# ONLINE APPENDIX

"When Commitment Fails –

Evidence from a Field Experiment"

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May 2017

## I. Model Extensions

### *I.1. Stochastic Sophistication and Rational Expectations*

The model studies a simple form of partial sophistication. The assumption of a deterministic and incorrect $\tilde{\beta}$ was introduced by O'Donoghue and Rabin (1999), and suffices to highlight the main mechanism of undercommitment. In reality, individuals may have more complex belief distributions about their future preferences, or preferences themselves may be stochastic. Consider the special case of rational expectations, and let future $\beta$ be distributed according to some known $F_{\beta}(x)$, with an implied distribution for the minimum effective penalty, $G_{D_{min}}(x)$. The agent now faces an additional risk: The required $D_{min}$ realized in period 1 may exceed a given penalty chosen in period 0, leading to costly default. The probability of default can be reduced by choosing a higher penalty, but this increases her vulnerability to shocks. The period 0 agent responds by maximising expected utility under commitment with respect to the optimal penalty $D*$: She weighs equation A2 by the probability that a chosen penalty will be effective, $P[D_{min} \leq D*] = G_{D_{min}}(D*)$, otherwise she receives $2(1-\lambda)-D*$. This generalized optimization accounts for the possibility of no commitment, $D* = 0$ (the agent does not know whether she is able to save in autarky, thus $G_{D_{min}}(0) > 0$).

Comparing a situation with stochastic versus full sophistication, commitment becomes less attractive: In both cases, the agent correctly assesses the effective penalty level in expectation, $E(D_{min})$. Even with risk-neutral preferences, expected utility from adopting $E(D_{min})$ is lower in the stochastic case: The default risk from a higher realized $D_{min} > E(D_{min})$ is not compensated by any benefit in the case of $D_{min} < E(D_{min})$. The agent's response depends on the shape of $G_{D_{min}}(x)$, as well as on how much she gains from effective commitment, $(1-\lambda)^2(b-p)$. If this gain is large, she will move towards higher penalties, $D* > E(D_{min})$. If it is not, she will move away from commitment altogether. Uncertainty in agents' beliefs thus results in low commitment take-up and high conditional penalties. This prediction is amplified if the agent is risk-averse, as the variance of consumption increases under commitment.

The predictions of stochastic sophistication and rational expectations are in contrast with the patterns observed in the data - they cannot explain why individuals choose very low default penalties, why default is so frequent, and why commitment adoption is high. To reconcile stochastic beliefs about time-inconsistency with the empirical evidence, one needs to allow for belief distributions which are systematically biased towards naiveté. With beliefs that are skewed towards $\beta = 1$, predictions closely resemble those of Section 2.5. For instance, when agents assign zero probability to the true $\beta$ (or lower), default is deterministic, and all predictions of the simple model with $\tilde{\beta} > \beta$ hold. Formally, $F_{\tilde{\beta}}(\beta) = 0$ implies $G_{D_{min}}(D^*) = 0$. Alternatively, if agents assign some probability $\theta$ to their true $\beta$ , and $1 - \theta$ to some higher $\beta_h > \beta$ (similar to Eliaz and Spiegler (2006)), undercommitment and default will occur below some threshold $\hat{\theta}$.

### *I.2. Learning*

Empirical evidence suggesting partial sophistication about time-inconsistent preferences has been widely discussed.[1] But is it plausible that individuals permanently hold incorrect beliefs about their time preferences, despite being able to observe their own past behaviour? Ali (2011) shows that rational learning about time-inconsistency may be perpetually partial: The agent learns about her $\beta$ only if she exposes herself to temptation. If she is (or stochastically becomes) sufficiently pessimistic about being present-biased, she may use commitment to restrict her choice set to a point where she no longer learns. However, this argument cannot rationalise perpetual overoptimism, captured in the assumption that $\tilde{\beta} > \beta$: Optimistic planners undercommit and expose themselves to temptation, which should allow them to learn and update their beliefs over time. Yet, there are numerous reasons why Bayesian learning may be slow, or fail altogether: First, learning may be context-specific. The nature and degree of an individual's time-inconsistency may vary across dimensions (saving money, gym visits, food choices, work effort) or even across settings (saving on an installment-savings plan versus unscheduled savings). Context-specificity is likely to impede and slow down learning. In addition to learning about the temptations she faces, a new decisionmaking environment may also present the agent with a need to learn about the benefits and costs of her actions: How difficult is it to take a set amount of money out of the budget each period?[2] Ali (2011) discusses how such multidimensional learning problems may create challenges of identification, and further slow the updating of beliefs.

Applying context-specificity to the model, a plausible case is that the period 0 agent is familiar with her savings behaviour absent commitment, but unfamiliar with her savings behaviour under a formal commitment savings product. In particular, she may realize from past observation whether she is able to save for the nondivisible good by herself, i.e., the inequality $\beta \geq \hat{\beta}$ is observed. Nevertheless, not having first-hand experience with commitment, knowing that $\beta < \hat{\beta}$ does not help her to assess how much commitment it will take to make her save. This argument relates to Bénabou and Tirole (2004)'s model of willpower and limited recall, in which the agent fails to remember past motives and feelings, but forms beliefs about her willpower based on past actions.

A second potential impediment to Bayesian updating are self-serving beliefs: Despite observing her own behaviour, the agent may prefer to attribute past failures to save to taste shocks ("I didn't want the good anymore") or to income shocks ("business was bad last month"), rather than admit to herself that she is present-biased. It is intuitive that agents may like to think of themselves as disciplined savers, healthy eaters, or frequent gym-goers. To the author's knowledge, no study directly considers the possibility that individuals derive utility from believing they are

[1] Examples include DellaVigna and Malmendier (2006), Duflo et al. (2011), and Acland and Levy (2015).

[2] Admittedly, almost all individuals in this study had previously experienced installment structures through their loan repayments. However, the different labels 'savings' versus 'loan repayment' could imply a difference in cost, for instance in justifying them to one's family.

time-consistent. However, concerns about self-image and 'ego utility' have recently attracted research interest in the domain of skills and ability (Bénabou and Tirole (2002); Kőszegi (2006); Möbius et al. (2014)). Bénabou and Tirole (2002) argue theoretically that rational individuals may prefer optimistic views of themselves to accurate ones, due to consumption, signaling and motivational reasons. Möbius et al. (2014) find empirical evidence that subjects systematically overweigh positive feedback relative to negative, and update their beliefs too little in response to either type of signal. They reconcile the evidence with a model of 'optimally biased Bayesians,' who misinterpret the informativeness of signals, but then correctly apply Bayes' rule. The information processing bias is 'optimal' in that it weighs the utility from enjoying a favorable self-image with the cost of being more likely to make bad decisions. Similar arguments may apply to beliefs about time-inconsistency, in that individuals 'optimally' choose their degree of naiveté.

A potential third impediment comes from neuroscience: Recent evidence suggests that time-inconsistent behaviour may itself be a result of stress (Cornelisse et al. (2013); McClure et al. (2004)), or a cognitive consequence of scarcity (Mani et al. (2013); Shah et al. (2012)). It is an open question to what extent the same factors affect learning. For instance, if present-biased behaviour results from a lack of mental bandwidth allocated to specific tasks, and is thus situational and temporary in nature, then people may struggle to anticipate when, and for which tasks, they will adopt this behaviour. Similarly, at calm and reflective moments, they may underestimate how much their tastes will change when they are stressed, as evidenced by the literature on projection bias (Loewenstein et al. (2003)). This argument suggests an interesting contrast: In the standard model, partial sophistication is caused when the agent *underestimates* how much her future taste for immediate gratification resembles her current taste for immediate gratification. With contextual time-inconsistency and projection bias, partial sophistication is caused when the agent *exaggerates* how much her future tastes will resemble her current tastes, in that her current self is patient but her future 'stressed' self is not.

