Time preference and liquidity: experimental evidence from Pakistan
Pre-Analysis Plan

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1 Introduction

This document outlines the pre-analysis plan for a field experiment on time preference and liquidity amongst female microfinance clients in Pakistan. The primary purpose of the experiment is to test whether apparent inconsistencies in individuals' responses to time preference elicitation activities — typically attributed to quasi-hyperbolic discounting — can alternatively or additionally be explained by individuals

integrating their background income expectations into their responses. To achieve this, I introduce experimental treatments which provide anticipated and unanticipated shocks to individuals' liquidity constraints, and which vary the salience of those shocks at the time of eliciting individuals' time preferences.

The document summarises: i) the theoretical framework and hypotheses that the experiment is designed to test; ii) the survey, activities and treatment arms; iii) the sample and randomisation; iv) the data (including the planned construction of variables and treatment of outliers); v) the tests of balance that will be performed; and vi) the regression specifications (including robustness checks) that will be reported.

The blinded data has been regularly checked to identify: i) enumerator diligency (missing observations and missing values); ii) attrition; iii) correct adherence to replacement procedures (using wait-list participants); and iv) correct adherence to payment procedures (including frequency of outcomes in payment draws). Otherwise the data remain unexamined, and will not be unblinded nor analysed until after registration of this pre-analysis plan.

2 Theoretical Framework

The experimental design contains four measurements of individual participants' time preferences:[1]

- From the "near frame" questions at $t = 0$: the amount of money $x_{1.0}$ that would make the subject indifferent between receiving a given, fixed sum x at $t = 0$ or $x_{1.0}$ at $t = 1$.
- From the "far frame" questions at $t = 0$: the amount of money $x_{2.0}$ that would make the subject indifferent between receiving the same fixed sum x but now at $t = 1$ instead of $t = 0$, or $x_{2.0}$ at $t = 2$

[1] Each of these is measured via two different elicitation methods — multiple price lists and present-equivalents — as described in section 3.

- From the "near frame" questions at $t = 1$: the amount of money $x_{2.1}$ that would make the subject indifferent between receiving the same fixed sum x at $t = 1$ or receiving $x_{2.1}$ at $t = 2$.
- From the "far frame" questions at $t = 1$: the amount of money $x_{3.1}$ that would make the subject indifferent between receiving the same fixed sum x at $t = 2$ or receiving $x_{3.1}$ at $t = 2$.

Observations of $x_{1.0} \neq x_{2.0}$ for a given individual constitute static choice reversals, which violate the property of "stationarity". Similarly, observations of $x_{2.0} \neq x_{2.1}$ constitute dynamic choice reversals, which violate "time-consistency". Finally, observations of $x_{1.0} \neq x_{2.1}$ and/or $x_{2.0} \neq x_{3.1}$ constitute a violation of "time-invariance" (Halevy, 2015).

Before eliciting these measurements, I introduce experimental variation in individuals' liquidity constraints via the treatment arms described in section 3. The specifications for estimating treatment effects, as listed in section 6, place no specific assumptions on the underlying utility function generating the observations. However, the following theoretical framework – a variant on the canonical "beta-delta" model — will be used to guide predictions and possibly eventual structural estimation.

An agent maximizes her individual utility function of the form:

$$max \;\; U_{it} = u_i(\kappa_i y_{it} + x_{it}) + \beta_i \sum_{s=t+1}^{T} \delta^{s-t} E_{it}[u_i(\kappa_i y_{is} + x_{is})], \tag{1}$$

where y_{it} is background income, x_{it} is experimental income from the time preference elicitation questions, δ_i is i's discount factor as applied to period t, β_i is i's degree of present bias ($\beta_i = 1$ yields time-consistent preferences) and $0 \leq \kappa_i \leq 1$ is the degree to which i integrates her background income when making experimental decisions. To illustrate the main intuition, equation 1 contains the simplifying assumption

that the individual can neither save nor borrow.[2]

At $t = 0$, for an individual to be indifferent between receiving x at $t = 0$ versus the chosen amount $x_{1.0}$ at $t = 1$, it must be the case that:

$$u_i(\kappa_i y_{i0} + x) + \beta_i \delta_i E_{i0}[u_i(\kappa_i y_{i1})] = u_i(\kappa_i y_{i0}) + \beta_i \delta_i E_{i0}[u_i(\kappa_i y_{i1} + x_{i1.0})]. \tag{2}$$

Similarly, at $t = 0$ for her to be indifferent between x at $t = 1$ versus $x_{2.0}$ at $t = 2$, it must be the case that:

$$E_{i0}[u_i(\kappa_i y_{i1} + x) + \delta_i u_i(\kappa_i y_{i2})] = E_{i0}[u_i(\kappa_i y_{i1}) + \delta_i u_i(\kappa_i y_{i2} + x_{i2.0})]. \tag{3}$$

Combining equations 2 and 3, an observation of $x_{1.0} > x_{2.0}$, i.e. a static choice reversal in the direction of present-bias, will occur if:

$$\frac{u_i(\kappa_i y_{i0} + x) - u_i(\kappa_i y_{i0})}{\beta_i E_{i0}[u_i(\kappa_i y_{i1} + x_{i1.0}) - u_i(\kappa_i y_{i1})]} = \delta_i > E_{i0}[\frac{u_i(\kappa_i y_{i1} + x) - u_i(\kappa_i y_{i1})}{u_i(\kappa_i y_{i2} + \boldsymbol{x_{i1.0}}) - u_i(\kappa_i y_{i2})}]. \tag{4}$$

This may be driven by $\beta_i < 1$; and $y_{i0} = E_{i0}[y_{i1}] = E_{i0}[y_{i2}]$ or $\kappa_i = 0$. However, if $\kappa_i > 0$, equation 4 may instead be driven by a variety of expectations over income such that the individual foresees a higher marginal rate of substitution between consumption at $t = 0$ and $t = 1$ than between $t = 1$ and $t = 2$, i.e. that the individual expects to be less liquidity-constrained in the "far frame" then the "near frame".

[2] In an extension in the paper, I will discuss the implications of relaxing the assumption that she cannot save but maintaining the assumption that she cannot borrow.

Turning to choices at the later date, at $t = 1$, for her to be indifferent between x at $t = 1$ versus $x_{2.1}$ at $t = 2$, it must be the case that:

$$u_i(\kappa_i y_{i1} + x) + \beta_i \delta_i E_{i1}[u_i(\kappa_i y_{i2})] = u_i(\kappa_i y_{i1}) + \beta_i \delta_i E_{i1}[u_i(\kappa_i y_{i2} + x_{i2.1})]. \tag{5}$$

Combining equations 3 and 5, an observation of $x_{2.1} > x_{2.0}$, i.e. a dynamic choice reversal in the direction of present bias, will occur if:

$$\frac{u_i(\kappa_i y_{i1} + x) - u_i(\kappa_i y_{i1})}{\beta_i E_{i1}[u_i(\kappa_i y_{i2} + \boldsymbol{x_{i2.0}}) - u_i(\kappa_i y_{i2})]} > \delta_i = E_{i0}[\frac{u_i(\kappa_i y_{i1} + x) - u_i(\kappa_i y_{i1})}{u_i(\kappa_i y_{i2} + x_{i2.0}) - u_i(\kappa_i y_{i2})}]. \tag{6}$$

Once more, this may be driven by $\beta_i < 1$ and either $\kappa_i = 0$ and/or $y_{i1} = E_{i0}[y_{i1}]$ and $E_{i0}[y_{i2}] = E_{i1}[y_{i2}]$. However, it may instead be driven by $\kappa_i > 0$, and a realisation of y_{i1} such that $u_i(\kappa_i y_{i1} + x) - u_i(\kappa_i y_{i1}) > E_{i0}[u_i(\kappa_i y_{i1} + x) - u_i(\kappa_i y_{i1})]$, i.e. by a higher realised marginal utility of consumption at $t = 1$ than was anticipated at $t = 0$, because of a low draw from i's subjective probability distribution over y_{i1}. Alternatively, it may be driven by $\kappa_i > 0$, and an downward-revision of $E_{i1}[u_i(\kappa_i y_{i2} + \boldsymbol{x_{i2.0}}) - u_i(\kappa_i y_{i2})]$ compared to $E_{i0}[u_i(\kappa_i y_{i2} + x_{i2.0}) - u_i(\kappa_i y_{i2})]$: i.e. a downward revision in the expected marginal utility of consumption at $t = 2$, for example if the individual receives a signal between $t = 0$ and $t = 1$ that the harvest at $t = 2$ is going to be better than originally anticipated.

