Disaster, Aid and Preferences: The Long-run Impact of the Tsunami on Giving in Sri Lanka

Published version:

DOI:10.1016/j.worlddev.2016.12.014

Disaster, Aid and Preferences:

The Long-run Impact of the Tsunami on Giving in Sri Lanka

Leonardo Becchetti (corresponding author), University of Rome “Tor Vergata”, Via Columbia 2, 00133, Rome (Italy). becchetti@economia.uniroma2.it, Cell +39 338 6331883, Fax +39-06-2020500

Stefano Castriota, Free University of Bozen, Piazza dell’Università 1, 39100, Bozen (Italy). stefano.castriota@unibz.it

Pierluigi Conzo, University of Turin, Lungo Dora Siena 100 A, 10153, Turin (Italy); CSEF (Naples, Italy) and Collegio Carlo Alberto (Turin, Italy). pierluigi.conzo@unito.it

Disaster, Aid and Preferences:

The Long-run Impact of the Tsunami on Giving in Sri Lanka

First version: *May 2012*

This version: *November 2016*

Abstract

Do natural disasters produce effects on preferences of victims in the long-run? We test the impact of the tsunami shock on generosity of a sample of Sri Lankan affected/unaffected microfinance borrowers seven years after the event.

Specifically, 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 a dictator game. Moreover, at the intensive margin, we compare the participants based on the amount of damage experienced and recovery aid received. The advantage of this last comparison is that differences in observables between the groups are minimized. 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, likely to share some important common traits that are usually unobservable to researchers. We complete our identification strategy with weighted least squares, instrumental variable estimates and a sensitivity analysis on the exogeneity assumption.

The main findings of the paper support the hypothesis that the shock affects participants' preferences in the long-run. First, the tsunami negatively affects generosity at the extensive margin as those who suffered at least one damage give and expect less than those who did not. Second, while large recovery assistance does not directly affect giving and expected giving, it increases especially the latter indirectly, i.e., when interacted with the number of damages.

Our results reconcile that part of the literature showing evidence of natural shocks having a detrimental effect on social preferences (Fleming et al., 2011; Cassar et al., 2013) with that supporting, instead, a positive link (Solnit, 2009; Whitt and Wilson, 2007; Cassar et al., 2011). Furthermore, since our study focuses on the long-run impact of a natural disaster, previous results on short-run effects are not necessarily inconsistent with ours.

Keywords: natural disasters; tsunami; giving; dictator game; recovery aid.

JEL codes: C90, D03, O12.

1. INTRODUCTION

Natural disasters are dramatic shocks that may produce, beyond all of the visible damages, relevant consequences at a micro level by affecting expectations, preferences and choices of economic agents with consequences on their consumption/savings and human capital investment decisions (see Morris and Wodon, 2003; Gitter and Barham, 2007; Carter et al., 2007; Becchetti and Castriota, 2011; Arouri et al., 2015).

A recent branch of empirical papers has started to investigate this topic with conflicting conclusions related to the disaster's effects on time preferences (Cassar et al., 2011; Callen, 2015), attitudes toward risk (Eckel et al., 2009; Cassar et al., 2011; Cameron and Shah, 2015; Willinger et al., 2013; Kim and Lee, 2014) and prosocial behaviors (Whitt and Wilson, 2007; Solnit, 2009; Cassar et al., 2011; Castillo and Carter, 2011; Fleming et al., 2011). For what concerns the latter, Whitt and Wilson (2007) consider individuals affected by Hurricane Katrina and find increased group cooperation among the evacuees in Houston shelters. Similarly, Solnit (2009) provides evidence that disasters are often catalysts for social capital increase and Cassar et al. (2011) - by exploiting the variation in victimization status at village level - find that tsunami victims are more trusting and moderately more trustworthy. Castillo and Carter (2011) use the community-level variation in rainfalls during the Hurricane Mitch in 1998 as a proxy for damage intensity and find that intermediate shocks promote cooperation while extreme shocks undercut it. On the contrary, Fleming et al. (2011) find that residents of the areas affected by the Chilean 2010 earthquake reveal significantly less trustworthiness than those of non-affected ones. Only a few studies, however, focus on generosity. In particular, Li et al. (2013) compare dictator-game behavior of Chinese children before and after the 2008-earthquake and find that its effects on generosity vary by age, being negative on 6-year-olds and positive on 9-year-olds children. The 2008 Chinese Wenchuan earthquake has been shown by Rao et al. (2011) as affecting positively adults' generosity. This effect is shown to increase with the levels of residential devastation.

In this study, we focus on the long-term impact of the 2004-tsunami on the generosity of inhabitants of the Sri Lankan southern-coast affected by the shock at different degrees. We contribute in four main respects to the current debate that clearly presents mixed evidence regarding the direction of the effects of disaster.

First, we argue that the factors that may help to explain the observed heterogeneous results in the literature are, on the one hand, the degree of damage suffered and, on the other hand, the contextual recovery aid received by damaged villagers. For this purpose, we collect and use information on both the type of damages and of recovery aid received.

Second, Callen (2015) and Cassar et al. (2011) collected data and ran experiments on the effects of the tsunami on trust, risk and time preferences in mid-2007 and mid-2009, respectively, while our database dates December 2011, seven years after the catastrophe. This longer time horizon allows us to capture longer run disaster and recovery effects on victims' preferences.

Third, different from both Callen (2015) and Cassar et al. (2011), by exploiting information on individuals' victimization statuses as well as on the intensity of damages and recovery aid within each village, we do not measure the impact of the shock at the village level but rather at the individual level. This approach reduces heterogeneity between the treatment group (village inhabitants affected by the tsunami) and the control group (unaffected village inhabitants).

Fourth, both damaged and non-damaged individuals in our study belong to a selected group of individuals borrowing from the same microfinance organization (MFI). This implies that they share some common unobservable factors (i.e., sense of entrepreneurship, trustworthiness) that are typically inaccessible to the experimenter and are the main drivers of self-selection. These factors also help this group of individuals to pass the screening of the same MFI that has salient incentives to select only potentially successful borrowers. Furthermore, because the MFI under our scrutiny organizes frequent borrower meetings (as many others traditionally do), we also reasonably assume that damaged and non-damaged individuals share similar cultural features represented by the organization *ethos*. In addition to it, only damaged individuals who were AMF borrowers before the tsunami could receive aid according to donors' rules and AMF policies and this institutional aspect rules out the suspect of correlation between unobservable characteristics and the status of damaged borrowers in our sample.

The combination of these features, jointly with the results of inverse p-score weighted least squares as well as instrumental variable estimations and a sensitivity analysis on the exogeneity assumption implemented as robustness checks, contribute to mitigate the identification problem arising from the impossibility of randomizing ex-ante the calamity experience and from the lack of pre-tsunami individual information. To address the non-representativeness of our sample composed by microfinance borrowers, we implement a post-stratification weighted least squares estimate based on Census data.

Our findings show that the tsunami negatively affects generosity after seven years from the calamity; however, especially for the highly damaged individuals, this effect is compensated by the recovery assistance received. These results suggest the existence of an indirect and non-material channel (i.e. indirect reciprocity) through which properly targeted recovery aid compensates for the losses in pro-social attitudes caused by the disaster.

The paper is divided into seven sections. In the second, we formulate research hypotheses, illustrate the experiment background and describe our research design. In the third we present descriptive findings and in the fourth we discuss results on hypotheses testing. In the fifth and sixth we illustrate and comment on our econometric findings and robustness checks. The seventh section concludes the paper.

2. RESEARCH DESIGN

In what follows we first formulate research hypotheses (subsection 2.1). We then sketch the historical scenario in which our research is conducted (subsection 2.2) and discuss the details of our experiment design (subsection 2.3).

2.1 Research hypotheses

Our aim is to test whether the tsunami shock affects generosity as proxied for in a dictator game (see section 2.3 for details) by:

i) sender's giving;

ii) receiver's expectation regarding sender's giving.

Given the longer time distance from the shock in our experiment with respect to similar studies in the literature, our hypotheses may be considered as tests on the long-run effects of the tsunami on social preferences. More formally, we first test the null hypothesis that the tsunami has no long-run impact at the extensive margin on giving or on expected giving using the following two specifications (see subsection 2.3 for a detailed description of the damages variables):

i)	*Giving*	H_0: $G^{Dam} = G^{NonDam}$
ii)	*Expected Giving*	H_0: $\mathrm{E}[G]^{Dam} = \mathrm{E}[G]^{NonDam}$

where G^{Dam} *and* G^{NonDam} are, respectively, the amounts given by damaged and non-damaged senders, while $\mathrm{E}[G]^{Dam}$ and $\mathrm{E}[G]^{NonDam}$ are the amounts that recipients from the two groups expect to receive from the sender.

