

Jansesberger, Viktoria; Spilker, Gabriele

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Silencing dissent in the wake of catastrophe? An examination of the repression dynamics following weather-related rapid-onset events in autocracies

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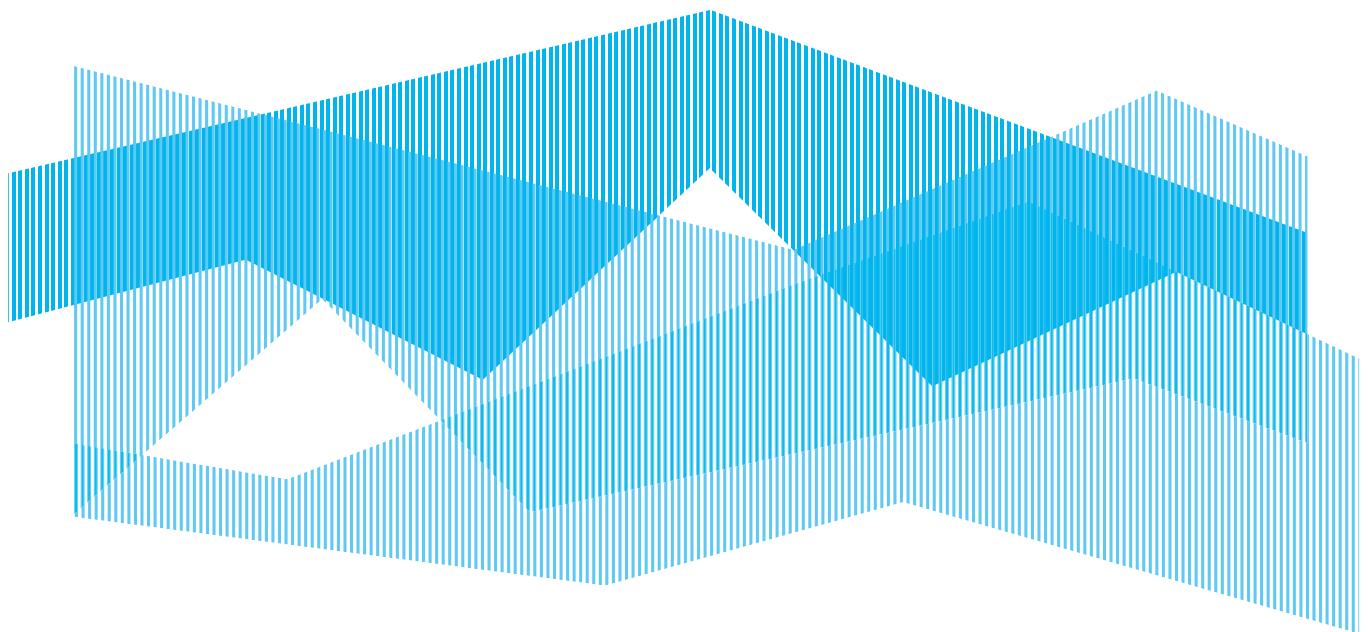
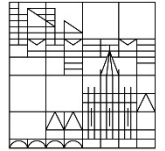
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Silencing Dissent in the Wake of Catastrophe? An Examination of the Repression Dynamics Following Weather-Related Rapid-Onset Events in Autocracies

Viktoria Jansesberger, University of Konstanz, viktoria.jansesberger@uni-konstanz.de

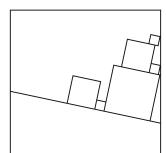
Gabriele Spilker, University of Konstanz, gabriele.spilker@uni-konstanz.de

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About the authors

Viktoria Jansesberger is a postdoctoral researcher at the Cluster of Excellence "The Politics of Inequality" at the University of Konstanz, working under the chair of "International Politics – Global Inequalities" led by Prof. Dr. Gabriele Spilker. Viktoria is involved in the "Climate Inequalities in the Global South: from Perceptions to Protest" project at the University of Konstanz and the "Investigating Climate Justice Preferences for Financing Instruments for Loss and Damage" project at the University of Salzburg. Her research interests include political participation, inequality, gender, and populism.

Gabriele Spilker is Professor of "International Politics – Global Inequality" at the Department of Politics and Public Administration, part of the Excellence Cluster "The Politics of Inequality" and principal investigator of the Cluster project: "Climate Inequalities in the Global South: from Perceptions to Protest". Her main research interests concern both the determinants and the consequences of inequality. In particular, she investigates the consequences of climate change for migration and conflict as well as inequalities in perceiving globalization processes.

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Silencing dissent in the wake of catastrophe?

**An examination of the repression dynamics following weather-related rapid-onset events
in autocracies**

Viktoria Jansesberger & Gabriele Spilker

ABSTRACT

Climate change will increase the frequency of destructive weather events, such as storms and floods, posing heavy burdens on affected societies, particularly in the Global South. These disasters can create fractures between the state and its citizens, potentially leading to social unrest. To manage such situations, autocratic governments might resort to repression. This paper examines whether rapid-onset weather events influence governmental repression in autocracies in the Global South. We argue that such events could trigger repression through two pathways: government-initiated repression to prevent anti-government mobilization and reactive repression in response to existing unrest. Using spatially disaggregated event data for Africa and Latin America, and logistic regression models with fixed effects on a coarsened exact matching sample, we find that government-initiated repression becomes more likely after destructive storms and floods, while reactive repression does not. Thus, autocratic governments prefer preemptive measures over reactive ones when disasters strike.

INTRODUCTION

The consequences of climate change, in the form of more frequent occurrences of extreme weather phenomena, such as storms and floods, do not only manifest themselves already today and therefore much earlier than previously expected but also more intensively (IPCC, 2018, 2021). Such incidents often bring about immense destruction and entail devastating consequences for the affected population (Strömberg 2007; Poggione et al. 2012; Tierney 2019). These extreme weather events and the accompanying scarcity of essential resources often put state-society relations under great strain. In such times, ruptures in the relationship between citizens and their governments become apparent, which, according to recent research, can lead to societal unrest, such as protests and riots (Gizelis et al. 2021; Ide et al. 2021a; Ide et al. 2021b; Koubi et al. 2021).

Up to now, most of the existing research has focused on citizens' responses to extreme weather events. However, numerous examples illustrate that extreme weather events, especially rapid-onset ones, might alter government behavior as well. For example, after Hurricane Matthew hit Cuba in 2016, many journalists who were trying to report on the event suffered attacks and arbitrary arrests by government officials (CPJ, 2016; Translating Cuba, 2017). This oppressive episode was aimed at shutting down potentially unfavorable coverage of how the government handled the disaster. And while repression and censorship are no rare occurrences for Cuban reporters, harassment to this degree was indeed outstanding. Similar incidents in association with flood catastrophes occurred also in Somalia, China, and Bangladesh (NUSOJ 2022; CPJ 2021, 2000). Furthermore, while attacks against members of the press are one prominent example of how rapid-onset events can induce repression, journalists are by no means the only group exposed to this risk. Examples of repression targeting protestors in the wake of rapid-onset events abound too, e.g., after disastrous floods hit the Sinai Peninsula in 2010 (Reuters, 2010).

These anecdotes suggest that governments, especially autocratic ones, seem to frequently tighten their grip on citizens after rapid-onset weather events; either by initiating repression towards potential opponents or by responding with repression against public dissent. Therefore, this paper investigates whether and how rapid-onset events lead to governmental repression in autocratic societies in the Global South. Academic research on this topic is scarce. Findings from the few existing studies, however, indeed corroborate the anecdotal evidence mentioned above .

