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 for direct contacts. 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.

Among the diverse array of tools organized interests use to pursue their goals in Congress, such as making campaign contributions (e.g., Hall and Wayman 1990), testifying before committees (e.g., Hansen 1991), and grassroots lobbying (e.g., Kollman 1998), 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 organized 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 (Miler 2010; Wright 1990), and allows interests to cultivate interpersonal relationships with members (Levine 2009; Schlozman and Tierney 1986). Though organized interests' influence over the policymaking process and policy outcomes is difficult to detect (see De Figueiredo and Richter 2014; Schlozman, Verba, and Brady 2012, 295-304), interests with access are generally thought to be better positioned to mobilize members to engage in lawmaking activities amenable to their preferences (e.g., Hall and Wayman 1990) and to achieve their preferred policy outcomes (e.g., Baumgartner et al. 2009).

In order to leverage access, organized interests must first 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 (e.g., Kalla and Broockman 2016), signaling expertise (e.g., Brodbeck, Harrigan, and Smith 2013), and leveraging network connections (e.g., Hirsch et al. n.d.). However, few studies consider the strategic considerations organized interests face in deciding which members to target for direct contacts. Further, those studies that examine these considerations offer an inconclusive set of findings concerning which members organized interests target, leaving unresolved key questions such as whether interests target supporters (e.g., Hall and Deardorff 2006) or opponents and undecideds (e.g., Austen-Smith

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[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, as is common in previous studies of targeting strategies (e.g., Hojnacki and Kimball 1998; You n.d.).

[2]Among the organized interests who participated 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 the relevant committees (see also Schlozman and Tierney 1986). Further, organized interest representatives often report in qualitative interviews that direct contacts with legislators are among the most important and effective tactics with which they can advance their goals (Drutman 2015; Levine 2009).

and Wright 1994), members with influential institutional roles (e.g., Hojnacki and Kimball 1998), and members whose electoral environments make them susceptible to lobbying (e.g., Hansen 1991; Wright 1990). 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 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 results across studies. For example, organized interest representatives' retrospective self-reports of which members of Congress they target are susceptible to social desirability bias; respondents may exaggerate the degree to which they target powerful members so as to appear influential or under-report their targeting of members who are disliked by their interest's membership, such as legislators who oppose the interest's policy preferences. Additionally, in drawing on observational data, these studies are limited in their ability to account for unobserved confounders and to isolate the independent effects of variables which are highly correlated in the real world, such as members' seniority and their possession of leadership roles.

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 organized interests' targeting strategies—members' expressed preferences, institutional roles, and electoral concerns.[4]

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[3]I focus on the context of live legislative proposals (i.e., bills that have already 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 (e.g., Austen-Smith and Wright 1994; Heberlig 2005; Hojnacki and Kimball 1998, 1999, 2001; Rothenberg 1992; You n.d.). However, organized interests also leverage direct contacts to influence other types of legislative activity, such as the preceding policy formulation process (e.g., Wright 1996) and the conduct of oversight (e.g., Hall and Miler 2008). In the Conclusion, I discuss how organized interests' targeting strategies may differ when they seek to influence other such activities.

[4]Though not explicitly examined in extant studies of organized interests' targeting strategies, relationships with policymakers are also posited to inform interests' access-seeking behavior (e.g., Hirsch et al. n.d.). I do not incorporate this consideration in the experiment because interpersonal relationships are difficult to manipulate in externally valid ways. Whereas organized interest representatives are routinely presented with a multitude of members whose characteristics vary independent of the representatives themselves, their interpersonal relationships are relatively fixed and tied to their lived experiences. Thus, asking respondents to imagine interpersonal relationships is less realistic than asking them to evaluate composite profiles of members of Congress. This design choice does not threaten the validity of the results because the experiment isolates the causal effect of the considerations I include independent of other unobserved factors. I further discuss this limitation in the Conclusion.

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, these representatives are asked to assess members' multidimensional profiles and select those with the highest expected value of direct contact. Consequently, we can use these representatives' choices to evaluate the relative effects of members' characteristics on organized 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 common forms of response bias, such as social desirability or mistaken recollections.

The results indicate that organized interests seeking to advance a legislative proposal make targeting decisions 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 heightened 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 previously donated to their campaigns.

