans with $L/[(N-1)\cdot M]$ equal to 1 or 1.1. No subject with $\beta < 1$ takes savings contracts with payout ratios of 1 or below. All subjects take credit contracts with a payout ratio of 1 but none of the saving contracts with the same payout ratio.

There are two further points worth noting from the two top panels of Figure 1. First, in each case, the cutoff value of β at which a respondent is indifferent as to taking a savings contract with

[11] Specifically, the condition in equation 1 implies:

$$\frac{\mathbb{E}\left[U(y+L)\right] - \mathbb{E}\left[U(y)\right]}{(N-1)(\mathbb{E}\left[U(y)\right] - \mathbb{E}\left[U(y-M)\right])} \geq \frac{\beta - \beta^N}{(N-1)(1-\beta)},$$

where, under linear utility, the lefthand side of this expression simplifies to $L/[(N-1)\cdot M]$. Note that the condition in equation 2 implies:

$$\frac{\mathbb{E}\left[U(y+L)\right] - \mathbb{E}\left[U(y)\right]}{(N-1)(\mathbb{E}\left[U(y)\right] - \mathbb{E}\left[U(y-M)\right])} \geq \frac{\beta - \beta^N}{(N-1)\beta^N \cdot (1-\beta)}.$$

[12] The downward-sloping line in the upper section of each figure shows the indiffence curve for saving; points above the line imply take-up of a saving contract with payout ratio $L/[(N-1)\cdot M]$. The upward-sloping line in the lower section of each figure is the indifference curve for borrowing: points above it imply take-up of a loan with $L/[(N-1)\cdot M]$.

a 1.1 payout ratio is very close to the value of β at which a respondent is indifferent as to taking a credit contract with a 0.9 payout ratio.[13] The model thus predicts that the proportion of subjects who *take* a loan with a *low* L is approximately the same as the proportion of subjects who *reject* a savings contract with a *high* L. Second, if subjects have stable time preferences – and thus a time-invariant β – Figure 1 generates testable consistency conditions on choices made across rounds. For instance, someone who takes a loan with payout ratio 0.9 in one round (and thus has a $\beta < 0.965$) should reject a savings contract with a 1.1 payout in another round.

Scenario 3: Subjects cannot hold onto cash but have a preference for a lumpsum: Predictions are different if subjects have a specific desire to accumulate a lumpsum. Subjects may value receiving a lumpsum L above its monetary value because it allows them to purchase a durable good producing a flow of services with future discounted value larger than L, as for example in Brune and Kerwin (2019), Attanasio and Pastorino (2020) and Besley, Coate, and Loury (1993).[14] Alternatively, someone may wish to spend L on a ceremonial expenditure that also generates memories and social capital of value greater than L.

The effect on take-up of a preference for a lumpsum can be examined by multiplying the payout ratio $L/[(N-1)\cdot M]$ in equations (1) and (2) by a parameter $\theta \geq 1$ and redrawing Figure 1. Such a change is illustrated in Panels C and D for $\theta = 1.05$. We see that setting $\theta > 1$ shifts both indifference curves down, expanding the range of values of β at which taking up some of the contracts is optimal. The shift is different for loan and savings contracts, however. The range of β's for which it is optimal to *take* a loan with a *low* L is substantially larger than the range of β's that *reject* a savings contract with a *high* L. Put differently, the proportion of subjects who *take* a credit contract with $L/[(N-1)\cdot M] < 1$ is substantially larger than the proportion of subjects who *reject* a savings contract with a positive return. Further, for a contract with a payout ratio of 1, there is now a range of β values for which individuals take both loan and savings contracts. This is empirically testable: conditional on having stable preferences, these subjects would take up both savings and credit contracts across experiment waves.

Alternatively, some subjects may have a $\theta < 1$ instead, for example because they have no particular use of a lumpsum and wish to avoid the cost of making the installments or, alternatively,

[13] This is not a coincidence: in logs, the expression $\frac{\beta-\beta^N}{(N-1)(1-\beta)}$ has a slope of approximately $0.5N$ while the expression $\frac{\beta-\beta^N}{(N-1)\beta^N\cdot(1-\beta)}$ has a slope of approximately $-0.5N$.

[14] A relevant example in the context of our experiment is bulk purchases that reduce the unit cost of, say, flour, oil, or kerosene, relative to small daily purchases.

because they wish to smooth their consumption.[15] In those cases, the indifference curves shift upwards and this logic reverses: only very impatient subjects take a costly loan (i.e., for which $L/[(N-1)\cdot M] < 1$) and only very patient subjects take a savings contract with a positive return. This is illustrated in Panels E and F. If β and θ are time-invariant, these predictions can be tested by comparing subjects' take-up behavior across experiment waves.

If θ varies across rounds – for example, because of fluctuations in the utility of lumpsum accumulation or in the anticipated utility cost of installments – it might be possible to observe subjects borrowing in some rounds and saving in others. It has long been noted that liquidity constraints can distort measurement of discount factors – for example, by affecting experimental measurements of time preference (Cassidy, 2019) or by causing respondents to turn down profitable savings opportunities or to take expensive credit (Noor, 2009; Gerber and Rohde, 2015; Epper, 2017; Dean and Sautmann, 2020). In our framework where β denotes an individual-specific and time-invariant parameter, any immediate demand for funds due to unforeseen circumstances (Frederick, Loewenstein, and O'Donoghue, 2002) manifests itself as a sudden and temporary increase in the demand for a lumpsum and thus in θ. We revisit this point in the empirical section when we discuss changes in take-up behavior across rounds and the motives behind the demand for lumpsums.

Testing strategy on take-up: Our analysis of the take-up data is organized around the above ideas. We first check whether observed choices contradict the pure arbitrage argument of scenario 1 – i.e., subjects take savings contracts with $L < (N-1)M$ or refuse credit contracts with $L \geq (N-1)M$. Such evidence indicates that some subjects find it difficult to hold onto cash, thereby justifying a closer examination of scenarios 2 and 3.

We then use the cutoff values of β's reported for each of the models in Figure 1 to draw inference about the range of β's consistent with observed choices. Since different values of θ generate very constrasted patterns of cutoff β's, we do this separately for different θ's. We then look for the θ that generates an implied distribution of β's that makes the most sense. The maintained assumption throughout this analysis is that the distribution of time preference parameters β across subjects remains the same across the three contract cycles.

Next we extend this approach to the behavior of individual subjects across cycles. Here the maintained assumption is that each subject has a stable time preference parameter. This assumption is common to most of the literature on credit and savings. We then check whether the range of β's implied by a subject's contract choices in one cycle overlaps with the range of β's implied by the choices they make in subsequent cycles. We do this separately for different values of θ. If β's

[15] To see the latter, focus on the case where $L = (N-1)M$ and consider the take-up inequalities (1) and (2). The concavity of $U(.)$ implies that, unlike in the linear case, the utility gain from receiving a large transfer L (the numerator) is less than $N-1$ times the utility loss of making installment M (the denominator) – hence their ratio is less than 1. The effect of this on take-up can be mimicked by multiplying the lefthand side of equations 1 and 2 by $\theta < 1$.

overlap over the three cycles for a particular value of θ, we conclude that the observed behavior is consistent with a data generating process with that constant θ. If, however, choices made cannot be reconciled with a single value of θ, we conclude that observed behavior implies that θ changes over time – for example, because the expected benefit from accumulating a lumpsum L has increased ($\mathbb{E}[U(y+L)] - \mathbb{E}[U(y)]$ is higher), or because the ability to pay the installments M varies with income and other shocks (i.e., $(\mathbb{N} - \mathbb{1})(\mathbb{E}[U(y)] - \mathbb{E}[U(y-M)])$ is larger).

