Most of the recent literature that investigates voter turnout and mobilization finds little or no impact on turnout as a result of get-out-the-vote (GOTV) phone drives. This article describes four field experiments embedded within GOTV phone drives conducted in the San Francisco Bay Area in November 2002 and October 2003. One of the drives was strictly nonpartisan, two were explicitly partisan (Democratic), and the last was quasi-partisan in that it was explicitly in opposition to a municipal proposition. These experiments were designed to investigate whether GOTV phone drives are effective tools for increasing turnout. None of the GOTV phone drives with a partisan or quasi-partisan component resulted in a detectable increase in voter turnout. The overall results raise serious questions about the efficacy of GOTV phone drives, particularly those with the intent of affecting electoral outcomes.

**Keywords:** voting; turnout; GOTV; mobilization; partisan; experimental design; random assignment

This article seeks to test the hypothesis that telephone-based voter mobilization drives increase voter turnout. It also explores the additional question of whether partisan cues are relevant to the efficacy of a mobilization effort. Studying the impact of a mobilization drive on turnout within different partisan contexts improves the external validity of the findings because it reflects the different varieties of get-out-the-vote (GOTV) drives to better reflect diverse political realities.

**Phone-Based GOTV—What’s on the Line? Field Experiments with Varied Partisan Components, 2002-2003**

**By**

JOHN E. MCNULTY

John E. McNulty is an assistant professor of political science at the State University of New York at Binghamton. His areas of interest include political behavior, voting behavior, campaigns and elections, political parties, American politics, technology and politics, and methodology. His dissertation is on the costs and benefits of voter turnout. He has been published in the American Political Science Review, Perspectives on Politics, and American Politics Quarterly. Together with Henry E. Brady, he won the APSA’s Political Methodology section’s 2004 Harold F. Gosnell Prize for the best paper presented in political methodology in the past year.

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I seek to determine whether the express intent of different types of GOTV drives has any effect on their success. Increases in turnout due to partisan mobilization are not spontaneous and do not produce extra marginal votes in proportion to the population; rather, they are carefully designed by partisan elite actors with the goal of increasing the vote share of one side of a political question only by some nonzero amount. Therefore, I seek to measure not just the question of whether Democrats do better with some undifferentiated amount of higher turnout but whether campaigns—Democrat, Republican, or nonpartisan—can tangibly help themselves win by running a telephone GOTV campaign targeted at registered supporters they have preidentified. This is distinct from a general broad-based GOTV drive that is motivated (at least overtly) by a (small-d) democratic goal, the desire for an electorate that is maximally representative of the citizenry.

To investigate their effectiveness, this article uses an experimental design embedded within one nonpartisan phone drive, one phone drive affiliated with a proposition campaign, and two partisan phone drives affiliated with the Democratic Party to investigate these questions. The experimental design is useful because it provides a means for directly observing the effect of the independent
variable on the dependent variable while holding all other possible variables constant via random assignment to treatment and control groups. Random assignment is critical here for dealing with other covariates that affect voter turnout, especially ones such as political interest or lifestyle that are unobservable for all practical purposes.

The research design for all of these experiments is as follows. In a universe of registrants, randomly assign some to be contacted by a voter mobilization campaign (the treatment group) and some not to be contacted (the control group). Then, observe the difference in turnout between the two groups and the differences in turnout between comparable subsets in the two groups. The data on whether a registrant voted is available as a matter of public record, and voters can be tracked by registration number.

San Francisco “No on D” Phone Drive (2002)

During the 2002 general election, I obtained the cooperation of an initiative campaign in the city of San Francisco, “No on D.” Proposition D was the latest version of a proposition that San Francisco voters had rejected in several earlier incarnations. The activist community of San Francisco has long sought to transfer control of the city’s power utilities, currently controlled by Pacific Gas & Electric (PG&E), to municipal government. The campaign opposing Proposition D, San Franciscans against the Blank Check, was composed of civic, business, and political groups that opposed the municipal seizure of the assets of the power company. The effort was organized, coordinated, and funded by PG&E itself.

PG&E and its primary political consultant, Solem & Associates, agreed to permit me to embed an experimental design into their GOTV drive just before Election Day. A control group of 1,485 registrants was selected from a list of voters targeted as likely to oppose the proposition. The remaining portion of the list—totaling 28,479 registrants—was targeted to receive a heavy GOTV treatment.

