ESSAYS ON FORMAL INSURANCE AND INFORMAL SOLIDARITY IN DEVELOPING COUNTRIES

Inauguraldissertation
zur Erlangung des akademischen Grades eines Doktors der
Wirtschaftswissenschaften der Universität Mannheim

vorgelegt von
Andreas Landmann

April 2013

Abteilungssprecher:	Prof. Dr. Martin Peitz
Referent:	Prof. Dr. Markus Frölich
Korreferent:	Prof. Dr. Dirk Engelmann
Tag der Verteidigung:	04. Juni 2013

Acknowledgements

First of all, I would like to thank my advisor Markus Frölich. I consider myself privileged to have an advisor demonstrating such approachability, supportive attitude, fairness and – together with all these personal qualities – intellectual capacity. I am also grateful to my co-advisor Dirk Engelmann who greatly supported my endeavors in behavioral economics through excellent comments on my papers and future project ideas.

Furthermore, I want to acknowledge the influence Björn Vollan had on the development of this thesis. His enthusiasm, creativity and never-ending discussions spurred many research ideas that found their way into my papers. In addition, my other past and present colleagues at the Chair of Econometrics – Boris Branisa, Getinet Haile, Niels Kemper, Michael Maier, Markus Olapade, Robert Poppe, Pia Unte, Manuel Wiesenfarth and our secretary Anja Dostert – created a productive and enjoyable working environment. I moreover value the contribution of our student assistants to the research projects. For my studies at the graduate school I gratefully acknowledge the financial support by the Deutsche Forschungsgemeinschaft. Also my fellow PhD students from the class of 2008 were essential for surviving the program, especially during the 'first year'.

I thank my co-authors Markus Frölich and Björn Vollan for stimulating discussions and excellent analytical input. For the behavioral experiment in the Philippines, I furthermore acknowledge financial and organizational support by 'Deutsche Gesellschaft für Internationale Zusammenarbeit' (GIZ) and its 'Microinsurance Innovations Program for Social Security' in the Philippines (GIZ-MIPSS). Many thanks also to Philip Saliba and the team of local assistants for their enthusiastic support in the field. In the context of the impact evaluation in Pakistan I am grateful for support from the International Labour Office (Microfinance for Decent Work Programme) and the National Rural Support Programme of Pakistan; in particular I thank Hillery Midkiff (ILO) and Tahir Waqar (NRSP).

Finally, I want to thank my family: My parents Bernd and Marliese as well as my brother Christian all supported me simply by existing as the wonderful persons they are and certainly brought me back to real life whenever necessary. Yet, without doubt my wife Natascha had to bear the major burden of my life as an academic. She was confronted with my stays abroad, late-night shifts, trial presentations, my moods after an unproductive day and many more inconveniences, but nevertheless supported my efforts with all her love whenever I needed her. The very last "thank you" goes out to our daughter Sophie who shows me every day how fascinating life can be.

Table of Content

List of Tables

List of Figures

1. Introduction

In developing countries, shocks such as illness, death of family members, natural catastrophes, price fluctuations, unemployment and many more constitute a real economic threat. Especially poor and vulnerable households are often forced to sell productive assets, reduce consumption, take children out of school and send them to work, all of which may have long-term negative consequences on human capital accumulation and income. Hence, low-income households face the danger of being trapped in a vicious circle of poverty by economic shocks. This dissertation deals with the potential to insure against such risks in developing countries. I do not consider insurance in the narrow sense. Instead I focus on two aspects: First, I assess informal risk-sharing arrangement in social networks that act as an informal insurance. While many arrangements are based on explicit reciprocity (reward and punishment), I focus on pro-social solidarity transfers that require trust and altruism. Second, I consider the potential of formal insurance products for the poor – so-called microinsurance.

In different chapters I explore conditions under which solidarity transfers occur and how they can be affected by the availability of formal insurance. The latter is also the reason for focusing on solidarity instead of reciprocity: While reciprocity based on rational self-interest should be easily restored after a change in economic incentives, the same is probably more difficult if pro-social motivations were crowded out. In the last part of the dissertation I focus on a positive side effect of reduced vulnerability and evaluate the impact of formal health insurance on child labor.

The data is collected using different techniques. Standard household surveys are used for items that can be quantified in a relatively objective way, while behavioral experiments are employed to gather pro-social solidarity that is otherwise hard to measure. All causal analyses rely on experiments, i.e. truly random variation. I also use theoretical models in two cases: to further advance identification of effects from the data analysis and to rationalize empirical results. I believe that this mix of methods is well suited for the variety of analytical challenges. The three chapters of the dissertation are described in more detail in the following.

In the context of economic uncertainty and when formal insurance is weak, informal risk-sharing mechanisms play a large role. Hence, informal transfers are especially important in developing countries. One important component of such transfers is solidarity within the social network. Chapter 2 (joint work with Björn Vollan) examines two potential drivers of such solidarity transfers: network strength and collective action. While the former relates to trust in a specific relationship, the latter is often associated with more generalized trust and conditional cooperation type in the literature. We build a simple model to illustrate how solidarity might be influenced by specific trust, generalized trust and cooperator type. We then exploit the unique setup of a behavioral game we conducted in the Philippines to test the causal effect of network strength on solidarity and whether collective action relates to solidarity through generalized trust or cooperator type. We find that network strength has a positive effect on solidarity transfers. We also find that – depending on organization type – collective action might relate to both generalized trust and cooperator type.

Chapter 3 (joint work with Markus Frölich and Björn Vollan) tests the impact of insurance on solidarity in a behavioral experiment with rural Filipinos when people have the possibility to hide part of their income. We find that offering insurance crowds out solidarity if income shocks are observable such that benefits of insurance are completely offset. Reduced solidarity might persist even if insurance is removed in a later round. However, when hiding income is possible, solidarity is already low without insurance and there is no additional negative effect of insurance. Hence, formal insurance is not effective when the monetary situation can be closely monitored, but it might be an important complement when financial resources cannot be observed.

Chapter 4 (joint work with Markus Frölich) assesses another aspect of insurance for the poor. Microinsurance is an actively advocated financial instrument at the moment, and thought to reduce vulnerability of poor households to economic shocks. Many evaluations of microinsurance explore effects on financial protection, access to health care and risk taking in other domains (e.g. agricultural investment). Yet, there is an especially undesirable consequence of shocks that might be mitigated by microinsurance and has received surprisingly little attention until now: child labor. Taking children out of school and sending them to work may have long-term consequences on children’s human capital or health. We are therefore interested in the impact of insurance on school attendance and child labor outcomes. We exploit the recent extension of a health and accident insurance scheme by a Pakistani microfinance institution (MFI) that was set up as a randomized control trial and accompanied by a household panel (baseline plus four follow-up surveys). Together with increased coverage the MFI offered assistance with claim procedures in the treatment area. Difference-in-difference techniques are employed to control for accidental selection across the limited number of randomization clusters. We find evidence for lower incidence of child labor and strong evidence for households to rely less on child labor earnings after the innovation in the treatment branches. Separating the two parts of the innovation package, the effects of claim assistance are mostly insignificant, while increased insurance coverage has large and highly significant effects on child labor outcomes and days missed at school. Consistent with a theoretical model we develop in this paper, the effect is largely due to an ex-ante feeling of protection as opposed to a shock-mitigation effect.

2. How Network Strength and Collective Action Influence Solidarity: A Field Lab in the Philippines

I. Introduction

Malfunctioning public institutions and missing markets for financial services often hinder people's economic advancement in developing countries. Natural catastrophes, illnesses, economic crises, unemployment or crime at the same time threaten to destroy the economic basis of countless households. Many people rely on networks of mutual support instead of formal insurance. Transfers within the household or within networks of family, neighbors and friends or the whole village help in the management of income fluctuations caused by all kind of shocks. Such risk-sharing allows households to smoothen income shocks throughout their network, the whole village or even across villages (Townsend 1994; Ligon 1998; Morduch 2002). Solidarity – the "willingness to help people in need who are similar to oneself but victims of outside influences" (Selten and Ockenfels 1998, 518) – is an integral part of the risk-sharing mechanism. It is closely related to explicit reciprocity, as solidarity transfers are made with the implicit expectation that the recipient would behave similarly in the reverse situation.[1] Behavioral experiments confirm that even without repeated interaction or other commitment incentives a substantial level of helping persists.[2] Hence, understanding the determinants of solidarity is highly relevant for an understanding of risk-sharing, especially in the context of developing economies.

Besides relying on networks of mutual support instead of formal insurance, many people form groups for collective action to circumvent market and government failure in producing public goods. Participation in collective action like a collectively maintained irrigation channel within a farmers' association is an example where access to social resources yield direct benefits. Yet, participation in collective action for irrigation not only yields benefits through a greater harvest yield. It also increases the size and strength of someone's

[1] Compare the definition and explanations on solidarity of Selten and Ockenfels (1998, 518). The difference to reciprocal risk-sharing arrangements is that also purely self-interested individuals can under some conditions be induced to cooperate. Theoretical models on enforceable implicit risk-sharing contracts with purely self-interested agents are for example provided by Coate and Ravallion (1993) and Attanasio and Ríos-Rull (2000).

[2] For example Barr and Genicot (2008) show that also absent external commitment risk-sharing takes place based solely on intrinsic motivation and Leider et al. (2009) observe helping behavior despite anonymity of the sender, especially if the recipient is not anonymous. Finally, Comola and Fafchamps (2010) using household data from the Philippines find that many transfer relationships are often unilateral and not reciprocal. All this evidence suggests that solidarity transfers are a lower bound, but still a good approximation to real risk-sharing.

network that might be activated in times of need. Over and above, collectively managing irrigation canals leads to changes in beliefs about other peoples' behavior and the learning of important norms of behavior required for teamwork. People learn to trust people outside their network (i.e. generalized trust) and to reciprocate trust (i.e. become conditional cooperators). Such changes in beliefs and norms are very likely to also affect risk-sharing in the network. Hence, risk sharing and collective action are two important and intertwined aspects embedded in social networks. Both entities on their own are widely researched but the important question remains how risk sharing and collective action depend on each other. The contribution of the paper is threefold: First, we propose a simple model for solidarity transfers that serves as a frame for the analysis of an adapted solidarity game conducted in the Philippines (compare Landmann, Vollan, and Frölich 2012). Second, we use data from this solidarity game and exploit experimental variation in network strength to explain changes in solidarity. Third, we use questionnaire data on collective action to analyze its relationship with solidarity. The model in combination with survey data and game outcomes disentangles channels through which collective action influences solidarity.

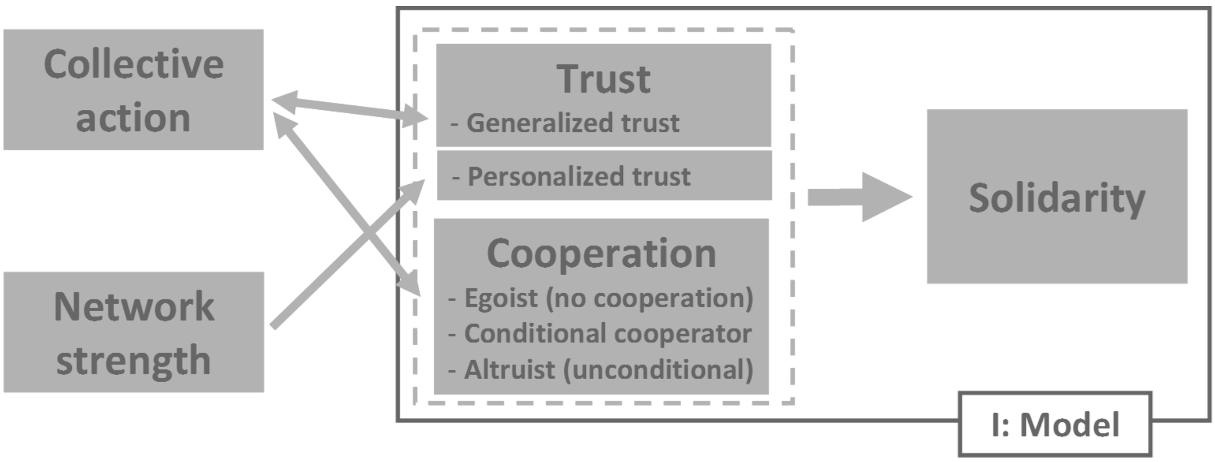
When thinking about determinants of solidarity, there is a clear emphasis on the aspect of reciprocity in the definition of Selten and Ockenfels (1998). Thus, not everybody seems to transfer all the time, but at least for some cooperation is conditional on trust that the potential recipient would also transfer in case the situation was reverse. Similarly, experimental economists attribute large scale cooperation to a high share of conditional cooperators in the society.[3] Likewise, many authors working on 'social capital' agree that most of the beneficial effects of social capital are in fact outcomes of generalized trust (Uslaner 2002; Bjørnskov 2006).[4] Lastly, people vary their level of cooperation according to how well they know a specific person. These aspects of conditional cooperation, generalized trust and personalized trust are all incorporated as drivers of solidarity in our model. In the following we will explain the main research idea, illustrated in Figure 1.

[3] For example Rustagi, Engel, and Kosfeld (2010) find that forest user groups with a higher share of conditional cooperators monitor the forest more often and have better forest outcomes.

[4] We refrain from using the general concept of social capital in the course of this paper because of its imprecise definition and operationalization. According to several definitions from the social capital literature risk sharing networks as well as participation in collective action constitute to the stock of social capital while other authors use them as outcome measures of social capital. Based on Coleman (1988), Putnam (1995) includes membership in organizations and participation in collective action in the stock of social capital (i.e. an investment for future returns). For Brehm and Rahn (1997, 999) individual social capital facilitates collective action at the community level. Similar definition issues arise with in cash and in kind transfers. Most often these are subsumed as "norms of reciprocity" or "network characteristics" with a series of other variables. Only few authors like Lin (2001) or Portes (1998, 6) focus solely on the resources people can access from their network ties.

Network strength – the intensity of personal relation between individuals in the social network – is an obvious determinant of solidarity transfers. The literature finds that risk pooling is higher within certain forms of relationship, such as ethnic groups or family (Grimard 1997; Fafchamps and Lund 2003). Behavioral experiments also find significantly higher levels of pro-social giving towards friends and family members in the context of dictator type games (Leider et al. 2009; Ligon and Schechter 2012) and trust games (Vollan 2011). In our research framework, higher network strength should relate to more personalized trust and consequently to more solidarity.

Figure 1: Research Framework



Collective action can be related to solidarity through several mechanisms. First, the willingness to cooperate might be different for organization members since they might be more likely to be conditional cooperators.[5] We are not aware of studies that link variation in organizational membership to different degrees of conditional cooperation, though.[6] However, Hayo and Vollan (2011) discuss the difference in cooperation behavior related to collective action. They examine several group characteristics in a social dilemma experiment in the field and find that in groups of five people where *all* members contributed to community work people were more co-operatively. Cooperation rates decline in heterogeneous groups where only some people contributed working days to the community. According to the authors voluntary work is signaling to other group members that it is unlikely that co-operators will be

[5] Numerous social preference models predict more cooperative outcomes as a function of the number of conditionally cooperative players in a group (Rabin 1993; Fehr and Schmidt 1999; Bolton and Ockenfels 2000; Charness and Rabin 2002; Dufwenberg and Kirchsteiger 2004) which has also been established by many experiments in the lab (Gächter and Thöni 2005; Gunnthorsdottir, Houser, and Mccabe 2007; Kurzban and Houser 2005; Ones and Putterman 2007).

[6] Few studies in the context of collective action even measure conditional cooperation.

exploited. Once people who are known to be free riders join the group, those who report doing more community work refuse to collaborate since they only cooperate with people they know do also cooperate. People doing collective action hence seem to act as conditional cooperators.

Secondly solidarity might arise due to higher disposition to have generalized trust in the community. Putnam (2000, 137) states that "those more engaged in community life are both more trusting and more trustworthy". He further argues that participation in virtually any social activity generates trust, on the assumption that the particularized trust generated in such face-to-face interactions also generates social trust (Putnam 1995; Putnam 2000). This view is supported empirically, for example by Paxton (2007) and Traunmüller (2011).[7] In our case where we only look at trust within the community general trust might account for solidarity transfers to community members who are unlike the individual (different socio-economic status, age and gender) and are not so well known.

Thirdly collective action might spur solidarity by creating stronger network ties (increasing personalized trust) or larger networks.[8] Beliefs about other people within (and outside) the organizations might be affected by repeated interaction. New links (larger network) is a potential outcome (Feigenberg, Field, and Pande 2010). In our behavioral experiment we fix the size of the network and therefore shut down this channel of influence. However, the strength of existing or new-established links might also be affected by collective action. To reduce complexity and facilitate the analysis, we also restrict this channel of influence in our model.[9]

In our experiment participants can offer each other mutual support to share the risk in their group of three participants (Selten and Ockenfels 1998). However, opposed to most lab experiments people could interact non-anonymously with each other and communicate prior to their solidarity decision, but they were not able to infer the solidarity transfers they received from one person during or after the game. We randomly determined for each participant whether or not she would lose some of her initial endowment. This information was then given to all three members of the risk-sharing group before they made their private

[7] Sonderskov (2010) finds the same correlation, but argues that high generalized trust types self-select into collective action.

[8] Note that these two effects might also work in opposite directions. Collective action might create larger networks and thereby reducing social contact to the network members which might decrease the level of personalized trust. However, this possibility does not interfere with our model assumptions in Section II.

[9] We introduce assumptions on the relation between collective action and network strength. We try to test them explicitly and secondly as a robustness check drop members from the same organizations to exclude obvious collective action effects here.

solidarity decision. While we fixed the network size to three we randomly varied the strength of peoples' network in order to measure the causal link between network strength and solidarity. [10] One third of the participants in our experiment were randomly selected some days ahead of the experiment. Each of them had two additional invitation letters as well as the instruction to invite representatives from two more distinct households of their choice, completing the subject pool. In half of the villages, participants remained in their *self-selected groups* of three, i.e. they had registered themselves together with two family members or friends. In the other half of the villages, participants were *randomly re-assigned* to two new co-players. Thus, we are able to compare stronger networks consisting of self-selected family and friends with weaker networks consisting of random villagers. We further collected individual data on membership in organizations which is used as a measure of collective action. Based on the model for solidarity transfers we can distinguish between two of the above mentioned channels through which collective action affects solidarity: conditional cooperation and generalized trust.

The model's identification strategy is based on the unique setup of our experiment and has two central components. First, with higher conditional cooperation trust becomes critical for cooperation which increases the importance of personalized trust for solidarity. In contrast, higher generalized trust decreases the importance of personalized trust because trust exists even towards people not known very well. Second, the experimental re-assignment of participants to groups in half of all cases decreases personalized trust, but leaves all other personal characteristics unchanged. This combination now allows discriminating between collective action's relation with conditional cooperation and generalized trust because each of the channels predicts effects to be more positive in a different type of groups. Effects of conditional cooperation should be more positive in self-selected groups, but generalized trust should increase solidarity more in random groups.

We find that network strength has a positive effect on solidarity transfers which in our view is mainly a consequence of increased personalized trust. We furthermore assess which groups and associations promote higher solidarity and whether this is due to higher

[10] We rely on a controlled experiment since this is the only way to exogenously vary network strength but also because questionnaire measures of solidarity transfers are flawed. Comola and Fafchamps (2010) show that the information on monetary transfers from the receiver and sender is largely inconsistent. It could be possible that respondents are prone to interviewer effects or memory errors that lead to an over-reporting of financial support extended to family and friends and an underreporting of financial transfers to the larger network. Problems with not incentivized subjects are that they might be more likely to give socially desirable or strategic answers. Furthermore biases in misreporting transfers over a longer period (one week, month, year) could be due to primacy effects, recency effects, availability heuristics or egocentric bias. Besides measurement issues, risk-sharing on village level might be implicit where consumables are traded against non-consumables or risk-sharing may take other forms of solidarity (e.g. offering temporarily labor, taking care of the children in order that someone else can migrate to work, offering goods, etc.).

generalized trust or an increased share of conditional cooperators. Depending on organization type, collective action might in fact relate to both generalized trust and cooperator type. While local political activity predominantly correlates with conditional cooperation, membership in associations and cooperatives is rather linked to higher generalized trust. An aggregate analysis of collective action thus hides important differences across organizations. Although we cannot identify the causal direction between collective action and norms/beliefs we can distinguish the two above-mentioned channels.

The remainder of the paper is organized as follows. Section II describes the experimental setup including treatments, hypotheses, implementation and subject pool. Section III presents a simple model to illustrate how solidarity might be influenced by specific trust, generalized trust and cooperator type. We discuss the measures of network strength and collective action in Section IV. Empirical results on their relation with solidarity transfers are shown in Section V and Section VI concludes.

II. Setup of the experiment

[This section – in an adapted form – is drawn from chapter 3 of this dissertation which is also published as a discussion paper (Landmann, Vollan, and Frölich 2012).]

We model risk in a behavioral game using lotteries that involve rolling a dice.[11] 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.[12] This design reflects the risk to lose money instead of providing participants with the possibility of winning money.[13] Transfers are implemented in groups of three according to the standard solidarity game procedure (Selten and Ockenfels 1998).[14] 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. This is different from Selten and Ockenfels (1998) who use the strategy method and let players decide about transfers before the shock happens. Even though in principle everybody can transfer to everybody, we focus on transfers from the better-off to the worse-off as a measure of solidarity.

- Treatments -

Our behavioral experiment is designed to test the effect of network strength. The groups of three players were thus 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

[11] We benefited from the work of Barr and 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 and 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.

[12] 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.

[13] Harrison and Rutström (2008, 90) stress the importance of the reference point, referring to prospect theory (Kahneman and Tversky 1979) that allows subjective probability weighting, a reference point and different utility functions for losses and gains. We therefore consider it important to play in the loss domain.

[14] 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.

experiment. The players who knew each other very well would form a solidarity group. We will refer to these as *self-selected groups* or as strong networks. In the other half of the villages, however, groups were re-matched and participants would play with two random partners from their village (*random groups*). The analysis of this treatment is an important part of this paper and we will further describe characteristics of the two group types with different link strength in Section III.

As an additional experimental variation, we account for the fact that people can actively avoid risk (at some ex ante cost). Besides the standard lottery we thus offer *safer lottery options* in some village-rounds. We first explain the standard lottery, which we refer to as Option A. Every participant has an initial endowment of 200 Philippine Pesos. This is the amount they can keep 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 (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 alternative variants are called option B and option C. For option B participants have to pay 45 Pesos in advance and half of all losses can be avoided. The price for option C is only 20 Pesos, but half of only the catastrophic loss can be avoided. (The prices 20 and 45 are chosen to reflect some economic cost when deliberately averting risk.) Table 1 shows the payout for the different lottery options. The advance cost of less severe shocks is 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.

Table 1: Losses (in PhP) under different lottery 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 lottery 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 to have suffered 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.[15] 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 network strength under different circumstances, the behavioral experiments were implemented as outlined in Table 2. In eleven villages groups were self-selected and in the other half they were formed at random. In order to test interaction effects with the secret saving possibility, the secret saving device was implemented in the first two rounds in half of all villages.[16] Lottery choice also varies across villages and rounds. Note that while the different lottery choices are balanced across random and self-selected groups in each round, the secret saving possibility is slightly unbalanced. In the regressions we will therefore control for the experimental setup.

[15] 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.
[16] 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.

Table 2: Treatment plan for network strength under different circumstances

	Self-selected groups 11 villages	**Random groups** 11 villages
Round 1	3 A / 4 AB / 4 AC	3 A / 4 AB / 4 AC
	5 save / 6 no save	6 save / 5 no save
Round 2	11 A	11 A
	5 save / 6 no save	6 save / 5 no save
Round 3	3 ABC / 4 AB / 4 AC	3 ABC / 4 AB / 4 AC
	1 save, 10 no save	2 save, 9 no save

Note: In each block in some of the villages the games were played with different choice between the three lottery options (A,B,C). "A" means no choice, "AB" means choice between lottery option A and B, "AC" means choice between lottery option A and C, "ABC" means choice between lottery option A, B and C. The notion "11 A" in round 2 means that there was no lottery choice whatsoever in the second round. Also, village-rounds were divided into one part with "secret-saving device" and another part without. This is indicated by "5 save, 6 no save", for example. The notion "5 save, 6 no save" in Round 1 and 2 within self-selected groups means that there were 5 villages with the secret-saving device and 6 Villages without.

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.

- 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 labeled "random group", where random group takes the value 0 in the former villages and the value 1 in the latter villages. This is to indicate that in the former villages, the groups were self-selected whereas in the latter villages, the groups were randomly assigned.

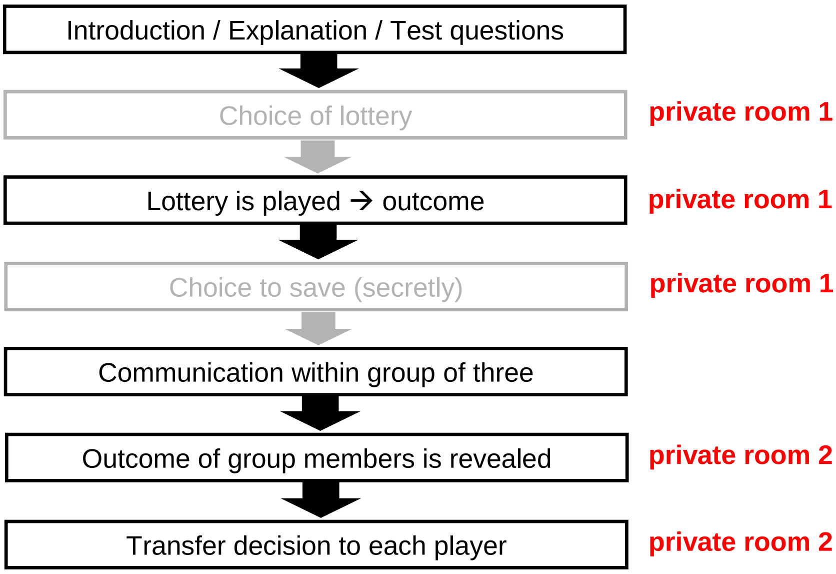
The groups stayed together for all three rounds and people in a group knew the other two members. After answering the pre-questionnaire, participants were seated to receive the introduction to the game. In an effort to make the rounds as independent from each other as possible, we made sure that signaling, 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.

The complete experimental procedure of one round is summarized in Figure 2. It is repeated three times (three rounds). 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, participants are given a choice of lotteries (see Table 2). 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 (see Table 2) 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 (the only moment when communication is possible), 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 which lottery was chosen, or whether shocks took place or whether secret savings have been made. From these payouts, however, one can induce which lottery option was chosen. 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: 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.

[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.

Figure 2: Experimental Procedure (repeated three times)



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 participants within the selected barangay (lowest administrative level on the Philippines and

[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.

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 – 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=450)			
Variable	Mean	Std.	Min	Max
Male	0.32		0	1
Household head	0.31		0	1
Married	0.81		0	1
Highest education: highschool	0.45		0	1
Highest education: college or above	0.25		0	1
Age (in years)	42.6	12.01	18	69
Regular monetary income? (dummy)	0.24		0	1
Skipped meals in hh last month	0.30		0	1
In debt with more than 1000 Pesos?	0.57		0	1

Descriptive statistics of the participants are presented in Table 3. Most of them are female (68%), 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 (45% stopped at this level and an additional 25% reached college). Average age is 43 years. 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]

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

III. A model of solidarity

According to Selten and Ockenfels (1998, 518):

> "Solidarity means a willingness to help people in need who are similar to oneself but victims of outside influences such as unforeseen illness, natural catastrophes, etc. [...] [Gifts] are made to recipients who presumably, if one were in need oneself, would have made a gift to oneself."

This implies important prerequisites for transfers to take place. (1) People do not always help, but their willingness to transfer depends on their beliefs about the recipient. Thus, people are *conditional cooperators*. (2) The important belief is that the recipient would also help in case the sender was in need. In other words the sender has to *trust* the recipient in the sense of expecting the recipient to be a cooperative type. From these observations we start out with a simple model. Together with the modeling assumptions we will also discuss several distributional assumptions. They will allow translating implications of the model to observations in the data.

- A simple model -

Assume that there are three types of participants. *Unconditional cooperators* help independent of their belief about the recipient. The solidarity of *conditional cooperators*, on the other hand, depends on their trust in the recipient. Finally, *non-cooperators* independently of their trust in the recipient never help. The latter two can be related to types identified by Fischbacher, Gachter, and Fehr (2001) – conditional cooperators and free riders.[25] From the categorization into the three cooperator types it follows that trust is only important for conditional cooperators. Let us distinguish two forms of trust at this point. *Generalized trust* applies to people in general, i.e. also people I do not know very well and that are not part of my social network. *Personalized trust* in contrast applies to a specific personal link. It is the simple answer to the question whether I believe a specific person would help me in case I need help.[26]

[25] Fischbacher, Gachter, and Fehr (2001), as well as Kocher et al. (2008) in a replication across three continents, do not find unconditional cooperation, but in principle this kind of behavior is possible and we account for it in the model. Fixing the share of unconditional cooperators at zero would change nothing of our results.

[26] Conceptually distinguishing generalized and personalized trust is based on social capital literature and especially supported by recent research on 'spheres of trust' (Freitag and Traunmüller 2009).

Thus cooperation occurs if the sender is an unconditional cooperator, or if the sender is a conditional cooperator and there is trust (general or personalized to recipient). Assuming that willingness to cooperate leads to transfers the above ideas can be translated into the following formal model:

$$T_{ij} = f(coop_{ij}) + \varepsilon_{ij} \quad \text{with} \quad \begin{array}{l} coop_{ij} = cc_i \bullet 1(gt_i = 1 \cup pt_{ij} = 1) + uc_i \qquad \cap \\ f(1) > f(0) \end{array} \tag{1}$$

where T_{ij} is the transfer from individual *i* (sender) to individual *j* (recipient), ε_{ij} is an (independent) idiosyncratic error term, $coop_{ij}$ is a dummy representing willingness of the sender *i* to cooperate with recipient *j*, cc_i is a dummy indicating whether the sender *i* is a conditional cooperator, uc_i identifies unconditional cooperators, gt_i indicates generalized trust of sender *i*, and pt_{ij} is a dummy for personalized trust i.e. whether sender *i* trusts recipient *j*. Trust variables are binary, but they can also indicate whether a minimum level of (otherwise continuous) trust is reached.

Here, willingness to cooperate is the only systematic factor determining transfers and there is always a constant (positive) effect of increasing willingness to cooperate.

Let us now make some assumptions on the distribution of personalized/generalized trust and cooperator type. They will allow translating implications of the model to observations in the data.

(Assumption 1)

$$P[pt_{ij} = 1 \mid gt_i = 0, cc_i = 1, act_i, rand_i = 0] - P[pt_{ij} = 1 \mid gt_i = 0, cc_i = 1, act_i, rand_i = 1]$$
$$=: \Lambda_+ > 0$$

Remember that in half of the villages groups were self-selected ($rand_i = 0$) and in the other half participants were randomly reassigned to a new group ($rand_i = 1$). The above assumption means that conditional cooperators without generalized trust in self-selected groups have a higher average level of personalized trust than in random groups, irrespective of the collective action status (act_i). The reason behind this is that the group members know each other better. Thus there is a higher probability that the co-players are in the set of persons the participant trusts. With random co-players this should obviously be the case less often. Additionally the assumption implies that the positive effect of being in a self-selected group on personalized trust should be the same irrespective of collective action status. The latter would be trivially fulfilled if personalized trust did not depend on collective action at all.[27] However, we allow for level effects, i.e. organization members may have different average levels of personalized trust, but the difference should not depend on whether they are in self-selected or random groups.

(Assumption 2) $act_i, gt_i, uc_i, cc_i, nc_i$ are jointly independent of group formation type

The allocation of participants to exogenous (random) or endogenous (self-selected) groups is a purely random process. Thus, while generalized trust might dependent on co-operator type, their joint distribution should be the same for both group types. This is very plausible because neither *generalized* trust, nor cooperator type should depend on which people I play with.

It is important to emphasize that assumption (2) only holds because group formation is a randomized treatment within our experiment. This is the core of our identification strategy, as we can alter personalized trust (assumption 1), holding everything else constant (assumption 2). This feature will allow us to conveniently exchange certain conditional moments of the distribution with unconditional moments (w.r.t. group formation).

As discussed in the introduction, collective action is sometimes associated with a higher share of conditional cooperators and more generalized trust in the literature. There are consequently two obvious channels through which collective action might be linked to solidarity:

I) mainly through conditional cooperation (in the following sense):

$$P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] > P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0]$$

II) mainly through generalized trust (in the following sense):

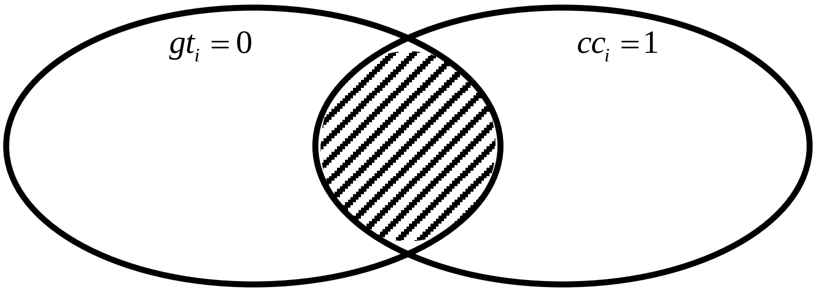
$$P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] < P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0]$$

To illustrate the above idea consider Figure 3 below. If collective action relates predominantly to conditional cooperation, then the intersecting set ($cc_i = 1 \cap gt_i = 0$) should increase. On the other hand, if collective action relates predominantly to generalized trust, then the share of individuals without generalized trust is lower and the intersecting set should decrease. Why is this so important for the identification strategy? The logic is as follows: An increase of

[27] To justify this assumption and as a robustness check, we exclude recipients that are in the same organization from the regression analysis in one specification.

personalized trust should only affect conditional cooperators without generalized trust. If the share of such types is higher, the effect of personalized trust is higher, and vice versa.

Figure 3: Trust and cooperator type distributions



Given Assumptions (1) and (2) we can derive the following corollaries:

(Corollary 1) If collective action works through channel (I) it follows

$$E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 0]$$
$$> E[T_{ij} \mid act_i = 1, rand_i = 1] - E[T_{ij} \mid act_i = 0, rand_i = 1]$$

Corollary 1 means that increasing collective action (and thus influencing mainly conditional cooperation) in self-selected groups has a more positive (or less negative) effect than in groups selected at random. We cannot say whether the effect of increasing collective action in general has a positive or negative effect, but the relative statement clearly follows from the model.

(Corollary 2) If collective action works through channel (II) it follows

$$E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 0]$$
$$< E[T_{ij} \mid act_i = 1, rand_i = 1] - E[T_{ij} \mid act_i = 0, rand_i = 1]$$

Increasing collective action and mainly increasing generalized trust implies exactly the opposite. Here, groups formed at random profit more. Proofs of the corollaries can be found in the appendix.

- Extending the model -

Let us depart from the simple assumption that there are no other factors determining transfers other than trust and cooperator type. Wealth, age, gender and prior obligations might be amongst alternative drivers of redistribution. More importantly, they might correlate with membership in collective organizations (e.g. members in organizations are more likely to lend each other money). Thus assume that characteristics of the sender and recipient x_{ij} are also functionally related to transfers (together with the cooperation formula):

$$T_{ij} = f(coop_{ij}, x_{ij}) + \varepsilon_i \qquad \text{with}$$

$$\left.\begin{array}{l} ccop_{ij} = cc_i \bullet 1(gt_i = 1 \cup pt_{ij} = 1) + uc_i \\ f(1, x_{ij}) > f(0, x_{ij}) \end{array}\right. \cap \qquad (1^*)$$

Transfers are now not solely determined by cooperation but rather also by other factors. The only thing we require is *on average* transfers will be higher with willingness to cooperate than without (for any combination of x_{ij}).

According to the new setup we have to strengthen assumption (2) slightly.

(Assumption 2*) $gt_i, uc_i, cc_i, nc_i, x_{ij}, \varepsilon_i$ are jointly independent of group formation type

This should be also fulfilled by construction, as the sample is split at random in two halves which should have the same joint distribution of covariates, types, etc. Also, we introduce two new assumptions.

(Assumption 3) $Cov[f(1, x_{ij}) - f(0, x_{ij}), h(act_i, x_{ij}) \mid act_i]$ is constant for $act_i = 0,1$

with $h(act_i, x_{ij}) = E[coop_{ij} \mid act_i, rand_i = 0, x_{ij}] - E[coop_{ij} \mid act_i, rand_i = 1, x_{ij}]$

Assumption (3) requires that effect of increasing willingness to cooperate from 0 to 1 on transfers has the same covariance with the effect of altering group formation type from random to self-selected on (expected) willingness to cooperate amongst those with and without collective action.

(Assumption 4) $E[f(1, x_{ij}) - f(0, x_{ij}) \mid act_i] = E[f(1, x_{ij}) - f(0, x_{ij})]$

Assumption (4) requires that effects of increasing willingness to cooperate from 0 to 1 on transfers do not depend on collective action status on average.

We do not see an ad-hoc reason why these two new assumptions should be violated. In fact, for example assuming a constant effect of willingness to cooperate, such that $T_{ij} = f_A(coop_{ij}) + f_B(x_i, x_j) + \varepsilon_i$, would already imply assumptions (3) and (4). Yet, we are less restrictive than that. Besides assumptions (1) to (4), there can be arbitrary selection, especially regarding collective action status and potentially unobserved covariates influencing transfers.

From here, one can show that

$$\begin{aligned} &E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 0] \\ &\quad < (>) E[T_{ij} \mid act_i = 1, rand_i = 1] - E[T_{ij} \mid act_i = 0, rand_i = 1] \end{aligned} \tag{2}$$

$$\begin{aligned} &\Leftrightarrow E[coop_{ij} \mid act_i = 1, rand_i = 0] - E[coop_{ij} \mid act_i = 1, rand_i = 1] \\ &\quad < (>) E[coop_{ij} \mid act_i = 0, rand_i = 0] - E[coop_{ij} \mid act_i = 0, rand_i = 1] \end{aligned} \tag{3}$$

On the way to corollary (1) and (2), the latter statement was already shown to be equivalent to

$$P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] < (>) P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0]$$

We have shown that – with somewhat adapted assumptions – a much more flexible model implies the same differential effects of collective action on transfers in self-selected versus random groups. Hence, we can still translate these differential effects on transfers to different channels of influence.

The assumption can even be relaxed further, as we can repeat all proofs *conditional on observed covariates*, such that the above assumptions have to hold for unobserved covariates only. Controlling for observed covariates will add further credibility to the empirical results and its theoretical explanation.

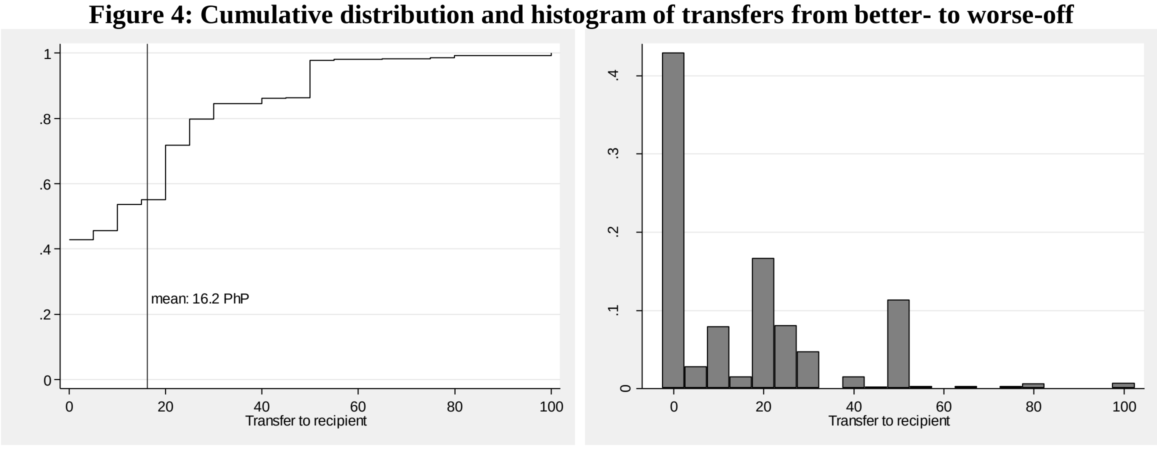
IV. Network strength, collective action and transfers in our sample

In this section we empirically describe the three key concepts of this paper. We start with a description of our solidarity measure, which is the outcome of interest. Afterwards, we characterize groups with different network strength. Randomized variation in group formation is part of the particular design of our experiment. Self-formed groups have closer links than groups formed at random which also shows up in our data and should translate to higher personalized trust. Finally, we will describe participants doing collective action.

- Solidarity transfers -

In principle each participant can transfer to any other group member in our experiment. Yet, we focus on transfers from the better-off to the worse-off as a measure of solidarity. These are the transfers that align best with the definition of solidarity presented at the beginning of Section III. Transfers from the worse- to the better-off should be rather driven by considerations unrelated to the game, such as real world wealth differences, or by other motives than solidarity. These transfers from the worse-off are considerably less frequent and lower in magnitude: 33% transfer something, but only 17% transfer more than 10 pesos (as compared to the 57% and 46% from the better-off). Figure 4 displays the distribution of the 968 observed payments from (better-off) sender to (worse-off) recipient.[28]

Figure 4: Cumulative distribution and histogram of transfers from better- to worse-off

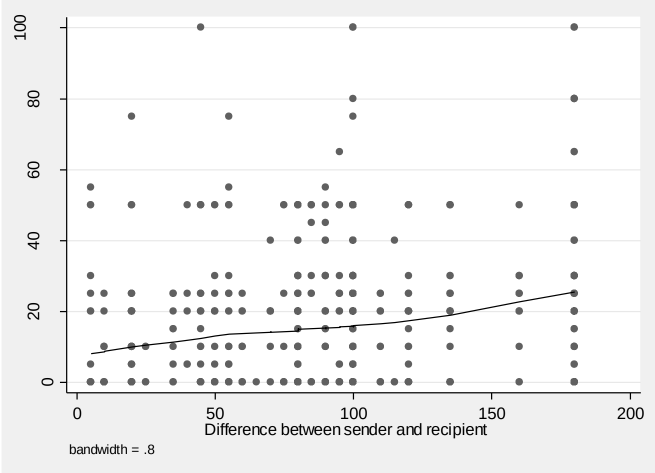


Solidarity transfers after shocks vary greatly between 0 and 100, with a mean of 16.2 pesos. The standard deviation is 19.4 pesos. In 43% of all cases transfers are zero. To put the values into perspective, remember the loss schedule presented in table 1 for different lottery types

[28] Each of the 450 participants makes two transfer decisions per round, but in less than half of the cases there is a strictly positive difference between their payoffs.

