B For Online Publication – Online Appendix

B.1 Experimental materials

Sampling and randomisation protocol

- Prior to the start of the baseline survey, I privately assigned all clients a random number in Stata using pure randomisation.
- Clients were then sorted on this number to determine their order of priority for surveying within each village.
- The first ten clients in the sorted list constituted the designated subjects, whilst the remainder constituted the reserve subjects and were to be used as replacements in order of their position in the sorted list: for example, if one designated subject was not available then enumerators would seek out the eleventh subject in the list, and so on.
- This procedure was used to obtain a random sample of NRSP borrowers and avoid "cherry-picking" of replacement subjects, for example if the survey team tried to select replacements out of those clients who had a particularly good relationship with NRSP, or who appeared particularly in need of the participation fee.
- The exact same randomisation procedure was used for villages.
- Following an enumerator error, three subjects were incorrectly administered the wrong frame order for the time preference tasks on day fifteen. These subjects' data is kept for analyses involving day one data only, but dropped from all analyses involving day fifteen.
- A further forty-eight subjects received their follow-up interview on day fourteen rather than day fifteen due to a public holiday. Following the pre-

analysis plan, all analyses were run both with and without these individuals, and since there is no significant difference these individuals are included in the full sample.

Figure A.3: Structure of experimental sessions

<table>
<tr><td colspan="2">1. Baseline survey, day one session only
2. Income and expenditure survey</td></tr>
<tr><td colspan="2">3. Participation fee explanation & payment (payment on day 1 vs. day 15)
4. Explanation of activity incentivization</td></tr>
<tr><td>Activities – “salience” order

5. Time preferences (near vs. far first)

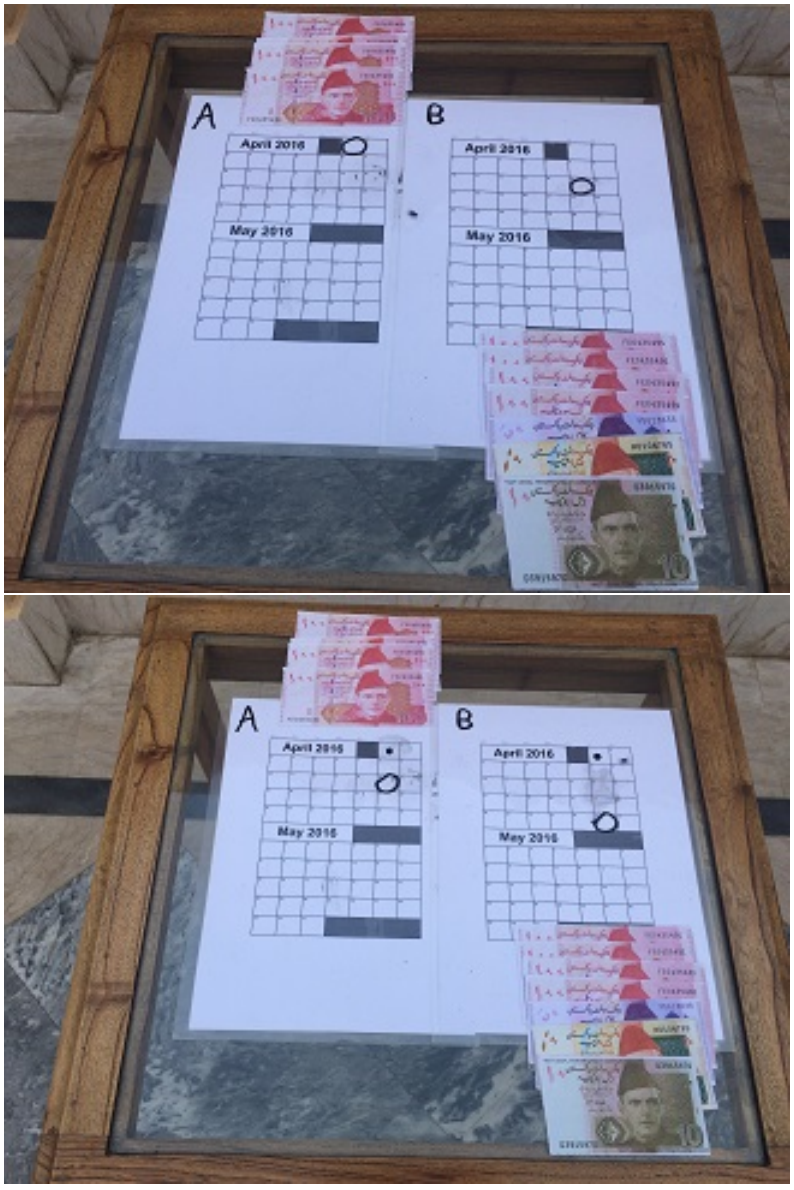
6. Control activities</td><td>Activities – “control” order

5. Control activities

6. Time preferences (near vs. far first)</td></tr>
<tr><td colspan="2">7. Activity payments</td></tr>
</table>

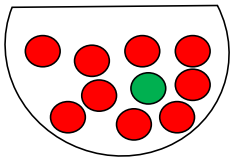
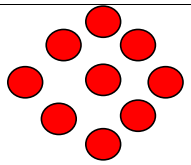
Notes: The ordering of items in italics was subject to randomisation. Time preference activities consisted of multiple price lists. Cognitive tasks comprised a maths test, digit span test, and numerical Stroop test. Risk preference activities consisted of certainty equivalents. Optimism was measured by a novel task eliciting subjective probabilities over the probability of winning a given lottery with set objective probabilities. The respondent was asked how likely she thought it was that an average person would win that lottery, and how likely it was that she herself would win.

Figure A.4: Multiple price list demonstrations



Notes: The top panel illustrates the near frame (now versus two weeks from now), and the bottom panel illustrates the far frame (two weeks from now versus four weeks from now).

Figure A.5: Risk task (certainty equivalents)

