On Whose Door to Knock? Organized Interests' Strategic Pursuit of Access to Members of Congress*

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Abstract

Organized interests routinely seek direct contacts with, or access to, members of Congress to advance their policy goals. While studies often explore the tactics interests use to obtain access, few examine interests' strategic considerations in choosing which members to target. Further, the few empirical studies that examine organized interests' targeting strategies yield inconclusive results concerning the types of members interests target. This paper presents a conjoint experiment with federal lobbyists and policy advocates—a novel experimental sample in political science—to evaluate which types of members organized interests target when trying to advance a proposal in an empirical framework that addresses some of the inferential challenges faced by prior studies. The results indicate that organized interests target members who are undecided on or are weak supporters of the proposal of interest and who possess institutional roles providing them with influence over the proposal at its current stage of the legislative process.

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Among the diverse array of tools organized interests use to pursue their goals in Congress, such as campaign contributions, testimony before committees, and grassroots lobbying, practitioners and scholars assert that direct contacts, or in-person meetings, with members of Congress are among the most common and consequential activities in which interests engage (e.g., Baumgartner et al. 2009; Schlozman and Tierney 1986).[1,2] Direct contacts constitute access, which is a crucial antecedent to influence (Wright 1996), as access provides organized interests with a member's attention (Hall and Wayman 1990), makes interests' preferences salient to members (Wright 1990), and allows interests to cultivate relationships with members (Schlozman and Tierney 1986). Though organized interests' influence over policymaking is difficult to detect (see De Figueiredo and Richter 2014), interests with access are thought to be better positioned to mobilize members to engage in lawmaking activities amenable to their preferences (Hall and Wayman 1990) and to achieve their preferred outcomes (Baumgartner et al. 2009).

In order to leverage access, organized interests must establish direct contacts with members of Congress. Existing studies of organized interests' access-seeking activities focus on the tactics used to cultivate direct contacts, such as providing campaign contributions (Kalla and Broockman 2016) and signaling expertise (Brodbeck, Harrigan, and Smith 2013). However, few studies consider the strategic considerations interests face in deciding which members to target for direct contacts. Further, studies that examine these considerations offer an inconclusive set of findings concerning which members interests target, leaving unresolved key questions such as whether they target supporters or opponents and undecideds, members with influential institutional roles, and members whose electoral environments make them susceptible to lobbying. Importantly, extant empirical studies of organized interests' targeting behavior rely exclusively on observational data obtained through self-reporting (Austen-Smith and Wright 1994; Hall and Miler 2008; Hojnacki and Kimball

[1]Following Salisbury and Shepsle (1981), I conceive of members of Congress as the heads of "enterprises," such that their staffs are extensions of themselves. Consequently, references to organized interest contacts with members of Congress encompass contacts with both members and their staffs.

[2]Among the organized interests participating in their survey, Baumgartner et al. (2009) report that the most common lobbying activity in which respondents indicated engaging (81%) is personal contact with rank-and-file members of Congress, and over half of respondents reported making personal contact with leaders or members of relevant committees (see also Schlozman and Tierney 1986). Further, organized interest representatives often report in interviews that direct contacts are among the most important tactics through which they can advance their goals (Drutman 2015; Levine 2009).

1998, 1999, 2001; Wright 1989, 1990) or records of manifested direct contacts (Heberlig 2005; You n.d.), both of which pose inferential challenges that could account for inconsistent findings across studies. For example, retrospective self-reports are susceptible to response biases such as social desirability that could yield erroneous inferences. Additionally, in drawing on observational data, these studies have limited ability to account for unobserved confounders and isolate the independent effects of variables which are highly correlated in the real world.

This paper utilizes a conjoint experiment completed by organized interest representatives, such as federal lobbyists and policy advocates—a novel experimental sample in political science—to examine which types of members of Congress organized interests target for direct contacts when seeking to advance a live legislative proposal.[3] The experiment incorporates the three clusters of member of Congress characteristics commonly considered in extant studies of interests' targeting strategies—members' expressed preferences, institutional roles, and electoral concerns. The conjoint framework (Hainmueller, Hopkins, and Yamamoto 2014) enables us to glean insights into how organized interest representatives think by recreating the strategic environment in which they decide which members to target; as in their professional lives, they are asked to assess members' multidimensional profiles and select those with the highest expected value of direct contact. Consequently, we can use their responses to evaluate the relative effects of members' characteristics on interests' targeting choices. Further, an experimental approach helps address some of the inferential challenges encountered in observational studies of targeting choices. For example, whereas unobserved confounders and correlations among congressperson characteristics make isolating the effects of each characteristic difficult, randomization of characteristics facilitates the identification of independent causal effects. Again, by departing from data collected through retrospective self-reports, the experimental setting mitigates concerns about response bias.

The results indicate that organized interests seeking to advance a proposal make targeting deci-

[3] I focus on the context of live legislative proposals (i.e., appointments or bills that have been introduced) in order to keep my theoretical exposition and empirical analysis consistent with previous studies of organized interests' targeting behavior, which center predominantly on this context (Austen-Smith and Wright 1994; Heberlig 2005; Hojnacki and Kimball 1998, 1999, 2001; Rothenberg 1992; You n.d.). However, interests also leverage direct contacts to influence other legislative activities, such as the preceding policy formulation process (Wright 1996) and the conduct of oversight (Hall and Miler 2008), and different legislative contexts may spur different types of targeting strategies (see Baumgartner and Leech 1996*b*, 1998). In the Conclusion, I discuss how organized interests' targeting strategies may differ when they seek to influence other such activities.

sions on the basis of members' expressed preferences and institutional roles. In the former instance, organized interests are more likely to target members who have not declared a position on or are weak supporters of a proposal relative to members identified as opponents or strong supporters. In the latter case, organized interests are more likely to target members whose institutional roles provide influence over the proposal given its current stage in the legislative process, including members of the proposal's committee of jurisdiction, majority party leaders on that committee, and rank-and-file majority party members. Differently, the results provide no evidence that organized interests target members on the basis of their electoral concerns, such as their electoral vulnerability, the interest's latent strength in their districts, and whether the interest donated to their campaigns.

These results contribute to our understanding of how organized interests seek to influence the policymaking process in Congress. First, organized interests' focus on targeting weak supporters or undecideds suggests that interests seek to influence legislative outcomes by changing the underlying distribution and intensity of preferences rather than reinforcing the pre-existing biases of their allies. Given that interests tend to over-represent businesses and the upper-class (Gilens and Page 2014; Schlozman, Verba, and Brady 2012), this finding implies that lobbying may lead members to adopt preferences that differ from those that best serve their constituents. Second, organized interests' attention to members' institutional roles indicates that they recognize the ability to influence outcomes indirectly through procedural, as opposed to substantive, means; through access to agenda setters, organized interests can accomplish their goals by promoting or inhibiting a proposal's progress (Fouirnaies 2018; Fouirnaies and Hall 2018; Powell and Grimmer 2016). These findings have troubling normative consequences because they imply that organized interests can use direct contacts to distort policy outcomes relative to those preferred by the public.

The Strategic Targeting Choices of Organized Interests

While resources are distributed unevenly in the organized interest universe, such that a small subset of interests wield a disproportionate share (Schlozman, Verba, and Brady 2012), even the most well-resourced interests face time and resource constraints precluding them from seeking direct contact

with all 535 members of Congress. Thus, organized interests must decide which members to target by considering the expected value of direct contact with each member and choosing to target the subset of members with whom direct contacts would best advance their goals.

Organized interests' expected value of direct contact with a given member hinges on their assessments of the degree of influence the member wields over the proposal and the extent to which the member is susceptible to the interests' lobbying (Hall and Deardorff 2006; Hojnacki and Kimball 1998; Levine 2009). While organized interests lack full information about members' influence and susceptibility to lobbying in any given situation, they can draw on members' characteristics to enlighten their calculations. Previous studies of organized interests' targeting behavior posit three clusters of member characteristics which interests use to inform their choices: members' expressed preferences regarding the proposal, institutional roles, and electoral concerns. In the following subsections, I first describe the theoretical underpinnings of each cluster, and then discuss the empirical evidence presented in previous studies and the inferential challenges they face.

Expressed Preferences

One of the enduring questions in the study of organzied interests is whether interests seek to influence supporters—members who share their preferences—or opponents and undecideds—members who hold preferences opposed to those of the interest or whose preferences are unclear. While this question applies to other facets of organized interests' lobbying activities, such as grassroots operations (Kollman 1998), it lies at the core of nearly all studies of interests' targeting of members for direct contacts. Proponents of the theory that interests pursue direct contacts with supporters argue that they can most effectively pursue their goals by working with members who share their preferences. Because organized interests cannot participate in legislative activities themselves, they need members of Congress to perform these activities for them, and like-minded members are more receptive audiences to interests' entreaties for action than are opponents or undecided members (Levine 2009). Convincing an opponent or undecided member to act for an interest requires both changing the member's preferences and inducing the member to act, but prompting a supporter to act requires only the latter task, making allies more efficient partners (Hall and Deardorff 2006).

Therefore, organized interests seek direct contacts with supporters in order to encourage them to undertake legislative activity and lobby their colleagues in pursuit of their shared preferences, and to subsidize their activities through providing information, expertise, and other resources (e.g., Hall and Deardorff 2006; Hojnacki and Kimball 1998, 1999, 2001; Schnakenberg 2017).

In contrast, advocates of the theory that organized interests emphasize direct contacts with opponents and undecideds argue that interests focus on expanding the size of their supportive coalitions. A proposal's success throughout the legislative process ultimately hinges on the distribution of support for the proposal among legislators; excepting rare circumstances, a bill only advances from committee, to the floor, and through a final vote if it enjoys support from a majority of the members at each step. When the number of members who share the preferences of an organized interest is too small to achieve the interest's desired outcome, the interest must persuade opponents and undecideds to adopt its position (Austen-Smith and Wright 1994; Heberlig 2005; Rothenberg 1992). Successful persuasion of opponents and undecideds helps interests enlarge their coalitions in two ways. First, each opponent or undecided they enlist increases the size of their coalition by one. Second, opponents and undecideds that an interest converts can more credibly lobby their colleagues than can members who initially shared the interest's preferences, thereby helping the interest augment their coalition (Awad 2020; Ellis and Groll 2020). Thus, organized interests focus their direct contacts on opponents and undecideds who they seek to transform into supporters rather than on members whose support they already enjoy (e.g., Austen-Smith and Wright 1994; Awad 2020; Ellis and Groll 2020; Heberlig 2005; Rothenberg 1992).

Institutional Roles

While all members retain a vote of equal weight when a proposal faces a floor vote, some members wield additional influence over the proposal by nature of their institutional roles, making them valuable targets for organized interests. Members' augmented influence can manifest in two different ways. First, some members hold institutional roles that provide them formal procedural control over a proposal. For example, given the party-centric institutional structure of the modern Congress (Cox and McCubbins 2005), majority party support is a virtual prerequisite for a proposal's success,

rendering majority party members more consequential than minority members. Further, members of a proposal's committee of jurisdiction maintain nearly exclusive control over the proposal prior to its consideration on the floor; most substantive work on a proposal takes place in committee, and a proposal rarely advances to the floor without the committee's recommendation (Deering and Smith 1997). Additionally, congressional leaders, particularly those affiliated with the majority party, possess unique leverage over a proposal's fate. Chamber leaders wield several procedural powers that can affect a proposal's progress, such as the Speaker of the House's ability to appoint members of the Rules Committee and refer a proposal to one or more committees (Oleszek 2014). Leaders of the committee of jurisdiction also possess tools with which to influence a proposal's fate; committee and subcommittee chairs exert control over all aspects of the process, including scheduling hearings and markups while the proposal is in committee, and often serving as managers when the proposal moves to the floor (Deering and Smith 1997; Hall 1996; Oleszek 2014).

Second, some institutional roles allow members to informally influence their colleagues. Much of this influence operates through cue-giving; among the field of forces informing members' decisions is the information provided by and preferences of colleagues (Kingdon 1973). Senior members are often looked to as cue-givers because their experience suggests that their inclinations optimize electoral and policy goals (Hall and Deardorff 2006). Members also take cues from colleagues on the committee of jurisdiction, as committee members' expertise and familiarity with the proposal implies that they have intimate knowledge of the proposal and its implications (Krehbiel 1991). Finally, members attend to cues provided by party leaders in expectation that they will guide members towards the party's best course of action and that the party will reward or sanction them on the basis of their cue-taking (Cox and McCubbins 2005). Taken together, organized interests are more likely to target members with institutional roles that provide them with formal or informal influence over a proposal, including majority party membership, membership on the committee of jurisdiction, chamber or committee leadership positions, and seniority (e.g., Hall and Deardorff 2006; Hojnacki and Kimball 1998, 1999, 2001; Wright 1990; You n.d.).

Electoral Concerns

As single-minded seekers of re-election (Mayhew 1974), members of Congress are responsive to outside actors such as organized interests who can help them achieve their electoral goals. One of the key electoral resources that organized interests offer members is information about the anticipated consequences of a proposal for their constituents (Hansen 1991). While this information is valuable to all members given intrinsic electoral uncertainty, it is particularly valuable to members from marginal districts who have little room for error. In addition, organized interests can activate realized and latent bases of support in members' districts in aid or opposition to members' electoral efforts (Hojnacki and Kimball 1998, 1999, 2001), and organized interests' electoral activities can be consequential when they enjoy a broad base of support in the district (Hall and Reynolds 2012). Further, as congressional elections become ever more expensive (Jacobson 2004), incumbents are attuned to donors' preferences in order to attract campaign contributions, and organized interests use contributions as tools to attain access to and induce responsiveness from members of Congress (Miler 2010; Powell and Grimmer 2016). Thus, organized interests are more likely to target members whose electoral concerns make them more receptive to interests' entreaties, such as members who are electorally vulnerable, whose districts contain larger bases of support for the interest, and to whom they have made campaign contributions (Austen-Smith and Wright 1994; Heberlig 2005; Hojnacki and Kimball 1998, 1999, 2001; You n.d.; Wright 1989, 1990).

Extant Empirical Evidence

Despite clear theoretical expectations concerning which types of members organized interests target for direct contacts, extant empirical studies of targeting behavior yield a collectively inconclusive set of results. In some cases, empirical analyses of the same characteristic produce conflicting findings. For example, whereas Hojnacki and Kimball (1998, 1999, 2001) find that interests are more likely to seek direct contacts with members who share their preferences, Austen-Smith and Wright (1994), Heberlig (2005), and Rothenberg (1992) conclude that interests emphasize contacts with opponents or undecideds. Additionally, though Austen-Smith and Wright (1994) and Hojnacki and Kimball (1998, 1999, 2001) find a direct relationship between an interest's base of support in a member's

district and the likelihood of that interest targeting that member, Rothenberg (1992), Wright (1990), and You (n.d.) fail to find evidence for this relationship, and Heberlig (2005) presents evidence of an inverse relationship. In other cases, analyses of the same characteristic provide ambiguous results. For instance, in the case of campaign contributions, though many studies argue that organized interests make contributions to obtain access (e.g., Ansolabehere, De Figueiredo, and Snyder 2003; Powell and Grimmer 2016), Hojnacki and Kimball (2001) assert that the effect of contributions on targeting is small and manifests only for legislative opponents, and Wright (1989) argues that the association between contributions and targeting is spurious, driven by the intensity of the interest's support in the member's district.

Though the contexts utilized in the above-cited studies differ in several ways, such as their temporal scope and the focal type of legislative activity,[4] they are all motivated by the same fundamental question—which types of members of Congress organized interests target—and share at least two design features that may underlie the inconclusiveness of their results. First, each of these empirical studies rely on observational data collected through records of organized interests' direct contacts (Heberlig 2005; Rothenberg 1992; You n.d.) or retrospective surveys administered to organized interests (Austen-Smith and Wright 1994; Hojnacki and Kimball 1998, 1999, 2001; Wright 1989, 1990), both of which pose inferential challenges that may yield divergent results across studies. One such challenge stems from the presence of unobserved confounders, an ever-present concern in observational analyses; should any omitted variable, such as a member's personal expertise, be correlated with member characteristics of interest and targeting behavior, then parameter estimates for those characteristics will be biased. In addition, many of the member characteristics of interest exhibit substantial correlation, such as members' seniority and possession of leadership positions,

[4]For example, whereas Austen-Smith and Wright (1994) examine organized interests' targeting choices during a Supreme Court confirmation fight, other studies focus on interests' efforts to advance specific bills (You n.d.), stimulate oversight activity (Hall and Miler 2008), or participate in policymaking in broad issue areas (Hojnacki and Kimball 1998), and different types of members may be attractive targets for organized interests across each of these goals. Again, these studies cover a wide temporal range, spanning from the 1950s (Heberlig 2005) to the 2000s (You n.d.), and institutional changes and polarization may condition interests' targeting choices across studies. As Baumgartner and Leech (1996*b*) note, these and other contextual differences across studies, including the the salience of and degree of conflict surrounding the underlying issues and the procedural mechanisms surrounding the proposals, may contribute to inconsistencies in their empirical findings (see also Baumgartner and Leech 1996*a*, 1998). While contextual differences are of theoretical interest, I focus here on the studies' common design features which pose challenges to inference and encourage researchers to investigate how the contexts in which organized interests lobby inform their strategies with designs that address these challenges in future work.

which makes it difficult to isolate independent effects.

Studies relying on retrospective self-reports from organized interests are susceptible to a further challenge in the form of response bias, or the possibility that respondents' responses deviate from their realized experiences. Some deviations may arise randomly, such as respondents' failure to remember all of their organizations' contact attempts; this random error would, in aggregate, attenuate estimates. Differently, deviations may arise non-randomly due to social desirability concerns, such as respondents exaggerating their influence by over-reporting contacts with congressional leaders or concealing their contacts with opponents in order to avoid upsetting their donors should their responses become public (Li 2018). This non-random error would bias estimates, in the first case overestimating targeting of congressional leaders, and in the second case underestimating targeting of opponents. Because studies retrospective self-reports rely on small numbers of unique respondents (e.g. 75 in Austen-Smith and Wright 1994; 69 in Hojnacki and Kimball 1998), the presence or absence of only a few respondents providing biased answers could produce substantively different estimates across studies.

Second, the above cited studies seldom consider how the stage at which a proposal resides may condition targeting behavior (but see Heberlig 2005; You n.d.), such that inconsistent findings across studies may stem from differences in legislative context (see Leech 2010). Once a legislative proposal is introduced in Congress, it must pass through two general stages in its chamber of origin: the committee and floor stages. Most studies of targeting behavior focus their attention on either the committee (Hojnacki and Kimball 1998, 1999, 2001; Wright 1989, 1990) or floor stage (Austen-Smith and Wright 1994; Rothenberg 1992), or remain agnostic as to the stage of the legislative process on which they focus (Awad 2020; Ellis and Groll 2020; Schnakenberg 2017). However, as a proposal moves through the legislative process, its advancement hinges on different types of members, and organized interests should focus their targeting choices on members whose support is most critical at the present stage. A few examples of potential stage-conditional effects illustrate this point. First, because the committee of jurisdiction maintains primary control over a proposal until it is reported to the floor, committee members and leaders enjoy unique power over the proposal during the committee stage, making them attractive targets (Hojnacki and Kimball

1998). Second, once a proposal reaches the floor, where its ultimate fate depends on the outcome of the final roll call vote and prerequisite procedural votes, "the operative goal for [organized interests] will be to expand their coalition to winning size" (Hojnacki and Kimball 1998, 777; see also Hall and Deardorff 2006, 78-80). At this stage, organized interests can expand their coalition by persuading members who are undecided or opposed to their position (Austen-Smith and Wright 1994; Ellis and Groll 2020; Heberlig 2005) or enlisting into their coalition members who exert procedural control at the floor stage, such as leaders and majority party members (Hall and Deardorff 2006; Hojnacki and Kimball 1998).

Recognizing that a proposal's stage can condition organized interests' targeting behavior may clarify some discordant findings in extant empirical studies. For example, empirical studies suggesting that organized interests target supporters focus on the committee stage (Hojnacki and Kimball 1998, 1999, 2001; Wright 1990), whereas studies indicating that organized interests target opponents and undecideds concentrate on the floor stage (Austen-Smith and Wright 1994; Heberlig 2005; Rothenberg 1992). This dichotomy suggests that organized interests may target supporters when they need them to exert extra effort to advance a proposal through committee, but shift attention to opponents and undecideds once the proposal moves to the floor and success depends on votes. In fact, Hall and Deardorff (2006), often invoked as a canonical cite for interests' tendency to target legislative allies, anticipate this dichotomy in positing that "lobbying as subsidy" directed at supporters reigns when lobbyists' objectives require legislators to exert effort, but that lobbyists likely default to "a conventional preference-centered strategy" focused on undecideds and opponents under certain conditions, such as when "[t]he legislator is perceived to have a weak preference... a specific matter is likely to be decided by a public vote... [and] the outcome of the vote is thought to be in doubt" (Hall and Deardorff 2006, 79)—scenarios which characterize the committee and floor stages, respectively. Again, when considering organized interests' targeting of committee members, Heberlig (2005)'s finding that the AFL-CIO was no more likely to contact committee members when legislation of interest was considered on the floor may indicate that organized interests' preference for targeting committee members dissipates once the bill reaches the floor. More careful attention to targeting efforts across stages of the legislative process may

elucidate such stage-conditional effects.

Research Design

To address the challenges described above, I conduct a conjoint experiment with organized interest representatives, such as federal lobbyists and policy advocates, which offers several inferential advantages for examining interests' targeting choices. First, an experimental framework that randomizes member characteristics enables me to isolate the causal effect of each characteristic on targeting choices without concerns about unobserved confounders or correlations among characteristics. Second, by using a conjoint experiment to observe organized interest representatives' behavior, I can collect data on the phenomenon of interest while mitigating the response bias inherent in retrospective self-reports. Third, a flexible experimental setting facilitates randomization of the legislative stage in which the proposal is situated, providing an opportunity to assess how targeting differs across stages of the legislative process.

