

## Pandering in the Shadows: How Natural Disasters Affect Special Interest Politics<sup>†</sup>

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*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 coverage of politics in general and of individual legislators in particular, and members of the House of Representatives become significantly more likely to adopt special interest donors' positions. The evidence implies that politicians are more inclined to take actions benefiting 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 requirements. (JEL D72, L82, Q54)*

One of the main democratic benefits of media is to inform the citizenry. Information in the hands of voters not only has 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, “sunlight is said to be the best of disinfectants” (Brandeis 1914, 92)

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 US 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

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and its actions are less likely to be scrutinized in the media.<sup>1</sup> Even US presidents appear to time the release of controversial executive orders to evade public scrutiny (Djourelouva and Durante 2022).

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 US 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 are thus extremely important. Qualitative accounts of politician behavior suggest that contemporaneous scrutiny can greatly affect electoral accountability. According to Kingdon's 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" (1973, 46). Kingdon further remarks that "much of the problem of explaining one's vote is directly tied to the media coverage which reaches the district" (1973, 204). Drawing on his time shadowing members of Congress in their districts, Fenno reports:

A recent critical newspaper story nearly traumatized [Congressman B]. "It gets you right in the stomach and makes you want to throw up all over the floor," he said. He avoids taking controversial positions because he hates controversy. (Fenno 1978, 74)

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 toward 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—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

<sup>1</sup> Hollywood has taken this one step further. A 1997 film, *Wag the Dog*, imagines a president starting a war in order to cloak the 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 US 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., Dallek 1998).

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 with 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 the 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 dataset that uses public announcements to record the positions of several hundred interest groups on particular pieces of legislation, these groups' donations to individual politicians, and 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.<sup>2</sup> Our measure of support for special interests thus varies across congresspeople voting on the same bill depending upon which members of Congress received money from which position-taking interest groups. It also varies across bills "within" a given congressperson depending upon 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 percent 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 around the onset of natural disasters.

In Section III, we show that in the three days following the onset of a natural disaster, congresspeople are about 6 percentage points more likely to support the positions of their donors than in the three days prior to such an event. In the first part of our analysis, we resort to randomization inference as a simple and transparent method to assess whether the observed increase in support for special interests might be due to

<sup>2</sup> As we show in Section II, situations in which legislators receive nearly equal amounts from interest groups on opposite sides of a bill are rare.

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 percent 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 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 pretrends 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 IV, we confirm that disasters crowd out attention to politics. We present evidence of a crowd-out of politics reporting in national broadcast media and, more tentatively, of a reduction in newspaper coverage of local congresspeople and 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 toward legislators' special interest donors.

In Section V, 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 disasters aid gets added on to preexisting bills that then garner more legislative support.

The last section discusses different interpretations of our findings. It also discusses their implications for theories of electoral accountability.

## I. Related Literature

The two most closely related papers are Snyder and Strömberg (2010) and Balles, Matter, and Stutzer (2024). Snyder and Strömberg (2010) 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 quasirandom 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 work, Balles, Matter, and Stutzer (2024) also investigate the effect of disasters on legislator alignment with special interests. Balles, Matter, and Stutzer (2024) 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. Balles, Matter, and Stutzer (2024) attempt to measure constituent preferences. We do not. We do, however, directly document attention crowd-out, which allows us to provide evidence in support of the claim that legislator behavior is moderated by a reduction in attention to politics. Moreover, our empirical strategy eschews man-made disasters as sources of identification. In particular, 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 National Rifle Association (NRA) and the fight for gun control. Finally, we conduct an array of additional tests that point to moral hazard—rather than, say, agenda setting—as the mechanism behind our findings.<sup>3</sup>

More broadly, 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, de Figueiredo, and Snyder Jr. (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, de Figueiredo, and Snyder Jr. argue that “rent-seeking donors lack the leverage to extract large private benefits from legislation” (2003, 125).<sup>4</sup>

Nevertheless, special interests do appear to allocate their donations strategically (see, e.g., Barber 2016; Bertrand et al. 2020, 2018; Bombardini and Trebbi 2011; Fourniaies and Hall 2018; Powell and Grimmer 2016). Fourniaies 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, extant work creates “an appearance of corruption—the key word being *appearance*” (2016, 986).

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

<sup>3</sup>Arguably less related is recent work by Gagliarducci, Paserman, and Patacchini (2019), who study how members of Congress change their support for environmental regulation in response to hurricanes. Gagliarducci, Paserman, and Patacchini (2019) find that congresspeople whose districts are 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.

<sup>4</sup>This assessment is not uncontroversial. Considering the same set of studies reviewed by Ansolabehere, de Figueiredo, and Snyder Jr. (2003), Stratmann (2005) rejects the null hypothesis of no effect.

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 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 and attention to politics more generally. A burgeoning literature has demonstrated impacts of media on the behavior of politicians (for reviews, see DellaVigna and Gentzkow 2010; Prat and Strömberg 2013; Strömberg 2015a, b). Particularly important prior contributions include Strömberg (2004), Eisensee and Strömberg (2007), Snyder and Strömberg (2010), and Durante and Zhuravskaya (2018).<sup>5</sup> 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; Djourelouva and Durante 2022). With the exception of Snyder and Strömberg (2010) there exists little quantitative evidence of how the media affect the behavior of rank-and-file legislators.

## II. Data and Descriptive Statistics

To test the idea that contemporaneous scrutiny reduces politicians' support for special interests, we assemble a new dataset 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 (2017), a non-partisan nonprofit organization that strives to reveal money's influence on politics. MapLight's research staff comb through publicly available sources (such as 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 109th 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.

