Supporting Information

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Study 1

In the summer of 2006 we recruited 12 House members to hold either one or two online deliberative sessions on the question of immigration reform, a highly salient topic, with some of their own constituents, selected at random. These were small group discussions and the number of participants in each session ranged from 8 to 30, and each discussion lasted for 35 min. Constituents participated by typing comments or questions into a textbox, and these were immediately posted to a queue visible only to a moderator. The moderator posted the comments and questions to the town hall, visible to all participants, in roughly the order they were received, although priority was given to participants who had not already asked a question. The moderator also screened questions that were duplicative. The member of Congress read the posted question and then responded orally through a telephone linked to a computer. Constituents with audio capacity on their computers could listen to the member's responses, and all participants could read the member's responses via real-time captioning. The session lasted 35 min, and afterward the member logged off; constituents were then directed to an open forum and invited to discuss the member's responses and immigration more generally. The open forum lasted 25 min. We include in Movie S1 clips from a video from one of the online sessions.

In study 1, the "treatment" is the deliberative session with the member combined with the postsession chat. Allowing constituents to discuss the session with each other enhances external validity because it lends a greater realism to the experiment; citizens typically engage in politics in interaction with others (1).

S1. Background Materials for Study 1 (Immigration Policy). In this section we reproduce the background reading material provided to all subjects in our sample.

Introduction. Noncitizens can enter the United States legally on a permanent basis, or on a temporary basis. If a person is granted permission to come into the country permanently, he or she is known as a legal immigrant and gets a "green card." In 2004, 362,000 people came into the United States this way. After five years, if they learn English and meet other conditions, legal immigrants can become citizens. About 537,000 people completed the process to earn citizenship in 2004. Noncitizens can also enter the country on a temporary visa, as a tourist, student, or temporary worker. These visitors are not expected to stay beyond the term of their visas. Anyone without a green card or a current visa is considered an illegal immigrant.

Illegal immigrants. About 12 million illegal immigrants live in the U.S., according to recent estimates. Every year, about half a million (500,000) new illegal immigrants enter the country. Between half and two-thirds come from Mexico. Sometimes crossing the border can be dangerous. Smugglers known as "coyotes" often use unsafe methods to sneak their customers across the border. The U.S Border Patrol believes that nearly two thousand people died trying to cross the border between 1998 and 2004.

California is home to the largest number of illegal immigrants, followed by Texas, Florida, New York, Arizona, Illinois, New Jersey, and North Carolina. Illegal immigrants can be deported if they are caught. In 2004, 1.2 million were caught; some left voluntarily while others were deported. Deporting illegal immigrants can be complicated if the immigrants have children who were born in the U.S., because under current law, these children are legal citizens, even if their parents are not.

Economic implications. People are often concerned about how illegal immigration affects the job market, as well as taxes and social services like health care and education.

Right now, illegal immigrants make up about 5% of the US work force. Many immigrants work in textiles, food manufacturing, construction, agriculture, food services, and janitorial services, where they earn 27% less than US citizens with similar education and experience in the same industries. About 75% of the illegal immigrant population works. While it is very difficult to say with precision how illegal immigration affects wages, a report by the Congressional Budget Office suggests that it primarily affects American workers without high school diplomas. The wages for such jobs go down (by about 4%), which hurts these workers, but raises profits for American employers and businesses, and lowers prices for American consumers. Immigrants are consumers too, who pay for American products when they are here, so they contribute to the economy in that way as well. And some argue that immigration encourages American workers to invest in education to compete for higher-wage jobs.

Taxes & social services. It is also difficult to know exactly how illegal immigration affects taxes and social services. Although many illegal workers pay social security and other taxes, they are not eligible for many government benefits. On the other hand, over a quarter of illegal immigrants live in poverty. Many use emergency health care, and their children attend US schools (although some of those children were born here, and so are legal citizens who are entitled to public school education).

Legislative efforts. Taking on the issue of illegal immigration, both the House of Representatives and the Senate have passed legislation in recent months. The bills are very different, and to pass a law to set immigration policy, the two houses must come up with one bill that will pass in both chambers. Then the President must sign the bill to establish new immigration law.

The Senate bill, called the **Comprehensive Immigration Reform Act of 2006**, contains a path for illegal immigrants to become permanent residents if they pay a fine and go through a process to qualify as legal citizens. The bill also grants more visas to immigrants coming to work in certain industries where demand for their labor is higher (guest workers). Under current law, an American company who wants to use foreign workers under such programs must prove that doing so will not hurt the employment of current US citizens.

The House of Representatives bill, called the **Border Protection, Antiterrorism, and Illegal Immigration Control Act of 2005**, makes it a felony to be in the United States without proper documentation. Under this proposal, anyone who knowingly helps illegal immigrants can be prosecuted for a felony as well.

Details of the Senate bill. When certain industries have high demand for workers, this bill sets up temporary visas for workers to come to this country to get jobs in those industries. These guest workers must have a job lined up before they enter the country. They can stay up to three years, and can bring their families with them. No illegal immigrants currently living and working in the US would be eligible for this program. For the first year, 325,000 workers could enter the country under this program. After that, the number would be adjusted every year, depending on the demand for workers in each industry.

Path to citizenship. Illegal immigrants currently in the US are not eligible for the guest worker program, but they may be allowed to become legal permanent residents. The bill sets up three different categories of illegal immigrants: those who have been in the country

5 y or more, those who have been here for 2–5 y, and those who have been here less than 2 y.

The immigrants who have been here longest, since 2001 or earlier, can become permanent residents if they have been working for at least three of the five years. They have to pay a $5,000 fine. Their spouses and children will also get green cards. Once they have their green cards, they can eventually become citizens if they decide to go through that process too. Immigrants who came after 2001 and before 2004 (have been here 2–5 y) can get permission to stay and work for three years, provided they also pay a fine, of $1,000, and have been working already for the last two years. With their new three-year visa, they can apply for other visas that allow for longer stays. To do this, they have to go to a point of entry on the border and file their application there.

Immigrants who have been in the US less than two years will not receive any opportunities in the guest worker programs or paths to citizenship. They have to go back to their countries of origin and compete for a visa like everyone else.

Employer sanctions. Under the Senate bill, fines for employers who knowingly hire illegal immigrants would be raised from their current amounts to $20,000. Repeat offenders would get jail time. Within 18 mo, all employers would be required to use a database to verify that their employees are legal.

Border security. This bill would call for 370 miles of fencing along the U.S.-Mexico border, and another 500 miles of vehicle barriers. The Border Patrol, which has 11,000 agents right now, would be increased by 1,000 agents right away, and by 14,000 by 2011, for a total of 25,000 agents. The National Guard currently assists at the border, but under this bill, there would be a limit of 21 d to National Guard assignments there, to free up Guard troops when they are needed elsewhere.

English as the national language. The Senate bill establishes English as the official national language of the United States.

Details of the House bill.

Border security. The House Bill provides money for guarding the border with satellites, sensors in the ground, cameras, and radar. It also calls for 700 miles of fences along the U.S.-Mexico border, and more border patrol agents to patrol the fences.

Illegal entry and smuggling. Anyone caught smuggling illegal immigrants into the country can be prosecuted for aggravated felony charges, and could face mandatory minimum prison sentences. The bill also makes it a felony to be in the United States illegally. Immigrants face prison for entering the US without proper documentation, and those who do so more than once face mandatory minimum prison sentences. People who marry illegal immigrants to help them get green cards face criminal penalties. So does anyone else who helps an illegal immigrant commit immigration fraud.

