Moral Hazard and Special Interests in Congress

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Abstract
We exploit the precise timing of domestic natural disasters to study the connection between public attention to politics and legislator support for special interests. Our findings show that when a disaster strikes, the news media reduce both their coverage of politics in general as well as that of individual congresspeople in particular. In addition, citizens are less likely to search for Congress-related keywords online. At the very same time, members of the U.S. House of Representatives become substantially more likely to adopt the positions of special-interest donors as they vote on bills. Taken together, the evidence we present suggests that politicians are more inclined to take actions that benefit special interests when the public is temporarily distracted. More broadly, our findings imply that contemporaneous attention improves electoral accountability, even in an environment with stringent transparency and disclosure requirements.

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1. Introduction

A couple of years before his nomination to the Supreme Court, Louis Brandeis (1914, p. 92) famously remarked that “sunlight is said to be the best of disinfectants.” Underlying this now iconic statement is the widespread belief that transparency and public disclosure are necessary to hold government officials accountable. Although Brandeis advocated strongly for transparency, he also understood that public disclosure alone would likely be insufficient to ensure good government. Unless both citizens and the press are sufficiently attentive to politics, even perfect transparency need not guarantee that elected officials act in the best interest of their constituents.

Principal-agent models in political economy share Brandeis’s emphasis on the role of transparency and *ex post* sanctioning in holding government officials accountable (see, e.g., Barro 1973; Ferejohn 1986; Austen-Smith and Banks 1989; Strömberg 2004a; Besley 2006; Ashworth 2012). Notably absent from most theories, however, is voter attention and contemporaneous scrutiny as potential disciplining devices.\(^1\) Suppose, for instance, that the public stopped paying attention to politics in years without elections. Would members of Congress behave differently, even if their choices were publicly disclosed and could be held against them during the next campaign? On the one hand, the threat of compiling an unfavorable record may loom large enough for incumbents to ignore temporary fluctuations in scrutiny. On the other hand, retrospective sanctioning may be imperfect, which could allow politicians to exploit transient windows of opportunity. Ultimately, whether, and to what extent, contemporaneous attention helps to improve accountability is an empirical question. It is the question that we study below.

Estimating the impact of attention on politician behavior is difficult for at least four reasons. First, it is *a priori* not obvious how to measure attention to politics. Second, attention to politics may, at least in part, be a function of incumbent behavior. Both the media and voters may monitor corrupt politicians more intensely than honest ones, which may result in a spuriously positive raw correlation between public scrutiny and misconduct. Third, scrutiny is also a function of institutional disclosure requirements. Greater transparency makes it arguably easier for voters to *ex post* evaluate incumbents based on their record. Fourth, constituent preferences over the actions of their representatives are typically not observed. Even if contemporaneous attention is found to affect politician behavior, its impact on electoral accountability and the quality of representation may be ambiguous.

In order to deal with the last two of these challenges, we turn to the U.S. House of Representatives and examine whether congresspeople are more likely to vote in support of the

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\(^1\)A recent exception is Prato and Wolton (2016), who develop a model of elections in which successful communication of campaign messages requires effort by candidates as well as attention from voters.
positions of their special-interest donors when the public is temporarily distracted. Roll-call votes in support of special interests make for an interesting empirical setting because both campaign donations by special-interest groups as well as roll-call votes are a matter public record. We, therefore, study the effect of fluctuations in attention to politics, holding fixed a set of relatively stringent transparency requirements.

To be clear at the outset, we do not assume that the positions of voters and special interests are always opposed. Rather, we conduct a joint test of the idea that there are at least some issues on which the preferences of a representative’s constituents conflict with those of her donors, and that she is more likely to cater to the latter when the former are distracted. A null finding in this setting could thus either be due to no effect of attention or insufficient conflict of interests. A rejection of the null of no effect, however, is informative about the connection between attention and accountability.

To see this, suppose that only the wishes of constituents and special-interest groups enter a legislator’s calculus of voting. If the preferences of both groups are aligned, then the politician faces no trade-off, and fluctuations in voter attention should have no effect on her vote. If, however, less public scrutiny causes legislators to become more supportive of special interests, then there must have been a trade-off to which the politician reacted. That is, on the issue in question, the positions of the two groups must have been opposed. As a result, increasing support for special interests goes hand-in-hand with worse representation of constituents. Below, we extend this argument to the case in which politicians care about more than voters and special-interest groups. As long as preferences over the passage of bills remain fixed, we can continue to conclude that an increase in support for special interests is, on average, detrimental to the representation of distracted constituents.

To address the endogeneity of attention to politics, we exploit domestic natural disasters, such as earthquakes, tornadoes, and hurricanes, as sources of plausibly exogenous variation. Intuitively, the occurrence of such an event raises the opportunity cost of following day-to-day politics—for both the media as well as voters. Attention itself is measured by three different daily proxy variables: (i) the share of politics reporting on the evening news, (ii) the number of newspaper articles that mention a congressperson from the same state, and (iii) the volume of Google searches for a set of Congress-related keywords.

Consistent with the idea that adverse events distract the public, the first part of our empirical analysis provides evidence that, when a disaster occurs, the evening news temporarily scale back coverage of politics, local newspapers write fewer articles mentioning in-state representatives, and citizens conduct fewer Congress-related online searches. The second part of our analysis shows that when congresspeople vote on important (i.e., non-ceremonial) pieces of legislation in the immediate aftermath of a domestic natural disaster, they become
significantly more likely to support the positions of the special-interest groups that donated to their campaigns. In other words, for a couple of days, legislators whose donors support the bill vote more often in favor of passage, whereas lawmakers who are backed by special interests that oppose the measure become more likely to vote against it. The disaster-induced reduction in attention to politics, therefore, coincides with members of Congress catering more than usual to special-interest groups.

Identification in our empirical setup comes from the precise timing of disasters. Although some adverse events can be anticipated a few days in advance, there is no evidence of effects on roll-call votes in the days immediately before a disaster strikes. In its direct aftermath, however, even legislators whose constituents were not directly affected by the event tilt their votes. This latter fact allows us to rule out that disasters’ direct economic impact is responsible for the change in how congresspeople behave. In addition, we find no evidence to suggest that our results are driven by abstentions or the strategic scheduling of politically sensitive votes, i.e., agenda setting. Although agenda setting might have been an a priori plausible mechanism behind the effect that we uncover, we discount it based on four observations. 
(i) Roll-call votes held in the immediate aftermath of a disaster are neither associated with more interest-group money, nor with a greater number of position-taking interest groups. (ii) There are no detectable changes in the issue composition of bills and (iii) no differences in how quickly bills are being brought up for a vote. (iv) Estimates that attempt to condition on the content of a particular piece of legislation show, if anything, larger effects.

Broadly summarizing, our results suggest that a lack of contemporaneous attention and scrutiny induces moral hazard in congressional roll-call votes. This effect emerges even in an environment with stringent transparency and disclosure requirements. Although our identification strategy does not allow us to claim that money buys votes, or draw conclusions about welfare, it appears to be clear that special interests benefit. If one believes that the positions of voters and interest groups are not always well aligned, then our findings support the view that even transient reductions in attention to politics are detrimental to the representation of the distracted groups. At a minimum, our results are consistent with the core idea behind formal theories of accountability, i.e., that public scrutiny disciplines elected officials. At the same time, it is worth emphasizing that contemporaneous attention plays little to no role in extant models. Our findings thus point to a hitherto overlooked aspect in the principle-agent relationship between voters and politicians.

The remainder of this paper proceeds as follows. The next section reviews related work and clarifies our contributions to different literatures. Section 3 provides a conceptual framework for why contemporaneous attention to politics may affect accountability and the quality of representation. Section 4 lays out our econometric strategy. Section 5 discusses the data,
followed by our main results in Section 6. The penultimate section considers different explanations for our findings, and the last section concludes.

2. Related Literature

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

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

We add to this literature by shedding light on the conditions under which politicians are especially likely to support the positions of their donors. Although we cannot identify the causal effect of money on legislator behavior, we do provide evidence that attention to politics mediates the extent to which the positions of special interests are reflected in passage votes. Our results, therefore, suggest that, even if campaign contributions as well as roll-call votes are public record, continuous scrutiny may be important for curbing the influence of special interests.

We also contribute to a rapidly growing 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). 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 provision by the U.S. government decreases when 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 United States. An important difference between their work and ours is that natural disasters are difficult to anticipate far in advance. In fact, Durante and Zhuravskaya (2018) rely on disasters as a placebo test for their argument that only events that were foreseeable affect Israeli attacks. Hence, the findings of Durante and Zhuravskaya (2018) speak to governments’ ability to strategically schedule politically sensitive actions, whereas our results suggest moral hazard in legislatures, even conditional on the agenda. In addition, we expand on the findings of Eisensee and Strömberg (2007) and Durante and Zhuravskaya (2018) by showing that attention also matters for special interest politics.

Snyder and Strömberg (2010) explore 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. In addition, by exploiting changes in market congruence due to redistricting, Snyder and Strömberg (2010) provide 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 use the quasi-random timing of natural disasters to present evidence of moral hazard in Congress, holding the media’s effect on the selection of officeholders as well as their medium- to long-run incentives fixed. We show that even in a world of temporarily reduced scrutiny, special interests benefit because congresspeople systematically tilt their votes in favor of campaign donors. Our results, therefore, suggest that contemporaneous attention to politics is important for electoral accountability because it helps to discipline politicians on a day-to-day basis.

In simultaneous, unpublished work, Balles et al. (2018) also investigate the effect of disasters on legislator alignment with special interests. Balles et al. (2018) rely on similar data, and they estimate econometric models that are broadly comparable to the ones below. As a consequence, their headline result is virtually the same as ours: members of Congress are more likely to vote with their special-interest donors when a disaster strikes. One noteworthy partisan control of the media, Martin and Yurukoglu (2017) on media bias and polarization, as well as DellaVigna and Kaplan (2007), Gentzkow et al. (2011), Chiang and Knight (2011), and Enikolopov et al. (2011) on the effects of (biased) media on electoral outcomes.
difference between the two papers is that we directly document attention crowd-out. That is, we empirically show that disasters are associated with a reduction in coverage of politics on the evening news, fewer newspaper articles mentioning local congresspeople, as well as fewer internet searches for Congress-related keywords. We are, therefore, able to provide direct evidence in support of the claim that legislator behavior is moderated by a reduction in attention to politics.\(^4\) Also important, our empirical strategy eschews man-made disasters as sources of identification. Including adverse events such as terrorist attacks or school shootings might be problematic if either congressmen or interest groups adapt their positions in response to the incident—think, for instance, of the NRA and the push for gun control. A third important distinction is that we conduct an array of additional tests that point to individual-level moral hazard—rather than, say, agenda setting—as the mechanism behind our findings.\(^5\)

### 3. Conceptual Framework

Why would contemporaneous attention affect the behavior of politicians, especially in a setting with strict transparency and disclosure requirements? Below, we discuss two plausible reasons.

Principal-agent models in political economy emphasize the information contained in an incumbent’s record for holding politicians accountable. Yet, it is inherently difficult to evaluate legislators’ decisions \textit{ex post}. In the words of Austen-Smith and Banks (1989, p. 121),

“although roll-call votes and so forth are easily recorded, [...] legislatures are complex institutions in which many different types of agents engage in a wide variety of activity under various degrees of uncertainty. [...] to ascribe responsibility for any policy outcome to some particular legislator is often impossible.”

In an environment like this, constituents may only be able to confidently judge incumbents

\(^4\) To be clear, Balles et al. (2018) do show that Eisensee and Strömberg’s (2007) index of “news pressure” increases in the aftermath of a disaster. This index, however, is based in part on news reports about politics. It is, therefore, \textit{a priori} unclear whether news pressure increases due to more disaster reporting, increased coverage of congressmen voting with special interests, or both. By actually measuring the extent of politics reporting, we are able to provide direct evidence in favor of crowd-out.

