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Climatic disasters and armed intrastate conflict

A quantitative analysis assessing how abrupt hydrometeorological disasters affect the risk of conflict termination, covering the years 1985 to 2007

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Abstract

This thesis covers the relatively unstudied connection between hydrometeorological disasters and the duration of armed intrastate conflict, and aims to discover how abrupt climate changes affect the prospects for conflict termination. By performing several Weibull-distributed survival models, it specifically examines the effects of the rapid-onset climatic disasters floods, windstorms, waves, and extreme temperatures on the risk of conflict termination. The central hypothesis leans on a number of theoretical arguments holding that disasters have the capacity to act as catalysts for peace. The results of the analysis do however indicate that disasters reduce the risk of conflict termination, but with the caveat that this effect might reverse with time. With somewhat indistinct empirical results, the thesis falls in line with existing research on the topic arguing that closer, more disaggregated analyses of the mechanisms at play between climatic disasters and conflict dynamics are in demand.

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1. Introduction

This thesis covers the relatively unstudied connection between hydrometeorological changes and conflict duration, and aims to discover whether abrupt climate changes, for better or worse, change the prospects of conflict termination. Specifically it examines the effects of the rapid-onset climatic disasters that are floods, windstorms, waves, extreme temperature and landslides on the chance of conflict termination, following the research question; *how do hydrometeorological disasters affect the duration of armed intrastate conflict?*

Climate change and armed conflict are both individually, and recently also in unison, a hot topic in both policy circles and among researchers. The policy debate seems particularly engaged with how the climate can affect conflict risks, and the possibility of armed conflict resulting from climate changes and subsequent environmental degradation and depletion, receives a lot of attention in both the media and by political authorities. Two Nobel laureates, Al Gore and US President Barack Obama, have conveyed grim scenarios and been amongst the many who link the effects of environmental change to armed conflict. Likewise, the Intergovernmental Panel on Climate Change (IPCC) recently devoted a whole chapter in their latest assessment report (2014) to climate changes and conflict. However, as far as the empirical evidence goes, the public debate is preceding the research, and even running against it at times. Looking at the long term trend of armed conflicts, it is clear that the number and severity of armed conflicts have decreased since the 1990s, while global warming, measured as elevation of temperature, has increased (Buhaug, Gleditsch and Theisen 2010). Theisen, Gleditsch and Buhaug (2013, p.613) explain that "taken together, extant studies provide mostly inconclusive insights, with contradictory or weak demonstrated effects of climate variability and change on armed conflict". Both the negative correlation between conflict and climate change and the inconclusiveness of existing research warrants more research on the topic, but also caution in terms of rhetoric used by the UN and other influential policy makers.

Although there is a lot of research on climate changes and conflict risk, mostly separate but also some combined, there is minimal research on the specific connection between climate changes and conflict dynamics. Conflict dynamics differ from conflict onset (usually investigating what affects the risk of conflict) and involve duration, severity, diffusion, type of termination and recurrence. The "mainstream" theoretical contribution on the climate-conflict nexus is the so-called environmental security literature, arguing that climate change will add to existing strains in already conflict-ridden societies and lead to more conflicts. Although not directly concerning climate change and conflict dynamics, it adds to the understanding of the mechanisms at play also when it comes to conflict duration. With the case of the 2004 tsunami's focal role in the peaceful resolution of the conflict in the Aceh province in Indonesia in mind however, it is the theoretical contributions arguing the opposite of the environmental security that provide the most convincing and thorough accounts of how climatic disasters affect the duration of armed conflict. Disaster sociology, disaster diplomacy and ripeness theory contribute to the compilation of the central argument of the thesis, namely the hypothesis that those conflicts hit by a climatic disaster will have a higher chance of conflict termination than those conflicts that are not affected by such disasters.

In order to answer the research question, I have constructed a dataset covering all intrastate armed conflicts in the world between 1985 and 2007, incorporating a series of pertaining hydrometeorological hazards. The conflict data are gathered from the Uppsala Conflict Data Program (UCDP) and the Peace Research Institute in Oslo's (PRIO) joint dataset on armed conflict, while the disaster data are gathered from the Dartmouth Flood Observatory (DFO) and the Centre for Research on the Epidemiology of Disasters' (CRED) Emergency Events Database (EM-DAT). To test the hypothesis then, a series of parametric survival analyses using the Weibull distribution was performed, looking at the various disaster indicators both with and without a time lag. The results indicate that overall, the occurrence of a hydrometeorological disaster decreases the risk of conflict termination, countering the proposed hypothesis. However, this finding is objected to when looking at the disasters with a time lag. When measuring whether or not a disaster took place the last six months of conflict, the effect suggests that the risk of termination is greater for the conflicts that have experienced a disaster. Because of this, and lacking statistical significances, it is hard to substantially dismiss or confirm the hypothesis. Nevertheless, it is clear that the relationship between climatic disasters and conflict duration is complex, and the need for both empirical and consequent theoretical implications is apparent.

The thesis will begin with a survey of the theoretical contributions on the climate-conflict nexus, before the relevant empirical findings that exist will be presented. Then the research method will present the dataset, the variables and the descriptive statistics, before the survival models will be shown in the fourth chapter presenting the empirical results. Subsequently the discussion will tie together the empirical findings with the theoretical contributions, and point to shortcomings and avenues for future research. The final chapter thereafter reviews the main findings and concludes the thesis.

2. Theoretical contributions and previous research

This chapter begins with a review of existing theory on the climate-conflict nexus. The first section looks at the theoretical contributions holding that natural disasters, and particularly those disasters that are susceptible to the impacts of climate change, increase the risk of conflict. As climate-variability's possible impact on conflict dynamics has been largely ignored in the literature, the section relies on theory predicting the relationship between disasters and conflict onset. First the Environmental Security literature, with Homer-Dixon's (1999) disaster-induced scarcity concept is reviewed. Then Nel and Righarts' (2008) classification of the possible impacts that natural disasters can have on (the risk of) violent civil conflict is presented. Nel and Righarts suggest that natural disasters, including hydrometeorological ones, can create *motives*, *incentives,* and *opportunities* that will increase the risk of civil war. Although these contributions concern disasters and conflict onset, I argue that they also have explanatory power in terms of conflict duration and termination.

On the other hand, environmental peacemaking has also received attention in the recent academic debate. Section 2.2 assesses the contributions within the literature arguing that natural disasters can precipitate peace, beginning with disaster sociology offering a micro-level account of the disaster-conflict nexus. On the macro-level, disaster diplomacy presents the possibilities for peace talks post-disaster, in accordance with Birkland's (1998) notion of focusing events – for example disasters – as policy altering. Finally, ripeness theory argues that disasters can serve as a ripe moment for conflict resolution in that the belligerents realize that there are more to gain from ending the conflict than from continued warfare.

After the two theoretical camps have been accounted for, Section 2.3 presents the status of the relevant research. The research on natural, and particularly climatic, disasters and armed civil conflict is rather short in supply, and especially so when it comes to conflict dynamics. The research that exists is nevertheless presented before looking at other possible drivers of the climate-conflict link. The chapter will be rounded up with a section on the implications of the theory and existing research on this thesis, culminating in the hypothesis that is to be tested in the analysis.

Before turning to the theoretical contribution of environmental security however, the two most important concepts of the thesis must be defined, namely hydrometeorological disasters and armed conflict. As for the first, the United Nations Office for Disaster Risk Reduction (UNISDR) (2007, emphasis added) define hydrometeorological hazards as a "process or phenomenon of atmospheric, hydrological or oceanographic nature that *may cause* loss of life, injury or other health impacts, property damage, loss of livelihoods and services, social and economic disruption, or environmental damage". More specifically the definition holds that

> "Hydrometeorological hazards include tropical cyclones (also known as typhoons and hurricanes), thunderstorms, hailstorms, tornados, blizzards, heavy snowfall, avalanches, coastal storm surges, floods including flash floods, drought, heat waves and cold spells. Hydrometeorological conditions also can be a factor in other hazards such as landslides, wildland fires, locust plagues, epidemics, and in the transport and dispersal of toxic substances and volcanic eruption material" (UNISDR 2007).

The analysis will include the rapid-onset disasters floods, windstorms, heat waves, cold spells and waves[1], and – despite their more indirect character – landslides. The occurrence of all these hazards is expected to increase concurrently with climate change. The reason for leaving out the slow-onset events, most notably droughts, is the fact that the unit of analysis is conflict-months, and not years. It would be exigent to determine when a drought starts and ends with respect to month, and to avoid discrepancies among the indicators, they are not included. The terms hydrometeorological disaster, climatic disaster and disaster will be used interchangeably throughout the thesis.

Furthermore, I adopt the Uppsala Conflict Data Program's (UCDP) definition of armed conflict which is "a contested incompatibility that concerns government and/or territory where the use of armed forces between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths in one calendar year" (Themnér 2014) [2]. Conflicts involving 1000 or more battle-related deaths over a calendar year are defined as wars, while those conflicts with between 25 and 999 battle-related deaths within a year are defined as minor conflicts. The conflicts considered in this analysis are intrastate and internationalized intrastate, where the government is always on one side of the incompatibilities. It is not distinguished between internationalized intrastate and intrastate conflicts, as conflicts may alternate between these two types. For the duration of this thesis the terms conflict, armed conflict, and violent conflict all refer to armed intrastate conflict.

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[1] For an in-depth analysis of how climate change through rising sea levels is likely to increase the number of tidal waves and subsequent threats, see Spanger-Siegfried, Fitzpatrick and Dahl (2014).
[2] A more in-depth discussion of the operationalization of the definitions in the UCDP/PRIO dataset can also be found here.

2.1 Climatic disasters as drivers of conflict

The classical connection between the environment and armed conflict was portrayed by Homer-Dixon in his 1999-book *Environment, Scarcity and Violence.* Here he puts forward the argument that through various social effects, environmental scarcity leads to violent conflict. Resource scarcity, he argues, has three different origins. First, it can stem from a decrease in the supply of resources as a result of resource devastation (Homer-Dixon 1999, p.15). With regards to natural disasters, supply-induced scarcity can for instance occur in the wake of a climatic disaster such as flood or drought. If flooding destroys crops and livestock, supply-induced scarcity might materialize. The second type of scarcity is what is referred to as demand-induced scarcity, taking place when demand exceeds supply. In the climate perspective, mass-migration caused by climatic disasters can conceivably increase demand for resources in the areas where the migration flow ends up, causing scarcity.

The final cause for environmental scarcity delineated is the so-called structural scarcity, or put differently – unequal distribution of resources. Structural scarcity occurs in almost all cases where scarcity results in conflict, and "often the imbalance is deeply rooted in institutions and class and ethnic relations" (Homer-Dixon 1999, p.15). The two former types of scarcity will not lead to conflict unless there is also an element of structural scarcity present. In terms of climatic disasters, it is plausible that even more strain on an already skewed distribution of resources as a consequence of a crop-devastating flood or a house-shattering storm, could contribute to (outbreak of) violent conflict. Hoarding (and looting) of aid inflow in the wake of climatic disasters might also reinforce the existing structural scarcity. An example of this can be found in the aftermath of the 2005 earthquake in Kashmir;

> "For days after the quake, basic help had not reached entire towns and villages that were flattened, prompting angry and frustrated backlashes from many of those affected. Looting is commonplace and when relief supplies do reach affected areas, the most deserving are usually crowded out" (Sajjad 2005 cited in Rajagopalan 2006, p.458).

The social effects through which Homer-Dixon argues that environmental scarcity leads to conflict should also be mentioned. With the risk of over-simplifying his theory, resource scarcity can through different patterns of interaction (resource capture, ecological marginalization) lead to a number of social changes that affect the conflict risk in a given country. Both agricultural and economic productivity could be constrained, affected people might migrate, the society might become (even) more segmented, and the state institutions could be disrupted. Homer-Dixon (1999, p.80) notes that environmental scarcity alone is not enough to cause any of the above, and one must always take consideration of other contextual factor, such as the physical environment (the climate), culture and other social relations[3]. Nevertheless, the notion of environmental scarcity is well applicable on the narrower concept of disaster-induced scarcity.

Strictly speaking, Homer-Dixon's conceptions concern how environmental scarcity is assumed to lead to increased risk of conflict *onset*, and not conflict duration, which is the focus of this analysis. However, it is possible to draw inferences and argue that if something increases risk of onset, it *could* also prolong an ongoing conflict. Specifically, supply-induced scarcity could be thought to fuel the motivation to keep up the fighting in order to obtain scarce resources such as food and shelter. One can also imagine that a devastating storm or a landslide occurring in a situation with already unequally distributed resources, could be seen as an opportunity for intensifying the conflict. Renner and Chafe (2007, p.5) note how "recriminations may occur over such post-disaster realities as unequal relief efforts, inadequate compensation, contentious aid distribution, unwelcome resettlement, or lack of consultation with those who are most affected", perpetuating the conflict. In a more general fashion, the power balance between the actors is also considered an important predictor of conflict. As long as this balance is stable, relations are stable (be they peaceful or not). In the same manner, the power relations between the warring parties also affect the duration of conflict. Assuming that a climatic disaster can affect the power relations, consequently means that the disaster influences the course of the conflict.

Nel and Righarts (2008) employ a framework that attempts to bridge the fairly well researched macro-level with the more unexplored micro-level of the climate-conflict nexus. Their concepts are familiar within peace research, and they argue that in order for natural disasters[4] to lead to violent conflict, *motives, incentives* and *opportunities* all need to be present. These three take on different exemplifications depending on the type of disaster. For instance it is separated between slow and rapid onset type of disasters, the former being exemplified by drought with its subsequent consequences. There is also a distinction in terms

[3] The importance of context will be returned to in the discussion of how disasters and the subsequent scarcity can affect conflict dynamics.
[4] Hydrometeorological disasters pass under the natural disaster-umbrella, and I will not specify the disaster category in this respect as there is no reason why hydrometeorological disasters differ from other types of natural disasters within Nel and Righarts' framework.

of the natural disasters' proximity of the impact. Material destruction and (mass) deaths are immediate effects while less immediate effects can be "disrupting economic development, increasing income and wealth inequality, marginalizing certain groups, and (…) large-scale migrations" (Nel and Righarts 2008, p.162). The latter are called the structural effects of disasters, and it is distinctly possible to draw a parallel to Homer-Dixon's structural scarcity concept.

Their first conceptualization, *motive* refers to the grievances of the adversaries. There needs to be dissatisfaction in the population, for example that the natural disasters lead to widespread suffering, destruction or displacement, or that it increases the resource allocation inequalities. It is important to keep in mind that the natural disaster in itself will hardly create a motive for insurrection as the blame rarely can be put on the state for the occurrence of a natural disaster. Nevertheless, motive alone will not lead to civil conflict. The belligerents also need to be aware of "the gains to be had from acting" (Nel and Righarts 2008, p.164). Following a rational choice mentality, realizing that the reward from engaging in violence exceeds the cost is often a prerequisite for fighting to begin, and this epitomizes the *incentive* precondition. Within this, Nel and Righarts (2008, p.163) point out that natural disasters can create "acute competition for scarce resources" and "incentives for elite resource grabs". Among the three this is probably the concept where disasters have the least direct impact on conflict formation.

Finally there is the *opportunities*-part, capturing the fact that even though there might be both grievances and gains to be had from engaging in violence, there also needs to be an opportunity to do so. In this, overcoming the problem of collective action is central, as "political violence occurs only in a subset of societies, namely those that have conditions in which discontent can be organized, and in which violence is an attractive outlet for grievances" (Nel and Righarts 2008, p.164). Natural disasters can serve as catalysts for such opportunities. For instance, the state might shift its focus away from the (potential) rebels. Another scenario is that in which the disaster aid is captured by insurgents, and the disaster provides an opportunity for increased revenues for the rebels.

Despite the fact that a majority of scholars and policy-makers have tended to lean in this direction, theoretical arguments predicting how climate changes and the accompanying hazards will influence the *dynamics* of conflict in a similar manner are absent. The next section will present the opposite side of the spectrum, where the prognoses in much larger part encompass how natural disasters might impact the conflict dynamics.

2.2 Climatic disasters as catalysts for peace

First up is *disaster sociology*, providing a contrasting image of the disaster-conflict nexus in emphasizing how "nations and communities typically demonstrate amazing toughness and resiliency in absorbing and coping with the disintegrative effects of disaster" (Fritz 1996, p.19). In other words and contrary to the prevailing comprehension of disasters today, this strand of research holds that disasters do not necessarily make people antagonistic. Disaster sociology rests on research from the two world wars in addition to miscellaneous disasters in Northern America throughout the 20th century. The findings maintain that disasters, defined as "an event so encompassing that it involves most of the prevailing social system, so destructive that it disrupts the ongoing system of survival, meaning, order, and motivation" (Fritz 1996, p.21) [5], often hold *conflict-resolving powers*. Fritz (1996, p.47) remarks that "every modern disaster-struck community has not only been quickly restored, but the inhabitants have often proceeded to reorganize their social life with added vitality, integration, and productivity", predicting the opposite picture of the one outlined in the previous section.

The explanation for such post-disaster improvement rests on the notion that disasters are seen to create a *community of sufferers.* Moore (1958 cited in Fritz 1996, pp.31-32) notes how disaster leads "persons and institutions [to] submerge their particular aims in a common effort. Old rivalries are forgotten, or at least become subliminal, in the face of what seems to be an overwhelming task". Ergo, the disaster changes the attention of the contenders. This renders the possibility that when disasters happen, the motives of the affected (groups) change. Following a disaster sociology perspective, a flood or a landslide hitting a conflict-affected area might actually be what is needed for the belligerents to change their focus away from fighting. At best it might even be the beginning of the end of the conflict. For instance, a disaster could change the attention of the fighting groups towards disaster relief, and cooperation might seem necessary in order to cope with the consequences of the event.

Despite the good news that "disasters are not only characterized by "death", "destruction", "disintegrations", and "disease", [and that] they also provide conditions for "vitality", "reconstruction", "integration", "growth", and "health"" (Fritz 1996, p.20), there are limitations in terms of the applicability of disaster sociology. The condition that "disaster survivors are permitted to make a natural, unimpeded social adjustment to the effects of the disaster and also have the opportunity to interact freely with one another" (Fritz 1996, p.21)

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[5] Disasters are not narrowed down to any particular type of disaster, and include natural disasters, bombings and shipwrecks.

restricts the transferability of disaster sociology from the disasters of the 20th century industrialized West, to today's conflict affected areas in the developing parts of the world. It is rarely the case that the people affected by a natural disaster in a conflict zone are free to adjust as they wish, both because they lack resources and because they might be severely oppressed. Nevertheless Fritz (1996, p.22) – knowingly or not – takes consideration of this as he also argues that disasters "undermine many of the cultural and personal distinctions of everyday life and force people to make critical choices under similar conditions". With the idea that disasters erase disparities across time and space, the explanatory power is increased beyond Fritz's narrow sample of disasters.

In a similar manner, *disaster diplomacy*[6] asks whether "some form of non-conflict disaster striking a conflict zone [could] lead to compassion, desire to help, or collaboration in order to deal with that disaster?" (Kelman 2012, p.1). The initial hypothesis within disaster diplomacy held that when a disaster occurred, it would support efforts of diplomacy. Even so, little evidence was found to support neither this hypothesis nor the opposite, leading Kelman (2012, p.14) to propose that "disaster diplomacy has a tangible, but not an overriding presence (…) [and] disaster-related activities can act as a catalyst, but not as a creator, of diplomacy, although catalysis is not always seen". For instance, a destructive disaster might reveal a need to cooperate to secure future coping mechanisms and emergency preparedness, and as such be an instigator of peace negotiations. Gaillard, Clavé and Kelman (2008, p.512) notes that "media coverage of the 2003 devastating floods in Sri Lanka (…) highlights that the Tamil Tigers donated relief supplies amid recent tensions with the Colombo government". However, they point out that the time-span of such efforts are pivotal, as it seems that disaster-related activities have more of an impact in the short-term and then non-disaster factors eventually take over the role as drivers of diplomacy.