Our alternative hypotheses ($H_1^{i,ii}$) are that the calamity does reduce giving and expected giving. The channels through which this might occur are of economic and psychological type. With respect to the first mechanism, natural catastrophes can decrease generosity through reduced savings and increased propensity to precautionary savings. After a natural hazard – and especially in developing countries where insurance schemes are weakly implemented – damaged people might be forced to use their personal resources to recover, for example by re-building the house or the office, rebuying the working tools or raw materials or just to survive until the economic situation improves. For the same reason, even well after recovery, individuals might react by increasing their savings to cope with possible similar events in the future (Roson et al., 2007). In a theoretical model of constant absolute risk aversion, Freeman et al. (2003) show that the optimal amount of precautionary savings depends on the expected losses, which in turn depend on the probability of the event, the damages suffered and the individual risk aversion. Therefore, the rise in the expected probability of negative events and the damage entity are expected to increase savings, especially if people become more risk averse (Cassar et al., 2011; Cameron and Shah, 2015) and more patient (Callen, 2015).

With respect to the second mechanism, the literature has shown that natural disasters produce unhappiness, bad mood and anger, which can negatively affect interactions and cooperation. Västfjall et al. (2008) find that the effect elicited by reminding Swedish undergraduates about the 2004-tsunami disaster negatively influences their judgments on wellbeing, future optimistic thinking and risk perceptions. Happiness levels are shown in turn to influence the taste for social comparisons as the willingness to lower another person's payoff below one's own is correlated with unhappiness (Charness and Grosskopf, 2001). The aforementioned channels could possibly have played a role in reducing generosity as calamities decrease happiness of damaged people in developing and developed contexts (Becchetti and Castriota, 2010 and 2011; Luechinger and Raschky, 2009).

Previous studies have found evidence of a non-linear impact of shocks magnitude on preferences (e.g., Castillo and Carter, 2011). This leads us to consider also the impact of the tsunami at the intensive margin, by testing the null hypotheses of insignificant differences in giving and expected giving between those with a large *vis-à-vis* those with a small amount of damages:

iii)	*Giving*	H_0: $G^{HighDam} = G^{LowDam}$

iv)	*Expected giving*	H_0: $E[G]^{HighDam} = E[G]^{LowDam}$

Furthermore, to test whether damaged individuals are affected differently according to the amount of damage experienced and the recovery aid received[1], we draw additional hypotheses on the effect of solidarity help on giving and expected giving of respondents who incurred a high amount of damage:

v)	*Giving*	H_0: $G^{HighDam\|HighHelp} = G^{HighDam/LowHelp}$
vi)	*Expected giving*	H_0: $E[G]^{HighDam\|HighHelp} = E[G]^{HighDam/LowHelp}$

where *HighDam* are victims who suffered more than two damages (two is the median number of damages) and *LowDam* are people who suffered less than or equal to two damages; *HighHelp* are individuals who received more than the sample average level of help (i.e., *Helpindex*>0.113), while *LowHelp* are individuals who received an amount of help equal to or below the average level of help (i.e., *Helpindex*≤0.113).

The null hypotheses test herein states that individuals who receive high damages and help above the average do not show differences in their long-term preferences from those who did not receive help above the average level in the presence of serious damages. Accordingly, the rejection of the null hypotheses implies that significant help in the presence of high damages matters. The rationales supporting the alternative ($H_1^{v,vi}$) are in line with the economic and psychological arguments described above. From an economic point of view, unprecedentedly high compensation from public institutions and foreign donors might decrease precautionary savings through moral hazard effects since, in case of future catastrophes, people rely on external support. This phenomenon, known as the Samaritan's Dilemma (Buchanan, 1975; Coate, 1995) or the Charity Hazard (Raschky and Weck-Hannemann, 2007; Dobes et al., 2014) has been found empirically by Berlemann et al.

[1] Regarding the role of post-shock relief on social behavior see, among others, Fearon et al. (2009) and Korf et al. (2010).

(2015) with German GSOEP data.[2] On the psychological side Becchetti and Castriota (2010, 2011) document that post-disaster aid produces convergence of damaged individuals to previous levels of both economic and psychological wellbeing. This result is consistent with the above-mentioned nexus between psychological wellbeing and giving attitudes, which is grounded in the literature and in line with our alternative hypothesis.

A further rationale for the positive effect of post-disaster aid on giving is indirect reciprocity. This type of preference is widely recognized as an important mechanism for the evolution of cooperation in natural and social sciences, both theoretically and empirically (e.g. Axelrod, 1984; Nowak and Sigmund, 2005; Leimar and Hammerstein, 2001). So far experimental evidence of strong indirect reciprocity was available mainly in lab-experiments involving undergraduate students (e.g. Dufwenberg et al., 2001; Guth et al., 2001; Stanca, 2009) and not tested on individuals who lived strong real life experiences of shock and recovery. Specifically, the rejection of the null hypothesis in favor of $H_1^{v,vi}$ can be supported by the fact that highly damaged individuals witnessed very critical conditions after the tsunami but received larger recovery assistance, as documented also in our data and in previous studies (Becchetti and Castriota, 2010 and 2011). This, in turn, would have activated in them a higher and long-lasting sense of gratitude than in those who received less or no damages from the tsunami. A plausible rationale for our alternative hypothesis is therefore that those who have lived in their past a strong memorable and intense experience of a shock followed

[2] The authors study the European Flood of August 2002 and find that, as a possible consequence of the Samaritan's Dilemma, the natural disaster depressed individual saving decisions. In Germany, well-established compulsory insurance schemes, public funding and donations were so high that they turned out to provide almost total compensation (Linnerooth-Bayer et al., 2001), which is not our case. Nevertheless, those people who registered the highest number of damages and received more financial assistance might have reduced precautionary savings and thus increased generosity.

by a large recovery aid do not forget the help received and therefore behave (and expect others to behave) generously even under milder circumstances as those reproduced in our experiment.

2.2 The Background

Sri Lanka was severely affected by the 2004-tsunami. Over 1,000 km of coast (two-thirds of the country's coastline) were affected by the storm. The disaster 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 offered aid and support. The specific characteristics of this event was that it may have randomly affected individuals living a short distance from each other based on their location with respect to the waterline at the moment of the tsunami struck (Figure 1 in the online Appendix). As it typically occurs in natural disasters the variation in damage was quite strong and randomly affecting inhabitants along the coast (and specifically in the area considered in our research) due to factors such as the irregular shape of land but also the irregularity of the same run-up and height of the wave, of other manmade structures, of natural barriers between the coast and the damaged houses. As a consequence each affected member had between her/him and the coast different natural and manmade obstacles not depending on her/his will that could reduce/not reduce (together with the irregularity of wave intensity) the impact of the wave on their houses. Evidence of the importance of these factors in the tsunami event is provided, among others, by Liu et al. (2005) for Sri Lanka, Tanaka (2011) for West Java. Thus, this unfortunate event created a particularly favorable scenario for investigating the effects of calamities and aid on individual preferences in a quasi-experimental environment.

In November 2011, our research team conducted the field portion of this study in Sri Lanka with the support of a local staff. Using a list of clients obtained from a local microfinance institution, Agro Micro Finance (AMF), we randomly selected 382 borrowers (all of them identified and interviewed thanks to the help of the AMF staff thereby eliminating the related attrition problem). To the best of our knowledge, only one microfinance organization is present in the selected villages and therefore no randomization is possible at the organizational level. Of those borrowers, with the help of the AMF staff, we identified a group of individuals affected by the 2004-tsunami and a group of individuals who were not affected by the seaquake. Participants in our experiment originated from three villages located on the southern coast of Sri Lanka, namely, Galle, Matara and Hambantota. As documented by Figure 2 in the online Appendix, the three chosen villages were only partially affected by the calamity. This circumstance provides us with the opportunity to exploit such within-village heterogeneity. Hence, in contrast to the studies summarized in the introduction, where all damaged people were selected from one village and all non-damaged people were from another one, we sampled both damaged and non-damaged participants within each of the three villages. 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.

AMF loan officers informed us about the damaged/non-damaged status of the borrowers prior to beginning the experiment. Hence we were able to assign ex-ante participants to the correct group, damaged or non-damaged, in each village and therefore avoid potential framing effects arising from asking players for their damaged/non-damaged status before the beginning of the game.

Moreover, our sample is not likely to suffer from post-tsunami migration because of the economic activity held *in loco* and the high incentives for damaged individuals to recover from the calamity and stay. Such incentives include incoming flows of aid and the concession of micro-loans at favorable conditions as a result of the AMF's portfolio recapitalization after the tsunami (Becchetti and Castriota, 2010 and 2011). Around 24.4 percent of the MFI loan portfolio was damaged after the shock, but the liquidity provided by foreign institutions (USAID, UNDP, the Italian MFI Etimos) allowed AMF to avoid credit restrictions. The loans received by AMF helped damaged clients to recover from the calamity so that already in 2007 the gap with respect to non-damaged had reduced or disappeared. This could contribute to explain the evidence provided by AMF showing that only six out of four thousands of borrowers migrated the year after the Tsunami (2005). Likewise, with data on tornado victims in Bangladesh, Paul (2005) provides similar empirical evidence showing that there is no out-migration because in monetary terms the disaster relief received was bigger than the damage incurred.

2.3 The Experiment

The fieldwork consists of three parts: i) an experimental session to elicit generosity and risk preferences, ii) a socio-demographic survey, iii) a final lottery game to elicit (and control for) time preferences.