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

<sup>5</sup>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; and DellaVigna and Kaplan (2007), Gentzkow, Shapiro, and Sinkinson (2011), Chiang and Knight (2011), and Enikolopov, Petrova, and Zhuravskaya (2011) on the effects of (biased) media on electoral outcomes.

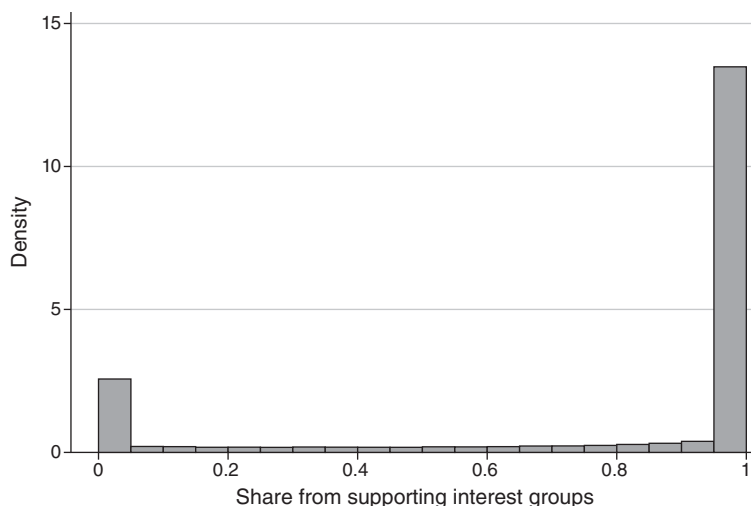


FIGURE 1. DONATIONS BY FRACTION OF MONEY IN FAVOR OF PASSAGE

*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 percent of bill-legislator combinations.

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 1 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.<sup>6</sup>

Note that 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 percent of individual 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 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. In ancillary analyses, we also incorporate the votes of unconnected legislators, that is, congresspeople who did not receive donations from any of the position-taking interest groups. The evidence suggests that these legislators are no more or less likely to support

<sup>6</sup>In 77.3 percent of cases, more than 99 percent of funds come from special interest groups on the same side of the issue; in 90.3 percent of cases, more than 75 percent of funds come from special interest groups on the same side of the issue.

passage of the bill when the vote happens during or immediately after a natural disaster (see Section V).

*Natural Disasters.*—Following Eisensee and Strömberg (2007), we obtain data on natural disasters from the Centre for Research on the Epidemiology of Disasters (CRED 2018). 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. We further limit our sample to the top tercile of the distribution in terms of the number of deaths, people affected, or damages. By sudden-onset disasters we mean events for which the start date is defined precisely enough to obtain sharp identification. That is, we include floods, hurricanes, blizzards, etc. into our analysis (even though such events tend to be predictable a few days in advance), but we exclude epidemics, heat waves, and wildfires. We focus on large domestic disasters because these events receive greater coverage by US 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.<sup>7</sup>

*TV News.*—Again, following Eisensee and Strömberg (2007), we use abstracts from the Vanderbilt Television News Archive (VTNA 2017) 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, and 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 machine learning as an alternative to keyword and rules-based approaches. Specifically, we leverage the prowess of IBM Watson (2017) to classify each news story in VTNA based on the provided summary.

Watson relies on artificial intelligence to analyze user-provided text. Importantly for our purposes, Watson uses a proprietary supervised machine-learning model to categorize the content of text based on an enhanced version of the Interactive Advertising Bureau (IAB) Quality Assurance Guidelines Taxonomy (Interactive Advertising Bureau 2013). That is, for each news segment, Watson reads the human-generated abstract from VTNA and predicts topics according to a predefined

<sup>7</sup>Our main result remains qualitatively unchanged if we also include large foreign disasters or all domestic events recorded in EM-DAT (see Supplemental Appendix Table A.4).

set of categories.<sup>8</sup> These categories were originally designed to accurately and consistently describe the content of, say, a website or video clip in order to facilitate targeted advertising. Among the different categories in Watson's taxonomy is one for content related to "law, government, and politics." This high-level category contains several subcategories for which Watson returns confidence scores. Since subcategories are not mutually exclusive, we sum the respective 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 choose a threshold score that balances type I and type II errors on this test set. News stories that exceed the threshold score are said to contain political content. Given an accuracy of 91.6 percent and a false positive (negative) rate of 7.7 percent (11.1 percent), our automated detection of political content performs well, though it is certainly not perfect (see Supplemental Appendix for details).

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,

$$(1) \quad News_{n,t} \equiv \frac{\sum_{s \in P_{n,t}} Duration_s}{\sum_{s \in S_{n,t}} Duration_s},$$

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. According to our measure, on an average day the median network contained in VTNA spends about 29 percent of airtime reporting on political issues.

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 analysis below restricts attention to news reports on ABC, CBS, and NBC.<sup>9</sup> 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, that is,  $News_{n,t}$ . Inevitably, using machine learning to classify news segments involves both type I and type II errors. Since

<sup>8</sup>Categories being predefined is an important difference between our approach leveraging Watson and other, more frequently used approaches to classifying text, such as Latent Dirichlet Allocation (LDA) models. LDA topic modeling is an unsupervised machine-learning technique in which documents are represented as "bags of words" and "topics" emerge endogenously based on word frequencies and co-occurrences.

<sup>9</sup>In the Supplemental Appendix, we show that our conclusions remain unaffected if we include data from all channels contained in VTNA (see Supplemental Appendix Table A.3.)

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 (see Supplemental Appendix C). Since the implied correction leaves our conclusions qualitatively unchanged, we relegate this part of the analysis to the Supplemental Appendix and present the more 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 reports 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 (NewsLibrary 2019). 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 two-thirds of an article mention. 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.<sup>10</sup>

*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” (Google Trends 2019). 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.”

