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Calamity, Aid and Indirect Reciprocity: the Long Run Impact of Tsunami on Altruism in Sri Lanka

Leonardo Becchetti, Stefano Castriota* and Pierluigi Conzo**

Abstract

Natural disasters have been shown to produce effects on social capital, risk and time preferences of victims. We run experiments on altruistic preferences on a sample of Sri Lankan microfinance borrowers affected/unaffected by the tsunami shock in 2004 at a 7-year distance from the event (a distance longer than in most empirical studies). We find that people who suffered at least a damage from the event behave in dictator games less altruistically as senders (and expect less as receivers) than those who do not report any damage. Interestingly, among damaged, those who suffered also house damages or injuries send (expect) more than those reporting only losses to the economic activity. Since the former are shown to receive significantly more help than the latter we interpret this last finding as a form of indirect reciprocity.

JEL classification: C90, D03, O12.

Keywords: tsunami, disaster recovery, social preferences, altruism, development aid.

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Table of contents

1. Introduction

Natural disasters are dramatic shocks which produce severe consequences on at least two main economic dimensions. At macro level they cause widespread destruction of material wealth and capital stock - with consequent job losses - creating the premises for a following phase of reconstruction. At micro level they affect expectations, preferences and choices of economic agents with consequences on their consumption/saving and human capital investment decisions. A first and still on-going branch of the literature has mainly focused on empirical research at macro level (Skidmore, 2001; Toya and Skidmore, 2007; Kahn, 2005; Cuaresma et al. 2008; Noy, 2009) while, more recently, a new branch of empirical papers has started to analyse with experimental data the impact of calamities on individual preferences finding that natural (and manmade) disasters affect victims' discount rates, risk aversion and social capital. These studies have drawn conflicting conclusions even when they use similar research designs and/or they are implemented in the same contexts. For example, as far as natural calamities are concerned, Cassar et al. (2011) find that Thai tsunami victims became slightly more impatient since calamities lead to a restatement of how much the future is uncertain[1] while Callen's (2010) empirical findings go in the opposite direction documenting a significant reduction of impatience in the Sri Lankan victims of the same tsunami calamity. Whitt and Wilson (2007) find increased group cooperation among individuals who were evacuated from New Orleans to Houston shelters in the aftermath of Hurricane Katrina, while Solnit (2009) provides evidence that disasters are more often catalysts for social capital increase than for social order collapse. Castillo and Carter (2011) estimate the impact of the 1998 hurricane Mitch on altruism, trust and reciprocity on a sample of Honduran victims and find a non-linear effect of the severity of the shock on the mean and variance of behaviours---intermediate shocks help coordination around a higher equilibrium while extreme shocks undercut such cooperation. Fleming et al. (2011) show that people hit by the Chilean 2010 earthquake reveal significantly lower trustworthiness while Cassar et al. (2011) find that tsunami victims are more trusting, moderately more trustworthy and more risk-averse. Conversely, Eckel et al. (2009) document that survivors of Hurricane Katrina in New Orleans tended to act more risk lovingly in the short term while, similarly to Cassar et al. (2011), Willinger et al (2013) find that Indonesian villagers living in volcanic areas become more risk tolerant after exposure to a high level of threat with no clear-cut direction in the documented changes of time preferences. Similarly, Cameron and Shah (2011), register a significant increase in risk aversion among individuals who experienced a natural disaster in Indonesia because of the unrealistically higher probabilities of natural disasters reported by the survivors in the months following the event. When looking at manmade calamities, Voors et al. (2010) find that people exposed to violence in Burundi have higher discount rates. Cassar et al. (2013) document that victimization during the civil war Tajikistan lead to lower trust among people of the same village and, consistently with this result, Becchetti et al. (2014) find that violence suffered during the 2007 political outbreaks in Kenya negatively affects trustworthiness.

[1] See Vastfjall et al. (2008) for a psychological research on this issue using a sample of Swedish undergraduate students.

We intend to contribute originally to this debate in four main respects. First, we believe that, given the heterogeneous results in the literature, one of the most relevant contributions would be the attempt of interpreting such variability by considering a more complex pattern of relationships than that resulting from the simple extensive margin analysis (namely, victimization *vis-à-vis* non-victimization). We are aware that part of the heterogeneity in the existing results on catastrophes and social preferences may be well due to cultural differences and/or differences in experimental designs and methodologies. However, we argue that the factors which may help to explain such variation are, on the one hand, the degree of damage suffered and, on the other hand, the contextual recovery aid received by victimized villagers. We collect information on both the amount of damages and recovery aid received through a field survey and use it to explain the variation in individuals' giving and expected giving in a dictator game. In particular, we compare individuals not just on the basis of the damaged/non-damaged status but also - within the more homogeneous group of damaged - on the basis of the intensity of the aid and damage reported. Second, Callen (2010) and Cassar et al. (2011) collected data and ran experiments on the effect of tsunami respectively in mid 2007 and mid 2009, while our database refers to December 2011, seven years after the catastrophe. This longer time horizon allows us to capture longer run calamity and recovery effects on victims' preferences. Third, differently from both Callen (2010) and Cassar et al. (2011), by exploiting information on individuals' victimization status as well as on the intensity of damage and recovery aid within each village, we do not measure the impact of the shock at the village but at the individual level. This approach helps to reduce heterogeneity between the "treatment" (inhabitants of a village who are hit by the tsunami) and the control group (inhabitants of the same village who are not hit by the calamity). Fourth, both damaged and non-damaged in our study belong to a selected group of individuals borrowing from the same microfinance organisation (MFI). This implies that they share some common unobservable factors (i.e. sense of entrepreneurship, trustworthiness, which are typically out of reach for the experimenter and are the main suspect of self-selection). These factors helped them to pass the screening of the same MFI which has salient incentives to select only potentially successful borrowers. Furthermore, since the MFI under our scrutiny organises frequent borrower meetings (as many others traditionally do), we also reasonably assume that damaged and non-damaged individuals share similar cultural elements represented by the organization *ethos*.

The combination of these four original features also contributes to solve the identification problem arising from the impossibility of randomizing ex ante the calamity experience and give further support to a causal interpretation of the correlation between tsunami shock and social preferences. First, as documented in the descriptive and econometric analysis, limited differences on observables exist between damaged and non-damaged living in the same villages (and belonging to the same MFI). Second, differences on observables (and, arguably, on unobservables) are significantly reduced in the within-victims analysis when we look at the tsunami impact at the intensive margin and at the effect of the recovery assistance received. Finally, the causal interpretation of our analysis is supported also by the results of weighted least squares and instrumental variable estimations we implement as further robustness checks.

The main findings of the paper support the hypothesis that the shock affects participants' preferences even in the long run. First, the tsunami negatively affects altruism at the extensive margin since those who suffered at least one damage give and expect less than those who did not. Second, large recovery assistance indirectly increases altruism only when interacted with the number of damages. A plausible rationale for this effect hinges on the indirect reciprocity type of preferences of the participants more intensively hit by the tsunami. As documented in our data, survivors were in a harsh situation after the calamity, but experienced a great deal of generosity by local, national or international donors. The combination of a large damage and a substantial aid may have persistently shaped social preferences in terms of greater generosity towards (or expected generosity from) other neighbouring villagers. Therefore our results identify a possible virtuous interaction between high damages and a well-targeted recovery assistance which – by harnessing a form of positive indirect reciprocity – may counterbalance the negative and time-persistent impact of the calamity on altruistic preferences.

These findings are important at least for two main reasons. First, by interpreting the superior generosity of highly damaged/largely assisted victims as a form of indirect reciprocity we provide evidence of an indirect and non-material channel through which a properly targeted recovery aid can compensate for calamity driven loss in pro-social attitudes documented by a branch of the literature. Second, since we find both a negative (i.e. through damages) and positive (i.e. through recovery aid) effect of the calamity on altruism, our results may also reconcile the literature showing evidence of natural shocks being detrimental for social preferences (Fleming et al., 2011; Cassar et al. 2013; Becchetti et al., 2014) with those papers supporting instead a positive link between the two (Solnit, 2009; Whitt and Wilson, 2007; Cassar et al., 2011). Consider also that our study focuses on the long-run impact of a natural disaster so that all the results provided by the papers which analyse short-run effects are not necessarily inconsistent with ours.

The paper is divided into seven sections (introduction and conclusions included). In the second we show our research design. In the third section we present descriptive findings and in the fourth our research hypothesis. In the fifth and sixth sections we illustrate and comment our empirical findings. The seventh section concludes.

2. Research Design

In what follows we briefly sketch the historical scenario in which our research is carried out (subsection 2.1) and then enter into details of our experiment design (subsections 2.2 and 2.3).

2.1 The Background

Sri Lanka was severely hit by the 2004 tsunami. Over 1,000 kilometres of coast (two thirds of the country's coastline) were affected by the wave. The calamity caused dramatic human (over 35,000 dead and 443,000 displaced people) and economic losses (24,000 boats, 11,000

businesses and 88,500 houses damaged or destroyed). Several international organizations and NGOs stepped in to provide help and support. The specific characteristics of this event was that of affecting almost randomly individuals living at short distance from each other based on their location at the moment of the tsunami with respect to the waterline (see Figure 1). This unfortunate event therefore created a particularly favourable scenario to investigate the effects of calamities and aid on individual preferences in a quasi-experimental environment with reduced identification problems.

In November 2011 our research team carried out the field part of the study in Sri Lanka with the support of a local staff. From a list of borrowers of a local microfinance institution (Agro Micro Finance, hereon AMF) we randomly selected 380 borrowers. Out of those, with the help of the AMF staff, we identified a group of individuals hit by the 2004 tsunami and a group of them who were not.[2] Participants to our experiment originate from three villages located on the southern coast of Sri Lanka, namely Galle, Matara and Hambantota. As documented by Figure 2, the three chosen villages were only partially affected by the calamity and this gave us the opportunity to exploit such within-village heterogeneity. Hence, differently from the studies summarized in the introduction (in which all damaged people were selected from one village whereas all non-damaged from another village not exposed to the shock), we sampled both damaged and non-damaged participants within each village.[3]

We decided to carry out our analysis on a sample of borrowers from the same microfinance institution for two reasons. First, the initial screening by AMF (and/or potential self-selection into it) is likely to reduce heterogeneity between the two groups whose social preferences are to be compared, i.e. damaged vs. non-damaged borrowers. Second, AMF loan officers informed us about the damaged/non-damaged status of their borrowers before implementing the experiment. Thanks to this prior information, we were able to assign ex-ante participants to the two groups of damaged/non-damaged in each village and to avoid potential framing effects arising from asking players for their damaged/non-damaged status before the beginning of the game.[4] Moreover, as we argue below, our sample is not likely to suffer from post-tsunami migration because of the high incentives for damaged individuals to stay provided both by the incoming flows of aid and the concession of micro-loans at favourable conditions thanks to the AMF's portfolio recapitalization after the tsunami (Becchetti and Castriota, 2010 and 2011) .

2.2 The Experiment

The fieldwork was composed of three parts, i.e. in the order: i) an experimental session to elicit altruistic and risk preferences, ii) a socio-demographic survey, iii) a final lottery game to elicit (and control for) time preferences.

[2] The damaged/non damaged status of the borrowers is checked and confirmed in the ex post-experimental survey. Damaged are slightly oversampled with respect to the control sample (207 against 175) in order to have sufficient observations for the within damaged analysis.

[3] The distribution of damaged borrowers within villages is as follows: 65.8 percent in Galle, 54.5 percent in Matara and 44.5 percent in Hambantota.

[4] A borrower is classified as "damaged" if she/he suffered at least one type of physical or material harm from the 2004 tsunami (see Table 1 for details).

As far as the experimental session was concerned, we implemented two games, i.e. a "Dictator Game" (DG) and a "Risky Investment Game" (RG). We randomly alternated the two games to avoid order effects. For sake of consistency with the main objective of this study we describe here only the DG and report the detailed description of both the RG and the final lottery in Appendix A.[5] The DG is a standard and simple game largely adopted in the literature to elicit altruistic preferences in an incentive compatible way (see, for instance, Eckel and Grossman, 1996 and Engel, 2011). The game involves two players, a Sender (S) and a Receiver (R). Their true identity is not revealed so that no player can identify whom (s)he is playing with. S is endowed with 900 LKR (the equivalent of 5.74 €) and has to decide how much of it to send to R; R takes no actions in this game and receives the amount of money S has sent. According to the classic utility theory, the Sender maximum utility is reached by sending 0 LKR and keeping the whole endowment (900 LKR). Any Sender deviation from 0 can be interpreted as a measure of altruism. [6]

After participants make their choice, the game ends and they are asked to answer to questions concerning socio-demographic information, their social preferences[7], the damage they received in 2004 with respect to seven dimensions (i.e. personal injuries, injuries to family members, damages to house, economic activity, buildings/assets, working tools, raw materials) and the recovery aid they received soon after the tsunami with respect to eight dimensions (i.e. money, credit, food, medicines, raw materials, working tools, consumption, other)[8] Interviews and games were conducted house-by-house by three teams composed of a field-researcher and a translator. This scheme ensured the respect of the full anonymity condition since participants could not even recognize each other in the subjects pool as it usually happens in standard group laboratory experiments. Translators were intensively trained on the questionnaire, the game and standard experimental protocol; the project did not begin until a satisfactory level of comprehension was reached.

2.3. The protocol

In the experiment participants were told about the sequence of the interview process, i.e. an experimental session composed of two games, a survey and a final lottery. Participants were informed they would be paid for just one randomly extracted game. The game was extracted before playing so that their decisions in the game did not affect the selection of the paid game. In particular, the randomly extracted game was contained in a sealed envelope shown to participants before the beginning of the experimental session.

[5] Note as well that, as outlined below, we use time and risk preferences as controls since we did not find any significant long-run effects of the tsunami-damages and/or recovery aid on the participants' behaviour in the RG and in the lottery-game.

[6] A recent meta-paper of Engel (2011) actually shows that departures from the self-interested benchmark are huge. Using data from 328 different dictator game experiments for a total of 20,813 observations the author finds that the share of individuals following Nash rationality is around 36 percent. The share of dictators giving zero falls to 28 percent if the endowment property rights are of the recipient and the dictator may take from her/him, 25 percent if players handle real money in the game, and 19 percent if the recipient is deserving (ie. is identified as poor). It falls further for adult or elder dictators.

[7] We used some standard GSS questions on social capital. See the questionnaire in the Appendix B.

[8] See Table 1 and Appendix B for further details.

