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Special Interest Groups Versus Voters and the Political Economics of Attention

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Abstract

We examine whether representatives are more likely to serve long-term campaign donors instead of constituents during times of low media attention to politics. Based on 425 roll calls between 2005 and 2014 in the US House of Representatives, we show that representatives are more likely to vote with special interests and against constituency interests when the two are in conflict. Importantly, the latter effect is significantly larger when there is less attention on politics due to exogenous newsworthy events. The opportunistic behavior seems not to be mediated by short-term scheduling of sensitive votes right after distracting events.

Keywords: Attention, campaign finance, interest groups, legislative voting, mass media, roll call voting, US House of Representatives

JEL classifications: D72, L82, L86

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1 Introduction

Representatives in democracies want to be re-elected. In order to win an election, they have to convince their constituents to vote for them. Electoral support depends on the extent to which voters perceive representatives to support legislative bills in line with their preferences, as well as on persuasive campaigning, which is largely funded by special interest groups rather than constituents. These groups in turn contribute more if a representative votes as they desire. In this intuitive framework – conceptualized by Kau et al. (1982) – a conflict of interest can emerge. If, for a particular policy issue, special interests and the electorate's interests are not aligned, the representative faces a trade-off between serving the electorate and following the wishes of special interests.[1]

In this paper, we study the fundamental role that media attention plays in this trade-off. Most importantly, voters rely on media outlets as intermediaries for political information, while wealthy special interest groups are generally well informed about the representatives' actions in office. Accordingly, media attention to politics is expected to crucially affect whether representatives pursue the interests of their constituency when those are in conflict with the positions of special interest groups that donate to their campaigns. The implied strategic calculus has been noted in interviews with former congressmen. For example, Representative Vin Weber (R-MN, 1995) reports that *"If nobody else cares about it very much, the special interest will get its way. [...] If the company or interest group is (a) supportive of you, (b) vitally concerned about an issue that, (c) nobody else in your district knows about or ever will know about, then the political calculus is quite simple."* (Schram, 1995, p. 4).

Following this notion, we hypothesize that a representative is more likely to support a bill that goes against her voters' interests but is favored by special interests (that financially contribute to her campaign) at times of low media attention to the legislative process. In order to test this hypothesis, we exploit that media outlets in a competitive market need to assess the 'newsworthiness' of political information vis-à-vis non-political information, as resources for coverage and broadcasting time are limited. Accordingly, an extended coverage of non-political events or issues crowds out political coverage. Moreover, it induces variation in media attention to the legislative process that is independent from what is currently being debated in the legislature. The validity of exploiting exogenous variation in media attention due to newsworthy 'distracting' events is well established in the literature, pointing to media attention as a strategic factor which political agents bear in mind when they take their decisions.[2] However, measuring the electorate's *as well as* special interest groups' preferences regarding particular issues across a broad array of policy domains is challenging.

[1] We use the terms *interest groups*, *organized interests* and *special interests/special interest groups* interchangeably.

[2] Eisensee and Strömberg (2007) provide pioneering work on the US government's foreign aid decisions in response to natural disasters. Adopting an instrumental variable strategy based on a compiled measure of general news pressure (the measure that we use to select periods of low media attention to politics due to exogenous shocks), they show that a country is more likely to receive financial support if the disaster is covered by the US evening news. Garz and Sörensen (2017) find that politicians resign with a higher probability after their political immunity is lifted if their cases receive more exogenously determined media attention, and Durante and Zhuravskaya (2018) show that Israeli military forces attacks against Palestinians are more likely to occur one day before anticipated newsworthy US events take place. In a similar vein, Djourelova and Durante (2019) show that unpopular US presidential executive orders are signed in a strategic manner at times before other newsworthy and predictable events take place. Thereby, the behavior is only observed for periods of divided government when negative publicity due to congressional opposition is likely.

In our empirical investigation focusing on voting decisions in the US House of Representatives, we are able to approximate special interests' and voters' preferences in the context of a specific vote by a specific representative. By combining data on campaign finance donations from special interest groups with information on the same groups' positions on a particular bill, we construct a representative-vote-specific measure of interest group pressure. More precisely, we can observe how much money a representative receives over the election cycle prior to a certain vote from groups favoring the bill, as well as from groups publicly opposing the legislation. Importantly, we are neither assuming nor testing a particular model of strategic campaign donations but rather take campaign donations as a reflection of a long-term exchange relationship, approximating interest group pressure. Analogously, we define a representative-vote-specific measure of voters' interests which accounts for the extent of electoral pressure faced by a single representative with respect to a particular bill. Broadly speaking, we count the number of actively contributing citizens who are connected to groups that either favor or oppose specific pieces of legislation.[3] While the actively donating citizens form a rather small and comparatively wealthy fraction of the overall voting age population, we document that our measure well approximates the policy positions of the overall voting age population in specific domains. As part of the validation of our new measure, we show (at the level of individual bills) that it is strongly correlated with voters' policy preferences revealed in ballot measures (for the case of popular votes in California) as well as with voters' policy preferences expressed in election surveys (across the US).

Taken together, for a given US representative, we know the amount of campaign support she received over a long period of time from special interests supporting or opposing a particular bill, as well as the number of politically active (donating) citizens in her district that are rather in favor of or rather against this same bill. Overall, our unique data set includes information on individual level exposure to interest positions for 425 roll calls on 410 different bills between 2005 and 2014 in the US House of Representatives, leaving us with a base sample of over 175,000 observations.

Based on this data set, we test our baseline hypothesis by regressing representatives' voting decisions ('Yes' or 'No') on our measures for special and constituent interest, taking into account whether their interests are aligned and whether the roll call falls on a day with low attention to politics. We thus compare representatives' voting decisions in a situation of exogenously low media attention with the same representatives' decisions under normal media attention. Specifically, we exploit exogenous variation in the amount of news coverage given to the US lawmaking process that is driven by distracting events such as natural disasters, terror attacks, and shooting rampages.

Two main findings emerge from our analyses. First, if representatives face a conflict of interest as outlined above, their voting behavior follows the position of their special interest campaign donors with voter pressure losing out as a statistical determinant. Second, given a conflict of interest and in addition the occurrence of a distracting event, representatives are even more likely to vote in favor of a bill if they have close ties to special interest groups that support this bill. One standard deviation more (about $37,000) in donations from special interest groups favoring a particular bill increases the probability

[3]For example, if a bill intended to increase power production from renewables comes to the vote, our measure largely reflects the share of the donating population in a representative's district that is employed in the alternative energy sector or supports environmental protection groups minus the share of donating citizens working for traditional energy producers.

that a conflicted representative votes in line with the position of her donors and against the constituent interests by more than 27 percentage points if there is low media attention. Constituents' interests in a bill cannot account for their representatives' roll call voting behavior under this condition.

The two central findings are robust to various robustness checks. First, we show that alternative specifications for both the special and constituent interests measure do not change the main results qualitatively. Second, we restrict the selection of relevant shocks to those after which we document an actual crowding out of political news in local television and newspapers. Third, we check the sensitivity of our results to excluding those bills with policy content that might in some way be related to a shock event (and the assumption of exogeneity might therefore be violated). We find quantitatively very similar results consistent with our main findings. Fourth, we vary the level of intensity in our measures for shocks and representatives' conflicts of interest. Consistent with our theoretical framework, we find an increasing reaction in representatives' voting behavior, from none to medium up to serious shocks and show that the clearest conflict situations according to our measures for special and constituent interests also exhibit the most pronounced effects. Fifth, in a similar vein, we show that the distracting effect of shock events fades out after the events' news-crowding period. Sixth, we perform a placebo test in which we randomly assign the legislative votes to the shock treatment group. The results suggest that our main findings are very unlikely to be just due to chance. Finally, we show that the findings from our baseline linear model specification are robust to various alternative sample restrictions, alternative fixed effect structures, as well as to a Probit specification.

In order to better understand the mechanism behind the main findings, we extend our analysis in two directions. First, we show, based on two alternative tests, that there is no indication of systematic agenda-setting after shock events as a possible mechanism driving our results. That is, our results seem to be driven by individual representatives' opportunism rather than short-term agenda setting organized by the majority leader. Second, we further explore whether our results are more or less pronounced for representatives who face strong electoral competition (versus those with a relatively safe seat) or who take their decisions in an election versus a non-election year. We find that conflicted representatives are less responsive to special interest money after shocks when confronted with high electoral pressure (with an effect size reduced by more than 60%). For the corresponding effect in election and non-election years, no systematic difference is observed.

Our findings contribute to the literature on the role of campaign contributions in representatives' policy decisions. Important theoretical considerations are discussed in Kau et al. (1982) and Grossman and Helpman (1994). Empirical evidence for a positive relationship between campaign donations and legislative voting in line with the interests of donors is provided by many studies (see, e.g., Wilhite and Theilmann, 1987; Langbein and Lotwis, 1990; Stratmann, 1991, 1995, 2002; Fellowes and Wolf, 2004; Mian et al., 2010) – but not by all (see, e.g., Wright, 1985; Grenzke, 1989; Bronars and Lott, 1997; Wawro, 2001).[4] While together these contributions cover special interests' influence through

[4] Stratmann (2005) as well as Ansolabehere et al. (2003) provide excellent reviews of the literature, though they come to opposite overall conclusions regarding the general effectiveness of money in affecting policy outcomes. While Ansolabehere et al. (2003) emphasize that donations can to a large extent be understood as consumption of some expressive value, Stratmann (2005) focuses on money from special interest groups effectively affecting representatives' voting behavior.

campaign contributions on various issues, each study individually is rather selective as to what particular bills and interest groups it focuses on. This is due to the difficulty of measuring interest groups' and voters' preferences on a large number of diverse bills simultaneously. We rise to this challenge and propose a new way of measuring these preferences, allowing us to take into consideration a wide array of bills across the full range of policy domains simultaneously.[5] A prominent question in this literature is whether and to what extent donations actually change the positions of representatives or whether given positions attract donations. Our analysis does not aim at addressing this long-standing identification issue when interpreting the correlation between campaign contributions and voting behavior. We rather assume long-term exchange relationships between politicians and interest groups, which we try to approximate through campaign funds, and then study the behavioral consequences of variation in media attention to politics.

Our study importantly complements previous work examining the interaction between interest groups' influence through campaign money and attention to politics (e.g., Schroedel, 1986; Jones and Keiser, 1987; Neustadtl, 1990; Witko, 2006; Matter and Stutzer, 2019).[6] For specific issues, these studies provide evidence that media attention shapes the role of financial campaign support provided by interest groups in representatives' policy decisions, conditional on high or low attention to exactly the bills under consideration. In contrast, our study covers a large range of different policy issues and exploits exogenous variation in media attention to Congress. Hence, our results do not suffer from a potential selection bias, as our treatment, low attention to politics due to distracting newsworthy events, is independent of the bills under consideration.[7]

Further, our findings are important for the emerging literature that sheds light on the interaction between media markets and political markets. Contributions in this literature have documented how media access and news reporting influence government responsiveness and accountability, redistributive spending, and voter turnout (Besley et al., 2002; Besley and Burgess, 2002; Strömberg, 2004; Oberholzer-Gee and Waldfogel, 2009; Snyder and Strömberg, 2010; Gentzkow et al., 2011; Enikolopov et al. 2011). These studies therefore crucially contribute to our understanding of the media's role as the 'fourth estate'. In this context, our contribution stresses a potential systemic problem of the fourth estate based on free

[5] In fact, our data set contains the universe of non-amended bills in the US House between 2005 and 2014 on which at least one roll call on final passage took place and for which at least one organization publicly announced opposition or support (i.e., bills for which we can reliably construct preferences of voters and special interest donors).

[6] This literature does, in part, refer to different terms for what we here call 'media attention' or 'attention'. Among these, 'visibility' or 'salience' are the terms most often used.

[7] In concurrent independent research, Kaplan et al. (2018) investigate the influence of US natural disasters on the likelihood that House representatives vote with special interests. Their main finding is that the influence of special interest donors on post-disaster voting increases statistically significantly. Like us, they use MapLight data on the positions of interest groups regarding certain bills and campaign contributions to representatives in order to generate a measure for special interest group pressure with regard to specific legislative bills. In contrast to our approach, however, they leave it open whether the special interest group position is in conflict with that of the voters (and use both individual and PAC donations to construct their measure for special interests). A key difference to our contribution thus arises as we explicitly model the (bill-specific) preferences of voters living in the constituency of the representatives in order to distinguish the latter from the preferences of the special interest groups providing campaign contributions to the representatives.

media markets, when the role of money in politics and media attention are inherently interdependent. That is, media outlets' competition for the audience's attention (with the necessary focus on newsworthy events) gives special interest groups more influence over legislative politics, at the expense of voters.[8]

The remainder of this paper is organized as follows. Section 2 introduces our theoretical considerations on how media attention shapes a single representative's voting calculus, develops our main hypothesis, and introduces our empirical strategy. In Section 3, we describe the construction of our main explanatory variables measuring special interests' and voters' preferences with regard to particular policy issues. In the same section, we also back up the choice of distracting events that we use as indicators for reduced media attention to politics and validate our measure of voters' policy preferences in several ways. Section 4 presents our main results. Robustness tests are provided in Section 5. In Section 6 we investigate a potential mechanism where we hypothesize strategic agenda-setting by majority leaders as a possible force mediating our results. Section 7 further explores whether our results are more or less pronounced for representatives from highly competitive districts and in election versus non-election years. Finally, we offer concluding remarks in Section 8.

2 Basic framework and econometric model

Our basic mechanism regarding the interaction between media attention and the voting behavior of elected representatives can be easily developed within the conceptual framework of office-motivated representatives suggested by Kau et al. (1982). A representative is motivated to maximize net electoral support by taking into account the electorate's concerns as well as the policy preferences of special interest groups supporting her campaign financially. In cases where these two groups' preferences diverge, representatives face a trade-off.

Specifically, a representative is confronted with citizens who vote retrospectively, i.e., they evaluate a representative's performance on the basis of her past policy decisions. The higher the congruence of the representative's actions with the preferences of the voters, the more direct electoral support she gets (see Key, 1966 and Fiorina, 1981 for two seminal papers on retrospective voting).[9] In addition, interest groups provide campaign contributions which are used by representatives to finance the costly election campaigns, thereby fostering electoral support indirectly.[10] How much they provide depends on

[8] Importantly, this interpretation of our results is, theoretically, independent of the extent of media ownership concentration. A popular critical view expressed in earlier contributions on media economics (see, e.g., Herman and Chomsky, 1988) is particularly concerned with corporate/special interests' influence on public policy due to a lack of diversity and competition in free media markets. An argument is made that if a large majority of news outlets (in terms of the audience size they reach) is owned and controlled by a handful of large media corporations, which in turn are owned by other corporations, special interests have ways to directly influence legislative decisions via media attention because they control the media. Our results, of course, can neither contradict nor support this conjecture. They do, however, point to a complementary concern of more fundamental systemic/institutional nature than the conjectured supply-side driven influence on media attention through direct control of the media. Even with a highly competitive media market with many media outlets, a simple demand-side driven feature such as the focus on 'newsworthy' catastrophic events, tends to align legislative politics with special interest groups' rather than with voters' preferences.

[9] Barber et al. (2017) show that an alignment between individual campaign donors' policy positions and senators' voting decisions increases the likelihood of giving.

[10] In a recent empirical study, Spenkuch and Toniatti (2018) have evaluated the quantitative importance of campaign expenditures for a politician's vote share (see also Stratmann, 2019, for a review of the related literature). By comparing

a representative's position as well as its malleability regarding the policy issues they consider important. We think of campaign contributions primarily as reflecting long-term exchange relationships between interest groups and some individual representative. Thereby, donations serve as a potential channel of access that provides opportunities for further lobbying activities.[11]

We further assume interest groups always to be well informed about representatives' voting decisions, while voters are assumed to primarily rely on information provided by the media.[12] Consequently, the direct electoral support a representative receives from taking a certain policy position does not solely depend on voters' preferences, but also on how well voters are informed about and pay attention to their representative's voting behavior. In the most extreme scenario, assuming no media attention and a conflict of interest (e.g., special interests favor and voters oppose a particular bill), voters will not learn about their representative's voting decision, making a voter-congruent behavior costly in terms of losses in campaign donations provided by interest groups (and no gain in direct electoral support). More generally, whenever voters' and special interest groups' preferences on a particular issue/bill diverge, the trade-off faced by the representative is contingent on whether voters are currently paying attention to the lawmaking process. The less attention that is paid to the legislative process, the stronger the incentives for representatives to serve special interests and the lower the pressure to vote in deference of constituent interests.[13]

Based on our theoretical framework, we derive the following econometric model:

$$\begin{aligned} \textit{Vote Yes}_{ij} = \beta_0 &+ \beta_1 \textit{SIG Money Yes}_{ij} + \beta_2 \textit{Constituency Yes}_{ij} \\ &+ \beta_3 \textit{SIG Money Yes}_{ij} \times \textit{Shock}_j + \beta_4 \textit{Constituency Yes}_{ij} \times \textit{Shock}_j \\ &+ \beta_5 \textit{SIG Money Yes}_{ij} \times \textit{Conflict}_{ij} + \beta_6 \textit{Constituency Yes}_{ij} \times \textit{Conflict}_{ij} \\ &+ \beta_7 \textit{SIG Money Yes}_{ij} \times \textit{Shock}_j \times \textit{Conflict}_{ij} \\ &+ \beta_8 \textit{Constituency Yes}_{ij} \times \textit{Shock}_j \times \textit{Conflict}_{ij} + \beta_9 \textit{Conflict}_{ij} \\ &+ \textit{Representative}_i \times \textit{Party-of-Sponsor}_j \; FE \\ &+ \textit{Vote}_j \; FE + \varepsilon_{ij}. \end{aligned} \tag{1}$$

The dependent variable subsumes representatives' voting behavior on legislative proposals. $\textit{Vote Yes}_{ij}$ is an indicator that takes a value of 100 if representative i votes Yes in vote j (zero if she votes

neighboring counties that are (for exogenous reasons) assigned to different media markets, and therefore experience a different intensity in campaign advertising in the context of presidential elections, they find a large significant effect of advertising on the vote shares achieved.

[11] Access-oriented campaign donations are analyzed in Hall and Wayman (1990), Austen-Smith (1995), Kalla and Broockman (2016), and Fouirnaies and Hall (2018). Snyder (1992) and Kroszner and Stratmann (1998, 2005) emphasize the long-term motives in political giving.

[12] While organized interest groups have an advantage in monitoring the activities of representatives, a single rational voter has little incentive to actively learn about their positions (Lohmann, 1998). In the context of national US politics, organized interest groups are particularly well-known to keep track of representatives' actions in office with the help of professional lobby firms. For example, various influential special interest groups rank members of Congress based on how they vote in a number of roll call votes on issues considered particularly important to the interest groups, which obviously implies that these groups follow the members of Congress' actions very closely (see Fowler, 1982, for an early scholarly discussion of this interest group activity).

[13] In a model with endogenous news coverage, Prat and Strömberg (2013) show that voters are better able to hold their politicians accountable on issues that are receiving more attention from the media.

No). *SIG Money Yes*$_{ij}$ and *Constituency Yes*$_{ij}$ are our (continuous) measures for special and electoral interests' pressure that single representatives i face with regard to specific legislative votes j. In order to separate a situation where a representative faces voter and special interests that are aligned from a situation where they are in conflict with each other, we define an indicator, *Conflict*$_{ij}$, which reflects whether representative i faces a conflict of interest in vote j. It is one if either *SIG Money Yes*$_{ij} > 0$ and *Constituency Yes*$_{ij} < 0$ or *SIG Money Yes*$_{ij} < 0$ and *Constituency Yes*$_{ij} > 0$. To distinguish votes that have taken place with high attention from those that have been given less attention, we define the indicator *Shock*$_j$. It takes a value of one if vote j is taken within a defined time interval after a day with serious and exogenous shock activity (whereas the selected time intervals differ for the event types we consider). We include fixed effects for each vote and representative, whereby we interact the latter with the party the bill's sponsor is affiliated with. The representative $\times$ party-of-sponsor effects thus take into account the general willingness of a representative to vote in favor of a bill that was sponsored by a Democrat or Republican, respectively (i.e., two dummies for each representative). As campaign funds from specific groups interested in particular bills and representatives' ideologically fixed stances towards these bills are mutually dependent, it is crucial to include these individual fixed effects in the model.[14] The vote-specific effects take into account that there may be tendencies which lead, independently of single representatives, to a higher or lower acceptance regarding a particular bill at vote j. For example, vote-specific effects control for the impact of successful bipartisan negotiations on representatives' voting decisions. In such cases, voting No may be strongly sanctioned by party leaders.[15]

The interaction terms of *SIG Money Yes* and *Constituency Yes* with *Shock* and *Conflict* along with the triple interactions test our main hypothesis. Based on the estimated coefficients, we can compute and compare the marginal effects of campaign money and voters' preferences on representatives' voting behavior under different constellations. Specifically, we can distinguish four constellations, i.e., 1) a baseline with no shock and no conflict, 2) a first control with a shock in attention to politics, but no conflict, 3) a second control with no shock, but conflict and 4) a treatment with both shock and conflict. Compared to all other situations, we expect the highest effect of special interests as well as the lowest effect of the constituents' preferences under 4) – i.e., when the representative is conflicted and the vote is taken after a serious shock event. Note that our estimation strategy requires that any effect of special and constituent interests on representatives' voting decisions is identified by differences within a particular vote and within a particular representative who votes on a bill sponsored by one of the two parties. Concerning the models that contain interactions with *Conflict*$_{ij}$ we also include its main effect in order not to force the intercept for the relationship between special/constituent interests and conflict to zero. The main effect of *Shock*$_j$ in our model is already captured by the vote fixed effects we use in all our regressions. Finally, it seems plausible that the remaining factors, such as experience, which explain representatives' voting behavior on particular bills – captured by the error term ε_{ij} – are not independent of each other within a single representative. It is also likely that the observations within a particular

[14] We exclude 20 observations from an independent representative from the analysis.

[15] See Table D6 in the Online Appendix for a series of variations of our main specification differing in the fixed effects structure used. Our main findings are generally robust to these alternative specifications.

vote are correlated with each other. For example, the same interest groups may try to influence the roll call decisions of different representatives regarding a particular bill. We therefore two-way cluster the (heteroscedasticity-robust) standard errors at the representative and vote level.

3 Data

Our empirical strategy involves the compilation of a novel data set combining various data sources. In order to compare representatives' chosen policy positions with the preferred positions of the electorate and special interest campaign donors, we rely on information in the context of legislative voting in the US House of Representatives. Individual voting records, the so-called roll call votes, serve as our dependent variable. The roll call data are directly collected from Congress.gov (previously Thomas.gov) as provided by GovTrack. To construct our main explanatory variables, special and constituent interests, we link data on campaign finance donations from interest groups and individual citizens (provided by the Center for Responsive Politics, hereafter CRP) to information on which bills these two groups support or oppose (provided by MapLight). Overall, the data on roll call votes covers the period 2005 to 2014 (109th to 113th Congress), and consists of 175,223 individual voting decisions, taken in 425 roll call votes on 410 different bills. This selection corresponds to all final passage votes on (non-amended) bills for which MapLight has collected positions.[16] Finally, we link these political variables to data on exogenous shock events from databases on natural and technological disasters, terrorist attacks, and shooting rampages. This section describes in detail how these data are compiled and prepared, as well as how the resulting data set is used to test our main hypothesis.

3.1 Measure for special interests

We construct a representative-vote-specific variable of interest group pressure. For each representative i's voting decision regarding a particular bill (with j denoting the vote), we measure the extent to which organized interests that take a stand on the bill financially contribute to the campaign of representative i. For this purpose, we link data on campaign donations provided by the CRP with information on the donors' bill positions collected by MapLight (see sections A.1/A.2 in the Online Appendix for information on data access and specifics about the data compilation). Both of these research organizations are non-profit and non-partisan.

Originally, the campaign contribution data is from the Federal Election Commission (FEC), the US agency that regulates campaign finance in federal elections. Building on the raw FEC records, the CRP assigns a group code to each single transaction, identifying the donor's ties to industries or ideological

[16] Fifteen bills from our sample were voted on twice (resulting in 425 votes in total). There are two reasons for this. There are ten cases where the bill did not get the necessary majority in the first run, but was then voted on again at a later point in time. In another five cases the bill was passed by the House, but the Senate did not agree with the House version of the bill (both chambers must agree on the same version of the bill), and as a result an agreement was achieved involving a second vote by the House (on the adopted version of the bill). As there are no official amendments in the bill histories for the latter cases, we consider the corresponding bills to be unchanged. However, our results do not change either qualitatively or quantitatively if we exclude them from the sample (the additional results are available upon request).

groups.[17] The donors may be political action committees (PACs)[18] or individuals, whereby we only consider PAC contributions for our special interests measure. If, for example, the donation comes from an alternative energy producer's PAC, the corresponding group/industry code the CRP assigns is E1500 (*Alternative Energy Production & Services*). Since MapLight uses the same group categorization, we can directly link representatives' sources of campaign funding to specific pieces of legislation that are of documented interest to the contributing groups. MapLight uses public sources, such as news archives, congressional hearing testimonies, and groups' websites to document organizations' bill positions. Each record includes the bill number, the organization's name, the corresponding interest group code and its position on the bill (support, oppose or neutral), and the source MapLight used to identify the organization's position on the bill. If the above-mentioned producer of alternative energy adopts a clear position in favor of a particular bill (and this is considered by MapLight to be representative of the interest group *Alternative Energy Production & Services*), we code its campaign funds to a specific representative as money to vote in favor of the bill.

We restrict our analysis to the subset of final passage votes on bills for which MapLight provides positions on the associated bills. MapLight generally selects bills "that move forward in Congress or that are mentioned in the news or blogs. [MapLight does] not research support/opposition for ceremonial bills (such as naming post offices)."[19] Importantly, MapLight collects organizations' policy positions on bills, and does not assess their preferences with regard to single amendments on these bills. Therefore, we restrict our analysis to final passage votes on (non-amended) bills (excluding votes on amendments, committee reports, and procedural issues related to these bills).[20] For our selection of 425 votes in the House of Representatives, we cover 6,770 documented positions taken by 3,035 organizations, assigned to 371 different industry/ideological group codes. On average, for a single bill in our sample, we observe about 16 organizations that take a stand, belonging to nine different interest groups. The vast majority of the interest group positions (83%) were recorded by MapLight before the corresponding bills were voted on, with a median position quoted four days before the vote.[21]

[17] A complete list of the interest groups and sectors in the taxonomy of the CRP can be found under the following link: https://www.opensecrets.org/downloads/crp/CRP_Categories.txt.