### *I.3. Pessimism and Overcommitment*

While the model focuses on agents who are optimistic about their degree of time-inconsistency ($\tilde{\beta} \geq \beta$), it easily accommodates pessimistic beliefs ($\tilde{\beta} < \beta$). Pessimistic agents overestimate the penalty required to make them save. Where pessimism results from agents believing they are strongly present biased in absolute terms (low $\tilde{\beta}$), commitment becomes very unattractive: As outlined in Propositions 3 and 4, the cost of commitment $\lambda \tilde{D}_{min}$ decreases in $\tilde{\beta}$ while the benefit $(1-\lambda)^2(b-p)$ is invariant to it. The perceived minimum effective penalty is likely to be prohibitive. Where it is not, agents will overcommit, with excessively large penalties $\tilde{D}_{min} > D_{min}$. Excessive penalties are costless absent shocks, but they increase the damage in the case of 'rational default.' Summarising, pessimistic agents do not adopt commitment when it would indeed be optimal;[3] and when they do, they choose penalties which harm them more than necessary in the case of shocks. Yet, they correctly assess that their chosen contracts are ex-ante improvements relative to autarky. Thus, offering commitment contracts remains weakly welfare-improving.

It is difficult to derive testable predictions for pessimistic agents ($\tilde{\beta} < \beta$): They will be pooled with time-consistent and sophisticated time-inconsistent agents at all times. The following empirical analysis should be viewed in this light: Theory predicts that the pool of non-adopters will consist of time-consistent agents, time-inconsistent agents who believe they can save by themselves, and time-inconsistent agents with a prohibitively high perceived

[3] This occurs when $\lambda D_{min}(\beta) \leq (1-\lambda)^2(b-p) < \lambda \tilde{D}_{min}(\tilde{\beta})$: The benefit of commitment outweighs its cost using the true effective penalty, but not using the higher penalty $\tilde{D}_{min}$ the pessimist believes she requires.

minimum effective penalty.[4] Similarly, the group of successful commitment adopters pools fully sophisticated and pessimistic agents, since both adopt effective penalties. Finally, the group of unsuccessful (defaulting) commitment adopters pools partial sophisticates and all adopters who suffered a shock.

## II. Proofs

**Proposition 1.** *In the No-Commitment Equilibrium, the nondivisible good is bought by sufficiently time-consistent agents, i.e., those with a time-consistency parameter $\beta$ above a threshold $\hat{\beta}$. The threshold $\hat{\beta}$ increases in the shock frequency $\lambda$ and the price p, and decreases in the benefit b.*

*Proof.* The period 1 agent prefers to save $s_1 = p-1$ rather than zero iff

$$\underbrace{1-(p-1)}_{c_1}+\beta\underbrace{[\lambda(p-1)+(1-\lambda)b]}_{c_2} \geq \underbrace{1}_{c_1'}+\beta\underbrace{(1-\lambda)}_{c_2'} \tag{A1}$$

Rearranging yields that, absent shocks in periods 1 and 2, the nondivisible good is purchased for

$$\beta \geq \hat{\beta} \equiv \frac{p-1}{\lambda(p-1)+(1-\lambda)(b-1)}.$$

It is easy to see that $\frac{\delta\hat{\beta}}{\delta\lambda} > 0$, $\frac{\delta\hat{\beta}}{\delta p} > 0$, and $\frac{\delta\hat{\beta}}{\delta b} < 0$. □

**Proposition 2.** *The minimum penalty that is effective in enforcing the savings plan, denoted $D_{min}$, strictly decreases in the time-consistency parameter $\beta$. Further, $D_{min}$ strictly increases in the shock frequency rate $\lambda$.*

*Proof.* The result directly follows from equation 2, noting that $\frac{\delta D_{min}}{\delta\beta} < 0$ and $\frac{\delta D_{min}}{\delta\lambda} > 0$. □

**Proposition 3.** *Equilibrium with Full Sophistication: (a) Conditional on adopting commitment, individuals will adopt the minimum effective penalty, $D_{min}$. (b) Individuals who are sufficiently time-consistent to save in autarky (those with $\beta \geq \hat{\beta}$, see Proposition 1) never adopt commitment. (c) Individuals who cannot save in autarky (those with $\beta < \hat{\beta}$) adopt commitment if i) $\beta$ is sufficiently high, and ii) the shock frequency rate $\lambda$ is sufficiently low. The adoption decision is summarized in the condition $\lambda D_{min} \leq (1-\lambda)^2(b-p)$, where $\lambda D_{min}$ represents the expected cost of commitment due to rational default, and $(1-\lambda)^2(b-p)$ captures the expected benefit of a successful savings plan. (d) With full sophistication, offering commitment weakly increases welfare (it strictly increases the expected welfare of adopters).*

*Proof.* (a) For any commitment contract with an effective penalty, $D \geq D_{min}(\beta)$, expected utility from period 0's perspective is

$$E(U_0^D) = (1-\lambda)[\underbrace{1-(p-1)}_{c_1}+\underbrace{\lambda(p-1)+(1-\lambda)b}_{c_2}]+\lambda[\underbrace{-D}_{c_1'}+\underbrace{(1-\lambda)}_{c_2'}], \tag{A2}$$

[4] Allowing for multiple periods and limited liability, there is a fourth group of non-adopters: Sophisticates who realize their penalty is not enforceable until a stock of savings has been accumulated. However, accumulating this stock is not incentive-compatible without an enforceable penalty. This is a relevant consideration for the empirical analysis, see Section 6.3.

where $\lambda[-D+(1-\lambda)]$ captures the risk of 'rational default' due to a shock in period 1. Choosing $D > D_{min}$ is costly. Period 1's incentive constraint (equation 1) only depends on whether $D \geq D_{min}(\beta)$, thus choosing the minimum penalty always dominates choosing larger penalties. Choosing $D < D_{min}$ is strictly dominated by choosing no penalty at all, since period 1's incentive constraint is violated, and default occurs with certainty. As a result, the period 0 agent chooses either $D = D_{min}$ or $D = 0$.

(b) For those who are sufficiently time-consistent to save in autarky ($\beta \geq \hat{\beta}$), $D_{min} = 0$. Part (a) then implies that it is never optimal to adopt positive amounts of commitment.

(c) Without commitment, individuals who cannot save in autarky ($\beta < \hat{\beta}$) face an expected utility of $E(U_0^A) = 2(1-\lambda)$. Comparing $E(U_0^A)$ to the expected utility with commitment (equation A2), the period 0 agent prefers to adopt commitment if

$$\lambda D_{min} \leq (1-\lambda)^2(b-p) \tag{A3}$$

From period 0's perspective, the benefit of commitment is the ability to purchase the nondivisible good (absent shocks), $(1-\lambda)^2(b-p)$. A key result is that this benefit does not depend on the time-consistency parameter $\beta$. In contrast, $\beta$ determines the cost of commitment: The expected loss due to 'rational default' is $\lambda D_{min}$, which decreases in $\beta$ by Proposition 2. Perhaps counter-intuitively, commitment is most attractive to those with the *lowest* degree of time-inconsistency (or the highest $\beta$), as the penalty required to enforce the savings plan is small, and poses little risk in the presence of shocks. In consequence, conditional on $\beta < \hat{\beta}$, agents adopt commitment for sufficiently high $\beta$. The connection between commitment adoption and the shock frequency rate $\lambda \in [0,1]$ is straightforward: By Proposition 2, the cost of commitment $\lambda D_{min}$ increases in $\lambda$, while the benefit from commitment $(1-\lambda)^2(b-p)$ decreases in $\lambda$. Commitment is adopted if shocks are sufficiently rare.