Experimental Sample

To recruit organized interest representatives to participate in the survey which included the experiment, I draw on persons listed as points of contact on quarterly Lobbying Disclosure Act (LDA) reports filed between the first quarter of 2017 and the third quarter of 2018.[5] The reports in my sampling frame contain 5,938 unique individuals, each of which provides an email address. Initial invitations to complete the survey were sent to all points of contact on November 15, 2018, and reminder emails were sent to those who had not yet responded on November 29, 2018 and December 13, 2018.[6] Data collection ceased on December 31, 2018, at which point 670 respondents participated in the experiment; excluding points of contact whose email addresses were identified as

[5]See the Appendix section "Sampling Procedure and Descriptive Statistics" for details about reporting requirements under LDA and the sampling procedure.

[6]The survey was fielded during the "lame duck" session of the 115th Congress, which offers two important advantages for the experiment. First, given the dearth of new legislative activity during lame duck sessions, organized interest representatives have fewer work commitments and are thus more likely to complete the survey. Second, the experiment minimizes concerns about pre-treatment by asking respondents to prospectively consider targeting in the upcoming 116th Congress, which would not only include new members but also witness a change in party control from the Republicans to the Democrats. In doing so, respondents are less likely to draw on idiosyncratic experiences from the 115th Congress to inform their responses.

invalid when initial invitations were sent, the response rate for the experiment is 12.3% ($\frac{670}{5458}$).[7] Information about the descriptive characteristics of the sampling frame and respondents are presented in Tables A.4 and A.5.

Experimental Design

To simulate organized interest representatives' targeting decisions, I design a conjoint experiment (Hainmueller, Hopkins, and Yamamoto 2014). In brief, a conjoint framework presents respondents with a set of profiles and asks them to evaluate each profile and/or select the profile they most prefer. All profiles contain information about a set of attributes, with each profile randomly assigned a level for each attribute. Through this randomization, researchers can estimate the effect of each attribute-level on respondents' evaluations or choices. Conjoint experiments promote external validity because they accommodate a large number of attributes, allowing researchers to recreate the contextually rich environment in which respondents entertain several considerations when making evaluations and choices, and because the inclusion of several attributes decreases social desirability bias by enabling respondents to base choices on preferences they might otherwise conceal (Hainmueller, Hopkins, and Yamamoto 2014; Horiuchi, Markovich, and Yamamoto n.d.).[8]

After completing pre-treatment questions, respondents are presented with two consecutive conjoint tasks. In each task, respondents are asked to imagine they have been retained as a lobbyist for a fictitious organized interest to manage its lobbying efforts in support of a proposal under consideration in the House of Representatives in the upcoming 116th Congress. Respondents are told the interest wants them to arrange in-person meetings with House members as part of their efforts and presented with profiles of two members which executives have suggested as possible targets. Finally, respondents are asked to indicate their level of interest in meeting with each member on a

[7] This response rate compares favorably with those of mass public surveys and recent survey experiments of American political elites. See Appendix section "Sampling Procedure and Descriptive Statistics" for details.

[8] Mitigating social desirability bias is important because respondents may be reticent to reveal that their targeting strategies rely on socially undesirable considerations, such as whether their organized interest donated to a member's campaign. While organized interest representatives may leverage contributions to secure direct contacts, they may conceal this tendency because it gives the impression that they "buy" access. However, respondents would be more likely to reveal this preference, if it exists, in a conjoint framework because the large number of attributes gives the impression that such socially undesirable choices cannot be detected and allows them to draw on other attributes to rationalize socially undesirable choices.

Imagine that you have been retained as a lobbyist by the American Coalition of Retailers (ACR), a national trade association of brick-and-mortar retailers.

ACR executives have asked you to manage their strategy for lobbying Congress to pass a House bill that would require online retailers to collect and remit sales tax payments for all in-state and out-of-state transactions. This bill was recently voted out of the Judiciary Committee and will soon be considered on the floor.

As part of your lobbying efforts, ACR suggested that you try to arrange meetings with House members. Presented below are the profiles of 2 House members that ACR executives suggested that you consider contacting to set up meetings.

	Member 1	**Member 2**
Number of retail employees in member's district	30,000 to 34,999	More than 40,000
Did ACR's PAC donate to the member in the last cycle?	No	Yes; $1000
Party	Democrat	Democrat
Margin of victory in last election	20% to 30%	10% to 20%
Judiciary Committee member	No	No
Judiciary Committee leadership position	None	None
Stated position on internet sales tax bill	Undeclared	Support
Cosponsor of internet sales tax bill	No	Yes
Number of previous terms served	7 terms	Freshman

How interested would you be in meeting with each of these members to lobby in support of this bill?

	Not at all interested	Slightly interested	Somewhat interested	Very interested	Extremely interested
Member 1	○	○	○	○	○
Member 2	○	○	○	○	○

If you could only arrange a meeting with **one** of these House members, with which member would you prefer to meet?

Member 1

Member 2

Figure 1: **Example of Conjoint Tasks Provided to Respondents.**

five-point scale and select which member they would prefer to meet. Figure 1 presents an example conjoint task.

The House member profiles consist of nine attributes drawn from the three clusters of member characteristics posited by extant studies to inform organized interests' targeting strategies.[9] Two attributes provide information about the member's expressed preferences—the member's stated position on the proposal and whether the member is a cosponsor of the proposal. Four attributes convey information about the member's institutional roles—the member's party affiliation, membership on the committee with jurisdiction over the proposal, leadership role on the committee of jurisdiction, and seniority.[10] Finally, three attributes impart information about the member's electoral concerns—the member's margin of victory in the last election, the number of residents in the member's district who could be directly affected by the proposal (i.e., the size of the interest's latent base of support in the district), and the amount of money the PAC of the organized interest retaining the respondent donated to the member in the last election cycle.

Two other elements of each task are randomized. First, to encourage respondents to consider each task independently and demonstrate generalizability across issue contexts, each respondent completes one task centering on legislation to facilitate an internet sales tax, and another task concentrating on a proposal to allow the Centers for Medicare & Medicaid Services to negotiate prices with pharmaceutical companies, with the ordering of issue contexts randomized across re-

[9]See Tables A.1 and A.2 for information about attributes, attribute-levels, and restrictions. While most profile attributes are independently randomized, two sets of restrictions are implemented to exclude a small number of implausible profiles. First, members can only be cosponsors if their stated position for the proposal is "support." Second, members can only serve as subcommittee chairs or ranking members on the committee of jurisdiction if they are members of the committee and Democrats or Republicans, respectively.

[10]The three levels for the leadership role attribute are none, subcommittee chair, and subcommittee ranking member. The choice to utilize subcommittee leadership roles rather than full committee leadership roles, which have been the focus of previous studies of (e.g., Hojnacki and Kimball 1998) and have more institutional power, is a consequence of research design. Given that attribute-levels are independently randomized across profiles, both profiles in a given randomization can take on the same levels for the same attribute. While this feature is often not troublesome, it can yield implausible scenarios if both profiles are assigned an attribute-level which only one can possess, such as holding the chair of a full House committee. Consequently, I use subcommittee leadership positions as attribute-levels, as each committee has multiple subcommittees with their own chairs and ranking members. While subcommittee chairs and ranking members wield less power than their full committee counterparts, they exercise authority on proposals before their subcommittees and their positions signal influence with committee and party leadership (Hall 1996). At worst, using subcommittee rather than committee leadership positions should bias downward estimates of the effect of members holding leadership positions on organized interests' interest in or probability of targeting those members, such that the effect of members' possession of a full committee or chamber leadership role would be greater than that recovered here.

spondents.[11] Second, to account for the conditional effects of the stage of the legislative process at which targeting occurs, the vignettes prefacing each task randomly situate the proposal in the committee or floor stage.

External Validity

An experiment's ability to provide insights into real-world behavior hinges on its' external validity (Gaines, Kuklinski, and Quirk 2007), and both the sample of respondents completing the experiment and the mechanics of the conjoint tasks promote external validity. First, because targeting members of Congress for direct contacts is a concern exclusive to organized interest representatives, who draw on their specialized knowledge and expertise to perform this behavior, recruiting participants from that population, as opposed to the general public, enables me to draw conclusions generalizable to the population of interest. Second, a conjoint experiment is well-tailored to simulate the decisionmaking process in which organized interest representatives engage. When representatives make real-world targeting decisions, they start with a set of potential targets (e.g., all 535 members of Congress, members of a single committee), and winnow to a subset of members that collectively offers the highest expected value from targeting those members for direct contacts, subject to their interest's resource constraints. In that winnowing process, these representatives make choices by comparing the multidimensional profiles of each member, which includes attributes such as the member's expressed preferences and committee membership, to those of the other members under consideration.[12] Similarly, in a conjoint framework, respondents are presented with a set of multidimensional profiles and asked to evaluate each profile and/or select the profile they

[11] Ensuring that respondents process each task independently is critical to satisfy the stability and no carryover effects assumption (Hainmueller, Hopkins, and Yamamoto 2014, 8). If respondents completed multiple tasks focused on the same issue, they may condition responses in later tasks on their responses in earlier tasks. For example, if a respondent selected a member of the committee with jurisdiction over the proposal in the first task, then she may be more likely to select a member in the second task who is not a committee member but has other desirable traits. In selecting issue contexts, I draw on bills introduced in the 115th Congress that lack clear partisan implications and that members of either party could plausibly support or oppose; this selection criterion ensures that respondents would not encounter profiles with implausible combinations of stated position and party and that respondents could not draw informative inferences about members' underlying preferences from their party affiliation. Please see Appendix section "Conjoint Experiment Protocol" for more information about the issue contexts and how they satisfy this criterion.

[12] The interviews in Leech (2014) illustrate this process. In several chapters, the interviewer asks the lobbyist from which members of Congress they seek direct contacts, and the lobbyist responds with a list of member characteristics which make them desirable targets.

most prefer. Thus, this method of eliciting organized interest representatives' targeting preferences captures the microfoundational behavior—comparisons among members under consideration—that underlies their higher-order activities, such as deciding which members to ultimately target. Therefore, this conjoint experiment simulates the real-world choice task of interest with the very political actors who make these choices.

Like all experiments, the experiment inevitably simplifies organized interest representatives' strategic environments such that some factors relevant to their decisionmaking process are absent. For instance, while lobbyists and policy advocates are often familiar with many of the policymakers relevant to their issues and their existing relationships with their potential targets inform their targeting choices (Hirsch et al. n.d.; McCrain 2018; Vidal, Draca, and Fons-Rosen 2012), the experiment omits this consideration because it is difficult to manipulate respondents' lived experiences in externally valid ways. However, despite this omission, the experiment reflects common scenarios representatives encounter in the real-world and isolates the causal effect of members' characteristics where this and other omitted considerations are held constant. Focusing again on the the role of representatives' relationships with potential targets, even well-connected lobbyists and policy advocates face targeting contexts where they have little experience with some potential targets. Congressional turnover, variation in members' priorities given constituent demands or changes in the national agenda, and members' voluntarily or involuntary movement between committees (Powell and Grimmer 2016) can force even the most well-connected interest representatives to consider targeting members relevant to their issue areas with whom they have little experience. Additionally, as proposals move through the legislative process, more legislators become relevant to their dispositions and representatives may need to target members outside of their issue areas with whom they are not familiar. Further, even when representatives have similar relationships with all members they consider targeting, they must still select among those members, and differences in members of Congress' characteristics inform those decisions. Thus, in a general sense, by abstracting away other considerations in interest representatives' targeting decisions, the experiment embodies a scenario where those absent considerations, such as representatives' relationships with potential targets, are held constant across the members from which respondents are asked to

choose in each task, therefore isolating the causal effects of the member of Congress characteristics presented in the profiles.

Empirical Analysis

Estimation Strategy

For both the ordinal and binary outcomes, I use the estimation strategy outlined by Hainmueller, Hopkins, and Yamamoto (2014). The causal quantity of interest is the average marginal component effect (AMCE), or the change in the expected outcome for a profile with a given attribute-level relative to an otherwise identical profile with the baseline level of that attribute. When assessing the effect of attribute-levels conditioned by other factors, such as legislative stage, the causal quantity recovered is the average component interaction effect (ACIE), or the change in the expected outcome for a profile with a given attribute-level relative to an otherwise identical profile with the baseline level of that attribute subject to the specified condition. The estimates presented below are obtained through linear regression using the `cjoint` package in `R`. To account for non-independence across respondents, I utilize cluster robust standard errors clustered on respondent. Finally, in light of the large number of hypothesis tests in each set of analyses—20 when pooling responses across legislative stage, 40 when conditioning by legislative stage—I implement Bonferroni corrections to account for multiple comparisons.[13]

Results

Figure 2 presents the AMCEs for each attribute-level on the probability that an organized interest representative selects a House member to target when pooling responses across issue context and legislative stage.[14] Overall, respondents' targeting choices are influenced by members' expressed

[13]The number of hypothesis tests includes comparisons between each attribute-level and its baseline and between the non-baseline levels for a member's position on the proposal. The number of hypotheses tested when conditioning by legislative stage doubles because a hypothesis test is conducted twice for each attribute-level (once for each stage).

[14]Due to space constraints, I present results using respondents' choice of which member they would prefer to meet here, and present results for respondents' ordinal measures of interest in meeting with each member in Appendix Tables A.8 and A.9. The results obtained using the ordinal outcome are substantively similar to those presented here, though a few attribute-levels are statistically distinguishable from their baselines when using the binary outcome, but not when using the ordinal outcome. In Appendix section "Empirical Results," I discuss these differences and explain

preferences and institutional roles. With respect to members' expressed preferences, respondents are most likely to select members who have not yet declared a position on the proposal; as compared to members who support or oppose the proposal, respondents are 22 and 19 percentage points more likely to target members who had not yet declared a position, respectively. Further, respondents are 14 percentage points less likely to select supporters who cosponsored the proposal, who can be seen as strong supporters,[15] relative to members who merely expressed support for the proposal but did not exert a higher level of effort through cosponsorship.[16] These results suggest that organized interest representatives target members whose preferences or levels of effort are perceived as malleable; organized interests can expand the size of their supportive coalition by persuading members who have not yet declared positions to support the proposal (Austen-Smith and Wright 1994), and they can increase the level of effort exerted on behalf of the proposal by encouraging and subsidizing supporters who are not cosponsors (Hall and Deardorff 2006).

Respondents are also more likely to target members whose institutional roles provide them with influence over the proposal. Looking first at party affiliation, respondents are 10 percentage points more likely to select members of the Democratic majority, whose support is crucial for the proposal's procedural advancement, than of the Republican minority (Cox and McCubbins 2005). Turning to committee membership, respondents are 17 percentage points more likely to select members of the committee with jurisdiction over the proposal, who spend more time on and exercise more control over the proposal relative to other members (Hojnacki and Kimball 1998). Among committee members, subcommittee chairs are particularly attractive targets, as respondents are 16 percentage points more likely to select a subcommittee chair relative to a rank-and-file majority party committee member.[17] However, unlike the other attributes indicative of members' institutional roles, respondents are no more likely to target more senior members.

why the binary outcome more appropriately characterizes the targeting decisions organized interest representatives make.

[15]Members' preferences are expressed not only by their issue positions, but also through the level of effort they exert in expressing those preferences. Thus, members who go beyond perfunctory activities such as roll call votes and engage in more active levels of participation, such as cosponsorship, are seen as stronger supporters or opponents (Hall 1996).

[16]Because restrictions require that members identify as supporters in order to also serve as cosponsors, the baseline level for the cosponsorship attribute is a supporter who is not a cosponsor.

[17]As with the cosponsorship attribute, restrictions on the subcommittee leader (ranking member) attribute-level make rank-and-file majority (minority) party committee members the appropriate baseline level.

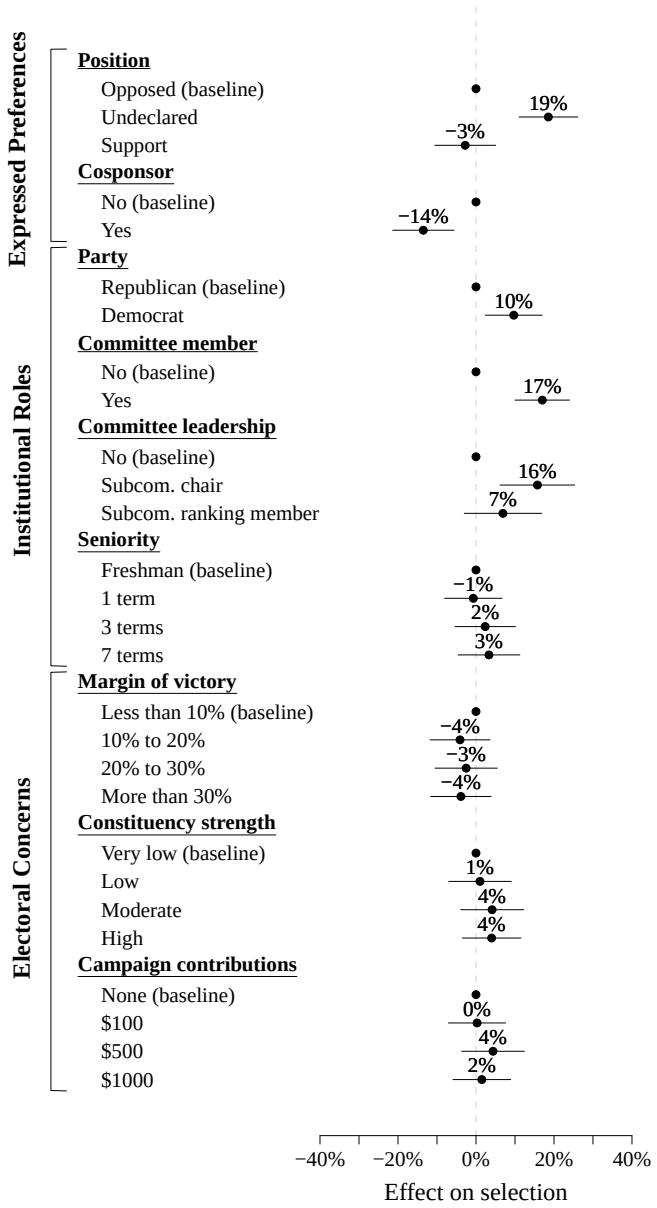

Figure 2: **Effect of Legislator Attributes on Targeting Choices.** Points represent average marginal component effects (AMCEs) for each attribute-level (relative to the baseline attribute-level) on the probability of selection, and bars around point estimates represent Bonferroni-adjusted 95% confidence intervals ($\alpha = \frac{0.05}{20} = 0.0025$). Positive (negative) values along the x-axis reflect the percentage point change in the probability that a member with a given attribute-level will be selected as a target in a binary choice task relative to an otherwise identical member with the baseline attribute-level. Respondents' targeting choices are largely informed by members' expressed preferences and institutional roles. For example, as compared to members who are opposed to or supportive of the legislation of interest, members who have not declared a position are 19 percentage points and 22 percentage points more likely to be selected as a target, respectively.

In contrast to the attributes related to members' expressed preferences and institutional roles, none of the attributes respecting members' electoral concerns—the members' margins of victory in their previous elections, the number of residents in members' district who could be directly affected by the proposal, or the amount of money the organized interest's PAC donated to members—manifests a distinguishable effect on respondents' choices. This lack of evidence contrasts with expectations that organized interests target members whose electoral environments make them more receptive to the interests' entreaties (Austen-Smith and Wright 1994; Hojnacki and Kimball 1998), as well as with studies of lobbying and money in politics which suggest that organized interests make campaign contributions in expectation that recipients will provide them access (Ansolabehere, De Figueiredo, and Snyder 2003; Powell and Grimmer 2016).

To assess whether organized interests' targeting choices vary by legislative stage, Figure 3 presents the ACIEs for each attribute-level on the probability that a respondent selects a member to target, conditioned by whether the proposal is at the committee (left panel) or floor (right panel) stage. Most of the patterns observed when pooling across legislative stages persist, though the magnitudes of some of the statistically distinguishable characteristics vary. For example, respondents are between 15 and 27 percentage points more likely to target members who have not yet declared a position on the proposal relative to declared supporters and opponents in the committee and floor stages, a strategy consistent with expanding their supportive coalition rather than working with legislative allies. Subcommittee chairs remain attractive targets in both stages, with respondents more likely to target them by 13 and 18 percentage points across the committee and floor stages, respectively.[18] Finally, there is no evidence that respondents' targeting decisions are influenced by members' seniority or electoral concerns in either stage.

However, the effects of three attribute-levels differ in substantively meaningful ways when conditioned by legislative stage.[19] First, among legislative supporters, organized interest representatives

[18]The estimates for holding a subcommittee chair are statistically distinguishable at the Bonferonni-adjusted 90% confidence level in both stages, but only the floor stage estimate is statistically distinguishable at the Bonferonni-adjusted 95% confidence level.

[19]Only the ACIEs associated with committee membership are statistically distinguishable across stages, such that the effect of committee membership on the probability of selection is greater in the committee stage than in the floor stage. For all other attribute-levels, the ACIEs across the two stages are not statistically distinguishable from each other.

