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Saving, Microinsurance: Why You Should Do Both or Nothing. A Behavioral Experiment on the Philippines[*]

Andreas Landmann[†], Björn Vollan[‡], Markus Frölich[†‡]

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Abstract

This paper analyzes data from a novel field experiment designed to test the impact of two different insurance products and a secret saving device on solidarity in risk-sharing groups among rural villagers in the Philippines. Risk is simulated by a lottery, risk-sharing is possible in solidarity groups of three and insurance is introduced via less risky lotteries. Our main hypothesis is that formal market-based products lead to lower transfers among network members. We also test for the persistence of this crowding-out of solidarity. We find evidence for a reduction of solidarity by insurance if shocks are observable. Depending on insurance design, there is also evidence for persistence of this effect even if insurance is removed. Simulations using our regression results show that the benefits of insurance are completely offset by the reduction in transfers. However, if secret saving is possible solidarity is very low in general and there is no crowding out effect of insurance. This suggests that introducing formal insurance is not as effective as it is hoped for when the monetary situation can be closely monitored, but that it might be a very important complement when savings inhibit observing financial resources.

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[†] University of Mannheim [‡] University of Mannheim, Leibniz-Centre for Marine Tropical Ecology (ZMT) [†‡] University of Mannheim, Forschungsinstitut zur Zukunft der Arbeit (IZA), Zentrum für Europäische Wirtschaftsforschung (ZEW).

I. Introduction

A large majority of the population in the world's poorest countries is without formal insurance.[1] Shocks such as natural catastrophes, illnesses, economic crises, unemployment or crime, just to mention a few, destroy the economic basis of countless households. As a response, informal transfers within networks of friends, neighbors and relatives are important in the management of these income fluctuations, with transfers consisting of e.g. loans, monetary gifts, goods (such as food) or labor. These support schemes allow households to spread the effects of income shocks throughout their network or village. In this sense, the mutual support in case of a shock is an informal insurance mechanism relying on intrinsic motivation to act solidarily.[2] This intrinsic motivation is based on friendship and kinship, altruism, inequity aversion or reciprocity (Barr and Genicot, 2008).

There is ample evidence for the importance of such mechanisms in developing countries (e.g. compare Morduch, 1999; Fafchamps, 2008). Also on the Philippines risk-sharing networks play a major role and respondents may raise funds through gifts and loans (Fafchamps & Lund, 2003), where loans are often zero-interest or do not have to be repaid fully (Fafchamps & Gubert, 2007a). However, if other members are also suffering income shocks (covariate risk), it is more difficult for respondents to raise funds via informal ways. Furthermore, mutual insurance does not appear to take place at the village level; instead, households receive help primarily through networks of friends, relatives and those living close (Fafchamps & Lund, 2003; Fafchamps & Gubert, 2007b). Other evidence from developing countries around the globe suggests that informal insurance only smoothes a fraction of income shocks (Townsend, 1994; Morduch, 1999).[3] Moreover, some do not regard these transfers as genuine risk-sharing. Platteau (1997) for example argues that donors in fact often expect a return for their payment instead of truly internalizing the spirit of mutual insurance. Ultimately, effectiveness of risk sharing rests on the willingness of those who were lucky to look after the less favored. Many unlucky remain excluded.

[1] Besides social security, only between 0.3% (Africa), 2.7% (Asia) and 7.8% (The Americas) of the target population in the 100 poorest countries is covered with formal insurance available to the poor (Roth, McCord, & Liber, 2007, pp.15-19). Similarly, Banerjee & Duflo (2007) report that less than 6% of the extremely poor are covered by any kind of health insurance. Also, coverage with savings is rather low in developing countries. According to Banerjee and Duflo (2007) the fraction of people with savings account is below 14%.

[2] The ILO Micro Insurance Compendium (Churchill, 2006, p. 34) also mentions informal group-based mechanisms (burial societies etc.) as informal insurance. However, this already is a step towards formal mechanisms with an explicit obligation to pay contributions in order to receive benefits. We mean more flexible and non-contractual arrangements.

[3] Morduch (1999) summarizes some literature and concludes: "Most informal insurance mechanisms are typically weak and often provide only inadequate protection to poor households" (Morduch, 1999, p. 188).

These imperfections and drawbacks of informal mechanisms have made people consider how to remedy the situation for a long time. Some countries introduced universal insurance for some risks, e.g. free health insurance in India. Yet, even though public facilities should (at least by law) be for free under these schemes there have often been problems with low compliance, understaffing, corruption, quality of care as well as a high fiscal burden, which often led people to attend private clinics. Consequently, substantial interest in private insurance products remains (e.g. weather insurance, health, life). Especially the recent rise of microcredit and -saving concepts that has led governments, private banks, NGO's and mutual benefit associations to the question: can we apply these new concepts to insurance, designing products especially suited for poor clients? In this spirit, many microinsurance initiatives are currently being launched and several pilot schemes are already in the field. Despite this effort, demand for microinsurance is so far still very low (Cole et al., 2009; Giné & Yang, 2007; Ito & Kono, 2010) and practitioners are working to implement an affordable insurance design that complements traditional informal risk sharing schemes. However, the potential effect of formal microinsurance on informal mechanisms is still an open question.

There are good reasons to believe that formal insurance will reduce solidarity, and that a flawed formal microinsurance system could even have negative overall effects on economic stability under certain circumstances (e.g. dependent on the fraction of insured people, strength of network, etc.). It is well established in the economic and psychological literature (Bowles, 2008) that market-based mechanisms can crowd-out intrinsic pro-social behavior, and the introduction of formal insurance schemes can similarly reduce pro-social behavior. Instead of relying on the intrinsic motivated solidarity payments individuals can pay a price to benefit from more security. At least two of the causes for crowding-out identified in Bowles (2008) may apply: First, people could perceive the availability of costly insurance as a signal that 'buying' security is everyone's own responsibility (framing effect). Second, the fact that other people choose insurance might hint at their low commitment or trust in the existing solidarity transfer scheme and provoke a negative response by reducing one's own pro-social giving (information effect for conditional co-operators). The crowding-out effect might also lead to individualization and the breaking apart of traditional structures which also affects other spheres of life.

There is already some literature specifically suggesting that insurance might crowd out solidarity transfers within the network. However this literature is either theoretical (Attanasio & Rios-Rull, 2000) or is based on non-experimental data. In addition, transfers are not measured directly (Dercon & Krishnan, 2003; Jowett, 2003).[4] In observational studies it is also not possible to disentangle the process leading to the crowding-out effect. Are the insured reducing their transfers or the uninsured? Which are the motives that drive peoples' decisions?

This paper delivers the first experimental evidence on whether informal solidarity is reduced by formal insurance in developing countries. Our design tries to reflect reality as much as possible. We model risk in a behavioral game using lotteries that involve rolling a dice. Every participant is provided with an initial endowment and depending on the dice roll she is allowed to keep all or part of it. Informal risk-sharing is implemented in non-anonymous groups of three broadly following the design of Selten & Ockenfels (1998). After the lottery is played, each group member can transfer some money to the other group members. Insurance is introduced via offering alternative lotteries that are safer but require some ex-ante fixed payment. We test two variations of insurance; one protects only against catastrophic losses and the other is a more comprehensive type and covers more cases.

Since acts of giving are not always voluntary but are "demanded" from the network members (Comola & Marcel Fafchamps, 2010; Grimm et al., 2010), we implemented the possibility of secretly saving money in a "lockbox treatment" where players can pretend to have a medium shock (instead of no shock) and hide their money from the risk sharing group. The treatment relates to another aspect of microfinance: Resources allocated to (formal) saving products might be harder to monitor or might be regarded as non-liquid by the network. Formal saving thus reduces informal risk-sharing. In fact, many people in developing countries are willing to pay considerable premia and even accept negative real interest rates in order to keep their money at a safe place such as formal banks, rotating savings and credit associations (ROSCAs) or deposit collectors.[5] There is very little research on the effect of such saving mechanisms on informal risk sharing. Especially research focusing on the 'hiding mechanism' is completely missing up to now.[6]

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[4] Attanasio & Rios-Rull (2000) present a mathematical model explaining that under some conditions public insurance leads to a decrease in informal transfers. Dercon & Krishnan (2003) show that consumption is more responsive to shocks if there is food aid in rural Ethopia, and Jowett (2003) find that there is less health insurance uptake if informal financial networks and social capital are stronger (or vice versa). This empirical evidence is in line with crowding out of private by public support schemes, however, both studies are based on non-experimental data and transfers are not measured directly, amongst others. The research is made more difficult by the fact that the measurement of informal solidarity and transfers is a difficult task. Comola & Fafchamps (2009) for example use data where the receiver and sender both report on transfers. They show that the information from the two parties is largely inconsistent.

[5] In Africa, for example, especially women are willing to entrust their money with "Susu men" to withdraw it from their network (Besley, 1995: 2150) or to put it in formal saving accounts with effectively negative interest rates (Dupas & Robinson, 2009).

[6] To the best of our knowledge there are only two (unpublished) papers on the effect of savings on informal risk-sharing. Flory (2011) analyzes a randomized field experiment on the effect of a mobile van offering saving

In contrast to real world data the controlled environment of the behavioral game allows monitoring the transfers and choices of participants perfectly and thus delivers much more reliable empirical results.

We find that secret savings that inhibit the observability of shocks substantially reduce solidarity transfers. This is evidence for a strong role of extrinsic and for less intrinsic solidarity, as participants use the secret saving device most of the time and strongly decrease their willingness to transfer if it is available. Under these circumstances insurance does not lead to significant crowding-out effects on transfers and formal insurance is effective in significantly smoothing the loss distribution. This is not surprising, as solidarity is already low and additional crowding-out is difficult. However, if participants cannot secretly save and solidarity is relatively high there is significant crowding-out. The positive effect of the insurance mechanism on the lower tail of the distribution is completely offset by the negative effect of decreased solidarity transfers in this case.

The fact that the crowding-out effect can completely offset the protection offered by the insurance hinges on incomplete take-up. If everybody was insured nobody would be left with a catastrophic outcome even in the complete absence of solidarity transfers. Yet, while around half of all participants opt for insurance if they have the choice, there is a substantial part remaining uninsured. Those uninsured now face a much higher risk of being left alone with a bad outcome than in the scenario when nobody can be insured.

In sum, insurance is ineffective when no saving device is available and solidarity is potentially high, or effective when there secret saving is possible and informal solidarity is limited. One lesson is to offer insurance if saving devices are in place or should be introduced (for reasons beyond the scope of our experiment). In fact, under these circumstances insurance decreases the risk of bad outcomes to levels without the saving device, thus exactly offsetting the breakdown of informal solidarity. On the other hand, when resources within the risk-sharing network can be closely monitored and solidarity is high introducing market-based insurance products with incomplete coverage of the whole population could have disappointing results – causing administrative costs without affecting overall vulnerability.

services, amongst others, in India. Surprisingly, he finds a positive effect of savings on the incidence of gifts received by poor non-savers, but no effect on the amount. Chandrasekhar, Kinnan, & Larreguy (2010) conducted a behavioral field experiment in India to test the effect of saving, focusing on its inter-temporal income smoothing role. They find no effect of saving on risk-sharing. Yet, they make saving perfectly observable and only test its effect when risk sharing is already low due to a limited commitment treatment.

The remainder of the paper is organized as follows. Section II describes the experimental setup including treatments, hypotheses, implementation and subject pool. We discuss empirical results in section III and conclude in section IV.

