Hidden Hostility, Donor Attention and Political Violence *

Siwan Anderson[1], Patrick Francois[2], Dominic Rohner[3], Rogerio Santarrosa[4]

[1]Vancouver School of Economics and CEPR

[2]Vancouver School of Economics

[3]University of Lausanne and CEPR

[4]INSPER Institute of Education and Research

July 31, 2023

Abstract

Political violence within countries has been on the rise for the last few decades. This paper examines a potential international dimension to this domestic repression and dissent. We focus on possible impacts from international attention on the behavior of governments and opposition groups. We consider that governments who depend heavily on foreign aid could be more likely to attack political opponents when international donors are distracted by their own major domestic events. In anticipation of this, opposition groups have incentives to reduce agitations that incite such crackdowns. We study this interaction in a simple strategic model, and scrutinize the predictions of this model using fine-grained data for Africa. The theory surmises that oppositions will reduce agitations when shocks are anticipated (elections). In contrast, when unanticipated shocks (natural disasters) hit, and when agitations are already under way, the theory predicts that the opposition will substitute visible forms of unrest (riots) for more covert operations on soft targets (targeted violence against civilians). This pattern is precisely reflected in the data. International donor inattention hurts political oppositions through the out-of-equilibrium threat of increased repression, and observed political crackdowns may only represent the “tip of the iceberg". Enhancing international scrutiny would help safeguard public demonstrations of dissent, and reduce violence against civilians.

JEL Classification: D74, F59, O19.

Keywords: Donor Attention, Political Violence, Agitation, Repression, Riots, Protests, Violence Against Civilians, Natural Disasters, Elections, Foreign Aid.

---

*We gratefully acknowledge financial support from the "Institutional legacies of violent conflict" program of UNU WIDER. We are also grateful for helpful comments from Bruno Caprettini, James Fenske, Maria Petrova, Thorsten Rogall, Arthur Silve, Tommaso Sonno and seminar and conference participants at Princeton, UBC, CERGE-EI, Paris School of Economics, University of Bern, Universidad del Rosario (Bogotá), LMU Munich, CIS (ETH-University of Zurich), UNU-WIDER, CEPR Symposium Paris, CEPR RPN on Preventing Conflict (Lausanne), Conflict Conference Padova, BFI-IOG (Chicago), EUDN Geneva, Bologna, EUI, and POLEC (Bocconi).

1 Introduction

The past few decades have seen a worldwide rise in political violence and repression of dissent within countries (UCDP (2022)). This has not only been reflected in a growing interest from policymakers and the media, but has also spurred a surge in academic research on armed conflict and political violence, as well as on protests (see below our discussion of these literatures). One thing that these somewhat distinct literatures on armed conflict and protests have in common is that they mostly focus on *domestic* factors, paying much less attention to *international* determinants.

In contrast to this significant literature in both economics and political science, in this paper we investigate an international dimension to this domestic repression and dissent. In particular, we investigate possible impacts of international attention on the behavior of governments and opposition groups. We posit that in places with a heavy dependence on foreign aid (such as countries in Africa), the attention of donor countries could be particularly salient to domestic political actors.[1] In this sense, it is natural to surmise that donor governments may exert some leverage on recipient governments when they make large (relative to recipient budget) aid contributions. It is also likely that – at least in some contexts – donor countries and recipient governments may differ considerably in their desires to accommodate democratic forms of dissent. Donor countries may hence affect the scope for repression and violence against opposition protesters. Yet the degree to which donor countries will scrutinize the actions of recipient governments depends, at least in part, on the amount of attention that the public in donor countries pays to recipient country domestic events.

On the recipient side, countries with low state capacity (the case for many aid recipients), may have governments that find it difficult to capitalize on donor inattention and launch (unprepared) military operations against hard military opposition targets. However, these government may react more harshly and violently against easily reachable soft targets, or in response to provocations from opposition forces. This could take the form of beating up protesters, shooting into crowds or rounding up organizers. In reaction, one expects the opposition to factor in that the government has "freed hands", and moderate their own anti-government agitation. This could, in equilibrium, even result in overall declines in government repression.

To help clarify the effects that donor scrutiny might have on government repression and opposition moderation, we first build a formal model. The model focuses on how strategic conflict behavior changes when donor scrutiny is temporarily diverted. It delivers testable predictions on how inattention affects the scope for different types of conflict. A key role is played by whether donor inattention is anticipated or unanticipated, and whether, at the moment of diversion, agitation was already under way or not.

In particular, the model delivers the prediction that in the absence of agitation, donor inattention should result in a down-scaling of all types of opposition action. In contrast, when agitation had already been under way, one should expect greater polarization in actions – away from "intermediate" forms of violence (demonstrations) towards both greater caution for part of the population (staying home) as well as more extreme violence (targeted killings) from the more militant parts of the opposition. While anticipated

[1]In 2019, the annual amount of state donor aid counted as Official Development Assistance (ODA) sometimes exceeded half of government budgets in Africa (see https://data.worldbank.org/indicator/DT.ODA.ODAT.XP.ZS).

shocks generally discourage agitation, unanticipated shocks, by sometimes arriving when agitation is already present, can lead to this polarizing of opposition action and more extreme violence against civilians.

In a second step, we perform an empirical investigation of the predictions from our model. We posit that attention is likely to be affected by the occurrence of major news-worthy events at home, and it is the quasi-random timing of these events that we exploit to explore the impact of donor country attention on recipient countries. Specifically, we conjecture that when donors are distracted (by major natural disasters, or elections at home), recipient governments will be less constrained in undertaking unpopular (to donors) domestic repressive acts.

For the empirical investigation, we assemble a new dataset covering all African countries for the period 1990-2018. For each country in our dataset, we build a network of key foreign partners (i.e. major donors) and construct a fine-grained high-frequency measure of unanticipated (natural disasters) and anticipated (elections) shocks to which these key partners are exposed. We then study how these shocks to donor countries affect various types of political violence in recipient countries.

We find that in moments of "inattention" precipitated by (donor country) shocks, there is a significant reduction in domestic (recipient country) "demonstrations" (protests and riots). In line with the model, this pattern is present in all scenarios (with the drop in demonstrations being either due to all agitation being put on hold or due to re-shuffling of political opposition action towards polarized outcomes). As further predicted, with anticipated shocks (elections), extreme forms of revolt (targeted killings) decline (as all agitation is cancelled), while for unanticipated shocks (disasters), the surge in polarization of modes of revolt leads to a rise in targeted killings committed by the opposition. Interestingly, our event-study evidence suggests that any effects found are confined to the time periods close to the shocks, with no "catching up" taking place later (i.e. "cancelled" agitation is called off for good, and not simply rescheduled later). This is in line with the notion that the re-shuffling of forms of opposition action may deploy lasting effects.

An important implication of our findings is that international inattention may hurt political oppositions beyond observed repression – which only represents the "tip of the iceberg". In fact, the out-of-equilibrium threat of crackdowns weakens peaceful opposition to bad regimes, and at the same time tends to fuel radicalisation. The study of these periods of inattention thus highlights an ordinarily obscured key positive effect of international scrutiny. This scrutiny helps to safeguard public demonstrations of dissent. While at the same time reducing the incentives for politically motivated violence against civilians, such as targeted killings.

The findings here are related to the literature on the impact of information, scrutiny and public attention on policy choices of politicians (Besley and Burgess (2002); Strömberg (2004); Eisensee and Strömberg (2007); Djourelova and Durante (2019)).

Another relevant literature is the one on drivers of protests (see e.g. Sangnier and Zylberberg (2017); Cantoni et al. (2019); González (2020); Manacorda and Tesei (2020); Hager et al. (2022); Bursztyn et al. (2021)). While many of the contributions link protests to (domestic) social media (Battaglini (2017); Qin et al. (2017); Barbera et al. (2020); Battaglini et al. (2020); Enikolopov et al. (2020)), or (domestic) public policy (Campante and Chor (2014); Passarelli and Tabellini (2017)), international influence on protests is severely understudied.

The picture is similar for the literature on armed conflict and political violence (for recent surveys, see Rohner and Thoenig (2021); Anderton and Brauer (2021); Rohner (2022)).[2] While many articles focus on local price or weather shocks (see e.g., recently, Berman et al. (2017); Harari and Ferrara (2018); McGuirk and Burke (2020)) or specific domestic policies (for example, Cilliers et al. (2016); Blattman and Annan (2016); Marcucci et al. (2023)), articles focusing on international interventions or influence are very rare, and often confined to the realm of military aid or armed intervention (Dube and Naidu (2015); König et al. (2017); Dell and Querubin (2018); Dimant et al. (2020)) or of foreign aid (Collier and Hoeffler (2002); De Ree and Nillesen (2009); Nunn and Qian (2014)).

Of particular relevance is the small literature linking public inattention to military attacks. While there is ample qualitative work providing case study evidence that public scrutiny constrains foreign policy (see Mueller (1973); Sobel (2001); Baum (2004); Canes-Wrone (2010)), econometric studies are very rare. One exception is the study by Durante and Zhuravskaya (2018) who find that Israeli military attacks on Palestinian targets are significantly more likely to occur during major (anticipated) political/sport events dominating U.S. news, while they are unrelated to the (non-anticipated) onset of natural disasters. This pioneering contribution is extremely valuable in showing, with concrete data, that donor scrutiny affects recipient actions in Israel.

From this well identified study, one may have been tempted to conclude that donor inattention would similarly lead to increased recipient government attacks on opposition groups worldwide. As we will show, such a conclusion would not be justified. In retrospect, this is perhaps not surprising. Israel is an "outlier" among aid recipients, given its unique geopolitical situation, mature democratic political institutions, extensive state and military capacity, and advanced level of economic development.[3] The context is extremely different for many aid recipients around the world, and particularly so in aid dependent African countries. They tend to have lower state capacity, experience state conflicts with a larger number of actors and have military targets that are harder to identify. Democracy levels also vary widely across the continent, and African states also typically rely on multiple aid donors, with foreign aid amounting to a much larger share of the overall budget. Additionally, there is heterogeneity in the political orientation of donors across time. As we will see, the marked differences in the effects of shocks to donor inattention in our African sample compared with the effects of Durante and Zhuravskaya (2018) for Israel, would seem to be attributed to differences in state capacity.[4] We undertake a thorough analysis across the whole sample exploring other dimensions of heterogeneity in effects.

The paper is organised as follows. Prior to turning to the central analysis, the next section briefly considers suggestive evidence that the mechanism we explore here is relevant. Section 3 then introduces the formal model. Unlike previous studies, a formal model is helpful in our case as it clarifies the sometimes subtle implications of donor inattention on opposition groups in contrast with the direct effects on recipient governments which are more straightforward. The data used to test the model's predictions are described

[2]Articles studying (violent) repression in particular, include Acemoglu and Robinson (2000); Besley and Persson (2009); Hill and Jones (2014).

[3]It is also characterized by a particularly strong bond with the U.S.. It is among the top two aid recipient from the U.S., with most of the aid being linked to the military (3.8 billion USD in 2020). Yet, aid represents a relatively small part of budget ($134 billion USD).

[4]For example, in terms of total military spending, according to recent data from the Stockholm International Peace Research Institute (SIPRI), Israel is ranked among the top-15 armies worldwide, well ahead of any African country (see SIPRI (2023)).

in Section 4 and the estimation strategy which is focused on citizens (potential protesters) rather than politicians is laid out in Section 5. Our main estimation results are presented in Section 6. A heterogeneity analysis exploring the likely mechanisms of effect is undertaken in Section 7. The paper concludes in Section 8.

2 Illustrating Media Attention and Distraction

2.1 Are citizens of donor countries distracted when major domestic news occur?

The claim here is that donor inattention affects political activities in African recipient countries. As discussed earlier, a motivation for this study was Durante and Zhuravskaya (2018). A feature of their work was their ability to identify media coverage as the channel via which donor distraction could be measured. U.S. elections, the super bowl, etc. were events of high viewer interest on cable news. These received disproportionate airtime leaving less media attention to be directed to any events that may occur in Israel.

That type of inquiry was feasible in their context because they restricted attention to a bilateral donor/recipient pair – The U.S. and Israel – and were hence able to focus on a small number of media outlets (US cable news). Our focus, with multiple donors and multiple recipient countries, makes a direct replication of that method beyond our scope. Our empirical focus will instead be on the relationship between donor distraction events and political activities in recipients without a direct scrutiny of fluctuations in media coverage that links these events. In a sense, we are applying only the reduced form aspect of their study to the African sample.

Suggestive evidence of a similar channel can be obtained by considering citizen distraction in donor countries via other media. To this end, Google searches are useful, since these are timed by the day, are place specific and can be synchronized with significant events occurring in donor countries. If the mechanism is similar here, we should at least expect some indication of this via reduced searches of recipient countries from searchers initiating in donor countries, when donor countries are experiencing significant domestic events (such as natural disasters and elections). The panels of the Figure 1 below display some motivating evidence along these lines. We focus on the major donor (defined as the country who donated the most to a given recipient country over the entire sample period 1997-2018) for a recipient-donor pair in the panels of the figure below. The duration of events lasting more than one day is represented in yellow. We focus on the search terms "recipient country name" (e.g. "Egypt") and the "donor country name" (e.g. United States) for google searches taking place in that particular donor country. In all panels, the onset of significant donor events corresponds to dips in searches of particular African recipient countries.

2.2 Are citizens in recipient countries following the news taking place in donor countries?

Another reality check of our framework concerns the information set of the opposition in the recipient countries. As sketched above, we assume that the recipient country opposition is able to observe when their major donor country is distracted by major events. If this is the case, we should expect google search patterns to reflect media attention in recipient countries for events (elections / disasters) taking place in

Figure 1: Distraction by domestic events (elections / disasters)

Source: Google trends.

donor countries. For example, when there are French elections, more people in Senegal are expected to google the term "French". As illustrated by Figure 2, this assumption is not far-fetched. In the examples of the four panels we see that major international news items are picked up in recipient countries.

Of course, neither panel represents a direct test of the mechanism, but it does point to some potential relevance. We explore a more specific empirical investigation after the formal model, and its predictions, are developed in the next Section 3.

Figure 2: Distraction by domestic events (elections / disasters)

Source: Google trends.

3 The Model

Each period, leaders of the opposition may have the opportunity to instigate protests against the government. With probability $\rho < 1$ the opportunity arises in period t because the government has acted in ways to incense the population, or is perceived to have done so, or because of some other such, external to the model, factor. With probability $1 - \rho$, no opportunity arises.[5]

3.1 Opposition Leader Choice

If the opportunity presents itself in period t, then opposition leaders choose either $D_t = 1$ to avail themselves and foment dissent starting in t, or $D_t = 0$ to ignore the opportunity. If dissent is started in t, it is common knowledge that it will last for $n > 1$ periods. So, effectively, the opposition are able to foment dissent via agitating citizens and thus inducing them in to the streets to protest and riot. But the substantive assumption is that they cannot immediately turn this off. Once started, the dissent necessarily runs for n periods.

Opposition leaders are a single entity choosing based on expected costs and benefits. Denote the cost to the leaders of fomenting dissent by the random variable k_t. Where k_t is drawn from some stationary distribution, $g(k)$ on support $[0, \infty)$ the properties of which are unimportant. No costs are born if $D_t = 0$,

[5]For example, there may have been heavy-handed violence by the police, overt corruption or favoritism, or past instances of such, come to light. Such events act as lightning rods allowing opposition leaders to organize civil actions amongst opponents of the government in response. While of course incompetent or corrupt governments offer more reasons for grievances, the exact timing of scandals erupting is hard to predict and can be seen as an exogenous shock.

and costs are k_t if $D_t = 1$.

The opposition leader's benefit to fomenting dissent, B_t, depends on the extent of uptake from the general population. When $D = 0$, $B_t = 0$. When $D = 1$, $B_t = \sum_{\tau=t}^{\tau=t+n} B(M_\tau)$. Where M_τ is the total mass of private individuals partaking in some form of dissent in period τ and B is a monotonically increasing function.