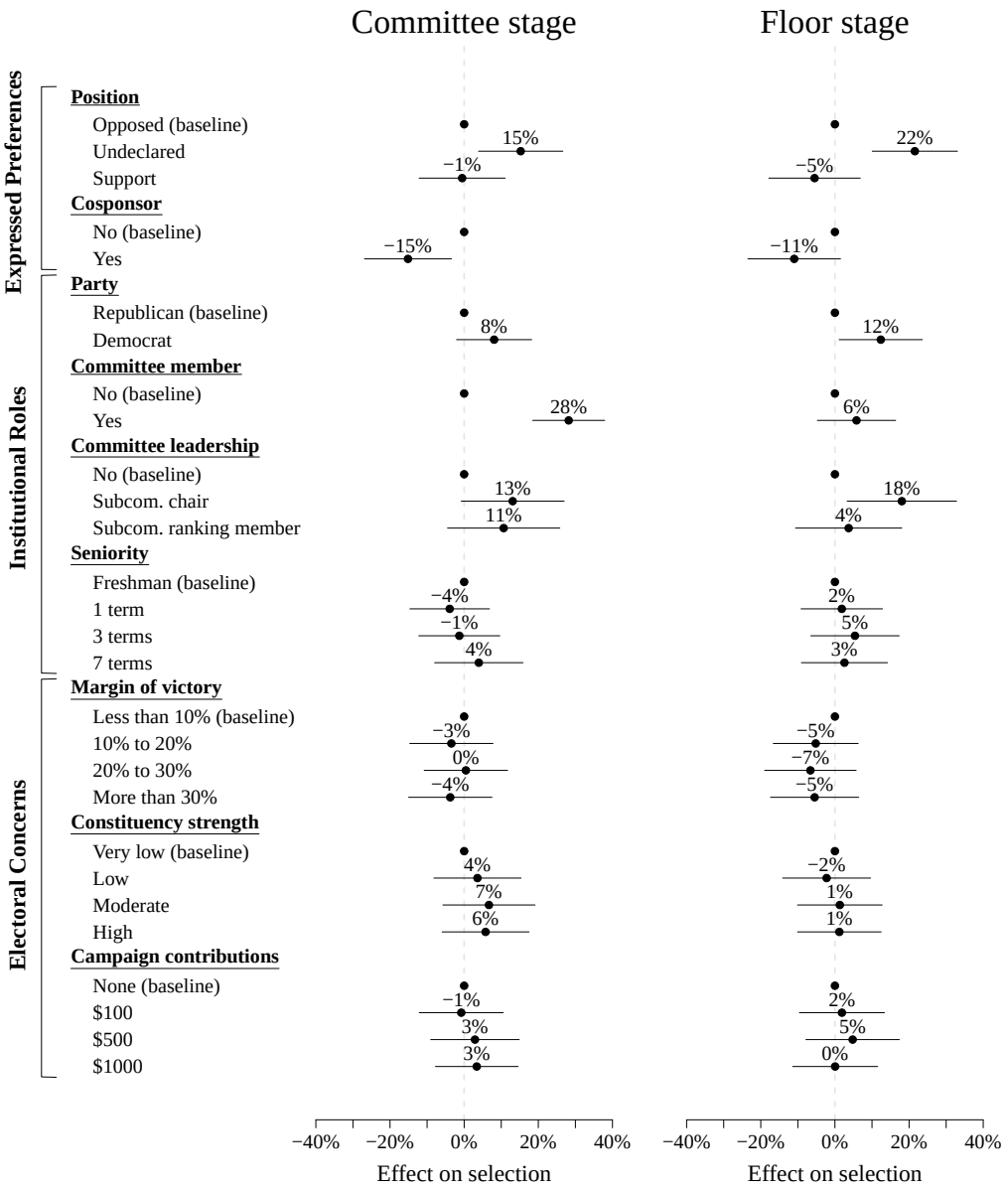

Figure 3: **Effect of Legislator Attributes on Targeting Choices Conditioned by Stage.** Points represent average component interaction effects (ACIEs) for each attribute-level (relative to the baseline attribute-level) on the probability of selection as conditioned by the stage of the legislative process, and bars around point estimates represent Bonferroni-adjusted 95% confidence intervals ($\alpha = \frac{0.05}{40} = 0.00125$). Positive (negative) values along the x-axis reflect the percentage point change in the probability that a member with a given attribute-level will be selected as a target in a binary choice task relative to an otherwise identical member with the baseline attribute-level when situated in the specified stage of the legislative process. The effects presented resemble those displayed in Figure 2, albeit with a few substantively important distinctions across legislative stages. For example, whereas members who sit on the committee of jurisdiction for the proposal are 28 percentage points more likely to be selected as a target as compared to members who do not sit on the committee during the committee stage, this difference shrinks to 6 percentage points once the proposal moves to the floor stage and is no longer statistically distinguishable.

are 15 percentage points less likely to target cosponsors in the committee stage, but are no more or less likely to contact cosponsors in the floor stage. This result suggests that organized interests seek to convert weak supporters into strong supporters at the committee stage, but fails to provide evidence that this behavior continues at the floor stage. Second, respondents are no more or less likely to target majority or minority party members at the committee stage, but they are 12 percentage points more likely to target majority party Democrats at the floor stage—a pattern consistent with the importance of majority party support to advance the proposal through procedural motions on the floor (Cox and McCubbins 2005). Third, whereas membership on the committee of jurisdiction exerts the largest effect on targeting among all attribute-levels when the proposal is at the committee stage, respondents are no more or less likely to target committee members when the proposal moves to the floor stage. Though nearly all previous studies suggest that committee members are highly attractive lobbying targets by providing evidence (Austen-Smith and Wright 1994) or selecting on committee members as the population of interest (Hojnacki and Kimball 1998, 1999, 2001; Wright 1989, 1990), this result indicates that committee members are only attractive targets when they have exclusive influence over the proposal; once the proposal's fate rests in the hands of the full chamber, committee members become less valuable targets. Taken together, these conditional effects highlight the importance of considering how lobbying activities evolve during the legislative process, and how failing to do so obscures theoretically and substantively important empirical patterns (Leech 2010).

Robustness Checks and Additional Analyses

I conduct a series of analyses to demonstrate the robustness of my results and to investigate potential conditional effects underlying the results presented above. I discuss these analyses in detail in the Appendix, but summarize them here. First, to account for substantively small but statistically distinguishable differences in the characteristics of organized interest representatives in the sampling frame and the participants in the experiment, I re-estimate the models presented above with survey weights. The results from these weighted models, presented in Tables A.10 and

A.11, are substantively similar to those presented in Figures 2 and 3.[20] Second, to demonstrate generalizability across the two issue contexts used in the conjoint tasks, I condition attribute-level effects by issue context rather than legislative stage. The results from this model, presented in Table A.12, are again substantively similar to those in Figure 2.

Third, I consider whether the overall treatment effects are driven by heterogeneous treatment effects tied to respondents' personal characteristics. For example, because lobbyists with previous government experience have specialized knowledge about the policy process, and thus may have a different understanding of which types of congresspersons are attractive targets than other lobbyists (LaPira and Thomas 2017), they may respond to the conjoint tasks differently than lobbyists without government experience. Again, because lobbyists employed by firms that contract with organized interests might be better able to "imagine" working for a new client than lobbyists employed by their interests directly, these firm lobbyists may engage differently with the conjoint tasks than their in-house counterparts. Further, lobbyists might target members who share their ideological or partisan leanings out of a sense of homophily (Broockman and Ryan 2016) or because they believe that members will perceive them as more credible (Hirsch et al. n.d.). While the expectations drawn from previous empirical studies of lobbying targeting described above do not posit heterogeneous effects driven by respondent characteristics, identifying such effects, if present, provides a more nuanced understanding of how interest representatives make targeting choices. Additionally, though presence of heterogeneous effects would not pose a threat to the experiment's internal validity, it could limit the experiment's generalizability given the substantively small but statistically distinguishable differences between the characteristics of the respondents and the sampling frame (Coppock, Leeper, and Mullinix 2018; Coppock 2019). In the Appendix, I estimate additional models that condition attribute-level effects by five respondent characteristics that could plausibly induce heterogeneous effects—whether the respondent is employed by their client directly or by a firm, whether the respondent has previous government experience, and the respondent's partisan affiliation, lobbying experience, and generational alignment with the profiles presented to

[20] In a few alternative specifications, some attribute-levels which exhibit statistically distinguishable effects in the main analyses presented in Figures 2 and 3 do not present distinguishable effects. However, the directions and magnitudes of the effects associated with these attributes across alternative specifications are consistently similar to those presented above. See Appendix section "Empirical Results" for further details.

them. These models, presented in Tables A.13-A.17, are again substantively similar to those in Figure 2 and do not detect heterogeneous treatment effects which would circumscribe the experiment's generalizability.

Fourth, I explore whether the overall treatment effects obscure interactive effects among the attribute-levels featured in the member profiles. While the above analysis considers the effects of each attribute-level independent of the others, specific combinations of attribute-levels in the same profile may induce different effects than those exhibited independently. For instance, because the majority party exerts a high level of control over committee outcomes in the modern era (Aldrich, Perry, and Rodhe 2013), organized interests might target members who are both in the majority party and on the committee with jurisdiction over the proposal more frequently than members with only one of those characteristics. To probe such potential attribute-level-conditional effects, I estimate an additional model for each attribute which conditions the effect of all other attribute-levels on the unique levels of the selected attribute. Across each of these models, presented in Tables A.18-A.26, I do not detect any attribute-level-conditional effects, suggesting that interactions among the attribute-levels do not underlie the main results.

Conclusion

The foregoing results indicate that organized interests target members of Congress on the basis of their expressed preferences on the proposal of interest and the degree of influence their institutional roles afford them over the proposal given its stage in the legislative process. These results enable us to address several inconsistent findings in previous studies. In considering members' expressed preferences, the finding that organized interests trying to advance a live legislative proposal target members who are undecided on or weak supporters of a proposal suggests that interests target members whose preferences or levels of effort they perceive as malleable in order to strengthen their supportive coalition rather than members who already share and actively promote their preferences. Turning to members' institutional roles, the results indicate that organized interests target congressional leaders throughout the legislative process, but that they target members of the committee of jurisdiction and majority party members when those members exercise heightened influence—at

the committee and floor stages, respectively. This set of findings highlights not only that organized interests recognize the importance of legislative procedure and agenda setters, but also that studies of their targeting choices, and of their lobbying activity more broadly, must consider how procedural context conditions their behavior (Leech 2010). Finally, in contrast with the expectations embedded in the grassroots lobbying and money in politics literatures, the results do not provide evidence that organized interests target members on the basis of members' electoral concerns.

Some of these findings prompt normative concerns related to how organized interests interfere with members of Congress' representational function. While conceptualizations of what constitutes representation abound (e.g., Mansbridge 2003; Pitkin 1967), most suggest that representatives seek to promote constituents' welfare. However, if interests exert influence in Congress by changing the distribution of preferences by targeting malleable members rather than merely supporting their allies, then their lobbying might encourage members to adopt preferences at odds with their constituents' welfare. This danger is particularly acute when considering that interests tend to overrepresent businesses and the upper-class, whose preferences often conflict with optimal societal outcomes (Gilens and Page 2014; Schlozman, Verba, and Brady 2012). Additionally, because procedural mechanisms are often exercised behind closed doors, targeting members who control those mechanisms helps interests advance their goals without arousing the general public's suspicion.

Other findings encourage us to reconsider our theoretical expectations concerning organized interests and access. For example, though we must take care in interpreting null results, the lack of evidence that organized interests target members to whom they have previously made campaign contributions diverges from extant theories and empirical evidence on the influence of money in politics which assert that donors use contributions to "buy" access (Fouirnaies 2018; Fouirnaies and Hall 2018; Kalla and Broockman 2016; Powell and Grimmer 2016). This null finding should encourage us to think carefully about the mechanisms underlying organized interests' use of contributions to obtain access. One consideration for theorizing about the linkage between contributions and access is their temporal ordering. In the conjoint experiment, organized interests are assumed to use contributions to increase members' receptivity to future access requests, such that contributions precede access. However, interests could also use contributions to reward members for granting

access, such that contributions follow access. Though several empirical studies establish an explicit or implied relationship between contributions and access, we often lack information on the timing of these activities such that we cannot evaluate whether organized interests use contributions to "grease the skids" for future access or to reward members for granting access. Future studies can clarify our understanding of the mechanism underlying this relationship by leveraging the relative timing of contributions and access.

This study also highlights several avenues for future inquiry on organized interests and their lobbying activities in Congress. First, while this study focuses on organized interests' targeting choices when trying to advance a live legislative proposal—an objective emphasized in most previous studies—it does not consider whether the unique institutional contexts surrounding other types of legislative activity may condition how member characteristics inform targeting choices (Baumgartner and Leech 1998). For example, whereas this study focuses on bills that would change federal law—the most common type of proposals in Congress—organized interests also seek to influence other, less common types of proposals, such as appropriations and presidential nominations, which operate under specialized procedures that may prompt interests to adopt different targeting strategies (Austen-Smith and Wright 1994; Baumgartner and Leech 1996*a*,*b*). Differently, if organized interests seek to block, rather than advance, a proposal, they may focus their efforts on a key agenda setter who can stymie the proposal on her own rather than constructing a supportive coalition of sufficient size to defeat it (Rothenberg 1992). Alternatively, when organized interests' goals require members to commit substantial effort, such as introducing amendments or performing oversight of the bureaucracy, they may target supporters rather than undecideds because they can more efficiently subsidize members who already share their preferences (Hall and Deardorff 2006; Hall and Miler 2008). Future work should explore how interests' targeting choices vary across the different types of "asks" they make, and the experimental framework utilized here can be adapted to examine targeting strategies across other legislative activities.

Second, this study centers on organized interests' targeting choices, but does not account for the tactics interests employ as they seek to obtain and leverage direct contacts with members. While organized interests can draw on a range of tactical options, such as advertising policy ex-

pertise, offering electoral support, or promising information about the preferences of a member's constituents, to get what they want from a member (Hojnacki and Kimball 1999), they can maximize their odds of success by selecting the tactic that will exert the most influence over that member. For instance, policy expertise may be most valuable to members who lack knowledge of the policy, such as members who are newly-elected or not on the relevant committee(s). While several recent field experiments consider how the tactics interests use affect members' willingness to provide access (Brodbeck, Harrigan, and Smith 2013; Kalla and Broockman 2016; Wiener 2020), their random assignment of interests' tactics ignores interests' strategic incentives to match their tactics to members' needs. Additionally, interests who specialize in certain tactical options may focus their targeting strategies on members most susceptible to those tactics. For example, an interest that maintains a PAC may target electorally vulnerable members who may be influenced by campaign contributions more often than an interest without a PAC.[21] Future research that examines the role of organized interests' tactics in pursuing direct contacts can contribute to our understanding of how interests exert influence over policymakers.

Third, this study reminds scholars that studying the persons who work for organized interests—lobbyists and policy advocates—can help us improve our understanding of organized interests and enable us to probe under-explored research questions. For instance, since interests often function as principals who delegate political activity to their lobbyists and policy advocates as their agents, the lobbying activity we ultimately observe reflects how lobbyists and policy advocates resolve discrepancies between the preferences of themselves and their employers. However, with the exception of a few formal models and qualitative studies (Drutman 2015; Hirsch et al. n.d.; Kersh 2002), we have little empirical insight on how interests and lobbyists and policy advocates navigate

[21] Interests' tactical options are most likely to inform their targeting behavior when interests face resource constraints that preclude certain tactics (e.g., lacking a PAC). However, this potential conditionality should not have influenced respondents' choices in the experiment because the conjoint tasks provided several cues that the interests retaining respondents' services are well-resourced and capable of employing a wide range of tactics. For instance, the interests featured in both tasks are national member-based groups, thus enabling the interest to leverage grassroots lobbying. Additionally, because one of the member of Congress attributes indicates the amount of money the interest's PAC donated to each member, respondents know that the interest is among the ≈20% of interests who make campaign contributions (Ansolabehere, De Figueiredo, and Snyder 2003). Further, that the interest's executives retained the respondent as a lobbyist, are involved in the interest's lobbying activities, and want the respondent to conduct in-person lobbying signals that the interest is political engaged and sufficiently resourced to fund a Washington lobbying operation. Taken together, these cues make it unlikely that respondents intuited that the interests featured faced tactical constraints and consequently conditioned their responses on those constraints.

this principal-agent problem (but see Holyoke 2017; Schiff et al. 2015), and bringing lobbyists themselves more prominently into our research can help shed light on this dynamic. Again, while recent studies on the revolving door have brought attention to the importance of lobbyists' previous government experience (e.g., LaPira and Thomas 2017; McCrain 2018; Vidal, Draca, and Fons-Rosen 2012), we otherwise have scant knowledge of the characteristics of the persons who populate the lobbying community and how those characteristics might inform their advocacy behavior such as their partisanship and ideology (but see Hirsch et al. n.d.), professional experience, substantive expertise, gender, and race. By surveying lobbyists directly, the present study is able to measure some of these characteristics, and, though I do not find evidence that interest representatives' targeting choices are conditioned by personal characteristics (see Tables A.13-A.17), it is possible that other aspects of their lobbying behavior, such as which interests they choose to represent and how hard they advocate for their interests' priorities, are informed by their characteristics. Just as policymakers' personal characteristics inform how they discharge their official duties (Burden 2007; Butler and Broockman 2011; Carnes 2013; Harden 2015), interest representatives' characteristics may condition their lobbying behavior, and future studies designed to collect information on representatives themselves can enlighten our understanding of how personal characteristics influence their policy advocacy.

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Appendix

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Conjoint Experiment Protocol

After providing consent and completing pre-treatment questions, respondents are informed that they will be presented with two hypothetical scenarios situated in the 116th Congress, and are then asked to complete the two tasks, each of which focuses on a different policy issue but otherwise share the same structure (see "Issue Contexts" below for more details). Both tasks are constructed as stylized versions of real-world proposals introduced in the 115th Congress.[1] The order in which respondents were presented with each task is randomized to account for potential task-ordering effects.

In each task, respondents are told to imagine that they have been retained as a lobbyist for a hypothetical organization that wants to lobby Congress to pass a specific proposal. Respondents are then told that the organization wants the respondent to arrange meetings with House members as part of their lobbying efforts, and are asked to consider the profiles of two House members with which the organization's executives have suggested they meet. Finally, respondents are asked to express their level of interest in meeting with each member and to indicate the member with which they would prefer to meet. A screenshot of a sample task is presented in Figure 1.

Each profile includes nine fully or conditionally independently randomized attributes. The attributes and levels, as well as any restrictions on randomization, are presented in Tables A.1 and A.2. The order in which the attributes are displayed is randomized in each task to account for potential attribute-ordering effects. The wording and substance of some attributes and attribute-levels vary slightly across the two issue contexts; for example, the attributes corresponding to the strength of the relevant latent interest in the district—the number of retail employees and the number of Medicare-eligible residents in the internet sales tax and prescription drug tasks, respectively—are substantively different but conceptually similar. To enhance external validity, the numerical ranges of the ordinal attribute-levels were drawn from real-world data.[2] Finally, to allow for stage-conditional effects, the stage at which the proposal is situated—either the committee

[1]The tasks use stylized versions of real-world legislative proposals in order to balance concerns about external validity and pre-treatment effects. Specifically, the tasks must resemble real-world scenarios in order to apply inferences gleaned to lobbyists' strategic behavior while not priming respondents' outside knowledge and experiences which could influence their responses. If the tasks make use of specific legislative proposals from the real world, then lobbyists who worked on those proposals may respond to the experiment differently than lobbyists who did not work on those proposals. Differently, if the tasks feature purely hypothetical proposals, then the scenarios may not sufficiently mirror real-world legislative contexts, thus limiting the usefulness of the results in understanding lobbyists' behavior. For more on concerns about external validity and pre-treatment, see Gaines, Kuklinski, and Quirk (2007).

[2]The attribute-levels for the number of previous terms served reflect the minimum (freshman, 0 terms), first quartile (1 term), median (3 terms), and third quartile (7 terms) values for House members in the 115th Congress. The attribute-levels for margin of victory in the last election approximately reflect the first decile (less than 10%), the range between the first decile and the first quartile (10% to 20%), the range between the first quartile and the median (20% to 30%), and the range above the median (more than 30%) for House members in the 115th Congress (i.e. their two-party margins of victory in the 2016 election). The attribute-levels for the number of retail employees in the member's district approximately reflect the first quartile (fewer than 30,000), the range between the first quartile and the median (30,000 to 34,999), the range between the median and the third quartile (35,000 to 39,999), and the range above the third quartile (40,000 or more) according to the Census Bureau's 2016 County Business Patterns dataset. The attribute-levels for the number of Medicare-eligible residents in the member's district approximately reflect the first quartile (fewer than 110,000), the range between the first quartile and the median (110,000 to 129,999), the range between the median and the third quartile (130,000 to 149,999), and the range above the third quartile (150,000 or more) according to a 2018 report from the Centers for Medicare & Medicaid Services. The attribute-levels for PAC contributions approximately reflect the first decile ($100), third decile ($500), and median ($1000) values for contributions by non-individuals to candidates for federal office in the 2013-2014 election cycle according to the Federal Election Commission data curated by Adam Bonica and presented in the DIME database.

stage or the floor stage—is independently randomized across choice tasks and specified within the vignette.

Issue Contexts

The conjoint tasks presented to respondents focused on two different stylized scenarios based on real-world proposals concerning an internet sales task and prescription drug pricing introduced in the 115th Congress. The internet sales tax task is based on H.R. 2193, the Remote Transactions Parity Act of 2017, and the prescription drugs task is based on H.R. 242, the Medicare Prescription Drug Price Negotiation Act of 2017. In identifying bills and issue areas to utilize for the experiment, I strove to find bills whose partisan implications are not clear-cut, especially when provided to respondents in generalized summaries. Identifying and using such bills was important in order to isolate the effects of members' positions on each proposal and members' partisan affiliations. If the bills selected were partisan, then respondents may have inferred the positions of undecided members using their partisan affiliations. Additionally, partisan bills would have created unlikely scenarios in which members would take positions on proposals that do not match with their partisan affiliations (e.g., a Democrat supporting a bill to restrict abortion access), confusing respondents and threatening the experiment's external validity. As the legislative histories and broader issue milieus for both bills suggest, members of Congress of either party could plausibly support, oppose, or be undecided on the proposal, and therefore respondents should have been unlikely to confront implausible position/party combinations or use members' party affiliations to ascertain their preferences with certainty.

