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Don't blame the weather! Climate-related natural disasters and civil conflict

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Abstract

The issue of climate change and security has received much attention in recent years. Still, the results from research on this topic are mixed and the academic community appears to be far from a consensus on how climate change is likely to affect stability and conflict risk in affected countries. This study focuses on how climate-related natural disasters such as storms, floods, and droughts have affected the risk of civil war in the past. The frequency of such disasters has risen sharply over the last decades, and the increase is expected to continue due to both climate change and demographic changes. Using multivariate methods, this study employs a global sample covering 1950 to the present in order to test whether adding climate-related natural disasters to a well-specified model on civil conflict can increase its explanatory power. The results indicate that this is the case, but that the relation is opposite to common perceptions: Countries that are affected by climate-related natural disasters face a lower risk of civil war. One worrying facet of the claims that environmental factors cause conflict is that they may contribute to directing attention away from more important conflict-promoting factors, such as poor governance and poverty. There is a serious risk of misguided policy to prevent civil conflict if the assumption that disasters have a significant effect on war is allowed to overshadow more important causes.

Keywords

armed conflict, climate change, natural disasters

Introduction

The academic, policy, and popular discussions that surround the issue of climate change predict changing weather patterns to increase natural disasters. Hurricane Katrina, which hit the city of New Orleans in 2005, has given much impetus to this discussion. Tropical cyclones, floods, heat waves (even cold spells), and droughts are apparently likely to be more destructive and carry higher consequences for humans in the future because rising temperatures (global warming) is expected to increase the frequency and intensity of these events (Aalst, 2006; IPCC, 2007a,b). Many even expect these disasters to increase the risk of violent conflict, which would create double burdens to states and societies trying to cope and adjust to climate change. In this article, I investigate whether there is a systematic tendency that climate-related natural disasters[1] cause civil conflict to arise, or re-ignite, within the same or the following year.

In order to assess whether natural disasters have any real effect on the risk of armed conflict, an existing, well-tested model should be used as a starting point. Only if natural disasters can add explanatory power to such a model might one be able to be confident in the proposition that disasters also increase the risk of armed conflict. I will use one candidate for such a model, developed by Fearon & Laitin (2003), to test how disasters affect the risk of civil war onset, when controlling for the most important other relevant factors. Dixon (2009)

[1] Unless otherwise specified, I use the term 'disaster' to refer to climate-related natural disasters (storms, droughts, floods, landslides, extreme temperatures, and wildfires).

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finds that the model used by Fearon & Laitin (2003) has fared very well, as the academic community has achieved some level of consensus on the role of all the variables they found to be significant. Hence, the scope of this study is limited to violent security issues (Goldstone, 2001), which may possibly be a consequence of the non-violent security challenges brought about by natural disasters. By studying how climate-related natural disasters have affected the likelihood of civil war in the past, this article aims to increase knowledge about to what extent future disasters, possibly exacerbated by the effects of climate change, can be expected to affect the likelihood of outbreaks of civil war. Thereby, this study may also contribute to informing governments about response strategies for managing the potential double impacts of natural disasters.

What is a natural disaster?

A climate-related natural disaster occurs when a natural hazard affects a vulnerable population so forcefully that it causes substantial death and/or damage. 'Natural disaster' can thus be broken down into three components: hazard, exposure, and vulnerability. The hazard is a natural phenomenon, such as strong wind, heavy rainfall, or extreme temperatures, while exposure and vulnerability are mainly human. Exposure refers to whether settlements are placed in locations that are exposed to natural hazards, while vulnerability is to what extent people and settlements are physically and institutionally prepared to withstand the damaging effects of natural hazards. Hence, apart from the very strongest hazards which always can be expected to cause severe damage, where and when natural disasters occur is highly contingent on human preparations (Birkmann, 2006; Wisner et al., 2004). For example, a sturdy, storm-proof building may escape damage from cyclones that would devastate fragile slum-dwellings. Similarly, constructing dikes along flood-prone rivers and avoiding construction of houses in areas that are at risk from becoming flooded can contribute to reducing the risk of disastrous floods.

The examples above also illustrate that natural disasters mainly affect poor people who cannot afford storm-proof buildings, who have no choice but to build their homes in the most exposed areas, and who subsist by what they are able to grow themselves. These problems are mainly related to poverty and, often, poor governance, not nature. Failure to adapt to and anticipate potential natural hazards will result in disaster sooner or later. At times, environmental degradation, such as deforestation, even moves societies in the opposite direction, making them more vulnerable. Similarly, uncontrolled urbanization may cause exposed, risky areas (such as steep hills and low-lying, flood-prone areas) to become densely populated – a recipe for disaster in the long run.

Even if weather conditions do not change, the number of climate-related disasters will increase if human habitations are placed in exposed areas without sufficient protection. Climate change projections indicate that many areas will face increasing exposure from climate-related hazards in the future. This, combined with current trends in urbanization, indicates that the significance of this issue is likely to increase. Without improving the resilience of affected societies substantially, an increased number of disasters will follow. This is highly unsettling in itself, but if claims that natural disasters increase the risk of conflict are correct, there is far more to worry about.

Why do disasters matter for conflict?

The debate on security implications of climate change has contributed to increasing the attention to how environmental factors may affect the risk of armed conflict. Environmental shocks generate insecurity, frustration, scarcity of important resources, and weakened enforcement of law and order, which are frequently suggested to increase the likelihood of outbreaks of armed violence (Brancati, 2007; Burke et al., 2009; Homer-Dixon, 1999; Miguel, Satyanath & Sergenti, 2004; Nel & Righarts, 2008). While no one debates that severe environmental shocks cause suffering and destruction, there is no consensus on how humans should be expected to respond to these challenges. On the contrary, sociological research on post-disaster behavior has reached the opposite conclusion of the popular perception, finding that the likelihood of anti-social behavior tends to drop during and after disasters.

