

MICROFINANCE AND NORMS:
INDIVIDUAL AND GROUP BEHAVIOR IN THE LAB AND IN THE
FIELD

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DISSERTATION



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Part I

PREFACE

PREFACE

The emergence of behavioral economics in the 1950s has sparked a debate whether agents are best represented by the "classical" – rational and selfish – homo economicus or by "behavioral" decision-makers subject to various biases. This debate has been long-standing and behavioral economics shifted from being a niche towards an important field within economics, documenting systematic evidence for behavioral biases and "mistakes" in decision making. This increasing importance is also mirrored in three Nobel Prizes awarded to influential behavioral researchers, Daniel Kahneman and Vernon Smith (2002), Al Roth (2012) and most recently, Richard Thaler (2017). With increasing acknowledgement of behavioral phenomena and their importance for economics, the debate has shifted from arguing in favor of either the classical or the behavioral view towards accepting the co-existence of the two. Nowadays, economists rather focus on understanding in which situations behavioral biases are economically relevant: When and where should agents be understood as behavioral and in which circumstances do neoclassical assumptions suffice to predict empirical observations?

Development economics is an especially relevant field in which to investigate this question for two main reasons. First, given generally lower levels of income and available resources, the same biases or mistakes have relatively more severe consequences. Second, recent evidence suggests that scarcity impairs decision making by limiting cognitive function or focusing attention on one issue while ignoring others (Mani et al., 2013; Shah et al., 2012). This could imply more severe or additional biases for poor decision-makers.

Within development economics, microfinance offers exciting opportunities for reducing poverty, advancing social change and enlarging markets. The provision of financial services (credit, saving and insurance) to the poor was celebrated as a first step toward financial inclusion. In particular, microcredit was thought to provide a new tool to overcome market failures related to inter alia

high transaction costs and problems in contract enforcement and was even termed "revolutionary" by e.g. M. S. Robinson (2001). The concept was widely embraced, which led to annual growth rates in terms of clients of on average 30 percent between 1997 and 2007 (Armendáriz and Morduch, 2010) and culminated in the Nobel Peace Prize for Muhammad Yunus and the Grameen Bank in 2006 for their pioneering work. Only more recently, the impact of microfinance products was rigorously evaluated via randomized controlled trials (RCTs). These RCTs, however, revealed rather modest effects: Microcredit appears to have a somewhat positive but no transformative impact (Banerjee et al., 2015a).¹ Microsavings, on the other hand, seem to have more promising effects (Karlan et al., 2014). However, it is still not fully understood why savings rates in developing countries remain low. While several of the constraints to saving are "classical" in nature, such as transaction costs or liquidity constraints, more recent studies have found that behavioral biases play an important role as well.

This dissertation investigates both aspects, microsavings and microcredit. Chapter 1 contributes to understanding "undersaving", exploring a widely observed behavioral bias in this new setting. To this end, I study how participants make an individual savings decision. Chapter 2 implements a credit repayment game to examine specific design features of microcredit products as a first step to developing innovative and more effective products. Although choices are also taken individually in this study, the contractual structures we test include a group component. Some repayment decisions can impose externalities on others' payoffs and participants can punish each other for their (non-)repayment choices. For both credit repayment and sanctioning decisions we provide evidence that norms guide behavior. As unwritten codes of conduct, norms are an essential determinant of behavior, not only in developing countries, where typically weaker institutions require more informal guidance on behavior. Given the multitude of situations in which decisions are not taken by one individual but teams or groups, the interaction of groups and norms is an important line of research, especially since groups allow learning about

¹ Initially, microcredit was thought to enable poor individuals to become successful self-entrepreneurs, creating jobs for others and ultimately lifting themselves out of poverty, which would "transform" their lives.

(and eventually changing) a norm. While Chapter 2 has a group component but no interaction of groups, Chapter 3 explicitly studies the norm of honesty and how it relates to group and individual decision making. It focuses on different mechanisms and incentive structures within groups by investigating communication and how group interaction shapes the perceived strength of the honesty norm. In contrast to Chapter 2, norms can be expressed in the group interaction, but cannot be enforced or punished. Studying questions in microfinance and norms, this dissertation thus gradually increases the focus on group decision making, ranging from a purely individual decision in Chapter 1 to studying processes inherent to group interactions in Chapter 3.

All chapters rely on the same methodology: experimental economics. Together with behavioral economics, experiments have become increasingly popular. Their main advantage is the exogenous assignment of treatments that creates a valid counterfactual. This, in turn, allows identifying causal relationships. In general, experiments range from abstract settings in the lab with student participants to field settings in which participants take natural decisions and are not aware of the ongoing experiment (for a classification see Harrison and List, 2004). This span is mirrored in this dissertation. It consists of i) a lab experiment conducted with students who make abstract choices that do not have a direct real-life equivalent (Chapter 3), ii) a framed field experiment in which microcredit clients play a loan repayment game (Chapter 2) and iii) a field experiment in which microfinance clients make a real-life savings decision (Chapter 1). In the lab and the framed field experiment, choices do not have consequences for real life (beyond affecting payoffs) and participants are aware of being part of an experiment. Both studies need a certain level of abstraction and control to measure otherwise hard to identify concepts such as norms and beliefs that in turn help uncover mechanisms. In comparison to the abstract lab experiment, decisions in the framed field experiment are presented using familiar terms (e.g. repayment, installment, loan term), but, as discussed in Chapter 2, not all features of the experimental choices represent actual loan repayment. The level of abstraction is further reduced in the field experiment in which participants make a familiar choice and are unaware of any treatment manipulation. Both field experiments study behavior of a sam-

ple of interest, namely microfinance clients, while the lab experiment uses a convenience sample of university students to examine a more general mechanism. This dissertation thus provides causal evidence of individual and group behavior in the lab and in the field, studying microfinance and norms.

THE ENDOWMENT EFFECT AND SAVINGS DECISIONS OF THE POOR In Chapter 1, I study formal savings that have been shown to be a promising tool to reduce poverty, increase investment and empower women. However, savings rates of the poor are usually puzzlingly low (Karlan et al., 2014). Recent explanations for this phenomenon have focused on behavioral biases, most prominently time-inconsistent decision making and inattention. Interventions to increase savings have ranged from providing commitment devices to sending reminders, but take-up and usage rates have remained mostly modest. One potentially important but so far neglected explanation is one of the best known behavioral biases, the endowment effect. The relative reluctance of individuals to give up things they have been endowed with has been shown to cause market inefficiencies and can have important implications for the design of contracts and institutions. In many developing countries, poor households receive their income in cash. Being "endowed" with cash that needs to be physically deposited into a savings account might increase the psychological costs of saving.

I experimentally study the causal effect of cash endowments on savings deposits of microfinance clients, using a simple, high-stakes natural decision with external validity. In addition, I control for potential confounds such as transaction costs and time preferences. Participants are invited to take part in paid individual interviews. During the interview, participants are asked whether they want to save (some of) their earnings for participation in their existing savings account. The main treatment is designed according to endowment effect experiments in the lab and varies between-subjects when earnings are handed over. Endowed individuals receive the cash at the very beginning of the interview and thus make the savings decision after holding on to cash and by handing back the amount they want to save. Individuals who only know

about their earnings but have not yet received the cash, verbally state their savings decision and receive the (remainder of) cash afterwards.

Cash endowments make individuals feel richer. This is reflected in higher reports of how much cash participants think they will have at the end of the day. Given a balanced sample and in particular similar wealth levels across treatments, this provides evidence that the widely used endowment manipulation from the lab also works in field settings. The feeling of being endowed, however, does not translate into lower savings, neither in absolute nor in relative terms. In this setting, cash appears to be fully fungible. This stands in stark contrast to the literature in marketing, finding lower spending levels in the presence of cash as opposed to e.g. debit cards. I provide several robustness checks that confirm the null result and discuss potential explanations for the absence of the endowment effect in this setting.

Despite studying a poor population that is often more prone to biased behavior, I do not find any differences in savings with and without the physical presence of cash. In the context of depositing the cash to be saved, the classical model seems to be sufficient to describe decisions.²

HIGH LOAN REPAYMENT AND LOW PEER PRESSURE? REPAYMENT FLEXIBILITY IN MICROFINANCE GROUP LENDING Chapter 2 is joint work with Kristina Czura and Anett John. We examine two critical features of microcredit: joint-liability and rigid repayment schedules. Joint-liability, holding a group of borrowers responsible for repayment of all loans held by group members, has been praised for leading to high repayment rates. Rigid repayment schedules, i.e. same-sized, small and frequent repayment installments, have been thought to further induce repayment discipline by establishing a repayment habit and making the repayment process less cognitively demanding. However, both features come at a cost. Rigid repayment schedules interfere with fluctuating incomes and joint-liability can induce excessive peer pressure. With these downsides in mind, flexible repayments and individual-liability have been proposed, but the two features also exhibit drawbacks. Both might

² It should be kept in mind, however, that savings decisions more generally have been shown to be affected by other biases such as time-inconsistent decision making or inattention.

lead to lower repayment rates, flexibility via a reduction in repayment morale and individual-liability by via the lack of mutual insurance and the absence of group pressure. Microcredit design thus poses two trade-offs: one between mutual insurance and low peer pressure in the liability structure and one between shock coping capacity and repayment morale in the repayment schedule.

We study whether the interaction of flexible repayment features and joint-liability allows reaping the benefits of joint-liability and flexibility – high repayment and shock coping capacity – while keeping their downsides – low repayment morale and excessive peer pressure – at bay. We implement a version of flexibility that allows deferring one repayment installment to the future. This grants self-insurance in case of an idiosyncratic shock. To keep the burden and costs of implementation low, clients decide themselves when to use flexibility. This, however, leaves room for misuse, i.e. using flexibility absent a shock and increasing immediate consumption at the cost of future insurance.

We implement framed field experiments with microcredit borrowers in the Philippines and analyze individual repayment choices over several periods. Using a 2×2 design, we observe individual choices under both liability structures, with and without flexibility. In the joint-liability treatments, we elicit punishment choices and beliefs about others' choices, which both can serve as proxies for the prevailing norm.

Although the experiment has been set up such that non-repayment would be rational in terms of payoff maximization, repayment rates are high in all treatments. This strong repayment norm is also reflected in high punishment of non-repayment absent a shock. In addition to the norm, we document excessive peer pressure in the form of punishment for an observable shock. Interacting joint-liability with repayment flexibility can halve this punishment when flexibility is used to self-insure. Moreover, joint-liability enhances the responsible use of flexibility. However, repayment rates are lower with flexibility, independent of the liability scheme. When simulating default rates by adding shock realizations and partner matching to individual decisions, we confirm that flexibility has an insurance value, especially in early periods. However, joint-liability without flexibility yields the lowest default rates in

our setting. This is not due to behavioral differences in individual-liability and joint-liability, but automatic enforcement of joint-liability.

This study is the first one to examine the liability and flexibility trade-offs in microcredit and points towards interesting avenues for future research, for instance relaxing some of our assumptions or testing other versions of flexibility. Further, this study nicely illustrates the benefits of framed field experiments. While field experiments are generally unable to cleanly measure punishment and disentangle repayment choices from shocks and decisions of the borrowing group, a pure lab experiment with students who are mostly inexperienced with borrowing might have yielded vastly different results.

I LIE? WE LIE! WHY? EXPERIMENTAL EVIDENCE ON A DISHONESTY SHIFT IN GROUPS Chapter 3 is joint work with Martin G. Kocher and Simeon Schudy and builds on the observation that unethical behavior such as dishonesty, cheating and corruption occurs frequently in organizations or groups. Recent experimental evidence suggests that there is a stronger inclination to behave immorally in groups than individually. We ask if this is the case, and if so, why.

Using a parsimonious lab setup, we study how individual behavior changes when deciding as a group member. We focus on the incentive structure within the group and the deliberation process taking place via exchanging arguments and learning about the strength of a norm. By design, we exclude better understanding and diffusion of responsibility as explanations for increased dishonesty in groups. To achieve individual observability of lying, we modify the widely used die rolling task (Fischbacher and Föllmi-Heusi, 2013). We show participants a randomly selected video of a die roll and ask them to report the number they have seen. Payoffs depend on the reported number, not the number seen. A "classical" payoff maximizing agent would thus simply report the number associated with the highest payoff. In contrast, participants who follow the honesty norm would report the number they have seen. Our experimental design allows us to observe behavioral change (within-subjects) across different (between-subjects) individual and group treatments. Groups are allowed to communicate via an anonymous chat before all group members individu-

ally report a number. Two kinds of groups exist: with payoff commonality (i.e. other group members benefit from one's lie) or without. Lastly, we elicit beliefs about others' reporting behavior before and after the group communication to assess the prevailing norm.

Even without norm enforcement and despite the abstract setting, the honesty norm is clearly reflected in individual choices. However, we observe a strong dishonesty shift in groups. This shift is mainly driven by communication within groups and turns out to be independent of whether group members face payoff commonality or not. Group members come up with and exchange more arguments for being dishonest than for complying with the norm of honesty. Thereby, group membership shifts the perception of the validity of the honesty norm and of its distribution in the population.

While many studies, including Chapter 2, point to beneficial aspects of groups, we find that in the presence of a weak norm (unframed setting and no consequences of misreporting), communication in groups can lead to large shifts in undesirable behavior. How to counteract these shifts, e.g. by strengthening the norm or introducing sanctioning mechanisms as in Chapter 2 are fruitful avenues for future research. Likewise, given the importance of communication in groups, it would be interesting to include the possibility to discuss repayment and the use of flexibility in the setting of Chapter 2. Given the strong repayment norm, communication might further enhance "responsible" behavior as opposed to the dishonesty shift that we observe in Chapter 3.

All three chapters of this dissertation are self-contained, they have their own introductions and can be read independently. Each chapter has its own appendix and all appendices are added after Chapter 3. The bibliography containing all references can be found at the end of this dissertation.

Part II

MICROFINANCE AND NORMS: INDIVIDUAL AND GROUP BEHAVIOR IN THE LAB AND IN THE FIELD

THE ENDOWMENT EFFECT AND SAVINGS DECISIONS OF THE POOR

1.1 INTRODUCTION

Recent field experiments support that improvements in access to and usage of formal savings can reduce poverty (Burgess and Pande, 2005) and debt (Kast and Pomeranz, 2014). Savings can also increase investment and income (Brune et al., 2016; Dupas and J. Robinson, 2013a; Prina, 2015) and even lead to higher female empowerment (Ashraf et al., 2010). In general, impacts of microsavings seem to be very promising for improving the lives of the poor, especially when compared to the modest effects of microcredit (cf. Banerjee et al., 2015b). However, savings levels among the poor remain very low and reasons for this are still not sufficiently understood (Karlan et al., 2014). In many cases, low formal savings do not seem to be exclusively driven by liquidity constraints and being too poor to save (Banerjee and Duflo, 2007). In addition to direct costs such as transaction costs, regulatory barriers and social constraints, explanations have focused on behavioral biases. Time-inconsistent decision making and inattention have received substantial consideration (Ashraf et al., 2006a,b, 2010; Brune et al., 2016; Dupas and J. Robinson, 2013b; Karlan et al., 2017), while other biases have been neglected so far.

One of the best known findings in behavioral economics is the endowment effect, the relative reluctance of individuals to give up things they have been endowed with (for recent surveys see e.g. Ericson and Fuster, 2014; Morewedge and Giblin, 2015; Zeiler, 2018). The endowment effect can have severe consequences for the design of contracts and institutions. It has inter alia been shown to cause market inefficiencies by influencing trading and investment behavior, both in a wide range of lab studies and in field settings (e.g. Giné and Goldberg, 2016; List, 2003). In the context of formal savings, the endowment effect

can play an important role if it imposes an additional cost on depositing cash into an account.

As compared to savings at home that can be subject to social demand or theft, savings accounts provide a safe place, often coupled with interest payments or even commitment devices. In many situations, however, poor individuals need to hold on to their cash income until they have an opportunity to deposit it. The deposit itself usually entails handing the cash to someone else, e.g. a staff member of a microfinance organization who visits the village once a week or maybe only once a month. While this seems to be a very natural choice environment, holding onto cash can create a sense of endowment. For instance, consumer research shows that cash is salient with respect to its physical form (Soman, 2003), such that parting money is vividly felt (Prelec and Simester, 2001; Thaler, 1999). This results in lower spending when cash is used (Feinberg, 1986; Prelec and Loewenstein, 1998; Prelec and Simester, 2001; Runnemark et al., 2015; Soman, 2003). If this also holds true in the context of savings deposits, the endowment effect can be an important factor contributing to undersavings.

I study the effect of cash endowments on savings deposits in a field experiment with 300 microfinance clients in the Philippines. This setting is ideal since it provides a relevant sample in which nearly everyone receives at least some of their income in cash and everyone already has a savings account, which takes care of several potentially confounding factors such as hassle costs of opening an account. The overall design of this study is straightforward: I embed a savings decision in paid individual interviews in which participants can save (some of) the earnings in their existing savings account. Participants are endowed with about two daily incomes, implying a high-stakes decision. The main treatment variation alters the timing of receiving the cash endowment: Either participants receive the cash upfront and thus hold on to the cash for on average 15 minutes before making the savings decision (cash in hand, *CIH*), or clients decide without having the cash in hand, but knowing that they will receive the remainder of the endowment not saved just after the decision (cash

announced, *CA*).¹ Participants with *CiH* thus need to physically hand over the amount they want to save whereas individuals in *CA* simply state the amount they want to save. This design provides a powerful test for the endowment effect of cash that only varies the physical presence of cash. Throughout this chapter, I use the term endowment effect because my treatment manipulation is closely related to this particular literature.² The main difference to the existing endowment studies is that I do not investigate trading of goods (vs. money) but "trading" of cash now vs. cash in the future.

This chapter relates to two strands of literature by combining behavioral insights with an application to development economics. Advancing the understanding of savings behavior, two recent field experiments compare defaulting payments into a savings account to handing out payments in cash and find higher savings with defaults (Brune et al., 2017; Somville and Vandewalle, 2018).³ These effects are in line with the effect proposed in this study but could also be due to status quo bias or inertia in decision making. The aforementioned authors do not relate their findings to the endowment effect. Giné and Goldberg (2016), however, interpret lower switching rates of old customers to cheaper savings accounts as evidence for the endowment effect (although the results could also be explained by status quo bias or inertia in decision-making). In contrast to these three studies, I explicitly investigate the effect of

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- 1 The time with *CiH* is the upper bound of time that participants spend with their endowment in the lab and since cash is held physically and participants possess it at the time of decision making, the endowment effect should be stronger in *CiH* (Bushong et al., 2010; Knetsch and Wong, 2009; Peck and Shu, 2009; Reb and Connolly, 2007; Strahilevitz and Loewenstein, 1998).
 - 2 It should be acknowledged, however, that other, inter-related concepts such as fungibility/tangibility and mental accounting exist. So far, there exists no agreement regarding the relationship of these concepts. For instance, McGraw et al. (2003) treat the endowment effect and mental accounting as similar concepts (both resulting into lower fungibility), while Ericson and Fuster (2014) point out two potential influences of mental accounting on the endowment effect and Thaler (1985) equates the endowment effect with loss aversion and uses it to develop a theory of mental accounting. Abeler and Marklein (2017) and Thaler (1985) discuss mental accounting as a mechanism for violations of fungibility. A similar disagreement still exists regarding the cause of the endowment effect (Zeiler, 2018), ranging from inter alia loss aversion (Bateman et al., 1997; Kahneman et al., 1991), trade aversion (Engelmann and Hollard, 2010), biased information processing (Carmon and Ariely, 2000) to psychological ownership (Morewedge et al., 2009; Reb and Connolly, 2007) and assignment (Heffetz and List, 2014).
 - 3 Relatedly, and similar to Thaler and Benartzi (2004) in the US, Blumenstock et al. (2017) default Afghan employees into payroll deductions which increases savings. These default interventions combine several behavioral biases such as time preferences, inertia, status quo bias as well as the endowment effect, and appear to be a powerful tool to increase savings. However, they cannot be applied in settings that mostly rely on cash transactions and these settings still abound: In 2014, 66 percent of the worldwide population aged 15 and above did not make or receive a digital financial payment (Demirguc-Kunt et al., 2015).

cash endowment on savings decisions and I am able to disentangle the endowment effect from inertia since all participants make an active choice regarding the savings deposit.

Harigaya (2017) provides evidence that the way in which deposits are made matters. He assesses the effects of a transition to mobile banking which leads to a decline in both savings balances and deposit frequency, mainly driven by lower peer pressure and increased salience of transaction fees. It is therefore important to understand how the need to physically deposit cash, a feature that has not been changed in the study, impacts savings decisions.⁴

Most of the experimental research on the endowment effect is conducted in the lab with students who are endowed with e.g. mugs and pens for a short period of time (usually around 0-15 minutes) before the endowment effect is measured. The measurement relies on either the exchange paradigm, in which trading rates for different initial endowments are observed, or the valuation paradigm with an elicitation of willingness-to-pay (WTP) for non-endowed and willingness-to-accept (WTA) for endowed participants. Results are presented in terms of trading rate differences or a WTA/WTP "gap". The exchange paradigm is valued for its easy procedures but cannot measure the monetary size of the gap, whereas monetarily quantifying the gap is the advantage of the valuation paradigm that comes at the cost of complex procedures (usually using a version of the Becker-DeGroot-Marschak mechanism (BDM), Becker et al., 1964). The valuation paradigm has sparked an inconclusive debate whether the endowment effect is (uniquely) due to the misunderstanding of the complex WTP/WTA elicitation (Bartling et al., 2015; Brebner and Sonnemans, 2018; Isoni et al., 2011; Plott and Zeiler, 2005, 2011). In contrast to most studies, my task is a natural decision that combines the advantages of the two measurements: It avoids misconception and allows monetarily quantifying the endowment effect.

More importantly, this study is the first one to test the endowment effect for cash in a field setting. In addition, the design permits assessing external valid-

⁴ In treated villages, savings can flexibly be deposited with designated store owners who will send the money to the bank via phone. This flexibility can also be a reason for this change in behavior. For a more detailed discussion of flexibility and how it can affect (repayment) behavior, see Chapter 2.

ity, a long standing criticism of lab experiments in general. So far, endowment effects for cash or fungible goods have only been examined in the lab and the studies have found mixed results, often depending on the uncertainty of the value of the exchange good.⁵ Despite the certain value of cash, the two studies that explore the endowment effect for cash have found mixed results. Both studies physically endow student participants and start the elicitation of the endowment effect directly afterwards. Bateman et al. (2005) endow some subjects with cash (chocolate vouchers) and subsequently elicit WTP and WTA for chocolate vouchers (cash) with 25 multiple dichotomous choices one of which was randomly determined for payment. The authors replicate the WTA/WTP gap for chocolate and also find an endowment effect for cash, albeit weaker than for chocolate. Svirsky (2014) endows participants with US\$ 8 in cash or chocolate coins and offers them to exchange their endowment for a given basket of goods, including cash and chocolate coins. A baseline group is told that they are endowed with 80 experimental tokens worth US\$ 0.1 each that can buy the same set of goods. Only when chocolate coins are not framed as currency but rather as chocolate, a difference in the chosen consumption bundle emerges as compared to the baseline group. Subjects endowed with cash end up choosing a similar amount of goods and cash as those with the experimental tokens. However, the robustness of these result is unclear since the study achieves low statistical power ex-post. These conflicting results could also be due to differences in the elicitation procedure and the comparison group that are used to assess the endowment effect for cash. Moreover, some participants in Bateman et al. (2005) are required to use their own cash to buy chocolate which might result in lower WTP and thus a larger WTA/WTP gap. The present study uses simple elicitation and compares the physical endowment with cash to the absence of cash, abstracting from effects of artificial currencies, exchange rates and de facto differences in endowment. Moreover, the endowment is earned (by survey participation) in all treatments and the time with *CiH* is the upper bound of time that participants usually spend with their endowment in the lab before making a decision. My setting thus repre-

⁵ While e.g. Kahneman et al. (1990) and Van Dijk and Van Knippenberg (1996) do not find an endowment effect for induced value tokens, Yechiam et al. (2017) detect differences in valuations for lottery tickets in a meta-analysis of 35 articles.

sents relevant decision environments in which cash endowment effects can be present.

More generally, in light of the recent policy debate in developing countries concerning the advantages and disadvantages of transitioning (faster) to a cashless economy, it is worthwhile to further understand which role cash plays in economic decisions. Theoretically, cash should be fully fungible, yet it has been shown in various contexts that fungibility can be reduced by labeling (e.g. Abeler and Marklein, 2017; Hastings and Shapiro, 2013; Kooreman, 2000). Beyond labeling, surprisingly little is known about whether and how cash itself influences decision making. For policy design, however, it is important to understand in which instances fungibility is reduced due to "behavioral" responses. For instance, if holding on to cash reduces savings deposits, more frequent deposit collections, a quicker transition to mobile money or at least an early adoption of these technologies by microfinance institutions (MFIs) could potentially help increase savings of more than 116 million microfinance clients (mixmarket, 2015).⁶ More importantly, if cash endowments impact savings, they possibly also affect decisions in other economic domains overlooked in previous research.

My results show that subjects with *CiH* do feel "endowed", i.e. they expect to take home more money than the *CA* group. Given balance on all non-cash related observables, this provides evidence that the standard lab treatment manipulation also works in my field setting. This sense of endowment, however, does not alter savings decisions of individuals with *CiH*: Both the mean share of endowment saved and its distribution are identical across treatments. The null effect is robust to different estimation techniques and is quite tightly estimated: Under conventional power and significance thresholds, the minimal detectable effect size is 0.3 standard deviations. This effect size is comparable to related studies and the experimental task is externally valid as the savings decision correlates with the savings balance in the account. To test for treatment effect heterogeneity, I classify the 300 participants into different saver types based on administrative data on weekly savings deposits of more than

⁶ These effects would have to be analyzed carefully since they might be counterbalanced by e.g. lower peer pressure in the collection process, as indicated by Harigaya (2017).

4700 microfinance clients. Using a model selection algorithm that also controls for multiple testing, I do neither detect a treatment effect nor treatment effect heterogeneity for the different types. In this high-stakes decision, the endowment effect does not appear to play a role. The physical deposit of cash into a savings account does not seem to be an obstacle to savings. Instead, cash appears to be fungible in this setting.

This chapter is structured as follows: Section 1.2 introduces the research design including data sources, setting, sampling and procedures. Section 1.3 presents results and Section 1.4 addresses potential confounds such trust and time preferences. Section 1.5 discusses design choices and other applications of the endowment effect to savings and Section 1.6 concludes.

1.2 RESEARCH DESIGN

1.2.1 *Data*

I use a mixture of a controlled environment and a field setting, in which I observe actual savings decisions of a relevant population and randomly assign cash holdings at the time of decision making. Embedding the savings decision in an interview, I have a rich set of background characteristics. All these data are collected on tablets during the sessions. I am able to link these interview data to administrative data of weekly savings deposits and withdrawals as well as some basic demographics, including a poverty measure collected at the time of the last loan application. The administrative data allow assessing how the experimental savings decision relates to savings behavior outside the experiment. An explanation of variables can be found in Appendix A.7.

1.2.2 *Setting and Sampling*

To ensure that I sample from a relevant population (who receive their income in cash), I work with clients from the Filipino microfinance organization Ahon Sa Hirap (ASHI) who provides financial services to poor women. Clients join

the partner MFI to borrow for productive purposes, but when joining, the MFI also automatically opens a savings account for them.⁷ The account offers an interest rate of four percent p.a. if the balance is at least 500 Philippine peso (₱) over the duration of twelve months.⁸ While the combination of saving and borrowing might seem counter-intuitive, it is a widespread practice in micro-finance settings Armendáriz and Morduch (2010) and in this particular case, the two are complements rather than substitutes: Early down-payments of the loan are not possible, so savings can serve as an insurance against potential future shocks and resulting repayment problems. Moreover, loans are usually taken to invest into the own business, whereas participants state emergencies (58 percent) and education (38 percent; up to three answers possible) as the main savings goals.

Clients usually self-select into groups of five and apply together to become members of the MFI. Two to eight borrower groups form a "center" and meet weekly in a designated place to publicly conduct all transactions in cash. Attending the weekly center meetings is mandatory and non-excused absence results in lower credit ratings. The marginal transaction cost of using the savings account is thus zero as clients are already at the meeting and can just deposit (or withdraw) savings. In addition, since all clients have an account by default, hassle costs of opening an account do not matter in this setting.

The majority of participants is self-employed (73 percent own a business) and 80 percent receive at least half and 59 percent receive all their income in cash. While mobile banking has progressed significantly in other countries, its coverage in the Philippines remains quite low with only eleven out of 467 rural banks offering electronic banking facilities (one rural bank offers mobile banking) in the first half of 2017 and this is unlikely to change soon (Central Bank of the Philippines, 2017). The Philippines thus constitute a relevant setting to conduct such a test since a transition to cashlessness will take time.

Three branches of the partner MFI were selected based on their geographical proximity to minimize travel times for the research team. Within each branch,

⁷ This is not necessarily true for all microfinance clients. Yet, since clients do not join the MFI to save, my sample is still comparable to clients of other MFIs.

⁸ ₱ 500 correspond to about 2.5 average daily wages of the sample population and were worth €9.38 (US\$ 9.96) at the time of the experiments. The inflation rate in the Philippines was about 2.5 percent.

centers have been selected based on meeting times and distance to each other, such that two centers could be visited per day. Section 1.3.5 provides evidence that this procedure did not result in a selected representative sample and Figure A.7 shows the location of the centers. The selection of participants in sample centers is closely linked to the experimental procedures and is therefore described in Section 1.2.4.

1.2.3 *Experimental Design*

The experiment is embedded in a paid individual interview that consists of three parts. The first part of the interview comprises questions regarding personal characteristics, the composition of the household, its financial situation and personal business activities. Part 2 contains an incentivized elicitation of risk and time preferences as well as loss aversion and is described in more detail in Appendix A.3. Part 3 includes survey questions regarding savings behavior, financial literacy and hypothetical questions on narrow bracketing and attention to finances.

The experiment consists of the savings decision and a cash payment for participation in the interview. At the end of Part 1, participants are asked whether they want to save (some of) their endowment in their existing savings account. Before making the decision, it is explained that if the amount saved is still in the account after one month, it will be matched with 20 percent. This match is added to the savings account by the researcher and has been employed to reduce potential influences of time preferences.

Two treatments are implemented in a 2×2 between-subject design. The main dimension of interest varies "cash-in-hand": Participants receive the cash payment for participation either at the beginning of the interview or after the savings decision. Table 1.1 illustrates how much cash participants in *CiH* and *CA* hold during each part of the interview. Participants in *CiH* hold on to the cash during the first part. Treated participants thus make the savings decision by handing over (parts of) their cash endowment to the interviewer. In contrast, participants in *CA* make the savings decision without holding the money in

their hands, but knowing that they will receive the remainder of their earnings just after making the savings decision. They simply state the amount they want to save. The second dimension varies the size of the endowment, amounting to either ₱ 300 or ₱ 500. One week in advance, participants are informed that the interviews will take place and that they will pay at least ₱ 300 (see also Section 1.2.4). While the announced earnings of ₱ 300 might already have established a reference point, entering participants' budget plans, the additional ₱ 200 should be treated as a true windfall gain.⁹

The structure of the interview serves two main purposes: First, it provides the possibility to control for potential spillover effects from the treatment onto the preference elicitation (i.e. subjects in *CiH* save less and thus are richer in Part 2 which in turn might alter their decisions). Cassidy (2017) for instance shows experimentally that liquidity constraints can result into higher elicited present bias. Giving everyone the remainder of their earnings before the elicitation reduces this concern. Still, all questions, even those in Part 2 and 3, can potentially be influenced by the cash-in-hand treatment manipulation. On the one hand, I make use of this feature as a manipulation check comparing answers from Part 1 in *CiH* and *CA*, as explained in Section 1.3.1. On the other hand, I show that answers in Part 3 are the same in the two groups (see Table A.1) and use administrative data where possible to show that respondents' answers are reliable and not influenced by the treatment (see Appendix A.6). Second, asking savings-related questions only in Part 3 prevents priming participants before the experimental savings decision. The decision is the first time savings are mentioned to the participants.

1.2.4 Procedural Details

ANNOUNCEMENT OF INTERVIEWS One week before the session takes place in a selected center, all clients in the center receive an announcement letter (see Appendix A.9.1), informing them about the possibility to take part

⁹ Note that I cannot disentangle income effect (participants with ₱ 500 are richer) and the surprise effect. The treatment necessary to disentangle the two would have been an announcement of ₱ 500, which in turn might have induced selection effects. To rule out this selection, I opted for the present design.

Table 1.1: Timing and Cash Holding during the Interview

Minutes	Activity	Cash that participant holds	
		CiH Treatment	CA Treatment
0:00	Part 1: General survey HH characteristics, income, assets, own business <i>Savings decision</i> S	Receive cash E	0
15:00		E	0
		Hand back S	Receive cash E-S
17:00	Part 2: Preference elicitation Time, risk, loss	E-S	E-S
25:00	Part 3: Savings survey Inventory, attitude, goals, bracketing, financial lit.	E-S	E-S
40:00	End: Preference payouts	E-S (+X)	E-S (+X)

Notes: S denotes the amount saved, E the endowment received for participating in the interview (₹ 300 or 500) and X potential payoffs from experimental preference elicitation.

in paid individual interviews that will earn at least ₱300.¹⁰ This announcement serves two main purposes. First, it creates a reference point of ₱300 for participation and second, it establishes trust as the sessions will take part as described in the announcement.

RECRUITMENT OF PARTICIPANTS Each session takes place at the announced day during the weekly center meeting and starts at the beginning of the meeting to make sure the interviews finish within the typical duration of the meeting (1.5-2 hours). At the beginning of each session, the research team is briefly introduced and the expected duration of an interview (40 minutes) is stated before clients can volunteer to participate. From the pool of all volunteers, ten participants are selected by a publicly drawn lottery. In all sessions, all present clients volunteered to take part, such that selection of present clients into the sample is not a concern. The number drawn in the lottery not only determines participation but also treatment assignment - *CiH* or *CA* - and the interviewer.¹¹

RANDOMIZATION OF TREATMENTS The two treatments are randomized on two different levels: *CiH* is randomized at the individual level and all interviewers conduct interviews in both treatments. The advantage of within-interviewer randomization is balance on interviewer-specific effects. To rule out confusion of treatments, the computer program of the questionnaire provides detailed scripts and requires treatment-specific entries (e.g. where cash in *CiH* is kept during Part 1). Relying on individual level randomization of *CiH*, in combination with this particular setting, I can rule out other potential explanations for undersaving (see also Section 1.4) and cleanly estimate the cash-in-hand effect on savings decisions. The endowment amount is random-

¹⁰ To increase trust, the announcement letter was read out and distributed by the MFI, a trusted institution (96 percent of participants think their savings are safe with this MFI). A question during the interview checked whether participants received the letter. While eight percent (7.3 percent) of participants in *CiH* (*CA*) stated not having received it and these shares do not differ across treatments (χ^2 , $p=0.828$). The vast majority of participants can read and write (92 percent have at least completed elementary school) and is used to receiving written documents from the MFI.

¹¹ Randomization of interviewers avoids selection of interviewer-interviewee pairings from either side. Randomization was done prior to the start of all sessions in Stata and the randomization protocol was implemented by myself.

ized at the session level and is only announced after the recruitment to avoid selection effects.¹²

SESSIONS AND EARNINGS 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 (see Figure A.7). Center meetings take place Monday to Thursday and usually start either at 9am or at 1pm, resulting in two sessions per day that were conducted in spring 2017. Each center was revisited four weeks after the initial session to deposit the match in the savings decision where applicable. Average earnings from the sessions were ₱ 417 (€7.82 or US\$ 8.30), including payouts for survey participation and preference elicitation. 85 percent of all participants (93 percent of those who save) additionally received the match after four weeks.

ADDITIONAL LOGISTICS A team of five local interviewers has been trained to conduct the individual interviews on Surface Pro tablets from the Munich Experimental Laboratory for Economic and Social Sciences (MELESSA) using z-tree (Fischbacher, 2007). First, five participants were interviewed in one round. Selected participants were interviewed one-on-one by a local research assistant in private. Once these interviews were over, the next five interviews were conducted. Only two rounds of interviews were conducted in each center to avoid information flow from already interviewed to to-be-interviewed participants. Additionally, at the end of the interview, all participants were asked not to talk about the details of the interview with others. All questions and instructions have been translated into the local language, Tagalog, (and back-translated to English) and piloted before the start of the experiments.¹³ The study was approved by the Ethics Commission, Department of Economics,

¹² Since I have an uneven number of centers in my sample, 15 centers receive ₱ 300 and 16 ₱ 500. Cell sizes are thus as follows: 73 (73) individuals in *CiH* (*CA*) with ₱ 300 and 77 (77) in ₱ 500. Randomization at the session level is necessary since the amount is publicly announced to increase trust. Moreover, to avoid any denomination effects (see also Section 1.4), the different bills are displayed during the announcement of the amount. Pre-tests have shown that this establishes trust in receiving the money.

¹³ The entire questionnaire including oral consent and detailed instructions for the savings decisions can be found in Appendix A.9.2.

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

1.3 RESULTS

1.3.1 *Manipulation Check and Balance*

While I make use of a standard manipulation that is widely used in lab experiments, evidence regarding its effectiveness in the field is limited. I therefore first assess whether the treatment manipulation was successful in my setting. As briefly pointed out before, questions from Part 1 (general household survey questions) might be influenced by the *CiH* treatment. I use this as a manipulation check as specified in the pre-analysis plan. Questions related to cash, such as income, might be especially prone to influence from the *CiH* treatment. As the main manipulation check, I use the following question from Part 1: "How much money do you think you will take home at the end of today?" (*money today*). When being asked, *CiH* individuals already hold on to cash, whereas individuals in *CA* only know that they will receive money later on. If *CiH* successfully makes treated individuals feel endowed, they should report higher *money today*. Note that at this point of the interview, individuals do not know about the subsequent savings decision and can therefore not anticipate their savings behavior and incorporate this into their report of *money today*.

Figure 1.1a presents the cumulative distribution function of reported *money today* and indicates higher reports for *CiH* throughout most of the support of the distribution (means: *CiH* ₱707, *CA* ₱606; Fligner-Policello test, $p=0.032$; Kolmogorov-Smirnov test, $p=0.039$).¹⁴ It is important to verify that the difference in feeling endowed is not driven by actual differences in wealth. Evidence for this comes from Figure 1.1b, showing that the two CDFs of *CiH* and *CA* are identical for an asset index that is constructed based on ten wealth-related

¹⁴ The Fligner-Policello robust rank order test should be used to test whether two samples are drawn from the same distribution w.r.t. their median when distributions differ across groups (by more than just a shift - which is the assumption of the Wilcoxon rank-sum test). The more widely used Wilcoxon rank-sum test provides similar results ($p=0.032$). The Kolmogorov-Smirnov test assesses differences in distributions more generally (regarding e.g. central tendency, dispersion or skewness).

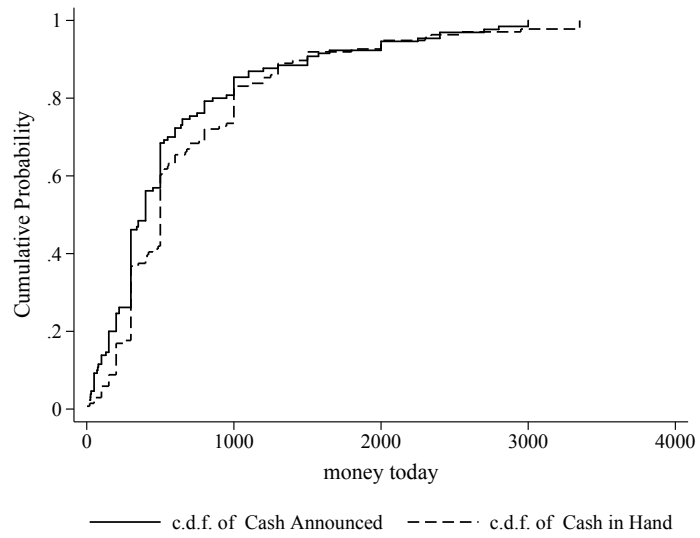
questions also asked in Part 1 (means: *CiH* 0.49, *CA* 0.48; Wilcoxon rank-sum test, $p=0.681$; Kolmogorov-Smirnov test, $p=1$).¹⁵ The administrative data also provides a measure for poverty, the Progress out of Poverty Index (PPI).¹⁶ Based on ten questions regarding household wealth, the PPI score is comparable to the asset index but cannot be influenced by my experiment. The PPI score also shows that in terms of wealth, *CiH* and *CA* are the same (means: *CiH* 43.9, *CA* 43.1; Wilcoxon rank-sum test, $p=0.51$; Kolmogorov-Smirnov test, $p=0.88$). I provide additional manipulation checks in Appendix A.2 that suggest a more general tendency to over-report cash income in *CiH*.

Randomization checks using interview data, savings and other administrative data reaffirm that differences in *money today* are related to the treatment manipulation. Table A.1 provides results from OLS regressions with the treatment dummies as independent variables where *CA*₃₀₀ is the omitted category. The F-test of the treatment dummies jointly explaining the respective variables is always insignificant. Together with low R^2 values, this constitutes evidence that the randomization was successful and that the effect on *money today* is indeed driven by the *CiH* manipulation. The standard lab treatment is making individuals in the field feel endowed, which is summarized in Result 1.

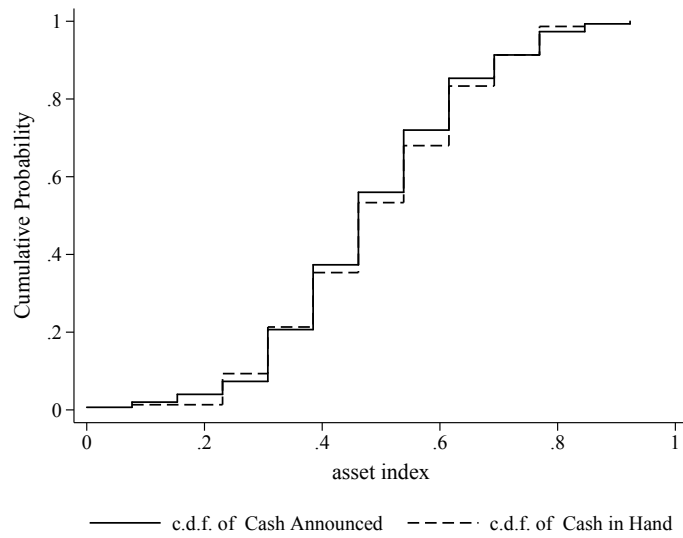
Result 1: *Individuals with cash in hand feel endowed, i.e. they expect to take home more money than the control group.*

¹⁵ Questions range from households' access to running water and electricity to possession of assets such as TVs or mobile phones. All questions are binary and are aggregated into an equally weighted index, as described in the pre-analysis plan and in Appendix A.7.

¹⁶ The Progress out of Poverty Index (PPI) is a poverty measure ranging from 0-100 that is managed by Innovations for Poverty Action (IPA). For more information see Appendix A.7 and www.povertyindex.org.



(a) CDF of Money Today



(b) CFD of Asset Index

Figure 1.1: Cumulative Distribution Functions

1.3.2 Main Results

The successful randomization allows for simple non-parametric comparisons of *CiH* and *CA*. To make choices comparable across the two earnings amounts, I focus on the share of endowment that subjects choose to save. Despite creating the feeling of endowment, *CiH* does not lead to lower savings (see Figure 1.2). This holds true irrespective of the size of the actual endowment. Similarly, the absolute amounts saved (₹ 135 in *CA*₃₀₀, ₹ 133 in *CiH*₃₀₀ and ₹ 199 in *CA*₅₀₀, ₹ 194 in *CiH*₅₀₀) do not differ across the cash in hand treatments.¹⁷

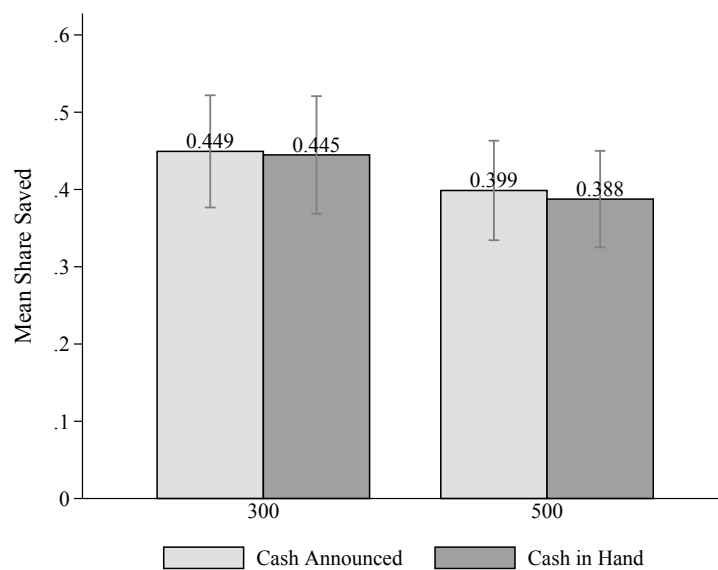


Figure 1.2: Mean Share of Endowment Saved
Notes: Mean of share saved and 95 percent CIs.

Interestingly, the share of the endowment saved also does not differ by endowment size (Wilcoxon rank-sum test, pooling *CiH* and *CA* to increase power and reduce the number of tests, $p=0.5$); on average, participants save 42 percent of their earnings. In addition, Figure 1.3 illustrates that not only the mean shares saved are the same in *CiH* and *CA* (for both endowment amounts, respectively), but there is also no difference in the distribution of choices (Kolmogorov-Smirnov test, $p=1$, irrespective of pooling or testing the two endowment amounts separately). In all treatments, focal points clearly

¹⁷ Note how participants in ₹ 500 save more in absolute terms, a tendency that might also exist in the *CiH* treatment (due to feeling richer) and counter-balance the cash endowment effect. I discuss this in more detail in Section 1.5.

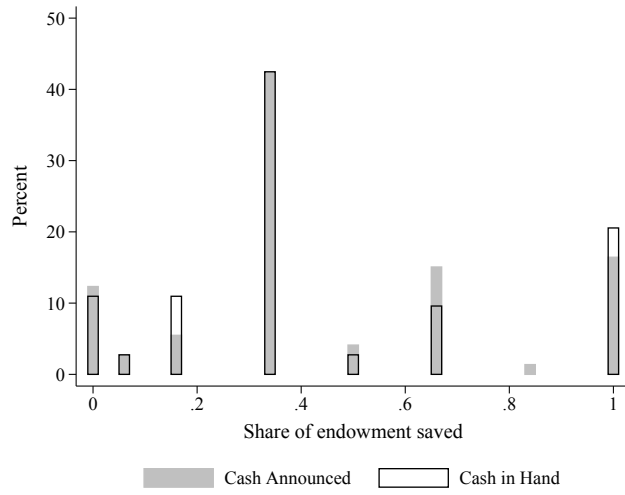
matter and reduce the quasi-continuous choice to a discrete one.¹⁸ I summarize these findings as follows:

Result 2: *Cash in hand does not lead to lower savings.*

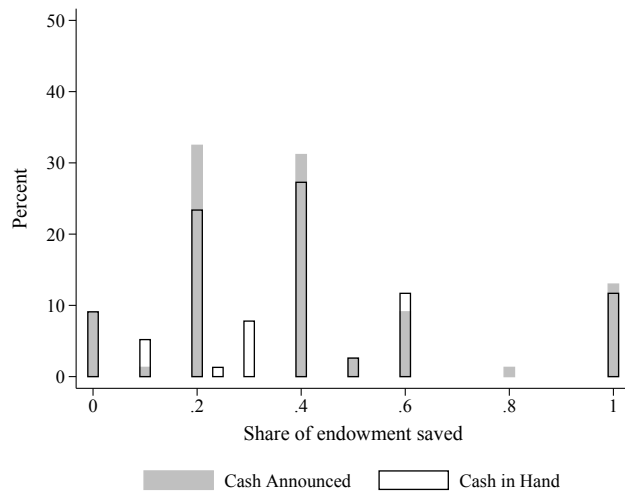
Using OLS regressions, I control for potential influences on the savings decisions such as financial literacy, age, education, household size, owning a business, having money left after buying all necessities, decision making power and transaction costs in terms of time and money needed to travel to the center meeting. Table 1.2 confirms the previous findings. The inclusion of control variables in Columns (2) and (4) does not alter the results.¹⁹ The coefficients of the *CiH* treatment dummies are small and statistically indistinguishable from zero. When testing the two coefficients in Columns (3) and (4) for joint significance, the p-values of 0.79 and 0.95 clearly indicate that the null hypothesis of no *CiH* effect cannot be rejected. These null results are further strengthened by Table A.3 showing that *money today*, used for the manipulation check, has no influence on savings.

¹⁸ Participants received five ₱ 20 bills, two ₱ 50 bills and either one or three ₱ 100 bills. Individuals in *CiH* knew they could change the bills into coins as well, but they did not receive coins as the amount of coins needed to make decisions in ₱ 1 steps has been perceived as unnatural and even offensive during pre-testing. Participants in *CiH* did not request change and participants in *CA* did not state amounts that would have required change.

¹⁹ The results are also robust to the inclusion of indicators for above median risk aversion, loss aversion, present bias and future bias. Due to potential problems of reverse causation (e.g. *CiH* could result in higher elicited loss aversion), however, they are not included as controls in the reported regressions.



(a) Endowment ₱300



(b) Endowment ₱500

Figure 1.3: Distribution of Choices by Endowment Amount

Table 1.2: Treatment Effect on Savings Decision

	Share Saved			
	(1)	(2)	(3)	(4)
CiH	-0.00796 (0.0354)	-0.00869 (0.0358)	-0.00457 (0.0575)	-0.0206 (0.0572)
Endowment 500	[-0.0762 - 0.0615]	[-0.0747 - 0.0588]	[-0.113 - 0.105]	[-0.125 - 0.086]
CiH ₅₀₀			-0.0506 (0.0630)	-0.0643 (0.0589)
			[-0.177 - 0.0680]	[-0.177 - 0.0423]
			-0.00660 (0.0711)	0.0236 (0.0716)
Constant	0.423*** (0.0317)	0.138 (0.157)	0.449*** (0.0490)	0.160 (0.164)
	[0.364 - 0.483]	[-0.179 - 0.469]	[0.359 - 0.542]	[-0.172 - 0.508]
p: CiH+CiH ₅₀₀ = 0				
Observations	300	300	300	300
Adj. R ²	0.000	0.083	0.008	0.090
Clustered SEs	yes	yes	yes	yes
Controls		yes		yes

Notes: OLS estimates, robust SE clustered on session level in parentheses, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets; *** p<0.01, ** p<0.05, * p<0.1; Controls: age, education, financial literacy, household size, business owner, money left, decision making power, time to center, travel cost to center, interviewer FE.

1.3.3 *Power*

Whereas the null effect has been shown to be robust, one might be concerned regarding the statistical power of the analyses. While I conducted *ex-ante* power calculations to determine the sample size, I now provide *ex-post* power calculations for the minimal detectable effect sizes (MDE). Considering $\alpha=0.05$ and $1-\beta=0.8$ and using a two-sided t-test, I would be able to detect a ten percentage point difference in shares saved between *CiH* and *CA* (pooling the two endowment treatments), which represents 0.33 SD of the mean share saved. In terms of amounts saved, I would be able to detect a difference of ₱ 40, which is equivalent to ten percent of the average endowment size or 0.32 standard deviations of the amount saved.²⁰ I use the MDE of ten percentage points and calculate standardized effect sizes for several lab studies of the endowment effect (see Appendix A.4 for details). Table A.11 provides evidence that my power is sufficient to detect an effect size that is in the range of previous findings.

1.3.4 *Treatment Effect Heterogeneity and Covariates*

In light of the null findings presented above, it is interesting to examine whether the null effect masks treatment effect heterogeneity. Apart from characteristics elicited in survey questions that have been used as controls in the regressions, the administrative data can be a fruitful source to determine specific saver types. The classification of types tries to capture various dimensions of savings behavior and is based on the amounts and regularity of deposits: Regular vs. irregular deposits, high vs. small amounts and equally-sized vs. variable amounts deposited (see Appendix A.5 for a detailed description). For all three dimensions, I create binary indicators based on median splits of weekly savings deposits of all clients in the three participating branches since July 2016 (up to the date on which the first interviews were announced). Regular depositors make deposits in at least 84 percent of the weeks, large weekly deposits

²⁰ Calculated with Stata's `power twomeans` command.

amount to more than ₪ 33 and those with equally-sized deposits have a ratio of standard deviation to average deposit amounts of 1.03 or less. Table A.12 shows the resulting distribution of types in my sample, for non-participants in sample centers and for all clients in non-sample centers. The types are equally distributed across treatment groups, as shown in Table A.13. I use these types and the deposit dummies (regular, high and equally-sized) to investigate treatment effect heterogeneity.

Instead of arbitrarily subsetting the data, I use a model selection procedure based on machine learning that automatically controls for multiple testing. As this estimation is post-inferential, it describes effects that are apparent in my data and can point to interesting questions to be considered for future research. In contrast to manual data mining techniques, using an algorithm has the advantage of reducing researcher degrees of freedom which have been shown to increase false positive rates (see e.g. Simmons et al., 2011). I use LASSOplus (Ratkovic and Tingley, 2017) that estimates both treatment effect heterogeneity and important covariates. The algorithm is a Bayesian method in which effects are simultaneously estimated and selected. First, each effect of potential covariates and their interaction with CiH is consistently estimated and then, following a thresholding rule estimated from the data, small effects are trimmed to zero. Ratkovic and Tingley (2017) describe the method in detail and show in simulation studies that the method is conservative and has a low false discovery rate.

