

Does Grassroots Lobbying Work?

A Field Experiment Measuring the Effects of an e-Mail Lobbying Campaign on Legislative Behavior

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There are few reliable estimates of the effect of grassroots lobbying on legislative behavior. The analysis in this article circumvents methodological problems that plague existing studies by randomly assigning legislators to be contacted by a grassroots e-mail lobbying campaign. The experiment was conducted in the context of a grassroots lobbying campaign through cooperation with a coalition of groups lobbying a state legislature. The results show that grassroots lobbying by e-mail has a substantial influence on legislative voting behavior. The article concludes with a number of possible extensions of the study's design to other forms of lobbying and other problems in political science.

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Surveys of interest group activities have found that many groups use grassroots or outside lobbying strategies. That is, many groups attempt to mobilize members to contact a policymaker to influence a specific policy outcome. Kollman (1998) in a survey of interest groups finds that 56% of groups surveyed regularly mobilize group members and an additional 38% of groups occasionally mobilize members. Similar percentages of groups organize letter-writing campaigns. Nownes and Freeman (1998)

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find that large percentages of groups use grassroots lobbying at the state level. For example, 73% of labor unions and 90% of citizen groups in the sample mount grassroots lobbying efforts. These percentages are similar to earlier estimates: In Schlozman and Tierney's (1983) survey of a sample of 175 Washington lobbying organizations, 84% reported orchestrating letter-writing campaigns, and 80% used grassroots lobbying efforts.

The Internet has allowed many citizen groups to carry out their outside lobbying activities online. The Internet has made it cheaper for groups to form lists and easier for activists to join groups and contact legislators through e-mail. Many groups use software packages to maintain lists, alert e-mail activists about upcoming bills, and make it easy for individuals to contact their legislators. Other groups have developed their own software to develop and maintain e-mail lists and to conduct grassroots e-mail campaigns.

How effective are these grassroots campaigns?¹ Despite the pervasiveness of grassroots lobbying, there have been few recent studies of the impact of outside lobbying, perhaps because earlier studies encountered methodological problems, in particular endogenous regressors (see Smith, 1995, for a review). Field experimentation can provide accurate estimates of the effects of contacting legislators, avoiding the problem of endogenous regressors by randomly assigning legislators to treatment and control groups. This method has experienced a resurgence in recent years in political science (e.g., Gerber & Green, 2000; Green & Gerber, 2004). Through random assignment legislative contacts, field experimentation allows researchers to estimate the effects of lobbying campaigns without the specification problems that plague earlier studies.

In this article, I will present results from a field experiment involving a grassroots e-mail lobbying campaign. In early 2006, a coalition of public health-related groups, the Clean Air Works for New Hampshire coalition, aimed to promote smoke-free workplace legislation in New Hampshire. Two groups in the coalition, the American Cancer Society (ACS) and the Campaign for Tobacco-Free Kids (TFK), asked activists to contact legislators to influence voting on the bill. The objective of the field experiment in this article is to estimate the effectiveness of e-mail campaigns in influencing legislative behavior.

In the first section, I will review the theoretical and empirical literature on outside lobbying. The second and third sections will provide some background on the campaign and will describe the field experimental design, respectively. The method involves randomly assigning some legislators to a control group who will not be contacted by the activists. The fourth section

will discuss the results. I find that e-mail has a large influence on legislative behavior. The final section concludes with some possible extensions for this method.

Research on Outside Lobbying

Why should grassroots lobbying work? Most theoretical work has suggested that grassroots lobbying informs legislators about the salience of an issue, as citizen contacts to legislators indicate that constituents are willing to take a costly action to inform their legislators about their preferences on an issue (Ainsworth, 1993; Ainsworth & Sened, 1993; Kollman, 1998). Note that according to this theory, the cost of sending an e-mail may be so low that legislators do not interpret these communications as a strong indicator of constituent concern about an issue. A related view of grassroots lobbying is that this method can signal to legislators that groups are able to mobilize supporters, and may be able to do so in the next election, giving the legislator an incentive to support the groups' preferred policies (Caldeira & Wright, 1998). A second mechanism by which grassroots lobbying could work is by increasing the ability of constituents to monitor legislative behavior (Arnold, 1990; Goldstein, 1999). That is, grassroots lobbying campaigns simultaneously inform group members of the actions that a legislator is taking on an issue (such as legislative votes) and let legislators know that group members are informed and paying attention to legislative actions.

These theories describe mechanisms by which grassroots lobbying might work, but they have not been rigorously tested. Most of the evidence about the effectiveness of grassroots campaigns is anecdotal. Truman (1951), writing before many technological and strategic developments in grassroots lobbying campaigns, suggested that direct contacts with legislators were likely to be more effective than grassroots lobbying. Kollman (1998) and Goldstein (1999), using recent examples, claim instead that outside lobbying tactics can be very effective. State legislators and lobbyists suggest that few communications are necessary to influence state legislators (e.g., Rosenthal, 2001).

There have been a number of attempts to estimate the effect of grassroots lobbying on legislative behavior. These methods typically involve regressing some form of legislative behavior, such as voting, on a measure of grassroots lobbying. There are two major problems with these studies. One problem concerns the measurement of outside lobbying efforts. Early

studies of the effect of citizen groups on legislative voting used group size as a proxy for group lobbying activity (e.g., Kau & Rubin, 1982). However, this variable may not accurately measure the extent to which these groups actually conduct grassroots lobbying. More recent studies have relied on self-reports of interest group behavior. For example, Fowler and Shaiko (1987) rely on self-reports from environmental group members to determine which senators were contacted by activists. Self-reports of political behavior such as voting, however, are known to be unreliable (e.g., Sigelman, 1982). The poorly measured independent variable may account for the mostly null results of the Fowler and Shaiko study. Other studies rely on self-reports from interest group leaders, who report on what methods of lobbying were used and which legislators were targeted (e.g., Caldeira & Wright, 1998; Wright, 1990; see also Evans, 1996). As these data are collected after votes, groups may provide self-serving reports as to the legislators they lobbied, claiming to have lobbied legislators who ultimately supported the bills in questions. Langbein and Lotwis (1990), instead of using self-reports from group activists or leaders, rely on self-reports from legislators and their staffs, who report whether they were lobbied. All of these self-report-based indicators of grassroots lobbying may be tainted by the incentives of respondents to misreport or by respondents not accurately remembering outside lobbying strategies.

