

**Getting Out the Youth Vote:
Results from Randomized Field Experiments**

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Abstract. Prior to the November 7, 2000 election, randomized voter mobilization experiments were conducted in the vicinity of college campuses in New York State, Colorado, and Oregon. Lists of registered people under the age of 30 were randomly assigned to treatment and control groups. A few days before Election Day, the treatment group received a phone call or face-to-face contact from Youth Vote 2000, a nonpartisan coalition of student and community organizations, encouraging them to vote. After the election, voter turnout records were used to compare turnout rates for the treatment and control groups. The results indicate that phone canvassing and face-to-face voter mobilization campaigns were highly effective in stimulating voter turnout.

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Executive Summary

Randomized experiments were conducted to examine the effectiveness of phone canvassing and face-to-face contact on voter turnout. Canvassing campaigns were conducted by a coalition of groups affiliated with Youth Vote 2000. The target population consisted of registered voters ages 18-30 living in the vicinity of large public universities.

- Phone canvassing increased turnout by an average of 5 percentage-points. This finding, based on six experiments involving nearly 10,000 people, is statistically significant.
- Face-to-face canvassing increases turnout by an average of 8.5 percentage-points. This result is also statistically significant.
- Face-to-face canvassing produces “spillover” effects. Adults living with voters in the treatment group vote at significantly higher rates than adults living with voters in the control group.
- There is some evidence that providing voters with information about the location of their polling place makes mobilization campaigns more effective. Turnout rates are approximately 3 percentage-points higher when respondents are provided polling place information, but this estimate is not statistically robust.
- Two phone calls seem no more effective than a single call.
- Post-election surveys reveal no significant differences between treatment and control groups. Being contacted by a voter mobilization campaign, in other words, has no lasting effect on interest in politics, confidence in the political system, or feelings of civic duty.
- Nonpartisan get-out-the-vote campaigns targeting young voters mobilize voters at a rate of \$12 to \$20 per vote.

Getting Out the Youth Vote: Results from Randomized Field Experiments

1. Introduction

Research dating to the 1940s has consistently shown that young citizens vote at lower rates than older citizens and are less likely to feel connected to the electoral process (Highton and Wolfinger 2001). Low voter turnout among young citizens has grown more acute since 1972, when the nationwide voting age was lowered to 18. The proportion of eligible citizens age 18-24 who went to the polls declined from 49.6% in 1972 to 40.8% in 1984 to 32.4% in 1996, three presidential elections in which incumbents won by large margins. This low and diminishing level of involvement reflects something of a vicious cycle. Because young people vote at low rates, they are less likely to be courted by campaigns (Bennett 1991). As campaigns attend to other constituencies, young voters are neither central to the framing of campaign issues nor the object of voter mobilization efforts. The question is what, if anything, can be done to break this vicious cycle.

During the months leading up to the November 7, 2000 election, the Youth Vote 2000 coalition of nonpartisan groups on several college campuses held registration drives aimed at encouraging young voters to go to the polls. Some of these campus organizations also conducted voter mobilization campaigns that went into the field during the last week of the election. These get-out-the-vote (GOTV) campaigns used young volunteers to call or visit other young people, some of whom had recently been registered by Youth Vote coalition groups. Although each site developed its own script emphasizing the importance of making

one's voice heard, in practice these calls were chatty and informal, sometimes lasting for five or ten minutes, and conveyed information about the where and when to vote. Both in tone and content, these calls were very different from those typical of commercial phone banks (Gerber and Green 2000a, 2001), which have been found to be ineffective at mobilizing voters.

Our research gauges the effectiveness of these phone and face-to-face canvassing campaigns. The methodology we employ is randomized field experimentation. Lists of registered voters were randomly assigned to treatment and control groups. Treatment groups were called or visited during the days leading up to the election. Control groups were not contacted. After the election, we obtained voter turnout records from each county and calculated the turnout rates in each control and treatment group. This report begins with a brief overview of our statistical model and estimation procedure. Next, we describe the experiments conducted at each of the phone canvassing sites. We then analyze the voter turnout results and examine the results of a post-election survey that interviewed respondents from treatment and control conditions. We conclude by discussing the cost effectiveness of youth-oriented GOTV campaigns.¹

2. Research Design and Underlying Statistical Model

Unlike observational studies of voter mobilization (e.g., Rosenstone and Hansen 1993; Kramer 1970), which examine the correlation between voting and contact with campaigns, experimental studies randomly manipulate whether voters are approached by campaigns. Experimental control eliminates two problems associated with observational data. First, if campaigns target voters who are especially likely to go to the polls, then the observed correlation between contact and voter turnout may be spurious. We might

¹ Our evaluation focuses on the effectiveness of the GOTV campaign. It should be noted that we do not assess other potentially important Youth Vote coalition activities, such as hosting public debates and distributing voter education materials.

observe a strong correlation even if GOTV were ineffective. Second, if respondents' recollection of whether they were contacted is vague or distorted, the correlation between self-reported contact and turnout will misrepresent the true causal influence of contact.

The principal complication that arises in experimental studies of voter mobilization is that only some citizens assigned to the treatment group can actually be reached by phone. We must therefore distinguish between the intent-to-treat effect and the effects of actual contact. The intent-to-treat effect is simply the observed difference in voter turnout between those assigned to the treatment and control groups. If everyone in the treatment group is actually contacted, the intent-to-treat effect is identical to the actual treatment effect. In practice, however, contact rates are lower than 100%.

Consider the linear probability model

$$Y = a + bX + u,$$

where Y is a dichotomous $\{0,1\}$ variable indicating whether a citizen cast a vote, and X is whether he or she was actually contacted by a phone canvassing campaign. The coefficient b is the treatment effect, the boost in turnout caused by contact with the mobilization campaign. Contact is itself a function of whether a person was assigned to the treatment or control condition of the experiment. Let the variable Z , also a dichotomous $\{0,1\}$ variable, represent the random assignment to one of these experimental groups, such that

$$X = cZ + e.$$

To estimate the actual treatment effect (b) given a contact rate (c), we must adjust the intent-to-treat effect (t) as follows:

$$t/c \Rightarrow b.$$

In other words, to estimate the actual treatment effect, we take the intent-to-treat estimate and divide it by the observed contact rate. This estimator is equivalent to performing a two-stage least squares regression of vote (Y) on actual turnout (X) using randomization (Z) as an instrumental variable (Angrist, Imbens, and Rubin 1990; Gerber and Green 2000a). So long as we have information about the probability with which subjects assigned to the treatment group were actually contacted, we can accurately estimate the effects of contact with a sufficiently large number of subjects.

3. Phone Canvassing Sites

The sites for the experiments reported here are each large college campuses and the surrounding neighborhoods. Volunteers and paid staff from public interest groups and student organizations crafted a voter mobilization campaign in the two weeks leading up to the election. During the week before the election, facilities were set up to make calls, and callers were given a brief orientation on how to work through the list, what questions to expect, and how to record the disposition of each call.

Site 1: Albany, New York. The population used in this study consisted of voters registered by student organizations during September and October of 2000. Calls were made during the day before Election Day. Callers were instructed to work their way through the entire treatment list before calling numbers a second

time. Due to the small scale of the phoning operation, 37% of the 1,341 numbers assigned to the treatment group were never called. However, because the list consisted of recent registrants, the callers encountered relatively few wrong numbers.

Site 2: Stonybrook, New York. Again, the population used in this study consisted of voters registered by student organizations during September and October of 2000. Calls were made during the day before Election Day. The callers were extremely effective at reaching subjects in the treatment group, speaking with or leaving messages for 89% of them. Although this contact rate surpassed that of the other sites, the Stony Brook site actually spoke to approximately one-fourth of the treatment group, a rate that was comparable to the other sites working with student-generated lists.

