Do Voters Prefer Relief over Preparedness? Evidence from Disaster Policies in Malawi

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Abstract

Growing evidence suggests that voters reward politicians for spending on disaster relief but not disaster preparedness. Yet, we know little about the mechanisms that underpin this pattern. I propose that voters value effective preparedness as much as effective relief. However, voters have pessimistic expectations about the effectiveness of preparedness policies compared to relief policies. I test the mechanisms using a conjoint experiment in rural Malawi where participants choose between two hypothetical candidates randomly varying attributes about their prevention and relief policies. I find that respondents reward relief efforts over preparedness efforts, but they value effective preparedness similarly to effective relief. Additionally, respondents are more likely to reward preparedness efforts if they lead to effective outcomes. These findings have important implications for the design of disaster policies.

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1 Introduction

While the number of weather-related disasters like floods or droughts is rising, government investment in public preparedness policies that could mitigate their effects remain limited compared to relief transfers (Clarke and Dercon, 2016).[1] This problem is especially salient in low-income countries where the population is particularly vulnerable to natural disasters' effects (Hallegatte et al., 2016). One explanation for this underinvestment suggests that voters provide the wrong incentives to politicians by rewarding them for relief, but not preparedness, expenditure. Previous studies found a positive association between relief spending and re-election rates for incumbents, but a weaker or non-association for preparedness spending.[2] But why do voters reward relief over preparedness policies?

I argue that voters value effective prevention similarly to effective relief, but they have pessimistic expectations about the effectiveness of preparedness policies. That is, voters expect that preparedness efforts are less likely to deliver the promised outcomes than relief efforts. In settings where it is difficult for voters to ascertain how policy efforts affected outcomes, pessimistic expectations will lead to low electoral returns for incumbents. This is the case for preparedness policies, as it is difficult for voters to evaluate the extent to which efforts like preparedness plans mitigated destruction.[3] The effectiveness of relief efforts is easier to evaluate as voters are the direct recipients.

Empirically, this paper relies on a conjoint experiment embedded in a face-to-face survey fielded in Southern Malawi to measure voter preferences for disaster policies. The region is an ideal case to study disaster policy preferences because it experiences an annual wet season that leads to severe floods almost every year, and politicians are always involved in disaster preparedness and relief. The experiment informed participants about a hypothetical scenario in which a flood occurred but indicated that two candidates running for MP implemented different prevention and relief policies. By randomly manipulating features of contrasting disaster policies, the conjoint design allows me to identify the marginal causal effect of each candidate's policy choice on voters' support. Furthermore, conjoint experiments have been shown to reduce social desirability bias (Horiuchi et al., 2022), making them ideal for studying preferences in sensitive issues like disaster policies. To study voter expectations about policy effectiveness, a first set of attributes informed participants if a candidate exerted effort to implement a prevention and relief policy, while a second set informed participants of the efforts' efficacy. Because attribute levels are randomized, I can infer voter expectations from the marginal effect of policy efforts.

[1] Healy and Malhotra (2009) use data from the United States and estimate that US$1 invested in disaster prevention translates into roughly US$15 investment in mitigated future damages.

[2] See Healy and Malhotra (2009); Bechtel and Hainmueller (2011); Gasper and Reeves (2011); Cole et al. (2012); Gallego (2018); Cavalcanti (2018); Cooperman (2022). Importantly, most studies measure the electoral returns to relief and prevention spending *after* disasters have occurred. Therefore, the effects cannot be driven by voter uncertainty about the need for prevention policies.

[3] Preparedness and prevention measures include planting vegetation to retain excess water, building channels to divert water from flooding, terrace slopes to reduce slope flow or the construction of dams. Preparedness plans typically include locating safe places for each type of disaster, determining evacuation routes, and preparing disaster supply kits.

The results broadly support the hypotheses. First, I find evidence that voters value relief efforts over preparedness efforts. Yet, respondents value preparedness similarly to relief if it effectively mitigates disaster damage. Strictly speaking, this paper cannot specify why voters hold pessimistic expectations for preparedness policies. However, I found suggestive evidence that respondents update their beliefs about the effectiveness of preparedness policies. As the conjoint experiment was conducted over six rounds, I show that respondents are more likely to reward preparedness efforts if they repeatedly observe preparedness efforts leading to effective outcomes.

The paper contributes to several strands of literature. In a narrow sense, the findings add to the discussion on the electoral incentives for natural disaster policies (Healy and Malhotra, 2009; Gasper and Reeves, 2011; Bechtel and Hainmueller, 2011; Cole et al., 2012). The findings do not invalidate previous findings but instead call for a more nuanced interpretation of the underlying mechanisms. Some authors have interpreted the positive association between relief spending and incumbent voting and the lack of association between preparedness spending and incumbent voting as evidence that voters prefer relief over prevention. However, Gailmard and Patty (2019) have shown formally that voters would reward relief efforts over prevention efforts if they were uncertain about the effectiveness of prevention.[4] This paper finds empirical evidence that supports this view. Voters seem to have more pessimistic expectations about the effectiveness of preparedness efforts compared to relief efforts, leading them to value relief efforts over preparedness efforts. However, voters value effective prevention similar to effective relief. While this paper remains agnostic about the sources of the pessimistic expectations, I find no evidence they are driven by the corruption of politicians.

2 Voter Expectations About Relief & Preparedness Policies

It is a fundamental question as to what kind of policies voters prefer. In a seminal paper, Healy and Malhotra (2009) study this question by examining voter reactions to government performance regarding natural disaster policies in the US. Although politicians have no control over the occurrence of a disaster itself, they can influence outcomes through their prevention and relief policies. Prevention and preparedness policies typically take the form of public goods such as dams and irrigation systems, flooding zones, and evacuation plans.[5] Relief policies typically provide private goods, such as drinking water, food, or temporary housing, in response to a disaster.[6]

[4] In their model, prevention spending is a bad signal for voters about the quality of politicians because voters are less informed than politicians about the need for prevention and because politicians can be "corrupt" in the sense that they can privately benefit from prevention spending.

[5] They are public goods because, within the catchment area, nobody can be excluded from the benefits of those goods and its consumption by one citizens does not prevent simultaneous consumption by other consumers.

[6] The provision is rival in the the sense that the consumption by one victim reduces the amount available for others. The distribution of relief goods is also excludable. In fact, it is a common complaint by citizens that disaster relief is selective and excluded subsets of the affected populations.

Healy and Malhotra (2009) show that citizens reward incumbents for relief but not prevention spending. In particular, the study only finds a significant association between incumbent vote share and relief transfers to individual voters but not for collective relief or prevention. Cavalcanti (2018) finds similar evidence studying droughts in Brazil. While voters rewarded the President's party for relief spending and preparedness spending after a drought, the former effects are larger in magnitude and more robust to different specifications. This suggests that voters are less likely to reward previous preparedness even when a disaster subsequently happens. The paper also shows that voters are more likely to vote for an incumbent mayor aligned with the central government, arguing that voters do so because they expect better access to private relief transfers. Several studies support the proposition that voters reward incumbents for relief spending. Gallego (2018) finds tentative evidence that local mayors in Colombia used the increased influx of aid after a disaster to target relief spending in the forms of private transfers and local public goods to buy votes. However, the study only finds significant effects for private transfers. Gasper and Reeves (2011) and Cole et al. (2012) show that voters punish politicians less for natural disasters if they provide effective disaster relief. Bechtel and Hainmueller (2011) find that the positive effects of relief spending can last several years. In line with these findings, Cooperman (2022) observes that Brazilian mayors issue drought declarations, triggering relief payments, in the run-up to elections.

A key limitation of the existing empirical studies is that they typically rely on observational research designs that use variations in incumbents' re-election rates and disaster prevention or relief. However, do these correlations reject the underlying policy preferences of voters? More fundamentally, why would voters prefer direct relief transfers over public prevention? This study explores one mechanism: voters might prefer relief over prevention because they expect prevention policies to be less effective.

2.1 Expectations About Preparedness and Relief Policies

It is a common assumption that voters' political behavior is based on their preferences, their expectations about the world, and some external constraints.[7] In particular, voters form subjective beliefs (expectations) about how policy efforts translate into desired outcomes.[8] Thus, voters face the problem of inferring the welfare consequences of a candidate's disaster policy efforts given their expectations. Assuming voters attempt to maximize welfare, they must consider how specific policy actions will influence outcomes. When incumbents peruse policies, systematic differences in voters' expectations about the effectiveness of investment in prevention compared to investment in relief would lead to a difference in candidates' support.

There are several reasons why voters might hold more pessimistic expectations about the effectiveness of prevention efforts than relief efforts. First, the benefits of prevention policies will only materialize once a disaster has occurred. However, this type of uncertainty should be less important when disasters fre-

[7]See Gintis (2014) for a general discussion.

[8]I define an expectation as a probability distribution that maps policy efforts into outcomes.

quently occur. In these contexts, voters can be certain that well-implemented prevention policies would be beneficial. However, voters might doubt that politicians can tap into the state capacity needed to implement prevention policies, while relief policies, such as payments and handouts, are easier to implement (Besley and Persson, 2009). Lastly, voters might be concerned that prevention policies leave more room for corruption and discretionary allocation than relief policies (Gailmard and Patty, 2019).[9] While there is also some empirical evidence on the misuse of public spending in the context of disaster relief (Garrett and Sobel, 2003; Gallego, 2018), prevention spending may be even more prone to corruption, as it is less visible and harder to monitor. These last two mechanisms might be most prevalent in young democracies with widespread corruption and low state capacity. Regardless of the exact mechanism, there are two predictions we can derive if voters expect prevention efforts to be less effective than relief efforts.

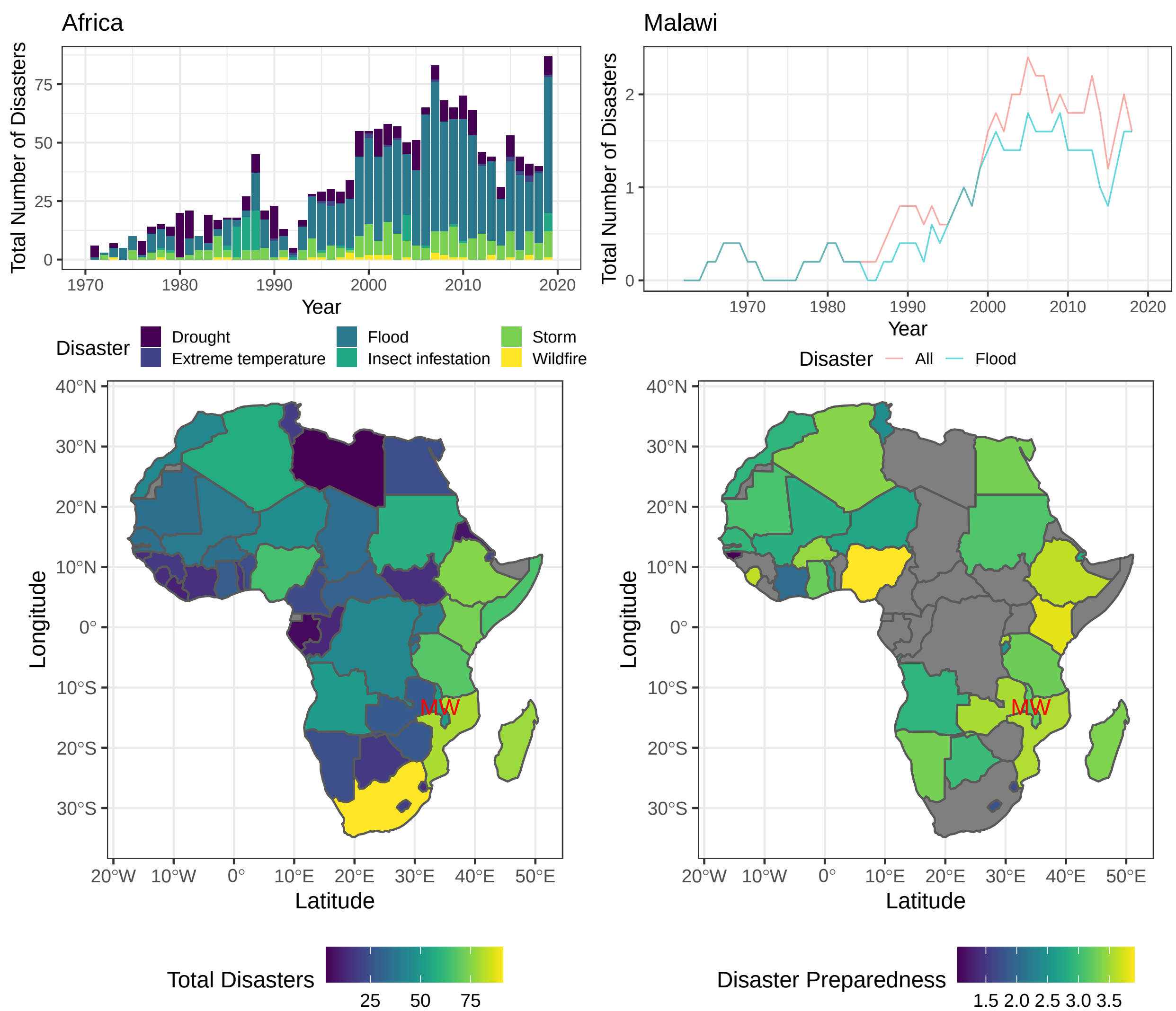
- H_{1a}: Voters will be more likely to reward candidates for relief efforts than prevention efforts.
- H_{1b}: Voters will be indifferent between candidates who provide effective relief and candidates who provide effective prevention.

