Pandering in the Shadows: How Natural Disasters Affect Special Interest Politics*

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Abstract

We exploit the quasi-random timing of natural disasters to study the connection between public attention to politics and legislators' support for special interests. We show that when a disaster strikes, the news media reduce both their coverage of politics in general as well as that of individual legislators in particular. At the same time, members of the House of Representatives become significantly more likely to adopt the positions of special-interest donors as they vote on bills. Taken together, the evidence implies that politicians are more inclined to take actions that benefit special interests when the public is distracted. More broadly, our findings suggest that attention to politics improves electoral accountability, even in an environment with stringent transparency and disclosure requirements.

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1. Introduction

One of the main democratic benefits of media is to inform the citizenry. Information in the hands of voters does not only have the potential to affect the selection of politicians but it might also help to curb corrupt behavior of officeholders. In the famous words of Supreme Court Justice Louis Brandeis (1914, p. 92), "sunlight is said to be the best of disinfectants."

Political economists have extensively studied the effects of media on the executive branch. In a seminal contribution, Strömberg (2004) shows that the introduction of radio significantly increased government transfers during the New Deal. Eisensee and Strömberg (2007) demonstrate that the U.S. Agency for International Development (USAID) is more likely to provide relief to victims of natural disasters in foreign countries if the event receives greater coverage on the domestic news. In a similar vein, Durante and Zhuravskaya (2018) provide evidence that the Israeli government strategically chooses to attack Palestinians when news pressure is high and its actions are less likely to be scrutinized in the media.[1] Even U.S. presidents appear to time the release of controversial executive orders to evade public scrutiny (Djourelova and Durante 2021).

By contrast, there has been little quantitative research on the connection between media coverage, attention to politics, and actions of the legislative branch. In this paper, we examine this connection. We ask whether members of the U.S. House of Representatives vote differently on the passage of legislation when the public is temporarily distracted. Congressional votes are at the heart of the policy-making process and thus extremely important. Qualitative accounts of politician behavior suggest that contemporaneous scrutiny can greatly affect electoral accountability. According to Kingdon's (1973, p. 46) seminal description of legislators' voting decisions, members of Congress "are constantly called upon to explain to constituents why they voted as they did. [...] They not only actually experience being called upon, but they also anticipate that the situation will arise." Kingdon further remarks that "much of the problem of explaining one's vote is directly tied to the media coverage which reaches the district." Drawing on his time shadowing members of Congress in their districts, Fenno (1978, p. 74) reports:

"A recent critical newspaper story nearly traumatized [Congressman B]. 'It gets you right in

[1] Hollywood has taken this one step further. A 1997 film, Wag the Dog, imagines a president starting a war in order to cloak an impending release of a sex scandal. Referencing this movie, pundits have speculated that the bombing of Iraq just before the Clinton impeachment vote and the U.S. involvement in the conflict in Kosovo between the House impeachment and the Senate confirmation vote of the impeachment were an attempt at media distraction (see, e.g., LA Times 1998)

the stomach and makes you want to throw up all over the floor,' he said. He avoids taking controversial positions because he hates controversy."

This and similar anecdotes notwithstanding, it is not obvious that contemporaneous attention does indeed improve accountability—especially when legislators' votes are public record, when politicians are already subject to strict transparency and disclosure requirements, and when voter memories are potentially short lived. Formal theories of electoral accountability, for instance, emphasize the information contained in an incumbent's record rather than contemporaneous scrutiny for disciplining politicians (e.g., Barro 1973; Ferejohn 1986; Austen-Smith and Banks 1989; Ashworth 2012). Whether the timing of scrutiny actually matters, and whether any such effect is quantitatively important is an open empirical question.

Consistent with extant anecdotes, we present evidence that when attention to politics is temporarily reduced, members of Congress behave systematically differently. They tilt their votes towards the positions of their special-interest donors.

Estimating the effect of public scrutiny and attention on legislator behavior is difficult for at least two reasons. First, it is *a priori* not clear how to measure attention. Second, attention to politics may not only influence lawmakers' behavior, but it is also a function of voter interest, interest-group power, and the actions of officeholders themselves. Even if contemporaneous scrutiny does discipline politicians, such an effect may be difficult to detect if, in equilibrium, corrupt representatives are more closely monitored than others.

We address the first of these challenges by using machine learning to detect politics reporting on the nightly news. To obtain a more comprehensive picture, we supplement this measure of attention to politics in the national media with information on how often individual representatives are mentioned in local newspapers and information on Congress-related Google searches. To deal with the endogeneity in attention to politics, we exploit randomness from the precise timing of domestic natural disasters. Intuitively, natural disasters raise the opportunity costs of following day-to-day politics—for both citizens and the press. They temporarily crowd out attention. Moreover, natural disasters likely reduce attention of citizens and the press to a significantly greater degree than that of interest groups. As long as the timing of these events is not systematically related to the political process, we can rely on the disaster-induced temporary crowd out to learn about the impact of contemporaneous attention on the behavior of politicians.

To measure legislator support for special interests, we draw on a novel data set that uses public announcements to record the positions of several hundred interest groups on particular pieces of legislation, these groups' donations to individual politicians, as well as lawmakers' subsequent votes. For any particular bill, we say that a member of Congress votes with special interests whenever she votes "yea" ("nay") and the set of interest groups that supported this bill contributed more (less) money to her campaign than did the opposing groups. For a concrete example, consider Patrick McHenry's (R, NC–10) choice on the "Reforming CFPB Indirect Auto Financing Guidance Act," which sought to weaken consumer protections in auto lending. McHenry received a total of $254,050 from organizations that supported the bill—mainly financial services and auto companies—compared to $1,000 from groups that opposed it. Since he voted in favor of passage, we classify him as having voted with his special interests donors.[2] Our measure of support for special interests thus varies across congresspeople voting on the same bill (depending on which groups donated to their campaigns) as well as across bills "within" a given congressperson (depending on which positions her donors took on different pieces of legislation).

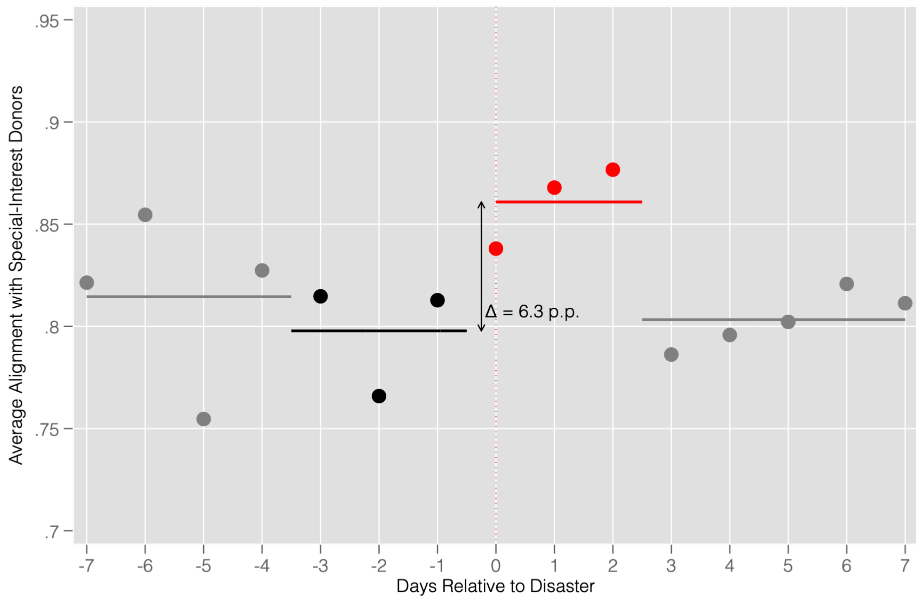
In the raw data, members of the House of Representatives support the positions of their special interest donors about 81% of the time. This could be because interest groups buy votes from politicians who disagree with them or, more innocuously, because special interests support politicians with whom they are ideologically aligned. For us, the key question is whether legislators' support for the positions of their donors increases systematically at the onset of natural disasters.

As Figure 1 shows, in the three-day window *before* a major disaster strikes, support for special interests is at about 80%. Focusing on the three days *after* the onset of the disaster, however, we observe that 86% of votes align with the positions of politicians' special interest donors. The raw data, therefore, hint at a connection between natural disasters and how congresspeople vote on new legislation.

In Section 4, we resort to randomization inference as a simple and transparent method to assess whether the apparent increase in support for special interests might be due to chance. Holding the actual number of natural disasters in our data fixed and randomly drawing a new start date for each event, we show that the observed increase in support for special interests exceeds more than 98% of placebo estimates. Based on this evidence, we conclude that in the days just after a natural disaster strikes, congresspeople are significantly more likely to support the positions of their special interest donors—both relative to how they usually vote as well as how they tend to

[2]As we show in Section 3, situations in which legislators receive nearly equal amounts from interest groups on opposite sides of a bill are rare.

Figure 1: Alignment with Special Interests, Before and After Natural Disasters



Notes: Figure shows how frequently congresspeople's votes align with the position of their special interest donors around the time of a domestic natural disaster. Day 0 denotes the reported onset of the event. The position of a legislator's special interest donors corresponds to the side that contributed the most to her campaign.

vote just before disasters.

In addition to conducting randomization inference, we present event-study evidence that supports this conclusion. Our event-study findings reveal *(i)* no pre-trends in how congresspeople vote leading up to the day of the disaster, *(ii)* an immediate impact of the disaster on votes, and *(iii)* almost complete dissipation after about three days. To ensure that our findings do indeed reflect the (reduced-form) effect of disasters, we conduct an array of robustness checks. These include but are not limited to showing that our results are qualitatively and quantitatively robust to seasonality controls, day-of-the-week fixed effects, legislator fixed effects, and even legislator-by-Congress fixed effects.

In Section 5, we confirm that disasters crowd out attention to politics. We present evidence of crowd-out of politics reporting in national broadcast media, and, more tentatively, of a reduction in newspaper coverage of local congresspeople as well as a temporary reduction in politics-related Google searches. Broadly summarizing, our results document a simultaneous decline in attention to politics and a realignment of roll-call votes towards legislators' special-interest donors.

In Sections 6 and 7, we discuss potential mechanisms behind this pattern in the data. Drawing on ancillary evidence, we rule out many ex ante reasonable alternative interpretations of our findings. For instance, we find no support for the idea that, in the aftermath of a disasters, aid gets added on to pre-existing bills which then garner more legislative support. Instead, we present a moral hazard framework for understanding our findings, and discuss conditions under which our results are consistent with a reduction in voter welfare.

2. Related Literature

Our findings contribute to a large body of work on special-interest politics. While many theoretical models predict "quid pro quo"-like arrangements between politicians and interest groups (see, e.g., Baron 1989; Denzau and Munger 1986; Grossman and Helpman 2001), actual evidence on such relationships has been inconclusive. In influential work, Wawro (2001) and Ansolabehere et al. (2003) demonstrate that the correlation between campaign contributions and roll-call votes either strongly diminishes or, in many cases, entirely disappears upon controlling for legislator fixed effects. Based on their review of the literature, Ansolabehere et al. (2003, p. 125) argue that "rent-seeking donors lack the leverage to extract large private benefits from legislation."[3]

Nevertheless, special interests do appear to allocate their donations strategically (see, e.g., Barber 2016; Bertrand et al. 2020, 2018; Bombardini and Trebbi 2011; Fouirnaies and Hall 2018; Powell and Grimmer 2016). Fouirnaies and Hall (2018), for instance, demonstrate that interest groups seek out members of relevant committees and lawmakers with procedural power. Bertrand et al. (2020) even show that corporate donations to politicians' pet charities follow a similar pattern. These and similar findings have led to the conclusion that the observed patterns of campaign contributions are consistent with a market for political influence. In the words of Powell and Grimmer (2016, p. 986), extant work creates "an appearance of corruption—the key word being *appearance*."

We do not take a position on whether contributions buy votes. Interest groups may buy votes from politicians who disagree with them or they may support politicians with whom they are ideologically aligned. Either way, we add to this literature by studying the conditions under which politicians are especially likely to support the positions of their donors. Although our research design does not allow us to estimate how representatives would have behaved in the absence of ties to special-interest groups, we provide evidence that attention to politics mediates the extent to which the positions

[3]This assessment is not uncontroversial. Considering the same set of studies reviewed by Ansolabehere et al. (2003), Stratmann (2005) rejects the null hypothesis of no effect.

of their donors are reflected in passage votes.

While we do not take a position on why politicians' votes often align with those of their special-interest donors, we do argue that our results shed light on the impact of media coverage of legislative alignment with donors. A burgeoning literature has demonstrated impacts of media on the behavior of politicians (see DellaVigna and Gentzkow 2010; Prat and Strömberg 2013; and Strömberg 2015a,b for reviews). Particularly important prior contributions include Strömberg (2004), Eisensee and Strömberg (2007), Snyder and Strömberg (2010), and Durante and Zhuravskaya (2018).[4]

As explained in the introduction, however, extant work focuses on actions of the executive branch (e.g., Strömberg 2004; Eisensee and Strömberg 2007; Durante and Zhuravskaya 2018; Djourelova and Durante 2021). There exists little quantitative evidence of how the media affect the behavior of rank-and-file legislators. The most important exception is Snyder and Strömberg (2010), who establish that local newspapers report more about congresspeople when their market overlaps to a greater extent with the representative's district. In turn, members of Congress vote less along party lines and are more likely to stand witness before congressional hearings when voters are better informed. By exploiting the quasi-random timing of natural disasters rather than variation in media market structure, our analysis provides high-frequency evidence of moral hazard in Congress, holding selection into office as well as the long-run incentives of incumbents fixed. In addition, we explore the role of attention in special-interest politics—an outcome which is important in its own right, especially when it comes to new legislation.

In simultaneous, unpublished work, Balles et al. (2023) also investigate the effect of disasters on legislator alignment with special interests. Balles et al. (2023) rely on similar data, and they estimate econometric models that are broadly comparable to the ones below. As a consequence, their headline result mirrors ours: members of Congress are more likely to vote with their special-interest donors when a disaster strikes. There are, however, a number of important differences. First, we directly document attention crowd-out. We are, therefore, able to provide evidence in support of the claim that legislator behavior is moderated by a reduction in attention to politics. Second, our empirical strategy eschews man-made disasters as sources of identification. In particular,

[4]Other notable contributions include Groseclose and Milyo (2005) and Gentzkow and Shapiro (2010) on measuring media bias, Durante and Knight (2012) on partisan control of the media, Martin and Yurukoglu (2017) on media bias and polarization, as well as DellaVigna and Kaplan (2007), Gentzkow et al. (2011), Chiang and Knight (2011), and Enikolopov et al. (2011) on the effects of (biased) media on electoral outcomes.

we exclude adverse events such as terrorist attacks or school shootings, which might be problematic if either legislators or interest groups adapt their positions in response to the incident—think, for instance, of the NRA and the push for gun control. Third, we conduct an array of additional tests that point to moral hazard—rather than, say, agenda setting—as the mechanism behind our findings.[5]

3. Data and Descriptive Statistics

To test the idea that contemporaneous scrutiny reduces politicians' support for special interests we assemble a new data set with information on *(i)* the positions of special-interest groups on particular pieces of legislation, *(ii)* legislators' votes on congressional bills, *(iii)* contributions from interest groups to politicians, *(iv)* the occurrence of natural disasters, and *(v)* attention to politics.

MapLight Data Information on *(i)*–*(iii)* comes from MapLight, a nonpartisan nonprofit organization that strives to reveal money's influence on politics. MapLight's research staff comb through publicly available sources, like congressional testimony, news databases, and trade associations' websites, to compile lists of organizations and interest groups that either supported or opposed important pieces of federal legislation (i.e., bills that are not merely ceremonial). Starting with the 109^{th} Congress (2005), MapLight links an organization's position on a particular bill to its donations to individual members of Congress in the same election cycle as well as to the relevant roll-call votes.