In this paper, we build on these studies but differentiate between two possible pathways toward repression: *government-initiated* versus *reactive repression*. We argue that in order to understand why autocratic governments use repression after rapid-onset disasters, a more careful distinction between the different types of repression and more detailed theorizing is necessary. More precisely, with respect to government-initiated repression – the preemptive pathway – we argue that governments are inclined to use this type of repression in the direct aftermath of rapid-onset disasters. Deprivation and chaos typically accompany disasters and such circumstances constitute the perfect breeding ground for political discontent, possibly escalating into protests later on. Research has shown that potential threats to a government's survival often result in violent preemptive repression on the side of the government . As a consequence, governments, especially autocratic ones, might be tempted to use the period immediately after a catastrophe as a „window of opportunity“ to stage repressive attacks while citizens are still fully occupied with disaster clean-up.

It is, however, also plausible that disastrous weather events cause an increase in reactive repression. This reactive pathway is based on the assumption that incidents of rapid-onset disasters typically lead to massive destruction and thus constitute a setting in which large parts of the population feel severely aggrieved and potential deficits of a state become visible therefore providing conditions ripe for societal unrest (Berrebi und Ostwald 2013; Ide et al.

2023; Poggione et al. 2012). Further assuming that a government wants to stay in power and that protest is perceived as a threat to political survival in most autocracies, repression becomes especially likely when dissent in the population rises . Anti-government mobilization shortly after disastrous weather events could be especially threatening, as during such periods, governments have little to no means available to accommodate protesters' demands and thus pacify the unrest. In order to resolve this challenging situation, employing reactive repression might represent a viable solution.

We test our theoretical argument using event data coded at the sub-national level, first-order administrative units in particular, for both Africa and Latin America based on the Social Conflict Analysis Database (Salehyan et al., 2012). This approach allows for a more detailed measurement of the extent and nature of repression by including both information on government-initiated and reactive repression events. In doing so, our paper not only enlarges existing research in theoretical terms but also empirically. In contrast to existing studies (Wood und Wright 2016; Pfaff 2020; Apodaca 2017), we move beyond the country level and use a higher level of spatial resolution. We consider this to be important as most rapid-onset events only hit parts of a country and, thus, any country-level study might hide crucial sub-national variation. Furthermore, we disaggregate temporally by looking at monthly variations in governmental repression instead of yearly changes. This enables us to gain deeper insights into the potential time delay between disaster onset and repression by the government.

Applying matching to pre-process our data and using logistic fixed effects regression analyses, our results provide robust support for non-democratic governments relying on the preemptive repression pathway. Our results clearly show that the likelihood of initiating repressive attacks in regions that have been exposed to extreme weather events in the previous months increases significantly in autocracies compared to months that do not follow such extreme weather events. Our results further suggest that this effect materializes already within the first three months

after a disaster has hit a region. In contrast, we do not find any significant effects of extreme weather events on reactive repression. These findings are robust to various alternative specifications and modeling choices. We, therefore, conclude that special attention should be paid to how non-democracies treat journalists and opposition members directly after rapid-onset destructive weather events take place, as these seem to be the actors most prone to repressive attacks by autocratic governments. Moreover, our results underline how important it is to distinguish between different pathways toward repression as our results suggest that extreme weather events while increasing preemptive repression do not seem to trigger reactive repression.

LITERATURE REVIEW

Extreme weather events and state-society relations

Considering the hardship, destruction, and scarcity of essential resources extreme weather events often cause, a vast strand of literature investigates how such events impact state-society relations (Ide et al., 2023; Koubi, 2019; Von Uexkull and Buhaug, 2021). Despite much progress regarding our understanding of the conditions and pathways through which weather-related disasters may cause or intensify societal conflicts, much of this knowledge pertains to severe types of violence as the majority of studies in this research area focuses on civil conflict (for recent examples see: Buhaug et al., 2021 and Linke and Ruether, 2021). Especially in the recent past, building upon the pioneering study of Hendrix and Salehyan (2012), scholars have begun to investigate the impact of extreme weather events on societal unrest (e.g., strikes, riots, and protests) in greater detail (Koren et al. 2021; Yeeles 2015; Ide et al. 2021b; Ide et al. 2021a; Gizelis et al. 2021; Koubi et al. 2021; Petrova 2021). Overall, the findings demonstrate that extreme weather events can indeed impact on societal conflict, but often in an indirect way,

mostly acting as a threat multiplier. Furthermore, the effect often depends on the type of societal conflict as well as the precise context (Mach und Kraan 2021).

However, when looking at what is known about how extreme weather events influence coercive action by the government, other than civil war, a gap becomes apparent. This is somewhat surprising as the numerous examples outlined in the introduction seem to suggest that governments frequently employ repression following extreme weather events. To the best of our knowledge only three studies have so far investigated how weather-related disasters impact governmental repression . And while all three studies are comparable, their findings are somewhat different. Wood and Wright (2016) find a repression-enhancing effect of rapid-onset disasters, which is mitigated by the influx of humanitarian post-disaster aid if the disaster takes place in a democracy. Apodaca (2017) observes an even more complex relationship: She shows that rapid-onset disasters tend to trigger protest and that governments, especially in anocracies, tend to react with repression to this protest, which, however, then triggers even more protest. Finally, Pfaff (2020), focusing on non-democracies, finds a repression-enhancing effect of rapid-onset disasters, which, however, depends on the existence of pre-existing grievances.

Whether these discrepancies between the studies are driven by different sample compositions, model specifications, or something else remains an open question. What is, however, more important and greatly motivates our study is that the theoretical pathways presented in Wood and Wright (2016), Apodaca (2017), and Pfaff (2020) as well as their empirical analysis treat repression as one category, something we argue should be differentiated. The reason being that without distinguishing between government-initiated and reactive repression, it is difficult to understand the rationale behind why governments step up repression in the aftermath of an extreme weather event. As we will argue below, different types of repression are designed to fulfill different functions, some of them more aimed at deterring dissent and opposition in the population others rather in response to existing unrest. While a plausible argument can be made

why extreme weather events should impact both, differentiating between government-initiated and reactive repression allows for a more precise test of the underlying mechanism. We, therefore, build on the important work by Wood and Wright (2016), Apodaca (2017), and Pfaff (2020) but believe their findings leave open the question of whether repression occurs because the government is in a vulnerable position and tries to divert from this with government-initiated repression, or because the occurrence of protest places an additional burden on the government and therefore triggers a repressive response.

In addition, this paper innovates on existing work by taking recent results in repression research, such as Christensen (2018), seriously who emphasize the importance of considering within-country variation in repression, especially so, if interested in different types of repression. Depending on the region of a country, the repressive tactics governments employ may differ greatly. Therefore, and in contrast to the hitherto typical country-year format, we investigate sub-national and temporally disaggregated data as doing so has the additional advantage of investigating the causal mechanism more precisely. Most disasters do not hit the entire territory of a country but are usually confined to sub-national regions or even smaller geographical areas. Thus, assuming that a disaster in one part of a country is causally linked to the use of repression in another part of the country, might not be entirely plausible. Furthermore, a correlation may appear between a disaster happening in one part of a country and acts of repression in another. Despite no causal connection, country-level comparisons could thus suggest an effect of disasters on repression while, in reality, there is none.

Our study, therefore, attempts to address these theoretical and empirical research gaps by investigating whether sudden-onset-related disasters, such as floods or storms, trigger different forms of government repression at the sub-national level. This approach allows us to, first, test whether such rapid-onset weather events influence repressive activities intended to serve

different purposes in different ways; second, ensure a more precise spatial overlap between disaster events and repression, and, third, model temporal dynamics more precisely.

Before turning to our theoretical argument and explaining in greater detail how we think rapid-onset extreme weather events might impact government-initiated as well as reactive governmental repression, we review the more general literature on repression. We do so because we believe this facilitates the understanding and situating of our arguments in the broader debates in contemporary repression research.

Repression

We start by briefly outlining the definition of repression as in this paper, we follow existing research by Davenport (2007) and define repression as “...*involving the actual or threatened use of physical sanctions against an individual or organization, within the territorial jurisdiction of the state, for the purpose of imposing a cost on the target as well as deterring specific activities and/or beliefs perceived to be challenging to government personnel, practices or institutions.* (Goldstein, 1978: xxx–xxx). Consequently, repression is not just a term against which to evaluate a political government but describes concrete actions a government uses to intimidate and control its population. Examples are illegal detentions, violation of personal security and personal integrity rights, protest policing, executions, politically-motivated disappearances, or torture (Davenport and Inman, 2012).

