Global Sustainability

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Prosociality as response to slow- and fast-onset climate hazards

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Research Article

Cite this article: Steimanis I, Vollan B (2022). Prosociality as response to slow- and fast-onset climate hazards. *Global Sustainability* **5**, e10, 1–13. https://doi.org/10.1017/sus.2022.9

Received: 4 June 2021
Revised: 16 March 2022
Accepted: 27 March 2022

Key words:
antisociality; behavioral experiments; in-group favoritism; prosociality; slow and fast-onset climate hazards

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Non-technical summary

More and more people around the globe experience climate hazards. For vulnerable populations, these hazards not only cause significant physical damages, but can also affect the way people interact with each other. How such interactions are affected by climate hazards is particularly important for understanding the vulnerability of communities. Prosocial behavior is key for communities that heavily rely on informal social support to deal with these threats and for cooperative solutions to provide and maintain public goods. To investigate these effects, we talk to people living on the front lines of climate change and measure their prosociality using behavioral tasks. Our results show that both fast- and slow-onset hazards increase prosociality, underscoring the importance of well-functioning social relationships for dealing with hardship and uncertainty in a variety of contexts.

Technical summary

People's willingness to engage in prosocial behavior can affect how vulnerable and resilient populations are to climate hazards. We study how different types of climate hazards, fast-onsetting cyclones and slowly rising sea-levels, might affect peoples' prosociality using incentivized behavioral tasks. We sample people who are at the forefront of climate change and either experienced Typhoon Haiyan in the Philippines (study 1; $n = 378$) or are from sea-level rise hotspots (study 2; $n = 1047$) in Solomon Islands, Bangladesh, and Vietnam. We experimentally manipulate the salience of these hazards through recall or informational videos. Results from study 1 show that increases in prosociality are (i) independent of whether supportive behaviors or conflicts are recalled, (ii) are not only targeted to a narrow in-group, and (iii) do not come with increases in antisocial behaviors. In study 2, we also find that people behave more prosocial when they are informed about the impacts of rising sea-levels. Our survey evidence suggests that people who already perceive the threat of displacement due to rising sea-levels are also more prosocial. Overall, peoples' responses to both types of hazards are geared toward collective action, which could strengthen their adaptive capacity to deal with climate risks.

Social media summary

People severely affected by sea-level rise and rapidly emerging climate hazards are responding with increases in prosocial behaviors to fellow villagers.

1. Introduction

A growing number of people around the world are already suffering the consequences of climate change, manifested in rising sea-levels, flooding, extreme tropical cyclones, and land degradation (IPCC, 2021, 2019). In particular, people living in coastal regions and on low-lying islands are disproportionally exposed to impacts caused by rising sea-levels (Nicholls et al., 2021; Storlazzi et al., 2018; Vitousek et al., 2017) and tropical cyclones (Eberenz et al., 2021; Edmonds et al., 2020). Much research and policy responses focus on strengthening physical infrastructures (Aldrich & Meyer, 2015; Esteban et al., 2019; Norris et al., 2008) but these are unlikely to reach their full potential without functioning social systems (Cinner et al., 2018; Cinner & Barnes, 2019). One particular aspect of why social support systems are crucial is that the vast majority of people living in the most affected regions do still not have access to formal insurance to reduce their risks. Therefore, communities rely on informal risk-sharing support systems (Attanasio et al., 2012; Fafchamps & Lund, 2003; Townsend, 1994; Udry, 1994) to cope with climate shocks. Prosociality forms the basis of formal and informal institutions responsible for collective activities such as risk reduction, coping, and reconstruction.

In two related studies, we show whether prosocial behaviors are affected by climate hazards combining methods from experimental economics and psychology. We sample people from communities on the front lines of a fast-onset climate hazard (Study 1: victims of Typhoon Haiyan 2013, Philippines) and slow-onset sea-level rise (SLR) (Study 2: people from the small island nation of Solomon Islands, and people living in river deltas in Bangladesh and

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Vietnam). To measure prosociality, we use incentivized behavioral tasks and manipulate the salience of climate hazards through recall or informational videos. Here, we focus on prosociality in situations where both the helper and the helped are affected by the fast- or slow-onset hazards. In addition, we investigate whether fast-onset climate hazards have the potential to lay the groundwork for discrimination within communities or even promote spiteful behaviors.

It is said that disasters bring out both the worst and the best in people, where the sharing of resources and that people help each other is often overshadowed by news reports that focus on looting, hoarding, envy, or violence. Decades of disaster research have shown that in mass emergencies and disasters people predominantly support each other and act as one instead of selfishly[1] (Drury et al., 2013; Helsloot & Ruitenberg, 2004; Quarantelli, 2001; Quarantelli & Dynes, 1977; Solnit, 2009). Building on self-categorization theory (Turner et al., 1987), recent research in social psychology has identified that the strengthening of prosocial behaviors is enabled by an emerging shared social identity created by a sense of common fate as the underlying psychological mechanism among a variety of disasters (Drury, 2018; Drury et al., 2019; Ntontis et al., 2018, 2021; Ntontis et al., 2020). Less is known about the prolonged effects on prosociality in communities struck by fast-onset natural disasters. Recently, Ntontis et al. (2021) show how social identity processes shape community resilience among a flooded community in the recovery period 8–21 months after a severe flood.

Whether people's prosocial behaviors are affected by fast-onset disasters is also increasingly studied by economists who try to identify causal effects using incentivized measures of prosociality. These studies find in some contexts that people affected by fast-onset (natural) disasters respond with increases in prosociality (Cassar et al., 2017; Li et al., 2013; Rao et al., 2011; Whitt & Wilson, 2007) while in other contexts decreases are found (Becchetti et al., 2017; Fleming et al., 2014, p. 20). These are not only short-term fluctuations in prosociality immediately after the disaster but in some cases persist for up to several years (Becchetti et al., 2017; Cassar et al., 2017). Behavior of other people, if someone strongly identifies with these other people, is a strong predictor of one's own behavior in such extreme situations (Drury et al., 2016; Reicher, 1984, 1996). If after a natural disaster people predominantly observe selfish behaviors and tend to seek the familiar (Mawson, 2005), multiple small groups might emerge based on preexisting social ties (family, friends) instead of one shared disaster identity. If the former is the case, people might be more prosocial with their close group potentially coming at the expense of more distant social groups within a community, so-called in-group favoritism (Turner et al., 1987). Eroding social relationships in disaster-struck communities could negatively affect social support systems (Ligon et al., 2002) potentially undermining individual and communal resilience to climate risks (Kaniasty, 2020). In the worst-case, in-group favoritism could lay the foundations for discrimination within communities (Greenwald & Pettigrew, 2014) or even spur antisocial behaviors which often coexist with prosocial behaviors (Basurto et al., 2016; Jensen, 2010; Prediger et al., 2014).[2]

In the first study, we varied whether participants had to recall acts of social support or conflicts, such as over the distribution of relief supplies, that happened after Haiyan to identify the effects of post-disaster conditions. We conjecture that whether acts of support or conflict overshadowed the recovery period could explain earlier mixed results on prosociality. To further explore the potential for negative effects, we measured in-group favoritism and antisociality. We find significant increases in prosociality but not in antisociality, or in-group favoritism when participants had to recall Typhoon Haiyan. Surprisingly, these results do not depend on whether participants had to recall supportive activities or conflicts. Additionally, strengthened prosociality does not come at the expense of increases in antisocial behaviors or in-group favoritism, reducing some concerns that climate hazards act as dividers between people that could undermine existing informal risk-sharing schemes or could potentially lay the ground for interpersonal conflicts at the local level.

