Joint Discussion Paper
Series in Economics

by the Universities of

Aachen · Gießen · Göttingen
Kassel · Marburg · Siegen

ISSN 1867-3678

No. 19-2023

Ivo Steimanis, Max Burger, Bernd Hayo, Andreas Landmann, and Björn Vollan

A Storm Between Two Waves: Recovery Processes, Social Dynamics, and Heterogeneous Effects of Typhoon Haiyan on Social Preferences

Coordination: Bernd Hayo • Philipps-University Marburg
School of Business and Economics • Universitätsstraße 24, D-35032 Marburg
Tel: +49-6421-2823091, Fax: +49-6421-2823088, e-mail: hayo@wiwi.uni-marburg.de

A Storm Between Two Waves: Recovery Processes, Social Dynamics, and Heterogeneous Effects of Typhoon Haiyan on Social Preferences

Authors: Ivo Steimanis[1], Max Burger[1], Bernd Hayo[2], Andreas Landmann[3], and Björn Vollan[1,*]

Affiliations: [1] Sustainable Use of Natural Resources, Philipps University Marburg, Am Plan 1, 35037 Marburg, Germany.
[2] Marburg Centre for Institutional Economics, Philipps University Marburg, Universitätsstr. 24, 35037 Marburg, Germany.
[3] Institute of Economics, Friedrich-Alexander-Universität Erlangen-Nürnberg, Germany

***Corresponding author**: bjoern.vollan@wiwi.uni-marburg.de, Tel: +49-6421 2823725

ORCID: Steimanis: 0000-0002-8550-4675; Burger: 0000-0003-2334-3885; Hayo: 0000-0001-9037-4513; Landmann: 0000-0002-9835-6413; Vollan: 0000-0002-5592-4185

Keywords: Social preferences, environmental hazard, natural experiment, social dynamics, Typhoon Haiyan, Philippines

JEL codes: Q54, C93, D91, O12

Abstract: The literature regarding the effects of environmental hazards on social preferences is mixed and partially contradictory. The lack of a baseline in these studies is a severe methodological constraint, as it is hard to identify heterogeneous treatment effects through experience in the recovery process. We exploit a panel of incentivized behavioral measures of solidarity conducted before and after the devastating damages caused by Typhoon Haiyan in the Philippines. We find that Haiyan's impact on individuals' degree of solidarity was non-linear: solidarity was negatively affected in villages with medium damages, whereas no significant impact was observed in those villages that were most and least affected. A potential explanation for this non-linear effect is differences in people's experiences concerning the aid process and help from other villagers. In villages with medium damages, the quality of the aid process and help from other villagers was perceived to be significantly worse than that received by more and less affected villages. Lastly, survey evidence shows that the non-linear effects persist almost 10 years after the disaster.

Acknowledgements

We are grateful for excellent support in the field by Karla Henning, Lukas Kampenhuber, Tatjana Till, and Trang Pham Thi, and the team of local assistants. Moreover, we thank seminar/conference participants at the ESA World Meeting in Berlin, WOW6 in Indiana, Experimental Workshop for the Environment in Heidelberg, Analytical Sociology: Theory and Empirical Applications in Venice, Beijing Normal University, Karlsruhe Institute of Technology, Kochi University, Luxembourg University, University of Nottingham, University of Norwich, University of Passau, TU Munich, and Zhuhai University for helpful comments.

We are thankful for financial support by the Centre for Tropical Marine Ecology (ZMT), the Robert Bosch Foundation (Grant number 32.5.F082.0001.0), the Prof. Dr. Anna und Prof. Dr. Jörg Jiri Bojanovsky Foundation, TAMNEAC, and the Research Center (SFB) 884 "Political Economy of Reforms" funded by the German Research Foundation (DFG).

Ethical approval has been granted by the Ethics Commission of the School of Business, Economics and Society of the Friedrich-Alexander University Erlangen-Nürnberg.

1. Introduction

In November 2013, the Philippines were struck by typhoon Haiyan, locally known as Yolanda, one of the most devastating super typhoons on record (Mas et al. 2015). According to official reports, about 16 million Filipinos were affected, including around 6,300 deaths, 29,000 injured people, and over one million destroyed houses (NDRRMC 2013). The country's geography, which comprises thousands of islands, made it challenging for the central government to provide immediate assistance and facilitate reconstruction. In our study area, Panay Island, small communities in the municipality Concepcion were cut off from the mainland when their boats were destroyed. For several weeks, people had to rely on food and water deliveries by helicopter. Unfortunately, people in areas vulnerable to environmental hazards often lack access to formal institutions, such as insurance companies or banks, which could facilitate reconstruction efforts (Stéphane Hallegatte et al. 2020).[1] Casual observations suggest that mutual aid among community members was crucial in recovering from Haiyan, particularly in the first days and weeks when people needed to rely on each other to share food, provide shelter, and start rebuilding their communities. This behavior aligns with evidence on the importance of risk-sharing mechanisms, especially in low-income countries (Townsend 1994; Udry 1994), such as the Philippines (Fafchamps and Gubert 2007a; 2007b; Fafchamps and Lund 2003).

In this paper, we study the effect of Typhoon Haiyan on individuals' willingness to provide financial assistance to their community members three years after the event and whether the experiences encountered during the reconstruction period influenced these solidarity transfers. We measure solidarity using incentivized economic games and operationalize the strength of the environmental shock by the distance of a village from the eye of the storm. We utilize a survey conducted among the villagers participating in our games to learn about their experiences in connection with Haiyan. Using a panel dataset with measurements before and after Haiyan, we then examine whether the shock's strength and self-reported experiences in the aftermath of the catastrophe affect solidarity transfers.

Research on disasters unfolding from environmental hazards and extreme weather events exacerbated by climate change[2] has shown that prosociality can be strengthened temporarily (Drury, Novelli, and Stott 2013; Helsloot and Ruitenberg 2004; Quarantelli and Dynes 1977; Steimanis and Vollan 2022),

[1] While some initiatives have set up affordable insurance products to cover natural disasters accessible to low-income communities, they do not cover all losses or remain too expensive, resulting in a low take-up. Indeed, in the Philippines, only 0.47% of households were insured against property damages in 2016 (Gonulal 2019). With a projected increase in environmental hazards like tropical storms (Seneviratne et al. 2021), offering affordable insurance products becomes even more challenging in the future.

[2] Whether an environmental hazard leads to a disaster largely depends on the pre-existing vulnerabilities of the population hit by the event (Kelman 2020; Lahsen and Ribot 2022). Thus, it is not the extreme event causing the disaster but rather the social choices that cause the exposed population to be vulnerable. For the rest of the paper, we call such extreme events "environmental hazards." We use the word "disaster" when referring to the combined outcome of exposure and vulnerability to the environmental hazard and completely avoid the problematic term "natural disaster," which, despite its conceptual problems, is often used in the literature.

which is potentially due to the emergence of a shared social identity after experiencing a disaster (Drury 2018; Ntontis et al. 2018; 2021). However, there is limited knowledge about the longer-term effects of such events on mutual help beyond the immediate recovery period, particularly in the case of large-scale disasters such as Typhoon Haiyan. Our review of studies applying economic games suggests that the general relationship between environmental hazards and solidarity is unclear. The conflicting evidence presented in the literature may arise due to differences in study contexts and analyses, including the type of disaster, post-disaster conditions, time elapsed between the disaster and data collection, cultural characteristics, and econometric strategies.

The longer-term impact of environmental hazards on solidarity can be explored through the conceptual lens of social capital. By understanding how social capital is created and maintained, one can better understand how communities can leverage their social networks to cope with and recover from disasters and build long-term resilience. Important debates center around whether social capital can be seen as "capital" in which people invest with the expectation of future returns (Adler and Kwon 2002) and whether it depletes without proper 'maintenance' through the absence of regular contact when relationships become broken, trust is violated, or when individuals or groups become isolated from their social networks. Contrarily, social capital may increase with the development of solid relationships, trust, and reciprocity within a network. In the aftermath of a large-scale disaster, social capital can be highly valuable for addressing immediate needs and facilitating long-term recovery, but it may also grow or decline depending on the experiences made in this process. To cope with a catastrophe, people likely rely on leveraging resources and relationships through their informal and communal support networks. The related experience should, in turn, affect the creation and maintenance of social capital, which might materialize as prosocial behavior in the future. This discussion shows that (i) heterogeneous responses to a disaster are possible depending on specific experiences and that (ii) differences in experiences might be related to initial levels of prosociality.

There are numerous obstacles when establishing the causal impact of environmental hazards on *incentivized* prosociality. A particularly noteworthy challenge arises from our inability to forecast catastrophic environmental hazards. The unpredictability or limited forecastability of such extreme environmental hazards hampers the adequate preparation of those potentially affected. At the same time, the random occurrence of such climate extremes makes it unfeasible to pre-plan a data collection before the event occurs. As a result, none of the reviewed studies applying economic games can use data before and after the hazard as well as exogeneous variation in the affectedness by the hazard.[3]

[3] The study by Vardy & Atkinson (2019) conducted in Vanuatu is the only one with baseline data, but they rely on the variation of self-reported measures of affectedness. Their panel analysis of 164 subjects relies on within-subject comparisons of pre- and post-cyclone dictator game giving.

Our analysis contributes to closing this gap in the literature by studying solidarity as measured by incentivized economic games before and after Haiyan, which allows us to construct a panel dataset at the individual level. We conducted our study in two waves, one year prior (summer of 2012) and three years after Typhoon Haiyan (summer of 2016). The first wave was designed to measure the effect of insurance on solidarity, and it provides us with baseline data containing a relevant measure of solidarity; it also contains a sample of villages relatively more and less affected by Haiyan. Hence it offers the rare opportunity to compare an individual's solidarity before and after an unexpected large-scale disaster, conditional on different levels of exposure. We observe that more severely affected communities also suffered more financially, making a comparison with non-affected communities difficult. However, three years after the disaster, income differences caused by the disaster no longer existed.[4] Lastly, neither the intensity nor the occurrence of Typhoon Haiyan followed a foreseeable pattern, as our study area (Panay) lies outside the typical typhoon belt. We find no systematic differences in solidarity and exposure to Haiyan one year before the disaster. Thus, our explanatory variable, distance from the eye of the storm, should be exogenous with respect to the level of solidarity.

The first wave of our sample in 2012 consisted of 810 randomly invited individuals who participated in the economic games. We strove to obtain the same individuals from 2012 for another round of economic games in 2016, but the initial sample suffered considerable attrition. Our balanced panel consists of 450 Filipino coastal villagers. However, a comparison of individual characteristics suggests that the loss of participants does not result in a biased sample in terms of solidarity transfers and expectations. In addition, we collected empirical evidence regarding the long-term impacts of Haiyan on prosociality in a 2022 follow-up survey. For the follow-up study, we were able to track and interview 330 out of the 450 people that participated in both prior waves. Using appropriate control variables, we can limit the influence of characteristics that differ between those who participated in both waves and those who only participated in the first wave.

Our study contributes to the extant literature in three important ways. First, we find that the relationship between hazard exposure and solidarity is non-linear (Andrabi and Das 2017; Bai and Li 2021; Castillo and Carter 2011). Most studies cannot assess this, given a binary exposure measure. We explore non-linear effects in our dataset by using both (reversed) distance and quadratic (reversed) distance of a village to the eye of the storm as indicators of hazard exposure. As the distance from the eye of the storm strongly correlates with damage (see Figure 3), we can interpret the estimated impact of the (reversed) distance indicators as a proxy for the share of affected people in a specific location. Based

[4] This was primarily due to the many national and international donors that provided emergency help. As a consequence of these efforts, sometimes people were not only compensated for their losses but actually became better off. For example, some fishermen in the municipality of Concepcion were able to purchase better fishing boats after the disaster (O'Neill et al. 2019).

on these considerations, we provide evidence of the typhoon's non-linear negative effect on our incentivized measure of solidarity. Disaster exposure appears to reduce the extent of people's solidarity in a non-linear way, where negative effects are strongest for people in communities that experienced medium damage. Note that systematic changes in solidarity beliefs do not drive these effects, as these were unaffected by Haiyan (see Supplementary Section 2.6).

Second, having a baseline for all our measures, including solidarity, allows us to explore people's concrete experiences in the post-disaster recovery period helping us understand what drives the non-linear effect. Other studies suffer from the fundamental econometric problem that, even if an environmental hazard strikes exogenously, *ex-post* recovery and social dynamics might be endogenous to baseline prosociality. Thus, it is difficult to credibly attribute different effects to different experiences, even though this is exactly what we would expect from a social capital perspective (compare our arguments on page 3). In our analysis, we can cancel out such individual fixed effects related to individual prosociality differences by taking first differences of the variables. An analysis of heterogeneous effects indicates that the average effects do not depend on pre-Haiyan differences in terms of gender, socioeconomic status, education, or age. Survey evidence on post-Haiyan differences in terms of whether participants needed external aid, their retro perspective regarding their experiences with external aid, and interactions with other villagers reveals that the inverted U-shape is mainly driven by people who needed aid after Haiyan. In addition, in medium-affected villages, where the effects were mostly negative, the quality of external aid and help within villages was also U-shaped and thus lower compared to more and less affected villages.

Third, our study offers a long-term perspective missing from the literature. We find that the non-linear effects persist in panel data even nine years after Haiyan. To the best of our knowledge, the longest-term effect is reported by Kuroishi & Sawada (2019), who find no effect on altruism five years after a flood.

The remainder of the paper is organized as follows. Chapter 2 reviews the literature on the stability of social preferences. The impact of Typhoon Haiyan and our research methodology and dataset are discussed in Chapter 3. Chapter 4 presents our empirical findings, and Chapter 5 concludes.

2. Literature Review on the Malleability of Social Preferences

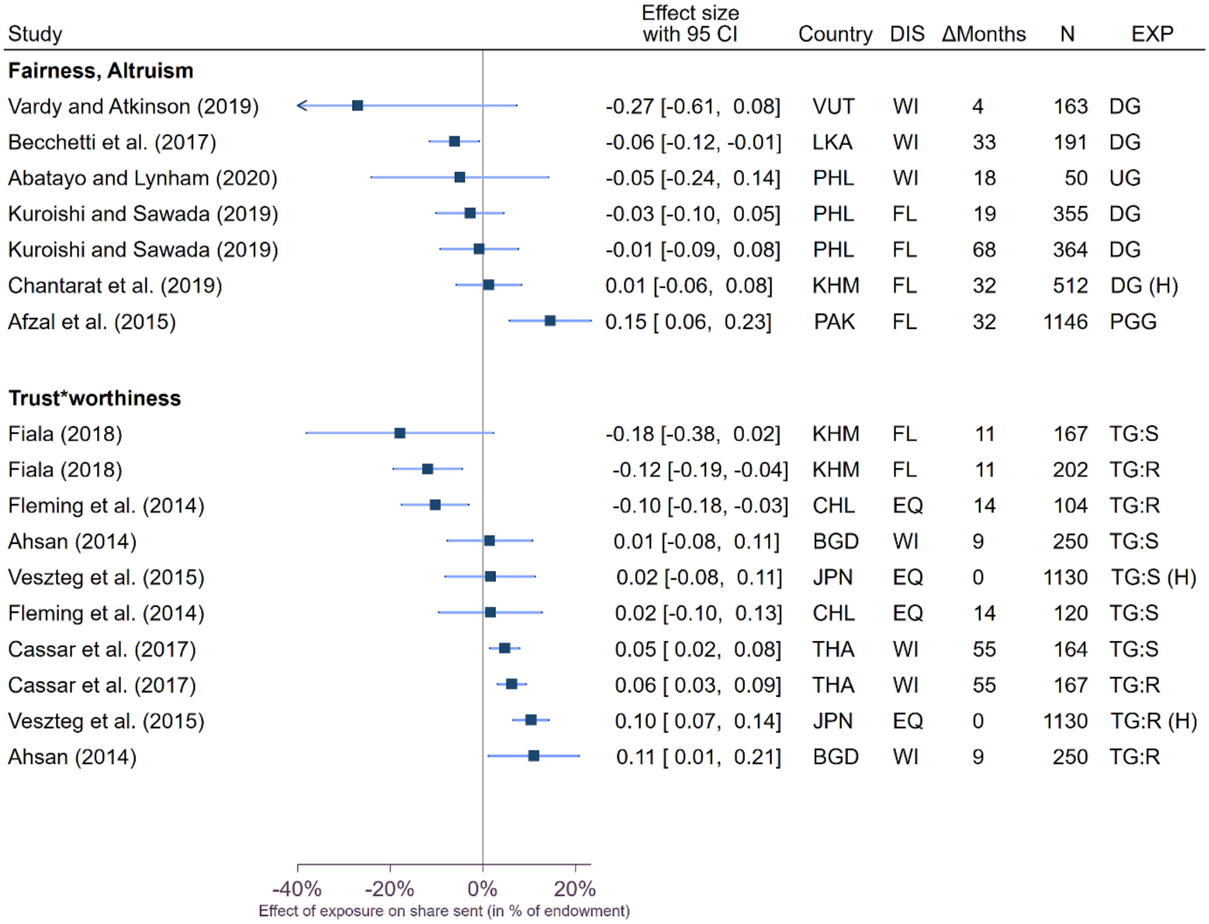
Standard economic theory generally assumes fundamental economic preferences, such as risk, time, or social preferences, to be stable over time (Stigler and Becker 1977). However, major events in people's lives, such as environmental hazards, can affect preferences. On the one hand, there is evidence that major events in the form of large macroeconomic shocks (Malmendier and Nagel 2011; 2016) and violent conflicts (Bauer et al. 2016) can alter fundamental economic preferences. On the other hand, Chuang and Schechter (2015) note that research on the impact of environmental hazards on risk preferences finds “amazingly divergent results” (p.154). The question of how adverse life events affect people’s prosociality is of interest beyond the construction of economic theory, as it affects our social relations and resilience in response to negative shocks.

Here we review studies using established economic games to elicit prosociality, such as the dictator game (Becchetti, Castriota, and Conzo 2017; Chantarat et al. 2019; Kuroishi and Sawada 2019; Vardy and Atkinson 2019), the trust game (Ahsan 2014; Cassar, Healy, and von Kessler 2017; Fiala 2017; Fleming, Chong, and Bejarano 2014; Veszteg, Funaki, and Tanaka 2015), the ultimatum game (Abatayo and Lynham 2020), and the public good game (Afzal, Turner, and Said 2015). All studies but one (Castillo and Carter 2011) were published within the last 10 years and focus on the Asia-Pacific region, with only two exceptions that instead focus on the Americas (Castillo and Carter 2011; Fleming, Chong, and Bejarano 2014). All studies except Chantarat et al. (2019) and Veszteg et al. (2015) use incentivized games. While the specific motives (altruism, fairness, reciprocity, etc.) for sharing resources with others vary between games, they all measure some form of prosociality with different degrees of strategic concerns (Levitt and List 2007).

