

# DO VOTERS PREFER RELIEF OVER PREPAREDNESS? EVIDENCE FROM MALAWI

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**ABSTRACT.** Growing evidence suggests that voters reward politicians for spending on disaster relief but not on disaster preparedness. Yet, we know little about the mechanisms that underpin this pattern. I propose that voter value effective preparedness as much as effective relief. However, voters have pessimistic expectations about the effectiveness of preparedness policies compared to relief policies. Additionally, income losses due to disasters can induce a demand for relief. I test both 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. Respondents who have suffered economic losses are more likely to value effective relief. The findings have important implications for the design of disaster policies.

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## 1. INTRODUCTION

Investment in public goods is an essential task of governments to ensure their citizens' welfare. One area in which investment in public goods plays a crucial role for citizens' welfare is natural disasters. While the number of weather-related disasters like floods or droughts is rising (Strömberg 2007; Guha-Sapir and Hoyois 2015), investment in public preparedness policies that could potentially mitigate their effects remain few compared to relief transfers (Clarke and Dercon 2016).<sup>1</sup> The problem is especially important in low-income countries where the population is particularly vulnerable to the effects of natural disasters (Hallegatte et al. 2016).

Why do governments invest so little in prevention compared to relief? One explanation suggests that voters might provide the wrong incentives to politicians. Previous studies found a positive association between relief spending and re-election rates for incumbents, but a weaker association or no association for preparedness spending (Healy and Malhotra 2009; Bechtel and Hainmueller 2011; Gasper and Reeves 2011; Cole, Healy, and Werker 2012; Gallego 2018; Cavalcanti 2018; Cooperman 2021).<sup>2</sup> Can we interpret these findings as evidence that voters prefer relief over prevention? So far, we lack empirical evidence that directly measures voters' preferences for relief and preparedness policies.

This article fills this gap. 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 compared to 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 because it is

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<sup>1</sup>Disaster preparedness is a special type of public good that only provides non-excludable benefits in case of a disaster. However, this paper focuses on settings in which disasters occur frequently. Therefore, well implemented prevention policies can be considered local public goods. 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.

<sup>2</sup>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.

difficult for voters to evaluate to what extent efforts like preparedness plans mitigated destructions from disasters.<sup>3</sup> The effectiveness of relief efforts is easier to evaluate because voters are the recipients of relief aid. Second, even if prevention policies are effective, economic losses due to disaster can induce a demand for relief; the reason being that voters need to compensate for lost income, and welfare gains from relief are immediate private transfers. By contrast, welfare gains from prevention come in the form of public goods that have to be shared among groups. Consequently, voters are more likely to support candidates who provided effective relief.

Empirically, it is challenging to infer voter preferences relating to natural disaster policies from election data. It is usually unclear what information voters had about the disaster, politicians' disaster policies, and their effectiveness when they cast their vote.<sup>4</sup> To overcome this issue, this paper relies on a conjoint experiment embedded in a face-to-face survey fielded in Southern Malawi. The sample consists of 810 respondents across 36 villages evaluated six pairs of conjoint profiles, resulting in 4,860 contests and 9,720 profiles. 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 involved in disaster preparedness and relief. The experiment informed participants about a hypothetical scenario in which a flood disaster has occurred but indicates that two candidates running for MP implemented different prevention and relief policies. By randomly manipulating features of contrasting disaster policies, the design allows me to identify the marginal causal effect of each candidate policy choice on voters' support. To study voter expectations about policy effectiveness, a first set attributes informed participants if a candidate exerted effort to implement a prevention and relief policy, while a second set of attributes informed participants if the efforts were effective or not. Because attribute

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<sup>3</sup>Preparedness and prevention measures include planting vegetation to retain excess water, building channels to divert water from flooding, terrace slopes to reduce slope flow, the construction of dams, or the construction of holding tanks to store extra water during flood periods. Preparedness plans typically include locating safe places for each type of disaster, determining evacuation routes, and preparing disaster supply kits.

<sup>4</sup>For example, relief policies might be more salient or visible to voters than prevention policies (Ashworth 2012; Healy and Malhotra 2013).

levels are randomized, I infer voter expectations from the marginal effect of policy efforts. To study the effect of income losses on voter demand for relief or prevention, I evaluate if the treatment effects identified in the conjoint experiment are similar for participants who report having experienced economic losses due to a disaster.

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 is effective in mitigating disaster damage. Strictly speaking, the paper cannot specify why voters hold pessimistic expectations for preparedness policies. However, I find suggestive evidence that respondents update their beliefs about effectiveness of preparedness policies. Since the conjoint experiment is conducted over six rounds, I can show that respondents are more likely to reward preparedness efforts if they repeatedly observe preparedness efforts leading to effective outcomes.

Second, I find evidence that recent economic losses can influence voter preferences. Voters who report having experienced economic losses due to a recent disaster are more likely to prefer candidates who can deliver effective community relief and are also more forgiving for candidates who engaged in vote buying when distributing disaster relief. While this evidence is not causal, I exclude several other mechanisms that could drive the results. First, I test if the results are driven by psychological distress potentially caused by the economic losses. Using a randomly assigned prime that increased economic worries, I show that groups with more or less financial distress have similar disaster policy preferences. Second, I find no evidence that the results are driven by poverty levels per se. Together, these findings show that the low returns for candidates preparedness spending can be explained by pessimistic expectations of voters about the effectiveness of preparedness efforts and an increased demand for relief in the aftermath of disasters.

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, Healy, and Werker 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.<sup>5</sup> This paper finds empirical evidence that balances both views. 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 that it is driven by the corruption of politicians.

Relatedly, the paper also adds evidence on the effect of vulnerability on the demand for private goods (Dixit and Londregan 1996; Bobonis et al. 2017). Previous research suggests that politicians are more likely to engage in vote buying after a disaster using the increased resource available through relief aid (Gallego 2018; Cavalcanti 2018). To date, we lack evidence as to whether voters have a demand for relief aid in the aftermath of natural disasters. I find no evidence that voters have a general preference for a candidate who engages in vote buying in the context of disaster relief. However, I find suggestive evidence that respondents who report being economically harmed by a disaster are more likely support candidates who handed out private goods and forgive candidates who handed out cash to buy votes. These findings complement evidence from Visconti (2021) who found that disaster exposure in Chile made people more likely to prioritise housing. Unlike Visconti (2021), I find that these effects did not vanish three years after the disaster. This also suggest that disasters themselves, by increasing demand for relief, could undermine the electoral incentives for politicians to invest in disaster preparedness.

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<sup>5</sup>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. In this paper voters are certain that a disaster has occurred but they hold pessimistic expectations if the spending will be effective in mitigating its effects.

In this sense, the results also point to the importance of disaster insurance *ex ante* (Clarke and Dercon 2016).

The study also has implications beyond natural disasters. The low electoral returns from prevention policies are often interpreted as an indication that voters prefer private over collective goods (Healy and Malhotra 2009). Therefore, the results speak to the large literature on the electoral incentives for public and private goods provision in developing democracies (Wantchekon 2003; Keefer and Khemani 2005; Mani and Mukand 2007). Previous work suggests that politicians in developing democracies cannot commit to public goods policies because voters do not trust that politicians will deliver on their promises and more likely to vote for candidates who make promises about private, clientelistic goods (Kitschelt 2000; Wantchekon 2003; Keefer 2007; Stokes et al. 2013). While conceptually different from campaign promises, my results suggest that similar dynamics hold for policy efforts. Voters seem to have lower expectations about the welfare returns from public goods policies compared to private transfer. However, I find no indication that voters have a general preference for effective private transfer over effective public goods provision. Similar to findings by Dionne and Horowitz (2016), I find that voters value effective programmatic policies.

## 2. THEORETICAL BACKGROUND

The literature on electoral accountability and retrospective voting suggests that voters reward the past performance of politicians (Key 1966; Barro 1973; Ferejohn 1986; Fearon 1999; Besley 2006, 2006). In turn, politicians' policy efforts are driven by the anticipation that they will look good in the eyes of voters (Besley and Burgess 2002). However, Maskin and Tirole (2004) have shown that well-functioning accountability can incentivize incumbents to implement policies that pander to public opinion but can be suboptimal for voter welfare. In this regard, it is a fundamental question as to what kind of policies voters prefer. In particular, do voters reward candidates for investment in public goods or do they reward candidates who provide them with private transfers?

In a seminal paper, Healy and Malhotra (2009) study this question by examining voter reaction to government performance with respect to natural disaster policies in the US. Although politicians have no control over the occurrence of a disaster itself, they can influence outcomes by their *prevention* and *relief* policies (Ashworth, Mesquita, and Friedenberg 2018). Prevention and preparedness policies typically take the form of public goods such as the construction of dams and irrigation systems, the installation of flooding zones, and the preparation of evacuation plans.<sup>6</sup> On the other hand, relief policies typically provide private goods such as drinking water, food, or temporary housing in response to a disaster.<sup>7</sup>

Healy and Malhotra (2009) show that citizens reward incumbents for relief spending but not for 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 nor collective 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. Gallego (2018) finds tentative evidence that local mayors in Colombia used the increased inflow of aid after a disaster to target relief spending, in the forms of private transfers and local public goods, to buy votes. The study only finds significant effects for private transfers. Several studies lend further support to the proposition that voters reward incumbents for relief spending. Gasper and Reeves (2011) and Cole, Healy, and

<sup>6</sup>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, (i.e., they are non-rival).

<sup>7</sup>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 (see: <https://www.nyasatimes.com/dodma-shifts-blame-on-dcs-for-alleged-accusation-of-selective-distribution-of-relief-aid-for-flood-victims-in-balaka/>).

Werker (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 from relief spending can last for several years. In line with the findings on the positive electoral effects of relief, Cooperman (2021) finds that mayors in Brazil issue drought declarations, which trigger 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 the variation in incumbents' re-election rates and the variation in disaster prevention or relief. However, do these correlations reflect the underlying policy preferences of voters? More fundamentally, why would voters prefer direct relief transfers over public prevention? This study explores two mechanisms that can shape voter preferences: voters might prefer relief over prevention because (1) they expect that prevention policies will be less effective, and (2) voters might be exposed to economic losses due to natural disasters that induce demand for short-term material relief. I discuss both mechanisms in turn.