Employer sanctions. The House bill calls for fines of as much as $40,000 each time an employer hires an undocumented worker. Repeat offenders could face as much as 30 y of prison time. Within 6 y, employers would have to use a database to check Social Security numbers for each employee.

Sources: Congressional Research Service Reports for Congress: "Immigration: Policy Considerations Related to Guest Worker Program (October 2005)" "Immigration Legislation and Issues in the 109th Congress (January 2006);" Congressional Budget Office Papers: "Immigration Policy in the United States (February 2006);" The Role of Immigrants in the US Labor Market (November 2005);" Congressional Research Service Summary of bills H.R.4437 and S.2611.

S2. Questions for Study 1 (Immigration Policy). This section includes only the survey questions reported in the article. Many other questions were also asked during this questionnaire. In each case, the coding appears in parentheses at the end of each of the responses, which are in small caps.

Willingness (to deliberate). You have the chance to participate in a unique project! The Digital Citizens Project is designed to help citizens and their members of Congress to communicate about local and national issues. By participating, your voice will be representing thousands of citizens where you live! This project is being conducted by researchers at Ohio State University, Harvard University, University of California, Riverside, and the Congressional Management Foundation. The researchers are solely responsible for choosing the survey questions, and designing all other aspects of the study. For this project, some respondents will have a chance to express their opinions to their member of Congress through questionnaires only, while some will also participate in an online discussion. You'll get to voice your opinions about a variety of topics focusing on the issue of immigration in the United States. If you are selected for the online discussion, here's what you'll be asked to do:

1. Complete this survey.
2. Read a short article, and complete a survey we'll send you in approximately one week called "Digital Citizens Project Background Materials"
3. Participate in a one hour online discussion session. Although there will only be one online session, for planning purposes, we have to know whether you would be available for **BOTH** of the following dates: **[DATE1]** AND **[DATE2].** The discussion will be on one of these dates, and will be followed by a short survey. As a token of our appreciation for your time and effort, you will receive $25 for your participation in this discussion.
4. Complete a survey approximately one week after the discussion called "Digital Citizens Followup"
5. Complete a survey in November called "Digital Citizens Post-Election"

No technical experience is required to participate. We are interested in hearing everyone's opinions! If you are selected, we will notify you via email and send you a package in the mail containing everything you need to know. Are you willing to participate in all phases of this project, and are you available both **[DATE1]** and **[DATE2]?**

Yes, I am willing to participate

No, I am unable to participate

If response was "No", ask. Would you be able to participate on just ONE of the dates?

Yes, I can participate on **[DATE1]**

YES, I CAN PARTICIPATE ON **[DATE2]**

NO, BUT I WISH TO COMPLETE SURVEYS TO BE A PART OF THIS PROJECT

NO, I DO NOT WISH TO PARTICIPATE AT ALL.

Path to citizenship (recoded by MOC's position). Some argue that providing opportunities for citizenship would reward illegal behavior and draw too many people across the border. Others argue that such opportunities are true to the nation's heritage as a country of immigrants, and would recognize that illegal immigrants contribute to the economy through hard work. How about you? If you were faced with this decision, would you vote for or against giving some illegal immigrants the opportunity to eventually become legal citizens?

For (that is, to create a path to citizenship)

Against (that is, NOT to create a path to citizenship)

Don't know

If response was "Don't know", ask. Would you say that you lean toward voting for or against giving some illegal immigrants the opportunity to eventually become legal citizens?

Lean toward voting for it (0.67, 0.33)

Lean toward voting against it (0.33, 0.67)

Don't know (0.5)

If response was "For" or "Against", ask. Are you somewhat or strongly [FOR/AGAINST] giving some illegal immigrants the opportunity to eventually become legal citizens?

Somewhat [FOR/AGAINST] **(0.83, 0.17)**

Strongly [FOR/AGAINST] **(1, 0)**

Legal immigration (recoded by MOC's position). Do you think that the number of immigrants from foreign countries who are permitted to come *legally* to the United States each year to live should be:

Increased

Left the Same (0.5)

Decreased

If response was "Increased" or "Decreased," ask. Do you think that the number of immigrants from foreign countries who are permitted to come *legally* to the United States to live should be [INCREASED/DECREASED] by a little or a lot?

A little (0.75, 0.25)

A lot (1, 0)

Trust MOC. How much of the time do you think you can trust **[MOC]**, your Member of Congress, to do what is right?

Always (1)

Most of the time (0.67)

Some of the time (0.33)

Not at all (0)

Don't Know (missing)

Approve of MOC. Do you approve of the way that **[MOC]** is handling [HIS/HER] job as Congressperson?

Strongly approve (1)

Somewhat approve (0.75)

Neither approve nor disapprove (0.5)

Somewhat disapprove (0.25)

Strongly disapprove (0)

Don't Know (missing)

Vote Intent. If the vote for the House of Representatives were held today, who would you vote for?

Definitely **[MOC] ([PARTY]) (1)**

Probably **[MOC] ([PARTY]) (0.75)**

Undecided (0.5)

Probably **[CHALLENGER] ([PARTY]) (0.25)**

Definitely **[CHALLENGER] ([PARTY]) (0)**

Other candidate (0)

Would not vote (0)

Actual Vote (asked only in November). Did you vote in the election on Tuesday November seventh, 2006?

No (0)

Yes

If response was "Yes," ask. For whom did you vote for the US House of Representatives?

[MOC] ([PARTY]) (1)

[CHALLENGER] ([PARTY]) (0)

S3. Sampling Procedures for Study 1 (Immigration Policy). We contracted with Knowledge Networks (KN) (www.knowledgenetworks.com), an online survey research firm, to draw the sample. KN maintains a sample panel of survey respondents that is similar to a probability sample in that it is demographically representative of the US population (www.knowledgenetworks.com/ganp/index.html).

KN recruited constituent subjects from members of its panel in the corresponding congressional districts and administered all surveys. Each participant was asked to complete a pretest survey and then was randomly assigned to one of three groups: an information only condition, a deliberative condition, or a true control condition, as described in the main text. The response rate to the baseline survey was 0.736 by American Association for Public Opinion Research (AAPOR) response rates 1 and 3, and 0.771 by rates 2 and 4. In total, we assigned 2,237 subjects who completed the baseline survey to the three experimental conditions.

All participants have access to the background materials that appear in section S1. Some participants also take part in a deliberative session (DS), whereas the others comprise the control group that we call information only (IO). To assign participants to DS, we first asked the "Willingness" question from Section S2. Participants who indicated that they could participate in one or both sessions could then be randomized into DS, IO, or the "true control" (TC) group. All others could be randomized into either IO or TC. We focus only on participants who were willing to participate and who were assigned to either DS or IO, which accounts for the single manipulation that we study.

Approximately 1 wk following the deliberative session in a given congressional district, KN administered a posttest survey to subjects in all treatment arms. Immediately after the November 2006 election KN administered another survey to participants. We assigned 1,084 subjects to the DS condition, of which 374 complied by attending the deliberative session. Among all subjects assigned to DS, irrespective of their compliance, 479 responded to the follow-up survey, and 264 of the DS compliers responded. We assigned 175 subjects who were willing and able to attend a deliberative session to the IO condition, and 97 of these responded to the follow-up survey.

