Climate Change, Natural Disasters and Civil Unrest:
A Quasi-experiment and Beyond*

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*The authors would like to acknowledge the thoughtful contributions of Scott Althaus, Jose Cheibub, Wendy Cho, Jude Hays, James Kuklinski and Milan Svolik. Joseph Bajjalieh did yeoman's work on data preparation. Ajay Singh and Gabriel Rodriguez did a superb job training and managing the student operators of our software, while Matthew Hayes provided overall supervision. We especially want to thank the nine students who enrolled in the 2009-10 Cline Center Research Practicum and collected the event data upon which much of this analysis is based: Kesha Butler, James Keller, Max Knierim, Juha Lee, Liz Pfafflin, Alex Sapone, Jacob Shinder, Michael Slana and Mark Svalina.

This paper was prepared for "Climate Change and Security," which is the 250th Anniversary Conference organized for The Royal Norwegian Society of Sciences and Letters, held in Trondheim, Norway, June 21-24, 2010.

Beginning in the latter third of the 20th century, various scholars have argued that adverse human effects on the environment pose an important threat to human security. With increasing scientific evidence on climate change, and growing public awareness of the problem, concerns about its security implications reached a new level after the turn of the 21st century. Two recent Nobel Peace Prizes (2004, 2007) have been awarded for work related to environmental change and peace. A number of government sponsored reports about the effects of climate change on resource scarcity, the main link between climate change and conflict, have painted a troubling picture of the future (IPCC, 2007; Stern Review, 2006). Various observers have attributed armed conflicts in Rwanda, Kenya, Assam, Chiapas and Sudan to the effects of climate change. Moreover, several reputable groups have conducted thoughtful and in-depth reports assessing the potential security threats of climate change (WPGU, 2007 CNA, 2007, Campbell, et. al., 2007).

Despite the increasing attention given to the security dimension of climate change, the empirical basis for this concern is highly tenuous, as several observers have noted. Barnett (2003), for example, concludes

> It is necessary to be cautious about the links between climate change and conflict. Much of the analogous literature on environmental conflicts is more theoretical than empirically driven, and motivated by Northern theoretical and strategic interests than informed by solid empirical research (Barnett, 2000; Gleditsch, 1998) … On the basis of existing environment-conflict research there is simply insufficient evidence and too much uncertainty to make anything other than highly speculative claims about the effect of climate change on violent conflict, a point that both policy makers and climate scientists should not lose sight of (Barnett, 2003: 10).

Theisen (2008) reviews the leading "large n" statistical studies of the link between climate change and security that were published between 1998 and 2007. He concludes that only one quantitative study (Hauge and Ellingsen, 1998) "found substantial support for eco-scarcity theory (Theisen, 2008: 801)." However, Theisen is unable to replicate the results of that study. He concludes that the results of his replication "lend little support to a purported link between resource scarcity and civil conflict, whereas it replicates earlier findings on the importance of poverty, instability and dependence on fuel export (Theisen, 2008: 801)." Compounding the implications of Theisen's inability to replicate Hauge and Ellingsen's results, Buhaug, Gleditsch and Theisen (2008: 5) note an important paradox that raises profound questions about the hypothesized link between climate change and security: While the physical and human processes affected by climate change have only begun to emerge over the past fifteen years, that time frame has been characterized by a significant reduction in the overall incidence and severity of armed conflict. In the final analysis, Buhaug, Gleditsch and Theisen concur with Barnett's earlier assessment. They note that while several case studies have concluded that resource scarcities associated with climate change contribute to violent conflict, statistical analyses have "failed to converge on any significant and robust association between resource scarcity and civil war (Buhaug, Gleditsch and Theisen, 2008: 2)."

Since these reviews were conducted a handful of more ambitious and sophisticated analyses of the relationship between climate change and conflict have appeared. However, they simply provide poignant examples that support the conclusions of Buhaug, et. al. (2008). Zhang, et. al. (2006) examines the relationship between climate change and conflict for the last millennium in China. Tol and Wagner (2009) replicated Zhang, et. al.'s analysis for Europe. Both employ far more sophisticated analyses than prior researchers. However, they find that increases in violent conflict are related to colder temperatures rather than warmer temperatures. Tol and Wagner also find that the relationship varies over time. Confounding these confounding results is an equally sophisticated analysis by Burke, et. al. (2009). They examine changes in temperatures in Africa for the period from 1981 and 2002 and assessed their impact on battlefield deaths. They find that conflict is positively related to temperature and predict an additional 390,000 battlefield deaths by 2030.

The failure of existing research to generate a clear, empirically based consensus in this area is hardly surprising. This field of inquiry is in its infancy and its central concerns are challenging to address in an empirically rigorous manner. The causal chains between climate change and conflict are remarkably long and complex. Moreover, they may vary over time and across different national contexts and regions of the world – as the contradictory findings from China, Europe and Africa suggest. Also, the analyses to date have employed very coarse measures of conflict, which do not capture many plausible effects of climate change on human security. Recognizing the infancy of this field, and the profoundly important nature of the questions with which it is concerned, Buhaug, Gleditsch and Theisen (2008: 37) argue that the greatest challenges lie with "generalizable, statistical research."

This paper reports the results of a pilot study that was designed to address this challenge. It differs from previous research in a number of ways that comport with Buhaug, Gleditsch and Theisen's (2008) assessment of deficiencies in existing research. First, at the foundation of this effort is a randomized, quasi-experimental design. That is, we randomly selected a set of natural disasters associated with climate change (floods, storms) in order to examine their impact on civil unrest. The study was designed to provide for comparisons between the levels of civil unrest in a pre and post period (eighteen months each) using two independent sources of data on civil unrest. Second, our measures of civil unrest – the Cline Center's Intensity of Political Protest and Intensity of Political Violence gauges (discussed below) – are composite measures of "small-bore" indicators of instability. The pertain to a range of destabilizing events by non-state actors (demonstrations, riots, symbolic acts, suicide attacks, assassinations, political kidnappings, executions, etc.) and incorporate event characteristics (# of participants, # killed, # injured, type of attack, lethality of weapon, etc.) to differentiate among events. Third, we use a set of context and institutional measures to model the differences in the impact of these disasters on the intensity of civil unrest.

There are several potential benefits to the research design employed here. First, in a field of study in which the causal linkages are both long and complex, quasi-experimental designs have the capacity to determine whether: (1) the expressed concerns about climate change and security are warranted, and (2) empirical efforts to unravel complex causal chains justify the investment. While quasi-experimental designs cannot delineate causal linkages, a series of well-designed quasi-experiments has the potential to provide empirically based insights into whether continued investigation is justified. Second, because we focus on abrupt developments related to climate change (floods and storms), we are more likely to detect destabilizing effects than if we studied incremental developments (desertification, famine, drought, sea-level rise, etc.). These incremental developments provide national and international actors with the opportunity to respond preemptively. Thus, Buhaug, Gleditsch and Theisen expect "sudden or unexpected climate-induced events, such as flash floods, tropical storms, and droughts to constitute a larger threat to human security than gradual reductions in resource availability (2008: 6)." Third, our focus on "small-bore" indicators of civil unrest enhances the likelihood of uncovering whatever effects climate-related developments have on instability. Most prior empirical research in this area has focused on interstate and civil wars. In many ways, the types of civil unrest indicators examined here are precursors to militarized conflict, as well as other "large-bore" indicators such as refugee movements and political instability. If developments related to climate change do not affect small-bore indicators such as those studied here, it is unlikely that climate change is causally related to large-bore events that impinge upon human security in more profound ways.

Two observations underscore the utility of the instability indicators employed in this analysis. First, the most fine-grained indicators of security previously used to examine the eco-scarcity thesis appear to be PRIO data on battlefield deaths. In order to be included in PRIO's database, however, an event must involve: (1) an encounter must be between government forces and an insurgent group, and (2) at least 25 battlefield deaths. Events meeting these criteria constitute only .75% of the non-U.S. events included in the measures of civil unrest employed here.[1] Second, while Buhaug, Gleditsch and Theisen (2008: 5) note that war-related deaths decline about the time that the destabilizing effects of climate change should have been increasing, the opposite is true for civil unrest (see Figure 1). Figure 1 displays the distribution of the Intensity of Political Protest and Intensity of Political Violence gauges (1946-2005); it is based on a sample of 25,000 destabilizing events. [2] As is clear, while the incidence of both violent and non-violent political acts decline in the late 1980s, both increase noticeably in the mid to late 1990s. Indeed, the Intensity of Political Violence index approaches it post-war high in 2005.

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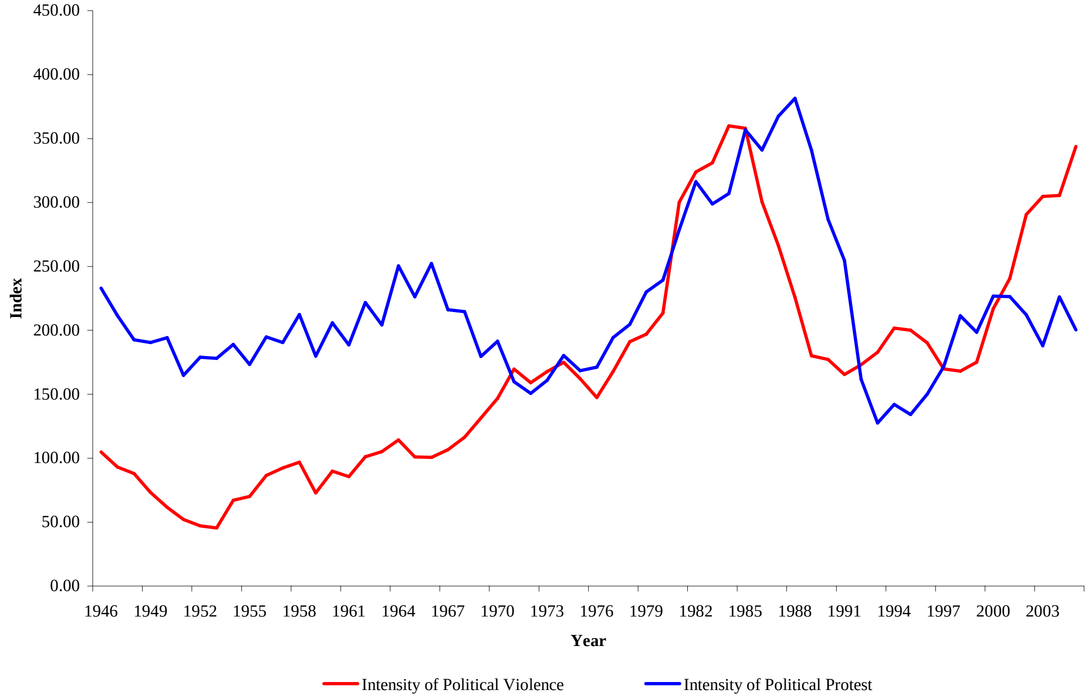
[1] This statistic comes from a random sample of instability events derived from New York Times articles from the post WWII era, not the set of focused case studies used in this analysis. If the PRIO criteria are applied to the data used here, the comparable statistic is .63%.

[2] The data displayed in Figure 1 are seven-year moving averages of the sum of each index for all events that occurred within a given year. Because hundreds of thousands of destabilizing events remain to be coded, the data in Figure 1 are preliminary. However, because the figure is based on a random sample of events the general contours are unlikely to change dramatically with the addition of more events.

Figure 1
Intensity of Political Protest and Political Violence, by Year



The next section briefly introduces the project that generated the data on civil unrest events for this research: the Social, Political and Economic Event Database project (SPEED); SPEED is a component of a more encompassing project, the Societal Infrastructures and Development project (SID). Next, the research design is described, including the derivation of the political protest and violence indicators. The third section reports the results of the quasi-experiment. The fourth section discusses some factors that contaminated the analysis (i.e., the existence of other natural disasters in the pre and post periods) and offers a restructuring and reanalysis of the data. The fifth section describes an analysis of the factors that affect differences in the impact of natural disasters on political violence across the episodes studied here.

The Research Base

The SID Project

SID is a long-term and on-going program of research designed to provide an empirically well-grounded knowledge base for institutions-oriented development strategies; it is the signature initiative of the Cline Center for Democracy. SID includes 175 nations (all nations over 500,000 in population as of 2004); its data archives begin in the post WWII era (1946) and will be extended to the present. SID's substantive foci are on national institutions (political, legal and economic) as well as the contexts within with those institutions operate (wealth, educational attainment, structure and depth of social cleavages, natural resource endowments, etc.). More than a score of faculty and over 300 students have contributed to SID's development since 2005 and the project is currently at an advanced stage of development. Some of the data in the SID archives are the result of original research; other data has been derived from existing sources.

Various components of the SID archive are relevant for studying the destabilizing effects of climate change. Data on changes in GDP, agricultural production and natural resource production are useful in gauging the impact of natural disasters on societal processes that directly affect human welfare; they speak directly to the eco-scarcity thesis. Data on per capita GDP, educational attainment and social equality can be used to capture differences in the capacity of societies to adapt to climate-related disasters. Finally, difference in the design of political and legal regimes are thought to be relevant to civil unrest, as well as how it is handled. Legal and political institutions are societal mechanisms that affect how issues, such as those related to developments rooted in climate change, are addressed. They also affect how disputes stemming from those issues are resolved. Institutional designs both reflect and shape cultural values and norms across societies. Differences in these designs can have a marked effect on the reactions of civil society to adverse developments.