These results illuminate 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 the fate of proposals making their way through the legislative process by changing the underlying distribution and intensity of preferences (e.g., Austen-Smith and Wright 1994) rather than reinforcing the pre-existing biases of their allies (e.g.,

Hall and Deardorff 2006). Given that organized interests tend to overrepresent the interests of business and the upper-class, this finding implies that these privileged segments of society use direct contacts not to merely impart their "political voice" to allies, but to expand the size of their coalitions and alter policy outcomes relative to those preferred by society at large (Gilens and Page 2014; Schlozman, Verba, and Brady 2012). Second, organized interests' attention to members' institutional roles indicates that they recognize and seek to leverage 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, often out of public view (Fouirnaies 2018; Fouirnaies and Hall 2018; Powell and Grimmer 2016).

This paper proceeds as follows. In the next section, I provide an overview of the three clusters of member characteristics posited by previous studies to inform organized interests' targeting strategies and discuss the inferential challenges faced in these studies. In the third section, I describe the design of the conjoint experiment that assesses the effect of nine different member characteristics drawn from those three clusters on organized interests' targeting choices, and in the fourth section I present the results. In the final section, I conclude by situating my findings in the literatures on organized interests and Congress, discussing the limitations of my empirical approach, and suggesting avenues for future research.

The Strategic Targeting Choices of Organized Interests

While resources are distributed unevenly within 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 policy 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 efforts (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 (e.g., Kollman 1998), it lies at the core of nearly all studies of interests' targeting of members for direct contacts. Proponents of the theory that organized interests pursue direct contacts with supporters argue that interests 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 on their behalf, and like-minded members are more receptive audiences to interests' entreaties for action than are opponents or undecided members (Bauer, Pool, and Dexter 1963; Hall and Deardorff 2006; Levine 2009). Convincing an opponent or undecided member to act on an interest's behalf 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. 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 (Bauer, Pool, and Dexter 1963; 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 use direct contacts to expand the size of their supportive coalitions in Congress. 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 organized interests enlarge their coalitions in two ways. First, each opponent or undecided they enlist increases the size of their coalition by one member. Second, opponents and undecideds that an organized interest converts can more credibly lobby their colleagues than can members who initially shared the interest's preferences, thereby helping the interest further augment their coalition (Awad 2020; Ellis and Groll 2020; Groll and Prummer n.d.). 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 (Austen-Smith and Wright 1994; Awad 2020; Ellis and Groll 2020; Groll and Prummer n.d.; Heberlig 2005; Rothenberg 1992).

Institutional Roles

While all members retain a vote of equal weight when a proposal faces a roll call on the floor, some members wield additional influence over the proposal by nature of their institutional roles, making them valuable targets for organized interests seeking direct contacts. Members' augmented influence can manifest in two different ways. First, some members hold institutional roles that provide them with more formal procedural control over a proposal than other members. For example, given the party-centric institutional structure of the modern Congress (Aldrich and Rohde 2001; Cox and McCubbins 2005; Sinclair 2006), majority party support for a proposal is a virtual prerequisite for its 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 of the substantive work on a proposal takes

6

place in committee, and a proposal rarely advances to the floor without a favorable recommendation from the committee of jurisdiction (Deering and Smith 1997). Additionally, congressional leaders, particularly those affiliated with the majority party, possess unique leverage over a proposal's fate. Chamber leaders, such as the Speaker in the House context, wield several procedural powers that can affect a proposal's progress, including appointment of Rules Committee members and referral of 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 and conducting negotiations while the proposal is before the committee, and often serving as floor managers when the proposal is considered by the full chamber (Deering and Smith 1997; Oleszek 2014).

Second, some institutional roles enable members to informally influence their colleagues. Much of this informal influence operates through cue-giving; among the field of forces informing members' decisions is the information provided by and preferences of their colleagues, informs their own behavior (Kingdon 1973). More senior members are often looked to as cue-givers, as their political and procedural experience suggests that their inclinations on a proposal likely 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 best course of action for the party 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 (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; Wright 1996). While this information is valuable to all members given intrinsic electoral uncertainty, it is particularly valuable to members from marginal districts who are sensitive to this uncertainty because they have relatively little margin 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; Kollman 1998), 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 and continue to receive campaign contributions, and organized interests who engage in campaign finance use contributions as tools to attain access to and induce the responsiveness of members of Congress (Miler 2010; Powell and Grimmer 2016; Wright 1996). 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 previously 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) find that organized interests are more likely to seek direct contacts with members who share their preferences on a given proposal (see also Hojnacki and Kimball 1999, 2001), Austen-Smith and Wright (1994), Heberlig (2005), and Rothenberg (1992)

conclude that organized interests seek direct 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 organized 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), and Kalla and Broockman (2016) demonstrate that members provide higher levels of access to interests active in campaign finance, 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.