The roots of sociological research on disaster behavior can be traced at least back to Durkheim (1952[1897]), who found that great social disturbances and wars tend to increase social integration, thereby reducing the risk of anti-social behavior. Prince (1920) is credited with the first study aimed specifically at studying human behavior when faced with catastrophic events. Prince studied victims of the Halifax Explosion in December 1917, finding – as would be expected from Durkheim's observations – that the disaster had contributed to increased social cohesion in the affected community.

The first large-scale systematic studies of human behavior in disaster were conducted by US researchers in the aftermath of World War II. The research projects were motivated by two main factors: worries about how the population could be expected to respond to a nuclear attack and an interest in how to cause maximum disruption to an enemy during war (Fritz, 1961). With only two cases of nuclear bombing to study, the researchers turned to natural disasters and conventional bombing during World War II in order to increase knowledge about human behavior in disaster situations. The initial expectation was that bombing and natural disasters would cause massive panic and a breakdown of law, order, and social norms, but this was strongly contradicted by the findings. Summarizing the results of more than 16,000 interviews and questionnaires conducted after 144 peacetime disasters in the USA and among victims of Allied bombing in Germany and Japan (Disaster Research Group, 1961; US Strategic Bombing Survey, 1947a,b), Fritz (1996: 10) wrote that

> Even under the worst disaster conditions, people maintain or quickly regain self control and become concerned about the welfare of others.... antisocial behavior, such as aggression towards others and scapegoating are rare or nonexistent. Instead, most disasters produce a great increase in social solidarity among the stricken populace, and this newly created solidarity tends to reduce the incidence of most forms of personal and social pathology.

The article was written in 1961, but not made publicly available until 1996.[2] Although this is an old work, Fritz states in his 1996 introduction that he has not seen anything in more recent studies that would change the basic framework of his analysis (Fritz, 1996: 17).

Fritz suggests that the importance of cultural and individual differences diminish in disaster situations, as people are forced to confront similar dilemmas under similar conditions, irrespective of class or ethnicity (Fritz, 1961: 655). Further, such group identities are reported to be superseded by a common identity among the disaster victims, a 'community of sufferers', as they are united by an external threat that is common to all. The emergence of such communities is posited as a universal feature of disasters where the survivors are permitted to interact freely and to make an unimpeded social adjustment to the disaster (Fritz, 1996: 28).

A core implication from disaster sociology is, then, that disasters should reduce the likelihood of violent conflict through generating a sense of unity among the victims and reducing the importance of divides that might otherwise be conducive to conflict. This is starkly opposed to the suggestion that disasters increase the likelihood of conflict due to relative deprivation, ethnic security dilemmas, or fighting over diminishing resources. While the theory focuses mainly on the people directly affected by the disaster, one may speculate that the unifying effect may spread to the rest of the population as well – the frequent desire of outsiders to help in disaster situations is well known.

Within this theory, there are some important caveats to consider. First, if disasters are to trigger violent conflict, this is expected to happen in poor, unstable countries, not in the well-organized industrial countries where the cases cited by Fritz were located. Therefore, the theory must be tested on a representative sample of countries in order to assess its explanatory power relative to other theories – which is part of the rationale for this article. Second, Fritz's theoretical argument is founded on the assumption that disaster victims are allowed to make an unimpeded adjustment to the disaster situation (Fritz, 1996: 28). He does not specify this assumption in detail, but it appears to have been fulfilled in the cases he builds his conclusions on – which includes wartime Germany and Japan. Authoritarian regimes may be suspected of having an interest in preventing the formation of any group capable of collective action – including disaster communities. This appears even more likely when considering that disasters may increase the likelihood for demands for social and political change, which may be highly unpopular among authoritarian leaders (Fritz, 1996: 55).

Within the current debate on how environmental factors may affect the risk of conflict, scarcity of important resources holds a prominent place. Acute scarcities, caused by reduced supply, increased demand or skewed distribution, are suggested as a significant current and future source of violent conflict (Homer-Dixon, 1999). Most natural disasters occur relatively abruptly (the main exception is drought), but the after-effects may linger on for a long time, causing or exacerbating scarcities similar to those described by Homer-Dixon. From the perspective of disaster sociology, however, this is where the similarities end. Of the three main theoretical connections between scarcity and conflict risk suggested by Homer-Dixon (1999: 136f), two are directly contradicted by disaster sociology, while the third is at least partially contradicted.

The two first connections suggested by Homer-Dixon are frustration-aggression theories and group identity

[2] It was originally intended to be part of an edited book on disaster topics edited by Charles Fritz and Harry Williams. However, both changed jobs before the book was finished, and Fritz's chapter was left out of the book after what appears to be mainly editorial disagreements. For a fuller description, see Fritz (1996: iii-iv).

theories. Frustration and relative deprivation as well as political leaders' exploitation of ethnic and social identity by pitting different groups against each other are proposed as plausible connections at the individual and group levels. Homer-Dixon's third connection is located at the systemic level: environmental scarcities are expected to reduce constraints facing insurgents, for example through weakening state institutions, thereby increasing the opportunity for rebellion. Homer-Dixon (1999: 142f) expects that the combination of relative deprivation, increased intergroup segmentation, and perceptions of a weakened state is likely to cause insurgency.

The first two suggested connections run directly counter to Fritz's arguments that disasters reduce the relevance of group identities and the likelihood of aggressive behavior, thereby also reducing the risk of group-identity-driven conflict. With respect to the weakening of state institutions, disaster sociology expects the increased sense of solidarity among disaster victims to arise in the absence of any overarching authority. Increased levels of conflict due to this are therefore not expected. However, the findings summarized by Fritz are from countries with strong systems of governance, so it is unlikely that insurgent organizations should be able to exploit temporary reductions in state capacity. Such a scenario appears far more realistic in many of the countries that are already among the most vulnerable to natural disasters.

Natural disasters will, almost by definition, overstrain governments' capacities in affected areas for a certain amount of time. However, it is not straightforward that this should increase the opportunities for insurgency. First, the insurgents may be set back by the disaster as well. Second, a time when potential insurgents and their families are busy trying to survive and recover from a disaster may not be the best period to attempt to instigate a rebellion. Also, if disasters contribute to reducing grievances and group differences, the foundation for an insurgency may be severely undermined.