## 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.<sup>8</sup> Voters might have a preference (i.e., desire) for individual or community welfare as the literature on egotropic and sociotropic voting suggests (Fiorina 1978; Kinder and Kiewiet 1981; Healy, Persson, and Snowberg 2017). But voters also form subjective beliefs (expectations) about how policy efforts translate into the outcomes they desire.<sup>9</sup>

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 have to 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

<sup>8</sup>See Gintis (2014) for a general discussion.

<sup>9</sup>I define an expectation as a probability distribution that maps policy efforts into outcomes.



difference in candidates' support. There are several reasons why voters might hold 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 in settings in which disasters frequently 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 compared to relief policies (Gailmard and Patty 2019).<sup>10</sup> 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 might be even more prone to corruption because 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 that we can derive if voters have the expectation that prevention efforts are less effective than relief efforts.

- $H_{1a}$ : Voters will be more likely to reward candidates for relief efforts over prevention *efforts*.
- $H_{1b}$ : Voters will be less likely to reward candidates for relief outcomes over prevention *outcomes*.

## 2.2. Vulnerability and Affectedness

Apart from having different expectations about the effectiveness of prevention and relief efforts, a set of theories suggests that some voters might be subject to external constraints

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<sup>10</sup>Gailmard and Patty (2019) show formally that voters would prefer relief over prevention policies if they were more uncertain about the effectiveness of prevention policy efforts. 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 are 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 is known to the voter.

that can influence preferences. In particular, the literature offers two reasons why poor voters might prefer private transfers over public goods. An extensive literature on clientelism argues that voters have a diminishing marginal utility from income, leading low-income voters to derive more utility from private transfers (Dixit and Londregan 1996; Bardhan and Mookherjee 2012). Alternatively, poor voters might be more risk-averse than wealthy voters (Stokes et al. 2013), leading them to reward private relief over public prevention if they perceive relief transfers as less risky (less uncertain) than prevention. Importantly, the former mechanism would influence preferences even if there was no uncertainty attached to the delivery of public prevention benefits. Consistent with the both mechanisms, Bursztyn (2016) finds that low-income voters in Brazil prefer short-term transfers over public education.

While this prediction mainly concerns income levels (i.e., poverty), recent evidence suggests that similar dynamics may also hold for income shocks. Bobonis et al. (2017) have shown that a positive welfare shock made voters less reliant on clientelism and reduced the vote share of the incumbent. If the opposite mechanism also holds, negative income shocks may increase the marginal utility of income and shift preferences towards private transfers. In terms of natural disaster policies, this mechanism could lead to a preference for candidates who provide relief transfers and/ or offer cash in the form of vote buying.

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

Second, natural disasters can also affect the psychological state of mind. Thus, a second potential mechanism suggests that the psychological effects of disasters can shape subsequent political preferences. In this regard, previous research has documented that exposure to natural disasters has made individuals more risk-averse (Cameron and Shah 2015; Cassar, Healy, and Von Kessler 2017).<sup>11</sup> Related research has found that people affected by natural disasters that caused economic harm prioritized their personal welfare (Visconti 2021) and act more selfishly (Li et al. 2013). Thus, alongside economic well-being, the

<sup>11</sup>Some studies also suggest that natural disasters make people more likely to engage in risk-taking (Hanaoka, Shigeoka, and Watanabe 2018).

disaster itself might induce risk aversion and a focus on private welfare. Consequently, voters might prefer benefits that are delivered with little risk. Voters might prefer candidates that offer effective relief to mitigate their economic hardship and be more forgiving to candidates who try to buy votes using cash benefits.

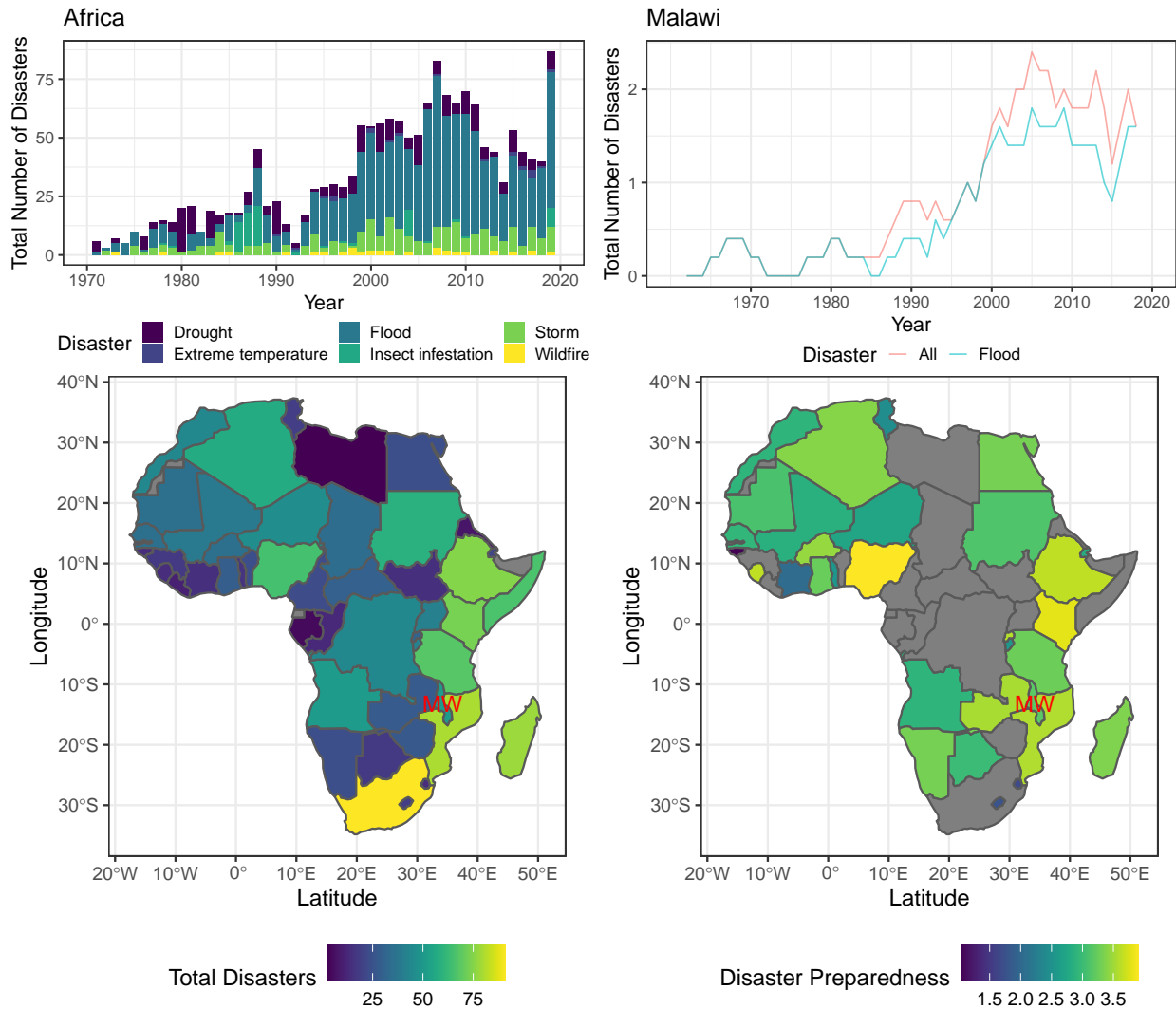
- $H_{2b}$ : Voters who have experienced psychological distress will be more likely to reward candidates who delivered relief benefits and provided cash to buy votes.

### 3. CASE: MALAWI

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 we can see in the upper left panel of Figure 1, there has been a rapid increase after 1990, and the most common type of disaster is floods. 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. In fact, in the years leading up to this study in 2018, the country suffered a flood almost every other. 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 each year because of droughts and floods. The population is particularly vulnerable to natural disasters because 80 % of people live off agriculture. This is especially the case in the Southern Shire basin in Malawi, the focus of this study, which annually experiences 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 people displaced and killing 106. The flood led to massive destruction of crops and agricultural production more generally and destroyed social infrastructure – specifically, schools, health facilities, and housing (PDNA-Report 2015; Šakić Trogrlić et al. 2019).

With a total of 51 disasters in the time between 1970 and 2020 Malawi ranks 12 out 55 countries in the data. In terms of disasters preparedness the country takes on a middle position compared to other African states (see lower right panel of Figure 1). With the

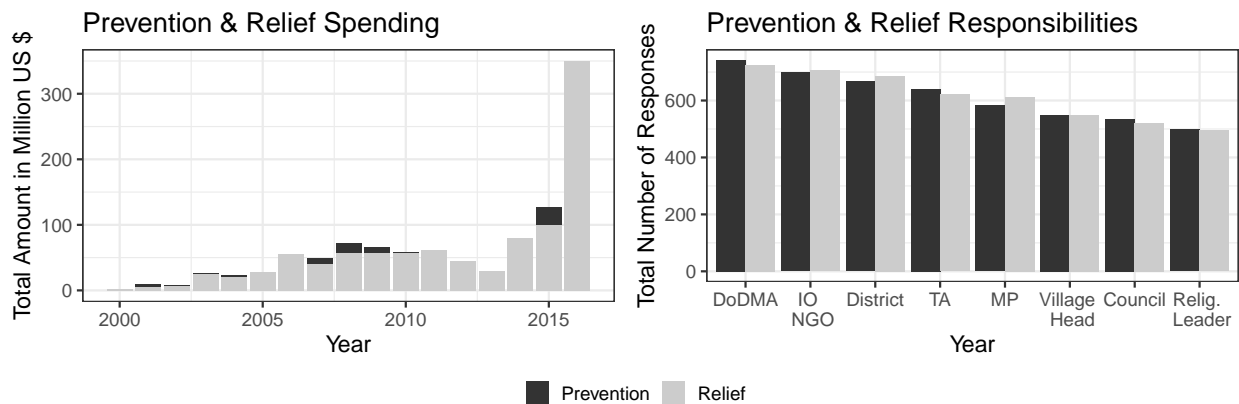
FIGURE 1. Natural Disasters in Africa (by year and country) 1970-2020 and disaster preparedness (by country)



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 panel left: total number of natural disasters by country. Lower panel right: 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.

number of disasters rising, so has spending on disaster relief. The left panel of Figure 1 depicts the increasing trend for relief spending in Malawi since the start of the century, which culminated in roughly US\$350 million spent after the flood disaster of 2015. By comparison, prevention and and preparedness spending only constitutes a small fraction

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 the Department of Disaster Management Affairs, National Resilience Strategy Report. Prevention aid spending (2000-2015) based on data from Peratsakis et al. (2012), including projects for disaster risk reduction and mitigation. To identify the year, I rely 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 our opinion, which actors are responsible for disaster response and relief?" Graph displays the sum of respondents who choose "very responsible" for a given actor.

of relief spending each year. In fact, for some years, there are no reported prevention and preparedness projects in the data.