The contexts utilized in the above-cited studies differ in several ways, such as their temporal scope and the focal type of legislative activity, which may account for some differences in the conclusions they draw.[5] However, each of these studies is motivated by the same broad theoretical question—which types of members of Congress organized interests target for direct contacts—and share at least two common design features that may underlie the inconclusiveness of their collective 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 con-

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[5]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.) or participate in policymaking in broad issue areas (Hojnacki and Kimball 1998), and different types of members may be more attractive targets for organized interests across each of these policy goals. Again, these studies cover a wide temporal range, spanning from as early as the 1950s (Heberlig 2005) to as recently as the 2000s (You n.d.), and changes in congressional institutions and polarization over this period may condition interests' targeting choices across studies. While these contextual differences are of theoretical interest, I focus here on these studies' common design features which pose challenges to inference and encourage researchers to explore such contextual effects with designs that address these challenges in future work.

founders, an ever-present concern in observational analyses; should any omitted variable, such as a member's personal expertise or interest in an issue, 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, which makes it difficult to isolate independent causal effects in naturally occurring data.

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; the consequent random error would, in aggregate, attenuate parameter 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 members and donors in the event that their responses become public (Li 2018). The resulting non-random error would bias parameter estimates, in the first case overestimating organized interests' targeting of congressional leaders, and in the second case underestimating their targeting of opponents. Because studies of targeting behavior using survey data 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 whose answers are affected by random or non-random response bias 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 be attributable to differences in legislative context (see Leech 2010). Once a legislative proposal is introduced in Congress, it must pass through two general stages within its chamber of origin: the committee stage and the floor stage. 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; Groll and

Prummer n.d.; 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 those members whose support is most critical at the present stage. A few intuitive 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 for organized interests (Hojnacki and Kimball 1998; Powell and Grimmer 2016). 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). At this stage, organized interests can expand the strength of their coalition by persuading members who are undecided or opposed to their position (Austen-Smith and Wright 1994; Awad 2020; Ellis and Groll 2020; Groll and Prummer n.d.; Heberlig 2005) or enlisting into their coalition members who exert procedural control at the floor stage, such as chamber and committee leaders and majority party members (Hall and Deardorff 2006; Hojnacki and Kimball 1998; Levine 2009).

Recognizing that a proposal's stage can condition organized interests' targeting behavior may clarify some discordant findings in extant empirical studies. For example, 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 then shift attention to opponents and undecideds once the proposal moves to the floor and success depends on votes (see Hall and Deardorff 2006, 78-80). 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 (Hojnacki and Kimball 1998, 1999, 2001; Wright 1990) 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 identified above, I conduct a conjoint experiment with a sample of organized interest representatives, such as federal lobbyists and policy advocates, which offers several inferential advantages for examining lobbying targeting strategy. 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 of interest. Second, by using a conjoint experiment to observe organized interest representatives' behavior, I can collect data on the phenomenon of interest while mitigating the problem of 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 choices differ across stages of the legislative process.

Further, the choices of both sample and experimental design promote external validity, which is a key concern when using survey experiments to study real-world behavior (Gaines, Kuklinski, and Quirk 2007). 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 (e.g., Bauer, Pool, and Dexter 1963; Levine 2009), 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 these representatives make targeting decisions in the real world, they do so by starting with a set of potential targets (e.g., all 535 members of Congress, members of a single chamber or committee), and winnowing that set down to a subset of members that collectively offers the highest expected value from targeting those members for direct contacts, subject to the interest's resource constraints. In that winnowing process, these representatives make choices by comparing the multidimensional profile of each member, which includes attributes such as the member's position on the proposal of interest and

12

committee membership, to those of the other members under consideration.[6] Similarly, in a conjoint framework, respondents are presented with a choice set of multidimensional profiles and asked to evaluate each profile and/or select the profile they most prefer. Thus, while this method of eliciting organized interest representatives' targeting preferences differs from those used in previous studies, which asked representatives to indicate the members of Congress from which they sought direct contacts or leveraged official records of direct contacts, it captures the microfoundational behavior—comparisons among members under consideration—embedded in these higher-order activities. Therefore, this conjoint experiment conducted with organized interest representatives simulates the real-world choice task of interest with the very political actors who perform this task.