*Summary Statistics.*—Table 1 presents descriptive statistics for the most important variables in our analysis. A few facts are worthy of note. Disasters account for 4 percent of days over the time period spanned by our data. Of the 1,525 bills in our data, 186 were voted on the day of or within two days after a disaster. The

<sup>10</sup> Without these fixed effects, the estimated impact of disasters is slightly larger.

TABLE 1—DESCRIPTIVE STATISTICS

Variable	Mean	SD	Min	Median	Max
<i>Daily time series</i> ( $N = 4,619$ ):					
Disaster	0.043	0.204	0	0	1
Share politics reporting on median network	0.290	0.115	0	0.291	0.885
Share disaster reporting on median network	0.068	0.105	0	0.035	0.699
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
<i>Legislator level</i> ( $N = 872$ ):					
Democrat	0.448	0.498	0	0	1
Republican	0.550	0.498	0	1	1
Number of votes with position-taking donors	564	384	0	518	1,385
Number of newspaper mentions on average day	0.980	0.915	0.071	0.667	8.789
<i>Bill level</i> ( $N = 1,525$ ):					
Number of passage votes	1.05	0.23	1	1	3
Number of supporting special interest groups	13.5	29.6	0	3	507
Number of opposed special interest groups	6.8	17.5	0	0	196
Total contributions by supporting groups (in \$100,000)	120	218	0	34	2,092
Total contributions by opposing groups (in \$100,000)	36	85	0	0	944
<i>Vote level</i> ( $N = 693,062$ ):					
Net money from special interest groups (in \$100,000)	0.304	0.879	0	0.05	52.19
Net money from special interest groups   any money (in \$100,000)	0.429	1.018	0	0.128	52.19
Vote with dominant special interest	0.812	0.391	0	1	1
Vote “yea”	0.845	0.362	0	1	1
Abstain	0.026	0.160	0	0	1

*Notes:* Entries are descriptive statistics for the most important variables used throughout the analysis. The reported number of observations corresponds to the maximum among the variables in each section. The number of usable observations for any particular variable can be slightly lower due to missing data. For precise definitions of all variables, see the Supplemental Appendix.

average (median) bill is supported by 13.5 (3) interest groups and opposed by 6.8 (0) groups. 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. To put these numbers in context, for the average bill with interest group support, supportive interest groups together account for around 4 percent of total campaign contributions. The same number for opposing interest groups is around 1.2 percent. The median congressperson casts 518 votes on bills on which some of her current donors took a position. This means that, on nearly one in three bills in our data, she might potentially be beholden to special interests. Conditional on receiving any money from position-taking special interests, the average congressperson receives about \$43,000 more from groups on one side of the issue than from groups on the other side.<sup>11</sup>

### III. Disasters and Roll Call Votes

Our empirical analysis proceeds in three steps. In the current section, we document that legislators align more closely with their special interest donors following

<sup>11</sup> The average representative receives about \$670,000 in contributions from individuals and approximately \$650,000 in donations from political action committees (PACs).

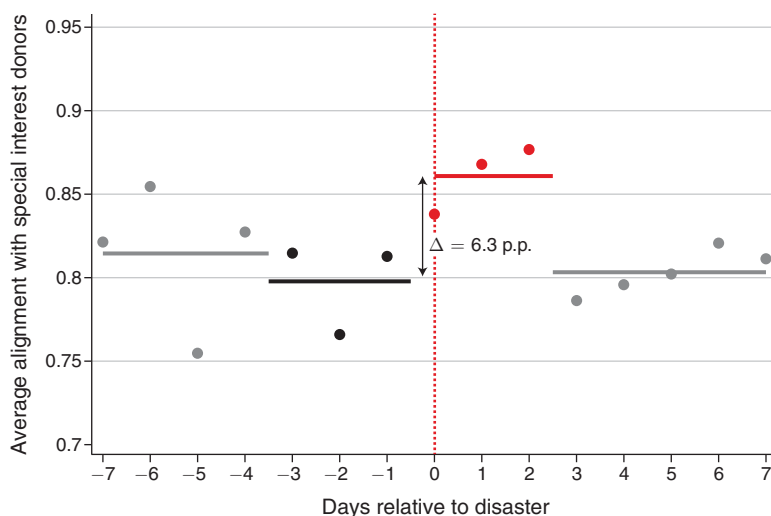


FIGURE 2. ALIGNMENT WITH SPECIAL INTERESTS, BEFORE AND AFTER NATURAL DISASTERS

*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 zero 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.

natural disasters. In the next section, we provide evidence that disasters crowd out news reports on politics. Finally, in Section V we explore different mechanisms that might explain the simultaneous decline in attention to politics and the realignment of roll call votes toward special interest donors.

### A. Raw Data and Randomization Inference

We begin our analysis by analyzing congresspeople's support for their special interest donors in the raw data. As Figure 2 shows, in the three-day window before a major disaster strikes, support for special interests is at about 80 percent. Focusing on the three days after the onset of the disaster, however, we observe that 86 percent of votes align with the positions of politicians' donors. The raw data, therefore, hint at a connection between natural disasters and how congresspeople vote on new legislation.