With respect to the experimental session, we implemented three games: a dictator game (DG), a risky investment game (RG) and a lottery game (LG). The first game is the target of our investigation while, as it is customary in many experiments, the risk and preference games were conceived just as more clean ways of measuring important controls in our estimates given that risk aversion and time preferences have often been found to affect the payoffs of experiments (e.g. Curry et al., 2008; Kanagaretnam et al., 2009). We randomly alternated the two games to avoid order effects.[3]

The DG is a standard game largely adopted in the literature to elicit generosity in an incentive-compatible way (see, for instance, Eckel and Grossman, 1996 and Engel, 2011). The game involves two types of players, a sender (S) and a receiver (R). Their true identity is not revealed, and thus no player can identify the person with whom (s)he is playing. S is endowed with 900 LKR (the equivalent of 5.74 €) and must decide how much of it to send to R; R takes no actions in this game and receives the amount of money sent by S. According to the classic utility theory, the sender's

[3] We do not find any significant effect of the damaged/non-damaged status or the kind of damages on risk/time preferences (results are omitted for reasons of space and are available upon request). This suggests lack of collinearity problems when using them in our estimates.

maximum utility is reached by sending 0 LKR and keeping the entire endowment (900 LKR). Any sender amount deviating from 0 is interpreted as a measure of generosity. The role of experiment participants is randomly selected. The experimental protocol is described in details in the online Appendix A.2b.

The RG used to capture risk aversion consists of a simple investment decision where participants endowed with 300 and must decide whether to keep the money (option 1) or invest any portion *x* of the money in a risky asset that has a 50 percent chance of success (option 2). The investment pays *3x* the amount invested if successful, but zero if unsuccessful. The LG is implemented to measure time preferences and it is based on a real lottery in which each participant choose whether to receive a given amount of money two months from the interview date or a larger amount (of varying size) eight months later. Further details on these games and the experimental sheets are reported on line in Appendix A (sections A1a-A1b) and Appendix B respectively.

At the end of the experimental session, participants are asked questions concerning socio-demographic information, social preferences, damages incurred 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 recovery aid they received shortly after the tsunami with respect to eight dimensions (i.e., money, credit, food, medicines, raw materials, working tools, consumption, other). Survey-based information on social preferences are also collected through standard GSS questions (see the questionnaire in the online Appendix B).

Information on type of damage and recovery aid experienced are collected through direct survey questions asked to AMF borrowers and verified by the AMF loan officers (see Table 1 and the online Appendix B for further details). Using this information, we classify individuals in our sample into damaged vs. non-damaged (variable *damaged*) and high vs. low level of help (variable *help_ab_mean*), respectively, if they have vs. have not experienced at least one damage from the tsunami and the sum of recovery aid they received (across all of the above-mentioned dimensions) is above vs. below the sample mean. By using the total sum of damages the individuals reported (variable *n_damages*), we evaluate the impact of the tsunami on generosity at the intensive margin as well (see Variable Legend in Table 1 for further details). For the description of the interview process and the experimental protocol see the online Appendix A.2a-A.2b.

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)
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	number 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 online Appendix B for the options in each lottery.
Galle	= 1 if the respondent lives in the Galle district
Matara	= 1 if the respondent lives in the Matara district
Hambantota	= 1 if the respondent lives in the 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 = Must be careful, 2 = Most people can be trusted
Cant_rely	respondent answers 1 to 5 Likert scale agreement with the statement, "Currently, you cannot rely on anybody."
People_take_advantage	respondent answers 1 to 5 Likert scale agreement with 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 number 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 of 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* conditional 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 aid after the tsunami
Helpindex	= sum of *_aid dummies /8
Help_ab_mean	= 1 if *Helpindex* > 0.113 [0.113 is the sample mean of *Helpindex*]

The incentives provided to participants should encourage truthful reporting as the amount at stake is very large considering the 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, approximately 51 percent of the median per capita monthly food expenditure.

3. DESCRIPTIVE FINDINGS

Summary statistics of our sample document that the average age of the participants is 47 and most of them, around 94 percent, are women. This replicates the data of some of the main microfinance organizations in Asia (Panel A, Table 2).[4] The average number of household members is 4.5, the majority of our sample participants is married (84 percent) and the average number of schooling years is 10.5 (two and a half years of secondary school). Slightly more than half of the participants (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 for the full sample. Most individuals received food (19 percent), money or medicines (respectively, 16 percent and 15 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_mean*). As presented in Panel C of Table 2, most of the villagers with at least one damage experience losses

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

to economic activity (77 percent) and to office buildings/assets (44 percent), while approximately 40 percent report damages to working tools or raw materials, 26 percent declare damages to the house, 23 percent report injuries to relatives and only a small fraction, 9 percent, receives personal injury aid. Among the damaged, the mean number of damages is 2.6 (variable *N_damages*) with

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
Agriculture	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_mean	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
Receiver	382	0.500	0.501	0	1
Riskloving_ratio	382	0.590	0.285	0	1
Switch	382	5.861	2.987	1	9

Variable legend: see Table 1

approximately 45 percent reporting a number of damages above this number (variable *N_dam_ab_mean*).

Aid is significantly correlated with damage while uncorrelated *per se* with most of other individual's observable characteristics (results omitted for reasons of space). When looking at the aid received by types of damage, in general those who report damages to the house and or injuries show a larger amount of recovery assistance received than those who report only losses to the economic activity (the difference is statistically significant under all comparisons; for details see the online Appendix A.2c). Finally, Panel D of Table 2 indicates that almost 63 percent of the participants are relatively impatient[5] and, on average, 60 percent of the amount at disposal is invested in the risky option (variable *Riskloving*, see Table 1 and the online Appendix A1a-b and B for further details).

To determine whether the identification problem is serious, we implement non-parametric tests to check for satisfaction of the balancing properties between damaged and non-damaged individuals. Given the lack of pre-tsunami information on our respondents across the considered socio-demographic and economic dimensions, we use current individual characteristics to check for satisfaction of balancing properties even though they may be endogenous (the tsunami could have affected them). Consider, however, that some of our controls are variables that are not likely to be affected by the shock (e.g., age, education, gender, etc.). We find that some differences are

[5] They switch from option A to option B in a potential lottery number greater than or equal to the median (i.e., seven). See online Appendix A.1b for details of the lottery game.

significant at the 5 percent level (Panel A, Table A3.1 in the online Appendix) as the damaged individuals are, on average, 4.5 years older, married and, as expected, living closer to the coast (3.5 Km vs. 10.9 Km) than the non-damaged individuals. This is consistent with higher survival rates to the tsunami shock of young adult males as documented by Frankenberg et al. (2011). However, most of the significant differences at the 5 percent level in the observables vanish if we discriminate within the damaged group using the number of damages reported (Panel B, Table A3.1 in the online Appendix) or when comparing all individuals receiving more vs. those receiving less than the sample average amount of aid (Panel C, Table A3.1 in the online Appendix). Furthermore, under this last comparison, the recovery assistance seems to have reached the most affected individuals as people with a larger number of damages also received significantly more aid than those reporting fewer damages. In sum, when comparing individuals either by the number of damages or the amount of the help received, our sample is balanced regarding most of the socio-demographic characteristics. The only exceptions are the civil status (in particular, *separated*) and the employment sector (in particular, *agriculture*), which are controlled for in the econometric analysis.

4. HYPOTHESIS TESTING: RESULTS

In Table 3, we report the results of all of the non-parametric tests for the hypotheses outlined in section 2.1 plus some additional checks that are useful for placing our results in a clearer framework. Hypotheses i and ii are tested in Panel A.1, while hypotheses iii and iv in Panel B.1.

Results from the non-parametric tests suggest a negative and significant relationship between the tsunami experience and giving and expected giving at the extensive margin with damaged participants showing less generosity than non-damaged ones (Panel A.1). Moreover, a preliminary statistical analysis at the intensive margin documents a weakly significant correlation between number of damages and giving and expected giving among the damaged as well as a non-linear relationship between damages and generosity for the whole sample. As we further discuss in section 5.2, this result can depend on the fact that the *N_damages* variable indirectly captures also the amount of recovery aid received by the damaged individuals. In fact, for these persons both variables affect generosity in the same direction, though their effect is (marginally) significant only for receivers (see Panels B.1 and B.2 of Table 3). For this reason, we try to disentangle the effect of the recovery aid from that of the damages through the statistical tests presented below and, in order to net out potential confounders, through the regression analysis on the whole sample (section 5.2).