(d) Since welfare has been defined as the ex-ante utility of the period 0 planner, $W = U_0 = E[c_1 + c_2]$, the result follows trivially from the fact that a fully sophisticated planner does not make mistakes. Commitment is adopted *iff* it increases welfare, $E(U_0^D) > E(U_0^A)$, which simplifies to equation A3. □

**Proposition 4.** *Equilibrium with Partial Sophistication: (a) Conditional on adopting commitment, individuals who are partially sophisticated about their time-inconsistency will adopt penalties strictly below the required effective minimum, $\tilde{D}_{min} < D_{min}$. As a result, adopters' incentive constraints in period 1 are systematically violated, triggering contract default. (b) Individuals who believe themselves to be sufficiently time-consistent to save in autarky (those with $\tilde{\beta} \geq \hat{\beta}$) never adopt commitment. (c) For those who realize they cannot save in autarky ($\tilde{\beta} < \hat{\beta}$), sophistication negatively predicts commitment adoption. For a given $\beta$, commitment is most attractive to those with the largest amount of naiveté, $\tilde{\beta} - \beta$. (d) With partial sophistication, offering commitment weakly decreases welfare (it strictly decreases the expected welfare of adopters).*

*Proof.* All arguments are analogue to the case of full sophistication, except that the period 0 agent believes the period 1 agent will apply $\tilde{\beta} > \beta$ in making intertemporal choices.

(a) From period 0's perspective, a penalty is perceived to be effective when it satisfies $D \geq D_{min}(\tilde{\beta})$. For ease of notation, denote the perceived minimum effective penalty as $\tilde{D}_{min} \equiv D_{min}(\tilde{\beta})$. $D_{min}$ strictly decreases in $\beta$, thus $\tilde{\beta} > \beta$ implies $\tilde{D}_{min} < D_{min}$. By Proposition 3, conditional on adopting commitment, agents will adopt $\tilde{D}_{min}$. Upon reaching period 1, and realizing one's true value of $\beta$, the incentive constraint (equation 1) is violated. Period 1 prefers to abandon the savings plan and incur the penalty.

(b) Directly follows from Proposition 3(b), noting that $\tilde{D}_{min} = 0$ for $\tilde{\beta} \geq \hat{\beta}$.

(c) Agents compare the *perceived* cost of commitment with the *perceived* benefit, and adopt commitment if $\lambda \tilde{D}_{min} \leq (1-\lambda)^2(b-p)$ (this is a corollary of Proposition 3c). The cost of commitment decreases in $\tilde{\beta}$, while the benefit is invariant to it. Holding factual time-inconsistency $\beta$ fixed, a higher degree of naiveté $\tilde{\beta} - \beta$ implies that a lower penalty is regarded as effective, and thus less is at stake in case of a 'rational default.' Conditional on $\beta$, as well as on the agent's perceived inability to save without commitment ($\tilde{\beta} < \hat{\beta}$), adoption increases with naiveté.

(d) Faced with an ineffective penalty $\tilde{D}_{min} < D_{min}$, the period 1 agent chooses to default, and pays $D$. Thus, offering commitment contracts decreases the ex-ante utility of adopters from $E(U_0^A) = 2(1-\lambda)$ to $E(U_0^D) = 2(1-\lambda) - \tilde{D}_{min}$. Note that naiveté has an ambiguous effect on this welfare loss: A more naive agent believes a lower penalty $\tilde{D}_{min}$ to be effective, and is therefore more likely to adopt commitment, but she also suffers a smaller welfare loss after default. □

## III. Supplementary Figures and Tables

TABLE A1. PERSONAL SAVINGS GOALS

| | All | All (%) | IS adopters | WR adopters |
|---|---|---|---|---|
| Education | 163 | 21.79 | 18 | 21 |
| General Savings/Not specified | 148 | 19.79 | 37 | 21 |
| House/Lot purchase/construction/repair | 106 | 14.17 | 20 | 12 |
| Christmas/Birthday/Fiesta/Baptism | 91 | 12.17 | 12 | 16 |
| Capital for Business | 69 | 9.22 | 9 | 5 |
| Household Item (Appliance/Furniture) | 41 | 5.48 | 5 | 4 |
| TV/DVD Player/Laptop/Cellphone | 33 | 4.41 | 3 | 2 |
| Emergency Buffer | 31 | 4.14 | 1 | 0 |
| Health/Medical | 26 | 3.48 | 3 | 2 |
| Agricultural/Livestock | 19 | 2.54 | 2 | 6 |
| Motorbike/Car/Boat | 17 | 2.27 | 4 | 2 |
| Travel/Vacation | 4 | 0.53 | 0 | 1 |
| Total | 748 | 100 | 114 | 92 |

TABLE A2. IS PRE-ORDER

| 2nd Round IS Pre-Order<br>1st Round IS Status | Yes | No | Total |
|---|---|---|---|
| Successful | 33 | 18 | 51 |
| Default | 18 | 43 | 63 |
| Total | 51 | 63 | 114 |

TABLE A3. PREDICTING SAMPLE ATTRITION

| | Sample size | Control Mean | IS | WR | F-Stat P-value |
|---|---|---|---|---|---|
| Baseline Survey | 913 | – | – | – | |
| Reached for Marketing | 852 | 0.921 | 0.00455 | 0.0395* | 0.16 |
| | | | (0.0217) | (0.0221) | |
| Willing to Make Savings Plan | 748 | 0.838 | -0.0303 | -0.0132 | 0.61 |
| | | | (0.0307) | (0.0351) | |
| Endline Survey | 732 | 0.776 | 0.0224 | 0.0570 | 0.30 |
| | | | (0.0334) | (0.0371) | |
| Endline * Savings Plan | 615 | 0.658 | 0.00731 | 0.0482 | 0.47 |
| | | | (0.0385) | (0.0436) | |

The table reports coefficients from regressing participation in the various samples on assignment to groups IS and WR. All regressions are estimated using the baseline sample of 913 participants. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