- The internet sales tax task is based on the 115th Congress' H.R. 2913, the Remote Transactions Parity Act of 2017. This bill was one of several introduced during the 115th Congress which sought to establish a national framework by which states could collect sales tax from online retailers.[3] The bill's legislative history and advocacy activity surrounding it demonstrates that members of Congress' party affiliations did not provide clear cues on their positions on the bill. H.R. 2193 was introduced by a Republican, Kristi Noem of South Dakota, and the bill's 50 cosponsors were roughly evenly distributed along partisan lines (24 Democrats and 26 Republicans). Rep. Noem framed the bill not as a matter of increasing taxes—a policy which Republicans would typically oppose—but rather as a way to help "level the playing field" between online retailers and local businesses.[4] Further, stakeholders of all political

[3]The Supreme Court's ruling in *Quill Corp. v. North Dakota* (1992) had invoked the Dormant Commerce Clause to prohibit states from collecting sales tax on e-commerce purchases unless the seller maintained a "substantial nexus" in the state. Given this ruling, states could only collect sales tax on all e-commerce purchases by their residents if Congress acted to sanction such regulation of interstate commerce. Accordingly, state governments and businesses placed at a disadvantage by having to collect sales tax for the same goods sold by e-commerce companies (e.g., physical retailers) had long lobbied Congress to enable states to collect sales tax from these e-commerce transactions. The Supreme Court's ruling in *South Dakota v. Wayfair, Inc.* (2018)—issued during the 115th Congress—overturned *Quill Corp. v. North Dakota* and allowed states to begin collecting taxes on these sales, but this decision provided no streamlined national framework under which e-commerce companies would remit sales tax to individual states, leading to confusion among both state governments and e-commerce companies. Thus, both before and after *South Dakota v. Wayfair, Inc.*, state governments and components of the business community sought congressional action to bring clarity and organization to the remittance of state sales tax.

[4]Stambor, Zak. "An Online Sales Tax Battle is Brewing on Capitol Hill." *Digital Commerce 360*, March 16, 2018, `https://www.digitalcommerce360.com/2018/03/16/an-online-sales-tax-battle-is-brewing-on-capitol-hill/`.

persuasions encouraged congressional action on this issue around the time that the experiment was fielded. For instance, the non-partisan National Conference of State Legislatures passed a resolution encouraging Congress to legislate "a framework of standards for states to follow" in collecting sales tax from online transactions, and that online retailers themselves also called for congressional action to replace the various remittance policies put forth by individual states[5]. Finally, H.R. 2193 received support from a bipartisan coalition of business and state and local government associations such as the the National Association of Counties National Association of Realtors, the National Retail Federation, and the US Conference of Mayors.[6] Thus, the Remote Transactions Parity Act, and the broader issue of congressional regulation of internet sales tax remittances used to frame the experiment, is unlikely to have led respondents to infer members of Congress' positions from their party affiliations.

- The prescription drugs task is based on the 115th Congress' H.R. 242, the Medicare Prescription Drug Price Negotiation Act of 2017. This bill, which has been introduced in every Congress since the 108th in 2003, aims to lower prescription drug prices by empowering the Centers for Medicare and Medicaid Services to negotiate drug prices with pharmaceutical companies. Relative to the Remote Transactions Parity Act, H.R. 242 and previous iterations of the same bill have enjoyed more Democratic than Republican support, but the bill has received support from congressional Republicans at several points in the past two decades. For instance, Republican Francis Rooney (FL) joined 44 Democrats in cosponsoring H.R. 242 and cosigned a Dear Colleague letter with the lead sponsor, Rep. Peter Welch (I-VT), encouraging support among other House members.[7]. Other Republicans have also cosigned the bill in previous Congresses,[8] and, when the bill faced a floor vote in the House in 2007, 24 Republicans joined 231 Democrats to pass it. Further, in the more general political milieu, the Republican Party's standard-bearer, President Donald Trump, advocated for authorizing the Centers for Medicare and Medicaid Services to negotiate prescription drug prices during his 2016 campaign[9] and announced a plan to do so in October 2018, shortly before the experiment was fielded.[10] Thus, while enabling the Centers for Medicare and Medicaid services to negotiate prices with pharmaceutical companies is a proposal more commonly with the Democratic Party, it has also enjoyed some Republican support—including from a Republican president shortly before the experiment was conducted—such that members of Congress'

[5]Prete, Ryan. "New Digital Tax Bill in Wings at Congress: State Official." *Bloomberg Law*, July 31, 2018, `https://news.bloombergtax.com/daily-tax-report-state/new-digital-tax-bill-in-wings-at-congress-state-official`.

[6]According to Crosson, Furnas, and Lorenz (2020)'s IGscores, which are measures of ideology for organized interests derived from the positions they take on congressional bills, the mean and standard deviation of these organizations on a unidimensional scale are 0.12 and 0.22, respectively, suggesting a fairly moderate coalition.

[7]Welch, Peter and Francis Rooney. "Fight the Rising Cost of Prescription Drugs: Cosponsor the Medicaire Prescription Drug Price Negotiation Act." Dear Colleague Letter, `http://dearcolleague.us/2019/01/fight-the-rising-cost-of-prescription-drugs-cosponsor-the-medicare-prescription-drug-price-negotiation-act/`.

[8]Two Republicans cosponsored H.R. 2248 in the 112th Congress; one Republican cosponsored H.R. 4752 in the 111th Congress; one Republican cosponsored H.R. 4 in the 110th Congress; and one Republican cosponsored H.R. 3299 in the 108th Congress.

[9]Politico Pro Staff. "Trump Backs Medicare Negotiating Drug Prices," *Politico*, January 25, 2016, `https://www.politico.com/story/2016/01/trump-backs-medicare-negotiating-drug-prices-218215`.

[10]Lovelace Jr., Berkeley. "Trump Outlines New Plan to Lower Medicare Drug Prices, End 'Rigged' System." *CNBC*, October 25, 2018, `https://www.cnbc.com/2018/10/25/trump-says-medicare-to-negotiate-lower-drug-costs-to-end-rigged-system.html`.

party affiliation should not have been a wholly informative cue of members' preferences on the underlying policy.

Pre-Treatment Questions

- What is your gender?
  - Male
  - Female
- How old are you?
  - 18-29
  - 30-49
  - 50-64
  - 65 and over
- How much school or college have you completed?
  - Some high school or less
  - High school graduate or GED
  - Some college, no 4-year degree
  - College graduate
  - Post-graduate degree
- Which best describes your race?
  - White
  - Black
  - Asian
  - Hispanic
  - Other
- Which best describes your household income?
  - Less than $25,000
  - $25,000-$50,000
  - $50,000-$75,000
  - $75,000-$100,000
  - $100,000-$200,000
  - $200,000 or more
- When it comes to politics, would you describe yourself as liberal, conservative, or neither liberal nor conservative?
  - Very conservative
  - Somewhat conservative
  - Slightly conservative
  - Moderate
  - Slightly liberal
  - Somewhat liberal
  - Very liberal
- Generally speaking, do you think of yourself as a Democrat, Republican, Independent, or what?
  - Democrat (subsequent question to distinguish between "strong" and "not so strong")
  - Republican (subsequent question to distinguish between "strong" and "not so strong")

  - Independent (subsequent question to assess whether "closer to Democratic Party," "closer to Republican Party," or "neither")
  - Other
- How many years have you worked in lobbying, government relations, policy advocacy, or a related field? Please do not include any time during which you worked for the federal government.
  - Less than 5 years
  - 5 to 10 years
  - 10 to 15 years
  - 15 to 20 years
  - More than 20 years
- Have you ever worked or served in the federal government in any of the following capacities? Select all that apply.
  - Member of Congress
  - Staffer of a member of Congress or congressional committee
  - Presidential appointee in a federal agency
  - Employee of the Executive Office of the President
  - Civil servant in a federal agency (outside the Executive Office of the President)
  - Other
  - None of the above
- Which of the following best describes your role in working for (your client)?
  - Lobbyist or government relations/policy advocacy professional
  - Executive officer at (your client) with ultimate responsibility for lobbying/government relations/policy advocacy
  - Executive officer at (your client) without ultimate responsibility for lobbying/government relations/policy advocacy
  - Other

Post-Treatment Questions

- How interested would you be in meeting with each of these members to lobby in support of this bill? (*Asked separately for each member, see Figure 1*)
  - Not at all interested
  - Slightly interested
  - Somewhat interested
  - Very interested
  - Extremely interested
- If you could only arrange a meeting with one of these House members, with which member would you prefer to meet?
  - Member 1
  - Member 2

Conjoint Experiment Tasks

Common Instructional Text

The following **two hypothetical scenarios** will ask you to assume the role of a lobbyist retained by a specified organization, and to provide the organization with advice concerning lobbying strategies.

For the purposes of these scenarios, assume that you are working in the context of the 116th Congress, which will convene on January 3, 2019. **The Democrats will control the House of Representatives, and the Republicans will control the Senate and the White House.**

Internet Sales Tax Vignette

Imagine that you have been retained as a lobbyist by the American Coalition of Retailers (ACR), a national trade association of brick-and-mortar retailers.

ACR executives have asked you to manage their strategy for lobbying Congress to pass a House bill that would require online retailers to collect and remit sales tax payments for all in-state and out-of-state transactions. This bill [**COMMITTEE STAGE:** *was referred to the Judiciary Committee, and the committee will soon hold hearings on the bill*/**FLOOR VOTE STAGE:** *was recently voted out of the Judiciary Committee and will soon be considered on the floor*].

As part of your lobbying efforts, ACR suggested that you try to arrange meetings with House members. Presented below are the profiles of 2 House members that ACR executives suggested that you consider contacting to set up meetings.

[**MEMBER PROFILES PRESENTED HERE; SEE FIGURE 1 FOR VISUAL EXAMPLE, SEE TABLE A.1 FOR ATTRIBUTE-LEVELS)**]

Prescription Drugs Vignette

Imagine that you have been retained as a lobbyist by Senior Citizens for America (SCA), a national membership organization of senior citizens.

SCA executives have asked you to manage their strategy for lobbying Congress to pass a House bill that would lower prescription drug prices by allowing the Centers for Medicare & Medicaid Services to negotiate drug prices with pharmaceutical companies. This bill [**COMMITTEE STAGE:** *was referred to the Ways and Means Committee, and the committee will soon hold hearings on the bill*/**FLOOR VOTE STAGE:** *was recently voted out of the Ways and Means Committee and will soon be considered on the floor*].

As part of your lobbying efforts, SCA suggested that you try to arrange meetings with House members. Presented below are the profiles of 2 House members that SCA executives suggested that you consider contacting to set up meetings.

[**MEMBER PROFILES PRESENTED HERE; SEE FIGURE 1 FOR VISUAL EXAMPLE, SEE TABLE A.2 FOR ATTRIBUTE-LEVELS)**]

Table A.1: Conjoint Experiment Member Attributes and Levels (Internet Sales Tax)

Attribute	Levels	Restrictions?
Stated Position on Internet Sales Tax Bill	Oppose (baseline)	None
	Undeclared	None
	Support	None
Cosponsor of Internet Sales Tax Bill	No (baseline)	None
	Yes	Stated Position must be "Support"
Party	Republican (baseline)	None
	Democrat	None
Judiciary Committee Member	No (baseline)	None
	Yes	None
Judiciary Committee Leadership Position	No (baseline)	None
	Subcommittee chair	Party must be "Democrat" & Judiciary Committee Member must be "Yes"
	Subcommittee ranking member	Party must be "Republican" & Judiciary Committee Member must be "Yes"
Number of Previous Terms Served	Freshman (baseline)	None
	1 term	None
	3 terms	None
	7 terms	None
Margin of Victory in Last Election	Less than 10% (baseline)	None
	10% to 20%	None
	20% to 30%	None
	More than 30%	None
Number of Retail Employees in Member's District	Fewer than 30,000 (baseline)	None
	30,000 to 34,999	None
	35,000 to 39,999	None
	40,000 or more	None
Did ACR's PAC Donate to the Member in the Last Cycle?	No (baseline)	None
	Yes; $100	None
	Yes; $500	None
	Yes; $1000	None

Table presents the attributes, attribute-levels, and attribute-level restrictions for each of the nine House member characteristics used in the internet sales tax conjoint experiment task. In the task, respondents are presented with two House member profiles which consist of randomly assigned levels for each of the nine attributes; unless otherwise noted in the table, attribute-level assignments are completely randomized (i.e. no restrictions conditional on assignment of other attribute-levels). The ordering of the attributes is also randomized across respondents and tasks, though two pairs of conceptually related attributes are always presented together to ease cognitive demands—the members' stated positions on the bill are always followed by an indication of whether they are a cosponsor, and the members' membership on the Judiciary Committee is always followed by an indication of whether they hold a leadership position on the committee.

Table A.2: Conjoint Experiment Attributes and Levels (Prescription Drugs)

Attribute	Levels	Restrictions?
Stated Position on Prescription Drug Price Bill	Oppose (baseline)	None
	Undeclared	None
	Support	None
Cosponsor of Prescription Drug Price Bill	No (baseline)	None
	Yes	Stated Position must be "Support"
Party	Republican (baseline)	None
	Democrat	None
Ways and Means Committee Member	No (baseline)	None
	Yes	None
Ways and Means Committee Leadership Position	No (baseline)	None
	Subcommittee chair	Party must be "Democrat" & Ways and Means Committee Member must be "Yes"
	Subcommittee ranking member	Party must be "Republican" & Ways and Means Committee Member must be "Yes"
Number of Previous Terms Served	Freshman (baseline)	None
	1 term	None
	3 terms	None
	7 terms	None
Margin of Victory in Last Election	Less than 10% (baseline)	None
	10% to 20%	None
	20% to 30%	None
	More than 30%	None
Number of Medicare-Eligible Residents in District	Fewer than 110,000 (baseline)	None
	110,000 to 129,999	None
	130,000 to 149,999	None
	150,000 or more	None
Did SCA's PAC Donate to the Member in the Last Cycle?	No (baseline)	None
	Yes; $100	None
	Yes; $500	None
	Yes; $1000	None

Table presents the attributes, attribute-levels, and attribute-level restrictions for each of the nine House member characteristics used in the internet sales tax conjoint experiment task. In the task, respondents are presented with two House member profiles which consist of randomly assigned levels for each of the nine attributes; unless otherwise noted in the table, attribute-level assignments are completely randomized (i.e. no restrictions conditional on assignment of other attribute-levels). The ordering of the attributes is also randomized across respondents and tasks, though two pairs of conceptually related attributes are always presented together to ease cognitive demands—the members' stated positions on the bill are always followed by an indication of whether they are a cosponsor, and the members' membership on the Ways and Means Committee is always followed by an indication of whether they hold a leadership position on the committee.

Sampling Procedure and Descriptive Statistics

Under the Lobbying Disclosure Act of 1995 (LDA) and subsequent amendments, individuals who meet the thresholds for designation as a lobbyist[11] must complete and submit a quarterly report, known as an LD-2 form, for each of their clients detailing their lobbying activities on behalf of the client (see A.1 for an example). Each quarterly report identifies a point of contact for the lobbyist or for the organization employing the lobbyist, or the registrant, and each point of contact is required to provide an email address at which they can be reached. While this point of contact can be an individual who is not formally recognized as a lobbyist under the LDA, nearly two-thirds of points of contact are LDA lobbyists, and those individuals who are not LDA lobbyists often perform government relations or policy advocacy functions and are familiar with lobbying activity (see Table A.4). Therefore, the sampling frame of LDA points of contact is appropriate to learn about the behavior of lobbyists and policy advocates more generally.

The sampling frame for this survey is the full universe of individuals identified as points of contact on quarterly LDA reports from the first quarter of 2017 through the third quarter of 2018. For each individual, his or her most recent appearance on a report was selected so as to obtain the most up-to-date contact and employment information; in cases where the same individual was identified as the point of contact for more than one client in a given quarter, one report on which that individual appeared as the point of contact was randomly sampled. To minimize email bounces and improve response rates, the email addresses provided in the selected reports were screened to check for appropriate formatting and to identify duplicates. Some organizations, such as large lobbying firms, provided the same generic email address for all of their filings, such that many individuals were tied to the same generic email address; when such generic email addresses were identified, every effort was made to obtain a unique email address for that individual (searching the organization website, LinkedIn, other social media platforms, etc.). After de-duplicating the list of individuals and screening email addresses, the final sample consisted of 5,938 individuals.

Initial invitations to complete the survey were sent to all 5,938 individuals on November 15, 2018, and reminder emails were sent to all individuals who had not yet completed the survey on November 29, 2018 and December 13, 2018. Data collection ceased on December 31, 2018. Excluding the points of contact whose email addresses were identified as invalid when invitations were sent, the overall response rate for the conjoint experiment is 12.3% ($\frac{670}{5458}$).

This response rate compares favorably to those obtained in surveys of the American public and in academic survey experiments of political elites. In the former case, major polling firms such as the Pew Research Center and Gallup report that the typical response rates in their mass public surveys in recent years are 6% and 7%, respectively.[12] In the latter case, because no benchmark for

[11] Under the LDA, a lobbyist is an individual who, in working on behalf of a client, makes a "lobbying contact," or an "oral, written, or electronic communication" regarding the conduct of public policy, with more than one "covered official," which includes most members of the executive and legislative branches—include the president, vice-president, and members of Congress—and spends 20 percent or more of her time working for the client on lobbying activities within a quarterly period. As of January 2017, a lobbyist employed directly by a client that spends less than $13,000, or a lobbyist contracted by a client that spends less than $3,000, on lobbying activities in a given quarter, is not required to file a report for that quarter.

[12] Kennedy, Courtney and Hartig, Hannah. 2019. "Response Rates in Telephone Surveys Have Resumed Their Decline." *Pew Research Center*, February 27. `https://www.pewresearch.org/fact-tank/2019/02/27/response-rates-in-telephone-surveys-have-resumed-their-decline/`; Marken, Stephanie. 2018. "Still Listening: The State of Telephone Surveys." *Gallup*, January 11. `https://news.gallup.com/opinion/methodology/225143/listening-state-telephone-surveys.aspx`.

response rates for survey experiments fielded with samples of federal lobbyists and policy advocates exists, I compare my response rate to that obtained in other studies conducting survey experiments with American political elites[13] published in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics* between January 1, 2010 and April 23, 2020.[14] Information about the survey experiments contained in these studies is presented in Table A.3. Across the 13 unique experiments presented in the 10 published studies meeting these criteria for which response rates were calculable, the mean and median response rates are 13.4% and 11.8%, respectively.

It is difficult to assess the representativeness of my survey respondents to the points of contact in the sampling frame because scant information is available regarding the points of contact and the clients for which they work; unlike more publicly visible political actors in Washington, DC, such as members of Congress, whose personal information is collated in the Biographical Directory of the United States Congress and can be systematically coded for inclusion in research, no central repository for similar personal information, such as partisanship and career history, exist for lobbyists and policy advocates. However, four pieces of information about the points of contact and their clients can be gleaned from their LDA filings and the Center for Responsive Politics (CRP), which cleans and aggregates the LDA filings: the client's quarterly lobbying expenditures with that point of contact's employer (i.e. the client's own expenditures if the point of contact is employed directly, or the client's expenditures with a given firm if the point of contact is a contract employee); whether the filer is the client or a lobbying firm contracted by a client; the client's sector coding, as assigned by CRP; and whether the point of contact is a registered lobbyist under the LDA.[15] Table A.4 compares the distribution of these four characteristics in both the full sampling frame and the sample of respondents who took part in the experiment. These comparisons reveal some differences for each of the four characteristics that are substantively small but are statistically distinguishable at the $p < 0.05$ level using difference in means and χ^2 tests (where applicable). To demonstrate robustness and generalizability, I re-estimate all models presented in the main text using survey weights which incorporate these four characteristics. Each of these re-estimated models yield results substantively similar to those presented here (see Tables A.10 and A.11). Additionally, points of contact who are also registered lobbyists were more likely to complete the survey than were points of contact who were not registered lobbyists, providing further confidence that the survey was completed by the very individuals of interests—lobbyists and policy advocates.[16]

Finally, Table A.5 provides information on the descriptive characteristics of the individuals

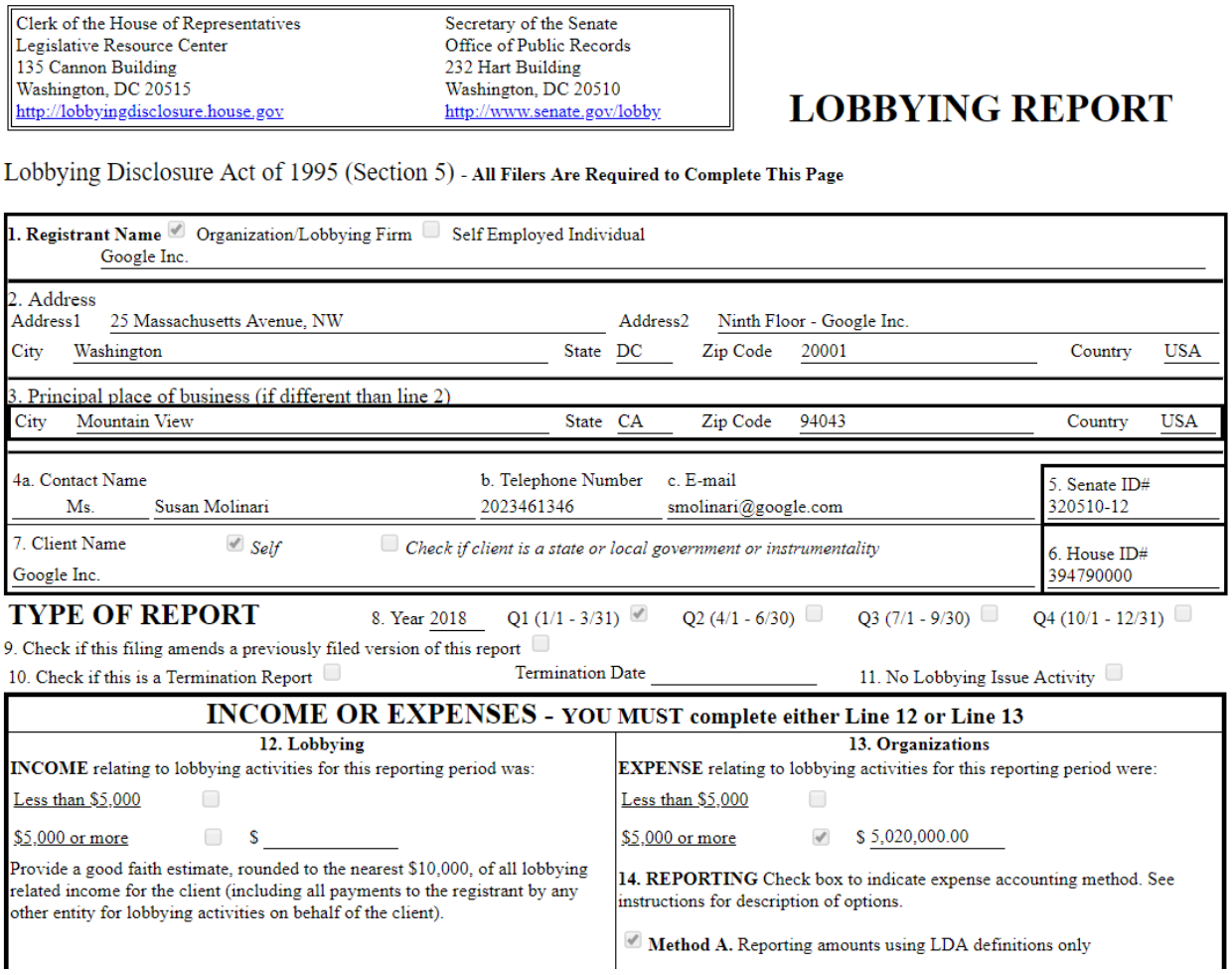
[13] I define "political elites" as members of specialized, identifiable populations who actively engage in high levels political activity by nature of their membership in those populations. While this definition includes government officials, it also includes campaign donors, members of organized interest groups, and party officials.