3 Background and Case

Malawi provides an important setting to study voters' natural disaster policy preferences. Natural disasters have become increasingly frequent across Africa over the last three decades. As shown in the upper left panel of 1, there has been a rapid increase since 1990; flooding is the most common type of disaster. The upper right panel of Figure 1 shows that Malawi is no exception to this trend and frequently suffers from floods, droughts, and harvest failures. Leading up to this study in 2018, the country suffered a flood almost every other year. Pauw et al. (2011) use data prior to 2010 and estimate that at least $1,7\%$ of Malawi's gross domestic product (GDP) is lost yearly because of droughts and floods. The population is particularly vulnerable to natural disasters because 80% of people live off agricultural income. This is particularly salient in the Southern Shire basin, the focus of this study, which experiences annual flooding caused by seasonal rainfall between November and January. In 2015, the region experienced the highest seasonal rainfall ever recorded, damaging about 89,000 hectares of land and 500,000 houses, affecting 1,000,000 people, leaving 230,000 displaced, and killing 106. The flood led to massive destruction of crops, devastated agricultural production, and destroyed social infrastructure – specifically, schools, health facilities, and housing (PDNA-Report, 2015).

[9] Gailmard and Patty (2019) assume that voters are informed about the prevention measures, occurrence of a disaster, and the relief measures. However, voters are uncertain about the initial probability of a disaster and if politicians are corrupt or not. The assumption is that corrupt politicians would invest in prevention even if it is not beneficial to the voter because they can engage in rent-seeking. As a consequence, investment in prevention policies is a signal for corruption and voters will be less likely to reward it. In this paper, the level of corruption of a candidate and the probability of a disaster in known to the voter.

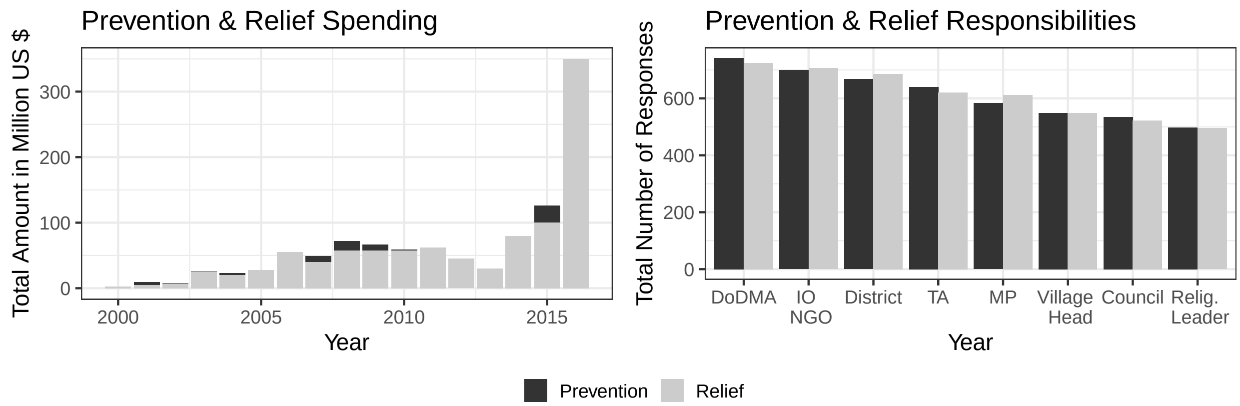
Figure 1: Natural Disasters in Africa (by year and country) 1970-2020 and Disaster Preparedness (by country)



Notes: Upper Panel: Natural disasters by year, data from EM-DAT, (Guha-Sapir and Hoyois, 2015). Data for Malawi is displayed as five-year moving averages. Lower Left Panel: Total number of natural disasters by country. Lower Right Panel: Data from the Adaptive Capacity Indicator 2 on disaster preparedness (average of 2007, 2009, 2011) from the Notre Dame Global Adaptation Initiative (ND-GAIN) (Chen et al., 2015). Location of Malawi is marked with MW.

With a total of 51 disasters between 1970 and 2020, Malawi ranks 12 out of 55 countries in the data. In terms of disaster preparedness, the country is average compared to other African states (see lower right panel of Figure 1). However, with disasters increasing, so has spending on disaster relief. The left panel of 1 depicts the increasing trend for relief spending in Malawi since the start of the century, culminating in roughly US$350 million spent after the flood disaster of 2015. By comparison, prevention and preparedness spending only constitutes a small fraction of relief spending each year. In fact, for some years, there are no reported prevention and preparedness projects.

Figure 2: Malawi Disaster Spending and Perceived Responsibilities



Notes: Left: Prevention & Relief Aid Malawi, 2000-2016; relief aid spending (2000-2016) is based on data from DoDMA and the National Resilience Strategy Report. Prevention aid spending (2000-2015) was based on data from Peratsakis et al. (2012), including disaster risk reduction and mitigation projects. To identify the year, I relied on the date when the aid agreement was signed. If the date is missing, I use the completion date. Right: Perceived Responsibilities; own survey data collected 2018; Based on the question: "In your opinion, which actors are responsible for disaster response and relief?" The graph displays the sum of respondents who chose "very responsible" for a given actor.

Elected officials play a key role in disaster prevention and relief (Kita, 2017). While the main authority for disaster preparedness and relief lies with the Department of Disaster Management Affairs (DoDMA), district commissioners and councils typically identify and distribute disaster relief.[10] Malawi is divided into 28 districts, each administered by a district council under the direction of a district commissioner appointed by the president. Each district council consists of elected councilors (one for each ward within the district), members of parliament (MPs), ex-officio members, and chiefs (traditional authorities-TA's).

While the formal responsibility for the provision of local public goods lies also with district councils (Chinsinga, 2005), MPs play a key role in providing local public goods, both formally and informally. Since 2006, MPs have had discretion over constituency development funds to implement development projects in their district (Ejdemyr et al., 2018). MPs also support community-level disaster prevention and relief by mobilizing resources through the constituency development fund (Kita, 2017), organizing resettlements[11], providing logistics for relief items, and facilitating post-disaster reports (Kita, 2017). The lower right panel of Figure 2 depicts survey evidence on various actors' perceived responsibility for disaster prevention and relief. In line with the expectations, most respondents see DoDMA as responsible,

[10] "The Department of Disaster Management Affairs (DoDMA) says it does not distribute disaster relief items but rather hand them over to District Councils based on reports from the District Commissioners, who in turn identify the needy beneficiaries." See: `https://www.nyasatimes.com/dodma-shifts-blame-on-dcs-for-alleged-accusation-of-selective-distribution-of-relief-aid-for-flood-victims-in-balaka/`

[11] For example, in the context of relief efforts in response to the 2015 floods, an article notes: "*As of Sunday, most people who were stranded had been rescued, but the government continues to monitor the situation. Meanwhile, government appreciates the willingness of people to move upland, thanks to discussions and negotiations with the people using chiefs and Members of Parliament.*" [ReliefWeb]

followed by the district commissioner, traditional authority, and MPs. Notably, international organizations and NGOs are the second most popular category. This is not surprising, given respondents also noted that most of the help came from international donors, followed, by a wide margin, by DoDMA, MPs, and the district commissioner (see Figure A2 in the Appendix).

While MPs play a key role in disaster prevention and relief, they are also reported as misusing their central position to engage in corruption and vote-buying, especially during the delivery of disaster relief. Using evidence from interviews with district-level governments and NGO officers, Kita (2017, 11) also noted: *"For the majority of cases, most MPs and councilors are seen to be more interested in realizing personal goals than the common good. With frequent disasters, humanitarian aid has been taken as a tool for vote-buying and bolstering clientelism."* One source of discretionary funding comes from disaster response funds, to which MPs can apply and are rarely rejected (Kita, 2017, 12).

To summarize, disasters, disaster policies, and the responsibilities of elected officials are salient to voters in Malawi. The frequency and magnitude of disaster events and the responsibilities voters assign to politicians also indicate that citizens recognize the need for preparedness and relief policies. Previous research further suggests that MPs are perceived as central and visible figures in disaster prevention and response. However, there is considerable variation in the degree to which MPs promote public well-being or pursue personal electoral goals. In this sense, southern Malawi is a likely case to find evidence for electoral rewards for disaster prevention.

4 Experimental Design and Data

I test my main hypothesis experimentally. While voters are usually uncertain if disasters will occur and what policy actions politicians have taken in preparation and response, I design a survey experiment that alleviates these information asymmetries by informing participants that a disaster has occurred, but that counterfactual MPs prepared and reacted differently. To identify voter support for candidates implementing different disaster policies, I use a full factorial voting conjoint experiment embedded in a face-to-face survey (Hainmueller et al., 2014). In the paired conjoint design, I randomly vary seven attributes of two political candidates running for MP regarding their prevention and relief policies, and respondents are forced to choose between them. Using this design, we can observe the effect of those manipulations on respondents' choice of who is elected.

The sample consists of 810 respondents from thirty-six villages in the districts of Nsanje and Chikwawa in southern Malawi. Regions are rather ethnically homogenous; Chikwawa and Nsanje are part of the Sena region (Robinson, 2016). Each of the 810 respondents evaluated six pairs of conjoint profiles, resulting in 4,860 contests and 9,720 profiles. The order of the seven attributes within each pair was also randomized. The data was collected in November 2018. The sample is particularly useful in studying disaster policy preferences, as different village subsets were affected differently by the 2015 floods.[12] The data collection was not linked to the 2015 flood, and the villages were drawn from a stratified random sample.[13]

4.1 Measuring Preferences for Candidates' Disaster Policies

The conjoint design allows me to evaluate the marginal effect of a large set of candidates' prevention and relief policy attributes on respondents' approval. The main outcome variable is a binary measure of Profile Choice: the respondent's answer to the question "Which MP would you vote for?". The treatments are a set of seven prevention and relief policy attributes randomly assigned across two hypothetical candidates. The list of attributes is shown in Table A3.

To study voter expectations about the effectiveness of prevention and relief policies (H1), I include different attributes for disaster *policy efforts* and *policy outcomes*. Typically, expectations are measured through subjective probabilities, asking respondents about the probability that certain actions will lead to certain outcomes (Manski, 2018).[14] I infer voter expectations about preparedness and relief policies from the marginal effect of policy efforts on candidate support. In particular, a first set of attributes informed participants if candidates invested low or high efforts into preparedness and relief policies. A second set of attributes informs participants if the prevention and relief efforts successfully changed welfare outcomes. Because each attribute is randomly assigned using a uniform distribution, high efforts are effective in roughly half the vignettes and ineffective in the other half. Thus, differences in voter support for policy efforts should be driven by voters' prior expectations about the likelihood of a policy translating into welfare outcomes, i.e., prior expectations about effectiveness. This measurement strategy assumes policy outcomes are a combination of candidates' efforts, competencies, and (un)lucky circumstances the candidates cannot influence:

$$Outcome = Effort + Competence + Luck \tag{4.1}$$

Table 1 displays the attributes and corresponding levels. I measure *preparedness efforts* by the time candidates invest in a disaster preparedness plan. I use disaster preparedness plans as they cover entire communities or regions and are a clear case of a local public good. Additionally, disaster preparedness and emergency plans were widely discussed in the aftermath of the 2015 floods.[15] I then measure effective

[12] See Figure A4 in the Appendix.