Arguably, the two most important findings of the broader literature on repression are the so-called “domestic democratic peace” and the “law of coercive responsiveness.” Starting with the domestic democratic peace, literature argues that democracies use particularly little repression. In a democratic political system, mechanisms for accommodating the demands of demonstrators are well-established and developed. Norms prevail that deem the repression of protests an inappropriate reaction. Consequently, the oppression of dissent in a democracy is

typically not needed to protect the government from being thrown out of office since citizens can rely on elections as a tool to hold governments accountable. In a democratic regime, suppressing protests, therefore, does not necessarily help the government stay longer in office. Rather to the contrary, it might increase the probability of being voted out of office in the next election. Following the domestic democratic peace argument, our study focuses on non-democratic countries as under such circumstances, an increase in repression after sudden-onset extreme events seems most likely.

The second highly established finding in the literature is the “law of coercive responsiveness,” which is the most important ingredient for our theoretical argument as to *why* autocratic governments react to extreme events with repression. It has become consensus in the literature that repression becomes especially likely when dissent in the population rises. Since a government assumably wants to stay in power, protest, unrest, or terrorism represents opposition to the ruling government and poses a threat to its survival. Yet, various studies show that not all protests are perceived as equally threatening and a government is more likely to respond with repression to some types of dissent than to others.

There is still debate, however, as to what characteristics and circumstances serve as the most important trigger. Regan and Henderson (2002), for instance, find that as the degree of organization and the number of participants grows, the likelihood of a repressive reaction increases. Moreover, the difference between peaceful and violent protests seems to play an important role, too, as Carey (2010) observes that violent behavior on part of the citizenry is more often answered with repression than peaceful demonstrations. Some studies, for instance Davenport (1995), further show that if protestors behave in ways that are outside their usual repertoire of actions, the government is less likely to tolerate and thus responds with more repression. Finally, as the perception of threat is crucial, several studies suggest that repression is more likely to happen if the regime needs to deal with several menaces at once (e.g., Hendrix

and Salehyan, 2017, 2019; Ritter, 2014). Examples of this include situations in which a government is involved in multiple conflicts at the same time and opposition comes from different societal groups concurrently.

To summarize the literature on repression, two main findings stand out. First, according to the “domestic democratic peace” it is mainly autocratic governments that rely on repression, which is why we focus in our theoretical argumentation and empirical investigation on this most likely context. And second, the “law of coercive responsiveness” suggests that repression becomes more likely with rising discontent in the population, a finding on which we will build the main gist of our theoretical argument presented in the following section.

WEATHER-RELATED RAPID-ONSET DISASTERS AND REPRESSION

When rapid-onset extreme weather events, like floods or storms, hit vulnerable societies, the consequences are most of the time disastrous. Poor infrastructure and low administrative capacities severely limit the ability of the respective governmental agencies to adequately prepare for and respond to such extreme weather events. Where means for hazard prevention and effective emergency response are absent, large-scale material destruction and loss of lives frequently accompany rapid-onset extreme weather events. This applies to many countries in the Global South, the set of countries we are interested in, which implies that extreme weather events turn particularly often into disasters there .

These circumstances pose major challenges to the state and citizens alike, especially because such events tend to happen very abruptly. Even if citizens are used to living in conditions with often only elementary public services and might not expect much from the state, we argue that in the aftermath of a rapid-onset weather event citizens will still expect their government to provide disaster relief to help people survive as well as first-aid supplies to cover their basic needs. And while many citizens living in countries with low administrative capacities may have

become used to a lack of governmental services in normal times, they should care about a lack of support in times of despair when even basic needs cannot be met. This does not mean that we expect citizens to hold their government accountable for the weather event as such; citizens should, however, very well become upset about a lack of disaster management and first-aid . And while many states indeed do react to such disasters by providing aid, studies have shown that many countries in the Global South suffer from an “adaptation deficit” where a lack of resources hinders appropriate response efforts (Fankhauser und McDermott 2014).¹ Therefore, we argue that insufficient or inexistent assistance on the government’s side to mitigate hardship after a disaster should create severe discontent in the population.

Correspondingly, we expect that when rapid-onset destructive weather events materialize in low-capacity societies, the oftentimes dire situation that tends to follow should create an unstable environment prone to unrest. On one hand, the pre-existing lack of state capacity, malfunctions, and inadequacies of the government become most visible during such circumstances. On the other hand, dissatisfaction is likely to increase in the disaster-hit population. Apart from having suffered hardship and losses because of the disaster, citizens can become frustrated with the government because of the lack of assistance (Carlin et al., 2014; Pelling and Dill, 2010; Poggione et al., 2012). Dynamics unfolding during the COVID-19 pandemic illustrate the kind of pressure disastrous events and perceived inadequacies in governmental disaster management can put on state-society-relations (Beeson, 2010; Wood et al., 2022).

Previous literature has shown that when (autocratic) governments perceive their power to be at risk , they become more likely to rely on repression to get a hold of the situation. In particular, at least two different pathways exist for how extreme weather events could lead to a rise in repression. On one hand, an autocratic government could rely on repression preemptively

¹ We, of course, control for the allocation of international disaster relief as this type of international aid often substitutes for the lack of domestic disaster response.

without any previous unrest among citizens, for instance, by repressing journalists or members of the opposition. On the other hand, autocratic governments may employ repression reactively as a response to protest and instability, for example, inappropriate protest policing through using excessive force. In the following, we discuss first how rapid-onset weather events may trigger repression directly through increasing incidents of *government-initiated repression*. Secondly, we explain how an increase in repression might happen indirectly by causing unrest in the population and triggering a *repressive reaction* by the state.

Direct Relationship: Government-initiated Repression as Preemption

As discussed above, rapid-onset weather events tend to cause severe destruction, often accompanied by tremendous physical harm, thereby leading to a deterioration in the living conditions of the affected citizenry. Under these circumstances, especially low-income countries tend to struggle with effective disaster management (Fankhauser and McDermott, 2014). Such an insufficient governmental response in dealing with the consequences of the disaster as well as the resulting reduced availability of important resources, such as access to drinking water or safe housing, should increase dissatisfaction with the government.

Although expectations towards the government may be low, to begin with, mismanagement of disasters demonstrates quite clearly what a government is unable or unwilling to do. When citizens' discontent with its performance reaches extraordinarily high levels, government-opposing activities could seem all the more tempting (Homer-Dixon, 2010). This expectation is in line with research showing that extreme weather events may indeed heighten the likelihood of social unrest. The government, therefore, is confronted with an increased risk of instability.

However, it might take some time until citizens' frustration vents into riots or protests against the government. First, in the immediate aftermath of destructive events, citizens will be

occupied with securing their survival and reconstruction efforts. Nonetheless, the period thereafter could be particularly prone to instability. Especially so as governments in low-income states face a double burden: disaster management, inadequate or not, is costly, which means that fewer resources would be left for pacifying potential unrest if it arose. Thus, rapid-onset weather events put governments in developing countries in a very vulnerable situation (Berrebi and Ostwald, 2013; Carlin et al., 2014; Poggione et al., 2012). Anticipating that actual unrest might be a threat to their survival in office, autocratic governments could consider intimidating citizens with repression as an option to circumvent the real threat from materializing in the first place. The underlying incentive, therefore, is to avoid unrest before it even arises.

The period shortly after a disaster could be an ideal “window of opportunity” to quell potential dissent as long as citizens have not regained the means to revolt. A government that believes its power is potentially at risk in the aftermath of an extreme weather event can find it useful to, for instance, suppress objective news coverage. However, such forms of repression are not necessarily confined to the direct aftermath of the event as examples of attacks against journalists reporting about the misuse of relief funds illustrate (CPJ 2000, 2006). Hence, we hypothesize that *rapid-onset weather events should increase the likelihood of incidents of government-initiated repression in autocracies (H1)*.

