NEWS DROUGHTS, NEWS FLOODS, AND U.S. DISASTER RELIEF*

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This paper studies the influence of mass media on U.S. government response to approximately 5,000 natural disasters occurring 1968–2002. These disasters took around 63,000 lives and affected 125 million people, per year. We show that U.S. relief depends on whether the disaster occurs at the same time as other newsworthy events, such as the Olympic Games, which are obviously unrelated to need. We argue that the only plausible explanation of this is that relief decisions are driven by news coverage of disasters, and that the other newsworthy material crowds out this news coverage.

I. Introduction

In May 1999, a storm struck India, reportedly killing 278 people and affecting 40,000. On the same day, a 15-year-old sophomore shot and wounded six classmates at the Heritage High School in suburban Atlanta. The two events competed for news time. Since this was just a month after the Columbine high school tragedy, the events at the Heritage High School were extensively covered by the U.S. television network news, while the Indian storm was not covered. About one year earlier, a storm of similar size struck

*We thank David Balan, Stefano DellaVigna, James Hamilton, Ethan Kaplan, Torsten Persson, Doug van Belle, Fabrizio Zilibotti, Eric Zitzewitz, two editors, and three anonymous referees for useful comments. We also thank Wes Mossburg at OFDA, and Marshall Breeding at the Vanderbilt Television News Archive for providing data. Finally, we are grateful to Christina Lönnblad for editorial assistance and Mirco Tonin for research assistance. Views expressed here are those of the authors and do not necessarity reflect the official opinion of the Ministry of Finance, Sweden.

India (killing 250 and affecting 40,000 people). At that time, there was less other breaking news around, and the storm was covered by the television network news. Two days later, the U.S. Ambassador in India, Richard F. Celeste, declared this storm a disaster, and its victims consequently received U.S. relief. He did not issue a disaster declaration for the first mentioned storm and its victims received no U.S. government relief.

This paper studies mass media's influence on the U.S. government's response to natural disasters abroad. Although it is widely believed that news coverage influences government policy, little conclusive evidence has been produced to this effect. The problem is that news coverage and policy will be correlated even if news has no effect on policy, since news coverage depends on unobserved issue salience and political agendas, both of which directly affect policy. We attack these problems by using the availability of other newsworthy material as an instrument for whether the disaster was in the news.[1] In other words, we are asking whether a natural disaster is less likely to receive relief, because news about this disaster was crowded out by, for example, the shootings at the Heritage High School, the Olympics, or the O.J. Simpson trial. Equally important, we ask whether moderately sized disasters are more likely to receive relief simply because they appear on the evening news when there are few competing news stories.

We study approximately 5,000 natural disasters occurring between 1968 and 2002. These disasters were identified and documented by the Centre for Research on the Epidemiology of Disasters (CRED). On average, 150 natural disasters occurred each year during the period taking around 63,000 lives and affecting 125 million people. According to our estimates, about 20 percent received relief from USAID's Office of Foreign Disaster Assistance (OFDA), and about 10 percent were covered in the evening news broadcasts of the major U.S. networks (ABC, CBS, NBC, and CNN). This was found using data provided by the Vanderbilt News Archives, and relief data from OFDA.

There is no reason to believe that the severity of natural disasters in foreign countries is related to the availability of other news. Still, we find that U.S. policy makers are less likely to declare disasters during the Olympics, and in general when other newsworthy stories are in abundance. We will show that these correlations remain after accounting for the number of people killed and affected, as well as other relevant factors.

[1] We will use the term "newsworthy" to mean the audience appeal of the news as perceived by the media.

We argue that the reason is that news affects relief decisions. Under this assumption, the impact of news on relief can be estimated using Instrumental Variables (IV). The estimated effect of news on disaster relief is large and significant.

News biases relief in favor of certain disaster types and regions: for every person killed in a volcano disaster, 40,000 people must die in a drought to reach the same probability of media coverage. Similarly, it requires 40 times as many killed in an African disaster to achieve the same expected media coverage as for a disaster in Eastern Europe of similar type and magnitude.

The result that news stories influence policy is closely related to a few recent studies investigating the effects of media penetration. Media penetration has been found to influence redistributive spending [Strömberg 1999, 2004], government accountability [Besley and Burgess 2002], voter turnout [Gentzkow 2006 and Strömberg 1999, 2004], and voting patterns [Della Vigna and Kaplan 2005]. The question we ask is different: given media penetration, how large is the effect of publishing a news story?

As already mentioned, this question is hard to answer empirically since more severe disasters are both more likely to be in the news and to receive relief, so news and relief will be correlated even if news has no effect.[2] Similarly, there will be a spurious correlation if policy makers alert the press of disasters that they want to provide relief.[3] Our IV-strategy avoids both of these problems.

While we find news coverage to affect disaster relief, we do not uncover the exact mechanism through which this happens. First, information about the disaster can spur citizens' lobbying of political representatives to provide relief (Public Action). Second, disaster relief typically delivers favorable publicity,[4] and therefore politicians may act swiftly to disasters reported in

[2] Drury, Olson and Van Belle [2005] make the point that salience drives disaster relief, and that media coverage is a measure of salience. Their paper is closely related to ours in studying the empirical allocation of U.S. foreign disaster assistance to 1,900 disasters 1964–1995.

[3] For example, one of the most cited cases of media influence on foreign policy is the 1992 intervention in Somalia. However, Mermin [1997] writes, "before television made the decision to cover the crisis in Somalia, influential politicians had spoken out on it, indicating to journalists [...] that Somalia constituted a significant concern of American foreign policy and warranted consideration for space in the news." The working paper version of this paper contains a simple model of engogenous disaster news provision where policy makers leak news.

[4] Adamson et al [1998], for instance, find that 59 percent of the U.S. population

the media (Publicity Management). Third, a news publication is a signal to the policy maker that the disaster is highly salient to the American public (Salience Cue) and thus, deserves relief. Finally, the news broadcast in itself could increase the importance people attach to the disasters (Agenda Setting, McCombs and Shaw [1972]). These mechanisms are modelled in detail in the working paper version of this paper.

The paper is organized as follows. Section II gives a background and presents our data, while section III discusses the results. Section IV discusses what continents and disasters types are more likely to receive relief because of the media effects, and concludes.

II. Background and Data

This section presents the data on disasters, disaster relief and television news. Table I provides summary statistics of our data.

Table I about here

II.A. *Disasters*

We use data on natural disasters from the Emergency Disaster Database (EM-DAT) as provided by the Centre for Research on the Epidemiology of Disasters (CRED). In this database, an event qualifies as a disaster if at least one of the following criteria are fulfilled: 10 or more people are reported killed; 100 or more people are reported affected, injured and/or homeless; there has been a declaration of a state of emergency; or there has been a call for international assistance. We measure the severity of a disaster by two variables: the number of killed, denoted *killed*, and the total number of affected, denoted *affected*. The variable *killed* includes persons confirmed dead and persons missing and presumed dead. The variable *affected* is the

support U.S. foreign economic assistance and that 30 percent believe that foreign disaster relief should be given the highest priority in U.S. foreign aid policy. The average priority given to foreign disaster relief was 7.4 on a 0–10 scale.

sum of "injured",[5] "homeless"[6] and "affected"[7] as reported in EM-DAT. The data provided in EM-DAT is based on official figures when available.[8]

We analyze a sub sample of this disaster data. The attention is restricted to natural disasters, since their incidence and severity is arguably an exogenous process. Thus, we do not consider complex emergencies[9] and technological disasters (e.g. airplane crashes). We also drop the 40 observations on disasters that occurred in 1968 prior to August 5, the date when the Vanderbilt Television News Archives started collecting data, and 408 observations for which we only have information about the year of the event. Finally, since our specification allows for country-fixed effects, we only include countries that have had more than one disaster and have received OFDA relief at least once. On this account, we drop 1,104 observations.

This leaves us with a total of 5,212 natural disasters, occurring in 143 countries 1968–2002. On average, 150 natural disasters occurred each year taking around 63,000 lives and affecting 125 million people.[10] Each disaster took on average 590 casualties and affected the lives of 1.2 million people (see Table II).

Table II about here

The majority of natural disasters were caused by floods (32 percent), storms (23 percent) or epidemics (14 percent). Droughts took most casualties and affected most people per incident. Fires and landslides caused fewest casualties per incident, while infestations had the smallest number of affected.

[5] People suffering from physical injuries, trauma or an illness requiring medical treatment as a direct result of a disaster.

[6] People needing immediate assistance in the form of shelter.

[7] People requiring immediate assistance during a period of emergency, i.e. requiring basic survival needs such as food, water, shelter, sanitation and immediate medical assistance.

[8] CRED's source ranking is as follows: (1) United Nations, (2) national government, (3) U.S. government, (4) International Federation of Red Cross and Red Crescent Societies, (5) World Bank, (6) Reinsurance companies, (7) AFP, and (8) others.

[9] Manmade disasters (complex disasters) raise other non-trivial issues (type of war, safety for aid workers etc.) which are beyond the scope of this paper.

[10] Some disasters have missing values for *killed* and *affected*. Therefore, the values on killed and affected that we report refer to the sub-sample where data exists. We treat 0-entries in *killed* and *affected* as missing values, after correspondence with CRED.

There is a slight downward trend in the severity of disasters as measured by the mean number of *killed* and *affected*. This may partly be driven by improvements in data collection procedures towards the end of the sample period, which have increased the availability of data on less severe disasters.

II.B. *U.S. Emergency Relief and OFDA*

The United States is the largest provider of emergency and distress relief by far, accounting for around a third of the total emergency aid provided by OECD countries to developing countries. In 2002, for instance, OECD countries provided \$3,869 million in emergency and distress relief, of which the United States accounted for \$1,382 million or 35.7 percent [OECD 2004].

We will study disaster responses by the Office of Foreign Disaster Assistance (OFDA) over the period 1968–2002. The OFDA is an office within the U.S. Agency for International Development (USAID). It has been given flexible authority permitting it to respond quickly to the needs of disaster victims. Hence, it is generally the first U.S. agency to be on the scene of a disaster and also influences other relief efforts. For example, the largest U.S. disaster relief program, under the Office of Food for Peace, requires an OFDA disaster declaration to trigger some of its disaster assistance. Although the OFDA only contributes around 20 percent of the total disaster relief provided by the United States, each dollar spent by the OFDA on a disaster was, on average, matched by four dollars from other U.S. agencies, in the fiscal year of 2002 [USAID 2002].