Elected officials play a key role in disaster prevention and relief (Kita 2017). While the main authority for disaster preparedness and relief lies at the Department of Disaster Management Affairs (DoDMA), district commissioners and councils typically identify and distribute disaster relief.<sup>12</sup> 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 councils consists of elected councillors (one for each ward within the district) and members of parliament (MPs), together with ex-officio, and chiefs (traditional authorities-TA's).

The formal responsibility for the provision of local public goods lies also with district councils (Chinsinga 2005). In general, MPs play a key role in providing local public goods,

<sup>12</sup>"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/>

both formally and informally. Since 2006, MPs have discretion over constituency development funds to implement development projects in their district (Ejdemyr, Kramon, and Robinson 2018). MPs also support community-level disaster prevention and relief by mobilizing resources through the constituency development fund (Kita 2017) and help to organize resettlements<sup>13</sup>, provide logistics for relief items, and facilitate post-disaster reports (Kita 2017). The lower right panel of Figure 1 depicts survey evidence on the perceived responsibility for disaster prevention and relief by various actors. In line with the expectations, most respondents see the DoDMA as responsible, 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 that respondents also noted that the overwhelming majority of help came from international donors, followed, by a wide margin, by that from the DoDMA, MPs, and 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 from 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 that MPs can apply for and which they rarely rejected (Kita 2017, 12).

To sum up, 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

<sup>13</sup>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]

is considerable variation in the degree to which MPs promote public well-being or pursue their 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

This section proceeds as follows. First, I provide an overview of the design of the survey experiment to test the hypotheses. In sub-section 4.2, I provide an overview of the sample, sampling strategy, and time-line of the study. In sub-section 4.3, I proceed with the measurement strategy for the disaster policies used in the conjoint experiment. Lastly, I present details on estimating the treatment effects.

### 4.1. Research Design

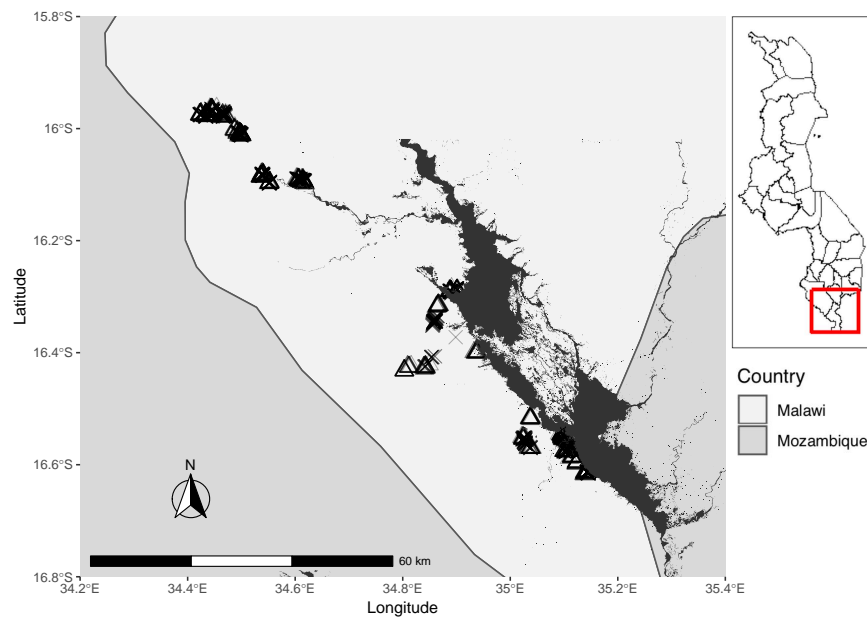
I test my main hypothesis experimentally. While voters are usually uncertain if disasters will occur and what policy actions politicians have taken in preparation and response, I design a survey experiment that alleviates these information asymmetries by informing participants that a disaster has occurred, 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 (Luce and Tukey 1964; Hainmueller, Hopkins, and Yamamoto 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.

### 4.2. Sample

The sample consists of 810 respondents from thirty-six villages in the districts of Nsanje and Chikwawa in southern Malawi. 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 randomized as well. While political competition in Malawi

is mostly organized around ethnic identities, regions themselves are rather ethnically homogenous. Chikwawa and Nsanje are both part of the Sena region (Robinson 2016). The data was collected in November 2018. As we can see in Figure 3, the sample is particularly useful in the study of preferences for disaster policies, as different subsets of villages were more or less affected by the 2015 floods. The data collection was not linked to the 2015 flood, and the villages were drawn from a stratified random sample.<sup>14</sup>

FIGURE 3. Map of Southern Malawi depicting the extent of the flooded area in 2015 (in black) and locations of surveys in 2016 ( $\triangle$ ) and 2018 ( $\times$ )



#### 4.3. 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*: that is, 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 that are randomly assigned across two hypothetical candidates. The list of attributes is shown in Table A4.

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,

<sup>14</sup>For the details, please refer to Appendix E.



expectations are measured through subjective probabilities, asking respondents about the probability that certain actions will lead to certain outcomes (Manski 2004; Delavande 2014; Manski 2018).<sup>15</sup> I infer voter expectations about preparedness and relief policies from the marginal effect of policy effort on candidate support. In particular, a first set of attributes informed participants if candidates invested low or high effort into preparedness and relief policies. A second set of attributes informed participants if the prevention and relief efforts were successful in changing welfare outcomes. Because each attribute was randomly assigned using a uniform distribution, high efforts were effective in roughly half of 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 that a policy will translate into welfare outcomes, i.e. their prior expectations about their effectiveness. This measurement strategy is based on the assumption that policy outcomes are a combination of candidates' efforts, candidates' competence and (un)lucky circumstances that the candidates cannot influence:

$$(4.1) \quad Outcome = Effort + Competence + Luck$$

Table 1 displays the attributes and corresponding levels. I measure *preparedness efforts* by the time candidates invest in a disaster preparedness plan.<sup>16</sup> I use disaster preparedness plans because they cover entire communities or regions and are a clear case of a local public good. I measure *effective preparedness* using a binary attribute that captures the extent to which a preparedness policy mitigated the negative impact of a disaster or not. 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. I measure *effective relief* coordination by donations of MP's to a village. I

<sup>15</sup>For example, one could imagine a question like: "What do you think is the percent chance that a prevention/relief policy is effective?"

<sup>16</sup>Previous scholarship has defined candidate efforts in terms of the number of bills submitted and approved by legislators (Ferraz and Finan 2009) or the attendance, participation, and initiative of MPs in parliament (Humphreys and Weinstein 2012).

choose this measure because MP's often use coordination with other authorities to raise funds and target supporters.<sup>17</sup>

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. 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 symbolic actions to signal to voters that they care about their constituency (empathy) and take their opinions and problems seriously.<sup>18</sup> Therefore, I include an attribute in which candidates visit the disaster, talk to victims, and declare their solidarity. Lastly, candidates may use relief policies to buy votes or embezzle money. If candidates have sufficient oversight over post-disaster relief, they may channel payments to voters in exchange for their votes i.e., vote buying. To measure both occurrences, I include an indicator for embezzling resources for personal use (corruption) and handing cash to buy votes (vote buying).<sup>19</sup>

Voters might also select politicians depending on other characteristics such as gender, political affiliation, or ethnicity (Adida 2015; Eifert, Miguel, and Posner 2010; Robinson 2016). To mitigate these concerns, I try to hold those factors constant by introducing candidates who are similar on those characteristics, but differ on their 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 came from the same tribe. However, there are important differences between the two:'*

<sup>17</sup>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".

<sup>18</sup>For example, Lazarev et al. (2014) found a strong positive correlation between Putin's visits to villages affected by a wildfire in Russia and his subsequent popularity.

<sup>19</sup>See wording in Table

TABLE 1. Conjoint Experiment: Exemplifying profiles of candidates, as shown to respondents

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

#### 4.4. 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, Hopkins, and Yamamoto 2014; Cuesta, Egami, and Imai 2019). The baseline is always level 0 of a given attribute.<sup>20</sup> As shown by Abramson, Koçak, and Magazinnik (2019), the AMCE combines both 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

<sup>20</sup>I use a uniform distribution when randomizing over levels of factors. Because attributes are randomly assigned, the given attribute level and attribute baseline profiles will have, in expectation, the same distribution for all the other attributes.

