

Mental Accounting and Savings Decisions of the Poor

Pre-Analysis Plan

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Abstract

This document describes the planned analyses for the lab-in-the-field experiment assessing the impact of cash-in-hand on savings decisions. Between February and March 2017, 300 microfinance clients took part in paid, individual interviews. All clients were informed one week in advance that at the next weekly (borrower) center meeting, a randomly drawn subsample would be invited to take part in interviews that would pay ₱300 (\$6). During the interview, clients were asked whether they wanted to save some of their payment in their existing savings account. The study uses a 2x2 design with two levels of randomization. First, cash-in-hand was randomized at the individual level: 50 percent of the participants received their show-up fee at the very beginning of the survey and before making the savings decisions whereas the other half received the remainder of the cash they did not save directly after the decision. Second, at the day of the interviews, in half of the 31 centers a participation fee of ₱500 (\$10) was announced after the recruitment of volunteers but before the start of the interviews. The present document describes the outcome variables and econometric methods that I will employ to assess the effect of cash possessions on savings decisions.

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1 Motivation

“Cash creates needs” or “once I receive it, I have already spent it” are some explanations poor people give for why they do not save more, even though they would like to. While it has been established that even poor people would have the capacity to save (Banerjee and Duflo 2007), it remains a puzzle why undersavings occur. Savings would have the same benefits as the widely used loans, such as insurance against adverse income shocks and financing for both human and capital investments, and would even come at a lower cost. Indeed, several studies have shown that even small amounts of savings can have large positive impacts on live outcomes (Ashraf et al. 2006, 2010; Brune et al. 2015; Dupas, Green, et al. 2016; Dupas and Robinson 2013; Prina 2015). So, why don’t the poor save more? Recent research in this active field has suggested many potentially relevant frictions that can be broadly classified into five categories (Karlan, Ratan, et al. 2014): Transaction costs, lack of trust and regulatory barriers, information and knowledge gaps, social constraints and behavioral biases. In the last category, self-control problems and inattention have received most of the scholarly attention.

While take-up of products designed to increase savings, for instance commitment savings accounts, is rather low (between 20-40%), usage rates remain even lower (often measured in “at least one deposit during the study period”). Similarly, reminders have some positive effects but do not massively increase savings (e.g. Karlan, McConnell, et al. 2016 find that reminders increase savings by 6% in a very large sample).

If cash itself in combination with mental accounting constitutes the problem, this might explain why usage rates remain so low: receiving income in cash, individuals need to deposit this cash into their accounts, irrespective of commitment accounts or receiving reminders. Even if their marginal willingness to save is increased by the intervention, the ‘painful’ act of handing over the money might still outweigh the intentions.¹

2 Idea and related literature

With this research, I test for the existence of an endowment effect for cash that inhibits people from saving. More specifically, the hypothesis is that having received cash and holding on to it makes it harder to hand it back to e.g. the savings officer that will deposit the savings into the account.

¹The literature on spending decisions has established that with cash, the ‘pain of paying’ is particularly pronounced (see e.g. Prelec and Loewenstein 1998).

While it has been suggested that loss aversion plays a role in savings decision (e.g. Karlan, Ratan, et al. 2014; Thaler and Benartzi 2004), a corresponding theory or an empirical test is still lacking. I experimentally examine the endowment effect which can theoretically be inter alia explained by a combination of mental accounting and loss aversion (Tversky and Daniel Kahneman 1991) and thereby contribute to the literature on undersavings (for a recent survey see e.g. Karlan, Ratan, et al. 2014).²

The idea that cash might be subject to an endowment effect is related to a literature in marketing that has established that consumer spending is lower when payments are made in cash as the feeling of money departing is felt more vividly, increase the 'pain of paying' (Prelec and Simester 2001; Soman 2003; Thaler 1990). Closely related, the endowment effect literature has shown that physical presence of a good and the duration of ownership are positively related to the strength of the endowment effect (Bushong et al. 2010; Strahilevitz and Loewenstein 1998). While the endowment effect has been tested with many different goods, evidence regarding cash is rare as a WTP-WTA test in the lab seems highly artificial. To the best of my knowledge, two early contributions test the endowment effect for money or closely related concepts: Kahneman et al. (1990) find no endowment effect for 'value tokens', but Knetsch and Sinden (1984) demonstrate that part of the WTP-WTA gap is explained by subjects refusing to give up cash endowments.