The reverse argument also applies for static and dynamic choice reversals in the direction of *future* bias: a static choice reversal in the direction of future-bias can potentially be explained by an anticipated *downward* trend in background income; and a dynamic reversal in the direction of future-bias can potentially be explained by a subjectively *high* draw of income at $t = 1$, or a *downward* revision of i's income forecast for $t = 2$.

3 Experiment

3.1 Survey and activities

Each individual in the sample was interviewed twice: a baseline session on day one and a revisit session on day fifteen. Thus 50% of the sample were assigned to each treatment arm. Each session consisted of the following elements:

Surveys:

1. Baseline survey (baseline session only)
2. Income and expenditure survey

Payment explanations:

3. Participation fee treatment
    - Baseline: explanations to all participants; payment of basic fee to first half of participants
    - Revisit: payment of basic fee to second half of participants; bonus draw and payment for second half of participants
4. Explanation of incentive structure for activities

Activities:

5. Time preference activities, near frame:
    - Multiple price list

- Present equivalent

6. Time preference activities, far frame:
    - Multiple price list
    - Present equivalent
7. Control activities:
    - Risk preferences (certainty equivalent task)
    - Probability expectations (novel task)
    - Cognitive tasks (mathematics, digit span, Stroop test)
8. Activity payment draw
    - Baseline: bonus draw and payment for first half of participants; activity payment draw
    - Revisit: activity payment draw for participants who drew not to be paid for activities at baseline

The order of activity blocks 5, 6 and 7 was randomised across participants (see section 3.3). Each participant received the same activity block order at revisit as she had received at baseline.

3.2 Incentivization

The time- and risk-preference activities were incentivized, whilst the probability expectations and cognitive tasks were not, precisely because the latter tasks were designed to measure biases and thus I did not want to induce artificial effort or reporting of the mathematically "correct" answer. Due to budget limitations and to ensure that activity payments did not overwhelm any effect of the participation fee, respondents were paid for just one of their choices within one of the activities, either at baseline or at revisit.

The incentive structure for the activities was explained carefully to participants during step 4, as was the fact that the selection of a day, activity and question for payment would be determined randomly by them drawing out balls from a bag. It was emphasised to participants that any single one of their forthcoming responses had a chance of being selected for payment.

The procedure for paying respondents for the activities was as follows:

- At the end of the baseline interview, the individual first drew a ball from a bag to determine whether she would be paid for her responses to the baseline activities or for her responses to the revisit activities. The probabilities were tilted 10%-90% towards being paid for the revisit activities, so that most respondents would not be answering hypothetically at revisit (since those who had been paid for their responses at baseline knew at revisit that they would not be paid again for their responses at revisit).[3]

- Next — or at the end of the revisit session, if the respondent drew to be paid at revisit — the respondent drew another ball to determine whether she would be paid for the risk or the time activities; and if she drew for the time activities, she drew further balls to determine which activity (multiple price list or present equivalent) and which frame (near or far) she would be paid for.

- Once the exact risk or time activity had been determined, she then drew a further ball to determine which question number within the activity she would be paid for.

- The enumerator then displayed her response and paid her in accordance with the response she had given.

- If the response involved payments in the future, the enumerator made out a payment voucher clearly stating the time and amount of the future payment, and a member of the survey team returned to the

[3] This of course in principle leads to more "high-powered" incentives for the remaining respondents at revisit, as those respondents who have not yet been paid for an activity know that they will be paid for one of their responses at revisit. However, this should not affect the results as there should be no difference in this effect across treatment and control; see section 3.3.

household on that date to make the payment.[4]

3.3 Treatment arms

There are three cross-cutting axes of randomisation in the experiment. Assignment within each of these treatment arms was pre-determined by computer at the beginning of the study, as detailed in section 4.2.

1. **Participation fee timing** *(randomised at the individual level)*:

    - When gaining individuals' consent to participate at the start of the baseline interview, individuals were informed that there would be two interviews — one that day and one exactly two weeks from that day — and that they would receive a 1000 PKR combined participation fee for participating in both interviews. The timing of the payment of this fee was not mentioned.[5]
    - Once the baseline session had begun, after the surveys and immediately prior to the activities, respondents were given one of two participation fee treatments: half of the participants in each village were paid the 1000 PKR fee instantly and were given a receipt, whereas the other half were informed that they would be paid during the revisit interview and were given a voucher. The purpose was to induce an unanticipated immediate positive liquidity shock for those paid at baseline, and an anticipated positive liquidity shock arriving in two weeks' time for those who were informed they would be paid at revisit.
    - The revelation of treatment status occurred after the surveys but before the activities so that treatment status could not influence responses in the baseline survey and baseline income and

[4] This procedure was also explained at the start of the time preference activities to reassure participants that if they selected amounts in the future then they would be paid and would not incur transaction costs since the enumerator would come to their home.

[5] It is reasonable to assume that if respondents inferred anything about the timing of the participation fee from this description, it was that the fee would be paid at the end of the second interview. Thus those who were eventually paid on day one received a positive surprise, whereas those who were paid on day fifteen were not deceived in any way when consenting to participate.

expenditure survey, but so that all participants were informed of the presence and timing of their 1000 PKR base fee prior to responses in the activities.

- For transparency and in case participants communicated after the baseline interviews, participants were told that other participants might have been paid either at baseline or at revisit due to budget management reasons, and that this was determined entirely randomly by computer.
- **Bonus participation fee:** An additional source of random variation in the easing of liquidity constraints was introduced via a random participation fee bonus of an additional 1000 PKR:
  - The probability of this bonus was 20%, and whether a participant would receive the bonus or not was determined ex post by allowing the participant to draw a ball from a bag with one green and four red balls.
  - Participants who were paid the basic 1000 PKR participation fee before the baseline activities drew for the bonus at the end of the baseline session; whereas participants who were paid the 1000 PKR before the revisit activities also drew for the bonus before the revisit activities. In this way, no participant knew before the baseline activities whether she would receive the bonus, and every participant knew before the revisit activities whether she would receive the bonus.
  - The possibility of the bonus, and the procedure and timing of the bonus draw, was explained to all participants during the baseline payment explanations, at the same time as the explanation of the base 1000 PKR participation fee and whether the participant would receive that fee on day 1 or day 15. In this way, all participants understood the possibility of the bonus 1000 PKR participation fee, and the timing of when the bonus draw would be made, before answering the baseline activities.

2. **Activity order** *(randomised at the individual level)*:

- Within each of the above treatment arms, I randomised whether the respondent received the

block of time preference activities or the block of control activities first after the payment explanations. The purpose of this was to randomly vary the salience of background liquidity constraints (as elicited in the income and expenditure survey) and experimentally-induced liquidity constraints (as provided by the payments) while respondents were answering the time-preference activities.

- In addition, I also randomised whether the individual received the near or the far frame activities first within the block of time preference activities. The purpose of this was to vary whether liquidity constraints were most salient in the near or the far frame, and indeed to test whether any inconsistency across and individual's responses in the near and far frames was robust to order effects.

3. **Survey timing** *(randomised at the village level)*:

    - All villages in the original sample were randomly assigned to one of the baseline survey dates, and thus mechanically to a revisit survey date fifteen days later. The wheat harvest began approximately halfway through the survey period; thus this procedure randomly determined which villages received both baseline and revisit interviews before the start of the harvest, which villages received baseline interviews before the start of the harvest and revisit interviews around the start of the harvest, and which villages received both baseline and revisit interviews after the start of the harvest (and just before the start of Ramadan). The purpose of this was to randomly vary to what extent liquidity constraints had begun to be eased by the reaping and selling of crops and/or tightened again by the onset of Ramadan.

3.4 Predictions

The treatment arms provide a way to test whether $\kappa_i > 0$ as outlined in the theoretical framework:

1. **Effect of participation fee treatments:** Table 1 describes the predicted treatment effects of the participation fee timing, and the additional participation fee bonus. For ease of exposition in relation to the theoretical framework, the predictions for treatment effects (1) and (2) are described relative to no participation fee at all. However, since by design the predictions for treatment effects (1) and (2) go in opposite directions, it follows that the same predictions also hold (and with greater predicted magnitude) for the relative difference between treatment arms (1) and (2).