\(^5\) Arguably less related to our work is a small literature that uses adverse events, like natural disasters or shark attacks, to draw inferences about voter competence (see, e.g., Achen and Bartels 2016; Bechtel and Hainmueller 2011; Healy and Malhotra 2009, 2010; Healy et al. 2010, but also the critiques of Fowler and Montagnes 2015; Fowler and Hall 2018; Ashworth et al. 2018). Also less related is recent work by Gagliarducci et al. (2019), who study how members of Congress change their support for environmental regulation in response to hurricanes. Gagliarducci et al. (2019) find that congresspeople whose district is hit by a storm become more likely to support green legislation. This effect persists for several years, consistent with permanent changes in beliefs. By contrast, the effects we document are present even for members of Congress whose districts were unaffected by the disaster, and they dissipate within a matter of days. Both sets of findings thus speak to very different mechanisms through which adverse events can alter politician behavior.
when they possess information on the context in which important votes were cast. By paying close attention to day-to-day politics, voters can acquire information that makes an incumbent’s public record interpretable. Contemporaneous attention also puts voters in a better position to evaluate claims by rival campaigns during election seasons, which may or may not be misleading. Viewed solely through the lens of standard theories of accountability, attentive voters likely receive more accurate signals about incumbent conduct. This, in turn, may mitigate moral hazard on the part of politicians.

In line with this view, research in the field of political behavior emphasizes the role of the news media for citizen information. Most voters make little effort to actively acquire political knowledge. They, therefore, remain uninformed on many questions, unless they can passively absorb the relevant facts through the media (see, e.g., Barabas et al. 2014, Jerit et al. 2006, and the studies cited therein). Absent news reports, voters tend to know little about incumbent (mis)conduct.

An alternative explanation for our findings is that media attention affects the flow utility from holding office. Drawing on his time shadowing congressmen in their districts, Fenno (1978, p. 74) provides a striking example:

“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.”

Moreover, contemporaneous attention might affect the need to explain. According to Kingdon’s (1973, p. 46) seminal account of legislators’ voting decisions, representatives “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.” He further remarks that “much of the problem of explaining one’s vote is directly tied to the media coverage which reaches the district. [. . .] [Congressmen] can implicitly assume that if the local media are not covering a story, the chances are that district attentive publics have not heard of it” (p. 204). If these and similar arguments are correct, then attention from both media outlets and voters may discipline politicians by influencing their flow utility. In fact, even incumbents who are electorally safe may try to avoid having to deal with angry constituents.7

Whether any of the forces above are, in fact, operative and strong enough to measurably affect members’ support for their special-interest donors is, of course, an empirical question. Before looking at the data, however, it is important to clarify what we can and cannot learn

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6In a similar vein, media attention is known to mediate the likelihood that allegations of misconduct spark political scandal (Nyhan 2017).
7For example, after voting to repeal the Affordable Care Act in 2018, a number of Republican legislators reportedly avoided town-hall meetings in anticipation of enraged voters.
from the findings in this paper—and under which assumptions.

A large literature in both economics and political science debates whether campaign contributions buy legislative favors, whether special-interest groups use donations to gain access, or simply help ideologically aligned legislators get elected. We do not take a stance in this debate. The results below show that, when natural disasters distract the public, politicians tilt their votes toward the positions of their special-interest donors. The data do not allow us to assess how representatives would have voted had they not received any donations. Regardless of why special-interest groups benefit, we can, under some assumptions, conclude that that the observed increase in support for special interests is detrimental to the representation of distracted voters.

This conclusion holds even if the preferences of a legislator’s constituents are well aligned with the positions of her donors on most, but not all, issues. To see the intuition behind this claim, it is useful to conceptualize a legislator’s calculus of voting as a game of tug of war between constituents and special interest groups. For now, assume that a legislator’s decisions are only influenced by these two groups. Sometimes, both pull in the same direction, whereas at other times, they are on opposite sides. When constituents are distracted, they pull less forcefully. Special-interest groups, however, always keep their eyes on the prize. Importantly, for issues on which constituent and interest-group preferences are aligned, it does not matter how hard the former pull. Legislators do not face a trade-off and thus always vote the same way. Hence, we would not expect to see attention effects when constituents and special interests agree. How hard constituents pull matters only when both groups are opposed. As a consequence, when special interests win more frequently than usual, constituents must have lost out.

It is straightforward to extend this argument to situations in which legislators care about more than just constituents and special interests. For simplicity, let us combine all other influences and call the resulting net impact a politician’s “personal” preferences, recognizing that it may also incorporate party influence and other considerations. Since roll-call votes are binary, a legislator’s personal preferences must either be aligned with those of her constituents, her donors, neither, or both. Taking the position of constituents as given, Table 1 enumerates all possible preference configurations. It also lists the predicted impact of voter distraction on legislators’ tendency to side with special-interest groups.

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

There are thus preference configurations in which increasing support for special interests is bad for voter representation, and ones in which it is good. Critically, these configurations yield opposite empirical predictions about the impact of voter attention. Conditional on observing that reductions in attention to politics go hand-in-hand with increased support for special interests, we can conclude that cases 3 and 4 are the empirically relevant ones. As a result, even if constituent and donor preferences are not the only factors entering a politician’s calculus of voting, increasing support for special interests when voters are distracted is, on average, detrimental to electoral accountability.

Clearly, this framework for thinking about the interpretation of our results is highly stylized in several respects. First, we have not only implicitly assumed that politicians should do what voters ask them to do, but also that voters are aware of what is in their own best interest. Neither is necessarily true. In principle, it is possible that when the public is inattentive, politicians’ incentives to pander weaken, which is why they might tilt their votes towards the option that is actually better for their constituents. For this reason, we refrain from making statements about welfare.  

The second simplification we have made is that voters are homogeneous. Relaxing this assumption would add additional groups and, therefore, cases to Table 1, but it would leave our analysis unchanged in all ways but one. When constituents have heterogeneous preferences, then we can no longer conclude that an increase in support for special interests is detrimental to electoral accountability in general. We can, however, continue to conclude that it worsens the representation of the distracted group of constituents, i.e., the group that pulls less hard.

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

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8 An additional reason for why we do not intend to make claims about welfare is that doing so would require us to assign utility weights to particular groups of voters and issues. Inevitably this would involve a series of normative, and potentially controversial, judgment calls.
4. Econometric Approach

Testing the idea that members of Congress are more inclined to support the positions of special-interest donors when their decisions are less likely to be salient to constituents requires us to deal with an important confounding issue. As pointed out in the introduction, attention to politics may influence lawmakers’ behavior, but it is also a function of voter interest, interest-group power, and the actions of officeholders themselves. Even if public attention does, in fact, discipline politicians, such an effect may be difficult to detect in raw data if, in equilibrium, corrupt representatives are more closely monitored than others. To overcome this likely source of bias, we rely on natural disasters as a source of plausibly exogenous variation in the degree of public attention to politics.

Our empirical analysis proceeds in two steps. First, we verify that natural disasters temporarily crowd out attention. We then examine whether politicians vote differently on bills around the time a disaster strikes. Throughout the first step, we follow an event-study approach and estimate \( \{ \varphi_t \} \) in variants of the following econometric model:

\[
\text{Attention}_t = \sum_{s \in W} \varphi_{t+s}\text{Disaster}_{t+s} + \kappa_m + \eta_t,
\]

where \( \text{Attention}_t \) corresponds to one of three proxies for public attention to politics on day \( t \), i.e., the amount of politics reporting on nightly news broadcasts, newspaper mentions of local legislators, and the volume of Congress-related Google searches. \( \text{Disaster}_t \) is an indicator for the start date of a disaster, \( W \) denotes the event window, and \( \kappa_m \) are month-by-year fixed effects. By including the latter, our estimates are identified by the precise timing of disasters rather than, say, seasonal variation.

In the second step of the analysis, we provide evidence from two complementary approaches. Specifically, we continue to estimate event-study specifications with lawmakers’ support for their special-interest donors as the outcome, i.e.,

\[
SIV_{l,r,t} = \sum_{s \in W} \varphi_{t+s}\text{Disaster}_{t+s} + \kappa_m + \mu_l + \xi_{l,r,t},
\]

where \( SIV_{l,r,t} \) is an indicator variable equal to one if and only if legislator \( l \)’s vote on roll call \( r \) aligns with the interest groups’ position that gave the most money to her campaign, and \( \mu_l \) denotes a legislator-by-congress fixed effect. All other symbols are as defined above.

In addition, we estimate models that more closely resemble those in the literature on special-interest politics. Prior work has often regressed roll-call votes on campaign contributions. An important limitation of this approach is that 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 causal effect of money on votes. In light of this issue, we do not attempt to estimate how representatives would have voted had they received no campaign contributions. Rather, we ask whether, in the immediate aftermath of a disaster, politicians become more likely than usual to vote in their financial contributors’ interests. In symbols, we are interested in \( \{\gamma_t\} \) in the specification below:

\[
SIV_{l,r,t} = \beta NetMoney_{l,r,t} + \sum_{s \in W} \gamma_{t+s} NetMoney_{l,r,t} \times Disaster_{t+s} + \kappa_m + \mu_l + \varepsilon_{l,r,t}.
\]

Clearly, if \( \text{Cov}(NetMoney, \varepsilon) \neq 0 \), then we cannot recover the true \( \beta \). 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 is “yes.” That is, even if \( \text{Cov}(NetMoney, \varepsilon) \neq 0 \), as long as \( \text{Disaster} \) is distributed independently of the controls 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 more than usual.

Besides aligning closely with previous work, an advantage of the model in eq. (3) is that it also leverages intensive-margin information for identification. Intuitively, specification (3) scales the effect of disasters by the amount of money a congressperson received. That is, the legislators who accept the largest donations from special interests should also be the most likely to change their behavior. Imposing this assumption comes at the cost of parametric restrictions, but it is potentially useful because it helps to improve precision.

Also note, since it is not clear what it means to support the position of one’s donors if none of them actually take a position on the bill in question, the outcome variable in specifications (2) and (3) is not defined for about 29% of roll-call votes in our data. Put differently, our main results restrict attention to votes that were cast by representatives who, based on our data, might be cross-pressured by constituents and special interest groups. Below we present robustness checks from alternative specifications that include all roll-call votes (cf. Table 7).

5. Data and Descriptive Statistics

Implementing our empirical strategy requires 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.

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9Legislator fixed effects do not fully resolve this issue because they do not control for within-legislator heterogeneity in the assessment of different bills, which may well be correlated with the positions of connected special-interest groups.
**MapLight Data**  Information on (i)–(iii) comes from MapLight, a nonpartisan nonprofit organization that strives to reveal money’s influence on politics. MapLight’s research staff comb through publicly available sources, like congressional testimony, news databases, and trade associations’ websites, to compile lists of organizations and interest groups that either supported or opposed important pieces of federal legislation (i.e., bills that are not merely ceremonial). 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 members of Congress in the same election cycle as well as the relevant roll-call votes. Starting with the 109th Congress (2005), MapLight publishes these data on its website.

Our empirical work 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. 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. Also, it is much rarer for interest groups to take an explicit, public stand on amendments. Important for our purposes, in 87% of instances MapLight was able to identify an interest group’s position prior to the vote. This mitigates the potential for disasters to a small which interest groups are recorded as having a stake in the bill.

Nonetheless, it is unrealistic to assume that the MapLight data include the universe of interest-group positions. To get a better sense of how complete our data are, we have cross-referenced the bills covered by MapLight with those in Box-Steffensmeier et al. (2019). The latter identifies pieces of legislation that were important to special-interest groups based on the universe of electronic Dear Colleague letters sent between members of the House during the 106th–111th Congresses. These letters are “the official [mode of] correspondence among members of Congress, used to promote and build support for legislation. [...] members rely on Dear Colleague letters to know what legislation has been introduced and quickly assess why they should support or oppose a bill” (Box-Steffensmeier et al. 2019, pp. 167). Importantly, in trying to bolster support for a bill, the respective sponsors generally list the organizations that have already endorsed it. Although the data of Box-Steffensmeier et al. (2019) pertain to supportive interest groups only, and despite the fact that they only capture interest-group backing up to the time the last letter related to a particular measure was sent, it may still be instructive to compare the set of bills that MapLight identifies as having drawn interest-group support with what is arguably the best alternative source of similar data.