A more serious objection against disaster sociology can be derived from the argument of how disaster victims are unified by their common experiences. Even if this is generally true, it begs the question of who become victims. Disasters generally do not select their victims at random – the poorest and most vulnerable are most likely to be affected. These groups usually live in the least disaster-resistant habitations, in the most exposed areas, and are generally less able to secure the resources needed for quick and effective recovery (Wisner et al., 2004). If disaster damages follow ethnic or social divides, this may be suspected to increase the likelihood of conflict, while damage – and aid efforts – across such divides may reduce it.

A potentially important difference between Homer-Dixon's scarcity perspective and disaster sociology is the time frame. Environmental degradation is a slow and gradual process while a disaster, which by definition is a severe exception from everyday life, has a shorter duration. Although a drought can linger on for months and years, most natural disasters pass in a matter of days or weeks. Reconstruction, however, may take substantially longer. This raises the question of how long after a disaster the reduced likelihood of conflict is likely to last, and whether it can be expected to be replaced by increased friction during the reconstruction process.

Quarantelli & Dynes (1976) find that conflicts related to the allocation of blame or the distribution of resources tend to arise in the reconstruction process after disasters. As their analysis is limited to non-violent conflict in USA, it is less clear how this should be expected to play out in other countries. Goldstone (2001) suggests that the political outcomes of disasters hinge on how they are handled; poorly managed disasters in developing countries are argued to increase the risk of political unrest and violence, while competent disaster management may actually increase the support for the government. This argument is mirrored by Olson & Gawronski (2010), who find that disasters may cause governments' popularity to rise or fall, depending on whether they are able to demonstrate competence in their management and concern for the victims. A case study of the different effects that the 2004 Tsunami in the Indian Ocean had on the conflicts in Sri Lanka and Aceh (Enia, 2008) offers further support for this contention by illustrating how governments and insurgents in both conflicts attempted to use the situation to advance their own goals, with very different outcomes.

Initially, one would expect the 'best' disaster management to be found in rich, Western countries. However, this is also where the expectations are likely to be highest. Disaster management must, at least in this case, be gauged relative to people's expectations. This means that governments with a weaker absolute performance may still perform better relative to the population's expectations, and therefore emerge with strengthened legitimacy after a disaster. Therefore these findings do not suggest a systematic effect of disasters on the risk of conflict. To address this question, large-scale systematic research seems necessary. This need is further underscored by the criticism that prominent research within both environmental security and disaster sociology may not have used sufficiently representative cases (see, for example, Gleditsch, 1998). This study is an attempt to move in that direction.

One of the first cross-national quantitative studies aimed at investigating the relation between natural disasters and the risk of violent conflict was conducted by Drury & Olson (1998). They find a positive relation between disaster severity and the level of political unrest, using a sample of the 12 countries that experienced one or more disasters that killed at least 1,500 persons[3] between 1966 and 1980. Country-years from two years before the first disaster until seven years after the last are included in the analysis. They find a positive association between disaster severity and the risk of post-disaster conflict and present six cases where severe natural disasters have been followed by conflict. Although path-breaking at its time, the study suffers from a sample that is limited both in time and in the number of countries included. The model also lacks important controls. The findings are highly interesting, but would benefit from a revisit with an improved and expanded model that incorporates a broader sample and sufficient control variables. This study proceeds in this spirit.

Brancati (2007) finds that earthquakes are significantly related to an increased risk of violent conflict. She argues that among natural disasters, earthquakes are particularly likely to trigger conflict due to their rapid and unpredictable onsets. Although earthquakes are not climate-related, mechanisms that connect them to an increased risk of conflict can be expected to be relevant for rapid-onset climatic disasters as well, despite the differences between climate-related and geological disasters just mentioned. Brancati tests three different dependent variables[4] and finds that the relationship is strongest for low-level violence.

Although the author claims that 'earthquakes can actually stimulate intrastate conflict by producing scarcities in basic resources, particularly in developing countries where the competition for scarce resources is most intense' (Brancati, 2007: 715), there is no variable measuring this in her models. One should be cautious about drawing conclusions about causal connections on such a weak basis, in particular within an area with so many plausible causal mechanisms (for an overview, see Buhaug, Gleditsch & Thiesen, 2010). While the dependent variable of this study is the *onset* of armed conflict, Brancati uses *incidence* in the models aimed at analyzing earthquakes' effect on civil war. The factors explaining why a conflict broke out are not necessarily the same as those explaining why it keeps going (Fearon, 2004). While we can discuss which measure should be used, replication reveals that earthquakes lose their explanatory power when onset is used as a dependent variable. This indicates that Brancati's results are not valid for *outbreak* of civil war. Onset seems like the appropriate measure here, since incidence per definition follows after an onset that may or may not have been caused by a disaster.

The most extensive cross-national analysis focusing specifically on natural disasters to date was undertaken by Nel & Righarts (2008). Using a sample of 183 political units covering the period 1950–2000, they find a positive relationship between natural disasters and the risk of armed conflict. They test measures for geological disasters and climate-related disasters and a measure combining all natural disasters on dependent variables of minor violent civil conflict onset (less than 1,000 killed), major conflict onset (more than 1,000 killed), and a variable including both these. Their findings indicate that disasters increase the risk of conflict onset in the same year and following year. Climate-related disasters are found to follow this pattern, except that they do not appear significantly related to minor conflict.

The indicator for disasters used by Nel & Righarts is the number of disasters per capita for each country-year. Although this approach has some merit as a proxy for the share of a population that may be affected by disasters, I am skeptical for two main reasons. First, it reduces the weight of disasters that affect large populations. A disaster does not get diluted by hitting a large population – if anything, it becomes more severe. Second, this approach is used instead of including population size as a control variable in the model. Large populations are, *ceteris paribus*, more likely to experience both natural disasters and armed conflict. Not including population size in the models may therefore cause the effect of disasters on conflict risk to become confounded with the effect of population size.[5]

The increased risk of disasters is mainly due to the fact that large populations hold more potential victims of natural hazards. This makes natural hazards turn into disasters more often, and the death toll is likely to be

[3] These countries are Bangladesh, China, Guatemala, Honduras, India, Iran, Nicaragua, Nigeria, Pakistan, Peru, the Philippines, and Turkey

[4] These are conflict events, level of rebellion, and incidence of civil war.

