ONLINE APPENDIX

"When Commitment Fails -

Evidence from a Field Experiment"

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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 $\hat{\beta}$ 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 β 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 maximizing 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 β (or lower), default is deterministic, and all predictions of the simple model with $\hat{\beta} > \beta$ hold. Formally, $F_{\hat{\beta}}(\beta) = 0$ implies $G_{D_{min}}(D^*) = 0$. Alternatively, if agents assign some probability θ to their true β , 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 β 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

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

to a point where she no longer learns. However, this argument cannot rationalize perpetual overoptimism, captured in the assumption that $\hat{\beta} > \beta$: 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 $\beta \ge \beta_{NC}$ is observed. Nevertheless, not having first-hand experience with commitment, knowing that $\beta < \beta_{NC}$ 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 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

²Admittedly, 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.

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 ($\hat{\beta} \ge \beta$), it easily accommodates pessimistic beliefs ($\hat{\beta} < \beta$). 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 $\hat{\beta}$), commitment becomes very unattractive: As outlined in Propositions 3 and 4, the cost of commitment $\lambda \hat{D}_{min}$ decreases in $\hat{\beta}$ 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 $\hat{D}_{min} > D_{min}$. Excessive penalties are costless absent shocks, but they increase the damage in the case of 'rational default.' Summarizing, 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 ($\hat{\beta} < \beta$): They will be pooled with timeconsistent and sophisticated time-inconsistent agents at all times. The 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 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 β above a threshold β_{NC} . The threshold β_{NC} increases in the shock frequency λ 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

$$\underbrace{1-(p-1)}_{c_1} + \beta \underbrace{[\lambda(p-1)+(1-\lambda)b]}_{c_2} \ge \underbrace{1}_{c'_1} + \beta \underbrace{(1-\lambda)}_{c'_2}$$
(A1)

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

⁴Allowing 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 5.4.

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

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

It is easy to see that $\frac{\partial \beta_{NC}}{\partial \lambda} > 0$, $\frac{\partial \beta_{NC}}{\partial p} > 0$, and $\frac{\partial \beta_{NC}}{\partial b} < 0$.

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

Proof. The result directly follows from equation 5, noting that $\frac{\partial D_{min}}{\partial \beta} < 0$ and $\frac{\partial D}{\partial \lambda} > 0$.

Proposition 3. Equilibrium with Full Sophistication: (a) Conditional on adopting commitment, individuals will adopt the minimum effective penalty, D_{min} . (b) Commitment is adopted in an intermediate range $\beta \in [\beta_{min}, \beta_{NC})$: Individuals who are sufficiently time-consistent to save in autarky ($\beta \ge \beta_{NC}$) never adopt commitment. At very high levels of time-inconsistency ($\beta < \beta_{min}$), the minimum effective penalty is prohibitively high for adopting commitment. The adoption decision is summarized in the condition $\lambda D_{min} \le (1-\lambda)^2(b-p)$, where λD_{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. (c) 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, $D \ge D_{min}(\beta)$, expected utility from period 0's perspective is

$$E(U_0^D) = (1 - \lambda)[\underbrace{1 - (p - 1)}_{c_1} + \underbrace{\lambda(p - 1) + (1 - \lambda)b}_{c_2}] + \lambda[\underbrace{-D}_{c'_1} + \underbrace{(1 - \lambda)}_{c'_2}],$$
(A2)

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 4) only depends on whether $D \ge D_{min}(\beta)$, 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 ($\beta \ge \beta_{NC}$), $D_{min} = 0$. Part (a) then implies that it is never optimal to adopt positive amounts of commitment. In contrast, individuals who cannot save in autarky ($\beta < \beta_{NC}$) face an expected utility of $E(U_0^A) = 2(1 - \lambda)$ absent commitment. Comparing $E(U_0^A)$ to the expected utility with commitment (equation A2), the period 0 agent prefers to adopt commitment if

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

Plugging D_{min} as defined by equation 5 into equation A3 yields the participation threshold

$$\beta \ge \beta_{\min} \equiv \frac{p - 1 - \frac{(1 - \lambda)^2}{\lambda} (b - p)}{\lambda (p - 1) + (1 - \lambda) (b - 1)}.$$
(A4)

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 β . In

contrast, β determines the cost of commitment: The expected loss due to 'rational default' is λD_{min} , which decreases in β by Proposition 2. Perhaps counter-intuitively, for individuals who cannot save in autarky, commitment is most attractive to those with the *lowest* degree of time-inconsistency (or the highest β), as the penalty required to enforce the savings plan is small, and poses little risk in the presence of shocks. In consequence, conditional on $\beta < \beta_{NC}$, agents adopt commitment for sufficiently high $\beta \ge \beta_{min}$. The connection between commitment adoption and the shock frequency rate $\lambda \in [0, 1]$ is straightforward: By Proposition 2, the cost of commitment λD_{min} increases in λ , while the benefit from commitment $(1-\lambda)^2(b-p)$ decreases in λ . Commitment is adopted if shocks are sufficiently rare (i.e. if λ is sufficiently low).

(c) 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, partially sophisticated individuals will adopt penalties strictly below the required effective minimum, $\hat{D}_{min} < D_{min}$. As a result, adopters' incentive constraints in period 1 are systematically violated, triggering contract default. (b) Commitment is adopted in the range $\hat{\beta} \in [\beta_{min}, \beta_{NC})$: Individuals who believe themselves to be sufficiently time-consistent to save in autarky (those with $\hat{\beta} \ge \beta_{NC}$) never adopt commitment. For those who realize they cannot save in autarky ($\hat{\beta} < \beta_{NC}$), sophistication negatively predicts commitment adoption: For a given β , commitment will be adopted above a threshold level of naiveté $\hat{\beta} > \beta_{min}$. (c) With partial sophistication, offering commitment weakly decreases welfare. It strictly decreases the expected welfare of adopters by D.

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

(a) From period 0's perspective, a penalty is perceived to be effective when it satisfies $D \ge D_{min}(\hat{\beta})$. For ease of notation, denote the perceived minimum effective penalty as $\hat{D}_{min} \equiv D_{min}(\hat{\beta})$. D_{min} strictly decreases in β , thus $\hat{\beta} > \beta$ implies $\hat{D}_{min} < D_{min}$. By Proposition 3, conditional on adopting commitment, agents will adopt \hat{D}_{min} . Upon reaching period 1, and realizing one's true value of β , the incentive constraint (equation 4) is violated. Period 1 prefers to abandon the savings plan and incur the penalty.

(b) Directly follows from Proposition 3(b), noting that $\hat{D}_{min} = 0$ for $\hat{\beta} \ge \beta_{NC}$. Agents compare the *perceived* cost of commitment with the *perceived* benefit, and adopt commitment if $\lambda \hat{D}_{min} \le (1-\lambda)^2(b-p)$. The cost of commitment decreases in $\hat{\beta}$, while the benefit is invariant to it. Holding factual time-inconsistency β fixed, a higher degree of naiveté $\hat{\beta} - \beta$ implies that a lower penalty is regarded as effective, and thus less is at stake in case of a 'rational default.'

(c) Faced with an ineffective penalty $\hat{D}_{min} < D_{min}$, 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 $E(U_0^D) = 2(1 - \lambda) - \hat{D}_{min}$. Note that naiveté has an ambiguous effect on this welfare loss: A more naive agent believes a lower penalty \hat{D}_{min} to be effective, and is therefore more likely to adopt commitment, but she also suffers a smaller welfare loss after default.

Corollary. Commitment adoption decreases with the introduction of a lower bound on the penalty, \underline{D} . Defaults also decrease in \underline{D} . Welfare effects of \underline{D} are negative for full sophisticates. For partial sophisticates, introducing

<u>D</u> increases welfare if it either deters them from adopting commitment, or imposes an incentive-compatible penalty $(\hat{D}_{min} < D_{min} \leq \underline{D})$. For insufficient bounds $\hat{D}_{min} < \underline{D} < D_{min}$, it reduces welfare by increasing the cost of default.

Proof. The adoption decision $\lambda \hat{D}_{min} \leq (1 - \lambda)^2 (b - p)$ is replaced by $\lambda max\{\underline{D}, \hat{D}_{min}\} \leq (1 - \lambda)^2 (b - p)$, thus mechanically reducing adoption rates. Conditional on adopting, agents choose weakly higher penalties, $max\{\underline{D}, \hat{D}_{min}\} \geq \hat{D}_{min}$. Default rates are unchanged for full sophisticates, and decrease for partial sophisticates, due to the possibility that $\hat{D}_{min} < D_{min} \leq \underline{D}$.