In the second study, we focus on whether prosocial behaviors are affected by slow-onset hazards which have so far not received adequate attention given the climate change realities and prospects of millions of coastal inhabitants (Nicholls et al., 2021). In addition to the incentivized measure of prosociality, we asked participants to indicate how they perceive the risk of having to move because of rising sea-levels. Given the lack of empirical evidence on whether slow-onset climate hazards affect prosociality, our conjecture is based on cooperative game theory. It provides a useful framework for understanding outcomes in strategic interactions, that is, one person's best response to another person's action, by simplifying the decision environment. With regards to cooperation, game theory predicts that prosocial behaviors can be sustained only when there are repeated opportunities to interact for people without a clear endpoint (Axelrod & Hamilton, 1984; Dal Bo, 2005). This uncertainty over the possibility of future interactions has been coined the 'shadow of the future' by Axelrod and Hamilton (1984). Experimental evidence shows that when people know they only interact for a certain amount of time, prosocial behaviors (i.e. cooperation) cannot be sustained and dwindle over time (Blake et al., 2015; Dal Bo, 2005; Dal Bó & Fréchette, 2011, 2018). For example, people shy away from precise tests of a severe illness and would prefer only rough information about a bad outcome to remain hopeful for the future (Schweizer & Szech, 2018). We hypothesize that participants who expect future climate hazards forcing them to resettle might perceive this undesirable future state as a lifting of the 'shadow of the future' (i.e. moving from an infinite interaction to a finite interaction) and respond with more selfish behaviors. This conjecture rests on the assumption that people believe they have to relocate individually and cannot continue to interact with their fellow community members. Furthermore, they must be able to bridge the large psychological distance of climate-induced displacement so that it can already influence their behavior today (Brügger et al., 2015). We sample participants from low-lying atoll islands and delta regions who already experience severe SLR

[1]There are certainly exceptions where individualistic behaviors are observed depending on the conditions of the extreme situation. For example, using the sinking of the Titanic and Lusitania, it has been identified that prosocial behaviors according to existing social norms prevailed on the Titanic while selfish behaviors did on the Lusitania (Frey et al., 2010a, 2010b, 2011). The authors argue that these findings can be attributed to the fact that the Lusitania sank in 18 minutes while people had much more time (nearly three hours) on the Titanic.

[2]Climatic changes may trigger resource scarcity, negative income shocks, migration, or institutional failure, all of which can spark social conflict. The climate-conflict literature has associated temperature fluctuations and other climatic events with an elevated risk of conflict across all spatial scales, from the local to the macro level (Burke et al., 2015; Hsiang et al., 2011, 2013). At the individual level, resource scarcity has been shown to increase anti-social behavior (Prediger et al., 2014).

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hazards and induce variation in their perceived affectedness using informational videos. Similar to the results of study 1, we find increases in prosociality when slow-onset threats are made present. Analyzing participants' relocation beliefs supports the experimental results and addresses some of the concerns that the treatment effects are only short-lived and artificially induced. Participants who are certain they must move because of SLR are also more prosocial than participants who do not strongly believe that they need to resettle soon.

2. Methods

Studying cause and effect relations of fast- and slow-onset climate hazards is inherently difficult. For fast-onset hazards, it is difficult to predict when and where they will occur, rendering targeted data collection of incentivized behavioral data before they occur impossible, whereas for slow-onset hazards, there is no clear starting and ending point that allows measurement of an unbiased treatment effect. If one would know where a fast-onset hazard strikes, it is unlikely to be an unanticipated causal event, as people living there would also know and prepare accordingly. Therefore, we rely on the 'priming' technique to introduce random variation in the awareness of the respective hazard to measure the causal effects on prosociality. Even outside psychology, priming has been increasingly used to study how the (social) environment shapes preferences and behavior, for example, the effects of identities (Benjamin et al., 2016, 2010; Cohn et al., 2015), culture (Cohn et al., 2014), and traumatic events (Callen et al., 2014). We actively prompt people to recall past experiences (study 1) or think about specific concepts and events (study 2), which is said to activate memories or associations which make the concept or event salient and focal (Cohn & Maréchal, 2016). One potential concern related to priming could be that the effects are short-lived and lack external validity. To reduce some of these concerns, we sample people who were strongly affected by Typhoon Haiyan or live in a low-lying small island state and river deltas where SLR is already affecting people's livelihoods and investigate how participants' actual relocation beliefs correlate with our measures of prosociality.

2.1 Commonalities in experimental design choices

In both studies, participants had to complete two main tasks as shown in Figure 1. First, the manipulation tasks induced variation in how salient climatic hazards are for participants using either guided interviews (study 1, Figure 1 top) or information videos (study 2, Figure 1 bottom). Second, we elicited participants' prosociality (study 1 and study 2) and additionally antisociality, and in-group favoritism in response to Typhoon Haiyan (study 1). The advantage of such incentivized behavioral tasks is that the researcher can observe decisions in a controlled environment changing one aspect at a time. This is challenging in everyday life, where people might act prosocial or antisocial because of a variety of reasons that are unknown to the researcher. The implementation followed established procedures[3] using standardized protocols, which can be found in Supplementary Section S1 and S2.

[3]We hired native speakers to translate the experimental materials into the local languages (Tagalog, Pidgin English, Bengali, or Vietnamese) and back to English for validation (a second translator). Local research assistants, whom we trained and supervised, carried out the data collection using tablet computers. In all samples, participants (aged 18 and older) had to give their consent to take part in the study and were free to stop at any time. Before participants received their payments, they had the chance to ask questions and give feedback.

Across all study sites, we incentivize the outcome measures and ensure anonymity of decisions to reduce the risks of hypothetical bias or social desirability biases common in self-reported survey measures. Incentives were adjusted to the length of participation and the average earnings of daily laborers in each study site. All monetary amounts are converted using the purchasing power parity conversion factors from the World Bank for each study site to adjust for the relative price differences between study sites. On average, participants in the Philippines earned $\$14.9 \pm 3.6$ and in Solomon Islands $\$10.7 \pm 1.6$ for taking part in the three to four-hour workshops, while earnings in the survey experiments (on average 45 minutes) were $\$3.6 \pm 1$ in Bangladesh and $\$7.3 \pm 2.6$ in Vietnam.