Site 3: Colorado. The Colorado experiments consist of two populations: citizens registered by student groups and a list of names purchased from a commercial vendor. This distinction enables us to test whether voter mobilization campaigns are unusually effective when a group tries to mobilize people that it had registered during the preceding months. Both lists were mixed together, and no distinction was made between them during the course of the mobilization campaign. One hypothesis is that this population would be less receptive to GOTV blandishments, insofar as they did not participate in student-organized registration drives. On the other hand, it could be argued that this group would be more strongly influenced by a phone call, because these young people have less day-to-day exposure to political activities in and around campus and fewer social contacts with other politically active young people (Bennett 1991).

Site 3a. Boulder, Colorado, Vendor-generated list. A list of registered voters age 30 and under was purchased from a private vendor for zip codes adjacent to the University of Colorado-Boulder campus.

These 2,318 names were randomly assigned to treatment and control groups. The treatment group received calls between Friday and Monday prior to Election Day. Much of the information on the vendor-supplied list proved to be outdated, and 60% of the phone numbers were incorrect or disconnected. Of the 1,143 people in the treatment group, 7% were spoken to directly, and 35% were contacted in some fashion.

Site 3b. Boulder, Colorado, Student-generated list. During the months prior to the October registration deadline, student volunteers worked with a coalition headed by Campus Greenvote registered voters. These 1,094 names were randomly assigned to treatment and control groups. The treatment group received calls between Friday and Monday prior to Election Day. Because callers knew the precinct information of the respondent, they were able to remind them of the location of their polling places. Of the 653 people in the treatment group, 28% were spoken to directly, and a total of 72% were contacted in some fashion.

Site 4a. Eugene, Oregon, Vendor-generated list. This list was intended to target young people who were not living on or adjacent to campus. (This list also contains information on other registered voters living at the same address; see below.) The phone canvassing campaign targeted the youngest registered voter 30 years old or younger living in a household. As in Boulder, the contact rate was lower when Oregon callers attempted to reach people on this purchased list. Half the phone numbers proved inoperative. Callers spoke directly with approximately one in four subjects in the vendor-generated treatment list, as compared to one in two subjects in the student-generated treatment list. Taking all forms of contact into account, the student-generated list was significantly more accessible than the purchased list, 74% to 50% ($p < .01$, one tailed).

Site 4b. Eugene, Oregon, Student-generated list. Despite the length of the list, the callers, who began to contact voters six days before the election, were able to make a second pass through it in an effort to reach

numbers for which there was no answer on the first attempt. As a result, the contact rate for the student-generated list was high, 74.3%.

The disposition of calls made to the numbers in the treatment group is summarized for each experiment in Table 1. In no case were the callers particularly successful in talking to the intended respondent. A large fraction of the “contact” in fact represented the communication of a message through an answering machine or a roommate. It should be stressed that our broad definition of contact makes the estimates that we report in the next section especially *conservative*. That is, the intent-to-treat effect is divided by a larger number than would be the case were we to assume that messages left for the respondent had no effect on turnout. The analyses that follow in essence gauge the average treatment effects of speaking with or leaving messages for a people on a canvassing list.

Several features distinguish the aforementioned phone canvassing sites. First, callers in New York State were attempting to mobilize citizens whom the group had registered a few weeks earlier, while callers in Oregon and Colorado were working from lists that included both people who had been registered by student groups as well as previously-registered young voters. Second, the sites were situated in somewhat different political contexts. Although New York State featured a high-profile Senate contest between First Lady Hillary Rodham Clinton and Rep. Rick Lazio, New York was not a battleground state in the presidential race. Colorado was somewhat more closely contested, although Gore remained a long-shot. Oregon, on the other hand, was the focus of considerable attention in the waning days of the presidential campaign, as Ralph Nader’s electoral strength in Oregon raised the odds of a Bush victory. Elections in both Colorado and Oregon featured ballot measures on topics ranging from taxation to medical use of marijuana.

Perhaps the most interesting and potentially important difference among these states concerns the manner in which ballots are cast. Oregon's balloting is conducted by mail. Voters receive their ballots two weeks before the election and must return them by Election Day. New York has a traditional in-person balloting system. Colorado blends elements of both systems, insofar as it has an early-voting period followed by a traditional balloting system on Election Day.

4. Statistical Power of Each Phone Canvassing Experiment

The accuracy with which the treatment effects may be detected in each site depends on the particular characteristics of each experiment. Typically, evaluation research examines whether the apparent effects of an intervention are sufficiently strong to rule out the null hypothesis that the true effect of the treatment is zero and that the observed results occurred simply by chance. Classical hypothesis testing generally attempts to reject the null hypothesis by showing that it has less than a 5% chance of producing the observed results. The *power* of an experiment to detect an effect of a given magnitude is the probability of rejecting the null hypothesis when the data are collected. Intuitively, four factors increase the power of an experiment: larger sample sizes, treatment and control groups that are similar in size, high contact rates, and large treatment effects.

A number of factors tended to limit the power of each phone canvassing experiment. First, sample sizes proved to be somewhat smaller than originally projected, in that the participating organizations registered thousands but not tens of thousands of new voters. Second, these groups were understandably reluctant to assign half of their hard-earned registration list to a control group. To some extent, these concerns were allayed when the authors furnished these canvassing groups with vendor-supplied lists of

names; still, in the interests of diplomacy, we assigned the majority of names to the treatment group. Contact rates were far from 100% and proved especially low for the vendor-supplied sample.

Table 2 summarizes the power of each experiment and of all of the experiments taken together. The top panel of Table 2 describes the power of detecting an effect of 2.5 percentage-points against a one-sided hypothesis test. The results suggest that none of the experiments reliably detects an effect of this size. Indeed, even pooling all of the data together (N=9,424) produces a power of just .40. In other words, all of these experiments considered together would produce statistically significant results with a 40% probability if the true effect of phone canvassing were to raise turnout by 2.5 percentage-points. The picture improves when we assume that the true effect of phone canvassing is a 5 percentage-point increase in turnout. The middle panel indicates that each experiment has fairly low power (ranging from .21 to .35), but the experiments jointly have a power of .88. These results suggest that a true effect of 5 percentage-points should produce a mixed pattern of results, some significant, others not; but the results taken together will very likely prove significant. When the effect size grows to 10 percentage-points, as shown on the bottom panel of Table 2, each experiment has a high probability of showing significant results.

In advance of seeing the data, we expected the results to resemble the middle panel of Table 2, based on a 5 percentage-point effect. That is, the anticipated effect size was thought to fall below the face-to-face canvassing effects reported in Gerber and Green (2000a) but exceed the minimal effects of commercial phone banks Gerber and Green (2001). In this case, the significance of the experimental results can be expected to fluctuate from site to site, but the effect can be detected with reasonable certainty for all of the experimental sites combined.

5. Experimental Findings Concerning Phone Canvassing

After the election, we obtained voting histories and registration lists from local registrars. These lists were merged with names in the treatment and control groups in order to calculate voter turnout rates. Any attempt to match lists of names and addresses involves some risk of misclassification. Fortunately, the use of random assignment makes this risk equivalent for treatment and control groups. Thus, the difference in turnout rates between groups gives us an unbiased estimate of the experimental treatment effect.²

Intent-to-Treat Effects. The intent-to-treat effects for New York, Colorado, and Oregon are presented in Table 3. In New York, these results show sharp differences in turnout rates between treatment and control groups. For example, those *assigned* to the treatment group in Albany voted at a rate of 46.8%, whereas just 42.3% of the control group voted. The Stony Brook results are similar in magnitude, with 78.8% of the treatment group voting as compared to 70.6% of the control group. Both of these intent-to-treat effects are statistically significant at the .05 level, using Fisher's exact one-tailed test. In other words, had the GOTV campaign not influenced turnout at all, we would observe a difference as large as 8.2 percentage-points in fewer than 5 of every 100 experiments we conduct.