On welfare: Full sophisticates pay a weakly higher penalty $max\{\underline{D}, D_{min}\}$ in case of rational default, thus decreasing welfare. Welfare is also reduced for sophisticates who are deterred from adopting commitment because of the bound, i.e. those where $\lambda D_{min} \leq (1-\lambda)^2 (b-p) < \lambda \underline{D}$. In contrast, welfare increases for partial sophisticates who are deterred from commitment, $\lambda \hat{D}_{min} \leq (1-\lambda)^2 (b-p) < \lambda \underline{D}$ (this is a corollary of Proposition 4(d)). For partial sophisticates who still adopt commitment, welfare effects of \underline{D} depend on whether or not \underline{D} is high enough to prevent default: If $\hat{D}_{min} < \underline{D} < D_{min}$, the agent is strictly worse off, as she still defaults but pays a higher penalty. If $\hat{D}_{min} < D_{min} \leq \underline{D}$, the agent is strictly better off, as she obtains the nondivisible and avoids the penalty (barring shocks). In contrast to the full sophisticate, welfare increases even if $\underline{D} \gg D_{min}$: The adoption decision ensures that the agent prefers to commit with \underline{D} rather than not get the nondivisible good (this internalizes the risk of rational default, $\lambda \underline{D}$). Without \underline{D} , the partial sophisticate would not obtain the nondivisible good *and* pay the penalty. In contrast, a full sophisticate would already obtain the nondivisible good without a bound \underline{D} , but face a lower cost λD_{min} for rational default.

III. Sources and Notes for the Literature Table

Table 1 provides a review of the commitment device literature, reporting both the observed demand for, and 'followthrough' on, commitment contracts. Due to considerable heterogeneity in the design of commitment contracts, I propose 'follow-through' measures based on the individual setting. Importantly, successful completion of a commitment contract should not be equated to welfare, since adopters may benefit from trying even if they fail to meet a binary threshold of success. To the author's knowledge, Table 1 includes all studies which satisfy the following criteria:

- a) field studies (randomized or observational), which
- b) include 'pure' commitment contracts, i.e., contracts which are at least weakly dominated, and would thus not be attractive to time-consistent agents, and where
- c) follow-through on the commitment can be both defined and observed.

In addition, the table includes Brune et al. (2016) and Toussaert (2018), which do not fit the above (Brune et al. (2016) violates c), and Toussaert (2018) violates a) and c)). They are included for comparison due to their immediate relevance for the paper.

This appendix provides notes and sources for all statistics in Table 1. References are ordered as in the table (first by commitment domain, then by year).

Savings

Ashraf et al. (2006)

Notes on take-up: 710 individuals were offered the SEED account, 202 (28 percent) accepted. Of 202 accounts, 62 (31 percent) had amount-based goals, and 140 had date-based goals.

Notes on follow-through: (1) Of 62 amount-based accounts, 6 reached the goal amount within one year. (2) In Section VI.A., the authors report that "After twelve months about half of the clients deposited money into their SEED account after the initial opening deposit. Fifty percent of all accounts are at P100, the minimum opening deposit."

Jones and Mahajan (2015)

Notes on take-up: The table focuses on treatment arm T4, "the commitment control group", as all other commitment treatments offer additional bonuses for committing to save. Strictly speaking, T4 is closer to a prediction exercise than a pure commitment: People receive a reward if they pre-commit in February to save their tax refund in October, and then actually do save their tax refund in October. This constitutes a commitment rather than a bonus because they could also receive a reward by pre-committing not to save in February, and then actually not save in October. The design differs from a pure commitment in that a) a time-consistent agent who plans to save also benefits from committing to save (it is strictly dominant to commit to something, either save or not save), and b) the rewards for pre-committing to save (and follow-through) and pre-committing not to save (and follow-through) are not quite identical (\$100 and \$75, respectively). In both cases, someone who pre-commits in February and then makes an inconsistent choice in October does not get the reward. The take-up

rate is conditional on consenting to the study by phone: Of 137 individuals in group T4, 42 could be reached by phone, and 19 consented to the study. Out of these 19 individuals, 7 pre-committed to save.

Notes on follow-through: Out of 7 individuals who pre-committed to save in February, 3 actually saved in October (source: authors). Those who ended up not saving could have earned \$75 by pre-committing not to save, instead of pre-committing to save.

Brune et al. (2016)

Notes on take-up: Farmers were given the option to automatically deposit their harvest proceeds into either an ordinary savings account, or a withdrawal-restriction account. In the WR account, savings could not be withdrawn until planting season. The paper reports ITT effects on saving, but not the fraction of individuals who allocated positive amounts to the WR account. Information obtained from the authors reveals that 72 out of 1314 farmers offered commitment (5.5 percent) deposited positive amounts in the WR account. This low fraction is partly driven by a trigger rule design combined with optimism about harvest proceeds: Farmers made the decision how much to allocate to each account before they learned the realization of their revenue. A trigger rule ensured that funds were only placed in the WR account if the farmer's revenue exceeded the amount allocated to the ordinary account. Harvest proceeds were bad in the study year, so that many farmers allocated more revenue than they actually received. The fraction of farmers who deposited a positive amount in either account (which is immune to optimism) was 20.7 percent.

Notes on follow-through: Farmers' plans were carried out automatically. Contrast to e.g. Ashraf et al (2006), these savings accounts were not intended for later manual deposits.

Health

DellaVigna and Malmendier (2006)

Notes on take-up: Taken from Table 4. Sample of 866 health club members whose first contract was monthly, and 145 health club members whose first contract was annual. Only unsubsidized memberships are considered.

Notes on follow-through: Taken from Table 4. For monthly contracts, the 20th percentile of price per attendance is \$10.18, which is above the \$10 pay-per-visit fee (using 10-visit passes). Thus, less than 20 percent of monthly members pay less using the membership compared to pay-per-visit. For annual members, the 25th percentile of price per attendance is \$11.27, and thus less than 25 percent pay less using the membership. The figure of \$600 of foregone savings is based on average membership duration, compared to using 10-visit passes (\$100) and individual passes (\$12) to pay for average visits.

Burger and Lynham (2010)

Notes on take-up: Sample of 51 adopters, i.e. individuals entering weight loss bets with a commercial bookmaker. The bookmaker offers odds between 5:1 and 50:1 depending on weight loss targets, thus this is not a pure commitment. Time-consistents who want to lose weight might benefit from these

bets, though one could argue that they are less likely to have weight-control problems in the first place. The presence of time-consistents in the adopter sample should bias the success rate upwards. Of those bettors who discuss their motivation for entering the bet in the observed correspondence, 70 percent report using the bet as a commitment mechanism.

Notes on follow-through: Bettors must be weighed by a doctor both at the start and at the end of the bet to ensure enforcement, and to claim their prize. 20 percent of bettors win their bet. Losing bettors lose their betting stakes (average \$143, using 2007 US dollar equivalents).

Giné et al. (2010)

Notes on take-up: The bank offered the CARES smoking cessation contract to 781 smokers (approached on the street, and conditional on agreeing to a short baseline survey). Of these, 83 (11 percent) agreed to take the contract. The WR savings account connected to CARES paid no interest and carried no other benefits, thus CARES is a pure commitment.

Notes on follow-through: After six months, CARES clients had to pass a nicotine test (with zero tolerance) to avoid forfeiting their savings. 66 percent of clients failed the nicotine test, losing average savings of 277 pesos (U.S. \$6.60). Interestingly, those who failed their nicotine test had saved much less (chosen lower stakes) relative to those who succeeded (successful quitters saved 1,080 pesos on average). Causality plausibly runs both ways – those most likely to succeed choose higher stakes (save more), and higher stakes increase the chance of success. This study is an important illustration of why commitment follow-through cannot be (directly) equated with welfare – it is possible that those who attempted to quit smoking derived health benefits even if they failed the test.

Royer et al. (2015)

Notes on take-up: Commitment was offered to 346 individuals who had previously received incentives to go to their company gym. The commitment contract was a pledge not to go more than 14 calendar days in a row without attending the company gym over an 8-week period. Out of 346 individuals, 43 (12 percent) took up the commitment, and chose a self-funded stake that would be lost in case of failure. The average chosen stake was \$58, with a maximum of \$300.

Notes on follow-through: 63 percent of adopters followed through on their commitment (reported in Section F).

Bai et al. (2018)

Notes on take-up: The study offered commitment contracts to individuals at high risk of hypertension. Commitment consisted in the pre-payment of three visits to preventive health camps, which needed to be attended over a period of six months. The cost per visit was Rs. 30. 284 subjects were offered unsubsidized 'fixed' commitment contracts, which required them to pay Rs. 135 upfront, and included a commitment amount of Rs. 15 for each visit, which they would receive back when attending the health camp. 39 out of 284 individuals (13.7 percent) accepted these contracts (Section 3.2). An additional 283 individuals were offered 'personalized commitment contracts', which allowed them

to choose the commitment amount themselves, but always required pre-payment of Rs. 90 for the visits. 40 out of 283 individuals (14.1 percent) accepted these contracts. Two additional, subsidized commitment treatments are not considered here.

Notes on follow-through: The introduction of the study reports that "between 62-77% of those who paid for a commitment contract failed to make even one visit to a health camp." Further information obtained from the authors reveals that, of the 39 people signed up for a fixed commitment contract, 15 made at least one visit. Of the 40 people signed up for a personalized commitment, 12 made at least one visit. In sum, 27 of 79 participants with pure commitment contracts made at least one visit, resulting in the follow-through statistic of 34 percent. Welfare implications are more straightforward here than in other studies: At least for the 52 individuals who made no visit to the camp, the commitment contract cost them Rs. 90–135, but had no obvious health benefits.