A more serious problem is that group activism, even if measured accurately, may be endogenous. For example, in studies that regress legislative voting behavior on whether the legislator is a target of a lobbying campaign and controls, targets of the lobbying campaign may be more favorable to the relevant policy, meaning that estimates of the effect of lobbying will be biased. This is a problem that affects all of the studies listed above, as interest groups do not randomly select which legislators to lobby. Previous studies have attempted to control for legislative support with legislative voting scores and other variables, but these variables imperfectly control for legislative support for a particular measure.

Attempts to estimate the impact of grassroots lobbying may therefore involve specification error. The direction of bias is unclear, as the literature on lobbying strategy does not provide consistent results concerning the targets of lobbying campaigns. Early literature on lobbying suggests that lobbyists tend to contact only policymakers who agree with their position on an issue (Bauer, de Sola Pool, & Dexter, 1972; see also Hojnacki & Kimball, 1998, 1999). Later results show instead that lobbyists contact legislators who are undecided as these members are most likely to be swayed by lobbying (Rothenberg, 1992; Schlozman & Tierney, 1986; Smith, 1984;

Wittenberg & Wittenberg, 1989). The formal model of Austen-Smith and Wright (1992, 1994) suggests that group lobbying is conditional on the actions of other interest groups and that in certain circumstances groups will lobby supportive legislators. Given these conflicting findings, the direction of bias in observational studies of the effectiveness of outside lobbying is unknown; it is not clear whether groups lobby supportive legislators, thereby inflating estimates of the effectiveness of grassroots lobbying, or whether groups target legislators who are undecided about an issue.

Random assignment of legislators to treatment and control can eliminate the potential bias that results from groups strategically choosing whom to lobby. Field experiments have been used mainly to study mass political behavior, such as voter turnout (e.g., Green & Gerber, 2004), but the method is also suited to study the effects of lobbying on legislative behavior. The description of the study below will show that field experimental designs can also improve on the measurement of lobbying contacts. In the current design, targets of the lobbying campaign are determined ahead of time, eliminating the need for retrospective self-reports. The following sections will explain the campaign and the design of the field experiment, respectively.

The Smoke-Free Workplace Campaign

This article analyzes the effects of a grassroots lobbying campaign targeting the lower chamber of the New Hampshire General Court. The House of Representatives in New Hampshire is the largest state legislature and the most amateur by a number of standards. Only legislative leaders and some committees have staffs. For rank and file members of the house, salaries are US\$100 per year, and no office space is available to rank and file members (Fistek, 1997).

The state's political system has been described as "porous and accessible" (Egbert & Fistek, 1993, p. 203). This is probably in part because of the amateur nature of the legislature, but other factors could contribute as well. Political parties are weak and disorganized (Fistek, 1997), but there are a large number of groups competing for access, meaning that no single group dominates the legislative process (Egbert & Fistek, 1993; Morehouse, 1981). Most interest groups spend only in modest amounts in lobbying the legislature (Egbert & Fistek, 1993). Interviews with key political actors determined that citizen groups are powerful, not as influential as realtors, housing, the banking lobby, and other predevelopment groups, but as powerful as state

and school employees, the medical professional lobby, fish and game interests, and public utilities (Egbert & Fistek, 1993).

In the spring of 2006, Clean Air Works for New Hampshire, a coalition of antitobacco groups, lobbied the New Hampshire legislature to pass a smoke-free workplace bill. The group envisioned a bill that would ban smoking in all workplaces in the state. The Commerce Committee produced a bill that would ban smoking in restaurants and bars. Coalition allies on the committee suggested that this scaled-down bill would be more likely to pass both chambers.

Grassroots e-mail campaigning was a part of the coalition's legislative strategy when the bill went to the floor. Two of the groups in the coalition, the ACS and the Campaign for TFK, e-mailed group members to contact their legislators. The groups' software allowed activists to click on a Web address in the e-mail that would direct them to a Web site. The activist could then click on a button on the Web site that would automatically e-mail all of the activist's legislators in the state house. The software kept records of the number of e-mails that went to each legislator.

Sample texts of the e-mails sent to the activists during the campaign as well as those forwarded from activists to legislators are included in the appendix. Although there is a standard text for e-mails sent to legislators, activists had the option of adding a personal message to their legislators. There were 756 e-mails sent to legislators during the course of the campaign that lasted from February 26 until March 21.

There were five roll call votes on the bill. The coalition supported a committee amendment to the bill that would clarify the language of the bill and change the start date of the ban. The coalition also supported the vote on final passage of the bill. There were two floor amendments to the bill that the coalition opposed. One would have extended the ban to private clubs and the other would have prohibited smoking when a child is present, including in private residences. The coalition also opposed a vote to table the bill, which would have postponed the vote on the bill's final passage indefinitely.

The coalition and other groups were active in many other ways during the campaign. The coalition directly lobbied members, ran ads in the newspaper, and held legislative luncheons where they discussed their position on the bill with legislators. The e-mail messages, then, take place in the context of a larger campaign with many other efforts to sway legislators. This may diminish the influence of the e-mail campaign in swaying legislators' votes but should enhance the external validity of the study as most e-mail grassroots lobbying will take place in the context of a larger campaign.