In most cases, regressions were reported, with the dependent variables being the share of the endowment sent, returned, or contributed. For studies that used continuous outcome measures, we converted the results into percentages of the endowment given. Since these games allow us to represent explanatory variables as binary measures, and outcome measures as shares, comparisons across studies are straightforward. In Figure 1, for comparability, we use the estimates[5] from the regression models with the fewest control variables and a binary variable for exposure, measured either at the individual (Afzal, Turner, and Said 2015; Ahsan 2014; Becchetti, Castriota, and Conzo 2017; Fiala 2017; Kuroishi and Sawada 2019; Veszteg, Funaki, and Tanaka 2015) or village-level (Abatayo and Lynham 2020; Cassar, Healy, and von Kessler 2017; Chantarat et al. 2019; Fleming, Chong, and Bejarano 2014). Some studies estimated additional models based on *ex-post* self-reports of individual affectedness, such as the number of damages (Becchetti, Castriota, and Conzo 2017), financial damages, family members injured or killed (Cassar, Healy, and von Kessler 2017), and injuries to oneself or loved ones (Vardy and Atkinson 2019). These results will be summarized later in this chapter.

[5] Since in the study of Vardy and Atkinson (2019), no such statistics are provided, an estimation with an index is used in this case.

Figure 1. Experimental evidence of the effects of environmental hazards on prosociality



Notes: Figure shows a Forest plot of effect sizes with 95% confidence intervals. Each square represents the effect size of one study together with its confidence intervals; arrows mean that the confidence interval extends beyond the range of the graph. The vertical grey line represents zero effect. The column Country indicates the country where the study was conducted (VUT: Vanuatu, KHM: Cambodia, CHL: Chile, LKA: Sri Lanka, PHL: Philippines, BGD: Bangladesh, JPN: Japan, THA: Thailand, PAK: Pakistan); DIS indicates the disaster investigated (WI: Heavy wind, FL: Flood, EQ: Earthquake); ΔMonths indicates the months passed between occurrence of the disaster and data collection; N indicates the sample size; and EXP indicates the type of game (UG: Ultimatum Game; PGG: Public Goods Game; TG-S: Trust Game share sent; TG-R: Trust Game share returned; and DG: Dictator Game. An H was added if the game was non-incentivized, i.e., hypothetical). The explanatory variables are binary measures differentiating between affected and non-affected observations in all but one case where the explanatory variable is an index (Vardy and Atkinson 2019). The outcome variables are the shares sent and returned, taking values between zero and one, with an exception where participants could only decide whether to send (back) or not (Veszteg, Funaki, and Tanaka 2015). The reported results are estimated differences between affected and non-affected observations. For two studies, values from t-tests were used as the necessary statistics from the regression were not reported (Cassar, Healy, and von Kessler 2017; Veszteg, Funaki, and Tanaka 2015). Positive effect sizes indicate an increase in the share of sent/returned being exposed. A more detailed summary of the studies can be found in Appendix S1.

Independent of the economic games used, the effects of environmental hazards on prosociality are highly mixed and paint an incoherent picture (see Figure 1). Three out of four studies using the *dictator game (DG)* find no significant effect of exposure on prosociality (Chantarat et al. 2019; Kuroishi and Sawada 2019; Vardy and Atkinson 2019). The 2011 megaflood in Cambodia did not impact hypothetical "giving to random fellow villagers" among participants residing in flood-affected villages compared to non-affected villages (Chantarat et al. 2019). However, at the individual level, those classified as affected based on the submergence of their rice fields for 15 days or more showed an increase in giving. In contrast, the number of days rice fields remained submerged had no significant effect.

Similarly, 19 and 68 months after the 2012 flood in East Laguna, Philippines, Kuroishi and Sawada (2019) find no effect of exposure on participants' "giving" in a dictator game. Vardy and Atkinson (2019) conducted a modified dictator game in Vanuatu before and after Cyclone Pam in 2015. The study indicates that while self-reported individual-level damages do not affect giving, participants tended to give less after the event, whereas witnessing other people in distress appears to have a positive effect on giving. A study by Becchetti and colleagues (2017) is the only one of the four dictator games reviewed here that reports a significant effect of exposure on giving. Exposure to the 2004 tsunami in Sri Lanka is associated with both a decrease in giving and expected giving by others. Estimating the effect of each damage individually suggests that the effect is driven by economic damages, not injuries or property damage. Furthermore, receiving above-average help in the aftermath neither increases giving nor expectations.

Studies using the *trust game (TG)* to elicit prosociality also show inconclusive results. Cassar et al. (2017) find that exposure to an environmental hazard positively affects both trust *(TG:S)* and trustworthiness *(TG:R)*, whereas Fiala (2017) finds the opposite. Veszteg et al. (2015) report positive effects only for trustworthiness, and Fleming et al. (2014) find negative effects only for trustworthiness. In contrast, Ahsan (2014) reports no impact on either trust or trustworthiness. Cassar et al.'s (2017) analysis suggests that higher levels of sending and returning are found among participants in villages affected by the 2004 tsunami in Thailand. However, when controlling for either "help by other community members" or "external aid received," the binary village-level exposure to the flood becomes insignificant. This suggests that the perceived post-disaster conditions and individual damages are key to explaining different prosocial responses. However, these different perceptions could very well be endogenous to baseline levels of trust and trustworthiness, i.e., it is possible that less trusting community members already received less help from others in the community before the environmental hazard.

Veszteg et al. (2015) measure trust and trustworthiness shortly before and after the 2011 earthquake in Japan. However, since participants sampled *ex-post* were not the same as those sampled *ex-ante,* the data only allows for between-subject comparisons. In their study, participants could choose to send all or nothing of their hypothetical endowment (trust), and to return all, half, or nothing (trustworthiness). The between-subject comparison suggests that exposure to the earthquake increased trustworthiness while having no effect on trust (regression results resemble results from the t-test used in Figure 1). Fiala (2017) conducted a trust game after the 2013 flood in Battambang Province, Cambodia. Using a 10% level of significance, he found that being affected by the flood was associated with a decrease in both trust and trustworthiness; however, the influence on trustworthiness disappeared when controlling for village fixed effects. A study conducted in Chile after the 2010 earthquake discovered that living in an affected village had no statistically significant effect on the money sent but reduced the amount returned (Fleming, Chong, and Bejarano 2014). Lastly, Ahsan (2014) analyzes the effect of the 2010 cyclone, Aila, in Bangladesh and reports that being affected had no statistically significant effect on trust or trustworthiness.

Furthermore, one *public good game (PGG)* and one *ultimatum game (UG)* were conducted to study the effect of environmental hazards on social preferences. The public good game was implemented after the 2010 flood in Punjab, Pakistan (Afzal, Turner, and Said 2015). The authors found that exposure to this great flood, but no additional floods, positively impacted the overall contribution, while exposure to this flood, along with other floods, did not. The ultimatum game was conducted after Typhoon Bopha in the Philippines (Abatayo and Lynham 2020). Comparing participants from affected villages with those from unaffected villages suggests that exposure to the typhoon does not influence the offers made in the ultimatum game.

There is also research using hypothetical survey measures to examine the effects of exposure to an environmental hazard on social preferences. In their review, Chuang and Schechter (2015) note that survey measures tend to be more stable over time compared to experimental measures. However, outcome variables vary between studies, and results based on surveys tend to be inconclusive too. In two studies examining exposed regions, lower levels of prosociality were found (Biener and Landmann 2023; Rahman et al. 2020), whereas in four others, there was no change in prosociality (Andrabi and Das 2017; Calo-Blanco et al. 2017; Lee 2021; Maki et al. 2019), and in four additional studies, there were higher levels (Ahmad and Younas 2021; Bai and Li 2021; Shupp et al. 2017; Yamamura 2016). A detailed review of survey studies can be found in Supplementary Section S1.

3. Methodology and Data

Haiyan is the second strongest typhoon since modern meteorologic recordings. It caused extraordinary damage affecting nearly half the population in Western Visayas. From this region, we specifically studied Panay Island, as seen in Figure 2. There, average sustained winds reached 215 km/h, with gusts over 250 km/h, damaging almost 500,000 houses. According to our survey, the median time respondents had to prepare for Haiyan was only two hours, and over 90% had less than 10 hours to prepare. Thus, 76% of participants experienced Haiyan at home, while only 11% found shelter in a local evacuation center.

Typically, Panay residents are less exposed to severe typhoons due to the island's location outside the regular tropical storm belt; it is rarely hit by storms with winds exceeding 119 km/h (Skoufias et al. 2020). Moreover, Haiyan did not make landfall until well after the official end of the typhoon season on November 1. Therefore, it is unlikely that more risk-averse or less solidarity-oriented people systematically moved away due to the risks associated with strong typhoons. This is corroborated by the fact that out-migration numbers from our study villages post-Haiyan do not differ between more and less affected villages, as reported by local village officials (see Supplementary Table S2).

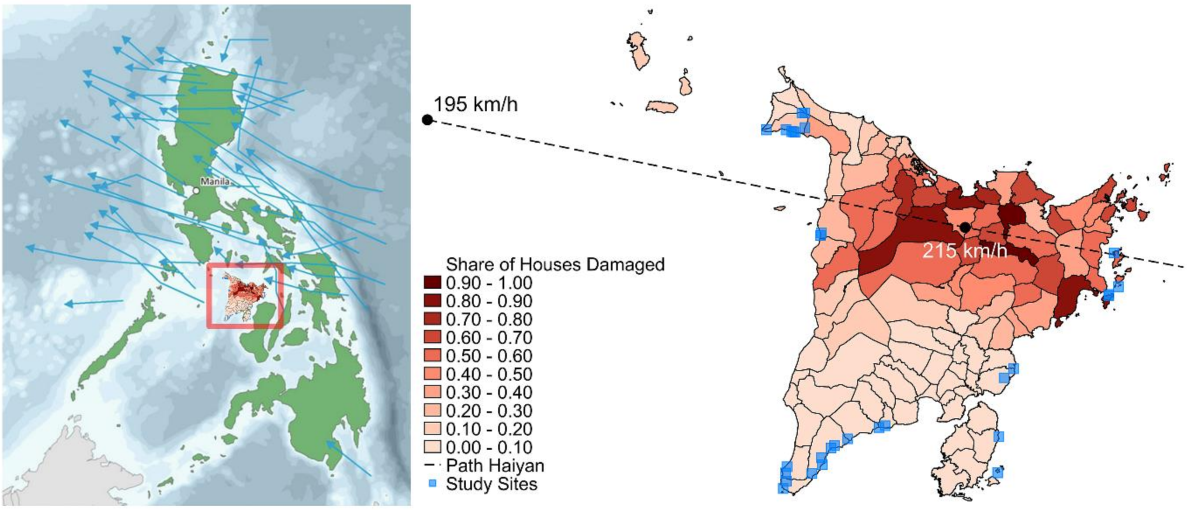
3.1. Sample

On Panay, we collected three samples spanning over 10 years: (1) baseline experiments in 2012, about one year before Haiyan; (2) follow-up experiments in 2016, about three years after Haiyan; and (3) a follow-up survey in 2022, about nine years after Haiyan.

A two-stage random sampling procedure was used to select participants for the baseline experiment in 2012. First, we randomly selected 30 villages in the rural coastal areas of Western Visayas on Panay, including some remote locations (Figure 2).

Figure 2. Study sites overview: damages caused by Haiyan

A: Paths of super-typhoons **B:** Paths of Haiyan and houses damaged on Panay Island



Notes: Panel A gives an overview of the paths of super-typhoons (>200 km/h; blue arrows) in the Philippines from 1945 to 2005, adapted from Zorn (2018). Our study area, Panay Island, is highlighted in red. Panel B shows the share of houses in each municipality on Panay Island that were damaged, with partial and total damages combined. The blue squares indicate the 30 villages where we collected data, and the dashed line represents Haiyan's path. Wind speed data comes from three weather stations reporting average sustained wind speeds over 10-minute intervals. The graph was constructed from freely available municipality data on the number of households (2010 Census), the number of houses damaged by Haiyan, and the wind speeds reported by weather stations.

In the second sampling stage, households were randomly chosen within each selected village. Local recruiters arrived at the location before the experimental workshop, gained permission from the "punong barangay" (the elected village representative or mayor), ensured facility availability for the games, and requested a list of households from which nine were randomly selected. The recruiters then sent invitation letters to a member of each household, preferably the household head, if available. The invitees received instructions to invite two close friends or relatives from different households.

We carried out one experimental workshop in each of the 30 villages with nine groups of three participants each. Only five groups did not show up or participate fully, giving us a sample of 265 groups, totaling 795 participants for the baseline. For the follow-up experiments in 2016, we went to the same 30 communities and tried to contact the 2012 participants again. In total, we were able to include 56% (N=450) of them in the second wave, and we filled up the remaining slots with new participants, giving us a total sample of 810 participants (270 groups). Those who decided not to participate in 2016 mainly stated that they did not have the time. In 2022, we carried out a short follow-up survey with some items relating to Haiyan. We again contacted participants from years prior, with 330 of the original 450 villagers participating in both data collections. An additional 639 new participants were interviewed for a total of 969 observations.

3.2. Data

3.2.1. Measuring solidarity transfers

Our measure of solidarity comes from the "solidarity game" introduced by Selten & Ockenfels (1998), where motivations for helping are primarily intrinsic. Thus, it is distinct from the risk-sharing arrangements cited in the introduction, which are self-enforcing and mainly based on the knowledge of future interaction. Nevertheless, the solidarity game has a strategic component, as participants need to reflect upon how they would like others to help them if they were in a bad situation. Therefore, the setup offers a relevant measure of solidarity that captures the relationship between the inherent risks of everyday life and mutual aid.

In our study, participants play the solidarity game in groups of three. The groups always consist of two players who know each other ("friends") and a third, randomly assigned player from the same community. The anonymous third player ("stranger") does not know the identity of the friends nor their relationship to one another, nor do the friends know the identity of the stranger. All participants play the solidarity game twice. For the primary analysis, we focus on those transfers where players do not know the recipient's identity. The stranger makes one transfer decision for each of the two friends in the group without knowing their identity. For strangers, we use the average of all four transfers across both rounds. For the two friends, we use the average of two decisions to transfer to the stranger across both rounds.

Each player starts with the same endowment of 200 Philippine pesos (PHP), equivalent to four US dollars in 2016. Then, one group member loses her endowment by chance due to an exogenous shock. The shock was implemented via a lottery by drawing balls from an opaque bag containing one red ball and two white balls.[6] If a red ball is drawn, the player loses her endowment. This design ensures that one group member always ends up losing her endowment. Before the draw, each player makes two transfer decisions assuming that they keep their endowment while one of the other two players loses theirs. Players can transfer between 0 and 70 pesos in increments of ten. To better reflect the aftermath

[6] If the solidarity game was relevant for payout (randomly decided at the end of the workshop), we would let the two friends in each group draw one ball from the same bag. If none of them drew a red ball, it was clear that the stranger in the group lost her endowment. This ensured that even after the workshop had ended, the two friends could not infer who the stranger in their group was and vice versa the stranger could not infer the identity of the other two group members.

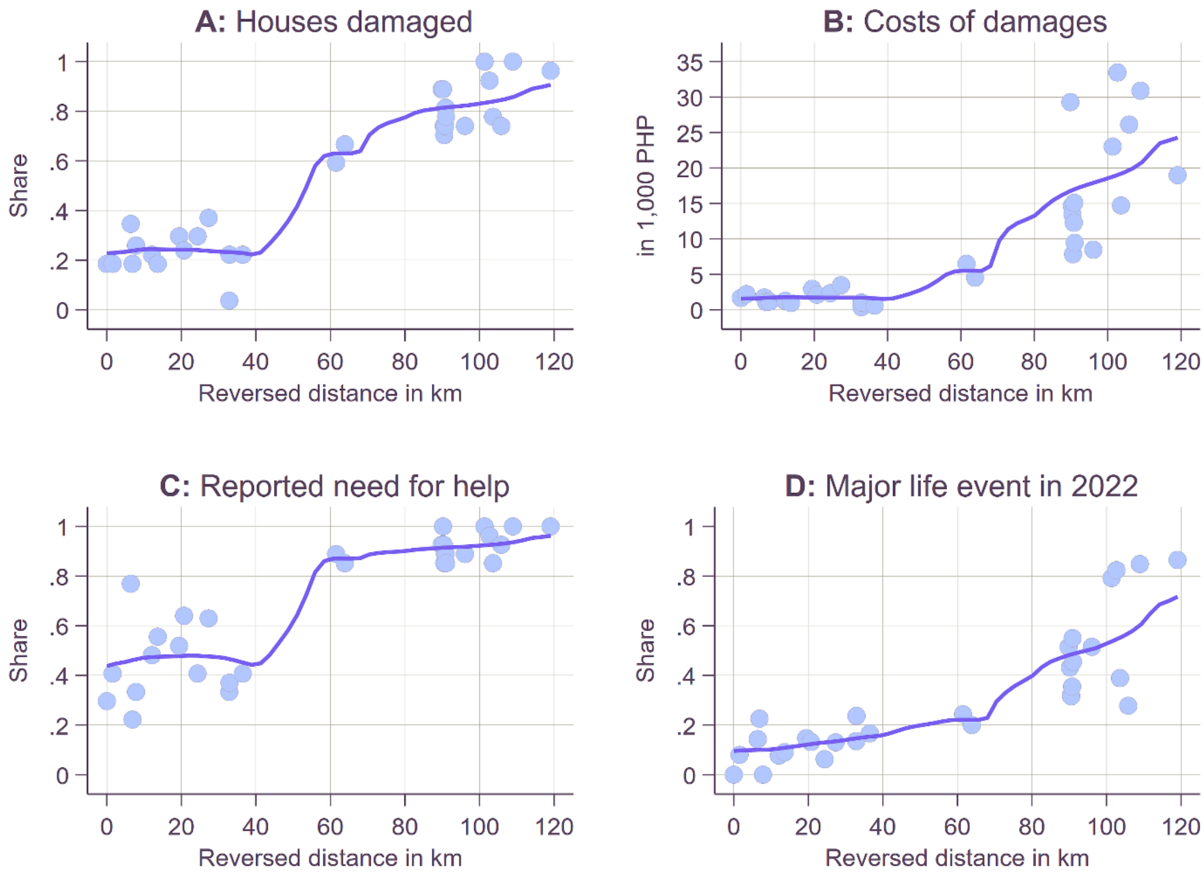
of a disaster, we introduced an upper limit of 70 PHP to exclude the possibility that the shock victim might be much better off than the two "winners" of the lottery. The transfer decisions are not disclosed at the end of the workshop, and decisions cannot easily be traced back, as the show-up fee included a fixed component of 100 PHP and a random component of 50 PHP. Additionally, before each transfer, each player has to guess how much they expect from the other players' transfers if they become the shock victim. Correct guesses are incentivized with 10 PHP payouts.