[5] The number of people affected by disaster could be used as a control variable, but the high share of apparently non-randomly distributed missing information on the variable provided by EM-DAT (CRED, 2007) would diminish and potentially bias the sample.

higher. With regard to conflict, a large population size is among the few factors that appear robustly related to an increased risk of civil war (Dixon, 2009; Hegre & Sambanis, 2006). Despite the robustness of the relation, the causal link is open to several interpretations. One is that large populations are more difficult to administer, thereby making it harder for governments to detect and prevent insurgencies (Fearon & Laitin 2003). A second possibility is that large populations hold more potential insurgents, as well as larger groups of potential sympathizers and recruits. This suggestion is supported by Raleigh & Hegre (2009), who find that the frequency of conflict events in Africa tends to be proportional to the population size of the area in question. Large populations are also likely to be more heterogeneous, indicating that a larger number of identity groups may be used for mobilizing conflict. On average, the number of members in each group can be expected to be greater in large populations. If conflict that involves identity groups starts in such countries, the sheer size of the groups increases the likelihood of a larger number of persons being killed, thereby also increasing the likelihood that a conflict is recorded in armed conflict databases.

If population size is not included in a regression model aiming to explain the occurrence of armed conflict, and some other phenomenon that occurs more often in large populations is included in the model, this phenomenon is likely to capture some of the explanatory power of population size. Above, I argued that, *ceteris paribus*, large populations experience more natural disasters and more armed conflict. This means that without population size in the model, the effect disasters have on conflict risk is likely to be overestimated. This may be what happens in the model used by Nel & Righarts. As shown in Table I, a replication of their models on climate-related disasters shows that the results are not robust to the inclusion of population size.[6]

The environmental security literature and disaster sociology reach opposite conclusions regarding how disasters should be expected to affect the risk of armed conflict. The middle road offered by Goldstone (2001), Enia (2008), and others complicates matters further. The quantitative studies presented offer support for the environmental-security perspective, but the robustness of the findings have been called into question. On the positive side, two theories pitted against each other offer an interesting opportunity for empirical testing, which is attempted in this article.

Table I. Accounting for civil conflict: Replication of Model 11 in Nel & Righarts (2008) and model with population size included

	Replication	*Population size included*
Number of climatic disasters per capita	1.113***	0.348
	(0.310)	(0.386)
Infant mortality rate $_{t-1}$	0.0230***	0.0231***
	(0.00566)	(0.00566)
Infant mortality rate squared $_{t-1}$	–7.80e–05***	–7.57e–05***
	(2.68e–05)	(2.71e–05)
Mixed regime	0.524***	0.447***
	(0.155)	(0.156)
GDP growth	–0.0671***	–0.0773***
	(0.0197)	(0.0198)
Brevity of peace	0.900***	0.621***
	(0.162)	(0.176)
Total population (ln)		0.250***
		(0.0394)
Constant	–5.139***	–7.206***
	(0.251)	(0.402)
Observations	7,829	7,829

Relogit regression coefficients with robust standard errors in parentheses.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Interactive effects between disasters and other phenomena?

Theoretical interest and the strong effect of population size in the Nel & Righarts (2008) study indicate that the relation between disasters and population size should be examined in greater detail. Logically, countries with a large population will experience more disasters, because there are more potential victims and because these countries tend to have a larger area. However, disasters happen only where people live, so severe weather events in uninhabited areas will not turn into disasters.[7]

What happens, then, if a large population with all its potential insurgents is hit by a natural disaster? If disasters, as has been suggested, produce feelings of relative deprivation, increase frustration with the government,

[6] Geological disasters, on the other hand, remain significantly related to conflict risk after the inclusion of population size in the model, indicating that climate-related and geological disasters may affect the risk of conflict differently.

[7] Area does not contribute significantly in a model where population size is included.

and cause fighting over diminishing resources, the conflict-instigating effect should be larger in a country with a large population. If, on the contrary, disasters contribute to increased societal cohesion, an increased sense of unity in the face of adversity, or simply a widespread interest in maintaining fragile stability, disasters should reduce the individual propensity to engage in rebellion. This, in turn, should lead to a decrease in the risk-increasing effect of population size for some period of time after a disaster. The inclusion of an interaction term between disaster and population size (Model 6, below) constitutes a significant improvement of the model and reinforces the impression that disasters reduce the risk of conflict.

Weak, semi-authoritarian countries are frequently found to face a greater risk of civil war than authoritarian and democratic countries (Hegre et al., 2001). Often corrupt and ineffective, without strong incentives to aid the population in times of crisis, but also lacking the ability to quash nascent rebellions by force, these regimes may also plausibly be expected to be more at risk from disaster-induced uprisings. An interaction between disasters and semi-authoritarian regime, measured as country-years scoring between –5 and 5 on the Polity IV scale, was tested. The inclusion of this measure did not constitute a significant improvement of the model (not shown).

Poverty is one of the main predictors for civil war and poor countries are also, in general, less able to prevent severe weather events from turning into disasters. In a similar vein as the semi-authoritarian regimes, these countries may also be expected to be less able to prevent unrest from escalating once it has started. This was tested by running an interaction between natural disasters and a dummy that takes the value of 1 if a given country-year has among the 25% lowest GDP per capita. This measure contributed to a weak improvement of the overall model (not shown).

Data

The issue of how to define and measure disasters deserves some extra comments, as it is not straightforward. Two main ways of defining natural disasters are used in the literature on environment and security. One focuses on 'physical' measures, such as millimeters of precipitation, temperature, or score on the Richter scale for earthquakes.[8] These will be referred to as 'force-based' measures. The alternative emphasizes human consequences rather than physical effects. Here, disasters are coded by the damage they cause to humans rather than their 'objective' force. This definition-type is the basis for the EM-DAT database (CRED, 2007) and is used for example by Nel & Righarts (2008), as well as in this study. While these two methods will often yield similar results, there are some important differences.