In addition to the type data, I include preference data dummies for present bias, above median risk and loss aversion, and for consistency the control variables from the regressions.²¹ Figure A.2 shows the density of selected effects. Consistent with previous results, the CiH dummy has not been selected as a determinant of the savings decision. However, the algorithm has detected an interaction effect of CiH with present bias: Those who are present biased and hold cash in their hands save on average a 14 percentage points *larger* share of

²¹ Note that including "irrelevant" variables does not change the selection as they are shrunk to zero: For instance, only including the selected variables in the LASSOplus estimation results in all four variables being selected. I include saver types to test for treatment effect heterogeneity based on real-life savings behavior. In addition, preferences and covariates are included as they have previously been shown to influence savings. It is thus interesting to know whether they are related to the experimental savings decision and whether they interact with the treatment.

their endowment.²² It might be possible that those present biased individuals are sophisticated and by holding the cash, their awareness of the fungibility of money is increased. However, only 15 percent of my sample is present biased and the experiment has not been designed to test for this effect, so this result should be interpreted with caution. Future research could try to replicate and explore the reasons for this significant interaction term. Further, three covariates, high financial literacy, interviewer 2 and large households significantly influence the savings decision.²³

1.3.5 External Validity

I assess external validity with regard to the savings decision and the representativeness of my sample. External validity of the savings decision made during the experiment is examined by comparing the decision to actual savings. The decision is positively correlated with the amount in the account (Spearman's $\rho=0.138$, $p=0.017$), but not with last week's deposit (Spearman's $\rho=0.052$, $p=0.37$), which is likely due to the volatility of deposits. Moreover, the decision is related to the total savings stock (Spearman's $\rho=0.173$, $p=0.003$) that aggregates all savings amounts reported in the survey. The experimental setting thus reflects actual decision making and does not appear to be overly complicated or artificial. Similarly, I find that participants' answers in my survey are consistent with administrative data and are thus not influenced by the experiment (see Appendix A.6).

One remaining concern might be the representativeness of my sample. Selection into the sample might occur on different levels. First, participants might be different from those who were also present at the center meeting but did not participate. As everyone who was present volunteered to participate and participants were randomly drawn from the volunteers, this should not be a

²² Note that neither *CiH* nor the variable present bias have been selected. The coefficient on the interaction term can thus be interpreted without considering the effects of the two variables. For illustrative purposes, I provide a standard OLS regression including the selected variables, *CiH* and present bias in Table A.4. Columns (1) and (2) show the estimation results for amount saved and share of endowment saved, respectively and Column (3) provides evidence that present bias is balanced across treatments.

²³ All selected covariates are balanced across treatments: Interviewer 2 by design and household size and high financial literacy by randomization (see also Table A.1).

problem. Second, as I sent announcement letters one week in advance, microfinance clients not interested in participating in the study might just have decided not to attend the meeting during which the session took place. This is unlikely as all clients are required to attend the meetings and non-attendance negatively influences the credit rating. In terms of savings balance, poverty and age, I find that the non-participating clients from the sample centers are comparable to my sample (see Table A.5). However, it seems that non-participants are newer members and have smaller loans.²⁴ Given that all other characteristics, most importantly savings and poverty, do not differ, I argue that selection into the experiment is not a concern. Lastly, the selection of centers based on location and meeting times might have induced some bias. I therefore also compare my sample to the remaining 3735 clients of the three study branches that are part of centers that are not in my sample. Overall, I do not find sizeable differences between my sample and non-sample center clients (Table A.5). The sample was thus successfully selected as a random subset of the sample population with respect to observables.

1.4 CONFOUNDING FACTORS

Based on the factors contributing to undersavings reviewed by Karlan et al. (2014) and the factors that can potentially explain the endowment effect (Ericson and Fuster, 2014; Morewedge and Giblin, 2015), I first discuss which other effects might reduce savings in my experiments, especially in *CiH*. I then address factors that might increase savings and thus could cancel out a cash-in-hand effect.

TRANSACTION COSTS Transaction costs have been shown to be an important factor inhibiting savings (e.g. Dupas and J. Robinson, 2013b; Prina, 2015) and can also contribute to endowment effects. In my setting, marginal transac-

²⁴ The MFI grants larger loans in later loan cycles (after the successful repayment of a smaller loan), therefore membership duration and loan amount are highly correlated (Spearman's $\rho=0.790$, $p<0.001$). The randomization was carried out at the beginning of the meeting. One reason for this difference of my sample and the non-participants could therefore be late arrivals at the center meeting, if newer members are less disciplined and thus have a higher likelihood of being late.

tion costs for saving are zero since individuals are required to take part in the center meeting for their loan repayment and there are no account opening or withdrawal fees. In addition, travel time to the meeting is very low (mean=6 minutes, SD=6) and costs are negligible (mean=₦0.77, SD=3.88). Both variables are balanced across treatments (see Table A.1) and *CiH* does not alter transaction costs beyond the psychological costs of making the physical transaction. Transaction costs thus do not play a role in the experimental savings decision.

LACK OF TRUST AND REGULATORY BARRIERS Since my sample consists of clients of one MFI, regulatory barriers to savings are constant. Mistrust in banks in general is quite high in my sample, but 96 percent consider savings with the MFI safe. Both trust variables are balanced across treatments (see Table A.1). Trust in the banking system therefore should not lead to lower savings in my setting. However, since earnings have already been handed over in *CiH*, the treatment might increase the credibility of and thus the trust in the interviewer. It is unclear how lower trust in receiving the money in *CA* would affect savings. Possibly, it increases the variance in savings. The standard deviation of both savings measures, however, are very similar in *CiH* and *CA* (0.31 vs. 0.30 for share of endowment saved and 125 vs. 126 for amount saved). Moreover, the procedures have been designed to foster credibility and trust: An announcement letter was sent a week in advance, sessions took place as described and the cash to be earned was publicly displayed in front of all clients and the loan officer before the interviews. In addition, receipts and vouchers were shown during explanations and handed out after decisions were made. Although I cannot fully rule it out, it is very unlikely that trust differentially affected the savings decision.

INFORMATION AND KNOWLEDGE GAPS Both education and financial literacy are balanced across treatments (see Table A.1).²⁵ Since *CiH* does not alter

²⁵ While CA_{500} appears to be a positively related to financial literacy when considering $\alpha=0.1$, its coefficient is not statistically different from CiH_{300} or CiH_{500} . This is reflected in the insignificant F-test. Adjusting significance levels for multiple testing would render the coefficient insignificant.

information and all questions regarding savings are only asked after the decision to save, information and knowledge gaps are not influencing savings differently across treatments.

SOCIAL CONSTRAINTS Especially in developing countries, where informal risk-sharing plays an important role, both intra- and inter-household claims can result in substantial constraints to savings. In my sample, both claims from family and friends and the husband are balanced across treatments (see Table A.1). The same holds true for decision making power within the household, the levels of which are comparable to decision making power of females in a different study in the Philippines (Ashraf et al., 2010).²⁶ Since all transactions in this MFI are public, depositing savings also leads to requests from other clients to help out with loan repayment. While this is an important factor and 63 percent would like to have a private account, these two variables are also balanced across treatments. Moreover, participation in the study as well as earnings are common knowledge, such that neither saving nor keeping the money provides an opportunity to hide money. Social constraints thus do not lead to lower savings in the *CiH* treatments.

BEHAVIORAL BIASES The most prominent bias associated with lower savings is present bias. Time preferences and in particular temptation should not differentially affect the experimental savings decision. Although holding cash could increase temptation, spending the cash during the experiment is not possible. To further reduce the potential influence of present bias, savings during the experiment are incentivized, offering a lucrative interest rate of 20 percent for the first month. Present bias is balanced across treatments (see Column 3 of Table A.4) and as the analysis of treatment effect heterogeneity has shown, present bias, if anything, increases savings in the *CiH* treatments. The second most famous bias in the savings literature is inattention, both to savings and emergencies that could require savings. I elicited attention to household finances and show that both attention to short run (day-to-day) and medium run

²⁶ While *CiH*₅₀₀ appears to be a negatively related to decision making power when considering $\alpha=0.1$, its coefficient is not statistically different from *CA*₅₀₀. The considering the coefficients of the two *CiH* treatments jointly, they become statistically indistinguishable from zero.

finances are balanced (see Table A.1). Attention to decision making more generally is addressed by incentivization, making the savings decision high-stake.²⁷ The match, high stakes and successful randomization thus rule out that other biases lead to differentially lower savings.

MISCONCEPTIONS REGARDING THE DECISION Misunderstanding of WTP/WTA elicitation can result in an overestimation of endowment effects (Plott and Zeiler, 2005). My task, however, is very simple and the savings decision and the match are described in the same way in all treatments before making the decision. In addition, my sample is experienced both in making savings decision and transacting in cash. While experience with a particular transaction has been shown to reduce the endowment effect (Engelmann and Hollard, 2010; List, 2003), this implies that I might estimate a lower bound of the effect. However, being experienced with cash transactions is at odds with individuals feeling richer with *CiH*.

The combination of sample balance on all relevant observables and design features that prevent the differential influence of trust and temptation on the savings decision allows to rule out any reduction in savings in the *CiH* treatments other than the endowment effect. However, there might be effects that operate in the opposite direction and therefore cancel out the *CiH* effect.

INCOME EFFECTS The cash endowment effect on savings has at least one important difference as compared to standard endowment effect experiments. A common feature of the latter is that by endowing some participants, these participants are made richer, which might affect their valuation of the good in question. This "income effect" is usually assumed to be negligible in the lab since the value of goods relative to participants' wealth is small. In their review paper, Ericson and Fuster (2014), however, note that even with small increases in wealth, mental accounting could lead to WTA/WTP gaps. Indeed,

²⁷ This might lead to an estimation of the lower bound for the endowment effect in all treatments. For instance, if the bias is comparatively small it might only exist when impulsive, less important decisions are made (e.g. (over)reporting *money today* in the survey). For incentivized, rather high-stakes decisions (the savings decision in my experiment), individuals might be able to override this bias.

Morewedge et al. (2009) show that when buyers already have a mug and bid for a second one, their valuations are indistinguishable from valuations of sellers and higher than bids of non-endowed buyers (the authors attribute this to ownership, but do not discuss the increase in "wealth"). In these conventional endowment effect experiments, both the income and the endowment effect can increase the valuation of a good.

Contrary to other endowment effect experiments, the income and the endowment effect operate in opposite directions in my setting, at least for absolute savings: An endowment effect would result in lower amounts handed over for depositing, whereas higher income should lead to higher absolute savings. In light of income uncertainty that arguably exists in my sample, a positive income shock should be used to build up precautionary savings (Carroll and Samwick, 1998; Sandmo, 1970). Indeed, comparing absolute savings in the different endowment amount treatments yields direct evidence for such an income effect: Individuals in *CA* with ₱500 save significantly more than those with ₱300. Since participants with *CiH* feel richer, both mechanisms could be at play in the treatment. This would imply that I am estimating the lower bound of the pure *CiH* effect.²⁸ However, it is unlikely that the income and the endowment effect cancel each other out in both endowment amount treatments.

MOTIVATION TO SAVE Related to income effects, the motivation to save for later might override the cash endowment effect. Indeed, my sample is motivated to save: 81 percent state that they are currently saving and in response to an open-ended question regarding the use of a hypothetical windfall, 44 percent state that they would save (some of) it. However, I cannot assess how this motivation compares to a potential *CiH* effect.

INSTITUTIONAL INCENTIVES TO SAVE The account only bears interest when a minimum balance of ₱500 is maintained for a year. This might create

²⁸ An ideal treatment would vary cash endowment without increasing income. This, however is impossible: Even if people are equally rich, this study has shown that holding cash results into feeling richer. An alternative would be to reduce the endowment to lower the income effect but this will render the savings decision meaningless.

an additional incentive for the 147 participants (73 in *CiH* and 74 in *CA*) below this threshold to save. This is not a concern since participants above the threshold save significantly more (Wilcoxon rank-sum test, $p=0.054$) and the amount needed to make the threshold is not correlated with the savings decision of those who have fewer than ₹ 500 in savings (Spearman's $\rho=-0.062$, $p=0.456$).

DENOMINATION EFFECTS The treatment variation might also result in different savings decisions if participants in *CA* think about the cash in a different denomination than the ones in *CiH* who actually hold the bills in their hands. At least for spending, it has been shown that a single, larger denomination bill reduces spending as compared to the same amount of money in smaller bills (Raghubir and Srivastava, 2009). While it is not clear how this would impact savings, the equality of distributions of the share and amount saved in *CiH* and *CA* (as depicted in Figures 1.3 and A.1) show that this is not a concern.

INTERVIEWER DEMAND EFFECTS Especially in a culture in which keeping face is important, participants might want to save just to please the interviewer. Although the instructions make it clear that any amount from zero up to the endowment can be saved, the match could be interpreted as a signal that the interviewer or the experimenter values savings. However, it is unclear whether and how interviewer demand would interact with *CiH*. If anything, one could imagine the demand effects to be stronger in *CiH*, e.g. due to positive reciprocity. This would lead to larger savings in *CiH* and thus reduce the endowment effect. Moreover, as discussed in Section 1.3.5, the experimental decision is related to pre-experimental savings behavior, further alleviating demand concerns. Differential demand effects across interviewers are ruled out by balancing *CiH* and *CA* within each interviewer.

1.5 DISCUSSION

1.5.1 *Design Choices*

In addition to ruling out all other factors that would result in lower savings, the experimental design was set up to cleanly measure the lower bound of the *CiH* effect. This conservative choice was made to convincingly argue for this effect to exist instead of providing an upper bound that might be driven by a variety of factors. Changing several parameters of the experiment might thus increase the chances of finding an effect.

Participants held on to the cash for on average 15 minutes before making the decision. Whereas this is the upper bound of standard endowment experiments in the lab, it is likely that the effect grows stronger with an increase in duration. Since participants might need to hold on to cash before making a savings deposit for up to one week in the real-life setting, increasing the time with *CiH* would make the experiment more realistic and would arguably increase the treatment effect. However, this would also open the door for other mechanisms such as temptation. If participants were to take home the cash and keep it for e.g. a day, they might be tempted to spend it instead of saving it. Therefore, an interaction of temptation and *CiH* might lead to a larger reduction in savings.

Another design feature that might reduce the *CiH* effect in the experiment is the high endowment. Making the endowment large enough such that the savings decision is meaningful was crucial. Despite the endowment being framed as compensation for taking part in the survey, it is a very high incentive (1.5 or 2.5 average daily wages) and might be treated as a windfall gain rather than earned money. Earned money, however, could create a greater sense of attachment. Loewenstein and Issacharoff (1994) show that goods earned by performing well create a larger valuations compared to this good being obtained by chance. Moreover, the public randomization of participation was necessary to avoid fairness concerns, but might have reinforced the perception of the endowment as "won" rather than "earned", which in turn translates into a lower

bound of the *CiH* effect. Strengthening the feeling of earning the money by increasing the time spent on the task or its demandingness, or reducing the endowment might thus lead to a larger the *CiH* effect.

1.5.2 *The Endowment Effect and Savings in General*

Even if the endowment effect does not influence savings *deposits*, it could still impact savings behavior more generally. For instance, if individuals are reluctant to give away cash, they automatically save it at home. In this case the endowment effect would increase savings. In a setting in which the majority of transactions are done in cash and individuals are subject to a variety of claims, it is less likely that an endowment effect results into higher savings at home. Other samples that use a mix of cash and card payments may have a higher propensity of saving at home by spending less when using cash. While this points to other interesting questions, this study focuses on deposits because the promising effects of savings have been found for formal savings (that require a deposit). This is *inter alia* due to the fact that saving at home is often unsafe in developing countries (55 percent agree or strongly agree that this is the case vs. five percent who say so for the MFI) and these savings bear no interest.

The endowment effect might also lead to higher savings if individuals feel endowed with their savings in the account. This might foster the reluctance to withdraw and therefore increase the balance. In comparison to the endowment effects on deposits and savings at home, however, this effect is likely to be small. First, cash needs to be deposited into the account before individuals can feel endowed with their savings and second, money in the account is less tangible and therefore less salient than cash because savings in the account are represented by a number rather than physical banknotes.

1.6 CONCLUSION

This study tests the endowment effect as a new explanation for undersavings and establishes both internal as well as external validity, providing a manipulation check and showing that the savings decision taken during the experiment is related to actual savings behavior. In this controlled setting, I can rule out that holding on to cash increases the burden of savings decision making on top of other known biases such as present bias and inattention. Given sufficient power to detect meaningful effect sizes, this null effect is an interesting result that complements the literature in several aspects.

First, it shows that stronger endowment effects for physically present goods found in the lab (Bushong et al., 2010; Peck and Shu, 2009) even apply to a fungible "good" in the field. Using a treatment manipulation similar to these studies, I find that individuals feel more "endowed" when they already hold cash in their hands.

Second, it helps interpreting the currently mixed evidence regarding cash endowment effects. I show that despite feeling endowed, participants do not alter their savings behavior with cash endowments. The low powered null result from Svirsky (2014) is confirmed outside the lab. This is important, as one of the major critiques of lab studies is limited external validity, a concern I specifically address and dismiss.

Third, by examining a poor sample, this study also broadly relates to the growing literature of poverty and decision making (e.g. Haushofer and Fehr, 2014; Mani et al., 2013). While these studies show that poverty can cause additional or more severe biases, I do not find a cash endowment effect. In that sense, the null effect is good news as I do not detect an additional bias that further impairs decision making of the poor.

In addition to complementing the literature, this chapter also points towards interesting questions for future research. While this study has investigated the lower bound of the cash endowment effect to provide a conservative test, it could be instructive to do the opposite by increasing the attachment to cash via the factors discussed in Section 1.5. Finding no effect of cash endowment at the

upper bound would be a strong indication of no cash endowment effect at all. Moreover, it would be fruitful to investigate the interaction of cash endowment and present bias in more detail. First, a replication of this finding would be necessary to establish the effect and second, the interaction could be explored in several decision environments to assess whether and how it could be used to design (policy) interventions. The present design could easily be modified to test the competing hypothesis that cash increases temptation which would be an interesting, different angle on decision making with cash in hand.

HIGH LOAN REPAYMENT AND LOW PEER PRESSURE? REPAYMENT FLEXIBILITY IN MICROFINANCE GROUP LENDING*

2.1 INTRODUCTION

Microfinance organizations successfully offer small loans to poor borrowers with extremely high loan repayment rates. Good repayment performance is often attributed to joint-liability and a rigid, high-frequency repayment structure.

Joint-liability refers to loans given to a group of borrowers that are jointly responsible for the group's repayment obligation. Theoretical models show that with joint-liability, social capital is seized for providing mutual insurance (Armendáriz, 1999; Ghatak and Guinnane, 1999) and that the possibility of peer sanctions within joint-liability groups improves repayment further (Besley and Coate, 1995). Empirical evidence confirms that social capital is important for high repayment (Karlan, 2007) and that joint-liability leads to high repayment rates. This is due to its successful trade-off between risk-sharing via mutual insurance and free-riding on the other group members' loan repayment when dynamic incentives through follow-up loans are present (Abbink et al., 2006). However, more recent evidence shows that joint-liability is not per se responsible for high repayment (Attanasio et al., 2015; Giné and Karlan, 2014) and that peer monitoring and punishment helping to improve loan repayment can also have adverse effects. High-risk, high-return investments are usually discouraged in joint-liability groups to reduce instances of mutual insurance among group members (Fischer, 2013) which may be one of the reasons why only modest positive effects of access to microcredit on poverty reduction are found (Angelucci et al., 2015; Attanasio et al., 2015; Augsburg et al., 2015; Banerjee

* This chapter is based on joint work with Kristina Czura and Anett John.

et al., 2015a,b; Crépon et al., 2015; Tarozzi et al., 2015). In addition, evidence on excessive peer pressure in joint-liability borrowing groups is mounting: Czura (2015b) presents rigorous evidence from framed field experiments on excessive peer punishment and social anthropological studies by Karim (2008), Montgomery (1996), and Rahman (1999) provide anecdotal evidence of drastic social pressure on defaulting borrowers. These include verbal harassment, shaming in public, the raiding of houses to confiscate liquidable assets to cover the loan installments and even stripping down the defaulter's house completely. While individual-liability lending does not suffer from these potential problems related to group monitoring and peer pressure, it also does not offer any insurance against income and expenditure shocks. Thus, the liability structure presents a trade-off between mutual insurance and low peer pressure.

The second potential driver for high repayment rates in microcredit lending is the rigid, high-frequency repayment structure which refers to same-sized, small and frequent installments for loan repayment. This is a feature of the typical microcredit loan with most lending contracts requiring weekly repayments of the same amount. It is believed to make repayment a less cognitive demanding process and to instill repayment routine and high repayment morale (Armendáriz and Morduch, 2010; Labie et al., 2017; Meyer, 2002). Two reasons are proposed in the literature. First, tight repayment schedules force borrowers to cross-finance their loan repayment by borrowing from informal lenders and hence allow microfinance institutions to free-ride on the information advantage of informal lenders (Jain and Mansuri, 2003). Second, for present-biased borrowers smaller repayment burdens are subject to lower temptation, which is particularly important when rewards in the form of access to credit in the future are farther away (Fischer and Ghatak, 2010). However, the frequent, same-sized repayments are hard to reconcile with income and expenditure shocks and can lead to a cash flow disconnect (Karlan and Mullainathan, 2007). Possible resulting consequences may be over-indebtedness of borrowers (Jain and Mansuri, 2003) or underinvestment in profitable investment opportunities (Barboni and Agarwal, 2018; Czura, 2015a; Field et al., 2013). Although empirical studies do not find an increase in delinquencies with larger repayment intervals (Field and Pande, 2008), microfinance practitioners share an almost

universal belief that frequent repayment schedules improve repayment rates (Fischer and Ghatak, 2010).

More flexible repayment schedules can possibly increase shock coping capacity of clients (Czura, 2015a; Shonchoy and Kurosaki, 2014) and might render microcredit more interesting for entrepreneurs with riskier projects (Barboni and Agarwal, 2018; Field et al., 2013). On the other hand, flexibility may reduce repayment morale and make the repayment itself a more cognitively demanding process (Fischer and Ghatak, 2010; Jain and Mansuri, 2003; Labie et al., 2017). The repayment schedule thus posits a trade-off between shock coping capacity and repayment morale.

In light of these two trade-offs in the design of microcredit loan contracts – the one between mutual insurance and peer pressure in the liability structure and the one between shock coping capacity and repayment morale in the repayment schedule – we jointly analyze the two loan contract features of joint-liability and repayment flexibility. In order to understand the benefits and costs of the two features and their interaction, we conduct framed field experiments with microcredit borrowers in the Philippines. Participants play a microcredit repayment game with risky income in which we exogenously vary the liability structure (individual, *IL*, or joint-liability, *JL*) and the availability of flexibility (flexibility, *flex*, or no flexibility, *no flex*). We focus on strategic default in loan repayment, i.e. the choice not to repay despite being able to, and elicit participants' repayment decisions of their loan installments during the loan cycle.

We consider a structured version of repayment flexibility that offers clients to defer one repayment installment and make up for it later. Flexibility thus allows a borrower to self-insure against an idiosyncratic shock. In joint-liability, this implies that each borrower can reduce mutual insurance obligations in the group which may alleviate peer pressure among group members. An important practical aspect for flexibility is its easy and low-cost implementation without the need of costly verification of eligibility, for example when the borrower suffered an income or expenditure shock. Therefore, we design our flexibility as a "flexibility token" that each borrower can use once during their loan duration at her own discretion. This leaves room for misusing repayment flex-

ibility to increase current consumption instead of coping with a shock – the intended use of repayment flexibility.

Both studied contract features, joint-liability and repayment flexibility, come with important advantages and disadvantages. We hypothesize that the interaction of both features helps to seize the advantages of each feature while keeping its disadvantages at bay. We see three reasons for potential gains from combining joint-liability and repayment flexibility. First, repayment flexibility and joint-liability are partial substitutes as they offer individual insurance and mutual insurance, respectively. Therefore, flexibility in joint-liability group lending may reduce peer pressure. The group structure with possibilities of social sanctions can help ensure that, second, flexibility is exercised properly and only used in case of a shock and that third, the deferred installments are eventually repaid and not indefinitely delayed.

In our microcredit repayment game, participants decide in three periods between loan repayment with uncertain delayed rewards and consumption with instant gratification. More specifically, participants decide in each period whether to spend their per period income y on repayment and consumption, RC , or exclusively on consumption, $2C$ (with $y = RC = 2R = 2C$). In each period, participants may suffer an idiosyncratic shock that destroys income in the respective period with probability $\theta=0.25$. As soon as a participant defaults in any of the three periods, she will not receive the delayed gratification symbolizing the continuation value of a good relationship with the bank. Default on loan repayment depends on the liability structure of the loan. In a 2×2 design, we cross the liability structure, IL vs. JL , and the availability of flexibility, *flex* vs. *no flex*.

In IL , default is characterized by non-repayment in at least one of the three periods, either due to choosing no repayment and high consumption ($2C$) or due to suffering a shock. In JL , group members are jointly responsible for repaying $2R$. In our design, joint-liability is automatically enforced, so that the lending group defaults when both borrowers do not repay in at least one period. The reason for non-repayment, i.e. whether it is because of choosing no repayment and high consumption or suffering a shock (and any combination

of these among both borrowers), is irrelevant for determining default.¹ Flexibility allows borrowers to cope with a shock and defer the repayment obligation to the next period. In this way, default in the current period is prevented and a double repayment is due in the next period. Each borrower can exercise the flexibility once, either in period one or two. Although intended as insurance against idiosyncratic shocks, the use of flexibility is not restricted to having a shock. Flexibility that is used to increase consumption instead of shock coping will be labeled as *misuse* of flexibility.

Our results suggest that flexibility provides insurance against shocks. Defaults due to a shock are significantly lower, especially in the first period, since borrowers can insure themselves by exercising their repayment flexibility. The reduction in default is highest in the absence of mutual insurance: We find 12 percent of borrowers default in *IL-flex* in the first period compared to 40 percent in *IL*. Individual repayment decisions in both *IL* and *JL* with and without flexibility are high – participants chose to repay 73 percent of their overall outstanding debt over the three-period loan cycle. While repayment choices do not differ across the liability structure, we find about 10 percent lower repayment in both flexibility treatments.

While we find no difference in individual repayment behavior in *IL* and *JL*, the mere enforcement of *JL* yields the best insurance against shocks and thus the lowest default rates (39 percent after all three periods). Although *JL* performs best in terms of defaults, it comes at high social costs in the form of excessive anti-social punishment. Fifty percent of participants punish their partner when they have an observable shock and cannot repay, such that joint-liability is enforced. In *JL-flex*, in contrast, group members are able to insure themselves against a shock without relying on their partner. This is mirrored in the fact that punishment is halved when flexibility is used (only 25 percent of participants choose to punish). Conversely, the threat of punishment leads to less misuse in *JL-flex* than in *IL-flex* (58 percent misuse vs. 74 percent).

We chose the design features to keep the experiment as simple as possible in the complex interaction of liability structure and repayment flexibility. Our

¹ Automatically enforcing joint-liability payments reduces the decision space and helps to simplify the design. It is widely used in other lab experiments focusing on repayment choices and ex post moral hazard, see Abbink et al. (2006) and Cassar et al. (2007).

design features have some mechanical influence on eventual default: The use of flexibility in *JL-flex* excludes the possibility of mutual insurance in the same period. In addition, defaulting on the repayment of flexibility automatically leads to group default in the next period since the double repayment obligation cannot be covered by mutual insurance. In addition, flexibility provides no insurance in period 3. Consequently, after all three periods, a similar percentage of participants in *IL*, *IL-flex* and *JL-flex* is in default (72 percent, 69 percent and 73 percent respectively).

Our framed field experiment has several advantages. In comparison to (natural) field experiments, we are able to introduce fully observable individual idiosyncratic shocks and cleanly measure group pressure. Even more importantly, we can separate behavioral responses to specific contract features from the mechanical effects of these features.² The results from our experiment thus allow us to pin down different mechanisms at work as we are able to observe counterfactual behavior, have perfect knowledge of shocks, individual repayment choices, flexibility use as well as eventual loan repayment, and we can measure group pressure and beliefs. In contrast to lab studies with student samples, our setting is ideal to test our hypotheses for at least two reasons. First, we observe the behavior of the relevant population that uses microfinance products and is experienced in borrowing in group settings. Second, participants are used to enforcing repayment and mutual insurance norms by applying social pressure in their real-life borrowing groups.

Our study relates, first, to a recent but growing strand of literature on repayment flexibility in microcredit and its effect on investments, repayment, and more recently, borrower self-selection. While most studies find some evidence on increased investments, evidence of flexibility on loan repayment is mixed. Field and Pande (2008) study repayment flexibility in terms of repayment frequency by randomly assigning borrowing groups to weekly and monthly repayment installments. They only study repayment performance and do not find any difference across different installment frequencies. Studies on more structured flexibility with some pre-specified rules on how to exercise flexibil-

² For instance, do clients with joint-liability have lower default rates because they choose to repay more reliably or is this entirely driven by mechanical effects such as automatic enforcement of joint-liability that lead to better shock coping capacities?

ity which also study investment choices find more heterogeneous effects on default. Field et al. (2013) study the effects of a grace period between loan disbursement and the start of the loan repayment and find increases in business profits at the expense of higher default. Our results are similar in that we find lower repayment (and higher default) with flexibility. Czura (2015a) examines two types of flexible repayment schemes, one tailored to the production cycle of the typical borrower in the sample, and another allowing for skips in repayment similar to the one studied here. She finds some suggestive evidence of improved investments and higher income but cannot say much on default due to a unique default environment in her setting. Barboni and Agarwal (2018) offer a waiver of repayment for a three-month period every 12 months of the loan duration and analyze which type of borrowers take-up this loan and what are its effects. They find positive effects on profits with an increased variance, and no differences in repayment performance. All of the later three studies argue that flexibility enables higher return yet higher risk investments, but they cannot disentangle other mechanisms influencing repayment. We complement these studies and exclusively examine strategic default with and without flexibility that lies at the heart of borrowers' repayment discipline. In contrast to these studies, our design enables us to identify different channels such as behavioral effects in individual repayment choices, flexibility use, mutual insurance and shock exposure that determine eventual loan repayment.

Second, our study relates to a rich literature on punishment in cooperation that studies punishment behavior in Public Good Games (e.g. Fehr and Gächter, 2000, 2002). Following Fehr and Gächter (2000, 2002), we design peer punishment as costly and pecuniarily non-beneficial so that it should not be applied by selfish rational agents. This constitutes a theoretical benchmark and it allows us to test whether peer punishment is applied excessively. However, in this behavioral literature, non-credible punishment (monetary and non-monetary) is frequently reported (Fehr and Gächter, 2000, 2002; Henrich et al., 2010, 2006; Masclet et al., 2003) suggesting that the theoretical benchmark for selfish rational agents is too conservative. There is broad consensus that peer punishment crucially depends on intentions for non-cooperation (Charness

and Levine, 2007; Rand et al., 2015). Therefore, we apply a second benchmark to classify punishment as excessive: Unjust punishment, i.e. punishment of involuntary defaulters that is exercised despite any evidence of bad intentions. In this respect, our study is most closely related to Czura (2015b) who rigorously documents excessive peer pressure in microcredit group lending. While our results confirm her findings, we complement her study by analyzing how the credit contract design can alleviate excessive pressure.

Third, our experimental design builds upon the literature on microcredit mechanisms for strategic default. Besley and Coate (1995) show that strategic default is reduced if the social sanctions borrowers can impose on their joint-liability group members are severe enough, such that the credible threat of social sanctions is sufficient to induce higher repayment in joint-liability group lending than in individual lending. Armendáriz (1999) shows that strategic default among borrowers can be prevented if the cost of monitoring is sufficiently low relative to the size of possible social sanctions. By relying on the assumption that only strategic default is punished, both models explain high repayment in joint-liability group lending but fail to explain positive levels of punishment in equilibrium. Our design complements existing studies on strategic default by simultaneously analyzing the role and use of joint-liability, repayment flexibility and peer pressure for individual loan repayment choices.

The remainder of this chapter is structured as follows: Section 2.2 describes the setting as well as our experimental design and procedures. In Section 2.3, we present our results which we discuss in Section 2.4. Section 2.5 concludes.

2.2 EXPERIMENTS

2.2.1 *Subject Pool*

We conducted framed field experiments with borrowers of the Filipino micro-finance provider Ahon Sa Hiras, Inc. in two different locations in the Philippines: First in the neighboring Laguna and Rizal provinces and second in Antique province (see Figure B.3). In Laguna and Rizal, all clients receive a joint-

liability loan for general business activities, whereas in Antique, some clients receive an individual-liability loan for agricultural activities that they repay lump-sum after harvest. Loan sizes range from 2,000 to 100,000 Philippine Pesos (₱).³

Regular joint-liability loans have to be repaid in 25, 50 or 100 weeks with an interest rate of 46 percent on the declining loan balance. These loans are disbursed to borrowing groups of five borrowers. Each group is part of a borrowing center, consisting of two to eight groups, in which all financial transactions with the lender take place (loan application, disbursement and repayment). Upon application to become a member of the microfinance institution, borrowers form the borrowing group themselves which ensures that borrowers know each other well. This makes joint-liability for loan repayment easy to enforce: Borrowers have better information on each other and they can exert social pressure to ensure all group members repay their loan. This is important in joint-liability lending, since with joint liability the borrowing group is considered in default as soon as one group member defaults on her loan. Therefore, side-payments among borrowers occur very often: If one borrower cannot repay her loan, other group members will cover her loan repayment to maintain a good credit rating for the borrowing group. Often, long center meetings are the consequence since the group and the loan officer wait until all loan installments have been repaid.

The microfinance institution uses various measures to ensure high loan repayment rates. Joint-liability among borrowing group members increases repayment by mutual insurance as well as peer monitoring and punishment. In addition, all transactions are made publicly and loan applications have to be approved by the fellow group members. This helps align borrowers' actions and the lender's requirements for good borrowers due to little room for hiding misbehavior. Further, the lender takes actions to instill norms of solidarity and repayment. Borrowers and loan officers hold a pledge at every weekly center meeting and promise to repay their loan and support each other. In addition to the weekly meetings, social activities are organized within the borrowing centers to build solidarity between borrowers. All these measures result in

³ One Euro was worth around ₱ 51 in March 2016 when the experiments were conducted.

high repayment rates among borrowing groups, often at the expense of good borrowers ending up repaying the loan installments for bad borrowers.

For their agricultural activity, farmers receive an individual-liability loan with a lump-sum repayment of the principal at harvest time. Interest needs to be repaid weekly throughout the loan cycle which can last up to six months and depends on the cropping cycle and the type of farming. Despite individual-liability, clients are also organized in borrower centers that meet once a week. The lender states that these groups are intended to 'provide mutual support and learning'. The same social activities and pledges as described above are in place.

Every time the client applies for a new loan, some basic demographics of the borrower and her household are collected. We use these data to present some background characteristics of our participants in Table 2.1 and to provide evidence that the between-subject randomization of treatments was successful. Our sample is predominantly female, on average 46 years old and 49 percent have not completed secondary school. For 37 percent, the main source of the household income consists of their own business and for 22 percent of farming. Forty-eight percent have a monthly household income of below ₪ 5,000 and the mean likelihood of our participants to live below the national poverty line is 46 percent as measured by the Progress out of Poverty Index (PPI). Eighty percent of participants' households are connected to the electricity grid (23 percent to a water line and two percent to the telephone grid). Most house sizes range between small and medium (on a scale from small to large) and are either poor or medium in strength (in terms of the materials used). Participants have an average loan size (most recent loan only) of ₪ 14,352.⁴

2.2.2 *Design*

We use a microfinance repayment game to analyze the enforcement problem (ex-post moral hazard) under risk. We exogenously vary the liability structure and the availability of repayment flexibility in a 2×2 design: Individual vs.

⁴ This equaled about 281 euros at the time the experiments were conducted.

Table 2.1: Balance: Borrower Characteristics

	(1) Mean in IL Sessions	(2) Mean in JL Sessions	(3) Difference
Female	0.951 (0.217)	0.908 (0.290)	-0.043 (0.514)
Age	46.626 (12.301)	46.135 (11.153)	-0.491 (0.774)
Education of female head: no secondary graduate (y/n)	0.527 (0.500)	0.450 (0.498)	-0.077 (0.305)
Main income: Enterprise (y/n)	0.341 (0.475)	0.414 (0.493)	0.073 (0.511)
Main income: Farming (y/n)	0.185 (0.389)	0.253 (0.435)	0.068 (0.582)
Electricity (y/n)	0.785 (0.412)	0.814 (0.390)	0.029 (0.741)
Water (y/n)	0.179 (0.384)	0.273 (0.446)	0.093 (0.339)
Landline (y/n)	0.028 (0.165)	0.017 (0.128)	-0.011 (0.548)
House size (0-2)	0.669 (0.662)	0.558 (0.560)	-0.111 (0.318)
House strength (0-2)	0.757 (0.646)	0.769 (0.478)	0.012 (0.919)
Probability of living below NPL	47.431 (32.690)	43.398 (30.651)	-4.034 (0.507)
Monthly income below ₱ 5000 (y/n)	0.469 (0.500)	0.498 (0.501)	0.029 (0.808)
Loan Amount in ₱ 1000	13.712 (10.140)	15.054 (12.068)	1.342 (0.398)
Observations	308	261	569

Notes: Means and SDs in parentheses in Columns (1) and (2). Differences and p-values from regressions with clustered standard errors at the session level in parentheses in Column (3). IL Sessions consists of *IL* and *IL-flex* treatments, whereas *JL* Sessions consists of *IL*, *JL* and *JL-flex*. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

joint liability and flexibility vs. no flexibility. Throughout the experiment, we use the strategy method.⁵

The standard game (individual-liability and no flexibility, *IL*) models a simple credit repayment choice under risk over three periods. We abstract from the investment choice and the presence of any savings technology and assume the investment yields a steady income $y=2$ per period. In each period, the bank requires a repayment $R=1$, and clients face the choice of repaying or using the entire income for consumption. Individual idiosyncratic shocks occur with probability $\theta=0.25$ and destroy the entire income of a period, thus leading to non-repayment. Clients make their repayment choice conditional on being able to make the repayment installment. When they suffer an income shock, they can neither repay nor consume in that period. Following the first non-repayment, whether due to choice or bad luck, clients are in default for the rest of the game. If the client repays in all three periods, she receives the continuation value V , a monetary reward which symbolizes the value of the future relationship with the bank. As this value only materializes in the future, V is paid one month after the experiment. In contrast, all experimental income allotted to consumption during the experiment (income not spent on repayment or lost to the shock) can be spent right after the experiment on a vast selection of consumption items – resembling the temptation of immediate consumption.⁶ We artificially induce temporal discounting by reducing the consumption value of income tokens earned in the experimental tasks from round to round. The same mechanism implies that future repayments are discounted.

We model joint-liability (*JL*) as a two-person borrowing group that is jointly responsible for repaying $2R$ in each period. Joint-liability is automatically enforced in case of non-repayment of any member of the borrowing group. While the reduction of the usual five-person group to two persons is a simplification of reality, automatic enforcement is a realistic representation of how microfi-

⁵ The strategy method was first introduced by Selten (1967). It is used to elicit the complete strategies of players of the game, and it allows information to be collected on subjects' behavior in different hypothetical decision making scenarios.

⁶ Consumption items were visible throughout the session on a consumption table (see Figure B.2 in the appendix), and participants were encouraged to familiarize themselves with the items before the start of the explanations with the help of a 'consumption catalog'.

nance institutions put joint-liability into practice.⁷ The repayment choice thus becomes a coordination game: Clients simultaneously choose whether to repay or not. If they choose to repay, but their partner does not, they automatically repay for their partner as well. To keep the experiment simple, we only elicit unconditional repayment choices. Further, the bank does not distinguish between the source of repayment: As long as $R=2$ in each period, both clients will receive V . A measure of peer pressure and punishment is introduced via the possibility to send punishment points which are framed as "dislike" messages to one's partner, conditional on their behavior. Participants can choose between allocating zero, one or two punishment points (at the cost of ₱ zero, five and ten respectively), reducing the partner's show-up fee by ₱ zero, 15 and 30 respectively. The deduction from the show-up fee ensures that punishment does not influence repayment capacity. Shock outcomes and repayment decisions are perfectly observable when making punishment choices. In addition to these incentivized measures for repayment and punishment, we ask for beliefs of whether the partner repays and of how much she punishes i) repayment, ii) non-repayment and iii) having a shock.

We design repayment flexibility as the option to defer one repayment to the next period. This sets the repayment obligation for the current period to zero, but requires a double repayment in the subsequent period. In this way, default in the current period is prevented for sure. Flexibility is implemented using a flexibility token that can only be used once, either in period 1 or 2. This results in three possible realizations of flexibility: First, the borrower does not use flexibility at all. Second, the borrower uses flexibility in the first period and is required to make up her missed repayment with a double repayment in the second period. Third, the borrower uses flexibility in the second period and is required to make up her missed repayment with a double repayment in the third period. Failure to make the double repayment results in default, as do shocks once the flexibility token has been used. While the provision of flexibility is intended to allow the borrower to self-insure her repayment against an idiosyncratic shock, it can also be used to increase early consumption (in

⁷ Our partner organization, for example, instructs the loan officer to extend the weekly repayment meeting until all repayments are made.

the absence of shocks). Early consumption is attractive because future income is discounted by design.

We examine the combination of joint-liability and flexibility in *JL-flex*. In a two-person borrowing group, both partners have one flexibility token and can defer one repayment obligation to the next period in any of the first two periods. In our setting, this has two important implications. First, when a borrower uses the flexibility token, she is unable to insure her partner's repayment obligation in the same and in the next period. By using the flexibility token, the repayment obligation of the borrower is reduced to zero and no repayment can be made by this borrower in this period, not even to cover her partner's repayment.⁸ In the next period, the borrower is required to make a double repayment and hence use her entire income for her own loan repayment, which again leaves no scope for insuring her partner. In addition, if she faces a shock when the double repayment is due, her partner would be unable to insure her, since group repayment obligation ($R_{\text{group}}=3$) exceeds group income ($y_{\text{group}}=2$) in this case. The use of flexibility in our setting thus results in a substitution of mutual group insurance with individual self-insurance. These very strong assumptions are necessary to simplify the design and make it understandable and practicable.⁹ As before in the *JL* treatment, the possibility to punish repayment choices as well as the use of flexibility is known before making choices. All punishment decisions are made under full information, i.e. clients know whether their partner defaulted voluntarily. In addition, we ask for beliefs about the partner's use of flexibility in period 1 and period 2, both with and without a shock.¹⁰ More details on the exact elicitation procedure of flexibility use and punishment can be found in Appendix B.4.

⁸ This simplification is necessary to exclude unofficial side-payments between borrowers. Paying for the partner while officially making use of flexibility would allow the bank to infer a misuse of flexibility, which we rule out.

⁹ Strong temptation was necessary to induce a positive amount of strategic defaults. Piloting with an individual income of 3 and consumption of 2 yielded insufficient temptation to induce strategic defaults. Increasing income beyond 2 would also have made calculations more complex for participants.

¹⁰ We did not elicit beliefs for punishment in *JL-flex*, so we assume that beliefs about the partner's punishment of repayment and non-repayment does not change with the availability of the flexibility option. This assumption is in line with general punishment behavior, as discussed below.

2.2.3 Procedures

We use a mixture of within- and between-subject designs. We randomize treatment allocation on session level and conduct sessions consisting of *IL* and *IL-flex*, as well as sessions consisting of *IL*, *JL* and *JL-flex*. Due to the natural order of these treatments and to keep the experiment as simple as possible, the order of treatments stays the same in all sessions.¹¹ At the end, we randomly select one treatment to be paid out, realize the shocks and, where applicable, the group matching without revealing partners' identities. Participants draw the shock realizations themselves from an opaque bag and where applicable, are randomly and anonymously matched with a partner from the same session by a research assistant.

To provide a behavioral measure of time-inconsistent preferences, participants are allowed to revise their choices *after* the payoff-relevant treatment has been selected, but *before* the shock realizations. While consumption realizations occur after around one hour of the experimental choices (with the exception of *V*, which is paid after one month), the use of the strategy method introduces a level of abstraction. This may be sufficient to induce more forward-looking behavior, as if making decisions for one's future self. Once one treatment is randomly chosen for payout at the end, the choices become very tangible, and respondents receive their rewards within 15 minutes. The possibility to revise choices at this point in time is not announced beforehand.

The general setup of the microfinance repayment game was explained extensively using flip chart graphics, test questions and a practice round with shock realizations (see Appendix B.7 for detailed instructions). We used loaded framing, referring explicitly to loan repayment and consumption, explained the individual idiosyncratic shock as a thief that steals all of the period's income and introduced flexibility as a "pass token" (the concept of passing was known from card games). Each of the treatments was explained in the same manner and test questions were asked. If more than five persons failed a specific ques-

¹¹ In Section 2.3.6 we argue that order effects do not play a role.

tion, the explanation would be repeated before choices were made.¹² Choices were noted in private by local research assistants using paper and pen.

Together with our partner organization, we determined centers of clients that would be eligible for participation.¹³ From these centers, we received the exhaustive member lists, out of which we randomly selected 20 borrowers to be invited for participation; five borrowers were invited as back-up. Invitation letters were handed out one week in advance during the center meeting. One session consisted of at most 20 participants. Participation was voluntary and all participants gave oral informed consent before taking part. The study was approved by the Ethics Commission, Department of Economics, LMU Munich.

In total, 569 participants took part in 33 sessions. On average, sessions lasted about three hours. All sessions were conducted from March 14 to April 21, 2016. Sessions took place at the center meeting hall on different days than the weekly meetings. After registration, participants took part in a small individual survey covering incentivized measures for risk and time preferences as well as survey questions regarding their borrowing group (see Appendix B.7.7). We randomly allocated seating to the participants.

Average earnings amounted to ₱202 (roughly four euros), which equals approximately a daily wage for this population. The earnings came from three different sources. First, the show-up fee of ₱70 was paid in cash. It would be reduced by any punishment activity (₱ five (ten) for allocating one (two) punishment points and ₱15 for each punishment point allocated; so a maximum of ₱40 could be deducted). Second, the continuation value V was paid in form of a voucher that would be redeemed to ₱100 in one month when a research assistant re-visited the borrowing centers to cash in the vouchers. Third, the income tokens earned in the microcredit repayment game could be traded for consumption items from the consumption table with a variety of products, such as sweets, food staples, household items and beauty products,

¹² We excluded participants with limited understanding based on the following rule: If less than 50 percent of the test questions from one treatment or 25 percent of all test questions of all treatments were answered correctly, the participant would be excluded from the analysis. This exclusion does not change our results (see also Section 2.3.6)

¹³ Center eligibility was determined based on the following criteria: The center has at least 20 borrowers and a center meeting hall with seating. We aimed at a balance of centers across the three regions (Laguna, Rizal and Antique) and across the applied center rating (very good, good, bad) by the microfinance institution.

offered roughly at market prices. The income tokens of the first (second, third) period were worth ₱ 40 (30, 20) representing the reducing purchasing power of consumption income over time.

2.2.4 *Strategic Considerations and Behavioral Predictions*

All decisions were elicited using the strategy method, i.e. before shocks are realized. Repayment choices are conditional on being able to repay: Whenever a shock occurs, there is no repayment choice and the borrower defaults on her loan repayment in this period in *IL*. In the joint-liability treatments, the repayment choices and shock realization of the partner are important for determining group default. An essential feature of the repayment game is that the delayed rewards in form of the ₱ 100 voucher are lost as soon as a borrower or the borrowing group defaults in any of the three periods. Consequently, two strategies exist: The first strategy is to repay in each period and receive the continuation value at the end of the loan cycle if the borrower did not suffer a shock and actually repaid her loan. The second strategy is to default in all periods. This clearly dominates default in only one of the three periods, since the continuation value is already lost with strategic default in only one period already. The income tokens and the continuation value are designed in a way that default in all periods is the optimal strategy both in the individual-liability and the joint-liability treatments unless participants incur a psychological cost for defaulting.¹⁴ We chose this calibration since all participants are real-life microcredit borrowers and we still expected a substantial repayment rate.

In a first step, we focus on individual repayment choices and disregard mechanical effects of loan repayment in joint-liability groups driven by automatic enforcement of mutual insurance. Borrowers feeling responsible for the group and more importantly, the threat of punishment may lead to higher repayment in the joint-liability treatments. These behavioral aspects of the individual re-

¹⁴ Calculations are based on expected income tokens per period (income token value times probability of not suffering a shock) plus the expected value of V if applicable. Discounting is not considered; it would make the strategy to default in all periods even more attractive.

payment choice are independent of the availability of flexibility and should hold for both liability structures. Therefore, we hypothesize

H1a: Repayment in *JL* is higher than in *IL*.

H1b: Repayment in *JL-flex* is higher than in *IL-flex*.

Further, the threat of punishment and feeling responsible for the group may further reduce misuse of flexibility. Misusing flexibility reduces the capacity to self-insure, strains the group's insurance requirements and, in some cases, erodes the mutual insurance capacity completely. Therefore, groups will seize their possibilities to minimize the misuse of flexibility. Two other strategic considerations are important in this regard. In the first period, the misuse of flexibility results in no shock coping capacity in the second period, both in *IL-flex* and *JL-flex*. In addition, in *JL-flex* no mutual insurance is possible by design neither in the period in which flexibility is used nor in the subsequent one. Group default is the consequence when one of the partners suffers a shock and cannot or does not use flexibility. This provides an additional incentive not to misuse flexibility in *JL-flex*. In the second period, however, the misuse of flexibility is dominant in *IL-flex*, since it increases the value of consumption income and flexibility cannot be exercised in the third period in any case. In *JL-flex* treatment, in contrast, the above named constraint still applies. We therefore hypothesize:

H2: Flexibility is misused less in *JL-flex*.

The second part of our motivation for this research was high punishment in joint-liability lending. Since joint-liability is enforced automatically in both *JL* treatments in our experiment, participants' only opportunity to express their disapproval of their partner's choices is by engaging in costly peer punishment.¹⁵ The realization of punishment at the very end of the experiment leaves no room for reputational concerns in the punishment choice. Backward induction yields the prediction of zero punishment as punishment is costly for the sender and cannot influence the partner's behavior *ex post*. Since we elicit the

¹⁵ In the real-life borrowing situation, different forms of peer punishment are available, such as social sanctions, destroying reputation by gossip, appeals at the local arbitration court, or eventually, ending cooperation completely by stopping repayment.

punishment choices with the strategy method, we may detect lower punishment levels than when using direct responses since emotions may be curbed (Brandts and Charness, 2011). While these considerations are the same across treatments, the main difference that can affect punishment behavior is the option to self-insure instead of relying on the partner that is provided by repayment flexibility. More specifically, we are interested in punishment behavior in the case of observable shocks that have previously been documented to be very high (Czura, 2015b) and hypothesize:

H3: Punishment for observable shocks is lower in *JL-flex* than in *JL* if self-insurance is used.

2.3 RESULTS

One of the main advantages of our framed field experiment is that we can distinguish between pure choices and outcomes that arise due to contractual features, such as enforcement of joint-liability. Since contracts and the structure they impose vary widely in the real world, we put more emphasis on behavioral responses to choice environments and thus focus predominantly on individual choices. More precisely, we examine pure choice data regarding loan repayment, the use of flexibility and peer punishment in detail. To understand how these choices translate into final outcomes in our particular setting, we also provide results from a simulation including shock realizations and partner matching.

2.3.1 *Loan Repayment*

To compare repayment across all treatments, we focus on the share of tokens repaid considering all periods jointly. This measure ranges from zero (no tokens repaid) to one (all three tokens repaid) and is robust to the dynamics of flexibility use since a "repay strategy" will result in three tokens repaid, both

with and without flexibility.¹⁶ We focus on individual choices to avoid confounding effects stemming from group matching and only consider the case of no shock. Comparing *IL* and *JL*, we find no differences in repayment (see Figure 2.1). The average share of tokens repaid is 76.3 (76.8) percent in *IL* (*JL*). This is in line with findings of Attanasio et al. (2015) and Giné and Karlan (2014) who find similar repayment rates in individual- and joint-liability, but in contrast to Carpena et al. (2013) who find increased repayment rates after a switch from individual to group liability. In contrast to these studies, we can distinguish between repayment choices and defaults that are driven by shocks. In our setting, knowing that a partner will insure non-repayment and prevent default in case of joint-liability and that this partner has the possibility to punish, does not affect overall repayment levels.

Flexibility reduces repayment by ten (nine) percent in *IL-flex* (*JL-flex*), evaluated at the mean share repaid of 0.763 in *IL*. Our finding of lower repayments with flexibility reaffirms Field et al. (2013) who find higher defaults with a grace period. However, they are in contrast to Barboni and Agarwal (2018) who does not detect repayment differences with a three-months waiver. While the coefficient of *JL-flex* depicted in Figure 2.1 is not statistically significant, we fail to reject a difference between the coefficients of *IL-flex* and *JL-flex* ($p=0.87$). This reaffirms the above finding that the liability structure does not influence individual repayment choices. We therefore reject our hypotheses H1a and H1b and summarize these findings in Result 1:

Result 1: *Repayment rates do not differ across liability structure. Flexibility reduces repayment rates by ten percent.*

Beliefs can play an important role for repayment choices. Here, we focus on beliefs about the partner's repayment and whether it is expected that the partner punishes (non)-repayment. These beliefs have been elicited in the *JL* treatment. Believing that the partner will repay is associated with a 32 percent higher share repaid (evaluated at the mean of those who do not believe in

¹⁶ See Appendix B.2 for an analysis of repayment choices per period and Appendix B.4 for a more detailed discussion on how we measure repayment.