As far as the DG was concerned, the participant was told that, if that game was extracted for payment, (s)he could earn real money (up to 900 LKR) according to her/his own or the matched counterpart's choices in the game. Then the game was explained and the participant was informed on her/his role, i.e. S or R[9]. If the participant was selected to be S, (s)he was endowed with 900 LRK and had to decide how much out of it to send to another player in the village. If the participant was chosen to play as R, (s)he was first showed a close envelope containing the answer sheet of the S-player (s)he was randomly matched with; then (s)he was informed that no choice was required for that role and we elicited her/his First Order Beliefs (FOBs), i.e. how much (s)he thought S had sent to her/him (we paid 50 LKR for a correct guess). Each participant was aware that her/his identity was unknown to the assigned counterpart. The protocol was similar for the RG (except from the matched-player answer sheet since no roles were involved in this game).

After the participants' choices in the games were made, the experimental session ended; then the socio-demographic survey was delivered and, finally, the lottery-game was implemented. We decided to pay at the end of the interview process (i.e. when decisions were no longer required) in order to avoid potential confounding effects of pay-off revelation at later stages of the interview. More specifically, when the whole interview process ended, S-players were given the amount of money they decided to keep if the DG was selected for payment; R-players were shown the answer sheet of the matched partner and paid accordingly[10]. See Appendix A for details on the payment procedure for the RG and the lottery game.

We believe in truthful reporting since the amount at stake is very large considering participants' standards of living. Even if we ignore the payment from the lottery, the maximum payoff from one of the games (900 LKR) represents in our sample about 51% of the median per capita monthly food expenditure.

3. Descriptive findings

Summary statistics of our sample document that participants' age is 47 on average while the gender split (most of our participants are women)[11] reproduces that of some of the main

[9] We kept the wording neutral in all games in order to avoid framing effects (for instance, we never presented the game as a "dictator game", but we rather called it "DG". Roles were phrased as "player 1" and "player 2" respectively for S and R).

[10] We interviewed first S-players and then R-players in order to make this payment procedure feasible.

[11] A potential weakness of our strategy aimed at reducing heterogeneity by relying on a sample of microfinance borrowers is the prevalent female composition. Thus, it could be argued that our results are not generalizable to the whole population but are valid only for the female gender. As summarized in the literature review provided by Croson and Gneezy (2009), a number of studies has shown that on average in Dictator Games women give more than men. However, women appear to be more sensitive to the experiment setting, therefore these results are to be taken with care. Furthermore, Ben-Ner, Kong and Putterman (2004) run dictator games with men and women and find that (i) when the experiment is blind there is no gender difference in the giving amount but (ii) when the gender of the partner is known women give to women less than they give to men. Therefore, since our sample is composed mainly by women, even if our results in the dictator game were magnified by the prevalence of females who on average are more generous, this would have been counterbalanced by a lower inter-gender generosity.

microfinance organizations in Asia (Panel A, Table 2).[12] The average number of household members is 4.5. The majority of our sample participants is married (83 percent) and the average number of schooling years is 10.5 (two and a half years of secondary school). Slightly more than half of them (54 percent) suffered from at least one type of damage from the tsunami (variable *damaged*). Panel B in Table 1 reports the distribution of the recovery aid in the full sample. Most individuals received food (19 percent), money or medicines (respectively 15 and 16 percent), and tools (14 percent). The sample mean of the standardized sum of aid types is 0.113 (variable *helpindex*) with 34 percent of people receiving an amount of help above this number (variable *help_ab_med*). As shown in Panel C of Table 2, most of the villagers with at least one damage witness damages to the economic activity (77 percent) and to office buildings/assets (44 percent) while around 40 percent report damages to working tools or raw materials; 26 percent declare damages to the house, whereas 23 percent report injuries to relatives and only a small fraction receives personal injuries (9 percent). Among the damaged, the mean number of damages is 2.6 (variable *N_damages*) with around 45 percent reporting an amount of damages above this number (variable *N_dam_ab_med*). Finally, Panel D of Table 2 shows that almost 63 percent of participants are relatively impatient[13] and, on average, 60 percent of the amount at disposal is invested in the risky option (variable *Riskloving*; see Table 1 and Appendix A for further details).

In order to have a clue on whether the identification problem is serious we implement non-parametric tests to check for satisfaction of the balancing property between damaged and non-damaged. We find that some differences are significant at 5 percent level (Panel A, Table 3), with the damaged being on average 4.5 year older and married in a higher proportion and, as expected, less distant from the coast in terms of house location (3.5 against 10.9 Km) than non-damaged; the significant difference in the number of house members is, however, negligible in magnitude. The fact that a higher share of damaged people work in either fishery or trading, while a lower share of them in agriculture, is also consistent with the difference in geographical distance from the coast. Recovery assistance in our sample appears to be properly targeted since damaged people receive significantly more aid. Specifically, the damaged receive almost 17 percent of the total aid in the sample while non-damaged only 5 percent (variable *helpindex*) with 47 (18) percent of the former (latter) receiving more than the average number of helps (variable *help_ab_med*).

This important point confirms that - in terms of observables - the tsunami shock was not perfect in randomly assigning villagers to the damaged and non-damaged groups. However, if we discriminate within damaged using the number of damages reported, we find that most of the significant differences at 5 percent level in the observables vanish (Panel B, Table 3). More specifically, among the damaged, we discriminate between those who are above the sample median number of damages (i.e. two) *vis-à-vis* those who are below. A similar improvement in

[12] Roodman (2012) documents that, after 1985, the year in which the policy of lending to women becomes official, Grameen converged to a 97 percent of loans to women. This figure is close to the 93 percent share of the other main microfinance institution (BRAC) operating in South Asia.

[13] They switch from option A to option B in a potential lottery number greater or equal then the median one (i.e. seven). See Appendix A.2 for details on the lottery game.

terms of balancing properties is reached when comparing all individuals receiving more vs. those receiving less than the sample average amount of aid (Panel C, Table 3). Also under this last comparison the recovery assistance seems to have reached the most affected individuals since people with a greater number of damages also received significantly more aid than those reporting less damages. In sum, apart from civil status (in pacticular, *separated*) and employment sector (in particular, *agricolture*) which are controlled for in the econometric analysis, when comparing individuals either on the number of damages or on the amount of the help received our sample is balanced on most of the socio-demographic characteristics.

It is important to notice that under these preliminary tests, damaged and non-damaged participants do not show significant differences in terms of risk and time preferences. We also run further econometric analysis on the relations between tsunami damages and risk/time preferences without finding significant patterns.[14,] For this reason, we focus on altruistic preferences since under this preliminary inspection they seem to be significantly and persistently affected by the tsunami even at a seven year distance from the event. Hence, in the next sessions we concentrate our analysis on the variation in the behavioural responses in the dictator game and use risk and time attitudes as controls.

4. Hypothesis testing and results

In this section we first outline the sets of testable hypotheses concerning the impact of the tsunami damages and the recovery aid at the extensive (ie. between damaged and non damaged) and intensive (ie. within damaged) margin on altruistic preferences (subsection 4.1) and then comment the preliminary results of the parametric and non-parametric tests (subsection 4.2).

4.1 Altruism by Damage and Aid: Hypotheses.

The hypothesis we want to test is whether the tsunami shock in dictator games affects:

i) sender's giving,
ii) receiver's expectation on sender's giving;
iii) a "solidarity norm" which, if exists, we assume as being equal to the amount given for the sender and the expectation about the amount to be received for the receiver.

This third point refers to the ample literature on social norms as explicit or implicit rules which individuals from the same community follow in order not to incur in informal sanctions from the same community members or in psychological sanctions arising from deviations from the social norms when these are interiorized and become also moral norms.[15] We call

[14] Econometric results on the effects of tsunami damages on time and risk preferences are omitted for reasons of space and for the lack of significant patterns. They are, however, available upon request. This evidence does not necessarily contradict the hypothesis that the calamity may have affected risk and time preferences soon after the event. Thus, it does not necessarily run counter neither Callen et al. (2010) findings on tsunami-damaged people's discount rates at 2.5 years from the event, nor those by Cassar et al. (2011) at a 5-year distance from it. It however documents that in a longer run perspective such an effect is not present in our study.

[15] According to Bicchieri (2006), two conditions must be satisfied for a social norm to exist in a given population. First, a sufficient number of individuals must know that the norm exists and applies to a situation. Second, a

the latter "solidarity norm" since the motivation for the sender's giving may just be pure altruism or conformity to a solidarity norm. In the same way the rationale for the receiver's expectation is the average forecast on what an anonymous individual of the same village would do in these situations. The rational expectation in this case is therefore the social norm of the village about solidarity and giving.

What also seems to justify the existence of such rule is the closeness of the average giving (34 percent) to the 1/3 rule of thumb and to the world modal giving interval documented by the most important meta paper on dictator games (Engel, 2011) (Table 2, Panel D). The average amount expected by the receiver does not coincide but is also close (40.5 percent). Receivers therefore reveal excess optimism in their expectations on the amount received by senders.

Given the longer time distance from the shock in our experiment with respect to similar results in the literature, our hypotheses may be considered as tests on the long run effects of the tsunami calamity on social preferences. More formally, as far as the impact of the tsunami at the extensive margin is concerned, we test the following set of hypotheses:

i)	*Giving*	H_0: $G^{Dam} = G^{NonDam}$	*vs.*	H_1: $G^{Dam} < G^{NonDam}$
ii)	*Expected Giving*	H_0: $\mathrm{E}[G]^{Dam} = \mathrm{E}[G]^{NonDam}$	*vs.*	H_1: $\mathrm{E}[G]^{Dam} < \mathrm{E}[G]^{NonDam}$
iii)	*Solidarity norm*	H_0: $Sn^{Dam} = Sn^{NonDam}$	*vs.*	H_1: $Sn^{Dam} < Sn^{NonDam}$

where G^{Dam} *and* G^{NonDam} are, respectively, the amounts given by damaged and non-damaged senders, $\mathrm{E}[G]^{Dam}$ and $\mathrm{E}[G]^{NonDam}$ the amounts that recipients from the two groups expect to receive from the sender and *Sn* the solidarity norm which is the amount sent for senders and the expectation about sender's giving for recipients. The rationales behind hyp. i)-iii) derive from the related literature discussed in the introduction showing that the tsunami modifies the victims' social preferences because of the post-traumatic stress disorder, underlying changes in risk aversion and discount rates, the perceived lack of future opportunities or the overweighed probability of similar events in the future.

Similarly, for what concerns the impact of the tsunami at the intensive margin, we test the hypotheses below:

iv)	*Giving*	H_0: $G^{HighDam} = G^{LowDam}$	*vs.*	H_1: $G^{HighDam} < G^{LowDam}$
v)	*Expected giving*	H_0: $\mathrm{E}[G]^{HighDam} = \mathrm{E}[G]^{LowDam}$	*vs.*	H_1: $\mathrm{E}[G]^{HighDam} < \mathrm{E}[G]^{LowDam}$
vi)	*Solidarity norm*	H_0: $Sn^{HighDam} = Sn^{LowDam}$	*vs.*	H_1: $Sn^{HighDam} < Sn^{LowDam}$

Furthermore, if damaged individuals are affected differently according to the amount of damage received and the recovery aid enjoyed, we can draw another set of hypotheses that can be summarized as follows:

sufficient number of individuals must have a *conditional preference* to comply with the norm, given the right expectations are satisfied. This second condition—the presence of a sufficient number of *conditional followers*—is the one that justifies distinguishing social and moral norms.

vii)	*Giving*	H_0: $G^{HighDam/HighHelp} = G^{LowDam}$	*vs.*	H_1: $G^{HighDam/HighHelp} > G^{LowDam/LowHelp}$
viii)	*Expected giving*	H_0: $E[G]^{HighDam/HighHelp} = E[G]^{LowDam/LowHelp}$	*vs.*	H_1: $E[G]^{HighDam/HighHelp} > E[G]^{LowDam//LowHelp}$
ix)	*Solidarity norm*	H_0: $Sn^{HighDam/HighHelp} = Sn^{LowDam/LowHelp}$	*vs.*	H_1: $Sn^{HighDam/HighHelp} > Sn^{LowDam/LowHelp}$

where *HighDam* are victims who suffered more than two damages (two is the median number of damages) while *LowDam* are damaged people suffering a number of damages less than (or equal to) two; *HighHelp* are individuals who received more than sample median help (i.e. *Helpindex*>0.113), while *LowHelp* are those receiving an amount of help equal or below to the median help (i.e. *Helpindex*≤0.113). The rationale behind hyp. iv)-ix) is the time-persistency of the indirect reciprocal kind of preferences (Stanca, 2010, Nowak and Sigmund, 2005) which would make most needy individuals that received a significant amount of recovery aid at the time tsunami from a (local, national or international) donor acting more pro-socially toward a "third party" (i.e. neighbouring villagers) even seven years after the event .

4.2 Altruism by Damage and Aid: Testing and Results.

In Table 4 we report the results of all the parametric and non-parametric tests for the hypotheses outlined in the previous subsection. We first perform tests on the hypotheses i)-iii) and find that they are rejected in all of the three cases (Panel A.1, Table 4). When considering dictator's giving, receiver's expected giving and solidarity norm we find that on average damaged people give 31 percent of their endowment, while non-damaged 6.2 percent more. [16] When testing hypothesis ii) the null is rejected at 95 percent level with non-damaged receivers expecting on average 43.3 percent, while damaged receivers 38.3 percent (p-value .012 in non-parametric and .040 in parametric tests). As it is reasonable to expect, aggregated results on giving and expected giving generate significant differences in terms of solidarity norms leading to strong rejection of the null of hypothesis iii). In the comparison between damaged and non-damaged the average share (given or expected to receive) is 40 percent for non-damaged while 35 percent for damaged (p-value .001 in the non-parametric test and .003 in the parametric test). This indicates a strongly significant tsunami-impact on the solidarity norm even in the long run.

As a corollary to hypotheses i)-iii), in order to check whether the recovery aid may have directly affected altruistic preferences, we repeat the previous tests for those receiving an amount of aid below vs. those receiving an amount of aid above the sample median recovery aid. Results reported in Panel A.2 of Table 4 clearly show that those receiving more or less assistance do not significantly differ in terms of giving or expected giving.