Schilbach (forthcoming)

Notes on take-up: The study offered rickshaw drivers daily payments in exchange for coming to the study office. In the 'Choice' group, 75 subjects could choose between unconditional payments (Rs. 90, Rs. 120 or Rs. 150), or payments conditional on passing a blood alcohol test (Rs. 120 if passing BAC, Rs. 60 if not). One choice was valid for one week of daily payments. The choice between an unconditional Rs. 90 and the sobriety incentive contract is not a pure commitment, and is thus not considered here. When the unconditional payment was Rs. 120, 46.7 percent chose incentives in week 1, and 52 percent chose incentives in week 2 (Appendix Table B.9). Week 3 is not considered as it is only incentivized with 5 percent probability. The two weeks are averaged to a 49 percent take-up rate for a weakly dominated commitment contract (in the sense that the participant does not lose money as long as he passes the BAC). For the Rs. 150 unconditional payment, commitment is strictly dominated. In this case, 30.7 and 33.3 percent chose incentives in weeks 1 and 2, respectively, averaged to a 32 percent take-up rate for costly commitment.

Notes on follow-through: The relevant follow-through statistic is the percentage of individuals who pass the BAC when subjected to conditional incentives ('Option A' is implemented). The relevant 'commitment adopter' sample are those who choose unconditional payments of Rs. 120 over conditional incentives (Rs. 120 if passing BAC, Rs. 60 if not). From author information, this percentage is 63.24 percent in week 1, and 63.03 percent in week 2. An important qualification is that the commitment contract is chosen for an entire week, i.e., for six days of conditional daily payments. Follow-through is thus no longer binary: Individuals can pass the BAC on four days out of six, leading to a follow-through of 67 percent on an individual level.

Work and Effort

Ariely and Wertenbroch (2002)

Notes on take-up: Study 1: In the context of an MIT Executive Education class, 44 students (the 'free choice' group) could self-impose deadlines for three assignments, as long as the deadlines were

before the last lecture. Out of the resulting 132 potential deadlines, 89 deadlines (68 percent) were set before the last week of class, and thus earlier than necessary. All deadlines were binding, and late submission entailed a one percent grade penalty per day late. Study 2: This study consisted of proofreading three papers of 10 pages each, in exchange for payment. 20 students were assigned to a self-imposed deadlines condition, in which they chose their own deadlines for each of the three papers within a 21-day window. Late submissions were possible, but penalized at \$1 per day. The authors report that participants "chose to space out their proofreading tasks, F(2,38) = 63.28, p < .001". The fraction of deadlines that was set strictly before the 21-day limit could not be obtained.

Notes on follow-through: Study 1: All assignments were submitted on time (see Footnote 3), perhaps due to the high threat of academic grade penalties. However, performance was lower with self-set deadlines, compared to evenly spread exogenous deadlines. Study 2: The fraction of papers that were submitted with delay, conditional on choosing a self-imposed deadline, could not be obtained. From Figure 2, participants in the self-imposed deadlines condition submitted with an average of 8 days delay across three papers, thus incurring \$8 in penalties.

Kaur et al. (2015)

Notes on take-up: Over a period of 8 months, 102 data-entry workers (64 at any given time, plus turnover) were randomized into one of four treatments on a daily level, resulting in 8423 observations. In the two commitment treatments, workers could choose work targets (accurate data fields entered). If they met the target, they earned a standard piece rate. If they did not meet the target, they earned half the piece rate for each field entered. The weakly dominant target choice is zero, thus choosing a positive target constitutes a pure commitment contract. Of 4193 worker-days assigned to the commitment treatments, 1168 adopted commitment (chose a positive target, see Tables 3 and 9). The take-up rate of 36 percent is conditional on being present on the day of the commitment choice.

Notes on follow-through: 97.4 percent of workers met their self-chosen work targets (Table 5). This study differs significantly from other studies, in that self-chosen work targets were significantly *below* the status quo: From Table 9, the average conditional target is $2753 (= 767 \cdot \frac{4193}{1168})$. The average output in the control group is 5355. In contrast with commitment studies where participants set ambitious and previously unachieved goals, workers in this study used commitment to resist the temptation to go home early, effectively cutting off the left tail of the effort distribution. Consistent with this interpretation, the authors predict that 9.1 percent of workers would have missed their self-chosen target, had they been assigned to the control contract that day (Table 5).

Bisin and Hyndman (2018)

Notes on take-up: In a framed field experiment, students were asked to sort clouds of 150-200 words into alphabetic order. Study 1 consisted of doing 1 task within one week and paid \$20 for completion. Study 2 consisted of doing three tasks within two weeks and paid \$15 per completed task. An 'endogenous deadline' treatment allowed students to self-impose additional earlier deadlines. In Study 1, 11 out of 35 students (31 percent) chose a deadline earlier than necessary. In Study 2, of the 67

students assigned to the endogenous deadlines condition, 48 percent chose a deadline for the first task, 46 percent for the second task, and 34 percent for the third task (calculated from sample sizes in Table 1 and take-up rates in Table 3). The figure reported in the literature overview is that for the first task. Study 2 is not comparable to Study 1 because it contains more (controlled) variation in the time within the semester and the number of words in each task. The equivalent figures for the mid-semester 150-word task are 62, 57, and 43 percent for the first, second, and third task.

Notes on follow-through: The deadlines here eliminate the option of submitting late (contrast to Ariely and Wertenbroch (2002)), but do not affect the cost of not completing the task. Thus, participants do not commit to submit, they commit to not submitting late. In this sense, follow-through is not defined, as the commitment is enforced automatically (within the literature table, this is comparable to Brune et al. (2016) and Toussaert (2018)). Nevertheless, the reader may be interested in the effect of the deadlines on task completion – which are affected through the elimination of the option to submit late. These should be seen as a treatment effect rather than a follow-through statistic, and are reported as Intent-to-Treat effects. In Study 1 (one task), the endogenous deadlines treatment increased task completion relative to the control group (57% vs 46%, from Table 4), but this difference is not significant. In Study 2 (three tasks), the deadlines treatment significantly decreased completion rates (37% vs 47%).

Toussaert (2018)

Notes on take-up: The paper studies commitment in the context of costly self-control preferences (Gul and Pesendorfer (2001)), rather than time-inconsistency. In the context of a lab experiment, 120 participants were instructed to look at a screen for up to 60 minutes, where four-digit numbers were displayed and updated every second. At five random times, they received prompts to enter the last number seen, and were paid \$2 per correct answer. The study introduced a temptation good in the form of a tempting story (a strange experience that happened to a peer), and elicited menu preferences for having access to the story, at the cost of receiving only four prompts. 38.3 percent of subjects prefer not to have access to the story during the task (this is calculated as 35.8 percent with the ordering $\{0\} \succ_1 \{0,1\} \succ_1 \{1\}$ plus 2.5 percent with the ordering $\{0\} \succeq_1 \{0,1\} \sim_1 \{1\}$, Section 4.1.1). Furthermore, 25.8 percent have a positive willingness to pay to exclude the option from their menu (from Section 4.1.2, calculated as 23.3+2.5).

Notes on follow-through: Where participants' choices were implemented, access was impossible. The study includes several practical provisions to prevent participants from exchanging the story after the session. Thus, follow-through is not defined, as commitment is enforced automatically.

IV. Supplementary Figures and Tables

IV.1. Details on Savings Plans

	Personal Savings Plan											
Name: Sample					Pu	Purpose of Savings: 7uition Fees						
Ado	dress: <i>Giu</i>	ıqooq City, '	Mindanao		Go Go	al Date: al Amount:	4 July 50007	DhP				
wk	Date Due	Deposit Due	Date of Deposit	Deposit made? (tick!)	wk	Date Due	Deposit Due	Date of Deposit	Deposit made? (tick!)			
1	22 Feb	250			11	2 May	250					
2	29 Feb	250			12	9 May	250					
3	07 Mar	250			13	16 May	250					
4	14 Mar	250			14	23 May	250					
5	21 Mar	250			15	30 May	250					
6	28 Mar	250			16	6 Jun	250					
7	04 Apr	250			17	13 Jun	250					
8	11 Apr	250			18	20 Jun	250					
9	18 Apr	250			19	27 Jun	250					
10	25 Apr	250			20	4 Jul	250					

FIGURE A1. PERSONAL SAVINGS PLAN (ALL TREATMENT GROUPS)

