

Field experiment evidence of substantive, attributional, and behavioral persuasion by members of Congress in online town halls

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Do leaders persuade? Social scientists have long studied the relationship between elite behavior and mass opinion. However, there is surprisingly little evidence regarding direct persuasion by leaders. Here we show that political leaders can persuade their constituents directly on three dimensions: substantive attitudes regarding policy issues, attributions regarding the leaders' qualities, and subsequent voting behavior. We ran two randomized controlled field experiments testing the causal effects of directly interacting with a sitting politician. Our experiments consist of 20 online town hall meetings with members of Congress conducted in 2006 and 2008. Study 1 examined 19 small meetings with members of the House of Representatives (average 20 participants per town hall). Study 2 examined a large (175 participants) town hall with a senator. In both experiments we find that participating has significant and substantively important causal effects on all three dimensions of persuasion but no such effects on issues that were not discussed extensively in the sessions. Further, persuasion was not driven solely by changes in copartisans' attitudes; the effects were consistent across groups.

persuasion | field experiment | political science | immigration | terrorism

As thinkers ranging from Aristotle (1) to our own day have argued, persuasion—a change in the attitude or behavior of an individual caused by an appeal from a political elite—is integral to leadership. Although the question of persuasion by leaders is relevant to almost every form of collective human behavior over time and place, there is remarkably little evidence that unmediated, interpersonal appeals from specific leaders affect the attitudes or behavior of individuals. In contrast, the link between indirect, mediated persuasion and mass opinion has been studied intensively (2, 3), with one long-standing literature arguing that elites generally have a minimal impact on mass opinion (4). Others argue that the causal arrow is actually reversed—that political elites are particularly adept at calibrating their statements and political aims in response to their followers' opinions (5, 6).

One likely reason for this impasse is that virtually all of the evidence we might use to measure the effects of persuasive appeals by leaders is either indirect and observational, or based on laboratory experiments that only simulate a few features of real elite–mass interactions. Political scientists and psychologists have accumulated aggregate-level evidence of elite persuasion by studying mass media messages and advertising (7, 8), large-*N* surveys (2, 5, 9–14), and laboratory experiments with hypothetical elite–mass interactions (15, 16). However, there is little publicly available evidence that speaks directly to whether and how individual elites directly persuade their individual constituents. Although the contours of such persuasion may mirror those in the aggregate, and hypothetical scenarios offered in laboratory settings might yield dynamics similar to those in real political interactions, there is reason to think that they do not reliably track each other (17, 18). For example, Neblo et al. (19) demonstrate a sharp reversal in attitudes and behavior regarding government and political participation.

Interestingly, as well documented in *The Victory Lab* (20), there has been a proliferation of proprietary field experimental research in election campaigns testing candidates' ability to persuade and mobilize voters—for example, Rick Perry, in his reelection campaign in 2010, was randomly assigned to certain campaign stops to evaluate impact on voter turnout (20). In contrast, we are aware of no publicly available evidence that appeals from specific political elites affect the attitudes or behaviors of specific citizens, despite prominent accounts of representative democracy hinging on persuasion in such relationships. The absence of such evidence is all the more striking in that elites frequently do appeal directly to their constituents. Town halls, stump speeches, and personal contact between individual elites and members of the public are standard features of modern politics (21, 22).

We identify three dimensions of persuasion: substantive, attributional, and behavioral. Substantive persuasion involves changes in attitudes about an issue. Attributional persuasion involves changes in attitudes about the leader. Behavioral persuasion involves changes in political participation.

Substantive persuasion has been the focus in the existing literature (23) and is important because it affects support for particular decisions and may lead to future behavioral changes. However, most elected officials' primary communication goals focus on presentation of self (21, 22), that is, persuading constituents regarding their personal qualities (e.g., being trustworthy or competent). This emphasis on attributional persuasion is necessary for effective leadership, because positive attributions mean that

Significance

Persuasion is at the core of leadership, especially in a democracy, yet there is remarkably little evidence that direct appeals from leaders causally affect the attitudes and behaviors of their followers. The available evidence is either observational and indirect or based on laboratory experiments that simulate selected features of interactions with leaders. We fill this void with two randomized controlled field experiments in which 12 US representatives and a senator each met with samples of their constituents in online town halls. We find evidence of substantial persuasion effects on specific policy issues, attributions of trust and approval, and ultimately the decision to vote for the leader.