Consistently with the previous results, when we perform the test in the subsample of damaged (which has been shown to be more homogenous) we find that the null hypotheses v)

[16] Note that these average giving shares are consistent with the world modal value of the distribution of giving in the meta paper of Engel (2011) which is in the 30-40 percent interval. Furthermore, the share of experiment participants which are fully self-interested is very low and equal to 2.62 percent (only five players with two tsunami-damaged among them). This share is far lower than that reported by Engel (2011) in his meta paper on dictator games (36 percent). Note however that the Engel's share falls considerably in the subgroups of deserving, adult and non student recipients. We may as well think that a further fall may be caused by the impact of the tsunami event even on non victims.

and vi) are rejected in favour of the alternatives (Panel B.1, Table 4). In particular, the more damaged expect 8 percent less than the less damaged (p-value .06 in the non-parametric test and .021 in the parametric test), while the solidarity norm is 5 percent lower for the former than for the latter (p-value .049 in the non-parametric test and .023 in the parametric test). In accordance with the alternative in hyp. iv), giving is 2 percent larger for the more damaged relative to the less damaged but the difference is not statistically significant.

Results from parametric and non-parametric tests so far suggest a negative, significant and time-persistent relationship between the tsunami experience and altruistic preferences both at the intensive and the extensive margins with (more) damaged participants showing less altruism than non- (less) damaged ones. Incidentally, we also show that recovery aid does not directly change altruistic preferences in the long run.[17] To check for indirect effects of the aid on behaviour (i.e. through the damages caused by the shock), we restrict our sample only to tsunami-hit individuals and test for hyp. vii)-ix) both parametrically and non-parametrically. As shown in Panel B.2 of Table 4, altruism weakly varies according to the amount of aid received among the damaged--- the main difference (significant at 5 percent level under parametric tests) is driven by the higher expected giving of the receivers and solidarity norm of players who received more relative to those who received less aid.

Consistently with the indirect reciprocity argument, when we compare the more needy (i.e. the highly damaged) individuals on the basis of amount of help received, the nulls of hyp. viii)-ix) are all strongly rejected in favour of the alternatives (Panel B.3, Table 4). Specifically, the more damaged receiving a large recovery assistance show higher altruism than the more damaged receiving little aid with a statistically significant difference reaching respectively 17 percent for expected giving and 11 percent for solidarity norm. To the same direction points the test for the hyp. vii) although the difference in terms of giving between the two groups is not statistically significant. Moreover, differences in altruism on the amount of aid received are no longer significant when we restrict the sample to less damaged individuals (Panel B.4, Table 4).

All these facts together suggest the existence a positive role of the well-targeted recovery aid (i.e. when it reaches the most needy persons) in counterbalancing the negative impact of the tsunami experience on altruistic behaviour. As already discussed in the previous section and in the introduction, it is unlikely that all our results are driven by a process of selection into victimization or recovery aid, both on logical grounds and after observing balancing properties in Table 3.[18] As outlined in subsection 4.1, the rejection of the null in direction of

[17] Note also that results on the impact of the shock on giving and expected giving point to the same direction. This fact supports the assumption that the shock affects the way participants behave as senders and they expect to be treated as receivers, presumably because receivers expect to be treated as they would do in the senders' position and vice versa.

[18] Note that participants to the experiment know that their identity (and therefore their damaged/non damaged status) is not revealed to the counterpart. Furthermore, the design eliminates any reference to the damage experience since the survey including questions on the tsunami experience is administered after the experiment). Hence damaged receivers cannot expect more because they assume that senders will give more to them knowing their damaged status. On the other hand, senders may think they have the right to give less since they have been damaged, even though they cannot share this motivation with the receivers who, in turn, cannot internalize it in their utility function.

the alternative for the hypotheses viii) and ix) may be conceived here as a test of indirect reciprocity if we interpret the result in the light of the difference in the help received by the two groups, with damaged individuals receiving significantly more aid than those suffering less damages. Hence our findings support the hypothesis on the existence of an indirect reciprocity norm according to which a kind (or unkind) action received directly or indirectly (in our case by development aid agencies or other donors [19]) is reciprocated towards a third agent (in our case the receiver in the dictator game).

Note that our result is particularly strong since the indirect reciprocating act occurs in a one-shot anonymous interaction and it cannot therefore be explained by reputational concerns as it occurs in some empirical tests of indirect reciprocity with iterated interactions (e.g. Wedekind and Milinski, 2000, Engelmann and Fischbacher, 2009, Seinen and Schram, 2006, Bolton, Katok and Ockenfels, 2005, Greiner and Levati, 2005).[20] Furthermore, the first action triggering indirect reciprocity is not produced experimentally but is a 7-year distance event, even though is an event certainly more important and memorable to affected players than those produced in artificial experiments.

5. Econometric analysis

Econometric estimates enrich our parametric and non-parametric tests by verifying the impact of additional covariates on giving, expected giving and solidarity norm as well as by checking for the robustness of our previous results to the introduction of covariates.

The specification we test is:

$$Y_i = \alpha_0 + \alpha_1 Damaged_i + \Sigma_k \gamma_k X_{ki} + \Sigma_j \gamma_j G_{ji} + \varepsilon_{it}$$

where Y is the dependent variable, that is - according to the specification chosen - the share of the endowment sent for senders (variable *giving* in senders' estimates), the amount that receivers expect to receive (variable *expected giving* in receivers' estimates) or both (variable *solidarity norm* in full sample estimates); *Damaged* is the "treatment" dummy variable and the *X* socio-demographic controls include age, gender, years of education, village dummies, marital status dummies, household's monthly food expenditure (*food_exp_std)*, the number of household's components (*n_house_members*), a proxy for social preferences (*trustindex*), a variable measuring borrower's seniority (the number of loan cycles) plus three dummies for

[19] Agro Micro Finance reported direct and indirect losses on 620 clients in the districts of Galle, Matara, and Hambantota and estimated that they amounted to almost 24.4% of the MFI loan portfolio at the tsunami date. Support to AMF refinancing needs came from USAID, UNDP, and an Italian MFI (Etimos). On the short run effects of this intervention see Becchetti and Castriota (2010 and 2011) .

[20] Relatively less evidence is available on strong indirect reciprocity (e.g. Dufwenberg et. al, 2001, Guth et al., 2001), and the results are generally not conclusive.

the respondent's working activity (*trading*, *fishery* and *manufacturing*).[21] The *G* variables are the experimental measures of time and risk attitudes we elicited in different games. [22]

In an alternative specification, in order to look at the impact of the tsunami on altruistic preferences at the intensive margin, as well as at the interaction between damages and recovery aid, we replace the *damaged* variable with the number of damages reported by each individual (variable *N_damages*), a dummy equal to one for those receiving an amount of help above the sample average (variable *help_ab_med)* and the interaction term between these two (*N_damages*help_ab_med*).

5.1 The impact of the tsunami experience

First of all, when considering giving as dependent variable (the *giving* variable) the estimates document that none of the controls is significant at 5 percent (Table 5), except for the amount invested into the risky option (*riskloving_ratio*) which is positive and significant [23]. The damage dummy is negative and significant with senders hit by the tsunami giving about 6 percent less than those who are not hit (a magnitude equal to the effect measured in parametric/non-parametric tests in section 4). Tobit estimates taking into account the left and right limit of our dependent variable confirm this first result.

Second we repeat the econometric analysis for receivers' expectation about sender's giving (Tables 6) and find exactly the same pattern of results observed for senders; the only difference is that now time preferences matter--- more impatient participants tend to expect around 7 percent less than less impatient ones. Regarding the impact of tsunami, having received at least a damage reduces receiver's expected giving by 5 percent in the baseline estimate and by around 8 percent when we include other covariates.

Finally, we extend the econometric analysis also to hypothesis iii) as in the previous section. Specifically, we check for the impact of the tsunami shock on the solidarity norm in the overall sample of participants (Tables 7.1 and 7.2). The chosen specification allows us to control for the heterogeneity in the sender/receiver status with a *receiver* dummy. Also in this case, results are consistent with the previously shown; in particular, damaged deviate from the solidarity norm by 5-6 percent more relative to non-damaged (Table 7.1).

5.2 The impact of tsunami damages and the recovery aid

To investigate the tsunami impact at the intensive margin and the role of recovery aid we re-estimate the model in Table 7.1 replacing the *damaged* dummy with *N_damages*, *help_ab_med* and their interaction. Results are reported in Table 7.2 and confirm previous section findings.

[21] For details on the construction of such controls see variable legend in Table 1.

[22] See variable legend in Table 1 and the appendix B for details on the proxies of risk and time preferences. As already explained above, we do not find any significant effect of the damaged/non damaged status or the kind of damages on risk/time preferences. For this reason we use these experimental measures just as additional controls in the estimates concerning altruistic preferences.

[23] On the relations between risk attitudes and social preferences see, among others, Beck (1994) and Bohnet et al. (2008). A part from being a possible proxy for income- and wealth-related factors not fully captured by the food expenditure variable, the risk-preference variable is significant also because of the multi-game nature of experiment according to which payments depend on one randomly selected game.

Specifically, each additional damage has a detrimental effect on altruistic behaviour (variable *N_damages*) which is compensated by a positive one deriving from the interaction between a large amount of aid and the number of damages (variable *N_damages*help_ab_med*).[24] The aid-compensating effect is even larger in magnitude when restricting the sample only to damaged individuals (columns 4 and 8, Table 7.2).

As a robustness check, in order to confirm the absence of a direct long-term impact of aid on altruism we re-estimate the models in Tables 6-7.1 introducing as additional regressors the variable *help_ab_med* and its interaction with the *damaged* dummy. None of the two terms - as expected - turn out to be significant while all the previous findings are substantially unchanged (see Table A in Appendix A.3).[25]

5.3 Interpretation of the empirical results

To summarize the main findings, our econometric results confirm that i) having received at least one damage from the tsunami reduces giving, expected giving and - more in general - the solidarity norm of affected individuals, ii) there is no evidence of a direct effect of recovery aid on altruism, and iii) a large damage reduces altruism (especially in terms of expected giving and solidarity norm) while the interaction between more damages and more recovery aid significantly restores it.

We do not find evidence of a long-term *direct* effect of recovery aid on behaviour but we do find its *indirect* effect on altruistic preferences - i.e. only through the intensity of victimization - even after seven years from the calamity. This result provides support to our assumption that social preferences of the more damaged/more helped tsunami victims have been time-persistently shaped by indirect reciprocity--- because of the large and memorable assistance received soon after the tsunami, the more needy victims seem to recall the generosity experienced in the past and reciprocate it to other villagers even seven years after the shock.

6. Tackling endogeneity

A possible bias affecting the causal interpretation of our results may derive from the non-random assignment of the *damaged* status as signalled by the balancing properties in Table 3. Even though the impact of this source of bias is arguably limited by selecting a sample of only microfinance borrowers and comparing mostly balanced subsamples on the basis of the amount of damaged and aid, we account for the remaining potential endogeneity with i) a weighted least squares estimation (subsection 6.1), and ii) an instrumental variable regression (subsection 6.2). In any case we believe that, as suggested by the balancing

[24] As far as the magnitude is concerned, one standard deviation increase in *N_damages* generates a reduction of about 4.6 percent in *solidarity norm* while a standard deviation increase in *N_damages*help_ab_med* rises *solidarity norm* by about 5 percent (column 3, Table 7.2)

[25] Columns 2, 4, and 6 of Table A in Appendix A.3 clearly show that the interaction between the two dummies is insignificant. As documented below, only the combination of large damage and large aid counterbalances the negative effect of the tsunami victimization on altruism. This evidence supports our assumption that the social preferences of very needy persons who memorably experienced after the shock a great deal of solidarity have been persistently shaped by (positive) indirect reciprocity.

properties in Panels B and C of Table 3, the bias (if any) may only affect the results on the impact of the tsunami at the extensive margin (i.e. damaged vs. non-damaged) but not also those on its impact at the intensive margin (i.e. within damaged according to damage intensity) and on the aid-compensation effect.

6.1 Weighted east square estimations

In order to reduce identification concerns arising from the potentially endogenous self-selection of the villagers into the damage status (*damaged*), we re-estimate the models in Tables 5, 6, 7.1 and 7.2 with weighted least squares by weighting each observation with the inverse of its estimated propensity score for receiving at least a damage from the tsunami[26]. All the main results illustrated above are robust to this check and are reported respectively in Tables 5A, 6A, 7.1A and 7.2A in Appendix A.

6.2 Instrumental variable regressions

We enrich our identification strategy through an IV re-estimation of the specification mostly suspected of endogeneity (i.e. the one on the damaged/non-damaged tsunami effect) given the documented presence of some differences in observables (see Table 3). Specifically, we repeat the estimates of Table 7.1 instrumenting the *damaged* dummy. We believe the first natural instrument is the individual's distance from the coast at the moment of the tsunami (even though the presence/absence of natural barriers makes the protecting capacity of such distance heterogeneous). The instrument seems logically and statistically relevant since those living closer to the coast were more likely to get damaged by the tsunami (see Table 3). It is also likely to be logically valid since it is difficult to justify how the difference in terms of distance from the coast across individuals may affect their preferences through non-observables factors other than victimization status. However, we discuss this possibility below. [27]

A second instrument we use is individual's body mass index (BMI) defined as the individual's body mass divided by the square of her/his height. Also in this case the instrument appears logically valid since it is hard to think of an unobservable and statistically significant link between body characteristics and social preferences a part from victimization status. In addition, if we interpret the BMI as a proxy for health conditions or fitness, we may expect BMI to be a valid instrument since more fit/more healthy individuals (i.e., for instance, neither over- nor under-weighted, nor in poor health conditions) were reasonably more likely to escape harsh damages (and presumably recover faster) than less fit/less healthy ones.

[26] Specifically, for each individual, the weights are computed as $\frac{\text{damaged}}{\widehat{\text{pscore}_{(\text{damaged})}}} + \frac{1-\text{damaged}}{1-\widehat{\text{pscore}_{(\text{damaged})}}}$, where *pscore* is a non-parametric estimate of the propensity score for the probability of receiving at least one damage (i.e. *damaged* dummy). The *pscore* is estimated using as regressors the respondent's years of schooling, employment sector and village dummies, the credit seniority (variable *loancycle*), the body mass index (variable *BMI*) and the distance of the house from the coast at the time of the tsunami (variable *distant*) (see variable legend in Table 1). For details on this methodological approach see, among others, Blattman and Annan (2010) and Hirano, Imbens and Ridder (2003).

[27] Note also that the few observables in which the two groups of damaged/non damaged differ do not affect altruism in previous econometric estimates (see Tables 5-7.1).