II. Setup of the experiment

We model risk in a behavioral game using lotteries that involve rolling a dice.[7] Every participant is provided an initial endowment of 200 Philippine Pesos (PhP) and depending on the dice roll she is allowed to keep all or part of it.[8] This design reflects the risk to lose money instead of providing participants with the possibility of winning money.[9] Informal risk-sharing is implemented in groups of three according to the standard solidarity game procedure (Selten & Ockenfels, 1998).[10] Contrary to most economic lab experiments we do not restrict our sample to students, nor do we make groups anonymous. The participants are rural villagers in the Philippines. We are convinced that this is more compatible with the idea of risk sharing at the village level and strengthens external validity of our results. After the lottery is played, the group is allowed to talk. Thereafter each member of the group can transfer some of his money to each of the other group members. Insurance is introduced via offering alternative lotteries that are safer but require some fixed payment ex ante.

- Treatments -

We test *two variants of insurance*, compared to no-insurance. One insurance protects against half of all loss types and is more expensive, while the other insurance covers half of catastrophic shocks only.

We first explain the no-insurance treatment, which we refer to as Option A. Every participant has an initial endowment of 200 Philippine Pesos. This is the amount paid out if the dice roll gives a 1, 2 or 3, i.e. no shock (no loss). If the dice shows a 4 or 5 a medium shock occurs

7 We benefited from the work of Barr & Genicot (2008) who combine a lottery choice with risk-sharing after the result is determined. They test different enforcement mechanisms in their experiment and find strong evidence for intrinsic motivation of giving, as substantial risk sharing takes place even if individuals can secretly opt out of the solidarity group. However, in their experimental procedure, lottery choice is not a treatment, so the effect of introducing insurance cannot be identified. Also, interpretation of the many gamble choices (according to Binswanger, 1980) as different insurance products is difficult. There are other experiments that come closer to our idea. Trhal & Radermacher (2009), for example, compare solidarity in treatments with and without gamble choice. Yet, the 'non-insurance' lotteries are not the same in both treatments and some other details do not fit our purpose. We consequently designed a novel behavioral experiment that is described in the following.

8 The amounts where such that the expected payoff of participating in the experiment (237 PhP, or about 5-6 USD) equals about one day of minimum wage in the formal sector. The expected amount includes a show-up fee of 100 PhP that every participant received for sure.

9 Harrison & Rutström, (2008: 90) stress the importance of the reference point, referring to prospect theory (Kahneman & Tversky, 1979) that allows subjective probability weighting, a reference point and different utility functions for losses and gains.

10 There are problems with the 3-player approach since often winners do not anticipate that the other winner might also give. This leads to the strange situation that the player with the worst shock leaves the experiment with the highest earning. However, this happened only eight out of 279 times. We also believe that two player relations are different from risk-sharing groups and thus not adequate for our experiment.

(losing half), and a 6 implies a catastrophic shock (losing almost everything). If the medium shock occurs participants lose 100 of their initial 200 Pesos and if the catastrophic event occurs they lose 180. In case of no shock, participants do not lose any money, but can keep all their 200 Pesos.

The two insurance variants are called option B and option C.[11] For option B participants have to pay 45 Pesos in advance and half of all losses are covered. The price for option C is only 20 Pesos, but half of only the catastrophic loss is covered. (The prices 20 and 45 are chosen to reflect the higher administrative costs of more comprehensive insurances schemes in reality. The more comprehensive insurance covers more shocks and is therefore confronted with more claims, and also higher administrative costs, which translates to lower expected payoffs.) Table 1 shows the payout for the no-insurance case and the two insurance options B and C.

The advance cost of insurance thus is always the 'guaranteed loss' in case of no shock. In general, different options lead to a different spread of payoffs; the lower the standard deviation, the lower the expected total payoff. Option B is most costly, not only regarding absolute price but also when looking at the expected loss. Yet the risk – as represented by standard deviation of the payoff – is smaller than in option A and C. Option C is an intermediate case with an interesting additional feature: Due to the low price and the focus on the catastrophic loss it can secure an even higher minimum payoff than option B. Because of this, individuals with minimax preferences would prefer C over B. Both options B and C reflect typical insurance products where full coverage is impossible. E.g. in most developing countries, health insurance covers only the medical expenses (often below 100%), but not lost income due to lost labor. The more comprehensive insurance mimics the state owned medical insurance scheme and the catastrophic insurance reflects different rainfall or crop insurance in the region. With two insurance products, we are able to discuss demand for different insurance products and create a different take-up which might lead to more or less crowding-out. We are also able to detect product inherent effects that interact with solidarity.

[11] We would have expected a higher crowding-out effect by labeling the lotteries as "insurance" instead of "option" but decided to leave this for future research.

Table 1: Losses (in PhP) under different (insurance) options

Dice Result	1,2,3: no shock	4,5: medium shock	6: catastrophic shock	Expected Loss	Std-Deviation of Loss
Option A	**- 0**	**- 100**	**- 180**	-63.3	68.7
Option B	**- 45**	**- 95**	**- 135**	-76.7	34.4
Option C	**- 20**	**- 120**	**- 110**	-68.3	48.5

Note: The initial endowment is 200 PhP in each round. The loss in case of "no shock" is the price of the insurance options participants have to pay upfront, i.e. 45 PhP for option B and 20 PhP for option C.

In real life, observing what everybody gives to you is normally unproblematic, but perfectly observing individual shock levels of others is maybe not possible. Thus we decided to allow participants to pretend a negative shock. Catastrophic losses might on the other hand be observable to everybody. Therefore, observability of medium shocks was reduced in a *secret saving* treatment.[12] If the dice result was 1, 2 or 3 (no shock) individuals could decide to save the monetary difference to a medium shock in a secret lockbox. This information was private to the individual and group members were only told the amount the person had left after the lottery/lockbox stage. Saving in the lockbox thus made it impossible for the co-players to distinguish between no shock and a medium shock. The aim of this treatment is to increase external validity of our study and to show the effect of secret saving on solidarity, a potential side effect of (formal) saving products. It is still possible for people with no shock to help others in case of need, but a lot of solidarity based on peer pressure will be reduced.

To test the effects of the two insurance types and of the secret saving device, the behavioral experiments were implemented as outlined in Table 2. In six villages (treatment block A) no insurance is offered in round one and two. Hence, participants have no choice and always play option A. In round three both insurance types are introduced and participants can choose between all three options. In eight villages (treatment block AB), insurance option B is offered in round one, no insurance in round two and again insurance option B in round three. In another eight villages (treatment block AC), the same is done with option C. In order to test the main and interaction effects of the secret saving possibility, the secret saving device was implemented in the first two rounds in half of all villages.[13]

[12] To not influence participants we did not call this 'saving', but rather explained the possibility to "put money in a lockbox" and the related mechanics.

[13] In the third round it was removed again in most cases. We intended to analyze persistence in its effect by this design choice, but we do not find any evidence.

It is important to note that each group plays all three rounds, but that for only one of these three rounds a real payout takes place. The round that is being paid out is randomly chosen *after* all three rounds have been completed. The participants knew this in advance. Hence, apart from possible learning effects, no dynamic, strategic or endgame effects can occur.

Table 2: Treatment plan for insurance types

	Block A (6 villages)	Block AB (8 villages)	Block AC (8 villages)
Round 1	Option A	Choice: Option A or B	Choice: Option A or C
	3 save, 3 no save	4 save, 4 no save	4 save, 4 no save
Round 2	Option A	Option A	Option A
	3 save, 3 no save	4 save, 4 no save	4 save, 4 no save
Round 3	Choice: Option A, B or C	Choice: Option A or B	Choice: Option A or C
	3 save, 3 no save	8 no save	8 no save

Note: In each block in half of the villages the games were played with the "secret-saving device" and in the other half without. This is indicated by "3 save, 3 no save" or "4 save, 4 no save". The notion "8 no save" means that in Round 3 the secret-saving device was not available anymore.

Our main hypotheses formulated prior to conducting the experiment are:

(I) Solidarity transfers are reduced by the availability of insurance.

(II) There is a persistent reduction of solidarity even if insurance is removed.

(III) Solidarity transfers are reduced by the possibility of secret saving

The effect of the different insurance types can be tested by comparing treatment block A versus block AB versus block AC in the first round (Hypothesis I). Treatment A serves as a control here. The persistent effect of access to insurance on solidarity can be tested in the second round (Hypothesis II). The effect of secret saving can be assessed by comparing the 11 villages, where secret saving was possible, with the other half (Hypothesis III). Besides these three hypothesis the experiment permits the analysis of many more aspects. The third round allows for a comparison of demand for the insurance variants while competing in one market in block A. This is the only scenario where participants can choose between two insurance options. In the other scenarios only insurance versus no-insurance is possible. With the simultaneous availability of two insurance options we can examine how individuals choose from a menu of insurance options. Additionally, it is possible to compare take-up of option B and option C on separate markets with changed ability to observe shocks (secret saving).[14] Furthermore, the third round also delivers more observations for the pooled regressions at a later stage.

As an additional treatment, different *network strengths* of the player groups is examined. The groups of player were formed in two ways: In half of the villages, a randomly selected person had to invite two other household heads that he knows very well to join the experiment. The players who knew each other very well would form a solidarity group. We will refer to these as *endogenous* groups or as *strong* networks. In the other half of the villages, however, we implemented an *exogenous* treatment where groups were mixed up and participants would play with two random partners from their village.[15] The analysis of this additional covariate (*network strength*) is not the core of this paper and it is impossible that it causes a bias of the main treatment effects because it is balanced across insurance treatment blocks. However, we will control for it in regressions since it is not exactly balanced in each subcell (e.g. in Block A there are only 3 villages with saving and 3 without saving) and because there might be interesting interactions between network strength and the main treatments.

- Implementation -

All participants were assigned to groups of three and received player numbers upon arrival. The composition of groups was done in two ways: In half of the villages, they would remain in their self-selected groups of three, i.e. they had registered themselves together with two friends (see details on the recruitment below in the description of the subject pool). In the other half of the villages, they were randomly re-assigned to two new co-players. To indicate the group-allocation-scheme, we will later use a dummy variable *Exogenous Group* that takes the value 1 in the latter villages and the value 0 otherwise. This is to indicate that in the former villages, the groups were self-selected (stronger network) whereas in the latter villages, the groups were randomly – i.e. exogenously – assigned (weaker network).

[14] In treatment blocks AB and AC the research team changed the ability to observe shocks (described above as secret saving) in some of the villages to test more hypotheses, e.g. persistence of the secret saving device effect.

[15] Differences between the two types of groups can be found in participants' self-assessed relation to their group members. While more than 55% described their co-players as "close family" in endogenous groups, less than 30% do so in exogenous groups. Even though we do not believe that participants were very accurate in their classification (30% seems very high for groups formed at random), the differences between the two types of groups is considerable.