Our dependent variable, *relief*, indicates whether the OFDA provided relief to the disaster. Over the period 1968–2002, the OFDA responded to 19 percent of the disasters in the sample or, on average, 28 natural disasters per year.

A disaster receives OFDA relief if and only if it is declared. A disaster can be declared by the U.S. Ambassador or the Chief of the U.S. Mission in the affected country or, if a U.S. Mission is not located in the country, by the appropriate U.S. Assistant Secretary of State. A disaster declaration allows the Chief of Mission to allocate up to \$50,000 (until 2002, \$25,000) to host country relief efforts. Subsequently, USAID and the local mission jointly determine whether additional assistance for the disaster is warranted.

Disaster relief is intended to address *immediate* life threatening concerns [USAID 2005]. The disaster should meet three criteria: (1) it is of a magnitude with which the affected community cannot cope, (2) recognized representatives of the affected population desire the assistance, and (3) it is in the U.S. Government's interest to respond.

The share of disasters responded to by the OFDA differs substantially across disaster types, see Table II. Infestations, droughts, and volcanoes receive the highest response rates, while cold waves and landslides receive the lowest. As expected, the OFDA responds to disasters that are more severe than the average, with an average of 1,920 killed and 2.6 million affected.

II.C. *News Coverage*

Our explanatory variable of interest is U.S. news coverage of disasters. In this paper, we restrict the attention to television news. Data on news coverage is taken from the Vanderbilt Television News Archive (VTNA). VTNA contains more than 30,000 individual network evening news broadcasts and 700,000 news stories from the major U.S. national broadcast networks (ABC, CBS, NBC, and CNN) since 1968.[11] News coverage is captured by an indicator variable, *news*, for whether a disaster was covered in a news broadcast within a certain time span/window. In our benchmark specification, we use a window of -2 to +40 days relative to the time of the event.[12] Within this interval, a news segment is coded as covering a disaster if it contains certain keywords (country and type of disaster, e.g. "earthquake"). For example, according to CRED, an earthquake occurred in Afghanistan on March 26, 2002. We code a disaster as being covered in the news if the headline or abstract in VTNA contains the words "Afghanistan" and "Earthquake",[13] and if the story was aired March 24–May 7, 2002 (-2,+40 days).

Network news covered around 10 percent of the disasters in our sample. Figure I plots the timing of the first news story on a disaster relative to the date of the disaster. The first news story is typically aired during the first days following the disaster. The likelihood of covering the disaster then falls rapidly until around 20 days after the disaster and then remains relatively constant.

Figure I about here

[11] CNN since October 1995.

[12] The window is extended to -2 days since, for example, storms, floods and volcanic eruptions are sometimes predictable and reported on in advance.

[13] Generally more advanced Boolean searches are conducted; see Appendix 1.

II.D. *Crowding Out of Disaster News*

If two equally newsworthy disasters occur, we would expect the disaster occurring when there is a great deal of other breaking news around would have a lower chance of being covered by the news than the disaster occurring when there is little other news around. This crowding out is probably particularly strong for television news broadcasts that are usually of a fixed length (half an hour for ABC, CBS and NBC, and one hour for CNN).

The challenge is to construct meaningful and operational measures of the availability of newsworthy material. Here, we implement two such measures. The first measure only depends on the dates of the Olympic Games. We use the Olympics since it is a large media event that is not directly related to politics, and that does not occur every year (not colinear with seasonal effects). Among other potentially useful media events, elections and domestic U.S. disasters are clearly not suitable since they directly affect politics. The World Series, the Academy Awards and the Super Bowl generate far fewer stories in the network news than the Olympics and occur around the same time every year.[14] World Series games still generate considerably more stories than the Academy Awards or the Super Bowl and we will discuss crowding out by these games below.

In our sample period, there are 18 Olympic Games, ranging from the 1968 summer Olympics in Mexico City to the 2002 winter games in Salt Lake City. The Olympics are well covered by the network news. In our sample period, 2,443 news stories have "Olympic" in the headline. These stories are usually aired around the dates of the Olympic Games. Figure II shows the daily number of network news stories covering the Olympics in 1992. The thick vertical lines mark the beginning and the end of the Olympic Games in Albertville February 8–23 and in Barcelona, July 25–August 9. As expected, news stories about the Olympics are mainly aired between these dates. On an average day during an Olympic Game, the network news broadcasts 3.6 news stories about the Olympics.

Figure II about here

[14] The World Series generates only 1/3 as many network news stories as the Olympics on the day of, or after, the event, while both the Academy Awards and the Super Bowl generate less than 1/12 as many stories.

Below we will investigate whether these stories crowd out news about natural disasters and whether this affects relief decisions. Natural disasters appear in the news not just on the day of the disaster, but also in the days following the disaster, see Figure I. Consequently, we will use a measure of the number of Olympic Game days just following the disaster. Specifically, we will use the weighted average of days, where the weights are the empirical distribution of disaster news stories per day following the disaster, given by Figure I. We will call this weighted average variable *Olympics*. Roughly 9 percent of the disasters in our sample (448 to be exact) occur when the variable *Olympics* is greater than zero. We construct a similarly weighted average of World Series baseball game days, and call this variable *World Series*.

Our second measure of the availability of newsworthy material is the median (across broadcasts in a day) number of minutes a news broadcast devotes to the top three news segments in a day (*daily news pressure*),[15] see Appendix 2 for a detailed example. When a large media event takes place, then that story is usually placed as one of the first news stories in a broadcast and more time is devoted to the story. For instance, on October 3, 1995, a jury found O.J. Simpson not guilty of two counts of murder. That night, the ABC, CBS and NBC devoted all of their first three news segments to that story. The top three news segments comprised an average of 16 minutes and 30 seconds — the highest value of that year. The Simpson verdict effectively crowded out other news. NBC only covered this story, while CBS also reported on one other story, and ABC included 4 other stories. This suggests that the amount of time devoted to the first three news segments is a good indicator of how much newsworthy material is available on a given day. We use the median value across news broadcasts in a day rather than the mean, to reduce the effect of measurement error in the reported time for news segments.

One worry is that *daily news pressure* is endogenous to stories about disasters, since the airing of a disaster story will affect the amount of time devoted to the top three stories. To diminish this problem, we will use the average of *daily news pressure* over the 40 days following the disaster in most specifications. This variable will be called *news pressure*. In the robustness section, we also report results when computing the average similarly weighted

[15] This instrument is related to that used in Erfle and McMillan [1990], the weekly average percentage of total news time devoted to the two leading non-energy news topics.

as for the *Olympics* variable,[16] *weighted news pressure,* and the unweighted 20-day average.

Figure III about here

Figure III plots *daily news pressure* from 1968 to 2003. We have marked the date with the highest value of *daily news pressure* for each year. The news stories corresponding to these dates are listed in Table III below. The table also lists the main stories corresponding to the second highest yearly values of *daily news pressure*. The overall highest value (90 minutes) was recorded for September 11, 2001. However, we coded September 11 as missing because the exceptional event changed the 30-minute format of the news broadcast. Note also that there is a slight upward trend in *daily news pressure*, which could reflect a general upward trend in the availability of breaking news stories or changes in the news technology. We will include year dummy variables in all regressions to pick up this type of variation. There is also seasonal variation in *daily news pressure*. There is an early summer news drought with exceptionally low *daily news pressure* in May and June, and news floods in the fall with higher *daily news pressure* in September and November. For this reason, we will include month-fixed effects in all regressions.

Table III about here

Is *daily news pressure* a reasonable measure for the occurrence of newsworthy events? Figure IV plots *daily news pressure* during 405 days (15 March 2001–23 April 2002). The horizontal flat line depicts the mean for the 1968–2002 period. The figure also displays the events occurring during the peaks of *daily news pressure*. Apparently, our measure captures the major events during these 405 days quite well, starting with the U.S.-China Spy plane incident (1 April 2001–11 April 2001), reaching its maximum around the September 11, 2001 terrorist attacks and ending with the Siege at the Church of Nativity in Bethlehem (2 April 2002–10 May 2002). Plots for other time periods do an equally good job in capturing major events. Also included

[16] That is, weighted by the empirical distribution of disaster news stories per day following the disaster.

in the graph is our 40-day average, *news pressure*. This measure puts an equal weight on all days during the 40 days. We also plot our *weighted news pressure* variable, which is naturally characterized by more pronounced peaks and troughs. The variable *daily news pressure* is available 1968–2003 for download,[17] since it may be useful in identifying effects of media inattention on other outcome variables.

Figure IV about here

III. Results

This sections contains our empirical results. We first discuss the empirical specification. Then, we analyze in detail the empirical determinants of news and relief. Finally, we discuss the robustness of the results.

III.A. *Specification*

Our econometric specification will be of the following form. For disaster i, the latent variable $relief_i^*$ (reliefworthiness) describes the benefits of providing relief from the decision maker's perspective,

$$relief_i^* = \alpha_1 news_i + \alpha' \theta_i + \varepsilon_i, \tag{1}$$

where $news_i = 1$ $[news_i = 0]$ indicates that the disaster was covered [was not covered] in the news. The vector θ_i contains disaster specific variables, such as *killed* and *affected,* and fixed effects for disaster type, country, year, etc. Relief is provided if $relief_i^*$ is above a threshold value,

$$relief_i = \begin{cases} 1 & \text{if } relief_i^* > 0 \\ 0 & \text{if } relief_i^* \leq 0 \end{cases}, \tag{2}$$

where $relief_i = 1$ $[relief_i = 0]$ is the event that OFDA provided [did not provide] disaster relief to disaster i. We will test the hypothesis that news coverage has a positive effect on the relief decision, $\alpha_1 > 0$.

Similarly, the latent variable $news_i^*$ (newsworthiness) describes the benefits of covering disaster i from the TV network's perspective,

$$news_i^* = \beta_1 news\ pressure_i + \beta_2 Olympics_i + \beta' \theta_i + \omega_i. \tag{3}$$

[17] See http://www.iies.su.se/~stromber/

This latent variable determines the news decision according to

$$news_i = \begin{cases} 1 & \text{if } news_i^* > 0 \\ 0 & \text{if } news_i^* \leq 0 \end{cases} . \tag{4}$$

Our hypothesis here is that disasters are less likely to be covered when there is a great deal of other breaking news available, as measured by *news pressure* and *Olympics*, that is $\beta_1 < 0$ and $\beta_2 < 0$.