(with an initial endowment of 200). After the lottery stage, the better-off have on average 173.4 PhP and the difference to the worse-off varies between 5 and 180 PhP, with a mean of 93.9 PhP. Figure 5 displays a nonparametric smoothing of the relation between transfers and the post-lottery difference of the sender to the recipient.

Figure 5: Lowess smoothing: solidarity transfers and pre-transfer differences



While there is a lot of variation, there is clearly a positive trend of transfers with differences. This illustrates that transfers are more strongly triggered with increasing inequality. When analyzing the relationship between network strength, collective action and solidarity, the differences in inequality should not be a problem, as they are caused by random shocks. Nevertheless, we will use this difference as an important covariate reducing unexplained variation in our regression analysis.

- Network strength -

Table 4 shows that self-selected groups have a considerably higher share of close family members, see each other more frequently, know each other longer, work together more often and spend more free time together. Most of the differences are significant. Even when accounting for potential correlation of error terms on the village level most of them are significant at least at the 5% level.[29]

[29] Note that any test assuming independent draws is a very strict balancing test because it does not account for correlation of variables for example on the village level. Such clustered errors lead to less precision in means than predicted with independent draws. Therefore, a test assuming independence could reject equality even when the difference in means is simply due to a (random) effect on the village level. We account for correlation of error terms on the village level by estimating a simple panel regression model where a dummy for type of group formation is the only explanatory variable, but we allow for village random effects. Note that fixed effects models are no alternative, as randomization of groups is taken on the village level. Also, there is no reason to believe that the village-specific error component should be correlated with any other variable due to our randomized design.

We believe that there is a lot of misreporting or measurement error in the data. First, it seems implausible that there is still such a high proportion of close family members in groups formed at random (29% vs. 53% in self-selected groups). Also, there are inconsistencies when comparing the information that co-players provide about each other. However, even though misreporting and measurement error cannot be ruled out, the information provided by participants is far from being useless. There are indeed large differences between random versus self-selected groups and correlations between both answers from both sides of a dyad are highly significant. This shows that reality is at least a substantial part of the answer.[30]

Table 4: Link strength characteristics, by type of group formation

	All	Self-selected groups	Random groups	Z values:
Relation: close family	0.41	0.53	0.29	3.078***
Relation: extended family	0.20	0.15	0.25	-2.400**
Relation: close friend	0.20	0.18	0.21	0.497
Relation: neighbor	0.16	0.12	0.21	-1.758*
Relation: do not like / know	0.03	0.01	0.05	-2.024**
Observations	895	460	435	
Frequency of seeing: every day	0.49	0.57	0.40	2.023**
Frequency of seeing: almost every day	0.16	0.18	0.14	1.129
Frequency of seeing: once/twice in a week	0.28	0.22	0.35	-2.098**
Frequency of seeing: do not know / never	0.07	0.04	0.10	-2.002**
Observations	692	376	316	
Duration of knowing: whole life	0.29	0.38	0.19	2.199**
Duration of knowing: many years	0.51	0.51	0.51	-0.078
Duration of knowing: some years	0.16	0.10	0.23	-2.977***
Duration of knowing: recently	0.04	0.01	0.07	-2.644***
Observations	689	376	313	
Common activities: working	0.12	0.18	0.06	2.435**
Common activities: voluntary work	0.49	0.46	0.53	-1.223
Common activities: free time	0.38	0.41	0.33	1.367
Observations	692	376	316	

Note: observations are statements about link to co-player (each participant has two co-players), stars indicate significant difference between groups accounting for intra village correlation (*** p<0.01, ** p<0.05, * p<0.1), test implemented via regression on group formation dummy with village random effects

Due to the randomized assignment to treatments, we expect that all characteristics should be balanced between random and self-selected groups, but in reality there can always be small sample correlations. When comparing self-selected and random groups in table 5 most differences are indeed insignificant and samples seem to be well balanced. Only the proportion of participants with regular income is significantly higher for self-selected groups,

[30] Compare table A1 in the appendix and the related discussion for more information on consistency of the answers.

but accounting for potential correlation of error terms on the village level this significance is only weak.

Table 5: Descriptive statistics of participants (means), by type of group formation

	All	Self-selected groups	Random groups	Z values:
Male	0.32	0.34	0.29	0.665
Household head	0.31	0.31	0.31	0.103
Married	0.81	0.80	0.82	-0.444
Highest education: highschool	0.45	0.42	0.47	-0.923
Highest education: college or above	0.25	0.23	0.28	-1.014
Age (in years)	42.61	42.39	42.84	-0.415
Regular monetary income? (dummy)	0.24	0.28	0.19	1.657*
Skipped meals in hh last month	0.30	0.31	0.28	0.477
In debt with more than 1000 Pesos?	0.57	0.57	0.58	-0.084
Observations	450	231	219	

Stars indicate significant difference between groups accounting for intra village correlation (*** p<0.01, ** p<0.05, * p<0.1), test implemented via regression on group formation dummy with village random effects

- Collective action -

The data we collected also includes information regarding collective actions. One aspect of the latter is the membership in organizations, networks and associations. We have information on up to three such formal or informal institutions (in the following called organizations) that participants visit regularly to talk about things, do activities or simply get together. Around 40% belong to at least one organization. Table 6 shows the most common organizations by type of game group formation. Many of the participants are engaged in the barangay affairs either as officials (elected or appointed), or with some other job (e.g. health or day care worker).[31] Each of these categories has an incidence between eight and nine percent. An equally large fraction is organized in women organizations. Around four percent meet in cooperatives/associations (e.g. for farmers and fishermen) or in lending groups, respectively. Additionally there is a significant fraction of 12 percent in different organizations whose purpose is difficult to categorize, many of which are religious.

[31] Each barangay has a legislative council of 8 members, plus an appointed secretary, tanods (barangay police) and members of committees for each councilor. Given that on average our communities have 235 households the fraction of household representatives in our sample seems realistic.

Table 6: Participation in collective action, by type of group formation

	All	Self-selected groups	Random groups	Z values:
Any membership?	0.40	0.45	0.34	-1.699*
Barangay: official	0.09	0.11	0.07	-1.007
Barangay: other job	0.08	0.10	0.06	-1.201
Cooperative/association	0.04	0.05	0.02	-1.583
Women organization	0.08	0.07	0.09	0.300
Lending group	0.04	0.06	0.02	-1.366
Other organization	0.12	0.14	0.11	-0.456
Observations	450	231	219	

Note: observations on participant level, participants can mention more than one organization, stars indicate significant difference between groups accounting for intra village correlation (*** p<0.01, ** p<0.05, * p<0.1), test implemented via regression on group formation dummy with village random effects

For a separate analysis of network strength effects and collective action, it is important that both concepts are not interrelated. Table 6 also shows participation in collective action for random and self-selected groups separately. Membership in organizations is somewhat lower in villages where groups were formed at random. However, average membership amongst the invited participants should obviously not be influenced by the type of group formation which was announced after everybody has turned up and filled out the pre-game questionnaire. After accounting for village random-effects we indeed only find a low significance level of the difference for organizations in general and no significant differences at all in the subcategories.[32]

Note that contrary to network strength our collective action measures are purely observational. Thus there are good reasons to believe that members of organizations systematically differ from non-members. People might select into collective action because they are motivated by social norms, preferences or beliefs. This relates to the debate about the causal effect of collective action versus self-selection, as discussed in the introduction (e.g. Hayo and Vollan 2012). On the other hand, participation in organizations could also relate to other aspects such as socioeconomic status, where causality again could go either way (Adjakidje, Metangmo, and Segla 2010). Table 7 confirms that observable characteristics of participants vary much more between members in organizations (collective action) and non-members (no collective action) than between random and self-selected groups (in table 5). Most characteristics differ at least at a significance level of 5% level and there is virtually no

[32] The way we controlled for error correlation on the village level is by estimating a linear panel model with group formation type as the only explanatory for each of the outcome variables. The procedure is analogous to what is explained in footnote 29.

change when accounting for village random effects.[33] There are more women and fewer household heads among participants engaged in collective action. This is consistent with the high participation in women's organizations. Also, collectively organized individuals seem to be higher educated, older, much more often earn regular income and are more often indebted. The fact that those involved in collective action are more indebted remains true even when excluding those active in lending organizations.[34]

Table 7: Descriptive statistics of participants (means), by participation in collective action

	Collective action	No collective action	Z values:
Male	0.26	0.35	2.006**
Household head	0.25	0.35	2.011**
Married	0.84	0.79	-1.275
Highest education: highschool	0.45	0.44	-0.197
Highest education: college or above	0.35	0.19	-3.822***
Age (in years)	44.64	41.26	-2.901***
Regular monetary income? (dummy)	0.47	0.08	-10.993***
Skipped meals in hh last month	0.27	0.31	0.224
In debt with more than 1000 Pesos?	0.70	0.49	-4.650***
Observations	179	271	

Stars indicate significant difference between groups accounting for intra village correlation (*** p<0.01, ** p<0.05, * p<0.1), test implemented via regression on collective action dummy with village random effects

[33] Collective action varies within the village. Hence, there are not too many reasons to expect that village random effects matter too much at this point.
[34] The major part of the difference is due to a higher proportion of loans from banks, microfinance institutions (MFI) or money lenders. More loans from banks and MFI's would hint at better access to formal lending for organization members. Unfortunately, we cannot distinguish between the three categories.

V. The effect of network strength and collective action on solidarity transfers

In the prior section we described characteristics of solidarity transfers, network strength and collective action in our data. We will now link these variables to solidarity transfers to answer two questions: 1) how network strength affects solidarity transfers and 2) how membership in organizations correlates with solidarity transfers. We use descriptive comparisons and matching analysis separately for the two questions, and bring them all together using multivariate regressions.

As emphasized before, variation in network strength is exogenous, i.e. participants in random and self-selected groups should on average have the same characteristics except the strength of their relation within groups. Membership in organizations, on the other hand, is not exogenous – participants doing collective action might be very different from the rest in many ways. We thus cannot interpret differences in solidarity transfers as a causal consequence of collective action. Still, solidarity is an important characteristic of social interaction and it is important to characterize those doing collective action along this dimension as well. Furthermore, the model presented in Section III implies that, depending on which norms or beliefs are related to membership in organizations, collective action should have different effects in random versus self-selected groups. Using our unique experimental setup to identify the channels through which membership in different organizations works is another contribution of our analysis.

- Network strength and solidarity -

Table 8 compares self-selected and random groups regarding solidarity transfers (T_{ij}). Remember that the initial endowment is 200 PhP, the mean difference between better- and worse-off is 93.9 PhP, and that we only consider transfers from the better- to the worse-off as solidarity transfers. The difference between self-selected and random groups regarding transfers is also shown with and without secret saving possibility to assess whether stronger relationships are important when there is private information.

Table 8: Solidarity transfer amounts from better-off to worse-off, by type of group formation

	Descriptive comparison			Exact matching comparison (ATE)		
	Self-selected groups	Random groups	Diff.	Self-selected groups	Random groups	Diff.
All	18.7	13.5	**+5.2*** **			**+4.1*** **
Observations (on/off support)	494	474		400 / 94	408 / 66	
Without secret saving	19.8	16.5	**+3.3*** **			+2.9
Observations (on/off support)	337	271		274 / 63	225 / 46	
With secret saving	16.3	9.6	**+6.7*** **			**+6.2*** **
Observations(on/off support)	157	203		126 / 31	183 / 20	

a) Descriptive comparison: Stars indicate significance level of Wilcoxon ranksum test for differences in distributions. (*** p<0.01, ** p<0.05, * p<0.1)
b) Exact matching: matching on shock distribution, available insurance type, round and saving possibility, stars indicate significance level obtained by 200 bootstrap iterations (*** p<0.01, ** p<0.05, * p<0.1)

Self-selected groups display a significantly higher level of solidarity as measured by absolute transfer amounts. They on average transfer 5.2 PhP more than random groups, which is about one third more. The difference is higher when secret saving is possible. It seems that groups formed at random react more to the availability of secret saving.

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 (compare figure 5). 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.[35] Also, there is a small imbalance in the treatment plan, as there is one village more with secret saving possibilities in the sample with random groups. To deal with this issue, we examine the following nonparametric matching estimates of the average treatment effect (ATE). Transfers are only compared when the shock distributions in the groups and treatment combinations are identical.

Results are similar when performing the matching. Significance levels are very high for the overall sample and for those with the saving device, but insignificant for the no-saving subsample. In sum, exact matching and descriptives both suggest that people in stronger networks are willing to transfer more and are less easily influenced by secret saving possibilities. Both aspects are compatible with higher personalized trust in self-selected groups (compare model in Section III). While personalized trust should be important for

[35] 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.

solidarity in general, it might become even more important when there is private information. We will come back to the above analysis when we integrate all aspects it in a regressions subsection further below.

- Collective action and solidarity -

In this part we describe the relation between collective action – measured by membership in organizations – and solidarity. Also remember that our model developed in Section III predicts different effects of collective action in random versus self-selected groups. If collective action was correlated with conditional cooperation, effects should be higher in self-selected groups (corollary 1). The reason is that trust is more important for conditional cooperators and personalized trust is higher in self-selected groups. Conversely, if collective action was correlated with generalized trust, effects should be higher in random groups (corollary 2). The reason here is that generalized trust becomes more important if personalized trust is low. We therefore also separately consider collective action effects in random vs. self-selected groups.

The following table 9 shows descriptive results as well as matching results (analogous to the methods used in table 8). There is no conclusive evidence for an association between collective action of the sender and solidarity transfers. Descriptive comparisons somewhat hint at higher levels of transfers, but the differences are insignificant, and controlling for the exact shock distributions and the setup of the experiment via exact matching suggests even smaller differences. Differentiating between random and self-selected groups also does not hint at large differences.

Table 9: Solidarity transfer amounts from better-off to worse-off, by collective action

	Descriptive comparison			Exact matching comparison (ATE)		
	Collective action	No collective action	Diff.	Collective action	No collective action	Diff.
All	17.9	15.0	+2.8			-0.4
Observations (on/off support)	382	586		291 / 91	344 / 242	
Self-selected groups	19.4	18.1	+1.3			-1.0
Observations (on/off support)	221	273		170 / 51	167 / 106	
Random groups	15.8	12.3	+3.5			+0.4
Observations (on/off support)	161	313		121 / 40	177 / 136	

a) Descriptive comparison: Stars indicate significance level of Wilcoxon ranksum test for differences in means. (*** p<0.01, ** p<0.05, * p<0.1)
b) Exact matching: matching on shock distribution, group formation type, available insurance type, round and saving possibility; stars indicate significance level obtained by 200 bootstrap iterations (*** p<0.01, ** p<0.05, * p<0.1)

Even though we do not find significant differences there could still be a causal link from collective action to solidarity that is hidden by omitted variables biasing the comparisons. Yet, the collective action effect is similar in self-selected and random groups. According to our model, this shows that our overall measure of collective action (membership in any organization) is neither predominantly related to conditional cooperation nor predominantly to generalized trust. Our data, however allows a more detailed perspective on collective action. There are many types of organizations we can distinguish and it is straightforward to repeat our crude analysis for different organization types. Table 10 presents differences of solidarity transfers between several types of organizations and no collective action.

Table 10: Difference of solidarity transfers to no collective action status, by type of collective action

	Difference to no collective action (Z-value of ranksum test)		
	All	Self-selected groups	Random groups
Barangay: official	**+8.1***** (2.974)	**+9.8***** (2.866)	+4.0 (0.585)
Observations	87	51	36
Barangay: other job	+1.6 (0.226)	-0.6 (-0.667)	+1.9 (0.473)
Observations	72	51	21
Cooperative/association	**+**4.3 (1.458)	-2.8 (-0.654)	**+21.0***** (2.685)
Observations	40	31	9
Women organization	-2.6 (-0.744)	**-7.0*** (-1.729)	+1.2 (0.318)
Observations	74	35	39
Lending group	+2.8 (-0.227)	-2.7 (-1.226)	+10.2 (0.708)
Observations	40	26	14
Other organization	**+**4.0 (1.519)	2.7 (1.109)	+4.2 (0.290)
Observations	126	73	53

Stars indicate significance level of Wilcoxon ranksum test for differences in means.
(*** p<0.01, ** p<0.05, * p<0.1)

Indeed there are different patterns across organization types. Overall (first column) it seems that especially barangay officials transfer substantially more. Also members of cooperatives/associations appear more willing to give, but the difference is just below statistical significance. What is more interesting at this point is the large effect heterogeneity when repeating the exercise for random versus self-selected groups. While the positive effect for barangay officials appears more concentrated in self-selected groups, the effect of cooperatives seems to be confined to random groups. Also for other organizations there are large differences between group types (e.g. for women organizations), but significance levels are much lower.

Hence, contrary to the findings when looking at general collective action there are some organizations that might be related to specific norms and beliefs. Barangay officials tend to give more, but only in self-selected groups where personalized trust is high. According to our model, this hints at a stronger role of conditional cooperation in this subpopulation. On the other hand, cooperatives/associations give more in random groups only. Here behavior corresponds to higher generalized trust because generalized trust is more important in random groups when personalized trust is low.

- Regressions explaining solidarity by network strength and collective action -

In this subsection we bring together the separate perspectives from the descriptive analysis. We explain transfers using network strength and collective action as regressors at the same time. Furthermore, we will tackle part of the problem that members in organizations differ systematically from non-members (selection on observables) through additional control variables. Also non-systematic small sample imbalances in observed determinants of solidarity can be corrected by such controls.

Table 11: Tobit regressions explaining transfers from better- to worse-off

	(1)	(2)	(3)	(4)
Own Money	-0.011	-0.011	-0.007	-0.005
Difference to recipient	**0.17*** **	**0.17*** **	**0.16*** **	**0.17*** **
Saving sender	**-0.12*** **	**-0.12*** **	**-0.12*** **	**-0.15*** **
Saving recipient	0.15	0.15	**0.16***	**0.17** **
Random group	**-7.20** **	**-7.96*** **	**-8.13*** **	**-6.28***
Village official (political)	**9.02** **	**14.4*** **	**14.3*** **	**10.2*** **
Other village job	-1.68	-3.60	-2.92	1.80
Cooperative / association	6.05	2.19	0.90	1.93
Women organization	-1.58	-2.81	-4.62	2.30
Lending group	-5.06	-5.68	**-8.29***	-4.42
Other organization	0.32	-2.47	-3.12	-2.09
Random x village official		**-15.0*** **	**-17.2*** **	**-18.1*** **
Random x other village job		8.06	6.73	5.06
Random x coop / assoc		**21.9*** **	**24.2*** **	**20.0** **
Random x women organization		2.84	3.20	0.035

Random x lending group		0.54	1.67	1.79
Random x other organization		5.52	6.61	4.94
Game setting controls	NO	NO	YES	YES
Individual controls	NO	NO	NO	YES
Observations	968	968	968	968

Standard errors clustered at the village level, *** p<0.01, ** p<0.05, * p<0.1
The coefficient for the control variables are shown in the Appendix (table A2).
The individual control variables are: *Regular income, Debt larger than 1000 Pesos, Male, Household head, at least high school.*
Village-round controls are: *Treat B, Treat C, Treat B x Treat C, PseudoTreat B (lagged effect of TreatB), PseudoTreat C (lagged effect of TreatC), Round, Income Class, partially urban,people living in this community, % of HHs with members abroad, people are selfish.*

Table 11 shows a simple Tobit regression of solidarity transfers on type of group formation (*Random group*) and different types of collective action.[36] We control for the post-lottery income (*Own money*), difference between sender and recipient (*Difference to recipient*) and the amount of money secretly saved by the sender (*Saving sender*) and recipient (*Saving recipient*). Not surprisingly, differences between sender and recipient are a major driver of transfers.[37] If, however, money can be secretly saved by the sender this reduces transfers substantially. Conversely, if money is secretly saved by the recipients (and differences thus appear larger) willingness to transfer increases, but this effect is insignificant. These effects show that apparently observed transfers are most important in determining transfers and that participants use imperfect information to increase their income. The magnitude of all those four control variables remains stable across specification and the recipients' saving becomes significant in later specifications.

Besides a dummy for random groups, specification (1) includes indicators of membership in different organization types. Solidarity transfers are much lower in groups with random co-players, confirming the negative impact of lower network strength. Group formation is a randomized treatment, so the regression results reflect the causal effect of network strength on solidarity transfers. Our model implies that the effect works through increased personalized trust. Of the different collective action types only village officials exhibit a significantly higher willingness to transfer. Yet, these relationships might be confounded by many factors and an interpretation is difficult.

[36] Note that transfers cannot be negative (they are left-censored at zero). Thus Tobit regressions are the natural choice to circumvent a bias that ordinary least squares regressions would suffer from. Yet, coefficients should not be one-to-one interpreted as effects on observed transfers, but rather as effects on the unobserved latent willingness to transfer. Especially interaction effects and their significance levels have to be interpreted as effects on this latent willingness to transfer.
[37] Also compare figure 3 for a nonparametric smoothing of the relationship between post-lottery differences and transfers.

In specification (2) we introduce interactions between collective action types and group formation type. Our model predicts that if collective action is associated with increased conditional cooperation effects should be larger in self-selected groups, and the interaction between random group and collective action should be negative. On the other hand, if generalized trust is higher for organization members, then the effect of collective action should be higher in random groups and the interaction effect should be positive. Results suggest that both directions are possible. While the interaction is strongly negative for barangay officials, it is positive for participants in cooperatives/associations. In both cases effects are highly significant. Interestingly, the interaction effects remain relatively stable and highly significant when introducing further game setting controls (variables fully characterizing the experimental setup) and individual characteristics (sex, education, indebtedness, indicator for household head, regular income, community size, prevalence of migration, urbanization and a measure of selfishness) in specification (3) and (4).

It appears that depending on the type of organization it is possible that either conditional cooperation or generalized trust is related to collective action. Could this simply be an artifact of too many (wrong) modeling assumptions? To provide further answers we resort to items we included in our questionnaire answered by all participants. There are three questions that are used to assess generalized trust on the village level. People were asked to agree/disagree with the following statements:

i) Most people in this village are basically honest and can be trusted

ii) People in this village are mostly interested in their own well-being

iii) In this village one has to be alert, or someone will take advantage of you.

The answers to the three questions were used for a factorial analysis, resulting in a generalized trust factor, which is normalized to mean zero and has standard deviation one half. Additionally, there is a question that relates to reciprocity / conditional cooperation. Participants were asked to choose from several items the one which best characterizes helping behavior of the people in the community. One of the items is:

iv) People help only those people they know will also help them

The above statement is clearly about conditional cooperation. It is not clear whether the statement also requires real reciprocity to take place, or whether simple trust in the partner is enough. In any case conditional cooperation is an important part of the above characterized behavior. Asking directly about the participant's mode of behavior incurs the risk that respondents will give socially desired answers. Therefore, this question asks not about the respondents, but about all others in the village which reduces the social desirability bias associated with it. It is plausible that answers to questions are influenced by what oneself thinks is appropriate, mixed with real observations about others. We will thus use individual variation in answers as a measure for conditional cooperation. The measure is a dummy with mean 0.65.

In table 12 we compare our measures of conditional cooperation and generalized trust between organization members and non-members. Interestingly, members seem to have both, slightly higher values of conditional cooperation and generalized trust, even though differences are insignificant. When disaggregating the comparisons, an interesting picture emerges. While village officials display higher conditional cooperation and almost no difference regarding generalized trust, it is exactly the opposite for cooperatives/associations. Even though differences are substantial in both cases, they are insignificant due to measurement error and the small number of observations.[38] However the two observations are perfectly compatible with our model-based interpretation of the regression results.

Table 12: Relation between collective action type and conditional cooperation / generalized trust

	Conditional cooperation	Generalized trust
Any membership?	0.06 (1.38)	0.06 (1.29)
Village official (political)	0.10 (1.27)	-0.02 (-0.19)
Other village job	-0.00 (-0.00)	**0.16* (1.85)**
Cooperative / association	-0.03 (-0.28)	0.17 (1.41)
Women organization	0.07 (0.85)	0.02 (0.24)
Lending group	**0.20* (1.67)**	0.12 (0.94)
Other organization	0.04 (0.62)	0.06 (0.82)

Stars indicate significance of difference between organization members and non-members,
Z-values of Wilcoxon ranksum test are provided in brackets
*** p<0.01, ** p<0.05, * p<0.1

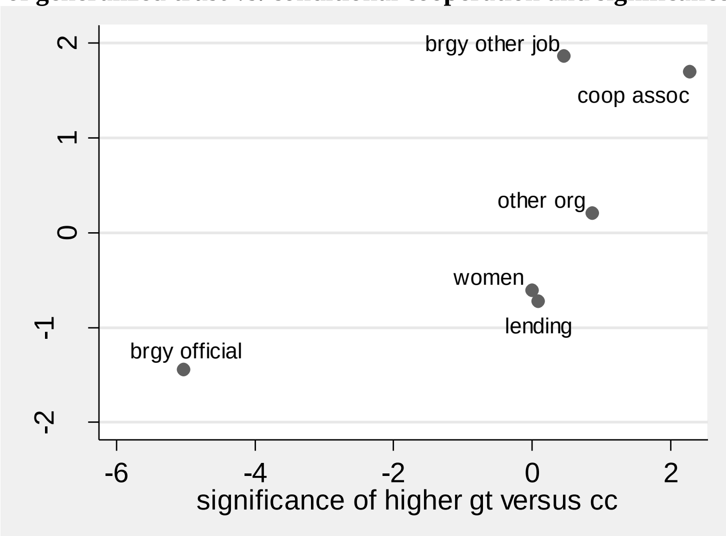
There are two other interesting observations. Participants doing some 'other village job' display significantly higher generalized trust only, and people in 'lending groups' have significantly higher conditional cooperation levels as well as relatively high generalized trust. Interestingly, this is again perfectly in line with our regression results. Participants in other village jobs also have a more positive relation to transfers in random groups. After cooperatives/associations they have the second most positive – though insignificant –

[38] Both above concepts are difficult to measure and therefore we expect a lot of measurement error. Especially the conditional cooperation measure is likely to be a mixture of the respondent's own norms (which we want to measure) and observations about others.

interaction effect. For participants in lending groups the interaction effect is around zero, as none of the two channels seems to outweigh the other.

Let us test our theoretical prediction more formally: The more generalized trust effects dominate conditional cooperation effects, the more clearly positive the interaction effect in the regression should be, and vice versa. Unfortunately, we cannot quantify the importance of a one-point increase in generalized trust versus the same increase in conditional cooperation. We thus subtract z-values of generalized trust from those of conditional cooperation in Table 12, and compare the difference with t-values of the interaction effects in regression specification (4). Those organizations with predominantly more generalized trust (measured by relative significance levels) should have more positive interaction effects (also measured by significance levels). This is illustrated in Figure 6.

Figure 6: Dominance of generalized trust vs. conditional cooperation and significance of interaction effects



Notes: Correlation coefficient 0.73 (p-level: 0.10), the x axis displays difference between z-values for generalized trust and conditional cooperation in table 12 for different organization types, the y axis displays the t-value of the interaction coefficient in table 11 for the same organizations.

The pattern in the table confirms the predicted pattern and despite the small number of observations the correlation is marginally significant. We take this as an indication that our model captures important aspects of cooperation behavior in the context of solidarity transfers.

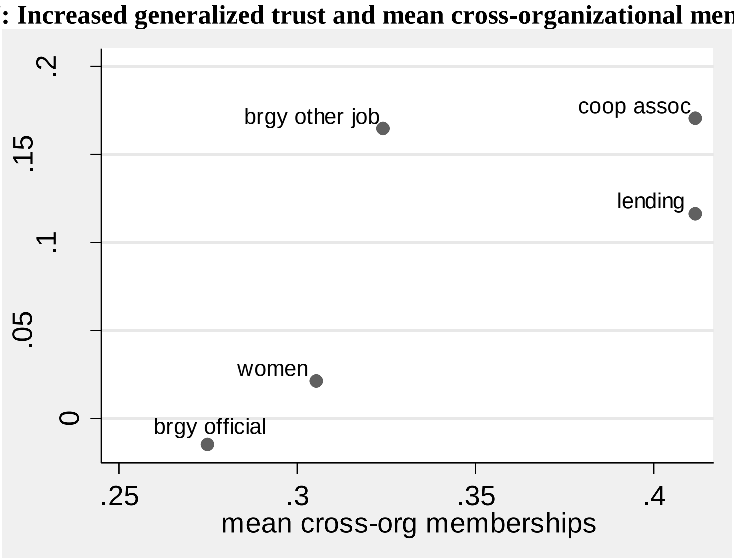
Our findings are consistent with the literature that finds generally higher generalized trust amongst members, but not for all organization types (Paxton 2007; Freitag, Grießhaber, and Traunmüller 2009; Sonderskov 2010; Traunmüller 2011), Based on Paxton (2002), organization types are often separated into connected versus isolated to rationalize where most generalized trust is built. Organizations whose members are often in other organizations should build more generalized trust. Table 13 shows the average number of additional memberships in other organizations, by organization type. Figure 7 shows that those organizations that more often have multiple memberships also feature higher generalized trust levels (correlation coefficient 0.42, p-level: 0.35).[39] Even though the finding lacks statistical significance, it fits neatly to our interpretation and modeling assumptions.

Table 13: Membership in other organizations, by collective action type

	Mean # of other memberships
Any membership?	0.240
Village official (political)	0.275
Other village job	0.324
Cooperative / association	0.412
Women organization	0.306
Lending group	0.412
Other organization	0.473

Notes: Looking at each type of organization separately we have a higher share of multiple members (which appear multiple times) than in the general pool of people with any type of membership.

Figure 7: Increased generalized trust and mean cross-organizational membership



Notes: Correlation coefficient 0.74 (p-level: 0.15), the x axis displays mean membership in other organizations, the y axis displays the difference of the generalized trust score to non-members (shown table 11).

The literature extensively deals with the relationship between collective action and generalized trust. Explanations for the connection have been developed and we explicitly

[39] The category 'other organizations' includes many religious organizations and their members disproportionally often report membership in multiple organizations, but of the same (religious) category. This does not correspond to being a connected organization in the sense of Paxton (2002) and we therefore drop this observation.

looked at the prominent hypothesis that generalized trust is built in connected organizations. In contrast, to the best of our knowledge there exists no prominent explanation *why* the norm of conditional cooperation could be stronger in organizations. When looking at our results we nevertheless think that such a relationship between organization membership and conditional cooperation is plausible. Politics involves rewarding allies and punishing opponents much more than activities in other types of organizations and hence it is plausible that local political activity strengthens conditional cooperation rather than trust. In associations/cooperatives on the other hand there should be less strategic thinking and a spirit more conductive to generalized trust. We cannot formally test this ’strategic thinking’ hypothesis without additional data, but at first glance it rationalizes our results.

VI. Conclusion

In this paper we analyze two important determinants of solidarity transfers: Network strength and experience of collective action in groups. Previous research suggests that social networks, cultural learning but also institutional and environmental factors might be determinants of being a conditional cooperator or having generalized trust. We contribute to the literature in several ways: First, we formalize these ideas within a model of solidarity transfers that is based on trust and cooperator types. We use the model to predict empirical patterns in the data and find evidence that the model captures real channels of influence. Second, the causal effect of network strength on pro-social behavior has so far only been identified in dictator-type and trust games (Leider et al. 2009; Vollan 2011; Ligon and Schechter 2012). We show the effect in the context of solidarity transfers and link the analysis to our model. Third, we relate collective action in organizations to trust and norms about cooperation. In contrast to the vast majority of the literature we rely on incentivized rather than hypothetical measures of pro-social behavior. This is especially important as recent evidence suggest that hypothetical measures of trust might not correctly capture the concept of trust in incentivized experiments (Thöni, Tyran, and Wengström 2012).

We find that people who regularly participate in collective action tend to show higher solidarity in risk-sharing networks (significantly so in some cases). While our results are not conclusive whether specific collective action causes a certain behavior (type or attitude) or whether people with a certain behavior self-select into specific forms of collective action we nevertheless find that different types of collective action might relate to different trust and cooperator types. This might partly account for the problems authors have had with measuring associational social capital as put forward by Putnam (1995). Certainly not all groups correlate with trust beyond their own sphere and not all relate to norms about cooperative behavior. Instead, there is heterogeneity in collective action types. An aggregate analysis of collective action thus hides important differences across organizations. To our knowledge there exists no similar study that tries to disentangle these two important effects on generalized trust and cooperator types for heterogeneous organizations.

We argued that generalized trust has a stronger effect than conditional cooperation in randomly formed social groups (corollary 2). This model finding reflects the theoretical idea that having generalized trust is the precondition for cooperation with anonymous partners. Such cooperation beyond one’s personal network is important for economic development as more welfare increasing market transactions and delegation to higher level institutions will take place. Knowing the type of associations where people have particularly higher generalized trust might give NGOs and Government a rationale not to interfere with the development of these groups.

We furthermore find that exogenously increasing network strength has a positive effect on solidarity transfers which in our view is mainly a consequence of increased personalized trust. This finding is consistent with the literature emphasizing the importance of friends and kinship networks for informal risk sharing.

Questionnaire measures of trust and conditional cooperation also relate to different organizations in the way predicted by the model. Although hypothetical questionnaire items might not exactly reflect real beliefs and norms, we take this as an indication that our model captures important patterns of cooperative behavior in solidarity networks. Our findings indicate the need for further research in this direction, including more direct measures of trust and conditional cooperation.

Appendix I

Table A1: Correlation between answers by two co-players about each other, by type of group formation

	Self-selected groups	Random groups
Relation	0.4986***	0.4212***
Observations	229	217
Frequency of seeing	0.2889***	0.2992***
Observations	187	157
Duration of knowing	0.3910***	0.4145***
Observations	187	154
Common activities: working	0.2182***	0.0616
Common activities: voluntary work	0.0331	-0.0351
Common activities: free time	-0.0553	-0.0352
Observations	187	157

Note: observations are dyads (i.e. pairs of co-players providing information about each other), stars indicate significance of correlation coefficient (***,…)

We believe that there is a lot of misreporting or measurement error in data that group members provide about their relation. First, it seems implausible that there is still such a high proportion of close family members in groups formed at random (29% vs. 53% in self-selected groups, compare table 4). Also, there are inconsistencies when comparing the information that co-players provide about each other. The above table A2 shows the correlation between answers from two sides by type of group formation. These correlations are far from one, meaning that there are many inconsistencies. While the correlation is highest for the variable describing the degree of relationship, it is indistinguishable from zero for almost all dummies indicating common activities (voluntary work, free time and working in the case of random groups). In general it seems that correlations are sometimes higher for self-selected groups (compare values for relation and working together), indicating more consistent answers for groups with closer links.

Even though misreporting and measurement error can not be ruled out, the information provided by participants is far from being useless. The many significant differences between self-selected and random groups (Table 4) show that there are indeed large differences between both types of groups and also reveals different link characteristics. Furthermore, the highly significant correlations between both sides of a dyad – especially regarding degree of relationship, frequency of contact and duration of knowing each other – shows that reality is often a substantial part of the answer.

Table A2: Regression results of Table 11 for covariates

	(1)	(2)	(3)	(4)
Own Money	-0.011	-0.011	-0.0065	-0.0047
Difference to recipient	**0.17*** **	**0.17*** **	**0.16*** **	**0.17*** **
Saving sender	**-0.12*** **	**-0.12*** **	**-0.12*** **	**-0.15*** **
Saving recipient	0.15	0.15	**0.16***	0.17**
Random group	**-7.20** **	**-7.96*** **	**-8.13*** **	**-6.28***
Village official (political)	**9.02** **	**14.4*** **	**14.3*** **	**10.2*** **
Other village job	-1.68	-3.60	-2.92	1.80
Cooperative / association	6.05	2.19	0.90	1.93
Women organization	-1.58	-2.81	-4.62	2.30
Lending group	-5.06	-5.68	-8.29*	-4.42
Other organization	0.32	-2.47	-3.12	-2.09
Random x village official		**-15.0*** **	**-17.2*** **	**-18.1*** **
Random x other village job		8.06	6.73	5.06
Random x coop / assoc		**21.9*** **	**24.2*** **	**20.0** **
Random x women organization		2.84	3.20	0.035
Random x lending group		0.54	1.67	1.79
Random x other organization		5.52	6.61	4.94
Treat B			4.01	2.15
Treat C			1.31	-1.85
Treat B x Treat C			-7.04	-0.97
PseudoTreat B			7.23	6.67
PseudoTreat C			6.13	3.31
Round			-0.098	-0.93
People living in the community				**-0.0041** **
HHs with family members abroad (%)				-0.14
People are selfish				0.033
Income Class				-0.73
Partially urban				3.12
Male				**12.1** **
At least high school				-3.12
HH head				-2.02
Regular income?				0.15
Debt > 1000 Pesos?				3.16
Constant	-1.37	-1.04	-3.04	3.18
Observations	968	968	968	968

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

Treat B is a dummy variable that indicates availability of insurance type B, It is 1 for villages in treatment block AB in round 1 as well as round 3, and in block A in round 3. PseudoTreat B is a dummy variable that is 1 for villages in treatment block AB in round 2, i.e. where insurance had been available in the previous round but not in the current round.

Treat C and PseudoTreat C are defined analogously for villages in treatment block AC.

The interaction term (Treat B x Treat C) is a dummy variable that is 1 for villages in treatment block A in round 3 only, where both insurance types where offered at the same time.

The regressor *Round* takes the values 1, 2 and 3.

The regressor *Random group* is a dummy variable that is 1 if the groups of three players had been formed at random.

Appendix II: Proofs of corollaries

From the definition in equation (1) we know that the average transfer given collective action status and group formation type is

$$E[T_{ij} \mid act_i, rand_i] = E[f(coop_{ij}) \mid act_i, rand_i] + E[\varepsilon_{ij} \mid act_i, rand_i]$$

Let us normalize $E[\varepsilon_{ij} \mid act_i, rand_i]$ to zero and realize that $coop_{ij}$ is a dummy:

$$E[T_{ij} \mid act_i, rand_i] = f(0) + (f(1) - f(0)) \bullet E[coop_{ij} \mid act_i, rand_i] \qquad \text{(A1)}$$

Thus, $E[T_{ij} \mid act_i, rand_i]$ is a positively monotone transformation of $E[coop_{ij} \mid act_i, rand_i]$. Let us take a closer look at the expected cooperation level. Cooperation takes place if and only if one of the following three cases is fulfilled:

- The sender is an unconditional cooperator
- The sender is a conditional cooperator and has generalized trust
- The sender is a conditional cooperator without generalized trust, but has personally trusts the recipient

Thus:

$$\begin{aligned}
&E[coop_{ij} \mid act_i, rand_i] \\
&= P[uc_i = 1 \mid act_i, rand_i] \\
&\quad + P[cc_i = 1 \cap gt_i = 1 \mid act_i, rand_i] \\
&\quad + P[cc_i = 1 \cap gt_i = 0 \mid act_i, rand_i] \bullet P[pt_{ij} = 1 \mid cc_i = 1, gt_i = 0, act_i, rand_i]
\end{aligned}$$

Given assumption (2) (group formation independent of types) we have

$$\begin{aligned}
&= P[uc_i = 1 \mid act_i] \\
&\quad + P[cc_i = 1 \cap gt_i = 1 \mid act_i] \\
&\quad + P[cc_i = 1 \cap gt_i = 0 \mid act_i] \bullet P[pt_{ij} = 1 \mid cc_i = 1, gt_i = 0, act_i, rand_i]
\end{aligned}$$

Thus the mean difference between self-selected and random groups with a certain collective action status is

$$\begin{aligned}
&E[coop_{ij} \mid act_i, rand_i = 0] - E[coop_{ij} \mid act_i, rand_i = 1] \\
&= P[cc_i = 1 \cap gt_i = 0 \mid act_i] \\
&\quad \bullet (P[pt_{ij} = 1 \mid gt_i = 0, cc_i = 1, act_i, rand_i = 0] - P[pt_{ij} = 1 \mid gt_i = 0, cc_i = 1, act_i, rand_i = 1])
\end{aligned}$$

By assumption (1) (constant, positive effect of group formation on personalized trust) this is

$$= P[cc_i = 1 \cap gt_i = 0 \mid act_i] \bullet \Lambda_+ \qquad \text{(A2)}$$

Proof of Corollary 1 (channel I):

(Effect predominantly through conditional cooperation)

With: $P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] > P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0]$

To show:

$$E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 0]$$
$$> E[T_{ij} \mid act_i = 1, rand_i = 1] - E[T_{ij} \mid act_i = 0, rand_i = 1]$$

or equivalently

$$E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 1, rand_i = 1]$$
$$> E[T_{ij} \mid act_i = 0, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 1]$$

or equivalently by plugging in equation (A1)

(*)
$$E[coop_{ij} \mid act_i = 1, rand_i = 0] - E[coop_{ij} \mid act_i = 1, rand_i = 1]$$
$$> E[coop_{ij} \mid act_i = 0, rand_i = 0] - E[coop_{ij} \mid act_i = 0, rand_i = 1]$$

From equation (A2) we have that

$$E[coop_{ij} \mid act_i, rand_i = 0] - E[coop_{ij} \mid act_i, rand_i = 1]$$
$$= P[cc_i = 1 \cap gt_i = 0 \mid act_i] \bullet \Lambda_+$$

it follows that (*) is equivalent to

$$P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] \bullet \Lambda_+ > P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0] \bullet \Lambda_+$$

By the assumption that collective action is predominantly associated with higher conditional cooperation the above statement holds.

Proof of Corollary 2 (channel II):

(Effect predominantly through generalized trust)

With: $P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] < P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0]$

To show:

$$E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 0]$$
$$< E[T_{ij} \mid act_i = 1, rand_i = 1] - E[T_{ij} \mid act_i = 0, rand_i = 1]$$

The proof can be obtained by simply repeating the steps above with reverse ‘unequal signs’.