[18] Organizations (but not individuals) that want to contribute to a candidate's campaign cannot do so directly. They have to establish a PAC that is regulated by the Federal Election Commission. Corporations, trade associations and unions establish a connected PAC, ideological or single-issue groups a non-connected PAC. Whereas for connected PACs, the establishing organization is allowed to provide funding for start-up and administrative costs, providing funds for the purpose of campaign contributions to a candidate is not allowed. Instead, connected PACs have to raise funds from individuals associated with the sponsoring organization, who are usually managers and executives in the case of corporations and members in the case of unions, trade and professional associations. Non-connected PACs, however, may accept funds from the general public, but are not sponsored by an associated organization.

[19] See http://classic.maplight.org/us-congress/guide/data/support-opposition.

[20] Previous studies on special interest politics in the US House have pointed to the importance of bill amendments for special interests to pass their favorite policies (see, e.g., Stratmann, 1992). Our theoretical framework does not distinguish between votes on amendments and final passage votes and would suggest the same rationale for representatives in a conflict of interest in either situation. If it is indeed the case that long-term exchange relationships with special interest groups have most influence on representatives' voting on amendments, our findings based on final passage votes would underestimate the magnitude of the phenomenon.

[21] A potential issue might be that the shock events that we use for identification have an impact on interest groups' bill positions. This would be particularly relevant for those bills that are related to the shocks in terms of content. In Section 3.4,

We sum up all the direct PAC donations a representative receives prior to a vote from interest groups supporting and opposing the related bill (*SIG Money Yes* and *SIG Money No*). In our baseline model, we consider all the donations that were made within the last election cycle before the vote (that is, the money donated during the last two-year term which helped with re-election). Specifically, we compute for each representative i the net donations in support of a specific bill that she received during her last election cycle before deciding on vote j, i.e., $\textit{SIG Money Yes (net)}_{ij} = \textit{SIG Money Yes}_{ij} - \textit{SIG Money No}_{ij}$.[22] For some bills, we observe contrasting positions taken by organizations associated with the same group. If this is the case, we calculate the share of supporting organizations among the total number of organizations supporting or opposing the bill (i.e., a bill support index), and distribute the money according to this weight. As the industry codes in our data set are rather highly granular (the financial sector alone is split into 36 distinct groups/sub-sectors), this potential caveat concerns only a very small fraction of our observations.[23]

In Section B in the Online Appendix, we analyze the determinants of the amount of campaign money (*SIG Money Yes* + *SIG Money No*) individual representatives receive from interest groups in the last election cycle before a particular vote and put the findings in perspective with the previous literature. The results from multiple regression analyses in Table B2 show that a Republican gets more money per vote than a Democrat, a member of the majority party more than a member of the minority party, a more experienced representative more than a less experienced one, a representative elected with a small margin of victory more than one with a safe margin, and a more moderate representative more than a more extreme one. Finally and not surprisingly, a more contested bill as well as a higher share of economic organizations interested in the bill attract more campaign money, on average.

3.2 Measure for constituent interests

We measure voters' preferences regarding a particular bill based on the idea that information related to donations from individual citizens allow us to approximate the fraction of citizens in an electoral district that are either negatively or positively affected by it. This section first describes the construction of the measure, and then validates it using ballot proposition votes, survey data, and official employment figures at the county/industry level.

Construction of the measure

The US *Federal Election Campaign Act* of 1971 requires candidates, parties and PACs to disclose their donors. Regarding individual contributions, they have to identify donors who give them more than $200 in an election cycle and to disclose their address and employer information. As for PACs, the CRP also assigns an industry or ideological group code to each individual transaction. Group assignment

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we systematically examine whether there is a contextual link between the bills that receive a shock treatment and the nature of the respective shock. Our results are very similar if we exclude those votes from the analysis where we cannot exclude a potential link between bill content and shock (see Section 5.3 in the robustness part).

[22] Previewing the empirical analyses, our results are robust against changing the time frame to different possible alternatives, e.g., last year, last six months or last ten years (see Figure 4 in the robustness part). This supports the perspective of a long-term relationship between special interest groups and policy-makers.

[23] Figure A1(a) in the Online Appendix shows how different sectors are represented in our special interests measure.

is based on the donor's employer if the contribution is to a party or to a PAC that her employer is associated with (usually corporations and trade associations). However, if an individual contributes to a labor union's PAC or to an ideological and/or single-issue PAC (e.g., environmental protection, human rights, or gun control) the CRP assigns their corresponding group code. If a citizen contributes to a candidate, either the employer's group code, or, if the donation is identified as being ideologically motivated, the corresponding ideological group code is assigned. The CRP codes a single transaction to a candidate as ideological if the donor also donates to an ideological PAC, and the candidate also receives funds from the same or similar ideological groups.[24] Relying on the CRP, we assume that individual donors and assigned industries/unions/ideological groups share the same political interests. We link the information on individual donors' interests with the MapLight data on groups' preferences over specific bills in the same way as with the special interests measure. Based on the revealed position of the interest group she is associated with, we can derive whether a single donor (likely) prefers or opposes a certain piece of legislation. In a next step, we count all individual donors in an electoral district (represented by representative i) with links to groups that are in favor of or against the observed bill j. We calculate the net support for a given bill subtracting the number of opposing donors from the number of supporting donors (#supporting − #opposing donors), resulting in *Constituency Yes (net)*$_{ij}$.[25] Note that with this approach all the contributions of citizens in a particular district are considered independently of whether they went to their representative in the House (including, for example, donations to presidential candidates, candidates for the Senate, or to PACs of any kind).

With regard to the time period, we count all donations made by individuals in the constituency of a representative during the last election cycle before the vote takes place (for example, if the vote takes place in May 2011, we count all donations made between the November elections 2008 and 2010). This holds for all donations except for those to presidential candidates. In the latter case, we consider donations made by individuals in a representative's district within the last presidential election cycle, i.e., the two years before the last presidential election (i.e., for the vote in May 2011 we count all donations to presidential candidates made between the 2006 elections and the 2008 presidential elections).[26] See also Online Appendix A.3 for more specific information on the compilation of the measure.

The advantages of our approach and the resulting measure of citizens' preferences lie, first, in the general applicability across policy issues. In the same way, we are able to gather and aggregate informa-

[24] See https://www.opensecrets.org/industries/methodology.php.

[25] An alternative approach is not to count individual donors, but to aggregate their contribution amounts, giving a higher weight to citizens who contribute more. We adopt this approach in a robustness test. We also estimate specifications in which we divide the absolute number of net supporters for a given bill by the total number of donations. This alternative construction of the measure for constituent interests scales net support for a given bill by the overall political activity level in a constituency. The corresponding results are presented in the part on robustness (see Figure 5).

[26] In cases where a citizen who is assigned to a particular group contributes more than once per election cycle, we count all of her transactions. An individual contributes about 2.3 times per two-year cycle on average. We thus take repeated contributions by the same individual into account by assigning a higher weight to this individual's preference in our measure for district interests. Only on rare occasions is the same donor assigned to different groups within a cycle (e.g., if the individual contributes to her employer's PAC and additionally to an ideological PAC). In such a case, we also count both transactions. On average, an individual has links to 1.1 groups per cycle. Depending on whether the groups the individual is linked with share the same position with respect to the observed bill, the individual donor gets a higher (if they agree) or lower (if they disagree and offset each other) weight. The median individual is assigned to one group and donates once per election cycle.

tion on individual donors linked to different kinds of groups like corporations, business associations, labor unions, non-profits, single-issue or ideological groups. Second, the measure captures people's affectedness with regard to specific legislative proposals reflecting preference intensities. Thus, people who care (and who are not close to being indifferent) are the likely donors. Importantly, political giving is not only positively correlated with turnout but also with volunteer campaign activities (Buchanan and Bird, 1966). Accordingly, our variable for constituent interests captures the subset of citizens that potentially generates a large proportion of the electoral pressure representatives face.

Validation of the measure

We validate our measure of voters' bill-specific policy preferences primarily in two ways. First, we adopt the most direct validation test possible and compare citizens' voting behavior on particular issues with our measure based on the number of donors. Second, we compare our measure with citizens' policy positions on certain bills as reported in election surveys, thereby neglecting preference intensities that affect turnout. Both analyses test whether our measure is consistent with other measures for voter preferences referring to the *exact same bills*. Moreover, we can check whether our measure based on comparatively wealthy citizens actually correlates with the revealed and reported policy preferences of the overall voting age population. In addition to the two tests focusing on expressed bill-specific preferences, we contrast the latent economic interests captured in our measure with industry-level employment figures across counties. This latter test validates that the selection of actively donating citizens in a district is in fact approximately representative for the employed voting age population of the same district.

Voting on propositions. Election returns of ballot propositions are a particularly attractive way to directly measure voters' preferences regarding a specific legislative proposal. They reflect people's choice ideally after a public debate leading to a binding collective decision involving those people who felt sufficiently affected. Measuring voter preferences in this way seems intuitive and arguably very close to the definition of bill-specific voter preferences. Moreover, this approach has been proofed useful in the recent literature.[27] Besides positions on federal bills, MapLight also documents positions on selected state legislation. This allows us to compare our measure for constituent interests with actual popular voting results for those ballot propositions that involve legislation previously passed in a state legislature, and for which MapLight has collected positions from interest groups. This applies to three ballot measures in the state of California from the years 2014 and 2016.[28] For these, we calculate the percentage of people in a county who voted in favor of the proposition in question (denoted *Yes share*), as well as the percentage of individual donations coming from people who are in favor of the bill in

[27]For example, Matsusaka (2017) constructs a bill-specific measure for voter preferences by using referendum election returns in nine US states; Stadelmann et al. (2013) follow the same approach in the Swiss context.

[28]The popular votes examined include Propositions 1 and 42 of 2014, and Proposition 67 of 2016. Proposition 1 dealt with Assembly Bill 1471 of 2014 (AB 1471) a legislatively referred bond act, Proposition 42 with Senate Constitutional Amendment 3 of 2013 (SCA 3) a legislatively referred constitutional amendment, and Proposition 67 related to Senate Bill 270 of 2014 (SB 270) a veto referendum. AB 1471 authorized California to raise $7.12 billion for water infrastructure projects; SCA 3 obliged local agencies to disclose government documents, and SB 270 banned the use of plastic bags in retail stores. All three measures were accepted. Regarding AB 1471, MapLight has documented positions of 15 different interest groups, for SCA 3 and SB 270 we observe positions of 2 and 20 different groups, respectively.

net terms, scaled by the total number of donations coded for and against the bill (therefore ranging from -100 to 100). We refer to the latter as *Yes donor share (net)*. Note that this corresponds to the normalized *Constituency Yes* (our baseline measure for constituent interests used in the main part of the paper). In cases where no donor at all is assigned to groups with positions, we replace the individual donation-based Yes shares with zeros, assuming that nobody is substantially affected by the proposed law. We estimate an OLS model in which we correlate the Yes shares for a particular bill based on individuals' donations, *Yes donor share (net)*, with the corresponding Yes shares based on ballot election returns (*Yes share*). Our sample for this analysis consists of 174 observations (3 ballot proposals $\times$ 58 counties). The related results are presented in columns (1) and (2) of Table 1. Both regressions account for bill fixed effects (some bills may per se affect voters more than others). We thus only exploit variation in the Yes shares for a given bill across the different counties. In column (2) we restrict the sample to those counties where the Yes shares in the popular vote lie between 40 and 60%. By only considering situations where a confrontation of different interests in a county is likely, we exclude the possibility that the correlation found between the two preference measures is primarily the result of rather clear cases where most voters agree or disagree on the bill in question. On average, referring to model (1), a ten percentage points higher proportion of people voting for the ballot proposition is associated with a 15-percentage-point higher share of donations from people supporting the related bill. Thus even though campaign donors probably make up the tail end of the distribution in terms of preference intensity and economic potency, they seem to reflect well the broader distribution of voter preferences in an electoral district.

Responses to survey questions. In a second validation, we check whether our measure for voter interests also correlates with a bill-specific preference measure obtained from survey data. For this purpose, we use information from the Cooperative Congressional Election Study (CCES), a web-based survey on congressional elections that also includes questions on important legislation discussed in Congress (Ansolabehere, 2010).[29] Using the CCES survey waves from 2006, 2008, 2010, 2012, and 2014, we are able to identify 23 bills for which MapLight also documents positions of interest groups (and we can thus construct our measure for constituent interests).[30] Analogous to analysis of the ballot proposition, we test in a regression framework whether more people in a congressional district who indicate in the survey their support for a certain bill (number of people who answer Yes divided by the number of people who say Yes or No, referred to as *Yes share*) is related to a higher share of donations from people in the district who support the bill in net terms based on the positions of the groups the CRP assigned to them – corresponding to *Yes donor share (net)*. As before, we consider all individual campaign donations in connection with federal elections that were made in the last (two-year) election cycle before the citizens were asked about the bill in question. The OLS regression results can be found in columns (3) to (5) of Table 1. As in the ballot proposition analysis, the dependent variable is our

[29] Ansolabehere and Jones (2010) study voters' responses in the CCES to analyze whether senators are held accountable based on voters' beliefs about their voting behavior. This seems to be the case. In a related study, Nordin (2019) constructs a bill-specific measure for voters' preferences using the CCES. He observes that voters with better access to relevant local television are more likely to evaluate their senators based on the alignment between their preferences and their senators' actual roll call decisions.

[30] Table A3 in the Online Appendix lists the bills under consideration.

measure for voter interests based on individual campaign donations. The estimates with bill fixed effects reveal a statistically significant correlation between the share of CCES respondents supporting a certain bill and the percentage of campaign donors from the district who have preferences for that bill. With reference to column (3), a ten-percentage-point higher Yes share in the CCES is on average related to a 2.8-percentage-point higher share of donations from people supporting the bill in net terms. In model (4) we restrict the sample to those districts where more than 60 people were asked in the CCES survey about the respective bill and where we observe more than 34 donations from people assigned to groups with preferences regarding the bill in question (i.e., with the number of observations above the 1st quintile for both variables). Finally, in column (5) we apply both the latter restriction and additionally limit the sample to only those cases where the CCES Yes share is between 40 and 60%. A correlation of similar magnitude is observed.

Table 1: Validating the constituent interests measure using election returns on ballot propositions in California and CCES survey data

Dependent variable: *Yes donor share (net)*	Ballot proposition votes		CCES survey data		
	(1)	(2)	(3)	(4)	(5)
Yes share (ballot/survey)	1.502***	1.746**	0.277***	0.236***	0.269***
	(0.236)	(0.754)	(0.040)	(0.038)	(0.086)
Bill FE	X	X	X	X	X
Yes share range	[0,100]	[40,60]	[0,100]	[0,100]	[40,60]
Respondents per district	-	-	> 0	> 60 (1st quint.)	> 60
#Donations per district	-	-	> 0	> 34 (1st quint.)	> 34
Observations	174	93	10,005	6,770	2,878
Adjusted R^2	0.156	0.046	0.758	0.884	0.916

Notes: OLS regressions with robust standard errors clustered by county or district in parentheses. The unit of observation is county/district-bill (county in California for the ballot proposition analysis and congressional district for the estimates based on CCES survey data). The dependent variable counts the individual donations from people in the county/district with links to groups that support the bill in net terms (relative to the total number of donations for and against the bill), with a range from -100 to 100. Its respective means/SDs (for the full sample) are 72.97/43.02 for the ballot analysis and 31.70/64.13 for the CCES data. The main explanatory variable in the ballot proposition analysis captures the share of people in the county voting in favor of the respective ballot measure. In the analyis based on CCES survey data, the main explanatory variable is the share of CCES respondents in the district who say that they support the bill in question, relative to the total number of respondents who say Yes or No. Their respective means/SDs are 53.84/11.86 and 51.96/17.52. * p<0.1, ** p<0.05, *** p<0.01.

Industry structure. Economic interests tied to people's jobs are an important driver of policy preferences (and have often been in the center of previous empirical applications, see, e.g., Mayda and Rodrik, 2005, in the context of individuals' preferences over trade policy).[31] In our third validation exercise, we

[31]The authors show that an individual's attitude towards trade correlates with the extent to which the sector where the individual works is exposed to trade. For example, people from sectors with a comparative disadvantage are more likely to have a protectionist attitude.

thus assess whether the industry structure is also reflected in our broader measure of constituent interests offering some economic face validity for the construct. Based on the US Census Bureau's *County Business Patterns (CBP)* statistics and the matching of the CRP industry codes to the classification used there, we find that county level employment across industries is strongly positively correlated with individual campaign donations of people who work in these industries (see Table A2 in the Online Appendix for the detailed results). Specifically, an increase in the employment share of a particular industry by ten percentage points is on average related to a four-percentage-point higher share of donors assigned to that industry.

Together, we interpret the results of the preceding validation tests as evidence that the presence of different policy preferences in a particular constituency can be well approximated by individual donors and the interest groups that the CRP assigned to them.[32]

3.3 Special interest groups versus voters

Figure 1 plots our measures for special and constituent interests against each other. Each point summarizes the situation a representative is confronted with when she votes on a particular bill, with campaign donors on the horizontal and voters on the vertical axis. The measure for special interests ranges from $-\$449,700$ to $\$1,076,000$, i.e., in the most extreme observation, a representative received more than a million dollars from groups supporting the bill during the last election cycle. On the other hand, voters' preferences range from about -10,170 to 22,490, i.e., a single representative faces an electorate where more than 22,000 politically active citizens (in net terms) are linked to groups which support the bill. All observations in the top-right and bottom-left quadrant reflect constellations where special and constituent interests are aligned regarding the issue being decided on. By contrast, observations in the top-left and bottom-right quadrant indicate a conflict of interest. This is the case for 8% of the individual voting decisions in the sample. The indicator $Conflict_{ij}$ from our estimation equation (1) accordingly takes on a value of one.

For example, on October 13, 2011, the House of Representatives voted on the *Protect Life Act* (H.R. 358 – 112th Congress), an anti-abortion bill. A total of 142 voting representatives (118 Republicans and 24 Democrats) faced a conflict of interest. Amongst these, 92 faced a conflict of the type interest groups Yes and voters No, with campaign funds from conservative and anti-abortion groups in favor of the bill on one side, and, on the other, an average voter against the bill, with links to pro-abortion,

[32]Moreover, the assumption that donors share the same political preferences as their employers (or those of unions or ideological groups, if the donation goes to such groups) seems quite reasonable given the context of the bills that we were able to investigate in terms of representativeness. Specifically, we have examined whether the bills from the California ballot proposition analysis and the CCES survey data are representative of the full set of 410 bills we use in our main analysis. Figure A2 in the Online Appendix shows the frequency with which interests of a particular sector take positions regarding the bills from the three different sets of bills. The largest share in all three sets of bills are interests associated with *Misc. Business*. Somewhat overrepresented in both the CCES and the Californian sample of bills are interests from the *Agribusiness* sector (15/16% versus 9%). In the CCES sample, we have seven bills (out of 23) in which we observe positions from interest groups associated with *Agribusiness*. An estimate of models (3), (4) and (5) from Table 1 excluding these bills provides point estimates of 0.43, 0.35 and 0.19 (p-values < 0.01, < 0.01 and < 0.1). The fact that we have only three bills that allow us to validate our measure of voter preference by ballot proposition voting in California explains why for some sectors we do not observe any positions of interest groups associated with them.

women's issues and human rights groups.[33] Of the representatives with such a type of conflict (all Republicans), the average received $9,000 from interest groups that supported the bill, and faced a constituency of which 126 citizens (who donate to campaigns) were against it (both in net terms). For the remaining conflicted representatives (26 Republicans and 24 Democrats), it was the other way around: special interests that opposed the bill (the average receiving −$13,700) and a constituency in favor of it (84 on average).

In 59% of the sample, our special interests measure takes on non-zero values. For the constituent interests variable, the values are non-zeros in 85% of the cases. And there are 56% where both measures take on non-zero values. Since the zeros for the variables capturing special and constituent interests are largely due to the fact that the observed representative does not receive any money from groups with positions or that there are no individual donors in her constituency, which are assigned to groups with positions regarding the bill that is being voted on (there are only a few cases where the money/donors in favor and against a particular bill cancel each other out), we code the variable $Conflict_{ij}$ with zero in cases where one measure is zero, arguing that campaign donors or voters may have no bill-specific policy preferences.[34]

3.4 Identification of (limited) media attention to politics

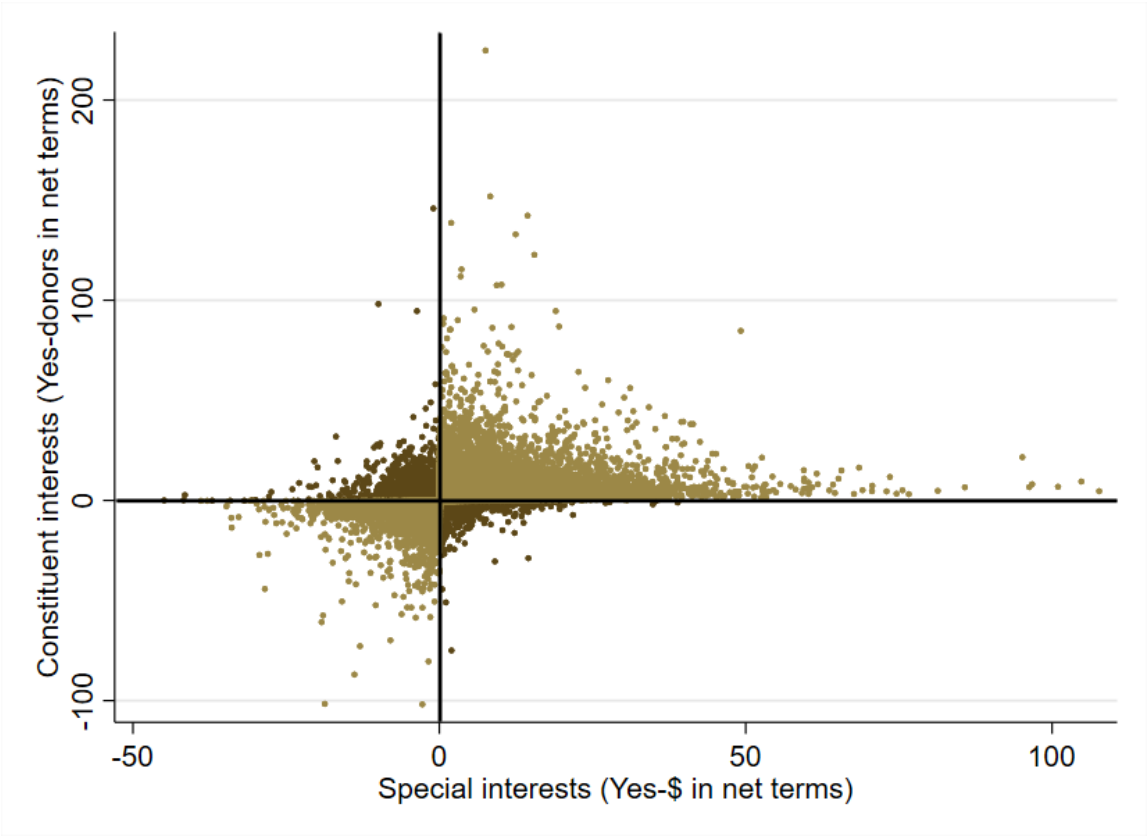
In previous research on attention and politics (see, e.g., Jones and Keiser, 1987 and Neustadtl, 1990), attention is measured by the media coverage of the bills under consideration. That is, the influence of labor union campaign spending on the representatives' voting decisions is studied by comparing how they voted when a labor-related bill got a lot of media attention with a situation when another labor-related bill got less media attention. There are substantial endogeneity concerns with such an approach, as there might well be factors, like the content of a bill, that also influence media attention to the bill, voters' and special interests' positions on this bill, as well as representatives' decisions when voting on it. Instead of measuring actual media coverage of certain bills, we therefore adopt a different *indirect* approach, building on the idea of *news pressure* pioneered by Eisensee and Strömberg (2007). The focus here is on competing newsworthy information that crowds out reporting on the legislative process, but is otherwise not related to it.[35]

[33]The bill was publicly supported (among others) by the Family Research Council, the Heritage Foundation, and the National Right to Life Committee. Opposition arose, e.g., from the American Civil Liberties Union, the Center for American Progress, the Human Rights Campaign, the National Women's Law Center, and the National Organization for Women.

[34]Excluding those observations from the analysis for which our measures for special and constituent interests take on zero values has a small impact on the results (the additional estimates are available on request).

[35]A potential alternative approach is to directly model voting decisions as a function of special interests' money and constituency preferences conditional on media coverage, and instrument media coverage with exogenous distracting events. In Section D.1 in the Online Appendix we show that such an IV approach would lead to qualitatively similar results. However, we give priority to the reduced form approach. While the IV approach does incorporate more structure (i.e., it directly models the role of media coverage), we should be careful not to interpret this structure as capturing fully the relevant economic mechanisms that play a role when a shock event occurs. Reduced attention to politics is arguably a market reaction following the demand for 'newsworthy content'. Such reduced attention to politics can be quantified/approximated by a reduction in coverage (number of articles in newspapers, number of minutes on television, etc.). But, realistically, it also involves a less prominent coverage of political issues (in terms of placement) as well as the media consumers paying less attention to political news (and more to the

Figure 1: Alignment and conflict between special interests' and voters' preferences faced by individual representatives



Notes: The unit of observation is a representative-vote-specific pair of positions. Observations are for the full sample of N=175,223. The special interests measure refers to *SIG Money Yes* (in $10,000 units), the constituent interests measure to *Constituency Yes* (one unit corresponds to 100 individual donations from citizens supporting the bill in question).

The selection of shock days

Our identification strategy draws on unpredictable events which reduce attention to politics but are arguably exogenous to the representatives and the bills they are voting on at/around the time of the event. For example, on October 12, 2011, a mass shooting occurred at a hair salon in Seal Beach, California. Eight people were killed. It was the deadliest mass killing in the history of Orange County.[36] The next day, October 13, 2011, the House of Representatives voted on the final passage of the previously-mentioned *Protect Life Act*.[37] Plausibly exogenous to the incident in California, we consider this vote as one that took place with little media attention to politics due to the distracting event.

shock-related news). Thus potentially many additional aspects are relevant for a more structural modelling of the phenomenon under investigation, but they are very hard to measure. Apart from potential measurement issues, the IV approach cannot change the essence of the key insights we can gain in this paper.

[36] https://www.latimes.com/archives/la-xpm-2011-oct-12-la-me-1013-seal-beach-shooting-20111013-story.html.

[37] It passed the House by a vote of 251-172. However, the bill did not make it through the Senate.