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 reports filed between the first quarter of 2017 and the third quarter of 2018 in compliance with the Lobbying Disclosure Act of 1995 (LDA) and subsequent amendments.[7] The LDA 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 all individuals who had not yet completed the survey on November 29, 2018 and December 13, 2018.[8] Data collection ceased on December 31, 2018, at which point 670 points of contact participated in the experiment; excluding the points of contact whose email addresses were identified as invalid when initial invitations were sent, the overall response rate for the experiment is 12.3% ($\frac{670}{5458}$).[9]

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[6]The interview transcripts in Leech (2014) illustrate this decisionmaking 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 direct contacts.

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

[8]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 in anticipation of the new Congress, organized interest representatives have fewer work commitments and are thus more likely to complete the survey. Second, the experiment minimizes concerns about pretreatment 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.

[9]This response rate compares favorably with those obtained in surveys of the mass public and in recently published survey experiments of American political elites. See Appendix section "Sampling Procedure and Descriptive

Information about the descriptive characteristics of the sampling frame and the respondents are presented in Appendix Tables A.4 and A.5.

Experimental Design

To simulate the choice tasks in which organized interest representatives engage when making targeting decisions, I implement 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 each of a set of attributes, and each profile is randomly assigned a level for each attribute. Given this randomization, researchers can estimate the effect of each attribute-level on respondents' evaluations or choices. Conjoint experiments provide increased 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 conceal in a survey or simpler experimental framework (Hainmueller, Hopkins, and Yamamoto 2014; Horiuchi, Markovich, and Yamamoto n.d.).[10]

After providing consent and completing pre-treatment questions, respondents are presented with two consecutive conjoint tasks. In each task, respondents are asked to imagine that 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 that the organized 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

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Statistics" for details.

[10]Mitigating social desirability bias is important for the present context because respondents may be reticent to reveal that their lobbying 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 not report this tendency in a survey or a survey experiment with few randomized attributes because it would give the impression that they "buy" access. However, respondents would be more likely to reveal a preference for targeting to members to whom their interests donate (if this preference exists) in a conjoint framework because the large number of attributes both gives the impression that such socially undesirable choices cannot be detected and allows them to draw on other attributes to rationalize socially undesirable choices.

in meeting with each member on a five-point scale and to select the member with which 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 described above that extant studies posit organized interests use when constructing their targeting strategies.[11] 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.[12] 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 in addition to the profile attribute-levels. First, in order to encourage respondents to consider each task independently and to 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

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[11]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 identified as 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 identified as members of the committee and as a Democrat or a Republican, respectively.

[12]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 past studies of lobbying targeting (e.g., Hojnacki and Kimball 1998) and have more influence over a proposal as it progresses through the legislative process, 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 requirement is often not troublesome, it can yield implausible scenarios if both profiles are assigned an attribute-level which only one profile can possess—such as holding the chair of a full House committee. In light of this challenge, I use subcommittee leadership positions as attribute-levels, as each House committee has multiple subcommittees for which there are 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. At worst, the choice to use subcommittee rather than committee leadership positions should bias downward estimates of the effect of a member holding a leadership position on organized interests' interest in or probability of targeting the member, such that the effect of a member's possession of a full committee or chamber leadership role would be greater than the effect of holding a leadership position recovered here.

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	O	O	O	O	O
Member 2	O	O	O	O	O

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.**

ordering of issue contexts randomized across respondents.[13] Second, in order 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.

Empirical Analysis

Estimation Strategy

For both the ordinal and binary outcomes, the estimation strategy outlined by Hainmueller, Hopkins, and Yamamoto (2014) is used. The causal quantity of interest is the average marginal component effect (AMCE), which is 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 the legislative stage in which the task is situated, the causal quantity recovered is the average component interaction effect (ACIE), which is 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 ordinary least squares regression using the `cjoint` package in R. To account for non-independence across respondents, the results presented below 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—Bonferroni corrections are implemented to account for multiple comparisons.[14]

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[13]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 situated in the same issue context, 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 member of the committee but has other desirable characteristics, such as majority party membership. By offering each respondent tasks situated in unique issue contexts, the design encourages them to consider each task separately.

[14]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).

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.[15] Overall, respondents' targeting choices are influenced by the attribute-levels associated with members' expressed preferences and institutional roles. With respect to members' expressed preferences, respondents are most likely to express interest in meeting with members who have not yet declared a position on the proposal; as compared to members who have announced opposition to or support of the proposal, respondents are 19 and 22 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,[16] relative to members who merely expressed support for the proposal but did not exert a higher level of effort through cosponsorship.[17] 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

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[15]Due to space constraints, only results using respondents' choice of which member they would prefer to meet are presented here, and the results for respondents' ordinal measures of interest in meeting with each member are presented in 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 respective baselines when using the binary outcome, but not when using the ordinal outcome. In Appendix section "Empirical Results," I discuss these differences and explain why the binary outcome more appropriately characterizes the targeting decisions organized interest representatives make.

