DO VOTERS REWARD RELIEF OVER PREVENTION? EVIDENCE FROM DISASTER POLICY PREFERENCES

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Abstract. Growing evidence suggests that voters reward politicians for disaster relief but not for prevention. Yet, there is little consensus on the mechanisms that underpin this pattern. This paper begins to fill this gap by exploring how voter expectations and income losses shape the demand for relief and prevention. I argue that voters have more pessimistic expectations about the welfare returns from prevention 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 prevention and relief policies. I find that voters do value relief efforts over prevention efforts when outcomes are uncertain. When prevention policies are known to be effective, voters value them similarly to effective relief policies. However, respondents who have suffered economic losses are more likely to reward effective relief.

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1. Introduction

Investment in public goods is an essential task of governments to ensure their citizens' welfare. Nevertheless, governments often allocate resources to private transfer at the expense of public goods (Keefer and Khemani 2005). One area in which investments in public goods play a crucial role for citizens' welfare is natural disasters. While the number of floods, epidemics, and storms is rising (Strömberg 2007), investment in public prevention policies that could potentially mitigate their effects remain few compared to individual relief transfers.[1] This is especially important in developing democracies where the population is 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 by rewarding incumbents more for investment in disaster relief compared to investment in preparedness spending (Healy and Malhotra 2009; Gallego 2018; Cavalcanti 2018). Importantly, these studies measure the electoral returns to relief and prevention policies after a disaster has occurred. Therefore, the effects cannot be driven by voter uncertainty about the need for prevention policies. However, can we interpret these patterns as evidence that voters prefer relief over prevention? So far, we lack empirical evidence that directly measures voters' disaster policy preferences outside advanced economies.

This paper argues that voters do not prefer relief over prevention per se. Instead, this paper proposes two mechanisms that constrain voters and explain the observed patterns. First, voters have more pessimistic expectations about the effectiveness of prevention policies compared to relief policies. That is, voters expect that prevention policies are less likely to deliver the promised outcomes and increase their welfare compared to relief policies. Consequently, they will be more likely to support candidates who have implemented relief policies compared to prevention policies. However, if prevention efforts are effective, voters will reward them similarly to effective

[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. Disaster prevention is a special type of public good. It requires upfront costs and only provides non-excludable benefits in case of a disaster. This paper focuses on settings in which disasters occur frequently. Therefore, well implemented prevention policies can be considered local public goods with immediate benefits.

relief. Second, even if prevention policies are effective, I argue that economic losses due to disaster can induce a demand for short-term 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 will be more likely to support candidates who provided effective relief.

Empirically, it is challenging to infer voter preferences relating to natural disaster policies from voting data. It is usually unclear what information voters had about the probability of a disaster, disaster policies, and the performance of politicians when they cast their vote. For example, relief policies might be more salient or visible to voters than prevention policies (Mani and Mukand 2007; Ashworth 2012; Healy and Malhotra 2013). To overcome this issue, this paper relies on a conjoint experiment (Luce and Tukey 1964; Hainmueller, Hopkins, and Yamamoto 2014; Bansak et al. 2020) embedded in a face-to-face survey fielded on a representative sample across thirty-six rural villages in Southern Malawi, a region frequently hit by natural disasters. The region experiences an annual wet season that leads to severe floods almost every year. The experiment informed participants about a hypothetical scenario in which a flood disaster has occurred in a region similar to their own but indicates that two candidates engaged in different prevention and relief policies. By randomly manipulating various features of contrasting disaster policies, the design allows me to identify the causal effect of each candidate policy choice on voters' support. To study the importance of voter expectations about policy effectiveness, a first set attributes informs participants about the efforts a candidate exerted to implement a prevention and relief policy, leaving the effectiveness uncertain. The second set of attributes informs participants if the prevention and relief efforts were effective. This design allows us to compare voter support for relief and prevention under unknown or known effectiveness. 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 experimental evidence that voters value relief efforts over prevention efforts if effectiveness is uncertain. Contrary to interpretations suggesting voters' general distaste for prevention policies, I find that respondents value prevention similarly to relief if it is shown to be effective in mitigating disaster damage. Strictly speaking, the paper cannot specify why voters prefer relief efforts over prevention efforts if effectiveness is uncertain. However, the experiment informs respondents that a disaster has occurred in a region similar to their own and how different candidates prepared and responded. Therefore, the low support for prevention policies cannot result from the fact that voters are uncertain about the occurrence and prior probability of the disaster. Second, I find suggestive evidence that recent economic losses can influence voter preferences. Voters who report having experienced actual 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. 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 find no evidence groups that show more or less financial distress have different preferences. Second, I find no evidence that the results are driven by poverty levels per se. Together, these findings call for a nuanced interpretation of the low electoral returns for prevention and high returns for relief. At least partly, the low returns to public prevention policies can be traced back to pessimistic expectations of voters about effectiveness. In fact, I only find a higher demand for material relief over effective public prevention for the subgroup of respondents who suffered recent economic losses.

The paper contributes to several strands of literature. In a narrow sense, it adds to the literature on the political economy of natural disasters. Recent observational studies have uncovered a positive correlation between incumbents re-election rates and relief spending (Healy and Malhotra 2009; Gasper and Reeves 2011; Bechtel and Hainmueller 2011; Cole, Healy, and Werker 2012), but no or weaker correlations with prevention spending (Healy and Malhotra 2009; Cavalcanti 2018). Some authors have interpreted this association as evidence that voters prefer relief over

prevention. However, Gailmard and Patty (2019) have shown formally that voters would reward relief over prevention if they were uncertain about the effectiveness of prevention.[2] This paper finds evidence that balances both views. On the one hand, I find that voters are less likely to reward candidates for prevention efforts than relief efforts even in cases where voters know a disaster has happened and in contexts where disasters are frequent. On the other hand, if the effectiveness of prevention policies is certain, voters will value it in the same way as effective relief. Thus, I do find an indication that low electoral returns to prevention spending could be driven by lower expected welfare returns for prevention policies.

Relatedly, the paper also adds evidence on the effect of voter vulnerability and the demand for private goods (Dixit and Londregan 1996; Bobonis et al. 2017). Empirical evidence suggests that politicians are more likely to engage in vote buying after a disaster due to increased resource availability, thereby increasing their re-election rate (Gallego 2018; Cavalcanti 2018; Visconti 2018). To date, we lack evidence as to whether voters have a demand for private and clientelistic goods after 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.

More broadly, the results speak to the large literature on the electoral incentives for public and private goods provision in developing democracies (Keefer and Khemani 2005; Mani and Mukand 2007; Harding 2015). In this regard, the low electoral returns from prevention policies are often interpreted as an indication that voters prefer private over collective goods (Healy and Malhotra 2009).[3] Previous work suggests that politicians in young democracies cannot commit to public goods policies because voters do not trust that politicians will deliver on their programmatic pre-electoral promises. As a result, voters are more likely to vote for candidates who make

[2]In their model, the uncertainty about the effectiveness of prevention efforts arises because voters are less informed than politicians about the need for prevention and if politicians are "corrupt" in the sense that they privately benefit from prevention spending.

[3]Alternatively, voters may be myopic in the sense that they are more likely to reward short term benefits over long term benefits (Healy and Malhotra 2009; Achen and Bartels 2017). However, this paper does not focus on this channel.

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.

The importance of policy effectiveness also speaks to the mixed evidence from field experiments that study the effect of performance information on voting (Banerjee et al. 2011; Humphreys and Weinstein 2012; Dunning et al. 2019a). Studies typically provide information on different performance dimensions such as policy efforts (i.e., legislative efforts) and/or outcomes (public goods provision, misallocated spending) (Adida et al. 2020). The current paper is novel in being able to separate voters' preferences for policy efforts and policy outcomes within the same policy area. My findings indicate that voters reward both policy efforts and outcomes, but care more about outcomes.

2. Theoretical Background

The literature on electoral accountability and retrospective voting suggests that voters reward the past performance of politicians, for example policy efforts or welfare outcomes (Key 1966; Barro 1973; Ferejohn 1986), or use past performance as an indicator to select candidates who will increase their welfare in the future (Fearon 1999; Besley 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. On the other hand, relief policies typically provide private goods such as food, cash, or building materials. 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 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.