2. **Effect of activity order treatments:** If the magnitude of κ_i depends positively on the salience of y_i, then the following predictions hold:

    - If the salience of background income and participation fees, declines over the course of the experimental session, each of the treatment effects in table 1 is predicted to be larger in magnitude for those who answer the time preference activities rather than the control activities first.
    - If salience declines even within the space of one activity, those who answer the near frame first or the far frame first may exhibit significantly stronger or weaker treatment effects from the participation fees; and this may vary across treatment arms.

3. **Effect of survey timing treatments:**

    - Individuals in those villages which receive their baseline, or baseline and revisit, surveys before the onset of the harvest are likely to anticipate higher future income (as a result of the harvest) compared to present income. They should therefore exhibit more static reversals in the direction of "present-bias" and fewer in the direction of "future-bias".
    - These individuals are also likely to be more liquidity-constrained, and thus should exhibit stronger treatment effects from the participation fees as they are less able to smooth experimental payments via saving or borrowing.

- Individuals who are surveyed entirely after the harvest are more likely to expect their future income to remain constant, or even to decline given the onset of Ramadan. They should therefore exhibit fewer static reversals in the direction of "present-bias".
- These individuals are also more likely to have experienced an easing of liquidity constraints following the harvest, and thus they should exhibit smaller treatment effects from the participation fees as they are more able to smooth experimental payments.

4 Sampling and randomisation

4.1 Sample

The experimental sample consists of 530 female microfinance clients of the agricultural intervention and microfinance organisation National Rural Support Program. These individuals were sampled at a rate of ten individuals per village across 53 villages, in the district of Sargodha in the province of Punjab, Pakistan.

Sampling frame: The original sampling frame consisted of all villages within Sargodha district which had more than ten active NRSP clients.[7,8] From this list I eliminated those villages which had already

[6] The effect of winning the bonus at $t = 0$ is not clear, since the bonus is drawn after the baseline time preference activities and so cannot be incorporated into $t = 0$ choices, but may have been spent by $t = 1$.

[7] I sampled active clients since they are the most relevant potential target population for a range of financial products with commitment features that NRSP is in the process of developing or has already rolled out in other regions of Pakistan. Furthermore, the existence of a strong ongoing relationship with NRSP was likely to maximise the respondents' trust that NRSP would deliver future-dated payments as part of the experiment, and also minimize attrition from the second survey round.

[8] The requirement of at least ten active borrowers per village was imposed because pre-piloting suggested that it would be feasible to sample ten individuals from each village, given that the experimental design allots one day to each village for each round of interviews (see section 3.3).

Table 1: Predicted treatment effects - participation fees

	Treatment	**Theoretical effect**	**Prediction if $\kappa_i = 0$ and/or perfect borrowing and saving**	**Prediction if $\kappa_i > 0$ and imperfect borrowing and saving**
(1)	Announcement at baseline that there will be a participation at revisit ($t = 1$)	$\uparrow E_0[y_1]$; $\uparrow y_1$	No effect	$\uparrow x_{1.0}$ i.e. less patient in near frame; $\downarrow x_{2.0}$ i.e. more patient in far frame; $\uparrow$ number of "present-biased" reversals & amount of "present-bias"; $\downarrow$ number of "future-biased" reversals & amount of "future-bias"; $\uparrow$ increasing patience & $\downarrow$ decreasing patience in near frame; $\downarrow$ increasing patience & $\uparrow$ decreasing patience in far frame
(2)	Participation fee at baseline ($t = 0$)	$\uparrow y_0$	No effect	$\downarrow x_{1.0}$ i.e. more patient in near frame; $\downarrow$ number of "present-biased" reversals & amount of static "present-bias"; $\uparrow$ number of "future-biased" reversals & amount of "future-bias"; $\downarrow$ increasing patience & $\uparrow$ decreasing patience in near frame
(3)	Wins bonus at $t = 1$[6]	$y_1 > E_0[y_1]$	No effect	$\downarrow$ $x_{2.1}$ i.e. more patient in revisit near frame; $\uparrow$ number of "future-biased" static reversals at $t = 1$ & amount of static "future-bias" at $t = 1$; $\downarrow$ number of "present-biased" revisions & amount of dynamic "present-bias"; $\uparrow$ number of "future-biased" dynamic revisions & amount of dynamic "future-bias"; $\uparrow$ increasing patience & $\downarrow$ decreasing patience in near frame

been randomly selected to participate (as either treatment or control villages) in another ongoing research project evaluating a healthcare insurance product, to avoid any danger of contaminating that research project. This left 54 villages spread across the three Tehsils of Sargodha district: Sargodha, Bhalwal and Shahpur. Following the randomisation procedure outlined below in section 4.2, three of these villages were randomly assigned to be pilot villages, 48 to be designated survey villages, and three to be reserve villages in case unforeseen circumstances would necessitate the dropping of one or more of the 48 designated villages. Indeed, two of the 48 designated villages turned out to have non-standard NRSP client selection procedures, such that the actual NRSP clients there were male rather than female. These two villages were therefore dropped before any interviews took place, and replaced with the first two reserve villages. The final reserve village was also surveyed following a minor breach of protocol in another designated survey village, as described below.

Breach of interview date protocol: According to the experimental protocol, each individual is to be interviewed twice: a baseline interview on "day one", a date randomly determined at the village level (as detailed in section 4.2); and a revisit interview on "day fifteen", exactly two weeks after the baseline. There was a minor breach of experimental protocol in the second-round survey in five villages: specifically, following a national holiday the survey team miscalculated the revisit date for these villages and conducted the second survey on day fourteen rather than day fifteen. I propose to treat these five villages in the following manner:

- Given that this error could not have been foreseen by the respondents in these villages during the baseline interviews, the baseline data from these villages will be kept in the main sample for the analyses which involve baseline data only.
- For the analyses which involve data from the revisit, given that this a relatively minor breach of protocol (by one day), I will determine empirically whether these five villages should remain in the

main sample:

- I will conduct the analyses involving revisit data both with and without the five breached-protocol villages.
- If there is no significant difference, then for reasons of power I will keep the breached-protocol villages in the analyses involving revisit data.
- If the results are significantly different then I will drop these five villages from the analyses involving revisit data.

Additional villages: To preserve power in the eventuality that the five breached-protocol villages would have to be dropped from the analyses involving revisit data, five additional villages were brought into the sample (receiving both baseline and revisit interviews). The first of these is the final reserve village from the original sample, as detailed above. NRSP then identified four additional suitable villages within the survey area. These four additional villages were not included in the original sampling frame because they are not administered as separate "revenue villages" by NRSP, given that they are all in close proximity to one field office. Nonetheless, all four have the key features of the villages in the original sampling frame: all are large, self-contained villages which are geographically distant from one another, and contain at least ten NRSP borrowers per village. The individuals from these villages were sampled and assigned to individual-level participation fee treatment status in exactly the same way as individuals from the original sample (see section 4.2). However, given that these villages were added to the sample later in the experiment, the timing of their day one interviews was non-random. I therefore propose to use these four villages in the following way:

- I will include the individuals from these villages in estimations of the participation fee treatment effects and activity order effects.
- I will exclude these villages from estimations of the survey timing effects.

Attrition: Five respondents (two in village 7, one in village 40 one in village 52 and one in village 53) were absent at revisit, in each case because they were seeking medical attention or with a relative seeking medical attention. As a conservative measure, I will drop these individuals from baseline as well as revisit analyses, in case these individuals already anticipated at baseline that they might attrit at revisit.

Table 2 summarises the sample which I will use for each of the analyses.

4.2 Randomisation

4.2.1 Individual-level randomisations

The following procedures were used to select the participants to be interviewed in each village, and to assign participants to their treatment status within the participation fee timing treatment and the activity order treatment. Given that no baseline data was available at the time of randomisation, pure randomisation was used in each case and no blocking, checks of balance or re-randomisation were performed.