Indirect Relationship: Repression as Reaction to Unrest

Another mechanism of how rapid-onset weather events can increase repression is plausible as well. Apart from incentives to initiate repression against its citizens before they actually revolt, catastrophic storms and floods could make a government more inclined to repressively react to actual unrest in the population that materializes in the aftermath of the catastrophe. Based on the “law of coercive responsiveness”, which exposit that dissent makes repression more likely (Davenport, 2007), research has shown that governments are more induced to use repression if they feel threatened and overwhelmed, for instance, when having to deal with several issues at

once (Davenport, 1995; Ritter, 2014). We reason that societal unrest in the aftermath of destructive weather events should put the regime in such a position that requires the handling of several issues at once. Handling unrest and trying to cope with an extreme weather event are each demanding tasks in themselves. In low-capacity states, disaster management, even if it may fall short in many regards, will use up large parts of already few resources. Hence, having to deal with both an extreme weather event and a protest at the same time could simply overstrain capabilities. We argue that resorting to a repressive response may seem like a viable option whenever anti-government protests and chaotic post-disaster conditions coincide, especially so in an autocratic context.

Moreover, existing literature has shown that dissenting activities are especially likely to be met with repression if the regime regards the opponent as serious and difficult to accommodate. We argue that this should apply to unrest in the wake of rapid-onset weather events. First, governments should know well how desperate citizens who are victims of disastrous weather events are. Second, governments should also understand that protests will, therefore, probably not dissolve quickly unless the underlying concerns are met, which, as discussed, is unlikely to be quickly the case due to the above-mentioned capacity and resource constraints. As, however, the government cannot easily accommodate the grievances of the protesting citizenry, the one alternative option might be to respond to unrest after disastrous weather events with repression. This leads to our second hypothesis that *rapid-onset weather events should increase the likelihood of incidents of reactive repression in autocracies (H2)*.

RESEARCH DESIGN

To test our two hypotheses, we rely on a quantitative event analysis covering the period from 1990-2017. Our units of analysis are 1st order administrative areas based on the data provided by the Global administrative areas - project (GADM, 2022). Relying on a subnational analysis

allows us to carry out a spatially disaggregated analysis based on politically relevant areas that are at the same time large enough to account for meaningful spatially lagged independent and dependent variables and are immune to potential imprecisions in the geo-location of disaster and repression events. One of the advantages of administrative units in contrast to the rather apolitical grid cells is that they correspond to political entities. We argue that in a study in which governmental actions are such a central component, it is preferable to rely on a unit of analysis that follows political demarcation lines. To avoid measuring the temporal dynamics too roughly and grasp the precise timely sequences, we perform our analyses at the monthly level. Due to data availability for our dependent variables, as discussed in more detail below, we are only able to carry out analyses with admin1-units in Africa, Central America, and the Caribbean.

Following our theoretical argument, we limit our empirical analysis to non-democratic countries. Since our argument does not pertain to the level of democracy but to the absence of a minimum level of democracy, we rely on several different dichotomous indicators to select our countries of analysis. In particular, we rely on the following three indicators of democracy/autocracy respectively, which are all well established in the literature: Boix et al. (2013), Cheibub et al. (2010), and Geddes et al. (2014). We chose three different indicators in order to show that our results are not driven by the choice of the respective measure but hold independent of how we specify an absence of the minimum level of democracy, as postulated by the domestic democratic peace.

Data and Variables

To measure our two dependent variables, government-initiated and reactive repression, we rely on the Social Conflict Analysis Database (SCAD) (Salehyan et al., 2012). We consider the Social Conflict Analysis Database most suitable to test our theoretical argument since it is unique in its detailed account of different types of repression. It does not only provide data on

repression events in general but distinguishes between government-initiated and reactive repression.

SCAD captures information about government-initiated repression through a category in the variable “Etype” labeled “Pro-Government Violence (Repression).” We construct our dependent variable by creating a dichotomous variable taking on the value 1 for each 1st-level administrative area experiencing such a repression event in a given month. This variable captures incidents like illegal arrests, harassment, or attacks against political opponents and journalists that were *initiated* by the government. However, this variable does not collect information about protest policing or other immediate repressive responses to citizen mobilization. To measure this second type of repression and, therefore, situations where the government employed repression in reaction to anti-government protests and activities by dissidents, we make use of the variable named “Repress” in the SCAD database. This variable indicates for each anti-government event whether it was met with repression by the government, e.g., when the police used disproportionate force while handling protests. We, thus, create our second dependent variable by constructing a dichotomous variable taking on the value 1 whenever an anti-government event that was *repressed* by the government took place in a 1st-order administrative area in a given month. The correlation coefficient between the two forms of repression is only 0.01, indicating that they are indeed distinct event types.

As SCAD data is only available for Africa, Central America, and the Caribbean, data availability partially drives our case selection. However, we argue that in addition to data availability, the countries included in SCAD represent the most-likely cases for answering our research question. Since the scope conditions for our theoretical argument constitute a low level of economic development paired with low levels of administrative capacity, we consider the countries included in the Social Conflict Analysis Database most likely cases to test our

argument in that if we do not find any effects here it seems unlikely to find them using a different sample.

Our central independent variable are weather-related rapid-onset events, like storms, floods, or landslides. To measure if a rapid-onset event took place in a given admin1-area in a certain period of time, we use the dataset provided by the International Disaster Database/EM-DAT (CRED 2022). EM-DAT monitors extreme events worldwide. It includes an event if at least one of the following criteria is fulfilled: 1) ten or more persons reported to be killed, 2) one hundred or more persons reported to be injured, or 3) the declaration of a state of emergency or a call for international assistance. While the original dataset includes various different events, given our theoretical argument, we only include disasters that are weather-related *and* rapid-onset in nature which leaves us with storms, floods, and landslides. Relying on a refined version of the EM-DAT dataset, the GDIS dataset (Rosvold and Buhaug, 2021), which assigns the exact location of the events in the EM-DAT, we are able to match disasters to 1st-level administrative regions at a certain point in time. In order to accommodate that the proposed mechanism might take some time to materialize, we create a variable measuring whether an admin1-area has been exposed to a destructive rapid-onset weather event in at least *one out of the three* preceding months.

We include several control variables to account for important potential alternative explanations for repression, some of which are measured at the country level and others at the 1st-order administrative level. In particular, we rely on data measured at the administrative area level for political instability, population density, regional economic development, and ethnic discrimination, while we use country-level data for state capacity and humanitarian aid.

Controlling for **instability**, especially anti-government mobilization, seems important as existing literature tells us that unrest in the population might serve as a trigger for government repression . In our view, the SCAD dataset provides a good way to measure this. For all forms

of conflict accounted for in the dataset (e.g., demonstrations, riots, strikes, terrorist attacks, anti-government violence), two separate variables, “cgovtarget” and “rgovtarget,” indicate whether this event was directed against the central or regional government of a state. Similar to our dependent variable, we create a dichotomous variable taking on the value 1 for each administrative unit experiencing anti-government events in year t . The rationale behind this variable is that a volatile region might be more likely to be targeted by governmental repression independent of destructive extreme weather events. A government that has witnessed unrest should be more inclined to stage repressive attacks or respond with repression in regions where it perceives the potential for anti-regime-mobilization as high. We lag this variable by one month.

We further control for **population density** because repression tends to be higher in more densely populated regions. Not only is the probability of effective mobilization in such regions higher, but repression is also more visible (Christensen, 2018). To calculate population density, we draw on census-based data provided by Gridded Population of the World (CIESIN, 2022). Originally made available on a very fine-grained grid-cell level, we aggregated these estimates up to the 1st-order administrative unit. As CIESIN only published these datasets with five-year intervals, we used linear interpolation to avoid an exorbitant number of missing values.