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ambiguous events will be interpreted to the leader's benefit, facilitating survival and providing some freedom for movement politically. Behavioral persuasion is necessary for leaders to stay in power—to mobilize voting in reelection. There is an enormous literature on political behavior (e.g., the correlates of voting behavior and participation more generally) (24), the role that elections play in mobilizing or demobilizing voters, and the role that networks play in mobilizing other forms of political action (25, 26). However, relatively little has been written on the behavioral effects of direct appeals from elites.

Oftentimes, on topics such as immigration and terrorism, democratic politics centers on questions of good and bad, or right and wrong, and uses of governmental power and hence cannot be solved by coordination among constituents who share common interests (27). In these situations, leaders must use direct appeals to persuade their constituents. Following Aristotle, we identify three mechanisms that leaders rely on: providing good reasons (*logos*), invocation of authority (*ethos*), and activating emotions (*pathos*). These kinds of activities affect listeners on a dyadic basis, with the action of the leader directly altering the attitude or behavior of the follower via one or more mechanisms.

To study direct interpersonal persuasion by political elites we designed two field experiments, in which members of the public interacted directly with their members of Congress (MOCs) in an online town hall meeting. Here we follow the path of recent field experimental studies of peer effects (28, 29), which use randomization to identify treatment effects in naturalistic online settings. Specifically, we recruited sitting members of the US House of Representatives (study 1) and a US senator (study 2) to interact with their constituents via a real-time online forum (voice mediated with real-time transcription). Participants could address their MOC and listen to their member's responses to the questions and comments posed by the group. The participants were recruited from high-quality district samples (*Supporting Information*) and were compensated. Constituents were randomly assigned to receive reading material and participate in the discussion or to a control group that only received the reading material. Using responses to pretreatment and posttreatment survey questions, we test whether MOCs were effective in their persuasive appeals—that is, the hypothesis that, on average, meeting with a political elite changes attitudes and behaviors of members of the public in the direction sought by the elite.

Results

Experimental Design. Studies 1 and 2 used a similar research design, in which experimental subjects who expressed willingness to participate in an online forum with their MOC were randomly assigned to either a treatment or control condition. In the control condition, here referred to as “information only,” participants read background materials about the issue (background materials are included in *Supporting Information*). In the treatment condition, referred to as the “deliberative session,” in addition to reading the background materials participants were invited to attend an online town hall meeting with their MOC. Information in the background materials was drawn from nonpartisan sources (e.g., Congressional Research Service and Office of Management and Budget reports), edited to a ninth-grade reading level, and vetted by the participating MOCs' staffers (the background materials appear in *Supporting Information*). Both the information only and deliberative session treatments succeeded in increasing knowledge on immigration (17), meaning that subjects actually read and retained much of the policy information assigned to them. Subjects who attended the deliberative session were more likely to do so than those who were assigned to information only or did not comply with their assignment to attend the session.

Each session focused on a single policy issue. Discussion in study 1 focused on the issue of immigration and in study 2 on terrorist and detainee policy. Sessions were lightly moderated by

one of the authors. During each session, constituents typed comments and questions into an online discussion platform. After reviewing these contributions, a screener posted them to the whole group in approximately the order in which they were received. The screener played no active role in facilitating the discussion and had no knowledge of the study hypotheses or the content of the surveys. Questions were screened if they were duplicative of a prior question. The MOC responded through a telephone linked to a computer. Constituents received the MOC's responses by listening over computer speakers and/or reading a real-time transcription. After 35 min, the MOC and staff logged off. In study 1, the constituents were then directed to a chat room to have an open-ended discussion, which lasted 25 min. In study 2, the main session was extended and the chat session dropped because the larger number of participants made a plenary chat impractical. Fig. 1 illustrates one of the sessions with Representative George Radanovich (R-CA). Sample video is available in *Supporting Information*.

We are interested in substantive, attributional, and behavioral persuasion, and therefore we asked participants to complete questionnaires before and after the sessions. Subjects received background materials 1 wk after the baseline questionnaire, and sessions were held 1 wk after that. A postsession survey was fielded 1 wk after the session, and a final survey was fielded the day after the subsequent November election. In study 1 the sessions were held over several months, so the time between the postsession survey and the postelection survey varied from 1 to 4 mo (median = 60 d). In study 2, the session was held in July, meaning a time lapse of 3 mo between postsession survey and postelection survey. The items used to measure substantive and attributional persuasion are drawn from the follow-up surveys, and the item used to measure behavioral persuasion is from the postelection survey.