Along an interchangeable line of reasoning and drawing on insights from policy analysis, climatic disasters also fit the criteria for being so-called *focusing events*. Focusing events are events that are "sudden; relatively uncommon; can be reasonably defined as harmful or revealing the possibility of potentially greater future harms; has harms that are concentrated in a particular geographical area or community of interest (…)" (Birkland 1997 cited in Birkland 1998, p.54), all suitable descriptions of a climatic disaster. It can for instance be envisaged

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[6] Diplomacy is not restricted to bilateral relations between states are considered, as "those in conflict or collaborating (…) could be sovereign states, international organisations, non-profit groups, businesses, or non-sovereign territories" (Kelman 2012, p.3).

that the state needs to rearrange their priorities in order to deal with the disaster, and with focusing events the emphasis is put on how they have often served as triggers for policy change. In addition, focusing events can alter the existing power balance.

> "While out-of-power groups can often and do often take advantage of focusing events to advance their policy preferences, more powerful groups must carefully plan how they will respond to focusing events. If an event threatens to reduce the power of advantaged groups to control the agenda, these groups are likely to respond defensively to focusing events" (Birkland 1998, p.57).

Both Birkland and Kelman add to the notion that in order for natural disasters to have a positive impact on the peace prospects of a country in violent conflict, several preconditions need to be present. The conditions need to be of a favorable kind, should the unfortunate event of a natural disaster have any positive impact on the termination of armed conflict. Or, put differently, "the key to successful conflict resolution lies in the timing of efforts for resolution" (Zartman 2000, p.225). Zartman is one of the pioneers within *ripeness theory*, arguing that

> "parties resolve their conflict only when they are ready to do so – when alternative usually unilateral, means if achieving satisfactory results are blocked and the parties find themselves un an uncomfortable and costly predicament. At that point they grab on to proposals that usually have been in the air for a long time and that only now appear attractive" (Ibid.).

The condition becomes the ripeness of the conflict – something necessary, but not sufficient in order for the initiation of negotiations. Following a cost-benefit perspective, "ripeness theory is not predictive in the sense that it can tell when a ripe moment can appear in a given situation. It is predictive, however, in identifying the elements necessary (even if not sufficient) for the productive inauguration of negotiations" (Zartman 2000, p.228). These conditions are fulfilled "if the (two) parties to a conflict (a) perceive themselves to be in a hurting stalemate and (b) perceive the possibility of a negotiated solution (a way out), [and] the conflict is ripe for resolution (i.e. for negotiations toward resolution to begin)" (Zartman 2000, pp.228-229).

It is in the situation of stalemate that the occurrence of natural disasters comes into play. Egorova and Hendrix (2014, p.2) portray natural disasters as possible windows of opportunities for more peaceful relations "temporary delegitimizing further violence and presenting an opportunity for peaceful settlement". If the parties are already in a deadlock (most likely seeking a way out), a *catastrophe* can be the contingency necessary for the belligerents to realize that "pain can be sharply increased if something is not done about it now" (Zartman 2000, p.228). A natural disaster can thus serve as a scare for both parties, in line with a cost-benefit viewpoint.

Mason and Fett's (1996, p.549) parameters of negotiated settlements provide specific situations in which the conditions discussed above apply.

> "Any factors that (1) reduce both party's estimate of their chances of victory, (2) increase the rate at which both are absorbing costs, (3) extend both parties' estimate of the amount of time required to achieve victory, or (4) increase the utility from a settlement relative to the utility from victory will make them both more willing to agree to a negotiated settlement rather than continue to fight in hope of achieving victory"

It is not inconceivable that a climatic disaster might fulfill several of the points above. A disaster might impede on the stronger belligerent's opportunity for victory as well as being potentially devastating (both materially and humanly) for both parties, affecting the estimated chances of winning. With attention to disaster relief, condition (4) above might be fulfilled in a situation in which the aid relief adds to the utility of agreeing to a settlement. Finally, material and human damages and disaster reconstruction can potentially increase both the costs and the time of continued fighting for both parties.

2.3 Empirical state of the art

As mentioned in the introduction, studies summing up existing research on the conflict-climate nexus conclude that the extant findings are inconclusive. Buhaug, Gleditsch and Theisen (2010) holds that such non-results should not be interpreted as a confirmation that climate has no effect on the risk of armed conflict, but rather it warrants further research. This section will take a closer look at the research in question and point to the lack of research on climate variability and the dynamics of armed conflict. Most of the studies so far have looked at how climate changes affect the risk of conflict outbreak, while studies looking at duration, severity and proliferation – the dynamics of conflict – are short in supply.

Environmental changes that have been frequently studied include temperature deviations, rising sea levels and natural disasters such as droughts and floods. Taken together, studies that have looked at how precipitation and temperature affect the risk of conflict onset have come to different conclusions, as summed up both by Buhaug and Theisen (2012, pp.45-46) and

Theisen, Gleditsch and Buhaug (2013, pp.615-619). Table A.1 in Appendix A displays their synopsis of the results on different environmental hypotheses about the way precipitation, temperature changes, natural disasters and other incidences such as water scarcity, land degradation and fluctuations in food prices impacts the risk of organized conflict. The table reveals that 17 studies on the effect of climate factors together yield inconclusive results, while 14 studies can account for weak or some relationship between the indicators. The blame for the inconsistency is given both to the fact that it is rarely the same aspects that are investigated (such as different geographical areas, different times and different analyses/estimations), and also the fact that the indicators studied differ.

Most relevant for this thesis is the seven studies that focus on how natural disasters increase risk of civil conflict. Of these, three find support for the relationship, two find only some support while one finds no relationship and one finds that there is an opposite relationship. In the first group we find Nel and Righarts' (2008, p.197) result that rapid-onset disasters increase the risk of conflict onset, although they "are less confident that [they] have exhausted the factors that determine when and where natural disasters increase the risk of major violent civil conflict". Drury and Olson's (1998) findings also indicate that disaster severity is positively related to the level of political unrest. At the opposite end of the spectrum Slettebak (2012a, 2012b) finds that climatic disasters contribute to *decreased* risk of conflict. He finds that it is not the quantity of disasters that matter, but rather "the main difference is between those who experience disaster and those who do not: the number of disasters that occur (…) appears less important" (Slettebak 2012a, p.174). If one takes into account research that investigates the intermediate effects, for instance whether natural disasters could lead to civil conflict via economic growth (or absence thereof), the results become even more inconclusive. Bergholt and Lujala (2012) conclude that climate-induced natural disasters negatively impacts economic growth – a factor usually associated positively with conflict onset – but that this does not impact the risk of conflict outbreak.

Other important contributions scrutinizing the link between climate-induced hazards and conflict include Gartzke (2012), Theisen, Holtermann and Buhaug (2011-12), Koubi, Bernauer, Kalbhenn and Spilker (2012) and Omelicheva (2011). Weather variability and conflict has been addressed in several studies, and except for the very recent study where O'Loughlin, Linke and Witmer (2014) investigated the impact of temperature and precipitation anomalies on the level of observed violence in sub-Saharan Africa, Theisen, et al. (2013) provides a review of this research.

In a similar manner, Buhaug and Theisen (2012, p.51) call for a widened understanding of conflict, and ask the question "do increasing scarcities and loss of livelihood contribute to intensifying prevalent conflicts or do they increase the prospect for peaceful resolution?". The question address a so far unexplored field of the climate-conflict nexus, namely how climate change affects conflict dynamics, and not just conflict onset. Conflict dynamics involve duration, severity, diffusion, termination and recurrence, and the understanding of how these dynamics interact (if at all) with climate changes is yet at its beginning stages.

Research on climate variability and conflict *severity* is scant. Wischnath and Buhaug (2014, p.13) investigate how loss of food production – a plausible consequence of climate variability - affects conflict severity in India. They find that "a loss of harvest is significantly associated with an increase in severity of fighting during the subsequent year". If this holds also for the rest of the conflict-affected parts of the world, climate variability has the potential to intensify ongoing conflict. Similarly, Rajagopalan (2006) investigates the connection between disaster and ongoing conflict in the three cases Sri Lanka, the Maldives and Kashmir. The case studies indicate that disasters can aggravate existing conflicts. Nardulli, Peyton and Bajjalieh (2015, p.330) investigates the impact of rapid-onset disasters on civil unrest, and their "most important substantive finding is that these disasters have a highly variable effect on civil unrest, particularly violent unrest".

In a similar fashion, many have studied how the 2004-tsunami was followed by intensified conflict in Sri Lanka while peace talks was the result in Indonesia. Le Billon and Waizenegger (2007, p.423) comparatively assert that these two cases confirm "the two main arguments in the literature: disasters can foster political change, and change largely reflects the context in which disasters take place". Beardsly and McQuinn's (2009) analysis of the 2004 tsunami in the Indian Ocean lead them to propose a theoretical framework where the resource admissions and territorial objectives are decisive for how insurgents behave, and consequently whether or not disasters can act as catalysts of peace. Although the tsunami did not lead to peace in and of itself, they found that the subsequent aid altered the insurgent's presumptions, as "the influx of international aid could not be blocked by GAM [the Free Aceh Movement] without harming its relationship to the community" (Beardsley and McQuinn 2009, p.638). The result was that "in exchange for giving up its demands for independence, the Acehnese could gain international legitimacy and the resources linked to this exposure" (Ibid.). The fact that the same tsunami did not act as a peace catalyst in Sri Lanka is attributed to the resource structure of the insurgents. The Liberation Tigers of Tamil Eelam (LTTE) largely relied on financing from Diasporas, and are not reliant on the Tamil community in Sri Lanka in the same way that the GAM relied very much on the local support of the Acehnese. As such, the LTTE is even seen as "war entrepreneurs in that they have incentives to perpetuate the violence and keep remittance flowing" (Beardsly and McQuinn 2009, p.639). In the same fashion Ackinaroglu, DiCicco and Radziszewski (2011) look at how earthquakes[7] affected communal violence in Kashmir and Izmir, Greece. Like Le Billon and Waizenegger, their cases diverge on the outcome – and they find that disasters *can* lead to peacemaking, but it depends on the citizens' attitude.

In terms of *duration* exclusively, Ghimire, Ferreira and Dorfman (2014, p.622) investigate how displacement caused by floods affect civil conflict. By use of flood data from the Dartmouth Flood Observatory (DFO) and the UCDP/PRIO conflict dataset, they find that "mass displacements caused by large, catastrophic floods increase the probability of continuation of existing conflicts, rather than contributing to the emergence of new conflicts" (Ghimire, Ferreira and Dorfman 2014, p.622). Contrary to this, Kreutz (2012, p.498) – empirically assessing more than 400 disasters in 21 countries – finds that "there is an increased probability that talks are initiated and that ceasefires are concluded following natural disasters, but that there is no similar effect on peace agreements". He attributes this apparent window of opportunity to redeployment of disaster relief, changing the government's priorities, and not because people change their attitudes in the aftermath of disaster.

Summing up then, it is clear that the research on climatic disasters and armed conflict is scarce, and that the research that does exist has failed to produce conclusive results. One reason for this might be the fact that almost all the existing studies inscribe their findings to various intermediate effects. There is unfortunately a duality in this; on the one hand it is fairly well established that disasters, and particularly those susceptible to climate changes, do not directly increase the risk of armed conflict, but one the other hand, all the possible intermediary effects are hard to measure and the existing results are hard to compare. With the duplexity of the empirical findings, an investigation on how climatic disasters affect the chances of conflict resolution seems overdue, and this thesis will serve to fill at least part of the gap in the literature.

[7] Granted, earthquakes are not climatic disasters. The lack of research on climatic disasters and conflict duration does however warrant including the studies on geological hazards in this section.

2.4 Other drivers of conflict duration and resolution

Notwithstanding the direction of the relation between climatic disasters and the duration of armed conflict, there are other factors likely to affect this relationship. The main debate within the research on conflict duration has centered on ethnic fractionalization. However, more attention has more recently been attributed to the insurgents themselves and the dyadic relationship between rebels and the government (Cunningham, Gleditsch and Saleehyan 2009, Wucherpfennig, Metternich, Cederman and Gleditsch 2012). Collier, Hoeffler and Söderbom (2004) point to the structural conditions in the affected countries as well as how the conflict evolves, in determining the course and duration of the conflict. An important structural factor that favors insurgency is state weakness, or state capacity as DeRouen Jr. and Sobek (2004) calls it. It is conceivable that a state's capacity also will influence whether – and how – a disaster alters the conflict dynamics. In terms of the climate-conflict nexus, the more accurate term would probably be *state vulnerability* rather than state capacity. A vulnerable – for whatever reason – state will most likely be further weakened by a severe climatic disaster, and it might boost the incentives for rebel groups to continue, or even intensify fighting and fuel existing grievances if the disaster-relief is not satisfactory. An eventual influx of (foreign) aid might escalate the situation further, and be attractive both for a weak state and for the insurgents.

Defining state vulnerability could be done in a variety of ways, as a state's capacity encompasses numerous aspects and the literature provides many different particulars on this. Busby, Smith, White and Strange (2013, pp.133-134) point to the importance of "recognizing where physical exposure to climate change conjoins with other dimensions of vulnerability", and define the (subnational) vulnerability to climate change "as situations in which large numbers of people are put at risk of mass death as a result of exposure to climate-related phenomena". Thus, it is not only the physical exposure to these hazards that matter, but also political and demographical factors. As for the first, the stability of the regime – in terms of how long the regime has persisted – is likely to influence the post-disaster situation, arguably more than whether the country is a democracy or not. There is no reason why a stable autocracy should not be as prepared as a democracy for a disaster and might handle its consequences just as efficiently – or the opposite. A new state is likely to face more insurrection in the aftermath of a conflict than an old one simply because it takes less for the balance of power between the insurgents and the state to change than for an older, more inveterate state.

In the same matter, the better the economic situation in the country, the better prepared and better equipped the state is for coping with a disaster, as "poor countries are also, in general, less able to prevent severe weather events from turning into disasters" (Slettebak 2012a, p.169). Even the UNISDR- definition (2007) of hydrometeorological hazards takes direct consideration of the possible economic ramifications following disaster. As is the case in a durable regime, economic prosperity usually means better infrastructure and accordingly also better disaster-coping mechanisms. It follows logically that the smaller the impact of the disaster, the lesser its effect in terms of either prolonged or shortened conflict. Poverty is usually measured by GDP per capita. However, this measure says nothing about the distribution of the income, meaning that even though a country might have a high GDP capita, the general population might still be very vulnerable to natural hazards. A country's infant mortality rate (IMR) has been used in several similar analyses (among them, Nel and Righarts 2008, Buhaug and Theisen 2012 and Ghimire et al. 2014), and is thought to capture both economic disparity and overall development. It is reasonable therefore to assume that the IMR will influence how a disaster affects a community, or how the community is able to respond to disaster.

As for the demographic part of the state vulnerability, "disasters happen only where people live, so severe weather events in uninhabited areas will not turn into disasters" (Slettebak 2012a, p.168). Therefore, the population (size) influences how a climatic hazard will manifest once it hits. The more people that live in a country, the more people are affected by a disaster – increasing the likelihood of both human and material destruction. In addition, a large population means more people to please, making the aftermath of a conflict different in a densely populated country than in a populous one where more people need aid and more people are fighting over scarce resources.

2.5 Implications for theory on disasters and conflict resolution

From the above then, it seems clear that the theoretical considerations can be cited in support for two contradictory propositions. On the one hand climatic disasters can contribute to increased risk of armed conflict, but on the other hand they might produce the silver lining of laying the groundwork for conflict resolution. The existing research presented in Section 2.3 also proved ambiguous, as the lack of research on conflict dynamics and climate variability is remarkable. Nevertheless, it is not unfair to assume that even if disasters do not *lead* to conflict, disasters striking a conflict-ridden area might change the conflict dynamics, be it severity, diffusion or duration. Apart from Kreutz (2012) and the very recent – and similar to this – analysis by Ghimre et al. (2014), research on hydrometeorological disasters and conflict duration/termination is nonexistent. The fact that these two yield contradictory results testifies to the perplexity of the subject.

Looking at the theoretical assumptions then, Homer-Dixon's conception of environmental scarcity undoubtedly concerns how climatic disaster might affect the risk of conflict onset. It does not shed much light on the effect on conflict dynamics beyond the general assertion that the factors influencing conflict onset, conceivably can have the same directional effect on duration, to the extent that there is an effect. Looking at Nel and Righarts (2008) contribution, it provides very useful distinctions in terms of separating between the immediate and long-term effects of a disaster. Nevertheless the framework is not the most parsimonious, and it is difficult to test (quantitatively) whether their specific preconditions necessary for a conflict to break out actually arise in the aftermath of a climatic disaster. Looking at the empirical evidence, the studies finding that disasters affect conflict dynamics in a positive relation, rely on intermediary effects, be it loss of food production or mass displacement. The conditions that need to be fulfilled are many, and the knowledge on the climate-conflict connection is feeble. Thus, concluding that, based on the idea that climatic disasters might increase the risk of conflict onset, climatic disasters will prolong existing conflicts, makes for an erroneous inference.

That being said, the theoretical arguments assuming the opposite prediction are far more applicable in terms of how disasters impact conflict dynamics, and specifically conflict termination. Of course, there is only one of the cases studied so far where a natural disaster did lead to a peaceful outcome in intrastate armed conflict, but since there are also very few studies with the opposite conclusion, the lack of results cannot be taken as a confirmation that the latter is the case. This is particularly true because there are several plausible theoretical arguments for why natural disasters can provide a ripe moment for conflict resolution. The understanding that a climatic disaster has the effect of changing the conflict-scene has important implications for the policy debate, and the attention has lately also been turned to this. The idea that disasters undermine previous differences makes it possible to infer that if a disaster could act as a catalyst of peace in Aceh, it can do so other places as well – even if the initial situation is different. Needless to say, one must be careful to keep in mind that the idea of ripeness risks a tautological pitfall. If the conflict is already ripe for peace talks, is the disaster really "necessary" for the conflict resolution? Bridging this with the idea of focusing events however seems reasonable. If the conflict is ripe and a focusing event (i.e. disaster) occurs, a climatic disaster can act as a catalyst for peace – regardless of whether it is the attitudes of people that change or the focus/priorities of the government.

In line with the recent research on conflict duration generally, disasters can be seen to affect the conflict dynamics by influencing the warring parties differently. As Cunningham, et al. (2009, p.6) points out, the capacities of the government and the rebels are not likely to be symmetrical, and "the decision to resort to violence will hinge on an actor's vulnerability to attacks from the other party to the conflict". An alteration of the capacities of the actors involved will affect the course of the conflict, and a climatic disaster can presumably provide such an alteration. Buhaug et al. (2009) also assert that rebel capacity is crucial in determining the outcome of conflict. If the rebels are hit hard by a climatic disaster, their capacities are weakened and they will be both easier to defeat for the government, while at the same time consider peace talks the most beneficial course of action.

In this chapter the prevailing assumption that the increment of climate-induced disasters will lead to more armed conflict and more war has been challenged, and the considerations above warrant at least a modification of this rather common conception. In line with Slettebak (2012a, 2012b) Gartzke (2012) and Kreutz's (2012) findings, I propose a hypothesis that is contradictory to the popular assumption, namely that;

H_1: armed intrastate conflicts that are affected by climatic disasters experience increased risk of impending conflict resolution.

3. Research methods

In order to answer the research question, and to be able to test the hypothesis laid forward in the previous chapter, I carry out a survival analysis, analyzing how the contingency of termination of armed civil conflict is affected by abrupt hydrometeorological disasters. In line with the lion's share of the research on conflict and climate variability, the analysis has a quantitative design. The advantages of doing a quantitative analysis are several. First, the transferability of the eventual results is greater in a quantitative analysis than in a qualitative study. In addition, the quantitative design makes it possible to cover a broader range of cases, both in space and in time. The analysis covers all intrastate armed conflicts in the world between 1985 and 2007. Needless to say, a qualitative study would not be able to cover such a span, and even if significant relationships fail to appear, the quantitative approach still allows for time- and space-trends to be uncovered.

Studying the duration of conflict can be done with a handful of different analyses. For instance it is possible to do an Ordinary Least Squares (OLS) regression with the duration of each conflict as the dependent variable. However, this poses problems with the underlying assumptions of the method. In order to be able to generalize the results, the OLS-approach holds an assumption that the residuals have to be normally distributed. Since all durations are positive, i.e. the time to conflict resolution (failure) is always positive; the distribution of the residuals will not be normal, and most likely nonsymmetrical, resulting in accordingly biased findings. In addition, because several conflicts are still ongoing at the end of the analysis-period, it is not the case that all the observations (conflicts) in the dataset have actually failed. This is called right censoring and poses a problem in the OLS-estimation. For these reasons, survival analysis was chosen, and more specifically a parametric survival model using the Weibull distribution will be performed.