The SPEED Project: An Overview

SPEED is a technology-intensive effort to collect a comprehensive body of global event data for the Post WWII era; it was designed to provide insights into key behavioral patterns and relationships that are valid across countries and over time. SPEED data will produce insights that complement those generated by other component of the SID project (constitutional data, archival data, survey-based data, etc.) because it generates "bottom-up" data from news reports. In generating these event data SPEED leverages tens of billions of dollars that have been invested in compiling news reports from throughout the world since 1946. SPEED data are derived from a global archive of approximately 40 million digitized news reports that has been assembled from tens of thousands of news outlets from virtually every country. It begins on January 1, 1946 and is updated daily. An automatic text categorization program (**BIN**) was developed to identify news reports containing information on civil unrest.[3] To extract information from "binned" news reports, human operators used **EXTRACT** – a suite of electronic modules developed by SPEED programmers to facilitate the coding process.[4] The "front end" of **EXTRACT** is

[3] **BIN** uses statistically derived algorithms based on key words, word correlations, and semantic structures to generate a probability that a news report contains information on civil unrest events. **BIN**'s algorithms were developed by using training data derived from thousands of human-categorized reports. These training data provided insights on the semantic attributes that characterize reports belonging to a specific category; it has proven to be very robust. Thresholds for inclusion in a bin were set relatively low, so as not to discard news reports with information on relevant events. Consequently, repeated tests examining random samples of discarded news reports (i.e., those not deemed relevant to contain information on civil unrest events), document that **BIN** has a false negative rate of 1%.

[4] For example, **EXTRACT** contains a calendaring module facilitates the ascertainment of the date upon which an event occurred. The **GEOCODER** module uses natural language processing (NLP) techniques in conjunction with two large geospatial databases containing 8M place names (GIS, GNIS) to identify the event's location. In addition, **EXTRACT** employs chaining technologies to link related events that are contained in different news reports (antecedent events, post-hoc reactions, etc.). NLP techniques are also used with lexicons of social group names (religious, ethnic, racial, tribal, nationality, insurgent, etc.) to

composed of a category-specific protocol and a web-based interface that integrates the digitized reports and the protocol. SPEED protocols are carefully pretested and operators are extensively trained and tested before they gain access to "production queues."

This study is based on SPEED's historical news archive, which is composed of the digitized historical archives of the New York Times and Wall Street Journal for the 1946-2006 period, as well as news reports from the Foreign Broadcast Information Service (CIA) and the Summary of World Broadcasts (BBC). These contain millions of news articles and broadcasts that were translated into English from scores of languages. However, the FBIS and SWB archives were not digitized. Digitizing, segmenting and cleaning these archives have required a multi-year investment and all of the archives are not currently "on-line." Thus, the event data used here was generated by utilizing the Societal Stability Protocol in conjunction with the New York Times (NYT) and reports from the post-1980 SWB archive.

SPEED's Societal Stability Protocol

The event ontology underlying the Societal Stability Protocol was developed during a year-long pretest involving the analysis of thousands of news reports. There are six tier-one categories that structure the ontology: political expression events, politically motivated attacks, destabilizing state acts, political reconfiguration events, mass movements of people and cataclysmic events. These basic categories capture a wide range of destabilizing activity. Moreover, each of these has at least one tier of categories below the first tier and some have as many as three additional tiers. For example, political expression events include everything from verbal and written expressions to symbolic expressions, demonstrations, strikes and riots; the latter include a host of different acts that vary in their potency. Politically motivated attacks include extraordinary attacks (assassinations, suicide attacks, kidnappings, executions, etc.), garden variety attacks on people and property, and organized mass attacks – as well as unexecuted attacks (conspiracies and attempted attacks). Destabilizing state acts include extraordinary actions (censorship, states of emergencies, curfews, disruptions of communication channels, etc.), armed attacks, coercive actions, and a number of ordinary state actions performed with malice (punitive dismissals, facility closures, service suspensions, etc.).

The rationale for using such an extensive and refined ontology to identify and extract information on events is that it provides the means to generate insights into the dynamics of instability. Large-bore developments such as insurgencies, civil wars and political coups are not the only type of disruptive societal behavior. Moreover, as a number of scholars have long argued (Gurr, 1970, Schwartz, 1970, Hopper, Singer, 1972), they are the endpoints of an extended and often convoluted process, one replete with critical junctures, missed opportunities, and strategic moves. Most situations that have the potential to evolve into extended violent episodes do not; others that could have been

capture the identity of event participants (initiators, targets, victims, etc.) and external facilitators/ collaborators (other nations, NGOs, etc.). Finally, **EXTRACT** provides for on-going quality control: it can feed a set of pre-coded "test" articles to all operators and generate reports on the accuracy and reliability of operators by question set.

short-circuited were not. Capturing events such as provocative speeches, demonstrations, symbolic actions, violent attacks, and state exercises of coercive force makes it possible to identify escalatory patterns that can yield insights into the dynamics of conflict across contexts. This makes it possible to both gauge the impact of climate change on conflict as well as provide insights into how violent conflict can be anticipated and avoided.

The extensive amount of event-specific information collected by SPEED's Societal Stability Protocol is also useful in enhancing our understanding of how episodes of destabilizing events evolve, and perhaps escalate, over time. The protocol includes over 350 queries, though most are relevant only to specific event subtypes and are highly branched. Indeed, 97% of the protocols questions are response-activated by over 600 branching commands embedded within **EXTRACT**. These queries are designed to provide event-specific information on who, what, how, where, when and why:

- **Who**
  - Initiators; Targets/Victims
  - International involvement
- **What**
  - Event type
  - Impacts (people, property, society)
  - Consequences (for initiators)
  - Reactions (to event)
- **How**
  - Weapon, mode of expression, type of natural force
- **Where**
  - Geo-spatial location, geo-physical setting
- **When**
  - Date
- **Why**
  - Societal context
  - Attributed origins

For example, with respect to "who" is involved in the event, the protocol captures information on initiators, targets and victims. An extensive pretest led to the development of list sets that captures thirty-seven types of non-governmental actors (social groups, workers, civic leaders, clergy, etc.) and twenty-three types of government actors (public safety officers, soldiers, bureaucrats, presidents, dictators, generals, etc.). Lexicon-based modules provide a uniform method of capturing the identity of social, political and insurgent groups that are involved in an event. Information is also recorded on the number of individuals involved. Other parts of the protocol pertain to the involvement of foreign countries or international organizations. If either type of entity is involved, a lexicon-based module captures it name. With respect to "what" the event entailed, the protocol provides for information on both the multi-tier event type and its scope/intensity. A set of scope/intensity question sets capture information on the number of initiators and victims as well as the event's effects (e.g., impact on individuals/communities/society, property damage, etc.). Another dimension to "what" the event entailed deals with post-event developments. The protocol has question sets to capture the direct consequences for initiators, as well as the post-hoc reactions (condemnations, boycotts, retaliatory attacks, strikes, protests, etc.) of entities not involved in the event (governments, civic groups, international organizations, etc.). Finally, **EXTRACT**'s **LINK** module creates electronic links between a focal event and related events (attacks that led to a protest demonstration, repressive government act that precipitated a violent attack, an attack by Sunnis on Shiites that led to an attack on a Sunni marketplace, etc.).

Within the "how" category, the protocol captures information on weapons used (if any), modes of expression, and types of natural forces. Geo-spatial information (latitude and longitude) is provided on "where" the event occurred; a list set containing types of geophysical locations (market, residence, recreational area, house of worship, airspace, etc.) provides additional information on where the event occurred that can be useful in providing important insights. Date information is collected on when the event occurred as well as how long it lasted (where relevant). Finally, with respect to "why" an event occurred, an extensive amount of information is collected on the societal context of the event (on-going turmoil, penumbra or anniversary of a symbolically important happening, war time, etc.) as well as its attributed origins (dissatisfaction with government, ethnic animosities, ideological concerns, basic human needs, etc.).

Gauging Civil Unrest with SPEED Data:
The Intensity of Political Protest and Violence Indexes

The Societal Stability Protocol required the type of encompassing, nested structure outlined above because the pretest revealed that civil discontent can be manifested in a variety of ways: words, non-violent actions, symbolic gestures, violent actions, etc. These modes of expression require different levels of initiator commitment, organization and resources and, consequently, they send different signals to both other citizens and the state. A lonely citizen advocating civil disobedience in a public square requires minimal effort; a mass demonstration requires a great deal. An act of violence employing crude weapons signals a significant level of threat – but not as much as a symbolically timed violent attack by an organized group employing sophisticated weapons. Compounding these distinctions is the fact that marked differences exist within similar modes of expression. A sit-in sends a different signal than a self-immolation; a march of 100,000 reflects more discontent than a march of 5,000. Some violent acts involve small arms and discrete targets; others involve powerful explosives and kill scores of people.

If differences in civil unrest across time and space are to be gauged accurately, differences in how discontent is manifested must be captured – and this requires going beyond simple "event counts." Using only event counts risks missing differences in the intensity of civil unrest in the period after a natural disaster. While the number of attacks may remain the same, the lethality of the weapons used and the number of individuals killed could increase (or decrease) dramatically after the disaster. Capturing these differences, however, can be clumsy and tedious, for two reasons. First, a large number of factors can affect intensity (number of people involved in an event, the existence of a weapon, the lethality of the weapon, the number of people killed/injured, etc.). Second, intensity indicators vary with the type of instability event: one set of factors is relevant for marches and demonstrations; another is relevant for violent attacks. The plethora of potential intensity factors can impede our efforts to examine the impact of natural disasters on civil unrest by obfuscating patterns in the data. Thus, to capture differences in intensity succinctly, we used factor analysis to construct composite measures of intensity for two broad categories of events: political protests and politically motivated attacks. These categories of behavior are simply too different to derive a single measure of civil unrest. Moreover, as political protests can be precursors to violent attacks, having two distinct measures that are spatially and temporally referenced can be useful.

Preliminary factor analyses revealed that four intensity indicators were most relevant for each category. For political protests the key indicators are: mode of expression (speech, symbolic act, forming an organization, mass demonstration/strike and riot), number of participants, whether a weapon was involved, and whether anyone was injured. For political attacks the key indicators are: the type of attack (e.g., whether it was against persons or property), the lethality of the weapons employed (none, crude weapons, small arms, explosives, military grade weapons), the number injured, and the number killed. The factor analyses employing these variables produced the composite measures displayed in Figure 1: the Intensity of Political Protest and the Intensity of Political Violence. Table 1 reports the results of the factor analysis.[5] The most central intensity indicators for political protests are the mode of expression and the number of participants (factor loadings of .92 and .81, respectively), with the weapon and personal injury dummy variables having loadings about half as strong (.50 and .51, respectively). For politically motivated attacks the type of attack, the number injured, and the number killed are the most central (factor loadings of .69, .59 and .67, respectively); the lethality of the weapon employed is somewhat less central (factor loading of .4).

The Randomized, Quasi-experimental Design: An Overview

To gauge the impact of natural disasters on our measures of civil unrest we devised a quasi-experimental design because it was thought to maximize our ability to capture the disaster's destabilizing effects. Our design is structured to compare the intensity of civil unrest in the "pre-period" with that in the "post-period" for the country affected by the disaster;[6] each period is eighteen-months long. Eighteen months is a fairly long time frame for a study of this nature. But, in the absence of prior knowledge about the temporal reach of a natural disaster's destabilizing effects, it was a conservative choice. It allows us to capture the existence of cascading effects that could lead to medium-term

[5] The factor analyses used to derive the weights used in constructing the Intensity of Political Protests and the Intensity of Political Violence indexes were conducted on a random sample of instability events during the post WW II era: the sample used to construct the trend lines in Figure 1. Deriving these weights from this more general random sample provides for well-grounded composite measures that do not vary across samples and, hence, enhance comparability.

[6] We focus on instability events that fell within the country of the disaster because the country-level was the most refined screen available for identifying relevant news reports (i.e., we did not have the capacity to select on the basis of the province most directly affected by the disaster).

developments such as political instability and the re-invigoration of insurgencies. If had we adopted a shorter time frame we would risk missing some of the "tails" of the disaster's destabilizing effects and we would be less confident that we captured the totality of their effects. Thus, adopting an extended time frame provides us with the opportunity to generate insights into "optimal" periods that can inform future efforts. This notwithstanding, adopting such long pre and post periods is not without its own risk. Longer time frames provide for the intervention of extraneous factors that can undermine efforts to isolate the effects of the natural disaster. However, there is little reason to think that such interventions would not be randomly distributed. Moreover, while it is possible to develop post-hoc reductions in the pre and post periods, they cannot be easily extended given what is involved in assembling episode-specific queues of relevant news reports.