The groups stayed together for all three rounds and people in a group knew the other two members. After answering the pre-questionnaire, participants were seated to receive the introduction to the game. In an effort to make the rounds as independent from each other as possible, we made sure that signaling, punishment and the like cannot take place. Therefore decisions of co-players were not revealed and we did not allow for communication after the transfer choice. Group members were even seated separately to inhibit communication. The instructor pointed out that communication within groups is forbidden outside the communication stage, that violations of the treatment protocol will lead to the exclusion from the experiment, that three games will be played independently from each other and that only one of them will be paid out at random.[16]

The complete experimental procedure of one round is summarized in Figure 1. First, the instructor explains the game to all participants jointly, and everybody receives a plastic envelope with graphical instructions for this round and their initial endowment of 200 PhP in play money. Before participants go to a private room 1, they answer a set of questions in order to test their understanding of the game.[17] If the current round permits insurance options (see Table 2), participants are given a choice of lotteries. Otherwise only the standard lottery is available.[18] After the participants make their lottery choice and pay the related price, they roll a dice to determine the loss. Where secret saving was available, players with no shock could then decide to hide a fixed amount of their money or not. After all have chosen their amount to hide, the members are allowed to talk for approximately five minutes, before each individual separately goes to another private room 2. At this point, the amount that the two co-players took out of the first private room is revealed (endowment, minus insurance premium, minus loss due to shock, minus secret saving). Only the *net payout* is revealed, and not whether insurance has been bought, or whether shocks took place or whether secret savings have been made. From these payouts, however, one can induce who has purchased insurance and who did not. The participant then decides if and how much to give to each of the co-players. Everybody is completely free in the way he or she shares the money. These transfers are *never revealed* to anyone. Only after all three rounds have been completed and after one round has randomly been chosen for pay-out, do the players receive any feedback:

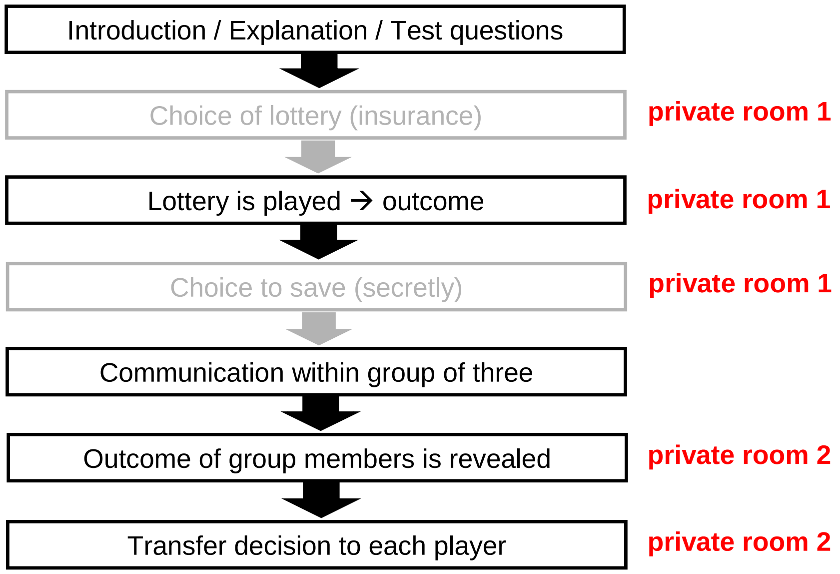
[16] The amounts where such that the expected payoff of participating in the experiment (237 Philippine Pesos, or about 5-6 USD) equals about one day of minimum wage in the formal sector. The total amount includes a show-up fee of 100 PhP for every participant.

[17] The test questions can be found in the appendix. When participants made mistakes, the research assistants explained the setup once more. Only those who finally answered all questions correctly were allowed to participate, but fortunately we only had to exclude few participants.

[18] Option A is not framed as the default option, but lotteries are instead assigned neutral names: Angola (A), Botswana (B) and Cameroon (C). However, participants knew that one option is for free, while potential alternatives would require an ex-ante payment from the initial endowment.

They receive cash in hand and from the received cash they can partly deduce whether they have received any transfers, but without knowing from whom. Hence, transfers from the past cannot affect the behavior in future rounds.

Figure 1: Experimental Procedure



To ensure that experimental conditions did not change, the same team of assistants was employed for the same job all the time, strictly adhering to the experimental protocol (i.e. the same person always read the protocol, the same assistants were sitting in room 1 and room 2 etc). In both private rooms, decisions were recorded by the research team. Communication within a group was restricted to the communication stage. Whenever there was an unclear situation, the researcher was present to decide on the issue. After all three rounds had been played, and after the completion of the post-game questionnaire and the random determination of which round to be paid out, the participants were handed out their winnings in *private*. All participants received a fixed show-up fee of 100 PhP in addition to their payoff from the relevant round.

- Subject pool -

The experiment was conducted in the Western Visayas (Region VI), in the province of Iloilo. Existing databases suggest that the region is a slightly disadvantaged but not unrepresentative region within the Philippines.[19] A two-stage random sampling procedure was applied throughout. First, we randomly determined the experimental sites, and then we drew

[19] The Demographic and Health Survey 2008 and a household survey conducted by the University of Mannheim in 2009 that is available to the authors suggest the following: educational attainment is slightly below national average, poverty is higher and coverage with public health insurance is around average.

participants within the selected barangay (lowest administrative level on the Philippines and often comparable to a village regarding size and structure). The exact combination of treatments played in one location according to the treatment plan was also determined randomly, but the randomization had to pass a balancing test regarding village size across the treatments.

The target population consists of low-income households in rural or partially urban areas. We therefore drew a random sample of 22 barangays whereby municipalities from the first income class (high income) and urban locations were excluded from the sampling process.[20] Also very small (population below 500) and very big (population higher than 3000) barangays were not considered to make the sample more homogenous.[21] Permission of the Punong Barangay (elected village representative) to conduct the research was obtained in all but one barangay, leading to its replacement by another random site. We made all possible effort to visit also remote locations, and all 22 locations of the sample could finally be reached.

In the second sampling stage, the households were randomly chosen within a barangay. Our recruiters went to the location some days prior to the experiment, asked the barangay officials for permission to run the experiment, ensured the availability of facilities for the games and requested a list of households from which eight households were randomly selected.[22] The recruiters then noted the names of the eight households and handed out invitation letters to them. Only the household head or the spouse of a household – in special cases also adult children still living in the household – were allowed to take part in the game. We also checked with the Punong Barangay whether the invited household representatives are too old to participate.[23] Each invitation had two additional invitation letters attached as well as the instruction to invite representatives from two more distinct households by choice. The sample size varied from 15 to 24 per village. The total number of observations is 466.

[20] Income Classification based on Department of Finance Department Order No.20-05 Effective July 29, 2005 (source: http://www.nscb.gov.ph).

[21] Four of the 22 barangay were already chosen at random for an earlier household survey. To link the data from both studies they were included even though one barangay was slightly too small (350) and another one slightly too large (3123).

[22] Every barangay was able to provide a complete household list.

[23] Our preferred age was between 18-60 years, but we mainly relied on the judgment of the Punong Barangay regarding the fitness of participants. Participants above age 70 are not considered, though.

Table 3: Descriptive statistics of participants

	All (N=466)				A (N=132)	AB (N=167)	AC (N=167)
Variable	Mean	Std.	Min	Max	Mean	Mean	Mean
Male	0.31		0	1	0.30	0.29	0.35
Household head	0.31		0	1	0.24	0.30	0.37**
Married	0.81		0	1	0.83	0.80	0.80
Highest education: highschool	0.44		0	1	0.49	0.48	0.37*
Highest education: college or above	0.25		0	1	0.23	0.30	0.21
Age (in years)	42.7	12.13	18	69	42.7	41.2	44.2
Regular monetary income? (dummy)	0.23		0	1	0.23	0.25	0.22
Skipped meals in hh last month	0.30		0	1	0.30	0.23	0.35
In debt with more than 1000 Pesos?	0.57		0	1	0.55	0.64	0.51

Stars indicate significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Descriptive statistics of the participants are presented in Table 3. Most of them are female (69%), and therefore the share of household heads is only 31%. Educational level is relatively high with more than two thirds having attended at least high school (44% stopped at this level and an additional 25% reached college). Below 18 year olds were not allowed to take part in the game and senior individuals with 70 years and above are excluded from the analysis. Regarding the financial situation of households, less than a quarter report regular monetary income. Also, in 30% of households members had to reduce meals for financial reasons in the last month, which serves as a rough measure of poverty. 57% are in debt with more than 1000 Pesos, the equivalent of roughly 22 US dollars.[24]

Due to the randomized assignment to treatments, we expect that all characteristics should be balanced, but in reality some small-sample correlation can remain. For example there is a higher share of household heads in treatment AC than in the control A and the educational attainment is slightly lower. The same is true for village characteristics, shown in Table A1 of the appendix. Especially income class of the municipality is somewhat different by chance across treatment blocks. Otherwise most characteristics are balanced. Nevertheless, the small-sample correlation in some characteristics hints at the importance of controlling for covariates in regressions.

[24] Around half of them owe the money to friends or relatives.

III. Empirical Results

In the following, we will first consider some descriptive results on the effect of insurance and secret saving using the comparisons implied by the treatment plan (compare Table 2). Afterwards we will control for different shock distributions[25] across treatments via matching, before employing a parametric regression model to control for more potentially confounding covariates and to gain further insights. Using the regression results we simulate loss/payout distributions under different insurance and saving regimes.

- Descriptive results -

A first finding is that the safer lottery options are frequently demanded by participants: On average 46% 'buy' insurance if they have the possibility to do so. Figure 2 illustrates the demand in treatment blocks AB and AC by round, and shows that lottery type C is more popular than type B, especially in the last round. It is interesting to note that the demand for insurance C increases from Round 1 to 3, whereas the demand for insurance B decreases. Since the participants did not receive *any* information or feedback about received transfers during the game, their change in behavior can only be explained by learning or by experienced shocks. It appears that while buyers of insurance C where happy with what they bought (insurance against catastrophic losses), for many buyers of insurance B the product might have been too expensive. This difference in 'client satisfaction' is reflected in different retention rates from round 1 to 3. While 72% of the insured with type C in the first round chose insurance in round three again, only 57% renewed their insurance B.

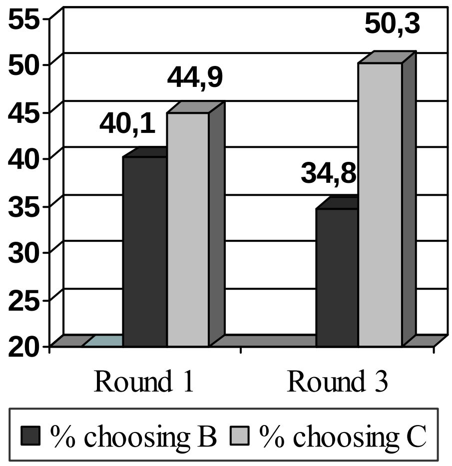
Figure 2 and its interpretation up to this point focus on treatment blocks AB and AC where different insurances are offered on separate markets. Numbers should not be mixed with the situation when type B and C compete in one market, because then samples in round 1 and 3 would not be comparable any more. When separately considering competition in one market (treatment block A in round three), demand for type C (43.4%) is also clearly higher than for type B (17.8%).

Characteristics of the insured versus non-insured by insurance treatment block (again using block AB and AC) in rounds one and three can be found in Appendix I (tables A4 and A5). While no clear picture emerges across all comparisons the poor seem to have a slightly higher tendency to take up (the insured tend to be more indebted and have to skip meals more

[25] By the nature of the experiment, the shock distributions would be identical across treatments if sample size was sufficiently large. But given our sample size, imbalances occur.

often). This suggests that low-income participants have more need for money from the experiment.

