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Abstract

We exploit the precise timing of natural disasters to provide empirical evidence on the connection between electoral accountability and politicians' support for special interests. We show that, in the immediate aftermath of a disaster, the evening news substantially reduce their coverage of politics. At the very same time, members of Congress become more likely to adopt the positions of special-interest donors as they vote on bills. Our findings are consistent with standard theories of political agency, according to which politicians are more inclined to serve special interests when, for exogenous reasons, they are less intensely monitored.

Keywords: natural disasters, moral hazard, toll-call voting, special interests, Congress.

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1. Introduction

Principal-agent models in political economy predict that politicians are more likely to act in the best interest of their constituents when the latter are better informed about the actions of the former (see, e.g., Barro 1973; Ferejohn 1986; Austen-Smith and Banks 1989; Strömberg 2004a; Besley 2006; Ashworth 2012). In this paper, we adopt the lens of the canonical political agency framework to shed new light on the connection between electoral accountability and legislators' support for special interests. To test the idea that public scrutiny disciplines members of Congress, we turn to natural disasters, such as earthquakes, tornadoes, and hurricanes, as a source of exogenous variation in attention to politics. Consistent with the notion that adverse events distract the public, we demonstrate that when a disaster occurs, the evening news temporarily scales back coverage of politics. We then show that, when congressmen vote on bills immediately after such an event, they become significantly *more* likely to support the positions of their special-interest donors.

The effects we document are identified from the precise timing of disasters, and they are present even for legislators whose constituents were not directly affected. We find little evidence of either crowd-out of politics reporting or moral hazard in roll-call votes in the days immediately before a disaster strikes. In addition, we find no evidence to suggest that bills which are voted on in the aftermath of the event are systematically different from those that are considered before. They are neither associated with more interest group money, nor with a greater number of interest groups taking a public stand. Instead, natural disasters cause legislators whose donors support the bill to vote in favor of passage, whereas lawmakers who are backed by special interests that oppose the measure become more likely to vote against it. In sum, our results suggest that a lack of attention and scrutiny induces moral hazard in congressional roll-call votes, which benefits special interests.

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, disappears upon controlling for legislator fixed effects. Based on their review of published findings, Ansolabehere et al. (2003, p. 125) argue that "rent-seeking donors lack the leverage to extract large private benefits from legislation." At the same time, there exists ample evidence that special interests allocate campaign contributions strategically (e.g., Barber 2016; Bertrand et al. 2018; Bombardini and Trebbi 2011; Fouirnaies and Hall 2018; Powell and Grimmer 2016), creating a suspicion that they expect to receive something in return.

We add to this literature by demonstrating that members of Congress are more likely to adopt the position of special-interest donors when disasters distract the public. Although we cannot identify the causal effect of money on legislator behavior, we do provide evidence that informational frictions are important for electoral accountability and the extent to which the positions of special interests are reflected in policy.

We also contribute to the literature on the political economy of mass media (see DellaVigna and Gentzkow 2010; Prat and Strömberg 2013; and Strömberg 2015a,b for reviews). Particularly relevant for us is prior work by Eisensee and Strömberg (2007), Snyder and Strömberg (2010), and Durante and Zhuravskaya (2018).[1] Eisensee and Strömberg (2007) and Durante and Zhuravskaya (2018) both explore the connection between attention and actions of the executive branch. The former demonstrate that disaster relief by the U.S. government depends on whether the news is preoccupied with other, unrelated events. The latter show that the Israeli government schedules military attacks to coincide with times of predictably high news pressure in the U.S. and thus low levels of attention to the Israeli-Palestinian conflict.

Snyder and Strömberg (2010) study the effect of voter information on electoral accountability. Relying on variation in the extent to which newspaper markets overlap with congressional districts, they establish that members of Congress vote less along party lines and are more likely to stand witness before congressional hearings when voters are better informed about their representatives due to more relevant reporting. In addition, by exploiting changes in market congruence introduced through redistricting, Snyder and Strömberg (2010) provide suggestive evidence "that selection effects are entirely responsible for the ideological moderation in roll-call voting, whereas incentive effects are entirely responsible for the increase in witness appearances" (Strömberg 2015a, p. 616).

Going beyond prior work, we exploit the timing of natural disasters to identify moral hazard in roll-call votes, holding the media's effect on the selection of officeholders fixed. In a world of reduced scrutiny, special interests benefit because members of Congress systematically tilt their votes in favor of campaign donors. Our results, therefore, suggest that, even in the short-run, attention to politics is important because it disciplines politicians.

In independent, concurrent work, Balles et al. (2018) also explore the effect of disasters on legislators' alignment with special interests. Balles et al. (2018) rely on nearly the same

[1]Other notable contributions to this literature include Strömberg (2004b) on radio's impact on public spending, 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), Knight and Chiang (2011), and Enikolopov et al. (2011) on the effects of (biased) media on electoral outcomes.

set of data sources, and they estimate econometric models that are similar to the ones below. As a consequence, their headline result is virtually the same as ours. A noteworthy difference between both papers is that we actually measure news coverage of politics. We are, therefore, able to provide direct evidence in support of the claim that disasters crowd out relevant reporting. Further, our empirical strategy eschews man-made disasters as sources of identification. Including adverse events, such as terrorist attacks and mass shootings, might be problematic if interest groups themselves modify their positions in response to the incident—think, for instance, of the NRA and the push for gun control. A downside of excluding man-made disasters is that we are left with less statistical power.

3. Econometric Approach

Any test of the idea that legislators are more inclined to support the positions of special-interest donors when their decisions are less likely to be scrutinized has to deal with two separate endogeneity issues. First, special interest groups tend to donate to like-minded politicians. Thus, even if lawmakers vote in line with interest group preferences, it is *a priori* unclear whether such a correlation reflects the impact of contributions on votes or vice versa. Second, news reporting may influence politicians' behavior, but it is also a function of voter interest, interest group power, and the actions of officeholders themselves. To overcome these potential biases, we pursue an empirical strategy that relies on natural disasters as a source of plausibly exogenous variation in how much attention the public pays to politics.

There are two key conditions for our approach to be informative about the connection between electoral accountability and lawmakers' alignment with special interests. *(i)* The timing of natural disasters must be independent of U.S. politics. *(ii)* Disasters must temporarily reduce scrutiny, for which our measure of news reporting must be a valid proxy. Although Snyder and Strömberg (2010) show that media coverage of politicians improves different dimensions of accountability, this is an assumption that we cannot directly test because legislators' true incentives are not observable.

We can, however, verify that natural disasters reduce politics coverage on the evening news. To this end, we follow an event-study approach and estimate $\{\varphi_t\}$ in the following econometric model:

$$(1) \qquad PoliticalNews_t = \kappa_m + \theta_d + \sum_{s=-5}^{5} \varphi_{t+s} Disaster_{t+s} + \eta_{l,r,t},$$

where $PoliticalNews_t$ corresponds our summary measure of politics coverage on day t, $Disaster_t$ is an indicator for the start date of a disaster, while κ_m and θ_d denote month-by-year and day-of-the-week fixed effects, respectively. By including these, our estimates are identified by the precise timing of disasters rather than, say, seasonal variation.

To be clear, crowd-out of politics coverage on the national evening news is by no means the only channel through which disasters may affect attention to politics. Disasters may also displace relevant reporting by other media, such as local newspapers and radio, and they may reduce discussions among citizens. We, therefore, think of politics reporting on the evening news as merely a proxy for attention and public scrutiny.

In order to provide evidence that natural disasters lead to a simultaneous increase in the tendency of legislators to support the positions of their special-interest donors, we estimate the reduced-form model

$$SIV_{l,r,t} = \kappa_m + \theta_d + \mu_l + \beta NetMoney_{l,r,t} + \sum_{s=-5}^{5} \gamma_{t+s} NetMoney_{l,r,t} \times Disaster_{t+s} + \varepsilon_{l,r,t}, \tag{2}$$

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 position of the interest groups that gave the most money to her campaign. $NetMoney_{l,r,t}$ corresponds the monetary contributions from these groups minus any potential donations from organizations that took the opposite stand on the bill, and μ_l denotes a legislator-by-congress fixed effect. All other symbols are as defined above.

Compared to other studies in the literature on special interest politics, the specification in equation (2) differs primarily by allowing for the correlation between campaign contributions and votes to vary depending on whether the bill in question was considered promptly before or after a natural disaster. As in previous work, the causal effect of interest group donations cannot be consistently identified if special interests are more likely to give to lawmakers who already share their views. That is, we cannot recover the true β if $\text{Cov}(NetMoney, \varepsilon) \neq 0$.[2] For our purposes, however, the more important question is whether we can consistently identify $\{\gamma_t\}$, the disaster-induced difference in the observed correlation. In the appendix, we prove that, as long as the occurrence of natural disasters is as good as random, the answer turns out to be "yes." That is, even if $\text{Cov}(NetMoney, \varepsilon) \neq 0$, as long as $Disaster$ is independently distributed of the covariates as well as the error term, $\text{plim}\, \hat{\gamma}_{OLS} = \gamma$. As a consequence, we can test whether disasters cause members of Congress to side with their special-interest donors.

Note, it is not directly relevant whether campaign contributions buy votes or whether politicians change their behavior because weaker electoral constraints allow them to vote their own conscience, which happens to be aligned with the positions of special interests.

[2] Legislator fixed effects do not fully resolve this issue because they do not control for *within*-congressman heterogeneity in the assessment of bills, which may well be correlated with the positions of connected special interest groups.

What matters is whether the distraction that follows disasters induces moral hazard in roll-call voting. This is the theoretical prediction of interest, and the one that we explore below.

4. Data and Descriptive Statistics

Implementing our empirical strategy requires information on *(i)* connections between politicians and special interests, *(ii)* the positions of special interest groups on particular pieces of legislation, *(iii)* legislators' votes on the same measures, *(iv)* the occurrence of natural disasters, and *(v)* media coverage of politics.

Information on *(i)*–*(iii)* comes from MapLight, a nonpartisan organization that strives to reveal money's influence on politics. MapLight draws on 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 or amendments that are not merely ceremonial). Using campaign contribution data provided by the Center for Responsive Politics, MapLight links an organization's position on a particular bill to its donations to individual congressmen as well as the relevant roll-call votes. Starting with the 109th Congress (2005), MapLight publishes these data on its website.

Our empirical approach 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 final passage votes rather than votes on individual amendments because it is much rarer for interest groups to take an explicit, public stand on an amendment. Moreover, these votes are substantively important. Any bill requires a House vote on final passage before it can be signed into law—though many uncontroversial measures receive only voice votes, which are not recorded.

We say that a member of Congress votes with special interests whenever her roll-call vote coincides with the position of the interest groups that gave the most money to her campaign. Consider, for example, Patrick McHenry's (R, NC–10) choice on the recent "Reforming CFPB Indirect Auto Financing Guidance Act," which sought to weaken consumer protections in auto lending. According to MapLight, McHenry received a total \$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 special interests. We further, define the variable $NetMoney$ as the absolute value of the difference in total contributions from groups on both sides of the issue, and we use it to proxy for the strength of a congressman's ties to special interests.[3]

[3] As Bertrand et al. (2018) note, there are many other ways in which interest groups can influence lawmakers, including lobbying and even donations to connected charities. The reporting requirements for these

Following Eisensee and Strömberg (2007) and Durante and Zhuravskaya (2018), 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. 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 assesses these criteria based on various sources, including UN agencies, non-governmental organizations, insurance companies, press agencies, as well as other research institutes.