The next section discusses the design of the experiment. The groups' software allowed the groups to deselect legislators who had been randomly assigned to a control group, preventing them from receiving e-mails from activists. This feature of the software is exploited in the experimental design, which randomly assigns some legislators to a control group who will not be contacted by e-mail.²

Field Experimental Design

The design of the field experiment involved randomly assigning some legislators to a treatment group to be contacted by activists from ACS and TFK and others to a control group who were not contacted by activists from the two groups.³ This section first discusses the matched pair design of the study; because the lower chamber of New Hampshire's state legislature has multimember districts, I was able to stratify legislators by district, ensuring that the treatment and control groups had similar constituencies. The section will then describe the sample frame of the study, the distribution of legislators assigned to treatment and control, the implementation of the study, and some potential problems with internal validity and external validity.

Matched Pair Design

One of the unique features of the design is that it matched legislators by party and by district and then randomly assigned them to treatment or control. The design of the experiment exploits the New Hampshire state legislature's multimember districts. As of 2002, in New Hampshire's lower chamber, there are 103 districts represented from between 1 and 13 legislators. The median district is represented by 3 legislators. Stratifying the legislators by district and party and randomly assigning members of each stratum to treatment and control ensures that the party composition and constituencies of the treatment and control groups are similar. The multimember legislature allowed matching on district, which is important in a study of constituent contacts on policy as there are no systematic differences between the treatment and control groups other than whether the legislator was exposed to grassroots lobbying contacts.

In the regression analyses below, some models include strata indicators. Because legislators were assigned to strata by party and district, including strata indicators controls for all district-specific effects (e.g., district partisanship, percentage of smokers in district, etc.).⁴

Legislators Excluded From the Study

A number of legislators were excluded from the study. Legislators without e-mail addresses were excluded from the field experiment. Members of the Commerce Committee and the party leadership were also excluded as the coalition wished to contact these members directly. The coalition had kept track of the legislators' positions on the bill prior to the e-mail lobbying campaign, recording which legislators had publicly taken a position on the bill in the media or who had stated their support or opposition to the bill directly to one of the coalition members. Legislators who were recorded at the start of the campaign as either supporting or opposing the bill were excluded from the study. Therefore, only legislators who were undecided at the start of the grassroots campaign were included in the study.

Members who do not have another member of the same party in their district were also excluded from the study. This is because the groups did not want to have any districts where no members were contacted. Because the groups wanted to limit the number of districts involved in the study, I randomly selected 10 of the districts with two members and eliminated these.⁵

The largest group of members excluded were those who had already taken a position on the bill in question or, in virtue of their committee assignment, had already been lobbied heavily by the coalition. The coalition did not expect to move these legislators' opinions with the grassroots lobbying campaign. Therefore, to the extent that prior to a floor vote, groups target grassroots lobbying appeals toward those legislators who are likely to be influenced by a grassroots lobbying campaign, this should not affect the external validity of the study. That is, the study applies to legislators who are likely targets of grassroots lobbying campaigns.

Treatment Assignment

Table 1 presents the breakdown of treatment assignment by strata. Members in each stratum had an equal probability of being assigned to the treatment or control group. There were 71 members assigned to the control group and 72 assigned to the treatment group. The table lists the party and district of each stratum. (Districts are referred to in the lower chamber by county and district number within the county). The sample here is 25% Democrat (note that this is less than the 38% of Democrats in New Hampshire's house as a whole). Democrats were assigned in roughly equal proportions to treatment and control at 27% and 24%, respectively.

Table 1
Assignment to Treatment and Control by Stratum

Party	District	Control	Treatment
Republican	Belknap 3	1	1
Republican	Belknap 4	2	1
Republican	Belknap 5	2	2
Republican	Carroll 3	1	2
Republican	Carroll 4	2	1
Republican	Grafton 1	1	1
Republican	Grafton 3	1	1
Republican	Grafton 5	1	1
Republican	Hillsborough 3	1	2
Republican	Hillsborough 4	1	2
Republican	Hillsborough 5	1	1
Republican	Hillsborough 7	2	1
Republican	Hillsborough 17	2	1
Republican	Hillsborough 18	2	3
Republican	Hillsborough 19	2	3
Republican	Hillsborough 21	1	1
Republican	Hillsborough 26	1	2
Republican	Hillsborough 27	4	5
Republican	Merrimack 2	0	2
Republican	Merrimack 6	2	0
Republican	Merrimack 8	1	1
Republican	Merrimack 9	1	1
Republican	Rockingham 1	2	2
Republican	Rockingham 3	2	3
Republican	Rockingham 4	4	4
Republican	Rockingham 5	4	3
Republican	Rockingham 7	2	2
Republican	Rockingham 8	2	2
Republican	Rockingham 9	0	2
Republican	Rockingham 13	2	0
Republican	Strafford 3	2	2
Democrat	Coos 4	2	1
Democrat	Grafton 9	2	0
Democrat	Hillsborough 14	1	1
Democrat	Hillsborough 22	0	2
Democrat	Hillsborough 25	1	1
Democrat	Hillsborough 26	2	0
Democrat	Rockingham 16	2	3
Democrat	Strafford 2	2	2
Democrat	Strafford 3	1	1
Democrat	Strafford 6	2	1
Democrat	Strafford 7	1	2

(continued)

Table 1 (continued)

Party	District	Control	Treatment
Democrat	Sullivan 1	1	1
Democrat	Sullivan 2	1	1
Democrat	Sullivan 4	1	1
Total		71	72

Note: Members were stratified by party and district; members of each stratum were assigned to treatment or control with equal probability. Districts in New Hampshire's lower chamber are denoted by county and district number.

Study Implementation

The implementation of the study was simple: The groups sent action alerts to all activists. The lobbying software used in the campaign allowed the groups to deselect legislators in the control group so that these legislators were blocked from receiving any e-mails. This meant that the coalition could simply be provided with the list of randomly selected control legislators, and the groups could use the software to deselect these members, thereby preventing them from receiving any e-mails. All other legislators were contacted as they would have been had the study not been conducted.