We re-estimate the OLS specifications of Table 7.1 by instrumenting the *damaged* dummy first with a dummy equal to one if the individual lived at above the median sample distance from the coast at the time of tsunami (*distant*) and then with both instruments (*distant* and *BMI*). Results are reported in Table 7.3. In all the specifications (with/without demographic controls) with the *distant* instrument the effect of receiving at least one damage from the tsunami on the solidarity norm is significant and strong in magnitude (i.e. tsunami damaged send/expect roughly 9-10 percent less than non-damaged). When both instruments are used, the damaged effect remains significant and relatively close in magnitude to that found in the previous estimates. Importantly, the first stage F-statistics are significantly high in all the cases, confirming the logical relevance of our instruments; furthermore, the Sargan test (1958) suggests we cannot reject the null overidentifying restrictions (Table 7.3, columns 3 and 4) and the endogeneity C-test (GMM distance) cannot reject the null that *damaged* dummy can be treated as exogenous. Therefore these statistics provide further evidence in favour of the validity of our identification strategy.

6.3 Discussion

The validity of our *distant* instrument hinges on the assumption that distance from the coast and altruism are correlated only through victimization. It can be possible, however, that the individuals' location choice is endogenously based on unobservable factors that influence both altruism and victimization. One of such factors can be, for instance, the pre-tsunami risk attitudes towards natural events since individuals with higher (lower) expectation of a shock and/or more (less) risk averse can decide to live more (less) far away from the coast. Since we do not have pre-tsunami data, we cannot control for ex-ante risk preferences. It has to be noticed, however, that the 2004 tsunami was a completely unexpected event so that location decisions may be hardly driven by a pre-existing background risk of tsunami.

Another possible third omitted factor affecting the validity of the instrument *distant* is the pre-tsunami profitability of the employment sector which may be thought as being correlated with social preferences. More specifically, individuals expecting higher returns from agriculture may have decided to live farther away from the coast than those expecting higher returns from fishing who instead opted to live on the seaside. A rough check for this is the comparison of the average per-capita food expenditure (our proxy for income) between farmers and fishermen. We find that the difference is not statistically significant (two sided test p-value = 0.8654) supporting the validity of the exclusion restriction.

Another source of endogeneity may arise from post-tsunami differential migration based on unobservable factors (i.e., for instance, individual's ability to be in social networks) which can be correlated with both altruism and tsunami exposition. We do not believe migration can affect our estimates since it turns out to be very limited (as documented by AMF) and, above all, there would have been little incentive for borrowers to migrate after the tsunami because of i) the extremely favourable conditions on the micro-loan offered by AMF and ii) the huge amount of local and international aid flows (Becchetti and Castriota, 2010 and 2011).

Another potential spurious explanation of our findings may be related to a more general income effect, i.e. the differential between giving and expected giving on the basis of damage and aid may be explained by wealth or income variation before and after the tsunami which we cannot observe. Even though we do not have the pre-tsunami levels of income, we do not believe that income is the main hidden driver of our results for two main reasons, i.e. i) income levels are proxied by many controls in our estimates such as the current employment sector [28], the current level of food expenditures, the level of education and the risk and time preferences; ii) in similar field studies conducted in the same villages by Becchetti and Castriota (2010 and 2011), in 2007 – already – damaged borrowers seem to have almost completely converged to non-damaged ones in terms of income, productivity and life-satisfaction. Our study highlights instead that such a convergence is not yet complete in terms of social preferences when considering the long run effect of the tsunami at the extensive margin.

7. Conclusions

The tsunami shock is an unfortunate event which creates a unique framework for investigating the effects of a calamity on individual preferences. The characteristics of the event are such that people living or being at a few meters from each other at the time of the shock are almost randomly affected or unaffected. The opportunity has been already exploited by several studies in the past. The originality of our paper is in testing similar hypotheses at a longer time distance, using within village variability between damaged and non-damaged and exploiting the variability across damage and recovery aid intensity.

In particular, we test the effect of the shock at the extensive margin by comparing damaged with non-damaged individuals in terms of giving and expected giving in the dictator game. Moreover, at the intensive margin, we compare the participants on the basis of the amount of damage and recovery aid received. The advantage of this last comparison is that differences in observables between the groups almost vanish. We reduce further identification problems by selecting a random sample of damaged and non-damaged borrowers belonging to the same microfinance organization who are therefore very likely to share some important common traits (i.e. entrepreneurial and social skills) usually unobservable to researchers and suspected to be among the main determinants of self-selection bias. We complete our identification strategy with weighted least squares and IV estimates documenting that our main findings remain significant when using instruments which we show as being valid and relevant.

The empirical analysis highlights two original results, i.e. i) both at intensive and extensive margin, individuals damaged by the tsunami give and expect less than non-damaged ones even after seven years from the event; ii) recovery aid does not directly affect altruistic preferences but it does in a positive way only when highly damaged individuals receive above

[28] The employment sector can be reasonably assumed to be persistent in time since most businesses are family-based and the job skills are usually transmitted inter-generationally within the household.

median assistance for their recovery. We believe the superior pro-sociality (expected pro-sociality) of the latter can be interpreted as a form of indirect reciprocity.

In conclusion, we deem our results identify an original hidden effect of recovery after calamities documenting that the benevolence experienced from donors heal the time-persistent loss of pro-social attitudes generated by the calamity shock. Our long-term results provide an interpretative key to reconcile the branch of the literature showing evidence of natural shocks being detrimental for social preferences (Fleming et al., 2011; Cassar et al. 2013; Becchetti et al., 2014) with the other branch supporting instead a positive link between the two (Solnit, 2009; Whitt and Wilson, 2007; Cassar et al., 2011).

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Table 1 - Variable legend

Variable	Description
Giving	amount sent by the sender / initial endowment (900 LKR)
Expected_Giving	sender's amount expected by the receiver / sender's initial endowment (900 LKR)
Solidarity Norm	= *Giving* if the player is a Sender or *Expected_Giving* if the player is a Receiver.
Receiver	= 1 if the player is a Receiver; = 0 if the player is a Sender.
Age	respondent's age
Male	=1 if the respondent is male
Married	=1 if the respondent is married
Widowed	=1 if the respondent is widowed
Separated	=1 if the respondent is separated
Single	=1 if the respondent is single
N_house_members	n. of house components
Years_schooling	respondent's years of schooling
Food_exp_std	monthly respondent's household food expenditure (in LKR, scaled by 1000).
Agriculture	= 1 if the respondent works in the agricultural sector
Manufacturing	= 1 if the respondent works in the manufacturing sector
Fishery	= 1 if the respondent works in the fishery sector
Trading	= 1 if the respondent works in the trading sector
Riskloving	amount invested in the risky option of the risky investment game.
Riskloving_ratio	amount invested in the risky option of the risky investment game / maximum amount investible (300 LKR).
Switch	potential lottery number at which the participant switches from option A (*receive 10.000 LKR after 2 months*) to option B (*receive 10.000 + x LKR after 8 months*). It is a real number between 1 and 9; it is =1 if the participant chooses B from the first potential lottery and never switches to A (maximum degree of patience); it is =9 if the participant chooses A from the first potential lottery and never switches to B (maximum degree of impatience). See relevant game sheets in the Appendix B for the options in each single lottery.
Impatient	= 1 if switch ≥ 7, i.e the respondent is equal-or-above the median level of impatience--- (s)he has switched to option B (highest payoff with latest payment) from or after the seventh lottery-choice. See relevant game sheets in the Appendix B for the option list for each lottery.
Galle	= 1 If the respondent lives in Galle district.
Matara	= 1 If the respondent lives in Matara district.
Hambantota	= 1 If the respondent lives in Hambantota district.
Most_can_be_trusted	"Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?". 1 = Have to be careful ; 2 = Most people can be trusted.
Cant_rely	respondent's 1-5 Likert scale agreement on the statement: "Nowadays, you can't rely on anybody"
People_take_advantage	respondent's 1-5 Likert scale agreement on the statement: "If you are not careful, other people will take advantage of you"
Trustindex	= (*most_can_be_trusted+cant_rely+ people_take_advantage*)/3
BMI	respondent's body mass index = weight/height^2
Distance_housecoast	respondent's distance from the coast at the time of 2004 tsunami (in Km)
Distant	=1 if respondent lived above the median distance from the coast (3 Km) at the time of 2004 tsunami
Loancycle	total n. of loan repaid (borrower's seniority)
Personal_Injury	=1 if the respondent reports personal injuries caused by tsunami
Family_Injury	=1 if the respondent reports injuries to relatives caused by tsunami
Damage_house	=1 if the respondent reports damages to the house caused by tsunami
Damage_econ_activity	=1 if the respondent reports damages to the economic activity caused by tsunami
Damage_assets	=1 if the respondent reports damages to assets caused by tsunami
Damage_tools	=1 if the respondent reports damages to working tools caused by tsunami
Damage_raw_materials	=1 if the respondent reports damages to raw materials caused by tsunami
N_damages	= sum of all the above-described damages reported by the respondent
N_dam_ab_med	= 1 if *N_damages* > 2 [2 is the sample median of *N_damages* conditionally on *Damaged* =1]
Damaged	=1 if the respondent reports at least one type of damage.
Money_aid	=1 if the respondent received financial aid (non microfinance) after the tsunami
Credit_aid	=1 if the respondent received financial support (microfinance) after the tsunami
Food_aid	=1 if the respondent received assistance in terms of food after the tsunami
Medicines_aid	=1 if the respondent received assistance in terms of medicines after the tsunami
Rawmaterials_aid	=1 if the respondent received assistance in terms of raw materials for repairing/rebuilding house after the tsunami
Tools_aid	=1 if the respondent received assistance in terms of working tools after the tsunami
Consumption_aid	=1 if the respondent received consumption aid after the tsunami
Other_aid	=1 if the respondent received other kind of aid after the tsunami
Helpindex	= sum of *_aid dummies /8
Help_ab_med	= 1 if *Helpindex* > 0.113 [0.113 is the sample mean of *Helpindex*]

Table 2 - Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A. Socio-Demographic Variables					
Age	382	46.793	12.100	12	71
Single	382	0.045	0.206	0	1
Widowed	382	0.099	0.300	0	1
Married	382	0.838	0.369	0	1
Separated	382	0.018	0.134	0	1
Male	382	0.065	0.248	0	1
Food_exp_std	381	8.701	6.927	0.4	120
Galle	382	0.223	0.416	0	1
Hambantota	382	0.288	0.453	0	1
Years_schooling	374	10.505	2.499	0	16
N_house_members	382	4.521	1.412	1	10
Agricolture	382	0.220	0.415	0	1
Trading	382	0.374	0.485	0	1
Fishery	382	0.037	0.188	0	1
Manufacturing	382	0.317	0.466	0	1
Trustindex	380	1.212	0.342	0.667	2.667
Loancycle	382	2.050	3.214	0	28
Distance_housecoast	372	6.867	10.756	0	100
Distant	382	0.492	0.501	0	1
BMI	379	23.517	5.409	12.095	74.002
Damaged	382	0.542	0.499	0	1
Panel B. Recovery Aid after tsunami					
Money_aid	382	0.162	0.369	0	1
Credit_aid	380	0.061	0.239	0	1
Food_aid	381	0.192	0.394	0	1
Medicines_aid	381	0.155	0.362	0	1
Rawmaterials_aid	382	0.079	0.269	0	1
Tools_aid	382	0.144	0.352	0	1
Consumption_aid	382	0.102	0.303	0	1
Other_aid	376	0.011	0.103	0	1
Helpindex	372	0.113	0.201	0	0.875
Help_ab_med	372	0.339	0.474	0	1
Panel C. Damages after tsunami (only damaged)					
Personal_Injury	207	0.087	0.282	0	1
Family_Injury	206	0.228	0.421	0	1
Damage_house	206	0.262	0.441	0	1
Damage_econ_activity	206	0.767	0.424	0	1
Damage_assets	206	0.437	0.497	0	1
Damage_tools	206	0.417	0.494	0	1
Damage_raw_materials	206	0.393	0.490	0	1
N_damages	207	2.580	1.782	0	7
N_dam_ab_med	207	0.449	0.499	0	1
Panel D. Game Variables					
Giving	191	0.339	0.188	0	1
Expected_giving	191	0.405	0.194	0	1
Solidarity_norm	382	0.372	0.194	0	1
Receiver	382	0.500	0.501	0	1
Riskloving_ratio	382	0.590	0.285	0	1
Impatient	382	0.628	0.484	0	1