Although EM-DAT contains data on adverse events worldwide, we limit our sample to natural disasters that occurred after 2005 and within the United States. There are two reasons for this restriction. First, our data on special interest groups' positions on bills starts only in 2005. Second, domestic disasters draw much more coverage by U.S. media and are, therefore, more likely to crowd out politics reporting than foreign ones.[4] We further restrict attention to sudden-onset disasters that fall into the top tercile of events in terms of either the number of deaths, people affected, or damages. This restriction is useful to filter out minor incidents that are unlikely to distract a significant part of the public as well as those for which the start date is too imprecisely defined to obtain sharp identification (i.e., epidemics, heat waves, or wildfires). All in all, we are left with 200 domestic disasters over a thirteen-year period.[5]

Again, following Eisensee and Strömberg (2007) and Durante and Zhuravskaya (2018), we rely on the Vanderbilt Television News Archive (VTNA) for information on TV news broadcasts. 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 Fox News (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 and order of appearance. 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

activities, however, are typically less strict, which makes campaign contributions one of the few proxies that are readily quantifiable (de Figueiredo and Richter 2014).

[4]In Appendix A, we show that including large foreign disasters yields qualitatively similar but weaker results (cf. Appendix Figure A.10 and Table A.2). Interestingly, the effect of disasters on politics coverage on the evening news is almost solely due to domestic events. In other words, even large foreign disasters lead to negligible crowd-out of politics reporting, which may explain why legislators are less likely to react to them.

[5]In Appendix A, we show that our main results remain qualitatively unchanged if we included all domestic natural disasters recorded in EM-DAT, although some of the estimates become smaller (cf. Appendix Figure A.8).

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 (cf. Interactive Advertising Bureau 2013). This taxonomy 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 and provide a foundation for *ex post* analysis, such as A/B testing. Critical for our purposes, Watson's taxonomy contains a category for content related to "law, government, and politics." We say that a particular story covers politics if Watson assigns a positive probability to the story belonging in either this high-level category or one of its subcategories, most of which are plausibly related to day-to-day politics, legislation, or other current issues that might be debated in Congress.[6]

With this classification of stories 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, $Politics_{n,t} \equiv (\sum_{s \in P_{n,t}} Duration_s)/(\sum_{s \in S_{n,t}} Duration_s)$, where $P_{n,t}$ denotes the set of news segments that Watson classifies as containing political content and $S_{n,t}$ is the set of all segments, including commercials. According to this measure, on an average day, the median network contained in VTNA spends about 29.7 percent of airtime reporting about political issues, with considerable day-to-day swings in either direction.

In our context, there are at least two reasons one might be concerned about a simple measure like this. First, politics reporting by CNN and Fox News is different in both scale and content from that on the evening news of the "big three," which may make newscasts on the former networks less representative of the content to which most Americans are exposed. Second, in 2014 VTNA stopped producing human-generated summaries of stories from weekday newscasts on CBS, NBC, and Fox News. As a consequence, conclusions about politics reporting in 2014 and thereafter could be systematically skewed by idiosyncrasies of the remaining networks.[7]

We address these issues by restricting attention to newscasts on ABC, CBS, and NBC, and by statistically controlling for systematic differences across networks. Specifically, we

[6]Watson assigns a confidence score of exactly zero to 70.53 percent of news segments. Cursory inspection of a random subset of results suggests that a cutoff of zero accords well with human judgment.

[7]In private communication, representatives from VTNA indicated that they scaled down on human-generated content in order to experiment with automated techniques. Unfortunately, these experiments have not yet been as successful as they had hoped.

estimate the following regression model

$$(3) \qquad Politics_{n,t} = \chi_t + \nu_{n,d} + \xi_{n,t},$$

where $Politics_{n,t}$ is our network-specific measure defined above, χ_t is a fixed effect for day t, and $\nu_{n,d}$ corresponds to a network-by-day-of-the-week fixed effect. The parameter of interest is χ_t, which recovers the amount of politics coverage that one would expect to see after adjusting for network idiosyncrasies and day of the week patterns in coverage. We, therefore, rely on $\hat{\chi}_t$ as our summary measure of politics reporting on day t.

To see that $\hat{\chi}_t$ captures meaningful variation, consider Figure 1. For illustrative purposes, we focus on 2012 and we scale $\hat{\chi}_t$ so that it corresponds to the duration of politics reporting on a standard 30-minute newscast (thick line). We then superimpose the start date of natural disasters (dashed lines). Several patterns stand out. First, there is substantial high-frequency variation in politics coverage on the evening news, with some of the peaks occurring around the same time as significant political events, such as Election Day. Second, although we already restrict attention to nontrivial disasters, adverse events like floods, tornadoes, or hurricanes, are not particularly rare. Third, many of the disasters in our data coincide with temporary lows in politics reporting. For instance, landfall of Superstorm Sandy on October 29 coincided with politics coverage roughly 4.5 minutes, or about 50 percent, below normal—even though the presidential election was little more than a week away.

Table 1 presents descriptive statistics for the most important remaining variables in our analysis. On average special interest groups that support a bill give a total of about \$12 million to legislators, while those that oppose the measure contribute approximately \$3.6 million. The lawmakers who receive these donations vote with their special-interest donors roughly 81% of the time. Of course, it is unclear whether this is because legislators were bought off or because special interests support like-minded politicians.

5. Disasters, News, and Roll-Call Votes

As explained above, our goal is to test the idea that a reduction in public scrutiny benefits special interest groups by inducing moral hazard in roll-call voting. We do not claim to identify the effect of campaign contribution on votes. In principle, a reduction in scrutiny may benefit special interests by increasing the effectiveness of their donations, or it may enable legislators to pursue their personal goals, which happen to be correlated with those of the groups that support them. Either mechanism is consistent with our argument and with the results in Figure 2.

The upper panel of Figure 2 contains estimates of $\{\varphi_t\}$ in equation (1), i.e., the effect of natural disasters on politics coverage on the evening news. The lower two panels display estimates of $\{\gamma_t\}$ in equation (2), the impact of disasters on the correlation between special-interest money and legislative votes. The bottom and middle panel differ in that the former excludes all legislators from states that were directly affected by the disaster.

Consistent with the idea that natural disasters cause the public to pay less attention to the political process, the evidence in Figure 2 shows that, on the day the disaster strikes, the news reduce politics reporting by slightly more than .15 standard deviations, or approximately 0.6 minutes. This effect lasts for about three days, after which coverage returns to normal.

Interestingly, there is no evidence to suggest that crowd-out of politics reporting occurs before the onset of the disaster. This is surprising because some events, like hurricanes or major storms, are often predictable a few days in advance, and newscasts tend to issue warnings. In fact, in Appendix A we show that the evening news does air more disaster-related content in the days before the event. Taken together, the evidence suggests that natural disasters crowd out other content, such as celebrity news, before they affect stories about politics. In any case, what is essential for our argument is that the occurrence of a disaster temporarily reduces media coverage of politics.

Moreover, the reduction in coverage *coincides* with a change in the behavior of congressmen, regardless of whether their constituents were directly affected by the disaster. In particular, the estimates in the lower two panels of Figure 2 imply that, as a result of the event, lawmakers vote more in support of the position taken by their special-interest donors. This effect is statistically distinguishable from zero on the day of the disaster as well as the day after, when it begins to dissipate.

To put the point estimates into perspective, note that, in our data, $\hat{\beta} \approx .020$, whereas $\hat{\gamma}_0 \approx .035$ and $\hat{\gamma}_1 \approx .025$. Thus, the correlation between money and votes more than doubles in the immediate aftermath of a disaster.

Given that natural disasters affect legislators' support for special interests irrespective of whether their constituents were actually affected, we can rule out that our findings are driven by the direct fallout from the event, i.e., loss of life or economic damages. Moreover, since the effect of disasters on votes disappears as soon as attention to politics returns to normal, we also dismiss any kind of long-term mechanisms, such as changes in beliefs or changes in fundraising strategies. In our view, the most plausible explanation for the patterns in Figure 2 is moral hazard.

Though we cannot observe the strength of legislators' true electoral concerns, it is worth noting that the overall effect of disasters on how much scrutiny is applied to individual members of the House is probably small. Natural disasters distract from politics for only so long, meaning that some constituents might still learn about their representative's behavior a few days after the fact. Furthermore, regardless of whether the public is watching contemporaneously, legislators' roll-call votes become part of the congressional record and may be held against them during a potential reelection campaign. Our estimates thus suggest that legislators are quite responsive to temporary changes in incentives.

An inherent threat to our identification strategy is that natural disasters may not be entirely exogenous to the business of Congress. While year-by-month fixed effects deal with the concern that our findings are due to covariation in political cycles and seasonal patterns in, say, hurricanes or tornadoes, exogeneity could fail if a disaster induces last-minute changes in the content of a bill that is about to receive a passage vote.

In this context, it is important to distinguish between changes or amendments to bills that are caused by a reduction in scrutiny and are designed to please special interests—including the strategic rescheduling of sensitive votes—and more innocuous changes, which may nonetheless be correlated with the positions of large donors. The former mechanism is fully consistent with the idea that a temporary reduction in electoral accountability benefits interest groups; though it implies that the effect in Figure 2 may operate through actions of the leadership rather than the choices of rank-and-file members. The latter mechanism is not consistent with our argument, which is why we address it explicitly.

For last-minute changes to bias our estimates, it would have to be the case that the content of a bill is altered in a nonstrategic way that is directly related to the occurrence of a disaster and, at the same time, makes it better aligned with the positions of interest groups. Perhaps the most likely scenario is that an amendment providing disaster relief is getting attached to a measure that is already scheduled for a vote. Such an amendment would likely increase how palatable the overall package is to individual representatives; and, since most special-interest money is given in support of bills, it may lead to upward-biased estimates of $\{\gamma_t\}$. We deal with this potential issue in four ways.

First, in Appendix A, we turn to congressional speech to explore whether legislators become more likely to discuss disasters and potential relief efforts. The answer is "no"—at least not in the immediate aftermath of the event (cf. Appendix Figure A.4).

Second, we ask whether the House is more likely to vote on amendments around the time a disaster strikes. Again, the answer is "no" (cf. Appendix Figure A.5).

Third, we re-estimate the model in equation (2), excluding all bills whose title or description by the Congressional Research Service indicates that they may, in some way, be related to an emergency or disaster.[8] Our results are qualitatively and quantitatively robust to discarding these measures (cf. Appendix Figure A.11).

[8]Specifically, we exclude any bill whose description contains one of the following strings: "disaster," "emergency," "relief," "help," "rebuild," "assistance," "victim," "storm," "hurricane," "tornado," "flood," "landslide," "earthquake," or "volcano."

Fourth, we assess whether disasters directly affect support for bills by estimating the following econometric model:

$$
\begin{aligned}
(4) \qquad Yea_{l,r,t} = {} & \beta^{(+)} Money^{(+)}_{l,r,t} + \beta^{(-)} Money^{(-)}_{l,r,t} + \\
& \gamma^{(+)} Money^{(+)}_{l,r,t} \times Disaster^{(01)}_t + \gamma^{(-)} Money^{(-)}_{l,r,t} \times Disaster^{(01)}_t + \\
& \delta Disaster^{(01)}_t + \kappa_m + \theta_d + \mu_l + \varepsilon_{l,r,t}
\end{aligned}
$$

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. To increase statistical power, we let $Disaster^{(01)}_t$ be an indicator for whether the roll call occurred within a day after a disaster. If the effect of disasters on votes operates through relief amendments, then we would expect that $\hat{\delta} > 0$, while $\hat{\gamma}^{(+)} = \hat{\gamma}^{(-)} = 0$. If, however, the effect operates through a temporary reduction in accountability, then we should see that $\hat{\gamma}^{(+)} > 0$, $\hat{\gamma}^{(-)} < 0$, while $\hat{\delta} = 0$. That is, representatives should bifurcate, depending on which side of the issue their donors stand.