Table 1
The Factor Analysis for the Intensity Measures

	Factor Loadings for Political Protest
$\text{EXPRESSION}_{\text{MODE}}$	0.92
# PARTICIPANTS	0.81
$\text{WEAPON}_{\text{USED}}$	0.50
$\text{INJURY}_{\text{PERSONAL}}$	0.51
Eigenvalue	2.00
N	4625
	Factor Loadings for Political Violence
PERSONAL ATTACK	0.69
$\text{WEAPON}_{\text{LETHALITY}}$	0.40
# $\text{INJURED}_{\text{LOG}}$	0.59
# $\text{KILLED}_{\text{LOG}}$	0.67
Eigenvalue	1.44
N	3708

To identify the natural disasters to be studied we selected a random sample from a rich archive compiled and maintained by the Centre for Research on the Epidemiology of Disasters (CRED).[7] We initially chose a sample of 100 natural disasters associated with

[7] More information on the CRED archive can be found on its website: http://www.preventionweb.net/english/professional/contacts/v.php?id=712

climate change: droughts, heat waves, floods and storms. Two criteria were used as screens in selecting these disasters; they had to: (1) occur in the post WW II era; and (2) have killed at least 100 people or affected at least 10,000 people. We chose the post-WW II time frame because it corresponded with the time frame of the SID project. We imposed a minimum severity threshold on the selection process because minor weather events were unlikely to have a significant effect on civil unrest. While the criteria used were arbitrary, they were selected based upon a review of CRED's data on the number of people killed/ affected for all post-1945 disasters associated with climate change. Based on this review it was thought that these thresholds would provide us with a sample of relatively major disasters.

After selecting this random sample of disasters, two considerations led us to focus on a subset of them; we ultimately examined forty-five episodes involving floods and storms that occurred after 1980. We focused on storms and floods because, when attempting to identify news reports concerning instability events that fell within the pre-post period, it became clear that many heat waves and droughts had no precise starting point. Their vague temporal boundaries made them incompatible with the type of intervention analysis that is at the heart of this effort. This decision led to the elimination of eighteen of the randomly selected disasters. One other episode was eliminated because of an error in the CRED archive (the start-date listed was later than the end-date of the disaster). We eliminated the pre-1980 cases because, after we began extracting information from episode-specific New York Times articles, we developed the capacity to integrate the post-1980 archive of the Summary of World Broadcast reports (SWB) into our protocol system. Because SWB is constructed from local news sources and is thought to provide more detailed coverage than the New York Times (see Table 3), this provided us with the opportunity to replicate the results based on the New York Times articles. But limiting the time frame to post-1980 disasters led to the elimination of twenty-one episodes.[8] Because both archives ended in 2005, and we had an eighteen month post-period, we had to eliminate all episodes that began after July 1, 2004. This eliminated fifteen episodes.

The randomly selected episodes that survived all screens are listed in Table 2, along with additional details on the disasters. As can be seen in Table 2, the forty-five episodes (28 floods and 17 storms) that survived all screens differ somewhat in their magnitude. For example, while the average number killed was 213 (3 episodes had no information on deaths), the range was from 1 to over 1,000 with 2 episodes involving more than 1,000 deaths. The average number of people "affected" by the disasters (e.g. left homeless, without food, without medical attention, etc.) was over 2,000,000; only one episode was missing information. However, as the range was from 500 to over 33,000,000; (three episodes affected more than 15,000,000 and one affected 7,000,000), the mean is skewed by a handful of outliers. Excluding these four cases brings the average number affected to about 500,000. It is also important to note that the disasters occurred in twenty-three nations. Several countries were represented in the sample several times, reflecting differences in their susceptibility to climate-related disasters. To illustrate the saliency of

[8] Because our research design involved the use of a pre-test period that extended 18 months before the event this meant that we could only include events that began on or before July 1, 1981.

these cross-country differences consider that India had eight episodes, the Philippines had seven, China had six and Sri Lanka had three. Four other countries were represented twice (Bangladesh, Madagascar, Mexico and Zambia). The vulnerability to climate-related disasters illustrated here introduces significant problems in evaluating their impact on civil unrest, as will be seen later.

Table 2

Profile of Floods and Storms Examined

Random Order #	CRED Disaster ID Code	NYT Queue Number	SWB Queue Number	Type of Natural Disaster	Country of Disaster	Location of Disaster	Number Killed	Number Affected	Disaster Start Date	Disaster End Date
1	1993-0085	38	94	Storm	India	Tamil Nadu, Karaikal regi ...	61	72000	12/4/1993	12/4/1993
2	2004-0159	39	95	Flood	Zambia	Senanga, Mongu, Kalabo, L ...	2	196398	2/1/2004	6/2/2004
3	1982-0038	40	96	Storm	Philippines	Central province	151	789031	03/23/82	03/24/82
6	2004-0158	41	97	Flood	Djibouti	Djibouti city	51	100000	4/12/2004	4/16/2004
8	1983-0464	42	98	Flood	Ecuador	N.A.	N.A.	200000	8/4/1983	8/4/1983
11	2002-0788	43	99	Flood	Sri Lanka	Batticaloa, Polonnaruwa, ...	2	500000	12/16/2002	12/20/2002
13	1983-0102	45	101	Flood	India	Central - Western	130	N.A.	8/1/1983	8/1/1983
14	1990-0592	49	105	Storm	China P Rep	Zheijian province	68	200000	8/31/1990	8/31/1990
16	2003-0218	47	103	Storm	Madagascar	Vatomandry, Andevoranto d ...	89	162086	5/8/2003	5/8/2003
18	1992-0483	48	104	Flood	India	Tamil Nadu, Kerala, Karna ...	179	500	11/10/1992	11/10/1992
19	1997-0342	46	102	Flood	China P Rep	Jinping county, Qujing pr ...	300	318000	7/10/1997	7/30/1997
21	1986-0003	50	106	Flood	Bolivia	Santa Cruz, La Pazn Cocha ...	29	310000	1/6/1986	1/6/1986
23	1986-0081	51	107	Storm	China P Rep	Guangdon province	172	4001250	7/11/1986	7/11/1986
24	1998-0203	52	108	Flood	Bangladesh	Mymensingh, Jamalpur, She ...	1050	15000050	7/5/1998	9/22/1998
25	1983-0154	53	109	Flood	Indonesia	Java, Yogyakarta	7	410497	12/1/1983	12/1/1983
27	1994-0168	54	110	Flood	Philippines	Pampanga, Tarlac, Zambale ...	50	774000	6/22/1994	6/22/1994
28	1995-0101	55	111	Flood	Bangladesh	Chittagong, Bhola, Coxbaz ...	50	461325	5/15/1995	5/15/1995
31	2003-0443	56	112	Storm	China P Rep	Shenzhen, Guangdong provi ...	38	69925	9/1/2003	9/3/2003
36	2000-0788	58	114	Storm	Philippines	Mindanao Isl. and Central ...	48	164093	11/30/2000	12/3/2000
38	1986-0042	59	115	Storm	Madagascar	Toamasina, Fenerive east, ...	99	84309	3/15/1986	3/15/1986
39	2000-0715	60	116	Storm	Philippines	Rizal, Laguna, Cagayan, N ...	94	1747872	11/3/2000	11/5/2000
40	1982-0087	61	117	Flood	India	Northeast	932	33500000	8/1/1982	8/1/1982
41	1987-0126	62	118	Flood	Chile	Santiago, central Chile & ...	73	116364	7/17/1987	7/17/1987
47	1997-0203	65	121	Storm	Philippines	Manilla, Luzon	30	309111	5/24/1997	5/26/1997
49	1986-0085	66	122	Flood	India	Andhra Pradesh	187	245000	8/1/1986	8/1/1986
52	1994-0519	68	124	Storm	Philippines	Arora, Quezon, Laguna, Ba ...	32	258751	10/18/1994	10/22/1994
55	1984-0050	69	125	Flood	Brazil	Santa Catarina	10	120400	6/1/1984	6/1/1984
60	2000-0741	70	126	Flood	Sri Lanka	Ampara, Batticaloa, Polon ...	3	300000	11/18/2000	11/22/2000
61	2001-0515	71	127	Flood	India	Gopalganj, East Champaran ...	158	7000000	8/20/2001	8/20/2001
64	1988-0430	72	128	Storm	Mexico	Yucatan and Gulf coast	240	100000	9/14/1988	9/14/1988
69	1992-0472	75	131	Flood	Sri Lanka	N.A.	1	405950	12/1/1992	12/1/1992
73	1996-0106	77	133	Storm	India	Andhra Pradesh, Karnataka ...	731	1842100	6/14/1996	6/16/1996
75	1989-0053	78	134	Flood	Tanzania Uni Rep	Mahurunga, Kilambo, Kihim ...	10	141056	4/7/1989	4/7/1989
76	1989-0041	79	135	Flood	Iran Islam Rep	Zabol (Balouchestan)	N.A.	150000	4/4/1989	4/4/1989
78	1995-0177	80	136	Flood	Morocco	Marrakesh (Ourika region) ...	730	35000	8/17/1995	8/18/1995
80	2000-0484	81	137	Flood	Bhutan	Pasakha, Phuentsholing, C ...	200	1000	8/2/2000	8/30/2000
83	1994-0352	82	138	Storm	India	Tamil Nadu, Andhra Prades ...	208	428500	10/31/1994	11/4/1994
86	1992-0233	83	139	Flood	Tajikistan	N.A.	1346	63500	5/25/1992	5/25/1992
88	1997-0180	85	141	Storm	Philippines	All country	18	53659	8/18/1997	8/19/1997
89	1994-0064	86	142	Flood	Cambodia	Battambang, Takeo, Kompon ...	506	29000	7/31/1994	7/31/1994
91	1989-0028	88	144	Flood	Zambia	Chawama, Chilanga, Kanyam ...	N.A.	800000	1/23/1989	1/23/1989
92	2004-0284	89	145	Flood	China P Rep	Huaihua, Yiyang, Xiangxi ...	47	168500	6/20/2004	6/25/2004
94	1999-0428	90	146	Flood	Viet Nam	Da Nang, Quang Nam, Thanh ...	622	3504412	10/25/1999	11/9/1999
96	1996-0202	91	147	Storm	China P Rep	Zhanjiang, Maoming, Yangj ...	197	15005000	9/8/1996	9/12/1996
98	2002-0609	92	148	Storm	Mexico	Yucatan Peninsula	13	500030	9/20/2002	9/20/2002

A queue within SPEED's protocol system was created for each of the disaster episodes depicted in Table 2 (see column 3 and 4). Each queue was "populated" with news reports that met three criteria. First, SPEED's **BIN** module had to identify the news report as having a high probability of containing information on one or more destabilizing events, as defined with SPEED. Second, SPEED's **GEOCODER** module had to designate that the article had a high probability of containing information pertinent to the country within which the disaster occurred. Third, header information from the Cline Center's global news archive had to be such that the publication date of the report fell within the eighteen month period surrounding the natural disaster. News reports that met these criteria were fed to operators who then used SPEED's **EXTRACT** program to complete the Societal Stability Protocol.[9] The event data were then downloaded and entered into SAS files where they were integrated with other data from the SID project.

Table 3 provides details on the news reports that met the abovementioned criteria and were assigned to episode-specific queues within SPEED. With respect to the New York Times archive, almost 3600 articles (3582) survived the screens. There was a great deal of variance across queues: from 2 to 330 (mean= 80; median=41). Article counts do not translate directly into destabilizing event counts, for a variety of reasons. For example, some of these events may have occurred outside the pre-post period; others may pertain to happenings in a different country. Also, simply because the **BIN** module suggests that an article has a high probability of containing information on destabilizing events does not mean it actually contains such information. However, SPEED operators do a full text scan of all articles. Thus, while some articles may contain no information on destabilizing events, it is quite common to capture information on 3-4 events; the average number of events per relevant article is about 2. Moreover, we focus on only two types of destabilizing events: political protests and political attacks initiated by non-state actors. Finally, news providers often provide what we term "recapitulations." Recapitulation passages provide summary information on prior happenings that speak to a current event or locale; they can provide invaluable information on past destabilizing events.[10]

Table 3 reports data on the number of "relevant codings" upon which our analyses are based, by queue.[11] It reveals a great deal of variance across the randomly selected episodes. While there is an average of 15 codings across all New York Times queues, the median is 4. Indeed, 12 queues had no information on destabilizing events and another 9 had less than 5. Ten queues had more than 25 codings. However, despite the variance in queues, the NYT analysis captures a great deal of information on destabilizing events. The political protest component of the NYT analysis derives from the coding of 280 political protest events and 58 recapitulation passages; these protests involved an estimated 6,839,135 participants. These protesters were involved in 138 mass demonstrations, 15 job actions, 79 riots and 41 symbolic actions (sit-ins, boycotts, self-immolations, etc.). There were another 62 more solitary forms of political protests (speeches, postings, banners, etc.). The political violence component of the NYT analysis

[9] The operators were ten students who enrolled in a section of the Cline Center's Research Practicum for the 2009-10 AY that focused on climate change and civil unrest. The students received an orientation and were trained for a three-month period before they began "production" coding. They also had to pass a "gold standard" test that measured their performance on a set of articles that had been coded by a set of supervisors trained in the Societal Stability Protocol.