The basic econometric problem is that newsworthiness and reliefworthiness are both increasing in the salience of the disaster to the American public. There are many facets of this salience that we cannot observe, for example, how severe the situation is for those affected (but not killed). This unobserved severity affects both news coverage and the provision of relief. As mentioned earlier, $news$ and $relief$ may also be correlated because both are driven by unobserved political agendas. Consequently, there is little hope of identifying the causal effect of $news$ on $relief$ from a regression of the latter on the former.

To determine whether *news* has a causal effect on *relief*, we instead use the instrumental variables *news pressure* and *Olympics*. Assuming a linear probability model and that *news pressure* and *Olympics* are uncorrelated with the unobserved reliefworthiness, ε, and unobserved newsworthiness, ω, conditional on the variables in θ, the model is identified and the parameters may be consistently estimated using Two Stage Least Squares (TSLS). In the robustness section, we also report the result from estimating the relief equation as probit and the news equation as a linear-probability model, as well as estimating the above system of equations as a bivariate probit. We focus on TSLS, since it consistently estimates the effect of *news* on *relief* under weaker assumptions.

It is important to include certain controls to satisfy the key identifying assumption that our instruments are uncorrelated with the severity of the disaster. There are seasonal patterns in the severity of disasters, such as storms and floods, and also seasonal patterns in *Olympics* and *news pressure* (with the typical summer news droughts). For this reason, we include month dummy variables. There is also a yearly trend in *news pressure* and *relief*, so we include year dummy-variables. Controlling for year and month effects, we see no a priori reason why the severity of natural disasters in foreign countries should be correlated with our measures of the availability of newsworthy material. Additional concerns regarding identification will be addressed in the robustness section.

III.B. *How the availability of other news impacts disaster news and relief*

We first examine how the pressure for network news time affects news coverage of, and relief to, disasters; see Table IV. All regressions include country, year, disaster type, month and year fixed effects, and report heteroscedastic robust standard errors.

Table IV about here

Higher *news pressure* significantly reduces both the probability that the networks cover a disaster, and the probability that the disaster receives relief. This can be seen in the baseline specifications of columns (1) and (5) The implied effects are that 2.4 extra minutes spent on the first three news segments (two standard deviations) decrease the probability that a disaster is covered in the news by 4 percentage points, and the probability that the disaster receives relief by 3 percentage points. Recall that around 10 percent of all disasters are covered in the news and that 20 percent receive relief, so the effects are sizeable. Furthermore, *Olympics* is significantly negatively correlated with *news* and *relief.* The estimated coefficients imply that a disaster occurring during the Olympics is 5 percent less likely to be in the news and 6 percent less likely to receive relief, on average.[18] Note that the estimated coefficients on *World Series* and *Olympics* are very similar both in the news and the relief equation. However, the crowding out due to coverage of World Series games is not statistically significant. Since news coverage of these events is only a fraction of that of the Olympics, this is not surprising. To avoid problems with weak instruments, we only use *Olympics* and *news pressure*; see columns (2) and (6).

Controlling for the severity of the disaster does not significantly change the estimated coefficients on *news pressure* and *Olympics.* Columns (3) and (7) control for the log number of killed and affected. This primarily reduces the precision of the estimates since the sample size is reduced by 44 percent. In columns (4) and (8), we impute the missing values to the average for each type of disaster. Since we also include fixed effects for the interaction between missing data and the type of disaster,[19] the value at which killed

[18] To compute the average effect, the coefficients are multiplied by 0.5, which is the average value of *Olympics* for days during the Olympic Games.

[19] Indicator variables for: Earthquake and data on killed exists, Earthquake and data on killed missing, Earthquake and data on affected exists, Earthquake and data on affected missing, Volcano and data on killed exists, etc.

and affected are imputed is of no importance for the estimated coefficients on *news pressure* and *Olympics*.

Disaster relief is equally responsive to the number of killed and affected, while news coverage responds much more to the number of killed. The estimates imply that as the number of killed increases by a factor of ten, the probability of receiving relief and news coverage both increase by about ten percentage points. When the number of affected increases tenfold, then the chance of receiving relief increases by around ten percent, but the probability of being covered in the news increases by only three percent.

Note that an endogeneity problem may arise when including the number of killed and affected in the regression. If disaster relief is effective, in the sense of reducing the number of killed and affected, then these variables are endogenous to relief. Further, sample selection problems are likely to arise. Relief work often involves data collection, and USAID is one of the main data providers. So data availability may depend on relief having been provided.

As we add more controls the residual variance is reduced – R^2 is almost doubled. Yet, the estimated coefficients on *news pressure* and *Olympics* are hardly affected. This is because these variables are uncorrelated with the controls, in particular with *killed* and *affected.* A regression of *news pressure* and *Olympics,* respectively, on (the log of) *killed* and *affected* confirms that there is no significant relationship between these two sets of variables, controlling for year, month, country and disaster type fixed-effects, see Table V.

Table V about here

We also explore the existence of "disaster fatigue" in the media. If a disaster occurs shortly after another disaster, then individuals and the media may become bored, pay less attention, and thus generate less aid. In contrast, we find that a disaster is significantly (and around 7 percent) more likely to be covered if the media covered a disaster of the same type, and on a different continent, no more than three days earlier. However, we do not use this as an instrument for *news* in affecting relief. The recent occurrence of another disaster could directly affect the relief decision, and the joint coverage of close disasters could be an indication of high disaster salience at this point in time.

III.C. *How news affects disaster relief*

We now examine what the above estimates imply regarding the effect of news on relief. There are strong reasons to believe that the effect of news is heterogeneous across disasters. For some disasters, news coverage has little effect. For example, the 2004 Indian Ocean earthquake was certain to receive relief, irrespective of news coverage. Many other disasters were certain not to receive relief, and news coverage contributes little to change this.

Figure V about here

It is likely that the effect of news on relief is greater for disasters that are marginal in the news decision. The reason is that these disasters are also more likely to be marginal in the relief decision, in the sense of receiving relief if and only if they receive news coverage. Figure V illustrates why this is reasonable. The x-axis contains the predicted probability of a disaster being in the news, based on the number of killed and affected, disaster type, year, month and country. The solid diamonds show the share of disasters in each decile that are covered in the news, while the crosses show the share of disasters receiving relief. For example, of the disasters that have predicted probabilities of being in the news above 90 percent, 80 percent received OFDA relief. In contrast, only 10 percent of the disasters received relief when their predicted probability of being in the news was below 10 percent. The figure shows that *news* and *relief* are driven in a similar way by observables. Thus, some disasters are highly likely both to be in the news and to receive relief. Others are very unlikely both to be in the news and to receive relief. Those close to 50 percent in the news decision are also close to 50 percent in the relief decision.

If there are heterogeneous effects of news on relief, then consistent OLS measures the average effect of news on relief across all disasters, while TSLS estimates the average effect in the subgroup of disasters that are marginally newsworthy in the sense of being in the news if and only if there is little other news around (see, for example, Björklund and Moffit [1987], Heckman and Robb [1985], and Imbens and Angrist [1994]).[20] Since the effect of news on relief is presumably greater for disasters that are marginal in the news

[20] The following simple example illustrates the point. Suppose that a single variable, p, measures the pressure for news space and can only take two values: 0 or $\overline{p}$. We estimate

decision, the TSLS estimates are likely to be larger than those of consistent OLS.

Table VI contains the results from regressions analyzing the relation between *relief* and *news*. The first three columns show the results of OLS regressions of *relief* on *news*, where *killed* and *affected* are treated in the same way as in columns (2) to (4) of Table IV. The first column shows that being covered in the *news* is associated with a 29 percent increase in the probability of receiving relief. The coefficient on *news* drops significantly, from 29 to 13–16 percent, when we add log *killed* and log *affected* in columns (2) and (3). This is because *relief* and *news* are both positively correlated with *killed* and *affected*.

Table VI about here

As discussed earlier, the above correlations do not measure the effect of *news* on *relief*. What we learn is that the overall average effect of *news* on *relief* is unlikely to be larger than 16 percent, since the bias through unobserved severity of disasters is most likely positive. However, the effect can be substantially higher for the subset of disasters that are close to marginal in the news decision.

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the system,

$$\begin{aligned} relief &= \alpha_0 + \alpha_1 news + \widetilde{\varepsilon}_r \\ news &= \beta_0 - \beta_1 p + \widetilde{\varepsilon}_n. \end{aligned}$$

Disasters may be categorized into three groups, (a) those that will not receive relief with or without news coverage, (b) those that will receive relief in either case and (c) those that only receive aid if covered by the news (the marginally reliefworthy disasters). The probability that news publication would induce relief to a randomly selected disaster (average effect) equals the share of all disasters that lie in group (c), and consistent OLS uses this sample share as an estimate of α_1. Similarly, disasters may be categorized into three groups based on newsworthiness, (i) those that will not be in the news, irrespectively of the news pressure, (ii) those that will be in the news, irrespectively of the news pressure, and (iii) those that are in the news only if the pressure for news space is low (the marginally newsworthy disasters). IV estimates α_1 as the share of group (c) disasters in group (iii). This is the average effect in the group of marginally newsworthy disasters (iii). The intuition from this simple example generalizes to applications involving multiple continuous instruments.

If the effect of *news* on *relief* is greater for disasters that are marginal in the news decision, then the correlation between *news* and *relief* would be higher for disasters with close to a 50 percent probability of being in the news. Therefore, we include the interaction between *news* and the absolute distance of the predicted probability of the disaster being in the news from 0.5 in the regression of *relief* on *news*. The results are shown in column (4). The estimated coefficient on *news* is 23 percent when the predicted probability of being in the news is 0.5. When the estimated probability of being in the news is 1 or 0, the partial correlation is zero $(0.23 - 0.5 * 0.49)$. Column (5) displays the results from probit-estimation of this model. These effects are estimated using coarse measures of whether a disaster is marginal in the news decision. The effect of *news* on *relief* could be substantially higher in the subgroup of disasters that were truly marginal in the news decision.