3.2.2.Measuring exposure to Haiyan

We measure exposure to Haiyan based on the distance of each village to the eye of the storm. For each village, we calculate the shortest distance between the path of Haiyan and the center of the village. In the primary analysis, we use the reversed distance in kilometers for ease of interpretation. Figure 3 shows that a higher reversed distance is related to stronger affectedness by Haiyan. The nearest village to the eye of the storm (the eye went directly through this village) is about 120 km closer than the village furthest away. The reversed distance explains 89% of the variation in the share of damaged houses (panel A). A village 10 km closer to the eye of the storm is associated with a 7 percentage point (pp) increase in the share of damaged houses - on average costing 2,030 PHP more in damages from each household (panel B) - and a 6 pp increase in people reporting that they needed assistance (panel C). Thus, distance from the storm is a good indicator of the exposure and affectedness of a community by Haiyan.

In the 2022 follow-up study, we asked participants open questions about positive and negative major life events they had experienced in the past 20 years. The data clearly indicate that Haiyan is still considered influential in people's lives almost 10 years later. On average, 62% of all respondents (597 out of 969) described Haiyan as a major life event (see panel D). In the village directly hit by Haiyan (Talotoan, east coast of Panay), 87% of respondents identified Haiyan as a major life event, whereas no one did in the village furthest away.

Figure 3. Overview of impacts caused by Haiyan



Notes: Shown are the share of self-reported houses damaged (panel A), costs of damages to assets (panel B), self-reported need of aid (panel C), and whether Haiyan was named as a major life event in 2022 (panel D). Costs of damages to assets is the sum of damages caused by Haiyan to the house, boat, crops, bike, car, or other productive assets. All measures are aggregated at the village level and plotted against each village's reversed distance from the eye of the storm (km). The purple line in each panel is from a kernel-weighted local polynomial regression (kernel = Epanechnikov, degree = 0, bandwidth = 10).

3.2.3. Additional explanatory variables

Additional socioeconomic information and attitudes (see Table 1) were collected in a post-experimental survey from 2012 and 2016.[7] Using the 2012 data, we describe the balanced sample, which contains only persons participating in both waves (remainers). On average, participants in our study are 41 years old and 60% female. Typical jobs in the study region are farming, fishing, fishing-related occupations like fish vendors, and manual labor. About 50% of our participants identify themselves as the household head, responsible for making important decisions for the household, which consists of five members on average. Most participants are married (90%), and 27% only finished elementary school.

A wealth index is calculated using principal components analysis (PCA) based on self-reported monthly income (mean=3800 PHP in 2012) adjusted by household size, savings of at least 1000 PHP (17% of remainers in 2012), debt of over 5000 PHP (33% of remainers in 2012), and whether meals had to be reduced in the past 12 months due to finances (65% of remainers in 2012). A higher score

[7] Surveys were conducted via pen and paper in 2012 and via tablet computers in 2016.

indicates that the participant is wealthier (higher income, savings, and less likely to be indebted and to reduce meals). Lastly, we include a measure of trust in the national government using a 5-point Likert-scale item, with higher values indicating more trust.[8]

3.2.4. Attrition and selection

In our sample, 56% of respondents are remainers, and 44% are dropouts (not participating in the 2016 wave). To understand whether attrition affects the internal validity of our estimates, Table 1 compares the mean outcomes in 2012 by attrition status (Ghanem, Hirshleifer, and Ortiz-Becerra 2021). We observe significant differences between remainers and dropouts regarding wealth, age, gender, identity as household head, being single, and household size. One possible explanation why women are more likely to participate in the economic games again could be lower opportunity costs, as they are less likely to have a regular job or are homemakers (as is the case for 40% of our female sample). Overall, remainers are significantly different from dropouts in terms of the presented characteristics ($F_{(11, 29)}=3.19$, $p<0.01$). This implies we should control for these relevant characteristics in our regression models. Importantly, we do not find evidence for significant differences in terms of solidarity transfers and expectations. In our primary analysis, we use the balanced sample and account for selective participation by controlling for all characteristics that differ between remainers and dropouts.

[8] Trust in national government: "In general, when thinking about the national government, how much to you trust it? Please indicate on a scale from 'completely distrust' to 5 'completely trust' the national government."

Table 1. Summary statistics by attrition status (2012)

	(1) Dropouts		(2) Remainers		T-test Difference
	N	Mean/SE	N	Mean/SE	(1)-(2)
Solidarity transfer (0,70)	342	34.93	450	33.67	1.27
		[1.90]		[1.27]	
Expected transfer (0,70)	342	28.76	450	27.52	1.24
		[1.59]		[1.13]	
Distance eye reversed in km	345	54.57	450	60.44	-5.87
		[8.09]		[7.71]	
Trust in national government	345	3.66	450	3.66	-0.00
		[0.07]		[0.05]	
Wealth index (PCA)	345	-0.46	450	-0.63	0.17*
		[0.07]		[0.07]	
Age (years)	345	40.42	450	41.95	-1.54*
		[0.62]		[0.50]	
Female (=1)	345	0.47	450	0.60	-0.13***
		[0.03]		[0.03]	
Household head (=1)	345	0.59	450	0.50	0.09**
		[0.03]		[0.03]	
Single (=1)	345	0.18	449	0.10	0.07***
		[0.02]		[0.02]	
Household size	345	4.88	450	5.08	-0.20*
		[0.09]		[0.10]	
Education: Elementary (=1)	345	0.21	450	0.27	-0.06
		[0.02]		[0.03]	
F-test of joint significance (F-stat)					3.19***
F-test, number of observations					792

Notes: The value displayed for t-tests are the differences in the means across the groups with standard errors clustered at the village level. ***, **, and * indicate significance at the 1, 5, and 10% critical level. Joint F-tests of orthogonality are reported to test whether differences in all control variables can explain the attrition status.

3.3. Econometric strategy

To examine Haiyan's effect on solidarity, we take advantage of having incentivized measures of solidarity transfers before and after the typhoon. Our identification strategy exploits variation in the distance of our sample villages from the eye of the storm, which, as argued in Section 3.2.2, is a proxy for the village's affectedness by the storm. In Equation (1), we control for time-invariant unobserved characteristics using a model based on the first differences of the variables. This specification allows us to capture the effect of Haiyan by comparing variation before and after Haiyan across participants who differ in their level of exposure, assuming the response would have been the same across groups in the absence of Haiyan.

Based on our review of the literature in Section 2, we make use of the variation in affectedness as measured by the reversed distance and include the possibility of a non-linear effect (Andrabi and Das 2017; Bai and Li 2021; Castillo and Carter 2011). Thus, our preferred model specification includes a nonlinear effect (quadratic, u-shape) of distance on solidarity transfers:

$$\Delta Y_{ij} = \alpha + \beta D_j + \delta D_j^2 + \gamma \Delta X_{ij} + \theta X_{ij} + \rho Y_{ijt-1} + \varepsilon_{ijt} \qquad (1)$$

The difference between variables one year before (2012) Haiyan and three years later (2016) is indicated by Δ. The dependent variable ΔY_{ij} captures the change in incentivized solidarity transfers of individual i in village j. The average change in solidarity transfers is estimated by α. β measures the impact of being closer to the center of Haiyan, and δ captures the non-linear effect of distance from

Haiyan. The vector ΔX_{ij} includes the following time-varying individual characteristics in first differences: expected transfers, trust in the national government, and wealth. We can easily control for changes in expected transfers to improve the precision of our estimates, as expectations are not significantly affected by Haiyan (see Supplementary Section 2.6).

We control for changes in wealth to rule out the possibility that they drive variations in solidarity transfers. Additionally, we account for changes in people's trust in the national government, as they could be influenced not only by Haiyan but also by the election of Duterte in 2016, which occurred shortly before the second wave of our data collection. This helps single out the effects of Haiyan on the underlying solidarity preferences.

There are some indications that the attrition of participants between the two data-collection waves was non-random (see Table 1). To account for selective participation in terms of time-invariant characteristics that differ between remainers and dropouts, we control for age, gender, household head, marital status, and education as captured by vector X_{ij}. Additionally, we consider regression to the mean effects (Barnett, van der Pols, and Dobson 2005) by controlling for the baseline value of the dependent variable, Y_{ijt-1}. Lastly, Equation (1) is estimated using ordinary least square regression with clustered standard errors at the village level to account for the fact that the impact of the typhoon is measured at this level.

An important aspect of first differencing is that we remove individual fixed effects. Other studies suffer from the fundamental econometric problem that, even if an environmental hazard strikes exogenously, *ex-post* experiences and social dynamics might be endogenous to individual-specific traits (i.e., fixed effects regarding the outcome variable). This implies that heterogeneous treatment effects cannot be credibly estimated by post-disaster experience. In Supplementary Section 2.3, we document that (i) those in need of aid and with higher perceived quality of help differ from other villagers in terms of baseline solidarity and that (ii) these differences change with increasing distance from Haiyan (and thus the share of villagers in need of aid). This suggests a complex correlation between individual fixed effects, post-disaster neediness, and perceived quality of help. The neediness and perceived quality of help are critical dimensions of heterogeneity, which we can analyze rigorously in contrast to the existing literature.

We conduct several robustness checks: (i) excluding observations from the two villages that suffered medium damages due to Haiyan; (ii) instead of using distance from the eye of the storm, we employ an affectedness index derived from a factor analysis based on village averages of damages, costs, aid neediness, and perceived financial and personal pressure; (iii) using tobit regression models to account for censoring the change in solidarity transfers; and (iv) constructing an alternative measure of solidarity based on three survey questions regarding the impact of Haiyan on the extent of help provision in communities.[9] Our results in response to these changes are robust in size and significance (see Supplementary Section 2.5). Using broader survey measures covering helping and social interactions, collected only in the second wave, confirms the internal validity of the results from incentivized experimental solidarity measures.

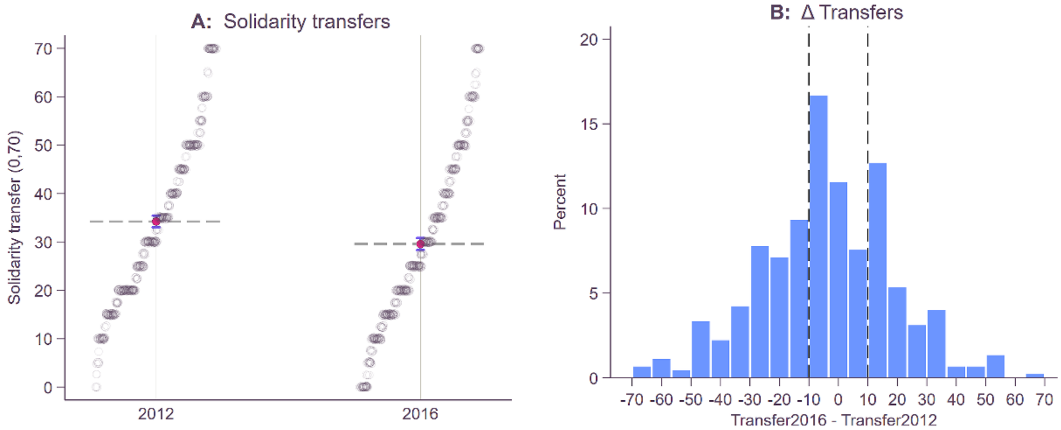
4. Results

This section begins with a descriptive overview of changes in solidarity transfers before using regression analysis to estimate the non-linear impact of Haiyan. Next, we use sample splits to explore whether Haiyan had different effects on transfers depending on pre-Haiyan characteristics and post-Haiyan individual experiences during the recovery. We conclude with long-term survey evidence showing how Haiyan still impacts solidarity almost 10 years later.

4.1. Impact of Haiyan on solidarity transfers

On average, participants transferred about 5 PHP less in 2016 than in 2012 (t-test $\Delta=0$, $t_{898}=4.35$, $p<0.00$), indicating a reduction in transfers over time. Changes in transfers range between ±70 PHP, but very few participants completely changed their transfers after Haiyan. More than 40% of the participants are characterized by relatively stable transfers: 42% (N=190) deviated in their transfers by no more than 10 PHP, 36% (N=163) reduced their transfers by more than 10 PHP, and 22% (N=97) increased their transfers by more than 10 PHP. Below we analyze whether these changes are systematically related to Haiyan.

Figure 4. Distribution of solidarity transfers



Notes: Panel A shows the distribution, means (grey reference line), and 95% confidence intervals of solidarity transfers before and after Haiyan. Distribution of changes in solidarity transfers (panel B) are drawn as percentages.

Table 2 reports the main effects caused by Haiyan using the quadratic model specification outlined in Section 3.3. The results show that sample participants farthest from or closest to Haiyan did not

[9] Principle-component analysis (PCA) based on three 7-point Likert-type items: (1) "I received help from people that I would not have thought would have helped me." (2) "I tried to help wherever I could." (3) "I feel closer to the people in my barangay than before Haiyan." The first component (eigenvalue=1.5) explains 50% of the variation in these three items and all items mainly load onto the first component.

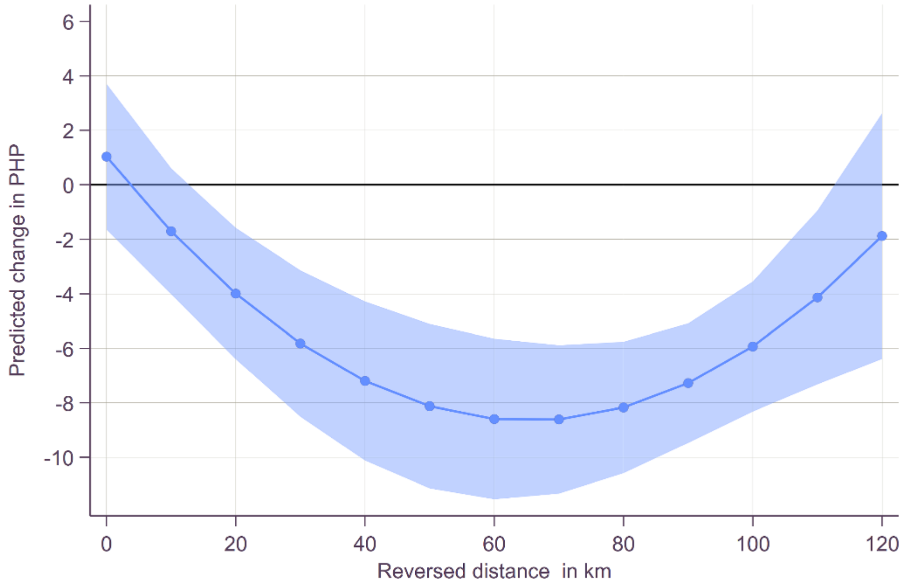
significantly alter their solidarity transfers, while participants from villages in between revealed a negative impact. The size and significance of this effect do not change when we control for changes in wealth and trust in the national government, in addition to individual characteristics that differed between remainers and dropouts (model 3). Based on the model reported in column (3), villages 50 km away from the eye of the storm saw transfers decrease by an average of about 8.6 PHP (ß=-8.61, p<.01, 95% confidence interval (CI)=[-11.32, -5.89]). This corresponds to half a standard deviation (SD) decrease in transfers. Figure 5 illustrates the non-linear effect on transfers along the entire reversed distance from the eye of the storm.

Table 2. Effect of Haiyan on transfers

	Δ Average transfers		
	(1)	(2)	(3)
Reversed distance	-0.230***	-0.287***	-0.296***
	(0.079)	(0.067)	(0.067)
Reversed distance^2	0.002**	0.002***	0.002***
	(0.001)	(0.001)	(0.001)
2012 Solidarity transfer (0,70)	-0.870***	-0.659***	-0.662***
	(0.070)	(0.065)	(0.066)
Δ Expected Transfer		0.367***	0.370***
		(0.035)	(0.035)
Δ Trust national government			-1.258**
			(0.582)
Δ Wealth Index (PCA)			-0.583
			(0.690)
Additional controls	Yes	Yes	Yes
N	450	450	450
Cluster	30	30	30
Adjusted R-squared	0.394	0.538	0.538
F-test: Distance & distance^2	6.627***	13.192***	12.642***

Notes: Additional controls are age, gender, household head, marital status, and differences in education between remainers and dropouts to account for attrition. Robust standard errors are clustered at the village level in brackets: * p < 0.10, ** p < 0.05, *** p < 0.01. The full regression table with all additional controls is reported in Supplementary Table S4.

Figure 5. Model predictions for changes in transfers



Notes: The graph shows the predicted change in solidarity transfers using a quadratic and linear fit at 10km intervals from the eye of the storm. Predictions are based on model (3) reported in Table 2, where all other independent variables are set to their means. The shaded area indicates a 95% confidence interval.

4.2. Exploring heterogeneous effects

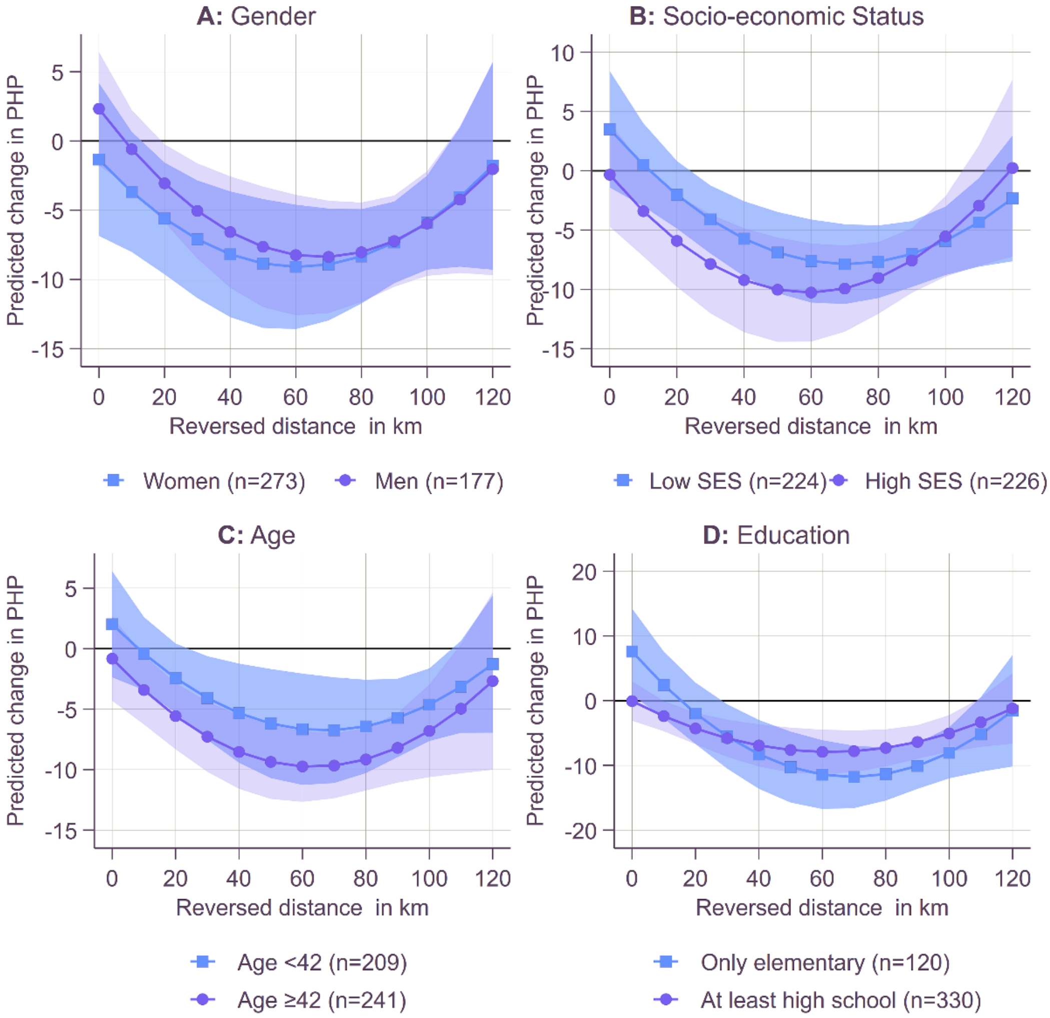
We have shown that people who experienced damages caused by Haiyan exhibit, on average, less solidarity, especially in villages that experienced medium damage. Next, we try to identify possible drivers and channels affecting transfers. In particular, we ask two questions: (i) Is the U-shape dependent on pre-Haiyan characteristics? (ii) Or perhaps the post-Haiyan recovery period? We study these questions by splitting the sample in different ways. Regarding question (i), we differentiate participants according to important characteristics measured in 2012: gender, socioeconomic status (SES), baseline solidarity (median split), age (median split), and education (median split). Regarding question (ii), we examine the post-Haiyan recovery phase using indicators such as aid neediness (binary), social interactions (median split), and aid quality (median split). Answers to (i) allow us to assess whether different groups reacted differently to Haiyan, whereas answers to (ii) help us understand the channels through which Haiyan affected solidarity transfers.