[10] Recapitulations, by definition, do not provide specific information on dates and cities; otherwise they would be coded as events. They do, however, make references to events that happened "last week" or "last month" within a specific city, province or country. Thus, as much event data is aggregated (e.g., we aggregate into pre-post periods for focal countries), it is valuable to know that "hundreds of thousands protested in Anbar province last month" or that "insurgents killed 150 last week." These passages are based on investigative reporting and often compensate for gaps in news coverage.

[11] For a coding to be "relevant" it had to be within the focal country, within the thirty-six month time frame, and it had to extract information on a political protest or a political attack initiated by a non-state actor.

is based on the coding of 260 politically motivated attacks and 82 recapitulation passages; these attacks killed an estimated 31,830 individuals and injured another 5,683. The attacks included 298 garden-variety attacks against individuals, 79 attacks against property, 76 assassinations, 3 suicide attacks, 34 kidnappings and 5 executions.

Table 3
Information on Episode-specific Queues for News Reports

Random Order #	Country of Disaster	Type of Natural Disaster	Sampling Start Date	Sampling End Date	NYT Queue #	SWB Queue #	NYT Article Count	SWB Article Count	Relevant NYT Codings of Protests and Attacks	Relevant SWB Codings of Protests and Attacks
1	India	Storm	6/4/1992	6/5/1995	38	94	120	2416	30	561
2	Zambia	Flood	8/2/2002	12/2/2005	39	95	6	883	0	34
3	Philippines	Storm	9/21/1980	9/23/1983	40	96	57	236	30	47
6	Djibouti	Flood	10/12/2002	10/16/2005	41	97	9	1103	0	19
8	Ecuador	Flood	2/2/1982	2/2/1985	42	98	7	51	0	.
11	Sri Lanka	Flood	6/16/2001	6/20/2004	43	99	17	897	2	157
13	India	Flood	1/30/1982	1/30/1985	45	101	226	1804	97	276
19	China	Flood	1/9/1996	1/29/1999	46	102	178	4805	70	10
16	Madagascar	Storm	11/6/2001	11/6/2004	47	103	8	905	10	186
18	India	Flood	5/12/1991	5/12/1994	48	104	124	2263	40	119
14	China	Storm	3/1/1989	3/1/1992	49	105	207	8907	7	234
21	Bolivia	Flood	7/7/1984	7/8/1987	50	106	20	102	12	6
23	China	Storm	1/9/1985	1/10/1988	51	107	120	3993	32	126
24	Bangladesh	Flood	1/3/1997	3/23/2000	52	108	20	440	5	184
25	Indonesia	Flood	6/1/1982	6/1/1985	53	109	23	448	0	11
27	Philippines	Flood	12/21/1992	12/22/1995	54	110	18	362	1	78
28	Bangladesh	Flood	11/13/1993	11/13/1996	55	111	20	585	3	344
98	Mexico	Storm	3/21/2001	3/21/2004	92	112	80	17468	4	41
31	China	Storm	3/2/2002	3/4/2005	58	114	104	2754	21	.
36	Philippines	Storm	6/1/1999	6/4/2002	59	115	41	57	0	0
38	Madagascar	Storm	9/13/1984	9/14/1987	60	116	2	2701	16	.
39	Philippines	Storm	5/5/1999	5/7/2002	61	117	38	1495	44	.
40	India	Flood	1/30/1981	1/31/1984	62	118	177	204	21	20
41	Chile	Flood	1/15/1986	1/15/1989	65	121	55	717	2	153
47	Philippines	Storm	11/23/1995	11/25/1998	66	122	23	2420	73	.
49	India	Flood	1/30/1985	1/31/1988	67	123	231	367	0	.
52	Philippines	Storm	4/18/1993	4/22/1996	68	124	17	427	5	105
55	Brazil	Flood	12/1/1982	12/1/1985	69	125	320	768	9	.
60	Sri Lanka	Flood	5/20/1999	5/24/2002	70	126	17	1100	6	.
61	India	Flood	2/19/2000	2/19/2003	71	127	214	10038	45	.
64	Mexico	Storm	3/16/1987	3/16/1990	72	128	104	212	16	.
69	Sri Lanka	Flood	6/2/1991	6/2/1994	75	131	8	308	4	.
73	India	Storm	12/14/1994	12/16/1997	76	132	119	3587	0	.
75	Tanzania	Flood	10/7/1987	10/7/1990	77	133	5	3094	32	.
76	Iran	Flood	10/4/1987	10/4/1990	78	134	330	547	1	.
78	Morocco	Flood	2/15/1994	2/16/1997	79	135	15	4861	4	.
80	Bhutan	Flood	2/1/1999	3/1/2002	80	136	3	1370	0	.
83	India	Storm	5/1/1993	5/5/1996	81	137	126	195	0	.
86	Tajikistan	Flood	11/24/1990	11/24/1993	82	138	9	2601	12	.
88	Philippines	Storm	2/17/1996	2/18/1999	83	139	28	1307	0	.
89	Cambodia	Flood	1/29/1993	1/30/1996	84	140	43	4987	0	.
91	Zambia	Flood	7/25/1987	7/25/1990	85	141	17	729	3	.
92	China	Flood	12/20/2002	12/25/2005	86	142	56	1444	7	.
94	Viet Nam	Flood	4/25/1998	5/10/2001	87	143	69	430	0	.
96	China	Storm	3/10/1995	3/14/1998	91	144	151	563	16	.

Despite the extensive reach of the NYT's coverage we operate under no illusion that the articles reviewed here capture all of the destabilizing events that occurred in the period studied for each country. News coverage, particularly by a single provider, is affected by a range of factors (country biases, threshold levels for news worthiness, a flurry of news worthy events in other locales or on other topics, etc.). However, there is no reason to suspect that these factors operate differentially across the pre and post periods identified here. This notwithstanding, to compensate for shortcomings associated with the use of single news providers, the SPEED project is committed to the use of multiple sources of information and has invested heavily in the digitization of multiple sources of historical news reports. The value of these efforts can be seen in Table 3. Using the same criteria used to identify NYT articles, we identified 27 times as many news reports in the SWB archive, almost 100,000 altogether.

Unfortunately, the volume of the SWB reports – and the fact that they came "on-line" late in the planning of this project – outstripped the resources we had available to extract information from them. Thus, we had to focus our efforts on a subset of episodes; we were ultimately able to complete twenty-one episodes. However, because of the number of news reports in some queues we could analyze only a sample of them. If there were less than 1,000 news reports in a queue we analyzed all reports; if there was between 1,000 and 3,000 reports we selected a random sample totaling one-third of the reports. If a queue had 3,000 or more news reports we sampled one out of ten reports. In selecting these episodes our primary criterion was to select them in accord with their order in the random sample of disasters. However, one episode, a 1983 flood in Ecuador, was eliminated because it lacked information on the province within which the disaster occurred; this prevented us from calculating a proximity measure that was central to the empirical analysis, as noted below. Moreover, at an early stage of the process a miscommunication led to the processing of a handful of smaller queues that were coded out of sequence (SWB queues # 115, 118, 121, 124; see Table 3).

As can be seen in Table 3 there are more SWB codings per queue than NYT codings and far less variance across queues. Only one queue had no codings and only two had less than ten; the average relevant codings from the SWB queues is 129, while the median is 105. Eleven queues had more than 100 codings of protests and attacks; four had more than 200. Altogether, the political protest component of the SWB analysis is based on the coding of 862 political protest events and 150 recapitulation passages; these protests involved approximately 5,126,079 participants. These participants were involved in 397 mass demonstrations, 165 job actions, 135 riots, and 204 symbolic acts. There were another 324 more solitary forms of political protests captured (speeches, postings, banners, etc.). The political violence component of the SWB analysis is based on the coding of 1,187 politically motivated attacks and 269 recapitulation passages; these attacks killed approximately 13,021 individuals and injured approximately 9,531. The attacks included 1,311 garden-variety attacks against individuals, 407 attacks against property, 79 assassinations, 4 suicide attacks, 71 kidnappings and 7 executions.

It is instructive to compare the raw levels of political protest and violence activity derived from the NYT and SWB archives. To do so, however, requires that adjustments be made in the data; the SWB numbers are based on a sampling strategy applied to a subset of the episodes studied using the NYT archive. Thus, the NYT data must be adjusted to reflect data just those queues that were captured in the SWB analysis. Also, the SWB data must be adjusted to reflect the sampling strategy that was used to extract information. If these adjustments are made the comparable number for the NYT archive are protests involving 4,936,827 individuals participating in political protests and political attacks that killed

24,534 and injured 1,035. The comparable numbers for the SWB archive are 8,465,753 protesters and 25,617 individuals killed and 23,793 injured in political attacks. While the SWB estimates for the number killed is very comparable to the of the NYT estimates (104%), the disparity is much greater for the number of protesters and those injured. The SWB estimate for protesters is 171% of the NYT estimates; the SWB estimate for injuries is 230% of the NYT estimates. This suggests, of course, that the local sources used in the SWB do a better job of capturing less salient, non-lethal events than the NYT. While this is unsurprising it is important to consider when interpreting the final results.

Floods, Storms and Levels of Civil Unrest: The Results of the Quasi-experiment

The design of this research makes possible a relatively straightforward, but demanding, examination of the impact of floods and storms on civil unrest. Our core concern is whether the intensity of civil unrest increases significantly after the occurrence of a major flood or storm. To provide a probing examination of this core concern, however, it is important to address some nuances in the relationship between natural disasters and civil unrest; we examine two. The first has to do with proximity to the natural disaster. As noted above, we captured instability events from screened news reports that make mention of the country in which the flood/storm occurred. However, many of the focal countries listed in Table 2 (China, India, Mexico, etc.) are large, diverse entities that have multiple drivers of civil unrest. Moreover, it is reasonable to suspect that destabilizing effects of natural disasters would be localized in the region where the disaster occurred. The second has to do with the effect of natural disasters on social group relations (religious, ethnic, tribal, national identity, etc.), which account for a good deal of civil unrest in many countries. Where group-based animosities are deeply rooted the deprivations caused by natural disasters can aggravate them and lead to a great incidence of destabilizing events involving social groups. In light of these nuances, we use the intensity of political protest and political violence measures in conjunction with the eighteen month pre-test/post-test periods to test three null hypotheses:

$\mathbf{H_1}$: There is no increase in the intensity of civil unrest after a flood or a storm within the country in which the disaster occurred.

$\mathbf{H_2}$: There is no increase in the intensity of civil unrest after a flood or a storm within areas proximate to where the disaster occurred.

$\mathbf{H_3}$: There is no increase in the intensity of social group-based civil unrest after a flood or a storm within the country in which the disaster occurred.

To test these null hypotheses we used variants of a simple, differenced summation procedure. Consider the test for $\mathbf{H_1}$. For each episode we first summed the scores of the Intensity of Political Protest index (**PROTEST**) and Intensity of Political Violence index (**VIOLENCE**) for the pre and post periods. Second, we subtracted the sum of the pre-period from the sum of the post-period, creating two differenced variables: $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$. To illustrate consider the flood that occurred in India in 1983. There were thirty-three politically motivated attacks in the pre-period; their intensity scores summed to 84. There were fifty attacks in the post period; their intensity scores summed to 145. The difference between these two scores is 61, which suggests that there is an increase in the intensity of political violence in the aftermath of the flood. We used this procedure to calculate differenced scores for each country-specific episode. Then we use a simple t-test to determine whether the differences are statistically significant for the episodes studied. To examine $\mathbf{H_2}$ we simply repeat this procedure for events that unfolded within 500 miles of the province(s) within which the natural disaster occurred.[12] To examine $\mathbf{H_3}$ we repeated this procedure using only events in which the initiator of an event was a member of an identifiable social group (Hindu, Muslim, Falun Gong, Abu Sayyaf, Sikh, etc.) or the target of the was a member of an identifiable social group.

The results of the t-tests for the NYT analysis are reported in Table 4. Two sets of tests were conducted: one uses unweighted differences; the second weights the differences by the number of events used to calculate them. The weighted analyses are considered to be more appropriate because, while some of the differences are based on the comparison of scores of destabilizing events, others are based on very few. As documented in Table 3, the number of relevant codings ranged from 0 to 97. Indeed, with respect to the protest variable we uncovered no political protests in 7 of the episodes (15%); the mean number of protests is 31 (median=10) and the range is from 0 to 173. More than 25% of the episodes had more than 50 protest events during the three-year period we examined. The numbers are smaller for the tests of $\mathbf{H_2}$ and $\mathbf{H_3}$ because they are based on subsets of the destabilizing events.

As can be seen in Table 4 the results are mixed. In no case did the unweighted t-test generate statistically significant results. Examining the results for the weighted analyses demonstrates that $\mathbf{H_1}$ cannot be rejected. The analysis of $\mathbf{H_2}$ suggests that, while there is no change in the intensity of political protests, there is a *decline* in the intensity of political violence. However, the examination of $\mathbf{H_3}$ reveals statistically significant *increases* in both political protests and violence involving social groups; both are significant beyond the .001 level. Thus, $\mathbf{H_3}$ can be rejected.