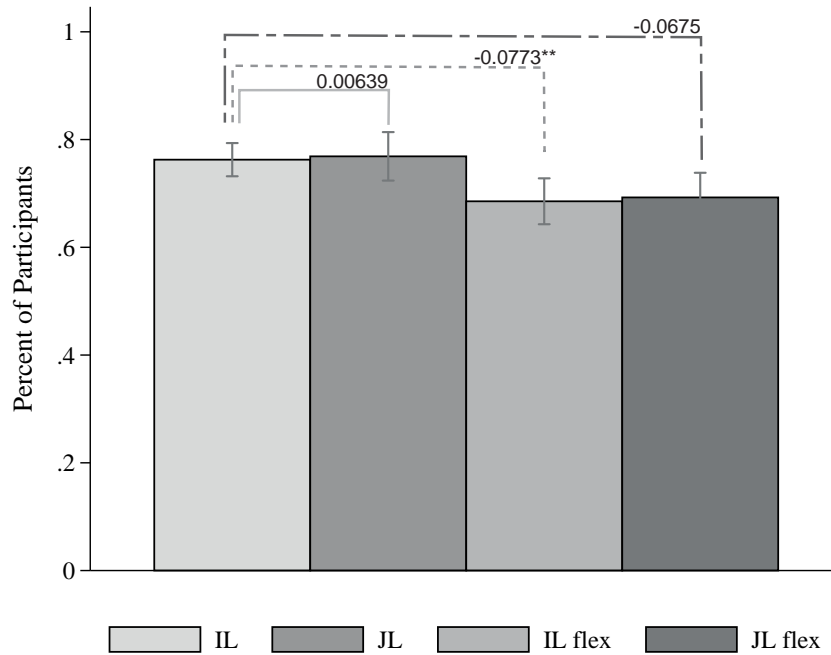


Figure 2.1: Individual Repayment (Share Repaid)

Notes: Share of tokens repaid. Coefficients from OLS regressions with session FE and SE clustered at session level (not shown). *IL* is the reference category with a mean share repaid of 0.763. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the repayment of their partner, see Column 1 in Table B.1). Believing that the partner would punish non-repayment is related to similarly higher repayment of 36 percent, whereas beliefs about being punished for repayment are not related to the amount repaid (Columns 2-4). The strongest association with repayment exists for believing that the partner repays and that she would punish non-repayment. This doubles repayment as compared to those who believe in neither. Taken together, the overall high repayment rates and strong beliefs that the partner would repay (67 percent) can be interpreted as evidence that participants bring a strong real-life repayment norm to the lab. Having internalized the norm of high loan repayment in the group might be one reason why we do not find differences in loan repayment choices.

2.3.2 Flexibility

Flexibility can be used in either period 1 or 2, but there is only one flexibility token available per participant. Although flexibility is intended to help borrowers cope with an income shock, the use of flexibility is not contingent on having a shock. When facing a shock, nearly all participants choose to use flexibility with hardly any difference between the *IL-flex* and *JL-flex* treatment (in period 2 participants in *JL-flex* use flexibility slightly less, see Figure 2.2). This shows that participants understand the value of flexibility in general.¹⁷ In our *JL-flex* treatment, this implies that participants prefer self-insurance against income shocks using their flexibility token over relying on mutual insurance.

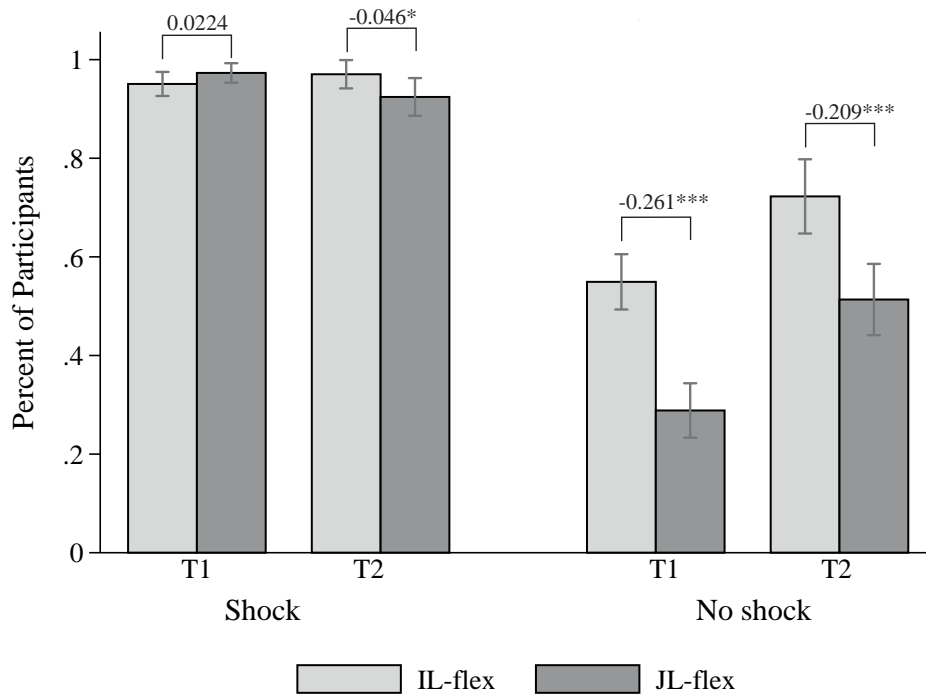


Figure 2.2: Use of Flexibility

Notes: Percent of participants who use flexibility conditional on not having used it before. Coefficients from four OLS regressions comparing the use of flexibility in the respective scenario, with *IL* as the reference category and SE clustered at session level (not shown). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

¹⁷ In *IL*, the use of flexibility with a shock in period 1 should amount to 100 percent if participants try to avoid default. Out of the 15 participants who chose not to use flexibility in this case, only one repays in both subsequent periods.

We observe misuse of flexibility without a shock in 55 (29) percent in *IL-flex* (*JL-flex*) in period 1 and 72 (51) percent in *IL-flex* (*JL-flex*) in period 2, conditional on not having used it before (see Figure 2.2). The misuse rate is significantly lower in *JL-flex* in both period 1 and 2: in period 1, misuse is reduced by 48 percent in *JL-flex* and in period 2 by 29 percent (based on regression results and evaluated at the mean of *IL-flex*; coefficients shown in Figure 2.2). Considering both periods jointly, 88 (43) percent in *IL-flex* (*JL-flex*) misuse flexibility. These findings are in line with our hypothesis H2 postulating less misuse in *JL-flex*.

In addition to choices regarding flexibility, we have elicited beliefs about the partner's use of flexibility in *JL-flex*. Actions and beliefs are well aligned in case of a shock: in period 1 (2), 91 (78, conditional on being able to use it) percent decide to use flexibility and also believe their partner would. Actions and beliefs correspond less in case of no shock: 58 (30) percent decide not to use flexibility in period 1 (2) and also believe their partner would not.¹⁸

One of the main concerns regarding flexibility is its repayment. In our experiment, repayment of flexibility implies giving up the entire income of the period which is a very strong requirement that we discuss further in Section 2.4.1. Repayment rates of the double repayment are lower than repayment rates of 76 percent in *IL* and *JL* (which also explains our Result 1), yet more than 50 percent of participants repay flexibility in each case (see Figure 2.3).

Having used flexibility as self-insurance for a shock in period 1, 64 (74) percent repay the double installment in *IL-flex* (*JL-flex*) in period 2 and 59 (68) percent in period 3 (see Figure 2.3). When misusing flexibility, 76 (85) percent in *IL-flex* (*JL-flex*) repay it in period 2. In period 3, repayment of flexibility amounts to 61 (67) percent in *IL-flex* (*JL-flex*).

While the difference is not statistically different at conventional levels, there seems to be a tendency towards more double repayments in *JL-flex*, hinting at a disciplining feature of the group.¹⁹ This constitutes additional evidence for

¹⁸ Early misuse is strongly related to thinking that the partner would do the same (Spearman's $\rho=0.369$, $p<0.001$). For those who still have the flexibility token left in period 2, the use in case of a shock is related to believing that the partner would do the same (Spearman's $\rho=0.178$, $p=0.039$). The other two correlation coefficients are also positive but not statistically significant.

¹⁹ All coefficients shown in Figure 2.3 are positive. The minimal detectable effect sizes for $\alpha=0.05$ and $1-\beta=0.8$ the four comparisons are 0.11, 0.16, 0.15 and 0.19. Power is highest in the case in

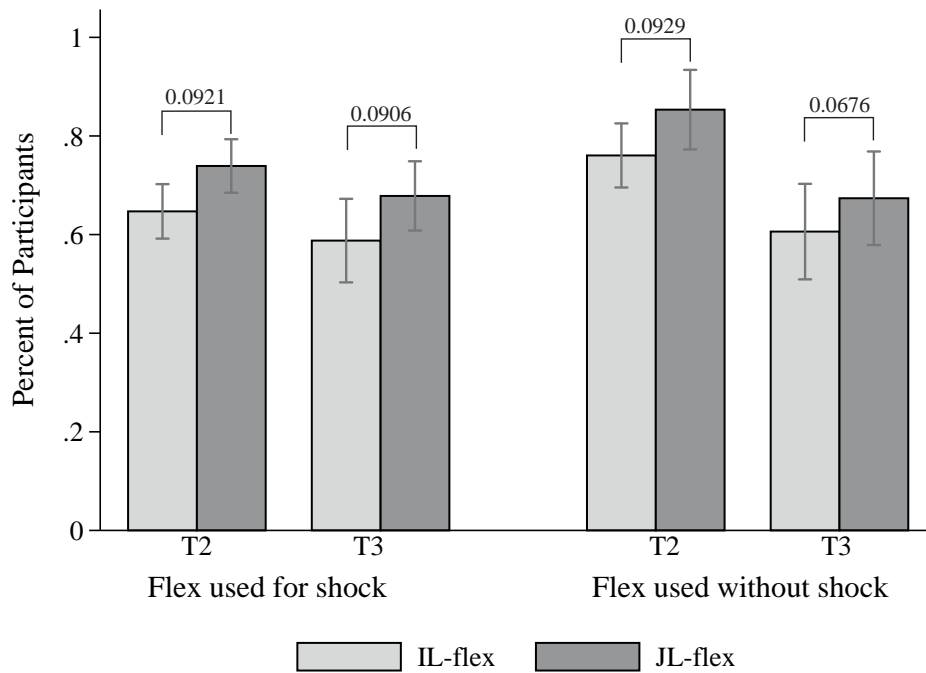


Figure 2.3: Repayment of Double Installment

Notes: Percent of participants who repay flexibility conditional on having used it for either insurance (left) or increased income (right). Coefficients from four OLS regressions comparing the repayment of flexibility in the respective scenario, with *IL* as the reference category and SE clustered at session level (not shown). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

rejecting hypothesis H1b of higher repayment in *JL-flex*. Result 2 summarizes these findings:

Result 2: *Flexibility is used when needed. However, there is substantial misuse, especially in IL-flex. Joint-liability can half this misuse. The double repayment of flexibility is lower than standard repayment and does not differ across liability scheme.*

2.3.3 Peer Punishment

We first notice that punishment is widely used, both at the extensive and at the intensive margin: In both *JL* treatments, 89 percent of participants want to

which nearly everyone uses flexibility as in the other cases treatment influences cell sizes as shown in Figure 2.2.

punish their partner if she does not repay (see Figure 2.4). The intensive margin of punishment for this situation amounts to 1.23 (1.22) punishment tokens in *JL* (*JL-flex*). We classify this type of sanctioning as *deterrent punishment* as this is clearly directed towards irresponsible behavior that increases the likelihood of group default. Deterrent punishment does not differ across treatments, neither at the extensive nor at the intensive margin (see also Panel A of Table 2.2 that provides within-participant estimates).

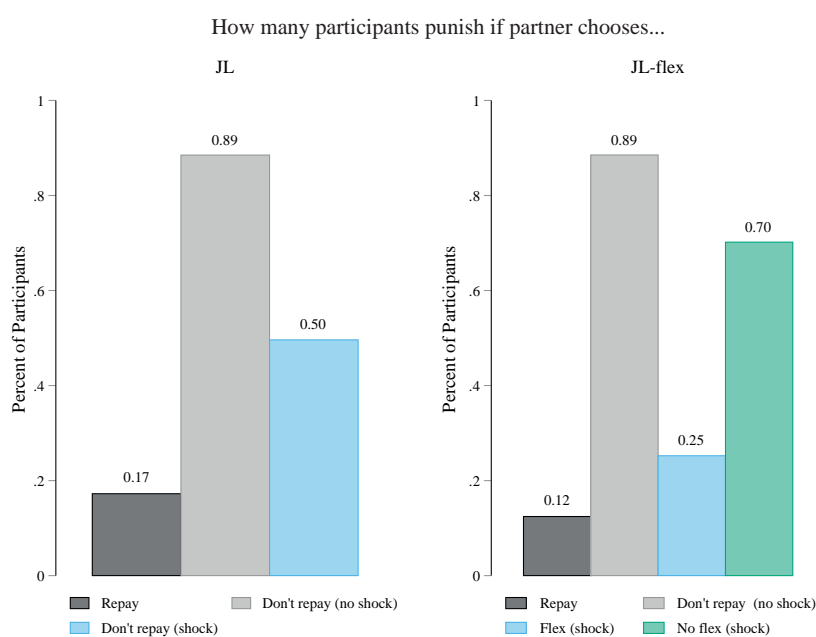


Figure 2.4: Punishment Choices (Extensive Margin)

In line with the previous literature, we also find high levels of *non-deterrent punishment* that participants use in case of a shock, irrespective of the partner's motive and despite full observability of shocks. This type of punishment could be interpreted as an expression of aversion to repay for the partner. Figure 2.4 illustrates that in *JL*, 50 percent punish their partner if she cannot repay because she has been hit by a shock (intensive margin: 0.73 punishment tokens). With flexibility, participants who are hit by a shock face two options: Using flexibility as self-insurance or not using it and relying on the partner to repay. The former reduces non-deterrent punishment both at the extensive margin by 25 percentage points (46 percent evaluated at the mean of *JL* when con-

sidering within-participant estimates in Table 2.2) and at the intensive margin by 0.40 punishment points (54 percent). Flexibility thus has the possibility to significantly reduce non-deterrent punishment, both at the intensive and the extensive margin as compared to punishment for a shock in *JL*. This is in line with hypothesis H3. If flexibility is not used despite being available, punishment increases at the extensive margin by 20 percentage points (44 percent) and at the intensive margin by 0.29 punishment points (41 percent).

One concern is that flexibility could increase both deterrent and non-deterrent punishment if defaulting on the double repayment for flexibility is punished more severely than a simple default. Deterrent punishment for not making the double repayment is the same as simple non-repayment, both at the extensive (Column 3) and intensive margin (Column 6 of Panel A in Table 2.2). The same holds true for non-deterrent punishment in case of a shock (see Panel B). This is remarkable since not repaying the double repayment obligation of flexibility leads to group default for sure.

One last concern is that the possibility to use flexibility also impacts punishment of actions unrelated to flexibility. For instance, participants might change their punishment behavior also for cases in which the partner repays or does not repay the single installment. As discussed above, this is not the case for simple non-repayment and Columns (1) and (4) of Panel A in Table 2.2 show that there is also no difference in extensive and intensive punishment of repayment.²⁰ Result 3 summarizes these findings:

Result 3: *Flexibility can reduce the high levels of punishment in JL when a partner defaults due to a shock by 46 percent at the extensive and 54 percent at the intensive margin. Flexibility has no adverse effects on punishment behavior for (double) repayment and default in the absence of a shock.*

²⁰ Figure B.1 shows all punishment choices for *JL-flex* at the extensive margin. Examining the intensive margin yields a similar pattern.

Table 2.2: Punishment

Panel A: Punishment in Case of no Shock						
Choice in JL/JLflex	Extensive Punishment		Intensive Punishment		(6) Don't Repay (double)	
	(1) Repay	(2) Don't Repay	(3) Don't Repay (double)	(4) Repay		
JLflex	-0.0230 (0.0434)	-0.00461 (0.0354)	-0.0783 (0.0697)	-0.0461 (0.0662)	-0.00922 (0.0924)	0.0922 (0.143)
Mean of DV in JL	0.172	0.885	0.885	0.241	1.233	1.233
Observations	478	478	478	478	478	478
R ²	0.682	0.718	0.702	0.717	0.642	0.652
Panel B: Punishment in Case of Shock						
Choice in JLflex	Extensive Punishment		Intensive Punishment		(6) No flex left (Double Default)	
	(1) Flex	(2) No Flex	(3) No flex left (Double Default)	(4) Flex		
JLflex	-0.229* (0.128)	0.220* (0.107)	-0.0734 (0.0686)	-0.394** (0.177)	0.303* (0.154)	-0.0917 (0.0927)
Mean of DV in JL	0.496	0.496	0.496	0.735	0.735	0.735
Observations	478	478	478	478	478	478
Adj. R ²	0.644	0.648	0.718	0.640	0.668	0.745

Notes: DV in Columns (1-3) is percent of participants who punish and in Columns (4-6) number of punishment points (0-2). The omitted category is JL. There is only one choice of punishment in case of shock for JL (as participants will default for sure). OLS regressions with individual FE and SE clustered on session level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.3.4 Outcome Simulation

We complement the analysis of individual decisions by a simulation of outcomes considering shock realizations and, in the *JL* treatments, the partner matching. We randomly and independently draw one shock outcome for each period for each individual with probability 0.25. For the *JL* treatments, we randomly match pairs within a session and automatically enforce *JL*. Both the shock outcome and the group matching stay the same across treatments. To avoid results being influenced by this one draw, we repeat this simulation 100 times and take averages of participants' outcomes in these 100 simulations. This simulation allows us to compare default rates across treatments. Note that a borrower (group) is considered in default if she does not repay her loan installment in at least one period.

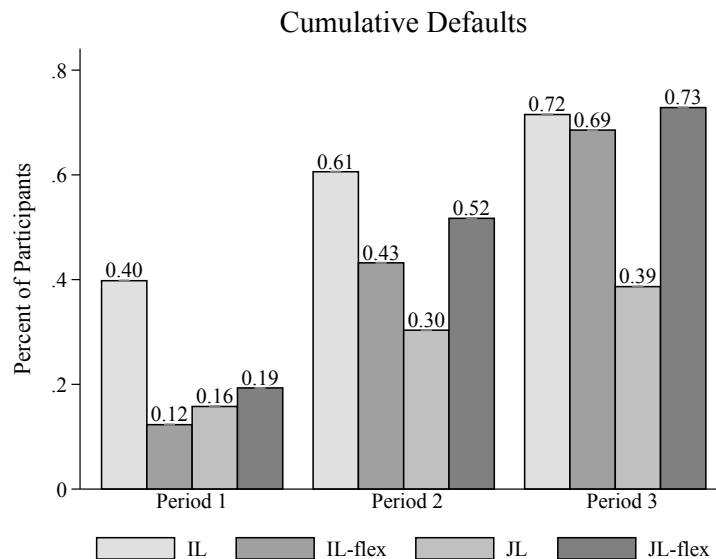


Figure 2.5: Percent of Participants in Default by Period

Figure 2.5 presents simulated cumulative default rates for each period and treatment. After the first period, 40 percent of participants in *IL* are in default. As no insurance for shocks is available in this treatment, the lower bound of this number corresponds to the shock probability of 25 percent. In the other three treatments, at least one form of insurance is possible (either via flexibil-

ity or via joint-liability) which is reflected in significantly lower default rates: 12 percent default in *IL-flex*, 16 percent in *JL* and 19 percent in *JL-flex*.

After the second period, an additional 21 percent are in default in *IL*. The increase in defaults is larger for the flexibility treatments (31 percent in *IL-flex* and 33 percent in *JL-flex*), as repayment morale for the double repayment is lower than repayment in the no-flex treatments (compare Figure 2.2 and 2.3 above): For *IL*, we have at least 74 percent of participants choosing repayment in a given period, while in *IL-flex* repayment of double installments can be as low as 59 percent (in period 3 after using flexibility for insurance). Comparing *JL* and *JL-flex*, a similar picture emerges: At least 74 percent choose repayment in a given period in *JL*, whereas the repayment of flexibility can be as low as 67 percent (in period 3 after misusing flexibility). Even for participants who choose not to use flexibility, the repayment morale in these two treatments can drop to levels as low as 61 (63) percent for *IL-flex* (*JL-flex*). Due to better repayment morale than in the flexibility treatments and the automatic enforcement of joint-liability, only an additional 14 percent are in default in *JL* after period 2.

After the third period, 72 percent of participants are in default in *IL* – either due to their repayment choices or the shock realization. In the two flexibility treatments, insurance via flexibility is no longer available – there is no flexibility token to be used in the third period – such that an additional 26 percent default in *IL-flex*. The overall default rate cumulates to 69 percent. While in *JL-flex* mutual insurance is still possible, the limitations that flexibility imposes on mutual insurance appear clearly in the last two periods. Default rates cumulate to 73 percent in the third period. In our experimental setting, automatic enforcement, not the behavioral implications (see Result 1) of joint-liability is the most powerful shock coping capacity with the lowest possible overall default rates of 39 percent.

Comparing the two individual-liability treatments, the insurance value of flexibility in the first two periods is evident despite the high rates of flexibility misuse. When contrasting the two joint-liability treatments, we find that the additional insurance by flexibility (ten percent of participants default because both partners are hit by a shock in *JL-flex* compared to 17 percent in *JL*, $p < 0.001$,

paired t-test) is outweighed by a substantially higher percentage of cases in which both partners voluntarily default (35 percent in *JL-flex* and ten percent in *JL*, $p < 0.001$, paired t-test).²¹

The comparison of the two flexibility treatments indicates that the behavioral advantages of *JL-flex* discussed above are offset in particular by mechanical implications of defaulting on the repayment of flexibility (see Table 2.3). Due to our experimental setup, defaulting on the double repayment implies group default, thus mechanically doubling the number of participants in default as compared to a double default in *IL-flex*. Overall, 39 percent of participants in *IL-flex* default on the double repayment (17 percent due to a shock and 22 percent due to non-repayment), while 49 percent in *JL-flex* default due to not repaying flexibility (25 percent due to a shock and 28 percent due to non-repayment).²² Indeed, when only examining individual choices and not matching participants to their partner in *JL-flex*, only 29 percent of participants default on their flexibility repayment (14 percent due to a shock and 15 percent due to non-repayment). Thus, an additional 20 percentage points are in default due to mechanical effects of the double repayment in *JL-flex* and the partner matching. These findings are summarized in our last result:

Result 4: *Mutual insurance in JL leads to the lowest defaults due to automatic enforcement of joint-liability. Behavioral benefits of JL-flex are outweighed by mechanical effects in our setting.*

2.3.5 Treatment Effect Heterogeneity

As discussed above, participants bring their experiences and norms as micro-finance clients into the lab. Most clients have joint-liability contracts (82 percent), but we also conducted sessions with clients who have individual-liability (ASHI GRAMEEN Agricultural Program) AGAP loans. This allows us to com-

²¹ Here, we define "voluntarily" as all cases in which participants risk default, e.g. by not-repaying or by not using flexibility in case of a shock.

²² Note that the percentages in *JL-flex* do not need to add up, as there are some cases in which both participants default on the double repayment for different reasons.

Table 2.3: Percent of Participants Defaulting on the Repayment of Flexibility

	IL-flex	JL-flex	JL-flex (no group matching)
Double defaults due to a shock	0.17	0.25	0.14
Double defaults due to non-repayment	0.22	0.28	0.15
All double defaults	0.39	0.49	0.29

pare behavior of individual and joint-liability clients in the different treatments. Carpena et al. (2013) use a natural field experiment to examine the effects of introducing joint-liability in an individual-liability setting and find higher repayment in joint-liability. Based on these findings, one could expect higher repayment of AGAP clients in *JL*. In contrast, when considering the findings of Giné and Karlan (2014) who study the removal of joint-liability and find no difference in repayment, one would expect no differences in repayment in both *IL* and *JL* between standard and AGAP clients. The first panel of Table B.2 in the appendix presents the results that are in line with the latter study: We find no differences in repayment choices in any treatment.

Different types might also lead to differences in behavior. Making use of the within-design feature, we can compare repayment choices of one individual both in *IL* and *JL*. This gives rise to an interesting pattern: While 70 percent of participants do not change their repayment strategy with the liability structure (51 percent repay their full share and 19 percent do not), the remaining 30 percent adjust repayment behavior: 16 percent repay in *JL*, but not in *IL* and 14 percent repay in *IL* but not in *JL*.²³ This results in similar repayment rates in *IL* and *JL*.

Another source of potentially relevant heterogeneity are preferences. In particular, it is conceivable that time preferences play a role in repayment choices and in the misuse of flexibility. Regarding repayment, present-biased participants might discount the continuation value more and thus prefer higher con-

²³ This change in behavior is reflected in beliefs about partners' repayment and punishment choices: 80 percent of those who switch to repayment in *JL* believe that their partner repays and 91 percent think that they will be punished for non-repayment. In contrast, 62 percent of those who repay in *IL* but not in *JL* think that their partner repays and 79 percent believe that they will be punished for non-repayment.

sumption at the expense of default. However, we do not find treatment effect heterogeneity along these lines (see Panel 2 of Table B.2 in the appendix). With respect to the misuse of flexibility, present-biased participants might value the income from two early consumption tokens more than the insurance value of flexibility in the second period. Column 1 of Table B.3 in the appendix shows suggestive evidence that present biased individuals use flexibility more in the first period. However, this effect is not robust to controlling for *JL-flex* (see Column 2).

Risk aversion might play a role in both repayment choices and the use of flexibility: Given that the continuation value of ₱ 100 can only be obtained without having a shock (in *IL*), risk averse participants might choose 'safe' consumption over repayment (and thus lower consumption) with a risky continuation value. Depending on beliefs and their strength, risk aversion might also be relevant in the *JL* treatments as it is not known how the partner decides. The last panel of Table B.2 in the appendix shows that risk does not matter for repayment. Regarding the use of flexibility, risk averse participants might misuse flexibility less in period 1 to keep the insurance value for the second period. Columns 3 and 4 of Table B.3 in the appendix clearly show that risk aversion does not matter for the use of flexibility, either.

2.3.6 Robustness

RANDOMIZATION AND ORDER EFFECTS We have randomly allocated centers to either *IL* or *JL* sessions. In both types of sessions, we administer the *IL* treatment, such that choices in this treatment can serve as a randomization check. Column 1 in Table B.4 in the appendix shows that the number of tokens repaid is the same in both session types. This also holds true if we examine the three periods in *IL* separately (Column 2). Given the additional balance of observable characteristics shown in Table 2.1, we are confident that our randomization was successful.

However, as we did not randomize the order of treatments, one might be concerned about order effects. Several points alleviate these concerns. First, since

we elicited choices with the strategy method and only realized shock outcomes, matching (where applicable) and corresponding earnings after all decisions had been made, participants did not receive intermediate feedback that would have led to learning. Second, we can assess "rationality" of choices by examining the repayment decision after default in each treatment. As pointed out above, absent norms, participants should choose not to repay once in default. Columns 3-5 in Table B.4 in the appendix show that about 23 percent of participants still repay after default. If participants would learn with the progress of the experiment, this fraction should decrease in the flexibility treatments. This fraction, however, is the same across treatments with all coefficients of the treatment dummies being small and not statistically distinguishable from zero, even when focusing on within-session (Column 4) and within-individual variation (Column 5). This alleviates the concern of learning.²⁴

Lastly, we can use the punishment decisions in *JL* sessions in *JL* and *JL-flex* to verify whether the same choice situation results in the same punishment decision. Two punishment choices are the same in the two treatments: Whether to punish the partner for repayment (of one token) and whether to punish the partner for non-repayment (of one token). The first two bars in Figure 2.4 show that extensive punishment for repayment (17 vs. 12 percent) and non-repayment (89 vs. 89 percent) in *JL* and *JL-flex* is very similar. Columns 1 and 2 of Panel A in Table 2.2 provide parametric evidence that they are indeed indistinguishable. Participants thus make consistent choices and the same "mistakes" in the different treatments. Therefore, we are confident that not the order but rather the treatments cause behavior.

EXCLUSION OF PARTICIPANTS In our main analysis, we excluded participants with a limited understanding based on their answers of the test questions. To make sure that this is not driving our results, we repeat the analysis for our main results including all participants. Table B.5 in the appendix shows that the share repaid, the misuse of flexibility and its repayment are robust to the inclusion. The same holds true for punishment choices: In Table B.6 in the

²⁴ Note that repayment after default is not correlated with the number of correctly answered test questions (Spearman's ρ and respective p-values in parentheses: *IL* 0.002 (0.969), *JL* 0.094 (0.129), *IL-flex* -0.017 (0.780) and *JL-flex* 0.111 (0.090)).

appendix, we replicate Columns 1-2 and 4-5 of Panel B in Table 2.2 with nearly identical coefficients.²⁵ The exclusion is thus not a driver of our results.

CHOICE REVERSALS The choice reversals are a robustness test regarding how our results would change with more present bias. With respect to the share repaid, we find lower repayment in the revision (Column 1 in Table B.7 in the appendix). It appears that especially in *JL-flex*, more present bias would lead to lower repayment rates. Only in *JL*, the revision does not lead to statistically significantly lower repayment rates.

The second result that might change due to more present bias is misuse and repayment of flexibility. Pooling the two flexibility treatments and time periods 1 and 2, we find that misuse is higher in revised choices (Column 3 in Table B.7). Column 4 pools the two periods and shows that participants misuse flexibility less (as compared to the *IL-flex* treatment) both in *JL-flex* and the revision of *JL-flex*. In *JL-flex*, the revision does not change misuse but in *IL-flex*, it increases misuse. Joint-liability thus significantly reduces the misuse, even in a situation in which consumption is imminent and thus very tempting (as can be seen in the increase in misuse in the reversion in *IL-flex*). However, if flexibility is used in *JL-flex*, it is repaid significantly less in the revision (see Column 6).

2.4 DISCUSSION

2.4.1 *Design Choices*

We designed our experiment as realistically as possible while keeping it as simple as necessary to guarantee good understanding of our participants. Our results are influenced by assumptions made to simplify the experimental design. First, borrowing groups in our design consist of two borrowers instead of five borrowers as in the real-life borrowing situation. We seize the advantage that a two-person borrowing group is easier to explain and that strategic

²⁵ For ease of presentation, we only focus on the main results. All other results are equally robust to the inclusion of participants with limited understanding.

considerations regarding the partner's choices can be made. This is particularly important for the strategy method without realization during the decision making phase of the experiment. In five person groups, it would not be feasible to apply the strategy method, and decisions could only be elicited using actual realizations of idiosyncratic shocks and partners' decisions. Of course, this undermines potentials for risk-sharing in larger groups in which risks are diversified and more persons can offer mutual insurance when one member suffers a shock.

Second, we assume that joint-liability is automatically enforced. This reduction in the decision space is important to focus on repayment choices and ex post moral hazard and is widely used in other lab experimental studies (Abink et al., 2006; Cassar et al., 2007). It is also quite realistic: In our partner microfinance institution loan officers usually wait until repayment installments have been made, irrespective of who made the payment, before they close the center meeting. This assumption mechanically decreases default in the joint liability treatments: As long as one borrower can repay both loans, she will do so automatically.

Third, we study a short time horizon of three repayment periods in our experimental design whereas in reality, a loan cycle lasts at least 25 weeks. Three periods is the smallest possible number of periods to test this version of flexibility, but it substantially limits the number of possible flexibility tokens – one in our design. In addition, the last period in which no flexibility is possible and which is needed to make up for all repayment installments delayed by flexibility in the previous period, carries more weight with such a low number of periods. This is an important aspect when deducing policy implications from our framed field experiment.

Fourth, the idiosyncratic shocks in our experiment are modeled as a very severe shock that destroys all available income in a given period and the probability that the shock occurs is quite high with 25 percent. The simplifying assumption of entire income loss prevents partial repayment choices which would complicate the experimental decisions unnecessarily. However, it oversimplifies reality and neglects possible partial mutual insurance. The probability of 25 percent is easy to understand and high enough to have an impact

on decisions in our design. In addition, it sets the baseline level of defaults in *IL* high enough such that we can detect meaningful reductions in other treatments.

Fifth, by design borrowers have to pay half their income for their loan installment and their full income in the flexibility treatments when they have used their flexibility token. Repaying the double installment after flexibility use becomes very unattractive: No consumption is possible when the double repayment is made. For the joint-liability treatments this assumption implies that partners cannot mutually insure each other in the period in which they use their flexibility token and in the following period. Default increases due to this very strict assumption that renders flexibility and mutual insurance pure substitutes in certain combinations of shocks and repayment choices. In reality, it is very unlikely that a double repayment cannot be insured by the (five-person) group, thus defaults may be significantly lower. However, in reality, shocks may last for more than one period which has to be considered when designing an actual flexible loan product.

Despite all these simplifying assumptions, we can study the interactions of joint-liability and repayment flexibility in a clean manner and are able to detect underlying mechanisms. For instance, it has been documented before that joint-liability per se is not responsible for high repayments (Attanasio et al., 2015; Giné and Karlan, 2014). We can draw a more differentiated conclusion and we can show that this is due to different behavioral types. Our results show the potential benefits when joint-liability and flexibility are interacted: The responsible use of flexibility is enhanced and anti-social punishment is reduced. These behavioral insights should be taken into account when further studying the improvement of microcredit products.

2.4.2 *Implementation of Flexibility*

There are several ways in which repayment flexibility could be implemented. In this study, we opted for a deferral that needs to be repaid in the next period. While this version of flexibility is helpful for small shocks that last only one

period and are not correlated across time, the burden of the double repayment does not seem to be negligible. The clear advantage of this implementation is that, in theory, it can be flexibly used (or not used) throughout the cycle and one could easily add another flexibility token in a given loan cycle.

Another way to implement flexibility would be a "repayment pause" that simply skips a repayment but instead of making up for it with the next installment, it would be priced into all remaining installments. While the repayment of the pauses is smoothed over the cycle and thus reduces the burden of comparatively large installments, it would be necessary to determine ex-ante how many pauses will be used (non-use would not be an option). While it was not possible to thoroughly test this version of flexibility, we conducted discrete choice experiments with a subsample of clients to get an idea of the demand of these two versions of flexibility (see Appendix B.3 for a detailed description). We find that clients would be willing to pay an additional ₱552 in interest charges for the repayment pause as compared to a standard loan, but no additional willingness to pay for the flexibility token as we implemented it. This shows that the repayment pause is a promising way of offering flexibility that should be explored in future research.

2.5 CONCLUSION

In our study, we analyze an innovative design feature for microloans: flexibility in loan repayment. It offers borrowers the option to self-insure against income shocks. However, lenders may be reluctant to offer flexibility since it could deteriorate repayment morale. While repayment morale is usually high in joint-liability lending due to mutual insurance and peer monitoring, peer pressure is usually excessive with high levels of anti-social punishment. We propose that interacting joint-liability and repayment flexibility helps to seize advantages of both loan features while reducing the disadvantages.

Our results from the framed field experiments with microfinance borrowers in the Philippines partially confirm our hypotheses: There are potential benefits when joint-liability and flexibility are interacted as the responsible

use of flexibility is enhanced and anti-social punishment is reduced. However, we do find lower repayment rates with flexibility, irrespective of the liability structure. While we have to make strong simplifying assumptions to keep the experimental design straightforward and understandable, we learn important aspects for flexible loan features in real-life loan products. First, the grade of substitutability between mutual insurance in joint-liability and flexibility is crucial for default levels. In our design, mutual insurance is not possible after the flexibility token has been used which results in high default levels. If mutual insurance is still possible after using flexibility, default levels will be lower. Second, the repayment installment relative to the income of borrowers is an important aspect in determining the temptation to default on the loan repayment, in particular the double repayment to make up the payment deferred to a later period by flexibility. In our design, the loan repayment installment constitutes 50 percent of the income in this period. A double repayment requires using the total period income for repayment. With a lower repayment burden also for the double repayment, temptation to default on repayment is reduced. Third, the number of flexibility tokens in relation to loan repayment periods is crucial for the restrictions imposed by make-up payments for flexibility. In our design, one flexibility token could be used in two out of three periods so that repayments can only be delayed for one period and make-up payments are due in the next period. A higher number of repayment periods will ease these restrictions: Payments can be delayed for longer time periods and make-up payments can be made when income levels are high.

More research is necessary to identify how these aspects translate to an implementable design for flexible features of real loans. Our findings guide the design of flexible repayment features that may help to improve the impact of microloans on borrower welfare.

I LIE? WE LIE! WHY? EXPERIMENTAL EVIDENCE ON A DISHONESTY SHIFT IN GROUPS*

"I did steal from Enron. We stole from Enron."

The Enron Trial: Testimony of Andrew Fastow (former CFO of Enron)

"The conduct was fairly open and notorious, I would say.

It was no great secret what we were doing."

Christopher Loehr (former analyst for Enron)

3.1 INTRODUCTION

Groups and organizations sometimes fail to comply with a moral norm. They lie, they cheat, they are dishonest, they are corrupt and they commit fraud. However, it is not organizations that take those decisions; it is individuals that are part of the organization. Can we thus explain undesired behavior in organizations simply by aggregating individual failures to comply with the norm? Or, are there other elements inherent to the organization or to its structure that can help us better understand how undesired behavior emerges? And, how does undesired behavior of individuals differ from behavior within or by an entire organization? While these are relevant questions, surprisingly little empirical evidence exist (Conrads et al., 2013; Sutter, 2009). This chapter addresses these questions in a parsimonious setup that allows us to identify some of the potential reasons for collective failure to follow a moral norm or to comply with desired behavior.

Recent years have provided several prominent examples of unethical behaviors in groups and organizations. Fraudulent accounting methods and malpractice of groups of executive officers have led to the marked bankruptcies of WorldCom and Enron. More recently, it has been discovered that *inter alia*

* This chapter is based on joint work with Martin G. Kocher and Simeon Schudy and is forthcoming in *Management Science*.

the German car producer Volkswagen has sold diesel cars with emissions certificates based on potentially faulty information.¹ To improve emission test results, Volkswagen has allegedly installed software in their diesel engines that could detect when the cars were on the test stand and adjust the engine performance accordingly. After investigations by the Environmental Protection Agency (EPA), newspaper articles say that "VW must have had a chain of management command that approved fitting cheating devices to its engines".² However, it is not only for-profit firms that are involved in unethical behavior: There are cases of charities that commit embezzlement, sports organizations and executives that generate financial scandals or engage in morally and legally questionable practices in the context of doping and sports teams that violate established norms.³

This study provides a twofold contribution. First, we implement a parsimonious laboratory setup to investigate whether groups (as our proxy for small organizations) are indeed more inclined to engage in dishonest or unethical behaviors than individuals, as casual observation and some previous results in the literature suggest (e.g., Chytilova and Korbelt, 2014; Conrads et al., 2013; Gino et al., 2013; Muehlheusser et al., 2015; Sutter, 2009; Weisel and Shalvi, 2015). We find that the answer is affirmative. Individuals lie less frequently when deciding alone as compared to groups. Second, we discuss and single out explanations for the "dishonesty shift" in groups. There are several candidate explanations: (i) a simple aggregation of individual inclinations as a consequence of aggregation rules (i.e. decision making procedures) within the group; (ii) the incentive structure inherent to many group decisions (often-times, all members share group payoffs equally and an individual deviation from either of the strategies – behaving dishonest or honest – can sometimes reduce payoffs for everyone dramatically); (iii) the decreased observability of one's actions within a group, potentially making the individual less account-

1 See <http://www.telegraph.co.uk/finance/newsbysector/industry/11880433/How-has-Volkswagen-tricked-the-US-over-car-emissions.html>, last downloaded: Feb 3, 2016.

2 See also <http://www.bbc.com/news/business-34324772>, last downloaded: Nov 29, 2015.

3 See e.g. <http://www.nytimes.com/2015/05/20/business/4-cancer-charities-accused-in-ftc-fraud-case.html>,

<http://www.economist.com/blogs/gametheory/2015/09/fifa-corruption-scandal> and

<http://www.bbc.com/sport/olympics/36542577>, last downloaded: June 16, 2016.

able for their actions when they are group members; and (iv) the deliberation process inherent to group interaction involving, for instance, the exchange of arguments and learning about the strength and prevalence of a norm.

Our laboratory experiment uses a variant of the die-rolling task introduced by Fischbacher and Föllmi-Heusi (2013). Participants are asked to report the result of a die roll and their payoffs depends on their reports.⁴ Hence participants face a trade-off between being honest by reporting the true number and potentially forgoing a monetary profit (i.e. following the norm of honesty), on the one hand, and being dishonest and potentially earning more (i.e. violating the norm of honesty), on the other hand. We implement individual decision making situations and group decision making situations using an experimental design that allows us to study behavioral change (within subjects) across several (between subjects) treatments. Our setup reduces the effects from the decreased observability of one's actions as a member of a group and enables us to assess whether the exchange of arguments and learning about the strength and prevalence of a norm or whether incentive structures and preference aggregation are the main drivers of the observed dishonesty shift.

In the die roll paradigm, a pure payoff maximizer would want to always report the number that yields the highest monetary payoff, regardless of the actual die roll. This is true for both the individual and the group decision making situation. If one assumes sufficiently high moral costs of lying, individuals might want to report truthfully, and the group outcome then depends on the aggregation of individual preferences. For instance, unanimity should lead to less lying than other aggregation mechanisms. Adding social image concerns, accountability considerations and changes in the perception of the norm, group interaction can drag the comparison between the individual and the group setting in any direction – towards more or less dishonest behavior (Bénabou, 2013; Bénabou and Tirole, 2006, 2011; M. Dufwenberg and M. A. Dufwenberg, 2018; Falk and Tirole, 2016). Ultimately, the question of whether groups or individuals are more or less dishonest (and the causes and conse-

⁴ Our variant of the task is computerized and allows the experimenter to observe whether individuals misreport (for a similar idea, see also Gneezy et al., 2018). We explain the variant and the reasons for our design choice in more detail in Section 3.2.

quences of any potential shift from the individual to the group setting) can only be answered empirically.

A laboratory experiment has several advantages when it comes to the identification of the effects we are interested in: It allows us (i) to exogenously vary group membership and incentives for the group members to behave dishonestly; (ii) to observe individual behavior before individuals become members of a group as well as behavior/communication when the group decides; and (iii) to elicit individual beliefs about dishonest behavior of others. Naturally, there are limits to laboratory experiments. The task that we use is specific in the sense that it implies a zero fine for violating the norm of honesty. In bearing with the growing experimental literature on cheating and lying, this enables us to compare our results to existing studies that focus on individual decisions to lie. We implement group decisions in small groups with anonymous real-time chat interactions in order to keep as much experimental control as possible. These design choices lend themselves to extensions that bring the experimental setup closer to existing organizations with their hierarchies and with face-to-face interactions. Ultimately, field experiments are a desired methodology. Our aim here is to establish a set of explanations in a rigorously controlled environment that feeds into the design of future studies, relaxing some of our restrictions systematically.

The results from our setup reinforce the conclusion from the small existing literature: Groups are (much) more inclined to lie than individuals. Drawing on the older psychology literature that detected a shift from individuals to groups in terms of risky decisions making, coining it the "risky shift" (Pruitt and Teger, 1969; Teger and Pruitt, 1967), we refer to our finding as the "dishonesty shift". However, our main contribution is providing an explanation for this shift: The shift can neither be explained by a higher level of strategic sophistication of groups than individuals (Sutter, 2009), nor by a decreased observability of one's action as a group member. Both explanations are excluded by our design. Using appropriate treatment variations, we can also rule out that groups lie more than individuals because the other group members benefit from lying (Gino et al., 2013) or because group decisions require unanimity.

Our findings provide strong evidence for the importance of two mechanisms that drive the dishonesty shift and that are inherent to almost any group interaction: communication and learning about norm compliance. Communication exposes group members frequently to arguments in favor of violating the norm. The exchange of arguments and talking to people that argue in favor of violating the norm also changes the norm perception. We show that the expectation that other people (out-of-sample) lie increases significantly after the group interaction. A detailed analysis of the protocols from the group interaction suggests that groups lie more because communication enables them to justify dishonest behavior in a different way than individuals. Further, we find that the dishonesty shift in groups is very strong such that the group composition (in terms of the number of initially dishonest group members) only weakly affects the extent of dishonesty in a group.

Understanding the mechanisms that contribute to the dishonesty shift is essential as it is a prerequisite for designing institutions and incentives that are conducive to norm compliance. Our results show that the availability and exchange of arguments that justify norm violating behavior is an important aspect. Such exchange is occurring naturally in groups where group members discuss how they should act. A next step could be to analyze potential mechanisms in the group interaction that are able to counterbalance this effect such as reminders of the norm or other related interventions.

The remainder of the chapter is organized as follows: Section 3.2 describes the details of our experimental design and procedures, gives an overview of the literature, and provides behavioral predictions. In Section 3.3, we present the results from our experiment. Section 3.4 discusses our findings, and Section 3.5 concludes the chapter.

3.2 EXPERIMENTAL DESIGN, RELATED LITERATURE AND PREDICTIONS

3.2.1 *Experimental Design and Procedures*

We use a variant of the die-rolling task introduced by Fischbacher and Föllmi-Heusi (2013), in which a pure payoff maximizer would want to always report the number that yields the highest monetary payoff (irrespective of the observed die roll). Experimental participants see a video of a die roll on their computer screen. The computer randomly chooses one out of six videos, each showing one possible outcome. Participants are informed about this, i.e. they know that the computer chooses each video with the same probability. The outcome of the die roll in the video is clearly visible for about 10 seconds. The participants' task is to enter the outcome shown in the video on the next screen, in a field stating "die number seen:_____". Participants can enter any number between 1 and 6. Importantly, payoffs depend on the number entered, not on the number actually seen. We use the same payoff structure as the original experiment, i.e. die numbers 1 to 5 yield one to five points respectively and die number 6 yields zero points. As participants can enter any number between one and six, they have the possibility to report dishonestly. As we explicitly asked participants to report the number *seen*, we refer to deviating from truthful reporting as "dishonesty" or "lying" for the rest of the chapter.

Die rolls shown in the video are chosen randomly by the computer and known by the experimenter such that participants cannot disguise their lies.⁵ Thus, in contrast to the original die-rolling task, misreporting the die number in our experiment is clearly dishonest, but liars cannot disguise (for a similar task with observability, see also Gneezy et al., 2018). This is a desirable feature for our purposes for at least two reasons. First, the paradigm ensures that groups do not lie more than individuals because it is *easier to disguise* lies in a group, as individual decisions are clearly observable by the experimenter in individual and group treatments. Second, the paradigm allows us to study in a within-subject design how individual behavior changes (i.e. when reporting

⁵ Note that participants are informed about the experimental procedures. Hence, incomplete lying should occur rarely in our experiment.

alone as compared to when reporting after communication in a group). Further, the task is easy to understand and we can thus exclude that groups have a significantly *better understanding* of the task. Nonetheless, full observability may affect the level of lying (as compared to no observability). Our main analysis focuses therefore on the relative comparison between individual decision making and group decision making (and not on the absolute levels of dishonest behavior). Interestingly, direct comparisons between situations of full observability by the experimenter and full privacy (Gneezy et al., 2018) show minor differences in the absolute extent of lying and similar conclusions can be drawn from comparable experiments that introduce an anonymous observer of the private die roll other than the experimenter (see, e.g., Baeker and Mechtel, 2015; Houser et al., 2016; Van de Ven and Villeval, 2015).

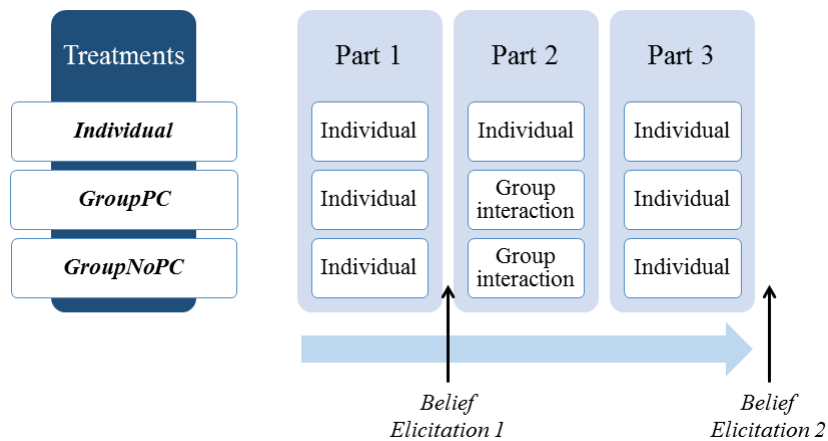


Figure 3.1: Experimental Design and Elicitation of Beliefs

Notes: Subjects are randomly allocated either to treatment *Individual*, *GroupPC* or *GroupNoPC* (between-subject) and participate in all three parts (within-subject).

We implement a mixture of a within-subject and a between-subject design with three different between-subject treatments (*Individual*, *GroupPC* and *GroupNoPC*). Figure 3.1 illustrates the experimental design. Each treatment consists of three independent parts, for which instructions are displayed on-screen at the beginning of each part. One of the three parts is randomly determined to be payoff-relevant at the end of the experiment and this is common knowledge. In the *Individual* treatment, participants make the same decision in each of the three parts: They see a video of a die roll on their screen and are asked to enter the die number seen on the next screen. In the two group treatments, the

first part and the third part are equivalent to the decision in the same parts in *Individual*. In the second part of the group treatments, subjects decide in groups of three participants that are randomly assembled. Each participant in a group sees the same video (and this is common knowledge). After group members have seen the video, they have five minutes' time to discuss in a group chat. The real-time chat allows for free-form communication without revealing one's identity (excluding the possibility of threats and side-payments). All three group members see all messages that are sent in the chat. The three members can also decide unanimously to leave the chat before its automated ending. After the chat, each participant privately enters the number (as in *Individual*), making each individual decision fully observable by the experimenter in all treatments. The two group treatments reflect different organizational structures as we vary whether or not group members earn a common payoff. In *GroupPC*, participants face a decision with payoff commonality. Each member of a group has to enter the same number to receive a payoff and each participant receives the payoff that corresponds to the number entered. If entered numbers differ within a group, all group members receive zero payoffs. Such a protocol implements a strong unanimity component. In *GroupNoPC*, participants face no payoff commonality, i.e. they receive payoffs according to the number they enter, irrespective of the number entered by the other group members. *GroupNoPC* is thus identical to *Individual* except for the group chat after having seen the same video. It is important to notice that there is no pressure to contribute to the chat and that the content of the chat is totally up to the participants (i.e. they could talk about the weather, the weekend and anything they wanted, except for revealing their identities in any way).

As the first part is identical in all treatments (and also decisions are very similar), we can use the between-subject comparisons between *Individual* and *GroupPC* as well as *Individual* and *GroupNoPC* in Part 2 to establish the difference in dishonest behavior between individual and groups (i.e. the potential existence of the dishonesty shift). The comparison between behavior in *GroupPC* and *GroupNoPC* in Part 2 allows us to address the effect of payoff commonality, i.e. the relevance of the other-regarding concerns argument in

group decision making that involves a trade-off between payoff maximization and norm compliance.⁶

To observe whether the group chat changes participants' beliefs about others' behavior in terms of (dis)honesty, we elicit our participants' beliefs about the lying behavior in a past experiment (i.e. in the baseline condition of Fischbacher and Föllmi-Heusi, 2013). Following Fischbacher and Föllmi-Heusi (2013), we elicit participants' beliefs about the distribution of payoffs. We inform our participants that they have to guess the behavior of other participants in a similar, previously run experiment (from now on "reference experiment"). Participants then guess the shares of participants that earned a specific payoff, i.e. 0 points, 1 point, 2 points, etc. Implicitly, the distribution provides the belief about honesty of similar decision makers. Participants earn five euros if they guessed all shares correctly. For every percentage point deviation from the correct shares we reduce participants' payoff by 0.04 euros. The minimum payoff they can earn in the belief elicitation task is 0.50 euros. To avoid potential problems of hedging, we randomly selected one of the two belief elicitation tasks at the end of the experiment to be payoff-relevant.⁷ Two aspects of the belief elicitation are important: First, the way we elicit beliefs makes it clear to participants that we ask them about past behavior of other participants, which is exogenous to the current experiment. Second, we make clear that the data from the reference experiment is exactly the same for Belief Elicitation 1 and Belief Elicitation 2. Hence, changes in elicited values indicate a change in beliefs that can only be explained by the experience in our experiment.

During our experiment, each participant sat at a randomly assigned, separated PC terminal and received a copy of printed general instructions upfront.⁸ These informed participants that the experiment consists of three independent parts (one randomly determined to be payoff-relevant at the end) and that the specific instructions for each part would be displayed at the beginning of

⁶ As our focus is on how lying behavior changes in groups as compared to individuals (Part 1 and 2) we do not provide a detailed analysis of the data from Part 3. Lying rates in Part 3 are high (71.8 in *Individual*, 82.1 in *GroupPC* and 84.6 in *GroupNoPC*) and do not differ significantly across treatment.

⁷ This random draw was independent of the random draw that determined payoffs for the lying task.

⁸ The translated version of the instructions can be found in Appendix C.4.

each part on the participants' computer screen. A set of on-screen control questions ensured the understanding of the game in each part.⁹ Each part started only after all subjects answered the control questions correctly. Participants could fail the control questions repeatedly and were allowed to ask the experimenter to provide an explanation. No form of communication was allowed during the experiment (except through the computerized chat environment in the group treatments). We conducted all sessions at the Munich Experimental Laboratory for Economic and Social Sciences (MELESSA) at LMU Munich. The MELESSA subject pool includes undergraduate and graduate students of all fields of study. The data for the main treatments was collected over 14 sessions between June and September 2015, with 273 participants in total (39 in *Individual*, 117 in *GroupPC* and 117 in *GroupNoPC*).¹⁰ In the first two sessions, the computer randomly selected for each group in each part one out of the six possible videos of a die roll with equal probability. To increase statistical power and simplify non-parametric comparisons between treatments, we used these randomly determined sets of videos in the later sessions, such that in each part, the same die rolls were observed in each treatment. This procedure ensures that die rolls displayed are held constant across treatments and we can compare the number of dishonest reports across treatments, holding the monetary costs of being honest constant. In our main analysis we compare the individual-level data of the control treatment to the collapsed data from the group treatments.¹¹ Hence, we have 39 statistically independent observations in each treatment.

Participants received a show-up fee of four euros that was added to the earnings from the experiment. Subjects could earn points, where one point was equal to two euros. The experiment took about an hour. The average income

⁹ As Part 3 was identical to Part 1, we displayed the control questions in Part 3 again, but showed the subjects the correct answers. The correlation between the number of times participants failed to answer the control questions correctly and the extent of (dis)honesty is small and fails to be statistically significant (individual level data, Spearman's ρ , Part 1: -0.096, $p=0.112$; Part 2: -0.027, $p=0.663$). Hence, problems in understanding the experimental procedures are unlikely to explain variation in lying behavior between treatments in the experiment.