Our analysis relies on the linked records for all 1,525 bills that *(a)* received a passage vote in the House of Representatives prior to October 2017, and *(b)* were supported or opposed by at least one interest group. We focus on passage votes because they are consequential and because it is much rarer for interest groups to take an explicit, public stand on amendments. Important for our purposes, in 87% of instances where MapLight was able to identify an interest group's position, it was able to identify the position *prior* to the vote. This mitigates the potential for disasters to affect which interest groups are recorded as having a stake in the bill.

Nonetheless, it is unrealistic to assume that MapLight captures the universe of interest-group positions. To get a better sense of how complete our data are, we have

[5]Arguably less related is recent work by Gagliarducci et al. (2019), who study how members of Congress change their support for environmental regulation in response to hurricanes. Gagliarducci et al. (2019) find that congresspeople whose district is hit by a storm become more likely to support green legislation. This effect persists for several years, consistent with permanent changes in beliefs. By contrast, the effects we document are present even for members of Congress whose districts were unaffected by the disaster, and they dissipate within a matter of days. Both sets of findings thus speak to very different mechanisms through which adverse events can alter politician behavior.

cross-referenced the bills covered by MapLight with those in Box-Steffensmeier et al. (2019). The latter identify pieces of legislation that were supported by special-interest groups based on the universe of electronic Dear Colleague letters sent between members of the House during the 106^{th}–111^{th} Congresses. Focusing on years that are included in both data sets, and ignoring commemorative pieces of legislation as well as those that did not receive a passage vote, we find that only 9% of the bills in Box-Steffensmeier et al. (2019) do not appear in the MapLight data, and among the bills that MapLight misses, only one appeared on the "Legislative Hot List." The "Legislative Hot List" highlights contentious pieces of legislation, major policy initiatives, appropriations bills, and other important measures that were considered in each Congress, as determined by the Secretary of the U.S. Senate (Box-Steffensmeier et al. 2019). For comparison, among all interest-group-endorsed bills, approximately 27% are classified as "hot." Based on this evidence, it appears that the MapLight data cover the vast majority of the most salient bills considered by Congress.

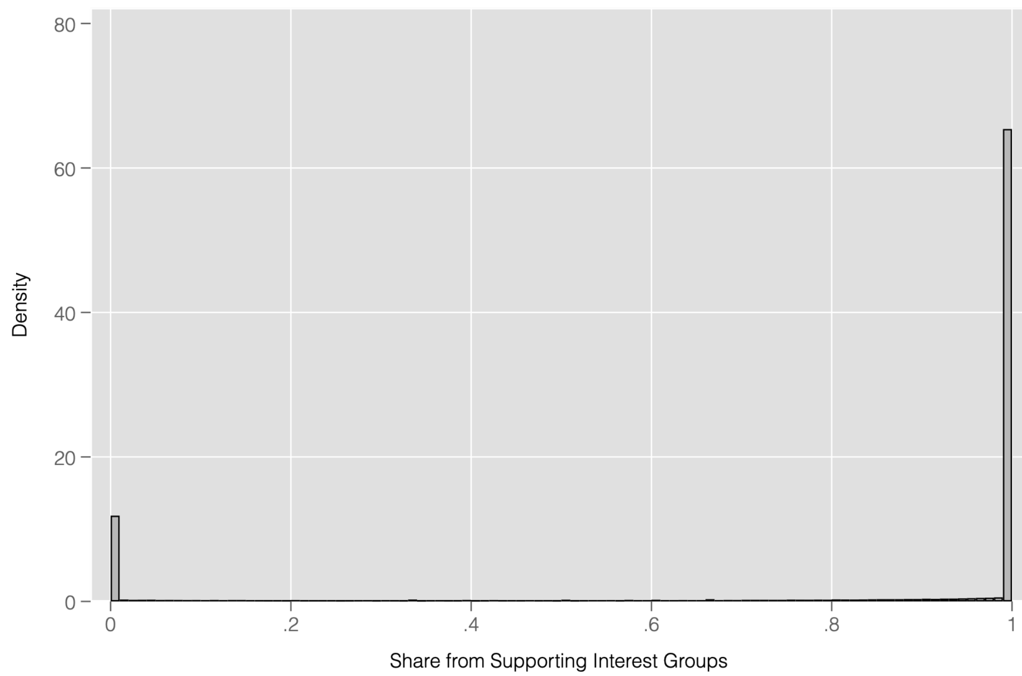
We say that a member of Congress votes with special interests if and only if her roll-call vote coincides with the bill-specific position of the special interest groups that donated to her campaign. If she received donations from supporting as well as opposing groups, then we classify her as supporting special interests if she votes with the side that gave more. As the histogram in Figure 2 illustrates, conditional on a legislator having received any money from groups taking a public stand on the bill, there is typically little ambiguity in whether her vote aligned with the position of her donors.[6]

Conditional on receiving any money, representatives collect about forty-three thousand dollars more from groups on the dominant side. In a given election cycle, the average representative in our data receives about $670,000 in contributions from individuals and approximately $650,000 in donations from political action committees (PACs). Thus, on average, the contributions from groups that took a position on a particular bill amount to nearly 7% of all receipts from PACs and to roughly 3% of donations from all sources combined.

Note, since it is not clear what it means to support the position of one's donors if none of them actually take a position on the bill in question, our congressperson-by-bill measure of interest group support is not defined for about 29% of roll-call votes in the data. Unsurprisingly, the vast majority of bills passed in Congress do not have interest groups announcing public positions according to the Maplight data. Most bills are not

[6]In 77.3% of cases, more than 99% of funds come from special interest groups on the same side of the issue; in 90.3% of cases, more than 75% of funds come from special-interest groups on the same side of the issue.

Figure 2: Donations by Fraction of Money in Favor of Passage



Notes: Figure shows the distribution of donations from interest groups on both sides of an issue as a fraction of all contributions that a congressperson received from groups taking a public stand on the respective bill. Not shown are cases in which none of a representative's special interest donors took a position on the bill in question. These account for about 29% of bill-legislator combinations.

controversial. However, when it comes to the bills in our sample (i.e., legislation on which at least one interest groups takes a public position), the majority of legislators receive at least some money from position-taking special interests. Our main results restrict attention to votes that were cast by representatives who, based on the available data, might be beholden to special interests. As a robustness check, we present evidence that legislators who did not receive any money from position-taking interest groups do not vote differently if the vote happens during or immediately following a natural disaster.

Natural Disasters Following Eisensee and Strömberg (2007), we obtain data on natural disasters from the Centre for Research on the Epidemiology of Disasters (CRED). CRED maintains the EM-DAT database, which collects core information on the occurrence and effects of both natural and man-made disasters worldwide. For an adverse event to be recorded as a disaster in EM-DAT, it must satisfy at least one of the following criteria: 10 or more people dead, 100 or more people affected, an officially declared state of emergency, or a call for international assistance.

We limit our sample to sudden-onset domestic natural disasters that occurred between 2005 and 2017—the time frame covered by MapLight—and fall into the top tercile of events in terms of either the number of deaths, people affected, or damages. We focus on large domestic disasters because these events receive greater coverage by U.S. media and are, therefore, more likely to crowd out politics reporting than more minor incidents and foreign ones. All in all, we consider 200 disasters over a thirteen-year period.[7]

TV News Again, following Eisensee and Strömberg (2007), we use abstracts from the Vanderbilt Television News Archive (VTNA) to construct a measure of politics reporting on the nightly news. VTNA collects and archives daily recordings of the regularly scheduled evening newscasts on ABC, CBS, NBC (starting in 1968), as well as one hour per day from CNN (since 1995) and the Fox News Channel (since 2004). For each day and network, the archive strives to make available a short, human-generated abstract of every story that aired, including its duration.

In contrast to previous work, we cannot rely solely on keyword searches to classify content. Coverage of politics is complex and there are simply too many terms that may (or may not) be indicative of political content for this approach to be promising. We, therefore, use state-of-the-art machine learning as an alternative to keyword and rules-based approaches. Specifically, we leverage the prowess of IBM Watson to classify each news story in VTNA based on the provided summary.

Watson uses natural language processing and neural nets, among other methods, to extract concepts, entities, and sentiment from unstructured text. It also categorizes the content of the text according to an enhanced version of the IAB Quality Assurance Guidelines Taxonomy, which defines contextual categories that were originally designed to accurately and consistently describe the content of, say, a website or video clip, in order to facilitate better-targeted advertisements (Interactive Advertising Bureau 2013). Critical for our purposes, Watson's taxonomy contains a category for content related to "law, government, and politics." This high-level category contains several subcategories for which Watson returns confidence scores. Since Watson's subcategories are not mutually exclusive, we sum the confidence scores and compare them to the judgments of a human coder on a test set of 1,000 randomly drawn news segments. Under the assumption that the human coder correctly classifies news stories, we then determine a threshold score that balances type-I and type-II errors on this test set.

[7]Our main result remains qualitatively unchanged if we also include large foreign disasters or all domestic events recorded in EM-DAT (cf. Appendix Table A.4).

Given an accuracy of 91.6% and a false positive (negative) rate of 7.7% (11.1%), our automated detection of political content performs well—though it is certainly not perfect (see Appendix Table A.1).

With our automated classification of news segments in hand, we measure politics coverage by network n on day t as the fraction of total airtime the newscast devoted to political matters. In symbols:

$$(1) \qquad News_{n,t} \equiv \Big(\sum_{s \in P_{n,t}} Duration_s\Big) / \Big(\sum_{s \in S_{n,t}} Duration_s\Big),$$

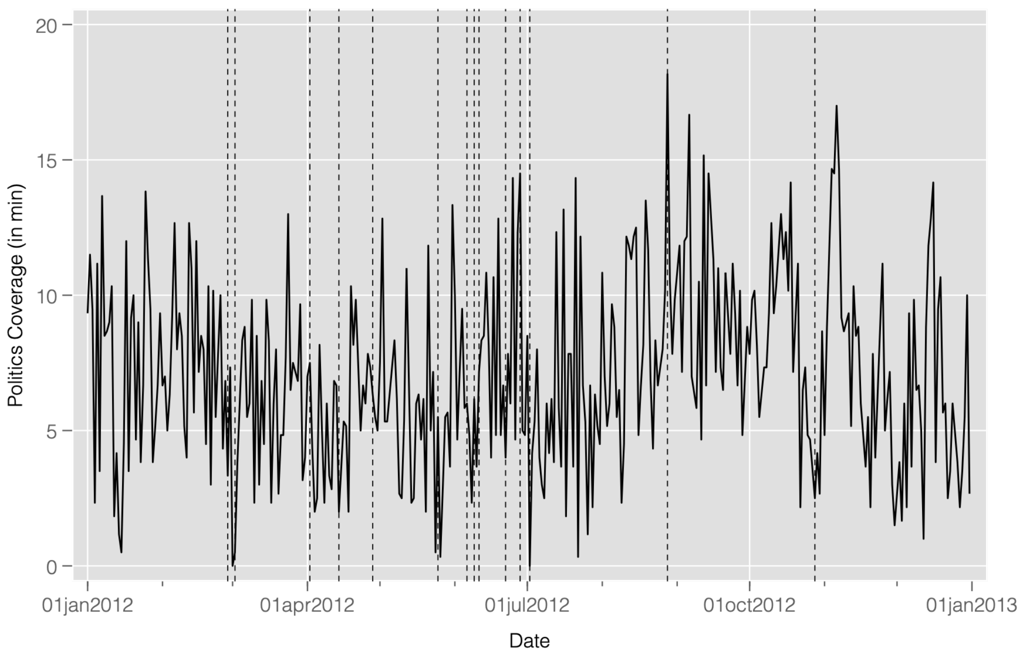
where $P_{n,t}$ denotes the set of news segments that are deemed to contain political content and $S_{n,t}$ is the set of all segments, including commercials.

According to our measure, on an average day, the median network contained in VTNA spends about 29% of airtime reporting on political issues. More importantly, our measure appears to capture meaningful variation. Consider, for example, Figure 3, which plots the nightly duration of politics coverage on the ABC evening news during 2012 (thick line), superimposing the start dates of natural disasters (dashed lines). Several patterns stand out. First, there is substantial high-frequency variation in politics coverage on the evening news. While some of that variation is undoubtedly measurement error, we find it reassuring that many of the local peaks occur around the same time as significant political events, such as the Republican National Convention (August 27–30), the (vice-)presidential debates (October 3, 11, 16, and 22), and Election Day (November 6).[8] Second, although we already restrict attention to nontrivial disasters, adverse events like floods, tornadoes, or hurricanes, are not terribly rare. Third, many, but by no means all, of the disasters in our data coincide with temporary lows in politics reporting. For instance, landfall of Superstorm Sandy on October 29 coincided with next-day politics coverage roughly 4.4 minutes, or about 62%, below normal—even though the presidential election was little more than a week away.

To verify that changes in politics coverage coincide with changes in disaster-related reporting, we also use Watson to detect news coverage of disasters. One concern about the VTNA data is that news reports on cable channels tend to be different in both scale and content from those on the evening news of the "big three" broadcast networks. As a result, news segments on the former may be less representative of the content to which most Americans are actually exposed. To deal with this potential issue, the

[8]Coincidentally, the RNC occurred during the landfall of hurricane Isaac.

Figure 3: Politics Reporting on the ABC Evening News in 2012



Notes: Figure shows the duration of politics coverage (in minutes) on the ABC evening news in 2012. Dashed vertical lines indicate the onset of natural disasters, as reported in EM-DAT. For a detailed description of the underlying data, see the Data Appendix.

analysis below restricts attention to news reports on ABC, CBS, and NBC.[9] We further note that, in 2014, VTNA stopped producing human-generated summaries of stories from weekday newscasts on CBS and NBC, which results in an unbalanced panel. We address this potential problem by adding network-specific day-of-the-week fixed effects to the relevant econometric models. The results below thus control nonparametrically for the idiosyncrasies of the subset of networks for which VTNA provides information on news content on a particular day.

In addition, we develop a method to adjust our regression estimates for measurement error in the left-hand side variable, i.e., $News_{n,t}$. Inevitably, using machine learning to classify news segments involves both type-I and type-II errors. Since the measured variable is binary, this type of error is necessarily nonclassical and it can be shown to introduce attenuation bias—even if disasters do not directly affect error rates (cf. Appendix C). Since the implied correction leaves our conclusions qualitatively unchanged, we relegate this part of the analysis to the appendix and present the more-

[9]In the appendix, we show that our conclusions remain unaffected if we include data from all channels contained in VTNA (cf. Appendix Table A.3.)

straightforward, unadjusted estimates in the main text. Here, we merely note that our method for debiasing the OLS coefficients may also be useful in other applications that use machine learning to measure outcomes.

Newspaper Mentions Although politics coverage on the evening news is useful to gauge overall media attention to politics, national broadcast news rarely report on rank-and-file congresspeople. Nonetheless, members of Congress are concerned with how they are perceived. We, therefore, complement our daily measure of politics reporting with a second one that focuses on media mentions of individual representatives. Specifically, for each congressperson, we search the NewsLibrary database for articles from in-state newspapers that mention her by name. We then use this information to construct a daily panel of mentions of individual congresspeople in local newspapers.

The NewsLibrary database indexes more than 6,500 newspapers from all around the United States. In these data, the average representative is mentioned in about one article per day. Though the distribution of mentions is right-skewed as higher profile members get mentioned more, on an average day, the median representative gets mentioned in two-thirds of an article. Unfortunately, the number of news sources in the database varies significantly by region as well as over time. Although there is little reason to believe that indexing decisions would be correlated with the occurrence of natural disasters, we address any potential issues due to changes in panel composition by controlling for legislator-specific year-by-month fixed effects in all models with newspaper mentions on the left-hand side.[10]

Google Searches To directly gauge citizen (rather than media) attention to politics, we rely on the daily volume of Google searches for the following terms: "politics," "Congress," "Congressman," "Representative," "government," "House of Representatives," and "vote." Focusing on the time period covered by MapLight, we downloaded these data from Google's Trends tool and standardize the daily time series for each term. To measure disaster-related searches, we proceed in analogous fashion, focusing on the following set of terms: "disaster," "volcano," "earthquake," "flood," "landslide," "storm," "hurricane," "blizzard," and "tornado."