An obvious attractiveness of the force-based measures is that they appear precise and exogenous to human affairs. While the decision of what qualifies as a 'disaster' may vary in different contexts, temperature and precipitation can be measured accurately – to the extent that there are measurement stations in the relevant areas.[9] Also, unlike their social consequences, these measures are independent of the political situation in the areas where they occur. Further, they are less dependent on media coverage than consequence-based measures, although both types of information may be difficult to obtain from poorly developed or strongly authoritarian countries.

While the activities of political actors are indeed unlikely to affect wind speeds, temperatures, or precipitation, I argue that for research on how natural factors may influence the risk of conflict, these factors are not what matter. Although the two are closely related, humans react to the consequences of natural forces, not the forces in themselves. These consequences are shaped by the interplay of hazard, exposure, and vulnerability, as outlined above. Theories on how environmental factors may cause people to fight are based on the level of adversity and pressure people face rather than some objective force of hazard events. Thus, the most relevant measure is the level of adversity resulting from a severe weather event, not the force of the weather event in itself. Another important advantage of using a consequence-based measure is that it takes adaptation to natural hazards into account endogenously. Experience with facing such hazards as well as societal and technological development can be translated into practices that reduce vulnerability, so that the hazards do not become disasters. Such adaptation to environmental factors is not captured by analyzing only physical effects, which implicitly assume a time- and place-invariant vulnerability to severe weather events.[10]

[8] For some examples of these types of measures, see Miguel et al. (2004), Burke et al. (2009), or Brancati (2007).

[9] For some examples on problems with such measures, see for example Tiedemann (1984), Knutson et al. (2010), and Landsea et al. (2006).

[10] As an illustration one may consider the Bhola Cyclone, which killed several hundred thousand people in Bangladesh in 1970 and the more powerful Yasi Cyclone, which did not lead to direct fatalities when it struck Australia in 2010.

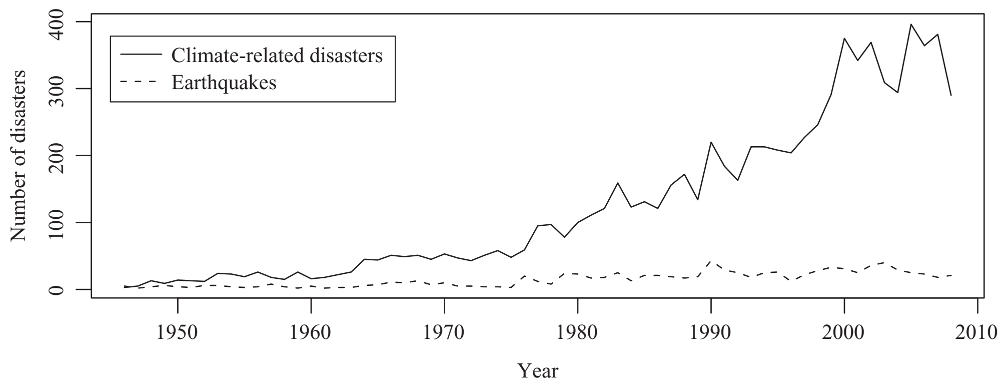

Figure 1. Number of recorded climate-related natural disasters and earthquakes, 1946–2008
Source: CRED (2007).

One weakness of the consequence-based measures is that the likelihood that a severe event translates into a disaster depends on both the situation in the affected area and, to some extent, the practices of how and when calls for international assistance are issued or states of emergency are declared. However, among the available data, I consider that this is the best choice for analyzing the relation between natural disasters and conflict risk. Since my area of interest is the consequences of climate change, I include only natural disasters that are expected to be affected by climate change. These are storms, floods, droughts, extreme temperatures, wildfires, and landslides.[11] The data come from the EM-DAT database (CRED, 2007). In order to be included in the database, an event must fulfill at least one of the following criteria: ten or more people reported killed, 100 or more reported affected, a declaration of a state of emergency, and/or a call for international assistance. In this study, disasters have been coded at the country-year level. This means that a single disaster that has affected two countries is recorded as one disaster for each country. As illustrated in Figure 1, the number of recorded climate-related natural disasters has increased substantially since the mid-1970s. This trend alone illustrates why the question of natural disasters and conflict is relevant, regardless of the discussion about climate change. While some of this increase is likely to be driven by increased access to information, the distribution of earthquakes has not undergone a similar change. This indicates that the increase in climate-related disasters is not driven only by increased reporting.[12] A total of 7,833 disasters are included in this study.

In addition to the increase across time, there is substantial variation between countries. Clearly, this is largely due to the distribution of hazards, vulnerability, and exposure, but it also appears likely that some of the variation is caused by different reporting practices. The United States stands out clearly among the most disaster-prone countries,[13] as it has experienced an annual number of five or more disasters 38 times since 1945. As illustrated by Figure 2, the most common is zero disasters, while only a small minority experience five or more.

Figure 3 illustrates geographical distribution of disasters in the period analyzed. The three most disaster-affected countries are the United States (699 disasters), China (462 disasters), and India (445 disasters). The high prevalence of disasters in the United States gives some reason for concern that the trends are driven by changes in US disaster-reporting practices. However, the trends displayed above, as well as the overall results of this study, are largely unaffected by excluding any or all of these countries from the analysis.

As outlined above, my starting point for analyzing the relation between climate-related natural disasters and the risk of civil war is the model developed by Fearon & Laitin (2003). However, their original dependent variable only runs until 1999. For this reason, and also

[11] EM-DAT distinguishes between 'wet' and 'dry' mass movements. The 'dry' are considered as geophysical events originating from solid earth. As these are not climate-related, they are not included. For more on the disaster classifications, see http://www.emdat.be/classification.

[12] The results presented in this study are robust to excluding the earlier parts of the time period.

[13] The three most disaster-prone country years, with 32, 33, and 35 disasters were all in the United States (1998, 1997, and 1991, respectively). Of 26 country-years experiencing 20 or more disasters, 16 were in the United States, 6 in China, three in India, and one in the Philippines.