4.2.1. Pre-Haiyan differences

The literature shows that environmental hazards affect deep preferences, such as solidarity, differently conditional on gender (Hanaoka, Shigeoka, and Watanabe 2018) and education (Biener and Landmann 2023). However, Abatayo and Lynham (2020) do not find an asymmetric effect between men and women regarding the influence of disaster exposure on giving.

In our case, solidarity responses to Haiyan were not asymmetric for different groups of people. Figure 6 shows how the heterogeneous effects of pre-Haiyan characteristics impacted transfers. Effects were U-shaped across the board, independent of sex, SES, education, and age. Given our belief that Haiyan's effect was non-linear, we conducted joint significance tests of distance and distance squared. These tests indicated that the effect was insignificant for men ($F_{2,29}$=2.27, p=0.12) and only at the 10% level for younger participants ($F_{2,29}$=3.18, p=0.057). Thus, for these groups, Haiyan's impact was slightly less pronounced.

Figure 6. Heterogeneous effects of pre-Haiyan characteristics on transfers



Notes: The graphs show the predicted change in solidarity at 10km intervals from the eye of the storm for all subgroups depending on gender (panel A), socio-economic status (panel B), age (panel C), and education (panel D). Predictions are based on the models reported in Supplementary Table S5. The shaded area indicates a 95% confidence interval. The dependent variable in all models is the solidarity transfer in PHP. We always control for changes in wealth, trust in government, and imbalances in participation in terms of age, gender, household head, marital status, and education. Robust standard errors are clustered at the village level.

4.2.2. Post-Haiyan differences

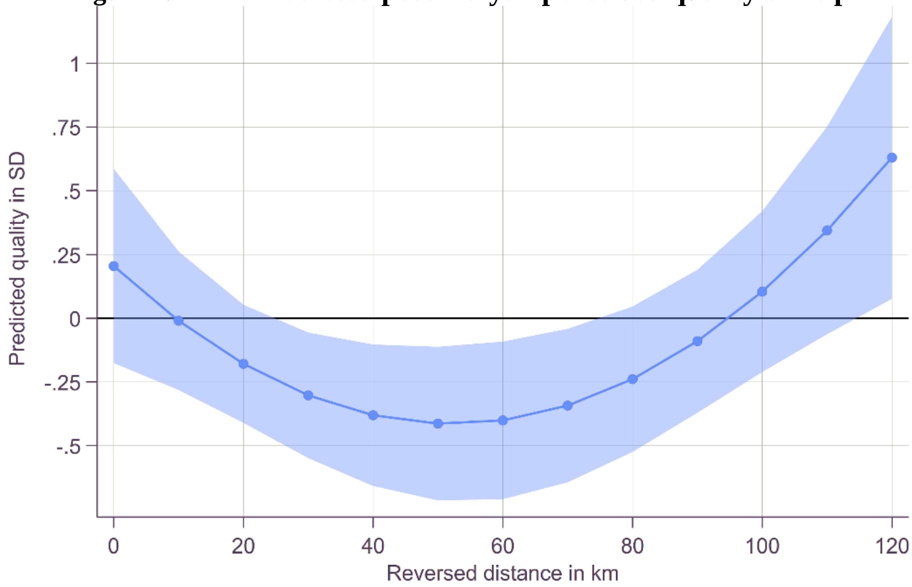
As shown above, Haiyan did not significantly affect solidarity transfers if damages were light or extensive but significantly reduced transfers in the case of medium damages. We would argue that if the damages were minimal, not much changed from the pre-crisis situation, so there would be no reason for people to adjust their solidarity behavior. If the damages were extensive, almost everyone in the local community would be worse off. We would conjecture that this has an equalizing effect, which prevents a loss of solidarity. In the case of medium damages, solidarity arguably declines, as some people are affected, and others are not, creating an asymmetry in overall losses. Moreover some households may have received more external support than others. Thus, a possible explanation for the non-linear relationship between damage exposure and solidarity relies on social experiences during the post-Haiyan recovery phase. Relevant questions here are: Was there conflict within the communities over the distribution of external aid or envy, as some received more than others? Does the non-linear impact on solidarity depend on whether people depended on external help?

While we do not have direct evidence to address these questions, we do have survey evidence detailing the need for external aid, retro perspectives regarding experiences with external aid, and interactions with other villagers. Using PCA with ten 7-point Likert items, we identify one main component explaining about 30% of the variation in the data.[10] This component loads positively on perceptions of the quality of help (external aid and internal help) and negatively on experience-related conflicts and selfish behaviors. Overall, it provides a good proxy of whether experiences in the recovery period were positive or negative. These items measure the vast social interactions people experienced after Haiyan well, but they do introduce some error since these questions were asked retrospectively almost three years after Haiyan.

Figure 7 shows that the relationship between Haiyan and the perceived quality of help is also U-shaped. In villages that experienced medium damage, the perceived quality of help was lower relative to villages with extensive or light damage. This corroborates our interpretation that there might have been more friction related to the social experiences in medium-affected villages. Thus, these survey-based measures of helping confirm the results based on the incentivized experimental solidarity measures, which suggests a considerable degree of internal validity.

[10] Supplementary Table S7 provides more details on eigenvalues and cumulative loadings of the components from the PCA, while Table S8 presents the loadings of the first component which we use here for analysis.

Figure 7. **Predicted post-Haiyan perceived quality of help**



Notes: The figure shows the effects on the perceived quality of help derived from PCA. The shaded area indicates a 95% confidence interval. We always control for changes in wealth, trust in government, and imbalances in participation in terms of age, gender, household head, marital status, and education. Robust standard errors are clustered at the village level. Full regression output is reported in Supplementary Table S9.

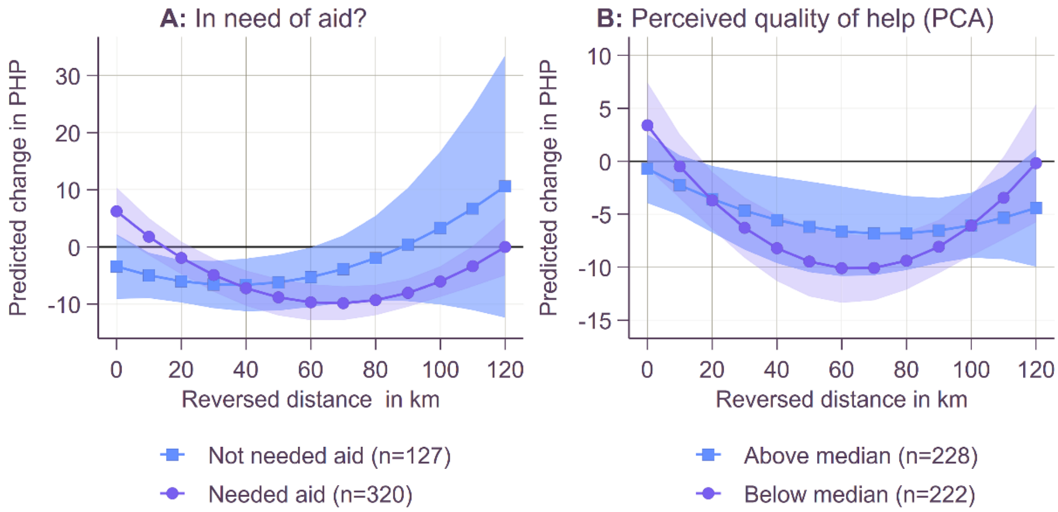
Next, we look at heterogeneous effects depending on post-Haiyan social experiences and dynamics[11]. Here, we can exploit our baseline data to account for the fact that *ex-post* experiences and social dynamics are endogenous to baseline solidarity transfers (see Figure S1). In terms of having needed aid, there is no significant effect for people that did not report being in need after Haiyan ($F_{2,24}$=1.04, p=0.37), whereas the largest negative effect is found for people who needed aid in medium-affected villages (panel A). Arguably, people who face smaller damages do not require aid and therefore have more time to show solidarity with more affected community members.

In addition to the heterogeneous effect on solidarity transfers, we also observe heterogeneous effects regarding changes in solidarity expectations after the disaster (see Supplementary Table S18). We find an inverted U-shape relationship for solidarity expectations, especially pronounced in medium-affected villages. Thus, in villages that fall into the category of medium-affectedness, people expect more but receive less, potentially fostering disappointment. However, when considering villagers who reported needing aid versus those who reported not needing aid, we again observed a U-shaped relationship for expected transfers, which aligns with the results obtained from actual transfers. This suggests that the impact of Haiyan on solidarity transfers depends on the fulfillment of expectations regarding the quality of the recovery process. If the experience of receiving help is poor (relative to the expectation), then the effect of the extent of the disaster on solidarity is more negative (panel B). For participants who evaluated the recovery process as above average, joint tests of distance and

[11] We also investigated whether respondents that were relatively worse off in terms of wealth after Haiyan differ from respondents that were relatively better off. We find no evidence for divergent responses based on changes in relative wealth status on solidarity, see Supplementary Table S6 for details.

distance squared indicate that the effect was significant only at the 10% level ($F_{2,29}$=2.97, p=0.067). Thus, if the help is perceived as being well done, it can, to some extent, help attenuate the negative impact on transfers in medium-affected villages.

Figure 8. Heterogeneous effects dependent on post-Haiyan social dynamics



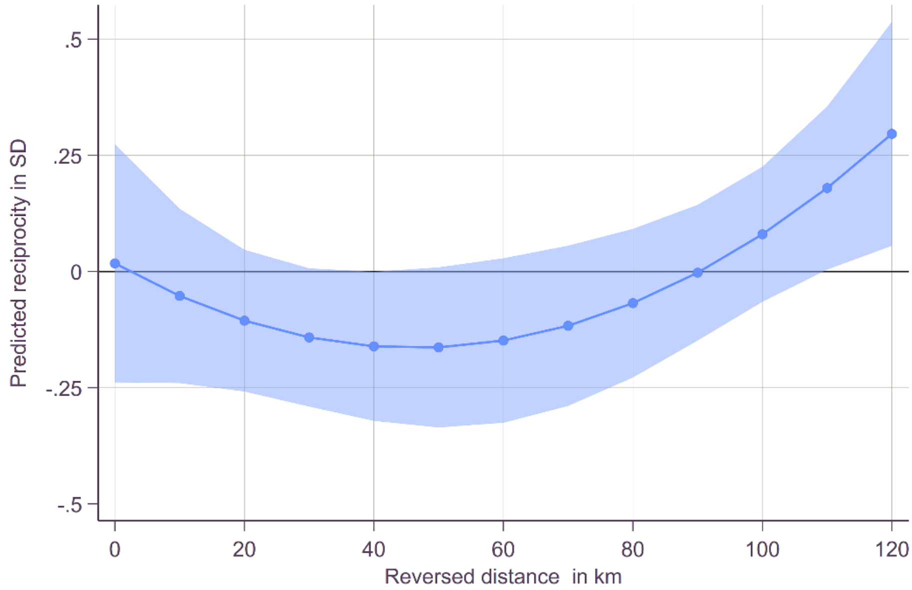
Notes: The graph shows the predicted change in solidarity at 10km intervals from the eye of the storm for all subgroups depending on having required aid (panel A) and perceived quality of help (panel B). For panel B, median splits were used to determine above (=high) and below (=low) median scoring participants. Predictions are based on the models reported in Supplementary Table S6. The shaded area indicates a 95% confidence interval. The dependent variable in all models is the change in solidarity transfer in PHP. We always control for changes in wealth, trust in government, and imbalances in participation in terms of age, gender, household head, marital status, and education. Robust standard errors are clustered at the village level.

4.3. Long-term impacts of Haiyan

In 2022, we collected survey data on reciprocal help[12] from individuals who had previously participated in both 2012 and 2016 (N=330). These data allow us to test for the long-term effects of Haiyan on reciprocity – an essential component of the solidarity concept employed in the previous analysis. Panel B in Figure 9 plots the model predictions of the standardized willingness to reciprocate survey item. Even nine years post- Haiyan, the relationship remains U-shaped. Respondents significantly affected by Haiyan tend to be much more willing to reciprocate help. For example, for villages 10 km away from the eye of the storm, our model predicts that reciprocity is 0.2 SD higher (ß=0.18, p=0.045, 95% CI=[.01, .36]), whereas in villages 70 km away from the eye we predict reciprocity to be negatively affected by almost 0.2 SD (ß=-0.16, p=0.049, 95% CI=[-.32, -.00]).

[12] This was measured using an 11-point Likert-type item: "How well does the following statement describe you as a person: 'when someone does me a favor, I am willing to return it.'? Please indicate your answer on a scale from 0 to 10. A 0 means 'does not describe me at all,' and a 10 means 'describes me perfectly.' You can use any number between 0 and 10."

Figure 9. Haiyan still matters



Notes: The figure shows the effects on self-reported positive reciprocity using only participants that remained for all three data collections. The shaded area indicates a 95% confidence interval. We control for changes (2022-2016) in wealth, trust in government, and imbalances in participation in terms of age, gender, household head, marital status, and education. We have seven missing values for the wealth index in 2022. Full models are reported in Supplementary Table S10.

5. Discussion and conclusion

Using the specific case of Typhoon Haiyan in the Philippines, we examine the effect of environmental hazards on social preferences. Data from incentivized solidarity games collected before and after the disaster allow us to analyze how a major environmental shock and the recovery experience of this shock affected Filipino social preferences. This study goes beyond the existing literature by exploring the concrete social dynamics that arise in the aftermath of such an event. Existing research has primarily focused on identifying the causal effect of a hazard on one or more outcome indicators of interest, but no clear trends have emerged. In contrast, we argue in line with Kelman (2020) that it is our actions and experiences that ultimately turn a hazard into a disaster. Thus, one must examine the social processes that emerge during the post-disaster recovery period to understand why some hazards lead to positive outcomes while others do not. Methodologically, we contend that the availability of baseline data is essential to achieving this goal. Although the occurrence of an environmental hazard is exogenous, post-disaster recovery and social dynamics are likely endogenous to pre-shock levels of prosocial behavior. By having access to a relevant measure of solidarity from pre- and post-disaster, while observing varying degrees of exposure to Haiyan, we are presented with a unique opportunity to compare an individual's level of solidarity before and after the disaster. Moreover, we can account for differences in the extent of storm-related damage across our sample village, as well as individual subjective differences regarding efforts to mitigate those damages.

Our results show that the relationship between hazard exposure and solidarity is non-linear. People show significantly less solidarity in communities that experienced medium damages than in more or less affected ones. Only a few studies can identify non-linear effects, as hazard exposure is typically measured in a binary way and at the village level. However, Castillo & Carter (2011) find an inverse U-shape of Hurricane Mitch on altruism, trust, and trustworthiness in Honduras. Likewise, Bai and Li (2021) find evidence of an inverse U-shape of earthquakes in Indonesia from 2004 to 2007. In light of our study, the explanation for this specific result − and the mixed findings in the literature more generally − likely lies in people's concrete experiences during the recovery period. This conclusion is also supported by research reported by Cassar et al. (2017), which suggests that the specific post-disaster conditions experienced by individuals (external aid or internal help) and the extent of their personal damages play a crucial role in explaining variations in prosocial behavior.[13]

Our analysis reveals that individual experiences in villages with medium damages related to the quality of external aid and help are significantly worse than in communities with lower and higher damages, i.e., U-shaped like the direct impact of Haiyan on solidarity. If the damage from the disaster is extensive, likely, almost everyone in the local community is negatively affected. This shared experience could have an equalizing effect and potentially prevent a loss of solidarity. However, in moderate damage, solidarity may decline, as some individuals are affected while others are not, creating an asymmetry in losses. This supports our interpretation that aid distribution is more challenging in these moderately-affected areas and potentially explains the negative effects on solidarity discovered there. In terms of pre-Haiyan differences, we find that the effects persist independent of socioeconomic status and education – all groups were similarly affected by Haiyan. Only men's and younger participants' solidarity appeared less affected by Haiyan, presumably because these groups are potentially less vulnerable *ex-ante* and so better equipped to recover from Haiyan or because they were able to acquire more resources from external help.

Lastly, our survey evidence from the same participants collected in 2022 shows that the effects on solidarity persisted almost 10 years post-Haiyan. This highlights the importance of long-term studies to identify the persistence of effects beyond the immediate recovery period. Evidence from the immediate post-disaster period (~0-12 months after the disaster struck) finds that people predominantly support each other (Drury, Novelli, and Stott 2013; Helsloot and Ruitenberg 2004; Ntontis et al. 2020; Steimanis and Vollan 2022). Steimanis & Vollan (2022) asked participants in the strongly affected regions they studied to recall either positive or negative events during the recovery period post-Haiyan. Similar to the above studies, the results of their investigation indicate that game

[13] Note that the authors cannot account for the possibility that these *ex-post* recovery and social dynamics might be endogenous to baseline prosociality.

participants showed a higher degree of solidarity when they remembered Haiyan versus respondents who did not. Thus, although environmental hazards can temporarily strengthen prosociality, our study suggests that this is conditional on the extent of the damage and the personal experience of the recovery process; the long-term effects can differ from the immediate response during the recovery period (0-12 months after the disaster). In other words, these related experiences appear to affect the creation and maintenance of social capital, which impacts prosocial behavior.