Table 1 presents descriptive statistics for the most important variables in our analysis. A few facts are worthy of note. Disasters account for 4% of days over the time period spanned by our data. On average, special-interest groups that support a bill give a total of about $12 million to legislators, while those that oppose the measure

[10]Without these fixed effects, the estimated impact of disasters is slightly larger.

Table 1: Descriptive Statistics

Variable	Mean	SD	Min	Median	Max
Daily Time Series (N = 4,497):					
Disaster	.043	.204	0	0	1
Share Politics Reporting on Median Network	.290	.115	0	.291	.885
Share Disaster Reporting on Median Network	.068	.105	0	.035	.699
Web Search Index for Median Congress-Related Term	21.02	8.92	0	19.95	83.00
Web Search Index for Median Disaster-Related Term	11.10	2.97	0	11.00	38.00
Legislator Level (N = 872):					
Democrat	.448	.498	0	0	1
Republican	.550	.498	0	1	1
Number of Votes with Position-Taking Donors	564	384	0	518	1,385
Number of Newspaper Mentions on Average Day	.980	.915	.071	.667	8.789
Bill Level (N = 1,525):					
Number of Passage Votes	1.05	.23	1	1	3
Number of Supporting SIGs	13.5	29.6	0	3	507
Number of Opposed SIGs	6.8	17.5	0	0	196
Total Contributions by Supp. Groups (in $100,000)	120	218	0	33	2,092
Total Contributions by Opp. Groups (in $100,000)	36	85	0	0	944
Vote Level (N = 674,726):					
Net Money from SIGs (in $100,000)	.304	.879	0	.05	52.19
Net Money from SIGs | Any Money (in $100,000)	.429	1.018	0	.128	52.19
Vote with Dominant Special Interest	.812	.391	0	1	1
Vote "Yea"	.845	.362	0	1	1
Abstain	.026	.160	0	0	1

Notes: Entries are descriptive statistics for the most important variables used throughout the analysis. For precise definitions of all variables, see the Data Appendix.

contribute approximately $3.6 million. To put these numbers in context, for the average bill with interest group support, supportive interest groups together account for around 4% of total campaign contributions. The same number for opposing interest groups is around 1.2%. Of the 1,525 bills in our data, 186 were voted on the day of or within two days after a disaster.

4. Disasters and Roll-Call Votes

Our empirical analysis proceeds in three steps. In the current section, we document that following natural disasters, legislators align more closely with their special interest donors. In the next section, we provide evidence that disasters crowd out news reports on politics. Finally, in Section 6, we explore different mechanisms that might explain the simultaneous decline in attention to politics and the realignment of roll-call votes towards special interest donors.

4.1. *Randomization Inference*

We begin our analysis by using randomization inference as a simple and transparent way to test the sharp null hypothesis of no effect of disasters on legislators' votes. Holding both the sample time period and the actual number of disasters in our data fixed, we randomly draw a new start date for each event. We then restrict ourselves to legislators and votes for which the politician received money from a position-taking special interest group, and estimate the following linear probability model:

$$SIV_{l,r,t} = \alpha + \beta_{\text{pre}} Disaster_t^{(-3,-2,-1)} + \beta_{\text{post}} Disaster_t^{(0,1,2)} + \varepsilon_{l,r,t}, \tag{2}$$

where $SIV_{l,r,t}$ is equal to one if and only if legislator l's vote on roll call r on date t aligns with the position of the interest groups that gave the most money to her campaign. $Disaster_t^{(0,1,2)}$ is an indicator for whether date t falls on or within two days after the onset of a disaster, while $Disaster_t^{(-3,-2,-1)}$ denotes an indicator for the three-day window prior to a disaster. The coefficients of interest are β_{pre} and β_{post}. The former captures differences in alignment relative to usual just before a disaster strikes, while the latter measures differences in alignment shortly afterwards.

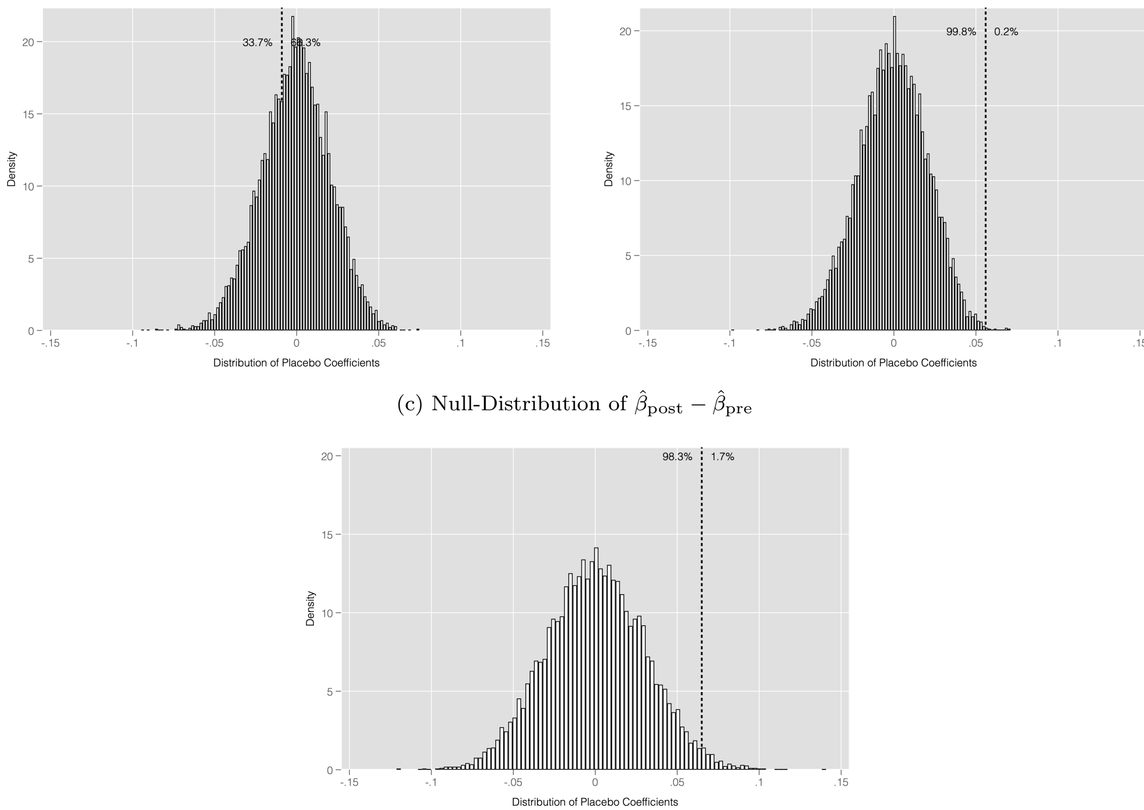
Repeating this procedure 10,000 times, Figure 4 presents both the true values of $\hat{\beta}_{\text{pre}}$ and $\hat{\beta}_{\text{post}}$ in our data (vertical lines) as well as their distributions under the sharp null hypothesis of not effect of disasters. The evidence in panel (a) implies that, for votes that are cast just before a disaster strikes, we cannot reject the null. On these days, congresspeople are statistically no more or less likely to vote with special interests than they usually are. For votes on days just after the onset of a disaster, however, panel (b) shows that the true point estimate exceeds 99.8% of placebo coefficients. For completeness, panel (c) demonstrates that the observed *difference* between $\hat{\beta}_{\text{post}}$ and $\hat{\beta}_{\text{pre}}$ is also very unlikely to arise by chance. Based on the evidence in Figure 4, we conclude that, in the days just after a natural disaster strikes, congresspeople are significantly more likely to support the positions of their special interest donors. They are significantly more likely to do so both relative to their usual level of support and relative to that just prior to the onset of a disaster.

Table 2 probes the robustness of this finding. The results in this table are based on the regression model in eq. (2), controlling for different sets of fixed effects. Col. (1) simply replicates the findings in Figure 2. We notice a number of important points. First, the baseline rate of alignment with a contributing SIG is 80.7%. Second, the rate of alignment decreases to 79.8% in the three days before the onset of a disaster but is

Figure 4: Randomization Inference

(a) Null-Distribution of $\hat{\beta}_{\text{pre}}$ (b) Null-Distribution of $\hat{\beta}_{\text{post}}$

(c) Null-Distribution of $\hat{\beta}_{\text{post}} - \hat{\beta}_{\text{pre}}$



Notes: Figure shows the distribution of β_{pre} and β_{post} in eq. (2), as well as their difference under the sharp null that disasters have no effect on legislators' votes. Dashed vertical lines indicate the size of the actual estimates based on our data. All panels are based on 10,000 regressions, with randomly reshuffled start dates of disasters.

not statistically distinguishable from baseline. Third, the rate of alignment is 86.2% in the the three days following a disaster, which is statistically distinguishable from baseline at the 1%-level. Fourth, the standard errors on both coefficients are nearly equally large. The difference in statistical significance is solely due to $\hat{\beta}_{\text{post}}$ being five to six times larger than $\hat{\beta}_{\text{pre}}$.

Cols. (2) and (3) account for potential seasonality in disasters and roll-call votes by respectively controlling for month and year-by-month fixed effects. In order to address the potential concern that important votes might be disproportionately held on particular days of the week and that the EM-DAT data might systematically over- or under-report disasters on such days, col. (4) additionally adds day-of-the-week fixed effects to account for selectivity of types of bills as well as legislator presence in Congress by day of the week. Cols. (5) and (6) further account for legislator and legislator-by-

Table 2: Natural Disasters and Support for Special Interests

	Vote with Special-Interest Donors					
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate Aftermath of Disaster (β_{post})	.056***	.059***	.068***	.061***	.061***	.060***
	(.019)	(.019)	(.020)	(.021)	(.021)	(.022)
Immediately Before Disaster (β_{pre})	-.009	-.009	.002	.008	.008	.008
	(.020)	(.020)	(.022)	(.019)	(.019)	(.019)
Constant	.807***					
	(.008)					
Hypothesis Tests [p-values]:						
H_0: $\beta_{\text{post}} = \beta_{\text{pre}}$	.023	.018	.024	.038	.037	.038
Fixed Effects:						
Month	No	Yes	No	No	No	No
Year × Month	No	No	Yes	Yes	Yes	Yes
Day of the Week	No	No	No	Yes	Yes	Yes
Legislator	No	No	No	No	Yes	No
Legislator × Congress	No	No	No	No	No	Yes
R-Squared	.002	.006	.041	.059	.067	.085
Number of Observations	478,946	478,946	478,946	478,946	478,946	478,946

Notes: Entries are OLS point estimates estimating the model in eq. (2) by OLS. "Immediate Aftermath of Disaster" is defined as an indicator equal to one if and only if the roll call occurs on the day of the disaster or within two days after its reported onset; and "Immediately Before Disaster" is an indicator for the three-day time window just before the disaster strikes. Standard errors are reported in parentheses and are two-way clustered by legislator and year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively. For a detailed description of the underlying data, see the Data Appendix.

Congress fixed effects in order to control for changes in the partisanship of legislators and the types of bills that are introduced in a particular Congress. Consistent with the idea that disasters are plausibly exogenous, the point estimates in Table 2 are stable, with t-statistics close to three. Moreover, for each specification, we test and reject the null hypothesis that legislators' support for special interests is the same just before and after disasters strike. The evidence in this table thus implies that our main result is qualitatively and quantitatively robust to controlling for seasonality, day-of-the week, legislator, and even legislator-by-Congress fixed effects.

One potential concern with the above estimates is that MapLight relies in part on news databases to identify the interest groups that took a position on a particular bill. If disasters do crowd out politics reporting and if interest groups do not reveal their positions until just before the issue comes up for a vote, then the MapLight data might undercount the number of interest groups that took a position on bills that were voted upon directly after a disaster. In practice, however, this issue is likely minor. On average, interest groups' positions were made public 47 days before the vote (median = 49 days), and MapLight identified these groups before the day of the vote in 87% of cases. Moreover, in Section 6 we empirically show that the number of position-taking interest groups reported by MapLight as well as their average donations do not significantly vary with the onset of disasters.

One might also worry that interest groups that support a bill are more vocal and more easily identifiable than those who oppose it. If correct, then the MapLight data might systematically understate total campaign donations from opposing groups, which means that the outcome variable in eq. (2) could be upward biased. Note, however, that for bias in the outcome variable to translate into biased estimates of the reduced-form effect of disasters, it would need to be the case that the bias in the outcome is systematically correlated with the precise timing of disasters.[11] Although we cannot categorically rule out this possibility, we are not aware of any reasons for why this would be the case.

4.2. *Event-Study Evidence*

We now turn to a simple event study framework in order to examine the dynamic impact of disasters on alignment with special interests. Our estimating equation is given by:

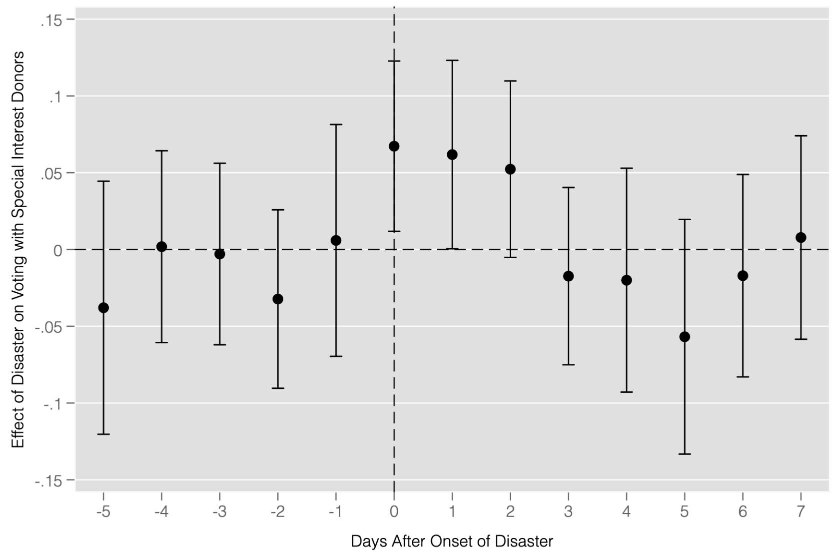
$$SIV_{l,r,t} = \sum_{s \in W} \varphi_{t+s} Disaster_{t+s} + \kappa_m + \mu_l + \epsilon_{l,r,t}, \tag{3}$$

where $SIV_{l,r,t}$ is an indicator variable equal to one if and only if legislator l's vote on roll call r aligns with the interest groups' position that gave the most money to her campaign on day t, $Disaster_t$ is an indicator for the start date of a disaster, W denotes the event window, and κ_m and μ_l are month-by-year and legislator-by-Congress fixed effects, respectively.

The specification in eq. (3) differs from a standard event-study model in that we do not restrict attention only to votes that occur within the event window and that we do not normalize the coefficient for $t = -1$ to zero. Instead, we estimate eq. (3) on the full sample of votes that might be subject to influence from special interests (i.e., the same

[11]To see this, let $\widetilde{SIV}_{l,r,t}$ denote the true value of the outcome, so that $\widetilde{SIV}_{l,r,t} = SIV_{l,r,t} + \xi_{l,r,t}$, with $SIV_{l,r,t}$ corresponding to what we observe in the MapLight data. Eq. (2) thus becomes: $SIV_{l,r,t} = \alpha + \beta_{\text{pre}} Disaster_t^{(-3,-2,-1)} + \beta_{\text{post}} Disaster_t^{(0,1,2)} - \xi_{l,r,t} + \varepsilon_{l,r,t}$. Since $\xi_{l,r,t}$ is part of the error term, a standard omitted variables bias argument applies.