Figure 2: Demand for insurance on separate markets

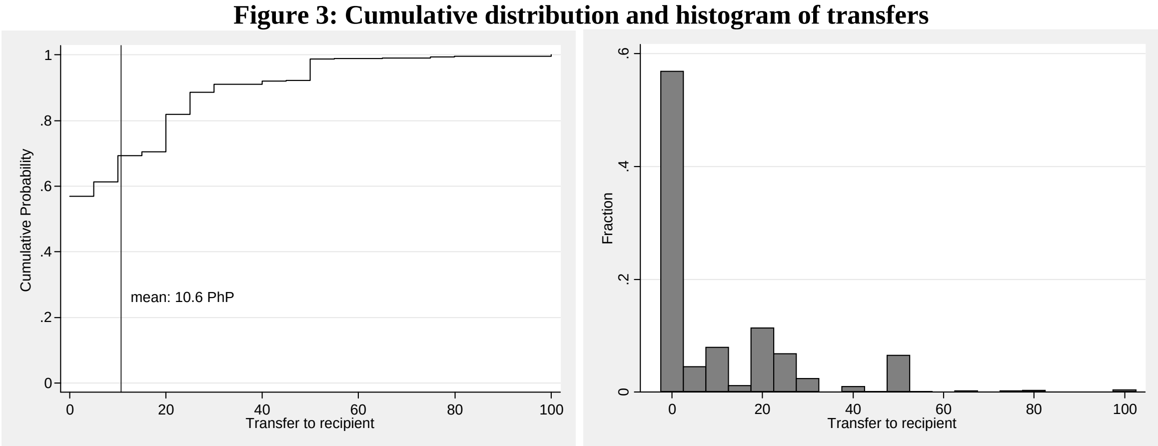


Note: only treatment blocks AB and AC (without block A, round 3)

Before we examine transfers between group members in the following, we note that "secret saving" was used overwhelmingly. Whenever secret saving was possible, in almost all cases (94%) it was used.[26]

Now, we examine transfers between group members. These transfers vary greatly between 0 and 100, with a mean of 10.6 pesos. Figure 3 displays the distribution of the 2730 observed payments from sender to recipient.[27] In 57% of all cases transfers are zero. The standard deviation is 16.5 pesos and the mean is 10.6 pesos.

Figure 3: Cumulative distribution and histogram of transfers



[26] Remember that participants can only save and thus pretend a medium shock if they have no shock.
[27] Each participant of the 466 makes two transfer decisions per round. However, one group dropped out in round two and another group in round three, because at least one player could not continue the game due to sickness or personal reasons. Also transfers from and to participants older than 69 are excluded.

The transfers are described in a compact form in figure 3; however they do not necessarily indicate effective redistribution of the money, as every group member can transfer to the other *and vice versa*. Let T_{ij} be the transfers from player i to j. Real redistribution is the result of *net* transfers, that is transfers from player i to j minus transfers from j to i ($T_{ij} - T_{ji}$). Therefore it will not be sufficient to compare average transfers across treatments, as they might simply reflect a different inclination to give in general, which is completely irrelevant for redistribution.[28] Solidarity works (in the sense of risk sharing) if the better-off give more to the worse-off than the other way around. For the descriptive analysis of the treatment effects we will therefore start with a comparison of net transfers from those with a zero or less severe shock to those with a more severe shock. This means we only look at net redistribution from those without a shock to co-players with at least a medium shock, and of those with a medium shock to co-players with a catastrophic shock. We refer to these as "net transfer to disadvantaged co-player".

Table 4 shows the average net transfers to the disadvantaged co-player by round and treatment block. Net transfers in treatment blocks AB and AC are shown relative to block A, net transfers in the secret saving treatment relative to the no saving case. Remember that the comparison in the first round allows testing the effect of different insurance types by comparing treatment block A with block AB and with block AC in the first round (Hypothesis I). Treatment A serves as a control. (Note that in these cases participants never could choose between insurance B and C. They could only choose between one insurance type versus option A.) One should also keep in mind that these comparisons across treatment blocks give the effect of *insurance availability*, not of take-up itself.

The persistent effect of insurance on solidarity can be tested in the second round (Hypothesis II). In round one, insurance was available in the blocks AB and AC. However, in the second round, insurance was not available anywhere. Hence, one should not see any difference in transfers between blocks A, AB and AC, unless the availability of insurance in Round 1 had a persistent effect. Finally, we can also estimate the effect of the secret saving device by comparing saving with no saving treatment in both rounds (Hypothesis III).

The third round does not play a role for the descriptive comparisons, as the design is not suited for nonparametric identification of the insurance effect (neither insurance availability nor its persistence). First, there is no control group, because both insurance types

[28] Imagine a treatment that leads *all* participants to give more. If this effect is the same for the better- and the worse-off, the two level effects will just cancel out after mutual transfers and redistribution is unaffected.

are available in the third round of treatment block A. Second, the comparisons are not balanced, as observability of shocks (secret saving device) was changed in the third round of treatment blocks AB and AC (compare treatment plan in Figure 2).

Table 4: Net transfers to disadvantaged co-players

Round	Variable	All Mean (Std)	BlockA Mean	BlockAB Mean	BlockAC Mean	Block AB vs. A Difference	Block AC vs. A Difference	No saving Mean	Saving Mean	Saving vs. no saving Difference
1	Net Transfer to recipient	8.6 (20.8)	10.9	9.8	5.7	-1.1	-5.2*	12.2	4.7	-7.5**
	No. obs	270	68	107	95	175	163	141	129	270
2	Net Transfer to recipient	15.1 (23.5)	16.1	18.3	11.7	+2.2	-4.4	19.9	10.4	-9.5***
	No. obs	282	69	101	112	170	181	140	142	282

Stars indicate significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Net transfers to disadvantaged co-players (Only sessions where saving was not possible)

Round	Variable	No saving subsample					
		All Mean (Std)	BlockA Mean	BlockAB Mean	BlockAC Mean	Block AB vs. A Difference	Block AC vs. A Difference
1	Net Transfer to recipient	12.2 (21.2)	18.4	10.4	9.1	-8**	-9.3*
	No. obs	141	40	52	49	92	89
2	Net Transfer to recipient	19.9 (23.5)	23.4	23.9	14.2	+0.6	-9.2*
	No. obs	140	37	47	56	84	93

Stars indicate significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

On average, participants redistribute 8.6 pesos in the first round and 15.1 pesos in the second round from the better-off to the worse-off. Solidarity thus seems to work in tendency. However, net transfers vary greatly in both rounds which leads to high standard deviations. Still, a Wilcoxon ranksum test statistic shows significantly lower solidarity transfers at the 10% level when insurance type C is available. The differences in the second round, i.e. when no insurance is available in any villages, are not significant. On first sight, this would indicate that there are no persistent effects of insurance, i.e. that the option of insurance in the first round would not affect behavior in the second round. However, as we will see below, this result changes once we later control for the option of secret savings in Table 5.

When we examine the effects of the possibility of secret saving (in the last three columns of Table 4), we find that the option of secret saving has a very strong and significant *negative* effect on solidarity. We also had noted before that almost all people (94%) make use of the secret saving option when possible. This rather strong finding indicates that if people know that they themselves as well as others can hide some resources, net transfers break down by around two thirds in round 1 and around half in round 2. These estimates are significant at the 5% and 1% level, respectively.

In Table 5 we compare across treatment blocks, using only those sessions where secret saving was not possible. We thereby estimate the effects of insurance availability, conditional on no possibility of secret saving. Hence, shocks are *fully* observable to other participants. Now we find that the effects of insurance become much more pronounced. Table 5 shows that the effects become larger and more significant. Here both treatment effects are significant at the 5% (AB) and 10% (AC) level, respectively, and have about the same size. Results suggest that net redistribution with insurance is only around half of what it would be without formal insurance.

We also find a persistent effect of insurance into the second round. In the second round, no insurance is available anywhere and any differences in mean transfers can only be due to persistent effects (Hypothesis II). Here we find that availability of insurance C displays a marginally significant persistent effect in the second round. This is not the case for the more comprehensive type B, though. While we do not have conclusive evidence to explain this, we suppose that the framing effect (availability of insurance signals that participants are no longer responsible for smoothing shocks) is more persistent for insurance type C, as it is much cheaper and thus might be considered affordable to all. We provide additional information with the regression results and a more detailed discussion in section IV.

Repeating the exercise for those villages with the secret saving possibility leads to no significant differences (compare Table 6). Also note that the level difference between treatment blocks A and AB in the first round – though not significant – might well be explained by the lower amount that can be secretly saved with insurance B.[29] In general it is perhaps not surprising that we cannot observe further crowding-out if net transfers are already dramatically lower on average in the subset with the secret saving device (4.7 versus 12.2 PhP in round one, 10.4 versus 19.9 PhP in round two).

[29] Remember that to guarantee non-observability participants can always save the difference to the medium shock. This difference is lower (50 instead of 100 PhP) if participants are insured against half of the medium loss (with insurance type B). This fact is an additional reason to control for the amount saved in regressions.

Table 6: Net transfers to disadvantaged co-players (Only sessions with saving)

		Saving subsample					
		All	BlockA	BlockAB	BlockAC	Block AB vs. A	Block AC vs. A
Round	Variable	Mean (Std)	Mean	Mean	Mean	Difference	Difference
1	Net Transfer to recipient	4.7 (19.7)	0.2	9.2	2.1	+9.0	+1.9
	No. obs	129	28	55	46	83	74
2	Net Transfer to recipient	10.4 (22.6)	7.7	13.4	9.1	+5.8	+1.5
	No. obs	142	32	54	56	86	88

Stars indicate significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

When interpreting the descriptive results, we need to keep in mind that the amount of redistribution is likely to depend on the degree of inequality in the group. For large sample sizes, the distributions of the dice-rolling-results would be equal across treatment blocks and rounds. For our small samples, though, the shock distributions implied by the dice rolling results are not exactly balanced. To deal with this issue, we proceed in two ways. First, we examine in the following nonparametric matching estimates, where transfers are only compared for identical shock situations. Second, we will thereafter use parametric regression models to control for the shock distribution.[30]

- Matching estimation results -

A nonparametric way to control for different shock distributions across blocks is via exact matching. We do this separately for comparing block AB versus A, and for comparing block AC versus A, and separately for round one and two, respectively. For every sender and recipient pair in treatment block AB with a certain shock combination, we look for a sender and recipient pair in treatment block A (control) with exactly the same shock combination. In addition, we also require that the shock of the third group member is also matched. We furthermore also require that the round number, the network strength and the availability of the saving device are the same. We test the effect of saving in almost the same way, simply making secret saving the treatment variable and adding insurance availability and type as a

[30] Comparing the shock dispersion across treatments and rounds does indeed reveal differences in the shock dispersion that are significant at the 10% level in some cases. As this is a result of dice rolls, it is by definition pure chance and large differences should never be present in large samples. However, in our case this is a small-sample correlation that might nevertheless bias results. Figures are not shown here but can be obtained from the authors upon request.

control variable. Table 7 shows the average treatment effect on the treated (ATT) of the two insurance treatment blocks and the secret saving option using exact matching, separately for round one and two. The last two columns of Table 7 repeat the insurance treatment matching for the subset of villages without the secret saving possibility.

The results of Table 7 strongly confirm the results of the previous tables, often with higher significance levels. Availability of insurance type C is associated with lower solidarity transfers from the better to the worse-off. When restricting attention to villages without the secret saving lockbox we see larger effects, and also the persistent effect of insurance type C is significant (at the 10% level). Effects are insignificant for the more comprehensive scheme B. Matching for the set of villages with a secret saving lockbox again does not show any significant effects (results shown in table A2 of the appendix). Effects of the secret saving device are negative, large and highly significant in both rounds.

Table 7: Average treatment effect on the treated (ATT) of treatment blocks on net transfers to disadvantaged co-players (all and without saving)

		all			no saving	
Round	Outcome variable	Block AB vs. A (ATT)	Block AC vs. A (ATT)	Saving vs. no saving (ATT)	Block AB vs. A (ATT)	Block AC vs. A (ATT)
1	Net Transfer to recipient	-1.2	**-7.9****	**-7.4*****	-5.2	**-11.9****
	Obs (On/off support)	80 / 27	81 / 14	123 / 6	43 / 9	42 / 7
2	Net Transfer to recipient	+4.2	-3.9	**-10.1*****	-1.5	**-11.0***
	Obs (On/off support)	84 / 17	97 / 15	136 / 6	35 / 12	47 / 9

Stars indicate significance level of ATT using bootstrap standard errors, *** p<0.01, ** p<0.05, * p<0.1, exact matching on shock distribution, saving possibility / available insurance type and network strength.