3.2 Empirical results

Average take-up: We start by documenting average take-up frequencies for the six combinations of lumpsum amount and lumpsum week offered in the two phases of experimental sessions. Control respondents are omitted since they were not offered the contracts. Take-up frequencies are obtained by estimating a linear probability model of the form:

$$a_{it} = \sum_{w=1}^{2} \sum_{r=1}^{3} \beta_{wr} \cdot T_{it}^{w} \cdot T_{it}^{r} + \varepsilon_{it}, \tag{3}$$

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 in week 1 or N and with a negative, zero or positive net balance.[16]

Table 4 shows results for all subjects, from both experimental phases.[17] We observe high take-up for contracts with a high-lumpsum provided in week 1: 47% in Phase 1 and 37% in Phase 2. As anticipated, demand falls sharply for lower lumpsums and for later payouts. We also find that the sensitivity of take-up to the size of the lumpsum is larger for credit than savings contracts, in line with existing evidence (Karlan et al., 2010). We nonetheless note that take-up is positive and

[16] In both phases, some subjects said they were not interested in any contract and, consequently, staff members did not insist that they draw out a card to determine T_{it}^{w} and T_{it}^{r}. These subjects thus refused all six possible contracts, each of which they would have been offered with probability 1/6. We treat these cases as six different refusal observations each given a weight of 1/6. Standard errors are clustered at the individual level. We examine the characteristics of automatic refusers in both phased in Tables A1 and A2 and find that automatic refusal is less likely among subjects who are currently participating in a committee, those who have higher debt and, in the case of phase 1, those who are currently running a business.

[17] In the Online Appendix (Table A3) we show the same analysis excluding 'automatic refusers' – that is, respondents who refused the contract before learning the contractual terms. The proportion of automatic refusers in each cycle of each phase is reported in Table A18. Across all three cycles, automatic refusers account for 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 subjecs are never automatic refusers in phase 1 and 2, respectively. The proportion of subjects who automatically refuse in all experimental cycles is 25% and 20% in phase 1 and 2, respectively.

statistically significant for all six contracts in both experimental phases.[18] This is consistent with earlier results obtained by Afzal et al. (2018) using a similar contract design but a much shorter contract duration.

Arbitrage and ability to hold onto cash: Next we examine the observed take-up behaviour in light of the three stylised scenarios described earlier. We begin by considering arbitrage and the ability to hold cash across periods.

We first note that a non-negligible fraction of subjects take a savings contract that simply accumulates their installments: 8.9% in Phase 2 and 4.3% in Phase 1. Subjects could have accumulated the money themselves and saved the trouble of going to the MFI branch to pay the installments. We even see take-up of savings contracts with a *negative* net return: in Phase 1, we observe a 2.7% take-up for contracts offering a payment of *PKR* 4500 in week 6 after five payments of *PKR* 1000, and in Phase 2, 4.1% of participants accept a contract that pays *PKR* 3200 in week 8 after 7 installments of *PKR* 500. In both cases, participants could have accumulated the installments themselves and end up with a surplus. By the arbitrage argument presented earlier, take-up of any of these contracts requires that the subjects be unable to save cash on their own.

Similar evidence (in reverse) can be found for subsidized credit. In Phase 1, more than half of the subjects (53%) refuse to receive *PKR* 5500 in week 1 in exchange for *PKR* 5000 in 5 installments of *PKR* 1000; in Phase 2, 62.8% of subjects refuse *PKR* 3800 in week 1 in exchange for *PKR* 3500 in seven installments. This suggests that a large proportion of subjects either face substantial costs of holding cash, or consider that the time cost of visiting the MFI to pay the installments exceeds the value of the subsidy. Taken together, the evidence rejects the stylised arbitrage model discussed in scenario 1.

Implied discount factors for subjects who cannot hold onto cash: Next we turn to scenario 2 – namely, when subjects cannot save on their own but react to contract offers in a way consistent with a standard model of saving and borrowing with stable time preferences. We have seen in Table 4 that take-up responds to contractual terms, as predicted in Figure 1. However, contrary to predictions of the second scenario, take-up of the positive-balance saving contract and take-up of the negative-balance loan contract do not sum to anything close to 100 percentage points. For instance, take-up of the low-balance loan is 8.2% in phase 1 while take-up of the positive-balance savings contract is 11% – i.e., a sum of 19.2%, very far from 100%. In phase 2, the corresponding proportions are 11.0% and 11.3% – a sum of just 22.3%. Similarly, for the zero-balance loan contract,

[18] The take-up rate of a savings contract offering 4500 in phase 1 is statistically significant at 5% level. Take-up for all other contracts is statistically significant at the 1% level. For phase 2 we repeated the analysis for subjects who were offered the basic contract without reminders, Sunk or Flex treatments. Although take-up is on average slightly higher in the basic treatment, all other patterns are qualitatively similar to those described here, with and without refusers.

the sums are 30.2% + 4.3% = 34.5% in phase 1 and 26.0% + 8.9% = 34.9% in phase 2. This suggests that subjects have a stochastic demand for lumpsums; that is, their behaviour is not well captured by a framework with stationary utility functions and a stable discount factor.

To verify this conjecture, we use the pattern of take-up to infer the distribution of the time preference parameter β under different scenarios. We do so separately for credit and savings contracts. Since contract offers are randomized across subjects within each cycle, the populations of subjects offered credit contracts and savings contracts are comparable and thus they should have the same distribution of time preference parameters β. Hence for a model scenario to fit the data, the two distributions should overlap. To implement this idea, we compute, for each of the six graphs in Figure 1, the cut-off values of β for each of the six possible contracts. We then use actual take-up to infer the proportion of subjects that are *below* these cut-off values, separately for the credit and savings contracts. The implied cumulative distribution of β's should line up in a monotonic and smooth fashion. If it does not when we use the β cutoff values corresponding to a particular scenario, it means that this model scenario is rejected by the data.

The results are presented in Figure 2, combining Phase 1 and Phase 2 subjects. Four sets of markers are shown, corresponding to four values of θ, i.e.: $\theta = 1$ (linear model); $\theta = 1.1$ (high demand for lumpsum accumulation); and $\theta = 0.9$ and $\theta = 0.8$ (low lumpsum demand + desire for consumption smoothing). Each set of markers represents the estimated cumulative distribution of β's in the study population under a particular scenario. We immediately see that markers do not line up for $\theta = 1$ or $\theta = 1.1$ (they overlap instead) in both experimental phases of Table 4. In contrast, the markers for $\theta = 0.9$ and 0.8 line up in a monotonic fashion. We further note that the implied distribution of β's is smoother when assuming $\theta = 0.8$. These results imply that if we impose a constant θ across cycles, this θ is inconsistent with a demand for lumpsum accumulation.

Consistency of behavior across rounds: To summarize our results so far, we have found evidence that a large fraction of subjects cannot hold onto cash on their own (otherwise they would arbitrage), and that, on average across cycles, demand for our contracts is dominated by the time and utility cost of making installments, i.e., by $\theta < 1$. This does not imply, however, that subjects do not have occasional demand for lumpsum accumulation.

To test this idea, we investigate whether subjects vary their take-up decisions across contract cycles in ways that agree with having a single θ. To this effect, we can use a key model prediction illustrated in Figure 1: if demand for lumpsum accumulation is high ($\theta > 1$), subjects can demand both credit and savings contracts; on the other hand, if $\theta < 1$, they demand either one or the other, never both. Hence if subjects' θ is constant over time and less than 1, they should not switch between credit and saving contracts across cycles: either they only take loans, or only saving contracts, or neither.