2.2 Differences in implementation across study sites

However, there are two important differences in how we measured outcomes across study sites. Firstly, prosocial behaviors were measured either through a solidarity game (study 1) or dictator decisions (study 2), see Figure 1. We used the solidarity game in the Haiyan context, as it creates a decision environment that captures important components, such as diffusion of responsibility and informal transfers in risk-sharing networks to deal with adverse shocks. For the slow-onset context, we decided to go with non-strategic dictator decisions which capture people's underlying degree to which they value the (monetary) wellbeing of another person. Such decisions have been shown to be predictive for cooperative behaviors in widely applied laboratory experiments (Balliet et al., 2009) and are relevant for a broad range of real-world behaviors, such as helping, sharing, or volunteering (Franzen & Pointner, 2013; Lange et al., 2007). Secondly, in the Philippines and Solomon Islands, we elicited outcomes as part of longer lab-in-the-field experimental workshops with several people participating simultaneously allowing for complex strategic interactions such as the solidarity game. In Bangladesh and Vietnam, we conducted face-to-face surveys, in which we can still sample from relevant populations. Interviewer effects might be stronger in these surveys than in the workshops where always the same research assistant explains the tasks. To alleviate some of these concerns, we trained and explicitly instructed enumerators to hand over the tablet for the incentivized measures, so that participants could take these decisions in private. As a robustness check, we control for interviewer effects (see separate estimates for Bangladesh and Vietnam reported in Supplementary Table S14).

We provide further details on measurement and treatment manipulations in the following study-specific section and Supplementary Sections S1 and S2 (field implementation, sampling, balancing tests, summary statistics).

3. Study 1: fast-onset climate hazards

We conducted lab-in-the-field experiments three years after Typhoon Haiyan with 378 people from 14 randomly selected coastal villages on Panay in 2016. In each village, 27 people participated simultaneously in the workshops which were held in locations such as schools, daycare centers, or roofed basketball courts. The 14 communities were in the direct pathway of Haiyan (see Supplementary Figure S1 for details). Over 80% of participants in our sample report that their houses were at least

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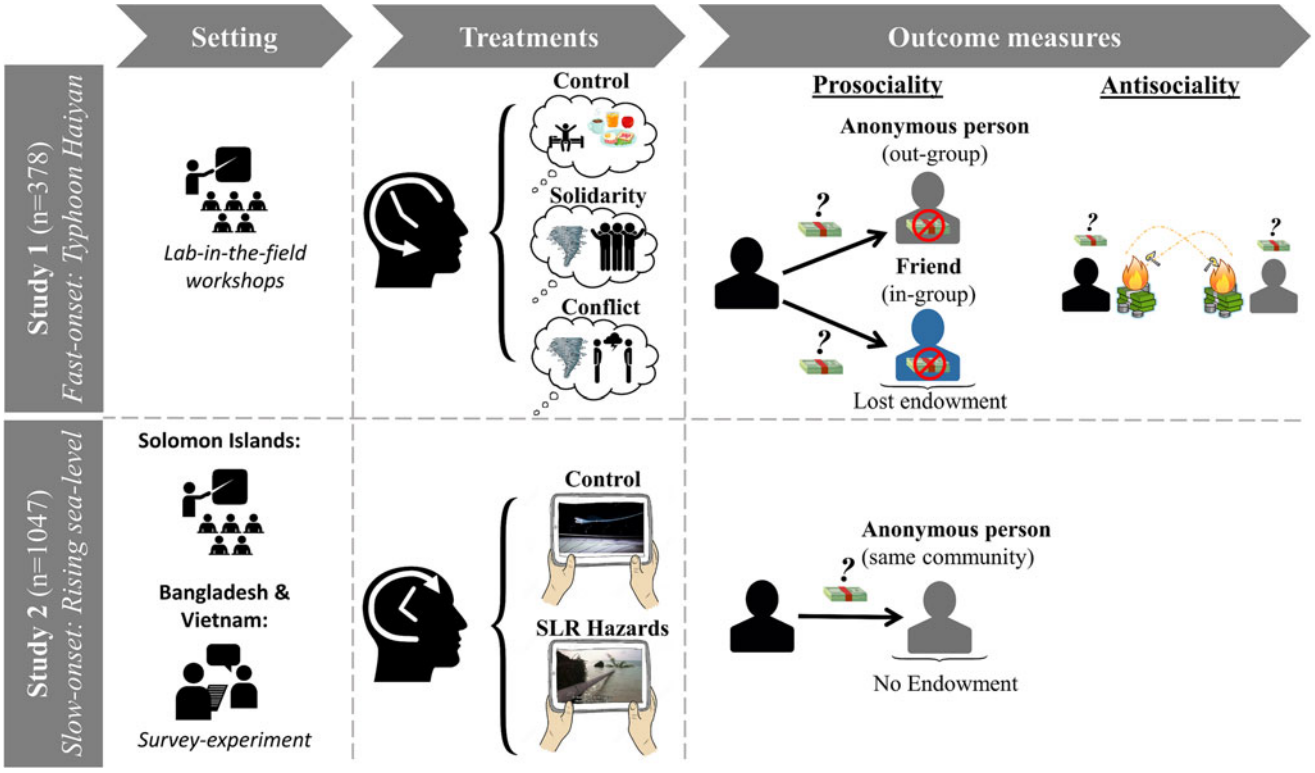

Fig. 1. Overview of experimental design across both studies. Notes: The backward clock (study 1) illustrates that participants had to recall something that already happened depending on their randomly assigned condition. Similarly, the forward clock (study 2) illustrates that participants received information about potential future states of the environment they live in. The red circle above the stack of banknotes illustrates the situation in where a participant lost their endowment in the solidarity game. Participants had to make transfer decisions conditional on their other group members losing their endowment while they keep it. This enables us to elicit transfer decisions for all participants in study 1. After measuring the outcome variables, participants answered a survey on socio-demographics and survey measures of risk aversion and patience.

partially damaged and 94% said they needed external aid. On average, participants estimate the damages to their assets (house, motorbike, boat, work materials, crops) at $990 (median = 527, standard deviation (SD) = 1,410). A large amount was given as an average monthly household income of $269 (median = 185, SD = 277).

3.1 Outcome measures

3.1.1 Prosocial behavior

Following Selten and Ockenfels (1998), participants play the solidarity game in groups of three. The groups always consisted of two players who knew each other (referred to as 'friends') and a third player who is someone else from the same community. The two friends did not know who the third player in their group was, since the third player was always randomly assigned to each group.[4] Similarly, the anonymous third player (referred to as 'stranger') did not know about the identity of the other two group members nor their relationship as friends. All participants played the solidarity game twice. The first time they played before being assigned to one of the guided interviews (baseline) and the second time afterwards.