Figure 5: Event-Study Estimates of the Effect of Disasters on Alignment with Special Interests



Notes: Figure shows point estimates and 95%-confidence intervals for the impact of natural disasters on congresspeople's tendency to vote with their special-interest donors, i.e., φ_t in eq. (3). Estimates control for legislator-by-Congress and year-month fixed effects. Confidence intervals account for two-way clustering by legislator and year-month.

set of votes as in Table 2), which means that votes outside of the event window serve the omitted category.

The reason we do not limit our event study solely to the days surrounding natural disasters is that there are many days without any roll calls in the House or without roll calls on which special interests took a position. This is because Congress might be out of session, legislation is rarely voted upon on Mondays, Fridays, and weekends, or simply because there are many fewer important bills to be passed than there are days in the legislative calendar. As a consequence, it is not feasible to compare a legislator's alignment with special interests just before and just after the same event without dramatically reducing the effective sample size. We note, however, that as long as the precise timing of disasters is as good as random, our estimates of φ_t can still be interpreted as changes in legislators' alignment with her special interest donors *relative to usual*.

As we see in Figure 5, there is no pre-trend in alignment in the five days leading up to a disaster. All five leads are statistically insignificant and three of them are essentially zero in magnitude. Starting on the day of the disaster, alignment with special interests jumps to more than five percentage points higher than usual. The jump slightly declines over the next two days, but it remains statistically significant or marginally so. Out of the thirteen point estimates in Figure 5, the three largest ones pertain to the day of the disaster and the two following ones. Assuming independence, if disasters had no effect on votes then the probability that precisely the three days after onset are associated with the three highest estimates is less than 1%.[12] After three days, alignment rates drop to just below average and are statistically indistinguishable from usual.[13]

In summary, the results in this section imply that, following natural disasters, legislators increase their alignment with their special interest donors for a period of three days. In the next section, we show that natural disasters crowd out information about politics for roughly the same period of time.

5. Disasters and Attention to Politics

In order to study disaster-induced crowd-out of attention, we build on the event-study framework above, but replace our left-hand side variables. Figure 6 begins by focusing on news coverage of disasters themselves. The estimates show newscasts airing more disaster-related content in the days leading up to the event, with a peak one day after its onset. Given that disasters like major storms can often be anticipated a few days in advance, the gradual increase in disaster reporting should not be surprising.

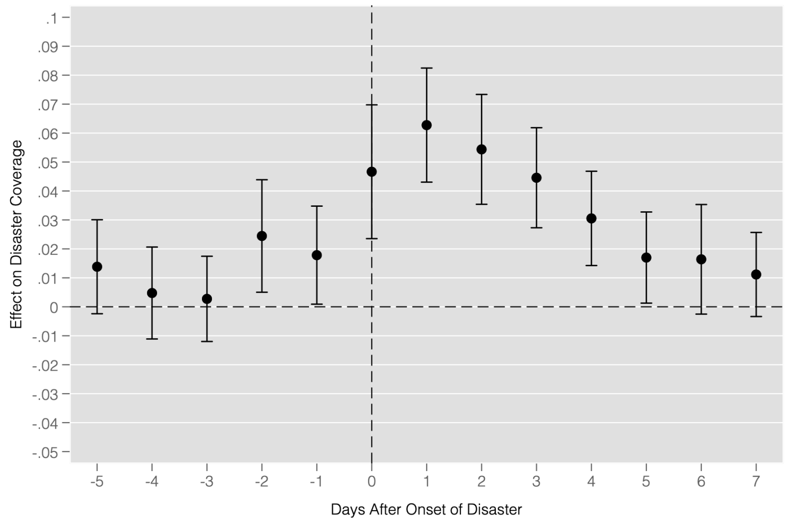
Figure 7 examines the impact on coverage of politics. There is little evidence of crowd-out before the disaster occurs, suggesting that non-political content gets displaced first. On the day of the event, however, we do find a significant reduction in politics reporting. The relevant point estimate equals about 2% of total air time, which amounts to approximately 7% of the time devoted to politics on an average day.[14] The disaster-induced crowd-out lasts for a total of three days—the same time length for which we observe greater alignment between congresspeople and their special-interest donors—after which politics coverage on the evening news returns to normal.

[12] There are thirteen days and the first three after the disaster are the three largest. There are 13! orderings of days but only 3! ways of arranging the the first three days and 10! ways of arranging the ten other days. This leads to a probability of $\frac{10!3!}{13!} \approx 0.003$.

[13] As a robustness check, we have estimated eq. (3) dropping all votes that that fall into the event window of more than one disaster. The results are noisier but qualitatively robust, and are presented in Appendix Figure A.1.

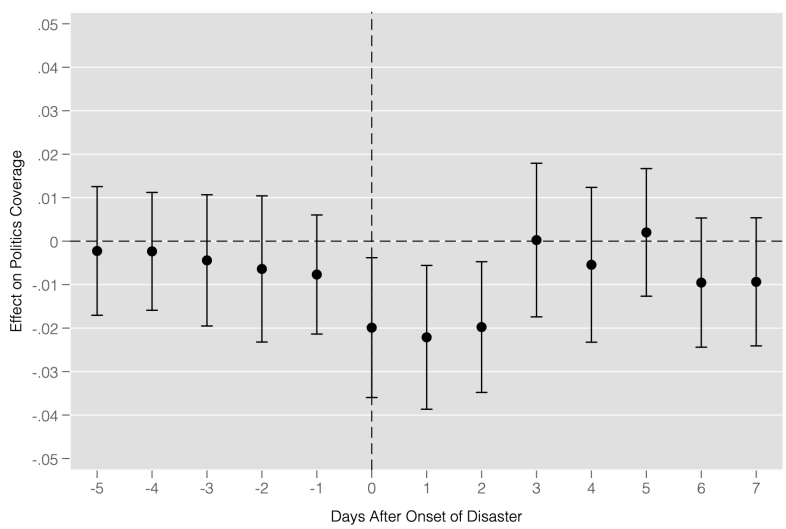
[14] Recall, given non-classical measurement error in the dependent variable, these numbers likely understate the true effect size.

Figure 6: Disaster Coverage on the Evening News



Notes: Figure displays point estimates and 95%-confidence intervals for the impact of natural disasters on disaster-related reporting on the evening news. Estimates are based on regression models analogous to eq. (3), controlling for year-by-month and network-specific day-of-the-week fixed effects in order to account for the unbalanced nature of the VTNA database. Confidence intervals account for clustering by year-month. For estimates that correct for measurement error introduced through the machine-learning classifier, see the Online Appendix.

Figure 7: Crowd-Out of Political Content in the Evening News



Notes: Figure displays point estimates and 95%-confidence intervals for the impact of natural disasters on politics reporting on the evening news. Estimates are based on regression models analogous to eq. (3), controlling for year-by-month and network-specific day-of-the-week fixed effects in order to account for the unbalanced nature of the VTNA database. Confidence intervals account for clustering by year-month. For estimates that correct for measurement error introduced through the machine-learning classifier, see the Online Appendix.

In the Appendix, we present a series of robustness checks, some of which rely on data from the universe of channels included in the VTNA. All estimates imply a temporary decrease in politics reporting, which is statistically significant at either the 5%- or 1%-level (cf. Appendix Table A.3).

Appendix Figure A.2 displays estimates of the impact of disasters on newspaper reports about local congresspeople. Although these results are less precise than the previous ones, there is a statistically significant reduction on the first day after the event. Corresponding to nearly 5% of the sample mean, the estimated effect is nontrivial in size. Interestingly, point estimates a few days before and after onset are positive and about equally large (but not statistically distinguishable from zero). Members of Congress are mentioned in the press for many reasons unrelated to contemporaneous policy. Some may even be mentioned as a result of a disaster—say, because of grandstanding. Nonetheless, we see that three of the four days with the lowest estimated coefficients are the day of the disaster and the two following ones. Assuming independence, such a pattern would arise by chance with slightly less than a 2% probability.[15] Despite the evidence being weaker than that with respect to news broadcasts, our findings suggest that local newspapers pay less attention to congresspeople right after disasters strike.

The evidence in Figure 7 and Appendix Figure A.2 pertain to the national and local media, respectively. Although it is more difficult to measure changes in attention among ordinary citizens, Appendix Figures A.3 and A.4 attempt to do so using data from Google Trends. Specifically, Appendix Figure A.3 shows estimated effects on the volume of disaster-related Google searches, and Appendix Figure A.4 shows effects on the volume of Congress-related searches. The former begin to gradually increase a few days before a disaster strikes, and they peak on the day following the event. Afterwards, the number of disaster-related searches declines monotonically.

The results on Congress-related searches are noisier but qualitatively similar to those in the previous figure. The imprecision of the point estimates in Appendix Figure A.4 notwithstanding, it is possible to reject the null hypothesis that they are jointly equal to zero in the week following the event ($p = .011$). Moreover, three of the five days with the lowest search volume are the three days right after onset. The day with the third lowest number of searches is the day just before the event. Overall, our findings suggest that disasters lead to a temporary reduction in how much attention both the

[15] There are thirteen days and the first three after the disaster are in the top four. There are 13! total orderings of days but only 4! ways of ordering the top four and 9! ways of ordering the remaining nine days. Moreover, there are 10 other possible days besides the three days following the disaster to make the top four. We, therefore, get a probability of $\frac{4!*10*9!}{13!} \approx 0.01$.

media as well as citizens pay to politics.

6. Potential Mechanisms

We now turn to potential mechanisms for the simultaneous decline in attention to politics and the realignment of roll-call votes towards special-interest donors. In particular, we consider the possibility that the change reflects: (1) strategic behavior of congressional elites in selecting which bills to put up for a vote, (2) a drop in funding and the resulting reorientation of legislator priorities, (3) greater party cohesion under reduced scrutiny, (4) increased willingness of legislators to pass bills in general—perhaps because bills are amended to include disaster relief funds—and (5) moral hazard on the part of legislators. We provide additional empirical evidence that ultimately points towards moral hazard as the mechanism by which special interests benefit in times of reduced scrutiny.

Agenda Setting The first mechanism we consider is agenda setting. By agenda setting we mean that the House leadership strategically brings up politically sensitive issues on days when natural disasters distract the public. If correct, then selection of bills might explain why these events coincide with increased support for special interests. In other words, moral hazard might manifest itself in the decisions of the leadership rather than those of rank-and-file members.

The first piece of evidence against agenda setting comes from the results in Table 3. The estimates therein come from regressing the outcome on the left of each row on an indicator for the immediate aftermath of a natural disaster (i.e., the day of the event and the two following days) as well as fixed effects to control for seasonality and day-of-the week effects. If the leadership tried to exploit temporary "windows of opportunity" in order to pass sensitive legislation, then one might expect to observe a flurry of activity after disasters strike. The estimates in the upper part of the table imply that this is not the case. In particular, we find no evidence that more words are being spoken on the House floor, nor are more roll calls being held. In fact, the relevant point estimates are negative and quantitatively small. Perhaps more importantly, the House *does not* become more likely to temporarily suspend its rules. Rule suspension would be a way for the leadership to circumvent procedural requirements and, thereby, speed up the legislative process.

Table 3: Agenda Setting?

Outcome	Effect of Disaster (1)	(2)
Congressional Speech:		
Number of Disaster-Related Words (in SD units)	-.077	.007
	(.077)	(.072)
Total Words Spoken on House Floor (in SD units)	-.023	.032
	(.083)	(.071)
Number of Roll Calls:		
Total Number of Roll Calls	-.332	-.265
	(.208)	(.169)
Number of Passage Votes	-.054	-.049
	(.068)	(.069)
Roll Calls on Amendments	-.123	-.056
	(.152)	(.120)
Procedural / Other Votes	-.156	-.160
	(.061)	(.061)
Number of Votes to Suspend the Rules /	.001	.004
under Suspended Rules	(.062)	(.064)
Roll-Call Issue:		
Agriculture	-.013**	-.016
	(.006)	(.011)
Defense	.027	.020
	(.034)	(.031)
Energy & Environment	-.027	-.051**
	(.017)	(.023)
Labor & Education	.016	.016
	(.016)	(.018)
Health	.020	.017
	(.015)	(.012)
Foreign Trade & International Affairs	-.002	-.001
	(.012)	(.016)
Domestic Commerce	-.002	.010
	(.014)	(.015)
Social Welfare & Housing	.012	.004
	(.013)	(.016)
Government Operations	.012	.011
	(.026)	(.023)
Other	-.043	-.009
	(.042)	(.019)
Involvement of Special Interests in Roll Call:		
Any Special Interests Take Position	-.024	-.029
	(.017)	(.018)
	-.495	-.257
	(3.244)	(3.345)
Average Total Donations per Group (in $100,000)	-.574	.805
	(1.702)	(1.884)
Fixed Effects:		
Year × Month	No	Yes
Day of the Week	No	Yes

Notes: Entries in column (1) are point estimates and standard errors from regressing the outcome on the left of each row on an indicator for the immediate aftermath of a natural disaster, i.e., the day of the event and the two following days. The estimates in column (2) additionally control for year-month and day-of-the week fixed effects. Standard errors are reported in parentheses and are clustered by year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3 additionally shows that bills that are voted on in the immediate aftermath of disasters are *not* systematically more important to special-interest groups than those that are considered on ordinary days. Bills during and in the direct aftermath of a natural disaster are neither more likely to have *any* interest group associated with them nor are there a higher number of position-taking interest groups. Position-taking interest groups also *do not* donate more money.

The second piece of evidence against the agenda-setting mechanism comes from committee discharge dates. To receive a passage vote in the House, a bill must have been formally reported on by all relevant committees, or it must have been brought to the floor by means of a discharge petition. If the leadership strategically scheduled certain votes to occur in the aftermath of disasters, then one might expect differences in the distribution of how long bills have been out of committee.

Of course, it is possible that leadership quickens the the length of time to final vote for some bills and simultaneously brings up difficult to pass bills which have been waiting for a vote for a long time. In this case, bills coming up for a vote in the aftermath of a disaster might on average have been out of committee for the same length of time as during normal times but might combine bills out for both a longer and a shorter time period than usual. Relying on information on discharge dates provided by MapLight, we count the number of days until a final passage vote is held and compare the CDFs for bills that were and were not considered in the immediate aftermath of a disaster. Appendix Figure A.5 depicts the result. Based on this figure as well as a formal Kolmogorov-Smirnov test for equality of distributions, we conclude that there are no material differences in the timing of bills.

In sum, we do not find any evidence of agenda setting in connection with natural disasters. Based on the results above, we conclude that moral hazard on part of the leadership is unlikely to be an important mechanism behind our main result.

Economic Damages Another potential explanation for why we observe increased support for special interests is that disasters may make it harder for legislators to raise money from their constituents, which might necessitate a shift in fundraising strategies toward large donors. If such a shift is accompanied by greater reliance on quid pro quos, then disasters' economic fallout may drive our findings.

One reason to doubt this explanation is that the effect of disasters on votes disappears after a couple of days, when attention to politics returns to normal. In our view, any kind of medium- to long-run mechanism is difficult to square with this particular pattern in the data.