In addition to shooting rampages in the US, we consider worldwide disasters (natural and technological) and terrorist attacks as potential distracting events. The information on disasters is from EM-DAT, the International Disaster Database (Guha-Sapir et al., 2015).[38] The terrorism data originates from the Global Terrorism Database (GTD), introduced by LaFree and Dugan (2007). Regarding shooting rampages in the US, we rely on a list compiled by the *Los Angeles Times*, gathering the deadliest mass shootings over the last few decades.[39]

Previous work in media studies and communication studies shows that the perceived newsworthiness of a single event depends on its severity as well as on the place where it happened (Koopmans and Vliegenthart, 2011, provide an overview of these arguments as well as empirical evidence regarding the drivers of news coverage of natural disasters). The more disastrous an event is, the more likely it will make the headlines. Similarly, incidents which happen on US territory will attract more attention by US media makers than events taking place somewhere else. We therefore distinguish between events occurring in the US and those occurring in the rest of the world (ROW), and only select the most devastating events. The number of deaths involved serves as an approximation for evaluating the severity of single incidents. For each type of event and separately for the US and the rest of the world, we aggregate the number of deaths per day. We then define a 'shock day' if the number of deaths lies above the 99th percentile of its (event- and region-specific) distribution. This approach ensures that we just consider the most serious incidents which likely distract from the legislative process.[40]

We only consider incidents that last no longer than one day (whereas the consequences may well be experienced for weeks or months). This concerns natural and also some technological disasters.[41] The reason for this approach is pragmatic: based on the information at hand we cannot infer the peak of a disaster when it lasts several days.[42]

We end up with a list of clearly distinguished disasters that potentially crowd out news segments on other topics. Among the 26 one-day natural disasters that happened between 2005 and 2014 in the US, we mainly observe hurricanes and tornadoes (20 out of 26). Further, we record four earthquakes, one flood and one landslide. For events outside the US, one third refers to storms, one quarter to earthquakes and floods, and the rest to landslides, epidemics, extreme temperatures, volcanic activities and wildfires. Table 2 shows descriptive statistics for each type of shock event including the resulting 99th percentile thresholds. Note that for all the types of shocks in the US, the number of deaths is zero on over 99%

[38] EM-DAT reports a disaster if one of the following criteria is satisfied: i) Ten or more people dead; ii) 100 or more people affected; iii) The declaration of a state of emergency; iv) A call for international assistance.

[39] See http://timelines.latimes.com/deadliest-shooting-rampages/ (accessed August 8th, 2017).

[40] In Section 5.4 we show that our results are robust to selecting less severe incidents as 'shock events'. As expected, the theoretically 'stronger' treatment of selecting only the most severe incidents also has stronger effects on the representatives' strategic behavior, in line with our main hypothesis.

[41] In our sample period 2005-2014, 12% of the natural disasters in the US are one-day events, 50% last between one and five days, 26% between five and fifteen days, and the remaining 12% longer than fifteen days. The respective distribution for natural disasters in the rest of the world is 28%, 25%, 16% and 31%. For technological disasters in both the US and the rest of the world, over 90% are one-day incidents. All terrorist attacks and shooting rampages are one-day incidents.

[42] In a robustness check, we also consider disasters which last up to one week, for which we distribute the number of deaths that a disaster caused equally over all the days it took place. The correlation between the corresponding treatment (i.e., shock) indicators is 0.84, and the results are very similar if we use the modified indicator. The corresponding results are available upon request.

of the days in our sample period 2005-2014. That is why we only use days with a positive number of deaths here.[43] Concerning shooting rampages in the US, we do not rely on distribution characteristics, since the incidents on the list we use are already a selection of the most fatal incidents. We are ultimately left with 206 shock days in total.

Table 2: Shock events: The number of deaths per day (by type of event and region)

Type of event	Region	Mean	Min.	Max.	99th pctl.	#shock days (#incidents)
Natural disaster	USA	0.06	0	54	0	16 (26)
	ROW	121.80	0	222570	135	36 (1028)
Technolog. disaster	USA	0.14	0	50	0	31 (32)
	ROW	18.36	0	1199	173	36 (2284)
Terrorist attack	USA	0.02	0	15	0	28 (151)
	ROW	24.23	0	1542	200	36 (64478)
Shooting rampage	USA	-	-	-	-	23 (-)

Notes: We define a day as potentially distracting from the legislative process (i.e., a shock day) if the number of deaths per day lies above the 99th percentile of its (event- and region-specific) distribution. In the case of natural and technological disasters, we restrict the sample to one-day incidents (i.e., disasters which last no longer than one day). Regarding shooting rampages in the US, we use a list containing the deadliest incidents in the last decades (compiled by the *Los Angeles Times)* and, therefore, do not rely on distribution characteristics. The sample period is 2005-2014. ROW refers to the rest of the world and aggregates all countries outside the US.

As we want to measure potential distraction from the legislative process due to increased media attention to newsworthy shock events, in almost all the cases the relevant votes are those which take place afterwards. It is, of course, possible that votes are already affected on the same day, depending on the time of day a terrorist attack occurs, or even before the officially recorded first day of a natural disaster (for example, the days before a hurricane actually hits the shore). The consequence is that we may assign some treatment days to our control group. The same happens if we fail to capture, e.g., a newsworthy natural disaster, as the number of deaths it caused is below the 99th percentile threshold we use. Previewing the main analysis, any misallocation of days to treatment or control days attenuates any possible effect of media attention on voting behavior. The effects we find are therefore lower bounds to the true effect.

In order to validate our approach as well as to assign the shock events as precisely as possible to control group and treatment group, we study i) whether we indeed observe a crowding out of news stories after the days we marked as shock days, and ii) how far the appropriate time frame reaches into the future. The analysis is based on an indicator of *daily news pressure* as developed and made available by Eisensee and Strömberg (2007).[44] It measures the median length of the first three stories in the US

[43]The respective thresholds are 99.56% (natural disasters), 99.15% (technological disasters) and 99.23% (terrorist attacks), i.e., regarding terrorist attacks, the number of deaths caused by terror in the US is zero at 99.23% of days between 2005 and 2014 (3,624 out of 3,652 days)

[44]The measure covering the years 1968-2018 is accessible via David Strömberg's homepage under http://perseus.iies.su.se/~dstro. In their original paper, Eisensee and Strömberg (2007) in part 'remove' coverage of natural disasters from

evening news (across the television networks ABC, CBS, CNN and NBC). The idea behind *daily news pressure* is that if a major media event occurs, the news stories usually become longer and the events are placed at the beginning of a bulletin. As total airtime is limited to 30 minutes, the length of the first three segments is a good measure for how much newsworthy material is available on a particular day.[45] Depending on editors' evaluations regarding the newsworthiness of competing news stories, some events and topics will receive less attention just because something else happened by chance.

We estimate models with *daily news pressure* at different times around a particular shock as the dependent variable. Given the day of a shock t, we examine day t and the six days following the shock (t+1, t+2, ..., t+6), the subsequent time spans [t+7, t+10] and [t+10, t+20] as well as two intervals preceding the shock, [t–10, t–6] and [t–5, t–1]. The coefficients of the shock indicators then display the magnitude of the crowding out effects at the considered times. Table D3 in the Online Appendix shows the OLS regression results. We include year-specific month fixed effects and fixed effects for each day of the week in order to ensure that the estimated crowding out effects are not simply due to seasonal and intra-weekly fluctuations in news coverage. In addition, we control for particularly severe shocks that have led to an excessive crowding out of news (such as the 2011 Fukushima nuclear accident or the 2010 Haiti earthquake). Figure 2 depicts for each type of shock how the respective effects evolve over time. We find significant crowding out effects for all events that happen on US territory, as well as for natural disasters and terrorist attacks outside the US. On their peak days, natural disasters and shooting rampages in the US as well as terrorist attacks both in and outside the US exhibit the strongest crowding out effects (80-120% of a standard deviation), followed by technological disasters on US territory and natural disasters outside the US (40-50% of a standard deviation). Importantly, the relevant reporting time frames seem to depend on the type of event and on whether the US is affected or not, but basically cover the period between the day of the shock event and five days after it. For the case of natural disasters in the US, we already observe crowding out effects before they happen. This is to be expected as most of the natural disasters recorded in the US are storms which are predictable to a certain extent and are typically covered in the news before they turn into a disaster (i.e., the days before a hurricane hits the coast). As we observe no considerable crowding out effects after technological disasters outside the US, we exclude them from further analysis.

Based on the actual crowding out effects following big shock events, we decide to define shock-specific time intervals for the relevant legislative votes. We think this is the most reasonable approach to appropriately distinguish between treatment and control votes (i.e., votes taken under low and high attention, respectively). Using the findings revealed by our analysis, we set the intervals for each type of shock as shown in Table 3. We are finally left with 57 treatment votes in the House of Representatives

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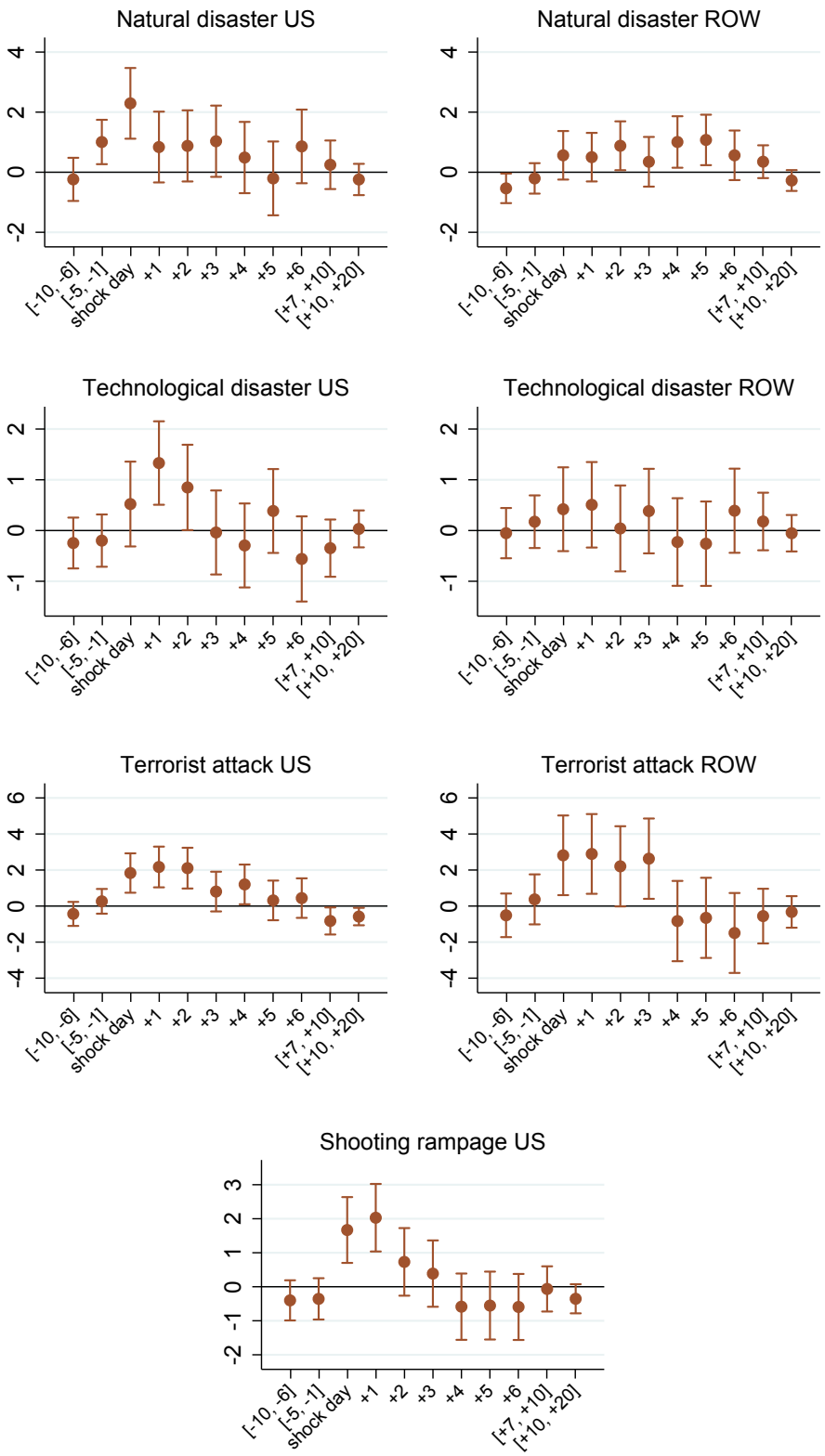
their news pressure measure due to the research question under investigation. In particular, they check the robustness of their main results based on average news pressure by removing all days with reports on natural disasters from the main sample and compute the average news pressure based on this restricted sample. However, in the full data set provided by David Strömberg, all news reports are considered in the measure. Obviously, we rely here on the latter, including news on natural disasters, as the variation in news pressure induced by natural disasters is at the very core of our empirical strategy.

[45] In several validation checks discussed below, we show that the variation captured in the general news pressure measure based on reporting of the major US networks is also reflected in the actual content reported in local newspapers and local television stations.

Figure 2: News pressure following shock events in the United States, 2005-2013



Notes: The graphs show the effects on news pressure around the day of the shock for each type of shock event. The estimates are based on OLS regressions. The dependent variable of *daily news pressure* on different days or intervals around the shock is from Eisensee and Strömberg (2007). Table D3 in the Online Appendix shows the full regression outputs. 95% confidence intervals are included.

out of a total of 425, i.e., votes on the final passage of bills which take place in the relevant reporting time frames after serious shock events. For these votes, the treatment variable $Shock_j$ from our econometric specification (1) takes a value of one (involving 13.5% of the observations in our sample).

Table 3: The relevant reporting time frames following shock events

Type of shock	Region	Time frame	#Votes
Natural disaster	USA	[t+1, t+3]	5
	ROW	[t+2, t+5]	8
Technolog. disaster	USA	[t+1, t+2]	10
Terrorist attack	USA	[t+1, t+4]	14
	ROW	[t+1, t+3]	19
Shooting rampage	USA	[t+1, t+2]	3

Notes: We assign a vote to the shock treatment group (that is, a vote taken under low attention) if it lies in the relevant period with increased *daily news pressure* after a shock at time t (see Table D3). Two votes fall within two time frames.

News reporting on politics and shock events

We critically assess our choice of shock events and the chosen time periods with increased news pressure by investigating to what extent they are actually related to less political news reporting and simultaneously more reporting on the respective shock events. Thereby, we particularly focus on state and local news outlets as these are only partly covered by the general measure of news pressure (Eisensee and Strömberg, 2007). This critical assessment is motivated primarily by two potential concerns regarding our general measure of news pressure. First, it might be the case that voters consume news on the Congress and, in particular, news on their representative primarily through local news outlets. In our main analyses we assume that the pattern captured by our general measure of news pressure is reflected similarly in local news outlets (in television, print, and online) (or at least the response in the national outlets is not counteracted). That is, if there is a crowding out effect in major national television news shows due to a shock event, a similar crowding out effect is assumed in smaller/more local news outlets. With the additional analysis in this section we can validate whether that assumption is actually supported by the data. Second, and in a similar vein, our general measure of news pressure captures the idea of the broadcast time constraint with which television news editors are confronted. The pressure due to limited resources (here particularly time) is thus especially pronounced in the television context but arguably less so in the context of (online) newspapers. Thus, if voters were to obtain news on the Congress primarily from online news sites, it would be questionable whether they would be affected at all by a reduced supply captured by our general measure of news pressure (while they might still shift their priorities in the content they consume). Developing the argument further, if it were the case that there is no measurable crowding out of political news in (online) newspapers and a large share of the voters

consumed news from such outlets, there might even be a potential extensive margin effect of shocks leading not to fewer but to more voters being informed about politics (more people following the news due to the shock event and thereby also reading/hearing about legislative politics).[46]

In order to address these two concerns, we construct a data set on the coverage of national politics as well as shock events in local television and newspapers as follows. We collect closed captions data of US television news shows of 51 state and local television stations from the Internet Archive's TV News Archive.[47] Similarly, we collect data on the coverage of national politics and shock events by local newspapers from Media Cloud.[48] For both data sets, we measure political and shock coverage with the percentage of articles per day mentioning the corresponding keywords (either related to politics or shock events). In the case of political news, we subtract all news reports that are likely to deal with politics related to the shock in question (i.e., reports in the context of the shock mentioning shock-related keywords *and* politics-related keywords). The reason for adjusting the measure is that shocks can obviously trigger a political response and we do not expect fewer news reports about, for example, the US government's response to terrorism after a terrorist attack. We then regress the coverage of politics and the respective shocks on the shock event indicators used in our main analysis.

In brief, we find that, overall, reporting on national politics decreases right after shock events, while reporting on the topic of the respective shock events increases. A summary of these additional results is given in Table 6 (Section 5.2). The pattern is thus clearly consistent with the idea that 'newsworthy' tragedies crowd out political news across media types and media outlets and thus distract from the political process. Given the documented pattern, we think that extensive margin effects are not a first order concern for our approach. With the documented crowding out effects it seems rather unlikely that shocks can lead to more rather than fewer citizens being informed about legislative politics. A detailed description of the data set, the compilation of the variables and the results are presented in Section C of the Online Appendix (see, in particular, Tables C1/C2).

The additional insights gained in this analysis are further employed in the robustness part of the paper (see Section 5.2). Specifically, we estimate models in which we only use those shocks as distracting events after which a statistically significant displacement of political news is detected. For the main part of the paper, however, we continue with our choice of shocks and the relevant reporting periods therafter, as described above and specified in Table 3. Given that the news coverage estimates only allow us to

[46]While a valid argument, we see it as a second order concern given that television is still the most important source of information for a majority of people in the United States. In 2018, 49% of US adults surveyed said that they often get news from television (compared to 33% from news websites, which rank second) (see https://www.pewresearch.org/fact-tank/2018/12/10/social-media-outpaces-print-newspapers-in-the-u-s-as-a-news-source). This argument holds even more for the observation period of our main sample.

[47]We use the GDELT Television Explorer as a search interface for the TV News Archive. Specifically, we capture the relative frequency with which specific keywords (either related to politics or shock events) are mentioned within 15-second segments on television news shows on a daily basis.

[48]Media Cloud is an open-source platform to track online news feeds provided by the MIT Center for Civic Media and the Berkman Klein Center for Internet & Society at Harvard University. We use programmatic access to their database via the Media Cloud API.

measure the extensive margin of political reporting, we believe that our strategy based on the general measure of news pressure (Eisensee and Strömberg, 2007) is the most reliable approach to approximate the truly distracting effect of shocks.

The content of the bills voted on after shocks

In order to address potential endogeneity concerns, we examine whether the bills voted on after shocks are related to the shocks in terms of content. For example, think of a vote on gun control after a mass shooting has taken place. This would be problematic (even if the vote was scheduled long before the shooting took place) for at least three reasons. First, we would expect not less, but more news reports about such a bill, so the distracting effect of the shock is at least questionable. Second, we cannot rule out that the shock may also directly affect representatives' voting decisions in such a case, and third, the shooting may also affect the positions that voters and special interest donors have regarding gun control legislation.

To make sure that there is no contextual connection between the bills voted on after shocks in our sample and the shock events, we systematically check the content of the bills that fall into our shock treatment period. We both qualitatively and quantitatively analyze the respective bill's content. For the latter, we rely on two different bill classifications, the official one from Congress.gov and another one from Voteview.com. Congress.gov assigns legislative subjects, organizations and geographical entities to each bill, of which there are about 1,000 in total (new categories have been added over time). The classification of Voteview.com (Lewis et al., 2018) assigns each bill to one or more of 109 different legislative issues.[49] For each of our 57 shock treatment votes, we check whether the bill in question is assigned to one or more categories that indicate a possible connection to the preceding shock. Table D9 in the Online Appendix shows the corresponding bill subjects/issues that we consider relevant to each type of shock. For example, any bill voted on after a terrorist attack that (partially) addresses immigration, military, or public safety issues is included in our selection. This way we can identify 14 votes with a connection between shock and bill based on the categorization of Congress.gov, and two votes where a link based on the bill topics of Voteview.com seems plausible (both of the two latter cases also appear with the first approach). For these 14 votes/bills (all votes refer to different bills), our assumption of exogeneity may not be (fully) satisfied.[50] In order to make sure that our baseline estimates are not biased due to this potential endogeneity concern, we run robustness checks where we exclude the corresponding bills from the sample (see Section 5.3/Online Appendix Table D7). The results remain similar both quantitatively and qualitatively.

4 Main results

Based on the data compiled, we are able to answer five questions – 1) how representatives voted on bills, 2) how much money they got from interest groups interested in these bills, (3) how much electoral

[49]For an overview of the respective categories, see https://www.congress.gov/browse/legislative-subject-terms/ and https://voteview.com/articles/issue_codes.

[50]The 14 bills are divided among the shock types as follows: three after natural disasters, one after technological disasters, nine after terrorist attacks, and one after shooting rampages.

pressure they faced regarding these bills, 4) whether there was a conflict of the type interest groups versus voters and, 5) whether there was a serious shock event prior to the vote, strongly attracting the attention of media producers – allowing us to test our main hypothesis by estimation of model equation (1). Table 5 shows the OLS regression results for the different specifications. These range from the simplest one without any interaction term in column (1) to the most elaborate specification that includes the triple interactions in column (5).[51] Descriptive statistics for all variables that we use in our empirical analysis are presented in Table 4.

Table 4: Descriptive statistics for the main variables

Variable	Mean	Std. Dev.	Min.	Max.	N
Vote Yes	77.88	41.51	0	100	175,223
SIG Money Yes (net)	0.872	3.661	-44.97	107.6	175,223
SIG Money Yes (abs.)	1.237	3.530	0	107.9	175,223
SIG Money No (abs.)	0.365	1.460	0	44.97	175,223
Constituency Yes (net)	0.823	3.847	-101.7	224.9	175,223
Constituency Yes (abs.)	1.342	3.638	0	227.4	175,223
Constituency No (abs.)	0.519	2.039	0	111.5	175,223
Conflict	0.080	0.271	0	1	175,223
Shock	0.135	0.342	0	1	175,223
Shock x Conflict	0.014	0.117	0	1	175,223

Notes: The money variables are in $10,000 units; the constituency measures are expressed in the unit of 100 campaign donors. *Conflict* takes a value of one if *SIG Money Yes (net)* > 0 and *Constituency Yes (net)* < 0 or vice versa. The unit of observation is representative-vote.

The reduced model in column (1) reveals that both campaign contributions from interest groups favoring the passage of a bill as well as the support of constituents in favor of a bill increase the probability of a representative voting Yes. The coefficient estimated for special interests indicates that an additional $37,000 of campaign donations (about one standard deviation) is associated with a 4.8 percentage point higher probability that the benefiting representative votes in favor of the position preferred by the donating interest groups, ceteris paribus. This effect size is comparable to that associated with a three standard deviation change in the measure for district interests. In column (2), we replace the net money and district measures by the underlying gross numbers in absolute terms. Instead of calculating net support regarding a particular bill, we thus use the total amounts of campaign money in favor of and against it. Likewise, voters' preferences are included based on the number of politically active citizens (i.e., campaign donors) supporting and opposing the bill. The results suggest that there might be larger effects on voting behavior if support comes from special interests opposing a bill.[52] A possible explanation for this finding is that with the vote on final passage of the bill we observe the

[51] Note that we also estimate a probit model which yields similar results in terms of magnitude, sign and statistical significance. The estimates are presented in column (5) of Table D7 in the Online Appendix.

[52] To shed additional light on the possible differential effects of moneyed interests on representatives' voting decisions, we estimate a model with dummy variables for different intervals of money in favor of and against particular bills (see Online Appendix D.3). Figure D1 in the Online Appendix shows the estimated coefficients for these dummies (the reference category is no money at all for a specific vote). We find that pressure against bills in each range is associated with a stronger reaction of the representatives than the same pressure in favor of bills. The point estimates are 60-120% larger.

last step in the legislative process. For the supporters, representatives have probably already invested (successfully) to bring the bill to this stage and provided a sufficient quid pro quo. We proceed using the net variables.

In column (3), we add the interaction terms with our indicator for low attention. If a vote is taken after a serious shock event, neither the effect of special interest money, nor the effect of constituent interests changes significantly. Thus, there does not seem to be a general effect of low attention on the voting behavior of representatives with regard to constituents or special interests.[53] The next model in column (4) reveals that conflicted representatives (i.e., their donors and voters disagree) follow strong preferences of their donors with greater likelihood but no longer follow strong preferences of their constituents when voting on legislative proposals. Quantitatively, the marginal effect of money from special interests increases by 150%. An additional $37,000 of campaign donations now increases the probability of supporting the position of special interests by 9.8 percentage points. In contrast, the impact of voter interests decreases compared to a situation without conflict and is statistically not different from zero.

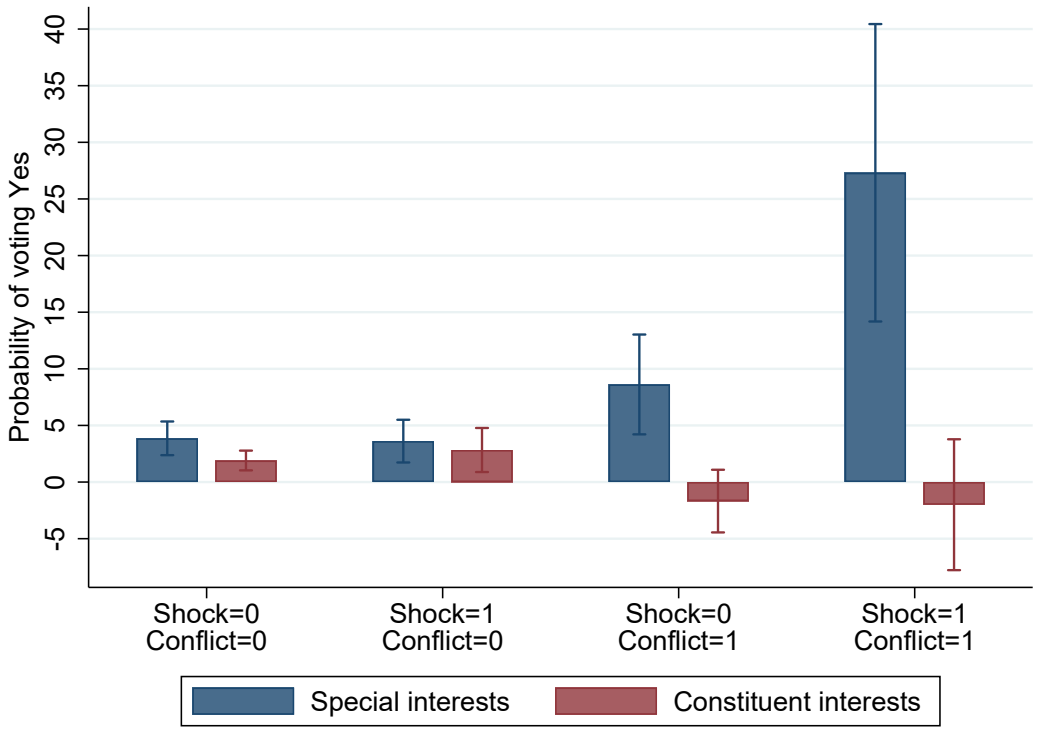
Finally, the model in column (5) includes all simple interaction terms along with the triple interactions. When constituency and interest group positions diverge *and* if the vote takes place right after a shock event reducing media attention, money from special interests is even more effective in affecting a representative's voting behavior. For a net difference of one standard deviation (roughly $37,000), the probability of voting Yes increases by an additional 18.9 percentage points.