(Bansak et al. 2020).<sup>21</sup> Put differently, the AMCE in this context captures the selection process of the candidate (Ganter 2021). Additionally, I also report marginal mean, which is a factor level mean outcome ignoring other factors (Leeper, Hobolt, and Tilley 2020). 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:

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

where  $Y$  is the chosen candidate policy profile,  $j$  indexes the treatment level,  $\gamma_m$  indicates individual fixed effects, and  $Z$  is a set of indicators corresponding to the attributes, here  $Z = \{\text{Preparedness Effort, Relief Effort, Preparedness Effective, Relief Effective, Ask, Visit, Corruption}\}$ . Second, I estimate the conditional AMCE with respect to moderating variables  $T$ , economic losses and the psychological stress prime (Leeper, Hobolt, and Tilley 2020). I estimate equation 4.3 using OLS with interactions of attributes and moderators:

$$(4.3) \quad Y_{im} = \sum_{j \in Z} \beta_j Z_i^j + \sum_{j \in Z} \theta_j (Z_i^j * T_i) + \alpha T_i + \sum X_i + \gamma_m + \epsilon_i$$

The conditional ACMEs ( $\theta_j$ ) of self-reported economic losses must be interpreted with care because economic losses were not randomly assigned. The difference in conditional AMCEs ( $\theta_j$ ) is only causally identified for the randomly assigned prime. However, the extent of the flood in 2015 and the resulting economic losses had a large random component. So it is unlikely that individuals could predict the extent of the flood and therefore select out of the flood zone.<sup>22</sup> 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 MP.<sup>23</sup>

<sup>21</sup>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.

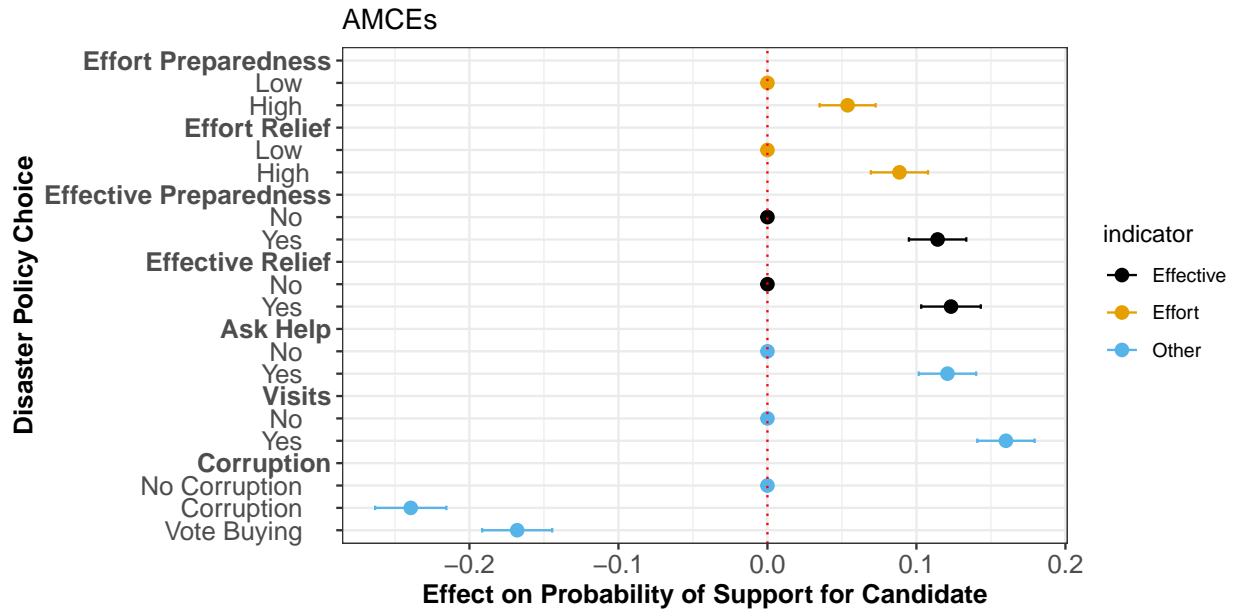
<sup>22</sup>See Figure A3 in the appendix for the predicted flood extent based prior data and the actual flood extent.

<sup>23</sup>See Appendix F for the description of the survey measurements.

## 5. EMPIRICAL FINDINGS

I will present three sets of results. First, I will examine the effects of the candidate's disaster policy attributes. Second, I will split the sample by respondents who suffered economic losses and those who did not and evaluate to what extent those two groups reacted differently to the attributes in the conjoint experiment. Third, I examine the effect of the financial stress prime by splitting the conjoint into those respondents who randomly received the prime and those who did not.

FIGURE 4. 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 4 displays the ACMEs for the complete sample. Five things can be noted. (1) On average, voters value effective outcomes over pure effort, regardless if the effort was exerted before or after the disaster. (2) Examining the effort measures in more detail, voters value candidate's relief efforts over preparedness efforts. (3) However, effective preparedness policies that mitigated the destruction of the disaster are as equally rewarded as actual post-disaster relief spending.<sup>24</sup> (4) Voters are not only value material benefits but

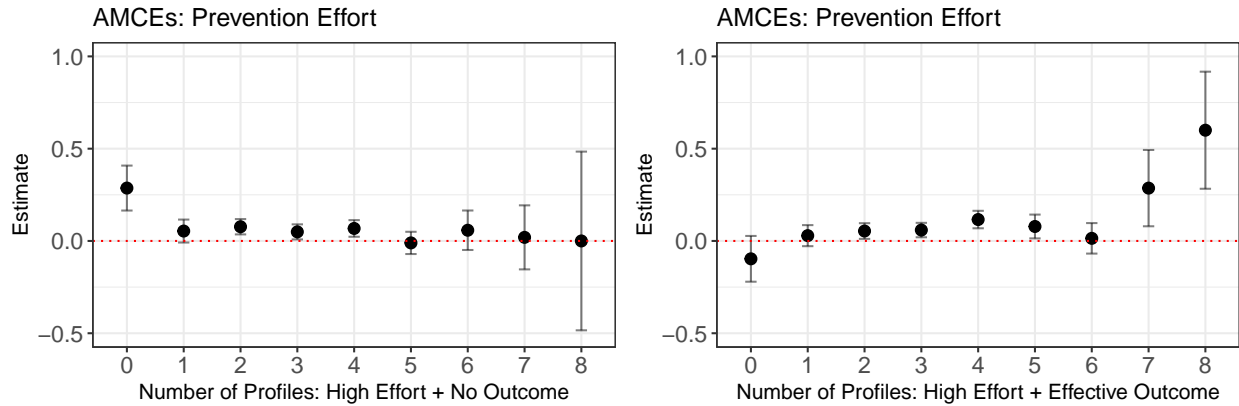
<sup>24</sup>See Tables A6 and A7 in the appendix for the formal analysis of the linear hypotheses.

also personal visits and solidarity. Personal visits by the candidate to the disaster site have the strongest positive treatment effect, indicating a strong signalling effect. It is striking that asking international actors for help is as equally valued as providing actual relief. These effects are likely driven by context, because international actors' aid is the primary source of relief (see Figure A2 in the appendix). (5) 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) and 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"). 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 across different rounds of the experiments, the patterns are robust and emerge in earlier and in later rounds (see Figure A8 in the appendix).

In conclusion, I can reject the null hypotheses for  $H_{1a}$  and  $H_{1b}$ . Pessimistic expectations about the effectiveness on preparedness seem to drive policy support. I find that voters value relief efforts significantly more than preparedness efforts. If preparedness is shown to mitigate disaster outcomes effectively, voters value it similarly to effective relief provision. The source for these pessimistic expectations can be manifold. It is worth noting that none the interaction effects between any attribute is 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 efforts are not driven by voter beliefs that candidates are corrupt and therefore may misallocate funding. Alternatively, these expectations might be a 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.

To provide further evidence on the mechanism, Figure 5 plots the AMCE of prevention efforts (Y) along different levels of effectiveness (X). I proxy the perceived effectiveness

FIGURE 5. Marginal Effect of Prevention Effort conditional on (In)Effective Outcome Combinations



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.

by the number of high effort and effective outcome profile combinations a respondent saw over the course of six profiles.<sup>25</sup> Descriptively, we can see in the left panel that the marginal effect of prevention effort is decreasing in the number ineffective effort profiles a respondent saw over the course of 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 prevention effort with respect to its effectiveness. The results provide some suggestive evidence that voters hold a-priori pessimistic expectations, but that these expectations might change if voters observe a sufficient amount of successful prevention policies.

### 5.1. Conditional AMCE's by Respondent Affectedness

Having established that expectations about effectiveness of prevention and relief policy influences incumbent support, I investigate 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

<sup>25</sup>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.

primed *psychological, financial distress* due to a natural disaster. Economic losses are defined as a binary measure that takes on the value of 1 if the respondents reported in the survey that they were very badly harmed by the flood in 2015 and 0 otherwise.<sup>26</sup> 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. Bartoš et al. (2018) 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.<sup>27</sup> I use a hypothetical scenario about locusts destroying the harvest because it is a common problem that people encounter.<sup>28</sup> 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).<sup>29</sup> 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 0.04 percentage points on average. 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 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

<sup>26</sup>See the exact wording in Appendix F.4 and Figure A5 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 A7 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.

<sup>27</sup>The design was developed by Mani et al. (2013).