¹⁰ Two sessions ($n=39$) of an additional control treatment (*IndividualDeliberation*) that allows us to look into one of the potential explanations were conducted in February 2016 (see also Section 3.4).

¹¹ We collapse the group treatment data on the behavior of the group median to ease applicability of non-parametric tests. Using group averages (where applicable) yields very similar results.

(including the show-up fee) amounts to 14.62 euros. The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). We recruited participants using the online recruiting system ORSEE (Greiner, 2015) and excluded all subjects with previous experience in die rolling tasks or other similar experiments.

3.2.2 *Related Literature and Behavioral Predictions*

In our task, a rational selfish individual would want to report a "6", regardless of the actual die roll. This is true for both the individual and the group decision making situation. The experimental literature in economics has shown that frequently individuals are willing to forego monetary benefits to behave honestly in such situations (see, e.g., Abeler et al., 2014; Cappelen et al., 2013; Erat and Gneezy, 2012; Fischbacher and Föllmi-Heusi, 2013; Glätzle-Rützler and Lergetporer, 2015; Gneezy, 2005; Gneezy et al., 2018, 2013; Kröll and Rustagi, 2017; Lundquist et al., 2009; Mazar et al., 2008). This contrasts behavior of purely self-interested payoff maximizers but is in line with models that incorporate moral costs of lying.

Recent work has started to investigate dishonesty in groups (e.g., Baeker and Mechtel, 2015; Chytilova and Korbelt, 2014; Conrads et al., 2013; Muehlheusser et al., 2015; Sutter, 2009). The group decision making setup adds several dimensions to the problem. First, it adds the aggregation problem of individual preferences in case they are not completely aligned. Second, the group setup might make social aspects more relevant: hiding behind the other group members (accountability), payoff commonality and the need to coordinate as well as social image concerns. Based on existing work, we may expect groups to lie more than individuals for at least three reasons: First, groups may generally apply significantly higher levels of reasoning than individuals (Kocher et al., 2006; Kocher and Sutter, 2005). Thus, groups may lie more as they have a *better understanding* of the game (Sutter, 2009). Second, groups may lie more, as it can be *easier to disguise* lying in groups than individual lies (Conrads et al., 2013). Both arguments should play a minor role in the context of our exper-

imental design. The task is easy and individual choices are perfectly observable by the experimenter in all treatments. Third, recent work suggests that groups could lie more because *others may benefit* as well from dishonest behavior (Gino et al., 2013; Weisel and Shalvi, 2015; Wiltermuth, 2011). The comparison between treatments *GroupPC* and *GroupNoPC* will address this argument explicitly. However, there are also good arguments against a stronger prevalence of lying among groups than among individuals or arguments that do not provide a signed prediction. First, social image concerns could be stronger when deciding in a group (see e.g. Bénabou, 2013; Bénabou and Tirole, 2006, 2011; M. Dufwenberg and M. A. Dufwenberg, 2018).¹² Second, changes in the perception of the norm in the course of the group interaction can drag the comparison between the individual and the group setting towards more or less dishonest behavior. To be conservative, we formulate the following null hypotheses:

H1: The shares of dishonest reports in Part 2 in *Individual* and *GroupPC* do not differ.

H2: The shares of dishonest reports in Part 2 in *GroupPC* and *GroupNoPC* do not differ.

3.3 RESULTS

We structure the results section as follows. First, in Section 3.3.1, we present results on whether groups lie more than individuals and if so, whether dishonesty becomes more prevalent in groups due to payoff commonality and how group communication affects beliefs about norm compliance (out-of-sample). Then we analyze the effect of group composition in terms of Part 1 liars on group decisions (in Section 3.3.2). In Section 3.3.3, we provide a content analysis of the group chat. Section 3.3.4 provides additional robustness tests for our main results, controlling for personal characteristics of group members.

¹² Any effects from social image concerns may however be less pronounced in our setting, as the group interaction is anonymous and the experimenter observes individual behavior in all treatments.

3.3.1 Dishonesty Shift and Payoff Commonality

Figure 3.2 illustrates participants' reporting behavior in Part 1, 2 and 3 by showing the numbers subjects reported (y-axis) conditional on the number they have seen (x-axis). Note that the figure organizes the axis based on points, which means that the "0" is shown at the origin. It is clear that we observe either honest reporting (dots along the 45° line) or dishonest reporting by stating the number that yields the highest returns, i.e. "3".¹³ The fraction of subjects misreporting in Part 1 ranges from 31 to 41 percent and – as Part 1 is identical for all treatments – does not differ significantly between treatments (Fisher's exact tests, *GroupPC* vs. *Individual*: $p=0.810$; *GroupNoPC* vs. *Individual*: $p=0.479$; and *GroupPC* vs. *GroupNoPC*: $p=0.816$). In Part 2, we observe significantly more lying in *GroupPC* than in *Individual*. The fraction of dishonest reports amounts to 89.7 percent in *GroupPC* whereas in *Individual*, 61.5 percent of participants misreport their number (Fisher's exact test, $p=0.007$). The results are qualitatively similar and remain statistically significant if we exclude participants who saw a "0" in Part 1 (who had no monetary incentive to lie).¹⁴ We thus reject $H1$.

In *GroupNoPC*, the fraction of dishonest reports amounts to 86.3 percent which is also significantly larger than the 61.5 percent in *Individual* (Fisher's exact test, $p=0.007$), but does not differ significantly from the median of misreports in *GroupPC* (Fisher's exact test, $p=1.00$). Hence, we cannot reject $H2$. We summarize our findings in Result 1.

Result 1: *Groups lie significantly more than individuals, irrespective of payoff commonalities.*

¹³ The absence of incomplete lying is in line with the fact that there is nothing to disguise in our experiment (in contrast to Fischbacher and Föllmi-Heusi, 2013). It also confirms the findings of a similar individual treatment in Gneezy et al. (2018) that uses an "observed game".

¹⁴ As a robustness test, we conducted all analyses reported in this chapter also for a subsample that excludes participants who have seen a "0" in Part 1. All results remain unchanged, i.e. they are qualitatively similar and differences remain significant, except for one result concerning the influence of the group composition on misreporting (which we discuss in Section 3.3.2). Appendix C.2.1 provides more details.

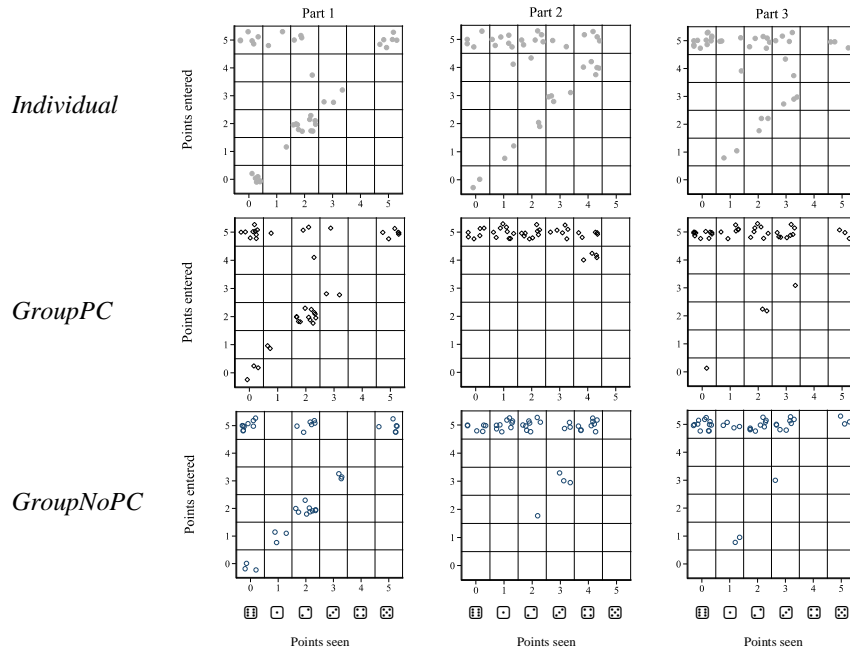


Figure 3.2: Numbers Seen and Numbers Reported across Treatments and Parts
Notes: In *Individual*, each dot represents one participant's reported number. In the group treatments, each dot represents the median number reported in a group. For readability, the figure is jittered and shows points (i.e. a die role of "1") is shown as 0 points).

Both payoff commonality and mere communication in *GroupNoPC* strongly foster coordination. In *GroupPC*, all 39 groups coordinate, i.e. all members enter the same number (see Table 3.1). In *GroupNoPC*, group members do not have to enter the same number to receive a positive payoff and, as compared to *GroupPC*, slightly less members do so (Fisher's exact test, $p=0.025$). Still, 33 out of 39 groups coordinate after the group chat. In five out of the six remaining groups, two out of three members report dishonestly. To compare coordination in the group treatments with a benchmark, we simulate "coordination" behavior in *Individual* based on participants' actual reporting from Part 2 in *Individual*.¹⁵

As can be seen in Table 3.1, actual coordination rates in both *GroupPC* and *GroupNoPC* are significantly higher than coordination in simulated groups in

¹⁵ Following the literature on peer effects (Falk and Ichino, 2006), we randomly place three different individuals who have seen the same number (i.e. have similar costs of lying) into a group and repeat this procedure until we have generated the same number of groups that have seen the particular number in each of the two group treatments. For this set of groups, we check how many groups coordinate. In total, we simulate 500 sets of groups in this way and take the average over how many groups coordinate in each simulated set.

Table 3.1: Coordination among Group Members in Part 2

	Coordination	Number of dishonest reports	Simulated groups	<i>Group PC</i>	<i>Group NoPC</i>
Lying decision of indi- viduals Part 2	Yes (all honest)	0	2	4	3
	No	1	11	0	1
	No	2	17	0	5
	No (all dishonest)	3	3	0	0
	Yes (all dishonest)	3	6	35	30
		Σ	39	39	39
Coordination rate			20.5%	100%	84.6%

Notes: Simulated groups are based on Part 2 decisions in *Individual*. Note that for three simulated groups all group members lie, but they still do not coordinate (due to partial lying by one member.)

Individual (Fisher's exact tests, $p < 0.001$ for the two comparisons). We summarize our findings in Result 2.

Result 2: *Communication increases coordination.*

Next, we focus on how the group interaction affects participants' beliefs about reporting behavior of subjects in a reference experiment. We elicited these beliefs once before Part 2 and once after Part 3. Figure 3.3 shows how participants' beliefs about reporting behavior of subjects in the reference experiment changed from Part 1 to Part 3 for the three treatments. In *Individual*, the expected share of subjects reporting a "☹" in the reference experiment increases from Part 1 to Part 3 by 12.8 percentage points in *Individual* whereas the expected fractions of subjects reporting "☺" and "☐" decrease. In contrast, when participants interacted in a group, their beliefs change to a much larger extent. The expected share of subjects reporting a "☹" in the reference experiment increases by 35.7 (31.3) percentage points in *GroupPC* (*GroupNoPC*). We thus observe a significant change in the skewness of beliefs on the reported numbers in the reference experiments for group treatments (Wilcoxon signed-rank exact test, *GroupPC*: $p = 0.002$; *GroupNoPC*: $p < 0.001$) but not for *Individual*

(Wilcoxon signed-rank exact test, $p=0.735$).¹⁶ Consequently, the difference in the belief changes is larger in the group treatments (Kolmogorov-Smirnov exact test, *GroupPC* vs. *Individual*: $p=0.090$, *GroupNoPC* vs. *Individual*: $p=0.006$). We conclude with Result 3.

Result 3: *Group communication decreases beliefs about honest behavior of other participants in a reference experiment.*

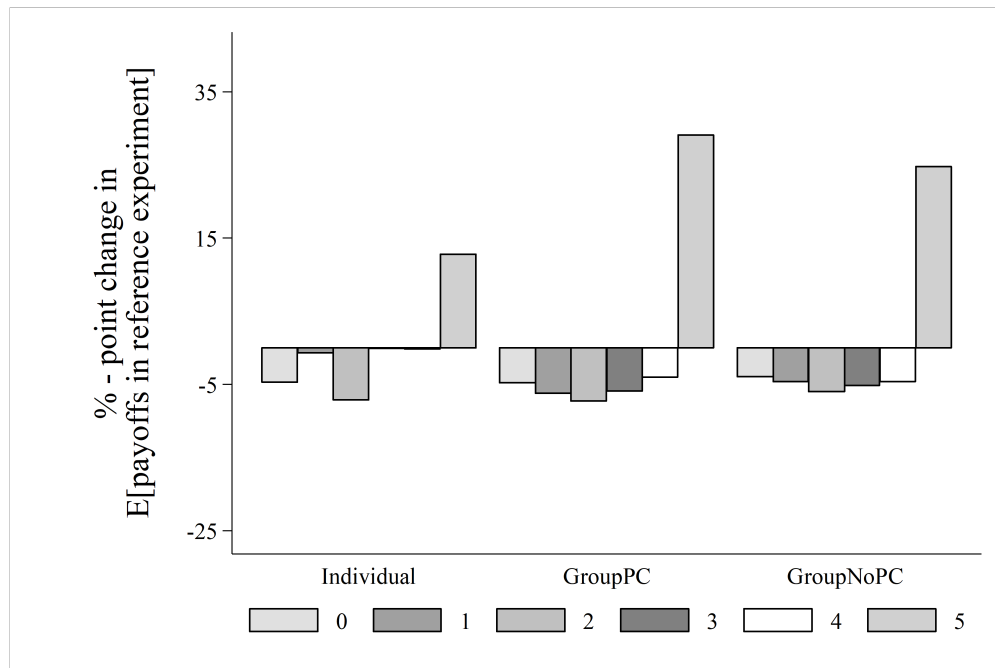


Figure 3.3: Change in Beliefs

Notes: Change in beliefs about reported payoffs in a reference experiment in terms of the share of participants that reported a payoff $\pi \in \{0;1;2;3;4;5\}$.

3.3.2 The Role of (Dis)honest Individuals in Groups

We document a strong dishonesty shift in both group treatments casting doubt on the idea that behavior in groups is a simple result of individual preference

¹⁶ We calculate the non-parametric skewness measure $S_{it} = (\mu - v)/\sigma$ for each individual's belief distribution and compare this measure for Part 1 and Part 3, where μ represents the mean, v the median and σ the standard deviation. The tests for the group treatments uses group-level data (collapsed on the median).

aggregation due to payoff commonalities. Our experimental design allows us to shed more light on how dishonesty evolves in groups, by studying how the group composition (in terms of participants' individual tendency to report dishonestly in Part 1) affects the propensity to lie in Part 2. Figure 3.4 displays the share of group members reporting dishonestly in Part 2 conditional on the number of group members who misreported in Part 1.

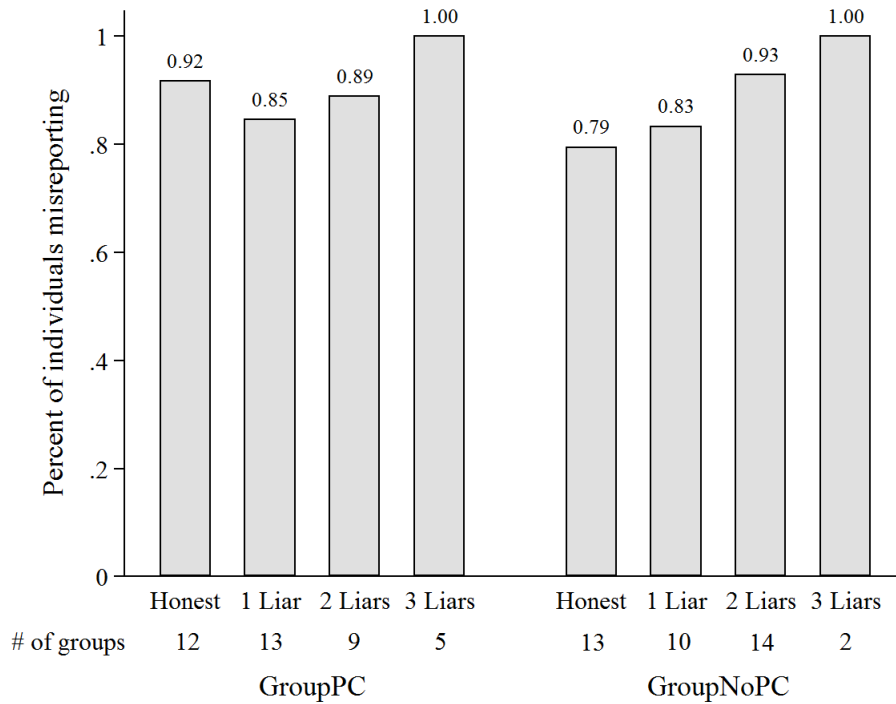


Figure 3.4: Dishonest Reporting in Groups Conditional on Previous Reporting
Notes: Reporting conditional on the number of group members who misreported in Part 1.

Surprisingly, the group composition does (if at all) play a weak role for dishonest reporting in Part 2 of the group treatments (Spearman's $\rho=0.135$, $p=0.237$; Fisher's exact test $p=0.489$). Even in groups consisting of three previously honest individuals, the vast majority decides to report dishonestly after the group interaction. Only when excluding those participants who have seen a \boxtimes in Part 1, i.e. those for whom we do not know whether they would have reported dishonestly if they had seen a different number, we find weak evidence for differences in lying levels across different group compositions (Spearman's

$\rho=0.253$, $p=0.040$, Fisher's exact test $p=0.213$).¹⁷ We summarize this finding in Result 4.

Result 4: *Group composition in terms of lying in Part 1 affects lying in groups in Part 2 only weakly. It does not matter for GroupPC and it matters weakly for GroupNoPC.*

3.3.3 *The Impact of Arguments Used in the Group Chat*

Results 1 to 3 show that communication has a detrimental effect on honest reporting behavior as well as on beliefs about others' honesty. To understand how the group chat affected reporting behavior, four research assistants (to whom the purpose of the study was unknown) independently coded each of the chats using a codebook with a predefined set of variables of interest.¹⁸ Naturally, some coders interpreted the chat protocols differently than others. To obtain reliable values for our variables of interest which, on the one hand, reflect the majority opinion of coders and are, on the other hand, not systematically biased to extreme values, we used the median value for each variable of interest if coders disagreed.¹⁹ We observe on average 17.45 (SD=8.76) messages in our 78 groups and chats last about 162 seconds (SD=85.7).²⁰ First, we examine which arguments are used and whether their use is intended to encourage honest or dishonest behavior. Then, we analyze the impact of these arguments on participants' actions.

In 51 percent of our groups (40 out of 78) arguments for dishonesty are explicitly mentioned, whereas only 19 groups make arguments for honesty (see Table 3.2). Thus, dishonest arguments are made more frequently than honest arguments (χ^2 -test, $p=0.086$). The average share of messages containing argu-

¹⁷ For a more detailed analysis, see the Appendix C.2.1.

¹⁸ The codebook including the complete list of variables can be found in Appendix C.3.

¹⁹ Note that in 83 percent of cases at least three coders agreed on a value of a variable. Our results are robust to using average ratings instead of medians.

²⁰ Chats in *GroupNoPC* are longer than in *GroupPC*, both in terms of messages and duration (Wilcoxon ranksum exact tests, $p=0.003$ and $p<0.001$, respectively). Otherwise we find only minor differences in the structure and the content of the chats (as discussed below).

ments for dishonest behavior (i.e. the number of messages using arguments for dishonest reporting in a group divided by the number of messages including arguments for honest and dishonest reporting in the group) amounts to 43.4 percent and is significantly higher than the share of honest messages (15.6 percent; within-group comparison, Wilcoxon signed-rank exact test, $p < 0.001$).

Table 3.2: Number of Groups Using Honest and Dishonest Arguments

Arguments for Honesty mentioned	Arguments for Dishonesty mentioned		Σ
	Yes	No	
Yes	13	6	19
No	27	32	59
Σ	40	38	78

First suggestions on which number to report are made by both honest and dishonest individuals (classified according to their behavior in Part 1) in equal proportion. Comparing whether groups make no argument for (dis)honesty or at least one argument we do not observe any significant differences between *GroupPC* and *GroupNoPC* concerning dishonest arguments (χ^2 -test, $p = 0.365$). More groups without payoff commonality send, however, at least one argument for honesty ($p = 0.065$). We summarize our findings in Result 5.

Result 5: *Arguments for dishonesty occur significantly more frequently than arguments for honesty, irrespective of payoff commonality.*

Let us now turn to what arguments are made in favor of honest and dishonest reporting and how these arguments relate to actual reporting behavior. Figure 3.5 illustrates the share of groups in which specific arguments were made to encourage dishonest (Panel A) and honest reporting (Panel B) for *GroupPC* and *GroupNoPC* separately. We refer to *Money* if the argument made related to the monetary consequences of reporting (e.g. "we will earn more if we choose to report a higher number than the number shown"). *Honesty* arguments directly refer to honesty as a norm or value (e.g. "there is no need to be honest"

or "it is important to be honest"). *Insecurity* refers to insecurity concerning the task (e.g. "I am uncertain about the task" or "I thought we should enter the number we want to enter"). *Rules* refer to explicit arguments that include (non-)compliance with the rules (e.g. "we should stick to the rules" or "there is no need to stick to the rules"). *Others' behavior* refers to honesty of others outside the group (e.g., people's behavior in general or other participants' behavior).

The majority of arguments refer to *Money*, *Honesty* and *Insecurity* (see Figure 3.5). In both *GroupPC* and *GroupNoPC*, participants use *Money* mainly to encourage dishonest reporting and they do so to a similar extent (Fisher's exact test, $p=0.233$). Explicitly referring to *Honesty* is the main argument used in favor of honesty, being raised significantly more often in *GroupNoPC* (Fisher's exact test, $p=0.008$).²¹ *Insecurity* is used to encourage both dishonest and honest behavior.

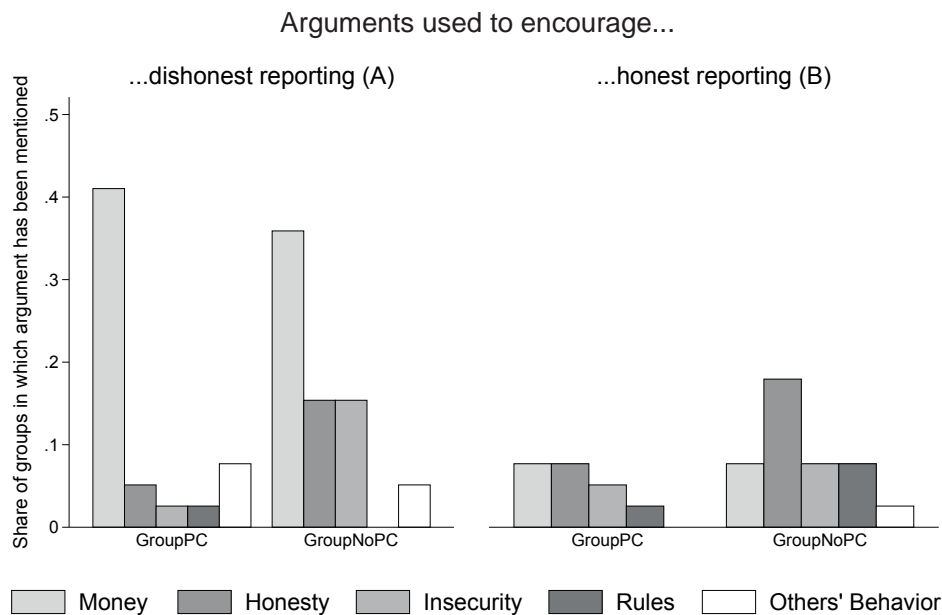


Figure 3.5: Arguments to Encourage (Dis)Honest Reporting

²¹ Notice that reporting in Part 1 is related to the use of arguments in expected ways: Honest behavior in Part 1 relates positively to the use of honest arguments in Part 2 (Spearman's $\rho=0.144$, $p=0.027$) and dishonest behavior in Part 1 relates positively to the use of dishonest arguments (Spearman's $\rho=0.112$, $p=0.088$).

As the use of arguments relating to *Insecurity* does not correlate with wrongly answered control questions in Part 2 (Spearman's ρ , referring to honesty: -0.05, $p=0.65$; referring to dishonesty: -0.06, $p=0.59$), participants may use this type of arguments in particular as a justification for their preferred behavior, i.e. as an excuse for dishonest behavior or as support for honest reporting.

We further find that the group composition affects the use of arguments. Groups composed of none or one participant who behaved dishonestly in Part 1 use a large variety of arguments (but more dishonest than honest arguments). Groups with two previously dishonest participants use fewer arguments and focus mainly on arguments relating to money. Groups with three previously dishonest people use only arguments relating to money.²²

Models (1) – (3) in Table 3.3 regress the probability of a dishonest report on arguments used for encouraging both honest and dishonest behavior. Model (1) shows that, given the high lying rates we observe, arguments that encourage honest reporting explain variation in lying best. We find that arguments in favor of honest reporting that refer to *Money*, *Honesty*, *Insecurity*, and *Rules* reduce the probability of dishonest reporting significantly. In Model (2), we introduce the treatment dummy *GroupPC*, which does not significantly influence dishonest reporting (compared to *GroupNoPC*). In Model (3) we additionally include whether or not an individual has misreported in Part 1. Misreporting in Part 1 significantly increases the probability of reporting dishonestly in Part 2. Introducing this additional control does not strongly affect the magnitude of the other coefficients much. However, the coefficient for *Honesty* (Honest Use) and *Rules* (Honest Use) becomes statistically insignificant in Model (3).²³ We summarize these findings in Result 6.

Result 6: *Arguments encouraging honesty significantly reduce lying behavior.*

²² We thank an anonymous referee for suggesting this analysis.

²³ Adding the number seen in Part 2 as an explanatory variable (proxy for the monetary gain from a lie) in Models (1) to (3) leaves the results qualitatively unchanged.

Table 3.3: Lying Behavior and Arguments Used

	Probit (ME)		
	Misreporting in Part 2		
	(1)	(2)	(3)
GroupPC		-0.00245 (0.0579)	0.00225 (0.0545)
Money (Dishonest Use)	0.0629 (0.0549)	0.0631 (0.0560)	0.0572 (0.0532)
Money (Honest Use)	-0.291*** (0.131)	-0.291*** (0.131)	-0.320*** (0.134)
Honesty (Dishonest Use)	0.0437 (0.0984)	0.0429 (0.0949)	0.0475 (0.0845)
Honesty (Honest Use)	-0.0773* (0.0382)	-0.0777* (0.0401)	-0.0526 (0.0353)
Insecurity (Dishonest Use)	0.0187 (0.0763)	0.0179 (0.0750)	0.00132 (0.0532)
Insecurity (Honest Use)	-0.375*** (0.125)	-0.376*** (0.131)	-0.343*** (0.126)
Rules (Dishonest Use)			
Rules (Honest Use)	-0.266** (0.132)	-0.268** (0.138)	-0.215 (0.133)
Others' Behavior (Dishonest Use)	-0.162 (0.151)	-0.163 (0.151)	-0.127 (0.149)
Others' Behavior (Honest Use)			
Misreporting in Part 1			0.0869** (0.0364)
Observations	234	234	234
Cluster	78	78	78
Pseudo-R ²	0.422	0.422	0.450

Notes: The dependent variable is a binary indicator for whether the individual has misreported in Part 2. Arguments relating to others being honest and arguments referring to rules to encourage dishonest reporting have been dropped due to multicollinearity. Robust standard errors clustered on group level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.3.4 *Individual Characteristics and Lying*

In addition to our treatment effects, individual characteristics may play an important role for the decision to report dishonestly. As a robustness test, we report results from a series of probit regressions on dishonest reporting including additional controls for individual characteristics (e.g. Machiavelli Scores, Big 5, Risk attitudes, Religiousness, Political attitudes, Gender, Age) in Tables C.1-C.3 of Appendix C.1. Our treatment effects remain robust and we find no significant effect of any control variable except for gender.²⁴ The gender effect that we observe is in line with most previous studies: Females tend to be less likely to lie as individuals (see, e.g., the survey by Rosenbaum et al., 2014). Regression model (1) in Table 3.4 shows that females tend to misreport less in Part 2. The gender effect persists when controlling for our treatments in Model (2). However, the treatment effect on females is larger than on males (see Model (3) and (4), showing separate regressions for females and males). The coefficient for males is still positive but not statistically significant at conventional levels ($p=0.140$ for *GroupPC* and $p=0.314$ for *GroupNoPC*).

Table 3.4: Lying and Gender

	Probit (ME) - Misreporting in Part 2			
	All (1)	All (2)	Females (3)	Males (4)
GroupPC		0.205*** (0.0585)	0.266*** (0.0781)	0.116 (0.0744)
GroupNoPC		0.169*** (0.0576)	0.231*** (0.0770)	0.0763 (0.0745)
Female	-0.0878** (0.0381)	-0.0824** (0.0366)		
Observations	273	273	167	106
Cluster	117	117	100	76
Pseudo-R ²	0.017	0.079	0.0829	0.031

Notes: Robust standard errors clustered on group level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

²⁴ Note that other controls do not systematically affect lying behavior in Part 1.

3.4 DISCUSSION AND ALTERNATIVE EXPLANATIONS

Our results document that communication in groups can have a detrimental effect on honest reporting behavior. One rationale for this result is that communicating in a group provides possibilities for individuals to deliberate and formulate justifications for dishonest behavior (see also Gino and Ariely, 2012; Mazar et al., 2008; Shalvi et al., 2012). Another rationale is that exchanging justifications enables group members to coordinate and establish the validity of a norm regarding honesty and to what extent it is shared among others. In order to address these two aspects, we conduct an additional control treatment which allows for deliberation but not for communication; *IndividualDeliberation* ($n=39$). Part 1 and 3 of *IndividualDeliberation* are identical to the *Individual* treatment described in Section 3.2. In Part 2, *IndividualDeliberation* offers participants the possibility to deliberate before entering the number seen in Part 2: After observing the die roll, participants have five minutes to write down their thoughts and only afterwards enter their number. As in the group treatments (where participants could leave the chat), participants in *IndividualDeliberation* were allowed to leave the entry screen before its automated ending. Thus *IndividualDeliberation* allows participants to deliberate and formulate justifications for dishonest behavior but excludes establishing the validity arguments and sharing of the norm among group members.

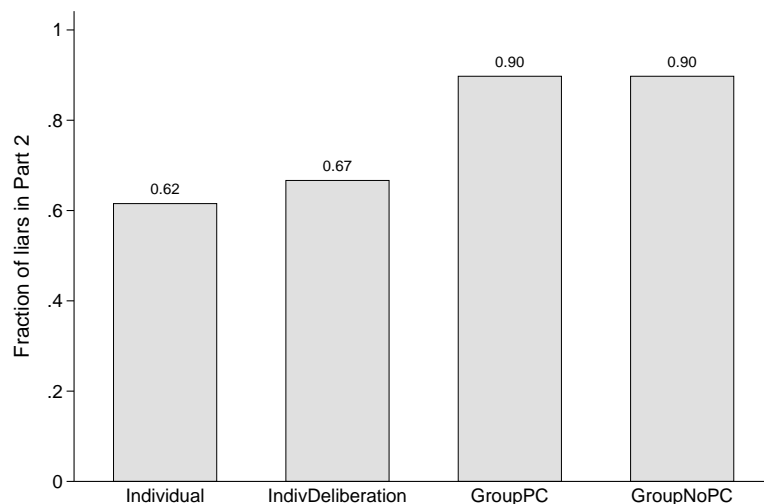


Figure 3.6: Lying in Part 2, by Treatment

Figure 3.6 illustrates the fraction of liars in Part 2 which is significantly smaller in both individual treatments as compared to the group treatments (Fisher's exact test, $p=0.026$ for both *IndividualDeliberation* vs. *GroupPC* and *IndividualDeliberation* vs. *GroupNoPC*; $p=0.007$ for both *Individual* vs. *GroupPC* and *Individual* vs. *GroupNoPC*; $p=0.814$ for *Individual* vs. *IndividualDeliberation*). That is, lying with deliberation is not significantly different from lying in *Individual* but does significantly differ from lying behavior in groups.²⁵ Also (and again in contrast to group communication), deliberation does not significantly change the skewness of beliefs about other participants' honesty in the reference experiment (Wilcoxon signed-rank exact test, $p=0.164$). One may expect individuals in *IndividualDeliberation* deliberate less than individuals in the group treatments. However, the number of individuals in *IndividualDeliberation* coming up with at least one dishonest argument is not statistically different from the number of groups who do so in *GroupNoPC* (χ^2 -test, $p=0.352$) and it is even larger than the number of groups in *GroupPC* (χ^2 -test, $p=0.068$).²⁶ Hence, we are confident that deliberation alone does not explain the increase of lying behavior in groups. In combination with Result 1, we conclude that it is the exchange of arguments and moral views within the group that shift group members' expectations and behavior.²⁷

Finally, let us briefly discuss other potential explanations for the observed *dishonesty shift*. First, one may suspect that experimenter demand effects could play a role in our setting. For instance, groups might be more susceptible to experimenter demand effects than individuals and thereby lie more (or less). Identifying such demand effects is usually difficult and there is no existing evidence for a difference in susceptibility between individuals and groups. As we are able to observe communication of our participants, we analyzed group chats with regard to any signs of experimenter demand effects. We find that only four out of 78 groups (five percent) mention that the "honesty – money trade-off" might be part of the research question and one group considered

²⁵ Note that levels of lying in Part 1 and Part 3 in *IndividualDeliberation* (48.7 and 76.9 percent) do not significantly differ from levels of lying in *Individual* (30.8 and 71.8 percent, Fisher's exact test, Part 1: $p=0.165$ and Part 3: $p=0.796$).

²⁶ This also holds true for the absolute number of dishonest arguments. Appendix C.2.3 provides further results from the content analysis of *IndividualDeliberation*.

²⁷ This conclusion is further supported by decision times which we analyze in Appendix C.2.2.

that the experimenter may be interested in the effect of the group chat on the decision. Also in *IndividualDeliberation*, only five percent (two participants) refer to the research question. Hence, we find little (direct) evidence for an experimenter demand effect that interacts with our group treatments. Such a demand effect is thus unlikely to explain the observed *dishonesty shift*.

Second, we observe increasing lying rates in the *Individual treatment* over the three parts. One may suspect that subjects learn over time that any consequences of dishonest behavior are absent. If so, there is the possibility that communication in groups facilitates this "learning" and thereby increases lying rates faster. However, three empirical facts cast doubt on this interpretation. First, 74 percent of our groups do not address the potential consequences (positive or negative) of dishonest behavior and the discussion of potential consequences does not significantly correlate with the group decision to lie (Spearman's ρ : positive consequences -0.069 , $p=0.550$; negative consequences -0.129 , $p=0.260$). Second, we do not observe a statistically significant correlation between incorrectly answered control question and lying behavior. Third, faster learning in the group treatments in Part 2 is unable to explain the significant increase in beliefs about others' dishonesty in a reference experiment that we observe only for participants who communicated in a group. While participants in *Individual* have also learned about the consequences of lying, they do not change their beliefs about others' lying behavior much. Thus, we are confident that group communication indeed changes how people rationalize morally questionable behavior, i.e. it is learning about the norm that mainly drives our results, not about the consequences of lying.

Third, "hiding behind the group" as an individual (accountability argument) is a potential explanation for the dishonesty shift. However, this argument is hard to sustain in an environment with perfect observability by the experimenter, which we implemented on purpose to address this aspect. Further, if hiding behind the group was a main driver, lying in the group treatments should decrease significantly in Part 3 (which we do not observe).

3.5 CONCLUSION

Our results substantially improve the understanding of (dis)honest behavior in groups. First, complementing recent evidence on the role of collaboration opportunities for dishonesty when communication is absent (see Weisel and Shalvi, 2015), we observe that groups lie significantly more than individuals when group members face payoff commonality and have to coordinate on an action. Second, we show that the payoff commonality is not a necessary condition for groups to behave more dishonestly than individuals. If individuals communicate within a group but do not have to coordinate to receive a payoff, their behavior is very similar to the behavior of groups facing payoff commonality. Hence, we provide evidence that communication itself can have a detrimental effect on ethical behavior of small groups. We term this increased inclination to lie *dishonesty shift*. Evidence from our additional control treatment (*IndividualDeliberation*) shows that the *dishonesty shift* is not due to the fact that communicating in a group provides possibilities for individuals to deliberate or simply justify their actions to themselves. Instead, it is the exchange of justifications that enables group members to coordinate on dishonest actions and change their beliefs about moral behavior. The content analysis of our chat protocols backs up this interpretation, as it documents that group members indeed use the chat primarily to formulate and exchange arguments in favor of dishonest behavior. Additionally, we find that communication shifts group members' beliefs about the prevailing honesty norm in a reference experiment, suggesting that group members indeed established a new norm regarding (dis)honesty.

Our findings also provide important insights for the design of institutions. We document a new argument for the prevalence of dishonesty in groups: Groups tend to lie more because they are able to communicate and thereby to rationalize morally wrong behavior in a different way than individuals. In turn, organizations fostering interaction and communication within groups are likely to provide room for group members not only to coordinate their actions, but also to adjust their beliefs and thus the norms they follow. Conse-

quently, members of a group or unit within an organization may hold different beliefs about ethically acceptable behavior than individuals outside the group. While communication is obviously important and necessary to provide efficient coordination which can be beneficial in many organizational contexts, it may also have detrimental effects on the evolution of norm perceptions within groups. Therefore, organizational structures that foster communication might require to be paired with strong codes of conduct or exogenous monitoring (and punishment) to avoid the erosion of honesty norms. While reminders appealing to morality or religious beliefs have been shown to be effective in the short run (Mazar et al., 2008), long run impacts of such interventions are less well understood. More research is also needed to investigate how scrutiny (see e.g., Baeker and Mechtel, 2015; Houser et al., 2016; Van de Ven and Villeval, 2015) and efficient reporting mechanisms can be designed (Friesen and Gangadharan, 2013). Our results hint at the fact that such designs need to take into account how dishonest behavior evolves in groups and raises many interesting questions for future research, e.g. on the role of hierarchies in organizations, heterogeneity in the benefits from lying, monitoring and punishment as well as self-selection of honest and dishonest individuals in leading roles.

Over and above the analysis of potential organizational design features that are able to countervail the dishonesty shift, our results highlight a more general fact: Communication in groups and organizations changes group members' beliefs about moral behavior of others and increases coordination of group members' behavior. While we observe a strong shift towards violating the honesty norm in groups, it remains an open question whether group members may coordinate on moral actions, depending on the original strength of the norm.

Part III

APPENDIX

APPENDIX TO CHAPTER 1

A.1 ADDITIONAL TABLES AND FIGURES

Table A.1: Balance

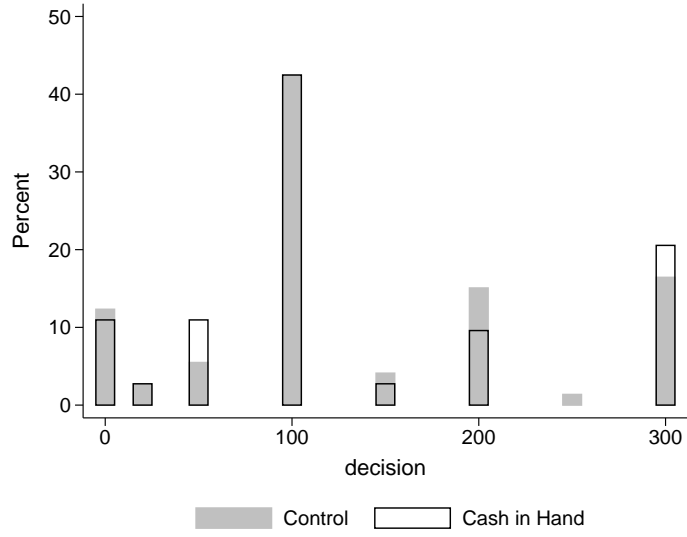
	CA ₃₀₀	CiH ₃₀₀	CA ₅₀₀	CiH ₅₀₀	R ²	F-test
Self-Reports						
Age	43.19	0.986	0.535	-1.532	0.001	0.945
Education (yrs)	8.712	-0.164	-0.102	0.450	0.003	0.718
Financial literacy (0-1)	0.514	0.027	0.077*	-0.073	0.018	0.229
SR attention to finances (y/n)	0.274	-0.0685	-0.0792	0.146	0.008	0.471
MR attention to finances (y/n)	0.301	-0.0822	-0.0936	0.121	0.007	0.337
HH size	5.288	0.466	0.024	-0.687	0.011	0.529
HH earners	2.151	0.178	0.018	-0.165	0.005	0.569
Owns business (y/n)	0.658	0.082	0.109	-0.095	0.009	0.617
Cash income (y/n)	0.918	0.014	0.017	0.0123	0.004	0.577
Currently saving (y/n)	0.753	0.082	0.078	-0.108	0.007	0.473
Savings at home (₱)	1,034	441.8	318.7	-662.1	0.005	0.606
Savings in account (₱)	662.3	-16.17	364.5	-267.1	0.010	0.630
Travel cost to center (₱)	0.685	-0.397	0.419	0.280	0.007	0.270
Travel time to center (min)	5.658	-0.164	0.680	0.489	0.006	0.788
Banks untrustworthy (0-1)	0.336	0.00685	-0.0694	0.00614	0.008	0.465
Savings in ASHI are safe (0-1)	0.932	0.0274	-0.0386	0.0408	0.018	0.297
Decision making power (0-2)	1.023	0.104	0.001	-0.169*	0.013	0.155
Would like a private account (0-1)	0.616	0.0548	0.0394	-0.00285	0.006	0.611

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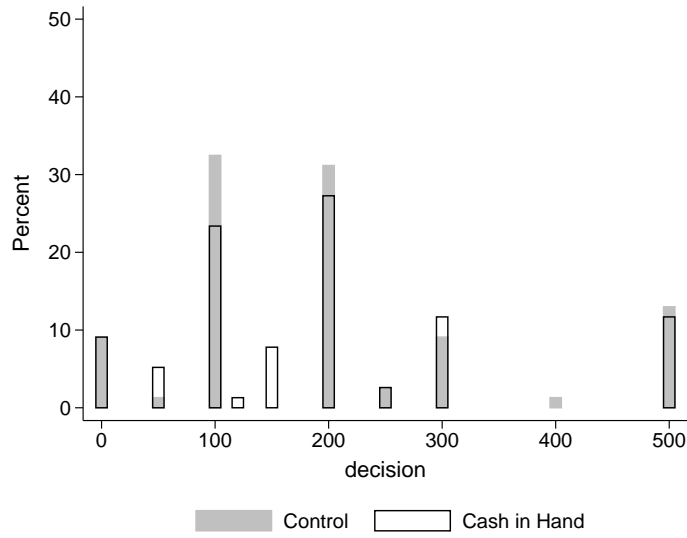
Balance continued

	CA ₃₀₀	CIH ₃₀₀	CA ₅₀₀	CIH ₅₀₀	R ²	F-test
Self-Reports (0-1)						
Saving less due to...						
...claims from husband	0.538	-0.00905	-0.00861	-0.0246	0.002	0.939
...claims from family & friends	0.168	0.0137	0.0108	-0.0689	0.006	0.589
...claims from clients	0.432	0.0411	0.0490	-0.0508	0.002	0.894
...overoptimistic wrt saving	0.260	0.0685	0.00271	-0.0912	0.007	0.614
Administrative Data						
Savings balance (₪)	771.3	50.65	-23.87	-6.604	0.000	0.975
Loan amount (₪)	20,903	916.7	-5,396**	632.8	0.045	0.105
PPI score (0-100)	46.79	2.667	0.797	-4.115	0.005	0.916
Main income: Enterprise (y/n)	0.767	0.0137	0.0548	-0.123	0.009	0.656
Electricity (y/n)	0.556	0.0278	-0.0898	0.0558	0.008	0.992
Water (y/n)	0.264	4.02e-16	-0.0721	0.0899	0.006	0.426
Landline (y/n)	0.0139	0.0139	-0.0139	-0.0139	0.013	.
House size (0-2)	0.528	-0.0417	0.143	-0.0521	0.016	0.344
House strength (0-2)	0.750	-0.0417	-0.0240	0.0339	0.001	0.913
Membership (months)	60.07	0.153	-17.62	-6.056	0.044	0.153

Notes: The upper panel presents results for variables elicited during the experiment and the lower panel variables from pre-experimental administrative data. Higher values indicate larger agreement/better outcomes. Mean of the CA group and coefficients from OLS regressions with treatment dummies as independent variables and clustered SE (not shown) at the center level. The last column shows p-values of the F-test of joint significance of the treatment dummies. *** p<0.01, ** p<0.05, * p<0.1.



(a) Endowment P 300



(b) Endowment P 500

Figure A.1: Distributions of Amount Saved

Table A.2: Treatment Effect on Amount Saved

	(1)	(2)	(3)	(4)
	Amount Saved	Amount Saved	Amount Saved	Amount Saved
CIH	-3.533 (13.25)	-3.522 (13.85)	-1.370 (17.26)	-7.961 (18.07)
	[-28.49 - 22.11]	[-28.87 - 22.69]	[-34.30 - 31.42]	[-41.00 - 25.59]
Endowment 500			64.56** (24.62)	60.44** (22.98)
			[17.28 - 111.1]	[17.19 - 103.3]
CIH x 500			-4.215 (27.09)	8.068 (27.24)
			[-54.18 - 46.89]	[-41.51 - 59.23]
Constant	167.9*** (13.51)	58.24 (71.96)	134.8*** (14.71)	39.56 (71.34)
	[142.6 - 194.1]	[-76.78 - 205.3]	[106.6 - 163.0]	[-106.3 - 182.2]
p: CIH+CIH*500 = 0				
			0.856	0.998
Observations	300	300	300	300
Adj. R ²	-0.003	0.035	0.053	0.097
Clustered SEs	yes	yes	yes	yes
Controls		yes		yes

Notes: OLS estimates, SE in parenthesis, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets. Controls: age, education, financial literacy, hh size, business owner, money left, decision making power, distance to center, time to center, interviewer FE. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Effect of Money Today on Share and Amount Saved

	(1)	(2)	(3)	(4)
	Share Saved	Share Saved	Amount Saved	Amount Saved
Money Today (ln)	0.0164 (0.0179)	0.0150 (0.0204)	9.120 (8.614)	10.06 (9.857)
	[-0.0174 - 0.0503]	[-0.0235 - 0.0528]	[-6.833 - 25.20]	[-8.311 - 28.25]
Constant	0.326*** (0.113)	-0.00916 (0.179)	112.8** (52.12)	-15.31 (94.02)
	[0.109 - 0.538]	[-0.334 - 0.386]	[15.25 - 210.9]	[-166.2 - 175.8]
Observations	266	266	266	266
Adj. R ²	0.003	0.088	0.006	0.091
Clustered SEs	yes	yes	yes	yes
Controls		yes		yes

Notes: OLS estimates, robust SE clustered on session level in parenthesis, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets. Controls: age, education, financial literacy, household size, business owner, money left, decision making power, time to center, travel cost to center, interviewer FE. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Present Bias, CiH and Savings

	(1) Amount Saved	(2) Share Saved	(3) Present Bias
CiH*Present Bias	64.50* (36.84) [-5.896 - 133.2]	0.150* (0.0846) [-0.0066 - 0.307]	
High Deposit		0.103*** (0.0250) [0.055 - 0.151]	
High Fin. Lit.	38.93*** (11.91) [16.62 - 61.30]	0.0620** (0.0297) [.0057 - 0.119]	
Interviewer 2	41.22** (16.71) [9.475 - 72.16]	0.122*** (0.0431) [0.0412 - 0.203]	
Large HH	41.98** (18.84) [6.730 - 77.89]	0.108*** (0.0352) [0.0423 - 0.175]	
CiH	-13.32 (15.25) [-41.71 - 16.16]	-0.0338 (0.0368) [-0.103 - 0.0366]	0.0685 (0.0512) [-0.0246 - 0.162]
Present Bias	-16.72 (21.96) [-57.67 - 25.36]	-0.0317 (0.0583) [-0.141 - 0.0758]	
Endowment 500			0.0340 (0.0507) [-0.0609 - 0.129]
CiH x 500			0.00943 (0.0870) [-0.158 - 0.170]
Constant	131.0*** (14.58) [103.9 - 158.7]	0.284*** (0.0356) [0.218 - 0.350]	0.0959*** (0.0336) [0.0340 - 0.158]
Observations	300	300	300
Adj. R ²	0.088	0.119	0.014
Clustered SEs	yes	yes	yes

Notes: OLS estimates, robust SE clustered on center level in parentheses, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets; *** p<0.01, ** p<0.05, * p<0.1.

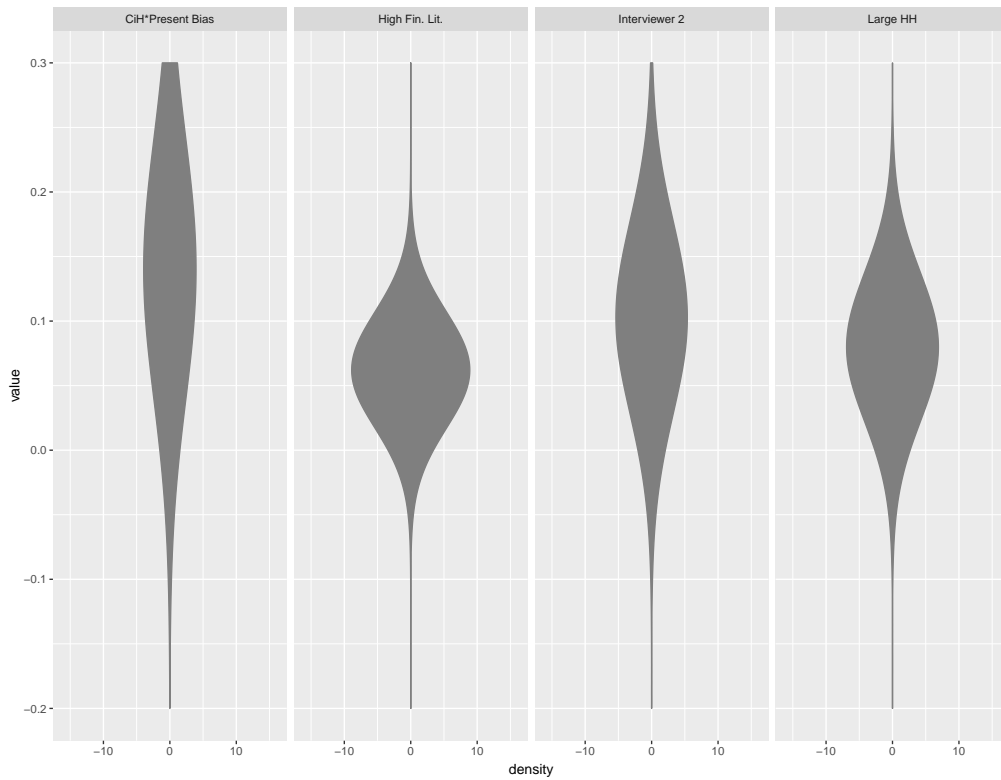


Figure A.2: Density of Selected Effects on Share Saved

Notes: LASSOplus using linear estimation and allowing for interactions with the CiH dummy. Binary indicators (for above median value where applicable) included: Age, education, financial literacy, household size, business owner, money left, decision making power, time to center, travel cost to center, narrow bracketing in both questions, narrow bracketing in one question, risk aversion, loss aversion, present bias, future bias, high savings balance, high deposit, regular deposit, equally-sized deposits as well as type dummies and interviewer dummies.

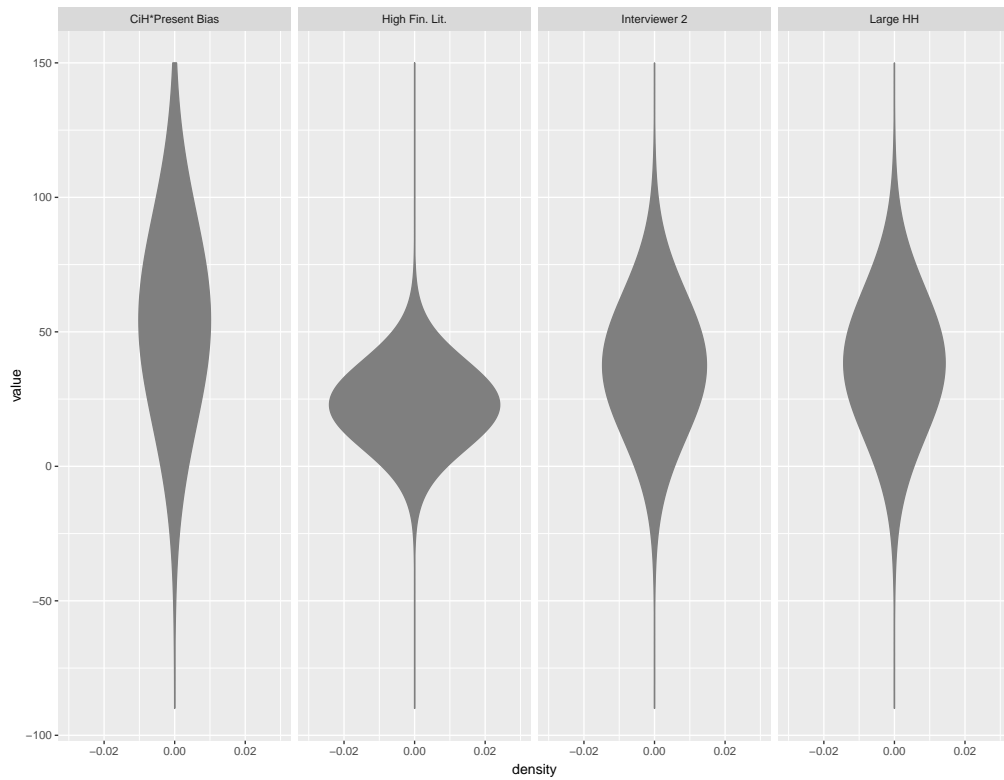


Figure A.3: Density of Selected Effects on Amount Saved

Notes: LASSOplus using linear estimation and allowing for interactions with the CiH dummy. Binary indicators (for above median value where applicable) included: Age, education, financial literacy, household size, business owner, money left, decision making power, time to center, travel cost to center, narrow bracketing in both questions, narrow bracketing in one question, risk aversion, loss aversion, present bias, future bias, high savings balance, high deposit, regular deposit, equally-sized deposit as well as saver type dummies and interviewer dummies.