Table 2 presents results from estimating variants of equation (4) on our data. Three of four estimates of δ have the "wrong" sign, and none are statistically distinguishable from zero. By contrast, all of the estimates for $\hat{\gamma}^{(+)}$ and $\hat{\gamma}^{(-)}$ imply bifurcation. The evidence in Table 2, therefore, suggests that passage of a bill does not become intrinsically more desirable. Instead, disasters cause congressmen to pivot towards their respective donors.[9]

Even if we accept the assumption that the precise timing of natural disasters is exogenous to the political process, it is plausible that our results are driven by the strategic scheduling of politically sensitive roll calls, i.e., by agenda setting as opposed to moral hazard. Indeed, the negative (but statistically insignificant) estimate for γ on day $t = -1$ may suggest that votes on sensitive bills are strategically delayed. In order to directly test for anticipation and rescheduling effects, we ask two related questions. *(i)* Does the House use disasters as an opportunity to pass legislation? If so, we would expect a flurry of activity around the time of the event. *(ii)* Are bills that are considered around the time a disaster strikes associated with a larger number of position-taking interest groups or with more special-interest money (as one might expect if the leadership strategically timed politically sensitive votes)?

Figure 3 shows that neither of these hypotheses is supported by the data. Specifically, none of the outcomes we study exhibit a sharp "on impact" effect akin to that in Figure 2. Moreover, roll calls held within a couple days after the disaster are *not* associated with more

[9]Note, for psychological effects such as goodwill or guilt to explain our findings in Table 2, it would need to be the case these effects are, on average, positive for lawmakers whose special interest donors support the bill, but negative for those whose backers oppose it. In other words, the sign of some psychological effect would need to depend on the position of connected special interests.

special interest groups taking a stand, and there appears to be *no* effect of disasters on the average amount of money given by interest groups. In Appendix A, we provide additional evidence suggesting that the House considers about the same number of amendments but holds fewer procedural votes when a disaster strikes. It also does not appear to be the case that the leadership attempts to accelerate the legislative process by suspending the rules (cf. Appendix Figures A.5–A.7). In sum, we find no evidence of either a flurry of activity or an "on impact" effect on the political delicacy of bills that receive a vote.

6. Conclusion

Principal-agent models in political economy predict that politicians are more likely to act in the best interest of their constituents when the latter are better informed about the former's actions (see, e.g., Ashworth, 2012). Extant empirical work supports this idea by showing that newspaper coverage of politicians increases different proxies of accountability (Snyder and Strömberg, 2010). We build on these insights in order to provide novel evidence on short-term information frictions and moral hazard in Congress.

Our results show that, when a disaster occurs, the evening news scales back coverage of politics for about three days. While we measure coverage only on the national level, it is plausible that politics reporting on the evening news proxies for attention to politics more generally. Any reduction in coverage of politics on the "big three" networks is likely accompanied by a reduction in relevant reporting in local newspapers, coverage of individual legislators, and conversations about current affairs. Though all roll-call votes become public record and can be held against legislators running for re-election, votes in the aftermath of a disaster are associated with less media attention in the short-run and, thus, conceivably altered incentives—especially if politicians receive disutility from negative press, if voters are subject to information-processing constraints, or if they are more likely to believe media reports about contemporaneous wrongdoing than allegations by a rival campaign. Members of Congress react to the temporary reduction in scrutiny by becoming significantly *more* likely to support the positions of their special-interest donors on important pieces of legislation. Taken together, our findings imply that special interest groups benefit when the public is not watching. More broadly, our results suggest that media coverage and attention to politics improve accountability by disciplining politicians.

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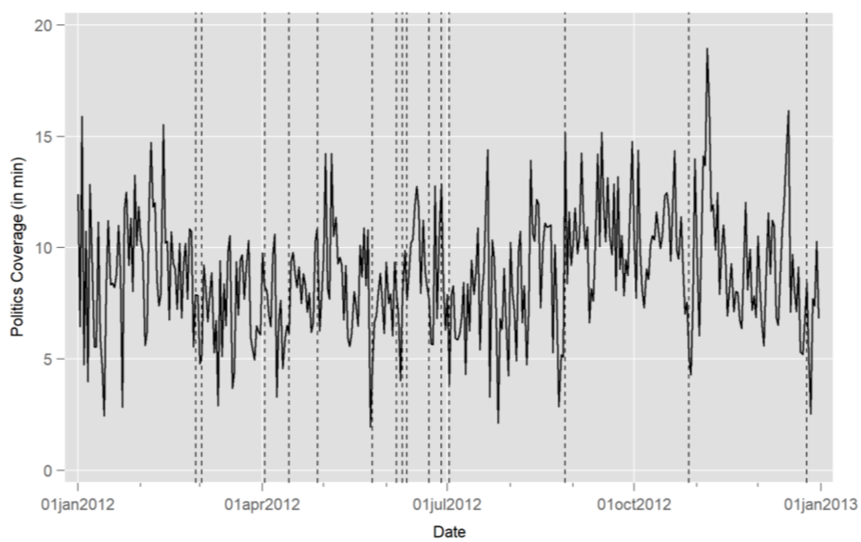
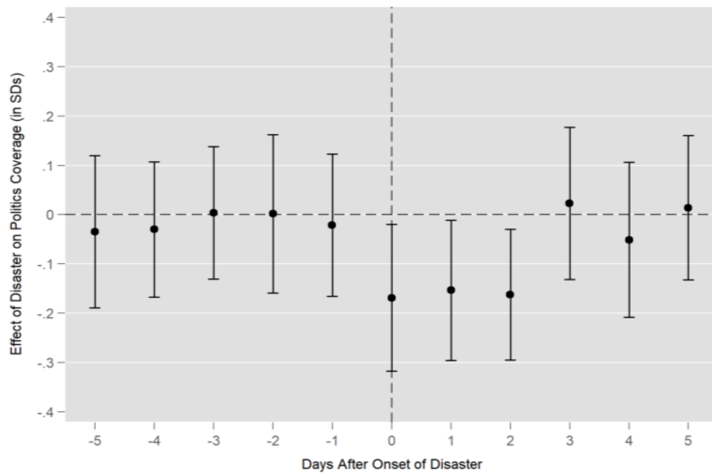
Figure 1: Coverage of Politics on the Evening News in 2012



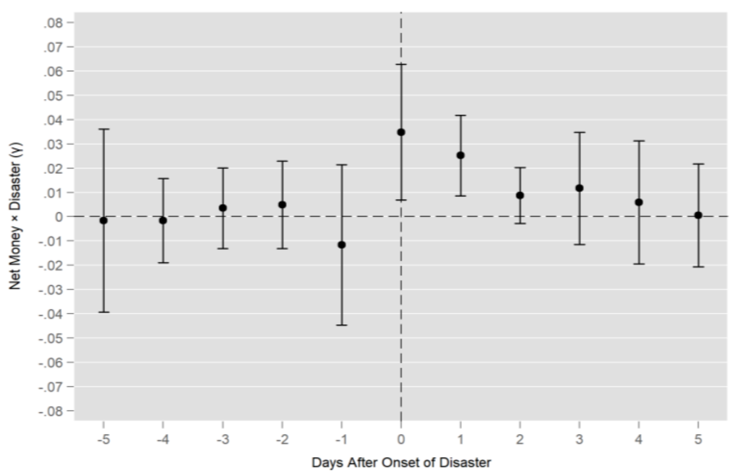
Notes: The thick line shows politics coverage in minutes on a standard 30-minute newscast, based on a rescaling of χ_t in equation (3). The dashed lines indicate the onset of natural disasters in 2012. For a detailed description of the underlying data, see the Data Appendix.

Figure 2: Impact of Natural Disasters

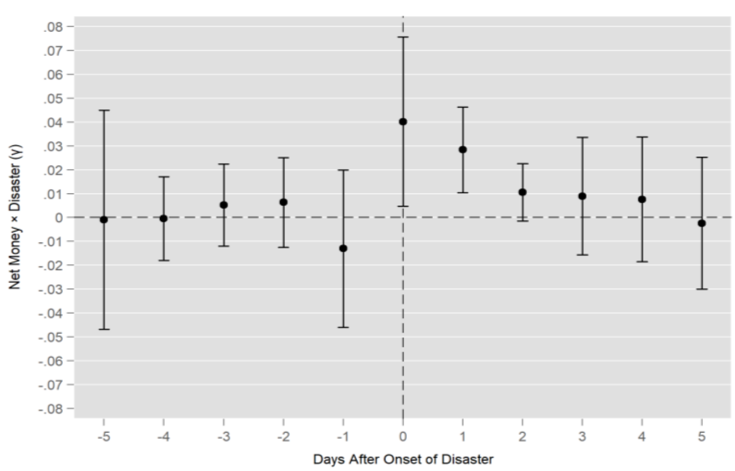
A. Coverage of Politics



B. Voting with Special Interests, All Representatives



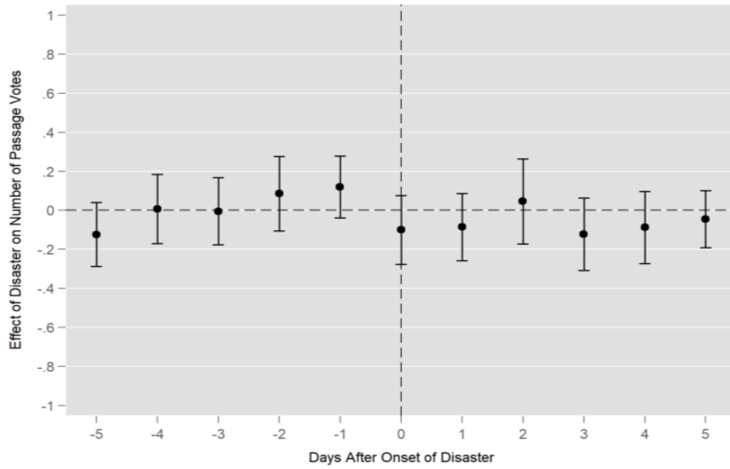
C. Voting with Special Interests, Excluding Representatives from Affected States



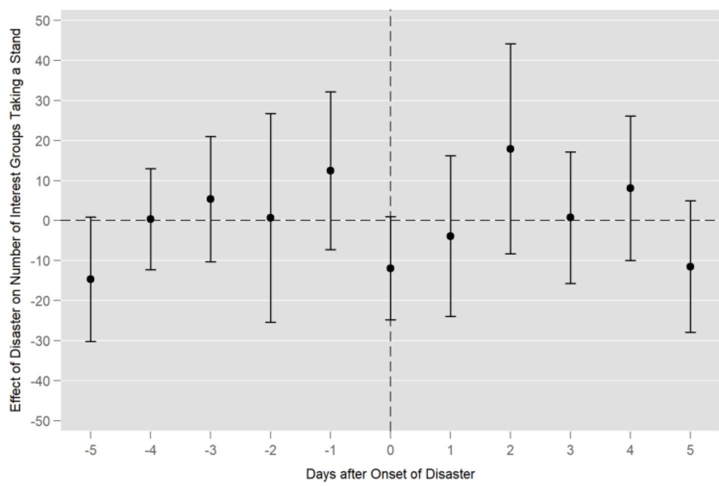
Notes: The upper panel displays point estimates and 95%-confidence intervals for the impact of natural disasters on politics coverage, i.e., φ_t in equation (1). The lower two panels display estimates of the effect of disasters on the partial correlation between interest group donations and congressmen's votes, i.e., γ_t in specificaton (2). Results in the middle panel are based on the votes of all representatives who received donations from special interest groups that took a position on the bill. The bottom panel excludes all representatives from states that are affected by the disaster. Confidence intervals in the upper panel account for clustering by year-month, while those in the lower two panels are two-way clustered by year-month and legislator.

Figure 3: Strategic Rescheduling of Roll Calls?