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10 In 1998, the House established a listserv to distribute Dear Colleague letters via email instead of surface mail. In 2008, the House further streamlined the process by creating a web-based tool. Straus (2012) reports that, by 2009, 93% of House members used this system.
Focusing only on Congresses that are included in both data sets, and ignoring commemorative pieces of legislation as well as those that did not receive a passage vote, there are a total of 377 bills that were endorsed by at least one group. Of these, only 35 (9%) do not appear in the MapLight data, and among the bills that MapLight misses, only one appeared on the “Legislative Hot List.”\(^{11}\) For comparison, among all interest-group-endorsed bills, approximately 27% are classified as “hot.” Based on this evidence, it appears that the MapLight data cover the vast majority of bills that are relevant to special interests, and that bills that are not included—for whatever reason—are, on average, less controversial.

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. As the histogram in Figure 1 illustrates, conditional on a legislator having received any money from groups taking a stand on the bill, there is typically little ambiguity in whether her vote aligned with the position of her donors.\(^{12}\)

We further define the variable $NetMoney$ as the absolute value of the difference in total contributions between groups in favor of bill passage and those against. We use this measure to proxy for the strength of a congressperson’s ties to special interests. Conditional on receiving any money, representatives collect about forty-three thousand dollars more from groups on the dominant side. To put this number into perspective, we turn to the universe of campaign contributions reported to the FEC (cf. Bonica 2019). In a given election cycle, the average representative in our data receives about $670,000 in contributions from individuals and approximately $650,000 in donations from political action committees (PACs). Thus, on average, the contributions from groups that took a position on a particular bill amount to nearly 7% of all receipts from PACs (of which special-interest groups are a subset) and to roughly 3% of donations from all sources combined.

Besides campaign donations, there are, of course, many other ways in which interest groups

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\(^{11}\)The “Hot List” highlights contentious pieces of legislation, major policy initiatives, appropriations bills, and other important measures that were considered in each Congress, as determined by the Secretary of the U.S. Senate (Box-Steffensmeier et al. 2019). It is published by the Senate Library.

\(^{12}\)In 77.3% of cases, more than 99% of funds come from special interest groups on the same side of the issue; in 90.3% of cases, more than 75% of funds come from special-interest groups on the same side of the issue.
can influence lawmakers, including lobbying and even donations to connected charities (see Bertrand et al. 2018a). The reporting requirements for these activities, however, are typically less strict, which makes campaign contributions one of the few proxies for congresspeople’s ties to special interests that are readily observable (de Figueiredo and Richter 2014).

**Natural Disasters** 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 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. CRED assesses these criteria based on various sources, including UN agencies, nongovernmental organizations, insurance companies, press agencies, as well as other research institutes.

We limit our sample to domestic natural disasters that occurred between 2005 and 2017—the time frame covered by MapLight—and fall into the top tercile of events in terms of either the number of deaths, people affected, or damages. We focus on large domestic disasters because these events receive greater coverage by U.S. media and are, therefore, more likely to crowd out politics reporting than more-minor incidents and foreign ones.\(^\text{13}\) We further restrict attention to sudden-onset disasters. This restriction removes events for which the start date is too imprecisely defined to obtain sharp identification (i.e., epidemics, heat waves, or wildfires).\(^\text{14}\) All in all, we consider 200 disasters over a thirteen-year period. Of the 1,525 bills in our data, 186 were voted on within two days after one of these events.

**TV News** 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. For example:


\(^{13}\)In the appendix, we show that including large foreign disasters yields qualitatively similar but somewhat weaker results (cf. Appendix Figure A.6 and Table A.3). Interestingly, the effect of disasters on politics coverage on the evening news is primarily due to domestic events. In other words, even large foreign disasters lead to less crowd-out of politics reporting than domestic ones, which may explain why legislators are less likely to react to them.

\(^{14}\)Our main results would remain qualitatively unchanged if we included all domestic natural disasters recorded in EM-DAT (cf. Appendix Figures A.7–A.9 and Table A.3).
Romney in Utah with his hosting of party donors featured; details given about campaign donations and the super donors running the PAC Americans for Prosperity. [Center for Responsive Politics Sheila Krumholtz explains who a bundler is; asks why Romney won’t supply the list of bundlers’ names.]” (CBS Evening News, June 23, 2012)

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

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

The upper two panels of Figure 2 depict the distribution of politics scores. There is a very large mass point at exactly zero. Fewer than one in three segments receive a strictly positive score. Among these, however, we observe significant mass in the middle of the distribution. For segments with intermediate scores, it is \textit{a priori} unclear whether or not they should be classified as “political.” To make this decision in a principled way, we tasked a research assistant with manually coding a random subset of 1,000 segments. Taking the human judgment as the truth, we then search for the cutoff score that maximizes the overall classification accuracy on this “test set.”

As shown in the lower panel of Figure 2, there is a clear relationship between the average human judgement and Watson’s politics score, as defined above. Interestingly, for low values, higher scores are associated with large increases in predictive power. For values above .3, however, the relationship flattens. Overall, a score of .144 maximizes the share of correctly classified news segments. Using this cutoff, Table 2 constructs the confusion matrix. Given an accuracy of 91.6% and a false positive (negative) rate of 7.7% (11.1%), our automated
detection of political content appears to perform well—though it is certainly not perfect.\footnote{In the appendix, we present robustness checks in which we classify all news segments with strictly positive politics score as political. The results are quantitatively and qualitatively very similar (cf. Appendix Figure A.10).}

With this 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, \( News_{n,t} = \frac{\left( \sum_{s \in P_{n,t}} \text{Duration}_s \right)}{\left( \sum_{s \in S_{n,t}} \text{Duration}_s \right)} \), where \( P_{n,t} \) denotes the set of news segments that are deemed to contain political content and \( S_{n,t} \) is the set of all segments, including commercials. To demonstrate that changes in politics coverage coincide with changes in disaster-related reporting, we also use Watson to detect news coverage of disasters (see Appendix B for details).

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

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.\footnote{In the appendix, we show that our conclusions remain unaffected if we include data from all channels contained in VTNA (cf. Appendix Table A.2).} 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 econometric model in
Our results, therefore, control nonparametrically for the idiosyncrasies of the subset of networks for which VTNA provides information on news content on a particular day. In addition, we develop a method to adjust our regression estimates for measurement error in the left-hand side variable, i.e., $\text{News}_{n,t}$. Inevitably, using machine learning to classify news segments involves both type-I and type-II errors. Since the measured variable is binary, this type of error is necessarily nonclassical and it can be shown to introduce attenuation bias—even if disasters do not directly affect error rates (cf. Appendix C). If one is willing to rely on a subset of human-coded segments as “ground truth,” then it is possible to estimate both types of error rates and parametrically correct for the attenuation bias. Since the implied correction leaves our conclusions qualitatively unchanged, we relegate this part of the analysis to the appendix and present the more-straightforward, unadjusted estimates in the main text. Here, we merely note that, given the absence of comparable results in the literature, our method for debiasing the OLS coefficients may also be useful in other applications that use machine learning to measure outcomes.

**Newspaper Mentions** Although politics coverage on the evening news is useful to gauge overall media attention to politics, national broadcast news rarely report on the conduct of rank-and-file congresspeople, which may be especially important for holding legislators accountable. We, therefore, complement our daily measure of politics reporting with a second one that focuses on individual representatives. Specifically, for each congressperson, we search the NewsLibrary database for articles from in-state newspapers that mention her by name. We then use this information to construct a daily panel of mentions of individual congresspeople in local newspapers.

At the time of our queries, the NewsLibrary database indexed 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. 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.\footnote{Without these fixed effects, the estimated impact of disasters would be slightly larger. We, therefore, err on the side of being too conservative.}

**Google Searches** In order to directly gauge citizen (rather than media) attention to politics, we rely on the daily volume of Google searches for the following terms: “politics,” “Congress,” “Congressman,” “Representative,” “government,” “House of Representatives,” and “vote.” Focusing on the time period covered by MapLight, we downloaded these data from Google’s Trends tool, which provides information on the daily search volume for arbi-
trary keywords. For each query, the maximum of the time series that Google Trends returns is indexed by 100. Since it is currently not possible to download daily data for a period longer than three months, we proceed by downloading, for each keyword, the daily data for any given month, which we then multiply by the monthly search volume index for the same keyword. This procedure follows Durante and Zhuravskaya (2018), and it ensures that the indexed daily search volume for a given keyword is comparable over time. We then standardize the time series for each term and use the stacked standardized variables as outcome in the regression model in eq. (1).

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

Table 3 presents descriptive statistics for the most important 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.

6. Disasters, Attention, and Roll-Call Votes: Empirical Evidence

As explained above, our goal is to exploit the precise timing of natural disasters in order to test the idea that a temporary reduction in public scrutiny might benefit special interests. Figures 4–6 present evidence in support of the key assumption behind our identification strategy, i.e., that disasters crowd out attention to politics.

Figure 4 focuses on the content of the evening news. The estimates in the upper panel show newscasts airing more disaster-related content in the days leading up to a disaster, with a peak one day after the reported 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.

The lower panel of Figure 4 examines coverage of politics. There is little to no evidence of crowd-out before the disaster occurs, suggesting either that non-political content gets displaced first or that a broad set of topics are simultaneously crowded out, which may render the impact on politics reporting statistically indistinguishable from zero. On the day of the event, however, we do find a significant reduction in politics reporting. The relevant point estimate equals about 2% of total air time, which amounts to approximately 7% of the time devoted to politics on an average day. The disaster-induced crowd-out lasts for a

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19Recall, given nonclassical measurement error in the dependent variable, these numbers likely understate
total of three days, after which politics coverage on the evening news returns to normal.

In Appendix Table A.2, we present a series of robustness checks, some of which rely on data from all channels included in the VTNA data rather than just the “big three.” We also estimate models in which we standardize the left-hand-side variable by network as well as specifications with coverage in minutes per night as outcome. All estimates imply a temporary decrease in politics reporting, which is statistically significant at either the 5%- or 1%-level.

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

The evidence in Figures 4 and 5 pertains to the media. Although it is more difficult to measure changes in attention among ordinary citizens, Figure 6 attempts to do so using data from Google Trends. Specifically, Figure 6 shows estimated effects on the volume of disaster- as well as Congress-related Google searches. The former begin to gradually increase a few days before a disaster strikes, and they peak on the day following the event. Afterwards, the number of disaster-related searches declines monotonically.

The results on Congress-related searches are noisier and qualitatively similar to those in the previous figure. This is sensible. Compared to the usual search activity for the respective terms, the fraction of Congress-related Google searches that are crowded out by natural disasters is likely much smaller than the relative change in disaster-related queries. The imprecision of the point estimates in the lower panel of Figure 6 notwithstanding, it is possible to reject the null hypothesis that they are jointly equal to zero in the week following the event ($p = .011$). Moreover, three of the five days with the lowest search volume are the three days right after onset. The day with the fourth lowest number of searches is the day just before the event. Overall, the findings in Figures 4–6 suggest that disasters lead to a temporary reduction in how much attention both the media as well as citizens pay to politics.

The true effect size. For estimates that correct for attenuation bias, see Appendix C.
Having provided empirical support in favor of the assumption that disasters distract the public, we now ask whether the reduction in attention coincides with a change in the behavior of politicians. Based on the evidence in Figure 7, the answer appears to be “yes.” The upper panel of this figure displays estimates of $\varphi_t$ in eq. (2), i.e., the reduced-form effect of disasters on the alignment between representatives’ votes and the positions of their special-interest donors. The lower panel examines how the correlation between donations from special interests and votes changes in the aftermath of such an event. The estimates therein correspond to $\{\gamma_t\}$ in eq. (3). Regardless of specification, the results in Figure 7 imply that, when disasters strike, congresspeople tilt their votes to support their special-interest donors. More specifically, the impact of disasters on votes materializes “on impact,” and it lasts for about three days—the same amount of time that disasters crowd politics coverage on the evening news.

To put the point estimates in the upper panel into perspective, a six-percentage-point change in how often congresspeople vote with their donors amounts to approximately 7% of the mean frequency (cf. Table 3). Thus, taking the evidence at face value, the disaster-induced temporary reduction in attention to politics goes hand-in-hand with a large, short-lived change in legislator behavior.