Table 4: Alternative Outcomes and Samples

	Vote with Special Interests				Partly-Line Vote		Abstention	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Immediate Aftermath of Disaster	.055***	.063***	.065***	.064***	.007	.000	.002	.002
	(.020)	(.022)	(.020)	(.022)	(.006)	(.006)	(.003)	(.003)
Fixed Effects:								
Year × Month	No	Yes	No	Yes	No	Yes	No	Yes
Day of the Week	No	Yes	No	Yes	No	Yes	No	Yes
Legislator × Congress	No	Yes	No	Yes	No	Yes	No	Yes
Sample	Excl. Repr. from Affected States		Excl. Bills Related to Aid & Relief		No Restriction		No Restriction	
R-Squared	.002	.087	.003	.091	.000	.079	.000	.079
Number of Observations	441,547	441,547	417,072	417,072	674,553	674,553	693,062	693,062

Notes: Entries are OLS point estimates and standard errors on the effect of natural disasters on whether representatives' votes align with the positions of their special-interest donors (columns (1)–(4)), with the the majority of their co-partisans (columns (5)–(6)), and on abstentions (columns (7)–(8)). "Immediate Aftermath of Disaster" is defined as an indicator equal to one if and only if the roll call occurs within two days after the reported onset of the disaster. Standard errors are reported in parentheses and are two-way clustered by legislator and year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

The first two columns in Table 4 provide additional evidence against mechanisms operating through direct economic effects. Specifically, we find that natural disasters appear to increase legislator support for special interests even among representatives from states that were not directly impacted by the event. In light of this observation, we discount economic damages as a plausible channel.

Party Pressure and Strategic Abstention A third set of potential mechanisms operates through changes in party pressure and abstention. For instance, a disaster might affect the ability of party elites to influence the decisions of rank-and-file members, or it may affect individual legislators' willingness to cast a recorded vote.

To test these explanations, we examine abstention rates and party-line votes as outcomes. The evidence in the four right-most columns of Table 4, however, suggests that disasters do not meaningfully affect either outcome. In other words, there appears to be virtually no change in abstention and voting along party lines. In connection with our main result, the latter finding implies that some legislators break from their party when they tilt their votes to support special interests, while others become aligned.

Disaster Relief A fourth explanation for our finding of increased support for special interests might be that disasters induce last-minute changes to the content of bills. In this context, it is important to distinguish between changes or amendments that are caused by a temporary reduction in scrutiny and are designed to please special interests, and more innocuous changes, which could be spuriously correlated with the positions of large donors.

Perhaps the most likely scenario along these lines is that an amendment providing disaster relief gets attached to a measure that is already scheduled for a vote. Such an amendment would likely increase how palatable the overall package is; and, since most special-interest money is given in support of bills, it may make it seem as if representatives pivot towards the positions of their donors. We address this possibility in five ways.

First, we search the Congressional Record for mentions of "disaster," "emergency," "relief," "help," "rebuild," "assistance," "victim," "storm," "hurricane," "tornado," "flood," "landslide," "earthquake, and "volcano" in order to explore whether legislators become more likely to discuss disasters and potential relief efforts in the immediate aftermath of the event. As shown in the top row of Table 3, the answer turns out to be "no."

Second, we ask whether the House votes on more amendments around the time a disaster strikes. Again, the answer is "no" (cf. Table 3).

Third, we re-estimate our workhorse empirical model excluding all bills whose title or description by the Congressional Research Service contains any of the above keywords. The results in columns (3) and (4) of Table 3 show that, if anything, our main result becomes slightly stronger.

Fourth, we assess whether disasters *uniformly* increase legislator support for bills. If disaster relief is added to bills, then we would expect to find a general increase in "yea"-votes. We should not see an increase in "nay"-votes for congresspeople whose special-interests donors are opposed to the bill. This logic motivates the following econometric specification where we separately estimate the effect of natural disaster for interest groups who favor passage and for interest groups who favor defeat:

$$\begin{aligned} Yea_{l,r,t} = {} & \beta^{(+)} Money^{(+)}_{l,r,t} + \beta^{(-)} Money^{(-)}_{l,r,t} + \\ & \gamma^{(+)} Money^{(+)}_{l,r,t} \times Disaster^{(0,1,2)}_t + \gamma^{(-)} Money^{(-)}_{l,r,t} \times Disaster^{(0,1,2)}_t + \\ & \delta Disaster^{(0,1,2)}_t + \kappa_m + \mu_l + \varepsilon_{l,r,t}. \end{aligned} \tag{4}$$

Here, $Yea_{l,r,t}$ is an indicator equal to one if and only if legislator l votes "yea" on roll-call r, while $Money^{(+)}_{l,r,t}$ and $Money^{(-)}_{l,r,t}$ denote the contributions she received from interest groups that support and oppose the bill, respectively. $Disaster^{(0,1,2)}_t$ is an indicator for whether the roll call occurred within two days after a disaster. If the effect of disasters on votes operates solely through relief amendments or other last-minute changes that make the bill more attractive, then we would expect that $\hat{\delta} > 0$, while $\hat{\gamma}^{(+)} = \hat{\gamma}^{(-)} = 0$.

Table 5 presents results from estimating variants of eq. (4) on our data. Although all estimates of δ are positive, they are statistically indistinguishable from zero and only about one-third the size of the reduced-form effect we found in Table 2. Perhaps more importantly, with p-values ranging from .001 to .033, we can reject the null hypothesis that $\hat{\gamma}^{(+)} = \hat{\gamma}^{(-)} = 0$. In other words, legislators do not become generally more supportive of bill passage. Instead, legislators whose donors support the bill become more likely to vote "yea," while representatives whose donors oppose the measure become more likely to vote "nay." This kind of bifurcation is inconsistent with explanations that are predicated upon bills becoming generally more palatable after disasters strike.[16]

[16]Also, note that the model in eq. (4) does not require us to restrict attention to votes cast by representatives whose special-interest donors took a position on the bill. By considering all votes, the results in Table 5 demonstrate that our main result is not an artifact of the sample restrictions that are inherent to inherent to our measure of interest-group support.

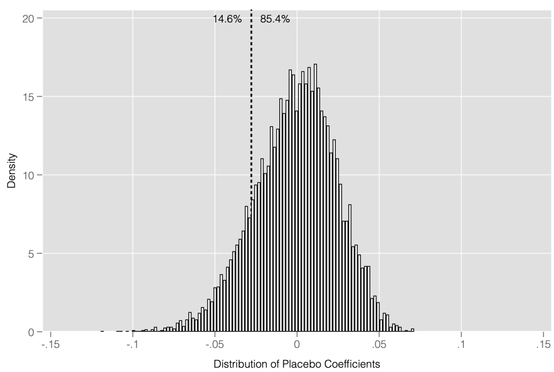
Table 5: Bifurcation

	Vote "Yea" on Passage			
	(1)	(2)	(3)	(4)
Money from Supporting Interest Groups $\beta^{(+)}$	.020***	.018***	-.004	.008**
	(.004)	(.004)	(.003)	(.004)
Money from Opposed Interest Groups $\beta^{(-)}$	-.181***	-.175***	-.157***	-.127***
	(.025)	(.024)	(.022)	(.018)
Money from Supporting Interest Groups		.006	.014**	.012
$\times$ Immediate Aftermath of Disaster ($\gamma^{(+)}$)		(.007)	(.006)	(.008)
Money from Opposing Interest Groups		-.067***	-.053***	-.032**
$\times$ Immediate Aftermath of Disaster ($\gamma^{(-)}$)		(.017)	(.017)	(.014)
Immediate Aftermath of Disaster (δ)	.019	.022	.015	.021
	(.019)	(.020)	(.015)	(.018)
Hypothesis Tests [p-values]:				
H_0: $\gamma^{(+)} \leq 0$	–	.181	.014	.068
H_1: $\gamma^{(-)} \geq 0$	–	.000	.001	.009
H_2: $\gamma^{(+)} = \gamma^{(+)} = 0$	–	.001	.001	.033
Fixed Effects:				
Legislator $\times$ Congress	No	No	Yes	Yes
Year $\times$ Month	No	No	No	Yes
Day of the Week	No	No	No	Yes
R-Squared	.046	.047	.238	.315
Number of Observations	674,726	674,726	674,726	674,726

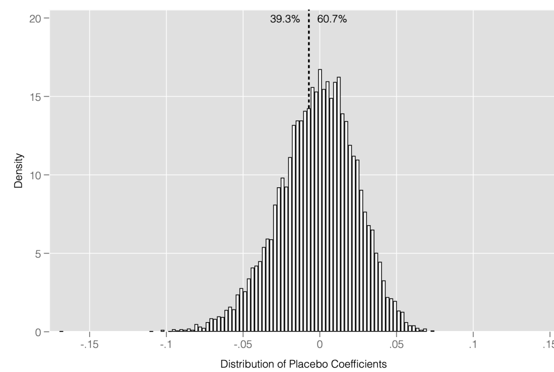
Notes: Entries are coefficients and standard errors from estimating variants of the empirical model in eq. (4) by OLS. Interest-group donations have been scaled so that the respective coefficient refers to the change in the probability of voting "yea" associated with an additional $100,000. "Immediate Aftermath of Disaster" is an indicator equal to one if and only if the roll call occurs within two days after the reported onset of the disaster. Standard errors are reported in parentheses and are two-way clustered by legislator and year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 8: Do Disasters Affect How Unconnected Legislators Vote?

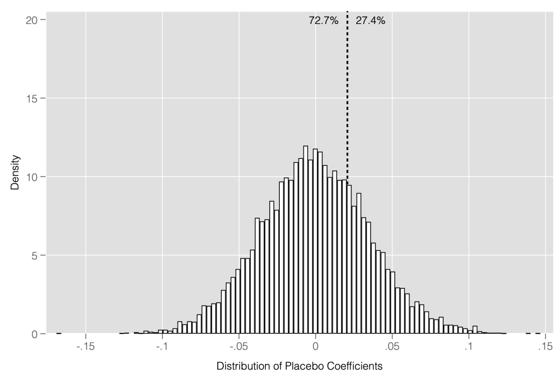
(a) Null-Distribution of $\hat{\beta}_{\text{pre}}$ for Unconnected Legislators



(b) Null-Distribution of $\hat{\beta}_{\text{post}}$ for Unconnected Legislators



(c) Null-Distribution of $\hat{\beta}_{\text{post}} - \hat{\beta}_{\text{pre}}$ for Unconn. Legislators



Notes: Figure shows the distribution of β_{pre} and β_{post} in eq. (2) with voting "yea" as outcome, as well as the difference between both estimated coefficients under the sharp null that disasters have no effect on how *unconnected* legislators vote. Dashed vertical lines indicate the size of the actual estimates based on our data. All panels are based on 10,000 regressions, with randomly reshuffled start dates of disasters.

Fifth, we examine whether natural disasters affect the votes of legislators who did *not* receive any donations from the special-interest groups that took a position on a particular bill. To this end, we again turn to randomization inference but replace the outcome variable in eq. (2) with an indicator for whether a lawmaker voted "yea."[17] Figure 8 presents both the true values of $\hat{\beta}_{\text{pre}}$ and $\hat{\beta}_{\text{post}}$ in our data (vertical lines) as well as their distributions under null hypothesis of not effect of disasters on the votes of "unconnected" legislators. If the effect of disasters resulted in last-minute changes to bills, or if it meant that more desirable bills were more likely to be voted upon, then we would expect to see effect an effect even among "unconnected" legislators. This is not the case. The evidence in Figure 8 is fully consistent with the null hypothesis of no effect on disasters on the votes of unconnected legislators, both just before and after a disaster strikes.

Moral Hazard The fact that we observe an effect of disasters only for "connected" legislators, who then appear to bifurcate towards the positions of respective their donors, supports the view that the mechanism behind our main result is a temporary reduction in electoral accountability, i.e., moral hazard on the part of rank-and-file congresspeople. In our view, this kind of moral hazard is the most plausible explanation for why less attention to politics coincides with greater incumbent support for special interests. We come to this conclusion because moral hazard in roll-call votes is the only mechanism that is consistent with all of our findings, and because it resonates with some of the seminal qualitative accounts of decision-making by politicians.

7. Conceptual Framework

The purpose of this section is to think more broadly about how to interpret our results in terms of electoral accountability to voters and in terms of voter welfare. To see what our findings mean for electoral accountability even if a representative's constituents agree with her donors on many but not all issues, it is useful to analogize a legislator's calculus of voting as a game of tug of war between constituents and special interest groups.[18] Sometimes, both pull in the same direction, whereas at other times, they are on opposite sides. When constituents are distracted, they pull less forcefully. Special-interest groups, however, always keep their eyes on the prize. They are not subject

[17] Recall, by construction the outcome variable in eq. (2) is not defined for lawmakers that did not donations money from any of the position-taking interest groups.

[18] We note that the tug of war can affect the legislator's decision through fear of losing votes or because of embarrassment of caving to special interests in the view of voters. Both of these channels work identically for the purposes of this conceptual framework.

Table 6: Preference Configurations and Empirical Predictions

	Alignment with Politician Preferences		Effect of Voter Distraction on
	Voters	Interest Groups	Support for Special Interests
1.	aligned	aligned	no effect
2.	misaligned	aligned	weakly more support
3.	aligned	misaligned	weakly more support
4.	misaligned	misaligned	weakly less support

Notes: Figure refers to the thought experiment in Section 7. It shows all possible preference configurations and the associated prediction for the impact of voter distraction on legislator support for the positions of special-interest groups.

to the same distractions as most citizens and thus are less likely to be influenced by contemporaneous media coverage. Moreover, politicians are aware of this reality.

For the moment, assume that legislators are only concerned with balancing their constituents' preferences and that of special interests. Importantly, for issues on which constituent and interest-group preferences are aligned with each other, it does not matter how hard the former pull. Legislators do not face a trade-off and thus always vote the same way. Hence, we would not expect to see attention effects when constituents and special interests agree. How hard constituents pull matters only when both groups are opposed. As a consequence, when special interests win more frequently than usual, constituents must have lost out.

It is straightforward to extend this argument to situations in which legislators care about more than just constituents and special interests. For simplicity, let us combine *all* other influences and call the resulting net impact a politician's "personal" preferences, recognizing that it may also incorporate party influence and other considerations. Since roll-call votes are binary (conditional on participation), a legislator's "personal" preferences must either be aligned with those of her constituents, her donors, neither, or both. Taking the position of constituents as given, Table 6 enumerates all possible preference configurations. It also lists the predicted impact of voter distraction on legislators' tendency to side with special-interest groups.

As before, if all interests are aligned, constituents becoming less attentive should have no effect (case 1). Similarly, if constituents and special interests are on opposite sides of the issue, then, all else equal, constituents becoming distracted should benefit special interests (cases 2 and 3). This is true irrespective of the politician's preference. The only case that is appreciably different from the simplified thought experiment above is the one in which constituents and special interests both oppose the politician's personal position (case 4). In such a situation, we would expect that constituents pulling less hard would reduce the total influence of their coalition with special interests, and would, therefore, make it weakly *less* likely that the politician's vote aligns with the position of her donors.

There are thus preference configurations in which increasing support for special interests hurts voter representation, and ones in which it is good. Critically, these configurations yield opposite empirical predictions about the impact of voter attention. Conditional upon observing that reductions in attention to politics go hand-in-hand with increased support for special interests, we can conclude that cases 2 and 3 are the empirically relevant ones. As a result, even if constituent and donor preferences are not the only factors entering a politician's calculus of voting, increasing support for special interests when voters are distracted is, on average, detrimental to electoral accountability.

Clearly, this framework for thinking about the interpretation of our results is highly stylized. Perhaps the most important simplification we have made is that voters are homogeneous. Relaxing this assumption would add additional groups and, therefore, cases to Table 6, but it would leave our analysis unchanged in all ways but one. When constituents have heterogeneous preferences, then we can no longer conclude that an increase in support for special interests is detrimental to electoral accountability in general. We can, however, continue to conclude that it worsens the representation of the distracted group of constituents, i.e., the group that pulls less hard in the aftermath of the distracting event.

The key assumption that we cannot weaken is that preferences are fixed. If shocks to attention directly affect either the positions of voters, politicians, or interest groups, then our empirical results do not yield sharp takeaways about electoral accountability. All we would be able to learn from our work is that special interests benefit during times in which voters are distracted.