Because of unexpectedly high costs of implementing this novel study with our survey vendor, about midway through the study we agreed to discontinue sending follow-up surveys to subjects we identified as "chronic nonresponders," or those with the strongest histories of not responding to the surveys or the other experimental tasks. As a result of this decision, a total of 269 subjects, or about 12% of the sample, did not receive a follow-up survey. These were the subjects who, even if we had sent these subjects the survey, were very unlikely to have filled them out. We knew this because the change in the survey procedures occurred after we had fielded the study to more than half of the sample. As a result, 349 of the subjects who would fall into the category of chronic nonresponders were in fact administered a follow-up survey, and among these, only 25.7% responded. Based on these estimates, we can state that among the 269 chronic nonresponders who were not sent the follow-up survey, we would have observed only about an additional 67 surveys returned, which would amount only to 3% of the total sample.

S4. Treatment Group Balance for Study 1 (Immigration Policy). The survey firm provided responses to questions about several background characteristics, and the baseline survey we fielded asked several others including baseline answers to the questions reported on in the article (Table S1). All balance analyses were conducted with the xBalance command from the R package RItools. An omnibus test for balance (within the strata of congressional districts) indicates that the randomization was successful ($\chi^2 = 18.1$ on 22 df, $P = 0.697$). However, slight imbalances appeared on several individual covariates. In one case, whether the MOC and the constituent identify with the same political party, the imbalance was relatively large (first row of Table S1). Imbalance on a single covariate is likely based on chance alone. It is also likely, however, that this particular covariate is associated with our potential outcomes of interest. Therefore, we condition on an indicator for shared partisanship within the MOC–constituent dyad in our analysis.

S5. Compliance and Attrition in Study 1 (Immigration Policy). Two possible factors that would confound causal inferences based on study 1 are compliance and attrition, which are common issues in field experiments (2). For example, it is possible that only those individuals who most liked their members were likely to participate in the online town halls. Similarly, individuals who disapproved of their member might be less likely to respond to surveys after participating in a town hall. Without proper adjustments in the analysis, this could make it seem like the online town halls increased approval of the member when they really did not.

An initial analysis suggests that these issues are less damaging than might be expected. For each of the outcome variables, we compared descriptive statistics for the pretest responses based on the possible different compliance and reporting types. Because we only have compliance information for participants who were assigned to treatment, we restrict attention here to those subjects ($n = 1{,}084$).

We coded these subjects as one of four types:

"Complier-Reporter" if she attended the session and responded to the relevant follow-up question

"Noncomplier-Reporter" if she did not attend the session but she did respond to the follow-up question

"Complier-Nonreporter" if she attended the session but did not respond to the follow-up

"Noncomplier-Nonreporter" if she did not attend the session and did not respond to the follow-up

Table S2 reports sample means and observation counts across types, which are detailed below. These data do reveal sporadic and small tendencies for pro-member subjects to be more likely to comply with treatment or to report on some questions. For example, complier-reporters seem to have been more likely to approve of their members on the pretest than were complier-nonreporters. However, none of the differences in means revealed in Table S2 are larger than a single SE. In an effort to determine whether there were systematic differences across types, we conducted an analysis of variance for each response variable. Although the resulting F statistics and P values cannot be interpreted as tests of similarity across groups, these tests reveal scant evidence that groups differ.

S6. Research Design and Estimation Method for Study 1 (Immigration Policy). Study 1 employs a single manipulation experimental design. To motivate causal interpretations of our results we use the potential outcomes model (3, 4) and rely on the potential outcomes model interpretation of instrumental variables (IV) (5).

Before describing the research design in detail, we develop some notation. Let Y_T be the observed response variable measured at time T, and let Z be the assignment variable that equals 1 if a participant was assigned to DS. Let D be the observed attendance variable that equals 1 if a participant attended the session.

The potential outcomes are the four responses $Y_{T=1}(Z, D)$ that a participant would make in the possible cases that she is or is not assigned to treatment ($Z = 0, 1$) and actually attends treatment ($D = 0, 1$). Thus, we define $D(Z)$ as potential treatment attendance depending on assignment Z. Some subjects who were assigned to the deliberative sessions did not attend, but none of the other subjects (those assigned to IO) attended the sessions. Therefore, we face a situation of one-sided noncompliance. Because participants who were not assigned to treatment could not attend, we have $D(Z = 0) = 0$ for all subjects, meaning that noncompliance is not a problem in the case that $Z = 0$. However, treatment attendance among those assigned to treatment, $D(Z = 1)$, varies. Following ref. 5, we divide the population into compliance types based on the potential outcomes $D(Z)$ for $Z = 0, 1$. Compliers are the subpopulation with $D(Z = 1) = 1$, and noncompliers are the subpopulation with $D(Z = 1) = 0$.

Given this design, we rely on IV regression to estimate the complier average causal effect (CACE), or the causal effect of attending the deliberative session (D) on responses ($Y_{T=1}$) among those participants who would actually attend if assigned to do so [those with $D(Z = 1) = 1$]. In the notation we have developed,

$$\mathrm{CACE} = \mathbf{E}[Y_{T=1}(Z, D=1) - Y_{T=1}(Z, D=0) | D(Z=1) = 1],$$

where $\mathbf{E}$ is the expectation operator.

To warrant our inferences, we rely on a set of well-known and standard assumptions (6). First, we make the SUTVA. Essentially, we assume that the potential outcomes (both responses and assignment) for each participant depend only on the treatment assignment for that participant, and not the treatment status for the other participants.

Our second assumption is known as the excludability of treatment assignment. Essentially, this assumption requires that our treatment assignment affects potential outcomes only through its effect on actual attendance. More concretely, we must assume that those who were not assigned to the treatment did not react in a way that would move them away from the member's position, negatively affect their trust in the member, and so on. Conversely, we must assume that those who were assigned to the treatment did not move toward the member's positions via the mere invitation. In justifying this assumption we note that the study was explicit in telling the participants that assignment was determined entirely by the political science researchers. Therefore, we assume exclusion of the random assignment. This assumption enables us to write $Y_{T=1}(Z, D) = Y_{T=1}(D)$, reducing the number of potential outcomes for each subject from four to two.

Third, we assume that assignment Z has some positive effect on the probability of treatment, so that $\mathbf{E}[D(Z = 1) - D(Z = 0)] > 0$. In fact, compliance with assignment to treatment was about 34.5%, which is ample evidence to warrant this assumption.

Based on these assumptions, we estimate the CACE by estimating a two-stage least squares regression model (7). In this model, our estimate of actual attendance is better represented as a latent variable, notated D^*, because we estimate it for all participants, including those who have no opportunity to attend (i.e., those with $Z = 0$). Here we also include as predictors covariates $\boldsymbol{X}$, which include indicator variables for all levels of the pretreatment response (including missingness) as well as an indicator for whether the subject and MOC belong to the same party (same party as MOC) to adjust for the imbalance in that variable. The model has two equations, the first of which is a model of the outcome variable:

$$Y_{T=1} = \beta_0 + \beta_1 D^* + X\gamma + u.$$

The effects reported in the manuscript are estimates of β_1, the CACE.

The treatment assignment variable (Z) does not appear in the outcome equation because we have assumed that it had no effect on potential outcomes except through actual attendance. However, treatment assignment does appear in the second model, which captures compliance:

$$D^{*}=\alpha_0+\alpha_1 Z+X\delta+e.$$

The CACE estimates we report were calculated with the ivreg function from the R package AER (6). The SEs we report are calculated using a nonparametric bootstrap with 10,000 resamples, stratified by congressional district. The P values we report are all two-tailed. To calculate these values, we first found the percentage of the resamples in which the estimate was greater than zero, and the percentage in which it was less. We then take the minimum of these two percentages and double it to yield a two-sided P.