For example, suppose three activists in a particular district click on the appropriate button to send an e-mail to their legislators. Suppose that the activists have one house member who has been assigned to the treatment group and one who has been assigned to the control. The treatment member would receive all three of the e-mails, and the control group member would receive none. The software allowed the experimental design to be implemented easily, as the coalition had to just point and click to deselect control legislators from the lobbying campaign. As discussed below, the record of e-mails forwarded to legislators confirms that the study was implemented properly.

Experimental Design and Internal Validity

Despite the random assignment to treatment and control, there are potential threats to internal validity. One threat to internal validity is that legislators from the same district and party may influence one another. That is, a member of the treatment group may convince a member in the same party and district that the smoking ban is a popular and salient measure. If the latter is a member of the control group, the treatment could indirectly

affect members of the control group. This may bias the results downward. It is unclear to what extent this occurred, so the results below should be interpreted as a lower bound on the effectiveness of the treatment.

Another problem is that groups, knowing that some legislators are not contacted, may want to compensate by more heavily lobbying legislators in the control group in other ways. There are reasons that this is implausible in this particular experiment. Certain aspects of the campaign were conducted in a compartmentalized fashion, meaning that other group members were not aware which legislators were placed in the control group. For example, the e-mail software for TFK was operated by an associate in Washington, D.C., far from the rest of the campaign in New Hampshire. This was the only member of the group who was aware of the composition of the control group. The author participated in coalition phone conferences and was on the e-mail list for coalition leaders during the later portions of the campaign, and it was never apparent that any type of compensating strategy was used to influence control group members. In any case, to the extent that this occurred, it suggests again that the estimates produced below are a lower bound on the true effect of lobbying on legislative behavior.

External Validity

There are a number of reasons that the results of this experiment may not be generalizable to other contexts. New Hampshire's legislature is a citizen legislature. There is high turnover in the lower chamber, and there are no staffs for rank and file members, meaning that legislators do not have the resources to learn about issues. This may make legislators more dependent on interest groups for information. Because the legislative districts are small, there are probably fewer constituent and other lobbying contacts made to legislative offices. Also, there was little opposition to the coalition on this issue. The magnitude of the effects estimated here may therefore be smaller than in other settings.

In defense of the current context, because there was little organized opposition and fewer constituent contacts to legislators, the current context is ideal to determine whether it is possible for grassroots lobbying to work. A null result in this case would suggest that e-mail lobbying campaign, a low-cost type of grassroots lobbying, would not work in any state or national legislature, with or without significant opposition. A statistically significant result would suggest that grassroots lobbying can work in at least some contexts, even though it may be vulnerable to other influences such as opposing groups' lobbying efforts.

In any case, the generalizability of these results to other contexts cannot be determined without further experimentation in different contexts. Possibilities for future research to address this concern are discussed in the conclusion.

Results

There were a modest number of e-mails sent to each legislator in the treatment group. A mean of 3 e-mails were sent to each legislator with a minimum of 0 and a maximum of 11.⁶ Ninety percent of legislators in the treatment group received at least 1 e-mail. According to the groups' records, no e-mails were sent to the members of the control group, confirming that the experimental design was implemented correctly.

The next subsection will compare legislative voting in the treatment and control groups. The following subsection will analyze the treatment effect using regression analyses.

Comparison of Legislative Voting in Treatment and Control Groups

What were the effects of the campaign on legislative voting behavior? Table 2 presents the mean support of members of the control and treatment groups for each of the five roll call votes on the bill. The votes are grouped according to whether the vote was on an amendment or on a pivotal vote. The first pivotal vote involves a motion to table the amendment that would have suspended the bill indefinitely and could have effectively killed the bill; the coalition opposed this motion. The second pivotal vote is the final passage vote. The treatment should move legislators toward the coalition-supported position on these votes. Recall that the amendments involve clarifying changes to the bill (the committee amendment) or extensions of the smoking ban (the floor amendments). It is unclear what effect the campaign would have on these amendments, as they concern small changes to the bill (the committee amendment), or could potentially involve strategic voting (both floor amendments). For example, members wishing to kill the bill may vote for a stronger ban, hoping to amend the bill in a way that would prevent final passage. The coalition, aware of this point, opposed the two floor amendments although they were apparently strengthening the ban. This ambiguity makes it difficult to anticipate the effect of the campaign (that is, should the campaign increase a legislator's apparent opposition to smoking or increase a legislator's willingness to support the coalition?).

Table 2
Mean Voting Behavior of Treatment and Control Groups

	Control	Treatment	Difference of Means <i>p</i> Value (One-Tailed)
Amendment votes			
Committee amendment	0.41(.06)	0.46(.07)	.31
Floor Amendment 1	0.85(.05)	0.80(.05)	.80
Floor Amendment 2	0.87(.04)	0.87(.04)	.47
Sum of three amendment votes	2.15(.09)	2.14(.11)	.53
Pivotal votes			
Vote to table	0.48(.06)	0.62(.06)	.07
Final passage	0.42(.06)	0.50(.07)	.19
Sum of two pivotal votes	0.92(.12)	1.12(.12)	.12
Sum of five votes	3.08(.20)	3.26(.21)	.21

Note: Cell entries are mean support on each vote. Standard errors in parentheses. All votes are coded so that 1 = *coalition-supported position*. Votes 2, 3, and 4 reversed (*yea* = 0, *nay* = 1). Sum of three amendment votes is an additive scale of the committee amendment and two floor amendments; 3 = *support coalition on all 3*, 0 = *support coalition on none*. Sum of two pivotal votes is an additive scale on the vote to table the bill and the final passage; 2 = *support coalition on both votes*, 0 = *oppose coalition on both*. Sum of five votes adds recoded votes; 5 = *support coalition on all 5 votes*, 0 = *oppose coalition on all 5*. Sum of two pivotal votes is an additive scale on the vote to table the bill and the final passage; 2 = *support coalition on both votes*, 0 = *oppose coalition on both*.

Additive scales are created for the pivotal votes and the amendments as well as all five votes. All votes in the table have been coded so that the coalition's favored position is higher. That is, Votes 1 and 5 are coded 1 for *yea* and 0 for *nay*; Votes 2 through 4 are reversed (that is, *nay* = 1 and *yea* = 0). The additive scales are coded in the same way. Legislators not voting are excluded from the analysis.