The results of the t-tests using the events derived from the SWB archive are reported in Table 5. Here again, no statistically significant differences emerged. Moreover, careful reviews of the raw data reveal that the overall lack of significant results and the puzzling patterns of findings are not due to the existence of a handful of outliers. Rather, a highly diverse set of differenced unrest measures exists in the data. Some countries reveal marked declines in unrest after a natural disaster; others reveal marked increases. In most episodes no impact is evident. Moreover, some nations with repeat episodes (China, the

[12] Because our event data are geographically referenced we had the latitude and longitude of the city in which the event took place. We then calculated the difference between the centroid of the city and the centroid of the province where the disaster occurred. In many instances the natural disaster affected more than one province and we calculated up to three distance measures. We used the smallest of the distance measures to determine whether an event occurred within 500 miles of the disaster. Thus, if one distance measure suggested the event was 600 miles from the centroid of one affected province and a second distance measure suggested that the event was 450 miles from the centroid of another affected province, we would use the second distance measure in deciding whether to include the event in the test of $\mathbf{H_2}$.

Table 4

Test of Statistical Significance for the Change in the Level of Civil Unrest, New York Times Archive (Pre-period to Post-period)

	Mean	Standard Error	t-value	Probability Level	Number of Observations
All Political Protests					
Unweighted	-3.14	6.98	-0.45	0.66	45
Weighted by # of Events	-20.1	13.4	-1.5	0.14	45
All Political Attacks					
	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	3.23	4.82	0.67	0.51	45
Weighted by # of Events	8	7.8	1	0.3	45
Political Protests Within 500 Miles of Disaster					
	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	-3.05	3.33	-0.91	0.37	45
Weighted by # of Events	-5.53	4.85	-1.14	0.26	45
Political Attacks Within 500 Miles of Disaster					
	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	-1.72	2.4	-0.72	0.47	45
Weighted by # of Events	-7.6	3.6	-2.1	0.04*	45
Political Protests Involving Social Groups					
	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	2.1	1.2	1.8	0.08	45
Weighted by # of Events	10.3	1.67	6.16	.0001***	45
Politically Attacks Involving Social Groups					
	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	4.42	3.07	1.44	0.16	45
Weighted by # of Events	25.1	6.9	3.62	.001**	45

Philippines) evidence all possible outcomes: marked increases in unrest after some disasters, marked decreases in others, and no effect in still others.

Serial Disasters, "Best Made Plans" and Noisy Data

The mixed and puzzling results reported in Tables 4 and 5 led to an examination of the distribution of destabilizing events by week and month for each episode in the sample. The existence of oddly distributed peaks in the intensity variables in these temporal displays suggested that other factors might be at work that were contaminating the results of the research design. The SPEED data on cataclysmic events suggested that some of these peaks could be related to other natural disasters that occurred during the study period. This led to a more comprehensive review of the CRED archive to identify all other natural disasters (droughts, earthquakes, epidemics, temperature extremes, floods, mass movements, storms, wildfires and volcanoes) that occurred during the three-year time frame for each of the episodes studied here. We used the same severity criteria employed in our random sample (100 killed or 10,000 affected). The results are reported in Table 6; they are highly informative.

Table 6 makes it clear that most of the episodes examined here are located in highly vulnerable countries that suffer from serial disasters. Over 300 comparable natural disasters occurred in the forty-five aggregated *pre-periods*, killing over 95,000 and affecting over 2 billion people; 309 comparable disasters occurred in the aggregated *post-periods*, killing almost 105,000 and affecting 1.7 billion people. On average each episode had seven comparable disasters in both the pre and post periods. Only two episodes had no other disasters in the study time frame (Bhutan, 2000 and Zambia, 2004). The most contaminated episodes were those involving Bangladesh, India, the Philippines and China, which account for all of the highest incidences of serial disasters. The data reported in Table 6 make it clear that the 45 randomly selected floods and storms examined above constitute little more than a ripple in a turbulent sea of natural disasters. The existence of these serial disasters call into question the results reported in Tables 4 and 5 – as well as the utility of the research design employed to generate them.

Fortunately, the length of the pre and post periods adopted here makes it possible to make post-hoc adjustments in our analysis that comport with the realities of serial disasters. Thus, we restructured our analysis to focus on all 684 of the disasters instead the 45 randomly selected disasters.[13] This reorientation involved: (1) identifying temporal periods within a country that were plagued by natural disasters; (2) re-aggregating the data on political protests and violence to conform to those temporal periods; and (3) comparing the levels of civil unrest between periods plagued by natural disasters and those that were not. To implement this restructuring two challenges had to be addressed. The first was to define *study periods* associated with each of the 684 natural disasters. These study periods are the functional equivalent of the post-period in the initial design.

[13] This figure includes the 45 randomly selected floods and storms and the 639 other natural disasters reported in Table 6.

Table 5

Test of Statistical Significance for the Change in the Level of Civil Unrest, Summary of World Broadcasts Archive (Pre-period to Post-period)

All Political Protests

	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	-1.72	38.9	-0.04	0.96	21
Weighted by # of Events	15.6	48	0.32	0.74	21

All Political Attacks

	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	15.3	19.2	0.8	0.43	21
Weighted by # of Events	26.2	25.8	1	0.32	21

Political Protests Within 500 Miles of Disaster

	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	4.7	12.9	0.36	0.72	21
Weighted by # of Events	19.5	21.1	0.92	0.37	21

Political Attacks Within 500 Miles of Disaster

	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	11.2	12.3	0.91	0.37	21
Weighted by # of Events	37.7	21	1.8	0.09	21

Political Protests Involving Social Groups

	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	-6.24	7.7	-0.81	0.43	21
Weighted by # of Events	-4.5	7.8	-0.58	0.57	21

Politically Attacks Involving Social Groups

	Mean	Standard Error	t-value	Probability Level	Number of Observations
Unweighted	-1.83	6.9	-0.27	0.79	21
Weighted by # of Events	-7.54	8.6	-0.88	0.39	21

Table 6
Summary of Other Natural Disasters in Study Period

County of Disaster	Period Start Date	Period End Date	NYT Queue Number	SWB Queue Number	Pre-Period			Post-Period		
					Number of Other Disasters	Deaths	Affected	Number of Other Disasters	Deaths	Affected
India	6/4/1992	6/5/1995	38	94	11	11865	132336000	6	3218	12548035
Zambia	8/2/2002	12/2/2005	39	95	2	179	13835	1	0	1200000
Philippines	9/21/1980	9/23/1983	40	96	13	1799	2631309	7	385	2030352
Djibouti	10/12/2002	10/16/2005	41	97	0	0	0	1	0	150000
Ecuador	2/2/1982	2/2/1985	42	98	2	407	700000	0	0	0
Sri Lanka	6/16/2001	6/20/2004	43	99	1	0	1000000	1	235	695000
India	1/30/1982	1/30/1985	45	101	9	2966	139885793	12	6711	21086900
China P Rep	1/9/1996	1/29/1999	49	105	21	6142	177069874	14	4619	261832131
Madagascar	11/6/2001	11/6/2004	47	103	4	707	1171545	2	395	1032429
India	5/12/1991	5/12/1994	48	104	10	3340	9849383	11	13393	141437500
China P Rep	3/1/1989	3/1/1992	46	102	19	4093	186259341	13	5334	218856914
Bolivia	7/7/1984	7/8/1987	50	106	0	0	0	1	25	20000
China P Rep	1/9/1985	1/10/1988	51	107	12	1859	7867900	10	720	5409132
Bangladesh	1/3/1997	3/23/2000	52	108	7	681	4860105	5	256	727580
Indonesia	6/1/1982	6/1/1985	53	109	3	345	33000	6	361	390500
Philippines	12/21/1992	12/22/1995	54	110	11	710	4290541	21	1937	5336523
Bangladesh	11/13/1993	11/13/1996	55	111	6	693	1120059	9	1663	20507610
China P Rep	3/2/2002	3/4/2005	56	112	29	2524	501973403	20	700	63571230
Philippines	6/1/1999	6/4/2002	58	114	13	911	9725506	14	729	3897748
Madagascar	9/13/1984	9/14/1987	59	115	0	0	0	1	0	28223
Philippines	5/5/1999	5/7/2002	60	116	12	817	7977634	15	777	4061841
India	1/30/1981	1/31/1984	61	117	7	1602	116432193	9	2420	6799300
Chile	1/15/1986	1/15/1989	62	118	2	39	84924	1	54	81000
Philippines	11/23/1995	11/25/1998	65	121	7	103	542221	6	753	9661138
India	1/30/1985	1/31/1988	66	122	14	4425	14069133	7	1735	321680000
Philippines	4/18/1993	4/22/1996	68	124	16	703	5506211	14	1843	4540286
Brazil	12/1/1982	12/1/1985	69	125	5	493	23388300	3	301	672000
Sri Lanka	5/20/1999	5/24/2002	70	126	1	2	100000	2	5	1375000
India	2/19/2000	2/19/2003	71	127	16	22472	117606627	10	2975	344840200
Mexico	3/16/1987	3/16/1990	72	128	1	48	25000	0	0	0
Sri Lanka	6/2/1991	6/2/1994	75	131	2	41	547151	3	8	480000
India	12/14/1994	12/16/1997	77	133	7	2949	33831535	14	4713	45069943
Tanzania Uni Rep	10/7/1987	10/7/1990	78	134	1	0	110000	2	383	162000
Iran Islam Rep	10/4/1987	10/4/1990	79	135	2	91	160000	2	40100	710217
Morocco	2/15/1994	2/16/1997	80	136	0	0	0	1	25	60000
Bhutan	2/1/1999	3/1/2002	81	137	0	0	0	0	0	0
India	5/1/1993	5/5/1996	82	138	9	13268	140262000	7	2949	33831535
Tajikistan	11/24/1990	11/24/1993	83	139	1	243	0	1	5	75357
Philippines	2/17/1996	2/18/1999	85	141	4	74	546939	6	804	9933999
Cambodia	1/29/1993	1/30/1996	86	142	1	0	5000000	0	0	0
Zambia	7/25/1987	7/25/1990	88	144	0	0	0	0	0	0
China P Rep	12/20/2002	12/25/2005	89	145	23	1065	219927024	30	1505	135176054
Viet Nam	4/25/1998	5/10/2001	90	146	6	625	4080417	3	623	7188701
China P Rep	3/10/1995	3/14/1998	91	147	20	7586	294334806	16	1484	16436973
Mexico	3/21/2001	3/21/2004	92	148	0	0	0	2	31	228603

They must be defined in order to determine the levels of civil unrest that occur in the wake of natural disasters. The second challenge was to create *benchmark periods* with which the levels of unrest in the study periods could be compared. These benchmark periods are the functional equivalent of the pre-periods in the initial design.

Identifying Study and Benchmark Periods

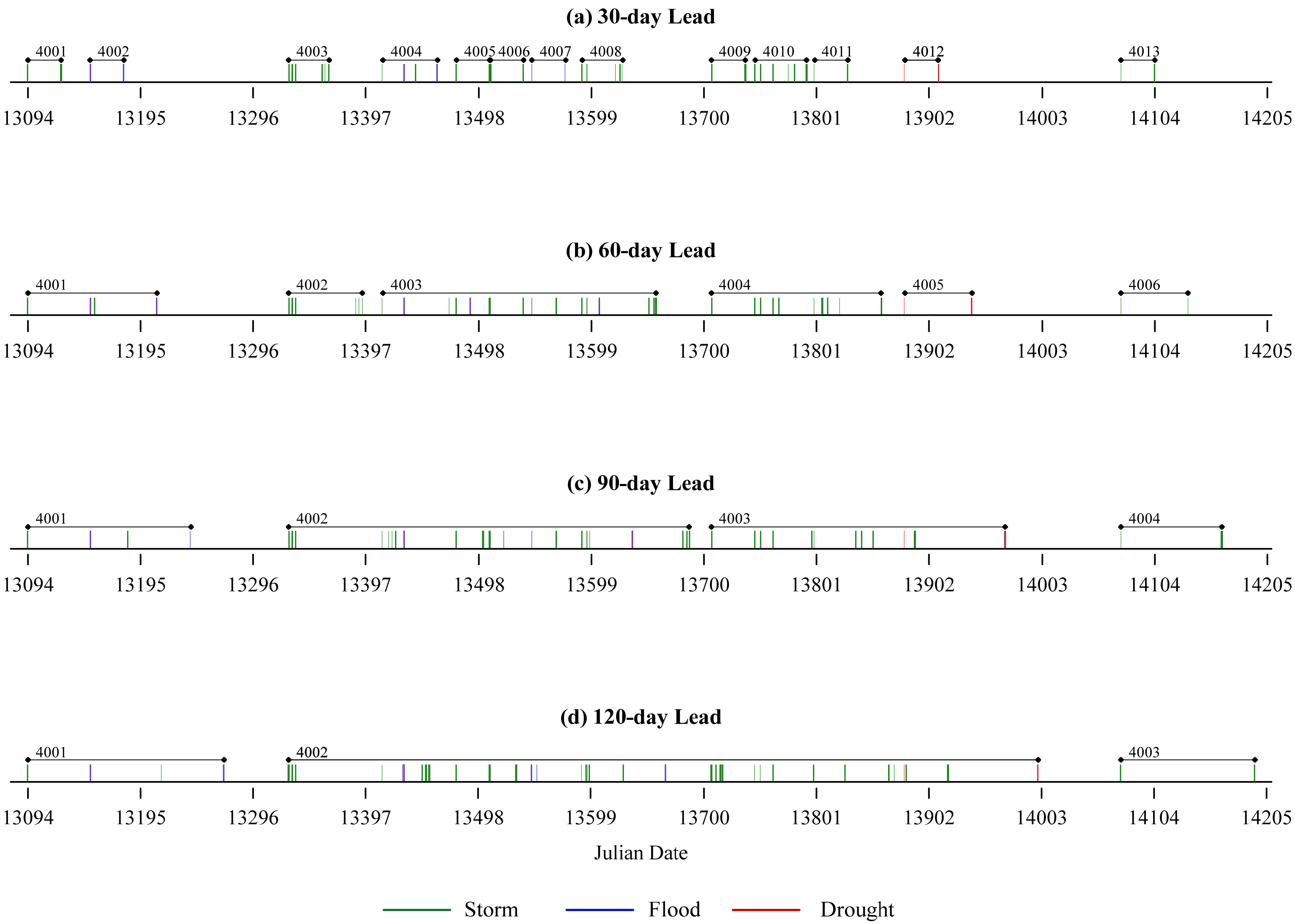
The number of natural disasters that occurred during the 45 episodes meant that the length of the study periods had to be substantially shorter than the eighteen month post-period, while still being long enough to capture the destabilizing effects of the natural disaster.[14] Because we had no prior knowledge concerning the length of time that disasters exerted destabilizing effects, we experimented with periods using four lead times: 30 days, 60 days, 90 days and 120 days after the start date of the disaster. Efforts to use these lead times to define the study periods were compounded by the fact that many of the disasters in Table 6 were closely sequenced. Indeed, the average length of the interval between disasters was about a month. To deal with the issues created by these temporal distributions we created flexible study periods that varied in length and often included more than one disaster. Any days in the thirty-six month period used to collect event data for the initial country-episode that were not assigned to a study period were included in the benchmark period. They were used to derive a "normal" level of unrest against which unrest in the wake of disasters was compared.