A key limitation of the existing empirical studies is that they 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. **Effectiveness of Prevention 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.[4] 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). Indeed, there is also considerable empirical evidence from developing democracies that citizens vote on the basis of material considerations such as cash, jobs, or other private goods (Hicken 2011). But voters also form subjective beliefs (expectations) about how policy efforts translate into the outcomes they desire.[5]

Thus, voters face the problem of inferring the welfare consequences of a candidate's disaster policy given their beliefs and expectations. Assuming voters attempt to maximize welfare, they have to consider how specific policy actions will influence their welfare. 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. Indeed, Gailmard and Patty (2019) show formally that voters would prefer relief over prevention policies if they were more uncertain about the effectiveness of prevention.[6] There are several reasons why voters might be more uncertain about the effectiveness of prevention efforts than relief efforts. First, the benefits of prevention policies will only materialize once a disaster has

[4]See Gintis (2014) for a general discussion.
[5]I define an expectation as a probability distribution that maps policy actions into future outcomes.
[6]In Gailmard and Patty (2019), 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.

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). While there is also some empirical evidence on the misuse of public spending in the context of disaster relief (Garrett and Sobel 2003), 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. First, candidates' relief efforts should be more valued than prevention efforts. Second, however, there should be no difference in voter rewards for candidates who provided effective prevention or effective relief.

- H_1: Voters will be more (less) likely to reward candidates for relief policies over prevention policies if effectiveness is uncertain (certain).

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 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 from long-term to short-term consumption. In terms of natural disaster policies, this mechanism could lead to a preference for candidates who provide short-term relief and/ or offer cash in the form of vote buying.

- H_{2a}: Voters who experienced economic losses will be more likely to vote for candidates who deliver relief benefits and provide cash to buy votes.

Second, natural disasters are a bundled treatment, affecting economic status and 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).[7] Related research has found that people affected by natural disasters that caused economic harm prioritized their personal welfare (Visconti 2018) and act more selfishly (Li et al. 2013). Leaving economic well-being constant, the disaster itself might induce short-term thinking and risk aversion. Thus, voters might prefer benefits that are delivered with little risk and that materialize in the short-term. Voters might prefer candidates that offer effective short-term 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 vote for candidates who deliver relief benefits and provide cash to buy votes.

[7]Some studies also suggest that natural disasters make people more likely to engage in risk-taking (Hanaoka, Shigeoka, and Watanabe 2018).

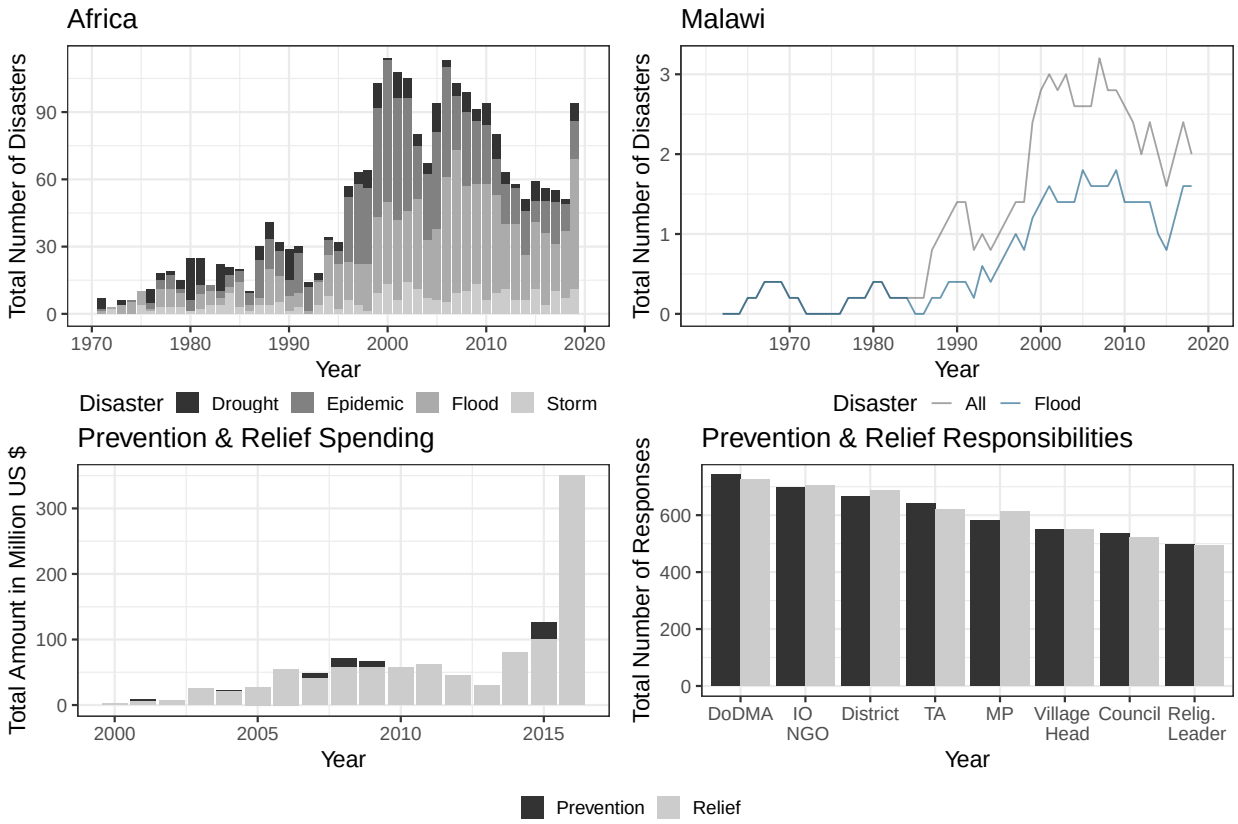
3. Case: Malawi

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 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 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). In March 2019, just a few months after this study was conducted, the country was hit by cyclone Idai leading to further destruction.

With the 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. However, the same figure also shows that the trend does not hold for spending on disaster prevention. By comparison, prevention spending only constitutes a small fraction of relief spending each year. In fact, for some years, there are no reported prevention and preparedness projects in the data. Elected politicians play a key role in disaster prevention and relief (Kita 2017).[8] While the main authority for disaster

[8]Malawi is divided into 28 districts, each administered by a District Council under the direction of a District Commissioner appointed by the President. District Councils consist of a combination of democratically elected councilors and members of parliament (MPs), together with ex-officio, non-voting members, including higher-ranked chiefs (traditional authorities-TA's). MPs are elected by plurality vote in 193 single-member electoral districts for 5-year terms.

FIGURE 1. Natural Disasters and Disaster Spending, 1970-2020.



Notes: Upper Panel: EM-DAT, Centre for Research on the Epidemiology of Disasters - CRED. Data for Malawi is displayed as five-year moving averages. Lower Panel: Prevention & Relief Aid Malawi, 2000-2016; Perceived Responsibilities. Left: 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: 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.

policies lies at the Department of Disaster Management Affairs (DoDMA), district officials, MPs, local councilors, and traditional authorities often play an essential role in disaster prevention and relief. Formal responsibility for the provision of local public goods lies with District Councils (Chinsinga 2005). However, district assemblies do not have much authority because of a limited ability to raise taxes, and they are dependent on the central government (Basurto, Dupas, and Robinson 2017). In general, MPs play a key role in providing local public goods, both formally and informally. Since 2006, MPs also have discretion over constituency development funds to implement development projects in their district (Ejdemyr, Kramon, and Robinson 2018). MPs

support community-level disaster prevention by mobilizing resources through the constituency development fund (Kita 2017). In terms of response, MPs help organize resettlements[9], 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 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 and MPs (see Figure 7 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 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 most likely case to find evidence for electoral rewards for disaster prevention.