Table 4 probes the robustness of our main result. To increase statistical precision, we do not estimate separate effects for each day in the event window. Instead, we modify the specification in eq. (2) by regressing legislators’ votes on a single indicator for the day of the disaster and the two following ones, as well as different sets of fixed effects. The evidence in columns (1)–(4) shows that controlling for legislator, legislator-by-Congress, year-by-month, or day-of-the-week fixed effects leaves the point estimate essentially unchanged.

Columns (5) and (6) add bill and legislator-by-bill fixed effects, respectively. To see how the coefficients in these columns are identified, it is useful to note that the House sometimes votes multiple times on the same piece of legislation. Repeated passage votes may, for instance, be necessary if the Senate makes changes to a bill that the House has already passed, after reconciliation, or if a measure that was initially defeated is being reconsidered. Out of the 1,525 bills in our data, 68 receive more than one passage vote. Among this subset, 25 are considered once within two days after a disaster. These are the bills that identify the coefficient of interest in columns (5) and (6). Clearly, this set is very small and unlikely to be representative of all legislation. It may nonetheless be reassuring that, if anything, the estimated effect increases when we account as much as possible for what is being voted upon.\(^{20}\) Unfortunately, controlling for bill fixed effects also increases the standard errors. We can,\(^{20}\)

\(^{20}\) Of course, even bill fixed effects do not perfectly condition on the content of the proposed legislation. This is because if a bill were exactly unchanged, then a second passage vote would generally not be necessary.
therefore, neither reject very large nor small effects based on these specifications. Perhaps the greatest value of the estimates in columns (5) and (6) comes from providing suggestive evidence against agenda setting, which we regard as an *a priori* plausible mechanism behind our main result.

The standard errors in Figure 7 and Table 4 are two-way clustered by year-month and legislator. If there is correlation in the residuals across legislators for a longer period of time, then this clustering scheme may not be conservative enough. As a robustness check, we conduct randomization inference, which does not require any assumptions on the correlation between residuals. Specifically, holding the actual number of disasters in our data fixed, we randomly draw new start dates and estimate the specification in column (4) of Table 4 on the surrogate data. Repeating this procedure 10,000 times, Figure 8 presents the distribution of placebo coefficients. Relative to its simulated counterparts, the actual point estimate is an outlier. It falls within the top 0.3% of placebos. We can, therefore, reject the sharp null of no effect.\(^{21}\)

Lastly, in order to alleviate potential concerns that our estimates are driven by unobserved heterogeneity across time, we have also estimated models with disaster-specific fixed effects, i.e., indicator variables for the +/- 7-day period around each event. The estimates from this specification are identified from local-in-time comparisons of politician behavior. As shown in Appendix Figure A.5, we continue to find that support for special interests increases by nearly six percentage points, which falls within the top 1.5% of placebo coefficients.

### 7. Potential Mechanisms

Our main results show that legislators become more likely to support the positions of their special-interest donors when natural disasters crowd out attention to politics. We now consider five separate explanations for why voting with special interests is more prevalent in the aftermath of these events.

#### 7.1. 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. Moral hazard might thus manifest itself in the decisions of the leadership rather than those of rank-and-file members. However, the

\(^{21}\)In Appendix Figures A.3 and A.4, we repeat the exercise for the day-by-day effects in Figure 7. The \(p\)-value for the contemporaneous effect in the upper panel is 2.6%, while that for the coefficient in the lower panel is 3.4%.
fact that we continue to observe an effect of disasters on votes, even after controlling for bill fixed effects, provides a first piece of evidence pointing away from this mechanism.

More evidence against agenda setting comes from the results in Table 5. If the leadership tried to exploit temporary “windows of opportunity” in order to pass sensitive legislation, then one might expect to observe a flurry of activity after disasters strike. The estimates in the upper part of the table imply that this is not the case. In particular, we find no evidence that more words are being spoken on the House floor, nor are more roll calls being held. In fact, the relevant point estimates are negative and economically small. Perhaps more importantly, it is not the case that the House becomes more likely to temporarily suspend its rules, which would be a way for the leadership to circumvent procedural requirements and, thereby, speed up the legislative process.

In addition to suggesting that disasters do not cause an increase in activity in the House, Table 5 also examines the distribution of issues that are voted upon. To construct the relevant estimates, we draw on information from the PIPC Roll-Call Database and the Comparative Agendas Project (Crespin and Rohde 2018). In these data, every roll-call vote is manually assigned to one of twenty-one “major topics.” We further aggregate their classification scheme and use the resulting issue indicators as outcomes in our workhorse regression model. All in all, we find little evidence of shifts in the issue distribution. Some of the estimates for issues that typically attract attention from special-interest groups are positive (i.e., labor, defense, and health), whereas others are negative (i.e., energy and agriculture). Only two of twenty one coefficients are statistically significant at the 5%-level. Assuming independence, the chance of observing two or more significant point estimates is about 28%.

The remaining estimates in Table 5 suggest 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. The former are neither more likely to have any interest group associated with them nor are there a higher number of position-taking interest groups. Moreover, there is no evidence of more money being given by the groups that did take a stand.

The last 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. Relying on information on discharge dates provided by MapLight, we count the number of days until a passage vote is held and compare the CDFs for bills that were and were not considered in the immediate aftermath of a disaster. Figure
8 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.\footnote{Not all bills in the MapLight data have a discharge date—either because there truly is no committee report or because MapLight fails to transcribe the relevant information. Important for our purposes, the probability of MapLight reporting a discharge date does \textit{not} depend on whether a bill was considered right after a disaster. Thus, nonrandom missingness is unlikely explain the finding in Figure 9.}

In sum, we do not find any evidence of agenda setting in connection with natural disasters. We hasten to point out, however, that this does not imply the absence of agenda setting in general. It may be the case that politically sensitive bills are more likely to be considered during times of \textit{predictably} high news pressure. As in Durante and Zhuravskaya (2018), the fact that natural disasters are difficult to anticipate far in advance could simply mean that other events, such as the Super Bowl or the Word Series, make for better opportunities to engage in strategic scheduling. Based on the results above, we can only conclude that moral hazard on part of the leadership is unlikely to be an important mechanism behind our main result.

7.2. Economic Damages

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

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

The first two columns in Table 6 provide additional evidence against mechanisms operating through direct economic effects. Specifically, we find that natural disasters appear to increase legislator support for special interests even among representatives from states that were not directly impacted by the event. In light of this observation, we discount economic damages as a plausible channel.

7.3. Party Pressure and Strategic Abstention

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

To test these explanations, we examine abstention rates and party-line votes as outcomes.23 The evidence in the four right-most columns of Table 6, 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.

7.4. Disaster Relief

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

Perhaps the most likely scenario along these lines is that an amendment providing disaster relief gets attached to a measure that is already scheduled for a vote. Such an amendment would likely increase how palatable the overall package is; and, since most special-interest money is given in support of bills, it may make it seem as if representatives pivot towards the positions of their donors. We address possibility in four 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 5, the answer turns out to be “no.”

Second, we ask whether the House votes on more amendments around the time a disaster strikes. Again, the answer is “no” (cf. Table 5).

Third, we re-estimate our workhorse empirical model excluding all bills whose title or description by the Congressional Research Service contains any of the above keywords. The results in Table 6 show that, if anything, our main result becomes slightly stronger.

Fourth, we assess whether disasters uniformly increase legislator support for bills. To this

23A party-line vote is defined as a legislator voting in the same way as the majority of her co-partisans, excluding herself (cf. Spenkuch et al. 2018).
end, we rely on the following econometric model:

\[
\begin{align*}
Y_{\text{ea}t, r, t} &= \beta^{(+)} \text{Money}_{l, r, t}^{(+)} + \beta^{(-)} \text{Money}_{l, r, t}^{(-)} + \\
&\quad + \gamma^{(+)} \text{Money}_{l, r, t}^{(+)} \times \text{Disaster}_{t}^{(012)} + \gamma^{(-)} \text{Money}_{l, r, t}^{(-)} \times \text{Disaster}_{t}^{(012)} + \\
&\quad + \delta \text{Disaster}_{t}^{(012)} + \kappa_{\text{m}} + \mu_{l} + \varepsilon_{l, r, t}.
\end{align*}
\]

Here, \(Y_{\text{ea}t, r, t}\) is an indicator equal to one if and only if legislator \(l\) votes “yea” on roll-call \(r\), while \(\text{Money}_{l, r, t}^{(+)}\) and \(\text{Money}_{l, r, t}^{(-)}\) denote the contributions she received from interest groups that support and oppose the bill, respectively. \(\text{Disaster}_{t}^{(012)}\) is an indicator for whether the roll call occurred within two days after a disaster. If the effect of disasters on votes operates solely through relief amendments or other last-minute changes that make the bill more attractive, then we would expect that \(\hat{\delta} > 0\), while \(\hat{\gamma}^{(+)} = \hat{\gamma}^{(-)} = 0\).

Table 7 presents results from estimating variants of eq. (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 5. Perhaps more importantly, with \(p\)-values ranging from .001 to .033, we can reject the null hypothesis that \(\hat{\gamma}^{(+)} = \hat{\gamma}^{(-)} = 0\). In other words, legislators do not become more generally 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 more palatable after disasters strike.\(^{24}\)

7.5. Moral Hazard

By contrast, bifurcation of representatives towards the positions of their donors is precisely what one would expect to see if the mechanism behind our main result is a temporary reduction in electoral accountability, i.e., moral hazard on the part of rank-and-file congresspeople. In our view, individual-level moral hazard is the most plausible explanation for why less attention to politics coincides with greater incumbent support for special interests. We come to this conclusion for three reasons: (i) moral hazard in roll-call votes is the only mechanism that consistent with the entirety of the empirical evidence above; (ii) it is consistent with the broad idea that public scrutiny disciplines politicians; and (iii) it resonates with some of the seminal qualitative accounts of congressmen’s decision-making (cf. Section 3).

\(^{24}\)Also, note that the model in eq. (4) does not require us to restrict attention to votes cast by representatives whose special-interest donors took a position on the bill. By considering all votes, the results in Table 7 demonstrate that our main result is not an artifact of the sample restrictions that are inherent to specifications (2) and (3).
8. Conclusion

The findings in this paper shed some of the first light on a hitherto overlooked aspect in the principal-agent relationship between voters and politicians. Relying on natural disasters as a source of plausibly exogenous variation, we provide high-frequency evidence in support of the view that contemporaneous attention matters for politicians’ support of special interests and thus electoral accountability.

Specifically, our results show that, when a disaster occurs, the evening news temporarily scales back coverage of politics. We also find suggestive evidence that newspapers report less on local congresspeople and that citizens conduct fewer Congress-related online searches. At the very same time, members of Congress become significantly more likely to cast votes that align with the positions of their special-interest donors on important pieces of legislation. This effect materializes “on impact,” and it lasts for about three days. Taken together, our findings imply that special-interest groups benefit and electoral accountability likely suffers when the public is temporarily distracted. More broadly, the results in this paper suggest that attention to politics helps to discipline incumbents on a day-to-day basis.