This project is further related to research on default effects in savings. Thaler and Benartzi 2004 use a combination of status quo bias, present bias and loss aversion and design a powerful tool to increase retirement savings in the US. While the product is very successful in increasing savings, the authors are not able to disentangle the different mechanisms at work. Moreover, the product relies on bank transfers, which makes it hard to apply in developing countries where many people manage their monetary flows in cash.

More closely related to my setting, Somville and Vandewalle (2016) compare savings in developing countries when income is deposited into a bank account vs when it is paid in cash and they find that savings are indeed higher in the former case. Yet, they are not able to distinguish between a default effect (money in the account was automatically counted as savings and no active savings decision is required) and the endowment effect.³

²Note that a combination of mental accounting and a particular version of salience (Bordalo et al. 2012) could also explain the effect. This project focuses in on cleanly measuring the endowment effect of cash and leaves the documentation of underlying mechanisms to future research.

³In addition, as they acknowledge, other confounds such as perceived transaction costs cannot be accounted for.

3 Experiments

As there are many potential explanations for undersaving, a controlled setting is necessary to isolate and causally estimate the endowment effect of cash on savings. However, a pure lab setting with elicitation of willingness to pay and willingness to accept for cash seems overly artificial. I therefore examine savings as a natural decision that can be influenced by endowments and use a mixture of a controlled environment and a field setting, in which I observe real savings decision of poor people and can simultaneously randomly assign cash holdings at the time of decision making while holding other potential confounds constant.

3.1 Setting

To abstract from hassle costs of opening a savings account, I work with clients of the Filipino microfinance organization Ahon Sa Hiras who all have a savings account that offers 4% interest if the balance is at least ₱500 over the duration of 12 months. Clients can deposit or withdraw savings with the loan officer during the weekly center meetings that all clients are supposed to attend.⁴ The majority of clients is self-employed and earns their income in cash. While mobile banking has progressed significantly in other countries, its coverage in the Philippines remains quite low (Harigaya 2016) and this is unlikely to change soon. The Philippines thus constitute a relevant setting to conduct such a test.

3.2 Design

One week before a session takes place in a given center, all clients receive an announcement letter, informing them about the possibility to take part in paid individual interviews that will earn at least 300 pesos. The session then takes place at the announced day during the weekly center meeting. At the beginning of each session, the purpose of the interviews, research about financial decision making, is explained and clients can volunteer to participate. From the pool of all volunteers 10 participants are selected by a publicly drawn lottery. Participants provide oral consent and are subsequently interviewed one-on-one by a local research assistant.

The first part of the interview consists of questions regarding personal characteristics, the composition of the household, its financial situation and personal business activities. At the end of the first part, participants are asked whether they want to save (a part of) their participation fee

⁴Clients are organized in borrower groups of five. 2-8 groups form a center and meet weekly to conduct all transactions in cash.

in their savings account and if so, how much. Before making the decision, it is explained that savings will be matched with 20% after one month that will be added to the savings account. The second part consists of an incentivized elicitation of risk and time preferences as well as loss aversion via multiple price lists and lotteries visualized as urns. The third part includes survey questions regarding savings behavior and financial literacy that have not been included in the first part to avoid priming effects.

Two treatments are implemented in a 2x2 design: 'cash-in-hand' vs control and normal (₱ 300) vs surprise (₱ 500) participation fee. The 'cash-in-hand' treatment variation consists of paying the remuneration at the beginning of the first part or after the savings decision and is randomized on the individual level (and balanced across enumerators). Participants in the treatment hold onto the cash during the first part. Based on endowment experiments regarding goods, it is expected that physical possession (Bushong et al. 2010) and the duration of 'ownership' (Strahilevitz and Loewenstein 1998) create an endowment effect w.r.t. the money. Treated participants might thus have a harder time handing the money back to the interviewer who will give it to the loan officer at the end of the interview. In contrast, participants in the control group make the savings decision without holding the money in their hands, but knowing that they will receive the remainder of their participation fee just after the savings decision.