Proof for extended model:

Let us again normalize the mean of ε_i to zero and realize that $coop_{ij}$ is a dummy:

$$\begin{aligned} &E[T_{ij} \mid act_i, rand_i, x_{ij}] = E[f(coop_{ij}, x_{ij}) \mid act_i, rand_i, x_{ij}] \\ &= f(0, x_{ij}) + (f(1, x_{ij}) - f(0, x_{ij}))\bullet E[coop_{ij} \mid act_i, rand_i, x_{ij}] \end{aligned} \qquad \text{(A2)}$$

Then the expected difference between self-selected and random groups given collective action status is

$$\begin{aligned} &E[T_{ij} \mid act_i, rand_i = 0] - E[T_{ij} \mid act_i, rand_i = 1] \\ &= E[E[T_{ij} \mid act_i, rand_i = 0, x_{ij}] \mid act_i, rand_i = 0] - E[E[T_{ij} \mid act_i, rand_i = 1, x_{ij}] \mid act_i, rand_i = 1] \end{aligned}$$

As $F_{x_{ij}|act_i, rand_i} = F_{x_{ij}|act_i}$ by assumption (2*) ($rand_i$ independent of act_i, x_{ij})

$$\begin{aligned} &= \int E[T_{ij} \mid act_i, rand_i = 0, x_{ij}] dF_{x_{ij}|act_i} - \int E[T_{ij} \mid act_i, rand_i = 1, x_{ij}] dF_{x_{ij}|act_i} \\ &= \int \underbrace{(E[T_{ij} \mid act_i, rand_i = 0, x_{ij}] - E[T_{ij} \mid act_i, rand_i = 1, x_{ij}])}_{g(act_i, x_{ij})} dF_{x_{ij}|act_i} \end{aligned}$$

Now plug equation (A2) into $g(act_i, x_{ij})$:

$$\begin{aligned} &g(act_i, x_{ij}) \\ &= f(0, x_{ij}) + (f(1, x_{ij}) - f(0, x_{ij}))\bullet E[coop_{ij} \mid act_i, rand_i = 0, x_{ij}] \\ &- f(0, x_{ij}) - (f(1, x_{ij}) - f(0, x_{ij}))\bullet E[coop_{ij} \mid act_i, rand_i = 1, x_{ij}] \\ &= (f(1, x_{ij}) - f(0, x_{ij}))\bullet \underbrace{(E[coop_{ij} \mid act_i, rand_i = 0_i, x_{ij}] - E[coop_{ij} \mid act_i, rand_i = 1, x_{ij}])}_{h(act_i, x_{ij})} \end{aligned}$$

It follows

$$\begin{aligned} &E[T_{ij} \mid act_i, rand_i = 0] - E[T_{ij} \mid act_i, rand_i = 1] \\ &= E[(f(1, x_{ij}) - f(0, x_{ij}))\bullet(h(act_i, x_{ij}) \mid act_i] \\ &= E[f(1, x_{ij}) - f(0, x_{ij}) \mid act_i]\bullet E[h(act_i, x_{ij}) \mid act_i] + Cov[f(1, x_{ij}) - f(0, x_{ij}), h(act_i, x_{ij}) \mid act_i] \end{aligned}$$

We can now plugging this into a rearranged statement (2) from the model chapter and simplify using assumption (3) $Cov[f(1, x_{ij}) - f(0, x_{ij}), h(act_i, x_{ij}) \mid act_i]$ independent of act_i and using assumption (4) ($E[f(1, x_{ij}) - f(0, x_{ij}) \mid act_i] = E[f(1, x_{ij}) - f(0, x_{ij})]$), which yields:

$$\begin{aligned} &E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 0] \\ &\quad < (>) E[T_{ij} \mid act_i = 1, rand_i = 1] - E[T_{ij} \mid act_i = 0, rand_i = 1] \end{aligned} \qquad \text{(2)}$$

$$\Leftrightarrow$$

$$E[T_{ij} \mid act_i = 1, rand_i = 0] - E[T_{ij} \mid act_i = 1, rand_i = 1]$$
$$< (>) E[T_{ij} \mid act_i = 0, rand_i = 0] - E[T_{ij} \mid act_i = 0, rand_i = 1]$$
$$\Leftrightarrow$$
$$E[h(act_i, x_{ij}) \mid act_i = 1] < (>) E[h(act_i, x_{ij}) \mid act_i = 0]$$

Plug in definition of $h(act_i, x_{ij})$

$$\Leftrightarrow E[coop_{ij} \mid act_i = 1, rand_i = 0] - E[coop_{ij} \mid act_i = 1, rand_i = 1]$$
$$< (>) E[coop_{ij} \mid act_i = 0, rand_i = 0] - E[coop_{ij} \mid act_i = 0, rand_i = 1] \quad (3)$$

On the way to corollary (1) and (2), the latter statement (3) was already shown to be equivalent to

$$P[cc_i = 1 \cap gt_i = 0 \mid act_i = 1] < (>) P[cc_i = 1 \cap gt_i = 0 \mid act_i = 0]$$

Appendix III: Test questionnaire

(Notes: Example for treatment block AB, round 1, with hiding. 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 ☐

3. Insurance in Solidarity Networks with Hidden Income: A Behavioral Experiment in the Philippines

I. Introduction

A large majority of the population in the world's poorest countries is without formal insurance.[40] Shocks such as natural catastrophes, illnesses, accidents, economic crises, unemployment or crime, droughts and floods, 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.

There is ample evidence for the importance of such mechanisms in developing countries (compare seminal papers of Townsend 1994, Udry 1994, Morduch 1999 or more recently Fafchamps 2008). Also on the Philippines, where our study is conducted, risk-sharing networks play a major role. Respondents may raise funds through gifts and loans (Fafchamps and Lund 2003), where loans are often zero-interest or do not have to be repaid fully (Fafchamps and Gubert 2007a). However, if other members of the network 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 and Lund 2003; Fafchamps and Gubert 2007b). There is ample evidence that informal insurance only smoothes a fraction of income shocks.[41]

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

[40] 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, and Liber 2007, pp.15-19). Similarly, Banerjee and Duflo (2007) report that less than 6% of the extremely poor are covered by any kind of health insurance. Numbers are currently rising around the globe, though, "…approaching 500 million risks covered" (Churchill and Matul 2012, p. 11).

[41] Morduch (1999) for example summarizes part of the literature and concludes: "Most informal insurance mechanisms are typically weak and often provide only inadequate protection to poor households" (Morduch 1999, p. 188).

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, health, and life insurance). Especially the recent rise of microcredit and -savings concepts has led governments, financial institutions, NGOs and mutual benefit associations to wonder whether they could 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 (Giné and Yang 2009; Cole et al. 2009; Ito and Kono 2010) and practitioners are working to implement an affordable insurance design that complements traditional informal risk sharing schemes. However, the possible effects of formal microinsurance on informal mechanisms are still an open question. To analyze possible reasons for a crowding-out of informal risk-sharing by formal insurance it is important to understand what drives informal transfers. Two motives can be broadly distinguished. One is an incentive-based and self-enforcing arrangement to engage in direct reciprocal risk-sharing. The value of complying with the arrangement is derived by the prospect of future interaction and thus the threat of punishment in case of non-compliance. The other motive is intrinsic solidarity or altruism. It does not require any future interaction and is based on pro-social preferences.[42] While both types of motives have been shown to play a role with altruism explaining the largest part of transfers in experiments (Leider et al. 2009; Ligon and Schechter 2012),[43] they imply different channels for the influence of formal insurance on informal transfers:

The effect of the availability of formal insurance products on incentive-based risk-sharing is straightforward. 'Externally' reducing risk leads to a lower value of the informal arrangement and therefore means a lower incentive to comply with it. Theoretical models (Coate and Ravallion 1993; Attanasio and Ríos-Rull 2000) show how such risk-sharing contracts can be self-enforcing and how reduced risk might crowd out the arrangement, possibly leaving the agents worse off overall.

Also regarding intrinsic motivation, there are good reasons to believe that formal insurance provides incentives that will crowd out informal insurance and reduce solidarity. 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

[42] Also intermediate forms such as indirect recioprocity (Engelmann and Fischbacher 2009) require future interaction, even though not necessarily between the same two individuals.

[43] Leider et al. (2009) estimate that the prospect of future interaction increases levels of pro-social giving by 24% in a behavioral experiment (leaving around 80% explained by altruism towards friends) and Ligon and Schechter (2012) find similar modest increases of transfers in a dictator game when adding future interaction.

insurance schemes can similarly reduce pro-social behavior.[44] Instead of relying on the intrinsically motivated solidarity payments individuals may simply pay for formal insurance in order obtain 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 would provoke a negative response by reducing one's own pro-social giving (*information effect* for conditional co-operators). Different to incentive-based transfers, crowding out effects on pro-social motivation might persist even if insurance is removed again. Gneezy and Rustichini (2000) find such an unintended effect after introducing and removing a fine for late comers. A flawed formal microinsurance system could therefore have negative overall effects in the long run (e.g. dependent on the fraction of insured people, strength of network, etc.). Motivational crowding-out might also lead to individualization and the breaking apart of traditional structures which affects other spheres of life as well.

There is empirical evidence in line with crowding out of informal risk-sharing by formal insurance. Dercon and Krishnan (2003) find that consumption is more responsive to shocks if there is food aid in rural Ethiopia, Albarran and Attanasio (2003) show that public transfers displace private transfers in Mexico in a way compatible with limited commitment models of risk-sharing, and Jensen (2003) similarly finds pension transfers to reduce private transfers in South Africa. However, none of the papers explicitly assesses the effect of insurance products.[45] Other problems are the lack of exogenous variation (except Albarran and Attanasio 2003) and the difficulty to accurately measure actual informal transfers.[46] More generally, transfers in a multi-period setting can be based on both dynamic incentives and pro-social motivation. While the above literature largely assumes incentive-based transfers to explain crowding-out, pro-social behavior is neglected. Given the evidence on the importance of altruistic motivation (Leider et al. 2009; Ligon and Schechter 2012), we could imagine a large influence through crowding-out of pro-social motivation, yet empirical evidence on its magnitude is missing.

[44] This effect is different from the investment crowding-out (Spencer and Yohe 1970) where government spending crowds out private investment spending caused by an increase in interest rates.

[45] Jowett (2003) uses a reverse argument and correlates stronger informal financial networks and social capital to lower health insurance uptake.

[46] Dercon and Krishnan (2003) and Jowett (2003) infer transfers by comparing income/consumption patterns on the village level (with strong assumptions involved). Direct measurement might be similarly difficult. Comola and Fafchamps (2010) 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.

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 and we discuss the external validity of our results in Section V. Most importantly, we focus exclusively on solidarity as motivation to help others and deliberately exclude the possibility for intertemporal risk-sharing based on reciprocity – otherwise it would be hard to separate motivational crowding-out from changing dynamic incentives. We model risk in a behavioral game using lotteries that involve rolling a die. Every participant is provided with an initial endowment and depending on the result of the die she is allowed to keep all or only part of it. Informal risk-sharing is implemented in non-anonymous groups of three broadly following the design of Selten and Ockenfels (1998). After the lottery is played and outcomes are revealed, each group member can transfer some money to the other group members. Insurance is introduced via offering alternative lotteries that are less risky but require some ex-ante fixed payment. We test two variations of insurance: one protects only against catastrophic shocks, while the other is a more comprehensive type and also covers medium shocks.

Since acts of giving are not always fully voluntary but are "demanded" from the network members (Platteau 2000; Hoff and Sen 2006; Grimm et al. 2011; Comola and Fafchamps 2010), we implemented additionally the possibility of *income hiding* where players could pretend to have had a bigger income shock than really experienced, thereby hiding their money from the risk sharing group. In real life, the main reasons for hiding income is to prevent criminals from stealing and perhaps even more importantly the necessity to hide income from the demands of one's personal network. It is a well-known observation in developing countries that individuals with financial resources face strong demands from relatives and friends to share their fortune. Field studies describe the latter phenomenon, often in the context of saving schemes.[47] In this sense the treatment could be interpreted as a (formal) saving product that makes it harder to monitor the flow of resources for the network.

[47] In Africa, for example, especially women are willing to entrust their money with "Susu men" to withdraw it from their network (Besley 1995, p. 2150) or to put it in formal saving accounts with effectively negative interest rates (Dupas and Robinson 2009). There is also evidence that people prefer saving products that reduce their liquidity even without an increase in interest rates (Ashraf, Karlan, and Yin 2006). Anderson and Baland (2002) show how, in the Nairobi slums, income-earning women use ROSCAs to protect their savings even from their husbands.
To the best of our knowledge there are four empirical papers (three unpublished) on the effect of saving / hiding money on informal risk-sharing. Brune et al. (2011) show that a commitment saving product increases savings and decreases transfers to the social network. Flory (2011) analyzes a randomized field experiment on the effect of a mobile van offering saving 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. Kinnan (2011) on the other hand finds evidence in Thailand that hidden income reduces transfers within the network. Chandrasekhar, Kinnan, and Larreguy (2012) test the effect of saving across periods and limited commitment to share risk within a behavioral experiment in India.

It is not limited to this interpretation, though, and broadly relates to research on transfers under limited commitment (Kocherlakota 1996; Ligon 1998; Ligon, Thomas, and Worrall 2002) and motives for pro-social giving (Leider et al. 2009; Ligon and Schlechter 2010; Jakiela and Ozier 2011). Note that our "savings product" disables any inter-temporal elements. Hence, we isolate the income-hiding effect, whereas a true savings-product would affect transfers through several channels.[48]

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

Our setup consists of the following four main scenarios: With or without formal insurance and with or without the option to hide money. In our baseline scenario (i.e. without formal insurance and without the option to hide money) we find solidarity transfers of substantial magnitude. When the hiding-money-option becomes available but no formal insurance is offered, solidarity transfers decrease drastically. People almost always use the option to hide money. This is evidence for a strong role of extrinsic instead of intrinsic solidarity, as participants use the hiding device most of the time and strongly decrease their willingness to transfer if it is available.[49]

When, in addition to the option to hide money, also formal insurance is introduced, the observed transfers are similar to the scenario with only the hiding-money option. Once hiding-money is possible, the introduction of formal insurance does not lead to significant crowding-out effects on transfers, and formal insurance is then effective in significantly smoothing the loss distribution. The reason is that solidarity is already low when hiding is possible such that additional crowding-out is limited.

The situation is different in the scenarios in which hiding-money is not possible. In those scenarios the introduction of formal insurance leads to major changes in the willingness to transfer money. When participants cannot secretly hide money we had observed that solidarity is relatively high in the absence of insurance. However, the introduction of formal

[48] Real savings products play an important role for intertemporal risk smoothing and hence might affect informal transfers for very different reasons. For example the value of reciprocity-based mechanisms is reduced if savings provide alternative means to cope with shocks (Ligon, Thomas, and Worrall 2000). Chandrasekhar, Kinnan, and Larreguy (2010) conducted a behavioral field experiment in India to test the effect of saving, focusing on its inter-temporal role.

[49] Our notion of extrinsic motivation for solidarity is different from fear of punishment or explicit group pressure, as transfers are never revealed throughout the game. The only thing that changes when secretly hiding money is that the recipient in need does not know about the potential resources available to the sender. Hence, the expectation levels regarding transfers are decreased, something that might be internalized by the sender who uses the secret hiding device.

insurance led to 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. We also find that the introduction of formal insurance can have persistent effects and crowd out voluntary transfers even when insurance is removed.

The fact that the crowding-out effect can completely offset the protection offered by the insurance hinges on the incomplete take-up. Insurance is voluntary in our setup as in a typical market transaction of private insurance. Simulations confirm that large decreases in vulnerability would be achieved with full insurance uptake. If everybody was insured (i.e. mandatory insurance) 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 a scenario without insurance. Hence, voluntary insurance can lead to a higher vulnerability compared to either no formal insurance at all or to mandatory insurance. This situation changes, though, when hiding money is possible.

The remainder of the paper is organized as follows. Section II describes the experimental setup including treatments, hypotheses, implementation and subject pool. We present the experimental results in Sections III and illustrate the relevance of regression results using simulations in Section IV. We discuss our results in Section V before concluding in Section VI.

II. Setup of the experiment

We model risk in a behavioral game using lotteries that involve rolling a dice.[50] 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.[51] This design reflects the risk to lose money instead of providing participants with the possibility of winning money.[52] Informal risk-sharing is implemented in groups of three according to the solidarity game procedure (Selten and Ockenfels 1998).[53] 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: Insurance and hiding money -

We test *two variants of insurance*, compared to no-insurance. One type of 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 shows a 1, 2 or 3, i.e. which we label as “no shock” (no loss). If the dice shows a 4 or 5 a

[50] We benefited from the work of Barr and 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 and Radermacher (2009), for example, compare solidarity in treatments with and without gamble choice. However, 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.

[51] 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.

[52] Harrison and Rutström (2008) stress the importance of the reference point, referring to prospect theory (Kahneman and Tversky 1979) that allows subjective probability weighting, a reference point and different utility functions for losses and gains. We therefore consider it important to play in the loss domain.

[53] 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 earnings. 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.

medium shock occurs (losing half), and a 6 implies a catastrophic shock (losing almost everything). More precisely, 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 keep all their 200 Pesos.

The availability of two insurance products is varied across treatments as will be explained in detail below The two insurance variants are called option B and option C.[54] 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 that more comprehensive schemes entail higher administrative costs. 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 (Option A) and the two insurance options B and C.

The advance fee 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 in terms of the expected loss. Yet the risk, as represented by the standard deviation of the payoff, is smaller than for options 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.[55] 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. (If people had a constant relative risk aversion utility function $u(c) = (c^{1-\rho})/(1-\rho)$ with risk parameter ρ, where $\rho > 0$ for risk-averse individuals, in the absence of any solidarity transfers, all people with a risk-aversion parameter $\rho > 0.34$ would buy insurance C if available, whereas only people with $\rho > 0.65$ would buy insurance B.)

[54] 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.

[55] E.g. in most developing countries, health insurance covers only the medical expenses (often below 100%), but not lost income due to lost labor, i.e. working time. The more comprehensive insurance could be like the state owned medical insurance scheme and the catastrophic insurance could be e.g. rainfall or crop insurance, which only pay out when huge losses occur.

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 normal life, observing how much money is given to you is unproblematic, whereas perfectly observing individual shock levels of others is usually not possible. Thus in some scenarios (labeled *secret hiding money*) we did allow participants to pretend a medium shock. Catastrophic losses might on the other hand be observable to everybody. Therefore, observability of medium shocks was reduced in the *secret hiding* treatment. (In order to not influence participants we did not call this 'hiding', but rather explained the possibility to "put money in a lockbox" and the related mechanics.) If the dice result was 1, 2 or 3 (i.e. no shock) individuals could decide to hide the monetary difference to a medium shock in a secret lockbox. This information was private to the individual and group members were only informed about the amount the person had after the lottery/lockbox stage. Permitting hiding-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, on the one hand, to increase external validity of our study by allowing uncertainty about neediness and, on the other hand, to show the effect of secret hiding on solidarity. The latter also embraces potential side effects of new (formal) saving products, e.g. mobile banking. It is still possible for people with no shock to help others in case of need, but a lot of solidarity based on internalized peer pressure may be reduced.[56]

To test the effects of the two insurance types and of the secret hiding device, separately and in combination, the behavioral experiments were implemented as outlined in Table 2. In six villages (treatment block A) no insurance is offered in rounds 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

[56] Note that individual transfers are never revealed, only the final payoff from one random round. Consequently, there should be no fear of punishment or explicit group pressure. The only thing that changes when secretly hiding money is that the recipient in need does not know about the potential resources available to the sender. Hence, the expectation levels regarding transfers are decreased, something that might be internalized by the sender who uses the secret hiding device.

(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 hiding possibility, the secret hiding device was implemented in the first two rounds in half of all villages.[57]

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 and only then the final payoff is revealed, without information about individual transfers. 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 hide, 3 no hide	4 hide, 4 no hide	4 hide, 4 no hide
Round 2	Option A	Option A	Option A
	3 hide, 3 no hide	4 hide, 4 no hide	4 hide, 4 no hide
Round 3	Choice: Option A, B or C	Choice: Option A or B	Choice: Option A or C
	3 hide, 3 no hide	8 no hide	8 no hide

Note: In each block, in half of the villages the games were played with the "secret-hiding device" and in the other half without. This is indicated by "3 hide, 3 no hide" or "4 hide, 4 no hide". The notion "8 no hide" means that in Round 3 the secret-hiding 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 hiding

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 persistence effect of access to insurance on solidarity can be tested in the second round (Hypothesis II), comparing the same treatment blocks as in the first round. The

[57] 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 statistically significant evidence.

effect of secret hiding (Hypothesis III) can be assessed by comparing the 11 villages, where secret hiding was made possible, to the other half.

In addition to these three hypotheses the experiment permits the analysis of many more aspects. For example, the third round of Treatment A allows for a comparison of demand for the insurance variants when both options are available simultaneously. This is the only scenario where participants can choose between two insurance options. In the other scenarios only insurance versus no-insurance is available. 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 limited ability to observe shocks (secret hiding).[58] Furthermore, the third round also delivers more observations for the pooled regressions that jointly analyze all treatments at a later stage.

As an additional treatment, different *network strengths* of the player groups are examined. The groups of players were formed in two ways: As a standard procedure a randomly selected person had to invite two other household heads that he knows very well to join the experiment. In half of the villages these players would form a group. We will refer to them as endogenous groups or as *strong* networks. In the other half of the villages, however, we mixed up groups, and participants would play with two random partners from their village.[59] These groups will be called exogenous or random or "*weaker* networks". 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 sub-cell (e.g. in Block A there are only 3 villages with hiding and 3 without hiding) and because there might be interesting interactions between network strength and the main treatments.

[58] In treatment blocks AB and AC we varied the ability to observe shocks (labeled as "secret hiding") in some of the villages to test more hypotheses, e.g. persistence of the secret hiding device effect.

[59] The 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.

- Testing strategy -

To test the hypotheses we measure solidarity via net transfers. Redistribution between two group members is achieved by the difference between the transfer from i to j (T_{ij}) and from j to i (T_{ji}). So T_{ij} alone is not a good measure of solidarity because the intended redistribution, which is the expectation of i regarding $T_{ij} - T_{ji}$ or in short $E_i[T_{ij} - T_{ji}]$, depends on $E_i[T_{ji}]$ which might not be constant across treatments. As we do not observe $E_i[T_{ji}]$ we use T_{ji} as a proxy for $E_i[T_{ji}]$ and thus use real redistribution $T_{ij} - T_{ji}$ as our measure of solidarity to test hypotheses (I)-(III). Results are presented in Tables 4 – 6.

When we use parametric regressions we are still interested in net transfers as a measure of solidarity, but we also want to be able to control for the payoff distribution and other contextual factors. We will do this in two steps: First, we analyze determinants of individual gross transfers, in particular how transfers from i to j depend on incomes Y_i and Y_j. In a second step, we examine the implications for net transfers. So instead of modeling $T_{ij} - T_{ji} = f(Y_i, Y_j)$ we separately model $T_{ij} = g(Y_i, Y_j)$ and $T_{ji} = g(Y_j, Y_i)$ as censored outcomes in a Tobit model. Later, we can easily calculate $T_{ij} - T_{ji} = g(Y_i, Y_j) - g(Y_j, Y_i)$ for different treatments, holding the distribution of payoffs constant. The main reason for examining gross transfers first before calculating net transfers is that in the scenario with hidden income, the information set is asymmetric: Player i knows whether he has hidden income, but does not know whether j has hidden income. We want to know how this affects the willingness to give. Additionally, there are also technical reasons for the two step approach.[60]

Nonparametric analysis reveals that $g(Y_j, Y_i)$ is sensitive to $Y_j - Y_i$, see Figure 4. The more sensitive individuals are to this difference, the more redistribution we will observe, holding the distribution (Y_i, Y_j) constant. Changes in transfers (hypotheses (I) – (III)) can occur first by changing pre-transfer payoff differences $Y_j - Y_i$, e.g. through buying insurance, and second by a change in sensitivity to this difference. A change in the coefficient that measures how much transfers depend on $Y_j - Y_i$ therefore reveals motivational crowding-out effects already in the first regression step. Specifically, the hypothesis that insurance crowds out solidarity motivation translates into the hypothesis that the sensitivity to pre-transfer

[60] For instance, net transfers are a result of two censored variables (transfers cannot fall below zero) and an adequate econometric treatment would be more complicated.

differences is lower if insurance is available. Similarly, persistent motivational crowding out can be tested by assessing whether sensitivity is still lower after insurance is removed again. We provide more details in the ‘regression specification’ subsection and results in Table 7 and Table 8.

- Implementation -

All participants were assigned to groups of three and received player numbers upon arrival. The composition of groups was done in two ways, as explained above: 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. 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 labeled “weaker network”, where weaker network takes the value 0 in the former villages (i.e. where groups were self-selected) and the value 1 in the latter villages (i.e. where groups were formed at random).

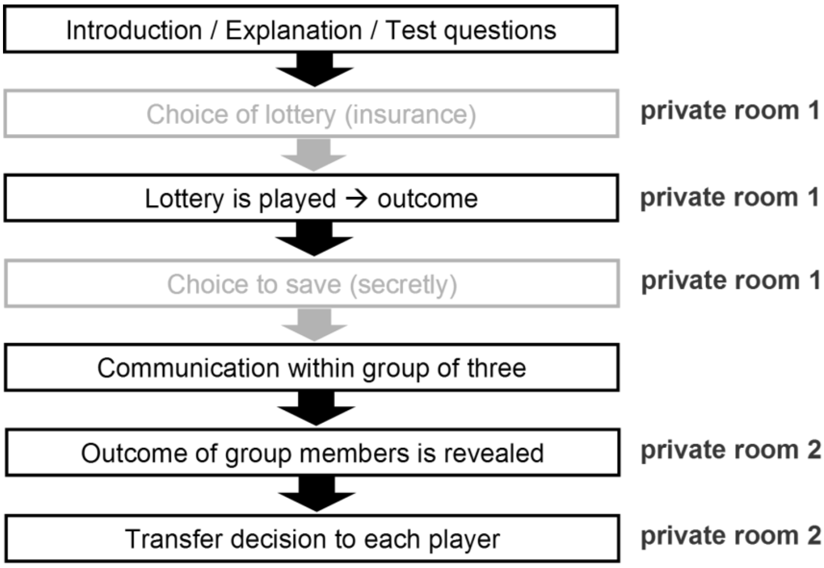
The groups stayed together for all three rounds, and people in a group could identify the other two members. After answering the pre-questionnaire, participants were seated to receive the introduction to the game. In an effort to make the rounds as independent from each other as possible, we made sure that signaling, punishment and the like cannot take place (i.e. an assistant was sitting in the room as an observer). Therefore decisions of co-players were never revealed and we did not allow for communication after the transfer choice. Group members were even seated separately to inhibit communication and no group was ever without supervision of at least one assistant. 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.

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 private room 1 to play the lottery, they answer a set of questions in order to test their understanding of the game.[61] If the current round permits insurance options (see Table 2), participants are given a choice of lotteries. Otherwise only

[61] 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 less than 1% of all participants.

the standard lottery (Option A) is available.[62] After the participants make their lottery choice and pay the related price, they roll a dice to determine the loss. Where secret hiding is available, players with no shock could then decide to hide a fixed amount of their money or not. After all have chosen whether to hide or not, 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 hidden income). Importantly, only the *net payout* is revealed, and not whether insurance has been bought, or whether shocks took place or whether resources have been hidden. From these payouts, however, one can induce who has purchased insurance and who did not. The participant then decides about transfers, i.e. 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: 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



[62] 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.

To ensure that experimental conditions did not change, the same team of ten 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.[63] A two-stage random sampling procedure was applied throughout. First, we randomly determined the experimental sites, and then we drew 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.[64] Also very small (population below 500) and very big (population higher than 3000) barangays were not considered to make the sample more homogenous.[65] 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

[63] The Demographic and Health Survey 2008 (http://www.measuredhs.com/data/dataset/Philippines_Standard-DHS_2008.cfm) and a household survey conducted by the University of Mannheim in 2009 suggest the following: educational attainment is slightly below national average, poverty is higher and coverage with public health insurance is around average.

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

[65] 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).

efforts 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.[66] 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 head – 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. 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.

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 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.[67]

[66] Every barangay was able to provide a complete household list.
[67] Around half of them owe the money to friends or relatives.

Table 3: Descriptive statistics of participants

	All (N=466)				Block A (N=132)	Block AB (N=167)	Block 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: high school	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 Household 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 block A. (See Table 2 for definition of the treatment blocks.) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Due to the randomized assignment to treatments, in expectation all characteristics are balanced, nevertheless in small samples some correlation remains. 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.

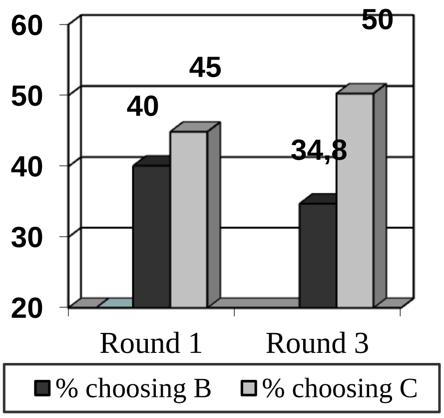
III. Empirical Analysis

In the following, we will first consider some descriptive results on the effect of insurance and secret hiding using the comparisons implied by the treatment plan (see Table 2). Afterwards we will employ a parametric regression model to control for possibly imbalanced covariates[68] and to gain further insights. Using the regression results we simulate loss/payout distributions under different insurance and hiding 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 imitating others' choices or by learning from experienced shocks. It appears that while buyers of insurance C were 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: Demand for insurance on separate markets



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

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

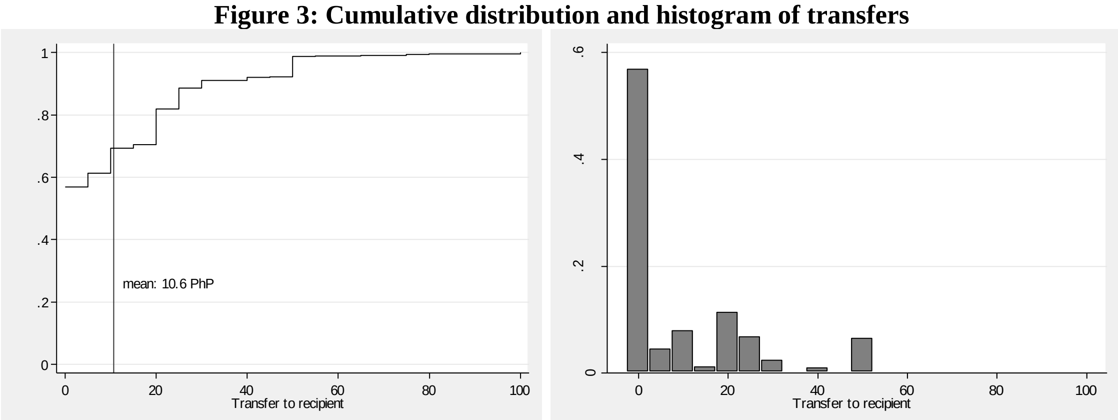
Figure 2 only shows take-up for treatment blocks AB and AC, where either only option B or only option C are offered. In addition we note that in treatment block A in round three, both insurance options B and C are available at the same time. There 43.4% chose option C and 17.8% chose option B (while the remaining 40% chose the no-insurance option A).

The 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, i.e. the insured tend to be more indebted and had to skip meals more often. This may suggest that low-income participants have more need for money from the experiment and behave more risk averse.

Before examining transfers, we note that "secret hiding" was used overwhelmingly: Whenever possible, secret hiding was used in 94% of the cases. This clear pattern to hide money when possible leads us to believe that people's behavior in real life is similar.[69]

In Figure 3 we display transfers, pooling observations from all scenarios. Transfers between group members vary greatly between 0 and 100, with a mean of 10.6 pesos and a standard deviation of 16.5 pesos. Figure 3 displays the distribution of the 2730 observed payments from sender to recipient.[70] In 57% of all cases transfers are zero.[71]

Figure 3: Cumulative distribution and histogram of transfers



The transfers are described in a compact form in figure 3; however they do not necessarily indicate effective re-distribution of the money because every group member can transfer to

[69] Remember that participants can only hide and thus pretend a medium shock if they have no shock.
[70] 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. Note that the distribution function in Figure 3 is non-smooth since transfers were only possible as multiples of five, i.e. 0, 5, 10, 15… We used play money where the smallest note was 5 Peso.
[71] 43% of transfers are zero if the sender is better off than the recipient.

the other *and vice versa*. Let T_{ij} be the transfers from player i to j. Real redistribution is the result of *net* transfers $T_{ij} - T_{ji}$, that is transfers from player i to j minus transfers from j to i. 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.[72] 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. 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 also shown relative to block A. Similarly, net transfers in the secret hiding treatment are shown relative to the no hiding 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.[73]

Table 4: Net transfers to disadvantaged co-players

		All	BlockA	BlockAB	BlockAC	Block AB vs. A	Block AC vs. A	No hiding	Hiding	Hiding vs. no hiding
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Round	Variable	Mean (Std)	Mean	Mean	Mean	Difference	Difference	Mean	Mean	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 from treatment A
*** p<0.01, ** p<0.05, * p<0.1

72 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.

73 The third round is *not* shown in Table 4 as it is less comparable: First, there is no control group because both insurance types are available in the third round of treatment block A. Second, the comparisons are not balanced, as observability of shocks (secret hiding device) was changed in the third round of treatment blocks AB and AC (see treatment plan in Figure 2). Hence, simple descriptive statistics would not be meaningfully comparable for round 3. (Round 3 will be used in regression analysis later.)

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 in Round 2, unless the availability of insurance in Round 1 had a persistent effect. Finally, we can also estimate the effect of the secret hiding device by comparing hiding with the no-hiding scenario in both rounds (Hypothesis III).
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, although transfers have large standard deviations. In columns (2) to (6) we examine how transfer behavior changes when insurance is available. Columns (2) to (3) give the averages in each treatment block, and columns (4) and (5) show the differences to Block A, augmented by a Wilcoxon ranksum test. We see that the availability of option C decreases transfers by 5.2 in round 1 and also in round 2. (These results are rather noisy, and will be examined further below.) For insurance option B the results are more noisy.

In columns (7) to (9) we examine the effects of the possibility of secret hiding. The option of secret hiding has a very strong and significant *negative* effect on solidarity in both rounds: Net transfers drop by around two thirds in round 1 and around half in round 2. We also had noted before that almost all people (94%) make use of the secret hiding option when possible. This rather strong finding indicates that if people know that they themselves as well as others can hide some resources, they drastically reduce solidarity transfers.

In the following, we examine the interaction effects between secret hidings and insurance availability. In Table 5 we compare across treatment blocks, using only those sessions where secret hiding was *not* possible. We thereby estimate the effects of insurance availability when 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 consider the *framing effect* to be more persistent for insurance type C, as it is much cheaper and thus might be considered affordable to all. (We use the word framing effect in the sense of Bowles (2008): People could perceive the availability of insurance as a signal that 'buying' security is everyone's own responsibility. Thus, availability of insurance signals that participants are no longer responsible for smoothing shocks.) A more detailed discussion on this is given in Section V.

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

		No hiding 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	12.2 (21.2)	18.4	10.4	9.1	**-8.0****	**-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 in distribution to treatment A
*** p<0.01, ** p<0.05, * p<0.1

Table 6 shows the results when using only villages where *secret hiding* was possible. Here we do not find any statistically significant differences. Note that the difference between treatment blocks A and AB in the first round – 9.0 albeit not significant – might well be explained by the lower amount that can be secretly hidden with insurance B.[74] 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 hiding device (4.7 versus 12.2 PhP in round one, 10.4 versus 19.9 PhP in round two).

[74] Remember that to guarantee non-observability participants can always hide the difference to the medium shock. Hence, participants can effectively hide less (50 instead of 100 PhP) if they are insured against half of the medium loss (with insurance type B) which might lead to less adverse effects of hiding than in Treatment A. This fact is an additional reason to control later for the hidden amount in regressions.

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

		Hiding 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 in distribution to treatment A
*** p<0.01, ** p<0.05, * p<0.1

When interpreting these 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.[75] We pursue two approaches to deal with such small sample imbalances: In the Appendix (Table A2) we examine nonparametric matching estimates, where transfers are only compared for identical shock situations. These nonparametric matching estimates confirm our previous findings. Second, we will use parametric regression models in the following, which control for the shock distribution and also other covariates.

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 solely relying on the descriptive comparisons, 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. In order to learn more about the possible explanatory channels and given our small samples sizes, we use parametric regression models. Using a regression model and controlling for differences in

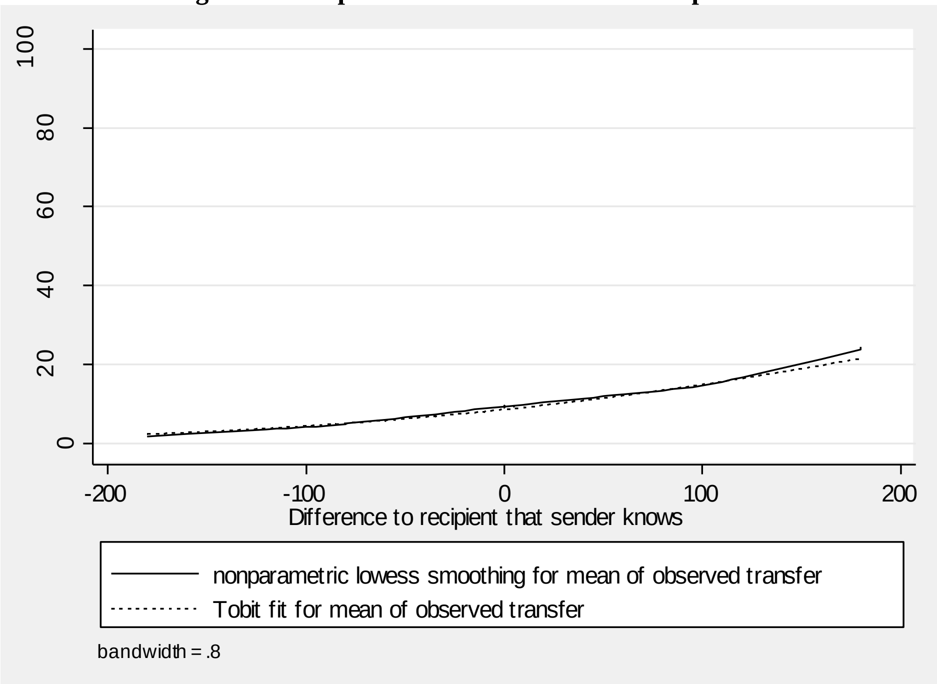
[75] Comparing the shock dispersion across treatments and rounds reveals differences in the shock dispersion that are significant at the 10% level in some cases. As this is a result of dice rolls, it is by definition pure chance and large differences should never be present in large samples. However, in our case this is a small-sample correlation that might nevertheless bias results.

play money, we can disentangle the effects of insurance via reduced inequality and the additional crowding-out effects of insurance.

By including covariates, we can also control for small-sample imbalances in shock distributions, implied by the dice rolling, and for small-sample imbalances in individual characteristics. Furthermore, we are able to eliminate some statistical noise and 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.[76] We use Tobit regressions where the *latent willingness to give* is permitted to be proportional[77] to the *observable* pre-transfer difference between giver and recipient. (i.e. the amount secretly hidden by the recipient is not observable to the giver.) For visual comparison, 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.

Figure 4: Comparison between Tobit and nonparametric fit



N = 2730

[76] 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 hiding (3.0% vs. 2.2%, p-value = 0.17).

[77] We also tried non-linear terms, but these were insignificant.

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 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.[78]

We consider the following regression model of individual transfers:

$$T_{ij}^{*} = \begin{pmatrix} Y_i - (Y_j - S_j) \\ Y_i \\ S_i \\ S_j \\ X_i \\ X_j \end{pmatrix}^{T} \beta + \varepsilon_{c,r} + \varepsilon_{ij}$$

where T_{ij}^{*} is the latent transfer from *i* to *j* and

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

$$T_{ij} = 0 \qquad \text{if} \qquad T_{ij}^{*} < 0$$

Latent transfers are possibly influenced by the difference in incomes between sender and recipient ($Y_i - Y_j$). In the scenario with secret hiding the sender can only observe ($Y_i - (Y_j - S_j)$), i.e. the recipient's income after hiding. We also allow an effect of own income after the lottery (Y_i). Additionally including the own hidden income (S_i) is a direct test whether solidarity transfers are voluntary or obliged. If giving was truly intrinsically motivated, own secret hiding should not have any effect because it changes the information available to the recipient – not to the sender. On the other hand, if transfers are driven by perceived group pressure, only the observable difference ($(Y_i - S_i) - (Y_j - S_j)$) should matter, i.e. if I can hide some money I will also transfer less since I "feel" less group pressure.
Further, including hidden income of the recipient (S_j) as a regressor is a test whether hiding of the recipient is really unobserved. (S_j is unobservable to *i* by design, but during the communication stage person *j* could have revealed some signals about his true shock or his hidden income S_j.) If S_j is completely unobserved, only the observable difference should matter and the coefficient on S_j should be zero.

[78] For our analysis, we examine transfers from i to j instead of *net transfers*. 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. Furthermore, inference with net transfers might become even more complicated once we incorporate dependence between the error terms of individual transfers.

Individual covariates of sender/recipient (X_i, X_j), community-round fixed effects ($\varepsilon_{c,r}$) and an individual error term (ε_{ij}) are also allowed to affect T_{ij}^*. Note that all level effects that do not vary within a village-round cell (such as treatment, round and community effects) are included in the fixed effect $\varepsilon_{c,r}$. These level effects are common to both the sender and the receiver and are thus not of immediate interest.

Of particular interest to us is the coefficient β_1, which indicates by how much voluntary transfers increase if income differences increase. A large β_1 implies more solidarity of the better-off with the worse-off, and transfers will be more sensitive to inequality. We are particularly interested in how solidarity changes with the availability of formal insurance. To this end we will also incorporate interaction terms between $Y_i - (Y_j - S_j)$ and different treatment blocks and rounds.