3.2 Citizen Choices

If dissent is fomented by leaders, individual citizens choose how, if at all, to participate. We model those decisions similar to Cantoni et al. (2019). Individuals are ordered by θ along a continuum representing their willingness to act. At one extreme are people who are government supporters, with negative values of θ. At the other, individuals with high θ values, are strongly opposed to the government, and strongly motivated to take actions against it. Assume that θ is distributed across the population by the density function $f(\theta) \to (-\infty, +\infty)$ with full support.[6]

There are three actions available to individuals when leaders have fomented dissent ($D = 1$). They can partake in one of: Protests, Riots or Violence Against Civilians. We normalize the utility gain to non-participation in any action to zero.

3.2.1 Protests

The mildest form of dissent is peaceful protests, denoted p.[7] Let $V^p(\theta)$ represent the individual value to protesting, and assume it is monotonic in $\theta : \partial V^p / \partial \theta > 0$. The more disgruntled, the more one's intrinsic utility to taking to the streets to peacefully express it.

The expected cost to an individual of protesting depends only on the state of government repression at time t, S_t.[8] There are two states of government repression, regular, $S_t = R$, and high; $S_t = H$. Under high repression, the expected cost of taking part in a protest is given by $C^p(H)$ which is strictly greater than that under regular repression: $C^p(H) > C^p(R)$.[9]

3.2.2 Riots

A more intense expression of dissent is to riot, r.[10] The value to an individual of rioting, $V^r(\theta)$, is again monotonic in $\theta : \partial V^r / \partial \theta > 0$. The expected cost of rioting also depends on the state of government

[6] In reality this single dimension summarizes at least two separate impulses. One is the extent to which an individual is aggrieved, and the other is the degree to which an aggrieved individual is willing to take (possibly violent) actions. A high value of θ corresponds to an individual with both strong personal grievances, and a high propensity to act violently. A low or negative value corresponds to someone who is a government supporter. A medium value could be someone who is strongly aggrieved by the government but unwilling, or unable, to take to the streets or to partake in violence. Alternatively, a medium value may correspond to someone who is only mildly aggrieved but is inherently disposed to take on violent acts when aggrieved.

[7] In standard databases, protests are defined as "Continuous, and largely peaceful action directed towards members of a distinct "other" group or government authorities" (Refer to the Social Conflict Analysis Database (SCAD)).

[8] See e.g. the evidence of González and Prem (2022) that the cost of street protesting increases in repression.

[9] We could model these costs in more detail as the product of the probability of being detected by the authorities for taking part in an action, and the punishment if caught, i.e. as: $C^p(H) \equiv \pi^p(H)P^p(H)$. The substantive assumption here is simply due to either one, or both of these factors being greater with a more repressive government. I.e.: $C^p(H) > C^p(R) \equiv \pi^p(R)P^p(R)$.

[10] In standard databases, riots are defined as "Continuous and violent action directed toward members of a distinct "other" group or government authorities. The participants intend to cause physical injury and/or property damage." (Refer to the Social Conflict Analysis Database (SCAD)).

repression, S, again with $C^r(H) > C^r(R)$.

3.2.3 Violence Against Civilians

The most intense form of dissent constitutes an even more violent set of actions aimed directly at civilian supporters of the government. This involves seeking out and targeting government supporters with the intent of hurting or killing them, and is termed "Violence Against Civilians" or VAC. The value of this to the perpetrator is denoted $V^v(\theta)$ and again is enjoyed more by the more extreme supporters: $\partial V^v/\partial\theta > 0$. Violence Against Civilians can be done privately and opportunistically, when a target can be found and is vulnerable.

Since the aim is to kill or hurt civilians, the penalties for being caught in VAC are uniformly high. And distinct from protests and riots, the costs of undertaking VAC do not vary. That is: $C^v(H) = C^v(R)$. Protests and riots are cracked down on more frequently and violently in states of high repression.[11] But the punishment if caught committing VAC is high independently of the government's state of repression.

We order valuations across activities, that is: High θ individuals value more violent forms of dissent relatively more. So we assume that $\partial V^p/\partial\theta < \partial V^r/\partial\theta < \partial V^v/\partial\theta$. The net value of undertaking a dissenting action $d = p, r, v$ is given by:

$$U^d(\theta) \equiv V^d(\theta) - C^d(S), \tag{1}$$

with $S = R$ or H.

When leaders have not fomented dissent, i.e., when $D = 0$, there is no value to private individuals from partaking in any form of dissent. When $D = 1$ the individuals' utility from partaking is as above.

3.2.4 Optimal opposition choice

Each period, with probability $1 - \rho$ there is no possibility of dissent and no choice for the opposition leaders to make. With remaining probability ρ the opportunity to foment dissent avails itself, and opposition leaders observe the period t costs of dissent, k_t.

If they choose no dissent ($D_t = 0$), their utility is 0. If they choose to initiate dissent ($D_t = 1$), then their expected benefits are denoted as $B_t \equiv \sum_{\tau=t}^{\tau=t+n} B(M_\tau)$. Recall that M_τ is the total mass of private individuals partaking in some form of dissent in period τ and dissent will necessarily last for n periods once started. Since the individual benefits to participating in dissent are monotonic in θ and the support, $f(\theta)$, is unbounded, in any period t in which dissent is fomented, there will exist some θ_t (this will be formally shown subsequently) such that all individuals for whom $\theta \geq \theta_t$ participate, and $\theta < \theta_t$ stay home. Then, we can, with slight abuse of notation, simply express B as a function of the cut-off to participation, θ_t. Clearly, this function is monotonically decreasing in the cut-off θ_t.

Given this, the decision rule for opposition leaders is simple:

$$D_t = 1 \text{ iff } B_t \equiv \sum_{\tau=t}^{\tau=t+n} B(\theta_\tau) \geq k_t. \tag{2}$$

[11] In these contexts, we expect greater security force presence on the streets, more violence if caught by security forces, shooting into protesters or rioters, etc.

The choice to foment dissent is thus a straightforward trade-off between costs, as captured by k_t drawn from $g(k)$ each period, and expected benefits, which depend on the degree to which citizens will respond to a call for action. It is thus possible to define a cut-off level of costs, k_t^*, below which dissent will be chosen in t and above which it will not. k_t^* will, of course, vary depending on the degree to which citizens will participate, which we now consider.

3.2.5 Optimal citizen choices

The optimal choice for citizen θ is to participate in at least one form of dissent if and only if:

$$\max_{d=p,r,v} U^d(\theta) > 0, \tag{3}$$

where U_d is defined in (1). And to stay home otherwise.

3.3 Analysis

Since C^d, where $d = r, p$ or v, is independent of θ, since $\partial V^d/\partial\theta$ where $d = r, p$ or v is increasing in θ and since $f(\theta)$ is continuous, and θ has full support, there exists a unique value of θ at which $U^p(\theta) = 0$, a unique value of θ at which $U^r(\theta) = 0$, and a unique value of θ at which $U^v(\theta) = 0$. Denote these θ_p^0, θ_r^0 and θ_v^0 respectively. Such points exist irrespective of whether $S = H$ or R.

As θ is unbounded, there always exist some individuals who will choose VAC when $D_t = 1$, irrespective of whether $S = R$ or H. In order to rule out the uninteresting case where either protests or riots are always dominated for all individuals, we make the following assumption:

Assumption Non-Empty

When $S = R$:

(i) $U^p(\theta_p^0) > max\{U^r(\theta_p^0), U^v(\theta_p^0)\}$.

(ii) At value θ_{pv} defined as θ such that $U^p(\theta_{pv}) = U^v(\theta_{pv})$, $U^r(\theta_{pv}) > U^v(\theta_{pv})$.

This implies the following partition of the θ space in terms of agents' chosen actions of dissent.

Proposition 1 *When $D = 1$ and $S = R$, there exists a level of θ, denoted θ^p, and defined by:*

$$U^p(\theta^p) = 0, \tag{4}$$

such that any individual with $\theta < \theta^p$ does not partake in dissent. There exists a level of θ, denoted $\theta^r > \theta^p$, and defined by:

$$U^p(\theta^r) = U^r(\theta^r), \tag{5}$$

such that any individual with $\theta \in [\theta^p, \theta^r)$ Protests. There exists a level of θ, denoted $\theta^v > \theta^r$, and defined by:

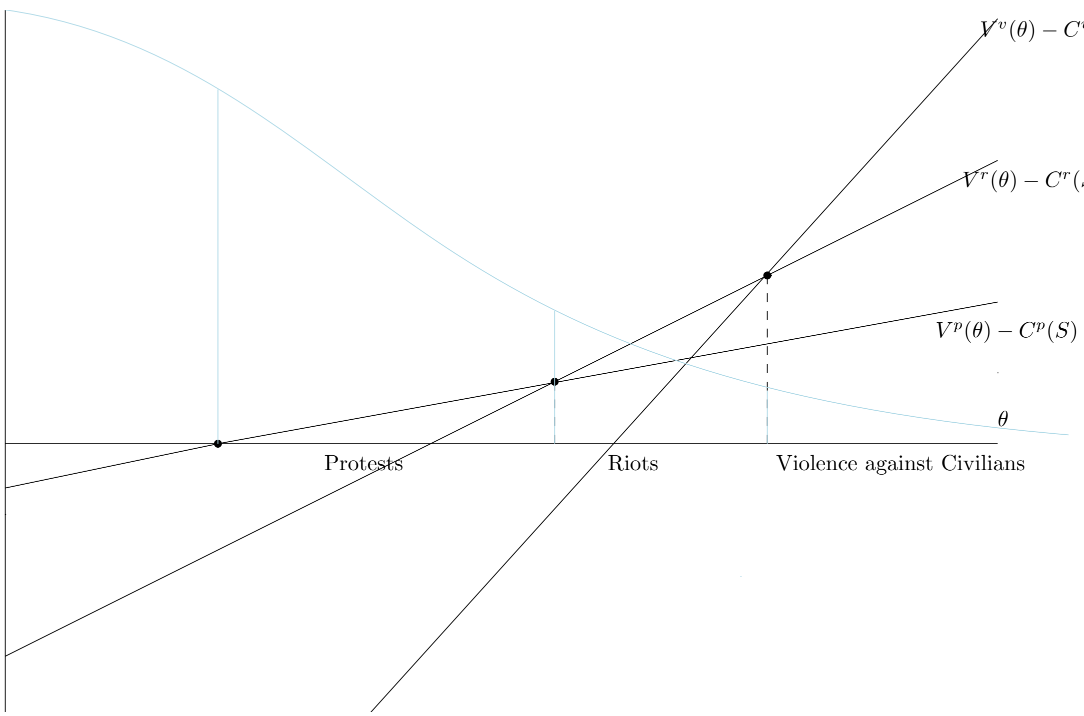
$$U^r(\theta^v) = U^v(\theta^v), \tag{6}$$

such that any individual with $\theta \in [\theta^r, \theta^v)$ riots. Any individual for whom $\theta \geq \theta^v$, undertakes VAC.

All proposition proofs are in Section B of the Appendix.

The following, Figure 3, illustrates the cut-offs as the points where the U^d schedules intersect in θ space, and depicts the θ regions corresponding to the differing types of dissent. The blue line is an example of the density, $f(\theta)$.

Figure 3: Distribution of types of dissent



A state of heightened government repression, $S = H$, increases the costs to individuals from rioting and protesting. This has the following overall effect.

Proposition 2 *Under $D = 1$ and $S = H$, let the relevant cutoffs be denoted $\theta^p(H)$, $\theta^r(H)$ and $\theta^v(H)$. We then have: $\theta^p(H) > \theta^p$ and $\theta^v(H) < \theta^v$. These imply that when dissent occurs ($D = 1$) during a state of high government repression ($S = H$) we have:*

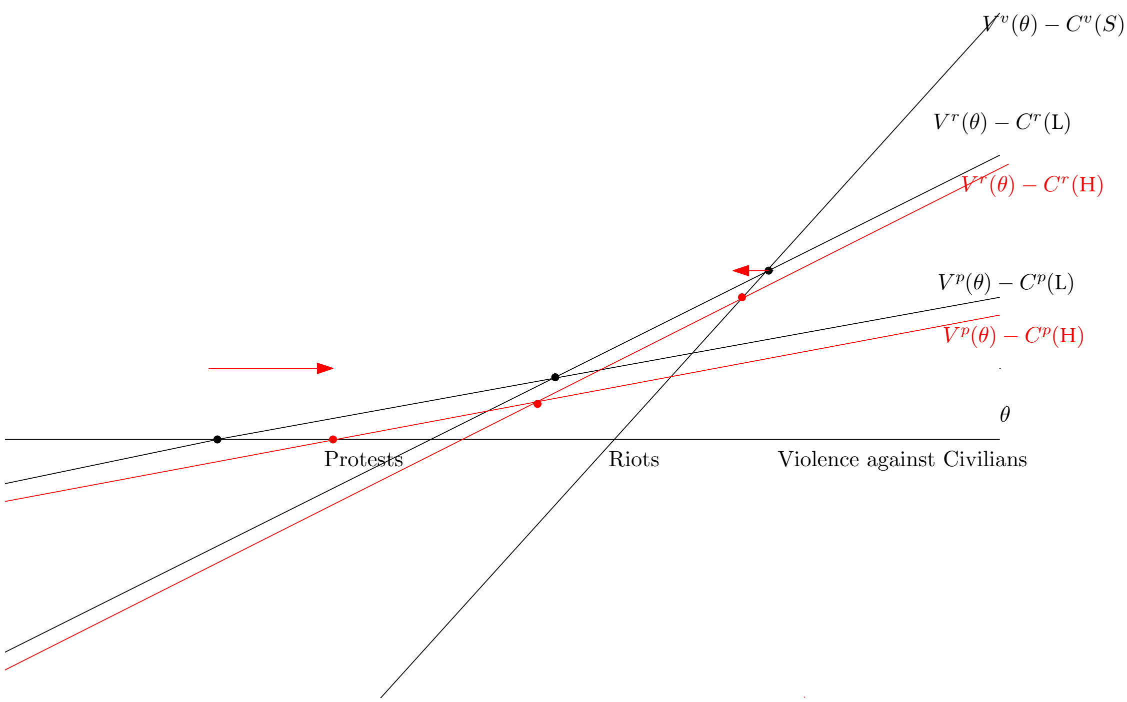
(i) The frequency of non-participation increases.

(ii) At least one of protesting or rioting falls.

(iii) Violence against civilians rises.

Under a more repressive government the costs of undertaking a public protest are higher, so fewer individuals choose to participate in any form of dissent. Some prospective rioters will seek to avoid the increased repression occurring in the streets, and will instead directly target civilians who are supporters of the government. Both of these effects imply that the total number of rioters and protesters must fall. However, since there is also substitution between rioting and protesting, depending on how increased repression affects the relative costs of the two, it is not immediately clear whether a particular one of these falls under repression. The frequency of violence against civilians unambiguously increases since violently inclined protesters shift away from public actions of dissent that have become relatively costly, towards private ones directly targeting vulnerable government supporters.

Figure 4: Impact of heightened government repression



The Figure 4 above illustrates how the regions of dissent are altered under heightened government repression. The red lines indicate a heightened state of repression, and the red arrows indicate the directional change in the cut-offs. These indicate that with heightened repression, the indifference point to engaging in protests over not dissenting rises (the right pointing arrow) and the indifference point between VAC and riots falls (the left pointing arrow).

Since dissent, once chosen by the opposition, lasts a number of periods, $n > 1$, it is possible that a movement from $S = R$ to $S = H$ occurs once a period of dissent is already under way. An implication is that a dissent already under way will be affected as indicated in the statement of the proposition by the government moving into $S = H$. Namely, conditional upon a dissent being in progress, increasing government repression leads to less overall participation in public dissent (a decline in either protests, riots or both) but an increase in violence against civilians, as indicated by the cut-off value of θ beyond which VAC is undertaken in Figure 4 moving to the left.

The proposition implies that dissent instigated by opposition leaders is less effective when the government is in a highly repressive state. Given this, when faced with the choice of initiating dissent, opposition leaders will choose dissent less frequently. That is:

Proposition 3 *If, at time t, the opportunity for dissent arises, opposition leaders are strictly less likely to choose dissent when $S = H$ than when $S = R$. That is k_t^* is strictly lower when $S_t = H$ than when $S_t = R$.*

Proposition 3 implies that under high government repression the opposition will take the opportunity to start dissent less frequently, and proposition 2 implies that any dissent that is incited in that state will instigate less public participation. The propositions together imply that any period of high government repression should be accompanied by less overall dissent; total protests (both peaceful and riots) should occur less frequently.