	A. Uncertain option			B. Certain amount	A or B
		GREEN	RED		
1		 +400 Rs	 +0 Rs	0 Rs	ce_g_11
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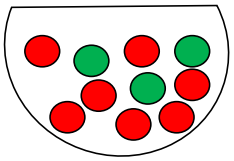
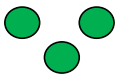
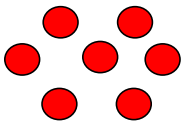
				100 Rs	ce_g_13
				150 Rs	ce_g_14
				200 Rs	ce_g_15
				250 Rs	ce_g_16
				300 Rs	ce_g_17
				350 Rs	ce_g_18
				400 Rs	ce_g_19
2		 +400 Rs	 +0 Rs	0 Rs	ce_g_21
				50 Rs	ce_g_22
				100 Rs	ce_g_23
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				200 Rs	ce_g_25
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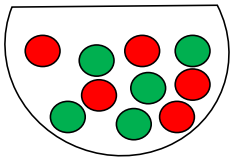
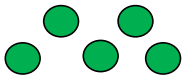
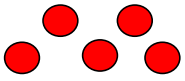
				300 Rs	ce_g_27
				350 Rs	ce_g_28
				400 Rs	ce_g_29
3		 +400 Rs	 +0 Rs	0 Rs	ce_g_31
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				100 Rs	ce_g_33
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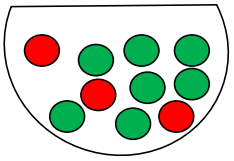
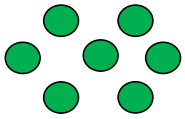
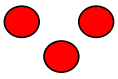
				350 Rs	ce_g_38
				400 Rs	ce_g_39
4		 +400 Rs	 +0 Rs	0 Rs	ce_g_41
				50 Rs	ce_g_42
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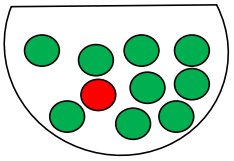
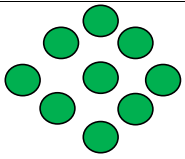
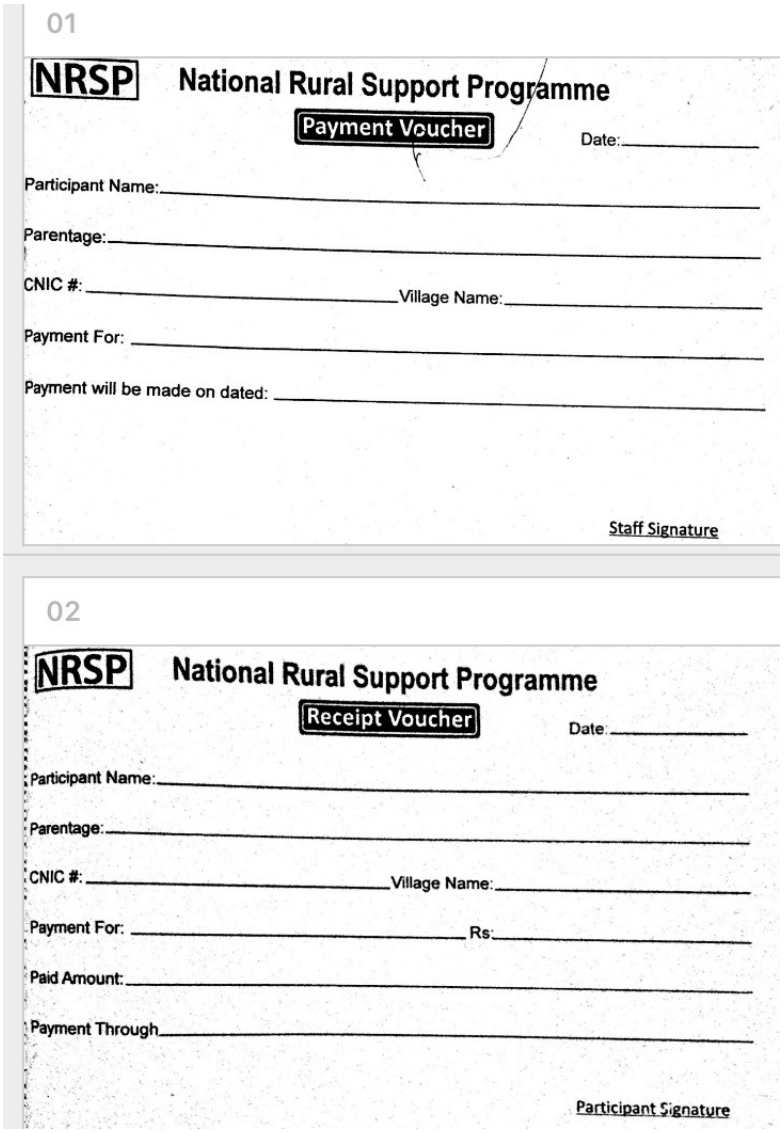
5		 +400 Rs	 +0 Rs	0 Rs	ce_g_51
				50 Rs	ce_g_52
				100 Rs	ce_g_53
				150 Rs	ce_g_54
				200 Rs	ce_g_55
				250 Rs	ce_g_56
				300 Rs	ce_g_57
				350 Rs	ce_g_58
				400 Rs	e_g_59

Notes: For each row, the subject was asked to choose between the lottery on the left (A) and the certain amount on the right (B). Participants were also presented with a bag of balls and monopoly money as visual aids. The enumerator explained the task (script available on request) and filled in the response for each question on a computerised tablet.

Figure A.6: Payment vouchers



Notes: The top panel displays the vouchers for promise of future payment. The bottom panel shows the vouchers for receipt of payment. These vouchers were used for participation fees, and for payments in the time preference and risk preference activities.

Activity payment protocol

- At the end of the day one session, the individual first draws a ball from a bag to determine whether she will be paid for her responses to today's activities or for her responses to the day fifteen activities.

- The probabilities are tilted 10%-90% towards being paid for the day fifteen activities, so that most subjects do not end up answering hypothetically at follow-up (since those who are paid for their responses on day one know at follow-up that they will not be paid for their responses at follow-up).[23]

- Next — or at the end of the follow-up session, if the subject draws to be paid at follow-up — the subject draws another ball to determine whether she will be paid for the risk or the time activities; and if she draws for the time activities, she draws further balls to determine which frame (near or far) and question she will be paid for.

- Note that subjects were not incentivized in the cognitive functioning and optimism tasks, as these tasks were designed to measure subjects' instinctive responses inclusive of any biases they held.

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

- The enumerator then displays her response and pays her accordingly.

- If the response involves payments in the future, the enumerator writes out a payment voucher clearly stating the time and amount of the future payment,

[23]Note this in principle leads to more "high-powered" incentives on day fifteen for the subjects who have not yet been paid for an activity on day one, as these subjects know that they will be paid for one of their responses at follow-up. However, this should not affect the results as there should be no difference in this effect across treatment arms.

and a member of the survey team returns to the household on that date to make the payment.

B.2 Additional data

Table A.5: Static inconsistency on day one & revisions on day fifteen

	Day 15 dynamic revisions			
Day 1 static choices	"Present-biased"	"Time-consistent"	"Future-biased"	Total
"Present-biased"	95	11	30	136
	(18.2%)	(2.1%)	(5.7%)	(26.1%)
"Time-consistent"	81	69	52	202
	(15.5%)	(13.2%)	(10.0%)	(38.7%)
"Future-biased"	83	19	82	184
	(15.9%)	(3.6%)	(15.7%)	(35.2%)
Total	259	99	164	522
	(49.6%)	(19.0%)	(31.4%)	(100.0%)

Notes: For static reversals on day one, "present-biased", "time-consistent" and "future-biased" are dummy variables indicating that a subject's near-frame switch-point on day one was respectively greater than, less than or the same as her far-frame switch-point in the multiple price list activity on day one. Subjects are classified as either "present-biased", "time-consistent" or "future-biased". For dynamic revisions on day fifteen, "present-biased", "time-consistent" and "future-biased" are dummy variables indicating that a subject's near-frame switch-point in the multiple price list activity on day fifteen was respectively greater than, less than or the same as her far-frame switch-point on day one. Subjects are again classified as either "present-biased", "time-consistent" or "future-biased". Subjects received the same activity order and frame order on day fifteen as they had received on day one.