As discussed, IV estimates the effects in this subgroup. The results from IV estimation are reported in the last three columns. The estimated effects of *news* on *relief* are large and significant when using the full sample. In the sub sample where data on killed and affected is available, the effect is only significant at the 10 percent level; see column (7). Publishing a story on a disaster will increase the probability of subsequent relief by around 68 percentage points, based on the estimates from the specification in column (8). Note that the standard errors are large, 25 percent.

To sum up. The average effect of news on relief is unlikely to be larger than 16 percent. However, in the subgroup of disasters that are marginal in the news decision, the estimated effects are higher, around 70 percent.

III.D. *Robustness*

We now discuss a number of potential problems of identification. The results of this robustness analysis are shown in Table VII. Vertically, the table contains results for the reduced form estimates with *news* and *relief* as dependent variables, as well as the resulting IV-estimates of the effect of *news* on *relief*. The first column displays the benchmark specifications corresponding to columns (2) and (6) of Table IV and column (6) of Table VI. The following columns contain other specifications that we will now discuss.

Table VII about here

Many of the endogeneity issues are related to the instrument *news pressure*. On a priori grounds, it is easily argued that the dates of the Olympic Games are exogenous with respect to disaster relief and news coverage, controlling for month and year effects. We add the *news pressure* instrument to increase power, and then use the over-identifying restrictions to test its exogeneity. These restrictions are not rejected in any specification in Tables VI and VII, see the last row. In other words, the IV-estimates using *news pressure* only as an instrument produce a similar coefficient estimate as the IV-estimates instrumenting with *Olympics* only.[21] Furthermore, the instruments are not weak. The F-test for excluding the instruments in the first stage is typically larger than 10 (displayed in the last row of the first-stage results).[22]

We will start by testing our assumption that *news pressure* is uncorrelated with the error in the disaster news equation, ω. Ideally, we would like to measure what *news pressure* would have been, had there been no disaster news. The problem is that on some days, there is disaster news and we must consider how this affects our instrument. If news about a disaster is placed among the top three stories, then this would generate a positive bias (towards zero) in the coefficient β_1. If news about the disaster is placed outside the top three, then it might cause less time to be devoted to the top three segments, inducing a negative bias in β_1.

We construct two alternative measures of the pressure on news time to investigate the potential size of this bias. Recall that *news pressure* is the unweighted average of *daily news pressure* for the 40 days following the disaster. First, we compute *news pressure* as the average during the 40 days after the disaster, but remove all days when any disaster story was aired. This has a minimal impact on the estimated coefficients, see column (2). Next, to gauge the maximum size of the bias, we computed a new measure of *news pressure* that was intentionally biased in the most extreme way. For every news broadcast, we increased the time devoted to the top three news segments (*daily news pressure*) by the time devoted to disaster stories. This

[21] Regressions using *news pressure* and *Olympics* separately as instruments, reported in the working paper version of this paper, yield IV-estimates of the effect of *news* on *relief* of 0.74 and 1.1, respectively. Both are significant at the five-percent level and have significant reduced form coefficients on both instruments.

[22] Stock and Yogo [2002] show that a F-statistic above 10 can be interpreted as a test with approximately a 5 percent significance level of the hypothesis that the maximum relative bias is at least 10 percent.

corresponds to the extreme assumption that all disaster news was placed outside the top three segments and only took time from the top three news segments. In reality, the news time freed up by not airing the disaster news would partially be allocated to news segments outside the top three. As shown in column (3), the resulting bias in the estimated coefficient on *news pressure* is modest; it changes from -0.016 to -0.013. The likely reason is that we average over 40 days, and only introducing bias in the few days when disasters were covered has little impact. Most (85 percent) of the disasters that are in the news are only covered in one day. In sum, this type of bias is of minor concern.

Columns (4) and (5) check whether it is important that we use the 40-day average for *news pressure*. Here, we instead measure *news pressure* as an average that puts higher weight on days closer to the disaster. More precisely, column (4) uses the unweighted 20-day average, and column (5) uses the *weighted news pressure* variable discussed in Section II.D. This produces minor changes. We use the 40-day average in most specifications since it is not sensitive to the type of bias described in the previous paragraph.

Next we add a number of controls in column (6). We include the imputed log *Killed* and the imputed log *Affected*, dummy variables for the interaction of disaster type and missing data as discussed in Section III.B. To account for non-linear effects, we also include two sets of dummy variables indicating whether *Killed* and *Affected* lie in the percentile regions 0th–25th, 25th–50th, 50th–75th, 75th–95th percentiles, respectively (omitted category is killed above 95th percentile). In case there are seasonal variations within months, we now include week-fixed effects. Furthermore, the United States may be more willing to provide support to allies and there may be changes in U.S. relations to countries over time not captured by the country-fixed effects. For example, Iran was a formal ally of the United States until 1979, but not thereafter. To control for this, we include a variable indicating whether the United States has any formal alliance with the country.[23] This produces insignificant changes. Moreover, the coefficient on being a U.S. ally (not reported) is not significant in the news coverage or relief decision, controlling for country-fixed effects.

The next three columns report the results from regressions on different samples. The first sub sample only includes disasters where the outbreak is

[23] The Correlates of War Formal Interstate Alliance Data Set, v3.03. [Gibler and Sarkees 2003].

typically well defined: earthquakes, volcanoes, fires, landslides, floods and storms. As is evident from column (7), the coefficient estimates only change marginally in this sub sample. However, the standard errors rise because of the reduced sample size. Column (8) shows that including observations from countries that never received relief does not affect the estimates.

A concern is that the correlation between our instruments and relief may arise since events like the O.J. Simpson scandal distract policy makers from thinking about natural disasters. This concern is much smaller for *Olympics* than the more general *news pressure*. It could, however, be that Olympic Games generate political issues demanding policy makers' attention. Most Olympic Games, like the recent games in Torino, are not very political. Yet, the summer games of 1968, 1972, 1980 and 1984 created political issues. The negative correlation between *Olympics* and *relief* is not driven by these games. When they are removed from the sample, the negative correlation between *relief* and *Olympics* is equally strong, and significant at the 5 percent level (not reported).[24] A further indication that the political component of *Olympics* is not driving the results is that the less political *World Series* variable is correlated with *news* and *relief* in a very similar fashion, see the discussion in Section III.B.

Regarding *news pressure*, this type of problem is particularly worrying for big events. For example, policy makers could have been less inclined to declare disasters because of the 9/11 attacks. However, it seems unlikely that they are more inclined to declare a natural disaster because nothing interesting is happening on the network news, i.e. when the *news pressure* is exceptionally low. For this reason, we remove all observations when the *news pressure* was in the highest 1/3 that year. The remaining observations contain situations where the *news pressure* was medium to low. The coefficient on news pressure now identifies effects because marginally newsworthy disasters, which would typically not be on TV, get lifted into the network news when the *news pressure* is low. Column (9) shows the results in this sub sample. The estimated effects of our instruments on *news* and *relief* are similar or higher. However, since this specification explicitly reduces both the sample size and the sample variation in *news pressure*, the standard errors are much larger and the estimated effects of *Olympics* on *news* and *news pressure* on *relief* are not significant. The IV-estimate of the effect of *news*

[24] The estimate is -.126 (.058), compared to -.123 (.052) in the full sample, s.e. in parenthesis.

on *relief* is not affected to any considerable extent.

The effect of *news* on *relief* remains significant when estimating the relief and news equations as probits, using Maximum Likelihood (ML) estimation. The IV-estimation in column (10) models relief provision as a probit and news coverage as a linear probability model, while that used in column (11) models both as a bivariate probit. In both models, the errors in the news and relief equations may be correlated. In the "reduced form" part of the table, we report the linear first stage in column (10), and the results from two single-equation probit estimations in column (11). The estimated effects of *news* on *relief* are highly significant and imply marginal effects (evaluated at the sample mean) of *news* on *relief* of 0.87 and 0.63 percent, respectively. The ML estimates use the assumption that the errors are jointly normally distributed both to identify how much of the correlation between *news* and *relief* is caused by the effect of news and by the correlation in the errors, and to estimate the marginal effects. While the estimates are more efficient if the assumption is correct, they are inconsistent if it is not. For this reason, we focus on the TSLS estimates which consistently measure marginal effects without specific assumptions on the functional form of the error distribution.

A final remark concerns the interpretation of the estimated coefficient on *news*, given that we only consider television network news and ignore other mass media. We focus on TV news since most people cite TV as their main source of national and international news.[25] We implicitly assume that the U.S. Ambassadors declaring the disasters, or their principals in Washington, D.C., care about the image of disaster relief among the general public. In general, if other news sources are important in affecting the relief decisions, then we must re-interpret our results. Suppose, for example, that coverage in the New York Times has an independent effect on the relief decision. If disaster coverage in the television network news and the New York Times are correlated, then the OLS estimates of α_1 do not only include the policy effect of the publication of a network news story. They also include the effect of a New York Times publication multiplied by the increased probability that the disaster is covered in the New York Times that is implied by TV coverage. Similarly, to the extent that coverage in the New York Times is correlated with *Olympics* and *news pressure*, our IV-estimates will also include the effect through the New York Times.

To conclude, there are strong a priori reasons to believe that the dates of

[25] 82 percent in a Survey conducted by the PEW Research Center in January 2002.

the *Olympics* are exogenous with respect to the characteristics of disasters, and we argue that *news pressure* is also exogenous. Table V shows that the instruments are not correlated with the measured severity of disasters in our sample. For this reason, the estimated coefficients of interest are unaffected by accounting for nonlinearities in the effect of killed and affected, or other plausible covariates, see column (6) of Table VII. Column (7) in the same table shows that the estimated effects are also very similar in the sub sample of disasters where the strike dates are better defined. Regarding the exogeneity of *news pressure*, the over-identification tests show that instrumenting with *news pressure* and *Olympics* yield similar results. In columns (2) and (3), we show that the endogeneity of *news pressure* with respect to disaster news is not important. In addition, we have shown that the 40-day average assumption is not important. The events causing very high *news pressure* do not seem to directly affect the disaster declarations, since the coefficient estimates remain unchanged when only using observations where *news pressure* is medium to low.