Several limitations to this study should be considered when interpreting the results. First, the sample is not randomly selected at the village level, and there are indications of selective participation in both waves, which may limit the generalizability of the findings. However, using only the 2016 samples still reveals a U-shaped effect of Haiyan on solidarity (Supplementary Table S11). In addition, the analysis could be affected by the specific cultural context of the Philippines; hence, one must be cautious when extrapolating these findings to other populations and settings. Second, the sample size for medium-exposed villages is small, which may limit the precision of the estimates. However, removing these villages from the analysis does not alter the U-shaped relationship between exposure to Haiyan and solidarity (Supplementary Table S12). Hence, the relationship between exposure to Haiyan and solidarity holds true even when medium-exposed villages are excluded from the analysis. Finally, it could be the case that participants who are less inclined towards solidarity systematically relocated temporarily or permanently in response to Haiyan.[14] Data from local village officials covering out-migration after Haiyan show that overall, out-migration remained low and was not correlated with the severity of damages caused by Haiyan (Supplementary Table S2). While this study strives to account for these limitations, the main findings should be seen as estimates of the impact of Haiyan on the selected "balanced sample."

Future endeavors to replicate our findings in other contexts and explore people's specific experiences in the aftermath of disasters necessitate the availability of appropriate baseline data. It would be particularly valuable to conduct studies in areas with contexts such as inequality, governance systems, or past experiences with environmental hazards that differ significantly from those in the Philippines. Past experiences with environmental hazards, especially, may shed light on the role that specific cultural factors play in disaster recovery; e.g., in the Philippines, there is a "culture of disaster" which supports mutual support in times of hardship (Bankoff 2003; Steimanis and Vollan 2022). Moreover, it is vital to consider the influence of media attention and external funding on the observed relationships. Typhoon Haiyan received extensive media coverage globally, resulting in substantial financial aid for disaster relief efforts. Consequently, the U-shaped relationship observed in our study,

[14] Temporary relocation becomes an issue only if people do not return before the second wave of our data collection, i.e., within a period of three years.

where the most exposed individuals were not negatively affected, may not hold in contexts where the disaster did not elicit such a significant humanitarian response.

An increasing number of people worldwide are already experiencing more severe floods, erosion, and heavy winds due to climate change (Seneviratne et al. 2021). Losses and damages will be particularly severe among low-income populations, where both exposure and vulnerability to environmental hazards is high (Byers et al. 2018; Stephane Hallegatte et al. 2016; Jongman et al. 2015; Winsemius et al. 2018). How such environmental hazards shape solidarity in the long term has important implications for policymakers and disaster management practitioners when deciding how to support at-risk communities. Solidarity and informal support networks are relevant factors for facilitating disaster recovery and rebuilding efforts, particularly in low-income countries such as the Philippines. Disaster management efforts should recognize and support the role of these informal support networks in the recovery process. Finally, our findings suggest that experiences encountered during the reconstruction period can have a lasting impact on individual solidarity. Thus, to foster long-term community resilience, disaster management efforts should prioritize creating opportunities for positive experiences during the reconstruction period, and special attention should be given to villages with uneven exposure and damage.

References

Abatayo, Anna Lou, and John Lynham. 2020. "Risk Preferences after a Typhoon: An Artefactual Field Experiment with Fishers in the Philippines." *Journal of Economic Psychology* 79 (August): 102195. https://doi.org/10.1016/j.joep.2019.102195.

Adler, Paul S., and Seok-Woo Kwon. 2002. "Social Capital: Prospects for a New Concept." *Academy of Management Review* 27 (1): 17–40. https://doi.org/10.5465/amr.2002.5922314.

Afzal, Uzma, Ginger Turner, and Farah Said. 2015. "Natural Disasters and Social Capital Evidence from a Field Experiment in Pakistan." *CREB-WP* 02–15.

Ahmad, Noman, and Muhammad Zeeshan Younas. 2021. "A Blessing in Disguise?: Assessing the Impact of 2010–2011 Floods on Trust in Pakistan*." *Environmental Science and Pollution Research* 28 (20): 25419–31. https://doi.org/10.1007/s11356-020-12332-4.

Ahsan, Dewan A. 2014. "Does Natural Disaster Influence People′s Risk Preference and Trust? An Experiment from Cyclone Prone Coast of Bangladesh." *International Journal of Disaster Risk Reduction* 9 (September): 48–57. https://doi.org/10.1016/j.ijdrr.2014.02.005.

Andrabi, Tahir, and Jishnu Das. 2017. "In Aid We Trust: Hearts and Minds and the Pakistan Earthquake of 2005." *The Review of Economics and Statistics* 99 (3): 371–86. https://doi.org/10.1162/REST_a_00638.

Bai, Yu, and Yanjun Li. 2021. "More Suffering, More Involvement? The Causal Effects of Seismic Disasters on Social Capital." *World Development* 138 (February): 105221. https://doi.org/10.1016/j.worlddev.2020.105221.

Bankoff, Greg. 2003. *Cultures of Disaster : Society and Natural Hazard in the Philippines*. Routledge. https://doi.org/10.4324/9780203221891.

Barnett, Adrian G., Jolieke C. van der Pols, and Annette J. Dobson. 2005. "Regression to the Mean: What It Is and How to Deal with It." *International Journal of Epidemiology* 34 (1): 215–20. https://doi.org/10.1093/ije/dyh299.

Bauer, Michal, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel, and Tamar Mitts. 2016. "Can War Foster Cooperation?" *Journal of Economic Perspectives* 30 (3): 249–74. https://doi.org/10.1257/jep.30.3.249.

Becchetti, Leonardo, Stefano Castriota, and Pierluigi Conzo. 2017. “Disaster, Aid, and Preferences: The Long-Run Impact of the Tsunami on Giving in Sri Lanka.” *World Development* 94 (June): 157–73. https://doi.org/10.1016/j.worlddev.2016.12.014.

Biener, Christian, and Andreas Landmann. 2023. “Recovery Mode: Non-Cognitive Skills after the Storm.” *World Development* 164 (April): 106152. https://doi.org/10.1016/j.worlddev.2022.106152.

Byers, Edward, Matthew Gidden, David Leclère, Juraj Balkovic, Peter Burek, Kristie Ebi, Peter Greve, et al. 2018. “Global Exposure and Vulnerability to Multi-Sector Development and Climate Change Hotspots.” *Environmental Research Letters* 13 (5): 055012. https://doi.org/10.1088/1748-9326/aabf45.

Calo-Blanco, Aitor, Jaromír Kovářík, Friederike Mengel, and José Gabriel Romero. 2017. “Natural Disasters and Indicators of Social Cohesion.” Edited by Ignacio Correa-Velez. *PLOS ONE* 12 (6): e0176885. https://doi.org/10.1371/journal.pone.0176885.

Cassar, Alessandra, Andrew Healy, and Carl von Kessler. 2017. “Trust, Risk, and Time Preferences After a Natural Disaster: Experimental Evidence from Thailand.” *World Development* 94 (June): 90–105. https://doi.org/10.1016/j.worlddev.2016.12.042.

Castillo, Marco, and Michael Carter. 2011. “Behavioral Responses to Natural Disasters.” *Working Papers 1026 George Mason University, Interdisciplinary Center for Economic Science*, June.

Chantarat, Sommarat, Sothea Oum, Krislert Samphantharak, and Vathana Sann. 2019. “Natural Disasters, Preferences, and Behaviors: Evidence from the 2011 Mega Flood in Cambodia.” *Journal of Asian Economics* 63 (August): 44–74. https://doi.org/10.1016/j.asieco.2019.05.001.

Drury, John. 2018. “The Role of Social Identity Processes in Mass Emergency Behaviour: An Integrative Review.” *European Review of Social Psychology* 29 (1): 38–81. https://doi.org/10.1080/10463283.2018.1471948.

Drury, John, David Novelli, and Clifford Stott. 2013. “Psychological Disaster Myths in the Perception and Management of Mass Emergencies.” *Journal of Applied Social Psychology* 43 (11): 2259–70. https://doi.org/10.1111/jasp.12176.

Fafchamps, Marcel, and Flore Gubert. 2007a. “Risk Sharing and Network Formation.” *American Economic Review* 97 (2): 75–79. https://doi.org/10.1257/aer.97.2.75.

———. 2007b. “The Formation of Risk Sharing Networks.” *Journal of Development Economics*, Papers from a Symposium: The Social Dimensions of Microeconomic Behaviour in Low-Income Communities, 83 (2): 326–50. https://doi.org/10.1016/j.jdeveco.2006.05.005.

Fafchamps, Marcel, and Susan Lund. 2003. “Risk-Sharing Networks in Rural Philippines.” *Journal of Development Economics* 71 (2): 261–87. https://doi.org/10.1016/S0304-3878(03)00029-4.

Fiala, Oliver. 2017. *Natural Disasters and Individual Behaviour in Developing Countries*. Contributions to Economics. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-53904-1.

Fleming, David A., Alberto Chong, and Hernán D. Bejarano. 2014. “Trust and Reciprocity in the Aftermath of Natural Disasters.” *The Journal of Development Studies* 50 (11): 1482–93. https://doi.org/10.1080/00220388.2014.936395.

Ghanem, Dalia, Sarojini Hirshleifer, and Karen Ortiz-Becerra. 2021. “Testing Attrition Bias in Field Experiments,” February. https://doi.org/10.26085/C38C76.

Gonulal, Serap Oguz. 2019. “Philippines Financial Sector Assessment Program: Insurance Sector - Targeted Assessment of ICPS and Development Issues.” Washington, DC: World Bank. https://openknowledge.worldbank.org/handle/10986/36184.

Hallegatte, Stephane, Joeri Rogelj, Myles Allen, Leon Clarke, Ottmar Edenhofer, Christopher B. Field, Pierre Friedlingstein, et al. 2016. “Mapping the Climate Change Challenge.” *Nature Climate Change* 6 (7): 663–68. https://doi.org/10.1038/nclimate3057.

Hallegatte, Stéphane, Adrien Vogt-Schilb, Julie Rozenberg, Mook Bangalore, and Chloé Beaudet. 2020. “From Poverty to Disaster and Back: A Review of the Literature.” *Economics of Disasters and Climate Change* 4 (1): 223–47. https://doi.org/10.1007/s41885-020-00060-5.

Hanaoka, Chie, Hitoshi Shigeoka, and Yasutora Watanabe. 2018. “Do Risk Preferences Change? Evidence from the Great East Japan Earthquake.” *American Economic Journal: Applied Economics* 10 (2): 298–330. https://doi.org/10.1257/app.20170048.

Helsloot, I., and A. Ruitenberg. 2004. “Citizen Response to Disasters: A Survey of Literature and Some Practical Implications.” *Journal of Contingencies and Crisis Management* 12 (3): 98–111. https://doi.org/10.1111/j.0966-0879.2004.00440.x.

Jongman, Brenden, Hessel C. Winsemius, Jeroen C. J. H. Aerts, Erin Coughlan de Perez, Maarten K. van Aalst, Wolfgang Kron, and Philip J. Ward. 2015. “Declining Vulnerability to River Floods and the

Global Benefits of Adaptation.” *Proceedings of the National Academy of Sciences* 112 (18): E2271–80. https://doi.org/10.1073/pnas.1414439112.

Kang, Sung, and Mark Skidmore. 2018. “The Effects of Natural Disasters on Social Trust: Evidence from South Korea.” *Sustainability* 10 (9): 2973. https://doi.org/10.3390/su10092973.

Kelman, Ilan. 2020. *Disaster by Choice: How Our Actions Turn Natural Hazards into Catastrophes*. Oxford, New York: Oxford University Press.

Kuroishi, Yusuke, and Yasuyuki Sawada. 2019. “Motivations behind Prosocial Behavior: Evidence from the Philippines.” *Journal of Asian Economics* 64 (October): 101127. https://doi.org/10.1016/j.asieco.2019.06.003.

Lahsen, Myanna, and Jesse Ribot. 2022. “Politics of Attributing Extreme Events and Disasters to Climate Change.” *WIREs Climate Change* 13 (1): e750. https://doi.org/10.1002/wcc.750.

Lee, Juheon. 2021. “The Social Impact of Natural Hazards: A Multi-level Analysis of Disasters and Forms of Trust in Mainland China.” *Disasters* 45 (1): 158–79. https://doi.org/10.1111/disa.12410.

Levitt, Steven D., and John A. List. 2007. “What Do Laboratory Experiments Measuring Social Preferences Reveal about the Real World?” *The Journal of Economic Perspectives* 21 (2): 153–74.

Maki, Alexander, Patrick C. Dwyer, Susanne Blazek, Mark Snyder, Roberto González, and Siugmin Lay. 2019. “Responding to Natural Disasters: Examining Identity and Prosociality in the Context of a Major Earthquake.” *British Journal of Social Psychology* 58 (1): 66–87. https://doi.org/10.1111/bjso.12281.

Malmendier, Ulrike, and Stefan Nagel. 2011. “Depression Babies: Do Macroeconomic Experiences Affect Risk Taking?*.” *The Quarterly Journal of Economics* 126 (1): 373–416. https://doi.org/10.1093/qje/qjq004.

———. 2016. “Learning from Inflation Experiences.” *The Quarterly Journal of Economics* 131 (1): 53–87. https://doi.org/10.1093/qje/qjv037.

Mas, E., J. Bricker, S. Kure, B. Adriano, C. Yi, A. Suppasri, and S. Koshimura. 2015. “Field Survey Report and Satellite Image Interpretation of the 2013 Super Typhoon Haiyan in the Philippines.” *Natural Hazards and Earth System Sciences* 15 (4): 805–16. https://doi.org/10.5194/nhess-15-805-2015.

NDRRMC. 2013. “Final Report of the Effecty of Typhoon ‘Yolanda’ (Haiyan).” Philippines: Natural Disaster Risk Reduction and Management Council. https://ndrrmc.gov.ph/attachments/article/1329/FINAL_REPORT_re_Effects_of_Typhoon_YOLANDA_HAIYAN_06-09NOV2013.pdf.

Ntontis, Evangelos, John Drury, Richard Amlôt, G. James Rubin, and Richard Williams. 2018. “Emergent Social Identities in a Flood: Implications for Community Psychosocial Resilience.” *Journal of Community & Applied Social Psychology* 28 (1): 3–14. https://doi.org/10.1002/casp.2329.

Ntontis, Evangelos, John Drury, Richard Amlôt, G. James Rubin, Richard Williams, and Patricio Saavedra. 2021. “Collective Resilience in the Disaster Recovery Period: Emergent Social Identity and Observed Social Support Are Associated with Collective Efficacy, Well-Being, and the Provision of Social Support.” *The British Journal of Social Psychology* 60 (3): 1075–95. https://doi.org/10.1111/bjso.12434.

Ntontis, Evangelos, John Drury, Richard Amlôt, Gideon James Rubin, and Richard Williams. 2020. “What Lies beyond Social Capital? The Role of Social Psychology in Building Community Resilience to Climate Change.” *Traumatology* 26 (3): 253–65. https://doi.org/10.1037/trm0000221.

O’Neill, Elizabeth Drury, Beatrice Crona, Alice Joan G. Ferrer, and Robert Pomeroy. 2019. “From Typhoons to Traders: The Role of Patron-Client Relations in Mediating Fishery Responses to Natural Disasters.” *Environmental Research Letters* 14 (4): 045015. https://doi.org/10.1088/1748-9326/ab0b57.

Quarantelli, E L, and R R Dynes. 1977. “Response to Social Crisis and Disaster.” *Annual Review of Sociology* 3 (1): 23–49. https://doi.org/10.1146/annurev.so.03.080177.000323.

Rahman, Muhammad Habibur, Grace H.Y. Lee, Nourin Shabnam, and Susantha Jayasinghe. 2020. “Weathering Trust.” *Journal of Economic Behavior & Organization* 178 (October): 449–73. https://doi.org/10.1016/j.jebo.2020.07.027.

Selten, Reinhard, and Axel Ockenfels. 1998. “An Experimental Solidarity Game.” *Journal of Economic Behavior & Organization* 34 (4): 517–39. https://doi.org/10.1016/S0167-2681(97)00107-8.

Seneviratne, Sonia I., Xuebin Zhang, M. Adnan, W. Badi, Claudine Dereczynski, Alejandro Di Luca, S. Ghosh, et al. 2021. “Weather and Climate Extreme Events in a Changing Climate.” In *Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change*, edited by Valérie Masson-Delmotte, Panmao Zhai, Anna Pirani, Sarah L. Connors, Clotilde Péan, Sophie Berger, Nada Caud, et al., 1513–1766.

Cambridge, United Kingdom and New York, NY, USA: Cambridge University Press. https://doi.org/10.1017/9781009157896.001.
Shupp, Robert, Scott Loveridge, Mark Skidmore, Jungmin Lim, and Cynthia Rogers. 2017. "Trust and Patience after a Tornado." *Weather, Climate, and Society* 9 (4): 659–68. https://doi.org/10.1175/WCAS-D-16-0135.1.
Skoufias, Emmanuel, Yasuhiro Kawasoe, Eric Strobl, and Pablo Acosta. 2020. "Identifying the Vulnerable to Poverty from Natural Disasters: The Case of Typhoons in the Philippines." *Economics of Disasters and Climate Change* 4 (1): 45–82. https://doi.org/10.1007/s41885-020-00059-y.
Steimanis, Ivo, and Björn Vollan. 2022. "Prosociality as Response to Slow- and Fast-Onset Climate Hazards." *Global Sustainability* 5: e10. https://doi.org/10.1017/sus.2022.9.
Stigler, George J., and Gary S. Becker. 1977. "De Gustibus Non Est Disputandum." *The American Economic Review* 67 (2): 76–90.
Townsend, Robert M. 1994. "Risk and Insurance in Village India." *Econometrica* 62 (3): 539–91. https://doi.org/10.2307/2951659.
Toya, Hideki, and Mark Skidmore. 2014. "Do Natural Disasters Enhance Societal Trust?: Do Natural Disasters Enhance Societal Trust?" *Kyklos* 67 (2): 255–79. https://doi.org/10.1111/kykl.12053.
Udry, Christopher. 1994. "Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria." *The Review of Economic Studies* 61 (3): 495–526. https://doi.org/10.2307/2297901.
Vardy, Tom, and Quentin D. Atkinson. 2019. "Property Damage and Exposure to Other People in Distress Differentially Predict Prosocial Behavior After a Natural Disaster." *Psychological Science* 30 (4): 563–75. https://doi.org/10.1177/0956797619826972.
Veszteg, Róbert Ferenc, Yukihiko Funaki, and Aiji Tanaka. 2015. "The Impact of the Tohoku Earthquake and Tsunami on Social Capital in Japan: Trust before and after the Disaster." *International Political Science Review* 36 (2): 119–38. https://doi.org/10.1177/0192512113509501.
Winsemius, Hessel C., Brenden Jongman, Ted I. E. Veldkamp, Stephane Hallegatte, Mook Bangalore, and Philip J. Ward. 2018. "Disaster Risk, Climate Change, and Poverty: Assessing the Global Exposure of Poor People to Floods and Droughts." *Environment and Development Economics* 23 (3): 328–48. https://doi.org/10.1017/S1355770X17000444.
Yamamura, Eiji. 2016. "Natural Disasters and Social Capital Formation: The Impact of the Great Hanshin-Awaji Earthquake: Natural Disasters and Social Capital." *Papers in Regional Science* 95 (March): S143–64. https://doi.org/10.1111/pirs.12121.
Zorn, Matija. 2018. "Natural Disasters and Less Developed Countries." In *Nature, Tourism and Ethnicity as Drivers of (De)Marginalization: Insights to Marginality from Perspective of Sustainability and Development*, edited by Stanko Pelc and Miha Koderman, 59–78. Perspectives on Geographical Marginality. Cham: Springer International Publishing. https://doi.org/10.1007/978-3-319-59002-8_4.