## Table A4. Summary Statistics Across Samples

| | Savings Plan Sample | | | | Endline Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | IS | WR | Control | F-stat P-value | IS | WR | Control | F-stat P-value |
| Age (yrs)* | 43.938 | 43.230 | 44.005 | 0.789 | 43.959 | 43.392 | 44.192 | 0.818 |
| | (0.658) | (0.930) | (0.902) | | (0.679) | (0.886) | (0.946) | |
| Female* | 0.940 | 0.941 | 0.948 | 0.939 | 0.945 | 0.958 | 0.938 | 0.684 |
| | (0.012) | (0.017) | (0.016) | | (0.012) | (0.015) | (0.018) | |
| Married | 0.856 | 0.862 | 0.848 | 0.931 | 0.858 | 0.858 | 0.853 | 0.989 |
| | (0.018) | (0.025) | (0.026) | | (0.018) | (0.025) | (0.027) | |
| Weekly HH income (pesos) | 2.955 | 2.434 | 3.241 | 0.042 | 2.747 | 2.423 | 3.146 | 0.086 |
| | (0.145) | (0.186) | (0.308) | | (0.136) | (0.186) | (0.330) | |
| No. of appliances owned | 2.290 | 2.133 | 2.230 | 0.609 | 2.162 | 2.111 | 2.040 | 0.735 |
| | (0.094) | (0.116) | (0.132) | | (0.091) | (0.117) | (0.132) | |
| No. of HH members | 5.125 | 5.191 | 5.581 | 0.058 | 5.142 | 5.311 | 5.644 | 0.045 |
| | (0.109) | (0.159) | (0.171) | | (0.111) | (0.156) | (0.179) | |
| Education (yrs) | 10.602 | 10.529 | 10.479 | 0.924 | 10.377 | 10.386 | 10.665 | 0.657 |
| | (0.180) | (0.261) | (0.271) | | (0.183) | (0.267) | (0.277) | |
| Received real rewards* | 0.523 | 0.553 | 0.539 | 0.789 | 0.504 | 0.521 | 0.537 | 0.768 |
| | (0.026) | (0.036) | (0.036) | | (0.026) | (0.036) | (0.038) | |
| Present Bias* | 0.175 | 0.163 | 0.153 | 0.809 | 0.188 | 0.157 | 0.155 | 0.537 |
| | (0.020) | (0.027) | (0.027) | | (0.021) | (0.027) | (0.028) | |
| Perceived | 2.274 | 2.122 | 2.453 | 0.198 | 2.398 | 2.284 | 2.367 | 0.781 |
| Temptation (range 0-10) | (0.093) | (0.123) | (0.135) | | (0.099) | (0.124) | (0.134) | |
| Impatience | 0.321 | 0.391 | 0.333 | 0.255 | 0.327 | 0.416 | 0.339 | 0.109 |
| | (0.025) | (0.036) | (0.035) | | (0.025) | (0.036) | (0.037) | |
| Faces strong financial | 0.409 | 0.396 | 0.382 | 0.829 | 0.399 | 0.386 | 0.407 | 0.919 |
| claims from others* | (0.026) | (0.036) | (0.035) | | (0.026) | (0.036) | (0.037) | |
| Risk aversion (range 0-6) | 4.241 | 4.654 | 4.152 | 0.023 | 4.211 | 4.653 | 4.113 | 0.014 |
| | (0.104) | (0.132) | (0.140) | | (0.106) | (0.133) | (0.146) | |
| Cognitive ability | 2.957 | 2.878 | 2.911 | 0.795 | 2.929 | 2.842 | 2.977 | 0.606 |
| (range 0-5) | (0.065) | (0.098) | (0.110) | | (0.066) | (0.099) | (0.105) | |
| Financial literacy | 1.864 | 1.851 | 1.853 | 0.986 | 1.860 | 1.853 | 1.910 | 0.830 |
| (range 0-5) | (0.051) | (0.075) | (0.077) | | (0.052) | (0.073) | (0.074) | |
| HH Bargaining Power (0-5) | 2.648 | 2.543 | 2.634 | 0.811 | 2.633 | 2.574 | 2.627 | 0.934 |
| | (0.097) | (0.134) | (0.133) | | (0.097) | (0.133) | (0.138) | |
| Distance to Bank (km) | 1.351 | 1.383 | 1.343 | 0.923 | 1.357 | 1.306 | 1.287 | 0.709 |
| | (0.052) | (0.087) | (0.077) | | (0.052) | (0.078) | (0.070) | |
| Existing Savings Account | 0.477 | 0.468 | 0.455 | 0.890 | 0.444 | 0.474 | 0.407 | 0.436 |
| | (0.026) | (0.036) | (0.036) | | (0.026) | (0.036) | (0.037) | |
| Donated to charity | 0.358 | 0.383 | 0.435 | 0.208 | 0.386 | 0.368 | 0.424 | 0.540 |
| in the last 12 months | (0.025) | (0.036) | (0.036) | | (0.026) | (0.035) | (0.037) | |
| #Emergencies last yr | 0.417 | 0.388 | 0.429 | 0.844 | 0.419 | 0.384 | 0.429 | 0.807 |
| | (0.038) | (0.051) | (0.051) | | (0.038) | (0.049) | (0.054) | |
| Global Signif. Test (P-value) | 0.96 | 0.23 | 0.56 | | 0.94 | 0.34 | 0.20 | |
| Observations | 369 | 188 | 191 | | 365 | 190 | 177 | |

Note: A starred variable indicates that the randomisation was stratified on this variable. Variables are as described in Table 1. The "Global Significance Test" at the bottom of the table is a test of joint nullity of coefficients in a regression of treatment assignment on the set of covariates.

***Heterogeneous Treatment Effects*** Table A5 examines treatment effect heterogeneity across a number of dimensions of interest. As in Table 3, column (1), the change in savings held at the partner bank is regressed on indicators for assignment to the treatment groups. In addition, the indicator for the Installment Savings group is interacted with variables which have been shown to predict take-up or default, or which are of interest in themselves.

Heterogeneity in treatment effects is most pronounced for existing savings account holders: In response to being offered the Installment Savings product, they increased their savings by 295 pesos more than those without an existing account. This seems particularly surprising in light of the fact that, in *absence* of the Installment Savings treatment, existing account holders saved only 75 pesos more than those without existing accounts. The evidence suggests that existing account holders were not necessarily active savers before the intervention, but felt strongly motivated by the Installment Savings treatment. A possible explanation relates to mistrust and negative preconceptions towards banks, which were common in the population.[5] Existing account holders were more likely to be familiar with basic bank transactions, and more trusting of the banking system as a whole.

Treatment effects appear to be relatively uniform across measures of present bias and sophistication (column (1)). All interaction terms are statistically insignificant. Taking into account the composition effects inherent in ITT estimates, the interaction coefficients are consistent with theoretical predictions: Consider a present-biased agent with a low level of sophistication. As the previous subsections have shown, such agents are likely to adopt commitment and subsequently default. The net effect of commitment on savings (*IS*Present Bias*) should be zero, or negative after accounting for default penalties. As sophistication increases, the agent becomes more likely to choose an incentive-compatible contract, and to successfully complete her savings plan. However, sophisticated agents are also less likely to select into commitment (as suggested theoretically in Section 2 and empirically in Table 4). Hence, the effect of offering commitment on sophisticated agents' savings levels (*IS * Soph. Present Bias*) will be dampened by composition effects. While theory can explain small or zero coefficients for these interactions, it is an open question why the baseline treatment effect, *Installment Savings (IS)*, remains so high after controlling for time preferences. In column (1), the omitted category is time-consistency, suggesting that even time-consistent agents derived a large benefit from the IS product.

Columns (2), (4), and (5) show that the estimated treatment effect is also relatively uniform across levels of cognitive ability, household income, and household bargaining power.

[5] It was a common belief that banks were "not for poor people". In addition, some individuals believed that savings deposited at a bank would likely be lost if the bank became insolvent. Deposit insurance does exist in the Philippines, but may be associated with years of waiting time. See e.g., Dupas et al. (2012) on trust-related challenges in banking the poor.

TABLE A5. HETEROGENEOUS TREATMENT EFFECTS: BANK SAVINGS

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Installment Savings (IS) | 493.212*** | 307.896** | 287.405*** | 480.978*** | 304.154*** |
| | (141.478) | (154.863) | (63.639) | (89.539) | (101.791) |
| Withdrawal Rest. (W) | 129.494*** | 148.412*** | 147.299*** | 154.503*** | 148.877*** |
| | (40.627) | (40.991) | (40.733) | (42.628) | (41.431) |
| IS * Present Bias | -283.293 | | | | |
| | (226.639) | | | | |
| Present Bias | 57.671 | | | | |
| | (83.916) | | | | |
| IS * Soph. Present Bias | -12.666 | | | | |
| (IS*Pres.Bias*Temptation) | (55.949) | | | | |
| Soph. Present Bias | -6.042 | | | | |
| | (18.188) | | | | |
| IS * Perceived Temptation | -4.394 | | | | |
| | (43.274) | | | | |
| Perceived Temptation | -4.940 | | | | |
| | (9.286) | | | | |
| IS * Cognitive Ability | | 40.803 | | | |
| | | (52.951) | | | |
| Cognitive Ability | | 5.085 | | | |
| | | (8.575) | | | |
| IS * Ex. Savings Account | | | 294.932** | | |
| | | | (138.703) | | |
| Existing Savings Account | | | 74.996* | | |
| | | | (41.358) | | |
| IS * Weekly HH income | | | | -17.131 | |
| | | | | (18.438) | |
| Weekly HH income (1000 pesos) | | | | 7.611 | |
| | | | | (5.483) | |
| IS * HH bargaining power | | | | | 47.072 |
| | | | | | (36.388) |
| HH bargaining power | | | | | 6.966 |
| | | | | | (10.387) |
| Constant | 34.948 | 12.3578 | -7.0002 | 2.7796 | 8.814 |
| | (26.711) | (24.8741) | (18.9203) | (17.7093) | (29.905) |
| $R^2$ | 0.05 | 0.04 | 0.06 | 0.04 | 0.05 |
| Observations | 718 | 746 | 746 | 743 | 746 |

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Variables are as described in Table A4.