For substantive persuasion, we asked participants policy questions that related to the topic of the sessions, and we recoded responses within each MOC–constituent dyad so that higher values indicate more agreement between the two. To determine MOC positions, we relied on statements made during the deliberative sessions and on statements made in other contexts. Each MOC either strongly agreed or strongly disagreed with a given policy, so a move in either direction unambiguously constituted more or less agreement. For attributional persuasion, we asked participants whether they trusted or approved of their MOC. For behavioral persuasion, we asked whether participants intended to vote for their MOC in the upcoming election. Finally, the survey fielded the day after the election asked participants how they actually voted.

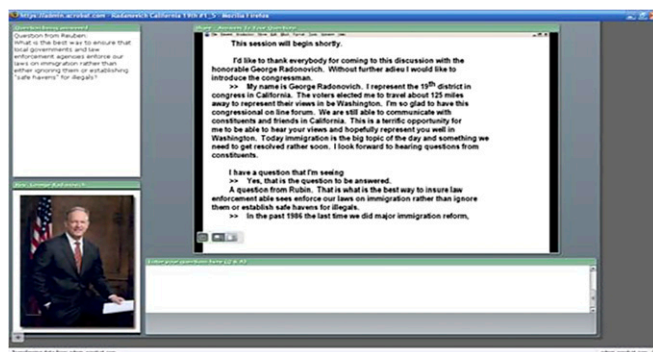


Fig. 1. A screen capture from the experimental interface. The image depicts what a subject saw during one of the experimental sessions.

Table 1. Participating members of Congress

Member	Party	State	Study
Rep. Earl Blumenauer	D	OR	1
Rep. Michael Capuano	D	MA	1
Rep. James Clyburn	D	SC	1
Rep. Mike Conaway	R	TX	1
Rep. Anna Eshoo	D	CA	1
Rep. Jack Kingston	R	GA	1
Rep. Zoe Lofgren	D	CA	1
Rep. Don Manzullo	R	IL	1
Rep. Jim Matheson	D	UT	1
Rep. David Price	D	NC	1
Rep. George Radanovich	R	CA	1
Rep. Dave Weldon	R	FL	1
Senator Carl Levin	D	MI	2

D, Democrat; R, Republican.

Study 1. Study 1 includes 19 sessions with 12 MOCs that took place between June and October 2006 (Table 1). There was robust variation among MOCs: five Republicans and seven Democrats, spread across all four major geographical regions, two women, an African American, and representatives of both parties' leadership. All were running for reelection, and they were diverse ideologically, including one MOC from each party who voted against their party on the topic under discussion (i.e., recent immigration legislation).

Participants in study 1 were recruited by an online survey firm from a probability sample panel of survey respondents designed to be demographically representative of the US population. Given the novelty of the study, before we ran the study we did not have sufficient information to reliably estimate compliance rates. Therefore, we used a two-step process to assign each participant to a treatment condition. All participants completed a baseline survey in which they were asked a filter question about whether they would like to participate in the deliberative session, to complete surveys only, or to opt out entirely. Here, we focus exclusively on subjects who were willing to participate. Of the 2,237 participants who completed this initial survey, 1,566 indicated that they would like to participate. Of these, 1,259 participants were then randomly assigned to either the deliberative session or information only.[†] In all, there were 19 small (~20 people each) group meetings with MOCs.

To differentiate substantive persuasion from attitude change via other mechanisms, we conducted a placebo test in which we compare responses on two subtopics, one that received a great deal of attention in the sessions and another that received almost no attention. For study 1, the high-attention issue was whether to create a path to citizenship for undocumented immigrants, and the low-attention issue was whether to change the number of immigrants allowed to enter the United States legally. The path to citizenship issue came up frequently in the sessions, whereas changes in legal immigration levels received almost no discussion—the words “amnesty” and “citizenship” appear more than 150 times in the session transcripts, whereas the phrase “legal immigration” appears only six times. Moreover, in those cases when the latter phrase appeared it was obvious from context that the topic of

[†]Some participants were also assigned to a third “true control” group that did not receive the materials, but we focus on the information only condition as the control group here because a different survey protocol governed whether they received the follow-up surveys, which complicates causal inferences. We also assigned 201 participants who indicated that they were not willing to participate in the deliberative session to receive information only. Those participants are also excluded in the analysis presented here because of their unwillingness to participate in the session.

discussion was actually whether illegal immigrants should be able “earn” citizenship.