We next turn to 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 and simultaneously draw a new start date for each event without replacement. 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:

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

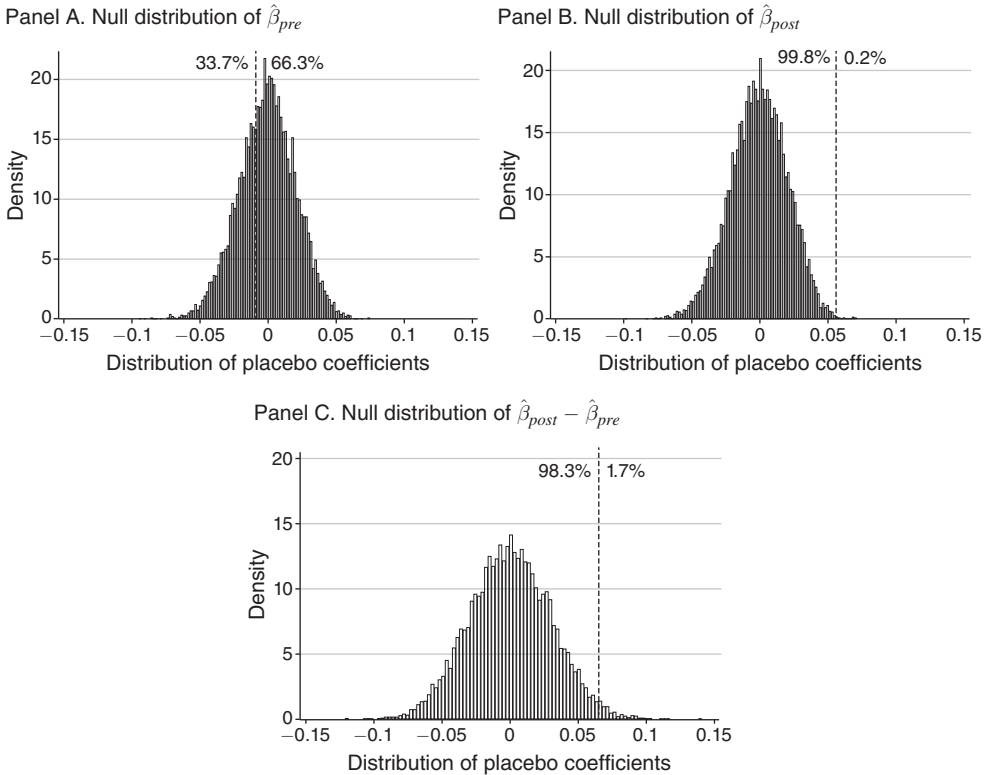


FIGURE 3. RANDOMIZATION INFERENCE

Notes: Figure shows the distribution of  $\beta_{pre}$  and  $\beta_{post}$  in equation (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.

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  $\beta_{pre}$  and  $\beta_{post}$ . 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 3 presents both the true values of  $\hat{\beta}_{pre}$  and  $\hat{\beta}_{post}$  in our data (vertical lines) as well as their distributions under the sharp null hypothesis of a null 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 percent of placebo coefficients. For completeness, panel C demonstrates that the observed difference between  $\hat{\beta}_{post}$  and  $\hat{\beta}_{pre}$  is also very unlikely to arise by chance. Based on the evidence in Figure 3, we conclude that congresspeople are significantly more likely to support

TABLE 2—NATURAL DISASTERS AND SUPPORT FOR SPECIAL INTERESTS

	Vote with special interest donors					
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate aftermath of disaster ( $\beta_{post}$ )	0.056 (0.019)	0.059 (0.019)	0.068 (0.020)	0.061 (0.021)	0.061 (0.021)	0.060 (0.022)
Immediately before disaster ( $\beta_{pre}$ )	−0.009 (0.020)	−0.009 (0.020)	0.002 (0.022)	0.008 (0.019)	0.008 (0.019)	0.008 (0.019)
Constant	0.807 (0.008)					
Hypothesis tests [ <i>p</i> -values]: $H_0: \beta_{post} = \beta_{pre}$	0.023	0.018	0.024	0.038	0.037	0.038
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
$R^2$	0.002	0.006	0.041	0.059	0.067	0.085
Observations	478,946	478,946	478,946	478,946	478,946	478,946

Notes: Entries are OLS point estimates estimating the model in equation (2). “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; “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. For a detailed description of the underlying data, see the Supplemental Appendix.

the positions of their special interest donors in the days just after a natural disaster strikes. 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.

B. Controlling for Potential Confounders

Table 2 probes the robustness of this finding. The results in this table are based on the regression model in equation (2), controlling for different sets of fixed effects. Column 1 simply replicates the findings in Figure 2. We notice a number of important points. First, the baseline rate of alignment with a contributing special interest group is 80.7 percent. Second, the rate of alignment decreases to 79.8 percent in the three days before the onset of a disaster but is not statistically distinguishable from baseline. Third, the rate of alignment is 86.2 percent in the three days following a disaster, which is statistically distinguishable from baseline at the 1 percent level. Fourth, the standard errors on both coefficients are nearly equally large. The difference in statistical significance is solely due to  $\hat{\beta}_{post}$  being five to six times larger than  $\hat{\beta}_{pre}$ . Columns 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 underreport disasters on such days, column 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. Columns 5 and 6 further account for legislator and legislator-by-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 around three. Moreover, we test the null hypothesis that legislators' support for special interests is the same just before and after disasters strike. In all specifications, we reject this null at the 5 percent significance level. The evidence in Table 2 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 respectively.<sup>12</sup>

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. In Section V, we show empirically that the number of position-taking interest groups reported by MapLight and their average donations do not vary significantly 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 that oppose it. If correct, then the MapLight data might systematically understate total campaign donations from opposing groups, which means that the outcome variable in equation (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.<sup>13</sup> Although we cannot categorically rule out this possibility, we are not aware of any reasons why this would be the case.

### C. 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:

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

<sup>12</sup> We have also experimented with inferring the position of a congressperson's special interest donors based on her long-run relationships with different interest groups. While our preferred measure takes donations only from contemporaneous donors into account (see Section II), including donations from past donors in the construction of the left-hand-side variable in Table 2 would lead to slightly larger, though not statistically distinguishable, estimates of  $\beta_{post}$ .