Figure 3 summarizes the results captured in the specification presented in column (5) and shows the effects of special and constituent interests on representatives' voting decisions for all four possible constellations. Each bar depicts the effect of a one standard deviation change in the special and constituent interests measure, respectively, on the probability that the corresponding representative will vote in their interests. If special interests and the majority of voters share the same position regarding a particular bill in the first constellation, greater support from both sides is related to a higher probability that the representative is also supporting the bill. For special interests this effect amounts to 3.9 percentage points, for constituents' interests the corresponding effect amounts to 1.9 percentage points. If representatives decide after a shock event, the correlations between special/constituent interests and representatives' voting behavior do not change significantly. This is still for a constellation without a clear conflict. If there is a conflict, i.e., in case of the third and forth constellation, money from special interests turns out to make an even bigger difference in predicting roll call voting. During periods of normal attention, one standard deviation (about $37,000) more money from special interests is associated with an 8.7 percentage point higher probability that the representative will take on the donors' position. In the situation where a representative faces a conflict of interest *and* the vote is taken after a shock event, the same amount increases the probability of a representative voting with the donors' position

[53] One might argue that political disagreements over an issue become less salient among voters and representatives in the face of tragedy and representatives then vote differently due to the change in salience. Such a conjecture would suggest that shock events affect voting behavior independent of a conflict of interest. Our results do not support this conjecture.

by 27.6 percentage points.[54] [55] In contrast, under this latter condition voters' policy preferences have no significant explanatory power for their representatives' roll call decisions, i.e. stronger support from constituents does not translate into a higher probability of representatives' voting in favor of the bill.

Figure 3: The effects of special and constituent interests on representatives' voting behavior in the US House of Representatives, 2005-2014



Notes: The graph illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious shock events and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill). Each bar shows the effect of a one standard deviation change in the special or constituent interests variable on representatives' voting decisions. The underlying results are taken from column (5) in Table 5. 95% confidence intervals are included.

[54] It is important to note here that the number of votes and representatives for whom our indicators of shock and conflict are both turned on is by no means limited to a handful. We document a total of 2,418 observations (or 1.4% of the sample), i.e., representative-vote pairs where the respective representatives face a conflict of interest regarding a particular bill and where these representatives vote on that bill after a distracting shock event. More specifically, the situation of both shock and conflict involves a total of 560 different representatives in 26 different legislative votes. In other words, we observe 560 representatives voting at least once after a shock on a bill for which they face a conflict of interest, and 26 votes taking place after shocks where at least one representative is in a conflict situation (25 bills decided on 22 different days). In sum, the within-variation we use to identify the effect of both shock and conflict is therefore not driven by only a few representatives and votes.

[55] As we rely on donations to approximate the issue-specific exchange relation between special interests and representatives, we cannot directly extrapolate this finding to other forms of exchange relationships or to those representatives who do not have such ties according to our measure. In other words, it is fair to say that our results reflect the media attention effect on 'persuadable' representatives who rely on donations from special interests.

Table 5: Attention and interest representation in roll call voting
in the US House of Representatives, 2005-2014

Dependent variable: *Vote Yes* [0/100]	(1)	(2)	(3)	(4)	(5)
SIG Money Yes (net)	1.304***		1.305***	1.041***	1.055***
	(0.212)		(0.231)	(0.184)	(0.207)
SIG Money Yes (abs.)		0.747***			
		(0.153)			
SIG Money No (abs.)		-3.850***			
		(0.681)			
SIG Money Yes x Shock			-0.010		-0.067
			(0.426)		(0.306)
SIG Money Yes x Conflict				1.600**	1.300**
				(0.620)	(0.583)
SIG Money Yes x Shock x Conflict					5.173***
					(1.905)
Constituency Yes (net)	0.410***		0.390***	0.518***	0.495***
	(0.099)		(0.100)	(0.115)	(0.116)
Constituency Yes (abs.)		0.278***			
		(0.095)			
Constituency No (abs.)		-0.787***			
		(0.203)			
Constituency Yes x Shock			0.208		0.241
			(0.231)		(0.248)
Constituency Yes x Conflict				-0.958**	-0.932**
				(0.391)	(0.401)
Constituency Yes x Shock x Conflict					-0.324
					(0.925)
Conflict				-6.837***	-6.625***
				(2.098)	(2.056)
Representative x Party-of-Sponsor FE	X	X	X	X	X
Vote FE	X	X	X	X	X
Observations	175,223	175,223	175,223	175,223	175,223
Adjusted R^2	0.469	0.476	0.469	0.473	0.473

Notes: OLS regressions with robust standard errors two-way clustered by representative and vote in parentheses. The unit of observation is representative-vote. See Table D6 in the Online Appendix for a series of variations of our main specification (5) differing in the fixed effects used. In column (5) of Table D7 we present a probit estimation of the latter. * p<0.1, ** p<0.05, *** p<0.01.

5 Robustness

We test the robustness of our main results in several ways. First, we propose alternative codings for both the special and constituent interests measure (*SIG Money Yes* and *Constituency Yes*). Second, we restrict the selection of relevant shocks to those after which we document an actual crowding out of political news in local television and newspapers. Third, we estimate a specification excluding those votes where the bills in question are (potentially) related to the previous shocks in terms of content (and the assumption of exogeneity might therefore be violated). Fourth, we vary the level of intensity in our measures for *Shock* and *Conflict* by contrasting strong versus moderate shock activity and separating a clear conflict situation from a situation where no conflict is likely. Fifth, we explore whether effects are observed beyond the relevant reporting time frame we have defined. Finally, we perform a placebo test in which we randomly assign the legislative votes to the shock treatment group, instead of assigning them according to the occurrence of big, distracting events.

5.1 Alternative codings for special and constituent interests

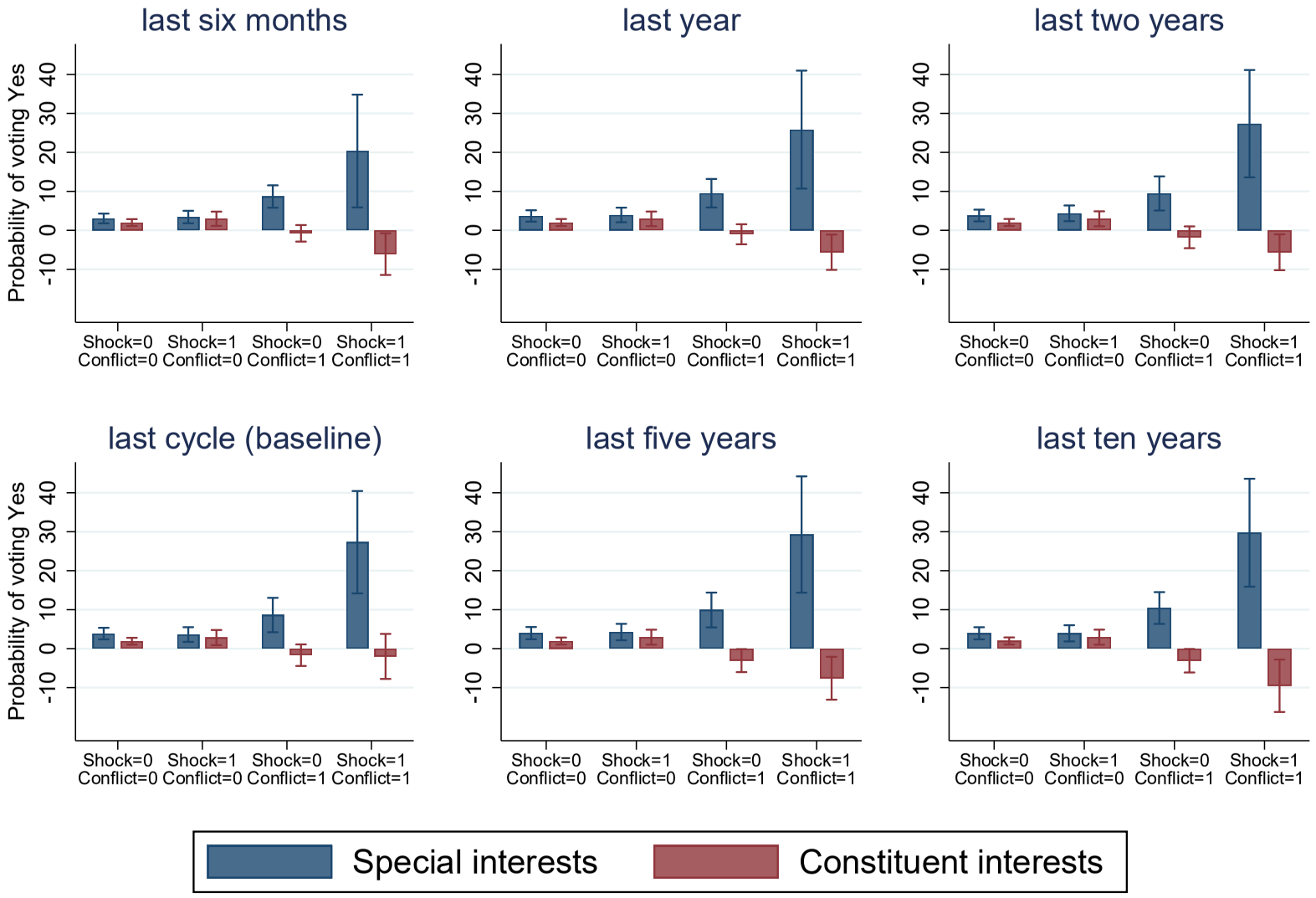
Aggregating special interest money over different time periods

With our special interests measure (*SIG Money Yes*) we want to capture a representative's long-term exchange relationship with organized interests. As the choice of a time frame to aggregate donations in order to capture long-term exchange relationships is to some extent arbitrary, we test the sensitivity of our results with regard to the choice of the time interval within which we count all campaign contributions a representative receives in the run-up to a vote. In our baseline specification, we use the last election cycle. In addition, we now also consider the last six months, the last year (excluding the month of the vote), the last two years, the last five years and the last ten years, respectively, when constructing *SIG Money Yes*. Figure 4 depicts for each aggregation (analogous to Figure 3) the differential effects of special and constituent interests on representatives' voting behavior, depending on whether a representative is conflicted and/or on whether the vote is taken after serious shock events.[56]

Regarding the magnitude of the effects of money from special interests (as well as the effects of the constituency) for changes by one standard deviation, they barely differ across the various aggregations. They all show that when a representative faces a conflict of interest and the vote takes place under low attention, money from special interests strongly affects voting behavior. These results suggest that the long-term relationships are well reflected in the money flows within the last election cycle (or even a shorter time period) and that they are honored over fairly long time periods. In fact, the estimated effect of a one standard deviation increase in the (net) financial support from special interests is largest when money flows over a ten year period are considered.

[56]Based on the newly calculated variables for money flows from special interests, we also adjust our conflict indicator, i.e., a representative faces a conflict of interest if, for example, within the last year or the last six months (depending on the chosen time interval) she received a positive net amount of campaign donations from groups supporting the bill and at the same time the electorate is against the bill in net terms (or the other way around).

Figure 4: Robustness – Applying different time frames with respect to the special interests variable
Campaign donations are aggregated over the ...



Notes: The graphs illustrate the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious shock events and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill). Each specification applies a different time interval for the money flows a representative received in the run-up to a vote from groups that have documented positions regarding the bill. The effects were calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included.

Aggregating individual campaign funds from constituents

As an alternative approach to capturing the electorate's preferences with regard to specific bills (*Constituency Yes*), we aggregate all the campaign donations that were made by individuals in the representative's district with links to groups affected by a particular bill ($-amount of net support), instead of using the weighted number of donations (taking into account that the same individual might donate several times) as in our baseline specification.[57] The argument is that wealthier donors contribute larger amounts of money and may have generally more influence, since they are the more attentive voters and may be more willing to withdraw support from representatives who deviate from their preferences. Wealthy donors may also convince others to stand up for or withdraw support from the incumbent representative.[58] Figure 5 shows the corresponding results if we use the alternative measure for voters' preferences (top right). In addition, we also estimate a specification where we divide the $-amount of net support regarding the bill in question by the total amount of donations from the constituency (regardless of whether or not the donors are affected by the bill). We do the same with our baseline measure, i.e., here we divide by the total number of donations from the constituency. The idea behind this normalization is to rule out any differences on the basis of politically active citizens among the electorate from driving our results. We do not discern systematically different results when more weight is given to the potential influence of wealthy donors, or if we normalize the absolute number and $-amount of net support in favor of a particular bill using the respective totals. Voters' preferences are similarly disregarded in the case of a conflict with the preferences of special interests. In fact, given conflict and shock, the effect sizes for a standard deviation change in the explanatory variables lie within the confidence intervals of the baseline estimates.

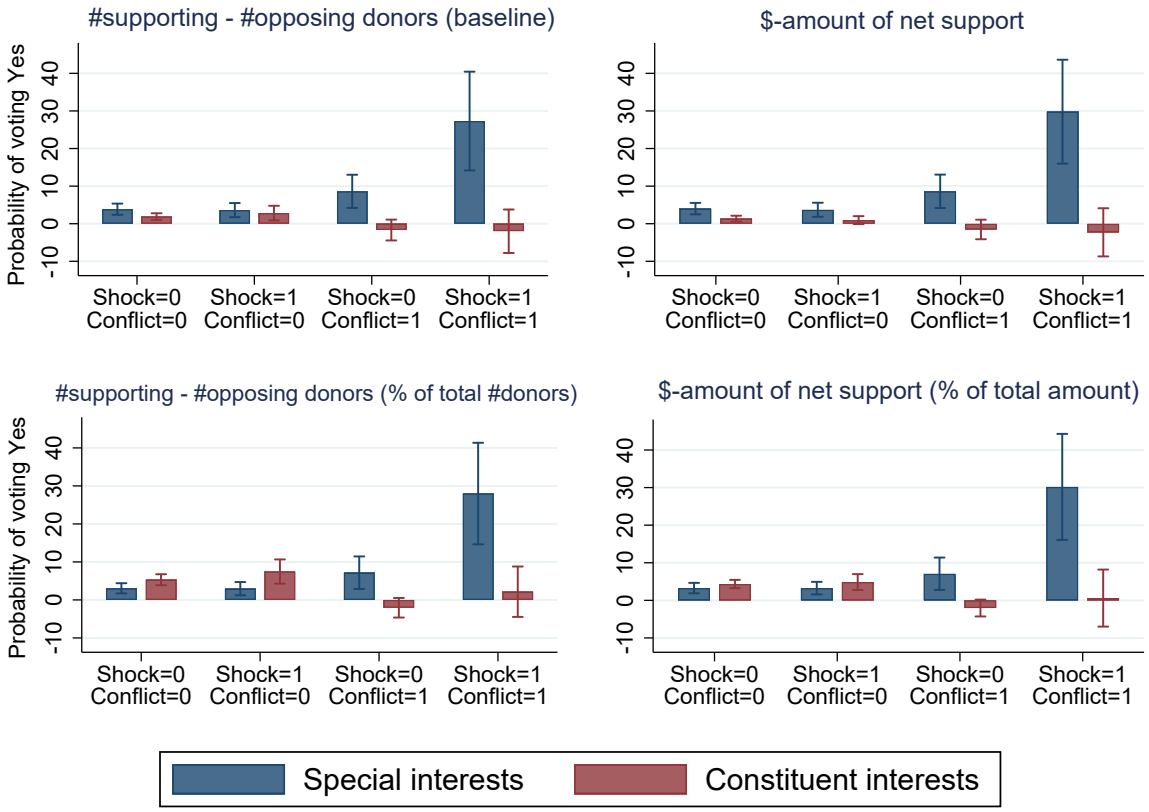
5.2 Alternative selection of shocks based on actual political reporting

The news coverage estimates in Online Appendix C indicate that not all the shocks that we study (and so far consider relevant) seem to similarly crowd out political news across media. This may weaken the validity of our assumption that the votes thereafter are cast with less attention to congressional politics. As summarized in Table 6 below (the related estimates are presented in Online Appendix Tables C1/C2), we detect, for example, no statistically significant displacement or even an increase in reporting about politics after terrorist attacks outside the US (and in newspapers also related to technical disasters in the US). Accordingly, we define two additional shock treatment indicators ($Shock_j$ with j denoting the vote) which only comprise those shocks which lead to a statistically significant crowding out of political news, depending on the outlet studied (see Table 6). With the alternative definitions for $Shock_j$, we

[57] We do not exclude refunds here (as we do in our baseline measure, see Online Appendix Table A1), since it is important to consider the net amount actually donated. In the case of donations to presidential candidates, we consider (as in the baseline measure) the structure of donations in the two years before the last presidential election (instead of the last congressional elections).

[58] Note that the individual donations considered are moderate sums. In US federal elections, there is a limit to the maximum amount that a citizen can donate to candidates or PACs within an election cycle. In the 2013-14 election cycle, an individual was allowed to contribute a maximum of $2,600 to federal candidates throughout the election cycle, and up to $5,000 to PACs and $32,400 to national party committees per year (https://transition.fec.gov/info/contriblimitschart1314.pdf). In the underlying data, the median donation amounts to $500, the 90th percentile is $1,500, with only one percent of donations greater than $2,600.

Figure 5: Robustness – Using alternative measures for constituent interests



Notes: The figure illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious shock events and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill). Each specification applies a different measure for constituent interests. The two above are based on the absolute number/amount of donations from citizens affected by the bill. Below, these are set in relation to the total number/amount of donations made by citizens of the district. The effects were calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included.

re-estimate our model equation (1), also excluding in each case from the sample those votes taken after shocks that involve increased political coverage in the corresponding outlet. These tests therefore clearly distinguish between those votes held with little media attention – based on effectively displaced news segments on television and in newspapers – and those that took place under normal media attention. For example, in the case of television, we use all events except worldwide terrorist attacks for our shock treatment, whereby we also exclude from the control group those votes held after worldwide terror. The corresponding results are presented in Online Appendix Table D7, columns (2) and (3).

We find no substantial differences in the estimated effects when we define our shock treatment votes based on effectively crowded out news. If anything, for situations in which voters and donors are at odds regarding the bill in question, a larger influence of special interest money is estimated when the vote takes place after shock events that displace political news on television. A standard deviation more campaign money in favor of the bill (about $37,000) is in such a situation associated with a 37.3 percentage points higher probability that the representative will vote Yes (compared to the 27.6 percentage points from the

Table 6: Displacement of political news after shocks (summary)

	Television	Newspapers
News category	National Politics	National Politics
Natural disaster US	−	−
Natural disaster ROW	−	−
Techn. disaster US	−	+
Terrorist attack US	−	−
Terrorist attack ROW	+	not sign.
Shooting rampage US	−	−

Notes: Minus signs indicate a significant (p-value < 0.1) displacement of news of the examined category after the respective shock in the relevant period with increased news pressure (plus signs indicate more coverage of politics). See Table 3 for the relevant reporting periods after each shock (depending on the type of shock) as well as Tables C1 and C2 for the related news coverage estimates. National political news is approximated by news segments mentioning "Congress", "White House" or "Federal Government". A comprehensive description of the data used and their compilation can be found in Online Appendix C.

baseline estimate). We therefore conclude that the shocks selected on the basis of a general measure of news pressure (Eisensee and Strömberg, 2007) fairly well approximate periods of limited voter attention to congressional lawmaking.

5.3 Excluding votes contextually related to shock events

Based on a rather general content analysis of the legislative bills voted on in our shock periods referring to broad topical categories, we have identified 14 votes where the assumption of exogenity between shock and bill content might be violated (see Section 3.4). We have further evaluated the texts of all of these bills, checking for a plausible connection between shock and bill. We have only found one shock (involving two votes/bills) where a violation of exogeneity seems plausible.[59] In order to check whether our results change substantially if we only consider those shocks and votes where we can exclude a thematic link with high probability, we estimate our model in equation (1) excluding the relevant votes.[60] The corresponding results are presented in column (4) of Online Appendix Table D7. We document a not significantly smaller impact of campaign money on the roll-call decisions of representatives when the positions of the special interest donors collide with those of the electorate. One standard deviation more campaign support in favor of a bill (around $37,000) is now associated with a 25.5 percentage point higher probability that the representative will vote Yes (versus the 27.6 percentage points from the baseline estimate as shown in Figure 3).

[59]The two bills involve the *Cybersecurity Enhancement Act of 2013* as well as the *Advancing America's Networking and Information Technology Research and Development Act of 2013* (H.R. 756 / H.R. 967 – 113th Congress). They were passed by the House one day after the Boston Marathon bombing (April 15, 2013) and addressed the strengthening of cybersecurity and the associated protection of information systems against attacks from outside.

[60]See Table D9 for the list of relevant bill topics that we use to identify potential links between bills and shocks.

5.4 Shock and conflict intensity

We examine whether our main findings are robust to (and qualitatively consistent with what we would expect under) different treatment intensities regarding shock and conflict. In particular, we test 1) whether our results are indeed driven by the legislative votes which took place after the events that crowd out most news, and 2) whether the clearest conflict situations according to our measures for special and constituent interests also exhibit the most pronounced effects.

Shock intensity

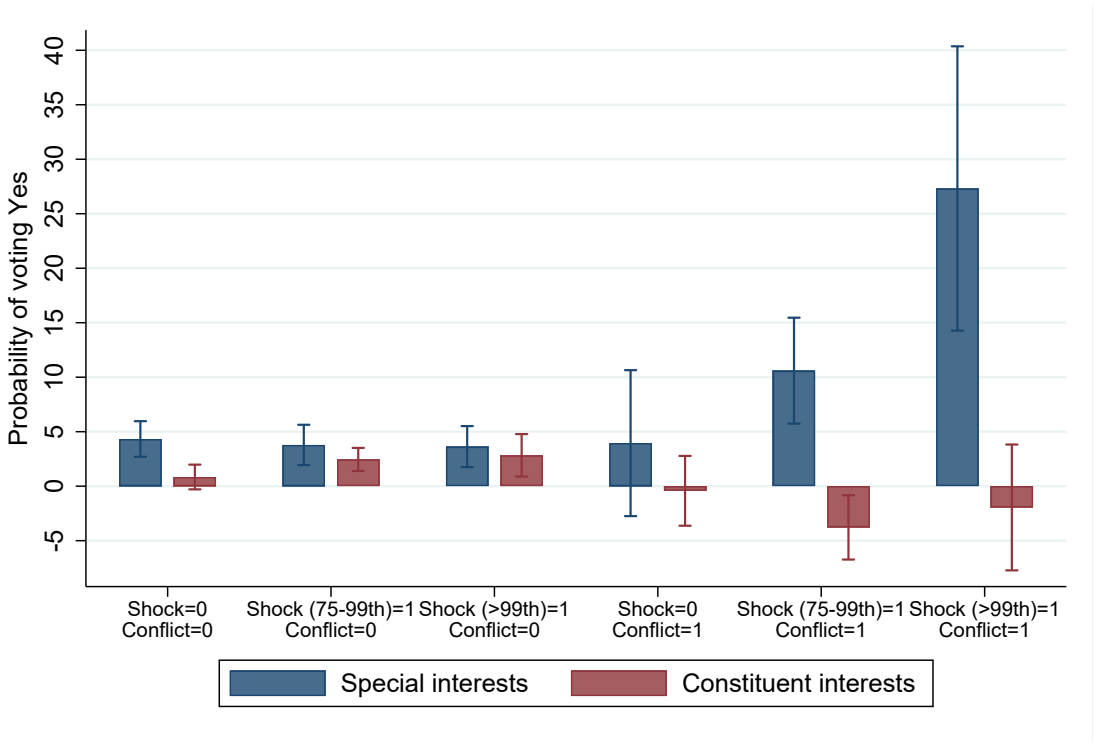
In order to take into account a broader set of shock events, we select events with less severe outcomes than the ones in our main analysis. This weak shock treatment group contains all the votes after days on which the number of deaths caused by a particular event type lies between the 75th and 99th percentile (in addition to our baseline shock indicator that only considers votes that took place after the most serious events above the 99th percentiles). Note that for all event types in the US, the number of deaths is zero on more than 99 percent of the days between 2005-2014 (and we do not use distributions for shooting rampages) – see Table 2. Accordingly, the newly-defined shock group, referred to as *Shock (75-99th)*, will only include votes that took place after natural disasters and terrorist attacks in the rest of the world.[61] The constructed additional indicator is one in 59.8% of the observations in our sample, compared to *Shock (>99th)* which is one in 13.5% of all votes. In Figure 6 we present the differential effects of special and constituent interests on representatives' roll call voting decisions under all six possible constellations (the full regression output is reported in Table D8 in the Online Appendix). When representatives face no conflict, we find that the effect of special interest money on voting Yes is rather similar independently of whether there is limited attention to politics. If voters' and special interests' preferences are not aligned, the impact of money increases sharply as observed before, provided that there is at least some distraction due to a less severe shock. Consistent with our theoretical framework, we observe the largest reaction in representatives' voting behavior after the most serious shocks.

Conflict intensity

In order to vary conflict intensity, we distinguish a clear conflict from an ambiguous situation where our measures for special and constituent interests are close to zero (referred to as *Tension*). We define the indicator *Clear Conflict* that takes a value of one if *SIG Money Yes* > 1 and *Constituency Yes* < -1, or *SIG Money Yes* < -1 and *Constituency Yes* > 1 (i.e., if special interests that favor the bill donated at least \$10,000 more in net terms and at least 100 donating individuals more in the constituency oppose the bill, or vice versa). This definition of a clear conflict captures by construction cases i) that involve more donors from the constituency and ii) for which the policy position of the constituency is more clear cut. The cases labelled as clear conflicts are therefore arguably also the more 'relevant' votes for the representatives who care about direct electoral support. *Tension* is equal to one if either *SIG Money Yes* or *Constituency Yes* lies within -1 and 1. The control group therefore consists of cases where no conflict is

[61] For natural disasters in the rest of the world, the number of deaths is zero on 83.4% of the days (thus we only consider days between the 83.4th and 99th percentile here); the threshold for terrorist attacks in the rest of the world is 29 deaths (75.1%).