The “No on D” campaign commissioned several polls to ascertain the attitudes of the city’s voters and the distributions of those attitudes across several demographic variables. Their efforts told them who their likely supporters were and how to reach them. The campaign sought to identify people on two dimensions. The first, obviously, was the likelihood of voting the “right” way—that is, against Proposition D. The second was the probability of turning out to vote at all. This probability was estimated based on past voting history and key demographic factors.

The group of voters that the “No on D” campaign eventually targeted were its putative supporters. They self-identified as relatively more conservative, and they had generally moderate attitudes about various city issues—they were more skeptical about progressive reform proposals from the Board of Supervisors and were less overtly hostile toward PG&E (although PG&E was not well liked by any subgroup). Home owners were also targeted because they tend to be reliably moderate to conservative on economic issues relative to renters. Home ownership also
serves as a proxy for several demographic variables that are indicators of probable opposition, such as age, socioeconomic status, and residential stability. In addition, this specific proposition would affect home owners more directly than renters because it could result in fluctuations in energy costs and changes in the local tax burden.

The targeted registrants’ predicted turnout was about three points higher than those not targeted. The overall partisan and ideological breakdown was more moderate, in terms of both party registration (all Republicans and Libertarians were targeted) and in terms of self-identified ideology. Additionally, Democrats were disproportionately targeted relative to decline-to-states (DTS) for two reasons: first, because in San Francisco Democrats tend to be slightly more centrist in their attitudes than DTS voters and second, because they tend to have a higher degree of political knowledge, which makes their responses to specific ideological cues more predictable (cf. Converse 1964; Zaller 1992). The treatment was a phone call from a commercial phone bank on either the Thursday or the Friday preceding the November 5 election. No messages were left on answering machines. Other campaign activities went on outside the experimental purview. For instance, a field operations team was employed to distribute door hangers in targeted areas; they worked from the Friday before the election through Election Day itself. They were not specifically charged to interact with voters; their task was to distribute the door hangers as widely as possible in areas known to be congenial to “No on D” from past election returns. The paid staff alone contacted nearly ten thousand homes; their work was supplemented by volunteers and supporters from the International Brotherhood of Electrical Workers, who cooperated with the field operations team but did not keep records on where they had been or how many people they had contacted. Experimental controls were not available on the precinct walkers.

There was extensive media activity, including advertisements on television, radio, and billboards. There were mass mailings describing the deficiencies of Proposition D. The mailings, unsurprisingly, went to approximately the same people who were on the GOTV phone list. These were viewed as a form of advertising and public education more than as GOTV, so no experimental controls were sought. Absentee ballot applications were included in the mailings, however, so these mailings did have substantial potential for mobilization. Since both the treatment and the control groups received the mailings, random assignment still obtains—the only concern is that the effect of the randomized telephone-based treatment might be decreased.

I obtained part of the “No on D” call list and was able to randomly assign approximately 5 percent of the people on this list to a control group. Members of the control group were removed from the list that was sent on to a commercial call center in the Midwest. Data were collected on the disposition of each call, including whether the call was completed successfully, the date and time of the call, and the registrant’s answer to the question on Proposition D vote intention, if one was provided.
The “No on D” campaign focused its effort on two distinct segments in the population of San Francisco registrants. The first segment included those San Francisco Republicans for whom GOTV would have the highest marginal effect—that is, those whose turnout prospects were uncertain—and expended little GOTV effort on the remaining Republicans. This conserved valuable resources for use on the second segment—other registrants who, while not registered as Republicans, were considered to be highly probable to oppose the proposition as well and for whom a GOTV contact might be of high utility to increase their probability of turning out to vote against D.

The randomized list was primarily composed (98 percent) of non-Republicans—Democratic and DTS registrants. Most Republicans targeted for GOTV activity were on a separate list that, for technical reasons, was not available for experimental manipulation. There are some scattered Republicans in the experimental sample (around four hundred), but the number is not large enough for intensive analysis. Fringe parties are also represented in the sample, but they are negligible in size. The main comparisons in this article, hence, will look at the differences between the treatment and control groups. This precludes me from drawing conclusions about the GOTV campaign’s effect on Republicans; however, I am able to draw excellent inferences about the other two main groups of Democratic and DTS registrants.\(^{12}\)

**Results**

The “No on D” coalition chose 44,796 registrants as likely supporters to be included in a GOTV phone drive held on Thursday, October 31, and Friday, November 1. As discussed above, only part of this group was included in the experimental process; this is the experimental subgroup, which has a treatment and a control group. This group of 29,964 registrants—mostly Democrats and DTS, non-Republicans—is the one to which random assignment was administered.