The parametric model is better than a semi-parametric one because the analysis contains several predictors, the former being intended primarily for binary models. In addition, there are several periods where there is no failure (conflict termination), something that yields information in a parametric model, but not in a semi- or a non-parametric model (Cleves, Gutierrez, Gould and Marchenko 2010). The Weibull distribution is a "standard model to capture the duration dependence of civil conflict" (Buhaug, Gates and Lujala 2009, p.559) because it assumes a baseline hazard rate that is allowed to grow (i.e. not be constant). Several studies use this type of analysis type in investigating the duration of conflict, among them Fearon (2004), DeRouen and Sobek (2004) and Buhaug, Gates and Lujala (2009).

Despite using the same model, this analysis differs in terms of its level of analysis. While most other analyses measure duration in years, and sometimes days, I measure conflict duration in months. This constitutes an improvement compared to the yearly-measure because of the rapid-onset characteristic of the disaster indicators. Measuring the duration in months is more precise than years, whereas a daily measure is more serviceable in studies where the data is geocoded. The survival analysis measures the months until failure – in this case conflict termination – and the climatic disasters are regarded as a *treatment*. The model then compares the duration of the conflicts that did not receive the treatment, i.e. experienced a climatic disaster, with the conflicts that did in order to find where the risk of termination is greatest.

The disaster indicators used in the analysis are floods, waves/surges, windstorms, extreme temperatures and landslides. The latter is not directly classifiable as hydrometeorological, but is certainly climate-related. Landslides are usually triggered by heavy precipitation and exhibit the same properties as the other hazards with respect to incidence and possible effects on the community. In order to make the analysis possible, data was gathered from several sources, and below each of the original datasets are described in more detail. Then the new dataset is then presented with a more thorough review of the variables used in the analysis, before a note on the challenges and limitations of the data construction rounds up the chapter.

3.1 UCDP/PRIO ACD

The UCDP/PRIO armed conflict dataset comes from the Uppsala Conflict Data Program (UCDP), the version being v.4-2014a. The dataset lists all armed conflicts in the world between 1946 and 2013. Included from the dataset are all "internal armed conflicts (…) between the government of a state and one or more internal opposition group(s)" (Themnér 2014, p.9) – both those with and those without intervention from other states – between 1985 and 2013[8].

The unit of analysis in this dataset is county-years, and consequently each observation corresponds to a year of conflict in each country. In order for a conflict to be recorded, at least 25 battle-related deaths must have taken place within that calendar year (Themnér and Wallensteen 2014). To be able to merge this data with the data on climatic disasters and the

[8] Since the disaster data are only available from 1985 onwards, the years of conflict before 1985 are excluded from the dataset. Data on climatic disasters from before 1985 do exist, but because this data is largely unreliable compared to the data after 1985 it has not been included in the analysis.

control variables, I constructed a unique id-variable that would make it possible to merge the different datasets. This unique variable became a combination of the three variables country, year and month. However since there is often more than one conflict in the same country at the same time, the possibility for a 1:1 match is impeded, and the country-year-month approach could not be used on the conflict data. Therefore, the other datasets were first merged together, having country year and month as the unique identifier. The joint disaster dataset was then merged into the UCDP-dataset where each country-year had been expanded to make the unit of observation conflict-months, based on an unique conflict id (the incompatibility) rather than country.

The monthly unit of observation furthermore means that if conflict takes place at least one day of the month, it is a conflict-month. Consequently a conflict starting mid January and ending the first of February, is coded as two months of conflict. In line with Rustad and Binningsbø's (2012) study of natural resources and duration of armed conflict inter alia, I applied the coding-rule predicating that a conflict break lasting 24 months or less should be coded as one continuous conflict instead of two separate conflicts (Gates and Strand 2006, p.10). In practice this means that for example conflict number 1-333 (UCDP ID-code), an Ethiopian conflict coded as ending in August 2004 but where fighting resumed in August 2006, have been coded as a continuous conflict over that time span. The coding-rule was applied in 44 conflicts.

The alterations of the dataset produced a dataset of 211 conflict episodes, amounting to 11262 conflict months in 81 countries between 1985 and 2011[9]. Although the UCDP/PRIO dataset covers the years until 2013, December 2011 is set as the final month of conflict in order to be able to secure consistency with regards to the two-year rule. The average duration of the conflicts between 1985 and 2011 is just above five years, at 61.4 months[10], with 29 conflicts still ongoing in December 2011. There are six conflicts that last the entire sample period, i.e. 324 months. These are the conflicts in Afghanistan, Columbia, the Philippines, Sudan, Turkey and Uganda. India and Myanmar have the highest number of conflict months, at 1480 and 822

[9] I have disregarded the time-aspect of countries. Namely, I have treated all countries as tough they have always existed. This pertains in particular to the earlier Soviet countries, which are coded as Russian Federation for the years before 1989. The same is true for the Balkan countries. As it is the state, and not the particular geographic location of conflict and disasters that are in focus, this simplification seems reasonable.

[10] Of course, the average is very sensitive to the extremes, and 31 of the conflicts only lasted for a month. The tables in Appendix C show several tables and histograms concerning conflict duration and distribution for the different time periods analyzed.

conflict months each. These are also the two countries with the highest number of unique conflicts over the time span analyzed; India experiencing 18 different conflicts and Myanmar 20.

3.2 DFO Floods

The dataset was gathered from Columbia University's Dartmouth Flood Observatory[11]. All floods in the world since 1985 are listed here, and the unit of analysis is floods, meaning that each flood event is listed as one observation, regardless of how many countries the flood hit or how long it lasted. There are no inclusion criteria in this dataset other than the fact that the flood has to be *reported somewhere*, and the dataset is "derived from a wide variety of news and governmental sources" (Brakenridge 2014). Some of the floods are recorded as *large* according to certain (rather loosely defined) criteria, but I have included all floods listed and thus maintained the exogeneity of the data.

There were originally 4187 unique flood events in the dataset. I expanded all the observations that had another country listed on the *other*-variable, but the lion's share of the floods that hit more than one country, hit more than two countries. Therefore, I manually checked all the observations and their respective locations. From this, the observations were expanded, such that instead of unique floods being the unit of analysis, country-floods became the unit of analysis. The total number of country-floods turned out to be 4916.

Then, in order to merge the flood data with the other two datasets the dataset had to be adapted so the unique identifier became country-flood months. Therefore, every flood (in every country) that lasted for more than one month was expanded according to how many months the flood lasted. A two-month long flood striking two countries simultaneously would thus result in four flood-months, two in each country. As long as the flood lasted at least one day in a respective month, it has been coded as a flood-month. This resulted in a dataset containing 6447 months of flood in 186 countries. The most flood-prone country in the sample was USA with slightly less than 8% of the total number of flood-months, closely followed by China at 7%. The next five on the list is India, Indonesia, the Philippines, Vietnam and Bangladesh, indicative of the fact that the distribution of floods across the world is somewhat concentrated.

However, for the merge to be possible there could only be one observation per country-month

[11] The dataset is available from http://floodobservatory.colorado.edu/Archives/index.html

of flood. This was not the case with the 6477 flood-months, as sometimes more than one flood occurs in the same country in the same month(s). A duplicate variable was therefore constructed, making it possible to remove all "redundant" floods, while at the same time generating a count variable recording how many floods that occurred in any given month. This left a dataset with 4913 country-months of flood before the merger.

3.3 EM-DAT

The last disaster-dataset is the EM-DAT dataset provided by CRED. Getting hold of these data proved very difficult, despite the availability of interactive data on the project's web page. I was able to secure a version containing data on emergency events for all countries from 1980 to 2007. On their website, CRED offers an interactive synopsis of all events up to today, and I considered adding the events from 2008 to 2014. For all that, I quickly discovered that manually adding 1884 events with all its appurtenant information would take more time than what was serviceable.

The first thing I did with the data was to eliminate events that were out of the scope of this thesis, such as earthquakes and droughts[12]. This left the following hazards in the dataset:

- floods
- windstorms
- extreme temperatures (separated between heat waves and coldspells)
- waves (including tidal waves and surges)
- landslides

As previously mentioned, landslides are not directly classified as hydrometeorological hazards, but UNISDR defines that such hazards are an important trigger factor in these events, and so they are included in the analysis.

For a disaster to appear in the dataset, it has to meet at least one of the following criteria; (1) ten or more people are reported killed, (2) hundred or more people are reported affected, (3) a state of emergency is declared or (4) a call for international assistance is made (CRED 2009a). This restricts the disasters that are included in the dataset, and also it means that the disasters are endogenous to the conflicts, as the effect of a disaster will be affected by the conditions where the disaster hits. A society in conflict will most likely be more vulnerable/exposed to a climatic disaster than a society that is not dominated by conflict, and

[12] This is principally because of the slow-onset characteristic of droughts and the non-climatic aspect of earthquakes.

two floods with identical force might not both be recorded in the dataset if they do not have the same impact on the two societies. It is plausible to assume that the same flood would result in more causalities if it hit a conflict-ridden area, than if it hit a community where the infrastructure and the emergency preparedness is better than in a conflict-area in the developing world. Hence, when the CRED sets death tolls as one of the inclusion criteria, whether or not a flood is registered in the database becomes dependent on the pre-disaster situation.

The EM-DAT data originally records each disaster event occurring in each country, and the data had to be expanded so that each month of disaster became the unit of observation. In order not to lose too many events, the dataset was split in six parts, corresponding to the six disaster types mentioned above. Thereafter the duplicates were recorded in each dataset along with a count variable. This method secures that if different type of events took place in the same country in the same month, both events are preserved. Each dataset thus records the incidence of each hazard, and also contains a count variable, recording whether more than one disaster (of the same type) took place in the same country the same month.

Before the data split, 6106 events were recorded in the dataset comprising of 7021 hazard-months. After the split and after removing duplicate months, I was left with 5746 months of EM-DAT disasters; some containing more than one type of hazard, but still securing that there was only one observation for each month in any one given country. Figure I depicts the distribution of the different hazards[13], making it clear that the number of flood months surpass all the other events taken together. The splitting of the event types meant that information on the disasters' impact, such as the number of people affected by the event was lost when the data was merged. However, the loss of this does not make up for the advantage of splitting the events, as well as the fact that the impact variables in the original dataset were largely inadequate with many missing observations and inconsistent coding.

[13] These observations are not to be confused with the distribution of disasters in the final dataset. Since each of the EM-DAT datasets are merged into the UCDP/PRIO dataset some of these events are discontinued, while others are multiplied. The latter is the case when there is more than one conflict going on at the same time in the same country. An example is the wave that hit India in August 1997. Since there were 8 different conflicts in India that month, the wave is recorded as 8 observations in the final dataset.

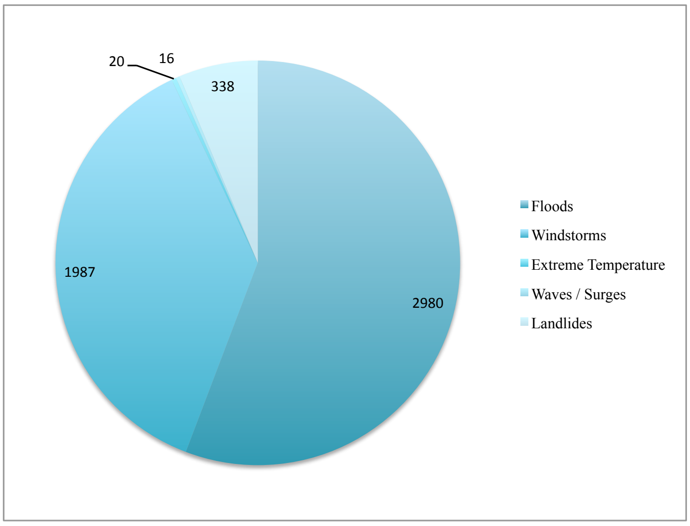

Figure I. Pie chart showing the distribution of the EM-DAT hazards.

3.4 The conflict/climatic disaster dataset

To make the merger of the disaster datasets and the subsequent conflict dataset more tractable, I made a frame dataset that each disaster-dataset was merged into. In the frame the unit of observation was months in every country between 1985 and 2013 and the result was a dataset with 348 months covering 29 years in 193 countries[14]. First, the disaster datasets was merged into the frame, before the control variables – that will be presented below – was merged in. Then, this new dataset was merged into the UCDP dataset, before the data was prepared for the Weibull survival analysis. That meant that only the months containing conflicts were kept, and censor and duration variables were made, something that resulted in a considerable decrease in the number of disaster-months[15].

Because the DFO data begin in 1985 and the EM-DAT data only extend to 2007, the time-span was duly reduced. The final dataset contains 9754 observations, each observation corresponding to a conflict month between 1985 and 2007. These 9754 observations amount to 192 continuous conflict episodes, with 34 ongoing conflicts in 76 countries. 28 of the conflicts began prior to the sample (i.e. before 1985), and these are listed in Table C.4 in Appendix C. From the list it is clear that the longest lasting conflict is the Palestine

[14] The list of countries is taken from the International Organization for Standardization's ISO-coding, but based on the UN member states.
[15] At the same time, disasters taking place in months with more than one conflict in the same country were accordingly increased.

insurgency in Israel beginning in 1949. The average duration of the conflicts between 1985 and 2007 is 58.9 months, slightly less than five years. From the median duration at 16 however, it is clear that the outliers heavily influence the average duration. These are the conflicts with the shortest duration (1 month) and the conflict that last the longest (throughout the whole sample period).

India is the country in the sample with the most conflict-months at 1306 observations dispersed on 16 different conflict-episodes. On second place we find Myanmar, with 718 conflict-months, but on average the conflicts in Myanmar last shorter than in India as there are 18 conflict-episodes here, two more than India's 16. The Philippines has the highest number of conflict-months per conflict, at slightly more than 175 conflict-months per conflict, more than double the number in India. The overall time trend reveals that the number of conflict-months has experienced some ups and downs, but the overall picture shows a decline in the number of conflict-months between 1985 and 2007, from 445 to 372. The histogram for the entire period can be found in Appendix C.

The dependent variable

The dependent variable in the survival analysis is the duration of armed civil conflict. The variable is made up of the three variables *c_startnd*, *c_endnd* and *c_status*. The first records the duration (in months) of the conflict up until the start of the observation. That means that the first observation in a conflict is equal to 0 while the second month is recorded as 1 (i.e. before the second month the conflict had lasted for 1 month). The variable *c_endnd* records the duration of the conflict – in months – such that the first month equals 1, the second 2 and so forth. The third variable, *c_status* documents whether the conflict ends or not, and it thus records whether failure takes place. Only the observations reporting the last month of conflict are recorded as having 1 on the *c_status* variable. Thus, any conflict that was still ongoing at the end of December 2011 will only have 0s on the *c_status* variable, indicating that the conflict did not end (as defined by the two-year coding rule).

Table I below lists the descriptive statistics for the variables that are used in the analysis, and their coding is also presented in more detail below. A more description of all the variables in the dataset can be found in the variable list in Appendix G.

The independent variables

The independent variables, or the treatment variables, are the disaster indicators recording whether or not a hydrometeorological hazard occurred each month of conflict. The first variable *f_flood* records whether or not a flood – as recorded by the Dartmouth Flood Observatory – occurred in any given (conflict-) month. There are 1791 conflict months containing floods between 1985 and 2007 in 76 countries[16]. 876 of these are unique floods. India and the Philippines are the most flood-prone countries with 611 and 227 conflict-months of floods recorded by the DFO respectively. However, looking at how many conflicts are going on in each country, the Philippines has far more unique months of flood as there are only three conflicts in the country, while there are 16 in India. This makes the average flood-months per conflict more than 75 in the Philippines, and "only" 38 in India. Appendix D shows the distribution of hazard-months for all the different hazards, both by country and type.

The next six indicators are all based on the EM-DAT data, recording extreme events between 1985 and 2007. Each of the disaster events is coded as individual dichotomous variables assuming the value 1 in the conflicts months that the event occurred. The variable names for all these start with *ed_* to indicate their origin, followed by the specific disaster type. In addition, the *ed_all* variable records *any* EM-DAT disaster, assuming the value 1 if at least one of the indicators occurred in the given month and 0 if no event occurred. The number of 1s on the *ed_all* variable is less than the sum of all the *ed_* variables as sometimes more than one disaster took place at the same time.

There are a 461 fewer flood-months originating from the EM-DAT dataset than from the DFO dataset. The pattern is nevertheless the same, with India and the Philippines as the two countries with the highest occurrence of all the EM-DAT indicators apart from coldspells, where India still has the most, but this time followed by Russia and Bangladesh. Looking at the time trend, it is clear that the incidence of all disaster indicators has increased since 1985, but as the sample only covers those months in those countries that are experiencing an armed civil conflict, no great inferences can be drawn from this. However, it is interesting to note that over the same period of time, the number of conflict-months has decreased. Appendix C and D contain several histograms displaying the incidence of conflicts and disasters over the period.

[16] The reduction of the number of floods from the encoding of the DFO-dataset when merging it onto the UCDP-PRIO dataset is clear here, and is a result of the fact that many of the floods took place in countries that was not affected by internal conflict and the time constraint imposed by the EM-DAT data.

Table I. Descriptive statistics over the variables included in the analysis, 1985-2007.

Variable	Obs	Mean	St. Dev.	Min	Max	Frequency 0 (%)	Frequency 1 (%)
Flood (DFO)	9754	-	-	0	1	7,963 (81,64)	1,791 (18.36)
Flood (EM-DAT)	9754	-	-	0	1	8,424 (86.36)	1,330 (13.64)
Windstorm	9754	-	-	0	1	9,128 (93.58)	626 (6.42)
Heat wave	9754	-	-	0	1	9,656 (99)	98 (1)
Cold spell	9754	-	-	0	1	9,648 (98.91)	106 (1.09)
Wave/surge	9754	-	-	0	1	9,738 (99.84)	16 (0.16)
Landslide	9754	-	-	0	1	9,469 (97.08)	285 (2.92)
All disasters (EM-DAT)	9754	-	-	0	1	7,679 (78.73)	2,075 (21.27)
Flood occurrence the past 6 months (DFO)	9754	-	-	0	1	6,016 (61.68)	3,738 (38.32)
Flood occurrence the past 6 months (EM-DAT)	9754	-	-	0	1	6,489 (66.53)	3,265 (33.47)
Windstorm occurence the past 6 months	9754	-	-	0	1	7,938 (81.38)	1,816 (18.62)
Heat wave occurence the past 6 months	9754	-	-	0	1	9,355 (95.91)	399 (4.09)
Coldspell occurrence the past 6 months	9754	-	-	0	1	9,344 (95.8)	410 (4.2)
Wave occurrence the past 6 months	9754	-	-	0	1	9,686 (99.3)	68 (0.7)
Landslide occurrence the past 6 months	9754	-	-	0	1	8,662 (88.8)	1,092 (11.2)
All EM-DAT indicators the past 6 months	9754	-	-	0	1	5,568 (57.08)	4,186 (42.92)
Regime durability	9754	17.006	24.165	0	191	-	-
Infant Mortality Rate (ln)	9754	4.311	0.697	1.504	5.365	-	-
Population (ln)	9754	17.184	1.689	12.850	20.797	-	-

ln refers to the natural logarithm.

Time-lagged indicators

Lagging the indicators or the predictors is a frequently used tool in time-series. This is done to see whether the effect of the indicator appears only after a certain time – a month or a year for instance. In this analysis the indicators are not directly lagged, but in order to look at how the effect of disasters is influenced by the time passed since the disaster occurred, variables recording whether or not each disaster took place in the past six months was constructed. To avoid problems of multicollinearity – where the independent variables are not independent, but co-vary substantially – the lagged indicator variables are tested in separate models.

Control variables

The control variables included are those that can argumentatively affect the relationship between disasters and the risk of conflict termination. The first control variable is the *durable* variable from the Polity IV dataset as presented by Marshall and Jagger (2002). The variable measures the regime durability in years, and more specifically "the number of years since the most recent regime change (defined by a three- point change in the (…) score over a period of three years or less) or the end of transition period defined by the lack of stable political institutions" (Marshall and Jagger 2002, p.16). In other words, it records the number of years the regime has lasted up until the year before conflict breaks out. To avoid problems of endogeneity (i.e. that the conflict affects the value of the variable), the variable records the duration of the regime in the country as it was the year before the conflict broke out. This means that if a conflict broke out in 1989, the *durable* variable measures the amount of years the appurtenant regime had lasted in 1988.