- Regression results -

Table 7 shows the results of the Tobit regressions. Specification (1) is the baseline specification, regression (2) includes village-round controls and specification (3) adds individual covariates of sender and recipient. The latent willingness to give rises by an additional 0.13 pesos with $Y_i - (Y_j - S_j)$. The coefficient on S_i is -0.11, i.e. nearly of the same magnitude. In fact, the difference is not statistically significant. Hence, if money is secretly hidden by the sender, this almost one-to-one reduces the inclination to give – indicating that most of the solidarity is in fact not intrinsically motivated. Hidden income of the recipient on the other hand does not matter much and is either insignificant or significant only at the 10% level. Remember that S_j is unobserved to *i*, but some signals about intended behavior could have been given during the communication stage. The small coefficient on S_j and the weaker significance level (as compared to the main effect) suggest that hiding is largely unobserved because the sender reacts more to the observable difference rather than to the real difference. It thus seems that mainly observable differences drive redistribution and money is hidden in order to avoid expectations of peers.

Table 7: Tobit regressions explaining transfer

	(1)	(2)	(3)	(4)	(5)	(6)
	All observations			All observations	Only observations without secret hiding option	Only observations with secret hiding option
Y_i	0.047	**0.048***	**0.046***	**0.048***	0.036	**0.083***
Observable Difference ($Y_i-(Y_j-S_j)$)	**0.13*****	**0.13*****	**0.13*****	**0.12*****	**0.15*****	0.082
Hidden income of sender (S_i)	**-0.11*****	**-0.13*****	**-0.14*****	**-0.14*****		**-0.15*****
Hidden income of recipient (S_j)	-0.018	**-0.033***	**-0.034***	**-0.034***		**-0.026***
($Y_i-(Y_j-S_j)$) x Treat B				-0.020	**-0.095****	**0.052***
($Y_i-(Y_j-S_j)$) x Treat C				-0.021	**-0.089****	0.033
($Y_i-(Y_j-S_j)$) x Treat B x Treat C				0.060	**0.15***	-0.021
($Y_i-(Y_j-S_j)$) x PseudoTreat B				0.025	-0.032	**0.098****
($Y_i-(Y_j-S_j)$) x PseudoTreat C				-0.029	**-0.11****	0.035
($Y_i-(Y_j-S_j)$) x Round				0.015	**0.030****	-0.0058
($Y_i-(Y_j-S_j)$) x Weaker Network				-0.031	-0.0092	-0.011
Village-round controls	NO	YES	YES	YES	YES	YES
Individual controls	NO	NO	YES	YES	YES	YES
Observations	2730	2730	2730	2730	1664	1066

Standard errors clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1
The coefficients for the control variables are shown in the Appendix (table A3).
Treat B is a dummy variable that indicates availability of insurance type B, It is 1 for villages in treatment block AB in round 1 as well as round 3, and in block A in round 3. PseudoTreat B is a dummy variable that is 1 for villages in treatment block AB in round 2, i.e. where insurance had been available in the previous round but not in the current round.
Treat C and PseudoTreat C are defined analogously for villages in treatment block AC.
The interaction term (Treat B x Treat C) is a dummy variable that is 1 for villages in treatment block A in round 3 only, where both insurance types where offered at the same time.
The regressor *Round* takes the values 1, 2 and 3.
The regressor *Weaker network* is a dummy variable that is 1 if the groups of three players had been formed at random.
The individual control variables are: *Regular income, Skip meals last month, Debt larger than 1000 Pesos, Male, Household head, Male interacted with Household head, Married, High school, College, Age, Age squared*. Both the sender's and the recipient's characteristics are included.
Village-round controls are: *Treat B, Treat C, Treat B x Treat C, PseudoTreat B, PseudoTreat C, Round, Weaker Network, Income Class, partially urban, people are selfish*

Columns (1) to (3) do not include any interaction terms between the observable pre-transfer difference $Y_i-(Y_j-S_j)$, and differ only in the set of control variables. Column (4) adds various interaction terms. Columns (5) and (6) use the same specification as in (4) but split the sample according to the groups where secret hiding was possible or not. The interaction term ($Y_i-(Y_j-S_j)$) x *Treat B* reflects the different behavior in willingness to give once the insurance type B is available compared to the baseline scenario where no insurance exists. Similarly for the interaction term with *Treat C*, which reflects the availability of insurance type C. Remember that insurance B is available in treatment block AB in rounds 1 and 3 and in block A in round 3. Insurance *C* is available in treatment block AC in rounds 1 and 3, and in block A in round 3. In treatment block A in round 3, *both* insurance options are available at the same time. We capture this simultaneous availability of both options affects by the interaction term $(Y_i - (Y_j - S_j))$ x Treat B x Treat C, which is only non-zero in block A round 3.

To examine possible effects of "*making insurance once available and then withdrawing it*", we introduce the dummies (Pseudo*Treat B*) for treatment block AB in round 2 and (Pseudo*Treat C*) for treatment block AC in round 2. In both cases, insurance was available in round 1 but not in the current round. (Note that there is a comparison treatment block A in which insurance was available in none of the first two rounds. Hence, Pseudo*Treat* is not confounded with the interaction term $(Y_i - (Y_j - S_j))$ x *Round*, which measures general changes during the game.) Finally, *Weaker Network* is a dummy variable which takes the value one if the group was formed at random.

In column (4) we find that differences in pre-transfer earnings and hidden income of sender/recipient are again highly significant and similar in size to before. However, all interaction terms are insignificant. The interaction terms become significant though, once we split the sample according to the possibility of secret hiding, columns (5) and (6).

Without the secret hiding possibility (column 5), the availability of any insurance reduces the willingness-to-give by more than half. Also, the significantly negative interaction term with (*PseudoTreat C*) suggests a persistence of the effect even if insurance is removed in the second round. (The estimate for *PseudoTreat B* is smaller and too imprecisely estimated to permit a clear interpretation.) Further, we find participants becoming more sensitive to differences in *Y* in later rounds, as indicated by the positive coefficient for $(Y_i - (Y_j - S_j))$ *x Round*, when players become more accustomed with the game and their co-players.[79]

In column (6), in contrast, only observations are used where secret hiding was possible. Nearly all coefficients are insignificant. Only two interaction terms are significant at the 5% and 10% level. Yet, note that the sample size for column (6) is smaller than for column (5) such that the estimates are expected to be less significant. However, they are also

[79] Remember that no feedback (on transfers or whatsoever) was given during the game. Players could communicate with each other, which increases trust and personal feelings and, although it is cheap talk, allows signaling their honesty and willingness to transfer. All such signals are non-verifiable later, however.

less robust when repeating the same checks as for non-hiding village-rounds (see Table 8 below).[80] Hence, when secret hiding is possible, the availability of insurance does not seem to affect behavior in a systematic way.

In the following Table 8 we augment the previous regressions with more control variables in order to examine the robustness of the previous findings and also to learn more about variables that affect the willingness-to-give. Adding these covariates also controls for any small-sample imbalances in these regressors between the treatment blocks. First, we add more interactions between $Y_i-(Y_j-S_j)$ and individual/village covariates. We include all covariates that were at least marginally significantly unbalanced between treatment blocks. The results of Table 7 are fully confirmed and most additional control variables are insignificant. Interestingly, household heads have a substantially larger inclination to give transfers proportional to $Y_i-(Y_j-S_j)$ in the no-hiding subsample. This may be because of their general responsibility to balance needs within the household. Interestingly, this finding of (mostly male) household heads transferring more contrasts with existing characterizations of women as more inequity-averse.

In columns (3) to (5) we examine various subsets of those participants where secret hiding was not possible. In column (3) we exclude individuals with a lower level of understanding according to our test questions or because of particularly ‘irrational’ transfers.[81] In columns (4) and (5) we examine the effects in different rounds. Since the sample sizes were quite small for each round, we always combine two rounds, i.e. rounds one and two in column (4) and rounds two and three in column (5). The coefficients are mostly significant for the more general insurance type B. This is not the case for catastrophic-insurance type C. While effects always point in the expected direction, they are insignificant. However, this is likely because of the limited sample size in the robustness checks. (We assess whether the insignificant effect is solely due to limited sample size or whether there is a systematic difference between the effects of insurance type B and C in appendix III. We conclude that there are no systematic differences.) The effect for type C again appears to be persistent if availability is removed (*PseudoTreat C*). The related coefficients are significant at least at the 5% level across all specifications. We cannot identify persistence of the effect for insurance B.

[80] In particular, effects disappear when adding more controls (see table 8 column 1) or when excluding those with lower understanding and irrational transfers (see footnote 81).

[81] We define irrational transfers as transfers from individuals that lost more than some other group member but still transferred more than 40% of their money and received less from the others than what they gave.

Table 8: Tobit regressions explaining transfer – further analyses

	(1) Only observations with secret hiding option	(2) Only observations without secret hiding option	(3) Only observations without secret hiding option	(4)	(5)
			High understand	Round 1+2	Round 2+3
Y_i	**0.082***	0.036	0.035	0.028	**0.061****
Observable Difference ($Y_i-(Y_j-S_j)$)	0.0028	0.13	**0.19*****	**0.14****	**0.22*****
Hidden income of sender (S_i)	**-0.15*****				
Hidden income of recipient (S_j)	**-0.027***				
($Y_i-(Y_j-S_j)$) x Treat B	0.041	**-0.091****	**-0.12****	**-0.10****	-0.066
($Y_i-(Y_j-S_j)$) x Treat C	0.031	**-0.096*****	-0.083	-0.041	-0.095
($Y_i-(Y_j-S_j)$) x Treat B x Treat C	-0.014	**0.15****	0.12		0.14
($Y_i-(Y_j-S_j)$) x PseudoTreat B	0.086	-0.020	-0.049	-0.029	-0.049
($Y_i-(Y_j-S_j)$) x PseudoTreat C	0.029	**-0.11*****	**-0.12****	**-0.11***	**-0.12****
($Y_i-(Y_j-S_j)$) x Round	-0.0029	**0.027***	0.021	0.041	
($Y_i-(Y_j-S_j)$) x Weaker Network	-0.016	-0.0012	0.010	-0.026	-0.012
($Y_i-(Y_j-S_j)$) x Income Class	0.020	0.020			
($Y_i-(Y_j-S_j)$) x Selfish	-0.0043	-0.0055			
($Y_i-(Y_j-S_j)$) x Higher Education	0.062	**-0.035***			
($Y_i-(Y_j-S_j)$) x HH Head	-0.0045	**0.054***			
Village-round controls	YES	YES	YES	YES	YES
Individual controls	YES	YES	YES	YES	YES
Observations	1066	1664	1234	890	1216

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

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

Overall, the empirical results suggest that there is a negative effect of insurance on solidarity if there is no secret hiding possibility. Under these circumstances, the effect of the catastrophic-insurance is persistent even if insurance is removed. Additional robustness checks confirm these results. The more persistent effect of type C could be due to higher ‘acceptance’ amongst participants, given that the price of this catastrophic-insurance is lower and take-up is higher. At first glance this might hint at 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.[82] 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-insurance type C is not much higher in the first round, but rather in the third round (see figure 2). It should be the first round, however, that leads to persistence effects in the second round. Hence, the *framing effect* (i.e. a change in the norm of what one is supposed to do) appears to be the main explanation for crowding-out, i.e. insurance availability signals that everybody is responsible for his/her own security. We discuss these results in some detail in Section V.

[82] 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).

IV. Simulation of poverty with insurance and secret hiding possibility

In the regressions we found substantial hiding and insurance effects. Yet, the real importance of these effects is difficult to infer from the size of the coefficients alone. We have examined how individual behavior depends on observed regressors. We need to combine these behavioral results with the mechanical implications of insurance to obtain the total outcome distribution. Transfers as well as the insurance options both lead to a reduction in the variance of the outcomes but their combined results cannot be obtained from the previous tables alone.[83] In principle we could simply compare outcome distributions in different treatment combinations to obtain a meaningful comparison. In our small sample, however, such comparisons can be blurred by differences in dice results and other covariates. In addition, we are also interested in counterfactual simulations. Therefore we use our previous regression results to simulate the counterfactual situation in which everybody receives access to insurance treatment (A/AB/AC) in all rounds. This is done separately for the regression results with and without secret hiding, always using the whole sample as a basis for the counterfactual. To effectively illustrate average treatment effects for a large population and to average out noise from the random shocks generated by dice rolling, we expand the datasets by the factor 100.[84] We draw shocks according to the theoretical probabilities, assign insurance take-up and secret hiding (if applicable) according to the observed probabilities, and also draw error terms for the transfer decision from the estimated (normal) distribution. Given shocks, insurance uptake and secret hiding decisions we can then predict transfers using our regression results (including random error terms). As a result we have a complete distribution of payoffs after insurance and informal solidarity for different counterfactual situations. Details on the simulation procedure can be found in Appendix II.

[83] 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 in this section.

[84] With such a large population variations of the outcome distribution by chance are very small. Repeating the simulation procedure 201 times leaves us with standard deviations of always less than 0.00115 using different points of the cumulative distribution function.

Table 9: Poverty rates for different poverty lines under each hiding/insurance regime (simulation)

Poverty line at:	Without secret hiding option			With secret hiding option		
	(1)	(2)	(3)	(4)	(5)	(6)
	No insurance offered	Insurance type B offered	Insurance type C offered	No insurance offered	Insurance type B offered	Insurance type C offered
40 PhP	4.6%	6.0%	4.8%	9.6%	5.6%**	5.6%**
50 PhP	6.6%	7.8%	6.8%	12.6%	8.2%**	8.3%**
60 PhP	9.4%	10.0%	9.0%	16.0%	11.6%**	11.8%**

Note: Stars indicate significance of the difference to the 'no insurance' state, based on bootstrapped simulation with 200 repetitions

Table 9 summarizes the simulation results of the income distributions by examining the density mass below various "poverty lines". By examining the results for different poverty lines, we can see how robust our main findings are. The poverty line of 50 PhP corresponds to 25% of the initial endowment. Table 9 also shows the results for 40 and 60 PhP. For a poverty line of 50 PhP the simulation results show that, when secret hiding is not available, offering insurance type B changes the poverty rate from 6.6 to 7.8%. Yet, when secret hiding is possible, the poverty rates are 12.6 versus 8.2% with and without availability of insurance B. The respective numbers for insurance type C are 6.8% without hiding, and 8.3% with secret hiding. In the case of no secret hiding 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, e.g. for insurance type B in particular. The main conclusions hold also for the other poverty lines in Table 9. (We also examined other poverty lines, but do not show the results here as they did not provide additional insights.)[85] Table 9 also shows significance levels for testing whether the numbers in columns (2) and (3) are statistically different from those of column (1). Analogously, statistically differences between columns (5) and (6) to (1) are tested. Significance levels are obtained by bootstrapping the combined estimation and simulation process. We observe significant improvements when secret hiding is possible, but no statistically significant differences when hiding is not available.

Whereas Table 9 was based on simulation results, for comparison we report the observed "poverty rates" in our data in Table 10. Since our simulations in Table 9 are based on regressions estimated from these data, both tables should show similar patterns. I.e. for a sample size big enough, Tables 9 and 10 should show the same numbers. But due to our small

[85] The complete distributions of payoffs after transfers under different insurance schemes can be found in Figures A1a and A1b in Appendix I.

sample size, the numbers in Table 10 partly also reflect differences in the shock and covariate distributions across the treatments, therefore the patterns are more noisy than in Table 9. Yet, the general tendency of the simulation can be confirmed in Table 10. Poverty rates are generally lower in the secret hiding case and the situation is less clear without secret hiding. (Table 10 shows the numbers for round 1 because circumstances are most comparable to the simulated situation above.)

Table 10: Observed poverty rates for different poverty lines under different regimes in round 1

Poverty line at:	Without secret hiding option			With secret hiding option		
	(1)	(2)	(3)	(4)	(5)	(6)
	No insurance offered	Insurance type B offered	Insurance type C offered	No insurance offered	Insurance type B offered	Insurance type C offered
40 PhP	1.6%	7.3%	0.0%	9.2%	3.6%	0.0%
50 PhP	4.7%	7.3%	2.6%	12.3%	3.6%	4.7%
60 PhP	9.4%	8.5%	6.4%	14.6%	4.8%	9.4%

The phenomenon that without secret hiding the unlucky are on average not better protected if there is insurance of one type also very directly shows up in another statistic: The poverty rate observed in the data (at 50 PHP) *among those who had a catastrophic shock* is not significantly lower when insurance type B or C is available (30% vs. 31%) compared to in the absence of any insurance (38%). The result changes if secret hiding is possible. Poverty rate amongst the very unlucky is now much lower with access to insurance type B or C (23% vs. 33%) than without any insurance (64%). This later result is significant at the 1% level for type B and at the 10% level for type C, using a two-group test of proportion.[86]

To summarize the findings from Table 9: As long as there is no secret hiding option, the informal insurance system seems to work rather well. The poverty rates with the formal insurance option C are the same as without any formal insurance option; see columns (1) and (3). An expensive insurance product (i.e. option B) may only do harm and increase poverty a little in column (2).

[86] Note that these comparisons are not necessarily balanced as the observations come from different rounds (see 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 small number of catastrophic shocks (N=52 compared to N=169 before) when looking at round one only we still find the same qualitative result. The poverty rate is lower with insurance B / C available if there is secret hiding and the differences are significant at the 1% / 5% level. Without secret hiding the difference is insignificant.

The introduction of secret hiding (i.e. limited observability of shocks) has large effects, though. It reduces the willingness-to-transfer dramatically and the informal insurance system "breaks down". As long as no formal insurance options are available poverty rates almost double (compare column (4) to (1)). Providing the option of formal insurance in this case helps to reduce inequality and poverty almost to levels without secret hiding (compare column (5) and (6) to column (4) and (1), respectively).

- Counterfactual simulations -

The inefficiency of insurance in the case without secret hiding has at least two aspects worth further exploring. First, crowding out reduces the potential of the informal solidarity transfer mechanism. But how substantial is this effect? How efficient would the insurance products be without crowding out? Second, the effectiveness of insurance is limited by incomplete uptake. If insurance availability crowds out solidarity transfers and some individuals remain uninsured, those are then even more vulnerable. How would mandatory insurance work in such a context? Would this decrease vulnerability? In the following we thus provide hypothetical simulation results with mandatory insurance (100% uptake) and with absence of crowding out. (As before, the simulations are based on the regression specifications of Table 8.)

Table 11 shows poverty risk under different insurance/hiding schemes if there was no crowding out effect of insurance. No crowding out effect means assuming the same sensitivity of transfers to payoff differences with and without insurance. Looking at the left part of the table (no secret hiding), we observe a decrease of poverty rates at all poverty lines when insurance becomes available.[87] This is contrary to the effects in table 9 because crowding out is disabled in Table 11. Comparing the poverty rates in Table 9 and 11 illustrates the importance of the crowding out effect for poverty risk.

On the other hand, the numbers on the right side of table 11 (with secret hiding) are more similar to (the right hand side of) table 9. Since we did not find significant crowding out when hiding money was possible, it is not surprising that setting the crowding-out effect to zero does not make a large difference. (Note that the poverty rates in the right hand side of Table 11 are somewhat larger than in Table 9. This is because the estimated coefficients (($Y_i-(Y_j-S_j)$) x Treat B/C) in column (1) of Table 8 were positive, even though not statistically

[87] The effect at all poverty lines can be seen from Figures A2a and A2b in the Appendix (showing complete distributions under different schemes).

significant.[88] The effects on the full distributions are shown in the appendix (figures A2a and A2b).

Table 12 illustrates the effect of introducing a mandatory insurance scheme. We assume the same degrees of crowding out as in table 9.[89] The effects at all poverty lines and under both hiding schemes are very large. In fact, comparing Table 12 to Table 11, we find that full uptake with crowding out has in all cases a much larger effect than voluntary insurance without crowding out. The effects on the full distributions are shown in the appendix (figures A3a and A3b).

Table 11: Poverty rates for different poverty lines– without crowding out (simulation)

	Without secret hiding option			With secret hiding option		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty line at:	No insurance offered	Insurance type B offered	Insurance type C offered	No insurance offered	Insurance type B offered	Insurance type C offered
40 PhP	4.6%	4.0%*	3.0%***	9.6%	6.6%***	6.3%***
50 PhP	6.6%	5.6%**	4.8%***	12.6%	9.4%***	9.1%***
60 PhP	9.4%	7.9%***	6.9%***	16.0%	12.8%***	12.7%***

Note: Stars indicate significance of the difference to the 'no insurance' state, based on bootstrapped simulation with 200 repetitions. Crowding out is disabled by setting the coefficients for ($Y_i - (Y_j - S_j)$) x Treat B and ($Y_i - (Y_j - S_j)$) x Treat C to zero.

Table 12: Poverty rates for different poverty lines– mandatory insurance (simulation)

	Without secret hiding option			With secret hiding option		
	(1)	(2)	(3)	(4)	(5)	(6)
Poverty line at:	No insurance offered	Insurance type B mandatory	Insurance type C mandatory	No insurance offered	Insurance type B mandatory	Insurance type C mandatory
40 PhP	4.6%	1.0%***	0.8%***	9.6%	2.2%***	2.3%***
50 PhP	6.6%	2.0%***	2.2%***	12.6%	3.9%***	4.9%***
60 PhP	9.4%	3.9%***	4.2%***	16.0%	6.9%***	8.3%***

Note: Complete insurance take-up and crowding out as under insurance choice is assumed.
Stars indicate significance of the difference to the 'no insurance' state,
based on bootstrapped simulation with 200 repetitions

[88] Also note that the simulated numbers of Table 11 are less noisy than those of Table 9 because fewer coefficients were estimated in each simulation replication since the coefficients measuring crowding out were fixed at zero.

[89] The only difference to Table 9 is that insurance uptake is set to one for the whole sample. Everything else remains unchanged in the simulation procedure. This simulation is therefore speculative because the introduction of *mandatory* insurance could also change the magnitude of the crowding out. With universal insurance the feeling of responsibility for other individuals could for example decrease further. On the other hand, with universal insurance income differences can no longer be attributed to a lack of precaution, but are purely random. This might even foster the feeling of responsibility and increase the willingness to provide transfers to the poorer. Hence, possible changes in crowding out with mandatory insurance could go either way.

V. Discussion

- External validity -

Our field lab experiment takes an intermediary position between a conventional laboratory experiment designed to test economic theory and a natural field experiment where participants are unaware of being in an experiment (Harrison and List 2004). In our setting participants are representative of the Filipino village population and they are experienced in household-decision making and dealing with income shocks. Further, people do not interact anonymously which each other, but know the person they donate to. We think these features increase external validity compared to conventional lab experiments on risk-sharing carried out with students in developed countries. Results from field lab experiments suggest a strong link between field laboratory and behavior outside the lab, even though there are important contextual differences between the lab and the field environment (Fehr and Leibbrandt 2011; Rustagi, Engel, and Kosfeld 2010; Benz and Meier 2008; Karlan 2005; List 2006). Nevertheless, we discuss three possible limitations to external validity coming from our behavioral field lab experiment in the Philippines shared by many other laboratory experiments.

First of all, one can question the external validity of a laboratory experiment since, almost inevitably in an experiment, all of the experimental outcomes represent gains instead of losses for the participants (e.g. even with the catastrophic shock participants still get 20 pesos of income they would not have had otherwise, plus a 100 peso show-up fee) and the stakes are lower than they would be in real life. Therefore, it remains the question whether the interplay of solidarity, insurance and secret hiding would be the same for true and large losses like a health shock or a crop failure. There is evidence that people become more risk loving and daring when they do not have to use their own money (Thaler and Johnson 1990) or more risk averse when stakes are high (e.g. Binswanger 1980; Holt and Laury 2002). Thus, we would expect less risk taking in real life than what we observed in our field lab. This could be a problem because norms on risk-taking behavior should affect crowding out. However, with more cautious behavior, the responsibility to take up formal insurance might be an even stronger norm and lead to more crowding out of solidarity in case people chose not to insure. Alternatively, windfall money and stake size could directly influence solidarity. Clark (2002), for instance, investigated the impact of endowment origin on contribution levels in a public goods experiment, in which half the sample had to use their own money, while the other half received their endowments as windfall money. He did not find contribution levels to differ between the groups. These results were confirmed in a one-shot variant of the public goods experiment by Cherry, Kroll, and Shogren (2005). There are also several studies pointing towards a negligible stake size effect for average behavior and its variance in cooperation and punishment experiments (Kocher, Martinsson, and Visser 2008) and related game shows where stakes go up to US$16,000 (Oberholzer-Gee, Waldfogel, and White 2010).

Secondly, one can question the interpretation of unframed decisions in our experiment. For example, we interpret the possibility of buying a more secure option in the experiment with formal insurance products. We decided to not label these options "insurance" to prevent confounding factors such as unrealistic expectations created by insurance marketing campaigns.[90] While the interpretation of 'option B' and 'option C' as insurance seems plausible, it might be less plausible to equate hiding money with saving money on e.g. a formal saving account. Nevertheless, given the evidence mentioned in the introduction that many saving products are used to withdraw money from the network we speculate that innovations such as formal savings and mobile banking may further add to making wealth less observable and thereby reducing solidarity. At least in rural areas, wealth and assets such as the number of cattle and livestock, the quality of crops, the size of landholdings, the size and quality of the house etc. are easily observable to everyone, whereas savings on a mobile phone would be invisible. Although spreading formal saving products is certainly a stepping stone in individual and societal development it might be used also for income hiding. Our paper suggests that such hiding possibilities reduce solidarity and that formal insurance could be an alternative protection in this case. While our setting excludes inter-temporal aspects (i.e. storage and explicit reciprocity) our conclusion is strengthened by theoretical models showing a negative effect of storage possibilities on reciprocal risk-sharing (Ligon, Thomas, and Worrall 2000; Wahhaj 2010), thus creating additional need for formal insurance.

Thirdly, for simplicity our experiment focuses on solidarity, which is one but not the exclusive channel for risk-sharing. Risk-sharing is motivated both by mutual benefits that are enforced through reciprocal arrangements and also by solidarity transfers that are motivated by intrinsic motivations coupled with peer pressure. Thus, our experimental measure of solidarity captures one important component of risk-sharing but excludes long-term reciprocal arrangements based on mutual benefits. We exclude reciprocity in our experimental setup by not revealing interpersonal transfers between rounds and to other members in the risk-sharing groups, randomly paying out the earnings from only one round and ex-ante announcing a fixed duration of three rounds. The first reason for this simplification is that, for ethical reasons, we did not want to reveal the transfers of the other players in each round and thereby

[90] Individuals sometimes have very skewed perceptions of (micro)insurance (Giesbert and Steiner 2011).

expose people to ex-post punishment from their co-players. One might argue that the magnitude of transfers would be higher in repeated interactions with public reputations and that the different nature of transfers might change the crowding-out story. However, based on Attanasio and Ríos-Rull (2000) we would expect reciprocity to be even more susceptible to a crowding-out effect, as the availability of insurance decreases future mutual benefits. This would be a good reason to measure aggregate crowding-out, but there is a second motivation to focus on solidarity transfers as in our study. Reciprocity, although pro-social, can be based on egoistic motives. In contrast solidarity is based on the motivation to help others in need. To separate motivational crowding-out effects from dynamic incentive effects it is therefore important to focus on solidarity transfers.

- Explanations for the crowding-out effect -

There is a more persistent negative effect of the catastrophic-only product (C) versus a more comprehensive scheme (B). At the same time we observe considerably lower take-up of the more comprehensive scheme. Thus, participation might be too low for type B to induce a 'common sense' or 'social norm' that formal insurance ought to be bought. The so-called 'framing effect' means that once good formal insurance products become available it becomes socially accepted that everyone is responsible on her own for insurance. Once such a social norm has evolved the moral obligation to provide voluntary transfers to people who willingly have not bought insurance dissipates quickly. Before such changes can be expected, a "good" insurance product must exist for most people. Judging from the take-up rates, insurance option C appears to be closer to a "good" product than option B. Possibly this could be because of the relatively high price of option B. While everybody with reasonably high risk aversion can be expected to purchase the catastrophic-only insurance C this is not the case for the more expensive version B, which would be appealing only to very risk-averse persons. A simple simulation using a constant relative risk aversion (CRRA) utility function [$u(c) = (c^{1-\rho})/(1-\rho)$], where risk-averse individuals have $\rho > 0$, shows that, in the absence of any solidarity transfers, all people with a risk-aversion parameter $\rho > 0.34$ would buy insurance C if available, whereas only people with $\rho > 0.65$ would buy insurance B. When comparing take-up of insurance type B and C in round one and three (figure 2) we would consequently classify around 35-40% as highly risk averse ($\rho > 0.65$), 5-16% as moderately risk averse ($0.34 < \rho < 0.65$) and 50-55% as less risk averse ($\rho < 0.34$). Interestingly, Holt and Laury (2002, p. 1649) in a high stake experiment estimate the following proportions: 39%

with $\rho > 0.68$, 23% with $0.41 < \rho < 0.68$, 38% with $\rho < 0.41$.[91] Even though the comparison of our participants with US students might be a little far-fetched, the proportions are similar.[92] Thus it seems plausible that insurance type C is considered a more convincing alternative. While the above explanations could rationalize the differential persistence of the crowding-out effect in the second round, they do not explain why the effect in the first round is *not* different. In this respect our results are inconclusive. However, we have to bear in mind that our insignificant estimate for the persistence of insurance type B effect does not mean that there is no effect. The confidence interval is large enough to allow for effects of a similar magnitude as for insurance type C. Both types of insurance thus might have instantaneous as well as persistent crowding-out effects.

An alternative explanation for generally lower motivation to transfer with insurance could be the information effect. 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 or commitment in the informal risk-sharing mechanism. This 'information effect' could apply if individuals observe the insurance-buying behavior of their co-players in previous rounds and then form expectations about the solidarity/selfishness-types of their co-players. This 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 (results available upon request) 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. Thus, the information effect is unlikely to be the main or the only effect.

- Implications of the crowding-out of intrinsic motivations for public policies -

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. Thus our experimental results suggest that in a world without the possibility to hide income it can be better to have no voluntary insurance at all or alternatively force everybody into a compulsory public insurance scheme. Otherwise, there might always be a considerable fraction without insurance, as our data shows for every round. Although

[91] We assume constant relative risk aversion and our reasoning is therefore independent of stake size.
[92] Cardenas and Carpenter (2008) provide a comparison of CRRA parameter estimates in developing countries and state: "There is some variation in the results (Table 5) but it is not explained by development. ... Overall, there does not appear to be much support for the idea that poor people in developing countries are more risk averse than richer people in developed countries." (Cardenas and Carpenter 2008, p. 326)

solidarity transfers are reduced by the access to insurance this reduction is ‘not sufficient’ 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 incentives to care for him or herself (i.e. to 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 to not provide help to uninsured individuals who face a loss, there will be an undesired underinsurance (compared with compulsory insurance) together with a crowding-out effect (compared to the no-insurance case). We also conducted various hypothetical counterfactual simulation analyses. These simulations indicated that mandatory insurance could solve the coordination problem and lead to reductions in vulnerability (see table 12).

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 could even be harmful. However, the story is different when anonymous saving options are available or will be introduced. Even though it might seem that introducing hidden income plays a harmful role in our experiment there are many good reasons to introduce such financial products. Especially, people will be able to use their savings for intertemporal income smoothing (or for saving for lumpy investments such as buying refrigerators, bicycles etc), an aspect we completely excluded from our analysis. 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.

VI. Conclusion

Informal risk-sharing is frequent in many developing countries, but usually cannot offer full protection because of limited enforcement mechanisms. In addition, even if people would voluntarily want to help each other in case of shocks they may not be able to do so in the case of covariate shocks, e.g. if the entire network is affected by a disease or other catastrophe. This gives rise to demand the introduction of formal insurance products tailored to the needs of the poor. In this paper we present a novel behavioral experiment with rural and partially urban villagers in the Philippines. This experiment – simulating a risky environment with solidarity networks and introducing two insurance options – delivers the first experimental evidence on whether informal solidarity is reduced by formal insurance in developing countries.

Our data highlight that the availability of insurance reduces solidarity and that this negative effect might even persist if insurance is removed. The latter finding is particularly relevant for policy guidance in indicating that a flawed introduction of formal insurance products, e.g. of too expensive or inappropriate insurance that is later withdrawn, can possibly have negative long-term consequences. The empirical analyses revealed that solidarity not only decreases due to lower inequality between those with insurance but that there is an additional motivational crowding-out effect on solidarity. However, this is only the case if shocks of network members are fully observable. When hiding income is possibly, almost all (=94%) participants used the possibility to pretend shocks and the solidarity system breaks down to a large extent. Hence, a lot of the solidarity transfers seem to be motivated by "internalized peer pressure" rather than altruism. This is consistent with evidence that in everyday life people use (or like to use) hiding devices and pretend to be illiquid. With solidarity transfers being so low, observing further reductions is hard. Hence, introducing the option of formal insurance cannot lower the already low solidarity transfers much more. This might explain why the insurance effect is found only in those villages where secret hiding was not possible, i.e. where substantial solidarity still existed.

To summarize, our experimental evidence suggests that the introduction of insurance in solidarity networks might have negative welfare effects under some circumstances. Especially if the network is able to observe the cash flow of members and 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).

Appendix I

Table A3: 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
How many different religious groups in this village	2.45	1.26	1	5	2.67	2.5	2.25
Households with family members abroad	9.2%	9.0%	0	34.5%	7.5%	13.0%	6.7%
Conflicts between people (0=none, 1=a little, 2=a lot)	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 (0 [absolutely agree] – 10 [abs. disagree])	6.36	2.97	0	10	5	7.88*	5.88
Trust to lend/borrow (0 [absolutely agree] – 10 [abs. disagree])	5.27	3.18	0	10	5	5.38	5.38
Always somebody willing to help (0 absolutely agree – 10 abs. disagree)	7.95	2.36	0	10	8.17	8.13	7.63
Income Class (officially assigned: 1 richest – 5 poorest)	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

Data obtained from the village representatives (includes personal assessment of village social climate).
Stars indicate significance level of Wilcoxon ranksum test for differences to mean from treatment A
*** p<0.01, ** p<0.05, * p<0.1

Complementary analysis to Tables 4 to 6

In Tables 4 to 6 we had examined descriptive statistics between the different treatment groups. One needs to keep in mind that the treatment groups were randomly assigned such that no systematic differences in characteristics as well as the random dicing results, i.e. the random shocks, can exist. Given our small sample size, though, some non-systematic differences are still very likely. I.e. in some scenarios large shocks might have occurred by chance more often. This will make any comparisons of the numbers within Tables 4 to 6 noisy. To deal with such differences, we used parametric regressions in the main text. In the following we describe a nonparametric alternative to control for different shock distributions across treatment blocks 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 identical. We furthermore also require that the round number, the network strength and the availability of the hiding device are the same. We test the effect of hiding analogously, simply making secret hiding the treatment variable and adding insurance availability and type as a control variable. Table A2 shows the average treatment effect (ATE) of the two insurance treatment blocks and the secret hiding option using exact matching, separately for round one and two. The last two columns of Table A2 repeat the insurance treatment matching for the subset of villages without the secret hiding possibility.

Table A4: Average treatment effect (ATE) of treatment blocks on net transfers to disadvantaged co-players (with and without hiding)

		all			no hiding		hiding	
Round	Outcome variable	Block AB vs. A (ATE)	Block AC vs. A (ATE)	Hiding vs. no hiding (ATE)	Block AB vs. A (ATE)	Block AC vs. A (ATE)	Block AB vs. A (ATE)	Block AC vs. A (ATE)
1	Net Transfer to recipient	-0.4	**-6.9***	**-8.5*****	-6.0	**-10.3****	+7.1	-2.0
	Obs (On/off support)	131 / 44	140 / 23	217 / 53	75 / 17	82 / 7	56 / 27	58 / 16
2	Net Transfer to recipient	+6.8	-4.8	**-10.9*****	+4.2	**-11.6****	+9.6	+2.4
	Obs (On/off support)	132 / 38	153 / 28	259 / 23	68 / 16	79 / 14	64 / 22	74 / 14

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

The results of Table A2 strongly confirm the results of Tables 4 – 6, 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 hiding 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 hiding lockbox again does not show any significant effects. Effects of the secret hiding device are negative, large and highly significant in both rounds. The descriptive and matching results clearly show a negative effect of the secret hiding device on solidarity (Hypothesis III). Hence, the option to hide resources is 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 hiding. 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 hiding 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: The point estimates are insignificant and are even sometimes positive.[93]

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.

Complementary analysis to Table 7

Table A3 complements Table 7 and shows the coefficients on the control variables. I.e. the first 11 lines are identical to Table 7, and thereafter show the coefficients on the control variables that have been omitted from Table 7. It seems that men tend to give more. Age has a positive marginal effect until 45 years, when the marginal effect turns negative. Being indebted and regular income are associated with higher latent values. Characteristics of the recipient are mostly insignificant. Regarding the experimental setup participants are less inclined to give in later rounds and in weaker networks. On the community level higher income class (= poorer) is associated with lower willingness to transfer, while partially urban communities and (curiously) those that are evaluated as more selfish by the barangay captain exhibit positive coefficients. These effects are all level effects and unrelated to differences in earnings.

Table A5: Regressions fromTable 7 with individual and village/round covariate coefficients

	(1)	(2)	(3)	(4)	(5)	(6)
		All observations		All observations	Only observations without secret hiding option	Only observations with secret hiding option
Y_i	0.047	**0.048***	**0.046***	**0.048***	0.036	**0.083***
Observable Difference ($Y_i-(Y_j-S_j)$)	**0.13*** **	**0.13*** **	**0.13*** **	**0.12*** **	**0.15*** **	0.082
Hidden income of sender (S_i)	**-0.11*** **	**-0.13*** **	**-0.14*** **	**-0.14*** **		**-0.15*** **
Hidden income of recipient (S_j)	-0.018	**-0.033***	**-0.034***	**-0.034***		**-0.026***
($Y_i-(Y_j-S_j)$) x Treat B				-0.020	**-0.095****	**0.052***

[93] Remember that the (insignificant) positive difference between treatment blocks AB and A in the first round might be explained by the lower amount that can be secretly hidden with insurance B.

$(Y_i-(Y_j-S_j))$ x Treat C				-0.021	**-0.089****	0.033
$(Y_i-(Y_j-S_j))$ x Treat B x Treat C				0.060	**0.15***	-0.021
$(Y_i-(Y_j-S_j))$ x PseudoTreat B				0.025	-0.032	**0.098****
$(Y_i-(Y_j-S_j))$ x PseudoTreat C				-0.029	**-0.11****	0.035
$(Y_i-(Y_j-S_j))$ x Round				0.015	**0.030****	-0.0058
$(Y_i-(Y_j-S_j))$ x Weaker Network				-0.031	-0.0092	-0.011
Treat B		-0.55	-0.37	-0.14	-5.95	-0.031
Treat C		-1.11	-1.74	-1.35	-3.53	-2.38
Treat B x Treat C		0.46	1.52	-0.098	7.23	**-11.2***
PseudoTreat B		0.58	0.96	-0.31	0.082	**-14.1****
PseudoTreat C		3.64	3.06	3.74	0.43	-4.36
Round		**-2.81*****	**-2.99*****	**-3.36*****	**-3.34*****	**4.73*****
Weaker Network		**-7.88****	**-6.95*****	**-6.09****	-4.11	**-8.42*****
Income Class		**-4.83****	**-4.43****	**-4.19****	**-6.52*****	**4.12****
Partially urban		**9.18****	**7.95****	**8.07****	**11.2*****	**-6.02****
People are selfish		**1.32****	**1.18***	**1.19***	**2.54*****	**-2.49*****
Sender…						
Regular income?			**4.11***	**4.13***	**5.41****	2.07
Skip meals last month			0.76	0.88	-1.19	0.75
Debt > 1000 Pesos?			**5.62****	**5.62****	**8.24****	2.50
Male			**10.9****	**11.0****	**17.9*****	3.57
HH head			0.45	0.56	-3.69	5.02
Male x Household head			-4.95	-5.23	-3.40	-4.12
Married			3.92	3.65	3.81	1.69
High school			-2.30	-2.62	2.26	**-6.00****
College			-2.16	-2.63	2.82	**-6.79*****
Age			**1.72*****	**1.75*****	**1.80****	**1.89*****
Age squared			**-0.019*****	**-0.020*****	**-0.020****	**-0.021*****
Recipient…						
Regular income?			1.09	1.18	1.33	0.69
Skip meals last month			2.57	2.72	0.69	1.24
Debt > 1000 Pesos?			0.96	0.97	3.57	-2.56
Male			-1.59	-1.78	4.79	**-8.44****
HH head			0.32	-0.041	-1.24	1.44
Male x Household head			2.17	3.02	-1.86	8.45
Married			1.25	1.30	1.25	0.94
High school			-2.15	-1.92	**-4.66***	3.01
College			1.83	2.17	0.31	**5.89****
Age			0.40	0.38	0.20	0.76
Age squared			-0.0030	-0.0029	-0.00027	-0.0066
Observations	2730	2730	2730	2730	1664	1066

Standard errors clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1
Treat B is a dummy variable that indicates availability of insurance type B, it is 1 for villages in treatment block AB in round 1 as well as round 3, and in block A in round 3. PseudoTreat B is a dummy variable that is 1 for villages in treatment block AB in round 2, i.e. where insurance had been available in the previous round but not in the current round.
Treat C and PseudoTreat C are defined analogously for villages in treatment block AC.
The interaction term (Treat B x Treat C) is a dummy variable that is only 1 for villages in treatment block A in round 3, where both insurance types where offered at the same time.
The regressor *Round* takes the values 1, 2 and 3.
The regressor *Weaker Network* is a dummy variable that is 1 if the groups of three players had been formed at random.

Analysis of buyers and non-buyers of insurance

Buying insurance was voluntary and a substantial number did not buy insurance. In the following two tables we examine how those individuals who bought insurance in our experiment differ from those who did not.

Table A6: Descriptive statistics of people who bought insurance in round 1 versus who did not

	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
High school 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

Table A7: Descriptive statistics of people who bought insurance in round 3 versus who did not

	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
High school 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

Complementary analysis to Tables 7 and 8

Table A6 shows alternative specifications of the Tobit regressions and complements Tables 7 and 8. We examine in particular whether controlling for village-round fixed effects or for a larger set of controls makes a difference to the results. In addition, we analyze the importance of clustering standard errors. Overall, we find stable results. In particular, it does not make a large difference whether we cluster standard errors at the village level, the individual level or whether we use the corresponding bootstrap estimate.