Table A.5: Representativeness

	Sample Mean	Non-Participant in Sample Center	Non-Participant in Non-Sample Center	R ²	F-test
Savings balance (₱)	782.7	-12.85	155.3	0.002	0.220
Loan amount (₱)	21,551	-3,098***	-1,905	0.002	0.001
PPI score (0-100)	43.53	-0.0249	-2.195	0.002	0.505
Main income: Enterprise	0.831	0.0102	-0.0324	0.002	0.272
Electricity (y/n)	0.463	-0.0135	0.0828	0.006	0.159
Water (y/n)	0.159	0.0537	0.0567	0.001	0.133
Landline (y/n)	0.00676	-0.000652	0.00449	0.000	0.343
House size (0-2)	0.527	0.0468	0.0585	0.001	0.411
House strength (0-2)	0.591	0.0523	0.0907*	0.002	0.131
Membership (months)	49.03	-7.193**	3.567	0.005	0.018
Age	43.94	-1.211	0.570	0.003	0.011
N	300	819	3735		

Notes: Mean of the sample and coefficients from OLS regressions (N= 4854) with dummies for non-participants and non-sample centers as independent variables and clustered SE (not shown) at the center level. The last column shows p-values of the F-test of joint significance of the non-participant dummies. *** p<0.01, ** p<0.05, * p<0.1.

A.2 ADDITIONAL MANIPULATION CHECKS

The manipulation check presented in Chapter 1 is further supported by Table A.6 that shows the same effects plotted in Figure 1.1 in a regression analysis of the mean effects.¹

The coefficient on the treatment dummy *CiH* in Column (1) shows that treated individuals report about 28 percent higher *money today* than in *CA*. This effect is robust to the inclusion of a set control variables (Column (2)). Column (3) shows that in terms of assets, both groups are the same. Since this feeling of endowment might also be visible in other income questions, Columns (2) and (3) of Table A.7 provide suggestive evidence that *CiH* influences reporting behavior more generally: For cash income, a variable that pools all income that respondents report to receive in cash, the *CiH* dummy indicates somewhat higher reports ($p=0.13$), whereas in terms of non-cash income, participants appear to be the same ($p=0.81$).² While this is only suggestive, the tendency is also reflected in the differences in coefficient size and R^2 .

¹ For regressions, I use log of money today as the variable is very skewed.

² Given the relevance of cash, I ask for each income source how this income is received (in cash, via check/ deposit or wire transfer). All these questions are asked in Part 1. For more details, see also Appendix A.7.

Table A.6: Parametric Manipulation Check

	(1)	(2)	(3)	(4)
	Money Today (ln)	Money Today (ln)	Assets	Assets
CiH	0.279** (0.128) [0.0413 - 0.526]	0.261** (0.0999) [0.0772 - 0.447]	0.00769 (0.0143) [-0.0190 - .0346]	0.00504 (0.0128) [-0.0187 - 0.0285]
Constant	5.890*** (0.105) [5.690 - 6.089]	4.759*** (0.440) [3.947 - 5.569]	0.482*** (0.0160) [0.451 - 0.512]	0.206** (0.0890) [0.0476 - 0.369]
Observations	266	266	300	300
Adj. R ²	0.014	0.238	-0.003	0.088
Clustered SEs	yes	yes	yes	yes
Controls		yes	yes	yes

Notes: "Money Today" is the answer to the question: "How much money do you think you will take home at the end of the day?" 34 participants (14 in *CiH* and 20 in *CA*) stated that they do not know. OLS estimates, robust SE clustered on center level in parentheses, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets. Controls: age, education, financial literacy, household size, business owner, money left, decision making power, time to center, travel cost to center, interviewer FE. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Manipulation Check: Other Income Measures

	(1)	(2)	(3)
	Cash Income (ln)	Non-Cash Income (ln)	PPI Score
CiH	0.314 (0.201) [-0.0529-0.693]	0.0853 (0.351) [-0.5778-0.740]	0.367 (1.861) [-3.141-3.920]
Constant	7.521*** (1.506) [5.694-10.90]	0.251 (1.925) [-4.521-2.346]	21.19** (9.623) [5.675-39.79]
Observations	300	300	296
Adj. R ²	0.205	0.111	0.121
Clustered SEs	yes	yes	yes
Controls	yes	yes	yes

Notes: "Cash Income" ("Non-Cash Income") comprises all income that the respondent reports to receive in cash (other means of payment). OLS estimates, robust SE clustered on center level in parentheses, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets. Controls: age, education, financial literacy, household size, business owner, money left, decision making power, time to center, travel cost to center, interviewer FE. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.3 PREFERENCE ELICITATION

To keep the decision as simple as possible, all preferences are elicited via multiple price lists (MPL).

Risk Aversion

The MPL for risk preferences asks participants to make nine choices between a riskier and a safer option, similar to Holt and Laury (2002). To avoid safety bias, both options are lotteries and for simplicity, the payoffs of the two lotteries remain constant throughout all choices, such that lottery A is always the less risky one. Only the probability of winning the larger amount increases in both lotteries from 10 percent to 90 percent (see Table A.8). Each screen only shows one lottery pair that is illustrated with two urns containing 10 differently colored balls (see Figure A.4 for an example).³

Table A.8: Risk Preference Choices

Screen	Lottery A		Lottery B		E[A]-E[B]
1	10% ₱ 150	90% ₱ 100	10% ₱ 250	90% ₱ 10	71
2	20% ₱ 150	80% ₱ 100	20% ₱ 250	80% ₱ 10	52
3	30% ₱ 150	70% ₱ 100	30% ₱ 250	70% ₱ 10	33
4	40% ₱ 150	60% ₱ 100	40% ₱ 250	60% ₱ 10	14
5	50% ₱ 150	50% ₱ 100	50% ₱ 250	50% ₱ 10	-5
6	60% ₱ 150	40% ₱ 100	60% ₱ 250	40% ₱ 10	-24
7	70% ₱ 150	30% ₱ 100	70% ₱ 250	30% ₱ 10	-43
8	80% ₱ 150	20% ₱ 100	80% ₱ 250	20% ₱ 10	-62
9	90% ₱ 150	10% ₱ 100	90% ₱ 250	10% ₱ 10	-81

³ Only displaying one pair at a time reduces the concerns of respondents choosing the midpoint in MPLs, see also (Harrison and Rutström, 2008).

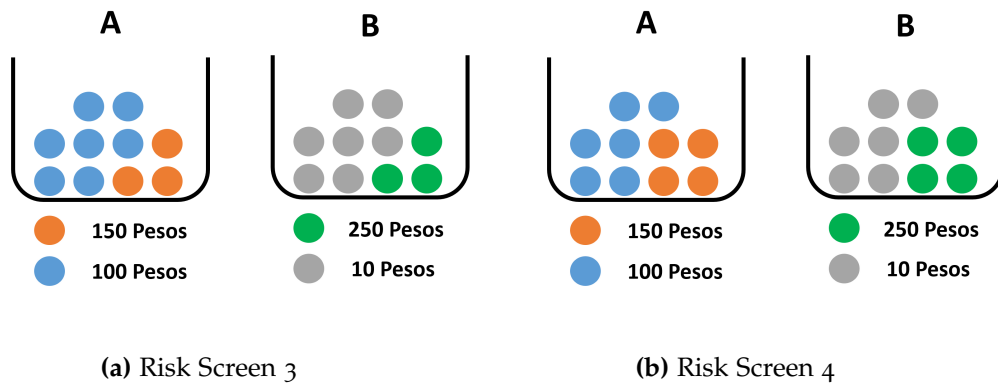


Figure A.4: Example Risk Elicitation

Loss Aversion

The MPL eliciting loss aversion is presented in a similar manner as the risk elicitation task (see Figure A.5 for an example), similar to e.g. Liu et al. (2014). Both lotteries consist of possible outcomes in the gain and the loss domain, with lottery A always entailing a lower loss (₱35) than lottery B (₱40-65; see Table A.9). In all choices, both lotteries keep the winning probability constant at 50 percent and vary one of the possible outcomes.

Table A.9: Loss Aversion Choices

Screen	Lottery A		Lottery B		E[A]-E[B]
1	50% ₱60	50% ₱-35	50% ₱75	50% ₱-65	7.5
2	50% ₱55	50% ₱-35	50% ₱75	50% ₱-65	5
3	50% ₱50	50% ₱-35	50% ₱75	50% ₱-65	2.5
4	50% ₱45	50% ₱-35	50% ₱75	50% ₱-65	0
5	50% ₱40	50% ₱-35	50% ₱75	50% ₱-50	-10
6	50% ₱40	50% ₱-35	50% ₱75	50% ₱-45	-12.5
7	50% ₱35	50% ₱-35	50% ₱75	50% ₱-40	-17.5

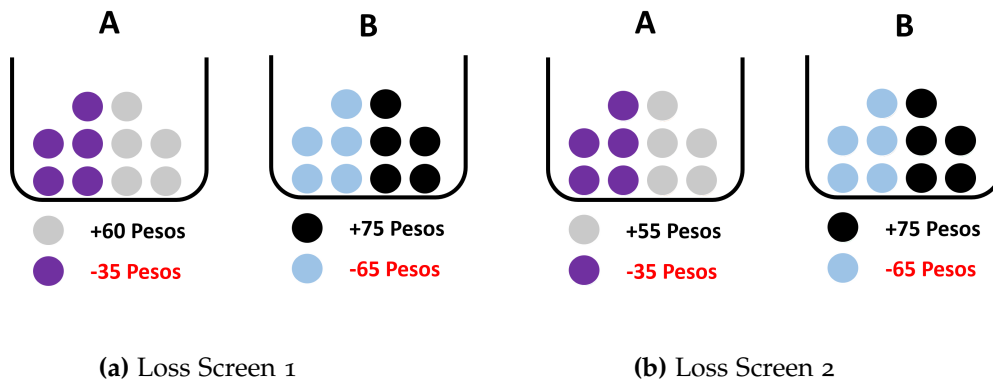


Figure A.5: Example Loss Elicitation

Time Preferences

Time preferences are elicited with MPLs presenting choices between a (smaller) sooner and a (larger) later reward for three time horizons: Today vs. 2 weeks, 2 weeks vs. 4 weeks and today vs. 4 weeks.⁴ The sooner amount is always ₱ 50, whereas the later amount increases from ₱ 40 to ₱ 200 as shown in Table A.10. To avoid differences in transaction costs for today's vs. later options, all earnings from the time preference elicitation were transferred as mobile phone credit. Trust in payments was established by giving all participants a certificate that listed their earnings from the time preference task, the date of the transfer as the phone number of the head RA. Payments for the same day were only made once the team had left the center to keep trust constant across immediate and later choices.

⁴ This time span has been chosen to increase trust that the payment will be made as the research team will come back to the center four weeks after the session. The span also lies within the range of other studies: for instance, Tanaka and Camerer (2006) vary the time span from three days to three months and in Chapter 2, we use one month and two months.

Table A.10: Time Preferences Choices

Choice	Sooner Payment	Later Payment
1	₱ 50	₱ 40
2	₱ 50	₱ 50
3	₱ 50	₱ 60
4	₱ 50	₱ 75
5	₱ 50	₱ 100
6	₱ 50	₱ 125
7	₱ 50	₱ 150
8	₱ 50	₱ 200

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. For the first block containing lottery questions (either risk or loss), the lottery urns were explained in detail and a test question was asked to ensure understanding. In total, each participant made 9 risk choices, 7 loss choices and 24 time preference choices. With a $1/6$ chance, participants would receive the payouts from one randomly drawn experimental decision. At the very end of the interview, participants rolled a die to determine whether they would be paid. If eligible, they drew a card that indicated the choice number for which they would be compensated. Lotteries were implemented with colored chips and paid out in cash.⁵

Attention and Bracketing

In addition to these incentivized preference measures, I ask hypothetical questions as in Stango et al. (2017) in Part 3 of the interview to address attention and bracketing. Attention to finances is divided into one short and one medium run question, whereas two bracketing questions ask participants to make two lottery choices each (A vs. B and C vs. D) after jointly examining all four payoffs. The two lotteries that should be considered jointly are displayed

⁵ There was no extra endowment for the preference elicitation part. The few participants who lost money needed to pay this from their initial endowment (₱300 or ₱500).

on the same screen using the same and thus familiar representation as in the risk and loss preference elicitation (see Figure A.6 for an example). The two bracketing question screens were separated by questions regarding the savings stock and savings goals (see also Appendix A.9.2).

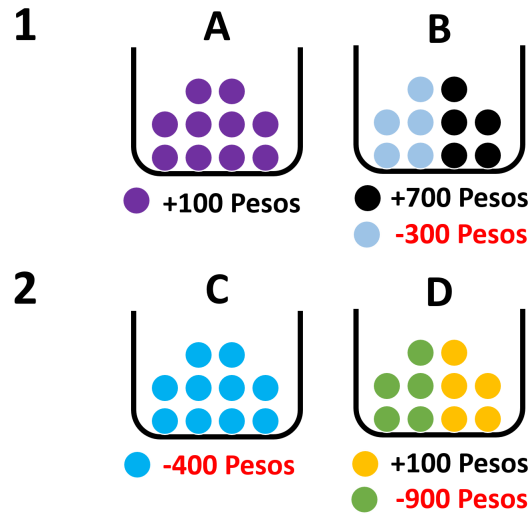


Figure A.6: Example Bracketing Question

A.4 COMPARISON OF EFFECT SIZES

To put the magnitude of my minimal detectable effect (MDE) sizes into perspective, I compare them to related studies. Since stake sizes and decision differ across studies, I calculate Cohen's d

$$d = \frac{m_1 - m_2}{\sigma} \quad (1)$$

where $m_{1(2)}$ is the mean of the treatment (control) group and σ is the pooled standard deviation,

$$\sigma = \sqrt{(\sigma_1^2 + \sigma_2^2)/2}. \quad (2)$$

Most studies in the endowment effect literature focus on the ratio m_1/m_2 or WTA/WTP, but for the comparison with my experiment, the difference between the two means provides a better benchmark.⁶ Taking the MDE of 10 percentage points in mean shares saved into account, I set the effect size in the treatment group to 0.32 and assume that the SD would be the same as the observed one. In Table A.11, I include all endowment effect studies (excluding surveys) cited in this paper that provide the necessary information to calculate Cohen's d .

⁶ Comparing the savings decision in absolute terms naturally comes with a larger SD than the other studies. Scaling the ratio of control and treatment with the large SD would thus result in very small MDEs by construction.

Table A.11: Comparison of Effect Sizes in Endowment Effect Experiments

	Comparison	Control (WTP)			Treatment (WTA)			Cohen's d
		Mean	SD	N	Mean	SD	N	
Svirsky (2014)	Money holding in baseline and money	6.56	2.17	21	5.43	2.53	18	-2.10
	Money holding in money and chocolate	4.55	3.07	40	5.43	2.53	18	1.62
Bateman et al. (2005)	WTA/WTP chocolate with money	4.66	2.82	40	9.95	4.58	40	8.80**
	WTP and equivalent gain, chocolate with money	4.66	2.82	40	8.17	5.05	40	5.43**
Morewedge et al. (2009)	Owner-buyers vs. nonowner-pair buyers	2.22	1.7	22	4.52	2.8	22	4.66**
Plott and Zeiler (2005)	Replication of Kahneman et al. (1990)	1.74	1.46	29	4.72	2.17	29	8.68***
	Pooled data from WTA/WTP mugs	6.62	4.2	36	5.56	3.58	38	-1.65
Isoni et al. (2011)	Replication Plott and Zeiler (2005) (Panel B)	3.70	1.53	33	2.75	1.76	33	-1.11
	WTA/WTP for large stakes lottery (Panel C)	4.86	1.59	36	4.81	1.48	36	3.71***
Bushong et al. (2010)	Bid in picture vs. real	0.71	0.53	17	1.13	0.61	20	3.17***
Strahilevitz and Loewenstein (1998)	Those with always mug to those who receive it later	4.32	0.41	37	5.26	0.39	48	15.14*
	Those with always mug to those who had it	3.36	0.27	34	5.26	0.39	48	36.86
This study	Share saved in <i>CiH</i> vs. <i>CA</i>	0.42	0.30	150	0.32	0.31	150	4.02

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ denote significance of the treatment difference as tested in the paper.

A.5 CLASSIFICATION OF SAVER TYPES

Types of savers are classified along three dimensions that reflect different aspects of savings behavior: First, making regular or irregular deposits, depositing large or small amounts and depositing equally-sized or variable amounts. Data come from all centers of the three study branches and comprises weekly deposits (and withdrawals) of savings, starting from July 2016 up to and including the week in which the first announcement letters were sent (the last day included is February 17, 2017).⁷ This leaves me with 4749 clients (300 sample clients, 676 clients from the same centers, but not participating and 3773 clients from different centers).⁸

Regular deposits averages the number of positive net deposits (deposits - withdrawals) within each client and compares this average to the median value of all clients' averages. The dummy variable regular deposits equals one if a given client's average is the same or above the median value of making positive deposits in 84.4 percent of weeks.

Large deposits indicates above median deposit sizes (P 33.13). In this calculation, I only include positive net deposits to avoid the influence of weeks in which no deposit was made or money was withdrawn, as no (or negative) deposit is already accounted for in regular deposits.

Equally-sized deposits indicates a below median value of the deposit variance to average deposit ratio (1.03).⁹ The variance is standardized with the average

⁷ Excluding all later sessions prevents any spill-over effects from the experiment on subsequent savings behavior. In addition, using deposits rather than the savings stock accounts better for behavior than the stock as the latter is highly correlated with membership length.

⁸ Some clients joined later than July 2016, for them, fewer weekly observations are available. To account for this, I use within-client averages before computing median values.

⁹ An alternative measure would be to count the number of weeks in which the same amount was deposited as in the previous week.

deposit to take care of the size of deposits, which is already measured in large deposits.

Table A.12: Saver Types

	% in Sample N=300	% in same Center N=676	% in same Branches N=3773
Irregular-small-variable	9.67	10.65	7.05
Irregular-small-equal	13.00	10.80	12.24
Irregular-large-variable	11.00	20.27	20.91
Irregular-large-equal	12.67	9.17	7.34
Regular-small-variable	12.00	12.13	11.77
Regular-small-equal	12.00	13.02	18.37
Regular-large-variable	12.67	9.76	11.93
Regular-large-equal	17.00	14.20	10.39

Notes: Regular vs. irregular deposits; small vs. large deposits; equally-sized vs. variable deposits (all based on median sample splits). "Sample" comprises all interviewees, "in same Center" are non-participants in sample centers and "in same Branches" are non-sample centers in the study branches.

Table A.13: Balance of Saver Types in Experimental Sample

	CA ₃₀₀	CiH ₃₀₀	CA ₅₀₀	CiH ₅₀₀	R ²	F-test
Saver Types						
Regular deposit	0.452	0.110*	0.119	-0.123	0.009	0.331
High deposit	0.479	0.0411	0.0660	-0.00213	0.006	0.606
Equally sized deposits	0.589	-0.0685	-0.0566	0.0815	0.003	0.833
Irregular-small-variable	0.178	-0.0959**	-0.113	0.0959*	0.025	0.133
Irregular-small-equal	0.178	-0.0548	-0.0742	0.0678	0.007	0.430
Irregular-large-variable	0.0548	0.0822	0.0621	-0.0692	0.011	0.313
Irregular-large-equal	0.137	-0.0411	0.00587	0.0281	0.003	0.795
Regular-small-variable	0.0822	0.0274	0.0737	-0.0534	0.007	0.657
Regular-small-equal	0.0822	0.0822	0.0477	-0.108	0.009	0.408
Regular-large-variable	0.0959	0.0548	0.0340	-0.0548	0.003	0.777
Regular-large-equal	0.192	-0.0548	-0.0359	0.0938	0.004	0.631

Notes: Mean of the CA₃₀₀ group and coefficients from OLS regressions with treatment dummies as independent variables and clustered SE (not shown) at the center level. The last column shows p-values of the F-test of joint significance of the treatment dummies. *** p<0.01, ** p<0.05, * p<0.1.

A.6 RELIABILITY OF SURVEY RESPONSES

The reliability of participants' responses during the interview ("interview data") can be evaluated based on administrative data that cover savings, characteristics of the person (e.g. age) and the house (e.g. connected to running water). While reports of age in the administrative and the interview data should be highly correlated, the correlation might be weaker for savings reports due to e.g. inattention or social concerns when reporting. Indeed, as Table A.14 shows, age is nearly perfectly correlated in self-reported and administrative data. Reports of savings in the account (interview data) are also positively correlated with actual savings (administrative data). Being composed of ten questions regarding household wealth, the PPI score from the administrative data is comparable to the asset index in the interview data and the two are positively correlated. Comparing single questions that are part of both indices, however, I find significantly higher asset possessions in the interview data (electricity, running water and landline phones).¹⁰ Overall, it seems that participants respond consistently regarding the most important aspects of this study.

Table A.14: Correlation of Self-Reported and Administrative Data

	Self-reported Data	Administrative Data	Correlation
Savings balance	767.4 (1494)	790.3 (1230)	0.734***
Total savings/savings balance (₪)	6108.7 (11175)	790.3 (1230)	0.454***
Age	43.57 (12.35)	43.94 (12.31)	0.979***
Assets/PPI score	0.486 (0.166)	43.53 (19.65)	0.370***
Electricity	0.95 (0.218)	0.468 (0.499)	0.047
Landline	0.056 (0.232)	0.00676 (0.082)	-0.020
Water	0.69 (0.463)	0.158 (0.366)	0.072

Notes: Means of raw data, SD in parentheses and Spearman's ρ . *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

¹⁰ This could either be due to at least two reasons. First, living conditions might have improved since that the administrative data was last collected. Second, participants might underreport their assets via à-vis the MFI in order to appear "needy". In theory, the organization committed to only serving the very poor as assessed by a progression out of poverty index. However, from discussions with the management, it appears that this rule is not strictly enforced, especially once the member has been accepted as a borrower.

A.7 DESCRIPTION OF VARIABLES

Variables from the interview

- Part 1
 - *Cash income*: Sum of all income that respondents report to receive in cash, measured in ₱. This can include business income, farm income, labor wages, pension, remittances, government aid and other income.
 - *Non-cash income*: Sum of all income that respondents report to receive in means other than cash (e.g. transfer, check), measured in ₱. This can include business income, farm income, labor wages, pension, remittances, government aid and other income.
 - *Money today*: Answer to "All in all, how much money will you personally be able to take home at the end of today?", measured in ₱.
 - *Assets*: Equally weighted index ranging from zero to one and indicating whether the household has the following: electricity, running water, radio, television, landline telephone, mobile phone, personal computer, refrigerator, washing machine, CD/DVD player, bicycle, motorcycle and animal-drawn cart.
 - *Decision making power*: Similar to Ashraf et al. (2010), eight questions are asked regarding "who decides" in the following situations: What to buy at the market, making expensive purchases, giving assistance to family members, recreational use of money, personal use of money, saving, number of children, schooling of children. If the husband decides, the item takes the value of zero, one if it is a joint decision and two if the respondent decides herself. An index is constructed by using the equally weighted mean of all answers.
 - *Education*: Indicates the level of education completed. Ranging from zero (no formal education) to eleven (beyond high school education).

- *Household size*: Number of persons living in the household.
 - *Business owner*: Indicator variable taking the value one if the respondent runs her own business.
 - *Time to center*: Travel time to center in minutes.
 - *Travel cost to center*: Amount in ₹ that is spent one-way to attend the center meeting.
- Part 2
 - *Present bias*: Indicator variable taking the value one if choices in the present (today vs. 2 weeks) are less patient than in the future (in 2 weeks vs. 4 weeks).
 - *Future bias*: Indicator variable taking the value one if choices in the present (today vs. 2 weeks) are more patient than in the future (in 2 weeks vs. 4 weeks).
 - *Risk aversion*: Index scaled on the interval $[0,1]$ with higher values indicating higher risk aversion (higher risk aversion implies later switches from lottery A to B).
 - *Loss aversion*: Index scaled on the interval $[0,1]$ with higher values indicating higher loss aversion (higher loss aversion implies later switches from lottery A to B).
 - Part 3
 - *Money left*: "Does your household have money left over at the end of the week after you have paid for food and other necessities?" encoded as follows: 1=yes, regularly; 2=yes, sometimes; 3=no
 - *Attention to finances*: Two questions from Stango et al. (2017) whether finances would improve with more attention given to a) day-to-day finances, routine expenses such as food (short-run attention) and b) medium-run finances, periodic expenses such as school fees (medium-run attention). Binary indicators are constructed for short-run attention and medium-run attention that are one if the household is paying attention to the respective finances.

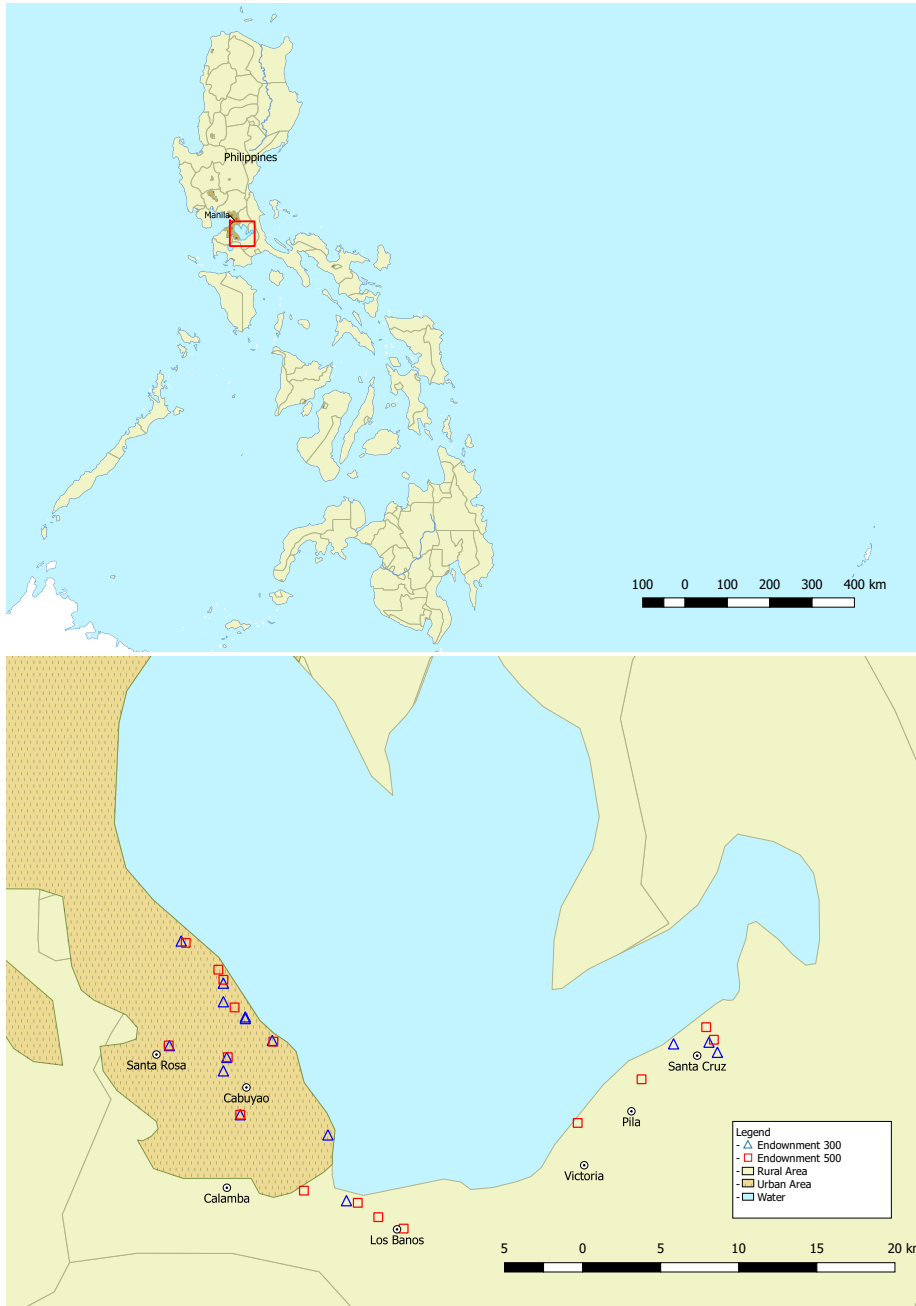
- *Financial literacy*: Equally weighted index of correctly answered financial literacy questions (Questions 1, 2, 3 and 6 from the World Bank's Financial Literacy Quiz), scaled to the interval [0,1].
- *Narrow bracketing*: Two questions adapted from Stango et al. (2017) that are coded as two indicator variables: "bracketing some" indicates narrow bracketing in at least one question and "bracketing both" indicates narrow bracketing in both questions.
- *Total savings*: Sum of the following variables (all measured in ₪): savings at home, savings with the family, formal savings, saving by lending money, savings in the savings account, savings in the current account (money that remains because the loan has not yet been fully spent) and savings at cooperatives and other organizations.
- *Savings in ASHI are safe**: Agreement to "Saving at ASHI is not safe".
- *Banks untrustworthy**: Agreement to "Banks cannot be trusted".
- *Would like private account**: Agreement to "I wish I had a savings account where I could hide my money from others".
- *Saving less due to...**:
 - * *...claims from husband*: Agreement to "I would save more but my husband needs the money."
 - * *...claims from family & friends*: Agreement to "If I save, I will only end up giving the money to my family and friends."
 - * *...claims from clients*: Agreement to "If I have savings with ASHI, I will need to spend it on abonohan [in-lieu payments] for others."
 - * *...being over optimistic regarding saving*: Agreement to "I always think I would save the next week, but then I keep postponing it."

* denotes a five-point likert scale agreement to a specific question. All answers have been recoded such that higher values represent higher agreement and lie in the interval [0,1].

Variables from administrative data

- *PPI score*: Ten questions that are being asked when applying for a new loan, e.g. "Do all children in the family of ages six to 14 go to school?" Answers are converted into points (e.g. no-0, yes-2, no children in this age range-4) and all points are added. The total score lies between zero and 100. Country-specific tables allow mapping the score to a probability of falling below a given poverty line. For instance, a PPI score of 47.5 (sample mean) indicates a 27 percent chance of being below the US\$ 2.50/day/2005 PPP poverty line and a 77 percent chance of living with less than US\$ 3.75 per day in 2005 PPP.
- *Main income: Enterprise*: Is an indicator variable that takes the value one if the main income source is an enterprise. Other income sources registered in the data are employment, farming and fishing.
- *House size*: is encoded as follows: 0-small, 1-medium, 2-large.
- *House strength*: is encoded as follows: 0-poor, 1-medium, 2-strong.

A.8 SETTING



A.9 INSTRUCTIONS

A.9.1 *Announcement Letter*

LMU · Geschwister-Scholl-Platz 1 · 80539 Munich · Germany

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Ludwig-Maximilians-Universität
München
Chair for Behavioral and
Experimental Economics
Giselastr. 10
D-80802 Munich
GERMANY

Munich, February 1, 2017

Dear ASHI member,

You are cordially invited to take part in a survey on household finances.

We are a team of independent researchers from the University of Munich in Germany and we would like to learn more about your needs and the way you use microfinance products. As a member of ASHI, you can provide us with valuable information that might help improve existing microfinance products.

We would like to interview several members of ASHI individually. The involvement in the interview will require about 30 minutes and interviews will be in Tagalog. Interview participants will receive at least 300 pesos as a token of appreciation and every member of the center has the same chance to participate in an interview.

The interviews will take place during the center meeting on [DATE] and all answers will be treated confidentially, i.e. we will not share your answers with ASHI. All interviews will be conducted in private by the team of independent researchers.

We would be very happy if many of you were interested in taking part in the survey and came to the center meeting on that day.

I am looking forward to meet you soon.

Lisa Spantig
Researcher at the University of Munich, Germany

The announcement letter was distributed via the loan officer one week before the session. The loan officer announced the visit and each member received her individual copy.

A.9.2 *Interview Questionnaire*

The survey was implemented in zTree (Fischbacher, 2007) and run on tablets. The formatted questionnaire thus only serves to display all questions in this document. Before each interview, I set the parameter of the zTree program such that all instructions would be shown according to the pre-determined treatment allocation. This enabled all surveyors to interview in both *CiH* and *CA* treatments without confusion, as the program would give detailed instructions on what to do and when.

Survey				
I - Identification				
11. What is your name? [FIRST, MIDDLE, LAST] _____				
12. Did you receive an announcement letter that this interview would take place?				
1 Yes		2 No		
13. Are you single, married/living with partner, separated or widowed?				
1 Single		2 Married/living with partner		
3 Separated/divorced		4 Widowed		
5 DON'T READ: REFUSED				
14. How many persons including yourself live in your household? (exclude guests, visitors, household members who do not sleep at home at least once a week) _____				
15. How many persons in your household including yourself earn money? _____				
16. How many persons in your household are currently attending school? _____				
<i>The following questions are about yourself.</i>				
17. How old are you? _____				
18. What is the highest formal education level you have completed? _____				
Education codes				
0 No schooling	1 Grade 1	2 Grade 2	3 Grade 3	4 Grade 4
5 Grade 5	6 Elementary Graduate	7 High school 1	8 High school 2	9 High school 3
10 High school 4	11 High school graduate	12 Vocational Incomplete	13 Vocational Complete	14 Some College
15 College graduate or higher				
H - Household income				
1 Government official		2 Professional or technical (non-production)		
3 Administrative or clerical (public)		4 Administrative or clerical (private)		
5 Sari-sari store owner		6 Tricycle, jeepney, taxi, or other transport		
7 Farmers, fisherman, hunters, loggers and related workers		8 Miners, quarrymen and related workers		
9 Craftsman or production-process		10 Plant and machine operators and assemblers		
11 Wage laborers		12 Entrepreneur (>5 employees): Service		
13 Entrepreneur (>5 employees): Buy/Sell		14 Microentrepreneur (>5 employees): Service		
15 Microentrepreneur (>5 employees): Buy/Sell		16 Retired personnel (GO & private org)		
17 Houseworker (without wage), student unemployed		18 Other		
19 No household income at all				

H1. What is your household's main source of income? [SOURCE THAT GIVES THE MOST INCOME]

H2a. Please let me know, does your household have any income from the following sources?

1 Yes	2 No
-------	------

Net business income	
Farm income	
Labor wages	
Pension	
Remittances	
Government aid/income subsidy	
Rental	
Other income source (specify)	

[IF H2a = YES, PROCEED WITH H2b FOR THIS CATEGORY]

	H2b. Amount	H2c. Time unit 1 - Daily 2 - Weekly 3 - Monthly 4 - Other specify	H2d. How is this income received? 1 - Cash 2 - Check 3 - Deposit 4 - Other, specify	H2e. Is it easy to estimate how much income you will receive in the next month? 1 - Yes 2 - No 3 - DON'T READ: DON'T KNOW 4 - DON'T READ: REFUSED
Net business income				
Farm income				
Labor wages				
Pension				
Remittances				
Government aid/income subsidy				
Rental				
Other income source (specify)				

H3a. This next question is about household budgets. A household budget is used to decide what share of your household income will be used for spending, saving or paying bills. Does your household have a budget?

1 Yes	2 No
-------	------

H3b. [If H3a. = yes]: Do you usually stick to the budget?

1 Yes	2 No
3 DON'T READ: REFUSED	

H4. All in all, how much money will you personally be able to take home at the end of today?

E - EXPENDITURES																													
<i>Now I'll ask you some questions about your expenses.</i>																													
<p>E1. About how much did you and your household spend on everything in the last 7 days? Please think about all bills such as rent, loan payments, utility and other bills, as well as all expenses such as food, clothing, transportation and any other expenses you and your household may have. _____ pesos</p>																													
<p>E2. In a typical week, how much of your own money do you spend food and beverages? _____ pesos</p>																													
<p>E3. In a typical week, how much of your own money do you spend on non-food items such as personal products, cleaning products or textiles? _____ pesos</p>																													
<p>E4. In a typical month, how much of your own money do you spend on bills? _____ pesos</p>																													
<p>E5. In a typical month, how much of your own money do you spend on medicine and medical equipment? _____ pesos</p>																													
<p>E6. In a typical month, how much of your own money do you spend on education? _____ pesos</p>																													
<p>E7. During the last week, how much of your own money did you spend on something and afterwards regretted spending the money? _____ pesos</p>																													
<p>E8. Which ONE of the following best describes the extent to which you personally monitor your regular expenses? [READ OUT ALL OPTIONS]</p>																													
<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">1 I don't keep an eye on expenses at all</td> <td style="padding: 2px;">2 I keep my eye on expenses a bit</td> </tr> <tr> <td style="padding: 2px;">3 Without keeping written records, I keep a fairly close eye on expenses</td> <td style="padding: 2px;">4 I use written records to keep a close eye on expenses</td> </tr> <tr> <td style="padding: 2px;">5 DON'T READ: REFUSED</td> <td></td> </tr> </table>	1 I don't keep an eye on expenses at all	2 I keep my eye on expenses a bit	3 Without keeping written records, I keep a fairly close eye on expenses	4 I use written records to keep a close eye on expenses	5 DON'T READ: REFUSED																								
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5 DON'T READ: REFUSED																													
A - Assets																													
<p>A1. Please let me know whether <i>your household</i> has the following</p>																													
<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">1 Yes</td> <td style="padding: 2px;">2 No</td> </tr> </table>	1 Yes	2 No																											
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<table border="1" style="width: 100%; border-collapse: collapse;"> <tr> <td style="padding: 2px;">A1a. Electricity</td> <td style="width: 20px;"></td> <td style="padding: 2px;">A1h. Refrigerator/freezer</td> <td style="width: 20px;"></td> </tr> <tr> <td style="padding: 2px;">A1b. Running water</td> <td></td> <td style="padding: 2px;">A1i. Washing machine</td> <td></td> </tr> <tr> <td style="padding: 2px;">A1c. Radio/radio cassette</td> <td></td> <td style="padding: 2px;">A1j. CD or VCD or DVD player</td> <td></td> </tr> <tr> <td style="padding: 2px;">A1d. Television</td> <td></td> <td style="padding: 2px;">A1k. Bicycle or trisikad/pedicab</td> <td></td> </tr> <tr> <td style="padding: 2px;">A1e. Landline telephone</td> <td></td> <td style="padding: 2px;">A1l. Motorcycle or tricycle</td> <td></td> </tr> <tr> <td style="padding: 2px;">A1f. Cellular phone</td> <td></td> <td style="padding: 2px;">A1m. Animal-drawn cart/sledge</td> <td></td> </tr> <tr> <td style="padding: 2px;">A1g. Personal computer or laptop</td> <td></td> <td></td> <td></td> </tr> </table>	A1a. Electricity		A1h. Refrigerator/freezer		A1b. Running water		A1i. Washing machine		A1c. Radio/radio cassette		A1j. CD or VCD or DVD player		A1d. Television		A1k. Bicycle or trisikad/pedicab		A1e. Landline telephone		A1l. Motorcycle or tricycle		A1f. Cellular phone		A1m. Animal-drawn cart/sledge		A1g. Personal computer or laptop				
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A1f. Cellular phone		A1m. Animal-drawn cart/sledge																											
A1g. Personal computer or laptop																													

A2. Who in the household makes the following decisions, you, your husband/partner or you and your husband/partner jointly?

0 Husband	1 Joint
2 Self	4 Others make the decision

A2a. What to buy at the market	
A2b. Whether to make an expensive purchase such as TV	
A2c. Whether to give assistance to family members	
A2d. The recreational use of money	
A2e. How the money you personally earned will be used	
A2f. Put money aside for savings	
A2g. Number of children	
A2h. Schooling of children	

O - Own business

I will now ask you some questions regarding your own business.

O1. Do you currently run your own business? [IF NO -> SKIP TO NEXT SECTION]

1 Yes	2 No
3 DON'T READ: REFUSED	

O2a. Do you plan your business cash flow?

1 Yes	2 No
3 DON'T READ: REFUSED	

O2b. IF O2a=yes: On which basis do you plan your business cash flow?

1 Daily	2 Weekly
3 Bi-weekly	4 Monthly
5 Bi-monthly	6 I don't plan
7 Other, specify:	8 DON'T READ: REFUSED

O3. How much do you agree or disagree with the following statements?

1 strongly agree, 2 agree, 3 neither agree nor disagree, 4 disagree, 5 strongly disagree
[SHOW SCALE]

O3a. It is easy to plan how much money I can make in one week from my business. [USE SCALE]

O3b. What do you think: How high will your personal gross business income in the next week be?

O3c. It is easy to plan how much money I will need to spend on my business in one week. [USE SCALE]

O3d. What do you think: During the next week, how high will the expenditures for your business be? _____ pesos

O4. How much cash do you typically need to hold for your business to run smoothly? For example, if you have a sari-sari store, how much money do you need to hold?
_____ pesos

M - Misc

Now, I have some very general questions for you.

M1. Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? [READ OUT OPTIONS AND ENTER 1 OR 2]

1 Most people can be trusted

2 Need to be very careful

M2. How long does it take you to get to the ASHI center meeting? (one-way, in minutes)
_____ minutes

M3. How much does it cost you (e.g. fares) to get to the ASHI center meeting?
(one-way, in Peso) _____ pesos

D - Decision
<p>TREATMENT: For participation in this survey, you have received 300/500 pesos. If you decide to save some of this money now in your ASHI personal savings account and the amount you deposited today is still in your account in four weeks' time, I will add 20% of this amount to your savings account. For example, if you decide to save 100 pesos and in four weeks you still have at least 100 pesos in your savings account, I will add 20 pesos in four weeks' time to your account. This additional payment is guaranteed and you will receive this confirmation [SHOW CONFIRMATION SHEET]. If, instead, there is less than the 100 pesos you decided to save today in your savings account in four weeks, I will add nothing to your savings account. If you want to save some money, you hand it to me and I will put it in this envelope and seal the envelope. We will give the envelope to the DO still during the center meeting once we have finished all interviews. You can save any amount you want between 0 and 300 pesos in multiples of 1 peso.</p> <p>You keep all the money that you do not want to save. Do you have any questions? [IF YES, CLARIFY]. Please now hand me the money you want to save. [Do NOT PRESSURE HER TO GIVE YOU ANY MONEY. IF THE NANAY OFFERS YOU SOME MONEY, COUNT IT AND PLACE IT IN THE ENVELOPE, WRITE HER NAME ON THE ENVELOPE. NOTE THE AMOUNT SAVED ON THE SCREEN. IF SHE DECIDED TO SAVE: FILL IN ONE CONFIRMATION SHEET]</p> <p>D: Amount Saved: _____</p> <p style="margin-left: 20px;">20 peso bills: _____</p> <p style="margin-left: 20px;">50 peso bills: _____</p> <p style="margin-left: 20px;">100 peso bills: _____</p> <p>CONTROL: For participation in this survey, you will receive 300/500 pesos. If you decide to save some of this money now in your ASHI personal savings account and the amount you deposited today is still in your account in four weeks' time, I will add 20% of this amount to your savings account. For example, if you decide to save 100 pesos and in four weeks you still have at least 100 pesos in your savings account, I will add 20 pesos in four weeks' time to your account. If, instead, there is less than the 100 pesos you decided to save today in your savings account in four weeks, I will add nothing to your savings account. The additional payment after four weeks is guaranteed, you will receive this written confirmation [SHOW CONFIRMATION SHEET]. If you want to save some money, I will put it in this envelope and seal the envelope. We will give the envelope to the DO still during the center meeting once we have finished all interviews. You can save any amount you want between 0 and 300 pesos in multiples of 1 peso.</p> <p>I will give you all the money that you do not want to save after preparing the envelope. Do you have any questions? [IF YES, CLARIFY] Please now tell me whether and if yes, how much money you want to save. [TAKE OUT ALL THE CASH, COUNT IT, PLACE THE SUM THE NANAY WANTED TO SAVE IN THE ENVELOPE, WRITE HER NAME ON THE ENVELOPE AND HAND THE REST OF THE MONEY TO HER. NOTE THE AMOUNT SAVED ON THE SCREEN. IF SHE DECIDED TO SAVE: FILL IN ONE CONFIRMATION SHEET]</p> <p>D: Amount Saved: _____</p>

PART 2: Experimental Preference

The following set of questions form part of a game in which you can earn additional money. Approximately 1 out of 6 people will actually be paid for one of these questions at the end of the interview. We will make a lucky draw at the end of the survey to determine whom of the participants will be paid. You don't know whether you will be one of the lucky ones and which question you might be paid for. Therefore, 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.

The following questions will ask you whether you want P50 now, or a different amount of money in two weeks. All amounts will be paid with mobile phone load. If you are paid, we will record your mobile phone number at the end of the survey. If you don't have a mobile phone, you can give us the number of a family member or friend.

If a question is selected where you chose P50 now, then we will transfer P50 of load today. If a question is selected where you choose an amount in 2 weeks, then we will transfer that amount in 2 weeks. This payment is guaranteed. We will also give you a paper voucher (SHOW VOUCHER) which states your name, the amount, and the date when we send the load. We will also give you a number you can contact if your phone number changes. Do you have any questions on this before I start?

[INSTRUCTORS:

- ASK EACH QUESTION SEPARATELY. LET THEM THINK ABOUT EACH ONE.
- DO NOT SHORTEN OR ABBREVIATE THE QUESTIONS IN ANY WAY
- AVOID SWITCHING BACK AND FORTH. IF RESPONDENT SWITCHES BACK AND FORTH; CHECK THEY UNDERSTOOD THE QUESTION.]

Which option do you prefer?

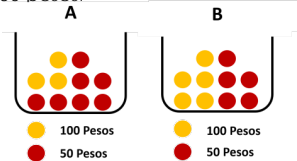
1 Now	2 Later
-------	---------

Ex1a. Do you prefer P50 guaranteed today or P40 pesos in 2 weeks?	
Ex1b. Do you prefer P50 guaranteed today or P50 pesos in 2 weeks?	
Ex1c. Do you prefer P50 guaranteed today or P60 pesos in 2 weeks?	
Ex1d. Do you prefer P50 guaranteed today or P75 pesos in 2 weeks?	
Ex1e. Do you prefer P50 guaranteed today or P100 pesos in 2 weeks?	
Ex1f. Do you prefer P50 guaranteed today or P125 pesos in 2 weeks?	
Ex1g. Do you prefer P50 guaranteed today or P150 pesos in 2 weeks?	
Ex1h. Do you prefer P50 guaranteed today or P200 pesos in 2 weeks?	

I will now ask you to compare different kinds of lotteries. There will always be a lottery A and a lottery B. If you decide to play lottery A and this question is selected, we will actually play lottery A and you will win the money from that lottery. All lotteries can be selected for payment, so think hard about which lottery you prefer.

Let me give you an example of such a lottery. Here, you can see one lottery A. Each lottery will consist of 10 balls and balls can be of two different colors and of different value. Here, we have three yellow balls, that are each worth 100 pesos and seven red balls worth 50 pesos. So, the chance of winning 100 pesos is three in ten and the chance of winning 50 pesos is seven in ten. If this question is selected for payment and you want to play lottery A, then we will draw one ball from an opaque bag. If it is yellow, you will receive 100 pesos, if the ball is red, you will receive 50 pesos. Do you have any questions?

Now let's compare the first two lotteries. This is only for practice and there will be no payment for this comparison. Lottery A has a three in ten chance of winning 100 pesos and a seven in ten chance of winning 50 pesos. Lottery B has a five in ten chance of winning 100 pesos and a five in ten chance of winning 50 pesos.



Ex2a. Which lottery do you prefer? A or B

Now let's compare the first two lotteries that you may play at the end of the survey. Here, there is no right or wrong.

Lottery A has a one in ten chance of winning 150 pesos and a nine in ten chance of winning 100 pesos. Lottery B has a one in ten chance of winning 250 pesos and a nine in ten chance of winning 10 pesos.

Ex2b. Which lottery do you prefer? A or B

Now chances for winning the high price increase in both lotteries.

Lottery A has a two in ten chance of winning 150 pesos and an eight in ten chance of winning 100 pesos. Lottery B has a two in ten chance of winning 250 pesos and an eight in ten chance of winning 10 pesos.

Ex2c. Which lottery do you prefer? A or B

[CONTINUES UNTIL CHANCE OF WINNING THE HIGH AMOUNT IS 9 IN 10]

The following questions ask you to choose between 50 pesos now, and a different amount in 4 weeks from now. As before, you might get paid for one of these questions in load. The payment is guaranteed. So please really think about which reward you prefer for each question.

1 Now	2 Later
-------	---------


Ex3a. Do you prefer P50 guaranteed today or P40 pesos in 4 weeks?	
Ex3b. Do you prefer P50 guaranteed today or P50 pesos in 4 weeks?	
Ex3c. Do you prefer P50 guaranteed today or P60 pesos in 4 weeks?	
Ex3d. Do you prefer P50 guaranteed today or P75 pesos in 4 weeks?	
Ex3e. Do you prefer P50 guaranteed today or P100 pesos in 4 weeks?	
Ex3f. Do you prefer P50 guaranteed today or P125 pesos in 4 weeks?	
Ex3g. Do you prefer P50 guaranteed today or P150 pesos in 4 weeks?	
Ex3h. Do you prefer P50 guaranteed today or P200 pesos in 4 weeks?	

I will now ask you whether you are willing to play different kinds of lotteries. In each lottery, you can lose some money or you can win some money. If you decide for lottery A and this question is selected, we will play lottery A. If you win a lottery, I will give you the additional money. If you lose a lottery, I will ask you to pay for it from your participation fee. All lotteries can be selected for payment, so think hard which one you prefer.

Now let's compare the first two lotteries that you may play at the end of the survey. Again, there is no right or wrong.


Lottery A has a five in ten chance of winning 60 pesos and a five in ten chance of losing 35 pesos. Lottery B has a five in ten chance of winning 75 pesos and a five in ten chance of losing 65 pesos.

A



● +60 Pesos
● -35 Pesos

B



● +75 Pesos
● -65 Pesos

Ex4a. Which lottery do you prefer? A or B

Now the amounts that you can win or lose change. Chances stay the same as before.

Lottery A has a five in ten chance of winning 55 pesos and a five in ten chance of losing 35 pesos. Lottery B has a five in ten chance of winning 75 pesos and a five in ten chance of losing 65 pesos.

Ex4b. Which lottery do you prefer? A or B

[CONTINUES WITH

- LOTTERY A: +50, -35; LOTTERY B: +75, -65
- LOTTERY A: +45, -35; LOTTERY B: +75, -65
- LOTTERY A: +40, -35; LOTTERY B: +75, -50
- LOTTERY A: +40, -35; LOTTERY B: +75, -45
- LOTTERY A: +35, -35; LOTTERY B: +75, -40]

The following questions ask you to choose between 50 pesos in two weeks from now, and a different amount in 4 weeks from now. As before, you might get paid for one of these questions in load. The payment is guaranteed. So please really think about which reward you prefer for each question.

Which option do you prefer?

2 Weeks	4 Weeks
---------	---------

Ex5a. Do you prefer P50 guaranteed in 2 weeks or P40 pesos in 4 weeks?	
Ex5b. Do you prefer P50 guaranteed in 2 weeks or P50 pesos in 4 weeks?	
Ex5c. Do you prefer P50 guaranteed in 2 weeks or P60 pesos in 4 weeks?	
Ex5d. Do you prefer P50 guaranteed in 2 weeks or P75 pesos in 4 weeks?	
Ex5e. Do you prefer P50 guaranteed in 2 weeks or P100 pesos in 4 weeks?	
Ex5f. Do you prefer P50 guaranteed in 2 weeks or P125 pesos in 4 weeks?	
Ex5g. Do you prefer P50 guaranteed in 2 weeks or P150 pesos in 4 weeks?	
Ex5h. Do you prefer P50 guaranteed in 2 weeks or P200 pesos in 4 weeks?	

Part 3

EM - EXCESS MONEY

I will now ask you some questions about your household's finances, whether you sometimes encounter difficulties and how you deal with them.

EM1a. Does your household have money left over at the end of the week after you have paid for food and other necessities?

1 Yes, regularly	2 Yes, sometimes
3 No	4 DON'T READ; REFUSED

EM1b. IF YES: What does your household do with this left over money? [DON'T READ; MAX. 3 ENTER 1-3 IN ORDER OF MENTIONING]

1 Spend on utility bills	2 Spend on food
3 Spend on school fees	4 Spend on treats (sweets, Jollibee, toys)
5 Spend on appliances	6 Lend to relative
7 Lend to friend	8 Lend to neighbor
9 Donate to relative/friend/neighbor	10 Invest in business
11 Pay off loan	12 Save at home
13 Save in ASHI	14 Save in institution (other than ASHI)
15 Other, specify:	

EM2. Does your household ever run short of money for food or other necessary items?

1 Yes, regularly	2 Yes, sometimes
3 Seldom	4 No
5 DON'T READ: REFUSED	

EM3a. Do you believe that your household's day-to-day finances' (dealing with routine expenses for example for food and other necessities, loan repayment, school allowance, transportation etc.) would improve if your household paid more attention to them? [READ OUT ANSWER POSSIBILITIES]

1 Yes, and I/we often regret not paying greater attention	2 Yes, but paying more attention would require too much time/effort
3 No, my household finances are set up so that they don't require much attention	4 No, my household is already very attentive to these matters

EM3b. Do you believe that your household's *medium-run finances*' (dealing with periodic expenses like house repair, school fees etc.) would improve if your household paid more attention to them?

1 Yes, and I/we often regret not paying greater attention	2 Yes, but paying more attention would require too much time/effort
3 No, my household finances are set up so that they don't require much attention	4 No, my household is already very attentive to these matters

Q – QUESTIONS ON WINDFALL GAINS AND BRACKETING I

I will now ask you about different hypothetical situations and what you would do in these situations.