To account for severe allocation inequality, the variable *IMR* measures infant mortality and records "the number of infants dying before reaching one year of age, per 1,000 live births in a given year" (World Bank 2015). The general implication is that the higher the IMR, the poorer the country. The data is gathered from the UN Database, and records the IMR the year before conflict outbreak. As this variable has a rather large dispersion of values, the variable used in the analysis has been logarithmically transformed.

The final control variable is population. Predictably, the population variable records the population in the country the year before conflict breaks out. This means that for Afghanistan for instance, all the observations record the population in the country in 1977, as 1978 was the first year of the conflict in Afghanistan. To counter the extreme dispersions on the population variable, I have taken its natural logarithm. The population data is gathered from the World Bank DataBank and Geoba.se.

Several variables were tested in the model-construction but were due to absence of effects left out of the final models. These include count variables of the number of conflicts and hazards in each conflict month. Also some conflict specific variables such as intensity and incompatibility were tested, but showed no significant effect. A list of all variables in the dataset can be found in Appendix G.

3.5 Changing the definition of conflict

To ensure consistency of the results, I performed an identical analysis on a dataset employing a different definition of armed conflict. Instead of using the 25 battle-related deaths as the criterion for conflict, I applied the UCDP-PRIO war-threshold, prescribing that for the conflict to be considered a war, there needs to be at least 1000 battle-related deaths within a calendar year (Themnér 2014, p.8). As this variable was already a part of the UCDP-PRIO dataset, the war-dataset consists of those conflicts where *c_war*[17] assumes the value 1. With this criterion, the dataset contains 21 different wars in 16 countries between 1985 and 2007. The longest lasting war in the dataset is the Afghan war, beginning in 1978 and lasting throughout the dataset, followed by one of two wars in Sri Lanka that started in 1975 and ended in 2001. A list of the wars, the appurtenant durations and descriptive statistics can be found in Appendix E. The war-dataset contains 1268 conflict-months between 1985 and 2011, but due to the constraints imposed by the disaster data, only 1141 of these are included in the analysis that extends from 1985 to 2007. There are four wars that are still ongoing in December 2007; namely the wars in Afghanistan, Iraq, Pakistan and Sri Lanka.

3.6 Challenges

The construction of the dataset engendered several challenges, of which this section surveys the most important ones. Firstly there were surprisingly many observations where the location and country did not match in the DFO dataset. In these instances I kept to the detailed location, and changed the country correspondingly. I did double-check every observation that seemed suspicious, and ended up changing the country in 23 cases[18]. As might be expected, it was beyond the scope of this thesis to double-check *every* location and its corresponding country, so this might still be a source of error. In the same fashion, 52 events in the EM-DAT dataset did not have a date, and were consequently left out of the analysis.

Another limitation is the information loss following the merge of particularly the EM-DAT disasters. Because there could only be one of each disaster event in each country in any given month, information on location, entry criteria and maybe most unfortunately the number of affected people had to yield. However, the fact that particularly the impact variables regarding people affected and people killed as a result of the disasters was rather insufficient, and would have drastically decreased the number of valid observations in the regression serves as a consolation. Moreover, it appeared that the EM-DAT data on waves included waves/surges originating from both geological and climatological factors, despite their stating that the

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[17] A dummy based on the intensity variable in the UCDP-PRIO dataset, assuming the value 1 if the intensity was coded as 2 (war) and 0 if it was coded as 1 (minor conflict).
[18] A list of these, and the events removed from the EM-DAT dataset, can be found in Appendix B.

waves included are hydrological only. Therefore, the variable was manually checked, and the tsunamis originating from earthquakes were excluded to make the variable in agreement with the climatic perspective applied.

Furthermore, I considered including GDP per capita as a control variable to capture even better the economic situation in the countries included. However, the correlation between the logarithmically transformed GDP capita-variable and the IMR variable was as high as 0.7. and consequently only the IMR was kept.

Summing up then, all the manual coding has probably expand the error margin quite a bit, in addition to taking more time than estimated. Having sais that, I have gone over the dataset a number of times, and by doing so I believe I have minimized the errors to any amount possible. Furthermore, the possible multicollinearity problem that was pointed out in the previous section was also dealt with by creating separate regression models for the disaster indicators. This is also why the DFO floods are in a separate analysis than the rest of the indicators. It is fair to assume that most of the floods from EM-DAT are covered in the DFO register, making it impossible to argue that they are independent and use them in the same model. Finally, problems with missing cases are more or less eliminated, as all the indicators in the analysis cover the whole period.

4. Empirical analysis

This chapter examines the survival models presented in Tables II–IV The models display the hazard ratios for each predictor, and the interpretation of the coefficients are as follows; the hazard ratios give the ratio of the hazard that the conflicts experiencing the treatment – i.e. experience a climatic disaster – ends, to the hazard that the conflicts not experiencing the treatment ends. Thus, if the hazard ratio equals one, the two groups have the same hazard of conflict termination, and it makes no difference if a disaster occurs or not. If the hazard ratio is above 1, the hazard of conflict termination is greater in the treatment group, and opposite if the hazard ratio is below 1. As Lujala and Bergholt (2012, p.7) so accurately describes, "the distribution of natural disasters across countries probably is non-random", but since the models are clustered on countries, they yield robust Z statistics, and the problem of unobserved heterogeneity is mitigated. The models were also specified as Cox proportional hazard models, but as the results were similar to the Weibull models, the parametric model was preferred – in line with Gates and Strand (2006, p.30). This chapter first presents the results for the force-based indicators, followed by the consequence-based EM-DAT disasters. The models from the war-dataset then rounds up the chapter.

4.1 The force-based indicator

Table II shows two models testing the effect of the only force-based disaster indicator included in the analysis, floods from the DFO dataset. In Model 1 a binary measure of the occurrence of floods in each conflict month is the disaster indicator, while Model 2 employs a measure where the predictor assumes the value 1 if a flood occurred within the past six months, and 0 if that is not the case. In both models the lack of significant coefficients is striking, and the only significant effect is the population variable's effect on conflict termination. Nevertheless, the flood variable has a hazard ratio below 1 in the first model, indicating that floods might actually prolong the conflict duration in that conflicts experiencing floods have a lower chance of termination than conflicts where no flood occurred. This goes against the hypothesis laid forward in Chapter 2, as the hazard ratio for floods state that the conflicts experiencing a flood the current month have a 28% lower chance of ending than the conflicts not experiencing a flood, although not a significant finding.

Looking at the control variables, the number of years the regime has lasted has no impact on the chance of conflict termination (hazard ratio of 1), while the higher the infant mortality the lower the chance of conflict termination. As mentioned, the population variable holds the only

Table II. Predicting the hazard of conflict termination using the DFO Flood data.

	(1)	(2)
Flood occurrence	0.782	
	(-1.02)	
Flood occurrence the past 6 months		1.341
		(1.46)
Regime durability (years)	1.000	1.000
	(0.11)	(0.13)
Infant mortality rate at onset (ln)	0.915	0.934
	(-0.76)	(-0.57)
Population (ln)	0.883	0.840
	(-2.56)**	(-3.43)**
Constant	1.544	3.169
	(-0.49)	(1.26)
Log pseudolikelihood	-330.0	-329.5
Number of conflicts	192	192
Number of failures	158	158
Observations	9754	9754

Estimates show Weibull hazard rates. Robust Z statistics, clustered on countries, in parentheses. * and ** denote significance at respectively 95 and 99 percent confidence levels.

significant effect in the model, positing that the larger the population the lower the chance of conflict termination.

Model 2 displays largely the same results as regards the control variables; the coefficients are fairly similar and the population variable is the only significant one. When it comes to the main predictor, namely floods, Model 2 differs substantially from Model 1. When using a binary measure for whether a storm occurred the past six months, the hazard ratio assumes a value above 1, indicating the opposite effect than found for the monthly measure in Model 1. The prospect for conflict termination is now 34,1% higher for the conflicts that *did experience* a flood the past six months than for the floods that did not. Although not significant, the main finding from Models 1 and 2 is that the time-span appears to change the effect of a flood on conflict duration. As the time-span is not stated in the hypothesis and because of lacking significance, the models neither confirm nor dismiss the hypothesis laid forward in Chapter 2.

4.2 The consequence-based indicators

Models 3 to 6, displayed in Table III all include different measures of the consequence-based disaster indicators derived from the EM-DAT dataset. There are six indicators in addition to an aggregated variable capturing the occurrence of any type of disaster. To avoid multicollinearity, the latter measure is estimated in separate models. Model 3 displays six binary variables capturing whether a flood, a windstorm, a heat wave, a coldspell, a wave or a landslide occurred the given month. The hazard ratio for the floods are fairly similar to the one found for the DFO floods in Model 1; below 1 indicating that the conflicts that experience a flood have a lower risk of conflict termination than those not experiencing a flood. The same is true for landslides. Their hazards ratios do not give support to the hypothesis, but both have a significance level that is too high for generalization.

Looking at windstorms however, the hazard ratio below 1 demonstrates that the occurrence of a windstorm serves to decrease the chance of conflict termination with 68%, a finding that is within the 5% significance limit. This means that for windstorms, the hypothesis that climatic disasters lead to shorter conflicts is dismissed. Likewise, the hazard ratio for heat waves is below 1. However the ratio here is perilously close to zero, indicating certain instability in the prediction, despite the fact that the coefficient is significant on the 1% level. This instability might stem from the fact that there are relatively few heat waves recorded in the dataset. This is certainly the case with the wave variable, with only 16 occurrences in the dataset, but a hazard ratio above 22 (!). Unlike all the other predictors, a wave increases the risk of conflict termination 22 times that of a conflict not experiencing a tidal wave or a surge. This is statistically significant, but only a handful of observations are driving the result[19]. Also the coldspell variable has a hazard ratio above 1, but the result is not statistically significant.

As for the control variables, they give virtually the same picture as in Models 1 and 2 with regime durability having asymptotically no impact on the risk of conflict termination, while both higher infant mortality and population points towards decreased the risk of conflict termination. Neither of the above effects are significant.

In Model 4, the disaster indicator being a binary measure of whether any one (or more) of the disaster events occurred in a given month, the pattern is the same as in Model 3. The control variables yield more or less the same results as in Model 3, without significance. Rather interesting though, is the disaster predictor in Model 4. With a hazard ratio of 0.488, conflicts experiencing either one of the EM-DAT disaster indicators have a 51% lower chance of ending than conflicts not experiencing disaster, a finding that is significant at the 1% level. This goes against the hypothesis, and serves to support the idea that disasters reduce the chance conflict termination.

[19] The cases this pertains to will be addressed in the discussion in Chapter 5.

Table III. Predicting the hazard of conflict termination using the EM-DAT indicators.

	(3)	(4)	(5)	(6)
Flood occurrence	0.549			
	(-1.84)			
Windstorm occurrence	0.318			
	(-2.54)*			
Heat wave occurrence	0.000			
	(-17.38)**			
Coldspell occurrence	1.344			
	(0.54)			
Wave occurrence	22.278			
	(3.76)**			
Landslide occurrence	0.610			
	(-0.87)			
All EM-DAT indicators		0.488		
		(-2.73)**		
Flood occurrence the past			1.306	
6 months			(1.07)	
Windstorm occurrence the past			0.662	
6 months			(-1.17)	
Heat wave occurrence the past			0.588	
6 months			(-0.93)	
Coldspell occurrence the past			0.610	
6 months			(-0.9)	
Wave occurrence the past			3.756	
6 months			(3.92)**	
Landslide occurrence the past			1.028	
6 months			(0.08)	
All EM-DAT indicators the				1.010
past 6 months				(0.04)
Regime durability (years)	1.002	1.001	1.001	1.000
	(0.44)	(0.22)	(0.31)	(0.09)
Infant mortality rate at	0.913	0.914	0.915	0.920
onset (ln)	(-0.77)	(-0.77)	(-0.73)	(-0.7)
Population (ln)	0.914	0.918	0.877	0.867
	(-1.75)	(-1.71)	(-1.99)**	(-2.59)**
Constant	0.890	0.843	1.663	1.994
	(-0.12)	(-0.18)	(0.47)	(0.72)
Log pseudolikelihood	-323.6	-327.0	-327.3	-330.5
Number of conflicts	192	192	192	192
Number of failures	158	158	158	158
Observations	9754	9754	9754	9754

Estimates show Weibull hazard rates. Robust Z statistics, clustered on countries, in parentheses.
* and ** denote significance at respectively 95 and 99 percent confidence levels.

Figure II depicts the Kaplan-Meier survival plot showing the survival rates for the conflicts that did not experience any EM-DAT disasters and the conflicts that did. It is evident that the conflicts experiencing a climatic disaster have a higher survival rate than conflicts not experiencing these disasters. A higher survival rate means that the conflicts have a higher risk for continuation, and it is clear from the plot that this discrepancy increases with the duration of the conflicts. Also clear from the plot is the fact that the chance of termination decreases drastically the first months of conflict, before it flattens out with time, a pattern that pertains to all the conflicts included.

Turning to the lagged disaster indicators, Model 5 presents the disaster indicators as binary variables recording whether the given disaster happened the past six months or not. In the same fashion as found in the case of the floods from the DFO dataset, when the floods are imposed to the temporal domain, their effect is reversed. The flood variable now has a hazard rate above 1, indicating that conflicts that experienced one or more floods the past six months have a higher chance of ending than those conflicts that was not hit by a flood the past six months. The hazard ratio of the windstorm variable increases a little from Model 3, but is rendered insignificant in the process. The same goes for heat waves and coldspells, although the former has an increased hazard ratio while the latter a decrease compared to Model 3. The opposite happens with the landslide variable, now having a hazard ratio above 1 pointing to higher risk of termination for the conflicts that experienced a landslide the past six months.

The only significant effect of the disaster indicators is found for the wave variable, where a hazard ratio of 3.7 indicate that conflicts that have experienced a tidal wave or a surge over the past six months have three times as high a chance for resolution than conflicts that did not experience such hazards, a considerable decrease from Model 3. There are indeed more observations assuming the value 1 on this lagged variable than the original one, but by and large the result is still driven by the few cases from the original variable.

Looking at the control variables, the coefficients are more or less the same as in Model 3, but somewhat strange, the population effect is now significant. The hazard rate for the population variable is still less than 1, demonstrating that conflicts taking place in populous countries have a lower chance of ending than conflicts in less densely populated countries.

Finally, Model 6 contains a binary measure recording whether any of the EM-DAT indicators from Model 5 took place within the past six month. Again, significant effects by and large fail to appear. The hazard rate for the disaster indicator has increased above 1, being

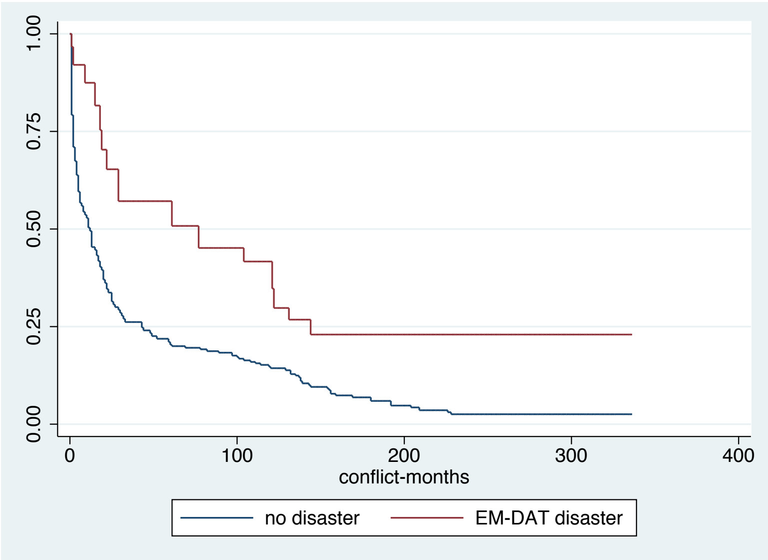

Figure II. Kaplan-Meier survival estimates for conflicts that did and did not experience an EM-DAT disaster.

symptomatic of shorter duration for the conflicts that experienced one or more EM-DAT hazards the past 6 months. Granted, the effect is not significant, but it seems reasonable to assume that the change in the direction of the effect is due to the change in the observed effect of floods. The control variables are practically the same as in the three preceding models, with the exception of the statistically significant population variable that appears first in the time-lagged models.

Summing up then, the models do not show a clear-cut picture of the way climatic disasters impact the duration of armed conflict. The main essence, although a lot of the coefficients lack significance, is nevertheless that a flood occurring does not increase the chance of conflict termination, but rather points to the opposite. There are a few exceptions such as tidal waves, surges and heat waves, but the general picture is that the working hypothesis should be dismissed. However, an interesting finding is the fact that when the time-lag of six months is introduced, the effect seems to be revered. This draws attention to a certain shock effect of the disasters, although it seems to be contrary to the one expected in that the initial shock effect is negative, but that there is a chance that the effects changes with time.

4.3 The war-dataset

In order to ensure that the results from the above analysis are somewhat robust, I applied the same models on a dataset employing a different definition of armed conflict than the one above. In this dataset, conveniently referred to as the war-dataset, the conflicts included are those conflicts where at least 1000 battle-related deaths took place within the calendar year. This narrows the sample quite a lot, and Table IV depicts the same models as above, run on the war-dataset. The models depict each coefficient's hazard ratio, and are interpreted in the same way as in Models 1 to 6.

Beginning with Model 7, which has the same predictors as Model 1, the hazard ratio below 1 reveals that even for wars, being hit by a flood means a smaller chance of the war ending than if it was not hit by such hazards. Nevertheless, neither this effect nor any of the control variables' effect are statistically significant. Looking at the coefficients for the control variables, they reveal the same pattern as found in the past models; regime durability has proximately no effect on the risk of termination, while higher infant mortality rates and populations both reduce the chance of a war ending.

Model 8 predicts the effect of the EM-DAT indicators, but because of the constriction of the number of observations, heat waves and waves are omitted. Looking at the flood variable, it is very similar to the DFO flood-measure prescribing that the wars being hit by a flood have a lower chance of conflict termination than those not being hit. As in Model 7, this effect is not significant. Effects that are significant however, are the effects of windstorms, coldspells and landslides. Still, their effects are all so close to zero that if anything; it suggests instability in the model(s). As is clear from Table E.2 in Appendix E, the number of these hazards in the dataset is very few[20].

The control variables reveals a familiar picture, with the same insignificant effects. This is also the case in Model 9, where the disaster predictor included is the one recording all the EM-DAT disasters. The effect of the catch-all variable is, like before, going against the hypothesis that climatic disasters shorten the duration of conflicts. The last three models in Table IV, Models 10 through 12, include the different time-lagged variables recording whether the specific indicators (or all in the case of Model 12) occurred the past six months of conflict. The control variables show effects that are all approximately alike, as well as being more or less identical to the rest of the models.

Overall, the models are different from the analogous models applied on the conflict-dataset, in that they seem to reinforce the findings from the first three models. Namely, when the time

[20] I ran the models without these predictors as well, but the result was more or less the same as shown in Table IV.

Table IV. Predicting the hazard of conflict termination using the war-dataset.