However, there are countervailing effects on violence against civilians. On the one hand, since dissent starts less often, there will be less VAC. On the other, when dissent occurs under $S = H$ some citizens substitute away from rioting towards VAC. The overall effect on VAC of $S = H$ is ambiguous. Yet, conditional upon an interval of dissent already having started, if S changes from R to H the model predicts VAC to unambiguously rise.

3.4 Empirical Implications

The key to exploring the model's empirical implications lies in identifying periods of regular versus high government repression. To do this, we utilize information on periods when donor countries are distracted. There are two types of situations we can identify. The first are natural disasters that occur in the donor country. The second are general elections. Both of these, when they occur, occupy an enormous amount of attention in donor countries, allowing recipient country governments a relatively free reign.

We treat these as shocks: one set is unanticipated (natural disasters), the other is anticipated (elections). There is a difference in the effects of the two shocks. If an opportunity arises for the opposition leaders to start an agitation at time t that will last for n periods, they do not know whether a disaster will arrive within the next n periods. Since the disaster increases repression and lowers public participation in the agitation, it may have affected their choice to instigate if it had been known. In contrast, donor country elections are predetermined. So opposition leaders will know when donors are distracted by an election at home and, in anticipation of increased repression by their governments may choose to forego opportunities for dissent that arise during a donor election period. There may be unused windows of opportunity (i.e. feasible agitations not instigated by opposition leaders) close to an election that would have been undertaken otherwise. This difference between the two types of shocks leads to differing predictions about the forms of dissent that will accompany them.

Some periods of dissent would not have been chosen had the opposition leaders known in advance that a disaster was about to occur. However this is not the case for elections which are known in advance to occur at specified times. Consequently, the dissent suppressing effect of elections should be greater than that of natural disasters.

Conditional upon dissent being already underway, both disasters and elections predict negative effects on riots and protests in totality. In both cases, there is a predicted substitution away from riots to VAC, and hence positive effects on VAC. However, it is less likely that dissent will be already underway when a donor election occurs.

To summarize, we have four main sets of predictions that we shall test with our data:

Prediction 1: (Unconditionally) Natural disasters: (a) lower the overall frequency of demonstrations;[12] (b) lower the frequency of at least one of protests and riots, and (c) have ambiguous effects on violence against civilians.

Prediction 2: Conditional upon an agitation already being underway, natural disasters: (a) lower the overall frequency of demonstrations; (b) lower the frequency of at least one of protests and riots, and (c) increase violence against civilians.

Prediction 3: (Unconditionally) Elections: (a) lower the overall frequency of demonstrations; (b) lower the frequency of at least one of protests and riots, and (c) have ambiguous effects on violence against civilians. (d) The coefficient on violence against civilians should be more negative than for natural disasters.

Part (d) of this Prediction 3 follows as the incentives for starting agitation are smaller before elections, so the positive substitution effect is weaker.

Prediction 4: Conditional upon an agitation already being underway, elections: (a) lower the overall frequency of demonstrations; (b) lower the frequency of at least one of protests and riots; and (c) increase violence against civilians.

A caveat to this prediction, is that the frequency of agitations already underway when donor elections occur should be lower due to the dissent suppressing effect of anticipated elections.

Before explaining our empirical strategy for testing these four predictions, we briefly describe our data sources.

[12]Note that when we use the term "demonstrations" we refer to the superset of "protests" and "riots".

4 Data

We draw on three main data sets for our core empirical analysis, which we describe below.

For our measure of natural disasters we rely on the **EM-DAT International Disaster Database** created by the Centre for Research on the Epidemiology of Disasters (CRED) at the School of Public Health of the Université Catholique de Louvain (Belgium).[13] This global data set focuses on natural and technological disasters, drawing on a variety of sources, primarily from international agencies (e.g the UN, Red Cross, national governments, etc.). It contains more than 21,000 disasters from 1900 to present. We focus on natural disasters and only those with 115 or more people dead. This restricts the analysis to the top 10% of natural disasters in terms of severity of fatalities.

For the national elections data, we put this together ourselves for all donor countries. We focused on the elections that were decisive on the identity of the head of government (i.e. presidential elections in the United States and France, Bundestag elections in Germany, and general elections in the UK and Japan). We hand-coded the dates of the elections as well as the parties elected, using a varieties of data sources, including Encyclopedia Britannica, CNN, France24, Die Welt, and the BBC.

To identify recipient and donor country pairs, we rely on the **OECD International Development Statistics**.[14] We focus on the total net donor-recipient-year flow, which includes 27 donors and 51 recipient countries in Africa for the period 1997-2018.

Our core measures of political violence come from the **Armed Conflict Location and Event Data Project (ACLED)**, which is derived from a wide range of local, regional and national sources that are collected by trained data experts worldwide.[15] It contains geographical (GPS) and time (day) precision for a large set of conflict events throughout Africa (and beyond) over the period 1997-present. For our purposes, ACLED tracks political violence, demonstrations and select (politically important) non-violent events. The types of events include battles, explosions/remote violence, violence against civilians, protests, riots, strategic development. In terms of actors, there are state forces, rebels, militias, identity groups, demonstrators, civilians and external forces. The key outcomes we focus on are defined as follows. *Protests*: public demonstration in which the participants do not engage in violence, though violence may be used against them. *Riots*: violent events where demonstrators or mobs engage in disruptive acts, including but not limited to rock throwing, property destruction, etc. We also define an aggregate measure, *Demonstrations*, which combines together the incidences of *Protests* and *Riots*. *Violence against civilians*: violent events where an organised armed group deliberately inflicts violence upon unarmed non-combatants (e.g beating, shooting, torture, rape, mutilation, kidnapping).

Assembling together these data, we created a data set spanning the period 1997-2018 for Africa. Our unit of analysis is at the country (recipient)-day level. The conflict outcome is a dummy variable indicating whether there was a conflict event on that day (repression, demonstration, riot, protest, violence against civilians). We are left with a final data set of approximately 400,000 observations. Refer to Table A1 in the appendix (Section A.1) for summary statistics of our data sample.

[13] See https://www.emdat.be/.

[14] Refer to: https://www.oecd.org/dac/financing-sustainable-development/development-finance-data/idsonline.htm.

[15] See https://acleddata.com/.

5 Empirical Strategy

We construct the bilateral link between donor country d and recipient country r in a given year y as follows:

$$Link_{rdy} = \frac{\sum_{\tau=y-10}^{\tau-y} ODA_{rd\tau}}{\sum_{d}\sum_{\tau=y-10}^{\tau-y} ODA_{rd\tau}}, \tag{7}$$

For a given bilateral pair, this link corresponds to the share of total transfers, from a particular donor country d with respect to all transfers, *ODA*, received by a recipient within the ten years preceding a given year *y*. Intuitively, this basically captures the relative importance of a given donor country. If, say, Senegal were to receive three quarters of its *ODA* from France, this number would become 0.75.

We construct a donor specific disaster variable using this link for a given day, denoted by *t*. We focus on the impacts of disasters the day before, *t-1*. This key explanatory variable is defined as follows:

$$DonorDisaster_{r,y,t-1} = \sum_{d} Disaster_{dt-1} \times Link_{rdy} \times \text{ODA/GNI}_{ry}, \tag{8}$$

where *ODA/GNI* is equal to share of the Gross National Income that stems from Overseas Development Assistance.

In a set of robustness checks explored in the appendix (Section A.4), we allow for a series of alternative methods to constructing the bilateral link. These include focusing only on the major donor, or using military transactions data instead of development assistance data. We also consider different time frames, such as receiving any donation at all during the entire period under consideration, or different moving average computations such as the average over the past 20 or 30 years.

The first main regression specification, which estimates the effects of (unanticipated) natural disaster shocks in donor countries on political violence, is defined as follows:

$$Y_{r,y,t} = \beta DonorDisaster_{r,y,t-1} + \gamma_{ry} + \alpha_m + \epsilon_{r,y,t}, \tag{9}$$

where $Y_{r,y,t}$ represents our outcome variables of interest pertaining to incidences of various forms of political violence in recipient country r, in year y and on day t. Our baseline specification, as described above, includes recipient country-year fixed effects, γ_{ry}, as well as month fixed effects, α_m. The standard errors are clustered at the country level.

Our second main regression specification instead estimates the effects of (anticipated) elections in donor countries:

$$Y_{r,y,t} = \beta DonorElection_{r,y,t} + \gamma_{ry} + \alpha_m + \epsilon_{r,y,t}, \tag{10}$$

where $DonorElection_{r,y,t}$ is defined analogously to $DonorDisaster_{r,y,t-1}$ in equation (11) for period t, so that[16]:

$$DonorElection_{r,y,t} = \sum_d Election_{dt} \times Link_{rdy} \times ODA/GNI_{ry}. \tag{11}$$

In the estimation results presented below, we also consider a series of additional specifications that address seasonality concerns. A threat to our identification strategy arises if seasonality is co-determining both disasters and political violence in respective donor and recipient countries. Our main specification, as described above, includes month fixed effects to alleviate these concerns. We further report results from empirical specifications with recipient country-year-quarter fixed effects, recipient country-year-month fixed effects, average monthly temperature measures in the recipient country, average monthly precipitation measures in the recipient country, growing season controls in the recipient country (defined as the proportion of land that is in the growing season for each month), and a recipient country specific (within year) cubic time trend.[17]

While below we shall also present some event study evidence, it is important to understand why our main specification draws on the aforementioned regression equations, and that the event study specifications are relegated to auxiliary robustness checks. In particular, there can be anticipation effects before elections, which are problematic for event studies. Thankfully they are less of a concern for our regression analysis that draws on different identifying variation (it compares the post-election effect to the average over the whole period rather than to the value on the election day). Further, our regression specifications allow for a continuous explanatory variable, while the event study requires to have a binary treatment, resulting in a substantial loss of information.

6 Estimation Results

In this section we test our main empirical predictions 1 to 4 with the data and empirical strategy described above. Before turning to this, we first explore the effects of our key explanatory variables of interest, $DonorDisaster_{r,y,t-1}$ and $DonorElection_{r,y,t}$, on direct measures of repression. We do this for two reasons. The first is to document our theoretically ambiguous prediction of two countervailing forces. In particular, as discussed above, the government has a greater tendency to repress when the world is not watching, yet the opposition is aware of that and moderates visible forms of demonstrations (riots / protests), which leads to an ambiguous overall effect on repression. The second is to compare our results directly to the work of Durante and Zhuravskaya (2018). In particular, given that the recipient countries we study have much lower military capacity than Israel (see SIPRI (2023), as discussed above), and less state capacity, the "moderation of opposition" effect should be stronger than in their case, resulting in a weakening of any impact of donor distraction on overall repression.

[16] Note that since elections are anticipated, we expect that the media is ready to cover them as they happen, i.e. on the same day in period t. For unanticipated disasters we instead expect that there is a delay and focus in on a media response a day later.

[17] Temperature, precipitation, and growing season information come from the Climate Change Knowledge Portal of the World Bank (https://climateknowledgeportal.worldbank.org/).

6.1 Repression

In the Israeli Defense Forces (IDF) case, studied by Durante and Zhuravskaya (2018), the IDF took advantage of donor distraction to undertake attacks against Palestinian targets. Clearly, the IDF had ample capacity to locate and initiate military action against such targets. In the African context, we have argued that it is more likely that the main impact on recipient governments under donor distraction is to affect the magnitude of their response to opposition actions against the government. These governments often lack readily locatable opposition targets, and in many cases also lack military capacity required to implement attacks against them where they are known.

There are thus no clear cut predictions regarding repression levels overall: government responses should be more harsh when donors are distracted, but anticipating this, opposition forces will moderate and avoid agitating these less fettered governments.

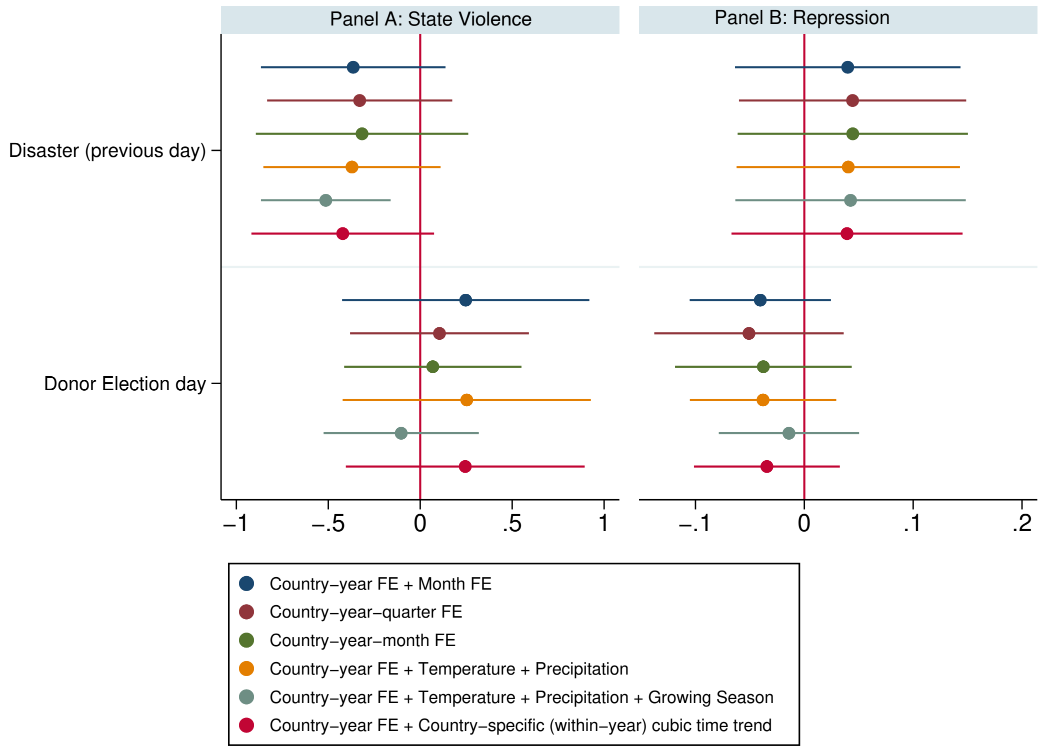
To explore the effects of disasters on repression we estimate equations (9) and (10) for two measures of repression. We first consider a measure, *State Violence* from ACLED which categorizes whether state forces were involved in any violent event. The second measure, *Repression* comes from an alternative data source, the Social Conflict Analysis Database (SCAD)[18], that also provides a measure of the incidence of repression by state forces.

The results are displayed in Figure 5. The two panels A (*State Violence*) and B (*Repression*) present plots of the estimated coefficients of β from equations (9) and (10) respectively.

Each panel contains the same specification variants, both for disasters and elections. The first estimate of β (in blue) in each figure is from our baseline specification, which includes recipient country-year fixed effects as well as month fixed effects. The subsequent five coefficient estimates are from specifications that include instead: (i) recipient country-year-quarter fixed effects (in red); (ii) recipient country-year-month fixed effects (in green); (iii) recipient country-year fixed effects plus temperature and precipitation controls (in yellow); (iv) recipient country-year fixed effects plus temperature and precipitation controls as well as growing season fixed effects (in grey); and (v) recipient country-year fixed effects plus a recipient country specific (within year) cubic time trend (in pink).

[18] https://korbel.du.edu/sie/research/data-downloads

Figure 5: Impact of Disasters and Elections on State Violence and Repression



Notes: Data sources are described in Section 4. Confidence interval bars are depicted for the 95% level.

For all specifications neither $DonorDisaster_{r,y,t-1}$ nor $DonorElection_{r,y,t}$ are significant determinants of repression in nearly all of our empirical specifications. Refer to Tables A3 through to A6 in the appendix (Section A.2.1) for the specific coefficient estimates from these regressions.