[14] To identify publications for inclusion, I searched for the terms "survey" and "experiment" in all articles published in the specified time frame, and then reviewed each of the search results to determine if the article included a survey experiment whose sampling strategy specifically targeted a population of American political elites. I exclude studies which include only field experiments involving deception (i.e., "audit studies"), as the communications sent to political elites in these studies do not reveal their connection to academic research upfront.

[15] The first three of these pieces of information are easily observable from CRP's aggregated LDA filings, but the fourth can only be determined by comparing the names of the points of contact provided on each LDA filing with the names of the registered lobbyists listed on the same LDA filing. To determine whether the point of contact listed is an registered lobbyist, I used approximate matching techniques to compare the name of the point of contact on each LDA filing to the names of all of the registered lobbyists also appearing on the filing, and visually inspected the best match for each LDA form to determine if the point of contact was also listed as a registered lobbyist.

[16] This difference may be attributable to non-registered lobbyists who do not work in policy advocacy recognizing that they were not the intended audience for the survey and thus declining to respond.

who completed conjoint experiment tasks. This descriptive information was collected as part of the survey, and thus only provides information about respondents. The high proportions of respondents who report education levels of "post-graduate degree" (68.1%), income levels of "$200,000 or more" (58.1%), experience levels of "more than 20 years" (41.1%), and professional roles as "lobbyists" or "executive officers responsible for lobbying" (88.6%) suggest that most survey respondents were themselves members of the population of interest—political elites who play a substantive role in lobbying and policy advocacy—rather than low-level employees who may respond to emails but lack significant lobbying experience.



Clerk of the House of Representatives
Legislative Resource Center
135 Cannon Building
Washington, DC 20515
http://lobbyingdisclosure.house.gov

Secretary of the Senate
Office of Public Records
232 Hart Building
Washington, DC 20510
http://www.senate.gov/lobby

LOBBYING REPORT

Lobbying Disclosure Act of 1995 (Section 5) - **All Filers Are Required to Complete This Page**

1. Registrant Name ☑ Organization/Lobbying Firm ☐ Self Employed Individual
Google Inc.

2. Address
Address1 25 Massachusetts Avenue, NW Address2 Ninth Floor - Google Inc.
City Washington State DC Zip Code 20001 Country USA

3. Principal place of business (if different than line 2)
City Mountain View State CA Zip Code 94043 Country USA

4a. Contact Name: Ms. Susan Molinari b. Telephone Number: 2023461346 c. E-mail: smolinari@google.com
5. Senate ID# 320510-12

7. Client Name ☑ *Self* ☐ *Check if client is a state or local government or instrumentality*
Google Inc.
6. House ID# 394790000

TYPE OF REPORT 8. Year 2018 Q1 (1/1 - 3/31) ☑ Q2 (4/1 - 6/30) ☐ Q3 (7/1 - 9/30) ☐ Q4 (10/1 - 12/31) ☐
9. Check if this filing amends a previously filed version of this report ☐
10. Check if this is a Termination Report ☐ Termination Date ____ 11. No Lobbying Issue Activity ☐

INCOME OR EXPENSES - YOU MUST complete either Line 12 or Line 13

12. Lobbying
INCOME relating to lobbying activities for this reporting period was:
Less than $5,000 ☐
$5,000 or more ☐ $ ____
Provide a good faith estimate, rounded to the nearest $10,000, of all lobbying related income for the client (including all payments to the registrant by any other entity for lobbying activities on behalf of the client).

13. Organizations
EXPENSE relating to lobbying activities for this reporting period were:
Less than $5,000 ☐
$5,000 or more ☑ $ 5,020,000.00

14. REPORTING Check box to indicate expense accounting method. See instructions for description of options.
☑ **Method A.** Reporting amounts using LDA definitions only

Figure A.1: **Example of a Lobbying Disclosures Act LD-2 Form.** This figure presents part of an LD-2 form filed by Google to report its lobbying activity for the first quarter of 2018. The name and contact information for Google's point of contact, Susan Molinari (then-Vice President of Public Policy and Government Affairs and a former member of Congress from New York), is presented on line 4. The full report can be accessed at `https://soprweb.senate.gov/index.cfm?event=getFilingDetails&filingID=F0996706-C49E-44ED-BD85-1DC4D3CE2E7B&filingTypeID=51`.

Table A.3: Response Rates in Survey Experiments of American Political Elites

Citation	Year Fielded	Elite Population	Mode of Recruitment	# Respondents	# Potential Respondents	Response Rate
Butler and Powell (2014)	2012	State legislators	Email	>1,000	≈7,000	≈15%*
Butler and Dynes (2016)	2012	State legislators	Email	>1,000	≈7,000	≈15%*
	2012	Municipal Officials	Email	N/A	N/A	≈ 23%**
Butler et al. (2017)	2012	Municipal Officials	Email	N/A	N/A	≈ 23%**
Flavin and Hartney (2017)	2015	California School Board Members	Email	325	1,231	26.4%
Li (2018)	2017	Campaign Donors (Compustat)	Mail	413	≈3,500	11.8%
	2017	Campaign Donors (Other)	Mail	1,347	16,391	8.2%
Teele, Kalla, and Rosenbluth (2018)	2014	Elected Officials (All Levels)	Email	NA	21,754	8.6%
	2017	Elected Officials (All Levels)	Email	NA	12,341	8.7%
Broockman, Ferenstein, and Malhotra (2019)	2013	Tech Entrepreneurs	Crunchbase	691	4,245	16.3%
	N/A	Partisan Donors	Mail	1,152	16,400	7.0%

Citation	Year Fielded	Elite Population	Mode of Recruitment	# Respondents	# Potential Respondents	Response Rate
Doherty, Dowling, and Miller (2019)	2016	Local Party Chairs	Email	853	5,148	16.6%
Hertel-Fernandez, Mildenberger, and Stokes (2019)	2016	Congressional Staff	Email	101	N/A	9.6%
Malhotra, Monin, and Tomz (2019)	2013	Audubon Society Members	Email	2,374	N/A	N/A
	2013	Care2 Petition Signers	Email	1,722	10,710	16.1%
	2015-2016	Government Officials (All Levels)	Email	1,533	20,597	7.4%

This table presents information about the sampling procedures and response rates for all studies conducting survey experiments with American political elites published in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics* between January 1, 2010 and April 23, 2020 (see section introductory text for more detail). Where only approximate numbers were provided by the authors in the original article and supplemental information, I denote these numbers with ≈. If certain pieces of information were not provided by the authors in the original article or supplemental information, I recorded this missing information with "N/A." Experiments whose response rates are accented with * or ** were used in multiple published studies and are only used once to compute the average response rate. Across the 13 unique experiments presented in the 10 published studies presented here for which response rates were calculable, the mean and median response rates are 13.4% and 11.8%, respectively. For more information about each of the experiments included in this table, please refer to the original articles.

Table A.4: Comparison of Respondents with Sampling Frame

Characteristic	% of Respondents (N)	% of Sampling Frame (N)
Lobbyist Employer		
Client	59.7% (400)	53.4% (2913)
Firm	40.3% (270)	46.6% (2545)
Lobbying Expenditures		
First Quartile	26.4% (177)	25.0% (1365)
Second Quartile	30.6% (205)	25.0% (1364)
Third Quartile	24.2% (162)	25.0% (1365)
Fourth Quartile	18.8% (126)	25.0% (1364)
CRP Category		
Agribusiness	4.3% (29)	4.2% (228)
Communications and Electronics	4.3% (29)	6.6% (360)
Construction	1.6% (11)	2.1% (112)
Defense	2.1% (14)	2.1% (117)
Energy and Natural Resources	6.9% (46)	8.1% (444)
Finance, Insurance and Real Estate	7.5% (50)	9.6% (526)
Health	19.1% (128)	18.5% (1011)
Ideological and Single-Issue	13.4% (90)	9.2% (500)
Labor	1.8% (12)	2.1% (116)
Lawyers and Lobbyists	1.6% (11)	1.2% (66)
Misc Business	10.9% (73)	11.8% (645)
Other	5.7% (38)	6.2% (339)
Transportation	6.3% (42)	6.1% (331)
Unknown	14.5% (97)	12.1% (663)
Registered Lobbyist		
Yes	75.1% (503)	62.5% (3409)
No	24.9% (167)	37.5% (2049)

Table A.5: Descriptive Statistics of Experimental Sample

Characteristic	% of Respondents (N)
Gender	
Female	28.2% (189)
Male	71.2% (477)
NA	0.6% (4)
Age	
18-29	4.0% (27)
30-49	38.4% (257)
50-64	35.8% (240)
65 or over	21.5% (144)
NA	0.3% (2)
Education	
Some college, no 4-year degree	1.8% (12)
College graduate	29.9% (200)
Post-graduate degree	68.1% (456)
NA	0.3% (2)
Race/Ethnicity	
Asian	1.3% (9)
Black	3.1% (21)
Hispanic	1.8% (12)
White	90.3% (605)
Other	2.1% (14)
NA	1.3% (9)
Income	
Less than $25,000	0.3% (2)
$25,000-$50,000	0.7% (5)
$50,000-$75,000	3.4% (23)
$75,000-$100,000	6.4% (43)
$100,000-$200,000	26.3% (176)
$200,000 or more	58.1% (389)
NA	4.8% (32)
Ideology	
Very liberal	13.6% (91)
Somewhat liberal	21.9% (147)
Slightly liberal	13.9% (93)
Neither liberal nor conservative	16.4% (110)
Slightly conservative	14.3% (96)
Somewhat conservative	14.3% (96)
Very conservative	4.6% (31)
NA	0.9% (6)
Party Identification	
Strong Democrat	36.9% (247)
Democrat	8.2% (55)

Characteristic	% of Respondents (N)
Lean Democrat	7.8% (52)
Independent	10.4% (70)
Lean Republican	6.7% (45)
Republican	10.9% (73)
Strong Republican	14.9% (100)
Other	2.2% (15)
NA	1.9% (13)
Lobbying Experience	
Less than 5 years	10.1% (68)
5-10 years	17.3% (116)
11-15 years	17.2% (115)
16-20 years	13.9% (93)
More than 20 years	41.0% (275)
NA	0.4% (3)
Past Government Experience	
Member of Congress	4.6% (31)
Congressional staffer	43.6% (292)
Presidential appointee	8.1% (54)
EOP staffer	4.5% (30)
Civil servant	13.3% (89)
Other	14.3% (96)
No experience	33.9% (227)
Current Role with Client	
Lobbyist	58.5% (392)
Executive officer responsible for lobbying	30.1% (202)
Executive officer not responsible for lobbying	3.9% (26)
Other	6.9% (46)
NA	0.6% (4)

Empirical Results

In this section, I present detailed results of the models presented in the main paper which use respondents' binary targeting choices as the outcome measure, as well as additional model specifications to demonstrate the robustness of the results. Each table in this section presents the point estimates, standard errors, and 95% confidence intervals (with appropriate Bonferroni corrections) for the corresponding attribute-level in the model indicated, as generated using the `cjoint` package in `R`. The point estimates (AMCEs or ACIEs) reflect the effect of the specified attribute-level on the probability of selection when a profile containing that attribute-level is paired with an otherwise identical profile containing the baseline level of the same attribute. For example, in Table A.6, whose results are graphically presented in Figure 2, the point estimates for the Undeclared and Support levels of the Position attribute indicate that a respondent is 19 percentage points more likely, or 3 percentage points less likely, respectively, to select a congressperson profile with those levels of the Position attribute when that profile is paired with an otherwise identical profile assigned the Oppose level for the Position attribute. The results presented in Tables A.6 and A.7 correspond with Figures 2 and 3, respectively, in the main text.

Alternative Specifications

Ordinal Rating Outcome

In addition to asking respondents to indicate which of the two members in each choice task they would prefer to meet, I also asked respondents to express their level of interest in meeting with each of the two members on a five-point scale. I present analyses analogous to those in the main text that use these ordinal measures as the outcome measure in Tables A.8 and A.9. When comparing the analyses using the binary and ordinal outcomes, the directionality of the AMCEs and ACIEs are generally consistent—particularly for the attribute-levels that exhibited statistically distinguishable effects in the main text—, but the statistical distinguishability of some attribute-levels differ across the two outcome measures. For instance, in Table A.8, which uses the ordinal measure and pools across stages, the AMCEs for only two attribute-levels—members having an "undeclared" position and sitting on the committee of jurisdiction—are statistically distinguishable. In contrast, in Table A.6, three additional attribute-levels—whether the member is a cosponsor, a Democrat, or a subcommittee chair—are statistically distinguishable.

While both outcomes measure the effect of member characteristics on members' targeting behavior, I argue that the binary measure better mirrors the real-world strategic targeting behavior we seek to understand. As discussed in the introduction of the main text, organized interest representatives regularly report that direct contacts with members of Congress rank among the most important lobbying behaviors in which they engage. Therefore, respondents' baseline interest in meeting with any member of Congress, irrespective of the member's characteristics, should be high. However, though organized interests may want to meet with any number of members of Congress, their time and resource constraints force them to target only a subset of the members with which they might want to meet. Given this reality that organized interests face, an outcome measure that imposes constraints on respondents' choices (i.e., only being able to select one member) better matches the real-world decision task organized interests face.

An inspection of the distributions of the binary and ordinal outcomes supports the argument that the binary outcome more appropriately mirrors the real-world decision-making environment

organized interest representatives face. By construction, the distribution of the binary outcome is balanced both across respondents and within choice tasks; each respondent must select one or the other member in each choice task, such that the number of 0s and 1s in the data overall and for each choice task are equal. However, the distribution of the ordinal outcome exhibits low variability both across respondents and within choice tasks, suggesting that respondents did not exercise much discrimination when rating their level of interest in meeting with each member. The distribution of the 2632 ratings offered by respondents is markedly left-skewed, such that the vast majority of the ratings (1844, or 70.1%) were "very interested" (4) or "extremely interested" (5).[17] Additionally, in the 1309 choice tasks in which respondents provided ordinal ratings for both members, respondents offered the same ratings for both members in 560 (42.8%) of the tasks.

Respondents' tendency to both offer high ratings and to express the same ratings for both members in each choice task illustrates that, unless real-world constraints are imposed on their decisions, they use the ordinal outcome to express a general preference for direct contacts with members of Congress instead of expressing discerning preferences over the members before them in the choice task.[18] Thus, while I present both outcome measures for the sake of completeness, the binary outcome is a more appropriate measure of the behavior of interest—organized interest representatives' targeting behavior under real-world constraints.

Weighted Analyses

As discussed in the "Sampling Procedure and Descriptive Statistics" section of the Appendix, the four characteristics for which I had measures among both the sampling frame and the sample itself exhibited substantively small but statistically distinguishable imbalance. If respondents with different values for these four characteristics responded to the conjoint tasks differently, than this imbalance could inhibit the experiment's generalizability (Coppock 2019; Coppock, Leeper, and Mullinix 2018). To account for imbalances on these observable characteristics, I reestimate both models presented in the main text with survey weights calculated to reflect those characteristics' distribution in the sampling frame. The results of these weighted models, presented in Tables A.10 and A.11, are substantively similar to the unweighted models presented in Tables A.6 and A.7.

Issue Context-Conditional Analysis

The analyses in the main paper pool across the two issue contexts featured in the vignettes—an internet sales tax and prescription drugs. To demonstrate generalizability across issue contexts, I estimate a model akin to that presented in Figure 3 conditioning on issue context rather than legislative stage; again, the results of this model, presented in Table A.12, are substantively similar to those obtained in Table A.6. Point estimates for all attribute-levels that are distinguishable from their baselines in the main text when pooling across issue contexts are in the same direction and of similar magnitude when conditioning on issue context, though a few of them do not similarly

[17] The full distribution of the ratings along the five-point scale is: not at all interested (1), 70 (2.7%); slightly interested (2), 209 (7.9%); somewhat interested (3), 509 (19.3%); very interested (4), 895 (34.0%); extremely interested (5), 949 (36.1%).

[18] A further empirical implication of the low discrimination respondents exhibited with the ordinal outcome is that effect sizes are small, such that it is less likely that the AMCEs are statistically distinguishable. Hence, fewer AMCEs are statistically distinguishable in the analyses using the ordinal outcome as compared to those using the binary outcome, though the estimates are in the same direction in both cases.

obtain statistical distinguishability.[19]

Respondent Characteristic-Conditional Analyses

While the analyses in the main paper assume constant treatment effects, we may speculate that different types of organized interest representatives respond differently to the same profiles as a consequence of their personal characteristics such that the overall treatment effects are driven by heterogeneous effects. For example, firm lobbyists, who typically perform work for multiple clients, may better "imagine" working for a hypothetical client than would in-house lobbyists more accustomed to working for their own organization, such that the lobbyists' type may condition treatment effects. Again, despite their professional training, lobbyists may allow their partisan affinities to inform their targeting choices, such that they are more likely to target their copartisans (e.g., Broockman and Ryan 2016). Additionally, if lobbyists and policy advocates believe that they are more effective when lobbying copartisan members of Congress because they are more credible agents, then lobbyists might again focus their efforts on copartisans Hirsch et al. (n.d.). Further, because lobbyists who previously served in government have specialized knowledge about the policy process, and thus may have a different understanding of which types of congresspersons are attractive targets than other lobbyists, they may target members of Congress differently than other lobbyists (e.g., LaPira and Thomas 2017).

This is important to consider because the sample is not perfectly representative of the population of potential respondents on the observable characteristics available (see Table A.4) and because we lack important demographic information about the sampling frame needed to determine whether it is representative along other relevant dimensions, such as potential respondents' partisan affiliation (see Table A.5). When using a sample with distributions of characteristics that are known to or may differ from those in the full population, the ability to generalize observed treatment effects to the population hinges on the degree to which the treatments induce heterogeneous effects conditioned by those characteristics. If treatments are homogeneous, such that respondents' responses to treatments are not conditioned by their personal characteristics, then the treatment effects observed in the sample reflect the treatment effects that would be observed in the full population. However, if the treatment effects are heterogeneous, such that respondents' responses to treatments are conditioned by observed or unobserved personal characteristics, then the treatment effects observed in the sample will reflect the distribution of those characteristics which condition the treatments effects and are unlikely to generalize to the full population (see Coppock 2019; Coppock, Leeper, and Mullinix 2018). To illustrate how heterogeneous treatment effects might limit generalizability, consider that the majority of respondents identify as Democrats or lean Democratic (52.9%). Unfortunately, because information on this characteristic was collected as part of the survey itself, we do not know if the full population of lobbyists and policy advocates similarly leans Democratic. If this distribution does not reflect that observed in the full population, then we might be concerned that the finding that respondents are more likely to target Democratic congresspersons is driven by respondents' copartisanship (i.e., respondents are more likely to target copartisans, and because Democrats make up a majority of the sample, Democratic congresspersons are targeted more often) rather than by the majority/minority status implied by

[19]While I am I am unable to make definitive statements about whether the lack of statistical distinguishability for some conditional attribute-level estimates stems from decreased power rather than the absence of effects, the general consistency of the point estimates implies that it is the former; however, because the data at hand do not lend themselves to a definitive statement in this regard, readers should interpret them as suggestive.

the congresspersons' partisan affiliations.

In light of this concern, I use information from the Center for Responsive Politics and from the survey's pre-treatment question battery to explore several plausible potential heterogeneous effects driven by lobbyists' characteristics (firm vs. in-house lobbyist, partisanship, previous government experience, lobbying experience, and generational alignment) by conditioning the model presented in Figure 2, which pools across legislative stages, by each of these characteristics. In each of these alternative specifications, I find no evidence of heterogeneous effects which underlie the overall effects. Because these specifications divide the data into many more strata, decreasing the number of observations in each stratum and thus the precision of the estimates, some attribute-levels that are statistically distinguishable from their baseline levels in the models in the main text do not exhibit distinguishable effects; however, the estimates of the conditional effects for the attributes of interest typically remain in the same direction, are of comparable magnitude across different levels of each respondent characteristic, and are not statistically distinguishable from the estimates associated with the other levels of each respondent characteristic.[20] For example, in Table A.14, the probability of targeting a Democratic congressperson rather than an otherwise identical Republican congressperson is fairly similar for respondents who identify as Democrats (10 percentage points more likely), Independents (8 percentage points more likely), and Republicans (12 percentage points more likely), and, while the estimates are not statistically distinguishable at the Bonferroni-adjusted 95% confidence level, they are of similar magnitude as the treatment effect presented in the main paper (10 percentage points more likely) and are not statistically distinguishable from each other.

