Three Papers on Congressional Communication and Representation

by

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Abstract

This project evaluates how elected officials communicate with their constituents and whether voters can tell if their interests are being represented. Specifically, I examine whether political communication strategies may inadvertently lead to suboptimal representation.

In my first paper, I evaluate whether members of Congress use criticism of Congress as a means to connect with their constituents, using approximately 10,000 campaign advertisements aired throughout the 2000s. In both this observational evidence and through an original experimental study, I show that when members criticize Congress, this message has little impact on attitudes toward Congress in general or the member in particular. However, survey respondents view a member who criticizes Congress as more “like them,” potentially introducing a distracting valence issue into elections.

In my second paper, I find clear evidence that legislative behavior does not change as a consequence of the rise or fall of military presence in a district. However, members’ communication with their constituents does change. Members who gain bases are more likely to emphasize military issues in their emails than they were prior to the redistricting, while those who lose bases reduce their mentions of military-related subjects. While members are not lying about their work in Congress, they are nonetheless painting a misleading picture of the scope of their efforts on behalf of district interests.

In my third paper, I show that, despite incentives not to mention other politicians, members of Congress do talk about their peers in DC in about 30 percent of their political communications. I claim this is a means of ideological signalling, where members cite others who share their ideological space. Additionally, I demonstrate through a series of survey experiments that the public makes reasoned judgments about the ideology of a member who talks about another politician. Members thus have the opportunity to shape how constituents view their representative through references to other politicians.

In these three papers, I show that members can use sometimes subtle techniques to influence their relationship with the district.

Thesis Supervisor: Charles Stewart III
Title: Kenan Sahin Distinguished Professor of Political Science
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Part I

Talking about Congress: The Limited Effect of Congressional Advertising on Congressional Approval
Mark Twain once famously quipped that congressmen are the only “distinctly native American criminal class.” To hear some congressional candidates talk, they also hold distinctly negative opinions of the legislature. Consider Iowa Senator Jim Leach, who noted in a television advertisement in 2002, “if there was one thing that I could change, it would be the moral fiber of Washington.” Florida Representative Vern Buchanan lamented in 2008 that “gas prices reach new highs, the border still not secure, spending out of control, and Congress does nothing.” South Carolina Senate candidate, Buddy Witherspoon, was more direct in 2008, stating, “Washington’s become the problem. I refuse to be a part of that system of corruption. Start with term limits, ethics, ban contributions from corporate lobbyists and big oil companies. It’s time for Americans to take back America.”1 Given what candidates appear to think of Congress, then, it is perhaps not surprising that approximately 85 percent of Americans disapprove of the way Congress is handling its job (Jones 2014).

Congressional researchers have long conjectured a direct link between criticism by members of Congress and low congressional approval (Fenno 1978; Parker 1981; Cook 1979; Patterson and Magleby 1992; Farnsworth 2003; Lipinski 2004). And political scientists are not the only ones concerned. A House Report from 1977 suggested that congressional approval would increase if only incumbents would stop attacking Congress (cited in Parker 1981). Ten years later, on the Senate floor, Wisconsin Senator William Proxmire, after noting that “no one and I mean nobody ever defends the Congress,” urged his colleagues to “stand up for this institution” (cited in Patterson and Caldeira 1990). It is now simply received wisdom that “members of Congress run for Congress by running against it” (Fenno 1978, p. 168). However, this particular claim has only rarely been put to an empirical test (c.f., Lipinski 2004) and no work to date has evaluated the effects of this electoral strategy on public opinion.

Using new data from congressional advertisements, I show that most of the expectations about the extent of and response to congressional criticism are not supported by the evidence. Congressional criticism is rare. Moreover, any effects of such criticism are lim-

1These are all direct quotes from advertisements obtained from the Wisconsin Advertising Project, which is described more below.
ited and ephemeral. Survey respondents living in media markets where many ads critical of Congress were aired are no less likely to express approval of Congress than respondents living in areas where no such ads were aired. These results are robust to a variety of model specifications and alternate data sources. I replicate these null findings using original experimental evidence, where neither an ad critiquing Congress nor one offering support for Congress influenced congressional approval relative to an ad that did not mention Congress.

Understanding what does or does not influence congressional approval is critical, as recent research has demonstrated that low approval of Congress can have wide-ranging consequences. Specifically, congressional approval influences congressional elections through attitudes toward the incumbent (Born 1990), majority party vote share (Jones and McDermott 2009), and changes in party seats, especially in key elections, such as 1994 or 2006 (Mann and Ornstein 1994; Hibbing and Theiss-Morse 1995; Hibbing and Tiritilli 1997; Jones and McDermott 2009; Jones and McDermott 2011). Second, low levels of congressional approval have also been found to change legislative behavior (Parker 1981; Cooper 1999; Dancey 2010). As one example of this, Lipinski (2004) surveyed members of Congress in the mid-1990s and found that a majority said it was harder to do their jobs during times of low approval, claiming that legislating was more difficult because of increased public apathy and reduced risk tolerance on the part of the representative. Third, interest in holding congressional office decreases when public opinion toward Congress is more negative (Fowler and McClure 1990; Maestas et al. 2006; Wolak 2007). Finally, Hetherington (1998) showed that an individual's trust in government more broadly was significantly linked to his approval of Congress.

In the following sections, I first review the literature surrounding the links between congressional criticism and congressional approval. Next, I describe new data on how candidates talk about Congress in their campaign ads, as well as the survey and experimental data I use to test claims about the role of congressional criticism. I then present results about what kinds of candidates are more likely to criticize Congress, as well as the effects such criticism has on congressional approval and for approval of the candidates themselves. I conclude by
exploring some of the implications of these findings for how we think about what is driving low congressional approval.

**Understanding the Received Wisdom: Running Against Congress**

Members of Congress run *for* Congress by running *against* Congress. The strategy is ubiquitous, addictive, cost-free and foolproof ... In the short run, everybody plays and nearly everybody wins. Yet the institution bleeds from 435 separate cuts. In the long run, therefore, somebody may lose. (Fenno 1978, p. 168)

The above quote from *Home Style* succinctly encapsulates theories of candidate behavior and the public reaction to it that are now deeply ingrained in how we think about campaigns. Specifically, Fenno makes three testable claims: (1) that all congressional candidates criticize Congress, (2) that they do so because it is effective electorally, and (3) that doing so has deleterious effects on how the public views Congress.

The only direct test of any of Fenno’s claims comes from Lipinski (2004), who analyzed franked mail from a sample of districts from 1991 to 1995.² He found that approximately 65 percent of members analyzed sent positive messages about Congress; only 22 percent sent critical messages. Positive messages, not surprisingly, came overwhelmingly from the majority. This finding is dramatically different from Fenno’s observation of “ubiquity,” but there are some reasons we might expect messages in franked mail to differ from those in public speeches. Most importantly, congressional rules mandate that franked mail cannot speak in partisan terms. When a member of the Democratic majority wants to claim credit for the passage of a key bill, therefore, she must do so in non-partisan terms. An easy way to do that is to frame the bill passage as an accomplishment of the Congress as a whole and hope informed constituents will make the appropriate connection to the majority party. In

²Franked mail is official correspondence from a member of Congress, signed by the member as postage in lieu of a stamp.
fact, 89 percent of members of Congress surveyed by Lipinski said they thought members’ franked mail often used "Congress" to refer to actions of the majority party. Even accounting for partisan motivations, however, Lipinski’s data show that members of the minority also frequently expressed support for Congress.

Fenno’s second contention is that criticizing Congress is electorally beneficial for candidates. Lipinski, Bianco, and Work (2003), using the same franked mail data, found that making positive statements about Congress (what the authors refer to as “loyalty”) increased the likelihood of defeat for Democratic incumbents in 1994, though institutional loyalty did not seem to impact Republican members’ electoral fortunes. Their data suggest that institutional disloyalty—or criticizing Congress in campaign ads—may lead to positive electoral outcomes. However, research on ads that attack an opponent would predict the reverse: Lau, Sigelman, and Rovner (2007) conducted a meta-analysis of over 100 studies on attack ads and found that the sponsor of such ads often faced a backlash from voters. The authors conclude that attacking one’s opponent is, in general, not a good campaign strategy.

Finally, Fenno predicts that criticism of Congress by its own members will depress public support for the institution. Many authors have taken Fenno’s conjecture at face value (Parker 1981; Cook 1979; Patterson and Magleby 1992). Others provide limited empirical evidence to support Fenno’s claims (Farnsworth 2003; Lipinski 2004). Several authors also point to the example of Newt Gingrich (Fried and Harris 2001; Mann and Ornstein 2012; Cook 1979) also suggests that his results lend empirical support to Fenno’s argument, though they do so far less directly than Lipinski’s tests.

While the literature uses a variety of terms to describe this kind of ad, including what might be ambiguous in this case—“negative”—I will always use the term “attack.” Ads that are negative toward Congress will generally, though not exclusively, be referred to as “critical.” Many of these studies suffer from endogeneity problems: a candidate down in the polls may be more likely to attack her opponent, but that does not mean the attack caused approval of the ad sponsor to fall. I take several steps, described below, to ameliorate similar concerns in this project.

There are many possible ways to measure such support for Congress; Hibbing and Theiss-Morse (1998) explore the variations in “approval” for Congress that come from differently-phrased questions. Throughout this work, I look specifically at the question “Do you approve or disapprove of the way the US Congress is handling its job?” This question (or slight variants) has been asked more than 600 times since the 1970s (data collected by Durr, Gilmour, and Wolbrecht 1997; Ramirez 2013; and myself).
Elving 1994). In the late-1980s and early-1990s, Gingrich led a concerted effort to depress congressional approval as a means of indicting the long-standing Democratic majority. Although no studies provide any causal evidence, these authors link the effort of Gingrich and his colleagues to the decreased congressional approval of the early 1990s. David Broder extends this argument, claiming that Republicans were able to avoid losing their new-found majority in 1996 by moderating their campaign strategy to boost congressional approval (cited in Jones and McDermott 2009).

Additional evidence that attacks on Congress may lead to lower public approval of Congress can also be found in the literature on attack ads. Lau, Sigelman, and Rovner’s (2007) meta-analysis finds that attitudes toward the target of an attack ad fall slightly. Moreover, while many of the studies analyzed by Lau and his colleagues had results that pointed in opposite directions (some showing positive effects, others showing equally sizable negative effects), one area in which the findings were overwhelmingly negative (if perhaps substantively small) was in terms of people’s trust in or satisfaction with government. Among 40 studies that evaluated opinions toward the political system, 33 report negative effects of attack ads. Both of these findings suggest that ads critical of Congress should indeed lead to more negative attitudes toward the legislature.

Harbridge and Malhotra’s (2011) well-designed experimental research on attitudes toward bipartisanship in Congress also lends credence to the idea that negative information about Congress can depress public attitudes. Their survey informed respondents about the level of bipartisanship during a recent session of Congress, either in bill introduction (low) or bill passage (high). When respondents were then asked about their confidence in Congress, those exposed to the bill introduction treatment were significantly less confident than those in the bill passage group.

As mentioned above, however, attitudes toward the sponsor of the ad fall even more, which is why the authors claim this is not a good campaign strategy.
To summarize the literature, while Fenno predicted criticizing Congress was a “ubiquitous” and “foolproof” strategy, the evidence to date suggests we might find that such critical ads are neither ubiquitous nor foolproof. In his analysis, Lipinski (2004) showed members were three times as likely to be loyal to Congress than to criticize it, though he and his colleagues did find that criticizing Congress was the better strategy electorally. The negative advertising literature, on the other hand, finds that members who attack their opponents do so at their own peril, a pattern that may extend to members who criticize Congress. Finally, while the empirical evidence is quite limited, it all appears to confirm Fenno’s conjecture that criticizing Congress depresses congressional approval.

This paper aims to directly evaluate who criticizes Congress through their television advertising and whether such criticism influences opinions toward the ad sponsor or toward Congress. Using a wealth of data on congressional communication and public opinion, as well as experimental results to bolster the findings, I am able to address many of the shortcomings of the research to date. First, the reach of advertising is wider than franked mail and the content of such ads is less strictly monitored by the Federal Election Commission than franked mail is by the Franking Commission. By developing new data on how advertisements talk about Congress, I am now able to use this much richer dataset of congressional communications. Second, the combination of observational and experimental results on the impact of criticism on electoral success is broader (in that it covers more candidates) and more nuanced than previous findings. Third, the recent increase in interest in attitudes toward Congress has made it possible to bring data to bear on these questions. In years past, there simply was not enough survey data on congressional approval to parse the relationship between criticism and approval. These observational results are also enhanced by experimental results testing an explicitly causal claim about the role of congressional criticism.
Data and Methods

Evaluating the extent and effect of congressional criticism requires data on congressional advertising as well as survey data on attitudes toward Congress. I use advertising data compiled by the University of Wisconsin Advertising Project for the campaigns in 2000, 2002, 2004, and 2008.8 These data include “storyboards” of each ad, which provide a complete transcript as well as images from every fourth second of the ad. The data track every instance that an ad was aired, including information on the date(s) and media market(s) in which the ad was broadcast.9 Additionally, the research team at Wisconsin compiled a variety of information about the content and sponsor of each advertisement. Unfortunately, the data do not include information on incumbent status or who ultimately won the election. To account for this, I merged Wisconsin’s data with information from the Federal Election Commission (FEC) about the candidates in each race.10

The work done to date on negative advertising has focused on criticism of the opposing candidate rather than attacks on other important political objects, such as the parties or Congress.11 The Wisconsin team helpfully rated each ad as to whether it promoted a sponsor, attacked an opponent, or contrasted the candidates; however, my data are the first to rate ads on whether they criticize Congress. Each of more than 10,000 ads aired in these campaigns is classified as to whether it mentions Congress and, if so, whether the comments are negative, positive, or neutral. I worked with coders on Amazon’s Mechanical Turk and a similar

8 This is a project of the University of Wisconsin Advertising Project that includes media tracking data from TNSMI/Campaign Media Analysis Group in Washington, D.C. The opinions expressed in this article are those of the author and do not necessarily reflect the views of the University of Wisconsin Advertising Project (Goldstein, Franz, and Ridout 2002; Goldstein and Rivlin 2005; Goldstein and Rivlin 2007; Goldstein, Niebler, et al. 2011).
9 In 2000, the data only cover the 75 largest markets; in 2002 and 2004, the data include the 100 largest markets; the 2008 data cover all media markets.
10 In a few cases, candidates aired ads without filing with the FEC, but in these cases, given the strict FEC requirements, it was obvious that these candidates were neither incumbents nor winning candidates and so the ultimate coding was relatively straight-forward.
11 Though not used in this paper, I also rated all ads as to how they discuss the political parties.
website, Elance, to make these classifications. While it is not always easy to determine how to code the candidates’ statements, I had 85 percent inter-rater reliability as to both whether Congress was mentioned and how the candidate discussed the institution. For more information on how I developed this measure, as well as text from some example advertisements, see Appendix A.

I also use data on public opinion toward Congress from the 2008 National Annenberg Election Study (NAES). The NAES study was implemented using a rolling cross-section design, which allows researchers to examine the election dynamics over the course of the survey period. Moreover, the data from this election are particularly appropriate for my work as congressional approval was asked throughout the survey period (from January through November 2008). Because of the rolling cross-sectional design, I can treat the date of interview as a random variable itself (Johnston and Brady 2002) and evaluate how respondents react to the campaign as it is happening, including how they react to any campaign ads that are critical of Congress. Overall, approval of Congress fell during the course of 2008, as

12For example, in 2008, Kentucky Representative John Yarmuth aired the following ad:

I’m John Yarmuth. To me, public service is about helping people, listening to their concerns and making their lives better. Over the past two years, I’ve been to more than a thousand community meetings and events to hear from you how we can get our country moving again. You said college was too expensive, so I helped pass the biggest increase in financial aid in more than 60 years. You wanted us to help families afford health care so I fought to pass the largest expansion of children’s health insurance in more than a decade and we doubled the child tax credit. And you felt Congress had not done enough to honor our veterans, so we passed the largest increase in veterans health care in history and a new GI bill giving our vets free college tuition. I also secured 45 million for our new VA hospital. Now I’m helping develop alternative energy, fighting to tap our oil reserves and increase production to reduce gas prices. I’m John Yarmuth and I approve this ad because I want to keep moving us forward, listening to you, taking your fight to Washington and getting results for Louisville families.

This ad discusses legislative successes in Congress, but does so only with an eye to the specific role Yarmuth played. It also criticizes Congress’ role in honoring war veterans and indicates that Yarmuth needs to “fight” in Washington. The ad was ultimately rated as speaking neutrally about Congress but, like many other ads, determining an appropriate rating was not straight-forward.

13In previous years, the NAES asked about confidence in Congress, but only during limited, pre-election, periods.
Figure 1-1: Approval of Congress throughout 2008 Election.

This figure shows average daily approval of Congress throughout 2008 (dotted line), along with average weekly approval (solid line). National approval of Congress fell from an average of 27 percent in February 2008 to 16 percent in October 2008.

In the main model below, I identify the ads aired by any House or Senate candidate in the survey respondent’s media market in the two weeks prior to the date the survey took place. Media markets and congressional districts do not neatly align, so in many cases, respondents may be seeing ads for candidates running in different districts or even different states. I discuss several models below, and provide further robustness checks and alternate models in Appendix B. In all cases, the results are largely similar to those reported below.

In addition to these observational results, I also ran a survey experiment to assess more explicitly causal claims about the effects of criticism of Congress. I created three mock ads, based on text from actual ads in the Wisconsin database: one ad criticized Congress, one ad
made positive statements about Congress, and the third ad did not mention Congress. The two treatment ads were designed to be as similar as possible in both visuals and language. Specifically, I identified ads with content that was positive toward Congress and borrowed language from several to create the positive ad. I then reversed the tone of all statements that implied support of Congress to create the negative ad. The video clips used in both were identical, though not necessarily in the same order. The control ad text was taken almost verbatim from a real ad, aired in 2002, and features some unique video clips. While one campaign ad may be insufficient to influence attitudes, the Harbridge and Malhotra (2011) results suggest that even a short statement about Congress can substantively change confidence in Congress.