Figure 2 (a-d) illustrates the procedure used to define the study and benchmark periods. It reports the distribution of disasters across the thirty-six month pre-post period for a storm in the Philippines that occurred on March 3, 1982 (CRED # 1982-0038, see Table 2). During this three-year time frame the Philippines had twenty-one disasters that met the criteria used here: sixteen were storms, three were floods, and two were droughts. These twenty-one disasters are arrayed in Figure 2 using a Julian calendar that defines the time frame within which we searched for information on destabilizing events in the NYT and SWB archives. The initial date is 13094, while 14205 is the last date; the period covers 1111 days altogether.

Figure 2 (a) displays the application of the 30-day lead periods to the Julian dates. Because the distribution of the disasters led to some overlap, the use of a 30-day lead resulted in thirteen study periods that varied in length from 30 days to 49 days. These thirteen periods covered 437 days of the 1111 in the entire period, leaving a residual of 674 days for the benchmark period. The destabilizing events that occurred on one of these 674 days were used to gauge the normal level of civil unrest for the Philippines during this time frame. Figure 2 (b) displays the application of the 60-day lead periods to the Julian dates. Because of the longer lead time more of the episodes overlap, resulting in more disasters being grouped together. This reduced the number of study periods to six; they varied in length from 60 to 245 days. These study periods consumed 699 of the 1111 days, leaving 412 for the benchmark period. The benchmark periods become progressively smaller with the longer lead times shown in Figure 2 (c) and 2 (d). Using 90-day leads resulted in only four study periods, ranging from 90 to 359 days, leaving 272 for the benchmark period. Using 120-day leads resulted in only three study periods,

[14] The range of other natural disasters across three-year episodes is from 1 to 30, with a mode of 4 and a mean of 8. Seventeen of the episodes had 10 or more natural disasters. The length of the study periods across the 45 episodes ranged from 1096 to 1260.

Figure 2.
Illustrative Temporal Restructuring of CRED Disasters: Philippines, 1982



ranging from 120 to 672 days, leaving only 192 for the benchmark period. The 90-day and 120-day leads reduced the days in the benchmark period to 24% and 17% respectively, of the total number of days in the thirty-six month period.

For the NYT analysis, applying the procedure outlined above led to 295 episodes with 30-day leads; 175 episodes with 60-day leads; 134 episodes with 90-day leads and 109 episodes with 120-day leads. When applied to the SWB analysis this procedure led to 153 episodes with 30-day leads; 93 episodes with 60-day leads; 68 episodes with 90-day leads and 53 episodes with 120-day leads.

Constructing the Differenced Measures

To gauge the effects of the disasters on civil unrest we summed the intensity of political protest and violence measures for the study and benchmark periods, as we did in the initial analysis. However, because the time frame for these sums varies across episodes in this analysis, we had to adjust them by dividing the summed values by the number of days in the period. Because of the existence of many episodes with '0' values for the protest and violence variables, we had to add '1' to the sums before dividing by the number of days in the period. Then, to generate a summary comparison measure for each episode, we differenced the adjusted intensity measures by subtracting the adjusted values in the benchmark period from the adjusted values in the study period. Because these adjusted differenced sums are difficult to interpret, we divided them by the adjusted sum for the benchmark period. This allows us to interpret them as a proportionate increase or reduction from the benchmark period. Thus, a value of .05 would indicate a 5% increase in civil unrest; a -.07 would indicate a 7% decrease in unrest.

The following formulas report the calculations used to construct the two civil unrest variables for the restructured analysis:

$$\mathbf{PROTEST_{DIF} = ((PROTEST_{SP} + 1)/ DAYS_{SP}) - (PROTEST_{BM} + 1)/DAYS_{BM})) / ((PROTEST_{BM} + 1)/DAYS_{BM})}$$

$$\mathbf{VIOLENCE_{DIF} = ((VIOLENCE_{SP} + 1)/ DAYS_{SP}) - (VIOLENCE_{BM} + 1)/DAYS_{BM})) / ((VIOLENCE_{BM} + 1)/DAYS_{BM})}$$

Lead-times and Benchmark Periods: A Cautionary Note

The formulas used to construct the $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$, in conjunction with the observations made in Philippines example about the implications of progressively longer lead times for constructing benchmark periods, suggests some caution in using the 90- and 120-day study periods. The concern here is that short benchmark periods can produce unreliable benchmarks. Ideally, the benchmark levels of civil unrest should not vary much depending upon the length of the study period; this will insure that the differenced measures outlined above are capturing only differences in unrest due to the length of the study period. Table 7 draws from an analysis of all study and benchmark periods and presents some pertinent data on this point.

Table 7
Information on Episode-specific Queues for News Reports

	Benchmark Periods								Study Periods							
	Days in Period				Proportion of Episodes with:		Mean Adjusted Score* for:		Days in Period				Proportion of Episodes with:		Mean Adjusted Score* for:	
	Minimum	Maximum	Mean	Median	Political Protests	Political Attacks	$PROTEST_{BM}$	$VIOLENCE_{BM}$	Minimum	Maximum	Mean	Median	Political Protests	Political Attacks	$PROTEST_{SP}$	$VIOLENCE_{SP}$
New York Times Analysis																
30-day Leads (n=295)	185	1066	583	535	0.73	0.51	0.066	0.036	30	913	518	545	0.11	0.13	0.003	0.003
60-day Leads (n=175)	83	1036	468	383	0.47	0.46	0.067	0.025	60	1018	634	705	0.21	0.18	0.006	0.005
90-day Leads (n=134)	50	1006	405	277	0.38	0.36	0.100	0.026	90	1051	697	808	0.27	0.23	0.008	0.008
120-day Leads (n=109)	20	976	370	248	0.27	0.30	0.134	0.032	120	1081	733	803	0.30	0.21	0.010	0.009
Summary of World Broadcasts Analysis																
30-day Leads (n=153)	185	1036	586	523	0.87	0.89	0.242	0.180	60	913	518	500	0.46	0.50	0.026	0.022
60-day Leads (n=93)	90	976	440	365	0.91	0.87	0.238	0.173	120	1008	666	713	0.67	0.59	0.042	0.036
90-day Leads (n=68)	57	916	367	265	0.91	0.76	0.265	0.167	180	1041	741	830	0.66	0.62	0.058	0.049
120-day Leads (n=53)	27	856	331	213	0.72	0.62	0.168	0.100	240	1073	777	891	0.60	0.55	0.059	0.056

* These values are adjusted for the number of days in the study or benchmark period

The first point to note from the data in Table 7 is that the relationship between the length of the study periods and the length of the benchmark period is a general problem, not one unique to the Philippines example. Using the 30-day lead periods about 53% of the days in the thirty-six month period are available for calculating the benchmarks, for both the NYT and the SWB analyses. Only a handful of the benchmark periods are calculated with less than 25% of the available days. For the 60-day lead periods between 40 and 43% of the available days are available for calculating benchmarks. However, for the 90 and 120-day lead periods, the average number of days used for the benchmarks drops to about 33%. Moreover, about half of the benchmarks are calculated with fewer than 25% of the available days. The implications of this for the stability of the benchmark levels of unrest can be seen in columns 7-8. For the NYT data the average values of **$PROTEST_{BM}$** and **$VIOLENCE_{BM}$** are similar for the 30- and 60- day lead periods. But they jump considerably for the 90- and 120- day periods, especially for **$PROTEST_{BM}$**. The same can be said for the SWB data: the average values of **$PROTEST_{BM}$** and **$VIOLENCE_{BM}$** are similar for the 30- and 60- day lead periods but they fluctuate considerably for the 90- and 120-day periods. The cause of these fluctuations is that the small denominators (i.e., the number of days in the benchmark period) lead to more variation depending upon the distribution of political instability events along the time continuum.

It should also be noted that, while there is some gain to be realized with respect to the study periods by using longer lead times, it is not appreciable. As the last four columns in Table 7 indicate, the proportion of study periods with some type of political protest activity increases from 11% within the 30-day lead periods to 30% in the 120-day lead periods for the NYT analysis; the increase is similar for the SWB study periods. The proportion of study periods with some incidence of political violence increases from 13% to 21% for the NYT analysis, but somewhat less for the SWB analysis (50% to 55%). However, when the average scores for **$PROTEST_{SP}$** and **$VIOLENCE_{SP}$**, when adjusted for the longer time frames, are examined (col. 16 and 17), there is much less change. For the NYT analysis, the average adjusted **$PROTEST_{SP}$** score increases from .003 to .01 while the average adjusted $\mathbf{VIOLENCE_{SP}}$ score increases from .003 to .009; the comparable figures for the SWB data are .026 to .059 for the protest averages and .022 to .056 for the violence averages.

Results of the Restructured Analysis

The results for the restructured analysis using the NYT data are reported in Table 8. In contrast to the data reported in Table 4, the results in Table 8 are remarkably consistent. All of the values for $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$ are negative, suggesting that civil unrest declines in the wake of natural disasters. Moreover, all but a handful of the differences using the 120-day leads (and one for the 90-day leads) are statistically significant at or beyond the .001 level. The weighted differences are always larger than the unweighted differences. If the weighted results for $\mathbf{PROTEST_{DIF}}$ are examined the data suggest that, for the country as a whole, protests occur at 20% to 25% below levels evidenced in the benchmark period. Moreover, declines in the intensity of protests in locales within 500 miles of the disaster are not much different from those for the nation as a whole, although the analysis was done only for the 30-day lead period due to issues related to the calculation of distances.[15] Perhaps the most significant differences in the results for political protests is that protests involving social groups decline to a considerably lesser extent than other types of protest. These protests register a 6% decline in the 30-day lead period and essentially revert to benchmark levels in the other periods.

A somewhat different pattern emerges with respect to political violence. All differences in $\mathbf{VIOLENCE_{DIF}}$ are statistically significant. If the weighted results for the nation as a whole are examined, they reveal that the proportionate decline in political violence is less than that in political protests. Depending on the period examined, political violence declines between 5% and 12%. For the whole country the largest decline is evidenced in the 30-day lead period; the declines are about half that level in the other periods as the data suggest a convergence to benchmark levels after the first month. The decline in levels of political violence in locales proximate to the disaster is not much different from those in the nation as a whole. As was the case with protests, political attacks involving social groups decline much less that for other types of political attacks. Moreover, they do not evidence much change across the different periods.

The SWB data are reported in Table 9. As was the case with the NYT analysis, all of the values for $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$ are negative. Also, all of the differences are statistically significant at or beyond the .001 level, with the exception of a handful from the unweighted analysis that use the 120-day leads and the 90-day leads. The data in Table 9 reinforce the conclusion that, on balance, civil unrest declines in the wake of natural disaster associated with climate change, whether we focus on political protests or

[15] Because the 60-, 90- and 120-day lead periods often included several disaster, many of which affected more than one province, calculating the difference between the location of an event and the nearest province became enormously complex. Moreover, it had the potential to generate misleading data. Thus, the distance analysis was restricted to the 30-day lead periods, where we were more confident of the data.