Even though there is some debate about the exact mechanism, **economic wealth** seems to be related to repression. A number of studies find that repression is more prevalent in poorer contexts. However, many states are characterized by large regional disparities in terms of economic development. Several studies, e.g., Bickenbach et al. (2016) and Koren and Sarbahi (2018), have shown that **yearly average stable nightlight-emissions** perform well in capturing local poverty or rather prosperity. Considering that highly aggregated measurements of economic development, like national GDP/capita, would not account for such sub-national

variation in economic wealth/development, we use yearly mean nightlight-emission per administrative area instead.

Although we selected our sample to include only countries with low levels of **state capacity**, there still exist considerable variation between them. For repression to occur, a government must not only be willing but also able to exercise repression. In states with higher coercive capacity, the likelihood that repression will actually be used in a power-threatening situation should be higher. We rely on the work of Hendrix (2010) suggesting that military capacity could be used as a proxy for coercive state capacity and include military expenditures per capita in our analyses. We use the natural logarithm to acknowledge decreasing marginal utility of coercive state capacity.

Ethnic discrimination: The use of government repression is found to be sometimes specifically directed against ethnic groups unpopular with the government (Beiser-McGrath 2019). We reason that the occurrence of a repressive event should be more likely in regions inhabited by already politically excluded groups. The PRIO-GRID-project (Tollefsen et al., 2012) provides data on this based on data collected by Wucherpfennig et al. (2011) within the framework of the GeoEPR-project. We matched the PRIO-grid cells with admin1-units and constructed a dichotomous variable indicating whether at least one excluded ethnic group inhabits the respective administrative area.

Another important control variable is the allocation of multilateral **aid** to disaster-struck regions as this could mitigate grievances that occurred or were reinforced by a disastrous extreme weather event. We use the Aid Data 3-dataset (Tierney et al., 2011) and rely on aid-flows labeled as emergency response. While this captures reactions by the international community to rapid-onset weather catastrophes, it likewise records financial assistance issued in response to epidemics and conflict. This means that it does not measure exclusively disaster aid but emergency aid more generally. In doing so, it encompasses material relief assistance,

emergency food assistance, relief coordination, reconstruction relief and rehabilitation, immediate post-emergency reconstruction, and rehabilitation. The data is in constant US dollars. Table A.1 in the Appendix provides a descriptive overview of all variables.

EMPIRICAL FINDINGS

Due to our dependent variable being dichotomous and the special structure of our data, which consists of 1st order administrative units nested in countries over a period of 27 years, we use logistic regression models with fixed effects at either the country or the administrative unit level. To account for potential endogeneity concerns, in that regions with and without disasters might differ systematically, we pre-processed the sample by performing coarsened exact matching (=CEM). A detailed description of the balance statistics before and after matching can be found in Table A.2 in the Appendix. Furthermore, we account for potential time effects by including the time sequence since the last repression event and their squared and cubed version as control variables (Carter and Signorino, 2010) and add year fixed effects to all our models.

Table 1 displays the results of the first set of regression models, in which the measure for autocracies is based on the definition by Boix et al. (2013). Models 1 and 2 test hypothesis 1 and, therefore, the effect of rapid-onset events on government-initiated repression, whereas Models 3 and 4 test hypothesis 2, focusing on reactive repression. The comparison between the two types of repression shows that it is indeed important to distinguish between them: While we observe a significant repression-triggering effect for government-initiated attacks, lending support to hypothesis 1, this is not the case for incidents of reactive repression. Irrespective of whether we include country-fixed effects (model 1 & model 3) or admin1-fixed effects (model 2 & model 4), the findings in Table 1 suggest that rapid-onset weather events indeed increase

the likelihood of more government-initiated repression but do not increase the likelihood that anti-government mobilization is met with repression.

Table 1: Logistic regression results for the association between weather-related rapid-onset disaster occurrence and governmental repression, sample composed of non-democracies according to the definition of Boix et al. (2013) and pre-processed with CEM

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
	M1	M2	M3	M4
Weather-related rapid-onset disaster (t-1-3)	0.431** (0.193)	0.429** (0.197)	-0.0435 (0.123)	-0.105 (0.124)
Anti-government event (t-1)	1.438*** (0.125)	0.992*** (0.128)	0.981*** (0.0741)	0.784*** (0.0748)
Log (Population density)	0.158*** (0.0558)	-0.486 (0.506)	0.271*** (0.0292)	-0.397 (0.277)
Nightlights	0.0178** (0.00775)	-0.00623 (0.0303)	0.0114*** (0.00371)	0.0266* (0.0158)
Log (Military expenditures/capita)	-0.0432 (0.0804)	-0.0686 (0.0820)	-0.115** (0.0531)	-0.193*** (0.0542)
Presence excluded ethnic group	0.0747 (0.138)	0.160 (0.252)	0.419*** (0.0721)	0.535*** (0.152)
Humanitarian emergency aid	1.23e-09 (1.35e-09)	7.77e-10 (1.46e-09)	-9.66e-10 (5.90e-10)	-1.45e-09** (6.21e-10)
Constant	-4.823*** (0.608)	-3.564 (3.076)	-4.576*** (0.351)	-4.029 (2.735)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	140,474	27,514	150,608	58,614
Log likelihood	-2232.2653	-1862.4375	-6478.3723	-5678.1256
AIC	4606.531	4076.875	13114.74	11996.25
BIC	5306.078	5524.026	13898.62	14869.44
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

To better illustrate our results, we also calculated predicted probabilities for the occurrence of government-initiated repression under different circumstances, see Table 2. The results show that in conditions where repression is most likely to occur – these are densely populated areas with high levels of nightlights emissions and in which, among others, an anti-government event had occurred in the past month – the exposure to a weather-related rapid-onset disaster within the past three months increases the likelihood of government-initiated repression by an additional 5 percentage points, from 13% to 18%.

Table 2: Probability of government-initiated repression based on M1

	Weather-related rapid-onset disaster (t-1-3) = 0	Weather-related rapid-onset disaster (t-1-3) = 1
Least likely setting for repression: Anti-government event (t-1) = 0 Log (Population density) = 0.227 Nightlights = 0 Time since last government-initiated repression event = 1 Time squared = 1 Time cubed = 1 All other variables at their mean values	0.002	0.004
Most likely setting for repression Anti-government event (t-1) = 1 Log (Population density) = 11.874 Nightlights = 62.35 Time since last government-initiated repression event = 1 Time squared = 1 Time cubed = 1 All other variables at their mean values	0.13	0.18

To assess whether our results might be an artifact of how we measure autocracy, we also conduct our analysis with a sample of observations based on the classification scheme by Cheibub et al. (2010). It is noteworthy that this restriction results in a stark decrease in observations due to data availability. Appendix A.8 includes the descriptive statistics for this considerably smaller sample. Again, the sample was pre-processed using CEM to improve the balance between treated and untreated observations (please see Appendix A.9).

Models 5-8 in Table 3 display the corresponding results. In line with the results of the first set of regressions, we find that extreme weather events significantly increase the likelihood of government-initiated repressive actions in non-democratic countries. The influence of disastrous weather events on reactive governmental repression, however, is again not existent and, therefore, does not provide any support for hypothesis 2. The only interesting disparity is that in model 8, in contrast to our theoretical expectations, the occurrence of a weather-related rapid-onset disaster significantly *decreases* the likelihood of reactive repression.