After viewing a randomly-assigned ad, respondents were asked about their likelihood of voting for the candidate appearing in the ad as well as their judgment about whether the candidate was honest, knowledgeable, or “someone like you.” Later in the survey, respondents were asked about their approval of Congress. The survey was administered to a diverse national sample by Survey Sampling International to 1,023 respondents; however, 52 respondents indicated they had trouble viewing the advertisements and were dropped from the analysis, giving a functional sample size of 971.

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14 See Appendix A for more information about how I created these ads.
15 I gratefully acknowledge the support of MIT’s Political Experiments Research Lab in funding this experiment.
16 Respondents were also asked two manipulation check questions: whether the candidate was an incumbent (he was portrayed as such) and what policy areas were mentioned in the ad (there was no explicit policy content). Although respondents were not very accurate in these manipulation checks, the results shown below are robust to including only those who answered the questions correctly.
17 SSI recruits participants through various online communities, social networks, and website ads. SSI makes efforts to recruit hard-to-reach groups, such as ethnic minorities and seniors. These potential participants are then screened and invited into the panel. When deploying a particular survey, SSI randomly selects panel participants for survey invitations. The survey did not employ quotas but asked SSI to recruit a target population that matched the (18 and over) census population on education, gender, age, geography, and income (based on pre-measured profile characteristics of the respondents). The resulting sample is not a probability sample, but is a diverse national sample.
Results

Who Criticizes Congress?

Criticizing Congress is easy, and may be an effective way for representatives to connect with their constituents, but how often does it happen? Rarely. In advertising data spanning four elections, only nine percent of ads even talk about Congress. Furthermore, not all of these ads are critical of the legislature. Seventy-eight percent of ads that mention Congress do so in a critical way, but many (19 percent) are neutral and some (3 percent) are even supportive of Congress. Looking at the 1701 candidates in my dataset, only 31 percent, or 525, of candidates air any ads that mention Congress; twenty-two percent (118) of these candidates only speak about Congress in a neutral or positive way. This leaves only 407 candidates across four elections who criticize Congress in at least one of their campaign ads.

If congressional criticism is not ubiquitous, who is more likely to criticize? Figure 1-2 shows the percent of ads aired by candidates that are critical of Congress. Challengers are much more likely to criticize Congress than are incumbents (panel a). On average, 11 percent of challengers ads are critical of Congress, while only 4 percent of incumbents’ ads were critical, a statistically significant difference. This accords with findings in the literature on attack ads, where challengers are also more likely to “go negative” (Kaid 2004). Other results in the left column show that more extreme candidates (panel b) and candidates who ultimately lose their elections (panel c) are also more likely to criticize Congress. However, both these results are driven by the fact that challengers are more likely to be extreme and

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18 Technically, these are candidate-election observations, as I have not merged candidate data across elections. However, there are relatively few candidates who advertised in multiple years, so the results are unlikely to be substantively different were I to take this additional step.

19 Throughout this paper, I will use a standard p-value of less than 0.05 to indicate statistical significance.

20 I measure extremity here by a CF score (Bonica 2013) outside the [-1,1] range, a criteria that includes about a third of the candidates in my dataset. CF scores are preferable to the more commonly-used DW-NOMINATE (Poole and Rosenthal 2000) given that they are also calculated for challengers who never enter Congress. Nonetheless, results are comparable when DW-NOMINATE is used instead.
to lose. Once I control for incumbency, these differences disappear.\textsuperscript{21}

Data in the right column show that there are no statistically significant differences between Senators and Representatives (panel d), nor are there differences by whether the candidates’ party is in the majority or minority in Congress (panel e).\textsuperscript{22} This finding runs contrary to Lipinski (2004), who found the minority party was much more likely to criticize Congress in their franked mail messages. The difference likely relates to constraints on partisanship in franked mail—the minority is not permitted to explicitly criticize the opposing party in franked mail and so uses “Congress” as a stand-in for the majority party. In television advertising, no such constraints exist.

Finally, there are large and statistically significant differences across election (panel f). Specifically, in the early 2000s, when public approval of Congress was relatively high (around 40 percent approved of the job the Congress was doing), few candidates criticized Congress in their advertisements. By 2008, however, congressional approval dropped; as figure 1-1 showed, approval was around 27 percent in February 2008 and had fallen to 16 percent by Election Day in November. In this election, nearly 15 percent of ads aired were critical of Congress, as opposed to an average of 5 percent of ads in the previous elections. Fenno (1978) believed that candidates criticized Congress as a means to connect with his constituents. If this is indeed the reasoning behind congressional criticism, then it is not surprising to find additional criticism being levied at Congress when more of a member’s district is likely to disapprove of the job they are doing.

\textsuperscript{21}These data do not support any causal claims about the effectiveness (or lack thereof) of ads that criticize Congress. As with research on attack ads, it may be that candidates resort to criticizing Congress when their campaigns are not going well. The fact that these same candidates go on to lose is not indicative of any effect of their strategy to criticize Congress. The experimental results below are better able to identify this causal relationship, at least for incumbent candidates.

\textsuperscript{22}I did not include data from 2002 in this last analysis, as the parties each controlled one of the two chambers. The analysis also drops independent candidates.
Figure 1-2: Percent of Ads that Criticize Congress.

This panels in this figure show comparisons of what percent of ads aired by different types of candidates were critical of Congress. Panel (a) compares incumbents and challengers; panel (b) compares ideologically extreme candidates to more moderate candidates; panel (c) compares candidates who ultimately won their election to those who lost; panel (d) compares members running for the House and Senate; panel (e) compares candidates in the majority and minority party; and panel (f) compares candidates running in the elections of 2000, 2002, 2004 and 2008. Differences in panels (a), (b), (c), and (f) are statistically significant, though differences in panels (b) and (c) are driven by incumbency status. Once incumbency is held constant, there are no differences in ads across ideology or electoral success.

Source: Analysis of Wisconsin Advertising Project data.
Does Running Against Congress Improve a Candidate's Electoral Prospects?

The observational evidence in panel (c) of figure 1-2 suggests that congressional criticism is not a winning strategy, but these data are not suited to make such causal claims. Instead, I turn to my experimental results. After viewing one of three ads—one that criticized Congress, one that supported Congress, and a control ad that did not refer to Congress—respondents answered questions about their probability of voting for the candidate and their impressions of the candidate’s characteristics. The results in figure 1-3 show that congressional criticism has very little impact on the public’s perception of the ad sponsor. This figure graphs coefficients and confidence intervals from regressions of the stated dependent variable (standardized) on dummy variables for the two treatment conditions. Because the dependent variables are standardized, the results should be interpreted as how many standard deviations the dependent variable changes when the respondent viewed a treatment ad relative to the control ad.

Panel (a) of figure 1-3 shows that viewing an ad critical of Congress made respondents no more or less likely to support the candidate relative to viewing the control ad. In fact, the critical ad only influenced perceptions of candidate honesty, where respondents who saw this ad were 0.25 standard deviations more likely to perceive the ad sponsor as honest relative to those who saw the control ad. These experimental findings lend credence to the observational results: running ads critical of Congress is unlikely to change a candidate’s electoral fortunes.

On the other hand, the results in figure 1-3 caution against supporting Congress. Respondents who saw the supportive ad were 0.18 standard deviations less likely to vote for the ad sponsor. This result seems primarily due to a similarly large decrease in respondents’ impressions that the candidate was “like them,” and lends credence to Fenno’s (1978) as-

\[23\text{The sense that these two variables are linked that is provided by figure 1-3 is born out in a regression of vote for candidate on the three candidate characteristics. While all have statistically significant effects on}\]
Figure 1-3: Effects of Congressional Criticism or Support.

The panels in this figure show experimental treatment effects (and 95 percent confidence intervals) on several variables related to the ad sponsor, where all comparisons are to the control ad. The top line in each panel is for the ad supportive of Congress; the bottom line is for the ad critical of Congress. The dependent variables in each panel have been standardized, so effects can be interpreted as the standard deviation change, given that the respondent saw the supportive (critical) ad relative to if the same respondent had viewed the control ad. Panel (a) shows effects on likelihood of voting for the candidate; panel (b) shows effects on impressions that the candidate is someone like you; panel (c) shows effects on impressions of the candidate’s knowledge; and panel (d) shows impressions of the candidate’s honesty.

Source: Experimental results.
sertion that supporting Congress could drive a wedge between candidates and their district. It also echoes Lipinski’s (2004) findings that institutional loyalty leads to electoral defeat.

Does Congressional Criticism Influence Congressional Approval?

The results thus far show that congressional criticism is far from ubiquitous and that it is unlikely to be foolproof. But does congressional criticism play a role in depressing people’s attitudes toward Congress? Looking at a basic comparison between survey respondents who were potentially exposed to any ads critical of Congress in the past two weeks and those who were not, it appears that negative advertising has a substantively and statistically strong effect, reducing congressional approval by approximately four percentage points. However, this result is driven primarily by the fact that respondents were more likely to see such ads during the later months of the election, just as approval of Congress was dropping.24 Including fixed effects for month eliminates any statistical or substantive relationship between the airing of negative advertisements and respondent congressional approval.25

Figure 1-4 shows that these null results are robust to a more complicated model. This figure shows coefficients and 95 percent confidence intervals for all demographic, political, and ad-related variables in my primary model, where the dependent variable is a binary measure of congressional approval. The independent variable of interest is the log of the number of ads critical of Congress that were aired in the survey respondent’s media market in the two weeks prior to the date of interview. In this regression, I control for a range of demographic and political variables known to correlate with congressional approval, including

probability of voting for the candidate, the relationship between vote and impressions that the candidate is “like you” is the strongest (both substantively and statistically).

24While approximately seven percent of NAES respondents interviewed prior to September were potentially exposed to ads critical of Congress, approximately 26 percent of respondents in September could have seen such ads, and nearly half of respondents interviewed in October had such ads aired in their media market.

25It is plausible that including fixed effects is inappropriate in this case—perhaps congressional approval is falling because of an increase in critical ads during this period. However, Appendix B presents results of an alternate analysis using a difference-in-difference design, where I also find null results. Because the difference-in-difference approach tracks “treated” and “control” units separately throughout the election, I can separate the general pattern of falling approval from the specific effects of airing critical ads.
presidential approval (Lebo 2008; Abramowitz February 11, 2010) and political knowledge (Mondak et al. 2007). I also control for media-related variables, such as the number of hours of television watched the previous evening and the total number of congressional campaign ads aired in the media market during the previous two weeks (also logged). Finally, the regression includes linear time trends for each media market (though these coefficients are not shown in the figure). All continuous variables have been standardized.

As Figure 1-4 shows, many variables behave as we would predict. Presidential approval is the strongest positive correlate with congressional approval (Lebo 2008). Additionally, as Mondak et al. (2007) and others have shown, when political knowledge increases, congressional approval falls. Democrats are much more likely to approve of a legislative body entirely controlled by Democrats (as it was in 2008) than are Independents or Republicans. While these expected relationships are all born out by this analysis, the coefficient on the variable of interest, “number of ads criticizing Congress (log)” is substantively small and statistically insignificant. There does not appear to be a relationship between critical advertisements and low congressional approval.

It may be, however, that congressional criticism only has an effect if viewers are subjected to a large number of such ads. The data shown in figure 1-5 investigates this possibility. This figure plots the coefficients and 95 percent confidence intervals for several alternate variables of interest. The top line in the figure provides the coefficient and confidence interval from the full model shown in figure 1-4 as a point of comparison. The next two lines show the results where, in place of the logged number of critical ads, I use dummy variables indicating whether (respectively) 200 or 500 critical ads were aired in the respondents’ media market in the previous two weeks. As the figure shows, both variables are statistically insignificant, though the coefficient on “more than 500 ads” is noticeably larger. Specifically, the coefficient (-0.025) is somewhat more negative than the coefficient on having some college or more in

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26Specifically, I include fixed effects for month and media market, as well as the interaction between the two.
Figure 1-4: What Varies with Congressional Approval?

This figure plots the coefficients and 95 percent confidence intervals from a regression of congressional approval on a wide range of potentially relevant independent variables. All continuous variables have been standardized. While not shown in the figure, the regression also includes linear time trends by media market. The variable of interest, number of ads criticizing Congress (logged), is emphasized in the figure.

Source: Analysis of Wisconsin Advertising Project and National Annenberg Election Study data.
the main regression (-0.017), though that coefficient was statistically significant.

**Figure 1-5: Alternate Dependent Variables.**

This figure shows coefficients and 95 percent confidence intervals from regressions where I varied the dependent variable of interest, with all other model specifications equivalent to figure 1-4. The top line replicates the results from the original model for reference. The second and third lines use a dummy variable indicating whether there were more than 200 or more than 500 critical ads aired in the media market in the previous two weeks. The fourth line uses the log of the number of critical ads aired by incumbent candidates only. Finally, the final line uses the log of the number of critical ads aired in the media market by candidates running in districts other than the respondent’s congressional district. In all cases, the effects are statistically insignificant.

![Diagram showing coefficients and 95% confidence intervals for alternate dependent variables.](source: Analysis of Wisconsin Advertising Project and National Annenberg Election Study data.)

The fourth line in figure 1-5 evaluates whether ads from different sources have more or less influence on public opinion. Specifically, constituents might expect challengers to criticize Congress as a means of demonstrating their superiority to the sitting candidate, and thus not react to their critique. Incumbents, on the other hand, are part of Congress and might be an unexpected source of criticism. Research has shown that people put more
stock in claims made by unexpected sources (DiFonzo and Bordia 2007) and, therefore, the effects of incumbent criticism may be greater. As members of the institution in question, moreover, the statements of incumbents might also be seen as more credible (Birnbaum and Stegner 1979). These data, though, show no differences in the effects of incumbent ads on congressional approval relative to ads aired by any candidates. Both effects are statistically insignificant and substantively small.\textsuperscript{27}

Finally, the final line in figure 1-5 attempts to correct for endogeneity in my estimates. It may be that candidates running in districts that are relatively more critical of Congress are more likely to air ads that criticize the legislature. Therefore, were I to observe a relationship between congressional criticism and low congressional approval, the direction of causation would actually be running from public attitudes toward critical advertising, rather than the direction I hypothesize. To account for this possibility, I exploit the fact that many media markets cover several congressional districts and that respondents living in these districts will view ads directed toward constituents in another district. The regression in the final line of figure 1-5, then, uses as a dependent variable the (logged) number of ads critical of Congress that were aired by candidates running in districts other than the respondent’s. The coefficient on this variable is almost exactly the same size as in the original regression and remains statistically insignificant.

Appendix B contains a variety of additional robustness checks and alternative measures, all of which show results of similar size and significance.

Observational results cannot necessarily identify a causal relationship between congressional criticism and congressional approval. However, I complement these findings with my experimental results. Because respondents were randomly assigned to view an ad critical of Congress, as opposed to an ad that is supportive or one that does not mention Congress, I

\textsuperscript{27}This regression depicted in the fourth line of figure 1-5 includes a control both for the total number of ads aired in the last two weeks, as well as the total number of ads from incumbent candidates; both were small and statistically insignificant but had some small effects on the coefficient on critical advertising.
can attribute any differences in congressional approval to viewing the critical ad. Figure 1-6 shows that, as with the observational results, viewing an ad critical of Congress does not influence congressional approval. Similarly, there is no effect of viewing the ad supportive of Congress. These results may be an indication that congressional approval is so low that an ad criticizing Congress cannot push it lower. However, only a third of respondents selected the most extreme negative attitude toward Congress and congressional approval does vary as a function of other variables. For example, respondents who indicated they would be likely to vote for the candidate in the ad were a statistically significantly 10 percentage points more likely to approve of Congress. It seems that congressional approval did move in ways we would expect, but that the advertising message about Congress may not have a particularly strong effect on approval.

Conclusion

The evidence presented in this paper is, to my knowledge, the first rigorous test of Fenno’s (1978) oft-repeated claims about congressional criticism. His wide-ranging insights into how members of Congress communicate with their constituents included observations that (1) all candidates criticize Congress, (2) this is an effective electoral strategy designed to connect a candidate with his constituents, and (3) this criticism causes public attitudes toward Congress to fall. I find that few candidates criticize Congress, a strategy that does not help them come Election Day, and that these messages have little to no effect on attitudes toward Congress. On the other hand, Fenno’s suggestion that congressional criticism serves as a way for candidates to establish connections with his constituents appears to be born out by the experimental results. In particular, candidates who criticize Congress may be seen as more similar to their constituents than those who support Congress, a key valence advantage during a time when many view Washington, DC, as geographically and psychologically distant.
Figure 1-6: Effects of Congressional Criticism or Support on Congressional Approval.

This figure shows experimental treatment effects (and 95 percent confidence intervals) of viewing an ad critical or supportive of Congress on congressional approval, where the comparison is to the control ad that did not mention Congress. The top line represents the effect of viewing the ad supportive of Congress; the bottom line is for the ad critical of Congress. The congressional approval variable has been standardized, so effects can be interpreted as the standard deviation change, given that the respondent saw the supportive (critical) ad relative to if the same respondent had viewed the control ad.

Source: Experimental results.
These results, though compelling, leave some open questions. First, are my results time-bound? Perhaps there may have been an impact of congressional criticism when Fenno did his research in the 1970s. But, since then, approval has fallen so far or criticism has become so common-place that the causal relationship has faded. This is unlikely, as congressional criticism appears to be, if anything, rarer than when Fenno did his work and, moreover, congressional approval has not linearly decreased since Fenno’s time. Indeed, the late 1990s and early 2000s saw some of the highest levels of congressional approval in the history of the time series.

Second, what are the differences in the effect of congressional criticism across mode of contact? It is notable that Fenno (1978), Lipinski (2004), and myself all evaluated different types of communication: stump speeches, franked mail, and television advertising, respectively. If, as the findings demonstrate, the likelihood of congressional criticism varies by mode, do people’s reactions to such criticism also vary? Also, do these disparate findings illustrate a larger pattern of stylistic differences across mode of communication? Political science research to date (understandably) has tended to focus on congressional communication via one outlet, such as television advertising (Lau, Sigelman, and Rovner 2007), press releases (Grimmer 2013), twitter feeds (Golbeck, Grimes, and Rogers 2010), or websites (Druckman, Kifer, and Parkin 2009). Perhaps we need to start evaluating whether and how communication strategies vary across these media.