There are no substantive or statistical differences in turnout between the treatment and control groups that can be ascribed to the treatment. The main finding for this experiment is null—there is no evidence that the GOTV phone calls had any systematic effect on turnout. Furthermore, the size of the difference is substantively small: 0.23 percent (with a standard error of 1.3). If the true treatment effect was 0.23 percent, that implies that the GOTV phone effort turned out about one hundred voters\(^{13}\)—a very poor return on forty-five thousand phone calls. The charge per call was 58.8c. Factoring in some rebates (busy signals, etc.), the total spent was just over $22,000; the estimated cost per vote (i.e., the amount spent to get one additional registrant to the polls) was more than $200.\(^{14}\)

Both the intended and actual recipients of the GOTV treatment as a whole, and the Republican and non-Republican groups within that whole, are significantly different in type from the populace as a whole, and they differ in very instructive ways. In total, the turnout rate of this group, 48.9 percent, is slightly lower than that of the
city at large, which was 50.1 percent, but that is entirely due to the GOTV effort’s focus on low-turnout Republicans. The Republicans in the sample turned out at a mere 32.5 percent, while the non-Republicans in the mobilization drive have a much higher turnout rate than the full universe of registered voters at 57.4 percent.

Contacting low-turnout Republicans was, as alluded to above, actually intentional on the part of the campaign. The assumption, based on theory and confirmed by polling data, was that Republicans were safe “No on D” partisans regardless of any other personal demographic or attitudinal variables. Therefore, efforts were focused on those Republicans with a relatively low probability of voting, based on their prior history. This decision is based on the theory that voting probabilities are not linear but logistic, following a basic S-shaped pattern with shallow slopes in the high and low ranges, steep slopes in the midranges, and an inflection point. If the impact of a GOTV phone call is constant on the underlying continuous propensity of voting, it will change the marginal probability of voting much more in the midrange than in the upper or lower range; that is, the sporadic voter with turnout in the 50 percent range will be much more affected than habitual voters or habitual nonvoters. If a campaign is a “rational prospector,” it should focus its efforts on those voters in the midrange (Brady, Schlozman, and Verba 1999) because that will maximize the marginal impact of the treatment on the voter. The “No on D” campaign assumed that a registered Republican, once aware of its content (essentially, an involuntary government takeover of the power company, with imminent tax and rate increases), would oppose Proposition D as a matter of fundamental principle. Conversely, the non-Republicans were more likely to vote, but less likely to be reliable no votes; the purpose of their phone calls was more persuasive.

The effects estimated in Table 1 are intention-to-treat effects, not actual treatment effects. An intention-to-treat effect is the expected change in the dependent variable incurred solely by assignment into the treatment group. Not everyone assigned to the treatment group will successfully receive the treatment; there is variation in how easy people are to reach and how receptive or unreceptive they are.

### Table 1

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>Democrat</th>
<th>Decline-to-State</th>
<th>Republican</th>
<th>Other</th>
<th>Turnout</th>
<th>Contact Rate*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control group</td>
<td>1,485</td>
<td>61.9</td>
<td>35.3</td>
<td>2.2</td>
<td>0.7</td>
<td>57.4</td>
<td>NA</td>
</tr>
<tr>
<td>Treatment group</td>
<td>28,479</td>
<td>62.7</td>
<td>35.5</td>
<td>1.4</td>
<td>0.4</td>
<td>57.6</td>
<td>50.2</td>
</tr>
</tbody>
</table>

*Contact rate refers to the percentage of people assigned to the treatment group who received a successful treatment; not everyone who was supposed to get a phone call received one, for a variety of reasons.
to unsolicited calls. Random assignment ensures that the proportion of easy-to-treat people to hard-to-treat people should be the same in both experimental groups, however, so it can be assumed that the contact rate would be comparable. Thus, the actual treatment effect—the impact on one’s probability to vote based on successfully receiving the treatment—can be simply estimated by dividing the intention-to-treat effect by the contact rate (a contact being the successful completion of a GOTV call). These results are displayed in Table 2. A two-stage least squares regression generates exactly the same figures as coefficients and provides the standard errors.