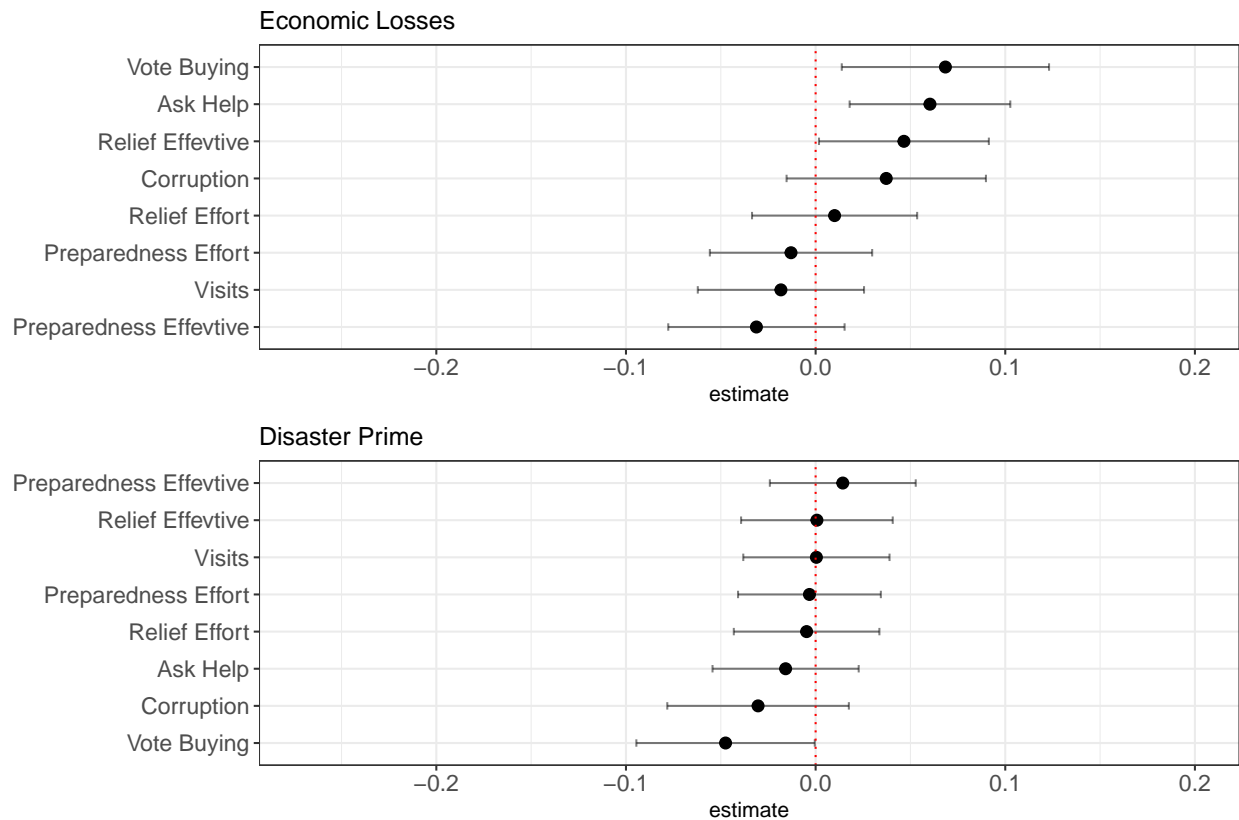
<sup>28</sup>The prime included an open-ended question: “*Treatment: Imagine you are a farmer and that locusts destroy your entire crop and the whole harvest is lost. How do you deal with this situation? Does it cause you serious financial hardship? Does it require you to make sacrifices? If so, what kind of sacrifices?*” For the details see Appendix F.1.

<sup>29</sup>The regression table including the interaction terms of treatments and attributes can be found in the section I in the appendix.



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.<sup>30</sup>

FIGURE 6. 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 in the appendix.

Next, I test the effect of psychological distress on voter preferences ( $H_{2b}$ ). Looking at the effect of the prime, 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 for the 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

<sup>30</sup>See regression Table A10 with controls in the appendix.

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 A3 in the appendix, 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 explore this possibility and evaluate the extent to which the prime had different effects for participants who experienced high losses or low losses during the natural disaster. As shown in Appendix I.1, 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). However, the average marginal effect of vote buying is still negative in the group with high losses. This economic effect persisted for two years after the disaster, likely because respondents did not receive sufficient help in the aftermath of the flood (see Figure A5). Thus, the results lend further support to findings that disaster events can alter political preferences (Fair et al. 2017) and can have long-lasting political consequences (Bechtel and Hainmueller 2011) and point to the importance of insurance against economic losses due to disasters (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 inflow of aid, potentially learn about the negative consequences, and become more dismissive of vote buying.

**5.2. Limitations and External Validity** Before reflecting on the substantive implications of the findings, there are several caveats to consider when generalizing the findings to (1) voting behavior in the real world, (2) to contexts where disasters are less frequent, (3) to other types of public goods, and (4) to other types of disaster exposures.

First, the study design did not allow for information asymmetries. Voters were perfectly informed about the occurrence of the disaster, the policy actions of candidates, and the subsequent outcomes. However, voters are typically uncertain about the probability of disasters and may lack information on the details of disaster policies. These aspects render the translation from preferences measured in this study to actual voting behavior difficult. In this regard, the results may be interpreted as an upper bound for the support of disaster prevention.

Second, 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 size. In this sense, it is a most likely case to find voter support for effective preparedness policies. 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, how informative are the findings about pessimistic expectations and policy effectiveness for debates about electoral rewards for other public goods? Before disasters occur, preparedness policies may constitute a particularly hard case to observe voter support because the costs are fixed and collective benefits only materialize if a disaster occurs, but not otherwise. However, this is the case for many other public goods like investments in public education, where benefits to citizens might only occur if the knowledge learned is useful in the labor market. Therefore, the findings should resonate well for public goods where the link between policy implementation and welfare results is fuzzy and hard to evaluate for citizens.

Fourth, the study explored the effect of financial worries and economic losses on voter preferences. However, I cannot rule out that actual disaster exposure manipulates other variables that also cause a change in voting behavior. Given that the timing of the natural disaster and the measurement of the preferences are three years apart, I can also not rule out that the economic losses shortly after the flood had a different effect on voter preferences.

## 6. CONCLUSION

Faced with the increasing number of natural disasters around the world, government action is central to ensure their citizens' welfare. The issue is especially important for the world's poorest regions, which are particularly vulnerable to natural disasters. To mitigate future calamities, politicians have to invest in disaster preparedness. 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 similar to effective relief. Second, I find observational evidence that respondents who experienced economic losses from recent disasters reward candidates who provide short-term relief payments and are more forgiving of candidates who provide short-term cash in exchange for votes.

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 because 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 preparedness. 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.

Second, the paper also points to the importance of insuring vulnerable populations to economic shocks from natural disasters. I find suggestive evidence that economic hardships due to natural disasters could induce a demand for relief, even in the form of vote buying. Given that politicians typically have a fixed budget, this might lead to less spending on public preparedness. This could have far reaching consequences. Suppose negative economic shocks causally increase the demand for relief and citizens learn about the benefits of public preparedness through their experience. In that case, frequent economic shocks could lead to an equilibrium with high demand for relief. Thus, the disasters themselves could undermine the long-term investment in public preparedness that would mitigate such disasters in the future, creating a “disaster trap”.

There are several tasks for future research. First, to better understand how to increase public support for preparedness policies, future research should explore what beliefs voters hold about the effectiveness of prevention policies. As emphasized by Gailmard and Patty (2019), informing citizens about the general efficiency gains of prevention vis-à-vis relief is likely not sufficient as long as there is asymmetric information about the need for and benefits from prevention in the 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 Manino (2018) show that information about the relative payoffs of preparedness can lead respondents to update their stated preferences in favor of preparedness. 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 incumbents who implement such policies. Lastly, more research is needed to understand the mixed results for the different measures of natural disaster exposure. Future research should also replicate the findings on the effects of self-reported economic losses, using stronger research designs that provide exogenous variation to income.

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## **APPENDIX A. CHANGES TO THE PRE-ANALYSIS PLAN**

- Hypothesis: The effectiveness hypothesis (H1) was not pre-registered.
- Hypothesis: To analyse 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 [A7](#) 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 in the main text and report the effect of distance to the disaster in the appendix.
- Analysis: Marginal means were not pre-registered
- Analysis: Marginal effect of effort conditional on high effort was not pre-registered

## **APPENDIX B. ETHICS**

The survey questionnaire was reviewed and approved by the Malawi Institutional Review Board (IRB) via the Institute of Public Opinion Research (IPOR), Malawi. In addition, this research followed the Swedish Data Services regulations and guidelines for research ethics.

## APPENDIX 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 rescaled	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

## APPENDIX D. BACKGROUND FLOOD 2015

FIGURE A1. Timing of Elections, Flood and Data Collections.