Table 8				
Test of Significance for Civil Unrest Measures, Restructured New York Times Analysis				
All Political Protests				
	30 Day Lead Period	**60 Day Lead Period**	**90 Day Lead Period**	**120 Day Lead Period**
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.05***	-.04***	-.04***	-0.03*
Weighted by # of Events	-.19***	-.22***	-.26***	-.22***
N	295	175	134	109
All Political Attacks				
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.03***	-.02***	-.01***	-.02***
Weighted by # of Events	-.12***	-.06***	-.05***	-.05***
N	295	175	134	109
Political Protests Within 500 Miles of Disaster				
	30 Day Lead Period	**60 Day Lead Period**	**90 Day Lead Period**	**120 Day Lead Period**
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.06***	N.A.	N.A.	N.A.
Weighted by # of Events	-.22***	N.A.	N.A.	N.A.
N	295			
Political Attacks Within 500 Miles of Disaster				
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.03***	N.A.	N.A.	N.A.
Weighted by # of Events	-.13***	N.A.	N.A.	N.A.
N	295			
Political Protests Involving Social Groups				
	30 Day Lead Period	**60 Day Lead Period**	**90 Day Lead Period**	**120 Day Lead Period**
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.01***	-.004***	-.005**	-.005
Weighted by # of Events	-.06***	-.007***	-.002	-.001
N	295	175	134	109
Political Attacks Involving Social Groups				
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.02***	-.003***	-.01**	-.01*
Weighted by # of Events	-.09***	-.05***	-.05***	-.06***
N	295	175	134	109

Table 9				
Test of Significance for Civil Unrest Measures, Restructured Summary of World Broadcasts Analysis				
All Political Protests				
	30 Day Lead Period	**60 Day Lead Period**	**90 Day Lead Period**	**120 Day Lead Period**
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.02***	-.02***	-.03***	-.07***
Weighted by # of Events	-.09***	-.08***	-.11***	-.13***
N	153	93	68	53
All Political Attacks				
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.03***	-.03***	-.03***	-.01
Weighted by # of Events	-.07***	-.09***	-.09***	-.08***
N	153	93	68	53
Political Protests Within 500 Miles of Disaster				
	30 Day Lead Period	**60 Day Lead Period**	**90 Day Lead Period**	**120 Day Lead Period**
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.17***	N.A.	N.A.	N.A.
Weighted by # of Events	-.27***	N.A.	N.A.	N.A.
N	161	100	75	60
Political Attacks Within 500 Miles of Disaster				
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.01***	N.A.	N.A.	N.A.
Weighted by # of Events	-.04***	N.A.	N.A.	N.A.
N	161	100	75	60
Political Protests Involving Social Groups				
	30 Day Lead Period	**60 Day Lead Period**	**90 Day Lead Period**	**120 Day Lead Period**
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.01***	-.01**	-.01*	-.01
Weighted by # of Events	-.08***	-.03***	-.04***	-.05***
N	153	93	68	53
Political Attacks Involving Social Groups				
	Mean	**Mean**	**Mean**	**Mean**
Unweighted	-.02***	-.02***	-.01*	-.01
Weighted by # of Events	-.05***	-.07***	-.06***	-.04***
N	153	93	68	53

political violence. Strengthening this general conclusion further is that, for the most part, the magnitude of the declines from the two analyses is fairly similar, with two exceptions.

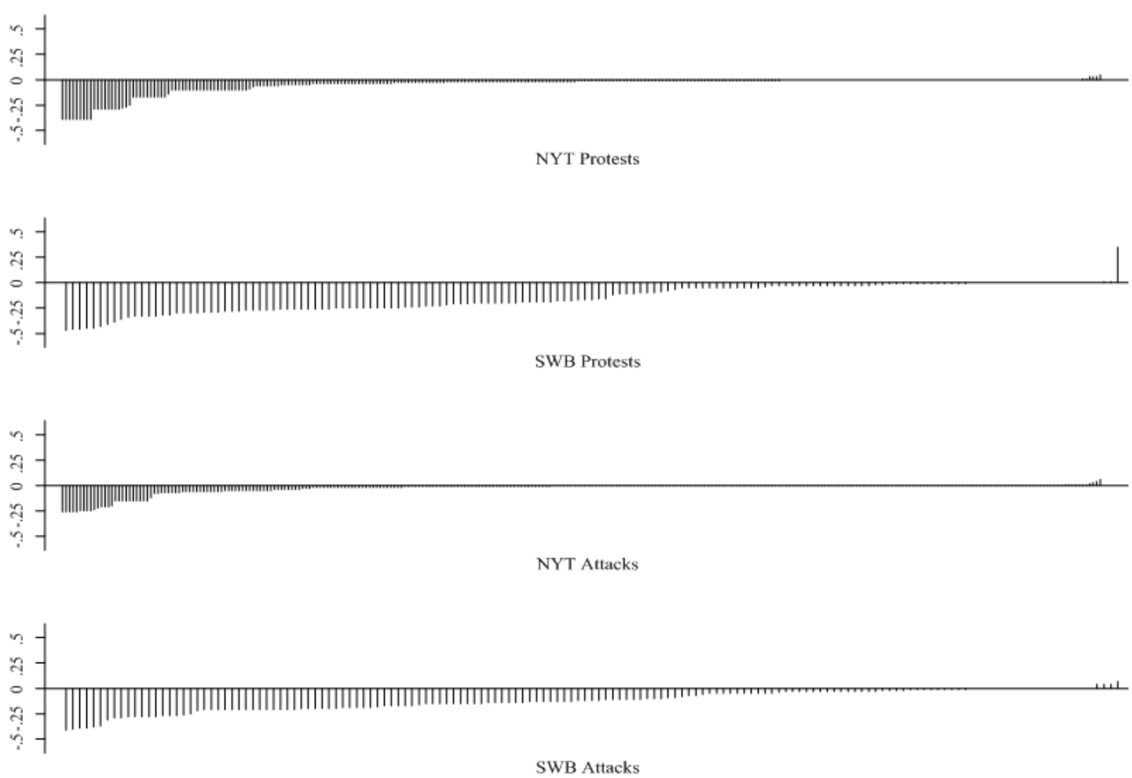
If the average of the four time periods for all political protests is compared across the NYT and the SWB analysis, the NYT data show a 22% decline in political protests; the SWB data show a 10% decline. If the NYT averages are recalculated to include just the queues for which we have SWB data, however, the difference is even larger: a 30% decline as opposed to a 10% decline. If the average for all political attacks is compared, the NYT analysis shows a 7% decline in political violence, compared to an 8% decline using the SWB data. These figures are unchanged if the NYT data is recalculated to include just the SWB queues. Comparing the numbers for political protests in locales proximate to the disaster, the NYT and SWB estimates are identical; both predict a 27% decline. However, the NYT estimates for a decline in political violence are significantly greater than the SWB estimates: 16% as opposed to 4%. Finally, the data from the two analyses provide a remarkably consistent picture of changes in civil unrest among social groups. The NYT analysis shows an average 2% decline in political protests involving social groups, compared to a 4% decline from the SWB analysis. If just the SWB queues are analyzed, the NYT average is 3%. A similar picture emerges with respect to political violence involving social groups. The NYT average is 3% compared to 5% for the SWB average; if the NYT average is calculated using only SWB queues the average is 6%.

It is clear from the restructured analysis that levels of political protest and violence decline in the wake of natural disasters associated with climate change. But the aggregated data reported in Table 8 and 9, and the general convergence between the aggregated NYT and the SWB data, obscure two important points that pertain to the episode-level data. The first point is that, at the episode level, the SWB estimates rest on a much stronger empirical base because of the greater number of events captured from the SWB archive. Consider, for example, that 87% of the 295 NYT study periods with 30-day leads had no political protests and 84% had no politically motivated attacks. In contrast, only 46% of the 156 SWB study periods with 30-day leads had no political protests; the same proportion had no politically motivated attacks. As a consequence, the values for $\mathbf{PROTEST_{SP}}$ in the 295 NYT episodes are based on an average of .33 events (range is from 0 to 10); the values for $\mathbf{VIOLENCE_{SP}}$ are based on an average of .52 events (range from 0 to 16). The values for $\mathbf{PROTEST_{SP}}$ in the 156 SWB episodes, on the other hand, are based on an average of 4.1 events (range from 0 to 129); the values for $\mathbf{VIOLENCE_{SP}}$ are based on an average of 3.8 events (range from 0 to 16). Thus, the SWB estimates are, on average, derived from between 7 and 12 times as many events as the NYT estimates. The firmer empirical base for the SWB analysis provides for more stable estimates of $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$, and likely accounts for the somewhat smaller SWB estimates cited above. [16]

[16] The values for $\mathbf{PROTEST_{BM}}$ and $\mathbf{VIOLENCE_{BM}}$ for the benchmark periods derived from using 30-day leads rest on larger numbers. But the SWB numbers are roughly 5-6 times as large as the NYT numbers. These larger numbers, in conjunction with the larger numbers for $\mathbf{PROTEST_{SP}}$ and $\mathbf{VIOLENCE_{SP}}$ simply provide for more stable estimates of $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$ when the SWB data are employed.

The second point is that there are significant cross-episode differences in the impact of natural disasters on civil unrest. The cross-episode differences in **$\mathbf{PROTEST_{DIF}}$** and **$\mathbf{VIOLENCE_{DIF}}$** are depicted in Figure 3; displayed are the values of for the 30-day lead periods for both the NYT and the SWB analyses, using all incidents of civil unrest. The range in **$\mathbf{PROTEST_{DIF}}$** is from a 47% decline to a 35% increase in the NYT data and from a 39% decline to a 3% increase in the SWB data. There is one clear outlier in the NYT data; without it the range for **$\mathbf{PROTEST_{DIF}}$** is from a 47% decline to a 1% increase. Only 1% of the episodes reveal an increase in the intensity of political protest in both samples; nearly 30% of the episodes reveal more than a 10% decline in both samples. The value of **$\mathbf{PROTEST_{DIF}}$** is '0' for 54% of the NYT episodes and 21% of the SWB episodes. A similar picture emerges with respect to **$\mathbf{VIOLENCE_{DIF}}$**. The range in **$\mathbf{VIOLENCE_{DIF}}$** is from a 41% decline to a 7% increase in the NYT data and from a 26% decline to a 6% increase in the SWB data. Only 2% of the episodes in the NYT sample show an increase in the intensity of political violence, compared to 3% in the SWB sample. Nearly 30% of the NYT episodes reveal more than a 10% decline, compared to only 7% in the SWB data. The value of **$\mathbf{VIOLENCE_{DIF}}$** is '0' for 54% of the NYT episodes and 56% of the SWB episodes.

Figure 3
The Distribution of $\mathbf{PROTEST_{DIF}}$ and $\mathbf{VIOLENCE_{DIF}}$ for the 30-day Lead Periods



Modeling Cross-episode Differences in Civil Unrest: An Exploratory Analysis

To enhance our understanding of the relationship between natural disasters and civil unrest in anticipation of a greater incidence of future disasters, it would be useful to know what accounts for the differences displayed in Figure 3. While it is premature to provide a refined and exhaustive analysis, some very basic factors can be examined that will provide insights that will guide future efforts. While this exploratory analysis is aided by the existence of two samples that makes replication possible, we are handicapped by several methodological problems. Two are most important. The first is that the countries that emerged from the random sample of natural disasters drawn here do not form a representative sample of countries. This makes it difficult to generate generalizable inferences about the importance of contextual and institutional factors (wealth, education, political system, etc.) because we are likely to be examining truncated distributions of these variables rather than representative distributions. There are only twenty-one nations represented in the NYT sample and twelve in the SWB sample; they are heavily skewed toward Asian nations, with a smattering of South American and African countries. The second problem is that the sample with the broadest cross-national reach rests on the weakest empirical base, as noted above.

These problems notwithstanding, the countries examined here are the ones that are most susceptible to the natural disasters associated with climate change and even an exploratory analysis, if properly conducted, can produce dividends. The next section discusses the types of factors that will be considered and briefly outlines their operationalization. Then the empirical analysis is introduced and the results are presented and discussed.

Conceptualizing and Operationalizing Factors that Affect Civil Unrest

We can look to four sets of factors are likely to generate differences in civil unrest, with a focus on political violence. Two of these deal with the impact of the disaster upon the country which it affected. The magnitude of the disaster has direct implications for people living in the locale affected; the disaster's impact on social and economic systems that provide for human welfare have broader societal effects. The other two sets of factors deal with the reactions to the adversities inflicted by the disaster. The societal context (wealth, education, social heterogeneity, etc.) can mediate the adversities that accompany disasters. A nation's institutions, particularly its political and legal system, are also important here. The design of these institutions can affect the manner in which societal problems are resolved because they embody values and norms that structure issue resolution and conflict management. These values and norms are expected to affect how problems that emerge in the wake of natural disasters are handled, which can affect levels of political violence.