Figure 6: Robustness – Shock intensity

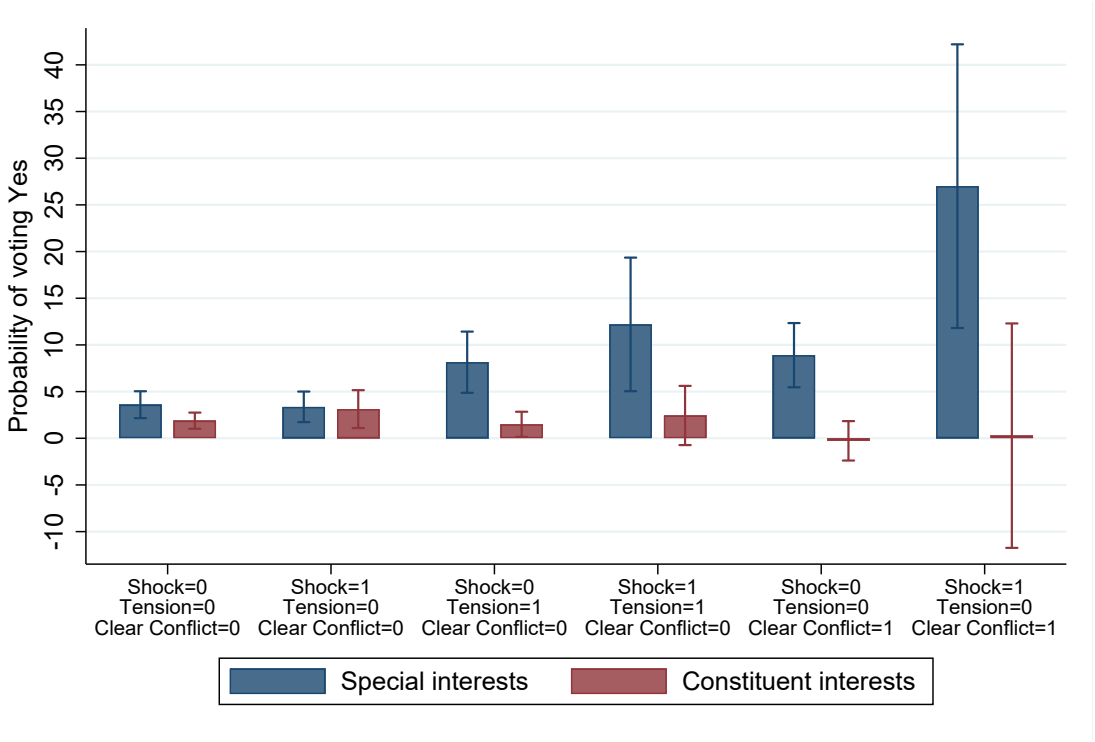


Notes: The graph illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious (>99th) or moderate (75-99th) shock events and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill). The effects were calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included. The full regression output is presented in the Online Appendix in Table D8, column (2).

likely. In addition, we assign cases where both *SIG Money Yes* and *Constituency Yes* take values of zero (mostly because we cannot assign any donating group and individual with preferences) to the control group.

With these newly defined indicator variables, we re-estimate our linear model. Figure 7 shows the effects of campaign money and voters' interests in all of the possible constellations a representative can face (full regression results are reported in Table D8 in the Online Appendix). When facing some tension rather than no conflict, campaign donations are related to a systematically larger effect on representatives' voting behavior. The predictive power of constituent interests remains unchanged in a tension situation and is completely lost in a situation of clear conflict. Regarding the differing consequences of a shock event, the effect of campaign donations from special interests increases by 200% in cases of clear conflict. In cases of tension, the increase is about 50%. In the aftermath of a shock, the effect of voters is not different from zero both in a tension situation and in a situation with a clear conflict. These findings are in line with the theoretical reasoning that the fundamental trade-off between campaign donors and voters (and thus the relevance of attention) arises particularly if representatives face a clear conflict of interest. The results suggest that our approach approximates the preferences of these two pressure groups and the resulting conflict situations in a consistent and meaningful way.

Figure 7: Robustness – Conflict intensity



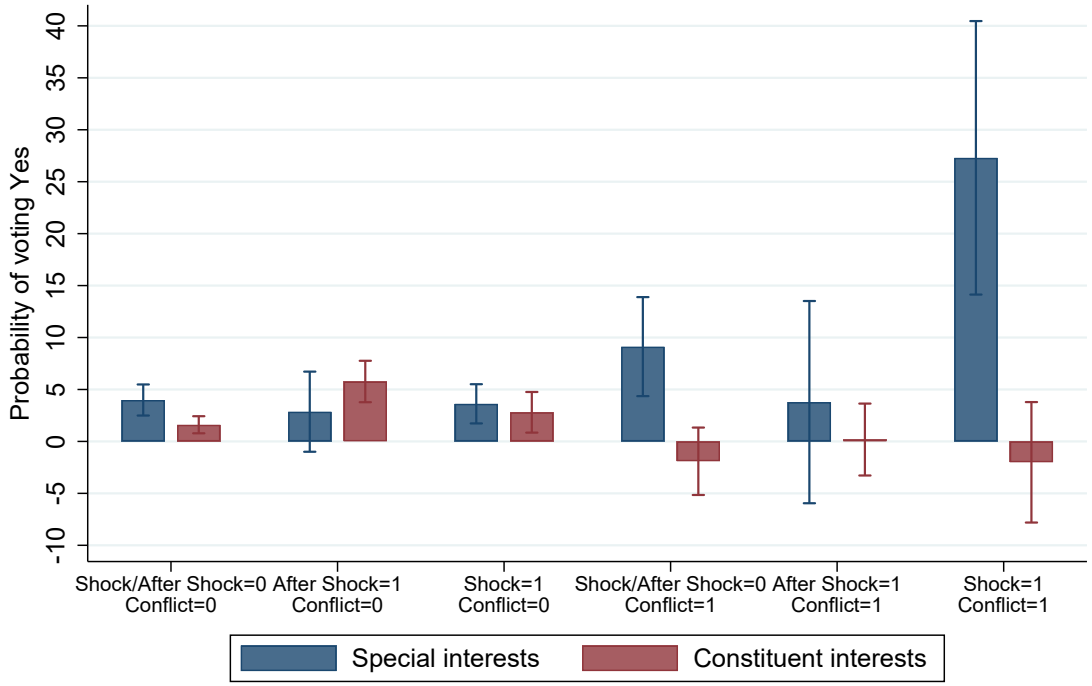
Notes: The graph illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious shock events and on whether the representative faces a clear conflict of interest (i.e., special and constituent interests clearly disagree regarding a particular bill), or an ambiguous situation where our measures special and constituent interests lie close to zero (denoted *Tension*). The effects were calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included. The full regression output is presented in the Online Appendix in Table D8, column (3).

5.5 Shock duration

More severe shock-events tend (as expected) to have a more pronounced effect on the representatives' calculus to vote in line with special interests, as shown above. In a similar vein, the distracting effect of each shock event is expected to fade out. So far, in our specification we even implicitly hypothesized that there would be no effect after the delimited shock period (see also Figure 2/Table D3). We test this by estimating our main specification with an additional treatment indicator, *After Shock*. It takes a value of one if the vote takes place one or two days after the end of the identified main news-crowding period (as defined in Table 3).[62] Figure 8 shows the estimated effects for our main specification. In cases of conflict, special interest money does have a statistically different influence after the relevant news-crowding period from what it had before, both when representatives face a conflict and when they do not. This finding is congruent with our hypothesis and indirectly validates the choice of narrow periods with arguably less attention to politics.

[62] *After Shock* is one in about 8% of the observations in our sample. If the vote falls both on a day within the initially defined news-crowding period of a shock event, and on a day that is one or two days after a relevant earlier shock period, we code *After Shock* with zero.

Figure 8: Robustness – Shock duration



Notes: The graph illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken in the relevant reporting time frame after serious shock events (Table 3) or on one of the two days after a relevant period has ended (*After Shock*), and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill). The effects are calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included. The full regression output is presented in the Online Appendix in Table D5.

5.6 Placebo test

If voting behavior is analyzed for differential effects of campaign money and constituent interests, the same patterns for the effect of limited attention as those reported in our main specification in Table 5 should be observed only rarely if the days considered shock days were randomly assigned. Based on this idea, we undertake a placebo test and randomly distribute the shock treatment days over all days with legislative votes in our sample (excluding real shock days). The number of placebo days is chosen in such a way that it matches the roughly 13% proportion of original treatment days. We perform this random assignment of placebo shock days 1,000 times and estimate our baseline model (1) for each draw.

The distributions of the estimated placebo coefficients are shown in Figure D3 in the Online Appendix. Regarding the triple interaction *SIG Money Yes* $\times$ *Placebo* $\times$ *Conflict*, the empirical p-value is 0.024, i.e., in only 2.4% of the cases is the estimated coefficient larger than the estimated coefficient of 5.173 from our baseline estimation. This thus suggests that the finding that campaign funds from special interests are more influential in the days following major shock events is highly unlikely to have occurred by chance.

6 Mechanism: Agenda-setting vs. individual short-term opportunism

So far, we have implicitly interpreted the observed patterns in voting behavior in terms of individual representatives' short-term opportunism. However, what if majority leaders take advantage of the limited attention caused by shock events and deliberately bring particular bills to the vote during them? On the one hand, the majority leadership might be directly pressured by organized interests to ensure passage of certain bills. On the other, majority party leaders might be aware of the fact that several of their party colleagues face a conflict of interest (special interest groups versus voters) in some upcoming votes. In order to improve their re-election chances, majority leaders would be tempted to time these votes in such a way that conflicted colleagues are less likely to be punished by voters when they vote against their electorate's interests. The institutional setting in the House of Representatives would theoretically allow for a short-term change of the agenda along these lines.[63] The body responsible for such changes is the Rules Committee, which disproportionately comprises members of the majority party, and thus to a substantial degree is under the control of the majority leadership. In particular, it is the Speaker of the House who exercises control over the Rules Committee.[64]

The former Speaker Thomas P. O'Neill (1977-1987) described the role of the Rules Committee as follows: "What makes the Rules Committee so important is that it sets the agenda for the flow of legislation in the House and ensures that the place runs smoothly and doesn't get bogged down."[65] Issues that are highly sensitive to organized interests, but likely to conflict with the public's interest, could thus be affected by strategic short-term agenda-setting through the Rules Committee. We investigate this mediating factor based on two tests.

6.1 Timing of votes with many conflicted representatives

First, based on our theoretical considerations, majority leaders should primarily have an incentive to push the Rules Committee to change the agenda if special interests have strong preferences that a particular piece of legislation be passed when large parts of the electorate are against it – but not the other way round. This follows from the idea that interest groups are very well informed about the voting behavior of the representatives they support, while voters' level of information depends on the availability of political news, which is affected by media producers' judgments as to relative newsworthiness. To test whether

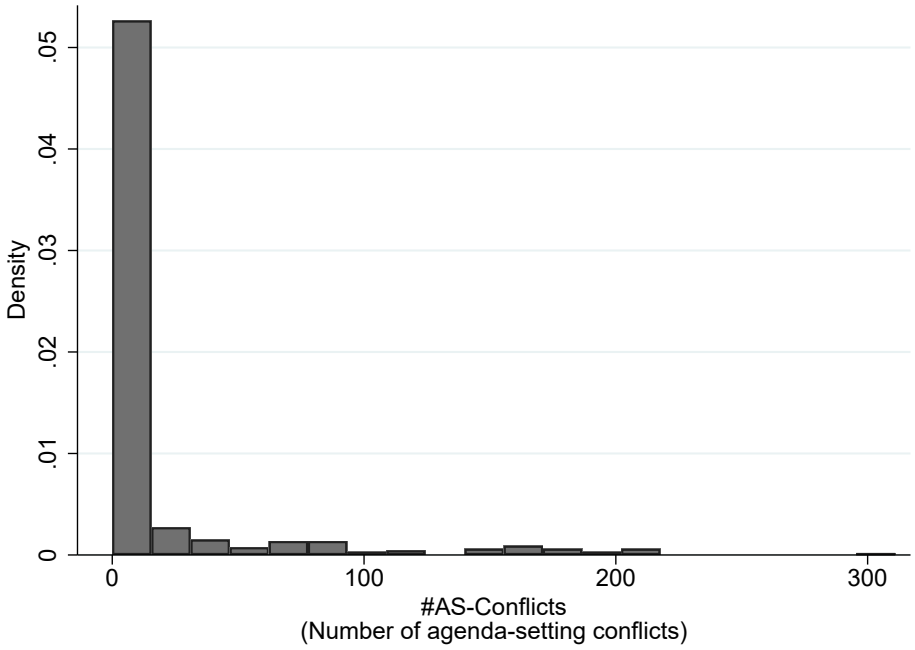
[63] The study of Lindstädt and Vander Wielen (2014) finds evidence consistent with the hypothesis that majority party leaders strategically schedule votes that divide the parties when elections are far off. In their theory, parties want to avoid situations in which representatives face the decision of showing party loyalty or not, due to the electoral costs of party loyalty shortly before the elections. This kind of agenda-setting, however, seems rather long-term, and differs from the short-term change of the agenda after major shock events that we have in mind.

[64] After a bill is introduced in the House of Representatives, it is sent to a Committee and Subcommittee for hearings, recommendations regarding amendments, and reporting. When a bill returns from the Committee, it is usually not sent directly to the House floor. In particular, the Rules Committee schedules when a specific bill comes to the floor for consideration, and sets the rules concerning amendment limitations and the amount of debating time that is allocated to each bill. After a simple majority of the entire House approves the rule, the bill is ready for debate, possible voting on amendments, and final passage voting (https://www.congress.gov/legislative-process and rules.house.gov). It is also possible that the final vote will be taken directly, without a ruling from the Rules Committee ('under suspension of the rules'). In this latter case, debate is limited to 40 minutes and no amendments are possible (requires a 2/3 majority).

[65] Quoted in https://archives-democrats-rules.house.gov/Archives/pre20th_rules.htm#N_4_.

bills with opposition in the constituency but supported by special interests are more likely to be voted on after shock events, we count, for each vote, the number of representatives who face a conflict of the type *special interest groups Yes and voters No* (i.e., *SIG Money Yes* > 0 and *Constituency Yes* < 0), denoted as *#AS-Conflicts* (number of agenda-setting conflicts). We use this variable as well as a 0/1-indicator, taking a value of one if *#AS-Conflicts* is positive, as dependent variables to test the agenda-setting hypothesis with two alternative specifications. Figure 9 depicts the distribution of *#AS-Conflicts* for the 425 votes in our sample. A high number of agenda-setting conflicts can be observed for only a small number of votes, but for about 30% of them, we observe at least one representative who faces an AS-conflict.

Figure 9: The number of conflicted representatives per vote (*#AS-Conflicts*)



Notes: The figure shows the distribution of the vote-specific characteristic *#AS-Conflicts*. The latter captures the number of representatives that face a conflict of type *special interests Yes and voters No* (i.e., *SIG Money Yes* > 0 and *Constituency Yes* < 0). The sample involves 425 votes.

The regression results in Table 7 reveal that, on average, there is no higher number of agenda-setting conflicts (or a higher probability of a positive number of conflicts) for the votes that are taken after shock events. This finding holds if we only use the number of agenda-setting conflicts for representatives affiliated with the majority party (as one might argue that majority leaders care more or are better informed about the conflicts their party colleagues face).

6.2 Elapsed time between first consideration and final passage

As an additional test of the agenda-setting hypothesis, we examine the elapsed time between a bill's first consideration in the House (i.e., after the bill is reported out of committee) and its final passage vote. If

Table 7: Test of a possible agenda-setting mechanism: Number of agenda-setting conflicts

Dependent variable:	#AS-Conflicts	#AS-Conflicts>0	#AS-Conflicts (Majority Party)	#AS-Conflicts>0 (Majority Party)
Shock	4.464	0.091	1.010	0.069
	(6.205)	(0.068)	(4.021)	(0.066)
Observations	425	425	425	425
R^2	0.001	0.000	0.000	0.003

Notes: OLS regressions with robust standard errors in parentheses. *#AS-Conflicts* refers to the number of individual representatives who face an agenda-setting conflict in any given vote. *#AS-Conflicts>0* is a binary indicator taking a value of one if the number of agenda-setting conflicts per vote is positive. The mean values for *#AS-Conflicts*, *#AS-Conflicts>0*, *#AS-Conflicts (Majority Party)* and *#AS-Conflicts>0 (Majority Party)* are 16.6, 0.29, 12.0 and 0.26. The unit of observation is vote. * p<0.1, ** p<0.05, *** p<0.01.

strategic short-term agenda-setting takes place right after shock events, the bills that are decided during the days with limited media attention are expected to reach their final vote faster (on average) than any other bills. Majority leaders may, for example, convince their party colleagues not to use up the time available for the debate or to withhold amendments which would otherwise delay the process. Any effect might, however, be more pronounced for those bills where many representatives face special interests that favor the bill and a constituency that is against the bill. For bills whose consideration by the House was initiated only after the shock event, the Rules Committee may provide a rather restrictive rule, i.e., a rule that limits the debate time and/or the possibilities for amendments. Another way to quickly pass legislation would be to ask for 'suspension of the rules' (requiring a 2/3 majority), which limits debate to 40 minutes and does not allow any amendments to the bill.[66]

For each bill, we count the days between the first consideration on the House floor and the vote on final passage. For the bills that were voted on twice, which concerns 15 bills (mostly because they did not get a majority in the first vote), we count the elapsed days between the renewed consideration and the final vote. In most cases, first consideration (or re-consideration) and final voting are on the same day (the average elapsed time is 0.22 days). The results of a regression analysis in Table 8 show that there are no statistically significant differences in the elapsed time for votes taken after shock events, as can be seen in column (1). Regardless of whether or not there was a shock before the vote, we find evidence in model (2) that a larger number of representatives with special interest donors on the Yes side but voters on the No side are associated with a reduced time span between consideration and final voting. With a baseline probability of 80% that a bill will reach its final vote on the same day that it was first considered, this probability increases by another six percentage points if the number of representatives with agenda-setting conflicts increases by 88 (two standard deviations).[67] This suggests that other (non-shock) events may be exploited to push through legislation that is sensitive to voter interests but

[66]We have also tested whether 'suspension of the rules' is increasingly used for the bills voted on after shocks. The corresponding proportions of votes held under suspension are 52.6% (for those after shocks) and 62.5% for all others (not statistically significantly different).

[67]This effect is taken from a regression in which a dummy variable indicating whether the bill is voted upon the same day is used as the dependent variable.

favored by special interest groups. Finally, column (3) shows that a high number of conflicts combined with distraction by shock events does not seem to be associated with agenda-setting by majority leaders. Contrary to our theoretical prediction, we only find a shorter elapsed time for votes taken after distracting shock events if the number of agenda-setting conflicts is relatively low (statistically significant at the 10% level for votes after a shock exhibiting less than 13 conflicts).

Table 8: Test of a possible agenda-setting mechanism: Elapsed time between a bill's first consideration and final passage vote

Dependent variable: *Elapsed time*	(1)	(2)	(3)
Shock	-0.0758		-0.1191**
	(0.0542)		(0.0599)
#AS-Conflicts		-0.0008**	-0.0011***
		(0.0004)	(0.0004)
Shock x #AS-Conflicts			0.0024
			(0.0015)
Observations	425	425	425
R^2	0.0032	0.0057	0.0145

Notes: OLS regressions with robust standard errors in parentheses. *Elapsed Time* ranges from 0 to 2 days (mean=0.224; SD=0.455), *#AS-Conflicts* from 0 to 311 (mean=16.6; SD=44.1). * p<0.1, ** p<0.05, *** p<0.01.

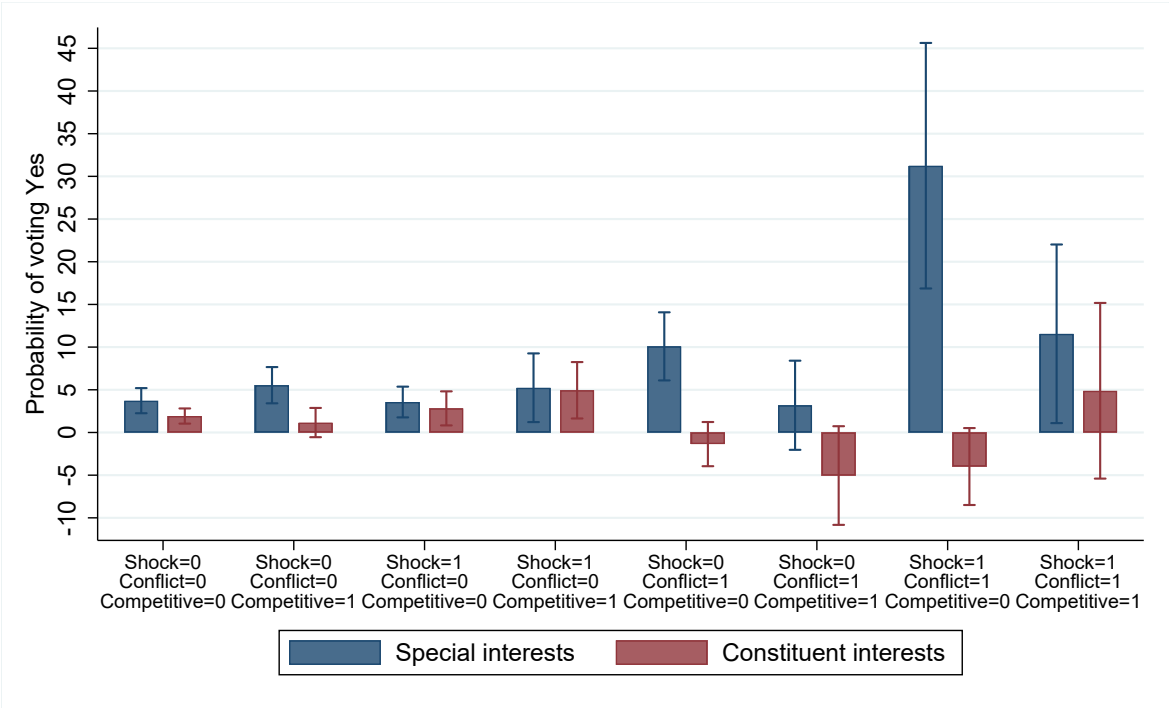
Overall, the results of the two tests speak against the hypothesis that short-term agenda-setting mediates the effect of attention on the influence of special interests. Our main finding for the effect of limited attention on the voting behavior of conflicted representatives instead seems due to individual representatives' short-term adjustment of their voting behavior.

7 Supplementary analysis: interaction with electoral competition and the election cycle

The core aspect of our theoretical framework underlying the hypothesis tested in this paper is the office-motivated incumbent, maximizing her re-election chances. If it is indeed re-election concerns that drive our results, we would expect heterogeneous effects when comparing representatives in situations where re-election concerns are arguably more or less pressing. Both direct support from voters (by voting in line with their interests) and financial support from special interests are increasingly valuable when re-election concerns are more pressing. However, from our basic framework it is theoretically unclear whether representatives will cater more or less to special interests under greater electoral competition. We explore the effect of varying levels of electoral competition by re-running our baseline regression analysis on two different pairs of sub-samples. Specifically, we compare i) representatives in rather contested districts (approximated by a victory margin in the last elections that lies below 5 %) with their colleagues sitting on rather 'safe seats' (margin of victory in the last elections at 5 % or above), and ii) representatives' voting decisions taken in election years versus those taken in non-election years.

Figures 10 and 11 show the results. They come from regressions where we interact all of our explanatory variables from model equation (1) with indicators for electoral competition and whether the vote was held in an election versus a non-election year (taking into account the main effects of the latter, whereby the main effect of the election year is already absorbed by the vote-specific effects).[68]

Figure 10: Differential effects of attention on representatives' voting behavior depending on electoral competition (margin of victory $< 5\%$)



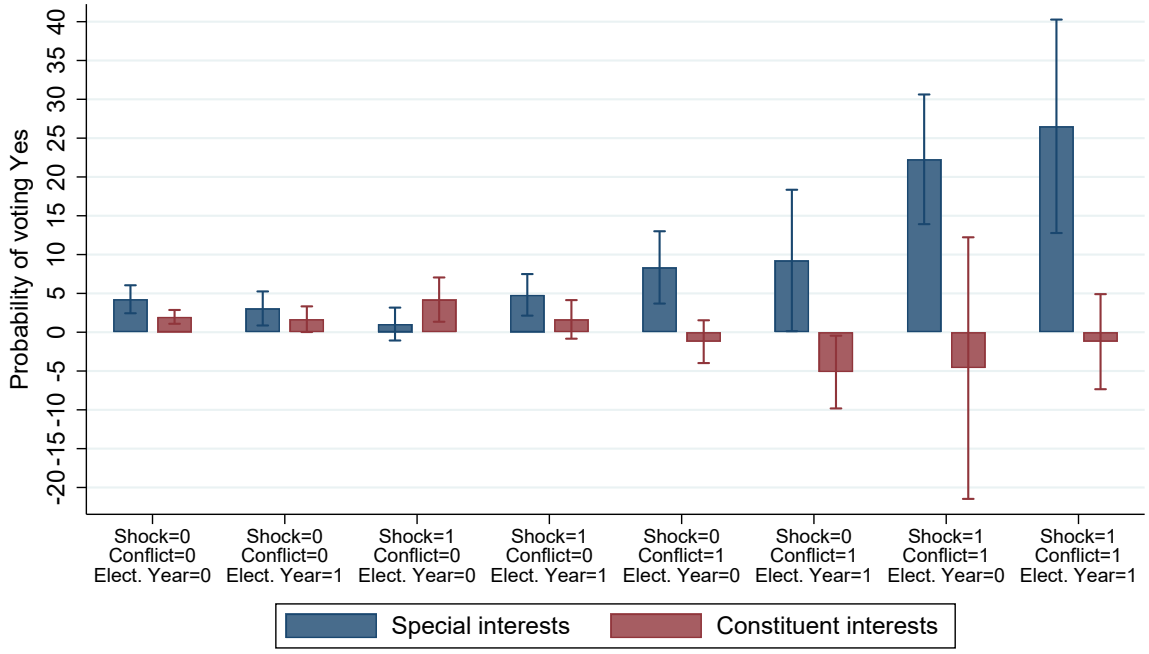
Notes: The figure illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious shock events and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill), and on whether the district of the representative is competitive, that is, if the victory margin in the last elections was less than 5 %. The effects were calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included.

We find that conflicted representatives from highly competitive districts are less likely to adapt their post-shock voting behavior in favor of the special interest donors than those with a relatively safe seat. Notably, we also document a reduced effect of special interests in a conflict situation without shock. This is consistent with the view that electoral competition reduces the influence of special interest donors in situations of conflicting interests with the electorate.

Regarding differential effects across election and non-election years, we observe generally larger point estimates for the effect of donations from special interest groups when their position is in conflict with a legislator's constituency in election years. However, relatively large standard errors do not allow us to reject that the partial correlations are of equal size.

[68] The corresponding regression results are available upon request.