Table A.6: Balance – salience activity order treatment (pre-specified)

	Mean	Salience= 1 Mean	Salience= 0 Mean	Mean Diff.	Total N	Salience= 1 N	Salience= 0 N
Liquidity							
HH income (100,000 PKR)	2.70	2.74	2.66	0.09	523	252	271
Savings (100,000 PKR)	0.44	0.45	0.43	0.02	525	252	273
Bank account	0.23	0.25	0.21	0.05	525	252	273
Could borrow	0.98	0.98	0.98	0.00	525	252	273
Could borrow formal	0.97	0.97	0.97	0.01	525	252	273
Could borrow informal	0.03	0.03	0.03	-0.00	525	252	273
Harvest							
Harvests wheat	0.37	0.38	0.36	0.03	525	252	273
Demographics							
Muslim	0.88	0.88	0.88	0.00	525	252	273
Education (years)	2.24	2.06	2.40	-0.34	525	252	273
Housewife	0.75	0.71	0.78	-0.06	525	252	273
Age	37.85	38.38	37.37	1.01	525	252	273
Married	0.88	0.85	0.91	-0.06*	525	252	273
HH size	6.30	6.38	6.22	0.16	524	252	272
HH head	0.06	0.05	0.06	-0.01	525	252	273
HH decisions (index 0-1)	0.11	0.11	0.11	-0.00	525	252	273
Trust							
Trust NRSP (1-5)	4.22	4.23	4.21	0.02	525	252	273
Trust self (1-5)	4.42	4.40	4.44	-0.03	525	252	273

Notes: All variables are taken from the baseline survey, conducted at the start of the day one session prior to revelation of treatment status. Treatment status — receiving the time-preference activities first ("salience"), or receiving the control activities first — is computer-randomised prior to session. Mean diff. represents difference in means across the two treatment arms. *, ** and *** indicate significance of this difference at the 10%, 5% and 1% levels respectively, as estimated from a regression of the variable of interest on the treatment indicator, with standard errors robust to individual heteroskedasticity.

Table A.7: Balance – frame order treatment (pre-specified)

	Mean	Near frame first Mean	Near frame second Mean	Mean Diff.	Total N	Near frame first N	Near frame second N
Liquidity							
HH income (100,000 PKR)	2.70	2.75	2.65	0.11	523	253	270
Savings (100,000 PKR)	0.44	0.45	0.43	0.02	525	254	271
Bank account	0.23	0.26	0.20	0.05	525	254	271
Could borrow	0.98	0.98	0.99	-0.00	525	254	271
Could borrow formal	0.97	0.97	0.97	0.01	525	254	271
Could borrow informal	0.03	0.02	0.04	-0.02	525	254	271
Harvest							
Harvests wheat	0.37	0.34	0.39	-0.05	525	254	271
Demographics							
Muslim	0.88	0.86	0.89	-0.03	525	254	271
Education (years)	2.24	2.22	2.26	-0.04	525	254	271
Housewife	0.75	0.76	0.74	0.02	525	254	271
Age	37.85	37.87	37.83	0.04	525	254	271
Married	0.88	0.87	0.90	-0.03	525	254	271
HH size	6.30	6.37	6.24	0.14	524	253	271
HH head	0.06	0.07	0.05	0.02	525	254	271
HH decisions (index 0-1)	0.11	0.11	0.11	0.00	525	254	271
Trust							
Trust NRSP (1-5)	4.22	4.25	4.20	0.05	525	254	271
Trust self (1-5)	4.42	4.44	4.41	0.04	525	254	271

Notes: All variables are taken from the baseline survey, conducted at the start of the day one session prior to revelation of treatment status. Treatment status — receiving the near-frame first or the far-frame first during the time preference activities — is computer-randomised prior to session. Mean diff. represents difference in means across the two treatment arms. *, ** and *** indicate significance of this difference at the 10%, 5% and 1% levels respectively, as estimated from a regression of the variable of interest on the treatment indicator, with standard errors robust to individual heteroskedasticity.

B.3 Additional results

Further evidence on effects of harvest timing (not pre-specified): Table A.8 shows the result cited in the main paper, that the range of income expectations for the next four weeks (i.e. income uncertainty) is much larger before the harvest.

Table A.8: Range of income expectations by survey timing (not pre-specified)

	(1) **Range of income expectations next four weeks** β / (s.e.)
Survey timing	
Pre-harvest	1357.68***
	(418.40)
Controls	✓
Observations	470
Control mean	3440.9

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. Income expectations are in Pakistani Rupees (100PKR ≈ 1 USD). The subject was asked her maximum and minimum expected household income for the next four weeks; the dependent variable is the difference between these two values. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Additional controls due to imbalance on survey timing are: trust in NRSP to keep a future appointment; trust in oneself to keep future appointments; and decision-making power within the household. The sample excludes the five additional villages which were included to boost power in the windfall timing experiment. N=476. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

Table A.9 shows the result cited in the main paper, that the effects on "time-inconsistency" and "future-bias" also hold for a smooth function of the survey date, rather than a simple pre-/post-harvest cut-off.

Effects on switch-points (also pre-specified): Table A.10 shows the effects of the participation fee windfall timing on individuals' switch-points in the near and the far frame separately. As discussed in the main paper, note that these

Table A.9: Effect of survey date on day one inconsistency (not pre-specified)

	(1) **"Present-biased"** **Day one** **All** Mfx / (s.e.)	(2) **"Time-consistent"** **Day one** **All** Mfx / (s.e.)	(3) **"Future-biased"** **Day one** **All** Mfx / (s.e.)
Day of survey period (1-48)	0.000	0.005**	-0.005**
	(0.001)	(0.002)	(0.002)
Controls	✓	✓	✓
Observations	476	476	476
Day 1 mean	0.20	0.3	0.5

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. "Present-biased" ["time-consistent"]("future-biased") is a dummy indicating a near-frame switch-point greater than [equal to] (less than) the far-frame switch-point in the multiple price list activity. Subjects are classified as either "present-biased", "time-consistent" or "future-biased". "Day of survey period" is the day (1-48) of the survey period on which a subject's village received its baseline surveys, as randomly assigned. The sample excludes the five villages which were included at the end of the survey period to boost sample size. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Additional controls due to imbalance on survey timing are: trust in NRSP to keep a future appointment; trust in oneself to keep future appointments; and decision-making power within the household. Reported effects represent the marginal effects at the mean. N=476.

outcome variables do not directly translate into the percentage of individuals appearing "present-biased" or "future-biased", since the latter depend on the relative movement in the near-frame and the far-frame switch-points for each individual.

Table A.11 shows the effects of the survey timing on individuals' switch-points in the near and the far frame separately. As columns (1) and (2) show, having a baseline interview prior to the harvest makes individuals appear more patient in both frames. This is in contrast with the pre-registered predictions, but may reflect a desire to save until uncertainty about harvest yields is resolved.

Effects on day fifteen measures (also pre-specified): Columns (3) and (4) of Table A.11 show that the pre-harvest treatment has no effect on the near-frame and far-frame switch-points on day fifteen.