To investigate this idea, we report in Table 5 the proportion of time that the same subject takes both a saving and a credit contract across cycles. In the first panel of the graph, we focus on individuals who take a loan with a negative net balance – implying impatience – as well as a savings contract with a zero or negative return – implying either a strong desire to postpone consumption or a desire to accumulate for a lumpsum. We have 107 individuals in phase 1 and 350 in phase 2. In both cases, the majority of subjects rejected both contracts. Of those who took a loan contract with zero or positive interest (that is, $L \leq (N-1)M$), about a quarter also took the savings contract; in Phase 2, this was 24 respondents out of 102, and in Phase 1, this was 6 respondents out of 30. Similarly, of those respondents who took at least one saving contract with a negative return, about two-thirds refused the high payout loan; in Phase 2, this was 84 respondents out of 123, and in Phase 1, this was 12 respondents out of 18. This kind of behavior cannot be accommodated by a model with a constant $\theta \leq 1$: as shown in Figure 1, when $\theta \leq 1$ there is no overlap in the ranges of β's that allow taking up both borrowing and savings contract. It follows that these subjects must have, in at least one of the cycles, a desire for lumpsum accumulation, that is, $\theta > 1$.

In the second panel of Table 5, we pursue this idea by focusing instead on individuals who *accept* either of the two low payout contracts discussed in panel 1 (i.e., with $L \leq (N-1) \cdot M$) while at the same time *refusing* a subsidized credit contract with $L > (N-1) \cdot M$. There are 101 individuals in phase 1 and 399 individuals in phase 2 who are offered both types of contracts. The majority of those who are offered low payout 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 high payout credit contract. As we have demonstrated in Figure 1, such behavior is impossible to account for with a constant θ.[19] The only way to account for it is to assume that, when they refuse the high payout credit contract, subjects are primarily concerned with smoothing consumption ($\theta < 1$) while when they take a low payout credit or savings contact, they are more concerned with lumpsum accumulation ($\theta > 1$). In other words, their θ changes over time.

Demand for lumpsum accumulation: The evidence presented so far suggests that demand for our products is primarily driven by a desire to accumulate a lumpsum. If so, we should observe that respondents use the contract payout to cover a lumpy expenditure.

To investigate this idea, we examine respondents' description of how they used the lumpsum payments in Phase 2. The top responses are shown in Figure A12 in Appendix. The top eight categories together cover about 80% of respondents. Of them, seven unambiguously correspond to

[19] Note that this behavior cannot be explained by transactions costs: individuals who face transactions costs large enough to deter them from taking a high payout credit contract should *a fortiori* refuse any other contract, especially contracts with a low payout.

lumpy purchases, whether in the form of consumption durables (home appliances and clothing), investment (home repairs and assets for a business), wedding and festival expenses, or medical expenses. The only category among the top eight that does not necessarily fit this categorization is 'food purchases', which represents 20% of respondents. We do not know the specific form of the food purchases, but food purchased in bulk often attracts quantity discounts and thus also produce a return to lumpsum accumulation (see, for example, Brune and Kerwin (2019) and Attanasio and Pastorino (2020)). We further note that several expenditure categories may be unforeseen and urgent (e.g., home repairs, medical expenses) or driven by anticipation of an income shortfall (e.g., food), as in Frederick et al. (2002). This can account for the variation in demand for lumpsum accumulation across experimental rounds.

We therefore conclude that, taken altogether, our findings suggest that the take-up of our contracts is best explained by an occasional – and sometimes unforseen – desire to accumulate a sum of money to cover a lumpy expenditure or a sudden cash need by individuals unable to save cash on their own.

Correlates of take-up: To conclude this section, we conduct a heterogeneity analysis of the demand for our product. We have argued that our savings and credit contracts open an avenue to lumpsum accumulation for individuals who find it difficult to hold onto cash. This difficulty may arise from a number of causes, such as low self-efficacy or pressure from relatives and neighbors. We therefore expect to find higher demand for commitment contracts among such individuals, provided they are able to make regular installments. Indeed our contracts are of little use to individuals for whom weekly installments are either impossible to meet because their income is too low or irregular, or inconvenient because their income is paid at intervals longer than a week. The first case rules out most casual laborers, whose wage income is highly unpredictable. It also rules out women whose low empowerment precludes commiting to future installments. The second case rules out permanent employees paid monthly. We therefore expect demand for our contracts to be highest among subjects in regular self-employment who are sufficiently empowered in their household to commit to the payment of future installments – to recall, all our respondents are women.

To investigate whether heterogenity in demand fits the above pattern, we adapt the machine learning method recently proposed by Chernozhukov, Demirer, Duflo, and Fernández-Val (2018) – see Appendix Section C for details. Following Chernozhukov et al. (2018), we describe the characteristics of the 20% of respondents with the highest adoption rate, and the 20% with the lowest.

For Phase 2, we present in Appendix Table A12 the covariates most likely to indicate a respondent's inability to hold cash, such as baseline saving difficulties, pressure to share, and low self-efficacy proxied by inability to keep track of tasks and finances.[20] The table shows large and

[20] In Appendix Table A22, we perform this comparison for all 58 of the baseline covariates used for our analysis.

highly significant differences in respondent characteristics for almost all of these covariates. Consistent with the idea that our basic contract provides a useful commitment device for individuals with low self-efficacy, we find that 89% of the highest adopters stated at baseline that they find it hard to save, compared to 54% of low adopters. Similarly, we find that the highest adopters are significantly *less* likely to describe themselves at baseline as: good at keeping track of time or finances; as following a strict schedule on finances or a tight routine; and as acting early to avoid forgetting (either generally or with respect to finances). We also find that demand for our contracts is correlated with pressure to share as reported at baseline: 95% report it among the highest adopters compared to 55% among the lowest adopters. The highest adopters are also more empowered at baseline: they report a significantly higher share of household decisions in which the woman's view is always considered, and are more than twice as likely to agree that it is appropriate for a woman to invest in her business without consulting her husband or to go shopping for a personal item on their own. All these findings for Phase 2 are in line with theoretical expectations about self-efficacy, pressure to share, and female empowerment.

Turning to Phase 1 (see Appendix Table A23), we find many similarities with Phase 2, but also a few differences: unlike in Phase 2, we find no significant difference between the highest and lowest Phase 1 adopters in terms of pressure to share. Furthermore, in Phase 1, the highest adopters are significantly *less* likely to have reported difficulties to save at baseline. We do, however, find that in both phases, the highest adopters are more likely to be self-employed than the lowest adopters, a finding that is also in line with theoretical expectations. Similarly, we find in both cases that the highest takers have larger households with higher household consumption and are more likely to be a member of a savings committee – indicating a capacity to save coupled with an interest in commitment saving contracts.

Interestingly, we note that Phase 1 respondents are much more likely to be self-employed than those in Phase 2: the *highest* adopters in Phase 2 have essentially the same self-employment frequency (22%) as the *lowest* adopters in Phase 1 (19%). The highest take-up respondents from Phase 2 also share other similarities with low take-up respondents from Phase 1 in terms of consumption expenditures, household size, membership in a savings committee, and pressure to share. This suggests that the Phase 2 and 1 samples form a continuum, with the highest take-up respondents from Phase 2 being most comparable to low take-up respondents from Phase 1. This is consistent with the fact that take-up increases, across both samples, with self-employment, income, family size, ability to save, and pressure to share.

4 Demand and explicit commitment

We now turn to the various add-ons. 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 instalments. Heterogeneity analysis suggests a role for self-commitment problems and peer pressure to share in determining demand for commitment. We directly test how demand for the product changes when the level of commitment built into the contract is raised or decreased by explicit contract features; or when the contract is augmented by nudges designed to address inattention and vary intrahousehold pressure, such as individual and family reminders respectively. The literature has showed that such features reduce the probability of default. What is less clear is whether subjects have an *ex ante* demand for these additional add-ons. Further, we are able to test whether these contractual features are valued differently when participants have to pay to save up for a lumpsum amount in a savings product or when they have to pay down to repay a lumpsum provided under a standard credit contract.