1. **Sampling of individuals:** The list of all active NRSP clients was obtained for all villages in the sampling frame. All clients were assigned a random number in Stata; then within each village, clients were sorted on this number to determine their order of priority for surveying. The first ten clients in the sorted list constituted the designated respondents, whereas the remainder constituted the reserve respondents and were to be used as replacements in order of their position in the sorted list. For example, if one designated respondent was not available then enumerators would seek out the eleventh respondent in the list; if the eleventh respondent could not be found, or if another designated respondent was not available, then they would seek out the twelfth respondent; and so on. This procedure was used to avoid cherry-picking of replacement respondents, for example if the survey team would have selected replacements out of clients who had a particularly good

Table 2: Sample villages and individuals

Village IDs	**Original sample**	**Status**	**# Villages**	**# Individuals**	**Day 14 or 15**	**Use in analysis**			
						Participation fee		**Village timing**	
						Day 1	**Day 15**	**Day 1**	**Day 15**
1-5, 11-29, 31-48	Y	Designated	42	419	15	Y	Y	Y	Y
50, 51	Y	Reserve	2	20	15	Y	Y	Y	Y
6, 7, 8, 10	Y	Designated	4	38	14	Y	tbc	Y	tbc
49	Y	Reserve	1	10	14	Y	tbc	Y	tbc
52-55	N	Additional	4	38	15	Y	Y	N	N
9, 30	Y	Dropped (male clients)	2	20	N/A	N	N	N	N
					Total # villages	**53**	**53** (48 if day 14 dropped)	**49**	**49** (44 if day 14 dropped)
					Total # individ-uals	**525**	**525** (478 if day 14 dropped)	**487**	**487** (439 if day 14 dropped)

relationship with NRSP, or who appeared particularly in need of the participation fee. This is an extension of the idea of a "waitlist design" (Hirshleifer et al., 2015).

2. **Participation fee timing:** Given the list sorted by random number, designated respondents 1, 3, 5, 7, and 9 were assigned to be paid their participation fee on day one whereas designated respondents 2, 4, 6, 8 and 10 were assigned to be paid their participation fee on day fifteen. To preserve equal numbers of treatment types within a village, if a reserve respondent was used then they inherited the treatment status of the designated respondent that they were replacing: for example, if respondent 13 was used to replace respondent 2, then respondent 13 was assigned to be paid on day fifteen. Note that there was no scope for differential refusal to participate on the basis of treatment status, since participants only discovered their treatment status (i.e. the timing of their participation fee) halfway through the baseline interview after participation consent was obtained.

3. **Activity order:** Within each treatment status — "pay on day one" or "pay on day fifteen" — all respondents were assigned a second random number between zero and one in Stata. This was done within treatment status in order to generate equal numbers of each activity order within each treatment status. The four possible activity orders — time activities and near frame first; time activities and far frame first; control activities and near frame first; control activities and far frame first — were assigned to those participants whose second random number fell within the bins $x \leq 0.25$; $0.25 < x \leq 0.5$; $0.5 < x \leq 0.75$; and $0.75 < x \leq 1$ respectively. Thus approximately 25% of the sample (and 25% of individuals within both participation fee treatment arms) was assigned to each of the combinations of activity order and frame order.

4.2.2 Village-level randomisations

The following procedure was used to determine which villages would be in the main sample and which would act as reserve and pilot villages, and to assign villages in the main sample to survey dates.

1. **Sampling of villages:** All villages in the sampling frame were assigned a random number in Stata and sorted on this number. Given the sorted list, the first 1-48 villages constituted the designated survey villages (48 villages was the desired sample size for budgetary reasons), villages 49-51 constituted the reserve villages, and villages 52-54 constituted the pilot villages. The three pilot villages were interviewed prior to the start of the main survey period. The reserve villages were to be used as follows: if one of the designated villages needed to be dropped because of unforeseen circumstances, then it was to be replaced by village 49; if a second was dropped then it was to be replaced by village 50, and if a third was dropped then it was to be replaced by village 51.

2. **Survey timing:** Within the designated survey villages, the village number also constituted the order in which the village was to be surveyed: for example, village 1 was to have its baseline interviews on day 1 of the main survey period, village 2 was to have its baseline interviews on day 2 of the main survey period, and so on. In cases where a reserve village was used, it inherited the survey order of the village that it was replacing. For example, village 49 was eventually used to replace village 9, and thus village 49 received baseline interviews on day 9 of the main survey period.

   In advance of the survey, local NRSP staff predicted that the modal date for farmers to start harvesting wheat would be 25th April. The timing of the survey period therefore meant that villages 1-24 received their baseline interviews before this approximate harvest start date and villages 25-48 received their baseline interviews after this approximate harvest start date.

5 Data and balance

5.1 Data

Appendix A.1 describes the variables which will be constructed for each individual. It also makes clear whether each set of variables will be used as treatment variables, outcome variables, variables for heterogeneity analysis, controls or robustness checks, or variables for exploratory descriptive analysis.

I will winsorize at the 95th percentile the few continuous variables where no lower or upper bound was imposed, specifically: `age`, `savings`, `hh_annual_income`, `Strooptime`, and all income and expenditure expectations and realisations from the income and expenditure survey.

5.2 Balance

Randomised assignment to treatment status within each of the treatment arms was pre-determined by computer for the designated respondents. However, in theory (although very unlikely) there might have been: differential use of replacement respondents by enumerators depending on the participation fee status of the individual being replaced; violations of the bonus draw protocol depending on respondent characteristics; or differential refusal to participate depending on the timing of a village's surveys in relation to the harvest. I will therefore report separate tests of balance comparing:

1. Those who received the participation fee on day one vs. day fifteen.
2. Those who won the bonus vs. those who did not.
3. Those who received the time-preference activities vs. the control activities first.

4. Those who received the near frame vs. the far frame first.

5. Those whose village received its baseline surveys prior to the harvest vs. after the harvest had begun.

Balance will be presented on the following characteristics (see table 5 for their construction):

- `hh_annual_income`
- `savings`
- `couldborrow_formal`
- `couldborrow_informal`
- `bank_account`
- `harvest_wheat`
- `muslim`
- `education`
- `housewife`
- `trust_NRSP`
- `trust_self`
- `age`
- `married`
- `hh_size`
- `hh_head`
- `hh_decisionmaking`

Within each of the five balance tests, I will regress the treatment indicator on this set of covariates and will report a joint F-test that the coefficients are not jointly different from zero (McKenzie, 2015).

6 Regression analysis

6.1 Participation fee treatments

To estimate the treatment effect of the participation fee timing on different dependent variables, I will estimate equations of the following form separately for each interview day $\in \{1, 15\}$:

$$y_{ivdt} = \beta_1 participationfee\_day15_{iv} + \delta \boldsymbol{X_{iv}} + \eta_{vdt} + \epsilon_{ivdt}, \tag{7}$$

using the Stata code:

```
ivreg2 y participationfee_day15 control_* i.villageid,
                                partial(control_* i.villageid) robust.
```
(8)

In each case, y_{ivtd} is the outcome variable of interest for individual i in village v on interview day $d \in \{1, 15\}$ at calendar date t; $participationfee\_day15$ is an indicator equal to one if individual i was assigned to being paid the participation fee on day 15 (rather than day 1); $\boldsymbol{X_{iv}}$ is a vector of time-invariant individual controls from the baseline survey, η_{vdt} is a village fixed effect; and ϵ_{ivtd} is an error term robust to individual heteroskedasticity. Given that I will estimate equation 7 separately for day 1 and day 15, and that all respondents in a village were surveyed on the same date for their day 1 and day 15 interviews, village fixed effects also correspond to village-day-date fixed effects. $\hat{\beta}_1$ represents the average treatment effect, since assignment to treatment and actual treatment status coincided by design.[9]

[9] Treatment-arm-specific instructions displayed on the enumerator's survey tablet, as well as treatment-arm-specific payment and voucher procedures which had to be followed and verified by the enumerator before she could proceed.

Controls: I will include the following baseline controls in the vector $\boldsymbol{X_{iv}}$, based on their likely predictive power for time-preference behaviour (especially in proximity to the wheat harvest and Ramadan): `hh_annual_income`, `savings`, `couldborrow_formal`, `couldborrow_informal`, `harvest_wheat`, `muslim`, `education`, `housewife`. I will also include any of the variables tested for balance which are not included in the above list but are found to be unbalanced across treatment arms.