Third, what distinguishes my experimental results from those of Harbridge and Malhotra (2011)? Why did they find statistically- and substantively-significant effects on confidence in Congress while I have null results? There are several key differences between our experiments. The message content—personal opinion in the case of my work and factual in the case of Harbridge and Malhotra—likely influenced the effects. Knowing that one more person dislikes Congress may not provide information that causes people to update their attitudes toward Congress in the same way that statistics about bipartisanship could. Similarly, the message
source may be important. Harbridge and Malhotra do not provide an explicit source cue, perhaps encouraging respondents to view the (presumably well-informed) survey researcher as the source. In my experiment, the ad is narrated by the purported candidate, who may be seen as less trustworthy than an academic researcher.

The fact that I find no substantive impact of congressional criticism on congressional approval over the course of a variety of empirical tests suggests the need for additional scholarly focus on the causes of low congressional approval and how, if at all, members can influence opinions of their institution. An easy answer in the past was that members should cease criticizing Congress, but it appears that will have little influence on public opinion. Perhaps Fenno (1978) may have an answer: he notes that members spend a lot of time educating their constituents about their own behavior but will not waste political capital on educating the public about Congress. The literature on congressional approval has coalesced on the idea that people dislike Congress largely because they do not understand the complex inner-workings of the legislature (Durr, Gilmour, and Wolbrecht 1997; Davidson 1999; Mann and Ornstein 2006). Hibbing (2002) concludes his aptly-titled article, “How to Make Congress Popular,” by noting

the most promising approach to making Congress popular may be to work at convincing people that the policy decisions made in the legislature are important and that political conflict often occurs because not all ordinary people care about the same policy issues and have the same preferences for solutions to policy problem.

If members of Congress did a better job explaining the hows and whys of legislative behavior, the theory goes, the public would have a better appreciation for the legislature.

However, my results, in combination with Lipinski’s (2004) earlier findings, suggest members of Congress are not likely to be willing to engage their constituents in this manner. If
supporting Congress (even implicitly through education efforts) lowers your chances of victory come election day, few members will agree to do so. As countless articles have noted (Mayhew 1974b; Elving 1994; Hibbing 2002), there is a collective action problem for Congress and no clear leader to hold members accountable for their behavior or to provide inducements to support Congress (Olson 1965). Moreover, Mondak et al. (2007) takes a common finding—that the more politically knowledgeable tend to like Congress less—to its logical conclusion by calling into question the education campaign that other scholars propose. If more knowledge leads to less trust in the legislature, will Congress be served by spending political capital to educate the public? These open questions suggest a continued need for research into what motivates public distrust in the legislature and how members (or others) may be able to affect what people think about Congress.

28 The party-based collective action solutions identified by Cox and McCubbins (2007) may not work for Congress as a whole given that the goals of the legislature may be conflict with those of the party.
Part II

Representing Their Former District: Do Members Do It and Do They Admit It?
When Nebraska redrew its congressional district lines after the 2010 Census, Congressman Jeff Fortenberry’s constituency expanded to include Orfutt Air Force Base, headquarters of US Strategic Command and home to more than 9,000 military personnel. In the wake of this change, Fortenberry increased how often he discussed military issues. Specifically, while about five percent of his emails to his constituents were related to the military in the fall of 2010, by the fall of 2012, nearly ten percent of his content focused on military-related subjects. This could be an example of a member changing his priorities to reflect a changed district. However, Fortenberry only changed his communication patterns; across several measures, Fortenberry’s activity in Congress did not appreciably change. Moreover, this story of Jeff Fortenberry reflects a broader pattern I find across all members and several district changes: legislative behavior remains fixed but the messages members send do shift.

This finding is important for two related reasons, the first of which highlights a problem and the second helps explain why the problem persists. First, although voters may be able to identify members who adequately represent their interests in open elections, the high incumbent re-election rate (approximately 95 percent of House incumbents won re-election in 2014) means that these members may continue to secure re-election even when district interests change. Second, members may be able to maintain their seat through misrepresentation of their efforts and activities in Congress, a normatively troubling finding.

This paper explores these questions of representation through the specific lens of changes to the military presence in a member of Congress’ district. In the following sections, I first provide some background on what our expectations are for members’ responsiveness to changes in their district. Then, I explain why military bases provide a uniquely well-suited case to study responsiveness. Next, I describe the diverse array of data I collected on legislative behavior and military bases to test my expectations, as well as explain the particular method that allows me to claim any changes I see are causally related to changes in military presence. Finally, I present my results and conclude.
Theories of Responsiveness

The quality of dyadic representation is typically measured by comparing district-level opinion to member votes in Congress. This tradition dates to Miller and Stokes (1963), who found relatively little alignment between district preferences and the votes their representatives made, especially on non-salient issues. As our ability to measure district opinion has improved over the ensuing years, political scientists have continued to judge representation by this metric (see, for example, Achen 1978; Bafumi and Herron 2010; Tausanovitch and Warshaw 2013).

While this approach is important, there are some key questions these studies typically cannot address. First, if there is a representational link between district opinion and member behavior, is this accomplished through electing the “right” members or because members can adjust to their districts’ changing needs? Second, if the representational link is found to be lacking, what explains this disconnect?

The textbook answer to the first question is that representation occurs through both avenues: conversion and selection. On the one hand, theory supports the idea that incumbents should adjust as their districts change. Downs (1957), Mayhew (1974a) and Fenno (1978) are among the first to advance the idea that members will cater to the needs of their districts as means of securing re-election. This idea is also reflected in the vast literature on “candidate-centered” politics, where members defy the party line so as to best accommodate the demands of their district. Mansbridge (2003) formalizes this idea as “anticipatory” representation, in which members vote in such a way that anticipates their districts preferences so as to fend off electoral competition. Finally, Stimson, MacKuen, and Erikson (1995) provide robust evidence showing that members maintain their elected office through “rational anticipation,” though these findings are at the aggregate level rather than looking

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29 For a good summary of this argument, see (Jacobson 1992, ch.2)
at individual members’ changes in response to their own districts.

On the other hand, in the selection view, districts elect someone who supports their interests. According to this theory, if those interests change and the member is unable to adjust, the district will elect someone new. Perhaps the best evidence that selection alone is what drives any representational link we find is Poole and Rosenthal’s (2000) famous declaration that members “die in their ideological boots” (100).

As for the second question—what drives any representational disconnect—I suggest that members face particular challenges in changing their behavior to reflect a changing district. In some cases, they are unwilling or unable to change. Unlike Mayhew’s (1974) simplifying assumption that members are only motivated by re-election, Fenno (1973) notes that members have many other goals, including congressional power and making good public policy. These goals might be particularly relevant for legislative behaviors that are not measured by roll call votes, though it can apply there as well. For example, a member might be reluctant to give up a senior position on the Armed Services Committee just because their district is no longer home to a large base, as this position endows the member with extra influence within the chamber and access to campaign contributions. Alternatively, a member motivated by an interest in education or environmental policy may not be able to easily shift their portfolios of committee service or interest group affiliations when a military base is suddenly added to the district.

To evaluate the strength of members’ motivations—specifically, whether re-election trumps other considerations—requires more than simply a cross-sectional analysis. To understand whether and how representatives are legislatively responsive to the changing array of material interests in their districts, we must consider data over time: whether members change with their districts. However, as the challenges I outline below suggest, it can be surprisingly hard to credibly test this question.

First, the change in district preferences or objective needs must be measured appropri-
ately. Glazer and Robbins (1985), one of the first studies to directly address this question, used presidential vote share of districts before and after redistricting. However, this is a notoriously fuzzy measure of district preferences (Levendusky, Pope, and Jackman 2008; Kernell 2009). Setting aside concerns about error in this measure, presidential vote share can only provide a generalized proxy for constituent ideology, rather than any specific policy preferences held by the district. Thus, it is unlikely to provide a powerful statistical level with which to address the question of responsiveness. More recent work employs some more sophisticated measures, such as using GIS and Census data to calculate the percent of new constituents in a district (Bertelli and Carson 2011), but this assumes the new constituents differ in their opinions from the previous constituents. The authors still use presidential vote share to identify changes in opinions. A related line of work may better reflect changes in objective district conditions by considering the change to racial composition in the district (Bullock 1995; Leveaux-Sharpe 2001).

Second, the change in member behavior must also be adequately measured. The studies to date overwhelmingly rely on roll call records to evaluate changes to member behavior. While this is certainly important—it is, after all, the most highly visible form of consequential legislative activity—there are reasons to be suspicious of findings that rely solely on voting records. Most critically, voting decisions are overdetermined, in that so many different (though often overlapping) considerations are relevant to this binary choice (Kingdon 1989; Hall and Wayman 1990). It could be hard to determine whether changes (or a lack thereof) on roll call measures can be attributed to district changes, especially as the influence of the party has increased over recent decades. Another reason to be critical of studies looking only at voting measures is that this comprises only a small portion of the activities undertaken by members of Congress. By the time a matter comes to a formal vote, other legislative

30But see also Caughey and Warshaw (2015), who find that the correlation between ideology and vote share is increasing in recent elections.
activity, most notably committee action and party leadership intervention, will have molded
the substance of what is being voted on. In other words, the agenda-setting process has a
strong influence on the substance of legislation, much of which is lost by focusing solely on
roll call votes.

Finally, we must be sure members recognize the changes to their district if we expect
them to respond—and if we expect to be able to measure such a response. Fenno (1978,
p. 6) comically observes a member consulting a map to determine whether a particular
neighborhood is even in his district! As candidates and campaigns become increasingly
sophisticated, this particular kind of uncertainty may be unlikely. But Leveaux-Sharpe
(2001) notes that members only responded to changes in the racial composition of their
districts when the change was particularly sizable. Other than quantifying the size of relevant
changes, other work in this area (for example, Glazer and Robbins 1985; Bertelli and Carson
2011) has not demonstrated that members are aware of changes to their districts.

My paper addresses all these concerns. With regard to measures of a changing district,
I follow the lead of scholars who looked at more objective changes to the material interests
of the district. I identify how the number of military bases in a district changes as a result
of both redistricting and decisions by the Department of Defense to close or realign bases.
The next section describes in more detail how base closure decisions are made and why this
constitutes a good measure for my purposes. With regard to member behavior, I evaluate
not only roll call records (specifically on defense-related issues) but also a variety of other
metrics, including committee membership and bill sponsorship, among others. This allows
me to draw more comprehensive conclusions about the effects of changes to the district
on member activity in Congress. Finally, I show evidence that members are likely aware
of the changes to their districts by looking at how their communication with their district
changes. This is particularly important as it highlights that even when members adjust their
communication, they do not adjust their behavior.

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Base Closures as Exogenous Changes

To properly attribute any changes we see in member behavior to changes in the member’s district, I need to show that the changes were exogenous. Specifically, it cannot be that member characteristics or behavior prior to base changes led to the changes. Redistricting, which is carried out by state legislatures and not Congress itself, provides one source of plausibly exogenous change to the composition of the members’ districts. Although incumbent members certainly try to influence the process of redistricting, legislatures are often pursuing multiple, possibly incompatible, goals—such as incumbent protection or party support—and may not be able to honor all requests for particular district lines (Gelman and King 1994).

Military base closures provide another plausibly exogenous shock to members’ districts. Congress created the Base Realignment and Closure (BRAC) process in 1988 to address what Goren (2003) referred to as “a prime example of parochial concerns superseding broader considerations over an extended period of time” (10). In the decade preceding the first BRAC round, the Department of Defense had been unable to close a single military installation due to Congressional stonewalling. No member of Congress wanted to be seen as responsible for the closure of a base in their district, something sure to cause at least short-term economic harm to the region. However, by 1988, politicians were increasingly concerned by the ballooning federal deficit and the geopolitical situation had altered significantly as a consequence of the increasingly crumbling Soviet Union (Sorenson 2007). There was a general recognition that base closures were necessary but Congress needed a way to isolate itself from these potentially fraught decisions (Arnold 1992).

31 The Government Accountability Office (GAO) estimated that in 4 BRAC rounds, approximately 120,000 civilian jobs were lost. In the longer-term, however, the same report suggested that more than 70 percent of these jobs were replaced through efforts to convert the bases to other uses (GAO-05-138). That said, Sorenson (2007) notes that through the “multiplier effect” bases spur many more indirect jobs than just those identified by the military. For example, he estimated that New Jersey’s bases accounted for 43,000 direct jobs and an additional 90,000 indirect jobs. The GAO report did not explicitly address these possible economic impacts, though they did find that unemployment in the areas surrounding closed bases was lower than the national average and growth in these areas was higher than the national average.
The BRAC process—implemented in 1988, 1991, 1993, 1995, and 2005—nice accomplishes the goals of base closure with limited congressional fallout. Although the particular details have varied over time, the essential steps are as follows: (1) the Department of Defense proposes a list of bases to be closed or “realigned” based on clearly articulated criteria; (2) a commission appointed by the President and confirmed by the Senate evaluates the list, generally by visiting bases slated for closure and holding public hearings to consider non-military perspectives; (3) the President and Congress each make all-or-nothing decisions on the final list forwarded by the BRAC commission (Sorenson 2007). Because Congress can only vote yay or nay on the list as a whole, this process allows members to place additional weight on the overall national military interests as opposed to specific local concerns.

This is not to say that members do not try to influence BRAC decisions. Several possible avenues for preventing the loss of a local base are available (Sorenson 2007; Goren 2003; Sorenson 1998). First, Congress can refuse to authorize new rounds of BRAC. Scholars note the 1995 BRAC round was particularly politicized and contentious (Sorenson 1998), for example, which may explain why it took a further decade to initiate another BRAC evaluation. Second, members can nominate commissioners likely to support the cause of particular bases. Third, members can pass other defense-related legislation which would tie the hands of the commission with regards to particular bases or military functions. Fourth, members can direct new investments to a base, which would increase the economic value of the base, a key consideration for closure. Finally, members can directly lobby the BRAC commission to alter the Department of Defense’s proposed list. While there is anecdotal evidence of influence over BRAC decisions, Sorenson (2007) notes that the evidence of congressional tampering is “mixed at best” (54).

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32Sorenson (2007) defines realignment as when a base loses or gains a particular military role or function, but other military activities continue at the base.
33GAO reviewed the criteria used in 1995 and 2005 and identified several general components, including military value, costs and savings expected from closure, and the economic and environmental impact on the nearby community (GAO-13-149).
Given the relatively indirect influence members of Congress have over the base closure process, then, it provides a second source of mostly exogenous change to the military presence in a district. Between these decisions and the redistricting process, members of Congress can find themselves needing to represent district interests they did not choose or anticipate. How they respond to this new reality is critical for understanding member responsiveness.

**Data and Methods**

I assembled a broad array of data both on legislative behavior related to the defense industry and on military bases across the country. In collecting data on legislative behavior, I tried to be as inclusive as possible while recognizing that some data are not available over a reasonable time horizon. In the end, most of the data I collect span the period from the 93rd Congress to the 113th Congress. Trying to measure behavior consistently prior to this period would be challenging as it is an era of rapidly changing Congressional rules and norms. However, this period (1973-2013) spans four redistricting sessions and five rounds of base closures by the military, giving me ample variation in the number of bases any member has in her purview.

Below, I provide a brief overview of the data collected and, in Appendix C, I provide additional details on how I measured each relevant variable, as well as the sources of my data.

The specific measures of legislative behavior I collected include, first, roll call records for members’ votes on military issues. Voting is one of the most obvious and well-publicized activities that members undertake in Congress, but as mentioned, can be overdetermined. As explained more fully in Appendix C, the measure I use is pro-military rank, relative to other members of Congress.

Second, I looked at membership on the Armed Services Committee.\(^{34}\) Due to relatively complicated committee transfer rules as well as the privileges of committee seniority, I expect

\(^{34}\)Or, as it was called during the 104th to 108th Congress, the National Security committee.
limited change on this measure compared to others included here.

Third, I identified the number of bills on military issues sponsored and cosponsored by each member during each session. Although the results presented below use the raw number of bills, the findings are unchanged if I use the percent of bills sponsored or cosponsored so as to better account for variation in total bill sponsorship behavior. Since most bills do not advance very far in the legislative process, this might be an opportunity for legislative “cheap talk.” This is particularly true for bill cosponsorship, which typically requires little effort on the part of the signatory. As such, I would expect more changes on these measures.

Fourth, as a measure of perceived expertise and legislative influence, I obtained data on PAC donations from the defense industry. As with bill sponsor and cosponsor data, I present information on raw donations, but note that results are unchanged if I substitute the percent of donations coming from the defense industry. While PAC donations are not necessarily driven by the legislator herself, members do solicit funds from industries and, moreover, this might be one of the best proxies for expectations of legislative behavior. PACs are unlikely to invest in members who will not support their causes (Hall and Wayman 1990; Tripathi, Ansolabehere, and Snyder Jr 2002).

Finally, to estimate how members of Congress communicate with their constituents about their activities in Congress, I use data on the emails sent by members to their constituents during the 2010-2012 redistricting period (Goodman et al. 2015). This new data source provides a view into what members think their constituents will be interested in learning about the member’s time in office. Members vary significantly in the types of emails they send. For example, Michael Capuano (D-MA) presents information on how he voted on most bills, along with tables showing how Democrats, Republicans, and other Massachusetts’ delegates voted. Another member, Jim Cooper (D-TN), sends links to his favorite 10 news stories from the week. However, most provide some insight into the members’ activities in Congress as well as any events or meetings they have had with people from the district.
Table 2-1: Legislative Behavior Descriptive Statistics.

Summary statistics describing the empirical range of the outcome variables in my dataset. For all analyses, however, the data were rescaled to range between 0 and 1 so that effect sizes could be compared across measures.

<table>
<thead>
<tr>
<th>Measure</th>
<th>Minimum</th>
<th>Median</th>
<th>Maximum</th>
<th>Mean</th>
<th>Std. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>defense roll call record</td>
<td>1.00</td>
<td>215.00</td>
<td>435.00</td>
<td>215.44</td>
<td>124.83</td>
</tr>
<tr>
<td>armed services committee</td>
<td>0.00</td>
<td>0.00</td>
<td>1.00</td>
<td>0.12</td>
<td>0.33</td>
</tr>
<tr>
<td>sponsor legislation</td>
<td>0.00</td>
<td>0.00</td>
<td>59.00</td>
<td>0.73</td>
<td>1.94</td>
</tr>
<tr>
<td>cosponsor legislation</td>
<td>0.00</td>
<td>7.00</td>
<td>64.00</td>
<td>8.42</td>
<td>7.30</td>
</tr>
<tr>
<td>donations from defense PACs</td>
<td>0.00</td>
<td>9,000.00</td>
<td>314,000.00</td>
<td>19,940.88</td>
<td>28,522.72</td>
</tr>
<tr>
<td>emails on military issues</td>
<td>0.00</td>
<td>0.02</td>
<td>0.19</td>
<td>0.02</td>
<td>0.02</td>
</tr>
</tbody>
</table>

Source: Various sources, as described in Appendix C.