Attribute-Level-Conditional Analyses

We may also speculate that the constant treatment effects presented in the main paper stem from interactive effects among the attribute-levels featured in the members of Congress' profiles. If such interactive effects exist, they may either underlie the overall effects we observe for specific attribute-levels or may indicate that some attribute-levels for which we obtain null results do exert an effect on respondents' targeting choices, but only when paired with another other attribute-level that might manifest in the same profile. For instance, because committee outcomes typically hinge on the support of all or nearly all committee members of the majority party, particularly in the modern, polarized context where parties exert strong influence over committee behavior (Aldrich, Perry, and Rodhe 2013), respondents may be more likely to target members who are identified as both committee members and majority party members than members who are identified as either minority part committee members or majority party non-committee members. Again, though Hojnacki and Kimball (2001) do not find an unconditional relationship between interest's campaign contributions and its direct contacts with members of Congress, they do find that interests are more likely to experience direct contacts with members to whom they donate who oppose their position, relative to members who are undecided on the issue or are aligned with the interest's preferences. In addition, the presence of some interactive effects could indicate conceptual limitations of the experiment. For example, while the experiment is designed to isolate the effects of members' expressed preferences and majority party status through their stated positions and party affiliation, respectively, the presence of interactive effects between those two attributes may signal that respondents used members' party affiliation to infer the underlying preferences of the members (see the "Issue Contexts" subsection of the "Conjoint Experiment Protocol" section). Specifically,

[20] See Footnote 19.

if respondents thought that both proposals were liberal in nature and used members' party affiliations to intuit their underlying preferences, then respondents might target undeclared Democrats more often than undeclared Republicans because the undeclared Democrats might be more likely to be yet-to-declare supporters to whom respondents could provide "matching subsidies" to prod into publicly supporting the bill Hall and Deardorff (2006).

I explore the potential for attribute-level conditional effects by, for each attribute, re-specifying the main model in the paper which pools across legislative context (depicted in Figure 2 and presented in Table A.6) to interact the given attribute's non-baseline levels with each of the other attribute. I repeat this process for each of the nine attributes included in the member profiles, such that I estimate all possible two-way ACIEs between attribute-levels.[21] I present the resulting models in Tables A.18-A.26. For instance, in Table A.18, I present the ACIEs for each attribute-level conditioned by the non-baseline levels of the expressed preferences attribute—Undeclared and Support.[22] Looking at the ACIEs for each-attribute-level when members' expressed preferences are set to Undeclared or Support, relative to the baseline level of Oppose, we observe that none of the ACIEs are statistically distinguishable from zero at the corresponding Bonferroni-corrected 95% confidence level, suggesting that none of the other attribute-levels exhibit expressed preferences-conditional effects on respondents' targeting choices.[23] This pattern repeats itself for each remaining attribute, presented in the subsequent tables, suggesting that the results presented in the main paper do not mask underlying attribute-level-conditional effects.

[21]Note that ACIEs are only estimated for attribute-levels which are independent of the conditioning attribute; attributes with restrictions related to the conditioning attribute, such as cosponsorship when conditioned by expressed preferences, are inestimable.

[22]Note that the ACIEs for the attribute-level-conditional effects are the differences between each attribute-level's effect on the probability of selection when the attribute conditioning its effect is set to the specified attribute-level relative to when it is set to its baseline. This presentation differs from that of the ACIEs for other conditioning variables, such as respondents' characteristics, where the ACIEs presented are the probabilities of selection given the specified combination of attribute-levels and respondent characteristics; this difference arises from the functionality of the `cjoint` package, which provides the ACIEs as the differences in probabilities for attribute-level-by-attribute-level interactions, but the probabilities themselves when the attribute-levels are interacted with variables other than other attribute-levels.

[23]Bonferroni corrections account for the comparisons between the ACIEs estimated for each attribute-level for each attribute-level of the conditioning attribute. For instance, in Table A.18, we estimate 16 ACIEs for each of the two non-baseline level of the expressed preferences attribute, which are compared with each other and with the attribute's baseline; thus, there are 48 total comparisons.

Table A.6: Conjoint Experiment Attributes and Levels (Binary Choice, Pooled Across Stages)

Attribute/Level	Estimate (SE)	95% CI
Position		
Oppose (baseline)	-	-
Undeclared	0.19* (0.02)	[0.11, 0.26]
Support	-0.03 (0.03)	[-0.11, 0.05]
Cosponsor		
No (baseline)	-	-
Yes	-0.14* (0.03)	[-0.21, -0.06]
Party		
Republican (baseline)	-	-
Democrat	0.10* (0.02)	[0.02, 0.17]
Committee member		
No (baseline)	-	-
Yes	0.17* (0.02)	[0.10, 0.24]
Committee leadership		
No (baseline)	-	-
Subcommittee chair	0.16* (0.03)	[0.06, 0.25]
Subcommittee ranking member	0.07 (0.03)	[-0.03, 0.17]
Seniority		
Freshman (baseline)	-	-
1 term	-0.01 (0.02)	[-0.08, 0.07]
3 terms	0.02 (0.03)	[-0.06, 0.10]
7 terms	0.03 (0.03)	[-0.05, 0.11]
Margin of victory		
Less than 10% (baseline)	-	-
10% to 20%	-0.04 (0.03)	[-0.12, 0.04]
20% to 30%	-0.03 (0.03)	[-0.11, 0.05]
More than 30%	-0.04 (0.03)	[-0.12, 0.04]
Constituency strength		
Very low (baseline)	-	-
Low	0.01 (0.03)	[-0.07, 0.09]
Moderate	0.04 (0.03)	[-0.04, 0.12]

Attribute/Level	Estimate (SE)	95% CI
High	0.04 (0.02)	[-0.04, 0.12]
Campaign contributions		
None (baseline)	-	-
$100	0.00 (0.02)	[-0.07, 0.08]
$500	0.04 (0.03)	[-0.04, 0.12]
$1000	0.02 (0.02)	[-0.06, 0.09]

Number of observations=2612 (666 unique respondents). This table presents the average marginal component effects (AMCEs) presented in Figure 2 which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target, pooling across the committee and floor stages of the legislative process. AMCEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the AMCEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the AMCEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (20 comparisons, each attribute-level to its respective baseline and the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{20} = 0.0025$). Null hypothesis signficance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.0025$.

Table A.7: Conjoint Experiment Attributes and Levels (Binary Choice, Stage-Conditional)

	Committee Stage		Floor Stage	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.15* (0.04)	[0.04, 0.27]	0.22* (0.04)	[0.10, 0.33]
Support	-0.01 (0.04)	[-0.12, 0.11]	-0.05 (0.04)	[-0.18, 0.07]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.15* (0.04)	[-0.27, -0.03]	-0.11 (0.04)	[-0.23, 0.02]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.08 (0.03)	[-0.02, 0.18]	0.12* (0.03)	[0.01, 0.24]
Committee member				
No (baseline)	-	-	-	-
Yes	0.28* (0.03)	[0.18, 0.38]	0.06 (0.03)	[-0.05, 0.16]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.13 (0.04)	[-0.01, 0.27]	0.18* (0.05)	[0.03, 0.33]
Subcommittee ranking member	0.11 (0.05)	[-0.04, 0.26]	0.04 (0.04)	[-0.11, 0.18]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	-0.04 (0.03)	[-0.15, 0.07]	0.02 (0.03)	[-0.09, 0.13]
3 terms	-0.01 (0.03)	[-0.12, 0.10]	0.05 (0.04)	[-0.07, 0.17]
7 terms	0.04 (0.04)	[-0.08, 0.16]	0.03 (0.04)	[-0.09, 0.14]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	-0.03 (0.03)	[-0.15, 0.08]	-0.05 (0.04)	[-0.17, 0.06]
20% to 30%	0.00 (0.03)	[-0.11, 0.12]	-0.07 (0.04)	[-0.19, 0.06]
More than 30%	-0.04 (0.03)	[-0.15, 0.08]	-0.05 (0.04)	[-0.17, 0.06]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.04 (0.04)	[-0.08, 0.15]	-0.02 (0.04)	[-0.14, 0.10]

	Committee Stage		Floor Stage	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.07 (0.04)	[-0.06, 0.19]	0.01 (0.04)	[-0.10, 0.13]
High	0.06 (0.04)	[-0.06, 0.18]	0.01 (0.03)	[-0.10, 0.12]
Campaign contributions				
None (baseline)	-	-	-	-
$100	-0.01 (0.04)	[-0.12, 0.11]	0.02 (0.04)	[-0.10, 0.13]
$500	0.03 (0.04)	[-0.09, 0.15]	0.05 (0.04)	[-0.08, 0.17]
$1000	0.03 (0.03)	[-0.08, 0.15]	0.00 (0.04)	[-0.11, 0.12]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs) presented in Figure 3, which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target during the committee stage (second and third columns) and floor stage (fourth and fifth columns) of the legislative process. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two stages for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.8: Conjoint Experiment Attributes and Levels (Ordinal Ranking, Pooled Across Stages)

Attribute/Level	Estimate (SE)	95% CI
Position		
Oppose (baseline)	-	-
Undeclared	0.34* (0.06)	[0.17, 0.50]
Support	0.08 (0.06)	[-0.09, 0.25]
Cosponsor		
No (baseline)	-	-
Yes	-0.09 (0.06)	[-0.26, 0.08]
Party		
Republican (baseline)	-	-
Democrat	0.14 (0.06)	[-0.03, 0.31]
Committee member		
No (baseline)	-	-
Yes	0.57* (0.05)	[0.41, 0.73]
Committee leadership		
No (baseline)	-	-
Subcommittee chair	0.16 (0.06)	[-0.03, 0.35]
Subcommittee ranking member	0.15 (0.06)	[-0.04, 0.34]
Seniority		
Freshman (baseline)	-	-
1 term	0.08 (0.06)	[-0.09, 0.24]
3 terms	0.07 (0.06)	[-0.10, 0.24]
7 terms	0.08 (0.06)	[-0.09, 0.25]
Margin of victory		
Less than 10% (baseline)	-	-
10% to 20%	0.11 (0.06)	[-0.06, 0.27]
20% to 30%	0.04 (0.06)	[-0.12, 0.21]
More than 30%	0.02 (0.06)	[-0.15, 0.19]
Constituency strength		
Very low (baseline)	-	-
Low	0.03 (0.06)	[-0.16, 0.21]
Moderate	-0.01 (0.06)	[-0.19, 0.17]

Attribute/Level	Estimate (SE)	95% CI
High	0.08 (0.05)	[-0.08, 0.25]
Campaign contributions		
None (baseline)	-	-
$100	-0.00 (0.06)	[-0.17, 0.17]
$500	0.02 (0.06)	[-0.15, 0.18]
$1000	0.04 (0.05)	[-0.12, 0.21]

Number of observations=2632 (670 unique respondents). This table presents the average marginal component effects (AMCEs) which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the level of interest in lobbying the congressperson on a five-point scale, pooling across the committee and floor stages of the legislative process. AMCEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the AMCEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the AMCEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (20 comparisons, each attribute-level to its respective baseline and the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{20} = 0.0025$). Null hypothesis signficance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.0025$.

Table A.9: Conjoint Experiment Attributes and Levels (Ordinal Ranking, Stage-Conditional)

	Committee Stage		Floor Stage	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.28* (0.07)	[0.04, 0.52]	0.38* (0.09)	[0.11, 0.66]
Support	0.18 (0.07)	[-0.05, 0.41]	-0.02 (0.08)	[-0.29, 0.25]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.07 (0.07)	[-0.29, 0.16]	-0.09 (0.08)	[-0.36, 0.18]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.06 (0.08)	[-0.20, 0.32]	0.25 (0.08)	[-0.00, 0.51]
Committee member				
No (baseline)	-	-	-	-
Yes	0.90* (0.07)	[0.66, 1.14]	0.24* (0.07)	[0.00, 0.47]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.16 (0.08)	[-0.09, 0.42]	0.14 (0.10)	[-0.18, 0.46]
Subcommittee ranking member	0.19 (0.08)	[-0.06, 0.44]	0.15 (0.10)	[-0.16, 0.47]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	0.01 (0.07)	[-0.23, 0.25]	0.10 (0.08)	[-0.16, 0.36]
3 terms	0.04 (0.08)	[-0.21, 0.28]	0.08 (0.08)	[-0.17, 0.34]
7 terms	0.08 (0.08)	[-0.18, 0.33]	0.09 (0.08)	[-0.17, 0.35]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	0.13 (0.08)	[-0.11, 0.38]	0.09 (0.08)	[-0.16, 0.35]
20% to 30%	0.15 (0.08)	[-0.10, 0.39]	-0.07 (0.08)	[-0.34, 0.19]
More than 30%	-0.00 (0.07)	[-0.24, 0.23]	0.01 (0.09)	[-0.27, 0.28]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.05 (0.08)	[-0.21, 0.31]	-0.01 (0.08)	[-0.28, 0.26]

	Committee Stage		Floor Stage	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	-0.04 (0.08)	[-0.29, 0.22]	0.03 (0.09)	[-0.25, 0.30]
High	0.04 (0.07)	[-0.19, 0.27]	0.10 (0.08)	[-0.16, 0.36]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.00 (0.07)	[-0.22, 0.22]	0.01 (0.09)	[-0.27, 0.30]
$500	-0.01 (0.07)	[-0.24, 0.23]	0.01 (0.08)	[-0.25, 0.28]
$1000	0.03 (0.07)	[-0.21, 0.27]	0.08 (0.08)	[-0.18, 0.34]

Number of observations=2632 (670 unique respondents). This table presents the average component interaction effects (ACIEs) which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target during the committee stage (second and third columns) and floor stage (fourth and fifth columns) of the legislative process. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the AMCEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two stages for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.10: Conjoint Experiment Attributes and Levels (Binary Choice, Pooled Across Stages, Weighted)

Attribute/Level	Estimate (SE)	95% CI
Position		
Oppose (baseline)	-	-
Undeclared	0.17* (0.03)	[0.09, 0.26]
Support	-0.02 (0.03)	[-0.11, 0.06]
Cosponsor		
No (baseline)	-	-
Yes	-0.13* (0.03)	[-0.22, -0.03]
Party		
Republican (baseline)	-	-
Democrat	0.11* (0.03)	[0.03, 0.18]
Committee member		
No (baseline)	-	-
Yes	0.17* (0.03)	[0.09, 0.25]
Committee leadership		
No (baseline)	-	-
Subcommittee chair	0.16* (0.04)	[0.05, 0.27]
Subcommittee ranking member	0.09 (0.04)	[-0.02, 0.20]
Seniority		
Freshman (baseline)	-	-
1 term	0.00 (0.03)	[-0.08, 0.08]
3 terms	0.02 (0.03)	[-0.07, 0.11]
7 terms	0.04 (0.03)	[-0.05, 0.13]
Margin of victory		
Less than 10% (baseline)	-	-
10% to 20%	-0.03 (0.03)	[-0.12, 0.05]
20% to 30%	-0.01 (0.03)	[-0.10, 0.07]
More than 30%	-0.03 (0.03)	[-0.11, 0.06]
Constituency strength		
Very low (baseline)	-	-
Low	0.03 (0.03)	[-0.06, 0.12]
Moderate	0.05 (0.03)	[-0.05, 0.14]

Attribute/Level	Estimate (SE)	95% CI
High	0.04 (0.03)	[-0.04, 0.13]
Campaign contributions		
None (baseline)	-	-
$100	0.02 (0.03)	[-0.07, 0.10]
$500	0.08 (0.03)	[-0.01, 0.17]
$1000	0.03 (0.03)	[-0.06, 0.12]

Number of observations=2612 (666 unique respondents). This table presents the average marginal component effects (AMCEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target, pooling across the committee and floor stages of the legislative process. AMCEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest, and employ the survey weights described above in the "Empirical Results" section. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the AMCEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the AMCEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (20 comparisons, each attribute-level to its respective baseline and the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{20} = 0.0025$). Null hypothesis signficance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.0025$.

Table A.11: Conjoint Experiment Attributes and Levels (Binary Choice, Stage-Conditional, Weighted)

	Committee Stage		Floor Stage	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.14* (0.04)	[0.01, 0.26]	0.22* (0.04)	[0.10, 0.34]
Support	0.00 (0.04)	[-0.12, 0.13]	-0.06 (0.04)	[-0.19, 0.07]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.14* (0.04)	[-0.27, -0.00]	-0.10 (0.04)	[-0.24, 0.04]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.07 (0.04)	[-0.05, 0.18]	0.16* (0.04)	[0.04, 0.28]
Committee member				
No (baseline)	-	-	-	-
Yes	0.28* (0.03)	[0.18, 0.39]	0.06 (0.04)	[-0.06, 0.18]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.13 (0.05)	[-0.02, 0.28]	0.19* (0.05)	[0.03, 0.36]
Subcommittee ranking member	0.13 (0.05)	[-0.04, 0.30]	0.05 (0.05)	[-0.11, 0.20]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	-0.03 (0.04)	[-0.15, 0.09]	0.03 (0.04)	[-0.09, 0.14]
3 terms	-0.01 (0.04)	[-0.13, 0.12]	0.04 (0.04)	[-0.09, 0.17]
7 terms	0.05 (0.04)	[-0.07, 0.18]	0.02 (0.04)	[-0.11, 0.15]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	-0.03 (0.04)	[-0.16, 0.09]	-0.05 (0.04)	[-0.17, 0.08]
20% to 30%	0.03 (0.04)	[-0.09, 0.16]	-0.08 (0.04)	[-0.21, 0.06]
More than 30%	-0.01 (0.04)	[-0.13, 0.11]	-0.06 (0.04)	[-0.18, 0.07]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.06 (0.04)	[-0.07, 0.19]	-0.02 (0.04)	[-0.15, 0.11]

	Committee Stage		Floor Stage	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.10 (0.04)	[-0.04, 0.24]	-0.02 (0.04)	[-0.14, 0.11]
High	0.09 (0.04)	[-0.04 ,0.21]	-0.01 (0.04)	[-0.14, 0.11]
Campaign contributions				
None (baseline)	-	-	-	-
$100	-0.01 (0.04)	[-0.14, 0.12]	0.04 (0.04)	[-0.08, 0.17]
$500	0.07 (0.04)	[-0.06, 0.20]	0.08 (0.04)	[-0.06, 0.22]
$1000	0.04 (0.04)	[-0.08, 0.17]	0.01 (0.04)	[-0.11, 0.14]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target during the committee stage (second and third columns) and floor stage (fourth and fifth columns) of the legislative process. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest, and employ the survey weights described above in the "Empirical Results" section. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two stages for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.12: Conjoint Experiment Attributes and Levels (Binary Choice, Issue-Conditional)

	Internet Sales Tax		Prescription Drugs	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.17* (0.04)	[0.05, 0.29]	0.21* (0.03)	[0.10, 0.32]
Support	-0.02 (0.04)	[-0.14, 0.10]	-0.03 (0.04)	[-0.15, 0.09]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.13* (0.04)	[-0.25, -0.01]	-0.14* (0.04)	[-0.26, -0.02]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.13* (0.03)	[0.01, 0.24]	0.07 (0.03)	[-0.03, 0.18]
Committee member				
No (baseline)	-	-	-	-
Yes	0.17* (0.03)	[0.06, 0.27]	0.17* (0.03)	[0.07, 0.27]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.12 (0.05)	[-0.03, 0.26]	0.20* (0.04)	[0.06, 0.34]
Subcommittee ranking member	0.02 (0.05)	[-0.13, 0.17]	0.11 (0.04)	[-0.03, 0.26]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	0.00 (0.04)	[-0.11, 0.11]	-0.01 (0.03)	[-0.12, 0.10]
3 terms	0.02 (0.04)	[-0.09, 0.14]	0.01 (0.04)	[-0.11, 0.14]
7 terms	0.05 (0.04)	[-0.07, 0.17]	0.02 (0.04)	[-0.09, 0.14]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	-0.05 (0.04)	[-0.17, 0.07]	-0.04 (0.04)	[-0.16, 0.07]
20% to 30%	-0.02 (0.04)	[-0.14, 0.10]	-0.04 (0.04)	[-0.16, 0.09]
More than 30%	-0.05 (0.04)	[-0.17, 0.07]	-0.03 (0.04)	[-0.15, 0.09]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.05 (0.04)	[-0.07, 0.17]	-0.03 (0.04)	[-0.14, 0.09]

	Internet Sales Tax		Prescription Drugs	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.02 (0.04)	[-0.10, 0.14]	0.06 (0.04)	[-0.06, 0.18]
High	0.06 (0.04)	[-0.06, 0.17]	0.02 (0.04)	[-0.09, 0.14]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.00 (0.04)	[-0.12, 0.12]	0.00 (0.03)	[-0.11, 0.11]
$500	0.01 (0.04)	[-0.11, 0.14]	0.08 (0.04)	[-0.04, 0.19]
$1000	0.06 (0.04)	[-0.06, 0.18]	-0.03 (0.03)	[-0.14, 0.08]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target when the issue context of the choice task is an internet sales tax (second and third columns) or prescription drug prices (fourth and fifth columns). ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two issues for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.13: Conjoint Experiment Attributes and Levels (Binary Choice, Respondent Employer Type-Conditional)