To evaluate the effects of attending the deliberative session, we used instrumental variables regression to estimate the complier average causal effect, which is the effect of attending the session on the attitudes and behavior of participants who would attend only if assigned (i.e., the compliers) (30, 31). We instrument for attendance at the deliberative session using invitation to attend, which was randomly assigned. We also have responses from the baseline survey for these questions, and we condition on levels of the pretreatment response, except in the case of actual vote, for which we condition on pretreatment vote intent.

Analysis of balance between observational units assigned to treatment and control revealed a significant difference between the two groups on whether the participant and MOC belonged to the same party (*Materials and Methods*). Imbalance on a single covariate can be expected simply due to chance, but this particular covariate is also likely to predict our outcome variables. Therefore, in study 1 we condition on this covariate as well.

Results from Study 1. The analyses reveal strong evidence of persuasion on substantive policy questions and on attributions and behavior toward the MOC. The results are presented in Table 2 and Fig. 2. The first column of Table 2 lists the complier average causal effect. Positive numbers represent more persuasion by the MOC, and measures of effect sizes are presented in the final column using Cohen's *d*.

On path to citizenship, participants who attended the session moved toward their MOC's position more than they would have in the information only condition ($P = 0.011$; all reported tests are two-tailed). As expected, however, attendees did not move significantly toward their MOC on the issue of legal immigration ($P = 0.354$). A formal placebo test rejects the null hypothesis of no difference between these two effects ($P = 0.079$). Beyond attitudes on issues, attendees also exhibited changes in their attitudes toward the member. On average, attendees showed markedly increased trust ($P = 0.003$) and approval ($P = 0.053$). Finally, we find evidence of behavioral persuasion, most clearly in the 13.8% increase in intent to vote for the member ($P < 0.001$). Attendance in the session with the member also caused a 9.8% increase in likelihood of voting for the member in the November election, although this result yields weaker statistical significance ($P = 0.183$). Moreover, effect sizes are substantial, most notably for the increase that attending the session had on trust.

We conducted many robustness checks (reported in *Materials and Methods* with details in *Supporting Information*), including estimation of intent-to-treat effects (which ignore compliance/attendance and focus simply on the effect of random assignment),

Table 2. Experiment results for study 1 (House of Representatives)

Outcome	CACE	SE	<i>P</i>	<i>N</i>	<i>d</i>
Policy attitudes					
Agree on path to citizenship?	0.144	0.058	0.011	565	0.365
Agree on legal immigration?	0.033	0.036	0.354	566	0.108
Attitudes toward MOC					
Trust	0.118	0.039	0.003	492	0.523
Approve	0.076	0.040	0.053	526	0.286
Behavior toward MOC					
Vote Intent	0.138	0.038	<0.001	565	0.452
Actual Vote	0.098	0.072	0.183	516	0.197

CACE, instrumental variables regression estimate of complier average causal effects; *d*, Cohen's *d*; *n*, number of observations; *P*, two-tailed bootstrapped *P* values; SE, bootstrapped SEs with 10,000 resamples. All variables have been rescaled 0–1.

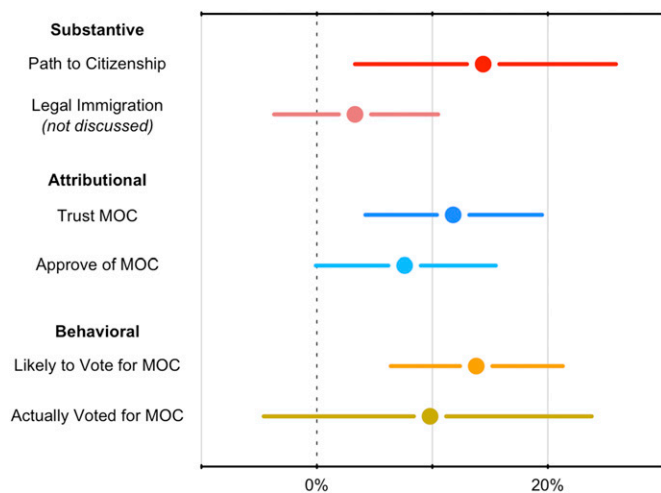


Fig. 2. Persuasion by members of the US House of Representatives. Complier average causal effects and 95% confidence intervals for subjects who would attend the online town hall if assigned to do so. Confidence intervals are constructed using the 2.5% and 97.5% quantiles from 10,000 bootstrap replicates. From top to bottom, $n = 565, 492, 526, 565,$ and 516 .