Table 2 in the main text presents the results of the analysis described in this section; these are the results depicted in Fig. 1 in the main text.

S7. Auxiliary Causal Effects Estimates for Study 1 (Immigration Policy). In addition to the CACE we also estimated the intent-to-treat effect (ITT), which reflects average differences based on assignment (Z) without regard for actual attendance (D). In the notation developed in section S6, the ITT is

$$ITT{=}\mathbf{E}[Y_{T=1}(Z=1)-Y_{T=1}(Z=0)].$$

In general, ITT estimates will be closer to zero than CACE estimates. The reason is that the CACE essentially magnifies the ITT to correct for noncompliance. Therefore, we expect the ITT point estimates to be closer to zero than the CACE estimates.

Estimation of the ITT is simpler than that of the CACE. To estimate the ITT, we regress the response variable ($Y_{T=1}$) on treatment assignment (Z) and the same covariates $\boldsymbol{X}$ as above. The first two columns of Table S3 report these ITT estimates.

It is clear from this table that the significance of treatment estimates is slightly diminished. This decrease results from decreases in the point estimates, as expected.

Next, to attend to ceiling and floor effects, we fit a series of tobit models to estimate ITT effects. We fit one of these models for each outcome except Actual Vote, which is binary. Coefficients are not directly comparable between this model and the previous ITT estimate, because the tobit model estimates effects on a latent. This incommensurability notwithstanding, there is a marked similarity in the statistical inferences between the tobits and those presented in the ITT column.

Given that a significant number of observations have missing outcomes in the follow-up, one possibility is that our findings are consistent with null treatment effects and result only from selection bias in the availability of responses. The interpretation of the results we present in the main text relies on the assumption that missingness is independent of the potential outcomes. This is a strong assumption that we cannot directly test. Therefore, we report in this section on two further analyses that rely on weaker assumptions about attrition.

First, we model missingness of follow-up responses based on baseline responses and treatment assignment. We then perform a weighted version of the instrumental regression (IV) analysis described in the previous section. Because some baseline observations were missing, we included a dummy variable $R_{T=0}$ that equals 1 only if a participant answered the baseline question and recoded missing observations $Y_{T=0}$ as equal to 0. Thus, $Y_{T=0}$ is effectively an interaction term. First we regress a dummy variable $R_{T=1}$ that equals 1 only if a participant answered the follow-up question using as predictors treatment assignment, each level of the pretreatment response (including missingness), and an indicator for whether the subject identifies as a member of MOC's party.

In the cases of Vote Intent and Actual Vote, the number of pretest nonresponders (those with $R_{T=0}=0$) is so small (i.e., for six subjects, ~0.5% of the sample) that the logistic regression estimates become unstable. In those two cases, we simply dropped those respondents for this weighted analysis, but not in the paper.

Based on this model, we generate predicted probabilities that the follow-up outcome was observed for each participant. We then weight each observation by the inverse of this predicted probability. In this way, observations with a lower probability of being observed are weighted more highly than observations that were very likely to have been observed. Because each outcome has a slightly different attrition rate, we conduct a separate weighted IV analysis for each question. In every case, the predicted probability that the follow-up outcome is observed is greater than zero. The weighted results can be interpreted as causal if we assume that missingness is independent of potential outcomes, conditional on pretreatment responses. This assumption is weaker than the unconditional version of the assumption. The result is the inverse probability weighted complier average causal effect, or IPW-CACE.

The fifth and sixth columns of Table S3 present results of the IPW-CACE based on this analysis. This weighted analysis yields estimates that are very similar to those we present in the paper. In several cases, the weighted analysis actually increases the point estimates, which may indicate that the point estimates in the main text are biased slightly toward zero. That said, the weighted analysis, like the unweighted analysis, relies on an assumption that is fundamentally untestable.

We also conducted an analysis of the change scores (CS) for the variables on which we have baseline responses. For study 1, the only variable for which we lack a baseline response is Actual Vote. Change scores are calculated as $Y_{T=1}-Y_{T=0}$ for all subjects who responded to both surveys. The sample sizes for the CS analysis are therefore lower than for other analyses. We conducted CS analyses using IV regressions that matched those in the paper, except that rather than conditioning on baseline responses we used those data in the outcome measure.

CS analyses are presented in the seventh and eighth columns of Table S3. The only interesting difference between these analyses and those presented in the paper is for Trust. Here, the difference is likely driven by the 74 nonresponders to the trust question on the baseline survey who then responded to the follow-up survey. Apart from this difference the CS analysis is remarkably similar to that presented in the main text.

As a final robustness check, we also estimate identification regions for the ITT effect using conditional trimming bounds (7). The ITT is an estimate of the effect of assignment to the DS (Z) rather than attendance at the DS (D). Using this technique, we can estimate possible values for the average causal effect for the subpopulation of "always responders" (i.e., the group of participants who would respond to follow-up survey regardless of whether they were assigned to DS or IO).

The idea behind trimming bounds is to make a set of "worst-case" assumptions about the missing observations. First, we subdivide observations into categories based on their pretreatment responses. Then, we identify a fraction of observations to trim within each category, based on the fractions of observed responses in the DS and IO groups. The fraction to trim is $Q=1-$ lower response rate/higher response rate. For the group (DS or IO) with the higher response rate, we trim the most extreme responses. To get two worst-case bounds, we first trim the Q lowest responses, which generates a maximum possible effect estimate. Then we trim the Q highest responses, which generates a minimum possible effect estimate. Differences in means based on these trimmed sets of responses yields a conditional identification region within each pretreatment response category. Finally, we aggregate these conditional identification regions across categories to yield a set of worst-case bounds of the average causal effect for the participants who always respond on the follow-up.

The final column of Table S3 present the results of the trimming analysis. The conditional trimming bounds span 0 in only two cases, for Legal Immigration and Path to Citizenship. We did not expect to find a positive effect in the case of Legal Immigration, because it was not discussed during the DS. In the case of Path to Citizenship, the vast majority of the identification region is positive. We can exclude a zero estimate in all other the cases.

S7. Treatment Effect Heterogeneity by District. The above analysis assumes a fixed treatment effect across districts. However, it is plausible that some MOCs were more persuasive than others, or similarly that constituents in some districts were more persuadable than others. If either were true, treatment effects would exhibit heterogeneity by district. To explore this possibility, we reanalyzed the data in two ways.

First, we used a leave-one-district-out procedure, in which the estimation method described in section S6 is repeated 12 times, using observations from 11 districts each time and leaving each district out of one analysis. We conducted two analyses using these models. First, we simply reestimated SEs by averaging the squared differences of these estimates and the original CACE estimate, weighting each by the number of included observations. If the SEs from this analysis differ substantially, that would indicate substantial treatment heterogeneity across districts.

The differences between the leave-one-district-out SEs and those reported in the main text are very slight. This finding indicates that, although there is likely some limited heterogeneity in the treatment effects across districts, it is small in magnitude compared with average effect sizes (Table S4, left side).

We also used these leave-one-district-out models to generate out of sample prediction errors for the left-out district. Here we report root mean squared prediction errors. If we were to see substantial and consistent differences in these errors in one or more districts, that would indicate that either there was more or less persuasion in that district. Instead, we see neither substantial nor consistent differences across districts (Table S4, left side).