Given the wide discretion members have over what they communicate in these emails, I consider this a particularly good view of what members think their constituents’ interests are.

Table 2-1 shows the distribution of these outcome variables in my data. In the analyses below, I scaled all legislative behavior variables to run between 0 and 1 so that effect sizes could be compared across measures.\(^{35}\)

I pair these data on legislative behavior with information on the number of military bases contained within a members’ district in each session. This requires data not only on currently operating bases, but also those from more than 40 years ago (the span of my data on legislative behavior). Figure 2-1 shows the location of all bases in my dataset, where closed bases are represented by a black, open circle and open bases are represented by a grey, filled-in circle.

Moreover, because it is not typically the base itself driving constituent interest in military issues, but the people employed at the base, I also collected data on base personnel throughout this period (including military, civilian, and contract employees). While many bases have no personnel associated with them, a few have tens of thousands. In the results below, I present two specifications to account for this disparity. First, I identify the total

\(^{35}\)I also tested models where I normalized the variables to be mean 0 with a standard deviation of 1. The results are substantively and statistically similar to those presented in the next section.
Figure 2-1: Location of Open and Closed Military Bases.

This map shows the location of all open (grey filled-in circles) and closed (black, open circles) military bases in my data. There are currently bases located in all states and approximately 60 percent of congressional districts.

Source: Department of Defense GIS data and other sources; see Appendix C for more information on data sources.
number of bases in operation during a particular congressional session. Then, I also note the number of bases with more than 10,000 personnel. While about half of the bases have less than 500 personnel, about eight percent of bases have more than 10,000 staff.

About 60 percent of members in my dataset have a base of any size in their district, while about 12 percent of members have a base with more than 10,000 personnel. Conditional on having a base of any size in the district, the mean number of bases is 2.7. As would be expected given the BRAC process, the average number of bases in a district has fallen over time from approximately 2.9 in the 1970s to 2.5 currently.

In the final analyses, I use data from approximately 9,200 member-congress observations, though not all data span the entire breadth of my time frame, which reduces the sample size for some analyses.

Methods Used

As described above, and like much of the previous literature on how members react to changes in their district, I rely here on the quasi-exogenous changes affiliated with redistricting but also incorporate the additional quasi-exogenous changes associated with base closures. Given the way my data are structured, for the majority of my analyses, I use a panel fixed effects model, regressing the outcome of interest on the number of bases in the district, with fixed effects for congressional session and member:

\[ Y_{it} = \alpha_i + \beta \text{bases}_{it} + \gamma_1 \text{mem}_i + \gamma_2 \text{congress}_t + \epsilon_{it} \]

36 There are many other possible measures, such as the total number of military and civilian personnel employed in a district (both logged and unlogged) or the number of anticipated bases, taking into account planned base closures. While I do not show these results here, they are substantively similar and available on request. I also tested other personnel cut-offs: bases with more than 500, 1,000, or 5,000 personnel. Results are equivalent regardless of personnel size. There are other possible cut-offs, but given the relative imprecision of the personnel data, I hesitate to put much stock in more refined measures.

37 Across all members, not conditioning on having any base in the district, the average number of bases per district has also fallen over time, from 1.7 to 1.4.

38 The specific time frames for which data are available are described in Appendix C.
where $Y_{it}$ is an outcome of interest, such as membership on the Armed Services Committee; $\text{bases}_{it}$ is the number of bases in the district of member $i$ during Congress $t$, alternately measured by the number of large bases; and $\text{mem}_i$ and $\text{congress}_i$ are fixed effects for member and session, respectively.\textsuperscript{39} Assuming any change in the number of bases is exogenous (specifically, that the error term is unrelated to the number of bases), I can interpret the coefficient on $\text{bases}_{it}$ to be a causal effect of a change in the number of bases on the outcome of interest.

In addition to this approach, I also tested my theory with a variety of other methodological approaches and found substantively similar results.\textsuperscript{40} These results are available upon request.

**Results**

I begin by demonstrating that members with bases in their districts differ along important behavioral measures from members without districts. The top panel of Table 2-2 shows that members with bases exhibit more pro-military voting records, are more likely to serve on the Armed Services Committee, and are more likely to sponsor military-related legislation than are members without bases. The bottom panel of the table shows that these differences become more stark when I consider members who have bases with more than 10,000 personnel.

However, this type of cross-sectional analysis cannot adjudicate the question of whether

\textsuperscript{39}I do not include any additional covariates for two reasons. First, most typical covariates (e.g., party) are not time-varying. By including fixed effects for member, most of these covariates are already accounted for. Second, some covariates that do change across congressional sessions (e.g., ideology) should be considered post-treatment.

\textsuperscript{40}Two specific additional tests are worth mentioning here. First, I tested a model that included $\text{bases}_{it}^2$ to account for possible non-linear effects of additional bases in the district. The results showed this variable was statistically insignificant in all but one regression (where it remained substantively small). For simplicity, then, I do not include it here. Second, I tested a model with clustered standard errors to help address concerns of the heteroskedastic error structure evident in panel data. Although the confidence intervals expanded slightly in most regressions, the changes were subtle enough—particularly for results already largely null—that I proceeded with the more straight-forward results here.
Table 2-2: Legislative Behavior for Members with and without Bases.

Results from t-tests showing that members with bases in their districts behave differently in Congress than do members without bases in their districts. The differences are both larger and more statistically significant when considering differences between members with large bases (more than 10,000 personnel) compared to members without large bases (who still may have smaller bases in their districts). All behavioral measures are rescaled to range from 0 to 1 and are defined as described in Appendix C.

<table>
<thead>
<tr>
<th></th>
<th>members with bases</th>
<th>members without bases</th>
<th>difference in means</th>
</tr>
</thead>
<tbody>
<tr>
<td>defense roll call record</td>
<td>0.521</td>
<td>0.454</td>
<td>0.066*</td>
</tr>
<tr>
<td>armed services committee</td>
<td>0.169</td>
<td>0.056</td>
<td>0.112*</td>
</tr>
<tr>
<td>sponsor legislation</td>
<td>0.014</td>
<td>0.010</td>
<td>0.003*</td>
</tr>
<tr>
<td>cosponsor legislation</td>
<td>0.131</td>
<td>0.132</td>
<td>-0.001</td>
</tr>
<tr>
<td>donations from defense PACs</td>
<td>0.055</td>
<td>0.040</td>
<td>0.015*</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>members with large bases</th>
<th>members without large bases</th>
<th>difference in means</th>
</tr>
</thead>
<tbody>
<tr>
<td>defense roll call record</td>
<td>0.582</td>
<td>0.482</td>
<td>0.100*</td>
</tr>
<tr>
<td>armed services committee</td>
<td>0.371</td>
<td>0.090</td>
<td>0.280*</td>
</tr>
<tr>
<td>sponsor legislation</td>
<td>0.025</td>
<td>0.011</td>
<td>0.015*</td>
</tr>
<tr>
<td>cosponsor legislation</td>
<td>0.145</td>
<td>0.130</td>
<td>0.015*</td>
</tr>
<tr>
<td>donations from defense PACs</td>
<td>0.106</td>
<td>0.042</td>
<td>0.065*</td>
</tr>
</tbody>
</table>

*p < 0.5

Source: Various sources, as described in Appendix C.

this association is causal, and which way the causality runs. To work out the causal connection, we need to examine how members behave when faced with changes to their district.

Panel Fixed Effects Results

Figure 2-2 shows the results of separate regressions where the dependent variable (stated legislative behavior) is indicated on the y-axis and the coefficient on the independent variable of interest (number of bases of a given size) is indicated by the circles (with the line representing the corresponding 95 percent confidence interval). The results indicate how behavior is expected to change given an addition of one base or one large base. For example, if the coefficient is 0.05, this would suggest that member activity in this metric increased 5 percent when a base was added to his district. (Alternatively, it also means that member activity fell by 5 percent after the closure or removal of a base from the district.) As these examples suggest, if we believe that members adjust their behavior to reflect changes in their districts,
we should see positive and statistically significant coefficients. However, across the majority of these regressions, I find statistically insignificant and substantively small changes.

**Figure 2-2: Legislative Behavior Changes after District Changes.**

The results shown here represent the parameter estimate and corresponding confidence interval on the coefficient of interest in 10 separate fixed effect regressions. The coefficient represents the behavioral change estimated from gaining one base (open circles) or gaining one large base (closed circles). For example, the top line in the graph shows that a member gaining one base with at least 10,000 personnel is expected to be approximately one percent more pro-defense, relative to her peers, on a 0 to 1 scale (this result is statistically insignificant). If members change to reflect the new situation in their district, we would expect positive coefficients (even if a member lost a base, the coefficient would still be positive because we would expect behavioral change in the opposite direction).

![Figure 2-2: Legislative Behavior Changes after District Changes.](image)

*Source: Various sources, as described in Appendix C.*

For example, in looking at the results for member cosponsorship behavior, the fourth set of lines in Figure 2-2 shows that gaining or losing a base of any size has no statistically significant effect (p<0.05). Indeed, the dark line, representing changes in the number of bases with 10,000 personnel is somewhat *negative*, though still insignificant. This is surprising given the expectation that members might be most inclined to signal support for military issues.
on these relatively low-importance activities.

Another surprising finding is that the results show some positive changes in terms of committee membership. Recall that complex rules and concerns about seniority led me to predict that members might be least likely to adjust their committee membership in the wake of changes to the district. However, this is the activity where I find the most positive results, though as I detail below, they remain substantively small.

In Figure 2-2, two of the regressions using larger bases feature positive coefficients that reach standard levels of statistical significance. While this is promising from the standpoint of representation via conversion, I caution that such changes are also quite rare. At the highest point, in 1989, only 10 percent of bases had more than 10,000 military or civilian personnel (throughout the time period, the exact number of such large bases ranged from 40 to 86). Although 12 percent of members in my data at one time represented a district with such a large base, only 6 percent of all members experienced a gain or loss of a base this size. I also note that members express concern about BRAC decisions even when the bases are not substantial. My data suggest that only 15 bases of this large size have ever been closed through BRAC. Looking specifically at the 2005 BRAC round, no bases were projected to lose 10,000 personnel and only three to lose more than 5,000. In contrast, two bases were expected to gain more than 11,000 staff through realignments (Department of Defense 2005).

Moreover, the statistically significant results may not reflect substantively interesting changes. The changes in Armed Service membership is about one-sixteenth of a standard deviation; the coefficient on bill sponsorship is relatively larger, but still represents only one-eighth of a standard deviation. Another way to compare these results is to look at the size of the coefficient relative to the cross-sectional results above. While a member with a base of any size is 11 percentage points more likely to serve on the Armed Service committee, a member who gains a base with 10,000 staff only becomes about 2 percentage points more likely to serve in this capacity. On this comparison, it does appear that bill sponsorship may
rise to the level of substantive significance, as the effect size (approximately 0.3 percentage points for a member who gains a base of 10,000 personnel) is roughly equivalent to the difference in means identified in Table 2-2 for members representing a base of any size, though still only about 20 percent of the observed difference between members representing large bases versus those without large bases.

**Communication Changes**

Perhaps my cautions about the relative rarity and substantively small changes are unfair to representatives. It may be that bases with fewer than 10,000 personnel are insignificant in a district that contains a few hundred thousand constituents. To test this, I look at whether members’ communication changes after the 2012 redistricting round.

Because my sample size is much smaller in this subset and, as mentioned, changes involving the largest bases are relatively rare, I use as my independent variable whether the member gained or lost a base with at least 5,000 personnel.\(^{41}\) I also use a different methodological specification, given that the data are structured somewhat differently than in the full data set. Here, I use a difference-in-differences (DID) regression, where I compare the change in behavior of members who did not experience a change in the number of bases in their district to those who either gained or lost a base.\(^{42}\) In the simplest DID regression, the treatment variable (change in bases) is interacted with a dummy variable indicating post-treatment status. In this case, post-treatment status is the members’ email style in the second half of 2012. The coefficient of interest is the interaction, which explicitly measures the comparison I am studying.

The top line in the left panel of figure 2-3 presents estimated change (and confidence interval) in the percent of member emails mentioning defense-related issues for members

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\(^{41}\)In the full data, no coefficient was statistically significant when bases was measured as the number of bases with at least 5,000 personnel. Those results are available upon request.

\(^{42}\)Specifically, I created a three-level variable, where a 0 indicates no change, a 1 indicates the member gained at least one base with at least 5,000 personnel, and a -1 indicates the member lost at least one such base.
who experienced a change in their district, relative to those who did not face any change to their district. Again, a positive coefficient would indicate that members emails reflected the change to the district. In this case, there is a positive and statistically significant coefficient, which is approximately the size of one standard deviation in the dataset. In other words, members emails to their constituents are notably different after a change in the presence of one military base.

**Figure 2-3: Email Communication Changes after District Changes.**

The results here come from difference-in-difference regressions where the outcome of interest is legislative behavior or communication (with the specific measure identified on the left) and the independent variable of interest is the number of bases in each district in each time period that had at least 5,000 personnel. Due to the availability of data on member emails, these data are restricted to the 2012 redistricting period. Moreover, because legislative bill topics have not been coded for the 113th Congress, I am unable to include data on roll call record or bill sponsorship and cosponsorship. Despite these limitations, the results in the full sample (left panel) clearly demonstrate that members change their email communication patterns but not their behavior in Congress. Further, the other two panels show that these results are driven by members who gain a base (middle panel), who show effect size nearly double that of the full sample. Members who lose bases (left panel) change neither their behavior nor their communication style.

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Only Members who Gained Bases</th>
<th>Only Members who Lost Bases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Armed Services Committee</td>
<td>-0.4 -0.2</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>PAC $</td>
<td>-0.4 -0.2</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>Email on Military Issues</td>
<td>-0.4 -0.2</td>
<td>0.0</td>
<td>0.0</td>
</tr>
</tbody>
</table>

*Source: Various sources, as described in Appendix C.*

As a comparison, I ran an equivalent DID model on the measures of legislative behavior that are available for the 2012 redistricting.\(^{43}\) These results appear below the email communication changes chart.

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\(^{43}\)Recall that bill topics are not available for legislation introduced in the 113th Congress, so roll call record, and bill sponsor or cosponsorship are not measured here. However, I can run DID models across the whole span of my data that do incorporate these measures. In no case do I find coefficients that rise to the level of statistical or substantive significance. Indeed, the coefficients are often negative, though still statistically significant.
munication results in figure 2-3. As you can see, neither are statistically significant and are both substantively much smaller.

It may be that members who lose bases are relatively averse to changing their behavior or their communication strategy. After all, national defense is a public good—even constituents who are not employed by the Department of Defense may be inclined to support measures that advance military interests. The middle and right panel of figure 2-3 illustrates that the email results I find above are driven by members who gain a base with at least 5,000 personnel. On the other hand, member behavior seems largely unchanged regardless of the direction of change to the district’s military presence.

Conclusion and Discussion

My results demonstrate that, at least in the domain of military bases, relatively sizable changes to objective district interests result in modest changes in legislative behavior in Congress—if there are any changes at all. This is true across a range of legislative behaviors, including voting decisions, committee membership, bill sponsor- and cosponsorship, and PAC donations. Although voters could react to this lack of responsiveness by voting in a new member (and my data cannot speak to this question), high incumbent re-election rates suggest that this may not be happening. In short, while we might be skeptical of the power of selection to replace misaligned representatives, conversion of sitting members does not seem to be ensuring representation for constituents either, at least on military issues.

While it is possible that constituents are unconcerned by the lack of responsiveness on the part of their member, the evidence I present on member communication strategies suggests that, at the least, members believe otherwise. Members appreciably change the content of their emails to constituents in the wake of changes to the district. Specifically, those members who gain a military base spend 20 percent more of their email content on military issues,
despite the fact that they are no more likely to serve on the relevant committee or obtain donations from relevant PACs. While these are perhaps some of the harder behaviors to change in the short-term, I note again that results on the other legislative behavior measures from previous redistricting periods do not show any statistically or substantively positive changes to member behavior. Members appear to be consciously hiding the fact that they are not addressing the interests of the district while in Congress.

There are many instances where we might praise members for taking on an advocacy role that does not strictly reflect their district’s interests. For example, members from non-majority-minority districts can nonetheless support civil rights legislation without criticism. Barney Frank also served as a surrogate representative for many gays and lesbians across the country for many years. However, my findings raise two concerns. First, that military-related behavior could be an example of a broader phenomenon, inclusive of issue areas where over-representation is more concerning. Second, that members are not communicating their legislative priorities to allow constituents to make an informed decision about how they want to be represented in Congress. Frank, for example, was openly gay and an outspoken advocate for gay rights—his constituents were not misled about his legislative activity in this arena. Perhaps members who do not change their behavior upon gaining a military base in their district would be equally supported by their constituents, but these constituents are misled about the legislative priorities of their members.

As a caveat to my results, I again note that this paper only looks at the relationship between members and their district across one policy area. To strengthen my conclusion that members of Congress do not change their behavior, another “case” would be helpful. However, this analysis depends on having interests that are geographically constrained, such that events such as redistricting could lead to relatively large changes in district preferences. Unfortunately, few other geographically-located interests exist that exert similar weight on member behavior. Moreover, this case is particularly useful given the additional quasi-
exogenous change through the BRAC process. Nonetheless, some of the areas that could be fruitful to study include prisons, ports, tribal reservations, or manufacturing facilities. For example, the closing of auto or steel plants in Michigan and Pennsylvania, respectively, could change the interests of the relevant congressional districts. Or, similarly, the opening of major auto plants in the south might influence legislative behavior.
Part III

Politicians as Positions: Citing Others as a Cue to Ideology
David Mayhew's classic *Congress: The Electoral Connection* (1974) describes a world in which members of Congress employ a variety of strategies to create an individual relationship with their constituents. He spends some time outlining why members do not want to be affiliated with the collective Congress or even the fortunes of their own party. Important political science work since Mayhew has also taken this atomistic relationship as given. For example, Fiorina (1989) claims members of Congress create intentionally vague laws in order to later help constituents navigate the Washington bureaucracy. The underlying goal in this vision is to provide individualized service to the district. A less pessimistic view comes from Cox and McCubbins (2007), who argue that leaders must work constantly to ensure members at least occasionally go against their individual (district) interest to support the party brand. Again, an explicit assumption in this work is that members are primarily interested in forming one-on-one relationships with their district. The question constituents appear to be asking is this: what has my member of Congress—as an individual—done for me lately?