<sup>13</sup> 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. Equation (2) thus becomes:  $SIV_{l,r,t} = \alpha + \beta_{pre} Disaster_t^{(-3,-2,-1)} + \beta_{post} Disaster_t^{(0,1,2)} - \xi_{l,r,t} + \epsilon_{l,r,t}$ . Since  $\xi_{l,r,t}$  is part of the error term, a standard omitted variables bias argument applies.

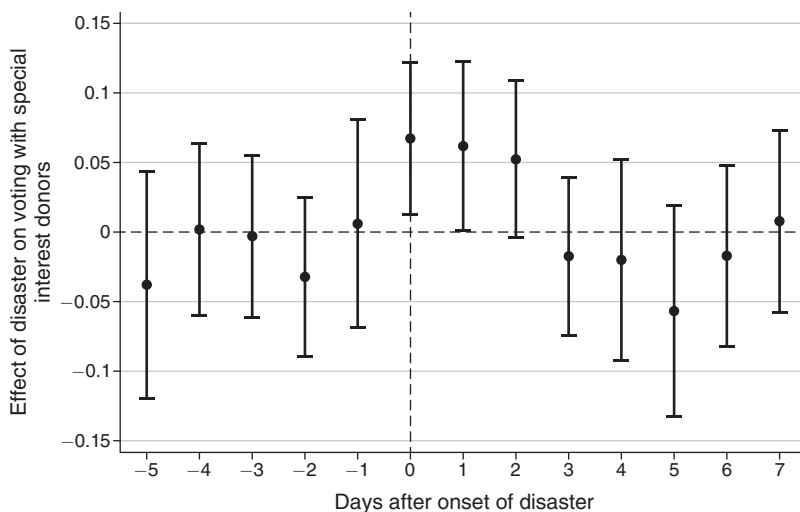


FIGURE 4. EVENT-STUDY ESTIMATES OF THE EFFECT OF DISASTERS ON ALIGNMENT WITH SPECIAL INTERESTS

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

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

The specification in equation (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 equation (3) on the full sample of votes that might be subject to influence from special interests (i.e., the same set of votes as in Table 2), which means that votes outside of the event window serve as 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  $\varphi_t$  can still be interpreted as changes in legislators' alignment with their special interest donors relative to usual.

As we see in Figure 4, there is no pretrend in alignment in the five days leading up to a disaster. All five leads are statistically insignificant, and three of them are

TABLE 3—AFFECTED VERSUS UNAFFECTED STATES

	Vote with special interests			
	(1)	(2)	(3)	(4)
Immediate aftermath of disaster	0.092 (0.069)	0.284 (0.069)	0.054 (0.020)	0.064 (0.022)
Fixed effects:				
Year × month	No	Yes	No	Yes
Day of the week	No	Yes	No	Yes
Legislator × Congress	No	Yes	No	Yes
Sample	Only representatives from affected states		Excluding representatives from affected states	
R <sup>2</sup>	0.009	0.318	0.002	0.085
Number of observations	13,650	13,650	465,296	465,296

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, estimated on different sub-samples of the data. “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.

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 declines slightly over the next two days, but it remains statistically significant or marginally so. Out of the thirteen point estimates in Figure 4, the three largest ones pertain to the day of the disaster and the two following days. 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 percent.<sup>14</sup> After three days, alignment rates drop to just below average and are statistically indistinguishable from usual.<sup>15</sup>

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 attention to politics for roughly the same period of time.

IV. Disasters and Attention to Politics

We begin the analysis in this section with a comparative static exercise that already hints at the importance of attention. An earthquake in California, for example, is likely to divert attention more among residents and news outlets in California than in, say, Florida. If attention matters for how congresspeople vote, then we would expect representatives from California to modulate their behavior to a greater extent than those from unaffected states. Table 3 presents evidence that is consistent with

<sup>14</sup> 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  $(10!)(3!)/13! \approx 0.003$ .  
<sup>15</sup> As a robustness check, we have estimated equation (3) dropping all votes that fall into the event window of more than one disaster. The results are noisier but qualitatively robust and are presented in Supplemental Appendix Figure A.2.

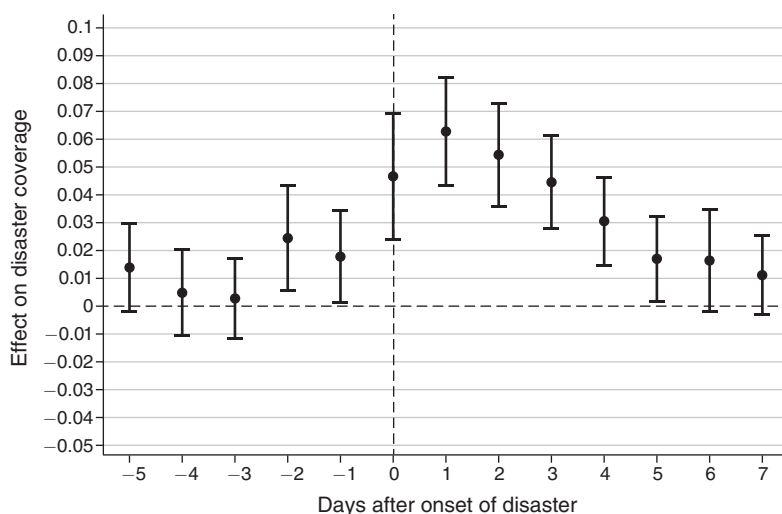


FIGURE 5. DISASTER COVERAGE ON THE EVENING NEWS

*Notes:* Figure displays point estimates and 95 percent confidence intervals for the impact of natural disasters on disaster-related reporting on the evening news. Estimates are based on regression models analogous to equation (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 Supplemental Appendix.

this prediction. Our estimates for representatives from affected states are imprecise without controlling for baseline fixed effects. This is due to the small size of the treated group. However, with fixed effects included, the estimates are statistically distinguishable from zero. Also, the respective point estimates do exceed those for congresspeople from states that were not directly impacted by the disaster. This is true both controlling for and without baseline fixed effects. We also observe that Table 3 reports a statistically significant effect even for representatives from unaffected states. This observation is consistent with the events in our data being large disasters that typically attract national attention. Finally, in the specification with fixed effects, neither the estimates for the affected group nor the estimates for the unaffected group lie in the confidence interval of the other.