[9]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]

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**

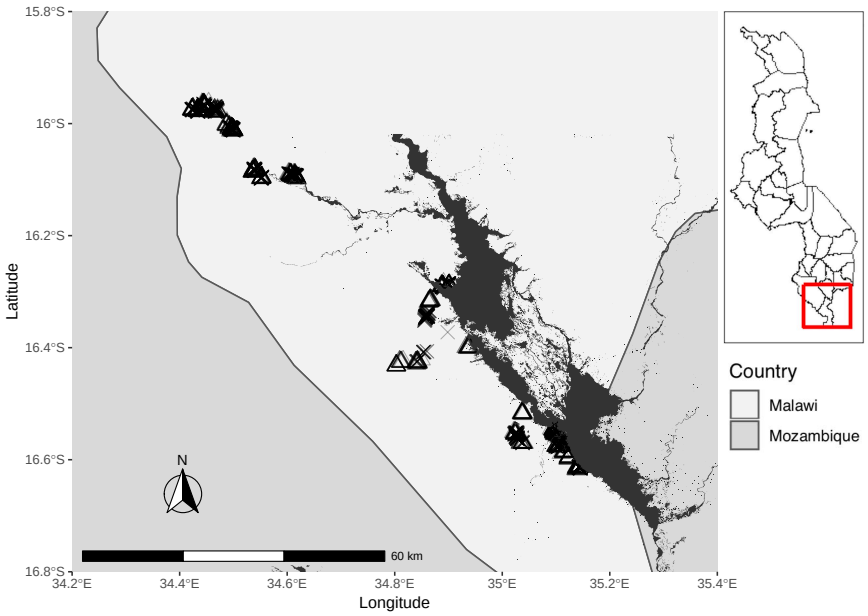
I test my main hypothesis experimentally. While voters are usually uncertain if disasters will occur and what policy actions politicians have taken in preparation and response, I design a survey experiment that alleviates these information asymmetries by informing participants that a disaster has occurred, but that counterfactual MPs prepared and reacted differently. To identify voter preferences for different disaster policy choices, I use a full factorial conjoint experiment (Luce and Tukey 1964; Hainmueller, Hopkins, and Yamamoto 2014). In the paired conjoint design, several disaster policy choices of two hypothetical MPs are randomly manipulated and respondents are forced to choose between them. Using this paired design, we can observe the effect of those manipulations on respondents' choice of who is elected. I randomly vary seven attributes of two political candidates running for MP regarding their prevention and relief policies. 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.

4.2. **Sample**

The sample consists of 810 respondents from thirty-six villages in the districts of Nsanje and Chikwawa in southern Malawi. The data was collected in November 2018. In particular, 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. As we can see in Figure 2, 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.[10]

FIGURE 2. Map of Southern Malawi depicting the extent of the flooded area in 2015 (in black) and locations of surveys in 2016 (△) and 2018 (×)



4.3. **Measuring Preferences for Candidates' Disaster Policies**

The conjoint design allows me to evaluate the marginal effect of a large set of prevention and relief policy attributes on respondents' approval of candidates. 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 4.

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 using direct questions asking respondents about the probability that certain actions will lead to certain outcomes (Manski 2004; Delavande 2014; Manski 2018).[11] I infer expectations from the marginal effect of policy effort on candidate support. The treatments informed participants

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

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

if candidates invested low or high effort into prevention and relief policies, leaving uncertain if they changed outcomes. 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 led to positive outcomes in roughly half of the vignettes and negative outcomes in the other half. Thus, voter support for the policy effort should be driven by voters prior expectations about the likelihood that a policy will translate into welfare outcomes, i.e. their expectations about their effectiveness.

I measure policy effort in the context of natural disasters in which candidates invest time into a disaster preparedness plan or allocate time to coordinate disaster relief.[12] I use disaster preparedness plans because they cover entire communities and are a clear case of a local public good. On the other hand, disaster relief is typically distributed to individuals. I measure effective outcomes using two attributes that capture the extent to which a prevention policy mitigated the negative impact of a disaster and the distribution of relief benefits by a candidate.

In addition to candidates' prevention and relief policies, 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, I include a treatment in which candidates visit the disaster, talk to victims, and declare their solidarity. Facing adverse events such as 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[13] 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). Before displaying the two candidate profiles, respondents read the following introduction:

[12]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).

[13]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.

'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:"

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

Factor (Z)	MP 1	MP 2
Effort		
Prevention	(0) *Did not* put a lot of work into a disaster preparedness plan	(1) *Did* put a lot of work into a disaster preparedness plan
Relief	(0) *Did not attend* meetings to co-ordinate disaster relief	(1) *Did attend* meetings to co-ordinate disaster relief
Effective Outcomes		
Prevention	(0) His disaster preparedness plan was of *low quality* and did not limit the damages from the flood	(1) His disaster 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 external funders	(1) *Did ask* for help from external funders
Visit	(0) *Did not visit* the disaster site	(1) *Did visit* the disaster site, talked to victims and declared his solidarity
Corruption	(0) Has no record of corruption	(1) Is convicted of corruption for embezzling humanitarian aid (2) Is convicted of corruption for handing out cash to buy votes
Profile Choice		
	☐	☐

4.4. **Estimand and Estimation**

The main estimand is the Average Marginal Component Effect (AMCE) for each attribute. The AMCE measures the marginal effect of a given factor (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.[14] 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 nessesarly 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. 2020).[15] First, I estimate the AMCE using an OLS regression with heteroskedasticity-robust standard errors (see equation 4.1). The standard errors are clustered at the level of the individual participant:

$$Y_{im} = \sum_{j \in Z} \beta_j Z_{ij} + \gamma_m + \epsilon_i \tag{4.1}$$

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 = {Prevention Effort, Relief Effort, Prevention 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 4.2 using OLS with interactions of attributes and moderators:

$$Y_{im} = \sum_{j \in Z} \beta_j Z_i^j + \sum_{j \in Z} \theta_j (Z_i^j * T_i) + \alpha T_i + \sum X_i + \gamma_m + \epsilon_i \tag{4.2}$$

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 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.[16] 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

[14] 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.

[15] 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, a marginal effects is informative.

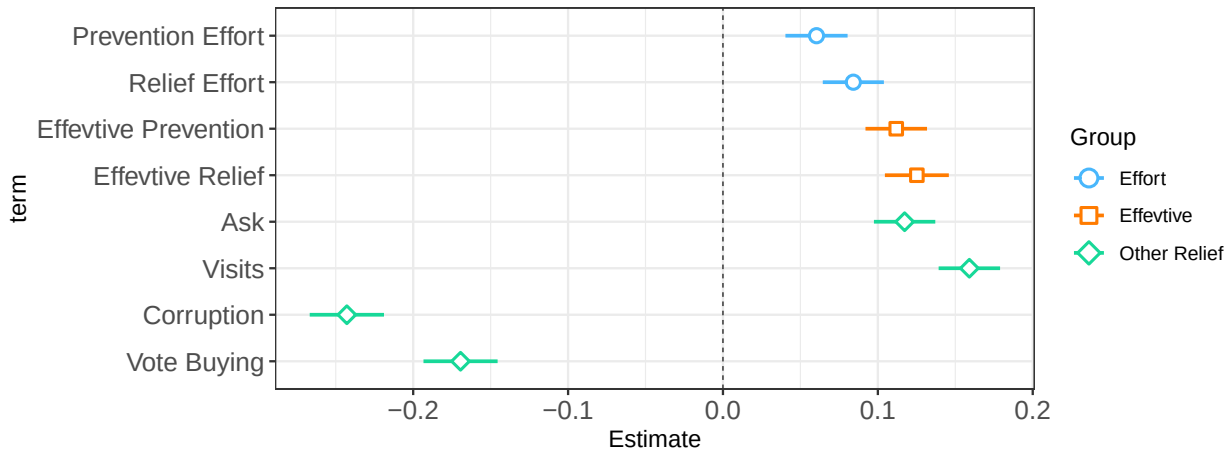
[16] See Figure 8 in the appendix for the predicted flood extent based prior data and the actual.

in politics, geographic distance to the flood in 2015, help received after the last disaster, and trust in MP.[17]

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 received the prime and those who did not. Lastly, I examine the interaction effects between economic losses and prime in the context of the conjoint experiment.