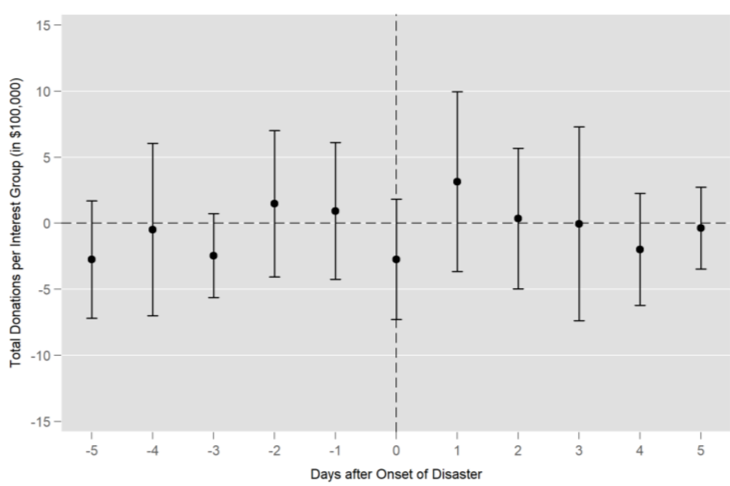
A. Total Number of Passage Votes



B. Number of Interest Groups Taking a Stand



C. Total Donations per Interest Group



Notes: The upper panel shows point estimates and 95%-confidence intervals for the impact of natural disasters on the total number of passage votes conducted in the House on a particular day. The middle panel considers the number interest groups taking a stand on any of the bills considered that day, while the lower panel displays the estimated effect on the total amount that a special interest group gives to all legislators voting on that day. All estimates are based on the regression model in equation (1), with different left-hand-side variables. Confidence intervals account for clustering by year-month.

Table 1: Descriptive Statistics

Variable	Mean	SD	Min	Median	Max
Daily Time Series (N = 4,497):					
Disaster	.043	.204	0	0	1
Coverage of Politics (χ)	.298	.107	-.028	.296	.909
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
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	17.4	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	.844	0	.05	52.194
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. A bill may receive more than one passage vote in the House when a representative who previously voted with the majority makes a motion to reconsider, or when the Senate changes the bill. For precise definitions of all variables, see the Data Appendix.

Table 2: Natural Disasters and Votes

	Vote "Yea" on Passage			
	(1)	(2)	(3)	(4)
Money from Supporting Interest Groups ($\beta^{(+)}$)	.020***	.018***	-.003	.008**
	(.004)	(.004)	(.003)	(.004)
Money from Opposed Interest Groups ($\beta^{(-)}$)	-.181***	-.178***	-.159***	-.128***
	(.025)	(.025)	(.022)	(.018)
Money from Supporting Interest Groups		.016*	.023**	.026**
× Immediate Aftermath of Disaster ($\gamma^{(+)}$)		(.009)	(.009)	(.010)
Money from Opposing Interest Groups		-.065**	-.064**	-.039*
× Immediate Aftermath of Disaster ($\gamma^{(-)}$)		(.029)	(.025)	(.022)
Immediate Aftermath of Disaster (δ)	-.012	-.012	-.022	.010
	(.022)	(.024)	(.022)	(.018)
Hypothesis Tests [p-values]:				
H_0: $\gamma^{(+)} \leq 0$	--	.050	.005	.004
H_1: $\gamma^{(-)} \geq 0$	--	.013	.006	.039
H_2: $\gamma^{(+)} = \gamma^{(-)} = 0$	--	.016	.001	.020
Fixed Effects:				
Legislator × Congress	No	No	Yes	Yes
Year × Month	No	No	No	Yes
Day of the Week	No	No	No	Yes
R-Squared	.046	.046	.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 equation (4) by OLS. Interest groups 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 a day 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. For a detailed description of the underlying data, see the Data Appendix.

APPENDIX MATERIALS

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Appendix A: Ancillary Results and Robustness Checks

In this appendix, we present several ancillary results and robustness checks. In Appendix Figure A.1, we show that the evening news starts to report about disasters a few days before the onset of the event, as mentioned in the main text. The measure of disaster reporting that underlies the results in this figure is constructed in the same way as our measure of politics reporting. That is, we use IBM Watson to identify news stories about natural disasters, calculate the fraction of airtime each news show devotes to such stories on a particular day, and then adjust this measure for the imbalanced nature of our panel using the regression model in equation (3) in the main text. The evidence in Figure A.1 also suggests that within five days after the event, news reporting about disasters returns to normal.

Appendix Figures A.2 and A.3 present the outcome of randomization inference for the effect of disasters on politics coverage and voting with special interests, respectively. More specifically, we create 10,000 surrogate data sets by randomly reshuffling the start dates of the disasters in our data. We then estimate the regression models in equations (1) and (2) on these placebo data and plot the distribution of the resulting $\{\hat{\gamma}_t\}$. Reassuringly, the evidence suggests that the true point estimates for $t = 0$ and $t = 1$ are large relative to what can be expected under the sharp null of no impact of natural disasters. Furthermore, our estimates for $t \in \{-5, ..., -1\}$ are consistent with no anticipatory effects.

Appendix Figures A.4–A.7 complement Figure 3 in the main text by shedding additional light on the business of Congress shortly before and after disasters strike. In Figure A.4, we rely on the data of Gentzkow et al. (2018) to study congressional speech. Restricting attention to the 109th–114th Congresses (i.e., the period covered by the data used in the main text), we process all speech in the House of Representatives by first removing stop words and then counting *(i)* all remaining words spoken on a particular day as well as *(ii)* the number of words that are plausibly related to natural disasters. Specifically, our keyword search for disaster-related words includes the following terms: "disaster," "emergency," "relief," "help," "rebuild," "assistance," "victim," "storm," "hurricane," "tornado," "flood," "landslide," "earthquake," or "volcano." We then estimate the regression model in equation (1) using these simple measures of speech as outcomes.

The upper panel in Figure A.4 shows that congressmen speak about as much as usual shortly before and after disasters. In other words, there does not appear to be an impact of disasters on the overall volume of speech in the House. The lower panel shows that there is also no effect on speech related to disasters, at least not immediately after the event. Thus, judging by what congressmen say on the floor, it does *not* appear to be the case that the House considers disaster relief bills immediately before or after the event. Unlike Figures 1 and 2, we find no "on impact" effect on congressional speech.

The upper panel in Appendix Figure A.5 demonstrates that there is a small negative effect on the total number of roll-call votes on the day after the disaster. The middle panel examines the number of votes on amendments. None of the estimates pertaining to amendments are statistically distinguishable from zero, though they are negative shortly after the event. A qualitatively similar picture emerges for "other" roll-call votes, i.e., votes that are neither on final passage nor amendments. In short, we find that there is no flurry of activity in the House around the time a disaster strikes.

Appendix Figure A.6 further shows that, if anything, disasters cause the House to consider fewer bills on which special-interest groups took a stand. Again, this effect is relatively small and short-lived. In addition, we show in Figure A.7 that there appears to be no effect of disasters on the number of roll calls to suspend the rules or on the number of votes under suspended rules. If the leadership was, in fact, trying to advance politically sensitive legislation while a disaster distracts the public, one might expect them to speed up the process by temporarily doing away with cumbersome rules and requirements. This is not the case.

Broadly summarizing, we find no evidence that disasters cause the House to expedite legislation that is important to special interests. If anything, legislative productivity seems to decline a little.

Appendix Figure A.8 replicates our main result in Figure 2 but considers *all* domestic natural disasters in the EM-DAT dataset (instead of only large sudden-onset events). The results are qualitatively very similar to those reported in the main text, though some of the point estimates are smaller and, as a result, only marginally statistically significant. The latter observation is consistent with the idea that only large disasters cause the public to pay less attention to politics.

Appendix Figure A.9 demonstrates that our finding of disaster-induced crowd-out of politics reporting is not sensitive to foregoing the regression adjustment in equation (3). More specifically, the figure shows the impact of natural disasters on coverage of politics, measured as $Politics_{n,t} \equiv (\sum_{s \in P_{n,t}} Duration_s)/(\sum_{s \in S_{n,t}} Duration_s)$ on the median of the "big three" networks rather than $\hat{\chi}_t$.

In addition, we show in Appendix Figure A.1 that our finding of an impact of disasters on politics coverage is robust to using alternative regression specifications. It is also robust to simply replacing the outcome with the raw duration of politics reporting on any of the networks covered by VTNA on a particular day.

Appendix Figure A.10 and Table A.2 present evidence using large domestic *and* foreign natural disasters as sources of exogenous variation. For this set of results, we continue to define large domestic disasters as in the main text, but we add 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. We restrict attention to the most adverse of events abroad because these are *a priori* the most likely ones to be covered by the American media. While the coefficients in Figure A.10 and Table A.1 exhibit patterns that are qualitatively similar to those in the main text, adding foreign disasters reduces the point estimates and renders many, but not all, of them statistically insignificant. In fact, replicating our results on news crowd-out and moral hazard in legislative voting by focusing exclusively on foreign disasters shows no effect on *either* outcome. This suggests that politicians react only to domestic disasters because only these events distract the public sufficiently much.

In Appendix Figure A.11, we replicate our findings in the lower panel of Figure 2, excluding all bills whose description by the Congressional Research Service suggests that they may be related to disaster relief. Specifically, as explained in footnote 8, we exclude all bills with a description containing at least one of the following terms: "disaster," "emergency," "relief," "help," "rebuild," "assistance," "victim," "storm," "hurricane," "tornado," "flood," "landslide," "earthquake," or "volcano." If anything, the results in Figure A.11 suggest that excluding these bills strengthens our main result.

Appendix Figure A.12 replicates our finding of disaster-induced moral hazard in congressional roll-call votes, i.e., the lower panel in Figure 2, using top-coded donations. This robustness check is useful because the distribution of interest group contributions is heavily right-skewed and top-coding total donations at $500,000 reduces the influence of outliers. Reassuringly, top-coding donations strengthens rather than weakens our findings.

Lastly, Appendix Figure A.13 provides an additional robustness check by controlling for disaster-specific fixed effects. That is, we exploit even more-granular identification by adding a fixed effect for each $+/-$ 10-day window around *every* disaster to the regression model in equation (2).[1] Again, the results are qualitatively and quantitatively similar to their counterparts in the main text.

Appendix B: Identification of $\{\gamma_t\}$

In the main text, we claim that $\{\gamma_t\}$ in equation (2) can be consistently identified as long as the occurrence of natural disasters is as good as random—even if interest group donations are correlated with the error term. Here, we modify a similar proof in Spenkuch (2012) to provide a formal econometric argument in support of this claim.

To see why $\{\gamma_t\}$ is well-identified consider the following simplified data generating process:[2]

$$\text{(B.1)} \qquad SIV_{l,r,t} = \alpha + \beta NetMoney_{l,r,t} + \gamma NetMoney_{l,r,t} \times Disaster_t + \varepsilon_{l,r,t}.$$

PROPOSITION: Consider the data generating process in equation (B.1) and suppose that $Disaster_t$ is independently distributed of other all covariates as well as the error term. Provided the usual full-rank condition is satisfied, the OLS estimate of γ is well identified. That is, $\text{plim}\,\hat{\gamma} = \gamma$, even if $\text{Cov}\left(NetMoney_{l,r,t}, \varepsilon_{l,r,t}\right) \neq 0$.

PROOF: By the Firsch-Waugh Theorem (Frisch and Waugh 1933),

$$\text{plim}\,\hat{\gamma} = \gamma + \frac{\text{Cov}\left(\widetilde{NetMoney_{l,r,t}Disaster_t}, \varepsilon_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney_{l,r,t}Disaster_t}\right)},$$

[1]Observations that fall within more than one event-specific window are assigned to the nearest disaster.

[2]Extending the proof to the model in equation (2) is straightforward, but requires considerably more notation.

where $NetMoney_{l,r,t}\widetilde{}Disaster_t$ denotes the residual from projecting $NetMoney_{l,r,t}Disaster_t$ on the vector $[1 \; NetMoney_{l,r,t}]$. It, therefore, suffices to show that $\text{Cov}\left(NetMoney_{l,r,t}\widetilde{}Disaster_t, \varepsilon_{l,r,t}\right) = 0$.