### East Bay Experimental Setting and Design

For the 2002 general election, I obtained the cooperation of the Youth Vote coalition at the University of California, Berkeley. Youth Vote is a national non-profit public service organization funded by the Pew Foundation; its mission is to increase voter turnout among the young. During an election season, chapters activate and mobilize around the country. A Youth Vote chapter coalesced at UC Berkeley, comprising a coalition of organizations affiliated with the university. The major components of this coalition were the Associated Students of UC (the student government); the Graduate Assembly (graduate student government); CalPIRG (an environmental and social activism organization); and the Cal-Berkeley Democrats (Cal Dems), a student group aligned with the party.

This coalition took on two tasks. The first was a voter registration drive in and around the UC Berkeley campus. The intent was to try to register young people (although citizens of any age who wanted to register were welcomed). This went on from shortly after the beginning of the fall semester until the registration deadline fifteen days before Election Day on November 5. The coalition retained copies of these registration forms for its planned second task, the voter mobilization drive.

Obtaining names through a registration drive supplies an interesting collection of voters that is not typical of the population of registrants as a whole. The universe of registered voters in general includes a large proportion of people whose turnout behavior can be predicted with a high degree of confidence regardless of any mobilization efforts. The best predictor of future behavior is past behavior—if one observes whether registrants participated in past elections, one can make sound

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### Table 2

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>Turnout</th>
<th>Intention-to-Treat (SE)</th>
<th>Contact Rate</th>
<th>Estimated Actual Treatment (SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment</td>
<td>28,479</td>
<td>57.6%</td>
<td>0.2%</td>
<td>50.2%</td>
<td>0.5%</td>
</tr>
<tr>
<td>Control</td>
<td>1,485</td>
<td>57.4%</td>
<td>(1.3%)</td>
<td>(2.6%)</td>
<td></td>
</tr>
</tbody>
</table>

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15. The actual treatment effect is an estimate of the impact on one's probability to vote based on successfully receiving the treatment.

16. A two-stage least squares regression generates exactly the same figures as coefficients and provides the standard errors.

17. This coalition took on two tasks. The first was a voter registration drive in and around the UC Berkeley campus. The intent was to try to register young people (although citizens of any age who wanted to register were welcomed). This went on from shortly after the beginning of the fall semester until the registration deadline fifteen days before Election Day on November 5. The coalition retained copies of these registration forms for its planned second task, the voter mobilization drive.

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guesses about their participation in future elections. However, the new registrants have no voter history to draw inferences from, so they are effectively blank slates. Following the registration deadline, voter registration information was recorded into a database. In the week before the election, the coalition called the new registrants to remind them to vote and to answer any procedural questions they might have (location of their polling place, hours of operation, etc.).

Here, the Cal Dems parted ways with the remainder of the coalition. Youth Vote and its other constituent members are all nonpartisan organizations, so they were enjoined from any overt partisan political activity. The mission of the Cal Dems, on the other hand, is to advance a partisan agenda. Thus, they took the portion of the list they registered and made calls not only encouraging participation at the polls but also endorsing specific candidates and ballot propositions.

The best predictor of future behavior is past behavior—if one observes whether registrants participated in past elections, one can make sound guesses about their participation in future elections.

At this stage, then, the narratives diverge. I will first describe the Youth Vote GOTV drive and then return to the activities of the Cal Dems.

Youth Vote GOTV Phone Drive (2002)

Working in part with Robert Thorman of Youth Vote and David Nickerson of Yale University, I randomly assigned the new registrants to different lists that would either receive a treatment of some kind—that is, a GOTV phone call—or would not. Calls were placed between 5 p.m. and 9 p.m. on Wednesday October 30, Sunday November 3, and Monday November 4 from various offices on the Berkeley campus made available for the effort. (The election was on Tuesday November 5.) The content of their message was essentially as follows:

Hello, this is [name] calling on behalf of the [National; Berkeley] Youth Vote Coalition. Youth Vote is a nonpartisan coalition that encourages young people to vote. We want to strongly encourage you to exercise your right to vote on Tuesday November 5th, and also to remind others to vote. Thank you for getting out the youth vote. Good-bye.
In addition to creating a control group, which did not receive any mobilization calls, the treatments were randomly varied, sometimes identifying as the National Youth Vote Coalition and sometimes the Berkeley Youth Vote Coalition. This was an additional twist to the experiment to see if branding the organization as national or local would make any difference. No distinction was made to indicate whether Berkeley Youth Vote referred to the city of Berkeley or the university; interpretation was left up to the registrants.²⁰ If callers were able to engage a registrant, they were instructed to offer help with locating their polling place, just as the Cal Dems were. Additionally, they would, when appropriate, direct registrants to www.youthvote.org for nonpartisan voting information. However, callers were specifically proscribed from mentioning candidates or discussing issues at all to preserve the nonpartisan nature of the effort. Some of the callers were paid as part of their work-study appointments, while others were volunteers. Most were undergraduates; all were thirty or younger.