4.1 Contractual add-ons and product demand

Figure 3 shows average take-up rates across our different contractual add-ons. The figure shows (on the far left) take-up rates for the basic contract (that is, the product with neither the 'flex'/'sunk' variation nor the 'self reminder' / 'family reminder' variation); it then shows take-up rates for each of the eight possible contractual add-ons. Error bars show 90% and 95% confidence intervals on the difference in take-up relative to the basic contract. These take-up rates – and confidence intervals – are obtained 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 net balance. Standard errors are clustered at the individual level and observations from all three product cycles are combined. We show the original regression coefficients, and pairwise significance tests, in Tables A19 and A20 in the appendix.[21] Panel A of Figure 3 (at the top) shows these results for credit contracts (that is, contracts where the lumpsum is offered to be paid in the first period); Panel B (at the bottom) shows the results for savings contracts (where the lumpsum is

[21] Panels 2B and 2C of these tables 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 family members. 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 family members, the positive effect on take-up in the 'flex' treatment is smaller in magnitude and no longer significant.

to be paid in the final period).

The implications of the figure are stark: contrary to what one might expect, clients do not value additional commitment features. This is particularly evident for credit contacts. Here, of the eight variations on the 'basic, no reminders' product, demand is lower in seven cases; in three of these cases ('flex, no reminders', 'sunk, no reminders', and 'sunk, reminder to family'), the demand reduction exceeds 25% (i.e. 5 percentage points), and is significant. A joint test that take-up is equal across all nine contracts is rejected with $p = 0.011$ (see Table A19).[22] In the saving domain, demand does not appear to decrease with the added contractual features – however, we cannot reject the null hypothesis that the relative take-up pattern that we observe for credit is the same pattern as for saving.[23]

Overall, these results on takeup by contractual add-ons provide direct evidence on the most empirically relevant obstacles to holding on to cash faced by women in our sample, which are consistent with those emerging from the heterogeneity analysis. Support for the fact that inattention issues are relevant in our setting is given by the fact that increased flexibility is associated with higher demand only when coupled with reminders. This result suggests that the rigid repayment structure of the basic product also serves the function of reducing the attention costs of meeting payment obligations. It is when these cognitive costs increase due to flexibility that reminders become valuable to participants.[24] Finally, the negative effect of family reminders on demand suggests that intra-household dynamics are perceived by individuals as having a negative rather than a positive influence of repayment: especially when the cost of missing an instalment is high, in the sunk treatment, knowing that your family members will be informed of your payment obligations significantly reduce take-up of the product. These obstacles appear to exert larger impact on demand in the credit than in the saving domain, which may be explained by the more severe consequences of defaulting on a credit contract than on a saving contract. In our view, these are novel results for understanding the demand for microfinance in developing countries.

Our result in this paper – namely, our finding that respondents value the commitment implicit in microfinance, but do not value additional explicit behavioral features – not only indicates that the level of commitment, built in the basic product, is probably right for our sample of participants. It also suggests that microfinance products with a fixed repayment schedule – an extremely common

[22] In Figures A7 and A8, we correct for multiple hypothesis testing and display sharpened False Discovery Rate (FDR) q-values. Difference in take-up and late payment rates in the Sunk product compared to the basic contract remain statistically significant.

[23] When we conduct a joint test here of the null hypothesis that the take-up rate is equal across all nine contracts, we find $p = 0.321$ and we do not reject; see Table A20. When we conduct a joint test across Panel A and Panel B, of the null hypothesis that the estimates in Panel B simply scale down those in Panel A by a common ratio, we also do not reject: we obtain $p = 0.206$.

[24] This result echoes findings from the behavioral literature on planning prompts, showing that planning is valued and effective when the cognitive cost of following through is higher (Rogers, Milkman, John, and Norton, 2015); see also Bonan, d'Adda, Mahmud, and Said (2020).

form of contract across developing countries – may represent an important form of what Laibson (2018) refers to as 'shrouded paternalism'.[25] Similarly, although studies from other domains show significant demand for contracts featuring explicit commitment (Kaur et al., 2015; Bai et al., 2017), existing evidence on soft and hard commitment devices in the saving domain confirms the greater success of the former (in the form of 'labeled savings accounts') over the latter (both in terms of demand and impact on outcomes) (Karlan and Linden, 2014; Benhassine, Devoto, Duflo, Dupas, and Pouliquen, 2015). The lower demand for explicit commitment features is also consistent with a related behavioral literature on 'avoiding the ask' and control aversion (Andreoni et al., 2017; Falk and Kosfeld, 2006; Fehr and List, 2004). Our results on demand for flexibility also emphasize the importance of the specific details of flexibility, in the context of the particular contract being offered. In particular, both Barboni and Agarwal (2018) and Battaglia et al. (2019) find significantly higher demand for contracts with more flexible repayment schedules than the one we offer – provided in the form of the possibility to take a three-month repayment holiday and spread the outstanding balance over the remaining monthly instalments (in the former); and of the option to delay up to two monthly instalments with a corresponding increase in the duration of the loan cycle (in the latter).

4.2 Contractual add-ons and payment of installments

How do contract features affect the payment of installments? This question is important for shedding light on our earlier take-up analysis. If, for example, late payment problems are widespread, this would have implications for the practical viability of the products studied; similarly, if late payment rates do not differ between the basic contract and the 'sunk' variation, this might suggest that respondents are naive about the value of the commitment device for their future behavior (DellaVigna and Malmendier, 2006; John, 2020). Our design, which allows us to identify the causal impact of contract features on product takeup, implies that we cannot answer this question, as differences in repayment by contract features are also driven by selection into the different contracts. In other words, in our experiment, we see the effect of these contractual add-ons only for those subjects who do accept them, thereby allowing us to see the combined effect on both take-up and repayment performance. While not causal, this evidence is important for policy makers.

With this caveat in mind, in Figure 4, we show the rate of late payment by contractual add-ons. The structure of the figure mirrors that of the earlier figure showing take-up rates (Figure 3): we show the rate of late payment both for the basic contract and for the contractual add-ons, and we divide the analysis between credit contracts (Panel A, top) and saving contracts (Panel B,

[25] As Laibson explains: "...lots of thriving institutions have bundled commitment features that appear to be specifically designed to help agents overcome their self-control problems. On the other hand, these institutions generally don't market these commitment features – i.e., the forcing mechanisms are shrouded."

below). The figure shows late payment that is not authorised by the contract – so, for example, a respondent under the 'flex' contract who is exercising her right to delay one payment by one week is not considered here to be late. Figure A15 (appendix) repeats the analysis, but recording those women as making a late payment.

Several stylized facts deserve noting here. First, on average, the probability that at least one of a client's payments is made late is about 12%. Second, this rate generally decreases with the various contractual add-ons – and is significantly lower in several of those cases. If we compare different basic treatments with or without reminders, we see that, in both the credit and the saving domains, late payment falls with reminders. Although not statistically significant, this result is in line with the literature. We do, however, find that when reminders are combined with the sunk or flex treatment, they are associated with statistically lower late payment – often by quite a large margin relative to the basic contract. In particular, subjects who choose to take up the product when it is combined with family reminders do appear to benefit from the pressure to repay coming from their family members. Interestingly, differences in late payment by contractual add-ons appear to be larger in the saving domain.