Figure 3. Estimated Average Marginal Component Effects (AMCEs)



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 1.

Figure 3 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. Investing time in prevention policies is especially low valued by voters. (2) Examining the effort measures in more detail, voters value candidate's relief efforts over prevention

[17]See Appendix F for the description of the survey measurements.

efforts. (3) However, this pattern is not observed if candidates provide effective prevention and relief. Effective prevention policies that mitigated the destruction of the disaster are are as equally rewarded as actual post-disaster relief spending.[18] (4) However, voters are not only interested in material benefits but also value 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 7 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 13 in the appendix). This suggests that respondents do not adjust their answers over the course of the experiment. It is also worth noting that none the interaction effects between any attribute is statistically significant (see Table 8 in the appendix).

In conclusion, I can reject the null hypothesis for the effect of effectiveness on prevention policy support. Once prevention is shown to mitigate disaster outcomes effectively, voters value it similarly to effective relief provision. The reasons can be at least twofold. Voters may, in general, be uncertain, about efficiency gains due to prevention investment. Alternatively, these preferences 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. Therefore, relief policies could be a better predictor of candidate type.

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

Having established that the uncertainty about policy effectiveness influences voter preferences,

[18]See Tables 6 and 7 in the appendix for the formal analysis of the linear hypotheses.

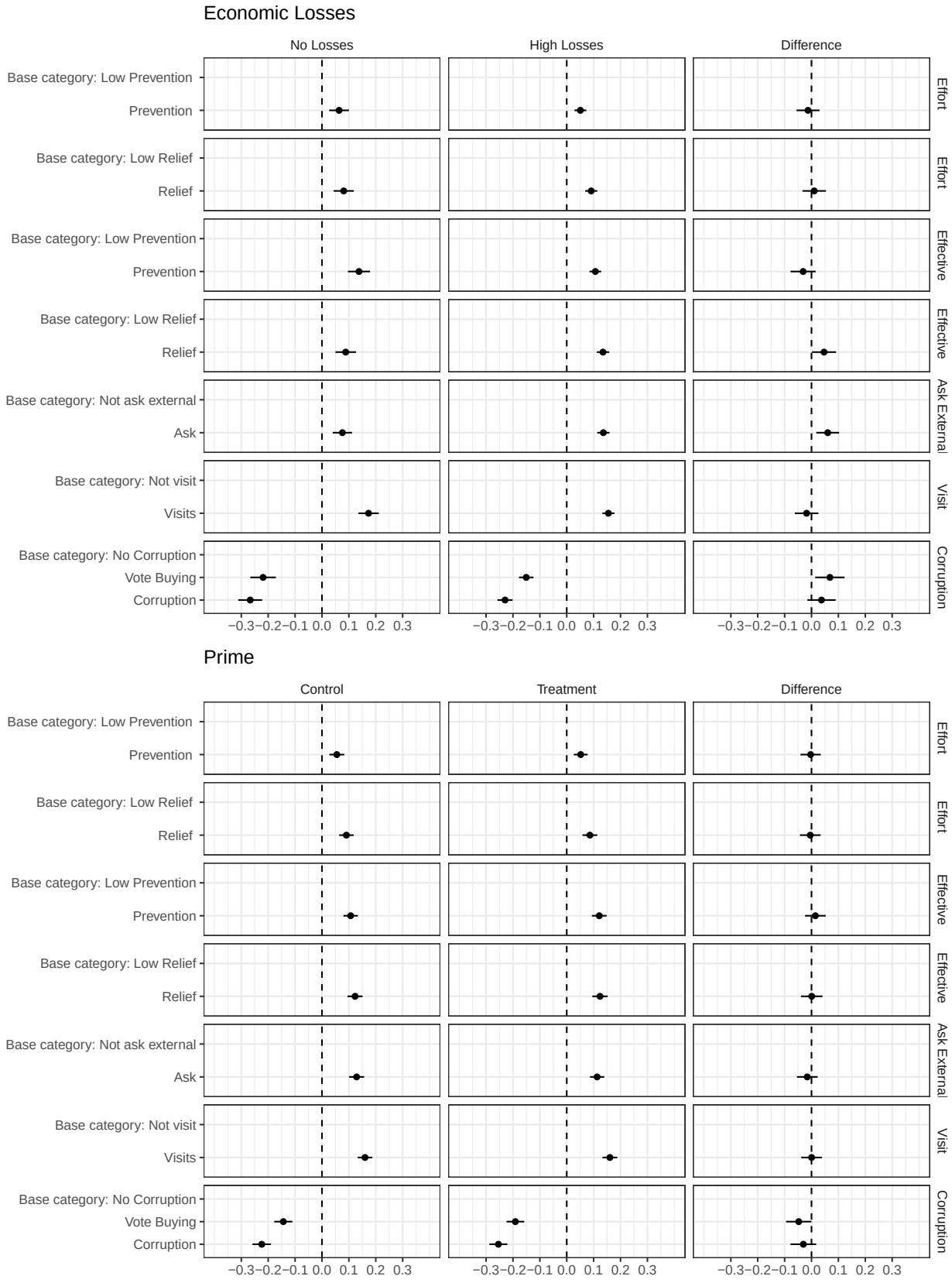
I investigate to what extent preferences are subject to change due to individuals' affectedness (Hypotheses 2a–2b). I measure exposure to the natural disaster with two indicators: self-reported *economic losses* due to the 2015 flood, and primed *psychological, financial distress* due to a natural disaster. 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.[19] 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.[20] I use a hypothetical scenario about locusts destroying the harvest because it is a common problem that people encounter.[21] The control group did not receive the prime.

[19]See the exact wording in Appendix G.1 and Figure 10 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 12 in the appendix, the pre-specified distance measure is a bad predictor of economic losses, the main concept of interest. Therefore, I report the effects of distance in the appendix.

[20]The design was developed by **mani2013poverty**.

[21]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.

FIGURE 4. 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 10 with controls in the appendix.

Figure 4 shows the conditional AMCEs, along with 95% confidence intervals, subset by economic losses and the disaster prime. The upper panels of Figure 4 display the conditional AMCE for respondents who experienced economic losses due to the flood and those who did not. The upper panels display the conditional AMCE for respondents who received the disaster prime or not. For each set, the far-right panel shows the point estimates of the differences in AMCEs for each attribute level between the two conditions, along with their 95% confidence interval. The regression table including the interaction terms of treatments and attributes can be found in the section H.1 in the appendix.

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 also worth noting that the point estimates for prevention efforts and effective prevention are lower for individuals who experienced high losses. The difference is not statistically significant. The results are robust to the inclusion of several control variables. Importantly, we do not observe the same effects if we subset the data into subgroups with low and high poverty (see Figure 14 in the appendix). Figure 5 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.

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

FIGURE 5. Subgroup ACMEs:Economic Losses.

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 10 with control variable in the appendix.

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 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 ?? 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 we can see in Figure 15, 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. Thus, we can conclude that the effects of the disaster prime null effects show some heterogeneous effects that were not predicted by the theory.

In sum, 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 10). 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). 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 Generalizability** Before reflecting on the substantive implications of the findings, there are several caveats to consider when generalizing the findings to voter behavior in the real world and possibly to other contexts. 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. In particular, the experiment informed participants that a disaster had occurred and how two different candidates prepared and reacted to it. Therefore, the results cannot be driven by any uncertainty about the disaster itself. However, voters and politicians are typically uncertain if and when a disaster may happen. Thus, the results could be interpreted as an upper bound for the support of disaster prevention. Nevertheless, 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 prevention policies. Second, 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 cause a change in voting behavior. Third, given that the timing of the natural disaster and the measurement of the preferences are three years apart, I cannot rule out that the economic losses shortly after the flood had a different effect on voter preferences. Lastly, the identified preferences may not translate directly into voting behavior. Voters can be subject to information asymmetries, cognitive limitations and be constrained by the collective nature of elections incentivizing them to free ride and not hold politicians accountable. These aspects render the translation from preferences for political selection measured in this study to actual voting behavior difficult.