Each player starts with the same endowment of 200 Philippine pesos (PHP), equivalent to US$4 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, which contained one red ball and two white balls.[5] If a red ball is drawn the player loses her endowment. Thus, this design ensured that always one group member ends up losing their endowment. Before the draw, each player makes two transfer decisions assuming that she keeps her endowment and one of the other two players loses her endowment in each case. Players could only make transfers between zero and seventy pesos in steps of ten (0, 10, …, 70). To better reflect the aftermath of a disaster, we decided to introduce an upper bound of 70 PHP to exclude the possibility that the shock victim could be much better off than the two 'winners' of the lottery. The transfer decisions were not disclosed at the end of the workshop and decisions could not easily be traced back even when the solidarity game was payout relevant as the show-up fee included a fixed component of 100 PHP and a random component of 50 PHP. Additionally, each player had to guess what they expect the other players would transfer if she would be the shock victim. Correct guesses were incentivized with 10 PHP each. For the main treatment effects on prosociality, we focus on those transfers

[4]We put up nine rows of three chairs each. The two friends were always sitting in the same row and the strangers were randomly assigned to the remaining seats, not necessarily in the same row as the two friends.

[5]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.

https://doi.org/10.1017/sus.2022.9 Published online by Cambridge University Press

where players do not know the identity of the recipient. The stranger made one transfer decision for each of the two friends in the group without knowing their identity. We use the average of both transfers in all our analysis. For the two friends we use the single transfer decision to the stranger. In addition, the two friends made a transfer decision to their friend, allowing us to determine whether the two friends discriminate in their transfers. The difference in these transfers to their friend and the stranger gives us a measure of in-group favoritism within communities. The exact wording used to explain the solidarity game is reported in Supplementary Section S1.

3.1.2 Antisocial behavior

Antisocial behaviors were elicited adopting a design of the joy-of-destruction (JoD) game similar to Prediger et al. (2014), which was always carried out after the solidarity game. To avoid any spill-over or endowment effects from the solidarity game, we did not disclose any choices or results from the solidarity game. The JoD game gives a measure of participants' willingness to engage in spiteful behavior by financially harming another person at a personal cost. Our matching procedure ensured that players were not assigned to a partner they had played with in the solidarity game. Both participants receive 200 PHP and simultaneously decide in private whether to reduce the other participants' endowment by 40 or 160 PHP at a personal cost of 10 and 40 PHP respectively or not. Thus, reducing does not entail any material benefit, as both are worse off compared to the situation where nobody engages in burning money. Strategic motives should play no role here as participants only interact once and do not know their partner's identity. In addition, their decisions were not disclosed to the partner or could be inferred from the final payment due to a random payment component. In the analysis, we use a binary specification of spite due to the extremely low prevalence of spite (only two participants invested 40 PHP). After participants made their decision (0, 10, 40), they had to guess what their partner will do. A correct guess earned an extra 10 PHP.

3.2 Treatments and manipulation check

After playing the solidarity game once, each participant went through a guided interview with one of our research assistants to induce variation in the subjective perception of the impacts of Haiyan. Participants had to either think about all events of the current day (Control), recall supportive behaviors (T1: Support), or recall conflicts (T2: Conflict) that happened in the aftermath of Haiyan. In each experimental workshop the groups of three participants were randomly allocated to one of the three priming conditions. Thus, all three group members were assigned to the same condition. Then, each participant went individually through the guided interview process where assistants asked for details to keep the participant reporting for about 5 minutes. Assistants took notes of the main talking points. The wording for the guided interviews was as follows:

- **Control:** 'We now would like to know a little bit more about what you already did today. What have you eaten for breakfast today? What have you been doing after breakfast until now?'
- **T1 Support:** 'We would like to know more about a specific disaster (typhoon Yolanda/Haiyan) that occurred in November 2013. We would like to know more about the behavior of people after such a disaster. Can you remember reports or personally witnessed incidents where people have helped each other after typhoon Haiyan/Yolanda hit the island? Think about specific incidents. Could you imagine other sorts of incidents that show that good things happen even though a disaster just occurred, and if yes, what may they be?'
- **T2 Conflict:** 'We would like to know more about a specific disaster (typhoon Yolanda/Haiyan) that occurred in November 2013. We would like to know more about the behavior of people after such a disaster. Can you remember any conflicts that happened because of Haiyan/Yolanda? These can be situations where people enriched themselves at the cost of others, committing criminal acts or behaved in other sorts of unwanted behavior. Think about specific incidents. Could you imagine other sorts of incidents that show that more bad things happen because of disasters, and if yes, what may they be?'

The treatments aimed at inducing variation in how participants perceive Haiyan before making their decisions in the solidarity game and JoD game. As an indication of whether the treatments induced such variation, participants rated on a 10-point Likert-item from 1 ('very unlikely') to 10 ('very likely') how likely they think it is that Haiyan resulted in a worse togetherness of people in their community. The question was asked as the first item in the survey, approximately 30 minutes after the participants took their solidarity and spite decisions. Thus, any induced variation between treatments might already be less pronounced than directly after the guided interviews.

Figure 2 shows that respondents in the support treatment perceive togetherness as less affected by Haiyan than participants in the control treatment (Mann–Whitney U (MWU) test $z_{251} = 1.66$, $p = 0.09$). However, also participants who were supposed to recall conflicts perceive the effects on togetherness not as worse than participants in the control condition (MWU $z_{251} = 1.17$, $p = 0.24$). The conflict treatment seems to have 'backfired' (Schwarz et al., 1991), meaning that it was not only difficult to remember selfish behaviors occurring in the aftermath of Haiyan but that participants actively recalled supportive behaviors.[6] Only 35 out of the 126 participants in the conflict treatment reported conflicts, for example, over the distribution of relief goods. These 35 participants are more likely to perceive negative impacts on togetherness than the other participants in the conflict treatment (MWU $z_{126} = -2.12$, $p = 0.03$). However, even these 35 participants do not have significantly different perceptions about togetherness than participants in the control condition (MWU $z_{160} = -0.60$, $p = 0.55$).

3.3 Results

We start with the main treatment effects on solidarity transfers, expected transfers, antisocial behavior, and in-group favoritism using linear ordinary least square regressions. The results visualized in Figure 3 are based on variations of the following equation:

$$outcome_i = \alpha_1 + \beta_1 T1\_Support_i + \beta_2 T2\_Conflict_i + \beta_3 X_i + \varepsilon_{i1}$$

[6]Examples of common responses in the conflict treatment are in line with perceptions of 'common fate' motivating supportive behaviors: 'People here during typhoons are more cooperative', 'Each of us help one another', 'There is nothing I remember only helpfulness of each people here', 'No incident happened here', 'No no no ! Sorry!', 'No because people here are all victims too.' 'No bad incidents happen here!'. Therefore, we deem it unlikely that many people simply avoided to talk about conflicts or corruption.