Second, we note that subjects do enjoy quite a bit of *de facto* flexibility in meeting their installment obligations – much more flexibility than what our formal and rigid flex treatment provides. Given this, it should not be a surprise that the flex treatment shows little appeal. In fact, the apparent fall in late payment associated with the flex and self reminder treatments is a mirage: when we include late payments allowed by the flex contract (see Figure A15 in appendix), we find that, if anything the flex treatment leads to *more* late payments – although the difference remains small and is never statistically significant. Given that our flex treatment does not improve take-up – showing clients don't care for it – and that it possibly increases late payments – which raises collection costs for the lender – it is quite clear that it is not a desirable feature to add to a setting where de facto flexibility is already prevalent.

Third, we note that – with the important exception of the 'sunk' contracts – the rate of late payment is higher under saving contracts than under credit contracts. This makes intuitive sense, for two related reasons. On the one hand, 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, at which point they are returned. Therefore – for clients not facing the 'sunk' contract – subjects essentially have the option to walk away from the contract. On the other hand, 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 reneging is individually optimal for the borrower and thus has to be disincentivized by a concerted debt recovery effort. 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.

5 Robustness

5.1 Respondent understanding

In this section, we test the robustness of these results. First, we check for respondent misunderstanding of the contract; is it possible, for example, that our earlier results are driven by respondents having been confused about the contracts being offered?

In our view, there are several reasons to have a strong prior against this interpretation. In particular, we were well aware of this issue from the outset, and took several steps to ensure that the products were well understood. First, we designed the new products around a contract that is familiar to almost all of our subjects (namely, the ROSCA). In particular, variation in the timing of lump-sum payout occurs naturally in ROSCAs; variation in interest rates similarly occurs naturally.[26] Second, we conducted the experiment in close collaboration with an established microfinance organisation, already known and trusted by our subjects – who were, at the time of the experiment, all current or recent clients of that organisation. Third, the general forms of behavioral variation that we introduced have been tested in other related field contexts, without generating evidence of substantial subject confusion; this is true in respect of reminders (see, for example, Karlan et al. (2016)), of repayment flexibility (see, for example, Field et al. (2013), Czura (2015), Barboni and Agarwal (2018), Battaglia et al. (2019) and Castellanos et al. (2019)) and of the sunk repayment feature (for example, see John (2020) – and, by analogy to life insurance contracts, Anagol, Cole, and Sarkar (2017)).

Empirical evidence supports this prior. First, when asked at baseline, our respondents overwhelming agreed that they were familiar with the concept of a savings committee (96% agreed in Phase 1; 92% in Phase 2). Indeed, a substantial share had participated in a committee themselves: 51% in Phase 1, and 27% in Phase 2. Second, when we asked respondents directly for reasons that they refused the product, the overwhelming majority (about 85% in Phase 1 and 75% in Phase 2) attributed this to a lack of funds on hand to pay; almost nobody blamed a lack of understanding of the product.[27] Third, we conducted an explicit 'right/wrong' test for a hypothetical contract in Phase 1; despite asking this question approximately six months after the product was initially explained (and at least six weeks after the final take-up decision had been elicited), we found that

[26] In many ROSCAs, this variation tends to occur at the time of the payout, which introduces uncertainty; in this respect, at least, our contract is actually simpler than many ROSCAs.

[27] Of those giving reasons, about 2% provided this reason in Phase 1. In Phase 2, respondents had the option to report this in the 'other' category, but not a single respondent did so.

about 85% of respondents answered correctly. Similarly, at the same time, we asked respondents whether they agreed with the statement that 'I understand how the new contracts work'; about 60% agreed (or strongly agreed), while only about 20% disagreed (or strongly disagreed). We provide further details on these figures in Online Appendix B.

5.2 Robustness and dynamic effects

In the same appendix, we provide further analysis on the dynamics of respondent behavior. First, we disaggregate our take-up patterns by experimental wave – and show that the general take-up patterns observed in the aggregate are also observed for each experimental wave separately. Second, we test for persistence effects across waves; to do this, we regress take-up in a given wave with take-up in the previous wave (instrumenting this lagged take-up by the contract terms randomly offered in the previous wave). We find a significant causal effect of lagged take-up; respondents who take up in a given wave are about 50 percentage points more likely to take up in the following period as a result. We interpret this as a strong familiarity effect (Mehrotra, Somville, and Vandewalle, 2016). This is an interesting separate finding in its own right – but has no implications for our earlier estimates on sensitivity to offered contractual terms. Because we randomized the contractual offers in each wave, the offer terms are uncorrelated to lagged take-up – and, therefore, the inclusion or omission of lagged take-up does not change our regression results. We show this empirically in the same appendix. Finally, we show the effect of dropping individuals who ever defaulted (to check that our conclusions are not driven by defaulters having been progressively excluded from the experiment). We show that overall take-up patterns are unaffected by this.

6 Consequences of adopting

Finally, we estimate the impact of treatment on business and household outcomes. We do this by exploiting the random assignment to the control group: we compare outcomes for control participants (who were not invited to take up any of our commitment contracts) with treated participants (who were). Given the relatively small size of our lumpsum – and given previous experimental results in the literature on microfinance – it would be surprising if this product were to have large effects on business or household outcomes. However, were we to find that product has large effects, this would shed a different light on our earlier explanations for product demand; for this reason, it is important to estimate these impacts.

We provide a detailed analysis in Online Appendix C. In short, we find no robust effects on business or 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 (2018a) and

Meager (2018b)) and is consistent with our preferred interpretation of the demand for lumpsum.[28]

7 Conclusions

The optimal design of commitment features remains an open question for empirical research. This paper makes progress on that issue by testing the role of commitment devices in microfinance, in two distinct ways. First, we test directly whether the rotating structure of a ROSCA 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 et al., 2018). In this paper, we show that the same structure can be used for a product with larger payments, over a longer period. We find substantial demand for such a product. Many microfinance clients 'borrow to save' (Collins et al., 2009; Armendáriz and Morduch, 2010; Bauer et al., 2012; Kast and Pomeranz, 2018; Afzal et al., 2018). But take-up is higher for credit contracts than for commitment savings contract, a finding we attribute to the unforseen and urgent demand for lumpsum accumulation. 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 use additional 'behavioral' add-on features in the form of reminders (both for respondents and for respondents' family members), formal flexibility in installments, and a cancellation fee. Our design allows to compare how demand for these features varies between the saving and credit domain. Our findings show that all these contract add-ons are not valued by clients – on the contrary, they appear to be actively disliked, particularly when combined with credit contracts. Inattention and intrahousehold pressures to share appear to lower takeup of our product. These results have important policy implications for thinking about the future design of microfinance products. Specifically, our results imply that microfinance institutions should *not* be seeking to build explicit commitment features into their products – not because their clients have no demand for commitment devices, but because that demand is already met through the regular payment schedule built into a standard microcredit or into a savings commitment contract of the type studied here.

[28] Further, we check for heterogeneity in these effects, by the quintiles of take-up rates estimated earlier. Specifically, we estimate treatment effects separately for each of those quintiles, using the bootstrap method of Chernozhukov et al. (2018) both for obtaining point estimates and for inference. We do not find, for example, that some quintiles are benefiting from being offered the treatment while others are not.

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Tables and Figures

Table 1: **An illustrative contract structure**

	Week 0	Week 1	Week 2	Week 3	Week 4	Week 5	Week 6
Participant pays	*take up*		1000	1000	1000	1000	1000
Bank pays	*decision*	4500					

This table shows a payment schedule for a basic contract with lump-sum in Week 1 and net balance of -10%.