	(7)	(8)	(9)	(10)	(11)	(12)
Flood occurrence (DFO)	0.805					
	(-0.2)					
Flood occurrence (EM-DAT)		0.885				
		(-0.11)				
Windstorm occurence		0.000				
		(-28.45)**				
Colsdpell occurrence		0.000				
		(-24.55)**				
Landslide occurrence		0.000				
		(-30.34)**				
All EM-DAT indicators			0.666			
			(-0.4)			
Flood (DFO) occurrence the past 6 months				0.414		
				(-1.04)		
Flood (EM-DAT) occurrence the past 6 months					0.157	
					(-1.5)	
Windstorm occurence the past 6 months					0.000	
					(-12.2)**	
Coldspell occurrence the past 6 months					0.000	
					(-28.84)**	
Landslide occurrence the past 6 months					6.958214	
					(1.7)	
All EM-DAT indicators the past 6 months						0.613
						(-0.55)
Regime durability (years)	0.986	0.985	0.986	0.987	0.990	0.986
	(-0.73)	(-0.76)	(-0.74)	(-0.67)	(-0.53)	(-0.71)
Infant mortality rate (ln)	0.734	0.732	0.733	0.723	0.675	0.728
	(-1.05)	(-1.03)	(-1.04)	(-1.13)	(-1.23)	(-1.07)
Population (ln)	0.712	0.729	0.720	0.756	0.788	0.748
	(-1.23)	(-1.03)	(-1.17)	(-1.04)	(-0.61)	(-0.99)
Constant	21.486	14.968	17.670	7.107	4.816	9.029
	(0.76)	(0.62)	(0.71)	(0.5)	(0.28)	(0.52)
Log pseudolikelihood	-31.079	-30.736	-31.020	-30.409	-26.785	-30.849
Number of conflicts	21	21	21	21	21	21
Number of failures	17	17	17	17	17	17
Observations	1411	1141	1141	1411	1141	1141

Estimates show Weibull hazard rates. Robust Z statistics, clustered on countries, in parentheses.
* and ** denote significance at respectively 95 and 99 percent confidence levels.

lag is applied, the difference in the risk of termination between those wars experiencing a disaster (lower risk) and those not experiencing these hazards, become even bigger (even lower risk). The exception is landslides, the only indicator with a hazard ratio above 1. Apart from windstorms and coldspells, suffering from the low number of cases and hazard ratios close to zero, none of the effects in either three models are statistically significant.

Summing up then, the first three models indicate that both flood measures and the catch-all EM-DAT measure have more or less the same (weak) effects as those found in the equivalent models applied on the conflict dataset. When it comes to the time-lagged models, the effects differ from those presented in the previous chapter, as the time lag seems to augment the effects in the models that are not temporal. However, the war-dataset suffers from the fact that the disaster indicators hinge on a very small number of cases. In addition, the time span is rather short when taking into consideration the strict conflict-definition. The result is a short time span with only a few conflicts (wars), but still fairly many observations with the unit of analysis being conflict-months, and a yearly approach would have maybe resulted in more stable models. On the other hand, doing this would have made the models difficult to compare to the models applying the more generous inclusion criterion and would thus mismanage the purpose of the models.

5. Discussion

In order to answer the research question, I performed a survival analysis investigating the duration of armed intrastate conflict. The analysis tested how the prevalence of hydrometeorological disasters affects the likelihood of conflict termination. The general picture is that the survival models do not support the hypothesis that climatic disasters increase the risk of termination of armed intrastate conflicts. This is because both the flood indicators and the aggregated disaster indicators accentuate that conflicts experiencing these hazards have a lower risk of conflict resolution than other conflicts. There are some exceptions to this pattern, serving to indicate that there might not be a universal effect across disaster type. In addition, the temporal effect of disasters hints towards a changing effect with time. However, these indications are weak, and must be investigated further before any conclusions can be made.

5.1 The main empirical results revisited

The first result from the analysis is that the occurrence of a flood, measured either by force or consequence, does *not* increase the chance of conflict ending. Instead, the survival models suggest that the opposite effect is more likely; namely that those conflicts experiencing a flood have a lower risk of conflict termination than those who do not experience such hazards. Despite the lack of statistical significance that makes it hard to state firmly that this is the case, the result counters the proposed hypothesis. The aggregate disaster indicator, capturing all EM-DAT incidences, reveals the same relationship; only here the relation is significant. Hence, when the measure used does not separate between disaster types, the occurrence of hydrometeorological disasters in a country that is experiencing conflict reduces the chance of conflict resolution with more than 50% towards countries in conflict that are not hit by these hazards. With a significant result, this does not only dismiss the hypothesis of this thesis, but it demonstrates that the relationship is in fact opposite.

This is in line with the environmental security literature postulating that climatic disasters act as drivers of conflict. The analysis does not make it possible to distinguish whether the disaster adds to existing structural scarcity by either creating supply- or demand-induced scarcity, or how exactly the opportunities, motives and incentives of the actors are affected by the disasters in a way that counteract the opportunities for discontinuance of conflict. This is in part due to limits imposed by the quantitative design of the analysis, but also the fact that this specific area of research is so underdeveloped makes it particularly hard to pinpoint how the dynamics between disasters and conflict duration work. Although the analysis does not reveal *why* the conflicts hit by climatic disasters last longer on average, it is imaginable that it could be due to both Homer-Dixon's (1999) notion of structural effects and Nel and Righart's (2008) conceptualization. Either way, it is probable that the occurrence of a disaster affects the warring actors' relative capacities in one way or another, and hence the conflict dynamics. In order to extend the knowledge and be able to pinpoint these mechanisms, process tracing and/or case studies would be of high value.

Secondly, the models where the disaster indicators for both flood measures and the aggregated disaster measure are subject to a time constraint reveal a contrary pattern. When the disaster variables are measured in a way that records whether or not the given disaster occurred during the past six months, the coefficients tell a different story than in the models where the disasters are recorded in the month(s) they occur. With the accumulated time-lag, the hazard ratios report that conflicts that have experienced a flood or any EM-DAT disaster in the past six months have a higher chance of conflict termination than conflicts where this is not the case. Although not significant, this does give some support to the hypothesis, even though the indicated delay in the effect is not fully consistent with the theoretical foundation.

Kelman's (2012) disaster diplomacy, Birkland's (1998) focusing events and Zartman's (2000) ripeness theory all argue that an exogenous shock such as a hydrometeorological disaster can create opportunities for conflict resolution. All the perspectives stress the timing, arguing that the disasters can create a window of opportunity. They indicate however that this window of opportunity appears fairly proximate in time to the disaster. The contrary is found in Nel and Righarts (2008), who also talk of the time-aspect of disasters' impact, pointing out that the immediate effects differ from the less proximate ones. In the latter framework however, both the immediate and the later effects of disasters affect the conflict negatively. The results from the survival analysis do point to a possible shock effect, as it indicates that in the long-term aftermath of disaster, the disaster seems to have a positive impact on the prospects for peace. Summing up then, there appears to be a shock effect, although it is the opposite of the one that would be expected based on the theoretical considerations. Initially, a disaster reduces the prospects of a conflict ending, but within six months' time, this effect is reversed, making those conflicts that have experienced a disaster in the past more prone to conflict termination.

Despite the fact that the two flood measures and the aggregate disaster measure yielded consistent results, concluding that all hydrometeorological disasters reduce the risk of conflict termination, at least in the short run, is premature. From both the model with and the model without the time lag, it seems as though some disaster types stand out with the opposite effect on conflict duration. Both the occurrence of waves, including tidal waves and surges, and landslides[21] are in the models associated with a higher chance for conflict termination, predicting that the conflicts hit by these types of disasters have a higher chance of termination than those conflicts unaffected by landslides or waves. This is in line with the hypothesis, and it serves to denote that some type of disasters can have a conflict-resolving power, supporting the theoretical arguments that disasters can act as catalysts of peace. What exactly it is that separates waves and landslides from for example floods is difficult to determine in this study. However, it is conspicuous to assume that the impact of these disasters is more devastating than is the case in the other types of hazards, at least in terms of being so encompassing it makes the warring parties see peace as more beneficial than war. Table D.4 in Appendix D lists the landslide-months where the conflict ended within the next year, and makes it clear that several of the landslides that occurred in India was rather quickly followed by conflict termination[22]. Fritz, Kelman and Birkland all indicate that the severity of the disaster can be crucial. My approach is nevertheless unsuitable to predict whether this stems from changed attitudes of people – in line with a community of sufferers mentality –, changing power relations among the actors, or the realization that the costs of continued conflict will outweigh the benefits.

A somewhat worrisome aspect of this result – although it is only for waves that the positive effect on conflict termination is significant – is the number of observations for these two hazard types, and particularly the number of waves recorded. There are only seven unique waves in the sample, but the number of months recording a wave is 16 because some of the waves hit countries with several ongoing conflicts. The Philippines is the most wave-prone country with three unique occurrences. However, it is the wave in India that appears to be the most influential. Table D.3 in Appendix D lists the conflicts that experienced a wave, plus the number of years the conflict lasted and how long after the wave the conflict ended. Only one wave hit India, but at the time there were eight ongoing conflicts. From the table it is clear that two of the conflicts ended the same year as the wave hit (1997). That the conflicts ended so rapidly after the disaster, makes these rather influential cases. Since there are relatively few combinations of waves and failures, these few cases become drivers of the coefficients.

[21] In the time-lagged models only.
[22] This is not to suggest causality, which will be further discussed below.

However, this is not to say that the relationship is causal, and in order to see whether it was in fact the waves that lead to conflict termination, process-tracing would be an attractive tool. Possible predictors of this relationship could be the proximity of the waves to the conflict location, as well as uncovering whether for example the wave occupied the government's resources and paved the way for rebel victory, or whether the disaster relief proved dependent on the cooperation of the warring parties.

When looking at the models applied on the war dataset, where the definition of conflict was altered in order to see whether the results from the main models would also hold for wars only, the lack of significance is striking. The coefficients that are significant all have hazard ratios very close to zero, being symptomatic of instability in the predictors. As have been discussed in the previous section, this is in all probability due to the extremely few cases per predictor. Looking beyond these covariates however, the pattern found in the original models without time lag is confirmed also in the cases of wars. Both flood indicators and the general EM-DAT disaster measures imply a lower chance of conflict termination when measured the month the hazards occur, supporting both the environmental security literature and popular debate. However, in the case of the time-lagged indicators, the models on the war-dataset are not in accordance with the models using the extensive conflict definition. Here, the coefficients actually signal that when either a flood or a disaster has occurred within the past six month of war, the wars experience an even lower chance of termination than when measured the month of hazard. This is opposite of the findings from the conflict-models, and cannot be taken to support the thesis' hypothesis. All things considered, the war-dataset supports the first trend observed in the original models, namely that climatic disasters is chiefly associated with longer lasting civil conflict. They do not support the finding that this changes to the opposite with time, as found in the time-lagged original models. This then, warrants even more caution in suggesting that the hypothesis might be true if the time aspect is considered.

5.2 What then, predicts conflict duration?

The fact that most of the control variables are hardly significant is of some concern. Only occasionally is the population variable significant across all the models. Most of the covariates included in the models do not have significant effects on the risk of conflict termination, a probable side effect of the disaggregated monthly unit of observation that is combined with more static and slow-changing control variables. The control variables are less suitable to predict changes over short intervals than they are at predicting the aggregated duration of conflicts, and this might be the reason for the lack of significance. Still, omitted variable bias is always present in this type of analysis, and there are highly likely to be other effects at play that are not captured by this analysis.

The analysis tests the better part of "the usual suspects" when it comes to country-specific control variables. Of course, the choice of measures could have been different, but for fear of multicollinearity, there are few country-specific variables that could be added without having to take one or more of the existing variables out. For example GDP per capita could have been included, but then the infant mortality rate would have had to go due to high correlation between the two, and as it serves as a better measure of vulnerability, the IMR is preferred. DeRouen and Sobek (2004), Collier et al. (2004), Fearon (2004), Cunningham et al. (2009) and this analysis all use more or less the same indicators, although the corresponding operationalization differ somewhat, and as such, earlier studies of conflict duration largely confirm the variables used in this analysis.

Some country- and conflict-specific predictors have also been left out of the analysis. The conflict-specific measures type of insurgency and the number of actors involved are omitted, as is the number of ongoing conflicts at the time. The first and last of these were included in the model-specification, but as they did not have any impact they were left out of the final models. Beyond this however, Fearon (2004) points to how the financing structures and opportunities for the insurgents play a role in determining the duration of conflict. He differentiates between different sources of income, and finds that the more stable the rebels' source of financing, longer the conflicts last. Another covariate that could be envisaged to impact the relationship between disaster and conflict duration that has not been tested, is the insurgents' capabilities. Cunningham et al. (2009) investigates the strength of the rebels and find that the stronger the rebels, the higher the risk of conflict termination.

The focus on the dyadic relationship between the actors can also be found in Buhaug et al. (2009). Measuring the capabilities, or the power, of rebels proves for a difficult task however, as it is hard to find covariates that are universal across both time and space. Recent literature has looked at territory (mountain and forest cover) and distance to the capital and borders as proxies for rebel capacity and hence the dyadic relationship with the government. Both these aspects of the rebels/the non-state actors in the conflicts have been left out of the analysis for somewhat pragmatic reasons. These measures would without doubt serve best in a disaggregated analysis, and as this analysis have looked at country-level measures, it would have been problematic to combine this with measures that are even more disaggregated. In addition, particularly the forest cover and distance variables would serve best in an approach employing geocoded data.

5.3 Avenues for future research

Insights gained from this study, and the larger literature, point to a number of possible avenues for future research. Firstly, a further investigation into the temporal aspect that this analysis hints towards would be very interesting. Is it in fact the case that the effect of a disaster on the conflict dynamics changes with time, and if so – how long after the disaster impact does the effect change to the opposite? And is it necessary with several shocks within a confined time interval for the effect to change? Another important endeavor would be to look more closely at what intermediary effects are at play when a disaster affects the conflict dynamics. A more thoroughgoing, qualitative, investigation of the theoretical propositions would be of value. In addition to providing empirical results, such an analysis would also serve to fill the theoretical gap between the predictors of conflict onset and the predictors of conflict dynamics.

Furthermore, there is a need for more disaggregated studies on the topic. Making use of geocoded data would be very serviceable because more force-based climate indicators are available in this format. Unfortunately converting these data was beyond the scope of this thesis, but should most definitely be aspired in further studies on this topic. Another advantage of using such data is that it would have taken the analysis down to a level where also the indicators on the local conditions are, probably increasing the explanatory power of the models considerably. Looking specifically at my analysis, adding a more extensive temperature measure (for example the data from O'Loughlin et al. (2014)) would be both interesting and supplementary. Unfortunately this was not possible due to the time-constraints of the thesis. Another thing that could be done is aggregating the level of analysis up to conflict-years in order to investigate the scale-sensitivity of the findings and make the analysis more immediately comparable to the results of earlier research. Nevertheless, in order to utilize the disaggregated monthly level of analysis presented in this thesis, the need for disaggregated covariates is pressing.

6. Concluding remarks

This thesis set out to test how climatic disasters affect the duration of armed civil conflict, and estimated the risk of conflict termination by use of survival analysis. From a theoretical perspective, the environmental security literature postulates that climatic disasters will lead to more conflict. However the direct impact of disasters on conflict dynamics (here manifested as conflict duration) is less clear within this paradigm, and following a series of theoretical arguments predicting that disasters have the characteristics necessary to create opportunities for peace – and hence affect its duration – the following hypothesis was delineated;

H_1: armed intrastate conflicts that are affected by climatic disasters experience increased risk of impending conflict resolution.

The hypothesis was tested in several Weibull-distributed parametric survival models. The main finding dismisses the hypothesis, and despite lacking statistical significance, it indicates that the relationship is the opposite. Namely, that those intrastate conflicts that are affected by climatic disasters have a lower risk of conflict termination, and thereby lasts longer than those conflicts not hit by these hazards. This is in line with the environmental security literature, but as the perspective does not directly concern conflict dynamics, the effects at play are hard to determine. Furthermore, the survival models also suggest that this effect reverses with time, giving at least some support to the hypothesis above. Although not a statistical significant result, the indication that after a certain amount of time has passed, climatic disasters increase the chance of conflict termination warrants further research into the time-aspect of this nexus. Finally, the analysis reveals that the effects of hydrometeorological disasters are not universal across disaster type, as some types of hazards seem to have an effect in line with the hypothesis while others do not. The overall absence of significance nevertheless prohibits a categorical conclusion of the impact of hydrometeorological disasters on conflict duration.

What has been made evident by this thesis is the need for further research, and particularly research on a disaggregated level that can make use of geocoded covariates. Performing the same kind of analysis as this, only with geocoded data would contribute to a more thorough understanding of the effects at play. The analysis also points to the lack of literature on the climate-conflict dynamics nexus. Both theoretical perspectives – the environmental security approach to the greatest extent – are surprisingly deficient in describing the mechanisms at work, particularly considering the assumingly scientifically informed policy debate on the topic. Although the results are somewhat diffuse, this thesis has begun to fill the omission that exists within the research on climate change and conflict dynamics, and it warrants at least a proviso when discussing the effects of climate change on armed conflict. In line with Nardulli et al. (2015, p.330), the thesis makes it clear that “future research should focus on identify their [the disasters’] destabilizing impact”, and that this should be done with the highest possible resolution.

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8. Appendices

Appendix A

Table A.1 Synopsis of the research on climate factors and conflict, reproduced from Theisen, Gleditsch and Buhaug (2013).

Table 1 Climate factors and organized conflict—quantitative studies

Hypothesis	Civil conflict	Communal violence	Riots
Short-term rainfall deficiency increases risk	2 support, 4 none, 1 opposite = inconclusive (7)	1 support, 2 weak support, 2 none, 2 opposite = inconclusive (7)	
Short-term rainfall deficiency increases risk via economic growth	1 support, 1 (more recent) weak support, 1 (more recent) none = inconclusive (3)		1 support = inconclusive (1)
Short-term warming increases risk	“2 support, 1 none = some relationship (3)	1 mixed support = inconclusive (1)	
Short-term warming increases risk via economic growth	1 very weak support = inconclusive (1)		
Natural disasters increase risk	3 support, 2 some support, 1 (more recent) none; 1 (more recent) opposite = some relationship (7)	1 support (in Indonesia) = inconclusive(1)	
Natural disasters increase risk via economic growth	2 no support = inconclusive (1)		
Land degradation (stable levels) increase risk	2 support, 2 none, but measure questionable = inconclusive (4)		
Water scarcity (stable levels) increase risk	2 some support, 2 none, 1 opposite = at most a weak relationship (4)		
Less vegetation increases violence	1 opposite = inconclusive (1)		
Short-term fluctuations in food prices increase risk			1 study finds peaks in wheat price growth and decline to increase risk = inconclusive (1)

Civil conflict is an armed conflict between a government and an organized rebel movement, communal violence is an armed conflict between two organized groups, and a riot is spontaneous protests by (mostly) unorganized actors. The figures in each cell denote the number of studies that support, weakly support, do not support, or contradict the proposed hypothesis; total number of reviewed studies per outcome is given in parenthesis. All relationships based on a single study are characterized as inconclusive. A more detailed description of the studies included is included in an online appendix at our replication site www.prio.no/cscw/datasets

Appendix B

Table B.1 Flood-months per country from the original DFO-data, 1985-2014. Sorted on the highest number of flood-months.