[13] For the details, please refer to Appendix E.

[14] For example, one could imagine a question like: "What do you think is the percent chance that a prevention/relief policy is effective?"

[15] As reported on by the Malawi News Agency on 30th of November 2015: "*Stakeholders in Mulanje have come up with a disaster preparedness plan of action to improve response and ease problems that arise in times of disasters. The action plan was discussed during a meeting organised by the United Kingdom's Department for International Development (DfID) on Thursday to map out an action plan for the district*." Source: https://mwnation.com/mulanje-develops-disaster-response-plan/.

preparedness using a binary attribute that captures the extent to which a preparedness policy mitigated the negative impact of a disaster. Next, I measure *relief efforts* by the time candidates allocated to coordinate disaster relief. While MP's are not officially in charge of disaster relief, they are often involved in oversight and coordination. Finally, I measure *effective relief* coordination by donations of MP's to a village. I choose this measure because MPs often coordinate with other authorities to raise funds and target supporters.[16]

In addition, I include several other disaster-policy attributes that voters might consider. First, rather than providing relief themselves, candidates might ask third parties for resources. Previous research found that foreign aid can signal government competence (Winters et al., 2018) and increase incumbent support (Springman, 2022). Therefore, I rely on an attribute indicating that a candidate asked an NGO or International Organization for material benefits. Second, faced with natural disasters, politicians often resort to visits and symbolic actions to signal that they care about their constituencies (empathy) and take their opinions and problems seriously (Lazarev et al., 2014). Therefore, I include an attribute in which candidates visit the disaster, talk to victims, and declare their solidarity. Third, candidates may not always allocate relief or prevention according to need. Instead, they may target resources to supporters for patronage, use resources to buy votes, or embezzle funds for personal use. Findings from Francken et al. (2012) and Eichenauer et al. (2020) provide evidence that political considerations can influence the allocation of relief aid. Accordingly, I include indicators for embezzling resources for personal use (corruption) and handing cash to buy votes (vote-buying). Beyond disaster policies, voters also select politicians based on gender, political affiliation, or ethnicity (Robinson, 2016). To mitigate these concerns, I try to hold those factors constant by introducing candidates with the same or similar characteristics but who differ on natural disaster policies:

'This section attempts to understand what kind of candidate you would support in an election. We will show you profiles of hypothetical local candidates running for MP and how they handled a recent flood. Imagine that you live in a different district similar to yours in this region that was affected by a flood and that you were voting for candidates in elections. Here are the two candidates who are running against each other. You should tell us whom do you prefer. They are both men, have the same age (around 50), and come from the same tribe. However, there are important differences between the two:"

4.2 Estimand and Estimation

The main estimand is the Average Marginal Component Effect (AMCE). The AMCE measures the marginal effect of a given attribute of a conjoint profile on respondents' support for the overall profile relative to a baseline, averaged over the joint distribution of other attributes (Hainmueller et al., 2014). The

[16] As Kita (2017) describes: *"Respondents cited numerous cases where councilors or members of parliament [...] diverted recovery funding from one area to another; presented developmental issues as disasters so as to benefit from humanitarian finance; added names of relations or supporters to lists of beneficiaries when they were not affected; or produced parallel lists of affected people to benefit from relief supplies".*

Table 1: Conjoint Experiment: Exemplifying Profiles of Candidates, as shown to Respondents

Factor (Z)	MP 1	MP 2
Effort		
Preparedness	(0) *Did not* put a lot of work into disaster preparedness plan	(1) *Did* put a lot of work into disaster preparedness plan
Relief	(0) *Did not attend* meetings to co-ordinate disaster relief	(1) *Did attend* meetings to co-ordinate disaster relief
Effective		
Preparedness	(0) Preparedness plan was of *low quality* and did not limit the damages from the flood	(1) Preparedness plan was of *high quality* and did limit the damages from the flood
Relief	(0) *Did not donate* funds to the village	(1) *Did donate* funds to the village
Other		
Ask	(0) *Did not ask* for help from funders	(1) *Did ask* for help from funders
Visit	(0) *Did not visit* the disaster site	(1) *Did visit* the disaster site
Corruption	(0) No record of corruption	(1) Convicted of corruption
		(2) Convicted of vote buying
Choice		

baseline of a given attribute is always 0.[17] As Abramson et al. (2022) show, the AMCE combines the direction and strength of preferences about individual profile attributes and does not necessarily present majority preferences. Instead, the AMCE can be interpreted as the average marginal causal effect of a given attribute on a candidate's expected vote share, given a particular randomization distribution (Bansak et al., 2022).[18] First, I estimate the AMCE using an OLS regression with heteroskedasticity-robust standard errors (see Equation 4.2). The standard errors are clustered at the level of the individual participant:

$$Y_{im} = \sum_{j \in Z} \beta_j Z_{ij} + \gamma_m + \epsilon_i \tag{4.2}$$

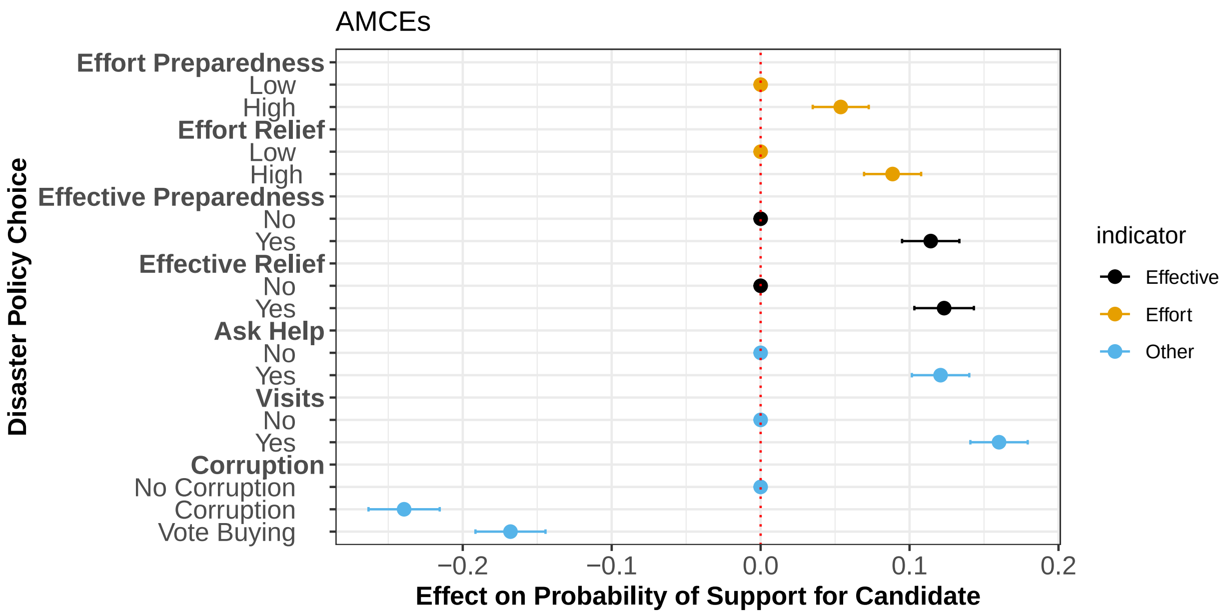
where Y is the chosen candidate policy profile, j indexes the treatment level, γm indicates individual fixed effects, and Z is a set of indicators corresponding to the attributes, here Z = {Preparedness Effort, Relief Effort, Preparedness Effective, Relief Effective, Ask, Visit, Corruption}.

[17] I use a uniform distribution when randomizing over levels of factors. Because attributes are randomly assigned, the given attribute level and attribute baseline will have, in expectation, the same distribution for all the other attributes.

[18] I employ this interpretation in the subsequent analyses because Malawi has a plurality systems and MPs often win with less than 50% of the vote. Therefore, marginal effects are informative.

5 Empirical Findings

Figure 3: Main Results



Notes: Beta coefficients from OLS regression with robust standard errors in parentheses. Standard errors are clustered at the individual level. Horizontal lines indicate 95% confidence intervals. The baseline is always the (0) level of the given attribute.

Figure 3 displays the ACMEs for the complete sample. Several findings can be noted. First, voters value candidate relief efforts over preparedness efforts. While the confidence intervals of the two coefficients overlap, the linear hypothesis test reveals that both are statistically different at the 0.05 level. Second, effective prevention policies that mitigated destruction are rewarded equally to post-disaster relief spending. This is supported by insignificant differences in the linear hypothesis test[19] Third, voters value not only material benefits but also personal visits and solidarity. Personal visits by the candidate to the disaster site have the strongest positive treatment effect, indicating a strong signaling effect. It is striking that asking international actors for help is valued equally to providing actual relief. These effects are likely driven by context, as international actors' aid is the primary source of relief.[20] Fourth, the strongest negative predictor for vote choice is the embezzlement of humanitarian aid and vote-buying. Notably, voters react negatively to the embezzlement of aid for private use (corruption) but are less sensitive to vote-buying. The magnitude of the vote-buying effect is relatively small given the treatment's strong wording ("convicted for vote-buying"). Moreover, the effects are not symmetrical; the embezzlement of aid (corruption) is more harshly punished than the delivery of benefits is rewarded. We can conclude that citizens prone to frequent disasters do not have a preference for vote-buying. While there is some heterogeneity

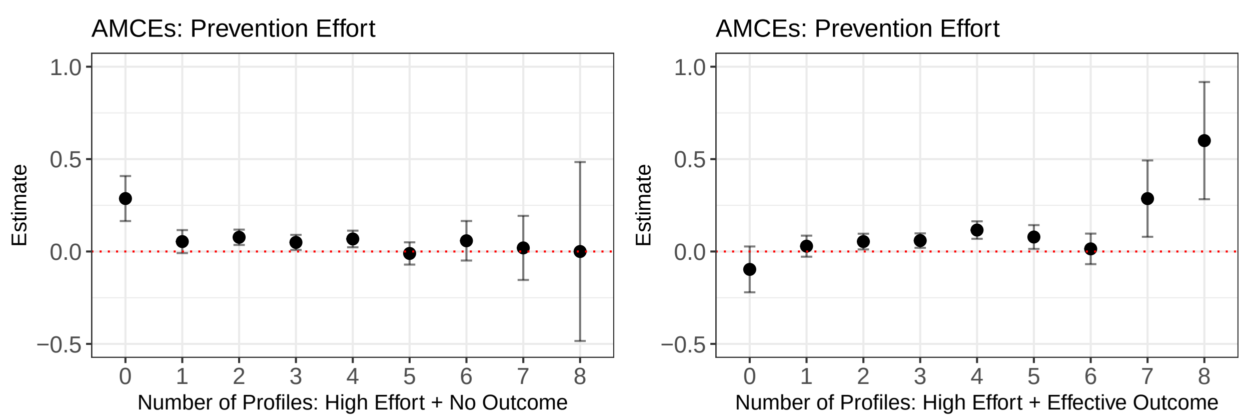
[19] See Tables A6 and A7 in the appendix for the formal analysis of the linear hypotheses.

[20] See Figure A2 in the Appendix.

across different rounds of the experiments, the patterns are robust and emerge in both earlier and later rounds.[21]

In conclusion, I can reject the null hypotheses for H_{1a} and H_{1b}. Pessimistic expectations about the effectiveness of preparedness seem to drive policy support. I find that voters value relief efforts significantly more than preparedness efforts. If preparedness is shown to effectively mitigate disaster outcomes, voters value it similarly to effective relief provision. The source of these pessimistic expectations can be manifold. It is worth noting that none of the interaction effects between any attributes are statistically significant (see Table A8 in the Appendix). Importantly, I find no significant interaction between corruption and preparedness efforts, suggesting that the low returns for preparedness are not driven by voter beliefs that candidates are corrupt and, therefore, may misallocate funding. Alternatively, these expectations might result from a learning process whereby voters did not observe politicians engaging in effective prevention efforts in the past but did observe politicians linked to effective relief outcomes.

Figure 4: Marginal Effect of Prevention Effort Conditional on Effectivity.