And yet I find members' communication patterns are often at odds with the motivations classical political science attributes to them. For example, early in the 112th Congress, Representative Steve Womack (R-AR) sent an email to his district in which he congratulated the new Republican leadership team, including John Boehner (R-OH) and Eric Cantor (R-VA), among others. This email draws attention away from Womack, and provides no new information about his efforts on behalf of the district or his opinions on policies that would benefit the district. On the other hand, it creates a clear association between Womack and other known politicians. Across all my data, approximately 32 percent of member communications mention other politicians. Crafting an individualized relationship seems incompatible with talking about the efforts of other politicians; why, then, do members do this?

This paper argues that citing other politicians is a way of ideological position taking.
Mayhew (1974a) claimed one of the primary activities of members of Congress was to stake out policy positions that would align with the interests of their district. Although he is not explicit on this point, it is clear from his description that these policy positions tend to be non-ideological, such as an urban politician supporting public transportation funding. I expand this vision to assume that politicians also have an incentive to stake out ideological ground and that, as such, members talk about others who share their ideological space or the image they wish to present. For Womack, who was one of the more moderate Republicans in the 112th Congress, his email is a chance to highlight his commitment to Republican leadership and connect him to that wing of the party. As another example, Representative Al Wynn (D-MD) sent an email in 2008 about his participation in a health care event that also featured Senator Kent Conrad (D-ND). There is no evident political gain to be made by mentioning Conrad’s participation in the event, but by doing so, Wynn connects himself to a Senator with an almost identical ideological position as his own.44

It should be noted that citing another politician is a relatively subtle ideological statement. However, in today’s highly partisan political environment, it may be that even subtle cues about members’ ideology can be publicly interpreted. With a clear distinction between parties (Hetherington 2001; Bartels 2000; Fiorina, Abrams, and Pope 2005), it is easier for members of the public to draw reasonable conclusions about a member’s partisanship and ideology based on whether they talk about a Democratic or Republican leader. Indeed, I run a series of experiments that show respondents making, in some cases, quite nuanced conclusions about the ideology of members who cite well-known politicians.

In the sections that follow, I first make the argument that citing other politicians is a strategic and meaningful decision. Next, I describe the data used in this research—a dataset which includes emails and press releases from all Representatives and Senators spanning more than four congressional sessions. I then discuss what these data reveal about the ways

44Wynn’s DW-Nominate score during the 110th Congress was -0.32 while Conrad’s was -0.31.
in which members of Congress refer to other politicians. Following that, I show results from a series of survey experiments designed to evaluate how the public may be interpreting emails or other messages from their representatives which mention other politicians. Finally, I conclude by drawing out the implications of my findings for understanding the strategies behind members’ political communications.

Strategic Decisions in Political Communication

In service of re-election, members of Congress spend a lot of time and money crafting messages for their constituency. Fenno (1978) noted that members saw their relationships with their constituents as more important than the votes they take in Congress. What the members said in the district mattered enormously. Members continue to focus on communication with their districts 40 years later. Sulkin and Swigger (2008) note “given the care with which candidates decide about the issue content, tone, and emotional valence of the language used in their advertisements, we should expect that they would choose other features of their ads just as carefully” (233). Indeed, they find that even subtle signals, such as the race or occupation of “extras” in a campaign advertisement, are meaningfully related to the work a member subsequently undertakes in Congress.

Grimmer (2013) finds that members are able to be strategic about what they convey to their constituents because so much activity happens in Washington, DC. It would not be possible to convey all on-going activity, so members are able to be selective. While Grimmer shows that members are unlikely to tell untruths about their work on the Hill, he also demonstrates that “lies of omission” in member press releases are quite common. Other research has shown members selectively discuss their policy positions in campaign advertisements and constituent emails in order to portray themselves as more moderate than their full record suggests (Cormack 2013; Henderson 2013).

Although most legislative activity depends on collaboration with other politicians, the
findings of Grimmer and others suggest that members could selectively omit any discussion of the work of their colleagues. Moreover, the incentives to do so are quite clear. Thinking in terms of Mayhew’s (1974) three re-election related activities—advertising, credit claiming, and position taking—it is not immediately clear what benefit would accrue to the member by talking about fellow politicians. The efforts of another person provides advertising for them but not for the member. Similarly, if a member mentions another person in conjunction with a legislative achievement, the message explicitly allocates some of the credit to that person, diluting the credit the member could otherwise claim. Finally, in most cases, there is no explicit policy position being taken by talking about another politician. 45

Schiller’s (2000) book on U.S. Senators makes this point clearly. In her telling, senators are constantly battling for media attention and must stake out differentiated platforms so as to avoid being lumped together with their same state colleague. In fact, her data show that, across several states, a minority of news stories (from 9 to 18 percent) mention both state Senators. The majority focus on the activities of one or the other. Grimmer (2013) showed that media coverage of a Senator closely replicates the Senator’s own press releases, so it is likely that the patterns uncovered by Schiller are reflective of the Senators’ preferences with regard to sharing media coverage.

While it may seem like the incentives are aligned against politicians citing their peers, there are a few reasons members might employ this tactic. First, members could use this strategy to imply a closeness to other powerful politicians. While endorsements from such powerful figures might be hard to attain, a member could discuss her work with Mitch McConnell (R-KY) or Paul Ryan (R-WI) to demonstrate proximity to the venues of power in Congress. 46 Second, members may be bolstering their claim for credit over a particular

45There are some politicians who become synonymous with particular policy positions, such as Barney Frank’s (D-MA) advocacy for gay rights. In this case, it may be that even mentioning Representative Frank would imply support for gay marriage.

46One piece of evidence in support of this contention is that while more than 1600 different politicians are cited in my data, nearly 20 percent mention the top 20 names. These “popular” politicians include presidents
legislative achievement by sharing the glory with other politicians. For example, a member could note that he co-sponsored a bill that benefits the district; mentioning the bill's sponsor or additional co-sponsors might increase the credibility that the member contributed meaningfully to the bill's passage.\footnote{While not precisely evidence of this pattern, I found many instances where members listed all of their colleagues that supported a bill or that co-signed a letter to the bureaucracy. In trying to quantify this impression, however, I found that only 7 percent of emails or press releases contain more than 5 citations to other politicians.} Finally, and the reason that will be explored in depth here, members may want to signal something about their partisanship or ideology by talking about other politicians.

As described in the introduction, I am proposing an expanded vision of Mayhew's position-taking. While my data cannot speak directly to changes over time, there are a few reasons this kind of activity may have been less common when Mayhew was writing. First, during a time of low partisan identification, many members may not have wanted to send a partisan signal to their constituents. And second, given the partisan ambiguity cultivated by many politicians of this period, the public may not have been able to accurately interpret a citation of all but the most well-known politicians.\footnote{I am not trying to argue that the citizenry is more knowledgeable today, but that for a given number of well-known politicians, increased partisanship has made it more likely that the public has a sense of their partisan affiliation. ANES data show, for example, that more people are accurately able to label the Republican party as conservative—from a low of 51 percent in 1970 to a high of 73 percent in 2012.} Now, however, a member highlighting her work with Rand Paul (R-KY) sends a message that can be interpreted by her constituents. Because this member would be free to not mention Senator Paul in her political correspondence, doing so is a strategic decision that should be evaluated.

This is not the first work to assume citations are a meaningful expression of member ideology. Groseclose and Milyo (2005) use citations of think tanks by members of Congress and by news media to place the media outlets on the same ideological scale as the members. The authors' assumption in this work is that both members and media cite think tanks that

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\footnote{I am not trying to argue that the citizenry is more knowledgeable today, but that for a given number of well-known politicians, increased partisanship has made it more likely that the public has a sense of their partisan affiliation. ANES data show, for example, that more people are accurately able to label the Republican party as conservative—from a low of 51 percent in 1970 to a high of 73 percent in 2012.}
share their policy positions (and therefore the underlying ideology). More recent work looks at observable connections in social media (such as Facebook or Twitter followers) to estimate ideology (Bond and Messing 2015; Barbera et al. 2015). Barbera et al. (2015) note that models of this sort “assume that the positions of individuals in an unobserved social space can be inferred on the basis of observed connections among them” (3), which is the assumption I also make in this research. By publicly linking themselves to a particular politician, I argue, members are making a statement that can be interpreted by their constituents.

Citation Database

Data for this work comes primarily from emails and press releases compiled by Justin Grimmer and his colleagues (Grimmer 2013; Grimmer, Westwood, and Messing 2014; Goodman et al. 2015). The data span 2004 to 2012 and contain political communications from both House and Senate members. Summary statistics on these data appear in table D1 in Appendix D.

I complemented these data with information on elected political figures at the federal, state, and local level who served during this period. Because some of these figures go by different names (e.g., Joe or Joseph Biden), I also identified the most common alternate names for each politician. I used this list to automatically search through the political communication data for references to known political figures. Note that this method has a few drawbacks. First, it may miss important political figures, such as cabinet members who oversee a contentious decision or non-mayoral local officials, like heads of public transportation organizations. Second, it will inflate the citations of political figures with common

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49 A closer analog to this research would be mapping a Twitter network based on retweets, but I was unable to find any work that makes use of this approach.

50 Specifically, I included the names of all Presidential and Vice Presidential candidates, all serving House and Senate members, all Governors and other major state-wide office-holders, state house and senate leadership, and the mayors of the 200 largest cities. In all, I collected data on more than 2,000 officials serving in approximately 20 different offices.

51 If the political figure did not use a common nickname, I did not include it (e.g., Christopher Cox (R-CA) does not go by Chris).
names, such as Tim Johnson (R-IL and D-SD). Although these drawbacks are important to consider, this approach seems to do a good job of avoiding both false positives and false negatives.

I also collected a range of relevant data on members of Congress, including state, party, and ideology (as measured by Poole and Rosenthal’s DW-Nominate score). While I include these data for the author of the emails or press releases, I also make use of it when analyzing the citation in the communication, as nearly 95 percent of cites are to federal officials. This allows me to compare information about the author of the document to the political figure he or she cites. In addition to these observational data describing what members of Congress actually do in their political communications, I also ran two experiments designed to evaluate how the public may be responding to these subtle signals from their elected officials. I created mock emails from members of Congress that included references to particular politicians. After reading the email, respondents were asked a series of questions about the member sending the email. These experiments are described below and additional details are provided in Appendix E.

**Ideological Cueing through Citations**

Despite theoretical incentives to avoid mentioning other politicians, I find virtually all members cited another politician in at least one of their emails or press releases between 2004 and 2012. Not only do all members engage in this behavior, they do so relatively frequently: nearly one-third of emails and press releases contain a reference to another politician. Moreover, members cite, on average, 93 distinct politicians (the median is 82). There does not

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52 An alternative approach would be to use automated software designed to extract the names of people or places from text. In practice, I found the software identified too many citations to be useful, with a large number of the cites unrelated to politics or even not people.

53 For example, although Tim Johnson is a common name, only 5 of more than 900 emails mentioning Tim Johnson were not referring to either the Representative from Illinois or the Senator from South Dakota.

54 Only eight members of Congress never cited another politician and, cumulatively, these members only sent 60 emails or press releases.
appear to be a meaningful difference in citation behavior across type of political communication, though Senators are more likely to cite other politicians in their press releases relative to their emails.  

Moreover, there are somewhat limited differences across parties. While Senate Democrats’ communications are more likely to cite others than are Senate Republicans, the difference is not large. There are no partisan differences in the House. Figure 3-1 illustrates that the lack of partisan differences largely extends to member ideology as well. This graphic shows the percent of each member’s communications that include a citation to another politician. I added lowess lines to the data for each party to illustrate the average number of communications that include a cite, given a particular ideological position. The only notable difference across the ideological spectrum is that moderate Democrats appear to cite other politicians at about two-thirds of the rate as do other members of Congress.

The biggest distinction is between House members and Senators. While Senators’ communications cite other politicians approximately 44 percent of the time, House members’ communications only include citations about 28 percent of the time. One plausible explanation for this difference is, perhaps, that Senators share a constituency with their same-state colleague. It might make sense for these two Senators to communicate jointly on a regular basis. However, as mentioned, Schiller (2000) illuminated the strong incentives to craft one’s own reputation separate from the other state Senator. More immediately, my data show that, on average, less than a third of Senator’s citations are to their same-state Senator. While this is sizable, House members are also quite likely to reference their state delegation,

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55 Forty percent of emails include a citation to another politician, while 44 percent of press releases do so. This is a statistically significant (p<0.05) but perhaps not meaningful difference.

56 Though, again, the four percentage point difference is statistically significant.

57 For another way of viewing these data, figure D1 in Appendix D shows a density plot identifying the ideology of the member sending each email or press release, separating out by party and whether the communication includes a citation. This also shows limited differences in ideology whether a communication includes a citation or not, though it appears that very liberal and very conservative Republicans may be somewhat less likely to cite others than do members of the center of the party.
Figure 3-1: Percent of Member Communications which Cite Other Politicians, by Ideology.

About one-third of all political communication cites another politician, regardless of ideology. The one exception is that moderate Democrats cite other politicians in approximately 20 percent of their communications.

Source: Data collected by Grimmer, Westwood, and Messing (2014) and Goodman et al. (2015) (hereafter referred to as “political communication dataset.”)
with an average of more than a quarter of citations to a member of the state's House delegation.\textsuperscript{58} Senators are also more likely to cite more distinct politicians—their communications cite an average of 118 different people while House members' communications cite 89 different people, on average (the medians are 115 and 74, respectively). I speculate, though cannot confirm, that the underlying collegiality of the Senate is driving these differences.

**Who Do Members Cite?**

The five most frequently cited politicians during this period (2004-2012) are Nancy Pelosi, Barack Obama, George Bush, Joe Lieberman, and Harry Reid. As table 3-1 makes clear, these citation statistics are driven by members of the House of Representatives, rather than Senators. Regardless, citations to these five leaders make up approximately 7.5 percent of all citations in my data, with the top 20 politicians comprising about 17 percent of the total.\textsuperscript{59}

Perhaps providing additional support for the idea that Senators cite others as a reflection of the collegiality of the chamber, the top citations for Senators are almost exclusively for other Senators (rather than to Presidents or House members). However, this trend is *not* reflected in the broader data. Across all citations, Senators are less likely to cite someone in the same chamber than are members of the House. Three-quarters of Senate citations are to other Senators; nearly 83 percent of House citations are to other House members.\textsuperscript{60}

Another trend illustrated by table 3-1 is that Republicans are more likely to cite Democratic leaders than Democrats are to cite Republican leaders. This is also true across the breadth of citations—figure 3-2 shows that communications authored by both extreme conservative Republicans (the most conservative quarter) and other Republicans are likely to cite Democrats. Specifically, the density plot for Republicans is bimodal—there are a lot of

\textsuperscript{58}This average understates the tendency among House members to some extent, as seven states have only one district and thus there is no opportunity for these members to cite a same-state colleague.

\textsuperscript{59}In all, I find members cite 1613 different politicians, of whom 884 are fellow members of Congress or presidential contenders. Although there are many different state and local politicians mentioned in these emails, they are not mentioned frequently. Only 6 percent of mentions are to non-federal officials.

\textsuperscript{60}This difference is statistically significant.
Table 3-1: Most Cited Politicians.

While presidential contenders and congressional leaders dominate citations overall, there are differences between parties and across chambers. Notably, Senators tend to cite their own colleagues more so than House members. Additionally, as figure 3-2 also makes clear, Republicans are more likely to cite someone from the opposite party than are Democrats (italicized names are from the opposite party).