The amount of the participation fee is randomized on center (i.e. session) level and is only announced after the recruitment to avoid selection effects. While the normal participation fee might already have entered the participants' budget plans, the additional ₱ 200 in the surprise treatment should be treated as a true windfall gain.⁵

The second part consists of an incentivized elicitation of risk and time preferences as well as loss aversion.⁶ Risk and loss preferences are elicited with lottery visualizations and short multiple price lists elicit time preferences. Blocks of preferences (risk, time I, time II, time III and loss) would appear in random order, but without two blocks of time preference questions directly following each other. The third part includes survey questions regarding savings behavior and financial literacy.

Relying on individual level randomization, in combination with this particular setting, this design allows to rule out other potential explanations for undersavings and to cleanly estimate

⁵Note that I cannot disentangle income effect in terms of size and the surprise effect. The needed treatment would have been an announcement of the ₱ 500, which in turn might have induced selection effects. To rule out this selection, I decided for the present design.

⁶With a 1/6 chance, participants would receive the payouts from one of the experimental decisions at the very end of the survey.

Cash in Hand Treatment	Control
Recruitment	Recruitment
Announcement of amount (₱ 300/500)	Announcement of amount (₱ 300/500)
<i>Receive cash</i>	
Part 1: General Survey	Part 1: General Survey
Savings Decision	Savings Decision
	<i>Receive remaining money in cash</i>
Part 2: Preference elicitation	Part 2: Preference elicitation
Part 3: Savings survey	Part 3: Savings survey

Table 1: Timing during Sessions

the cash-in-hand effect in savings decisions.⁷ The division into several parts is necessary for two reasons: first, to be able to control for potential spillover effects from the treatment onto the belief elicitation measures. While I cannot rule out a spillover effect from the treatment (i.e. subjects in the treatment saving less, they are richer in part 2 and this might alter their decisions), I will be able to control for the exact amount subjects hold in cash during the preference elicitation and test whether this has an impact on choices. Second, I would like to keep priming via survey questions in the first part as low as possible, as I am interested in the effect of cash in hand and not the interaction of cash in hand and priming. Therefore, all savings related questions will be asked only in part three. These questions will help me to address other potential explanations for undersavings.

3.3 Implementation

300 clients were interviewed in 31 different centers in semi-urban and rural areas of the Laguna Province on the main island in the Philippines, Luzon. Center meetings take place Monday - Thursday and usually start either at 9am or at 1pm, resulting in two sessions per day from February 20 - March 16. Each center was visited again four weeks after the initial session to pay the match to the savings decision if applicable.

A team of five local enumerators has been trained to conduct the individual interviews on Surface Pro tablets using z-tree (Fischbacher 2007). All questions and instructions have been translated into the local language, Tagalog, and piloted before the start of the experiments.

The study was approved by the Ethics Commission, Department of Economics, University of

⁷All questions in the first part, before everyone has received the (remainder of the) show-up fee, can potentially be influenced by the cash-in-hand treatment manipulation. I will make use of this feature as a manipulation check, as explained in subsection 5.6.

Munich (project 2016-13) and is registered in the AEA RCT Registry (Spantig 2017).

3.4 Sampling

Three branches of the partner MFI were selected based on their regional coverage. Within each branch, centers have been selected based on meeting and travel times. Centers were randomly chosen before the start of all sessions to be a normal or a high participation fee center. Within each center, all volunteers entered a lottery that would determine whether they could participate, and if so, in which treatment and by whom they would be interviewed.⁸

3.5 Power Calculations

The target number of interviewees is 300, which allows a detection of a 10 percentage point difference in mean share of endowment saved at the 5 percent significance level and with a power of 80 percent. Comparing these effects for normal and surprise participation fee separately can be done with a power of 50 percent.

4 Research Questions

My main questions are: (i) Does receiving cash before making the savings decision reduce the amount saved? (ii) Is the effect different for higher than previously announced endowments?

5 Econometric Specifications

5.1 Data Sources

In addition to data collected during the sessions, I will have access to administrative savings data consisting of weekly deposits and withdrawals, as well as some basic demographics, including a poverty measure, collected at the time of the last loan application. These data can date back to 2012, depending on the length of individual membership and they cover all clients in the three branches from which the centers have been selected.

⁸Randomization was done prior to the start of all sessions in STATA and the randomization protocol was implemented by myself.