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

	District	Offices	Respondents
Phase 1	Bhakkar	3	418
	Chakwal	5	372
Total		8	790
Phase 2	Khushab	5	725
	Mandi Bahauddin	4	674
	Jhelum	6	296
	Rawalpindi	2	721
Total		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**

Phase 1

Basic treatment (1/2) n = 394
Control group (1/2) n = 396

Phase 2

Basic treatment with **no reminders** (1/12) (n = 197)	**Basic treatment** with **respondent reminders** (1/12) (n = 204)	**Basic treatment** with **family reminders** (1/12) (n = 199)
Sunk treatment with **no reminders** (1/12) (n = 201)	**Sunk treatment** with **respondent reminders** (1/12) (n = 202)	**Sunk treatment** with **family reminders** (1/12) (n = 207)
Flex treatment with **no reminders** (1/12) (n = 202)	**Flex treatment** with **respondent reminders** (1/12) (n = 204)	**Flex treatment** with **family reminders** (1/12) (n = 198)

Control group (1/4) 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**

PHASE 1

Lumpsum amount:	4500	5000	5500
Net balance:	-10%	0%	10%
Lumpsum paid in...			
Week 1	8.2%	30.2%	47.0%
	(2.1%)	(3.3%)	(3.6%)
Week 6	2.7%	4.3%	11.0%
	(1.2%)	(1.5%)	(2.3%)

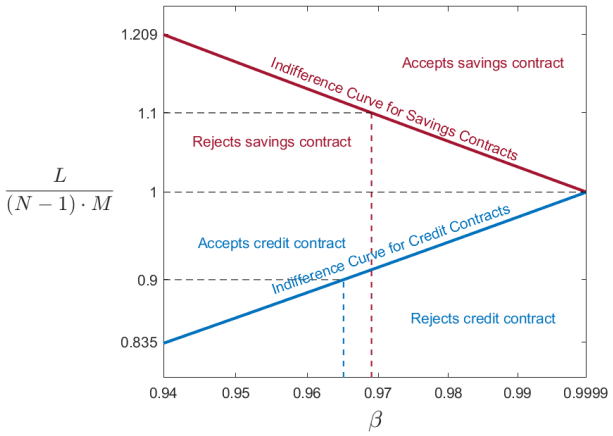
PHASE 2

Lumpsum amount:	3200	3500	3800
Net balance:	-8.6%	0%	8.6%
Lumpsum paid in...			
Week 1	11.0%	26.0%	37.2%
	(1.1%)	(1.5%)	(1.6%)
Week 8	4.1%	8.9%	11.3%
	(0.7%)	(1.0%)	(1.1%)

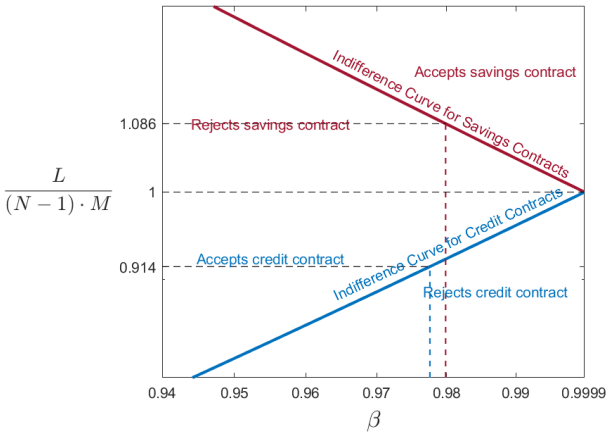
This table shows the average take-up rates by contractual terms (lumpsum value and timing). Standard errors of estimated take-up rates (in percentage points) are reported in parentheses. Weekly instalments were PKR 1000 in Phase 1 (paid in five of six weeks) and PKR 500 in Phase 2 (paid in seven of eight weeks); the table reports take-up for each combination of lumpsum amount (where we report both the lumpsum value and the net balance implied by that lumpsum amount) and the time of lumpsum payout.