Smaller intent-to-treat effects were obtained in the other sites. In Colorado, the control group derived from a vendor supplied list voted at a rate of 28.0%, as compared to 28.2% in the treatment group. The student-supplied list showed a somewhat larger intent-to-treat effect of 2.3 percentage-points; the control group voted at a rate of 62.6%, as compared to 64.9% in the treatment group. Although both effects are in the expected direction, however, each falls short of the .05 level of statistical significance.

In contrast to Colorado, where the effects of the phone canvass were greater for the student-generated list, the Oregon site was more successful mobilizing the vendor-supplied population. Among vendor-supplied voters, those assigned to the control group voted at a rate of 55.0% as compared to 57.6% in the treatment group. This contrast falls just short of conventional levels of statistical significance ($p=.12$). The Oregon results for the student-generated list reveals little mobilization effect. The treatment and control groups voted at rates of 61.0% and 60.8%.

The Effects of Actual Contact. In order to gauge the mobilizing effect of a phone call, one must take into account the fact that many people on the list were never contacted, whether directly or indirectly. The lower portion of Table 3 describes the actual contact effects. The influence of actual contact in Albany is estimated to be a 8.2 percentage-point jump in the probability of voting. This estimate is quite large, approaching the estimated effects of face-to-face contact reported by Gerber and Green (2000a). The results from Albany are echoed by the results from Stony Brook, which suggest a 9.3 percentage-point effect. Both of these experiments indicate that an eleventh-hour mobilization campaign can have profound effects on voter turnout among young registrants. Note that the percentage-point increase in voter turnout is similar in both cases, despite the fact that the level of turnout is much higher in the Stony Brook sample. These results are consistent with Gerber and Green's (2000a) finding that mobilization's effects are similar across voters with very different pre-existing propensities to vote.

Colorado's results were weaker than New York's. Contact with the phone canvassing campaign raised turnout by just .5 percentage-points in the vendor-supplied sample and 3.2 percentage-points in the student-generated sample. Oregon's GOTV call also appears to have had mixed effects. Subjects drawn

² Duplicate names – people who had inadvertently registered more than once – were discarded from the data file. Such people have different probabilities of being assigned to the treatment and therefore cannot be compared to the rest of the sample.

from a vendor-supplied list increased their voter participation by 5.4 percentage-points in the wake of a phone call. Although overshadowed by the effects observed in the New York sites, this estimate nonetheless remains substantial. (By comparison, the turnout difference between so-called battleground states and states that attracted little attention from the presidential campaigns in 2000 was typically less than 5 percentage-points.) Turning to the student-generated sample, it appears that the mobilization campaign seems to have done little to raise voter turnout. These results came as a surprise to us, given the favorable impression we developed of the GOTV campaign that they assembled. Nevertheless, these estimates remain unchanged when we add to our statistical model covariates such as past voting history or age. One may speculate that the limited effectiveness of this particular campaign reflects the high level of visibility that the mobilizing organizations had already achieved on campus in the weeks prior to the election or the problems associated with mobilizing voters in the waning days of a mail-in balloting process. As we will see in the next section, however, the latter explanation cannot explain the effectiveness of face-to-face mobilization among voters in the vendor-supplied sample.

Taken together, what do these experiments suggest about the effectiveness of phone canvassing? If we assume that each of the sites ran campaigns of comparable quality and effectiveness, we may combine all of the results (each weighted by the inverse of its sampling variance) to form a joint estimate. The actual treatment effect based on this calculation is 5.0 percentage-points, with a standard error of 1.7 percentage-points. This result, based on nearly 10,000 subjects, is statistically significant at the .01 level, implying that if canvassing in fact did nothing to stimulate voter turnout, we would obtain an estimate as large as 5.0 percentage-points in fewer than 1 in 100 studies of this kind. Although some individual experiments proved significant and other not, this pattern of results is entirely consistent with power calculations based on a true

effect of 5 percentage-points. Recall from Table 2 that each experiment has approximately a 1/3 probability of producing a significant effect estimate, and as expected we find significant results in 2 of 6 experiments.

It is possible that the variation we observe across sites reflects either the quality of their operations or the electoral systems in which they operated. Our site visits and interaction with staff at each location lead us to be skeptical of the operational explanation. Oregon struck us as the best organized and trained site, yet its phone mobilization campaign was not more effective than those conducted by the New York sites, which were put together in the final week of the campaign. It is possible that traditional voting systems such as New York's are more conducive to phone mobilization campaigns. Clearly, additional research is required to test the hypothesis that voting procedures, not sampling error, account for the variation in our results. If a new round of experiments conducted in different electoral systems should continue to reveal an average treatment effect of 5 percentage-points, sampling error will be the apparent cause. On the other hand, if the apparent effects of phone canvassing prove to be higher in states with traditional voting systems, sampling error will recede as an explanation.

Regardless of the precise manner in which one chooses to extrapolate from the five experiments, these results show quite clearly that mobilization of young voters is possible. One phone call during the waning days of the election campaign significantly reduces the age gap in turnout and reverses a long-term decline in turnout among young voters. We will return to the question of cost-efficiency below, after turning our attention to face-to-face mobilization campaigns.

6. Face-to-Face Canvassing Sites

Canvassing residential areas containing a high density of young voters requires labor and organization. Unlike a phone canvassing campaign, which can be run and supervised from a central location, door-to-door canvassing requires canvassers to wend their way through various neighborhoods, preferably with some prior knowledge about how they are laid out. The task of managing a canvassing campaign becomes somewhat more complex when embedded within an experiment. Experimental manipulation can take two forms. First, as was done in the Gerber and Green (2000a) study, one can assign each canvasser a list of individual names and addresses to canvass. In this case, the canvasser works through a list asking to speak with specific registered voters. Alternatively, a list of streets or blocks could be assorted into treatment and control groups on a random basis, as in Gerber and Green (2000b). Here, the unit of analysis becomes the street, and the relevant outcome variable is the street-level turnout rate. The advantage of the second approach is the ease with which it is implemented. Canvassers are simply assigned a random set of streets to canvass, and the remaining streets constitute a control group. The disadvantage of this approach is diminished statistical power. The typical canvassing campaign comprises fewer than 300 streets; assigning a small number of them to a control group does not enable the researcher to draw precise inferences about effect size.³

Individual-level canvassing experiments were conducted in Oregon, the results of which are described in Table 4. As in the case of the phone experiment, the Oregon samples were derived from two lists. The first was obtained from a commercial vendor. The youngest registered voter living in a household was randomly assigned to treatment or control. The other list was derived from a Youth Vote registration drive. Those who failed to list a phone number on their registration cards were placed in the door-to-door canvassing study and from there assigned to treatment or control groups.

³ This design could be improved by matching the streets according to historical levels of voter turnout. Unfortunately, the block level canvassing campaigns described here were crafted during the last few days of the campaign. Lacking sufficient time to

In the vendor sample, the intent-to-treat effects were both sizeable and statistically robust. In the control group, turnout was 55%, whereas those assigned to the canvassing condition voted at a rate of 59.3%. This contrast is significant at $p < .05$ using a one-sided Fisher test. This finding is bolstered by comparing the voting rates of treatment and control groups in the 1998 election, two years prior to the experiment. In 1998, 24.1% of the control group voted, as compared to 23.8% of the treatment group. In other words, the effects of face-to-face mobilization cannot be ascribed to pre-existing differences between the treatment and control groups. Randomization, as expected, produced treatment and control groups with similar voting proclivities. The door-to-door campaign had a genuine effect on the voting behavior of those in the treatment group.