tobit models (to account for ceiling/floor effects), analyses of attrition and missingness in follow-up surveys, and treatment effect heterogeneity by congressional district. Our findings consistently support the inferences presented in Table 2.

Study 2. Although the findings in study 1 are strong, several factors might limit their generalizability. First, the immigration issue may have been particularly suited to elite persuasion. Second, the small size of the sessions may have increased the likelihood or extent of persuasion. Third, because of the novelty of the study and cost considerations, we used an unusual assignment procedure (the filter question before random assignment).

We therefore conducted a second study in July 2008 using a similar research design but on a different issue, with a larger session, and using a streamlined assignment procedure. The session was again followed by a survey fielded 1 wk after the event, as well as a postelection survey 3 mo later, immediately after Election Day. Participants ($n = 900$) were recruited from a non-probability sample and randomly assigned to meet with their sitting US senator (Carl Levin, D-MI) in a single online forum. Of these, 462 were assigned to the deliberative session and 175 actually attended the session to discuss issues surrounding terrorism (e.g., torture, rendition, and the detainees held at Guantánamo Bay, Cuba). Measures of treatment group balance are presented in *Materials and Methods*.

Study 2 shared several qualities of study 1. Some participants were randomly assigned to receive only background materials on the issue, and they serve as our control group.[‡] Similarly, we report estimates of the complier average causal effect (i.e., the average effect of attendance on attitudes and behavior for participants who would attend the session only if assigned to do so). To estimate the effects of treatment (i.e., attendance at the deliberative session) we again use instrumental variables regressions (30, 31), and we continue to condition on levels of the pretreatment response from the baseline survey. Here, we lack baseline responses not only for actual vote behavior, but also for the substantive questions. For these outcomes we condition,

[‡]As in the first study, participants could also be assigned to a third “true control” group. In this article, we focus exclusively on subjects who were willing to participate in a deliberative session and who were assigned either to deliberative session or information only.

respectively, on levels of vote intent and on responses to a general question about the appropriateness of torture. However, in study 2 no variables presented balance problems, and therefore we do not add any further conditioning covariates (*Materials and Methods*). Finally, we again isolate substantive persuasion with a placebo test that compares effects on one subtopic that received a great deal of attention to one that received almost no attention. Here, the topic of waterboarding came up frequently during the session, whereas the topic of whether to close the detainment facility at Guantánamo did not come up at all.

Results from Study 2. The results from study 2 are presented in Table 3 and Fig. 3. Attendees moved toward Senator Levin’s position on waterboarding substantially more than they would have if they were assigned to the control condition ($P = 0.052$). In comparison, attendees moved slightly away from the senator’s position on the placebo issue of whether to close Guantánamo ($P = 0.300$). A formal placebo test indicates that these two estimates were different from each other ($P = 0.009$). Attendees also exhibited changes in their attitudes toward the senator, showing increases of about 11% of the scales for trust ($P = 0.002$) and approval ($P = 0.001$). Finally, we also find evidence of behavioral persuasion; there was a 10.5% increase in intent to vote for the senator ($P = 0.004$) and an even larger 13.1% increase in November ($P = 0.073$).

Study 2 also presented an opportunity to measure the decay of the effects of attending the session. The postelection survey in study 2 included the approval question, which allows a comparison of effects 1 wk and 3 mo postsession. The estimated effect 1 wk after the session was 0.107 (Table 3). Using the postelection measure of approval, the estimated effect of attendance at the deliberative session was 0.049 (SE = 0.046, $P = 0.303$, $n = 577$). The decline of 0.058, although substantial, does not rise to the level of statistical significance ($P = 0.254$). This finding indicates that the long-term effect of attendance at these sessions is surprisingly strong, at least on attributional persuasion.