Country	Number of flood-months	Percent of total flood-months	Country	Number of flood-months	Percent of total flood-months	Country	Number of flood-months	Percent of total flood-months
United States of America	513	7.96	Jamaica	28	0.43	Montenegro	7	0.11
China	453	7.03	Niger	28	0.43	Belgium	6	0.09
India	349	5.41	Tajikistan	27	0.42	Trinidad and Tobago	6	0.09
Indonesia	232	3.6	Ukraine	27	0.42	Uzbekistan	6	0.09
Philippines	213	3.3	Hungary	26	0.4	Belize	5	0.08
Vietnam	163	2.53	Yemen	26	0.4	Cameroon	5	0.08
Bangladesh	156	2.42	Greece	24	0.37	Djibouti	5	0.08
Australia	154	2.39	Paraguay	24	0.37	Gambia	5	0.08
Russian Federation	142	2.2	Ghana	23	0.36	Lebanon	5	0.08
Thailand	139	2.16	Madagascar	23	0.36	Liberia	5	0.08
Brazil	131	2.03	Poland	23	0.36	Mongolia	5	0.08
Mexico	109	1.69	Guatemala	22	0.34	Norway	5	0.08
Pakistan	99	1.54	Chile	21	0.33	Sierra Leone	5	0.08
Afghanistan	94	1.46	Panama	21	0.33	Solomon Islands	5	0.08
Iran	92	1.43	Czech Republic	20	0.31	United Arab Emirates	5	0.08
Canada	88	1.36	Georgia	20	0.31	Armenia	4	0.06
Kenya	80	1.24	Botswana	19	0.29	Bahamas	4	0.06
Nigeria	80	1.24	Fiji	19	0.29	Cayman Islands	4	0.06
Colombia	78	1.21	Papua New Guinea	19	0.29	Dominica	4	0.06
Malaysia	78	1.21	Saudi Arabia	19	0.29	Guinea-Bissau	4	0.06
United Kingdom	73	1.13	Serbia	19	0.29	Saint Lucia	4	0.06
Nepal	72	1.12	Uruguay	19	0.29	Singapore	4	0.06
South Africa	70	1.09	El Salvador	18	0.28	Vanuatu	4	0.06
Japan	64	0.99	Morocco	18	0.28	Antigua and Barbuda	3	0.05
Peru	62	0.96	Puerto Rico	18	0.28	Bhutan	3	0.05
Ethiopia	59	0.92	Mauritania	16	0.25	Guadeloupe	3	0.05
Mozambique	59	0.92	Albania	15	0.23	Martinique	3	0.05
Romania	59	0.92	Bosnia and Herzegovina	15	0.23	Netherlands	3	0.05
Sri Lanka	58	0.9	Burkina Faso	15	0.23	Palestine	3	0.05
Turkey	58	0.9	Slovakia	15	0.23	Saint Kitts and Nevis	3	0.05
Bolivia	54	0.84	Austria	14	0.22	Vincent and the Grenadines	3	0.05
Somalia	54	0.84	Israel	14	0.22	Timor-Leste	3	0.05
Argentina	49	0.76	Portugal	14	0.22	American Samoa	2	0.03
Korea, South	49	0.76	Rwanda	14	0.22	Comoros	2	0.03
France	47	0.73	Kazakhstan	13	0.2	Eritrea	2	0.03
Sudan	47	0.73	Senegal	13	0.2	Gabon	2	0.03
Tanzania	46	0.71	Belarus	12	0.19	Grenada	2	0.03
Myanmar	43	0.67	Mali	12	0.19	Lesotho	2	0.03
Namibia	43	0.67	Congo (Kinshasa)	11	0.17	Lithuania	2	0.03
Haiti	42	0.65	South Sudan	11	0.17	Mauritius	2	0.03
Italy	42	0.65	Switzerland	11	0.17	Micronesia	2	0.03
Taiwan	42	0.65	Syria	11	0.17	Seychelles	2	0.03

Spain	41	0.64	Benin	10	0.16	Suriname	2	0.03
Uganda	41	0.64	Croatia	10	0.16	Sweden	2	0.03
Zambia	41	0.64	Egypt	10	0.16	Virgin Islands, British	2	0.03
Cambodia	40	0.62	Guinea	10	0.16	Virgin Islands, U.S.	2	0.03
Malawi	40	0.62	Togo	10	0.16	Anguilla	1	0.02
Korea, North	39	0.6	Tunisia	10	0.16	Barbados	1	0.02
Bulgaria	38	0.59	Congo (Brazzaville)	9	0.14	Bermuda	1	0.02
Dominican Republic	36	0.56	Guyana	9	0.14	Estonia	1	0.02
Honduras	35	0.54	Laos	9	0.14	Finland	1	0.02
Venezuela	33	0.51	Oman	9	0.14	French Polynesia	1	0.02
Angola	32	0.5	Azerbaijan	8	0.12	Guam	1	0.02
Algeria	31	0.48	Burundi	8	0.12	Iceland	1	0.02
Zimbabwe	31	0.48	Central African Republic	8	0.12	Latvia	1	0.02
Costa Rica	30	0.47	Jordan	8	0.12	Maldives	1	0.02
Ecuador	30	0.47	Côte d'Ivoire	7	0.11	Montserrat	1	0.02
New Zealand	30	0.47	Iraq	7	0.11	Netherlands Antilles	1	0.02
Cuba	29	0.45	Ireland	7	0.11	New Caledonia	1	0.02
Nicaragua	29	0.45	Kyrgyzstan	7	0.11	Saint Martin (French part)	1	0.02
Chad	28	0.43	Macedonia	7	0.11	Slovenia	1	0.02
Germany	28	0.43	Moldova	7	0.11	Western Sahara	1	0.02
Total							6447	100

Table B.2 Countries that were altered in the original DFO dataset.

Flood Register	Country	Changed from
20	Puerto Rico	United States of America
990	Taiwan	India
991	India	Morocco
1000	Malaysia	United States of America
1001	United States of America	Albania
1002	Albania	Benin
1017	Nicaragua	United States of America
1025	Mexico	United States of America
1026	United States of America	Azerbaijan
1033	Bangladesh	United States of America
1034	United States of America	Canada
1035	Canada	Philippines
1069	Australia	Czech Republic
1077	Yemen	China
1078	United States of America	China
1080	Italy	India
1081	India	United States of America
1464	Dominican Republic	Honduras
1723	Puerto Rico	United States of America
1823	Puerto Rico	Cuba
3315	Germany, Belgium, Italy	France
3657	Poland	Hungary
4131	Mozambique	South Africa

Table B.3 Hazards that were deleted from the original EM-DAT dataset because of lacking dates.

Country	Type of disaster	Hazard ID	Year	People affected	Entry criteria
Australia	Flood	1151	1988	0	Kill
Burkina Faso	Flood	2256	1994	66500	Kill
Belize	Extreme temperature	1444	1990	0	Declar
Bolivia	Flood	1	1980	15000	Affect
Brazil	Flood	1149	1988	1000	Kill
Brazil	Flood	2970	1998	32000	Affect
Barbados	Wind storm	986	1987	230	Affect
Switzerland	Slides	1152	1988	2000	Affect
China	Flood	297	1982	0	Declar
China	Slides	318	1982	0	Kill
China	Wind storm	530	1984	0	Govern
China	Extreme temperature	723	1986	30000	Affect
Fiji	Flood	1299	1989	0	Kill
Guatemala	Flood	1296	1989	0	Kill
Hong Kong	Wind storm	404	1983	617	Affect
Honduras	Slides	1300	1989	0	Kill
Indonesia	Slides	457	1983	0	Kill
India	Flood	717	1985	0	Kill
India	Flood	838	1986	150000	Kill
India	Wind storm	839	1986	0	Kill
Japan	Slides	201	1981	0	Kill
Japan	Slides	458	1983	0	Kill
Japan	Slides	578	1984	0	Kill
Japan	Flood	840	1986	162000	Kill
Japan	Flood	1297	1989	0	Kill
Japan	Slides	1302	1989	0	Kill
Laos	Wind storm	1677	1991	38315	Affect
Liberia	Extreme temperature	1363	1990	1000000	Affect
Yugoslavia	Flood	721	1986	1000	Affect
Mongolia	Flood	320	1982	0	Kill
Malawi	Flood	319	1982	6000	Affect
Nepal	Slides	459	1983	0	Kill
Nepal	Wind storm	938	1987	0	Govern
Pakistan	Flood	93	1980	86200	Kill
Philippines	Slides	1303	1989	0	Kill
Puerto Rico	Slides	200	1981	0	Kill
North Korea	Flood	988	1987	20071	Kill
North Korea	Flood	989	1987	0	Kill
Russia	Slides	2076	1993	0	Kill
Soviet Union	Flood	199	1981	2000	Affect
Solomon Islands	Wind storm	1854	1992	0	SigDam
El Salvador	Flood	1295	1989	0	Kill
Yugoslavia	Flood	721	1986	1000	Affect
Turks and Caicos Islands	Wind storm	716	1985	770	Affect

Tanzania	Flood	836	1986	6000	Affect
United States	Flood	198	1981	0	Kill
United States of America	Flood	204	1982	0	Kill
United States of America	Wind storm	325	1983	0	Kill
United States of America	Flood	577	1984	0	Kill
United States of America	Wind storm	588	1985	0	Declar
United States of America	Flood	837	1986	2000	Affect
Vanuatu	Slides	1153	1988	3000	Affect

Table B.4 Hydrometeorological hazards included from the EM-DAT dataset.

Disaster generic group	Disaster subgroup	Disaster main type	Disaster sub-type
Natural Disaster	Meteorological	Wind storm	Cyclone
			Hurricane
			Storm
			Tornado
			Tropical storm
			Typhoon
			Winter
		Extreme temperature	Heat wave
			Coldspell
	Hydrological	Flood	Flood
			Lake flood
			Flash flood
			Riverine flood
		Landslide	Avalanche
			Landslide
			Mudflow
		Wave action	Tidal wave
			Surge

Appendix C

Table C.1 Conflict months, 1985-2011. Sorted on the highest number of conflict months.

Country	Conflict months	Number of unique conflicts	Percentage of total conflict months	Conflict-months per conflict
India	1,480	18	8.53	82.22
Myanmar	822	20	9.48	41.1
Ethiopia	636	8	3.79	79.5
Philippines	623	3	1.42	207.67
Israel	394	4	1.9	98.5
Turkey	347	3	1.42	115.67
Sudan	326	2	0.95	163
Afghanistan	324	1	0.47	324
Colombia	324	1	0.47	324
Uganda	324	1	0.47	324
Sri Lanka	281	3	1.42	93.67
Angola	278	6	2.84	46.33
Iran	276	7	3.32	39.43
Iraq	272	5	2.37	54.4
Chad	271	2	0.95	135.5
Algeria	241	1	0.47	241
Somalia	225	3	1.42	75
Peru	218	2	0.95	109
Russian Federation	192	7	3.32	27.43
Indonesia	177	5	2.37	35.4
Burundi	173	2	0.95	86.5
Pakistan	173	4	1.9	43.25
Rwanda	173	2	0.95	86.5
Cambodia	166	1	0.47	166
Senegal	160	2	0.95	80
Guatemala	132	1	0.47	132
Sierra Leone	129	1	0.47	129
United States of America	124	1	0.47	124
Nepal	122	1	0.47	122
Tajikistan	112	2	0.95	56
Bangladesh	106	2	0.95	53
Thailand	99	1	0.47	99
Congo (Kinshasa)	98	3	1.42	32.67
Mozambique	94	1	0.47	94
South Africa	92	2	0.95	46
El Salvador	84	1	0.47	84
United Kingdom	84	2	0.95	42
Bosnia and Herzegovina	82	3	1.42	27.33
Papua New Guinea	82	1	0.47	82
Egypt	69	1	0.47	69
Nicaragua	64	1	0.47	64
Morocco	59	1	0.47	59
Azerbaijan	57	3	1.42	19

Liberia	56	2	0.95	28
Central African Republic	46	3	1.42	15.33
Georgia	46	5	2.37	9.2
Lebanon	44	2	0.95	22
Croatia	43	1	0.47	43
Congo (Brazzaville)	42	3	1.42	14
Niger	41	5	2.37	8.2
Nigeria	36	3	1.42	12
Eritrea	32	2	0.95	16
Spain	32	2	0.95	16
Djibouti	31	2	0.95	15.5
Yemen	30	3	1.42	10
Côte d'Ivoire	29	2	0.95	14.5
Mali	26	4	1.9	6.5
Uzbekistan	25	2	0.95	12.5
Serbia	24	3	1.42	8
Mauritania	14	1	0.47	14
Haiti	13	3	1.42	4.33
Guinea-Bissau	12	1	0.47	12
Guinea	11	1	0.47	11
Venezuela	10	1	0.47	10
Laos	9	1	0.47	9
Libya	9	1	0.47	9
Mexico	5	2	0.95	2.5
Moldova	5	1	0.47	5
South Sudan	5	1	0.47	5
Macedonia	4	1	0.47	4
Suriname	3	1	0.47	3
Syria	3	1	0.47	3
Comoros	2	2	0.95	1
Trinidad and Tobago	2	1	0.47	2
Burkina Faso	1	1	0.47	1
China	1	1	0.47	1
Lesotho	1	1	0.47	1
Panama	1	1	0.47	1
Paraguay	1	1	0.47	1
Romania	1	1	0.47	1
Togo	1	1	0.47	1
Total	11,262	211	100	53.37

Table C.2 Conflict months 1985-2007. Sorted on the highest number of conflict months.

Country	Conflict months	Number of unique conflicts	Percentage of total conflict months	Conflict-months per conflict
India	1,306	16	8	81.625
Myanmar	718	18	9.38	39.89
Ethiopia	540	8	4.17	67.5
Philippines	527	3	1.56	175.67
Israel	346	4	2.08	86.5
Turkey	299	3	1.56	99.67
Afghanistan	276	1	0.52	276
Colombia	276	1	0.52	276
Sudan	276	1	0.52	276
Uganda	276	1	0.52	276
Sri Lanka	262	3	1.56	87.33
Angola	254	6	3.13	42.33
Chad	243	2	1.04	121.5
Iran	228	7	3.65	32.57
Iraq	224	5	2.6	44.8
Algeria	193	1	0.52	193
Peru	182	2	1.04	91
Indonesia	177	5	2.6	35.4
Somalia	177	3	1.56	59
Cambodia	166	1	0.52	166
Burundi	165	2	1.04	82.5
Senegal	159	1	0.52	159
Russian Federation	144	7	3.65	20.57
Rwanda	138	1	0.52	138
Guatemala	132	1	0.52	132
Sierra Leone	129	1	0.52	129
Nepal	122	1	0.52	122
Bangladesh	106	2	1.04	53
Tajikistan	101	1	0.52	101
Mozambique	94	1	0.52	94
South Africa	92	2	1.04	46
Congo (Kinshasa)	85	3	1.56	28.33
El Salvador	84	1	0.52	84
United Kingdom	84	2	1.04	42
Bosnia and Herzegovina	82	3	1.56	27.33
Papua New Guinea	82	1	0.52	82
Pakistan	77	4	2.08	19.25
United States of America	76	1	0.52	76
Egypt	69	1	0.52	69
Nicaragua	64	1	0.52	64
Morocco	59	1	0.52	59
Azerbaijan	57	3	1.56	19
Liberia	56	2	1.04	28
Thailand	51	1	0.52	51

Georgia	45	4	2.08	11.25
Lebanon	44	2	1.04	22
Côte d'Ivoire	43	1	0.52	43
Congo (Brazzaville)	42	3	1.56	14
Eritrea	32	2	1.04	16
Spain	32	2	1.04	16
Djibouti	31	2	1.04	15.5
Niger	30	5	2.6	6
Croatia	27	1	0.52	27
Uzbekistan	25	2	1.04	12.5
Serbia	24	3	1.56	8
Central African Republic	21	2	1.04	10.5
Haiti	13	3	1.56	4.33
Guinea-Bissau	12	1	0.52	12
Guinea	11	1	0.52	11
Mali	11	3	1.56	3.67
Venezuela	10	1	0.52	10
Laos	9	1	0.52	9
Nigeria	6	2	1.04	3
Mexico	5	2	1.04	2.5
Moldova	5	1	0.52	5
Yemen	5	2	1.04	2.5
Macedonia	4	1	0.52	4
Suriname	3	1	0.52	3
Comoros	2	2	1.04	1
Trinidad and Tobago	2	1	0.52	2
Burkina Faso	1	1	0.52	1
Lesotho	1	1	0.52	1
Panama	1	1	0.52	1
Paraguay	1	1	0.52	1
Romania	1	1	0.52	1
Togo	1	1	0.52	1
Total	9,754	192	100	50.8

Table C.3 List of conflict ID’s and appurtenant countries, entire dataset 1985-2013.

Conflict ID	Country	Conflict ID	Country	Conflict ID	Country	Conflict ID	Country	Conflict ID	Country
1	**Afghanistan***	45	El Salvador	89	Indonesia	133	Myanmar	177	Russian Federation
2	**Algeria***	46	Eritrea	90	Indonesia	134	Myanmar	178	Russian Federation
3	Angola	47	Eritrea	91	Indonesia	135	Myanmar	**179**	**Russian Federation***
4	Angola	48	Ethiopia	92	Iran	136	Myanmar	180	Rwanda
5	Angola	49	Ethiopia	93	Iran	137	Myanmar	*181*	*Rwanda**
6	Angola	50	Ethiopia	94	Iran	138	Myanmar	182	Senegal
7	Angola	51	Ethiopia	95	Iran	139	Myanmar	*183*	*Senegal*
8	**Angola**	**52**	**Ethiopia***	96	Iran	**140**	**Myanmar***	184	Serbia
9	Azerbaijan	53	Ethiopia	97	Iran	141	Myanmar	185	Serbia
10	Azerbaijan	54	Ethiopia	**98**	**Iran**	142	Myanmar	186	Serbia
11	Azerbaijan	**55**	**Ethiopia***	99	Iraq	143	Myanmar	187	Sierra Leone
12	*Azerbaijan*	56	Georgia	100	Iraq	144	Myanmar	188	Somalia
13	Bangladesh	57	Georgia	101	Iraq	145	Myanmar	189	Somalia
14	Bangladesh	58	Georgia	102	Iraq	146	Myanmar	**190**	**Somalia***
15	Bosnia and Herzegovina	59	Georgia	**103**	**Iraq***	**147**	**Myanmar***	191	South Africa
16	Bosnia and Herzegovina	*60*	*Georgia*	104	Israel	*148*	*Myanmar*	192	South Africa
17	Bosnia and Herzegovina	61	Guatemala	105	Israel	*149*	*Myanmar**	*193*	*South Sudan**
18	Burkina Faso	62	Guinea	**106**	**Israel***	150	Nepal	194	Spain
19	Burundi	63	Guinea-Bissau	107	Israel	151	Nicaragua	195	Spain
20	**Burundi**	64	Haiti	108	Laos	152	Niger	196	Sri Lanka
21	Cambodia	65	Haiti	109	Lebanon	153	Niger	197	Sri Lanka
22	Central African Republic	66	Haiti	110	Lebanon	154	Niger	**198**	**Sri Lanka**
23	Central African Republic	67	India	111	Lesotho	155	Niger	**199**	**Sudan***
24	*Central African Republic**	68	India	112	Liberia	**156**	**Niger**	*200*	*Sudan*
25	Chad	69	India	113	Liberia	157	Nigeria	201	Suriname
26	**Chad**	70	India	*114*	*Libya*	158	Nigeria	*202*	*Syria**
27	*China*	**71**	**India***	115	Macedonia	*159*	*Nigeria**	203	Tajikistan
28	**Colombia***	72	India	*116*	*Malaysia*	160	Pakistan	*204*	*Tajikistan*
29	Comoros	**73**	**India***	117	Mali	161	Pakistan	**205**	**Thailand***
30	Comoros	74	India	118	Mali	**162**	**Pakistan***	206	Togo
31	Congo (Brazzaville)	75	India	**119**	**Mali**	**163**	**Pakistan***	207	Trinidad and Tobago
32	Congo (Brazzaville)	76	India	*120*	*Mali*	164	Panama	**208**	**Turkey***
33	Congo (Brazzaville)	77	India	*121*	*Mali*	165	Papua New Guinea	209	Turkey
34	Congo (Kinshasa)	**78**	**India**	*122*	*Mali*	166	Paraguay	210	Turkey
35	**Congo (Kinshasa)**	79	India	*123*	*Mauritania*	167	Peru	**211**	**Uganda***
36	**Congo (Kinshasa)**	80	India	124	Mexico	**168**	**Peru**	212	United Kingdom
37	*Congo (Kinshasa)*	81	India	125	Mexico	**169**	**Philippines***	213	United Kingdom
38	*Congo (Kinshasa)*	**82**	**India**	126	Moldova	170	Philippines	**214**	**United States of America***
39	Croatia	*83*	*India*	127	Morocco	**171**	**Philippines***	215	Uzbekistan
40	Côte d'Ivoire	*84*	*India*	128	Mozambique	172	Romania	216	Uzbekistan
41	*Côte d'Ivoire*	*85*	*India*	*129*	*Mozambique*	173	Russian Federation	217	Venezuela
42	Djibouti	*86*	*India*	130	Myanmar	174	Russian Federation	218	Yemen
43	Djibouti	87	Indonesia	131	Myanmar	175	Russian Federation	219	Yemen
44	Egypt	88	Indonesia	132	Myanmar	176	Russian Federation	*220*	*Yemen**

Bold means the conflict had not ended in December 2007.* denotes those conflicts that had not ended – in accordance with the two-year rule – in December 2011. Italics means the conflict began after 2007 and is therefore not included in the analysis.