Deriving an estimate of the actual treatment effect is complicated by the fact that we do not have exact contact information for the entire canvassing campaign. Based on the records that were kept and the impressions formed while participating in the canvassing drive ourselves, we estimate the contact rate to be no higher than 30% for the vendor-generated sample. This figure is quite generous given the phone canvassing results, which demonstrate that a large portion of the list is invalid. Based on the 30% figure, the actual treatment effect is estimated to be 14.3 percentage-points, which is slightly larger than the canvassing effects reported in Gerber and Green (2000a). This estimate is statistically significant at the 5% level using a one-tailed test.

Turning to the student-generated sample, the canvassing effect is in the predicted direction but falls short of statistical significance. We find that 62.6% of the control group voted, as opposed to 64.6% of the treatment group, $p = .20$ using a one-sided test. Estimating the contact rate to be roughly 40%, the actual

research the turnout history by block or street, we fell back on simple random assignment.

treatment effect is 5 percentage-points, with a standard error of 5.6 percentage-points. This indeterminate result is hardly surprising given the limited power of each of the two individual-level canvassing experiments. Jointly, however, the two experiments have a power of .71 to detect a true effect of 10 percentage-points. Indeed, combining the results of the two Oregon experiments suggests that face-to-face contact raises turnout by 8.5 percentage-points, with a standard error of 4.5 percentage-points. Thus, the combined results are statistically significant at the 5% level using a one-tailed test.

Two street level canvassing experiments were also conducted, one in Ann Arbor, the other in Boulder. In both cases, streets within a defined canvassing region were enumerated and randomly assigned to treatment and control groups.⁴ The data may be analyzed in two ways, both of which are presented in Table 5. First, we may analyze data at the individual level, according to whether each voter resided in a treatment or control block. Examining the data in this way shows that voter turnout in Ann Arbor was 39.7% in the control group and 39.9% in the treatment group. The contrast was larger in Boulder, where 45.5% of the control group voted, as compared to 47.2% of the treatment group. The latter comparison is marginally significant at $p < .10$.

The problem with analyzing the data at the individual level, however, is that the observations may not be independent. People living on the same block may share unobserved characteristics that predict voter turnout. An individual-level analysis will produce unbiased estimates under these conditions, but the standard errors will understate the degree of statistical uncertainty. Another way to handle the data is to

⁴ Because the groups conducting the canvassing campaign were eager to include as much territory as possible in the treatment group, we randomly assigned streets such that the treatment observations outnumbered the control observations. This imbalance, coupled with the small total number of observations, undercuts the power of these experiments to detect the effects of door-to-door canvassing. The results are nevertheless interesting as a benchmark for future research. They provide, for example, some sense of how much heterogeneity to expect in block-level voting rates. In Ann Arbor the standard deviation in voting rate across blocks was 16 percentage-points; the corresponding figure was 12 percentage-points in Boulder.

aggregate the data by block and analyze block-level voting rates. Discarding blocks with fewer than 10 registered voters leaves 156 treatment and 29 control blocks in Ann Arbor and 73 treatment and 40 control blocks in Boulder. Using weighted least squares to account for the fact that more populous blocks have less statistical uncertainty, we find an incorrectly signed -1.8 percentage-point effect in Ann Arbor that is swamped by a standard error of 3.2 percentage-points. In Boulder, the results look more sensible, an effect of 2.5 percentage points with a standard error of 2.4 percentage-points. Jointly, the two results suggest an intent-to-treat effect of 1 percentage-point with a standard error of 1.9 percentage-points. The effects of actual contact are difficult to estimate, but on the assumption that canvassers spoke with 1 out of every 8 registered voters on each treatment block, actual treatment effects are roughly 8 percentage-points. The uncertainty surrounding these estimates limits their usefulness, but it should be pointed out that they are not inconsistent with previous findings placing actual treatment effects in the neighborhood of 8.5 percentage-points.

7. Spillover Effects

A complete assessment of the effects of phone and face-to-face voter mobilization on voter turnout must consider not only the intended target of the mobilization campaign but others who might come into incidental contact with the campaign.

The vendor-supplied list in Oregon affords us the opportunity to study what may be termed “spillover effects,” that is, the effects of voter mobilization on other registered voters living at the same address as the subject who was placed in the treatment group. Recall that calls or canvassing were directed at the youngest registered voter in each household. However, the mobilization campaign frequently spoke

with other members of the household in the course of leaving messages for the intended subject. These spillover effects may be estimated by comparing registered voters living at the same address as treatment subjects to registered voters living at the same address as control subjects.

Table 6 indicates that face-to-face canvassing clearly generated important spillover effects, raising turnout in the treatment group from 75.6% to 78.3%. Thus, incidental contact with the face-to-face canvassing campaign increased turnout by 2.7 percentage-points. This estimate, which is significant at the 5% level using a one-tailed test, is bolstered by the fact that the turnout rate in 1998 was .3 percentage-points *lower* among those living at the same address as the treatment subjects than among those living with the control subjects.

The magnitude of these spillover effects bears some emphasis. Because each of the intended subjects in the treatment group resided with an average of 1.5 other registered voters, spillover constitutes an important component of the overall effect of a targeted face-to-face canvassing effort. Based on intent-to-treat effects reported in Tables 4 and 6, we calculate that for every 100 treatment subjects assigned to a canvassing campaign, 4.3 additional votes are mobilized through intended contact and $2.7 * 1.5 = 4.1$ additional votes are mobilized through incidental contact. Indeed, one potential advantage of face-to-face canvassing as a mobilization tactic is that it produces substantial spillover effects.

The spillover effects of phone canvassing are somewhat unclear. Householders living with subjects in the control group voted at rate of 75.6%, as compared to 75.7% in the treatment group. This pattern would seem to suggest that spillover effects play no role in phone canvassing. We are somewhat cautious about this conclusion, however, because in prior elections the control group voted at higher rates than the

treatment group. Using regression to control for past voting in 1999, 1998, 1997, and 1996 we find a significant spillover effect of 2.1 percentage-points ($p < .05$). Since this result emerges from a multivariate analysis rather than a simple comparison of treatment and control groups, it must be considered somewhat tentative. Further investigation is required to examine whether incidental contact with phone canvassing campaigns mobilizes voter participation. Future work should also distinguish between two forms of spillover. One results from incidental contact between canvassers and other members of the household. The other occurs when the targeted member of the household, once contacted, in turn mobilizes others in the household. In order to isolate the latter effect, one could perform an experiment in which some canvassers would be instructed to speak only with the targeted member of the household.

8. The Content and Frequency of Campaign Contact

The experiments described above suggest that mobilization campaigns do in fact impel young voters to go to the polls. Two additional experiments were performed in an effort to refine our understanding of why mobilization campaigns work. One hypothesis is that voters lack information. They may have forgotten when the election will take place or where they must go to cast a ballot. If true, canvassing campaigns that remind voters when and where to vote should prove more effective than campaigns that simply urge voters to vote but offer no details about how to do so. A second hypothesis concerns social pressure. By this account, voters feel an obligation to vote after receiving a call from someone reminding them about the importance of the election. The more calls, the more pressure to fulfill this obligation.