We also conducted robustness checks to estimate intent-to-treat effects and account for attrition and ceiling/floor effects and reanalyzed voting behavior using validated votes (32). These results broadly support the inferences presented in Table 3.

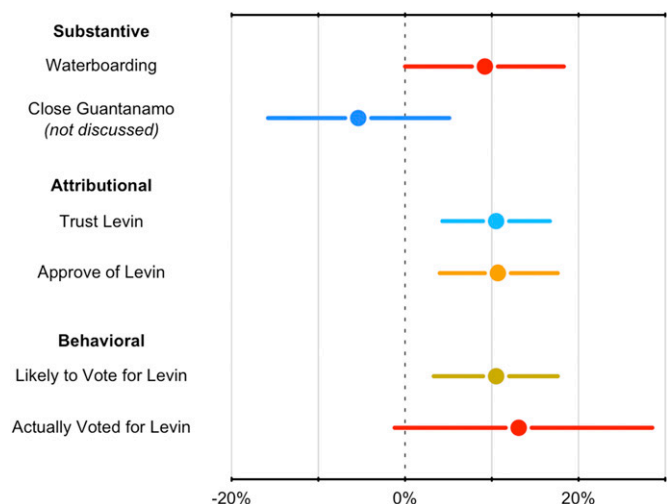


Fig. 3. Persuasion by Senator Levin. Complier average causal effects and 95% confidence intervals for subjects who would attend the online town hall if assigned to do so. Confidence intervals are constructed using the 2.5% and 97.5% quantiles from 10,000 bootstrap replicates. From top to bottom, $n = 495, 501, 487, 500,$ and 574 .

Table 3. Experiment results for study 2 (Senator Levin)

Outcome	CACE	SE	P	N	d
Policy attitudes					
Agree on waterboarding?	0.092	0.047	0.052	495	0.232
Agree on closing Guantánamo?	-0.054	0.053	0.300	497	-0.146
Attitudes toward Levin					
Trust	0.105	0.031	0.002	501	0.386
Approve (March survey)	0.107	0.034	0.001	487	0.330
Behavior toward Levin					
Vote Intent	0.105	0.036	0.004	500	0.292
Actual Vote	0.131	0.076	0.073	574	0.262

CACE, instrumental variables regression estimate of complier average causal effects; *d*, Cohen's *d*; *n*, number of observations; *P*, two-tailed bootstrapped *P* values; SE, bootstrapped SEs with 10,000 resamples. All variables have been rescaled 0–1. Supporting Information.

Discussion

To date there is surprisingly little evidence for direct, interpersonal persuasion by leaders. In this paper we have reported randomized controlled field experimental evidence that leaders (sitting MOCs and a senator) were able to persuade their followers (their constituents) regarding their opinions on policy questions, attributions about the leader, and their behavior (in this case, voting for the leader at increased rates) by meeting with and engaging them in online town halls.

However, another possibility is that these changes in attitudes and behavior are not reflective of persuasion, but instead demonstrate sorting. That is, perhaps the only thing that occurred during the online sessions is that participants who were politically ignorant but nonetheless receptive to their MOC's message learned what they "should" think. For example, constituents who were members of the MOC's party might have learned their party's position on the issue under discussion, or perhaps they learned that their MOC was also an adherent of their own political ideology or party. Perhaps some participants even learned that there was an upcoming election during which they could vote.

There are at least two reasons that this possibility is unlikely. First, if sorting were taking place, then participants who were not copartisans of the leader should also have been able to sort based on their involvement in the sessions, yet we see positive effects overall. Second, a series of conditional effects analyses reveals no systematic pattern (Fig. 4). Here, we included interaction terms to capture the moderating effect of shared partisanship within the member–constituent dyad. The majority of the differences in causal effects are not statistically significant, meaning that there are few detectable differences between how copartisans and other constituents reacted to their leaders during and after the sessions. Moreover, the few significant differences are not consistent across studies; they occur on different dimensions of persuasion. These significant effects actually indicate smaller effects for copartisans than for others. Similarly, there are neither significant nor consistent conditional effects based on differences in education, which further bolsters the claim that these estimates represent genuine persuasion. Further research should focus not only on the moderators of persuasion but also its mechanisms, which might include the provision of reasons, appeals to authority, or emotional stimuli.