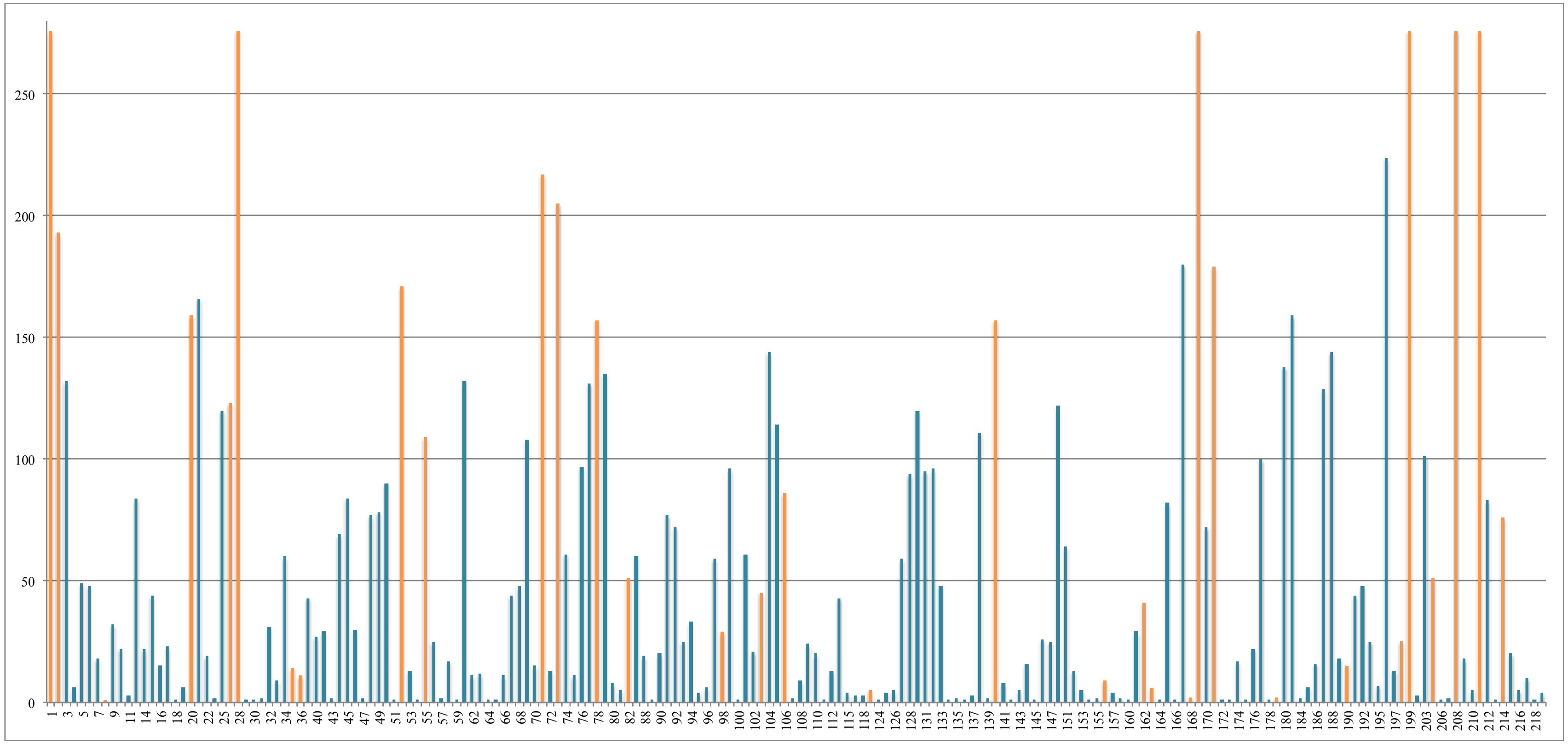

Figure C.1 Histogram showing the duration of all conflicts between 1985 and 2007. The orange pillars mark ongoing conflicts at the end of 2007.

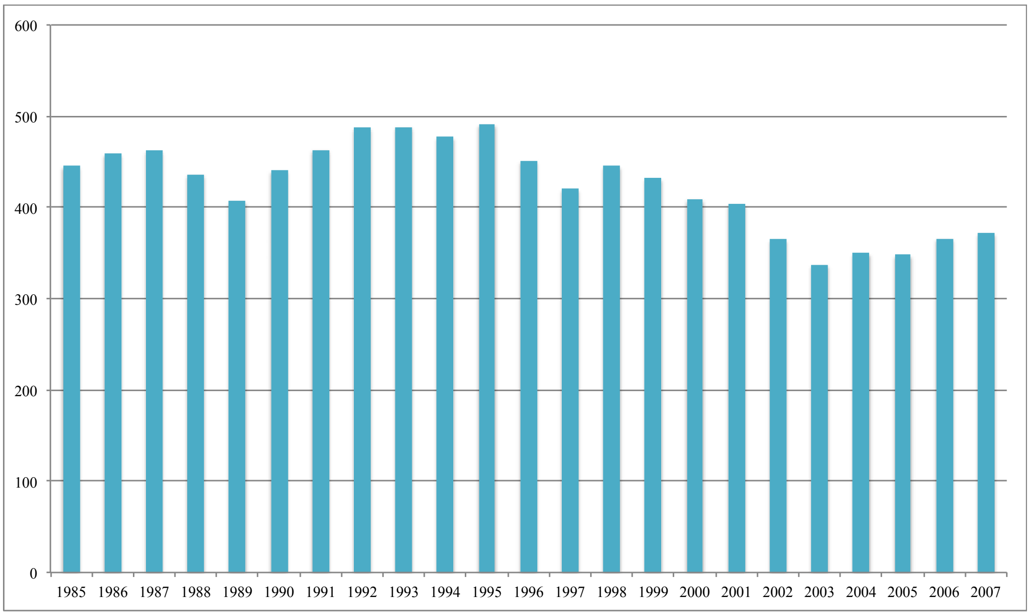

Figure C.2 Histogram showing the yearly distribution of conflict months, 1985-2007.

Table C.4 Conflicts beginning before 1985, listing the years the control variables stem from.

Conflict ID	Country	Year before conflict outbreak	Population year	GDP per capita year	IMR year
1	Afghanistan	1977	1977	1977	1977
3	Angola	1974	1974	1974	*1970-1975**
13	Bangladesh	1974	1974	1974	1974
21	Cambodia	1977	1977	1977	1977
25	Chad	1985	1985	1985	1985
28	Colombia	1963	1963	*1970*	1963
45	El Salvador	1978	1978	1978	1978
48	Ethiopia	1963	1963	*1990**	*1960-1965**
49	Ethiopia	1975	1975	*1990**	1975
50	Ethiopia	1982	1982	*1990*	1982
61	Guatemala	1964	1964	*1970*	1964
67	India	1978	1978	1978	1978
68	India	1981	1981	1981	1981
69	India	1982	1982	1982	1982
87	Indonesia	1974	1974	1974	1974
92	Iran	1978	1978	1978	1978
99	Iraq	1972	1972	1972	1972
104	Israel	1948	*1950*	*1970**	*1950-1955**
109	Lebanon	1981	1981	1981	1981
127	Morocco	1974	1974	1974	1974
128	Mozambique	1976	1976	1976	1976
130	Myanmar	1947	*1950*	*1970**	*1950-1955**
131	Myanmar	1948	*1950*	*1970**	*1950-1955**
132	Myanmar	1960	1960	*1970**	*1960-1965**
133	Myanmar	1975	1975	1975	1975
151	Nicaragua	1981	1981	1981	1981
167	Peru	1981	1981	1981	1981
169	Philippines	1968	1968	*1970*	1968
170	Philippines	1969	1969	*1970*	1969
188	Somalia	1981	1981	1981	1981
191	South Africa	1965	1965	*1970*	*1965-1970**
192	South Africa	1980	1980	1980	1980
194	Spain	1984	1984	1984	1984
196	Sri Lanka	1983	1983	1983	1983
199	Sudan	1982	1982	*1990**	1982
208	Turkey	1983	1983	1983	1983
211	Uganda	1978	1978	1978	1978
212	United Kingdom	1970	1970	1970	1970

Italics indicate that the year the variable stems from differs from the actual year before conflict because of lacking data. * indicates that the data is gathered from the UNdata database.

Appendix D

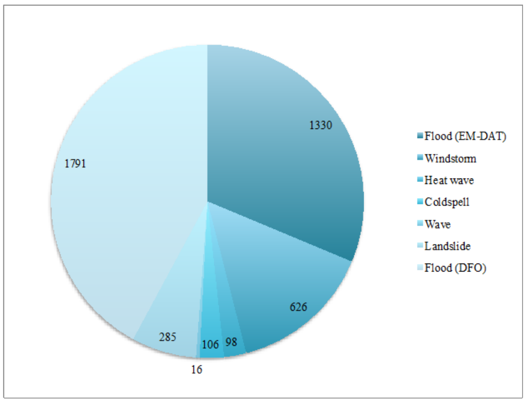

Figure D.1 Pie chart showing the distribution of hazard-months 1985-2007.

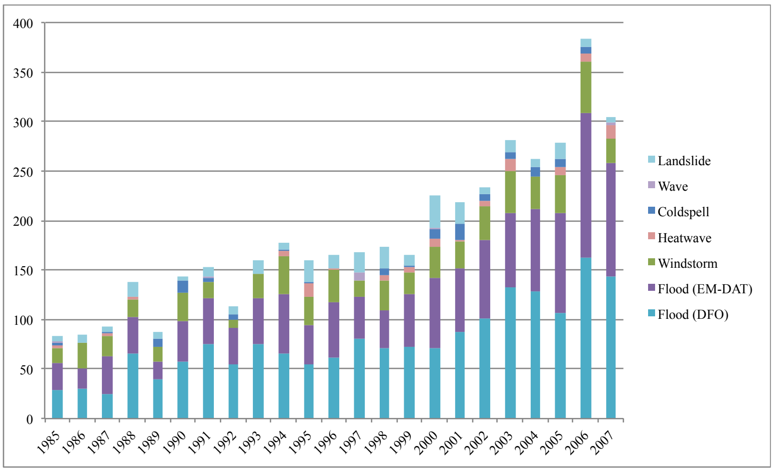

Figure D.2 Histogram showing the yearly distribution of hazard-months, 1985-2007.

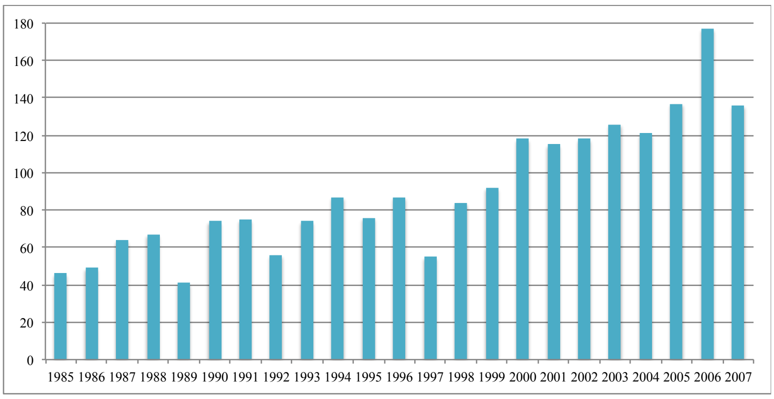

Figure D.3 Histogram showing the yearly distribution of the *ed_all* variable, 1985-2007.

Table D.1 Distribution of hazard-months across countries, 1985-2007.

Country	Total conflict-months	Number of conflicts	DFO flood	EM-DAT Flood	Windstorm	Heat wave	Coldspell	Wave	Landslide
Afghanistan	276	1	48	47	5	0	6	0	9
Algeria	193	1	21	27	3	2	0	1	1
Angola	254	6	13	14	0	0	0	0	1
Azerbaijan	57	3	0	0	0	0	0	0	0
Bangladesh	106	2	38	25	28	1	7	0	0
Bosnia and Herzegovina	82	3	0	0	0	0	0	0	0
Burkina Faso	1	1	0	0	0	0	0	0	0
Burundi	165	2	4	11	4	0	0	0	0
Cambodia	166	1	7	5	1	0	0	0	0
Central African Republic	21	2	0	0	0	0	0	0	0
Chad	243	2	16	12	1	0	0	0	0
Colombia	276	1	55	61	5	0	0	1	18
Comoros	2	2	0	0	0	0	0	0	0
Congo (Brazzaville)	42	3	6	4	0	0	0	0	0
Congo (Kinshasa)	85	3	8	6	0	0	0	0	0
Côte d'Ivoire	27	1	0	0	0	0	0	0	0
Croatia	43	1	0	0	0	0	0	0	0
Djibouti	31	2	0	1	0	0	0	0	0
Egypt	69	1	5	5	1	2	0	0	1
El Salvador	84	1	2	1	0	0	0	0	1
Eritrea	32	2	2	2	0	0	0	0	0
Ethiopia	540	8	86	80	0	0	0	0	7
Georgia	45	4	0	0	0	0	0	0	0
Guatemala	132	1	5	4	0	0	0	0	1
Guinea	11	1	0	0	0	0	0	0	0
Guinea-Bissau	12	1	0	0	0	0	0	0	0
Haiti	13	3	4	2	1	0	0	0	0
India	1,306	16	611	490	253	81	67	8	134
Indonesia	177	5	65	37	3	0	0	1	21
Iran	228	7	54	53	5	0	2	0	2
Iraq	224	5	3	2	0	0	0	0	0
Israel	346	4	16	2	2	0	2	0	0
Laos	9	1	0	0	0	0	0	0	0
Lebanon	44	2	0	0	0	0	0	0	0
Lesotho	1	1	0	0	0	0	0	0	0
Liberia	56	2	0	0	0	0	0	0	0
Macedonia	4	1	0	0	0	0	0	0	0
Mali	11	3	3	3	0	0	0	0	0
Mexico	5	2	2	1	1	0	0	0	0
Moldova	5	1	0	0	0	0	0	0	0
Morocco	59	1	0	0	0	0	0	0	1
Mozambique	94	1	3	2	2	0	0	0	0
Myanmar	718	18	57	27	10	0	0	0	2
Nepal	122	1	25	13	0	0	3	0	6

Nicaragua	64	1	2	0	1	0	0	0	0
Niger	30	5	6	5	0	0	0	0	0
Nigeria	6	2	2	2	0	0	0	0	0
Pakistan	77	4	23	23	6	3	0	0	6
Panama	1	1	0	0	0	0	0	0	0
Papua New Guinea	82	1	3	2	1	0	0	0	1
Paraguay	1	1	0	0	0	0	0	0	0
Peru	182	2	24	15	0	0	1	0	12
Philippines	527	3	227	106	193	0	0	5	40
Romania	1	1	0	0	0	0	0	0	0
Russian Federation	144	7	53	34	18	2	11	0	6
Rwanda	138	1	3	3	0	0	0	0	0
Senegal	159	1	6	6	1	0	0	0	0
Serbia	24	3	0	0	0	0	0	0	0
Sierra Leone	129	1	0	1	1	0	0	0	0
Somalia	177	3	15	13	1	0	0	0	0
South Africa	92	2	8	4	0	0	0	0	0
Spain	32	2	2	1	1	0	0	0	0
Sri Lanka	262	3	38	31	2	0	0	0	1
Sudan	276	1	28	27	1	0	0	0	0
Suriname	3	1	0	0	0	0	0	0	0
Tajikistan	101	1	7	7	1	0	0	0	3
Thailand	51	1	31	23	4	0	0	0	1
Togo	1	1	0	0	0	0	0	0	0
Trinidad and Tobago	2	1	1	0	1	0	0	0	0
Turkey	299	3	50	30	8	3	5	0	8
Uganda	276	1	32	23	3	0	0	0	1
United Kingdom	84	2	9	0	8	0	1	0	0
United States of America	76	1	62	37	50	4	1	0	1
Uzbekistan	25	2	0	0	0	0	0	0	0
Venezuela	10	1	0	0	0	0	0	0	0
Yemen	5	2	0	0	0	0	0	0	0
Total	9,754	192	1,791	1,330	626	98	106	16	285

Tables D.2 a)-g) Top 10 countries in terms of different types of hazard-months, 1985-2007.

a)

Country	Conflict-months with flood (DFO)	Conflict-months without flood (DFO	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	611	695	1,306	16	38.19	46.78
Philippines	227	300	527	3	75.67	43.07
Ethiopia	86	454	540	8	10.75	15.93
Indonesia	65	112	177	5	13	36.72
United States of America	62	14	76	1	62	81.58
Myanmar	57	661	718	18	3.17	7.94
Colombia	55	221	276	1	55	19.93
Iran	54	174	228	7	7.71	23.68
Russian Federation	53	91	144	7	7.57	36.81
Turkey	50	249	299	3	16.67	16.72

b)

Country	Conflict-months with flood (EM-DAT)	Conflict-months without flood (EM-DAT)	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	490	816	1,306	16	30.63	37.52
Philippines	106	421	527	3	35.33	20.11
Ethiopia	80	460	540	8	10	14.81
Colombia	61	215	276	1	61	22.10
Iran	53	175	228	7	7.57	23.25
Afghanistan	47	229	276	1	47	17.03
Indonesia	37	140	177	5	7.4	20.90
United States of America	37	39	76	1	37	48.68
Russian Federation	34	110	144	7	4.86	23.61
Sri Lanka	31	231	262	3	10.33	11.83

c)

Country	Conflict-months with windstorm	Conflict-months without windstorm	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	253	1,053	1,306	16	15.81	19.37
Philippines	193	334	527	3	64.33	36.62
United States of America	50	26	76	1	50	65.79
Bangladesh	28	78	106	2	14	26.42
Russian Federation	18	126	144	7	2.57	12.50
Myanmar	10	708	718	18	0.56	1.39
Turkey	8	291	299	3	2.67	2.68
United Kingdom	8	76	84	2	4	9.52
Pakistan	6	71	77	4	1.5	7.79
Afghanistan	5	271	276	1	5	1.81

d)

Country	Conflict-months with heat wave	Conflict-months without heat wave	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	81	1,225	1,306	16	5.06	6.20
United States of America	4	72	76	1	4	5.26
Pakistan	3	74	77	4	0.75	3.90
Turkey	3	296	299	3	1	1.00

Algeria	2	191	193	1	2	1.04
Egypt	2	67	69	1	2	2.90
Russian Federation	2	142	144	7	0.29	1.39
Bangladesh	1	105	106	2	0.5	0.94
Afghanistan	0	276	276	1	0	0
Angola	0	254	254	6	0	0

e)

Country	Conflict-months with coldspell	Conflict-months without coldspell	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	67	1,239	1,306	16	4.19	5.13
Russian Federation	11	133	144	7	1.57	7.64
Bangladesh	7	99	106	2	3.5	6.60
Afghanistan	6	270	276	1	6	2.17
Turkey	5	294	299	3	1.67	1.67
Nepal	3	119	122	1	3	2.46
Iran	2	226	228	7	0.29	0.88
Israel	2	344	346	4	0.5	0.58
Peru	1	181	182	2	0.5	0.55
United Kingdom	1	83	84	2	0.5	1.19

f)

Country	Conflict-months with wave	Conflict-months without wave	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	8	1,298	1,306	16	0.5	0.61
Philippines	5	522	527	3	1.67	0.95
Algeria	1	192	193	1	1	0.52
Colombia	1	275	276	1	1	0.36
Indonesia	1	176	177	5	0.2	0.56
Afghanistan	0	276	276	1	0	0
Angola	0	254	254	6	0	0
Azerbaijan	0	57	57	3	0	0
Bangladesh	0	106	106	2	0	0
Bosnia and Herzegovina	0	82	82	3	0	0

g)

Country	Conflict-months with landslide	Conflict-months without landslide	Total conflict-months	Number of conflicts	Disaster-months per conflict	Percentage of disaster months of total conflict-months
India	134	1,172	1,306	16	8.38	10.26
Philippines	40	487	527	3	13.33	7.59
Indonesia	21	156	177	5	4.2	11.86
Colombia	18	258	276	1	18	6.52
Peru	12	170	182	2	6	6.59
Afghanistan	9	267	276	1	9	3.26
Turkey	8	291	299	3	2.67	2.68
Ethiopia	7	533	540	8	0.88	1.30
Nepal	6	116	122	1	6	4.92
Pakistan	6	71	77	4	1.5	7.79

Table D.3 List of wave months, 1985-2007. Sorted on the lowest number of years between the wave and conflict end.