In order to test these complementary hypotheses, a 2 x 2 experiment was performed in Ft. Collins, Colorado. Subjects were randomly assigned to four conditions. In half the conditions, subjects were told

the location of their polling places and encouraged to vote by a nonpartisan group. In the other conditions, subjects were encouraged to vote but were not provided with polling place information. The other factor of the experiment was the number of calls made to each subject. Half of the subjects were called on Monday, the night before the election. The other half were called on both Sunday and Monday.⁵ Thus, the experiment enables us to test whether turnout rises when voters are provided with polling locations, as the information hypothesis would suggest, or in the wake of a second phone call, as the social pressure hypothesis would suggest.

The sample was supplied by the Elections Office of Larimer County, which provided a list of names of people aged 18-24 who had registered to vote during the six months prior to the registration deadline of October 24. Restricting the list to people who had been registered in the past six months was done to ensure the contact information was as accurate as possible. It also means that the list consists primarily of voters who were registered by the COPIRG chapter at Colorado State University.

The results suggest that information encourages voter turnout. Regardless of how many calls were attempted, subjects who were given polling information turned out at higher rates. Overall, those given polling information turned out at a rate of 76.2%, as compared to 73.6% among those who were not supplied any details about where to vote. Given the contact rate of 84.5%, the actual treatment effect is estimated to be 3.1 percentage-points. Although this estimate falls short of statistical significance ($p=.16$), it does suggest that canvassing campaigns work in part because they provide information or motivate respondents to acquire it themselves. This finding merits further investigation, but these results do suggest that mobilization campaigns should provide polling information, if possible.

⁵ A control group that was never called was not created due to an oversupply of callers and dearth of subjects.

No support is obtained for the social pressure hypothesis. Turnout rates among those called on both Sunday and Monday were no higher than among those called only on Monday. The data do not enable us to tell whether respondents were put off by callers badgering them to vote, thereby counteracting any additional effect it might have had, or whether they were simply indifferent to the second phone call. Either way, we see no evidence to support the view that additional phone calls stoke voters' motivation. It appears that when allocating resources, GOTV campaigns would be better advised to attempt to contact greater numbers of voters rather than make a second contact with some.

9. Survey Analysis

One of the methodological innovations of the current study is its use of post-election surveys to gauge attitudinal differences between treatment and control groups. Prior to random assignment, these groups were identical except for chance differences. Since we know that phone and face-to-face canvassing have a demonstrable effect on voter turnout, we might look to surveys for clues about the psychological imprint left by the GOTV campaigns. In particular, the treatment and control groups may be compared with reference to the leading constructs used to explain voter turnout: internal efficacy, sense of civic obligation, external efficacy, interpersonal trust, interest in politics, and so-called 'conative' attitudes toward the act of voting itself.

The survey was originally intended to go into the field during the week following the November 7 election. Due to the special circumstances surrounding the election, we chose to wait until the contested presidential outcome was resolved in an effort to obtain a more valid assessment of how people regarded the electoral process and the act of voting. Three weeks into the post-election stalemate, we were forced to

launch our survey for fear of losing our respondents to Winter Break. Since our target population is highly mobile, we expected that many of our phone numbers would prove inoperative were we to wait until January.

Because the sample of phone numbers was drawn from the phone canvassing lists, including those supplied by commercial vendors, problems of nonresponse were compounded by outdated phone numbers. Furthermore, the largest group of survey respondents were drawn from a Youth Vote 2000 site in Miami. This site conducted both phone and door-to-door canvassing experiments but has been excluded from our report because, as we discovered a few weeks after the survey occurred, the mobilization effort there contacted only a small number of voters. This leaves us with useable information from Stony Brook, Albany, and Boulder.⁶

Despite these problems, this survey has some important strengths. Unlike conventional surveys, this one is insulated from problems of low response rates. Our aim is to compare responses in the treatment and control groups, and nonresponse is the same across groups. Second, the analysis examines the effects of randomly-induced changes in attitudes. Conventional survey analysis, by contrast, attempts to infer causality from correlations between voting and naturally-occurring variation in attitudes, which is fraught with potential problems. Any attitude that naturally co-occurs with voting could be a spurious reflection of other causes of voting, rather than itself a cause. Experimental manipulation guarantees that such correlations must be due either to the treatment or to random sampling variability.

⁶ We intended to interview voters from Oregon but due to a clerical error only interviewed those in the treatment group. Boulder respondents who had been assigned to treatment and control conditions based on more than one registration list (vendor-supplied, student-supplied, or a list obtained from the county clerk) were omitted from the analysis.

The text of each survey question is presented in the Appendix, and the comparison between treatment (N=396) and control groups (N=237) is summarized in Table 8. As a preliminary step, we consider whether the two groups recalled receiving a phone call from a nonpartisan group. Despite the fact that the overall contact rate was 79% among survey respondents assigned to the treatment group, the treatment group was only somewhat more likely than the control to remember a nonpartisan campaign contact. Granted, restricting the analysis to those members of the treatment group who actually were contacted directly improves the rate of recall. In the treatment group, 29% of those who were not spoken to *directly* recalled a nonpartisan contact. Among those who were spoken to directly, this figure rises to 41%. This finding has two far-reaching implications. First, it reaffirms the notion that the interactions between campaigners and voters are brief and relatively unobtrusive. GOTV campaigns of the sort described here are seldom, if ever, memorable experiences. Second, this finding underscores the infirmities of recall data frequently used in nonexperimental analysis of voter mobilization. Given the contrast in voter participation in treatment and control groups, the lack of contrast in recalled campaign contact must be interpreted to mean that respondents' reports are unreliable.

The survey findings are easily summarized. Whether we examine questions that tap voter interest, feelings of civic duty, sense of political efficacy, or perceptions of the difficulty of voting, no statistically significant differences emerge between treatment and control groups.⁷ In every instance but one, the differences between the groups are substantively negligible. The lone exception is the very first question, which suggests that the treatment may have increased the overall level of interest in political campaigns. Again, however, this difference is not statistically robust (one-sided $p = .19$), and given the sheer number of comparisons that could be examined, probably an artifact of sampling variability.

⁷. Nor did we detect any differences in voting preferences or intensity between treatment and control condition.

This pattern of findings suggests that canvassing itself leaves a shallow imprint on the way that voters view politics. Exposure to a nonpartisan campaign is not a transformative experience, even if it does play an important role in encouraging voter participation in a given election. It may be that the accumulation of campaign contacts over a series of elections has effects that go beyond what we are able to detect here. In evaluating the repercussions of a single campaign, however, we would conclude it does little to change global orientations toward the political system. The significance of GOTV campaigns resides in the fact that they impel people to vote.

Conclusion

For young voters, nonpartisan contact represents a bridge to electoral participation. They sense that the election is important, but many are detached from the electoral process. They need the authentic encouragement of a peer to become a participant. Nonpartisan GOTV campaigns provide a link between young voters and the electoral system.

Our experimental results demonstrate that mobilization campaigns work and have the potential to substantially increase youth turnout during a presidential election year. Each successfully completed call to a young registrant raises the probability of turnout by roughly 5 percentage-points. This figure, it should be noted, is a conservative estimate in two respects. First, the effectiveness of phone calls may be somewhat greater in many parts of the country where traditional voting systems are used. Second, our broad definition of “contact” with canvassers (which includes messages left for the respondent) doubtless understates the effectiveness of an actual phone conversation with the respondent. The figure of 5 percentage-points should

be regarded as an “average effect” estimate that elides the distinction between direct conversations and presumably less effective phone messages. The estimated effect of face-to-face mobilization is 8.5 percentage-points. This estimate, too, tends to understate the likely influence of door-to-door canvassing. One of the most intriguing results to emerge from this study concerns the spillover effects of door-to-door campaigns. Not only do they raise turnout among the targeted population; they also increase turnout among others in the household who come into contact with the campaign.