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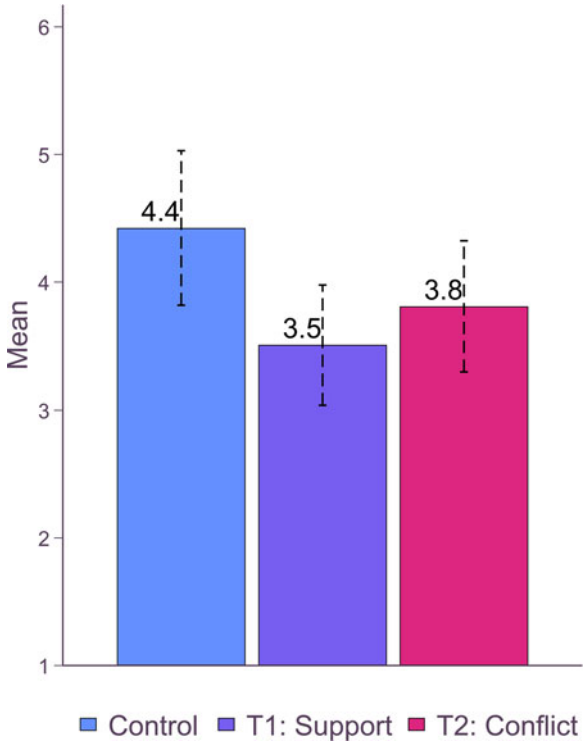

Fig. 2. Manipulation check: Worse togetherness due to Haiyan? Notes: Participants state their agreement on a ten-point Likert-type items ranging from 1 'very unlikely' to 10 'very likely'. Dashed-lines indicate 95% confidence intervals around the means in each group. There is one missing value for the togetherness question in the control group.

We regress each outcome on the variables of interest: a dummy for each treatment (estimates $\widehat{\beta}_1$ and $\widehat{\beta}_2$) and vector of explanatory variables (X_i) to account for some slight imbalances in covariates and generally increase precision of our estimates. For the outcomes related to the solidarity game, we can additionally control for the baseline transfers participants made before being assigned to the treatment conditions.

Figure 3 shows that participants in both treatments are more prosocial (panel A) and expect more prosociality in return (panel B). Recalls of support increase transfers by 6 PHP ($\beta = 6.28$; $p = 0.01$; 95% confidence interval (CI) 1.47–11.09), while recalls of conflicts also increase transfers by around 4 PHP ($\beta = 4.44$; $p = 0.05$; 95% CI 0.06–8.83), compared to participants in the control group. These are notable increases between 20 and 27% compared to the average transfer of 22 PHP in the control group. Addressing some concerns that not all participants in the conflict treatment recalled conflicts, we compare whether these participants behave differently in the solidarity game (see Supplementary Table S4). We find neither evidence that successfully primed participants are less prosocial ($\beta = 1.77$; $p = 0.60$; 95% CI −4.18 to 7.71) nor expect less prosociality ($\beta = 1.14$; $p = 0.70$; 95% CI −4.91 to 7.19) than not successfully primed participants in the conflict treatment. Thus, it is rather the recall of the disaster itself that drives the effects than the distinct post-disaster conditions.

While it has been essential for human survival to cooperate with fellow community members (Nowak, 2006), there is increasing evidence that pro- and antisocial behaviors coexist within individuals depending on the context (Rusch, 2014). In the following, we explore whether the salience of a fast-onset event, while increasing prosocial behavior, might at the same time spur antisocial behaviors. Overall, we find an exceptionally low prevalence of antisocial behavior in our sample (panel C). Only 18 out of 378 participants (5%) engage in costly spite across all treatments. Neither participants in the support ($\beta = 0.02$; $p = 0.32$; 95% CI −0.02 to 0.05) nor the conflict treatment ($\beta = 0.01$; $p = 0.62$; 95% CI −0.03 to 0.05) are more likely to invest in reducing another villager's earnings. Regarding the preferential treatment of friends over strangers, we find that the two friends discriminate significantly between their friend and the stranger before priming (*t*-test $\text{Mean}_{\text{friends}} - \text{Mean}_{\text{anonymous}} = 0$, $t_{251} = 8.75$, $p < 0.01$). On average, they transfer around 10 PHP more to their friend than the stranger. Thus, we find that the two friends when having to recall Haiyan (both in T1 Support and T2 Conflict) give almost as much to the anonymous third as they transfer to their friends in the baseline. Most importantly, there are no significant increases in the solidarity wedge across treatment conditions, indicating that increases in prosociality when recalling Haiyan are not only restricted to a specific group of people within a community. The solidarity wedge even slightly decreases in the Support treatment (T1 Support $\beta = -2.62$; $p = .25$; 95% CI −7.10 to 1.84) and does not significantly increase in the Conflict treatment (T2 Conflict $\beta = 0.84$; $p = 0.78$; 95% CI −5.14 to 6.81) compared to participants in the control (see panel D).

4. Study 2: slow-onset climate hazards

We conducted study 2 with 1,047 people living in SLR hotspots, either on low-lying atolls (Solomon Islands) or coastal delta regions (Bangladesh, Vietnam). The data collection timeline for study 2 and a summary of the sample specifics are outlined below. Details about the sampling strategy in each study site are reported in Supplementary Section S2, see also Figure S2.

- **Solomon Islands (2017 – March to May):** We conducted lab-in-the-field experimental workshops with 477 participants in the following study sites: (i) two randomly selected neighborhoods in the capital Honiara ($n = 117$), (ii) the two main settlements of atoll islanders in Honiara ($n = 120$), and (iii) in 10 communities on the low-lying atoll group Reef Islands[7] ($n = 240$).
- **Bangladesh (2018 – September):** We conducted a face-to-face survey experiment with 217 people from 12 randomly selected coastal villages in the Barisal region in southern Bangladesh.
- **Vietnam (2019 – April):** We conducted a face-to-face survey experiment with 347 people from eight randomly selected coastal villages in Ca Mau and Bac Lieu province in the Mekong Delta.

Participants are living in areas that are and continue to be highly exposed to SLR (Becker et al., 2012; Church et al., 2013), and, therefore, subject to hazards, such as strong storms, coastal erosion (Storlazzi et al., 2018; Vitousek et al., 2017), and increased flooding (Auerbach et al., 2015; Nicholls & Cazenave, 2010). The evidence from our surveys shows that participants feel highly exposed to these hazards and the risks they pose, for details see Supplementary Figure S4. Almost 60% of atoll island dwellers think they will have to move because of SLR hazards in the next five years. In the low-lying deltas, 40% of participants

[7]We visited every village with at least 14 households that were located either directly on the beach or one of the tiny islands.