Second, we developed a random slope IV regression model for each of the outcomes in study 1. These models use the same baseline as the models reported in the text, but with two differences. The first difference is that rather than estimate the models with two-stage least squares we now use a maximum likelihood approach that simultaneously estimates parameters in the two equations, one modeling attendance as a function of assignment and a second modeling the outcome as a function of attendance. The second difference is that the coefficients on attendance were modeled as independent draws from a normal distribution, whose mean and SD were also estimated. We used RStan to fit these models (8).

The estimated coefficients by district are displayed in Table S4 (right side). There are a few notable elements from these estimated models. First, the models recover point estimates for the mean effects that are very similar, although generally slightly larger than, the two-stage least squares estimates presented in the main text. Second, there is some variation in the estimates, especially for Path to Citizenship, Vote Intent, and Actual Vote. However, there is also not much coherence among these estimates. For example, the correlations among these random slope estimates are generally modest. The largest Pearson correlation is for Trust and Approval, at 0.47. However, for the outcomes with more variance, there is generally lower correlation. For example, the correlation of estimates for Path to Citizenship and Vote Intent is 0.22, for Path to Citizenship and Actual Vote is 0.04, and for Vote Intent and Actual Vote is 0.19.

S8. Conditional Effects for Study 1 (Immigration Policy). The above analysis averages effects across participants who are from the same political party as their MOC with those from the opposite (including independents). In this section, we reanalyze the data with an IV regression model that includes an interaction between attendance and an indicator for whether two potential moderators.

First, we test for conditional effects depending on whether participant and MOC belonged to the same party. We also add, as a second instrument, the interaction of treatment assignment and the same party indicator. We then bootstrap (with 10,000 resamples) and compare the resulting empirical distributions of conditional CACE estimates for same-party participants and opposite-party participants.

A straightforward way to compare these estimates is to look for differences between confidence intervals. In particular, if two intervals were disjoint, that would be excellent evidence of different effects. However, instead, for each outcome there is substantial overlap in the confidence intervals. To provide a statistical test of these comparisons, we calculated two-sided bootstrapped P values (Table S5). The only difference that is close to significant is trust, in which case the estimate was actually higher for opposite party participants than for those in the same party. This analysis supports a causal interpretation of our estimates rather than one in which the estimates are due simply to pretreatment partisan attachment. The analysis also indicates that leaders effectively persuaded constituents of all partisan stripes.

Second, we test for conditional effects depending on education, using an indicator for college graduation as a conditioning variable. As is the case for copartisanship, we again see scant evidence of significant or consistent conditional effects.

Study 2

Study 2 is similar to study 1 in many ways, and we conducted study 2 to see whether the effects we found in study 1 would scale up to a larger group of participants and to a different focal issue. In July 2008 a large group of citizens from Michigan were given the opportunity to discuss enemy combatant detainee policy with Senator Carl Levin (D-MI) via an online town hall. There were several important differences between study 1 and study 2. First, instead of small groups of 8–30 participants, the Levin session had 175 participants. Second, the topic for discussion was detainee policy, a far less salient issue than immigration policy, particularly at the times each study was conducted (9, 10). Third, as we describe below, we contracted with the online survey firm YouGov/Polimetrix to sample participants (www.polimetrix.com). Fourth, the session lasted 45 min instead of 35 min, and there was no open-ended forum among participants after the session. Finally, due to funding constraints, we combined the baseline and background materials surveys (those in a true control condition did not observe the background materials), and all of the surveys were shorter.

S9. Background Materials for Study 2 (Detainee Policy). In this section we reproduce the background reading material provided to all subjects in our sample.

Introduction. Following the terrorist attacks of 9/11, Congress gave President Bush the power "to use all necessary and appropriate force" against anyone who "planned, authorized, committed, or aided the terrorist attacks" against the United States." This began the war on terror. Soon thereafter, the United States invaded Afghanistan to overthrow their rulers, the Taliban, who had harbored and supported the terrorist group Al Qaeda. Then in 2003 the US invaded Iraq.

The war on terror and the law. During the course of the war on terror the US government transferred about 520 captured people to the US Naval Station in Guantánamo Bay, Cuba, for detention and possible prosecution for war crimes. These detainees — designated "enemy combatants" — were not initially granted the ability to challenge their detention in front of a judge.

The Bush Administration has argued that the war on terror is a new kind of conflict, requiring a new set of rules for the detention and treatment of persons suspected of posing a terrorist threat. The US Constitution guarantees prisoners the right, known as *habeas corpus*, to challenge the legitimacy of their detention in a court of law. The Bush Administration has argued that the circumstances of the war on terror give the government the authority to detain certain individuals without trial.

In 2004, the Supreme Court ruled against this, arguing that "a state of war is not a blank check for the president" and that enemy combatants have the right to challenge their detention before a judge or other "neutral decision-maker."

The Department of Defense then established Combatant Status Review Tribunals to meet the requirements of the right to a trial. These tribunals determine whether Guantánamo detainees were "enemy combatants" who could be detained for the duration of the war on terror and prosecuted in military commissions for any war crimes committed.

The Combatant Status Review Tribunals are similar to the procedures the Army uses to determine Prisoner of War (POW) status during traditional wars. When a Tribunal determines that a detainee is no longer an enemy combatant, the detainee is usually transferred to their country of citizenship. Those deemed unlawful enemy combatants are given a chance to argue, in a separate proceeding before the Tribunal, that they should be released because they are no longer a threat.

The tribunals, so far, have not been bound by the rules of evidence used in civilian courts. They use classified information to try the detainees. The detainee is not given access to classified government evidence. Instead, each detainee is assigned a military officer, who would serve as their attorney, and only this officer could view the classified information.

In an effort to clarify legal issues surrounding the detention process, the Republican controlled Congress passed the Military Commissions Act (MCA) in 2006, which was subsequently signed into law by President Bush. This act tried to take away the jurisdiction of civilian courts to hear *habeas corpus* challenges by Guantánamo detainees based on their treatment or living conditions.

After the Democrats regained majorities in both the House and Senate in 2006, the Senate Judiciary Committee sent The Habeas Corpus Restoration Act of 2007 to the full Senate in June of 2007. This legislation would restore the *habeas corpus* rights of detainees.

However, the Supreme Court ruled in *Boumediene v. Bush* that the 2006 MCA law setting up the tribunals was unconstitutional. Further, the Court ruled that the detainees have habeas corpus rights under the Constitution, and that the system the administration had put in place to classify them as enemy combatants and review those decisions was inadequate.

Treatment of detainees. The United States is party to both the Geneva Conventions and the United Nations Convention Against Torture. These international laws govern the treatment of civilians and combatants during wartime. The treaties give captured individuals who are affiliated with foreign armed forces special status known as Prisoners of War (POW). The United Nations Convention Against Torture prohibits torture under all circumstances and for any reason, and holds individuals responsible for violations of these prohibitions, regardless of orders from governments, courts, or superiors.

The US Detainee Treatment Act of 2005 requires uniform standards for interrogation and expressly bans cruel, inhuman, or degrading treatment of detainees in the custody of any US agency. The interpretation of these prohibitions is largely linked to practices that would be prohibited under the Fifth, Eighth, and Fourteenth Amendments to the US Constitution.

While US courts and administrative bodies have found that severe beatings, sexual assault, rape, and (in certain circumstances) death threats may constitute "torture," courts have not decided whether harsh, yet sophisticated, interrogation techniques of lesser severity (e.g., "water-boarding") constitute "torture" under either international treaties or US law.