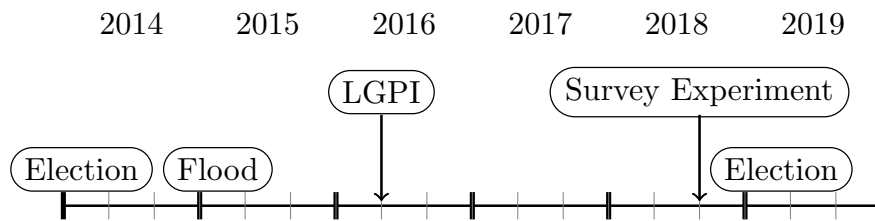
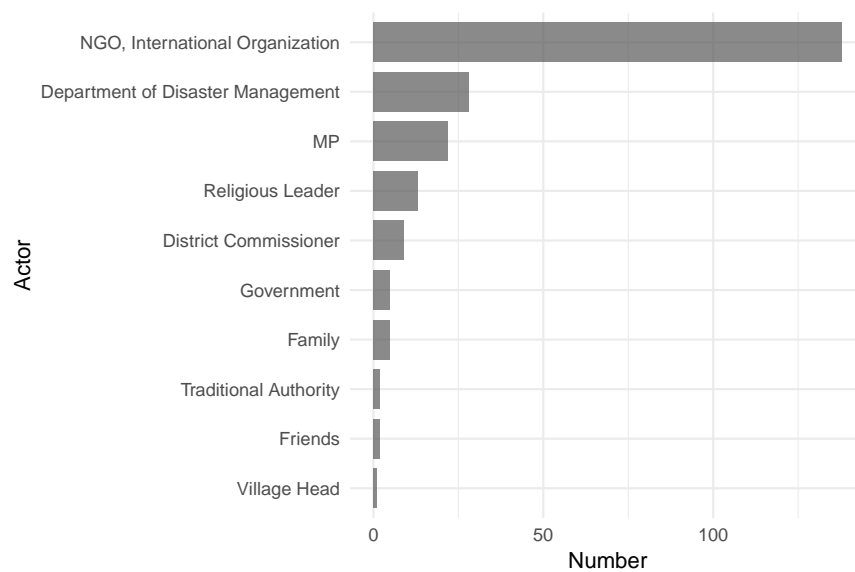
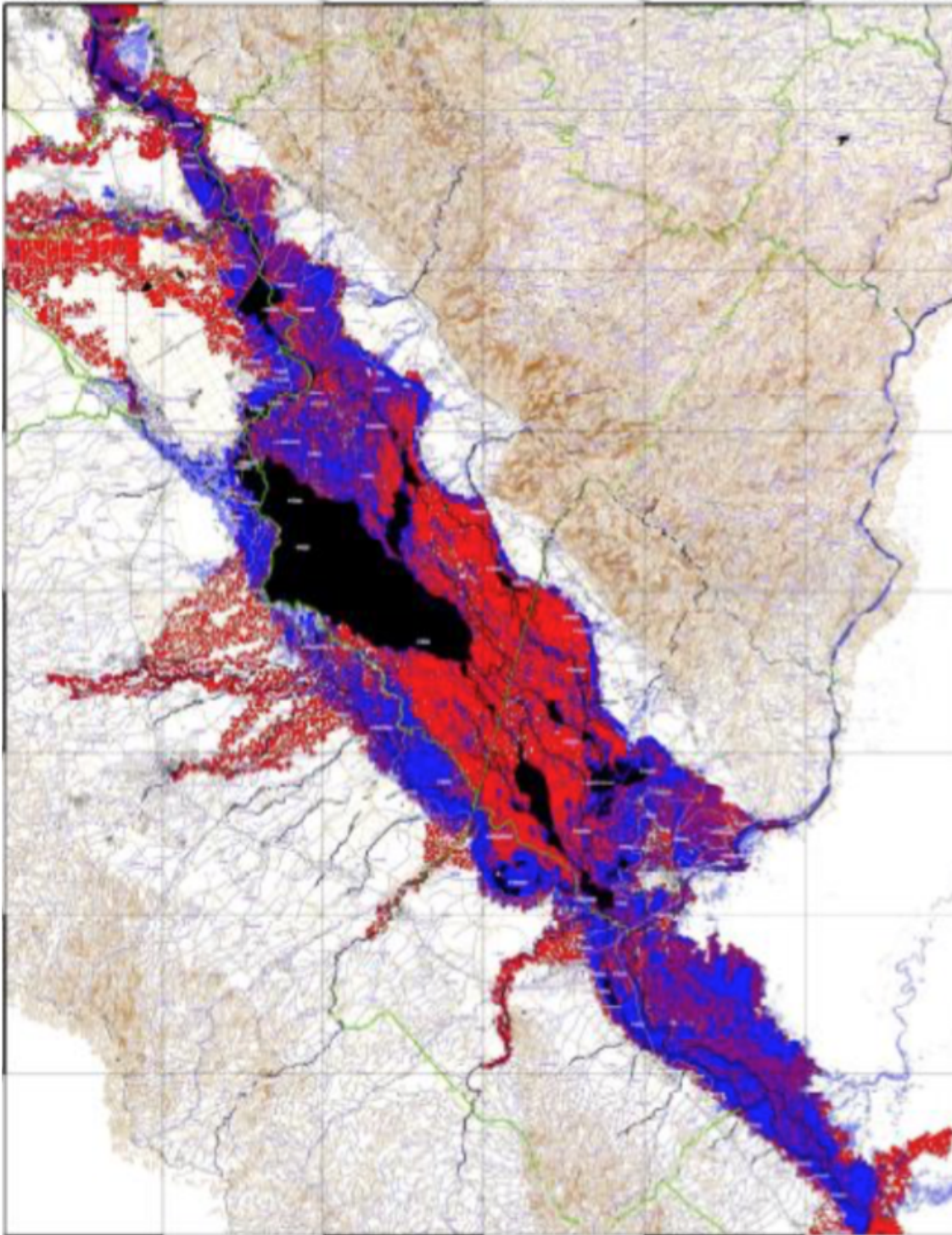


FIGURE A2. Help Received after the 2015 Flood



*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 the red color represented the modelled floods based on prior data. Black color represents permanent water bodies. Source: PDNA-Report ([2015](#)).



## APPENDIX 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 located 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”.

## APPENDIX F. SURVEY

**F.1. 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.<sup>31</sup> 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?

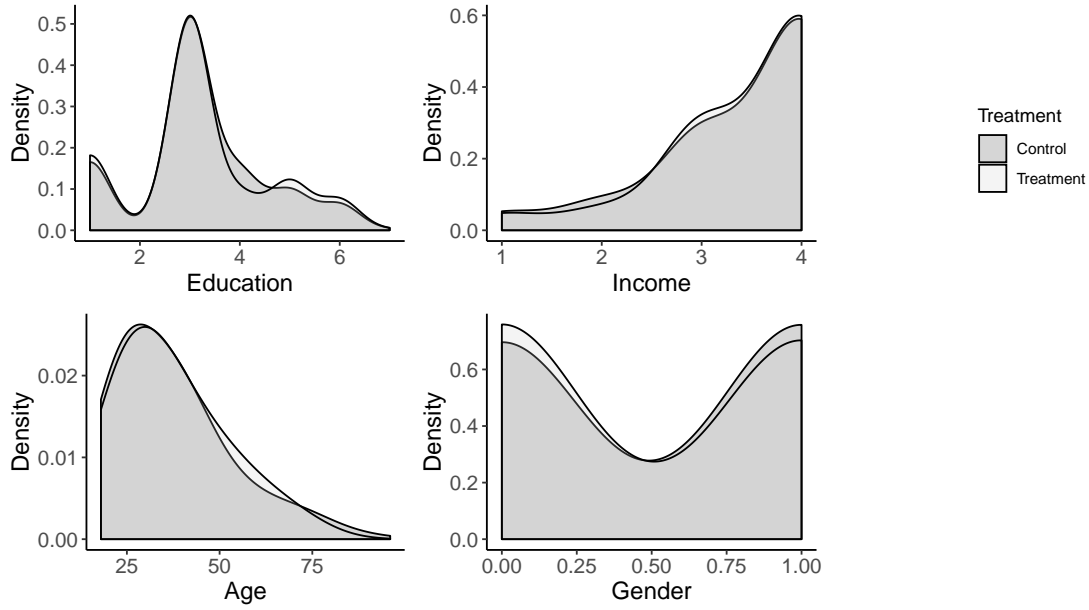
**Control:** [empty]

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<sup>31</sup>See: <https://mwnation.com/2016-locusts-worsened-food-shortage-in-shire-valley/>

**F.2. Balance Tests Prime-Conjoint Experiment** Figure A4 shows the balance of covariates for respondents who randomly received the prime (treatment) and those who did not (control).

FIGURE A4. Balance Tests : Respondent Characteristics (Distress Prime Experiment)



**F.3. Manipulation Check: Economic Distress Prime** Table A3 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

TABLE A3. 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

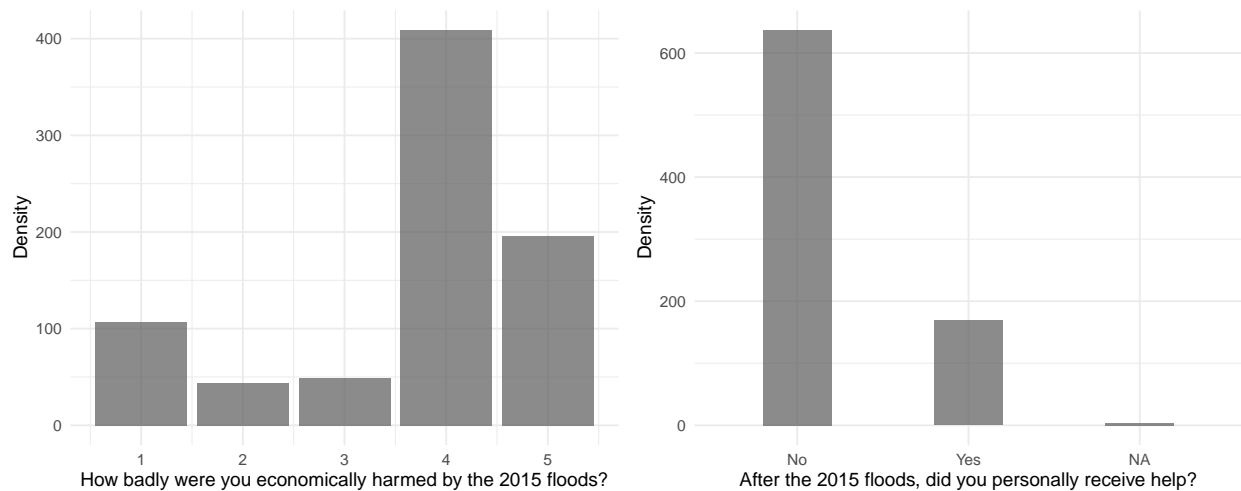
\*\*\* $p < 0.001$ , \*\* $p < 0.01$ , \* $p < 0.05$ ,  $p < 0.1$

**Notes:** OLS estimates with robust standard errors within parentheses.

#### F.4. Economic Losses

- How badly were you economically harmed by the 2015 floods?  
1 Not at all  
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 A5. Flood exposure and Help



**F.5. 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. MAS-DAP, 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):

$$(F.1) \quad D_i = -\left(\frac{x_i - \min(x)}{\max(x) - \min(x)}\right) + 1$$

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 A6 shows a graphical presentation of the measure for the surveys in 2016 and 2018 respectively.

FIGURE A6. Distance to Flood (2015) in 2018.

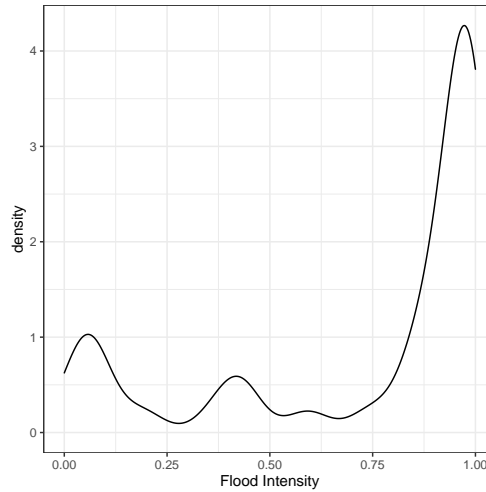
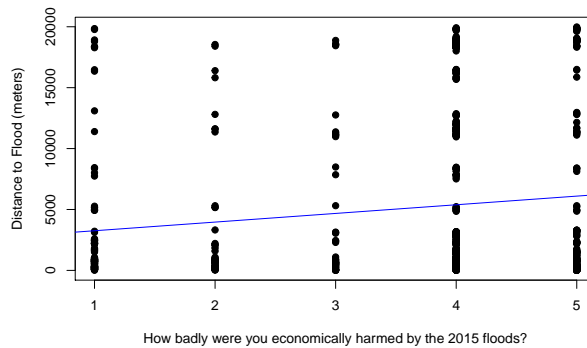


FIGURE A7. Scatterplot Economic Distress (X) and Distance to Flood (Y)?