The degree to which phone and face-to-face canvassing increase turnout is especially impressive given the meager budgets on which these campaigns operated. Our experimental results suggest that 20 successful phone contacts translated into 1 additional vote. Consider what this finding implies for a large scale GOTV campaign. If one were to hire campaign workers at a rate of \$10 per hour to make 10 contacts per hour, the cost-per-vote would be \$20. Similarly, our face-to-face canvassing experiments suggest that 12 contacts produce 1 additional vote. Leaving aside any additional effects due to spillover, it follows that a \$10 per hour campaign worker who makes 5 contacts per hour generates votes at a rate of \$24 per vote. When spillover effects are taken into account, the cost-efficiency of face-to-face canvassing improves to roughly \$12 per vote.

Extrapolating these figures to the college-age population of approximately 5 million voting-age adults suggests that an expenditure of between \$6 million and \$10 million could increase turnout by 500,000 votes in this age group. This figure would be greater or smaller depending on the wage rate and the accessibility of the target population, but the fact remains that this sum is a fraction of what US Senate candidates currently spend in a single closely contested election.

These experimental results, in sum, provide the clearest indication to date that breaking the cycle of low voter turnout is feasible using traditional GOTV techniques. In addition to replicating and refining the experiments described here, the task ahead is to expand the scale of youth mobilization campaigns so that they may have a detectable impact on the overall rate at which young voters go to the polls.

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Table 1: Percentage of the Treatment Groups who were Actually Contacted, by Site

	Albany New York	Stony Brook New York	Colorado (Vendor sample)	Colorado (student sample)	Oregon (Vendor sample)	Oregon (student sample)
Treatment Subject Contacted	24.8%	26.3%	6.6%	28.2%	25.0%	49.2%
Subject's Roommate Contacted/Message Left	29.9%	62.4%	27.9%	44.0%	24.0%	25.1%
Total Contact Rate	(54.7%)	(88.7%)	(34.5%)	(72.2%)	(49.0%)	(74.3%)
Wrong Number, Disconnected, Busy	8.2%	8.9%	59.6%	20.5%	50.6%	25.6%
Number not Attempted	37.0%	2.4%	5.9%	7.2%	0.4%	0.0%
N of Cases Assigned to Treatment Group	1341	680	1143	653	953	705

Table 2: Power Calculations for Each of the Youth Vote 2000 Phone Experiments

Site Location Source of Registered Voters List	Albany (Student)	Stony Brook (Student)	Boulder (Vendor)	Boulder (Student)	Oregon (Vendor)	Oregon (Student)	Total
Contact Rate	0.550	0.890	0.340	0.720	0.490	0.740	0.583
Fraction Assigned to Treatment Group	0.710	0.710	0.490	0.600	0.490	0.590	0.582
Total N	1891	959	2318	1094	1960	1202	9424
Expected Sampling Variance	0.002	0.002	0.004	0.002	0.002	0.002	0.000
Standard Error	0.046	0.040	0.061	0.043	0.046	0.040	0.018
Expected Treatment Effect	0.025	0.025	0.025	0.025	0.025	0.025	0.025
Power for a one-sided 5% test	0.14	0.16	0.11	0.15	0.14	0.16	0.40
Expected Treatment Effect	0.050	0.050	0.050	0.050	0.050	0.050	0.050
Power for a one-sided 5% test	0.29	0.35	0.21	0.32	0.29	0.35	0.88
Expected Treatment Effect	0.100	0.100	0.100	0.100	0.100	0.100	0.100
Power for a one-sided 5% test	0.70	0.81	0.50	0.76	0.70	0.81	1.00

Table 3: Effects of Phone Calling GOTV Campaigns, by Site

	Albany New York	Stony Brook New York	Colorado (Vendor sample)	Colorado (student sample)	Oregon (Vendor sample)	Oregon (student sample)	Total*
Percent Voting in Control Group N	42.3% (558)	70.6% (279)	28.0% (1175)	62.6% (441)	55.0% (1007)	61.2% (497)	
Percent Voting in Treatment Group N	46.8% (1341)	78.8% (680)	28.2% (1143)	64.9% (653)	57.6% (953)	61.7% (705)	
Difference between Treatment and Control (Intent-to-Treat Effect)	+4.5%	+8.2	+0.2%	+2.3%	+2.6%	+0.5%	
Percentage of the Treatment Group That was Actually Contacted	54.7%	88.7%	34.5%	72.1%	49.0%	74.3%	
Estimated Effect of Contact on Turnout Standard Error	8.2% (4.6)	9.3% (3.4)	0.5% (5.4)	3.2% (4.1)	5.4% (4.6)	0.7% (3.8)	5.0% (1.7)
P-value, one tailed test	.04	< .001	.46	.22	.12	.42	<.01

* Overall treatment effects are weighted by the inverse of the sampling variance associated with each experiment.

Table 4: Effects of Door-to-Door GOTV Campaigns in Oregon

	Oregon Vendor-Supplied List	Oregon Student-Supplied List	Total
Percent Voting in Control Group N	55.0% (1007)	62.6% (771)	
Percent Voting in Treatment Group N	59.3% (1003)	64.6% (1145)	
Difference between Treatment and Control (Intent-to-Treat Effect)	+4.3%	+2.0	
Percentage of the Treatment Group That was Actually Contacted*	30%	40%	
Estimated Effect of Contact on Turnout Standard Error	14.3% (7.4)	5.0% (5.6)	8.5% (4.5)
P-value, one tailed test	.03	.21	.03

* These figures are approximate, based on a sample of the canvassing records.

Table 5: Effects of Block-Level Door-to-Door GOTV Campaigns in Ann Arbor and Boulder

	Ann Arbor Vendor-Supplied List	Boulder Student-Supplied List	Total
Percent Voting in Control Group N	39.7% (1881)	45.5% (2564)	
Percent Voting in Treatment Group N	39.9% (12,626)	47.2% (4449)	
Difference between Treatment and Control (Intent-to-Treat Effect)	+0.2%	+1.7	
Weighted least squares estimate of turnout effect at the block level	-1.7%	2.5%	1.0
Standard Error	(3.2)	(2.4)	(1.9)
P-value, one tailed test	n.s.	.16	.33

Table 6: Spillover Effects of Door-to-Door and Phone Canvassing Campaigns in Oregon

	Face-to-Face	Phone
Percent Voting in Control Group in 2000 N	75.6% (1539)	75.6% (1539)
Percent Voting in Treatment Group in 2000 N	78.3% (1513)	75.7% (1467)
Difference between Treatment and Control (Intent-to-Treat Effect)	+2.7%	+0.1
P-value, one tailed test	.04	.47
Percent Voting in Control Group in 1998	60.8%	60.8%
Percent Voting in Treatment Group in 1998	60.5%	56.2%

Table 7: Effects of Providing Polling Place Information and Repeated Phone Calls

Number of Calls	Polling Information Provided?	Turnout Rate	Standard Error	N
1	No	73.9%	2.4	337
2	No	73.3%	2.4	337
1	Yes	76.3%	2.3	337
2	Yes	76.1%	2.3	336

Contact rate = 84.5%

Intent-to-treat effect of providing polling information: $76.2\% - 73.6\% = 2.6\%$ (SE=2.4), $p=.14$

Actual treatment effect of providing polling information: $2.6\% / .845 = 3.1\%$ (SE=3.1), $p=.16$

Intent-to-treat effect of two calls vs. one call: $74.7\% - 75.0\% = -0.3\%$ (SE=2.4), n.s.

Actual treatment effect of two calls vs. one call: $-0.3\% / .845 = -0.4\%$ (SE=3.1), n.s.