Table 3 - Balancing Properties

		Panel A: **Damaged (1) vs. Non-Damaged (0)**					Panel B: **N_Damages > 2 (1) vs. N_Damages ≤ 2 (0) [only damaged]**					Panel C: **Helpindex > 0.113 (1) vs. Helpindex ≤ 0.113 (0)**				
Variable		*Obs*	*Mean*	*Std dev*	*Z-stat, P-Value*		*Obs*	*Mean*	*Std dev*	*Z-stat, P-Value*		*Obs*	*Mean*	*Std dev*	*Z-stat, P-Value*
Age	0	175	44.326	12.554	-3.564	0	114	49.570	11.416	1.147	0	246	46.598	12.180	0.003
	1	207	48.879	11.320	0.000	1	93	48.032	11.205	0.251	1	126	46.794	12.135	0.998
Male	0	175	0.040	0.197	-1.847	0	114	0.088	0.284	0.043	0	246	0.069	0.254	0.503
	1	207	0.087	0.282	0.065	1	93	0.086	0.282	0.966	1	126	0.056	0.230	0.615
Married	0	175	0.897	0.305	2.893	0	114	0.763	0.427	-0.943	0	246	0.858	0.350	1.391
	1	207	0.787	0.410	0.004	1	93	0.817	0.389	0.346	1	126	0.802	0.400	0.164
Separated	0	175	0.006	0.076	-1.687	0	114	0.044	0.206	1.409	0	246	0.004	0.064	-2.922
	1	207	0.029	0.168	0.092	1	93	0.011	0.104	0.159	1	126	0.048	0.214	0.003
Widowed	0	175	0.069	0.253	-1.853	0	114	0.123	0.330	-0.134	0	246	0.098	0.297	0.072
	1	207	0.126	0.332	0.064	1	93	0.129	0.337	0.893	1	126	0.095	0.295	0.943
Single	0	175	0.029	0.167	-1.387	0	114	0.070	0.257	0.830	0	246	0.041	0.198	-0.651
	1	207	0.058	0.234	0.166	1	93	0.043	0.204	0.407	1	126	0.056	0.230	0.515
Hmembers	0	175	4.320	1.381	-2.945	0	114	4.526	1.365	-1.949	0	246	4.463	1.348	-1.765
	1	207	4.691	1.418	0.003	1	93	4.892	1.463	0.051	1	126	4.667	1.528	0.078
Yschool	0	174	10.736	2.440	1.767	0	112	10.545	2.504	1.466	0	240	10.746	2.456	2.586
	1	200	10.305	2.539	0.077	1	88	10.000	2.564	0.143	1	124	10.073	2.576	0.010
Foodexp_std	0	174	8.130	3.638	-1.076	0	114	9.408	11.217	-0.981	0	245	8.326	3.819	-0.399
	1	207	9.180	8.769	0.282	1	93	8.900	4.183	0.326	1	126	9.452	10.753	0.690
Agricolture	0	175	0.314	0.466	4.090	0	114	0.184	0.389	2.020	0	246	0.268	0.444	3.107
	1	207	0.140	0.348	0.000	1	93	0.086	0.282	0.043	1	126	0.127	0.334	0.002
Manufacturing	0	175	0.314	0.466	-0.095	0	114	0.281	0.451	-1.300	0	246	0.305	0.461	-0.559
	1	207	0.319	0.467	0.924	1	93	0.366	0.484	0.193	1	126	0.333	0.473	0.576
Fishery	0	175	0.011	0.107	-2.409	0	114	0.035	0.185	-1.556	0	246	0.024	0.155	-1.547
	1	207	0.058	0.234	0.016	1	93	0.086	0.282	0.120	1	126	0.056	0.230	0.122
Trading	0	175	0.309	0.463	-2.439	0	114	0.421	0.496	-0.286	0	246	0.362	0.481	-0.511
	1	207	0.430	0.496	0.015	1	93	0.441	0.499	0.775	1	126	0.389	0.489	0.609
Galle	0	175	0.166	0.373	-2.451	0	114	0.281	0.451	0.364	0	246	0.224	0.417	-0.143
	1	207	0.271	0.445	0.014	1	93	0.258	0.440	0.716	1	126	0.230	0.423	0.886
Matara	0	175	0.486	0.501	-0.137	0	114	0.482	0.502	-0.327	0	246	0.488	0.501	0.212
	1	207	0.493	0.501	0.891	1	93	0.505	0.503	0.743	1	126	0.476	0.501	0.832
Hambantota	0	175	0.349	0.478	2.402	0	114	0.237	0.427	0.005	0	246	0.289	0.454	-0.101
	1	207	0.237	0.426	0.016	1	93	0.237	0.427	0.996	1	126	0.294	0.457	0.919
Impatient	0	175	0.600	0.491	-1.050	0	114	0.649	0.479	-0.102	0	246	0.622	0.486	0.055
	1	207	0.652	0.477	0.294	1	93	0.656	0.478	0.919	1	126	0.619	0.488	0.956
Rg_investment	0	175	173.829	85.599	-0.565	0	114	170.526	84.318	-1.836	0	246	180.366	84.091	1.166
	1	207	179.710	85.672	0.572	1	93	190.968	86.427	0.066	1	126	169.048	87.986	0.243
Trustindex	0	175	1.208	0.327	0.029	0	113	1.212	0.354	-0.007	0	246	1.222	0.336	1.122
	1	205	1.216	0.354	0.976	1	92	1.221	0.357	0.995	1	124	1.199	0.358	0.262
BMI	0	173	22.976	4.578	-1.473	0	113	24.058	4.959	1.274	0	245	23.463	5.785	-0.556
	1	206	23.971	5.993	0.141	1	93	23.866	7.075	0.203	1	124	23.697	4.721	0.578
Distance	0	169	10.936	12.694	9.986	0	112	4.659	6.536	5.218	0	240	8.728	11.753	6.789
	1	203	3.479	7.286	0.000	1	91	2.027	7.912	0.000	1	123	3.282	7.641	0.000
N_damages	0	175	0.000	0.000	-16.531						0	105	2.086	1.722	-4.443
	1	207	2.580	1.782	0.000						1	95	3.126	1.690	0.000
Helpindex	0	172	0.049	0.131	-6.186										
	1	200	0.168	0.233	0.000										
Help_ab_med	0	172	0.180	0.386	-5.981										
	1	200	0.475	0.501	0.000										

Table 4 - Testing Altruism By Damage

Variable	Group	Obs	Mean	Std. dev.	Non-par. test: z-stat, p-value	t-test: P(T<t)	t-test: P(|T>t|)	t-test: P(T>t)
A) WHOLE SAMPLE								
panel A.1								
Giving	Non-damaged	89	0.372	0.205	2.129	0.012	0.024	0.988
	Damaged	102	0.310	0.168	0.033	.	.	.
Expected_giving	Non-damaged	86	0.433	0.179	2.519	0.040	0.080	0.960
	Damaged	105	0.383	0.204	0.012	.	.	.
Solidarity_norm	Non-damaged	175	0.402	0.194	3.219	0.003	0.006	0.997
	Damaged	207	0.347	0.190	0.001	.	.	.
panel A.2								
Giving	Helpindex<0.113	121	0.341	0.201	-0.175	0.489	0.979	0.511
	Helpindex>0.113	65	0.340	0.163	0.861	.	.	.
Expected_giving	Helpindex<0.113	125	0.391	0.190	-1.382	0.949	0.102	0.051
	Helpindex>0.113	61	0.440	0.202	0.167	.	.	.
Solidarity_norm	Helpindex<0.113	246	0.366	0.197	-0.936	0.855	0.291	0.145
	Helpindex>0.113	126	0.389	0.189	0.349	.	.	.
B) DAMAGED ONLY								
Panel B.1								
Giving	N_Damages≤2	57	0.301	0.171	-0.824	0.739	0.521	0.261
	N_Damages>2	45	0.322	0.165	0.410	.	.	.
Expected_giving	N_Damages≤2	57	0.346	0.197	-1.882	0.979	0.042	0.021
	N_Damages>2	48	0.427	0.205	0.060	.	.	.
Solidarity_norm	N_Damages≤2	114	0.323	0.185	-1.971	0.977	0.046	0.023
	N_Damages>2	93	0.376	0.193	0.049	.	.	.
Panel B.2								
Giving	Helpindex≤0.113	48	0.308	0.165	-0.308	0.673	0.654	0.327
	Helpindex>0.113	51	0.323	0.172	0.758	.	.	.
Expected_giving	Helpindex≤0.113	57	0.347	0.178	-1.843	0.985	0.030	0.015
	Helpindex>0.113	44	0.436	0.228	0.065	.	.	.
Solidarity_norm	Helpindex≤0.113	105	0.329	0.173	-1.261	0.957	0.086	0.043
	Helpindex>0.113	95	0.375	0.206	0.207	.	.	.
Panel B.3 - N_Damages > 2								
Giving	Helpindex≤0.113	15	0.282	0.164	-1.109	0.872	0.256	0.128
	Helpindex>0.113	30	0.342	0.165	0.268	.	.	.
Expected_giving	Helpindex≤0.113	18	0.337	0.134	-2.689	0.997	0.005	0.003
	Helpindex>0.113	26	0.508	0.220	0.007	.	.	.
Solidarity_norm	Helpindex≤0.113	33	0.312	0.148	-2.350	0.994	0.011	0.006
	Helpindex>0.113	56	0.419	0.208	0.019	.	.	.
Panel B.4 - N_Damages ≤ 2								
Giving	Helpindex≤0.113	33	0.319	0.167	0.772	0.310	0.620	0.690
	Helpindex>0.113	21	0.295	0.181	0.440	.	.	.
Expected_giving	Helpindex≤0.113	39	0.352	0.197	0.488	0.370	0.741	0.630
	Helpindex>0.113	18	0.333	0.202	0.626	.	.	.
Solidarity_norm	Helpindex≤0.113	72	0.337	0.183	0.975	0.256	0.512	0.744
	Helpindex>0.113	39	0.313	0.189	0.330	.	.	.

Table 5 - Determinants Of Giving

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Giving	OLS	OLS	OLS	TOBIT	TOBIT	TOBIT
damaged	-0.0614**	-0.0590**	-0.0604**	-0.0611**	-0.0582**	-0.0588**
	(0.0274)	(0.0277)	(0.0290)	(0.0282)	(0.0283)	(0.0283)
Riskloving_ratio		0.111**	0.112**		0.114**	0.115**
		(0.0454)	(0.0483)		(0.0466)	(0.0475)
Impatient		-0.0400	-0.0427		-0.0423	-0.0453
		(0.0286)	(0.0296)		(0.0292)	(0.0289)
Age			-0.00191			-0.00196
			(0.00122)			(0.00119)
Single			-0.0556			-0.0521
			(0.0688)			(0.0658)
Widowed			0.0273			0.0291
			(0.0347)			(0.0335)
Separated			0.112			0.111
			(0.0772)			(0.0734)
Male			0.0315			0.0285
			(0.0605)			(0.0598)
Food_exp_std			-0.000548			-0.000478
			(0.000958)			(0.000929)
Galle			-0.0108			-0.0101
			(0.0357)			(0.0352)
Hambantota			-0.0483			-0.0469
			(0.0355)			(0.0346)
Years_schooling			-0.00230			-0.00234
			(0.00650)			(0.00627)
N_house_members			-0.00657			-0.00721
			(0.0102)			(0.0100)
Trading			-0.0311			-0.0309
			(0.0280)			(0.0274)
Fishery			0.0412			0.0416
			(0.0491)			(0.0470)
Manufacturing			0.00212			-0.000159
			(0.0305)			(0.0302)
Trustindex			-0.0218			-0.0253
			(0.0511)			(0.0510)
Loancycle			-0.00195			-0.00208
			(0.00248)			(0.00243)
Observations	191	191	186	191	191	186
R-squared	0.027	0.068	0.129			

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Omitted benchmarks: *married*, *matara*, *agricolture*. Variable legend, see Table 1.

Table 6 - Determinants Of Expected Giving

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Expected Giving	OLS	OLS	OLS	TOBIT	TOBIT	TOBIT
Damaged	-0.0494*	-0.0504*	-0.0768***	-0.0490*	-0.0499*	-0.0774***
	(0.0277)	(0.0274)	(0.0291)	(0.0289)	(0.0284)	(0.0289)
Riskloving_ratio		-0.0317	-0.0363		-0.0387	-0.0424
		(0.0599)	(0.0618)		(0.0645)	(0.0636)
Impatient		-0.0670**	-0.0732**		-0.0695**	-0.0764***
		(0.0289)	(0.0293)		(0.0301)	(0.0293)
Age			-0.000138			-0.000350
			(0.00141)			(0.00142)
Sngle			-0.00555			-0.00265
			(0.0617)			(0.0595)
Widowed			0.0439			0.0464
			(0.0434)			(0.0422)
Separated			0.134**			0.138**
			(0.0526)			(0.0536)
Male			-0.0756			-0.0749
			(0.0580)			(0.0554)
Food_exp_std			-0.00365			-0.00371
			(0.00395)			(0.00395)
Galle			-0.0738**			-0.0759**
			(0.0343)			(0.0342)
Hambantota			-0.0354			-0.0400
			(0.0397)			(0.0404)
Years_schooling			-0.00958*			-0.0101*
			(0.00541)			(0.00534)
N_house_members			0.00711			0.00717
			(0.0121)			(0.0119)
Trading			0.0470			0.0500
			(0.0308)			(0.0303)
Fshery			0.0204			0.0208
			(0.0664)			(0.0642)
Manufacturing			-0.0452			-0.0470*
			(0.0276)			(0.0278)
Trustindex			-0.0245			-0.0219
			(0.0417)			(0.0424)
Loancycle			0.0110			0.0119
			(0.00785)			(0.00818)
Observations	191	191	186	191	191	186
R-squared	0.016	0.045	0.170			

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Omitted benchmarks: *married*, *matara*, *agricolture*. Variable legend, see Table 1.

Table 7.1 - Determinants of the Solidarity Norm

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Solidarity Norm	OLS	OLS	OLS	TOBIT	TOBIT	TOBIT
Receiver	0.0675***	0.0683***	0.0708***	0.0686***	0.0693***	0.0721***
	(0.0193)	(0.0192)	(0.0201)	(0.0200)	(0.0199)	(0.0204)
Damaged	-0.0554***	-0.0534***	-0.0627***	-0.0551***	-0.0528***	-0.0626***
	(0.0194)	(0.0196)	(0.0207)	(0.0202)	(0.0202)	(0.0209)
Riskloving_ratio		0.0412	0.0425		0.0399	0.0412
		(0.0379)	(0.0392)		(0.0399)	(0.0405)
Impatient		-0.0538***	-0.0611***		-0.0560***	-0.0637***
		(0.0205)	(0.0211)		(0.0212)	(0.0214)
Age			-0.00151			-0.00163*
			(0.000923)			(0.000938)
Single			-0.0251			-0.0212
			(0.0498)			(0.0489)
Widowed			0.0475			0.0501*
			(0.0302)			(0.0299)
Separated			0.132***			0.135***
			(0.0438)			(0.0431)
Male			-0.000919			-0.00219
			(0.0409)			(0.0412)
Food_exp_std			-0.000934			-0.000901
			(0.000870)			(0.000868)
Galle			-0.0483*			-0.0491*
			(0.0247)			(0.0250)
Hambantota			-0.0468*			-0.0480*
			(0.0250)			(0.0255)
Years_schooling			-0.00641			-0.00661
			(0.00406)			(0.00406)
N_house_members			-0.000993			-0.00103
			(0.00805)			(0.00808)
Trading			0.0124			0.0142
			(0.0205)			(0.0206)
Fishery			0.0422			0.0425
			(0.0370)			(0.0363)
Manufacturing			-0.0187			-0.0213
			(0.0207)			(0.0212)
Trustindex			-0.0238			-0.0243
			(0.0324)			(0.0333)
Loancycle			0.00386			0.00421
			(0.00389)			(0.00407)
Observations	382	382	372	382	382	372
R-squared	0.050	0.072	0.122			

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Omitted benchmarks: *married*, *matara*, *agricolture*. Variable legend, see Table 1.