TABLE A6. QUANTILE REGRESSIONS

| | | (1) Bank Savings | (2) Other Savings | (3) Change in Outstanding Loans |
|---|---|---|---|---|
| 10th Percentile | Installment Savings | 0.00 | 252.00 | -4,000.00* |
| | | (0.00) | (2,353.66) | (2,282.56) |
| | Withdrawal Restr. | 0.00 | -148.00 | -345.00 |
| | | (0.00) | (2,670.35) | (2,598.30) |
| 20th Percentile | Installment Savings | 0.00 | -271.00 | -2,000.00* |
| | | (0.00) | (630.63) | (1,021.07) |
| | Withdrawal Restr. | 0.00 | -1,071.00 | -1,000.00 |
| | | (0.00) | (715.48) | (1,162.30) |
| 30th Percentile | Installment Savings | 0.00 | -150.00 | -800.01** |
| | | (0.00) | (261.67) | (394.72) |
| | Withdrawal Restr. | 0.00 | -240.00 | -700.00 |
| | | (0.00) | (296.88) | (449.32) |
| 40th Percentile | Installment Savings | 0.00 | 0.00 | 0.00 |
| | | (5.45) | (53.89) | (129.39) |
| | Withdrawal Restr. | 0.00 | 0.00 | 0.00 |
| | | (6.29) | (61.15) | (147.28) |
| 50th Percentile | Installment Savings | 0.00 | 0.00 | 0.00 |
| | | (5.23) | (97.89) | (41.80) |
| | Withdrawal Restr. | 100.00*** | 56.67 | 0.00 |
| | | (6.03) | (111.06) | (47.58) |
| 60th Percentile | Installment Savings | 0.00 | 85.00 | 50.00 |
| | | (0.00) | (229.72) | (261.24) |
| | Withdrawal Restr. | 100.00*** | -135.00 | -100.00 |
| | | (0.00) | (260.62) | (297.38) |
| 70th Percentile | Installment Savings | 0.00 | 110.00 | -234.00 |
| | | (17.91) | (389.19) | (711.40) |
| | Withdrawal Restr. | 100.00*** | -343.44 | -800.00 |
| | | (20.64) | (441.56) | (809.80) |
| 80th Percentile | Installment Savings | 200.00 | -208.00 | 840.00 |
| | | (181.42) | (587.84) | (1,226.00) |
| | Withdrawal Restr. | 150.00 | -865.96 | 340.00 |
| | | (209.10) | (666.93) | (1,395.59) |
| 90th Percentile | Installment Savings | 2,051.87*** | -635.00 | 925.00 |
| | | (329.68) | (1,290.76) | (3,737.72) |
| | Withdrawal Restr. | 280.00 | -1,050.00 | -489.00 |
| | | (379.97) | (1,464.43) | (4,254.74) |
| Observations | | 748 | 603 | 720 |

Estimated standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Survey-based data (columns (2) and (3)) is truncated at 1 percent. All reported coefficients are Intent-to-Treat effects.

***Quantile Regressions*** Table A6 presents quantile treatment effects on savings and outstanding loans. The effect of offering the Installment Savings product on total bank savings is not apparent until the 90th percentile. This is consistent with a large effect on the 51 IS clients who successfully completed their contract, and a zero effect on non-adopters. The IS product was offered to 423 individuals, of whom 114 adopted the product. The 63 IS clients who defaulted largely achieved a zero change in savings: Most of them stopped depositing soon after opening their account (see Figure 2), and their opening balance was consumed by the default penalty.

The effect of offering the Withdrawal Restriction product on bank savings is 100 pesos at the median. This is likely the mechanical result of a 42 percent take-up rate and a 100 pesos minimum opening balance. In contrast to IS clients, those WR clients who stopped depositing after their opening balance (79 percent) did not lose their savings to a default penalty, but their savings remain frozen in their account (up to a goal date or amount, see Section 6.2).

The regressions in columns (2) and (3) are based on survey responses on individuals' outstanding loan balance, as well as on savings at home and at other banks. While there is a large amount of noise in the survey data, there is no systematic evidence of a substitution from other sources of savings into savings at the partner bank. However, offering the Installment Savings product may have facilitated the biggest reductions in loan demand (at 10th, 20th, and 30th percentile).


FIGURE A1. DISTRIBUTIONAL EFFECT OF TREATMENT ON SAVINGS, LOANS AND EXPENDITURES

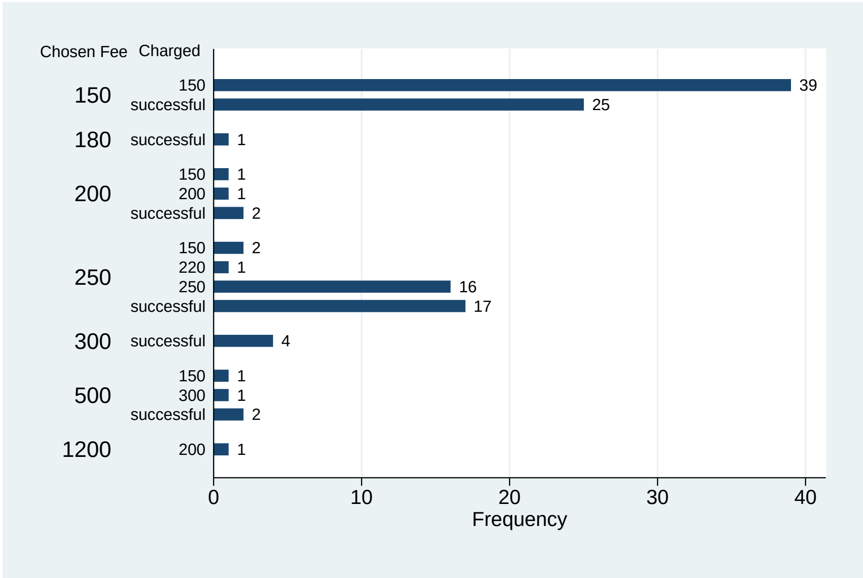


FIGURE A2. TERMINATION FEES (CHOSEN & CHARGED)

Figure A2 displays the termination fees (default penalties) that were charged to defaulting clients. They are grouped by the fee level chosen at contract signing. By construction, $charged \leq chosen$. No fee was charged to successful clients. Where $charged < chosen$, the chosen fee was not enforceable (see Section 3).