I will estimate equation 7 both with and without these controls. When including controls, for efficiency purposes I will partial them out as specified in equation 8 and the Stata code for the other estimating equations listed below.

Dependent variables: I will estimate equation 7 for each of the combinations of survey day and outcome variables listed in table 3. Table 3 translates the predictions from table 1 into predictions on the sign of $\hat{\beta}_1$. Given that in each case the treatment variable indicates being paid the participation fee on day 15 ($t = 1$) *rather than day 1* ($t = 0$), the predictions are generated by comparing table 1 row (1) to table 1 row (2). As table 1 sets out, for each primary outcome of interest — static reversals, dynamic reversals and time-variance — I will first present an analysis where the dependent variable is the monetary amount of the inconsistency across the two time frames concerned; then for ease of comparison with other studies of time preference I will present analyses where the dependent variables are dummies for exhibiting inconsistency in a "present-biased" or a "future-biased" direction respectively. For each dependent variable, I will report the p-value of the test of the null hypothesis that $\beta_1 = 0$. It is not necessary to correct for multiple hypothesis testing across the dependent variables, given that the model generates a separate hypothesis for each dependent variable (rather than the separate dependent variables acting as proxies of one underlying hypothesis test). However, to give a sense of how likely it is that my proposed model is driving the results, I plan to report how many of the nulls are rejected in the direction predicted by the model, how many fail to reject, and how many are rejected in the opposite direction to that predicted by the model.

Table 3: Predicted coefficient signs: $participationfee\_day15$

Day	**Outcome variable y_{ivtd}**	**Value in theoretical framework**	**Prediction on $\hat{\beta}_1$**
1	near_switch_MPL_1	$x_{1.0}$	$\hat{\beta}_1 > 0$
1	far_switch_MPL_1	$x_{2.0}$	$\hat{\beta}_1 < 0$
1	static_diff_MPL_1	$x_{1.0} - x_{2.0}$	$\hat{\beta}_1 > 0$
1	pb_static_MPL_1	Dummy: $x_{1.0} > x_{2.0}$	$\hat{\beta}_1 > 0$
1	fb_static_MPL_1	Dummy: $x_{1.0} < x_{2.0}$	$\hat{\beta}_1 < 0$
15	near_switch_MPL_15	$x_{2.1}$	$\hat{\beta}_1 < 0$
15	static_diff_MPL_15	$x_{2.1} - x_{3.1}$	$\hat{\beta}_1 < 0$
15	pb_static_MPL_15	Dummy: $x_{2.1} > x_{3.1}$	$\hat{\beta}_1 < 0$
15	fb_static_MPL_15	Dummy: $x_{2.1} < x_{3.1}$	$\hat{\beta}_1 > 0$
15	timevariance_near_MPL	$x_{1.0} - x_{2.1}$	$\hat{\beta}_1 > 0$
15	risingpatience_near_MPL	Dummy: $x_{1.0} > x_{2.1}$	$\hat{\beta}_1 > 0$
15	fallingpatience_near_MPL	Dummy: $x_{1.0} < x_{2.1}$	$\hat{\beta}_1 < 0$
15	timevariance_far_MPL	$x_{2.0} - x_{3.1}$	$\hat{\beta}_1 < 0$
15	risingpatience_far_MPL	Dummy: $x_{2.0} > x_{3.1}$	$\hat{\beta}_1 < 0$
15	fallingpatience_far_MPL	Dummy: $x_{2.0} < x_{3.1}$	$\hat{\beta}_1 > 0$

6.2 Heterogeneity on external liquidity constraints

According to the theoretical framework, treatment effects from the participation fee timing should be larger for those for whom the marginal utility of consumption from 1000 PKR is higher, and (according to the extended version of the framework which makes explicit the role of saving and borrowing) for those who are unable to borrow or to arbitrage own savings against experimental income. I will therefore re-estimate equation 7 for each the outcome variables listed in table 3 four times, each time including the main treatment variable `participationfee_day15` as before but also interacting it with one of the following variables:

- `hh_annual_income`[10]
- `savings`
- `couldborrow`
- `bank_account`

For each dependent variable, the four separate estimations interacting the treatment variable with a different proxy of liquidity constraints constitute separate tests of the same null hypothesis that liquidity constraints have no effect on the treatment effect for that dependent variable. Therefore within each dependent variable, for each of the coefficients on the interaction terms I will report two values: i) the uncorrected p-value; and ii) the False Discovery Rate q-value, taken across the interaction terms for all four proxies of liquidity constraints for that dependent variable (Anderson, 2012; Benjamini et al., 2006). Moreover, to give an overall picture of whether the predictions of the model hold across dependent variables, I also plan to report how many times the null is rejected in favour of liquidity constraints increasing the treatment effect, how many times the null fails to reject, and how many times it is rejected in favour of liquidity

[10] Household income can be taken as a proxy both for the marginal utility of income and for liquidity constraints, insofar as it acts as an additional proxy for savings and borrowing opportunities.

constraints actually decreasing the treatment effect.

6.3 Bonus participation fee

To examine the effects of winning the bonus, I will run an extension to equation 7 of the following form for day$= 15$.[11]:

$$y_{iv15t} = \beta_1 participationfee\_day15_{iv} + \beta_2 won\_bonus_{ivdt} + \beta_3 participationfee\_day15_i * won\_bonus_{ivdt} + \delta \boldsymbol{X_{iv}} + \eta_v + \epsilon_{ivdt}, \quad (9)$$

using the Stata code:

```
ivreg2 y participationfee_day15 won_bonus
       participationfee_day15*won_bonus control_* i.villageid,
                          partial(control_* i.villageid) robust.
```
(10)

$\hat{\beta}_2$ estimates the impact on responses at revisit of having won the 1000 PKR bonus at the end of the baseline session (when the draw was conducted for participants who had been paid the basic participation fee at baseline, i.e. who have $participationfee\_day15 = 0$). In the absence of saving, the model predicts that $\hat{\beta}_2 = 0$ since this extra income shock at baseline cannot effect responses at revisit.

$\hat{\beta}_3$ denotes the impact on responses at revisit of having just won the bonus as well as the basic participation fee. Given the theoretical predictions in table 1, table 4 describes the predicted sign of β_3 for each of the outcome variables of interest. As can be seen, in general $\hat{\beta}_3$ is predicted to have the same sign as $\hat{\beta}_1$ since winning the bonus essentially just doubles the dose of the participation fee treatment. However,

[11] Winning the bonus cannot affect responses in the day 1 activities, since no participants had done the bonus draw before responding to the day 1 activities.

the one divergence is that given that the outcome of the bonus is only known before the day 15 activities but not before the day 1 activities, the bonus is also predicted to lead to revisions in individuals' choices: specifically, a decrease in "present-biased" revisions and an increase in "future-biased" revisions.

Table 4: Predicted coefficient signs: $participationfee\_day1 * won\_bonus$

Day	**Outcome variable y_{ivtd}**	**Value in theoretical framework**	**Prediction on $\hat{\beta}_3$**
15	near_switch_MPL_15	$x_{2.1}$	$\hat{\beta}_1 < 0$
15	static_diff_MPL_15	$x_{2.1} - x_{3.1}$	$\hat{\beta}_1 < 0$
15	pb_static_MPL_15	Dummy: $x_{2.1} > x_{3.1}$	$\hat{\beta}_1 < 0$
15	fb_static_MPL_15	Dummy: $x_{2.1} < x_{3.1}$	$\hat{\beta}_1 > 0$
15	**dynamic_diff_MPL**	$\boldsymbol{x_{2.1} - x_{2.0}}$	$\boldsymbol{\hat{\beta}_1 < 0}$
15	**pb_revision_MPL**	Dummy: $\boldsymbol{x_{2.1} > x_{2.0}}$	$\boldsymbol{\hat{\beta}_1 < 0}$
15	**fb_revision_MPL**	Dummy: $\boldsymbol{x_{2.1} < x_{2.0}}$	$\boldsymbol{\hat{\beta}_1 > 0}$
15	timevariance_near_MPL	$x_{1.0} - x_{2.1}$	$\hat{\beta}_1 > 0$
15	risingpatience_near_MPL	Dummy: $x_{1.0} > x_{2.1}$	$\hat{\beta}_1 > 0$
15	fallingpatience_near_MPL	Dummy: $x_{1.0} < x_{2.1}$	$\hat{\beta}_1 < 0$
15	timevariance_far_MPL	$x_{2.0} - x_{3.1}$	$\hat{\beta}_1 < 0$
15	risingpatience_far_MPL	Dummy: $x_{2.0} > x_{3.1}$	$\hat{\beta}_1 < 0$
15	fallingpatience_far_MPL	Dummy: $x_{2.0} < x_{3.1}$	$\hat{\beta}_1 > 0$

I will estimate equation 9 for each of the day 15 dependent variable as listed in table 4. In each case, I will report the p-values of the tests of the null hypotheses that $\beta_2 = 0$ and $\beta_3 = 0$. As with the estimation of the main treatment effects in equation 7, it is not necessary to correct for multiple hypothesis testing across the different dependent variables since the model generates a separate hypothesis for each dependent variable. However, again I will report how many times the nulls are rejected in line with the predictions of the model, how many times the nulls fail to reject, and how many times they are rejected in the opposite direction to that predicted by the model.