[16]Members' preferences can be gauged not only by their issue positions, but also by 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 cosponsoring a proposal or making a floor speech, are seen as stronger supporters or opponents (Hall 1996).

[17]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.
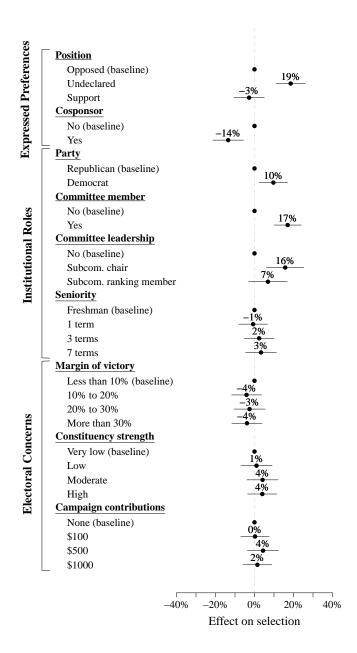
Figure 2: **Effect of Legislator Attributes on Targeting Choices.** Points represent average marginal component effects (AMCEs) for each attribute-level (relative to the baseline attribute-level) on the probability of selection, and bars around point estimates represent Bonferroni-adjusted 95% confidence 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.

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 than do most other members (Hojnacki and Kimball 1998). Among committee members, subcommittee chairs are particularly attractive lobbying targets, as respondents are 16 percentage points more likely to select a subcommittee chair relative to a rank-and-file majority party committee member.[18] However, unlike the other attributes indicative of a member's institutional role, respondents are no more likely to target more senior members.

In contrast to the attributes related to members' expressed preferences and institutional roles, none of the attributes respecting members' electoral concerns—the member's margin of victory in the previous election, the number of residents in the member's district who could be directly affected by the proposal, or the amount of money the organized interest's PAC previously donated to the member—manifested a distinguishable effect on respondents' targeting decisions. This lack of evidence contrasts with theoretical 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, 1999, 2001), as well as with studies of lobbying, and of money in politics more broadly, which suggest that organized interests make campaign contributions in expectation that recipients will provide them with access (e.g., Ansolabehere, De Figueiredo, and Snyder 2003; Kalla and Broockman 2016; Powell and Grimmer 2016).

To assess whether the effect of members' attribute-levels on organized interests' targeting choices varies 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 (Austen-Smith and Wright 1994) rather than working with legislative allies

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[18]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 (see Footnote 17).
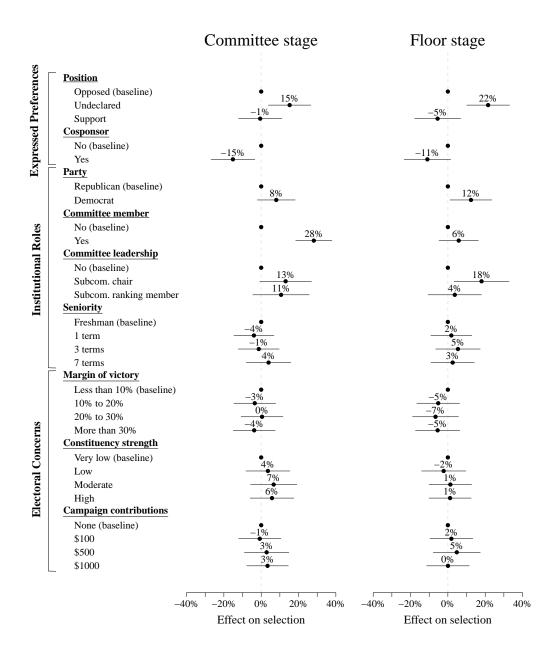
Figure 3: **Effect of Legislator Attributes on Targeting Choices Conditioned by Stage.** Points represent average component interaction effects (ACIEs) for each attribute-level (relative to the baseline attribute-level) on the probability of selection as conditioned by the stage of the legislative process, 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.

(Hall and Deardorff 2006).[19] Subcommittee chairs remain attractive targets in both stages, with respondents more likely to target members with this role by 13 and 18 percentage points across the committee and floor stages, respectively.[20] Finally, as in the pooled analysis, there is no evidence that respondents' targeting decisions are influenced by members' seniority or electoral concerns.