	Installment	IS	Withdrawal	WR	Control	F-stat
	Savings	adopters	Restr.	adopters		P-value:
	Treatment	only	Treatment	only		Equality IS,
						WR and C
Accepts ordinary savings account	0.92	1	0.92	1	0.93	0.95
with 100 pesos						
Accepts commitment account	0.27	1	0.42	1		0.00
Savings Plan Characteristics						
Median Goal Amount (pesos)	2400	2400	2825	2465	3000	0.00
Median Installment Size (pesos)	150	150	200	150	170	0.31
Median Time until Goal Date (days)	137	138	135	133	137	0.79
Commitment Account Characteristics						
Median chosen penalty (IS)		150				
Chooses minimum penalty (IS)		0.80				
Chooses date-based goal (WR)				0.58		
Chooses amount-based goal (WR)				0.42		
Outcomes - Ordinary Accounts (means)					
# Deposits into ordinary account	0.60	1.50	0.25	0.42	0.22	0.01
Makes at least 1 deposit after opening	0.21	0.50	0.07	0.10	0.08	0.00
Savings by goal date (pesos)	75.94	196.75	61.10	95.97	27.16	0.32
Outerman Committee and Accounts (and						
Unicomes - Commiment Accounts (me	ans)	(75	0.71	1 (0		0.00
# Deposits into commitment account	1.82	0.75	0.71	1.08		0.00
	successful	11.98	date-based	1.68		
	default	2.52	amount-based	1.69		
Makes at least 1 deposit after opening	0.19	0.69	0.09	0.21		0.00
	successful	1	date-based	0.25		
	default	0.44	amount-based	0.15		
	5					
Savings by goal date (pesos),	379.85	1233.67	114.30	233.56		0.00
net of penalties	successful	2636.25	date-based	286.09		
default (be	fore penalty)	300.99	amount-based	162.18		
default (d	fter penalty)	120.51				
Observations for account data	423	114	219	92	210	
for savings plan data	367	114	188	92	191	

TABLE A1. SAVINGS PLAN AND ACCOUNT STATISTICS

Note: Respondents were allowed to modify their initial savings plan after accepting a commitment product. Deposits refer to physical bank transactions, not to IS installments. Several IS installments could be made in a single deposit. The opening balance for IS and WR counts as the first deposit, while the opening balance for the ordinary accounts was paid by the research team. The sample for account outcomes are those respondents reached in marketing. The sample for savings plan outcomes are those willing to make a savings plan. In the IS group, 'successful' denotes the subsample who completed their IS contract successfully, while 'default' is the subsample who defaulted. Similarly, date-based and amount-based refer to the respective subsample of WR clients. Two outliers are excluded, see footnote 30.

	All	All (%)	IS	WR
			adopters	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. PERSONAL SAVINGS GOALS

TABLE A3. IS PRE-ORDER

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

TABLE A4. IS PRE-ORDER: NEW CONTRACT CHOICES

New penalty > old penalty?	New	New	New
New goal > old goal?	penalty lower	penalty equal	penalty higher
New goal lower	0.03 / 0.11	0.09 / 0.22	- / 0.06
New goal equal	- / -	0.03 / 0.06	- / -
New goal higher	0.03 / -	0.39 / 0.28	0.42 / 0.28

Cell entries: Percentage of IS Pre-Order clients who choose a higher or lower savings goal and default penalty, relative to their first IS contract. First cell entry: Percentage among Pre-Order clients who were successful in the first IS round (N = 33). Second cell entry: Percentage among Pre-Order clients who defaulted in the first IS round (N = 18).



FIGURE A2. RATIO OF CHARGED TERMINATION FEES TO IS DEPOSITS (IS DEFAULTER SAMPLE)



FIGURE A3. TERMINATION FEES (CHOSEN & CHARGED)

Figure A3 displays the termination fees (default penalties) that were charged to defaulting clients. They are grouped by the fee level chosen at contract signing. Not all chosen fees were enforceable: Whenever clients chose a fee strictly above the minimum and later defaulted on their contract, the charged fee was the lesser of chosen fee and savings balance at the time of default (see Section 3.2). The minimum fee was always enforceable through the opening balance. No fee was charged to successful clients.



FIGURE A4. WEEKLY SHOCK ARRIVAL RATE (BY CALENDAR MONTH)

IV.2. 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)			

TABLE A5. PREDICTING SAMPLE ATTRITION

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.

		Savings P	lan Sample	e	Endline Sample			
	IS	WR	Control	F-stat	IS	WR	Control	F-stat
				P-value				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)	01170	(0.099)	(0.124)	(0.134)	01101
Tempenton (tange o 10)	(0.095)	(0.123)	(0.155)		(0.055)	(0.121)	(0.151)	
Impatience	0.321	0.391	0.333	0.255	0.327	0.416	0.339	0.109
<u>F</u>	(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)	0.02)	(0.026)	(0.036)	(0.037)	0.919
	(0.020)	(0102.0)	(0.000)		(0.020)	(0102.0)	(0.027)	
Risk aversion (range 0-6)	4 241	4 654	4 1 5 2	0.023	4 211	4 653	4 1 1 3	0.014
ruon aversion (runge e e)	(0.104)	(0.132)	(0.140)	0.020	(0.106)	(0.133)	(0.146)	01011
Cognitive ability	2.957	2.878	2.911	0 795	2,929	2.842	2.977	0.606
(range ()-5)	(0.065)	(0.098)	(0.110)	0.795	(0.066)	(0.099)	(0.105)	0.000
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)	0.700	(0.052)	(0.073)	(0.074)	0.050
HH Bargaining Power (0-5)	2 648	2 543	2 634	0.811	2 633	2 574	2 627	0 934
The barganning rower (0.5)	(0.097)	(0.134)	(0.133)	0.011	(0.097)	(0.133)	(0.138)	0.951
Distance to Bank (km)	1 351	1 383	1 343	0.923	1 357	1 306	1 287	0 709
Distance to Dank (kin)	(0.052)	(0.087)	(0.077)	0.725	(0.052)	(0.078)	(0.070)	0.707
Existing Sovings Account	(0.052)	0.468	0.455	0.800	0.444	(0.070)	0.407	0.436
Existing Savings Account	(0.026)	(0.036)	(0.036)	0.090	(0.026)	(0.036)	(0.037)	0.4.00
Donated to charity	0.358	0.383	(0.030)	0.208	0.386	0.368	(0.037) 0.424	0.540
in the last 12 months	(0.025)	(0.036)	(0.036)	0.208	0.380	(0.035)	(0.037)	0.540
m uic iast 12 monuis	(0.023)	(0.030)	(0.050)		(0.020)	(0.055)	(0.057)	
#Emergencies last vr	0.417	0 388	0420	0.844	0/10	0 38/	0420	0.807
memergencies last yi	(0.038)	(0.051)	(0.051)	0.044	(0.038)	(0.040)	0.427 (0.054)	0.007
Global Signif Test (D value)	0.050)	0.031)	0.56		0.030)	0.24	0.034)	
Observations	360	188	101		265	100	177	
Costi valions	509	100	171		505	190	1//	

TABLE A6. SUMMARY STATISTICS ACROSS SAMPLES

Note: A starred variable indicates that the randomisation was stratified on this variable. Variables are as described in Table 2. 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.

IV.3. Additional Results

Predicting Commitment Demand & Default: Full Tables Table A7 shows the full list of coefficients for all control variables included in the take-up regressions in Table 4, columns (1) and (5), in the main text. The predictive power of covariates grouped under '*Self-Control*', '*Other-Control*' and '*Shocks*' is discussed in Section 5.4.

Looking beyond these main mechanisms, the first notable fact is the lack of any demographic predictors for the adoption of the installment-savings product: Age, gender, income, assets, marital status, education and household size are all uncorrelated with IS take-up. Instead, IS take-up is predicted by a measure of cognitive ability (based on Raven's matrices, see Figure A11). The positive association with cognitive ability is reassuring: The IS product is more complex than traditional savings accounts (but no more complex than a loan contract). The significance of cognitive skills suggests that clients who were more likely to understand the rules were also more likely to take up the product. This may be interpreted as evidence against possible manipulation by the bank marketers (discussed in Section 6.6). Furthermore, individuals with an existing bank account (at any local bank) were more likely to take up the product. Given a widespread skepticism towards banks in the study area, this may be interpreted as a sign of trust in and familiarity with the banking system. No other included variables predict IS take-up. Notably, neither distance to the bank branch (see the discussion on transaction costs in Section 6.4) nor charity attitudes (proxied by having donated any positive amount in the past 12 months, also see footnote 18) significantly relate to IS take-up.

The withdrawal-restriction product had no predictors beyond those discussed in Section 5.4: High education (in years of schooling), high risk aversion (choosing a safe lottery in Figure A10), high household bargaining power (measured using questions on who decides what in a household), and strong claims from others on own liquid assets. Given that education plausibly captures an aspect of household bargaining power beyond the corresponding direct measure, these predictors are grouped as an '*Other-Control*' mechanism.