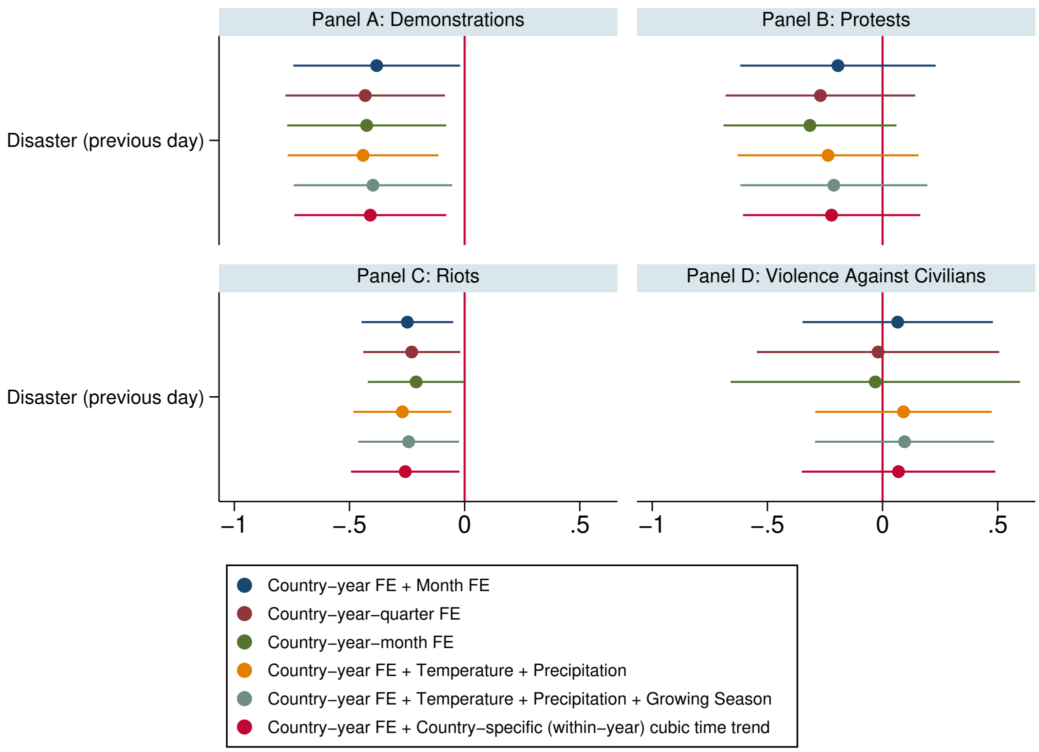
We now turn to testing the four main predictions of the model.

6.2 Testing Prediction 1: Disasters (Unconditional)

Prediction 1 is that unanticipated natural disasters in donor countries should lower the incidence of demonstrations in recipient countries. To test parts (a) and (b) of Prediction 1, we estimate equation (9) for three core outcome variables: demonstrations, protests and riots. The first three panels of Figure 6 present the estimated coefficient plots for these three outcomes. The format of the panels in the figure follows the specifications described above. As predicted, natural disasters in a donor country significantly decrease the incidence of demonstrations in recipient countries the next day. This holds for all specifications. The third panel demonstrates that this effect is primarily driven by the incidence of riots (rather than for protests (in the second panel)).

The fourth panel of Figure 6 presents the estimated coefficient plots from estimating (9) on the outcome variable of violence against civilians. Part (c) of Prediction 1 expects an ambiguous effect, so there is no real prediction to test here. In any case, we see that the estimated coefficient is not statistically significantly different from zero in all empirical specifications.

Figure 6: Impact of disasters in donor countries (unconditional)



Notes: Data sources are described in Section 4. Confidence interval bars are depicted for the 95% level.

Refer to Tables A7 through to A10 in the appendix (Section A.2.2) for the specific coefficient estimates from these regressions depicted in Figure 6. For an alternative specification, and an alternative visual representation of these core findings, we present an event study in Figure A1 in the appendix (Section A.5). We include our baseline set of control variables (country-year and month fixed effects) and we estimate the regressions for the window between -7 and +21 days around the disaster. The event study is helpful in showing that there is no evidence of pre-trends in these relationships. Moreover, it confirms the same conclusions reported above. Namely, in accord with Prediction 1, we observe moderation in demonstrations. This largely occurs via reduced rioting.

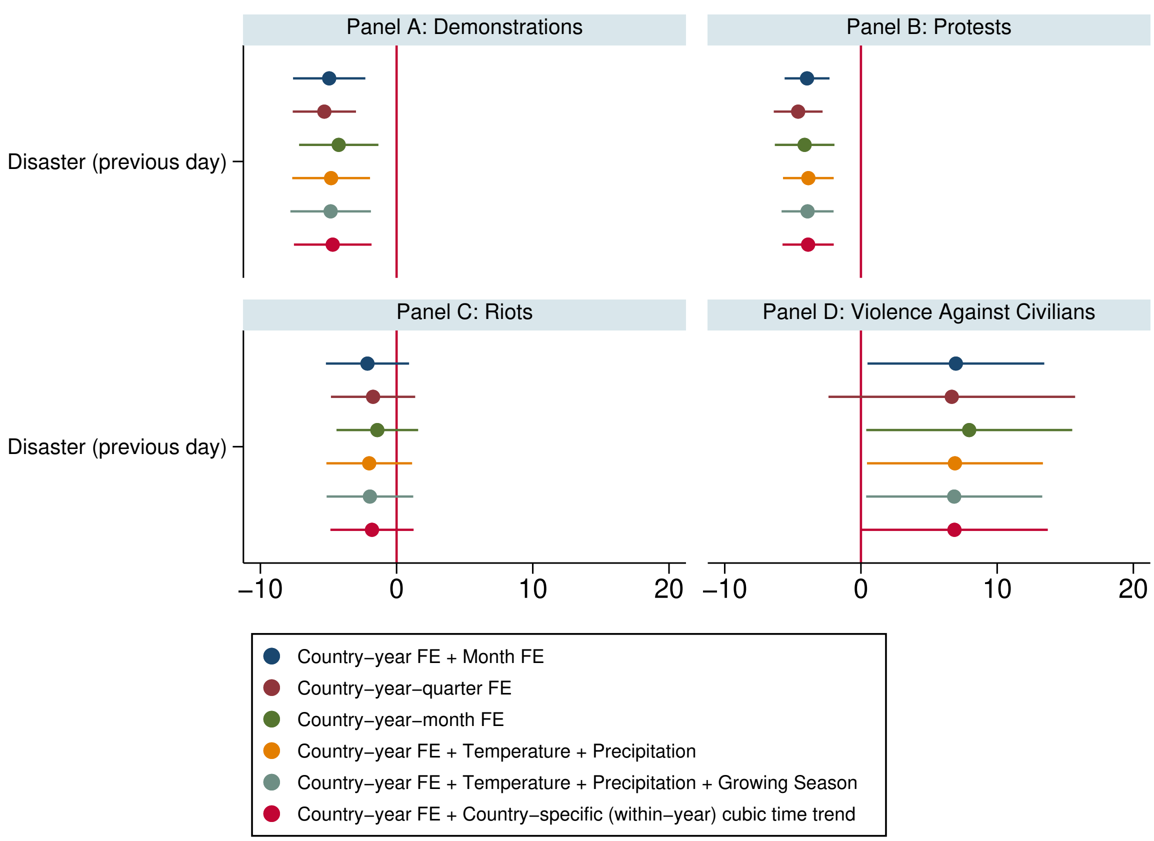
6.3 Testing Prediction 2: Disasters (Conditional)

Prediction 2 of the model can be tested by estimating (9) on the same four outcome variables conditional upon an agitation already being underway in a recipient country. To that end, we restrict the sample to recipient countries where a riot has already occurred sometime during the previous week (i.e., $t-1$ to $t-7$).

The first three panels of Figure 7 confirm parts (a) and (b) of Prediction 2. Natural disasters in the donor country in $t-1$ are significantly negatively related to the incidence of demonstrations in the recipient country in period t. We see from the second panel that this is mainly driven by a reduced incidence of protests.

The fourth panel of Figure 7 tests part (c) of Prediction 2. That is, there should be an increase in violence against civilians when a natural disaster strikes in a donor country once an agitation is already underway in a recipient country. Accordingly we see that natural disasters do lead to a significant increase in violence against civilians (in almost all empirical specifications) when conditioning upon a riot already being underway. Violence against civilians, conditional upon agitations already being underway, is the lone type of dissent that the model predicts will increase with a natural disaster shock. It is striking that this is, in fact, the only type of dissent that we observe to increase with a natural disaster shock.

Figure 7: Impact of disasters in donor countries (conditional)



Notes: Data sources are described in Section 4. Confidence interval bars are depicted for the 95% level.

Refer to Tables A11 through to A14 in the appendix (Section A.2.3) for the specific coefficient estimates from these regressions in Figure 7. Refer to Figure A2 in the appendix (Section A.5) for the event study analysis of these results. We include our baseline set of control variables (country-year and month fixed effects) and we estimate the regressions for the window between -7 and +21 days around the disaster and again confirm that there is no evidence of pre-trends. Importantly, we also detect no catch-up effect further down the road ("cancelled" agitation remains called off for good, and does not simply get rescheduled to take place some days later). Once again, this alternative estimation procedure corroborates the findings reported above.

6.4 Testing Prediction 3: Elections (Unconditional)

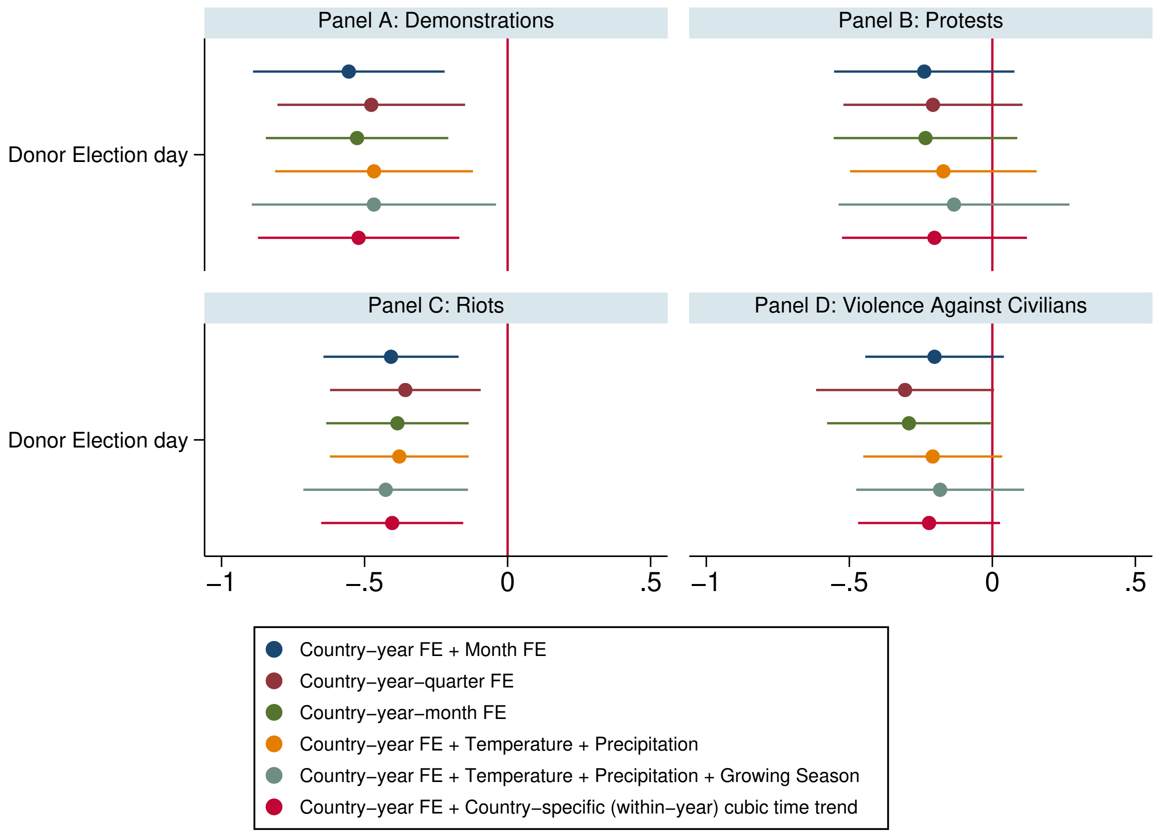
Elections in donor countries, which are of course anticipated, should also affect political violence in recipient countries. In this section we test this by estimating (10) for our four main outcome variables.

The first three panels of Figure 8 test parts (a) and (b) of Prediction 3. These confirm that elections in the donor country significantly decrease the incidence of demonstrations in the recipient country. This effect is primarily driven by a reduction in riots.

Part (c) of Prediction 3 (ambiguous effects) can not be tested of course. The fourth panel of Figure 8 simply reports that elections tend to lead to a decrease in violence against civilians and this is statistically significant across most specifications. Therefore, consistent with part (d) of Prediction 3, the estimated coefficient on violence against civilians is more negative for elections compared with natural disasters (the fourth panel of Figure 6). By comparing Tables A9 and A17 in the Appendix, we see that the estimated coefficient on VAC for elections is negative and often statistically significant (Table A17), whereas the corresponding coefficient for disasters is generally positive and statistically insignificant (Table A9).[19]

[19] As discussed in detail in Appendix A.5, the event study analysis is confined to the investigation of disasters, as there could be anticipation effects before elections.

Figure 8: Impact of elections in donor countries (unconditional)



Notes: Data sources are described in Section 4. Confidence interval bars are depicted for the 95% level.

Refer to Tables A15 through to A18 in the appendix (Section A.2.4) for the specific coefficient estimates from these regressions depicted in Figure 8.

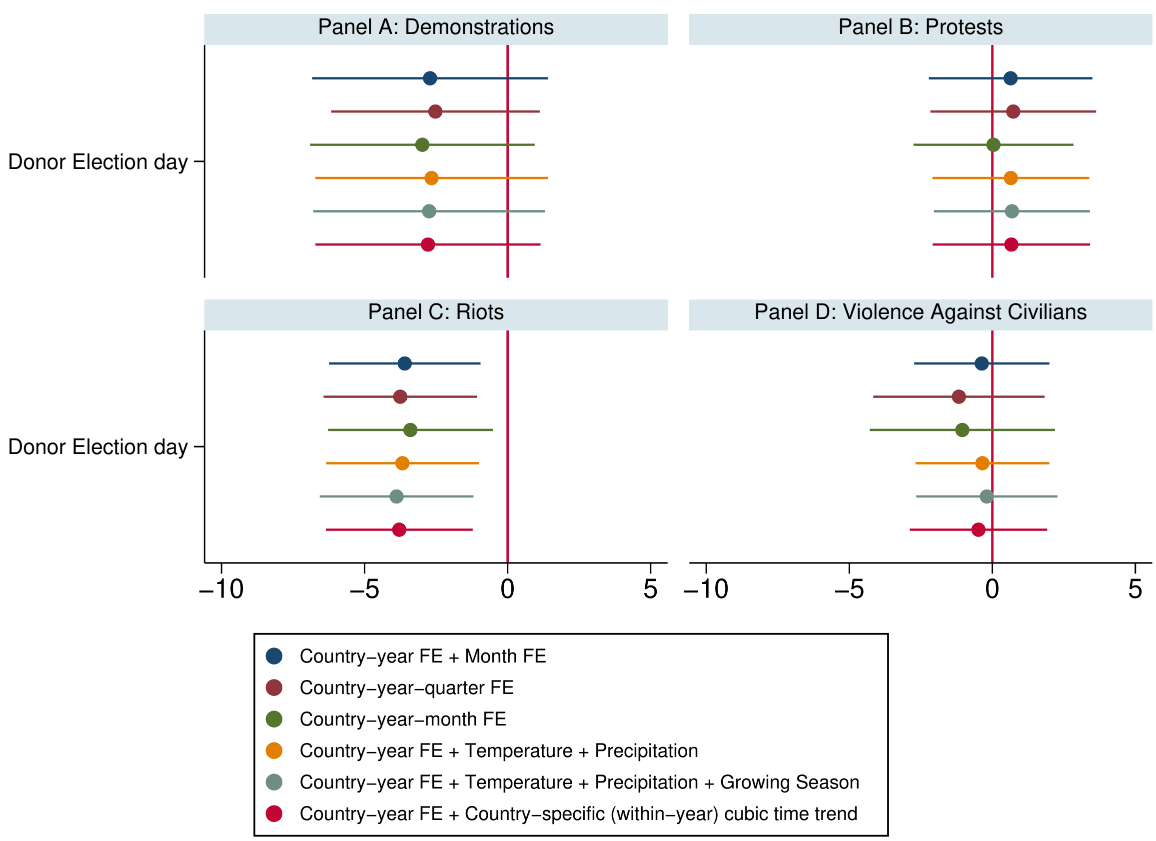
6.5 Testing Prediction 4: Elections (Conditional)

We finally estimate (10) for the same four outcome variables but conditional upon an agitation already being underway.