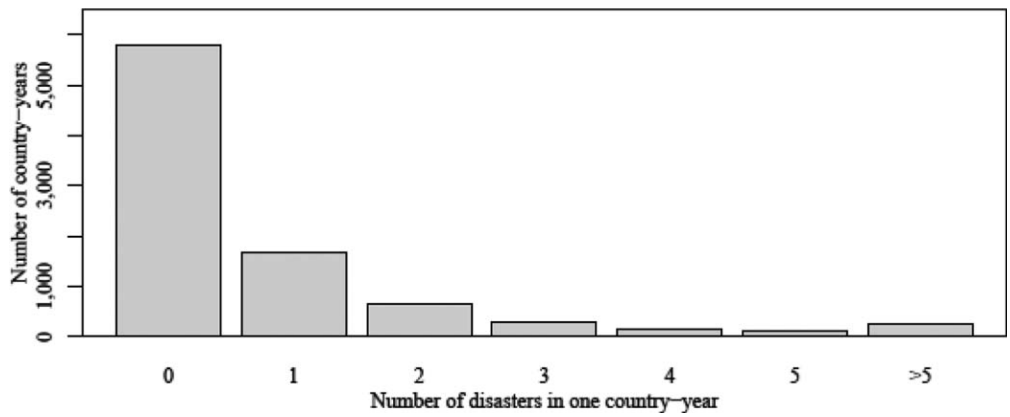

Figure 2. Number of climate-related natural disasters per country-year, 1946–2008
Source: CRED (2007).

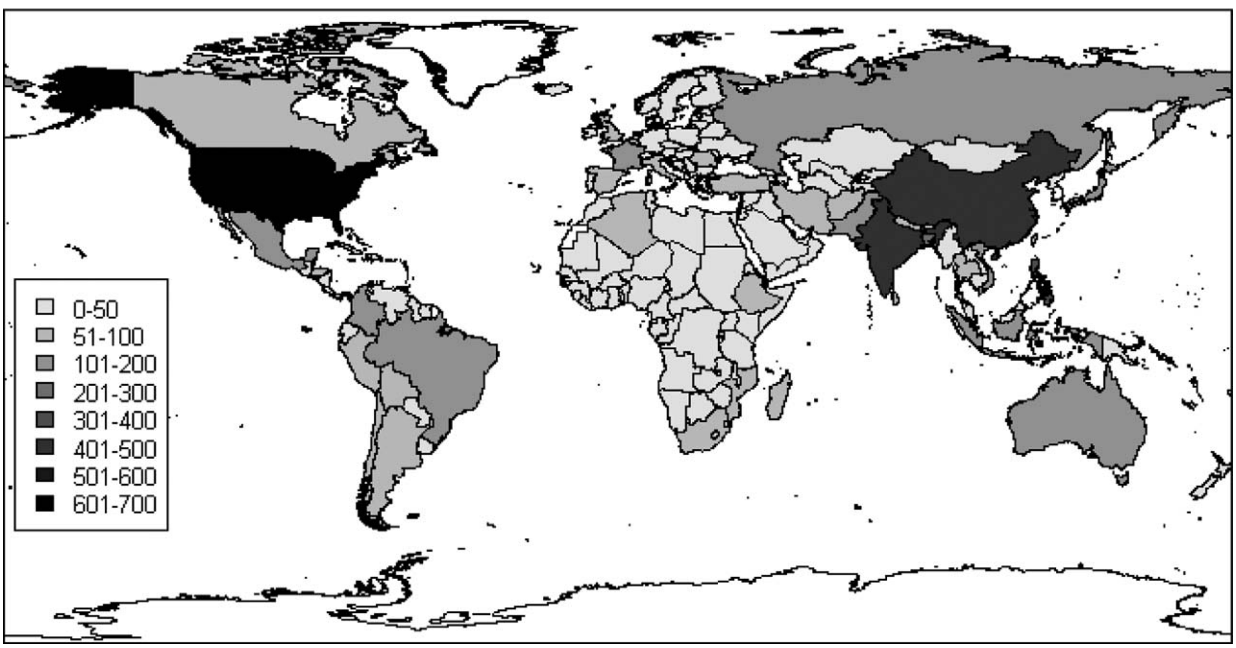

Figure 3. Geographical distribution of recorded climate-related natural disasters, 1946–2008

because I want a dependent variable that captures lower-level conflict as well, I use onset data from the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al., 2002; Strand, 2006). This dataset covers conflicts that cause at least 25 battle-related deaths in a calendar year. For robustness testing and increased consistency with previous studies, I also test a dependent variable with a threshold of 1,000 battle-related deaths. In order to control for temporal dependency, an indicator of whether or not a conflict was ongoing in the previous time period is included.

In order to extend the time period analyzed by Fearon & Laitin (2003), their time-invariant variables (percentage rough terrain, noncontiguous state, ethnic and religious fractionalization) are extrapolated, while other variables are replaced with similar data. The Penn World Tables (Heston, Summers & Aten, 2009) are used for retrieving information on GDP per capita while population size is derived from Banks (2009). Both these variables are log-transformed in the analysis and lagged one year to reduce the risk of picking up effects from conflict onsets. Data on oil exports are obtained from the World Bank (2007). These data are sample-constraining due to missing information and are therefore not used in all the models. This exclusion has a minimal effect on the other coefficients.

The time frame for the variables that indicate whether a state has recently joined the international system or has experienced instability in the system of governance (defined as whether or not the country experienced a change in score on the Polity IV index during the three years prior to the year in question) is extended using data from the Correlates of War Project (2008) and the Polity IV dataset (Marshall & Jaggers, 2009), respectively. Information on the level of democracy was also retrieved from Polity IV, but here I deviate from the procedure used by Fearon & Laitin (2003). Their main measure of level of democracy is a linear measure of score on the Polity scale (lagged one year). However, as the semi-authoritarian regimes in the middle of the Polity scale have been found to be substantially more at risk, I use a dummy variable instead. This variable takes the value of 1 if a country-year scores between –5 and 5 on the Polity IV scale, 0 otherwise.