6. Conclusion

Faced with the increasing number of natural disasters around the globe, 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 public preparedness that requires long-term investment with uncertain but potentially large benefits. Instead, politicians often rely on private transfers, such as relief payments. To date, we lack empirical evidence as to whether the underinvestment in prevention and preparedness reflects voters' preferences and, if so, what drives these preferences. Empirical evidence is especially rare for developing democracies.

This paper contributes to filling this gap by studying two mechanisms: how voter expectations about the effectiveness of prevention efforts shape support and how negative economic shocks can increase the demand for relief. The paper presents novel causal evidence using a survey experiment in Southern Malawi, a region that suffers from frequent disasters. Consistent with previous observational findings (Healy and Malhotra 2009), I show that voters reward candidates for relief over prevention efforts if their effectiveness is uncertain. However, if voters know that a candidate's prevention policy positively influenced outcomes, they value it similar to

effective relief policies. 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. Thus, the findings do not invalidate previous observational findings but instead call for a more nuanced interpretation. I find no difference in disaster preferences for respondents who were randomly assigned to a prime that altered financial stress. Thus, the evidence calls for a more nuanced interpretation of recent evidence concerning voters' reaction to natural disaster policies.

The results may also be informative to the broader discussion about the lack of public goods investment in many low and middle income democracies (Keefer and Khemani 2005; Mani and Mukand 2007; Keefer and Vlaicu 2008). I find that voters do reward candidates for the effective provision of broad public goods similarly to private transfers. However, the paper points to the central role of voter expectations about returns from public goods efforts as an explanatory factor. In particular, the low expectations for the effectiveness of public goods policies can undermine voter support. Given that many public goods policies come with some initial costs and collective benefits only unfold in the long-term, this could help to explain a low public goods equilibrium. One reason could be that insufficient prevention in the past undermines future investment in prevention. In particular, low expectations could be self-fulfilling if they lead politicians to invest less in preventative measures, making it less likely that citizens observe successful prevention policies, thereby leading to low expectations. However, prevention policies might be a particularly hard case because benefits are only realized if a disaster occurs.

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 prevention. This could have far reaching consequences. Suppose negative economic shocks causally suppress the demand for prevention and citizens learn about the benefits of public goods through their experience. In that case, frequent economic shocks could lead to an equilibrium with low demand for public goods. Thus, the disasters themselves could undermine the long-term investment that

would mitigate such disasters in the future, creating "disaster trap", similar in dynamic to accountability traps induced by pessimistic expectation about candidate type. My findings suggests that similar might hold also for policies.

There are several tasks for future research. First, to better understand how to increase public support for prevention 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 prevention in their concrete cases. To change voters' beliefs about the effectiveness of preparedness spending, they must be convinced that prevention will be effective in the context of their experience. Using a survey experiment in the US, Bechtel and Mannino (2018) show that information about the relative payoffs of prevention can lead respondents to update their stated preferences in favor of prevention. 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. 2019b). 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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Appendices

Appendix A. Changes to the Pre-Analysis Plan

- Theory: The effectiveness hypothesis (H1) was not pre-registered.
- Theory: The hypothesis on heterogenous treatment effect on self reported economic harm due to the flood (H2b) was not pre-registered.
- Analysis: 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 12 in the appendix, the pre-specified distance measure is a bad predictor of reported economic losses, the main concept of interest. Therefore, I report the effects of distance in appendix .
- Conjoint: The attribute “Visit” was called “Emotional Intelligence” in the pre-analysis plan and was categorized under “Compentence”

Appendix B. Ethics

The survey questionnaire was reviewed and approved by authorities in Malawi via the Institute of Public Opinion Research (IPOR), Malawi.

Appendix C. Summary Statistics

Table 2. 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%					
Choosen_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 3. 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

Appendix D. Background Flood 2015

Figure 6. Timing of Elections, Flood and Data Collections.

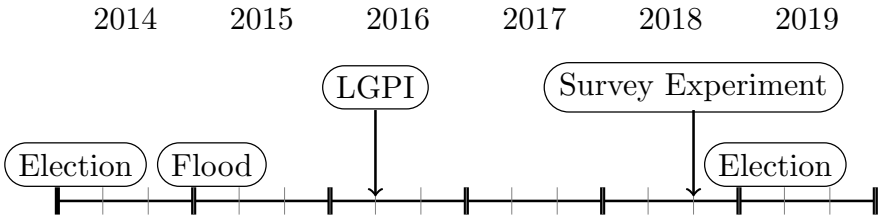
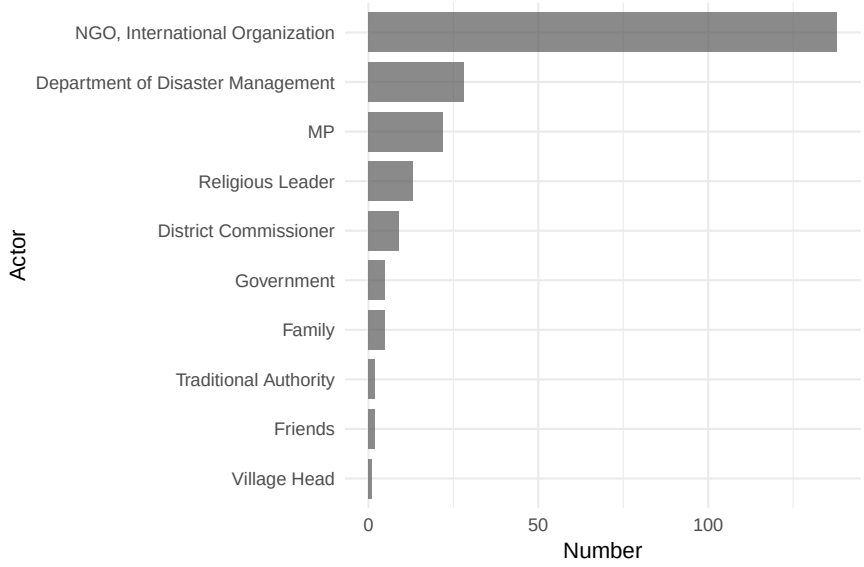
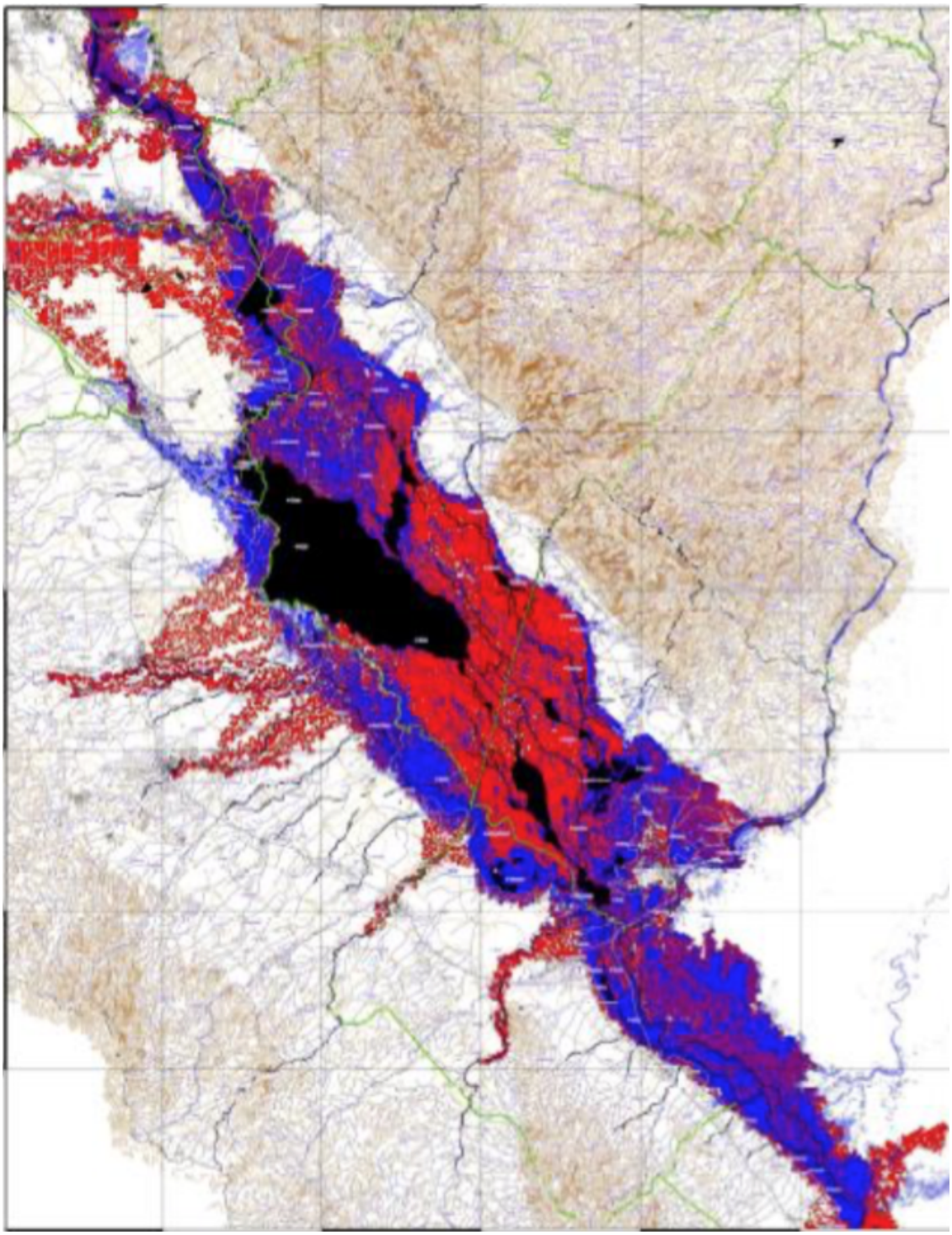