It is worth noting that the welfare implications of our findings are ambiguous if greater constituent attention induces pandering by congresspeople (see, e.g., Trombetta 2020). Another important implicit assumption is that interest group donations do not buy a *fixed* number of votes. If this were the case, then our empirical results would imply that natural disasters affect *when* rather than *how often* legislators support their special-interest donors. In such a case, legislators would still be less accountable to their constituents in times of reduced attention, but electoral accountability need not suffer on average.

8. Conclusion

The findings in this paper shed some of the first light on an overlooked aspect of the principal-agent relationship between voters and legislators. Relying on natural disasters as a source of plausibly exogenous variation, we provide high-frequency evidence of a simultaneous decline in attention to politics and a realignment of roll-call votes towards legislators' special-interest donor. Our preferred explanation for this finding is that a temporary reduction induces moral hazard on the part of politicians. More broadly, our results are consistent with view that contemporaneous attention and scrutiny matter for electoral accountability.

Viewed solely through the lens of formal theories of accountability, our findings present a bit of a puzzle. After all, representatives' roll-call votes become public record and can be held against them when they run for reelection. There are several candidate explanations for why attention matters even in settings with stringent transparency and disclosure requirements. First, concomitant media coverage and voter attention might discipline politicians by furnishing citizens with more or better information about their actions, i.e., information that complements the public record. Alternatively, the effects we uncover might operate through incumbents' flow utility—either because politicians derive disutility from negative press, as suggested by Fenno's (1978), or because they loathe having to explain themselves to angry constituents, as reported by Kingdon (1973). Disentangling these channels and integrating attention into a formal model of electoral accountability are important tasks for future research. Our contribution in this paper is to provide evidence to suggest that contemporaneous attention to politics matters for electoral accountability.

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Online Appendix

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Appendix A: Measuring Politics and Disaster Reporting on the Evening News

A.1. *Politics Coverage*

As explained in the main text, Watson uses natural language processing and neural nets, among other methods, to extract concepts, entities, and sentiment from unstructured text. It also categorizes the content of the text according to an enhanced version of the IAB Quality Assurance Guidelines Taxonomy, which defines contextual categories that were originally designed to accurately and consistently describe the content of, say, a website or video clip, in order to facilitate better-targeted advertisements (Interactive Advertising Bureau 2013). Critical for our purposes, Watson's taxonomy contains a category for content related to "law, government, and politics." This high-level category contains several subcategories for which Watson returns confidence scores. Since Watson's categorization is not mutually exclusive, we define a particular segment's overall "politics score" as the sum of the confidence scores for all subcategories, up to a maximum of one. In symbols, $PoliticsScore_s = \min\{\sum_{c\in C} Score_{s,c}, 1\}$, where C denotes the set of subcategories in "law, government, and politics."

Inspecting the distribution of politics scores, there is a very large mass point at exactly zero. Fewer than one in three segments receive a strictly positive score. Among these, however, we observe significant mass in the middle of the distribution. For segments with intermediate scores, it is *a priori* unclear whether or not they should be classified as "political." To make this decision in a principled way, we tasked a research assistant with manually coding a random subset of 1,000 segments. Taking the human judgment as the truth, we find that a cutoff score of .144 provides an a good balance between sensitivity and selectivity. Using this cutoff, Appendix Table A.1 constructs the confusion matrix. Given an accuracy of 91.6% and a false positive (negative) rate of 7.7% (11.1%), our automated detection of political content appears to perform well—though it is certainly not perfect.

A.2. *Disaster Coverage*

As noted in the main text, we also use IBM Watson to detect disaster-related reporting on the evening news. In analogous fashion to our politics classifier, we define the $DisasterScore_s$ of news segment s as the sum of the relevant confidence scores that Watson returns. As was the case with respect to politics-related content, most news segments have a score of exactly zero, and a much smaller number has a score of one.

Given that there is again nontrivial mass in the middle of the distribution, we proceed the same way as classifying political content. In the end, we say that news segment s is disaster-related if and only if $DisasterScore_s > .178$, where .178 corresponds to the chosen cutoff.

Appendix Table A.2 presents the confusion matrix. Given an overall accuracy of 97.9% with a false positive (negative) rate of 1.5% (15.2%), we again conclude that our automated measurement of disaster-related content works reasonably well.

Appendix B: Correcting for Measurement Error in News Coverage

Since we use machine learning to detect political content on the evening news, our measure of politics reporting will inevitably contain measurement error. This measurement error is necessarily non-classical.[1] In fact, the errors are by construction "one-sided," meaning that they are correlated with the true outcome. In what follows, we show that this causes attenuation bias in linear probability models, and we provide estimates that correct for the bias.

While we are focused on the specifics of our setting, we note that similar problems arise in virtually all applications in which researchers use a machine-learning classifier to measure outcomes. The theoretical results below are, therefore, much more broadly applicable.

We also note that our derivations differ from prior work on measurement error, which, for the most part, assumes i.i.d. errors in either the dependent or independent variables. While we aware of models with non-independent measurement error in a right-hand side variable, we do not know of results pertaining to measurement error that is correlated with the realization of the left-hand side variable, as in our application.

B.1. *Derivation of Bias*

B.1.1. *Main Result*

We first study the simple case in which reporting on a particular day is either about politics ($Y = 1$) or not ($Y = 0$). After deriving a correction for measurement error in this setting, we extend our result to the case in which the outcome variable is a weighted average of segments that do and do not cover politics (i.e., $\bar{Y} = \sum_j \omega_j Y_j$).

[1] To see this, note that the outcome is bounded by zero and one, which violates the assumptions in the classical measurement error model.

Consider the following linear probability model

$$Y = X\beta + \epsilon, \tag{5}$$

where Y is the a outcome, X is a (de-meaned) vector of covariates, and ϵ denotes the error term. The parameter of interest is β.

Let $\tilde{Y}$ denote the true outcome. If there were no measurement error, ϵ would be a binary random variable equal to $1 - X\beta$ with probability $Pr(\tilde{Y} = 1) = X\beta$ and equal to $-X\beta$ with the complementary probability. However, when using an automated classifier to measure the outcome, Y will generally contain some error. Assume that $\theta_0 = Pr(Y = 0|\tilde{Y} = 0)$ and $\theta_1 = Pr(Y = 1|\tilde{Y} = 1)$. In the language of machine learning, θ_0 denotes the specificity of the classifier (i.e., the probability of correctly identifying a "true negative"), whereas θ_1 corresponds to its sensitivity (i.e., the probability of detecting a "true positive"). With this notation in hand, the expectation of ϵ conditional on X is given by

$$\begin{aligned} E[\epsilon|X] &= X\beta[\theta_1(1 - X\beta) + (1 - \theta_1)(-X\beta)] + (1 - X\beta)[\theta_0(-X\beta) + (1 - \theta_0)(1 - X'\beta)] \\ &= X\beta(\theta_1 + \theta_0 - 2) + 1 - \theta_0. \end{aligned}$$

Thus, the expectation of the structural measurement error model in eq. (5) is

$$E[Y|X] = X\beta(\theta_0 + \theta_1 - 1) + 1 - \theta_0.$$

Further, using the standard formula, the expectation of the OLS estimator is given by

$$\begin{aligned} E[\hat{\beta}_{OLS}|X] &= (X'X)^{-1}X'E[Y|X] \\ &= (X'X)^{-1}X'X\beta(\theta_0 + \theta_1 - 1) + (X'X)^{-1}X'(1 - \theta_0) \\ &= \beta(\theta_0 + \theta_1 - 1), \end{aligned}$$

where the last equality uses the fact that $(X'X)^{-1}X'(1 - \theta_0)$ corresponds to regressing a constant on X, which returns zero. As a result, in the simple case, to correct for measurement error in the dependent variable, we must inflate the the OLS estimate by

$(\theta_0 + \theta_1 - 1)$, i.e.,

$$\beta = \frac{E[\hat{\beta}_{OLS}|X]}{\theta_0 + \theta_1 - 1}. \tag{6}$$

Eq. (6) shows that unless $\theta_0 = \theta_1 = 1$—in which case there is no measurement error—the OLS estimate will be attenuated, and the bias depends on both the specificity and sensitivity of the classifier.

B.1.2. *Extension to Weighted Averages*

The result above is likely to be useful in a broad array of applications in which researchers use machine learning methods to measure outcomes. In our specific setting, however, it is not directly applicable because our measure of politics coverage on the evening news is a weighted sum of mismeasured binary variables. Nonetheless, it is straightforward to extent our result to this case.

In particular, our regression model is given by

$$\bar{Y} = X\beta + \bar{\epsilon},$$

with $\bar{Y} \equiv \sum_j \omega_j Y_j$, $\bar{\epsilon} \equiv \sum_j \omega_j \epsilon_j$, and weights $\sum_j \omega_j = 1$. In our application, ω_j corresponds to the length of news segment j relative to the entire broadcast. Note, X does not need to averaged because it varies only on the daily level and not across the different news segments within a given show.

Proceeding as above,

$$\begin{aligned} E[\hat{\beta}_{OLS}|X] &= (X'X)^{-1}X'E[\bar{Y}|X] \\ &= (X'X)^{-1}X'X\beta + (X'X)^{-1}X'\sum_j \omega_j(X\beta(\theta_1 + \theta_0 - 2) + 1 - \theta_0) \\ &= \beta + (X'X)^{-1}X'(X\beta(\theta_1 + \theta_0 - 2) + 1 - \theta_0) \\ &= \beta(\theta_0 + \theta_1 - 1). \end{aligned}$$

As a result, even when the outcome is a weighted average of mismeasured binary variables, as in our application, we can continue to adjust our regression estimates for attenuation bias through inflating them by $(\theta_0 + \theta_1 - 1)$.

B.2. *Corrected Estimates*

Appendix Table A.3 presents the measurement-error-corrected estimates and contrasts them to estimates without correction. Taking the judgement of the human coder as the ground truth allows us to estimate θ_0 and θ_1 by looking at the relevant confusion matrices (e.g., Appendix Table A.1 and Table 2). Relative to the their uncorrected counterparts in the main text, the point estimates estimates of disasters' impact on politics coverage need to inflated by approximately 25%.

Since $\hat{\theta}_0$ and $\hat{\theta}_1$ are themselves random variables, we can use the delta method to calculate standard errors for the adjusted point estimates. Under the assumption that classification errors are i.i.d., the estimated variance of a corrected coefficient is given by

$$\widehat{Var}(\hat{\beta}) = [\widehat{Var}(\hat{\beta}_{OLS}) + (\hat{\beta}_{OLS}/(\hat{\theta}_0 + \hat{\theta}_1 - 1))^2(\widehat{Var}(\hat{\theta}_0) + \widehat{Var}(\hat{\theta}_1))]/(\hat{\theta}_0 + \hat{\theta}_1 - 1)^2. \tag{7}$$

Relative the standard errors in Figure 4 in the main text, the standard errors on the corrected estimates are slightly larger, but not enough to affect any of our qualitative conclusions.

For additional measurement-error-corrected estimates, see Appendix C.

Appendix C: Ancillary Results and Robustness Checks

Appendix Table A.3 probes the robustness of the effect of disasters on politics reporting with respect to: *(i)* standardizing the left-hand-side variable within each TV network (columns (1)–(6) and (13)–(18)); *(ii)* measuring coverage in raw minutes instead of relative shares (columns (7)–(12) and (19)–(24)); *(iii)* broadening the sample to include news shows on CNN and Fox News in addition to ABC, CBS, and NBC (columns (4)–(6), (9)–(12), (16)–(18), and (22)–(24)); and *(iv)* simultaneously correcting for the LHS measurement error introduced through machine learning, as explained in Appendix C (columns (13)–(24)). None of these changes materially affect our conclusions.

Appendix Tables A.4 and A.5 respectively replicate the results in Tables 2 and 5 in the main text, relying on either all domestic natural disasters reported in EM-DAT (upper panels) or large foreign as well domestic disasters (lower panels) instead of only the latter. As for the results in Appendix Figure A.4, when we refer to large foreign disasters, we mean the 178 foreign disasters that fall into the top-1% in terms of either the number of deaths, total number of people affected, or total damages. Again, the estimated effects decline somewhat in magnitude but are otherwise similar to their counterparts in the main text.

Appendix D: Data Description and Definitions

D.1. *MapLight*

As explained in the main text, information on connections between politicians and special interests, the positions of special interest groups on particular pieces of legislation, and congressmen's votes on the same measures comes from MapLight. MapLight is a nonpartisan, 501(c)(3) nonprofit organization whose goal it is to "reveal the influence of money in politics, inform and empower voters, and advance reforms that promote a more responsive democracy."

MapLight staff scour publicly available sources, like congressional testimony, news databases, and trade associations' websites, to compile lists of organizations and interest groups that either supported or opposed a particular piece of federal legislation, excluding bills and amendments that are purely ceremonial. Starting with legislation considered in the 109th Congress, MapLight provides data on interest group positions on more than 10,000 individual bills—most of which never receive a vote. MapLight also uses campaign contribution data provided by the Center for Responsive Politics in order to link interest groups' positions on a particular bill to their donations to individual congressmen, the relevant roll-call votes, and metadata on the bill. The linked records are then made publicly available at `http://classic.maplight.org/us-congress/bill`.[2]

Our analysis relies on the linked records for all 1,525 bills that (a) received a passage vote in the House of Representatives prior to October 2017, and (b) were supported or opposed by at least one special interest group.

D.2. *EM-DAT*

Data on natural disasters come from the Centre for Research on the Epidemiology of Disasters (CRED) at the Université Catholique de Louvain, which maintains the Emergency Events Database (EM-DAT). EM-DAT contains core information on the occurrence and effects of over 22,000 natural and man-made disasters worldwide. According to the CRED website "the main objective of the database is to serve the purposes of humanitarian action at national and international levels. The initiative

[2]For additional information on MapLight and its methodology, see `http://classic.maplight.org/us-congress/guide/data`.

aims to rationalize decision making for disaster preparedness, as well as provide an objective base for vulnerability assessment and priority setting."

For an adverse event to be recorded as a disaster in EM-DAT it must satisfy at least one of the following criteria: 10 or more people dead, 100 or more people affected, an officially declared state of emergency, or a call for international assistance. CRED staff assess these criteria based on various sources, including UN agencies, non-governmental organizations, insurance companies, press agencies, as well as other research institutes.

For our main analysis, we restrict attention to natural disasters that occurred within the United States. We further restrict attention to sudden-onset disasters that fall into the top tercile of adverse events in terms of either deaths, number of people affected, or damages. The latter restriction is intended to filter out relatively minor incidents that are unlikely to crowd out media attention, while the former one ensures that we only work with disasters for which the start date is precisely enough defined to obtain sharp identification. In practice, this means that we exclude epidemics, heat waves, and wildfires from our main analysis. In the robustness checks in Appendix C, we show that our findings remain qualitatively unchanged if we included all domestic disasters recorded in EM-DAT. After imposing these sample restrictions, we are left with 200 large domestic disasters that occurred between 2005 and the end of 2017.

D.3. *Vanderbilt Television News Archive*

Information on the content of TV news broadcasts comes from the Vanderbilt Television News Archive (VTNA). Starting in 1968, VTNA collects and archives daily recordings of the regularly scheduled evening news programs on ABC, CBS, and NBC. In 1995, coverage was expanded to include approximately one hour per day from CNN, and, in 2004, to also include Fox News. Originally, VTNA attempted to provide a short, human-generated summary of every story that aired, information on its duration, as well as its order of appearance. Unfortunately, in 2014 VTNA stopped producing human-generated summaries of stories from weekday newscasts on CBS, NBC, and Fox News. In private communication, representatives from VTNA indicated that they scaled down on human-generated content in order to experiment with automated techniques, which have not been as successful as they had hoped.