Supplementary Information for:

“A Storm Between Two Waves: Recovery Processes, Social Dynamics, and Heterogeneous Effects of Typhoon Haiyan on Social Preferences”

Authors: Ivo Steimanis[1], Max Burger[1], Bernd Hayo[2], Andreas Landmann[3], and Björn Vollan[1,]*

Affiliations: [1] Sustainable Use of Natural Resources, Philipps University Marburg, Am Plan 1, 35037 Marburg, Germany.
[2] Marburg Centre for Institutional Economics, Philipps University Marburg, Universitätsstr. 24, 35037 Marburg, Germany.
[3] Institute of Economics, Friedrich-Alexander-Universität Erlangen-Nürnberg, Germany

***Corresponding author**: bjoern.vollan@wiwi.uni-marburg.de, Tel: +49-6421 2823725

ORCID: Burger: 0000-0003-2334-3885; Hayo: 0000-0001-9037-4513; Landmann: 0000-0002-9835-6413; Steimanis: 0000-0002-8550-4675; Vollan: 0000-0002-5592-4185

Supplementary materials are organized as follows: Section S1 offers a detailed account of our literature review. Section S3 provides regression outputs, additional analyses, and robustness checks. Section S4 presents the experimental treatment materials for the interested reader.

Table of Contents

Supplementary Figures:

Supplementary Tables:

S1. Literature review

There is much literature on the general stability of preferences (Chuang and Schechter 2015) and the effects of violent conflict on social preferences (Bauer et al. 2016). In the main body of the paper, we review the literature examining whether social preferences are affected by exposure to natural hazards. For the review provided, we focus on studies applying games that aim to measure prosociality (the dictator game, trust game, public good game, and ultimatum game) and compare participant outcomes of those exposed to natural hazards to those unexposed. Additionally, we summarize the outcomes of studies applying survey items (measures of trust, self-reported voluntary work, and prosociality index) instead of games to elicit prosociality. All studies reviewed have been published within the last 10 years and focus on the Asia-Pacific region, with only two exceptions from the Americas (Castillo and Carter 2011; Fleming, Chong, and Bejarano 2014) and one being older than 10 years (Castillo and Carter 2011).

In all studies, models with different specifications were tested. To increase comparability, we applied three selection criteria to the specification used to visualize the literature review (Figure 1). First, we used the results from most parsimonious specifications to keep variables included as similar as possible across studies. Second, we used the results from the model in which exposure was measured as a binary variable (exposed vs. not exposed). In one study (Vardy and Atkinson 2019), an index measuring property damage had to be used since effects were not reported for a binary exposure variable. Third, we rely on specifications where the outcome variables are measured as the share sent and returned, accepting values between zero and one. Hence, the results report the effect of exposure on share send, send back, or contributed. In one study (Veszteg et al. 2015), participants could not decide on the amount to be sent (back) but only whether to send/return money or not. For two studies, t-test values were used since the necessary statistics from the regression were not reported (Cassar et al. 2017; Veszteg et al. 2015).

The results from the literature summary of games are summarized in Figure 1 in the main body. The results from the literature summary of surveys are summarized in Section 1.4 below. There we provide more details on the background and methods of the studies.

1.1. Dictator games

Of the studies applying the dictator game, one in four found a significant effect of natural disasters on prosociality (Becchetti, Castriota, and Conzo 2017). Chantarat and colleagues (2019) played a non-incentivized dictator game with 256 participants from 32 rice-growing villages in 32 months after a megaflood hit in August 2011. Participants were asked how they would allocate 50 USD between themselves and another random household from the same village using 10 five-dollar bills. While living in a “flooded village” (15 days or more flooded, determined by satellite imagery) does not affect giving, individual exposure (rice fields being flooded for more than 15 days, self-reported survey item) affects giving positively. (Specification Table 7, Column 1 used for illustration).

Kuroishi and Sawada (2019) investigated the impact of a flood from August 2012 on prosocial behavior. They explored the effects of the flood on giving behavior among 161 participants from the Philippines at two different time points, two years (study 1) and six years (study 2) after the flood. Participants were asked to divide 1,000 PHP between themselves and another random villager using ten 100-PHP notes (in 2014 and 2018, 1,000 PHP equaled 22 USD and 19 USD, respectively). The study does not find a statistically significant relationship between individual-level flood exposure and giving behavior at the two- or six-year mark. (Specification Table 3, Column 1 and Table 4, Column 1 used for illustration).

Vardy and Atkinson (2019) conducted a modified dictator game both shortly before (June 2014) and after (June 2015) Cyclone Pam (March 2015) with 164 participants from two communities on the small volcanic island of Tanna, Vanuatu. The initial aim of the study was to compare the giving towards in-groups (coreligionists from other villages) vs. out-groups (persons from different religions or other villages). For this aim, the study was conducted in one predominantly Christian community and another following traditional culture and religious lifestyle. After the first data collection and the cyclone, the authors decided to add a second round of data collection. In the study, the authors report the results from the dictator game in which the participant allocates 500 Vanuatu Vatu using 10 coins (4.75 USD) between two cups: (i) self vs. coreligionists from another village, (ii) self vs. person from another village with a different religion, (iii) another person from the participant's village vs. a person from another village being a coreligionist, and (iv) a person from another village being a coreligionist vs. a person from another village with another religion. We found that setup (i) most closely resembled the rest of our studies since, in all other cases, prosociality towards others from the same local unit was measured. In our review, we therefore only considered this decision. While the authors state that participants kept more for themselves after the cyclone than they allocated to their coreligionists, the exact magnitude was not reported. The regression analysis displays the effects of the level of affectedness on giving. However, self-reported individual damage has no statistically significant effect on the distribution of coins between self and coreligionists from other villages. Similarly, no significant effect has been found in the other in-group setup in which funds were allocated between another person from the participant's village and a coreligionist from a different village (decision iii). For both out-group scenarios, property damage was associated with a reduced allocation toward out-groups (ii and iv). (Specification Table 1, Column 1 used for illustration).

The study by Becchetti and colleagues (2017) is the only one reviewed to find a significant effect of exposure on giving in the dictator game. The authors conducted a dictator game with 382 participants from three villages in Sri Lanka seven years after the 2004 Indian Ocean tsunami. For implementation, the authors cooperated with a local microfinance institution, using borrowers as participants (94% women). Participants assigned the sender role (N=191) were endowed with 900 LKR (8 USD), which they could allocate in intervals of 30 LKR between themselves and another random villager. Participants assigned the receiver role (N=191) did not allocate funds; they only received them. Exposure to natural hazards is a binary variable taking the value one if participants reported any damage due to the tsunami and taking zero otherwise. The authors find that participants reporting damage gave and expected to receive significantly less than those not reporting damage. Estimating the effect of each damage individually suggests that the effect is driven by economic damages, not injuries or household damages. Furthermore, receiving above-average help in the aftermath of the hazard neither increases giving nor expectations. (Specification Table 4, Column 1 used for illustration.)

1.2. <u>Trust games</u>

In our literature review, we report the effects of natural hazards on trust (money sent by the trustor) and trustworthiness (money sent back by the trustee). We include five studies determining the effect of exposure to natural hazards on trust and trustworthiness.

Cassar and colleagues (2017) collected data five years after the 2004 tsunami from 334 participants from 27 Thai villages, each playing the role of either trustor or trustee. Trustors received 200 Baht (6 USD) in 10 notes of 20 Baht and could decide how much to keep for themselves and how much to send to their partner. On average, participants living in affected villages sent and returned a significantly higher share than people from unaffected villages (mean comparison). However, in the regression analysis (ordered logit), effects were not robust if individual levels of affectedness were used and aid received was introduced. The latter is interpreted as prosociality being a result of aid received. T-tests reported in Table 4 reflect trust and trustworthiness ratio differences between affected and unaffected villages and the corresponding lower and upper confidence-interval boundaries.

One additional study finds trustworthiness positively affected by exposure, with trust unaffected (Veszteg, Funaki, and Tanaka 2015). This study was conducted shortly before (N=842) and after (N=288) the March 2011 Tohoku earthquake in Japan using an online survey. The natural hazard interrupted the data collection and was – as in the case of Vardy and Atkinson (2019) – ad hoc used as a natural variation. In their trust game, senders had to decide whether to send their hypothetical endowment of 500 Yen (6.5 USD) to someone else in the sample or not. The amount was multiplied by four and given to trustors. Trustors were then asked whether they would want to send back half (1000 Yen) or keep all (2000 Yen). Since participants taking part in the study before and after the earthquake were different individuals, the comparison is made between, and not within, subjects. The authors found that participants who experienced the earthquake (i.e., interviewed *after* the earthquake) did not differ in trust from participants who did not experience the earthquake (i.e., interviewed *before* the earthquake), but they did show higher trust. (Specification Table 2 was used for illustration.)

Fiala (2017) conducted a study 11 months after Cambodia's flooding in September 2014, using 202 participants from five different villages. Trustors were provided 6,000 Riel (1.5 USD). The author found that households affected by the floods (with the level of affectedness not specified) sent less, whether designated as trustors or trustees. However, the effect for share sent as trustee is significant only at the 10% significance level and therefore appears to be non-significant in Figure 1. (Specification Table 3.10, Column 5, and Column 7 were used for illustration.)

Fleming et al. (2014) analyze the effects of exposure to the Chilean earthquake of February 27, 2010, by comparing the behavior of 236 participants from 10 rural villages one year after the event. Five of these 10 villages were affected, and the other five were unaffected. Each participant was assigned either the role of trustor or trustee. Trustors received 6,000 CLP (14 USD) as 1,000 notes of 6 CLP each and were asked to allocate these between themselves and the trustee. The authors found that living in a village classified as affected had no statistically significant effect on the money sent but reduced the amount returned by 10%. (Specification Table 1, Column 1, and Column 2 were used for illustration.)

Lastly, one study found exposure to neither affect trust nor trustworthiness (Ahsan 2014). The study was conducted in February 2010, one year after Cyclone Aila, on May 25 and 26, 2009. It had 250 participants from 10 villages in Bangladesh. While the classification as affected versus unaffected was not explicitly described, self-reported items from the survey were supposedly used to classify participants. Each participant first played as a trustor, then as a trustee. Trustors were endowed with 200 Taka (3 USD), which they could divide by increments of fifty between themselves and an anonymous trustee. (Specification Tables 4 and 6 were used for illustration.)

1.3. Other Games

One public good game and one ultimatum game were conducted to elicit social preferences. The public good game was conducted in April 2013, three years after a flood in Pakistan, using 384 participants from 16 villages (Afzal, Turner, and Said 2015). Exposure was defined at the village level based on self-reported answers from a survey of 30,000 households from the corresponding region conducted by the regional statistical office. Based on this classification, eight affected and eight unaffected villages were randomly chosen for data collection. For the public good game, participants played in groups of four. Each player was endowed with 100 Pakistan Rupees (1 USD). Contributions to the public good were doubled, pooled, and distributed equally among all players. The authors found that living in a village that experienced the 2010 floods did not increase the contribution, while the total number of floods experienced and only experiencing the 2010 flood (effect used) significantly affected the contribution. However, for those who experienced the 2010 flood, an increase in experiencing other floods negatively affected the contribution. The authors interpret these findings as evidence that "severe experience with a natural disaster negatively affects social capital, whereas frequent experiences with milder floods have a positive effect." This interpretation aligns with the findings from Castillo and Carter (2011). (Specification Table 3, Column 5 was used for illustration.)

The ultimatum game was conducted in 2014, 18 months after the 2012 Typhoon Bopha, using 100 participants from four villages from Siquijor Island, Philippines (Abatayo and Lynham 2020). The island was chosen as only one side (east) was affected by the typhoon while the other (west) was not, and it provided an otherwise homogenous sample pool. Fifty of the 100 participants were randomly assigned the sender role, while the other 50 were assigned the receiver role. Senders were endowed with 100 PHP (2 USD) that they had to allocate between themselves and their randomly matched, anonymous receiver. The receiver, in turn, could decide whether to accept or decline the offer. The authors found that being affected by the typhoon did not affect the offer made. As only four offers were rejected by receivers, these were not further analyzed.

1.4. Surveys

Beyond the studies that use games to elicit prosociality, some studies rely on survey data. We summarize the findings of these survey studies below in Table S1. Contrary to the studies above applying games with outcome measures ranging from zero (send/return/contribute nothing) to one (send/return/contribute all), the survey studies apply different measures with varying scales. Since not all studies report the necessary measures to standardize effects, we summarize the findings in a table indicating whether the study found a positive, negative, or zero effect of exposure on prosociality.

Table S1. *Natural hazards and prosociality across survey studies*

Study	-	0	+	Effect size with 95 CI	Country	DIS	ΔMonths	N	Measure
Secondary Data									
Rahman et al. (2020)				-0.13 [-0.13, -0.13]	BGD	FL	48	2,932	GT
Lee (2021)				-0.01 [-0.05, 0.02]	CHN	MULTI	N/A	5,819	GT
Calo-Blanco et al. (2017)				0.11 [-0.25, 0.47]	CHL	EQ	6	227	GT
Ahmad and Younas (2021)				0.09 [0.05, 0.14]	PAK	FL	23-36	6,953	RET
Bai and Li (2021)				0.05 [0.02, 0.09]	IDN	EQ	N/A	40,569	VOL
Yamamura (2014)				0.01 [N/A]	JPN	EQ	18	448,223	VOL
Primary Data									
Biener and Landmann (2023)				-0.12 [-0.21, -0.02]	PHL	WI	0	2,352	PS
Andrabi and Das (2017)				-0.001 [-0.003, 0.001]	PAK	EQ	48	4,610	RET
Maki et al. (2019)				0.05* [N/A]	CHL	EQ	7	2033	PS
Shupp et al. (2017)				0.66 [N/A]	USA	WI	3.5	259	GT

Effect of exposure on prosociality

Note: *t-test applied. This table summarizes the relationship between natural disasters and prosociality across survey studies (negative effect: orange; no effect: grey; positive effect: green). The column Country indicates the country where the study was conducted (BGD: Bangladesh; CHN: China; CHL: Chile; PAK: Pakistan; IDN: Indonesia; JPN: Japan; PHL: Philippines; USA: United States of America), DIS indicates the disaster investigated (Multi: Multiple disaster considered; WI: Heavy wind; FL: Flood; EQ: Earthquake), Δmonths represents the months passed between disaster occurrence and data collection, N the sample size, and Measure the type of measurement for prosociality (ST: Social Trust; GT: Generalized Trust; RET: Returning money/wallet lost; VOL: Voluntary work; PS: Prosociality measured by index over number of items). All studies but one (Calo-Blanco 2017) analyze changes at the individual level.

In these studies relying on survey measures, prosociality is measured either as generalized trust (Calo-Blanco et al. 2017; Lee 2021; Rahman et al. 2020; Shupp et al. 2017), trust that a lost wallet or money would be returned (Ahmad and Younas 2021; Andrabi and Das 2017), an index formed across several items measuring prosociality (Biener and Landmann 2023; Maki et al. 2019), or self-reported participation in voluntary community work (Bai and Li 2021; Yamamura 2016).

The first strand of studies relying on survey data combines secondary data on prosociality with secondary data on exposure. Data on prosociality and exposure was retrieved from national or supra-national survey organizations like the World Value Survey and the EM-DAT international database. Lower levels of prosociality in exposed regions were found in one study (Rahman et al. 2020), higher levels in three studies (Ahmad and Younas 2021; Bai and Li 2021; Yamamura 2016), and no effect in two studies (Calo-Blanco et al. 2017; Lee 2021).

In the study conducted by Rahman et al. (2020), flood data and data from the world value survey of 2,932 participants were combined, both before and after the 1998 flood in Bangladesh. Prosociality was measured by the general trust measure: “Generally speaking, would you say that most people can be trusted or that you can’t be too careful in dealing with people?” with binary answer categories (yes/no). The authors found that households living in divisions affected by the floods showed lower levels of trust.

Ahmad and Younas (2021) combine flood data with data from a nationally representative survey 23 and 36 months after the flood of 2010-2011 in Pakistan. The question asked was: “Presume you are walking down the street and you drop 100 Rupees. How likely is it that the following people would return the money to you?” with a Likert scale from one (very likely) to four (will never happen). Participants were asked about their trust in having the money returned by neighbors, family, fellow Pakistanis, and strangers. The best-suited specification for our literature comparison is the one using neighbors. The authors found that exposure (measured as the share of the population affected by the floods) is positively associated with trust in neighbors (as well as in fellow Pakistanis and foreigners).

Bai and Li (2021) combine earthquake data with data from a nationally representative survey from before and after the earthquakes occurring between 2004 and 2007 in Indonesia. Affectedness (binary and continuous) is based on data from the US Geological Survey (PGA: Peak Ground Acceleration being the amplitude of the highest acceleration record on an accelerogram during an earthquake at a specific location). The survey asked different questions regarding pro-sociality on the individual level. Regarding volunteering: “During the last 12 months, did you participate in voluntary labor (e.g., cleaning up the village)?” Regarding political participation: “Did you vote in the 1999 general election?” Regarding microfinance program participation: “Have you participated in an arisan in the last 12 months?”

The specification regarding volunteering and a binary exposure variable matches the other studies compared best. The authors found that exposure, combined with answering the survey after the earthquake phase, had a significant positive effect on volunteering. This result appears robust even when employing other measures of pro-sociality and affectedness. Using the non-binary exposure measure together with the squared term of the exposure measure suggests a non-linear, inverted-U shape relationship. This is interpreted as potentially explained by the “additional post-disaster aid provided by the government to particularly shocked areas.”