IV. Discussion and Conclusions

Given the large humanitarian stakes, it is essential that disaster relief is not driven by factors unrelated to the usefulness of the relief. Still, we show that U.S. disaster relief depends on the availability of other newsworthy material at the time of the disaster. We argue that the only plausible explanation of this is that relief decisions are driven by news coverage of disasters, and that this news coverage is crowded out by other newsworthy material.

We find that natural disasters are more likely to receive relief if they occur when the pressure for news time in the U.S. network news broadcasts is low. Quantitatively, disasters are, on average, around eight percent more likely to receive relief if they occur when *news pressure* takes on its highest values than when taking its lowest, and five percent less likely to receive relief during the Olympics than at other times. Using another metric, to have the same chance of receiving relief, the disaster occurring during the highest *news pressure* must have six times as many casualties as the disaster occurring when *news pressure* is at its lowest, all else equal.[26] Similarly, a disaster occurring during the Olympics must have three times as many casualties as a disaster on an ordinary day to have the same chance of receiving relief.

The impact of news on disasters appears to vary across disasters. We find that, on average, news coverage is not likely to increase the probability of providing relief by more than around 16 percent. However, for disasters that are marginally newsworthy, in the sense of being covered if and only if there is little other news available, the effect is much larger, around 70 percent. We argue that this is because disasters marginal in the news decision are also marginal in the relief decision.

Some types of disasters are less likely to receive relief because of the news effect. Sen [1984] argues that media would increase government relief to famines at the expense of relief to endemic hunger. The reason is that famines constitute more dramatic and therefore newsworthy events that receive coverage in the media. However, among the set of natural disasters, famines are among the least newsworthy. In the words of Andrew Natsios, administrator of USAID, "In a war or famine, the most common types of slow-onset disasters, there are fewer spectacular events to report on than there are in earthquake or volcanic disasters" [Natsios 1995].

Table VIII displays the estimated newsworthiness of different disaster types. While the networks cover around 30 percent of the earthquakes and volcanic disasters, less than 5 percent of the epidemics, droughts and food shortages are covered, see the first column. This is not because earthquakes and volcanoes are more severe in terms of the number of killed or affected. The third column contains the estimated disaster type fixed effects from a regression including log *Killed*, log *Affected*, and fixed effects for country, year and month. This only accentuates the differences. In the last column, we

[26] The specification in column (8) of Table IV includes estimates of how the probability of receiving relief depends both on *news pressure* and the number of killed. The equation we solve is

$$0.0442 \ln(killed_1) - 0.0078 * news\ pressure_1 = 0.0442 \ln(killed_2) - 0.0078 * news\ pressure_2,$$

where $news\ pressure_1$ is the sample minimum (4.42) and $news\ pressure_2$ is the sample maximum (14.19). This implies

$$\frac{killed_1}{killed_2} = \exp\left(\frac{0.0078 * (14.19 - 4.42)}{0.0442}\right) = 5.6.$$

have computed the casualties ratio that would make media coverage equally likely, all else equal (controlling for the same factors as in the fixed effects regression). For example, for every person that dies in a volcano disaster, 38,920 people must die of food shortage to receive the same expected media coverage. The conclusion is that media induces extra relief to volcano and earthquake victims, at the expense of victims of epidemics, droughts, cold waves and food shortages.

Table VIII about here

Network news also induces a relief bias against Africa, Asia and the Pacific, see Table IX. While the TV networks cover more than 15 percent of the disasters in Europe and South and Central America, they cover less than 5 percent of the disasters in Africa and the Pacific. Asian disasters are more in the news than African ones because they are of more newsworthy types. In particular, Africa has many droughts and food shortages relative to Asia. There is no significant difference in news coverage after controlling for disaster type, log *killed* and log *affected*, month and year. The remaining differences between Africa, Asia and the Pacific on the one hand, and Europe and South and Central America on the other, are huge. The estimates suggest that it requires 45 times as many killed in an African disaster to achieve the same probability of media coverage as for a disaster in Europe. We conclude that media coverage induces extra U.S. relief to victims in Europe and on the American continent, at the expense of victims elsewhere.

Table IX about here

How do these results generalize to other policy areas? The media's influence may be stronger over foreign than domestic policies, since people have more direct information about the latter. Still, it seems likely that the underlying mechanisms would be equally active for domestic policy. For example, the publicity management mechanism suggests that people judge politicians based on their observable actions, mainly those covered in the media. Consequently, politicians should act swiftly and well to issues in the media. It seems equally likely that this would apply to, for example, domestic disaster relief, or problems in health care or education.

This responsiveness of relief to other news events illustrates a dilemma in foreign policy. Most American voters are not directly affected by foreign policy and only sketchily informed through the news media. If politicians follow the resulting "moody" demands of the American public, then policies will depend on events like the O.J. Simpson scandal. This instability can be decreased at the cost of limiting electoral accountability. To create more stability, James Madison and Alexander Hamilton suggested entrusting foreign policy to the President and the Senate,[27] neither of which were directly elected by the people, and where the Senate was elected for long terms. Their mechanism is currently only partly at work since both the Senate and the President are now, in practice, directly elected.

Our findings have important implications for the literature on media and politics. First, as previously mentioned, we quantitatively document the relationship between the publication of news stories and government policies. Second, the measures of available newsworthy material that we construct can be used to identify the effects of media stories on other outcome variables. It is easy to think of examples: the effect of news publication of earnings announcements on subsequent stock return, or the effect of the publication of unemployment or inflation reports on inflation expectations.

Appendix 1: Procedure for identifying disaster news stories

This appendix describes the procedure used to identify news stories about disasters. We proceed in two steps. Step 1 identifies disaster stories from the headlines of news stories. A story is considered a disaster story if the headline contains *both* the name of the location *and* the name of the type of disaster. The name of the location is captured by the name of the country, a country alias or the name of the capital. Some examples are:

– "Iran" OR "Persia" OR "Tehran"
– "Ghana" OR "Gold Coast" OR "Accra".

In some cases, we qualify the search by excluding headlines containing certain keywords. For instance,

– "Jordan" OR "Transjordan" OR "Amman" NOT

[27] See discussion in *The Federalist Papers No. 63* [Publius 1788].

("Baseball" OR "Basketball" OR "Lewinsky")
– "Sri Lanka" OR Ceylon OR "Colombo" NOT "Mafia".

To identify the disaster type, we use the following Boolean search combinations:

– Earthquake: "Earthquake" OR "Quake" NOT "Quaker"
– Flood: "Flood"
– Cold wave: "Cold wave" OR "Cold weather"
– Drought: "Drought"
– Epidemic: "Epidemic"
– Fire: "Fire" NOT "Cease-fire"
– Landslide: "Landslide" OR "Avalanche"
– Storm: "Storm" OR "Tidal Wave" OR "Typhoon" OR "Cyclone" OR "Hurricane" OR "Tornado"
– Volcano: "Volcano" OR "Volcanic"
– Food shortage: "Food shortage" OR "Famine"
– Infestation: "Locust" OR "Infestation".

Some stories on disasters cannot be identified from the headlines only. In step 2 we therefore conduct an additional search on the abstract of the news stories. Here, a story is considered a disaster story if the abstract contains *both* the name of the country *and* the name of the type of disaster. The disaster type is generally identified in the same manner as in step 1. However, for epidemics we also search on specific diseases for each country. Some examples are:

– "Bangladesh" AND ("Epidemic" OR "Malaria" OR "Arbovirus" OR "Diarrhoeal" OR "Intestinal protozoal")
– "India" AND ("Epidemic" OR "Arbovirus" OR "Diarrhoeal" OR "Intestinal protozoal" OR "Leptosporosis" OR "Malaria" OR "Measles" OR "Meningitis").

The list of diseases by country is based on the epidemics that have occurred in a particular country during the sample period according to EM-DAT.

Step 1 and step 2 together define the set of disaster news stories.

Appendix 2: Construction of the *news pressure* variable

This appendix explains how the *news pressure* variable was constructed by means of example. On the second day of our sample, August 5, 1968 the three networks started their broadcasts as shown in Table X. The network ABC spent 490 seconds on the first three news segments, which all covered different aspects of the Republican Party Convention. Similarly, CBS spent 430 seconds and NBC 600 seconds. The median across networks is 490 seconds. We use the news *segments*, as reported by the Vanderbilt Television News Archives, in this definition. It might be argued that all three ABC segments are about the same story – the Republican Convention – and that one should rather use the amount of time spent on the top three *stories*. However, it is non-trivial to identify which news segments should be merged into the same story, and we did not attempt this.

Table X about here.

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TABLE I

SUMMARY STATISTICS

Variable	Observations	Mean	Std. Dev.	Min	Max
relief	5 212	0.19	0.39	0	1
news	5 212	0.12	0.32	0	1
killed	3 714	590	9 143	1	300 000
affected	4 004	1 092 508	9 858 292	1	300 000 000
news pressure	5 212	7.73	1.22	4.56	14.32
Olympics	5 212	0.02	0.09	0	0.77
world series	5 212	0.01	0.05	0	0.56
US ally	5 212	0.32	0.47	0	1

TABLE II

SUMMARY STATISTICS FOR DISASTERS

Disaster type	Number of disasters	Share of disasters	Killed per disaster	Affected per disaster	Share receiving OFDA relief
Flood	1 675	0.32	170	1 724 851	0.22
Storm	1 175	0.23	646	601 490	0.17
Epidemic	737	0.14	249	27 528	0.12
Earthquake	559	0.11	1 522	173 015	0.21
Drought	326	0.06	18 657	5 740 623	0.30
Landslide	310	0.06	84	38 789	0.06
Fire	129	0.02	19	69 552	0.13
Cold wave	114	0.02	103	46 656	0.01
Volcano	102	0.02	853	39 008	0.27
Infestation	47	0.01	na	1 100	0.68
Food shortage	38	0.01	4 293	734 630	0.13
Total	5 212	1.00	590	1 166 505	0.19