5.2 Non-Parametric Tests

In a first step, I will analyze all decisions in the different participation fee treatments jointly, thus pooling decision in CiH-300 and CiH-500 as well decisions in control-300 and control-500. To standardize choices, I will examine the share of the endowment saved (in percent). First, I will use the Kolmogorov-Smirnov test to assess whether the distribution of choices differs across treatment and control to determine the correct test for differences in means of the control and treatment group. If the distributions do not differ, I can use Wilcoxon's ranksum test to compare the means between treatment and control, otherwise I will use the Fligner-Policello test.⁹

5.3 Parametric Specification

While randomization allows for non-parametric tests without controlling for covariates, precision of the treatment effect could be increased when heterogeneity of participants is controlled for. The basic treatment effect specification to capture the endowment effect is:

$$y_i = \beta_0 + \beta_1 CiH_i + \epsilon_i \quad (1)$$

where y_i is the outcome of interest for an individual, CiH_i is a treatment indicator that takes value 1 for individuals who received the participation fee at the beginning of the interview ('cash in hand treated individuals') and 0 otherwise, and ϵ_i is an idiosyncratic error term.¹⁰ The omitted category are individuals who received cash after making the savings decision. β_1 thus estimates the treatment effect for treated individuals relative to control individuals.

A richer specification explores the size of the endowment and its interaction with the CiH treatment:

$$y_i = \beta_0 + \beta_1 CiH_i + \beta_2 500_c + \beta_3 CiH_i * 500_c + \epsilon_i \quad (2)$$

where c denotes variation on the center level, 500_c is an indicator that takes value 1 for centers in which ₱ 500 were paid for an interview. Centers in which the participation fee equaled the announced endowment of ₱ 300 are thus the omitted category. To improve precision, both specification will also be estimated including a vector of control variables: age, years of education, financial literacy, household size, indicator for whether the participant has her own

⁹While Wilcoxon's test assumes that the distributions are the same and only differ w.r.t. a shift, the Fligner-Policello test relaxes this assumption.

¹⁰I will use robust standard errors clustered at the center level, as this is the highest level of randomization (for the size of endowment).

business, assets, total household income, having money left at the end of the week, household decision making, cost and time associated with reaching the center meeting hall and interviewer fixed effects.¹¹

5.4 Treatment Effect Heterogeneity

It is conceivable that some groups are more vulnerable to the endowment effect of cash. While some hypotheses can be formed regarding treatment effect heterogeneity (see below), repeatedly subsetting the data leads to multiple inference problems and implicitly acknowledges that the current model is misspecified. I will therefore use LASSOplus, a Bayesian method to identify relevant subgroups (Ratkovic and Tingley 2017). The estimator both estimates and selects relevant treatment effects. It considers all lower order and interaction terms and returns the ones estimated to be different from zero. As shown by a simulation experiment in Ratkovic and Tingley (2017), LASSOplus estimates are conservative but powerful in identifying non-zero effects. As this estimation is post-inferential, it *describes* effects that are apparent in my data and can therewith point to interesting questions to be considered in future research.

While LASSOplus will let the data speak, I do have some ex-ante hypotheses which subgroups might be more prone to the endowment effect of cash:

- Subjects with lower cognitive abilities are more likely to violate fungibility (Abeler and Marklein 2016) and might thus also be more likely to exhibit the endowment effect for cash
 - Relevant variable: level of education (survey question)
- Loss averse individuals should be more likely to respond to the treatment if the effect is driven by loss aversion¹²
 - Relevant variable: loss aversion (experimental measure)
- Subjects with higher usage of their savings account are less likely to respond to the treatment as experience can reduce the endowment effect (Gine and Goldberg 2016)
 - Relevant variable: savings in account (survey questions & admin data)

¹¹All interviewers conducted the same number of interviews in treatment and control.

¹²Loss aversion is still cited as one of the main explanations of the endowment effect, c.g. Karlan, Ratan, et al. (2014).