Notes: Beta coefficients from OLS regression with standard errors in parentheses. Vertical lines indicate 95% confidence intervals. The baseline is low levels of prevention effort.

To provide further evidence on the mechanism behind the main results, Figure 4 plots the AMCE of prevention efforts (Y) along different levels of effectiveness (X). I proxy the perceived effectiveness by the number of high-effort and effective outcome profile combinations a respondent saw over six profiles.[22] Descriptively, we can see in the left panel that the marginal effect of prevention effort is decreasing in the number of ineffective effort profiles a respondent saw throughout the experiment. The right panel shows that the trend is reversed if we condition the marginal effect on the number of profiles that showed a high effort and effective outcome combination. Therefore, we can conclude that respondents interpret

[21] See Figure A9 in the appendix.

[22] Because each factor is randomly assigned to two MP profiles in each of the six rounds, a respondent could–in theory–see this combination 12 times at most, each of the six rounds for both candidates. In the data, the combination occurred at most eight times.

prevention efforts with respect to effectiveness. The results provide some suggestive evidence that voters hold a priori pessimistic expectations but that these expectations may change if voters observe enough successful prevention policies.

5.1 Limitations and External Validity

Before reflecting on the substantive implications of the findings, I discuss the generalizability of the study with respect to: (1) information about disasters policies, (2) contexts where disasters are less frequent, (3) contexts with different levels of corruption and state capacity.

First, the research design did not allow for information asymmetries. Voters were perfectly informed about the occurrence of the disaster, the candidates' policy actions, and the subsequent outcomes. However, voters are typically uncertain about the probability of disasters and may lack information on the details of disaster policies. Therefore, the results should be generalizable to settings where voters have access to information about prepardness and relief policies.

Second, the study was conducted on a sample frequently exposed to disasters. Therefore, these results may not be generalizable to other populations. However, the results should resonate well in contexts where disasters happen frequently. Southern Malawi is a context where voters can be certain that a disaster will happen in the future but are unsure about the exact timing and scale. In this sense, it would be highly likely to find voter support for effective preparedness policies. However, at the same time, Malawi lacks strong state capacity to implement public policies, making it more likely that voters hold pessimistic beliefs about the effectiveness of policy measures.

Third, the results should replicate well in settings with similar levels of corruption and state capacity. In theory, both factors influence the degree to which voters hold pessimistic expectations about the effectiveness of preparedness policies.

6 Conclusion

Faced with an increasing number of natural disasters around the world, government action is central to ensuring citizen welfare. The issue is especially important for the world's poorest regions, which are particularly vulnerable to natural disasters. To mitigate future calamities, politicians must invest in disaster preparedness. However, instead, politicians often rely on relief payments. To date, we lack empirical evidence as to whether the underinvestment in prevention reflects voters' preferences for disaster policies and, if so, what drives these preferences. Empirical evidence is especially rare for developing democracies.

This paper contributes to filling this gap by studying how pessimistic expectations about the effectiveness of preparedness efforts undermine support for preparedness policies and how negative economic shocks due to disasters can increase the demand for relief. The paper presents novel causal evidence using a survey experiment in Southern Malawi. Consistent with previous findings (Healy and Malhotra, 2009), I show that voters reward candidates for relief efforts over preparedness efforts. However, if voters know that a candidate's preparedness policy positively influenced outcomes, they value it similarly to effective relief.

The findings do not invalidate previous observational findings but instead call for a more nuanced interpretation of the underlying mechanisms. Contrary to conventional wisdom in the literature, I find no evidence that voters generally reward relief over collective preparedness. Rather, the findings suggest that voters value effective preparedness but hold pessimistic expectations about the effectiveness of preparedness policies. Pessimistic expectations will lead to low electoral returns for incumbents whenever it is difficult for voters to link preparedness efforts to welfare outcomes. This is likely in the context of preparedness policies as it is difficult for voters to evaluate to what extent efforts like preparedness plans mitigated destructions from disasters. One reason could be that insufficient preparedness in the past undermines future investment. In particular, low expectations could be self-fulfilling if they lead politicians to invest less in preparedness, making it less likely that citizens observe successful preparedness policies, thereby leading to low expectations.

There are several tasks for future research. First, to better understand how to increase public support for preparedness policies, future research should explore voters' beliefs concerning the effectiveness of prevention policies. As Gailmard and Patty (2019), emphasized, informing citizens about the general efficiency gains of prevention vis-à-vis relief is likely insufficient if there is asymmetric information about the need for and benefits from prevention in concrete cases. To change voters' beliefs about the effectiveness of preparedness spending, they must be convinced that preparedness will be effective in their context. Using a survey experiment in the US, Bechtel and Mannino (2021) show that information about the relative payoffs of preparedness can lead respondents to update their stated preferences. However, in contexts of low state capacity and widespread corruption, where voters have little experience with a functioning state, information campaigns might be insufficient to change those beliefs and influence voting behavior (Dunning et al., 2019). Direct observation of well-implemented prevention projects could be more successful in changing attitudes. Future research should study how exposure to successfully implemented prevention policies influences voter preferences and positive electoral returns for those who implement such policies.

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Appendices

A Changes to the Pre-Analysis Plan

- Hypothesis: The effectiveness hypothesis (H1) was not pre-registered.
- Analysis: Marginal effect of preparedness efforts conditional on effective preparedness outcomes was not pre-registered
- Hypothesis: To analyze H2, I pre-specified to also test heterogeneous effects on ACMEs depending on the distance to the flood. However, as we can see in Figure A8 in the Appendix, the pre-specified distance measure is a bad predictor of reported economic losses, the main concept of interest. Therefore, I focus on the effect of self-reported economic losses due to the disaster. Results are in section H.

B Ethics

This research seeks to maximize welfare for Malawian society while minimizing risk to participants in the study. The survey and conjoint experiment asked participants about their experience with natural disasters and preferences for natural disaster policies. Given the sensitivity of natural disasters, ethical questions concerning the participants and study team were important.

B.1 Impacts on Political Processes

The conjoint experiment was designed with reference to actual disaster policies from the political context. However, the scenarios were explicitly hypothetical. The main objective was to measure voter preferences and beliefs and not to influence those. Therefore, I do not expect any impacts on the political processes.

B.2 Trauma and Harm

I tried to minimize and monitor the risk of re-traumatization. Because the study was focused on the economic consequences of natural disasters, I shortened a standard disaster questionnaire and only included relevant questions. Legerski and Bunnell (2010) reviewed literature on participation in trauma-focused research. They conclude that most studies have found that only a minority of participants experienced distress. However, the negative effect disappeared quickly over time, and a majority of participants experienced their participation as positive and beneficial to society.[23] Another concern is that the disaster prime might have induced psychological harm. However, this concern is ameliorated because of two reasons. First, the disaster prime was hypothetical and did not reference past events. Second, to monitor re-traumatization due to the disaster prime, I included a battery of questions on psychological well-being.

[23] The authors did note, however, that participants typically self-selected into studies which could have induced bias.

I found no evidence that the prime induced psychological harm to participants. Therefore, I expect that there was minimal, if any, physical, psychological, social, and economic harm to research subjects, assistants, or staff. Lastly, I expect the broader social impacts of the research process to be net positive, as they allow me to inform policymakers about citizen preferences and beliefs about natural disaster policies.

B.3 Institutional Review

The survey questionnaire was reviewed and approved by the Malawi Institutional Review Board (IRB) via the Institute of Public Opinion Research (IPOR), Malawi. The review by a Malawian board helped to ensure that the survey did not violate any local norms. In addition, this research followed the Swedish Data Services regulations and guidelines for research ethics.

B.4 Invitation and Compensation

The enumerators from IPOR were experienced professionals who had conducted interviews in Malawi before. Participation in the survey was completely voluntary, and participants were not offered any compensation. I did not offer any financial incentives to participate in the study because it might have pressured on respondents.

B.5 Informed Consent

Informed consent was sought at the initial contact with potential participants. Interviews for the survey began with an introduction to the project and assurances of confidentiality. Specifically, the script read: "*Good day. My name is [name of enumerator]. I am from the Institute of Public Opinion and Research, which is working with [Name of the Univsersity]. I do not represent the government or any political party. We are studying the views of citizens in Malawi about how the country is governed and the quality of life in your area. We would like to discuss these issues with you. Your answers will be confidential. They will be put together with other people we are talking to, to get an overall picture. It will be impossible to pick you out from what you say, so please feel free to tell us what you think. There is no penalty for refusing to participate. Do you wish to proceed?*" The consent was obtained orally. Oral consent is most appropriate in Malawi because much of the rural population is illiterate and the provision of written documents can cause unnecessary confusion and stress to participants. The interviews proceeded only after getting the consent of potential participants.

B.6 Deception

As the survey included a conjoint experiment, randomly selected subgroups of the sample were presented with different disaster policies of candidates. Yet these statements constituted no deception: they were explicitly hypothetical and constructed with reference to politicians' actual policies in the context.

B.7 Data

All data collected is kept anonymous and stored in encrypted files. I do not distribute any data with names or GPS coordinates. All data is retained on encrypted servers.

C Summary Statistics

Table A1: Summary Statistics Conjoint Experiment

Variable	N	Mean	Std. Dev.	Min	Pctl. 25	Pctl. 75	Max
Effort prevention	9660						
... Low Preparedness	4738	49%					
... Preparedness Coordination	4922	51%					
Effort relief	9660						
... Low Effort	4808	49.8%					
... Relief Coordination	4852	50.2%					
Effective prevention	9660						
... Low Quality	4919	50.9%					
... Preparedness Effective	4741	49.1%					
Visit	9660						
... did not visit	4784	49.5%					
... Relief Visits	4876	50.5%					
Honesty	9660						
... No Corruption	3226	33.4%					
... Corruption	3204	33.2%					
... Vote Buying	3230	33.4%					
Effective relief	9660						
... did not donate	4821	49.9%					
... Relief Effective	4839	50.1%					
Ask	9660						
... did not ask for help	4835	50.1%					
... Relief Ask	4825	49.9%					
Chosen Candidate	9660	1.506	0.5	1	1	2	2
contest	9660	3.5	1.708	1	2	5	6
candidate	9660	1.5	0.5	1	1	2	2
Choice	9660	0.5	0.5	0	0	1	1

Table A2: Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Pctl. 25	Pctl. 75	Max
prime	9660						
... control	4776	49.4%					
... treatment	4884	50.6%					
education	9660	3.22	1.397	1	3	4	7
farmer	9660	0.953	0.212	0	1	1	1
manipulation	9660	4.217	1.285	1	4	5	5
income	9648	3.422	0.829	1	3	4	4
age	9660	36.892	14.966	18	25	45	96
gender	9648	0.502	0.5	0	0	1	1
income2	9660	1.401	0.674	1	1	2	4
worried	9576	2.694	0.585	1	3	3	3
life2015	9660	1.873	0.333	1	2	2	2
incumbent_votingMP	9564	2.12	1.276	1	1	3	4
incumbent_votingVC	9504	2.085	1.242	1	1	3	4
interested_politics	9660	2.666	1.073	1	2	4	4
trust MP	9588	2.403	1.254	1	1	4	4
flood econ	9648	3.674	1.271	1	4	4	5
flood psych	9660	4.027	0.88	1	4	5	5
help	9660	1.21	0.407	1	1	1	2
satisfied help	9660						
... .	7632	79%					
... 1	732	7.6%					
... 2	768	8%					
... 3	348	3.6%					
... 4	180	1.9%					
disaster post2015	9660	1.376	0.485	1	1	2	2
distance flood	9660	5155.782	6900.428	0	292.318	11070.839	19982.901
elevation	9660	121.343	65.937	47	59	191	234
normalized intensity	9660	0.258	0.345	0	0.015	0.554	1
normalized intensity resscaled	9660	0.742	0.345	0	0.446	0.985	1
exposed self	9660	0.75	0.433	0	1	1	1
poverty	9660	0.877	0.328	0	1	1	1
hours	9660	0.369	0.17	0.171	0.295	0.407	2.627

D Background Flood 2015

Figure A1: Timing of Elections, Flood and Data Collections.