The first three panels of Figure 9 explore parts (a) and (b) of Prediction 4. That is, anticipated elections in a donor country should significantly reduce the incidence of demonstrations overall, and at least one of riots or protests in a recipient country. The point estimate on demonstrations is negative but not significant. Riots fall in a statistically significant way.

The fourth panel of Figure 9 tests part (c) of Prediction 4. We expected to see that elections lead to a significant increase in violence against civilians, when agitation is already underway. The estimates are slightly negative, small in absolute value and not significantly different from zero. Unlike for natural disasters where this prediction holds, the data is not consistent with the model prediction here. We discuss possible reasons in the next sub-section (Section 6.6).

Figure 9: Impact of elections in donor countries (conditional)



Notes: Data sources are described in Section 4. Confidence interval bars are depicted for the 95% level.

Refer to Tables A19 through to A22 in the appendix (Section A.2.1) for the specific coefficient estimates from these regressions depicted in Figure 9.

6.6 Robustness checks

In what follows we shall briefly synthesise key robustness checks performed in the appendix. In particular, we consider alternative ways to define the bilateral link, considering a variety of weights and time horizons for defining the link between two countries. Further, instead of *ODA* transfers, we also consider military transfers. For this information, we use a dataset from Fearon and Hansen (2018) that draws on information from the Stockholm International Peace Research Institute (SIPRI) on arms transfers between states since 1950. Appendix Tables A23 through to A26 report the results to test Predictions 1 through 4 respectively, finding that our results prove robust to these sensitivity tests.

6.7 Model Performance

To recap, demonstrations decline overall, and at least one of riots or protests fall when donor countries are distracted by either elections, or by natural disasters (parts (a) and (b) of Predictions 1 and 3). Conditional upon an agitation already being underway in a recipient country, a donor distracting event similarly reduces one of protests or riots (parts (a) and (b) of Predictions 2 and 4). For all of these results, the directions of change are consistent with the model's predictions, and statistically significant in all specifications.

The anticipated substitution to violence against civilians (VAC) that was posited to occur under donor distraction with an agitation already underway, does occur under donor disasters, Prediction 2 (part (c)), and is again statistically significant. The one prediction not supported is a substitution towards VAC under elections when agitations are already underway, Prediction 4 (part (c)), as shown in panel D of Figure 9.

The model predicted an increase in VAC conditional upon an agitation because demonstrators would fear increased repression if taking to the streets. They therefore substitute towards direct attacks on civilian government supporters, rather than mobilizing en masse and in plain view of security forces. The estimation results show that this does indeed happen when a disaster is the source of donor distraction, but not when a donor election takes place. A possible reason is that since the timing of elections is known in advance, the opposition will only infrequently be involved in an agitation when a donor election arrives, making it harder to detect effects. This contrasts with a distraction event like natural disasters in the donor country. Since these are not anticipated, it is relatively more likely that an agitation will be already underway, and then opposition supporters will substitute away from rioting and into VAC.

This interpretation is consistent with the findings reported for unconditional effects. The point estimates show a more negative decline in VAC with elections than with disasters (panel (D) of Figure 8, compared with panel (D) of Figure 6). The model's interpretation is that agitations serious enough to induce VAC are less frequent when a donor election is coming, but not with disasters since these are unanticipated. Since the situations that would give rise to an increase in VAC with donor distraction occur less frequently under elections, the predicted positive force is smaller under elections.

The model assumed that the distribution of θ is such that there is positive mass in all regions, so that all agitations lead to VAC and riots, as well as the more minor forms of dissent such as protests. Relaxing this assumption in the model could make the model's predictions consistent with these findings. But since θ is not observed and essentially a free parameter, extending the model to fit these observations is hardly a meaningful exercise. We thus conclude from this section a relatively strong degree of support for the model with respect to donor distractions occasioned by disasters. With respect to elections, the evidence is more equivocal and would suggest that, at least, some (moderate) modification of the model would be needed to fully fit these findings.

7 Heterogeneity Analysis

In this section, we push further into model implications that would be implied by donor distraction due to disasters. The heterogeneity analysis explores the mechanisms through which reported effects might be occurring as implied by the model. We also seek to better understand the contrast between the findings here and those reported earlier for Israel (Durante and Zhuravskaya (2018)), using a similar type of identification.

7.1 Donor Country Heterogeneity

7.1.1 Democratic vs. Non-Democratic

The model assumes that donor countries care about the repressive behavior of countries they donate to. Naturally, donors that are democracies should be more likely to exhibit such concerns for what recipient governments do than are autocratic donors. In the extreme, an autocracy that itself is highly repressive to its own population is unlikely to be concerned that recipient governments repress their own populations. As such, the distraction of such an autocratic donor will have little effect. This is in contrast with a democratic donor government that may suffer significant political cost when states it supports harshly repress civilians. We can explore this channel only for the donor disaster shocks, as autocracies typically do not have meaningful elections.

Table 1 breaks the sample of donors into democratic (a Polity IV score ≥ 5) in Panel A and non-democratic (a Polity IV score < 5) countries in Panel B.[20] Columns 1 to 8 represent the coefficients in the main specification for each of the model predictions, conditional (columns (1) to (4)) and unconditional (columns (5) to (8)). The baseline results continue to hold only for the democratic donor sub-sample of Panel A. In the autocratic sub-sample nothing is statistically significant, and the coefficient estimates are very sensitive across specifications.

Panel C considers the case of one large donor in particular, The People's Republic of China, that is classified by Polity IV as autocratic. Since China is not an OECD member, and not included in the donor data set, the data to compute links for China are taken from AidData's Global Chinese Development Finance Dataset[21] and extend for the period 2000 to 2017. Since China is a large and wide ranging donor in Africa over that period, and also experiences a large number of qualifying disasters, there is sufficient power to test whether there are any effects of Chinese distraction events (natural disasters) on recipient country actions. As Panel C confirms, there are none.

[20] Refer to the Polity data set (https://www.systemicpeace.org/polityproject.html). The donor countries that are considered non-democratic are: Azerbaijan, Chinese Taipei, Kazakhstan, Kuwait, Russia, Saudi Arabia, Thailand, and United Arab Emirates. These countries have obtained Polity IV score below or equal to 5 in at least one year during our sample period. We cannot further disaggregate the "non-democratic" donor country grouping into Anocracies (-5 $\geq$ Polity IV Score $<$ 5) and Autocracies (Polity IV Score $<$ - 5) as the samples become too small to uncover sufficient variation.

[21] Refer to: https://www.aiddata.org/data/aiddatas-global-chinese-development-finance-dataset-version-2-0.

Table 1: Democratic vs. Non-Democratic Donors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Democratic Donors								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-0.382**	-0.193	-0.249**	0.066	-4.947***	-3.956***	-2.137	6.971**
	(0.180)	(0.211)	(0.099)	(0.206)	(1.320)	(0.820)	(1.518)	(3.227)
Observations	381828	381828	381828	381828	42489	42489	42489	42489
Panel B. Non-Democratic Donors								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	0.812	-0.661	1.717	-1.185	-1.8e+03	-784.835	-775.419	-545.613
	(2.905)	(1.381)	(3.316)	(0.972)	(1984.061)	(1783.340)	(1432.168)	(728.852)
Observations	373977	373977	373977	373977	42477	42477	42477	42477
Panel C. China								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	0.039	-0.054	0.130	-0.071	6.021	2.601	4.042	-0.490
	(0.108)	(0.073)	(0.120)	(0.084)	(6.794)	(3.739)	(6.750)	(1.761)
Observations	373274	373274	373274	373274	39965	39965	39965	39965

Notes: All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Significance level: *** 1%, ** 5%, and * 10%.

7.1.2 Left vs. Right Wing

Another informative dimension would seem to be whether donor governments lean more towards the left vs. right views of the political spectrum. Here we expect left leaning, or progressive, governments to be more affected by distraction events than are right leaning ones. This is because left-leaning governments may be more likely to be punished by their supporters for donating to repressive governments than are right leaning ones, so distraction events that reduce such countries' scrutiny are more likely to affect recipients (see e.g. Ettinger (2020)).

We collected the election results for our set of 5 major donor countries over the sample period and matched the winning political party to whether they stood right or left on the political spectrum. We focused on the elections that were decisive on the identity of the head of government (i.e. presidential elections in the United States and France, Bundestag elections in Germany, and general elections in the UK and Japan). We hand-coded the dates of the elections as well as the parties elected, using a variety of data sources, including Encyclopedia Britannica, CNN, France24, Die Welt, and the BBC.

Table 2 reports results for right-wing donor governments in Panel A and for left-wing ones in Panel B. As per the previous table, columns (1) to (8) report the coefficient on the natural disaster variable that test the model's core predictions on the four different outcomes (unconditional and conditional on agitation already underway) for the case of the main specification. Once again, we observe heterogeneous effects. The baseline set of findings replication only for the left-leaning sub-sample of governments (Panel B). For right-leaning donor governments there are no significant results (Panel A).[22]

Table 2: Right-Wing vs. Left-Wing Donors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Right-Wing Donors								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-0.233	-0.151	-0.122	-0.215	-5.097	2.565	-11.37	-0.616
	(0.214)	(0.185)	(0.111)	(0.247)	(17.42)	(14.17)	(7.691)	(7.068)
Observations	381,874	381,874	381,874	381,874	42,499	42,499	42,499	42,499
Panel B. Left-Wing Donors								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-0.820*	-0.478	-0.468*	0.848**	-4.674***	-4.015***	-1.435	7.642**
	(0.429)	(0.428)	(0.240)	(0.363)	(1.365)	(0.920)	(1.132)	(3.563)
Observations	381,874	381,874	381,874	381,874	42,499	42,499	42,499	42,499

Notes: All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Significance level: *** 1%, ** 5%, and * 10%.

7.2 Recipient Country Heterogeneity

7.2.1 State and Military Capacity

We have already conjectured a possible reason for why the results on increased repression found by Durante and Zhuravskaya (2018) for Israel in the event of donor distraction in the U.S. did not hold in our African sample. Namely, lower levels of state and military capacity to locate and attack targets in the African case.

This motivated our focus on state repression that is instead reactive to opposition protests, and the protests themselves, as more likely to be observable reactions to donor distraction in the African case. If this is the reason for the difference, then we should expect to see that outcomes within countries that are closer to Israeli levels of state and military capacity within our sample resemble the results of Durante and Zhuravskaya (2018) and those furthest from Israel (along these dimensions) being the ones driving our divergent results.

To do this, we consider a diverse set of state capacity measures: GDP per capita; Bureaucracy quality (as measured by International Country Risk Guide (ICRG)); Tax to GDP ratio, and Military Expenditure per capita. These measures both capture the level of fiscal and legal capacity (as highlighted e.g. by Besley and Persson (2011)), as well as determinants of national material/military capabilities (as highlighted e.g. by the Correlates of War Project (Singer and Small (2022))).

[22]The results for right-wing governments is somewhat sensitive to whether we include Japan in this category. In the reported table, we code Japan's ruling party since 1955, the Liberal Democratic Party, as right-wing. If we instead omit Japan from the estimations, the results reported for right-wing donors in Table 2 do not change for the unconditional findings but for the conditional findings, there is a negative effect on riots and also a negative effect on VAC.

Although there is considerable cross-country heterogeneity within our African sample over this period with respect to these variables, it should be noted that even the upper end of the support in the African sample does not come close to levels comparable to Israel over the period studied by Durante and Zhuravskaya (2018). This suggests that even the highest state capacity countries in our sample may not display outcomes consistent with those reported for Israel by Durante and Zhuravskaya (2018). But within the variation in measures we do observe across the countries of our sample, we expect that the low state (military) capacity countries should be the ones most consistent with the model's prediction.

Table 3 breaks the sample up into low and high state capacity sub-samples according to each of these measures by splitting the sample at the median country in each case. It reports results for low and high separately for each variable in each of the four panels (A to D). As can be seen, for each of the measures, the model's results continue to hold consistently only in the low capacity sub-sample. The high capacity sub-sample generally reports coefficient estimates closer to zero and in most cases insignificant. In particular, the estimated negative coefficient on riots for low state capacity as compared to high state capacity are statistically significantly different when state capacity is measured by *GDP per capita* or *Bureaucracy Quality* (Panels A and B). This direction of change does point to state-capacity differences as perhaps playing some role in explaining the divergence in results for Africa relative to the previous work on Israel.

Table 3: Heterogeneity by State Capacity levels: Impact of Disasters in Donor Countries (Unconditional)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Heterogeneity by *GDP per capita*								
	Low				High			
Dep Var	Demostrations	Protests	Riots	VAC	Demostrations	Protests	Riots	VAC
Disaster (previous day)	-0.400	-0.0116	-0.478***	-0.392	1.984	1.151	0.413	0.537
	(0.394)	(0.311)	(0.162)	(0.417)	(2.204)	(2.141)	(1.044)	(1.406)
Observations	176,965	176,965	176,965	176,965	176,602	176,602	176,602	176,602
R-squared	0.121	0.092	0.069	0.176	0.331	0.283	0.240	0.142
Panel B. Heterogeneity by *Bureaucracy quality*								
	Low				High			
Dep Var	Demostrations	Protests	Riots	VAC	Demostrations	Protests	Riots	VAC
Disaster (previous day)	-0.851*	-0.253	-0.666**	-0.499**	1.638	1.672	-0.260	1.203
	(0.434)	(0.328)	(0.226)	(0.209)	(2.467)	(2.393)	(0.705)	(1.428)
Observations	109,941	109,941	109,941	109,941	94,236	94,236	94,236	94,236
R-squared	0.149	0.153	0.051	0.161	0.364	0.308	0.265	0.174
Panel C. Heterogeneity by *Tax to GDP ratio*								
	Low				High			
Dep Var	Demostrations	Protests	Riots	VAC	Demostrations	Protests	Riots	VAC
Disaster (previous day)	-0.431*	-0.305	-0.168***	0.160	0.290	0.743	-0.702*	0.815
	(0.206)	(0.198)	(0.0530)	(0.154)	(1.782)	(1.843)	(0.389)	(1.105)
Observations	141,352	141,352	141,352	141,352	125,648	125,648	125,648	125,648
R-squared	0.154	0.131	0.067	0.214	0.327	0.275	0.261	0.075
Panel D. Heterogeneity by *Military Expenditure per capita*								
	Low				High			
Dep Var	Demostrations	Protests	Riots	VAC	Demostrations	Protests	Riots	VAC
Disaster (previous day)	-0.480**	-0.227*	-0.336	-0.424	0.322	0.610	-0.536	0.486
	(0.229)	(0.120)	(0.211)	(0.445)	(1.095)	(1.145)	(0.360)	(0.702)
Observations	152,680	152,680	152,680	152,680	152,315	152,315	152,315	152,315
R-squared	0.220	0.196	0.123	0.189	0.315	0.272	0.238	0.116

Notes: All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Significance level: *** 1%, ** 5%, and * 10%.

Table 4 presents analogous results conditional upon an agitation already being underway in a recipient country. We see that again our core set of results (consistent with Prediction 2) seem to mainly hold for low state capacity countries. A statistically significant difference is found in particular for the estimated coefficient on riots for state capacity measured by *GDP per capita*, *Bureaucracy Quality*, or *Military Expenditures per capita* (Panels A, B, and D). An exception is the model's predictions of violence against civilians increasing. This does not generally hold in the low state capacity sample (an exception being the *Tax to GDP ratio* measure but there is not a statistically significant difference in the estimated coefficients for low and high state capacity).