Yamamura (2016) uses secondary data from a large survey (Japanese national survey, STULA, conducted every five years) to determine the effect of the 1995 earthquake on prosociality. The dataset contains information on participation in voluntary community building work before the disaster in 1991 and after it in 1996, using more than 400,000 participants. These individual-level data are matched with measures of the person's distance from the earthquake epicenter (inverse). The authors regress volunteering on an interaction term of a post-disaster dummy with the inversed distance from the epicenter and find that after the disaster, being closer to the epicenter positively affects volunteering.

Calo-Blanco and colleagues (2017) combine secondary data on pro-sociality (Latinobarometro) with regional- and comuna-level data on exposure. Among the measures of pro-sociality is generalized trust (others can be trusted/cannot be too careful), measured before and six months after the earthquake. This data is available for individuals from 98 comunas. Trust is averaged on the comuna-level. Comunas are classified as exposed if (i) they were identified by the seismological service as a comuna hit by the 2010 Maule earthquake (Mercalli scale>VI), (ii) they suffered at least one fatal victim, or (iii) they requested economic aid. The difference in trust before and after the event of exposure is compared to the difference in trust of those unexposed. This difference-in-difference comparison suggests that exposure does not affect trust. Lastly, Lee (2021) uses secondary data to determine the effect of multiple natural disasters on general trust among 5,819 Chinese individuals with data from the Chinese General Social Survey. The analysis finds no significant relationship between natural hazards experienced and generalized trust.

Beyond these studies using secondary data, four studies use primary survey data to analyze the effect of exposure to environmental hazards on prosociality (Andrabi and Das 2017; Biener and Landmann 2023; Maki et al. 2019; Shupp et al. 2017). Andrabi and Das (2017) implemented a survey from 2009 to 2010 after the 2005 Earthquake in Pakistan using 4,672 participants from 126 villages between 1 and 75 km from the fault line. The survey question about trust asked, "Imagine you are walking down a street and drop a 1000 rupee-note without noticing. [Name] was walking behind you without you knowing and picked it up. Would they return the money to you?'' In this, [Name], a name representing being either (a) people in general; (b) your extended family; (c) people in your village; (d) people in your qaum/caste/clan/biradari (qaum translates roughly as clan and biradari as the kinship group); (e) people in your region; (f) other Pakistanis; (g) general foreigners; (h) Europeans/Americans; or (i) Islamic foreigners. The different names are summarized to represent foreigners, westerners, and people from their own region. For Table S1 we use results from the specification "own region" since it fits the other studies best. The authors found that the distance from the fault line did not significantly affect trust toward people from their own region (-0.001, SD=0.001).

Biener & Landmann (2023) conducted a survey before and after the 2013 Typhoon Haiyan using 2,352 participants from the Philippines. Reciprocity was measured by eight items. Dimensionality was reduced to one measure by principal component analysis. The authors find that reciprocity is reduced after the typhoon compared to before.

Maki and colleagues (2019) collected data in Chile before (only Santiago de Chile, N=644) and after the 2010 Chilean earthquake (six cities, N=1,389) on prosocial values (index over five items), helping motivations, and prosocial behaviors. Prosocial values of those interviewed after did not differ from those interviewed before the earthquake. However, those surveyed after the earthquake more strongly endorsed most of the helping motivations: religious motivations, self-enhancement motivations, selfprotective motivations, and social motivations were all significantly higher, while career motivations showed no difference.

Lastly, Shupp et al. (2017) conducted surveys in one place affected by a Tornado in 2013 (Moore, N=195) and one comparable place unaffected (Yukon, N=64) to compare whether general trust changed. Living in the impacted area was found to be associated with an increase in trust ($p<.1$).

Adding to the results reported above, Kang and Skidmore (2018) and Toya and Skidmore (2014) found that different types of natural disasters affect trust differently. The first authors find that while heavy rain, snow, and winds in South Korea have significant positive effects on trust, typhoons are associated with lower trust. In contrast, the latter study suggests that the number of floods, earthquakes, mass movements, and volcanic eruptions experienced does not affect trust, while only the number of storms experienced is associated with increased trust.

S2. Additional analysis and robustness checks

2.1. Selectivity of out-migration

In 2016 we collected information on the out-migration of villagers with the help of village officials. We obtained documents that provided information on whether people had moved away temporarily or permanently in the period between the two survey waves in the summers of 2012 and 2016. Additively, we obtained information on whether study participants had moved in the same period. Five of our participants migrated away from their villages after 2012, with four returning before 2016. We find no systematic relation between out-migration and reversed distance (correlation r=-0.16, p=0.45).

Table S2. *Out-migration (either permanent or temporal), according to official documents retrieved from village officials*

Village	Municipality	Reversed distance	out-migration	population (2007 census)	% of pop. out-migrating
Sagua	Anini-y	0	5	1057	0.5%
Lisub-A	Anini-y	1.54	13	729	1.8%
Igdalaguit	Tobias Fornier	6.42	0	1144	0.0%
Sinogbuhan	San Joaquin	6.88	0	1604	0.0%
Paciencia	Tobias Fornier	7.83	0	1018	0.0%
Igcondao	San Joaquin	12.1	16	445	3.6%
Cata-An	San Joaquin	13.67	5	1230	0.4%
Santa Rita	San Joaquin	19.41	0	1601	0.0%
Bucaya	San Joaquin	20.72	15	1648	0.9%
Tapikan	San Joaquin	24.37	0	317	0.0%
Igcawayan	San Lorenzo	27.32	3	1081	0.3%
Baras	Guimbal	32.86	0	1017	0.0%
Calampitao	Guimbal	32.91	0	691	0.0%
Suclaran	San Lorenzo	36.47	5	1662	0.3%
Paloc Bique	Dumangas	61.52	0	1017	0.0%
Nanding Lopez	Dumangas	63.85	2	1315	0.2%
Pajo	Libertad	89.84	0	538	0.0%
Bulanao	Libertad	90.21	1	327	0.3%
Cubay	Libertad	90.4	2	830	0.2%
Maramig	Libertad	90.54	0	328	0.0%
San Roque	Libertad	90.76	4	1028	0.4%
Igcagay	Libertad	90.89	18	539	3.3%
Paz	Libertad	91.01	0	644	0.0%
Pucio	Libertad	96.1	0	539	0.0%
Dungon	Concepcion	101.36	0	476	0.0%
Maliogliog	Concepcion	102.66	0	517	0.0%
Batonan-Sur	Culasi	103.62	11	663	1.7%
Balac-Balac	Culasi	105.84	5	668	0.7%
Polopina	Concepcion	108.96	0	3382	0.0%
Talotoan	Concepcion	119.07	2	2470	0.1%

Notes: Total number of households moving away from the village, either permanently or temporarily (out-migration), and in percent of the total village population according to the 2007 census (population 2007 census) between 2012 and 2016. Numbers from documents received from the corresponding village official.

2.2. Pseudo-treatment

Another potential concern is that transfers and expectations concerning the path of Haiyan may already have differed systematically before Haiyan happened in 2012. Although Panay is generally unaffected by strong tropical storms (relatively low background risk), due to unobserved factors, solidarity preferences could already have differed before Haiyan along the distance from the eye of the storm. Table S3 shows that pre-Haiyan transfers do not significantly differ in relation to the path of Haiyan. This holds when we control for transfer expectations alone and additionally for socio-economic characteristics using a multivariate linear regression model; the reversed distance is insignificant in explaining differences in pre-Haiyan transfers (see Supplementary). This gives us confidence about the exogeneity of the impact of Haiyan in terms of our primary outcome of interest, solidarity transfers.

Table S3. *Pseudo-treatment effects of Haiyan*

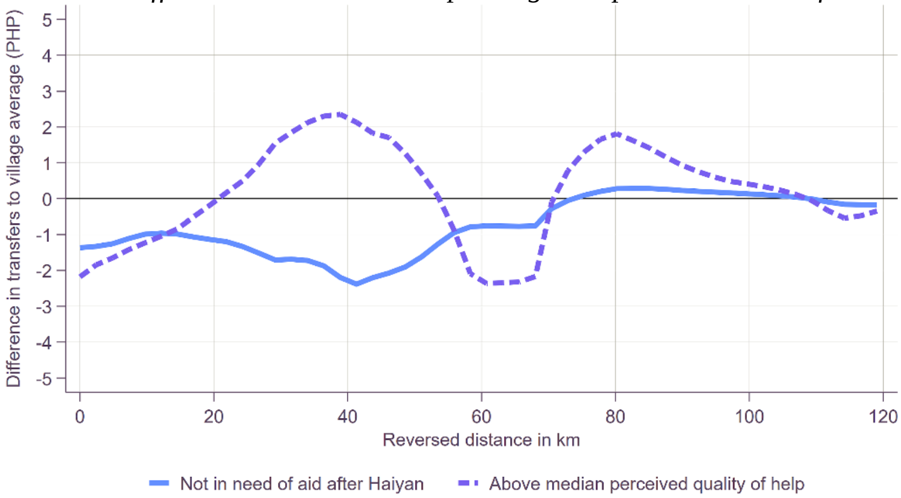
	Average transfers 2012		
	(1)	(2)	(3)
Reversed distance	0.019	0.088	0.090
	(0.14)	(0.08)	(0.09)
Reversed distance^2	-0.001	-0.001	-0.001
	(0.00)	(0.00)	(0.00)
Expected transfer (0,70)		0.521***	0.523***
		(0.04)	(0.04)
Age (years)			0.150**
			(0.06)
Female (=1)			0.354
			(2.51)
Household head (=1)			0.132
			(2.41)
Single (=1)			1.203
			(2.03)
Education: Elementary (=1)			-2.418
			(1.76)
Trust in national government			-0.718
			(0.54)
Wealth index (PCA)			0.234
			(0.63)
Constant	35.329***	19.416***	15.780***
	(2.59)	(2.26)	(4.96)
N	450	450	450
Cluster	30	30	30
Adjusted R-squared	0.01	0.36	0.36
F-Test: distance & distance^2	0.962	1.551	1.174

Notes: The dependent variable in all models is the solidarity transfers of 2012. Robust standard errors clustered at the village level with 95% confidence intervals in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.3. Baseline differences depending on post-Haiyan experiences

Exploring heterogeneous effects conditional on people's post-disaster experiences would be of concern if affected people already differed in their degree of solidarity in 2012. Our panel data allows us to test whether these social dynamics might be endogenous to baseline prosociality. In Figure S1, we plot the individual differences in transfers against average transfers in the community in 2012 for (i) people who reported needing aid after Haiyan and (ii) people with above-median perceived quality of help. On average, respondents who needed aid transferred almost 3 PHP less than respondents who did not need aid (t-test diff.=2.76, t_{445}=4.06 p<0.01). Regarding the perceived quality of help, there are no significant differences between those with above- and below-median perceptions (t-test diff.=.73, t_{448}=1.19 p=0.23). However, the figure demonstrates a complex relationship between baseline solidarity, post-disaster neediness, and perceived quality of help with increasing distance from Haiyan.

Figure S1. *Different baseline trends depending on experiences made after Haiyan*



Notes: Plotted are average differences in individual transfers to the village for respondents who needed aid (solid blue line) and those with above-median recovery experiences (dashed line). Lines are from a kernel-weighted local polynomial regression (kernel = Epanechnikov, degree = 0, bandwidth =10).

2.4. Main results: Effect of Haiyan on changes in solidarity transfers

Table S4. *Effect of Haiyan on solidarity transfers (Table 3)*

	Δ Average transfers		
	(1)	(2)	(3)
Reversed distance	-0.230***	-0.287***	-0.296***
	(0.079)	(0.067)	(0.067)
Reversed distance^2	0.002**	0.002***	0.002***
	(0.001)	(0.001)	(0.001)
2012 Solidarity transfer (0,70)	-0.870***	-0.659***	-0.662***
	(0.070)	(0.065)	(0.066)
Δ Expected Transfer		0.367***	0.370***
		(0.035)	(0.035)
Δ Trust national government			-1.258**
			(0.582)
Δ Wealth Index (PCA)			-0.583
			(0.690)
Age (years)			-0.015
			(0.065)
Female (=1)			-1.991
			(2.409)
Household head (=1)			-0.237
			(2.027)
Single (=1)			-3.884
			(3.631)
Education: Elementary (=1)			-1.133
			(1.800)
Constant	29.857***	23.762***	26.549***
	(2.754)	(2.464)	(4.537)
N	450	450	450
Cluster	30	30	30
Adjusted R-squared	0.394	0.538	0.538
F-test: Distance & distance^2	6.627***	13.192***	12.642***

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table S5. *Heterogeneous effects dependent on pre-Haiyan changes in transfers*

	Δ Average transfers							
	Men	Women	Low SES	High SES	Age (<42)	Age (≥42)	Only elementary	At least high school
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reversed distance	-0.255**	-0.316**	-0.322***	-0.336***	-0.262**	-0.282***	-0.558***	-0.252***
	(0.123)	(0.120)	(0.106)	(0.100)	(0.122)	(0.079)	(0.145)	(0.085)
Reversed distance^2	0.002*	0.002**	0.002**	0.003***	0.002*	0.002***	0.004***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2012 Average Transfer (0,70)	-0.745***	-0.628***	-0.850***	-0.480***	-0.627***	-0.690***	-0.849***	-0.613***
	(0.114)	(0.069)	(0.104)	(0.082)	(0.115)	(0.069)	(0.118)	(0.071)
Δ Expected Transfer	0.275***	0.416***	0.302***	0.407***	0.391***	0.346***	0.298***	0.386***
	(0.065)	(0.046)	(0.053)	(0.042)	(0.055)	(0.053)	(0.066)	(0.034)
Δ Trust national government	-2.106*	-1.133	-2.404***	-0.239	-0.594	-1.677**	-1.091	-1.002
	(1.136)	(0.681)	(0.784)	(0.791)	(0.989)	(0.787)	(1.251)	(0.737)
Δ Wealth Index (PCA)	-1.434	0.256	-1.445	0.332	-0.896	-0.072	-1.395	-0.164
	(1.250)	(0.889)	(1.203)	(0.940)	(0.806)	(0.894)	(1.248)	(0.691)
Age (years)	0.188*	-0.124	0.002	-0.048	0.094	0.194	0.061	-0.000
	(0.103)	(0.097)	(0.120)	(0.085)	(0.170)	(0.147)	(0.134)	(0.072)
Female (=1)	0.000	0.000	-4.874	1.805	7.698***	-7.985**	-10.997***	1.392
	(.)	(.)	(3.230)	(2.439)	(2.639)	(3.055)	(3.159)	(2.759)
Household head (=1)	8.557	-0.047	-4.837*	4.750*	6.988**	-4.894**	-4.742	1.608
	(8.285)	(2.047)	(2.794)	(2.578)	(2.676)	(2.203)	(3.765)	(2.398)
Single (=1)	2.266	-8.646	2.921	-8.298*	-5.189	-1.395	3.584	-10.294**
	(5.144)	(5.778)	(4.846)	(4.252)	(5.916)	(4.248)	(6.018)	(4.067)
Education: Elementary (=1)	0.595	-2.253	-2.733	1.222	0.217	-2.715	-1.651	-0.078
	(2.371)	(2.287)	(2.290)	(2.231)	(2.758)	(1.887)	(5.553)	(2.386)
Constant	8.775	29.356***	38.194***	16.443***	12.137	21.834**	43.708***	19.868***
	(8.556)	(4.892)	(6.965)	(4.827)	(9.682)	(9.295)	(9.186)	(4.839)
N	177	273	224	226	209	241	120	330
Cluster	30	30	30	30	30	30	26	30
Adjusted R^2	0.51	0.57	0.59	0.52	0.53	0.56	0.60	0.53
F-Test: Distance & distance^2	2.275	5.796***	5.639***	6.363***	3.178*	8.027***	10.484***	5.051**

Notes: Robust standard errors are clustered at the village level with 95% confidence intervals in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table S6. *Heterogeneous effects on transfers dependent on post-disaster social dynamics*

	Δ Average transfers					
	Not need aid	Needed Aid	Low-quality help	High-quality help	Equal or better off	Worse off
	(1)	(2)	(3)	(4)	(5)	(6)
Reversed distance	-0.179	-0.478***	-0.166*	-0.420***	-0.270***	-0.352***
	(0.156)	(0.085)	(0.092)	(0.092)	(0.087)	(0.095)
Reversed distance^2	0.002	0.004***	0.001	0.003***	0.002**	0.003***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2012 Solidarity transfer (0,70)	-0.552***	-0.738***	-0.612***	-0.692***	-0.704***	-0.613***
	(0.111)	(0.071)	(0.111)	(0.077)	(0.082)	(0.092)
Δ Expected Transfer	0.341***	0.351***	0.419***	0.336***	0.362***	0.376***
	(0.073)	(0.048)	(0.052)	(0.047)	(0.053)	(0.044)
Δ Trust national government	-0.546	-1.150	-1.412	-1.240	-1.297*	-1.259
	(1.831)	(0.738)	(0.904)	(0.880)	(0.669)	(0.903)
Δ Wealth Index (PCA)	0.944	-1.147	-1.342	0.097	-1.329	-0.003
	(1.055)	(0.815)	(0.955)	(0.987)	(1.422)	(1.642)
Age (years)	-0.061	0.054	-0.078	0.079	0.053	-0.052
	(0.117)	(0.083)	(0.086)	(0.105)	(0.095)	(0.104)
Female (=1)	0.004	-3.516	1.550	-5.468*	-0.761	-2.755
	(3.484)	(2.570)	(3.898)	(2.859)	(3.208)	(3.251)
Household head (=1)	3.098	-2.014	-0.089	-0.333	1.087	-0.988
	(3.318)	(2.159)	(2.641)	(2.937)	(2.984)	(3.127)
Single (=1)	-11.676	-0.932	-1.578	-5.094	3.659	-9.562**
	(6.858)	(4.107)	(4.333)	(6.030)	(5.030)	(4.041)
Education: Elementary (=1)	5.265*	-3.600**	3.088	-4.341*	-2.481	0.406
	(2.888)	(1.742)	(2.743)	(2.270)	(1.795)	(2.786)
Constant	17.122***	33.362***	23.139***	28.386***	25.172***	27.403***
	(5.385)	(5.418)	(6.477)	(6.166)	(6.213)	(6.998)
N	127	320	228	222	234	216
Cluster	25	29	30	30	30	30
Adjusted R-squared	0.51	0.57	0.56	0.52	0.53	0.54
F-Test: Distance & distance^2	1.039	17.554***	2.966*	11.879***	10.545***	7.399***

Notes: Robust standard errors clustered at the village level in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table S7. *Post-disaster experience PCA*

	Eigenvalue	Difference	Proportion	Cumulative
Comp1	2.73	0.97	0.27	0.27
Comp2	1.77	0.33	0.18	0.45
Comp3	1.43	0.46	0.14	0.59
Comp4	0.98	0.13	0.10	0.69
Comp5	0.85	0.18	0.09	0.78
Comp6	0.67	0.13	0.07	0.84
Comp7	0.54	0.15	0.05	0.90
Comp8	0.39	0.05	0.04	0.94
Comp9	0.34	0.05	0.03	0.97
Comp10	0.30	.	0.03	1.00