Meanwhile the U.S.'s treatment of detainees who remain in custody continues to be a source of contention with human rights groups and other nations. Photographs depicting the apparent abuse of Iraqi detainees at the hands of US military personnel at Abu Ghraib prison in Iraq have resulted in numerous investigations, congressional hearings, and prosecutions, raising questions regarding the applicable law. Some critics contend that US policy effectively continues to permit harsh treatment that falls below international standards.

S10. Questions for Study 2 (Detainee Policy). This section includes only the survey questions reported in the article. Many other questions were also asked during this questionnaire. In each case, the coding appears in parentheses at the end of each of the responses, which are in small caps.

Waterboarding. In a procedure known as "water-boarding," interrogators produce the sensation of drowning in a restrained prisoner by either dunking him in water or pouring water over his face. Do you think the US government should or should not be allowed to use this procedure to attempt to get information from suspected terrorists?

Strongly favor (0)

Favor somewhat (0.25)

Neither favor nor oppose (0.5)

Oppose somewhat (0.75)

Strongly oppose (1)

Close Guantánamo. As you may know, for the past six years the United States has been holding a number of suspected terrorists at a US military prison in Guantánamo Bay, Cuba. Based on what you have heard or read, do you think the US should continue to operate the prison, or do you think the US should close the prison and transfer the prisoners somewhere else?

Definitely continue to operate (0)

Probably continue to operate (0.33)

Probably close the prison and transfer (0.67)

Definitely close the prison and transfer (1)

Torture (baseline only). The use of torture against suspected terrorists to gain important information can: often be justified, sometimes be justified, rarely be justified, or never be justified?"

Often be justified (0)

Sometimes be justified (0.33)

Rarely be justified (0.67)

Never be justified (1)

Trust. How much of the time do you think you can trust Carl Levin, your Senator, to do what is right?

Always (1)

Most of the time (0.67)

Some of the time (0.33)

Not at all (0)

Don't Know (Missing)

Approve. Do you approve of the way that Carl Levin is handling his job as Senator?

Strongly approve (1)

Somewhat approve (0.75)

Neither approve nor disapprove (0.5)

Somewhat disapprove (0.25)

Strongly disapprove (0)

Don't Know (Missing)

Vote intent. If the vote for the Senate were held today, who would you vote for?

Definitely **Carl Levin (Democrat) (1)**

Probably **Carl Levin (Democrat) (0.75)**

Undecided (0.5)

Probably **Jack Hoogendyk (Republican) (0.25)**

Definitely **Jack Hoogendyk (Republican) (0)**

Other candidate (0)

Would not vote (0)

Actual Vote (asked in November 2008)

Which of the following best describes you?

I did not vote in the election this November (0)

I thought about voting this time – but didn't (0)

I usually vote, but didn't this time (0)

I attempted to vote but did not or could not (0)

I definitely voted in the November General Election

If response was "I definitely voted in the November General Election," ask. For whom did you vote for US Senator?

Carl Levin (1)

Jack Hoogendyk (0)

S11. Sampling Procedures for Study 2 (Detainee Policy). YouGov/Polimetrix (research.yougov.com), an online survey research firm, drew the sample. Polimetrix maintains a nationwide panel of potential subjects and for this study sampled only Michigan state residents. The Polimetrix panel is an opt-in panel, and because of resource constraints we did not match the respondents to the state population. Because the sample is not representative of the state, we can only make statements regarding our sample. Overall, the sample is more politically active and knowledgeable than the population. The sample is likely representative of the people who are politically active and who attend political events, and hence the sample likely reflects a subpopulation of interest.

The study consisted of a pretest survey administered to all participants, a web-based seminar available to some participants, a posttest survey available to all participants, and a postelection survey available to all participants. We administered the pretest survey July 18–25, 2008, and the posttest survey August 5–8, 2008. A total of 462 participants who completed the pretest survey were invited at random to the session with Senator Levin, and of those 175 chose to participate (a 38% compliance rate). We assigned 221 subjects to the IO condition. Among all participants, 70% responded to the posttest survey, and 85% responded to the postelection survey, as calculated using AAPOR RR6, which is the response rate calculation appropriate to opt-in survey panels (11).

As a part of the pretest survey all participants were informed that they might be invited to a web-based seminar with Senator Levin and had to confirm that they were willing and able to attend for the scheduled time. The respondents did not know their group assignments before they completed the first wave survey.

S12. Treatment Group Balance in Study 2 (Detainee Policy). Similar covariates as those from study 1 were available in study 2 (Table S6). 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.

S13. Compliance and Attrition in Study 2 (Detainee Policy). As in study 1, compliance and attrition could possibly confound causal inferences based on the evidence we present from study 2. Again, we break subjects who were assigned to treatment ($Z = 1$) into four categories (complier-reporter, noncomplier-reporter, complier-nonreporter, and noncomplier-nonreporter) as described in section S5. We focus on those assigned to treatment because they are the only subjects who had the opportunity to comply.

Table S7 reports descriptive statistics for the pretreatment responses from these four types.

Attrition was not as serious a problem in study 2 as in study 1. Again, there are sporadic differences among the three substantial compliance types. This difference rises to the level of significance only for the Trust Levin question, although this is attributable to the fact that there are no complier-nonreporters for this question. Furthermore, we observed larger average trust for noncomplier-nonreporters' pretreatment responses, which indicate that our estimates are, if anything, likely to be underestimates.

S14. Research Design and Estimation for Study 2 (Detainee Policy). Like study 1, study 2 employs a single manipulation experimental design. Some participants also take part in a DS, whereas the others comprise the control group that we call IO. All DS and IO participants have access to the background materials that appear in section S9.

The research design and estimation method use in study 2 follow the strategy that was used for study 1, as described in section S6. There is one important difference: In study 2, we do not use regression to adjust for Same Party as MOC, because there was no imbalance on this variable. The research design is otherwise identical to that of study 1.

Table 3 in the main text presents the CACE estimates that are also depicted in Fig. 3.

S15. Auxiliary Causal Effects Estimates for Study 2 (Detainee Policy). As in study 1, we also estimated the ITT effect, which reflects average differences based on assignment (Z) without regard for actual attendance (D), for study 2. Consult section S6 for a full description of the estimand and methods.

Again, we expected the ITT point estimates to be closer to zero than the CACE estimates. The first two columns of Table S8 report these ITT estimates. As in study 1, the significance of treatment estimates is diminished, although the direction of the statistical inferences is unchanged. The tobit models of the ITT also yield similar statistical inferences.

As in study 1, we also report two additional analyses intended to account for attrition: inverse probability of treatment weighted estimates and trimming bounds.

The fifth and sixth columns of Table S8 present the results of the IV regression analysis with observations weighted by the inverse probability of responding to the follow-up survey question.

Because response rates in study 2 were high, it is unsurprising that there are few differences between the results of the weighted and unweighted IV analyses. Indeed, the point estimates are all very similar, and the only interesting change is that the significance of the effect of attendance on attitude about waterboarding weakens slightly.

We also conducted a CS analysis for study 2. Here, we lack baseline responses not only for Actual Vote, but also for Waterboarding and whether to close Guantánamo. The results are presented in the seventh and eighth columns of Table S8, and these results are very similar to those presented in the main text.

We present the conditional trimming bounds of the ITT estimate in the last column of Table S8. The conditional trimming bounds never span 0 in study 2, adding considerable support to the causal interpretation advanced in the main text. Here, the only surprising result is that the identification region for Close Guantánamo, which we took to be the placebo question, now excludes zero, albeit barely.