In order to provide more direct evidence of disaster-induced crowd-out of attention to politics, we next build on the event-study framework above but replace our left-hand-side variable. Figure 5 begins by focusing on news coverage of disasters themselves. The estimates show TV 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 6 examines the impact on coverage of politics. There is little evidence of crowd-out before the disaster occurs, suggesting that nonpolitical 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 percent of total air-time, which amounts to approximately 7 percent of the time devoted to politics on

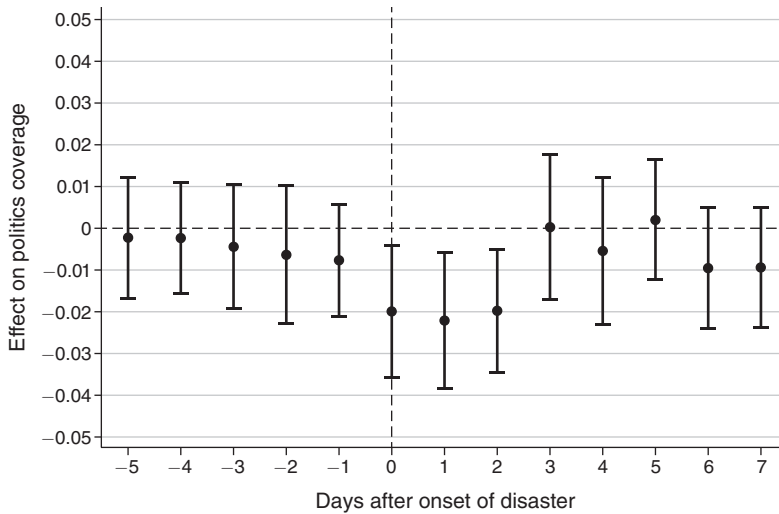


FIGURE 6. CROWD-OUT OF POLITICAL CONTENT IN THE EVENING NEWS

*Notes:* Figure displays point estimates and 95 percent confidence intervals for the impact of natural disasters on politics reporting on the evening news. Estimates are based on regression models analogous to equation (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 Supplemental Appendix.

an average day.<sup>16</sup> The disaster-induced crowd-out lasts for a total of three days—the same length of time for which we observe greater alignment between congresspeople and their special interest donors—after which politics coverage on the evening news returns to normal.

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

Supplemental Appendix Figure A.3 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 percent 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

<sup>16</sup> Recall that, given nonclassical measurement error in the dependent variable, these numbers likely understate the true effect size.

slightly less than a 2 percent probability.<sup>17</sup> 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 6 and the evidence in Supplemental Appendix Figure A.3 pertain to the national and local media, respectively. Although it is more difficult to measure changes in attention among ordinary citizens, Supplemental Appendix Figures A.4 and A.5 attempt to do so using data from Google Trends (2019). Specifically, Supplemental Appendix Figure A.4 shows estimated effects on the volume of disaster-related Google searches, and Supplemental Appendix Figure A.5 shows effects on the volume of Congress-related searches. The former begin to increase gradually 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 for Congress-related searches are noisier but qualitatively similar to those in the previous figure. The imprecision of the point estimates in Supplemental Appendix Figure A.5 notwithstanding, it is possible to reject the null hypothesis that they are jointly equal to zero in the week following the event ( $p = 0.011$ ). 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 media and citizens pay to politics.

## V. Potential Mechanisms

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

<sup>17</sup> 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  $4! \times 10 \times 9! / 13! \approx 0.01$ .

The first piece of evidence against agenda setting comes from the results in Table 4. 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 evidence neither that more or fewer words are being spoken on the House floor, nor that more roll calls are being held. In fact, the relevant point estimates are negative, statistically insignificant, 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.

Additionally, Table 4<sup>18</sup> 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 not more likely to have any interest group associated with them, and the number of position-taking interest groups is not higher. 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 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. Supplemental Appendix Figure A.6 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

<sup>18</sup> The classification of bills into types used in Table 4 comes from the dataset assembled by Roberts et al. (2018) with ancillary information from Lewis et al. (2018).

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.

The results in columns 3 and 4 in Table 3 provide additional evidence against mechanisms operating through direct economic effects. Since natural disasters appear to increase legislator support for special interests both among representatives from states that were and were not directly impacted by the event, 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. This could be seen as moral hazard at the behest of the party.

To test these explanations, we examine abstention rates and party line votes as outcomes. The evidence in the four leftmost columns of Table 5, 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 toward the positions of their donors. We address this possibility in several 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 4, 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" (see Table 4).

TABLE 4—AGENDA SETTING?