Table 7.2 - Determinants of the Solidarity Norm

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Solidarity Norm	OLS	OLS	OLS	OLS (only damaged)	TOBIT	TOBIT	TOBIT	TOBIT (only damaged)
Receiver	0.0681***	0.0704***	0.0703***	0.0802***	0.0694***	0.0717***	0.0717***	0.0811***
	(0.0199)	(0.0196)	(0.0206)	(0.0277)	(0.0206)	(0.0202)	(0.0208)	(0.0271)
N_damages	-0.0182***	-0.0215***	-0.0254***	-0.0144*	-0.0182***	-0.0214***	-0.0255***	-0.0142*
	(0.00660)	(0.00655)	(0.00697)	(0.00832)	(0.00674)	(0.00664)	(0.00688)	(0.00802)
Help_ab_med	-0.00354	-0.0121	-0.0185	-0.0265	-0.00233	-0.0112	-0.0177	-0.0250
	(0.0262)	(0.0260)	(0.0265)	(0.0563)	(0.0266)	(0.0263)	(0.0263)	(0.0546)
N_damages*help_ab_med	0.0231**	0.0297***	0.0308***	0.0316**	0.0232**	0.0299***	0.0311***	0.0316**
	(0.0103)	(0.0103)	(0.0107)	(0.0157)	(0.0105)	(0.0104)	(0.0106)	(0.0152)
Riskloving_ratio		0.0605	0.0629	0.0816		0.0594	0.0620	0.0761
		(0.0393)	(0.0404)	(0.0574)		(0.0412)	(0.0415)	(0.0574)
Impatient		-0.0638***	-0.0712***	-0.0569*		-0.0662***	-0.0740***	-0.0589*
		(0.0207)	(0.0215)	(0.0315)		(0.0213)	(0.0217)	(0.0313)
Age			-0.00190**	-0.00228			-0.00202**	-0.00244*
			(0.000923)	(0.00148)			(0.000937)	(0.00146)
Single			-0.0363	0.0350			-0.0326	0.0406
			(0.0498)	(0.0620)			(0.0489)	(0.0594)
Widowed			0.0484	0.0620			0.0509*	0.0633
			(0.0294)	(0.0412)			(0.0290)	(0.0394)
Separated			0.105**	0.136***			0.107**	0.137***
			(0.0443)	(0.0477)			(0.0434)	(0.0452)
Male			0.00113	0.00371			-0.000110	0.00110
			(0.0429)	(0.0484)			(0.0429)	(0.0480)
Food_exp_std			-0.00101	-0.000877			-0.000981	-0.000883
			(0.000865)	(0.000858)			(0.000859)	(0.000824)
Galle			-0.0456*	-0.0383			-0.0464*	-0.0431
			(0.0247)	(0.0336)			(0.0250)	(0.0336)
Hambantota			-0.0490*	-0.0630*			-0.0502*	-0.0672*
			(0.0258)	(0.0364)			(0.0262)	(0.0360)
Years_schooling			-0.00633	-0.00529			-0.00650	-0.00516
			(0.00416)	(0.00526)			(0.00415)	(0.00511)
N_house_members			-0.00185	0.00399			-0.00188	0.00416
			(0.00788)	(0.0112)			(0.00788)	(0.0108)
Trading			0.0147	0.0440			0.0165	0.0463*
			(0.0207)	(0.0278)			(0.0207)	(0.0271)
Fishery			0.0499	0.0400			0.0496	0.0388
			(0.0425)	(0.0522)			(0.0416)	(0.0501)
Manufacturing			-0.0114	-0.0205			-0.0140	-0.0259
			(0.0210)	(0.0335)			(0.0215)	(0.0336)
Trustindex			-0.0256	-0.0308			-0.0264	-0.0294
			(0.0322)	(0.0492)			(0.0331)	(0.0490)
Loancycle			0.00437	0.00494			0.00473	0.00550
			(0.00400)	(0.00476)			(0.00420)	(0.00486)
Observations	372	372	362	192	372	372	362	192
R-squared	0.047	0.081	0.130	0.178				

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Omitted benchmarks: *married*, *matara*, *agricolture*. Variable legend, see Table 1.

Table 7.3 - Determinants Of the Solidarity Norm (IV estimates)

Dep Var: *Solidarity Norm*	(1)	(2)	(3)	(4)
Damaged	-0.0939**	-0.0959**	-0.0953**	-0.0935**
	(0.0414)	(0.0475)	(0.0402)	(0.0463)
Receiver	0.0681***	0.0716***	0.0709***	0.0742***
	(0.0193)	(0.0195)	(0.0194)	(0.0196)
Riskloving_ratio		0.0427		0.0368
		(0.0382)		(0.0384)
Impatient		-0.0602***		-0.0617***
		(0.0204)		(0.0205)
Age		-0.00136		-0.00117
		(0.000955)		(0.000948)
Single		-0.0191		-0.0192
		(0.0504)		(0.0507)
Widowed		0.0519*		0.0497*
		(0.0296)		(0.0296)
Separated		0.145***		0.143***
		(0.0465)		(0.0466)
Male		0.00637		0.00535
		(0.0414)		(0.0414)
Food_exp_std		-0.000855		-0.000834
		(0.000838)		(0.000839)
Galle		-0.0451*		-0.0479*
		(0.0241)		(0.0246)
Hambantota		-0.0483*		-0.0442*
		(0.0247)		(0.0247)
Years_schooling		-0.00707*		-0.00678*
		(0.00406)		(0.00403)
N_house_members		0.000161		6.61e-05
		(0.00815)		(0.00812)
Trading		0.0166		0.0153
		(0.0210)		(0.0209)
Fishery		0.0536		0.0509
		(0.0384)		(0.0417)
Manufacturing		-0.0187		-0.0195
		(0.0202)		(0.0202)
Trustindex		-0.0229		-0.0232
		(0.0320)		(0.0320)
Loancycle		0.00447		0.00417
		(0.00389)		(0.00385)
Observations	382	372	379	369
R-squared	0.040	0.115	0.039	0.114
Instruments	*distant*	*distant*	*distant, BMI*	*distant, BMI*
Endogeneity C-test for damaged: p-value	*0.309*	*0.454*	*0.242*	*0.461*
Test of excluded instruments (Weak Id.Test): F-stat	*108.8*	*79.62*	*58.32*	*41.25*
Overid.test: χ^2			*0.0766*	*0.366*
Overid.test: p-value	*.*	*.*	*0.782*	*0.545*

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Omitted benchmarks: *married*, *matara*, *agricolture*. Variable legend, see Table 1.

APPENDIX A

A.1 - DESCRIPTION OF THE RISK GAME

The RG provides us with a behavioral measure of risk aversion through a simple game which does not require a great deal of participants' familiarity with numbers and probabilities nor it leaves much room for interpretation to translators/experimenters. The game, adopted in a slightly different framework also by Charness and Genicot (2009) and Gneezy and Potters (1997), consists of a simple investment decision. Each participant is endowed with 300 LKR and has to decide whether keeping the money (option 1) or investing any portion x of it in a risky asset that has a 50% chance of success (option 2). The investment pays $3x$ if successful but zero if unsuccessful; the decision maker keeps all uninvested units.

A part from being easy to understand, the use of a 50% probability of success also avoids problems of subjective over-weighting of low-probability events (Charness and Genicot, 2009). In order to further simplify the comprehension of the chances of success/failure, we assigned the outcome to the toss of a coin. The amount invested (x) provides us with a rough proxy of risk aversion (the higher the investment, the less risk averse being the individual).

Participants are told they can earn up to 900 LKR (if the RG is selected for payment) according to their choice and the outcome of a fair coin that will be tossed at the end of the interview process. Then the game is explained, the participant makes his/her decision and the game ends. As far as the payment is concerned, as soon as the interview process ends we open the envelope containing the game previously extracted for payment. If the game selected for payment is the RG, we toss the coin and pay the subject according to her/his choice if (s)he opted for option 2; we pay 300 LKR otherwise.

A.2 - DESCRIPTION OF THE LOTTERY GAME

The final stage is a lottery which provides us with a behavioural measure of all participants' time preferences. In order to elicit time preferences in a standard incentivized way we implemented a (simplified) procedure similar to that used by Andersen et al. (2008) and Cassar et al. (2011). We tell participants they are involved in a lottery we are running among all the 380 people we are interviewing. If (s)he will be extracted, s(he) can win at least 10,000 LKR. The participant has to choose among two payment methods for the lottery, i.e. receive a prize of 10,000 LKR after 2 months from the interview date (option A) or receive a prize of 10,000 LKR *+ x* after 8 months (option B). Each participant repeats this decision for eight *potential lotteries*; in each of those, we increment x in option B, rewarding the "patient" option more than the previous. The increments in x are accounted for by a variation of the interest rate from 2% to 100%. Further details on the lottery and payoff table in the relevant experimental sheet are reported in the Appendix B . We use the "switch point" - namely, the potential lottery number at which the participant switches from option A to option B - as a measure of impatience. In particular, the later (sooner) the switch from option A to B - i.e. the higher (lower) the switch number - the more (less) participants are considered "*impatient*".

As far as the payment for the lottery game is concerned, we inform the participant that when all the other field interviews are finished, we extract one out of all the names of the people interviewed; the extracted name will be the only winner of this lottery. Then, we extract from another urn a number from 1 to 8 and we pay the winner only according to his/her choice in the potential lottery number equal to the one extracted. For example, if the number selected is 5, we pay the winner the amount corresponding to his/her choice in lottery 5. If the winner in lottery 5 chose to receive "10,000 after two months", we transfer that amount via "Western Union" after two months from his/her interview date. [29]

[29] Note that in a preliminary version of the experiment we originally designed a more complex experimental scheme to elicit risk and time preferences by using an approach more closely related to Anderson et al. (2008) and Holt and Laury (2002). Once in the field, we instead opted for the simpler one described above, thus sacrificing completeness/complexity for an adequate level of comprehension for both translators and participants. Consequently, we managed to collect more reliable data since with the original framework each interview process would have lasted for more than two hours and a half with the risk of generating non reliable answers (because of the high stress induced to translators and participants).

A.3 - DIRECT IMPACT OF RECOVERY AID

Table A - Determinants Of Giving, Expected Giving and Solidarity Norm

VARIABLES	(1) OLS (giving)	(2) OLS (giving)	(3) OLS (ex. giving)	(4) OLS (ex. giving)	(5) OLS (solidarity norm)	(6) OLS (solidarity norm)
Receiver					0.0701***	0.0714***
					(0.0203)	(0.0204)
Damaged	-0.0609**	-0.0669*	-0.0807***	-0.0990***	-0.0665***	-0.0803***
	(0.0294)	(0.0363)	(0.0286)	(0.0351)	(0.0204)	(0.0252)
Help_ab_med	0.0219	0.00696	0.0431	0.00465	0.0361*	0.00501
	(0.0286)	(0.0403)	(0.0297)	(0.0384)	(0.0204)	(0.0264)
Damaged*help_ab_med		0.0232		0.0612		0.0487
		(0.0572)		(0.0576)		(0.0400)
Riskloving_ratio	0.119**	0.121**	-0.0280	-0.0229	0.0531	0.0565
	(0.0495)	(0.0502)	(0.0638)	(0.0643)	(0.0399)	(0.0403)
Impatient	-0.0439	-0.0449	-0.0759**	-0.0810***	-0.0626***	-0.0658***
	(0.0299)	(0.0299)	(0.0302)	(0.0305)	(0.0213)	(0.0213)
Age	-0.00205*	-0.00210*	-0.000485	-0.000385	-0.00166*	-0.00168*
	(0.00123)	(0.00123)	(0.00146)	(0.00148)	(0.000934)	(0.000934)
Single	-0.0585	-0.0582	-0.00709	-0.00919	-0.0289	-0.0290
	(0.0694)	(0.0701)	(0.0706)	(0.0740)	(0.0506)	(0.0520)
Widowed	0.0276	0.0279	0.0714*	0.0662	0.0601**	0.0586**
	(0.0361)	(0.0363)	(0.0421)	(0.0432)	(0.0292)	(0.0294)
Separated	0.102	0.0972	0.115*	0.102	0.114**	0.107**
	(0.0787)	(0.0799)	(0.0591)	(0.0633)	(0.0458)	(0.0472)
Male	0.0332	0.0337	-0.0925	-0.0882	-0.00403	-0.00325
	(0.0607)	(0.0607)	(0.0616)	(0.0625)	(0.0429)	(0.0428)
Food_exp_std	-0.000629	-0.000628	-0.00380	-0.00424	-0.000944	-0.000989
	(0.000945)	(0.000942)	(0.00406)	(0.00412)	(0.000866)	(0.000866)
Galle	-0.0128	-0.0139	-0.0619*	-0.0611*	-0.0428*	-0.0431*
	(0.0357)	(0.0365)	(0.0345)	(0.0343)	(0.0248)	(0.0248)
Hambantota	-0.0512	-0.0541	-0.0342	-0.0379	-0.0471*	-0.0512**
	(0.0368)	(0.0375)	(0.0400)	(0.0406)	(0.0256)	(0.0259)
Years_schooling	-0.00258	-0.00247	-0.00853	-0.00840	-0.00648	-0.00626
	(0.00664)	(0.00669)	(0.00565)	(0.00571)	(0.00414)	(0.00415)
N_house_members	-0.00619	-0.00620	0.00787	0.00706	-0.000763	-0.00133
	(0.0106)	(0.0106)	(0.0121)	(0.0121)	(0.00801)	(0.00800)
Trading	-0.0238	-0.0241	0.0428	0.0433	0.0150	0.0147
	(0.0285)	(0.0286)	(0.0309)	(0.0310)	(0.0205)	(0.0205)
Fishery	0.0365	0.0341	0.00990	0.0114	0.0399	0.0385
	(0.0506)	(0.0516)	(0.0716)	(0.0719)	(0.0382)	(0.0384)
Manufacturing	0.00612	0.00532	-0.0364	-0.0350	-0.0119	-0.0120
	(0.0313)	(0.0318)	(0.0280)	(0.0280)	(0.0209)	(0.0209)
Trustindex	-0.0267	-0.0283	-0.0211	-0.0194	-0.0227	-0.0233
	(0.0513)	(0.0510)	(0.0426)	(0.0425)	(0.0326)	(0.0327)
Loancycle	-0.000527	-0.000304	0.0103	0.0102	0.00465	0.00486
	(0.00244)	(0.00255)	(0.00777)	(0.00783)	(0.00403)	(0.00403)
Observations	181	181	181	181	362	362
R-squared	0.126	0.126	0.183	0.187	0.132	0.134

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Omitted benchmarks: *married*, *matara*, *agricolture*. Variable legend, see Table 1.