In line with Fearon & Laitin (2003), I include a variable measuring whether a country had an ongoing conflict in the previous year. The main variables of interest, disasters and conflict, may trend upwards without being causally connected, thereby giving a false impression of a causal relation. In order to control for this, the models are run with year-fixed effects (individual year dummies not shown in the model outputs). I also cluster the analysis by country, where I assume that the observations within countries are not independent whereas they are across countries. Clustering on units also allows the computation of standard errors that are robust to heteroskedasticity.

Results

The first model in Table II replicates Fearon & Laitin's (2003) Model 1, and is included as a basis of comparison for the other models. Model 2 is run using the UCDP/PRIO dependent variable with 25 battle-related deaths as the inclusion criterion. The main change is that the variables indicating whether a conflict was ongoing in the previous time period and whether states are noncontiguous lose significance. Ethnic fractionalization, on the other hand, becomes significant with a positive sign. Also, as others have reported, semi-authoritarian rule is significantly related to an increased risk of conflict. Oil exporters are also at higher risk of conflict, but this variable is sample-constraining and the other variables perform quite similarly with and without it. This variable is dropped from Model 3 onwards in order to increase the number of observations.

Including a count measure of the number of disasters that has affected a country in a given year does not contribute significantly to explaining the occurrence of civil war onsets. Nor is the overall performance of the model significantly improved.[14] The same conclusion holds if a squared term is added in order to control for a curvilinear relation (not shown). Model 4, on the other hand, is a significant improvement. Here, the count measure is replaced with a binary variable that takes the value of 1 in country-years that have experienced one or more disasters and 0 in years that have not. This variable is significantly negatively related to the onset of civil conflict. When running the model with only rapid-onset or destructive disasters included (not shown), the results for count and binary measures are similar in direction but generally somewhat weaker in significance than the results shown in Model 4. Thus, these results indicate that disasters do not raise the risk of conflict; on the contrary, they appear to lower it.

In Model 5, the binary disaster variable is replaced by the number of disasters of each type that have affected a country in a year (landslides, wildfires, heat and cold waves not shown). The number of droughts is negatively related to conflict onset and statistically highly significant, a result that goes against one of the most consistently-argued propositions about climate change and the consequences from the lack of water. None of the other types of disasters are statistically significant. When using single disaster types, there is less difference between the count and binary measures (binary not shown); the significance of droughts is reduced to just outside the 5% significance limit while the other types remain insignificant.

In sum, Model 4, where a binary measure of natural disasters is used, is the model where the disaster variable has the strongest effect. When keeping all other variables at their means, the risk of conflict is estimated at 4.0% when the disaster dummy is fixed at 0, while it drops to 2.9% when the disaster dummy is fixed at 1. The effect is similar but slightly stronger when using a disaster variable that is lagged one year, indicating that experiencing natural disasters limits the risk of conflict in the same year and in the following year.[15] This suggests that the conflict-inhibiting effect tends to last beyond the immediate aftermath of the disasters. Also, it improves confidence in the results, as the risk that they are

[14] The likelihood-ratio tests were run on models without robust standard errors, as the test does not handle this method.

[15] A binary disaster measure lagged two years was tested, but did not come close to contributing significantly to the model.

Table II. Accounting for civil conflict: Replication of Model 1 in Fearon & Laitin (2003) and with models with natural disasters included

Dependent variable	*(1)* *F&L onset*	*(2)* *Onset*	*(3)* *Onset*	*(4)* *Onset*	*(5)* *Onset*	*(6)* *Onset*
Conflict previous year	–0.954***	–0.047	–0.133	–0.116	–0.154	–0.152
	(0.314)	(0.264)	(0.268)	(0.260)	(0.270)	(0.271)
GDP/cap, lagged	–0.344***	–0.411***	–0.449***	–0.441***	–0.452***	–0.445***
	(0.072)	(0.087)	(0.095)	(0.094)	(0.096)	(0.095)
Pop. size (ln), lagged	0.263***	0.243***	0.241***	0.285***	0.231***	0.154**
	(0.073)	(0.059)	(0.053)	(0.056)	(0.055)	(0.076)
Rough terrain	0.219***	0.134**	0.109*	0.124**	0.114*	0.134**
	(0.085)	(0.064)	(0.062)	(0.061)	(0.063)	(0.061)
Noncontiguous state	0.443	0.299	0.340	0.387	0.347	0.338
	(0.274)	(0.232)	(0.259)	(0.254)	(0.255)	(0.258)
Oil exporter	0.858***	0.540**				
	(0.279)	(0.254)				
New state	1.709***	1.324***	1.293***	1.298***	1.295***	1.246***
	(0.339)	(0.353)	(0.357)	(0.360)	(0.357)	(0.352)
Recent instability, lagged	0.618***	0.166	0.117	0.125	0.122	0.143
	(0.235)	(0.193)	(0.189)	(0.189)	(0.190)	(0.191)
Level of democracy,[1] lagged	0.021	0.419**	0.505***	0.494***	0.491***	0.533***
	(0.017)	(0.189)	(0.185)	(0.181)	(0.184)	(0.183)
Ethnic fractionalization	0.166	1.080***	1.133***	1.143***	1.127***	1.139***
	(0.373)	(0.323)	(0.327)	(0.328)	(0.321)	(0.322)
Religious fractionalization	0.285	–0.511	–0.704*	–0.759*	–0.661	–0.770*
	(0.509)	(0.443)	(0.415)	(0.411)	(0.421)	(0.413)
Disasters, count			0.016			
			(0.033)			
Disasters, binary				–0.333**		–2.908***
				(0.161)		(0.998)
Storms					–0.027	
					(0.050)	
Floods					0.092	
					(0.065)	
Droughts					–0.554**	
					(0.268)	
Disaster * Population						0.264***
						(0.099)
Constant	–6.731***	–5.312***	–4.955***	–5.443***	–4.863***	–4.261***
	(0.736)	(1.450)	(1.435)	(1.436)	(1.433)	(1.495)
Observations	6327	6444	6954	6954	6954	6954
Pseudo R^2	0.108	0.119	0.113	0.115	0.117	0.119

[1] Model 1: score on Polity IV scale; all other models: anocracy dummy.
Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.
Fearon & Laitin's (2003) original dependent variable is used in Model 1, while the dependent variable in the other models is from UCDP/PRIO Armed Conflict Dataset (Gleditsch et al., 2002; Strand, 2006). All models but the original one from Fearon & Laitin are run with robust standard errors and year-fixed effects (year dummies not shown). The results are robust to a wide range of robustness and specification tests.

influenced by endogeneity or reverse causality is further diminished.