Column (1) of table A6 is the same as specification (1) in table 8. Column (5) is the same as specification (2) in table 8. Columns (2) and (6) in Table A6 add village-round fixed-effects. Most of the significant coefficients remain unchanged, particularly those most interesting to our analysis: The interaction terms which reflect crowding out and persistence. It shows that our regressions are not sensitive to the inclusion of further controls on the village-round level.

In columns (3) and (4) we use the same specification as in (1), but different methods for inference. (Hence, the estimated coefficients are all the same.) While in column (1) we used asymptotic standard errors clustered at the *village level*, column (3) uses clustering at the *individual level*. In column (4) we estimate standard errors using 1000 bootstrap replications, where the unit of resampling is the individual (i.e. all three rounds together are re-sampled). (Columns (7) and (8) are analogously for the villages without secret hiding option.) Overall, the different methods to obtain standard errors lead to rather similar results, with the bootstrap standard errors tending to be a little larger. Thus it is at least as difficult to obtain significant results of the simulation as with error clusters on the village level.

This comparison of the different methods to obtain standard errors is important for the following reason. In Section IV we examined various simulations of poverty lines. The simulation results depend on the estimated β coefficients, in a complicated way such that asymptotic standard errors would be difficult to obtain. We therefore use bootstrap methods to obtain some information about the statistical variability of the simulations of Section IV. Since the number of villages is too small for resampling villages, we have to resample individuals, thereby missing any within-village correlations. The results of Table A6 indicate that any within-village correlations do not appear sufficiently large to have a big impact on the variance of the estimated β coefficients. (Further details for the simulation procedure are given in appendix II: The simulation is repeated for different parameter estimates to obtain confidence bounds, each of which stems from bootstrap repetitions. To adequately translate clustered standard errors on the village level, one should cluster the bootstrap on the same level. Yet, with such large clusters we have too few clusters for the bootstrap. Instead one can cluster the bootstrap procedure on the individual level (two transfer decisions times three rounds per participant), where we are left with enough clusters.)

Table A8: Tobit regressions explaining transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Only observations with secret hiding option				Only observations without secret hiding option			
Y_i	**0.082***	0.046	**0.082***	0.082	0.036	0.024	0.036	0.036
	(0.042)	(0.043)	(0.049)	(0.053)	(0.026)	(0.030)	(0.022)	(0.024)
$(Y_i-(Y_j-S_j))$	0.0028	0.051	0.0028	0.0028	0.13	0.12	0.13	0.13
	(0.13)	(0.14)	(0.13)	(0.15)	(0.100)	(0.10)	(0.089)	(0.093)
Hidden income of sender (S_i)	**-0.15*****	**-0.13*****	**-0.15*****	**-0.15*****				
	(0.044)	(0.049)	(0.047)	(0.049)				
Hidden income of recipient (S_j)	**-0.027***	-0.022	-0.027	-0.027				
	(0.015)	(0.014)	(0.018)	(0.019)				
$(Y_i-(Y_j-S_j))$ x Treat B	0.041	0.046	0.041	0.041	**-0.091****	**-0.081***	**-0.091****	**-0.091****
	(0.041)	(0.043)	(0.062)	(0.068)	(0.045)	(0.048)	(0.044)	(0.044)
$(Y_i-(Y_j-S_j))$ x Treat C	0.031	0.028	0.031	0.031	**-0.096*****	**-0.097*****	**-0.096****	**-0.096****
	(0.062)	(0.064)	(0.063)	(0.068)	(0.035)	(0.036)	(0.038)	(0.039)
$(Y_i-(Y_j-S_j))$ x Treat B x Treat C	-0.014	-0.019	-0.014	-0.014	**0.15****	**0.14***	**0.15***	**0.15***
	(0.17)	(0.18)	(0.17)	(0.18)	(0.075)	(0.080)	(0.082)	(0.090)
$(Y_i-(Y_j-S_j))$ x PseudoTreat B	0.086	**0.099***	0.086	0.086	-0.020	-0.0098	-0.020	-0.020
	(0.055)	(0.060)	(0.058)	(0.065)	(0.038)	(0.042)	(0.046)	(0.049)
$(Y_i-(Y_j-S_j))$ x PseudoTreat C	0.029	0.027	0.029	0.029	**-0.11*****	**-0.11*****	**-0.11*****	**-0.11*****
	(0.038)	(0.037)	(0.054)	(0.059)	(0.037)	(0.037)	(0.041)	(0.043)
$(Y_i-(Y_j-S_j))$ x Round	-0.0029	-0.0046	-0.0029	-0.0029	**0.027***	**0.030***	**0.027***	**0.027***
	(0.056)	(0.058)	(0.057)	(0.060)	(0.015)	(0.016)	(0.015)	(0.015)
$(Y_i-(Y_j-S_j))$ x Weaker Network	-0.016	-0.012	-0.016	-0.016	-0.0012	-0.0074	-0.0012	-0.0012
	(0.030)	(0.030)	(0.034)	(0.037)	(0.029)	(0.030)	(0.023)	(0.025)
$(Y_i-(Y_j-S_j))$ x Income Class	0.020	0.016	0.020	0.020	0.020	0.023	0.020	0.020
	(0.023)	(0.025)	(0.033)	(0.037)	(0.025)	(0.025)	(0.025)	(0.026)
$(Y_i-(Y_j-S_j))$ x Selfish	-0.0043	-0.0060	-0.0043	-0.0043	-0.0055	-0.0057	-0.0055	-0.0055
	(0.0067)	(0.0061)	(0.0070)	(0.0077)	(0.0059)	(0.0060)	(0.0050)	(0.0052)
$(Y_i-(Y_j-S_j))$ x Higher Education	0.062	0.057	**0.062***	0.062	**-0.035***	-0.032	-0.035	-0.035
	(0.039)	(0.042)	(0.034)	(0.038)	(0.021)	(0.020)	(0.026)	(0.026)
$(Y_i-(Y_j-S_j))$ x HH Head	-0.0045	-0.0036	-0.0045	-0.0045	**0.054***	**0.050***	**0.054****	**0.054****
	(0.030)	(0.029)	(0.032)	(0.035)	(0.028)	(0.029)	(0.025)	(0.026)
Village-round fixed effects	NO	YES	NO	NO	NO	YES	NO	NO
Village-round controls	YES	-	YES	YES	YES	-	YES	YES
Individual controls	YES	YES	YES	YES	YES	YES	YES	YES
Standard errors	Clustered at village level	Clustered at village level	Clustered at individual	Bootstrap (ind. cluster)	Clustered at village level	Clustered at village level	Clustered at individual	Bootstrap (ind. cluster)
Observations	1066	1066	1066	1066	1664	1664	1664	1664

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

Complementary figures to poverty simulations for Tables 9, 11 and 12

In Tables 9, 11 and 12 we showed the fraction of people below a certain income level for three different thresholds. In the following we show the entire distribution functions.

The following Figure A1 complements Table 9. In Figures A1a and A1b we show the complete distributions of final payoffs under different insurance schemes. The distributions are obtained using the simulation procedure described in appendix II. Figure A1a compares the benchmark of no insurance with availability of insurance without the secret hiding mechanism (treatment AB: availability of type B; treatment AC: availability of type C). The lower tails of the three distributions are very similar and the cumulative distribution functions with insurance are even slightly above the one without insurance in the left tail. Hence, there is no evidence that insurance availability decreases poverty risk.

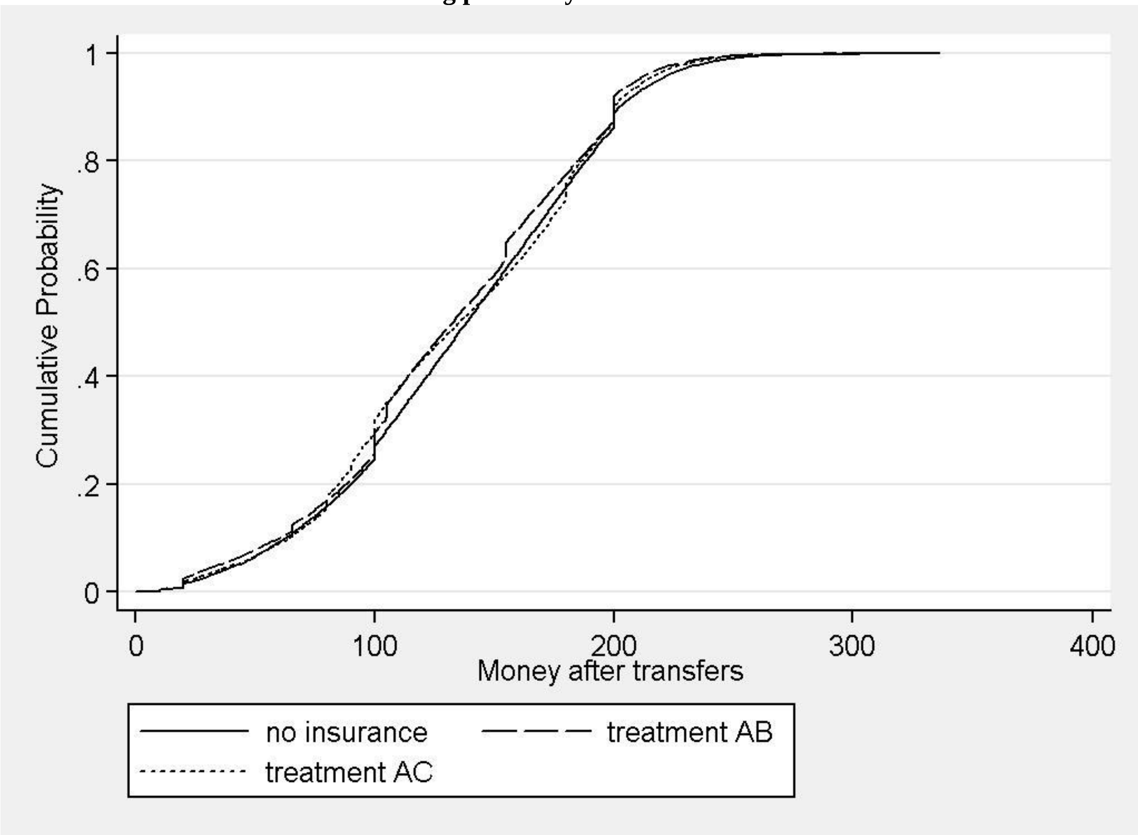
Figure A1b repeats the simulation exercise with the secret hiding device in place. The lower tail of the CDF without insurance visibly increases as compared to the figure A1a. This means that the possibility of secret hiding increases poverty risk. Different to figure A1a, however, it clearly decreases with both insurance types in the range between 20 and 80 pesos. Table 9 shows specific poverty rates under different schemes for (arbitrary) poverty lines of 40, 50 and 60 PhP.

The reason for these findings is that the informal insurance system seems to work rather well in the absence of secret hiding. The poverty rates with the formal insurance option C are the same as without any formal insurance option. An expensive insurance product (i.e. option B) may only do harm because any additional protection is cancelled out by the crowding out effect on informal solidarity transfers. In contrast, when secret hiding is possible, solidarity is low, no crowding out effect of insurance can be observed and insurance is effective in reducing vulnerability.

Figures A2a and A2b complement Table 11 and show the complete distributions of final payoffs under different insurance schemes when assuming no crowding out effects of insurance. Figures A3a and A3b complement Table 12 and show the distributions when assuming mandatory insurance with full take-up.

Figure A1: CDF of payoff under different insurance schemes

a. without secret hiding possibility



b. with secret hiding possibility

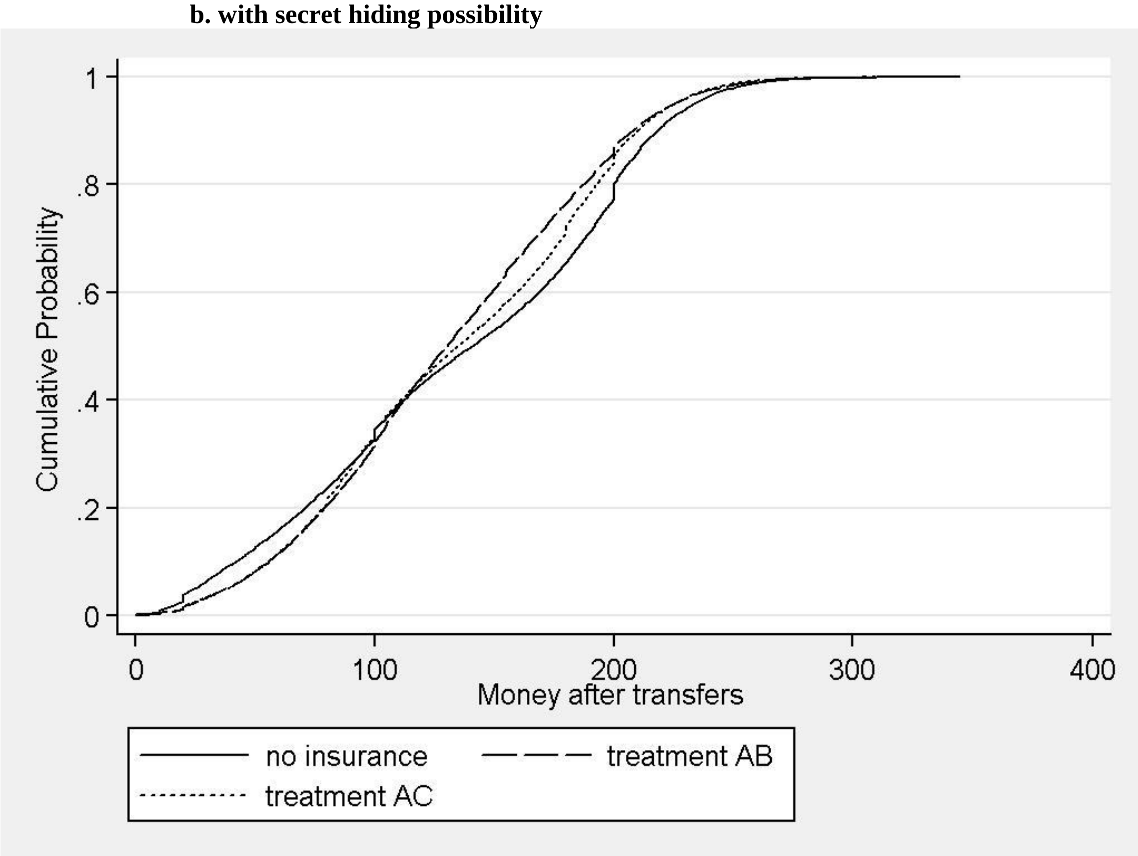
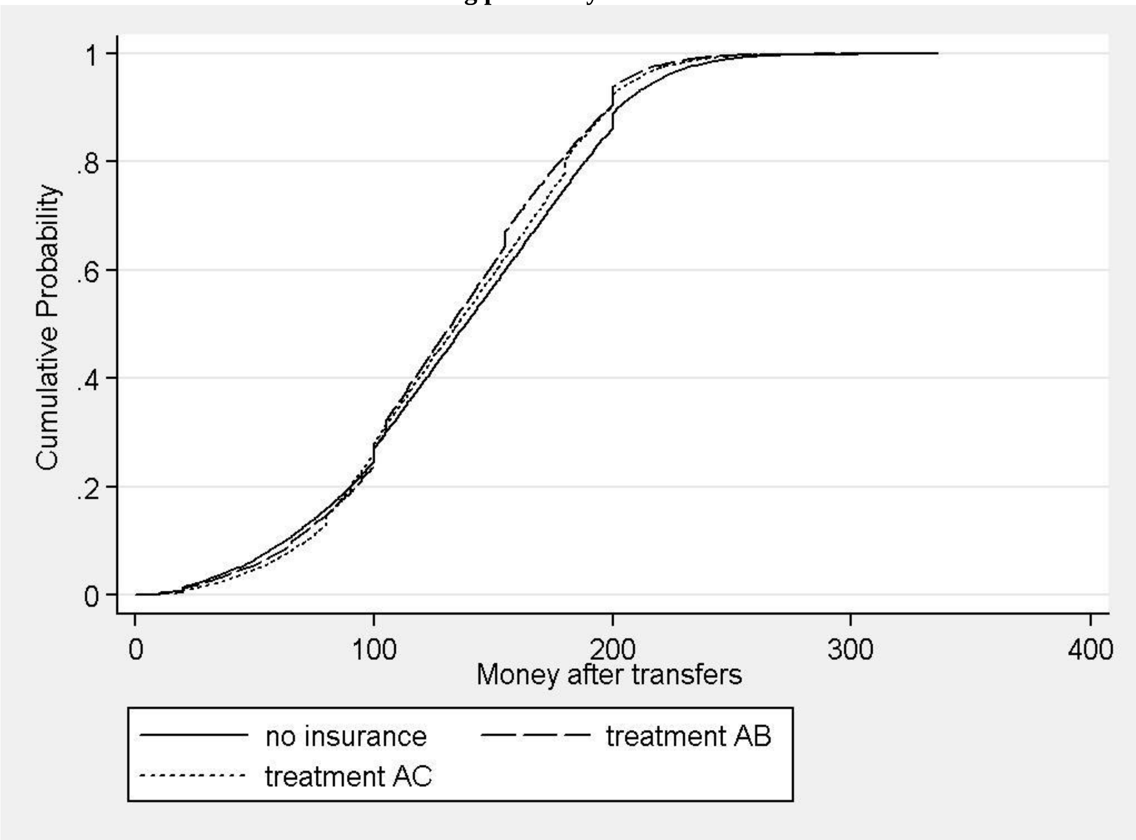


Figure A2: CDF of payoff under different insurance schemes –no crowding out effect of insurance

a. without secret hiding possibility



b. with secret hiding possibility

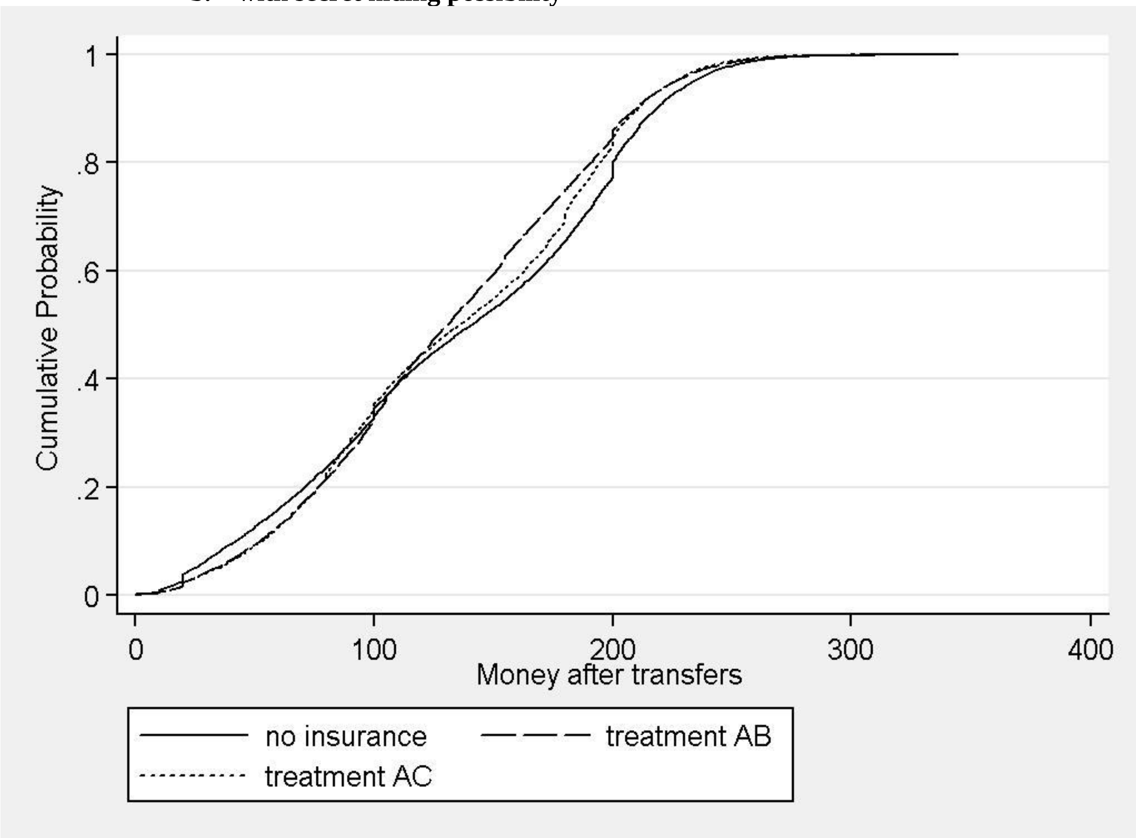
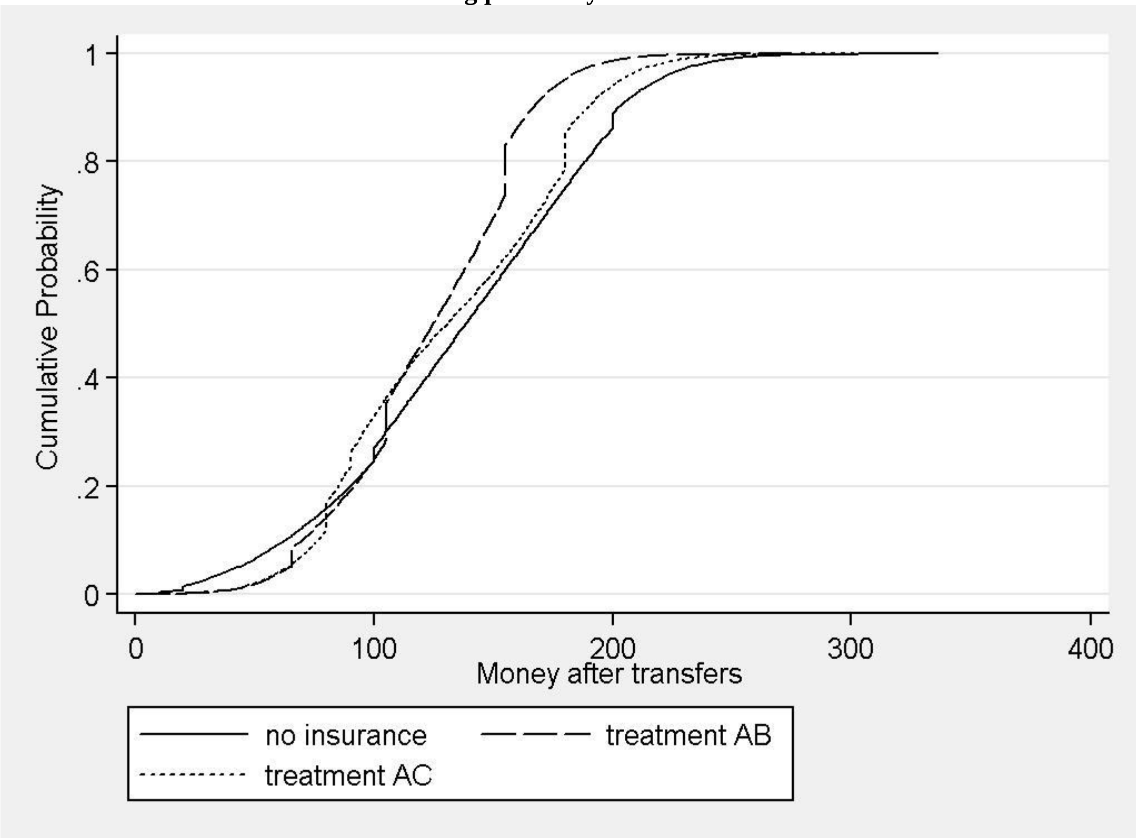
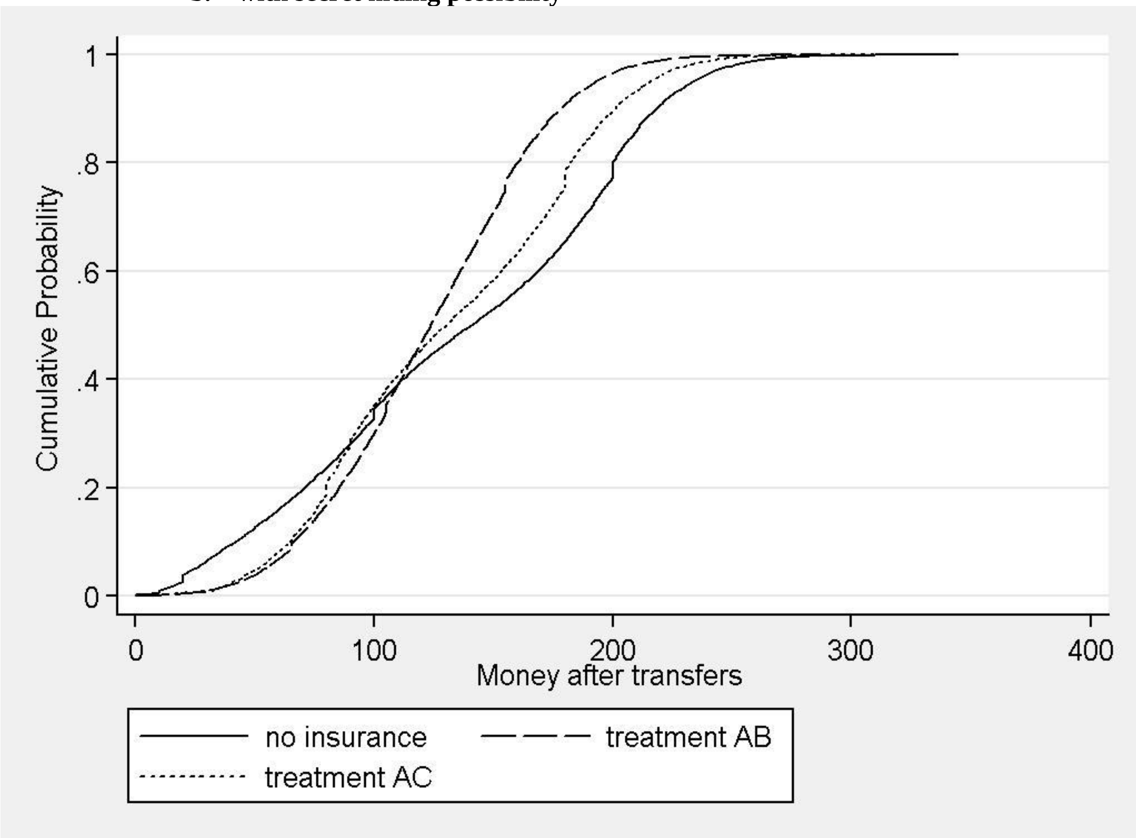


Figure A3: CDF of payoff under different insurance schemes –mandatory insurance

a. without secret hiding possibility



b. with secret hiding possibility



Appendix II: Simulation of transfers under different insurance regimes

For the simulations in Tables 9, 11 and 12 we estimate the models with and without the secret hiding device separately. Our reference regressions are models (1) and (2) from table 8. The details of the simulation protocol are described below.

In addition to simulating the poverty rates under different scenarios we would also like to know about the statistical variability of these estimated poverty rates. Given the complex simulation procedure, their asymptotic distribution is not trivial. As an alternative, noting that all estimation and simulation steps we use are “smooth”, we use the bootstrap to obtain confidence bounds for the simulated poverty rates, by bootstrapping the entire estimation and simulation process. Unfortunately, the number of villages is rather small, such that resampling entire villages could lead to very noisy estimates of the confidence intervals. Instead, we resample individuals. This means we resample at the individual level, i.e. always bundling six transfer observations (two recipients in three rounds for each participant).

Resampling individuals will imply that any within-village correlation structure in the willingness-to-give will get lost. Such possible within-village correlation structures, however, did not appear to have major impacts on the regressions as witnessed in Table A6. There we had analyzed how difference inference methods would change estimated standard errors for the willingness-to-give regressions. We found that asymptotic standard errors with clustering at the village level, asymptotic standard errors with clustering at the individual level as well as bootstrapping at the individual level led to rather similar results, such that we use bootstrap resampling at the individual level for the simulated poverty lines.

The following protocol describes the simulation of the poverty rates. After having estimated the coefficients, we draw random shocks and simulate transfer behavior. In order to eliminate the variance caused by drawing random numbers (shocks, error terms for willingness to transfer), we repeat this process many times. (The actual implementation of repeating this many times was done by expanding the original dataset 100 times.) This delivers an estimate of the expected poverty rate. For estimating the variability of this estimate, the entire simulation protocol is bootstrapped.

Simulation protocol:

1. Estimate model of transfers:

   We estimate regression model (1) and (2) from table 8 for the sample with/without secret hiding. 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 extend the whole data set 100 times (including observations both with *and* without secret hiding possibility).

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 choose insurance in our simulation, we draw an individual-round 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 is obviously a simplification, but in the game we observe that take-up in the first round is no perfect predictor of take-up in round three.)[94]

5. Draw hiding propensity (if applicable):

   94% of participants secretly hide if they have the possibility to do so. This is also the probability in the simulation procedure.

6. Draw error term

   We draw an error term from the normal distribution with the estimated variance from the regression model (1) and (2) of table 8. This is an independent term for each (individual-round-recipient) observation.

7. 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 (see step 4)

[94] We could explicitly estimate a model to explain insurance take-up, conditional on the covariates. However, this would very much complicate our exercise. Also, distribution is only affected by this simplification if there are substantial factors connected to insurance take-up *and* redistributive preferences at the same time.

c. Assign loss/payoff according to shock (see above) and insurance ‘choice’ (this is the pre-transfer outcome)
d. Assign secret hiding (if applicable)
e. Using the previously generated random variables, calculate all the regressors displayed in Table 8
f. Predict transfers, using the β coefficients estimated in step 1 and adding the error term of step 6
g. Left-censor transfers (according to Tobit specification) and right-censor at half of post-lottery income. (We censor at half of the income because each player can make transfers to two co-players. By censoring at half of the income we ensure that the total transfers made to both players are at most 100% of the income. I.e. in our design, people cannot transfer more money than what they have.)
h. From the post-lottery income and censored transfers calculate post-transfer incomes.

The income/loss distribution from each setting can now be further analyzed (e.g. for poverty rates, etc.). To obtain confidence bounds for the distribution, coefficients of the reference model are estimated repeatedly with new bootstrap samples (clustered at the individual level). With each bootstrap estimation, the simulation is repeated.

Appendix III: Differences in crowding-out between insurance type B and C

In table 8 we studied subsets of those participants where secret hiding was not possible as a robustness check. We examined the effects separately for a 'high understanding' subsample (column 3), for rounds one and two (column 4), and rounds two and three (column 5). The interaction coefficients are significant in two out of three cases for the more general insurance type B, but this is not the case for catastrophic-insurance type C. While effects always point in the expected (negative) direction, they are insignificant and smaller than for insurance type B. This raises the question whether the crowding-out effect of insurance type C is systematically smaller, or whether this is simply random variation. We therefore use round 1 and round 3 of treatment blocks AB and AC to directly compare the sensitivity to differences between both treatment blocks.[95] Note that we are in the case without secret hiding, so S_i, S_j do not play a role because they are both zero by design. Therefore the following table contains the regressor $(Y_i - Y_j)$ instead of $(Y_i - (Y_j - S_j))$.

Table A7 shows the sensitivity to differences between treatment B and C in round 1 (column 1), round 3 (column 2) and pooled in round 1+3 (column 3). The interaction *$(Y_i - Y_j)$ x Treat B* indicates differences in the crowding-out effect. Due to the limited sample size they are all insignificant. In specification (1) there is a substantial negative interaction effect, indeed indicating that in the first round the crowding-out of solidarity might be larger for treatment B, although not statistically significantly different. In specification (2), however, the sign of the coefficient is reversed. Pooling the two rounds gives a very small estimate of only 0.0035. This suggests that there is no distinguishable difference between the two crowding-out effects and that variation in the interactions across rounds is purely random.

Table A9: Tobit regressions explaining transfers – treatment B vs. C

	(1)	(2)	(3)
	Only observations without secret hiding option, only treatment block AB vs. AC		
	Round 1	Round 3	Round 1+3
Y_i	-0.054	0.038	0.014
$(Y_i - Y_j)$	**0.18*** **	**0.11*** **	**0.062***
$(Y_i - Y_j)$ x Treat B	-0.055	0.027	-0.0035
$(Y_i - Y_j)$ x Round			**0.029** **
$(Y_i - Y_j)$ x Weaker Network	-0.033	0.015	0.0012
Village-round controls	YES	YES	YES
Individual controls	YES	YES	YES
Observations	320	652	972

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

[95] See Table 2 (treatment plan) to notice that the experimental setup is perfectly balanced in this comparison.

Appendix IV: Test questionnaire

(Notes: Example for treatment block AB, round 1, with hiding. 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 ☐

4. Can Microinsurance Help Prevent Child Labor? An Impact Evaluation from Pakistan

I. Introduction

Poor households in developing countries are especially vulnerable to economic shocks. They often have limited access to savings, credit and tangible assets. As a consequence, households might have to sell productive assets important for long-term income generation, reduce consumption below critical values, take children out of school to save school fees, or send children to work as an additional income source. The economic literature (see Edmonds (2008) for an excellent review) confirms that economic shocks are an important determinant of child labor for low-income households (e.g. Beegle, Dehejia, and Gatti 2006; Duryea, Lam, and Levison 2007). At the same time many studies show substantial negative side-effects associated with child labor, such as lower human capital accumulation (e.g. Heady 2003; Rosati and Rossi 2003; Gunnarsson, Orazem, and Sánchez 2006), lower wages in adult life (Emerson and Souza 2011) and potentially even negative long-term health outcomes (Kassouf, McKee, and Mossialos 2001).

Insurance, on the other hand, is supposed to decrease vulnerability to shocks by smoothing its economic consequences. It is therefore straightforward to think about microinsurance as a potential tool to fight child labor. Given the importance of breaking the intergenerational poverty trap, this is a highly interesting topic. Surprisingly, there is no rigorous impact evaluation studying the effect of microinsurance on child labor outcomes up to now. To the best of our knowledge, there is only one working paper comparing individuals without microcredit, microcredit clients and microcredit clients who are covered by additional insurance regarding their child labor outcomes (Chakrabarty 2012). In contrast to our study, however, his study potentially suffers from selection bias and limited sample size.[96] Assessments so far have mainly focused on the impact of insurance on financial protection and access to medical services in the case of health insurance (Wagstaff 2010; Wagstaff et al. 2009; Dror, Koren, and Steinberg 2006; Dekker and Wilms 2010; Jütting 2004). Some other work has been done on agricultural investment decisions with insurance (Giné and Yang

[96] Chakrabarty (2012) finds that micro life and health insurance in combination with credit can reduce child labor for poor households. Unfortunately, the author cannot make use of exogenous variation in insurance coverage; the data does not have a panel dimension and the sample size is very small.

2009) and crowding out effects on informal risk-sharing (Landmann, Vollan, and Frölich 2012). The effect of microinsurance on child labor remains an open question, though.

This paper provides evidence on the effects of insurance on child labor. We exploit the extension of an accident and health insurance scheme offered by the National Rural Support Program (NRSP), a large MFI in Pakistan. It is a mandatory insurance for all clients, their spouses and their children below 18 years. In 2009, the program was extended to include supplementary household members (adult children of the client and other household members) on a voluntary basis. In addition, clients were assisted with claim procedures. This package of two innovations was implemented as a randomized controlled trial (RCT) in nine out of thirteen branch offices in the research area. The large panel data set with one baseline and four follow-up surveys allows the identification of treatment effects using difference-in-difference techniques. We thus do not rely solely on the randomized design, but we are furthermore able to control for small-sample imbalances in pre-treatment covariates. We are able to disentangle the effect of the two innovation components (assistance with claims, extended coverage of household members) by estimating treatment effects for those households consisting only of individuals with mandatory insurance. Those should not be affected by the voluntary extension of coverage, but only by the assistance with claims. We contrast the effects for 'mandatory households' with estimates for households including voluntary members to obtain an estimate of the coverage effect, using differences-in-differences-in-differences techniques.

We find robust evidence for decreased child labor outcomes as a result of the innovation package. There is especially strong evidence for households to rely less on child labor earnings and to reduce hazardous occupations. Effects tend to be larger for boys, which is not surprising as they are most affected by child labor in our sample. When separating the claim assistance effects from the effects of increased coverage of households, the assistance effects are mixed and insignificant, while increased coverage clearly decreases child labor incidence, hours worked and days missed at school. These latter effects are so large that they cannot be explained by shock mitigation only. In fact, even those households never having faced a shock, or experienced an insurance payout, display similar effects. We therefore conclude that insurance coverage leads to higher perceived protection and makes parents more confident to leave children out of work and instead make them visit school more regularly.

The remainder of the article proceeds as follows. In Section II we provide background information, details on the health insurance innovation and possible effect channels, including a formal model on dynamic responses of households to changes in risk via insurance. Section III describes data collection and the resulting panel dataset. We present empirical results in Section IV and conclude in Section V. Supplementary results and descriptions are given in the appendix.

II. The innovation and its background

Pakistan is a poor country: 22.3 percent of the population live below the poverty line of $1.25 per day and another 20.5 percent are classified as vulnerable (World Bank 2012, 19). According to the Pakistan Ministry of Health (2009, 6) public health expenditures are about 0.6% of GDP which is much lower than in comparable countries, and 75 percent of health expenditures are paid by patients out of pocket. The quality of health service providers corresponds to this low level of public health spending. While some private companies and insurers have contracts with hospitals or run their own hospitals (with varying quality), the options for the poor are limited. There are public health facilities that are supposed to be for free, but they often offer poor quality and many elements such as drugs must be paid privately as they are not covered.[97] The Pakistan Ministry of Health (2009, 5–6) describes the situation for low-income households as follows:

"*Poor are not benefiting from the health system whereas they bear major burden of diseases. Expanded infrastructure is poorly located, inadequately equipped and maintained resulting in inadequate coverage and access to essential basic services. Private health sector continues to expand unregulated mainly in urban areas. Factors contributing to inadequate performance of health sector are deep rooted including weak management and governance, partially functional logistics and supply systems; poorly motivated and inadequately compensated staff, lack of adequate supportive supervision, lack of evidence based planning and decision making, low levels of public sector expenditures and its inequitable distribution.*"

Due to the limited capacity and availability of public providers patients in some situations are forced to seek expensive private medical care. This makes health shocks a substantial economic risk for poor households. Consequently, illness and health are ranked as the top priority by potential microinsurance clients when it comes to unpredictable risk events in Pakistan (World Bank 2012, 28). Moreover, in this country with a majority of informal employment contracts there is no universal health insurance system. Instead, several arrangements coexist at a time. Social security (for police officers, soldiers, civil servants,

[97] This information was gathered through multiple country-specific reports (Asian Development Bank 2004; Asian Development Bank 2005; Qamar et al. 2007). They describe the status of the Pakistani health system prior to the innovation that took place in 2009.

etc.) only covers a tiny part of the population.[98] There are various alternative health insurance schemes on the provincial level or offered by a multitude of private insurers; however, they are often packaged with other insurance, restricted to formal sector corporate clients and have no national outreach (World Bank 2012, 11). In any case, only 1.9 percent of households are estimated to use any kind of formal insurance product (World Bank 2012, 21), and the most vulnerable households are generally not the target group. Only microfinance institutions currently provide insurance for the low-income population, but here mainly schemes combining credit with life insurance are prevalent. According to the World Bank (2012, 50), only the NRSP is offering health microinsurance with significant outreach.

NRSP is a Pakistani non-profit organization committed to support poor and vulnerable households all over the country. It is part of the Rural Support Programs Network (RSPN) consisting of 12 rural support programs that are all active in distinct regions of Pakistan. NRSP is the largest of these support programs and serves more than two million households by offering different microfinance services (mainly credit) and client training.[99] Figure 1 shows the geographic activity of NRSP within Pakistan.

Figure 1: Geographic activity of NRSP within Pakistan

Source: http://nrsp.org.pk/area_of_operation.htm

[98] (Asian Development Bank 2005, 2) estimates that "…less than 3% of the total employed labor force" are covered under this formal scheme.
[99] See Rural Support Programs Network (2012) for more detailed information.

Given the need to cover health shocks of poor households, NRSP in 2005 started to bundle health insurance to their microcredit product. Before the start of the research project, the insurance was built into the credit and was mandatory for loan clients, for their spouses and all children of the client below 18 years.[100] The product covers hospital stays of more than 24 hours with a cost ceiling of 15,000 rupees (approximately 175$ US). Covered expenses range from room charges, doctor's visits, drugs, operations, and pregnancy care to transportation costs. Also accidents leading to death or permanent disability are covered up to 15,000 rupees. Costs of hospitalization are reimbursed after contacting the MFI field officer and submitting bills along with other relevant documents. Similarly, claims after death or disability can be submitted to the MFI field officer. NRSP aims at settling all claims within 15 days.[101] However, it seems that not all clients and credit officers were aware of the new product, resulting in very low claim ratios (World Bank 2012, 50; Qamar et al. 2007). In an effort to increase the social impact of its activities, NRSP in 2008 conducted a diagnostic survey in the area of Hyderabad. In this district in the south of Pakistan (Sindh province) an estimated 9.3 percent of all households are organized through NRSP according to Rural Support Programs Network (2012). The survey indicated high prevalence of child labor especially in the hazardous glass bangle industry and still a high vulnerability to health costs, mostly caused by accident, surgery or illness.

Responding to the vulnerability of their clients, NRSP in 2009 introduced two components additional to the mandatory insurance as part of an experiment.[102] In randomly selected treatment branch offices only, additional household members (adult children of the client and other minor or adult household members) were offered a *voluntary* insurance for a premium of 100 rupees (approximately 1$ US) per adult and year.[103] Second, clients were visited monthly and asked whether they had incurred any medical costs and whether they needed assistance with claims. With increased coverage of individuals and easier filing of claims NRSP deliberately targeted child labor through a better protection of poor

[100] The insurance product gradually changed over time. It initially covered loan clients and their spouses and was expanded in 2009 (i.e. before the baseline data used in this paper was collected) to include minor children. Also other details changed, but the basic design is what we describe in the following. For a detailed description of early product characteristics and developments we refer to Qamar et al. (2007).
[101] Appendix E provides a more detailed description of the insurance package and reimbursement practices.
[102] The experimental introduction of the innovation was financially and methodologically supported by the ILO Social Finance Program's "Microfinance for Decent Work (MF4DW)" project.
[103] The average monthly per capita income in our sample is around 3000 rupees (30-35 $US) (see Table 3).

households.[104] These two components were introduced in the treatment, but not in the control branches. The clients in the control branches were not aware of the treatment. Before the modifications of the insurance were introduced, household baseline data was collected in all treatment and control branches at the same time. The geographic distribution of branches is shown in Figure 2.

