INTRODUCTION

The World Health Organization defines a disaster as “any occurrence that causes damage, ecological disruption, loss of human life, deterioration of health and health services, on a scale sufficient to warrant an extraordinary response from outside the affected community or area.” Such occurrences – whether induced by natural hazards, regulatory failure, or mismanagement of technology – have become widespread in our era, with significant implications for public life in many societies. When looking at natural disasters in particular a useful distinction can be drawn between rapid-onset and slow-onset disasters. The latter includes droughts, epidemics, and infestations. Hurricanes, floods, volcanic eruptions, and earthquakes stand out as examples of the former. These nature-induced events are becoming more frequent and are costly in terms of human life, destruction of livelihoods, and in terms of the cost of adaptation and reconstruction.

The question that has been raised by recent Political Science research is whether these events are also costly politically. Do they contribute to political-societal integration, or do they trigger conflict, rivalry and resistance? What I want to argue in this paper is that the answer is *It all depends*. To make this point I target a common framing of the Political Science debate about the political consequences of natural disasters, and then suggest a broader classification of the ‘it-all-depends’ factors that have received and/or deserve some research attention. I pay attention mostly to cross-national large $n$ studies, but what I suggest also has relevance for case study approaches.

**DEPENDENT AND INDEPENDENT VARIABLES**
Natural disasters are political events (Davis & Seitz, 1982; Seitz & Davis, 1984): A disaster is not sufficiently precipitated by a natural hazard such as torrential rain, for instance. At best, the torrential downpour provides a natural shock. It is only when/if this shock occurs in a population that are vulnerable to its effects, and there is inadequate response by local/national authorities to mitigate these vulnerabilities and deal with the effects of the hazard, that a natural disaster occurs. So, disasters are fundamentally political events (see also Olson and Gawronski, 2010). Natural disasters also contribute to political change, as exemplified by the history of Portugal in the 18th century, and Nicaragua and Haiti in more recent times (see Buchenau & Johnson, 2009). But, it is important to remember that the causal arrow points in both directions. This complicates the research challenge, which is already daunting enough as we are dealing with the interaction of complex, non-linear systems with each another. It is to be expected that this interaction is also non-linear, complex, and context dependent.

Despite the obvious political nature/consequences of natural disasters, Political Science has been slow to pick-up on the salience and significance of them. Large n studies on their political determinants and consequences have been undertaken only recently (some examples: Drury and Olson, 1998; Barncati, 2007; Nel and Righarts, 2008; Slettebak and Soysa, 2010; Omelicheva, 2011; Slettebak, 2012). Most of the work done focuses on the political consequences of natural disasters and specifically on the question whether they pose a risk for political conflict, variously defined. The academic literature looking at this is responding to the publically expressed expectations that one of the effects of climate change would be to increase the incidence of disasters and with it the risk of conflict, both within and between nations. As a result, climate-related disasters have received the most attention, almost to the exclusion of other natural disasters, in particular earthquakes and volcanic eruptions (but see Brancati, 2007, and Nel and Righarts, 2008).

So far the research results have been wide-ranging and even contradictory in some cases. As other reviewers also have pointed out (Salehyan, 2008; Bernauer et al. 2012; Gledittsch, 2012; Scheffran et al. 2012), this is to be expected in a field where there is no agreement on how political conflict should be operationalized, and where the data sources on both the dependent and independent variables are diverse in terms of specificity and reliability. Political conflict can range from, on the one hand politically-fuelled massacres, full-blown civil war with significant loss of life, adverse regime change, and state collapse (Goldstone et al., 2010), to irregular but largely violence-less skirmishes, riots, and demonstrations on the other hand. As the former are obviously disturbing and
eye-catching events, they are treated as clear signals of political disintegration or centrifugal tendencies, and because they are easier to identify and to quantify, much of the cross-national literature has focused on these (Omelicheva, 2011, for instance). However, a case can be made that the latter category of events can be significant cumulatively as they influence the distribution of collective action resources in a society and can thus destabilize a polity with significant long-term effects (Brancati, 2007; Drury and Olson, 1998). Note that ‘destabilizing’ is here not used in a pejorative sense: Political vibrant societies are healthy societies, although they are not necessarily stable. The most stable political context is a graveyard (Sanders, 1981).