The descriptive and matching results clearly show a negative effect of the secret saving device on solidarity (Hypothesis III). Hence, the option to hide resources is obviously used by individuals to reduce the social norm of providing transfers.

We also find a negative effect of insurance availability (Hypothesis I), but interestingly only if there is no secret saving. I.e. the negative effect is only found when shocks to co-players are fully observable. This is especially true for insurance option C. On the other hand, when the option of secret savings exists, and shocks are thus no longer fully observable, solidarity transfers are reduced greatly. But now the additional availability of insurance has no further detrimental effect on solidarity any more: The point estimates are insignificant and are even positive (compare tables 6 and A2).

We also find some evidence for a persistent effect of insurance availability. The availability of option C in round 1 reduces solidarity in round 2, i.e. after insurance has been removed (Hypothesis II). However, effects are not so clear for insurance type B.

As regards the results for our Hypothesis I, it is worthwhile to keep in mind that we do not yet know whether the effect is due to a crowding-out of motives or simply because insurance reduces inequality and thus lowers the need to redistribute. One cannot answer this question by the descriptive comparisons alone, as insurance on average implies a reduction in inequality. In the following analysis we will therefore try to separate these two channels.

- Regression specification -

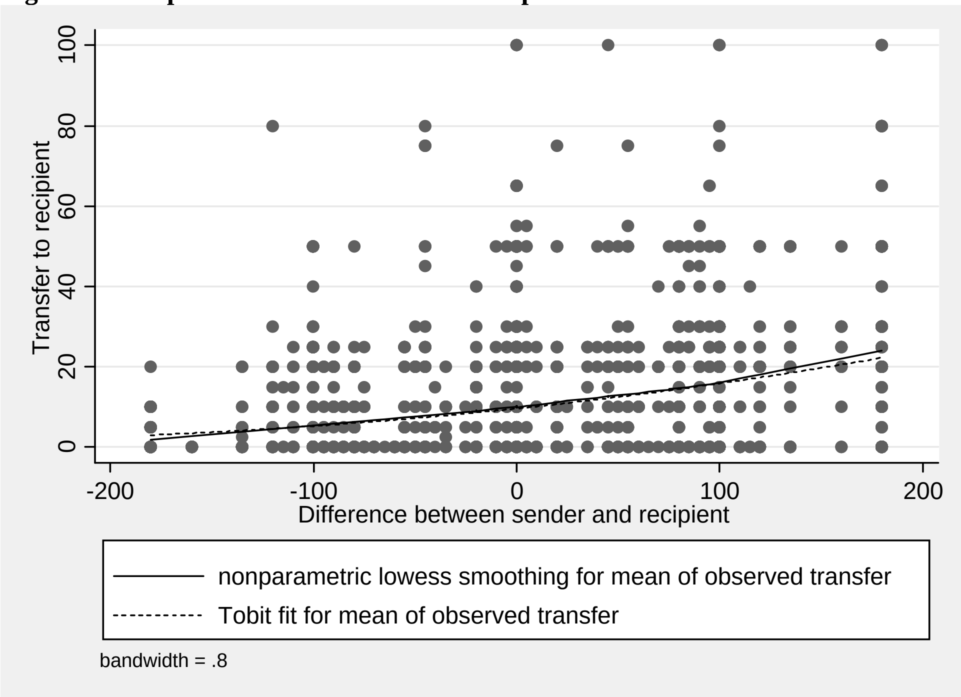
So far, we have examined unconditional effects of insurance availability only, controlling for small sample imbalances in shocks via matching. For learning more about the possible explanatory channels, we need parametric regression models, given our small samples sizes. Using a regression model and controlling for differences in inequality, we can disentangle the effects if insurance via reduced inequality and the additional crowding-out effects of insurance.

Via control variables, we can also control for small-sample imbalances in shock distributions, implied by the dice rolling, and in small-sample imbalances in individual characteristics. Furthermore we can also eliminate some statistical noise to reduce the large unexplained variation in transfers by including important background characteristics.

For specifying the regression model, we need to take into account that individual transfers are left-censored at zero because negative transfers are not allowed.[31] Since ordinary linear regression models are not suited for this problem, we rely on Tobit regression with the main assumption that the latent willingness to give is proportional to the pre-transfer difference between giver and recipient. Figure 4 shows a nonparametric fit of the relationship and the parametric Tobit regression fit. The comparison reveals that the Tobit regression fits the main relationship very well, with only a slight divergence at the both extremes.

[31] In principle, transfers are also right-censored as participants cannot transfer more than their money at hand. In reality, however, left-censoring is by far the most relevant problem. 57% of all transfers are zero, while only 2.5% percent of transfers are restricted by the money at hand. A two sample proportion test cannot reject equivalence of this proportion in the subsamples with and without secret saving (3.0% vs. 2.2%, p-value = 0.17).

Figure 4: Comparison between Tobit and nonparametric fit



N = 2730

The following analysis consists of two parts. First, we examine the willingness to give, i.e. the willingness of one individual to transfer money based on private own information and the observable information about the partners. Second, based on the estimates of the willingness to give we simulate the distributions of earnings, inequality and poverty under different scenarios. For the latter analysis, we are still interested in *net transfers*. However, net transfers are a result of the decision of two individuals and thus the difference of two censored variables. Even if the underlying *latent willingness* is linear in regressors, the difference of the *observed transfers* will not be linear.[32] The fact that net transfers are a function of the decisions by two people, who also have the option to transfer to a third person, implies that their error terms in their transfer decisions will be dependent, which would complicate the empirical analysis.

Therefore, we do not start our analysis with examining net transfers, but rather examine the transfer from individual i to individual j. Such an analysis is also useful in order to understand the willingness-to-give of individuals and its heterogeneity. We consider the following regression model of individual transfers:

[32] An exception is the case when all regressors for T_{ij} are the negative of the regressors for T_{ji}. Linearity of the expected value could then be shown. This would for example be the case when the difference between sender and recipient was the only relevant explanatory, but we allow for many more influential factors.

$$T_{ij}^* = \begin{pmatrix} Y_i - Y_j \\ Y_i \\ S_i \\ S_j \\ X_i \\ X_j \end{pmatrix}^T \beta + \varepsilon_{c,r} + \varepsilon_i$$ where T_{ij}^* is the latent transfer from i to j

and

$T_{ij} = T_{ij}^*$ if $T_{ij}^* \geq 0$

$T_{ij} = 0$ if $T_{ij}^* < 0$

Latent transfers are influenced by the difference in incomes $Y_i - Y_j$ between both group members and the level of income Y_i *after* the lottery. In those villages where secret saving was possible, the secret savings S_i and S_i may also influence latent willingness to give because they alter the observed differences between players. Individual level covariates of sender/recipient (X_i, X_j), community-round fixed effects ($\varepsilon_{c,r}$) and an individual error term (ε_i) are also allowed to affect T_{ij}^*. Note that all level effects that do not vary within a village-round cell are included in the fixed effect $\varepsilon_{c,r}$. This includes treatment and community fixed effects. However, these level effects are common to both the sender and the receiver and are thus not of immediate interest.

What is more interesting is the variation in β. If there is more solidarity of the better-off with the worse-off, transfers will be more sensitive to inequality. In other words, the β coefficient will be larger. By allowing different coefficients across treatment blocks and rounds we can capture treatment effects, at the same time controlling for real and observed differences as well as individual covariates of the sender/recipient.

- Regression results -

Table 8 shows the results of Tobit regressions. Specification (1) simply regresses transfers on level of pre-transfer earnings (*RealMoney:* Y_i), difference of earnings compared to the recipient (*Difference RealMoney:* $Y_i - Y_j$) and the amount saved by sender (*Saving sender:* S_i) and recipient (*Saving recipient:* S_j). Regression (2) includes the fixed effects and specification (3) adds individual covariates of sender and recipient.[33] We can see that the difference in earnings between sender and receiver is an important explanatory variable. The effect is highly significant and suggests that for each peso of difference the latent willingness to give rises by an additional 0.13-0.14 pesos. If money is secretly saved by the sender, this substantially reduces the inclination to give. Conversely, savings of the recipient increase transfers, as differences appear more in favor of the sender. The size of the saving effects is at large statistically indistinguishable from the effect of the real difference.[34] It thus seems that mainly observable differences drive redistribution and money is saved in order to avoid solidarity. Specifically *Saving recipient* seems to be mostly unobservable to the sender, otherwise we would expect it to have significantly weaker effects than *Difference RealMoney*. In this sense, comparing effect sizes of *Difference RealMoney* and *Saving recipient* is a test for the invisibility of secret saving.

Table 8: Tobit regressions explaining transfer

	(1)	(2)	(3)	(4)	(5)	(6)
	All observations			All observations	Only observations: no secret saving	Only observations: with secret saving
Own Money (Y_i)	0.047	0.034	0.030	0.032	0.024	0.048
Difference RealMoney ($Y_i - Y_j$)	0.13***	0.14***	0.14***	0.12***	0.15***	0.088
Difference x Treat B				-0.020	-0.084**	0.041*
Difference x Treat C				-0.037	-0.090**	-0.0077
Difference x Treat B x Treat C				0.059	0.13	0.0033
Difference x PseudoTreat B				0.035	-0.021	0.081*
Difference x PseudoTreat C				-0.030	-0.11**	0.022
Difference x Round				0.019	0.032**	0.0065
Difference x Exogenous Group				-0.019	-0.016	-0.0071
Saving sender (S_i)	-0.11***	-0.11***	-0.13***	-0.12***		-0.13**
Saving recipient (S_j)	0.11***	0.12***	0.12***	0.11***		0.099**
Village-round fixed effects	NO	YES	YES	YES	YES	YES
Individual controls	NO	NO	YES	YES	YES	YES
Observations	2730	2730	2730	2730	1664	1066

Standard errors in parentheses, clustered at the village level, *** p<0.01, ** p<0.05, * p<0.1

[33] The individual covariates used in regression (3) and their effect can be seen in Table A3 of the appendix. It seems that men tend to give more. Age has a positive marginal effect until age 47 years, when the marginal effect turns negative. Being indebted is associated with giving more. Characteristics of the recipient are all insignificant except age where effects are similar but weaker than for age of the sender. These effects are all level effects and unrelated to differences in earnings.

[34] In specification (1) – (3) we tested for equality of the following effects: i) *Difference RealMoney* = - *Saving sender* and ii) *Difference RealMoney* = *Saving recipient*. The first equality is never rejected while the second equality is rejected once, in specification (3), but merely at the 10% level.

Columns (1) to (3) impose that all participants react the same way to pre-transfer differences (β is constant). We can relax this assumption by including interactions with differences. For example, if we interact differences with *Treat B* (availability of insurance type B), the reaction on inequality of those with insurance option B will be reflected in the coefficient of *Difference RealMoney* plus the coefficient of the interaction. In columns (4) to (6) of Table 8, we allow the β vector to vary by including interactions between real differences before transfers and the following treatment variables: (*Treat B*) is the availability of insurance type B [in treatment block AB in rounds 1 and 3, in block A in round 3], (*Treat C*) is the availability of insurance type C [in treatment block AC in rounds 1 and 3, in block A in round 3], (*Treat B x Treat C*) is the availability of insurance type B and C [in treatment block A in round 3], (Pseudo*Treat B*) is the availability of insurance type B in the previous round [in treatment block AB in round 2], (Pseudo*Treat C*) is the availability of insurance type C in the previous round [in treatment block AC in round 2], (*Round*) is the round number and (*Exogenous Group*) is one if group was formed at random.