Table A8 lists full coefficients from the IS default and pre-order regressions in Table 4 (columns (2)–(4)). The IS take-up regression in column (1) of Table A8 is a repetition from Table A7 for easy reference. As in the case of the take-up regressions, the predictive power of covariates grouped under '*Self-Control*', '*Other-Control*' and '*Shocks*' is discussed in Section 5.4. Additional predictors of IS default include financial literacy (-) and cognitive ability (+). Financial literacy is less surprising: Individuals with poor numeracy skills tend to do worse at managing their household finances, and may fail to allocate a portion of the household budget to regular IS deposits. The positive significance of cognitive ability is partially explained by the predictive power that cognitive ability has for take-up of the IS product, as those who struggle to understand the product's rules don't select in. Explaining the correlation between cognitive ability and default within the take-up sample is more difficult. Possible channels include rationalizability of behaviour and a sunk-cost fallacy: An individual with high cognitive skills is more likely to realize when a contract is no longer optimal, even for time-inconsistent reasons.

Table A9 provides further information on the types of emergencies elicited during the endline survey (discussed in Section 5.3). The most frequent shock was serious illness of non-income earning household members, such as children and the elderly (0.172 shocks over the last 6 months, or 16.9 percent positive values). The second most frequent shock was house damage due to natural disasters (0.169 shocks, equivalent to 16.9 percent positive answers. See Section 6.1 for a discussion of the tropical storm which happened during the study period). The third most frequent shock was illness of income-earning household members (0.113 shocks, equivalent to 11.3 percent of positive answers). All other shock types had frequencies less than 0.05 over six months. Column 2

of Table A9 proceeds to replicate the default analysis conducted in Table 4, Column 2, but substitutes the count variable "#Emergencies since baseline" with a full list of shock type indicators. While Table A9 reports only the coefficients on the shock types for clarity, the magnitude and significance of other default predictors (notably the time preference measures) is unaffected. Column 3 of Table A9 replicates the main treatment effect analysis on bank savings (Table 3, Column 1), but adds shock type indicators to capture additional variation in savings at the partner bank. The results in Table A9 jointly suggest that serious illness of income-earning household members was likely the biggest source of risk for the Installment Savings account: It is among the most frequent shocks, it predicts lower savings, and it predicts default on the IS account (at 10% significance). In contrast, natural disaster damage was relatively frequent and predicts lower savings, but it is not associated with IS default.

Commitment	Ins	stallment Savir	igs	Withd	rawal Restric	cuon
Take-Up	(1)	(2)	(3)	(4)	(5)	(6)
Self-Control Motive						
Present Bias	0.0589	0.0811	0.0827	0.156	0.134	0.1046
	(0.0826)	(0.0872)	(0.0864)	(0.129)	(0.127)	(0.1301)
Soph. Present Bias	-0.0557**	-0.0613**	-0.0631**	-0.0736	-0.0616	-0.0530
(Pres.Bias*Temptation)	(0.0284)	(0.0298)	(0.0292)	(0.0543)	(0.0463)	(0.0532)
Perceived Temptation (0-10)	-0.0100	-0.0028	-0.0046	0.0090	0.0071	0.0006
	(0.0124)	(0.0123)	(0.0125)	(0.0207)	(0.0218)	(0.0209)
Other-Control Motive						
Faces strong financial	-0.0006		-0.0038	0.122*		0.1185*
claims from others	(0.0425)		(0.0414)	(0.0637)		(0.0646)
HH Bargaining Power (0-5)	0.0052		0.0053	0.0397**		0.0456***
	(0.0111)		(0.0113)	(0.0168)		(0.0167)
Education (yrs)	-0.0054		-0.0094	0.0285***		0.0315***
	(0.0061)		(0.0064)	(0.0084)		(0.0093)
Risk Aversion (0-6)	-0.0023		-0.0059	0.0552***		0.0500***
	(0.0108)		(0.0105)	(0.0166)		(0.0168)
Shocks						
#Emergencies last yr	-0.0064		-0.0161	-0.0005		-0.0215
	(0.0273)		(0.0277)	(0.0482)		(0.0493)
Additional Control Variable	5					
Age (yrs)		0.0008	-0.0003		-0.0000	0.0011
		(0.0019)	(0.0020)		(0.0029)	(0.0028)
Female		0.0630	0.0536		0.244	0.2300
		(0.0856)	(0.0868)		(0.172)	(0.1516)
Married		0.0432	0.0165		-0.123	-0.0952
		(0.0615)	(0.0630)		(0.0968)	(0.0887)
Weekly HH income (in thousand j	pesos)	-0.0030	-0.0028		-0.0014	-0.0052
		(0.0077)	(0.0079)		(0.0123)	(0.0109)
No. of appliances owned		-0.0133	-0.0084		0.0365*	0.0258
		(0.0137)	(0.0143)		(0.0217)	(0.0216)
No. of HH members		0.0144	0.0130		0.0186	0.0217
		(0.0094)	(0.0095)		(0.0161)	(0.0158)
Impatience		0.0028	-0.0008		-0.0221	-0.0219
		(0.0463)	(0.0464)		(0.0742)	(0.0688)
Cognitive Ability (0-5)		0.0359*	0.0363*		0.0094	0.0174
		(0.0190)	(0.0187)		(0.0256)	(0.0239)
Financial Literacy (0-5)		0.0311	0.0328		0.0239	-0.0120
		(0.0249)	(0.0250)		(0.0330)	(0.0321)
Distance to Bank (km)		-0.0257	-0.0265		-0.0041	0.0084
		(0.0206)	(0.0207)		(0.0267)	(0.0260)
Existing Savings Account		0.0907**	0.0998**		0.0616	-0.0022
		(0.0437)	(0.0444)		(0.0677)	(0.0667)
Donated to Charity		0.0180	0.0221		0.0017	-0.0100
		(0.0426)	(0.0424)		(0.0673)	(0.0625)
Marketer FE	YES	YES	YES	YES	YES	YES
Mean Dep. Variable	0.2673	0.2685	0.2687	0.4104	0.4171	0.4115
Observations	404	406	402	212	211	209

TABLE A7. PREDICTING DEMAND FOR COMMITMENT (PROBIT)

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Entries in the table represent marginal coefficients. Variables are as described in Table 2. The sample is restricted to clients who could be located for the marketing visit (see Table A5).

Dependent	(1) IS	(2) Default	(3) Default	(4) Pre-Orde
Variable	Take-Up	(IS-Sample)	(takeup-Sample)	(takeup-Sample
Self-Control Motive				
Present Bias	0.0827	0.1119*	0.4837*	-0.4860**
	(0.0864)	(0.0654)	(0.2550)	(0.2353)
Soph. Present Bias	-0.0631**	-0.0453**	-0.1718	0.2375**
(Pres.Bias*Temptation)	(0.0292)	(0.0230)	(0.1299)	(0.1170)
Perceived Temptation (0-10)	-0.0046	-0.0202*	-0.0655***	0.0109
	(0.0125)	(0.0105)	(0.0244)	(0.0301)
Other-Control Motive				
Faces strong financial	-0.0038	-0.0113	0.0095	0.0097
claims from others	(0.0414)	(0.0330)	(0.0848)	(0.0905)
HH Bargaining Power (0-5)	0.0053	-0.0116	-0.0778***	0.0792***
	(0.0113)	(0.0090)	(0.0236)	(0.0257)
Education (yrs)	-0.0094	-0.0030	0.0016	-0.0133
•	(0.0064)	(0.0053)	(0.0121)	(0.0142)
Risk Aversion (0-6)	-0.0059	-0.0181**	-0.0673***	0.0199
. *	(0.0105)	(0.0084)	(0.0192)	(0.0240)
Shocks				
#Emergencies last yr	-0.0161	0.0005	0.0558	-0.0855
	(0.0277)	(0.0213)	(0.0601)	(0.0677)
#Emergencies since baseline		-0.0033	0.1156*	-0.0414
		(0.0182)	(0.0687)	(0.0658)
Additional Control Variables				
Age (yrs)	-0.0003	-0.0024	-0.0057	-0.0080*
	(0.0020)	(0.0016)	(0.0041)	(0.0043)
Female	0.0536	0.1189	0.3663*	0.0745
	(0.0868)	(0.0911)	(0.1871)	(0.1942)
Married	0.0165	0.0064	0.0363	-0.2433
	(0.0630)	(0.0536)	(0.1369)	(0.1490)
Weekly HH income (in thousand pesos)	-0.0028	0.0034	0.0206	0.0124
	(0.0079)	(0.0057)	(0.0159)	(0.0177)
No. of appliances owned	-0.0084	-0.0093	-0.0384	0.0007
	(0.0143)	(0.0115)	(0.0238)	(0.0298)
No. of HH members	0.0130	0.0123	0.0159	0.0015
	(0.0095)	(0.0077)	(0.0143)	(0.0179)
Impatience	-0.0008	-0.0030	0.0293	0.0319
	(0.0464)	(0.0372)	(0.0882)	(0.1001)
Cognitive Ability (0-5)	0.0363*	0.0365**	0.0658*	-0.0385
	(0.0187)	(0.0143)	(0.0384)	(0.0428)
Financial Literacy (0-5)	0.0328	-0.0168	-0.1403***	0.0582
	(0.0250)	(0.0204)	(0.0401)	(0.0465)
Distance to Bank (km)	-0.0265	-0.0115	-0.0365	0.0643
	(0.0207)	(0.0165)	(0.0536)	(0.0615)
Existing Savings Account	0.0998**	0.0296	-0.0641	0.1694*
	(0.0444)	(0.0363)	(0.0860)	(0.0890)
Donated to Charity	0.0221	0.0422	0.0802	0.2122**
	(0.0424)	(0.0332)	(0.0888)	(0.0896)
Marketer FE	YES	YES	YES	YES
Mean Dep. Variable	0.2687	0.1468	0.5463	0.4630
Observations	402	402	108	108

TABLE A8. IS DEFAULTS & REPEAT TAKE-UPS (PROBIT)

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Entries in the table represent the marginal coefficients of the probit regressions. Variables are as described in Table 2.