	In-House		Firm	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.15* (0.03)	[0.05, 0.26]	0.24* (0.04)	[0.11, 0.36]
Support	-0.05 (0.03)	[-0.16, 0.05]	0.01 (0.04)	[-0.13, 0.15]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.12* (0.03)	[-0.23, -0.01]	-0.16* (0.04)	[-0.29, -0.03]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.09 (0.03)	[-0.01, 0.19]	0.11 (0.04)	[-0.01, 0.24]
Committee member				
No (baseline)	-	-	-	-
Yes	0.16* (0.03)	[0.06, 0.26]	0.18* (0.04)	[0.07, 0.30]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.18* (0.04)	[0.04, 0.31]	0.13 (0.05)	[-0.03, 0.29]
Subcommittee ranking member	0.08 (0.04)	[-0.06, 0.22]	0.06 (0.05)	[-0.10, 0.22]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	0.00 (0.03)	[-0.10, 0.10]	-0.01 (0.04)	[-0.15, 0.12]
3 terms	0.00 (0.03)	[-0.10, 0.11]	0.06 (0.04)	[-0.07, 0.19]
7 terms	0.02 (0.03)	[-0.08, 0.13]	0.05 (0.04)	[-0.09, 0.19]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	-0.03 (0.03)	[-0.13, 0.08]	-0.06 (0.04)	[-0.19, 0.07]
20% to 30%	-0.02 (0.03)	[-0.13, 0.09]	-0.03 (0.04)	[-0.17, 0.10]
More than 30%	-0.03 (0.03)	[-0.14, 0.07]	-0.05 (0.04)	[-0.18, 0.08]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	-0.02 (0.04)	[-0.13, 0.10]	0.05 (0.04)	[-0.08, 0.18]

	In-House		Firm	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.02 (0.04)	[-0.09, 0.14]	0.06 (0.04)	[-0.06, 0.19]
High	0.02 (0.03)	[-0.08, 0.13]	0.06 (0.04)	[-0.07, 0.19]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.02 (0.03)	[-0.08, 0.12]	-0.02 (0.04)	[-0.15, 0.10]
$500	0.08 (0.03)	[-0.03, 0.19]	-0.01 (0.04)	[-0.15, 0.12]
$1000	0.02 (0.03)	[-0.08, 0.13]	0.00 (0.04)	[-0.13, 0.12]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target when the respondent works directly for the client (second and third columns) or is employed by a firm (fourth and fifth columns). ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two employer types for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.14: Conjoint Experiment Attributes and Levels (Binary Choice, Respondent Partisanship-Conditional)

	Democrat		Independent		Republican	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position						
Oppose (baseline)	-	-	-	-	-	-
Undeclared	0.21* (0.04)	[0.09, 0.33]	0.16 (0.05)	[-0.01, 0.33]	0.17 (0.05)	[-0.00, 0.35]
Support	-0.03 (0.04)	[-0.15, 0.10]	-0.06 (0.05)	[0.10 , -0.22]	0.02 (0.06)	[-0.17, 0.20]
Cosponsor						
No (baseline)	-	-	-	-	-	-
Yes	-0.14* (0.04)	[-0.27, -0.01]	-0.14 (0.05)	[-0.31, 0.02]	-0.14 (0.05)	[-0.30, 0.03]
Party						
Republican (baseline)	-	-	-	-	-	-
Democrat	0.10 (0.03)	[-0.02, 0.22]	0.08 (0.05)	[-0.08, 0.23]	0.12 (0.05)	[-0.04, 0.28]
Committee member						
No (baseline)	-	-	-	-	-	-
Yes	0.17* (0.03)	[0.06, 0.29]	0.23* (0.05)	[0.07, 0.38]	0.11 (0.05)	[-0.04, 0.27]
Committee leadership						
No (baseline)	-	-	-	-	-	-
Subcommittee chair	0.14 (0.05)	[-0.01, 0.29]	0.20* (0.06)	[0.01, 0.39]	0.13 (0.07)	[-0.09, 0.36]
Subcommittee ranking member	0.05 (0.05)	[-0.12, 0.21]	0.01 (0.06)	[-0.20, 0.22]	0.16 (0.06)	[-0.04, 0.36]
Seniority						
Freshman (baseline)	-	-	-	-	-	-
1 term	-0.03 (0.04)	[-0.15, 0.10]	0.01 (0.05)	[-0.14, 0.16]	-0.01 (0.05)	[-0.18, 0.15]
3 terms	-0.01 (0.04)	[-0.13, 0.11]	0.04 (0.05)	[-0.13, 0.21]	0.05 (0.05)	[-0.13, 0.22]
7 terms	0.02 (0.04)	[-0.12, 0.15]	0.12 (0.05)	[-0.04, 0.29]	-0.03 (0.05)	[-0.20, 0.14]
Margin of victory						
Less than 10% (baseline)	-	-	-	-	-	-
10% to 20%	-0.01 (0.04)	[-0.14, 0.11]	-0.04 (0.05)	[-0.20, 0.13]	-0.12 (0.05)	[-0.29, 0.05]
20% to 30%	-0.05 (0.04)	[-0.18, 0.08]	0.00 (0.05)	[-0.16, 0.17]	-0.05 (0.06)	[-0.24, 0.14]
More than 30%	-0.02 (0.04)	[-0.15, 0.10]	-0.03 (0.05)	[-0.19,0.13]	-0.09 (0.06)	[-0.27, 0.09]
Constituency strength						
Very low (baseline)	-	-	-	-	-	-
Low	-0.05 (0.04)	[-0.17, 0.08]	0.05 (0.05)	[-0.14, 0.23]	0.06 (0.05)	[-0.12, 0.23]

	Democrat		Independent		Republican	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	-0.01 (0.04)	[-0.14, 0.13]	0.05 (0.05)	[-0.13, 0.22]	0.10 (0.05)	[-0.07, 0.27]
High	0.04 (0.04)	[-0.09, 0.16]	0.02 (0.05)	[-0.14, 0.18]	0.06 (0.05)	[-0.10, 0.23]
Campaign contributions						
None (baseline)	-	-	-	-	-	-
$100	0.03 (0.04)	[-0.09, 0.16]	-0.03 (0.04)	[-0.18, 0.11]	-0.01 (0.05)	[-0.18, 0.16]
$500	0.04 (0.04)	[-0.09, 0.17]	0.03 (0.05)	[-0.15, 0.21]	0.05 (0.05)	[-0.13, 0.23]
$1000	0.03 (0.04)	[-0.09, 0.15]	0.00 (0.05)	[-0.15, 0.15]	0.00 (0.05)	[-0.17, 0.17]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target when the respondent self-identifies as a Democrat or Democratic leaner (second and third columns), independent, (fourth and fifth columns), or Republican or Republican leaner (sixth and seventh columns). ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (60 comparisons—one comparison for each of the three partisanship categories for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{60} = 0.0008\overline{3}$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.0008\overline{3}$.

Table A.15: Conjoint Experiment Attributes and Levels (Binary Choice, Government Experience-Conditional)

	Previous Experience		No Previous Experience	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.24* (0.04)	[0.12, 0.36]	0.15 (0.03)*	[0.06, 0.25]
Support	-0.04 (0.04)	[-0.17, 0.09]	-0.02 (0.03)	[-0.12, 0.08]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.10 (0.04)	[-0.22, 0.03]	-0.15* (0.03)	[-0.25, -0.05]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.15* (0.04)	[0.03, 0.27]	0.07 (0.03)	[-0.02, 0.16]
Committee member				
No (baseline)	-	-	-	-
Yes	0.17* (0.04)	[0.04, 0.29]	0.18* (0.03)	[0.09, 0.26]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.20* (0.05)	[0.05, 0.36]	0.13* (0.04)	[0.01, 0.25]
Subcommittee ranking member	0.13 (0.06)	[-0.03, 0.30]	0.04 (0.04)	[-0.09, 0.16]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	0.03 (0.04)	[-0.10, 0.16]	-0.02 (0.03)	[-0.11, 0.07]
3 terms	0.02 (0.04)	[-0.11, 0.15]	0.03 (0.03)	[-0.07, 0.12]
7 terms	0.07 (0.04)	[-0.05, 0.19]	0.02 (0.03)	[-0.09, 0.12]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	-0.01 (0.04)	[-0.15, 0.12]	-0.05 (0.03)	[-0.15, 0.04]
20% to 30%	-0.05 (0.04)	[-0.18, 0.08]	-0.01 (0.03)	[-0.11, 0.09]
More than 30%	0.04 (0.04)	[-0.09, 0.16]	-0.07 (0.03)	[-0.17, 0.02]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.02 (0.05)	[-0.13, 0.16]	0.01 (0.03)	[-0.09, 0.10]

	Previous Experience		No Previous Experience	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.00 (0.05)	[-0.14, 0.15]	0.06 (0.03)	[-0.04, 0.16]
High	0.05 (0.04)	[-0.08, 0.18]	0.04 (0.03)	[-0.06, 0.13]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.00 (0.04)	[-0.12, 0.12]	0.00 (0.03)	[-0.09, 0.10]
$500	0.00 (0.05)	[-0.14, 0.14]	0.06 (0.03)	[-0.03, 0.16]
$1000	0.00 (0.04)	[-0.13, 0.12]	0.02 (0.03)	[-0.07, 0.11]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target when the respondent reports having previous government experience (second and third columns) or not (fourth and fifth columns). ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two experience types for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.16: Conjoint Experiment Attributes and Levels (Binary Choice, Lobbying Experience-Conditional)

	Less Experienced		More Experienced	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.14* (0.04)	[0.02, 0.27]	0.23* (0.03)	[0.12, 0.33]
Support	-0.04 (0.04)	[-0.17, 0.09]	-0.02 (0.03)	[-0.13, 0.09]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.12 (0.04)	[-0.25, 0.01]	-0.15* (0.03)	[-0.26, -0.03]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.06 (0.04)	[-0.06, 0.19]	0.12* (0.03)	[0.02, 0.22]
Committee member				
No (baseline)	-	-	-	-
Yes	0.19* (0.04)	[0.08, 0.31]	0.15* (0.03)	[0.05, 0.25]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.14 (0.05)	[-0.02, 0.30]	0.18* (0.04)	[0.04, 0.31]
Subcommittee ranking member	0.03 (0.05)	[-0.13, 0.19]	0.12 (0.04)	[-0.02, 0.26]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	0.03 (0.04)	[-0.09, 0.15]	-0.04 (0.03)	[-0.14, 0.06]
3 terms	0.02 (0.04)	[-0.10, 0.15]	0.02 (0.03)	[-0.09, 0.13]
7 terms	0.04 (0.04)	[-0.08, 0.16]	0.03 (0.04)	[-0.09, 0.15]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	0.02 (0.04)	[-0.10, 0.14]	()	[,]
20% to 30%	0.05 (0.04)	[-0.08, 0.19]	()	[,]
More than 30%	0.01 (0.04)	[-0.11, 0.13]	()	[,]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.01 (0.04)	[-0.12, 0.14]	-0.10 (0.03)	[-0.21, 0.01]

	Less Experienced		More Experienced	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.02 (0.04)	[-0.11, 0.15]	-0.10 (0.03)	[-0.21, 0.01]
High	0.06 (0.04)	[-0.07, 0.18]	-0.08 (0.04)	[-0.19, 0.03]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.02 (0.04)	[-0.10, 0.14]	-0.02 (0.03)	[-0.12, 0.09]
$500	0.02 (0.04)	[-0.10, 0.15]	0.06 (0.04)	[-0.06, 0.18]
$1000	0.02 (0.04)	[-0.10, 0.13]	0.01 (0.03)	[-0.09, 0.12]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target when the respondent identifies as having 15 years or less of lobbying experience (second and third columns) or 16 years or more of lobbying experience (fourth and fifth columns). ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two employer types for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.17: Conjoint Experiment Attributes and Levels (Binary Choice, Profile/Respondent Generational Alignment-Conditional)

	Aligned		Not Aligned	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	0.22* (0.03)	[0.10, 0.33]	0.16* (0.04)	[0.03, 0.28]
Support	-0.01 (0.04)	[-0.12, 0.11]	-0.05 (0.04)	[-0.18, 0.08]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.14* (0.04)	[-0.26, -0.02]	-0.13* (0.04)	[-0.25, -0.02]
Party				
Republican (baseline)	-	-	-	-
Democrat	0.10 (0.03)	[-0.01, 0.21]	0.10 (0.03)	[-0.01, 0.20]
Committee member				
No (baseline)	-	-	-	-
Yes	0.15 (0.03)	[0.05, 0.25]	0.19* (0.03)	[0.08, 0.29]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	0.17* (0.04)	[0.04, 0.31]	0.14 (0.05)	[-0.01, 0.29]
Subcommittee ranking member	0.08 (0.05)	[-0.07, 0.22]	0.07 (0.04)	[-0.07, 0.22]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	-0.01 (0.04)	[-0.13, 0.12]	-0.01 (0.03)	[-0.12, 0.09]
3 terms	0.04 (0.03)	[-0.07, 0.14]	-0.01 (0.03)	[-0.11, 0.10]
7 terms	0.03 (0.03)	[-0.08, 0.14]	0.03 (0.03)	[-0.08, 0.14]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	-0.07 (0.04)	[-0.18, 0.05]	-0.02 (0.04)	[-0.14, 0.11]
20% to 30%	-0.04 (0.04)	[-0.17, 0.08]	0.00 (0.04)	[-0.12, 0.12]
More than 30%	-0.05 (0.04)	[-0.17, 0.06]	-0.02 (0.04)	[-0.14, 0.10]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	-0.01 (0.04)	[-0.12, 0.11]	0.02 (0.04)	[-0.10, 0.14]

	Aligned		Not Aligned	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.04 (0.04)	[-0.08, 0.16]	0.03 (0.04)	[-0.09, 0.16]
High	0.04 (0.04)	[-0.08, 0.15]	0.03 (0.04)	[-0.09, 0.14]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.01 (0.04)	[-0.10, 0.13]	-0.01 (0.04)	[-0.13, 0.11]
$500	0.05 (0.04)	[-0.07, 0.17]	0.04 (0.04)	[-0.09, 0.16]
$1000	0.05 (0.04)	[-0.07, 0.17]	-0.02 (0.04)	[-0.13, 0.10]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of selection as a lobbying target when both the respondent and the member of Congress profile have similar (second and third columns) or dissimilar (fourth and fifth columns) tenures in their respective jobs. More specifically, a profile-respondent dyad is coded as "aligned" if the respondent reports less than 15 years of lobbying experience and the member of Congress is portrayed as being a freshman or having previously served 1 term, or if the respondent reports 16 years or more of lobbying experience and the member of Congress is portrayed as having previously served 3 or 7 terms; all other correspondences of respondent lobbying experience and member of Congress tenure are coded as "not aligned." ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (40 comparisons—one comparison for each of the two employer types for each attribute-level to its respective baseline and for the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{40} = 0.00125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00125$.

Table A.18: Conjoint Experiment Attributes and Levels (Binary Choice, Expressed Preferences-Conditional)

	Undeclared		Support	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	-	-	-	-
Support	-	-	-	-
Cosponsor				
No (baseline)	-	-	-	-
Yes	-	-	-	-
Party				
Republican (baseline)	-	-	-	-
Democrat	0.04 (0.07)	[-0.18, 0.26]	-0.09 (0.07)	[-0.31, 0.13]
Committee member				
No (baseline)	-	-	-	-
Yes	0.11 (0.06)	[-0.10, 0.32]	0.04 (0.07)	[-0.17, 0.26]
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	-0.06 (0.09)	[-0.34, 0.22]	0.09 (0.09)	[-0.20, 0.38]
Subcommittee ranking member	-0.01 (0.09)	[-0.31, 0.29]	0.03 (0.09)	[-0.27, 0.34]
Seniority				
Freshman (baseline)	-	-	-	-
1 term	0.06 (0.07)	[-0.18, 0.31]	-0.03 (0.07)	[-0.27, 0.21]
3 terms	0.01 (0.07)	[-0.23, 0.25]	0.01 (0.08)	[-0.24, 0.26]
7 terms	-0.03 (0.08)	[-0.27, 0.22]	-0.06 (0.07)	[-0.30, 0.18]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	0.10 (0.07)	[-0.14, 0.35]	0.04 (0.07)	[-0.21, 0.28]
20% to 30%	0.04 (0.07)	[-0.21, 0.28]	0.08 (0.07)	[-0.16, 0.32]
More than 30%	0.06 (0.07)	[-0.17, 0.30]	0.06 (0.07)	[-0.19, 0.30]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	-0.07 (0.07)	[-0.31, 0.17]	-0.06 (0.07)	[-0.31, 0.18]

	Undeclared		Support	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	-0.01 (0.07)	[-0.25, 0.22]	-0.15 (0.08)	[-0.41, 0.11]
High	-0.08 (0.07)	[-0.32, 0.15]	-0.10 (0.08)	[-0.34, 0.15]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.11 (0.08)	[-0.15, 0.36]	0.11 (0.08)	[-0.15, 0.37]
$500	-0.02 (0.08)	[-0.27, 0.22]	-0.02 (0.08)	[-0.28, 0.24]
$1000	-0.04	[-0.30, 0.22]	0.05 (0.08)	[-0.21, 0.32]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member's expressed preferences attribute-level is Undeclared (second and third columns) or Support (fourth and fifth columns) relative to when the attribute-level is Opposed (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (48 comparisons—one comparison for each attribute-level across the three attribute-values for the expressed preferences attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{48} \approx 0.001041667$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.001041667$.

Table A.19: Conjoint Experiment Attributes and Levels (Binary Choice, Cosponsor-Conditional)

		Cosponsor
Attribute/Level	Estimate (SE)	95% CI
Position		
Oppose (baseline)	-	-
Undeclared	-	-
Support	-	-
Cosponsor		
No (baseline)	-	-
Yes	-	-
Party		
Republican (baseline)	-	-
Democrat	-0.02 (0.06)	[-0.21, 0.17]
Committee member		
No (baseline)	-	-
Yes	-0.08 (0.06)	[-0.26, 0.11]
Committee leadership		
No (baseline)	-	-
Subcommittee chair	-0.18 (0.09)	[-0.44, 0.09]
Subcommittee ranking member	0.06 (0.09)	[-0.20, 0.32]
Seniority		
Freshman (baseline)	-	-
1 term	0.11 (0.07)	[-0.10, 0.31]
3 terms	0.08 (0.07)	[-0.13, 0.29]
7 terms	0.12 (0.07)	[-0.09, 0.33]
Margin of victory		
Less than 10% (baseline)	-	-
10% to 20%	0.02 (0.08)	[-0.21, 0.25]
20% to 30%	-0.01 (0.07)	[-0.22, 0.21]
More than 30%	-0.03 (0.08)	[-0.26, 0.19]
Constituency strength		
Very low (baseline)	-	-
Low	-0.01 (0.07)	[-0.22, 0.19]

		Cosponsor
Attribute/Level	Estimate (SE)	95% CI
Moderate	0.05 (0.07)	[-0.16, 0.26]
High	-0.02 (0.07)	[-0.23, 0.20]
Campaign contributions		
None (baseline)	-	-
$100	-0.02 (0.07)	[-0.24, 0.20]
$500	0.00 (0.08)	[-0.23, 0.22]
$1000	-0.04 (0.07)	[-0.44, 0.09]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member is identified as a cosponsor relative to when the member is not identified as a cosponsor (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. Because members cannot be cosponsors unless they are also supportive of the proposal, these comparisons are made only among members identified as supporters. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (16 comparisons—one comparison for each attribute-level across the two attribute-values for the cosponsor attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{16} = 0.003125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.003125$.

Table A.20: Conjoint Experiment Attributes and Levels (Binary Choice, Party-Conditional)

		Democrat
Attribute/Level	Estimate (SE)	95% CI
Position		
Oppose (baseline)	-	-
Undeclared	-0.01 (0.06)	[-0.20, 0.18]
Support	-0.10 (0.06)	[-0.29, 0.10]
Cosponsor		
No (baseline)	-	-
Yes	-	-
Party		
Republican (baseline)	-	-
Democrat	-	-
Committee member		
No (baseline)	-	-
Yes	0.00 (0.05)	[-0.14, 0.13]
Committee leadership		
No (baseline)	-	-
Subcommittee chair	-	-
Subcommittee ranking member	-	-
Seniority		
Freshman (baseline)	-	-
1 term	0.09 (0.06)	[-0.09, 0.27]
3 terms	0.04 (0.06)	[-0.15, 0.23]
7 terms	0.01 (0.07)	[-0.18, 0.21]
Margin of victory		
Less than 10% (baseline)	-	-
10% to 20%	-0.16 (0.06)	[-0.34, 0.03]
20% to 30%	-0.10 (0.07)	[-0.29, 0.09]
More than 30%	-0.11 (0.07)	[-0.30, 0.09]
Constituency strength		
Very low (baseline)	-	-
Low	-0.02 (0.06)	[-0.20, 0.17]

		Democrat
Attribute/Level	Estimate (SE)	95% CI
Moderate	-0.05 (0.07)	[-0.25, 0.15]
High	-0.05 (0.06)	[-0.23, 0.14]
Campaign contributions		
None (baseline)	-	-
$100	0.17 (0.06)	[-0.02, 0.36]
$500	0.04 (0.07)	[-0.15, 0.24]
$1000	0.06 (0.06)	[-0.13, 0.25]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member is identified as a Democrat relative to when the member is identified as a Republican (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (16 comparisons—one comparison for each attribute-level across the two attribute-values for the party attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{16} = 0.003125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.003125$.