Outcome	Effect of disaster	
	(1)	(2)
<i>Congressional speech</i> ( $N = 1,626$ ):		
Number of disaster-related words (in SD units)	−0.077 (0.077)	0.007 (0.072)
Total words spoken on house floor (in SD units)	−0.023 (0.083)	0.032 (0.071)
<i>Number and type of roll calls</i> ( $N = 4,619$ ):		
Any special interests take position	−0.024 (0.017)	−0.029 (0.018)
Total number of roll calls	−0.332 (0.208)	−0.265 (0.169)
Number of passage votes	−0.054 (0.068)	−0.049 (0.069)
Roll calls on amendments	−0.123 (0.152)	−0.056 (0.120)
Procedural/other votes	−0.156 (0.061)	−0.160 (0.061)
Number of votes to suspend the rules/under suspended rules	0.001 (0.062)	0.004 (0.064)
<i>Roll call issue</i> ( $N = 9,372$ ):		
Agriculture	−0.013 (0.006)	−0.016 (0.011)
Defense	0.027 (0.034)	0.020 (0.031)
Energy and environment	−0.027 (0.017)	−0.051 (0.023)
Labor and education	0.016 (0.016)	0.016 (0.018)
Health	0.020 (0.015)	0.017 (0.012)
Foreign trade and international affairs	−0.002 (0.012)	−0.001 (0.016)
Domestic commerce	−0.002 (0.014)	0.010 (0.015)
Social welfare and housing	0.012 (0.013)	0.004 (0.016)
Government operations	0.012 (0.026)	0.011 (0.023)
Other	−0.043 (0.042)	−0.009 (0.019)
<i>Involvement of special interests in roll call</i> ( $N = 1,599$ ):		
Number of position-taking special interest groups	−0.495 (3.244)	−0.257 (3.345)
Average total donations per group (in \$100,000)	−0.574 (1.702)	0.805 (1.884)
Fixed effects:		
Year $\times$ 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, that is, 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.

TABLE 5—ALTERNATIVE OUTCOMES AND SAMPLES

	Party line vote		Abstention		Vote with donors	
	(1)	(2)	(3)	(4)	(5)	(6)
Immediate aftermath of disaster	0.007 (0.006)	0.000 (0.006)	0.002 (0.003)	0.002 (0.003)	0.065 (0.020)	0.064 (0.022)
Fixed effects:						
Year × month	No	Yes	No	Yes	No	Yes
Day of the week	No	Yes	No	Yes	No	Yes
Legislator × Congress	No	Yes	No	Yes	No	Yes
Sample	No restriction		No restriction		Excluding bills related to aid and relief	
R <sup>2</sup>	0.000	0.079	0.000	0.079	0.003	0.091
Number of observations	674,553	674,553	693,062	693,062	417,072	417,072

Notes: Entries are OLS point estimates and standard errors on the effect of natural disasters on whether representatives’ votes align with the majority of their copartisans (columns 1–2), on abstentions (columns 3–4), and on voting with special interest donors (columns 5–6). “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.

Third, we reestimate 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 5 and 6 of Table 5 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 disasters for interest groups who favor passage and for interest groups who favor defeat:

(4) 
$$Yea_{l,r,t} = \beta^{(+)} Money_{l,r,t}^{(+)} + \beta^{(-)} Money_{l,r,t}^{(-)} + \gamma^{(+)} Money_{l,r,t}^{(+)} \times Disaster_t^{(0,1,2)} + \gamma^{(-)} Money_{l,r,t}^{(-)} \times Disaster_t^{(0,1,2)} + \delta Disaster_t^{(0,1,2)} + \kappa_m + \mu_l + \varepsilon_{l,r,t}.$$

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.  $Disaster_t^{(0,1,2)}$  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  $\hat{\gamma}^{(+)} = \hat{\gamma}^{(-)} = 0$ .<sup>19</sup>

<sup>19</sup>Schmidheiny and Siegloch (2023) present sufficient conditions for identification in event-study designs. These include (i) no anticipation effects and parallel trends, which we validate by showing the absence of trends in

TABLE 6—BIFURCATION

	Vote “yea” on passage			
	(1)	(2)	(3)	(4)
Money from supporting interest groups ( $\beta^{(+)}$ )	0.020 (0.004)	0.018 (0.004)	−0.004 (0.003)	0.008 (0.004)
Money from opposed interest groups ( $\beta^{(-)}$ )	−0.181 (0.025)	−0.175 (0.024)	−0.157 (0.022)	−0.127 (0.018)
Money from supporting interest groups × immediate aftermath of disaster ( $\gamma^{(+)}$ )		0.006 (0.007)	0.014 (0.006)	0.012 (0.008)
Money from opposing interest groups × immediate aftermath of disaster ( $\gamma^{(-)}$ )		−0.067 (0.017)	−0.053 (0.017)	−0.032 (0.014)
Immediate aftermath of disaster ( $\delta$ )	0.019 (0.019)	0.022 (0.020)	0.015 (0.015)	0.021 (0.018)
Hypothesis tests [ <i>p</i> -values]:				
$H_0: \gamma^{(+)} \leq 0$	—	0.181	0.014	0.068
$H_1: \gamma^{(-)} \geq 0$	—	0.000	0.001	0.009
$H_2: \gamma^{(+)} = \gamma^{(-)} = 0$	—	0.001	0.001	0.033
Fixed effects:				
Legislator × Congress	No	No	Yes	Yes
Year × month	No	No	No	Yes
Day of the week	No	No	No	Yes
$R^2$	0.046	0.047	0.238	0.315
Number of observations	674,726	674,726	674,726	674,726

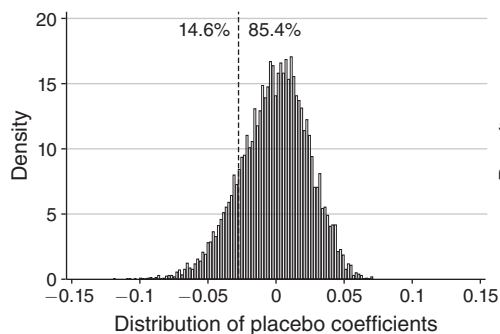
Notes: Entries are coefficients and standard errors from estimating variants of the empirical model in equation (4) by OLS. Interest group donations have been scaled so that the respective coefficient refers to the change in the probability of voting “yea” associated with an additional \$100,000. “Immediate 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.