Figure 2: Location of treatment (dark) and control (bright) branches within Hyderabad, Pakistan

Source: Google Earth with GPS coordinates of branch locations.

The sample consists of all clients of the 13 test branch offices whose credit appraisals were conducted in September/October 2009. Thus, the complete client cohort of two months and their households are included in the study: 777 households in four control and 1320 in nine treatment branches. (The exact numbers per branch are given in Appendix D.) All households were interviewed prior to the innovation. This baseline survey took place in September and October 2009. Four follow-up surveys were then conducted every six months: March/April 2010, October/November 2010, May/June 2011, October/November 2011. The attrition rate shown in Table 1 is between 0.4% and 3.8% for each wave, and similar in treatment versus control branches. In the follow-up surveys after 12, 18 and 24 months there are a few households 'dropping back in'. Differences in attrition in the final follow up after 24 months are not significant (4.8% in control versus 6.4% in treatment branches). There is also no evidence for differential non-response: a two-sample proportion test of the hypothesis that the fraction of households answering all survey waves are the same (92.5% in control versus

[104] Consequently, questions about child labor and schooling formed the core of the household questionnaire. The other sections of the questionnaire were very short to avoid annoying clients with long and repetitive surveys and to reduce administrative effort.

91.4% in treatment branches) was not rejected. Within household compositions likewise are unaffected by the innovation: We checked for treatment effects on household size, number of adults and number of children in different age categories. None of the regressions resulted in any significant differences.

Table 1: Attrition across waves, control versus treatment branches

	All	Control branches			Treatment branches		
	Households	Households	Drop-outs	Drop-ins	Households	Drop-outs	Drop-ins
Baseline	2097	777	-	-	1320	-	-
Follow up after							
6 months	2083	774	3 (0.4%)	-	1309	11 (0.8%)	-
12 months	2051	755	19 (2.5%)	0	1296	15 (1.2%)	2 (18.2%)
18 months	2003	745	18 (2.4%)	8 (36.4%)	1258	42 (3.2%)	4 (16.7%)
24 months	1975	740	19 (2.6%)	14 (43.8%)	1235	48 (3.8%)	25 (40.3%)

Note: Percentages in brackets indicate the fraction of the previous wave's observations (drop-outs) or of the previous wave's missings (drop-ins), respectively.

As stated above, the innovation consisted of two elements. We will make use of the family structure information to disentangle the effects of these two components: Families without additional household members (i.e. without *adult* children or other adult dependents) could not extend their coverage and were affected only by the technical assistance. Since the family structure information is also available for the control villages, we can use a type of triple difference approach to control for direct effects of the family structure.

– Uptake of the innovation –

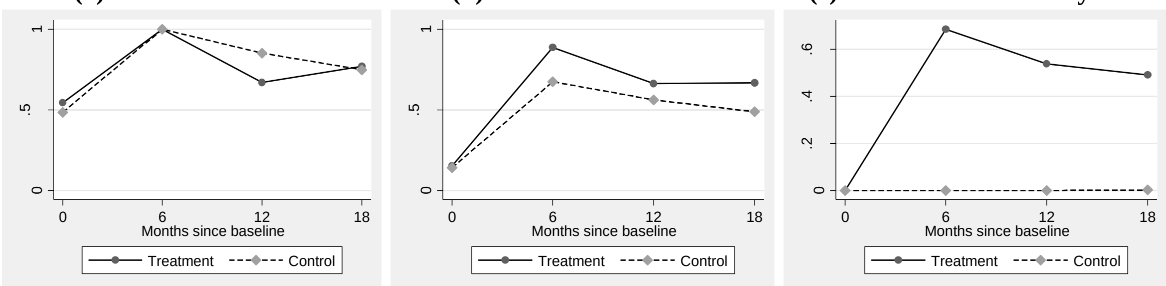
In the following, we will describe coverage rates and uptake decisions of the innovation. Note that the sample (further described in Section III) consists of all clients of the 13 branch offices in Hyderabad whose credit appraisals are conducted in September/October 2009. They take up their loans after the baseline is conducted. Figure 3(a) shows self-assessed insurance coverage of clients across waves, where each client represents a household. Note that there are many clients who had NRSP loans already before which explains the positive coverage at baseline. We further see that there is also a positive coverage at baseline (about 50%) in the control group. Note that the control group has insurance coverage of the nuclear family but does *not* receive the two innovation components, i.e. the help with claims and the extended coverage of other household members.

After the baseline all clients have a loan and 100% of clients are covered by mandatory insurance. Afterwards, coverage rates decrease due to clients repaying their loans. (After loan repayment insurance cannot be extended unless a new loan is taken.) The coverage rates are very similar in treatment and control branches, except for 12 months after the baseline. (The difference in month 12 is, in fact, only driven by a single control branch. Without that control branch, there would be virtually no differences.)[105]

While we see little difference between treated and control group in Figure 3(a) with respect to the insurance status of the client (usually the household head or the spouse), larger differences are visible in Figure 3(b) with respect to the *number* of individuals in a household insured: Only the households in the treatment branches had the option to voluntarily insure those additional household members who were not mandatorily insured. Figure 3(b) thus shows insurance coverage rates at the individual level. Take-up is substantially higher in treatment than in control branches from 6 months until 18 months after the baseline. This is the result of considerable voluntary take up in the treatment branches, which is examined in Figure 3(c). There we show the number of household members who are voluntarily insured. Around 70% of those without mandatory insurance are covered in the treatment areas. The figure gradually decreases to about 50% at 18 months. In the control branches, these figures are zero since voluntary insurance of additional household members was not offered there.[106]

Figure 3: Insurance coverage in treatment and control branches

(a) % of clients/households **(b) % of individuals** **(c) % of not mandatorily insured**



Note: Insurance coverage is self-assessed (cross-checked with MFI's information system in follow-up surveys)

The (self-assessed) take-up rates for all household members illustrated in Figure 3(b) are also shown in Table 2. Besides comparing coverage in treatment versus control branches (column 1), we also separate individuals into two categories: those with mandatory insurance (client, spouse or child<18) and those eligible for voluntary insurance (non-nuclear family or

105 There is one of the four control branches with 100% clients having a loan and consequently insurance. Without this branch, rates would be very similar at 12 months as well.

106 Note that due to data problems the insurance coverage information is not available for month 24. In the last survey wave at month 24, insurance coverage was no longer cross-checked with the register data and reliable information on individual insurance coverage is missing.

child≥18). Since information on family structure was collected also in the control villages, we can define these potentially eligible groups in the same way in treatment and control villages. (I.e. in Table 2 we define the group "eligible for voluntary insurance" in the control branches as those individuals who would have had access to voluntary insurance if they had lived in a treatment area.)

At baseline not all clients were yet aware of the coverage, so there the variable was measured with error. Nevertheless, the baseline values are very similar between treatment and control branches. After the baseline nearly all individuals with mandatory insurance (1a) also report coverage. Consistent with the randomized control design, additional voluntary insurance (1b) is taken up only in treatment branches. While virtually none in the control areas are voluntarily insured (as they had not been offered this option), nearly 70% in the treatment areas are voluntarily insured after 6 months. This number declines to about 50% until month 18, partly also because of early repayment of loans which makes them no longer eligible.

Table 2: Insurance take-up (percent of household members), by control vs. treatment branches

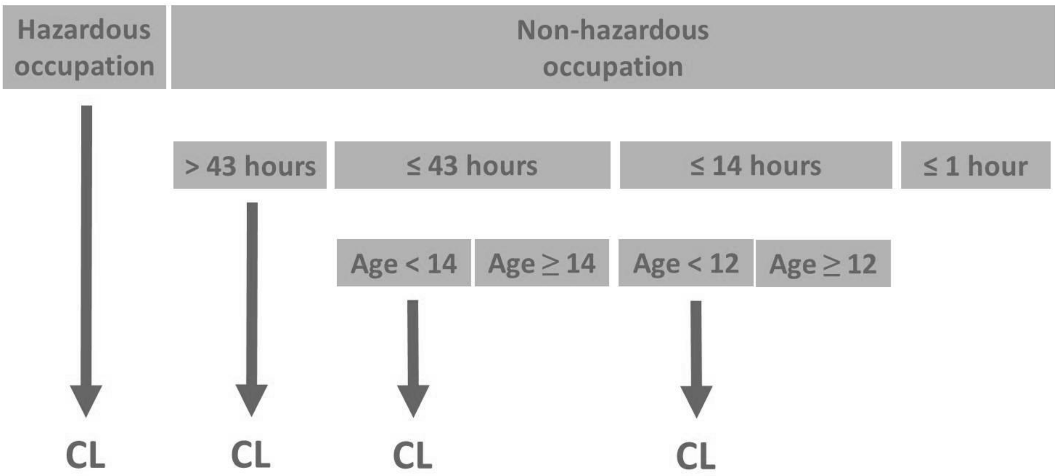
	(1) All household members		(1a) Mandatory insurance*		(1b) Eligible for voluntary insurance**	
	Control	Treated	Control	Treated	Control	Treated
Baseline [a]	14.1%	15.3%	20.6%	22.3%	0.0%	0.0%
N	4,742	8,182	3,250	5,594	1,492	2,588
6 months	67.5%	88.6%	99.2%	99.4%	0.0%	68.5%
N	4,781	8,051	3,252	5,238	1,529	2,813
12 months	56.2%	66.3%	84.5%	73.1%	0.0%	53.9%
N	4,666	7,926	3,105	5,125	1,561	2,801
18 months	48.8%	66.7%	74.3%	77.4%	0.2%	49.0%
N	4,592	7,809	3,014	4,877	1,578	2,932

Notes:
* Client/spouse/child<18 (nuclear family),
** Non-nuclear family or child ≥18,
[a] At baseline individuals were not always fully aware of their coverage.

– Definition of child labor –

In our empirical analysis we measure child labor in various ways. Our main specification follows the ILO definition of child labor, but our main empirical results are robust to alternative definitions. The definition of child labor is sketched in Figure 4. It is mainly based on the ILO Convention C138 from 1973.[107] According to the convention, child labor occurs if different conditions are met. First, all children working in hazardous occupations are automatically classified as child laborers. In our case these are mainly jobs in the dangerous production of glass bangles. But also welding and mechanics work belong to the hazardous occupations. If the occupation is in a non-hazardous occupation, child labor depends on age and hours worked. Young children below 12 years who work more than one hour per week, children between 12 and 13 who work more than 14 hours per week and juveniles between 14 and 17 who work more than 43 hours are defined as child laborers. Note that in our sample only children who are 5 years or older are considered as potential child laborers.

Figure 4: Child labor definition related to ILO Convention C138 (1973)



Note: CL = classified as child labor, hours are per week.

Of course this definition is arbitrary to some extent. Especially the age categories are important for classification as child labor. A slight transition from age 11 to 12 or from 13 to 14 might change child labor status from one to zero despite increasing hours. This is why we will control for the above age categories of children in our regressions (and why we examine various alternative definitions of child labor).

– Possible effect channels –

Health shocks may affect child labor through various channels. In the absence of insurance, health shocks will lead to health expenditures and lost labor due to sickness. This income risk affects child labor in various ways via ex-ante and ex-post effects. If a health shock happens,

[107] Edmonds (2008) describes the convention in detail in his terminology section.

additional health expenses are incurred, which can push families towards poverty. There are several papers modeling economic aspects of child labor (e.g. Basu and Van 1998; Baland and Robinson 2000; Ranjan 2001) and all of them relate poverty to more child labor. Hence shocks should increase child labor, especially if households in need lack alternative coping mechanisms. This phenomenon is also observed empirically (Beegle, Dehejia, and Gatti 2006). Yet, there are also ex-ante effects of risk and insurance, i.e. before a shock actually happens. To the best of our knowledge only Pouliot (2006) and similarly Estevan and Baland (2007) model household risk and its consequences on child labor. However, as all previously mentioned theoretical papers, they exclusively focus on childhood condensed into a single period and the effect it creates for incomes once children become adults. Education (abstaining from child labor) for those authors is an investment that generates future budget according to a human capital production function. The efficient decision is the child labor choice that maximizes budget over all periods. Budget shocks, for example on expenditure or parental income, should not affect child labor decisions in this setting. Consequently, Pouliot (2006) and Estevan and Baland (2007) focus on shocks affecting productivity of human capital, as those should be related to child labor.

In contrast, we are interested in within-childhood dynamics and effects of potential budget shocks such as health costs. In the absence of an adequate benchmark model, we develop a simple model, which is formally described in Appendix C. Here we discuss the basic setup and the main results. We consider two periods within childhood. In both periods, parents take a decision about the amount of child labor. They take negative consequences of child labor (such as the trade-off with school attendance and learning) into account by attaching a disutility to it. In that sense child leisure is a type of consumption for parents caring about their offspring for whatever reason. On the other hand, they profit from the income generated by child labor through consumption in each period. We use a log-shaped utility function which implies that the household is risk-averse. Households maximize expected total utility over the two periods using a discount factor. Our utility function allows for probability weighting, for example the overweighting of catastrophic health shocks. Household income is generated by adults that supply labor inelastically and potential child labor. In each period there is the risk of a health shock and the shock is observed prior to child labor and consumption choices. Households will react to shocks by increasing child labor. There are also inter-temporal considerations. The higher the probability or the costs of a shock, the higher is the incentive in period one to create additional resources via child labor as a precautionary buffer stock for period two.

The effect of introducing insurance is modeled via reducing the costs of a shock. In addition, the insurance premium to be paid reduces income. It is straightforward that insurance will decrease child labor in case of a shock. However, there will be an ex-ante effect as well. A fair insurance for period two in our model clearly decreases child labor already in period one. This is because households need to worry less about creating precautionary savings. The effect becomes more pronounced if households are poorer and if they overweight the shock probability.

There is an even stronger case of an ex-ante effect, though. If households buy insurance and have no shock they will have to pay premiums without profiting from the scheme. In our data this would be households that buy insurance, but have no shock or payout. In model terms, they buy insurance for both periods, but observe no shock in the first period. The result of the model is that if households are sufficiently poor or overweight shock probabilities enough, they will still reduce child labor in period one relative to the no-insurance case. This is because the effect of uncertainty in period two would be so large that the positive insurance effect outweighs the negative budget effect.

An additional aspect outside the model is that child labor can be seen as a tool to diversify labor market risk in a volatile economic environment. This idea relates to the literature on portfolio choices with different degree of risk exposure. In the area of agriculture high consumption risk seems to deter poor farmers from investing into more profitable but risky activities (Rosenzweig and Binswanger 1993; Dercon and Christiaensen 2011). In our case, if households are afraid of health events they might reduce risk in other domains, for example by diversifying labor market activities (including using child labor). Insurance creates a more secure environment in which households feel more comfortable to abstain from sending their children to work.

III. The data

Household data was collected by MFI credit officers who received special training on the questionnaire. The advantage of this approach is that the field staff knows households very well and that this way of data collection is very cost-effective. The disadvantage is that there might be stronger interviewer effects, for example due to respondents giving more 'socially desired' answers. Yet, any systematic bias should show up in both treatment and control branches, and should not affect our treatment effect estimates. Additional variation created by interviewers on the branch level will be accounted for in our regressions.

In Table 3 we provide information on the distribution of household characteristics across treatment and control branches before the innovation was introduced. These comparisons comprise all covariates that are also used as control variables in the regression analysis. Additional household level variables are presented in Table 4. Table 5 describes the most important child level characteristics between treatment and control branches. These comparisons help to justify whether treatment and control branches are indeed comparable. We use a test for equality of means that accounts for the branch as the level of randomization.[108]

Table 3: Descriptive statistics of households at baseline, by control vs. treatment branch

	Control branches		Treatment branches		All	
	mean	std	mean	std	min	max
Poverty score at baseline (PPI)	31	9.0	**32***	9.8	8	79
Spouse in household? (yes/no)	0.8	0.4	0.8	0.4	0	1
No. of Children age 0-4	0.6	0.9	0.6	0.9	0	5
No. of Children age 5-13	1.5	1.4	1.5	1.4	0	7
No. of Children age 14-17	0.8	0.9	0.9	1.0	0	4
No. of Adults	3.6	1.7	3.5	1.7	1	12
Mean parental age	43	10	43	10	18	71
Mean parental education (years)	3.1	3.7	**3.9****	4.3	0	16
Monthly income per capita [a]	3217	1150	3140	1300	190	16154
Observations	762		1293		2055	

Stars indicate significance of difference between control and treatment branches (*** p<0.01, ** p<0.05, * p<0.1), test is accounting for random effects on branch level (implemented as random-effects regression on treatment dummy),

[a] adjusted for minor household members (factor 0.6) and excluding income from child labor, income in Pakistani rupees (1000 Rs = approx. 11$ US).

[108] Randomizing at the branch level is different from individual randomization. One expects larger differences to remain with bigger randomization units and the test accounts for this by allowing for branch level random effects.

Households in treatment branches at baseline seem to have slightly higher values in the Progress out of Poverty Index (mean PPI score 32 vs. 31) and are also significantly better educated (mean of 3.9 vs. 3.1 years of parental education), but in all other aspects there are no significant differences between treatment and control branches. In both groups around 80% of clients have a spouse in the household. The households have on average three minor and three to four adult members (mean household size 6.5). Parents (client and spouse) are on average 43 years old and households have a mean monthly per capita income of around 3200 rupees (approx. 35$ US). Compared to the Pakistani average client, households seem to be fairly poor: According to World Bank (2012), the poverty rate for Pakistan was at 22.3 percent between 2010 – 2011. Even when including income from child labor, 51 percent of households (or 59 percent of individuals) in the sample report a per capita income below this poverty line (3100 rupees monthly or 1.25 $ / day). There might be some measurement error in income, but the data nevertheless indicates that NRSP is successful in targeting low-income households.

Table 4: Additional descriptive statistics of households at baseline, by control vs. treatment branch

	Control branches		Treatment branches		All	
	mean	std	mean	std	min	max
Monthly expenses: Total	13963	4725	12705	4714	3657	40780
Monthly expenses: Children	292	381	302	422	0	3500
Monthly expenses: Books	259	411	200	306	0	3000
Monthly expenses: Outpatient	501	559	380	435	0	4500
Monthly expenses: Hospital	107	478	59	365	0	10000
Credit with NRSP before? (yes/no)	0.73	0.45	0.77	0.42	0	1
Credit amount	16133	4387	15723	4916	5000	30000
Difficulties repaying loan? (yes/no)	.01	.11	.02	.15	0	1
Observations	772		1320		2092	

Stars indicate significance of difference between control and treatment branches (*** p<0.01, ** p<0.05, * p<0.1), test is accounting for random effects on branch level (implemented as random-effects regression on treatment dummy), monetary units in Pakistani rupees (1000 Rs = approx. 11$ US).

Table 4 shows average household expenditure for several categories and some credit related characteristics. Children-specific expenditures (e.g. school fees) and books together account for four percent of total expenses. Outpatient plus hospital costs make up for a similar amount. Three quarters of clients already have experience with NRSP. They take up average loans in the order of 15,000 rupees at baseline and only very few have difficulties repaying their loan. While there is some variation in numbers across treatment and control branches, none of the differences are significant.

The same is true for characteristics at the child level shown in Table 5. Average age, education level, child labor incidence (around 20%), earnings through child labor (290-340 rupees), hours worked per week (11-14 hours), hazardous occupations (9%) and school attendance (68-70%) are all similar for treated and control and none of the differences are significant. Note that the income generated through child labor corresponds to roughly ten percent of monthly per capita income which is a non-negligible amount.

Table 5: Descriptive statistics of children at baseline, by control vs. treatment branch

	Control branches		Treatment branches		All	
Children 5 – 17	mean	std	mean	std	min	max
Age	11	3.7	12	3.8	5	17
Education (years)	3.2	3.3	3.4	3.3	0	14
Child labor? (yes/no)	0.20	0.40	0.19	0.39	0	1
Monthly child labor earnings [a]	290	1038	342	1092	0	15000
Hours of work (weekly)	11	18	14	21	0	90
Hazardous occupation? (yes/no)	0.09	0.29	0.09	0.28	0	1
Observations	1650		2971		4621	
Children 5 – 14						
School attendance	0.68	0.47	0.70	0.46	0	1
Observations	1215		2065		3280	
Children 5 – 14						
Monthly school days missed	0.86	3.1	1.2	3.0	0	30
Observations	583		1442			

Stars indicate significance of difference between control and treatment branches (*** p<0.01, ** p<0.05, * p<0.1), test is accounting for random effects on branch level (implemented as random-effects regression on treatment dummy), income in Pakistani rupees (1000 Rs = approx. 11$ US),

[a] only earnings that are generated by work classified as child labor.

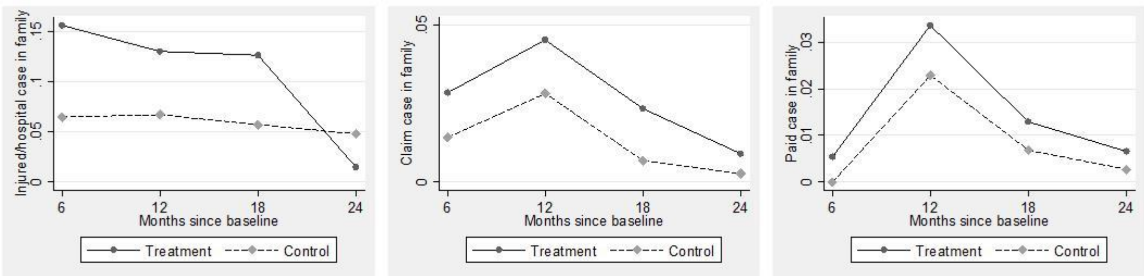
IV. Empirical results

The econometric methodology used is based on the cluster experimental design of the innovation. As we have seen in the previous section, treatment and control households are mostly balanced in baseline characteristics. We therefore first compare treatment to control households. Thereafter we control for covariates in order to increase precision.

In the following figure we compare the health expenses claiming behavior between treatment and control branches. One important potential effect channel for the treatment is a better protection of households in case of a shock. Insured individuals can get their hospital costs reimbursed after submitting a claim that should be settled within 15 days, as explained in Section II. In line with higher insurance coverage we would therefore expect more claims and ultimately more reimbursement for households in treatment branches. Figure 5(a) shows the percentage of households reporting an injury or hospitalization case since the last loan disbursement. Overall, a much higher percentage declares injury or hospitalization in the treatment branches. While around 6% of control households report a medical case it is two to three times as often the case for treated households, except in the final survey wave. Also the claim frequency (Figure 5(b)) is consistently about twice as high in treatment areas. Similarly, insurance payments are more frequent (Figure 5(c)).[109] Unfortunately, we do not have baseline data for injury and hospitalization, but Figure 5(a) to 5(c) are consistent with more individuals attending hospital in case of sickness if they are insured and thus do not bear the full costs of medical treatment. The higher frequencies are likely also influenced by the sensitivity and help offered by the credit officers in the treatment branches.

Figure 5: Medical incidence, insurance claim and payment (% of households), control vs. treatment

(a) Injured or hospitalized (b) Filed an insurance claim (c) Insurance paid medical costs



Note: There is no information available on insurance related events at baseline.

Panel (a) shows whether someone was injured or hospitalized, panel (b) shows whether an insurance claim was filed, whereas panel (c) shows whether the insurance claim was actually approved, i.e. whether medical expenses were reimbursed.

[109] Simple two-sample proportion tests show significant differences 6 months after baseline in all three variables. However, some events are extremely rare and we should be careful in interpreting the differences. While 274 injuries or hospitalizations are reported, only 48 submitted claims and 7 claim payments can be found at t = 6 months.

For child labor and schooling we also have baseline data. Hence for these outcome variables, the data available allows for the comparison of treatment and control branches before and after the innovation took place. For being able to interpret these as causal effects we need two assumptions: First, there should be no spillovers from the treatment into the control branches. The geographic proximity of the branches (all branches are within the urban area of Hyderabad) on the one hand increases comparability, but on the other hand facilitates spillover effects. A classical spillover effect would be the provision of additional insurance in control branches. This can fortunately be ruled out due to the rigorous control by the MFI (see Figure 3(c)). Second, outcomes in treatment and control group must follow a common trend. While this is true in expectation because of the random assignment of the treatment, the rather small number of branches entails the risk of small-sample differences in branch specific time trends. While we have little information about local branch-specific events such as changes in local economic conditions, we have relatively detailed information on the economic situation of the household and its members. We use this information to control for branch-specific trends that work through these variables.

In the following we use various panel data models where we incorporate covariates to improve precision of the estimates. As we have seen in Tables 3 to 5, the baseline characteristics of treatment and control branches are mostly balanced, yet incorporating them in the regression may help to reduce variance. We basically follow a type of difference-in-difference estimator, comparing outcomes before and after for treatment and control villages. As we have several follow up periods, we run the estimation for all periods simultaneously, while permitting that impacts may freely vary over time. (If we had only a single follow up period, our estimator would correspond to differences-in-differences.) Via random effects we account for correlation within village-wave clusters.

We estimate the econometric model:

$$Y_{ibt} = \beta_0 + T_b\lambda_0 + \delta_t + T_b\gamma_t + \eta_{bt} + \varepsilon_{ibt} \tag{1}$$

Y_{ibt} is the outcome of interest and possibly varies over individuals *i*, branches *b* and time $t \in \{0,6,12,18,24\}$. β_0 is the intercept and T_b is a binary indicator whether the individual is located in a treatment or a control branch. λ_0 seizes baseline differences between treatment and control branches and δ_t measures common time trends, while γ_t captures the time-specific treatment effect (γ_0 is restricted to zero). Besides the classical ε_{ibt} error term, which is assumed to be i.i.d., we allow for an unobserved branch-time specific error component η_{bt}, which is assumed to be uncorrelated with the treatment variable (random effect). η_{bt} is incorporated to capture within branch-wave correlation of individual outcomes. In model (2) we also include a vector of covariates X_{ibt} that may vary over individuals, branches and time:

$$Y_{ibt} = X_{ibt}\beta + T_b\lambda_0 + \delta_t + T_b\gamma_t + \eta_{bt} + \varepsilon_{ibt} \tag{2}$$

The regression results shown later always comprise different specifications. We first show treatment effects for each wave without controlling for covariates using random effects at the branch-wave level (η_{bt}) in specification (1). In most cases the precision of estimates is considerably lower with random effects, showing that those unobserved effects seem to be quite important. We also examined specifications with branch specific random effects only instead of branch-wave random effects. We obtained very similar and slightly more precise results. Here we only report the results for the more flexible specification with branch-wave random effects, i.e. where the branch effects are permitted to vary arbitrarily over time.

In specification (2) we control for household and individual covariates: poverty level at baseline, current monthly income per capita (excluding child labor earnings), health shocks, non-health economic shocks, death of family members as well as individual demographics. Note that non-child labor income could be endogenous. However, we did not find any independent treatment effect on income. Furthermore, we also examine an alternative specification in Table A2 in the appendix where we do not include income as a regressor, and obtain very similar results.

The treatment effect estimates have to be interpreted as the combined effect of technical assistance with claims (i.e. the monthly visits of credit officers assisting with claim procedures) and the *offer* of additional insurance coverage. Hence, we compare households who have been *offered* additional coverage with those who did not have this option. (I.e. we do not directly compare households who have or have not purchased extended insurance, but according to the binary treatment status of their location of residence.)

The following Table 6 shows treatment effects on child labor related variables: child labor incidence, monthly income generated through child labor, hours worked by children and work in hazardous occupations. Note that the child labor definition depends on hours worked and hazardous occupation and thus any effect on those two variables should also propagate through child labor incidence and earnings. In specification (1), i.e. without covariates, all treatment effects are negative. Effects are closer to zero 18 months after baseline, but substantial in all other waves. Despite the size of the effects, the coefficients are only statistically significant for hazardous work and monthly child labor earnings. Including control variables in specification (2) does not change coefficients much, but standard errors decrease by around one third. The individual and household covariates thus effectively reduce unexplained variation without any signs of selective treatment assignment. The relatively largest effect of the innovation is on the prevalence of hazardous occupations and income generated by child labor.[110] Hazardous work and child labor income clearly decrease. The effects on the other child labor indicators are also negative, but less precisely estimated. Furthermore, effects tend to be bigger for boys than for girls.

In Table 6 we only report the coefficient on the treatment indicator. The coefficients on the other control variables are given in Table A1 in the appendix. The results of Table A1 confirm the basic intuition that economic shocks and poverty drive children into child labor and out of school.

[110] See Table 5 (child level characteristics at baseline) to obtain an impression for relative effect sizes. Figure A1 in the appendix shows time trends in treatment versus control branches for all variables. These reflect the results of specification (1) and provide a visual impression of effect sizes.

Table 6: Treatment effects on child labor indicators

	(1)	(2)	Specification (2) for	
			Boys	Girls
Standard Errors	RE	RE	RE	RE
Controls	NO	YES	YES	YES
	Child labor (age 5-17) – binary definition according to ILO convention			
Treatment Effect 6 months	-0.039	-0.023	-0.067	0.019
	(0.057)	(0.041)	(0.044)	(0.061)
Treatment Effect 12 months	-0.069	**-0.067***	**-0.098****	-0.039
	(0.056)	(0.040)	(0.043)	(0.060)
Treatment Effect 18 months	-0.0040	0.015	-0.037	0.062
	(0.056)	(0.040)	(0.043)	(0.060)
Treatment Effect 24 months	-0.025	-0.029	**-0.10****	0.038
	(0.056)	(0.040)	(0.043)	(0.060)
N	20531	20491	10432	10059
	Hours worked by children (age 5-17)			
Treatment Effect 6 months	-2.42	-1.28	**-3.61***	0.99
	(2.32)	(1.76)	(2.04)	(2.49)
Treatment Effect 12 months	-3.09	-2.78	**-4.91****	-0.67
	(2.27)	(1.72)	(2.00)	(2.43)
Treatment Effect 18 months	-0.33	0.67	-1.78	2.97
	(2.27)	(1.72)	(2.00)	(2.43)
Treatment Effect 24 months	-1.75	-1.53	**-5.32*****	2.06
	(2.27)	(1.72)	(2.00)	(2.44)
N	20527	20487	10431	10056
	Hazardous occupation (age 5-17)			
Treatment Effect 6 months	**-0.057***	**-0.053****	-0.036	**-0.065***
	(0.034)	(0.022)	(0.023)	(0.034)
Treatment Effect 12 months	**-0.062***	**-0.061*****	**-0.061*****	**-0.058***
	(0.034)	(0.021)	(0.023)	(0.034)
Treatment Effect 18 months	-0.030	-0.024	-0.018	-0.030
	(0.034)	(0.021)	(0.023)	(0.034)
Treatment Effect 24 months	-0.036	-0.033	**-0.046****	-0.021
	(0.034)	(0.021)	(0.023)	(0.034)
N	21216	20491	10432	10059
	Monthly earnings through child labor (age 5-17)			
Treatment Effect 6 months	**-216****	**-162***	-231	**-107***
	(107)	(95.9)	(145)	(56.1)
Treatment Effect 12 months	**-195***	**-190****	**-272***	**-110****
	(105)	(93.8)	(142)	(54.9)
Treatment Effect 18 months	-5.66	44.9	87.0	-8.78
	(105)	(93.9)	(142)	(55.0)
Treatment Effect 24 months	-172	**-191****	**-377*****	-21.0
	(105)	(94.0)	(142)	(55.2)
N	20531	20491	10432	10059

*** p<0.01, ** p<0.05, * p<0.1, standard errors in brackets, random effects at the branch-wave level (RE).

The treatment effects on school attendance and school days missed per month are provided in Table 7. We find a modest but insignificant positive effect on school attendance, while the number of missed school days per month decreases (again not statistically significant).

Table 7: Treatment effects on schooling variables

	(1)	(2)	Specification (2) for	
			Boys	Girls
Standard Errors	RE	RE	RE	RE
Controls	NO	YES	YES	YES
	School attendance of children (age 5-14)			
Treatment Effect 6 months	0.0027	0.014	0.035	-0.0084
	(0.064)	(0.029)	(0.038)	(0.048)
Treatment Effect 12 months	0.025	0.023	0.037	0.0095
	(0.064)	(0.028)	(0.037)	(0.047)
Treatment Effect 18 months	0.013	0.0068	0.019	-0.0092
	(0.064)	(0.028)	(0.037)	(0.047)
Treatment Effect 24 months	0.014	0.026	0.025	0.027
	(0.064)	(0.029)	(0.037)	(0.047)
N	15444	14930	7717	7213
	Monthly school days missed by school children (age 5-14)			
Treatment Effect 6 months	0.19	-0.044	0.033	-0.12
	(0.56)	(0.41)	(0.49)	(0.46)
Treatment Effect 12 months	-0.12	-0.14	0.020	-0.30
	(0.56)	(0.40)	(0.48)	(0.45)
Treatment Effect 18 months	-0.29	-0.27	-0.29	-0.25
	(0.56)	(0.40)	(0.48)	(0.45)
Treatment Effect 24 months	-0.15	-0.18	-0.12	-0.25
	(0.56)	(0.41)	(0.48)	(0.45)
N	10,545	10,212	5,149	5,063

*** p<0.01, ** p<0.05, * p<0.1, standard errors in brackets, random effects at the branch-wave level (RE).

– Disentangling the treatment effects –

As explained in Section II, the innovation in the treatment branches consisted of two elements: (i) assistance with filing claims (i.e. frequent visits of credit officers to assist with claim procedures) and (ii) the option to voluntarily insure other household members (extended insurance coverage). In the following we will disentangle the effects of each element. We exploit a special feature of the innovation that creates an additional control group within the treatment group, allowing a type of triple difference approach. Remember that the so-called nuclear family (client, spouse and minor children) is covered by mandatory insurance. Therefore extending (voluntary) insurance coverage should have no effect on households consisting only of mandatorily insured individuals. We estimate the treatment effect for those households with potential voluntary members *relative to those with mandatory members only*. The latter hence acts as an internal control group and should capture either effects of monthly visits or deviations from the common time trend assumption. (Since we know the household structure also for the control branches, we can define these groups in all branches.) Thereby we can isolate the effect of extended coverage. We implement our strategy extending the original model as follows:

$$Y_{ibtv} = X_{ibt}\beta_v + T_b\lambda_{0v} + \delta_{tv} + T_b\gamma_t + V_iT_b\lambda_t + \eta_{bt} + \varepsilon_{ibt} \quad (3)$$

where $V_i \in \{0,1\}$ indicates whether the household contains "non-nuclear" members. In other words, the indicator shows whether the household includes other individuals besides client, spouse or minor children. In equation (3) we permit all coefficients to vary by 'household type' v (only mandatory members vs. potential voluntary members) which incorporates the flexibility of two separate regressions, but allows a direct test of the extended coverage effect ($H_0: \lambda_t = 0$).

Table 8 shows treatment effects for 'mandatory' household types and additional effects for 'voluntary' types. The upper half of Table 8 shows the estimated treatment effects for those households who have only nuclear-family members. I.e. these households could not extend their coverage and the estimated treatment effects thus can only reflect the effects of the assistance with filing claims (via the frequent visits of the credit officers). The lower half of Table 8, on the other hand, shows the estimate on the interaction term, i.e. for those households with at least one member that did not belong to the nuclear family and thus had the option to insure voluntarily. The estimates in the lower half of the table thus show the isolated effect of the extended coverage only. (The total effect on households with voluntary members is thus the sum of the estimates in the upper and the lower half of the table.)

In the upper half of the table most effects are insignificant. Only two out of 24 coefficients are significant at the 5% level and only at a single point in time. The other estimates are not even significant at the 10% level. On the other hand, we find many significant effects in the lower half, which all have the expected sign. We find large effects on child labor and hours worked. Effects on earnings through child labor and the incidence of hazardous occupations are significant only in the last wave, though. We also find a reduction of monthly school days missed through offering higher insurance coverage.

We examine an alternative specification in Table A3 in the appendix, where we do not include income as a regressor. The results on the coverage effect remain stable. Furthermore, the results are robust to alternative definitions of child labor. Table A4 in the appendix shows results for different definitions of child labor. The results for extending insurance coverage to additional household members remain very large and highly significant. In addition, we now also find several significant effects for the "nuclear family" households, i.e. for the assistance with filing claims, but their size varies substantially over time. Also, remember that – different to the lower half of the table – the upper part on claim assistance might capture deviations from the common time trend assumption such that we do not want to draw strong conclusions from those estimates.

Table 8: Disentangling treatment effects on children's outcomes

	Child labor	Hours worked	Child labor earnings	Hazardous occupation	School attendance	Days missed
TE 6 months – mandatory	0.046	1.69	-58.2	-0.029	-0.011	0.61
TE 12 months – mandatory	0.019	1.02	-59.5	-0.030	0.012	0.49
TE 18 months – mandatory	**0.10****	**3.24****	132	-0.0063	-0.014	0.62
TE 24 months – mandatory	0.069	1.57	-52.9	0.0066	0.025	0.64
ΔTE 6 months – voluntary	**-0.088****	**-3.87****	-137	-0.029	0.035	**-0.95*****
ΔTE 12 months – voluntary	**-0.12*****	**-5.13*****	-170	-0.043	0.019	**-0.93*****
ΔTE 18 months – voluntary	**-0.12*****	**-3.33****	-112	-0.024	0.030	**-1.33*****
ΔTE 24 months – voluntary	**-0.13*****	**-4.27*****	**-188***	**-0.057****	0.0049	**-1.24*****
Controls	YES	YES	YES	YES	YES	YES
N	20,491	20,487	20,491	20,491	14,930	10,212

*** p<0.01, ** p<0.05, * p<0.1, standard errors including random effects at the branch-wave level, upper part of table shows treatment effects for households with mandatory members only (no insurance coverage effect), lower part for households with at least one potential voluntary member (coverage effect only).

To interpret our findings, we need to keep in mind that our estimated effects for the coverage extension are *intention to treat effects* (ITT). I.e. in contrast to the assistance to claims, which is provided to everyone in the treatment group, the isolated effects for coverage extension capture the *option* to voluntarily buy further insurance. According to Table 8, this option decreases child labor by 8.8% after 6 months on average. Note that the percentage of child laborers 6 months after baseline in treatment branches in 'voluntary' households is 21.9%, thus without the innovation it would have been 30.7%. These effects are substantial and could arise through various channels. We first need to discuss how many household members actually receive additional insurance cover. In the aforementioned subsample, 86% of children see an additional household member being insured in the treatment branches. Almost half of them now live in households with *complete* coverage. This means that offering increased coverage indeed led to substantially higher coverage. On the other hand, the nuclear family is insured anyway and the additional coverage affects only part of the household. Also, claim and payment experience is limited, see Figure 4. These considerations make it rather unlikely that actual payout in case of a shock saves children from being pushed into child labor. Instead *ex-ante* effects, such as psychological factors and expectations of parents, appear to be very important. Families might feel more protected from costs arising from a health shock to one household member and thus might rely less on their children as a source of precautionary income. This argument for ex-ante effects would be in line with our theoretical results derived in Appendix C. If vulnerability of a household already increases child labor, even without a shock being present, covering the household *completely* should indeed have large effects as opposed to covering only part of it, as the risk of catastrophic health spending is only eliminated with complete insurance coverage. Also, according to our model the effects of introducing insurance should be strongest for poor households. Given that more than half of all households report an income below the poverty line at baseline, we would expect such ex-ante effects in our sample.

– Ex-post versus ex-ante effects –

In Table 9 we formally assess whether most of the effect indeed exists even *without* the insurance paying out a claim following a shock (ex-ante effect). The idea is as follows. Some households are randomly hit by a health shock whereas others are not. Assuming that health shocks are largely exogenous (i.e. that the presence of insurance does not change the health-risk taking behavior of individuals very much), the former households would be affected by the ex-ante and the ex-post effects, whereas the latter would only be affected by the ex-ante effect. We thus repeat the previous analyses separately for households hit by a health shock and those not hit by a shock. Unfortunately, the sample size is too small for a meaningful analysis for households with a shock, such that we can only examine the subsample without a shock. We thus compare the estimates for this subsample without a health shock (i.e. without ex-post effects) to the results for the total population of Table 8. If the effects found in Table 8 were mostly due to ex-post effects, the treatment effect estimates should become much smaller or vanish if we only examine households without shock in the following table. On the other hand, if ex-ante effects were important, the results in the following table should be similar to those of Table 8.

Table 9 shows the regression results in the subsample without the approximately 5% of households that received a claim payment at some point in time. The coefficients for “voluntary” households (i.e. the pure coverage effect in the lower half of the table) have about the same magnitude and significance as in the full sample in Table 8. Hence, focusing only on households without shocks and thereby “blocking” the channel through claim payments does not substantially change the estimates.

In Table A5 in the appendix we consider an alternative definition of a health shock to examine the robustness of the results of Table 9. There we exclude all households that at any time reported a death or health case in the household. The results for the coverage effect are similar to Table 9. (We defer Table A5 to the appendix as we are somewhat concerned with data quality regarding this survey item since households in the treatment group report injuries more often than those in the control group.)

Table 9: Disentangling treatment effects on children’s outcomes – ruling out insurance payment effects (without households with paid claims at any time)

	Child labor	Hours worked	Child labor earnings	Hazardous occupation	School attendance	Days missed
TE 6 months – mandatory	0.054	2.00	-61.6	-0.028	-0.015	0.48
TE 12 months – mandatory	0.022	1.14	-54.4	-0.030	0.016	0.31
TE 18 months – mandatory	**0.11****	**3.47***	140	-0.0051	-0.0034	0.56
TE 24 months – mandatory	0.077	1.88	-29.5	0.0062	0.031	0.53
ΔTE 6 months – voluntary	**-0.093****	**-3.86****	-135	-0.034	0.043	**-0.71****
ΔTE 12 months – voluntary	**-0.12*****	**-5.07*****	-175	**-0.045***	0.023	**-0.67****
ΔTE 18 months – voluntary	**-0.12*****	**-3.10***	-112	-0.025	0.025	**-1.13*****
ΔTE 24 months – voluntary	**-0.14*****	**-4.25*****	**-214***	**-0.057****	0.0087	**-1.01*****
Controls	YES	YES	YES	YES	YES	YES
N	19,575	19,571	19,575	19,575	14,201	9,667

*** p<0.01, ** p<0.05, * p<0.1, standard errors including random effects at the branch-wave level, upper part of table shows treatment effects for households with mandatory members only (no insurance coverage effect), lower part for households with at least one potential voluntary member (coverage effect only), sample excludes households that at any time received a payment from the insurance.