However, the effects of three attribute-levels differ in substantively meaningful ways when conditioned by legislative stage.[21] First, among legislative supporters, organized interest representatives 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 contact majority party Democrats rather than minority party Republicans 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 substantive 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 either 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 advances out of committee and its fate rests in the hands of the full chamber membership, committee members become less valuable targets. Taken

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[19]Even when a proposal is at the committee stage, expanding the size of its supportive coalition in the chamber at large can enhance its chances of success by signaling the chamber membership's interest in the proposal reaching the floor to the committee and the chamber leaders (e.g., Box-Steffensmeier, Christenson, and Craig 2019).

[20]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.

[21]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, but the ACIEs for some attribute-levels are distinguishable from zero in one stage but not the other.

together, these conditional effects highlight the importance of considering how lobbying activities evolve throughout 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 respondents in the survey 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 the results presented in Figures 2 and 3. 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 presented in Figure 2.

Third, I consider whether the overall treatment effects observed above are driven by heterogeneous treatment effects tied to respondents' personal characteristics. For example, because lobbyists who have 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 (Salisbury et al. 1989), they may respond to the conjoint tasks differently than lobbyists without government experience. While the empirical expectations drawn from previous studies of lobbying targeting and outlined above do not posit heterogeneous effects driven by respondent characteristics, identifying such effects, if present, provide a more nuanced understanding of how organized interest representatives' personal characteristics condition their targeting behavior. Additionally, though presence of heterogeneous effects would not pose a threat to the experiment's internal validity, it may 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). In the Appendix, I estimate additional models

that condition attribute-level effects by three respondent characteristics that could plausibly induce heterogeneous effects—whether the respondent is employed by their client directly or by a firm, the respondent's partisan affiliation, and whether the respondent has previous government experience. The results from these models, presented in Tables A.13, A.14, and A.15, respectively, are again substantively similar to those presented in Figure 2. Thus, I do not find evidence of heterogeneous treatment effects which would circumscribe the generalizability of 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 expectations and findings in previous studies. In considering members' expressed preferences, the finding that organized interests target members who are undecided on or weak supporters of a proposal suggests that, when trying to advance a live legislative proposal, organized interests target members whose preferences or levels of effort they perceive as malleable in order to strengthen their supportive coalition (Austen-Smith and Wright 1994; Heberlig 2005; Rothenberg 1992) rather than members who already share and actively promote their preferences (Hojnacki and Kimball 1998, 1999, 2001; Levine 2009). 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 (Fouirnaies 2018; Fouirnaies and Hall 2018; Powell and Grimmer 2016), 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 (e.g., Kollman 1998) and money in politics (e.g., Ansolabehere, De Figueiredo, and Snyder 2003) literatures, the results fail to provide evidence that organized interests target members on the basis of members' electoral concerns.

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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 warrants extended discussion, as it 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).[22] This null finding should encourage us to think carefully about the mechanisms underlying organized interests' use of contributions to obtain access. One important consideration in theorizing about the linkage between contributions and access is the temporal ordering of these actions. 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, organized 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 (Fouirnaies 2018; Fouirnaies and Hall 2018; Powell and Grimmer 2016), 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 that manage to discern the relative timing of contributions and access can clarify our understanding of the mechanism underlying this relationship.

Like all experiments, this study has limitations that should motivate future research. First, while the rich information environment presented in the conjoint experiment includes many important considerations that extant studies posited influence organized interests' targeting behavior, it does not include all potential considerations. For instance, the experiment does not account for respondents' interpersonal relationships with the members between which they are asked to choose. While the experiment omits these dynamics because it is difficult to manipulate respondents' lived experiences in externally valid ways, they may also inform organized interests' targeting choices. As an example, an organized interest that employs someone that used to work for a member of Congress might have more information about the member's anticipated reaction to their lobbying

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[22]As discussed previously, the conjoint framework diminishes social desirability bias (Hainmueller, Hopkins, and Yamamoto 2014; Horiuchi, Markovich, and Yamamoto n.d.), making it unlikely that this null finding stems from respondents' reticence to select member profiles to which the organized interest featured in the choice task previously made a campaign contribution.

efforts or might expect that the member is likely to fulfill their requests for access because of this existing relationship (Hirsch et al. n.d.; McCrain 2018; Vidal, Draca, and Fons-Rosen 2012). The absence of these considerations does not undercut the inferences gleaned by the present study, as the experiment isolates the causal effects of the featured member characteristics on organized interests' targeting choices independent of other considerations.[23] However, the exclusion of these relationships and other plausibly relevant considerations limits the extent to which the experiment can replicate the decision-making environment in which organized interests operate. Future studies should draw on other empirical approaches, such as rare observational records of targeting and access (Hirsch et al. n.d.) or field experiments (Brodbeck, Harrigan, and Smith 2013; Kalla and Broockman 2016), to explore how organized interests use considerations that are difficult to account for in a conjoint experiment, such as interpersonal relationships, to inform their targeting choices.