A.4 - ROBUSTNESS CHECKS: *THE INVERSE PROPENSITY SCORE WEIGHTING METHOD*

Table 5A - Determinants Of Giving

Dep. Var. *Giving*	(1) WLS	(2) WLS	(3) WLS	(4) I.W. TOBIT	(5) I.W. TOBIT	(6) I.W. TOBIT
Damaged	-0.0926***	-0.0813**	-0.0799**	-0.0611**	-0.0801**	-0.0782**
	(0.0329)	(0.0315)	(0.0337)	(0.0282)	(0.0323)	(0.0331)
Riskloving_ratio		0.139***	0.151***		0.147***	0.157***
		(0.0520)	(0.0525)		(0.0546)	(0.0525)
Impatient		-0.0701*	-0.0653*		-0.0749**	-0.0697**
		(0.0356)	(0.0351)		(0.0369)	(0.0347)
Age			-0.00154			-0.00157
			(0.00131)			(0.00128)
Single			-0.109			-0.107
			(0.0734)			(0.0707)
Widowed			0.0326			0.0342
			(0.0375)			(0.0362)
Separated			0.157**			0.155**
			(0.0781)			(0.0748)
Male			0.0316			0.0295
			(0.0663)			(0.0642)
Food_exp_std			-0.000688			-0.000547
			(0.00130)			(0.00129)
Galle			-0.0228			-0.0213
			(0.0422)			(0.0408)
Hambantota			-0.0431			-0.0406
			(0.0452)			(0.0453)
Years_schooling			-0.00358			-0.00341
			(0.00902)			(0.00877)
N_house_members			-0.00544			-0.00612
			(0.0131)			(0.0132)
Trading			-0.0258			-0.0261
			(0.0323)			(0.0321)
Fishery			0.0306			0.0304
			(0.0615)			(0.0587)
Manufacturing			0.00349			0.00227
			(0.0305)			(0.0301)
Trustindex			0.0276			0.0259
			(0.0783)			(0.0816)
Loancycle			-0.00174			-0.00214
			(0.00349)			(0.00358)
Observations	185	185	184	191	185	184
R-squared	0.050	0.122	0.173			

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. the weights are computed as: $\frac{damaged}{\widehat{pscore_{(damaged)}}} + \frac{1-damaged}{1-\widehat{pscore_{(damaged)}}}$, where *pscore* is a non-parametric estimate of the propensity score (probability of *damaged*). The *pscore* is estimated using as regressors the following variables: *years_schooling, galle, hambantota, years_schooling, trading, fishery, manufacturing, BMI, distant, loancycle* (see variable legend in Table 1). Omitted benchmarks: *married, matara, agricolture*.

Table 6A - Determinants Of Expected Giving

Dep. Var. *Expected Giving*	(1) WLS	(2) WLS	(3) WLS	(4) I.W. TOBIT	(5) I.W. TOBIT	(6) I.W. TOBIT
Damaged	-0.0360	-0.0436	-0.0523*	-0.0344	-0.0421	-0.0511*
	(0.0298)	(0.0276)	(0.0307)	(0.0307)	(0.0283)	(0.0302)
Riskloving_ratio		-0.0445	-0.0389		-0.0525	-0.0460
		(0.0614)	(0.0585)		(0.0645)	(0.0588)
Impatient		-0.0987***	-0.0984***		-0.102***	-0.102***
		(0.0283)	(0.0312)		(0.0290)	(0.0307)
Age			-0.000141			-0.000238
			(0.00126)			(0.00124)
Single			-0.0395			-0.0390
			(0.0540)			(0.0518)
Widowed			0.0234			0.0245
			(0.0404)			(0.0387)
Separated			0.114**			0.116**
			(0.0527)			(0.0506)
Male			-0.130*			-0.130**
			(0.0665)			(0.0633)
Food_exp_std			-0.00628			-0.00657
			(0.00440)			(0.00433)
Galle			-0.0535			-0.0537
			(0.0366)			(0.0363)
Hambantota			-0.0120			-0.0125
			(0.0368)			(0.0360)
Years_schooling			-0.00940*			-0.00971*
			(0.00555)			(0.00546)
N_house_members			6.80e-05			0.000604
			(0.0145)			(0.0140)
Trading			0.0609**			0.0633**
			(0.0293)			(0.0287)
Fishery			0.00489			0.00404
			(0.0610)			(0.0588)
Manufacturing			-0.0419			-0.0430
			(0.0280)			(0.0278)
Trustindex			-0.000538			0.00200
			(0.0397)			(0.0398)
Loancycle			0.00882			0.00910
			(0.00688)			(0.00695)
Observations	186	186	185	186	186	185
R-squared	0.009	0.079	0.183			

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The weights are computed as: $\frac{damaged}{\widehat{pscore_{(damaged)}}} + \frac{1-damaged}{1-\widehat{pscore_{(damaged)}}}$, where *pscore* is a non-parametric estimate of the propensity score (probability of *damaged*). The *pscore* is estimated using as regressors the following variables: *years_schooling, galle, hambantota, years_schooling, trading, fishery, manufacturing, BMI, distant, loancycle* (see variable legend in Table 1). Omitted benchmarks: *married*, *matara*, *agricolture*.

Table 7.1A - Determinants of the Solidarity Norm

Dep. Var. *Solidarity Norm*	(1) WLS	(2) WLS	(3) WLS	(4) I.W. TOBIT	(5) I.W. TOBIT	(6) I.W. TOBIT
Receiver	0.0580**	0.0634***	0.0602***	0.0586**	0.0644***	0.0610***
	(0.0232)	(0.0218)	(0.0229)	(0.0240)	(0.0225)	(0.0232)
Damaged	-0.0642***	-0.0625***	-0.0654***	-0.0633***	-0.0614***	-0.0645***
	(0.0223)	(0.0213)	(0.0228)	(0.0230)	(0.0219)	(0.0228)
Riskloving_ratio		0.0484	0.0583		0.0480	0.0574
		(0.0409)	(0.0411)		(0.0427)	(0.0419)
Impatient		-0.0852***	-0.0870***		-0.0891***	-0.0909***
		(0.0234)	(0.0240)		(0.0242)	(0.0243)
Age			-0.00116			-0.00122
			(0.000895)			(0.000898)
Single			-0.0617			-0.0591
			(0.0454)			(0.0446)
Widowed			0.0394			0.0417
			(0.0304)			(0.0300)
Separated			0.125***			0.127***
			(0.0376)			(0.0374)
Male			-0.0256			-0.0264
			(0.0480)			(0.0472)
Food_exp_std			-0.00112			-0.00108
			(0.00121)			(0.00121)
Galle			-0.0384			-0.0380
			(0.0266)			(0.0268)
Hambantota			-0.0321			-0.0310
			(0.0287)			(0.0292)
Years_schooling			-0.00747			-0.00756
			(0.00491)			(0.00489)
N_house_members			-0.00511			-0.00506
			(0.00979)			(0.00992)
Trading			0.0213			0.0226
			(0.0217)			(0.0218)
Fishery			0.0398			0.0398
			(0.0372)			(0.0364)
Manufacturing			-0.0138			-0.0151
			(0.0205)			(0.0208)
Trustindex			0.0146			0.0158
			(0.0382)			(0.0398)
Loancycle			0.00287			0.00281
			(0.00395)			(0.00408)
Observations	371	371	369	371	371	369
R-squared	0.047	0.100	0.139			

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. the weights are computed as: $\frac{damaged}{\widehat{pscore_{(damaged)}}} + \frac{1-damaged}{1-\widehat{pscore_{(damaged)}}}$, where *pscore* is a non-parametric estimate of the propensity score (probability of *damaged*). The *pscore* is estimated using as regressors the following variables: *years_schooling, galle, hambantota, years_schooling, trading, fishery, manufacturing, BMI, distant, loancycle* (see variable legend in Table 1). Omitted benchmarks: *married*, *matara*, *agricolture*.

Table 7.2A - Determinants of the Solidarity Norm

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Solidarity Norm	WLS	WLS	WLS	WLS (only damaged)	I.W.TOBIT	I.W.TOBIT	I.W.TOBIT	I.W. TOBIT (only damaged)
Receiver	0.0598**	0.0674***	0.0621***	0.0837***	0.0606**	0.0686***	0.0631***	0.0853***
	(0.0240)	(0.0223)	(0.0235)	(0.0305)	(0.0247)	(0.0229)	(0.0238)	(0.0294)
N_damages	-0.0254***	-0.0305***	-0.0295***	-0.0242**	-0.0253***	-0.0305***	-0.0294***	-0.0242**
	(0.00885)	(0.00841)	(0.00904)	(0.0103)	(0.00900)	(0.00855)	(0.00894)	(0.00992)
help_ab_med	-0.00209	-0.0197	-0.0221	-0.0821	-0.000686	-0.0190	-0.0210	-0.0806
	(0.0284)	(0.0275)	(0.0274)	(0.0583)	(0.0287)	(0.0277)	(0.0270)	(0.0560)
N_damages* help_ab_med	0.0219*	0.0342***	0.0312***	0.0445***	0.0218*	0.0345***	0.0313***	0.0445***
	(0.0120)	(0.0117)	(0.0120)	(0.0169)	(0.0121)	(0.0118)	(0.0119)	(0.0162)
Riskloving_ratio		0.0662	0.0762*	0.108*		0.0661	0.0756*	0.100
		(0.0428)	(0.0431)	(0.0627)		(0.0446)	(0.0438)	(0.0626)
Impatient		-0.0946***	-0.0951***	-0.0710**		-0.0987***	-0.0992***	-0.0742**
		(0.0240)	(0.0249)	(0.0338)		(0.0248)	(0.0252)	(0.0333)
Age			-0.00137	-0.00172			-0.00143	-0.00174
			(0.000913)	(0.00154)			(0.000913)	(0.00149)
Single			-0.0652	0.0223			-0.0625	0.0256
			(0.0461)	(0.0614)			(0.0451)	(0.0585)
Widowed			0.0365	0.0660			0.0391	0.0656
			(0.0312)	(0.0443)			(0.0307)	(0.0423)
Separated			0.102**	0.148***			0.103**	0.150***
			(0.0447)	(0.0498)			(0.0443)	(0.0472)
Male			-0.0318	-0.0200			-0.0326	-0.0216
			(0.0518)	(0.0502)			(0.0507)	(0.0484)
Food_exp_std			-0.00112	-0.00102			-0.00108	-0.00105
			(0.00124)	(0.00114)			(0.00124)	(0.00109)
Galle			-0.0398	-0.0418			-0.0392	-0.0461
			(0.0271)	(0.0382)			(0.0272)	(0.0379)
Hambantota			-0.0343	-0.0462			-0.0331	-0.0488
			(0.0296)	(0.0372)			(0.0299)	(0.0361)
Years_schooling			-0.00744	-0.00538			-0.00749	-0.00511
			(0.00500)	(0.00637)			(0.00496)	(0.00611)
N_house_members			-0.00478	0.00550			-0.00470	0.00571
			(0.00997)	(0.0122)			(0.0101)	(0.0117)
Trading			0.0200	0.0442			0.0213	0.0452*
			(0.0221)	(0.0285)			(0.0221)	(0.0273)
Fishery			0.0552	0.0484			0.0546	0.0469
			(0.0417)	(0.0465)			(0.0405)	(0.0444)
Manufacturing			-0.00607	-0.0252			-0.00740	-0.0312
			(0.0208)	(0.0369)			(0.0210)	(0.0367)
Trustindex			0.0166	0.0194			0.0178	0.0231
			(0.0383)	(0.0618)			(0.0399)	(0.0621)
Loancycle			0.00292	0.00363			0.00284	0.00401
			(0.00402)	(0.00444)			(0.00416)	(0.00444)
Observations	361	361	359	191	361	361	359	191
R-squared	0.039	0.106	0.144	0.205				

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The weights are computed as: $\frac{damaged}{\widehat{pscore_{(damaged)}}} + \frac{1-damaged}{1-\widehat{pscore_{(damaged)}}}$, where *pscore* is a non-parametric estimate of the propensity score (probability of *damaged*). The *pscore* is estimated using as regressors the following variables: *years_schooling, galle, hambantota, years_schooling, trading, fishery, manufacturing, BMI, distant, loancycle* (see variable legend in Table 1). Omitted benchmarks: *married*, *matara*, *agricolture.*

APPENDIX B – NOT FOR PUBLICATION

B1 - INSTRUCTIONS - GAME "DG"

Today you are given the chance to play and earn real money. In this game you will be asked some questions and depending on how your and the other player's answers you may earn up to 900 LKR.
This game is based on a division of money between two individuals with anonymity, that is each player does not know the identity of the other. You play with someone from your village who is not present in this session and you do not know his/her identity nor he/she knows yours.
The game involves two roles, i.e. player one and player two. You are randomly chosen to play just one of these two. The other role is played by another person in this village.
We give to player one 900 LKR. Player one has to choose how much of this amount to keep for him/herself and how much to send to player two. Player two makes no choice in this game. After the first player has made his choice, the game ends.
Once you have finished this game, if this game is selected for payment we randomly match you with another person who play in the other role and we will pay both of you according to the decision of the person who played as player one.
Now let's start the game.

******

P1) You are chosen to play as *Player 1*. You are given 900 LKR as initial endowment. So you have to decide how much of this amount to send to player 2.

1. **How much of the 900 LKR would you give to the other player?**

☐ *0*
☐ *30*
☐ *60*
☐ *90*
☐ *120*
☐ *150*
☐ *180*
☐ *210*
☐ *240*
☐ *270*
☐ *300*
☐ *330*
☐ *360*
☐ *390*
☐ *420*
☐ *450*
☐ *480*
☐ *510*
☐ *540*
☐ *570*
☐ *600*
☐ *630*
☐ *660*
☐ *690*
☐ *720*
☐ *750*
☐ *780*
☐ *810*
☐ *840*
☐ *870*
☐ *900*

*****

P2) You are chosen to play as *Player 2*. No action is required at this stage. Please just answer to the following questions (you can earn money for correct guess).

1. **How much do you think the first player has sent to you?** *(you can earn 50 LKR for correct guess)*

☐ *0*
☐ *30*
☐ *60*
☐ *90*
☐ *120*
☐ *150*
☐ *180*
☐ *210*
☐ *240*
☐ *270*
☐ *300*
☐ *330*
☐ *360*
☐ *390*
☐ *420*
☐ *450*
☐ *480*
☐ *510*
☐ *540*
☐ *570*
☐ *600*
☐ *630*
☐ *660*
☐ *690*
☐ *720*
☐ *750*
☐ *780*
☐ *810*
☐ *840*
☐ *870*
☐ *900*

B2 - INSTRUCTIONS - GAME "RG"

Today you are given the chance to play and earn real money; depending on your decision in this game you may earn up to 900 LKR. This game is based on an investment decision.