Viewed solely through the lens of canonical principal-agent models in political economy, 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 contemporaneous attention matters even in settings with stringent transparency and disclosure requirements. First, concomitant media coverage and voter attention might discipline politicians by furnishing citizens with more or better information about their actions, i.e., information that complements the public record. Alternatively, the effects we uncover might operate through incumbents’ flow utility, either because politicians derive disutility from negative press or because they loathe having to deal with angry constituents. Disentangling these channels and integrating attention into a formal model of electoral accountability are important tasks for future research. Our contribution in this paper is to empirically demonstrate that contemporaneous attention to politics is important for accountability even in an environment where one might not have expected it to matter.
References


Powell, Eleanor N. and Justin Grimmer. 2016. “Money in Exile: Campaign Contributions


Figure 1: Share of Donations from Supporting Interest Groups

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. As noted in the main text, these account for about 29% of bill-legislator combinations.
Figure 2: Detecting Political Content on the Evening News

A. Distribution of Politics Scores for all News Segments

B. Distribution of Strictly Positive Scores

C. Comparison with Human Judgement

Notes: The upper panel shows a histogram of all politics scores (as defined in the main text), while the middle panel restricts attention to strictly positive ones. The lower panel relates Watson’s politics scores to the judgments of a human coder, based on a sample of 1,000 randomly drawn news segments. The solid line in this panel corresponds to the smoothed mean frequency with which the human coder classified a segment as related to “law, government, and politics”, and the surrounding grey area shows the associated 95%-confidence interval. For additional information on the underlying data, see the Data Appendix.
Notes: The thick line shows the duration of politics coverage (in minutes) on the ABC evening news in 2012. The dashed lines indicate the onset of natural disasters, as reported in EM-DAT. For a detailed description of the underlying data, see the Data Appendix.
Figure 4: Impact of Natural Disasters on News Coverage

A. Disaster Reporting

B. Coverage of Politics

Notes: Figure displays point estimates and 95%-confidence intervals for the impact of natural disasters on disaster-related reporting (upper panel) and politics coverage (lower panel) on the evening news, i.e., $\phi_i$ in eq. (1). As explained in the main text, all estimates control 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 Appendix C.
Figure 5: Natural Disasters and Newspaper Articles Mentioning Congresspeople

Notes: Figure displays point estimates and 95%-confidence intervals for the impact of natural disasters on the number of newspaper articles mentioning an in-state congressperson, i.e., $\phi_t$ in eq. (1). The raw sample mean of the dependent variable is .968. As explained in the main text, the estimates control for legislator-specific year-by-month fixed effects in order to account for the time-varying number of newspapers in the Newslibrary database. Standard errors are two-way clustered by legislator and year-month.
Figure 6: Impact of Natural Disasters on Google Searches

A. Disaster-Related Searches

B. Congress-Related Searches

Notes: Figure displays point estimates and 95%-confidence intervals for the impact of natural disasters on the volume of Google searches for disaster-related keywords (upper panel) as well as terms related to Congress (lower panel), i.e., $q_t$ in eq. (1). All estimates are expressed in standard deviation units. Confidence intervals account for clustering by year-month. For a detailed description of the underlying data, see the Data Appendix.
Figure 7: Impact of Disasters on Voting with Special Interests

A. Reduced-Form Effect of Disaster

B. Change in Partial Correlation between Money and Votes

Notes: The upper panel displays point estimates and 95%-confidence intervals for the impact of natural disasters on Congresspeople's tendency to vote with their special-interest donors, i.e., $\phi_t$ in eq. (2). The lower panel shows point estimates and 95%-confidence intervals for the effect of disasters on the partial correlation between donations from special interests and congresspeople's votes, i.e., $\gamma_t$ in eq. (3). All estimates control for legislator-by-Congress and year-month fixed effects. Confidence intervals account for two-way clustering by legislator and year-month.
Figure 8: Randomization Inference for the Pooled Effect of Disasters on Votes

Notes: Figure shows the distribution of placebo estimates for the coefficient of interest in our workhorse regression model, i.e., column (4) of Table 4, based on 10,000 regressions with randomly reshuffled start dates of disasters. For details see the discussion in the main text as well as Appendix D.
Notes: Figure displays the CDF of the number of days that pass between the date a bill is being discharged from committee and when it receives a passage vote in the House. The solid line refers to bills that were voted upon within two days after onset of a natural disaster, while the dashed line refers to the remaining bills. A Kolmogorov-Smirnov test for equality of distributions yields a test statistic of .04 and a p-value of .98.
### Table 1: Preference Configurations and Empirical Predictions

<table>
<thead>
<tr>
<th>Alignment with Voter Preferences</th>
<th>Effect of Voter Distraction on Support for Special Interests</th>
</tr>
</thead>
<tbody>
<tr>
<td>Politician</td>
<td>Interest Groups</td>
</tr>
<tr>
<td>1. aligned</td>
<td>aligned</td>
</tr>
<tr>
<td>2. misaligned</td>
<td>aligned</td>
</tr>
<tr>
<td>3. aligned</td>
<td>misaligned</td>
</tr>
<tr>
<td>4. misaligned</td>
<td>misaligned</td>
</tr>
</tbody>
</table>

*Notes:* Figure refers to the thought experiment in Section 3. It shows all possible preference configurations and the associated prediction for the impact of voter distraction on legislator support for the positions of special-interest groups.
Table 2: Performance of Automated Content Classification

A. Confusion Matrix

<table>
<thead>
<tr>
<th>Human Coder</th>
<th>Watson</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Nonpolitical</td>
</tr>
<tr>
<td>Nonpolitical</td>
<td>73.2%</td>
</tr>
<tr>
<td>Political</td>
<td>2.3%</td>
</tr>
</tbody>
</table>

B. Performance Metrics

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

Notes: Entries in the upper panel are percentages comparing Watson's classification of 1,000 randomly drawn news segments as related to "law, government & politics" against the judgements of a human coder. As explained in the main text, Watson is said to classify a segment as "political" if the assigned score exceeds the cutoff score for maximizing accuracy, i.e., the fraction of segments that are correctly classified as either political or nonpolitical. Entries in the lower panel are descriptive statistics for the performance of the automated classification, taking the judgements of the human coder as ground truth.
Table 3: Descriptive Statistics

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

**Notes:** Entries are descriptive statistics for the most important variables used throughout the analysis. For precise definitions of all variables, see the Data Appendix.
Table 4: Natural Disasters Benefit Special Interests

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<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<tbody>
<tr>
<td>Immediate Aftermath of Disaster</td>
<td>0.056***</td>
<td>0.059***</td>
<td>0.059***</td>
<td>0.059***</td>
<td>0.086**</td>
<td>0.078**</td>
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<tr>
<td></td>
<td>(0.019)</td>
<td>(0.021)</td>
<td>(0.021)</td>
<td>(0.021)</td>
<td>(0.038)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>First Passage Vote</td>
<td>-0.035</td>
<td>-0.038</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.029)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fixed Effects:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year × Month</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Day of the Week</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Legislator</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Legislator × Congress</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Bill</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Legislator × Bill</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>R-Squared</td>
<td>0.002</td>
<td>0.059</td>
<td>0.067</td>
<td>0.085</td>
<td>0.354</td>
<td>0.858</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
</tr>
</tbody>
</table>

Notes: Entries are OLS point estimates and standard errors on the effect of natural disasters on whether congresspeople's votes on passage accord with the position of their special-interest donors on the same bill. "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, and "First Passage Vote" is an indicator for the first time a bill receives a passage vote in the House. 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.
### Table 5: Agenda Setting?

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Effect of Disaster</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td><strong>Congressional Speech:</strong></td>
<td></td>
</tr>
<tr>
<td>Number of Disaster-Related Words (in SD units)</td>
<td>-.077</td>
</tr>
<tr>
<td>(0.077)</td>
<td>(0.072)</td>
</tr>
<tr>
<td>Total Words Spoken on House Floor (in SD units)</td>
<td>-.023</td>
</tr>
<tr>
<td>(0.083)</td>
<td>(0.071)</td>
</tr>
<tr>
<td><strong>Number of Roll Calls:</strong></td>
<td></td>
</tr>
<tr>
<td>Total Number of Roll Calls</td>
<td>-.332</td>
</tr>
<tr>
<td>(0.208)</td>
<td>(0.169)</td>
</tr>
<tr>
<td>Number of Passage Votes</td>
<td>-.054</td>
</tr>
<tr>
<td>(0.068)</td>
<td>(0.069)</td>
</tr>
<tr>
<td>Roll Calls on Amendments</td>
<td>-.123</td>
</tr>
<tr>
<td>(0.152)</td>
<td>(0.120)</td>
</tr>
<tr>
<td>Procedural / Other Votes</td>
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</tr>
<tr>
<td>(0.061)</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Number of Votes to Suspend the Rules / under Suspended Rules</td>
<td>.001</td>
</tr>
<tr>
<td>(0.062)</td>
<td>(0.064)</td>
</tr>
<tr>
<td><strong>Roll-Call Issue:</strong></td>
<td></td>
</tr>
<tr>
<td>Agriculture</td>
<td>-.013**</td>
</tr>
<tr>
<td>(.006)</td>
<td>(.011)</td>
</tr>
<tr>
<td>Defense</td>
<td>.027</td>
</tr>
<tr>
<td>(.034)</td>
<td>(.031)</td>
</tr>
<tr>
<td>Energy &amp; Environment</td>
<td>-.027</td>
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<tr>
<td>(.017)</td>
<td>(.023)</td>
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<tr>
<td>Labor &amp; Education</td>
<td>.016</td>
</tr>
<tr>
<td>(.016)</td>
<td>(.018)</td>
</tr>
<tr>
<td>Health</td>
<td>.020</td>
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<tr>
<td>(.015)</td>
<td>(.012)</td>
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<tr>
<td>Foreign Trade &amp; International Affairs</td>
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</tr>
<tr>
<td>(.012)</td>
<td>(.016)</td>
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<tr>
<td>Domestic Commerce</td>
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<tr>
<td>(.014)</td>
<td>(.015)</td>
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<tr>
<td>Social Welfare &amp; Housing</td>
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</tr>
<tr>
<td>(.013)</td>
<td>(.016)</td>
</tr>
<tr>
<td>Government Operations</td>
<td>.012</td>
</tr>
<tr>
<td>(.026)</td>
<td>(.023)</td>
</tr>
<tr>
<td>Other</td>
<td>-.043</td>
</tr>
<tr>
<td>(.042)</td>
<td>(.019)</td>
</tr>
<tr>
<td><strong>Involvement of Special Interests in Roll Call:</strong></td>
<td></td>
</tr>
<tr>
<td>Any Special Interests Take Position</td>
<td>-.024</td>
</tr>
<tr>
<td>(.017)</td>
<td>(.018)</td>
</tr>
<tr>
<td>Number of Position-Taking Special-Interest Groups</td>
<td>-.495</td>
</tr>
<tr>
<td>(3.244)</td>
<td>(3.345)</td>
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<tr>
<td>Average Total Donations per Group (in $100,000)</td>
<td>-.574</td>
</tr>
<tr>
<td>(1.702)</td>
<td>(1.884)</td>
</tr>
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**Fixed Effects:**

<table>
<thead>
<tr>
<th>Year × Month</th>
<th>Yes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Day of the Week</td>
<td>No</td>
</tr>
</tbody>
</table>

**Notes:** Entries in column (1) are point estimates and standard errors from regressing the outcome on the left side of each row with an indicator for the immediate aftermath of a natural disaster, i.e., the day of the event and the two following days. The estimates in column (2) additionally control for year-month and day-of-the-week fixed effects. Standard errors are reported in parentheses and are clustered by year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.
### Table 6: Alternative Outcomes and Samples

<table>
<thead>
<tr>
<th></th>
<th>Vote with Special Interests</th>
<th>Partly-Line Vote</th>
<th>Abstention</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Immediate Aftermath of Disaster</td>
<td>.055***</td>
<td>.063***</td>
<td>.065***</td>
</tr>
<tr>
<td></td>
<td>(.020)</td>
<td>(.022)</td>
<td>(.020)</td>
</tr>
<tr>
<td>Fixed Effects:</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>Year × Month</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Day of the Week</td>
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<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Legislator × Congress</td>
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<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Sample</td>
<td>Excl. Repr. from</td>
<td>Excl. Bills Related</td>
<td>Baseline</td>
</tr>
<tr>
<td></td>
<td>Affected States</td>
<td>to Aid &amp; Relief</td>
<td>Sample</td>
</tr>
<tr>
<td>R-Squared</td>
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<td>.087</td>
<td>.003</td>
</tr>
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<td>Number of Observations</td>
<td>441,547</td>
<td>441,547</td>
<td>417,072</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Vote &quot;Yea&quot; on Passage</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Money from Supporting Interest Groups ($\beta^+$)</td>
<td>.020***</td>
<td>.018***</td>
<td>-.004</td>
<td>.008**</td>
</tr>
<tr>
<td></td>
<td>(.004)</td>
<td>(.004)</td>
<td>(.003)</td>
<td>(.004)</td>
</tr>
<tr>
<td>Money from Opposed Interest Groups ($\beta^-$)</td>
<td>-.181***</td>
<td>-.175***</td>
<td>-.157***</td>
<td>-.127***</td>
</tr>
<tr>
<td></td>
<td>(.025)</td>
<td>(.024)</td>
<td>(.022)</td>
<td>(.018)</td>
</tr>
<tr>
<td>Money from Supporting Interest Groups $\times$ Immediate Aftermath of Disaster ($\gamma^+$)</td>
<td>.006</td>
<td>.014**</td>
<td>.012</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.007)</td>
<td>(.006)</td>
<td>(.006)</td>
<td>(.008)</td>
</tr>
<tr>
<td>Money from Opposing Interest Groups $\times$ Immediate Aftermath of Disaster ($\gamma^-$)</td>
<td>-.067***</td>
<td>-.053***</td>
<td>-.032**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.017)</td>
<td>(.017)</td>
<td>(.014)</td>
<td>(.014)</td>
</tr>
<tr>
<td>Immediate Aftermath of Disaster ($\delta$)</td>
<td>.019</td>
<td>.022</td>
<td>.015</td>
<td>.021</td>
</tr>
<tr>
<td></td>
<td>(.019)</td>
<td>(.020)</td>
<td>(.015)</td>
<td>(.018)</td>
</tr>
</tbody>
</table>