A second limitation of this study is that it considers only one type of legislative activity in which organized interests engage—trying to advance a live legislative proposal. While this activity is the focus of most extant studies of organized interests' targeting behavior (Austen-Smith and Wright 1994; Heberlig 2005; Hojnacki and Kimball 1998, 1999, 2001), the unique institutional contours of other types of legislative activity may condition the extent to which certain member characteristics inform targeting choices. For example, whereas this study focuses on bills that would change federal law, which are 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 procedural rules and norms that may prompt interests to adopt different targeting strategies (Austen-Smith and Wright 1994). Differently, if organized interests seek to block, rather than advance, a proposal, they may focus their targeting efforts on a key agenda setter who can stymie the proposal on her own rather than construct a supportive coalition of minimum winning size to halt its advancement (Rothenberg 1992). Alternatively, when organized interests' goals require members to commit substantial legislative effort, such as introducing amendments, performing oversight of the bureaucracy, or making floor speeches to

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[23]In this sense, readers might interpret the results of the conjoint experiment as if other considerations are held constant within each choice task. For example, focusing on the role of interpersonal relationships, readers can interpret the effects identified as if all respondents had similarly positive or negative previous experiences with the members featured in the profiles.

increase attention to their issues, they may prefer to target supporters rather than undecideds because they can more efficiently subsidize the effort of 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 easily adapted to examine targeting strategies across other legislative activities.

Third, whereas this study sheds light on on organized interests' targeting strategies, or their demand for direct contacts, it does not consider members of Congress' responses to these targeting attempts, or their supply of direct contacts. This focus on organized interests' demand for contacts may limit the generalizability of the findings presented here if organized interests' targeting attempts are informed by their expectations of success, such that they target members likely to accede to their requests for in-person meetings and use the outcomes of their requests to update those expectations for future attempts.[24] More broadly, because access is the confluence of organized interests' demand for and members' supply of direct contacts, understanding how access manifests requires an appreciation of both supply and demand, and the present study contributes only to our understanding of the latter concept. A small number of studies explores members' supply of access, though they often focus on only one or a few considerations at a time, such as the requester's campaign finance activity, expertise, and status as a constituent (Brodbeck, Harrigan, and Smith 2013; Chin 2005; Chin, Bond, and Geva 2000; Kalla and Broockman 2016). Alternatively, Heberlig (2005) and You (n.d.) leverage rare information about realized access between organized interests and members of Congress, but their results can only offer insights about the patterns of access that manifest rather than the underlying dynamics of access-seeking and access-granting behavior. By integrating supply and demand, future work can illuminate our comprehension of how the motivations and actions of organized interests and members of Congress interact to yield access.

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[24]As with the exclusion of other member characteristics, readers could interpret the preceding results as if respondents expected that the members featured in each choice task were equally likely to provide access if requested (see Footnote 23).

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Appendix

Table of Contents

List of Tables

List of Figures

Conjoint Experiment Description & Question Wordings

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. One task focuses on a bill to regulate an internet sales tax at the federal level, and the other task focuses on a bill to enable the Centers for Medicare & Medicaid Services to negotiate prescription drug prices.[1] Both of these tasks are constructed as stylized versions of real-world proposals introduced in the 115th Congress.[2] 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.[3] Finally, to

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[1] 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. These bills were selected because their partisan implications are not clear-cut, especially when presented 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), potentially confusing respondents. In contrast, the broad goal of both bills—allowing states to collect sales tax on internet sales and lowering the price of prescription drugs—could be plausibly supported by members of either. Further, previous iterations of both bills have received support from both Democrats and Republicans, substantiating the notion that their partisan implications are ambiguous. For instance, the Remote Transactions Parity Act of 2017 was introduced by a Republican and had a bipartisan coalition of cosponsors (24 Democrats and 26 Republicans). While the members supporting the Medicare Prescription Drug Price Negotiation Act of 2017 were more decidedly Democratic (e.g., only 1 of the 45 cosponsors was a Republican), the same bill had Republican cosponsors in past congresses (e.g., 2 Republicans in the 112th Congress) and received 6 Republican votes when the Senate failed to invoke cloture on it in 2007.