Table 6 presents results from estimating variants of equation (4) on our data. Although all estimates of  $\delta$  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 0.001 to 0.033, 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.

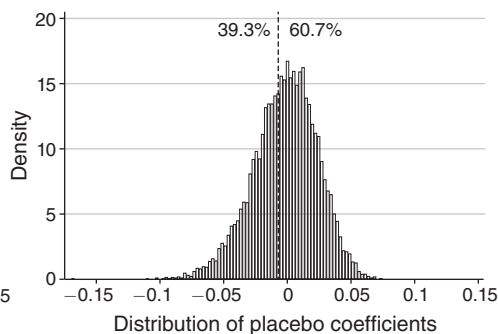
Note that the model in equation (4) does not require us to restrict attention to votes cast by representatives whose special interest donors took a position on the

leads; (ii) asymptotic stability of dynamics effects, which we validate by showing that lag effects stabilize after day two relative to the event; and (iii) all events occur sufficiently far from the beginning and the end of the panel that all lags and leads are estimated off of the same set of events. The last condition also holds in our case. Furthermore, since six members of Congress in our sample do not receive any contributions from any position-taking interest group, we can interpret our estimated effects as relative to this untreated group. As shown by Schmidheiny and Siegloch (2023), neither a balanced panel nor monotonicity of treatment over time is necessary for identification.

Panel A. Null distribution of  $\hat{\beta}_{pre}$  for unconnected legislators



Panel B. Null distribution of  $\hat{\beta}_{post}$  for unconnected legislators



Panel C. Null distribution of  $\hat{\beta}_{post} - \hat{\beta}_{pre}$  for unconnected legislators

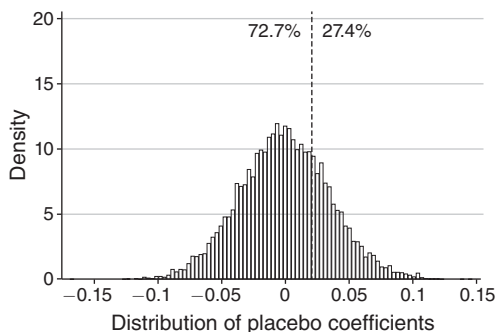


FIGURE 7. DO DISASTERS AFFECT HOW UNCONNECTED LEGISLATORS VOTE?

Notes: Figure shows the distribution of  $\hat{\beta}_{pre}$  and  $\hat{\beta}_{post}$  in equation (2) with voting “yea” as the 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.

bill. Since the sample in Table 6 also includes the votes of unconnected members, the evidence therein implies that our main finding is not an artifact of the sample restrictions that are inherent to our measure of interest-group support.

To provide additional, less parametric evidence on the absence of an effect of natural disasters on the votes of unconnected legislators, we again turn to randomization inference. Since the left-hand-side variable in equation (2) is by construction not defined for lawmakers who did not receive donations from any of the position-taking interest groups, we replace the outcome in this regression model with an indicator for whether a lawmaker voted “yea” on passage. Figure 7 presents both the true values of  $\hat{\beta}_{pre}$  and  $\hat{\beta}_{post}$  in our data (vertical lines) as well as their distributions under null hypothesis of no effect of disasters on the votes of unconnected members. 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 an effect even among “unconnected” legislators. This is not the case. The evidence in

Figure 7 is 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 toward the positions of their donors, supports the view that the mechanism behind our main result is a temporary reduction in electoral accountability, that is, 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 legislative 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.

*Interpretation.*—How should we interpret these results? Why would a decline in attention to politics coincide with increased alignment between legislators and the special interests that financially support them? If there was unanimity in support or, alternatively, in opposition to the proposed legislation, changes in attention to politics should have no effect on how congresspeople vote. The fact that we do observe an effect implies (i) that members of Congress represent different groups with conflicting interests and (ii) that the weight that each group receives in a legislator’s calculus of voting depends on attention. Whose interests are represented changes. If the positions of special interests are opposed to those of voters, then such changes may be detrimental to voter welfare.

## VI. 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 toward legislators’ special interest donors. Our preferred explanation for this finding is that politicians behave strategically: a temporary reduction in attention induces moral hazard. More broadly, our results are consistent with the view that contemporaneous attention and scrutiny matter for electoral accountability.

Certainly, absent competing incentives, temporary fluctuations in media coverage of politics should not affect how congresspeople vote. One possible interpretation of our findings to the contrary is that, in the absence of media attention, politicians tilt policy closer to their own views rather than those of the voters they represent. This interpretation explains contributions from special interests as a consequence of an electoral motive based upon similarity in desired policy between donors and politicians. Lessened media coverage then gives more space for legislators to enact their own preferred policies which happen to be aligned with those of special interests. A second possible interpretation of our findings is that the reduction in public scrutiny over politics gives politicians greater ability to demonstrate support for special interest groups, who likely retain a high level of attention even in the presence of lower

reporting. According to this interpretation, special interests do buy policy, while the media, in disproportionately raising the attention of constituents in opposition to large donors, intermediates the degree to which legislators follow through on reciprocating to special interests.

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, that is, 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 (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, as are addressing the external validity of our results with respect to other sources of attention crowd-out and determining the aggregate effects of attention on public policy.

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