Finally, we also have data on validated votes for study 2, which permits analysis that does not rely on self-reported voting behavior. Validated votes, authenticated with publicly available voting records, were available for many respondents, and this analysis confirms the finding presented in the main text (CACE = 0.121, SE = 0.077, P = 0.111, n = 327).

S16. Conditional Effects for Study 2 (Detainee Policy). The above analysis averages effects across participants who are from the same and from the opposite political party as Senator Levin. In this section, we reanalyze the data with an IV regression model that includes an interaction between attendance and an indicator for each of two potential conditioning variables.

First, we include an indicator for whether participant and MOC belonged to the same party. Here, we bootstrap (with 10,000 resamples) within congressional districts and then compare the resulting empirical distributions of conditional CACE estimates for same-party participants and opposite-party participants.

As in study 1, we compare pairs of confidence intervals, in which a lack of substantial overlap is evidence of a strong conditional effect (Table S9). In study 2 we see somewhat more evidence of conditional effects than in study 1. However, there is again little sustained evidence that the reported effects are due to participants who belong to the same party as Senator Levin. Instead, in study 2 it actually seems that same-party participants were less likely to be persuaded by Levin than were opposite-party members. Moreover, the significant differences here are on policy and Actual Vote, whereas the borderline significant difference in study 1 was on the Trust. This analysis further enhances the interpretation of our estimates as causal.

Second, we test for conditional effects depending on education, again using an indicator for college graduation as a conditioning variable. As is the case for copartisanship, we again see scant evidence of significant or consistent conditional effects across outcomes.

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Table S1. Balance statistics for study 1

Covariate	Control	Treatment	Z score	P
Same party as MOC	0.503	0.621	2.712	0.007
Party (1–7)	4.445	4.194	−1.319	0.187
Age in years	45.00	46.71	1.343	0.179
Education (1–4)	3.107	3.115	0.110	0.913
Income (1–9)	6.363	6.370	0.031	0.976
White	0.799	0.813	0.409	0.682
Black	0.055	0.046	−0.477	0.634
Latino	0.082	0.068	−0.609	0.543
Other race	0.064	0.073	0.372	0.710
Female	0.755	0.727	−0.711	0.477
Path to Citizenship (baseline)	0.526	0.560	0.967	0.334
Trust (baseline)	0.454	0.452	−0.104	0.917
Approve (baseline)	0.566	0.574	0.370	0.712
Vote Intent (baseline)	0.532	0.569	1.352	0.176

Mean values for covariates within treatment groups, by strata of congressional districts, along with tests for individual covariate balance.

Table S2. Means and counts by compliance/reporter types (study 1)

n = 1,084	Complier- reporters	Complier- nonreporters	Noncomplier- reporters	Noncomplier- nonreporters	F	P
Path to Citizenship	0.568 (262)	0.571 (112)	0.529 (210)	0.555 (500)	0.472	0.702
Legal Immigration	0.522 (262)	0.545 (112)	0.535 (210)	0.508 (500)	0.631	0.595
Trust MOC	0.504 (237)	0.469 (137)	0.472 (180)	0.481 (530)	0.821	0.482
Approve of MOC	0.643 (248)	0.590 (126)	0.590 (194)	0.600 (516)	1.771	0.151
Vote Intent	0.602 (262)	0.589 (112)	0.580 (210)	0.558 (500)	1.283	0.279
Actual Vote	0.602 (305)	0.580 (69)	0.566 (136)	0.564 (574)	1.100	0.348

Compliance-reporter type is defined with respect to reporting behavior on follow-up survey item named in the first column for those subjects assigned to attend the deliberative session. Cells present sample means for variables named in the first column with numbers of observations in parentheses.

Table S3. Auxiliary causal effects estimates (study 1)

Outcome	ITT	*P*	TOB	*P*	IPW	*P*	CS	*P*	CTB
Policy attitudes									
Path to Citizenship	0.079 (0.032)	0.012	0.140 (0.055)	0.012	0.137 (0.057)	0.019	0.138 (0.061)	0.024	[−0.035, 0.171]
Legal Immigration	0.019 (0.020)	0.342	0.029 (0.027)	0.277	0.033 (0.036)	0.359	−0.005 (0.039)	0.891	[−0.061 0.060
Attitudes toward MOC									
Trust	0.067 (0.022)	0.001	0.073 (0.025)	0.004	0.114 (0.040)	0.004	0.172 (0.049)	<0.001	[0.038, 0.107]
Approve	0.042 (0.022)	0.053	0.051 (0.028)	0.071	0.065 (0.040)	0.098	0.069 (0.047)	0.137	[0.010, 0.095]
Behavior toward MOC									
Vote Intent	0.077 (0.021)	<0.001	0.115 (0.034)	0.001	0.136 (0.039)	<0.001	0.145 (0.036)	<0.001	[0.025, 0.113]
Actual Vote	0.068 (0.049)	0.176	—	—	0.104 (0.070)	0.149	—	—	[0.010, 0.189]

CS, IV regression estimates of the CACE based on the change score (bootstrapped SEs); CTB, conditional trimming bounds on the ITT; IPW, inverse probability weighted IV regression estimates of the CACE (bootstrapped SEs); ITT, ordinary least squares regression estimates of the ITT (bootstrapped SEs); *P*, bootstrapped two-tailed *P* values for immediately preceding column; TOB, tobit model estimates of the ITT (bootstrapped SEs).

Table S4. District level heterogeneity (study 1)

	Leave-one-district-out analysis					Random effects IV CACE estimates				
	Path to Citizenship	Trust	Approve	Vote Intent	Actual Vote	Path to Citizenship	Trust	Approve	Vote Intent	Actual Vote
LOO SE	0.046	0.016	0.031	0.038	0.065					
District										
1	0.284	0.185	0.164	0.199	0.406	0.119	0.089	0.069	0.161	0.216
2	0.282	0.189	0.166	0.202	0.399	0.185	0.148	0.108	0.159	0.360
3	0.279	0.190	0.170	0.202	0.403	0.162	0.125	0.102	0.155	0.132
4	0.284	0.190	0.169	0.201	0.408	0.105	0.121	0.084	0.198	0.187
5	0.276	0.187	0.165	0.199	0.406	0.101	0.119	0.087	0.125	0.053
6	0.283	0.186	0.167	0.199	0.409	0.257	0.124	0.079	0.208	0.119
7	0.286	0.195	0.170	0.198	0.404	0.091	0.118	0.037	0.113	0.104
8	0.284	0.190	0.171	0.200	0.405	0.110	0.125	0.090	0.118	0.175
9	0.284	0.189	0.173	0.197	0.405	0.073	0.127	0.062	0.157	0.141
10	0.283	0.188	0.168	0.199	0.405	0.125	0.125	0.072	0.056	0.064
11	0.281	0.192	0.170	0.196	0.405	0.175	0.113	0.072	0.052	−0.013
12	0.281	0.191	0.170	0.195	0.406	0.141	0.127	0.092	0.200	−0.103

Under the column "Leave-one-district-out analysis," leave-one-out SEs (LOO SE) are presented on the first row. In the remaining rows, there are root mean squared errors from a series of leave-one-out cross validation exercises. For each cell, a model of the dependent variable in the column title was fitted leaving out the district specified in the row number. The root mean squared prediction error for the left-out district is then presented in the cell. On the right half of the table, we present estimates of random slopes from instrumental variables regression models with outcome variable specified by column title.