Table 3 - Testing Generosity By Damage

Variable	**Group**	**Obs**	**Mean**	**Std. dev.**	**Non-par. Mann–Whitney U test:** *(z-stat. in parenthesis; p-value in italic)*
			A) WHOLE SAMPLE		
			panel A.1		
Giving	Non-damaged	89	0.372	0.205	(2.129)
	Damaged	102	0.31	0.168	*0.033*
Expected_giving	Non-damaged	86	0.433	0.179	(2.519)
	Damaged	105	0.383	0.204	*0.012*
			panel A.2		
Giving	Helpindex<0.113	121	0.341	0.201	(-0.175)
	Helpindex>0.113	65	0.34	0.163	*0.861*
Expected_giving	Helpindex<0.113	125	0.391	0.19	(-1.382)
	Helpindex>0.113	61	0.44	0.202	*0.167*
			B) DAMAGED ONLY		
			Panel B.1		
Giving	N_Damages≤2	57	0.301	0.171	(-0.824)
	N_Damages>2	45	0.322	0.165	*0.41*
Expected_giving	N_Damages≤2	57	0.346	0.197	(-1.882)
	N_Damages>2	48	0.427	0.205	*0.06*
			Panel B.2		
Giving	Helpindex≤0.113	48	0.308	0.165	(-0.308)
	Helpindex>0.113	51	0.323	0.172	*0.758*
Expected_giving	Helpindex≤0.113	57	0.347	0.178	(-1.843)
	Helpindex>0.113	44	0.436	0.228	*0.065*
			Panel B.3 - N_Damages > 2		
Giving	Helpindex≤0.113	15	0.282	0.164	(-1.109)
	Helpindex>0.113	30	0.342	0.165	*0.268*
Expected_giving	Helpindex≤0.113	18	0.337	0.134	(-2.689)
	Helpindex>0.113	26	0.508	0.22	*0.007*
			Panel B.4 - N_Damages ≤ 2		

Giving	Helpindex≤0.113	33	0.319	0.167	(0.772)
	Helpindex>0.113	21	0.295	0.181	*0.44*
Expected_giving	Helpindex≤0.113	39	0.352	0.197	(0.488)
	Helpindex>0.113	18	0.333	0.202	*0.626*

Variable legend: see Table 1

As previously shown, recovery aid does not *directly* affect generosity when comparing damaged and non damaged individuals (Panel A.2 of Table 3). Results on the impact of the shock on giving and expected giving go in 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. To check for the *indirect* effects of aid on behavior (i.e., through the damages caused by the shock), we restrict our sample only to tsunami-affected individuals and test hypotheses v and vi non-parametrically (Panel B.3 of Table 3). Results show that generosity varies according to the amount of aid received among the damaged. All of these facts together suggest the existence of a positive role of well-targeted recovery aid (i.e., when it reaches the most needy persons) in counterbalancing the negative impact of the tsunami on generosity, especially in terms of expected giving.

5. ECONOMETRIC ANALYSIS

Econometric estimates enrich our non-parametric tests by verifying the impact of additional covariates on giving, expected giving and by checking for the robustness of our previous results to the inclusion of covariates. The specification we test is:

$$Y_i = \alpha_0 + \alpha_1 Damaged_i + \sum_k \beta_k X_{ki} + \sum_j \gamma_j G_{ji} + \varepsilon_{it}$$

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 (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)*, number of household members (*n_house_members*), a variable measuring borrower seniority (the number of loan cycles) plus three dummies for the respondent's working activity (*trading*, *fishery* and *manufacturing*). The *G* variables are our experimental measures of time and risk attitudes (see section 3 and online Appendix).

In an alternative specification, 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_mean)* and the interaction term between these two (*N_damages*help_ab_mean*). This allows us to consider the impact of the tsunami on generosity at the intensive margin as well the interaction between damages and recovery aid.

5.1 The impact of the tsunami experience

First, when considering giving as dependent variable (the *giving* variable) the estimates document that none of the controls is significant at 5 percent level (Table 4), except for the amount invested in the risky option (*riskloving_ratio*), which is positive and significant.[6] In general, *Single*, *Separated* and *Male* could turn to be insignificant possibly because of their low sample size (between 2 and 6 percent of cases). The damage dummy is negative and significant as senders affected by the tsunami

[6] For the relations between risk attitudes and social preferences, see, among others, Beck (1994) and Bohnet et al. (2008). Aside 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 the experiment according to which payments depend on one randomly selected game.

give approximately 6 percentage points less than those who are not affected (a magnitude equal to the effect measured in the non-parametric tests in section 4). Tobit estimates that take into account the left and right limits of our dependent variable confirm this first result.

Table 4 - Determinants of Giving

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Giving	OLS	OLS	OLS	TOBIT	TOBIT	TOBIT
Damaged	-0.0614**	-0.0579**	-0.0584**	-0.0611**	-0.0572**	-0.0570**
	(0.0274)	(0.0278)	(0.0290)	(0.0282)	(0.0284)	(0.0284)
Riskloving_ratio		0.108**	0.116**		0.112**	0.119**
		(0.0454)	(0.0478)		(0.0465)	(0.0471)
Switch		-0.00770*	-0.00806*		-0.00793*	-0.00833*
		(0.00448)	(0.00469)		(0.00459)	(0.00462)
Age			-0.00165			-0.00169
			(0.00123)			(0.00121)
Single			-0.0557			-0.0526
			(0.0684)			(0.0655)
Widowed			0.0226			0.0241
			(0.0344)			(0.0332)
Separated			0.110			0.109
			(0.0792)			(0.0756)
Male			0.0258			0.0225
			(0.0605)			(0.0603)
Food_exp_std			-0.000450			-0.000381
			(0.000995)			(0.000970)
Galle			-0.00880			-0.00820
			(0.0356)			(0.0353)
Hambantota			-0.0473			-0.0457
			(0.0354)			(0.0345)
Years_schooling			-0.00273			-0.00275
			(0.00647)			(0.00627)
N_house_members			-0.00582			-0.00645
			(0.00998)			(0.00984)
Trading			-0.0339			-0.0340
			(0.0273)			(0.0267)
Fishery			0.0313			0.0317
			(0.0478)			(0.0459)
Manufacturing			-0.000805			-0.00348
			(0.0305)			(0.0303)
Loancycle			-0.00200			-0.00214
			(0.00261)			(0.00257)

Observations	191	191	187	191	191	187
R-squared	0.027	0.072	0.131			

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variable: share of the endowment sent by senders in the dictator game. Omitted benchmarks: *married*, *matara*, *agriculture*. Variable legend for regressors, see Table 1.

Table 5 - Determinants Of Expected Giving

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Expected Giving	OLS	OLS	OLS	TOBIT	TOBIT	TOBIT
Damaged	-0.0494*	-0.0496*	-0.0753**	-0.0490*	-0.0490*	-0.0758***
	(0.0277)	(0.0272)	(0.0290)	(0.0289)	(0.0283)	(0.0289)
Riskloving_ratio		-0.0386	-0.0413		-0.0460	-0.0479
		(0.0600)	(0.0620)		(0.0647)	(0.0641)
Switch		-0.0135***	-0.0129***		-0.0140***	-0.0134***
		(0.00465)	(0.00460)		(0.00494)	(0.00469)
Age			-4.40e-05			-0.000258
			(0.00141)			(0.00142)
Sngle			-0.0101			-0.00697
			(0.0596)			(0.0575)
Widowed			0.0465			0.0489
			(0.0438)			(0.0425)
Separated			0.112*			0.118**
			(0.0574)			(0.0588)
Male			-0.0678			-0.0671
			(0.0584)			(0.0560)
Food_exp_std			-0.00365			-0.00369
			(0.00399)			(0.00399)
Galle			-0.0743**			-0.0763**
			(0.0340)			(0.0339)
Hambantota			-0.0296			-0.0338
			(0.0395)			(0.0402)
Years_schooling			-0.00909*			-0.00946*
			(0.00547)			(0.00543)
N_house_members			0.00592			0.00600
			(0.0117)			(0.0116)
Trading			0.0474			0.0505*
			(0.0297)			(0.0293)
Fshery			0.0330			0.0333
			(0.0623)			(0.0604)
Manufacturing			-0.0442			-0.0458*
			(0.0272)			(0.0274)
Loancycle			0.0107			0.0116
			(0.00779)			(0.00812)
Observations	191	191	186	191	191	186

R-squared	0.016	0.060	0.175

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variable: share of the sender's endowment expected by receivers in the dictator game. Omitted benchmarks: *married*, *matara*, *agriculture*. Variable legend, see Table 1.

Second, we repeat the econometric analysis for receiver's expectations about sender's giving (Table 5) and find the same pattern of results as those observed for senders. Time preferences matter in this analysis as the more impatient participants (i.e., those switching later to the higher but delayed payoff alternative in the lottery, variable *switch*) tend to expect less than the less impatient ones Regarding the impact of the tsunami, having received at least one damage reduces the receiver's expected giving by 5 percentage points in the baseline estimate and by approximately 8 percentage points when we include other covariates.

Finally, we test the impact of the tsunami shock for the entire sample of participants (Tables 6.1 and 6.2), controlling for the heterogeneity in the sender/receiver status with the *receiver* dummy. This specification allows us to test for the presence of a local social norm about giving that would exist if giving and expected giving were not statistically different. Since receivers expect significantly more than what senders actually give, no evidence of a local "sharing/solidarity norm" is found in our data. Controlling for the presence of such a norm is important as it may represent an additional hidden driver of respondents' choices in the game. Results are consistent with those previously presented, with the damaged sending/expecting 5 to 6 percentage points more than what non-damaged individuals do (Table 6.1).