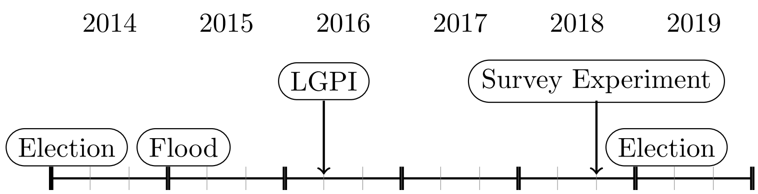
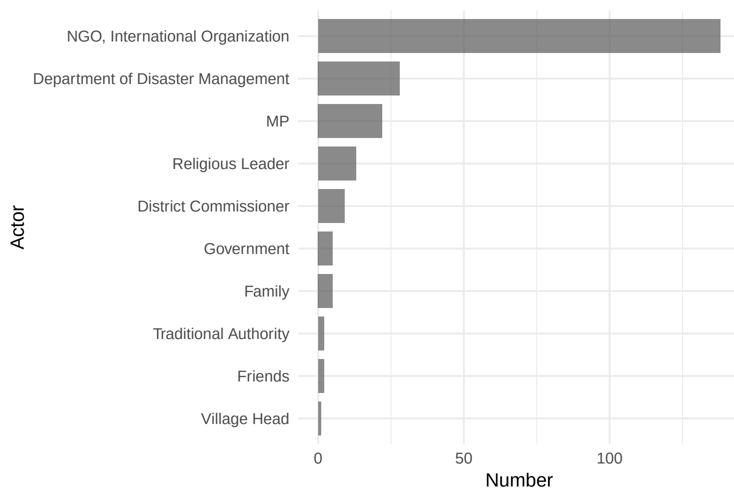
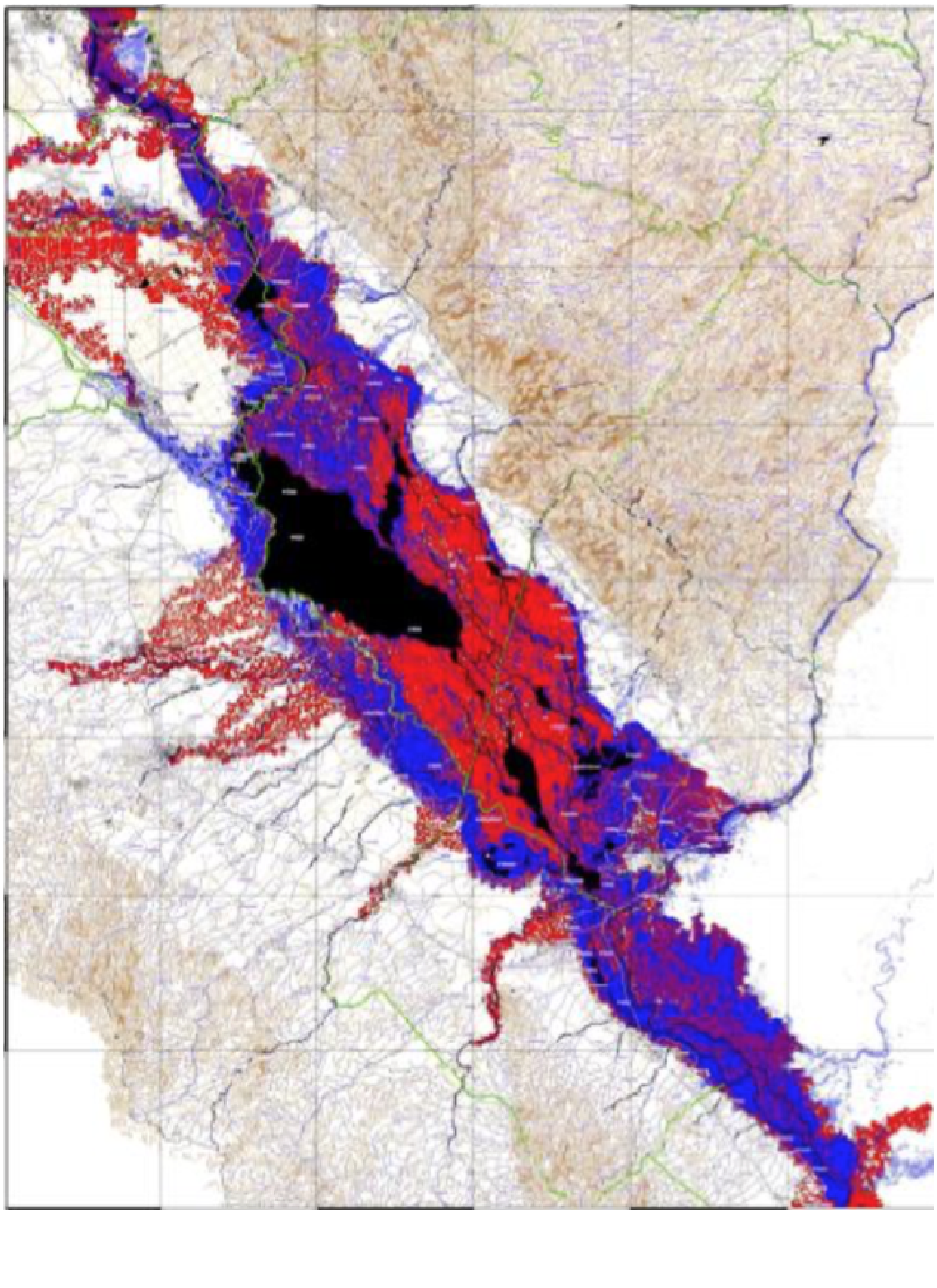


Figure A2: Help Received after the 2015 Flood



Notes: Data from survey in 2018. From whom did you receive help after the flood? (The district commissioner is the head of the district council.)

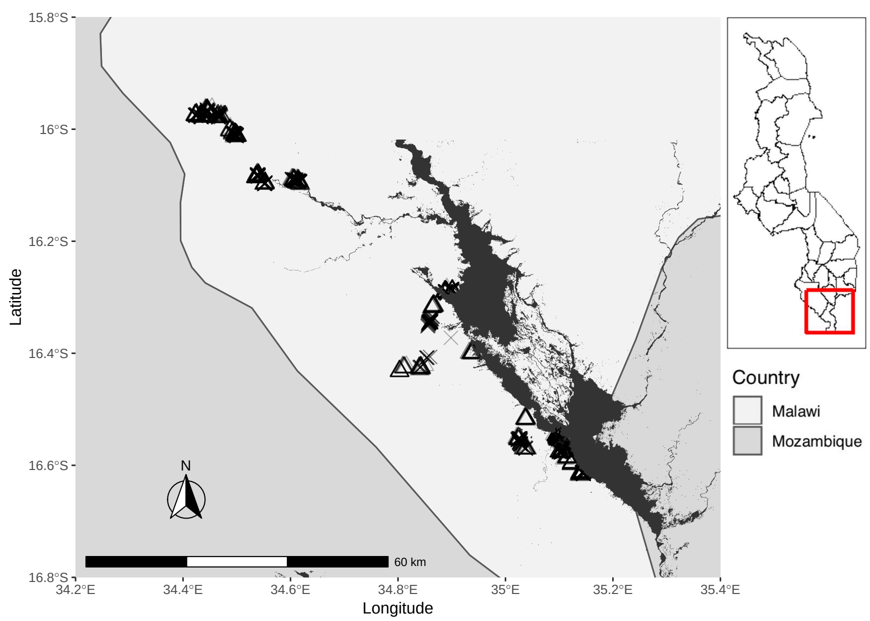
Figure A3: Blue color presents the actual floods, and red color represented the modelled floods based on prior data. Black color represents permanent water bodies. Source: PDNA-Report (2015).



E Sampling

I draw on the same villages that were part of the Local Governance and Performance Index (LGPI) in 2016 (Lust et al., 2016). The LGPI survey collected public opinion data on public service provision in Malawi and provides extensive background data on each village. The respondents from each data collection are not the same, but they were selected randomly from within the same villages. The LGPI sample was stratified on region (North, Central, South), the presence of matrilineal and patrilineal ethnic groups, and the 'urban'/rural divide. Because patrilineal groups are rare in Malawi and we wanted to maximize variation in matrilineal and patrilineal heritage, we oversampled Primary Sampling Units (PSUs) from the patrilineal stratum. We sampled 22 PSUs, namely 'Traditional Authorities' (TAs). These 22 sampled TAs are in 15 of Malawi's 28 districts. Within each TA (i.e., PSU), we selected randomly four enumeration areas (EAs) as Secondary Sampling Units (SSUs). EAs are comparable to census tracts. Both PSUs and SSUs were selected without replacement according to the principle of Probability of Selection Proportional to Measure of Size (PPMS). Within each EA, four villages were sampled based on known geographical points provided on the maps of the EAs produced for Malawi's latest population census. Once in the village, enumerators followed a random walk pattern to select households. After they entered the household, the interviewer collected the necessary data about composition of the household. Both the contact questionnaire and the main questionnaire we programmed on digital tablets, including the selection of the final respondent in the household through a digital version of the "Kish grid".

Figure A4: Map of Southern Malawi Depicting the Extent of the Flooded Area in 2015 (in black) and Survey Locations in 2016 ($\triangle$) and 2018 ($\times$)



F Survey

F.1 Conjoint Experiment

Table A3: Conjoint Experiment: Full Text

Factor	Level
Effort	The candidate:
Prevention	[0] *did not* put a lot of work into a disaster preparedness plan to limit damages from natural disasters
	[1] *did put* a lot of work into a disaster preparedness plan to limit damages from natural disasters
Relief	[0] *did not attend* meetings to coordinate post-disaster relief
	[1] *did attend* all meetings to coordinate post-disaster relief
Ask	[0] did not ask for help from external funders
	[1] wrote to international funders and NGOs asking to send resources
Effective	
Prevention	[0] his disaster preparedness plan was implemented, but was of *low quality* and did not limit the damages from the flood
	[1] his disaster preparedness plan was implemented, had *high quality* and did limit the damages from the flood
Relief	[0] did not donate any funds
	[1] donated funds to the village
Visit	[0] did not visit the disaster site
	[1] did visit the disaster site, talked to victims and declared his solidarity
Honesty	The candidate:
Corruption	[0] is convicted of corruption for embezzling humanitarian aid for personal use
	[1] has no record of corruption
Vote Buying	[2] is convicted of corruption for handing out cash to buy votes

F.2 Economic Distress Prime

I decided to not include an economic scenario that is directly linked to the flood because such a treatment could induce bias: it could influence the perception of some attributes in the conjoint experiment that are also directly linked to the disaster. Respondents are given some time to contemplate about how they might deal with these problems. Specifically, the treatment induces thoughts about financial worry and potential sources of help during such a crisis. This scenario shares some common features with flood disasters. Harvest failures are a big economic concern for many people in the sample villages in the Shire valley.[24] Farming is also common in the villages included in the sample. Using survey data from the same villages (Lust et al., 2016), I find that over 96% of respondents noted that they farm land and 85% stated that farming is their main sector of work.

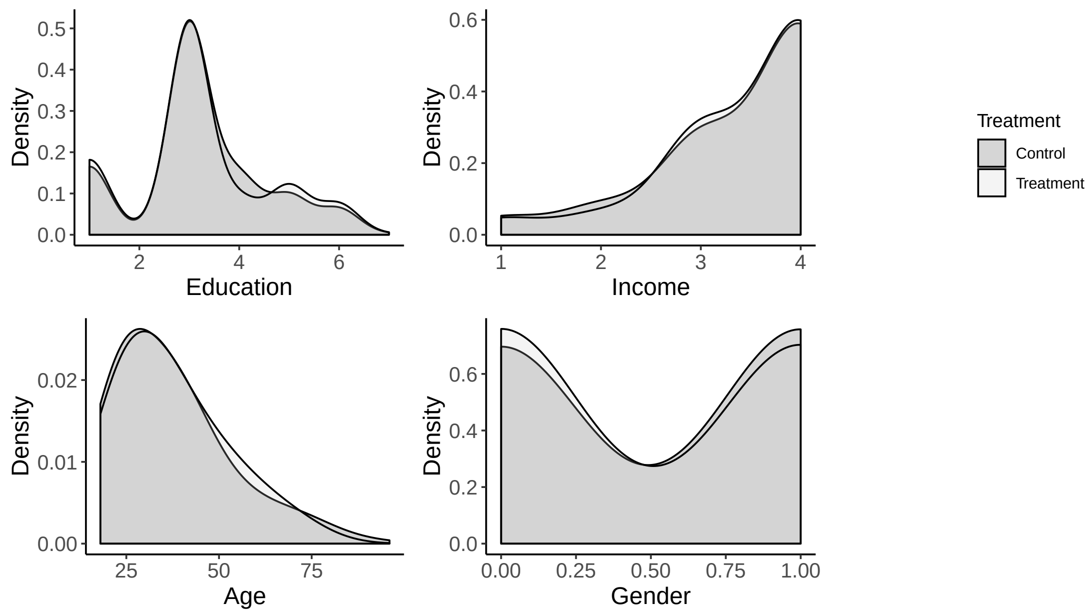
Treatment: Imagine you are a farmer and that locusts destroy your entire crop and the whole harvest is lost. How do you deal with this situation? Does it cause you serious financial hardship? Does it require you to make sacrifices? If so, what kind of sacrifices?
Control: [empty]

[24] See: https://mwnation.com/2016-locusts-worsened-food-shortage-in-shire-valley/

F.3 Balance Tests Prime-Conjoint Experiment

Figure A5 shows the balance of covariates for respondents who randomly received the prime (treatment) and those who did not (control).