[2] 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. For more on concerns about external validity and pre-treatment, see Gaines, Kuklinski, and Quirk (2007).

[3] 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.

allow for stage-conditional effects, the stage at which the proposal is situated—either the committee stage or the floor stage—is independently randomized across choice tasks and specified within the vignette.

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.

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[4] 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).

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.[5] In the latter case, because no benchmark for response rates for survey experiments fielded with samples of federal lobbyists and policy advocates exists,

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[4]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.

[5]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.

I compare my response rate to that obtained in other studies conducting survey experiments with American political elites[6] published in the *American Political Science Review*, *American Journal of Political Science*, and *Journal of Politics* between January 1, 2010 and April 23, 2020.[7] 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. 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.[8] 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).

Finally, Table A.5 provides information on the descriptive characteristics of the individuals 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%), and experience levels of "more than 20 years" (41.1%) suggest that most survey respondents were themselves political elites who play a substantive role in lobbying, rather than low-level employees who may respond to emails but lack significant lobbying experience.

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[6]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.

[7]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.

[8]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.

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   **b. Telephone Number**   **c. E-mail**   **5. Senate ID#**
Ms.   Susan Molinari   2023461346   smolinari@google.com   320510-12

7. Client Name   ☑ *Self*   ☐ *Check if client is a state or local government or instrumentality*   **6. House ID#**
Google Inc.   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	**13. Organizations**
INCOME relating to lobbying activities for this reporting period was:	EXPENSE relating to lobbying activities for this reporting period were:
Less than $5,000 ☐	Less than $5,000 ☐
$5,000 or more ☐ $ _____	$5,000 or more ☑ $ 5,020,000.00
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).	**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.

A.11

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).[9] 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.[10] 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, I reestimate both models presented in the main text with survey weights to account for differences in the distributions of the four observable characteristics available for the full sampling frame between individuals in the sampling frame and survey respondents. 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 obtain statistical distinguishability.

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[9]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%).

[10]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.

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 lobbyists working for their client directly, 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). 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., Salisbury et al. 1989).

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). As a result of the known and unknown unrepresentativeness of the sample, if heterogeneous treatment effects exist for characteristics on which the sample is unbalanced, then the overall treatment effects obtained from the experiment may be biased (i.e., the sample average treatment effect may differ from the population average treatment effect) (e.g., Coppock 2019). For example, given that the majority of respondents identify as Democrats or lean Democratic (52.9%) and that the experimental results indicate that respondents are more likely to target Democratic congresspersons, we might be concerned that the overall treatment effect is driven by respondents' homophily (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 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) 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 point estimates of the conditional effects for the attributes of interest typically remain in the same direction and are of comparable magnitude across different levels of each respondent characteristic. 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).

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 significance 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)

Attribute/Level	Committee Stage		Floor Stage	
	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 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]

A.24

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 significance 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)

Attribute/Level	Committee Stage		Floor Stage	
	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]

Attribute/Level	Committee Stage		Floor Stage	
	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 each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests and to construct 95% confidence intervals and the non-baseline levels for the "Position" attribute to each other), a Bonferroni correction is are constructed ($\alpha = \frac{0.05}{20} = 0.0025$). Null hypothesis significance 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)

Attribute/Level	Committee Stage		Floor Stage	
	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 each other), a Bonferroni correction is implemented to conduct null hypothesis significance tests 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)

Attribute/Level	Internet Sales Tax		Prescription Drugs	
	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]

Attribute/Level	Internet Sales Tax		Prescription Drugs	
	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)

Attribute/Level	In-House		Firm	
	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]

Attribute/Level	In-House		Firm	
	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)

Attribute/Level	Democrat		Independent		Republican	
	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]

Attribute/Level	Democrat		Independent		Republican	
	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 ($\alpha = \frac{0.05}{60} = 0.00083$). Null hypothesis significance tests and 95% confidence intervals utilize cluster robust standard errors (clustered on respondent). $^*p < 0.00083$.

A.37

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

Attribute/Level	Previous Experience		No Previous Experience	
	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]

Attribute/Level	Previous Experience		No Previous Experience	
	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).
$^*p < 0.00125$.

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