Results

The nonpartisan mobilization experiment was modestly successful (see Table 3). The phone calls seem to have had the desired effect, as turnout is generally driven in the proper direction. The engine of the treatment effect here is phone calls made to Democrats by people who identified themselves as from the Berkeley Youth Vote Coalition, as opposed to the alternate treatment of the callers identifying themselves as from the National Youth Vote Coalition. The positive turnout effect of these calls is quite strong and significant, with a treatment effect of 9 percent overall and between 11 and 26 percent among the treated (depending on how one defines a successful contact). Among Democrats, the local treatment results in turnout that is not only significantly greater than the turnout of the control group but also significantly greater than the turnout associated with the national treatment. The national treatment does better than the local among non-Democrats but falls short of statistical significance.

The causal relationship found with the local treatment on Democrats is strong enough to pull the local treatment for all parties, Democrats for all treatments, and all treatments for all parties to statistical significance as well. These other findings are all driven by this counterintuitive phenomenon of new Democratic registrants responding to a nonpartisan prompt from Berkeley Youth Vote. One would tend to expect precisely the opposite: that an overt appeal to party identification would tend to penetrate more deeply among partisans than a more general appeal to age cohort like the one Youth Vote offered. Why is this not the case here? To discount one possibility, randomization did not fail; there is no statistically or substantively significant difference between any of the groups in partisan affiliation, age, gender, contact rates, or anything else on which I have data. The effect appears to be real, in Berkeley at least, and the puzzlement of a nonpartisan drive having a decidedly partisan effect is intriguing.

The differences between turnout rates on calls to non-Democrats and on calls that received the “national” treatment and their corresponding control groups are
<table>
<thead>
<tr>
<th>Turnout</th>
<th>Contact Rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>n</td>
</tr>
<tr>
<td>Control group</td>
<td>570</td>
</tr>
<tr>
<td>Full treatment group</td>
<td>1,519</td>
</tr>
<tr>
<td>Intent-to-treat effect (SE)</td>
<td>4.4*</td>
</tr>
<tr>
<td>Treatment group, &quot;national&quot;</td>
<td>750</td>
</tr>
<tr>
<td>Intent-to-treat effect (SE)</td>
<td>3.6</td>
</tr>
<tr>
<td>Treatment group, &quot;local&quot;</td>
<td>769</td>
</tr>
<tr>
<td>Intent-to-treat effect (SE)</td>
<td>5.2*</td>
</tr>
</tbody>
</table>

a. Contact rate refers to the percentage of people assigned to the treatment group who received a successful treatment; not everyone who was supposed to get a phone call received one, for a variety of reasons. The base is the control group.

*p < .05. **p < .01 (one-tailed test).
not statistically significant absent the peculiar Democratic responsiveness to the "Berkeley" treatment. However, it is worth noting that virtually all of the treatment coefficients are positive. The small sizes of the differences mean that one cannot rule out random chance with much confidence; however, the results are suggestive to some degree that these calls increased turnout. The one minor exception of Republican turnout decreasing when assigned to receive the local treatment is logically consistent with the main finding: if Democratic registrants are energized to vote by a reference to Berkeley, it seems plausible that Republicans might be discouraged.

Cal-Berkeley Democrats GOTV Phone Drive (2002-2003)

In 2002, the Cal Dems had fewer resources for their drive than Youth Vote did. They had fewer callers, all of whom were undergraduates who worked as volunteers, and they had no direct access to a phone bank. They got Tom Bates, their endorsed candidate for Berkeley mayor, to donate use of the phone bank in his campaign headquarters from 8:00 p.m. to 10:00 p.m. on Sunday November 3 and Monday November 4. Given these constraints, they were only able to make about five hundred calls. However, the calls they did make transmitted a direct appeal to party preferences:

Hi, my name is __________, with the Cal Dems. I’m calling to remind you to vote for the Democrats this coming Tuesday, November 5. Here in Berkeley, we urge you to vote for Tom Bates for Mayor. For the city council, we urge you to vote for Kris Worthington in district 7 and Andy Katz in district 8. Also, it is important for you to remember to support the school bond, prop. 47, and same day voter registration, prop. 52. Thanks, and don’t forget to vote.