Table 6.1 - Determinants of giving and expected giving jointly considered

Dep. Var.	(1)	(2)	(3)	(4)	(5)	(6)
Respondent's behavior	OLS	OLS	OLS	TOBIT	TOBIT	TOBIT
Receiver	0.0675***	0.0677***	0.0681***	0.0686***	0.0687***	0.0693***
	(0.0193)	(0.0191)	(0.0198)	(0.0200)	(0.0198)	(0.0201)
Damaged	-0.0554***	-0.0523***	-0.0610***	-0.0551***	-0.0517**	-0.0609***
	(0.0194)	(0.0196)	(0.0207)	(0.0202)	(0.0202)	(0.0209)

Riskloving_ratio		0.0368	0.0409		0.0354	0.0395
		(0.0380)	(0.0391)		(0.0400)	(0.0404)
Switch		-0.0106***	-0.0113***		-0.0109***	-0.0117***
		(0.00327)	(0.00330)		(0.00341)	(0.00339)
Age			-0.00130			-0.00142
			(0.000911)			(0.000927)
Single			-0.0264			-0.0228
			(0.0500)			(0.0492)
Widowed			0.0464			0.0490
			(0.0303)			(0.0300)
Separated			0.124***			0.125***
			(0.0465)			(0.0458)
Male			-0.00202			-0.00304
			(0.0404)			(0.0408)
Food_exp_std			-0.000775			-0.000739
			(0.000891)			(0.000890)
Galle			-0.0462*			-0.0470*
			(0.0245)			(0.0248)
Hambantota			-0.0429*			-0.0439*
			(0.0251)			(0.0256)
Years_schooling			-0.00642			-0.00659
			(0.00404)			(0.00404)
N_house_members			-0.000852			-0.000887
			(0.00780)			(0.00784)
Trading			0.0109			0.0127
			(0.0201)			(0.0203)
Fishery			0.0428			0.0433
			(0.0350)			(0.0343)
Manufacturing			-0.0203			-0.0229
			(0.0205)			(0.0209)
Loancycle			0.00375			0.00407
			(0.00392)			(0.00410)
Observations	382	382	373	382	382	373
R-squared	0.050	0.081	0.125			

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variable: share of the endowment sent for senders while share of the sender's endowment expected for receivers in the dictator game. Omitted benchmarks: *married*, *matara*, *agriculture*. Variable legend, see Table 1.

5.2 The impact of tsunami damages and recovery aid

To investigate the tsunami impact at the intensive margin and the role of recovery aid, we re-estimate the model in Table 6.1 by replacing the *damaged* dummy with *N_damages*, *help_ab_mean* and their interaction. The results, reported in Table 6.2, confirm previous findings. On the one hand, receiving significantly more help does not directly affect behavior in the dictator game (variable *help_ab_mean*). Each additional damage, on the other, has a detrimental effect on generosity (variable *N_damages*). This effect, however, is compensated by the interaction between aid and damages (variable *N_damages*help_ab_mean*).[7] When we further divide our sample into three groups to consider separately those suffering high damages, low damages and no damages, we find that the interaction effect is significant only for the first and not for the third group (Table 6.2B in the online Appendix A.5).

Table 6.2 - Tsunami and aid effects on altruistic preferences

	(1) Whole sample OLS	(2) Giving OLS	(3) Exp. Giving OLS	(4) Whole sample TOBIT	(5) Giving TOBIT	(6) Exp. Giving TOBIT
Receiver	0.0678***			0.0691***		
	(0.0203)			(0.0206)		
N_damages	-0.0258***	-0.0245**	-0.0240***	-0.0258***	-0.0242**	-0.0236***
	(0.00693)	(0.0119)	(0.00895)	(0.00685)	(0.0115)	(0.00854)
Help_ab_mean	-0.0217	-0.0150	-0.0202	-0.0209	-0.0127	-0.0200
	(0.0264)	(0.0388)	(0.0392)	(0.0261)	(0.0373)	(0.0382)
N_damages*help_ab_mean	0.0331***	0.0234	0.0355**	0.0333***	0.0229	0.0359**
	(0.0104)	(0.0150)	(0.0173)	(0.0104)	(0.0145)	(0.0169)
Riskloving_ratio	0.0600	0.133***	-0.0223	0.0589	0.136***	-0.0294
	(0.0402)	(0.0505)	(0.0639)	(0.0413)	(0.0494)	(0.0655)
Switch	-0.0134***	-0.00880*	-0.0165***	-0.0138***	-0.00905*	-0.0171***
	(0.00334)	(0.00470)	(0.00486)	(0.00342)	(0.00460)	(0.00492)
Age	-0.00168*	-0.00184	-0.00103	-0.00180*	-0.00186	-0.00127
	(0.000904)	(0.00123)	(0.00148)	(0.000921)	(0.00120)	(0.00149)
Single	-0.0374	-0.0666	-0.0289	-0.0340	-0.0633	-0.0270
	(0.0505)	(0.0676)	(0.0794)	(0.0495)	(0.0643)	(0.0758)
Widowed	0.0480	0.0165	0.0582	0.0504*	0.0182	0.0604
	(0.0292)	(0.0379)	(0.0408)	(0.0288)	(0.0362)	(0.0396)
Separated	0.0954**	0.0951	0.0906	0.0967**	0.0940	0.0963
	(0.0473)	(0.0761)	(0.0699)	(0.0466)	(0.0725)	(0.0698)
Male	0.000229	0.0293	-0.0713	-0.000771	0.0262	-0.0707
	(0.0418)	(0.0591)	(0.0633)	(0.0419)	(0.0582)	(0.0600)
Food_exp_std	-0.000769	-0.000716	-0.00447	-0.000732	-0.000644	-0.00462
	(0.000887)	(0.000979)	(0.00412)	(0.000882)	(0.000950)	(0.00408)
Galle	-0.0430*	-0.0236	-0.0503	-0.0438*	-0.0226	-0.0517

[7] As far as the magnitude is concerned, a unit increase in *N_damages* generates a reduction of about 2.5/3% in giving and expected giving. A unit increase of *N_damages* if *help_ab_mean* is equal to one raises the aggregate behavior (i.e. giving and expected giving jointly considered) by about 3.3% (columns 1 and 4, Table 6.2).

	(0.0243)	(0.0362)	(0.0344)	(0.0246)	(0.0357)	(0.0341)
Hambantota	-0.0442*	-0.0538	-0.0248	-0.0451*	-0.0521	-0.0288
	(0.0259)	(0.0367)	(0.0411)	(0.0263)	(0.0356)	(0.0415)
Years_schooling	-0.00636	-0.00224	-0.00818	-0.00651	-0.00226	-0.00850
	(0.00413)	(0.00666)	(0.00585)	(0.00412)	(0.00642)	(0.00575)
N_house_members	-0.00221	-0.00591	0.00588	-0.00226	-0.00657	0.00608
	(0.00757)	(0.0103)	(0.0116)	(0.00759)	(0.0101)	(0.0114)
Trading	0.0132	-0.0235	0.0397	0.0150	-0.0234	0.0429
	(0.0202)	(0.0289)	(0.0301)	(0.0203)	(0.0280)	(0.0295)
Fishery	0.0493	0.0396	0.0309	0.0493	0.0405	0.0293
	(0.0398)	(0.0599)	(0.0694)	(0.0389)	(0.0571)	(0.0674)
Manufacturing	-0.0130	0.00337	-0.0296	-0.0155	0.000607	-0.0309
	(0.0207)	(0.0314)	(0.0282)	(0.0212)	(0.0309)	(0.0281)
Loancycle	0.00426	0.000576	0.00748	0.00459	0.000388	0.00826
	(0.00400)	(0.00274)	(0.00770)	(0.00420)	(0.00266)	(0.00799)
Observations	363	182	181	363	182	181
R-squared	0.137	0.126	0.182			

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variable: share of the endowment sent for senders while share of the sender's endowment expected for receivers in the dictator game, Omitted benchmarks: *married, matara, agriculture*. Variable legend, see Table 1.

The regression results in Table 6.2 may not however appear fully consistent with those reported in the descriptive analysis in Table 3 (see section 4) where highly damaged participants seemed not to differ much in terms of generosity with respect to non-damaged and less-damaged ranked the lowest (see Panel A.1 and B.1, Table 3). An explanation to this apparent inconsistency may derive from the fact that the *N_damages* variable in Panel B.1 in Table 3 would capture not only the tsunami impact at an intensive margin but also the effect of high recovery aid received by the highly damaged participants. For this reason, in order to make an intensive-margin analysis that takes into account simultaneously i) individual characteristics, ii) their risk/time preferences, and iii) generosity of those not damaged and less damaged by the tsunami - while at the same time disentangling the recovery aid effect from that of the number of damages - one would need to consider a regression analysis as that implemented in Table 6.2.

5.3 Interpretation of the empirical results

To summarize the main econometric findings, our econometric results confirm in general our alternative hypotheses ($H_1^{i,ii}$ and $H_1^{v,vi}$) showing that i) having received at least one damage from the tsunami reduces giving and expected giving, ii) while there is no *direct* effect of recovery aid, a large number of damages *per se* reduce generosity, and iii) this last effect is compensated by the large amount of recovery assistance targeting highly damaged individuals.