Table 4: Heterogeneity by State Capacity levels: Impact of Disasters in Donor Countries (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Heterogeneity by *GDP per capita*								
	Low				High			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-10.26**	-5.342*	-7.526**	-4.882***	1.367	-6.014	2.178	23.06*
	(4.384)	(2.829)	(3.279)	(1.092)	(10.64)	(8.831)	(12.86)	(13.23)
Observations	14,674	14,674	14,674	14,674	25,679	25,679	25,679	25,679
R-squared	0.140	0.144	0.078	0.177	0.284	0.257	0.206	0.173
Panel B. Heterogeneity by *Bureaucracy quality*								
	Low				High			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-16.22**	-9.588**	-10.26**	-4.549*	-1.352	-0.0187	-7.117	30.37**
	(6.111)	(3.223)	(4.368)	(2.468)	(5.183)	(6.918)	(6.702)	(11.50)
Observations	10,705	10,705	10,705	10,705	19,862	19,862	19,862	19,862
R-squared	0.139	0.169	0.063	0.156	0.301	0.262	0.213	0.198
Panel C. Heterogeneity by *Tax to GDP ratio*								
	Low				High			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-4.673***	-3.880***	-1.235	8.763***	3.837	-3.272	2.230	24.79*
	(1.298)	(0.623)	(1.175)	(1.190)	(10.10)	(7.118)	(13.01)	(12.17)
Observations	12,108	12,108	12,108	12,108	18,995	18,995	18,995	18,995
R-squared	0.139	0.149	0.067	0.296	0.296	0.265	0.228	0.066
Panel D. Heterogeneity by *Military Expenditure per capita*								
	Low				High			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-8.921*	-4.047	-6.926**	-4.942***	-2.587	-6.979	-1.583	18.82
	(4.429)	(2.820)	(3.220)	(0.869)	(8.946)	(7.319)	(9.585)	(12.99)
Observations	15,386	15,386	15,386	15,386	22,190	22,190	22,190	22,190
R-squared	0.257	0.261	0.125	0.206	0.276	0.256	0.216	0.098

Notes: All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Significance level: *** 1%, ** 5%, and * 10%.

7.2.2 Recipient Autocracy vs. Democracy

Recipient country governments that are autocratic should be less affected by donor distraction. If a country government is already autocratic, the reputational loss to being seen acting repressively to the opposition is lower than a country that is democratic and thus overall better regarded by OECD donors. The latter group of recipients are more likely to avail themselves of the opportunities for opposition oppression afforded by donor country distraction.

Table 5 tests this conjecture by breaking the recipient sample into three categories: Autocracies (Polity IV Score $<$ - 5) in Panel A, Anocracies (-5 $\geq$ Polity IV Score $<$ 5) in Panel B, and Democracies (Polity IV Score $\geq$ 5), in Panel C. Each column represents a specification of the main model for the relevant variable testing each one of the model's predictions. Again, there are eight corresponding columns. Columns (1) to (4) report the unconditional coefficient on the natural disaster variable that test the model's core predictions and columns (5) to (8) report those conditional on agitation already underway.

Table 5: Recipient Country Heterogeneity: Autocracy, Anocracy, Democracy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Autocracy								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-0.178	-0.151	-0.0305	-0.135	-34.97	-28.33	-4.370	-16.18
	(0.197)	(0.166)	(0.0455)	(0.287)	(20.71)	(25.09)	(6.755)	(14.68)
Observations	47,113	47,113	47,113	47,113	1,012	1,012	1,012	1,012
R-squared	0.027	0.020	0.017	0.096	0.112	0.103	0.086	0.105
Panel B. Anocracy								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-0.352**	-0.175	-0.233*	-0.197	-11.23**	-7.223*	-7.241**	-5.596***
	(0.172)	(0.179)	(0.135)	(0.162)	(5.021)	(4.084)	(3.227)	(1.453)
Observations	223,901	223,901	223,901	223,901	24,547	24,547	24,547	24,547
R-squared	0.206	0.189	0.100	0.203	0.185	0.209	0.092	0.248
Panel C. Democracy								
	Unconditional				Conditional			
Dep Var	Demonstrations	Protests	Riots	VAC	Demonstrations	Protests	Riots	VAC
Disaster (previous day)	-0.562	-0.407	-0.270	1.176	-3.026***	-3.046***	-0.521	11.25***
	(0.702)	(0.654)	(0.210)	(1.133)	(0.891)	(0.573)	(0.855)	(1.645)
Observations	110,860	110,860	110,860	110,860	16,940	16,940	16,940	16,940
R-squared	0.369	0.309	0.282	0.138	0.324	0.278	0.236	0.196

Notes: All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Significance level: *** 1%, ** 5%, and * 10%.

As conjectured, the model's main results entirely fail to hold for the Autocracy recipient sub-sample. The Anocracy recipient sub-sample is mixed, with the unconditional predictions of the model holding, but the conditional ones for VAC failing. In particular a negative and significant effect on VAC. In the Democracy recipient sub-sample the model's predictions are most strongly supported. Here each one of the main specifications lines up correctly in the point estimates for the unconditional specification (though no longer statistically significant) and is both as predicted, and remains statistically significant for the conditional specification.

7.2.3 Summary

Overall, the heterogeneity analysis reveals that the results are most strongly consistent with model predictions when donors are democracies and when they have elected left-wing governments. The recipient countries most likely to be affected by donor inattention are the non-autocracies with low state/military capacity.

In contrasting with the Israel study (Durante and Zhuravskaya (2018)), there is evidence in support of differences in effects being driven by lower state capacity in the African sub-sample; since the higher state capacity African countries do not follow the patterns we have reported. But even the highest state capacity countries in the African sub-sample are significantly below Isreali levels, so it is not surprising that even these countries do not resemble the Israeli pattern. The other differentiating factors, i.e., the strong political ties to the U.S., the detailed information about the location of anti-government targets, and their relative concentration, in the case of Israel, may also be reasons for why the effects of donor distraction in Israel do not generalize to Africa.

8 Conclusion

We have found that when donor countries experience natural disasters, or elections, there is less public civil unrest in African recipient countries. A theoretical model which focuses on the actions taken by opposition forces in recipient countries makes sense of these findings. It also suggests patterns of violence against civilians that are observed to occur around natural disasters, but not similarly around elections in donor countries.

In contrast with much of the literature on donor inattention, these findings suggest, at least for the countries of relatively low state capacity in our sample, that it is government reactions, rather than actions, which are most affected by donor inattention. This in turn affects the decisions made by opposition forces, and is consistent with the pattern of muted opposition dissent that we have documented surrounding donor distraction. It is also consistent with increased polarization in the expression of dissent. When agitations are already underway, natural disasters lead to the moderation of public unrest, but increased targeted private violence on the part of African opposition groups.

We have conjectured that the mechanism linking significant, newsworthy donor events (elections and natural disasters) to political activities in recipient countries is citizen inattention. In future work, undertaking a more systematic search for the connecting factor would be feasible (though perhaps difficult), using a more comprehensive media content analysis across countries.

Let us close this contribution by briefly highlighting the policy implications of our findings. First of all, it is important to keep in mind that absence of large-scale repression does not mean that a government is upholding human rights. In fact, when key donors are distracted, a given "bad" government is able to bully citizens into staying away from peacefully voicing their discontent in the street. Repression is an "out of equilibrium" threat that autocratic despots can use to muzzle free speech. Hence, the actually observed repression levels may simply constitute the "tip of the iceberg" and alongside repression the mere *threat of repression* serves the government as weapon to get its way. This highlights that authoritarian crack-downs on their population may actually be in reality way worse than what is picked up by commonly available data sources.

What can *we* do to foster the peaceful voicing of free speech? International attention and scrutiny! As shown in the current piece, when international attention fades, the range for peaceful demonstrations gets squeezed to the benefit of either apathetic endurance or violent extremism, which both do not bode well for the future perspectives of a nation.

References

Acemoglu, D. and J. A. Robinson (2000): "Democratization or repression?" *European Economic Review*, 44, 683–693.

Anderton, C. H. and J. Brauer (2021): "Mass atrocities and their prevention," *Journal of Economic Literature*, 59, 1240–92.

Barbera, S., M. O. Jackson, et al. (2020): "A Model of Protests, Revolution, and Information," *Quarterly Journal of Political Science*, 15, 297–335.

Battaglini, M. (2017): "Public protests and policy making," *The Quarterly Journal of Economics*, 132, 485–549.

Battaglini, M., R. B. Morton, and E. Patacchini (2020): "Social Groups and the Effectiveness of Protests," Tech. rep., National Bureau of Economic Research.

Baum, M. A. (2004): "How public opinion constrains the use of force: The case of Operation Restore Hope," *Presidential Studies Quarterly*, 34, 187–226.

Berman, N., M. Couttenier, D. Rohner, and M. Thoenig (2017): "This mine is mine! How minerals fuel conflicts in Africa," *American Economic Review*, 107, 1564–1610.

Besley, T. and R. Burgess (2002): "The political economy of government responsiveness: Theory and evidence from India," *The quarterly journal of economics*, 117, 1415–1451.

Besley, T. and T. Persson (2009): "Repression or civil war?" *American Economic Review*, 99, 292–297.

——— (2011): "Pillars of prosperity," in *Pillars of Prosperity*, Princeton University Press.

Blattman, C. and J. Annan (2016): "Can employment reduce lawlessness and rebellion? A field experiment with high-risk men in a fragile state," *American Political Science Review*, 110, 1–17.

Bursztyn, L., D. Cantoni, D. Y. Yang, N. Yuchtman, and Y. J. Zhang (2021): "Persistent political engagement: Social interactions and the dynamics of protest movements," *American Economic Review: Insights*, 3, 233–50.

Campante, F. R. and D. Chor (2014): ""The people want the fall of the regime": Schooling, political protest, and the economy," *Journal of Comparative Economics*, 42, 495–517.

Canes-Wrone, B. (2010): *Who Leads Whom?*, University of Chicago Press.

Cantoni, D., D. Y. Yang, N. Yuchtman, and Y. J. Zhang (2019): "Protests as strategic games: experimental evidence from Hong Kong's antiauthoritarian movement," *The Quarterly Journal of Economics*, 134, 1021–1077.

Cilliers, J., O. Dube, and B. Siddiqi (2016): "Reconciling after civil conflict increases social capital but decreases individual well-being," *Science*, 352, 787–794.

Collier, P. and A. Hoeffler (2002): "Aid, policy and peace: Reducing the risks of civil conflict," *Defence and Peace Economics*, 13, 435–450.

De Ree, J. and E. Nillesen (2009): "Aiding violence or peace? The impact of foreign aid on the risk of civil conflict in sub-Saharan Africa," *Journal of Development Economics*, 88, 301–313.

Dell, M. and P. Querubin (2018): "Nation building through foreign intervention: Evidence from discontinuities in military strategies," *The Quarterly Journal of Economics*, 133, 701–764.

Dimant, E., T. Krieger, and D. Meierrieks (2020): "Paying them to hate US: The effect of US military aid on anti-American terrorism, 1968-2014," .

Djourelova, M. and R. Durante (2019): "Media attention and strategic timing in politics: Evidence from US presidential executive orders," .

Dube, O. and S. Naidu (2015): "Bases, bullets, and ballots: The effect of US military aid on political conflict in Colombia," *The Journal of Politics*, 77, 249–267.

Durante, R. and E. Zhuravskaya (2018): "Attack when the world is not watching? US news and the Israeli-Palestinian conflict," *Journal of Political Economy*, 126, 1085–1133.

Eisensee, T. and D. Strömberg (2007): "News droughts, news floods, and US disaster relief," *The Quarterly Journal of Economics*, 122, 693–728.

Enikolopov, R., A. Makarin, and M. Petrova (2020): "Social media and protest participation: Evidence from Russia," *Econometrica*, 88, 1479–1514.

Ettinger, A. (2020): "Is there an emerging left-wing foreign policy in the United States?" *International Journal*, 75, 24–48.

Fearon, J. and B. Hansen (2018): "The arms trade, international alignments, and international conflict," Tech. rep., Working Paper—Stanford University and University of Copenhagen.

González, F. (2020): "Collective action in networks: Evidence from the Chilean student movement," *Journal of Public Economics*, 188, 104220.

González, F. and M. Prem (2022): "Police Violence, Student Protests, and Educational Performance," *The Review of Economics and Statistics*, 1–46.

Hager, A., L. Hensel, J. Hermle, and C. Roth (2022): "Group size and protest mobilization across movements and countermovements," *American Political Science Review*, 116, 1051–1066.

Harari, M. and E. L. Ferrara (2018): "Conflict, climate, and cells: a disaggregated analysis," *Review of Economics and Statistics*, 100, 594–608.

Hill, D. W. and Z. M. Jones (2014): "An empirical evaluation of explanations for state repression," *American Political Science Review*, 108, 661–687.

König, M. D., D. Rohner, M. Thoenig, and F. Zilibotti (2017): "Networks in conflict: Theory and evidence from the great war of africa," *Econometrica*, 85, 1093–1132.

Manacorda, M. and A. Tesei (2020): "Liberation technology: Mobile phones and political mobilization in Africa," *Econometrica*, 88, 533–567.

Marcucci, A., D. Rohner, and A. Saia (2023): "Ballot or Bullet: The Impact of the UK's Representation of the People Act on Peace and Prosperity," *The Economic Journal*, 133, 1510–1536.

McGuirk, E. and M. Burke (2020): "The economic origins of conflict in Africa," *Journal of Political Economy*, 128, 3940–3997.

Mueller, J. E. (1973): *War, presidents, and public opinion*, New York: Wiley.

Nunn, N. and N. Qian (2014): "US food aid and civil conflict," *American Economic Review*, 104, 1630–66.

Passarelli, F. and G. Tabellini (2017): "Emotions and political unrest," *Journal of Political Economy*, 125, 903–946.

Qin, B., D. Strömberg, and Y. Wu (2017): "Why does China allow freer social media? Protests versus surveillance and propaganda," *Journal of Economic Perspectives*, 31, 117–140.

Rohner, D. (2022): "Mediation, military and money: The promises and pitfalls of outside interventions to end armed conflicts," *Journal of Economic Literature*.

Rohner, D. and M. Thoenig (2021): "The elusive peace dividend of development policy: From war traps to macro complementarities," *Annual Review of Economics*, 13, 111–131.

Sangnier, M. and Y. Zylberberg (2017): "Protests and trust in the state: Evidence from African countries," *Journal of Public Economics*, 152, 55–67.

Singer, J. D. and M. Small (2022): "Correlates of war project: International and civil war data, 1816-1992," .

SIPRI (2023): "SIPRI Fact Sheet 2023: Trends in World Military Expenditure, 2022," *https://www.sipri.org/sites/default/files/2023-04/2304*$_{f}s_{m}$*ilex*$_{2}$022*.pdf*.

Sobel, R. (2001): *Impact of Public Opinion on U.S. Foreign Policy Since Vietnam*, Oxford University Press New York.

Strömberg, D. (2004): "Radio's impact on public spending," *The Quarterly Journal of Economics*, 119, 189–221.

UCDP (2022): "Uppsala Conflict Data Program," *https://ucdp.uu.se/*.

Appendix
For Online Publication

A Data Appendix

A.1 Summary Statistics

Our core outcome variables on political violence, defined at the recipient country level, are summarized in the first set of variables in Table A1. The data come from the Armed Conflict Location and Event Data Project (ACLED), which is derived from a wide range of local, regional and national sources that are collected by trained data experts worldwide.[23] ACLED contains geographical (GPS) and time (day) precision data for a large set of conflict events throughout Africa over the period 1997 through to the present. For our purposes, ACLED tracks political violence, demonstrations and select (politically important) non-violent events. The types of events include battles, explosions/remote violence, violence against civilians, protests, riots, strategic development. In terms of actors, there are state forces, rebels, militias, identity groups, demonstrators, civilians and external forces. The key outcomes we focus on are defined as follows. The variable *Demonstrations* combines all *Protests* and *Riots* as defined as follows. The variable *Protests* refers to public demonstrations in which the participants do not engage in violence, though violence may be used against them. The variable *Riots* refers to violent events where demonstrators or mobs engage in disruptive acts, including but not limited to rock throwing, property destruction, etc. The variable *Violence Against Civilians* refers to violent events where an organised armed group deliberately inflicts violence upon unarmed non-combatants (e.g beating, shooting, torture, rape, mutilation, kidnapping).