Table 3: Logistic regression results for the association between weather-related rapid-onset disaster occurrence and governmental repression, sample composed of non-democracies according to the definition of Cheibub et al. (2010) and pre-processed with CEM

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
	M5	M6	M7	M8
Weather-related rapid-onset disaster (t-1-3)	0.865*** (0.252)	0.734*** (0.257)	-0.211 (0.162)	-0.282* (0.163)
Anti-government event (t-1)	0.599*** (0.227)	0.215 (0.226)	0.871*** (0.101)	0.632*** (0.100)
Log (Population density)	0.263*** (0.0822)	0.780 (0.944)	0.312*** (0.0395)	0.0936 (0.425)
Nightlights	0.0264** (0.0113)	-0.0308 (0.0968)	0.00834* (0.00492)	0.0111 (0.0295)
Log (Military expenditures/capita)	-0.199** (0.0897)	-0.229** (0.103)	0.0653 (0.0761)	0.00919 (0.0811)
Presence excluded ethnic group	0.246 (0.228)	0.912** (0.419)	0.394*** (0.104)	0.815*** (0.253)
Humanitarian emergency aid	2.25e-09 (1.54e-09)	1.96e-09 (1.76e-09)	-1.20e-09 (7.88e-10)	-1.11e-09 (7.87e-10)
Constant	-6.958*** (1.038)	-14.10* (7.949)	-6.486*** (0.538)	-7.100*** (2.458)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	100,380	14,616	116,694	31,054
Log likelihood	-1233.354	-966.1461	-4047.4348	-3440.4804
AIC	2584.708	2168.292	8234.87	7330.961
BIC	3146.194	3063.897	8911.581	9208.244
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

As an additional robustness check, we rely on a third autocracy measure based on the definition by Geddes et al. (2014). Using this classification resulted in a sample size that is in between models 1-4, which are based on the operationalization by Boix et al. (2013), and models 5-8, which are based on the measure by Cheibub et al. (2010). Tables A.11 and A.12 in the Appendix show the descriptives and balance statistics for the additional robustness check. The results, as displayed in Table A.10 in the Appendix, show the same picture as before: We find a significant positive effect of rapid-onset weather events on repressive attacks initiated by the government and, thus, clear support for hypothesis 1. However, we do not find a significant effect of such

rapid-onset events on reactive repression. However, irrespective of the autocracy definition we use, none of the effects for reactive repression supports hypothesis 2. We can think of two potential reasons why we do not find support for the reactive pathway. For one, autocratic governments might be so “successful” using the preemptive pathway that there is little room for the reactive pathway to materialize. For another, since our sample only consists of autocracies, the likelihood of people protesting in the first place might be rather low, thus, making the reactive pathway less likely to occur.

Robustness Checks

To probe the robustness of our results, we conducted several further robustness checks. In accordance with the “law of coercive responsiveness,” we know that when a government has been facing challenges against its authority lately (measured in our analysis by the variable “anti-government event”), its inclination to draw on repressive measures rises. Since contentious behavior is one of the strongest predictors of repression, we want to make sure that our main findings remain robust once we change the way we model this factor. In all our models so far, we measure anti-government mobilization the month prior to governmental repression to make sure the contentious activity precedes the employment of repression. Appendix A.3 and Appendix A.4 include models with alternative specifications of controlling for anti-government mobilization. In Appendix A3, we measure anti-government mobilization in the same month as governmental repression. The resulting findings are very similar: the effect on government-initiated repression remains robust and the non-effect for reactive repression remains the same.

As can be seen from Table A.3, anti-government mobilization gets omitted from the models with reactive repression as the dependent variable since the two are collinear (there cannot be reactive repression if there is no anti-government mobilization taking place). As an additional

sensitivity analysis, we, therefore, re-estimate all models without controlling for anti-government mobilization, see Appendix A.4. These models again yield support for hypothesis 1, further underlining the robustness of these results.

Moreover, we re-ran models 1 to 4 on an unmatched sample (see Appendix A.5). The overall picture remains the same. Again, we observe a significant repression-enhancing effect of rapid-onset weather events on government-initiated repression, whereas the results do not corroborate the theoretical mechanism outlined in hypothesis 2 as we do never observe a significant relationship between disaster exposure and the use of reactive repression. Furthermore, Appendix A.6 includes an analysis of reactive repression events on a sample only containing observations that had experienced an anti-government event. Also, in this model no significant reactive repression triggering effect of weather-related rapid-onset disaster becomes apparent.

This main finding – a repression-enhancing effect of rapid-onset disasters on government-initiated, preemptive repression and a non-effect on reactive repression withstands further changes in model specification: when estimating conditional logistic regression models (see Appendix A.7) or multilevel logistic regressions (see Appendix A.8).

CONCLUSION

This paper set out to better understand the effect of rapid-onset climate disasters on the use of governmental repression in autocracies. While a vibrant strand of the literature studies the impact of extreme weather events on all kinds of societal conflicts, the potential link to government repression has remained relatively unaddressed. We argue that such a link is highly plausible since especially severe disasters that set in suddenly could influence a government's decision to use repression. Weather-related rapid-onset events, like storms and floods, can cause major disruption and upheaval in vulnerable societies, especially in autocracies in the Global

South on which this analysis focuses. These extreme events often leave behind an aggrieved population that needs help and assistance but is faced with a government that is often unable to accommodate these grievances. Building on previous scholarship indicating that situations in which a government feels threatened tend to increase the likelihood of repression, the question arises whether this also is the case in the aftermath of destructive storms and floods.

We argue in this paper that there exist at least two pathways as to how repression can materialize in the aftermath of the occurrence of severe weather events: On one hand, an autocratic government could increase repression in the aftermath of a disaster as a precaution against unrest in the population. We argue that the period after a rapid-onset weather event represents a “window of opportunity” during which a government could increase repression preemptively against unrest in the population. On the other hand, an autocratic government might be overwhelmed if anti-government mobilization arises shortly after a disaster takes place and thus respond to unrest in the population with reactive repression.

We test this argument by relying on the SCAD event data for Africa, Central America, and the Caribbean for the years 1990-2017. Since governments are unlikely to rely on repression to the same extent across the entire country, we expand on previous studies by using a higher level of spatial disaggregation. In our analyses, we examine whether the occurrence of rapid-onset weather events in a specific 1st order administrative area increases the likelihood that a government targets the local population with repression. Furthermore, we disaggregate temporally by modeling monthly repression dynamics.

The results based on a sample pre-processed with coarsened exact matching and using logistic regression models with fixed effects show a significant positive effect of weather-related rapid-onset events on the likelihood of government-initiated repressive events. An illustration of this effect would be incidents in association with a flood catastrophe in Somalia when police officers assaulted journalists who compiled information on flood damages in a Mogadishu

neighborhood in 2022 (NUSOJ, 2022). In contrast, we do not find evidence in line with the second pathway, namely that the effect of rapid-onset disasters on government repression should also unfold with respect to reactive forms of repression. These disparate findings underline, in our view, how important it is to distinguish between different pathways toward repression. Our findings suggest that the rationale of autocratic governments for why we often observe an increase in repression in non-democracies after destructive disasters is that governments seem to try to silence potential dissent mobilization before it even arises. The findings of our study, therefore, provide a better understanding of the way non-democratic governments might try to tighten their grip on the population in such periods.

Still, we think this finding is a reason for concern. Due to climate change, extreme weather events are expected to happen more frequently and become more severe already in the short to medium term. If certain governments resort to repression in precaution against unrest in the population, this not only has immediate negative consequences for the affected population but might also further decrease the chances of democratization (Davenport 2007).

Furthermore, there exist numerous additional types of repression, like censorship, digital repression in the form of internet shutdowns, etc., which we have not been able to include in this study. We think future research could investigate in detail whether our findings also apply to such forms of governmental repression. Moreover, future research could explore whether our findings hold for different geographical regions as well. Finally, scholars could trace in more detail whether this repression-enhancing effect is unique to weather events that set in suddenly or whether such dynamics also unfold during slow-onset disasters like droughts.