Importantly, these results apply to a specific population—constituents who were given background material, who were likely to attend a deliberative session, and who were compensated for their time—and not necessarily to citizens more generally. However, one positive sign from this study is that active citizens, the ones who are open to attend town hall meetings, are also open to new arguments and positions from elites. If deliberative sessions

like these were to become more popular, it is likely that they would resemble these sessions, perhaps only differing in compensation.

Although our setting is distinctly the 21st century—representatives talking to a dispersed group of constituents via new communication technology—persuasion is of central importance for leadership for any time or place. All leaders must decide whether it is worth their time to meet directly with their followers, rather than communicate solely via broadcast (e.g., through mass media). Our findings provide reason to think that it is worth it. Direct interaction can yield distinct and substantial persuasion on several dimensions that matter to leaders, and our forums proved scalable, ranging from small groups to nearly 200 participants.

Materials and Methods

Treatment Group Balance. We contracted online survey research firms to draw samples. The survey firms provided responses to questions about background characteristics, and our baseline survey asked others including several of our outcomes of interest. We tested for treatment group balance using the xBalance command from the R package Rltools (33). In study 1, an omnibus test for balance (within congressional districts) indicates that the

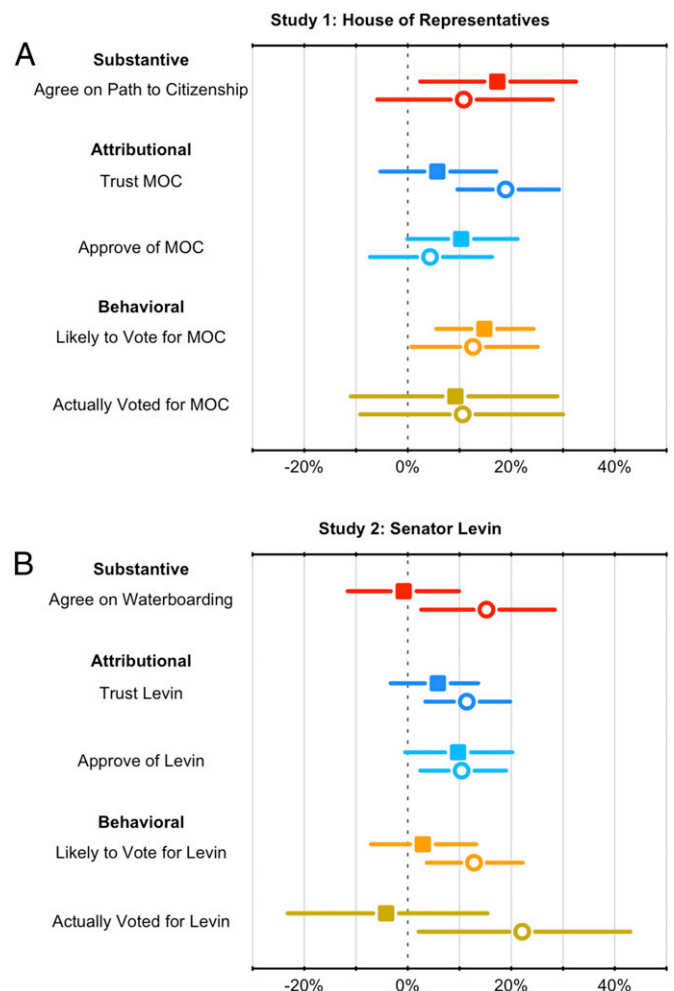


Fig. 4. Conditional effects by copartisanship. Complier average causal effects and 95% confidence intervals for subjects who identify with leader's political party (filled squares) and all others (open circles). Confidence intervals are constructed using the 2.5% and 97.5% quantiles from 10,000 bootstrap replicates. (A) From top to bottom, $n = 565, 492, 526, 565,$ and 516 . *P* values from bootstrapped tests of differences in causal effects between same party identifiers and all others are $P = 0.583, 0.074, 0.470, 0.823,$ and 0.879 . (B) $n = 495, 501, 487, 500,$ and 574 . *P* values from bootstrapped tests of differences in causal effects are $P = 0.061, 0.289, 0.919, 0.156,$ and 0.068 .

randomization was successful ($\chi^2 = 18.1$ on 22 df, $P = 0.697$). Slight imbalances appeared on several individual covariates. In one case, whether the MOC and the constituent share a political party, imbalance was relatively large. Imbalance on a single covariate is likely based on chance alone. However, because this particular covariate is associated with our potential outcomes of interest, we condition on an indicator for shared partisanship within the MOC–constituent dyad in our analyses for study 1. In study 2, an omnibus test for balance indicates successful randomization ($\chi^2 = 18.8$ on 18 df, $P = 0.403$). Only slight imbalances appear for individual covariates in study 2. Details on recruitment, questions, and balance are in [Supporting Information](#).