We also consider two additional outcome measures of repression. The first, *State Violence*, comes from ACLED and is coded as one if the main actor of the political violence is state forces. The second measure, *Repression*, comes from the Social Conflict Analysis Database (SCAD).[24] This information is based on searches of Associated Press and Agence France Presse news wires, as compiled by the Lexis-Nexis news service. The variable we focus on is their a measure of government repression in the form of pro-government violence.

The next set of variables in Table A1 describe our two explanatory variables of interest, disasters and elections in donor countries. For our measure of natural disasters we rely on the EM-DAT International Disaster Database created by the Centre for Research on the Epidemiology of Disasters (CRED) at the School of Public Health of the Université Catholique de Louvain (Belgium).[25] We focus on natural disasters and only those with 115 or more people dead. This restricts the analysis to the top 10% of natural disasters in terms of severity of fatalities.

For the national elections data, we put this together ourselves for all donor countries. We collected information on key national elections for all of the main donors since 1989. We have focused on the elections that were decisive on the identity of the head of government (i.e. presidential elections in the United States and France, Bundestag elections in Germany, and general elections in the UK and Japan). We

[23] See: https://acleddata.com.
[24] See: https://korbel.du.edu/sie/research/data-downloads.
[25] See: https://www.emdat.be.

have handcoded the dates of elections and the parties elected, using a varieties of data sources, including Encyclopedia Britannica, CNN, France24, Die Welt, BBC.

The next set of variables in Table A1 describe our climatic control variables included in some of our specifications. This data come from the Climate Change Knowledge Portal of the World Bank.[26]

The last group of variables described in Table A1 are those used in our recipient heterogeneity analysis of Section 7.2, namely GDP per capita; Bureaucracy quality (as measured by International Country Risk Guide (ICRG)); Tax to GDP ratio, and Military Expenditure per capita. The final three variables are constructed using information from the polity data base, the three categories are: Autocracies (Polity IV Score $<$ - 5); Anocracies (-5 $\geq$ Polity IV Score $<$ 5); and Democracies (Polity IV Score $\geq$ 5).

Table A1: Summary Statistics - Recipient Level

Variables	Mean	SD	Min	Max	N
Demonstrations	0.0694	0.254	0	1	393,715
Protests	0.0489	0.216	0	1	393,715
Riots	0.0315	0.175	0	1	393,715
Violence Against Civilians	0.0193	0.138	0	1	393,715
State Violence	0.0618	0.241	0	1	393,715
Repression	0.00245	0.0495	0	1	490,896
Disaster (Share within 10 preceding years)	1.74e-05	0.000851	0	0.277	525,017
Election (Share within 10 preceding years)	2.68e-05	0.000950	0	0.227	533,618
Mean monthly temperature	24.47	4.511	6.420	33.97	536,905
Mean monthly log(precipitation + 0.01)	3.265	2.063	-4.605	6.371	536,905
Growing Season	0.615	0.374	0	1	515,721
GDP per capita	4,561	5,190	436.7	41,249	489,789
Bureaucratic quality	1.635	0.894	0	3	275,392
Tax to GDP ratio	14.44	6.398	0.000148	30.89	367,798
Military Expenditure per capita	46.72	97.86	0.500	1,422	415,285
Autocracy	0.180	0.384	0	1	536,905
Anocracy	0.539	0.498	0	1	536,905
Democracy	0.281	0.449	0	1	536,905

Sources: ACLED, SCAD, EM-DAT, World Bank, PRIO-GRID, World Bank, Polity.

[26] Refer to: https://climateknowledgeportal.worldbank.org.

Table A2 provides summary statistics on our donor-level information used in the analysis. It describes the raw information used to compute our two key recipient-donor pair explanatory variables for disasters and elections. The sources of this data are described above. It also summarizes the information used in our donor heterogeneity analysis of Section 7.1.

Donor countries are characterized as democratic (a Polity IV score ≥ 5) or non-democratic (a Polity IV score < 5). The donor countries that are considered non-democratic are: Azerbaijan, Chinese Taipei, Kazakhstan, Kuwait, Russia, Saudi Arabia, Thailand, and United Arab Emirates. We cannot further disaggregate the "non-democratic" donor country grouping into Anocracies ($-5 \geq$ Polity IV Score < 5) and Autocracies (Polity IV Score < -5) as the samples become too small to uncover sufficient variation.

To characterise donor countries as either right or left-wing, we collected the election results for our set of 5 major donor countries over the sample period and matched the winning political party to whether they stood to the right or left on the political spectrum. We focused on the elections that were decisive on the identity of the head of government (i.e. presidential elections in the United States and France, Bundestag elections in Germany, and general elections in the UK and Japan). We hand-coded the dates of the elections as well as the parties elected, using a variety of data sources, including Encyclopedia Britannica, CNN, France24, Die Welt, and the BBC. Left-wing coded governments include the Democratic Party in the United States, the Labour Party in the United Kingdom, the Social Democratic Party in Germany, and the Parti Socialiste in France. Right-wing ones include the Republican Party in the United States, the Conservative Party in the United Kingdom, the Christian Democratic Union in Germany, the Front National in France.

Table A2: Summary Statistics - Donor Level

Variables	Mean	SD	Min	Max	N
Disaster (previous day)	0.000484	0.0220	0	1	281,043
Democratic	0.815	0.388	0	1	281,043
Non-Democratic	0.185	0.388	0	1	281,043
Election day	0.000786	0.0280	0	1	54,690
Right-wing	0.685	0.465	0	1	50,537
Left-wing	0.296	0.457	0	1	50,537

Sources: Polity, EM-DAT.

A.2 Main Estimation Results

A.2.1 Repression

The below tables report the estimation results depicted in Figure 5. Table A1 is for the top-left panel, Table A2 for the bottom-left panel, Table A3 for the top-right panel, and Table A4 for the bottom-right panel (respectively).

Table A3: Impact of Disasters on State Violence

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-0.364	-0.329	-0.316	-0.371	-0.513***	-0.421*
	(0.250)	(0.250)	(0.287)	(0.240)	(0.175)	(0.247)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	381875	381875	381875	381875	366716	381875

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A4: Impact of Elections on State Violence

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	0.247	0.105	0.069	0.253	-0.103	0.245
	(0.334)	(0.242)	(0.240)	(0.336)	(0.210)	(0.323)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	390428	390428	390428	390428	375088	390428

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A5: Impact of Disasters on Repression

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	0.040	0.044	0.045	0.040	0.042	0.039
	(0.051)	(0.052)	(0.053)	(0.051)	(0.053)	(0.053)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	488341	488341	488341	488341	468251	488341

Notes: Outcome variable is from SCAD. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A6: Impact of Elections on Repression

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-0.040	-0.051	-0.038	-0.038	-0.014	-0.034
	(0.032)	(0.043)	(0.040)	(0.033)	(0.032)	(0.033)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	488339	488339	488339	488339	468250	488339

Notes: Outcome variable is from SCAD. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

A.2.2 Testing Prediction 1: Disasters (Unconditional)

Tables A5 to A8 report the estimation results illustrated in Figure 6. Table A5 corresponds to the top-left panel; Table A6 to the top-right panel; Table A7 to the bottom-left panel; and Table A8 to the bottom-right panel (respectively).

Table A7: Impact of Disasters on Demonstrations

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-0.386*	-0.420**	-0.414**	-0.448**	-0.403**	-0.415**
	(0.203)	(0.186)	(0.186)	(0.187)	(0.195)	(0.188)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	381874	381874	381874	381874	366715	381874

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A8: Impact of Disasters on Protests

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-0.217	-0.270	-0.316*	-0.262	-0.235	-0.246
	(0.207)	(0.204)	(0.187)	(0.191)	(0.198)	(0.188)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	381874	381874	381874	381874	366715	381874

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A9: Impact of Disasters on Riots

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-0.233**	-0.218**	-0.198*	-0.255**	-0.227**	-0.242**
	(0.093)	(0.101)	(0.101)	(0.098)	(0.101)	(0.111)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	381874	381874	381874	381874	366715	381874

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A10: Impact of Disasters on Violence Against Civilians

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	0.073	-0.025	-0.037	0.099	0.103	0.076
	(0.216)	(0.264)	(0.314)	(0.200)	(0.202)	(0.218)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	381874	381874	381874	381874	366715	381874

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

A.2.3 Testing Prediction 2: Disasters (Conditional)

Tables A9 to A12 report the estimation results illustrated in Figure 7. Table A9 corresponds to the top-left panel; Table A10 to the top-right panel; Table A11 to the bottom-left panel; and Table A12 to the bottom-right panel (respectively).

Table A11: Impact of Disasters on Demonstrations (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-5.146***	-5.342***	-4.291***	-5.078***	-5.043***	-4.928***
	(1.446)	(1.161)	(1.452)	(1.598)	(1.612)	(1.573)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	42499	42499	42499	42499	41498	42499

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A12: Impact of Disasters on Protests (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-4.087***	-4.648***	-4.177***	-4.066***	-4.072***	-4.048***
	(0.857)	(0.890)	(1.094)	(1.028)	(1.030)	(1.012)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	42499	42499	42499	42499	41498	42499

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A13: Impact of Disasters on Riots (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	-2.205	-1.715	-1.403	-2.115	-2.025	-1.888
	(1.550)	(1.527)	(1.481)	(1.619)	(1.612)	(1.557)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	42499	42499	42499	42499	41498	42499

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A14: Impact of Disasters (Conditional) on Violence Against Civilians

	(1)	(2)	(3)	(4)	(5)	(6)
Disaster (previous day)	6.925**	6.653	7.928**	6.851**	6.796**	6.818*
	(3.223)	(4.497)	(3.759)	(3.209)	(3.209)	(3.402)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	42499	42499	42499	42499	41498	42499

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

A.2.4 Testing Prediction 3: Elections (Unconditional)

Tables A13 to A16 report the estimation results illustrated in Figure 8. Table A13 corresponds to the top-left panel; Table A14 to the top-right panel; Table A15 to the bottom-left panel; and Table A16 to the bottom-right panel (respectively).

Table A15: Impact of Elections on Demonstrations

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-0.555***	-0.477***	-0.526***	-0.467***	-0.467**	-0.521***
	(0.166)	(0.163)	(0.159)	(0.172)	(0.212)	(0.175)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	390428	390428	390428	390428	375088	390428

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A16: Impact of Elections on Protests

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-0.238	-0.208	-0.234	-0.171	-0.134	-0.202
	(0.157)	(0.156)	(0.160)	(0.162)	(0.200)	(0.161)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	390428	390428	390428	390428	375088	390428

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A17: Impact of Elections on Riots

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-0.408***	-0.357***	-0.385***	-0.378***	-0.426***	-0.403***
	(0.117)	(0.131)	(0.124)	(0.121)	(0.143)	(0.123)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	390428	390428	390428	390428	375088	390428

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A18: Impact of Elections on Violence Against Civilians

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-0.202*	-0.305*	-0.292**	-0.208*	-0.182	-0.221*
	(0.120)	(0.155)	(0.142)	(0.121)	(0.146)	(0.123)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	390428	390428	390428	390428	375088	390428

Notes: Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

A.3 Testing Prediction 4: Elections (Conditional)

Tables A17 to A20 report the estimation results illustrated in Figure 9. Table A17 corresponds to the top-left panel; Table A18 to the top-right panel; Table A19 to the bottom-left panel; and Table A20 to the bottom-right panel (respectively).

Table A19: Impact of Elections on Demonstrations (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-2.709	-2.524	-2.979	-2.657	-2.741	-2.783
	(2.049)	(1.814)	(1.952)	(2.020)	(2.012)	(1.957)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	44854	44854	44854	44854	43784	44854

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A20: Impact of Elections on Protests (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	0.640	0.732	0.040	0.646	0.685	0.665
	(1.422)	(1.441)	(1.391)	(1.364)	(1.356)	(1.369)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	44854	44854	44854	44854	43784	44854

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A21: Impact of Elections on Riots (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-3.595***	-3.752***	-3.397**	-3.676***	-3.881***	-3.788***
	(1.318)	(1.334)	(1.432)	(1.328)	(1.336)	(1.277)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	44854	44854	44854	44854	43784	44854

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

Table A22: Impact of Elections on Violence Against Civilians (Conditional)

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Election day	-0.370	-1.169	-1.050	-0.347	-0.193	-0.484
	(1.175)	(1.490)	(1.611)	(1.164)	(1.225)	(1.195)
CY + Month FEs	✓					
Country-year-quarter FEs		✓				
Country-year-month FEs			✓			
CY FEs + Temp. + Precip.				✓		
CY FEs + Temp. + Precip. + GS					✓	
Cy FEs + Country time trend						✓
Observations	44854	44854	44854	44854	43784	44854

Notes: Sample is conditional on riots in the previous week in the recipient country. Outcome variable is from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%. CY refers to country-year and GS refers to growing season.

A.4 Additional Estimation Results

In our main analysis, we constructed the bilateral link between donor country d and recipient country r in a given year y by the share of total transfers, from a particular donor country d with respect to all transfers, ODA, received by a recipient within the ten years preceding a given year y. Here we consider alternative ways to define this bilateral link.

We first construct this link with respect to the share of total transfers, with respect to all transfers, within the twenty and thirty (as opposed to ten in the main specification) preceding years. As a third alternative, we use the share of transfers over the entire sample period. At the other extreme, we use the share of transfers just in the previous year.

We also specified the link only if the recipient received a transfer from their major donor (defined as the country who donated the highest sum of donations to a given recipient country over the entire sample period) within the past year or over the entire sample period.[27]

Instead of *ODA* transfers, we also conceive that the link could be in terms of military transfers. For this information, we use a dataset from Fearon and Hansen (2018) that draws on information from the Stockholm International Peace Research Institute (SIPRI) on arms transfers between states since 1950. We consider two types of links: one based on the overall relative military shares across the entire sample period and also the share from the major military donor over the sample period.

Summary statistics on our core explanatory variables of interest using these alternative methods to define the link between donor and recipient countries are reported below.

Table A23: Summary Statistics - Alternative Links

Variables	Mean	SD	Min	Max	N
Disaster (Share within 10 preceding years)	1.74e-05	0.000851	0	0.277	525,017
Disaster (Share within 20 preceding years)	0.000183	0.00606	0	0.774	527,938
Disaster (Share within 30 preceding years)	0.000180	0.00596	0	0.728	527,938
Disaster (Share across entire sample period)	0.000193	0.00656	0	0.772	527,938
Disaster (Share in previous year)	0.000218	0.0137	0	8.393	527,938
Disaster (Major Donor in previous year)	0.000415	0.0204	0	1	527,938
Disaster (Major Donor over entire sample period)	0.000307	0.0175	0	1	527,938
Disaster (Military Partner - Share across entire sample period)	0.00115	0.0206	0	0.755	526,313
Disaster (Major Military Partner over entire sample period)	0.00127	0.0356	0	1	526,313
Election (Share within 10 preceding years)	2.68e-05	0.000950	0	0.227	533,618
Election (Share within 20 preceding years)	0.000315	0.00839	0	0.761	536,905
Election (Share within 30 preceding years)	0.000320	0.00857	0	0.717	536,905
Election (Share across entire sample period)	0.000350	0.00961	0	0.752	536,905
Election (Share in previous year)	0.000323	0.00952	0	1.750	536,905
Election (Major Donor in previous year)	0.000633	0.0252	0	1	536,905
Election (Major Donor over entire sample period)	0.000641	0.0253	0	1	536,905
Election (Military Partner - Share across entire sample period)	0.000299	0.0102	0	0.639	505,129
Election (Major Major Military Partner over entire sample period)	0.000441	0.0210	0	1	505,129

Sources: EM-DAT, Stockholm International Peace Research Institute (SIPRI), Fearon and Hansen (2018).

Tables A23 through to A26 report the results to test Predictions 1 through 4 respectively.

[27] We also considered defining the link by the former colonist of the recipient country. This variable tended to be highly correlated with the major donor.