From the definition of $NetMoney_{l,r,t}\widetilde{}Disaster_t$ and using the Frisch-Waugh Theorem again, we obtain:

$$\text{Cov}\left(NetMoney_{l,r,t}\widetilde{}Disaster_t, \varepsilon_{l,r,t}\right) =$$

$$\text{Cov}\left(NetMoney_{l,r,t}Disaster_t - \zeta - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)} NetMoney_{l,r,t}, \varepsilon_{l,r,t}\right)$$

$$= \text{Cov}\left(\left[Disaster_t - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)}\right] NetMoney_{l,r,t}, \varepsilon_{l,r,t}\right),$$

where $\zeta = \text{E}\left(NetMoney_{l,r,t}Disaster_t\right) - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)} \text{E}\left(NetMoney_{l,r,t}\right)$, and $\widetilde{NetMoney}_{l,r,t}$ corresponds to the residual from projecting $NetMoney_{l,r,t}$ on a constant.

From the definition of the covariance, and applying the Law of Iterated Expectations, it follows that

$$\text{Cov}\left(\left[Disaster_t - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)}\right] NetMoney_{l,r,t}, \varepsilon_{l,r,t}\right) =$$

$$\text{E}\left(\left[Disaster_t - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)}\right] NetMoney_{l,r,t}\varepsilon_{l,r,t}\right)$$

$$- \text{E}\left(\left[Disaster_t - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)}\right] NetMoney_{l,r,t}\right) \text{E}\left(\varepsilon_{l,r,t}\right)$$

$$= \left[\text{E}\left(Disaster_t\right) - \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney}_{l,r,t}\right)}{\text{Var}\left(\widetilde{NetMoney}_{l,r,t}\right)}\right] \text{E}\left(NetMoney_{l,r,t}\varepsilon_{l,r,t}\right),$$

since $Disaster_t$ is independent of $\left(NetMoney_{l,r,t}, \varepsilon_{l,r,t}\right)$ and $\text{E}\left(\varepsilon_{l,r,t}\right) = 0$.

Given that $\widetilde{NetMoney}_{l,r,t}$ corresponds simply to the deviation of $NetMoney_{l,r,t}$ from its mean, we have

$$\frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, \widetilde{NetMoney_{l,r,t}}\right)}{\text{Var}\left(\widetilde{NetMoney_{l,r,t}}\right)} = \frac{\text{Cov}\left(NetMoney_{l,r,t}Disaster_t, NetMoney_{l,r,t}\right)}{\text{Var}\left(NetMoney_{l,r,t}\right)}$$

$$= \frac{\text{E}\left(Disaster_t NetMoney^2_{l,r,t}\right) - \text{E}\left(Disaster_t NetMoney_{l,r,t}\right)\text{E}\left(NetMoney_{l,r,t}\right)}{\text{Var}(NetMoney_{l,r,t})}$$

$$= \text{E}\left(Disaster_t\right)\frac{\text{E}\left(NetMoney^2_{l,r,t}\right) - \text{E}\left(NetMoney_{l,r,t}\right)^2}{\text{Var}(NetMoney_{l,r,t})}$$

$$= \text{E}\left(Disaster_t\right).$$

This shows that $\text{Cov}\left(\widetilde{NetMoney_{l,r,t}}Disaster_t, \varepsilon_{l,r,t}\right) = 0$, as desired.

Q.E.D.

Appendix C: Data Appendix

This appendix provides a self-contained description of all data used in the paper, as well as precise definitions together with the sources of the most important variables.

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

Our empirical approach 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. We further define the following variables:

Special Interest Vote (SIV) is an indicator variable equal to one if and only if a particular legislator's roll-call vote is aligned with the position of the set of special interest groups that donated more money to his campaign than the groups on the other side of the issue. If a lawmaker received no contributions from both supporting and opposing interesting groups, then *SIV* is coded as missing.

Vote "Yea" is an indicator variable equal to one if and only if a particular congressman votes in favor of passing the bill in question. If a lawmaker did not cast either a "yea"- or "nay"-vote, then this variable is coded as missing.

Net Money corresponds to the absolute value of the difference in total contributions from interest groups supporting and opposing the bill.

Money from Opposed Interest Groups corresponds to the total amount that all interest groups which opposed the bill in question contributed to a particular legislator's campaign, as reported by MapLight.

Money from Supporting Interest Groups corresponds to the total amount that all interest groups which supported the bill in question contributed to a particular legislator's campaign, as reported by MapLight.

Number of Opposed SIGs corresponds to the number of special interest groups which opposed the bill in question, as reported by MapLight.

Number of Supporting SIGs corresponds to the number of special interest groups which supported the bill in question, as reported by MapLight.

Total Contributions from of Opp. Groups corresponds to the total of campaign contributions that were made to all members of the House by special interest groups which opposed the bill in question, as reported by MapLight.

Total Contributions from of Supp. Groups corresponds to the total of campaign contributions that were made to all members of the House by special interest groups which supported the bill in question, as reported by MapLight.

C.2. *EM-DAT*

Data on natural disasters come from the Centre for Research on the Epidemiology of Disasters (CRED) at the Universit 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 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. In Appendix A, we show that our results are qualitatively similar but weaker when we also include large foreign disasters. 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. Again, we show in Appendix A 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.

In our regression models, we rely on the following variables:

Disaster is an indicator variable that is equal to one on the very first day an adverse event occurs and zero otherwise. In other words, *Disaster* marks the onset of a natural disaster.

$Disaster^{(01)}$ is an indicator variable that is equal to one on the first and second day an adverse event occurs and zero otherwise.

C.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 (cf. Interactive Advertising Bureau 2013), which defines

[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`.

contextual categories that were originally designed to consistently describe web content in order to facilitate more relevant advertising and allow for *ex post* analysis.

Watson's taxonomy contains a category for content related to "law, government, and politics." We say that a particular story covers politics if Watson assigns a positive probability to the story belonging in either this high-level category or one of its subcategories, most of which are plausibly related to day-to-day politics, legislation, or other current issues that might be debated in Congress. We then measure politics coverage by network n on day t as the the fraction of total airtime the newscast devoted to political matters. In symbols,

$$Politics_{n,t} \equiv (\sum_{s \in P_{n,t}} Duration_s)/(\sum_{s \in S_{n,t}} Duration_s),$$

where $P_{n,t}$ denotes the set of news segments that Watson classifies as containing political content and $S_{n,t}$ is the set of all segments that aired on that network's evening newscast on the same day, including commercials.

Since politics reporting by CNN and Fox News is different in both scale and content from that on the evening news of the "big three," we restrict attention to newscasts on ABC, CBS, and NBC. We further account for the imbalanced nature of our panel measure of politics reporting by statistically controlling for systematic differences across networks. Specifically, we estimate the following regression model

$$\text{(C.2)} \qquad Politics_{n,t} = \chi_t + \nu_{n,d} + \xi_{n,t},$$

where $Politics_{n,t}$ is the quantity defined above, χ_t is a fixed effect for day t, and $\nu_{n,d}$ corresponds to a network-by-day-of-the-week fixed effect.

Our main analysis relies on $\hat{\chi}_t$ as a summary measure of politics reporting on day t. In Appendix A, we show that our findings are qualitatively and quantitatively similar if we instead use $Politics_{n,t}$ on the median network contained in the VTNA data for a particular day.

In our regression models, we rely on the following variables:

Political News corresponds to $\hat{\chi}$ for the same day, estimated based on the specification above.

C.4. *Other Data Sources*

C.4.1. *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 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 Appendix A.

More specifically, we define the following variables:

Total Words corresponds to number of words (which are not stop words) that were spoken on the House floor on a particular day, as captured by the Congressional Record.

Disaster-Related Words corresponds to number of times one of the following terms is spoken on the House floor (according to the Congressional Record) on a particular day: "disaster," "emergency," "relief," "help," "rebuild," "assistance," "victim," "storm," "hurricane," "tornado," "flood," "landslide," "earthquake," or "volcano."

C.4.2. *Number of Votes & Types*

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 Lewis et al.'s (2018) *voteview.com* 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.

Specifically, we define the following variables:

Total Votes corresponds to the number of all roll call votes in the House that were held on a particular day.

Total Passage Votes corresponds to number of all House roll-call votes on passage held on a particular day, i.e., all roll calls assigned type codes 11–19 or 30 in the PIPC data.

Total Amendment Votes corresponds to number of all House roll-call votes pertaining to amendments that were held on a particular, day, i.e., all roll calls assigned type codes 21–29 in the PIPC data.

Total Other Votes corresponds to the number of roll calls in the House on a particular day that were neither related to passage nor to amendments.

Votes On/Under Suspended Rules corresponds to number of all House roll-calls that were held on a particular day either on the question of suspending the rules or under suspended rules. In the PIPC data, the roll calls are identified by one of the following type codes: 15–19, 29, 33, and 68.

References

Crespin, Michael H., and David Rohde. 2018. Political Institutions and Public Choice Roll-Call Database. available at `https://ou.edu/carlalbertcenter/research/pipc-votes/`

Frisch, Ragnar, and Frederick V. Waugh. 1933. "Partial Time Regressions as Compared with Individual Trends," *Econometrica*, 1(4), 387–401.

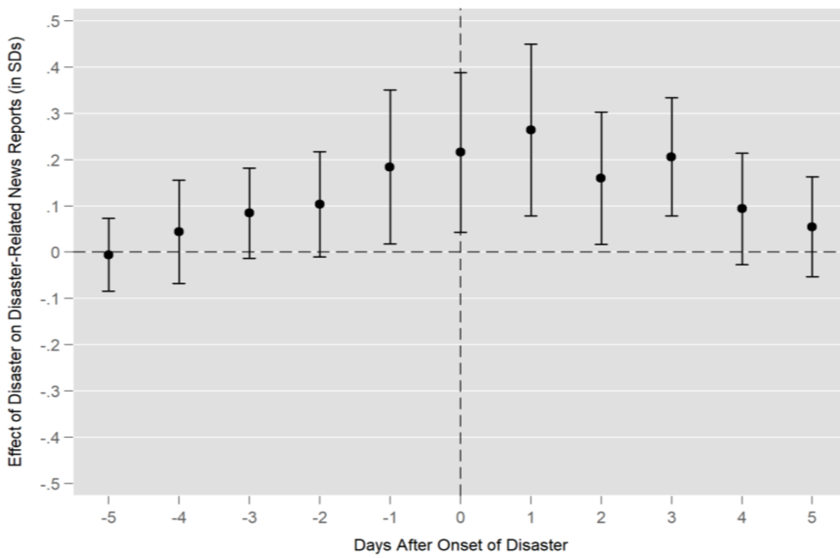
Gentzkow, Matthew, Jesse M. Shapiro, and Matt Taddy. 2018. Congressional Record for the 43rd–114th Congresses: Parsed Speeches and Phrase Counts. Palo Alto, CA: Stanford Libraries [distributor]. available at `https://data.stanford.edu/congress_text`.

Interactive Advertising Bureau. 2013. "IAB Quality Assurance Guidelines 2.0." retrieved from `https://www.iab.com/news/iab-releases-quality-assurance-guidelines-2-0` (July 1, 2017).

Lewis, Jeffrey B., Keith Poole, Howard Rosenthal, Adam Boche, Aaron Rudkin, and Luke Sonnet (2018). Voteview: Congressional Roll-Call Votes Database. available at `https://voteview.com/`.