	(1) Shock occurrence		(2) Predicts	(3) Predicts
	(# of tim	es, last 6M)	IS Default?	Bank Savings?
	Mean	SD	IS-Sample	Full Sample
			(Specification as in	(Specification as in
House domesed anniously due to a noticeal disaster	0.1604	0.2754		
House damaged seriously due to a natural disaster	0.1094	0.3734	-0.0104	$-227.2^{-0.0}$
(e.g. flooding)			(0.0489)	(65.09)
Crops lost due to natural disaster	0.0615	0.2404	-0.0894	129.1
			(0.114)	(172.5)
Loss of land due to erosion/flooding	0.0014	0.0370	_	-74.70***
				(18.27)
Loss of animals due to natural disaster	0.0096	0.0974	0.129	-214.5
			(0.141)	(163.2)
Serious illness or injury of income earning	0.1134	0.3173	0.111*	-127.9**
HH member (from sickness or accident)			(0.0647)	(56.64)
Serious illness or injury of non-income earning	0.1721	0.3814	-0.0375	-21.10
HH member (from sickness or accident)			(0.0501)	(87.74)
Income earning household member passed away	0.0068	0.0824	0.242*	370.8
8			(0.131)	(654.1)
Non-Income earning household member passed away	0.0055	0.0738	_	-280.0**
				(139.1)
HH member lost job	0.0478	0.2135	0.0839	-128.9**
,			(0.0739)	(62.82)
Income-earning HH member left the household	0.0191	0.1371	_	370.8
e				(598.7)
Income loss due to Mindanao Power Crisis	0.0423	0.2015	0.0482	28.69
			(0.108)	(90.28)
Marriage of household member	0.0068	0.0824	_	-245.4*
				(141.5)
Poisoning/damaging livestock by others	0.0164	0.1374	_	-98.15
				(83.17)
Legal Case/Dispute (e.g. marriage annulment)	0.0096	0.0974	-	-292.7***
				(108.0)
Theft	0.0246	0.1550	-0.0481	-82.43
			(0.134)	(138.9)
Mugging/robbery	0.0014	0.0370	_	8.186
				(64.52)
Other	0.0123	0.1103	_	-137.4
				(87.67)

TABLE A9. SHOCK TYPES (ENDLINE SURVEY)

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Column 2 reports marginal coefficients from a probit specification identical to that in Table 4, Column 2 (IS default analysis), but substitutes the count variable "#Emergencies since baseline" with a full list of shock type indicators. Missing coefficients indicate that the covariate was dropped from the regression for lack of variation in outcomes (for instance, none of the respondents who reported the death of a non-income earning household member were IS clients). The inclusion of shock types in the regression does not affect the magnitude and significance of the coefficients on time preferences (Present Bias 0.127* (0.0655), Soph. Present Bias -0.0449** (0.0221), Perceived Temptation -0.0188* (0.0104)). Column 3 reports OLS coefficients from a specification identical to that in Table 3, Column 1 (treatment effect on bank savings), but adds the shock type indicators as additional controls.

	Not Present-Biased	Present-biased	All	T-stat P-value
Prediction Gap (growth)	on Gap (growth) 3.290378		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	

TABLE A10. EVIDENCE OF INCOME OPTIMISM

Standard deviations in parentheses. All numbers are group averages.

Evidence of Income Optimism Table A10 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: First, the difference between predicted income growth and realized income growth,

$$PredictionGap(growth)_{i} = \sum_{m=Nov}^{Mar} (bracket_{m}^{pred} - bracket_{October}^{pred}) - \sum_{m=Nov}^{Mar} (bracket_{m}^{real} - bracket_{October}^{real}) - bracket_{October}^{real}) - bracket_{October}^{real} - bracket_{October}^{real}) - bracket_{October}^{real} - bracket_{October}^{real}) - bracket_{October}^{real} - bracket_{October}^{real}) - bracket_{October}^{real} - bracket_{October}^{real} - bracket_{October}^{real}) - bracket_{October}^{real} - bracket_{October}^{real} - bracket_{October}^{real}) - bracket_{October}^{real} - bracket_{October}^{real}$$

In other words, income growth is proxied by the sum of deviations from October income, in units of income brackets. Depending on the individual date of the baseline survey, October income was partially observed at this time, and thus serves as a benchmark. 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 realized 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 A10 are those individuals who participated in both the baseline and endline survey.

Heterogeneous Treatment Effects Table A11 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.⁵ 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.⁶ 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 A7). 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.

⁵There is an interesting parallel to Royer et al. (2015), who find that a gym attendance commitment contract has larger effects for those who are already members of the gym.

⁶It 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.

		-			
	(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

TABLE A11. HETEROGENEOUS TREATMENT EFFECTS: BANK SAVINGS

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Variables are as described in Table A6.

		(1) Bank Savings	(2) Other Savings	(3) Change in
104		0.00	252.00	Outstanding Loans
IUth	Installment Savings	0.00	252.00	-4,000.00*
Percentile		(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	Installment Savings	0.00	-271.00	-2,000.00*
Percentile		(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	Installment Savings	0.00	-150.00	-800.01**
Percentile		(0.00)	(261.67)	(394.72)
	Withdrawal Restr.	0.00	-240.00	-700.00
		(0.00)	(296.88)	(449.32)
40th	Installment Savings	0.00	0.00	0.00
Percentile		(5.45)	(53.89)	(129.39)
	Withdrawal Restr.	0.00	0.00	0.00
		(6.29)	(61.15)	(147.28)
50th	Installment Savings	0.00	0.00	0.00
Percentile		(5.23)	(97.89)	(41.80)
	Withdrawal Restr.	100.00***	56.67	0.00
		(6.03)	(111.06)	(47.58)
60th	Installment Savings	0.00	85.00	50.00
Percentile	-	(0.00)	(229.72)	(261.24)
	Withdrawal Restr.	100.00***	-135.00	-100.00
		(0.00)	(260.62)	(297.38)
70th	Installment Savings	0.00	110.00	-234.00
Percentile	C	(17.91)	(389.19)	(711.40)
	Withdrawal Restr.	100.00***	-343.44	-800.00
		(20.64)	(441.56)	(809.80)
80th	Installment Savings	200.00	-208.00	840.00
Percentile	C	(181.42)	(587.84)	(1,226.00)
	Withdrawal Restr.	150.00	-865.96	340.00
		(209.10)	(666.93)	(1,395.59)
90th	Installment Savings	2,051.87***	-635.00	925.00
Percentile	8-	(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

TABLE A12. QUANTILE REGRESSIONS

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 A12 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 3), 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 5.3).

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).





As shown in Maniadis et al. (2014), p-values are an insufficient criterion to assess the ex-post probability that a research finding represents a true association. This 'Post-Study Probability' (PSP) further depends the power of the design, the prior probability of the hypothesis, and the tolerance for false positives. Consider a simple framework where the association between a treatment T and an outcome Y is either true or false. Denoting π as the prior probability that it is true, κ as the power of the study, and α as the significance level used, the ex-post probability for a true association is

$$PSP = \frac{\kappa\pi}{\kappa\pi + \alpha(1-\pi)}$$

The PSP increases sharply in the number of successful replications, especially when priors are low (Maniadis et al. (2017)). For *r* successful replications out of *n* trials,⁷

$$PSP^{rep} = \frac{\binom{n}{r} \kappa^r (1-\kappa)^{n-r} \pi}{\binom{n}{r} \kappa^r (1-\kappa)^{n-r} \pi + \binom{n}{r} \alpha^r (1-\alpha)^{n-r} (1-\pi)}.$$

The methodology straightforwardly applies to the main experimental treatment effect of this paper: The effect of a novel commitment savings product on savings levels (see Table 3 column (1)).⁸ Table A13 estimates the PSP that a true positive relationship exists between the tested commitment products and individuals' savings levels. Note that PSPs concern only the existence of such a relationship, not their magnitude. I consider three estimates for the power κ of the study: First, the ex-ante power of 0.9 that was used to calculate the sample size (using data from other savings products). Second, the power that results from testing the null hypothesis $\gamma=0$ against the alternative hypothesis that $\gamma=429$, $SE(\gamma)=66$ (as estimated for the IS treatment, see Table 3). Third, the power that results from testing the null hypothesis $\gamma=0$ against the alternative hypothesis that $\gamma=148$, $SE(\gamma)=41$ (as estimated for the WR treatment). To the author's knowledge, no replication exists of the IS treatment at present. In contrast, the WR treatment constitutes a replication of the SEED product in Ashraf et al. (2006) (see Section 5.2 and footnote 31).