Research findings on the risk that natural disasters, and in particular climate-related disasters perturbations, pose for large-scale political conflict as discussed above have indeed been contradictory and specification dependent. Here are two examples: Original findings produced by myself and Marjolein Righarts (Nel and Righarts, 2008) that rapid-onset disasters do pose a small but statistically significant risk for the onset of violent political conflict that causes the death of at least 25 people have been shown to be dependent on model specification. Slettebak (2012) demonstrates that if one includes a measure of population size, the sign of the natural disaster coefficients change and these drop from the significant list. It is not altogether clear that population size is more relevant than the interactive term that captures the impact of disasters (total number of disasters*population size) – which when used confirms our initial findings. Also, the work of Raleigh (2010) raises important issues about the use of national geographic attributes such as population and territory size as conflict predictors. Nevertheless, these disputes do highlight the specification-dependency of findings given the current state of the data at our disposal. Another path-breaking cross-national study (strangely overlooked in some reviews), making use of the Political Inequality Task Force data on large-scale instability also concludes that specification and case selection make a difference:

Of all types of natural disasters, only floods, storms, earthquakes, and the total sum of all natural disasters appeared statistically significant in one, but not the other two sets of instability and control cases. I also found that rapid-onset disasters posed a higher and more significant risk, but the disaster’s intensity seemed to make no difference for the chances of the onset of political instability. (Omelicheva, 2011: 462-463)

These studies, and others in the same mold, also show the context-dependence of the risk factors associated with natural disasters – a point to which I will return below.

Before I do so, note should be taken of two further general features of the extant studies that
contribute to the general conclusion that results to date, either way, are not sufficiently robust to have full confidence in. The first is that almost no work has been done on political consequences of natural disasters other than those that fall under the heading of large-scale political instability. There is no reason to restrict research only to those consequences that constitute instability. Kreutz (2012, Essay II) for instance, have used the UCDP Conflict Termination Dataset to consider how natural disasters affect incumbents’ expectations and behavior, and how these contribute (or do not contribute) to the termination of conflict. He suggests that:

(w)hen incumbent government leaders depend on the support of the citizenry to maintain power, they need to allocate resources for disaster relief. One way for a government to improve its disaster management effort is to divert the resources it currently has engaged in the civil war. Thus, governments have incentives to reconsider the use of armed force against insurgents [after a natural disaster] (p.27).

Secondly, many of the studies rely on the natural disaster data collected by the World Health Organization Collaborating Centre for the Epidemiology of Disasters (CRED) and made available in CREDs EM-DAT database. As this data is gathered on the basis of self-reporting by the member states of the WHO, questions can be asked about the veracity of the figures dealing with the number of people affected or total number killed. In addition, the data in this dataset have country-year as its unit of analysis, which makes it difficult to link the exact timing of disaster events with the timing of the outbreak of civil strife (if there is any), for instance. Country-year data also makes it almost impossible to study local and regional effects. What we need are spatially and temporally disaggregated data, which can link the incidence of natural disasters more directly with political events, both proximate and distant. A good start – at least on the dependent variable side – are the Armed Conflict Location and Events Data (ACLED) project (See Raleigh et al. 2010; and http://www.acleddata.com/about-acled/) and the Uppsala Conflict Data Program Georeferenced Event Dataset (http://www.ucdp.uu.se/ged/). Raleigh (2010) sets an excellent example of how the ACLED data can be used to settle some of the disputes that arose out of the inaccuracies of country-year based panel data.