Figure 11: Differential effects of attention on representatives' voting behavior depending on votes taken in election versus non-election years



Notes: The figure illustrates the differential effects of special and constituent interests on representatives' voting behavior, depending on whether the vote is taken after serious shock events and on whether the representative faces a conflict of interest (i.e., special and constituent interests are at odds regarding a particular bill), and on whether the vote is taken in an election year versus a non-election year (assigning votes taken in November and December after the elections to non-election years). The effects were calculated using changes in the explanatory variables by one standard deviation. 95% confidence intervals are included.

8 Concluding remarks

The democratic process is fundamentally about serving citizens in decisions that lend themselves to being taken collectively. While interests of all kinds should be heard in this process, specific interests are often at an advantage, as they manage to become organized and collect money among their supporters. If then, for some policy issues, the interests of specific groups diverge from those of a large part of the population, concerns arise about interest groups having undue influence on policy making at the cost of consumers and taxpayers at large. In this, the representatives' reliance on campaign finance donations for electoral success is one prominent avenue by which special interests can influence politics. However, representatives face a trade-off when relying on financial support from special interests in running campaigns and winning elections in exchange for policy favors, as they may be sanctioned by their constituents if they support policies that run against voters' preferences.

Our study shows that media attention is a crucial factor affecting this trade-off. Representatives are systematically more likely to vote against their electorate's policy preferences but more aligned with those of special interest groups that support them over time when media attention is drawn away from politics due to an exogenous shock event (such as a natural disaster hitting the US). This suggests that special interests can leverage their advantage in monitoring representatives during times of limited media

attention on politics, an issue that has so far not been prominently discussed in the context of special interest politics. Importantly, constituent interests already lose out to special interests – if in conflict with them – when attention is not distracted from politics. In fact in such a situation, the empirical analysis shows that representatives' voting behavior is responsive to the amount of campaign donations from special interests, but not to the intensity of voter preferences.

Our findings open several avenues for further research in this context. First, information asymmetries between different types of (interest) groups in the population might deserve more attention in theoretical work on special interest politics as, mass-based interest groups such as unions probably rely on different information flows than well-funded but comparatively small business interest groups. Second, while we model attention as being uniformly distributed and affected by shock events across representatives, the organizational structure of media markets and its interaction with political markets might well create systematic variation in voters' exposure to political information about their representatives' behavior.

Finally, our findings raise some interesting issues regarding the role of media markets and media control in representative democracies. If attention to politics is an obstacle for special interests to overcome in influencing the political process when their preferences conflict with the desires of large fractions of the population, the value of owning/controlling media outlets wins a new and important aspect. A large part of the new literature at the intersection of media economics and political economics focuses on how the media work as the 'fourth estate', keeping the elected officials in line with the interests of the voters (see, for example, Prat and Strömberg, 2013, and DellaVigna and Gentzkow, 2010, for excellent reviews of the arguments). Complementary literature suggests a different role of the media in democracies, i.e., the role of representing corporate interests in order to secure rents in the democratic process (Herman and Chomsky, 1988, Gilens and Hertzman, 2000, and Corneo, 2006, for example). Taken together, the modus operandi under which many media outlets function – competition for the audience's attention and the necessary focus on newsworthy events – affect their role as the fourth estate and thus the role of special interests in politics.

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Special Interest Groups Versus Voters and the Political Economics of Attention

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A Data on campaign donations

A.1 Data access

The data from the CRP is accessible through its website OpenSecrets.org. We collected the campaign finance data via the Sunlight Foundation's Influence Explorer. The original data set (consisting of federal campaign finance records between 1990 and 2014) is available online under https://sunlightlabs.github.io/datacommons/#bulk-data. We have updated the original data set with subsequent additions of campaign finance data directly provided by the CRP.[1] The MapLight data is accessible via an API under https://maplight.org/data_guide/bill-positions-api. We accessed the data on June 26th, 2016. The GovTrack data is publicly accessible via an API under https://www.govtrack.us/developers.

A.2 Compilation of measure for special interests

As described in the main text, we sum up all the direct PAC donations a representative receives prior to a vote from interest groups supporting and opposing the related bill (*SIG Money Yes* and *SIG Money No*).

Table A1 provides an overview of the transaction types and interest group codes that we exclude before aggregating the donations. Note that we consider refunds when constructing the money variables, i.e., when donations are transferred from a candidate back to the donating PAC. In some cases, this results in a representative returning more money to groups than she received from them. In these cases, we replace the corresponding money variable with zero. Otherwise, we would be looking at a situation in which a representative returns more money to groups which support the bill than she receives from them as pressure to vote against the issue. This affects a total of 440 observations, i.e., 0.25% of the baseline sample.

[1] The original bulk data set provided by the Sunlight Foundation did not yet contain all records for the 2014 cycle, upon publication of the bulk data set. We therefore completed the data set once the respective records were made available by the CRP.

Table A1: Excluded transaction types and interest group codes in the campaign finance data

Special interests measure

- Excluded transaction types: 16c, 20c, 22h (loans to candidates and loan repayments), 18g, 24g (transfers in from and out to affiliated committees), 24e, 24f (independent expenditures and communication costs), 24c (coordinated party expenditures), 29 (electioneering communications)
- Excluded group codes: Y0000, Y2000, Y3000, Y4000 (unknown category, no employer listed or impossible to assign category), Z9000, Z9100, Z9500, Z9600, Z9700, Z9800, Z9999 (non-contributing categories and candidate self-finance)

Constituent interests measure

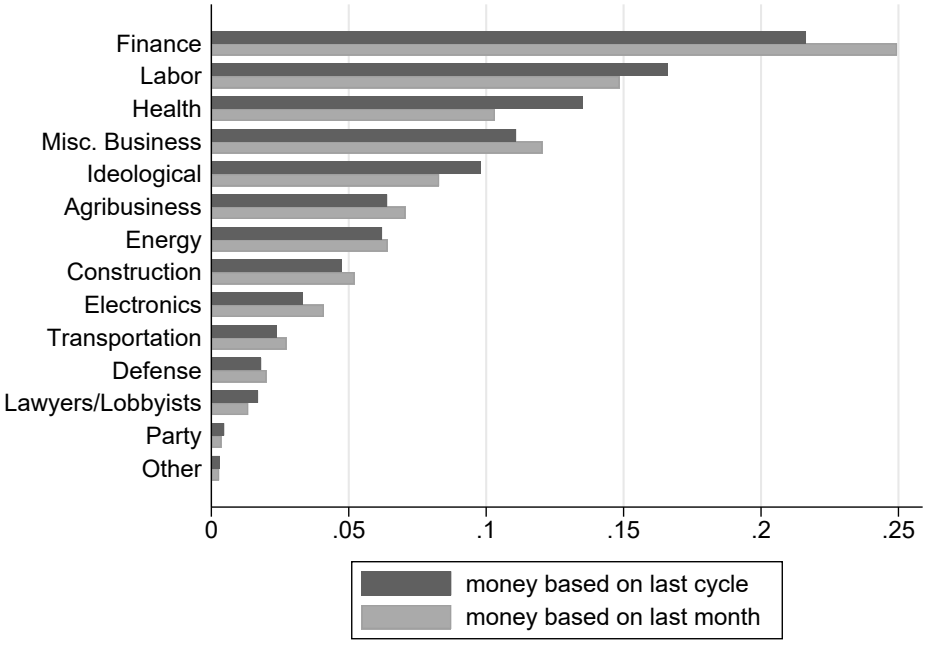
- Excluded transaction types: 10 (donations to Independent Expenditure-Only Committees, i.e., Super PACs), 10j, 11j, 15j (memo entries, i.e., the share of an individual's donation to a candidate or to another committee that was previously donated to a joint fundraising committee; such donations would be counted twice if we kept these transactions), 19 (electioneering communications), 22y (refunds; for example, if the maximum limit allowed for donations has been exceeded by the individual, the surplus money is returned; we would count such cases doubly if we did not exclude these transactions)
- Excluded group codes: Y0000, Y2000, Y3000, Y4000 (unknown category, no employer listed or impossible to assign category; note that we count individuals assigned to these codes when we calculate the total number of individual donations from citizens in the constituency), Z9000, Z9100, Z9500, Z9600, Z9700, Z9800, Z9999 (non-contributing categories and candidate self-finance)

Figure A1(a) shows how different sectors are represented in our special interests measure. Each bar aggregates campaign donations that we can assign to particular votes, made by groups in the respective sector (in percentages relative to the total assignable money from all groups). A possible concern with our measure of interest group pressure might be the double counting of some money flows (e.g., if a group that supports two bills donates to a representative who votes on both issues). In order to see to what extent this issue affects our special interests measure, we change the time frame and only consider the campaign donations a representative receives in the month before the vote. This is what the corresponding second bar in Figure A1(a) shows, indicating a distribution of money flows across sectors that is rather similar to the one for the main measure. In general, there is a trade-off between capturing the theoretically relevant long-term relationship between campaign donors and representatives, and the potential double counting of money in the special interests measure. However, as the overall pattern changes only slightly, we conclude that potential double counting of money is not a substantial concern for the interpretation of our findings (see also footnote 22).

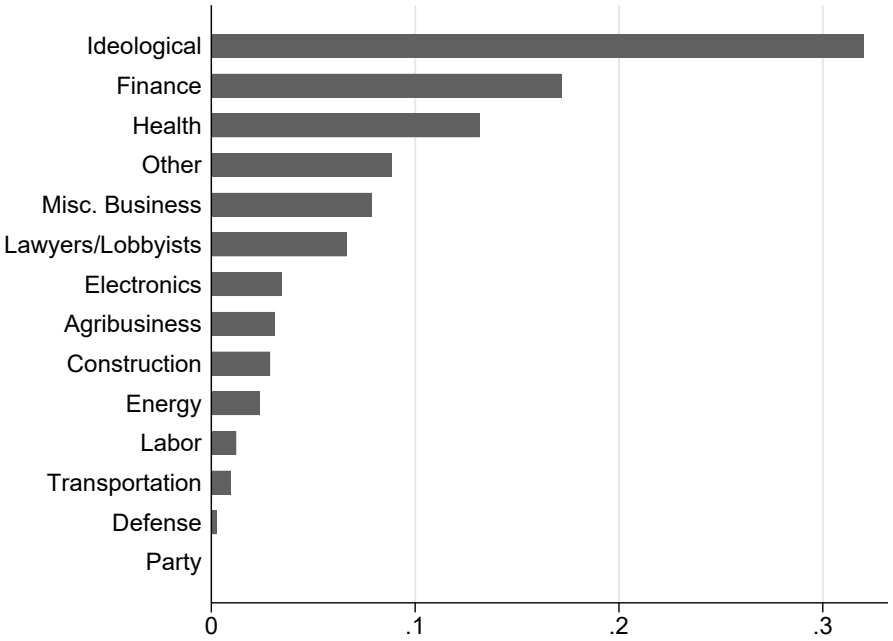
For the definition of the sectors, we follow the taxonomy of the CRP, except for the sector *Party*, which in our definition includes leadership PACs as well as party and candidate committees (single and joint). In the CRP's original definition, leadership PACs and candidate committees belong to the sector *Ideology/Single-Issue*, while joint candidate committees form a separate sector. Our sector *Ideological* corresponds to their sector *Ideology/Single-Issue*.

Figure A1: The relative strength of sectors in the special and constituent interests measures

(a) Special interests – Assigned campaign money per sector



(b) Constituent interests – Assigned individual campaign donations per sector



Notes: Each bar in figure (a) shows the share a particular sector makes up when aggregating all campaign donations that can be assigned to specific votes and made by groups in that sector (relative to the total assignable money by all groups). Figure (b) depicts the shares for the number of campaign donations made by individuals that we can assign to position-taking groups in each sector (relative to the total number of assignable individual donations). The sector *Other* includes education, civil servants, retired and non-profits. Figures on the total number of bill positions per sector are presented in Figure D2.

A.3 Compilation of measure for constituent interests

We calculate the net support for a given bill by subtracting the number of opposing donors from the number of supporting donors, resulting in *Constituency Yes (net)*. Given the way we measure special and voter interests we also capture well the cases in which these interests are aligned. For example, as firms are prohibited from directly contributing to candidates, they often found a company PAC to which the management of the firm is allowed to contribute. In such a case, the policy preferences of the firm's PAC and the contributing managers' preferences are likely aligned. By construction, our measures assign in such a case the individual donors (the managers/employees of the firm) to the same group as the PAC itself (which then donates to the campaign of a representative), thus assigning the same policy preferences to PAC and employees. The same holds more broadly. If a representative from Connecticut, for example, votes for insurance interests and a large share of the constituency benefits from the physical presence of the insurance industry in their state (that is, a large share of the population is employed in this industry), our measures would consider the policy preferences of the industry and the constituency as aligned and the representative would not face a conflict of interest in this situation. As we show below, the share of actively donating citizens working in a given industry in a given county/state is strongly correlated with the official employment share in this industry of the same county/state.

Individual donors are matched to congressional districts based on the ZIP codes in the campaign finance data (home or employer's address) and concordance tables provided by the US Census Bureau, which approximate the areas of US Postal Service ZIP codes using so-called ZIP Code Tabulation Areas (ZCTAs). The relationship files are available under https://www.census.gov/geo/maps-data/data/cd_national.html. Note that in 4% of the underlying individual transactions, we cannot allocate congressional districts because there is no corresponding entry in the US Census Bureau data. If a ZIP code falls into more than one district, we count the individual donors as if they belonged to all. In Table A1, we provide an overview of the transaction types and interest group codes that we exclude before we aggregate the individual donations.

A.4 Validation of measure for constituent interests

In addition to the explanations in the main text, we here provide some specific supplementary information regarding the three validation approaches.

Voting on propositions in California. For the constructed measure of constituent preferences, we take into account all individual campaign donations in connection with federal elections that were made in the election cycle before the respective ballot vote. To assign citizens' ZIP codes from the CRP donor data to the corresponding counties in California, a crosswalk from the US Department of Housing and Urban Development was used (https://www.huduser.gov/portal/datasets/usps_crosswalk.html; we took the 1st quarter 2014 file; in cases where a ZIP code lies in more than one county, the county in which most people live has been assigned to it). The county level voting results on ballot proposition are from the office of the California Secretary of State (see https://www.sos.ca.gov/elections/prior-elections/statewide-election-results).

Responses to survey questions. The surveys of the Cooperative Congressional Election Study for the different years can be downloaded from the Harvard Dataverse (https://cces.gov.harvard.edu). The number of respondents is about 35,000 in the 2006 and 2008 surveys, and about 55,000 in 2010,

2012 and 2014. Table A3 lists the bills included in our analysis. Note that we can only construct preferences on policies for which a bill exists and for which MapLight has documented interest group positions; i.e., we cannot consider preferences on legislative amendments for which the CCES includes questions. Also note that the questions are always asked before the congressional elections, and involve bills discussed in the current Congress, but also bills from past sessions. If the question about a particular bill is contained in several waves, we take the answers from the congressional session in which the bill appeared for the first time.

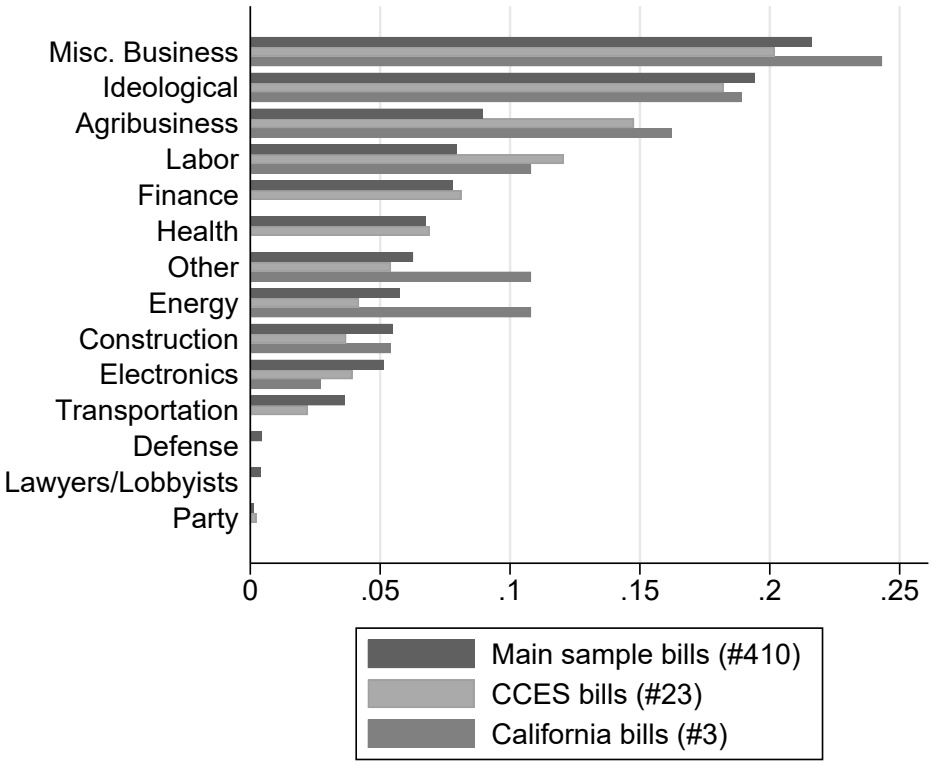
Industry structure. In Table A2 we present the results of the correlation analysis for three different statistical measures of employment in a particular industry. For the categorization of industries the US Census Bureau uses the North American Industry Classification System (NAICS). In order to assign the employment of a particular NAICS category to the broader CRP industry categories, we distribute its employment equally among all CRP industries where this NAICS code appears. Figure A2 documents what fraction of special interest groups is linked to a particular sector in the overall sample as well as in the two validation samples for propositions in California and for bills in the CCES.

Table A2: Comparing the industry structure from employment statistics and individual donor data

	Dependent variable:					
	#Donors	*#Donors (log)*	*Donor Share*			
	(1)	(2)	(3)	(4)	(5)	(6)
#Employees	0.009***					
	(0.002)					
#Employees (log)		0.399***				
		(0.015)				
Employment Share			0.413***	0.431***	0.431***	0.470***
			(0.055)	(0.051)	(0.050)	(0.057)
Constant						0.005***
						(0.0003)
State FE	X	X	X	X	X	
Cycle FE	X	X	X	X		
Industry FE	X	X	X			
Observations	44,254	44,254	44,254	44,254	44,254	44,254
Adjusted R^2	0.247	0.469	0.591	0.342	0.334	0.172

Notes: OLS regressions with robust standard errors clustered by county in parentheses. The unit of observation is county-industry-year. In specification (1) the dependent variable is the number of individual campaign donors associated with a specific industry per county and year according to the FEC's individual donations records and the industry assigned by the CRP. In model (2) we use the natural logarithm of the latter as the dependent variable. The dependent variable in specifications (3) to (6) is the share of donors associated with a specific industry per county and year. The explanatory variable *#Employees* measures the number of employees per industry-county-state according to the US Census Bureau County Business Patterns (CBP) statistics. The CBP figures are available at the level of 6-digit NAICS codes, and are more granular than the CRP industry categories. Employment in a particular NAICS industry is therefore evenly distributed among the CRP industries where this NAICS code occurs. In all specifications the sample is restricted to observations with more than 50 employees and more than 10 donors. The time span of our sample is 2000 to 2014. * p<0.1, ** p<0.05, *** p<0.01.

Figure A2: Interest group presence by sector in different sets of bills



Notes: The graph shows the relative presence of sectors in three sets of bills: The 410 bills from our main analysis, the 23 bills from the CCES survey, and the three Californian bills from the ballot proposition analysis. We use the latter two sets to validate our measure for constituent interests, which we construct for the 410 bills from our main analysis. We measure the frequency with which interest groups of a particular sector take positions regarding the bills from the respective set (i.e., the number of positions from interest groups within a sector relative to the total number of positions). The sector *Other* includes education, civil servants, retired and non-profits. See Section 3.2 for the corresponding validation tests.

Table A3: Bills from the CCES survey that we use to validate the measure for constituent interests

2006 CCES (109th Congress)

- Immigration Reform Act - S. 2611
- Central America Free Trade Agreement - H.R. 3045

2008 CCES (110th Congress)

- Withdrawal of US troops from Iraq - H.R. 2237
- Increase Minimum Wage - H.R. 2
- Stem Cell Research Funding - S. 5
- Health Insurance Program for Children - H.R. 976
- Housing and Economic Stimulus Act - H.R. 3221
- Extend NAFTA to Peru - H.R. 3688
- Bank Bailout of 2008 - H.R. 1424

2010 CCES (111th Congress)

- American Recovery and Reinvestment Act - H.R. 1
- Children's Health Insurance Program - H.R. 2
- American Clean Energy and Security Act - H.R. 2454
- Affordable Care Act - H.R. 3590
- Wall Street Reform Bill - H.R. 4173

2012 CCES (112th Congress)

- Ryan Budget Bill - H.Con.Res. 34
- Middle Class Tax Cut Act - S. 3412
- Tax Hike Prevention Act - S. 3413
- US-Korea Free Trade Agreement - H.R. 3080
- Repeal Affordable Care Act - H.R. 6079

2014 CCES (113th Congress)

- Temporary Debt Limit Extension Act - S. 540
- Farm Bill - H.R. 2642
- NSA Phone Surveillance Reform - S. 2685
- Changing Senate Cloture Rules - S.Res. 15

B Determinants of representative-vote-specific campaign money

The amount of campaign money individual representatives receive from special interests is likely the result of some strategic considerations to effectively influence the political process. We are therefore reluctant to make strong interpretations of the correlation with voting behavior and concentrate on the interaction with exogenous variation in media attention. However, we still want to provide an understanding of the covariates related to these money flows. Accordingly, we estimate models where the dependent variable is the total amount of money that a representative received in the last election cycle before a particular vote from interest groups with a position regarding the bill (*SIG Money Yes* + *SIG Money No*). As explanatory variables we use party affiliation, majority status, seniority, a dummy indicating retirement at the end of the session, electoral security and ideological moderateness. We also include two bill-specific measures capturing i) the potential for conflict and ii) the extent to which the bill tends to affect economic (business groups, unions, trade associations) or ideological/partisan groups. We measure *Electoral Security* by the margin of victory in the representative's last election; *Ideological Moderateness* is the negative of the absolute distance of the DW-NOMINATE score to zero (higher values are thus associated with more moderate representatives); *Bill Conflict Potential* is the number of organizations taking positions regarding the bill (support/oppose/indifferent) minus the absolute difference between supporting and opposing organizations; *Bill Economic* is the number of economic interest groups with positions on the bill divided by the total number of interest groups (economic, ideological and partisan) that have documented positions. Table B1 provides descriptive statistics for all the variables we use in our analysis.

For each vote, a representative gets about $16,000 from organized interests supporting or opposing the bill, on average. The regression results in Table B2 show that Democrats receive, on average, $4,800 less compared with their Republican colleagues (over one election cycle). This is consistent with the fact that business PACs tend to favor Republican candidates, just as they outspend labor and ideological interests.[2] When we exploit variation within representatives in column (3) we find that being a member of the majority party is linked with an additional $2,800 in campaign money per vote. This is in line with Rudolph (1999) and Cox and Magar (1999), who argue that majority party status is an important institutional asset. The estimated coefficients on seniority and retirement emphasize the investment motive of interest groups when engaging in political spending. Our results indicate that ten more years in office are associated with $42,000 more for each vote. Surprisingly and counterintuitively, a representative who is serving her last term before retiring does not get less money than before. A likely explanation is that in our approach (which measures long-term exchange relationships) the timing of money transfer and legislative vote may be far apart (in the most extreme case, up to almost four years, for example when the transfer takes place at the beginning of 2007 and the vote before the elections in 2010). In such cases, at the time of donation, special interests often will not know that the supported representative is retiring after her next term. We therefore estimated a model where the dependent variable is representatives' last year campaign funds (instead of over the last election cycle). This

[2]More than 70% of all PAC donations in the 2015-16 election cycle came from business PACs, of which two thirds were to Republican candidates (https://www.opensecrets.org/overview/blio.php?cycle=2016).

approach yields, as expected, a significantly negative coefficient on the retiring indicator. In the last year before the vote, retiring representatives receive on average $4,600 less from groups that are interested in the bills they vote on, whereas all other findings do not change substantially.[3]

Beyond that, a higher vote margin in the representative's last election leads to a decrease in vote-specific campaign funds: A 25 percentage point higher margin (one standard deviation) is associated with a loss of about $1,500. This seems plausible against the background that political investors see their chance rather in contested races where candidates rely on well filled war chests. Snyder (1992) as well as Grier and Munger (1993) test seniority and electoral security (among other factors). Their results also indicate a positive relationship between representatives' time in office and aggregate campaign contributions they receive, and a negative correlation between electoral security and campaign funds. Likewise, ideological moderation is associated with more campaign funds ($2,900 more for a position that is one standard deviation closer to zero in the DW-NOMINATE score). This suggests that interest groups may have stronger incentives to fund less extreme representatives whose voters are more likely to be located at the threshold between supporting and opposing a particular bill. As we have just one representative changing party in our sample and as ideological moderateness barely changes over time for a given representative, we exclude those covariates when we exploit variation within representatives. Finally and not surprisingly, a more contested bill as well as a greater share of economic organizations interested in the bill are correlated with more campaign money.

[3]The mean value for the amount of campaign funds that representatives receive in the last year before voting (excluding the month of the vote) is $9,600, with a standard deviation of $24,400. The additional results are available upon request.

Table B1: Descriptive statistics for the determinants of campaign money

Variable	Mean	Std. Dev.	Min.	Max.	N
Money Total	1.602	3.975	0	108.2	175,223
Democratic Party	0.503	0.500	0	1	175,223
Majority Member	0.552	0.497	0	1	175,223
Seniority	5.831	4.477	1	30	175,223
Retiring from Office	0.049	0.215	0	1	175,223
Electoral Security	0.343	0.245	0	1	175,223
Ideological Moderateness	-0.521	0.223	-1.361	-0.003	175,206
Bill Conflict Potential	5.288	14.236	0	208	175,223
Bill Economic	0.575	0.389	0	1	175,223

Notes: Money Total is measured in $10,000 units, *Seniority* is in two-year terms. The unit of observation is representative-vote.