Q1. Imagine you have 3000php in cash. What would you do with the money? [DON'T READ; MAX. 3 ORDER 1-3 IN ORDER OF MENTIONING]

1 Spend on utility bills	2 Spend on school fees
3 Spend on food	4 Spend on treats (sweets, Jollibee, toys)
5 Spend on appliances	6 Lend to relative
7 Lend to friend	8 Lend to neighbor
9 Donate to relative/friend/neighbor	10 Invest in business
11 Pay off loan	12 Save at home
13 Save in ASHI	14 Save in institution (other than ASHI)
15 Other, specify:	

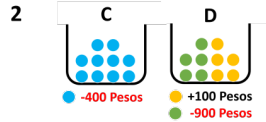
Q2. I will now ask you to make two decisions. There is no right or wrong and there is no payment involved. Please examine both decisions and then let me know which of the options you prefer.

Decision 1: A winning 100 for sure OR B a 5 in 10 chance of losing 300 and a 5 in 10 chance of winning 700

1

+100 Pesos
 +700 Pesos
 -300 Pesos

Decision 2: C losing 400 for sure OR D a 5 in 10 chance of losing 900 and a 5 in 10 chance of winning 100



Q2a. Decision 1: A or B Q2b. Decision 2: C or D

SI - SAVINGS INVENTORY

The next set of questions will concern your savings, where and how much you save.

SI1. Do you have cash savings held at home for safe keeping? If yes, how much do you have in savings right now in the form of cash at home? [WRITE 00 IF NONE]
_____ pesos

SI2. Do you keep money with your friends/family or employer for safekeeping? If yes, how much? [WRITE 0 IF NONE]
_____ pesos

SI3. Do you currently have any money lent out to others? If yes, how much? [WRITE 0 IF NONE, DO NOT COUNT MONEY GIFTS TO FRIENDS/FAMILY IF THEY WILL NOT GET IT BACK. DO NOT COUNT MONEY GIVEN TO OTHERS ONLY FOR SAFEKEEPING]
_____ pesos

SI4a. How much money do you currently have in your AHSI personal savings?
_____ pesos

SI4b. How much money from your last loan do you still have in your electronic card?
_____ pesos

SI5. Do you have savings at a bank, MFI, coop or other formal institution other than ASHI? If yes, how much?
_____ pesos

SI6. Are you currently a member of any savings organization with member coming from your church, neighbors, or friends? If yes, how much money do you currently have in savings in these organizations? [WRITE 0 IF NONE]

SI7. Do you have savings in the form of gold (or jewelries made of gold) at home?

1 Yes	2 No
3 DON'T READ: DON'T KNOW	4 DON'T READ: REFUSED

SI9. If you want to save at home or at your place of work, do you have a safe place where no one will take it away?

1 Yes	2 No
3 DON'T READ: REFUSED	

SI10. In general, are you able to save as much as you want?

1 Yes	2 No
3 DON'T READ: REFUSED	

SG - SAVING GOALS & BRACKETING II	
SG1a. Do you currently save money?	
1 Yes	2 No
3 DON'T READ: REFUSED	
SG1b. If SG1a=Yes: What is the main reason you save? [MAX 3, RANK 1-3]	
1 Capital for business	2 Christmas, birthdays (regular celebrations)
3 Weddings, baptisms, town fiestas, and functions	4 Unexpected Emergencies (illness, sudden loss of income etc)
5 Repay another debt	6 School fees/education
7 Personal use (entertainment, clothes, etc)	8 Health/Medical Costs
9 Cell phone, appliance, TV, etc	10 Utility bills (gas, water, electricity etc)
11 Future needs, e.g. retirement	12 For natural disaster (e.g. typhoon)
13 House Construction/repair	14 Other (specify):
If SG1a=NO: Why don't you save? [MAX 1-3, RANK 1-3]	
1 Not enough money to save	2 I don't need it
3 There are too many documentary requirements	4 I had a bad experience in the past
5 It is too expensive	6 I don't have knowledge about savings
7 Other, specify:	
SG2. I will now ask you to make two decisions. There is no right or wrong and there is no payment involved. Please examine both decisions and then let me know which of the options you prefer. Decision 1: winning 850 for sure OR a 5 in 10 chance of winning 100 and a 5 in 10 chance of winning 1600 Decision 2: losing 650 for sure OR a 5 in 10 chance of losing 1550 and a 5 in 10 chance of winning 100	
SG2a. Decision 1: A or B	SG2b. Decision 2: C or D
AS - ATTITUDE TOWARDS SAVINGS	
<i>I will now ask you a set of questions concerning your opinion and experiences with savings.</i>	
Please let me know how much you agree or disagree with the following statements and questions. Do you fully agree, agree, neither agree nor disagree, disagree or fully disagree?	
[CODE: 1 FULLY AGREE, 2 AGREE, 3 NEITHER AGREE NOR DISAGREE, 4 DISAGREE, 5 FULLY DISAGREE]	
AS1. Keeping money aside for a purpose is important for me	
AS2. Saving at home is not safe	
AS3. Banks cannot be trusted	
AS4. If I have savings with ASHI, I will need to spend it on abonohan for others.	
AS5. Savings are not useful	
AS6. Saving at ASHI is not safe	
AS7. I would save more but my husband needs the money	
AS8. I would like to save more but cash creates needs	
AS9. I would like to save but I forget to keep money aside	
AS10. I would like to save but then unforeseen expenditures are needed	
AS11. I would save more if my ASHI group members saved more	
AS12. Using my ASHI savings account takes too much time	
AS13. I have experienced problems with my savings in ASHI	

AS14. If I have savings with ASHI, I will not receive abonohan from others.	
AS15. I cannot access my savings when I need them as the center meeting is only once a week.	
AS16. If I save, I will only end up giving the money to my family and friends.	
AS17. When I have some savings, I will to soon take the money and spend it.	
AS18. If I have cash, I think about what I can buy but I don't think about savings.	
AS19. I wish I had a savings account where I could hide my money from others	
AS20. I always think I would save the next week, but then I keep postponing it.	

KS - KNOWLEDGE ABOUT SAVINGS

K1. What is the safest place to keep your savings?

1 At home	2 With friends/family	3 With employer
4 At ASHI	5 At Bank	6 Savings are never safe

K2. What is the most profitable place to keep your savings?

1 At home	2 With friends/family	3 With employer
4 At ASHI	5 At Bank	6 Savings are never profitable

K3. How large is this interest rate you can get in your AHSI personal savings account?
_____ %

K4. How much money do you need to keep in your ASHI personal savings account to receive some interest?
_____ pesos

K5. How many people in your ASHI group save? _____

K6. How many people in your AHSI center save? _____

K7. In the last four weeks, how often were savings used for abonohan in your center?

1 Never	2 Once	3 Twice
4 Three times	5 Four times	6 More than four times
7 Don't know	8 DON'T READ: REFUSED	

K8. Do your group members know how much money you currently keep in your AHSI personal savings account?

1 They do not now	2 They have a vague idea	3 They know exactly
4 DON'T READ: REFUSED		

FL - FINANCIAL LITERACY

The next set of questions concern different financial concepts that you might be familiar with. Please take your time to think about each question.

FL1. Imagine that five brothers are given a gift of 1,000 PHP. If the brothers have to divide the money equally, how much does each one get? _____

FL2. Now, imagine that the five brothers have to wait for one year to get their part of the 1,000 PHP and inflation stays at 10%. In one year's time will they be able to buy:

1 More with their share of money than they could today	2 The same amount
3 Less than they could buy today	4 It depends on the types of things that they want to buy [DO NOT READ OUT THIS OPTION]
5 DON'T READ: EXPLAIN INFLATION	6 DON'T READ: REFUSED

[IF FL2="EXPLAIN INFLATION", READ "INFLATION MEASURES THE AVERAGE PRICE INCREASE OF COMMODITIES", THEN ASK THE QUESTION AGAIN]

FL3. Suppose you put 100 PHP into a savings account with a guaranteed interest rate of 2% per year. You don't make any further payments into this account and you don't withdraw any money. How much would be in the account at the end of the first year, once the interest payment is made? _____

FL4. Which of the following statements best describes the primary purpose of insurance products?

1 To accumulate savings	2 To protect against risks
3 To make payments or send money	4 Other
5 DON'T READ: DON'T KNOW	6 DON'T READ: REFUSED

FL5. How high is inflation currently in the Philippines? _____%

This is the end of the survey. We will now determine whether you receive additional payment for one of your paid game choices in the survey. Please roll this die. If it shows a "6", you will be paid for one of your choices. [ENTER NUMBER IN SCREEN AND FOLLOW INSTRUCTIONS]

[IF SELECTED FOR PAYMENT] We will now decide which one of the paid game questions we will pay you for. Please draw a number from this bag. [ENTER NUMBER IN SCREEN AND FOLLOW INSTRUCTIONS] Your [xth] question has been selected. The questions was [READ FROM SCREEN] and you selected [READ FROM SCREEN].

[IF SELECTED LOTTERY]: we will now play the lottery you have selected. I place [X] white chips in this bag, symbolizing the [COLOR1] balls and [Y] blue/red chips to symbolize the [COLOR2] balls. Please now draw a chip from the bag. [ENTER CHIP COLOR] you win an additional [X] pesos/ you lose [X] pesos.

[IF SELECTED LOAD QUESTION]: you will receive [x] pesos in load [now/in 2 weeks/in 4 weeks]. [FILL IN THE VOUCHER ACCORDINGLY, NOTE PHONE NUMBER IN TABLET]

[FOR EVERYONE] Your total earnings are [READ FROM TABLET], 300/500 pesos for your participation and [X] pesos from the games. Please sign here that you have received this amount [USE RECEIPT; EVERYONE NEEDS TO SIGN A RECEIPT, ALSO THOSE WHO DID NOT WIN ADDITIONAL MONEY].

We will now go back to the center meeting.

IF APPLICABLE: I will hand your envelope with your savings to Lisa who will give it to the DO once we have finished all interviews.

Please do not talk to any nanay about the survey before the end of the center meeting.

APPENDIX TO CHAPTER 2

B.1 ADDITIONAL TABLES AND FIGURES

Table B.1: Individual Repayment (Share Repaid) and Beliefs

	Share of tokens repaid (JL)			
	(1)	(2)	(3)	(4)
belief: partner repays	0.215*** (0.0587)		0.210*** (0.0574)	0.279 (0.282)
belief: partner punishes non-repayment		0.241** (0.104)	0.227** (0.0886)	0.269 (0.194)
belief: partner punishes repayment		0.0198 (0.0532)	0.0165 (0.0506)	0.0171 (0.0507)
belief: partner repays x punishes non-repayment				0.472** (0.181)
Mean of DV in JL	0.769	0.769	0.769	0.769
Observations	231	231	231	231
Adj. R ²	0.122	0.083	0.148	0.149
Fixed effects	session	session	session	session

Notes: OLS regressions with SE clustered on session level in parentheses. All belief variables are binary indicators. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

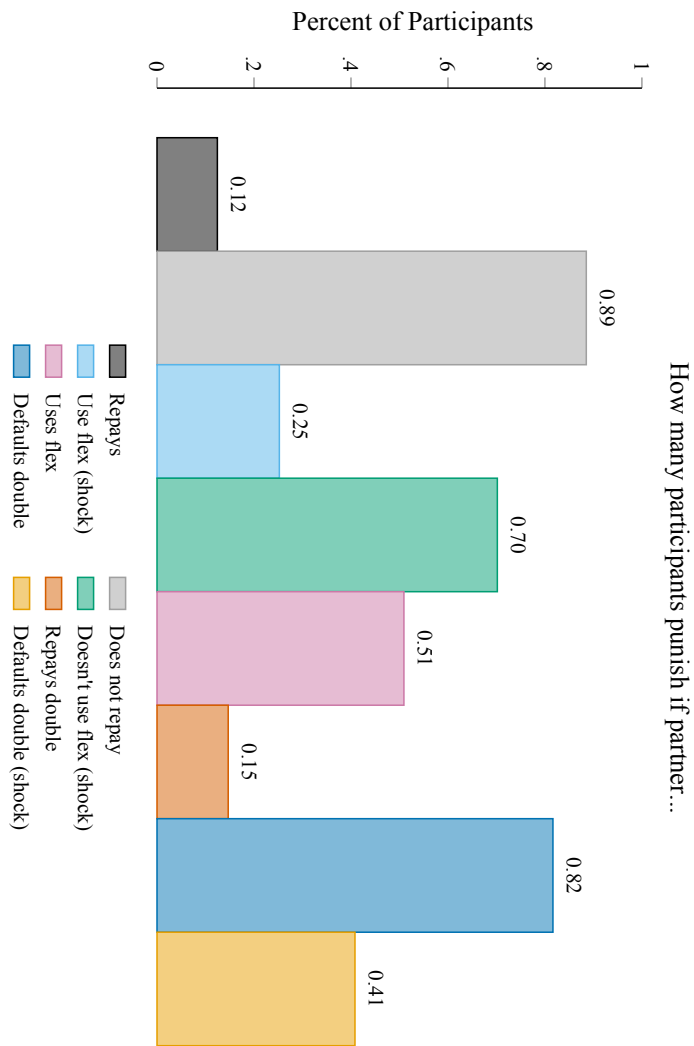


Figure B.1: Extensive Punishment in *IL-flex*

Table B.2: Heterogeneity in Repayment (Share Repaid)

	Share of tokens repaid					
	(1)	(2)	(3)	(4)	(5)	(6)
AGAP	0.0383 (0.0350)					
JL x AGAP		-0.0163 (0.0724)				
ILflex x AGAP		0.0720 (0.0526)				
JLflex x AGAP		0.127 (0.121)				
Present biased			-0.0246 (0.0261)	-0.0505 (0.0348)		
Present biased x JL				-0.0218 (0.0810)		
Present biased x ILflex				0.0886 (0.0528)		
Present biased x JLflex				0.0420 (0.0750)		
Risk averse					0.0182 (0.0244)	0.000357 (0.0354)
Risk averse x JL						-0.00281 (0.0543)
Risk averse x ILflex						0.0177 (0.0510)
Risk averse x JLflex						0.0842 (0.0558)
Mean of DV in IL	0.763	0.763	0.763	0.763	0.763	0.763
Observations	1,373	1,373	1,373	1,373	1,373	1,373
Adj. R ²	0.002	0.038	0.029	0.036	0.029	0.036
Fixed effects		session	session	session	session	session

Notes: OLS regressions with SE clustered on session level in parentheses. Columns (2), (4) and (6) include treatment dummies that are omitted for ease of presentation. *** p<0.01, ** p<0.05, * p<0.1.

Table B.3: Heterogeneity in Misuse of Flexibility in Period 1

	Misuse of Flexibility in Period 1			
	(1)	(2)	(3)	(4)
Present bias	0.105* (0.0548)	0.0760 (0.0648)		
Present bias x JLflex		0.0158 (0.0879)		
Risk aversion			0.0972 (0.0637)	0.0112 (0.0809)
Risk aversion x JLflex				0.121 (0.0998)
JLflex		-0.259*** (0.0931)		-0.306*** (0.107)
Mean of DV in ILflex	0.549	0.549	0.549	0.549
Observations	564	564	564	564
Adj. R ²	0.007	0.073	0.010	0.077

Notes: OLS regressions with SE clustered on session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.4: Randomization and Comprehension

	(1) Share Repaid in IL	(2) Repayment per period in IL	(3) Paid after default	(4) Paid after default	(5) Paid after default
JL-session	-0.000389 (0.0445)	0.0112 (0.0428)			
Period 2		-0.0525*** (0.0177)			
Period 3		-0.0393** (0.0192)			
JL-session x Period 2		-0.0165 (0.0297)			
JL-session x Period 3		-0.0181 (0.0277)			
JL			0.000501 (0.0496)	-0.00766 (0.0456)	-0.00766 (0.0591)
ILflex			-0.0394 (0.0280)	-0.0168 (0.0291)	-0.0138 (0.0367)
JLflex			-0.00146 (0.0569)	-0.0147 (0.0400)	-0.0124 (0.0515)
Mean of DV in IL	0.763	0.763	0.233	0.233	0.233
Observations	566	1,698	1,349	1,349	1,349
Adj. R ²	0.000	0.004	0.001	0.134	0.628
Fixed effects				session	individual

Notes: OLS regressions with SE clustered on session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.5: Inclusion of All Participants: Repayment and Flexibility

	(1)	(2)	(3)	(4)	(5)
	Share Repaid	Misuse in t1	Misuse in t2	Repay Misuse t2	Repay Misuse t3
IL	0.0195 (0.0281)				
ILflex	-0.103** (0.0347)				
ILflex		-0.220** (0.0865)	-0.218*** (0.0672)	0.0847 (0.0540)	0.0948 (0.0587)
Mean of DV in IL	0.763				
Mean of DV in ILflex		0.552	0.735	0.749	0.591
Observations	1,567	640	356	285	215
Adj. R ²	0.042	0.049	0.049	0.010	0.010
FE	session				

Notes: OLS regressions with SE clustered on session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.6: Inclusion of All Participants: Punishment in Case of a Shock

	Extensive		Intensive	
	(1) Flex	(2) No Flex	(3) Flex	(4) No Flex
JLflex	-0.223* (0.119)	0.223* (0.108)	-0.397** (0.165)	0.304* (0.159)
Mean of DV in JL	0.516	0.516	0.778	0.778
Observations	553	553	553	553
Adj. R ²	0.659	0.650	0.667	0.675
FE	individual	individual	individual	individual

Notes: OLS regressions with SE clustered on session level in parentheses.
 *** p<0.01, ** p<0.05, * p<0.1.

Table B.7: Choice Reversals: Share Repaid, Misuse and Repayment of Flexibility

	Share Repaid		Misuse of Flexibility		Repayment of Flexibility	
	(1) All	(2) All	(3) All	(4) All	(5) All	(6) All
Revision	-0.0529*** (0.0191)		0.152*** (0.0512)		-0.0684* (0.0400)	
JL		0.00291 (0.0250)				
ILflex		-0.0742** (0.336)				
JLflex		-0.0705 (0.0478)		-0.221*** (0.0628)		0.0499 (0.0558)
IL in revision		-0.0686** (0.0262)				
JL in revision		-0.0492 (0.0526)				
ILflex in revision		-0.0925* (0.0479)		0.103** (0.0425)		-0.0163 (0.0433)
JLflex in revision		-0.507*** (0.168)		-0.238*** (0.0472)		-0.335*** (0.0558)
Mean of DV in IL	0.763	0.763				
Mean of DV in ILflex			0.603	0.603	0.703	0.703
Observations	1,848	1,848	1,173	1,173	621	621
Adj. R ²	0.036	0.044	0.017	0.071	0.003	0.020
Fixed effects	session	session				

Notes: The treatment dummies indicate the decision in the main treatment, whereas the treatment "in revision" dummies indicate revised choices. OLS regressions with SE clustered on session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B.2 REPAYMENT CHOICES PER PERIOD

Table B.8 examines repayment choices in more detail. Column 1 shows repayment choices per period, pooling across all treatments.

As compared to Period 1, participants repay slightly less in Periods 2 and 3, but repayment remains above 70 percent. Column 2 compares repayment across treatments. The coefficient of *JL* reflects the non-parametric finding that liability does not influence average repayment choices. Reassuringly, this holds also true for the two flexibility treatments as all coefficients are very small and statistically indistinguishable from zero. Column 3 shows the full interaction of period and treatment dummies. The dynamics of slightly decreasing repayment in Periods 1 and 2 seems to be mainly driven by the *IL* treatment. For the other treatments, we do not find consistent dynamics across periods. Columns 4-6 replicate the first three columns but only using within-session variation. All coefficients are quantitatively similar, an additional indication for successful randomization.

Table B.8: Repayment Choices per Period

	Repayment in a given period					
	(1)	(2)	(3)	(4)	(5)	(6)
Period 2	-0.0268*		-0.0601***	-0.0267*		-0.0601***
	(0.0137)		(0.0146)	(0.0138)		(0.0147)
Period 3	-0.0856***		-0.0477***	-0.0850***		-0.0477***
	(0.0138)		(0.0139)	(0.0138)		(0.0139)
JL		0.00618	-0.00931		0.00639	-0.00910
		(0.0266)	(0.0268)		(0.0275)	(0.0297)
ILflex		0.00962	0.0534		0.00959	0.0533
		(0.0237)	(0.0331)		(0.0287)	(0.0361)
JLflex		-0.00481	-0.0332		-0.00364	-0.0331
		(0.0366)	(0.0452)		(0.0421)	(0.0517)
Period 2 x JL			0.0103			0.0103
			(0.0256)			(0.0257)
Period 2 x ILflex			0.0365			0.0366
			(0.0317)			(0.0318)
Period 2 x JLflex			0.125***			0.125***
			(0.0350)			(0.0351)
Period 3 x JL			0.0362*			0.0362*
			(0.0212)			(0.0213)
Period 3 x ILflex			-0.169***			-0.168***
			(0.0342)			(0.0344)
Period 3 x JLflex			-0.0456			-0.0423
			(0.0390)			(0.0364)
Mean of DV in t1	0.802		0.802	0.802		0.802
Mean of DV in IL		0.763	0.763		0.763	0.763
Observations	4,154	4,154	4,154	4,154	4,154	4,154
Adj. R ²	0.007	0.000	0.018	0.047	0.040	0.057
Fixed effects				session	session	session

Notes: OLS regressions with SE clustered on session level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.9: Choice Reversals: Repayment per Period

	Repayment in a given period				
	(1) All	(2) IL	(3) JL	(4) ILflex	(5) JLflex
Revision	-0.0453**	-0.0904*** (0.0296)	-0.0302 (0.0732)	0.0697** (0.0282)	-0.141 (0.196)
Period 2		-0.0601*** (0.0147)	-0.0498** (0.0231)	-0.0237 (0.0324)	0.0654* (0.0356)
Period 3		-0.0477*** (0.0140)	-0.0115 (0.0174)	-0.216*** (0.0319)	-0.0921** (0.0363)
Revision x Period 2		0.0401* (0.0225)	0.0290 (0.0277)	-0.124*** (0.0391)	-0.0321 (0.126)
Revision x Period 3		0.0211 (0.0211)	0.0323 (0.0471)	-0.0615 (0.0453)	-0.00792 (0.0874)
Mean of DV	0.765	0.799	0.789	0.852	0.765
Observations	5,818	2,601	927	1,436	854
Adj. R ²	0.039	0.071	0.051	0.087	0.100
Fixed effects	session	session	session	session	session

Notes: Mean of DV is the mean of the respectively omitted category. OLS regressions with SE clustered on session level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.3 DISCRETE CHOICE EXPERIMENTS

Participants were presented with eleven pairs of loans that each contained a loan A and a loan B. Loans varied across the following four dimensions:

1. the loan amount (₱ 5,000, ₱ 10,000, ₱ 15,000 or ₱ 20,000)
2. the interest rate (10 - 40 percent nominal on loan amount over 50 weeks)
3. the repayment schedule: this could be either regular repayment, a pass token, or a "repayment pause"
4. a loan administration fee (₱ 0, ₱ 500, or ₱ 1000)

We used a fractional factorial design of all possible combinations of the attribute levels and applied the D-efficiency criteria to pre-select a set of 32 hypothetical loan contracts for the comparison in our experiment. We verified the orthogonality of attribute level compositions ex-post. Based on the 32 contracts, we formed all possible binary comparisons of two contracts each. We eliminated all choices where one loan contract clearly dominated the other or that were too complex (i.e. where loan schemes differed in three or more attributes). The latter point is important to facilitate choices and ensure consistent decisions. We then selected 34 binary comparisons that we split into three blocks such that each participant only made eleven or 12 choices.

We analyze the choice between the loan contracts with a discrete choice model between alternatives and estimate expected utilities using a conditional logit model in reference to a loan contract that represents the ASHI general loan for new members: ₱ 5000, with zero administration fees, a regular repayment schedule, 50 weeks loan duration and an interest charge of ₱ 1000 (20 percent nominal interest rate). We use the following model:

$$\begin{aligned}
 U = & \beta_1 \text{interest} \\
 & + \delta_2 \text{amount}_{10000} + \delta_3 \text{amount}_{15000} + \delta_4 \text{amount}_{20000} \\
 & + \gamma_1 \text{schedule}_{\text{pass}} + \gamma_2 \text{schedule}_{\text{pause}} \\
 & + \lambda_1 \text{fee}_{500} + \lambda_2 \text{fee}_{1000} \\
 & + \text{constant}
 \end{aligned} \tag{1}$$

where *interest* is the only non-binary variable. With the results from (1) we estimate the willingness to pay:

$$\text{WTP}(\text{coefficient } X) = -\frac{\text{coefficient } X}{\beta_1} \quad (2)$$

The coefficients in Table B.11 show changes in utility with changes in the standard loan contract characterized by a loan amount of ₱ 5000, a regular repayment schedule, 20 percent interest (p.a.; ₱ 1000), loan duration of 50 weeks and no fees. As utility does not have an absolute interpretation, it can only be interpreted in relation to a reference state. Apart from the coefficient for the interest, all other coefficients indicate the change of utility due to a change of the attribute level in relation to the baseline alternative. For instance, increasing the loan amount from ₱ 5000 to ₱ 10000, raises the utility of that contract by 0.763. Table B.10 shows the corresponding WTP estimates in ₱.

Table B.10: Willingness to Pay for Contract Features

	Coeff	SE
Amount ₱ 20,000	2610.811***	489.3555
Amount ₱ 15,000	2225.76***	449.0226
Amount ₱ 10,000	1104.262***	372.8101
Pass	-189.84	437.8428
Pause	551.6475**	270.631
Fee ₱ 1,000	-2071.629***	571.0117
Fee ₱ 500	-914.5384*	526.6332

Notes: Baseline loan: ₱ 5000 loan amount, regular repayment schedule, no administrative fee. *** p<0.01, ** p<0.05, * p<0.1.

Table B.11: Choice of Credit

	(1)
Interest	-0.000691*** (9.80e-05)
Loan amount ₱ 20,000	1.804*** (0.467)
Loan amount ₱ 15,000	1.538*** (0.386)
Loan amount ₱ 10,000	0.763*** (0.287)
Schedule: Pass	-0.131 (0.294)
Schedule: Pause	0.381* (0.202)
Fee ₱ 1,000	-1.431*** (0.342)
Fee ₱ 500	-0.632* (0.340)
Constant	-0.632** (0.256)
Observations	5,270

Notes: Dependent variable: Choice of loan. Conditional logit maximum likelihood estimation, robust SE in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

B.4 DETAILS ON DECISION MAKING PROCEDURES AND THE ANALYSIS

B.4.1 *Elicitation of Choices*

FLEXIBILITY As with all other choices, we elicited decisions on the use of flexibility using the strategy method. First, subjects were asked whether they wanted to use their flexibility token in period 1 when there is no shock. If they decided to use it, they were subsequently asked about their repayment decisions in period 2 and 3. If they chose not to use it, they had the choice to use their flexibility token in period 2: first, when there is no shock and second, in case of a shock in period 2. Last, all participants decided whether to use their flexibility token in period 1 in case of a shock. This procedure lays out nearly the complete strategy on flexibility use.

PUNISHMENT In total, we elicited punishment decisions in *JL-flex* for eight different choices of the partner: repayment, non-repayment, flexibility use in case of a shock, misuse of flexibility, double repayment (repaying flexibility), non-repayment of flexibility, not using flexibility in case of a shock and non-repayment of flexibility due to a shock.

B.4.2 *Share of Tokens Repaid vs. Repayment per Period*

Comparing repayment choices across periods and all treatments comes with two difficulties. First, in the two flexibility treatments, we elicit repayment choices conditional on shock realizations in the first two periods as this is relevant for the use of flexibility. In contrast to *IL* and *JL*, active choices (regarding the use of flexibility) can still be made in case of a shock. We thus have more choices for different scenarios than in *IL* and *JL*. Hence, we analyze the choices that are the most comparable across all four treatments in the main analysis, repayment choices absent a shock. Second, when classifying the use of flexibility as repaying in a given period, the comparison of the flexibility treatments with *IL* and *JL* entails decisions both regarding repayment and the use of flexibility.

This might lead to mechanical effects such as higher repayment in period 1 although the strategy of the participant in a flex treatment is actually defaulting (but they use flexibility in the first period to ensure early consumption).

The share of tokens repaid circumvents these problems. The main difference to repayment per period is that in the latter, the use of flexibility is counted as repayment in the period it is used, independent of whether flexibility is actually repaid.

B.5 DESCRIPTION OF VARIABLES

Repayment

- *Share repaid*: tokens repaid divided by tokens to be repaid over all periods (three). The variable ranges from zero to one.
- *Repayment choice per period*: binary indicator taking the value one if the repayment obligation has been met in a given period (either by repaying or using flexibility).

Flexibility

- *Use of flexibility*: binary indicator taking the value one if flexibility has been used in a given scenario (e.g. period 2 without shock). Four indicators exist, one for each scenario.
- *Repayment of flexibility*: binary indicator that takes the value one if the double repayment obligation has been met in a given scenario (e.g. repay the double installment in period 3 after having misused it in period 2). Four indicators exist, one for each scenario.
- *Double default*: indicator variable taking the value one if the double repayment of flexibility has not been made. The simulation of this variable includes both defaults due to a shock and due to non-repayment.
 - *due to a shock*: indicator variable taking the value one if the double repayment has not been made due to a simulated shock.

- *due to non-repayment*: indicator variable taking the value one if the double repayment has not been made by choice.

Punishment

Punishment has been elicited in terms of punishment points in the two joint-liability treatments. Up to two punishment points can be used. At the *extensive* margin, we analyze how many participants use at least one point; at the *intensive* margin, we examine how many punishment points are allocated in a given scenario. All extensive punishment variables are thus binary indicators, whereas intensive variables can take the value zero, one or two.

Punish if partner chooses...

- *Repay*: allocation of punishment points for simple repayment. (*JL* and *JL-flex*)
- *Don't repay (no shock)*: allocation of punishment points for not repaying (the single installment) and consuming more instead. Here, an active choice against repayment was made by the partner. (*JL* and *JL-flex*)
- *Don't repay (shock)*: allocation of punishment points for not repaying due to a shock. Here, the partner had no choice. (*JL*)
- *Flex (shock)*: allocation of punishment points for using flexibility as self-insurance in case of a shock. (*JL-flex*)
- *No flex (shock)*: allocation of punishment points for not using flexibility as self-insurance in case of a shock. (*JL-flex*)
- *Flex (no shock)*: allocation of punishment points for misusing flexibility. (*JL-flex*)
- *Repay double*: allocation of punishment points for repaying the double installment after having used flexibility. (*JL-flex*)
- *Default double (no shock)*: allocation of punishment points for choosing not to repay the double installment after having used flexibility. (*JL-flex*)
- *Default double (shock)*: allocation of punishment points for not repaying the double installment due to a shock. (*JL-flex*)

Other data elicited during the sessions

- *Present biased*: indicator variable taking the value one if the choices between money today and in one month are more impatient than the choices between money in one and in two months. For instructions of the elicitation, see Section B.7.7.
- *Risk averse*: indicator variable taking the value one if participant's risk choice is less risky than the median. For instructions of the elicitation, see Section B.7.7.
- *JL-session*: indicator variable taking the value one if the session contained the treatments *IL*, *JL* and *JL-flex* (as compared to *IL* and *IL-flex*).

Administrative data

- *AGAP*: indicator variable taking the value one if borrowers have an individual (agricultural) loan.
- *Education of female head: no secondary graduate*: binary indicator taking the value one if participants have not completed secondary education.
- *Electricity*: binary indicator taking the value one if borrowers live in a house with access to the electricity grid.
- *House size*: takes values zero (small), one (medium) or two (large).
- *House strength*: takes values zero (weak), one (medium) or two (strong).
- *Landline*: binary indicator taking the value one if the house is connected to the telephone network.
- *Loam Amount*: Size of the current loan.
- *Main income: Enterprise*: binary indicator taking the value one if the main household income source is the own business.
- *Main income: Farming*: binary indicator taking the value one if the main household income source is farming.
- *Monthly income below ₱ 5000*: binary indicator taking the value one if the monthly household income is below ₱ 5000.

- *Probability of living below NPL*: Probability of living below the national poverty line, based on the Progress out of Poverty Indicator.
- *Water*: binary indicator taking the value one if the house has running water.

B.6 EXPERIMENTAL SETUP



Figure B.2: Consumption Table



Figure B.3: Study Area

B.7 EXPERIMENTAL INSTRUCTIONS

B.7.1 *Introduction*

Good day,

we are a team of independent researchers and we study the possibility for improving the design of loans in the Philippines. In today's meeting, you will take a set of decisions because we are very interested in your opinions and decisions. For all these the decisions, there is no "right" or "wrong". You can take your decisions freely and your decisions are very valuable for us.

We will provide you a fee for participation of ₱70. With the decisions that you make, you can earn more money in the games and receive small gifts. Some of your decisions may also affect your participation fee, but you will at least get ₱30. What you earn depends on the decisions you have made and luck. It also may depend on the decisions other participants have made.

All of your responses will be kept confidential. Only the researchers involved in this study and those responsible for research oversight will have access to all the information you provide. Your responses will be numbered and the code linking your number with your name will be stored in a separate locked file cabinet. You receive a participant ID to protect your privacy.

If you have any questions, please ask any one of the instructors. We kindly ask you not to talk to your neighbours. If you do have questions, please talk directly to one of the instructors. Also, in order not to disturb any other participant, please do not share your decisions with anybody else, unless explicitly asked to by one of the instructors.

By agreeing to participate, you consent that all information which you voluntarily share may be used purely for research purposes by the research team or other researchers. This includes financial information you provide and information that ASHI shares with us. All data is confidential and none of the information you provide will be used in connection with your name. By agreeing to participate, you also agree that the anonymized data set may be provided to other researchers. Your decision to participate does not affect your ASHI membership in any way, because we do not work for ASHI. We are independent researchers.

The session today lasts for two to three hours. We will calculate your earnings at the end of the session. Your earnings can be influenced by your decisions and decisions made by other participants. If you cannot stay the full time, please let us know now. We will not be able to pay you if you do not stay until the end of the session.

In order to understand the game fully, we request that you do not use your phones. We really appreciate that you are here today and we would already like to thank you very much!

General Instructions: Loan Repayment Game

We start with a basic decision making game. Some things may not be familiar, so please listen very carefully to the explanations we will provide. Some things may seem familiar, but this may mislead you in the game. All the information that you need to make the decisions will be provided in the explanations. We will clearly lay out which actions you can take in this game and which not. These may be different from the actions you can take in reality, so please remember at all times that we play a game now and that we have to stick to the rules of the game. We will have a quiz at the end of the explanations, so please pay close attention.

B.7.1.1 Explanation of the Game

We will play different variants of the game. For each variant that we play, we will place one token in a bag. At the end of the meeting, we do a lucky draw and select one of these tokens and from there, we will calculate how much you will receive.

[INSTRUCTOR: SHOW THE TOKENS THAT WE HAVE FOR EACH TREATMENT AND THE BAG IN WHICH YOU WILL PLACE THEM]

Each variant of the game will last for three terms. In each term you have to take decisions. Each decision that you take is equally important as each decision may determine how much money you will earn at the end of the game.

GENERAL SETUP - INDIVIDUAL-LIABILITY & NO FLEXIBILITY (PASS TOKEN) In this game, you got a loan from a bank. You have invested this loan and you will receive an income of two green tokens from your investment/business in each of the three terms. You can use these two green tokens of income to repay your loan installment and for your own consumption.

In each term, the repayment due is one green token.

You can also use your green tokens to go shopping at the consumption table. There are different items in the table worth different amounts. The more green tokens you have, the more items you can pick from the consumption table. You have already seen the shopping catalog that presents all the items and their prices available at the shopping table.

[INSTRUCTOR SHOW LAMINATED CATALOGUE]

[USE POSTER "REPAYMENT AND CONSUMPTION CHOICE" (See Figure B.4)]

You have to simultaneously decide how you want to split the income of two green tokens between repaying your loan and consuming from the consumption table. The tokens used for repayment are marked on the sheets with the bundle of cash. The tokens used for consumption are marked on the sheets with a cupcake. You can either repay one token and consume one token, to go shopping at the consumption table; or you do not repay your loan repayment and consume two tokens, to go shopping at the consumption table. You can

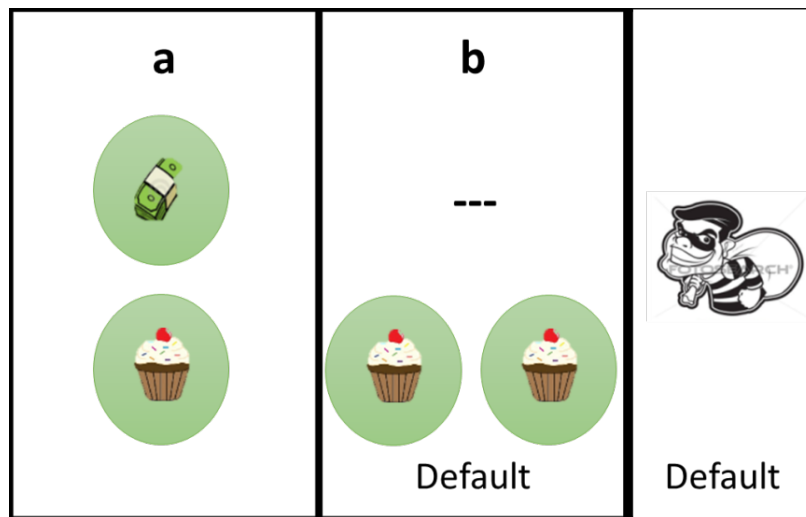


Figure B.4: Repayment and Consumption Choice

take home your shopping immediately after the game.

[USE POSTER IL]

You have to take this decision in every term. That is, in each of the three terms of the game, you get the income of two tokens, you have a repayment due of one token, and you have to decide how to split up your two tokens between repayment and consumption.

The value of a consumption token changes in each term. In the 1st term, each consumption token is worth ₱ 40. In the 2nd term, each consumption token is worth ₱ 30 , and in the 3rd term, each consumption token is worth ₱ 20.

[USE POSTER "VALUE CONSUMPTION TOKEN"]

If you repay your loan in every term, you will receive a voucher of ₱ 100. In one month's time, one of the instructors will come to visit your center again and distribute the ₱ 100 to everyone who received this voucher. If you do not repay in at least one term, you will not receive the voucher of ₱ 100 . This implies that if you do not repay in one term, it is your best strategy to also not repay in the following terms.

In each term, there is the possibility that you are robbed by a thief. The thief will come and steal all the money that you have earned in this term. It will not affect the income that you will receive in the next term and it will also not affect your prior consumption decisions. If the thief steals your income, you do not have any money left in this term. That is, you can neither repay your loan nor consume anything in this term. Only when the thief does not rob you, you can decide how to split your income between repayment and consumption.

In order to determine whether the thief has robbed you, we will place four cards in a bag. On three cards, there is no thief, on one card there is a thief. You take a draw from this bag, if you draw the thief, you will be robbed, if you don't draw the thief, you will be spared.

If the thief comes, how many green tokens of income do you have left?

[ASK THE PARTICIPANTS; STATE CORRECT ANSWER]

So if you do not have any income, can you repay your loan?

[ASK THE PARTICIPANTS; STATE CORRECT ANSWER]

How many tokens can you consume?

[ASK THE PARTICIPANTS; STATE CORRECT ANSWER]

Only you know whether you have been robbed by a thief. The bank will never know whether you have been robbed or not and you do not have any possibility to explain your situation to the bank.

Do you have any questions?

EXAMPLE A: REPAY IN ALL PERIODS

Let me give you examples on your total earnings in the game depending on your repayment decisions.

[USE POSTER "DECISION SHEET IL" (See Figure B.5)

GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION THAT YOU ARE DEMONSTRATING. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

NO THIEF REALIZATION

Let's consider a situation where you decide to repay your loan installment in all three terms. That is, you repay one token and you consume one token, that is you go shopping at the consumption table for one token. In total, after three terms, you can go shopping at the consumption table for three tokens. Remember that the token loses value over time. Remember that we only look at the situation now where the thief did not rob you.





In addition, since you did not default in any term, you get the voucher of ₱ 100 to use in one month's time. In total, you get all your shopping items you got for 3 tokens + plus the voucher of ₱ 100.





[INSTRUCTOR:





DEMONSTRATE THE EXAMPLE AS YOU DO IN THE PRACTICE ROUND WHEN CALCULATING THE PAYMENTS:

1. REPAYMENT CHOICE

IL- no flex decision sheet 1/2 Participant ID: _____

T1	
<p style="text-align: center;">a</p> <p>Repay: </p> <p>Consume: </p>	<p style="text-align: center;">b</p> <p>Repay: -----</p> <p>Consume:  </p>

T2	
<p style="text-align: center;">a</p> <p>Repay: </p> <p>Consume: </p>	<p style="text-align: center;">b</p> <p>Repay: -----</p> <p>Consume:  </p>

T3	
<p style="text-align: center;">a</p> <p>Repay: </p> <p>Consume: </p>	<p style="text-align: center;">b</p> <p>Repay: -----</p> <p>Consume:  </p>

 +
  +
  + **100 Pesos**

Figure B.5: Decision Sheet IL: Example A

SHOW THE CHOICE BY PLACING BOTH GREEN TOKENS ON "REPAY" IN T₁, T₂, AND T₃

2. CALCULATE THE PAYOFF WITHOUT A THIEF

- 1. TERM:
 - * "You have two tokens "1" worth ₱ 40"
 - * "You decide to repay" ⇒ TAKE AWAY REPAYMENT TOKEN
- 2. TERM:
 - * "You have two tokens "2" worth ₱ 30"
 - * "You decide to repay" ⇒ TAKE AWAY REPAYMENT TOKEN
- 3. TERM:
 - * "You have two tokens "3" worth ₱ 20"
 - * "You decide to repay" ⇒ TAKE AWAY REPAYMENT TOKEN
- "In total, you have T₁+T₂+T₃ consumption tokens=₱ 90 for consumption"
- "Will you get the ₱ 100?" ⇒ ASK AUDIENCE
- "Yes, you repay in every period, so you get the voucher of ₱ 100." ⇒ PUT ₱ 100 SIGN ON THE POSTER]

DRAW THIEF REALIZATION

Now let's draw the thief to see what happens.

[DEMONSTRATE THE EXAMPLE AS YOU DO IN THE PRACTICE ROUND WHEN CALCULATING THE PAYMENTS:

1. REPAYMENT CHOICE

- SHOW THE CHOICE BY PLACING BOTH GREEN TOKENS ON "REPAY" IN T₁, T₂, AND T₃

2. DRAW THE THIEF FOR EACH TERM. BEFORE CONTINUING, CALCULATE PAYOFF IN THIS TERM

- DRAW THIEF FOR 1ST TERM
- TAKE AWAY THE TOKENS REPAID OR STOLEN FOR 1ST TERM
- DRAW THIEF FOR 2ND TERM
- TAKE AWAY THE TOKENS REPAID OR STOLEN FOR 2ND TERM
- DRAW THIEF FOR 3RD TERM
- TAKE AWAY THE TOKENS REPAID OR STOLEN FOR 3RD TERM

3. CALCULATE THE PAYOFF AND WHETHER THE ₱ 100 WILL BE RECEIVED

⇒ MAKE THIS AN INTERACTIVE EXERCISE]

EXAMPLE B: NOT REPAY IN ANY TERM

[USE POSTER "DECISION SHEET IL" (See Figure B.6) GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION THAT YOU ARE DEMONSTRATING. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS

WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

NO THIEF REALIZATION

Let's consider a situation where you decide not to repay in all three terms. In each term, you do not repay your loan, and you consume two tokens, that is go shopping at the consumption table for two tokens and default with the bank. As soon as you do not repay in the 1st term, it is your best strategy strategy to also not repay in the following terms, since you have lost the voucher of ₱ 100 anyway and there are no other consequences to not repaying in this game.

In total, after three terms, you can go shopping at the consumption table for two tokens in each term if this variant of the game is chosen for your payment. Remember that we only look at the situation now where the thief did not rob you.

Since you default in all terms, you will not get the voucher of ₱ 100. In total, you get all your shopping items you got for two tokens in each term.

[DEMONSTRATE THE EXAMPLE AS YOU DO IN THE PRACTICE ROUND WHEN CALCULATING THE PAYMENTS:

1. REPAYMENT CHOICE

- SHOW THE CHOICE BY PLACING BOTH GREEN TOKENS ON "CONSUME 2" IN T₁, T₂, AND T₃

2. CALCULATE THE PAYOFF WITHOUT A THIEF





- 1. TERM:
 - * "You have two tokens "1" worth ₱ 40"
 - * "You decide to consume two tokens"
- 2. TERM:
 - * "You have two tokens "2" worth ₱ 30"
 - * "You decide to consume two tokens"
- 3. TERM:
 - * "You have two tokens "3" worth ₱ 20"
 - * "You decide to consume two tokens"
- "In total, you have $2T_1+2T_2+2T_3$ consumption tokens= ₱ 180 for consumption"
- "Will you get the ₱ 100?" ⇒ ASK AUDIENCE
- "No, you did not repay in every period, so you do not get the voucher of ₱ 100."





DRAW THIEF REALIZATION

Now let's draw the thief to see what happens.

1. REPAYMENT CHOICE

IL- no flex decision sheet 1/2 Participant ID: _____

T1	
a Repay:  Consume: 	b Repay: ----- Consume:  

T2	
a Repay:  Consume: 	b Repay: ----- Consume:  





T3	
a Repay:  Consume: 	b Repay: ----- Consume:  



Figure B.6: Decision Sheet IL: Example B

- SHOW THE CHOICE BY PLACING BOTH GREEN TOKENS ON "CONSUME 2" IN T₁, T₂, AND T₃
2. DRAW THE THIEF FOR EACH TERM. BEFORE CONTINUING, CALCULATE PAYOFF IN THIS TERM
 - DRAW THIEF FOR 1ST TERM
 - TAKE AWAY THE TOKENS REPAID OR STOLEN FOR 1ST TERM
 - DRAW THIEF FOR 2ND TERM
 - TAKE AWAY THE TOKENS REPAID OR STOLEN FOR 2ND TERM
 - DRAW THIEF FOR 3RD TERM
 - TAKE AWAY THE TOKENS REPAID OR STOLEN FOR 3RD TERM
 3. CALCULATE THE PAYOFF AND WHETHER THE ₱ 100 WILL BE RECEIVED
⇒ MAKE THIS AN INTERACTIVE EXERCISE]

Do you have any questions?

Let's summarize the game: you get a loan that gives you a regular income of two green tokens in each of the three terms. In each term, you may get robbed by a thief who steals all your income.

If you are spared by the thief, you have to decide how you want to spend your two tokens of income: repay your loan and consume items from the consumption table. If you repay your loan in all three terms, you get a voucher of ₱ 100 that you can cash in one month after the game. If you do not repay in any of the three terms, that is if you default in any term, you do not get ₱ 100.

[INSTRUCTOR SHOW VOUCHER]

If you are robbed by the thief, you cannot repay your loan installment, that is you default in this term.

B.7.1.2 Test questions - General

Now, we will ask you a few test questions. It is very important that you understand the game we have just presented, so we would like to test your understanding in this quiz. Please do not talk to your neighbors now and only tell the answer to the instructor or note it yourself on the sheet. Are you ready to take the quiz?

For all the answers, please encircle your answer choice:

1. When you consume two tokens in this term, can you repay your loan? - Yes/ No [NO]
2. You do not repay in one term: Do you get voucher of ₱ 100? - Yes/No? [NO]
3. You are robbed by the thief: Do you get the voucher of ₱ 100? - Yes/No? [NO]
4. How much is one token for consumption at the consumption table worth in the 2nd term? - ₱ 50 / 30 / 20 [₱ 30]

[INSTRUCTION TEST QUESTIONS:

- ⇒ ONLY ASK TEST QUESTIONS ONCE!
- ⇒ CHECK WHETHER MOST PARTICIPANTS HAVE UNDERSTOOD THE EXPLANATIONS AND ANSWERED THE TEST QUESTIONS CORRECTLY
- ⇒ IF TOO MANY PARTICIPANTS DID NOT ANSWER THE TEST QUESTION CORRECTLY, EXPLAIN THE POSTER ¹⁰⁰ AGAIN

HOW TO IDENTIFY "TOO MANY":

- ⇒ LEAD INSTRUCTOR: ASK INSTRUCTORS IF WE NEED TO REPEAT EXPLANATIONS.
- ⇒ INSTRUCTORS; RAISE YOUR HAND IF MORE THAN 2 PEOPLE IN YOUR GROUP ANSWERED 2 OR MORE QUESTIONS WRONGLY
- ⇒ IF MORE THAN 2 INSTRUCTORS RAISE THEIR HANDS; REPEAT THE EXPLANATION]

B.7.1.3 *Practice Term - General*

It is really important that you understand the consequences of your decisions very well because at the end you will earn real money based on the decisions that you have made. So before you will make the real decisions that count for calculating your earnings we will have a practice term.

[USE DECISION SHEET IL- no pass Decision 1/2, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET]

Each term you have to decide between two options:

Option A: repay one token and consume one token; and Option B: repay zero tokens and consume two tokens.

B.7.1.4 *Repayment Decision in Term T₁, T₂, T₃*

Decision term 1

Please take your repayment and consumption decision for the 1st term when you are not robbed by the thief. You can either

A) Repay one token and consume one token

or

B) Not repay and consume two tokens.

Please indicate your choice by distributing your two green tokens between option A and B

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PRACTICE PERIOD FOR TERM 1]

Decision term 2

Please take your repayment and consumption decision for the 2nd term when you are not robbed by the thief so far.

Please indicate your choice by distributing your two green tokens between Option A and Option B.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PRACTICE PERIOD FOR TERM 2]

Decision term 3

Please take your repayment and consumption decision for the 3rd term when you are not robbed by the thief so far.

Please indicate your choice by distributing your two green tokens between Option A and Option B.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PRACTICE PERIOD FOR TERM 3; LET SHE DRAW WHETHER SHE WAS ROBBED BY A THIEF OR NOT; CALCULATE CONSUMPTION ITEMS AND ₱ 100 FOR EACH PARTICIPANT AND STATE THEM TO PARTICIPANTS. NOTE RESULT ON TEST ROUND DECISION SHEET. CONTINUE WITH SECOND PARTICIPANT IN YOUR GROUP; THEN THIRD AND FOURTH PARTICIPANT; USE SHEET "PRACTICE TERM" TO NOTE ALL DECISIONS AND REALIZATIONS OF SHOCK VS. NO SHOCK]

You have now played the game one time. We will practice the game for two more rounds. None of your decision will determine your pay-off. We will tell you when we play the game for real money. You can use these terms to test how changes in your decision affect your earnings. You will also better understand how often a thief will rob you.

Practice Rounds 2 and 3

[RE-START PRACTICE PERIOD HERE FOR PRACTICE ROUND 3]

Each term you have to decide between two options:

Option A: repay one token and consume one token; and Option B: repay zero tokens and consume two tokens.

Please indicate your choice by distributing your two green tokens between option A and B.

Please take your repayment and consumption decision for the 1st, 2nd and 3rd term when you are not robbed by the thief. A) Repay one token and consume one token or B) Not repay and consume two tokens.

[REPEAT PRACTICE ROUND ONE MORE TIME (3 ROUNDS IN TOTAL); GO TO "RESTART PRACTICE PERIOD"]

Now that you have finished the practice term, you have a good understanding of the consequences of your decisions.

Do you have any questions?

Treatment Games

[NOTE: WE WILL EITHER PLAY IL-NOFLEX + IL-FLEX (TYPE 1) OR IL-NOFLEX + JL + JL-FLEX (TYPE 2). CHECK THE SCHEDULE FOR EACH SESSION ON WHICH TYPE IS PLAYED IN THIS SESSION]

We will now play different variants of the game. For each variant that we will play, we place one token in this bag.

[SHOW BAG AND TOKEN]

At the end of the session, we will draw one token with closed eyes. Your earnings in this game depend on the decisions made in this variant. All decisions that you make have the same chance of determining your final earnings. It is very important that you take all the decisions in this session. Otherwise we will not be able to pay your earnings.

B.7.2 *Individual-Liability (FOR TYPE 1 AND TYPE 2)*

B.7.2.1 *Explanation IL*

[USE POSTER 2) IL-NO FLEX]

In this variant of the game, you get an individual loan that gives you a regular income of two green tokens in all three terms. In each term, you may get robbed by a thief who steals all your income.

If you are spared by the thief, you have to decide how you want to spend your two tokens of income.

You have to simultaneously decide how you want to split up the income of two green tokens on repaying your loan and on consuming from the consumption table. You can either repay one token and consume one token, to go shopping at the consumption table; or you do not repay your loan repayment and consume two tokens, to go shopping at the consumption table. You can take home your shopping immediately after the game. Remember that the token loses value over time.

If you are robbed by the thief, you cannot repay your loan installment, that is you default in this term. The bank does not care why you defaulted, because she cannot observe this.

If you repay your loan in all three terms, you get the voucher of ₱ 100 after the game. If you do not repay in any of the three terms, that is if you default in any term, you do not get ₱ 100. There are no other consequences of not repaying in this game.

B.7.2.2 *Decisions IL*

Now we will play the game for real. All decisions will determine your payment when this variant is selected in the lucky draw.

REPAYMENT DECISION IN TERM T₁,T₂,T₃

[USE DECISION SHEET IL- no flex decision sheet 1/1, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS.]

Please take your repayment and consumption decision for the 1st, 2nd and 3rd term when you are not robbed by the thief.

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM T₁, T₂, AND T₃]

REPAYMENT DECISION AFTER THIEF

[USE DECISION SHEET IL- no pass decision 2/2, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS.]

Since you lose the voucher of ₱ 100 as soon as you are robbed by the thief in one term, and there are no other consequences of not repaying your loan in this game, you may want to change your decision in the 2nd term when you have been robbed by a thief in the 1st term.

Please take your repayment and consumption decision for the 2nd term when you have been robbed by the thief in the 1st period.