## APPENDIX G. CONJOINT EXPERIMENT

TABLE A4. Conjoint Experiment: Full Text

Factor	Level
<b>Effort</b>	The candidate:
Prevention	[0] <i>did not</i> put a lot of work into a disaster preparedness plan to limit damages from natural disasters [1] <i>did put</i> a lot of work into a disaster preparedness plan to limit damages from natural disasters
Relief	[0] <i>did not attend</i> meetings to coordinate post-disaster relief [1] <i>did attend</i> 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
<b>Effective</b>	
Prevention	[0] his disaster preparedness plan was implemented, but was of <i>low quality</i> and did not limit the damages from the flood [1] his disaster preparedness plan was implemented, had <i>high quality</i> 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
<b>Honesty</b>	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

## APPENDIX H. 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 Coordination	0.05* (0.02)
Relief Coordination	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 Coordination × Relief Coordination	-0.01 (0.02)
Preparedness Coordination × Preparedness Effective	0.00 (0.02)
Preparedness Coordination × Corruption	0.01 (0.02)
Preparedness Coordination × 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 A8. Heterogeneity of AMCE by Number of Contest

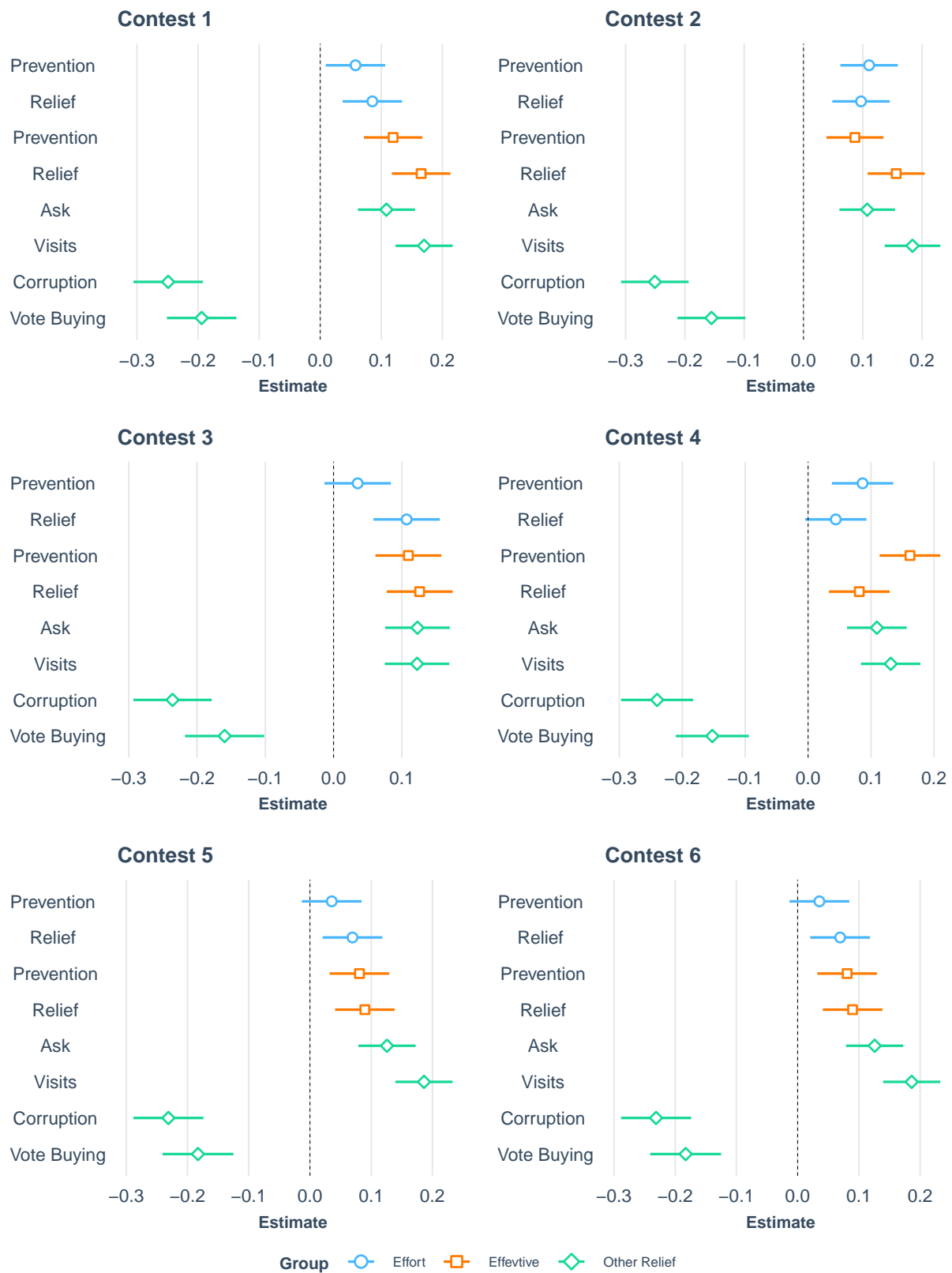
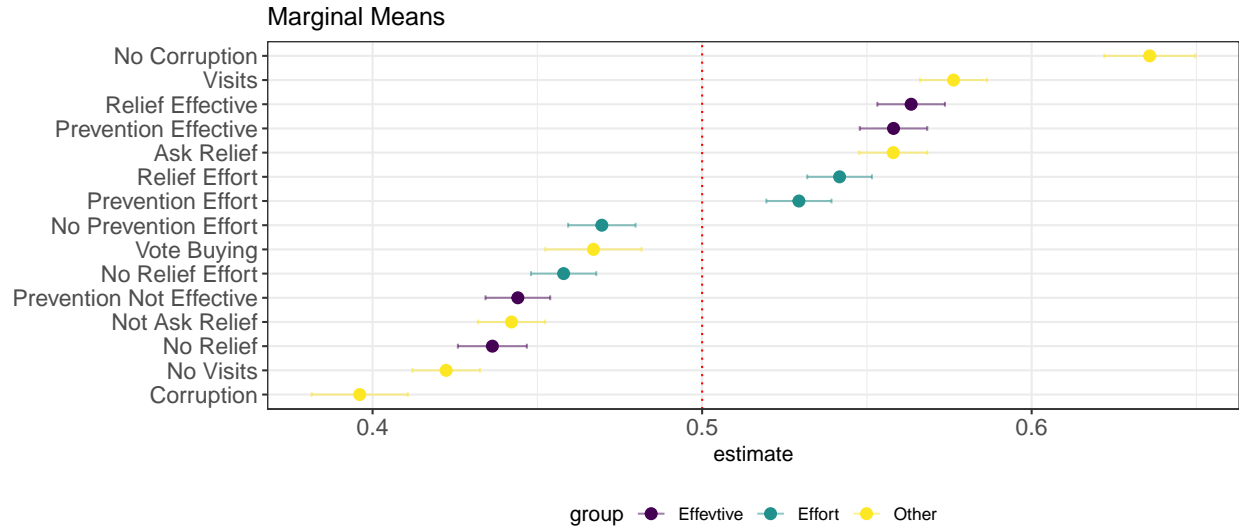


FIGURE A9. Marginal Means



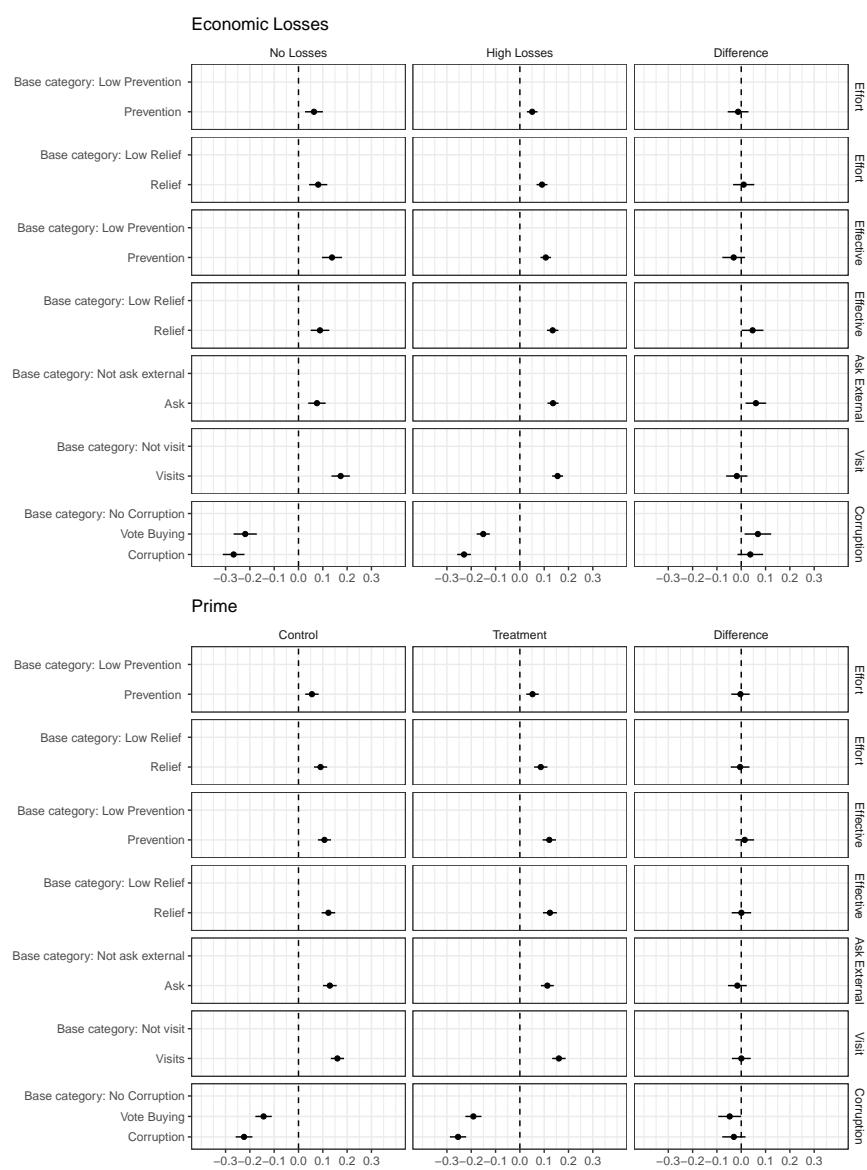
Notes: Beta coefficients from OLS regression with robust standard errors in parentheses. Standard errors are clustered at the individual level. Horizontal lines indicate 95% confidence intervals. The baseline is always the low level of the given attribute. For details of the attributes see Table ??.