Specification (4) in Table 8 shows the results of letting β vary. Difference in pre-transfer earnings and savings of sender/recipient are again highly significant and similar in size to before, but none of the interactions displays a clear effect. However, if we focus on the village-rounds without secret saving possibility (specification 5) an interesting picture emerges. Availability of both insurance types reduces the variability of transfers with observed differences by more than half. The effects are both significant at the 5% level. Also, the significantly negative interaction term with (*PseudoTreat C*) suggests a persistence of the effect even if insurance is removed in the second round. As in the descriptive comparisons and for the matching results, no persistence of type B can be found. We also find that participants become more sensitive to differences in later rounds, as indicated by positive the positive coefficient for *Difference x Round*). Below we run several robustness checks to assess the robustness of these results. Specification (6) only considers observations with the secret saving possibility. Effects are less clear and the only two significant effects prove to be not robust when repeating the same checks as for non-saving village-rounds.[35]

As a robustness check for the above result we repeat the regression with more controls and in different subsamples. Results are summarized in Table 9. First, we add more interactions between pre-transfer differences and individual/village covariates. We include all covariates that (by chance) differ at least marginally significantly across insurance treatment blocks. The same models are estimated for the sample with secret saving (1) and without

[35] Results are not shown here, but are available from the authors upon request.

secret saving (2) separately. Then (using only non-saving village-rounds) we exclude individuals with a lower level of understanding according to our test questions or particularly 'irrational' transfers (3).[36] We also restrict our sample to only round one and two (4) and round two and three (5), respectively.

Results largely confirm that there are only weak effects in the subsample with the secret saving possibility (1), while the effect of insurance treatment B and C always goes in the right direction in the subsamples without the saving device (2-5). The coefficients are mostly significant for type C and the effect for this type even appears to be persistent if availability is removed (*PseudoTreat C*). The related coefficients are significant at least at the 5% level across all specifications. Effects of the more general insurance type B are in line with those of the catastrophic-only insurance, but they loose statistical significance in the smaller subsamples Also, we cannot identify persistence of the effect.[37]

Table 9: Tobit regressions explaining transfer – further checks

	(1)	(2)	(3)	(4)	(5)
			No saving - subsets		
	With saving	No saving	High understand	Round 1+2	Round 2+3
RealMoney (Y_i)	0.022	0.024	0.032	0.023	0.042
Difference RealMoney ($Y_i - Y_j$)	-0.022	0.12	0.21	0.27***	0.15
Difference x Treat B	0.040	-0.081*	-0.074	-0.038	-0.058
Difference x Treat C	-0.023	-0.097***	-0.083*	-0.019	-0.11**
Difference x Treat B x Treat C	-0.0091	0.14*	0.091		0.14
Difference x PseudoTreat B	0.081*	-0.0098	0.0036	0.044	-0.032
Difference x PseudoTreat C	0.015	-0.11***	-0.11**	-0.10**	-0.13***
Difference x Round	0.019	0.030*	0.021	0.050	
Difference x Exogenous Group	-0.021	-0.0074	0.0044	-0.045	-0.016
Difference x Income Class	0.017	0.023	0.019	-0.031	0.046
Difference x Selfish	-0.0058	-0.0057	-0.014	-0.012**	-0.0073
Difference x Higher Education	0.028	-0.032	-0.031	-0.017	-0.043**
Difference x HH Head	-0.0054	0.050*	0.078**	0.11***	0.020
Village-round fixed effects	YES	YES	YES	YES	YES
Individual controls	YES	YES	YES	YES	YES
Observations	1066	1664	1234	890	1216

Standard errors in parentheses, clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

[36] Irrational means individuals that lost more than some other group member, but still transferred more than 40% of their money and got less from the others than what they gave.

[37] Further robustness checks, for example expanding the interactions with community and individual characteristics, show that the negative effect of the catastrophic-only insurance (*Treat C*) is more robust than the effect of the more comprehensive type B (results not shown here but available upon request).

We additionally ran regressions allowing different crowding-out for groups formed at random (*Exogenous Group*), but results are to imprecise to draw conclusions. Moreover, we tested whether the magnitude of crowding out depends on the number of insured players and also do not find significant effects (results available upon request).

Overall, empirical results suggest that there is a negative effect of insurance on solidarity if there is no secret saving possibility. Under these circumstances, the effect of the catastrophic-only insurance is persistent even if insurance is removed. Furthermore, additional robustness checks show that the effect of the catastrophic-only insurance is more robust and persistent than the effect of the comprehensive insurance type B. The stronger and more persistent effect of type C could be due to higher 'acceptance' amongst participants, given that the price of this catastrophic-only insurance is lower and take-up is higher. This would speak for the importance of the *information effect* in explaining the crowding-out of solidarity. If many people choose insurance it signals that commitment or trust in the existing solidarity transfer scheme is low and thus provokes a negative response with low transfers.[38] The stronger information effect would also explain differential persistence across rounds, as participants update their information about the co-players for the rest of the game. However, regressions controlling for insurance take-up do not reveal higher crowding out with higher take-up. Also, adoption of the catastrophic-only insurance type C is not much higher in the first round, but rather in the third round (compare figure 2). It should be the first round, however, that leads to persistence effects in the second round. We discuss these results in some detail in section IV.

- Simulation of poverty with insurance and/or secret saving possibility -

In the regressions we found substantial saving and insurance effects. Yet, the real importance of these effects is difficult to infer from the size of the coefficients alone.[39] In principle we could simply compare empirical loss/outcome distribution in different treatment combinations to get at a meaningful comparison. In our restricted sample, however, such comparisons are blurred by differences in dice results and other covariates. We therefore use our regression results to simulate the counterfactual situation where everybody receives a certain access to

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[38] Similarly, if many people cheat with their tax declaration (and this is known to the rest of the population) it might give taxpayers a signal that compliance with the law is rather low which activates a reciprocal bandwagon effect ('people are honest conditional that others are honest') which weakens the norm of honesty further (Traxler, 2010).

[39] Also note that these coefficients (effects on the latent willingness to give) are different from marginal effects (effects on the observed transfers). We abstain from calculating marginal effects, as they do not facilitate interpreting the results too much, contrary to the simulation results presented at this point.

insurance treatment (A/AB/AC) in all rounds. This is done separately for the samples with and without secret savings. To effectively illustrate average treatment effects for the whole population, we expand the datasets by the factor 100, assign dice results according to the theoretical probabilities and also draw error terms from the estimated (normal) distribution.[40]

The simulation shows that – defining an arbitrary poverty line of 50 PhP or 25% of initial endowment – insurance type B (C) only changes the poverty rate from 7.1 to 8.0 (7.1) % without secret savings, but from 12.2 to 7.7 (9.1) % with secret savings. In the case of no secret savings we thus do not observe any positive effect of insurance compared with the case of no insurance provision. Vulnerability remains the same and taking into account administrative costs, access to insurance leads to lower welfare. Table 9 shows the poverty rates for each regime using different poverty lines. Results are not sensitive to the choice between those three definitions. The complete distributions of payoffs after transfers under different insurance schemes can be found in Figures A1a and A1b in Appendix I.

Table 10: Poverty rates for different poverty lines under each saving/insurance regime

	No saving			With saving		
Poverty line at:	No insurance	Insurance type B	Insurance type C	No insurance	Insurance type B	Insurance type C
40 PhP	5.0%	6.1%	5.0%	9.7%	5.4%	6.5%
50 PhP	7.1%	8.0%	7.1%	12.2%	7.7%	9.1%
60 PhP	10.0%	10.6%	9.7%	15.1%	10.7%	12.2%

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The phenomenon that without secret saving the unlucky are on average not better protected if there is insurance of one type also very directly shows up in the data. The poverty rate (at 50 PHP) amongst those with a catastrophic shock in the data is not significantly lower when insurance type B/C is available (30% / 31%) versus when there is no insurance (38%). The result changes if secret saving is possible. Poverty rate amongst the very unlucky is much lower with access to insurance type B/C (23% / 33%) than without (64%). This later result is significant at the 1% / 10% level using a two-group test of proportion.[41]

[40] Details on the simulation procedure can be found in Appendix III.

[41] Note that these comparisons are not necessarily balanced as the observations come from different rounds (compare the treatment plan in table 2). A balanced comparison is possible when restricting the sample to observations in the first round. Even though we have a limited number of catastrophic shocks (N=52 compared to N=169 before) when looking at one round only we still find the same qualitative result. The poverty rate is lower with insurance B / C available if there is secret saving and the differences are significantly at the 1% / 5% level. Without secret saving the difference is insignificant.

IV. Discussion and Conclusions

We have conducted a novel behavioral experiment with rural and partially urban villagers on the Philippines. This experiment – simulating a risky environment with solidarity networks and the introduction of insurance – delivers the first experimental evidence on whether informal solidarity is reduced by formal insurance in developing countries. Informal risk-sharing is by far not compensating all major shocks, and in line with other empirical research gives rise to demand the introduction of formal insurance products tailored to the needs of the poor.[42]

However, our data highlights that the availability of insurance reduces solidarity and that this negative effect might even persist if insurance is removed. Regressions reveal that the negative effect is not only due to lower inequality between those with insurance but that there is an additional crowding-out effect on solidarity. However, this is only the case if shocks of network members are observable. Also, the evidence is stronger for insurance focusing on catastrophic shocks as compared to insurance for all types of shocks.

So why do effects only exist when shocks are observable? One important observation at this point is that the overwhelming majority secretly saves money and simulates a medium shock if there is the possibility to do so. As a consequence solidarity transfers are reduced dramatically (compare Table 4). With solidarity transfers being so low, observing further reductions is hard. This might very well explain why the insurance effect is focused on the non-saving villages where there is still solidarity in place.

Regarding the stronger negative effect of the catastrophic-only product we observe considerably lower take-up of the more comprehensive scheme. Thus, participation might be too low to induce a 'common sense' that the market mechanism should apply ('framing effect'). Some speculation allowed, this could have to do with the relatively high price of the comprehensive product. While everybody with reasonably high risk aversion can be expected to purchase the catastrophic-only insurance this is not the case for the more expensive version.[43] Stronger crowding-out with higher take-up would also be in line with the fact that reciprocity is important for risk-sharing and that buying insurance signals low trust in the informal risk-sharing mechanism ('information effect' described in the introduction). This

[42] Remember that depending on whether there is secret saving or not, between 38% and 64% of those with a catastrophic shock in our data end up below the poverty line of one quarter of initial endowment.

[43] A simple simulation using a constant relative risk aversion utility function [$u(c) = (c^{1-\rho})/(1-\rho)$] shows that in the absence of solidarity a much larger share should choose insurance in treatment AC [risk-aversion parameter $\rho > 0.34$] than in treatment AB [risk-aversion parameter $\rho > 0.65$]. Note that $\rho > 0$ for risk-averse individuals.

could then lead to a negative bandwagon effect with low transfers. Yet, we do not find that the impact increases with the number of insured in the group which should be the case when the information effect would be the main driver. Also, take-up of the insurance types differs mainly in the third round, which cannot affect information in the second round. We are thus referred to the other candidate to cause crowding out – the framing effect (availability of costly insurance signals that everybody is responsible for 'buying' security on his own). Contrary to the information effect, the framing effect is not testable given our data as it is simply related to the presence of the insurance product. Identifying the exact cause for crowding out is an interesting research question and certainly deserves more attention. At this point, however, we cannot clearly distinguish between the above two (or alternative) explanations.