Estimation of Causal Effects. We use instrumental variables regression to estimate complier average causal effects, or the effect of attending the session for participants who would attend only if invited to do so. We use attendance as the endogenous regressor and random assignment as the instrument. Where possible, we also condition on indicator variables for baseline response levels (including missingness). The only exception is for analysis for the questions on waterboarding and Guantánamo in study 2, and for Actual Vote, for which we had no baseline questions. In those cases we used baseline responses to a general question about whether torture is ever justifiable and baseline Vote Intent, respectively. Where baseline responses were available, we also conducted change score analysis, which confirms the results in the main text. All instrumental variables regressions were calculated with the `ivreg` command from the R package `AER` (34).

We rely on standard assumptions to warrant causal inferences (30, 31), most notably excludability of assignment and the stable unit treatment value assumption (SUTVA). In study 1, excludability is threatened by the association between assignment and whether a participant identified as a member of the same party as her MOC. Therefore, in study 1, we condition on this covariate. We also rely on SUTVA, which is threatened if leaders acted merely as coordinating devices. In that case, one subject's potential outcomes would depend on whether others (with varying ideologies perhaps) were assigned to attend the session. The changes we document would then not be persuasion per se, but rather would have occurred if the MOC had been replaced by any figure. There are several reasons to think that SUTVA is not violated here. First, the deliberation elicited brief questions from the experimental subjects rather than opportunities for persuasive speeches from them, which suggests that it would be difficult for one subject to move sentiment. Second, survey responses were collected 1 wk after the session, meaning that any desire to coordinate would need to extend past the end of the session. Given that it is unlikely that subjects knew each other personally (or could even identify each other if they did), it is unlikely that a drive for coordination then motivated responses. Finally, there was a surprising amount of intersection between sessions in the topics that were raised. This suggests that the treatment (at least within MOC) was fairly homogeneous and likely robust to the inclusion or exclusion of particular subjects.

Compliance and Attrition. Treatment noncompliance and attrition in follow-up surveys occurred in both studies. For detailed analysis of compliance (i.e., predictors of who attended the deliberative sessions) see ref. 21. To look for evidence that either compliance or attrition was associated with baseline responses, we stratified observations into four complier-reporter types (attended session/answered follow-up, attended/did not answer, etc.) and performed analyses of variance in baseline responses to test for differences across types. In only 1 case out of the 12 did this association reach conventional significance levels (noncompliers who responded to follow-up had lower baseline responses to the trust question in study 2). We also calculated intent-to-treat effects of random assignment (regardless of attendance). Tests of null hypotheses conducted on this basis yielded inferences nearly identical to those in Tables 2 and 3. Attrition varied by question but was more common in study 1. To address attrition, we first reran analyses reweighting by the inverse probabilities of responding to the follow-up, which again yielded inferences very similar to those in Tables 2 and 3. We also conducted a trimming analysis to bound intent-to-treat estimates. The resulting bounds straddle zero in only one case (Path to Citizenship in study 1). Details appear in [Supporting Information](#).

Heterogeneity of Causal Effects Across Districts in Study 1. Study 1 includes 12 congressional districts, and it is possible that some MOCs were more persuasive than others. We conducted two analyses to test for this possibility. First, we used a leave-one-district-out procedure in which we sequentially held out a district and calculated estimates based on the remaining districts. The SEs from this procedure are very similar to SEs in Table 2, and the root mean square prediction errors for each held-out district yield very small differences across districts. Finally, we estimated a version of the instrumental variables model with random coefficients at the district level. The random coefficient estimates exhibited considerable variation by district on some outcomes, but no district stood out as consistently having larger or smaller estimates across outcomes. Details appear in [Supporting Information](#). Research protocols were reviewed and approved by the institutional review boards at The Ohio State University, Harvard University, and the University of California, Riverside.

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