Table A.12 shows that the survey timing also does not affect measures of time

Table A.10: Treatment effects on switch-points – windfall timing (also pre-specified)

	(1) **Near frame Day one All** OLS	(2) **Far frame Day one All** OLS	(3) **Near frame Day fifteen All** OLS	(4) **Far frame Day fifteen All** OLS
Participation fee timing				
Pay on day 15	16.86	7.29	5.44	12.45
	(20.34)	(19.70)	(20.98)	(20.36)
Controls	✓	✓	✓	✓
Village f.e.'s	✓	✓	✓	✓
Observations	523	523	520	519
Control mean	666.7	679.2	753.9	728.2

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. Switch-points are in Pakistani Rupees (100 PKR ≈ 1 USD). In each frame, subjects were asked to choose between 400 PKR on the earlier date or the amount shown on the later date. The switch-point in each frame is equal to the first value at which the subject chose to receive the payment on the later date. In the near frame, the earlier date was today and the later date was two weeks from today. In the far frame, the earlier date was two weeks from today and the later date was four weeks from today. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). N=525. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

Table A.11: Treatment effects on switch-points – survey timing (also pre-specified)

	(1)	(2)	(3)	(4)
	Near frame	**Far frame**	**Near frame**	**Far frame**
	Day one	**Day one**	**Day fifteen**	**Day fifteen**
	All	**All**	**All**	**All**
	OLS	OLS	OLS	OLS
Survey timing				
Pre-harvest	-69.48**	-84.40***	-16.37	-35.88
	(27.07)	(26.03)	(24.11)	(23.15)
Controls	✓	✓	✓	✓
Observations	476	476	474	473
Control mean	694.6	716.2	761.3	751.1

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. Switch-points are in Pakistani Rupees (100PKR $\approx$ 1 USD). In each frame, subjects were asked to choose between 400 PKR on the earlier date or the amount shown on the later date. The switch-point in each frame is equal to the first value at which the subject chose to receive the payment on the later date. In the near frame, the earlier date was today and the later date was two weeks from today. In the far frame, the earlier date was two weeks from today and the later date was four weeks from today. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Additional controls due to imbalance on survey timing are: trust in NRSP to keep a future appointment; trust in oneself to keep future appointments; and decision-making power within the household. The sample excludes the five additional villages which were included to boost power in the windfall timing experiment. N=476. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

preference in the day fifteen interviews. This is intuitive, since whilst the pre-harvest treatment guarantees that subjects received their day one interview prior to the harvest, many of these subjects still received their day fifteen interview after harvesting had started, and so in the day fifteen interviews they faced similar conditions to subjects who received both interviews after harvesting had begun.

Columns (1)-(3) of Table A.13 show that there is no significant treatment effect when the dependent variable is appearing statically "present-biased", "time-consistent" or "future-biased" on day fifteen. This is at odds with the predictions if subjects do not exhibit narrow framing and if subjects who receive the participation fee on day one do not save much of it: in that case we would expect to see the reverse pattern of the treatment effects observed on day one, and indeed this was the set of predictions registered in the pre-analysis plan. A possible explanation is that subjects who received the participation fee on day one did in fact save a substantial amount of it. If so, they would look very similar on day fifteen to subjects who only just received the participation fee, and thus we would not see a significant difference in subjects' choices across treatment arms.

In contrast, there should be no effect on day fifteen revisions even if subjects do not exhibit narrow framing. This is because revisions are driven by subjects reacting to new information about income or consumption, which they receive after the day one choices but before the day fifteen choices. The experiment does not generate any difference in this information, since the timing of the participation fee is already revealed prior to the day one choices. Columns (4)-(6) of Table A.13 support this prediction: there is no significant treatment effect when the dependent variable is making "present-biased" revisions on day fifteen, appearing dynamically "time-consistent" on day fifteen, or making "future-biased" revisions on day fifteen.

Table A.12: Treatment effects on day fifteen inconsistencies – survey timing (also pre-specified)

	(1) **"Present-biased" revision All** Mfx / (s.e.)	(2) **"Time-consistent" no revision All** Mfx / (s.e.)	(3) **"Future-biased" revision All** Mfx / (s.e.)	(4) **"Present-biased" Day fifteen All** Mfx / (s.e.)	(5) **"Time-consistent" Day fifteen All** Mfx / (s.e.)	(6) **"Future-biased" Day fifteen All** Mfx / (s.e.)
Survey timing						
Pre-harvest	0.068	-0.035	-0.032	0.061	0.012	-0.070
	(0.056)	(0.037)	(0.047)	(0.052)	(0.052)	(0.046)
Controls	✓	✓	✓	✓	✓	✓
Observations	474	474	474	474	474	474
Control mean	0.462	0.220	0.318	0.241	0.514	0.245

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. In columns (1)-(3), "present-biased" ["time-consistent" ("future-biased") is a dummy indicating a near-frame switch-point on day fifteen greater than [equal to] (less than) the far-frame switch-point on day one. In columns (4)-(6), "present-biased" ["time-consistent"]("future-biased") is a dummy indicating a near-frame switch-point on day fifteen greater than [equal to] (less than) the far-frame switch-point on day fifteen. In each case, subjects are classified as either "present-biased", "time-consistent" or "future-biased". "Pre-harvest" indicates a day one session prior to 25th April 2016. The sample excludes the five additional villages which were included at the end of the survey period to boost sample size. The sign and significance of estimated coefficients remains unchanged when these villages are dropped. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Additional controls due to imbalance on survey timing are: trust in NRSP to keep a future appointment; trust in oneself to keep future appointments; and decision-making power within the household. Reported effects represent the marginal effects at the mean. N=476. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

Table A.13: Treatment effects on day fifteen inconsistencies – windfall timing (also pre-specified)

	(1) **"Present-biased" revision All** Mfx / (s.e.)	(2) **"Time-consistent" no revision All** Mfx / (s.e.)	(3) **"Future-biased" revision All** Mfx / (s.e.)	(4) **"Present-biased" Day fifteen All** Mfx / (s.e.)	(5) **"Time-consistent" Day fifteen All** Mfx / (s.e.)	(6) **"Future-biased" Day fifteen All** Mfx / (s.e.)
Participation fee timing						
Pay on day 15	0.002	0.039	-0.039	0.001	0.046	-0.052
	(0.048)	(0.039)	(0.042)	(0.043)	(0.048)	(0.041)
Controls	✓	✓	✓	✓	✓	✓
Village f.e.'s	✓	✓	✓	✓	✓	✓
Observations	510	435	510	480	520	461
Control mean	0.498	0.174	0.328	0.274	0.475	0.251

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. In columns (1)-(3), "present-biased" ["time-consistent" ("future-biased") is a dummy indicating a near-frame switch-point on day fifteen greater than [equal to] (less than) the far-frame switch-point on day one. In columns (4)-(6), "present-biased" ["time-consistent"]("future-biased") is a dummy indicating a near-frame switch-point on day fifteen greater than [equal to] (less than) the far-frame switch-point on day fifteen. In each case, subjects are classified as either "present-biased", "time-consistent" or "future-biased". Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Reported effects represent the marginal effects at the mean. N=525. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

LASSO-selected controls (not pre-specified): Table A.14 presents the main results including controls selected by a post-double LASSO procedure, as referenced in the main paper.

Frame and activity order (also pre-specified): Table A.15 shows that the salience treatment has no effect on measures of "time-inconsistency". In contrast, frame order effects are large: subjects are 17.5 percentage points less likely to appear "present-biased" if the near frame is elicited first (p-value 0.008, FDR-adjusted q-value 0.012 taken across the three outcome variables in Table A.15) and 12.3 percentage points more likely to appear "future-biased", although this is only borderline significant (p-value 0.059, FDR-adjusted q-value 0.128). It is possible that this is capturing a learning effect, or an effect in which subjects become more patient as tasks progress. However, these effects also persist in the multiple price lists elicited on day fifteen (results available on request), making a learning story less plausible. An alternative explanation is that subjects become more impatient about payments in the far frame when answering the far frame second. Evidence on the switch-points from the subject frames is inconclusive: point estimates suggest that on day one, answering the near frame first leads to both a small reduction in the near-frame switch-point — i.e. increased patience in the near frame — and an increase in the far-frame switch-point — i.e. decreased patience in the far frame — but neither estimate is significant; tables available on request.

Alternative specifications (also pre-specified): I also estimate a number of robustness checks using various alternative definitions of the sample and the dependent variables, as specified in the pre-analysis plan.