EM-DAT data do not help us to get a grip on macro-climatic events either. The latter may be important: While the cross-sectional evidence suggests that there is little reason to expect that slower-onset random and localized climate change events will precipitate conflict, one recent study of the effects of planetary-scale climate changes (El Nino Southern Oscillation) challenges this conclusion (Hsiang et al. 2011) Using data from 1950 to 2004, this study shows that the risk of new
civil conflict in the tropics double during El Nino years, compared to La Nina years. Global climate trends do affect political stability, but this should not be taken as ‘proof’ that anthropogenic climate change contributes to more political instability, the authors are careful to point out.

**FRAMES**

There is thus still considerable work to be done to develop new data sources on both the dependent and independent variables. This is bound to be time and resource intensive. However, that does not mean that we have to wait for these new/refined data sources before any significant research progress can be made. In this and the next section I argue that on of the most important immediate steps is to sharpen our understanding of the contextual (‘it-all-depends-on’) factors that determine when and how specific political responses flow from natural disasters. This section makes the theoretical case, while the next takes a stab at systematizing these contextual factors.

It has become quite popular recently to frame the debate as one between two broad and presumably mutually exclusive approaches (see Slettebak, 2012; and Bernauer et al. 2102). The one perspective, it is argued, expects the worst. Natural disasters such as floods, earthquakes and volcanic eruptions are said to exacerbate existing grievances and/or provide incentives to grab scarce resources. They may also effect the distribution of collective action resources in a society. When the effects of natural disasters overburden the resources of the state, disgruntled or greedy groups may grab the opportunity, calculating that the potential gains outweigh the costs. This perspective is influenced by the work on environmental scarcity and its effect on conflict behavior. This school is variously called the “environmental-scarcity”, “pathological”, or “neo-Malthusian” approach as it assumes that natural disasters increases scarcity and that under conditions of scarcity, humans engage in pathological (conflict) behavior *ceteris paribus*. The drawn-out conflict in Darfur, West Sudan remains a paradigmatic case study for this approach.

This perspective is contrasted with one that expects that those who are affected by disasters have an incentive to maintain and nurture society. Natural disasters can inspire actors to set aside pre-existing grievances or resolve long-standing conflicts. By altering the distribution of collective action resources, natural disasters can get actors to re-evaluate their expected gains from competitive behavior, and to opt for cooperation. This contrasting approach is called “Cornucopian”, “beneficent”, and perhaps more appropriately “disaster sociology”. The latter
appellation harks back to the early finding of Emile Durkheim that large social disturbances, including war, decrease the likelihood of anti-social behavior. It also draws on the early sociological work of Charles E. Fritz (1959; 1961; and 1996) who traced the behavior of societies under war-time (both hot and cold war) disaster-like stress, and the economic work by Hirshleifer (1987). Proponents of the disaster sociology approach suggest that there is substantial evidence that disasters, both in the form of war (which is what Fritz looked at) and natural disasters encourage co-operative behavior. The signing of a peace agreement in the Indonesian province of Aceh, within a year after the devastation caused by the Boxing Day tsunami of 2004, is often cited as a supportive example. In addition, the cooperative behavior of both the New Zealand and Japanese populations in the wake of the two major earthquakes that struck these nations in early 2011 seems to give credence to this way of thinking (see also Solnit, 2010).