As explained in the main text, we use state-of-the-art machine learning as implemented by IBM Watson to classify each news story in VTNA based on the provided summary.[3] In particular, Watson categorizes the content of unstructured text according to an enhanced version of the IAB Quality Assurance Guidelines Taxonomy Interactive Advertising Bureau (2013), which defines contextual categories that were originally designed to consistently describe web content in order to facilitate more relevant advertising and allow for *ex post* analysis.

With the classification from Appendix B in hand, we measure politics coverage by network n on day t as the fraction of total airtime the newscast devoted to political matters. In symbols, $News_{n,t} \equiv (\sum_{s \in P_{n,t}} Duration_s)/(\sum_{s \in S_{n,t}} Duration_s)$, where $P_{n,t}$ denotes the set of news segments that are deemed to contain political content and $S_{n,t}$ is the set of all segments, including commercials.

To measure disaster-related news reporting we use Watson and the VTNA data in an analogous fashion (see Appendix A for details).

D.4. *NewsLibrary*

As explained in the main text, we complement our daily measure of attention to politics on the evenings news with a second one that focuses exclusively on individual representatives. To this end, we have searched the NewsLibrary database for newspaper articles that mention an in-state congressperson by name. Specifically, for each representative and each year she is in office, we limit our search to newspapers from her home state and submit the following query: "(Congressman AND *name*) OR (Congresswoman AND *name*) OR (Representative AND *name*)", where *name* denotes the person's last name. We then count, for each day, the number of articles returned, and use this information to construct a daily panel of newspaper reports on local congresspeople.

At the time of our searches, the NewsLibrary database was owned by NewsBank, Inc. and indexed more than 6,500 newspapers from all around the United States—though coverage varies considerably across space and time. For more information on NewsLibrary, see `https://newslibrary.com`.

D.5. *Web Searches*

We measure citizens' interest in Congress using Google searches for the following terms: "politics," "Congress," "Congressman," "Representative," "government," "House of Representatives," and "vote." The relevant data come from Google Trends, which we

[3]We access Watson remotely through an API. For a free demonstration of Watson's text-analytic capabilities see `https://natural-language-understanding-demo.ng.bluemix.net`.

accessed via an API, and span the same frame as the MapLight data.[4]

Google Trends provides information on the daily search volume for arbitrary keywords. For each query the maximum of the time series that Google Trends returns is indexed by 100. Since it is not possible to download daily data for a period longer than three months, we proceeded by downloading, for each keyword, the daily data for any given month, which we then multiply by the monthly search volume index for the same keyword. In symbols,

$$v_{k,t} = \tilde{v}_{k,m,d}\bar{v}_m \tag{8}$$

where $v_{k,t}$ denotes the search volume for keyword k on date t, $\tilde{v}_{k,m,d}$ is the search volume for the same term on day d of month m, and $\bar{v}_t$ is the average volume during the same month. This adjustment follows Durante and Zhuravskaya (2018), and it ensures that the indexed daily search volume for a given keyword is comparable over time. We then standardize the entire time series for each keyword. The resulting variable serves as the outcome in the regression model in eq. (2) in the main text, i.e., $GS_{k,t}$.

To measure disaster-related searches we proceed in analogous fashion, focusing on the following set of keywords: "disaster," "volcano," "earthquake," "flood," "landslide," "storm," "hurricane," "blizzard" and "tornado."

D.6. *Other Data Sources*

D.6.1. *Congressional Speech*

Data on congressional speech come from Gentzkow et al. (2018). Gentzkow et al. (2018) obtained copies of the Congressional Record—which contains all text spoken on the floor of either the U.S. House or the U.S. Senate—for the 43rd to 114th Congresses from HeinOnline. They then used automated scripts to parse the text from each session in order to extract full-text speeches, metadata on speeches and their speakers, and counts of bigrams.

We use their data on full-text speeches in the House and the accompanying metadata for the 109th–114th Congresses. These restrictions are imposed to ensure that the setting for our analysis of congressional speech corresponds as closely as possible to the setting of our main analysis. We further process the full text of speeches by removing common stop words, such as "a," "about," "between," "because," etc., and by counting (i) the

[4]Google Trends is available at `https://trends.google.com/`.

total number of remaining words spoken on a particular day, as well as (ii) the number of words that are plausibly related to natural disasters. To identify the latter we conduct a simple keyword search for the following terms: "disaster," "emergency," "relief," "help," "rebuild," "assistance," "victim," "storm," "hurricane," "tornado," "flood," "landslide," "earthquake," or "volcano." These daily counts then serve as outcome variables in our ancillary results (see above).

D.6.2. *Number of Votes, Roll-Call Types & Vote Issues*

Data on the type of a roll-call vote come from the PIPC House Roll Call Database Crespin and Rhode (2018). Coverage of PIPC begins with the 83rd Congress. Among other information, these data contain a variable classifying each roll call as one of 59 mutually exclusive types, such "quorum call," "final passage / adoption of a bill," "final passage / adoption of conference report," "passage / adoption of a bill under suspension of the rules," "passage / adoption of a joint resolution under suspension of the rules," "straight amendments," "amendments to amendments," "motion to discharge," "motion to reconsider," etc. Roll calls from the 83rd to 100th Congresses were manually assigned to one of these categories. Starting with the 101st Congress, PIPC began using a supervised machine-learning model to assign types based on the roll call-specific description and other information provided on the Clerk of the House's website. In training this model, the hand-coded votes from priors years served as examples.

We restrict attention to House votes during the 109th–115th Congresses and rely on the classification in the PIPC database in conjunction with ancillary information from *voteview.com* Lewis et al. (2018) to count the total number of roll calls of particular type that were held on a given day. Since our categories are broader than those in the PIPC database, we aggregate over related types.

PIPC also contains hand-coded issue codes for each roll call. PIPC obtains the relevant information comes from the Comparative Agendas Project (PAP), which collects and organizes data from archived sources to track policy outcomes across countries.[5] Again, we aggregate over different issue codes in the raw data to define the issue categories used in the main text.

[5] See `https://www.comparativeagendas.net/`.

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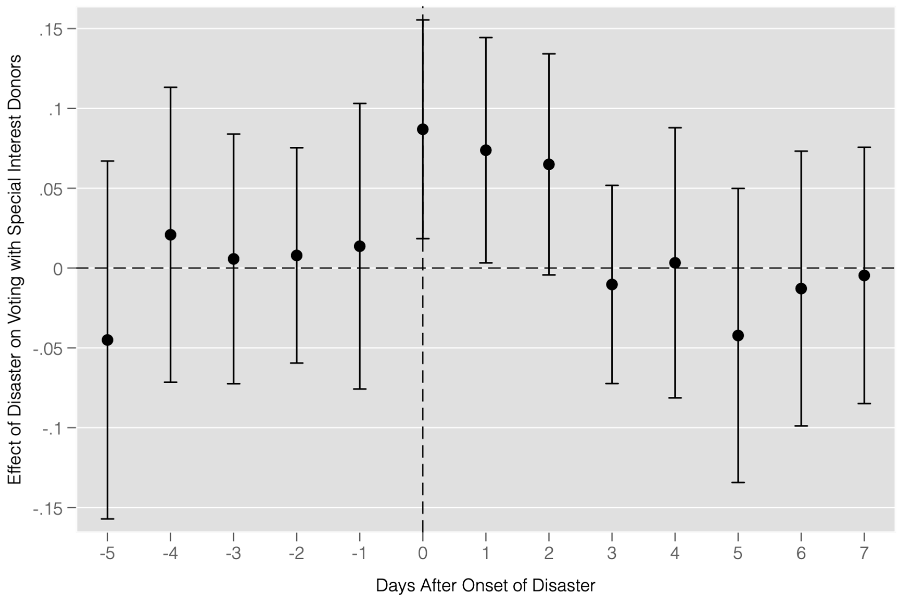
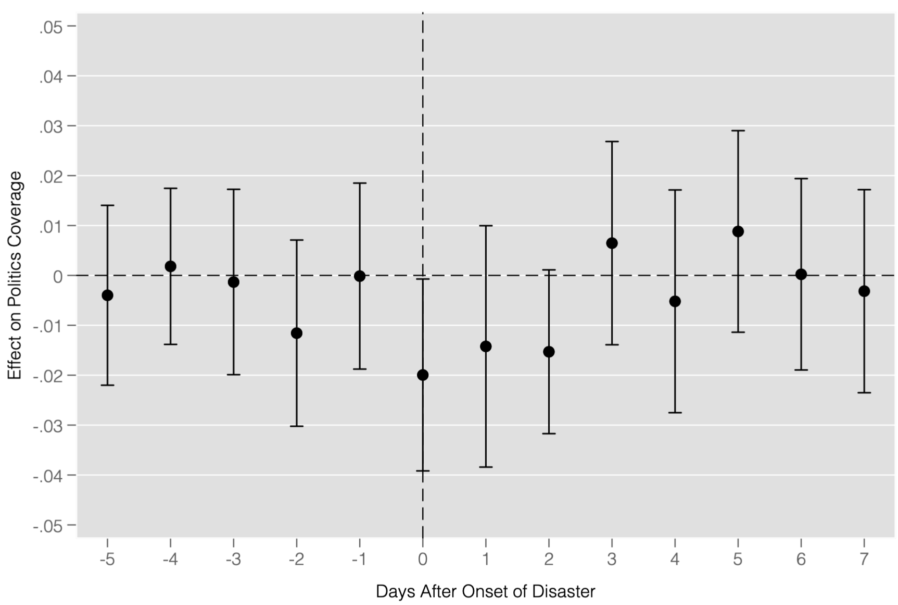
Appendix Figure A.1: Dropping Days that Fall in Multiple Event Windows

(a) Effect of Disasters on Votes

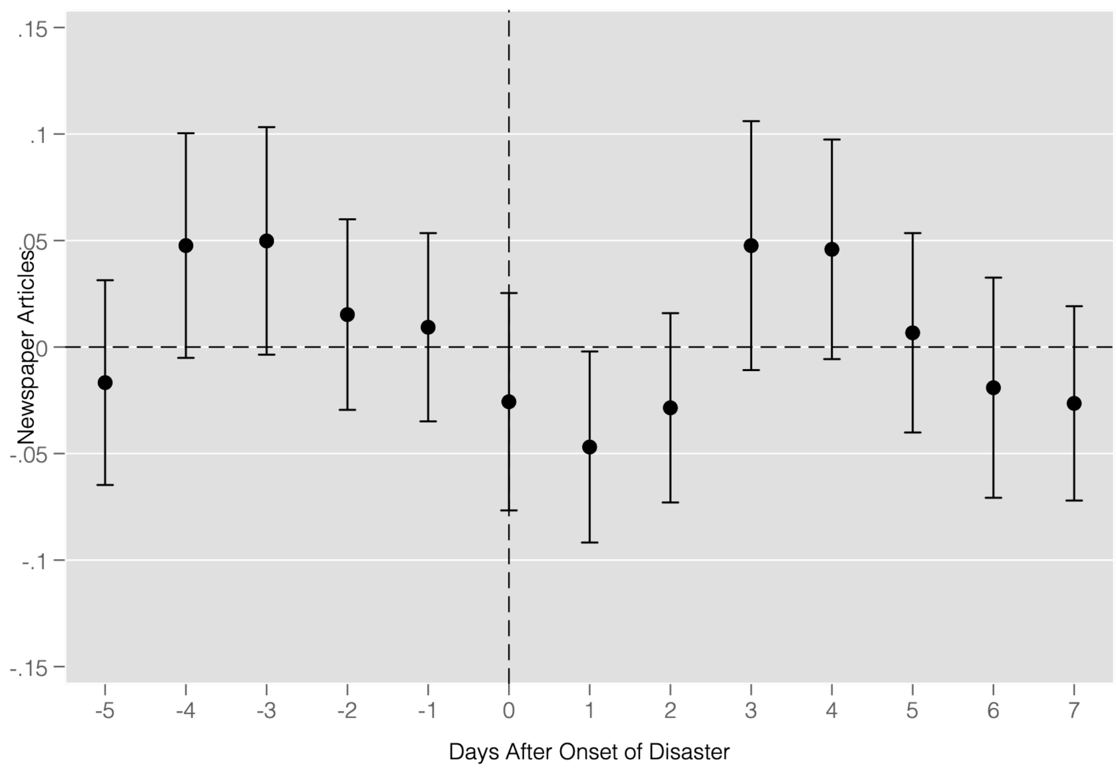


(b) Effect of Disasters on Politics Reporting

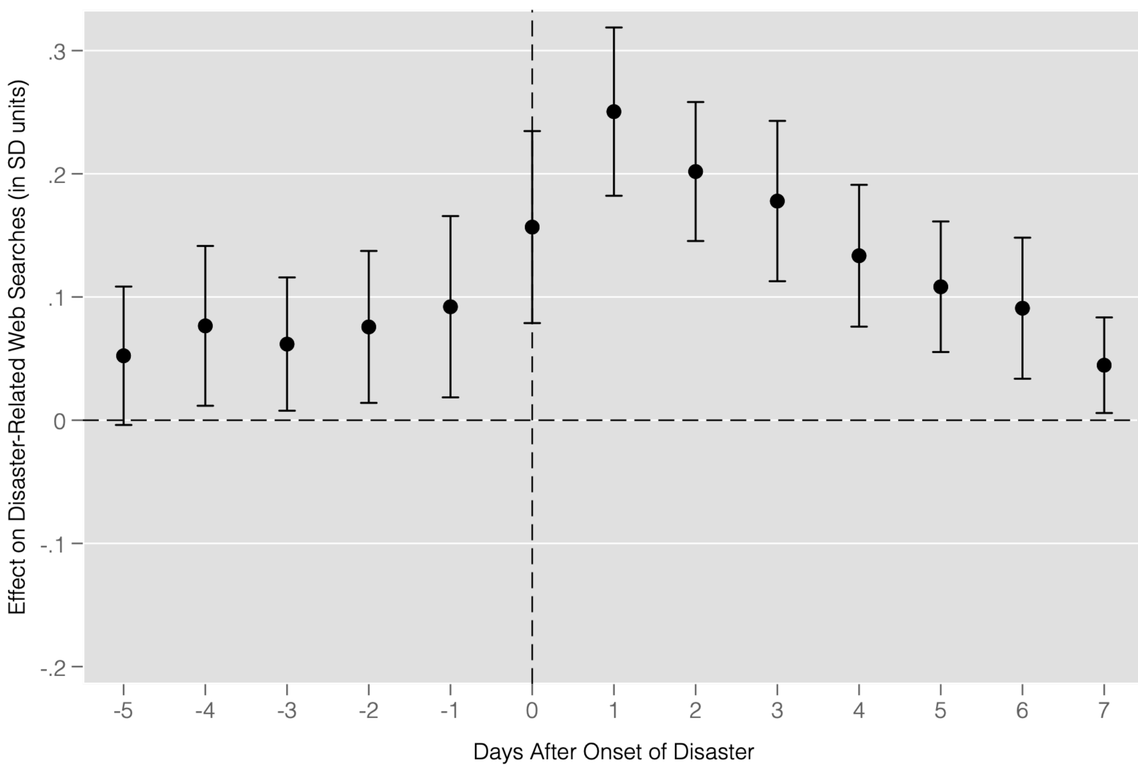


Notes: Figure replicates Figures 5 and 7 in the main text, dropping all votes (upper panel) and news reports (lower panel) panel that fall in the event window of more than one domestic natural disaster.