TABLE III

DATES OF TWO LARGEST *daily news pressure* AND MAIN STORY, BY YEAR

Year	Date	Main News Story
2003	14 Aug	*New York City Blackout*
	22 Mar	*Invasion of Iraq: Day 3*
2002	11 Sep	*9/11 Commemoration*
	24 Oct	*Sniper Shooting in Washington: Arrest of Suspects*
2001	13 Sep	*9/11 Attack on America: Day 3*
	12 Sep	*9/11 Attack on America: Day 2*
2000	26 Nov	*Gore vs. Bush: Florida Recount - Certification by Katherine Harris*
	8 Dec	*Gore vs. Bush: Florida Recount - Supreme Court Ruling*
1999	1 Apr	*Kosovo Crisis: U.S. Soldiers Captured*
	18 Jul	*Crash of Plane Carrying John F. Kennedy, Junior*
1998	16 Dec	*U.S. Missile Attack on Iraq*
	18 Dec	*Clinton Impeachment*
1997	23 Dec	*Oklahoma City Bombing: Trial*
	31 Aug	*Princess Diana's Death*
1996	18 Jul	*TWA Flight 800 Explosion*
	27 Jul	*Olympic Games Bombing in Atlanta*
1995	3 Oct	*O.J. Simpson Trial: The Verdict*
	22 Apr	*Oklahoma City Bombing*
1994	17 Jan	*California Earthquake*
	18 Jun	*O.J. Simpson Arrested*
1993	17 Jan	*U.S. Missile Attack on Iraq*
	20 Apr	*Waco, Texas: Cult Standoff Ends in Fire*
1992	16 Jul	*Perot Quits 1992 Presidential Campaign*
	1 May	*Los Angeles Riots*
1991	27 Feb	*Gulf War: President Bush Declares Kuwait Liberated*
	17 Jan	*Gulf War: Operation Dessert Storm Launched*
1990	4 Aug	*Iraq Invasion of Kuwait: Day 4*
	8 Aug	*Iraq Invasion of Kuwait: Mobilisation of U.S. Troops*
1989	9 Mar	*Senate Rejection of Tower Appointment to Secretary of Defence*
	23 Dec	*Romania Revolution*
1988	22 Dec	*Pan Am Plane Crash*
	14 Dec	*Arafat Condemns Terrorism and Accept Israel's Right to Exist*
1987	26 Feb	*Iran Arms Scandal: Tower Commission Report*
	18 May	*USS Stark Attack in Persian Gulf*
1986	29 Jan	*Challenger Explosion*
	15 Apr	*U.S. Attack on Libya*
1985	30 Jun	*TWA Flight 847 Hijacking: Release of Hostages*
	29 Jun	*TWA Flight 847 Hijacking: Release of Hostages*

1984	12 Jul	*Ferraro as Vice President Candidate*
	16 Aug	*Delorean Verdict*
1983	25 Oct	*U.S. Invasion of Grenada: Day 1*
	3 Sep	*USSR Downing of Korean Commercial Flight*
1982	4 Aug	*Israel Invasion of Lebanon*
	2 Jan	*Poland: Martial Law*
1981	30 Mar	*Ronald Reagan Assassination Attempt*
	13 Dec	*Poland: Martial Law Declared by Wojchiech Jaruzelski*
1980	10 Aug	*Hurricane Allen in Texas*
	26 Dec	*Iran Hostage Crisis: Iran Release Film of Hostages*
1979	31 Mar	*Three Mile Island Nuclear Accident*
	15 Dec	*Iran Hostage Crisis: Departure of Shah from U.S. Announced*
1978	19 Nov	*Guyana Incident: Sect Mass Suicide*
	6 Aug	*Death of Pope Paul VI*
1977	14 Jul	*New York City Blackout*
	11 Aug	*Serial Killer David Berkowitz Arrested*
1976	13 Jul	*Democratic Convention*
	9 Jun	*Jimmy Carter Wins in Primaries*
1975	3 Nov	*Nelson Rockefeller Decides Not to Run for Vice President*
	14 May	*Mayaguez Incident: U.S. Attacks*
1974	1 Mar	*Watergate Indictments Announced*
	21 Jul	*Turkey Invades Cyprus*
1973	12 Feb	*Vietnam War: U.S. Prisoners of War Released*
	24 Jan	*Vietnam War: Cease-Fire Agreement Reached*
1972	9 Jan	*Howard Hughes Telephone Conference*
	28 May	*Nixon Visit in USSR: SALT I signed*
1971	16 Jul	*Nixon Announces Trip to China*
	16 Aug	*Nixon Suspends Convertibility from Dollars to Gold*
1970	28 Sep	*Gamal Abdel Nasser Dead*
	7 Sep	*Dawson's Field Hijackings: Blow Up of Planes*
1969	15 Oct	*Vietnam Anti-War Demonstration (Moratorium)*
	28 Mar	*Eisenhower Dead*
1968	22 Aug	*USSR Invades Czechoslovakia: Day 2*
	1 Nov	*October Surprise: Vietnam Bombing Halt*

Note: Ordered by *daily news pressure.*

TABLE IV

EFFECT OF THE PRESSURE FOR NEWS TIME ON DISASTER *News* AND *Relief*

	Dependent variable: *News*				Dependent variable: *Relief*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
News Pressure	-0.0162	-0.0163	-0.0177	-0.0142	-0.0117	-0.0119	-0.0094	-0.0078
	(0.0041)***	(0.0041)***	(0.0057)***	(0.0037)***	(0.0045)***	(0.0045)***	(0.0058)	(0.0040)**
Olympics	-0.1078	-0.1079	-0.0871	-0.111	-0.1231	-0.1232	-0.1071	-0.1098
	(0.0470)**	(0.0470)**	(-0.0628)	(0.0413)***	(0.0521)**	(0.0521)**	(0.0763)	(0.0479)**
World Series	-0.1133				-0.1324			
	(-0.1065)				(0.1031)			
log Killed			0.0605				0.0582	
			(0.0040)***				(0.0044)***	
log Affected			0.0123				0.0376	
			(0.0024)***				(0.0024)***	
imputed log Killed				0.0491				0.0442
				(0.0034)***				(0.0037)***
imputed log Affected				0.0151				0.0394
				(0.0020)***				(0.0020)***
Observations	5212	5212	2926	5212	5212	5212	2926	5212
R-squared	0.1799	0.1797	0.3624	0.2875	0.1991	0.1989	0.4115	0.3726

Linear probability OLS regressions. All regressions include year, month, country and disaster type fixed effects. Regressions with imputed values ((4) and (8)) also include fixed effects for the interaction of missing values and disaster type. Robust standard errors in parentheses:* significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE V

CORRELATIONS BETWEEN INSTRUMENTS AND THE SEVERITY OF DISASTERS

	Dependent variable	
	News Pressure	*Olympics*
log Killed	-0.0082	0.0003
	(0.0113)	(0.0010)
log *Affected*	0.0005	-0.0006
	(0.0068)	(0.0006)
p-value: F-test of joint insignificance	0.75	0.62
Observations	5212	5212
R-squared	0.3110	0.2035

OLS regressions with the instruments *News Pressure* and *Olympics* as dependent variables, and including year, month, country and disaster type fixed effects. Robust standard errors in parentheses:* significant at 10%; ** significant at 5%; *** significant at 1%. The F-test tests the joint significance of log *Killed* and log *Affected* in the regression.

TABLE VI

DEPENDENT VARIABLE: *Relief*

	OLS					IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
News	0.2886	0.158	0.1309	0.2323	0.2611	0.8237	0.6341	0.6769
	(0.0200)***	(0.0232)***	(0.0178)***	(0.0328)***	(0.0569)***	(0.2528)***	(0.3341)*	(0.2554)***
News*abs(Pr(news)-0.5)				-0.4922	-0.302			
				(0.1059)***	(0.0840)***			
abs(Pr(news)-0.5)				0.5374	0.2959			
				(0.0943)***	(0.0831)***			
log Killed		0.0486					0.0198	
		(0.0046)***					-0.0208	
log Affected		0.0358					0.0299	
		(0.0024)***					(0.0048)***	
imputed log Killed			0.0378	0.0546	0.0307			0.0109
			(0.0038)***	(0.0049)***	(0.0046)***			-0.0132
imputed log Affected			0.0375	0.0445	0.0345			0.0292
			(0.0020)***	(0.0023)***	(0.0026)***			(0.0045)***
F-stat, instruments, 1st stage						11.0	6.1	11.1
Over-id restrictions, χ^2_{df} (p-value)						0.51_1(0.47)		0.64_1 (0.42)
Observations	5212	2926	5212	5212	5027	5212	2926	5212
R-squared	0.2443	0.4225	0.3800	0.3860				

All regressions include year, month, country, and disaster type fixed effects. Regressions with imputed values ((3), (4) and (5)) also include fixed effects for the interaction of missing values and disaster type. Robust standard errors in parentheses: * significant at 10%; ** significant at 5%; *** significant at 1%.

TABLE VII

ROBUSTNESS

	Reference	Changes in independent variables					Different samples			Probit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Reduced form regressions. Dependent variable: *News*											
News Pressure	-0.0163	-0.0157	-0.0124	-0.0133	-0.0155	-0.0143	-0.0193	-0.0137	-0.0242	-0.0156	-0.0984
	(0.0041)***	(0.0040)***	(0.0042)***	(0.0033)***	(0.0036)***	(0.0038)***	(0.0052)***	(0.0039)***	(0.0118)**	(.0041)***	(0.0273)***
Olympics	-0.1079	-0.1085	-0.1114	-0.1125	-0.1156	-0.1090	-0.1008	-0.1199	-0.1320	-0.1206	-0.5902
	(0.0470)**	(0.0469)**	(0.0470)**	(0.0470)**	(0.0470)**	(0.0460)**	(0.0572)*	(0.0451)***	(0.0864)	(0.0428)***	(0.3084)*
Observations	5212	5212	5212	5212	5212	5212	3950	6303	3473	5209	4266
R-squared	0.1797	0.1789	0.1779	0.1789	0.1795	0.2969	0.1862	0.1887	0.2072		
F-test (instr.)	11.05	10.5	7.4	11.19	12.3	10.0	8.6	10.33	3.4		
Reduced form regressions. Dependent variable: *Relief*											
News Pressure	-0.0119	-0.0116	-0.0104	-0.0092	-0.0085	-0.0082	-0.0126	-0.0105	-0.0125		-0.0611
	(0.0045)***	(0.0045)***	(0.0046)**	(0.0038)**	(0.0041)**	(0.0040)**	(0.0054)**	(0.0038)***	(0.0142)		(0.0233)***
Olympics	-0.1232	-0.1235	-0.1250	-0.1266	-0.1290	-0.1182	-0.1338	-0.0984	-0.1767		-0.6471
	(0.0521)**	(0.0521)**	(0.0520)**	(0.0521)**	(0.0521)**	(0.0515)**	(0.0583)**	(0.0421)**	(0.0893)**		(0.3200)**
Observations	5212	5212	5212	5212	5212	5212	3950	6303	3473		5209
R-squared	0.1989	0.1989	0.1986	0.1987	0.1985	0.3851	0.1835	0.2208	0.2271		
IV-regressions. Dependent variable: *Relief*										IV-Probit	Bi-Probit
News	0.8237	0.8351	0.9391	0.8012	0.6726	0.7124	0.7561	0.7846	0.8505	3.1735	1.9463
	(0.2528)***	(0.2595)***	(0.3225)***	(0.2561)***	(0.2341)***	(0.2637)***	(0.2581)***	(0.2366)***	(0.4597)*	(0 .4417)***	(0.2246)***
Observations	5212	5212	5212	5212	5212	5212	3910	6303	3446	5209	5212
Over-id test	0.51_1(0.47)	0.47_1 (0.49)	0.20_1 (0.66)	0.61_1 (0.43)	1.27_1 (0.26)	0.75_1 (0.39)	1.10_1 (0.30)	0.01_1(0.92)	0.85_1 (0.36)		