Table 8: Survey Responses by Treatment and Control Group

VAR	Question	Responses	Control	Treatment
Q1	Some people don't pay much attention to political campaigns. How about you? Thinking back to the election campaign, would you say that you were very much interested, somewhat interested, or not much interested in following the political campaigns this year?	1. Very Interested 2. Somewhat interested 3. Not much interested 4. Don't Know/Not Sure	38.4 45.1 16.5 0	41.7 45.7 12.6 0
Q2	In the election for President, which candidate did you prefer: George Bush, Al Gore, Ralph Nader, or some other candidate? [DO NOT READ RESPONSE OPTIONS]	1. Bush 2. Gore 3. Nader 4. Other 5. Don't Know/Not Sure 6. Refused	19.4 57.8 19.0 0.8 1.3 1.7	18.9 57.1 16.4 1.5 2.0 4.0
Q3	Did you feel strongly about your favorite candidate or did you have mixed feelings about him? [DO NOT READ RESPONSE OPTIONS]	1. Felt strongly 2. Had mixed feelings 3. Don't know	57.0 40.5 2.5	52.0 45.2 2.8
Q4	As you know, the political parties try to talk to as many people as they can in order to get them to vote for their candidate. Did anyone from one of the political parties call you up or come around and talk to you about the campaign this year? [DO NOT READ RESPONSE OPTIONS]	1. Yes 2. No 3. DON'T KNOW	16.0 83.5 0.4	23.2 76.8 0
Q5	How about someone from a nonpartisan organization, like Campus Greenvote or Youthvote 2000? Did anyone from a nonpartisan group call you up or come around and talk to you about the campaign this year? [DO NOT READ RESPONSE OPTIONS]	1. No 2. Yes, called 3. Yes, came around in person 4. Yes, called and came around in person 5. DON'T KNOW	76.4 7.6 13.1 2.1 0.8	67.2 13.1 16.4 2.8 0.5
Q6	During the last few weeks of the campaign, did you ever discuss politics with your family or friends? [DO NOT READ RESPONSE OPTIONS]	1. Yes 5. No 8. DON'T KNOW	94.1 5.9 0	95.2 4.8 0
Q7	During the campaign, did you talk to any people and try to show them why they should vote for or against one of the parties or candidates? [DO NOT READ RESPONSE OPTIONS]	1. Yes 5. No 8. DON'T KNOW 9. NO ANSWER; REF	66.7 32.5 0.8 0	67.9 30.8 0.5 0.8
Q8a	I'm going to read a pair of statements about voting, and please tell me which one come closer to your	1. Voting is exciting 2. Voting doesn't really interest me.	84.8 10.1	82.1 10.9

	view:	3. Not sure	5.1	7.1
Q8b	I'm going to read a pair of statements about voting, and please tell me which one come closer to your view:	1. I feel that voting is an important civic duty. 2. It's okay to skip an election." 3. Not sure	92.0 7.6 0.4	92.2 7.3 0.5
Q8c	I'm going to read a pair of statements about voting, and please tell me which one come closer to your view:	1. Voting gives me the opportunity to influence government 2. One vote really doesn't make any difference. 3. Note sure	87.3 9.7 3.0	84.6 14.4 1.0
Q8d	I'm going to read a pair of statements about voting, and please tell me which one come closer to your view:	1. I find voting easy to do. 2. I not always sure about where to vote or how to cast my ballot." 3. Not sure	84.8 14.8 0.4	86.4 12.6 1.0
Q9	Some people try to avoid getting into political discussions because they think that people can get into unpleasant arguments. Other people enjoy discussing politics even though it sometimes leads to arguments. What is your feeling on this -- do you usually try to avoid political discussions, do you enjoy them, or are you somewhere in between? [DO NOT READ RESPONSE OPTIONS]	1. AVOID DISCUSSIONS 2. ENJOY DISCUSSIONS 3. IN BETWEEN 8. DON'T KNOW 9. REF/NO ANSWER	8.9 51.1 39.7 0.4 0	10.6 51.5 37.1 0.5 0.3
Q10a	Sometimes politics and government seem so complicated that a person like me can't really understand what's going on.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent]: DON'T KNOW	11.8 43.9 3.4 16.0 24.5 0.4	10.9 38.9 3.5 24.0 22.5 0.3
Q10b	Having elections makes the government pay attention to what the people think.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent]: DON'T KNOW	30.0 51.1 3.8 12.7 2.5 0	35.4 43.2 3.5 11.1 6.6 0.3

Q10c	I feel that I have a pretty good understanding of the important political issues facing our country.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent: DON'T KNOW]	40.9 46.0 2.5 7.6 3.0	45.5 43.9 3.5 5.8 1.3
Q10d	There are important differences in what the Republicans and Democrats stand for.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent: DON'T KNOW]	49.8 30.8 3.0 10.5 5.5 0.4	48.5 31.8 3.8 9.3 5.8 0.8
Q10e	No matter who people vote for, it won't make any difference to what happens.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent: DON'T KNOW]	3.0 13.9 1.7 23.2 58.2 0	4.5 10.4 3.3 25.8 56.1 0
Q11	How much of a majority is required for the U.S. Senate and House to override a presidential veto? [DO NOT READ ANSWER CATEGORIES]	1. If the respondent answers "two-thirds majority." 2. If the respondent answers something else. 8. [if volunteered by respondent: DON'T KNOW]	54.0 11.0 35.0	58.8 9.1 32.1
Q12	Do you happen to know which party has the most members in the House of Representatives in Washington now? [DO NOT READ ANSWER CATEGORIES]	1. Republicans or Republican Party 2. Democrats or Democratic Party 3. Other 8. [if volunteered by respondent: DON'T KNOW]	66.7 16.0 3.8 13.5	73.0 11.4 2.5 13.1
Q13	Would you say that one of the parties is more conservative than the other at the national level? Which party is more conservative? [DO NOT READ ANSWER CATEGORIES]	1. Republicans, Republican Party 2. Democrats, Democratic Party 3. Other 8. [if volunteered by respondent:	84.4 3.0 1.3 11.4	85.6 5.3 1.5 7.6

		DON'T KNOW]		
Q14	What is your age?	[CODE ACTUAL AGE. REFUSED=99]		
Q15	What was the last level of schooling you completed?	1. Less than high school graduate 2. High school graduate 3. Some college 4. College graduate 5. Post graduate or beyond 6. NO ANSWER.	0 14.8 66.2 13.9 5.1	0.5 20.7 58.6 14.6 5.6
Q16	Are you black, white, Asian, or some other race? [DO NOT READ RESPONSE OPTIONS]	1. Black 2. White 3. Asian 4. Volunteer "Hispanic" or Latino 5. Other 6. NO ANSWER	7.2 74.7 4.2 3.8 6.8 3.4	8.3 70.2 7.1 4.5 8.1 1.8
Q17	Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?	1. Republican 2. Democrat 3. Independent 8. [if volunteered by respondent: DON'T KNOW, NO PARTY, NONE OF THESE]	17.3 46.0 27.8 8.9	15.4 50.0 27.5 7.1
Q18	Do you consider yourself liberal, moderate, or conservative?	1. liberal 2. moderate 3. conservative 8. Don't Know/Not sure	46.8 35.9 13.5 3.8	49.0 39.9 7.1 4.0
Q19	Finally, this year did you happen to see any TV commercials sponsored by "Rock the Vote"?	1. Yes 2. No 3. [if volunteered: Don't Know, Don't Watch TV]	54.0 41.4 4.6	63.6 32.1 4.3
Q20	Are you male or female?	1. Male 2. Female	50.6 49.4	45.5 54.5

Appendix:

1. Evaluation of Youth Vote Email Experiment

2. Table A1: Survey Results by Site

VAR	Question	Responses	Albany	Stony Brook	Boulder	Total
Q1	Some people don't pay much attention to political campaigns. How about you? Thinking back to the election campaign, would you say that you were very much interested, somewhat interested, or not much interested in following the political campaigns this year?	1. Very Interested 2. Somewhat interested 3. Not much interested 4. Don't Know/Not Sure	61 86 24 0	39 64 28 0	156 138 37 0	256 288 89 0
Q2	In the election for President, which candidate did you prefer: George Bush, Al Gore, Ralph Nader, or some other candidate? [DO NOT READ RESPONSE OPTIONS]	1. Bush 2. Gore 3. Nader 4. Other 5. Don't Know/Not Sure 6. Refused	34 107 22 0 2 6	17 88 12 1 7 6	70 168 76 7 2 8	121 363 110 8 11 20
Q3	Did you feel strongly about your favorite candidate or did you have mixed feelings about him? [DO NOT READ RESPONSE OPTIONS]	4. Felt strongly 5. Had mixed feelings 6. Don't know	95 73 3	72 54 5	174 148 9	341 275 17
Q4	As you know, the political parties try to talk to as many people as they can in order to get them to vote for their candidate. Did anyone from one of the political parties call you up or come around and talk to you about the campaign this year? [DO NOT READ RESPONSE OPTIONS]	1. Yes 2. No 3. DON'T KNOW	29 142 0	30 101 0	71 259 1	130 502 1
Q5	How about someone from a nonpartisan organization, like Campus Greenvote or Youthvote 2000? Did anyone from a nonpartisan group call you up or come around and talk to you about the campaign this year? [DO NOT READ RESPONSE OPTIONS]	1. No 2. Yes, called 3. Yes, came around in person 4. Yes, called and came around in person 5. DON'T KNOW	122 13 32 3 1	100 12 14 3 2	225 45 50 10 1	447 70 96 16 4
Q6	During the last few weeks of the campaign, did you ever discuss politics with your family or friends? [DO NOT READ RESPONSE OPTIONS]	1. Yes 5. No 8. DON'T KNOW	167 4 0	116 15 0	317 14 0	600 33 0
Q7	During the campaign, did you talk to any people and try to show them why they should vote for or against one of the parties or candidates? [DO NOT READ RESPONSE OPTIONS]	1. Yes 5. No 8. DON'T KNOW 9. NO ANSWER; REF	111 58 2 0	88 40 1 2	228 101 1 1	427 199 4 3
Q8a	I'm going to read a pair of statements about voting,	1. Voting is exciting	139	98	289	526

	and please tell me which one come closer to your view:	2. Voting doesn't really interest me.	14	25	28	67
		3. Not sure	18	8	14	40

Q8b	I'm going to read a pair of statements about voting, and please tell me which one come closer to your view:	1. I feel that voting is an important civic duty. 2. It's okay to skip an election." 3. Not sure	158 13 0	116 13 2	309 21 1	583 47 3
Q8c	I'm going to read a pair of statements about voting, and please tell me which one come closer to your view:	1. Voting gives me the opportunity to influence government 2. One vote really doesn't make any difference. 3. Note sure	144 27 0	108 22 1	290 31 10	542 80 11
Q8d	I'm going to read a pair of statements about voting, and please tell me which one come closer to your view:	1. I find voting easy to do. 2. I not always sure about where to vote or how to cast my ballot." 3. Not sure	150 21 0	115 14 2	278 50 3	543 85 5
Q9	Some people try to avoid getting into political discussions because they think that people can get into unpleasant arguments. Other people enjoy discussing politics even though it sometimes leads to arguments. What is your feeling on this -- do you usually try to avoid political discussions, do you enjoy them, or are you somewhere in between? [DO NOT READ RESPONSE OPTIONS]	1. AVOID DISCUSSIONS 2. ENJOY DISCUSSIONS 3. IN BETWEEN 8. DON'T KNOW 9. REF/NO ANSWER	12 91 66 2 0	13 59 57 1 1	38 175 118 0 0	63 325 241 3 1
Q10a	Sometimes politics and government seem so complicated that a person like me can't really understand what's going on.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent]: DON'T KNOW	13 74 9 32 42 1	22 58 5 27 18 1	36 126 8 74 87 0	71 258 22 133 147 2
Q10b	Having elections makes the government pay attention to what the people think.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent]: DON'T KNOW]	61 71 6 24 9 0	51 60 1 14 4 1	99 161 16 36 19 0	211 292 23 74 32 1

Q10c	I feel that I have a pretty good understanding of the important political issues facing our country.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent: DON'T KNOW]	78 77 5 11 0	48 55 3 16 9	151 151 12 14 3	277 283 20 41 12
Q10d	There are important differences in what the Republicans and Democrats stand for.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent: DON'T KNOW]	79 60 7 13 11 1	69 40 5 12 4 1	162 99 10 37 21 2	310 199 22 62 36 4
Q10e	No matter who people vote for, it won't make any difference to what happens.	1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly 8. [if volunteered by respondent: DON'T KNOW]	8 14 4 50 95 0	4 21 2 26 78 0	13 39 11 81 187 0	25 74 17 157 360 0
Q11	How much of a majority is required for the U.S. Senate and House to override a presidential veto? [DO NOT READ ANSWER CATEGORIES]	1. If the respondent answers "two-thirds majority." 2. If the respondent answers something else. 8. [if volunteered by respondent: DON'T KNOW]	98 19 54	76 13 42	187 30 114	361 62 210
Q12	Do you happen to know which party has the most members in the House of Representatives in Washington now? [DO NOT READ ANSWER CATEGORIES]	1. Republicans or Republican Party 2. Democrats or Democratic Party 3. Other 8. [if volunteered by respondent: DON'T KNOW]	117 22 3 29	89 18 1 23	241 43 15 32	447 83 19 84
Q13	Would you say that one of the parties is more conservative than the other at the national level? Which party is more conservative? [DO NOT READ ANSWER CATEGORIES]	1. Republicans, Republican Party 2. Democrats, Democratic Party 3. Other 8. [if volunteered by respondent:	139 13 2 17	101 7 1 22	299 8 6 18	539 28 9 57

		DON'T KNOW]				
Q14	What is your age?	[CODE ACTUAL AGE. REFUSED=99]				
Q15	What was the last level of schooling you completed?	1. Less than high school graduate 2. High school graduate 3. Some college 4. College graduate 5. Post graduate or beyond 6. NO ANSWER.	0 31 114 22 4	1 40 88 2 0	1 46 187 67 30	2 117 389 91 34
Q16	Are you black, white, Asian, or some other race? [DO NOT READ RESPONSE OPTIONS]	1. Black 2. White 3. Asian 4. Volunteer "Hispanic" or Latino 5. Other 6. NO ANSWER	25 126 4 5 9 2	22 44 27 12 22 4	3 285 7 10 17 9	50 455 38 27 48 15
Q17	Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?	1. Republican 2. Democrat 3. Independent 8. [if volunteered by respondent: DON'T KNOW, NO PARTY, NONE OF THESE]	27 97 38 9	17 72 31 11	58 138 106 29	102 307 175 49
Q18	Do you consider yourself liberal, moderate, or conservative?	1. liberal 2. moderate 3. conservative 8. Don't Know/Not sure	82 58 18 13	57 51 15 8	166 134 27 4	305 243 60 25
Q19	Finally, this year did you happen to see any TV commercials sponsored by "Rock the Vote"?	4. Yes 5. No 6. [if volunteered: Don't Know, Don't Watch TV]	109 57 5	82 43 6	189 125 17	380 225 28
Q20	Are you male or female?	1. Male 2. Female	85 86	63 68	152 179	300 333