1. Table A.16 presents the harvest timing results controlling for measures of cognitive functioning.

Table A.14: Treatment effects – controls selected by post-double LASSO (not pre-specified)

	(1) "Present-biased" Day one All β / (s.e.)	(2) "Time-consistent" Day one All β / (s.e.)	(3) "Future-biased" Day one All β / (s.e.)	(4) "Present-biased" Day one All β / (s.e.)	(5) "Time-consistent" Day one All β / (s.e.)	(6) "Future-biased" Day one All β / (s.e.)
Participation fee timing						
Pay on day 15	0.058	0.013	-0.072*			
	(0.037)	(0.039)	(0.039)			
Survey timing						
Pre-harvest				0.032	-0.140***	0.109**
				(0.036)	(0.052)	(0.045)
LASSO-selected Controls	✓	✓	✓	✓	✓	✓
Village f.e.'s	✓	✓	✓	✓	✓	✓
Observations	522	522	522	475	475	475
Control mean	0.231	0.381	0.388	0.247	0.448	0.305

Standard errors in parentheses
* p¡0.10, ** p¡0.05, *** p¡0.01

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. "Present-biased" ("future-biased") is a dummy indicating a near-frame switch-point greater than (less than) the far-frame switch-point in the multiple price list activity. Subjects are classified as either "present-biased", "future-biased" or "time-consistent" (not shown). "Pre-harvest" ("post-harvest") indicates a day one session prior to (later than) 25th April 2016. "Harvests wheat" is a dummy variable equal to one if the subject's household will harvest wheat at this harvest. The sample in columns (4)-(6) excludes the five villages which were included at the end of the survey period to boost sample size. Potential controls are all those listed in table A.7.

Table A.15: Windfall effects controlling for activity & frame order (also pre-specified)

	(1) **"Present-biased"** **Day one** **All** Mfx / (s.e.)	(2) **"Time-consistent"** **Day one** **All** Mfx / (s.e.)	(3) **"Future-biased"** **Day one** **All** Mfx / (s.e.)
Participation fee timing			
Pay on day 15	0.070*	0.009	-0.080*
	(0.041)	(0.048)	(0.046)
Salience & frame order			
Time prefs first	-0.051	0.080	-0.010
	(0.058)	(0.067)	(0.070)
Near frame first	-0.175***	0.072	0.123*
	(0.062)	(0.068)	(0.065)
Time prefs first*Near frame first	0.125	-0.146	-0.000
	(0.088)	(0.099)	(0.097)
Controls	✓	✓	✓
Village f.e.'s	✓	✓	✓
Observations	493	503	504
Control mean	0.323	0.323	0.355

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee, activity order and frame order treatment status. "Present-biased" ["time-consistent"]("future-biased") is a dummy indicating a near-frame switch-point greater than [equal to] (less than) the far-frame switch-point in the multiple price list activity. Subjects are classified as either "present-biased", "time-consistent" or "future-biased". Whether the subject is married is added as an additional control, due to imbalance on the salience treatment. Reported effects represent the marginal effects at the mean. N=525. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

2. Tables A.17 and A.18 present further, saturated specifications that were included in the pre-analysis plan but are underpowered.

3. To exclude any possible effects of making responses unincentivized, I re-estimate Equation 1 dropping any subjects who had already randomly drawn to be paid for their answers to the activities on day one. Results are identical to those for the main sample in columns (1)-(3) of Table 3; tables available on request.

4. As a maximally conservative way of excluding subjects who may have misunderstood the task or made a small mistake at some point, I re-estimate Equation 1 but this time drop all subjects who exhibit multiple-switching in any of the frames on either date. Point estimates for the treatment effects are virtually unchanged, but become marginally insignificant due to the reduction in sample size; tables available on request.

5. As well as dropping those subjects who never switch to the later payment in one or more frames, I also drop those who always choose the later payment, i.e. who already "switch" in the first question. Point estimates for the effect of the participation fee timing are virtually identical to the full sample, and remain highly significant for "future-bias" but become insignificant for "present-bias"; results available on request.

6. To allow for the possibility that subjects may be indifferent rather than having a strict preference at the point where they "switch", I re-set the dummies for "present-bias" and "future-bias" to equal to one only if the subject's responses differ by more than one question across the two frames concerned. I then re-run the estimations of 1 with these more conservative dummy variables as the dependent variables. The point estimates for the treatment effects of the participation fee timing are very similar to the initial results. However, they are no longer significant, since multiple villages are now dropped from the logit estimations with village fixed effects due to lack of variation in the new dependent variable; results available on request.

Table A.16: Treatment effects of survey timing – additional controls (also pre-specified)

	(1) **"Present-biased" Day one** Mfx / (s.e.)	(2) **"Future-biased" Day one** Mfx / (s.e.)	(3) **"Present-biased" Day one** Mfx / (s.e.)	(4) **"Future-biased" Day one** Mfx / (s.e.)	(5) **"Present-biased" Day one** Mfx / (s.e.)	(6) **"Future-biased" Day one** Mfx / (s.e.)
Survey timing						
Pre-harvest	0.020	0.126**	0.013	0.145***	0.018	0.129***
	(0.036)	(0.051)	(0.038)	(0.050)	(0.038)	(0.049)
Cognitive functioning						
Digit span 1 (score 1-7)	0.008	0.008				
	(0.023)	(0.022)				
Maths 1 (score 1-8)	-0.017*	0.017*				
	(0.009)	(0.009)				
Stroop time 1	-0.002**	0.001				
	(0.001)	(0.001)				
Risk preferences						
Certainty premium 1 (100 PKR)			-0.002	0.007*		
			(0.003)	(0.004)		
Optimism						
Probablity optimism 1					-0.000	0.000
					(0.000)	(0.000)
Controls	✓	✓	✓	✓	✓	✓
Village f.e.'s	✓	✓	✓	✓	✓	✓
Observations	471	471	476	476	476	476
Control mean	0.244	0.300	0.244	0.300	0.244	0.300

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee timing treatment status, as are cognitive functioning, risk preferences and optimism. "Present-biased" ("future-biased") is a dummy indicating a near-frame switch-point greater than (less than) the far-frame switch-point in the multiple price list activity. Subjects are classified as either "present-biased", "future-biased" or "time-consistent" (not shown). "Pre-harvest" indicates a day one session prior to 25th April 2016. "Certainty premium" is a PKR value aggregated across five sets of certainty-equivalent questions which involved different probabilities. "Optimism" is the difference between a subject's subjective belief of her own probability of winning a draw and the objective, given probability of winning that draw, aggregated across five draws with different probabilities. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Reported effects represent the marginal effects at the mean. N=476. Sample sizes differ where village fixed effects perfectly predict the outcome variable.