Figure 1: **Take-up predictions: Indifference curves**

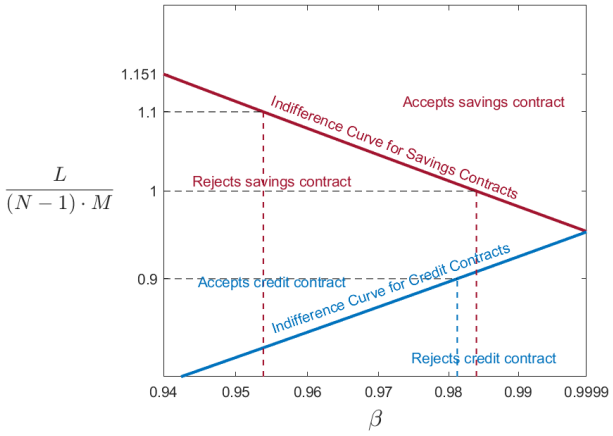
PANEL A: PHASE 1, $\theta = 1$



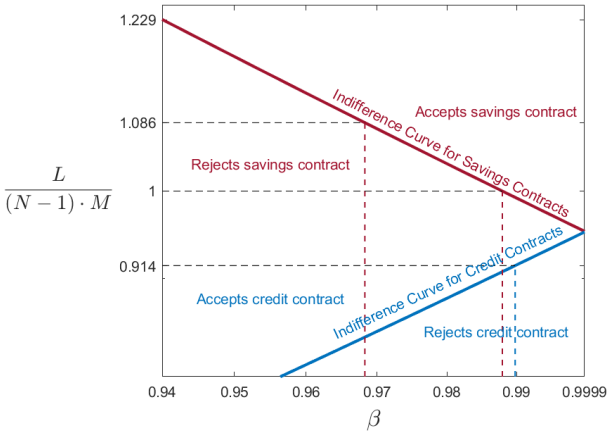
PANEL B: PHASE 2, $\theta = 1$



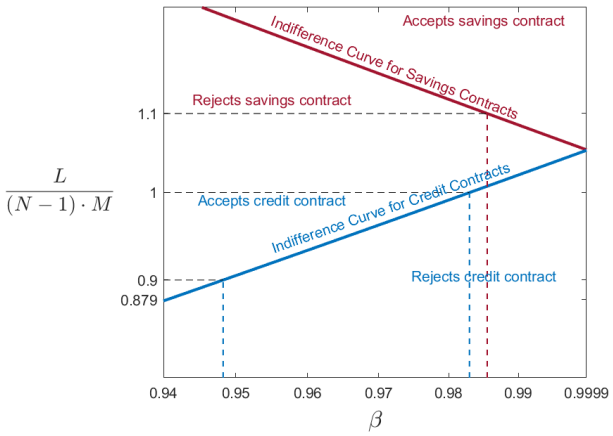
PANEL C: PHASE 1, $\theta = 1.05$



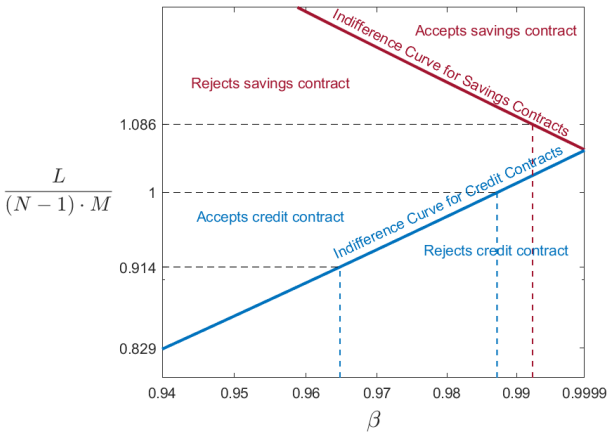
PANEL D: PHASE 2, $\theta = 1.05$



PANEL E: PHASE 1, $\theta = 0.95$

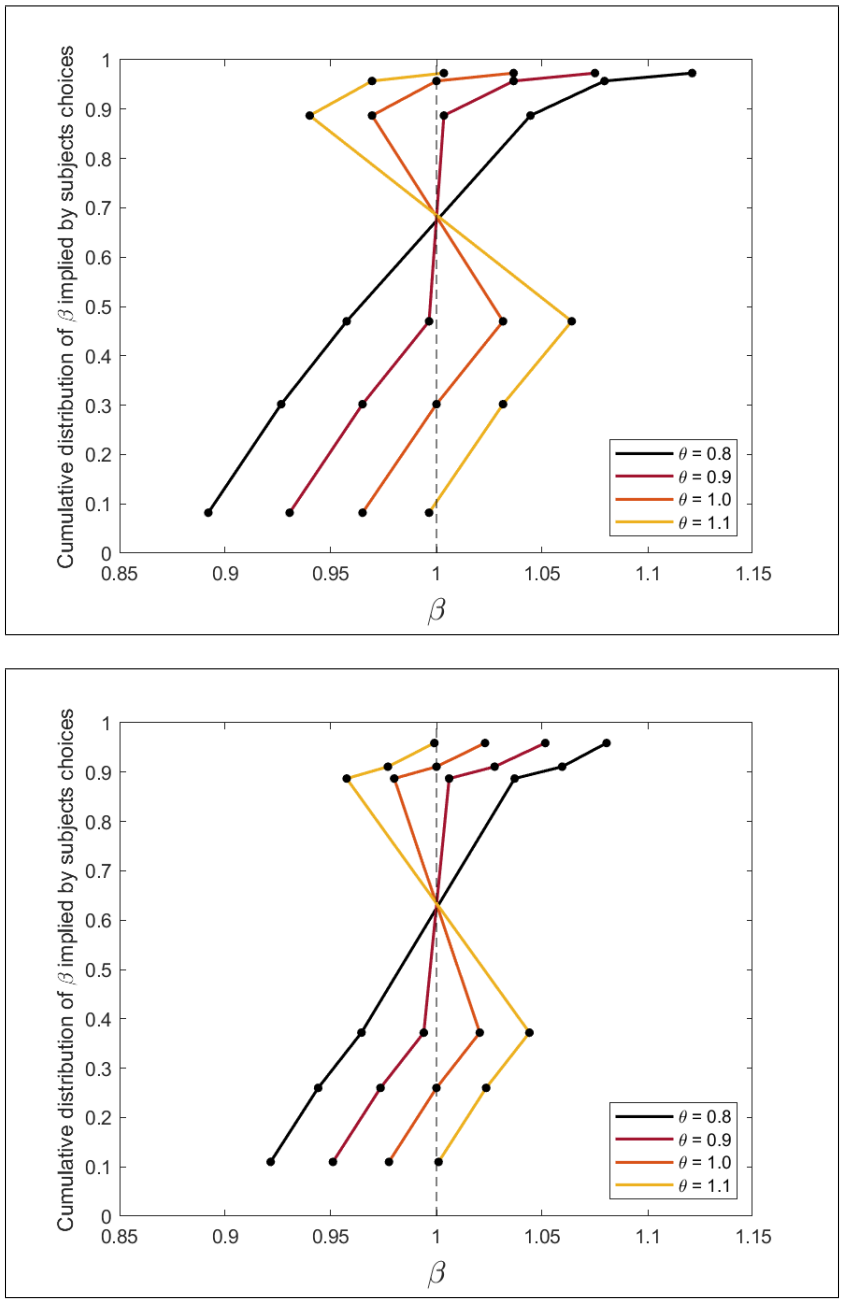


PANEL F: PHASE 2, $\theta = 0.95$



Each graph shows the two indifference curves from equations 1 and 2. Graphs on the left related to Phase 1 of the experiment; graphs on the right related to Phase 2. In each case, the horizontal axis shows variation in β, and the vertical axis shows the payout ratio $L/[(N-1)\cdot M]$. We use a log transformation for β because, with that transformation, the indifference curves are approximately linear. Each graph shows the three values of $L/(N-1)M$ used in the experiment: 1.1, 1, and 0.9 for Phase 1; and 1.086, 1 and 0.914 for Phase 2. The downward-sloping line in the upper section of each graph shows the indiffence curve for saving; points above the line imply take-up of a saving contract with payout ratio $L/(N-1)M$. The upward-sloping line in the lower section of each graph is the indifference curve for borrowing: points above it imply take-up of a loan with $L/(N-1)M$.

Figure 2: **Cumulative Distribution of β for different posited values of θ**



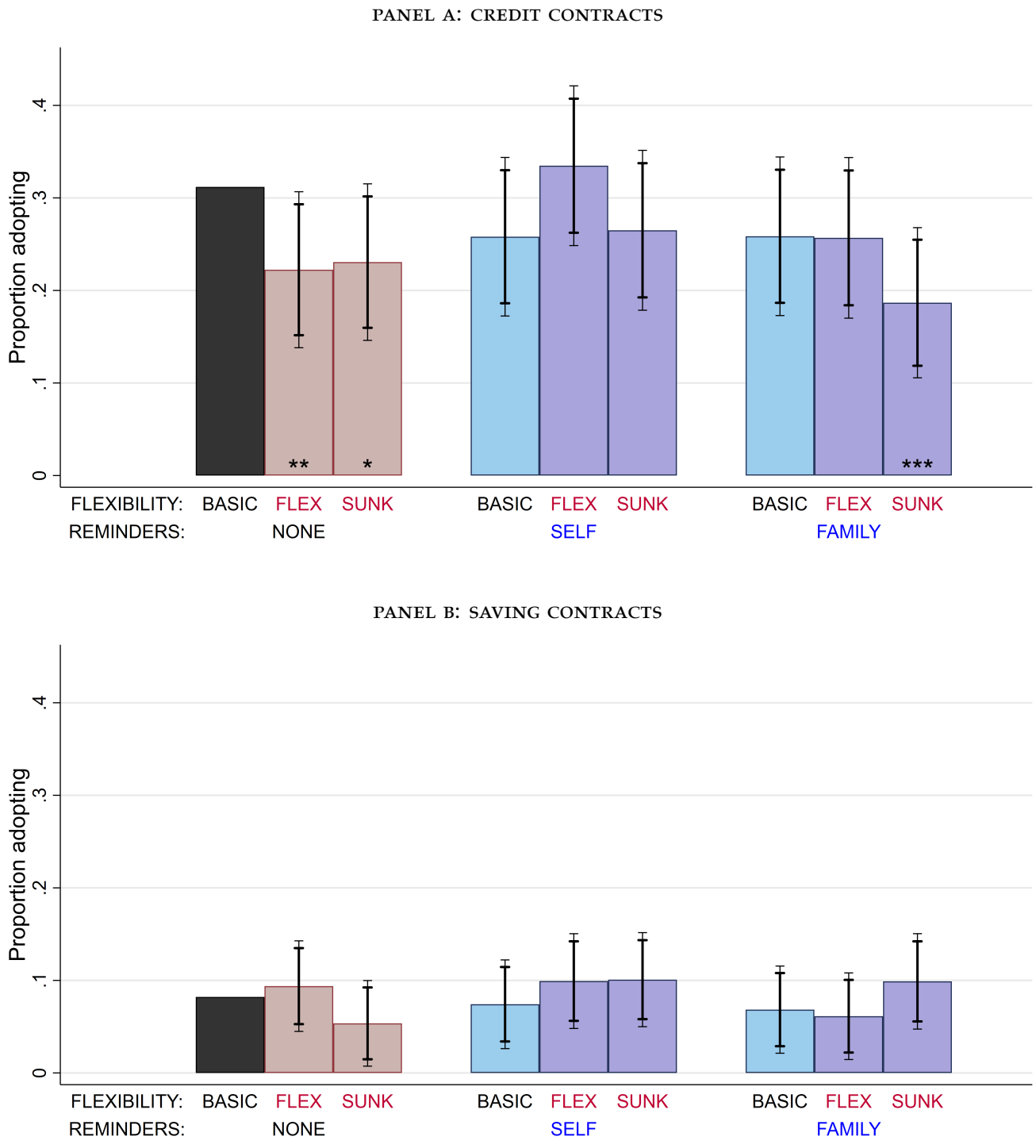
These figures show the cumulative distribution of discount factor β that is implied by take-up choices made by subjects across all contract cycles. Four distributions are constructed based on four different assumptions regarding parameter θ, i.e.: $\theta = 1$ (linear model); $\theta = 1.1$ (desire for lumpsum accumulation); and $\theta = 0.9$ and 0.8 (concerns for consumption smoothing). The top Figure reports the findings form the Phase 1 experiment; the bottom Figure reports the findings from the Phase 2 experiment. Each point on the x-axis represents the maximum value of β for which a choice would be made (either take-up or rejection), conditional on a value of θ. All cut-off values of β are obtained as the value that equalizes the two sides of either equations 1 and 2, with the left-hand side multiplied by the relevant θ. Some implied values of β exceed 1, implying that only individuals who wish to postpone consumption would make the relevant choice, conditional on θ and the other assumptions of the model. The y-axis shows the proportion of choices that imply at most a particular β cutoff. Each line therefore represents the implied cumulative distribution of β across subjects. Normally, that implied cumulative distribution should be monotonic. Hence failure of monotonicity (and smoothness) implies rejection of a particular value of θ.