Since this last effect occurs only for the highly damaged and not for the other two groups - those suffering fewer damages and those suffering no damage – the economic and psychological rationales jointly with the indirect-reciprocal type of preferences described in section 2 can explain our interaction result. In this last respect, our result is particularly strong as the indirect reciprocation occurs in a one-shot anonymous interaction, and therefore, it cannot be explained by reputational concerns as it occurs in empirical tests of indirect reciprocity with repeated interactions (e.g. Engelmann and Fischbacher, 2009; Bolton et al., 2005). Furthermore, the first action triggering indirect reciprocity is not produced experimentally, but rather, it is a 7-year distance event, and, as an event, it is certainly more important and memorable to affected players than those events produced in artificial experiments.

In a further robustness check where we look at giving/expected giving in relation to different type of damages we find that the effect is concentrated on the economic damage (Table 6.3). This finding is consistent with the rationales behind our alternative hypothesis ($H_1^{v,vi}$) indicating that the negative effect of giving should be higher for damaged individuals receiving relatively lower aid.

Table 6.3 - Tsunami and injury effects on altruistic preferences

VARIABLES	(1) Giving	(2) Expected Giving	(3) Giving and Expected Giving
receiver			0.0638***
			(0.0197)
Economic damages	-0.0625*	-0.0847**	-0.0741***
	(0.0324)	(0.0347)	(0.0235)
Injuries	-0.00132	0.00290	0.0114
	(0.0401)	(0.0493)	(0.0332)
Damages to house	0.0503	0.0573	0.0557*
	(0.0351)	(0.0471)	(0.0289)
riskloving_ratio	0.103**	-0.0458	0.0312
	(0.0493)	(0.0605)	(0.0389)
Switch	-0.00799*	-0.0126***	-0.0111***
	(0.00474)	(0.00447)	(0.00323)

Socio-demographic controls	YES	YES	YES
Observations	185	186	371
R-squared	0.128	0.180	0.130

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

6. TACKLING ENDOGENEITY

A possible bias affecting the causal interpretation of our findings may result from the non-random assignment of the *damaged* status as signaled by the balancing properties in Table A3.1 in the online Appendix. Although the impact of this source of bias is likely mitigated by the selection of only microfinance borrowers and the comparison of mostly balanced subsamples based on the amount of damage and aid, we account for the remaining potential endogeneity with a weighted least squares estimation (subsection 6.1), instrumental variable regressions (subsection 6.2-6.3) and a sensitivity analysis on the exogeneity assumption (subsection 6.4). We believe that, as suggested by the balancing properties in Panels B and C of Table A3.1 (in the online Appendix), the potential bias would only affect the results of the impact of the tsunami at the extensive margin (i.e., damaged vs. non-damaged) but not those of its impact at the intensive margin (i.e., within damaged according to damage intensity) or its impact on the aid-compensation effect. Using data from the Sri Lankan 2011-Census, we finally correct for the non-representativeness of our sample (subsection 6.5)

6.1 Weighted least squares

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 4, 5, 6.1 and 6.2 with weighted least squares by weighting each observation with the inverse of its estimated propensity score for receiving at least one damage from the tsunami[8]. All of the main results illustrated above are robust to this check and are reported, respectively, in Tables 4A, 5A, 6.1A and 6.2A in the online Appendix A.

6.2 Instrumental variable regressions

We enrich our identification strategy through an instrumental variable re-estimation of the specification mostly suspected of endogeneity (i.e., the damaged/non-damaged tsunami effect). 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 protection capacity of such distance heterogeneous. The instrument seems logically and statistically relevant as those living closer to the coast are more likely to be damaged by the tsunami (Table A3.1 in the online Appendix). It is also likely to be logically valid as 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[9].

---

[8] Specifically, for each individual, the weights are computed as $\frac{\text{damaged}}{\text{pscore}\widehat{(\text{damaged})}} + \frac{1-\text{damaged}}{1-\text{pscore}\widehat{(\text{damaged})}}$, 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 *BMI* and *distant* as excluded regressors (from the outcome equation) and *years_schooling, galle, hambantota, years_schooling, trading, fishery, manufacturing, loancycle* as regressors common to both the treatment and the outcome equations (see footnotes of Tables in the online Appendix A.4 and variable legend in Table 1). The latter are chosen as less sensitive to changes in time after the tsunami and so to guarantee the satisfaction of the balancing property. For details on this methodological approach, see, among others, Blattman and Annan (2010) and Hirano et al. (2003).

[9] Note also that the few observables in which the two groups of damaged/non damaged individuals differ do not affect generosity in previous econometric estimates (see Tables 4-6.1).

A second instrument we use is the individual's body mass index (BMI) defined as the individual's body mass divided by the square of her/his height. In this case, the instrument appears logically valid as it is difficult to consider an unobservable and statistically significant link between body characteristics and social preferences apart from victimization status. In addition, if we interpret the BMI as a proxy for health conditions or fitness, we may expect the BMI to be a valid instrument as more fit and healthy individuals (i.e., neither over- nor under-weight, nor in poor health) were reasonably more likely to escape damages (and presumably recover faster) than less fit/less healthy individuals. This is supported by our data as the mean BMI of tsunami-injured is greater than that of non-injured borrowers (t = -2.46125; p-value = 0.0071). The same is true when comparing BMI of those reporting injuries to family members vis-à-vis those who do not report such injuries (t = -1.5597; p-value = 0.0598). We as well test whether the BMI effect on injury is U-shaped and find that the hypothesis is not supported by our data since the overweight/obese vs. normal weight difference is significant while the underweight vs. normal weight difference is not. The interpretation is that higher BMI clearly reduces agility and possibility to escape while this is not always the case for the underweight share.

Table 6.4 - Determinants of giving and expected giving jointly considered (IV estimates)

Dep Var: *Respondent's behavior*	(1)	(2)	(3)	(4)
Receiver	0.0681***	0.0687***	0.0709***	0.0713***
	(0.0193)	(0.0193)	(0.0194)	(0.0194)
Damaged	-0.0939**	-0.0866*	-0.0953**	-0.0855*
	(0.0414)	(0.0472)	(0.0402)	(0.0459)
Riskloving_ratio		0.0413		0.0344
		(0.0381)		(0.0383)
Switch		-0.0112***		-0.0116***
		(0.00320)		(0.00322)
Age		-0.00119		-0.000983
		(0.000938)		(0.000928)
Single		-0.0219		-0.0217
		(0.0504)		(0.0507)
Widowed		0.0498*		0.0475
		(0.0296)		(0.0296)
Separated		0.134***		0.132***
		(0.0491)		(0.0494)
Male		0.00358		0.00269

		(0.0409)		(0.0409)
Food_exp_std		-0.000719		-0.000682
		(0.000858)		(0.000859)
Galle		-0.0437*		-0.0458*
		(0.0239)		(0.0245)
Hambantota		-0.0441*		-0.0397
		(0.0248)		(0.0248)
Years_schooling		-0.00691*		-0.00662*
		(0.00402)		(0.00399)
N_house_members		9.11e-05		1.48e-05
		(0.00791)		(0.00788)
Trading		0.0141		0.0127
		(0.0205)		(0.0205)
Fishery		0.0514		0.0506
		(0.0363)		(0.0394)
Manufacturing		-0.0203		-0.0211
		(0.0200)		(0.0200)
Loancycle		0.00422		0.00396
		(0.00392)		(0.00389)
Observations	382	373	379	370
R-squared	0.040	0.121	0.039	0.120
Instruments	*distant*	*distant*	*distant, BMI*	*distant, BMI*
Endogeneity C-test for damaged: p-value	0.309	0.560	0.242	0.545
Test of excluded instruments (Weak Id.Test): F-stat	108.763	79.891	58.319	41.48
Overid.test: c^2			0.077	0.181
Overid.test: p-value			0.782	0.670

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Dependent variable: share of the endowment sent for senders while share of the sender's endowment expected for receivers in the dictator game. Omitted benchmarks: *married*, *matara*, *agriculture*. Variable legend, see Table 1.

We re-estimate the OLS specifications of Table 6.1 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 6.4. In all of the specifications (with/without demographic controls) with the *distant* instrument, the effect of receiving at least one damage from the tsunami is significant and strong in magnitude (i.e., tsunami damaged send/expect approximately 9 percent to 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 cases, confirming the logical relevance of our instruments. Furthermore, the Sargan (1958) test suggests we cannot reject the null over-identifying restrictions (Table 6.4, columns 3 and 4), and the endogeneity C-test (GMM distance) cannot reject the null that the *damaged* dummy is exogenous. These statistics provide further evidence sopporting the validity of our identification strategy.

6.3 Discussion

The validity of the *distant* instrument hinges on the assumption that distance from the coast and generosity are correlated only through victimization. It is possible, however, that the individual's location choice is endogenously based on unobservable factors that influence both generosity and victimization. One such factor can be, for instance, the pre-tsunami risk attitudes towards natural events, as individuals with higher (lower) expectations of a shock and/or who are more (less) risk averse can decide to live farther from (nearer to) the coast. Because we do not have pre-tsunami data, however, we cannot control for ex-ante risk preferences. Thus, it must be noted that because the 2004 tsunami was completely unexpected, location decisions may be, at best, marginally driven by a pre-existing background risk of a tsunami.