6.4 Activity order treatments

To estimate whether the activity order influences the main treatment effects, I will estimate equations of the form:

$$\begin{aligned} y_{ivdt} = \beta_1 participationfee\_day15_{iv} + \beta_4 salience_{iv} + \beta_5 nearframe\_first_{iv} \\ + \beta_6 salience_{iv} * nearframe\_first_{iv} + \beta_7 participationfee\_day15_{iv} * salience_{iv} \\ + \beta_8 participationfee\_day15_{iv} * nearframe\_first_{iv} \\ + \beta_9 participationfee\_day15_{iv} * salience_{iv} * nearframe\_first_{iv} + \delta \boldsymbol{X_{iv}} + \eta_{vdt} + \epsilon_{ivdt}, \end{aligned} \quad (11)$$

using the Stata code:

```
ivreg2 y participationfee_day15 salience nearframe_first
      salience*nearframe_first participationfee_day15*salience
            participationfee_day15*nearframe_first
          participationfee_day15*salience*nearframe_first
     control_* i.villageid, partial(control_* i.villageid) robust.
```
(12)

I will estimate equation 11 for each of the outcome variables and dates listed in table 3. In each case, I will test the separate null hypotheses that:

- $\beta_4 = 0$, i.e. the activity order has no effect directly on the outcome variable of interest
- $\beta_5 = 0$, i.e. the frame order has no effect directly on the outcome variable of interest
- $\beta_6 = 0$, i.e. the the interaction of these two treatments has no effect directly on the outcome variable of interest

- $\beta_7 = 0$, i.e. the activity order has no effect directly on the treatment effect
- $\beta_8 = 0$, i.e. the frame order has no effect on the treatment effect
- $\beta_9 = 0$ i.e. the the interaction of these two treatments has no effect on the treatment effect

Each estimation with a different dependent variable ultimately constitutes a separate test of the same set of null hypotheses above: that activity order, frame order and their interaction do not affect responses to time preference tasks nor the treatment effect. Therefore for each hypothesis test above, I will report two values: i) the uncorrected p-value from a Wald test; and ii) the False Discovery Rate q-value, taken for that coefficient across the estimations with each of the dependent variables listed in table 3.

6.5 Survey order treatment

To test whether individuals interviewed before the harvest exhibit more "present-biased" static reversals, fewer "future-biased" reversals, and stronger treatment effects — and whether this is driven by those individuals who are expecting to harvest wheat — I will estimate the following equation for each of the dependent variables listed in table 3:

$$\begin{aligned} y_{ivdt} = \beta_1 participationfee\_day15_{iv} + \beta_{10} pre\_harvest_{iv} + \beta_{11} pre\_harvest_{iv} * harvest\_wheat_{iv} \\ + \beta_{12} participationfee\_day15_{iv} * pre\_harvest_{iv} \\ + \beta_{13} participationfee\_day15_{iv} * pre\_harvest_{iv} * harvest\_wheat_{iv} \\ + \delta \boldsymbol{X_{iv}} + \eta_v + \epsilon_{ivdt}, \quad (13) \end{aligned}$$

using the Stata code:

```
ivreg2 y participationfee_day15 pre_harvest pre_harvest*harvest_wheat
                participationfee_day15*pre_harvest
          participationfee_day15*pre_harvest*harvest_wheat
control_* i.villageid, partial(control_* i.villageid) cluster(villageid).
```

(14)

Standard errors will be clustered at the village level, since in this case the unit of randomisation is the village. In each case I will report the p-values of the tests of the null hypotheses that $\beta_{10} = 0$, $\beta_{11} = 0$, $\beta_{12} = 0$, and $\beta_{13} = 0$. Since the model generates separate hypothesis for each dependent variable, it is not necessary to correct for multiple hypothesis testing here.

7 Robustness

7.1 Alternative explanations

Trust: To check whether trust that future meetings and payments will take place affects the dependent variables and/or the treatment effects, I will also re-estimate equation 7 with each of the dependent variables listed in table 3 twice. In each estimation I will control for one of the following variables, and its interaction with the treatment variable `participationfee_day15`:

- Median `trust_NRSP`

- Median `trust_self`

For each dependent variable, the coefficents on each of these two controls (interaction terms) constitute separate tests of the same null hypothesis that trust that future meetings and payments will take place has no direct effect (effect on the treatment effect) for that dependent variable. Therefore for each dependent variable, for each proxy of trust I will report two values for the tests of the null hypothesis that the coefficient on that proxy of trust (its interaction term with the treatment) is zero: i) the uncorrected p-value; and ii) the False Discovery Rate q-value, taken across both proxies of trust for that outcome variable.

Risk preferences: The participation fee treatment could theoretically affect choices in the time preference activities via changing risk preferences, in addition to or instead of via easing liquidity constraints. This could occur if receiving the participation fee changes the curvature of the individual's within-period utility function, and thereby increases the individual's valuation of the deferred payment if that payment is seen as risky, or indeed changes the valuation of uncertain future background income. This would imply that participants take background income into account during risk-preference activities in addition to or instead of during time-preference activities. To explore this possibility, I will re-estimate equation 7 for the dependent variables in table 3 adding the extra control variable `certainty_premium_1` in the day 1 regressions and `certainty_premium_15` in the day 15 regressions.

Optimism: Similarly, it could be that winning the bonus makes participants feel more optimistic that uncertain events will go in their favour. This may change answers to time preference activities, in either a more or less patient direction depending on when and whether background income is predicted to rise or fall. To explore this, I will re-estimate equation 9 for the outcome variables listed in table 3 adding the extra control variable `optimism_self_15`.

Cognitive effects: Finally, it could be the case that receiving the participation fee or the promise of the future participation fee eases individuals' stress about financial resources. This may lead them to focus more or less on the cognitive processing required in the time-preference activities (Mani et al., 2013). To explore this, I will re-estimate equation 7 for the dependent variables in table 3 adding the extra controls variables `digitspan_1`, `maths_1` and `Strooptime_1` in the day 1 regressions and `digitspan_15`, `maths_15` and `Strooptime_15` in the day 15 regressions.

7.2 Alternative time preference measure

To examine whether the main treatment effects manifest themselves with an alternative measure of time preference, I will re-estimate equation 7 for the following outcome variables as measured via the present-equivalent task as opposed to the multiple price list measurement:[12]

- `pb_static_PE_1`
- `pb_static_PE_15`
- `fb_static_PE_1`
- `fb_static_PE_15`
- `pb_revision_PE_1`
- `fb_revision_PE_1`
- `risingpatience_near_PE`

[12] I focus on the outcome variables which are dummy variables, rather than the outcome variables which represent the monetary amount of inconsistency in responses across two time frames, since the latter is not directly comparable across the two time preference measures.

- `risingpatience_far_PE`
- `fallingpatience_near_PE`
- `fallingpatience_far_PE`

In each case I will report the p-value of the test of the null hypothesis that $\beta_1 = 0$. As in the original estimations of equation 7, it is not necessary to correct for multiple hypothesis testing across dependent variables since the model generates a separate prediction for each dependent variable. The predicted sign of $\hat{\beta}_1$ here in each case is the same as in table 3.