Now, it would be silly to claim that these two perspectives have played no role in the actual conception, construction, and conduct of research programmes. It could also be argued that they have some general heuristic value. However, what I want to dispute is the continued usefulness of farming the debate in terms that suggest that these perspectives are mutually exclusive, or that participants in the research debate necessarily belong to either of the two. Framing research contributions in such stark binary and oppositional categories is typical of pre-paradigmatic phase of research development, but that does not make it useful. A careful look at the large number of empirical contributions generated over the past ten to twenty years – both single case and large n studies - shows that there is a much larger degree of overlap in research focus and findings than is suggested by this oppositional categorization. This framing should also be dropped, I believe, because it ignores the fact that the behavioural assumptions that underlie both approaches share a family resemblance: They are both ‘macro-level theoretical frameworks’ (Urdal, 2006:609) that use general attributes of a society to explain how elite and/or mass driven behavior emerges. One important reason for this almost exclusive focus on the macro-level is the absence of micro-information on the ‘real’ motives of individuals when they are confronted with the choice between cooperation and conflict, and the opportunity and incentive calculations that determine their eventual choices. Hence, in terms of this shared logic, contextual factors bear the brunt of the explanatory weight. It comes as no surprise, therefore, that both perspectives are indeterminate in their predictions and that the empirical research time and again concludes that the likelihood that conflict (or cooperative behavior) will be triggered by a natural disaster, depends on the features of the society affected by the disaster as well as other contextual factors. If there is a difference
between the two perspectives, it is that those who are concerned about the conflict potential of natural disasters have so far been more diligent in testing for these contextual factors (see below) than have those who posit the likelihood of cooperative behavior (contrast Omilechya, 2011, and Slettebak, 2012, for instance). Given the important insights by Solnit (2010) on how pre-existing social capital makes a huge difference in contexts of emergencies and disasters, excluding measures of social capital is no longer on (and see Oliver-Smith 1996 for an important anthropological perspective). But I do not want to perpetuate the notion that these two perspectives are irreconcilable. They share the same logic and this very logic highlights that what matters most is to identify the contextual factors – intervening variables, if you wish – that determine the outcome one way or another.

**CONTEXT**

My reading of the burgeoning empirical literature is influenced by and influences the refusal to accept the oppositional frame discussed above as the most useful entry into the debate. As hinted above, it would seem that the political implications that natural disasters have are heavily dependent on contextual factors, as our consideration of the presumably opposing perspectives also suggests. In her study of the effect of natural disasters on political instability Omelicheva, for instance, concludes that

(w)hen those background conditions are controlled for, the effect of natural disasters lessens or disappears suggesting that natural disasters can trigger political instability in only those states, which already exhibit the attributes of the conflict-prone societies. (p.463)

While this is indeed a sensible conclusion to draw from the available evidence, the question still remains when and why exactly do these conflict-prone societies, given the incidence of a natural disaster(s), descend into conflict? Similarly, why is it that in some/many of these conflict-prone societies the political consequences do not fit the conflict pattern? I suggest that these questions can be answered only if we systematically review and test for the effect of a broader range of ‘background’ factors. I say ‘broader’ because work to date, including my own, have concentrated on the typical national political-economic variables that political scientists are interested in: level of socio-economic development; structurally-induced grievances and/or systematic patterns of discrimination; nature of the political regime; state capacity to insure citizens against natural
hazards and to deal with the aftermath of a disaster; and conflict contagion (both historical and regional). This category must also include indicators of underlying cohesion in a society, such as measures of social capital or generalized trust. I cannot resist observing that it is striking that the most vocal defenders of sociology of disaster fail to include available direct or instrumental measures of pre-existing social cohesion as a potential explanation why integrative behavior outbids disintegrative behavior in certain contexts.

I suggest that there are two additional categories of ‘background factors’ or intervening variables that call for investigation. For want of better terms I classify them as macro-systemic on the one hand and micro-behavioural on the other.