Figure A5: Balance Tests : Respondent Characteristics (Distress Prime Experiment)



F.4 Manipulation Check: Economic Distress Prime

Table A4 shows the manipulation test for the disaster prime. We can see that the prime was successful and only manipulated financial worries.

- **Financial Worries:** To what extent do you agree to the following statements? I am very worried about my financial situation. The scale is as follows:
  1 Strongly disagree
  2 Disagree
  3 Neither disagree nor agree
  4 Agree
  5 Strongly agree
  98 Don't know / refuse

F.5 Economic Losses

- How badly were you economically harmed by the 2015 floods?
  1 Not at all

Table A4: Manipulation Check

Outcome	Effects Size
Financial Worry	0.18^{*}
	(0.09)
MP Responsible Relief	0.06
	(0.05)
MP Responsible Preparedness	0.04
	(0.05)
Flood Worry	-0.00
	(0.04)
Hopeless	0.10
	(0.10)
Num. obs.	806

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Notes: OLS estimates with robust standard errors within parentheses.

2 Just mildly

3 Somewhat

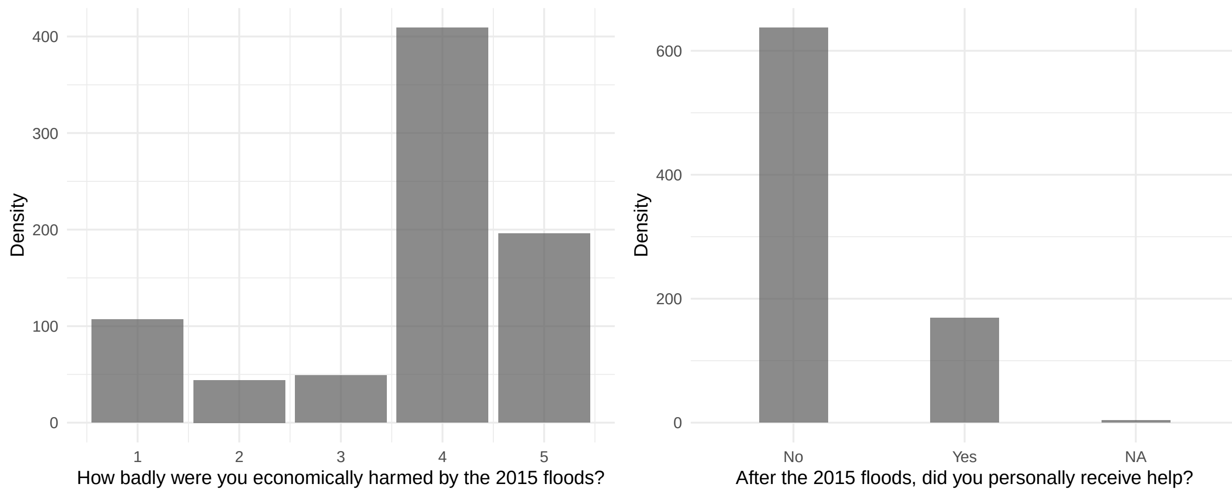
4 Very badly

5 Extremely badly

98 Don't Remember

- Binary measure takes on the value of 1 if respondents replied 4 or 5 and is 0 otherwise.

Figure A6: Flood exposure and Help



F.6 Flood Distance Measure

In order to assess the extent of a maximum flood and the distance from each respondent to the flood, I create a maximum flood polygon by merging publicly available GIS-data obtained from the Malawi Spatial Data Platform from several satellite programs: the TerraSAR-X, RADARSAT-2, and Copernicus EMS. MASDAP, see `http://www.masdap.mw/`. Flooded areas by RADARSAT-2 as of 13/01/2015, flooded areas by TerraSAR-X as of 10/01/2015, and flooded areas by Copernicus EMS as of 27/01/2015. The image with the highest resolution comes from RADARSAT-2 and has a spatial resolution of 6.25 meters. However, high-resolution satellite data was only available for the Shire valley and the Zomba district. This includes the districts Nsjanje, Chikwawa, Mulanje. This is partly because the meteorological situation was complex. In particular, the rainfalls occurred over a time period of about two weeks during early January. Heavy rains hit the country two times, first on January 8 and 9 with rainfall of up to 100 mm–subsequently leading to the riverine floods of the Shire river approximately on January 10-13–and on January 12 with up to 400 mm–leading to the the flash floods–with both riverine floods around the Shire river and flash floods in larger cities such as Blantyre (Kruczkiewicz et al., 2016). Since remote sensing satellites can only detect larger water areas as produced by riverine floods, I am not able to access the extent of the flash floods. To create a measure of relative flood intensity, I first compute the minimal euclidian distance in meters between any household surveyed and an area flooded. The variable has high values for observations that are far away from the flooded areas and small values for observation that are close. To create a measure of individual flood exposure (D_i), I invert the scale and re-scale the measure by the minimum to make all the elements lie between 0 (large distance to the flood) and 1 (small distance to the flood):

$$D_i = -\Big(\frac{x_i - min(x)}{max(x) - min(x)}\Big) + 1 \tag{F.1}$$

where x_i refers to the individual distance in meters in vector x, and $min(x)$ and $max(x)$ to its minimum and maximum respectively. Figure A7 shows a graphical presentation of the measure for the surveys in 2016 and 2018 respectively.

Figure A7: Distance to Flood (2015) in 2018.

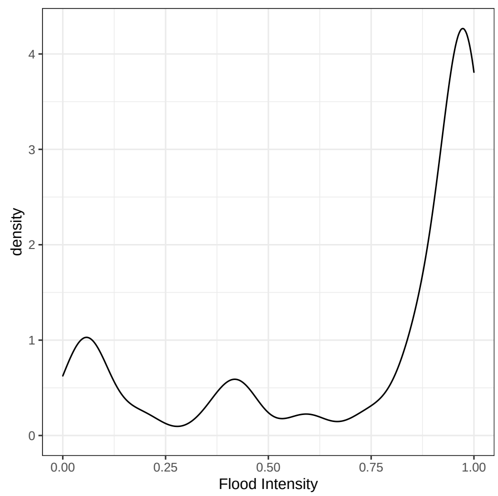
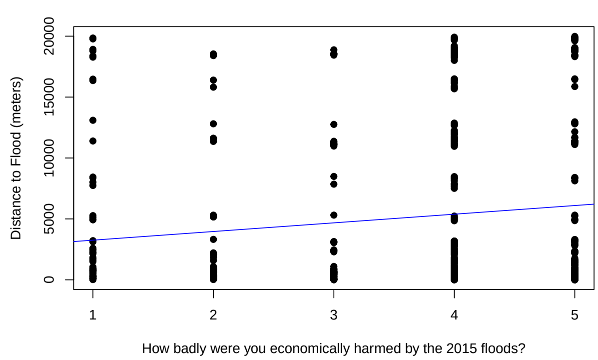


Figure A8: Scatterplot Economic Distress (X) and Distance to Flood (Y)?



G Main Results

Table A5: Main Results

	Model 1
(Intercept)	0.31***
	(0.01)
Preparedness Coordination	0.05***
	(0.01)
Relief Coordination	0.09***
	(0.01)
Preparedness Effective	0.11***
	(0.01)
Relief Effective	0.12***
	(0.01)
Relief Ask	0.12***
	(0.01)
Visits	0.16***
	(0.01)
Corruption	−0.24***
	(0.01)
Vote Buying	−0.17***
	(0.01)
Num.Obs.	9660
R2	0.117
R2 Adj.	0.116
Std.Errors	by: CaseID

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: OLS estimates with robust standard errors within parentheses.

Table A6: Main Results, Linear Hypothesis

	Model 1
(Intercept)	0.31***
	(0.01)
Preparedness Coordination	0.05***
	(0.01)
Relief Coordination	0.09***
	(0.01)
Preparedness Effective	0.11***
	(0.01)
Relief Effective	0.12***
	(0.01)
Relief Ask	0.12***
	(0.01)
Visits	0.16***
	(0.01)
Corruption	−0.24***
	(0.01)
Vote Buying	−0.17***
	(0.01)
Effort prevention - Effort relief = 0	−0.03**
	(0.01)
Num.Obs.	9660
R2	0.117
R2 Adj.	0.116
Std.Errors	HC2

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: OLS estimates with robust standard errors within parentheses.

Table A7: Main Results, Linear Hypothesis 2

	Model 1
(Intercept)	0.31***
	(0.01)
Preparedness Coordination	0.05***
	(0.01)
Relief Coordination	0.09***
	(0.01)
Preparedness Effective	0.11***
	(0.01)
Relief Effective	0.12***
	(0.01)
Relief Ask	0.12***
	(0.01)
Relief Visits	0.16***
	(0.01)
Corruption	−0.24***
	(0.01)
Vote Buying	−0.17***
	(0.01)
Preparedness Effective - Relief Effective = 0	−0.01
	(0.01)
Num.Obs.	9660
R2	0.117
R2 Adj.	0.116
Std.Errors	HC2

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: OLS estimates with robust standard errors within parentheses.

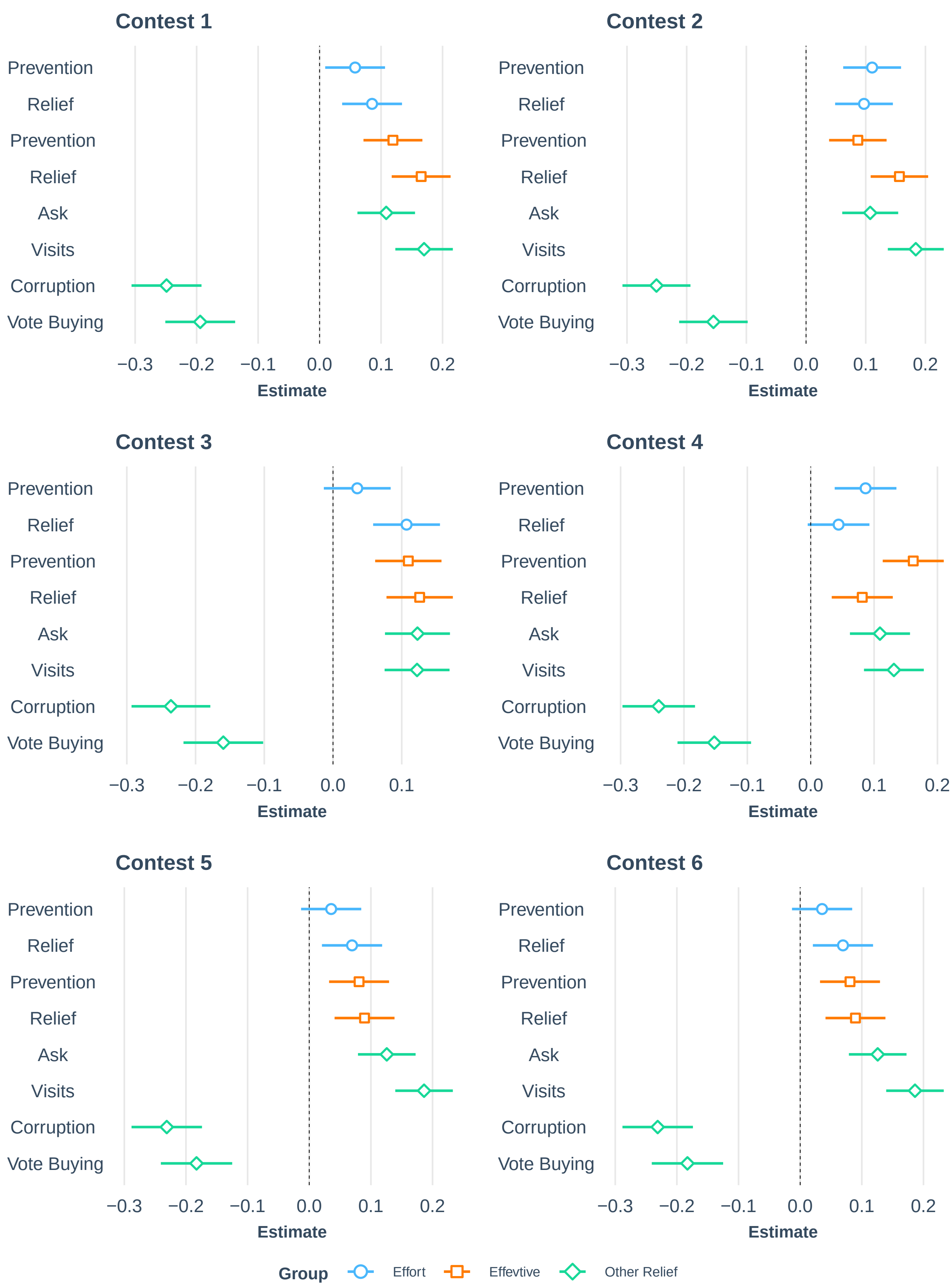
Table A8: Main Results with Interactions