Hypothesis Tests [p-values]:

- $H_0: \gamma^+ \leq 0$
  --
  .181
  .014
  .068

- $H_1: \gamma^+ \geq 0$
  --
  .000
  .001
  .009

- $H_2: \gamma^+ = \gamma^- = 0$
  --
  .001
  .001
  .033

Fixed Effects:

- Legislator $\times$ Congress
  No
  No
  Yes
  Yes
- Year $\times$ Month
  No
  No
  No
  Yes
- Day of the Week
  No
  No
  No
  Yes

R-Squared
.R046
.R047
.R238
.R315

Number of Observations
674,726
674,726
674,726
674,726

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

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Appendix A: Identification of \{\gamma_t\}

In the main text, we claim that \{\gamma_t\} in eq. (3) 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:\(^1\)

(A.1) \[ SIV_{l,r,t} = \alpha + \beta \text{NetMoney}_{l,r,t} + \gamma \text{NetMoney}_{l,r,t} \times \text{Disaster}_t + \varepsilon_{l,r,t}. \]

**Proposition:** Consider the data generating process in equation (A.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 \(\gamma\) is well identified. That is, \(\text{plim} \hat{\gamma} = \gamma\), even if \(\text{Cov} (\text{NetMoney}_{l,r,t}, \varepsilon_{l,r,t}) \neq 0\).

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

\[
\text{plim} \hat{\gamma} = \gamma + \frac{\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t, \varepsilon_{l,r,t} \right)}{\text{Var} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t \right)},
\]

where \(\text{NetMoney}_{l,r,t} \times \text{Disaster}_t\) denotes the residual from projecting \(\text{NetMoney}_{l,r,t} \times \text{Disaster}_t\) on the vector \([1 \ \text{NetMoney}_{l,r,t}]\). It, therefore, suffices to show that \(\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t, \varepsilon_{l,r,t} \right) = 0\).

From the definition of \(\text{NetMoney}_{l,r,t} \times \text{Disaster}_t\) and using the Frisch-Waugh Theorem again, we obtain:

\[
\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t, \varepsilon_{l,r,t} \right) = \\
\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t - \zeta - \frac{\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t, \text{NetMoney}_{l,r,t} \right)}{\text{Var} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t \right)} \text{NetMoney}_{l,r,t}, \varepsilon_{l,r,t} \right) \\
= \text{Cov} \left( \left[ \text{Disaster}_t - \frac{\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t, \text{NetMoney}_{l,r,t} \right)}{\text{Var} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t \right)} \right] \text{NetMoney}_{l,r,t}, \varepsilon_{l,r,t} \right),
\]

where \(\zeta = \text{E} (\text{NetMoney}_{l,r,t} \times \text{Disaster}_t) - \frac{\text{Cov} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t, \text{NetMoney}_{l,r,t} \right)}{\text{Var} \left( \text{NetMoney}_{l,r,t} \times \text{Disaster}_t \right)} \text{E} (\text{NetMoney}_{l,r,t})\),

and \(\text{NetMoney}_{l,r,t} \times \text{Disaster}_t\) corresponds to the residual from projecting \(\text{NetMoney}_{l,r,t}\) on a constant.

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

---

\(^1\)Extending the proof to the model in equation (2) is straightforward, but requires considerably more notation.
that

\[
\begin{align*}
\text{Cov} & \left( \frac{\text{Disaster}_t}{\text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t}} - \frac{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)}{\text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t} \right)} \right) \text{NetMoney}_{t,r,t}, \varepsilon_{l,r,t} \right) = \\
\text{E} & \left( \frac{\text{Disaster}_t}{\text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t}} - \frac{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)}{\text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t} \right)} \right) \text{NetMoney}_{t,r,t}, \varepsilon_{l,r,t} \right) \\
& - \text{E} \left( \frac{\text{Disaster}_t}{\text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t}} - \frac{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)}{\text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t} \right)} \right) \text{NetMoney}_{t,r,t} \varepsilon_{l,r,t} \right) \text{E} \left( \varepsilon_{l,r,t} \right) \\
= & \left\{ \text{E} \left( \text{Disaster}_t \right) - \frac{\text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t} \right)}{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)} \right\} \text{E} \left( \text{NetMoney}_{t,r,t} \varepsilon_{l,r,t} \right),
\end{align*}
\]

since \text{Disaster}_t is independent of \left( \text{NetMoney}_{t,r,t}, \varepsilon_{l,r,t} \right) and \text{E} \left( \varepsilon_{l,r,t} \right) = 0.

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

\[
\text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t} \right) = \frac{\text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \text{NetMoney}_{t,r,t} \right)}{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)}
\]

\[
= \frac{\text{E} \left( \text{Disaster}_t \text{NetMoney}_{t,r,t}^2 \right) - \text{E} \left( \text{Disaster}_t \text{NetMoney}_{t,r,t} \right)^2}{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)}
\]

\[
= \text{E} \left( \text{Disaster}_t \right) \frac{\text{E} \left( \text{NetMoney}_{t,r,t}^2 \right) - \text{E} \left( \text{NetMoney}_{t,r,t} \right)^2}{\text{Var} \left( \text{NetMoney}_{t,r,t} \right)}
\]

\[
= \text{E} \left( \text{Disaster}_t \right).
\]

This shows that \text{Cov} \left( \text{NetMoney}_{t,r,t} \text{Disaster}_t, \varepsilon_{l,r,t} \right) = 0, as desired.

\textit{Q.E.D.}

\textbf{Appendix B: Measuring Disaster Coverage on the Evening News}

As noted in the main text, we also use IBM Watson to detect disaster-related reporting on the evening news. In analogous fashion to our politics classifier, we define the \textit{DisasterScore} of news segment \textit{s} as the sum of the relevant confidence scores that Watson returns. Appendix Figure A.1 mirrors Figure 2 in the main text. It depicts the distribution of “disaster scores” (upper two panels) as well as their relationship with the judgments of our research assistant for the same “test set” of 1,000 randomly drawn news segments (lower panel). As was the case in Figure 2, most news segments have a score of exactly zero, and a much smaller number has a score of one. Given that there is again nontrivial mass in the middle of the distribution, we proceed the same way as classifying political content. That is, we search for the threshold score that maximizes the overall
classification accuracy relative to the classification done by the human coder. In the end, we say that news segment $s$ is disaster-related if and only if $\text{DisasterScore}_s \geq .178$, where .178 corresponds to the optimal cutoff.

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

**Appendix C: Correcting for Measurement Error in News Coverage**

Since we use machine learning to detect political content on the evening news, our measure of politics reporting will inevitably contain measurement error. This measurement error is necessarily non-classical.\(^2\) In fact, the errors are by construction “one-sided,” meaning that they are correlated with the true outcome. In what follows, we show that this causes attenuation bias in linear probability models, and we provide estimates that parametrically correct for the bias.

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

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

C.1. **Derivation of Bias**

C.1.1. **Main Result**

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

Consider the following linear probability model

(C.2) $Y = X\beta + \varepsilon,$

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

Let $\bar{Y}$ denote the true outcome. If there were no measurement error, $\varepsilon$ would be a binary random variable equal to $1 - X\beta$ with probability $Pr(\bar{Y} = 1) = X\beta$ and equal to $-X\beta$ with the comple-

\(^2\)To see this, note that the outcome is bounded by zero and one, which violates the assumptions in the classical measurement error model.
mentary probability. However, when using an automated classifier to measure the outcome, $Y$ will generally contain some error. Assume that $\theta_0 = Pr(Y = 0|\tilde{Y} = 0)$ and $\theta_1 = Pr(Y = 1|\tilde{Y} = 1)$. In the language of machine learning, $\theta_0$ denotes the specificity of the classifier (i.e., the probability of correctly identifying a “true negative”), whereas $\theta_1$ corresponds to its sensitivity (i.e., the probability of detecting a “true positive”). With this notation in hand, the expectation of $\varepsilon$ conditional on $X$ is given by

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

$$= X\beta(\theta_1 + \theta_0 - 2) + 1 - \theta_0.$$

Thus, the expectation of the structural measurement error model in eq. (C.2) is

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

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

$$E[\hat{\beta}_{OLS}|X] = (X'X)^{-1}X'E[Y|X]$$

$$= (X'X)^{-1}X'\beta(\theta_0 + \theta_1 - 1) + (X'X)^{-1}X'(1 - \theta_0)$$

$$= \beta(\theta_0 + \theta_1 - 1),$$

where the last equality uses the fact that $(X'X)^{-1}X'(1 - \theta_0)$ corresponds to regressing a constant on $X$, which returns zero. As a result, in the simple case, to correct for measurement error in the dependent variable, we must inflate the the OLS estimate by $(\theta_0 + \theta_1 - 1)$, i.e.,

$$\beta = \frac{E[\hat{\beta}_{OLS}|X]}{\theta_0 + \theta_1 - 1}. \tag{C.3}$$

Eq. (C.3) shows that unless $\theta_0 = \theta_1 = 1$—in which case there is no measurement error—the OLS estimate will be attenuated, and the bias depends on both the specificity and sensitivity of the classifier.

C.1.2. Extension to Weighted Averages

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

In particular, our regression model is given by

$$\mathbf{Y} = \mathbf{X}\beta + \mathbf{\varepsilon},$$

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

Proceeding as above,

\[
E[\hat{\beta}_{OLS}|X] = (X'X)^{-1}X'E[Y|X]
\]

\[
= (X'X)^{-1}X'X\beta + (X'X)^{-1}X' \sum_j \omega_j(X\beta(\theta_1 + \theta_0 - 2) + 1 - \theta_0)
\]

\[
= \beta + (X'X)^{-1}X'(X\beta(\theta_1 + \theta_0 - 2) + 1 - \theta_0)
\]

\[
= \beta(\theta_0 + \theta_1 - 1).
\]

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

C.2. Corrected Estimates

Appendix Figure A.2 presents the measurement-error-corrected estimates. Taking the judgement of the human coder as the ground truth allows us to estimate \( \theta_0 \) and \( \theta_1 \) by looking at the relevant confusion matrices (e.g., Appendix Table A.1 and Table 2). Relative to the corresponding results in Figure 4 in the main text, the point estimates pertaining to disaster reporting on the evening news need to be inflated by about 20\% (upper panel), while estimates of disasters’ impact on politics coverage need to inflated by approximately 23\% (lower panel).

Since \( \hat{\theta}_0 \) and \( \hat{\theta}_1 \) are themselves random variables, Figure A.2 uses the delta method to calculate standard errors for the adjusted point estimates. Under the assumption that classification errors are i.i.d., the estimated variance of a corrected coefficient is given by

\[
\text{Var}(\hat{\beta}) = \left[\text{Var}(\hat{\beta}_{OLS}) + \frac{(\hat{\beta}_{OLS}/(\hat{\theta}_0 + \hat{\theta}_1 - 1))^2}{((\text{Var}(\hat{\theta}_0) + \text{Var}(\hat{\theta}_1))/(\hat{\theta}_0 + \hat{\theta}_1 - 1))^2}\right]
\]

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

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

Appendix D: Ancillary Results and Robustness Checks

Appendix Figures A.3–A.5 present the outcome of randomization inference for the effect of disasters on voting with special interests. The results in Figure A.3 are based on the econometric model in eq. (2) in the main text, and thus pertain to the upper panel of Figure 7. The results in Figure A.4 pertain to the effect of natural disasters on the partial correlation between money and votes. They are based on the specification in eq. (3), and “replicate” the lower panel of Figure 7. The results in Figure A.5 “replicate” our workhorse empirical model in column (4) of Table 4 in the main text.
but additionally control for disaster-specific fixed effects, i.e., indicator variables for the +/− 7-day period around each event.