The effect of the campaign appears greatest for the two pivotal bills. The estimated effects on the vote to table, the final passage, and the sum of these two pivotal votes have low *p* values but are not statistically significant at conventional levels. The other results are less impressive. This is reflected in the sum of the amendment votes: The mean scores for the treatment and control groups are the same. The results in the table suggest that there may be an influence of the lobbying campaign on voting for the two pivotal votes but that amendments were largely not affected by the campaign. This is not surprising given the low stakes of the first amendment vote and the potential for strategic voting on the other amendments.⁷

The remainder of this section will use regression analysis to further explore the effects of the campaign on the two pivotal votes. Although the mean levels of support for these votes are not statistically distinguishable between treatment and control groups, regression analysis controlling for past support for tobacco legislation may yield statistically significant results. Regression analysis will allow us to control for strata. As mentioned above, as these strata are based on party and district, individual district effects are controlled for when strata indicators are included in the model.

Regression Results

The dependent variable of the analysis is a 2-vote score of pivotal votes. Legislators could support the coalition on 0, 1, or 2 pivotal votes. Ordered probit is the appropriate model as the dependent variable is ordered.⁸ The base category for the analysis is supporting the coalition on neither of the two votes.

The first model regresses the vote scale on the indicator for treatment group with no covariates. In the last three models, covariates are added. Because the legislators were randomly assigned to treatment and control groups, and therefore should have similar expected probabilities of supporting antismoking legislation, a large number of covariates is not necessary. Control variables include an indicator for party (1 = *Democrat*, 0 = *Republican*) and an additive scale on three tobacco-related roll calls in 2005. Tobacco votes is the sum of three votes on tobacco-related legislation: HB 645, a vote on fire-safe cigarettes (1 for a *yes* vote, 0 *otherwise*); HB 535, a tobacco tax bill (1 = *yes*, 0 = *otherwise*); and an amendment to the tobacco tax bill (0 = *yes*, 1 = *otherwise*). Note that the last vote is reversed. Members who did not vote were coded as voting nay on each issue to increase the sample size. A high score on the scale indicates opposition to tobacco (scale reliability coefficient = .69). All three of these votes occurred prior to treatment. Model c includes indicators for counties. There are nine different counties in the study, each of which includes a number of legislative districts. Model d includes indicators for strata; as mentioned above, the legislators were stratified according to party and legislative district.

The models show that the e-mail lobbying campaign influenced legislative voting behavior on the two pivotal roll calls. The coefficient for the treatment effect is statistically significant in all models except for the first model without covariates. This may be because controlling for past tobacco

Table 3
Ordered Probit Estimates of the Effect of e-Mail
Contacts on Vote to Table Bill and Final Passage

Model	a	b	c	d
Treatment	0.254 (0.218)	0.543* (0.246)	0.609* (0.265)	0.768* (0.347)
Democrat		0.090 (0.302)	0.509 (0.414)	-1.46 (1.17)
Tobacco votes		0.724*** (0.133)	0.726*** (0.143)	0.819*** (0.203)
Cut 1	-0.041 (0.159)	1.36* (0.298)	0.953 (0.488)	0.837 (0.875)
Cut 2	0.255 (0.159)	1.76* (0.311)	1.42* (0.494)	1.44 (0.878)
N	120	120	120	120
LR chi-square	0.136	48.72	67.67	100.72
Probability > chi-square	.244	.000	.000	.000
Pseudo R ²	.006	.209	.290	.432
County fixed effects?	N	N	Y	N
Strata fixed effects?	N	N	N	Y

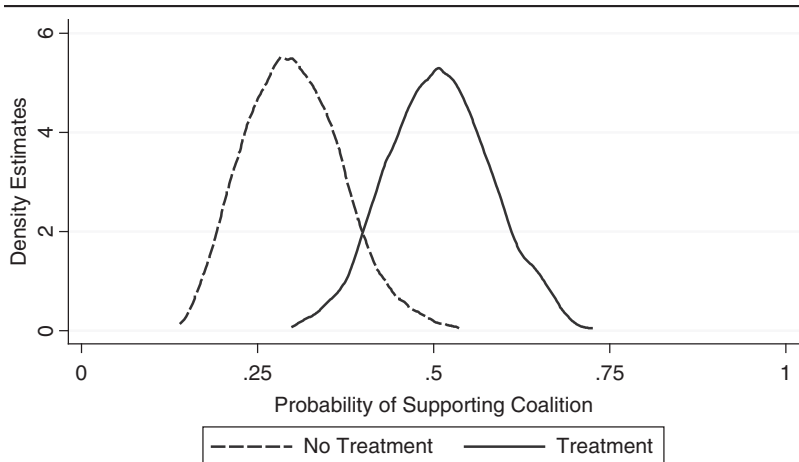
Note: Standard errors in parentheses. Dependent variable equals 2 for supporting the coalition-supported position on motion to table and final passage, 1 for supporting coalition on one of these bills, and 0 for not supporting the coalition-supported position on either of these bills. Nonvoters were excluded. *Treatment* is coded 1 if members were randomly assigned to be e-mailed by constituents and 0 otherwise. Tobacco votes is the sum of three votes on tobacco-related legislation: HB 645, a vote on fire-safe cigarettes (1 for a *yes* vote, 0 *otherwise*); HB 535 a tobacco tax bill (1 = *yes*, 0 = *otherwise*); and an amendment to a tobacco tax bill (0 = *yes*, 1 = *otherwise*); note that the last vote is reversed. A high score on the scale indicates opposition to tobacco (scale reliability coefficient = .69). LR = Likelihood ratio.

* $p < .05$. ** $p < .01$. *** $p < .001$, two-tailed.

votes may provide more accurate estimates as the control group had a slightly higher mean of past support for tobacco legislation.⁹ The last three models, which control for past tobacco votes, produce statistically significant results at conventional levels. This result is robust to the addition of county and strata indicators.