Table B2: The determinants of representative-vote-specific campaign money

Dependent variable: *Money Total*	(1)	(2)	(3)
Democratic Party	-0.479***	-0.482***	
	(0.160)	(0.161)	
Majority Member	0.201*	0.202*	0.279**
	(0.104)	(0.104)	(0.116)
Seniority	0.056***	0.056***	0.844***
	(0.014)	(0.014)	(0.241)
Retiring from Office	0.255	0.241	0.132
	(0.197)	(0.200)	(0.211)
Electoral Security	-0.551***	-0.564***	-0.593***
	(0.153)	(0.155)	(0.189)
Ideological Moderateness	1.287***	1.283***	
	(0.294)	(0.297)	
Bill Conflict Potential	0.085***		
	(0.025)		
Bill Economic	1.812***		
	(0.238)		
Congress FE	X		
Vote FE		X	X
Representative FE			X
Observations	175,206	175,206	175,223
Adjusted R^2	0.186	0.483	0.542

Notes: OLS regressions with robust standard errors two-way clustered by representative and vote in parentheses. The unit of observation is representative-vote. *Money Total* (in $10,000 units) is the sum of campaign contributions a representative received from interest groups with positions on the bill in the last (two-year) election cycle before the vote. The sample consists of 425 final passage votes between 2005 and 2014. Descriptive statistics of the variables used are presented in Table B1. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

C News reporting on politics after shocks

To check whether the news pressure caused by our shock events is actually related to reduced media attention to national politics, we estimate models in which we use the shock events to explain political news coverage. We study local television and newspapers. The underlying data come from the TV News Archive and Media Cloud. This section describes the data used and presents the results of the estimates. A more detailed description of the data sources used and their compilation can be found below in Section C.3.

C.1 Television

We draw on a selection of 51 local television stations in the US, of which we have closed captions of spoken news texts for the period from 2009 to 2014 (not all stations were monitored over the entire period). The dependent variables in Table C1 approximate the news airtime that a particular station allocates on a given day to national politics by the fraction of news segments that include either "Congress", "White House", or "Federal Government". In each case, we subtract the news segments that deal with politics related to the respective shock. If, for example, the political consequences of a mass shooting are discussed, we adjust our measure for these reports.[4] The mean value for that proxy is 0.8%, i.e., a local station allocates less than one percent of its news reports to national politics. This corresponds to 2.7 minutes per day, taking the 5.6 hours that a local station uses on average for local news in its program as a basis.[5] However, as the recorded news segments are in the form of 15-second clips (see Section C.3), and we get a hit each time one of our keywords is found in such a 15-second segment, we are likely to primarily capture the extensive margin of reporting, given that we do not know the actual length of the reports. Assuming that television stations produce not only fewer but also shorter news reports in the face of a shock, the crowding out effects reported here are therefore likely to be lower bound estimates. The explanatory variables are our shock treatment days, split up by event type (as given by Table 3). Their coefficients then indicate how much political news is crowded out in the specified reporting time frame after a shock on average. We also explore for each shock type how much shock-related news they generate. Here we search for news segments that contain a keyword related to the shock (see footnote 9 for the keyword list). In all regressions, we include fixed effects for each station, month (by year) and day of the week.

As the results in Table C1 show, the shock periods we have chosen are all associated with increased news reporting about the corresponding shock on local television. For example, after a serious technological disaster in the US, disaster-related news coverage increases by around one percentage point on average. This effect is quite large, given that the average news share for disasters is only about 3%. The increased coverage of the disaster seems to crowd out political news. In the two days following the incident (that is, if our indicator for technological disasters in the US takes a value of one), political news coverage is reduced by 0.13 percentage points on average. Evaluated at the mean share used by

[4] We subtract all news segments containing "Congress", "White House", or "Federal Government" as well as at least one keyword matching the respective shock event. See the section after the results for a more detailed description.

[5] This figure comes from a 2017 survey conducted by the Radio Television Digital News Association (RTDNA) in collaboration with Hofstra University; see https://rtdna.org/article/research_2018_local_news_by_the_numbers#televisionH.

broadcasters for political news (0.8%), this corresponds to a 16% decrease. Somewhat unexpectedly, we do not observe any crowding out effects after terrorist attacks in the rest of the world. Quite the opposite, we even observe more coverage of politics (without terror-related political news) in the days after serious terror activity outside the US. A possible ex post explanation is the 'rally-around-the-flag' effect (Mueller, 1970). Facing international terror, the US population becomes more supportive of the government, which broadcasters match with increased political reporting.

Table C1: News coverage of national politics on local television after shock events, 2009-2014

	Disaster news (1)	*Political news* (2)	*Terror news* (3)	*Political news* (4)	*Shooting news* (5)	*Political news* (6)
Natural disaster US	1.725***	-0.049*				
	(0.224)	(0.028)				
Natural disaster ROW	0.526***	-0.051***				
	(0.072)	(0.017)				
Techn. disaster US	0.981***	-0.130***				
	(0.258)	(0.033)				
Terrorist attack US			0.418***	-0.084***		
			(0.075)	(0.021)		
Terrorist attack ROW			0.112***	0.050***		
			(0.031)	(0.013)		
Shooting rampage US					1.199***	-0.063***
					(0.205)	(0.023)
Mean DV	2.956	0.798	1.049	0.799	0.632	0.811
Station FE	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X
Day-of-the-week FE	X	X	X	X	X	X
Observations	59,360	59,360	59,360	59,360	59,360	59,360
Adjusted R^2	0.462	0.329	0.319	0.330	0.337	0.330

Notes: OLS regressions with robust standard errors clustered by local television station in parentheses. The unit of observation is station-day. The dependent variables indicate the percentage news airtime dedicated to shock-related or national political news (hits for "Congress", "White House", or "Federal Government"), (potentially) ranging from 0 to 100. Those news segments that address political news related to the respective shock type were subtracted from *Political news* in each case. The explanatory variables indicate the relevant period with increased news pressure after each shock according to Table 3. A more detailed description of the data used and descriptive statistics for all the variables used in the regressions can be found in Section C.3/Table C3. * p<0.1, ** p<0.05, *** p<0.01.

C.2 Newspapers

In Table C2 we present results of estimates in which we examine the extent to which our shock events displace newspaper articles about national politics in local newspapers. Our sample draws on recorded articles from 334 newspapers (native online or online editions of native print newspapers) covering the period from 2008 to 2014 (not all outlets were tracked over the entire period). As before, we code an article as one about national politics if it contains either "Congress", "White House", or "Federal Government" (subtracting the articles that deal with politics related to the respective shock; see

Section C.3 for details). The data is daily, i.e., we know the number of articles from a newspaper that contain one of our keywords on a given day. We divide these figures by the total number of recorded articles per day and newspaper to approximate the proportions of news dealing with politics and the shock events – analogous to the local television analysis above (with a mean number of recorded articles per newspaper and day of around 20). The average proportion of articles that address national politics based on our keyword approach is between 3.6 and 4.2% (excluding articles mentioning both a shock and a political keyword). Notably, this share is substantially higher than the corresponding share that we get for local television (0.8% of news airtime).[6] The explanatory variables are again our shock treatment days (indicating whether the day falls in a period with increased news pressure following a shock; compare Table 3). Their coefficients then indicate whether an average shock day is associated with increased shock coverage and/or reduced political coverage. All estimates in Table C2 are with fixed effects for each newspaper, each month of each year and each day of the week.

Table C2: News coverage of national politics in local newspapers after shock events, 2008-2014

	Disaster news (1)	*Political news* (2)	*Terror news* (3)	*Political news* (4)	*Shooting news* (5)	*Political news* (6)
Natural disaster US	2.880***	-0.335***				
	(0.271)	(0.078)				
Natural disaster ROW	0.023	-0.114***				
	(0.119)	(0.043)				
Techn. disaster US	0.710***	0.128*				
	(0.179)	(0.072)				
Terrorist attack US			1.073***	-0.148***		
			(0.137)	(0.050)		
Terrorist attack ROW			0.119**	0.015		
			(0.055)	(0.043)		
Shooting rampage US					2.671***	-0.223**
					(0.207)	(0.087)
Mean DV	12.62	3.625	6.892	3.704	5.435	4.167
Newspaper FE	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X
Day-of-the-week FE	X	X	X	X	X	X
Observations	150,196	150,196	150,196	150,196	150,196	150,196
Adjusted R^2	0.266	0.276	0.324	0.260	0.276	0.296

Notes: OLS regressions with robust standard errors clustered by newspaper in parentheses. The unit of observation is newspaper-day. The dependent variables indicate the percentage of newspaper articles with hits for the respective shock keywords (see footnote 9), and the percentage of articles that address national politics, approximated by articles containing "Congress", "White House" or "Federal Government" (excluding articles that contain both one of the latter keywords and a shock keyword). All news variables (potentially) range from 0 to 100. The explanatory variables indicate the relevant period with increased news pressure after each shock according to Table 3. A more detailed description of the data used and descriptive statistics for the variables can be found in Section C.3/Table C3. * p<0.1, ** p<0.05, *** p<0.01.

[6]However, we have to keep in mind that our television estimates are more of a lower bound, as the underlying news segments that we search are divided into 15-second clips.

With the exception of natural disasters outside the US, all our shocks lead to an increase in newspaper coverage in the corresponding shock category. Regarding national political news (hits for "Congress", "White House" or "Federal Government"), we document significant crowding out effects after natural disasters (inside and outside the US), as well as terrorist attacks and shooting rampages occurring in the US. The observed crowding out effects range from 3 to 9% (evaluated at the means), i.e., on the relevant days with high news pressure after a certain shock, a newspaper reduces its political coverage by this amount on average. In contrast to television, where we observe an increase in news concerning national politics following international terrorist activity, we tend to see no change in the proportion of such news in newspapers (p-value 0.734). Counterintuitively, we document a higher share of newspaper articles assigned to national politics following technological disasters in the US. One possible explanation is that the demands on politics, as they are often part of the media debate in the wake of, for example, a transport or industrial accident, are not captured by our approach (that is, searching for news containing both a political and a disaster keyword). As a result, our measure for political reporting will not be adjusted for these reports.[7]

In summary, we find evidence for a crowding out of national political news (both on television and in newspapers) after natural disasters in and outside the US as well as after terrorist attacks and shooting rampages occurring in the US. To check whether the interpretation of our main result changes if we additionally take into account the results on current political coverage, we define alternative shock treatment groups, to which we assign only those votes taking place after shocks that actually displace political news (depending on the examined outlet). With these alternative definitions of $Shock_j$ (with j denoting the vote) we re-estimate our model equation (1), also excluding from the sample those votes that take place after events that are related to more reporting in the outlet studied (i.e., terrorist attacks outside the US in the case of television reporting as well as technological disasters in the US in the case of newspaper reporting). The goal here is to achieve a clear separation between votes taken under little documented media attention to congressional politics and votes taken under, rather, a normal level of media attention. Columns (2) and (3) from Table D7 show the related estimates.

C.3 Detailed description of the data sources and their compilation

Television. With regard to local television we collect data from the TV News Archive (a tool of the Internet Archive). From a selection of 163 stations (local, national and international), transcripts of the spoken texts in news broadcasts (so-called closed captions) have been archived here since 2009. We use the Television Explorer (a service provided by the Global Database of Events, Language, and Tone; GDELT) to systematically access this data via an API.[8] The analysis focuses on 2009-2014, the period for which we define our shock days, and we have information on local television stations' news reports. This selection leaves us with 51 local channels from six distinct market areas, of which we have recorded

[7] To shed more light on this, we estimated a model where the share of news articles containing political as well as disaster keywords (i.e., "Congress", "White House" or "Federal Government" and a disaster keyword) serves as a dependent variable. In fact, we observe no change in such news in the days following a technological disaster (p-value 0.996), while we document a positive effect for natural disasters inside and outside the US (p-values 0.000 and 0.135). Similarly, after terrorist activity in and outside the US and after US shooting rampages, we observe a higher share of newspaper articles that mention a political and a corresponding shock keyword (p-values 0.000, 0.026 and 0.000). The additional estimates are available upon request.

[8] GDELT 2.0 Television API: https://blog.gdeltproject.org/gdelt-2-0-television-api-debuts/.

news broadcasts (note that not all channels have been monitored over the entire period). We look for news segments that contain either "Congress", "White House", or "Federal Government", serving as our measure for political news. We clean these news clips of those that contain both one of the three terms as well as at least one keyword related to the respective shock.[9] This is important since we do not expect less coverage of, for example, the political consequences of a terrorist attack on US soil, or the financial aid promises of the US government after a devastating natural disaster abroad.

The recorded news segments are divided by GDELT's Television Explorer into 15-second clips. Thus, the search result of each request is the number of 15-second clips that contain our keywords. The total number of recorded 15 second-clips per channel and day is also provided. This makes an interpretation in terms of percentage airtime possible, allowing comparisons between stations as well as for a given station over time. We estimate specifications in which the percentage airtime given to national political issues by a particular station on a particular day (minus the political news related to shocks) is the dependent variable, with our shock-treatment days as explanatory variables (i.e., the days after our shocks when news pressure is high; see Figure 2/Table 3). We also use the airtime devoted to shock-related news as the dependent variable, whereby here we aggregate the news stories that contain at least one of the relevant shock keywords (compare footnote 9). The unit of observation is station-day. The monitored stations are from the markets (Designated Market Areas) Baltimore, Chicago, Los Angeles, Philadelphia, San Francisco and Washington. Descriptive statistics for the constructed variables are shown below (Table C3). The OLS regression results are presented in Table C1.

Newspapers. The underlying data are from Media Cloud, an open source project of the Berkman Klein Center for Internet & Society at Harvard University and the Center for Civic Media at MIT (https://mediacloud.org). Media Cloud tracks newspapers, websites, and blogs, and makes their content available in searchable form. Via their public API, we systematically queried when which newspapers mention our keywords in articles.[10] Our queries are based on more than 900 US newspapers (state and local), both print and online, published in English, and covering the period from 2008 to 2014 (not all newspapers were tracked throughout the entire period). For many newspapers, Media Cloud documents only very few articles per day in total, which is why we focus on those where more than at least 10 articles were recorded on the observed day. This restriction leaves us with 334 newspapers in the sample. For the average newspaper in our sample, we have observations for 450 days over the period from 2008 to 2014.

Again, our measure for news about national politics is based on all reports that mention "Congress", "White House", or "Federal Government", minus those mentioning both one of the political keywords

[9] The relevant keywords for shock-related news are as follows:

- Disasters (natural and technological): "Disaster", "Catastrophe","Flood", "Tsunami", "Flooding", "Earthquake", "Tornado", "Storm", "Hurricane", "Volcano", "Volcanic", "Landslide", "Epidemic", "Wildfire", "Fire", "Evacuation", "Accident", "Crash", "Casualty".
- Terrorist attacks: "Terror", "Terrorist", "Terrorism", "Attack", "Bomb", "Bombing", "Detonation" "Explosion", "Firebomb", "Killing".
- Shooting rampages: "Shooting", "Rampage", "Killing Spree"", "Shooter", "Gunman", "Gunfire", "Shootout", "Suicide".

[10] A guide to using the API can be found here: https://github.com/berkmancenter/mediacloud/blob/master/doc/api_2_0_spec/api_2_0_spec.md.

and one of the corresponding shock keywords (see footnote 9 for the keyword list). We subtract from all articles assigned to national politics those assigned both to national politics and to the shock category in question (to exclude articles that address politics related to the shock). Finally, for all newspaper articles that we assign to a specific news category, we calculate the respective percentage of the total number of articles recorded per newspaper and day. We thus receive the news share that the observed newspaper dedicates to national politics on the observed day. Descriptive statistics for all variables are presented below in Table C3. The estimation results can be found in Table C2.

Table C3: Descriptive statistics for the local television and newspaper estimates

Variable	Mean	Std. Dev.	Min.	Max.	N
Television (Table C1)					
Natural disaster US	0.014	0.117	0	1	59,360
Natural disaster ROW	0.039	0.193	0	1	59,360
Techn. disaster US	0.010	0.100	0	1	59,360
Terrorist attack US	0.035	0.183	0	1	59,360
Terrorist attack ROW	0.039	0.194	0	1	59,360
Shooting rampage US	0.016	0.125	0	1	59,360
Disaster news	2.956	3.656	0	49.82	59,360
Terror news	1.049	1.325	0	19.92	59,360
Shooting news	0.632	1.070	0	23.64	59,360
Political news (excl. disaster-politics news)	0.798	1.172	0	16.27	59,360
Political news (excl. terror-politics news)	0.799	1.170	0	15.35	59,360
Political news (excl. shooting-politics news)	0.811	1.184	0	16.27	59,360
Newspapers (Table C2)					
Natural disaster US	0.014	0.119	0	1	150,196
Natural disaster ROW	0.038	0.190	0	1	150,196
Techn. disaster US	0.012	0.109	0	1	150,196
Terrorist attack US	0.044	0.204	0	1	150,196
Terrorist attack ROW	0.064	0.244	0	1	150,196
Shooting rampage US	0.015	0.120	0	1	150,196
Disaster news	12.62	8.492	0	100	150,196
Terror news	6.892	7.174	0	100	150,196
Shooting news	5.435	5.366	0	100	150,196
Political news (excl. disaster-politics news)	3.625	4.151	0	93.02	150,196
Political news (excl. terror-politics news)	3.704	4.219	0	93.02	150,196
Political news (excl. shooting-politics news)	4.167	4.602	0	93.02	150,196

Notes: The unit of observation is station/newspaper-day. The news variables approximate the percentage of news airtime/news articles dedicated to shock-related or national political news (excluding news on politics related to the respective shock).

D Additional analyses and results

D.1 Instrumental variables estimation

Alternatively to the reduced form approach presented in the main text, we can follow an instrumental variables approach, using the re-defined shock treatment days (only those shocks that actually displace political news) to explain current media reporting on politics (*Political News*$_j$) in the first stage. Based on the predicted values of *Political News*$_j$ for political news coverage on the day vote j takes place, we then estimate the second stage, which corresponds to our baseline model equation (1), with *Shock*$_j$ being replaced by *Political News*$_j$:

$$\begin{aligned} \textit{Vote Yes}_{ij} = \beta_0 &+ \beta_1 \textit{SIG Money Yes}_{ij} + \beta_2 \textit{Constituency Yes}_{ij} \\ &+ \beta_3 \textit{SIG Money Yes}_{ij} \times \textit{Political News}_j + \beta_4 \textit{Constituency Yes}_{ij} \times \textit{Political News}_j \\ &+ \beta_5 \textit{SIG Money Yes}_{ij} \times \textit{Conflict}_{ij} + \beta_6 \textit{Constituency Yes}_{ij} \times \textit{Conflict}_{ij} \\ &+ \beta_7 \textit{SIG Money Yes}_{ij} \times \textit{Political News}_j \times \textit{Conflict}_{ij} \\ &+ \beta_8 \textit{Constituency Yes}_{ij} \times \textit{Political News}_j \times \textit{Conflict}_{ij} + \beta_9 \textit{Conflict}_{ij} \\ &+ \textit{Representative}_i \times \textit{Party-of-Sponsor}_j \; FE \\ &+ \textit{Vote}_j \; FE + \varepsilon_{ij}. \end{aligned} \tag{2}$$

Following the approach outlined above, we use the two different measures for *Political News*$_j$ based on the relative frequency of keywords related to national politics mentioned in news reports (the first based on state and local television, and the second based on local newspapers' articles mentioning specific keywords). Across all television stations/newspapers monitored on a given day, we calculate the average percentage of news segments allocated to national politics, whereby in the aftermath of a shock, we subtract all news reports that are likely to deal with politics related to the shock in question (since we only want to capture non-shock-related political reporting, from which we document a crowding out after shocks). The period for which we have news reporting data does not exactly overlap with the time frame of our main sample (the television news reports sample starts in 2008, the newspaper sample in 2009). In these additional analyses we can, therefore, only take into account roll calls between between 2008 respectively 2009 and 2014, while our baseline results are based on the period 2005 to 2014.[11] We instrument each of the interaction terms from model equation (2) containing *Political News*$_j$ by its respective counterpart, where *Political News*$_j$ is replaced by the appropriate *Shock*$_j$ indicator (taking a value of one when the vote is taken after shocks that displace news in the respective outlet, as summarized in Table 6). The columns in Table D1 show for local television and newspapers respectively the corresponding reduced form, OLS, and 2SLS estimates (descriptive statistics for the variables used are presented in Table D2).

The reduced form results in columns (1) and (2) differ only slightly from those of our baseline estimate, and lead to the same conclusion – namely, that moneyed interests win and voters lose when they are in conflict with each other, and that this tendency is even greater when the vote takes place after

[11] However, we also present estimates of our baseline specification based on the restricted sample for the years 2008 to 2014 (these correspond to the reduced form estimates in Table D1). The results remain qualitatively the same.

Table D1: 2SLS results using the shocks as instruments for national political news reporting

Dependent variable: *Vote Yes [0/100]*	Reduced form regressions. (only use shocks displacing news on politics in ...)			OLS and 2SLS regressions. Instrument political news with shocks (only those displacing national politics news in...)			
	Local TV newscasts	Local newspapers		Local TV newscasts		Local newspapers	
	(1)	(2)		(3)	(4)	(5)	(6)
SIG Money Yes (net)	1.017***	1.057***	SIG Money Yes (net)	0.980***	1.038***	1.018***	0.995***
	(0.231)	(0.215)		(0.198)	(0.226)	(0.193)	(0.190)
SIG Money Yes x Shock	-0.309	-0.179	SIG Money Yes x Polit. News	0.359	1.071	0.098	0.067
	(0.292)	(0.324)		(0.392)	(0.925)	(0.078)	(0.202)
SIG Money Yes x Conflict	1.746***	1.717***	SIG Money Yes x Conflict	2.099***	2.118*	2.434***	5.143***
	(0.534)	(0.505)		(0.571)	(1.144)	(0.632)	(1.638)
SIG Money Yes x Shock x Conflict	5.795***	5.722**	SIG Money Yes x Polit. News x Conflict	-2.668**	-16.22***	-0.304***	-1.816*
	(2.189)	(2.222)		(1.088)	(6.171)	(0.093)	(0.942)
Constituency Yes (net)	0.596***	0.559***	Constituency Yes (net)	0.605***	0.626***	0.580***	0.579***
	(0.120)	(0.120)		(0.118)	(0.123)	(0.125)	(0.137)
Constituency Yes x Shock	0.067	0.137	Constituency Yes x Polit. News	-0.034	-0.131	-0.063	-0.099
	(0.268)	(0.315)		(0.230)	(1.002)	(0.064)	(0.198)
Constituency Yes x Conflict	-0.820**	-0.814**	Constituency Yes x Conflict	-1.022***	-0.964**	-0.955***	-0.856
	(0.371)	(0.358)		(0.279)	(0.465)	(0.321)	(0.534)
Constituency Yes x Shock x Conflict	0.125	-0.385	Constituency Yes x Polit. News x Conflict	1.660**	-0.314	0.291	0.513
	(1.058)	(1.187)		(0.748)	(2.704)	(0.216)	(0.743)
Conflict	-8.259***	-7.372***	Conflict	-8.491***	-8.389***	-7.446***	-6.760***
	(2.285)	(2.099)		(2.273)	(2.350)	(2.108)	(2.196)
			Mean of *Political News*	0.852		4.495	
Representative x Party-of-Sponsor FE	X	X		X	X	X	X
Vote FE	X	X		X	X	X	X
Observations	99,581	124,837		99,581	99,581	124,837	124,837
Adjusted R^2	0.489	0.484		0.489	0.012	0.484	0.004

Notes: Robust standard errors two-way clustered by representative and vote in parentheses. The unit of observation is representative-vote. The sample covers the period 2008 respectively 2009 to 2014 (2008 for newspapers, 2009 for television). Columns (1) and (2) show reduced form estimates, only using those shocks that displace national politics news in the corresponding outlet (local television and newspapers). See Table 6 for a summary of the documented crowding out effects and Tables C1/C2 for the underlying estimates. In columns (3)-(6), instead of the shock indicator, we directly use the current volume of national political news reporting (*Political News*; demeaned) on the day of the vote (average news shares across all television stations/newspapers observed on the corresponding day). The mean values of *Political News* are shown in the table, their standard deviations are 0.457 and 1.986 (television/newspapers). For each examined media outlet, we show the OLS and 2SLS results respectively, with the latter estimates using the shocks (only those that lead to a displacement of national politics news in the respective outlet) to explain political news coverage in the first stage. Descriptive statistics for the used variables can be found in Table D2. * p<0.1, ** p<0.05, *** p<0.01.

Table D2: Descriptive statistics for the instrumental variables results (Table D1)

Variable	Mean	Std. Dev.	Min.	Max.	N
Local TV newscasts					
Vote Yes	75.94	42.75	0	100	99,581
SIG Money Yes (net)	1.190	4.468	-34.79	107.6	99,581
Constituency Yes (net)	1.023	4.475	-101.7	224.9	99,581
Conflict	0.105	0.306	0	1	99,581
Shock	0.104	0.305	0	1	99,581
Political News	0.852	0.457	0.157	3.247	99,581
Local newspapers					
Vote Yes	77.00	42.08	0	100	124,837
SIG Money Yes (net)	1.082	4.159	-34.79	107.6	124,837
Constituency Yes (net)	0.943	4.250	-101.7	224.9	124,837
Conflict	0.098	0.297	0	1	124,837
Shock	0.077	0.267	0	1	124,837
Political News	4.496	1.986	0	20	124,837

Notes: The table shows descriptive statistics for the variables used in the instrumental variables regressions, depending on the observed outlet (local television and newspapers). The special interest money variable is in $10,000 units; the measure for constituent interests is expressed in the unit of 100 campaign donors. *Conflict* takes a value of one if *SIG Money Yes (net)* > 0 and *Constituency Yes (net)* < 0 or vice versa. *Political News* (in %) corresponds to the average share of national political news in the corresponding outlet on the day of the vote. The unit of observation is representative-vote.

a shock event that actually displaces news about national politics.[12] In columns (3) and (5) we present the results of OLS estimates, where instead of the shock events we directly use political news reporting to interact with our measures for special and constituent interests. Note that this specification exploits variation in the day-to-day volume of national politics coverage for identification, and not just changes in news coverage that are due to the shocks. Therefore, the associated results should be treated with caution, as confounding factors, such as the content of a bill, may influence both news reporting and representatives' voting decisions. *Political News* has been demeaned, which is why the coefficients on money and voter interests as well as their interactions with *Conflict* show their effects at the mean, i.e., a normal level of media attention devoted to politics. The 2SLS results in columns (4) and (6) only rely on variation in national political news coverage caused by the exogenous shocks. Compared to the OLS results, we document a stronger interaction of our measure of special interests with political reporting, especially when the position of the donors is in conflict with that of the voters. Evaluated at an average level of *Political News* on television, for example, the estimated effect for the impact of special interest money on representatives' voting decisions (given that special interest donors and voters are at odds)

[12]The difference from the baseline model lies in the shocks used and the time period studied. Also note that the difference from the estimates in columns (2) and (3) of Table D7 is that in the latter we use the entire period 2005-2014 and also exclude those shocks from the control group after which we document increased political reporting according to Table 6 (which explains the tendency for higher effects here).

increases by 36% if *Political News* decreases by 0.075, the percentage that an average shock displaces political news in the relevant days with increased news pressure (see Table C1).[13] The corresponding effects of decreased political coverage in newspapers is about 6%.