Figure 7. Help Received after the 2015 Flood



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



Appendix E. LGPI Sampling

The respondents from each data collection are not the same, but they were selected randomly from within the same villages. The 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.1 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.

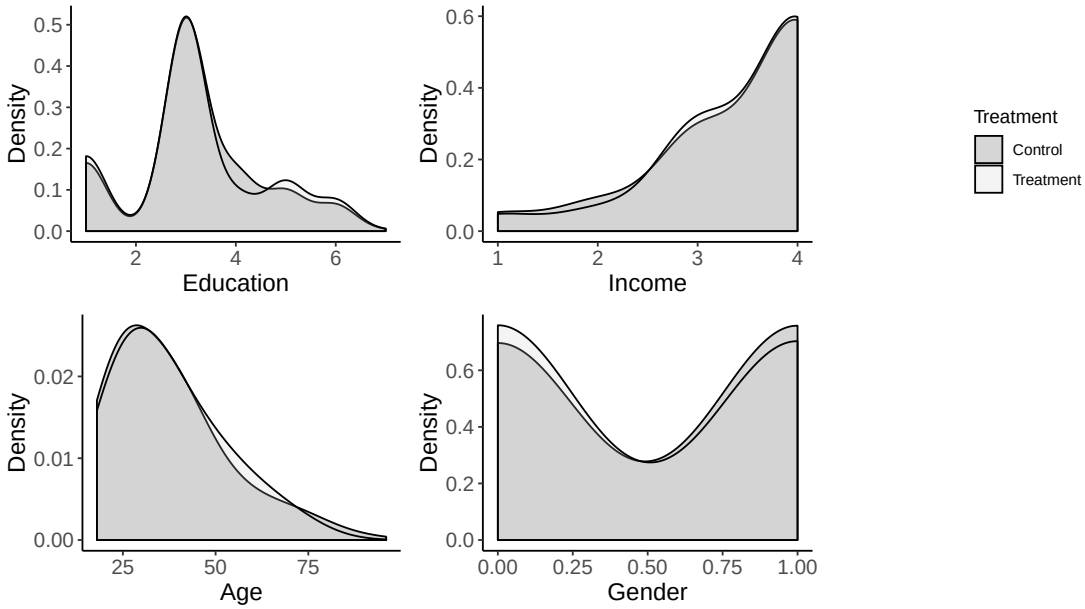
Appendix F. Survey and Survey Experiment

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.[22] 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]

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

FIGURE 9. Balance Tests : Respondent Characteristics (Distress Prime Experiment)



F.2. **Balance Tests Prime-Conjoint Experiment**

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

- **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

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.

Appendix G. Conjoint Experiment

G.1. **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 10. How badly were you economically harmed by the 2015 floods?

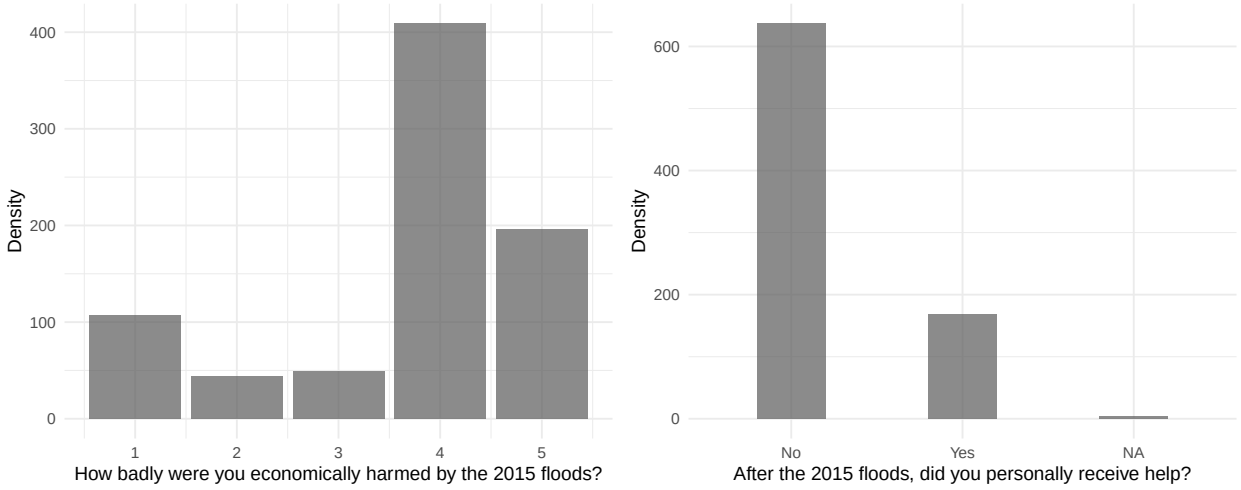


TABLE 4. Conjoint Experiment: Treatment Factors to Randomize for Two Candidate Profiles

	[Candidate Profile A] [Candidate Profile B]
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 NGO's 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
	Question: Please indicate which candidate you would vote for in the election.
	[Candidate Profile A] [Candidate Profile B]

G.2. **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):

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

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

Appendix H. Results

FIGURE 11. Distance to Flood (2015) in 2018.

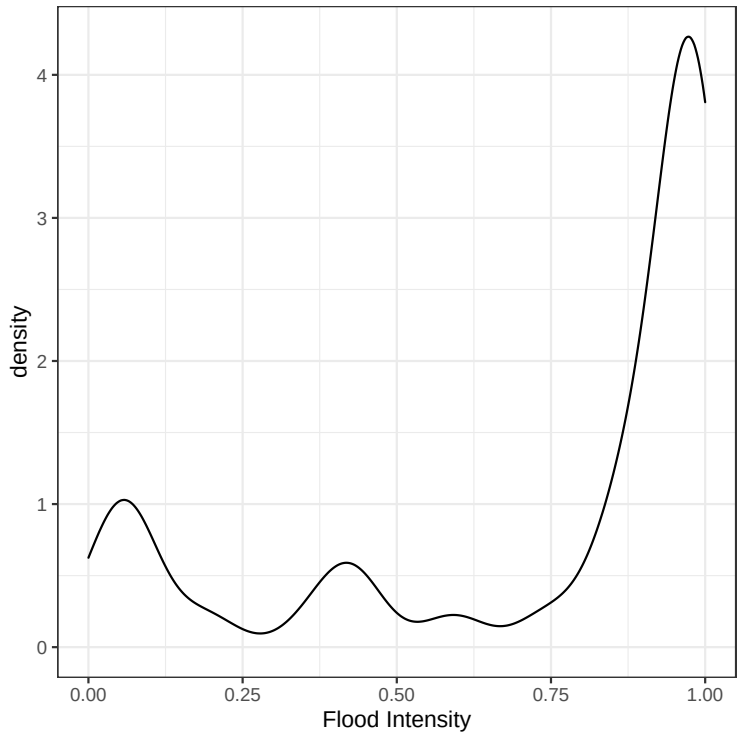


FIGURE 12. Scatterplot Economic Distress (X) and Distance to Flood (Y)?