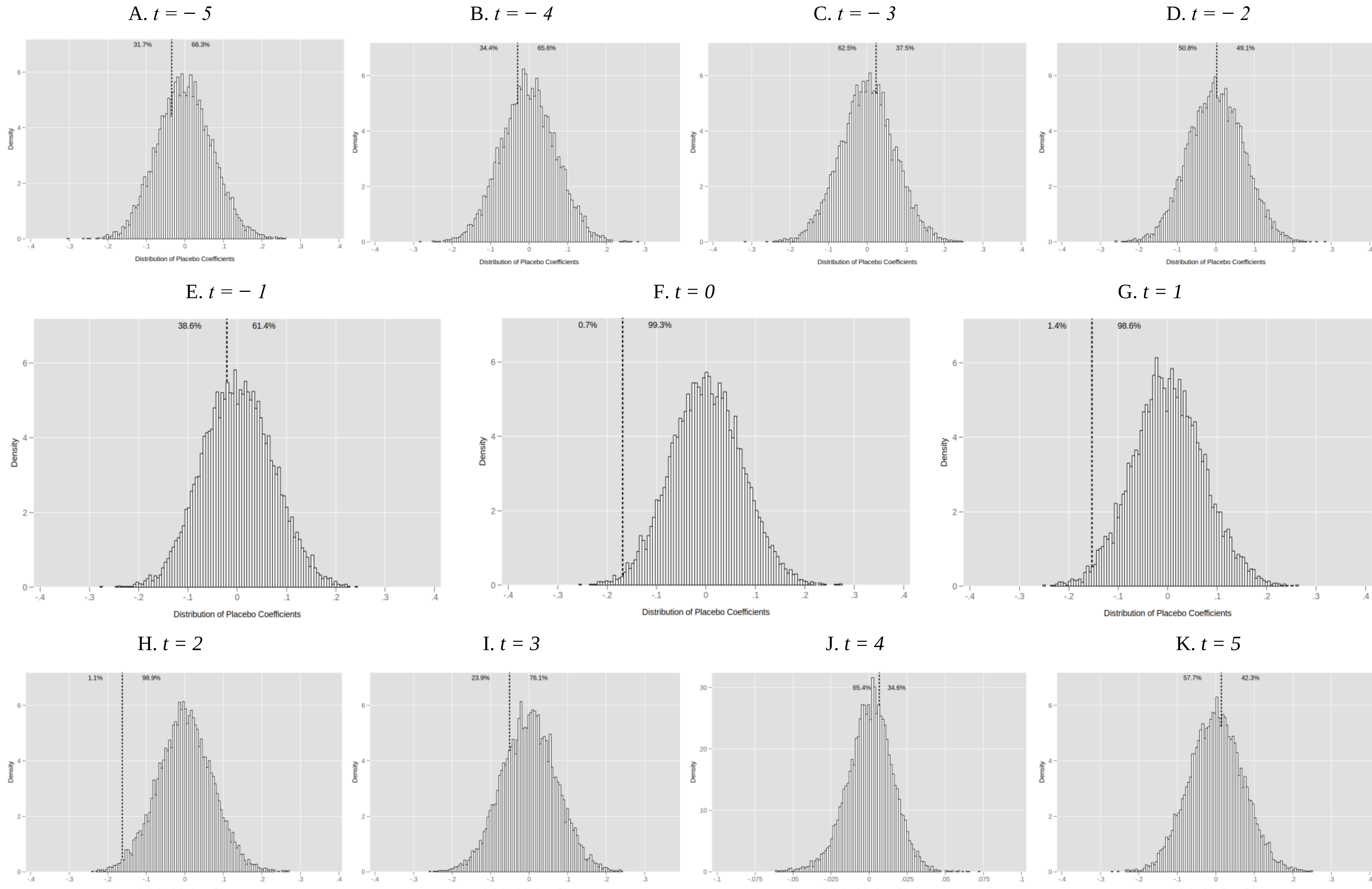
Spenkuch, Jörg L. 2012. "Moral Hazard and Selection Among the Poor: Evidence from a Randomized Experiment." *Journal of Health Economics*, 31(1): 72–85.

Appendix Figure A.1: Disaster-Related News Coverage



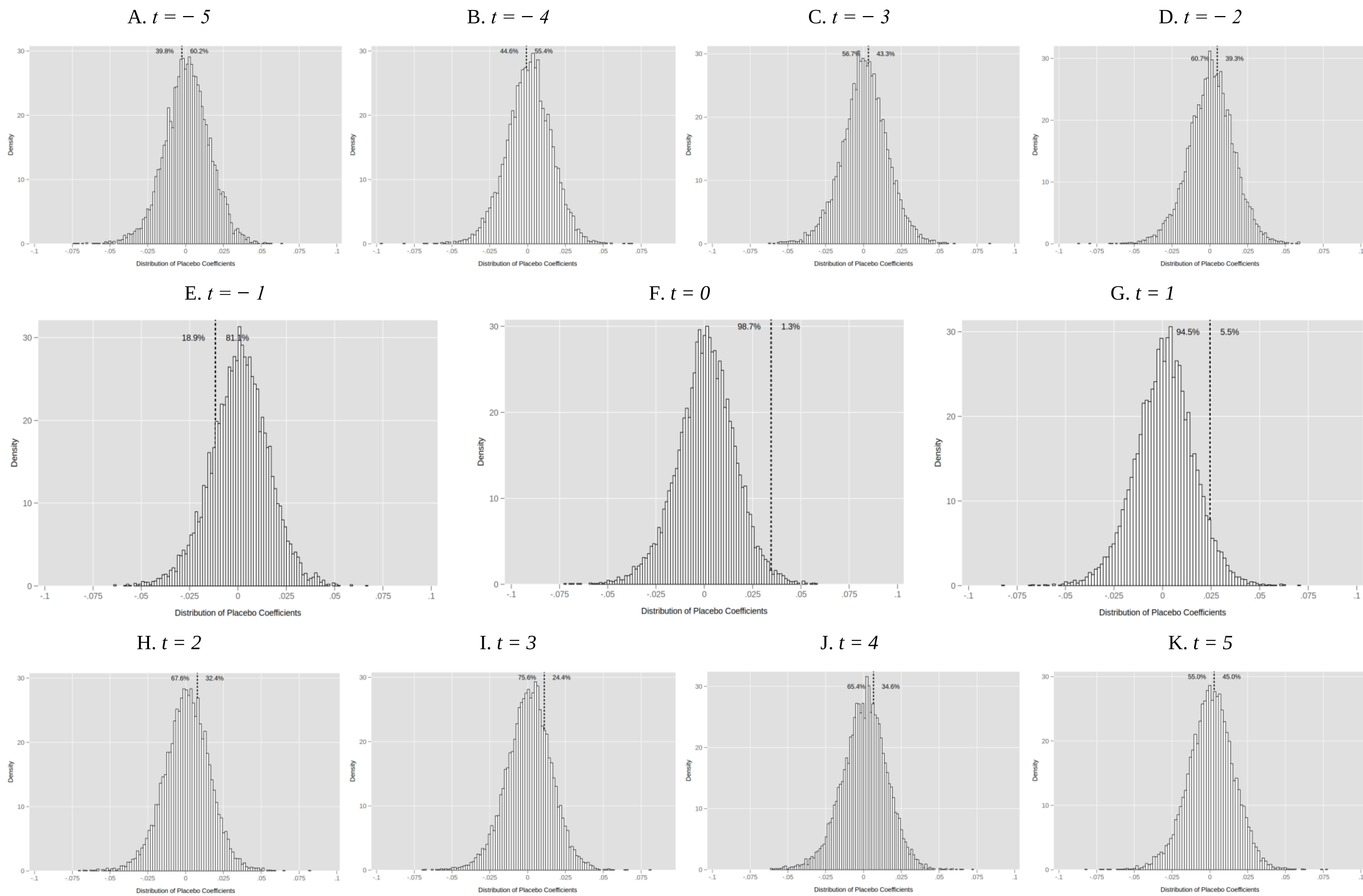
Notes: Figure shows point estimates and 95%-confidence intervals for the impact of natural disasters on disaster-related news coverage. All parameters are estimated by OLS, controlling for year-by-month and day-of-the-week fixed effects. Confidence intervals account for clustering by year-month.

Appendix Figure A.2: Randomization Inference for the Effect of Disasters on Politics Coverage



Notes: Figure shows the distribution of placebo estimates for the impact natural disasters on politics coverage, i.e., φ_t in equation (1). All graphs are based 10,000 regressions, with randomly reshuffled start dates of disasters.

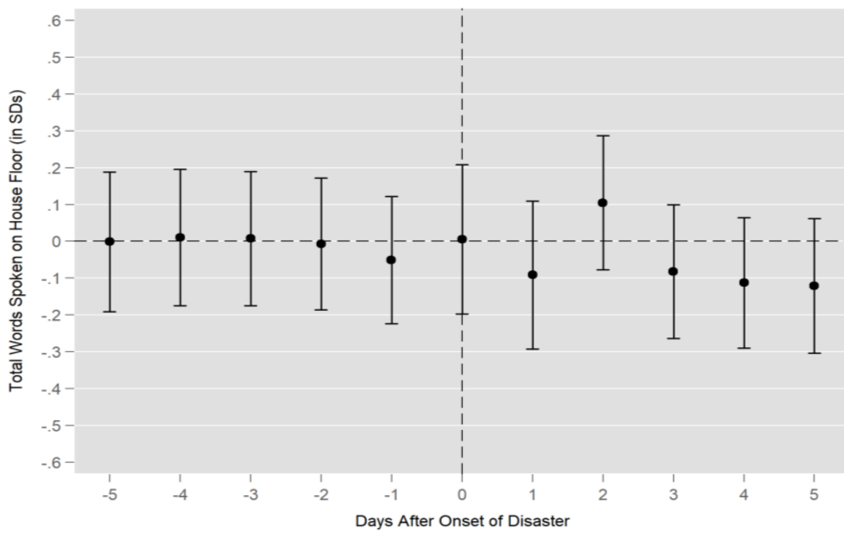
Appendix Figure A.3: Randomization Inference for the Effect of Disasters on Voting with Special Interests



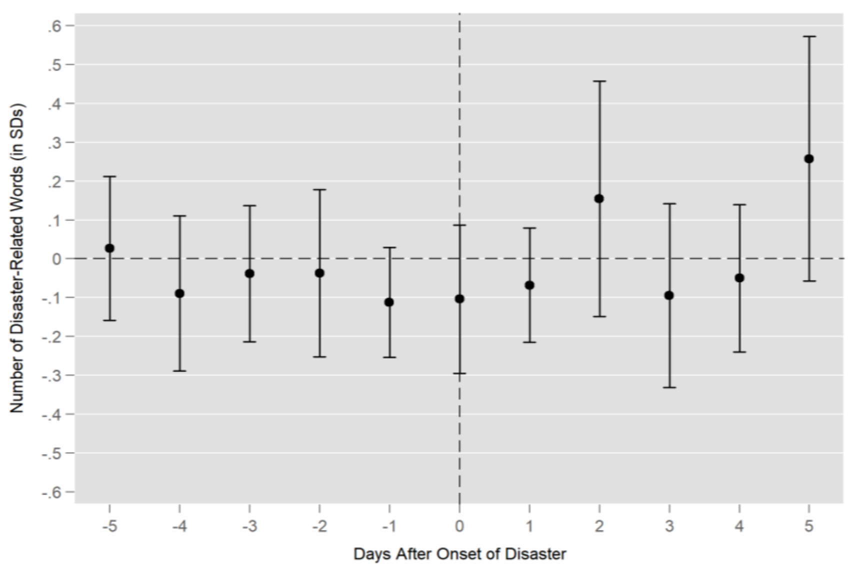
Notes: Figure shows the distribution of placebo estimates for the impact natural disasters on congressmen's tendency to vote with special interest donors, i.e., γ_t in equation (2). All graphs are based 10,000 regressions, with randomly reshuffled start dates of disasters.