<table>
<thead>
<tr>
<th>Rank</th>
<th>Overall</th>
<th>House Republicans</th>
<th>House Democrats</th>
<th>Senate Republicans</th>
<th>Senate Democrats</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Nancy Pelosi</td>
<td>Nancy Pelosi</td>
<td>Barack Obama</td>
<td>Mike DeWine</td>
<td>Joe Lieberman</td>
</tr>
<tr>
<td>2</td>
<td>Barack Obama</td>
<td>Barack Obama</td>
<td>Nancy Pelosi</td>
<td>Susan Collins</td>
<td>Chuck Schumer</td>
</tr>
<tr>
<td>3</td>
<td>George Bush</td>
<td>George Bush</td>
<td>George Bush</td>
<td>Olympia Snowe</td>
<td>Hillary Clinton</td>
</tr>
<tr>
<td>4</td>
<td>Joe Lieberman</td>
<td>John Boehner</td>
<td>Barney Frank</td>
<td>Harry Reid</td>
<td>George Bush</td>
</tr>
<tr>
<td>5</td>
<td>Harry Reid</td>
<td>Harry Reid</td>
<td>George Miller</td>
<td>Bill Frist</td>
<td>Ted Kennedy</td>
</tr>
<tr>
<td>6</td>
<td>Hillary Clinton</td>
<td>Paul Ryan</td>
<td>Henry Waxman</td>
<td>George Bush</td>
<td>Dick Durbin</td>
</tr>
<tr>
<td>7</td>
<td>Chuck Schumer</td>
<td>Tim Johnson</td>
<td>Blanche Lincoln</td>
<td>Jeff Bingaman</td>
<td>Susan Collins</td>
</tr>
<tr>
<td>8</td>
<td>Ted Kennedy</td>
<td>Eric Cantor</td>
<td>Mark Pryor</td>
<td>Joe Lieberman</td>
<td>Carl Levin</td>
</tr>
<tr>
<td>9</td>
<td>Susan Collins</td>
<td>Dennis Hastert</td>
<td>John Conyers Jr.</td>
<td>Saxby Chambliss</td>
<td>Frank Lautenberg</td>
</tr>
<tr>
<td>10</td>
<td>John Boehner</td>
<td>Roy Blunt</td>
<td>Steny Hoyer</td>
<td>Arlen Specter</td>
<td>Beau Biden</td>
</tr>
<tr>
<td>11</td>
<td>Olympia Snowe</td>
<td>Barney Frank</td>
<td>Ted Kennedy</td>
<td>Judd Gregg</td>
<td>Barack Obama</td>
</tr>
<tr>
<td>12</td>
<td>Dick Durbin</td>
<td>Duncan Hunter</td>
<td>Hillary Clinton</td>
<td>Lamar Alexander</td>
<td>Max Baucus</td>
</tr>
<tr>
<td>13</td>
<td>Blanche Lincoln</td>
<td>Steny Hoyer</td>
<td>Chuck Rangel</td>
<td>Johnny Isakson</td>
<td>Barbara Boxer</td>
</tr>
<tr>
<td>14</td>
<td>Mark Pryor</td>
<td>Hillary Clinton</td>
<td>Barbara Lee</td>
<td>Dianne Feinstein</td>
<td>Debbie Stabenow</td>
</tr>
<tr>
<td>15</td>
<td>Mike DeWine</td>
<td>Chuck Rangel</td>
<td>Carolyn Maloney</td>
<td>Ted Kennedy</td>
<td>John Kerry</td>
</tr>
<tr>
<td>16</td>
<td>Arlen Specter</td>
<td>Lamar Smith</td>
<td>Hilda Solis</td>
<td>John Sununu</td>
<td>Chris Dodd</td>
</tr>
<tr>
<td>17</td>
<td>John Kerry</td>
<td>Buck McKeon</td>
<td>Ed Markey</td>
<td>Barack Obama</td>
<td>Arlen Specter</td>
</tr>
<tr>
<td>18</td>
<td>Chris Dodd</td>
<td>John McCain</td>
<td>John Dingell</td>
<td>Max Baucus</td>
<td>Barbara Mikulski</td>
</tr>
<tr>
<td>19</td>
<td>Barbara Boxer</td>
<td>Mike Pence</td>
<td>Chuck Schumer</td>
<td>John McCain</td>
<td>Olympia Snowe</td>
</tr>
<tr>
<td>20</td>
<td>Ken Salazar</td>
<td>Peter King</td>
<td>Rosa DeLauro</td>
<td>Ken Salazar</td>
<td>Harry Reid</td>
</tr>
</tbody>
</table>

Source: Political communication dataset.
Figure 3-2: Ideology of Cited Politicians.

This figure shows the DW-Nominate score of the politicians cited by the extreme members of each party relative to the rest of their caucus. There are limited differences in the patterns of citations between moderate and extreme politicians in either party. However, Republicans are more likely to cite members of the opposite party than are Democrats.

Source: Political communication dataset.

citations to Republicans on the right of the figure but an almost equal number of citations to Democrats on the left of the figure. Communications from Democrats (extreme liberals or otherwise) are less likely to cite Republicans. This can be seen through the more unimodal shape of the Democrats' density curves. Overall, 40 percent of the citations that Republicans make are to Democrats, whereas only 30 percent of the citations of Democrats are to Republicans.
Figure 3-3: Relationship between Member and the Politician They Cite.

Members are likely to cite other politicians who are similar ideologically.

How Do Members Cite Others?

In some ways, table 3-1 provides a misleading picture of the general nature of members’ citation patterns, particularly for Republicans. Although opposition leaders are frequently cited, most citations are actually to politicians who are ideologically similar to the member. Figure 3-3 shows the relatively strong correlation between the ideology of the author of the communication and the politician cited—approximately 0.37.\textsuperscript{61}

The heatmap in figure 3-4 shows this relationship another way. In this figure, I divided

\textsuperscript{61}If you remove the top five most-cited politicians, the correlation increases to 0.42. The correlation between authors and the cited politician when only looking at citations to the top five politicians is actually negative, at approximately -0.08.
members of each party into thirds based on their ideology. For example, Democrats are divided into liberals, middle-of-the-road, and moderates. I then classified each citation based on the group of the communication author and the politician cited. The darker values in the heatmap show combinations of author-cite that are the most common. In these data, darker values fall along the diagonal, or where the author and the cite are in the same ideological category. Overall, 28 percent of citations are between authors and citations within the same category. If members were citing politicians at random, we would only see about 17 percent of citations with this characteristic.

Finally, you can also see this relationship reflected in figure 3-2. When citing a member of their own party, both extreme liberals and extreme conservatives are more likely to cite someone with a more extreme ideology (i.e., the density curves are shifted outward relative to the curves for the rest of their co-partisans). This pattern does not appear as evident for cross-party citations.

What these statistics and figures imply is that most citations are positively valenced. In other words, members discuss other politicians in supportive rather than critical ways. Indeed, in my review of the data, this is what I found. While partisan sniping happens, the more frequent citation is one demonstrating members of Congress working together. A communications staff for a Republican Senator told me that this kind of message was an important component of their communications, especially during times of low congressional

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62 I attempted to conduct an automated sentiment analysis on these data, but the method does not produce accurate results. Specifically, the most frequently used approach counts the number of “positive” and “negative” words in a text as a measure of valence. However, in my data, members will often say something like, “Senator X and I criticized Agency Y for their many failings.” The overall tone of this sentence is obviously negative and the sentiment analysis will identify “criticized” and “failings” as negative words. But the relationship implied between the author and Senator X is actually positive. This issue would also confound other approaches, such as a supervised learning method, whereby I classify a subset of texts and then use automated approaches to classify the remainder based on the use of similar words or phrases. In the end, I did not make use of any rating system, but having reviewed many of the citations, I can say members are much more likely to mention another politician in a positive way than in a negative one. Moreover, other data, such as the ideological correlation between author and cited politician, would not make sense if members were frequently using negative citations.
Figure 3-4: Ideological Heatmap.

I divided the members of each party into three evenly-spaced groups based on their DW-Nominate score (creating six groups total). I then identified the group for the author of and cited politician in each communication. The heatmap illustrates the likelihood of each author-cite combination, where darker colors are more likely. This shows that combinations along the diagonal (where author and cite are in the same ideological group) are the most common.

Source: Political communication dataset.
approval, when the public believes Congress cannot cooperate to get things done.\textsuperscript{63}

Table 3-2 provides a few example citations, including several that demonstrate how citations to top officials differ from those to other politicians.

**Can the Public Interpret these Cues?**

We know that members are incredibly strategic about the content of the messages they send to the press and the public. The evidence suggests that they also think strategically about who they mention in their communications. But these citations are usually relatively subtle. Can the public draw conclusions about the member based on who is being cited? My experiments show that the public can sometimes make sophisticated guesses about a member of Congress based on who he or she talks about.

Both experiments involve mock emails from a member of Congress and ask respondents to evaluate the member’s partisanship and ideology. In the latter experiment, I also ask about respondents’ warmth toward the member using the standard feeling thermometer scale.\textsuperscript{64}

**Experiment #1: Moderates versus Extremes**

In the first experiment, respondents read about the member attending an event with one of four well-known Senators—John McCain (R-AZ), Rand Paul (R-KY), Harry Reid (D-NV), or Elizabeth Warren (D-MA).\textsuperscript{65} This experiment was designed to evaluate whether the public made a distinction when a member of Congress cited a moderate or extreme partisan.\textsuperscript{66} Specifically, the email says “Senator [McCain, Paul, Reid, Warren] and I recently helped welcome World War II veterans from across the country to their memorial in Washington, D.C.”

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\textsuperscript{63}Personal communication on August 9, 2016.

\textsuperscript{64}Both experiments also asked respondents whether they would vote for the member if given the opportunity. While I do not show any results using this question here, they are available upon request.

\textsuperscript{65}These Senators were chosen because Google Trends data showed they were similarly “popular” in terms of being searched at similar volumes. Additionally, as will become clear, it was important that they represent both moderate and extreme branches of each party.

\textsuperscript{66}A previous pilot experiment on MTurk showed clear evidence that people perceived differences in members who cited Republican (Mitch McConnell, R-KY) or Democratic (Reid) politicians.
Table 3-2: Example Citations.

I randomly identified 10 quotes from my data and then selected four that best represented the types of citation I saw most frequently. I then randomly identified another 10 quotes that met two criteria: they cited one of the top five politicians and were from a party different from the cited member. Again, I chose four that provided a good general representation of the broader data within this small subset.

<table>
<thead>
<tr>
<th>Author</th>
<th>Text</th>
</tr>
</thead>
<tbody>
<tr>
<td>Walter Jones (R-NC)</td>
<td>Representative Walter B. Jones is an original cosponsor of HR 4464, a bill introduced this week by Congressman Tom Price, that would ensure that an employer may require employees to speak English while engaged in work.</td>
</tr>
<tr>
<td>Harry Reid (D-NV)</td>
<td>“Just as the GI bill half a century ago, this bill increases access to higher education for millions of Americans,” said Senator Charles Schumer. “Earning a college degree can decide a young person’s fortune, but it shouldn’t cost one.”</td>
</tr>
<tr>
<td>Timothy Bishop (D-NY)</td>
<td>100 Democrats and 20 Republicans signed the bipartisan letter to president Hu Jintao. Below is a list of all the signatories, in alphabetical order: Gary Ackerman, Tom Allen, Tammy Baldwin, ...</td>
</tr>
<tr>
<td>Rush Holt (D-NJ)</td>
<td>Because of the work of US Representatives Rush Holt and Patrick Murphy, the House-passed FY2010 Transportation funding bill could lead to significant discounts off the price of a peak fare ticket for commuters along Amtrak’s Northeast Corridor.</td>
</tr>
<tr>
<td>Mike Simpson (R-ID)</td>
<td>Simpson joined 22 other members in sending a letter to House Speaker Nancy Pelosi and Senate Majority Leader Harry Reid, urging them not to vote on controversial climate change legislation before the end of the year.</td>
</tr>
<tr>
<td>Steve Scalise (R-LA)</td>
<td>Congressman Steve Scalise today sent a letter of acceptance to President Barack Obama’s invitation to “come over to the White House and go over line by line” of the Health Care bill.</td>
</tr>
<tr>
<td>Mike Conway (R-TX)</td>
<td>Yet now, during the week of Christmas, the President, Nancy Pelosi, and Harry Reid are putting coal in the stockings of every single hard working American in this country in the form of a costly, government run plan that raises [sic] costs, funds abortion coverage, cuts Medicare ...</td>
</tr>
<tr>
<td>Jesse Jackson (D-IL)</td>
<td>His decision to escalate American involvement in the Iraq war goes against the interests of the United States and the will of the American people. Make no mistake: this is George W. Bush’s war.</td>
</tr>
</tbody>
</table>

Source: Selected quotes from political communication dataset.

*Note that Barack Obama is not named in this quote, so this would not count as a citation to him in my data.
The Senator is never mentioned again and no information is provided about the partisanship of either the Senator or the representative sending the email. Despite this lack of information, respondents clearly viewed the email author differently depending on whether the cited politician was a Republican or a Democrat.

In figure 3-5, I plot the actual ideological position of the cited politician along the x-axis, with the estimated partisanship of email author along the y-axis. Respondents rated the member who cited a Democratic politician as significantly more Democratic than the member who cited a Republican politician (a difference of about 0.8 points on the seven-point scale). Among knowledgeable respondents (those who answered at least four of five questions about politics correctly), the differences are even more stark, with the member citing a Democrat estimated about 1.65 points more Democratic than the alternative. Moreover, these respondents are able to draw nuanced conclusions based on differences within the party. The member citing Warren is considered by this knowledgeable group to be about a half a point more Democratic than the member citing Reid (a statistically significant difference). The gap is smaller for the Republican politicians: only 0.2 points separate the member citing McCain from that citing Paul (a insignificant difference), though the knowledgeable respondents do order the two politicians correctly (with Paul more extreme than McCain).

**Experiment #2: Support versus Opposition**

In the second experiment, I test another distinction: whether respondents react to citations with a positive or negative tone. Specifically, the emails either thanked a politician for their support or criticized them for their opposition:

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67 For realism’s sake, I did use names and photos of serving Representatives—Rodney Davis (R-IL) and Mike Quigley (D-IL)—where the Republican talked about McCain or Paul and the Democrat talked about Reid or Warren. While it is certainly possible that some respondents knew the partisanship of Davis and Quigley, results are unchanged when I remove respondents from these two members’ home state of Illinois.

68 I also asked about estimated ideology of the email author. The results are similar but somewhat more muted, perhaps because ideology is not as well-known a concept as party (Campbell et al. 1980; Converse 1964). An identical figure using estimated ideology is shown in figure D4.

69 A similar graphic plotting these results for knowledgeable respondents is also shown in figure D5.
Survey respondents view members who cite Democratic Senators as more Democratic than members who cite Republican Senators. They are less able to distinguish between extreme and moderate wings of the two parties.

I commend Nancy Pelosi for her strong support of our efforts to get the Veterans Affairs Support bill passed yesterday. Pelosi’s support was crucial given the deplorable conditions faced in our nation’s veterans’ facilities, and I’m thrilled she helped pass this important legislation.

We fought hard against the opposition of John Boehner, and, yesterday, we were able to get the Veteran’s Affairs Support bill passed. Boehner’s opposition was ridiculous given the deplorable conditions faced in our nation’s veterans’ facilities, and I’m thrilled it did not derail this important legislation.

As the two examples suggest, the experiment also varied whether the email mentioned...
Democratic leader Nancy Pelosi or Republican leader John Boehner. Finally, to determine whether bipartisanship mattered, I also informed respondents that the email author was either a Republican or Democrat.\textsuperscript{70}

Because I informed respondents of the email author’s partisanship, it is not surprising that they rated the Democratic member as more Democratic than the one identified as Republican across all equivalent treatments. In figure 3-6, therefore, the closed circles (representing treatments with a Democratic member) are all below the open circles (representing treatments with a Republican member). However, respondents also make reasonable assumptions about the email author based on how he talks about Pelosi or Boehner. Specifically, a member who talks positively about Pelosi or negatively about Boehner is rated as more Democratic than a member who does the reverse.\textsuperscript{71}

The design of this experiment also allowed me to determine whether negatively-valenced citations were politically risky. This is certainly the received wisdom in political advertising. Lau, Sigelman, and Rovner (2007) conducted a meta-analysis of over 100 studies on attack ads and concluded that these were, in general, detrimental to the campaign of the ad sponsor. Despite these findings, the criticism in my experiment does not seem to have any effect on how respondents viewed the email author. The solid line in figure 3-7 shows the feeling thermometer scores that members received when the email spoke positively toward Pelosi or Boehner. The dashed line is the score when the email was critical of Pelosi or Boehner. There is very little difference between these two lines and while the average feeling thermometer score for an “oppose” email was slightly lower than for a “support” email, the difference is substantively small and not statistically significant.\textsuperscript{72} Unlike with campaign ads, it appears

\textsuperscript{70}Thus, there are 2x2x2=8 conditions.

\textsuperscript{71}As with experiment #1, the results hold when looking at predicted ideology, though the differences are smaller. These results are shown in figure D6. Interestingly, for knowledgeable respondents, there is very little difference between a Republican candidate who opposes Pelosi and one who mentions Boehner in support or opposition. This may reflect knowledge of the split in the Republican party—a criticism of Boehner may be coming from the left or the right. The full results for knowledgeable respondents are shown in D7.

\textsuperscript{72}There are no statistically significant differences when I subset by partisan affiliation (or independence).
Survey respondents make reasonable assumptions about the partisanship of members who cite Nancy Pelosi and John Boehner in positive and negative ways.

Source: Survey experiment #2.

members do not pay a price for being critical of their political opponents in these emails.

On the other hand, my data show that members do not benefit from demonstrating a bipartisan spirit. Comparing the feeling thermometer scores for members who express support for a leader of their own party to those who are supportive of an opposition leader reveals no differences. On a 0-100 scale, the average score toward the member who spoke highly of an opposition leader is 0.4 degrees warmer than toward a member supportive of his own party.73

Indeed, Republicans are somewhat more warm toward the member using oppositional language than the supportive one, though the difference is still statistically insignificant.

73To correctly identify this signal as one of bipartisanship, respondents must both know the party of the member sending the email and the party of the cited politician, perhaps a high bar. To identify the subset of respondents who were most likely to meet this bar, I subsetted the data to only include those who
Figure 3-7: Feeling Thermometer Scores toward Members who Criticize Other Politicians.

Survey respondents do not penalize members who use negatively-valenced language in their emails.

Discussion and Conclusion

While theory suggests that members of Congress are unlikely to mention other politicians, I find evidence that they do so on a regular basis. Because members are most likely to talk about their relationships with politicians who are ideologically similar, I view this behavior as a strategy of ideological position-taking. Members use this tactic to illustrate to their con-

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correctly answered at least four of the five political knowledge questions and who correctly identified the partisanship of the member sending the email. Among this group, bipartisanship was actually penalized by nearly 10 points on the feeling thermometer scale, though due to small sample size, this difference was not statistically significant. In the real world, it is likely that the readership of a member’s emails and press releases is somewhere between the two groups—perhaps not as politically knowledgeable, but likely to know the partisanship of their own member. Thus, the response to the bipartisanship signal is likely to be muted but possibly negative.

85
stituents where they stand in ideological space, rather than on particular issues. Moreover, survey experiments show that people are able to interpret these cues, making assumptions about the partisanship and ideology of members who talk about politicians with known partisanship or ideological positions.

It may be the case that members of Congress collaborate most often with others who are ideologically similar. Under this assumption, my finding is simply a reflection of member activity in DC. However, there are plentiful opportunities to mention other politicians who are not frequent collaborators—they can discuss legislation sponsored by someone they do not know well; they can talk about other members of committees on which they serve; they can highlight efforts on the part of their state delegation; they can mention political leaders of the other party. Because we know members selectively choose what to discuss, we can assume they also selectively choose who to discuss. And the evidence suggests they often choose to discuss politicians who share their ideology.

There are several ways to extend this work to better understand how and when members cite other politicians, as well as what the public impact of citations might be. First, it would be useful to classify political communications with citations based on their content. Are members more likely to cite another politician when discussing legislative achievements or policy events or political criticism? A relatively easy approach would be to use a supervised learning approach to classify identified communications into these (and other) categories. This would provide statistics about the composition of political communications with citations. However, that approach does not illustrate how likely a citation is within each of these categories. This would require classifying all political communications and then determining which contain citations, a much more demanding task due to the number of political communications.74

Second, building on this question about how likely citations are, a useful approach would

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74 Recall that only about a third of political communications contain citations.
be to evaluate how often members cite others in a given collaboration. As an example, if a member cosponsors a piece of legislation, how likely are they to mention the sponsor of the legislation in their communications? Or, for a particular congressional hearing, do members mention the relevant committee chair or ranking member? Answers to these questions could provide additional insight into the strategy behind member citations.