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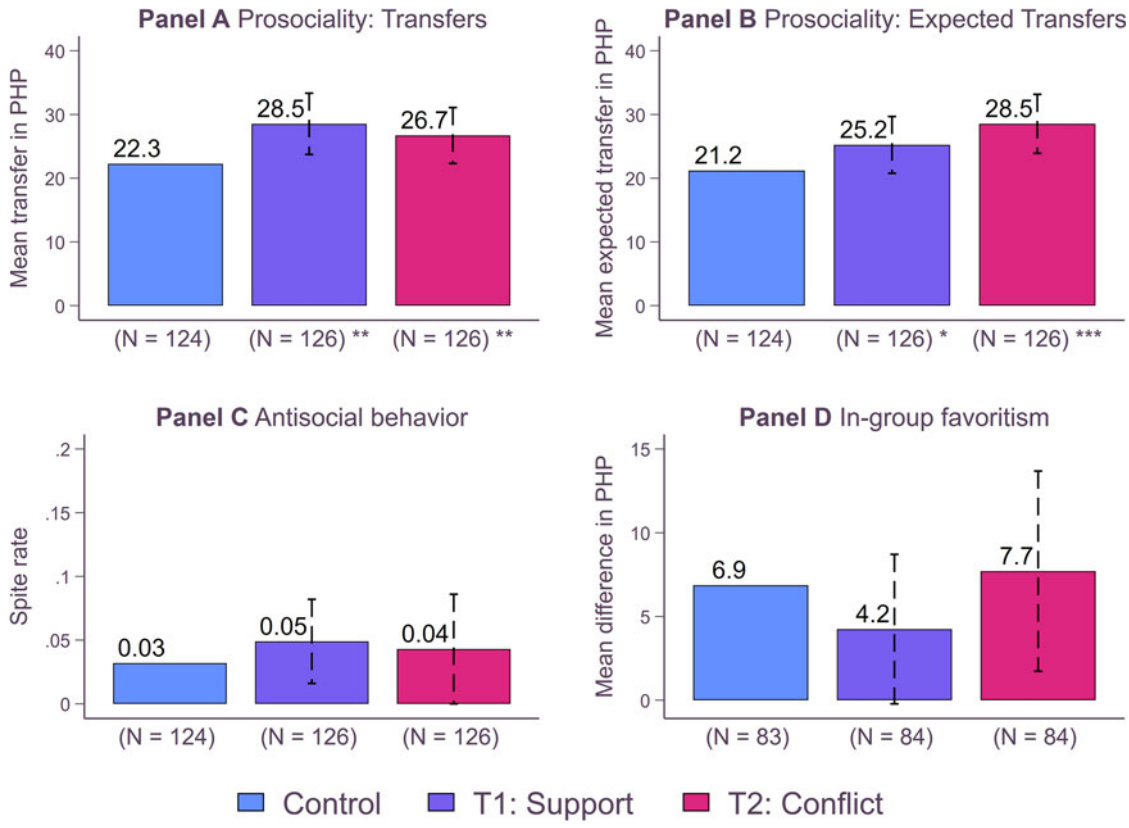

Fig. 3. Main treatment effects. Notes: We plot the regression estimates from multivariate least square regressions. Panel A shows the treatment effects on solidarity transfers when the receiver is anonymous and not a friend. Panel B shows participants expectations about what they think they will receive from the anonymous group member in case they lost their endowment in the solidarity game. Panel C show the prevalence of spite (spite rate) across treatments in the JoD game, i.e., the frequency of costly investments in reducing the partners earnings. Panel D plots the estimates for the average size of the wedge between giving to friends and strangers in the solidarity game. Positive values show that participants (the two friends) transfer more to their friends than to the stranger and vice versa for negative values. The control bar is the mean of the outcome variable of the control group. For each treatment group the bar is the sum of the value of the control bar and the regression estimates of the corresponding treatment dummy and a 95% confidence interval. To account for some slight imbalances in covariates and generally increase precision of our estimates, we include the following covariates: gender, marital status, age, education, household size, household income, time to prepare for Haiyan, patience, risk aversion, and trust. For the outcomes related to the solidarity game, we can additionally increase precision and account for potential regression to the mean by controlling for baseline (before priming) measures of the outcome variable in (i) transfers (panel A, (ii) expectations (panel B), and (iii) in-group favoritism (panel D). The dashed lines indicate 95% confidence intervals based on clustered standard errors at the group level to account for potential correlation at this level where the treatment was introduced (Abadie et al., 2017). The stars indicate whether differences are statistically significant at the following levels: *** p<0.01, **p<0.05, * p<0.1. Supplementary Table S3 reports the full regression outputs where uneven columns show estimates of treatment effects without added covariates and even columns correspond to the estimates with covariates which are plotted in Figure 3. Supplementary Table S5 shows robustness checks using alternative model specifications including village fixed effects.

think that floods and erosion are an 'extreme threat' (10 on a 10-point Likert item) to their livelihoods and 11% think it is 'absolutely certain' that they must permanently relocate because of slow-onset hazards.

4.1 Outcome measures

4.1.1 Prosocial behavior

In Solomon Islands, we measured how people value the payoff of another person in interdependent decisions, often referred to as Social Value Orientations (SVOs) or other regarding preferences. We use an incentivized version of the task developed by Murphy et al. (2011) to measure SVO. This SVO task consists of six dictator choices where participants must decide on how to distribute their endowments ranging from 15 to 17 SBD (US$1.9–2.1) between themselves (sender) and another person (receiver) from the experimental workshop. All participants were randomly selected from the same community; thus, participants knew that the receiver of their transfer would be from their community. For each allocation, participants marked their preferred distribution on a slider which was printed out on a laminated sheet, see Supplementary Figure S3. Based on these six decisions, we calculate a continuous outcome measure of SVO from competitive over individualistic and prosocial to altruistic. One decision was randomly chosen to be relevant for payout at the end of the workshop. To be able to elicit sender decisions for all participants, we introduced uncertainty about the role of senders and receivers. Thus, all participants made decisions about allocations as senders, and we randomly chose at the end of the workshop their role (sender or receiver) to avoid any strategical concerns.

In the survey experiments conducted in Bangladesh and Vietnam, we decided to use a single allocation decision in the style of a standard dictator decision (Forsythe et al., 1994; Kahneman et al., 1986), a well-established tool in the experimental literature to measure other-regarding preferences. We opted for a single decision for ease of implementation given the time

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constraints in the face-to-face interview setting. Participants were endowed with 120 Taka (~US$1.4, Bangladesh) or 25000 Dong (~US$1.2, Vietnam) and had to decide how much of this endowment they would like to send to someone else from the same community. They could send any amount, whole numbers only, between zero and the endowment. The receiver of this amount was always the next survey participant from the same community. We standardize all these outcome measures using z-scores to make treatment effects comparable in relative terms across study sites. In the main analyses we pool the data from all three study sites and equally weight all observations.

4.2 Treatments and manipulation check

Prior to taking their transfer decisions, participants watched a video on a tablet computer to induce variation in the awareness of SLR hazards and potentially negative emotions. Half of the participants watched a two-and-a-half-minute long video showing potential SLR impacts (land erosion, floods, stronger high tides, saltwater intrusion, loss of harvest) and testimonials of people that are in a comparable situation and had to resettle. The other half watched an emotionally neutral video but still interesting enough to pay attention as participants in pretests confirmed. In Solomon Islands, we could not find a video that participant perceived as neutral in pretests and decided to not show any video at all. Thus, we cannot rule out any effects of watching a video per se driving our results there. However, the findings from the survey experiments indicate that results are not driven by pure video effects such as pleasure or boredom from watching a video independent of the content. Across study sites, we hold the style and content of the videos constant. All videos are in the local language. In Vietnam, we additionally introduced two hypothetical scenarios at the end of the video to experimentally vary the relocation belief – either individual relocation or community resettlement. We find no significant differences between both treatments (see Supplementary Figure S6) and pool observation from both treatments for the analysis presented in the main text. The treatment was randomly assigned at the session level in the experimental workshops in Solomon Islands, and at the individual level in the survey experiments. Interested readers can find the exact content of the videos in the Supplementary Section S2.