Appendix Figure A.4: Disasters and Congressional Speech, 109th–114th Congresses

A. Total Number of Words Spoken on House Floor



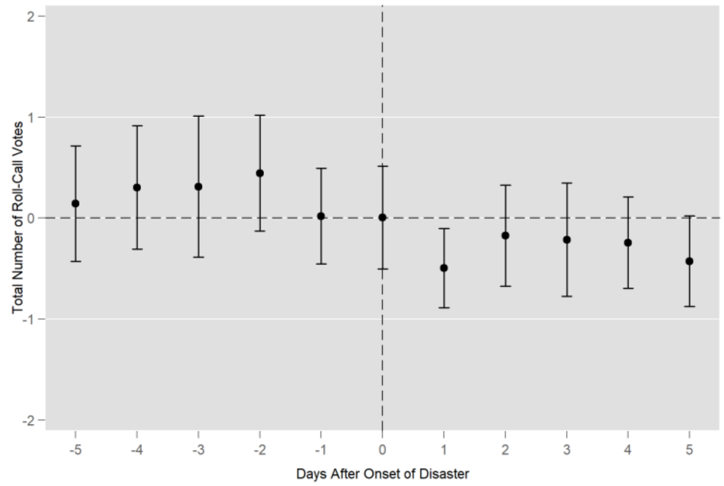
B. Number of Words Plausibly Related to Disasters



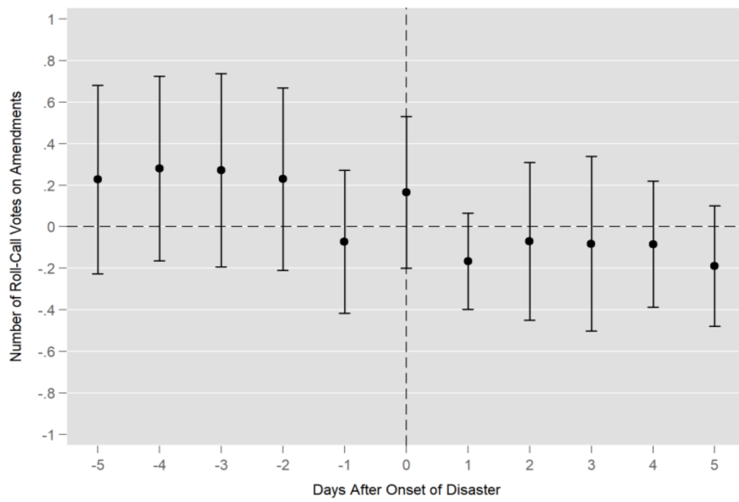
Notes: Figure shows the effect of disasters on the total number words (upper panel) as well as the number of words that are plausibly related to natural disasters (lower panel) that were spoken on the House floor on a particular day. As explained in Appendix A, disaster-related words are defined as the set of terms in footnote 8. All estimates are based on the regression model in equation (1), with different left-hand-side variables. Confidence intervals account for clustering by year-month.

Appendix Figure A.5: Disasters and Legislative Productivity

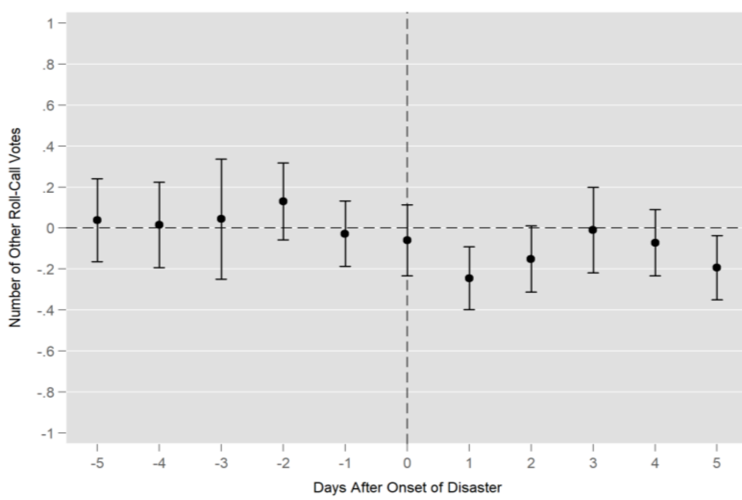
A. Total Number of Roll-Call Votes



B. Total Number of Votes on Amendments

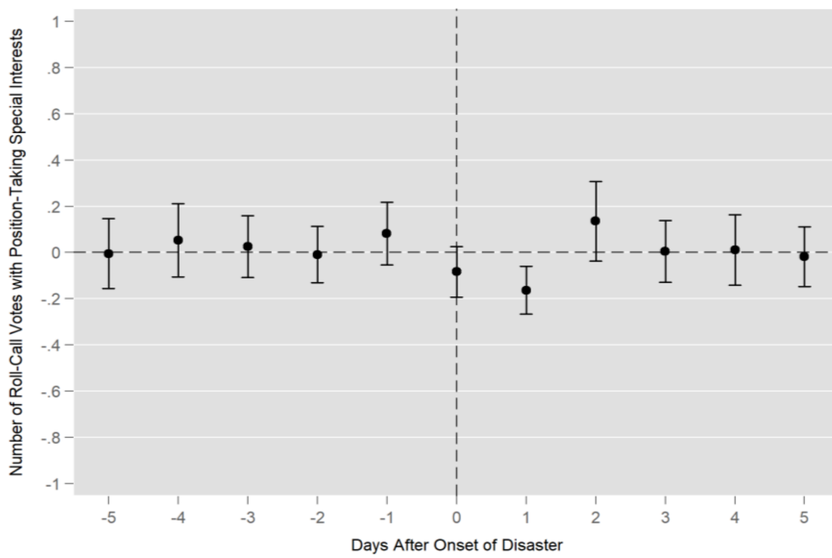


C. Total Number of Other Roll-Call Votes



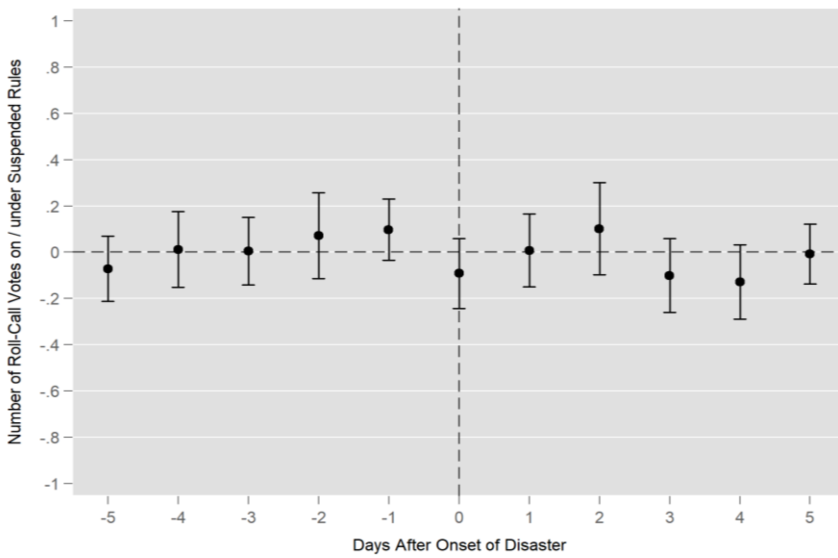
Notes: This figure complements Figure 3 in the main text by showing the effect of disasters on the total number of roll-call votes conducted in the House (upper panel), the total number of roll calls pertaining to amendments (middle panel), and the total number of other roll-call votes on a particular day (lower panel). All estimates are based on the regression model in equation (1), with different left-hand-side variables. Confidence intervals account for clustering by year-month.

Appendix Figure A.6: Number of Votes with Position-Taking Special Interests



Notes: This figure complements Figure 3 in the main text by showing the effect of disasters on the total number of roll-call votes per day on which special interest groups take a position. Estimates are based on the regression model in equation (1), but with a different left-hand-side variable. Confidence intervals account for clustering by year-month.

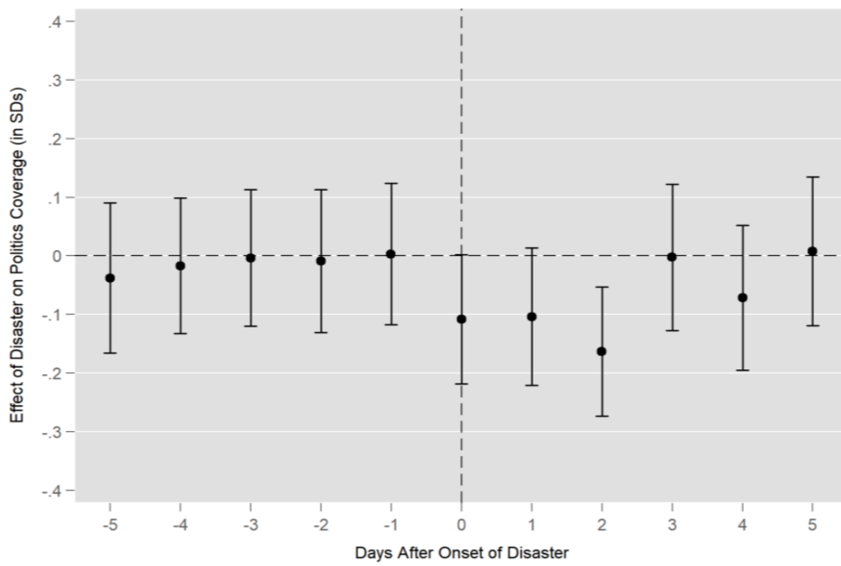
Appendix Figure A.7: Roll Calls on / under Suspension of Rules



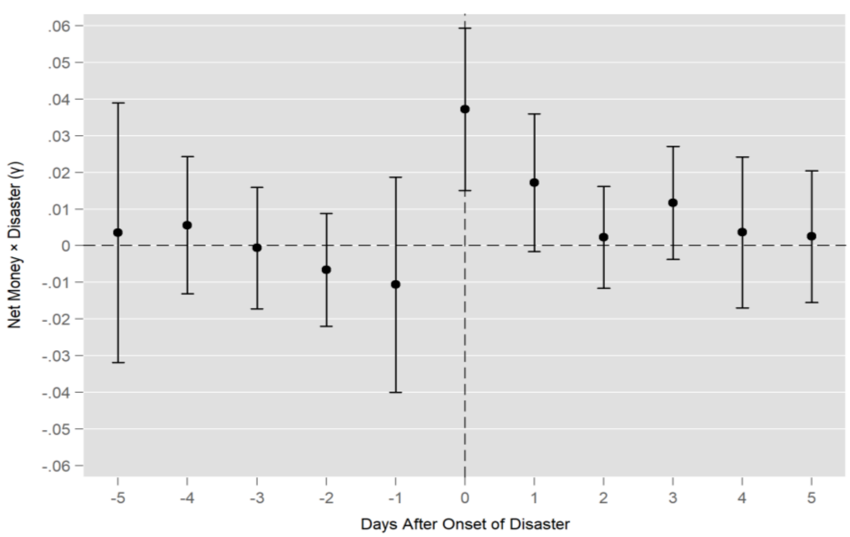
Notes: This figure complements Figure 3 in the main text by showing the effect of disasters on the total number of roll-call votes that are either held on the question of suspending the rules or under suspended rules. Estimates are based on the regression model in equation (1), but with a different left-hand-side variable. Confidence intervals account for clustering by year-month.