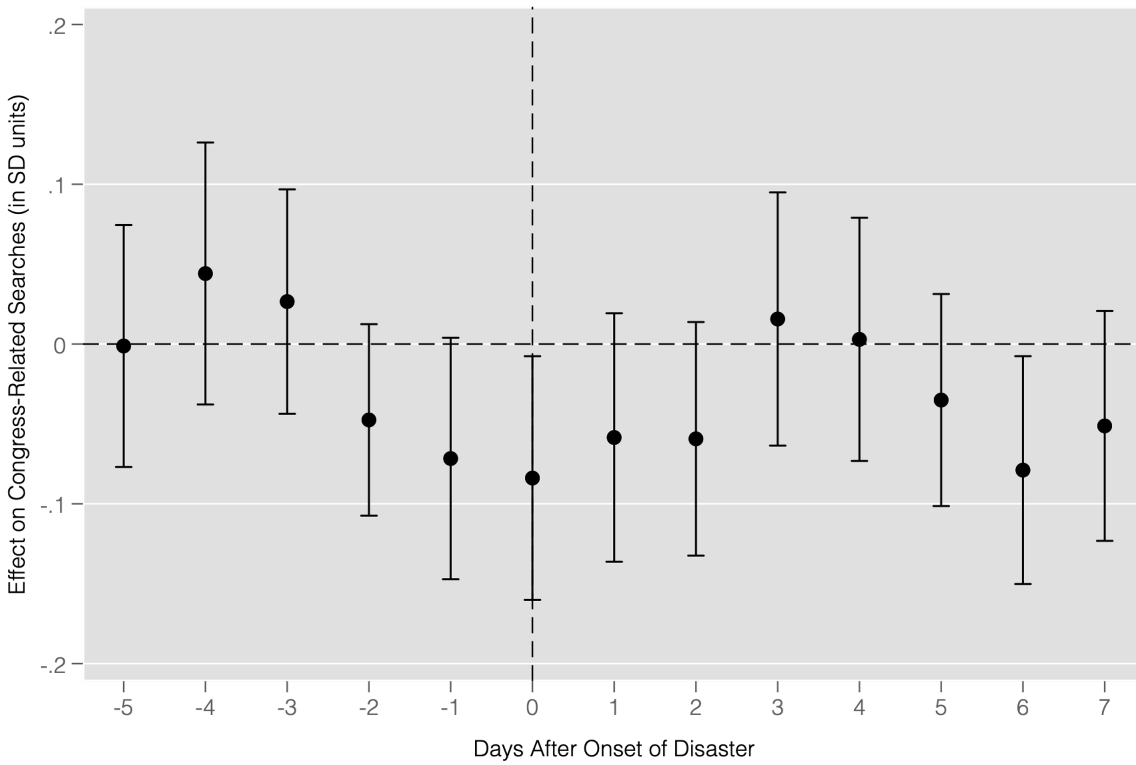
Appendix Figure A.2: Impact of Disasters on Newspaper Coverage of Local Representatives



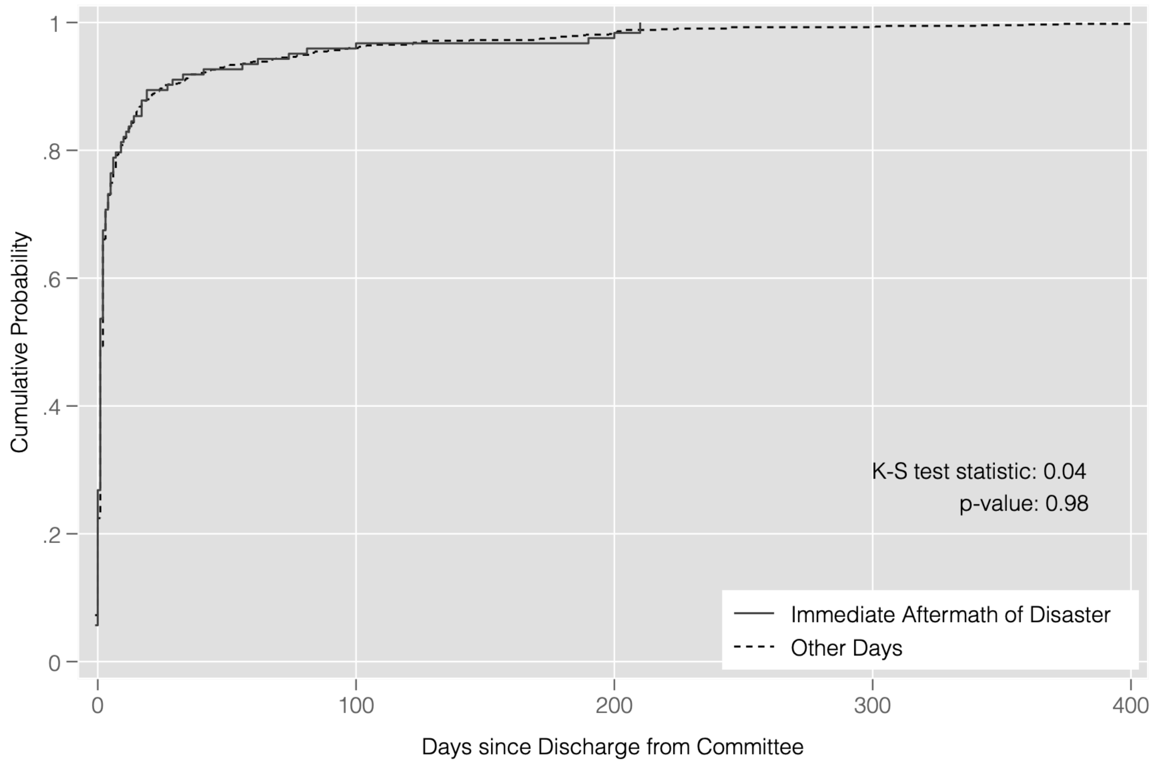
Appendix Figure A.3: Impact of Disasters on Disaster-Related Google Searches



Appendix Figure A.4: Impact of Disasters on Congress-Related Google Searches



Appendix Figure A.5: Kolmogorov-Smirnov Test for Delay in Bills Voted Upon Before vs. After Disaster



Appendix Table A.1: Performance of Politics-Reporting Classifier

A. Confusion Matrix

		Watson	
		Nonpolitical	Political
Human Coder	Nonpolitical	73.2%	6.1%
	Political	2.3%	18.4%

B. Performance Metrics

Correctly Classified:	91.60%
Sensitivity:	88.89%
Specificity:	92.31%
False-Positive Rate:	7.69%
False-Negative Rate:	11.11%

Notes: Entries in the upper panel are percentages comparing Watson's classification of 1,000 randomly drawn news segments as related to "law, government & politics" against the judgements of a human coder. As explained in the main text, Watson is said to classify a segment as "political" if the assigned score exceeds the cutoff score for maximizing accuracy, i.e., the fraction of segments that are correctly classified as either political or nonpolitical. Entries in the lower panel are descriptive statistics for the performance of the automated classification, taking the judgements of the human coder as ground truth.

Appendix Table A.2: Performance of Disaster Classifier

A. Confusion Matrix

		Watson: Not Disaster Related	Watson: Disaster Related
Human Coder	Not Disaster Related	94.0%	1.4%
	Disaster Related	.7%	3.9%

B. Performance Metrics

Correctly Classified:	97.90%
Sensitivity:	84.78%
Specificity:	98.53%
False-Positive Rate:	1.47%
False-Negative Rate:	15.22%

Notes: Entries in the upper panel are percentages comparing Watson's classification of 1,000 randomly drawn news segments as related to "law, government & politics" against the judgements of a human coder. As explained in the main text, Watson is said to classify a segment as "political" if the assigned score exceeds the cutoff score for maximizing accuracy, i.e., the fraction of segments that are correctly classified as either political or nonpolitical. Entries in the lower panel are descriptive statistics for the performance of the automated classification, taking the judgements of the human coder as ground truth.

Appendix Table A.3: Regression Evidence on the Effect of Disasters on News Crowd-Out

A. OLS Estimates	Standardized Meaure of Politics Coverage						Minutes of Politics Reporting on a Particular Network					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Immediate Aftermath of Disaster	-.152***	-.147***	-.147***	-.114***	-.116***	-.117***	-.581***	-.552***	-.556**	-.566**	-.711***	-.568***
	(.046)	(.042)	(.041)	(.038)	(.033)	(.033)	(.191)	(.172)	(.170)	(.263)	(.220)	(.200)
Hypothesis Tests [p-values]:												
H0: No Effect of Disaster	.001	.001	.000	.003	.001	.000	.003	.002	.001	.033	.002	.005
Fixed Effects:												
Year × Month	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Network × Day of the Week	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Sample	Big Three	Big Three	Big Three	All Channels	All Channels	All Channels	Big Three	Big Three	Big Three	All Channels	All Channels	All Channels
R-Squared	.003	.101	.110	.001	.102	.117	.002	.100	.120	.004	.073	.541
Number of Observations	10,932	10,932	10,932	17,207	17,207	17,207	10,932	10,932	10,932	17,207	17,207	17,207

B. Measurement-Error-Corrected Estimates	Standardized Meaure of Politics Coverage						Minutes of Politics Reporting on a Particular Network					
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Immediate Aftermath of Disaster	-.187***	-.182***	-.149***	-.140***	-.143***	-.144***	-.716***	-.680***	-.685***	-.697**	-.875***	-.700***
	(.058)	(.054)	(.053)	(.048)	(.043)	(.042)	(.242)	(.219)	(.217)	(.328)	(.280)	(.253)
Hypothesis Tests [p-values]:												
H0: No Effect of Disaster	.002	.001	.001	.004	.001	.001	.004	.002	.002	.035	.002	.006
Fixed Effects:												
Year × Month	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Network × Day of the Week	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Sample	Big Three	Big Three	Big Three	All Channels	All Channels	All Channels	Big Three	Big Three	Big Three	All Channels	All Channels	All Channels
Number of Observations	10,932	10,932	10,932	17,207	17,207	17,207	10,932	10,932	10,932	17,207	17,207	17,207

Notes: Entries are coefficients and standard errors from regressing different measures of politics reporting on an indicator variable for the day of and the two days after the reported onset of a large domestic disaster, as explained in Appendix D. The outcome in columns (1)–(6) and (13)–(18) is the within-network standardized number of minutes of politics coverage on the evening news. The outcome in columns (7)–(12) and (19)–(24) is the raw duration of politics reporting (in minutes). The sample in columns (1)–(3), (7)–(9), (13)–(15), and (19)–(21) includes only news shows that aired on ABC, CBS, and NBC, whereas the sample in columns (4)–(6), (10)–(12), (16)–(18), and (22)–(24) also includes shows on CNN and FNC. Entries in the upper panel are OLS estimates and the associated standard errors, while those in the lower panel use the measurement-error-adjustment developed in Appendix C. All standard errors are clustered by year-month. ***, **, * denote statistical significance at the 1

Appendix Table A.4: Replication of Table 2

A. Using All Domestic Natural Disasters	Vote with Special-Interest Donors					
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate Aftermath of Disaster (β_{post})	0.056***	0.060***	0.068***	0.061***	0.061***	0.060***
	(0.019)	(0.019)	(0.020)	(0.021)	(0.021)	(0.021)
Immediately Before Disaster (β_{pre})	-0.005	-0.004	0.006	0.010	0.010	0.010
	(0.018)	(0.019)	(0.022)	(0.018)	(0.018)	(0.018)
Constant	0.806***					
Hypothesis Tests [p-values]:						
H_0: $\beta_{\text{post}} = \beta_{\text{pre}}$	.027	.027	.032	.047	.046	.046
R-Squared	.002	.006	.041	.059	.067	.085
Number of Observations	478,946	478,946	478,946	478,946	478,943	478,938

B. Using Large Foreign and Domestic Natural Disasters	Vote with Special-Interest Donors					
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate Aftermath of Disaster (β_{post})	0.041***	0.044***	0.045***	0.035**	0.035**	0.035**
	(0.015)	(0.015)	(0.017)	(0.017)	(0.017)	(0.017)
Immediately Before Disaster (β_{pre})	0.005	0.006	0.007	0.011	0.011	0.011
	(0.015)	(0.015)	(0.018)	(0.015)	(0.015)	(0.015)
Constant	0.801***					
	(0.009)					
Hypothesis Tests [p-values]:						
H_0: $\beta_{\text{post}} = \beta_{\text{pre}}$	.107	.093	.098	.245	.245	.252
R-Squared	0.002	0.005	0.040	0.058	0.066	0.084
Number of Observations	478,946	478,946	478,946	478,946	478,943	478,938
Fixed Effects (Panels A & B):	(1)	(2)	(3)	(4)	(5)	(6)
Year × Month	No	Yes	Yes	Yes	Yes	Yes
Day of the Week	No	Yes	Yes	Yes	Yes	Yes
Legislator	No	No	Yes	No	No	No
Legislator × Congress	No	No	No	Yes	Yes	No
Bill	No	No	No	No	Yes	No
Legislator × Bill	No	No	No	No	No	Yes

Notes: Entries replicate Table 4 in the main text using all domestic natural disasters (upper panel) as well as large foreign and domestic disasters (lower panel), as explained in Appendix D.

Appendix Table A.5: Replication of Table 5

A. Using All Domestic Natural Disasters

	Vote "Yea" on Passage			
	(1)	(2)	(3)	(4)
Money from Supporting Interest Groups ($\beta^{(+)}$)	.020***	.018***	-.004	.008**
	(.004)	(.004)	(.003)	(.004)
Money from Opposed Interest Groups ($\beta^{(-)}$)	-.181***	-.174***	-.156***	-.126***
	(.025)	(.025)	(.022)	(.018)
Money from Supporting Interest Groups		.006	.011*	.011*
× Immediate Aftermath of Disaster ($\gamma^{(+)}$)		(.007)	(.006)	(.007)
Money from Opposing Interest Groups		-.052***	-.044**	-.027*
× Immediate Aftermath of Disaster ($\gamma^{(+)}$)		(.018)	(.018)	(.014)
Immediate Aftermath of Disaster (δ)	.031**	.033**	.029*	.020
	(.015)	(.016)	(.015)	(.016)
Hypothesis Tests [p-values]:				
$H_0 : \gamma^{(+)} \leq 0$	–	.181	.030	.049
$H_1 : \gamma^{(-)} \geq$	–	.002	.006	.031
$H_2 : \gamma^{(+)} = \gamma^{(-)} = 0$	–	.001	.009	.067
Fixed Effects:				
Legislator × Congress	No	No	Yes	Yes
Year × Month	No	No	No	Yes
Day of the Week	No	No	No	Yes
R-Squared	.047	.047	.239	.315
Number of Observations	674,726	674,726	674,726	674,726

B. Using Large Foreign and Domestic Natural Disasters

	Vote "Yea" on Passage			
	(1)	(2)	(3)	(4)
Money from Supporting Interest Groups ($\beta^{(+)}$)	.020***	.019***	-.004	.008**
	(.004)	(.004)	(.003)	(.004)
Money from Opposed Interest Groups ($\beta^{(-)}$)	-.180***	-.173***	-.155***	-.127***
	(.025)	(.025)	(.022)	(.018)
Money from Supporting Interest Groups		.005	.011*	.008
× Immediate Aftermath of Disaster ($\gamma^{(+)}$)		(.006)	(.006)	(.006)
Money from Opposing Interest Groups		-.052***	-.048***	-.023
× Immediate Aftermath of Disaster ($\gamma^{(-)}$)		(.019)	(.017)	(.015)
Immediate Aftermath of Disaster (*delta*)	.030*	.032*	.019	.013
	(.017)	(.018)	(.017)	(.017)
Hypothesis Tests [p-values]:				
$H_0 : \gamma^{(+)} \leq 0$	–	.224	.034	.097
$H_1 : \gamma^{(-)} \geq$	–	.004	.003	.056
$H_2 : \gamma^{(+)} = \gamma^{(-)} = 0$	–	.020	.004	.142
Fixed Effects:				
Legislator × Congress	No	No	Yes	Yes
Year × Month	No	No	No	Yes
Day of the Week	No	No	No	Yes
R-Squared	.047	.048	.238	.315
Number of Observations	674,726	674,726	674,726	674,726

Notes: Entries replicate Table 5 in the main text using all domestic natural disasters (upper panel) as well as large foreign and domestic disasters (lower panel), as explained in Appendix D.