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APPENDIX

Table A.1: Descriptive statistics, sample composed of non-democracies according to the definition of Boix et al. (2013)

DV:	Obs.	Min	Max	Mean	Std. Dev.
Government initiated repression event (dichotomous variable)	209940	0	1	0.004	0.063
Reactive repression event (dichotomous variable)	209940	0	1	0.010	0.098
IV:					
Weather-related rapid-onset disaster (dichotomous variable)	209940	0	1	0.011	0.010
Weather-related rapid-onset disaster Cumulative lag, 1-3 months (dichotomous variable)	209940	0	1	0.028	0.166
Controls:					
Anti-government event (dichotomous variable)	209940	0	1	0.023	0.149
Log (Population density)	209940	0.227	11.874	6.779	1.889
Nightlights	191460	0	62.35	3.108	7.765
Log (Military expenditures/capita)	186156	2.808	1.342	-3.426	7.432
Presence of excluded ethnic group(s) (recoded to dichotomous variable)	209940	0.516	0.50	0	1
Humanitarian emergency aid	184572	2.62	7.76	0	7.58

Table A.2: CEM-Matching statistics, sample composed of non-democracies according to the definition of Boix et al. (2013)

Imbalance pre-matching							
Multivariate L1 distance:	0.35690284						
Univariate imbalance:	L1	mean	min	25%	50%	75%	max
Log (Population density)	0.14663	0.37569	0.84828	0.44088	0.26898	0.26962	0
Nightlights	0.09051	0.17733	0	0.02991	0.14523	0.09869	-1.4737
Presence excluded ethnic group	0.04123	-0.04123	0	0	-1	0	0
Imbalance post-matching							
Multivariate L1 distance:	0.3372608						
Univariate imbalance:	L1	mean	min	25%	50%	75%	max
Log (Population density)	0.09982	-0.00193	0.19542	-0.00779	0.00122	0.03736	0
Nightlights	0.07652	0.05484	0	0.02515	0.12542	-0.00464	.
Presence excluded ethnic group	1.5e-13	-2.5e-14	0	0	0	0	0

	0	1
All	207891	2049
Matched	197487	2049
Unmatched	10404	0

Table A.3: Models 1-4, controlling for anti-government events in the same month instead of one month prior to the repression event

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
Weather-related rapid-onset disaster (t-1-3)	0.443** (0.193)	0.427** (0.198)	-0.0253 (0.156)	-0.0418 (0.174)
Anti-government event (t-1)	1.804*** (0.122)	1.344*** (0.124)	omitted	omitted
Log (Population density)	0.145*** (0.0559)	-0.448 (0.508)	0.126*** (0.0355)	0.422 (0.439)
Nightlights	0.0166** (0.00781)	-0.0101 (0.0306)	0.00169 (0.00540)	0.0251 (0.0228)
Log (Military expenditures/capita)	-0.0407 (0.0802)	-0.0629 (0.0820)	-0.153** (0.0654)	-0.183** (0.0766)
Presence excluded ethnic group	0.0497 (0.138)	0.0851 (0.254)	0.344*** (0.0974)	0.145 (0.211)
Humanitarian emergency aid	1.31e-09 (1.35e-09)	6.19e-10 (1.47e-09)	-2.00e-09** (7.85e-10)	-2.24e-09** (9.73e-10)
Constant	-4.856*** (0.606)	-3.826 (3.087)	-0.123 (0.406)	-6.004 (4.371)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	140,484	27,516	2,974	2,575
Log likelihood	-2193.4077	-	-2306.382	-1933.2241
AIC	4528.815	1837.987	4768.764	4330.448
BIC	5228.368	5475.138	5236.582	5688.485
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Table A.4: Models 1-4 without anti-government event as control variable

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
Weather-related rapid-onset disaster (t-1-3)	0.437** (0.193)	0.428** (0.195)	-0.0376 (0.122)	-0.114 (0.124)
Log (Population density)	0.201*** (0.0561)	-0.611 (0.503)	0.297*** (0.0293)	-0.476* (0.274)
Nightlights	0.0210*** (0.00762)	0.0108 (0.0299)	0.0115*** (0.00367)	0.0324** (0.0159)
Log (Military expenditures/capita)	-0.0534 (0.0812)	-0.0849 (0.0825)	-0.123** (0.0525)	-0.200*** (0.0542)
Presence excluded ethnic group	0.180 (0.138)	0.298 (0.248)	0.470*** (0.0721)	0.615*** (0.151)
Humanitarian emergency aid	8.23e-10 (1.38e-09)	4.41e-10 (1.49e-09)	-1.04e-09* (5.98e-10)	-1.46e-09** (6.25e-10)
Constant	-4.672*** (0.619)	-2.332 (3.053)	-4.409*** (0.352)	-3.147 (2.710)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	140,484	27,516	150,624	58,620
Log likelihood	-2291.3485	-1892.2334	-6560.5758	-5730.5311
AIC	4722.697	4134.467	13277.15	12099.06
BIC	5412.396	5573.408	14051.11	14963.31
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Table A.5: Models 1-4, sample according to Boix et al (2013) NOT pre-processed with CEM

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
Weather-related rapid-onset disaster (t-1-3)	0.425*	0.403*	0.0649	-0.0687
	(0.237)	(0.240)	(0.148)	(0.151)
Anti-government event (t-1)	1.164***	0.735***	0.994***	0.803***
	(0.142)	(0.143)	(0.0797)	(0.0800)
Log (Population density)	0.161***	0.00216	0.216***	-0.331*
	(0.0503)	(0.344)	(0.0263)	(0.188)
Nightlights	0.0174**	0.00806	0.0141***	0.0405***
	(0.00749)	(0.0275)	(0.00365)	(0.0146)
Log (Military expenditures/capita)	-0.0226	-0.0172	-0.0484	-0.110*
	(0.0899)	(0.0940)	(0.0603)	(0.0615)
Presence excluded ethnic group	0.191	0.258	0.371***	0.517***
	(0.135)	(0.256)	(0.0728)	(0.155)
Humanitarian emergency aid	7.69e-10	2.45e-10	-7.46e-10	-1.04e-09*
	(1.33e-09)	(1.43e-09)	(5.81e-10)	(6.07e-10)
Constant	-4.721***	-6.220***	-4.308***	-4.778**
	(0.597)	(2.275)	(0.356)	(2.021)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	143,510	30,286	153,992	61,590
Log likelihood	-2020.3389	-	-5850.9536	-
		1671.1689		5076.0603
AIC	4182.678	3706.338	11859	7980.12
BIC	4883.743	5220.294	12645.54	13714.25
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Table A.6: Model 3, sample to Boix et al (2013) ONLY including observations experiencing anti-government events

	Dependent variable: Reactive repression event
	Sample pre-processed with CEM
Weather-related rapid-onset disaster (t-1-3)	-0.0203 (0.156)
Log (Population density)	0.153*** (0.0361)
Nightlights	-0.00122 (0.00546)
Log (Military expenditures/capita)	-0.164** (0.0656)
Presence excluded ethnic group	0.365*** (0.0999)
Humanitarian emergency aid	-1.91e-09** (7.81e-10)
Constant	-0.0238 (0.418)
Time since last reactive repression event & polynomials	Yes
Year FE	Yes
Country FE	Yes
Observations	2,974
Log likelihood	-2297.5036
AIC	4751.007
BIC	5218.825
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Conditional Logit Models 1-4, sample according to Boix et al (2013) NOT pre-processed with CEM

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
Weather-related rapid-onset disaster (t-1-3)	0.423*	0.397*	0.0650	-0.0681
	(0.236)	(0.238)	(0.148)	(0.150)
Anti-government event (t-1)	1.161***	0.725***	0.992***	0.795***
	(0.142)	(0.142)	(0.0796)	(0.0795)
Log (Population density)	0.160***	0.00229	0.216***	-0.330*
	(0.0503)	(0.343)	(0.0262)	(0.187)
Nightlights	0.0173**	0.00802	0.0141***	0.0403***
	(0.00748)	(0.0274)	(0.00365)	(0.0146)
Log (Military expenditures/capita)	-0.0224	-0.0165	-0.0482	-0.109*
	(0.0899)	(0.0937)	(0.0603)	(0.0613)
Presence excluded ethnic group	0.190	0.257	0.371***	0.514***
	(0.134)	(0.255)	(0.0728)	(0.155)
Humanitarian emergency aid	7.71e-10	2.38e-10	-7.45e-10	-1.03e-09*
	(1.33e-09)	(1.42e-09)	(5.81e-10)	(6.05e-10)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	143,510	30,286	153,992	61,590
Log likelihood	-1951.5007	-	-5742.3045	-4668.976
AIC	3965.001	1485.9755	11546.61	9399.952
BIC	4271.1	3033.951	11854.89	9679.828
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Table A.8: Multilevel Models 1-4, sample according to Boix et al (2013) NOT pre-processed with CEM