For all figures, we create 10,000 surrogate data sets by randomly reshuffling the start dates of the disasters in our data. We then estimate the relevant regression models on these placebo data and plot the distribution of the resulting coefficients of interest. Reassuringly, the evidence in Figures A.3–A.5 allows us to reject the sharp null hypothesis of no effect of disasters on congressmen’s votes in the immediate aftermath of the disaster. This is especially true when we pool over the day of the disaster and the two days thereafter, as shown in Appendix Figure A.5 and Figure 8 in the main text.

Appendix Figure A.6 replicates our finding that natural disasters crowd out attention to politics. Instead of relying only on large domestic disaster, in these figures we use both large domestic and foreign natural disasters as sources of plausibly exogenous variation. Specifically, 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—for a total of 378 events. 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.6 exhibit patterns that are qualitatively similar to those in the main text, adding foreign disasters generally reduces the point estimates and renders a number, but not all, of them statistically insignificant. The reduction in point estimates suggests that even the largest foreign disaster do not distract the public as much as large domestic incidents.

Appendix Figures A.7–A.9 also replicate Figures 4–6 in the main text. Instead of only considering large sudden-onset events, the results in this figure are based on all domestic natural disasters in the EM-DAT database. Although qualitatively very similar to the results in Figures 4–6, we note that most point estimates in Figure A.6 are somewhat smaller than their counterparts in the main text and, as a result, only marginally statistically significant. Again, this is not surprising. Small domestic disasters are less likely to garner wide-spread attention, and are thus less likely to affect how much the public follows politics.

Appendix Figure A.10 shows that our results with respect to the impact of natural disasters on the content of the evening news are robust to using a cutoff score of zero to classify news segments as either disaster- or politics-related (rather then the cutoff that maximizes the overall accuracy of the respective machine learning classifiers). This suggests that our conclusions about politics crowd-out on the evening news are not particularly sensitive to the specifics of how we translate Watson’s confidence scores into binary classifications.

Appendix Table A.2 probes the robustness of the effect of disasters on politics reporting with respect to: (i) standardizing the left-hand-side variable within each TV network (columns (1)–(6) and (13)–(18)); (ii) measuring coverage in raw minutes instead of relative shares (columns (7)–(12) and (19)–(24)); (iii) broadening the sample to include news shows on CNN and Fox News in addition to ABC, CBS, and NBC (columns (4)–(6), (9)–(12), (16)–(18), and (22)–(24));
and (iv) simultaneously correcting for the LHS measurement error introduced through machine learning, as explained in Appendix C (columns (13)–(24)). None of these changes materially affect our conclusions.

Appendix Tables A.3 and A.4 respectively replicate the results in Tables 4 and 7 in the main text, relying on either all domestic natural disasters reported in EM-DAT (upper panels) or large foreign as well domestic disasters (lower panels) instead of only the latter. As for the results in Appendix Figure A.6, when we refer to large foreign disasters, we mean the 178 foreign disasters that fall into the top-1% in terms of either the number of deaths, total number of people affected, or total damages. Again, the estimated effects decline somewhat in magnitude but are otherwise similar to their counterparts in the main text.

**Appendix E: Data Description and Definitions**

This section 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.

E.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](http://classic.maplight.org/us-congress/bill).

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

---


9
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.

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

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

- $\text{Disaster}$ is an indicator variable that is equal to one on the very first day an adverse event occurs and zero otherwise. In other words, $\text{Disaster}$ marks the onset of a natural disaster.
- $\text{Disaster}^{(012)}$ is an indicator variable that is equal to one on the first day an adverse event occurs and the two days thereafter. It is zero otherwise.

E.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.\footnote{We access Watson remotely through an API. For a free demonstration of Watson’s text-analytic capabilities see \url{https://natural-language-understanding-demo.ng.bluemix.net}.} In particular, Watson categorizes the content of unstructured text according to an enhanced version of the IAB Quality Assurance Guidelines Taxonomy Interactive Advertising Bureau (2013), which defines contextual categories that were originally designed to consistently describe web content in order to facilitate more relevant advertising and allow for \textit{ex post} analysis.
Watson’s taxonomy contains a major category for content related to “law, government, and politics.” As detailed in the text, we define a particular segment’s overall “politics score” as the sum of the confidence scores for all subcategories, up to a maximum of one. In symbols, \( \text{PoliticsScore}_s = \min\{\sum_{c \in C} \text{Score}_{s,c}, 1\} \), where \( C \) denotes the set of subcategories in “law, government, and politics.” We then say that a particular story covers politics if \( \text{PoliticsScore}_s \geq .144 \). The cutoff score on the right-hand side of this equation is the threshold that maximizes the overall accuracy of the classifier relative to the judgments of a human coder on a “test set” of 1,000 randomly drawn news segments. As shown in Table 2 in the main text, our machine-learning classification of political content achieves an accuracy of 91.6% and a false positive (negative) rate of 7.7% (11.1%) relative to the judgment of the coder.

With this 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, \( \text{News}_{n,t} \equiv (\sum_{s \in P_{n,t}} \text{Duration}_s)/(\sum_{s \in S_{n,t}} \text{Duration}_s) \), where \( P_{n,t} \) denotes the set of news segments that are deemed to contain political content and \( S_{n,t} \) is the set of all segments, including commercials.

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

E.4. NewsLibrary

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

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

E.5. Web Searches

We measure citizens’ interest in Congress using Google searches for the following terms: “politics,” “Congress,” “Congressman,” “Representative,” “government,” “House of Representatives,” and “vote.” The relevant data come from Google Trends, which we accessed via an API, and span the same frame as the MapLight data.\(^5\)

\(^5\)Google Trends is available at https://trends.google.com/.
Google Trends provides information on the daily search volume for arbitrary keywords. For each query the maximum of the time series that Google Trends returns is indexed by 100. Since it is not possible to download daily data for a period longer than three months, we proceeded by downloading, for each keyword, the daily data for any given month, which we then multiply by the monthly search volume index for the same keyword. In symbols,

\[ v_{k,t} = \tilde{v}_{k,m,d} \bar{v}_m \]

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

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

E.6. Other Data Sources

E.6.1. Congressional Speech

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

We use their data on full-text speeches in the House and the accompanying metadata for the 109th–114th Congresses. These restrictions are imposed to ensure that the setting for our analysis of congressional speech corresponds as closely as possible to the setting of our main analysis. We further process the full text of speeches by removing common stop words, such as “a,” “about,” “between,” “because,” etc., and by counting (i) the total number of remaining words spoken on a particular day, as well as (ii) the number of words that are plausibly related to natural disasters. To identify the latter we conduct a simple keyword search for the following terms: “disaster,” “emergency,” “relief,” “help,” “rebuild,” “assistance,” “victim,” “storm,” “hurricane,” “tornado,” “flood,” “landslide,” “earthquake,” or “volcano.” These daily counts then serve as outcome variables in our ancillary results (see above).

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.”

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

Data on the type of a roll-call vote come from the PIPC House Roll Call Database Crespin and Rhode (2018). Coverage of PIPC begins with the 83rd Congress. Among other information, these data contain a variable classifying each roll call as one of 59 mutually exclusive types, such as “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 prior years served as examples.

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

PIPC also contains hand-coded issue codes for each roll call. PIPC obtains the relevant information from the Comparative Agendas Project (PAP), which collects and organizes data from archived sources to track policy outcomes across countries. Again, we aggregate over different issue codes in the raw data to define 10 issue categories.

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.

\(^6\)See https://www.comparativeagendas.net/.
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.

Agriculture is an indicator variable equal to one if and only if the PAP major topic code equals 4.

Defense is an indicator variable equal to one if and only if the PAP major topic code equals 16.

Energy & Environment is an indicator variable equal to one if and only if the PAP major topic code equals either 7 or 8.

Labor & Education is an indicator variable equal to one if and only if the PAP major topic code equals either 5 or 6.

Health is an indicator variable equal to one if and only if the PAP major topic code equals 3.

Foreign Trade & International Affairs is an indicator variable equal to one if and only if the PAP major topic code equals either 18 or 19.

Domestic Commerce is an indicator variable equal to one if and only if the PAP major topic code equals 15.

Social Welfare & Housing is an indicator variable equal to one if and only if the PAP major topic code equals either 13 or 14.

Government Operations is an indicator variable equal to one if and only if the PAP major topic code equals 20.

Other is an indicator variable equal to one if and only if the vote falls into none of the above categories.

References


Appendix Figure A.1: Measuring Disaster-Related Content on the Evening News

A. Distribution of Scores for all News Segments

B. Distribution of Strictly Positive Scores

C. Comparison with Human Judgement

Notes: The upper panel shows a histogram of all disasters scores (as defined in Appendix B), while the middle panel restricts attention to strictly positive ones. The lower panel relates Watson's disaster score to the judgments of a human coder, based on a sample of 1,000 randomly drawn news segments. The solid line in this panel corresponds to the smoothed mean frequency with which the human coder classified a segment as related to natural disasters, and the surrounding grey area denotes the associated 95%-confidence intervals. For additional information on the underlying data, see the Data Appendix.
Appendix Figure A.2: Measurement-Error-Corrected Estimates of the Impact of Natural Disasters on News Coverage

A. Disaster Reporting

B. Coverage of Politics

Notes: Figure replicates the results in Figure 4 in the main text, correcting both point estimates as well as standard errors for measurement error in the left-hand-side variable. This correction uses the method developed in Appendix C.
Appendix Figure A.3: Randomization Inference for the Reduced-Form Day-by-Day Effect of Disasters on Congresspeople's Votes

Notes: Figure shows the distribution of placebo coefficients for the estimates in the upper panel of Figure 7. All graphs are based on 10,000 regressions, with randomly reshuffled start dates of disasters (see Appendix D for details).
Appendix Figure A.4: Randomization Inference for the Day-by-Day Effect of Disasters on the Partial Correlation Between Money and Votes

Notes: Figure shows the distribution of placebo coefficients for the estimates in the lower panel of Figure 7. All graphs are based on 10,000 regressions, with randomly reshuffled start dates of disasters (see Appendix D for details).
Appendix Figure A.5: Randomization Inference for the Pooled Effect of Disasters on Votes, Controlling for Disaster-Specific Fixed Effects

Notes: Figure shows the distribution of placebo estimates for the coefficient of interest in a variant of the regression model in column (4) of Table 4 with fixed effects for the +/− 7-day period around each disaster in our data. The distribution of placebo coefficients is based on 10,000 regressions, with randomly reshuffled start dates of disasters. For details see the discussion in the main text as well as Appendix D.
Appendix Figure A.6: Attention Crowd-Out Using Large Foreign and Domestic Disasters

A. Coverage of Politics on the Evening News

B. Newspaper Mentions of Congresspeople

C. Congress-Related Internet Searches

Notes: Figure replicates the lower panels of Figures 4 and 6, as well as Figure 5 in the main text, using large foreign and domestic disasters instead of only the latter ones (see Appendix D for details).
Appendix Figure A.7: Replication of Figure 4 Using All Domestic Disasters

A. Disaster Reporting

![Graph](image)

Notes: Figure replicates both panels of Figure 4 in the main text, using all natural domestic disasters in EM-DAT instead of restricting attention only to large ones (see Appendix D for details).

B. Coverage of Politics

![Graph](image)
Appendix Figure A.8: Replication of Figure 5 Using All Domestic Disasters

Notes: Figure replicates Figure 5 in the main text, using all natural domestic disasters in EM-DAT instead of restricting attention only to large ones (see Appendix D for details).
Appendix Figure A.9: Replication of Figure 6 Using All Domestic Disasters

A. Disaster-Related Searches

![Graph showing effect on disaster-related web searches over days after onset of disaster.]