Table S8. *Loadings of first component and unexplained variation of each item*

	Comp1	Unexplained
Distribution of aid was fair.	0.43	.57
Amount of aid was sufficient.	0.48	.52
Aid was well organized.	0.43	.57
Unexpected help from others.	0.30	.70
Tried to help wherever possible.	0.09	.91
Feel closer to people now than before.	0.16	.84
People were acting selfish (R).	-0.31	.69
Some received more than needed (R).	-0.35	.65
Conflict over lack of aid (R).	-0.19	.81
Felt left alone (R).	-0.13	.87

Table S9. *U-shape perceived quality of help*

	(1)	(2)
Reversed distance	-0.026***	-0.024**
	(0.009)	(0.009)
Reversed distance^2	0.000***	0.000***
	(0.000)	(0.000)
Δ Trust national government		0.048
		(0.057)
Δ Wealth Index (PCA)		-0.007
		(0.065)
Age (years)		-0.012*
		(0.006)
Female (=1)		0.134
		(0.176)
Household head (=1)		0.098
		(0.171)
Single (=1)		-0.079
		(0.394)
Education: Elementary (=1)		-0.240
		(0.187)
Constant	0.242	0.720**
	(0.184)	(0.303)
N	445	445
Cluster	30	30
Adjusted R-squared	0.02	0.01
F-Test: Distance & distance	7.985***	7.281**

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table S10. *Long-term effects on reciprocal help*

	Positive reciprocity in SD	
	(1)	(2)
Reversed distance	-0.007	-0.008
	(0.005)	(0.005)
Reversed distance^2	0.000*	0.000**
	(0.000)	(0.000)
Δ Trust national government		-0.047
		(0.040)
Δ Wealth Index (PCA)		0.025
		(0.040)
Age (years)		0.004
		(0.005)
Female (=1)		0.351**
		(0.144)
Household head (=1)		0.001
		(0.130)
Single (=1)		-0.032
		(0.288)
Education: Elementary (=1)		0.062
		(0.114)
Constant	-0.003	-0.336
	(0.125)	(0.445)
N	330	330
Cluster	30	30
Adjusted R-squared	0.01	0.02
F-Test: Distance & distance^2	3.322*	3.904**

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.5. Robustness checks: Changes in solidarity transfers

Table S11. *Effects of Haiyan without baseline and full 2016 sample*

	Δ Average transfers		
	(1)	(2)	(3)
Reversed distance	-0.231***	-0.172***	-0.178***
	(0.064)	(0.049)	(0.053)
Reversed distance^2	0.001**	0.001***	0.001***
	(0.001)	(0.000)	(0.000)
Expected transfer (0,70)		0.506***	0.508***
		(0.031)	(0.031)
Age (years)			-0.019
			(0.049)
Female (=1)			-0.842
			(1.315)
Household head (=1)			-0.426
			(0.909)
Single (=1)			2.170
			(2.436)
Education: Elementary (=1)			-2.345*
			(1.218)
Trust in national government			-0.069
			(0.569)
Wealth index (PCA)			-0.303
			(0.512)
Constant	35.493***	20.001***	22.361***
	(1.045)	(1.256)	(3.123)
N	810	810	804
Cluster	30	30	30
Adjusted R-squared	0.02	0.36	0.36

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table S12. *Solidarity Transfers: Excluding the two villages that experienced medium damages*

	Δ Average transfers		
	(1)	(2)	(3)
Reversed distance	-0.189	-0.237***	-0.243***
	(0.114)	(0.085)	(0.081)
Reversed distance^2	0.001	0.002**	0.002**
	(0.001)	(0.001)	(0.001)
2012 Solidarity transfer (0,70)	-0.861***	-0.647***	-0.649***
	(0.074)	(0.067)	(0.069)
Δ Expected Transfer		0.374***	0.377***
		(0.037)	(0.036)
Δ Trust national government			-1.290*
			(0.647)
Δ Wealth Index (PCA)			-0.503
			(0.740)
Age (years)			-0.042
			(0.067)
Female (=1)			-1.004
			(2.364)
Household head (=1)			0.160
			(1.909)
Single (=1)			-4.629
			(3.970)
Education: Elementary (=1)			-1.205
			(1.984)
Constant	29.171***	22.894***	26.057***
	(2.926)	(2.544)	(4.938)
N	412	412	412
Cluster	28	28	28
Adjusted R-squared	0.38	0.53	0.53

Notes: Thirty-eight observations from the two medium-affected villages have been excluded from the analysis. Robust standard errors are clustered at the village in brackets: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table S13. *Solidarity Transfers: Affectedness index instead of distance*

	Δ Average transfers		
	(1)	(2)	(3)
Affectedness index (PCA)	-2.276**	-2.052**	-1.908*
	(1.043)	(0.948)	(0.973)
Affectedness index^2	1.401	1.559*	1.546*
	(0.995)	(0.864)	(0.856)
2012 Solidarity transfer (0,70)	-0.874***	-0.667***	-0.670***
	(0.070)	(0.066)	(0.067)
Δ Expected Transfer		0.363***	0.365***
		(0.035)	(0.035)
Δ Trust national government			-1.168*
			(0.584)
Δ Wealth Index (PCA)			-0.409
			(0.665)
Age (years)			-0.021
			(0.067)
Female (=1)			-1.756
			(2.503)
Household head (=1)			-0.120
			(2.202)
Single (=1)			-3.775
			(3.680)
Education: Elementary (=1)			-1.429
			(1.813)
Constant	23.226***	16.598***	19.474***
	(2.498)	(2.385)	(4.518)
N	450	450	450
Cluster	30	30	30
Adjusted R-squared	0.39	0.53	0.53

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table S14. *Solidarity Transfers: Tobit model to account for censoring*

	Δ Average transfers		
	(1)	(2)	(3)
Reversed distance	-0.226***	-0.285***	-0.294***
	(0.080)	(0.067)	(0.066)
Reversed distance^2	0.002**	0.002***	0.002***
	(0.001)	(0.001)	(0.001)
2012 Solidarity transfer (0,70)	-0.878***	-0.665***	-0.668***
	(0.070)	(0.065)	(0.066)
Δ Expected Transfer		0.369***	0.372***
		(0.035)	(0.035)
Δ Trust national government			-1.277**
			(0.583)
Δ Wealth Index (PCA)			-0.588
			(0.682)
Age (years)			-0.012
			(0.065)
Female (=1)			-1.924
			(2.417)
Household head (=1)			-0.152
			(2.038)
Single (=1)			-3.836
			(3.612)
Education: Elementary (=1)			-1.135
			(1.808)
Constant	30.015***	23.857***	26.441***
	(2.730)	(2.448)	(4.507)
N	450	450	450
Cluster	30	30	30
Pseudo R-squared	0.06	0.09	0.09

Notes: Tobit regression model accounting for the lower (-70) and upper (70) bound of the changes in solidarity transfers. Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table S15. *Survey-based index of helping post-Haiyan (PCA)*

	Quality internal help (PCA)	
	(1)	(2)
Reversed distance	-0.025***	-0.023***
	(0.008)	(0.006)
Reversed distance^2	0.000***	0.000***
	(0.000)	(0.000)
Δ Trust national government		0.033
		(0.050)
Δ Wealth Index (PCA)		-0.060*
		(0.032)
Age (years)		-0.003
		(0.004)
Female (=1)		-0.027
		(0.119)
Household head (=1)		-0.009
		(0.123)
Single (=1)		-0.431*
		(0.240)
Education: Elementary (=1)		-0.439***
		(0.153)
Constant	0.440***	0.742***
	(0.105)	(0.236)
N	447	447
Cluster	30	30
Pseudo R-squared	0.03	0.05

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.6. Effect of Haiyan on changes in expected solidarity transfers

Table S16. *Effect of Haiyan on solidarity expectations*

	Δ Transfer expectation		
	(1)	(2)	(3)
Reversed distance	0.146	0.033	0.019
	(0.107)	(0.120)	(0.127)
Reversed distance^2	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
2012 Expected transfer (0,70)		-0.853***	-0.854***
		(0.052)	(0.054)
Δ Trust national government			-0.661
			(0.843)
Δ Wealth Index (PCA)			-0.581
			(0.809)
Age (years)			0.085
			(0.111)
Female (=1)			3.525
			(2.790)
Household head (=1)			3.365
			(2.910)
Single (=1)			-3.037
			(3.740)
Education: Elementary (=1)			-0.141
			(1.922)
Constant	-3.704	22.344***	14.970**
	(2.601)	(2.561)	(6.106)
N	450	450	450
Cluster	30	30	30
Adjusted R-squared	0.00	0.39	0.39

Notes: Robust standard errors are clustered at the village level in brackets: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table S17. *Heterogeneous effects dependent on pre-Haiyan characteristics on changes in expected transfers*

	Δ Transfer expectation							
	(1) Men	(2) Women	(3) Low SES	(4) High SES	(5) Age (<42)	(6) Age (≥42)	(7) Only elementary	(8) At least high school
Reversed distance	0.016	0.023	-0.076	0.104	0.031	0.021	-0.392**	0.163
	(0.177)	(0.136)	(0.162)	(0.134)	(0.149)	(0.242)	(0.186)	(0.120)
Reversed distance^2	-0.000	-0.000	0.000	-0.001	-0.000	-0.000	0.003**	-0.002
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
2012 Expected transfer (0,70)	-0.836***	-0.868***	-0.911***	-0.792***	-0.866***	-0.857***	-0.893***	-0.837***
	(0.062)	(0.093)	(0.064)	(0.088)	(0.090)	(0.069)	(0.117)	(0.071)
Δ Trust national government	-3.062**	0.667	-0.395	-0.806	-1.152	-0.340	-1.651	-0.184
	(1.330)	(1.120)	(1.496)	(1.415)	(1.017)	(1.385)	(2.023)	(0.919)
Δ Wealth Index (PCA)	-0.172	-0.940	-2.020	0.225	-1.082	-0.050	-1.000	-0.393
	(1.109)	(1.174)	(1.410)	(1.083)	(1.313)	(1.174)	(1.711)	(1.027)
Age (years)	0.303*	-0.063	0.152	0.050	0.097	0.079	0.141	0.080
	(0.172)	(0.136)	(0.153)	(0.142)	(0.225)	(0.185)	(0.239)	(0.102)
Female (=1)	0.000	0.000	-0.214	7.346**	9.534**	-0.345	-0.647	5.375
	(.)	(.)	(4.354)	(3.197)	(3.643)	(4.089)	(4.723)	(3.400)
Household head (=1)	7.216	4.583	-1.096	8.179**	7.815**	1.024	1.975	4.573
	(8.950)	(3.218)	(4.663)	(3.153)	(3.269)	(4.056)	(5.161)	(3.000)
Single (=1)	4.714	-11.690**	5.894	-5.554	-7.137*	-0.824	1.017	-6.154
	(5.808)	(5.091)	(9.893)	(4.770)	(3.921)	(5.468)	(5.768)	(5.699)
Education: Elementary (=1)	1.924	-1.269	-2.553	2.188	-1.903	1.061	-2.885	0.389
	(2.896)	(3.050)	(2.840)	(3.161)	(3.057)	(3.091)	(6.657)	(3.645)
Constant	0.130	25.612***	22.284***	7.505	8.626	19.298	24.063*	11.351
	(9.360)	(7.066)	(6.959)	(8.774)	(10.297)	(12.369)	(12.835)	(7.114)
N	177	273	224	226	209	241	120	330
Cluster	30	30	30	30	30	30	26	30
Adjusted R-squared	0.41	0.38	0.42	0.35	0.41	0.35	0.36	0.39

Notes: Robust standard errors are clustered at the village level in brackets: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table S18. *Heterogeneous effects on expectations dependent on post-disaster social dynamics*

	Δ Transfer expectation					
	Not need aid	Needed Aid	High-quality help	Low-quality help	Equal or better off	Worse off
	(1)	(2)	(3)	(4)	(5)	(6)
Reversed distance	0.382**	-0.252*	0.132	-0.105	0.006	0.039
	(0.138)	(0.143)	(0.191)	(0.149)	(0.174)	(0.174)
Reversed distance squared	-0.002	0.002	-0.001	0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
L4.Expected transfer (0,70)	-0.783***	-0.906***	-0.878***	-0.818***	-0.917***	-0.780***
	(0.094)	(0.059)	(0.076)	(0.079)	(0.069)	(0.065)
Δ Trust national government	-0.886	-0.557	0.308	-1.396	0.881	-2.373**
	(1.929)	(0.963)	(1.251)	(1.391)	(1.594)	(0.899)
Δ Wealth Index (PCA)	0.931	-1.182	-0.739	-0.241	-0.094	-0.198
	(1.085)	(1.118)	(1.337)	(1.352)	(2.154)	(1.399)
Age (years)	0.043	0.152	0.111	-0.012	0.030	0.104
	(0.148)	(0.141)	(0.146)	(0.182)	(0.166)	(0.152)
Female (=1)	11.203***	-0.850	9.199**	-0.290	3.672	2.668
	(3.206)	(3.627)	(3.966)	(3.838)	(3.240)	(4.126)
Household head (=1)	11.141**	-0.715	7.224*	0.229	3.054	3.109
	(4.547)	(3.485)	(3.777)	(3.935)	(3.481)	(4.333)
Single (=1)	-6.220	-0.028	6.433	-7.257	-3.670	-1.342
	(7.836)	(3.421)	(6.271)	(4.612)	(5.769)	(5.764)
Education: Elementary (=1)	2.260	-1.452	-3.964	3.551	0.949	-1.309
	(4.450)	(2.480)	(2.335)	(2.703)	(3.411)	(2.627)
Constant	0.081	23.955***	8.319	22.367**	19.543**	11.915
	(8.065)	(6.209)	(8.741)	(8.365)	(9.025)	(7.372)
N	127	320	228	222	216	234
Cluster	25	29	30	30	30	30
Adjusted R-squared	0.43	0.40	0.44	0.33	0.42	0.33

Notes: Robust standard errors are clustered at the village level in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

S3. Experimental Materials

Solidarity Games 2012 & 2016:

All of you have 200 pesos at the beginning of the game. You will make your decisions on a sheet of paper only, but the decisions that you take are still about real money. For the rest of the game we have formed groups, each consisting of 3 players. Each of the originally invited participants [point to the left side where originally invited participants sit] brought along two friends. One sits in the middle and will play with you [point to the middle]. The other one who is sitting on the right-hand side will not be in the same group. Instead, the third player in your group will be someone from the right-hand side of the room, but you will never exactly know who it is. And the ones on the right-hand side will never know the two other group members they play with. From now on we will call the unknown players "Player X".

Whether you can keep the 200-peso given to you or lose them again will depend partly on your choices and partly on your luck. Remember, only one of the games will be randomly selected for the computation of the pay-out at the end of our session. For each group, we now have an opaque bag with 3 balls in it. This means that there are as many balls in the bag as we have players in a group. Each player draws one ball. Out of the 3 balls, there are 2 white balls and 1 red ball. If you draw a white ball you can keep your 200 pesos. If you draw a red ball you lose the 200 pesos you had at the start of the game. This means that one of the three players in each group will lose everything and two out of three will lose nothing.

[Hang up poster with example decision sheet on it]

In this game, the two winners can give money to the loser. Before you draw a ball, all of the players will be asked whether and how much they would like to transfer to the other two players in their group in case that they are unlucky, i.e. they draw a red ball and lose 200 pesos. Remember that one of the three players will lose for sure. Remember also that there will always be two players in your group who still have their 200 pesos. You can transfer between 0 and 70 of your 200 pesos to the unlucky person in your group. We will ask you to write down on a worksheet how much you would be willing to give to the losing player. Amounts are given in steps of 10 pesos. You can also decide to transfer nothing. Hence, possible transfers are 0, 10, 20, 30, 40, 50, 60 or 70. Every transfer decision you make is as good as any other – there are no wrong decisions. Your transfers will be kept in private, **so just choose the amount YOU like best! But remember it is going to be a transfer of real money.**

From now on, we will call the group member you know by his or her name (______) [ASSISTANTS ENTER THE NAME OF NON-ANONYMOUS PARTNER HERE] and the unknown group member Player X. For the players sitting on the right-hand side [point] there will be two unknown players Player X and Player Y. So, imagine you keep your 200 pesos and Player X loses his 200 pesos. We will ask you to write down on the worksheet how much you would be willing to give to Player X in this case (0, 10, 20, 30, 40, 50, 60 or 70). Now imagine you keep your 200 pesos and the friend you came here with and plays with you in your group loses his or her 200 pesos. Please write down on the worksheet how much you would be willing to give to him or her in this case (0, 10, 20, 30, 40, 50, 60 or 70).

We also want you to think about the transfer of the other winner in your group to the loser. Please guess the amounts that will be transferred. You will earn 10 pesos extra for each correct guess.

Lastly, it is, of course, possible that you draw the red ball and lose. We would like you to guess how much your friend in your group and Player X would be willing to give to you in this case. We will never tell you whether you were right or not. But Lukas will look at the choices made by your friend and Player X and compare their choices to your guess. You will earn 10 pesos extra for each correct guess. The best thing you can do to increase your payoff is to truthfully state what you think y and Player X would do.

[SHOW AND EXPLAIN PARTICIPANT FORM *make sure that the player is looking at the form and appears to be sufficiently concentrated*]

For non-anonymous players:

○ no loss	DECIDE TRANSFER TO ___________ ●	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70
○ no loss	DECIDE TRANSFER TO **PLAYER X** ●	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70
● lose 200	GUESS TRANSFER OF ___________ ○	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70
	GUESS TRANSFER OF **PLAYER X** ○	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70

For anonymous Players:

○ no loss	DECIDE TRANSFER TO **1st PLAYER X** ●	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70
○ no loss	DECIDE TRANSFER TO **2nd PLAYER X** ●	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70
● lose 200	GUESS TRANSFER OF **1st PLAYER X** ○	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70
	GUESS TRANSFER OF **2nd PLAYER X** ○	□ 0	□ 10	□ 20	□ 30	□ 40	□ 50	□ 60	□ 70

Before we start playing the game, we would like to ask you some questions to see if you understand the game.

What is the maximum amount of money you can earn in this game?	[ANSWER: 220]
What is the minimum amount of money you can win in this game?	[ANSWER: 0]
What is the highest amount you can transfer to the other player?	[ANSWER: 70]
What is the least amount of money you can transfer to the losing player?	[ANSWER: 0]
How much does the losing player earn, if the other two other players transfer nothing?	[ANSWER: between 0, 10 or 20]

Now we will distribute the decision sheets for the second game.

[Distribute decision sheets, collect them and bring them to XX when every participant is finished with filling them out.]