We give to you 300 LKR and ask you to choose between the following alternatives:

- option 1: you keep the 300 LKR with certainty and do not invest any money.
- option 2: you invest from 30 to 300 LKR in an economic activity. You keep with certainty the sum you decided not to invest. Then, with 50% probability you earn from the economic activity an amount of money equal to the invested sum *multiplied by 3*. Otherwise, with 50% probability the economic activity you invested in generates for you no returns.

Once you have chosen one of the two options, we pay you according to the following scheme:

- If you choose option 1, we give to you 300 LKR at the end of this session if this game is selected for payment.
- If you choose option 2, we toss a coin and a) if it's head we triple the amount you decided to invest and give it to you at the end of the session if this game is selected for payment (in addition to the amount you decided to keep); b) if not, we will give you just the money you decided to keep at the end of this session if this game is selected for payment (so no extra returns from the investment).

*****

We give to you 300 LKR. Do you choose:

- option 1: I keep 300 LKR and do not invest, or
- option 2: I invest _____LKR in an asset which, after tossing a coin, triples my investment if it's head or gives me no money otherwise. *Please specify one of the following amount*s:

☐ *30*
☐ *60*
☐ *90*
☐ *120*
☐ *150*
☐ *180*
☐ *210*
☐ *240*
☐ *270*
☐ *300*

B3 - THE SURVEY

Thanks a lot for your patience. Your answers will be kept anonymous to other people in the village and to the AMF's staff. We will really appreciate if you can answer in a truthful way.

Question	Answer

1	Experimenter name	
2	Date	
3	Time	
4	District	
5	Type of locality (urban/rural)	

Personal Information

6	Name		
7	Family name		
8	Full Address / Locality		
9	Sex	[1] [2]	Male Female
10	Birthday (DD/MM/YYYY)		
11	Years of formal education		
12	Civil status	[1] [2] [3] [4] [5] [6]	Single Married Widow Divorced Separated Cohabiting
13	Which is your relationship to the head of the household?	[1] [2] [3] [4] [5] [6] [7] [8]	Head of Household Wife/Husband Son/Daughter Parent Other Relative Domestic Servant Boarder Other. Specify
14	Number of people living in the house		
15	Number of children (under 15 years old) living in the house		
16	Years of formal education of your wife/husband/fiancée		
17	Years of formal education of your father		

18	Years of formal education of your mother		

Economic Performance Indicators

0.1 Labour and income (2011)

19	Employment status	[1] [2] [3] [4] [5] [6] [7] [8] [9]	Full-Time Employed (30 hours or more) Part-Time Employed (less than 30 hours) Self-Employed Unemployed Student Household Work Retired Unable to Work Other. Specify
20	Sector of employment	[1] [2] [3] [4] [5]	Agriculture Fishery Manufacturing Trading Others. Specify
21	Employment status of your wife/husband/fiancée (if any)	[1] [2] [3] [4] [5] [6] [7] [8] [9]	Full-Time Employed (30 hours or more) Part-Time Employed (less than 30 hours) Self-Employed Unemployed Student Household Work Retired Unable to Work Other. Specify
22	Sector of employment of your wife/husband/fiancée (if any)	[1] [2] [3] [4] [5]	Agriculture Fishery Manufacturing Trading Others. Specify
23	Monthly income of the household in local currency	[1] [2] [3] [4] [5] [6]	2,500 – 5,000 Rs. 5,000 – 7,500 Rs. 7,500 – 10,000 Rs. 10,000 – 12,500 Rs 12,500 – 15,000 Rs > 15,000 Rs
24	How many hours per week do you work?		
25	How many hours per week does your wife/husband/fiancée (if any) work?		
26	How important from 1 (min) to 10 (max) are theseincome sources for the household's livelihood?	[1] [2] [3] [4] [0]	Remittances Sri Lanka's Government subsidies Donations and grants from other institutions and Organizations Others. Specify. No

0.2 Consumption (2011)

27	How would you judge your standard of living in terms of consumption goods?	[1] [2] [3] [4] [5]	Very good Good Sufficient Mediocre Not sufficient
28	Does it happen to you to have problems in buying or providing daily meals?	[1] [0]	Yes No
29	How much do you usually spend for food per month within your household? (in local currency)		
30	How much do you manage to produce by yourself for consumption?	[0] [1] [2] [3] [4] [5]	Nothing Little Much Very much Everything Not applicable (no self consumption)
31	Do you usually spend money for these goods and services?	[1] [2] [3] [4] [5] [0]	Private medical consultation fees Not reimbursed medicines Cigarettes and tobacco/alcohol/gambling Entertainment and leisure (pic nic, restaurants, cinema, DVD, theatre, sport etc.) Others. Specify No
32	Does your household own any transportation mean? If yes, please specify if it is necessary for your **business (B) or personal (P):**	[1] [2] [3] [4] [5] [0]	Truck Van or car Tractor Motorbike or three-wheel Bicycle No

Loan or credit-related questions

0.3 Microcredit

33	Who gave to you the first loan in your life?	[1] [2] [3] [4] [5] [6]	Bank AMF MFI (other than Agro Micro Finance) Family member or close friends Others. Specify. Never received a loan
34	If the previous answer is [1], [2] or [3], how did it happen?	[1] [2] [3] [4] [5]	I *did not need* a credit and they (Bank, AMF, other MFI) went to my place to offer the possibility of obtaining one I *needed* a credit and they (Bank, AMF, other MFI) went to my place to offer the possibility of obtaining one I needed a credit and I **spontaneously** went to their place to ask for it (Bank, AMF, other MFI) I needed a credit and I went to their place (Bank, AMF, other MFI) to ask for it, *because of other people's suggestion* Others. Specify
35	How important was the support provided by AMF after the tsunami for your economic recovery (whether in terms of a new loan or in better conditions for the repayment of a previous loan)?	[1] [2] [3] [4]	Critical † Very important † Important † Not that important

		[5] Indifferent [9] N/A	
36	How far was your house from the AMF's office (in km) at the time of your first loan?		
37	Were you able to repay the loan obtained before the tsunami, soon after this event?	[1] Yes [0] No	

In the period 2007- today:

38	Have you ever stopped receiving or repaying loans from/to AMF?	[1] Yes [0] No	
39.1	Is yes, why?	[1] Impossibility to repay the loan [2] Conditions too strict [3] Co-signers refused to pay for me [4] No need for a loan [5] AMF refused [6] Other. Specify. [7] Do not remember [8] Refuse to answer	
39.2	Have you started receiving loans once again from AMF?	[1] Yes [0] No	
39.3	If yes, when?		

For the year 2011…

39	Have you borrowed from AMF during this year?	[1] Yes [0] No	
40	Are you currently repaying to AMF?	[1] Yes [0] No	
41	If 40 **or** 41 are yes, why did you take the loan? If 40 **and** 41 are no, go to question 45.	[1] Start a new business [2] Improve the outstanding business [3] Recover the damaged business [4] Change business [5] Consumption [6] Others. Specify	
42	How would you judge the loan granted by AMF?	[1] Sufficient [2] Insufficient [9] N/A	
43	How would you judge your attendance to the monthly-meetings?	[1] Excellent [2] Very good [3] Good [4] Seldom [5] None [9] N/A	
44	Have you asked for money, apart from Agro Micro Finance, and were refused?	[1] Bank [2] MFI (other than Agro Micro Finance) [3] Family member or close friends [4] Other people/others. Specify. [0] No	
45	Have you obtained loans, apart from AMF?	[1] Bank [2] MFI (other than Agro Micro Finance) [3] Family member or close friends [4] Other people/others. Specify [0] No	
46	If yes, was the sum of these amounts greater or smaller than the one granted by AMF?	[1] Greater [2] Smaller	

		[3] The same [9] N/A			
47	Please indicate if you/people you know have received these different types of aid		You (y)	Relatives (r)	Others (o)
	a. Money				
	b. Credit				
	c. Food				
	d. Medicines				
	e. Raw material for repairing/rebuilding your house				
	f. Tools				
	g. Consumption				
	h. Others. Specify.				
48	Have you lent money?	[1] Family members [2] Close friends [3] Other people. [0] No			

0.4 Savings

49	How much did you save during the last year?	[1] Very much [2] Much [3] Pretty much [4] Not much [5] Not at all

Happiness, life satisfaction and self-esteem

50	All considered you would say that you are:	[1] Very Happy [2] Happy [3] Quite happy [4] Not too happy [5] Not at all happy
51	All considered, how satisfied are you with your life from 1 (not at all satisfied) to 10 (fully satisfied)?		
52	All considered, which is your level of self-esteem from 1 (no self esteem at all) to 10 (full self esteem)?		

Social Capital

53	Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?	[1] Most people can be trusted [2] Have to be careful
54	How much do you agree on the following statements	a) "*Nowadays, you can't rely on anybody* " [1] Agree [2] Neither agree or not agree [3] Disagree [4] Can't choose [5] Refuse to answer b) "*If you are not careful, other people will take advantage of you*" [1] Agree [2] Neither agree or not agree [3] Disagree [4] Can't choose [5] Refuse to answer

55	Do you belong to any group?		[1] yes - [0] no
	a. Sporting group		
	b. Neighbour group		
	c. Religious group		
	d. Community groups		
	e. Cultural group (music, dance, etc.)		
	f. NGO		
	g. Political Party		
	h. Other. Specify		

Health

56	All considered, how would you judge your level of health from 1 (not at all satisfied) to 10 (fully satisfied)?		
57	What is your weight (in kg)?		
58	What is your height (in cm)?		

Wealth

59	Does the house where you live belong to your family?	[1] [0]	Yes No
60	If yes, do you have?		Bedrooms (number) Bathrooms (number) Toilets (number) Kitchen
61	How far was your house located from the coast at the time of the Tsunami? (in km)		
62	Did you make any of the following dwelling improvements to your house? (in the period 2007-2011)	[1] [2] [3] [4] [5] [0]	New walls When? _____ New floors When? _____ New roof When? _____ New sanitary services When? _____ Other. Specify When? _____ No
63	What material are the walls of the main dwelling predominantly made of?	[1] [2] [3] [4] [5] [6] [7] [8] [9]	Stone, Brick/Block Mud/Wood Mud/Cement Wood only Corrugated iron sheet Grass/Straw Tin Other. Specify
64	What material is the roof of the main dwelling predominantly made of?	[1] [2] [3] [4] [5] [6] [7]	Corrugated iron sheet Tiles Concrete Asbestos sheet Grass Tin Other. Specify
65	What is the main source of water for the household?	[1] [2] [3]	Piped into dwelling Public tap Tube-well/borehole with pump

		[4] Protected dug well [5] Protected spring [6] Rainwater collection [7] Unprotected dug well/spring [8] River/Lake/ponds/streams [9] Tankers/Truck/Vendor [10 Bottled water [11 Other. Specify	
66	What type of toilet facilities does the household use?	[1] Flush toilet [2] Ventilated improved pit latrine [3] Uncovered pit latrine [4] Covered pit latrine [5] Bucket [6] None [7] Other. Specify	
67	Which of the following things does your household own?		Yes [1] no [0]
	a. TV, DVD player		
	b. Mobile phone		
	c. Fridge		
	d. Water pump		
	e. Plowing machine		
	f. Gas stove		

Recalling Tsunami

What kind of damages did you suffer from the tsunami?		a) Family members
	[1]	Dead
	[2]	Permanently injured
	[0]	No
		b) House
	[1]	Totally damaged
	[2]	Partially damaged
	[0]	No
		c) Economic activity
	[1]	Totally damaged
	[2]	Partially damaged
	[0]	No
		d) Buildings/assets
	[1]	Totally damaged
	[2]	Partially damaged
	[0]	No
	[1]	e) Working tools
	[2]	Totally damaged
	[0]	Partially damaged
		No
		f) Raw materials
	[1]	Totally damaged
	[2]	Partially damaged
	[0]	No

B4 - LOTTERY

Now we give to you the chance to participate into a lottery we are running. If you will be selected among all the people we interview, you can win at least 10,000 LKR.

You have to decide which option you prefer in 8 cases. In each of these 8 cases, you will be asked if you prefer to receive *after two months* the lottery prize of 10,000 LKR or *after eight months* a prize of an increasing amount in each option. So, you have to choose which of the two alternative forms of payment would you prefer if you won the lottery.

For example, the first option will be "*would you prefer to win 10,000 LKR after two months after this interview, or 10,100 after eight months after this interview?*" So you choose one of the two alternatives. This option will be repeated 8 times; in each of these we keep fixed the amount to be received "*after two month*" (10,000 LKR) in case of winning while the amount "*after eight months*" will be gradually increased option-by-option until 14,142 LKR.

All the people interviewed in this research will participate in this lottery. At the end of this research, we will extract from an urn one out of all the names of people we interviewed; that person will be the only winner of this lottery. Then, we extract from another urn a number from 1 to 8 and we will pay the winner according to his/her choice in the option number equal to the one extracted. For example, if the number selected is 5, we will pay the winner the sum of money corresponding to his/her choice in option 5. If the winner chose to receive "10,000 after two months", we will transfer that amount via "Western Union" after two month from his/her interview date; if instead she/he chose to receive "10,368 after eight months", we will be paying 10,368 LKR after eight months from his/her interview date.

Is it clear?

Let´s start.

Please select only one of the two choices for each of the following 8 options.

Option n.		***A***	***B***
1	*If you won the lottery, would you like to receive:*	10,000 after 2 month	10,100 after 8 months
2	*If you won the lottery, would you like to receive:*	10,000 after 2 month	10,198 after 8 months
3	*If you won the lottery, would you like to receive:*	10,000 after 2 month	10,368 after 8 months
4	*If you won the lottery, would you like to receive:*	10,000 after 2 month	10,607 after 8 months
5	*If you won the lottery, would you like to receive:*	10,000 after 2 month	10,840 after 8 months
6	*If you won the lottery, would you like to receive:*	10,000 after 2 month	11,180 after 8 months
7	*If you won the lottery, would you like to receive:*	10,000 after 2 month	12,247 after 8 months
8	*If you won the lottery, would you like to receive:*	10,000 after 2 month	14,142 after 8 months

IMPORTANT: If you will be the winner you will receive the money according to your extracted choice. If you do not receive any notification nor payment after 8 months from the date of this interview, unfortunately you have not been extracted.