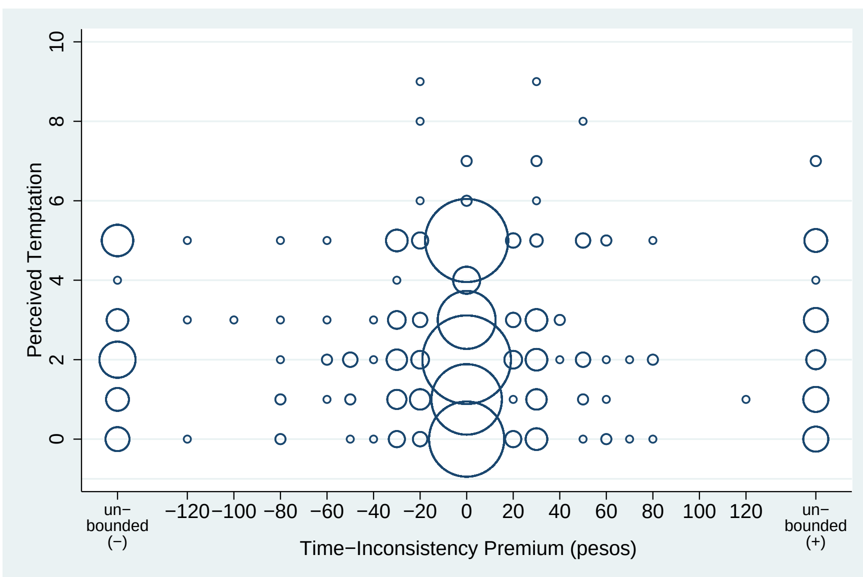


FIGURE A3. BIVARIATE FREQUENCY: PERCEIVED VS. OBSERVED TIME-INCONSISTENCY

*Time-Inconsistency Premium* is the difference between the switching points for current vs. future trade-offs in MPLs, i.e., it is the premium the individual requires to wait one month starting today, as opposed to waiting one month in one month. Positive premiums indicate $\beta < 1$, and switched on the *Present Bias* indicator. *Unbounded(+)* means the switching point ($t$ vs. $t+1$) was above the observed range, while the switching point ($t+1$ vs. $t+2$) was within the observed range, thus the premium is greater than 120 and unobserved. Vice versa for *unbounded(-)*. *Perceived Temptation* is the $tempted - ideal$ measure from Ameriks et al. (2007), and refers to the amount of extra restaurant vouchers (out of 10) the respondent would be tempted to consume in year 1, in excess of her stated ideal allocation. The proposed sophistication measure is $tempted - ideal * presentbias$. Note that *present bias* rather than *Time-Inconsistency Premium* is used to avoid strong functional form assumptions.

TABLE A7. EVIDENCE OF INCOME OPTIMISM

| | Not Present-Biased | Present-biased | All | T-stat P-value |
|---|---|---|---|---|
| Prediction Gap (growth) | 3.290378 | 3.677686 | 3.357041 | 0.81 |
| | (0.6976) | (1.2298) | (0.6146) | |
| Prediction Gap (level) | 1.269759 | -2.22314 | 0.6685633 | 0.22 |
| | (1.1704) | (2.6292) | (1.0698) | |
| Observations | 582 | 121 | 703 | |

| | No Take-Up | Take-Up | All | T-stat P-value |
|---|---|---|---|---|
| Prediction Gap (growth) | 1.738007 | 5.043011 | 2.582418 | 0.08 |
| | (0.9255) | (1.8137) | (0.8325) | |
| Prediction Gap (level) | 1.140221 | -2.569892 | 0.1923077 | 0.30 |
| | (1.8676) | (2.6941) | (1.552) | |
| Observations | 271 | 93 | 364 | |

| | Successful | Default | All | T-stat P-value |
|---|---|---|---|---|
| Prediction Gap (growth) | 4.227273 | 5.77551 | 5.043011 | 0.67 |
| | (2.6477) | (2.5106) | (1.8137) | |
| Prediction Gap (level) | -5.318182 | -0.1020408 | -2.569892 | 0.34 |
| | (3.7117) | (3.8799) | (2.6941) | |
| Observations | 44 | 49 | 93 | |

Standard deviations in parentheses. All numbers are group averages.

***Evidence of Income Optimism*** Table A7 presents group averages of income prediction gaps across three dimensions: The observed measure of present bias, take-up of the Installment Savings product, and default on IS. Prediction gaps are measured as follows: During the baseline survey in September and October 2012, individuals were asked to predict their average weekly household income for each month from October 2012 to March 2013. To make this task easier, individuals chose one of 31 income brackets, numbered from 1 for '0-50 pesos per week' to 31 for 'more than 10,000 pesos per week'. Six months later, in late March and April 2013, this exercise was repeated during the endline survey, except that individuals now stated their realised weekly income for the same time period. Two measures of optimism (or bad luck) are obtained: $PredictionGap(growth)_i$ is the difference between predicted income growth and realised income growth, where growth is measured as $Growth_i = \sum_{m=Nov}^{Mar}(bracket_m - bracket_{October})$. In other words, income growth is proxied by the sum of deviations from October income, in units of income brackets. This approach is conservative, in the sense that it is robust to individuals using different income benchmarks for their October income in baseline and endline survey. An alternative measure of optimism is $PredictionGap(level)_i$, obtained by the simple summed difference between predicted and realised income levels, $PredictionGap(level)_i = \sum_{m=Oct}^{Mar}(bracket_m^{pred} - bracket_m^{real})$. Consistent with noise in benchmark income levels, $PredictionGap(level)_i$ exhibits more variation than $PredictionGap(growth)_i$. Note that these measures cannot be included as covariates in take-up or default regressions – both because they are not meaningful on an individual level, and because they use data from the endline survey, and may thus not be orthogonal to treatment. The sample for Table A7 are those individuals who participated in both the baseline and endline survey.

## IV. Robustness Checks

This section tests robustness along several dimensions. Table A8 verifies that the estimation of average treatment effects is robust to the inclusion of unbalanced covariates (see Table I). Table A9 tests robustness of the take-up and default regressions of Section 6.3 with respect to the measurement of sophistication. As outlined in Section 4, sophistication is measured by interacting observed time-inconsistency (in MPLs) with a measure of *perceived* time-inconsistency. Instead of the previously used *Perceived Temptation* variable, Columns (4) and (8) of Table A9 use *Perceived Self-Control* to capture perception of time-inconsistency (both measures are discussed in Section 4). Note that 316 out of 402 (79 percent) individuals in the IS-sample report zero (or in 13 cases, negative) values of *Perceived Self-Control*. Interacted with the observed measure of present bias, this implies that only 21 out of 402 values of *Pres.Bias*Self-Control* are non-zero. While the relationship with take-up is not significant (likely due to a lack of variation), the coefficient on *Pres.Bias*Self-Control* is roughly comparable in magnitude and sign to the coefficient on *Pres.Bias*Temptation.*

Table A10 looks at the effect of using real incentives instead of hypothetical questions in the measurement of time-inconsistency. Section 4 outlines the multiple price list method which was used to elicit individuals' time preferences. The elicitation was first conducted with the entire sample using hypothetical questions. Towards the end of the survey (approximately 30min later), the elicitation was repeated for a randomly chosen half of the sample with real monetary rewards (Appendix V describes the randomisation). During the hypothetical round, individuals were not aware of the upcoming real-rewards round.

The regressions in the main text use the incentivised measures where obtained (468 of 913 individuals, equivalent to 230 of 457 in group IS), and rely on measures from the hypothetical round otherwise. Columns (2) and (5) of Table A10 exploit the fact that 'hypothetical measures' are available for the whole sample, and re-run the IS take-up and default estimations (treatment group IS) from Section 6.3 using only unincentivised measures of present bias and impatience. In contrast, Columns (3) and (6) restrict the sample to those who received real rewards, and rely only on incentivised measures. Table A10 indicates that the main results of this paper appear to be driven by the incentivised measures of time-inconsistency: The estimated effects in the real-rewards sample are highly significant despite the smaller sample size, while the coefficients for unincentivised measures of present bias (Columns (2) and (5)) are close to zero.