There are also a few experiments that could help further clarify how the public reacts to member citation behavior. While I have emphasized the position-taking rationale for member citations, another reasonable explanation involves bolstering member credit claiming. Specifically, members’ discussion of legislative achievements may seem more credible if they cite the involvement of other politicians. An easy way to test this is to run an experiment where the political communication does or does not cite someone involved in the passage of a bill. Follow-up questions could include ratings of member honesty and effectiveness, as well as vote likelihood and the feeling thermometer scale.

Finally, although my data showed that the majority of citations are to relatively less well known politicians, all of my experiments involve political leaders. To get a full view of the role of citations, it would be useful to understand the impact of citing someone the public is less likely to know. Again, a relatively straight-forward survey experiment could shed light on this question: including a cite to a household name in one condition and a cite to less known politician in another. The one challenge for this approach is that, since many known politicians are controversial, it might be tricky to find someone who does not activate partisan bias.

As these ideas suggest, many interesting and important questions remain in understanding how members use this strategic approach and how these subtle cues are interpreted by the public.
Part IV

Appendices for All Papers
Appendix A: Creating Mock Advertisements

To increase the credibility of any causal claim I made about the effects of congressional advertising, I wanted to include a survey experiment. As I explain further below, it was not possible to use real advertisements that I had analyzed for the observational component, though these served as useful sources for the text of the ads I ultimately designed. The following contains information on the steps I took to (1) analyze the text of advertisements for common phrases in support of or critical of Congress, (2) test the realism of the advertisements I designed, and (3) create realistic-sounding and -looking advertisements for use in a survey experiment.

I include this appendix as a resource for future researchers, as new technology has made it surprisingly easy to complete this third step. While the first two steps continue to be critical components of a well-designed survey experiment, it is gratifying that designing the actual ads is no longer as much of a constraint for researchers looking to test various media effects in the future.

Here are links to the ads used as treatment:

- Positive (http://youtu.be/TxTVZEsfpDY)
- Negative (http://youtu.be/Pvhvr8xv9r4)
- Control (http://youtu.be/8Fy6bAddao)

Analyzing Real Advertisements

The data on congressional advertisements comes originally from the wonderful resource at the University of Wisconsin Advertising Project. From this center, you can obtain PDFs of all advertisements aired in a subset of media markets for the 2000, 2002, 2004, and 2008 elections. These PDFs contain a transcript of the advertisement, along with screenshots of the video (there are typically 7-8 screenshots per 30 second commercial).
My first step in analyzing these ads was to convert the transcription into a readable format. This was relatively straight-forward for the later years of these data, as the PDFs are rendered with optical character recognition (OCR) and thus the text can simply be copied and pasted (or automated through PDF-to-TXT software). For the earlier two elections, I used workers from Amazon’s Mechanical Turk service to re-type the text from the PDFs.

Once I had the text from the approximately 10,000 ads in Wisconsin’s database compiled in a CSV file, I again engaged 4 workers from MTurk and a related website, Elance, to evaluate whether and how the advertisements talked about Congress. It wasn’t possible to use a simple word-search mechanism, as many ads included information about the ad sponsor (such as the “Democratic Congressional Campaign Committee” or “Pelosi for Congress”) that would inappropriately count some ads. Other ads discuss “Washington politicians” or other entities that implicate Congress without actually using the term. Indeed, the correlation between the data provided by the workers I recruited to those from a simple word search is only 16 percent.75

I went through several iterations to determine the best way for relatively untrained workers to complete this task. In the end, I found that it was more effective to work with specific people to rate many ads, rather than having thousands of workers rate only a few ads (a strategy that worked well with the more mechanistic task of re-typing the ad transcription). Ultimately, this decision allowed me to interact with the workers in the same way that researchers more typically work with undergraduate research assistants. Specifically, we had an on-going conversation about how they were rating the ads. I would spot-check their ratings and looked specifically at any ads that were rated as positive toward Congress, partly because their rarity suggested the increased possibility of a typo and partly because of a

75 Even after I used various text analysis tools to automatically rid the ad text of information about the ad sponsor, it is still the case that a word search approach identifies 45 percent of the ads as containing “Congress” while my raters only found 18 percent of ads to be about Congress. It is also the case that 40 percent of ads that do not include the word “Congress” were considered by my raters to have mentioned Congress—often in the “Washington politicians” sense.
personal interest in how candidates were speaking positively of Congress.

For each ad, two workers determined whether the ad mentioned Congress. If so, the worker then evaluated whether the statements about Congress were positive, negative, or neutral. Neutral ads could mean Congress was discussed in an impartial way or that the ad was both positive and negative. I also worked with an Elance worker as an arbiter if the two original raters disagreed. However, this was empirically rare, as I had 85 percent inter-rater reliability. This high level of reliability was surprising, given that I recruited non-trained workers and paid them very little (approximately 3 cents per ad).

Table A1 shows a few examples of ads rated in each of the four categories (not mentioned, negative, neutral, and positive).
Table A1: Examples of Campaign Ads.

| Not mentioned                                                                 |                               |
|                                                                              |                               |
| **Darcy Burner (D-WA)**                                                      | [Announcer]: Congressman Reichert says he's independent. But after four years in Congress, Reichert hasn’t passed one bill. He’s ranked 401st out of 429 members and dead last in the Washington State delegation in his ability to get things done. He even ranks behind non-voting members from Guam and American Samoa. So, when it comes to describing Reichert’s record in Congress, the only word that comes to mind is ineffective. For new solutions, we need Darcy Burner in Congress. [Darcy Burner]: I’m Darcy Burner, and I approve this message. |
| 2008 challenger                                                              |                               |
|                                                                              |                               |
| **Mike Briggs (R-CA)**                                                       | [Autry]: You know, it’s farms like these that are the heart and soul of the San Joaquin Valley. Mike Briggs understands that. That’s why he helped to secure $200 million in tax breaks when our farmers needed it the most. Mike loves his family, he loves his country and he loves this valley. And it’s values like that will not only provide strong leadership in Washington but leadership we can be proud of. Mike Briggs, a good man for Congress. [Announcer]: Mike Briggs, Republican for Congress. Let’s keep him working for us. |
| 2002 open seat                                                               |                               |

<p>| Negative                                                                     |                               |
|                                                                              |                               |
| <strong>Will Shafroth (D-CO)</strong>                                                     | [Video of football players hitting each other] [Will Shafroth]: You expect it out here, but in Washington, D.C., things just get ugly, and constant bickering in Congress means nothing gets done. I’m a political outsider who spent 27 years bringing people together to solve tough problems. I’m Will Shafroth, and I approve this message because Washington politicians need to stop hitting each other and finally start tackling America’s challenges. |
| 2008 challenger                                                              |                               |
|                                                                              |                               |
| <strong>Mark Pryor (D-AR)</strong>                                                        | [Jesse James]: “Alright open it up!” [Mark Pryor]: It’s like asking Jesse James to guard a bank. Washington all but opened the vault, looked the other way, and let greedy executives steal our savings. Congress caved in to special interests, weakened enforcement, and made it easier for corporations to hide losses and cheat stockholders. Washington is just too cozy with big business. Guilty companies should be held accountable and dishonest executives put behind bars. [Announcer]: Mark Pryor. Arkansas values. Arkansas first. |
| 2002 incumbent                                                               |                               |</p>
<table>
<thead>
<tr>
<th><strong>Neutral</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td>Steve Kuykendall (R-CA) 2000 incumbent</td>
</tr>
<tr>
<td>Roger Kahn (D-GA) 2000 challenger</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th><strong>Positive</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td>Phil Gingrey (R-GA) 2004 incumbent</td>
</tr>
</tbody>
</table>
Mark Foley (R-FL)
2004 incumbent

[Foley]: I'm Mark Foley, I'm proud of what we've accomplished in Congress to protect our children. We worked hard to keep predators away from kids by giving groups like Boys and Girls Clubs access to FBI background checks, cracked down on child pornography, and toughened penalties for anyone who hurts a child, but there is more to do. In Congress I'll keep up the fight to protect children. That's why I need your support. [Announcer]: Mark Foley, our Congressman. [Foley]: I'm Mark Foley and I approved this message.76

Creating and Testing Text of Mock Advertisements

I had hoped to use real ads as my experimental treatment, but this proved difficult for several reasons. First, these ads were originally aired many years ago, and often prior to the proliferation of YouTube channels for candidates, where ads are now routinely stored. Even if the video were available at the time of the election, it is unlikely that I would be able to find these videos today. Second, I also could not simply use the text from these ads and incorporate my own video, as many of the ads discuss policies or events that are time-bound. For example, a lot of ads in 2000 discuss the budget surplus—a situation that would not have much meaning given the current political concern with the deficit and debt ceiling. Finally, I wanted to control as much of the ads (and their purported sponsor) as possible, in a way that would not be possible when using real ad text. Given these constraints, I instead used the text from the real ads as a starting point for drafting mock advertisements.

Because it seemed harder to design a credible advertisement that spoke highly of Congress, I started there. I pulled the text of all ads rated as positive by any worker and read through each for phrases that were not time-bound and lacked policy content.77 I also looked online for various press accounts or blog posts that were supportive of Congress. These would have

76I recognize the irony of this particular ad given the ensuing scandal involving Representative Foley, but it is a nice representation of how members can be supportive of Congress.
77While some viewers would think more highly of Congress for passing a particular policy, others would not. I wanted statements that were as policy-free as possible.
the advantage of being more relevant to the political world of 2014 than ads aired 10-15 years ago.\footnote{In practice, most of the articles I found were also time-bound, in that they discussed the high productivity of the 111th Congress.} Using these key phrases, I drafted three mock ads that were generally supportive of Congress but that I hoped could be taken seriously during a time when approximately 85 percent of the public disapproved of Congress. I next drafted text for ads that were roughly parallel to the positive ads but that were nonetheless critical of Congress. Finally, for the control advertisement, I lightly edited text from an ad aired in 2002 by Rick Renzi.

After running these mock ads by colleagues for initial feedback and dropping one set of positive/negative ads, I again employed MTurk workers to evaluate the credibility of the remaining ads. I included text from three real ads for each mock ad and asked the MTurkers to identify which ad was fake. I did not select ads entirely at random, as I wanted to have a few parallels with the mock ads: specifically the use of one primary speaker and limited policy content. These features are not empirically unusual, so selecting on them did not seem to invalidate the effort. I also wanted ads in which the identity of the candidate could be obscured, so ads with references to district geography or committee assignments were excluded.

If MTurk workers were selecting among the four ads at random, we would expect that the mock ad was identified no more than 25 percent of the time. In only one case was the mock ad identified even that often. On average, the mock ad was selected about 13 percent of the time. This gave me confidence that the ad text was credible, even when the candidate made statements in support of Congress.

**Creating Audio and Video of Mock Advertisements**

With ad text now in hand, it was surprisingly easy to create videos that looked similar to real campaign ads. I used primarily stock photos available online that showed pictures of Congress or a town the hypothetical candidate would be from. I supplemented these photos
with stock video of a family gathering. I downloaded these royalty-free clips from Video Blocks, which offers an introductory 7-day period of free downloads. Because I knew what kinds of clips would be useful in my video, I was able to collect all of the video clips I wanted during this introductory period.

Obtaining a quality audio recording of the ad was more expensive, but not prohibitively so. I used an online voice actor service, VoiceBunny, which allowed me to “audition” 3 voice actors, who read a short segment of the ads. After providing the full text of the ad to the actor I selected, I obtained a full reading within 24 hours. The total cost, including VoiceBunny commission was approximately $120.

To knit these components together, I used iVideo (a video-editing software free on Macs) and Audacity (an audio-editing software that is free to download). Audacity allowed me to slow down the recording, to make the pacing more realistic for a commercial. I was also able to insert additional pauses into the recording where it seemed appropriate. Using iVideo, I pulled together the video clips to match the audio.\footnote{VoiceBunny actors can also be used with video in hand, so the actor is responsible for matching the provided video, but this is more expensive.} One technique I found helpful in iVideo was the “Ken Burns” effect, which pans the screen across a still photo.

Certainly, real campaigns spend more time and money crafting their advertisements (though, like me, a lot of the effort probably goes into message development rather than filming itself). Nonetheless, I was pleased at the realism of these ads given the small budget used to create them.
Appendix B: Alternative Tests and Robustness Checks

Alternative Measure of Criticism of Congress

As described in Appendix A, two raters reviewed each ad to determine whether and how Congress was depicted in the ad. At the same time, the raters evaluated whether and how the ad talked about the Democratic and Republican parties. If the two ratings differed (on either Congress or the parties), a third worker evaluated the ad on both dimensions. Finally, if the third rating was different than either of the two initial ratings (again, for either Congress or the parties), I personally reviewed the ad. In the end, less than 200 ads (out of more than 10,000) required my review based solely on the congressional dimension. My review included both an independent (blind) rating as well as a more comprehensive evaluation that considered how the other raters viewed the ad.

Given this process, my measure of ad tone used in the paper is calculated as follows: (1) if the ratings of the first two raters agree, this is the final rating; (2) if the third rater agrees with either of the first two raters, this is the final rating; (3) if there is no agreement amongst the first three raters, my comprehensive evaluation serves as the final rating.

However, because some ads were reviewed more than twice due to disagreement on the parties dimension (rather than the Congress dimension), there are ads where I have up to 4 ratings even when the original raters agreed. This allows for additional opinions about the content of the ad that, perhaps, should not be ignored. Therefore, a second measure of congressional criticism is the average rating given by anyone who evaluated the ad. If all raters considered the ad to have not mentioned Congress, the average is not calculated. I also created a 4-level variable based on this measure, where an average rating below a neutral rating was considered negative, an average above neutral was considered positive, and an ad where the average rating was exactly neutral was considered neutral.
Table B1: Ad Classification, by Original and Alternate Measure.

<table>
<thead>
<tr>
<th>Original measure</th>
<th>Alternate measure</th>
<th>Not mentioned</th>
<th>Negative</th>
<th>Neutral</th>
<th>Positive</th>
</tr>
</thead>
<tbody>
<tr>
<td>Not mentioned</td>
<td></td>
<td>8357</td>
<td>429</td>
<td>473</td>
<td>68</td>
</tr>
<tr>
<td>Negative</td>
<td></td>
<td>3</td>
<td>694</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Neutral</td>
<td></td>
<td>2</td>
<td>38</td>
<td>120</td>
<td>11</td>
</tr>
<tr>
<td>Positive</td>
<td></td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>28</td>
</tr>
</tbody>
</table>

The two measures correlate at a respectable 0.52, where additional comparisons confirm these initial results. Table B1 shows the number of ads classified in each of the categories. Approximately 90 percent of the ads were classified identically by both methods. The biggest difference seems to be that the original method was more likely to classify an ad as not mentioning Congress, with approximately 95 percent of the errors in the top row of the table. Importantly, no ads were considered positive by one method and negative by another—instead, the errors appear to be one of emphasis.

Given these findings, it seems that the two ratings methods produce substantially similar results. Indeed the results in the next section illustrate that using the alternate measure does not alter the results.

Robustness Checks

Figure B1 illustrates the coefficients and 95 confidence intervals on the “treatment” variable using a variety of alternate specifications of the model. While the estimated effect of ads critical of Congress varies somewhat, the confidence intervals always span zero and largely overlap each other. More details on each of the specifications are below:

1. **Number of ads critical of Congress (log):** This is the relevant variable from the model reported in the paper (Figure 1-4) for comparison with the alternative specifications.
2. Alternate rating of ads: As alluded to in the previous section, using the alternate measure for rating the ads in Wisconsin’s database had no effect on the estimated coefficient in the model of congressional approval.

3. 4-point approval: In the original model, I used a dichotomized specification of NAES’s congressional approval question: did the respondent either strongly or somewhat approve of Congress? However, the response options have more information that this binary obscures. This figure shows that using the full 4-point scale does not seem to influence the estimated coefficient on negative ads.

4. Using CCES data: This estimate uses 2008 Cooperative Congressional Election Study (CCES) data to show that results are robust to other survey data. The model is slightly different, due to different questions asked by the two survey groups. The most notable difference is the lack of over-time data, as CCES was only in the field during the month of October. The NAES data indicated that survey respondents were most likely to have had negative ads aired in their media markets during October,
so this result suggests that even as more critical ads are being aired, their effect is no greater. It is possible that the effects of negative ads are drowned out by the sheer volume of advertising that has occurred by this point in the campaign cycle. In looking more closely at the NAES data during this period, I used a dummy for campaign season (where respondents were coded as 1 if interviewed in September or October and 0 otherwise) and interacted this with the treatment. These results show that the original negative coefficient is driven by respondents earlier in the year when total advertising was lower. However, the results remain statistically insignificant.

I also ran a series of robustness checks that are shown in Figure B2 and described below.

**Figure B2: Robustness Checks.**

![Figure B2: Robustness Checks.](image)

1. **Number of ads critical of Congress (log):** Again, I provide the result from the paper as a point of comparison.
2. **Ads aired in the last week (log):** I chose a two-week time frame somewhat arbitrarily but this estimate of the effects of negative ads being aired in the last week provides credence to the claim that ads aired over a shorter time horizon are no more likely to influence congressional approval. Note that, in this regression, I control for the total number of ads aired in the last week rather than the two-week period. Other than this change, the regressions are equivalent.

3. **Ads aired in the last month (log):** Like the previous estimate, this is included to illustrate that the choice of a two-week period did not lead to unusual results. While likely not significant, the fact that the estimated coefficient is even closer to zero may point to the fact that any effects of negative ads are not long-lasting.

4. **Ads aired after survey (log):** This placebo test shows that respondents are not affected by ads aired in the two-weeks after they are surveyed by NAES. The estimate is approximately the same size and significance as in the original model. This may suggest that candidates are more likely to air negative ads in places that are predisposed to disapprove of Congress. However, the estimate in figure 1-5, showing that ads run by candidates in other congressional districts are no more or less correlated with approval, speaks to this point. Additionally, the difference-in-difference results below add confidence to my claim that candidates are not strategically targeting critical ads.