It is striking that none of the studies that I am aware of tests for the effect of changes that have taken place in the global system over time. This is surprising given the work of Goldstein (2011) who argues that violent conflict has become less of a norm, and the work of Kalyvas and Balcells (2010) who show how the demise of the Cold War has changed the availability and distribution of action resources for incumbents and challengers, and how this affect the technological dynamics of civil wars. Although the focus of Kalyvas and Balcells is restricted to civil war, the more general lesson that we can learn from their work is that change in global systemic factors can shape the options and behavior of local actors. It would be useful, hence to test for the effect of time – as an instrument for global normative changes. One option would be to include a simple binary temporal variable to distinguish between the period before and after the end of the Cold War, or to distinguish ‘before and after’ in terms of another important global hinge event. Many of the large n studies that I have been reviewing cover the last four or five decades of the previous century and the first of this century – a period of time in which global norms and the repertoire of political behavior have undergone significant changes. One must be careful, though, not to make the simplistic assumption that the end of the Cold War has contributed necessarily to a ‘more peaceful’ civil political scene, and that that explains why the increasing incidence of natural disasters in the recent two decades is not associated with a rising tide of political violence of the lethal kind. We might also want to consider how the global dispersal of new forms of social mobilization and expression of resistance via ubiquitous electronic social media contributes to changing political behavior and outcomes over time (Ben Gharbia, 2010; Kalathil, 2003; Beutz-Land, 2009).

This leads directly to the second category of intervening variables that I hope will receive some attention: Micro-behavioural variables that reflect/capture the localized processes that lead to
conflict or to integrative outcomes. Apart from general patterns of norm-guided behavior that would reflect normative changes on a global scale, there may be patterns of very specific local practices that contribute to the outcomes one way or another. By local practices I have in mind patterns of behavior by both the incumbents and potential challengers. State responses to natural disasters are an important factor, but it is under-investigated (however, see Olson and Gawronski, 2010; and Kreutz, 2012). More specifically, ‘state norms’ (if one can call them that) could be crucial. For instance, if it is the norm in a specific society that the incumbents use force to deal with resistance/challenges it is very likely that strains produced by natural hazards may result in violent conflict. The converse will hold as well.

Thanks to the work by McAdam, Tarrow, and Tilly (2001), Tarrow (1998), and Tilly (2003) we are much more aware of the importance of the variety of the ‘dynamics’ and ‘technologies’ of contention and how that shapes political outcomes. What do we know about the nature of collective political action in societies that are affected by natural disasters. So far, we have relied on national characteristics (such as nature and duration of regime) to generalize measures of political dynamics, but these measures obscure a wide-variety of means and practices of contention/mobilization. Who are affected by natural disasters and do they have existing channels of mobilization that they can rely on? Behavioural research in Anthropology, usefully reviewed by Oliver-Smith (1996) provide pointers to the importance of such process factors, while existing datasets such as the Minorities at Risk dataset may help to identify measures that can be used to capture at least some of them.

CONCLUSIONS

Albeit a late starter, Political Science has generated a range of useful theoretical models and empirical findings that can help us to determine the potential political effects of natural disasters. There is still much that Political Scientists do not understand, though. One challenge that remains for large n studies is to generate more sub-national data that is more precise in linking local disasters with local political results, both in space and time. Another is to explore further the links between regional, continental and planetary macro climate trends and cycles of conflicts. In addition, we have to understand better how global normative change and patterns of local norms and political processes determine how societies respond to natural disasters. Instead of categorizing existing research in mutually exclusive, oppositional schools of thought, it would be more useful if we could devote our energies to generate and evaluate the relative importance of empirical
measures of these factors. After all, we all agree that political outcomes are determined by contextual factors. Read this paper as a plea for an expansion of our palette in this regard.

However, this is not a plea that we should ignore the important goal of parsimony. As the results achieved by Goldstone et al. (2010) illustrate, parsimony is achieved only after a lengthy process of itemizing candidates for empirical testing, comparing their explanatory power, and discarding those that fail to significantly enhance our predictive/explanatory capability. Political Scientists, I believe, are at present only at the beginning of that process, and it is premature to demand and expect parsimony to be achieved. This applies especially as we broaden the range of political outcomes that we are interested in: We may have parsimonious models available of what determines the outbreak of civil war, but we are some way off explaining irregular disintegrative political behaviour, and how and why civil conflict ends. In short, we have to know more about what makes people band and/or stand together after a natural disaster. The good news, so far, is that it seems as if the nature of political outcomes after disasters depend on factors over which we humans have control. Blame for disastrous political outcomes should fall on our account and not on that of nature.

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