	Dependent variable:	
	Government-initiated repression event	Reactive repression event
Weather-related rapid-onset disaster (t-1-3)	0.457* (0.237)	-0.0297 (0.150)
Anti-government event (t-1)	0.974*** (0.152)	0.834*** (0.0808)
Log (Population density)	0.188*** (0.0604)	0.214*** (0.0476)
Nightlights	0.0208** (0.00989)	0.0369*** (0.00833)
Log (Military expenditures/capita)	-0.0456 (0.0760)	-0.0626 (0.0525)
Presence excluded ethnic group	0.188 (0.158)	0.480*** (0.115)
Humanitarian emergency aid	8.92e-10 (1.12e-09)	-6.99e-10 (5.64e-10)
Constant	-5.762*** (0.656)	-6.078*** (0.455)
Var (Country)	0.763*** (0.267)	0.410*** (0.139)
Var (Admin1)	0.592*** (0.228)	1.359*** (0.235)
Time since last government-initiated repression event & polynomials	Yes	No
Time since last reactive repression event & polynomials	No	Yes
Year FE	Yes	Yes
Observations	153,992	153,992
Log likelihood	-2068.1989	-5792.3876
AIC	4204.398	11652.78
BIC	4542.516	11990.89
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1	

Table A.9: Descriptive statistics, sample composed of non-democracies according to the definition of Cheibub et al. (2010)

DV:	Obs.	Min	Max	Mean	Std. Dev.
Government initiated repression event (dichotomous variable)	149160	0	1	0.002	0.042
Reactive repression event (dichotomous variable)	149160	0	1	0.006	0.081
IV:					
Weather-related rapid-onset disaster (dichotomous variable)	149160	0	1	0.008	0.092
Weather-related rapid-onset disaster Cumulative lag, 1-3 months (dichotomous variable)	149160	0	1	0.024	0.154
Controls:					
Anti-government event (dichotomous variable)	149160	0	1	0.016	0.127
Log (Population density)	149160	0.227	11.83	6.63	1.887
Nightlights	130308	0	57.328	2.616	7.051
Log (Military expenditures/capita)	140196	-3.426	6.436	2.771	1.317
Presence of excluded ethnic group(s) (recoded to dichotomous variable)	149160	0	1	0.535	0.499
Humanitarian emergency aid	149160	0	7.58	2.25	7.27

Table A.10: CEM-Matching statistics, sample composed of non-democracies according to the definition of Cheibub et al. (2010)

Imbalance pre-matching							
Multivariate L1 distance:	0.35856988						
Univariate imbalance:	L1	mean	min	25%	50%	75%	max
Log (Population density)	0.15044	0.43033	0.87466	0.54427	0.31629	0.26943	-0.02295
Nightlights	0.1235	0.13498	0	0.01913	0.13111	0.50293	-3.7359
Presence excluded ethnic group	0.0404	-0.0404	0	0	-1	0	0
Imbalance post-matching							
Multivariate L1 distance:	0.34798144						
Univariate imbalance:	L1	mean	min	25%	50%	75%	max
Log (Population density)	0.11565	0.00708	0.2218	-0.00842	0.01167	0.00225	0
Nightlights	0.11357	0.1145	0	0.01691	0.122	0.29056	.
Presence excluded ethnic group	1.3e-13	2.2e-13	0	0	0	0	0

	0	1
All	147897	1263
Matched	138453	1263
Unmatched	9444	0

Table A.11: Logistic regression results for the association between weather-related rapid-onset disaster occurrence and governmental repression, sample composed of non-democracies according to the definition of Geddes et al. (2014) and pre-processed with CEM

	Dependent variable:			
	Government-initiated repression event		Reactive repression event	
Weather-related rapid-onset disaster (t-1-3)	0.884*** (0.228)	0.948*** (0.230)	-0.120 (0.145)	-0.153 (0.146)
Anti-government event (t-1)	0.736*** (0.208)	0.399* (0.204)	0.891*** (0.0957)	0.708*** (0.0942)
Log (Population density)	0.313*** (0.0762)	0.409 (0.870)	0.316*** (0.0355)	0.0169 (0.409)
Nightlights	0.0276*** (0.0100)	0.0776 (0.0664)	0.0103** (0.00448)	-0.000773 (0.0251)
Log (Military expenditures/capita)	-0.0665 (0.107)	-0.200* (0.110)	-0.0250 (0.0687)	-0.0631 (0.0743)
Presence excluded ethnic group	0.509** (0.202)	1.395*** (0.526)	0.460*** (0.0965)	0.284 (0.247)
Humanitarian emergency aid	5.74e-10 (1.55e-09)	1.49e-09 (1.65e-09)	-1.22e-10 (5.76e-10)	-6.63e-10 (6.16e-10)
Constant	-7.445*** (0.921)	-10.93 (7.357)	-5.759*** (0.466)	-4.647* (2.385)
Time since last government-initiated repression event & polynomials	Yes	Yes	No	No
Time since last reactive repression event & polynomials	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country FE	Yes	No	Yes	No
Admin1 FE	No	Yes	No	Yes
Observations	115,236	16,860	130,830	36,730
Log likelihood	-1389.2302	-1103.9743	-4560.3232	-3889.5037
AIC	2906.46	2467.949	9268.646	8273.007
BIC	3524.364	3473.2	9992.489	10375.31
	Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Table A.12: Descriptive statistics, sample composed of non-democracies according to the definition of Geddes et al. (2014)

DV:	Obs.	Min	Max	Mean	Std. Dev.
Government initiated repression event (dichotomous variable)	164940	0	1	0.002	0.041
Reactive repression event (dichotomous variable)	164940	0	1	0.006	0.079
IV:					
Weather-related rapid-onset disaster (dichotomous variable)	164940	0	1	0.009	0.093
Weather-related rapid-onset disaster Cumulative lag, 1-3 months (dichotomous variable)	164940	0	1	0.026	0.158
Controls:					
Anti-government event (dichotomous variable)	164940	0	1	0.015	0.123
Log (Population density)	164940	0.227	11.874	6.667	1.9
Nightlights	144984	0	57.328	2.719	7.261
Log (Military expenditures/capita)	155340	-3.426	7.432	0.549	0.498
Presence of excluded ethnic group(s) (recoded to dichotomous variable)	164940	0	1	0.549	0.498
Humanitarian emergency aid	164940	0	7.58	2.54	7.89

Table A.13: CEM-Matching statistics, sample composed of non-democracies according to the definition of Geddes et al. (2014)

Imbalance pre-matching							
Multivariate L1 distance:	0.33113693						
Univariate imbalance:	L1	mean	min	25%	50%	75%	max
Log (Population density)	0.11797	0.31445	0.84828	0.41472	0.2114	0.04891	-0.06733
Nightlights	0.10173	0.21564	0	0.01794	0.08218	0.46189	-3.216
Presence excluded ethnic group	0.00919	-0.00919	0	0	0	0	0
Imbalance post-matching							
Multivariate L1 distance:							
Univariate imbalance:	L1	mean	min	25%	50%	75%	max
Log (Population density)	0.09405	-0.00254	0.19542	-0.0024	0.01398	-0.00833	0
Nightlights	0.09772	0.08109	0	0.01529	0.0749	0.25077	.
Presence excluded ethnic group	6.4e-14	-3.0e-14	0	0	0	0	0

	0	1
All	163490	1450
Matched	153530	1450
Unmatched	9960	0