Table A.17: Treatment effects on day one inconsistency – further interactions (also pre-specified)

	(1) **"Present-biased" Day one** Mfx / (s.e.)	(2) **"Future-biased" Day one** Mfx / (s.e.)	(3) **"Present-biased" Day one** Mfx / (s.e.)	(4) **"Future-biased" Day one** Mfx / (s.e.)	(5) **"Present-biased" Day one** Mfx / (s.e.)	(6) **"Future-biased" Day one** Mfx / (s.e.)
Participation fee timing						
Pay on day 15	0.020	-0.007	0.005	-0.038	-0.043	-0.026
	(0.051)	(0.070)	(0.044)	(0.056)	(0.070)	(0.090)
Survey timing						
Pre-harvest	-0.019	0.194***			0.002	0.142*
	(0.055)	(0.068)			(0.062)	(0.078)
Interaction of timings						
Pay on day 15*Pre-harvest	0.067	-0.133			0.087	-0.084
	(0.075)	(0.096)			(0.094)	(0.121)
Wheat harvest						
Harvests wheat	-0.007	0.011	-0.110*	0.037	-0.066	-0.079
	(0.047)	(0.051)	(0.064)	(0.064)	(0.099)	(0.098)
Pay on day 15*Harvests wheat			0.153**	-0.075	0.175	0.079
			(0.077)	(0.088)	(0.113)	(0.133)
Pre-harvest*Harvests wheat					-0.057	0.168
					(0.138)	(0.135)
Pay day 15*Pre-harv*Harvests wheat					-0.042	-0.160
					(0.172)	(0.188)
Controls	✓	✓	✓	✓	✓	✓
Observations	476	476	523	523	476	476
Control mean	0.225	0.303	0.231	0.388	0.225	0.303

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee treatment status. "Present-biased" ["time-consistent"]("future-biased") is a dummy indicating a near-frame switch-point greater than [equal to] (less than) the far-frame switch-point in the multiple price list activity. Subjects are classified as either "present-biased", "time-consistent" or "future-biased". "Pre-harvest" indicates a day one session prior to 25th April 2016. "Harvests wheat" is a dummy variable equal to one if the subject's household will harvest wheat at this harvest. The sample excludes the five villages which were included at the end of the survey period to boost sample size. Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Additional controls due to imbalance on survey timing are: trust in NRSP to keep a future appointment; trust in oneself to keep future appointments; and decision-making power within the household. Reported effects represent the marginal effects at the mean. N=476.

Table A.18: Trust controls – further interactions (also pre-specified)

	(1) "Present-biased" Day one All		(2) "Future-biased" Day one All		(3) "Present-biased" Day one All		(4) "Future-biased" Day one All	
	β / (s.e.)	Mfx / (s.e.)	β / (s.e.)	Mfx / (s.e.)	β / (s.e.)	Mfx / (s.e.)	β / (s.e.)	Mfx / (s.e.)
Participation fee timing								
Pay on day 15	0.978	0.185	0.018	0.004	-0.680	-0.129	1.994	0.450
	(1.445)	(0.273)	(1.282)	(0.290)	(1.935)	(0.366)	(1.872)	(0.422)
Trust								
Trust NRSP (1-5)	0.080	0.015	0.189	0.043				
	(0.213)	(0.040)	(0.206)	(0.047)				
Trust self (1-5)					-0.201	-0.038	0.439	0.099
					(0.305)	(0.058)	(0.282)	(0.064)
Controls	✓		✓		✓		✓	
Village f.e.'s	✓		✓		✓		✓	
Observations	493		504		493		504	
Control mean	0.231		0.388		0.231		0.388	

Notes: Standard errors in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively. All dependent variables are measured after revelation of participation fee timing treatment status. "Present-biased" ("future-biased") is a dummy indicating a near-frame switch-point greater than (less than) the far-frame switch-point in the multiple price list activity. Subjects are classified as either "present-biased", "future-biased" or "time-consistent" (not shown). "Trust NRSP" and "Trust self" are 1-5 Likert-scale responses ("how strongly do you agree or disagree with the following statement?") to the following statements: "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"; "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". Pre-specified controls are: household income, savings, and possession of a bank account; ability to borrow in the next two months (formally or informally) if needed; whether the household harvests wheat; household religion; respondent's education; and her occupation (housewife or other). Reported effects in columns (1) and (2) represent the marginal effects at the mean. N=525. Sample sizes in columns (1) and (2) differ where village fixed effects perfectly predict the outcome variable. The coefficient on *trust self* in column (3) has an FDR-adjusted q-value of 0.002 taken across the two proxies of trust (Anderson, 2012). The coefficient on *trust self* in column (4) has an FDR-adjusted q-value 0.002 taken across the two proxies of trust.

B.4 Theoretical Framework

This Appendix outlines a simple theoretical framework, to illustrate how "time-inconsistent" choices in standard monetary tasks are insufficient to identify time-inconsistent preferences. Specifically, I highlight how decreases in an individual's expected marginal rate of intertemporal substitution may cause her spuriously to appear "present-biased". In doing so, I draw together insights from both Epper (2015) and Dean and Sautmann (2014). In particular, like Epper I adopt a buffer stock model of savings with partial asset integration. However, in contrast to Epper and following Dean and Sautmann, I allow for the possibility that the subject saves future experimental payments, and that she is truly present-biased ($\beta < 1$). Unlike Dean and Sautmann, I do not assume that the agent is perfectly sophisticated.

B.4.1 Measuring "present-bias"

The canonical, "static" way to measure present-bias is to interview a subject once and to ask for her choices over a near frame and a far frame. These choices can be used to generate the following measures:[24]

- **The near-frame switch-point $x_{1.0}$:** at $t = 0$, the amount of money $x_{1.0}$ that makes the subject indifferent between receiving a given, fixed sum x now at $t = 0$ or $x_{1.0}$ at $t = 1$.
- **The far-frame switch-point $x_{2.0}$:** at $t = 0$, the amount of money $x_{2.0}$ that makes the subject indifferent between receiving x at $t = 1$, or receiving $x_{2.0}$ at $t = 2$.

[24] Activities such as multiple price lists and Becker-DeGroot-Marschak auctions generate such measures directly. More complex tasks such as convex time budgets — if understood by subjects — can be used to generate such measures, and also to infer information about the curvature of the utility function. The intuition of the argument presented here still holds for such tasks.

If a subject's choices imply $x_{1.0} \neq x_{2.0}$, this is taken to violate "stationarity" (Halevy, 2015). In particular, if $x_{1.0} > x_{2.0}$ — i.e. her near-frame switch-point is greater than her far-frame switch-point at $t = 0$ — she is conventionally assumed to have a higher discount rate in the near frame than in the far frame, and is therefore labelled as "present-biased".

Recent "dynamic" attempts to measure time-inconsistency comprise both an initial session at $t = 0$ and a follow-up session at $t = 1$. This adds decisions over two further time frames — a near frame at $t = 1$ and a far frame at $t = 1$ — and thereby generates the following additional measures:

- **The near-frame switch-point $x_{2.1}$:** at $t = 1$, the amount of money $x_{2.1}$ that makes the subject indifferent between receiving x at $t = 1$ (which is now the present) or receiving $x_{2.1}$ at $t = 2$.
- **The far-frame switch-point $x_{3.1}$:** at $t = 1$, the amount of money $x_{3.1}$ that makes the subject indifferent between receiving x at $t = 2$ or receiving $x_{3.1}$ at $t = 2$.

Since the far frame at $t = 0$ becomes the near frame at $t = 1$, observing $x_{2.1}$ allows us see whether a subject re-allocates money between $t = 1$ and $t = 2$ now that these dates are in the immediate future. If a subject's choices imply $x_{2.0} \neq x_{2.1}$, this is taken to violate "time-consistency". In particular, if $x_{2.0} > x_{2.1}$ — i.e. her near-frame switch-point at $t = 1$ is greater than the far-frame switch-point at $t = 0$ — then she this is again taken as evidence that she has higher discount rate in the near frame than in the far frame. She is therefore labelled as "present-biased" in a dynamic sense.[25]

[25] If a subject chooses $x_{1.0} \neq x_{2.1}$ and/or $x_{2.0} \neq x_{3.1}$, this is taken to constitute a violation of "time-invariance". This is interpreted to mean that an individual's time preference parameters about the exact same time delay and distance from the present may be non-constant over time. This is a separate property from declining or increasing discount rates with time horizon, i.e. present-bias or future-bias.