The results are robust to a wide range of specification tests and robustness checks, such as excluding single disaster-prone countries such as India, China, and the United States, as well as excluding the countries that constitute the highest GDP per capita quartile in each year. Excluding the lowest quartile, on the other hand,

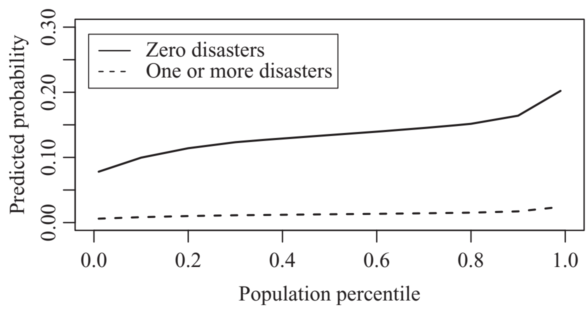

Figure 4. Predicted probabilities of onset with interaction between population size and disasters

weakens the effect of the disasters, but the negative sign is retained. However, it also reduces the number of onsets from 268 to 150. Running the analyses on a dependent variable that only includes conflicts that caused 1,000 or more battle-related deaths gives similar but somewhat weaker results (not shown).

Interactions

The interaction between natural disasters and population size in Model 6 significantly improves the model. Conflict risk increases substantially with increasing population, but it increases more rapidly at zero disasters than at any positive disaster count (tests of disaster count measures not shown). When the interaction term is introduced, the difference between zero and one or more disasters is further increased. In other words, when the interaction term is added, positive disaster counts strongly counterweigh the increased risk of conflict caused by increased population size. For country-years with zero disasters, on the other hand, the risk of conflict grows substantially steeper with increased population size than in a model without the interaction term between population size and disasters.

While the interaction term between disasters and population size significantly improved the model, the proposition that disasters have stronger effect on conflict risk in semi-authoritarian countries is not supported by the data. The interaction between poverty and disasters fares somewhat better; the model is improved, but significant only at the 10% level. An interaction with GDP per capita was also tested, but it did not contribute significantly to the model (not shown).

Discussion and conclusion

I set out to test whether natural disasters can add explanatory power to an established model of civil conflict. The results indicate that they can, but that their effect on conflict is the opposite of popular perception. To the extent that climate-related natural disasters affect the risk of conflict, they contribute to reducing it. This holds for a measure of climate-related natural disasters in general, as well as drought in particular. While this finding contradicts recent debate, it is consistent with a large amount of research, in particular research carried out in the 1950s and 1960s, on the relation between disasters and the risk of anti-social behavior.

An important weakness of these studies is the lack of variation in important factors such as political system, economic level, and cultural conditions. Despite this, Fritz (1996: 15) argues that disaster victims have a striking similarity in behavior across space, time, and culture. The results presented in this article support his argument; the effect of climate-related natural disasters appears robust and cross-nationally valid. An important question is the dependence of specification: while the binary measure is robust, other specifications such as a pure count of disasters, checks for a curvilinear effect, and various ordinal measures were tested but performed more poorly. The main difference is between those who experience disasters and those who do not: the number of disasters that occur within a country-year appears less important. This finding also underscores the importance of being cautious about assuming that adversity will automatically translate into increased levels of conflict – a perception that appears frequent among a number of vocal actors in the debate around the political consequences of climate change.

One explanation of the interaction effect between population size and natural disasters may be that the unifying effect described among others by Fritz (1996) may reduce the willingness to join insurgent organizations. Another possible effect relates to Goldstone's (2001: 46) suggestion that disasters provide an opportunity for governments to display both their competence and incompetence. The negative effect of disasters on conflict risk may be read as that governments tend to improve their popularity – the population in affected areas (and perhaps other areas as well) may be left with a more positive impression of the government than they had before the disaster. This should in itself contribute to reducing the pool of potential recruits for insurgent organizations. A third, less optimistic alternative is that disasters simply overstrain societies and would-be insurgents to such an extent that they contribute to limiting rather than expanding the window of opportunity for insurgents.

For further research, investigating the mechanisms connecting disasters to reduced conflict risk could possibly provide interesting avenues towards reducing conflict in general. While the country-year-level analytical approach holds merit in studying aggregate country-level effects, such as the absence or presence of civil war or natural disasters within a country, it appears less suited to capturing specific causal effects between them. Both are commonly limited to specific geographic areas, which means that future studies aimed at investigating in greater detail the relation between natural disasters and conflict risk are likely to benefit from geographically disaggregated designs.

One cannot read into these results that climate change is not dangerous. Even if climate-related natural disasters do not appear to increase the risk of armed conflict, people are still likely to suffer. However, adversity and suffering do not necessarily translate into severe violence. Citizens as well as investors and developers in poor countries seem to have scant reason to fear that climate change-driven disasters will cause armed unrest, even though climate-related natural disasters may cause increasing rates of death and destruction in years to come. Also, while this is the main trend, it is not unlikely that there will be exceptions, that a single disaster incident may be followed by an outbreak of armed conflict. These, like most other armed conflict onsets, should be expected in poor countries with low level of development and weak, inefficient regimes. Despite climate change, economic and political variables remain the most important predictors of conflict. Rather than over-emphasizing conflict as a result of climate change, I would recommend keeping the focus on societal development, including building resilience against adverse effects of climate change. While this promises the possibility of alleviating the danger of climate change, it can also lead to strengthened societies in the face of natural disaster and civil war.

Replication data

The dataset, codebook and do-files for the empirical analysis in this article can be found at http://www.prio.no/jpr/datasets. All analysis was done using Stata 11.1.

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