The results of Table 9 are in line with our theoretical model, which suggests that expectations of parents about risk might play a very large role. Being protected by insurance and thus expecting less severe shocks in the future might already reduce child labor today. This is especially true in a context where health shocks can have devastating effects. As Qamar et al. (2007, 8) state in the context of Pakistan: *“A major illness of just one member of the household (especially if he/she is the primary bread earner) can throw the entire family into poverty.”* Hence, the fear of this type of shock and the underlying feeling of vulnerability should be very strong. A precautionary and rational response to this fear is using child labor to accumulate additional financial resources.

V. Conclusion

Economic shocks play a large role for poor households. One of the undesired consequences might be that hardship forces parents to send children to work or take them out of school. This coping strategy is especially dangerous because it may harm long-term human capital accumulation or health for the next generation. Microinsurance is widely promoted as a tool to reduce vulnerability to shocks and hence potentially protects children from child labor. So far there are almost no studies assessing the effect of formal insurance on child labor and schooling outcomes, though.

It is straightforward to imagine that insurance will protect children from being pushed into child labor once medical costs arise. Yet, a change in economic uncertainty might also have effects *ex ante,* before a shock actually takes place. To improve our understanding of possible effect channels we formally model the relationship between economic risk and child labor input. The model shows that risk-averse households respond to high risk by using child labor as an additional precautionary income source. If households are sufficiently poor and afraid of the shock, they will react to the introduction of insurance by reducing child labor even without a shock taking place.

To estimate the actual effect of insurance we exploit a randomized controlled trial in Hyderabad, Pakistan. An innovation package consisting of (a) the extension of voluntary health insurance coverage and (b) regular visits sensitizing microcredit clients regarding claim procedures was introduced in nine treatment branches. We make use of a baseline and four follow-up survey waves to estimate time-varying treatment effects. We also account for unobserved random effects on the branch-wave level by using random effect regressions.

We find that the innovation package indeed helps to reduce child labor related outcomes. The combination of offering increased coverage and helping with claims decreases hazardous work and earnings generated through child labor.

To disentangle the effects of extended coverage and regular claim assistance visits we use the feature that certain household types are completely covered by mandatory insurance and have no possibility to extend coverage. We thereby isolate the effect of regular visits for those households with only mandatory members. These households by definition have the same coverage in treatment and control branches and can serve as an additional control group within treatment branches. Using this triple difference estimator we find that the main effect of the innovation is caused by extending insurance coverage to other household members. The extension reduces child labor incidence by around ten percent, weekly hours worked by children by around four and days missed at school by around one. Monthly visits alone, on the other hand, have little significant effects.

We present additional evidence suggesting that most of the coverage effect is an ex-ante effect. This means that increasing coverage within the household already protects children, even before health shocks taking place. Based on our theoretical model we argue that with a decrease in perceived vulnerability households seem to feel more comfortable to abstain from child labor as a precautionary income source. The channel is plausible: health is considered the most important factor of risk in Pakistan and medical costs are especially devastating for the poor. The microfinance clients in our sample to a large extent are below the poverty line and can therefore be expected to react strongly to a change in vulnerability.

Appendix I

Figure A1 shows time trends in treatment versus control branches for all outcome variables. These trends reflect the results of specification (1) of Table 6 and provide a visual impression of the sizes of the estimated treatment effects.

Figure A1: Plots of time trends for children's outcomes, by control vs. treatment branches

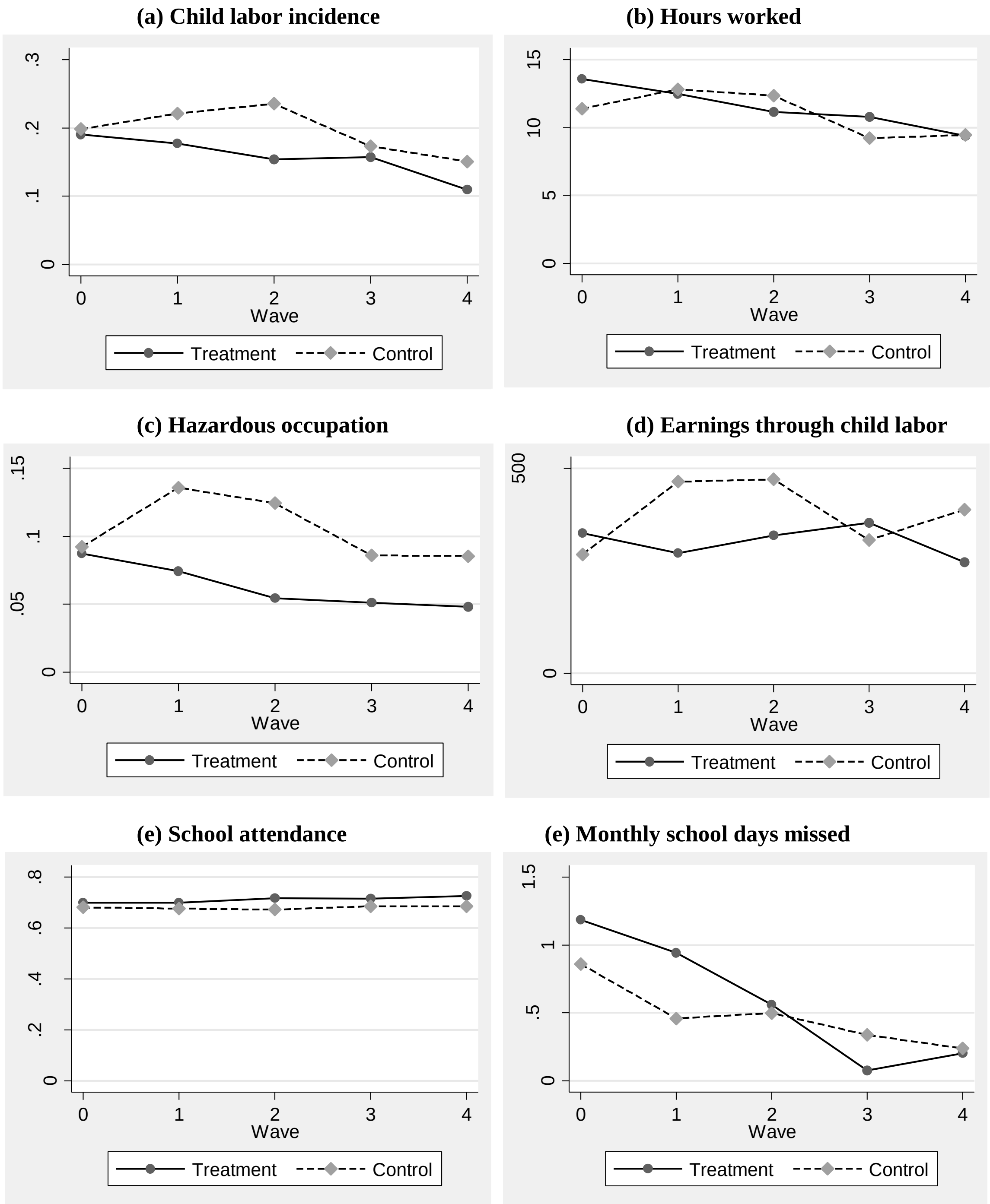


Table A1 complements Tables 6 and 7. Here we show all regression coefficients of specification (2) of Tables 6 and 7, whereas Tables 6 and 7 only reported the coefficient on the treatment indicator. (I.e. the first four rows in Table A1 are identical to the results in Tables 6 and 7.) Most effects of the covariates are plausible: Poorer, older and less educated parents more intensively use child labor. Also economic shocks increase child labor incidence. Girls are significantly less exposed to child labor.

Table A1: Effect of other covariates in regressions on child outcomes –specification (2) from Tables 6+7

	Child labor	Hours worked	Child labor earnings	Hazardous occupation	School attendance	School days missed
Treatment Effect 6 months	-0.023	-1.28	**-162***	**-0.053****	0.014	-0.044
Treatment Effect 12 months	**-0.067***	-2.78	**-190****	**-0.061*****	0.023	-0.14
Treatment Effect 18 months	0.015	0.67	44.9	-0.024	0.0068	-0.27
Treatment Effect 24 months	-0.029	-1.53	**-191****	-0.033	0.026	-0.18
Poverty Score at baseline (PPI)	**-0.0017*****	**-0.11*****	**-2.90*****	**-0.0010*****	**0.010*****	**-0.0096*****
Spouse in household ?	**-0.021*****	**-0.88*****	**-107*****	-0.0034	0.0043	**0.23*****
No. of Children age 0-4	**-0.0055***	0.023	0.30	-0.0037	-0.0016	**0.053****
No. of Children age 5-13	-0.0025	-0.14	**-16.1*****	0.0013	**0.0063****	**-0.038****
No. of Children age 14-17	**-0.012*****	**-0.57*****	**-54.2*****	**-0.0088*****	**0.023*****	0.025
No. of Adults	**-0.020*****	**-0.87*****	**-67.2*****	**-0.0083*****	-0.0012	-0.021
Mean parental age (years)	**0.0014*****	**0.074*****	**4.63*****	**0.00084*****	**-0.0027*****	-0.0026
Mean parental schooling (yrs)	**-0.0054*****	**-0.31*****	**-9.34*****	**-0.0016*****	**0.020*****	0.00024
Monthly income per capita ([a])	**-0.000041*****	**-0.0016*****	**-0.15*****	**-0.000013*****	**0.000018*****	**-0.000058*****
Health shock in family?	**0.046*****	**2.34*****	**88.6***	-0.0085	**-0.066*****	0.23
Non-health shock in family?	**0.066****	0.66	-32.3	-0.0066	**0.088****	**0.41***
Death of family member?	0.0037	0.57	**74.7***	**0.022****	**-0.10*****	0.076
Gender (0=male, 1=female)	**-0.085*****	**-3.76*****	**-526*****	**-0.0081****	**0.040*****	-0.040
Age (in years)	**0.034*****	**1.76*****	**74.1*****	**0.011*****	**0.036*****	0.015
Age 5 – 11 (dummy)	-0.011	**-11.5*****	**-294*****	**-0.093*****	**0.35*****	0.025
Age 12 – 13 (dummy)	-0.0070	**-11.3*****	**-351*****	**-0.083*****	**0.17*****	-0.021
N	20491	20487	20491	20491	14930	10457

*** p<0.01, ** p<0.05, * p<0.1, standard errors including random effects at the branch-wave level,
[a] adjusted for minor household members (factor 0.6) and excluding income from child labor, income in Pakistani rupees (1000 Rs = approx. 11$ US), treatment and wave effects omitted.

In Table A2 we examine the robustness of the results of Tables 6 and 7 when dropping income as a regressor. In Tables 6 and 7 income was included as a regressor, but its potential endogeneity was noted. The results of Table A2 are similar to those of Tables 6 and 7.

Table A2: Treatment effects on child labor - specification (2) of Tables 6 and 7 without regressor 'income'

	Child labor	Hours worked	Child labor earnings	Hazardous occupation	School attendance	Days missed
Treatment Effect 6 months	-0.030	-1.55	**-185***	**-0.053*****	0.0018	0.20
	(0.040)	(1.74)	(95.6)	(0.020)	(0.029)	(0.46)
Treatment Effect 12 months	-0.063	-2.60	**-172***	**-0.059*****	0.020	-0.12
	(0.039)	(1.70)	(93.4)	(0.020)	(0.029)	(0.46)
Treatment Effect 18 months	0.0082	0.40	21.2	-0.026	0.0086	-0.26
	(0.039)	(1.70)	(93.5)	(0.020)	(0.029)	(0.46)
Treatment Effect 24 months	-0.020	-1.19	**-158***	-0.031	0.021	-0.15
	(0.039)	(1.70)	(93.7)	(0.021)	(0.029)	(0.46)
Controls (without income) [a]	YES	YES	YES	YES	YES	YES
N	20,491	20,487	20,491	21,176	15,415	10,526

*** p<0.01, ** p<0.05, * p<0.1, standard errors in brackets, random effects at the branch-wave level (RE),
[a] control variables do not include income (otherwise control variables as in Table A1).

Similarly, Table A3 complements Table 8 when dropping income as regressor in specification (3). Again, results remain similar.

Table A3: Disentangling treatment effects on children's outcomes – specification as in Table 8 without the regressor 'income'

	Child labor	Hours worked	Child labor earnings	Hazardous occupation	School attendance	Days missed
TE 6 months – mandatory	0.040	1.49	-74.2	-0.022	-0.027	**0.83***
TE 12 months – mandatory	0.024	1.23	-47.8	-0.027	0.011	0.49
TE 18 months – mandatory	**0.096***	2.82	107	-0.0095	-0.013	0.62
TE 24 months – mandatory	0.075	1.95	-40.2	0.0090	0.024	0.65
ΔTE 6 months – voluntary	**-0.090****	**-3.91****	-149	-0.040	0.042	**-0.92*****
ΔTE 12 months – voluntary	**-0.11*****	**-5.08*****	-164	**-0.044***	0.018	**-0.90*****
ΔTE 18 months – voluntary	**-0.12*****	**-3.07***	-109	-0.023	0.030	**-1.32*****
ΔTE 24 months – voluntary	**-0.13*****	**-4.14*****	-162	**-0.056****	-0.00052	**-1.20*****
Controls (without income) [a]	YES	YES	YES	YES	YES	YES
N	20,491	20,487	20,491	21,176	15,415	10,526

*** p<0.01, ** p<0.05, * p<0.1, standard errors including random effects at the branch-wave level, upper part of table shows treatment effects for households with mandatory members only (no insurance coverage effect), lower part for households with at least one potential voluntary member (coverage effect only),
[a] control variables do not include income (otherwise control variables as in Table A1).

Table A4 complements Table 8 and shows estimates for various alternative definitions of child labor, where we vary the amount of hours required before being considered a child laborer. These alternative classifications differ from the ILO definition and are based only on hours worked, not on type of activity. (This explains the slight variation between the sample sizes.) In the second column a child is only defined as a child laborer if working at least 5 hours (if 11 years old or younger), at least 20 hours (if 14 years old or younger) or at least 43 hours (if older than 14 years) per week. In the last column, only children working very many hours are coded as child laborers, i.e. if working at least 10 hours (if 11 years old or younger), at least 30 hours (if 14 years old or younger) or at least 50 hours (if older than 14 years). The estimation results for the coverage effect are robust to these alternative definitions.

Table A4: Disentangling treatment effects on children's outcomes – alternative child labor definitions

	Child labor (ILO definition)	Child laborer if working many hours [a]	Child laborer if working very many hours [b]
Treatment effect 6 months – mandatory	0.046	0.051	**0.063***
Treatment effect 12 months – mandatory	0.019	0.020	0.024
Treatment effect 18 months – mandatory	**0.10****	**0.12*****	**0.092*****
Treatment effect 24 months – mandatory	0.069	**0.068***	**0.073****
Δ Treatment effect 6 months – voluntary	**-0.088****	-0.058	**-0.063****
Δ Treatment effect 12 months – voluntary	**-0.12*****	**-0.090*****	**-0.11*****
Δ Treatment effect 18 months – voluntary	**-0.12*****	**-0.095*****	**-0.082*****
Δ Treatment effect 24 months – voluntary	**-0.13*****	**-0.11*****	**-0.11*****
Controls	YES	YES	YES
N	20,491	20,487	20,487

*** p<0.01, ** p<0.05, * p<0.1, standard errors including random effects at the branch-wave level,
upper part of table shows treatment effects for households with mandatory members only (no insurance coverage effect), lower part for households with at least one potential voluntary member (coverage effect only),
[a] allows <5 weekly hours up to 11 years, <20 hours up to 14 years and <43 hours up to 17 years,
[b] allows <10 weekly hours up to 11 years, <30 hours up to 14 years and <50 hours up to 17 years.

Table A5 complements Table 9 and examines the sensitivity to the definition of a health shock. Instead of excluding households that received a claim payment at some point in time (as we do in Table 9), we exclude in Table A5 all households that at any time reported a death or a health case. The estimates for the coverage effect remain stable.

Table A5: Disentangling treatment effects on children's outcomes – ruling out insurance payment effects (without households with injuries/deaths at any time)

	Child labor	Hours worked	Child labor earnings	Hazardous occupation	School attendance	Days missed
TE 6 months – mandatory	0.063	2.08	-63.6	-0.026	-0.033	**0.77****
TE 12 months – mandatory	0.018	0.65	-48.4	-0.039	0.00041	**1.31*****
TE 18 months – mandatory	**0.10****	2.74	117	-0.014	-0.033	**0.88****
TE 24 months – mandatory	0.067	1.38	-45.9	0.0025	0.0028	**0.97*****
ΔTE 6 months – voluntary	**-0.097****	**-3.97****	-122	-0.030	0.072	**-0.97*****
ΔTE 12 months – voluntary	**-0.11*****	**-4.14****	-169	-0.030	0.032	**-1.52*****
ΔTE 18 months – voluntary	**-0.12*****	**-3.20***	-145	-0.0074	0.048	**-1.44*****
ΔTE 24 months – voluntary	**-0.14*****	**-4.43****	**-211***	**-0.054***	0.0057	**-1.42*****
Controls	YES	YES	YES	YES	YES	YES
N	14,321	14,317	14,321	14,321	10,415	7,106

*** p<0.01, ** p<0.05, * p<0.1, standard errors including random effects at the branch-wave level,
upper part of table shows treatment effects for households with mandatory members only (no insurance coverage effect), lower part for households with at least one potential voluntary member (coverage effect only),
sample excludes households that at any time reported a death or illness/injury.

Appendix II – Supplementary background statistics

The following supplementary tables are meant to provide some further background information about the situation in Pakistan, e.g. about the serial correlation of child labor and about the importance of insurance payouts for health expenditures. All statistics and regressions are rather descriptive and are therefore not included in the main text. They shall provide an impression of how important certain correlations are in our data set.

In Table A6 we examine child labor and how it is correlated over time. We find that child labor status is rather persistent across waves. Even though changes occur, more than half of the child laborers in one period (CL_{t-1}=1) remain child laborers in the follow-up period (CL_t=1). This is true for treatment and control branches. On the other hand, only about 10 % of the non-child laborers from the previous period (CL_{t-1}=0) become child laborers in the next period (CL_t=1). Especially compared with child laborers from the pre-period, their risk is substantially lower. This strong serial correlation indicates that the decision about sending children to work is not taken independently in each period.

Table A6: Persistence of Child labor prevalence over time, control vs. treatment branches

	Fraction of children providing child labor in period t, by child labor status in t-1					
	Control branches			Treatment branches		
	ALL	No child labor in time t-1	Child laborer in time t-1	ALL	No child labor in time t-1	Child laborer in time t-1
Baseline	0.20			0.19		
6 months	0.22	0.14	0.56	0.18	0.10	0.65
12 months	0.24	0.12	0.69	0.15	0.07	0.53
18 months	0.17	0.09	0.51	0.16	0.09	0.58
24 months	0.15	0.08	0.55	0.11	0.05	0.40

Note: children age 5-17

In Table A7 we examine health expenditures of households. The first panel of Table A7 shows how monthly health expenditures vary with reported injuries and illnesses. (The panels (b) and (c) examine treatment and control branches separately.) We control for wave and household fixed effects. We see that overall health expenditures increase over time, starting at about 500 rupees per month at baseline. A reported injury or illness almost doubles health expenditures, while a paid claim reduces a substantial part of those extra expenditures. Effects are generally stronger for hospitalization expenditures: At baseline the average cost is less than 80 rupees and a reported injury or illness is associated with an increase of more than 400 rupees. For cases with reimbursement the health expenses increase substantially less. This is consistent with the insurance scheme that helps to cover expenditures of hospitalization.

Panels (b) and (c) of Table A7 show the same analysis for treatment and control branches separately. The effects are estimated with less precision, especially for control branches, but qualitatively the results remain the same, particularly for hospitalization expenditures. A reported injury or illness increases expenditures for hospitalization in particular, while a paid claim substantially reduces extra expenditures.

Table A7: Fixed effects regression of monthly household health expenditures on health shocks

a) All branches

	All	Outpatient	Hospitalization
Injury/illness case happened	**465***	**50.7**	**414***
Injury/illness paid by insurance	**-212***	-58.0	**-155**
Time dummy: 6 months	41.3**	27.4**	13.5
Time dummy: 12 months	81.8***	49.9***	33.0**
Time dummy: 18 months	241***	130***	111***
Time dummy: 24 months	253***	244***	8.82
Constant	503***	424***	78.7***
Household fixed effects?	YES	YES	YES
N	10,102	10,103	10,091

b) Treatment branches only

	All	Outpatient	Hospitalization
Injury/illness case happened	**511***	28.5	**483***
Injury/illness paid by insurance	**-180**	-39.2	**-141**
Time dummy: 6 months	102***	90.2***	11.6
Time dummy: 12 months	52.0**	73.8***	-21.8
Time dummy: 18 months	252***	156***	96.3***
Time dummy: 24 months	166***	175***	-8.95
Constant	440***	380***	60.7***
Household fixed effects?	YES	YES	YES
N	6,352	6,352	6,350

c) Control branches only

	All	Outpatient	Hospitalization
Injury/illness case happened	**141**	-8.79	**147***
Injury/illness paid by insurance	-168	-28.1	-140
Time dummy: 6 months	-52.7	-69.3***	15.8
Time dummy: 12 months	142***	16.2	129***
Time dummy: 18 months	231***	94.7***	136***
Time dummy: 24 months	411***	362***	50.1*
Constant	610***	501***	109***
Household fixed effects?	YES	YES	YES
N	3,750	3,751	3,741

*** p<0.01, ** p<0.05, * p<0.1, level of analysis is the household,
'Injury/illness case' indicates that someone in the household reported an injury or hospitalization,
'Injury/illness paid' indicates whether a reimbursement of expenditures by the insurance took place.

Table A8 examines how households in treatment branches make use of the option to voluntarily insure additional household members. We examine whether households in treatment branches use the option of coverage extension to insure the *entire* household or whether they pick only selected members to be insured. Table A8 shows which *fraction* of the eligible household members is voluntarily insured. For a meaningful analysis we examine only those households with at least two eligible members, living in treatment areas. The table thus shows whether households insure individual members selectively (maybe based on risk characteristics), or whether households rather opt for complete coverage. At baseline, none of the additional household members is insured (since insurance was not yet available then). After 6 months there is a mixed picture. More than half of the households opt for complete (i.e. share equals 1) or zero coverage (i.e. share equals 0), but the other half only covered a part of the household. However, after 12 and 18 months there is a clear tendency to cover either all or none of the potentially insured: about 80% of the households have either complete or zero insurance coverage. This tendency remains stable when only considering those households that currently have a loan with NRSP. Hence, although some households remain that deliberately choose whom to insure, most households insure everyone or none at all.

Table A8: Distribution [%] of shares of voluntarily insured per household, per wave

Share of additional Household members insured	baseline	6 months	12 months	18 months
0	100.00	7.66	36.12	38.13
0.01 – 0.10	0.00	0.00	0.00	0.00
0.11 – 0.20	0.00	1.72	0.00	1.07
0.21 – 0.30	0.00	3.75	0.79	2.30
0.31 – 0.40	0.00	5.94	2.21	1.53
0.41 – 0.50	0.00	15.16	6.62	7.81
0.51 – 0.60	0.00	1.41	0.00	0.46
0.61 – 0.70	0.00	8.59	5.05	6.43
0.71 – 0.80	0.00	5.78	3.63	3.22
0.81 – 0.90	0.00	5.94	3.15	2.30
0.91 – 0.99	0.00	0.16	0.00	0.00
1	0.00	43.91	42.43	36.75
N	602	640	634	653

Note: Only households in treatment branches and with at least two potential voluntary members are considered. Example: Consider a household with 3 additional adult members, of whom 1 is voluntarily insured, i.e. 33%.

In Table A9 we examine who becomes voluntarily insured. We regress the insurance status on individual and household characteristics, using logit regression. The table shows marginal effects. We cluster standard errors at the household level and capture the decreasing time trend in voluntary coverage (apparent also in Figure 3(c)) by controlling for wave dummies. On the individual level, gender and age are significantly related to coverage. While males are covered with higher probability, household members who are between 25 and 35 years old have the highest coverage rate. Older individuals have a lower probability of receiving additional coverage. A nonparametric plot of the insurance probability as a function of age and gender is shown in Figure A2.

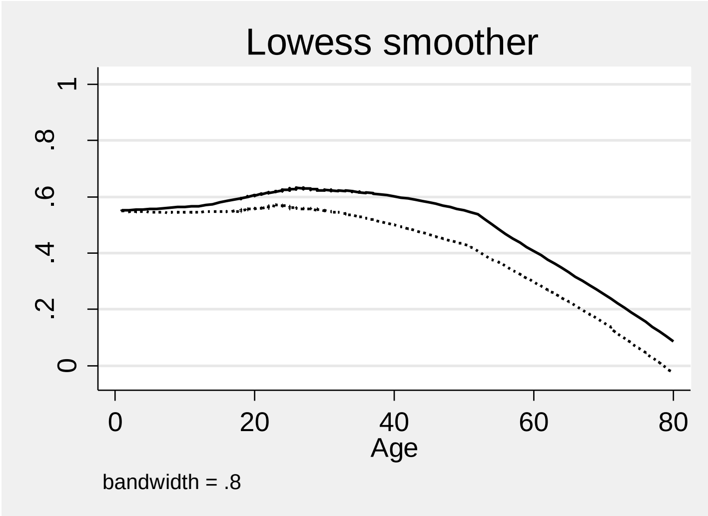
Table A9 also shows the effects of household characteristics. We find that voluntary insurance is more likely to be purchased by less educated, older and wealthier clients. Also the number of children in the household is positively related to additional coverage, in contrast to the number of adults which is negatively related to uptake.

Table A9: Logit regression of additional insurance uptake on individual and household characteristics

	All
Time dummy: 12 months	**-0.15*** **
Time dummy: 18 months	**-0.23*** **
Gender (0=male, 1=female)	**-0.030** **
Age	**0.019*** **
Age squared	**-0.00029*** **
Age 0 – 4	**0.20*** **
Age 5 – 11	**0.11* **
Age 12 – 13	-0.019
Age 14 – 17	**-0.094* **
Education	0.0021
Mean parental education (years)	**-0.0063* **
Mean parental age	**0.0025* **
Monthly income per capita (in 1000 Rs.)	**0.17*** **
Monthly income per capita (in 1000 Rs.) squared	**-0.012*** **
Spouse in household?	-0.032
No. of Children age 0-4	**0.038** **
No. of Children age 5-13	**0.019* **
No. of Children age 14-17	**0.039*** **
No. of Adults	**-0.016** **
N	7,819

Marginal effects shown, *** p<0.01, ** p<0.05, * p<0.1, standard errors clustered on household level, "parental" indicates characteristics of the client and spouse.

Figure A2: Plots of additional insurance uptake, by age and gender



Note: Male (solid line) and female (dotted line)

Appendix III – Modeling child labor decisions by households

In the absence of an adequate benchmark model from the theoretical child labor literature (as discussed in the main text), we develop a simple model of the role of risk for child labor input. We consider two periods within childhood. In both periods, parents take a decision about the amount of child labor l_t^c. They take negative consequences of child labor into account (such as the trade-off with school attendance and learning) by attaching a positive utility (with weight $\gamma > 0$) to child leisure $1 - l_t^c$. (Note that we define the time where the child does not work as "child leisure". Leisure time thus also includes the time in school and time for school homework. This definition might deviate from what the child itself considers as leisure time.)

On the other hand, they benefit from the income generated by child labor through consumption c_t in each period:

$$U_t = \log c_t + \gamma \log(1 - l_t^c)$$

Child labor is restricted to the interval $l_t^c \in [0,1]$. The log-shape of the utility function implies that the household is risk-averse. Households maximize expected total utility as defined by:

$$\tilde{E}[\sum_{t=1}^{2} \sigma^{t-1} U_t]$$

where $\sigma \leq 1$ is a discount factor and the expected value $\tilde{E}$ depends on the *subjective* expectation of a health shock defined below. We thus permit that the subjective probability of shocks may deviate from the true probability, i.e. households may overestimate the risk of rare events such as catastrophic health shocks.

Household income is generated by adults who supply labor inelastically, generating wage income w_A, and children supplying labor at wage rate w_C, generating income $w_C l_t^c$. In each period there is the risk of a (health) shock ($\theta_t = 1$), which generates additional (health) expenditures S to be paid. (We indicate the absence of a shock by $\theta_t = 0$.) A shock occurs with probability p, this is the true or *objective* shock probability. The household, on the other hand, expects the shock to happen with probability p_θ (subjective probability). This subjective probability is relevant for the decision making of the household, whereas the objective probability will be relevant later when a fair insurance is introduced. In each period t, shocks are observed *prior* to child labor and consumption choices. We allow for an initial endowment e_1. The budget constraint imposes that over the two periods total spending equals the budget:

$$\sum_{t=1}^{2} c_t \leq \sum_{t=1}^{2} (w_A + l_t^c w_C - \theta_t S) + e_1 \qquad \text{or equivalently}$$

$$\sum_{t=1}^{2} c_t + (1 - l_t^c) w_C \leq \sum_{t=1}^{2} (w_A + w_C - \theta_t S) + e_1 \tag{4}$$

To simplify the notation we define the total potential budget as

$$Y(\theta_1, \theta_2) := \sum_{t=1}^{2} (w_A + w_C - \theta_t S) + e_1 = 2w_A + 2w_C - (\theta_1 + \theta_2) S + e_1$$

which is the hypothetical budget if the child worked full-time, i.e zero leisure. Define also

$$Y_t = c_t + (1 - l_t^c) w_C$$

which can be interpreted as the budget in period t which is spent on buying consumption goods and consuming child leisure $(1 - l_t^c)$ at price w_C. (Hence, child leisure can be considered as a type of consumption, and Y_t is therefore not the real budget used, but total consumption consisting of consuming goods plus opportunity costs of foregone child labor earnings.) With this notation we can write the budget constraint equation (4) as

$$Y_1 + Y_2 \leq Y(\theta_1, \theta_2) \tag{5}$$

with strict equality for optimal household decisions.

Note that while we have no limit on borrowing or saving between the two periods, there is a limited time horizon ending after two periods.[111] This simplifying assumption is imposed to ease the tractability of the model. Solving the model requires specifying how to split money between periods (intertemporal decision) and how to allocate the period specific budget between consumption of goods and child leisure (intratemporal decision). We present first the intratemporal decision on allocation of a given period-specific budget between consumption and child leisure. Next, we show a solution for the intertemporal decision on how much budget to allocate to each period. Lastly, we use the results from the model to address the implications of an insurance policy.

[111] It might seem arbitrary that 'smoothing forward' via credits is not possible in the second period. Modeling a more dynamic setting, e.g. via Bellmann equations, is probably more realistic. However, it would make the formal problem much more complicated and our main goal is to show in a simple way that changing risk in the future already creates incentives for child labor today. These incentives will also show up in a more dynamic setting, especially if we introduce borrowing limits or an aversion to indebtedness.

The intratemporal problem

In each period the utility to be maximized is

$$\max_{c_t, l_t^c} U_t = \max_{c_t, l_t^c} \log(c_t) + \gamma \log(1 - l_t^c)$$

or with the budget constraint plugged in for consumption

$$\max_{l_t^c} U_t = \max_{l_t^c} \log(Y_t - (1 - l_t^c) w_C) + \gamma \log(1 - l_t^c).$$

If the budget available in each period is large relative to the child wage rate w_C, we might be at a corner solution where the optimal choice of child labor is $l_t^c = 0$. Yet, if we focus on very poor households, they would always supply a bit of child labor such that we would observe interior solutions. Assuming that the household is very poor, we can ignore corner solutions and solve the household decision making via the first order condition:

$$\frac{\partial U_t}{\partial l_t^c} = \frac{w_C}{Y_t - (1 - l_t^c) w_C} - \frac{\gamma}{1 - l_t^c} = 0$$

$$\begin{aligned}
&\Rightarrow w_C (1 - l_t^c) = \gamma (Y_t - (1 - l_t^c) w_C) \\
&\Leftrightarrow w_C (1 - l_t^c)(1 + \gamma) = \gamma Y_t \\
&\Leftrightarrow (1 - l_t^c) = \frac{\gamma}{1 + \gamma} \frac{Y_t}{w_C}
\end{aligned}$$

Inserting this result in the budget constraint we obtain

$$c_t = Y_t - (1 - l_t^c) w_c = \frac{1}{1 + \gamma} Y_t$$

Hence, we obtain the usual result that the household likes to spend a fraction $\frac{\gamma}{1+\gamma}$ of the budget on the good 'child leisure' at price w_C and a fraction $\frac{1}{1+\gamma}$ on consumption at price 1. The utility derived from the solution is

$$U_t = \log\left(\frac{1}{1 + \gamma} Y_t\right) + \gamma \log\left(\frac{\gamma}{1 + \gamma} \frac{Y_t}{w_C}\right)$$

The marginal utility from having one unit increase in the period-specific budget Y_t is then

$$\frac{\partial U_t}{\partial Y_t} = \frac{1 + \gamma}{Y_t} \tag{6}$$

The intertemporal problem

Having solved the intratemporal optimization we know the utility that can be derived from a given budget Y_t in each period. In particular, equation (6) gives the marginal utility, which should be equalized across periods. The intertemporal decision on how to split the budget between periods will be driven by the overall utility function of the household. It consists of the first period utility and the expected discounted second period utility:

$$U = U_1(Y_1) + \sigma \tilde{E}[U_2(Y_2)]$$

Note that the household knows the shock realization θ_1 in period 1 before taking any decision on consumption and child labor in the first period. Therefore, no expectation is involved in the first period utility part. On the other hand, θ_2 is unknown when making consumption and labor supply decisions for the first period. Total utility only depends on the budget available in both periods, which is used for consumption of child leisure and of goods. In equation (6) we have derived the marginal utility of money. For optimality of the budget split between the two periods we will need that the marginal utility of money in the first period equals the expected discounted marginal utility of money in the second period:

$$\frac{\partial U_1(Y_1)}{\partial Y_1} = \frac{\partial \sigma \tilde{E}[U_2(Y_2)]}{\partial Y_2} \tag{7}$$

We can derive the expected discounted marginal utility for the second period by making use of (6) and the budget constraint (5) and remembering that a shock is expected with subjective probability p_θ:

$$\frac{\partial \sigma \tilde{E}[U_2(Y_2)]}{\partial Y_2} = \sigma p_\theta \frac{1+\gamma}{Y(\theta_1,1) - Y_1} + \sigma(1-p_\theta)\frac{1+\gamma}{Y(\theta_1,0) - Y_1}$$

It will be helpful to define a function f as the difference between the marginal utility of money in period 1 and period 2:

$$f(Y_1;\theta_1) \equiv \frac{\partial U_1}{\partial Y_1} - \frac{\partial \sigma \tilde{E}[U_2(Y_2)]}{\partial Y_2} = \frac{1+\gamma}{Y_1} - \sigma p_\theta \frac{1+\gamma}{Y(\theta_1,1) - Y_1} - \sigma(1-p_\theta)\frac{1+\gamma}{Y(\theta_1,0) - Y_1}.$$

$f(Y_1;\theta_1)$ depends on the shock status θ_1 in period 1 and on the choice variable Y_1. For a particular choice of Y_1, the consumption of leisure and goods in time period two, i.e. Y_2, is fixed via the budget constraint (5). The optimal choice of Y_1, given θ_1, is determined by the equilibrium condition (7), which we can re-write in terms of the function *f* as:

$$f(Y_1^{optimal};\theta_1) = 0 \qquad (8)$$

For a value Y_1 where $f(Y_1;\theta_1)$ is positive, marginal utility of money is larger in the first period than in the second period, such that there would be an incentive to shift some consumption from period 2 to period 1, i.e. to increase Y_1. This will be the case for low levels of Y_1. The reverse is true for large values of Y_1. Formally, the function f is strictly decreasing in the choice variable Y_1 in the domain $[0, Y(\theta_1, 1)]$, starting out from $f(0) = \infty$ and reaching $f(Y(\theta_1, 1)) = -\infty$. Hence, there must be a unique Y_1 satisfying the equilibrium condition (7). This unique Y_1 depends on θ_1, and it determines child labor in period one. Future child labor then only depends on the realization of the shock in period two and the resulting budget.

The policy intervention: insurance

The effect of introducing an actuarially fair insurance can be modeled by reducing the shock cost (i.e. health expenditures) by the indemnity payment I (where $I \leq S$, i.e. the indemnity payment is not larger than the health shock) and at the same time reducing income by the fair premium Ip, where p is the *objective* shock probability, i.e. the true probability of a shock. If insurance is bought for both periods, the potential budget will now become

$$Y(\theta_1, \theta_2) = 2w_A + 2w_C + e_1 - (\theta_1 + \theta_2)S + (\theta_1 + \theta_2)I - 2 \cdot Ip \qquad (9)$$

where $(\theta_1 + \theta_2)I$ is the indemnity payment in case of a shock in period 1 and/or period 2. The premium to be paid for the insurance in both periods together is $2 \cdot Ip$.

We also consider the case if insurance is bought only for one period. We consider particularly the case where insurance is only bought for the second period in order to analyze the ex-ante effects in the first period. If only the second period is insured, the potential budget becomes

$$Y(\theta_1, \theta_2) = 2w_A + 2w_C + e_1 - (\theta_1 + \theta_2)S + \theta_2 I - Ip \qquad (10)$$

where only the second period shock is insured and the premium payment is therefore only Ip. If the indemnity I is zero, i.e. no insurance, the potential budget in (9) and (10) simplifies to the budget of the previous subsection.

From equations (9) and (10) we can calculate the marginal effects of increasing insurance on the potential budget, which will be used to examine the effects of insurance on child labor below. The total effect of the insurance on the overall budget depends on the periods covered and on the different shock realizations:

Table A10: Budget effect of insurance in different situations

	Insurance bought for period 2 only	Insurance bought for both periods
No shock in period 1 i.e. $\theta_1 = 0$	Case I: $\frac{\partial Y(0,0)}{\partial I} = -p$ $\frac{\partial Y(0,1)}{\partial I} = 1 - p$	Case III: $\frac{\partial Y(0,0)}{\partial I} = -2p$ $\frac{\partial Y(0,1)}{\partial I} = 1 - 2p$
Shock in period 1 i.e. $\theta_1 = 1$	Case II: $\frac{\partial Y(1,0)}{\partial I} = -p$ $\frac{\partial Y(1,1)}{\partial I} = 1 - p$	Case IV: $\frac{\partial Y(1,0)}{\partial I} = 1 - 2p$ $\frac{\partial Y(1,1)}{\partial I} = 2 - 2p$

Note that we distinguish these four cases because the periods covered by insurance as well as the shock realization in period 1 are known (also in our dataset) when deciding about child labor and consumption in the first period. In each case the household faces the risk of a good ($\theta_2 = 0$) or bad ($\theta_2 = 1$) state in period 2. Condition (8) and the corresponding function f reflect this risk by weighting the two states with subjective probabilities.

The function f changes with I because it will change the overall potential budget in different states. Households consequently need to adjust the budget split via Y_1 to preserve the optimality condition $f = 0$, see equation (8), in the following way:

$$\frac{\partial f}{\partial I} dI + \frac{\partial f}{\partial Y_1} dY_1 = 0$$

which implies

$$\frac{dY_1}{dI} = -\frac{\partial f / \partial I}{\partial f / \partial Y_1}. \qquad (11)$$

If we are able to sign $\partial f / \partial I$ and $\partial f / \partial Y_1$, we know the sign of dY_1 / dI. We therefore examine each partial derivative in turn:

$$\frac{\partial f}{\partial Y_1} = \left(-\frac{1}{Y_1^2} - \frac{\sigma p_\theta}{(Y(\theta_1,1)-Y_1)^2} - \frac{\sigma(1-p_\theta)}{(Y(\theta_1,0)-Y_1)^2} \right)(1+\gamma) < 0,$$

i.e. this derivative is always negative, as we had already discussed earlier, where we argued that f is strictly decreasing in Y_1. Inserting this result in (11), we obtain that

$$sign\left(\frac{dY_1}{dI}\right) = sign\left(\frac{\partial f}{\partial I}\right)$$

at the optimal choices. If $\partial f / \partial I > 0$ introducing (more) insurance would lead to an increase in the period one budget Y_1 and consequently to a decrease in period one child labor. We therefore need to consider $\partial f / \partial I$.

$$\begin{aligned}\frac{\partial f}{\partial I} &= \frac{\partial f}{\partial Y(\theta_1,1)}\frac{\partial Y(\theta_1,1)}{\partial I} + \frac{\partial f}{\partial Y(\theta_1,0)}\frac{\partial Y(\theta_1,0)}{\partial I} \\ &= \frac{(1+\gamma)\sigma p_\theta}{(Y(\theta_1,1)-Y_1)^2}\frac{\partial Y(\theta_1,1)}{\partial I} + \frac{(1+\gamma)\sigma(1-p_\theta)}{(Y(\theta_1,0)-Y_1)^2}\frac{\partial Y(\theta_1,0)}{\partial I}\end{aligned} \tag{12}$$

The sign of $\partial f / \partial I$ depends on the shock realizations and on the number of periods insurance was bought for (see Table A10). In the following we will distinguish all four cases shown in the table. We start with cases I and II (insurance in period two only) because this reflects the pure risk reduction effect of insurance (i.e. the ex-ante effect) without a budget effect on average. In contrast, cases III and IV will always involve a premium payment with or without insurance benefit, i.e. including the ex-post effect. This positive or negative budget effect makes it more complicated to interpret the insurance effect.