B.4.2 Consumption and liquidity constraints

Let the subject's utility maximisation problem be as follows:

$$\max_{s_t \in [0, w_t)} U_0 = u(c_0) + \beta E_0 \sum_{t=1}^{\infty} \delta^t u(c_t) \tag{1}$$

$$s.t. \quad c_t = y_t + (1+r)s_{t-1} - s_t = w_t - s_t > 0 \quad \forall t \tag{2}$$

$$and \quad s_t \geq 0 \quad \forall t. \tag{3}$$

Equation 1 describes utility of the canonical "beta-delta" form,where $c_t \geq 0$ is her consumption in period t, δ is her discount factor as applied to period t, and β is her degree of present-bias ($\beta < 1$ describes present-bias, while $\beta = 1$ yields time-consistent preferences). For simplicity, assume that present-biased subjects are fully naïve about β, i.e. they implicitly believe that $\beta = 1$.[26] Equation 2 describes the agent's budget constraint: y_t is income in period t, which is taken to be exogenous, and s_t are savings at the end of period t. Equation 3 is a no-borrowing constraint, which is a standard assumption in buffer-stock savings models used to characterise household behaviour in developing countries.[27]

B.4.3 Experimental choices

Let the first experimental session take place at $t = 0$. Further assume that savings at $t = 0$ are pre-determined with respect to experimental payments, i.e. a subject has already chosen her current savings to optimise discounted expected utility

[26] Dean and Sautmann, building on the results of Harris and Laibson (2001), show that a perfectly sophisticated subject's choices across a near and a far frame do not identify β, and indeed only deviate due to a statistical bias. Thus an assumption of at least partial naïveté is needed for a subject's choices over a near and a far frame to identify anything about her present-bias.

[27] Section 3.2 provides evidence that this is a reasonable assumption in the context of the experiment presented below. Dean and Sautmann adopt a different approach, assuming that the interest rate $r(s_t)$ is decreasing and concave in s. This effectively nests the case here in which the interest rate on borrowing is infinite, and the interest rate on saving is zero or some constant positive number.

and does not adjust this when offered x by the experiment at $t = 0$.[28] This assumption is highly plausible for many developing contexts, since most subjects' savings are low and and illiquid and thus unlikely to respond immediately to experimental payments. Furthermore, poor subjects are rarely able to access a significant positive interest rate on saving outside of the experiment. Thus it is highly unlikely that a subject would accept a sooner payment in the hope of saving it outside the experiment, at a higher interest rate than that offered by the experiment.

Let the subject place weight $\kappa \in [0, 1]$ on her outside wealth when making experimental decisions. This nests the cases of narrow framing ($\kappa = 0$), partial asset integration ($\kappa \in (0, 1)$), and full asset integration ($\kappa = 1$). 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:

$$u(\kappa c_0 + x) + \beta\delta E_0 V_1(\kappa w_1) = u(\kappa c_0) + \beta\delta E_0 V_1(\kappa w_1 + x_{1.0}),$$

which rearranges to:

$$\frac{u(\kappa c_0 + x) - u(\kappa c_0)}{E_0 \left[V_1(\kappa w_1 + x_{1.0}) - V_1(\kappa w_1)\right]} = \beta\delta, \tag{4}$$

where the value function $V_t(w)$ is given by:

$$V_t(w) = \max_{s_t \in [0, w_t)} u(w - s) + \delta E_t V_{t+1}(w_{t+1}). \tag{5}$$

Similarly, at $t = 0$, for her to be indifferent between x at $t = 1$ versus $x_{2.0}$ at

[28] Cubitt and Read (2007) establish that if the subject at least partially saves or borrows against the experimental payment x, this causes problems for identifying time preferences. The purpose of the exercise here is to show that similar problems arise even if the subject does not actively seek to arbitrage experimental payments in this way.

$t = 2$ it must be the case that:

$$E_0[V_1(\kappa w_1 + x) + \delta V_2(\kappa w_2)] = E_0[V_1(\kappa w_1) + \delta V_2(\kappa w_2 + x_{2.0})],$$

which rearranges to:

$$\frac{E_0\left[V_1(\kappa w_1 + x) - V_1(\kappa w_1)\right]}{E_0\left[V_2(\kappa w_2 + x_{2.0}) - V_1(\kappa w_2)\right]} = \delta. \tag{6}$$

Combining Equations 4 and 6, a static observation of "present-bias" ($x_{1.0} > x_{2.0}$) will occur if:

$$\frac{u(\kappa c_0 + x) - u(\kappa c_0)}{\beta E_0\left[V_1(\kappa w_1 + x_{2.0}) - V_1(\kappa w_1)\right]} > E_0\left[\frac{V_1(\kappa w_1 + x) - V_1(\kappa w_1)}{V_2(w_2 + x_{2.0}) - V_2(w_2)}\right]. \tag{7}$$

For a sufficiently small x, the left-hand side of Equation 7 becomes the marginal rate of intertemporal substitution between $t = 1$ and $t = 0$, and the right-hand side is the marginal rate of intertemporal substitution between $t = 2$ and $t = 1$.

Consistent with the traditional interpretation of static reversals as indicating present-biased preferences, condition 7 may hold if $\kappa = 0$ and $\beta < 1$. However, condition 7 may hold even if $\beta = 1$, if the subject is anticipating higher income from $t = 1$ onwards which she is unable to smooth into the present, due to the no-borrowing constraint.[29] If so, the subject appears "present-biased" because she is experiencing a higher marginal rate of intertemporal substitution now at $t = 0$ than she excepts to experience from $t = 1$ onwards. Moreover, note that it is her subjective expectation that matters: if a subject is optimistic about her future prospects, then she may appear "present-biased" even if objectively her liquidity constraints are likely to remain constant. Thus overall, measured "present-bias" could reflect true present-bias, but could also reflect a rational response to liquidity

[29] Alternatively, she may be experiencing a low draw of income at $t = 0$ that she was unable fully to smooth via prior saving, or was lower than expected.

constraints, or indeed overly optimistic income expectations.

Turning to choices at the follow-up session at $t = 1$, for the subject to be indifferent between x at $t = 1$ versus $x_{2.1}$ at $t = 2$ it must be the case that:

$$\frac{u(\kappa c_1 + x) - u(\kappa c_1)}{E_1 \left[V_2(\kappa w_2 + x_{2.1}) - V_2(\kappa w_2) \right]} = \beta\delta. \tag{8}$$

Combining Equations 6 and 8, a dynamic observation of "present-bias" ($x_{2.1} > x_{2.0}$) will occur if:

$$\frac{u(\kappa c_1 + x) - u(\kappa c_1)}{\beta E_1 \left[V_2(\kappa w_2 + x_{2.0}) - V_2(\kappa w_2) \right]} > E_0 \left[\frac{V_1(w_1 + x) - V_1(\kappa w_1)}{V_2(\kappa w_2 + x_{2.0}) - V(\kappa w_2)} \right]. \tag{9}$$

Once more, this may be driven by truly present-biased preferences, i.e. $\beta < 1$ and $\kappa = 0$. However, again if $\kappa > 0$ then such choices may be observed even if $\beta = 1$. For example, it may be that the subject's marginal utility of consumption at $t = 1$ is higher than she originally anticipated it to be, perhaps because she received a low draw from her subjective probability distribution over y_1. Alternatively, it could be that she has revised her expectation of the marginal utility of consumption at $t = 2$ downwards, for example if she has received a signal between $t = 0$ and $t = 1$ that y_2 is going to be higher than originally anticipated.