This raises an obvious question: What is the effect of real monetary incentives in the measurement of time preferences? Unfortunately, the effect is not identified in this study due to the possibility of learning effects between the hypothetical and the real-rewards round. However, Table A11 provides some preliminary evidence, under the assumption of no learning effects: The between-individual analysis is a simple cross-section regression of time-preference outcomes (incentivised where obtained, otherwise hypothetical) on whether or not the individual received monetary incentives. The within-individual analysis is restricted to the real-rewards sample, and uses two observations per individual: One to capture her time preferences using hypothetical questions, and one under monetary rewards. To illustrate, the estimated equation for present bias is $presentbias_{it} = \alpha + \beta * real_{it} + \mu_i + \varepsilon_{it}$, where $\mu_i$ is assumed to be random. The results suggest that monetary incentives may *decrease* the occurrence of time-inconsistency: Individuals were less likely to exhibit either present bias or future bias (although only the latter effect is significant), but developed more general impatience. The between-individual analysis confirms the sign of this effect (less time-inconsistency, more impatience), but remains statistically insignificant. In combination with

TABLE A8. AVERAGE TREATMENT EFFECTS - ROBUSTNESS TO UNBALANCED COVARIATES

| Dependent | Bank Savings | | Other Savings | | Change in Outstanding Loans | |
|---|---|---|---|---|---|---|
| Variable | (1) | (2) | (3) | (4) | (5) | (6) |
| Installment Savings | 428.633*** | 428.165*** | 426.811 | 407.846 | -840.258 | -753.549 |
| Treatment | (65.587) | (67.194) | (671.844) | (712.047) | (1,180.168) | (1,249.690) |
| Withdrawal Restr. | 148.243*** | 118.065*** | -328.159 | -499.315 | -308.549 | -632.082 |
| Treatment | (40.927) | (42.534) | (705.461) | (737.198) | (1,258.139) | (1,325.386) |
| Impatience | | -12.358 | | 772.746 | | 1,581.731 |
| | | (73.337) | | (546.751) | | (1,059.058) |
| Risk Aversion | | 32.748* | | -93.730 | | 335.040 |
| | | (17.497) | | (137.739) | | (237.874) |
| Weekly HH Income | | 0.620 | | -348.695* | | -65.010 |
| | | (7.277) | | (198.965) | | (246.792) |
| Constant | 27.160*** | -104.059 | 63.451 | 1,263.095 | 1,882.729** | 190.829 |
| | (9.399) | (73.736) | (531.028) | (969.466) | (920.828) | (1,437.423) |
| $R^2$ | 0.04 | 0.04 | 0.00 | 0.03 | 0.00 | 0.01 |
| Observations | 746 | 717 | 603 | 577 | 720 | 688 |

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Control variables are defined as in Table I. All coefficients measure Intent-to-Treat (ITT) effects, i.e., the effect of being offered the commitment product. Columns (1)-(2) use administrative data from the partner bank. Columns (3)-(6) use survey-based data. The dependent variable in columns (3)-(4) is the change in reported savings at home and at other banks (other than the partner bank) between the baseline survey and the personal savings goal date (3-6 months later). The dependent variable in columns (5)-(6) is the change in the reported outstanding loan balance between the baseline survey and the endline survey, 6 months later. Both loan and survey-based savings data are truncated at 1 percent.

TABLE A9. INSTALLMENT SAVINGS TAKE-UP & DEFAULT: ROBUSTNESS (SOPHISTICATION MEASURE)

| Dependent | IS Take-Up | | | | IS Default (within all assigned to IS group) | | | |
|---|---|---|---|---|---|---|---|---|
| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Present Bias | 0.0827 | -0.0405 | 0.0623 | 0.0000 | 0.1119* | 0.0281 | 0.0933 | 0.0749 |
| | (0.0864) | (0.0613) | (0.0825) | (0.0685) | (0.0654) | (0.0451) | (0.0616) | (0.0510) |
| Soph. Present Bias | -0.0631** | | -0.0570** | | -0.0453** | | -0.0371 | |
| (Pres.Bias*Temptation) | (0.0292) | | (0.0288) | | (0.0230) | | (0.0237) | |
| Perceived Temptation | -0.0046 | -0.0137 | -0.0095 | | -0.0202* | -0.0274*** | -0.0179* | |
| | (0.0125) | (0.0115) | (0.0123) | | (0.0105) | (0.0097) | (0.0105) | |
| Pres.Bias*Self-Control | | | | -0.0394 | | | | -0.0651* |
| | | | | (0.0391) | | | | (0.0375) |
| Perceived Self-Control | | | | -0.0273 | | | | -0.0211 |
| | | | | (0.0216) | | | | (0.0182) |
| Full Controls | YES | YES | NO | YES | YES | YES | NO | YES |
| Marketer FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 402 | 402 | 408 | 402 | 402 | 402 | 408 | 402 |

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Entries in the table represent the marginal coefficients of the corresponding probit regression. In columns (3) and (7), the set of control variables has been limited to age and gender (all other control variables may directly or indirectly represent choice variables). Columns (2) and (6) omit the interaction term $tempted - ideal * presentbias$ ("Pres.Bias*Temptation"), which is used as a measure of sophisticated time-inconsistency. Columns (4) and (8) instead use $expected - ideal * presentbias$ ("Pres.Bias*Self-Control") as an alternative measure of sophistication. See Section 4 for a detailed description of the sophistication measures.

TABLE A10. IS TAKE-UP & DEFAULT: ROBUSTNESS (REAL VS. HYPOTHETICAL INCENTIVES)

| Dependent Variable | IS Take-Up | | | IS Default (within all assigned to IS group) | | |
|---|---|---|---|---|---|---|
| | (1) Aggregate | (2) Hypothetical Questions | (3) Real Incentives | (4) Aggregate | (5) Hypothetical Questions | (6) Real Incentives |
| Present Bias | 0.0827 | -0.0508 | 0.3203** | 0.1119* | -0.0191 | 0.3383*** |
| | (0.0864) | (0.0820) | (0.1417) | (0.0654) | (0.0595) | (0.1095) |
| Soph. Present Bias | -0.0631** | 0.0005 | -0.1937*** | -0.0453** | 0.0025 | -0.1517*** |
| (Pres.Bias*Temptation) | (0.0292) | (0.0276) | (0.0558) | (0.0230) | (0.0175) | (0.0511) |
| Perceived Temptation | -0.0046 | -0.0149 | 0.0058 | -0.0202* | -0.0294*** | -0.0266* |
| | (0.0125) | (0.0130) | (0.0178) | (0.0105) | (0.0112) | (0.0158) |
| Impatience | -0.0008 | -0.0213 | 0.0487 | -0.0030 | -0.0247 | 0.0142 |
| | (0.0464) | (0.0474) | (0.0681) | (0.0372) | (0.0387) | (0.0554) |
| Full Controls | YES | YES | YES | YES | YES | YES |
| Marketer FE | YES | YES | YES | YES | YES | YES |
| Observations | 402 | 401 | 199 | 402 | 401 | 199 |

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Entries in the table represent the marginal coefficients of the corresponding probit regression. Variables are as defined in Table I. Columns (2) and (5) regress the dependent variable on unincentivised measures of present bias and impatience for the entire sample. Columns (3) and (6) restrict the analysis to the real-rewards sample, and use only incentivised measures of present bias and impatience. Columns (1) and (4) use incentivised measures where available, and hypothetical measures otherwise. They are identical to Columns (1) and (2) of Table IV.

the strong predictive power for commitment take-up and default observed in Table A10, these results are consistent with the idea that incentivising survey questions reduces noise and improves the quality of the answers.