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

Another source of endogeneity may arise from post-tsunami differential migration based on unobservable factors (i.e., individual's ability to be in social networks), which may be correlated with both generosity and tsunami exposition. We do not believe migration can affect our estimates, as it is a very limited occurrence (as documented by AMF). As previously discussed, there would have been also little incentive for borrowers to migrate after the tsunami because of the extremely favorable conditions on the micro-loan offered by AMF and the huge amount of local and international aid flows (Becchetti and Castriota, 2010 and 2011). Similar evidence is provided by Paul (2005) with respect to tsunami-victims in Bangladesh.

A potentially spurious explanation of our findings may also be related to a more general income effect. The differential between giving and expected giving based on damage and aid might be explained by wealth or income variation before and after the tsunami, a factor that we cannot observe. Although we do not have pre-tsunami levels of income, income is not likely to drive our results since it is proxied for by many controls in our estimates (e.g. current employment sector[10], current level of food expenditures, level of education and risk and time preferences).

All the above-mentioned remaining endogeneity issues are addressed below through a sensitivity analysis on the exogeneity assumption behind our results.

6.4 Sensitivity analysis

Admittedly, the unobserved pre-tsunami level of generosity – and/or other unobserved characteristics - may determine spurious results if correlated with our "treatment" (T, tsunami-damages) and the "outcome" (Y, ex-post levels of generosity) variables. If not controlled for, such unobservable characteristics may limit the internal validity of our results. In order to check to what extent unobservables of these types (U) may reduce the significance or the magnitude of our estimated average treatment effect (ATE) of the tsunami-damage on generosity, we implement the sensitivity analysis on departures from the exogeneity assumption as proposed by Imbens (2003), operationalized by Harada (2012) and applied to a relevant case by, among others, Blattman and Annan (2010). The methodology and the results are described in details in the online Appendix A.7. The results of the sensitivity analysis highlight that the baseline ATE of the tsunami-damage on

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

generosity estimated under the assumption of exogeneity is robust when this assumption is relaxed and the impact of unobserved characteristics (e.g., ex-ante levels of generosity) is simulated as being potentially correlated both with the damage status and ex-post generosity.

6.5 Correction for the non-representativeness of our sample

Since our sample is composed by microfinance borrowers, our results may suffer from limited external validity with reference to the entire Sri Lankan population. To account for this problem, we weight our main estimates with the inverse of the probability of being selected in the sample. Such probabilities are constructed as post-stratification weights by comparing the sample proportion of individuals in the age, gender, marital status, village and education categories to the population proportion. The population margins are obtained from the Sri Lanka Census in 2011 (the year of our fieldwork) and post-stratification weights are calculated with the raking method (Deming, 1943 and Kalton, 1983) that adjusts the sampling weights so that the marginal totals of the adjusted weights on the specified characteristics (gender, age, marital status, education, village) are consistent with the corresponding totals for the Sri Lankan population.

The algorithm involves repeatedly estimating weights across each set of variables until the weights converge and stop changing. Essentially, raking forces the survey totals to match the known population totals by assigning a weight to each respondent.[11] Estimation results reported in the online Appendix A (Tables A6a-A6c) are consistent with the main findings, in particular with those in our main regressions (Tables 4-6.2). This evidence shows that our results - behind being relevant *per se* as related to an important target of the population in terms of economic development and poverty reduction - seems to be externally valid also for the rest of the population.

7. CONCLUSIONS

[11] The procedure is discussed in details in the online Appendix A.6.

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

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 a dictator game. Moreover, at the intensive margin, we compare the participants based on the amount of damage experienced and recovery aid received. The advantage of this last comparison is that differences in observables between the groups are by far lower. 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, likely to share some important common traits (e.g. entrepreneurial and social skills). These characteristics are usually unobservable to researchers while suspected to be among the main determinants of self-selection bias. Given the lack of pre-tsunami data, we complete our identification strategy with inverse p-score weighted least squares, instrumental variable and a sensitivity analysis on the exogeneity assumption. Our main findings are robust to all these checks.

Our analysis highlights two original results. One, both at the intensive and extensive margins, individuals damaged by the tsunami give and expect less than non-damaged even seven years after the event. Two, though recovery aid does not generally affect generosity *per se* in the long-run, it does so in a positive way for individuals more intensely affected by the tsunami.

Despite of the above-mentioned robustness checks for endogeneity, residual risks of selection bias cannot be fully excluded if the more damaged individuals who are ex-ante not as cooperative and generous as the current borrowers are also more likely to join microfinance ex-post, thereby producing a spurious negative correlation between the extent of damage and generosity. This problem is, however, unlikely to arise since entry of highly damaged new members was excluded by AMF (and donor's rules) that focused on recovering from distress by supporting old members. Beyond this official policy, there were clearly no economic incentives for both AMF and group borrowers to accept after the tsunami new highly damaged members, especially if they would not share the same pro-social attitudes of the rest of the "club". The risk of adverse selection and strategic default of the former and the assortative matching approach of the latter (e.g. Gathak, 2000) should therefore converge to mitigate selection bias. These arguments would also apply to the hypothetical case in which AMF lowered the altruism standards to lend to low-damage individuals. Hiring borrowers with lower moral values for the organization (and for group borrowers) is problematic in the long run since uncooperative groups often fail to repay due to joint liability and are denied future loans.

As to the external validity of our results, looking at national Census data for the three considered districts of Galle, Matara and Hambantota[12] we find that our database of microfinance borrowers is undersampled in terms of gender composition (only 6.5 percent males against a three-province weighted average figure of 48.2 percent) and unemployment status (0 percent by definition in our sample against 8.2 percent in the whole country). In addition, being a dataset of microfinance self-employed workers, the sample average age of 46 years is higher than the country average of 31 , which includes children and young people who are not economically active. The remaining socio-demographic characteristics are in line with those of the local population (e.g. Sinhalese ethnic and Buddhist religion share close to 90-95 percent in the three provinces, 85 percent share of rural population and 90 percent literacy rate). Our results, however, are robust to a post-stratification weighted least squares estimate based on Census data aimed at addressing non-representativeness of the sample.

[12] The 2011-Census data is available at http://www.statistics.gov.lk

Beyond potential limits to the generalization of our findings to the overall population, our study on a specific population group (women microfinance borrowers) is nonetheless of great relevance. In South Asia microfinance is largely addressed to women and in Sri Lanka, as in many other countries, it is recognized as a fundamental tool to address poverty and social inclusion and as a gateway to access more traditional financing sources for small producers.

In addition, the behavioral literature tells us that giving is an important (non strategic) source of trust and trustworthiness in social dilemmas such as trust investment games (e.g. Johnson and Mislin, 2011 and Cox, 2004) and, as such, one of the most important sources of the superadditive benefits stemming from cooperation in the widespread "grey areas" of socio-economic interactions that are not covered by contract protection. In this sense the idea that tsunami may create a "wound" to giving is of great importance as solving social dilemmas is at the basis of cooperation and, therefore, a microeconomic source of productivity and growth.

References

[1] Arouri, M., Nguyen, C. & Youssef, A.B. (2015). Natural disasters, household welfare, and resilience: Evidence from rural Vietnam. *World Development*, *70*, 59-77.

[2] Axelrod, R. (1984). *The Evolution of Cooperation*. New York: Basic Books.

[3] Becchetti, L., & Castriota, S. (2010). The effects of a calamity on income and wellbeing of poor microfinance borrowers: The case of the 2004 tsunami shock". *The Journal of Development Studies*, *46* (2), 211-233.

[4] Becchetti, L., & Castriota, S. (2011). Does microfinance work as a recovery tool after disasters? Evidence from the 2004 tsunami". *World Development*, *39* (6), 898-912.

[5] Beck, J.H. (1994). An experimental test of preferences for the distribution of income and individual risk aversion”. *Eastern Economic Journal*, *20* (2), 131-145.

[6] Berlemann, M., Steinhardt, M., & Tutt, J. (2015). Do natural disasters stimulate individual Ssaving? Evidence from a natural experiment in a highly developed country. IZA DP No. 9026.

[7] Blattman, C., & Annan, A. J. (2010). The consequences of child soldiering. *Review of Economics and Statistics*, *42* (4), 882-898.

[8] Bohnet, I., Greig, F., Herrmann, B., & Zeckhauser, R. (2008). Betrayal aversion: evidence from Brazil, China, Oman, Switzerland, Turkey, and the United States. *American Economic Review*, *98* (1), 294-310.

[9] Bolton, G.E., Katok, E., & Ockenfels, A. (2005). Cooperation Among Strangers with Limited Information about Reputation. *Journal of Public Economics*, *89*, 1457-1468.

[10] Buchanan, J. (1975). The Samaritan’s dilemma. In Phelps, E.S. (Ed.), *Altruism, morality, and economic theory*. New York: Russell Sage Found, 71-85.