B. Congress-Related Searches

![Graph showing effect on congress-related web searches over days after onset of disaster.]

**Notes:** Figure replicates both panels of Figure 6 in the main text, using all natural domestic disasters in EM-DAT instead of restricting attention only to large ones (see Appendix D for details).
Appendix Figure A.10: Sensitivity of Estimates in Figure 4 to Setting Cutoff Score to Zero

A. Disaster Reporting

B. Coverage of Politics

Notes: Figure replicates the results in Figure 4 in the main text, classifying all news segments with a strictly positive disaster or politics scores as disaster- and politics-related, respectively.
Appendix Table A.1: Performance of Disaster Classifier

A. Confusion Matrix

<table>
<thead>
<tr>
<th>Human Coder</th>
<th>Watson</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Not Disaster Related</td>
<td>94.0%</td>
<td>1.4%</td>
</tr>
<tr>
<td>Not Disaster Related</td>
<td>94.0%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Disaster Related</td>
<td>.7%</td>
<td>3.9%</td>
<td></td>
</tr>
</tbody>
</table>

B. Performance Metrics

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

Notes: Entries in the upper panel are percentages comparing Watson's classification of 1,000 randomly drawn news segments as related to natural disasters against the judgements of a human coder. As explained in Appendix B, Watson is said to classify a segment as "disaster related" if the assigned score exceeds the cutoff score for maximizing accuracy, i.e., the fraction of segments that are correctly classified as either disaster related or not. Entries in the lower panel are descriptive statistics for the performance of the automated classification, taking the judgements of the human coder as ground truth.
### Appendix Table A.2: Regression Evidence on the Effect of Disasters on News Crowd-Out

#### A. OLS Estimates

<table>
<thead>
<tr>
<th>Immediate Aftermath of Disaster</th>
<th>Hypothesis Tests [p-values]:</th>
<th>Fixed Effects:</th>
<th>Sample</th>
<th>R-Squared</th>
<th>Number of Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Year × Month</td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td></td>
<td>No</td>
<td>Yes</td>
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<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Big</td>
<td>Big</td>
<td>Big</td>
<td>All</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Three</td>
<td>Three</td>
<td>Three</td>
<td>Channels</td>
</tr>
<tr>
<td></td>
<td></td>
<td>.003</td>
<td>.101</td>
<td>.110</td>
<td>.001</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td><strong>H_0:</strong> No Effect of Disaster</td>
<td>.001</td>
<td>.001</td>
<td>.000</td>
<td>.003</td>
</tr>
<tr>
<td></td>
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<td></td>
</tr>
</tbody>
</table>

|                                |                             | Network × Day of the Week |       |          |                        |
|                                |                             | No             | Yes    | No       | Yes                    |
|                                |                             | No             | No     | Yes      | No                     |
|                                |                             | Big            | Big    | All      | All                    |
|                                |                             | Three          | Three  | Three    | Channels               |
|                                 |                             | .003           | .101   | .110     | .001                   |
|                                 |                             |               |        |          |                        |
|                                 | **H_0:** No Effect of Disaster | .001          | .001   | .000     | .003                   |

|                                |                             |               |        |          |                        |

|                                |                             | R-Squared |       |          |                        |
|                                |                             | .003      |        |          |                        |
|                                |                             |           |        |          |                        |
|                                |                             | Number of Observations | 10,932 | 10,932   | 10,932                 |
|                                 |                             |               |        |          |                        |

#### B. Measurement-Error-Corrected Estimates

<table>
<thead>
<tr>
<th>Immediate Aftermath of Disaster</th>
<th>Hypothesis Tests [p-values]:</th>
<th>Fixed Effects:</th>
<th>Sample</th>
<th>R-Squared</th>
<th>Number of Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Year × Month</td>
<td></td>
<td></td>
<td></td>
</tr>
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</tr>
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<td></td>
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<td>No</td>
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<td>No</td>
</tr>
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<td>Big</td>
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</tr>
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<td></td>
</tr>
<tr>
<td></td>
<td><strong>H_0:</strong> No Effect of Disaster</td>
<td>.002</td>
<td>.001</td>
<td>.001</td>
<td>.004</td>
</tr>
</tbody>
</table>

|                                |                             | Network × Day of the Week |       |          |                        |
|                                |                             | No             | Yes    | No       | Yes                    |
|                                |                             | No             | No     | Yes      | No                     |
|                                |                             | Big            | Big    | Big      | All                    |
|                                |                             | Three          | Three  | Three    | Channels               |
|                                 |                             | .003           | .101   | .110     | .001                   |
|                                 |                             |               |        |          |                        |
|                                 | **H_0:** No Effect of Disaster | .002          | .001   | .001     | .004                   |

|                                |                             |               |        |          |                        |

|                                |                             | R-Squared |       |          |                        |
|                                |                             | .003      |        |          |                        |
|                                |                             |           |        |          |                        |
|                                |                             | Number of Observations | 10,932 | 10,932   | 10,932                 |
|                                |                             |               |        |          |                        |

### Notes:
Entries are coefficients and standard errors from regressing different measures of politics reporting on an indicator variable for the day of and the two days after the reported onset of a large domestic disaster, as explained in Appendix D. The outcome in columns (1)–(6) and (13)–(18) is the within-network standardized number of minutes of politics coverage on the evening news. The outcome in columns (7)–(12) and (19)–(24) is the raw duration of politics reporting (in minutes). The sample in columns (1)–(3), (7)–(9), (13)–(15), and (19)–(21) includes only news shows that aired on ABC, CBS, and NBC, whereas the sample in columns (4)–(6), (10)–(12), (16)–(18), and (22)–(24) also includes shows on CNN and FNC. Entries in the upper panel are OLS estimates and the associated standard errors, while those in the lower panel use the measurement-error-adjustment developed in Appendix C. All standard errors are clustered by year-month. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.
## Appendix Table A.3: Replication of Table 4

### A. Using All Domestic Natural Disasters

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<th>(3)</th>
<th>(4)</th>
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<th>(6)</th>
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<td>.037*</td>
<td>.036*</td>
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<td>(.020)</td>
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<td></td>
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<td>(.029)</td>
<td>(.029)</td>
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**Fixed Effects:**
- Year × Month: No, Yes, Yes, Yes, Yes, Yes
- Day of the Week: No, Yes, Yes, Yes, Yes, Yes
- Legislator: No, Yes, No, No, No, No
- Legislator × Congress: No, No, No, Yes, No, No
- Bill: No, No, No, No, Yes, No
- Legislator × Bill: No, No, No, No, No, Yes

<table>
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</tbody>
</table>

### B. Using Large Foreign and Domestic Natural Disasters

<table>
<thead>
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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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</thead>
<tbody>
<tr>
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<td>.033**</td>
<td>.033*</td>
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<td>(.017)</td>
<td>(.017)</td>
<td>(.017)</td>
<td>(.033)</td>
<td>(.033)</td>
</tr>
<tr>
<td>First Passage Vote</td>
<td>-.039</td>
<td>-.042</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(.029)</td>
<td>(.029)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Fixed Effects:**
- Year × Month: No, Yes, Yes, Yes, Yes, Yes
- Day of the Week: No, Yes, Yes, Yes, Yes, Yes
- Legislator: No, Yes, No, No, No, No
- Legislator × Congress: No, No, No, Yes, No, No
- Bill: No, No, No, No, Yes, No
- Legislator × Bill: No, No, No, No, No, Yes

<table>
<thead>
<tr>
<th></th>
<th>.002</th>
<th>.058</th>
<th>.066</th>
<th>.084</th>
<th>.354</th>
<th>.858</th>
</tr>
</thead>
<tbody>
<tr>
<td>R-Squared</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Observations</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
<td>478,946</td>
</tr>
</tbody>
</table>

**Notes:** Entries replicate Table 4 in the main text using all domestic natural disasters (upper panel) as well as large foreign and domestic disasters (lower panel), as explained in Appendix D.
### A. Using All Domestic Natural Disasters

<table>
<thead>
<tr>
<th></th>
<th>Vote “Yea” on Passage</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)  (2)  (3)  (4)</td>
</tr>
<tr>
<td>Money from Supporting Interest Groups (β⁺)</td>
<td>.020*** (.004) (.004) (.004)</td>
</tr>
<tr>
<td>Money from Opposed Interest Groups (β⁻)</td>
<td>-.181*** -.174*** -.156*** -.126*** (.025) (.025) (.022) (.018)</td>
</tr>
<tr>
<td>Money from Supporting Interest Groups × Immediate Aftermath of Disaster (γ⁺)</td>
<td>.006 (.007) .011* (.006) .011* (.007)</td>
</tr>
<tr>
<td>Money from Opposing Interest Groups × Immediate Aftermath of Disaster (γ⁻)</td>
<td>-.052*** -.044*** -.027* (.018) (.018) (.014)</td>
</tr>
<tr>
<td>Immediate Aftermath of Disaster (δ)</td>
<td>.031** (.015) .033** (.016) .029* (.015) .020 (.016)</td>
</tr>
</tbody>
</table>

#### Hypothesis Tests [p-values]:

<table>
<thead>
<tr>
<th>Null Hypothesis</th>
<th>p-values</th>
</tr>
</thead>
<tbody>
<tr>
<td>H₀: γ⁺ ≤ 0</td>
<td>.181     .030 .049</td>
</tr>
<tr>
<td>H₁: γ⁺ ≥ 0</td>
<td>.002     .006 .031</td>
</tr>
<tr>
<td>H₂: γ⁺ = γ⁻ = 0</td>
<td>.001     .009 .067</td>
</tr>
</tbody>
</table>

#### Fixed Effects:

<table>
<thead>
<tr>
<th>Factor</th>
<th>True</th>
<th>True</th>
<th>True</th>
<th>True</th>
</tr>
</thead>
<tbody>
<tr>
<td>Legislator × Congress</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year × Month</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Day of the Week</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>R-Squared</th>
<th>Number of Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>.047</td>
<td>674,726</td>
</tr>
</tbody>
</table>

### B. Using Large Foreign and Domestic Natural Disasters

<table>
<thead>
<tr>
<th></th>
<th>Vote “Yea” on Passage</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)  (2)  (3)  (4)</td>
</tr>
<tr>
<td>Money from Supporting Interest Groups (β⁺)</td>
<td>.020*** (.004) (.004) (.004)</td>
</tr>
<tr>
<td>Money from Opposed Interest Groups (β⁻)</td>
<td>-.180*** -.173*** -.155*** -.127*** (.025) (.025) (.022) (.018)</td>
</tr>
<tr>
<td>Money from Supporting Interest Groups × Immediate Aftermath of Disaster (γ⁺)</td>
<td>.005 (.006) .011* (.006) .008 (.006)</td>
</tr>
<tr>
<td>Money from Opposing Interest Groups × Immediate Aftermath of Disaster (γ⁻)</td>
<td>-.052*** -.048*** -.023 (.019) (.017) (.015)</td>
</tr>
<tr>
<td>Immediate Aftermath of Disaster (δ)</td>
<td>.030* (.017) .032* (.018) .019 (.017) .013 (.017)</td>
</tr>
</tbody>
</table>

#### Hypothesis Tests [p-values]:

<table>
<thead>
<tr>
<th>Null Hypothesis</th>
<th>p-values</th>
</tr>
</thead>
<tbody>
<tr>
<td>H₀: γ⁺ ≤ 0</td>
<td>.224     .034 .097</td>
</tr>
<tr>
<td>H₁: γ⁺ ≥ 0</td>
<td>.004     .003 .056</td>
</tr>
<tr>
<td>H₂: γ⁺ = γ⁻ = 0</td>
<td>.020     .004 .142</td>
</tr>
</tbody>
</table>

#### Fixed Effects:

<table>
<thead>
<tr>
<th>Factor</th>
<th>True</th>
<th>True</th>
<th>True</th>
<th>True</th>
</tr>
</thead>
<tbody>
<tr>
<td>Legislator × Congress</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Year × Month</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Day of the Week</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>R-Squared</th>
<th>Number of Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>.047</td>
<td>674,726</td>
</tr>
</tbody>
</table>

Notes: Entries replicate Table 7 in the main text using all domestic natural disasters (upper panel) as well as large foreign and domestic disasters (lower panel), as explained in Appendix D.