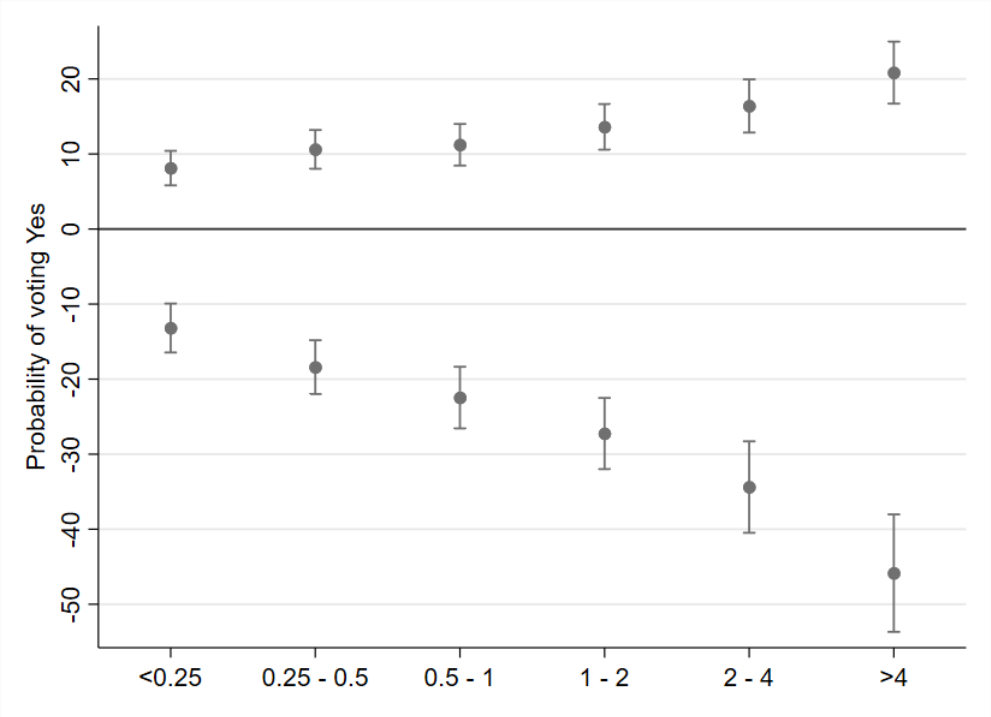
D.2 Complementary table on shock events and news pressure

Table D3 presents the full set of estimation results on how the various shock events affect the measure of daily news pressure. The estimated coefficients for the different time intervals are presented graphically in Figure 2 in the main text.

D.3 Correlation analysis of Yes and No money and representatives' roll call voting

Figure D1 shows the coefficients of the correlation analysis between the absolute Yes and No money received by representatives for a given bill and their roll call decisions regarding these bills (see also footnote 52 in the main text). The underlying estimation results are given in Table D4.

Figure D1: The correlation between Yes and No money and representatives' voting behavior



Notes: The graph shows the different effects of Yes and No money in the corresponding interval (in $10,000 units) on representatives' voting behavior. Above the x-axis are the coefficients for Yes money, i.e., the money donated by interest groups which support the bill on the probability of voting Yes; below, the corresponding effects for the money spent against bills on the probability of voting Yes. The effects are in percentage points (with no money at all as the reference category). See Table D4 for the underlying regression results. 95% confidence intervals included.

[13] The effect of *SIG Money Yes* (given conflict) at the mean value of *Political News* on television is $1.038 + 2.118 = 3.156$. If *Political News* increases by 0.075, the effect of *SIG Money Yes* decreases by $0.075 \times (16.22 - 1.071) = 1.136$.

Table D3: Shock events and news pressure in the US evening news

Dependent variable: *Daily News Pressure*	$[-10,-6]$	$[-5,-1]$	$shock = t$	$+1$	$+2$	$+3$	$+4$	$+5$	$+6$	$[+7,+10]$	$[+10,+20]$
Natural disaster US	-0.238	1.006***	2.293***	0.838	0.877	1.032*	0.489	-0.206	0.859	0.248	-0.242
	(0.366)	(0.377)	(0.601)	(0.601)	(0.604)	(0.605)	(0.605)	(0.627)	(0.625)	(0.412)	(0.266)
Natural disaster ROW	-0.537**	-0.206	0.563	0.501	0.880**	0.347	1.007**	1.075**	0.563	0.350	-0.277
	(0.251)	(0.258)	(0.412)	(0.413)	(0.415)	(0.422)	(0.437)	(0.429)	(0.421)	(0.279)	(0.177)
Techn. disaster US	-0.246	-0.199	0.522	1.330***	0.849**	-0.038	-0.294	0.385	-0.560	-0.347	0.032
	(0.256)	(0.263)	(0.426)	(0.419)	(0.429)	(0.422)	(0.422)	(0.422)	(0.428)	(0.288)	(0.186)
Techn. disaster ROW	-0.052	0.173	0.420	0.507	0.041	0.383	-0.226	-0.259	0.390	0.178	-0.053
	(0.253)	(0.265)	(0.421)	(0.430)	(0.432)	(0.425)	(0.440)	(0.424)	(0.423)	(0.289)	(0.183)
Terrorist attack US	-0.435	0.263	1.833***	2.168***	2.105***	0.804	1.201**	0.314	0.442	-0.828**	-0.582**
	(0.340)	(0.350)	(0.558)	(0.577)	(0.577)	(0.562)	(0.562)	(0.561)	(0.560)	(0.383)	(0.247)
Terrorist attack ROW	-0.514	0.371	2.817**	2.893**	2.205*	2.630**	-0.833	-0.654	-1.493	-0.557	-0.326
	(0.617)	(0.708)	(1.127)	(1.129)	(1.135)	(1.137)	(1.136)	(1.134)	(1.131)	(0.774)	(0.447)
Shooting rampage US	-0.400	-0.356	1.669***	2.030***	0.731	0.387	-0.586	-0.553	-0.594	-0.064	-0.354
	(0.301)	(0.310)	(0.494)	(0.507)	(0.507)	(0.498)	(0.497)	(0.509)	(0.495)	(0.339)	(0.219)
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Day-of-the-week FE	X	X	X	X	X	X	X	X	X	X	X
Observations	3,263	3,263	3,205	3,204	3,203	3,202	3,201	3,200	3,199	3,255	3,259
Adjusted R^2	0.437	0.406	0.197	0.194	0.185	0.180	0.181	0.183	0.187	0.345	0.563

Notes: OLS regressions with standard errors in parentheses. The dependent variable is *daily news pressure* (Eisensee and Strömberg, 2007) at different days around the shock event (or averaged over days if we consider time spans). The explanatory variables are indicators which take a value of one if, on day t, the number of deaths caused by the respective event lies above the 99th percentile of the (event- and region-specific) distribution. In case of natural and technological disasters, we restrict the sample to one-day incidents (i.e., disasters which last no longer than one day). ROW refers to the rest of the world and aggregates all countries except the US. In the case of shooting rampages in the US, the indicator is one if there was a serious incident on day t (based on a list compiled by the *Los Angeles Times*). The sample period is 2005-2013. * p<0.1, ** p<0.05, *** p<0.01.

D.4 Robustness tables/additional material

Table D4: Estimation results for the effects of Yes and No money

Dependent variable *Vote Yes*	SIG Money Yes (abs.)	SIG Money No (abs.)
\$0	(Reference category)	
<\$2,500	8.119***	-13.19***
	(1.169)	(1.659)
\$2,500-5,000	10.61***	-18.41***
	(1.316)	(1.824)
\$5,00-10,000	11.23***	-22.46***
	(1.409)	(2.090)
\$10,00-20,000	13.61***	-27.25***
	(1.546)	(2.412)
\$20,00-40,000	16.40***	-34.38***
	(1.802)	(3.101)
>\$40,000	20.84***	-45.85***
	(2.101)	(3.983)

Notes: The table shows the OLS regression results of a model which regresses representative voting behavior on dummy variables capturing different intervals for the absolute \$-amount of Yes and No money that a representative receives in the last election cycle prior to the vote from specific interest groups that are in favor of and against the bill. The other explanatory variables are district preferences (the absolute measures), representative x party-of-sponsor and vote fixed effects. Robust standard errors two-way clustered by representative and vote in parentheses; N=175,223; R^2=0.507. *** p<0.01.

Table D6: Robustness specifications I

Dependent variable: *Vote Yes* [0/100]	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SIG Money Yes (net)	0.998***	0.799***	1.359***	1.202***	1.805***	1.055***	0.887***
	(0.232)	(0.223)	(0.233)	(0.216)	(0.287)	(0.207)	(0.187)
SIG Money Yes x Shock	0.077	0.010	0.012	-0.067	-0.148	-0.067	-0.0061
	(0.356)	(0.338)	(0.328)	(0.310)	(0.397)	(0.306)	(0.286)
SIG Money Yes x Conflict	2.053**	1.883**	1.779**	1.541**	1.628**	1.300**	1.054**
	(0.900)	(0.846)	(0.819)	(0.744)	(0.675)	(0.583)	(0.527)
SIG Money Yes x Shock x Conflict	4.112*	3.477*	3.482*	2.792*	3.437**	5.173***	4.856**
	(2.114)	(1.978)	(1.811)	(1.634)	(1.721)	(1.905)	(1.952)
Constituency Yes (net)	0.416**	0.503***	0.418**	0.496***	0.739***	0.495***	0.451***
	(0.189)	(0.185)	(0.169)	(0.163)	(0.175)	(0.116)	(0.108)
Constituency Yes x Shock	0.068	0.164	-0.205	-0.107	0.052	0.241	0.216
	(0.568)	(0.517)	(0.412)	(0.356)	(0.369)	(0.248)	(0.223)
Constituency Yes x Conflict	-2.176***	-2.000***	-2.161***	-1.913***	-1.557***	-0.932**	-0.667*
	(0.727)	(0.662)	(0.717)	(0.638)	(0.516)	(0.401)	(0.383)
Constituency Yes x Shock x Conflict	-0.721	-0.645	0.139	0.198	-0.305	-0.324	-0.354
	(1.519)	(1.357)	(1.499)	(1.291)	(1.094)	(0.925)	(0.829)
Conflict	-20.30***	-19.74***	-14.00***	-12.87***	-7.472***	-6.625***	-5.654***
	(2.927)	(2.712)	(2.961)	(2.670)	(2.677)	(2.056)	(2.028)
Shock	-5.505**	-5.562**	-2.724	-2.657			
	(2.613)	(2.595)	(2.403)	(2.393)			
Representative FE		X		X	X		
Representative x Party-of-Sponsor FE						X	
Representative x Year x Party-of-Sponsor FE							X
Year x Month FE			X	X			
Day-of-the-week FE			X	X			
Vote FE					X	X	X
Observations	175,223	175,223	175,223	175,223	175,223	175,223	175,223
Adjusted R^2	0.046	0.110	0.119	0.187	0.303	0.473	0.500

Notes: OLS regressions with robust standard errors two-way clustered by representative and vote in parentheses. Column (6) corresponds to our baseline specification as given by model equation (1). * p<0.1, ** p<0.05, *** p<0.01.

Table D7: Robustness specifications II

Dependent variable: *Vote Yes* [0/100]	Baseline model	Only shocks displacing political news on television	Only shocks displacing political news in newspapers	Only votes unrelated to the shocks	Probit estimation of baseline
	(1)	(2)	(3)	(4)	(5)
SIG Money Yes (net)	1.055***	1.052***	1.084***	1.036***	0.0559***
	(0.207)	(0.207)	(0.208)	(0.205)	(0.0052)
SIG Money Yes x Shock	-0.067	-0.204	-0.144	0.041	0.0196**
	(0.306)	(0.375)	(0.332)	(0.325)	(0.0100)
SIG Money Yes x Conflict	1.300**	1.285**	1.289**	1.282**	0.0256***
	(0.583)	(0.582)	(0.578)	(0.578)	(0.0083)
SIG Money Yes x Shock x Conflict	5.173***	7.945***	6.113***	4.522**	0.2251***
	(1.905)	(1.504)	(2.194)	(2.005)	(0.0323)
Constituency Yes (net)	0.495***	0.497***	0.509***	0.492***	0.0187***
	(0.116)	(0.116)	(0.116)	(0.114)	(0.0041)
Constituency Yes x Shock	0.241	0.203	0.184	0.070	-0.0006
	(0.248)	(0.293)	(0.321)	(0.287)	(0.0057)
Constituency Yes x Conflict	-0.932**	-0.940**	-0.948**	-0.919**	-0.0095
	(0.401)	(0.401)	(0.400)	(0.398)	(0.0065)
Constituency Yes x Shock x Conflict	-0.324	-0.767	-0.395	-0.121	0.0022
	(0.925)	(1.075)	(1.230)	(0.951)	(0.0191)
Conflict	-6.625***	-6.188***	-6.671***	-6.340***	-0.3460***
	(2.056)	(2.189)	(2.075)	(2.080)	(0.0218)
Representative x Party-of-Sponsor FE	X	X	X	X	X
Vote FE	X	X	X	X	X
Observations	175,223	167,336	171,167	169,399	152,074
Adjusted/Pseudo R^2	0.473	0.477	0.474	0.478	0.562

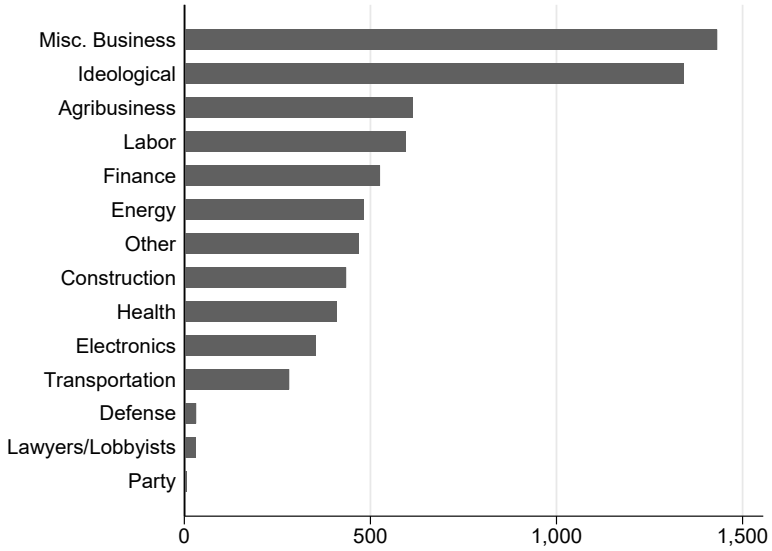
Notes: OLS regressions with robust standard errors two-way clustered by representative and vote in parentheses. Column (1) shows the results of our baseline specification (column (5)/Table 5). In models (2) and (3), our indicator of low attention to politics ($Shock_j$) only takes a value of one when the vote is held after such shocks that effectively displace news about national politics (hits for "Congress", "White House" or "Federal Government") in (2) television and (3) newspapers. See Section C (Tables C1/C2) for the related estimates as well as Table 6 for the summary list of which shocks replace political news on which outlet. The sample in column (4) excludes from the analysis all votes taken after shocks where the bills in question are potentially related to the previous shock in terms of content (see Section 5.3 as well as Table D9 for the bill topics that we classify as contextually relevant for each shock type). Finally, column (5) presents the results of a probit estimation of our baseline model (1); the standard errors from the probit estimation are only clustered at representative level. * p<0.1, ** p<0.05, *** p<0.01.

Table D5: Robustness – Shock duration

Dependent variable: *Vote Yes* [0/100]			
SIG Money Yes (net)	1.089***	Constituency Yes (net)	0.417***
	(0.209)		(0.109)
SIG Money Yes x After Shock	-0.307	Constituency Yes x After Shock	1.082***
	(0.559)		(0.258)
SIG Money Yes x Shock	-0.100	Constituency Yes x Shock	0.311
	(0.307)		(0.250)
SIG Money Yes x Conflict	1.404**	Constituency Yes x Conflict	-0.914**
	(0.652)		(0.457)
SIG Money Yes x After Shock x Conflict	-1.153	Constituency Yes x After Shock x Conflict	-0.538
	(1.260)		(0.743)
SIG Money Yes x Shock x Conflict	5.064***	Constituency Yes x Shock x Conflict	-0.337
	(1.932)		(0.951)
Conflict	-6.636***	Representative x Party-of-Sponsor FE	X
	(2.055)	Vote FE	X
		Observations	175,223
		Adjusted R^2	0.474

Notes: OLS regression with robust standard errors two-way clustered by representative and vote in parentheses. The mean value of *After Shock* is 0.08. For the remaining variables, the descriptive statistics can be found in Table 4 * p<0.1, ** p<0.05, *** p<0.01.

Figure D2: The number of interest groups with bill positions by sector



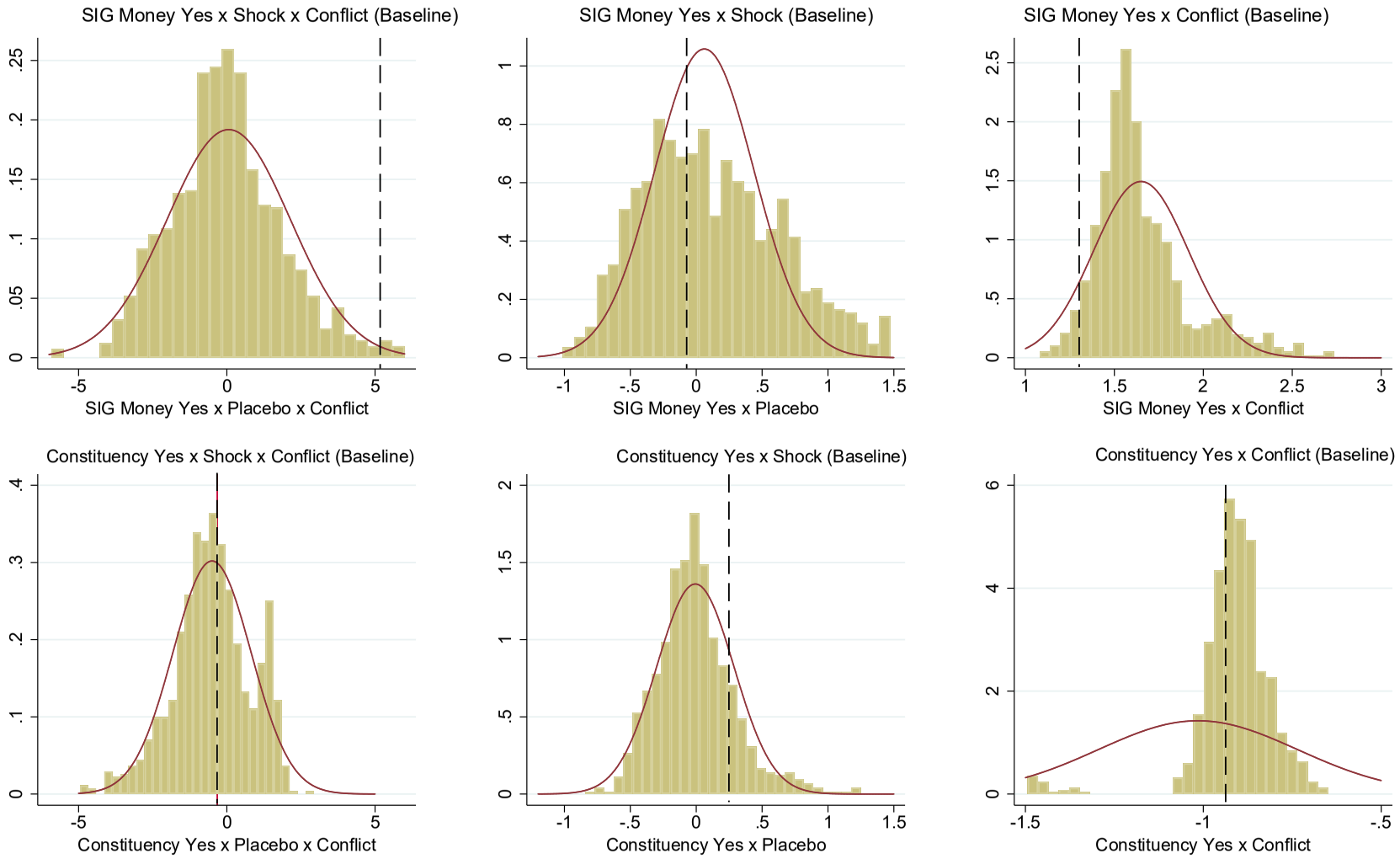
Notes: The graph shows the total number of interest groups per sector where assigned organizations have positions regarding the bills in our sample (the total number of interest group-bill combinations is 7,025). The sector *Other* includes education, civil servants, retired and non-profits.

Table D8: Robustness – Shock and conflict intensity

Dependent variable: *Vote Yes* [0/100]	Baseline model (1)	Shock intensity (2)	Conflict intensity (3)		(1) cont.	(2) cont.	(3) cont.
SIG Money Yes (net)	1.055***	1.182***	0.983***	Constituency Yes (net)	0.495***	0.219	0.489***
	(0.207)	(0.228)	(0.201)		(0.116)	(0.150)	(0.115)
SIG Money Yes x Shock	-0.067		-0.063	Constituency Yes x Shock	0.241		0.322
	(0.306)		(0.273)		(0.248)		(0.257)
SIG Money Yes x Shock (75-99th)		-0.149		Constituency Yes x Shock (75-99th)		0.419**	
		(0.313)				(0.189)	
SIG Money Yes x Shock (>99th)		-0.191		Constituency Yes x Shock (>99th)		0.518*	
		(0.332)				(0.281)	
SIG Money Yes x Conflict	1.300**	-0.104		Constituency Yes x Conflict	-0.932**	-0.330	
	(0.583)	(0.922)			(0.401)	(0.423)	
SIG Money Yes x Tension			1.234***	Constituency Yes x Tension			-0.101
			(0.430)				(0.156)
SIG Money Yes x Clear Conflict			1.449***	Constituency Yes x Clear Conflict			-0.561*
			(0.456)				(0.319)
SIG Money Yes x Shock x Conflict	5.173***			Constituency Yes x Shock x Conflict	-0.324		
	(1.905)				(0.925)		
SIG Money Yes x Shock (75-99th) x Conflict		1.966*		Constituency Yes x Shock (75-99th) x Conflict		-1.292**	
		(1.131)				(0.564)	
SIG Money Yes x Shock (>99th) x Conflict		6.574***		Constituency Yes x Shock (>99th) x Conflict		-0.914	
		(2.034)				(0.936)	
SIG Money Yes x Shock x Tension			1.168	Constituency Yes x Shock x Tension			-0.076
			(0.979)				(0.424)
SIG Money Yes x Shock x Clear Conflict			5.008**	Constituency Yes x Shock x Clear Conflict			-0.178
			(2.172)				(1.748)
Conflict	-6.625***	-7.179***		Representative x Party-of-Sponsor FE	X	X	X
	(2.056)	(2.068)					
Tension			0.253	Vote FE	X	X	X
			(0.973)				
Clear Conflict			-5.753**	Observations	175,223	175,223	175,223
			(2.760)	Adjusted R^2	0.473	0.474	0.472

Notes: OLS regressions with robust standard errors two-way clustered by representative and vote in parentheses. The mean values for the shock and conflict indicators are: *Shock (75-99th)* 0.598; *Shock (>99th)* 0.135; *Tension* 0.707; *Clear Conflict* 0.015. For the remaining variables, the descriptive statistics can be found in Table 4. * p<0.1, ** p<0.05, *** p<0.01.

Figure D3: Robustness – Distribution of the placebo coefficients



Notes: The graphs show the distribution of the coefficients that result from the placebo model. The placebo days were randomly distributed over all non-shock days for a total of 1,000 runs. The number of placebo days is chosen in such a way that it matches the proportion of original treatment days, i.e. about 13%. The vertical dashed lines in each graph depict the estimated coefficients from our baseline model that applies the real shock treatment.

Table D9: Legislative topics related to the shocks in terms of content

Legislative Subjects (Congress.gov)

- *Natural disasters:* Accidents; Atmospheric science and weather; Department of Agriculture; Department of Homeland Security; Disaster relief and insurance; Economic development; Emergency communications systems; Emergency planning and evacuation; Fires; First responders and emergency personnel; Floods and storm protection; Food assistance and relief; Foreign aid and international relief; Homeland Security; Homelessness and emergency shelter; Housing and community development funding; Life, casualty, property insurance; Natural disasters; Residential rehabilitation and home repair
- *Technological disasters:* Accidents; Emergency communications systems; Emergency planning and evacuation; Fires; First responders and emergency personnel; Hazardous wastes and toxic substances; National Transportation Safety Board (NTSB); Transportation safety and security; Water quality
- *Terrorist attacks:* Citizenship and naturalization; Conflicts and wars; Correctional facilities and imprisonment; Crime prevention; Crime Victims; Criminal investigation, prosecution, interrogation; Criminal justice information and records; Criminal procedure and sentencing; Department of Homeland Security; Department of Justice; Detention of persons; Director of National Intelligence; Federal Bureau of Investigation (FBI); Firearms and explosives; Foreign aid and international relief; Hate crimes; Homeland Security; Intelligence activities, surveillance, classified information; Law enforcement administration and funding; Law enforcement officers; Middle East; Military assistance, sales, and agreements; Military facilities and property; Military personnel and dependents; Political movements and philosophies; Racial and ethnic relations; Right of privacy; Sanctions; Terrorism; Visas and passports; War and emergency powers; War crimes, genocide, crimes against humanity
- *Shooting rampages:* Citizenship and naturalization; Correctional facilities and imprisonment; Crime prevention; Crime Victims; Criminal investigation, prosecution, interrogation; Criminal justice information and records; Criminal procedure and sentencing; Department of Homeland Security; Department of Justice; Detention of persons; Director of National Intelligence; Federal Bureau of Investigation (FBI); Firearms and explosives; Hate crimes; Homeland Security; Intelligence activities, surveillance, classified information; Law enforcement administration and funding; Law enforcement officers; Military assistance, sales, and agreements; Military facilities and property; Military personnel and dependents; Political movements and philosophies; Racial and ethnic relations; Right of privacy; Visas and passports

Issue Codes (Voteview.com)

- *Natural disasters:* Emergency Fuel Assistance; Food Stamps/Food Programs; Humanitarian Assistance (foreign); U. N.
- *Technological disasters:* Airlines/Airports/Airline Industry; Pollution and Environmental Protection; Public Safety; Railroads
- *Terrorist attacks*: Alien and Sedition Laws; Amnesty (all wars); Arms Control; CIA/Spying/Intelligence; Civil Rights/Desegregation/Busing/Affirmative Action; Death Penalty; Firearms; Immigration/Naturalization; Iran and Iraq; Nuclear Power; Nuclear Weapons; Peace Movements/Pacifism/Anti-Military; Public Safety; Treaties; U. N.; War on Terror (After 9-11)
- *Shooting rampages:* Arms Control; CIA/Spying/Intelligence; Civil Rights/Desegregation/Busing/Affirmative Action; Death Penalty; Firearms; Immigration/Naturalization; Public Safety