For Prediction 1, the core results reported in Figure 6 are confirmed in Table A23 for a set of these alternative link definitions. That is, our core finding of a significant negative impact of disasters (in the previous day) on riots is found when we alter the moving average to the past 20 years (the first row); to the past 30 years (the second row); or across the whole sample period (the third row). The results are also significant for the link defined by the major donor over the entire sample period (the fifth row). The signs of the estimated coefficients are as expected across all specifications in Table A23 but we do not find statistical significance when we define the link via military connections. Though not reported here, we do find significant results in alternative specifications which include a less stringent set of fixed effects (in particular without including month fixed effects).

For Prediction 2, the findings reported in Figure 7 are also confirmed in Table A24 in most specifications. In particular, a significant negative effects on demonstrations (protests) and a significant positive effect on VAC. We again find weaker support when we define the link via military connections.

For Prediction 3, the central findings reported in Figure 8 are again confirmed in Table A25. This time we find support also for defining the link through military connections instead. Table A26 likewise confirms the findings of Prediction 4 (reported in Figure 9) across all specifications of defining the link.

Table A24: Impact of Disasters (Unconditional) - Alternative Links

Variables	Demonstrations	Protests	Riots	VAC
Share within 20 preceding years	-0.479	-0.269	-0.283***	0.106
	(0.314)	(0.316)	(0.064)	(0.272)
Share within 30 preceding years	-0.326	-0.104	-0.298***	0.014
	(0.413)	(0.393)	(0.053)	(0.287)
Share across entire sample period	-0.132	0.049	-0.251***	-0.033
	(0.394)	(0.394)	(0.062)	(0.227)
Share in previous year	-0.458	-0.308	-0.233	-0.025
	(0.420)	(0.365)	(0.151)	(0.316)
Major Donor in previous year	-0.148	-0.124	-0.043	0.017
	(0.098)	(0.085)	(0.030)	(0.086)
Major Donor over entire sample period	-0.027	0.023	-0.070***	-0.030
	(0.096)	(0.102)	(0.019)	(0.073)
Share Military across sample	-0.007	-0.005	-0.008	0.003
	(0.015)	(0.009)	(0.011)	(0.011)
Major Military across sample	-0.005	-0.000	-0.008	0.005
	(0.010)	(0.008)	(0.006)	(0.006)
Observations	381,874	381,874	381,874	381,874

Notes: Reported coefficient estimates are for disasters in the previous day. All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Outcome variables are from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%.

Table A25: Impact of Disasters (Conditional) - Alternative Links

Variables	Demonstrations	Protests	Riots	VAC
Share within 20 preceding years	-3.639***	-2.876***	-1.440	5.882***
	(1.199)	(0.657)	(1.117)	(1.610)
Share within 30 preceding years	-6.086***	-4.652***	-2.866	7.632**
	(2.213)	(1.197)	(2.159)	(3.239)
Share across entire sample period	-8.999**	-6.678***	-4.756	7.113
	(3.448)	(2.017)	(3.142)	(4.602)
Share in previous year	-8.003***	-6.504***	-3.646	9.102**
	(2.779)	(1.657)	(2.783)	(4.231)
Major Donor in previous year	-1.050**	-0.966***	-0.375	2.282***
	(0.424)	(0.319)	(0.420)	(0.490)
Major Donor over entire sample period	-1.985	-1.452	-1.426	-0.383*
	(1.435)	(0.990)	(1.096)	(0.219)
Share Military across sample	-0.148	-0.161*	-0.043	-0.022
	(0.113)	(0.084)	(0.141)	(0.038)
Major Military across sample	-0.051	-0.049	-0.030	-0.036
	(0.061)	(0.061)	(0.069)	(0.028)
Observations	42,499	42,499	42,499	42,499

Notes: Sample is conditional on riots in the previous week in the recipient country. Reported coefficient estimates are for disasters in previous day. All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Outcome variables are from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%.

Table A26: Impact of Elections (Unconditional) - Alternative Links

Variables	Demonstrations	Protests	Riots	VAC
Share within 20 preceding years	-0.671***	-0.302	-0.478***	-0.239
	(0.235)	(0.215)	(0.134)	(0.169)
Share within 30 preceding years	-0.596***	-0.255	-0.444***	-0.138
	(0.215)	(0.188)	(0.152)	(0.189)
Share across entire sample period	-0.338*	-0.095	-0.321**	0.118
	(0.201)	(0.213)	(0.131)	(0.225)
Share in previous year	-0.428*	-0.163	-0.369*	-0.188
	(0.214)	(0.157)	(0.200)	(0.114)
Major Donor in previous year	-0.047	-0.044	-0.019	-0.054
	(0.077)	(0.058)	(0.053)	(0.041)
Major Donor over entire sample period	-0.034	0.041	-0.084**	0.043
	(0.102)	(0.111)	(0.034)	(0.0704)
Share Military across sample	-0.052	-0.004	-0.053***	-0.026
	(0.037)	(0.038)	(0.015)	(0.018)
Major Military across sample	-0.033	-0.012	-0.033***	-0.011
	(0.021)	(0.022)	(0.010)	(0.009)
Observations	390,428	390,428	390,428	390,428

Notes: Reported coefficient estimates are for disasters in previous day. All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Outcome variables are from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%.

Table A27: Impact of Elections (Conditional) - Alternative Links

Variables	Demonstrations	Protests	Riots	VAC
Share within 20 preceding years	-2.175	0.135	-2.592**	-0.709
	(1.433)	(0.950)	(1.217)	(0.667)
Share within 30 preceding years	-2.232	0.555	-3.224***	-0.370
	(1.608)	(1.360)	(1.145)	(0.631)
Share across entire period	-1.379	0.287	-2.008**	-0.085
	(0.854)	(0.980)	(0.869)	(0.371)
Share in previous year	-1.686	1.825	-3.852***	0.687
	(2.796)	(2.468)	(1.270)	(1.311)
Major Donation in previous year	-0.636**	-0.092	-0.636**	-0.032
	(0.267)	(0.276)	(0.249)	(0.258)
Major donation across sample period	-0.281	0.287	-0.579*	-0.186
	(0.327)	(0.388)	(0.322)	(0.140)
Share Military across sample	-0.215	-0.032	-0.199**	-0.065
	(0.132)	(0.129)	(0.079)	(0.041)
Major Military across sample	-0.111*	-0.059	-0.101*	-0.014
	(0.063)	(0.074)	(0.050)	(0.018)
Observations	44,854	44,854	44,854	44,854

Notes: Sample is conditional on riots in the previous week in the recipient country. Reported coefficient estimates are for disasters in previous day. All regressions include country-year and month fixed effects. VAC refers to "Violence Against Civilians". Outcome variables are from ACLED. Clustered standard errors at the country level are in parentheses. Significance level: *** 1%, ** 5%, and * 10%.

A.5 Event Study Analysis

Below we present the findings of the event study analyses. As mentioned earlier, the event study analyses are confined to the investigation of disasters, as there could be anticipation effects before elections. Note that while these anticipation effects before elections are problematic for event studies, they are less so for our regression analysis that draws on different identifying variation (it compares the post-election effect to the average over the whole period rather than to the value on the election day). Further, our regressions allow for a continuous explanatory variable, while the event study requires to binarize the treatment.

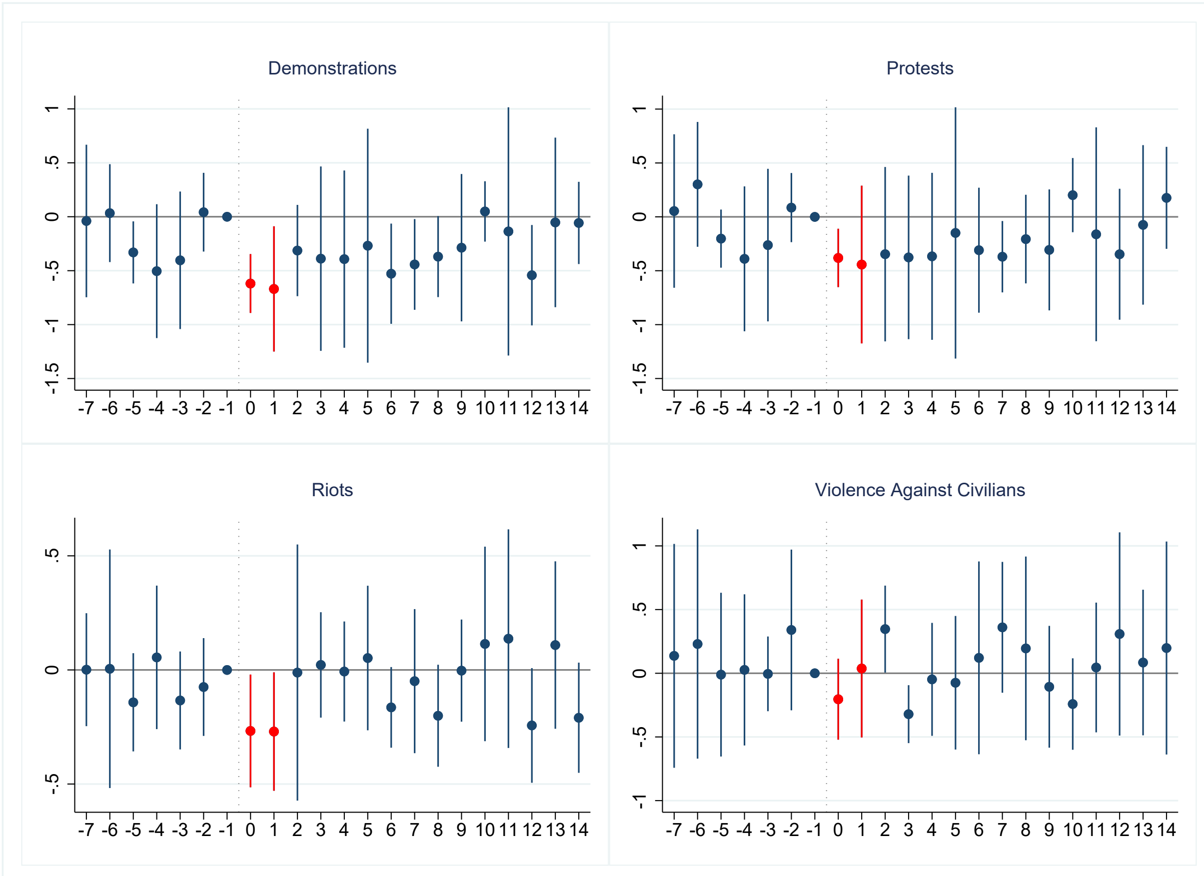


Figure A1: Dynamic Effects of Donor Natural Disasters (Unconditional)

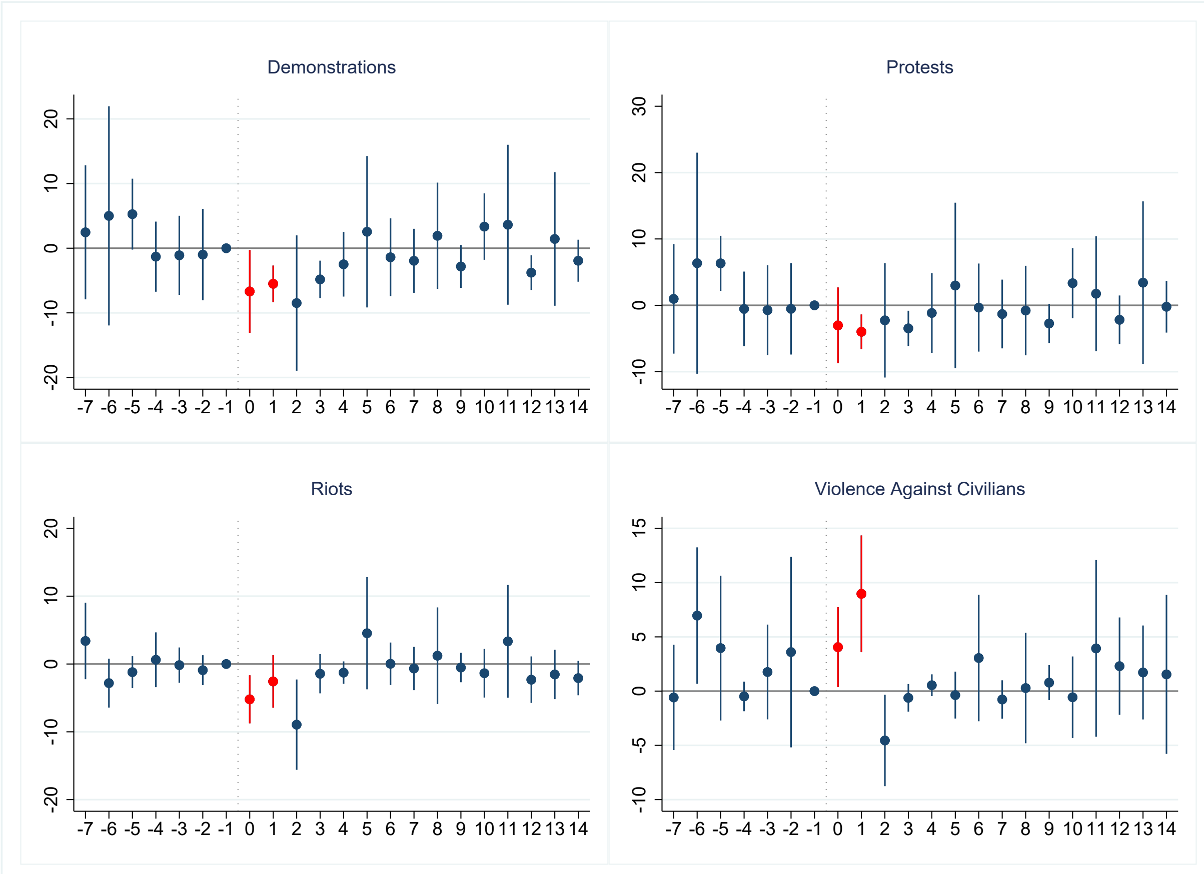


Figure A2: Dynamic Effects of Donor Natural Disasters (Conditional on Previous Riots)

B Model Appendix

B.1 Proof Proposition 1

The point defined as θ^p in the statement of proposition is equivalent to the defined point θ_p^0, which thus always exists. For $\theta < \theta^p$ any action of dissent yields negative utility, so no dissent is parttaken in. From part (i) of assumption *Non-Empty*, at θ^p protests are preferred to both riots and VAC. By continuity, there exists $\theta > \theta^p$ where protests are preferred to non-action as well.

Since $\partial V^r/\partial\theta > \partial V^p/\partial\theta$ for all θ, then necessarily $U^r > U^p$ for all $\theta > \theta^p$. And there exists a unique θ at which $U^r = U^v$, defined as θ^v in the statement. At θ^v, it follows from part (ii) of assumption *Non-Empty* that, necessarily, $U^r > U^p$ as well, and then necessarily also at $\theta = \theta^v$.

Finally, for $\theta > \theta^v$, since $\partial V^v/\partial\theta > \partial V^r/\partial\theta$ it follows that $U^v > U^r > 0$ and all such individuals choose VAC.

$\square$

B.2 Proof Proposition 2

Define $U^p(S)$ as the value of U^p, for a given θ, under repression state $S = R$ or H. Given that $C^p(H) > C^p(R)$, then necessarily $U^p(H) < U^p(R)$ for all values of θ. And since $C^r(H) > C^r(R)$, then also $U^r(H) < U^r(R)$ for all θ. However $U^v(H) = U^v(R)$. Since $U^p(H) < U^p(R)$ for all θ it is immediate that $\theta^p(H) > \theta^p$, and this implies statement *(i)*. Since $U^r(H) < U^r(R)$ and U^v is unchanged, then necessarily $\theta^v(H) < \theta^v$ and this implies statement *(iii)*. Statements *(i)* and *(iii)* imply statement *(ii)*.

□

B.3 Proof Proposition 3

From Proposition 2 part (i), under $S = H$ the total mass of individuals partaking in some sort of dissent is lower than under $S = R$, i.e., the cut-off θ_t for participation rises. This implies the opposition's benefit to choosing dissent, B_t (computed in equation 2) falls under $S = H$. Since benefits from dissent are lower, the level of cost at which the opposition will choose dissent falls; k_t^* is strictly lower when $S_t = H$.

□