	Power = 0.9 (ex ante)	Power=0.99 (IS data)	Power=0.975	5 (WR data)
Prior π	i=0		i=0	i=1
0.01	0.15	0.17	0.16	0.79
0.02	0.27	0.29	0.28	0.89
0.05	0.49	0.51	0.51	0.95
0.10	0.67	0.69	0.68	0.98
0.20	0.82	0.83	0.83	0.99
0.35	0.91	0.91	0.91	1
0.55	0.96	0.96	0.96	1

 TABLE A13. POST-STUDY PROBABILITIES

Values in the table represent ex-post probabilities that a positive effect of commitment savings products on bank savings exists, conditional on a significant finding (as reported in column (1) of Table 3). Power = 0.99 results from testing $H_0: \gamma = 0$ against $H_1: \gamma = 429$, $SE(\gamma) = 66$ (the estimated IS effect). Power = 0.975 results from testing $H_0: \gamma = 0$ against $H_1: \gamma = 148$, $SE(\gamma) = 41$ (the estimated WR effect). The probability of a type I error is constant at $\alpha = 0.05$. *i* denotes the number of replications.

⁷This abstracts from publication bias or researcher bias. See Maniadis et al. (2017) for details on assumptions.

⁸It is less applicable to the estimated non-experimental relationships between contract choices and preference measures (Tables A7–A8), which represent correlations.

V. Robustness Checks

This section tests robustness along several dimensions. Table A14 verifies that the estimation of average treatment effects is robust to the inclusion of unbalanced covariates (see Table I). Table A15 tests robustness of the take-up and default regressions of Section 5.4 with respect to the measurement of sophistication. As outlined in Section 3.3, sophistication is measured by interacting observed time-inconsistency (in MPLs) with a measure of *perceived* time-inconsistency. While the main text uses *Perceived Temptation* to capture perception of time-inconsistency, the question design allows for a second measure, *Perceived Self-Control*. This measure is briefly discussed in footnote 23 in the main text. The full question text as well as a discussion can be found in Appendix VI.2.

Columns (4) and (8) of Table A15 use *Perceived Self-Control* to capture perception of time-inconsistency, and thus *Pres.Bias*Self-Control* as a measure of sophisticated time-inconsistency. 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*. As discussed in Appendix VI.2, the possibility of image concerns means that individuals may not have been comfortable to admit their self-control problems to the interviewer.

Table A16 looks at the effect of using real incentives instead of hypothetical questions in the measurement of time-inconsistency. Section VI.1 outlines the multiple price list method which was used to elicit individuals' time preferences, and Appendix VI describes the randomisation of real incentives. 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 A16 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 5.4 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 A16 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 A17 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 $P(presentbias_{it} = 1) = \Phi(\alpha + \beta * real_{it} + \mu_i)$, where μ_i is assumed to be random.

AVERAGE TREATMENT EFFECTS - ROBUSTNESS TO UNBALANCED COVARIATES	Bank Savings Other Savings Change in Outstanding Loans	1) (2) (3) (4) (5) (6)	33*** 423.3*** 426.811 298.4 -840.258 -1,125	.587) (64.96) (671.844) (704.6) (1,180.168) (1,205)	43*** 131.0*** -328.159 -528.3 -308.549 -712.8	.927) (42.26) (705.461) (719.9) (1,258.139) (1,287)	-4.527 107.0 -443.3**	(14.98) (108.3) (225.6)	32.52* -81.20 341.4	(16.80) (132.5) (227.3)	1.218 -385.7** -66.43	(7.156) (190.1) (251.4)	60*** -85.97 63.451 1,004 1,882.729** 3,211*	399) (97.68) (531.028) (1,096) (920.828) (1,762)	0.04 0.044 0.00 0.032 0.00 0.01	
14. AVERAGE TREATMENT E)	Bank Savings	(1) (2)	428.633*** 423.3***	(65.587) (64.96)	148.243*** 131.0***	(40.927) (42.26)	-4.527	(14.98)	32.52*	(16.80)	1.218	(7.156)	27.160*** -85.97	(97.68) (97.68)	0.04 0.044	745 773
TABLE A	Dependent	Variable	Installment Savings	Treatment	Withdrawal Restr.	Treatment	No. of HH members		Risk Aversion		Weekly HH Income	(in thousand pesos)	Constant		R ²	Observations

							(
Dependent		IS Take-U	b		IS Defaul	lt (within all assig	ned to IS group)	
Variable	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(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)	(0.007)	(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 parent	heses, *** p<0.01, *	** p<0.05, * p<0.1	. Entries in the table	represent the margin	al coefficients of the c	corresponding probit	regression. In colu	mns (3) and (7),
the set of control variables has b	een limited to age ar	nd gender (all other	r control variables m	ay directly or indirec	tly represent choice va	ariables). Columns (2) and (6) omit the	interaction term
tempted-ideal*presentbias ("Pres.Bias*Temptat	ion"), which is use	ed as a measure of s	ophisticated time-inc	consistency. Columns	(4) and (8) instead	use <i>expected</i> -ide	al* presentbias
("Pres.Bias*Self-Control") as a	in alternative measur	re of sophistication	n. See Section VI.2	for a detailed descrip	ption of the sophistica	tion measures.		

TABLE A15. INSTALLMENT SAVINGS TAKE-UP & DEFAULT: ROBUSTNESS (SOPHISTICATION MEASURE)

TABLE A1	16. IS TAKE-UP	& Default: Rof	BUSTNESS (REA	L VS. HYPOTHEI	TICAL INCENTIVE	S)
Dependent		IS Take-Up		IS Default	(within all assigned	to IS group)
Variable	(1) Aggregate	(2) Hypothetical	(3) Real	(4) Aggregate	(5) Hypothetical	(6) Real
		Questions	Incentives		Questions	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	-0.008	-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 pare	entheses, *** p<0.01	, ** p<0.05, * p<0.1.	Entries in the table	represent the margir	all coefficients of the	corresponding probit
regression. Variables are as de	efined in Table I. Colu	umns (2) and (5) regre	ss the dependent var	riable on unincentivis	ed measures of present	t bias and impatience
for the entire sample. Column	ns (3) and (6) restrict	the analysis to the real	l-rewards sample, an	id use only incentivise	ed measures of present	t bias and impatience.
Columns (1) and (4) use incen	ntivised measures whe	re available, and hypot	thetical measures of	nerwise. They are ider	ntical to Columns (1) a	nd (2) of Table IV.

INCENTIV
HYPOTHETICAL
(REAL VS.
ROBUSTNESS
& DEFAULT:
IS TAKE-UP
TABLE A16.

The results suggest that monetary incentives may *decrease* the occurrence of time-inconsistency: Individuals were (insignificantly) less likely to exhibit present bias, and significantly less likely to exhibit future bias with an equal-size increase in the probability of general impatience (always choosing the earlier reward). The between-individual analysis confirms the sign of this effect (less time-inconsistency, more impatience), but remains statistically insignificant. In combination with the strong predictive power for commitment take-up and default observed in Table A16, these results are consistent with the idea that incentivising survey questions reduces noise and improves the quality of the answers.

A. BET	ween-Individual Co	OMPARISON (CROSS SEC	TION)
Dependent Variable	Present Bias	Future Bias	Impatience
Real Incentives	-0.0264	-0.0117	0.0253
	(0.0250)	(0.0264)	(0.0322)
Mean Dep. Variable	0.166	0.189	0.357
Observations	882	882	882

TABLE A17. REAL VS. HYPOTHETICAL INCENTIVES

B. WITHIN-INDIVIDUAL COMPARISON (PANEL DATA)

		· · · · · · · · · · · · · · · · · · ·
Present Bias	Future Bias	Impatience
-0.0163	-0.0714***	0.0719***
(0.0223)	(0.0244)	(0.0191)
0.161	0.219	0.338
462	462	462
903	903	903
	Present Bias -0.0163 (0.0223) 0.161 462 903	Present Bias Future Bias -0.0163 -0.0714*** (0.0223) (0.0244) 0.161 0.219 462 462 903 903

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).

VI. Survey Measurement

VI.1. Time Preference Measurement

In the context of a comprehensive baseline survey, I measured time-inconsistent preferences using multiple price lists (MPLs): Individuals were asked to choose between a fixed monetary reward in one period and various larger rewards in a later period. After six questions using a near time frame (now versus one month), the same six questions were asked for a future time frame (one month versus two months). The outcome of interest was the size of the later reward necessary to make the individual switch from preferring the (smaller) earlier reward. Consider the following sample questions:

- 1. Would you prefer to receive P200 guaranteed today, or P250 guaranteed in 1 month?
- 2. Would you prefer to receive P200 guaranteed in 1 month, or P250 guaranteed in 2 months?