Appendix Figure A.8: Replication of Figures 2(A) and 2(B) Using All Domestic Disasters

A. Coverage of Politics

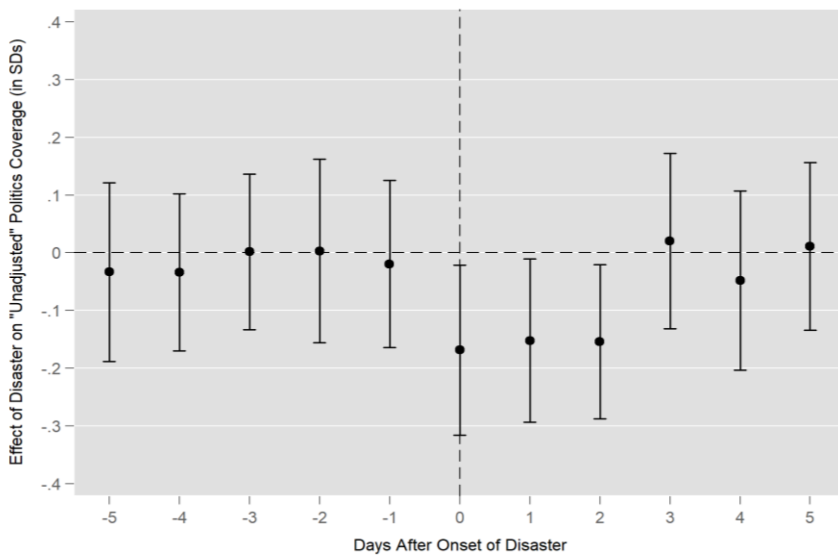


B. Voting with Special Interests



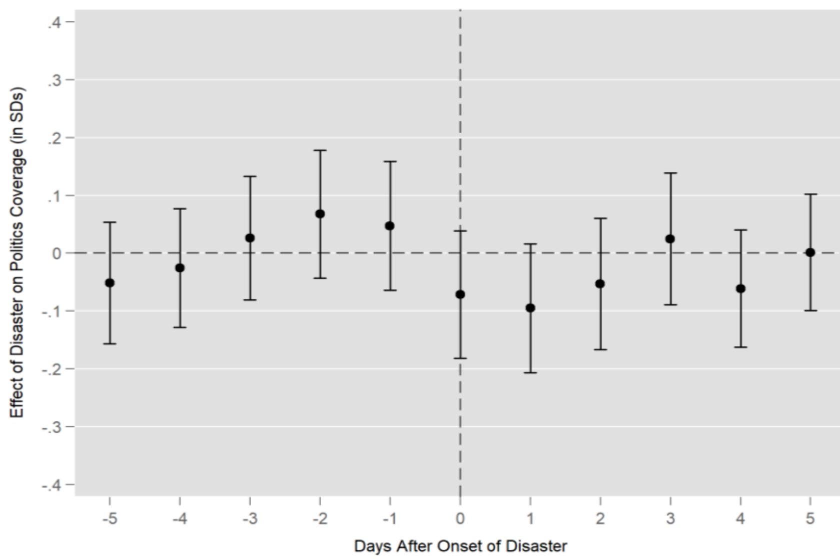
Notes: Figure replicates both panels of Figure 2 in the main text, using all natural domestic disasters in the EM-DAT data set instead of restricting attention to large ones. As in Figure 2, confidence intervals in the upper panel account for clustering by year-month, while those in the lower panel are two-way clustered by year-month and legislator.

Appendix Figure A.9: Replication of Figure 2(A) Using Alternative Measure of Politics Coverage



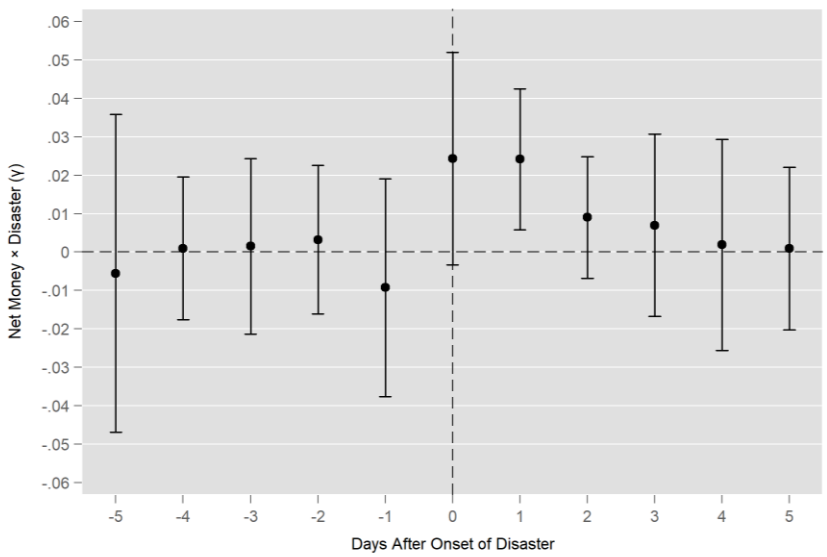
Notes: Figure replicates the upper panel of Figure 2 in the main text, using our unadjusted measure of politics reporting on the evening news of the "big three," i.e., ABC, CBS, and NBC. More specifically, our unadjusted measure of politics coverage foregoes the regression in equation (3). Instead, we use the median of politics reporting on the "big three" networks (see Appendix A for details). Confidence intervals account for clustering by year-month.

Appendix Figure A.10: Replication of Figure 2(A) Using Large Foreign and Domestic Disasters



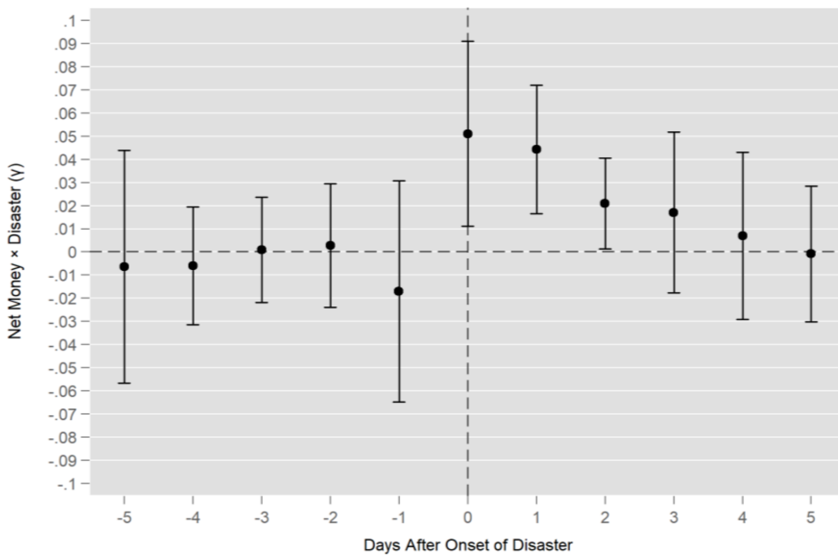
Notes: Figure replicates the upper panel of Figure 2 in the main text, using large foreign or domestic disasters instead of only the latter ones (see Appendix A for details). Confidence intervals account for clustering by year-month.

Appendix Figure A.11: Replication of Figure 2(B) Excluding Bills Plausibly Related to Disaster Relief



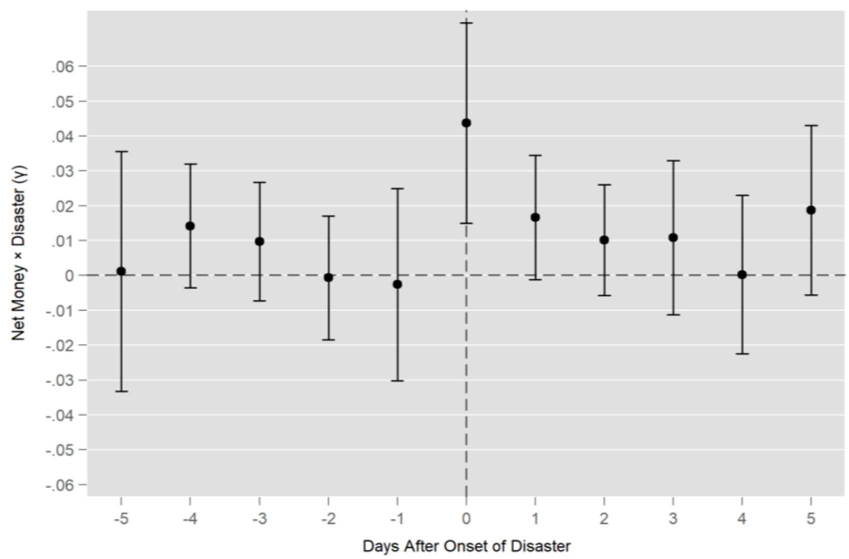
Notes: Figure replicates the lower panel of Figure 2 in the main text. The sample excludes all bills that are plausibly related to disaster relief, as explained in Appendix A. Confidence intervals account for two-way clustering by legislator and year-month.

Appendix Figure A.12: Replication of Figure 2(B) Using Top-Coded Donations



Notes: Figure replicates the lower panel of Figure 2 in the main text. As explained in Appendix A, we top-code total contributions from special interest groups to individual legislators at $500,000 in order to reduce the influence of outliers. Confidence intervals account for two-way clustering by legislator and year-month.

Appendix Figure A.13: Replication of Figure 2(B) Controlling for Disaster-Specific Fixed Effects



Notes: Figure replicates the lower panel of Figure 2 in the main text. As explained in Appendix A, the estimates are based on the regression model in equation (2), controlling for legislator, calendar month, day-of-the week, and disaster-specific fixed effects. By the latter, we mean a fixed effect for the +/− 10-day window around each disaster. Observations that fall within more than one event-specific window are assigned to the nearest disaster. Confidence intervals account for two-way clustering by legislator and year-month.

Appendix Table A.1: Disasters and News Coverage of Politics, Alternative Specifications

	Summary Measure of Politics Coverage (in SDs)			Minutes of Politics Reporting on a Particular Network		
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate Aftermath of Disaster (φ)	-.143**	-.154***	-.154***	-.567**	-.627***	-.610***
	(.063)	(.055)	(.055)	(.250)	(.209)	(.204)
Hypothesis Tests [p-values]:						
H_0: $\varphi = 0$	.024	.005	.006	.025	.003	.003
Fixed Effects:						
Year × Month	No	Yes	Yes	No	Yes	Yes
Day of the Week	No	No	Yes	No	No	Yes
Network Fixed Effects	No	No	Yes	Yes	Yes	Yes
R-Squared	.002	.187	.195	.025	.516	.518
Number of Observations	4,497	4,497	4,497	17,210	17,210	17,210

Notes: Entries are coefficients and standard errors from regressing different measures of politics reporting on an indicator variable for the day of and the day after the reported onset of a large domestic disaster, as explained in Appendix A. The outcome in columns (1)–(3) is the summary measure of politics coverage on the evening news that we use in the main text. The outcome in columns (4)–(6) is the duration of politics reporting (in minutes) on any given network for which VTNA provides content summaries on a particular day. Standard errors are reported in parentheses and are clustered by year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix Table A.2: Replication of Table 2 Using Large Foreign and Domestic Disasters

	Vote "Yea" on Passage			
	(1)	(2)	(3)	(4)
Money from Supporting Interest Groups ($\beta^{(+)}$)	.020***	.019***	-.003	.008**
	(.004)	(.004)	(.003)	(.004)
Money from Opposed Interest Groups ($\beta^{(-)}$)	-.180***	-.178***	-.159***	-.130***
	(.025)	(.026)	(.022)	(.018)
Money from Supporting Interest Groups		.007	.013*	.015*
× Immediate Aftermath of Disaster ($\gamma^{(+)}$)		(.009)	(.007)	(.009)
Money from Opposing Interest Groups		-.026	-.035*	-.005
× Immediate Aftermath of Disaster ($\gamma^{(-)}$)		(.024)	(.020)	(.018)
Immediate Aftermath of Disaster (δ)	.026	.026	.012	.001
	(.021)	(.022)	(.022)	(.019)
Hypothesis Tests [p-values]:				
H_0: $\gamma^{(+)} \leq 0$	--	.193	.034	.037
H_1: $\gamma^{(-)} \geq 0$	--	.138	.044	.392
H_2: $\gamma^{(+)} = \gamma^{(-)} = 0$	--	.437	.078	.199
Fixed Effects:				
Legislator × Congress	No	No	Yes	Yes
Year × 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 equation (4) by OLS. Interest groups 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. As explained in Appendix A, "Immediate Aftermath of Disaster" is an indicator equal to one if and only if the roll call occurs within a day after the reported onset of a large foreign or domestic 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. For a detailed description of the underlying data, see the Data Appendix.