TABLE A11. REAL VS. HYPOTHETICAL INCENTIVES

A. BETWEEN-INDIVIDUAL COMPARISON (CROSS SECTION)

| Dependent Variable | Present Bias | Future Bias | Impatience |
|---|---|---|---|
| Real Incentives | -0.0264 | -0.0117 | 0.0253 |
| | (0.0250) | (0.0264) | (0.0323) |
| Mean Dep. Variable | 0.166 | 0.189 | 0.357 |
| Observations | 882 | 882 | 882 |

B. WITHIN-INDIVIDUAL COMPARISON (PANEL DATA)

| Dependent Variable | Present Bias | Future Bias | Impatience |
|---|---|---|---|
| Real Incentives | -0.0825 | -0.3049*** | 0.5086*** |
| | (0.1127) | (0.1064) | (0.1452) |
| Mean Dep. Variable | 0.161 | 0.219 | 0.337 |
| Individuals | 462 | 462 | 462 |
| Observations | 903 | 903 | 903 |

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Entries in the tables represent the marginal coefficients of probit regressions. The dependent variable in Table A is incentivised for a random half of the sample, and hypothetical otherwise. Table B restricts the sample to those individuals who received real incentives, and uses a panel structure with 'real vs. hypothetical incentives' as the time dimension (thus, $T = 2$).

## V. Survey Measurement and Marketing Material

The ad-hoc randomization to determine who would receive real rewards for the time-preference questions was implemented as follows: At the start of the survey, enumerators verified respondents' ID as a part of the screening process. Enumerators then performed a calculation based on an individual's birth day, month and year. If the calculated number was odd, the respondent received a survey containing questions with real rewards. If the calculated number was even, the survey was administered with hypothetical questions.[6] Individuals were not informed about this randomisation when starting the survey, but the nature of rewards was transparent at the time of asking the questions. Serious consideration was given to the possibility of an uncertainty bias: In the presence of uncertainty about whether they would receive a promised future payment, even time-consistent agents would have an incentive to always pick the immediate reward. Choices in the future time frame would be unaffected, resulting in an upward bias on the present bias measure. To assure individuals that all payments were guaranteed, both cash and official post-dated bank cheques were presented during the game.

In addition to the measures for present bias and sophistication, the baseline survey obtained measures of other covariates of interest: A measure of the strength of financial claims from others is obtained using a methodology similar to that in Johnson et al. (2002): Individuals were presented with a hypothetical scenario in which they keep 3000 pesos in their house, set aside for a particular expenditure that is due in one month. If the people around them knew about this money, how many would ask for assistance, and how much would they ask? This hypothetical framing avoids the endogeneity inherent in asking respondents directly about actual transfers made to others (actual transfers were also observed, but not used in the analysis). The *Financial Claims* variable used in this paper is an indicator for individuals who reported to face above-median claims from others (the median was 500 pesos, which was also the mode). *Risk Aversion* is a score in [1,6], and represents the individual's choice when faced with a set of lottery options with increasing expected value and increasing variance (see Figure A4). Choosing the risk-free lottery A yielded a score of 6, for extreme risk aversion (this option was chosen by 48 percent of the sample). *Cognitive Ability* is proxied by the number of correct answers (out of five) to a brief culture-free intelligence test using Raven's matrices (see Figure A5 for a sample). A *Financial Literacy* score is given by the number of correct answers (again, out of five) to basic numeracy questions. *Household Bargaining Power* is measured as follows: Individuals were asked who was the main decision-maker for five types of household expenses (market purchases, durable goods, transfers to others, personal recreation, and schooling of children). For each type of expense at their discretion, their bargaining score increased by one, resulting in a measure with a range [0,5]. 94 percent of respondents were female; thus the variable measures predominantly female bargaining power. *Distance to the Bank* is measured as the linear geographic distance to the partner bank branch, obtained using GPS coordinates. An *Existing Savings Account* indicates that the individual reported to have an existing savings or checking account at any bank (not necessarily the partner bank) at the time of the baseline survey. *Donated to Charity* is a dummy that switches on if the individual reported to have given any positive amount of money to charity in the past 12 months. It is a proxy for the individual's attitude towards charitable giving, motivated by the fact that the IS default penalty was framed as a charitable contribution. 'Charity buckets' are common even in low-income areas of the Philippines, especially for disaster relief and the Red Cross. While charitable giving is unsur-

[6] The calculation was designed to give an odd number if the individual's birth year was odd, and even otherwise. The survey team was unaware of this connection. Given the availability of verified IDs which included birthdays, it was possible to check ex-post that the correct type of survey had been administered.


FIGURE A4. TEST OF RISK AVERSION (METHODOLOGY: BINSWANGER (1980))

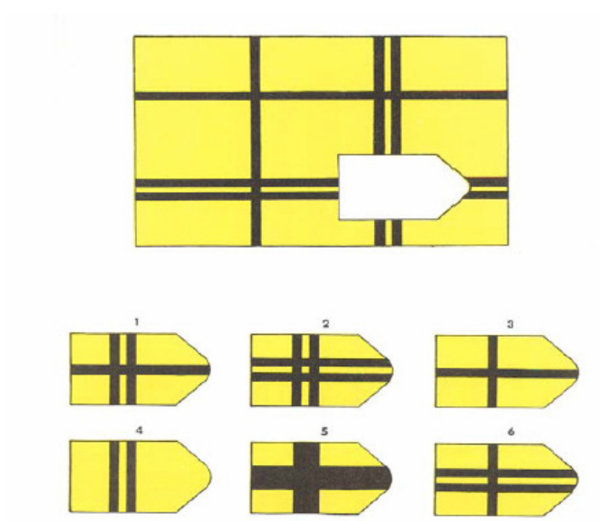


FIGURE A5. ILLUSTRATION: TEST OF COGNITIVE ABILITY

# Personal Savings Plan

1st VALLEY BANK
A RURAL BANK

Name: __*Sample*__________________

Address: *Gingoog City, Mindanao* ___

Purpose of Savings: _*Tuition Fees*_______

Goal Date: ____*4 July*________________

Goal Amount: ____*5000PhP*___________

| wk | Date Due | Deposit Due | Date of Deposit | Deposit made? (tick!) |
|---|---|---|---|---|
| 1 | 22 Feb | 250 | | |
| 2 | 29 Feb | 250 | | |
| 3 | 07 Mar | 250 | | |
| 4 | 14 Mar | 250 | | |
| 5 | 21 Mar | 250 | | |
| 6 | 28 Mar | 250 | | |
| 7 | 04 Apr | 250 | | |
| 8 | 11 Apr | 250 | | |
| 9 | 18 Apr | 250 | | |
| 10 | 25 Apr | 250 | | |

| wk | Date Due | Deposit Due | Date of Deposit | Deposit made? (tick!) |
|---|---|---|---|---|
| 11 | 2 May | 250 | | |
| 12 | 9 May | 250 | | |
| 13 | 16 May | 250 | | |
| 14 | 23 May | 250 | | |
| 15 | 30 May | 250 | | |
| 16 | 6 Jun | 250 | | |
| 17 | 13 Jun | 250 | | |
| 18 | 20 Jun | 250 | | |
| 19 | 27 Jun | 250 | | |
| 20 | 4 Jul | 250 | | |

FIGURE A6. PERSONAL SAVINGS PLAN (ALL TREATMENT GROUPS) prisingly related to income, 40 percent of the population reported positive contributions, many as small as five pesos (the median was 100 pesos, conditional on giving). Finally, *#Emergencies last year* and *#Emergencies since baseline* proxy the shock arrival rate before and during the study period. They are measured as the number of unexpected emergencies (such as death or illness of a household member, redundancy, natural disasters, damage to house and crops, theft, and a flexible 'other' category) that a household suffered in the last 12 months before the start of the treatment (*#Emergencies last year*), respectively, during the six-month observation period (*#Emergencies since baseline*).

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