Case I: *Insurance bought for period 2 only and no shock in period 1*

Inserting the respective derivatives of Table A10 in equation (12) we obtain

$$\frac{\partial f}{\partial I} = \frac{\sigma p_\theta (1+\gamma)(1-p)}{(Y(\theta_1,1)-Y_1)^2} - \frac{\sigma(1-p_\theta)(1+\gamma)p}{(Y(\theta_1,0)-Y_1)^2}$$

which is positive if

$$p_\theta(1-p)(Y(\theta_1,0)-Y_1)^2 \quad > \quad p(1-p_\theta)(Y(\theta_1,1)-Y_1)^2. \tag{13}$$

Noting that $Y(\theta_1,0)=Y(\theta_1,1)+S$ we can re-write condition (13) as

$$p_\theta(1-p)(Y(\theta_1,1)-Y_1+S)^2 \quad > \quad p(1-p_\theta)(Y(\theta_1,1)-Y_1)^2.$$

This condition is satisfied if $p_\theta \geq p$, i.e. if households are not underestimating the probability of a health shock. The reason is that the total potential budget with a shock in period 2 is lower than without, i.e. $Y(\theta_1,0)>Y(\theta_1,1)$. Therefore child labor decreases in period 1 if households buy insurance for period 2. Condition (13) is also satisfied if households underestimate the shock probability, as long as they do not underestimate too much.

Case II: Insurance bought for period 2 only and shock in period 1

The condition to be fulfilled is equivalent to Case I. Hence, irrespective of the shock drawn in period 1 households will increase period 1 budget (i.e. reduce precautionary saving) if they buy insurance for period 2. As a consequence, child labor decreases in period 1 (ex-ante effect).

Case III: Insurance bought for both periods and no shock in period 1

Inserting the respective derivatives of Table C1 in equation (12) we obtain

$$\frac{\partial f}{\partial I}=\frac{\sigma p_\theta(1+\gamma)(1-2p)}{(Y(0,1)-Y_1)^2}-\frac{\sigma(1-p_\theta)(1+\gamma)2p}{(Y(0,0)-Y_1)^2}$$

which is positive if

$$p_\theta(1-2p)(Y(0,0)-Y_1)^2 \quad > \quad 2p(1-p_\theta)(Y(0,1)-Y_1)^2 \tag{14}$$

There are two ways to fulfill this condition. First, if households overweight the shock probability by at least factor two ($p_\theta \geq 2p$), the condition (14) will be fulfilled for any overall budget. Second, even without overweighting loss probabilities, the condition will hold for sufficiently poor households. This is because in case of a shock they will have to restrict their second period budget, i.e. $Y(0,1)-Y$, to such low levels that the condition (14) holds because

$$\frac{Y(0,1)-Y_1}{Y(0,0)-Y_1}<<1 \text{ and consequently } \frac{(Y(0,1)-Y_1)^2}{(Y(0,0)-Y_1)^2}<\frac{p_\theta(1-2p)}{2p(1-p_\theta)}.$$

Case IV: Insurance bought for both periods and shock in period 1

Inserting the respective derivatives of Table C1 in equation (12) we obtain

$$\frac{\partial f}{\partial I}=\frac{\sigma p_\theta(1+\gamma)(2-2p)}{(Y(1,1)-Y_1)^2}+\frac{\sigma(1-p_\theta)(1+\gamma)(1-2p)}{(Y(1,0)-Y_1)^2}$$

which is positive if

$$p_\theta(2-2p)(Y(1,0)-Y_1)^2 \quad > \quad (2p-1)(1-p_\theta)(\underbrace{Y(1,1)-Y_1}_{<Y(1,0)-Y_1})^2 \tag{15}$$

Condition (15) is fulfilled under mild conditions. It will hold under the same conditions as in Case I and II (no underweighting of probabilities). Even if households were underweighting, the condition would hold as long as the objective shock probability p is not larger than 0.5, as the right hand side would become negative while the left hand side remains positive.

Even if a shock occurs with very large probability $p>0.5$, households must be severely underweighting probabilities before condition (15) is violated. Consider the worst case where S is very small relative to the budget such that $(Y(1,0)-Y_1)^2$ is hardly larger than $(Y(1,1)-Y_1)^2$. Even then condition (15) is satisfied as long as $p_\theta>2p-1$.

Suppose a shock occurs with probability $p=0.5+\varepsilon$, condition (15) is satisfied if

$$p_\theta>2\varepsilon.$$

This would only not be satisfied if households were extremely underestimating the probability of a shock. Condition (15) is even more likely to be fulfilled if the size of the shock S becomes larger relative to the budget.

The reason why (15) is likely to be fulfilled is that together with the substitution effect of budget from period 2 to period 1 through decreased risk there is a positive overall budget effect of the insurance because one shock is already paid, i.e. the ex-post effect of the payout in period 1 amplifies the ex-ante effect of having period 2 insured. Therefore child labor decreases in period 1 if households buy insurance.

Summing up, reducing risk in the future while keeping average overall budget constant decreases child labor already today (Cases I and II). This is because households need to worry less about creating precautionary savings. This effect becomes more pronounced if households are poorer and if they overweight the shock probability. If they are sufficiently poor or overweighting enough, even buying insurance in both periods and not profiting at present (negative budget effect, Case III), will reduce child labor. Note that we are not considering corner solutions here. From a certain income level onwards child labor is always zero and cannot be reduced further.

Appendix IV – Data structure

Table A11 provides the names and number of observations for the 9 treatment and 4 control branches. The number of households interviewed varies from 96 to 258.

Table A11: Observations per branch at baseline

Branch Name	Treatment?	Households	Individuals
Garhi Khata	YES	138	841
Gulshan e Hali	No	258	1,512
Hussainabad	YES	96	587
Islamabad	YES	153	908
Islamia Colony	No	192	1,145
Kotri-1	YES	198	1,218
Kotri-2	No	141	874
Latifabad-12	No	186	1,222
Latifabad-5	YES	147	897
Liaqat Colony	YES	120	723
Pathan Colony	YES	204	1,322
Phuleli	YES	105	564
Pretabad	YES	159	1,122
All		2,097	12,935

Appendix V – Description of insurance package

This appendix provides details on the insurance package provided by NRSP. (This information is taken from NRSP's innovation proposal with the ILO Microfinance for Decent Work Program.) The innovation in the treatment branches consisted in offering insurance also to other adult members of the household (in addition to the client, the spouse and their minor children who are mandatorily insured). In addition, clients in treatment branches were frequently visited by staff members to help with filing claims.

Coverage for Hospitalization

Insured	Self + Spouse + minor children + Adults 18 or above years
Age	18 or above
Hospitalization Confinement	Min 24 Hours
Sum Insured for Hospitalization	Rs.15,000/- for each family member
Premium Per family per annum	Rs. PKR 100 per adult/-

Benefits for Hospitalization

- Hospital Room charges
- Miscellaneous hospital services and services such as
- Physicians or surgeons visits
- Prescription drugs
- Prescribed Pathological and Radiological investigations
- Prescribed ECGs,ETT,ECHO,Angiography,M.R.I., C.T.Scan, Ultrasound or any other diagnostics test required
- Blood Transfusion
- Operation theater Charges
- Anesthetist's Fees
- Stitching Material
- Physiotherapy
- Radiotherapy and Chemotherapy
- Instruments required in the treatment
- Cutting operations
- *Pregnancy(sub limit 7500/-)
- Pregnancy complications
- Reasonable transportation cost of insured
- Direct Settlement Facility by NRSP offices to panel hospital

* Pregnancy claim should arise after seven to nine month of date of enrolment

Exclusions

- OPD Cases
- Injuries as a result of illegal Act
- Treatment in any sanatorium, nursing home or long term care facility that is not a hospital
- Expenses directly or indirectly resulting from birth defects, congenital defects/illness and deformities of any nature
- Experimental or unproven treatment

- Dental examinations, X-ray, extraction, and orthodontic treatment or oral surgery except as a result of accidental injury
- Pre existing diseases
- Routine medical examinations, routine eye or ear checkup, Vaccination, Contact lenses, hearing aids and examination for travel or employment
- Donor for organ transplant claims
- Air ambulance expenses
- Cost of limbs or supporting equipment for revival or correction of the function's of body
- Cost of medicines for cosmetic treatment and or treatment of falling hair or hair implant cosmetic/ Plastic surgery

Personal Accident Insurance
(Only for the Earning Members of the Family / Head of Household)

Benefits for Personal Accidents

• Death	100%
• Permanent Disability of both eyes	100%
• Permanent Disability of one eye and one limb	75%
• Permanent Disability of one eye or one limb	50%
• Permanent Disability of one finger or thumb or toe	10%

If at any time during any period of insurance, the Insured Persons shall sustain any bodily injury caused by accidental external and visible means which injury shall solely and independently of any other cause result in his death or permanent total disability, the Company will pay to the Insured Persons or in the event of his death, to his immediate Dependants, the compensation stated in the Schedule.

Provided always that:

1. Death or permanent disability (as mentioned in the Schedule of Compensation) takes place within three Calendar months of the occurrence of the injury.

a) Compensation shall not be payable under more than one of the items of the Schedule of compensation in respect of consequences of the same accident.

2. The total sum payable under Personal Accident of Policy in respect of any one or more claims shall not exceed in all, in any one period of insurance, the Sum Insured.

Death or permanent disability (as mentioned in the Schedule of Compensation) shall only be payable for the Earning Member of a Family and not to the rest of the Family Members. In case of Death, the Compensation shall be payable to the immediate Dependants only.

Exclusions for Accident Insurance

1. This policy does not cover death or bodily injury due to or resulting from:

    1. Intentional self injury, suicide or attempted suicide
    2. War, invasion, act of foreign enemy, hostilities, whether war will be declared or not, civil war, mutiny, rebellion, revolution, insurrection, strike, riot, civil commotion, military or usurped power.
    3. The insured being under the influence of, or being affected by intoxicating liquor or drug or drugs, venereal disease
    4. The insured engaging in flying or air travel of any kind (other than as a passenger in any fully licensed passenger carrying aircraft and/or helicopter but not as a member of the crew and not for purpose of undertaking any trade or technical operation therein), polo, steeple chasing, professional football, mountaineering necessitating the use of ropes or guides, winter sports, riding or driving in any kind of race speed or during test or practice thereof, or occupational use of power driven woodworking machinery.

In case of natural death of client Rs. 15000/-would also pay to deceased family if he /she availed loan facility.

Funeral expense coverage is only for client who availed loan facility.

Claim Settlement Procedure

i) The Insured shall contact the NRSP field officer for hospitalization, who will guide the patient to nearest Government and/or Armed forces hospitals and/or recognized private hospital, except in case of emergency, wherein at a later date NRSP officer shall be informed.

ii) The Insured on discharge from hospital shall pay all medical bills and/or charges and then submit the claim to NRSP field Officer for reimbursement.

iii) The claim shall be verified and subsequently be sent by NRSP fortnightly from date of receipt from Insured Person to the Company along with following documents;
  a) Filled-in claim form
  b) Hospital discharge report
  c) Original hospital bills
  d) Laboratory reports (Copies)
  e) Medicine Invoices along with doctor's prescription
  f) Copy of Premium Receipt
  g) Copy of Insured's National Identity Card
  h) Other supporting documents

The Company shall process the claim and send the cheque in the name of NRSP within 15 days, provided all documents are in order.

i) The injured Insured/immediate Dependants shall contact the NRSP for compensation.

ii) The NRSP field staff shall guide the Insured/ immediate Dependants to submit the claim along with the following documents within Seven days of incident.
  a) Death Certificate verified
  b) Police Report / Roznamcha
  c) Doctor's report on disability / death
  d) Copy of National Identity Card of Assured ('Form-B' in case of young children)
  e) Copy of National Identity Card of immediate Dependants in case of Death.
  f) Copy of Premium Receipt

iii) The Field staff shall forward the attested copies of all the above documents, except Police Report (required in original) within Seven days of receipt from Insured Person to the Company, which shall process the claim.

iv) Cheque shall be sent in the name of NRSP within 15 days, provided all documents are in order.

5. Conclusion

This dissertation analyzes the risk reduction potential of formal insurance and informal risk-sharing through solidarity transfers. Due to limited observability of informal transfers results on solidarity rely on behavioral experiments to measure pro-social behavior. Such incentivized economic games promise more reliable measures than purely hypothetical statements in surveys (see e.g. Holm and Nystedt 2008; Chang, Lusk, and Norwood 2009). Other data is collected from household surveys. For the analysis of causal effects I exploit experimental variation within the behavioral experiments as well as randomized control trials in the field.

The three chapters reveal multiple aspects of insurance in developing countries. First of all, there exists considerable solidarity towards fellow villagers as shown in case of the Philippines. This solidarity exists independent of reputation, reciprocal rewards or punishments and is stronger towards friends and family members. The results of chapter 2 suggest that trust and norms of cooperation play a large role for solidarity transfers. There also seems to be an interesting connection between collective activity in village organizations and trust or norms of cooperation. While activity in associations and cooperatives seems to relate to generalized trust, local political activity seems to relate more to a conditional type of cooperation. The substantial average level of transfers shows that – at least with idiosyncratic shocks – there is a high potential for informal insurance. Note that different to the following chapter only transfers from the better-off to the worse-off are considered. The reason is that these transfers are much more in line with the definition of solidarity and understanding drivers of solidarity is the main goal of this chapter.

In Chapter 3 transfers from the worse-off to the better-off are considered in the analysis as well, as they should also influence distributional outcomes (the main outcome of interest here). The research shows that informal solidarity networks are sensitive to the availability of formal insurance. with insurance products in place, villagers are less willing to help – even if recipients remain uninsured. This negative side effect to a large extent limits the efficiency of insurance products in the behavioral experiment. The situation is different if villagers can effectively hide their resources. In this case solidarity transfers break down and formal insurance schemes provide alternative economic protection. Note that the insurance discussed at this point focusses on idiosyncratic shocks. Examples would be illness/injury, damage of property, death of family members or theft. If the whole village is affected (for example by weather shocks) informal risk-sharing cannot work. Here, only risk pooling on higher levels via formal insurance schemes can offer protection.

The last chapter shows that formal insurance in Pakistan can decrease child labor outcomes considerably and that insurance coverage of additional household members rather than insurance awareness campaigns causes these effects. Increasing the feeling of protection seems to play a key role in explaining the results because most of the effect is caused without any insurance payout taking place. This is plausible because in Pakistan the poor face potentially catastrophic health costs and because there is no effective coverage with social or health insurance. Therefore, offering affordable health insurance to formerly uninsured household members will have large psychological effects. Given these high effects and the type of intervention (offering insurance to a normally uncovered *part* of the household), there seems to be an especially high effect of completing coverage, i.e. eliminating or minimizing the risk of catastrophic spending.

In sum, there seem to be contradicting aspects in formal insurance. On the one hand it negatively affects motivation of peers to help. This might actually be the reason for the difference between developing and industrialized countries with an established insurance system. In the former financial or in-kind transfers are common amongst peers and part of everyday life; in the latter we observe much less such transfers, maybe except in parent-child relationships.[112] This absence of economic transfers might be related to a much lower feeling of responsibility caused by existing insurance possibilities. In this sense our results could very well reflect the development taking place during a transition to a developed country.

On the other hand, despite the potential of informal solidarity, formal insurance might lend a better feeling of protection. This makes sense because solidarity transfers are always voluntary and therefore do not arrive automatically (insurance payouts should). In the case of Pakistan, households strongly react to increased health insurance coverage by reducing harmful risk mitigation strategies and sending children less often to work. Also, less solidarity transfers do not have to be a purely negative aspect. Households might also feel less under pressure by the expectations of their peers to help. Such ‘forced solidarity’ might have negative consequences as well, such as decreasing incentives for economic success (Grimm et al. 2010; Hadnes, Vollan, and Kosfeld 2012). The result from chapter 3 supports this aspect because individuals often hide income whenever possible to avoid high expectations of peers and then reduce solidarity transfers.

[112] “In developing and transition countries, inter-household transfers are typically larger and much more common than in Western countries.” (Mitrut and Nordblom 2010, 999)

Before deriving policy implications, let us revisit the reasons for the negative side effects of insurance in the context of solidarity networks. The biggest factor limiting an effective protection of the population by insurance in our experiment is that some individuals chose not to be covered. They bear a large risk of falling into poverty in case of a shock. Mandatory coverage of all individuals would make the negative crowding-out effect on solidarity less relevant (as shown in chapter 3). Comprehensive coverage of all individuals also promises to maximize the feeling of protection by households (according to chapter 4). In this sense the joint results suggest that the state should either provide insurance for all citizens or make private insurance mandatory.

So far I have not discussed other undesirable consequences that a reduction in solidarity could have, for example reduced collective action or social capital in general. While these further consequences remain speculative, they still highlight that there might be larger benefits from avoiding crowding-out effects during a transformation to a more formalized insurance system. Potential schemes that could foster solidarity are group or community-based arrangements. Here, the decision to buy insurance is taken jointly, thus preserving the spirit of communality. Joint take-up could also ensure that on the group level all individuals are insured. Additionally, one could think about group insurance that focuses on covariate shocks only, with the explicit group commitment to help those with an idiosyncratic shock (Clarke 2011). Such designs at least theoretically promise to combine the advantages of informal risk-sharing and a big formal risk pool. However, we need more research to generate reliable evidence on the actual efficiency of such group insurance schemes.

References

Adjakidje, Didier, Janvier Metangmo, and Komi A Segla. 2010. “Capital Social Et Pauvreté Au Cameroun.”

Albarran, P, and OP Attanasio. 2003. “Limited Commitment and Crowding Out of Private Transfers: Evidence from a Randomised Experiment.” *The Economic Journal* 113 (486): C77–C85.

Anderson, Siwan, and Jean Marie Baland. 2002. “The Economics of Roscas and Intrahousehold Resource Allocation.” *Quarterly Journal of Economics* 117 (3): 963–995.

Ashraf, Nava, Dean Karlan, and Wesley Yin. 2006. “Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines.” *Quarterly Journal of Economics* 121 (2): 635–672.

Asian Development Bank. 2004. *Social Protection Strategy. Final Report Vol . II : Health Insurance*. http://www2.adb.org/Documents/Reports/Consultant/37008-PAK/vol2/37008b.pdf.

———. 2005. *Technical Assistance to the Islamic Republic of Pakistan for the Developing Social Health Insurance Project*. http://www2.adb.org/Documents/TARs/PAK/tar-pak-37359.pdf.

Attanasio, Orazio, and José-Víctor Ríos-Rull. 2000. “Consumption Smoothing in Island Economies: Can Public Insurance Reduce Welfare?” *European Economic Review* 44 (7): 1225–1258.

Baland, JM, and JA Robinson. 2000. “Is Child Labor Inefficient?” *Journal of Political Economy* 108 (4): 663–679.

Banerjee, A.V., and Esther Duflo. 2007. “The Economic Lives of the Poor.” *Journal of Economic Perspectives* 21 (1): 141–167.

Barr, Abigail, and Garance Genicot. 2008. “Risk Sharing, Commitment, and Information: An Experimental Analysis.” *Journal of the European Economic Association* 6 (6): 1151–1185.

Basu, Kaushik, and Pham Hoang Van. 1998. “The Economics of Child Labor.” *American Economic Review* 289 (4): 84–91.

Beegle, Kathleen, Rajeev H. Dehejia, and Roberta Gatti. 2006. “Child Labor and Agricultural Shocks.” *Journal of Development Economics* 81 (1): 80–96.

Benz, Matthias, and Stephan Meier. 2008. “Do People Behave in Experiments as in the Field?—evidence from Donations.” *Experimental Economics* 11 (3): 268–281.

Besley, Timothy. 1995. “Savings, Credit and Insurance.” *Handbook of Development Economics* 3: 2123–2207.

Binswanger, H.P. 1980. “Attitudes Toward Risk: Experimental Measurement in Rural India.” *American Journal of Agricultural Economics* 62 (3): 395–407.

Bjørnskov, Christian. 2006. “The Multiple Facets of Social Capital.” *European Journal of Political Economy* 22 (1): 22–40.

Bolton, Gary E, and Axel Ockenfels. 2000. “ERC: A Theory of Equity , Reciprocity , and Competition.” *American Economic Review* 90 (1): 166–193.

Bowles, Samuel. 2008. “Policies Designed for Self-interested Citizens May Undermine ‘the Moral Sentiments’: Evidence from Economic Experiments.” *Science* 320 (5883): 1605–1609.

Brehm, J, and W Rahn. 1997. “Individual-Level Evidence for the Causes and Consequences of Social Capital.” *American Journal of Political Science* 41 (3): 999–1023.

Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang. 2011. “Commitments to Save: A Field Experiment in Rural Malawi.” *World Bank Policy Research Working Paper* 5748.

Cardenas, Juan Camilo, and Jeffrey Carpenter. 2008. “Behavioural Development Economics: Lessons from Field Labs in the Developing World.” *Journal of Development Studies* 44 (3): 311–338.

Chakrabarty, Sayan. 2012. “Does Micro Credit Increase Child Labour in Absence of Micro Insurance?” *Microinsurance Innovation Facility Research Paper* 12.

Chandrasekhar, Arun G., Cynthia Kinnan, and Horacio Larreguy. 2012. “Informal Insurance, Social Networks, and Savings Access: Evidence from a Framed Field Experiment.” http://faculty.wcas.northwestern.edu/~cgk281/SaI.pdf.

Chang, Jae Bong, Jayson L Lusk, and F Bailey Norwood. 2009. “How Closely Do Hypothetical Surveys and Laboratory Experiments Predict Field Behavior?” *American Journal of Agricultural Economics* 91 (2): 518–534.

Charness, Gary, and Matthew Rabin. 2002. “Understanding Social Preferences with Simple Tests.” *Quarterly Journal of Economics* 117 (3): 817–869.

Cherry, Todd L, Stephan Kroll, and Jason F Shogren. 2005. “The Impact of Endowment Heterogeneity and Origin on Public Good Contributions: Evidence from the Lab.” *Journal of Economic Behavior & Organization* 57 (3): 357–365.

Churchill, Craig, and Michal Matul. 2012. *Protecting the Poor: A Microinsurance Compendium. Vol. II*. Geneva: International Labour Organization.

Clark, Jeremy. 2002. “House Money Effects in Public Good Experiments.” *Experimental Economics* 5 (3): 223–232.

Clarke, Daniel J. 2011. “Reinsuring the Poor: Group Microinsurance Design and Costly State Verification.” In *Insurance Design for Developing Countries (PhD Thesis)*, ed. Daniel J. Clarke, 95–132. University of Oxford. http://www.stats.ox.ac.uk/~clarke/pdf/DanielClarkeDPhilThesis.pdf.

Coate, Stephen, and M. Ravallion. 1993. “Reciprocity Without Commitment: Characterization and Performance of Informal Insurance Arrangements.” *Journal of Development Economics* 40 (1): 1–24.

Cole, Shawn, Xavier Giné, Jeremy Tobacman, Petia B. Topalova, Robert M. Townsend, and James I. Vickery. 2009. “Barriers to Household Risk Management: Evidence from India.” *Harvard Business School Working Paper* 09-116.

Coleman, James S. 1988. “Social Capital in the Creation of Human Capital.” *American Journal of Sociology* 94 (Supplement: Organizations and Institutions: Sociological and Economic Approaches to the Analysis of Social Structure).

Comola, Margherita, and Marcel Fafchamps. 2010. “Are Gifts and Loans Between Households Voluntary?” *CSAE Working Paper* 2010-20.

Dekker, Marleen, and Annegien Wilms. 2010. “Health Insurance and Other Risk-Coping Strategies in Uganda: The Case of Microcare Insurance Ltd.” *World Development* 38 (3): 369–378.

Dercon, Stefan, and Luc Christiaensen. 2011. “Consumption Risk, Technology Adoption and Poverty Traps: Evidence from Ethiopia.” *Journal of Development Economics* 96 (2): 159–173.

Dercon, Stefan, and Pramila Krishnan. 2003. “Risk Sharing and Public Transfers.” *The Economic Journal* 113 (486): C86–C94.

Dror, David Mark, Ruth Koren, and David Mark Steinberg. 2006. “The Impact of Filipino Micro Health-insurance Units on Income-related Equality of Access to Healthcare.” *Health Policy* 77 (3): 304–17.

Dufwenberg, Martin, and Georg Kirchsteiger. 2004. “A Theory of Sequential Reciprocity.” *Games and Economic Behavior* 47 (2): 268–298.

Dupas, P., and Jonathan Robinson. 2009. “Savings Constraints and Microenterprise Development: Evidence from a Field Experiment in Kenya.” *NBER Working Paper* 14693.

Duryea, Suzanne, David Lam, and Deborah Levison. 2007. “Effects of Economic Shocks on Children’s Employment and Schooling in Brazil.” *Journal of Development Economics* 84 (1): 188–214.

Edmonds, Eric V. 2008. “Child Labor.” In *Handbook of Development Economics Volume 4*, ed. T P Schultz and J Strauss, 3607–3709. Amsterdam: Elsevier.

Emerson, Patrick M, and André Portela Souza. 2011. "Is Child Labor Harmful? The Impact of Working Earlier in Life on Adult Earnings." *Economic Development and Cultural Change* 59 (2): 345–385.

Engelmann, Dirk, and Urs Fischbacher. 2009. "Indirect Reciprocity and Strategic Reputation Building in an Experimental Helping Game." *Games and Economic Behavior* 67 (2): 399–407.

Estevan, Fernanda, and Jean-Marie Baland. 2007. "Mortality Risks, Education and Child Labor." *Journal of Development Economics* 84 (1): 118–137.

Fafchamps, Marcel. 2008. "Risk Sharing Between Households." In *Handbook of Social Economics*, ed. Jess Benhabib, Alberto Bisin, and Matthew O. Jackson, 1–42. Elsevier.

Fafchamps, Marcel, and Flore Gubert. 2007a. "Contingent Loan Repayment in the Philippines." *Economic Development and Cultural Change* 55 (4): 633–667.

———. 2007b. "The Formation of Risk Sharing Networks." *Journal of Development Economics* 83 (2): 326–350.

Fafchamps, Marcel, and Susan Lund. 2003. "Risk-sharing Networks in Rural Philippines." *Journal of Development Economics* 71 (2): 261–287.

Fehr, Ernst, and Andreas Leibbrandt. 2011. "A Field Study on Cooperativeness and Impatience in the Tragedy of the Commons." *Journal of Public Economics* 95 (9-10): 1144–1155.

Fehr, Ernst, and Klaius M. Schmidt. 1999. "A Theory of Fairness, Competition, and Cooperation." *Quarterly Journal of Economics* 114 (3): 817–868.

Feigenberg, Benjamin, Erica M Field, and Rohini Pande. 2010. "Building Social Capital Through Microfinance." http://www.nber.org/papers/w16018.

Fischbacher, Urs, Simon Gachter, and Ernst Fehr. 2001. "Are People Conditionally Cooperative ? Evidence from a Public Goods Experiment." *Economics Letters* 71: 397–404.

Flory, Jeffrey A. 2011. "Modern Institutions & Pre-Modern Safety Nets: Indirect Effects of Formal Savings Expansion on the 'Unbanked' and Ultra-Poor." http://www.aae.wisc.edu/mwiedc/papers/2011/Flory_Jeff.pdf.

Freitag, Markus, Nicolas Grießhaber, and Richard Traunmüller. 2009. "Vereine Als Schulen Des Vertrauens ? Eine Empirische Analyse Zur Zivilgesellschaft in Der Schweiz." *Swiss Political Science Review* 15 (3): 495–527.

Freitag, Markus, and Richard Traunmüller. 2009. "Spheres of Trust: An Empirical Analysis of the Foundations of Particularised and Generalised Trust." *European Journal of Political Research* 48 (6): 782–803.

Gächter, Simon, and Christian Thöni. 2005. "Social Learning and Voluntary Cooperation Among Like-Minded People." *Journal of the European Economic Association* 3 (2): 303–314.

Giesbert, Lena, and Susan Steiner. 2011. "Perceptions of (Micro)Insurance in Southern Ghana: The Role of Information and Peer Effects." *GIGA Working Paper* 183.

Giné, Xavier, and Dean Yang. 2009. "Insurance, Credit, and Technology Adoption: Field Experimental Evidence from Malawi." *Journal of Development Economics* 89 (1): 1–11.

Gneezy, U., and A. Rustichini. 2000. "A Fine Is a Price." *The Journal of Legal Studies* 29 (1): 1–17.

Grimard, Franque. 1997. "Household Consumption Smoothing Through Ethnic Ties: Evidence from Cote d'Ivoire." *Journal of Development Economics* 53 (2): 391–422.

Grimm, Michael, Flore Gubert, Ousman Koriko, Jann Lay, and Christophe J. Nordman. 2010. "Does Forced Solidarity Hamper Entrepreneurial Activity? Evidence from Seven West-African Countries." http://www.iza.org/conference_files/worldb2010/grimm_m4353.pdf.

Grimm, Michael, Flore Gubert, Jann Lay, and Christophe J Nordman. 2011. "Kinship-ties and Entrepreneurship in Western Africa." http://campus.iss.nl/~grimm/ForcedSolidarity_V2.pdf.

Gunnarsson, Victoria, Peter F. Orazem, and Mario A. Sánchez. 2006. "Child Labor and School Achievement in Latin America." *The World Bank Economic Review* 20 (1): 31–54.

Gunnthorsdottir, A, D Houser, and K Mccabe. 2007. "Disposition, History and Contributions in Public Goods Experiments." *Journal of Economic Behavior & Organization* 62 (2): 304–315.

Hadnes, Myriam, Björn Vollan, and Michael Kosfeld. 2012. "The Dark Side of Solidarity." *Mimeo*.

Harrison, Glenn W, and John A List. 2004. "Field Experiments." *Journal of Economic Literature* 42 (4): 1009–1055.

Harrison, Glenn W, and E.E. Rutström. 2008. "Risk Aversion in the Laboratory." *Research in Experimental Economics* 12: 41–196.

Hayo, Bernd, and Björn Vollan. 2012. "Group Interaction, Heterogeneity, Rules, and Co-operative Behaviour:, Evidence from a Common-Pool Resource Experiment, in South Africa and Namibia." *Journal of Economic Behavior & Organization* 81 (1): 9–28.

Heady, Christopher. 2003. "The Effect of Child Labor on Learning Achievement." *Word Development* 31 (2): 385–398.

Hoff, K, and A Sen. 2006. "The Kin System as a Poverty Trap?" In *Poverty Traps*, ed. Samuel Bowles, Steven N. Durlauf, and Karla Hoff, 95–115. Princeton, Woodstock, New York: Princeton University Press.

Holm, Hakan, and Paul Nystedt. 2008. “Trust in Surveys and games--A Methodological Contribution on the Influence of Money and Location.” *Journal of Economic Psychology* 29 (4): 522–542.

Holt, Charles A, and Susan K Laury. 2002. “Risk Aversion and Incentive Effects.” *American Economic Review* 92 (5): 1644–1655.

Ito, Seiro, and H. Kono. 2010. “Why Is the Take-up of Microinsurance so Low? Evidence from a Health Insurance Scheme in India.” *The Developing Economies* 48 (1): 74–101.

Jakiela, Pamela, and Owen Ozier. 2011. “Does Africa Need a Rotten Kin Theorem ? Experimental Evidence from Village Economies.” http://www.artsci.wustl.edu/~pjakiela/jakielaozier15jan11.pdf.

Jensen, Robert T. 2003. “Do Private Transfers ‘displace’ the Benefits of Public Transfers? Evidence from South Africa.” *Journal of Public Economics* 88 (1-2) (January): 89–112.

Jowett, Matthew. 2003. “Do Informal Risk Sharing Networks Crowd Out Public Voluntary Health Insurance? Evidence from Vietnam.” *Applied Economics* 35 (10): 1153–1161.

Jütting, Johannes P. 2004. “Do Community-based Health Insurance Schemes Improve Poor People’s Access to Health Care? Evidence From Rural Senegal.” *World Development* 32 (2): 273–288.

Kahneman, Daniel, and Amos Tversky. 1979. “Prospect Theory: An Analysis of Decision Under Risk.” *Econometrica* 47 (2): 263–291.

Karlan, Dean S. 2005. “Using Experimental Economics to Measure Social Capital and Predict Financial Decisions.” *American Economic Review* 95 (5): 1688–1699.

Kassouf, Ana Lúcia, Martin McKee, and Elias Mossialos. 2001. “Early Entrance to the Job Market and Its Effect on Adult Health: Evidence from Brazil.” *Health Policy and Planning* 16 (1): 21–28.

Kinnan, Cynthia. 2011. “Distinguishing Barriers to Insurance in Thai Villages”: 1–52. http://faculty.wcas.northwestern.edu/~cgk281/BtI.pdf.

Kocher, M, T Cherry, S Kroll, R Netzer, and M Sutter. 2008. “Conditional Cooperation on Three Continents.” *Economics Letters* 101 (3): 175–178.

Kocher, M, P Martinsson, and M Visser. 2008. “Does Stake Size Matter for Cooperation and Punishment?” *Economics Letters* 99 (3): 508–511.

Kocherlakota, Narayana R. 1996. “Implications of Efficient Risk Sharing Without Commitment.” *The Review of Economic Studies* 63 (4): 595–609.

Kurzban, Robert, and Daniel Houser. 2005. “Experiments Investigating Cooperative Types in Humans: a Complement to Evolutionary Theory and Simulations.” *Proceedings of the National Academy of Sciences of the United States of America* 102 (5): 1803–1807.

Landmann, Andreas, Björn Vollan, and Markus Frölich. 2012. "Insurance Versus Savings for the Poor: Why One Should Offer Either Both or None." *IZA Discussion Paper* 6298.

Leider, S., M.M. Möbius, T. Rosenblat, and Q.A. Do. 2009. "Directed Altruism and Enforced Reciprocity in Social Networks." *Quarterly Journal of Economics* 124 (4): 1815–1851.

Ligon, Ethan. 1998. "Risk Sharing and Information in Village Economies." *The Review of Economic Studies* 65 (4): 847–864.

Ligon, Ethan, and Laura Schechter. 2012. "Motives for Sharing in Social Networks." *Journal of Development Economics* 99 (1) (December): 13–26.

Ligon, Ethan, and Laura Schlechter. 2010. "Motives for Sharing in Social Networks." *Unpublished Manuscript*: 1–34. http://www.aae.wisc.edu/schechter/network_motives.pdf.

Ligon, Ethan, Jonathan P. Thomas, and Tim Worrall. 2000. "Mutual Insurance, Individual Savings, and Limited Commitment." *Review of Economic Dynamics* 3 (2) (April): 216–246.

———. 2002. "Informal Insurance Arrangements with Limited Commitment: Theory and Evidence from Village Economies." *Review of Economic Studies* 69 (1) (January): 209–244.

Lin, N. 2001. *Social Capital: A Theory of Social Structure and Action*. *Structural Analysis in the Social Sciences*. Vol. 19. Cambridge University Press.

List, John A. 2006. "The Behavioralist Meets the Market: Measuring Social Preferences and Reputation Effects in Actual Transactions." *Journal of Political Economy* 114 (1): 1–37.

Mitrut, Andreea, and Katarina Nordblom. 2010. "Social Norms and Gift Behavior: Theory and Evidence from Romania." *European Economic Review* 54 (8): 998–1015.

Morduch, Jonathan. 1999. "Between the State and the Market: Can Informal Insurance Patch the Safety Net?" *The World Bank Research Observer* 14 (2): 187–207.

———. 2002. "Consumption Smoothing Across Space: Testing Theories of Risk-sharing in the ICRISAT Study Region of South India."

Oberholzer-Gee, Felix, Joel Waldfogel, and Matthew W. White. 2010. "Friend or Foe? Cooperation and Learning in High-stakes Games." *The Review of Economics* 92 (1): 179–187.

Ones, Umut, and Louis Putterman. 2007. "The Ecology of Collective Action: A Public Goods and Sanctions Experiment with Controlled Group Formation." *Journal of Economic Behavior & Organization* 62 (4): 495–521.

Pakistan Ministry of Health. 2009. *National Health Policy 2009: Stepping Towards Better Health (draft 19 Feb 2009)*. www.pc.gov.pk/Policies/Health.doc.

Paxton, Pamela Marie. 2002. “Social Capital and Democracy: An Interdependent Relationship.” *American Sociological Review* 67 (2): 254–277.

———. 2007. “Association Memberships and Generalized Trust: A Multilevel Model Across 31 Countries.” *Social Forces* 86 (1): 47–76.

Platteau, Jean-Philippe. 2000. *Institutions, Social Norms and Economic Development*. Amsterdam: Routledge.

Portes, A. 1998. “Social Capital: Its Origins and Applications in Modern Sociology.” *Annual Review of Sociology* 24 (1): 1–24.

Pouliot, William. 2006. “Introducing Uncertainty into Baland and Robinson’s Model of Child Labour.” *Journal of Development Economics* 79 (1): 264–272.

Putnam, Robert D. 1995. “Bowling Alone: America’s Declining Social Capital.” *Journal Of Democracy* 6 (1): 65–78.

———. 2000. *Bowling Alone: The Collapse and Revival of American Community*. New York, NY: Simon & Schuster.

Qamar, Karim Khan, Peter Knoll, James Roth, and Ayesha Tayyab. 2007. *The Beginning of Health Microinsurance in Pakistan. A Review of RSPN-Adamjee Insurance Scheme*.

Rabin, Matthew. 1993. “Incorporating Fairness into Game Theory and Economics.” *American Economic Review* 83 (5): 1281–1302.

Ranjan, Priya. 2001. “Credit Constraints and the Phenomenon of Child Labor.” *Journal of Development Economics* 64 (1): 81–102.

Rosati, Furio Camillo, and Mariacristina Rossi. 2003. “Children’s Working Hours and School Enrollment: Evidence from Pakistan and Nicaragua.” *The World Bank Economic Review* 17 (2): 283–295.

Rosenzweig, Mark R., and Hans P. Binswanger. 1993. “Wealth, Weather Risk and the Composition and Profitability of Agricultural Investments.” *The Economic Journal* 103 (416): 56–78.

Roth, Jim, Michael J. McCord, and Dominic Liber. 2007. *The Landscape of Microinsurance in the World’s 100 Poorest Countries*. http://www.microinsurancecentre.org/resources/documents/doc_download/634-the-landscape-of-microinsurance-in-the-worlds-100-poorest-countries-in-english.html.

Rural Support Programmes Network. 2012. “Outreach.” *The Rural Support Programmes Social Mobilisation Newsletter* 15. http://www.rspn.org/Outreach/PDFs/outreach_issue_15.pdf.

Rustagi, Devesh, Stefanie Engel, and Michael Kosfeld. 2010. “Conditional Cooperation and Costly Monitoring Explain Success in Forest Commons Management.” *Science* 330 (6006): 961–965.

Selten, R, and A. Ockenfels. 1998. “An Experimental Solidarity Game.” *Journal of Economic Behavior & Organization* 34 (4): 517–539.

Sonderskov, K. M. 2010. “Does Generalized Social Trust Lead to Associational Membership? Unravelling a Bowl of Well-Tossed Spaghetti.” *European Sociological Review* 27 (4): 419–434.

Spencer, R W, and W P Yohe. 1970. “The Crowding Out of Private Expenditures by Fiscal Policy Actions.” *Federal Reserve Bank Of St Louis Review* 52 (Oct): 12–24.

Thaler, Richard H, and Eric J Johnson. 1990. “Gambling with the House Money and Trying to Break Even: The Effects of Prior Outcomes on Risky Choice.” *Management Science* 36 (6): 643–660.

Thöni, Christian, Jean-Robert Tyran, and Erik Wengström. 2012. “Microfoundations of Social Capital.” *Journal of Public Economics* 96 (7-8): 635–643.

Townsend, R.M. 1994. “Risk and Insurance in Village India.” *Econometrica* 62 (3): 539–591.

Traunmüller, R. 2011. “Moral Communities? Religion as a Source of Social Trust in a Multilevel Analysis of 97 German Regions.” *European Sociological Review* 27 (3): 346–363.

Traxler, Christian. 2010. “Social Norms and Conditional Cooperative Taxpayers.” *European Journal of Political Economy* 26 (1): 89–103.

Trhal, Nadja, and Ralf Radermacher. 2009. “Bad Luck Vs. Self-inflicted Neediness – An Experimental Investigation of Gift Giving in a Solidarity Game.” *Journal of Economic Psychology* 30 (4): 517–526.

Udry, Christopher. 1994. “Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria.” *The Review of Economic Studies* 61 (3): 495–526.

Uslaner, Eric M. 2002. *The Moral Foundations of Trust*. Cambridge University Press.

Vollan, Björn. 2011. “The Difference Between Kinship and Friendship: (Field-) Experimental Evidence on Trust and Punishment.” *Journal of Socio-Economics* 40 (1): 14–25.

Wagstaff, Adam. 2010. “Estimating Health Insurance Impacts Under Unobserved Heterogeneity: The Case of Vietnam’s Health Care Fund for the Poor.” *Health Economics* 19 (2): 189–208.

Wagstaff, Adam, Magnus Lindelow, Gao Jun, Xu Ling, and Qian Juncheng. 2009. “Extending Health Insurance to the Rural Population: An Impact Evaluation of China’s New Cooperative Medical Scheme.” *Journal of Health Economics* 28 (1): 1–19.

Wahhaj, Zaki. 2010. “Social Norms and Individual Savings in the Context of Informal Insurance.” *Journal of Economic Behavior & Organization* 76 (3) (December): 511–530.

World Bank. 2012. *Micro Insurance in Pakistan: A Diagnostic Study*. http://www.secp.gov.pk/corporatelaws/pdf/MI_Report_16102012.pdf.

Eidesstattliche Erklärung

Hiermit erkläre ich, die vorliegende Dissertation selbstständig angefertigt und mich keiner anderen als der in ihr angegebenen Hilfsmittel bedient zu haben. Insbesondere sind sämtliche Zitate aus anderen Quellen als solche gekennzeichnet und mit Quellenangaben versehen.

Mannheim, 08.04.2013

Andreas Landmann

Curriculum Vitae

2008 - 2013	PhD studies in Economics at the Center for Doctoral Studies in Economics, University of Mannheim, Germany Specialization: Development Economics, Applied Econometrics, Behavioral Economics Dissertation: *Essays on Formal Insurance and Informal Solidarity in Developing Countries*, Supervisor: Prof. Dr. Markus Frölich
2013	Visiting Scholar at University of Innsbruck, Austria
2001 – 2007	Diploma studies (M.A. equivalent) in economics at the University of Mannheim, Germany Specialization: Development Economics, Labor Economics Thesis: *Labor Market Regulation and Job Turnover in Latin American Countries*, Supervisor: Prof. Dr. Hartmut Sangmeister
2005 – 2006	Exchange student (M.A. program in Economics) at University of Toronto, Canada