Note that a number of legislators who did not vote on either of the two pivotal votes were excluded from the regression. Including these legislators does not substantively influence the results. In appendix Table A1, the same

Figure 1
Predicted Probability of Supporting Coalition on Both Pivotal
Votes Kernel Density of Ordered Probit Predictions



Based on ordered probit estimates from Table 3, Model b. Control variables include an indicator for party (1 = *Democrat*, 0 = *Republican*) and an additive scale on three tobacco-related roll calls in 2005 including HB 645, a vote on fire-safe cigarettes (1 for a *yes* vote, 0 *otherwise*) HB 535, a tobacco tax bill, (1 = *yes*, 0 = *otherwise*), and an amendment to the tobacco tax bill (0 = *yes*, 1 = *otherwise*). Note that the last vote is reversed. Members who did not vote were coded as voting nay on each issue to increase the sample size. A high score on the scale indicates opposition to tobacco (scale reliability coefficient = .69). All three of these votes occurred prior to treatment.

regressions are run coding these legislators as not supporting the coalition on each of these pivotal votes. The results are similar to the results in Table 3: When including a control for past tobacco voting, the treatment had a statistically significant effect on supporting the coalition position.¹⁰

What is the magnitude of the treatment effect? The increase in the probability of supporting the coalition on both pivotal votes (estimated in Model b)¹¹ given assignment to the treatment group is .2, with a 95% confidence interval (CI) of (.029-.377).¹² Figure 1 plots kernel density estimates (which are smooth histograms) of the probabilities of supporting the coalition on both

pivotal votes with and without grassroots lobbying. The probabilities of supporting the coalition are estimated from Model b in Table 3,¹³ setting *Democrat* to 0 (the median party identification is Republican) and the tobacco vote score to its mean. The treatment variable is set to 0 in the *no treatment* condition and 1 in the *treatment* condition. The figure shows the magnitude of the effect of the lobbying campaign on the probability of supporting the coalition on both of the pivotal votes.

Because there have been no previous field experimental analysis of different effects on legislative voting, it is hard to contextualize the magnitude of the effects, other than to observe that a 20 percentage point effect appears large. However, in Model b in Table 3, the marginal effect of the treatment is approximately .2 and the estimated marginal effect of an additional past vote to control tobacco is a little under .3. (Recall, however, that past votes on tobacco bills were not randomized, so this effect should not be interpreted as the causal impact of supporting a tobacco control measure.) The effect of belonging to the Democratic Party according to the observational estimate in the model is .04 (not significant). Placing the .2 effect in this context suggests that the treatment was very influential in legislators' voting decisions.

Table 4 presents probit regressions for each individual pivotal vote, with separate regression analyses for the vote on the motion to table and on the final passage of the bill. The results are similar to Table 3: When adding control variables, the treatment effects are statistically significant. The estimated increase in probability in voting for the coalition's supported position on the motion to table was .262 with 95% CI of (.066-.442) and on the final passage was .194 with 95% CI of (-.001-.382; estimated from Models b and f, respectively).

The results from this experiment suggest that outside lobbying has a large effect on legislative voting. The results are not surprising given that many groups use the tactic to influence legislative behavior and that a number of theories have described intuitively plausible mechanisms through which grassroots lobbying could work. The magnitude of the effect is large, with approximately a .2 effect or larger on voting on either of the pivotal votes. In summary, the study has shown that grassroots lobbying works and can have a large effect on legislative behavior. As mentioned above, this magnitude may not be generalizable to other legislatures or other issues. It will take more studies to determine the magnitude of the effects of lobbying campaigns in other settings.

Table 4
Probit Estimates of the Effect of e-Mail Contacts on Vote to Table Bill and Final Passage

Dependent Variable	Motion to Table (1 = <i>Oppose</i>)				Final Passage Votes (1 = <i>Support</i>)			
Model	a	b	c	d	e	f	g	h
Treatment	0.337 (0.229)	0.685* (0.273)	0.809** (0.307)	0.958* (0.415)	0.204 (0.228)	0.506 [†] (0.268)	0.559 [†] (0.290)	0.893* (0.439)
Democrat		-0.102 (0.338)	0.412 (0.474)	-7.42 (5125)		0.238 (0.320)	0.618 (0.449)	-0.350 (1.28)
Tobacco votes		0.776*** (0.149)	0.790*** (0.166)	0.952*** (0.248)		0.703*** (0.148)	0.730*** (0.162)	0.796** (0.241)
Constant	-0.040 (0.159)	-1.52 (0.325)	-0.815 (0.566)	5.04 (5125)	-0.204 (0.161)	-1.69 (0.340)	-1.42 (0.535)	-1.64 (0.938)
N	122	122	122	122	122	122	122	122
LR chi-square	2.18	45.08	63.07	90.07	0.80	43.17	58.69	97.33
Probability > chi-square	.140	.000	.000	.000	.371	.000	.000	.000
Pseudo R ²	.013	.268	.376	.536	.005	.257	.349	.578
County fixed effects?	N	N	Y	N	N	N	Y	N
Strata fixed effects?	N	N	N	Y	N	N	N	Y

Note: Standard errors in parentheses. Dependent variable equals 1 for supporting the coalition-supported position and 0 for opposing the coalition-supported position. Nonvoters were excluded. *Treatment* is coded 1 if members were randomly assigned to be e-mailed by constituents and 0 otherwise. Tobacco votes is the sum of three votes on tobacco-related legislation: HB 645, a vote on fire-safe cigarettes (1 for a *yes* vote, 0 *otherwise*); HB 535, a tobacco tax bill (1 = *yes*, 0 = *otherwise*); an amendment to a tobacco tax bill (0 = *yes*, 1 = *otherwise*); note that the last vote is reversed. A high score on the scale indicates opposition to tobacco (scale reliability coefficient = .69). LR = Likelihood ratio.