7.3 Alternative specifications

I will run the following alternative specifications of equation 7 to test for the sensitivity of my results to treatment of the data. In each case, I will report a cross-equation test that the treatment effect in the alternative specification listed below is equal to that in the original specification estimated in section 6.1, i.e. that $\beta_{1alternative} = \beta_1$. I will implement this test using a standard Seemingly Unrelated Estimation.

- To exclude any possible effects of making responses unincentivized, I will re-estimate equation 7 for each of the day 15 dependent variables listed in table 1 but this time will drop any individuals who had already randomly drawn to be paid for their answers to the activities on day 1.
- To allow for "fat" indifference curves, I will will re-set the dummies for
  - `pb_static_MPL_1`
  - `fb_static_MPL_1`
  - `pb_static_MPL_15`

  - `fb_static_MPL_15`
  - `risingpatience_near_MPL`
  - `fallingpatience_near_MPL`
  - `risingpatience_far_MPL`
  - `fallingpatience_far_MPL`

  equal to one only if the individual's responses differ by more than one question across the two time frames concerned. I will then re-run the estimations of equation 7 with these more conservative dummy variables as the dependent variables.

- To avoid possible issues of truncation, I will re-estimate equation 7 for each of the dependent variables listed in table 1 but this time will drop any individuals who never switch to the later payment in one or both of the frames concerned. This will also serve the purpose of excluding individuals who may have refused to take deferred payments because of lack of trust.

- Finally, to exclude individuals who may have misunderstood the task, I will re-estimate equation 7 for each of the dependent variables listed in table 1 but this time will drop all individuals who exhibit multiple switching (i.e. who at some point choose the later option but then go back to choosing the sooner option again) in one or both of the frames concerned.

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A Appendices

A.1 Variables

Table 5: **Variables to be constructed**

VARIABLE	DEFINITION	QUESTION
	TREATMENT VARIABLES	
`date`	Calendar date of interview	starttime
`day1orday15`	Interview day relative to baseline: $= 1$ for baseline; $= 15$ for revisit; $= 14$ for breached-protocol revisits	day1orday15
`participationfee_day15`	Dummy: $= 1$ if participation fee to be paid on day 15 (rather than day 1).	AorB

won_bonus	Dummy: $= 1$ if won participation fee bonus	bonus_outcome
salience	Dummy: $= 1$ if assigned to an activity order $\in \{1, 2, 5, 6\}$, with time preference activities before control activities	activity_order
nearframe_first	Dummy: $= 1$ if assigned to an activity orders $\in \{1, 3, 5, 7\}$ with near frame before far frame	activity_order
village_order	Actual village survey order: $\in [1, 48]$ for designated and replacement villages; $\in [146, 150]$ for additional villages, which will not be used in analysis of survey order effects	villageid
pre_harvest	Dummy: $= 1$ if baseline interview is before 15th April	villageid, date
post_harvest	Dummy: $= 1$ if baseline interview is on or after 15th April	villageid, date
TIME PREFERENCES (OUTCOMES)		
MULTIPLE PRICE LISTS (primary)		
near_switch_MPL_1 near_switch_MPL_15	Value at which respondent switched from choosing 400 PKR today to choosing that value in two weeks; day 1 and day 15 interviews respectively	tp_n_1 - tp_n_16
far_switch_MPL_1 far_switch_MPL_15	Value at which respondent switched from choosing 400 PKR in two weeks to choosing that value in four weeks; day 1 & day 15 interviews respectively	tp_f_1 - tp_f_16
static_diff_MPL_1 static_diff_MPL_15	near_switch − far_switch for days 1 & 15 respectively; positive values indicate "present-bias" and vice versa	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
pb_static_MPL_1, pb_static_MPL_15	Dummy: $= 1$ if near_switch > far_switch; day 1 & day 15 respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
fb_static_MPL_1, fb_static_MPL_15	Dummy: $= 1$ if near_switch < far_switch; day 1 & day 15 respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16

tc_static_MPL_1, fb_static_MPL_15	Dummy: = 1 if near_switch = far_switch; day 1 & day 15 respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
dynamic_diff_MPL	near_switch_MPL_15 − far_switch_MPL_1; positive values indicate "present-biased" revisions and vice versa	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
pb_revision_MPL	Dummy: = 1 if near_switch_MPL_15 > far_switch_MPL_1	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
fb_revision_MPL	Dummy: = 1 if near_switch_MPL_15 < far_switch_MPL_1	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
norevision_MPL	Dummy: = 1 if near_switch_MPL_15 = far_switch_MPL_1	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
timevariance_near_MPL	near_switch_MPL_1 − near_switch_MPL_15; positive values indicate that individual is more patient in the near frame at revisit than at baseline	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
timevariance_far_MPL	far_switch_MPL_1 − far_switch_MPL_15; positive values indicate that individual is more patient in the far frame at revisit than at baseline	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
risingpatience_near_MPL, risingpatience_far_MPL	Dummy: = 1 if near_switch_MPL_1 > near_switch_MPL_15, far_switch_MPL_1 > far_switch_MPL_15 respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
fallingpatience_near_MPL, fallingpatience_far_MPL	Dummy: = 1 if near_switch_MPL_1 < near_switch_MPL_15 or far_switch_MPL_1 < far_switch_MPL_15 respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
constantpatience_near_MPL, constantpatience_far_MPL	Dummy: = 1 if near_switch_MPL_15 = near_switch_MPL_1 or far_switch_MPL_15 = far_switch_MPL_1 respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16

multipleswitching_near_1, multipleswitching_far_1, multipleswitching_near_1, multipleswitching_far_15	Dummy: = 1 if participant switches from sooner amount to later amount but then switches back again; during day 1 near frame, day 1 far frame, day 15 near frame, or day 15 far frame respectively	tp_n_1 - tp_n_16, tp_f_1 - tp_f_16
factors_near_MPL_1, factors_far_MPL_15, factors_near_MPL_15, factors_far_MPL_15	Descriptive categories: what the respondent took into account when making her MPL choice (self-reported)	tp_n_17, tp_n_17s, tp_f_17, tp_f_17s
PRESENT EQUIVALENTS (robustness)		
pb_static_PE_1, pb_static_PE_15	The fraction of present-equivalent choice sets in which the switch-point in the near frame was greater than that in the far frame; day 1 & day 15	pe_n_11-pe_n_510, pe_f_11-pe_f_510
fb_static_PE_1, fb_static_PE_15	The fraction of present-equivalent choice sets in which the switch-point in the near frame was smaller than that in the far frame; day 1 & day 15	pe_n_11-pe_n_510, pe_f_11-pe_f_510
tc_static_PE_1, tc_static_PE_15	The fraction of present-equivalent choice sets in the switch-point in the near frame was equal to that in the far frame; day 1 & day 15	pe_n_11-pe_n_510, pe_f_11-pe_f_510
pb_revision_PE	The fraction of present-equivalent choice sets in which the switch point in the near frame at revisit was greater than that in the far frame at baseline	pe_n_11-pe_n_510, pe_f_11-pe_f_510
fb_revision_PE	The fraction of present-equivalent choice sets in which the switch point in the near frame at revisit was smaller than that in the far frame at baseline	pe_n_11-pe_n_510, pe_f_11-pe_f_510

norevision_PE	The fraction of present-equivalent choice sets in which the switch point in the near frame at revisit was the same as that in the far frame at baseline	pe_n_11-pe_n_510, pe_f_11-pe_f_510
risingpatience_near_PE, risingpatience_far_PE	The fraction of present-equivalent choice sets in which the switch-point in the near frame at baseline was larger than that in the near frame at revisit	pe_n_11-pe_n_510, pe_f_11-pe_f_510
fallingpatience_near_PE, fallingpatience_far_PE	The fraction of present-equivalent choice sets in which the switch-point in the near frame at baseline was smaller than that in the near frame at revisit	pe_n_11-pe_n_510, pe_f_11-pe_f_510
constantpatience_near_PE, fallingpatience_far_PE	The fraction of present-equivalent choice sets in which the switch-point in the near frame at baseline was equal to that in the near frame at revisit	pe_n_11-pe_n_510, pe_f_11-pe_f_510
multipleswitching_near_PE_1, multipleswitching_near_PE_15, multipleswitching_far_PE_1, multipleswitching_far_PE_15	The fraction of present-equivalent choice sets in which multiple switching (from option A to option B then back to option A) was exhibited	pe_n_11-pe_n_510, pe_f_11-pe_f_510
LIQUIDITY (HETEROGENEITY & CONTROLS)		
couldborrow_formal	Dummy: $= 1$ if could borrow from NRSP or another formal organisation in the next 2 months	loansource1_2, loansource1_3
couldborrow_informal	Dummy: $= 1$ if could borrow from family and friends or neighbours in the next 2 months	loansource1_1, loansource1_4
couldborrow	Dummy: $= 1$ if couldborrow_formal $= 1$ and/or couldborrow_informal $= 1$	loansource1_1, loansource1_2, loansource1_3, loansource1_4