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR "AFTER THIEF"]

B.7.3 *Joint-Liability (FOR TYPE 2 ONLY)*

B.7.3.1 *Explanation JL*

In this variant of the game, you will be paired with one other participant. Together, you will form a team. You both play the same game and you and your partner will make the decisions at the same time. You do not know who will be your partner; you only know it will be somebody from today's meeting. Your partner will also not know who you are; she just knows that her partner is somebody from today's meeting.

[USE POSTER JL]

You and your partner get a loan that gives you both a regular income of two green tokens in all three terms. In each term, you, your partner or both of you may get robbed by a thief who steals all your income.

If you are spared by the thief, you and your partner have to decide how you want to split your two tokens of income:

You can either repay one token and consume one token, to go shopping at the consumption table; or you do not repay your loan repayment and consume two tokens, to go shopping at the consumption table. You can take home your shopping immediately after the game. Remember that the token loses value over time.

Your team is jointly responsible for the two loans you have.

If one of the team partners does not repay her loan, the joint responsibility in the group will be applied automatically. This means, if you decide to repay one token and consume one token, you will only consume one token if you do not have to repay the loan for your partner. In the case you repay your loan, you will also repay the loan of your partner if she does not repay. In the case that you do not repay, and your partner repays, she will cover your loan installment. Your team loan is repaid in both cases. If you do not repay and your partner does not repay her loan, your team will default on the loan.

As long as the team loan is repaid in every term, you both will get the voucher of ₱ 100 after the game. As soon as your team loan is not repaid in one term, you both will not receive the ₱ 100. There are no other consequences to team default in this game. This implies that if your team defaults in one term, it is your best strategy to also not repay in the following terms.

If you or your partner or both of you are robbed by the thief, you cannot repay your loan installment. In this case you default in this term. The bank cannot observe this. Also, the bank does not know who repaid and who did not; she can only observe the total repayment of the team. As long as neither or only one of you is robbed by the thief, the joint responsibility will be applied automatically if you chose to repay. In this case you will not default.

B.7.3.2 *Examples JL*

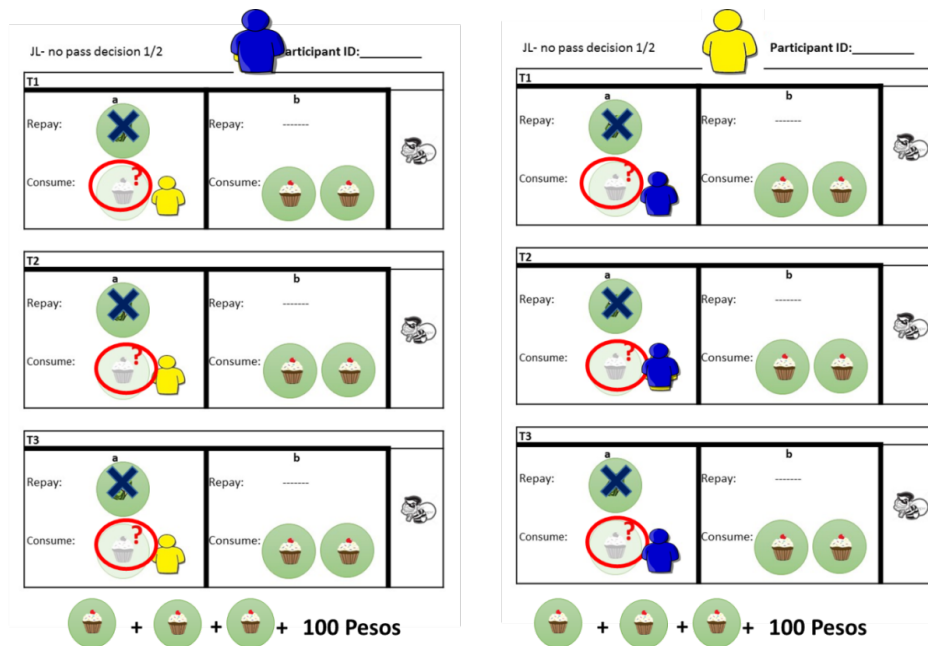
[INSTRUCTOR: USE THE DECISION SHEETS AND THE LAMINATED GREEN TOKENS FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE]

Now, your repayment, consumption and default does not only depend on your choices but also on the choices of your partner. I will now give you examples of different situations that can arise. For now, we only consider the cases where the thief does not rob you, because these are the cases when you can take a decision.

[USE POSTER "EXAMPLE A - JL" (See Figure B.7)
GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION THAT YOU ARE DEMONSTRATING – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

EXAMPLE A: BOTH REPAY IN ALL PERIODS

[INSTRUCTOR: GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]



JL

Figure B.7: Example A - JL

NO THIEF REALIZATION

Let's consider a situation where you decide to repay the loan installment in all three terms. That is, in each term you repay one token and consume one token if you do not have to pay for your partner. In total, in each of the three terms, you can go shopping at the consumption table for one token. Remember that we only look at the situation now where the thief did not rob you.

Let's assume your partner also decides to repay the loan installment in all three terms. That is, in each term she repays one token and consumes one token. Your partner can go shopping at the consumption table in each of the three terms for one token. Remember that we only look at the situation now where the thief did not rob her.

In addition, since your team did not default in any term, you both get the voucher of ₱100. In total, you and your partner get all your shopping items you got for three tokens each plus the voucher of ₱100.

DRAW THIEF REALIZATION

Now let's draw the thief to see what happens.

- 1) DRAW THE THIEF FOR THE PLAYER (BLUE)
- 2) DRAW THE THIEF FOR THE PARTNER (YELLOW)
- 3) DETERMINE WHO IS REPAYING; WHO IS NOT REPAYING; WHO PAYS ABONO FOR THE OTHER
 - TAKE AWAY A TOKEN FOR INDIVIDUAL REPAYMENT
 - TAKE AWAY A TOKEN FOR ABONO REPAYMENT FOR THE GROUP PARTNER; DEPENDING ON THIEF REALIZATION
- 4) CALCULATE PAYOFFS OF BOTH PLAYERS
 - CONSUMPTION TOKENS AND ₱100 VOUCHER

EXAMPLE B: REPAY IN ALL PERIODS, PARTNER DOES NOT REPAY

[USE POSTER EXAMPLE B - JL (See Figure B.8): GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

NO THIEF REALIZATION

Let's consider a situation where you decide to repay the loan installment in all three terms. That is, in each term you repay one token and consume one token if you do not have to pay for your partner. In total, after three terms, you can go shopping at the consumption table for one token in each of the three terms. Remember that we only look at the situation now where the thief did not rob you.

Let's assume your partner decides not to repay the loan installment in all three terms. That is, in each term she repays zero token and consumes two tokens. Your partner can go shopping at the consumption table for two tokens each term. Remember that we only look at the situation now where the thief did not rob her.

Since you choose to repay, your joint responsibility is automatically enforced and you have to repay the team loan for your partner who does not repay. Your team does not default in any term. The bank does not know who repaid and who did not; she can only observe the total repayment of the team. So you

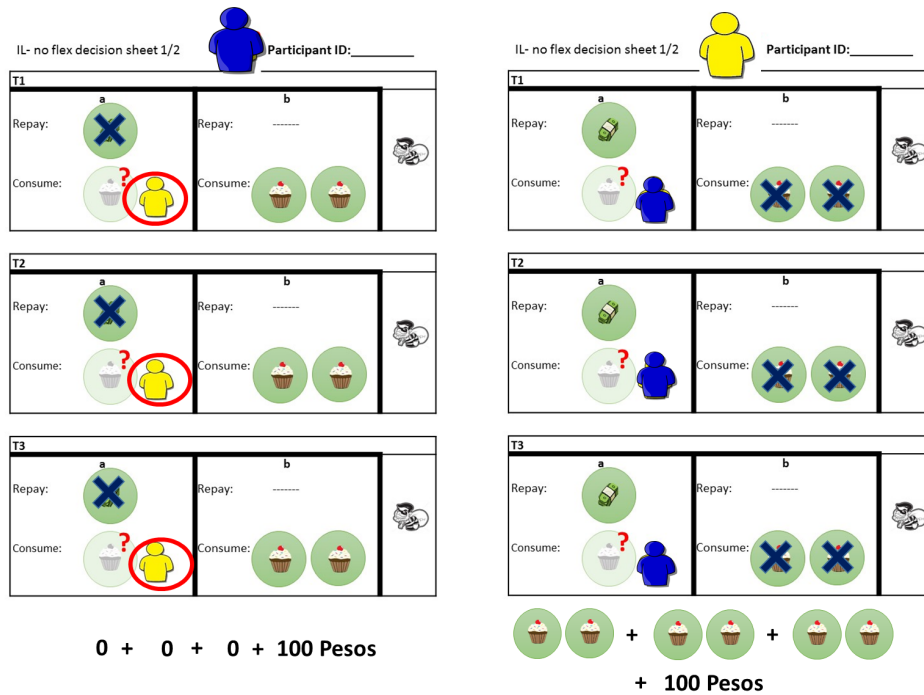


Figure B.8: Example B - JL

both get the voucher of ₱ 100. In total, you get zero consumption tokens plus the ₱ 100. Your partner gets 6 consumption tokens plus the ₱ 100.

DRAW THIEF REALIZATION

Now let's draw the thief to see what happens.

[USE POSTER EXAMPLE JL

- 1) DRAW THE THIEF FOR THE PLAYER (BLUE)
- 2) DRAW THE THIEF FOR THE PARTNER (YELLOW)
- 3) DETERMINE WHO IS REPAYING; WHO IS NOT REPAYING; WHO PAYS ABONO FOR THE OTHER
 - TAKE AWAY A TOKEN FOR INDIVIDUAL REPAYMENT
 - TAKE AWAY A TOKEN FOR ABONO REPAYMENT FOR THE GROUP PARTNER; DEPENDING ON THIEF REALIZATION
- 4) CALCULATE PAYOFFS OF BOTH PLAYERS
 - CONSUMPTION TOKENS AND ₱ 100 VOUCHER]

Do you have any questions?

Let's summarize the game: You and your partner are jointly liable for your loans. Each of you has a repayment obligation of one token per term. If both partners repay, you, individually, have one token left for consumption in that term.

If your partner defaults or the thief robs her, you have to automatically abono for your partner. That is, you have to repay two tokens and you cannot consume in this term.

B.7.3.3 *Test Questions - JL*

Now, we will ask you a few test questions. It is very important that you understand the game we have just presented, so we would like to test your understanding in this quiz. Please do not talk to your neighbors now and only tell the answer to the instructor or note it yourself on the sheet. Are you ready to take the quiz?

For all the answers, please encircle your answer choice:

1. You repay and your partner repays: How many tokens can you personally consume in this term? – None/1/2 [one token]
2. You repay and your partner does not repay. Will the team default? – No default/ default [NO DEFAULT]
3. You are robbed by the thief and your partner does not repay: Will the team default? – No default/ default [DEFAULT]
4. You repay and your partner has been robbed: How many tokens will you consume? – None/1/2 [NONE]

B.7.3.4 *Dislike - JL*

DISLIKE INTRODUCTION

In each term, you have the possibility to express your feelings about your partner's decision by choosing to tell her that you dislike her decision. Remember that you do not know who will be your partner; you only know it will be somebody from today's meeting. Your partner will also not know who you are; she just knows that her partner is somebody from today's meeting. If you decide to tell your partner you dislike her decision, she will know that her partner dislikes her decision, but she cannot connect it to you personally. The same holds true for your partner. She also can express that she does not like your decision; you will only know that your partner dislikes your decision but you do not know who your partner is.

[POSTER 1: DISLIKE OVERVIEW]

To show your partner you dislike her decision, you have eight red dislike tokens available each term. If you do not use your red tokens, you will thus have eight tokens. Each token is worth ₱ five. In total you have a dislike budget of ₱ 40. This is part of the participation fee that we will pay you.

You can choose to either use one red token or two red tokens to dislike your partner's decision. For each red token you assign to your partner, she will lose three red tokens. That is, if you use one red token, your partner loses three red tokens; you pay ₱ five from your dislike tokens and your partner loses ₱ 15 from her dislike tokens. If you use two red tokens, your partner loses six red tokens; you pay ₱ 10 from your dislike tokens and your partner loses ₱ 30 from her dislike tokens. Each dislike decision may influence your final payment if this variant of the game is chosen for payouts.

[POSTER 2: DISLIKE DETAIL – TABLE ON LEFT SIDE]

So you have the choice to use 1, 2 or 0/ no dislike token. If you use one, you will keep seven tokens, and your partner five. If you use two tokens, you will keep six and your partner two tokens. If you use zero tokens, both you and your partner will keep eight tokens.

[POSTER 2: DISLIKE DETAIL – TABLE ON RIGHT SIDE]

[INSTRUCTOR: USE THE LAMINATED RED TOKENS FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE]

Your partner also has the possibility to express her feelings about your decision by choosing to tell you that she dislikes your decision. Remember that she does not know that you will be her partner; she only knows it will be somebody from today's meeting who will receive the message. Your partner has eight red tokens available each term to tell you that she dislikes your decision. Each token is worth ₪ five.

Your partner also can choose to either use one red token or two red tokens to dislike your decision. For each red token your partner assigns to you, you will lose three red tokens.

Let me give you one example: you and your partner both have eight dislike tokens. In this example, you use one token so your partner loses three tokens. Your partner uses partner uses two tokens, so you lose six tokens. You are left with one token and your partner is left with 3 tokens.

[INSTRUCTOR: DEMONSTRATE THE EXAMPLE USING THE RED LAMINATED TOKENS]

For each dislike token that you send or that is destroyed by your partner, we will subtract ₪ 5 from your participation fee.

Do you have any questions?

TEST QUESTIONS - DISLIKE

Now, we will ask you a few test questions. It is very important that you understand the decision we have just presented, so we would like to test your understanding in this quiz. Please do not talk to your neighbors and only tell the answer to the instructor or note it yourself on the sheet. Are you ready to take the quiz?

For all the answers, please encircle your answer choice:

1. How many red tokens can you maximal use to show your dislike? – None/1/2 [two tokens]
2. If you decide not use any red tokens, how many tokens will your partner lose in this term? - None/1/2/3/4/6 [NONE]
3. If your partner decides to use one red token, how many tokens will you lose in this term? - 1/2/3/4/5/6 [3 TOKEN]
4. How many red tokens do you use if you like the decision of your partner? None/1/2 [NONE]

B.7.3.5 *Repayment Decisions JL*

Now we will play the game for real. All decisions will determine your payment when this treatment variant is selected in the lucky draw.

If this game is selected, we will randomly match you with an anonymous partner from today's meeting. You do not know who will be your partner; you only know it will be somebody from today's meeting. The same holds for your partner. You and your will determine the money that you earn if this variant is selected.

You have to take this decision in every term. Your partner has to take the same decision at the same time, so you do not know the decision of your partner and she does not know your decision.

REPAYMENT DECISION TERM T1, T2, T3 - REPAYMENT

[USE DECISION SHEET JL- NO PASS DECISION 1/2, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS.]

Please take your repayment and consumption decision for the 1st, 2nd and 3rd term when you are not robbed by the thief. Remember that you are in a group with your partner.

Your partner has the same decisions but you both decide at the same time, so you do not know what your partner chooses.

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM T₁, T₂, and T₃]

REPAYMENT DECISION AFTER TEAM DEFAULT

[USE DECISION SHEET JL- no pass decision 2/2, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS.]

Since you lose the voucher of ₪ 100 as soon as your team defaults in one term, and there are no other consequences to not repaying your loan in this game, you may want to change your decision in the 2nd term when your team has defaulted in the 1st term.

Please take your repayment and consumption decision for the 2nd term when your team has defaulted in the 1st period.

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR "AFTER THIEF"]

B.7.3.6 *Beliefs About Partner's Repayment - JL*

You are paired with somebody from today's meeting but you do not know her exact identity. What do you think will your partner chose?

Will she

A) Repay one token and consume one token

Or

B) Not repay and consume two tokens?

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

B.7.3.7 *Dislike - JL*

You have the possibility to express your feelings about your partner's decision by choosing to tell her that you dislike her decision. Your partner can also express that she does not like your decision. You both will only receive the message, but you do not know who sent it.

DISLIKE DECISIONS

[USE DECISION SHEET JL- NO PASS-DISLIKE INFO 1/1, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET]

Before you take your dislike decisions, remember that you do not know who your partner is and she does not know who you are. You only know you are paired with someone from today's meeting.

Please make your choices in private and tell it only to the instructor. Please take your decision to tell your partner you dislike her decisions for the following cases :

[DECISION SHEET DISLIKE (See Figure B.9), COLUMNS 1-3]

1) Your partner is not robbed by the thief. She repays her loan and earns one consumption token. How many dislike tokens do you assign?

2) Your partner is not robbed by the thief. She does not repay her loan and earns two consumption tokens. How many dislike tokens do you assign?

3) Your partner is robbed by the thief and she cannot repay. You have to repay for her and if you repay, the team will not default. How many dislike tokens do you assign?

[INSTRUCTOR NOTE THE DISLIKE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS. USE THE DECISION SHEET AND THE RESPECTIVE COLUMNS.]
















				
	Nagbayad:		Wala	
	Bili:			
		  	  	  

Figure B.9: Decision Sheet Dislike

BELIEFS ABOUT PARTNER’S DISLIKE

You are paired with somebody from today’s meeting but you do not know her exact identity. Your partner can assign you dislike points to tell you she does not like your decision. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor. [USE THE SAME DECISION SHEET AS ABOVE, DECISION COLUMN 1, 2 and 3]

How many red tokens do you think will your partner send you in the following cases:

- 1) You repay your loan and earn one consumption token. How many dislike tokens will your partner assign you?
- 2) You do not repay your loan and earn 2 consumption tokens. How many dislike tokens will your partner assign you?
- 3) You have been robbed and cannot repay your loan and earn zero consumption tokens. How many dislike tokens will your partner assign you?

Now you have taken all the decisions in this variant of the game. I will take this token and put it in this bag for the lucky draw at the end.

[LEAD INSTRUCTOR: PLACE TOKEN FOR THIS VARIANT INTO BAG]

B.7.4 *Individual-Liability - With Pass Token (FOR TYPE 1 COMPLETE SECTION; TYPE 2 ONLY B.7.4.1)*

B.7.4.1 *Explanation IL - Pass*

[USE POSTER IL-PASS]

In this variant of the game, you get an individual loan that gives you a regular income of two green tokens in all three terms. In each term, you may get robbed by a thief who steals all your income.

If you are spared by the thief, you have to decide how you want to spend your two tokens of income.

You have to simultaneously decide how you want to split up the income of two green tokens on repaying your loan and on consuming from the consumption table. You can either repay one token and consume one token, to go shopping at the consumption table; or you do not repay your loan repayment and consume two tokens, to go shopping at the consumption table. You can take home your shopping immediately after the game.

If you are robbed by the thief, you cannot repay your loan installment, that is you default in this term. The bank does not care why you defaulted, because she cannot observe this.

If you repay your loan in all three terms, you get the voucher of ₱ 100 after the game. If you do not repay in any of the three terms, that is if you default in any term, you do not get ₱ 100. There are no other consequences of not repaying in this game.

DESCRIPTION PASS TOKEN

[USE POSTER IL- PASS – OVERVIEW PASS TOKEN]

In this variant of the game you are offered a pass token by the bank presented by a yellow token.

[INSTRUCTORS: TAKE THE YELLOW CHIP FROM THE PARTICIPANT BAG AND SHOW THEM THAT THEY JUST HAVE ONE YELLOW TOKEN.]

You have only one pass token and you can use it to officially pass one repayment amount due to the next term. That is, if you use the pass token in one term, your repayment amount due in this term is reduced to zero, but you have to make-up this repayment installment in the next term, that is you have to repay two tokens in the next term. In the term in which you use the pass token, you do not default. If you are robbed by the thief, your non repayment does not count as default. If you are not robbed by the thief, you consume two tokens without defaulting.

Remember that you only have one pass token, you can only use the pass token in the 1st or the 2nd term. You cannot use it in the 3rd term.

There are three options to use the pass token:

1. you do not use the pass token in any term. In this case, your repayment amount due remains one token in each term.

2. you use the pass token in the 1st term. In this case, your repayment amount due in the first term is reduced to zero, but you have to repay two tokens in the 2nd term. In the 3rd period you have to repay one token.
3. you use the pass token in the 2nd term. In this case, your repayment amount due in the 1st term is one token. In the 2nd term you pass your repayment to the 3rd period. That is you have a repayment amount due of 0 in the 2nd term and a repayment amount due of two tokens in the 3rd period.

EXAMPLES - IL - PASS

I will now give you examples on the use of the pass token.

EXAMPLE A - USE PASS TOKEN IN T₁ - NO THIEF Let's consider the case where you are not robbed by a thief, but you decide to use the pass token in the 1st term.

[USE POSTER DECISION SHEET IL-PASS 1/ 4 (See Figure B.10)
USE THE LAMINATED TOKENS TO VISUALIZE THE EXAMPLE AS DEPICTED BELOW GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION THAT YOU ARE DEMONSTRATING. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

You have only one pass token and you can use it to officially pass one repayment amount due to the next term. That is, if you use the pass token in one term, your repayment amount due in this term is reduced to zero, but you have to make-up this repayment installment in the next term, that is you have to repay two tokens in the next term. In the term in which you use the pass token, you do not default. If you are robbed by the thief, your non repayment does not count as default. If you are not robbed by the thief, you consume two tokens without defaulting.

Your repayment amount due in the 1st term is zero and you can consume two tokens.

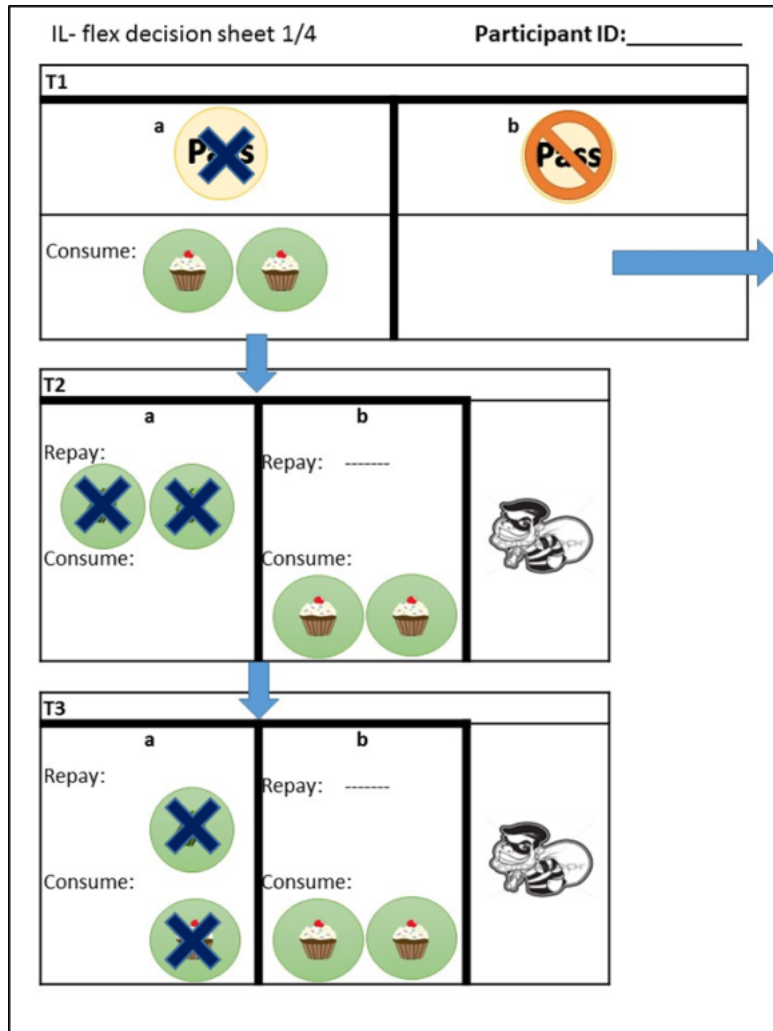
Your repayment amount due in the 2nd term is two tokens, you have to pay double because you passed the repayment from the 1st to the 2nd term.

Your repayment amount due in the 3rd term is one token.

If you are not robbed by the thief in any term and you repay in each term. You:

- Repay zero token, and consume two tokens in T₁
- Repay two tokens, and consume zero token in T₂
- Repay one token, and consume one token in T₃

Since you do not default in any term, you get the voucher of ₱ 100 in one month. You get two T₁ tokens, zero T₂ tokens and one T₃ token, that is you can go shopping for ₱ 100 and you get the voucher of ₱ 100 in one month.



+ 0 + + 100 Pesos

Figure B.10: Example A - IL-Pass

EXAMPLE B - USE PASS TOKEN IN T1- NO THIEF IN T1, THIEF IN T2
 Let's consider the case where you are not robbed by a thief, but you decide to use the pass token in the 1st term. [USE DECISION SHEET IL-PASS 1/4 (See Figure B.11)]

USE THE LAMINATED TOKENS TO VISUALIZE THE EXAMPLE AS DEPICTED BELOW. GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION THAT YOU ARE DEMONSTRATING. USE TOKENS WITH NUMBER 1 FOR T1, TOKENS WITH NUMBER 2 FOR T2 AND TOKENS WITH NUMBER 3 FOR T3 – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

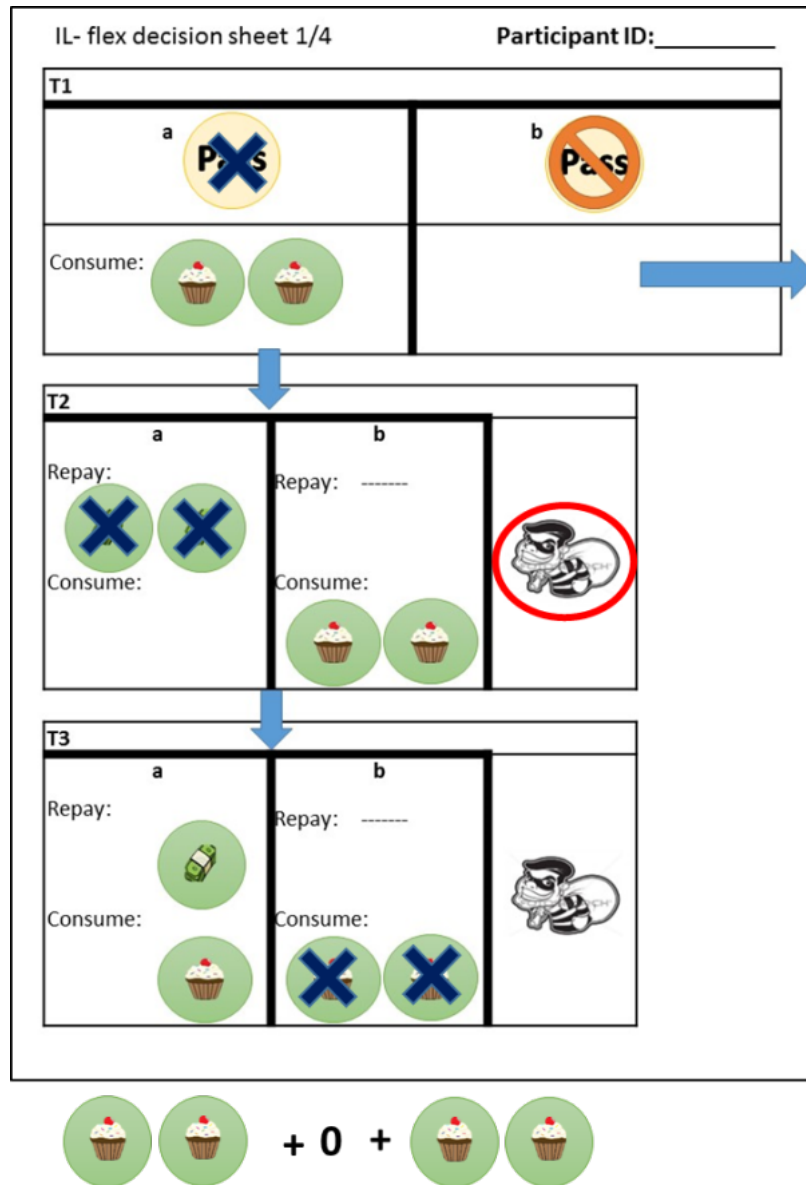


Figure B.11: Decision Sheet IL-Pass: Example B

Your repayment amount due in the 1st term is zero and you can consume two tokens.

Your repayment amount due in the 2nd term is two tokens, you have to pay double because you passed the repayment from the 1st to the 2nd term.
 Your repayment amount due in the 3rd term is one token.
 Now you are robbed by the thief in T₂. You cannot repay after the thief has robbed you.

- Repay zero token, and consume two tokens in T₁
- Are robbed by the thief and cannot repay in T₂. Default in T₂
- Repay zero token, and consume two tokens in T₃.

Since you do default in T₂, you do not get the voucher of ₱ 100 in one month. You get two T₁ tokens, zero T₂ tokens and two T₃ tokens, that is you can go shopping for ₱ 120.

EXAMPLE C: USE PASS TOKEN IN T₂

Let's consider the case where you are not robbed by a thief in the first two terms and you decide to use the pass token in the 2nd term. The thief robs you in the third term.

[USE DECISION SHEET IL-PASS 2/4 (See Figure B.12)
 USE THE LAMINATED TOKENS TO VISUALIZE THE EXAMPLE AS DEPICTED BELOW. GO THROUGH EACH EXAMPLE AND PLACE THE CORRECT LAMINATED GREEN TOKENS ON EACH DECISION THAT YOU ARE DEMONSTRATING. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER]

Your repayment amount due in the 1st term is one token.
 You use the pass token in the 2nd term. Your repayment amount due in the 2nd term is zero and you can consume two tokens.
 Your repayment amount due in the 3rd term is two tokens. You have to pay double because you passed the repayment from the 2nd to the 3rd term.
 Now you are robbed by the thief in T₃. You:

- Repay one token, and consume one token in T₁
- Repay zero token, and consume two tokens in T₂
- Repay zero token, and consume zero token in T₃

Since you default in the third term, you do not get the voucher of ₱ 100 in one month.

You get one T₁ tokens, two T₂ tokens and zero T₃ tokens, that is you can go shopping for ₱ 100.

Let's summarize the game: You have one pass token, that you can use in the first or in the second term. Using the pass token saves you from default, even if the thief came. In the term after using the pass token, you have to decide between repaying two and consuming two, as you have passed the repayment of one token to the next term.

B.7.4.2 Test Questions - IL - Pass

Now, we will ask you a few test questions. It is very important that you understand the decisions we have just presented, so we would like to test your

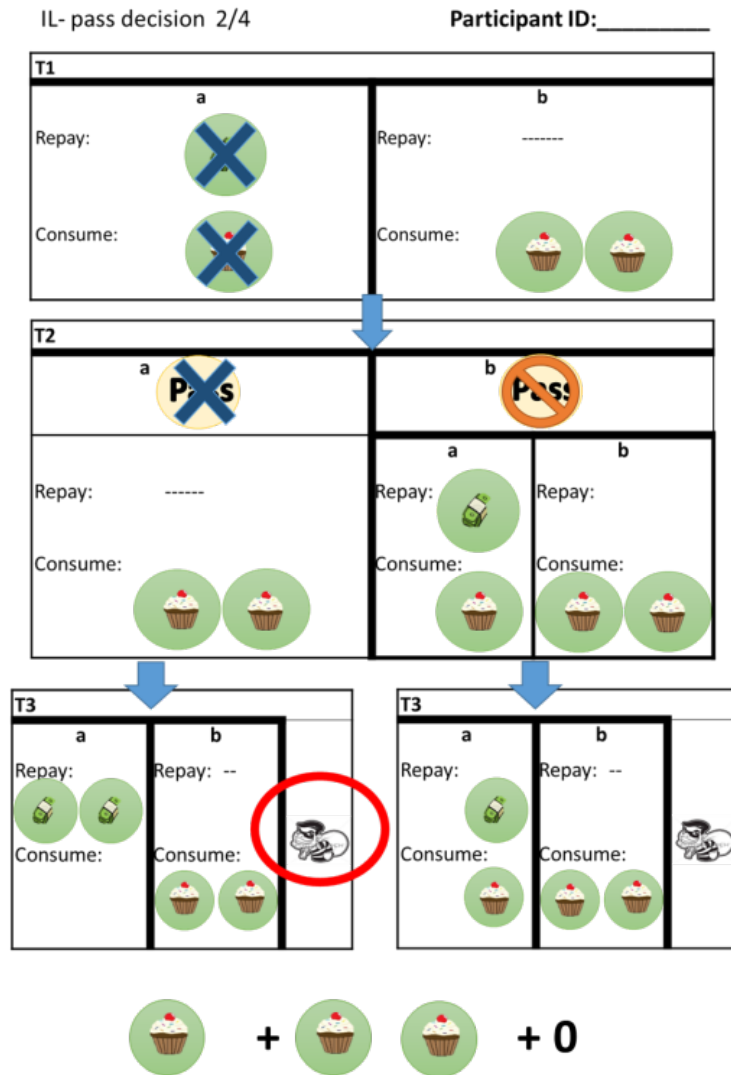


Figure B.12: Decision Sheet IL-Pass: Example C

understanding in this quiz. Please do not talk to your neighbors now and only tell the answer to the instructor or note it yourself on the sheet. Are you ready to take the quiz?

For all the answers, please encircle your answer choice:

1. In which terms can you use the pass token? Circle all that apply – Term 1/Term 2/Term 3 [Term 1 and Term 2]
2. When you use the pass token, will the bank consider you as being in default when the thief robs you? – No default/ default [NO DEFAULT]
3. Can you use the pass token when the thief did not rob you? – Yes/No [YES]
4. When you have used the pass token in one term, how many tokens is your repayment amount due in the next term? – None/1/2 [two tokens]

B.7.4.3 *Decision IL - Pass*

Now we will play the game for real. All decisions will determine your payment when this treatment variant is selected in the lucky draw.

In this game you have 1 yellow pass token that you can use it to officially pass one repayment amount due to the next term.

You can only use the pass token in the 1st or the 2nd term, but not in the 3rd term.

DECISION SHEET 1 - NOT ROBBED BY THE THIEF IN T1

[USE DECISION SHEET IL-PASS 1 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET IL 1/ 4]

First, let's consider the situation in which you have not been robbed by the thief.

Do you want to use the pass token in the 1st term? Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR, NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PASS TOKEN USE IN TERM 1 WHEN PARTICIPANT HAS NOT BEEN ROBBED BY THE THIEF.

INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 1. PREPARE THE RIGHT DECISION SHEET TO FOLLOW.]

We first look at only participants who chose to use the pass token in the 1st period when they are not robbed by a thief.

You have decided to use the pass token in the 1st period. Since you have not been robbed by the thief, you automatically consume two tokens in the 1st period. In the 2nd period, you have to repay two tokens since you passed your repayment with the pass token. When you are not robbed by the thief in the 2nd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

In the 3rd term, you have to take your standard repayment and consumption decision. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 2 - NOT USED PASS TOKEN IN T1 – NO THIEF IN T1

We now look at only participants who chose to not use the pass token in the 1st period when they are not robbed by a thief.

[USE DECISION SHEET IL-PASS 2 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET IL 2/ 4]

You have decided not to use the pass token in the 1st period. Since you have not been robbed by the thief, you have to take your repayment and consumption decision. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 1]

Since you have not used the pass token in the 1st term, you can still use it in the 2nd term. Let's first consider the situation when you are not robbed by a thief in the 2nd term. Do you want to use the pass token in the 2nd term? Please take your decision now by placing the pass token on the respective field.

[INSTRUCTOR, NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PASS TOKEN USE IN TERM 1 WHEN PARTICIPANT HAS NOT BEEN ROBBED BY THE THIEF.

INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2. PREPARE THE RIGHT DECISION SHEET TO FOLLOW.]

We first look at only participants who chose to use the pass token in the 2nd period when you are not robbed by a thief.

You have decided to use the pass token in the 2nd period. Since you have not been robbed by the thief, you automatically consume two tokens in the 2nd period. In the 3rd period, you have to repay two tokens since you passed your repayment with the pass token. When you are not robbed by the thief in the 3rd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

We first look at only participants who chose not to use the pass token in the 2nd period when you are not robbed by a thief.

You take your standard repayment and consumption decision for the 2nd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

You take your standard repayment and consumption decision for the 3rd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 3 - NOT USED PASS TOKEN IN T1 – THIEF IN T2

Now, we consider the situation where you have been robbed by a thief in the 2nd term that is you cannot repay your loan and you cannot consume. You can decide whether you want to use the pass token or not.

[USE DECISION SHEET IL-PASS 3 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET IL 3/ 4]

The pass token reduces your repayment amount due in the 2nd term to 0, but you have to make-up this repayment now in the 3rd term and pay 2 installments, that is your repayment amount due increases to two tokens. When you are not robbed by the thief in the 3rd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

If you do not use the pass token in the 2nd term and you are robbed by the thief, you default in this term. In this case, you have the standard decision in the 3rd term. Since you defaulted in the 2nd term, it is optimal for you to not repay and consume two tokens because you have lost the option to receive the voucher of ₱100 in one month anyway and there are no other consequences to default in this game. As before, there is the possibility that you are robbed by a thief in which case you earn 0 income and you cannot repay your loan and consume anything.

We first look at only participants who chose not to use the pass token in the 2nd period when you are not robbed by a thief.

You take your standard repayment and consumption decision for the 3rd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 4 - ROBBED BY THE THIEF IN T1

Now, let's consider the situation in which you have been robbed by a thief in the 1st term that is you cannot repay your loan and you cannot consume. You can decide whether you want to use the pass token or not.

[USE DECISION SHEET IL-PASS 4 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET IL 4/ 4]

Do you want to use the pass token in the 1st term?

We first look at only participants who chose to use the pass token in the 1st period when robbed by a thief.

You have decided to use the pass token in the 1st period. In the 2nd period, you have to repay two tokens since you passed your repayment with the pass token. When you are not robbed by the thief in the 2nd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor. [INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

In the 3rd term, you have to take your standard repayment and consumption decision.

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor. [INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDI-

INDIVIDUAL DECISION SHEETS FOR TERM 3]

We now look at only participants who chose to not use the pass token in the 1st period when they are robbed by a thief.

You have decided not to use the pass token in the 1st period. Since you have been robbed by the thief, you cannot repay your loan nor consume. Since you default on your repayment, you will not get the voucher of ₱ 100 in one month. There are no other consequences to your default in this game. It is your best option to default in the next 2 terms as well.

You take your standard repayment and consumption decision for the 2nd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

You take your standard repayment and consumption decision for the 3rd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 5 - REPAYMENT DECISION AFTER DEFAULT

[USE DECISION SHEET IL- PASS DECISION AFTER DEFAULT 1/1, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS.]

Since you lose the voucher of ₱ 100 as soon as you defaults in one term, and there are no other consequences to not repaying your loan in this game, you may want to change your decision in the 2nd term when you defaulted in the 1st term.

Repay one token and consume one token

Or

Not repay and consume two tokens.

Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR "AFTER THIEF"]

Now you have taken all the decisions in this variant of the game. I will take this token and put it in this bag for the lucky draw at the end.

B.7.5 *Joint-Liability – with Pass token (FOR TYPE 2 ONLY)*

B.7.5.1 *Explanation - JL - Pass*

DESCRIPTION JL

In this variant of the game, you will again be paired with one other participant. Together, you will form a team. You both play the same game and you and your partner will make the decisions at the same time. You do not know who will be your partner; you only know it will be somebody from today's meeting. Your partner will also not know who you are; she just knows that her partner is somebody from today's meeting.

[USE POSTER JL- PASS – OVERVIEW JL]

You and your partner get a loan that gives you both a regular income of two green tokens in all three terms. In each term, you, your partner or both of you may get robbed by a thief who steals all your income.

If you are spared by the thief, you and your partner have to decide how you want to split your two tokens of income: you can repay or consume as before.

[USE POSTER JL- PASS (See Figure B.13)]

Your team is jointly responsible for the two loans you have.

If one of the team partners does not repay her loan, the joint responsibility in the group will be applied automatically.

As long as the team loan is repaid in every term, you both will get the voucher of ₱100 in one month after the game. As soon as your team loan is not repaid in one term, you both will not receive the ₱100. There are no other consequences to team default in this game. This implies that if your team defaults in one term, it is your best strategy to also not repay in the following terms.

If you or your partner or both of you are robbed by the thief, you cannot repay your loan installment. In this case you default in this term. The bank cannot observe this. Also, the bank does not know who repaid and who did not; she can only observe the total repayment of the team.

You have to take this decision in every term. Your partner has to take the same decision at the same time, so you do not know the decision of your partner.

You have the possibility to express your feelings about your partner's decision by giving a dislike to her decision. Your partner also can express that she does not like your decision; you will only know that your partner dislikes your decision but you do not know who your partner is.

DESCRIPTION PASS TOKEN

[USE POSTER IL- PASS – OVERVIEW JOKER]

In addition, in this variant of the game you are offered a pass token by the bank presented by a yellow token. You have only one pass token and you can use it to officially pass one repayment amount due to the next term. That is, if

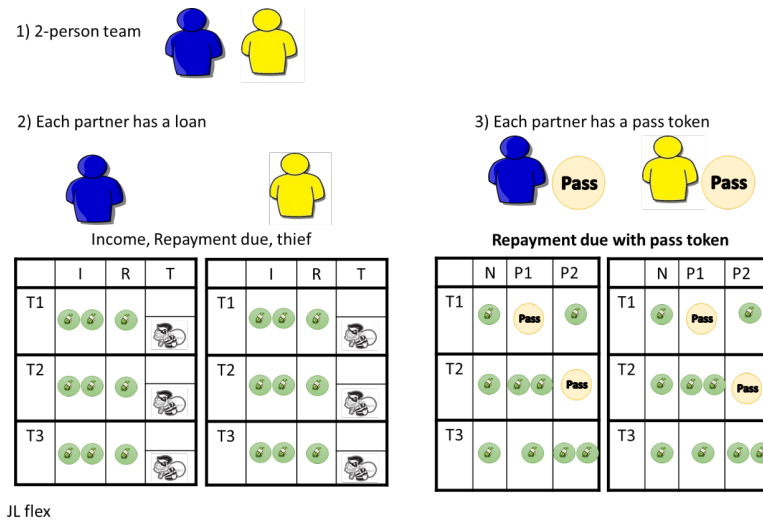


Figure B.13: JL-Pass Overview

you use the pass token in one term, your repayment amount due in this term is reduced to zero, but you have to make-up this repayment installment in the next term, that is you have to repay two tokens in the next term. In the term in which you use the pass token, you do not default. If you are robbed by the thief, your non repayment does not count as default. If you are not robbed by the thief, you consume two tokens without defaulting.

You can only use the pass token in the 1st or the 2nd term, but not in the 3rd term.

There are three options to use the pass token:

1. you do not use the pass token in any period. In this case, your repayment amount due remains one token in each term
2. you use the pass token in the 1st term. In this case, your repayment amount due in the first term is reduced to zero, but you have to repay two tokens in the 2nd term. In the 3rd period you have to repay one token.
3. you use the pass token in the 2nd term. In this case, your repayment amount due in the 1st term is one token. In the 2nd term you pass your repayment to the 3rd period. That is you have a repayment amount due of zero in the 2nd term and a repayment amount due of two tokens in the 3rd period.

Both your partner and you have a pass token that you can use to pass repayment from one term to the next term. This allows you to consume earlier or to deal with the thief yourself without relying on your partner to repay your loan.

If you use the pass token in one term, you have to repay two tokens in the next term. This implies, that when you have to repay your pass token in the next period, you cannot repay for your partner. The same holds true for your partner. If she uses has used her pass token in one term and has to repay the double installment in the next period, she can not cover your repayment. In this case, your team defaults on the loan.

Do you have any questions?

B.7.5.2 Examples JL - Pass

Let me give you four examples.

Example A: You use the pass token in term 1, your partner uses the pass token in term 1 – no thief in T₁, not thief in T₂ & T₃

Let's consider the case where both you and your partner are not robbed by a thief, but you both decide to use the pass token in the 1st term.

[INSTRUCTOR: USE THE POSTER AND THE LAMINATED GREEN AND YELLOW TOKENS FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE. USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER (See Figure B.14)]

Your repayment amount due in the 1st term is zero and you can consume two tokens.

Your repayment amount due in the 2nd term is two tokens, you have to pay double because you passed the repayment from the 1st to the 2nd term.

Your repayment amount due in the 3rd term is one token.

The same holds true for your partner since she also used her pass token in the 1st term when she was not robbed by a thief.

If both you and your partner are not robbed by the thief in any term and both of you repay in each term.

Your partner:

1. Repays zero token, and consumes two tokens in T₁.
2. Repays two tokens, and consumes zero token in T₂.
3. Repays one token, and consumes one token in T₃.

You:

1. Repay zero token, and consume two tokens in T₁. Your team does not default.
2. Repay two tokens, and consume zero token in T₂. Your team does not default.
3. Repay one token, and consume one token in T₃. Your team does not default.

Since your team does not default in any term, you both get the voucher of ₱ 100 in one month.

You have the possibility to express your feelings about your partner's decision by giving a dislike to her decision. Your partner also can express that she does not like your decision; you will only know that your partner dislikes your decision but you do not know who your partner is.

Example B: You use the pass token in term 1, your partner uses the pass token in term 1 – no thief in T₁, thief for partner in T₂, otherwise no thief

Let's consider the case where both you and your partner are not robbed by a thief, but you both decide to use the pass token in the 1st term.

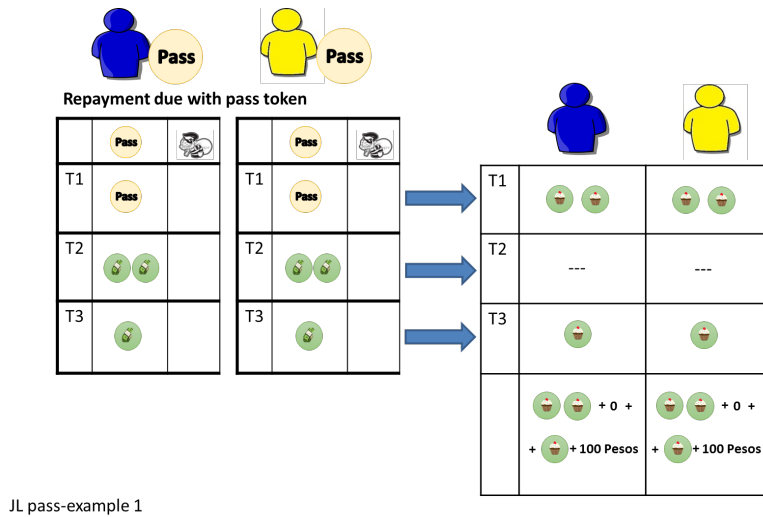


Figure B.14: JL-Pass - Example A

[INSTRUCTOR: USE THE POSTER AND THE LAMINATED GREEN AND YELLOW TOKENS FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE USE TOKENS WITH NUMBER 1 FOR T₁, TOKENS WITH NUMBER 2 FOR T₂ AND TOKENS WITH NUMBER 3 FOR T₃ – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER (See Figure B.15)]

Your repayment amount due in the 1st term is zero and you can consume two tokens.

Your repayment amount due in the 2nd term is two tokens, you have to pay double because you passed the repayment from the 1st to the 2nd term.

Your repayment amount due in the 3rd term is one token.

The same holds true for your partner since she also used her pass token in the 1st term when she was not robbed by a thief.

Now your partner is robbed by a thief in the 2nd term. If both of you repay in each term.

Your partner:

1. Repays zero token, and consumes two tokens in T₁.
2. Is robbed by the thief and cannot repay.

You:

1. Repay zero token, and consume two tokens in T₁. Your team does not default.
2. Repay two tokens, and consume zero token in T₂. Your partner cannot repay her loan.

You cannot repay for your partner since you have to make the double payment for the pass token. Your team loan is not repaid and your team defaults.

Since your team defaults in Term 2, you both do not get the voucher of ₱100 in one month. Since the team defaults, both will only consume in the last period. You have the possibility to express your feelings about your partner’s decision by giving a dislike to her decision. Your partner also can express that she does

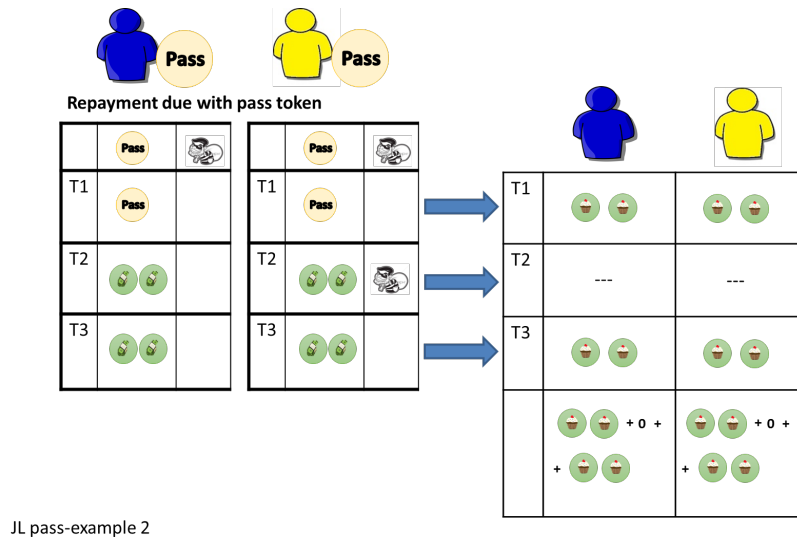


Figure B.15: JL-Pass - Example B

not like your decision; you will only know that your partner dislikes your decision but you do not know who your partner is.

Example C: You use the pass token in term 1, your partner does not use the pass token in term 1 - thief in T1, no thief in T2 & T3

Let's consider the case where you are robbed by the thief in the 1st term. You use the pass token in the 1st term. Your partner does not use her pass token and she is not robbed by a thief in any term. Both you and your partner repay in any term.

[INSTRUCTOR: USE THE POSTER AND THE LAMINATED GREEN AND YELLOW TOKENS FOR YOU AND YOUR PARTNER TO DEMONSTRATE THE EXAMPLE USE TOKENS WITH NUMBER 1 FOR T1, TOKENS WITH NUMBER 2 FOR T2 AND TOKENS WITH NUMBER 3 FOR T3 – USE SCOTCH TO PUT THEM ON THE TARPAULIN POSTER (See Figure B.16)]

Your repayment amount due in the 1st term is zero since you use the pass token. Although you were robbed by the thief, your partner does not have to repay your loan since you used the pass token.

Your repayment amount due in the 2nd term is two tokens, you have to pay double because you passed the repayment from the 1st to the 2nd term.

Your repayment amount due in the 3rd term is one token.

Your partner is not robbed by the thief and does not use the pass token.

If both of you repay in each term, you

Your partner:

1. Repays one token, and consumes one token in T1.
2. Repays one token, and consumes one token in T2.
3. Repays one token, and consumes one token in T3.

You:

1. Are robbed by the thief in T1. Your partner does not repay for you since you used the passed token. Your team does not default.
2. Repay two tokens, and consume zero token in T2 since you used the pass token in T1. Your team does not default.
3. Repay one token, and consume one token in T3.

Since your team does not default in any term, you both get the voucher of ₱ 100 in one month.

You have the possibility to express your feelings about your partner’s decision by giving a dislike to her decision. Your partner also can express that she does not like your decision; you will only know that your partner dislikes your decision but you do not know who your partner is.

Let’s summarize the game: You and your partner have one pass token each, that you can use in the first or in the second term. Using the pass token saves you from default, even if the thief came. In the term after using the pass token, you have to decide between repaying two and consuming two, as you have passed the repayment of one token to the next term.

If you use the pass token or your repayment due is who tokens you cannot abono for your partner in that term.

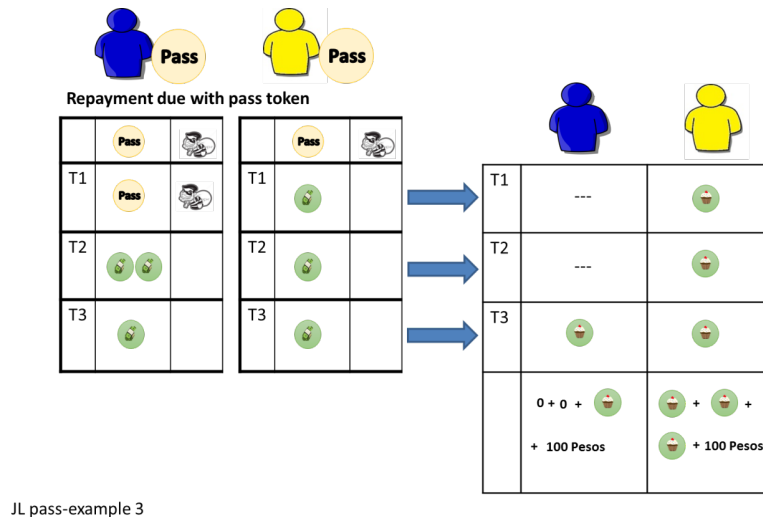


Figure B.16: JL-Pass - Example C

B.7.5.3 Test Questions JL - Pass

Now, we will ask you a few test questions. It is very important that you understand the decision we have just presented, so we would like to test your understanding in this quiz. Please do not talk to your neighbours now and only tell the answer to the instructor or note it yourself on the sheet. Are you ready to take the quiz?

1. You are robbed by the thief and you use the pass token: Does your partner have to abono for you? - Yes/No [NO]
2. You are not robbed by the thief and you use the pass token: How many items can you consume in this term? – Wala/1/2 [two tokens]

3. Your partner uses the pass token: Can she abonohan for you in this term?
- Yes/No [NO]
4. You have used your pass token in term 1 and now in term 2 you have to repay two tokens: Can you abonohan for your partner? - Yes/No [NO]

B.7.5.4 *Decisions JL - Pass*

Now we will play the game for real. All decisions will determine your payment when this treatment variant is selected in the lucky draw.

In this variant of the game, you will again be paired with one other participant in a team. You are jointly responsible for your team repayment and you have to automatically repay for your partner if she does not repay and the other way around. In addition, you have 1 yellow pass token that you can use to officially pass one repayment amount due to the next term.