[11] Callen, M. (2015). Catastrophes and time preference: Evidence from the Indian Ocean earthquake. *Journal of Economic Behavior & Organization*, *118*, 199-214.

[12] Cameron, L., & Shah, M. (2015). Risk-taking behavior in the wake of natural disasters. *Journal of Human Resources*, *50* (2), 484-515.

[13] Carter, M.L., Little, P.D., Mogues, T. & Negatu, W. (2007). Poverty Traps and Natural Disasters in Ethiopia and Honduras. *World Development*, *35* (5), 835-856.

[14] Cassar, A., Healy, A., & Von Kessler, C. (2011). Trust, risk, and time preferences after natural disasters: Experimental evidence from Thailand. Working Paper, University of San Francisco.

[15] Cassar, A., Grosjean, P., & Whitt, S. (2013). Legacies of violence: Trust and market

development. *Journal of Economic Growth*, *18* (3), 285-318.

[16] Castillo, M., and Carter, M. (2011). Behavioral responses to natural disasters. Working Paper No. 1026, George Mason University.

[17] Charness, G., & Grosskopf, B. (2001). Relative payoffs and happiness: an experimental study. *Journal of Economic Behavior & Organization*, *45*, 301-328.

[18] Coate, S. (1995). Altruism, the Samaritan's dilemma and government transfer policy. *American Economic Review*, *85* (1), 46-57.

[19] Cox, J.C. (2004). How to identify trust and reciprocity. *Games and Economic Behavior*, *46* (2), 260-281.

[20] Curry, O., Price, M.E., & Price, J.G. (2008). Patience is a virtue: Cooperative people have lower discount rates. *Personality and Individual Differences*, *44*, 780-785.

[21] Deming, W.E. (1943). *Statistical adjustment of data*. New York: Wiley.

[22] Dobes, L., Jotzo, F., & Stern, D.I. (2014). The economics of global climate change: A historical literature review", *Review of Economics*, *65*, 281-320.

[23] Dufwenberg, M., Gneezy, U., Güth, W., & Van Damme, E. (2001). Direct versus indirect reciprocity: An experiment. *Homo Oeconomicus*, *18*, 19-30.

[24] Eckel, C.C., El-Gamal, M.A., & Wilson, R.K. (2009). Risk loving after the storm: a Bayesian-network study of hurricane Katrina evacuees. *Journal of Economic Behavior & Organization*, *69* (2), 110-124.

[25] Eckel, C.C. & Grossman, P.J. (1995). Altruism in anonymous dictator games. *Games and Economic Behavior*, *16*, 181-191.

[26] Engel, C. (2011). Dictator games: a meta study. *Experimental Economics*, *14* (4), 583-610.

[27] Engelmann, D., & Fischbacher, U. (2009). Indirect reciprocity and strategic reputation

building in an experimental helping game. Games and Economic Behavior, *67* (2), 399-407.

[28] Fearon J. D., Humphreys, M., & Weinstein, J.M. (2009). Can development aid contribute to social cohesion after civil war? Evidence from a field experiment in post-conflict Liberia. *American Economic Review*, *99* (2), 287-291.

[29] Fleming, D., Chong, A., Alberto, E., & Bejarano, H.D. (2011). Do natural disasters affect trust/trustworthiness? Evidence from the 2010 Chilean earthquake. 2011 Annual Meeting, July 24-26, 2011, Pittsburgh, Pennsylvania 104522, Agricultural and Applied Economics Association.

[30] Frankenberg, E., T. Gillespie, S. Preston, B. Sikoki, and D. Thomas (2011). "Mortality, the Family and the Indian Ocean Tsunami". The Economic Journal, *121* (554), 162-182.

[31] Freeman, P., Keen, M., & Mani, M. (2003). Dealing with increased risk of natural disasters: Challenges and options. IMF Working Paper WP/03/197.

[32] Ghatak, M. (2000). Screening by the company you keep: Joint liability lending and the peer selection. *Economic Journal*, 110 (465), 601-631.

[33] Gitter, S.H. & Barham, B.L. (2007). Credit, natural disasters, coffee, and educational attainment in rural Honduras. *World Development*, *35* (3), 498-511.

[34] Guth, W., Konigstein, M., Marchand, N., & Nehring, K. (2001). Trust and reciprocity in the investment game with indirect reward. *Homo Oeconomicus*, *18*, 241-262.

[35] Harada, M. (2012). *Generalized sensitivity analysis*. Mimeo, University of Chicago.

[36] Hirano, K., Imbens, G.W., & Ridder, G. (2003). Efficient estimation of average treatment effects using the estimated propensity score. *Econometrica*, *71* (4), 1161-1189.

[37] Imbens, G.W. (2003). Sensitivity to exogeneity assumptions in program evaluation. *American Economic Review*, *93* (2), 126-132.

[38] Johnson, N.D. & Mislin, A.A. (2011). Trust games: a meta-analysis. *Journal of Economic Psychology, 32* (2), 865-889.

[39] Kalton G. (1983). *Compensating for missing survey data*. Survey Research Center, Institute for Social Research, University of Michigan.

[40] Kanagaretnam, K., Mestelman, S., Nainar, K., & Shehata, M. (2009). The impact of social value orientation and risk attitudes on trust and reciprocity. *Journal of Economic Psychology*, *30*, 368-380.

[41] Korf, B., Habullah, S., Hollenbach, P., & Klem, B. (2010). The gift of disaster: the commodification of good intentions in post-tsunami Sri Lanka. *Disasters*, *34* (1), 60-77.

[42] Leimar, O., & Hammerstein, P. (2001). Evolution of cooperation through indirect reciprocity. *Proceedings of the Royal Society Biological Sciences*, *268*, 745-753.

[43] Li, Y., Li, H., Decety, J., & Lee, K. (2013). Experiencing a natural disaster alters children's altruistic giving. *Psychological Science*, *24* (9), 1686-95.

[44] Linnerooth-Bayer, J., Quijano-Evans, S., Löfstedt, R., & Elahi, S. (2001). The uninsured elements of natural catastrophic losses. Tsunami Project Summary Report.

[45] Liu, P.L.F., Lynett, P., Fernando, H., Jaffe, B.E., Fritz, H., Higman, B., Morton, R., Goff, J., Synolakis, K. (2005). Observations by the international tsunami survey team in Sri Lanka. *Science, 308* (5728), 1595.

[46] Luechinger, S., & Raschky, P.A. (2009). Valuing flood disasters using the life satisfaction approach. *Journal of Public Economics*, *93* (3-4), 620-633.

[47] Morris, S.S. & Wodon, Q. (2003). The allocation of natural disaster relief funds: Hurricane Mitch in Honduras. *World Development*, *31* (7), 1279-1289.

[48] Nowak, M.A., & Sigmund, K. (2005). Evolution of indirect reciprocity. *Nature*, *437* (27), 1291-1298.

[49] Paul, B.K. (2005). Evidence against disaster-induced migration: the 2004 tornado in north-central Bangladesh. *Disasters*, *29* (4), 370-385.

[50] Rao, L-L., Han, R., Ren, X.-P., Bai, X.-W., Zheng, R., Liu, H., Wang, Z.-J., Li, J.-Z., Zhang, K., & Li, S. (2011). Disadvantage and prosocial behavior: the effects of the Wenchuan earthquake. *Evolution and Human Behavior*, *32* (1), 63-69.

[51] Raschky, P.A., & Weck-Hannemann, H. (2007). Charity hazard - A real hazard to natural disaster insurance? *Environmental Hazards*, *7* (4), 321-329.

[52] Roodman, D. (2012). Due diligence: an impertinent inquiry into microfinance. CGDEV Working Paper.

[53] Roson, R., Calzadilla, A., & Francesco, P. (2007). Climate change and extreme events: an assessment of economic implications. *International Journal of Ecological Economics and Statistics*, *7* (1), 5-28.

[54] Sargan, J.D. (1958). The estimation of economic relationships using instrumental variables. *Econometrica*, *26* (3), 393-415.

[55] Solnit, R. (2009). *A Paradise Built in Hell: The Extraordinary Communities that Arise in Disaster*. London: Penguin Books.

[56] Stanca, L. (2009). Measuring indirect reciprocity: Whose back do we scratch? *Journal of Economic Psychology*, *30* (2), 190-202.

[57] Tanaka N. (2011). Effectiveness and Limitations of Vegetation Bioshield in Coast for Tsunami Disaster Mitigation. *InTechOpen*, Published on: 2011-01-29.

[58] Västfjall, D., Peters, E., & Slovic, P. (2008). Affect, risk perception and future optimism after the tsunami disaster. *Judgment and Decision Making*, *3*, 64-72.

[59] Whitt, S., & Wilson, R.K. (2007). Public goods in the field: Katrina evacuees in Houston. *Southern Economic Journal*, *74* (2), 377-387.

[60] Willinger M., Bchir, M. A., & Heitz, C. (2013). Risk and time preferences under the threat of background risk: A case-study of Lahars risk in central Java. Working Paper 13-08, LAMETA, University of Montpellier.