Table 5: **Proportion of Respondents who Display Preference Violations**

	Phase 1	Phase 2
Subjects who were offered at least one high payout loan (i.e., with $L \geq (N-1)M$) and a low payout savings contract (i.e., with $L \leq (N-1)M$):		
took neither	76	225
took the high payout loan	30	102
took the low payout savings contract	7	47
took both	6	24
Total:	107	350
conditional on taking the loan once, the subject takes the savings contract	20%	24%
conditional on taking the savings contract once, the subject takes the loan	86%	51%
Subjects who were offered at least one high payout loan ($L > (N-1)M$) and at least one low payout contract ($L \leq (N-1)M$) – either a loan or a savings contract:		
took both	6	39
took the high payout loan	89	315
took the low payout loan or saving contract	18	123
took neither	0	0
Total:	101	399
conditional on taking a low payout contract once, refused the high payout loan	67%	68%
conditional on refusing a high payout loan once, took a low payout contract	100%	100%

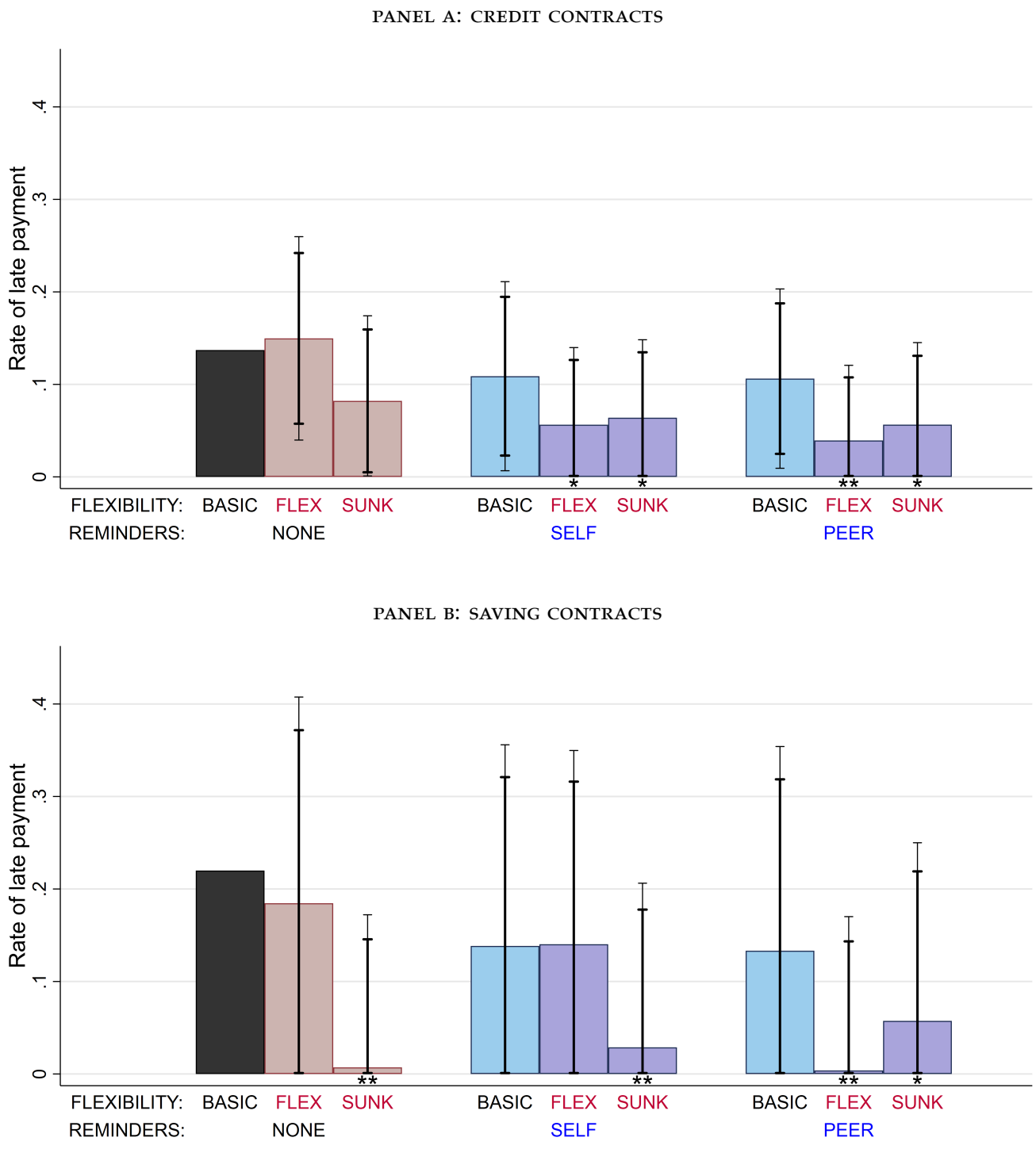
This table reports the take-up decisions made by subjects across contract cycles. Automatic refusers are omitted from these calculations.

Figure 3: **Average take-up by contractual add-ons**



This figure shows the average take-up for the basic product (that is, the product with neither the 'flex'/'sunk' variation nor the 'self'/'family' variation), and take-up for each of the eight possible add-ons. Error bars show 90% and 95% confidence intervals on the difference in take-up to the basic contract. Stars indicate a significant difference from take-up of the basic contract; that is, we reject a null hypothesis of equal take-up rates for the 'sunk' variation and for the 'sunk and family' variation, each at the 5% significance level.

Figure 4: **Rate of late payment by contractual add-ons**



This figure shows the rate of late payment for the basic product (that is, the product with neither the 'flex'/'sunk' variation nor the 'self'/'family' variation), and for each of the eight possible variations. Note that we are here studying the rate of late payment; that is, we use a linear probability model for having delayed payment, for the subsample of observations where the respondent agreed to the contract. Error bars show 90% and 95% confidence intervals on the difference in rate of late payment to the basic contract. Stars indicate a significant difference from the basic contract.