Participants live at the forefront of SLR hazards potentially worrying about displacement because of these hazards which can evoke strong negative emotions. Anticipatory negative emotions such as fear can be a strong motivator for people to engage in coping behavior to avoid any undesirable future state (Baumgartner et al., 2008; Lazarus, 1991). Figure 4 shows a significant increase in negative emotions induced by the information treatment by about 30% compared to the control group ($M_{control}$ = 1.30, $M_{treated}$ = 2.86, difference = −1.56; MWU z = −17.55, p = 0.00). Participants react with emotions such as being afraid, upset, or nervous to the information treatment. This puts us in a position to test whether the increase in negative emotions related to the salience of SLR hazards affects prosociality, a potential coping mechanism to deal with emotional distress (Dovidio et al, 2017; Midlarsky, 1991; Raposa et al., 2016).

4.3 Main pooled results

Table 1 shows an overview of the main pooled results for study 2. We find that slow-onset climate hazards have effects going in the same direction as recalling the experience of a fast-onset disaster.[8] On average, participants in the information treatment who watched the SLR video are 0.2 SD more prosocial compared to the control group (model (2): β = 0.14; p = 0.02; 95% CI 0.02–0.27). People might react differently to the information treatment depending on their relocation beliefs. Participants who are 'absolutely certain' (10 out of 10) about having to permanently relocate due to slow-onset hazards should be more likely to behave in line with the 'shadow of the future' predictions, that is, more selfish. However, in line with the main treatment effects, participants in the control group who are absolutely certain about having to relocate tend to be slightly more prosocial than participants who do not believe so (model (4): β = 0.20; p = 0.07; 95% CI −0.01 to 0.42). The interaction effect shows that the information video does not further increase prosociality for participants who believe relocation is unavoidable (model (4): β = −0.23; p = 0.09; 95% CI −0.50 to 0.04). Thus, it is only the participants who do not yet see the threat for permanent relocation that react to the information treatment (model (4): β = 0.22; p = 0.00; 95% CI 0.07–0.37). They are as prosocial as participants who believe relocation is unavoidable, as for the latter the treatment videos provide no new information because the risks are already very salient.

Lastly, we contrast our experimental findings with survey answers on what adaptation actions participants would recommend in case of half a meter SLR. Participants prefer cooperative strategies that require substantial collective efforts, such as building sea walls (65%) and planting mangroves (42%) more often than individual action – for example, moving away (38%) (details in Supplementary Figure S5). These preferences for in situ adaptation are both consistent with our findings of increased prosociality and evidence from case studies showing that people prefer to adapt locally rather than move away in response to SLR hazards (Esteban et al., 2019; Jamero et al., 2017).

5. Discussion and conclusion

Climate change will amplify the scale of environmental hazards already affecting the livelihood of marginalized people across the globe. Affected communities' adaptation and coping options in the absence of outside interventions will crucially depend upon their capacity to work collectively, uphold mutually beneficial cooperative norms, agreements about resource use, and solidarity in helping each other. The results of study 1 are consistent with existing evidence showing that fast-onset hazards reinforce prosociality. Based on our recall treatments, we extend knowledge by showing that post-disaster conditions, that is, a supportive environment as opposed to conflict, may not be as important. Participants in study 1 were as prosocial when recalling conflict over relief distribution as participants who recalled supportive activities happening in the aftermath of Haiyan. Further, we find no evidence that the strengthened prosociality relates only to a narrow group of individuals or is associated

[8]A concern for interpreting the slow-onset effects is that we only observe the behavior at one specific point in time. Thus, we do not know whether any shift toward selfishness already occurred prior to our data collection or if more selfish people already moved away. Comparing migrants from atolls to their former community members still living on the atoll, we find no evidence that these differ in their prosociality rendering prosociality as a reason for migration unlikely. Atoll migrants are not significantly less prosocial, controlling for socio-economic differences, than atoll inhabitants (see Supplementary Table S11). Atoll migrants are, however, slightly less attached to their homes than people who are still living on the atoll.

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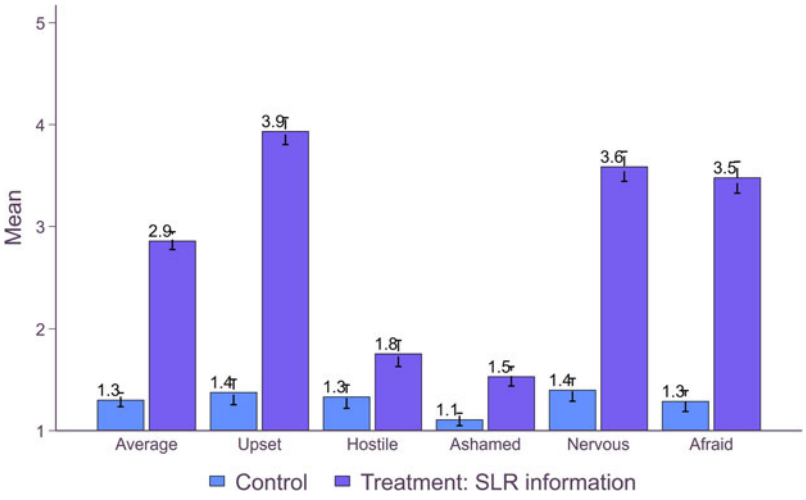

Fig. 4. Manipulation check: Negative emotions. Notes: Participants state their emotions on five-point Likerttype items ranging from 1 'not at all' to 5 'extremely'. Emotions were elicited in Bangladesh and Vietnam where participants watched a neutral video in the control. Dashed-lines indicate 95% confidence intervals around the means in each group.