	Model with interactions
Preparedness Effort	0.05*
	(0.02)
Relief Effort	0.09***
	(0.02)
Preparedness Effective	0.11***
	(0.02)
Relief Effective	0.10***
	(0.02)
Corruption	−0.24***
	(0.02)
Vote Buying	−0.18***
	(0.02)
Preparedness Effective × Corruption	−0.01
	(0.02)
Preparedness Effective × Vote Buying	0.01
	(0.02)
Relief Coordination × Relief Effective	0.01
	(0.02)
Preparedness Effective × Relief Effective	−0.01
	(0.02)
Preparedness Effort × Relief Coordination	−0.01
	(0.02)
Preparedness Effort × Preparedness Effective	0.00
	(0.02)
Preparedness Effort × Corruption	0.01
	(0.02)
Preparedness Effort × Vote Buying	0.00
	(0.02)
Relief Effective × Corruption	0.02
	(0.02)
Relief Effective × Vote Buying	0.03
	(0.02)
Num.Obs.	9660
R2	0.078
R2 Adj.	0.076
Std.Errors	by: CaseID

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: OLS estimates with robust standard errors within parentheses.

Figure A9: Heterogeneity of AMCE by Number of Contest



H Additional Analyses

- H_{2a}: Voters who experienced economic losses will be more likely to reward candidates who delivered relief benefits and provided cash to buy votes.
- H_{2b}:Voters who have experienced psychological distress will be more likely to reward candidates who delivered relief benefits and provided cash to buy votes.

H.1 Estimand and Estimation

To test the hypothesis, I estimate the conditional AMCE with respect to moderating variables T, economic losses and the psychological stress prime (Leeper et al., 2020). I estimate Equation H.1 using OLS with interactions of attributes and moderators:

$$Y_{im} = \sum_{j \in Z} \beta_j Z_i^j + \sum_{j \in Z} \theta_j (Z_i^j * T_i) + \alpha T_i + \sum X_i + \gamma_m + \epsilon_i \tag{H.1}$$

The conditional ACMEs (θ_j) of self-reported economic losses must be interpreted with care because economic losses were not randomly assigned. The difference in conditional AMCEs (θ_j) is only causally identified for the randomly assigned prime. However, the extent of the flood in 2015 and the resulting economic losses introduced a large random component as the flood exceeded any previous ones and was largely unpredictable.[25] So it is unlikely that individuals could predict the extent of the flood and therefore select out of the flood zone. For the effect of economic losses, I also control for a set of covariates X_i that could influence both economic losses and the reaction to the attributes in the conjoint: poverty levels, education, gender, interest in politics, geographic distance to the flood in 2015, help received after the last disaster, and trust in MPs.[26] I also report the marginal mean, which is a factor-level mean outcome ignoring other factors (Leeper et al., 2020).

H.2 Conditional AMCE's by Respondent Affectedness

Having established that respondents hold expectations about the effectiveness of prevention and relief policies, I investigated to what extent preferences are subject to change due to individuals' affectedness (Hypotheses 2a–2b). I measure exposure to the natural disaster with two indicators: self-reported economic losses due to the 2015 flood and primed psychological and financial distress due to a natural disaster. Economic losses are defined as a binary measure taking the value of 1 if the respondents reported

[25]See Figure A3 in the appendix for the predicted flood extent based prior data and the actual flood extent.
[26]See Appendix F for the description of the survey measurements.

they were very badly harmed by the 2015 flood and 0 otherwise.[27] To measure the effect of psychological distress, I randomly assign a natural disaster prime before the conjoint experiment. The prime is intended to induce financial worries while leaving the actual economic state of the respondent unchanged. **?** used a similar approach and found that the prime increased present bias, i.e., the value participants attach to present outcomes relative to all future outcomes. The design was developed by Mani et al. (2013). I use a hypothetical scenario about locusts destroying the harvest because it is a common problem.[28] The control group did not receive the prime.

Figure A10 shows the conditional AMCEs, along with 95% confidence intervals, subset by economic losses (upper panel) and the disaster prime (lower panel).[29] The results are mixed and only support hypothesis H_{2a} and not H_{2b}. On average, changing economic losses from control (no/small losses) to treatment (high losses) increases the probability of supporting a candidate delivering disaster relief by an average of 0.04 percentage points. The probability of supporting a candidate who used vote-buying strategies also moves in the predicted direction: voters are more likely to reward candidates who engaged in vote-buying (0.06) if they experienced recent economic losses. However, note that the coefficient on the vote-buying attribute is still negative. Voters are also more likely to support candidates who asked for help from external actors. All three point estimates are statistically significant at conventional levels (0.05). It is also worth noting that the point estimates for preparedness efforts and effective preparedness are lower for individuals who experienced high losses. The difference is not statistically significant. The results are robust to the inclusion of several pre-treatment control variables such as poverty, political engagement, and education levels.[30]

Next, I test the effect of psychological distress on voter preferences (H_{2b}). Looking at the prime's effect, preferences are remarkably stable across the treatment and control comparisons and do not show the predicted effects. Therefore, I cannot reject the null hypothesis of no effect of psychological distress on demand for relief benefits. If anything, respondents primed for economic distress became more dismissive of candidates engaging in vote-buying. The point estimates are negative and statistically significant at the 0.1 level. The other point estimates remained unchanged. One possible explanation is that the prime was too weak to induce financial distress. However, as we can see in Table A4, the prime increased financial worries in the treatment group. Alternatively, the prime might only have induced the desired effects for a subset of participants. I explored this possibility and evaluated how the prime affected participants who

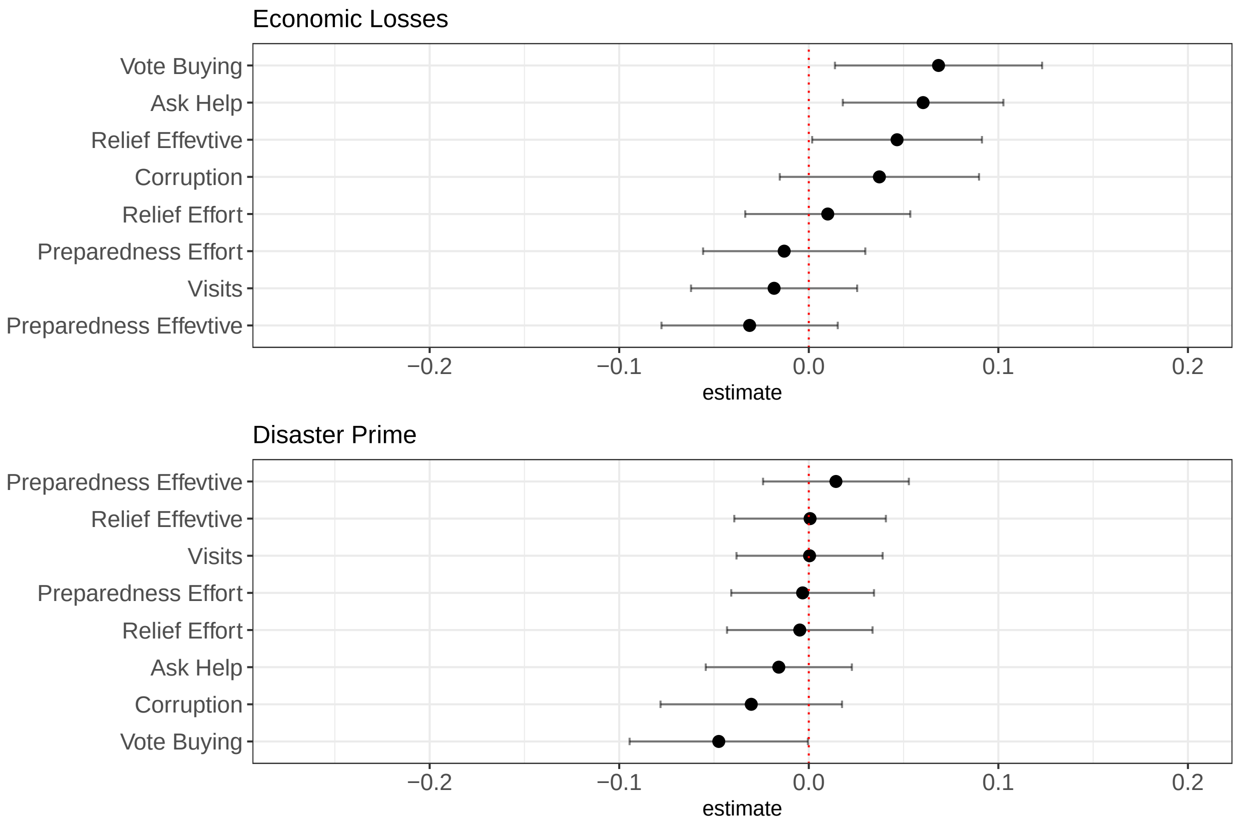
[27] See the exact wording in Appendix F.5 and Figure A6 for the distribution. In the pre-analysis plan, I specified to also test heterogeneous effects on ACMEs depending on the distance to the flood. However, as we can see in Figure A8 in the Appendix, the pre-specified distance measure is a bad predictor of economic losses, the main concept of interest. Therefore, I do not report the effects of distance.

[28] The prime included an open-ended question: "*Treatment: Imagine you are a farmer and that locusts destroy your entire crop and the whole harvest is lost. How do you deal with this situation? Does it cause you serious financial hardship? Does it require you to make sacrifices? If so, what kind of sacrifices?*" For the details see Appendix F.2.

[29] The regression table including the interaction terms of treatments and attributes can be found in the section A13 in the Appendix.

[30] See regression Table A10 with controls in the appendix.

Figure A10: Effects of Attributes on Respondents' Preference (by condition)



Notes: Beta coefficients from OLS regression with standard errors in parentheses. Standard errors are clustered at the individual. Vertical lines indicate 95% confidence intervals. See regression Table A10 with controls.