In sum, our experimental results suggest that the introduction of insurance in solidarity networks might have unintended consequences under some circumstances. Especially if the network is able to observe the cash flow of members and reciprocal solidarity works well, these effects have to be taken into account. Short- and long-run effects are in line with the general literature on crowding-out of pro-social behavior by market based mechanisms (Bowles, 2008). Nevertheless, formal insurance might have considerable positive net effects, because informal solidarity is not always very effective. If network members can simulate shocks, secretly save and thereby retreat from their solidarity commitment, availability of formal insurance can be a considerable improvement for individuals.

The partial ineffectiveness of insurance supply to protect against poverty heavily hinges on incomplete take-up. Participants below the poverty line in insurance treatments are mostly non-buyers.[44] Thus our experimental results suggest that in a world without saving products (as is still the case for a large majority of people in developing countries) it can be better to have no voluntary insurance at all or force everybody into a compulsory public insurance scheme. Otherwise, there might always be a considerable fraction without insurance, as our data shows independent of the round. Although solidarity transfers are reduced by the access to insurance this reduction is 'not enough' in order to voluntarily bring the uninsured individuals into voluntary insurance schemes. As pointed out by Buchanan (1975) helping somebody may undermine his or her incentive to care for him or herself (i.e. insure). As long as there are enough 'Samaritans' with altruistic motives that help people in need (even though there is the possibility to insure against risks) and the 'Samaritans' are unable to commit not to provide help to uninsured individuals who face a loss, there will be

[44] 87% of participants below the poverty line in treatments with insurance access were non-adopters.

an undesired underinsurance (compared with compulsory insurance) together with a crowding-out effect (compared to the no-insurance case).

While the above made statement favoring compulsory insurance rests on some assumptions (e.g. stability of crowding out over time, persistently incomplete take-up and other issues regarding external validity of our experiment) our main conclusion is that financial products serve people most when they are offered as a bundle. Introducing insurance in contexts without (formal) banking, with good monitoring in the network and strong informal solidarity might well lead to unintended consequences and be ineffective. However, the story is different when saving is available or will be introduced. Even though it might seem that introducing (secret) saving plays a harmful role in our experiment there are many good reasons to introduce this financial product. Especially, people will be able to use their saving for intertemporal income smoothing, an aspect we completely excluded up to now. In such situations, the combination of both financial products can be effective. While saving acts as an intertemporal smoothing device, insurance can compensate its negative side effect on risk-sharing within the network. Together, access to insurance and saving can then decrease vulnerability.

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Appendix I: Tables

Table A1: Descriptive statistics of villages

	All (N=22)				A (N=6)	AB (N=8)	AC (N=8)
	Mean	Std.	Min	Max	Mean	Mean	Mean
How many people live in this community?	1264	653	350	3123	1445	1284	1109
different religious groups in this barangay	2.45	1.26	1	5	2.67	2.5	2.25
HHs with family members abroad (percent)	9.19	8.96	0	34.48	7.52	12.98	6.67
Conflicts between people	1.50	0.67	0	2	1.33	1.88	1.25
number of village organizations	7.23	1.66	4	11	7.83	6.88	7.13
People are selfish	6.36	2.97	0	10	5	7.88*	5.88
trust to lend/borrow	5.27	3.18	0	10	5	5.38	5.38
always somebody willing to help	7.95	2.36	0	10	8.17	8.13	7.63
Income Class	3.45	0.60	3	5	3	3.75**	3.5**
1=partially urban / 0=rural	0.68		0	1	0.5	0.75	0.75

Stars indicate significance level of Wilcoxon ranksum test for differences to mean in treatment A
*** p<0.01, ** p<0.05, * p<0.1

Table A2: Average treatment effect on the treated (ATT) of insurance treatment blocks on net transfers to disadvantaged co-players (with saving subsample)

		saving subsample	
Round	Outcome variable	Block AB vs. A (ATT)	Block AC vs. A (ATT)
1	Net Transfer to recipient	+3.4	-3.5
	Obs (On/off support)	37 / 18	39 / 7
2	Net Transfer to recipient	+8.2	+2.9
	Obs (On/off support)	49 / 5	50 / 6

Stars indicate significance level of ATT using bootstrap standard errors, *** p<0.01, ** p<0.05, * p<0.1, exact matching on shock distribution, saving possibility / available insurance type and network strength.

Table A3: individual covariate coefficients for specification (3) of Table 7

Coefficients for characteristic of…	Sender	Recipient
Regular income?	**4.20***	0.92
Skip meals last month	-1.21	0.42
Debt > 1000 Pesos?	**4.30***	-0.15
Gender (0=fem, 1=male)	**9.96****	-1.93
HH head	-0.0098	0.37
Male x HH head	-3.77	2.17
Married	3.62	0.99
Highschool	-0.17	-0.39
College	0.26	3.85
Age	**2.17*****	**0.87****
Age squared	**-0.023*****	**-0.0074***
Village-round fixed effects	YES	
Observations	2730	

Standard errors in parentheses, clustered at the village level
*** p<0.01, ** p<0.05, * p<0.1

Table A4: Descriptive statistics of insured versus non-insured in round 1

	Treatment block AB		Treatment block AC	
	not insured (N=97)	insured (N=65)	not insured (N=87)	insured (N=72)
	Mean	Mean	Mean	Mean
Male	0.29	0.28	0.30	0.42
Household head	0.28	0.32	**0.31**	**0.44***
Married	0.79	0.80	0.76	0.86
Highschool education	0.46	0.51	0.34	0.40
College education	0.28	0.32	0.20	0.24
Age (in years)	41.5	41.5	45.4	43.0
Regular monetary income? (dummy)	0.26	0.23	0.21	0.24
Skip meals in last month	0.23	0.26	0.34	0.38
In debt with more than 1000 Pesos?	0.67	0.61	**0.45**	**0.60***

Stars indicate significance level of Wilcoxon ranksum test for different means comparing insured to non-insured
*** p<0.01, ** p<0.05, * p<0.1

Table A5: Descriptive statistics of insured versus non-insured in round 3

	Treatment block AB		Treatment block AC	
	not insured (N=97)	insured (N=65)	not insured (N=87)	insured (N=72)
	Mean	Mean	Mean	Mean
Male	0.29	0.28	0.31	0.40
Household head	0.30	0.30	0.35	0.40
Married	**0.85**	**0.70****	0.76	0.85
Highschool education	0.44	0.56	0.36	0.38
College education	0.33	0.23	0.18	0.25
Age (in years)	42.0	40.5	45.0	43.6
Regular monetary income? (dummy)	0.24	0.26	0.19	0.25
Skip meals in last month	**0.16**	**0.39*****	**0.28**	**0.42***
In debt with more than 1000 Pesos?	0.65	0.65	**0.45**	**0.58***

Stars indicate significance level of Wilcoxon ranksum test for different means comparing insured to non-insured
*** p<0.01, ** p<0.05, * p<0.1

Table A6: Tobit regressions explaining transfers – regression (1) and (2) of table 9 without fixed effects and different ways to obtain standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
	Without saving			With saving		
RealMoney	0.024	0.036	0.036	0.022	0.026	0.026
	(0.030)	(0.022)	(0.024)	(0.024)	(0.023)	(0.024)
Difference RealMoney	0.12	0.13	0.13	-0.022	-0.031	-0.031
	(0.10)	(0.089)	(0.093)	(0.080)	(0.096)	(0.11)
Difference x Treat B	-0.081*	-0.091**	-0.091**	0.040	0.038	0.038
	(0.048)	(0.044)	(0.044)	(0.028)	(0.046)	(0.048)
Difference x Treat C	-0.097***	-0.096**	-0.096**	-0.023	-0.024	-0.024
	(0.036)	(0.038)	(0.039)	(0.036)	(0.046)	(0.047)
Difference x Treat B x Treat C	0.14*	0.15*	0.15*	-0.0091	-0.0064	-0.0064
	(0.080)	(0.082)	(0.090)	(0.094)	(0.13)	(0.14)
Difference x PseudoTreat B	-0.0098	-0.020	-0.020	0.081*	0.073	0.073
	(0.042)	(0.046)	(0.049)	(0.042)	(0.045)	(0.050)
Difference x PseudoTreat C	-0.11***	-0.11***	-0.11***	0.015	0.015	0.015
	(0.037)	(0.041)	(0.043)	(0.030)	(0.040)	(0.042)
Difference x Round	0.030*	0.027*	0.027*	0.019	0.021	0.021
	(0.016)	(0.015)	(0.015)	(0.034)	(0.044)	(0.047)
Difference x Exogenous Group	-0.0074	-0.0012	-0.0012	-0.021	-0.022	-0.022
	(0.030)	(0.023)	(0.025)	(0.019)	(0.025)	(0.028)
Difference x Income Class	0.023	0.020	0.020	0.017	0.018	0.018
	(0.025)	(0.025)	(0.026)	(0.022)	(0.026)	(0.028)
Difference x Selfish	-0.0057	-0.0055	-0.0055	-0.0058	-0.0052	-0.0052
	(0.0060)	(0.0050)	(0.0052)	(0.0043)	(0.0053)	(0.0057)
Difference x Higher Education	-0.032	-0.035	-0.035	0.028	0.028	0.028
	(0.020)	(0.026)	(0.026)	(0.026)	(0.026)	(0.028)
Difference x HH Head	0.050*	0.054**	0.054**	-0.0054	-0.0064	-0.0064
	(0.029)	(0.025)	(0.026)	(0.020)	(0.022)	(0.024)
Village-round fixed effects	YES	NO	NO	YES	NO	NO
Village-round controls	-	YES	YES	-	YES	YES
Individual controls	YES	YES	YES	YES	YES	YES
Standard errors	Clustered at village level	Clustered at individual	Bootstrap (ind. cluster)	Clustered at village level	Clustered at individual	Bootstrap (ind. cluster)
Observations	1664	1664	1664	1066	1066	1066

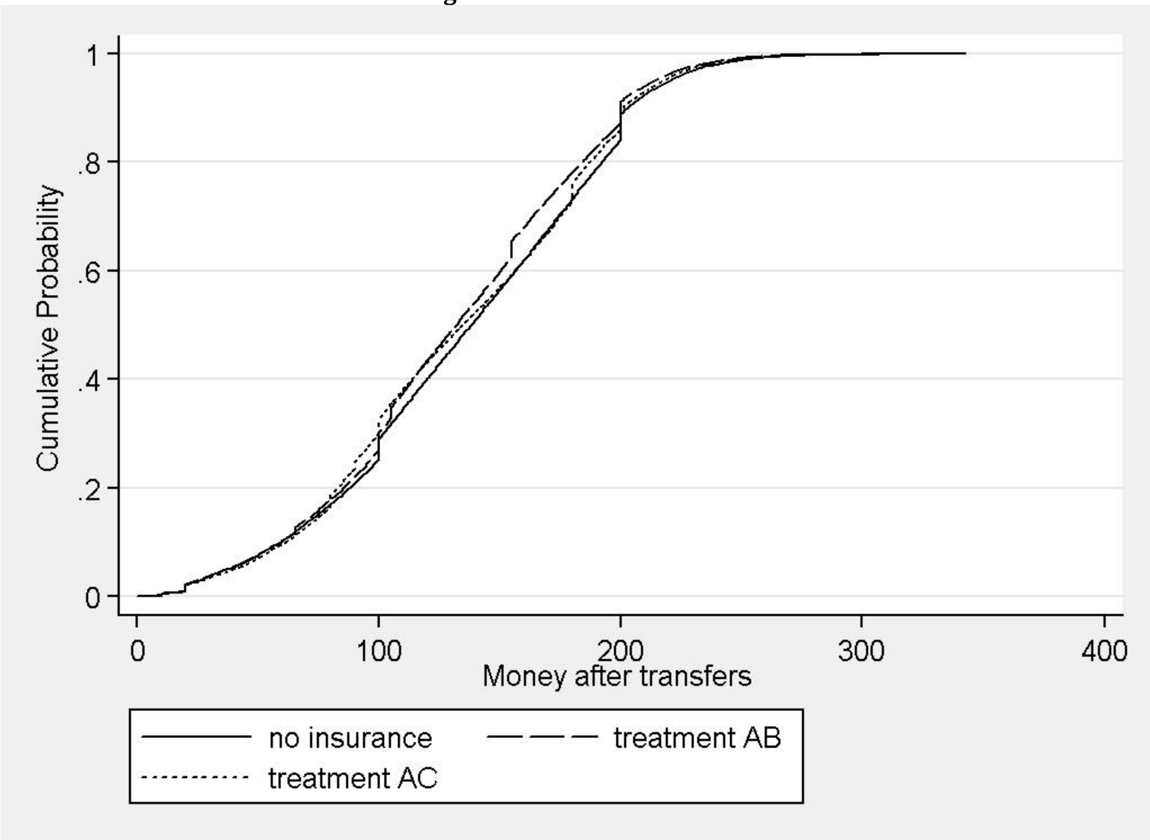
Standard errors in parentheses, clustered at the village or individual level or obtained via clustered (individual level) bootstrap (1000 repetitions), *** p<0.01, ** p<0.05, * p<0.1