Table S5. Conditional effects (study 1)

Outcome	Same party/all others	*P*	College grads/all others	*P*
Policy attitudes				
Path to Citizenship	[0.024, 0.325]	0.583	[−0.004, 0.413]	0.427
	[−0.059, 0.280]		[−0.031, 0.228]	
Legal Immigration	[−0.078, 0.124]	0.739	[−0.079, 0.100]	0.565
	[−0.053, 0.153]		[−0.048, 0.157]	
Attitudes toward MOC				
Trust	[−0.053, 0.171]	0.074	[−0.045, 0.185]	0.283
	[0.096, 0.292]		[0.055, 0.264]	
Approve	[−0.001, 0.212]	0.470	[−0.070, 0.146]	0.389
	[−0.073, 0.163]		[−0.008, 0.213]	
Behavior toward MOC				
Vote Intent	[0.055, 0.243]	0.823	[−0.022, 0.186]	0.234
	[0.007, 0.251]		[0.069, 0.278]	
Actual Vote	[−0.110, 0.289]	0.879	[−0.117, 0.282]	0.866
	[−0.092, 0.300]		[−0.090, 0.299]	

Bracketed terms are bootstrapped 95% confidence intervals. Each cell presents a pair of confidence intervals, one for the group named in the column heading (the interval above) and another for its complement (the interval below).

Table S6. Balance statistics for study 2

Covariate	Control	Treatment	*Z score*	*P*
Same party as Levin	0.452	0.504	1.267	0.205
Party (1–7)	4.281	4.368	0.491	0.624
Age in years	49.81	49.45	−0.318	0.750
Education (1–4)	3.140	3.074	−0.990	0.322
Income (1–9)	6.973	6.918	−0.248	0.804
White	0.878	0.857	−0.737	0.461
Black	0.054	0.082	1.311	0.190
Latino	0.014	0.011	−0.313	0.755
Other race	0.054	0.050	−0.250	0.802
Female	0.561	0.565	0.095	0.924
Torture (baseline)	0.643	0.650	0.270	0.787
Trust (baseline)	0.442	0.430	−0.537	0.591
Approve (baseline)	0.486	0.519	1.248	0.212
Vote Intent (baseline)	0.578	0.571	−0.244	0.808

Mean values for covariates within treatment group, along with tests for individual covariate balance.

Table S7. Means and counts by compliance/reporter types (study 2)

n = 462	Complier-reporters	Complier-nonreporters	Noncomplier-reporters	Noncomplier-nonreporters	*F*	*P*
Torture (reporting on Waterboarding)	0.663 (171)	0.917 (4)	0.616 (165)	0.669 (122)	1.597	0.189
Torture (reporting on Close Guantánamo)	0.667 (174)	1 (1)	0.616 (165)	0.669 (122)	1.188	0.314
Trust Levin	0.439 (175)	— (0)	0.390 (166)	0.475 (121)	3.585	0.029
Approve Levin	0.529 (172)	0.583 (3)	0.483 (159)	0.551 (128)	1.173	0.320
Vote Intent	0.571 (174)	1 (1)	0.542 (166)	0.607 (121)	1.268	0.285
Actual Vote	0.584 (157)	0.471 (17)	0.578 (217)	0.543 (70)	0.698	0.554

Compliance-reporter type is defined with respect to reporting behavior on follow-up survey item named in first column for those subjects assigned to attend the deliberative session. Cells present sample means for variables named in first column with numbers of observations in parentheses.

Table S8. Auxiliary causal effects estimates (study 2)

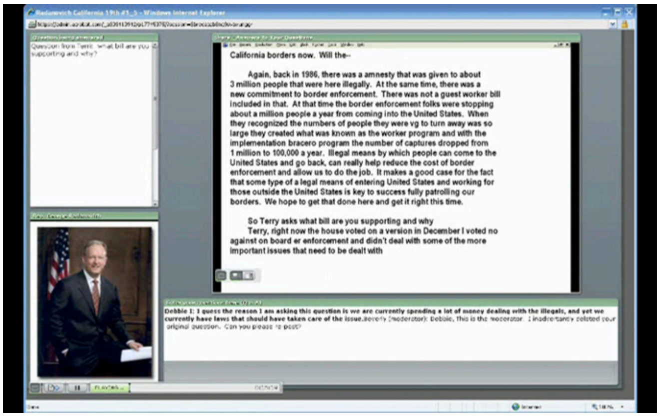
Outcome	ITT	*P*	TOB	*P*	IPW	*P*	CS	*P*	CTB
Policy attitudes									
Waterboarding	0.047	0.044	0.100	0.058	0.105	0.027	—	—	[0.059,
	(0.024)		(0.053)		(0.047)				0.090]
Close Guantánamo	−0.028	0.306	−0.054	0.298	−0.053	0.308	—	—	[−0.036,
	(0.026)		(0.050)		(0.052)				−0.004]
Attitudes toward Levin									
Trust	0.054	0.001	0.075	<0.001	0.107	0.003	0.120	<0.001	[0.034,
	(0.016)		(0.021)		(0.033)		(0.033)		0.084]
Approve	0.055	0.002	0.080	0.002	0.111	0.001	0.106	0.003	[0.017,
	(0.017)		(0.026)		(0.035)		(0.036)		0.085]
Behavior toward Levin									
Vote Intent	0.053	0.003	0.129	0.003	0.096	0.014	0.100	0.010	[0.019,
	(0.018)		(0.044)		(0.038)		(0.038)		0.105]
Actual Vote	0.053	0.092	—	—	0.099	0.116	—	—	[0.013,
	(0.031)				(0.064)				0.087]

CS, IV regression estimates of the CACE based on the change score (bootstrapped SEs); CTB, conditional trimming bounds on the ITT; IPW, inverse probability weighted IV regression estimates of the CACE (bootstrapped SEs); ITT, ordinary least squares regression estimates of the ITT (bootstrapped SEs); *P*, bootstrapped two-tailed *P* values for immediately preceding column; TOB, tobit model estimates of the ITT (bootstrapped SEs).

Table S9. Conditional effects (study 2)

Outcome	Same party/all others	*P*	College grads/all others	*P*
Policy attitudes				
Waterboarding	[−0.116, 0.099]	0.061	[−0.111, 0.199]	0.454
	[0.026, 0.284]		[0.004, 0.234]	
Close Guantánamo	[−0.157, 0.138]	0.335	[−0. 310, 0.031]	0.244
	[−0.249, 0.024]		[−0.132, 0.125]	
Attitudes toward MOC				
Trust	[−0.033, 0.136]	0.289	[−0.036, 0.142]	0.179
	[0.034, 0.198]		[0.055, 0.216]	
Approve	[−0.005, 0.202]	0.919	[−0.075, 0.151]	0.125
	[0.024, 0.190]		[0.063, 0.232]	
Behavior toward MOC				
Vote Intent	[−0.071, 0.132]	0.156	[−0.016, 0.165]	0.470
	[0.037, 0.222]		[0.025, 0.224]	
Actual Vote	[−0.232, 0.154]	0.068	[−0.050, 0.384]	0.717
	[0.021, 0.430]		[−0.099, 0.318]	

Bracketed terms are bootstrapped 95% confidence intervals. Each cell presents a pair of confidence intervals, one for the group named in the column heading (the interval above) and another for its complement (the interval below).



Movie S1. A short clip from one of the deliberative sessions, an online town hall with Rep. George Radanovich (R-CA).

Movie S1