savings	Current total household savings (summed across types of savings reported)	f_2
hh_annual_income	Household's approximate annual income	_1yearlyincome
bank_account	Dummy: $= 1$ if household has a bank account	f_44
harvest_wheat	Dummy: $= 1$ if household will harvest wheat this year	ic_20_check
TRUST (ROBUSTNESS)		
trust_NRSP	"If a female representative of NRSP made an appointment to see me about a different study, they would be unlikely to cancel or change that appointment"; Likert scale 1-5, 5 is strongly agree	oa_22
trust_self	"If I made an appointment to see someone, for example a female representative of NRSP involved in a different study, I would be unlikely to cancel or change that appointment."; Likert scale 1-5, 5 is strongly agree	oa_23
RISK PREFERENCES (ROBUSTNESS)		
certainty_premium_1, certainty_premium_15	Aggregate certainty premium in the certainty equivalent tasks: for each lottery, the expected value of the lottery minus the switchpoint at which the individual preferred the certain amount to the lottery; summed across the five lotteries presented. Day 1 & day 15 respectively.	ce_g_11-ce_g_59
OPTIMISM (ROBUSTNESS)		
expectations_heads_1, expectations_heads_15	Expected outcome of third coin toss after two heads: heads, tails or equal chance	e_ct_1
expectations_tails_1, expectations_tails_15	Expected outcome of third coin toss after two tails: heads, tails or equal chance	e_ct_2
optimism_other_1, optimism_other_15	Participant's belief as to the probability an average other individual would win a given draw, minus the actual given probability; summed over all 5 draws presented	e_bb_1a-e_bb_5a

optimism_self_1, optimism_self_15,	Participant's belief as to the probability she would win a given draw, minus the actual given probability; summed over all 5 draws presented	e_bb_1b-e_bb_5b
optimism_self-other_1, optimism_self-other_15,	Participant's belief as to the probability she would win a given draw, minus her belief that an average other individual would win the given draw; summed over all 5 draws presented	e_bb_1a-e_bb_5a, e_bb_1b-e_bb_5b
survey_lucky	"When something is a matter of luck it tends to go my way"; Likert scale 1-5, 5 is strongly agree	oa_13
COGNITIVE ABILITY (ROBUSTNESS)		
digitspan_1, digitspan_15	Score in digit span test, 0-7	m_1-m_7
maths_1, maths_15	Score in maths test, 0-12. Sum of scores in each of the three maths questions, in which: score 4 if answer option 1 (correct, quickly); 3 if answer option 2 (correct, slowly); 2 if answer option 3 or 4 (correct, with help); 1 if answer option 5 (incorrect but tried); 0 if answer option 6 (did not even try).	m_8-m_10
Strooptime_1, Strooptime_15	Time taken to complete all 25 questions of Stroop test	Strooptimetaken
education	Years of education	intro_11, intro_11_m
ACTIVITY PAYMENTS (ROBUSTNESS)		
activitypayment_day1	Dummy: $= 1$ if participant paid for answers to activities on day 1 (result of random draw)	activity_1or2to10
HOUSEHOLD (CONTROLS & DESCRIPTIVE)		
muslim	Dummy: $= 1$ if household is Muslim	ic_23_check
age	Age	intro_9
married	Dummy: $= 1$ if currently married	hh_1

hh_size	Number of household members	peopleinhousehold
hh_head	Dummy: $= 1$ if household head	relationshipto - headhousehold
hh_decisionmaking	Index between 0 and 1 : sum of 8 dummies each $= 1$ if respondent has some or all say in a given household decision, divided by 8	decisionsa-decisiong, finalDecision
family_demand	"Whenever I have money, family members ask for or take some of it". Likert scale 1-5, 5 is strongly agree	oa_3
spouse_hardtosave	"My spouse finds it hard to save for the future"; Likert scale 1-5, 5 is strongly agree	oa_4
spouse_unnecessarypurch	"My spouse spends money on unnecessary purchases"; Frequency scale 1-5, 5 is very often	oa_5
NON-EXPERIMENTAL INCOME (DESCRIPTIVE)		
housewife	Dummy: $= 1$ if respondent lists "housewife" as main occupation (as opposed to agriculture, business, etc.)	intro_10_9
betteroff4weeks	"Thinking about my income and expenditures, I expect to be better off financially in four weeks than I am today"; Likert scale 1-5, 5 is strongly agree	oa_14
return_400_2weeks	Profit after 2 weeks if given 400 PKR today and invested it in current opportunities	i_1
return_400_4weeks	Profit after 4 weeks if given 400 PKR today and invested it in current opportunities	i_2
mid_harvest	Midpoint of household's harvest income expectations	ic_mid_H
mid_Ramadan_Y	Midpoint of household's Ramadan income expectations	ic_mid_RI
mid_Ramadan_C	Midpoint of household's Ramadan expenditure expectations	ic_mid_RE
y0-e0[y1]; c0-e0[c1]	Income in last two weeks minus expected income in next two weeks, measured at baseline; Analogue for expenditure	ic_4, ic_6; ic_13, ic_15

`e0[y1]-e0[y2]; e0[c1]-e0[c2]`	Expected income in next two weeks minus expected income in the two weeks after that, measured at baseline; Analogue for expenditure	ic_6, ic_7; ic_15, ic_16
`y1-e1[y2]; c1-e1[c2]`	Income in last two weeks minus expected income in next two weeks, measured at revisit; Analogue for expenditure	ic_4, ic_6; ic_13, ic_15
`e1[y2]-e1[y3]; e0[c1]-e0[c2]`	Expected income in next two weeks minus expected income in the two weeks after that, measured at revisit; Analogue for expenditure	ic_6, ic_7; ic_15, ic_16
`y1-e0[y1]; e0[c1]-e0[c2]`	Income in last two weeks measured at revisit, minus expected income in next two weeks (i.e. same period) measured at baseline; Analogue for expenditure	ic_4, ic_6; ic_13, ic_15
`e1[y2]-e0[y2]; e0[c1]-e0[c2]`	Expected income in next two weeks measured at revisit, minus expected income in weeks three to four (i.e. same period) measured at baseline	ic_6, ic_7; ic_15, ic_16
	COMMITMENT SAVINGS (DESCRIPTIVE)	
`committee`	Dummy: $=1$ if currently member of a committee (ROSCA)	f_7
`hardtosave`	"I find it hard to save money for the future"; Likert scale 1-5, 5 is strongly agree	oa_1
`unnecessary_purchases`	"I spend money on unnecessary purchases"; frequency scale 1-5, 5 is very often	oa_2
`money_keeptrack`	"I am good at keeping track of my money"; Likert scale 1-5, 5 is strongly agree	oa_8
`uses_commitment`	Dummy: $=1$ if uses one or more types of commitment	oa_6
`fails_commitment`	Dummy: $=1$ if tries to use commitment but fails	oa_6
`withdrawal_4weeks`	Dummy: $=1$ if would take up an account with withdrawal restrictions for the next four weeks	oa_16

withdrawal_flexi	Dummy: $= 1$ if would take up an account with withdrawal restrictions until chosen date	oa_17
deposit_4weeks	Dummy: $= 1$ if would take up an account with deposit obligations for the next four weeks	oa_18
deposit_flexi	Dummy: $= 1$ if would take up an account with deposit obligations until chosen date	oa_19
withdrawaldeposit_4weeks	Dummy: $= 1$ if would take up an account with withdrawal restrictions & deposit obligations for the next four weeks	oa_20
withdrawaldeposit_flexi	Dummy: $= 1$ if would take up an account with withdrawal restrictions & deposit obligations until chosen date	oa_21
anycommitment_flexi	Dummy: $= 1$ if would take up any one or more of the commitment accounts offered	oa_16-oa_21