To operationalize these factors we draw on data from both the CRED archive and the SID archive, which contains both original data and data assembled from a variety of other sources (UN, Penn Tables, World Bank, etc.). As Tables 2 and 6 document, the number of humans killed and affected by these disasters varies enormously. Thus, two magnitude variables were constructed using CRED data: $\mathbf{KILLED_{DIS}}$ and $\mathbf{AFFECTED_{DIS}}$. The effects of disasters on social and economic systems can be captured by examining changes in economic, agricultural and natural resource production after the occurrence of a disaster. To measure these changes country-year data from the SID archive on GDP, the monetary value of agricultural production, and the monetary value of natural resource production were used. A comparison between the value of these variables in the year before the disaster and the year after the disaster led to the construction of three change variables: $\mathbf{GDP_{CHNG}}$, $\mathbf{AG\_PROD_{CHNG}}$, and $\mathbf{RES\_PROD_{CHNG}}$. Three other variables were used to capture differences in the social contexts. One deals with wealth ($\mathbf{GDP_{PC}}$); a second pertains to the average educational attainment of the twenty-five and older population ($\mathbf{E\_ATTAIN_{25+}}$); the third is a measure of social equality ($\mathbf{EQUALITY_{SOC}}$) that is based on disparities in educational attainment across a nation's population.

Another three variables were used to capture the impact of differences in institutional designs. Two measures of a country's commitment to a law-based order were derived from SID research initiatives.[17] The first variable captures a country's long-term commitment to creating the type of infrastructure that is necessary for a law-based order to function and prosper ($\mathbf{INFRASTRUCTURE_{LEG}}$). It is based on an analysis of the temporal and spatial distribution of 45,613 legal periodicals (1771-2008) and 2,193 legal education programs (1088-2008). The second variable captures a country's commitment to equality before the law ($\mathbf{EQUALITY_{LEG}}$). It is based on an analysis of the constitutional commitments to equality for a set of social groups (women, ethnics, religious sects, etc.). The measure captures which groups are protected, the length of time they have been protected, the existence of "rollbacks" in protection, etc. The data for $\mathbf{EQUALITY_{LEG}}$ was derived from the Comparative Constitutions Project (http://www.comparativeconstitutionsproject.org/). The third variable ($\mathbf{DEMOCRACY_{DUR}}$) captures the length of time a country has been a democracy. It is derived from the work of Cheibub, Gandhi and Vreeland (netfiles.uiuc.edu/cheibub/www/DD_page.html). If a country is not a democracy, as defined by the authors, for a particular year it receives a score of '0' for that year.

Despite the fact that the eleven variables introduced above relate to independent matters, preliminary analyses revealed that, in the sample of countries examined here, the subsets of factors are highly intercorrelated. Thus, using them within a regression model produced highly unstable results. To address this problem factor analysis was used in conjunction with three subsets of variables to construct composite measures; preliminary analyses suggested that a logged version of $\mathbf{KILLED_{DIS}}$ ($\mathbf{LN\_KILLED_{DIS}}$) was the superior magnitude indicator for the disaster being examined. The three variables relating to changes in the output of societal subsystems that provide for human welfare ($\mathbf{GDP_{CHNG}}$, $\mathbf{AG\_PROD_{CHNG}}$, and $\mathbf{RES\_PROD_{CHNG}}$) were joined in a measure that captures the disaster's material impact. It is labeled **MATL_IMPACT** and it is scaled such that larger values indicate a larger adverse impact on societal well-being. The three variables that capture

[17] Descriptions of these variables are contained in a document entitled "Measuring Cross-national Differences in Law-based Orders," which is available, upon request, from the Cline Center for Democracy.

differences in social contexts (**GDP_{PC}**, **E_ATTAIN_{25+}** and **$EQUALITY_{SOC}$**) were joined in a composite that measures social empowerment: **$EMPOWER_{SOC}$**. Wealthier and more educated societies, especially those with more open social structures, are likely to have a greater proportion of citizens with the skill sets, resources and social capital to handle the adversities precipitated by natural disasters in a more civil manner. This, in turn, should affect levels of political violence. Finally, the three institutional variables described above (**$INFRASTRUCTURE_{LEG}$**, **$EQUALITY_{LEG}$** and **$DEMOCRACY_{DUR}$**) were merged into a variable that gauges institutional empowerment: **$EMPOWER_{INST}$**. Societies in which rulers are held electorally accountable – and are limited in their discretion by legal rules that provide for at least formal equality before the law – are likely to develop routines for resolving issues and conflicts in a non-violent manner.

Hypotheses, Statistical Analyses and Empirical Findings

Based on the above discussion the statistical analyses will test four null hypotheses:

H_1: The magnitude of the natural disaster, gauged by **LN_KILLED_{DIS}**, is not manifested in *higher* levels of political violence after a natural disaster.

H_2: The material impact of the disaster, gauged by **MATL_IMPACT**, is not manifested in *higher* levels of political violence after a natural disaster.

H_3: The level of social empowerment, gauged by **$EMPOWER_{SOC}$**, is not manifested in *lower* levels of political violence after a natural disaster.

H_4: The level of institutional empowerment, gauged by **$EMPOWER_{INST}$**, is not manifested in *lower* levels of political violence after a natural disaster.

We test these hypotheses using the NYT and SWB episodes with 30-day leads because they provide for more stable estimates of **$VIOLENCE_{DIF}$** at little cost (see discussion accompanying Table 7). We did not test these hypotheses using **$PROTEST_{DIF}$** because the implications of **H_3** and **H_4** for political protests are not clear. More empowered citizens are more likely to engage in political protests and freedom of expression is a lynchpin of democracy that is often protected in the legal system. In order to generate meaningful empirical tests of these hypotheses a multivariate analysis is required. In order to assess contextual and institutional effects it is important to control for the magnitude of the disaster and its impact on societal well-being. Because the data being examined is generated by a handful of very different countries we use a fixed-effects regression model to conduct the multivariate analysis. N-1 dummy variables capturing the countries in the sample were used to control for country-specific effects. Moreover, despite the variable reduction techniques used, the high degree of intercorrelation between **$EMPOWER_{SOC}$** and **$EMPOWER_{INST}$** did not allow us to test **H_3** and **H_4** in the same regression model. Thus, for both the NYT sample and the SWB sample, we conducted two analyses of **$VIOLENCE_{DIF}$**; Model 1 examined the impact of **$EMPOWER_{SOC}$** while Model 2 examined the impact of **$EMPOWER_{INST}$**. We conducted the analyses using all political attacks in a country as well as for just those political attacks that occurred with 500 miles of the disaster, excluding events involving social groups.[18]

The empirical results are reported in Table 10. As is evident, the analyses produced mixed results. Examining all political attacks in the country affected by the disaster (the top set of entries in Table 10), $\mathbf{H_1}$ can be rejected for the SWB sample in both models; $\mathbf{LN\_KILLED_{DIS}}$ is associated with higher levels of political violence. But $\mathbf{H_1}$ cannot be rejected for the NYT sample in either model. $\mathbf{H_2}$ can be rejected for the NYT sample in both Model 1 and Model 2; **MATL_IMPACT** is correlated with higher levels of violence. But $\mathbf{H_2}$ cannot be rejected for the SWB sample in either model. More perplexing is the fact that the test of $\mathbf{H_3}$ produced contradictory effects in Model 1 as did the test of $\mathbf{H_4}$ in Model 2. $\mathbf{EMPOWER_{SOC}}$ had a negative effect in the SWB analysis, as expected; but it had a positive effect in the NYT analysis, suggesting that wealthier and more educated countries experience more violence than others in the wake of natural disasters. Similarly, $\mathbf{EMPOWER_{INST}}$ had a negative effect in the SWB analysis, as expected. But $\mathbf{EMPOWER_{INST}}$ had a positive effect in the NYT analysis, suggesting that democracies and law-based societies experience more violence than others in the wake of natural disasters.

The second set of entries in Table 10 report the results using only political attacks that occurred within 500 miles of the disaster. All of the coefficients are similar, with the exception that **MATL_IMPACT** drops out of the NYT analysis. Thus, while these analyses suggest that contextual and institutional factors play a role in mediating civil violence in the wake of disasters, the direction of that effect is unclear. Given theoretical expectations and the relative richness of the SWB data, it seems likely that empowering contexts and institutions lead to reduced levels of political violence. But it remains for future research to resolve that issue. What is clear from our analysis, however, is that contexts and institutions have less of an impact on violence involving social groups: if the social group attacks are included in the SWB analysis, neither $\mathbf{EMPOWER_{SOC}}$ nor $\mathbf{EMPOWER_{INST}}$ have a statistically significant effect.

[18] It was important to eliminate instability events rooted in social group animosities from the second analysis because they had the potential to contaminate the analysis. Not only are these instability events less likely to be affected by disasters, they are also less likely to be affected by contextual and institutional factors. Thus, given the restricted empirical base when limiting the analysis to events proximate to the disaster, it was important to also eliminate attacks involving social groups.

Table 10

Regression Results for $VIOLENCE_{DIF}$

Variable	**Fixed Effects Models for All Events Within a Country**			
	Model 1		**Model 2**	
	NYT Data	**SWB Data**	**NYT Data**	**SWB Data**
Intercept	-0.01	-0.06	0.48	-0.12
LN_KILLED_{DIS}	N.S	.01**	N.S	.01**
		(.003)		(.003)
MATL_IMPACT	.01**	N.S	.01**	N.S
	(.005)		(.005)	
$EMPOWER_{SOC}$	0.04***	-0.03*	---	---
	(.006)	(.01)		
$EMPOWER_{INST}$	---	---	.19***	-0.08*
			(.02)	(.04)
Adj R^2	0.4	0.5	0.46	0.5
N	286	154	287	154
n	21	12	21	12

Variable	**Fixed Effects Models Using Only Events Occurring Within 500 miles of the Disaster -- Excluding Events Involving Social Groups**			
	Model 1		**Model 2**	
	NYT Data	**SWB Data**	**NYT Data**	**SWB Data**
Intercept	-0.02	-0.04	0.13	-0.03
LN_KILLED_{DIS}	N.S	.01*	N.S	.01***
		(.002)		(.002)
MATL_IMPACT	N.S	N.S	N.S	N.S
$EMPOWER_{SOC}$	0.03***	-.03*	---	-.08*
	(.006)			
$EMPOWER_{INST}$	---	---	.08***	N.S
			(.02)	
Adj R^2	0.58	0.63	0.57	0.63
N	286	154	287	154
n	21	12	21	12

Summary and Conclusions

The restructured analyses conducted here present clear empirical evidence that, compared to benchmark levels, civil unrest occurs at lower rates in the wake of natural disasters. Indeed, less than 1% of the episodes examined here evidence levels of unrest that exceed benchmark levels. Because of the stronger empirical base of the SWB analyses, the estimates it produced are the most conservative to use in characterizing these reductions. For the nation as a whole these estimates suggest that in the wake of natural disasters the intensity of political protests is about 10% below benchmark levels; the intensity of political violence is about 8% below benchmark levels. Larger drop-offs (27%) in protest activity are found in locales proximate to the disaster. But the level of political violence in these locales is only about 4% below benchmark levels. It is also noteworthy that political activities involving social groups are less affected by natural disasters. In the 30-day lead periods political protest is only 4% below benchmark levels and political violence is only about 5% below them. Also, the political activities of social groups revert to benchmark levels fairly quickly in the period after the disaster.

These estimates are not as methodologically pure as those that would have been generated by a properly executed quasi-experimental design. But the unequivocal nature of the findings (see Figure 3) suggests that the findings generated here are well-grounded. Future research can make better use of quasi-experimental designs in examining the impact of natural disasters on civil unrest – and those designs can benefit from the insights provided here. It seems clear that, in the selection of episodes, more attention needs to be paid to serial disasters. Although these cannot be eliminated from any sample that aims to produce generalizable results, they should be explicitly identified from the outset and be integrated into a design that includes both isolated and serial disasters. It also is important to include some type of stratified sampling strategy that maximizes the variance in the type of countries selected and captures information on the role of organized social groups. Without examining a wider range of societies in a more in-depth manner it will be impossible to clarify some of the contradictory results involving the role of contextual and institutional factors. It is also clear that the role of these factors will not be clarified without taking into account social group conflict. Finally, before designing future quasi-experimental designs more empirical work needs to be done on defining optimal post-periods. Better information on optimal post-periods is important for conserving scarce research resources and for minimizing the problem of serial disasters – unnecessarily long post-periods can lead to excessive grouping of disasters (see Figure 2). Time-series techniques, combined with the SWB data collected here, can provide the basis for helping define these optimal periods.

At a substantive level, the results of this analysis cast additional doubt on the eco-scarcity theory and the broader relationship between abrupt disasters associated with climate change and civil unrest. Much empirical work remains to be done in this area, and research needs to be extended to more incremental developments related to climate change (desertification, sea-level rise, extended droughts and famines, etc.). But those who would assert that abrupt disasters are likely to spawn long-term destabilizing effects shoulder a heavy burden in making their case. If such instability occurs it certainly does not appear to be rooted in activities that unfold in the immediate aftermath of the disasters. Thus, proponents must clarify how the effects of natural disasters translate into paralyzing levels of civil unrest. Are dangerous levels of instability contingent upon other developments? Do they affect only certain types of societies? Are they rooted in the organized activities of social groups? This conceptual work must be accompanied by creative and well-designed research that provides empirical support for more elaborate linkages between disasters and unrest.

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