In this game you have 1 yellow pass token that you can use it to officially pass one repayment amount due to the next term.

Since you and your partner take the decisions at the same time, you will not know what your partner's choices are. We will ask you for your choices, but keep in mind that you are in a team with your partner.

DECISION SHEET 1 - NOT ROBBED BY THE THIEF IN T1

First, let's consider the situation in which you have not been robbed by the thief.

[USE DECISION SHEET JL-PASS 1 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET JL 1/ 4]

Remember that you can only repay for your partner when you have a repayment obligation of one token, that is not in the term when you use the pass token and not in the term afterwards. Also your partner can only repay for you when she has a repayment obligation of one token.

Do you want to use the pass token in the 1st term if you are not robbed by the thief?

Please take your decision now by placing the pass token on the respective field.

[INSTRUCTOR, NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PASS TOKEN USE IN TERM 1 WHEN PARTICIPANT HAS NOT BEEN ROBBED BY THE THIEF.

INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 1.

PREPARE THE RIGHT DECISION SHEET TO FOLLOW.]

We first look at only participants who chose to use the pass token in the 1st period when they are not robbed by a thief.

You have decided to use the pass token in the 1st period. Since you have not been robbed by the thief, you automatically consume two tokens in the 1st period.

In the 2nd period, you have to repay two tokens since you passed your repayment with the pass token. When you are not robbed by the thief in the 2nd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

In the 3rd term, you have to take your standard repayment and consumption decision. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 2 - NOT USED PASS TOKEN IN T1 – NO THIEF IN T1

We now look at only participants who chose to not use the pass token in the 1st period when they are not robbed by a thief.

[USE DECISION SHEET JL-PASS 2 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET JL 2/ 4]

Remember that you can only repay for your partner when you have a repayment obligation of one token, that is not in the term when you use the pass token and not in the term afterwards. Also your partner can only repay for you when she has a repayment obligation of one token.

You have decided not to use the pass token in the 1st period. Since you have not been robbed by the thief, you have to take your repayment and consumption decision. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 1]

Since you have not used the pass token in the 1st term, you can still use it in the 2nd term. Let's first consider the situation when you are not robbed by a thief in the 2nd term. Do you want to use the pass token in the 2nd term?

Please take your decision now by placing the pass token on the respective field.

[INSTRUCTOR, NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR THE PASS TOKEN USE IN TERM 1 WHEN PARTICIPANT HAS NOT BEEN ROBBED BY THE THIEF.

INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2. PREPARE THE RIGHT DECISION SHEET TO FOLLOW.]

We first look at only participants who chose to use the pass token in the 2nd period when you are not robbed by a thief.

You have decided to use the pass token in the 2nd period. Since you have not been robbed by the thief, you automatically consume two tokens in the 2nd period. In the 3rd period, you have to repay two tokens since you passed your repayment with the pass token. When you are not robbed by the thief in the 3rd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

We first look at only participants who chose not to use the pass token in the 2nd period when you are not robbed by a thief.

You take your standard repayment and consumption decision for the 2nd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 3 - NOT USED PASS TOKEN IN T1 – THIEF IN T2

Now, we consider the situation where you have been robbed by a thief in the 2nd term that is you cannot repay your loan and you cannot consume. You can decide whether you want to use the pass token or not.

[USE DECISION SHEET JL-PASS 3 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET JL 3/ 4]

Remember that you can only repay for your partner when you have a repayment obligation of one token, that is not in the term when you use the pass token and not in the term afterwards. Also your partner can only repay for

you when she has a repayment obligation of one token.

The pass token reduces your repayment amount due in the 2nd term to 0, but you have to make-up this repayment now in the 3rd term and pay 2 installments, that is your repayment amount due increases to two tokens. When you are not robbed by the thief in the 3rd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

If you do not use the pass token in the 2nd term and you are robbed by the thief, you default in this term. In this case, you have the standard decision in the 3rd term. Since you defaulted in the 2nd term, it is optimal for you to not repay and consume two tokens because you have lost the option to receive the voucher of ₱ 100 in one month anyway and there are no other consequences to default in this game.

As before, there is the possibility that you are robbed by a thief in which case you earn 0 income and you cannot repay your loan and consume anything.

We first look at only participants who chose not to use the pass token in the 2nd period when you are not robbed by a thief.

You take your standard repayment and consumption decision for the 3rd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 4 - ROBBED BY THE THIEF IN T1

Now, let's consider the situation in which you have been robbed by a thief in the 1st term that is you cannot repay your loan and you cannot consume. You can decide whether you want to use the pass token or not.

[USE DECISION SHEET JL-PASS 4 / 4, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET. LEAD INSTRUCTOR; CHECK WHETHER ALL INSTRUCTORS USE THE DECISION SHEET JL 4/ 4]

Do you want to use the pass token in the 1st term?

We first look at only participants who chose to use the pass token in the 1st period when robbed by a thief.

You have decided to use the pass token in the 1st period. In the 2nd period, you have to repay two tokens since you passed your repayment with the pass token. When you are not robbed by the thief in the 2nd term, you have to decide whether to repay or consume. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

In the 3rd term, you have to take your standard repayment and consumption decision. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

You have decided not to use the pass token in the 1st period. Since you have been robbed by the thief, you cannot repay your loan nor consume. Since you default on your repayment, you will not get the voucher of ₱ 100 in one month. There are no other consequences to your default in this game. It is your best option to default in the next 2 terms as well.

You take your standard repayment and consumption decision for the 2nd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

You take your standard repayment and consumption decision for the 3rd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

You have decided not to use the pass token in the 1st period. Since you have been robbed by the thief, you cannot repay your loan nor consume. Since you default on your repayment, you will not get the voucher of ₱ 100 in one month. There are no other consequences to your default in this game. It is your best option to default in the next 2 terms as well.

You take your standard repayment and consumption decision for the 2nd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 2]

You take your standard repayment and consumption decision for the 3rd term. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 3]

DECISION SHEET 5 - REPAYMENT AFTER TEAM DEFAULT

[USE DECISION SHEET JL- pass decision After Default 1/1, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS.]

Since you lose the voucher of ₱ 100 as soon as your team defaults in one term, and there are no other consequences to not repaying your loan in this game, you may want to change your decision in the 2nd term when your team has defaulted in the 1st term. Please take your repayment and consumption decision for the 2nd term when your team has defaulted in the 1st period. Please indicate your choice between option A and option B. Please make your choices in private and tell it only to the instructor.

[INSTRUCTOR NOTE THE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR "AFTER DEFAULT"]

B.7.5.5 *Beliefs About Partner's Use of the Pass Token – JL*

You are paired with somebody from today's meeting but you do not know her exact identity. What do you think will your partner chose?

Will she use the pass token in the 1st term when she is not robbed by a thief?

Will she use the pass token in the 2nd term when she is not robbed by a thief and when she can still use the pass token?

Will she use the pass token in the 1st term when she is robbed by a thief?

Will she use the pass token in the 2nd term when she is robbed by a thief when she can still use the pass token?

B.7.5.6 *Dislike - JL PASS*

You have the possibility to express your feelings about your partner's decision by choosing to tell her that you dislike her decision. Your partner can also express that she does not like your decision. You both will only receive the message, but you do not know who send it exactly.

Before you take your dislike decisions, remember that you do not know who your partner is and she does not know who you are. You only know you are paired with someone from today's meeting. Please take your decision to tell your partner you dislike her decisions for the following cases in term.

[USE DECISION SHEET JL- pass-dislike info 1/2 and 2/2, SHOW THIS DECISION SHEET IN THE INSTRUCTORS' CLEARBOOK TO PARTICIPANTS. FOR EACH DECISION, POINT AT THE CORRESPONDING FIELD ON THE DECISION SHEET]

1. Your partner is not robbed by the thief. She does not use the pass token. She has a repayment obligation of one token. She repays her loan and earns one consumption token. How many dislike tokens do you assign?
2. Your partner is not robbed by the thief. She does not use the token. She has a repayment obligation of one token. She does not repay her loan and earns two consumption tokens. How many dislike tokens do you assign?
3. Your partner is not robbed by the thief. She uses the pass token. She has a repayment obligation of zero token. She consumes two tokens. How many dislike tokens do you assign?
4. Your partner is not robbed by the thief. She has used the pass token in the previous term. She has a repayment obligation of two tokens. She repays two tokens. How many dislike tokens do you assign?
5. Your partner is not robbed by the thief. She has used the pass token in the previous term. She has a repayment obligation of two tokens. She consumes two tokens. How many dislike tokens do you assign?
6. Your partner is robbed by the thief. She has used the pass token in the previous term. She has a repayment obligation of two token. She cannot repay or consume. You cannot repay for her and the team defaults. How many dislike tokens do you assign?
7. Your partner is robbed by the thief. She uses the pass token. She has a repayment obligation of zero token and she cannot consume anything. You do not have to repay for her and if you repay, the team will not default. How many dislike tokens do you assign?
8. Your partner is robbed by the thief. She does not use the pass token. She has a repayment obligation of one token and she cannot repay. You have to repay for her and if you repay, the team will not default. How many dislike tokens do you assign?

[INSTRUCTOR NOTE THE DISLIKE DECISIONS OF THE PARTICIPANTS IN YOUR GROUP ON THEIR INDIVIDUAL DECISION SHEETS FOR TERM 1]

B.7.6 *Discrete Choice Experiment*

Now we would like to present you a different decision making game. We will look at the characteristics of loan contracts for your general loan. There are different attributes that characterize a loan. Some features may be good for you but costly for the bank. So different features may determine how much you have to pay in interest payments or fees for a loan.

In particular, we are interested in how we can improve the loan for you. We have conducted interviews in a pre-study in November 2015. In these interviews you told us about lengthy center meetings when you have to wait for some members who cannot repay their loan installment in this week, the abonohan-payments you make for each other and some difficulties with weekly repayments especially when you have school activities for your children, someone from your family is sick or your business is not going well this week. In these cases you can rely on your group for abonohan but may also be good to have some possibilities to deal with small shocks and unexpected expenses on your own. We will present you different loan contracts that may help you to better deal with small unexpected expenses on your own.

The decisions you are going to make in this game are related to the question which kind of loan contract you like and which you dislike. There is no right or wrong answer. We are very interested in your opinion and we appreciate your cooperation. Please remember, that we are an independent research team. Your information is very valuable to us and we will not share your answers with ASHI.

B.7.6.1 *Explanation*

GENERAL LOAN SET UP

Let me explain the general loan that we look at in this study. We look at one specific type of loan: we will study a loan of ₱ 10,000 that has to be repaid over 50 weeks in weekly installments.

[DEMONSTRATE WITH THE POSTER "LOAN SETUP". POINT TO EACH ITEM WHEN YOU START DESCRIBING IT]

Each loan is characterized by:

1. The loan amount
2. The loan duration in weeks
3. The interest rate you are paying
4. The repayment schedule
5. An administration fee that is paid at the beginning of loan disbursement

These five attributes describe the most important characteristics of the credit contract. The repayment schedule determines the number of repayments you have to make. The interest rate and the number of repayments determine the weekly repayment rate. For your convenience, we will state them with each credit contract.

LOAN CHARACTERISTICS - REPAYMENT SCHEDULE

The main feature that we will study is the repayment schedule. We have three different repayment schedules.

- Regular repayment
- Pass token
- Repayment pause

[USE POSTER "CREDIT EXAMPLE A; B; C" TO PRESENT THE FOLLOWING]

REGULAR REPAYMENT

The regular repayment schedule means that you have to repay a weekly installment every week.

Let's look at Credit Example A:

- You have a loan of ₱ 10.000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱ 2.000 in total amount
- With the regular repayment schedule you make 50 repayments, one every week
- One weekly repayment is ₱ 240 / week

The advantage of the regular repayment schedule is that you pay small amounts every week. The disadvantage is that you may have problems paying your installment in some weeks in which someone in your family gets sick, your children have school activities, or you had a bad business week.

PASS TOKEN

The pass token schedule means that you have to repay a weekly installment every week, but you get 2 pass tokens with which you can pass the repayment amount due in this week to the next week. The following week, you have to pay a double repayment installment.

Let's look at Credit Example B:

- You have a loan of ₱ 10.000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱ 2.000 in total amount
- With the pass token schedule you make 50 repayments, one every week.
- One weekly repayment is ₱ 240 / week
- If you use the pass token, you do not have to repay anything in this week, but you have to repay ₱ 480 the next week.

The advantage of the pass token schedule is that it allows you to pass repayment from one week to the next. That is, if you may have problems paying your installment in some weeks in which someone in your family gets sick, your children have school activities, or you had a bad business week, you can pass your repayment amount due without defaulting to the bank. You have to pay a double installment in the following week.

REPAYMENT PAUSE

The repayment pause schedule means that you have to repay a weekly installment every week, but you get 2 repayment pauses with which you can pause the repayment amount due in one week. The two installments due in the repayment pauses are split evenly across weeks so your weekly repayment amount increases slightly.

Let's look at Credit Example C:

- You have a loan of ₱ 10,000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱ 2,000 in total amount
- With the repayment pause schedule you make 48 repayments, one every week, except for two
- One weekly repayment is ₱ 250 / week
- The repayment pause installments are split evenly across the remaining weeks and you have a slightly increased weekly repayment

The advantage of the repayment pause schedule is that it allows you to pass repayment from one week to the next. That is, if you may have problems paying your installment in some weeks in which someone in your family gets sick, your children have school activities, or you had a bad business week, you can pause your repayment amount due without defaulting to the bank. You have to pay a slightly higher installment in every week. Do you have questions?

B.7.6.2 Test Choices & Questions

Before we proceed, we would like to test whether you have understood our explanations. So we will ask you five questions of understanding:

1. How many repayments do you make with the regular repayment schedule?
 - a. [POINT TO GENERAL LOAN SETUP POSTER; ANSWER: 50]
2. How many pass tokens do you have over your loan cycle?
 - a. [POINT TO GENERAL LOAN SETUP POSTER; ANSWER: 2]
3. If you use a pass token in one week, how much do you have to pay in the next week?
 - a. [POINT TO GENERAL LOAN SETUP POSTER; ANSWER: ₱ 480]
4. How many times can you pause your repayment in the repayment pause schedule?
 - a. [POINT TO GENERAL LOAN SETUP POSTER; ANSWER: 2]
5. How many repayments do you make with the repayment pause schedule?
 - a. [POINT TO GENERAL LOAN SETUP POSTER; ANSWER: 48]

[NOTE ANSWER ON TEST QUESTION DISCRETE CHOICE" DECISION SHEET]

[TEST QUESTIONS:

- ⇒ ONLY ASK TEST QUESTIONS ONCE!
- ⇒ CHECK WHETHER MOST PARTICIPANTS HAVE UNDERSTOOD THE EXPLANATIONS AND ANSWERED THE TEST QUESTIONS CORRECTLY.
- ⇒ IF TOO MANY PARTICIPANTS DID NOT ANSWER THE TEST QUESTION CORRECTLY, EXPLAIN THE RESPECTIVE CREDIT ATTRIBUTE AGAIN.

HOW TO IDENTIFY "TOO MANY":

- ⇒ LEAD INSTRUCTOR: ASK INSTRUCTORS IF WE NEED TO REPEAT EXPLANATIONS.

⇒ INSTRUCTORS; RAISE YOUR HAND IF MORE THAN 2 PEOPLE IN YOUR GROUP ANSWERED 2 OR MORE QUESTIONS WRONGLY.
 ⇒ IF MORE THAN 2 INSTRUCTORS RAISE THEIR HANDS; REPEAT THE EXPLANATION AS IN SECTION 3.1.2 "General loan setup"]

Test Comparison 1:

Now, let us look at two different loan contracts. One of these two schedules is better than the other. Let us look at CREDIT EXAMPLE A and CREDIT EXAMPLE D

[SHOW POSTER "TEST COMPARISON 1"]

We have CREDIT EXAMPLE A, the loan contract to your left, with

- You have a loan of ₱ 10.000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱ 2.000 in total amount
- With the regular repayment schedule you make 50 repayments, one every week
- One weekly repayment is ₱ 240 / week

We have CREDIT EXAMPLE D, the loan contract to your right, with

- You have a loan of ₱ 10.000
- You repay the loan over 50 weeks
- You pay 30 percent interest, that is ₱ 3.000 in total amount
- With the regular repayment schedule you make 50 repayments, one every week
- One weekly repayment is ₱ 260 / week

So which loan contract is better, A or D?

[NOTE ANSWER ON "TEST QUESTION DISCRETE CHOICE" DECISION SHEET – TEST COMPARISON A]

Test Comparison 2:

Let us look at another set of two different loan contracts. One of these two schedules is better than the other.

[SHOW POSTER "TEST COMPARISON 2"]

We have CREDIT EXAMPLE B, the credit contract to your left, with

- You have a loan of ₱ 10.000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱ 2.000 in total amount
- With the pass token schedule you make 50 repayments, one every week.
- One weekly repayment is ₱ 240 / week
- If you use the pass token, you do not have to repay anything in this week, but you have to repay ₱ 480 the next week.

- Administration fee of ₱0

We have CREDIT EXAMPLE E, the loan contract to your right, with

- You have a loan of ₱10,000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱2,000 in total amount
- With the pass token schedule you make 50 repayments, one every week.
- One weekly repayment is ₱240 / week
- If you use the pass token, you do not have to repay anything in this week, but you have to repay ₱480 the next week.
- Administration fee of ₱1,000

So which loan contract is better, B or E?

[NOTE ANSWER ON "TEST QUESTION DISCRETE CHOICE" DECISION SHEET – TEST COMPARISON 2]

Test Comparison 3:

Let us look at another set of two different loan contracts. One of these two schedules is better than the other.

[SHOW POSTER "TEST COMPARISON 3"]

We have CREDIT EXAMPLE C, the credit contract to your left, with

- You have a loan of ₱10,000
- You repay the loan over 50 weeks
- You pay 20 percent interest, that is ₱2,000 in total amount
- With the repayment pause schedule you make 48 repayments, one every week, except for two
- One weekly repayment is ₱250 / week
- The repayment pause installments are split evenly across the remaining weeks and you have a slightly increased weekly repayment.
- Administration fee of ₱0

We have CREDIT EXAMPLE F, the loan contract to your right, with

- You have a loan of ₱10,000
- You repay the loan over 50 weeks
- You pay 30 percent interest, that is ₱3,000 in total amount
- With the repayment pause schedule you make 48 repayments, one every week, except for two.
- One weekly repayment is ₱271 / week
- The repayment pause installments are split evenly across the remaining weeks and you have a slightly increased weekly repayment.
- Administration fee of ₱zero

So which loan contract is better, C or F?

[NOTE ANSWER ON "TEST QUESTION DISCRETE CHOICE" DECISION SHEET – TEST COMPARISON 3]

B.7.6.3 *Preference Check*

Before we proceed, we would like to know which of the repayment schedules you like best?

Please remember lengthy center meetings when you have to wait for some members who cannot repay their loan installment in this week, the abonohan-payments you make for each other and some difficulties with weekly repayments especially when you have school activities for your children, someone from your family is sick or your business is not going well this week. In these cases, it may also be good to have some possibilities to deal with small shocks and unexpected expenses on your own.

The regular repayment schedule, the pass token schedule, or the repayment pause schedule?

[USE EXAMPLE POSTER A; B; C]

[INSTRUCTOR: NOTE THE DECISIONS ON THE PARTICIPANTS ANSWER SHEET]

B.7.6.4 *Execution of the Experiments*

Now we have practised the understanding of the loan contracts.

From now on, we will present you different sets of two distinct loan contracts. In contrast to the test of understanding, there is no loan contract better than the other. Each loan contract is better in some way but worse in some other way.

We will ask you which loan contract you prefer. Your decision only depends on what you like and what you don't like. There is no right or wrong answer.

[SHOW COMPARISON 1 TO 11/12 NOTE DECISION OF PARTICIPANT ON THE ANSWER SHEET]

Comparison 1

Here are two insurance contracts. Contract A on the left side and contract B on the right side.

Contract A is characterized by

- Loan amount of ₱XXX
- Loan duration of 50 weeks
- Interest rate in XX percent , that is ₱XXX in total amount
- Repayment schedule of YYY
- Weekly repayment of ₱XXX / week
- Administration fee of ₱XXX

[DESCRIBE THE LOAN CONTRACT AS EXPLAINED ABOVE, PUT IN THE CORRECT AMOUNTS FROM THE LOAN CONTRACT].

Contract B is characterized by

- Loan amount of ₱XXXX
- Loan duration of 50 weeks
- Interest rate in XX percent , that is ₱XXX in total amount
- Repayment schedule of YYY
- Weekly repayment of ₱XXX / week
- Administration fee of ₱XXX

[DESCRIBE THE LOAN CONTRACT AS EXPLAINED ABOVE, PUT IN THE CORRECT AMOUNTS FROM THE LOAN CONTRACT].

Comparison 2-11 (for Block 1 and 2), 2-12 for Block 3)

[REPEAT AS COMPARISION 1]

B.7.7 *Short Survey*

Participant ID: _____ Session ID: _____

Intake Survey



0.1	Instructor
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Block A

The following set of questions form part of a game. Approximately 5 out of 20 people will actually be paid for one these questions at the end of today's meeting. You don't know whether you will be one of the lucky ones and which question you might be paid for. Therefore, 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.

	<p>The following questions will ask you whether you want P100 now, or a different amount of money in 1 month. All amounts will be paid with mobile phone load. If you are paid, we will record your mobile phone number at the end of the experiment. If you don't have a mobile phone, you can give us the number of a family member or friend.</p> <p>If a question is selected where you choose P100 now, then we will transfer P100 of load today. If a question is selected where you choose an amount in one month, then we will transfer that amount in one month. This payment is guaranteed. We will send you a confirmation text before you leave today which confirms the amount. We will also give you a paper voucher (SHOW VOUCHER) which states your name, the amount, and the date when we send the load. We will also give you a number you can contact if your phone number changes. Do you have any questions on this before I start?</p> <p>INSTRUCTORS:</p> <ul style="list-style-type: none"> - ASK EACH QUESTION SEPARATELY. LET THEM THINK ABOUT EACH ONE. - DO NOT SHORTEN OR ABBREVIATE THE QUESTIONS IN ANY WAY - AVOID SWITCHING BACK AND FORTH. IF RESPONDENT SWITCHES BACK AND FORTH; CHECK THEY UNDERSTOOD THE QUESTION.
A1 1	<p>Do you prefer to get P100 guaranteed today, or to get P100 guaranteed 1 month from now?</p> <p>(1) <i>P100 today</i></p> <p>(2) <i>P100 in 1 month</i></p>
A2 2	<p>Would you prefer to receive P100 guaranteed today, or P120 guaranteed in 1 month?</p> <p>(1) <i>P100 today</i></p> <p>(2) <i>P120 in 1 month</i></p>
A3 3	<p>What if it was P100 guaranteed today, or P150 guaranteed in 1 month?</p> <p>(1) <i>P100 today</i></p> <p>(2) <i>P150 in 1 month</i></p>
A4 4	<p>Would you prefer to receive P100 guaranteed today, or P200 guaranteed in 1 month?</p> <p>(1) <i>P100 today</i></p> <p>(2) <i>P200 in 1 month</i></p>
A5 5	<p>Would you prefer to receive P100 guaranteed today, or P250 guaranteed in 1 month?</p> <p>(1) <i>P100 today</i></p> <p>(2) <i>P250 in 1 month</i></p>

Participant ID: _____ Session ID: _____

 A6	What if it was P100 guaranteed today, or P300 guaranteed in 1 month?	(1) <i>P100 today</i> (2) <i>P300 in 1 month</i>
 A7	For this question, suppose we play a game where you flip a coin to win a prize of money. If this question is selected for payment, then we will actually flip a coin. If you get heads, you win a lower amount of cash, and if you get tails, you win a higher amount of cash. There are five different options for the amounts, and you can choose your favourite one. Which game do you prefer? SHOW LOTTERIES AND READ OPTIONS READ: Option A is to win 90 pesos if you get heads and 190 pesos if you get tails. <i>(pause)</i> CONTINUE READING OPTIONS B – E USING THE LOTTERY PICTURE	(1) A. (90, 190) (2) B. (80, 240) (3) C. (60, 300) (4) D. (20, 380) (5) E. (0, 400)

Block B

Your answers will only be used for the research study and expect that we will treat them with full confidentiality. Trust us that we will not share them to your organization ASHI or any of your group mates. The only reference that will appear in this survey is your “participants ID”, your name will not be mentioned or appear in the report.

For each question, please let me know whether, yes, the statement applies to you or no, the statement does not apply to you. [yes/no]







B1	I would know <u>immediately</u> if someone in my group has a problem in her business activities.	(1) Yes (2) No (3) <i>N/A</i>
B2	If a group member does not have the money to repay in one week, I would know the <u>exact</u> reasons.	(1) Yes (2) No (3) <i>N/A</i>
B3	In my borrowing group, I actively try to gain information about the other members’ business activities.	(1) Yes (2) No (3) <i>N/A</i>
B4	My group members would know <u>immediately</u> if I have a problem in my business activities.	(1) Yes (2) No (3) <i>N/A</i>
B5	If I don’t have the money to repay, my group members would know the <u>exact</u> reasons.	(1) Yes (2) No (3) <i>N/A</i>
B6	I am willing to share with others without expecting anything in return.	(1) Yes (2) No (3) <i>N/A</i>
B7	I am willing to pay abonohan for other members, even if they never pay abonohan for me.	(1) Yes (2) No (3) <i>N/A</i>

Participant ID: _____ **Session ID:** _____

B8	I will only pay abonohan for other members that have paid abonohan for me.	(1) Yes (2) No (3) N/A
B9	Have you ever given abonohan to a fellow group member?	(1) Yes (2) No (3) N/A
B10	Have you ever received abonohan from a fellow group member?	(1) Yes (2) No (3) N/A
B11	Think about last month. In how many weeks did abonohan in your group happen?	_____ <i>times</i>
B12	In your group, is abonohan usually paid back?	(1) Yes (2) No (3) N/A
B13	Do you feel pressure from your group members to pay abonohan for them?	(1) Yes (2) No (3) N/A
B14	When in a group meeting, if my group members ask me to pay for them, it would be hard to say no.	(1) Yes (2) No (3) N/A
B15	Do you feel pressure from your group members when you cannot repay your loan?	(1) Yes (2) No (3) N/A
B16	Are you worried to discuss your repayment problems with your group members?	(1) Yes (2) No (3) N/A

Participant ID: _____ Session ID: _____

Block C

The questions below ask you to choose between rewards 1 month from now, and rewards 2 months from now. As before, you might get paid for one of these questions in load. The payment is guaranteed. So please really think about which reward you prefer for each question.		
A8 	Do you prefer to get P100 guaranteed in one month, or to get P100 guaranteed in 2 months?	(1) P100 in 1 month (2) P100 in 2 months
A9 	Would you prefer to receive P100 guaranteed in one month, or P120 guaranteed in 2 months?	(1) P100 in 1 month (2) P120 in 2 months
A10 	What if it was P100 guaranteed in one month, or P150 guaranteed in 2 months?	(1) P100 in 1 month (2) P150 in 2 months
A11 	Would you prefer to receive P100 guaranteed in one month, or P200 guaranteed in 2 months?	(1) P100 in 1 month (2) P200 in 2 months
A12 	Would you prefer to receive P100 guaranteed in one month, or P250 guaranteed in 2 months?	(1) P100 in 1 month (2) P250 in 2 months
A13 	What if it was P100 guaranteed in one month, or P300 guaranteed in 2 months?	(1) P100 in 1 month (2) P300 in 2 months

APPENDIX TO CHAPTER 3

C.1 ADDITIONAL ROBUSTNESS TESTS

Table C.1: Lying and Social Preferences

	Probit (ME)								
	Misreporting in Part 2								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
GroupPC	0.209*** (0.0591)	0.209*** (0.0595)	0.207*** (0.0592)	0.209*** (0.0595)	0.209*** (0.0584)	0.208*** (0.0595)	0.208*** (0.0597)	0.205*** (0.0590)	0.206*** (0.0577)
GroupNoPC	0.170*** (0.0585)	0.173*** (0.0585)	0.173*** (0.0585)	0.174*** (0.0586)	0.174*** (0.0575)	0.173*** (0.0587)	0.173*** (0.0581)	0.167*** (0.0583)	0.163*** (0.0561)
Machiavelli (self)	0.00350 (0.00327)								0.00156 (0.00324)
Machiavelli (other)		0.00163 (0.00316)							0.00157 (0.00256)
Conscientiousness			-0.00713 (0.0113)						0.00282 (0.0121)
Neuroticism				0.00214 (0.0121)					0.000840 (0.0123)
Openness					-0.0206** (0.0105)				-0.0199* (0.0113)
Extraversion						-0.000513 (0.0119)			0.00132 (0.0120)
Agreeableness							0.00391 (0.0153)		0.0171 (0.0146)
Social Desirability								0.0202 (0.0147)	0.0188 (0.0166)
Observations	273	273	273	273	273	273	273	273	273
# of Clusters	117	117	117	117	117	117	117	117	117
Pseudo-R ²	0.070	0.065	0.064	0.064	0.078	0.063	0.064	0.076	0.095

Notes: Robust standard errors clustered on group level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table C.2: Lying and Risk Preferences

	Probit (ME)					
	(1)	(2)	(3)	(4)	(5)	(6)
GroupPC	0.208*** (0.0598)	0.213*** (0.0600)	0.210*** (0.0590)	0.208*** (0.0596)	0.205*** (0.0616)	0.211*** (0.0610)
GroupNoPC	0.173*** (0.0586)	0.180*** (0.0583)	0.173*** (0.0583)	0.173*** (0.0585)	0.172*** (0.0578)	0.177*** (0.0564)
Risk (General)	-0.00483 (0.0133)					-0.00925 (0.0203)
Risk (Car)		0.0126 (0.0145)				0.0141 (0.0159)
Risk (Money)			0.00610 (0.0145)			0.00920 (0.0177)
Risk (Sport)				-0.000757 (0.0114)		4.86e-05 (0.0124)
Risk (Career)					-0.00743 (0.0160)	-0.00931 (0.0236)
Observations	273	273	273	273	273	273
# of Clusters	117	117	117	117	117	117
Pseudo-R ²	0.064	0.067	0.064	0.063	0.064	0.071

Notes: Robust standard errors clustered on group level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table C.3: Lying and Socio-Economic Background

	Probit (ME)						
	Misreporting in Part 2						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
GroupPC	0.205*** (0.0585)	0.208*** (0.0594)	0.209*** (0.0590)	0.208*** (0.0590)	0.209*** (0.0595)	0.204*** (0.0594)	0.201*** (0.0574)
GroupNoPC	0.169*** (0.0576)	0.169*** (0.0584)	0.172*** (0.0581)	0.166*** (0.0585)	0.173*** (0.0588)	0.169*** (0.0583)	0.154** (0.0565)
Female	-0.0824** (0.0366)						-0.0717** (0.0363)
Importance of religious questions (1=not important at all, 7=very important)		-0.00870 (0.00933)					-0.00662 (0.00851)
Political Attitude (1=strongly left, 7=strongly right)			0.0239 (0.0194)				0.0222 (0.0199)
Age				0.00523 (0.00451)			0.00461 (0.00414)
Number of other participants known					-0.0285 (0.0650)		-0.0167 (0.0588)
Number experiments participated in						0.00539* (0.00290)	0.00447 (0.00284)
Observations	273	273	273	273	273	273	273
# of Clusters	117	117	117	117	117	117	117
Pseudo-R ²	0.079	0.066	0.070	0.069	0.065	0.078	0.104

Notes: Robust standard errors clustered on group level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

C.2 ADDITIONAL ANALYSES (GROUP COMPOSITION, DECISION TIMES AND INDIVIDUAL-DELIBERATION)

C.2.1 *Group Composition*

In our main analysis, we have shown that for the full sample (78 groups), the number of dishonest individuals in a group plays only a weak role for lying behavior in Part 2. Here we provide an additional analysis by excluding those participants who have seen a \boxtimes in Part 1 and thus had no incentive to lie in Part 1. Focusing on this subsample (66 groups) we find some evidence for differences in misreporting across group composition. In *GroupPC*, the correlation between the number of honest individuals in a group and honest reporting is small and fails to be statistically significant (Rho: 0.148, $p=0.41$) and Fisher's exact test fails to reject the independence of lying behavior from group composition at the 10-percent level ($p=1$). In *GroupNoPC*, for the subsample (33 groups) the share of dishonest individuals in Part 2 correlates significantly and positively with the number of group members who have reported dishonestly in Part 1 (Rho: 0.362, $p=0.0382$), but Fisher's exact test again fails to reject independence of group composition and lying behavior in Part 2 ($p=0.147$).

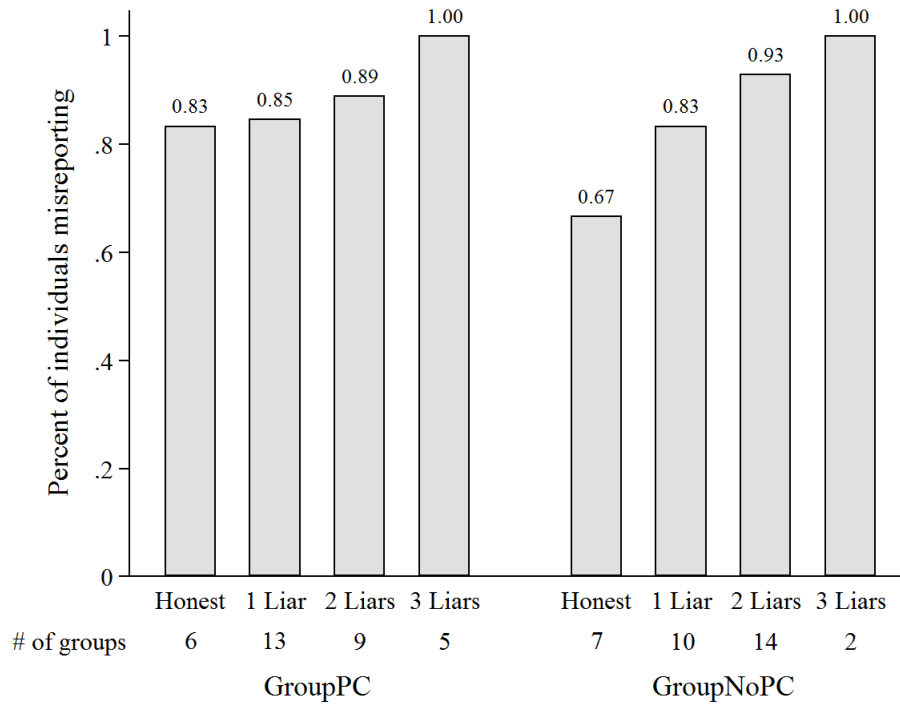


Figure C.1: Dishonest Reporting by Group Composition and Treatment
Notes: Subsample of groups who have not seen a ☒ in Part 1, 66 groups in total

C.2.2 Decision Times

A recent debate surrounds the question of whether honesty or dishonesty is the "natural state" or requires deliberation (Foerster et al., 2013; Shalvi et al., 2012, 2013; Van't Veer et al., 2014). In the context of group decision making, uncertainty about the norm may result in longer decision times, whereas certainty about a prevailing (dis)honesty norm may result in shorter decision times. We find that in Part 1, misreporting individuals (on average 37 percent) take significantly longer in their decision to lie as compared to the individuals reporting the die roll that was actually displayed (mean honest reporting time: 12.64 sec (SD: 4.71); mean dishonest reporting time: 16.89 sec (SD: 9.20), Kolmogorov-Smirnov exact test, $p=0.012$, Spearman's $\rho=0.296$, $p=0.001$). In Part 2, however, we find that chats that lead to honest reporting take longer: Mean chat time before honest reporting: 227 sec (SD: 46.84); mean chat time before dishon-

est reporting: 154.11 sec (SD: 86.14), (Kolmogorov-Smirnov exact test, $p=0.028$, Spearman's $\rho=-0.251$, $p=0.027$).¹

Having communicated in a group makes decisions in the lying task significantly faster in Part 3 (mean reporting time: *Individual*: 13.49 sec (SD: 6.24); mean reporting time for *GroupPC* and *GroupNoPC* pooled: 10.40 sec (SD: 3.46), Kolmogorov-Smirnov exact test, $p=0.001$).² Further, in Part 3, decision times and dishonest behavior do not significantly relate to each other (mean honest reporting time: 11.08 sec (SD: 3.79); mean dishonest reporting time: 11.52 sec (SD: 5.02), Kolmogorov-Smirnov exact test, $p=0.857$, Spearman's $\rho=0.019$, $p=0.838$). Notably, conditional on dishonest reporting in Part 3, participants who participated in the group chat report significantly faster non-truthfully than participants from the *Individual* treatment (Kolmogorov-Smirnov exact test, $p=0.004$; Spearman's $\rho=-0.27$, $p=0.008$; mean reporting time with communication: 10.48, SD: 3.71; without communication: 13.94, SD: 6.67). Having participated in the chat also speeds up decisions in the belief elicitation task (Kolmogorov-Smirnov exact test, $p=0.003$). This might be seen as additional evidence that the chat has shifted beliefs about the honesty norm.

c.2.3 Content Analysis of IndividualDeliberation

As for the chats of the group treatments, coders analyzed the content of the stream of thoughts in *IndividualDeliberation* according to the pre-defined codebook in Appendix C.3. Our results show that many participants did indeed deliberate in this treatment. Participants took on average 234 seconds (SD=114.3) to write down their thoughts and 67 percent of participants (26 out of 39) in *IndividualDeliberation* explicitly mention arguments for dishonesty, whereas only 9 participants write down arguments for honesty (see Table C.4). Only one participant decided not to write anything. Interestingly, frequencies of dishonest and honest arguments are not statistically different from the group treatments (χ^2 -test, *GroupPC* and *GroupNoPC* pooled, dishonest arguments:

¹ We thank an anonymous referee for this suggestion.

² As *GroupPC* and *GroupNoPC* do not differ significantly in terms of decision times in Part 3 (Kolmogorov-Smirnov exact test, lying decision: $p=0.467$; belief elicitation time: $p=0.862$) and to avoid multiple testing, we pool both group treatments and test them jointly against *Individual*.

$p=0.114$; honest arguments: $p=0.878$). The average share of statements containing arguments for dishonest behavior amounts to 59.2 percent and - as in the group treatments - is significantly higher than the share of honest messages (15.1 percent, Wilcoxon signed-rank exact test, $p<0.001$).

Table C.4: Number of Individuals Using Honest and Dishonest Arguments

Arguments for Honesty mentioned	Arguments for Dishonesty mentioned		
	Yes	No	Σ
Yes	6	3	9
No	20	10	30
Σ	26	13	39

To analyze the influence of arguments made on misreporting behavior, we replicate Table 3.3 from the main text including the data from *IndividualDeliberation* in Table C.5. The results are qualitatively similar and most importantly, when including a dummy for the group treatments in Model (2), we find that groups lie significantly more, even when controlling for the arguments made during the chat. This result is robust to controlling for misreporting in Part 1 (Model (3)). The analysis of *IndividualDeliberation* and its comparison with our two group treatments suggests that individuals come up with dishonest and honest argument to a similar extent as groups. Yet only the exchange and validation of these arguments within a group can lead to a shift in beliefs about the norm and corresponding changes in behavior.

Table C.5: Lying Behavior and Arguments Used

	Probit (ME)		
	Misreporting in Part 2		
	(1)	(2)	(3)
Money (Dishonest Use)	0.0485 (0.0480)	0.0630 (0.0474)	0.0537 (0.0455)
Money (Honest Use)	-0.406*** (0.154)	-0.416*** (0.160)	-0.448*** (0.158)
Honesty (Dishonest Use)	-0.00915 (0.111)	-0.00619 (0.0879)	-0.0263 (0.0827)
Honesty (Honest Use)	-0.0709 (0.0475)	-0.0785* (0.0450)	-0.0438 (0.0403)
Insecurity (Dishonest Use)	0.0288 (0.0663)	0.0111 (0.0666)	-0.00664 (0.0533)
Insecurity (Honest Use)	-0.354*** (0.144)	-0.277* (0.160)	-0.209 (0.155)
Rules (Dishonest Use)			
Rules (Honest Use)	-0.308* (0.173)	-0.316** (0.155)	-0.249* (0.144)
Others' Behavior (Dishonest Use)	-0.0468 (0.109)	0.0839 (0.104)	0.0926 (0.104)
Others' Behavior (Honest Use)			
Group Treatments (Dummy)		0.330*** (0.103)	0.351*** (0.107)
Misreporting in Part 1			0.120*** (0.0370)
Observations	273	273	273
# of Clusters	117	117	117
Pseudo-R ²	0.280	0.358	0.396

Notes: Arguments relating to others being honest and arguments referring to Rules to encourage dishonest reporting have been dropped due to multicollinearity. Robust standard errors clustered on group level in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.3 CODEBOOK: VARIABLES FOR THE CHAT ANALYSIS

This section includes the codebook that was used by our coders.

- **Chat in general**

- *Duration*: Duration (# of seconds)
- *Messages*: # of messages
- *Average length*: Average length of a message (# of characters incl. spaces)

- **Content**

Which arguments are being made to persuade other group members? How often are they alluded to during the entire chat? ONLY COUNT ARGUMENTS THAT ARE EXPLICITLY MENTIONED (e.g. if "€" was displayed and participant says "I think we should be honest and should enter €" → honesty=1, "I think we should enter €" → honesty=0)

- *Money (Dishonest Use)*: Money discussed in favor of lying (# of times money/points/payoffs are mentioned as an argument)
Example: We should enter 5 because it will maximize our payoffs.
- *Money (Honest Use)*: Money discussed in favor of honesty (# of times money/points/payoffs are mentioned as an argument)
Example: We should enter 3, it will still give us 6 euros.
- *Honesty (Dishonest Use)*: Honesty discussed in favor of lying (# of times mentioned)
Example: There is no need to be honest.
- *Honesty (Honest Use)*: Honesty discussed in favor of honesty (# of times mentioned)
Example: Let's be honest.
- *Insecurity (Dishonest Use)*: Refer to misunderstanding/insecurity concerning the task in favor of lying (# of times mentioned)
Example: I am uncertain about the task, I thought we can enter any number we want to enter.
- *Insecurity (Honest Use)*: Refer to misunderstanding/insecurity concerning the task in favor of honesty (# of times mentioned)
Example: I am uncertain about the task, I thought we have to enter the number we saw.
- *Rules (Dishonest Use)*: Discuss that "we should stick to the rules" (# of times mentioned)
- *Rules (Honest Use)*: Discuss that "we do not have to stick to the rules" (# of times mentioned)

- *Others' Behavior (Dishonest Use)*: Reference to dishonesty of others outside the group (e.g. experimenter, people in general, other participants, ...) (# of times mentioned)
- *Others' Behavior (Honest Use)*: Reference to honesty of others outside the group (e.g. experimenter, people in general, other participants, ...) (# of times mentioned)
- *Positive Consequences*: Refer to positive consequences (# of times mentioned)
- *Negative Consequences*: Refer to negative consequences (# of times mentioned)

C.4 TRANSLATED INSTRUCTIONS

Appendix C.4 includes the translated instructions (from German). Participants received the general instructions for the experiment in print. All further instructions were displayed on the computer screen at the beginning of the respective parts (see screenshots). We added comments in squared brackets [].

*General Instructions [printed]***Welcome to the experiment and thank you for your participation!**

Please do not speak from now on with any other participant

General procedures

In this experiment we study economic decision making. You can earn money by participating. The money you earn will be paid to you after the experiment privately and in cash.







The experiment consists of three parts in which you will take independent decisions. At the beginning of each part, you will receive detailed instructions. If you have any questions about the instructions or during the experiment, please raise your hand. An instructor will then come to you and answer your questions privately.

During the experiment, you and the other participants will make decisions and possibly you may interact with other participants. That is, both your own decisions and the decisions of the other participants may determine your payoff. How your payoff is exactly determined will be explained in the on-screen instructions.

Payment

In some parts of the experiment, we are not talking about euros but about points. Your income for these parts is calculated in points. We will convert the points into euros at the end of the experiment, with an exchange rate of:

$$1 \text{ point} = 2 \text{ euros}$$

We will randomly select one of the three parts to be payoff-relevant at the end of the experiment. The participant at seat 21 will roll a die. If the die shows a  or , the first part of the experiment will be payoff-relevant. If the die shows a  or , the second part is payoff-relevant and if the die shows a  or , the third part will be payoff-relevant. As all parts are equally likely to be payoff-relevant, it is optimal for you to decide according to your preferences.

In addition to the income that you earn during the experiment you will receive 4 euros for showing up on time. Your total income will be paid to you after the end of the experiment in cash. We will call each participant based on her seat number for individual payment. That is, no other participant will learn your payoff and also you will not learn the other participants' payoffs.

Anonymity

The analysis of the experiment will be anonymous. That is, we will never link your name with the data generated in the experiment. You will not learn the identity of any other participant, neither before nor after the experiment. Also the other participants will not learn your identity. At the end of the experiment, you have to sign a receipt to confirm the payments you received. This receipt will only be used for accounting purposes.

On screen instructions: Instructions for Part 1

Instructions for Part 1

Die roll
 In Part 1 of the experiment, you will see a video of a die roll showing , , , , , or . The video you will see is randomly selected by the computer and each of the 6 possible videos is chosen with equal probability.

Your task
 Your task is to remember the number of the die roll in the video and type it into a field showing up later.

Your points for this task
 The die number you enter determines your points as follows:

Die number entered	"1"	"2"	"3"	"4"	"5"	"6"
Points	1	2	3	4	5	0

Comprehension questions

1. What is your task in this part?
 To enter the the number displayed which you have memorized
 To enter a different number than the number you have seen and memorized
 To enter an arbitrary number

2. Suppose that you see a and you enter a "3".
 How many points do you receive?

3. Suppose that you see a and you enter a "2".
 How many points do you receive?

4. Suppose that you see a and you enter a "4".
 How many points do you receive?

OK

Decision Screen Part 1 [displayed after subjects observed the video]

Now enter the number from the die roll in the video.

Number seen:

The die number you enter determines the number of your points as follows:

Number entered:	"1"	"2"	"3"	"4"	"5"	"6"
	=	=	=	=	=	=
Points	1	2	3	4	5	0

Belief elicitation after Part 1 [displayed after subjects entered their decision]

We now ask you to assess behavior in a similar experiment. Please answer the following questions as accurately as possible.
 With your assessment you can earn additional money. At the end of the experiment, a randomly selected participant will draw one coloured chip out of an opaque bag containing ten chips.
 The bag contains five red and five blue chips. If a red chip is drawn, you will receive a payment for your assessment.

In a similar past experiment, the participants received the following points given their die number:

Number entered	"1"	"2"	"3"	"4"	"5"	"6"
	=	=	=	=	=	=
Points	1	2	3	4	5	0

Please assess the payment (in points) of the participants of the similar experiment. If your assessment is right and a red chip is drawn, you will receive 5 €. For every percentage point you differ from the correct fraction, your payment is reduced by 4 cents. The smallest possible payment is 50 cents.

What percentage of participants received a payment of 0?

What percentage of participants received a payment of 1?

What percentage of participants received a payment of 2?

What percentage of participants received a payment of 3?

What percentage of participants received a payment of 4?

What percentage of participants received a payment of 5?

Post belief elicitation question [displayed on single screen after belief elicitation]

How sure are you about your assessment?

very unsure very sure

Instruction for Part 2: GroupNoPC

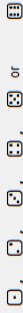
[For *GroupPC*, instructions were identical except for the fact that participants were informed that all group members receive a payoff of zero, if they do not enter the same number. For *Individual*, the instructions were identical to Part 1 with an additional sentence explaining that Part 2 is identical to Part 1.]

Instructions for Part 2

Part 2 of the experiment is similar to Part 1, except that you decide now in a group. We will randomly assign you to two other participants such that you form a group of three.

Die roll

In Part 2 of the experiment, you are going to see a video of a die roll showing



The video you will see is randomly selected by the computer and each of the 6 possible are chosen with equal probability. All members of your group will see the same randomly selected video.

Your task

Your task is to memorize the die number of the video and to enter it into a field showing up later.

Your points for this task

The die number entered determines your points as follows:

Die number entered	"1"	"2"	"3"	"4"	"5"	"6"
=	=	=	=	=	=	=
Points	1	2	3	4	5	0

The numbers entered by the other group members have no implications for your points and the number you entered does not affect the points of the other group members.

Comprehension questions

1. What is your task in this part?

- To enter the number displayed which you have memorized
- To enter a different number than the number you have seen and memorized
- To enter an arbitrary number

2. Suppose that you see a and you enter a "3". How many points do you receive, if...

1. Your group members enter the same number?
2. Your group members enter a different number?

3. Suppose that you see a and you enter a "2". How many points do you receive, if...

1. Your group members enter the same number?
2. Your group members enter a different number?

4. Suppose that you see a and you enter a "4". How many points do you receive, if...

1. Your group members enter the same number?
2. Your group members enter a different number?

OK

Instruction for chat [displayed after instructions for Part 2]

Group interaction

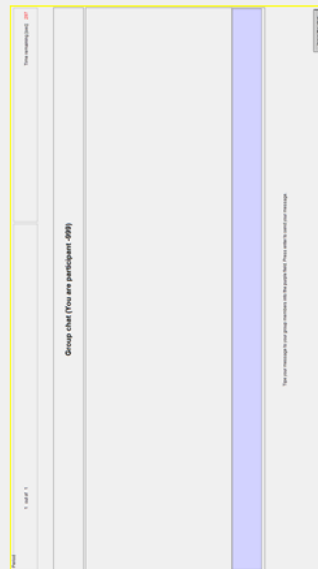
You have the possibility to chat with the other two group members via chat to clarify, which number each group member will type in.

You have 5 minutes to exchange information. The group discussion ends after the 5 minutes or as soon as all 3 members of the group have pressed the button "leave the chat". If only 1 or 2 members of the group press the button, the discussion will continue until all press the button or until the time runs out. If you have pressed the button "leave chat" but you do not want to leave the chat, you can press the button "back". After the group discussion, each member of the group enters a number on the screen.

Generally, the course of communication is up to you, although it is not allowed to mention any personal details. These include: name, age, sex (please use always gender-neutral terms), subject of study (this includes also specifically mentioning teachers, courses or course descriptions which allow an identification of the subject of study) or similar topics which could lead to your identification (e.g. your seat number or row). Furthermore, you are not allowed to agree upon side payments within your group. If you are breaking these rules, you will be excluded from the experiment and won't receive any payment for the entire experiment.

Within the given time, every group member can send as many messages to the other group members as he or she likes. Every message of you appears automatically on the screens of the other two group members. Messages to one single person are not possible.

The screen of the chat will look like this



To write a message, click on the purple field, enter your message and press "Enter". Then, your message appears in the grey field above the purple field. You can send as many messages as you want using the same procedure. The other participants will see the message only when you have pressed "Enter", i.e. when your message appears in the grey field.

Comprehension questions

Suppose that you have pressed the button "leave the chat". When do you leave the chat?

- Immediately
- When you are pressing "back"
- When all members of your group have pressed the button "leave the chat" or on expiry of time

Decision Screen in Part 2 and 3

[Decision screens in Part 2 and Part 3 were structured as in Part 1.]

Instructions for Part 3 and Belief elicitation after Part 3

[Instructions for Part 3 were identical to instructions for Part 1, except that answers to control questions were automatically selected and displayed. The belief elicitation after Part 3 was identical to the belief elicitation after Part 1, except for the color of the chip determining payoff-relevance, and it was explicitly mentioned that we are asking for the expected results for the same reference experiment.]

Instructions for the additional control treatment IndividualDeliberation

[Instructions and decision screens for Part 1 and Part 3 of *IndividualDeliberation* were identical to those from *Individual*. Instructions for Part 2 were identical to *Individual*, except that we added a sentence informing subjects that they have the option to write down their thoughts: "Part 2 is identical to Part 1, except that you have now the possibility to write down your thoughts." and an additional screen (see below) explaining how to write down one's thoughts.]

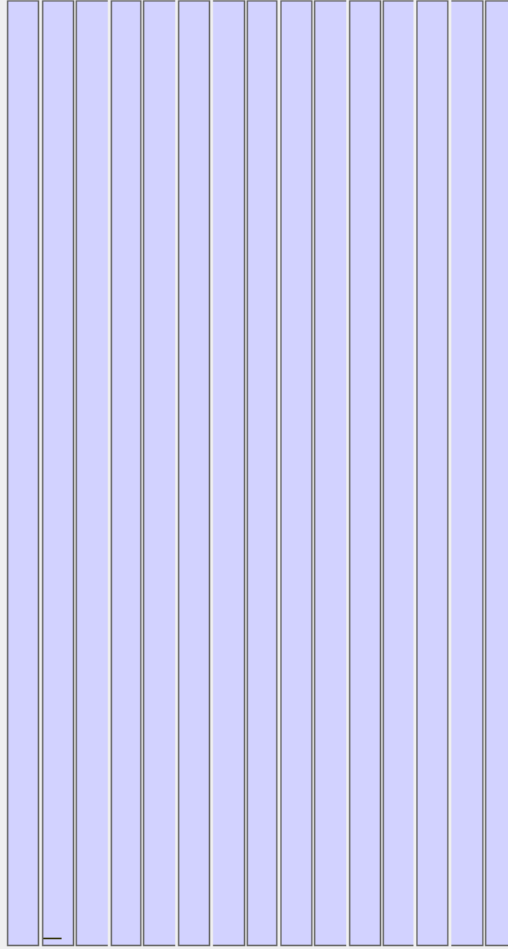
Procedures

You have the possibility to enter your thoughts concerning the number that has to be entered.

You will have 5 minutes time to write down your thoughts. After the 5 minutes, the possibility to enter your thoughts ends. If you have written down all your thoughts before the 5 minutes elapsed you can click on the button "Leave". After finishing, you will enter the number on screen.

Within the 5 minutes you may write down as many thoughts as you wish. You may formulate full sentences or use bullet points or short comments to express your thoughts.

The screen will look as follows:



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