## APPENDIX I. TREATMENT EFFECTS

Figure A10 displays the AMCE's for respondents with low or high economic losses in more detail. We can see that the two groups look very different. For individuals who experienced low losses, effective prevention has a stronger marginal effect compared to effective relief. The opposite is the case for individuals who experienced high losses. Both differences are statistically significant.

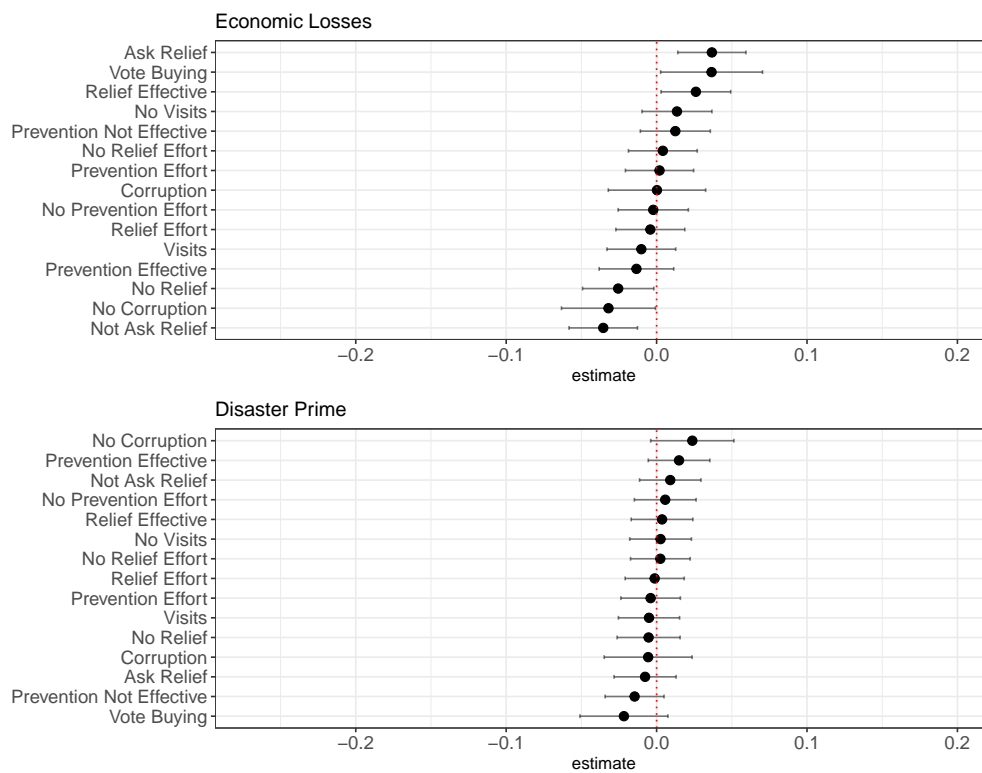


FIGURE A10. Effects of attributes on candidate support, by condition



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

FIGURE A11. 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.

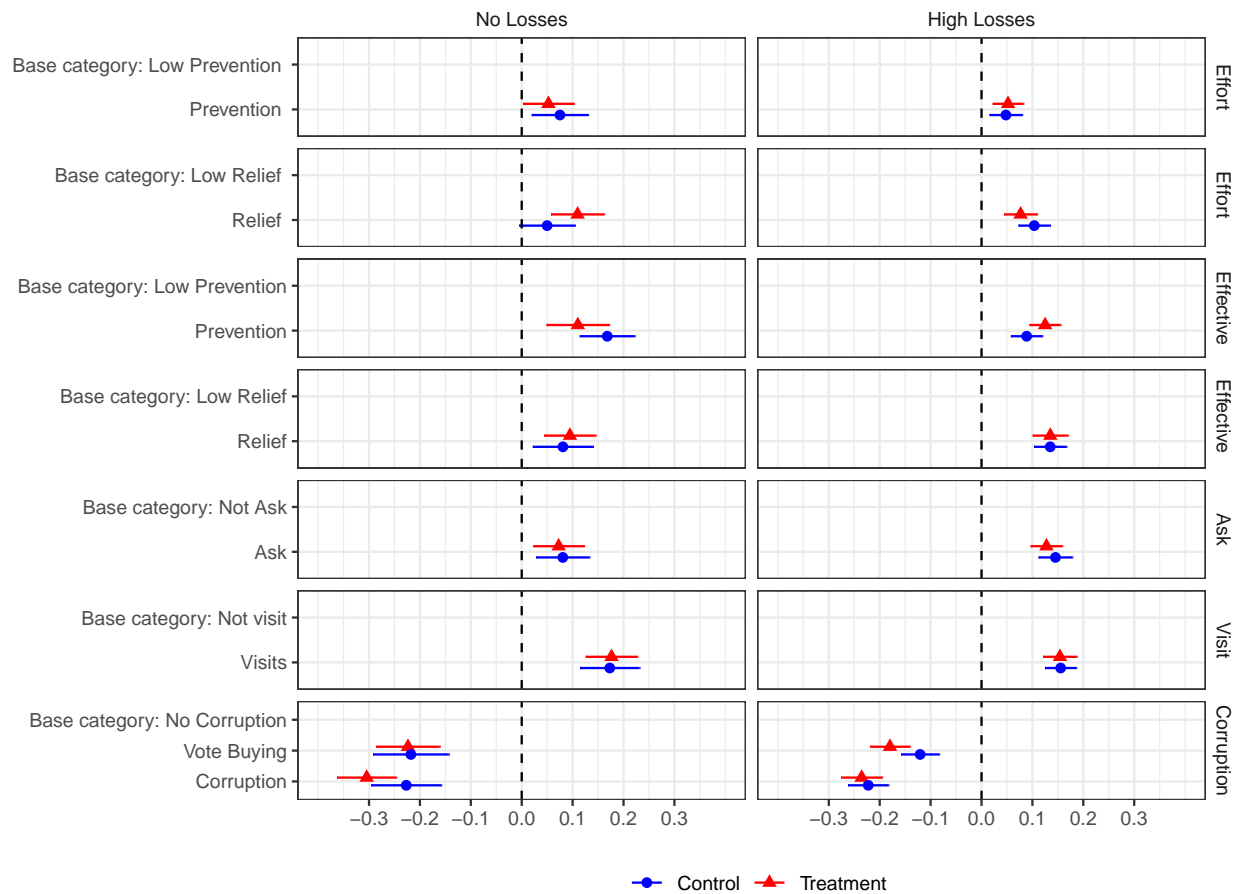
TABLE A9. Treatment Effects on ACMEs

Treatment	Prime	Economic Distress	Poverty
Preparedness Effort	−0.00 (0.02)	−0.01 (0.02)	0.02 (0.03)
Relief Effort	−0.01 (0.02)	0.01 (0.02)	0.00 (0.03)
Preparedness Effective	0.01 (0.02)	−0.02 (0.02)	0.01 (0.03)
Relief Effective	−0.00 (0.02)	0.04* (0.02)	0.01 (0.03)
Ask Relief	−0.01 (0.02)	0.06** (0.02)	0.05 (0.03)
Visits Effective	−0.00 (0.02)	−0.01 (0.02)	0.05 (0.03)
Corruption	−0.02 (0.02)	0.03 (0.03)	−0.03 (0.04)
Vote Buying	−0.04* (0.02)	0.07** (0.03)	−0.02 (0.04)
R <sup>2</sup>	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.

**I.1. Interaction Effect of Economic Losses and Prime** As we can see in Figure A12, 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 in corruption and increased the support for candidates who engaged in relief efforts.

FIGURE A12. 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. See regression Table A10 with control variables in the appendix.

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 (0.02)	−0.01 (0.02)	−0.01 (0.02)	−0.02 (0.02)	−0.02 (0.02)
Relief Effort x Economic Losses	0.01 (0.02)	0.01 (0.02)	0.01 (0.02)	0.02 (0.02)	0.01 (0.02)
Prevention Effective x Economic Losses	−0.02 (0.02)	−0.02 (0.02)	−0.02 (0.02)	−0.03 (0.02)	−0.03 (0.02)
Relief Effective x Economic Losses	0.04* (0.02)	0.04* (0.02)	0.04* (0.02)	0.04* (0.02)	0.04* (0.02)
Relief Ask x Economic Losses	0.06** (0.02)	0.06** (0.02)	0.06** (0.02)	0.05** (0.02)	0.06** (0.02)
Visits x Economic Losses	−0.01 (0.02)	−0.01 (0.02)	−0.01 (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** (0.03)	0.07** (0.03)	0.07** (0.03)	0.06** (0.03)	0.06** (0.03)
Poverty		−0.00 (0.01)	−0.00 (0.01)	0.00 (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 (0.00)	−0.01 (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 <sup>2</sup>	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.

## APPENDIX J. ADDITIONAL ANALYSES

### J.1. Disaster Preparedness

J.1.1. *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.

J.1.2. *Data Source* HFA National Progress

J.1.3. *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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