* $p < .05$. ** $p < .01$. *** $p < .001$, two-tailed. [†] $p < .1$.

Conclusion

Numerous grassroots campaigns are conducted by interest groups at the state and national level each year. This article has described a simple research design to estimate the effects of these efforts on public policy. The results here suggest that the effect of e-mail campaigns on legislators is substantial. This confirms the effect on policy of a fairly common form of participation; 28% of the public claims to have contacted an election official in the previous 5 years (General Social Survey 1972-2002), and many of these contacts result from the mobilizing efforts of citizen groups (Kollman, 1998).

Other methods of contacting legislators, such as phone campaigns, may be more effective. This is suggested by polls that show that legislators pay more attention to phone calls and personal visits than e-mails (e.g., Cornfield, 1999-2000). Such a result would also be consistent with research on vote drives (e.g., Green & Gerber, 2004) that has shown that more personal contacts with individuals, such as face-to-face contact, is far more effective than less personal contacts, such as e-mail.

As mentioned above, the campaign studied here was intended to influence a citizen legislature. Perhaps a larger number of e-mails or a more personalized form of communication is necessary to influence more professionalized legislatures. Also, there may be other factors particular to this campaign that preclude the generalization of the results to other outside lobbying efforts. The literature suggests many conditions that may aid or hinder lobbying campaigns, including the presence of an organized opposition (Fowler & Shaiko, 1987; Schlozman & Tierney, 1986), lobbying in a multigroup coalition (Fowler & Shaiko, 1987), and public support for the issue (Rothenberg, 1992). However, empirical support for each of these conditions in lobbying campaigns is lacking (see the discussion in Smith, 1995). A series of field experimental studies could improve our understanding of the conditions for the effectiveness of grassroots lobbying and constituent behavior, as has been done for get-out-the-vote drives (e.g., Green & Gerber, 2004).

Note that the results do not distinguish between the different theories of the mechanism by which grassroots lobbying works. That is, although the results show that grassroots lobbying works, they do not show why it works. The latter is a much more difficult question to answer. It is hard to conceive of a single experimental design that could determine whether grassroots lobbying works because, say, legislators perceive constituent contacts in a grassroots lobbying campaign as a costly signal from a

constituent or perceive contacts as a signal that constituents are informed about their legislator's actions on a particular issue. However, a series of experiments could shed light on this issue. For example, suppose that more costly methods of sending messages (e.g., handwritten letters, phone calls) are more effective than less costly messages (e.g., e-mails). This would support the theory that contacts are a signal of an issue's salience because more costly actions presumably are more effective because they indicate a greater willingness on the part of constituents to take an action on that issue. Although determining the mechanism by which lobbying works is difficult, a series of experiments could provide evidence as to the most plausible causal mechanism.

In addition to determining the effects of different interest group strategies, the method outlined in this article could be used to answer broad questions in political science. For example, political scientists have claimed that disproportionate participation in the political process by individuals with high socioeconomic status skews policy toward the preferences of the well-off (e.g., Verba, Scholzman, & Brady, 1995). This hypothesis could be tested experimentally. For example, a group of legislative districts could be randomly assigned to be targeted by get-out-the-vote and/or grassroots lobbying campaigns aiming to increase participation among the less well-off. Legislators in the treatment and control groups could then be compared in their support for policies favoring those with lower socioeconomic status (e.g., voting for minimum wage or Medicaid-related bills). This design could answer the question: Does increased participation by people with lower socioeconomic status move policy in the expected direction?

Field experimental studies can be used not only to study the effects of a treatment on mass participation but also to determine the effects of participation on elite behavior and ultimately on public policy. This is important because these are major questions for political scientists, and previous work on them is potentially biased. In answering these questions, field experimentation could get at some of the longstanding problems in the discipline, including the policy effects of class bias in participation and the effectiveness of interest group strategies on elite behavior.

Appendix

Figure 1A

Text From a Sample e-Mail Sent to Tobacco-Free Kids Activists

Dear E-Champion,

We have just over one week to make sure your state House members vote for a smoke-free Granite State!

March 21st is now the day the House will vote for a smoke-free New Hampshire.

If you haven't emailed your representatives urging them to support the smoke-free bill, this is your chance! **[Click here to send your email today.](#)**

79% of New Hampshire voters want smoke-free air in New Hampshire.

That's because secondhand smoke isn't just annoying- it's dangerous. **Just one puff of secondhand smoke contains over 4,000 chemicals - including 69 known to cause cancer.** It's time for your lawmakers to listen to their constituents and protect everyone's right to breathe clean air.

Please click here to tell your representatives that you want a smoke-free New Hampshire! Urge them to vote for a smoke-free New Hampshire on March 21st.

Please save the date for March 21st - you're invited to witness history as the House votes on the smoke-free bill. We'll send you details soon on where the vote will be and what time it's starting. We're going to be there and hope you can make it too.

Thank you for supporting a smoke-free Granite State! Please forward this message on to your friends and family - we'd love their help too.

Figure 2A
Text From a Sample e-Mail Sent From
Tobacco-Free Kids Activists to Legislators

Did you know that 79% of Granite Staters support smoke-free air? I am one of them and I hope you are too. Please vote yes on HB 1177 to make all New Hampshire bars and restaurants smoke-free.

Visit this link to see an ad that has been running in New Hampshire papers.

<http://www.tobaccofreekids.org/campaign/printads/pdf/NHForkAd.pdf>

These poll results shouldn't be a big surprise - secondhand smoke is a serious health hazard. In fact, we are the only state in New England that doesn't protect our residents and workers from this dangerous substance. Secondhand smoke is known to cause lung cancer, heart disease and other serious illnesses. Everyone has the right to breathe clean air - please act to protect that right.

Thank you for taking the time to read my email. I look forward to hearing back from you.