Robust standard errors in parentheses: * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include year, month, country and disaster-type fixed-effects. Column (1) reports the results from an OLS regression. In column (2) all days with news about disasters has been removed before computing the average *news pressure*. In column (3) an extreme bias has been intentionally induced in *news pressure*. In column (4) *news pressure* is the 20-day average of *daily news pressure*, and in column (5) it is the average using the weights reported in Figure I. The regression in column (6) contains controls for whether the country was a US Ally, week fixed effects, imputed log Killed and imputed log Affected, dummy variables for the interaction of disaster type and missing data, as well as two sets of dummy variables indicating whether *Killed* and *Affected* lie in the percentile regions 0^{th}-25^{th}, 25^{th}-50^{th}, 50^{th} -75^{th}, 75^{th}-95^{th} percentiles respectively (omitted category is killed above 95^{th} percentile). Column (7) contains a sub sample with only earthquakes, floods, fires landslides, storms and volcano eruptions. Column (8) excludes observations where *News Pressure* was in the highest third each year. Column (9) includes observations from countries that never received U.S. relief. The IV-estimate in column (10) shows the result from ML estimation of a model where the first stage is linear and the second stage is a probit. The "reduced form" regression on news shows the result from the first stage of this regression. The IV-estimate in column (11) shows the result from a ML estimation of a bivariate probit model with an endogenous binary variable. The "reduced form" regressions of this column show the results from single equation probit estimation of *news* and *relief* on the exogenous variables.

TABLE VIII
NEWSWORTHYNESS OF DISASTERS, BY DISASTER TYPE

	Share in news	(se)	Fixed effects	(se)	Equal coverage casualties ratio*
Volcano	0.30	(0.05)	0.64	(0.09)	1
Earthquake	0.33	(0.02)	0.59	(**)	2
Fire	0.14	(0.03)	0.49	(0.09)	12
Storm	0.14	(0.01)	0.30	(0.03)	280
Flood	0.09	(0.01)	0.25	(0.03)	674
Landslide	0.07	(0.01)	0.23	(0.03)	882
Epidemic	0.02	(0.01)	0.19	(0.03)	1 696
Drought	0.04	(0.01)	0.17	(0.07)	2 395
Cold wave	0.06	(0.02)	0.15	(0.07)	3 150
Food shortage	0.03	(0.03)	0.00	(0.10)	38 920

The fixed effects regression includes log *Killed*, log *Affected*, and country, year and month fixed effects.
*To have the same estimated probability of being covered by the television network news, a food shortage must have 38 920 times as many casualties as a volcano, all else equal (country, year, month, and number affected).
** Earthquake is the omitted category.

TABLE IX
NEWSWORTHYNESS OF DISASTERS, BY CONTINENTS

	Share in news	(se)	Fixed effects	(se)	Equal coverage casualties ratio*
Europe	0.18	(0.02)	0.25	(**)	1
S. and C. America	0.18	(0.01)	0.19	(0.04)	3
Asia	0.13	(0.01)	0.04	(0.04)	43
Africa	0.04	(0.01)	0.04	(0.04)	45
Pacific	0.03	(0.01)	0.00	(0.04)	91

The fixed effects regression includes log *Killed*, log *Affected*, and disaster type, year and month fixed effects.
*To have the same estimated probability of being covered by the television network news, a disaster in the Pacific must have 91 times as many casualties as one in Europe, all else equal (disaster type, year, month, and number affected).
** Europe is the omitted continent.

TABLE X

NEWS BROADCASTS ON AUGUST 5, 1968

	Length (s)
ABC	
Introduction Frank Reynolds (Miami Beach)	10
Eisenhower Heart Attack / Republican National Conv	130
Republican National Convention / Reagan / Rockefeller / Nixon	160
(Commercial: Johnson's Off Insect Repellent; Pledge Furniture Wax.)	60
Republican National Convention / Nixon	200
CBS	
Introduction Walter Cronkite (Miami Beach): Charles Kuralt (New York City)	10
Eisenhower Health / Nixon	170
(Commercial: Old Gold And Spring 100 Cigarettes.)	50
Republican National Convention / Nixon	180
Convention / Reagan	80
NBC	
Introduction Chet Huntley (Miami Beach); David Brinkley (Miami Beach)	10
Republican National Convention / Nixon / Reagan / Rockefeller	380
Convention / Money / Stassen	30
(Commercial: Geritol Tablets And Liquid; Serutan Laxative.)	70
Convention Candidates / Nixon / Rockefeller / Reagan	190

Headlines and time devoted to first five segments in news broadcasts of the evening news of ABC, CBS and NBC on August 5, 1968.