Table A.21: Conjoint Experiment Attributes and Levels (Binary Choice, Committee Member-Conditional)

		Committee Member
Attribute/Level	Estimate (SE)	95% CI
Position		
Oppose (baseline)	-	-
Undeclared	0.12 (0.06)	[-0.07, 0.31]
Support	0.04 (0.07)	[-0.16, 0.23]
Cosponsor		
No (baseline)	-	-
Yes	-0.08 (0.06)	[-0.27, 0.10]
Party		
Republican (baseline)	-	-
Democrat	0.01 (0.05)	[-0.13, 0.14]
Committee member		
No (baseline)	-	-
Yes	-	-
Committee leadership		
No (baseline)	-	-
Subcommittee chair	-	-
Subcommittee ranking member	-	-
Seniority		
Freshman (baseline)	-	-
1 term	-0.09 (0.06)	[-0.28, 0.10]
3 terms	-0.11 (0.06)	[-0.30, 0.07]
7 terms	-0.06 (0.07)	[-0.25, 0.14]
Margin of victory		
Less than 10% (baseline)	-	-
10% to 20%	0.04 (0.06)	[-0.14, 0.23]
20% to 30%	0.07 (0.06)	[-0.12, 0.26]
More than 30%	-0.03 (0.07)	[-0.23, 0.17]
Constituency strength		
Very low (baseline)	-	-
Low	-0.05 (0.06)	[-0.23, 0.13]

		Committee Member
Attribute/Level	Estimate (SE)	95% CI
Moderate	-0.01 (0.07)	[-0.21, 0.18]
High	-0.10 (0.06)	[-0.28, 0.09]
Campaign contributions		
None (baseline)	-	-
$100	-0.14 (0.06)	[-0.32, 0.05]
$500	-0.03 (0.07)	[-0.22, 0.17]
$1000	-0.04 (0.06)	[-0.23, 0.14]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member is identified as a committee member relative to when the member is not identified as a committee member (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (15 comparisons—one comparison for each attribute-level across the two attribute-values for the committee member attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{16} \approx 0.003125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.003125$.

Table A.22: Conjoint Experiment Attributes and Levels (Binary Choice, Committee Leadership-Conditional)

	Subcommittee Chair		Subcommittee Ranking Member	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position				
Oppose (baseline)	-	-	-	-
Undeclared	-0.05 (0.09)	[-0.33, 0.22]	-0.06 (0.09)	[-0.23, 0.36]
Support	0.13 (0.09)	[-0.17, 0.43]	-0.02 (0.09)	[-0.33, 0.29]
Cosponsor				
No (baseline)	-	-	-	-
Yes	-0.19 (0.09)	[-0.49, 0.11]	0.09 (0.09)	[-0.21, 0.38]
Party				
Republican (baseline)	-	-	-	-
Democrat	-	-	-	-
Committee member				
No (baseline)	-	-	-	-
Yes	-	-	-	-
Committee leadership				
No (baseline)	-	-	-	-
Subcommittee chair	-	-	-	-
Subcommittee ranking member	-	-	-	-
Seniority				
Freshman (baseline)	-	-	-	-
1 term	-0.02 (0.09)	[-0.30, 0.27]	0.19 (0.09)	[-0.09, 0.48]
3 terms	0.13 (0.09)	[-0.17, 0.43]	0.15 (0.09)	[-0.14, 0.44]
7 terms	0.03 (0.09)	[-0.28, 0.33]	0.02 (0.09)	[-0.27, 0.31]
Margin of victory				
Less than 10% (baseline)	-	-	-	-
10% to 20%	0.02 (0.09)	[-0.28, 0.32]	-0.19 (0.09)	[-0.49, 0.11]
20% to 30%	-0.02 (0.09)	[-0.32, 0.28]	-0.05 (0.10)	[-0.36, 0.26]
More than 30%	0.12 (0.09)	[-0.17, 0.41]	0.02 (0.09)	[-0.29, 0.33]
Constituency strength				
Very low (baseline)	-	-	-	-
Low	0.00 (0.09)	[-0.29, 0.29]	0.07 (0.09)	[-0.23, 0.36]

	Subcommittee Chair		Subcommittee Ranking Member	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.14 (0.09)	[-0.16, 0.44]	-0.04 (0.09)	[-0.34, 0.26]
High	0.07 (0.09)	[-0.22, 0.35]	-0.01 (0.09)	[-0.31, 0.29]
Campaign contributions				
None (baseline)	-	-	-	-
$100	0.01 (0.09)	[-0.29, 0.30]	0.16 (0.09)	[-0.12, 0.45]
$500	0.06 (0.10)	[-0.25, 0.38]	-0.05 (0.10)	[-0.36, 0.27]
$1000	0.08 (0.09)	[-0.22, 0.39]	0.07 (0.09)	[-0.22, 0.37]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member is identified as a Subcommittee Chair (second and third columns) or Subcommittee Ranking Member (fourth and fifth columns) relative to when the member is not identified as holding a committee leadership position (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. Because members cannot be subcommittee chairs (ranking members) unless they are also committee members and Democrats (Republicans), these comparisons are made only among members identified as committee members and Democrats (Republicans). ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (45 comparisons—one comparison for each attribute-level across the three attribute-values for the expressed preferences attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{45} \approx 0.001111111$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.001111111$.

Table A.23: Conjoint Experiment Attributes and Levels (Binary Choice, Seniority-Conditional)

	1 Term		3 terms		7 terms	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position						
Oppose (baseline)	-	-	-	-	-	-
Undeclared	0.08 (0.07)	[-0.17, 0.33]	0.00 (0.07)	[-0.25, 0.25]	-0.02 (0.08)	[-0.28, 0.23]
Support	-0.04 (0.07)	[-0.28, 0.21]	0.00 (0.08)	[-0.25, 0.25]	-0.06 (0.07)	[-0.30, 0.19]
Cosponsor						
No (baseline)	-	-	-	-	-	-
Yes	0.11 (0.07)	[-0.12, 0.35]	0.08 (0.07)	[-0.16, 0.32]	0.12 (0.07)	[-0.12, 0.37]
Party						
Republican (baseline)	-	-	-	-	-	-
Democrat	0.08 (0.06)	[-0.13, 0.28]	0.05 (0.07)	[-0.17, 0.27]	0.03 (0.07)	[-0.20, 0.26]
Committee member						
No (baseline)	-	-	-	-	-	-
Yes	-0.10 (0.06)	[-0.32, 0.11]	-0.12 (0.06)	[-0.33, 0.09]	-0.07 (0.07)	[-0.29, 0.16]
Committee leadership						
No (baseline)	-	-	-	-	-	-
Subcommittee chair	0.00 (0.09)	[-0.30, 0.30]	0.12 (0.09)	[-0.19, 0.42]	0.01 (0.09)	[-0.31, 0.32]
Subcommittee ranking member	0.21 (0.09)	[-0.08, 0.51]	0.16 (0.09)	[-0.14, 0.45]	0.06 (0.09)	[-0.24, 0.35]
Seniority						
Freshman (baseline)	-	-	-	-	-	-
1 term	-	-	-	-	-	-
3 terms	-	-	-	-	-	-
7 terms	-	-	-	-	-	-
Margin of victory						
Less than 10% (baseline)	-	-	-	-	-	-
10% to 20%	-0.04 (0.07)	[-0.28, 0.20]	0.00 (0.07)	[-0.25, 0.24]	-0.08 (0.08)	[-0.33, 0.18]
20% to 30%	-0.02 (0.07)	[-0.26, 0.22]	-0.01 (0.07)	[-0.25, 0.24]	-0.07 (0.07)	[-0.31, 0.17]
More than 30%	-0.03 (0.07)	[-0.21, 0.26]	0.08 (0.07)	[-0.17, 0.34]	0.05 (0.08)	[-0.21, 0.31]
Constituency strength						
Very low (baseline)	-	-	-	-	-	-
Low	-0.04 (0.07)	[-0.28, 0.20]	-0.08 (0.07)	[-0.31, 0.15]	-0.01 (0.07)	[-0.24, 0.23]

	1 Term		3 terms		7 terms	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	-0.02 (0.07)	[-0.26, 0.21]	-0.10 (0.07)	[-0.35, 0.15]	0.03 (0.08)	[-0.22, 0.29]
High	0.01 (0.07)	[-0.24, 0.25]	-0.02 (0.07)	[-0.26, 0.23]	0.13 (0.07)	[-0.11, 0.37]
Campaign contributions						
None (baseline)	-	-	-	-	-	-
$100	-0.06 (0.07)	[-0.30, 0.18]	0.00 (0.08)	[-0.26, 0.26]	-0.04 (0.08)	[-0.29, 0.22]
$500	-0.05 (0.07)	[-0.30, 0.19]	0.05 (0.08)	[-0.21, 0.32]	0.00 (0.08)	[-0.25, 0.26]
$1000	0.08 (0.08)	[-0.18, 0.33]	0.06 (0.08)	[-0.19, 0.31]	0.03 (0.08)	[-0.22, 0.29]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member's seniority attribute-level is 1 term (second and third columns), 3 terms (fourth and fifth columns), or 7 terms (sixth and seventh columns) relative to when the attribute-level is Freshman (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (64 comparisons—one comparison for each attribute-level across the three attribute-values for the seniority attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{64} = 0.00078125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00078125$.

Table A.24: Conjoint Experiment Attributes and Levels (Binary Choice, Margin of Victory-Conditional)

	10% to 20%		20% to 30%		More than 30%	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position						
Oppose (baseline)	-	-	-	-	-	-
Undeclared	0.09 (0.07)	[-0.16, 0.34]	0.01 (0.08)	[-0.24, 0.26]	0.05 (0.07)	[-0.20, 0.30]
Support	0.03 (0.07)	[-0.22, 0.28]	0.06 (0.07)	[-0.18, 0.31]	0.03 (0.07)	[-0.22, 0.28]
Cosponsor						
No (baseline)	-	-	-	-	-	-
Yes	0.01 (0.08)	[-0.25, 0.27]	-0.03 (0.07)	[-0.27, 0.22]	-0.04 (0.08)	[-0.30, 0.21]
Party						
Republican (baseline)	-	-	-	-	-	-
Democrat	-0.15 (0.07)	[-0.37, 0.08]	-0.11 (0.07)	[-0.34, 0.11]	-0.11 (0.07)	[-0.35, 0.12]
Committee member						
No (baseline)	-	-	-	-	-	-
Yes	0.04 (0.06)	[-0.17, 0.25]	0.08 (0.06)	[-0.14, 0.29]	-0.03 (0.07)	[-0.25, 0.19]
Committee leadership						
No (baseline)	-	-	-	-	-	-
Subcommittee chair	0.01 (0.09)	[-0.29, 0.32]	-0.02 (0.09)	[-0.33, 0.29]	0.12 (0.09)	[-0.18, 0.42]
Subcommittee ranking member	-0.16 (0.09)	[-0.46, 0.15]	-0.06 (0.09)	[-0.37, 0.26]	0.04 (0.10)	[-0.29, 0.36]
Seniority						
Freshman (baseline)	-	-	-	-	-	-
1 term	-0.04 (0.07)	[-0.28, 0.20]	-0.04 (0.07)	[-0.28, 0.20]	0.01 (0.07)	[-0.23, 0.24]
3 terms	0.02 (0.07)	[-0.22, 0.26]	0.00 (0.07)	[-0.24, 0.25]	0.07 (0.07)	[-0.17, 0.32]
7 terms	-0.06 (0.08)	[-0.32, 0.20]	-0.07 (0.07)	[-0.31, 0.17]	0.05 (0.08)	[-0.21, 0.31]
Margin of victory						
Less than 10% (baseline)	-	-	-	-	-	-
10% to 20%	-	-	-	-	-	-
20% to 30%	-	-	-	-	-	-
More than 30%	-	-	-	-	-	-
Constituency strength						
Very low (baseline)	-	-	-	-	-	-
Low	-0.01 (0.07)	[-0.25, 0.23]	-0.10 (0.08)	[-0.36, 0.15]	-0.24 (0.08)	[-0.51, 0.02]

	10% to 20%		20% to 30%		More than 30%	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	-0.01 (0.07)	[-0.26, 0.24]	-0.01 (0.07)	[-0.25, 0.23]	-0.06 (0.07)	[-0.31, 0.19]
High	-0.01 (0.07)	[-0.24, 0.23]	-0.01 (0.07)	[-0.26, 0.23]	-0.11 (0.07)	[-0.36, 0.14]
Campaign contributions						
None (baseline)	-	-	-	-	-	-
$100	-0.01 (0.07)	[-0.25, 0.24]	0.03 (0.08)	[-0.22, 0.29]	0.01 (0.07)	[-0.22, 0.25]
$500	-0.04 (0.07)	[-0.28, 0.20]	0.02 (0.07)	[-0.22, 0.26]	0.05 (0.08)	[-0.20, 0.31]
$1000	0.10 (0.08)	[-0.15, 0.36]	0.00 (0.08)	[-0.26, 0.26]	0.01 (0.07)	[-0.24, 0.26]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the member's margin of victory attribute-level is 10% to 20% (second and third columns), 20% to 30% (fourth and fifth columns), or more than 30% (sixth and seventh columns) relative to when the attribute-level is less than 10% (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (64 comparisons—one comparison for each attribute-level across the three attribute-values for the margin of victory attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{64} = 0.00078125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00078125$.

Table A.25: Conjoint Experiment Attributes and Levels (Binary Choice, Constituency Strength-Conditional)

	Low		Moderate		High	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position						
Oppose (baseline)	-	-	-	-	-	-
Undeclared	-0.08 (0.08)	[-0.34, 0.17]	-0.03 (0.07)	[-0.27, 0.22]	-0.07 (0.07)	[-0.31, 0.17]
Support	-0.11 (0.07)	[-0.36, 0.14]	-0.16 (0.08)	[-0.42, 0.11]	-0.10 (0.07)	[-0.35, 0.15]
Cosponsor						
No (baseline)	-	-	-	-	-	-
Yes	0.01 (0.07)	[-0.22, 0.25]	0.06 (0.07)	[-0.18, 0.31]	-0.01 (0.07)	[-0.25, 0.23]
Party						
Republican (baseline)	-	-	-	-	-	-
Democrat	-0.06 (0.06)	[-0.27, 0.16]	-0.01 (0.07)	[-0.24, 0.22]	-0.03 (0.07)	[-0.25, 0.20]
Committee member						
No (baseline)	-	-	-	-	-	-
Yes	-0.04 (0.06)	[-0.25, 0.17]	-0.03 (0.07)	[-0.25, 0.19]	-0.10 (0.06)	[-0.31, 0.12]
Committee leadership						
No (baseline)	-	-	-	-	-	-
Subcommittee chair	0.01 (0.09)	[-0.29, 0.31]	0.14 (0.09)	[-0.17, 0.44]	0.07 (0.09)	[-0.22, 0.37]
Subcommittee ranking member	0.04 (0.09)	[-0.26, 0.35]	-0.05 (0.09)	[-0.37, 0.26]	-0.05 (0.09)	[-0.36, 0.26]
Seniority						
Freshman (baseline)	-	-	-	-	-	-
1 term	-0.01 (0.07)	[-0.25, 0.23]	-0.01 (0.07)	[-0.25, 0.23]	0.01 (0.07)	[-0.23, 0.26]
3 terms	-0.07 (0.07)	[-0.31, 0.17]	-0.08 (0.07)	[-0.33, 0.17]	-0.01 (0.07)	[-0.25, 0.23]
7 terms	0.02 (0.07)	[-0.21, 0.26]	0.06 (0.08)	[-0.20, 0.31]	0.14 (0.07)	[-0.10, 0.38]
Margin of victory						
Less than 10% (baseline)	-	-	-	-	-	-
10% to 20%	-0.01 (0.07)	[-0.25, 0.23]	-0.01 (0.07)	[-0.26, 0.24]	0.01 (0.07)	[-0.22, 0.25]
20% to 30%	-0.10 (0.08)	[-0.35, 0.16]	0.01 (0.07)	[-0.23, 0.25]	-0.02 (0.07)	[-0.26, 0.23]
More than 30%	-0.25 (0.08)	[-0.51, 0.02]	-0.07 (0.07)	[-0.32, 0.18]	-0.11 (0.07)	[-0.35, 0.14]
Constituency strength						
Very low (baseline)	-	-	-	-	-	-
Low	-	-	-	-	-	-

	Low		Moderate		High	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	-	-	-	-	-	-
High	-	-	-	-	-	-
Campaign contributions						
None (baseline)	-	-	-	-	-	-
$100	0.05 (0.07)	[-0.19, 0.29]	0.00 (0.07)	[-0.23, 0.23]	-0.03 (0.07)	[-0.28, 0.22]
$500	0.06 (0.08)	[-0.20, 0.32]	-0.08 (0.08)	[-0.33, 0.18]	-0.01 (0.07)	[-0.26, 0.23]
$1000	0.10 (0.07)	[-0.15, 0.35]	-0.06 (0.08)	[-0.32, 0.19]	-0.01 (0.07)	[-0.26, 0.24]

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the interest's strength in the member's constituency is low (second and third columns), moderate (fourth and fifth columns), or high (sixth and seventh columns) relative to when the attribute-level is very low (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (64 comparisons—one comparison for each attribute-level across the three attribute-values for the constituency strength attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{64} = 0.00078125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00078125$.

Table A.26: Conjoint Experiment Attributes and Levels (Binary Choice, Campaign Contributions-Conditional)

	$100		$500		$1000	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Position						
Oppose (baseline)	-	-	-	-	-	-
Undeclared	0.13 (0.08)	[-0.13, 0.38]	-0.01 (0.08)	[-0.27, 0.24]	-0.03 (0.08)	[-0.29, 0.24]
Support	-0.03 (0.08)	[-0.29, 0.24]	-0.02 (0.08)	[-0.29, 0.25]	0.08 (0.08)	[-0.20, 0.35]
Cosponsor						
No (baseline)	-	-	-	-	-	-
Yes	-0.03 (0.08)	[-0.28, 0.22]	-0.01 (0.08)	[-0.26, 0.25]	-0.06 (0.07)	[-0.31, 0.18]
Party						
Republican (baseline)	-	-	-	-	-	-
Democrat	0.16 (0.07)	[-0.06, 0.38]	0.08 (0.07)	[-0.13, 0.30]	0.06 (0.07)	[-0.16, 0.29]
Committee member						
No (baseline)	-	-	-	-	-	-
Yes	-0.13 (0.06)	[-0.34, 0.08]	-0.03 (0.06)	[-0.25, 0.18]	-0.04 (0.06)	[-0.25, 0.17]
Committee leadership						
No (baseline)	-	-	-	-	-	-
Subcommittee chair	0.01 (0.09)	[-0.30, 0.32]	0.04 (0.10)	[-0.28, 0.36]	0.09 (0.09)	[-0.23, 0.41]
Subcommittee ranking member	0.15 (0.09)	[-0.14, 0.44]	-0.04 (0.10)	[-0.37, 0.29]	0.03 (0.09)	[-0.27, 0.33]
Seniority						
Freshman (baseline)	-	-	-	-	-	-
1 term	-0.05 (0.07)	[-0.29, 0.18]	-0.05 (0.07)	[-0.30, 0.20]	0.09 (0.08)	[-0.16, 0.35]
3 terms	0.09 (0.08)	[-0.16, 0.35]	0.04 (0.08)	[-0.21, 0.30]	0.06 (0.07)	[-0.19, 0.32]
7 terms	0.00 (0.08)	[-0.26, 0.26]	0.00 (0.08)	[-0.25, 0.26]	0.03 (0.08)	[-0.22, 0.29]
Margin of victory						
Less than 10% (baseline)	-	-	-	-	-	-
10% to 20%	0.01 (0.07)	[-0.24, 0.25]	-0.05 (0.07)	[-0.30, 0.20]	0.11 (0.08)	[-0.14, 0.36]
20% to 30%	0.03 (0.08)	[-0.23, 0.28]	0.01 (0.07)	[-0.24, 0.26]	0.00 (0.08)	[-0.26, 0.26]
More than 30%	0.04 (0.07)	[-0.20, 0.27]	0.06 (0.08)	[-0.19, 0.32]	0.04 (0.07)	[-0.21, 0.29]
Constituency strength						
Very low (baseline)	-	-	-	-	-	-
Low	0.05 (0.07)	[-0.19, 0.29]	0.06 (0.08)	[-0.20, 0.32]	0.12 (0.07)	[-0.12, 0.36]

	$100		$500		$1000	
Attribute/Level	Estimate (SE)	95% CI	Estimate (SE)	95% CI	Estimate (SE)	95% CI
Moderate	0.00 (0.07)	[-0.23, 0.23]	-0.09 (0.08)	[-0.35, 0.17]	-0.06 (0.08)	[-0.32, 0.19]
High	-0.02 (0.07)	[]-0.27, 0.22]	-0.03 (0.07)	[-0.28, 0.22]	-0.02 (0.07)	[-0.27, 0.23]
Campaign contributions						
None (baseline)	-	-	-	-	-	-
$100	-	-	-	-	-	-
$500	-	-	-	-	-	-
$1000	-	-	-	-	-	-

Number of observations=2612 (666 unique respondents). This table presents the average component interaction effects (ACIEs), which indicate the effect of each of the congressperson attribute-levels included in the conjoint experiment tasks on the probability of a member of Congress' selection as a lobbying target when the interest's contributions to the member in the last electoral cycle were $100 (second and third columns), $500 (fourth and fifth columns), or $1000 (sixth and seventh columns) relative to when the interest did not make any campaign contributions to the member (i.e., the baseline). These ACIEs are offsets from the corresponding AMCEs presented in Figure 2 and Table A.6, such that they represent the differences in the probability of selection between the attribute-level indicated by the column heading and the baseline attribute-level. ACIEs are estimated using the `cjoint` package in `R`, which uses ordinary least squares regression to estimate the causal quantities of interest. Because the conjoint design includes some restrictions (see Tables A.1 and A.2), the ACIEs for these affected attribute-levels are obtained by obtaining their effects in each stratum of the linked attribute(s) and then calculating the average of those estimates; otherwise, the ACIEs presented are the linear regression coefficients for those attribute-levels (see Hainmueller, Hopkins, and Yamamoto 2014). To account for multiple comparisons (64 comparisons—one comparison for each attribute-level across the three attribute-values for the campaign contributions attribute), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals are constructed ($\alpha = \frac{0.05}{64} = 0.00078125$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^{*}p < 0.00078125$.

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