Sincerely,

Your name
Your street address
City, State Zip

Table A1
Ordered Probit Estimates of the Effect of e-Mail Contacts on
Vote to Table Bill and Final Passage Including Nonvoters

Model	a	b	c	d
Treatment	0.191 (0.201)	0.404 [†] (0.223)	0.454 [†] (0.237)	0.625* (0.298)
Democrat		0.310 (0.277)	0.785* (0.396)	-0.985 (1.21)
Tobacco votes		0.619*** (0.123)	0.613*** (0.131)	0.835*** (0.186)
Cut 1	0.142 (0.148)	1.42 (0.281)	0.760 (0.472)	0.840 (0.852)

(continued)

Table A1 (continued)

Model	a	b	c	d
Cut 2	0.411 (0.150)	1.76 (0.290)	1.15 (0.475)	1.37 (0.854)
<i>N</i>	143	143	143	143
LR chi-square	0.90	46.77	66.95	111.85
probability > chi-square	.344	.000	.000	.000
Pseudo <i>R</i> ²	.003	.173	.248	.414
County fixed effects?	N	N	Y	N
Strata fixed effects?	N	N	N	Y

Note: Standard errors in parentheses. Dependent variable equals 2 for supporting the coalition-supported position on motion to table and final passage, 1 for supporting coalition on one of these bills, and 0 for not supporting the coalition-supported position on either of these bills. Nonvoters are counted as not supporting the coalition position. *Treatment* is coded 1 if members were randomly assigned to be e-mailed by constituents and 0 otherwise. Tobacco votes is the sum of three votes on tobacco-related legislation: HB 645, a vote on fire-safe cigarettes (1 for a *yes* vote, 0 *otherwise*); HB 535, a tobacco tax bill (1 = *yes*, 0 = *otherwise*); and an amendment to a tobacco tax bill (0 = *yes*, 1 = *otherwise*); note that the last vote is reversed. A high score on the scale indicates opposition to tobacco (scale reliability coefficient = .69). LR = Likelihood ratio.

* $p < .05$. ** $p < .01$. *** $p < .001$, two-tailed. [†] $p < .1$.

Notes

1. In this article, I will use e-mail lobbying campaigns and grassroots e-mail campaigns interchangeably; both refer to an interest group campaign that mobilizes activists to contact legislators through e-mail. The distinctive feature of an e-mail campaign is that a policymaker is contacted via e-mail rather than by phone, fax, or some other method.

2. There are a number of reasons that the groups allowed a subset of legislators to be randomly assigned to a control group: (a) e-mail was one method among many used to lobby legislators and was therefore not pivotal; (b) many public interest groups appear to believe that e-mail on its own is not an extremely effective form of lobbying; (c) key legislators, such as committee members and the leadership, were excluded from the study; and (d) the coalition was confident that the bill would pass the lower house of New Hampshire's legislature. These factors may be of interest to researchers in setting up similar experimental designs in the future as they may help researchers target groups that are most likely to participate in a similar experiment. In addition, all individual activists who were sent an e-mail and chose to click through to a legislator did e-mail at least one of their representatives.

3. This research was approved by a Human Subjects board.

4. Strata indicators would have to be included if there were different probabilities of assignment to control and treatment groups across strata. However, individuals within each stratum were assigned to treatment and control with equal probabilities.

5. There were 84 legislators without e-mail addresses, 21 legislators on the Commerce Committee, 3 legislators in the party leadership, and 96 legislators who had announced their position. This left 225 legislators (note that some of these categories overlap). Of these, a number of legislators were excluded at the behest of the coalition to (a) decrease the number of legislators who were not contacted and (b) to avoid having districts in which no legislators were contacted. Of the remaining 225 legislators, 26 had no other member in their district, and were therefore excluded from the study, and an additional set of legislative districts totaling 56 members were randomly selected to be excluded from the study. Excluding these members from the 400 members in New Hampshire's lower chamber left 143 members in the experiment.

6. Although this appears to be a modest amount, this number represents a sizeable percentage of the New Hampshire districts. The size of the constituencies represented by New Hampshire House members rank last in size among state legislatures with 3,089 individuals per district (National Council of State Legislatures, www.ncsl.org, accessed September 2008). This means that .001 of each district contacted a legislator (note that due to the multimember districts, some e-mails would go to more than one legislator); a comparable proportion from a U.S. House District with more than 650,000 members would result in roughly 650 e-mails to the house member.

7. These null results may also reflect the fact that the coalition's position on the pivotal votes may have been clearer to legislators than the position on amendment votes, as the former position had been emphasized throughout the campaign.

8. The analyses were also performed using multinomial logit and linear regressions; the results are substantively similar to those presented below. Also, the average treatment effected on the treated (ATT), or the average effect on legislators who were contacted by constituents, was estimated using 2SLS, coding as *treated members* in the treatment group who received at least one e-mail. That is, the independent variable in the ATT estimates was whether the legislators were contacted by at least one constituent. This independent variable is instrumented on treatment condition. Because treatment condition was randomly assigned, this is a valid instrument, as the only influence this variable has on legislative voting is through constituent contacts. See Angrist, Imbens, and Rubin (1996) for details on this method. Because nearly all legislators in the treatment group were contacted, these results are similar to those presented in the text.

9. The control group had a mean of 1.8 votes out of three for antitobacco legislation although the treatment group had a mean of 1.6; although this difference is not statistically significant ($p = .28$, two-tailed t test), it may be necessary to control for past tobacco support to obtain accurate estimates.

10. An attempt was made to detect the interaction of treatment with past tobacco votes. However, using ordinary least squares (OLS), no significant differences were found for the treatment effect among different levels of support for past tobacco legislation. This may reflect the small sample size and the imbalance of past support for tobacco regulations among the treatment groups (see Note 9 above).

11. Model b is used as it is the most parsimonious model incorporating controls for past tobacco votes; the results are similar using Models c and d.

12. First differences and their confidence intervals, as well as the probabilities in Figure 1, were simulated using CLARIFY (Tomz, Wittenberg, & King, 2003). See King, Tomz, and Wittenberg (2000) for details.

13. Results are similar with models that include controls.

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