Colum (1) of table A6 is identical with specification (2) of table 9, the main regression for those without secret saving possibilities. Colum (2) drops village-round fixed effects and instead uses controls on that level (treatment combination, round number as well as income class, rural/semi-urban status and a measure of selfishness of the village). Also, standard errors are clusterd at the individual instead of the village level (two transfer decisions times three rounds per participant). In colum (3) standard errors are estimated using 1000 bootstrap estimations, the bootstrap is also clustered at the indiviadual level. Colum (4)-(6) proceed similarly, only for those with the possibility to save.

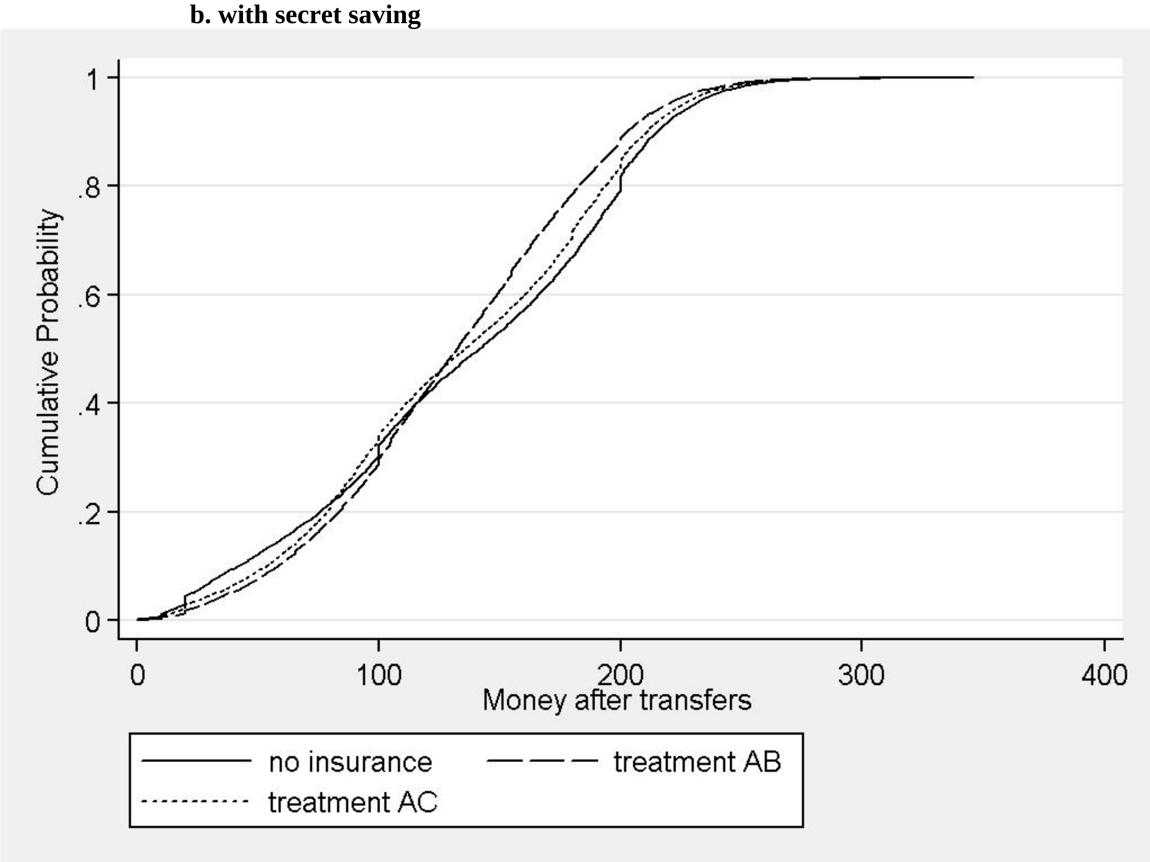
Table A6 shows that it does not make a large difference whether we control for village-round fixed effects or a set of controls, and whether we cluster standard errors at the village level, the individual level or whether we obtain the corresponding bootstrap estimate. This is important for our simulation. We use specifications (3) and (6) for our simulation of the outcome distribution with different insurance availability and without/with secret saving, respectively. To obtain confidence bounds, the simulation is repeated for each bootstrap repetition of specification (3) and (6). We provide details on the simulation procedure in appendix AIII.

Figure A1: CDF of payoff under different insurance schemes

a. without secret saving



b. with secret saving



Appendix II: Example Test Questionnaire

(Notes: Example for treatment block AB, round 1, with saving. In reality we called option A "Angola", B "Botswana" and C "Cameroon" to avoid a notion of order in the options. Correct answers given.)

When do you decide which option you choose?

- ☒ 1 before you throw the dice
- ☐ 2 after you throw the dice
- ☐ 3 whenever you like

CORRECT? YES ☐ NO ☐

Is the option BOTSWANA for free? YES ☐ NO ☒

CORRECT? YES ☐ NO ☐

How much does the option BOTSWANA cost? **45**

CORRECT? YES ☐ NO ☐

How much do you have left if...

	With option BOTSWANA	With option ANGOLA
... you roll a 1?	**155**	**200**
... you roll a 2?	**155**	**200**
... you roll a 3?	**155**	**200**
... you roll a 4?	**105**	**100**
... you roll a 5?	**105**	**100**
... you roll a 6?	**65**	**20**

CORRECT? YES ☐ NO ☐

---------------- ONLY IF WITH LOCKBOX ----------------

When can you put money in the lockbox? Can you put money in the lockbox if you choose option ANGOLA and...

... you roll a 1?	YES ☒	NO ☐	If yes, how much **100**
... you roll a 2?	YES ☒	NO ☐	If yes, how much **100**
... you roll a 3?	YES ☒	NO ☐	If yes, how much **100**
... you roll a 4?	YES ☐	NO ☒	If yes, how much ______
... you roll a 5?	YES ☐	NO ☒	If yes, how much ______
... you roll a 6?	YES ☐	NO ☒	If yes, how much ______

CORRECT? YES ☐ NO ☐

When can you put money in the lockbox? Can you put money in the lockbox if you choose option BOTSWANA and...

... you roll a 1?	YES ☒	NO ☐	If yes, how much **50**
... you roll a 2?	YES ☒	NO ☐	If yes, how much **50**
... you roll a 3?	YES ☒	NO ☐	If yes, how much **50**
... you roll a 4?	YES ☐	NO ☒	If yes, how much ______
... you roll a 5?	YES ☐	NO ☒	If yes, how much ______
... you roll a 6?	YES ☐	NO ☒	If yes, how much ______

CORRECT? YES ☐ NO ☐

Will your group members know if you put money in the lockbox? YES ☐ NO ☒

CORRECT? YES ☐ NO ☐

Appendix III: Simulation of Transfers under different insurance regimes

For the simulation we estimate the models with and without the secret saving device separately. Our reference regressions are models (1) and (2) from table 9. Unfortunately, we cannot use round-village fixed effects, because from the estimation in half of the villages we cannot infer the size of the fixed effects in the other half. We instead control for treatment combination, round number as well as income class, rural/semi-urban status and a measure of selfishness of the village. This does not at all change size and significance of the regression results. Also, as we bootstrap the whole process to obtain confidence bounds for the simulation, we cannot use clustered standard errors. Instead we cluster the bootstrap on the individual level, i.e. always bundling six transfer observations (two recipients in three rounds for each participant). This corresponds to a regression with standard errors clustered at the individual level. Table A6 compares the reference model, the model with village-round controls and standard errors clustered at the individual level, and the model with the bootstrap clustered at the individual level. They all exhibit very similar coefficients and significance levels. Hence, the uncertainty from the reference regression can be adequately translated into uncertainty of the simulation. For each simulation repetition we proceed as follows (separately for those with and one without the secret saving device):

1. Estimate model of transfers:

   We estimate regression model (1) / (2) from table 9 for the sample of, replacing the village-round fixed effects with the corresponding control variables. All coefficients are stored.

2. Expand data:

   For the results to be stable and to represent average treatment effects, we have to create a sufficiently large data set. We therefore duplicate the data set 100 times.

3. Draw shock:

   For each (participant-round) observation we draw a shock according to the theoretical probabilities (1/2 no shock, 1/3 medium shock, 1/6 catastrophic shock).

4. Draw 'risk aversion':

   To know which individual will be assigned insurance in our simulation, we draw an individual-specific risk aversion parameter from a uniform distribution between 0 and 1. If risk aversion is above a certain threshold, the individual will be treated as having chosen insurance in the insurance treatments. The thresholds are chosen such that the take-up rates of type B and type C equal the observed take-up rates in the experiment. This implies that the same persons take up insurance in each round. This is obviously a simplification, but in the game we observe that take-up in the first round is indeed a good – not a perfect one though – predictor of take-up in round three.[45]

5. Draw error term

   We draw an error term from the normal distribution with the estimated variance from the regression model (1) / (2), table 9. This is an independent term for each (individual-round-recipient) observation.

6. Simulate setting for each regime (A vs. AB vs. AC)
   a. Reset insurance treatment indicators according to setting
   b. Assign insurance to the risk-averse individuals (compare 4.)
   c. Assign loss/payoff according to shock (see above) and insurance 'choice' (this is the pre-transfer outcome)
   d. Calculate differences between players
   e. Calculate relevant interactions between differences and treatments etc. (all that is needed for the regressions)
   f. Predict transfers according to model estimated at the beginning (compare 1.), but with the new counterfactual covariates (predicted values, not censored yet)
   g. Add error term (compare 5.)
   h. Left-censor transfers (according to Tobit specification) and right-censor at half of post-lottery income. The latter is not in the model, but only applies to very few cases. Still it makes sense, because some few participants would end up transferring more than what they have.
   i. From the post-lottery income and censored transfers calculate post-transfer income/losses.

The income/loss distribution from each setting can now be further analyzed (e.g. for poverty rates, etc.)

[45] We could explicitly estimate a model to explain insurance take-up, conditional on the covariates. However, this complicates our exercise and a first glance at the insurance uptake does not hint at factors that might be connected to insurance take-up and redistributive preferences. Thus, distribution is unlikely to be affected by this simplification.