Conflict ID	Country	Year of wave	Month of wave	Conflict start year	Conflict end year	Conflict duration in years	Years from wave to conflict end
74	India	1997	August	1992	1997	5	0
80	India	1997	August	1997	1997	0	0
76	India	1997	August	1992	2000	8	3
87	Indonesia	1985	June	1975	1989	14*	4
169	Philippines	1991	November	1969	1995	26*	4
2	*Algeria*	*2007*	*August*	*1991*	*2013*	*22*	*6*
169	*Philippines*	*2007*	*November*	*1999*	*2013*	*14*	*6*
171	*Philippines*	*2007*	*November*	*1993*	*2013*	*20*	*6*
77	India	1997	August	1993	2004	11	7
79	India	1997	August	1997	2004	7	7
78	*India*	*1997*	*August*	*1994*	*2010*	*16*	*13*
169	*Philippines*	*2000*	*January*	*1999*	*2013*	*14*	*13*
171	*Philippines*	*2000*	*January*	*1993*	*2013*	*20*	*13*
28	*Colombia*	*1999*	*November*	*1964*	*2013*	*49**	*14*
71	*India*	*1997*	*August*	*1989*	*2013*	*24*	*16*
73	*India*	*1997*	*August*	*1996*	*2013*	*17*	*16*

Italics indicate that the end year of conflict is outside of the sample range, meaning that the conflicts are coded in the dataset as ongoing. * indicate that the conflict began before the dataset begins, so the number of years of conflict duration is lower in the dataset than shown here.

Table D.4 List of the landslide-months where the conflict ended within one year after the occurrence of the slide, 1985-2007.

Conflict ID	Country	Year of landslide	Month of landslide	Conflict start year	Conflict end year	Duration in years	Years from flood to conflict end
48	Ethiopia	1991	4	1964	1991	27*	0
49	Ethiopia	1991	4	1976	1991	15*	0
67	India	1988	3	1979	1988	9*	0
68	India	1988	3	1983	1988	5*	0
67	India	1988	7	1979	1988	9*	0
68	India	1988	7	1982	1988	6*	0
70	India	1990	9	1989	1990	1	0
69	India	1993	9	1983	1993	10*	0
74	India	1997	6	1992	1997	5	0
80	India	1997	6	1997	1997	0	0
74	India	1997	8	1992	1997	5	0
80	India	1997	8	1997	1997	0	0
76	India	2000	6	1992	2000	8	0
76	India	2000	7	1992	2000	8	0
76	India	2000	8	1992	2000	8	0
81	India	2000	8	2000	2000	0	0
87	Indonesia	1989	1	1975	1989	14*	0
88	Indonesia	1991	1	1990	1991	1	0
91	Indonesia	2005	2	1999	2005	6	0

91	Indonesia	2005	9	1999	2005	6	0
92	Iran	1990	2	1979	1990	11*	0
150	Nepal	2006	8	1996	2006	10	0
150	Nepal	2006	9	1996	2006	10	0
161	Pakistan	1996	3	1994	1996	2	0
167	Peru	1999	11	1982	1999	17*	0
174	Russian Federation	1991	5	1990	1991	1	0
203	Tajikistan	1998	2	1992	1998	6	0
209	Turkey	1992	1	1991	1992	1	0
209	Turkey	1992	2	1991	1992	1	0
53	Ethiopia	1994	12	1994	1995	1	1
69	India	1992	8	1983	1993	10*	1
74	India	1996	9	1992	1997	5	1
77	India	2003	3	1993	2004	11	1
79	India	2003	3	1997	2004	7	1
79	India	2005	2	1997	2006	9	1
79	India	2005	8	1997	2006	9	1
87	Indonesia	1988	2	1975	1989	14*	1
91	Indonesia	2004	1	1999	2005	6	1
91	Indonesia	2004	3	1999	2005	6	1
91	Indonesia	2004	4	1999	2005	6	1
127	Morocco	1988	2	1975	1989	14*	1
167	Peru	1998	1	1982	1999	17*	1
170	Philippines	1989	5	1970	1990	20*	1
170	Philippines	1989	9	1970	1990	20*	1
169	Philippines	1994	2	1969	1995	26*	1
169	Philippines	1994	9	1969	1995	26*	1
176	Russian Federation	1995	9	1994	1996	2	1
203	Tajikistan	1997	11	1992	1998	6	1

* indicate that the conflict began before the dataset begins, so the number of years of conflict duration is lower in the dataset than shown here.

Appendix E

Table E.1 List of wars in the war-dataset, 1985-2007.

Conflict ID	Country	Number of conflict-months	Percent of total conflict months
1	**Afghanistan**	276	24.19
9	Azerbaijan	32	2.80
15	Bosnia and Herzegovina	44	3.86
25	Chad*	12	1.05
48	Ethiopia	77	6.75
49	Ethiopia	78	6.84
58	Georgia	17	1.49
103	**Iraq**	45	3.94
113	Liberia	43	3.77
130	Myanmar*	48	4.21
131	Myanmar	95	8.33
163	**Pakistan**	6	0.53
176	Russian Federation	22	1.93
180	Rwanda*	46	4.03
185	Serbia	6	0.53
186	Serbia	16	1.40
191	South Africa	44	3.86
196	Sri Lanka*	204	17.88
198	**Sri Lanka**	25	2.19
218	Yemen	1	0.09
219	Yemen	4	0.35
Total		1141	100

Bold indicates that the conflict was still ongoing in December 2007, while * means that the conflict de-escalated from war to minor conflict after the end coded here, but did not end.

Table E.2 Descriptive statistics for the war-dataset.

Variable	Obs.	Mean	St. Dev.	Min	Max	Frequency 0 (%)	Frequency 1 (%)
Flood (DFO)	1141	-	-	0	1	1,037 (90.89)	104 (9.11)
Flood (EM-DAT)	1141	-	-	0	1	1,045 (91.59)	96 (8.41)
Windstorm	1141	-	-	0	1	1,130 (99.04)	11 (0.96)
Coldspell	1141	-	-	0	1	1,133 (99.3)	8 (0.7)
Landslide	1141	-	-	0	1	1,128 (98.86)	13 (1.14)
All disasters (EM-DAT)	1141	-	-	0	1	1,019 (89.31)	122 (10.69)
Flood occurrence the past 6 months (DFO)	1141	-	-	0	1	831 (72.83)	310 (27.17)
Flood occurrence the past 6 months (EM-DAT)	1141	-	-	0	1	842 (73.79)	299 (26.21)
Windstorm occurrence the past 6 months	1141	-	-	0	1	1,102 (96.58)	39 (3.42)
Coldspell occurrence the past 6 months	1141	-	-	0	1	1,113 (97.55)	28 (2.45)
Landslide occurrence the past 6 months	1141	-	-	0	1	1,085 (95.09)	56 (4.91)
All EM-DAT indicators the past 6 months	1141	-	-	0	1	796 (69.76)	345 (30.24)
Regime durability	1141	12.047	17.646	0	56	-	-
Infant Mortality Rate (ln)	1141	4.438	0.898	2.501	5.365	-	-
Population (ln)	1141	16.541	0.680	14.824	18.896	-	-

Appendix F

Table F.1 Do-file describing the preparation of the data for survival analysis.

```
**Generating a conflict id-variable**
sort country year month id
egen confl_id= group (country id)
order confl_id, before(f_id)


**Generating end and start counts (duration)**
sort confl_id, stable

quietly by confl_id:  gen c_endnd = cond(_N==1,0,_n)
replace c_endnd=1 if c_endnd==0

gen c_startnd=c_endnd-1


**stsetting the data,(c_status has been manually punched in)**
stset c_endnd, id(confl_id) failure(c_status==1) origin(time c_startnd)
```

Table F.2 Do-file showing the regression commands.

```
***Regression Models***
**(1)/(7)**
streg f_flood durable lnIMR lnpop if year<2008, dist(weib) cluster(country)

**(2)/(8)**
streg last6flood_dfodummy durable lnIMR lnpop if year<2008, dist(weib) ///
cluster(country)

**(3)**
streg ed_flood ed_windstorm ed_heatwave ed_coldspell ed_wave ed_slide ///
durable lnIMR lnpop if year<2008, dist(weib) cluster(country)

**(4)/(10)**
streg emdat_all durable lnIMR lnpop if year<2008, dist(weib) cluster(country)

**(5)**
streg last6flood_eddummy last6windstorm_dummy last6heatwave_dummy ///
last6coldspell_dummy last6wave_dummy last6slide_dummy durable lnIMR ///
lnpop if year<2008, dist(weib) cluster(country)

**(6)/(12)**
streg last6all_eddummy durable lnIMR lnpop if year<2008, dist(weib)///
cluster(country)

**(9)**
streg ed_flood ed_windstorm ed_coldspell ed_slide durable lnIMR lnpop ///
if year<2008, dist(weib) cluster(country)

**(11)**
streg last6flood_eddummy last6windstorm_dummy last6coldspell_dummy ///
last6slide_dummy durable lnIMR lnpop if year<2008, dist(weib) cluster(country)
```

Appendix G

Table G.1 All variables in the dataset.

Variable	Origin	Description
country	Frame dataset	UN member states, country name after ISO-coding.
year	Frame dataset	Year of observation.
month	Frame dataset	Month of observation.
confl_id		The unique identifier of all continuous conflicts-episodes.
c_status		Codes whether the conflict episode ended, assuming the value 1 the last month of conflict and 0 all the conflict-months the conflict was still ongoing.
c_startnd		The duration (in months) of the conflict up until the start of the observation. That means that the first observation in a conflict is equal to 0 while the second month is recorded as 1.
c_endnd		Records the duration of the conflict (in months), the first month of conflict equals 1, the second 2 and so forth.
f_flood	DFO Floods	Dummy recording whether one or more floods occurred in the given conflict-month.
ed_flood	EM-DAT	Dummy recording whether one or more floods occurred in the given conflict-month.
ed_windstorm	EM-DAT	Dummy recording whether one or more windstorms occurred in the given conflict-month.
ed_heatwave	EM-DAT	Dummy recording whether one or more heat waves occurred in the given conflict-month.
ed_coldspell	EM-DAT	Dummy recording whether one or more coldspells occurred in the given conflict-month.
ed_wave	EM-DAT	Dummy recording whether one or more tidal waves or a surge occurred in the given conflict-month.
ed_slide	EM-DAT	Dummy recording whether one or more landslides occurred in the given conflict-month.
emdat_all	EM-DAT	Dummy recording whether one or more of the EM-DAT disasters occurred in the given conflict-month.
last6flood_dfodummy		Dummy recording whether one or more floods (DFO) occurred the past six conflict-months.
last6flood_eddummy		Dummy recording whether one or more floods (EM-DAT) occurred the past six conflict-months.
last6windstorm_dummy		Dummy recording whether one or more windstorms occurred the past six conflict-months.
last6heatwave_dummy		Dummy recording whether one or more heat waves occurred the past six conflict-months.
last6coldspell_dummy		Dummy recording whether one or more coldspells occurred the past six conflict-months.
last6wave_dummy		Dummy recording whether one or more tidal waves or surges occurred the past six conflict-months.
last6slide_dummy		Dummy recording whether one or more landslides occurred the past six conflict-months.
last6all_eddummy		Dummy recording whether one or more EM-DAT disasters occurred the past six conflict-months.
durable	Polity IV	The number of years since the most recent regime change (defined by a three-point change in the Polity score over a period of three years or less) or the end of transition period defined by the lack of stable political institutions (denoted by a standardized authority score). In calculating the durable value, the first year during which a new (post-change) polity is established is coded as the baseline "year zero" (value = 0) and each subsequent year adds one to the value of the durable variable consecutively until a new regime change or transition period occurs.
lnGDPcapita		The natural logarithm of the gross national product per capita the year before conflict outbreak.
lnIMR		The natural logarithm of the infant mortality rate (per 1000 live births) the year before conflict outbreak.

lnpop		The natural logarithm of the population the year before conflict outbreak.
c_war		Dummy recording whether the conflict is defined as a war according to the UCDP-PRIO intensity variable.
c_territory		Dummy recording whether the incompatibility was territorial or governmental according to the UCDP-PRIO incompatibility variable.
c_internationalized		Dummy recording whether the conflict was internationalized according to the UCDP-PRIO type of conflict-variable.
c_bdbest	UCDP Battle-Related Deaths	The UCDP Best estimate for battle-related deaths in the conflict/dyad in the given year.
developing	World Bank	Dummy recording whether the country is classified as developing or not. The classification follows the World Bank's assertion that "developing" denotes low- and middle-income countries (lending-groups).
fragment	Polity IV	Codes the operational existence of a separate polity, or polities, comprising substantial territory and population within the recognized borders of the state and over which the coded polity exercises no effective authority. 0: No overt fragmentation 1: Slight fragmentation: Less than ten percent of the country's territory is effectively under local authority and actively separated from the central authority of the regime. 2: Moderate fragmentation: Ten to twenty-five percent of the country's territory is effectively ruled by local authority and actively separated from the central authority of the regime. 3: Serious fragmentation: Over twenty-five percent (and up to fifty percent) of the country's territory is effectively ruled by local authority and actively separated from the central authority of the regime.
polity4	Polity IV	The Polity score is computed by subtracting the autocracy score from the democracy score; the resulting unified polity scale ranges from +10 (strongly democratic) to -10 (strongly autocratic).
GDPcapita	United Nations	Gross domestic product in the country the year before conflict outbreak, measured in current US Dollars.
population	World Bank	Population in the country the year before conflict outbreak.
IMR	World Bank	Infant mortality rate, per 1000 live births, in the country the year before conflict outbreak.
c_id	UCDP-PRIO	The unique identifier of all conflicts.
c_incompt	UCDP-PRIO	A general coding of the conflict issue, the incompatibility is coded in three categories: 1. Territory. 2. Government. 3. Government and Territory.
c_intensity	UCDP-PRIO	The intensity level in the dyad per calendar year. Two different intensity levels are coded 1. Minor: between 25 and 999 battle-related deaths in a given year. 2. War: at least 1,000 battle-related deaths in a given year.
c_type	UCDP-PRIO	Two different types of conflict included here: 3. Internal armed conflict occurs between the government of a state and one or more internal opposition group(s) without intervention from other states. 4. Internationalized internal armed conflict occurs between the government of a state and one or more internal opposition group(s) with intervention from other states (secondary parties) on one or both sides.
c_startdate	UCDP-PRIO	The date, as precise as possible, of the first battle-related death in the conflict.
c_startdate2	UCDP-PRIO	The date, as precise as possible, when a given episode of conflict activity reached 25 battle-related deaths in a year.
c_enddate	UCDP-PRIO	The date, as precise as possible, when conflict activity ended.
c_duration		The duration of conflict in days, based on the c_startdate2 and c_enddate variables.
c_count		The number of unique conflicts going on in the same country at the same time.
iso3	Frame dataset	ISO three-letter code.

f_id	DFO Floods	Register uniquely identifying each flood.
f_detailedlocation	DFO Floods	Detailed location of where the flood hit, includes names of the states, provinces, counties, towns, and cities.
f_began	DFO Floods	The day the flood started. Occasionally there is no specific beginning date mentioned in news reports, only a conflict-month; in that case the DFO date will be the middle of that conflict-month.
f_ended	DFO Floods	The day the flood ended, often harder to assess than f_began; an estimate is then made.
f_duration	DFO Floods	The duration of the flood in days, derived from f_began and f_ended.
f_maincause	DFO Floods	Heavy rain, Tropical cyclone, Extra-tropical cyclone, Monsoonal rain, Snowmelt, Rain and snowmelt, Ice jam/break-up, Dam/Levy, break or release, Brief torrential rain, Tidal surge, Avalanche related. Information about secondary causes is in the Notes and Comments section of the table.
f_severity	DFO Floods	Assessment is on 1-2 scale. These floods are divided into three classes. Class 1: large flood events: significant damage to structures or agriculture; fatalities; and/or 1-2 decades-long reported interval since the last similar event. Class 1.5: very large events: with a greater than 2 decades but less than 100 year estimated recurrence interval, and/or a local recurrence interval of at 1-2 decades and affecting a large geographic region (> 5000 sq. km). Class 2: Extreme events: with an estimated recurrence interval greater than 100 years.
f_count		Recording the number of floods (DFO) that occurred in the given conflict-month.
edfl_count		Recording the number of floods (EM-DAT) that occurred in the given conflict-month.
edws_count		Recording the number of windstorms that occurred in the given conflict-month.
edsl_count		Recording the number of landslides that occurred in the given conflict-month.
emdat_count		Recording the number of EM-DAT disasters that occurred in the given conflict-month.
lagf_flood		Dummy recording whether one or more floods (DFO) occurred in the given conflict-month, lagged with one month.
lagf_count		Recording the number of floods (DFO) that occurred in the given conflict-month, lagged with one month.
laged_flood		Dummy recording whether one or more floods (EM-DAT) occurred in the given conflict-month, lagged with one month.
lagedfl_count		Recording the number of floods (EM-DAT) that occurred in the given conflict-month, lagged with one month.
laged_windstorm		Dummy recording whether one or more windstorms occurred in the given conflict-month, lagged with one month.
lagedws_count		Recording the number of windstorms that occurred in the given conflict-month, lagged with one month.
laged_heatwave		Dummy recording whether one or more heat waves occurred in the given conflict-month, lagged with one month.
laged_coldspell		Dummy recording whether one or more coldspells occurred in the given conflict-month, lagged with one month.
laged_wave		Dummy recording whether one or more tidal waves or a surge occurred in the given conflict-month, lagged with one month.
laged_slide		Dummy recording whether one or more landslides occurred in the given conflict-month, lagged with one month.
lagedsl_count		Recording the number of landslides that occurred in the given conflict-month, lagged with one month.
lagemdat_all		Dummy recording whether one or more of the EM-DAT disasters occurred in the given conflict-month, lagged with one month.
lagemdat_count		Recording the number of EM-DAT disasters that occurred in the given conflict-month, lagged with one month.
ever_count_flood_dfo		Cumulative count of the number of floods (DFO) occurring within each conflict.
ever_count_flood_ed		Cumulative count of the number of floods (EM-DAT) occurring within each conflict.

ever_count_windstorm		Cumulative count of the number of windstorms occurring within each conflict.
ever_count_heatwave		Cumulative count of the number of heat waves occurring within each conflict.
ever_count_coldspell		Cumulative count of the number of coldspells occurring within each conflict.
ever_count_wave		Cumulative count of the number of tidal waves or surges occurring within each conflict.
ever_count_slide		Cumulative count of the number of landslides occurring within each conflict.
ever_count_all_ed		Cumulative count of the number of EM-DAT disasters occurring within each conflict.
last6flood_dfo		Recording the number of floods (DFO) that occurred in the past six conflict-months.
last6flood_ed		Recording the number of floods (EM-DAT) that occurred in the past six conflict-months.
last6windstorm		Recording the number of windstorms that occurred in the past six conflict-months.
last6heatwave		Recording the number of heat waves that occurred in the past six conflict-months.
last6coldspell		Recording the number of coldspells that occurred in the past six conflict-months.
last6wave		Recording the number of tidal waves or surges that occurred in the past six conflict-months.
last6slide		Recording the number of landslides that occurred in the past six conflict-months.
last6all_ed		Recording the number of EM-DAT disaster that occurred in the past six conflict-months.
last12flood_dfo		Recording the number of floods (DFO) that occurred in the past 12 conflict-months.
last12flood_ed		Recording the number of floods (EM-DAT) that occurred in the past 12 conflict-months.
last12windstorm		Recording the number of windstorms that occurred in the past 12 conflict-months.
last12heatwave		Recording the number of heat waves that occurred in the past 12 conflict-months.
last12coldspell		Recording the number of coldspells that occurred in the past 12 conflict-months.
last12wave		Recording the number of tidal waves or surges that occurred in the past 12 conflict-months.
last12slide		Recording the number of landslides that occurred in the past 12 conflict-months.
last12all_ed		Recording the number of EM-DAT disaster that occurred in the past 12 conflict-months.
last12flood_dfodummy		Dummy recording whether one or more floods (DFO) occurred the past 12 conflict-months.
last12flood_eddummy		Dummy recording whether one or more floods (EM-DAT) occurred the past 12 conflict-months.
last12windstorm_dummy		Dummy recording whether one or more windstorms occurred the past 12 conflict-months.
last12heatwave_dummy		Dummy recording whether one or more heat waves occurred the past 12 conflict-months.
last12coldspell_dummy		Dummy recording whether one or more coldspells occurred the past 12 conflict-months.
last12wave_dummy		Dummy recording whether one or more tidal waves or surges occurred the past 12 conflict-months.
last12slide_dummy		Dummy recording whether one or more landslides occurred the past 12 conflict-months.
last12all_eddummy		Dummy recording whether one or more EM-DAT disasters occurred the past 12 conflict-months.