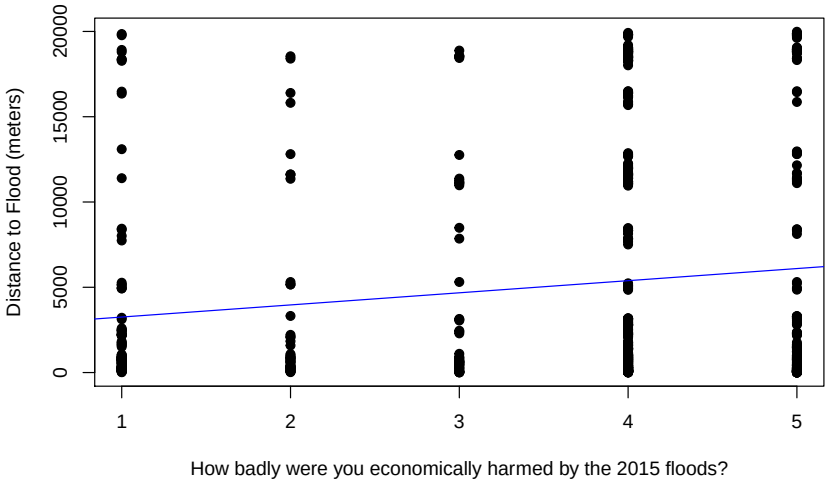


TABLE 5. Main Results

	Model 1
(Intercept)	0.31***
	(0.01)
Effort_preventionPreparedness Coordination	0.05***
	(0.01)
Effort_reliefRelief Coordination	0.09***
	(0.01)
Effective_preventionPreparedness Effective	0.11***
	(0.01)
Effective_reliefRelief Effective	0.12***
	(0.01)
AskRelief Ask	0.12***
	(0.01)
EQRelief Visits	0.16***
	(0.01)
HonestyCorruption	−0.24***
	(0.01)
HonestyVote 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 6. Main Results, Linear hypothesis

	Model 1
(Intercept)	0.31***
	(0.01)
Effort_preventionPreparedness Coordination	0.05***
	(0.01)
Effort_reliefRelief Coordination	0.09***
	(0.01)
Effective_preventionPreparedness Effective	0.11***
	(0.01)
Effective_reliefRelief Effective	0.12***
	(0.01)
AskRelief Ask	0.12***
	(0.01)
EQRelief Visits	0.16***
	(0.01)
HonestyCorruption	−0.24***
	(0.01)
HonestyVote 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 7. Main Results, Linear hypothesis 2

	Model 1
(Intercept)	0.31***
	(0.01)
Effort_preventionPreparedness Coordination	0.05***
	(0.01)
Effort_reliefRelief Coordination	0.09***
	(0.01)
Effective_preventionPreparedness Effective	0.11***
	(0.01)
Effective_reliefRelief Effective	0.12***
	(0.01)
AskRelief Ask	0.12***
	(0.01)
EQRelief Visits	0.16***
	(0.01)
HonestyCorruption	−0.24***
	(0.01)
HonestyVote 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 8. Main Results, and interactions

	Model with interactions
PreparednessPreparedness Coordination	0.05*
	(0.02)
EffortRelief Coordination	0.09***
	(0.02)
QualityPreparedness Effective	0.11***
	(0.02)
MaterialRelief Effective	0.10***
	(0.02)
HonestyCorruption	−0.24***
	(0.02)
HonestyVote Buying	−0.18***
	(0.02)
QualityPreparedness Effective × HonestyCorruption	−0.01
	(0.02)
QualityPreparedness Effective × HonestyVote Buying	0.01
	(0.02)
EffortRelief Coordination × MaterialRelief Effective	0.01
	(0.02)
QualityPreparedness Effective × MaterialRelief Effective	−0.01
	(0.02)
PreparednessPreparedness Coordination × EffortRelief Coordination	−0.01
	(0.02)
PreparednessPreparedness Coordination × QualityPreparedness Effective	0.00
	(0.02)
PreparednessPreparedness Coordination × HonestyCorruption	0.01
	(0.02)
PreparednessPreparedness Coordination × HonestyVote Buying	0.00
	(0.02)
MaterialRelief Effective × HonestyCorruption	0.02
	(0.02)
MaterialRelief Effective × HonestyVote 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 13. Heterogeneity of AMCE by Number of Contest

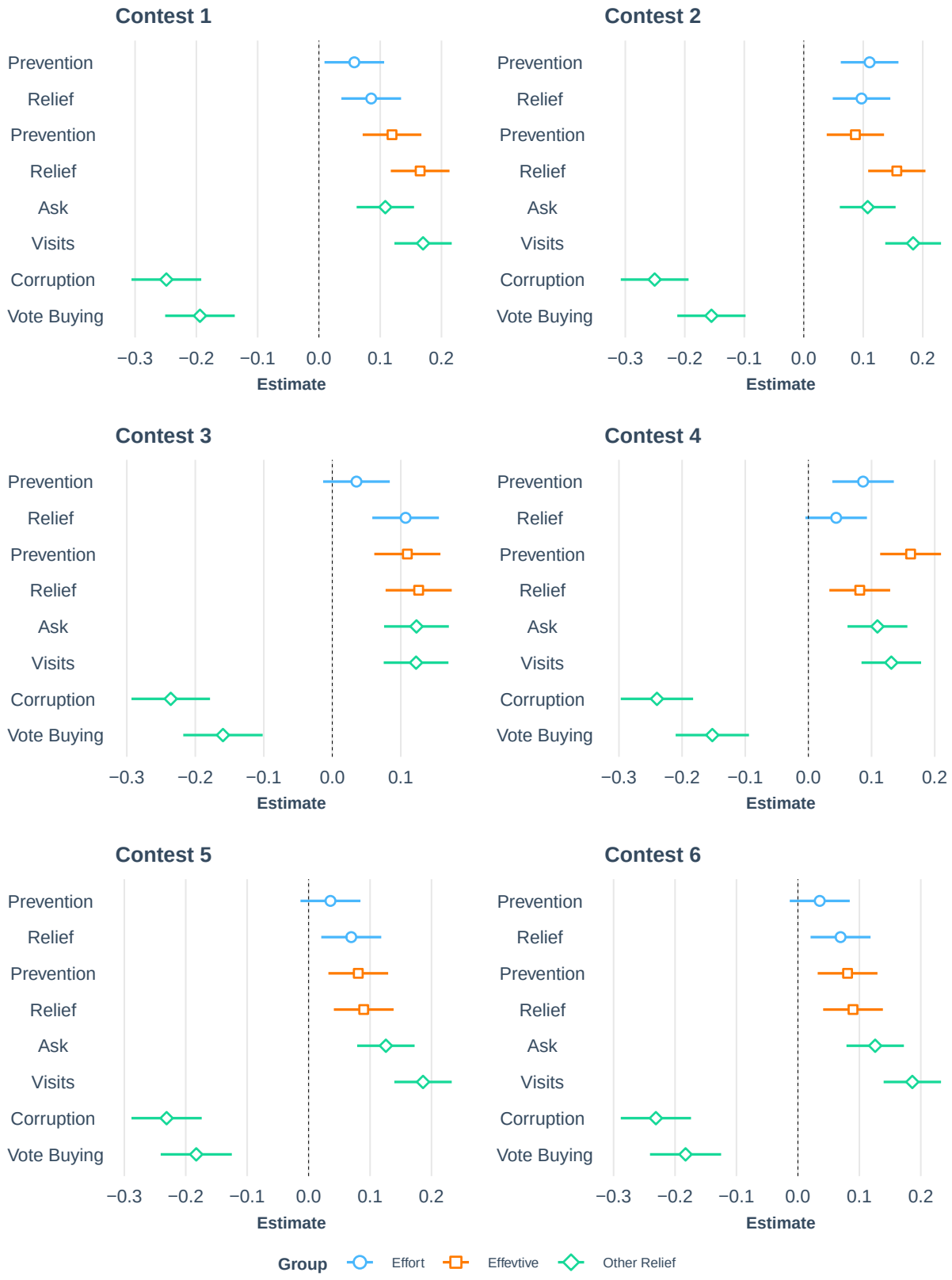


TABLE 9. Treatment Effects on ACMEs

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.1. **Treatment Effects** *Email address*: felix.hartmann@gu.se