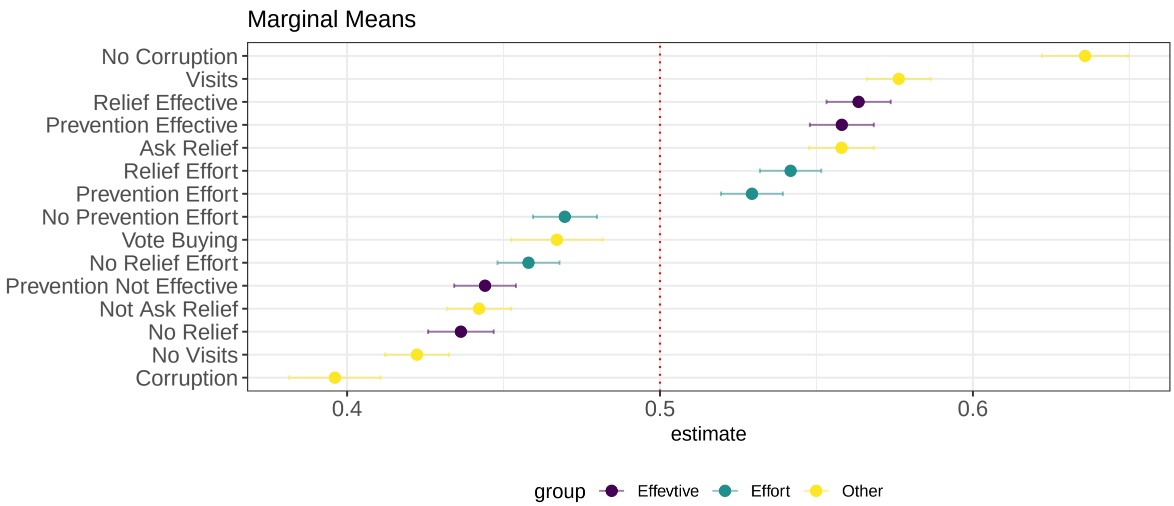
experienced high or low losses during a natural disaster. As shown in Appendix H.4, I find no significant interaction effects between economic losses and the prime.

Taken together, I find suggestive evidence that economic losses due to natural disasters induce demand for vote-buying and material benefits (Gallego, 2018; Cavalcanti, 2018), but the average marginal effect of vote-buying is still negative in the group with high losses. However, the economic effect persisted for two years after the disaster, likely because respondents did not receive sufficient help in the immediate aftermath (see Figure A6). Thus, the results lend further support to findings that disaster events can alter political preferences (Fair et al., 2017), can have long-lasting political consequences (Bechtel and Hainmueller, 2011), and point to the importance of insurance against disaster-related economic losses (Clarke and Dercon, 2016).

However, the prime did not alter voter preferences as expected. All point estimates are insignificant, and further subgroup analysis remained inconclusive. Voters value vote-buying significantly less when primed for psychological distress due to natural disasters. One speculation is that those who were more affected by the disaster are also more exposed to vote-buying due to the influx of aid, thereby potentially learning about the negative consequences and becoming more dismissive of vote-buying.

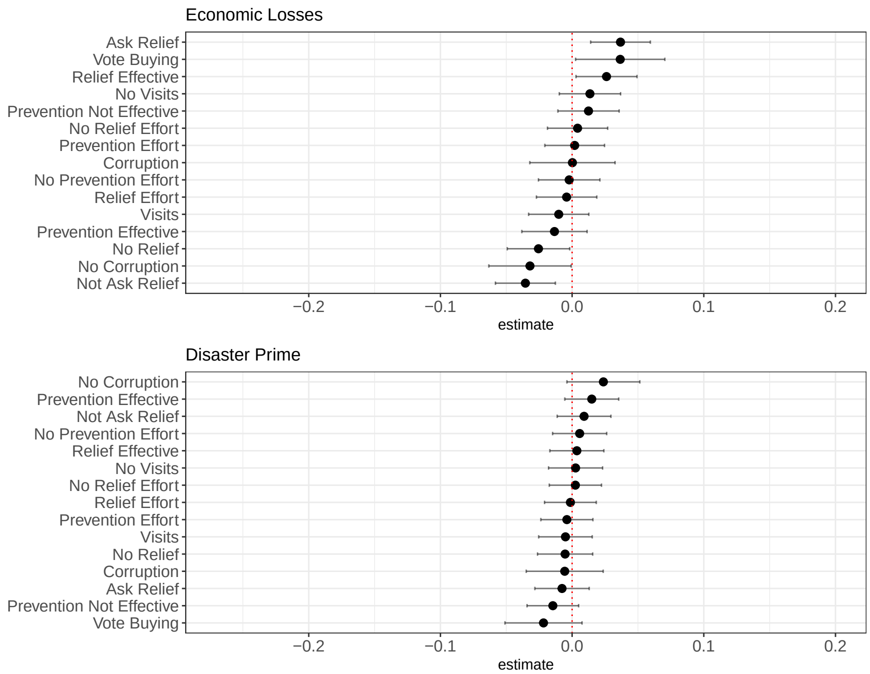
H.3 Marginal Means

Figure A11: Marginal Means



Notes: Beta coefficients from OLS regression with robust standard errors in parentheses. Standard errors are clustered at the individual level. Horizontal lines indicate 95% confidence intervals. The baseline is always the low level of the given attribute.

Figure A12: Difference in Marginal Marginal Means of Attributes on Candidate Support, by Condition



Notes: Beta coefficients from OLS regression with standard errors in parentheses. Standard errors are clustered at the individual. Vertical lines indicate 95% confidence intervals.

Table A9: Treatment Effects on ACMEs

Treatment	Prime	Economic Distress	Poverty
Preparedness Effort	-0.00	-0.01	0.02
	(0.02)	(0.02)	(0.03)
Relief Effort	-0.01	0.01	0.00
	(0.02)	(0.02)	(0.03)
Preparedness Effective	0.01	-0.02	0.01
	(0.02)	(0.02)	(0.03)
Relief Effective	-0.00	0.04^{*}	0.01
	(0.02)	(0.02)	(0.03)
Ask Relief	-0.01	0.06^{**}	0.05
	(0.02)	(0.02)	(0.03)
Visits Effective	-0.00	-0.01	0.05
	(0.02)	(0.02)	(0.03)
Corruption	-0.02	0.03	-0.03
	(0.02)	(0.03)	(0.04)
Vote Buying	-0.04^{*}	0.07^{**}	-0.02
	(0.02)	(0.03)	(0.04)
R^2	0.12	0.12	0.12
Num. obs.	9720	9720	9720

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: OLS estimates with robust standard errors within parentheses.

H.4 Interaction Effect of Economic Losses and Prime

As we can see in Figure A13, the treatment effects of the prime remain mostly unchanged across groups. For high-loss individuals, the prime appears to increase the demand for effective prevention but not for effective relief. For low-loss individuals, the prime further decreased the support for candidates engaging

Table A10: Effect of Economic Losses, Full Models, with Controls

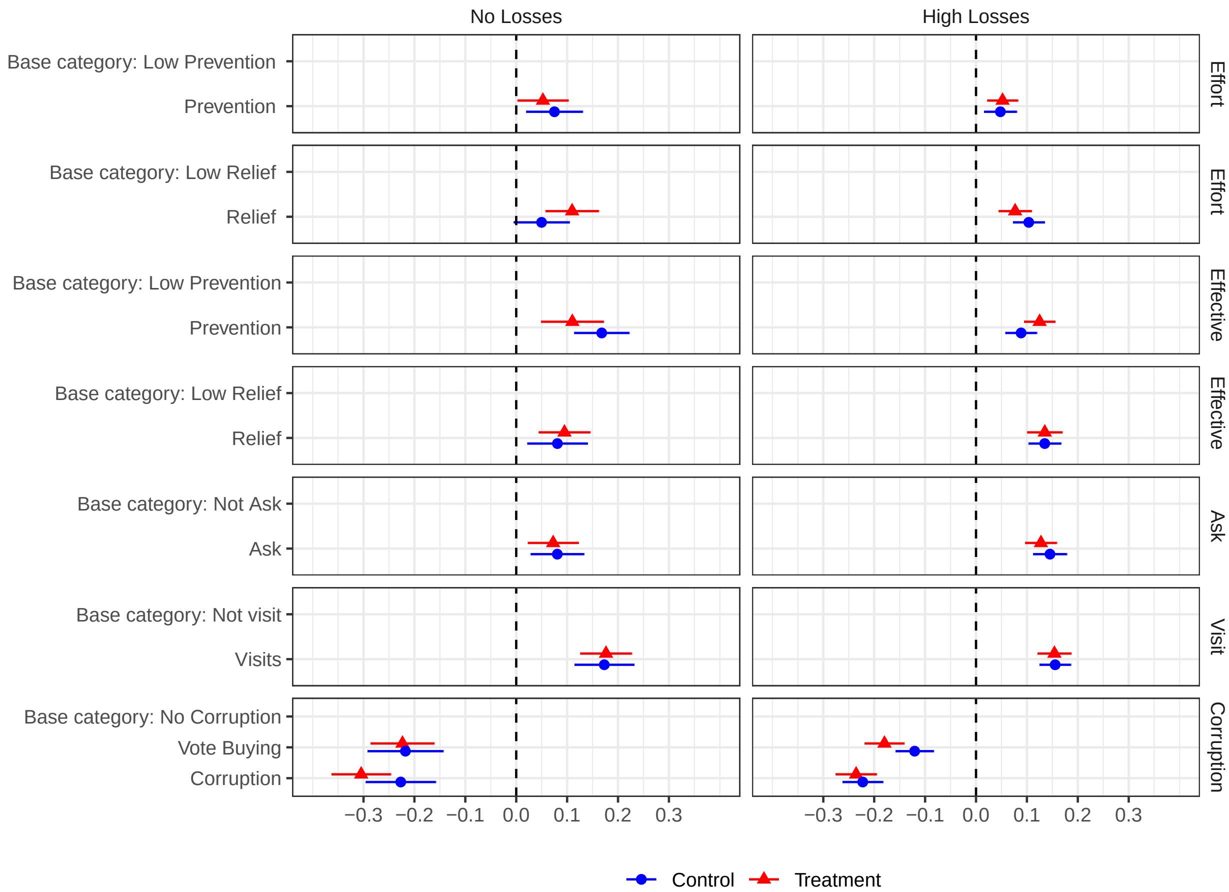
	Model 1	Model 2	Model 3	Model 4	Model 5
Prevention Effort x Economic Losses	-0.01	-0.01	-0.01	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Relief Effort x Economic Losses	0.01	0.01	0.01	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Prevention Effective x Economic Losses	-0.02	-0.02	-0.02	-0.03	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Relief Effective x Economic Losses	0.04^{*}	0.04^{*}	0.04^{*}	0.04^{*}	0.04^{*}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Relief Ask x Economic Losses	0.06^{**}	0.06^{**}	0.06^{**}	0.05^{**}	0.06^{**}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Visits x Economic Losses	-0.01	-0.01	-0.01	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Corruption x Economic Losses	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Vote Buying x Economic Losses	0.07^{**}	0.07^{**}	0.07^{**}	0.06^{**}	0.06^{**}
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Poverty		-0.00	-0.00	0.00	0.01
		(0.01)	(0.01)	(0.01)	(0.01)
Education				-0.00	-0.00
				(0.00)	(0.00)
Age				-0.00^{***}	-0.00^{**}
				(0.00)	(0.00)
Gender				-0.01	-0.01
				(0.00)	(0.00)
Interested_politics				0.00	0.00
				(0.00)	(0.00)
Trust MP				-0.00	-0.00
				(0.00)	(0.00)
Flood worried					0.00
					(0.00)
Recieved help					-0.00
					(0.00)
Economic Losses x Poverty					-0.01
					(0.02)
R^2	0.12	0.12	0.12	0.12	0.12
Num. obs.	9720	9720	9720	9588	9504

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: OLS estimates with robust standard errors within parentheses.

in corruption and increased the support for candidates who engaged in relief efforts.

Figure A13: Interaction Effect of Economic Losses and Prime on ACMEs.



Notes: Beta coefficients from OLS regression with standard errors in parentheses. Standard errors are clustered at the individual. Vertical lines indicate 95% confidence intervals.

H.5 Data Sources: Disaster Preparedness

H.5.1 Description:

An indication of capacities to deal with climate-related nature disasters. This indicator uses monitoring from the Hyogo Framework Action (HFA). The HFA outlined an action plan from 2005 to 2015 to establish five priorities for disaster preparedness. Countries are monitored in two-year intervals against the five priorities by self-reported data.

H.5.2 Data Source

HFA National Progress

H.5.3 Notes:

(1) HFA action plan was outlined in 2005 and the reports were not made until 2007, therefore, disaster preparedness was not tractable before that for all countries. (2) The self-reported data are not always comparable among countries. However, the HFA report still provides so far the most comprehensive data set that monitors the progress of capacity building in terms of preparing for natural disasters.

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