Table 1. Main treatment effect and interactions with relocation beliefs

	Dependent variable: prosociality (z-score)			
	Average treatment effect		Heterogeneous effects	
	(1)	(2)	(3)	(4)
SLR information (= 1)	0.14** (0.06)	0.14** (0.06)	0.21*** (0.08)	0.22*** (0.08)
Relocate belief (= 1)			0.17* (0.10)	0.20* (0.11)
SLR information × relocate			−0.22* (0.13)	−0.23* (0.14)
Constant	−0.08* (0.05)	−0.57*** (0.17)	−0.14** (0.06)	−0.64*** (0.18)
Country fixed effects	No	Yes	No	Yes
Controls	No	Yes	No	Yes
Observations	1,047	1,039	1,047	1,039
R^2	0.00	0.05	0.01	0.05
Adjusted R^2	0.00	0.03	0.00	0.04

Notes: The dependent variable is the standardized (z-score) measure of prosociality in all four models. In columns (2) and (4) we include a set of dummies for each country in which we conducted the experiments to account for unobserved differences. The relocation belief is based on (i) whether participants think they must relocate because of SLR impacts in the next five years or not (Solomon Islands) or on the likelihood of whether they must relocate because of SLR impacts, ranging from 0 'impossible' to 10 'absolutely certain' (Bangladesh & Vietnam). We categorize their relocation belief as one if they choose 10, and zero otherwise. Alternative configurations including '8' or '9' yield lower estimates. In columns (2) and (4), to account for some slight imbalances and generally increase precision of our estimates, we include the following covariates: gender, marital status, age, education, household size, income, patience, trust, risk aversion, and place attachment. Estimates are from OLS regressions with robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). The full regression tables, additional robustness checks, and estimates for each study site are reported in Supplementary Tables S12 to S14.

with antisocial behaviors. Results from study 2 also show that people react with prosociality when faced with slow-onset hazards caused by rising sea-levels, which have been so far underexplored. Bringing the studies on past and future disaster together, using similar incentivized methodologies in four different countries, highlights the potential universality of the prosocial response to natural hazards. Such a universal response can point toward the evolutionary origins of prosociality (Melis, 2018). Models of cultural group-selection and supporting empirical evidence have shown the role of environmental conditions explaining variation in prosociality (Gintis et al., 2003; Henrich, 2004). Our study suggests that in uncertain or hazardous environments, groups showing high prosociality could have higher evolutionary fitness.

Looking beyond ultimate evolutionary interpretations of the data, there are several more proximate factors that might explain the increase in prosociality. First, the increase in prosociality in study 1 is potentially related to the experience of kindness from other people made during or after Haiyan which outweighed any negative experiences. This became clear in the open-ended comments that participants made as part of the recall exercise. A post-disaster experience marked by acts of kindness might be specific to the Philippines and its 'culture of disaster' (Bankoff, 2003) as a response to living in an extremely hazardous environment. Religiosity supporting collective coping mechanisms and norms of mutual support might be stronger than in other societies. Second, prosociality is not only based on empathy and altruism but also has an important strategic component, for example, in informal risk sharing networks where people might not only help out of altruism but because they expect to also receive help if they are in need. In such a situation it is beneficial to have a reputation for being prosocial, showing others that one can count on their support. Thus, acting prosocial signals positive

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intentions (Gintis et al., 2001; Hardy & Van Vugt, 2006). Considering the above mentioned 'culture of disaster' people might have internalized signaling prosocial behaviors even in anonymized settings.

Our findings suggest that people react to disasters by increasing prosociality, at least in the short-run. It might be that people switch to 'survival mode' and thus more selfish behaviors in the future once everyone realizes that displacement becomes inevitable (Frey et al., 2010a, 2010b, 2011) or that urban dwellers with less initial bonds to their neighbors react differently. To better understand conditions when and how people start to act based on undesirable future states, research might look into further real-world examples of declining time horizons, such as losing one's job or learning about a severe illness (Schweizer & Szech, 2018). The reliance on prosocial behavior observed in our studies can be seen as an evolutionary well-adapted coping strategy for catastrophic events, as in situ adaptation is often preferred even under difficult circumstances (Esteban et al., 2019; Jamero et al., 2017). Such collective in situ adaptation responses are an important part of the community's historical legacy and could influence future adaptation pathways (Fazey et al., 2016). For example, case studies find that people who experience frequent flooding due to land subsidence are surprisingly reluctant to move (Esteban et al., 2019), even when nearby resettlement is available (Jamero et al., 2017). Jamero et al. (2017) show that despite more than 100 days of flooding per year, Filipino islanders have managed to continue their daily lives by adapting to the situation together. For now, this seems to work but with climate hazards increasing in frequency and severity, this might leave people more vulnerable in the long-term. Therefore, we believe it is important to also consider the potential risk of maladaptation in certain contexts (Magnan et al., 2016) due to increased prosociality, among other important aspects of adaptive capacity such as place attachment (Adams, 2016), preferences (Choquette-Levy et al., 2021), or culture more generally (Adger et al., 2013).

Applying the concept of path-dependency based on historical legacies to adaptation can help understand how climate hazards define adaptation trajectories (Fazey et al., 2016; Haasnoot et al., 2013). Thus, by responding to climate hazards with collective in situ adaptation, communities could be committing to pathways that limit future adaptation options. In the long-term, people could lose their initial capacity to migrate and face the risk of becoming immobile (Koubi et al., 2022) or even displaced (Bell et al., 2021; Steimanis et al. 2021). Similarly, collective recovery from a typhoon not only reduces financial pressure, but emotional support by family, friends, and neighbors reduces anxiety and worry about future events. Helping others and collectively rebuilding homes strengthens bonds, social relations, and possibly creates an identity of 'stay and fight' and, thus, a false sense of security. These are all factors which suggest that people may remain in hazardous regions exposed to multiple threats that undermine their livelihoods. Certainly, further research is needed to investigate under what circumstances, and for which groups an increase in prosociality leads to an undesirable adaptation pathway. The presence and maintenance of good social relationships may also enable timely collective relocation (Sherbinin et al., 2011) or support for people who want to move.[9] However, migration is not an option everywhere or for everyone and, if available, does not necessarily reduce vulnerability to climate hazards (Vinke et al., 2020; Warner & Afifi, 2014). Overall, thus, the fact that people respond in a predominantly prosocial manner provides hope that communities can emerge stronger from disasters and collectively prepare for future climate hazards if they receive appropriate policy support tailored to local conditions. This potential increase in adaptive capacity should not be misunderstood to mean that affected communities do not need outside support.

[9]We thank an anonymous reviewer for highlighting these aspects of prosociality.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/sus.2022.9

Acknowledgments. We are grateful to Matthias Mayer, Abu Siddique, and Max Burger for assistance in Bangladesh and Vietnam; Lukas Kampenhuber for collecting data in the Philippines; and all participants that agreed to take part in the experiments and surveys. We are especially grateful to our local research assistants in all study sites who significantly contributed to the success of the data collection and thereby the whole project. We thank Bernd Hayo, Esther Blanco, Tobias Vorläufer, and seminar/conference participants at the ESA World Meeting in Berlin, WOW6 in Indiana, Experimental Workshop for the Environment in Heidelberg, Beijing Normal University, Karlsruhe Institute of Technology, Kochi University, Luxembourg University, and TU Munich for helpful comments.

Author contributions. IS: conceptualization, data analysis, writing – reviewing and editing, project administration; BV: conceptualization, writing – reviewing and editing, supervision, funding acquisition.

Financial support. The Robert Bosch Foundation (Grant number 32.5.F082.0001.0) funded the research.

Conflict of interest. The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Research transparency and reproducibility. The replication package including the treatment videos, data, and scripts (STATA) to reproduce the tables and figures reported in the manuscript and supplementary online materials are available on GitHub (https://github.com/IvoSteimanis/prosociality_hazards) and zenodo (https://zenodo.org/record/6343977).

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