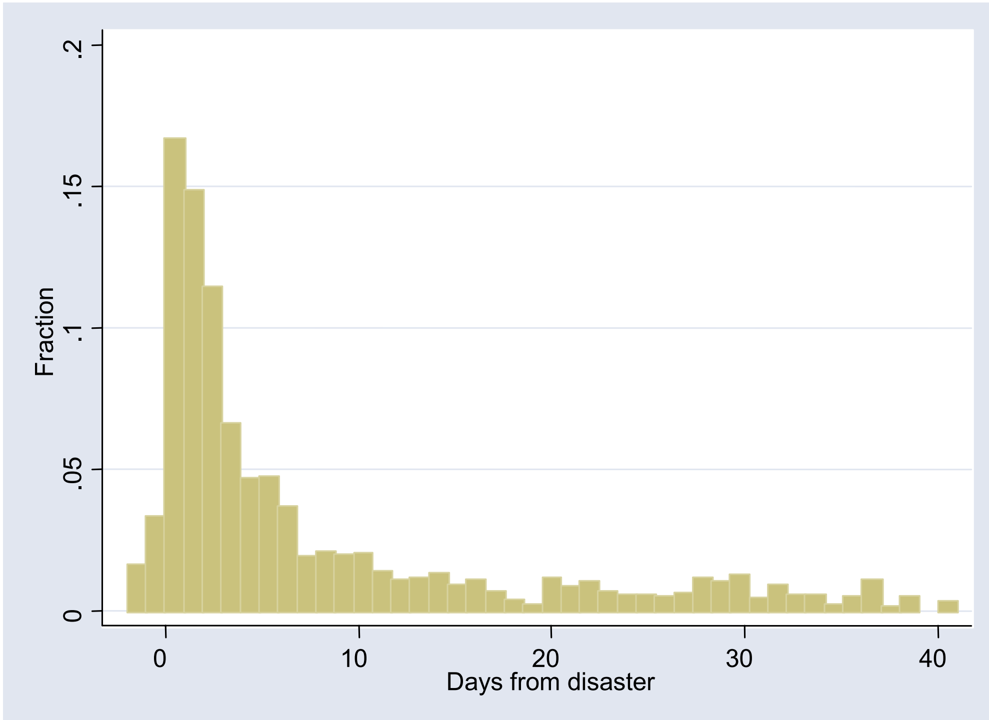

FIGURE I
News Stories on Disasters, by Days from the Disaster

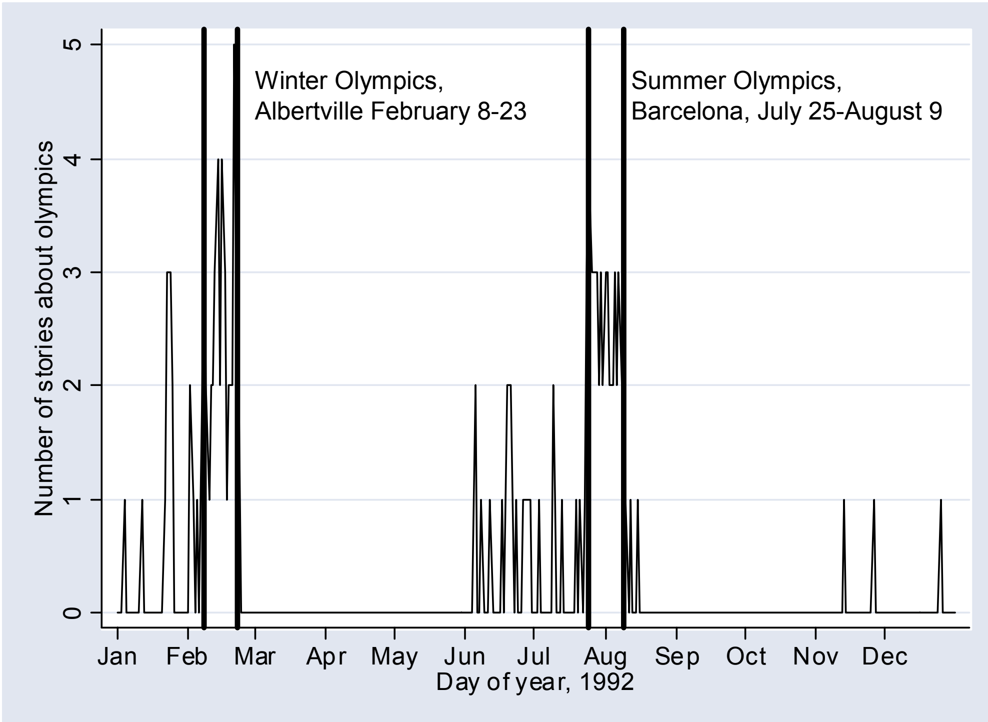

FIGURE II

Daily Number of News Stories about Olympic Games, 1992

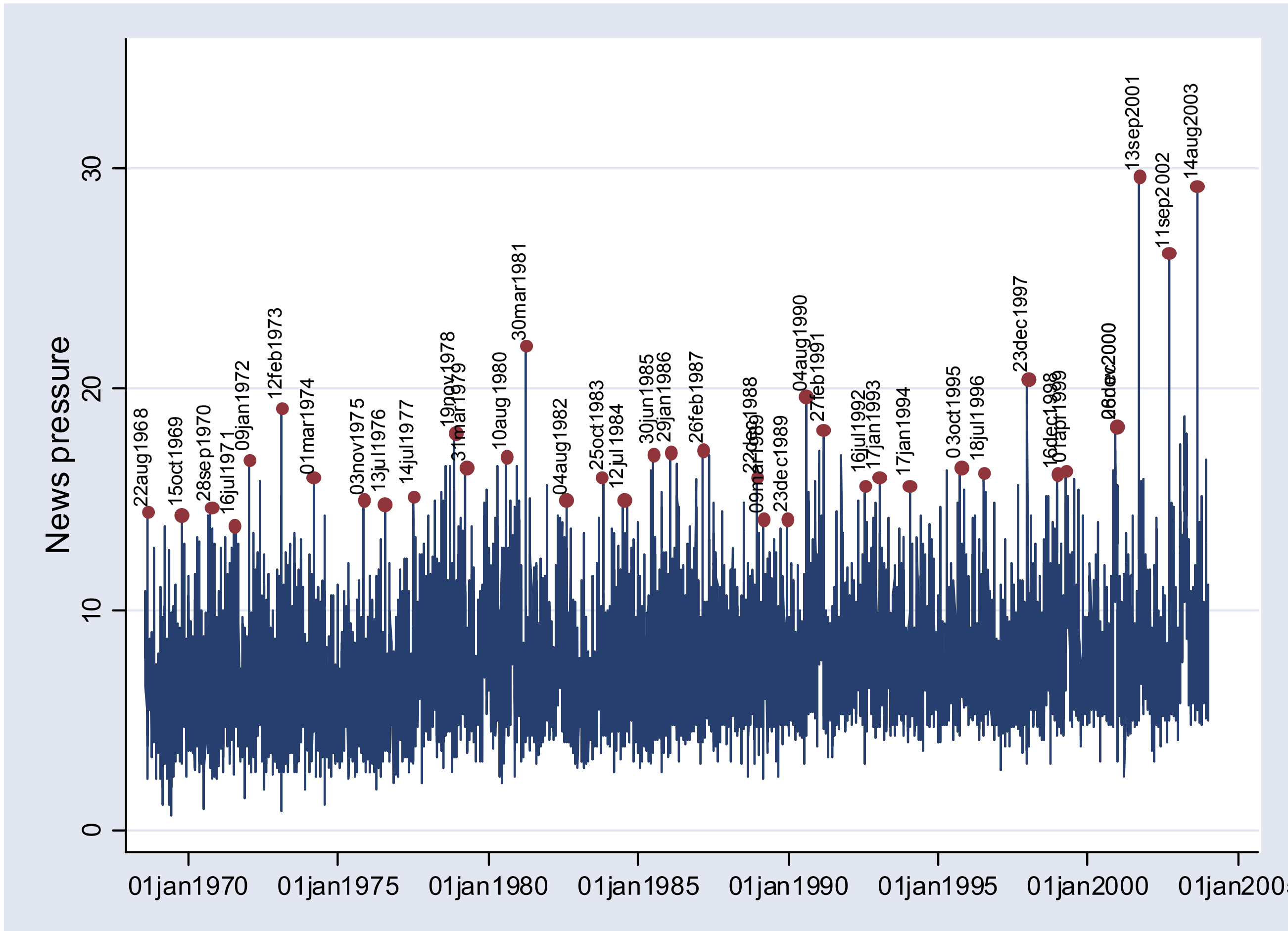

FIGURE III
Daily News Pressure (Minutes), by Day

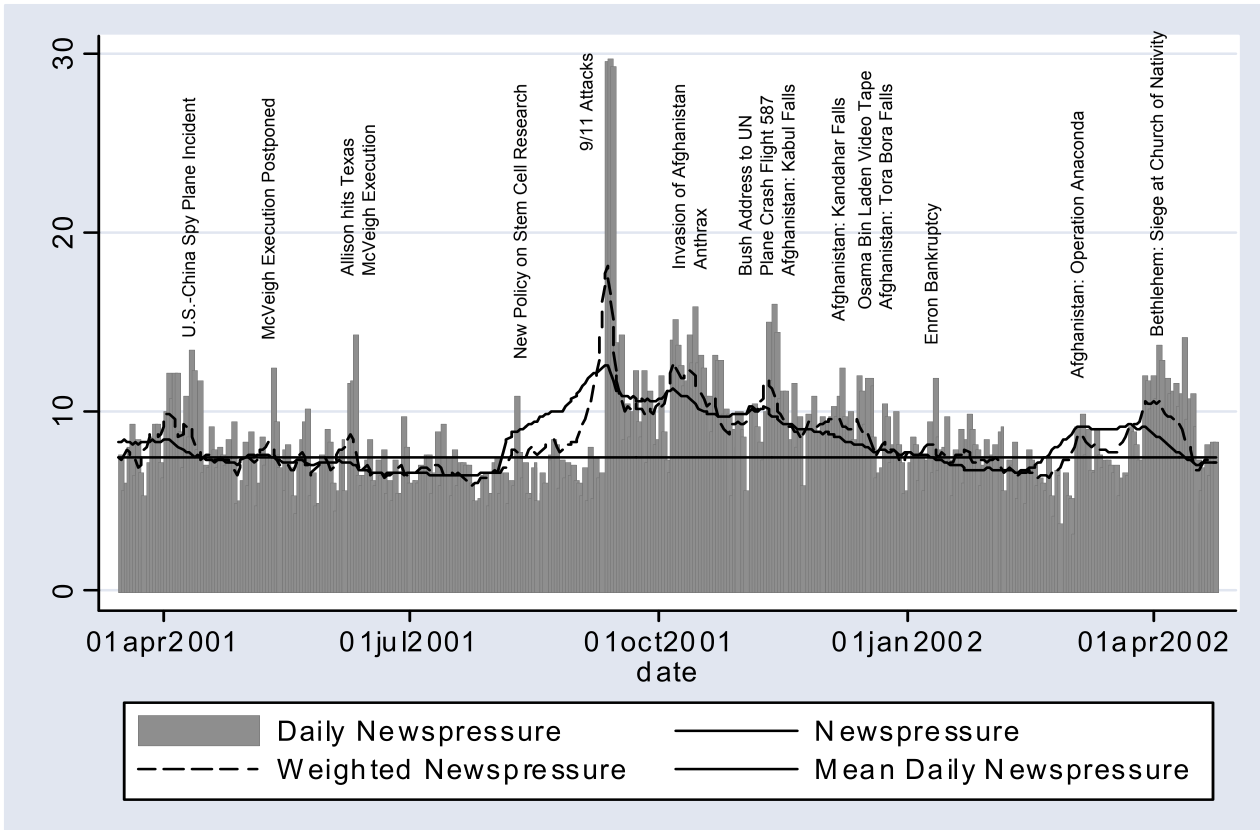

FIGURE IV
News Pressure (Minutes) during 405 Days, 15 March 2001 – 23 Apr 2002, by Day

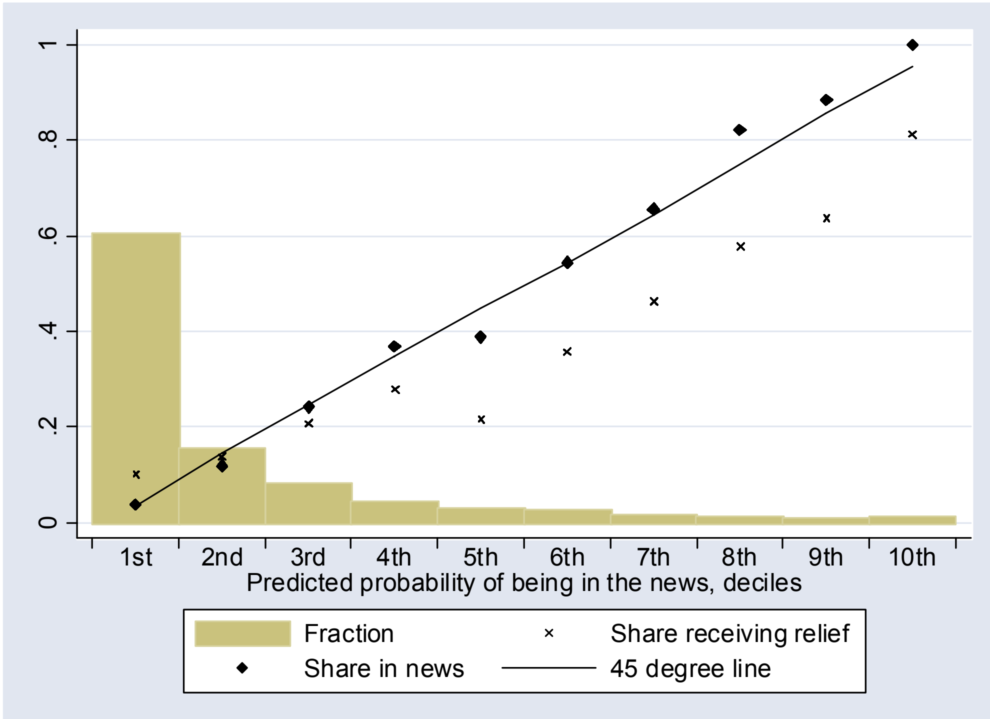

FIGURE V
Predicted Probability of a Disaster Being in the
News and Actual Shares of Disasters Receiving Relief