5.5 Experimental Savings Decision and Administrative Savings Data

The administrative data will be used for the following

- Based on the total amount of savings in the account at the time of the interview and the frequency and size of the deposits in the last three months, I will assess how representative the savings decision in this more artificial environment (potential interviewer demand effects and windfall gain rather than regular income) is for my sample.
- Using data from all clients of the three branches, I will try to classify clients w.r.t. their savings behavior. The classification will be based on amounts and regularity of deposits: regular (weekly or biweekly) vs irregular deposits, high vs small amounts and same size vs different size amounts. Using experimental preference measures, I will try to predict this classification. In turn, the classification will be used to predict whether clients are eligible for the match of their savings during the interview, whether they withdraw the respective amount once the match has been paid and whether the windfall gain has a lasting impact on their savings in the account. I will also try to link different savings goals to the saver types.

5.6 Manipulation Check

While the treatment effect in my experiment is a lower bound of the effect in reality,¹³ receiving cash should make participants feel richer. To establish that the manipulation has worked, I will test the differences in perceived wealth between treatment and control using questions from part 1. While I expect that most income is received in cash, the treatment should above all alter perceived wealth in income that is received irregularly and in cash. In contrast, I should not find differences in more objective, less fungible wealth as measured by the asset index. In addition, anecdotal evidence for the mechanism will come from the survey question what participants would do with a ₱ 3000 windfall gain.

5.7 Robustness Checks

- *Alternative Explanations:* The treatment might increase trust in the interviewers and the procedures as participants have received the money already which might increase credi-

¹³Note that participants hold the cash for about 20 minutes before making the savings decision, which is comparable to possession times in other experiments. Interaction with other biases, however, have largely been excluded by design. For instance, impatience should be less relevant than in reality as the money cannot be spent on anything during the experiment.

bility further.¹⁴ Thus, participants in control might be less likely to believe that they will receive the money. While it is unclear which effect this should have on the savings decision, it might increase variance in the decisions. I will therefore test whether the variance in treatment and control are the same.

- *Confounding Effects*: While many other explanations exist for undersavings, in this setting they should not matter. There should thus be no differential treatment effects w.r.t. trust in banks, time preferences, risk preferences and transaction costs (distance to center meeting).
- *Representativeness*: Using administrative data from all clients of the three branches, I will be able to assess the representativeness of my sample. In addition, some survey modules have been used that have previously been administered in representative samples in the Philippines that will also allow for checks of representativeness.
- *Verification of Self-Reports*: Certain survey questions can be contrasted with administrative data to assess the reliability of the self-reported data, for instance, questions from the asset index, self-reported savings, household size and age. Questions regarding the entire center, can be cross-checked with other members' answers from the same center (e.g. regarding the use of savings for loan repayment of members who cannot repay themselves).

6 Variables

6.1 Outcome Variables

Interview data

- Share of endowment saved
- Amount saved (in ₱)

Administrative data

- Classification of savers according to their behavior before the experiment
- Whether match was received (amount in account four weeks after experiment)
- Savings behavior after match has been paid

¹⁴Note that the experiment has been designed to establish trust and credibility: during the recruitment, the money to be received has already been shown to the participants. The explanation of the savings decision includes the display of vouchers for the savings match and the deposit of the savings is done in front of the loan officer which makes all actions as transparent and verifiable as possible.

6.2 Indices

Several questions from the interviews will be aggregated to indices as follows:

- Decision making: all questions regarding decision making can be answered by 'my partner' (0), 'jointly' (1) or 'myself' (2). The equally-weighted mean of the answers to these questions will be the decision making index (c.f. Ashraf et al. 2010)
- Assets: Electricity, running water, radio, television, landline, cellphone, computer, washing machine, refrigerator, CD/DVD player, bicycle/pedicab, motorcycle/tricycle, cart. The equally-weighted mean of possession indicator of each item will be the asset index.
- Financial literacy: four questions on simple division, inflation, interest and inflation taken from (World Bank 2015). The percent of questions answered correctly will be the financial literacy index.

6.3 Preferences

The incentivized elicitation of risk, loss and time preferences will be used to structurally estimate those preferences. In addition, a simple classification (without assuming a particular functional form of the utility function) will be used to compare the sample to other samples, e.g. in (Ashraf et al. 2006; John 2016).

Hypothetical questions address attention and bracketing as in Stango et al. (2017). Attention will be a combination of short and medium run attention question. Narrow bracketing is measured by two different questions, so it will be distinguished between 'no narrow bracketing', 'some narrow bracketing' and 'narrow bracketing'.

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