5. **Interest in politics:** Similar to the previous estimate, his model substitutes interest in politics as an alternate dependent variable. In this case, interest in politics is not a placebo test, but rather an alternate outcome of interest. The literature on negative advertising suggests that attack ads may depress interest in politics or turnout (Lau, Sigelman, and Rovner 2007). While I do not have data on turnout, this regression suggests that ads critical of Congress do not have any important effects on political interest.
6. **Presidential approval**: This model substitutes presidential approval as the dependent variable, with a similar array of dependent variables. This is another placebo test, as we would not expect criticism of Congress to have any influence on presidential approval. Indeed, the result is again small and statistically insignificant.

**Alternate Method and Different Survey Data**

There may be something about the method I used that is obscuring a public reaction to advertising that is critical of Congress. Therefore, I used publicly available survey data from Gallup and CBS and a difference-in-difference design to test these relationships. Specifically, I collected individual-level data from 14 surveys that asked about congressional approval and contained congressional district identifiers that were fielded between October 2007 and October 2008. I next compiled data into four periods: (1) the last quarter of 2007, (2) the first quarter of 2008, (3) the second quarter of 2008, and (4) the last week of October 2008.\(^8\) Within each of these four periods, I have data on congressional approval for approximately 3,000 respondents; from these individual-level responses, I aggregated the data to identify average district-level congressional approval in each of the periods.

I again paired these data with the data from the Wisconsin Advertising Project. Joining these datasets was a little more difficult than with the Annenberg survey data, as I do not know the media market of the respondents. Instead, I used Geographical Information System tools to determine the geographic overlap between districts and media markets. Some districts are wholly within a media market. Therefore, all of the constituents would be (potentially) exposed to the same advertisements. Districts which are split across media markets are more complicated. In these cases, I created a weighted average of negative advertising in a district. For example, if 70 percent of a district was in one media market,

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\(^8\)The choice of periods was driven, in large part, by the available survey data. I would have preferred post-election survey data as my final period, but this was unavailable. Instead, I chose data from the tail end of the election, the last week of October, as the final "post-treatment" time period.
Table B2: Results from Difference-in-Difference Analysis.

<table>
<thead>
<tr>
<th>Treatment (all values logged):</th>
<th># of negative ads</th>
<th># of incumbent negative ads</th>
<th># of late negative ads</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post-treatment period</td>
<td>0.27</td>
<td>-0.11</td>
<td>0.45</td>
</tr>
<tr>
<td></td>
<td>(0.54)</td>
<td>(0.60)</td>
<td>(0.49)</td>
</tr>
<tr>
<td>Treated</td>
<td>-0.91</td>
<td>1.51</td>
<td>-0.48</td>
</tr>
<tr>
<td></td>
<td>(6.90)</td>
<td>(8.21)</td>
<td>(6.97)</td>
</tr>
<tr>
<td>Post-treatment*Treated</td>
<td>-0.54</td>
<td>-0.84</td>
<td>-1.01</td>
</tr>
<tr>
<td></td>
<td>(1.09)</td>
<td>(1.68)</td>
<td>(1.41)</td>
</tr>
<tr>
<td>Intercept</td>
<td>16.05</td>
<td>18.50</td>
<td>15.69</td>
</tr>
<tr>
<td></td>
<td>(3.45)*</td>
<td>(2.95)*</td>
<td>(2.42)*</td>
</tr>
</tbody>
</table>

Standard errors in parenthesis; * p<0.05

where 100 ads critical of Congress were aired, and there were 500 negative ads aired in the media market that covers the remaining 30 percent of the district, I would estimate that survey respondents in this district saw \((.70 \times 100 + .30 \times 500 =)200\) negative ads.\(^{81}\) This weighted average is subsequently used as my treatment.

In Table B2, I show the results of three difference-in-difference regressions. Treatment in the first column is the log of the number of negative ads shown in the district over the course of the entire election. In the second column, treatment is the log of the number of negative ads aired by incumbent candidates. Finally, in column three, treatment is the log of the number of negative ads aired (by any candidate) from October 11 to October 25 (two weeks prior to the final round of surveys). For these analyses, I only use survey data from the final two periods (second quarter of 2008 and the last week before the election). In all cases, the coefficient of interest is the interaction term. For each of the three models, the coefficient is correctly signed but relatively small and statistically insignificant.

The key assumption of difference-in-difference designs is that, in the absence of treatment,

\(^{81}\)An obvious problem with this estimate is that geographic overlap is not the same as overlap in population centers. However, given the already ambiguous nature of media market boundaries (which typically follow county lines rather than the actual reach of television signal), it seemed a reasonable approximation.
both treated and control units would have changed (or not changed) similarly. One typical way to increase confidence in this assumption is through showing parallel movement in pre-treatment periods. I have attempted to do this with data from the first two periods in Figure B3 below. However, while the volume of advertising increased dramatically between the end of the second quarter of 2008 and the election, it is certainly the case that some negative advertisements were shown prior to that time. Therefore, the figures should be interpreted with some caution. In all cases, the continuous treatment has been discretized by splitting the data at the median; treated units are those districts where more than the median number of negative ads were aired.

The bottom-most of the three figures is probably the most credible case for parallel trends in the pre-treatment periods. In this case, the treatment of viewing more than the median number of negative ads in the previous two weeks is associated with a 5 percentage point drop in the level of congressional approval. While sizable, this effect remains statistically insignificant. When taken in conjunction with the null (and substantively small) results elsewhere, I do not put much stock in this effect. It is possible that ads critical of Congress have some influence on public approval of Congress, but at most this effect is small, lasts only a short time, and may only arise in certain circumstances, such as when incumbent candidates criticize Congress. Moreover, the fact that candidates only rarely criticize Congress should alleviate much of the concerns raised by Fenno in his classic statement.
Figure B3: Difference-in-Difference Analysis.

All negative ads

Incumbent negative ads

Late negative ads
Appendix C: Data Description

Below, I provide additional specifics on the sources of data and estimation strategy for the variables used in these analyses.

- **Roll Call Record.** I use roll call data from voteview.com; because I am only interested in defense-related bills, I select only those votes related to bills identified as defense-related by the Congressional Bills Project.\(^{82}\) Unfortunately, this classification is only available through the 112th Congress. While the data identify whether each member voted yes or no on each bill, it is unclear whether a yes vote is pro-military. Therefore, I use the \texttt{wnominate} program (Poole, Lewis, et al. 2011) to estimate how each member voted on this subset of issues. Because \texttt{wnominate} cannot scale across congresses (and there are various reasons we might not accept the assumptions needed to do so in this case), I instead identify members’ relative rank on the dimension recovered by \texttt{wnominate}. This is a reasonable measure, as I want to know whether members are becoming more or less pro-military as their district interests change and we can evaluate this relative to other members who are not experiencing similar changes.

- **Committee Membership.** The data for this come from Charles Stewart’s Congressional Data Page and were originally collected by Stewart and his coauthors. For the 113th Congress, I obtained committee membership data from http://media.cq.com/pub/committees/.

- **Bill Sponsor and Cosponsorship.** I created code to scrape these data from the congress.gov website. I again used the Congressional Bills Project data to classify each bill as either defense- or not defense-related. As with the roll call data, then, these are only available through the 112th Congress.

\(^{82}\)This project uses the Policy Agendas Project topic specification, which codes a variety of data into 19 major topics, one of which is defense. I do not include bills related to veterans affairs.
- **PAC Donations.** These data are available from the 101st to 113th Congress on the Open Secrets website. They were already classified as being from particular industries, so I selected only those from defense-related organizations.

- **Email Data.** These data were generously shared with me by Justin Grimmer and his colleagues (Goodman et al. 2015). While the complete dataset spans 2008-2012 and includes members of both the House and Senate, I focus only on emails sent by House members during the fall of 2010 and fall of 2012, so as to best capture the changes wrought during the 2012 redistricting. This sample ultimately includes approximately 5,000 emails from 439 different members. To analyze these data, I ran an unsupervised structural topic model (using the `stm` package in R) on the emails and identified what percent of members' emails referenced the topic related to military issues in both periods.\(^3\)

- **Base Location.** For bases currently in operation, I obtained their precise location from the Department of Defense (DOD), at https://catalog.data.gov/dataset/military-installations-ranges-and-training-areas.\(^4\) For bases that had been closed through BRAC, I first identified these bases with information from the 2005 BRAC Report, which listed all bases closed in previous rounds as well as those proposed for closing in 2005.\(^5\) I then found location information online, typically through the Wikipedia entry on the closed bases. Where Wikipedia did not provide location data, I was usually able to

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\(^3\) Again, I explicitly exclude emails related to veteran’s issues.

\(^4\) The GIS data include boundary files, showing the full size of each base. However, geographic size data are not necessarily helpful in understanding the impact such a base might have on the surrounding area. Moreover, it proved impossible to find boundary data for closed bases, so instead, my analyses are restricted to considering data that provides a single point for each base. I did not find information indicating how the point data was determined in the DOD data, though it does not appear to be the center of the boundary file, so I assume it is more likely to represent the location of the bases' headquarters or main gate.

\(^5\) Not all bases proposed for closure by the Report were ultimately shuttered, as the BRAC Commission could reverse the Report’s recommendations. If the DOD data on operating bases continued to list a base as open, I considered that to be correct. In many cases, I was also able to find information online specifically noting the Commission had rejected the Report’s recommendation.
find it through military reports or EPA hazardous waste remediation reports.

- **Base Personnel.** I collected information on the personnel presence at bases through Department of Defense Base Status Reports, which are issued annually and provide “a snapshot of real property data” (Department of Defense 2015). While there is a wealth of information contained in these reports, I focused on data on base personnel (military, contractors, and civilian) and base acreage. Moreover, I was not able to find all Base Status Reports: while my data span 1981 to 2013, I am missing many years of data, particularly in the 1990s. One notable feature of these reports is their inconsistency over time—bases appear and disappear; they are renamed; sub-locations may be listed separately from main bases or not; the personnel employed at base fluctuates, sometimes dramatically. I used my judgment—along with Wikipedia base histories—to make connections over time.

- **Gaps in Base Data.** Although my final dataset includes more than 750 bases, the data are incomplete. For approximately 420 additional bases—approximately 60 currently open and 360 closed—I was unable to match personnel data to the named base. I was also unable to find location information for about 20 additional closed bases. However, in looking at what information I did have on these bases (both open and closed), it appears most are small offices, such as a Data Processing Center or a Reserve Center. A few others seem to be locations primarily staffed and operated by state-level military units, such as the Texas National Guard. Finally, for some bases where I had incomplete information, I was able to determine that in at least one year there were no personnel assigned to the base. As an additional check on my data, I identified any base that was listed in the Base Status Reports that had, on average, at least 1,000 personnel.

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86 In practice, I only used the personnel data, for reasons cited above.
88 I specifically verified that any base closure identified by DOD as “major” was contained in my data.
For those not on my list of bases currently operating or closed through BRAC, I made a judgment call about whether they should be added to the overall tally of bases. Through this process, I added approximately 5 bases.

- **Connecting Bases to Districts.** Because the data are incomplete and vary somewhat from year-to-year, in almost all data I average the number of personnel employed at a base during a congressional session using data from two years. I believe this will provide a better approximation of what a member might reasonably expect from what is potentially a major employer in her district. I then combined the data on base location with GIS data on the boundaries of the districts, available from http://cdmaps.polisci.ucla.edu (Lewis et al. 2015). Using the R package **sp**, I identified the district within which each base falls throughout the period encompassing the 93rd through 113th Congresses. I aggregated this information to congressional district, which provided me with statistics on the number of bases in a given district throughout this period. Because many of these bases closed during this period, I removed bases from districts in two different ways. First, I removed them as of the year of the BRAC decision. This has the benefit of being a well-established date and also we might expect member behavior to start changing as of this decision. However, it can take many years for the base to actually close. Therefore, the second date (and the one used in the analyses above) is the date the base was formally shut down (where I was able to find this information). The results are unchanged if I instead use the date of the BRAC decision.
### Appendix D: Supplemental Tables and Figures

#### Table D1: Summary Statistics on Citation Patterns in Political Communication

Basic statistics describing how members are citing other politicians in their emails and press releases.

<table>
<thead>
<tr>
<th></th>
<th>Press Releases</th>
<th></th>
<th>Emails</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>House Senate</td>
<td></td>
<td>House Senate</td>
</tr>
<tr>
<td># of members sending communication</td>
<td>568 112</td>
<td>492 78</td>
<td></td>
</tr>
<tr>
<td>Total pieces of communication</td>
<td>170,530 72,856</td>
<td>32,869 4,241</td>
<td></td>
</tr>
<tr>
<td>Mean # of communications per member per month</td>
<td>7.1 11.4</td>
<td>0.9 0.7</td>
<td></td>
</tr>
<tr>
<td>% which mention other politicians</td>
<td>27.2 45.1</td>
<td>26.2 51.1</td>
<td></td>
</tr>
<tr>
<td>% of these which mention more than one politician</td>
<td>45.5 41.4</td>
<td>40.9 53.4</td>
<td></td>
</tr>
<tr>
<td># of unique mentions*</td>
<td>128,010 75,354</td>
<td>17,721 5,872</td>
<td></td>
</tr>
<tr>
<td>% of mentions from same state</td>
<td>25.9 29.5</td>
<td>18.5 18.3</td>
<td></td>
</tr>
<tr>
<td>% of mentions from same party</td>
<td>65.8 59.4</td>
<td>51.0 49.0</td>
<td></td>
</tr>
</tbody>
</table>

*Source: Political communication dataset.

*A “mention” happens any time a press release or email cites a new politician. If, for example, an email includes the names of 3 politicians (other than the author), it would count as one piece of communication but 3 distinct mentions.
Figure D1: Ideology of Member Sending Email or Press Release, by Party and Whether it Includes a Citation.

There are few differences across ideology in communications that include a citation and those that do not.

Source: Political communication dataset.
Figure D2: Ideology of Cited Politicians, without Most Cited Politicians.

Because political communications involving the top-cited politicians are more likely to be negative than those related to other politicians, the citation patterns may be different when we remove these from the data. However, this graph is quite similar to figure 3-2.

*Not Including Most Frequently Cited Politicians

Source: Political communication dataset.
Figure D3: Ideology of Member and the Politician they Cite, without Most Cited Politicians.

The relationship between members and the politicians they cite is somewhat stronger without the top cited politicians.

Source: Political communication dataset.
Figure D4: Predicted Ideology based on Cited Politician.

Results are similar to figure 3-5 when respondents are asked about member ideology.

Source: Survey experiment #1.
Figure D5: Predicted Partisanship in Experiment #1, Among Knowledgeable Respondents.

Respondents who are more knowledgeable about politics (correctly answering at least 4 of 5 questions) are better able to distinguish between the partisanship of members who cite moderate and extreme politicians.

Source: Survey experiment #1.
Figure D6: Predicted Ideology, by Treatment Condition.

Results are similar to figure 3-6 when respondents are asked about member ideology.

Source: Survey experiment #2.
Respondents who are more knowledgeable about politics (correctly answering at least 4 of 5 questions) show distinct patterns in interpreting the partisanship of members who cite Boehner and Pelosi. In particular, they do not perceive a difference between members who criticize Boehner and those who support him, perhaps reflecting knowledge of intra-party conflicts in the Republican party.

Source: Survey experiment #2.
Appendix E: Experimental Design and Prompts

Experiment #1: Moderates versus Extremes

In this experiment, 3,010 respondents were recruited through Amazon’s Mechanical Turk (MTurk), Survey Sampling International (SSI), and Research Now (approximately equal respondents per platform). Regardless of platform, respondents were randomly sorted into four treatment conditions and received identical questions. The only notable difference across platform was that MTurk respondents viewed the members citing McCain and Paul as more Republican and conservative than respondents on other platforms. They were also less likely to vote for this member. These results are not surprising, given that MTurk respondents tend to be more knowledgeable and more liberal than the general public (Berinsky, Huber, and Lenz 2012).

The survey treatment is shown below in figure E1.

This treatment was based on actual emails sent by members of Congress in the past year. I specifically chose an issue area, veteran’s affairs, likely to be seen as non-partisan and non-ideological so that the only cue about the member would come from the citation.

After receiving this treatment, respondents were asked how likely they would be to vote for the member sending this email. They were also asked what they thought the member’s partisanship and ideology was. Finally, respondents provided a host of demographic information and answered a series of five political knowledge questions.

Experiment #2: Support versus Opposition

In this experiment, 1,019 respondents were recruited through SSI. The respondents were randomly sorted into eight treatment conditions. Specifically, (1) they were informed that the email treatment was from a Democratic or Republican member, (2) the member discussed a bill that garnered either support or opposition from (3) either John Boehner or Nancy
Figure E1: Treatment in Experiment #1.

Respondents either saw this graphic or one that cited Rand Paul, Harry Reid, or Elizabeth Warren. Graphics with Republican Senators included the name and picture of Rodney Davis, while those with Democratic Senators were purportedly sent from Mike Quigley.

**Representative Rodney Davis**

**Illinois 13th District**

Dear Friend,

Senator John McCain and I recently helped welcome World War II veterans from across the country to their memorial in Washington, DC. I was thrilled to meet some local veterans who traveled all the way from Illinois to celebrate their service together. We remain forever grateful and humbled by the service of these men and women and I truly enjoyed getting to know these great Illinoisans.

*Source: Created based on actual emails.*
Pelosi. Two of the eight treatments are shown below in figure E2.

As in experiment #1, this treatment was based on actual emails sent by members of Congress. I again used a non-partisan issue and kept as much of the language as possible equivalent across conditions. Notably, the bill in question passed regardless of the support or opposition of the political leader.

After receiving this treatment, respondents were asked their vote likelihood for and their warmth toward the member sending the email. Respondents also guessed partisanship and ideology of the member (but note that they had been previously informed of the member's partisanship). As with the first experiment, I also asked a battery of demographic and political knowledge questions.
Respondents saw one of eight graphics, represented by the examples below, where the cited politician is either Nancy Pelosi or John Boehner and the member supports or criticizes the political leader. On the previous screen, respondents were informed whether the member sending the email was a Democrat or Republican.

**Figure E2: Treatments in Experiment #2.**

Source: Created based on actual emails.
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