The earlier reward was kept constant at 200 pesos, while the later reward gradually increased from 180 to 300 pesos. Exponential discounters will be time-consistent – i.e., the amount necessary to make them switch from the earlier to the later reward will be the same in both time frames. I identify as hyperbolic discounter (or 'present biased') those who put a higher premium on waiting for one month in the present than in the future. Individuals who exhibit more patience now than in the future are 'future biased'. An individual who always prefers the earlier reward in all questions and time frames is classified as 'impatient'. The two sets of questions were separated by at least 15 minutes of other survey questions in order to prevent anchoring.

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. 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 was given a survey which contained both the hypothetical question module and the real rewards module. If the calculated number was even, the respondent was given a survey containing only the hypothetical question module.⁹ Individuals were not aware of this randomisation, i.e., they did not know that the hypothetical questions would be followed by a real-rewards module. However, the nature of rewards was always transparent at the time of asking the questions (see Figure A6).

To determine payouts, one question was randomly selected using draws of numbered ping pong balls from a black bag. Payments in the present were made in cash at the end of the survey. Payments in the future were made using post-dated bank cheques from the partner bank (which was well-known in the area, and centrally located). While many stores directly accepted these cheques as payment (and participants frequently used them as such), there remains an asymmetry in the transaction cost of cashing present and future payments. Such transaction costs could lead to an upward bias of the present bias measure: Individuals are more likely to prefer the early (cash) payment in the near time frame, while the future time frame (one month versus two months) is unaffected. The

⁹The 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.

same logic applies in case there was any uncertainty about whether future payments would be guaranteed.¹⁰ A possible resulting upward bias on the present bias measure cannot be ruled out. However, note that Table A17 suggests a *negative* (though insignificant) association of real rewards with present bias. In addition, the estimates of present bias are well in line with those commonly found in the literature (see footnote 21 in the main paper).

Aggregating incentivized (where available) and unincentivized (otherwise) measures, I find 16.6 percent of individuals to be present-biased, 18.9 percent future-biased, and 35.7 percent impatient. 3.4 percent of observations are set to missing due to multiple switches in the MPL choices.

Instructions: I will now ask you questions that are very similar to the questions about the barangay lottery, which I had asked you earlier. However, this time is different: I will actually pay out one of your choices to you, so you will actually get the money. Therefore, you should really think about your answers. I will ask you some questions now, and some more later on. For each question, we will put a ping-pong ball with the question number in a bag. (SHOW THE RESPONDENT THE 12 PINGPONG BALLS WITH 12 NUMBERS, AND THE BAG). At the very end of this survey, when we are finished, I will ask you to pull one of the pingpong balls out of the bag without looking. The number on this pingpong ball will give us the number of the question 4. All questions will ask you whether you want P200 now, or a different amount of money in 1 month. For the question that is selected, if your choice was P200 now, then I will give you P200 at the end of the survey. If your choice in this question was an amount in 1 month, then I will give you an official IPA cheque (SHOW IPA CHEQUE) that is post-dated to 1 month from now. In one month, you can take this cheque to 1st Valley Bank, and they will hand out the money to you in cash. This is guaranteed, you only need to bring your ID and the cheque. You cannot do this earlier than 1 month from now. Do you have any questions on this before I start?

I will now start the questions. Since you don't know yet which pingpong ball you will get in the end, each of the questions below may be the one that you receive in the end. Hence, you should make all of your choices as if you are going to get each reward. So please really think about which reward you prefer for each question.

1	B15. Do you prefer to get P200 guaranteed today, or to get P180 guaranteed 1 month from now?	P200 today
2	B16. Would you prefer to receive P200 guaranteed today, or P200 guaranteed in 1 month?	P200 today
3	B17. What if it was P200 guaranteed today, or P220 guaranteed in 1 month?	P200 today1 P220 in 1 month
4	B18. Would you prefer to receive P200 guaranteed today, or P250 guaranteed in 1 month?	P200 today
5	B19. Would you prefer to receive P200 guaranteed today, or P280 guaranteed in 1 month?	P200 today
6	B20. What if it was P200 guaranteed today, or P300 guaranteed in 1 month?	P200 today

FIGURE A6. MULTIPLE PRICE LIST (NEAR-FRAME, REAL REWARDS)

VI.2. Measuring Sophistication About Time Preferences

In addition to a measure of preference reversals, the analysis requires a measure of sophistication which is not in itself derived from commitment demand (see the discussion in Section 3.3). I develop a survey-based proxy measure, adapted from the self-control measure proposed by Ameriks et al. (2007) (henceforth ACLT). ACLT infer sophistication from predictions about future temptations and behaviour. Simple hypothetical survey questions elicit individuals' ideal, tempted, and expected allocation of a fixed resource over time. While designed to identify the

¹⁰To minimize such uncertainty, both cash and official post-dated bank cheques were presented during the game.

parameters of the Gul and Pesendorfer (2001) model, the resulting measure reflects an individual's *perceived* (rather than actual) self-control problems. The actual wording of the questions is shown in Figure A7.

H12. Su receive Eastland and drin used im within tw	ppose that you win 10 restaurant certificates, each of which can be used (once) to a "dream restaurant night" at any restaurant you want, such as Garahe, Kamayan, Jollibee or Gingoog d Hotel. On each such night, you and a companion will get the best table and an unlimited budget for food k. There will be no cost to you: all payments including tips are part of the prize. The certificates can be mediately, starting tonight, and it is guaranteed that every restaurant will honor them if they are used to years. However, any certificates that are not used up within this two year period become valueless.	# of certificates (1-10)
H12a	Think about what would be the <i>ideal</i> allocation of these certificates for the first and the second year. From your current perspective, how many of the ten certificates would you <i>ideally</i> like to use in year 1 as opposed to year 2? (Choose number 1-10)	
H12b	Some people might be tempted to depart from this ideal allocation. For example, there might be temptation to use up the certificates sooner, and not keep enough for the second year. Or you might be tempted to keep too many for the second year. If you just gave in to your temptation, how many would you use in the first year?	
H12c	Think about both the ideal and the temptation. Based on your most accurate forecast of how you would <i>actually</i> behave, how many of the nights would you end up using in year 1 as opposed to year 2?	

FIGURE A7. ACLT SELF-CONTROL QUESTIONS (ADAPTED TO CONTEXT)

These questions provide two important measures: *Perceived Temptation* (from (2)-(1), *tempted-ideal*) and *Perceived Self-Control* (from (3)-(1), *expected-ideal*).¹¹ They were designed for the Gul-Pesendorfer model. Using the $\beta\delta$ -model (where costly self-control does not exist), the two measures should be the same: Both capture the difference between the ex-ante optimal allocation, and the allocation the agent expects to play in a subgame perfect equilibrium. This difference is a function of perceived time-inconsistency ($\hat{\beta}$ in Section 2).

To capture sophistication about present-biased preferences (rather than costly self-control or noise), I interact either of the two measures with an indicator for behaving time-inconsistently in MPLs. The resulting interactions (tempted - ideal) * presentbias and (expected - ideal) * presentbias both proxy $\hat{\beta}$ for agents with $\beta < 1$ (note the assumption $\beta \leq \hat{\beta} \leq 1$ in Section 2).¹² The choice to use (tempted - ideal) * presentbias throughout the main text is motivated by one theoretical and one practical consideration: First, suppose costly self-control does exist, and tempted - ideal differs from expected - ideal. It is tempted - ideal which determines the demand for commitment: While a commitment device may not affect the allocation consumed, it increases utility by removing temptation. Second, a more practical difference between tempted - ideal and expected - ideal emerges in the presence of image concerns: Admitting to an interviewer that one is subject to temptation may be easier than admitting that one gives in to these temptations. Consistent with this conjecture, 82 percent claimed positive differences between tempted and ideal, while only 22 percent admit differences between expected and ideal. If this gap was due to costly self-control, rather than to image concerns, then (expected - ideal) * presentbias rather than (tempted - ideal) * presentbias should predict default on the IS account. Table A15 shows that the opposite is true in the data.

¹¹Questions 1, 2 and 3 correspond to questions (a), (c) and (d) in ACLT, respectively. I censor values of temptation and self-control at zero. I interpret observed negative values as measuring something other than temptation and self-control – e.g., not having time to go to restaurants as often as individuals would ideally like. Negative values occurred in 4 (42) out of 910 cases for temptation (self-control).

¹²The interactions assume domain-generality of β across money and consumption.



FIGURE A8. HISTOGRAM OF RESPONSES TO ACLT SELF-CONTROL QUESTIONS

The left figure plots a histogram of *Perceived Temptation*, derived as *tempted-ideal* from the ACLT questions. The right figure plots a histogram of *Perceived Self-Control*, derived as *expected-ideal*. Negative values occurred in 4 (42) out of 910 cases for temptation (self-control), and are censored at zero.





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.

VI.3. Other Baseline Measures

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 A10). 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 A11 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 decisionmaker 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 unsurprisingly 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).



FIGURE A10. TEST OF RISK AVERSION (METHODOLOGY: BINSWANGER (1980))



FIGURE A11. ILLUSTRATION: TEST OF COGNITIVE ABILITY

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