FIGURE 14. AMCE Interaction Poverty

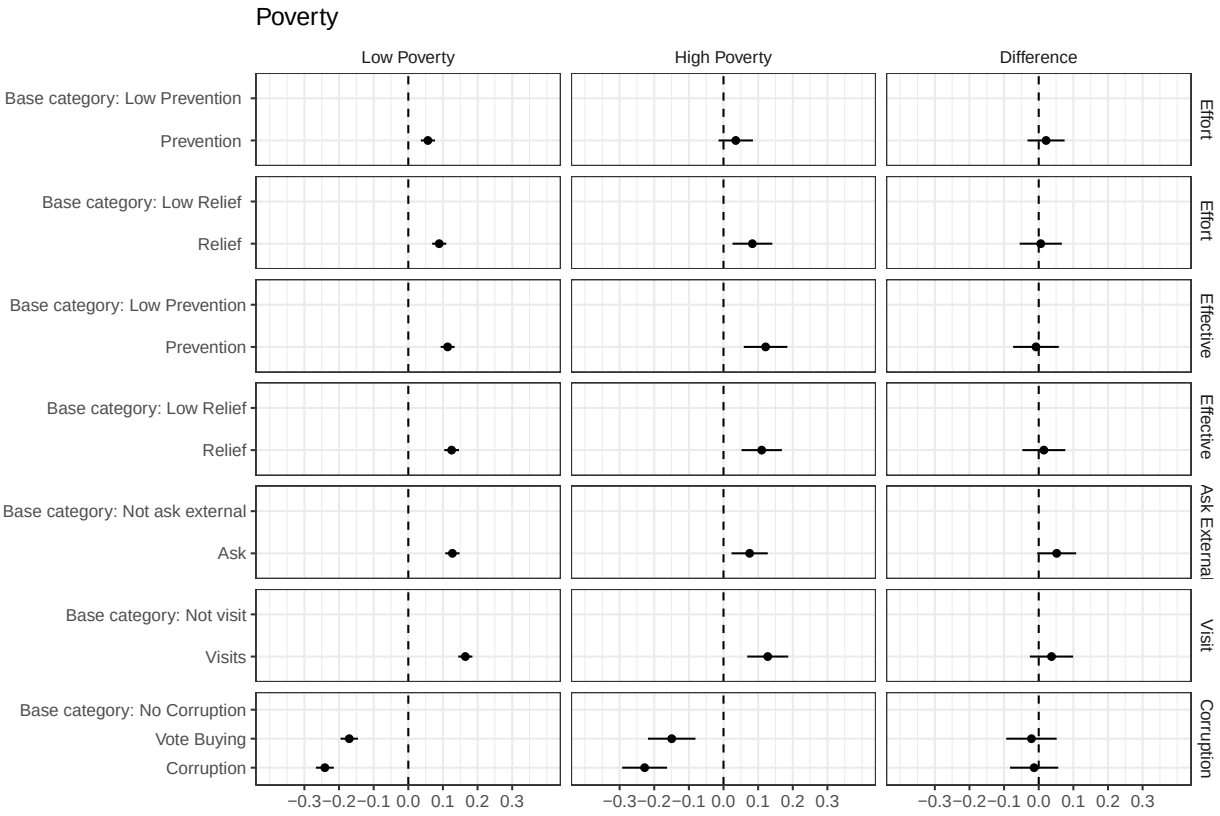
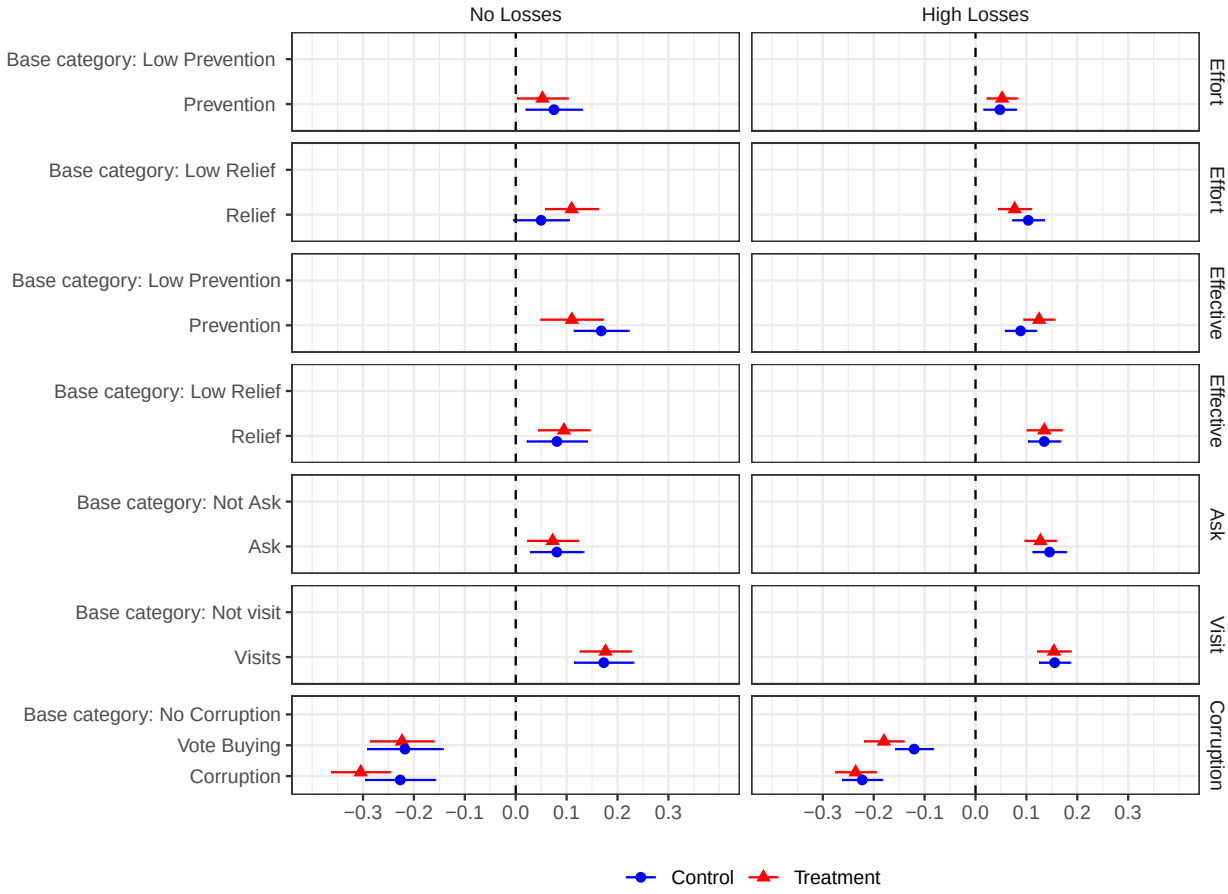


TABLE 10. Effect of Economic Losses, full models, with controls;

	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

FIGURE 15. 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 10 with control variables in the appendix.