The reverse arguments also apply for static and dynamic choice reversals in the direction of "future-bias". Such choices will be observed if the reverse of Equations 7 and 9 hold, respectively. Intuitively, a static observation of "future-bias" could be explained by the subject anticipating a *downward* trend in her background consumption between $t = 0$ and $t = 1$. Meanwhile, an observation of dynamic "future-bias" could be explained by her receiving a subjectively *high* draw of consumption at $t = 1$,[30] or a *downward* revision of her consumption forecast for

[30] If an agent is risk-averse, then even a draw of $y_1 = E_0[y_1]$ would leave an agent with higher utility at $t = 1$ than the expected utility at $t = 0$ of the draw over y_1. Thus technically, even a draw of y_1 equal to its expectation could be sufficient to induce lower marginal utility at $t = 1$

$t = 2$.

However, recall that the model's assumptions capture the stylized fact that poor subjects are often able to save, albeit imperfectly, but not to borrow. Therefore, subjects are partially able to smooth high current income or positive income shocks into the future via saving; whereas they are unable to smooth high future income into the present, or to smooth away negative income shocks via borrowing. This implies that conditions for observing spurious "future-bias" are less likely to hold than the conditions for observing spurious "present-bias". Thus the model also predicts that, as long as subjects are more credit-constrained than savings-constrained, more spurious "present-bias" than spurious "future-bias" is likely to be observed.

B.4.4 Predicted effects of income shocks

It remains an empirical question whether the magnitude of κ is large enough to generate a significant proportion of apparently "present-biased" and "future-biased" choices. Identifying κ — and more broadly whether changes in background consumption can have a causal effect on measures of "present-bias" and "future-bias" — requires exogenous variation in the size and timing of income shocks. In particular, the experiment is designed to use the following predictions to test between $\kappa = 0$ and $\kappa > 0$.

Proposition 1. If a subject receives a positive and unanticipated windfall at $t = 0$:

a. If $\kappa = 0$, there should be no effect on her experimental choices, which are a pure reflection of her preference parameters β and δ.

b. If $\kappa > 0$, from Equation 7 she is less likely to appear statically "present-biased" at $t = 0$ ($x_{1.0} > x_{2.0}$) and more likely to appear statically "future-

than was expected at $t = 0$, and thus an observation of dynamic "future-bias".

biased" at $t = 0$ ($x_{1.0} < x_{2.0}$).

Intuition: the positive income shock at $t = 0$ increases consumption at $t = 0$, which decreases the marginal utility of consumption at $t = 0$ and decreases the marginal rate of intertemporal substitution between $t = 0$ and $t = 1$.[31] This decreases the likelihood of Equation 7 holding, and increases the likelihood of its reverse holding.

Proof: For x and $x_{2.0}$ sufficiently small, for $\kappa > 0$ Equation 7 reduces to the condition that:

$$\frac{u'(\kappa c_0)}{\beta E_0 V_1'(\kappa w_1)} > E_0 \left[\frac{V_1'(\kappa w_1)}{V_2'(\kappa w_2)} \right]. \tag{10}$$

Conversely, a static choice reversal in the direction of "future-bias" will occur if:

$$\frac{u'(\kappa c_0)}{\beta E_0 V_1'(\kappa w_1)} < E_0 \left[\frac{V_1'(\kappa w_1)}{V_2'(\kappa w_2)} \right]. \tag{11}$$

If a subject receives positive and unanticipated windfall income Z at $t = 0$, a static choice reversal in the direction of "present-bias" will now occur on the condition that:

$$\frac{u'(\kappa(c_0 + Z))}{\beta E_0 V_1'(\kappa w_1)} > E_0 \left[\frac{V_1'(\kappa w_1)}{V_2'(\kappa w_2)} \right]. \tag{12'}$$

By the concavity of $u(.)$, condition 12' is less likely to hold than condition 10 for $Z > 0$ since the numerator of the left-hand side has decreased. Thus the subject is less likely to exhibit static choice reversals in the direction of "present-bias".

On the other hand, a static reversal in the direction of "future-bias" will now

[31] Recall that, by assumption, savings at $t = 0$ are pre-determined and thus the windfall is entirely consumed at $t = 0$. However, the same qualitative results hold if this assumption is relaxed, as long as the subject's marginal propensity to consume out of windfall income is sufficiently high such that she does not perfectly smooth the windfall into future periods. Thus technically, the joint null for the experiment is that $\kappa = 0$ and/or the agent does perfectly smooth the windfall into future periods via saving.

occur on the condition that:

$$\frac{u'(\kappa(c_0 + Z))}{\beta E_0 V_1'(\kappa w_1)} < E_0 \left[\frac{V_1'(\kappa w_1)}{V_2'(\kappa w_2)} \right]. \tag{13'}$$

Again, by the concavity of $u(.)$, Equation 13' is more likely to hold than Equation 11 for $Z > 0$, since the numerator of the left-hand side has decreased. *QED.*

Proposition 2. If a subject is told at $t = 0$ that she will receive a positive windfall at $t = 1$:

a. If $\kappa = 0$, there should be no effect on her experimental choices, which are a pure reflection of her preference parameters β and δ.

b. If $\kappa > 0$, from Equation 7 she is more likely to appear statically "present-biased" at $t = 0$ ($x_{1.0} > x_{2.0}$) and less likely to appear statically "future-biased" at $t = 0$ ($x_{1.0} < x_{2.0}$).

Intuition: The announced additional income at $t = 1$ decreases the expected marginal utility of consumption at $t = 1$. Since the subject is credit-constrained, this increases the marginal rate of intertemporal substitution between $t = 1$ and $t = 0$. This increases the likelihood of Equation 7 holding, and decreases the likelihood of its reverse holding.

Proof: If a subject is told at $t = 0$ that she will receive positive windfall income Z at $t = 1$, a static reversal in the direction of "present-bias" will now occur if:

$$\frac{u'(\kappa c_0)}{\beta E_0 V_1'(\kappa(w_1 + Z))} > E_0 \left[\frac{V_1'(\kappa(w_1 + Z))}{V_2'(\kappa w_2')} \right], \tag{12"}$$

where w_2' may differ from w_2 in Equation 10 if the subject chooses to adjust period-1 savings in response to the windfall.[32] By the concavity of $u(.)$, condition

[32]The assumption is only that current $t = 0$ savings are predetermined with respect to experimental income.

12" is more likely to hold than Equation 10 since the denominator of the left-hand side has decreased.[33]

Conversely, a static reversal in the direction of "future-bias" will occur if:

$$\frac{u'(\kappa c_0)}{\beta E_0 V_1'(\kappa(w_1+Z))} < E_0\left[\frac{V_1'(\kappa(w_1+Z))}{V_2'(\kappa w_2')}\right]. \tag{13"}$$

This condition is less likely to hold than condition 11, since again by the concavity of $u(.)$, the denominator of the left-hand side has decreased. *QED.*

[33]The numerator of the right-hand side has also increased; although the denominator of the right-hand side could also have increased identically to offset this, if the subject perfectly smooths the windfall across periods 1 and 2 via saving.