The Bates campaign office also had information on the location of polling places, poll hours, and any other logistical information an aspiring voter might require, so callers offered help on those matters. Callers also answered questions about the various candidates and initiatives when asked, although this happened infrequently. These calls were made to a stratified sample rather than a purely random one; the Cal Dems wished to focus their efforts on friendly partisans. Details of the stratification are available in Appendix D, part 3.

The unexpected emergence of the gubernatorial recall provided the Cal Dems with the opportunity to replicate their 2002 effort. The election was certified in August of 2003 to take place on October 7 as a special statewide election. The recall consisted of a two-part ballot question. The first part was whether the incumbent governor, Governor Gray Davis, should be recalled. A yes vote was a vote to recall the governor, and a no vote was a vote to retain the governor. The second part was a list of candidates to assume the governor’s office if it was indeed vacated by the previous question. The candidate with a plurality of votes would win the governorship. The requirements to gain ballot access, set nearly a century prior, were so low by modern standards that 135 candidates qualified. Media coverage and candidate
resources were able to effectively winnow the field of serious contenders to roughly eight, some of whom withdrew themselves from consideration before the election, leaving only Lieutenant Governor Cruz Bustamante, actor/businessman Arnold Schwarzenegger (the eventual winner following Davis’s recall), state Senator Tom McClintock, and Green Party standard bearer Peter Camejo as plausible candidates. The remaining 127 ballot qualifiers were novelty candidates. Given how different the circumstances of these two elections [2002 general and 2003 recall] were, taking place less than one year apart, that essentially the same results were generated in both field experiments is an indication of robustness in the finding of no effect.

By statute, certified statewide Propositions 53 and 54 appeared on the ballot as well. Prop. 53 was a run-of-the-mill transportation measure, but Prop. 54 was a controversial measure designed to prevent the state government from collecting data on racial classifications. This became a significant issue in the campaign, especially among Democrats; Lt. Gov. Bustamante, the only major Democratic candidate slated to replace Governor Davis, made opposition to the proposition one of the centerpieces of his campaign. (When Bustamante lost, in fact, he claimed victory on the logic that Prop. 54 was defeated.)

The Cal Dems undertook another registration drive on Berkeley’s campus, with essentially the same purpose: to register young voters. Youth Vote only operates biennially, so Cal Dems were now operating as an independent entity and were wholly free to pursue their own agenda explicitly; that is, to pursue the interest of the Democratic Party, as they saw it. The Cal Dems registered anyone who came up to their table, regardless of party affiliation, but only databased Democrats for future GOTV activities. The eventual data set assembled was composed of approximately nine hundred people, 99 percent of whom were Democrats. The Wednesday, Thursday, and Sunday before the recall election, the Cal Dems attempted to call roughly two-thirds of the Democrats whom they had registered, to encourage them to vote on Tuesday and to encourage them to vote against the recall, against Proposition 54, and for Cruz Bustamante. The remaining registrants were held out as a control group. Random assignment was used to place the registrants in the dif-
Young undergraduate volunteers made phone calls in the evenings (between 6:30 p.m. and 9:30 p.m.) from law offices in downtown Berkeley donated by the proprietors to the Cal Dems for phone banking. Callers contacted only local voters (Berkeley and surrounding communities in Alameda County and western Contra Costa County); however, unlike 2002, no special attention was paid to Berkeley residents since there were no local races. The phone script was as follows:

**ALL:** Hello, may I speak with [name of the voter]. My name is [your first name], and I am a member of Cal Berkeley Democrats. I am calling to remind you to vote NO in the October 7th recall election. The costly Republican recall has become a circus and is an embarrassment to California. If the Republicans are successful, our laws protecting higher education, the environment, civil rights, and a woman’s right to choose will be seriously threatened. Can the Democratic Party count on you to vote NO on the Republican Recall?

**OPTIONAL**—based on perceived receptiveness. I also encourage you to vote YES on Lt. Governor Cruz Bustamante, since he is the most qualified candidate on the second part of the ballot.

**ALL:** I also encourage you to vote NO on Prop 54, since it strips doctors of vital information to fight the spread of disease and prevents the police from enforcing hate crime laws.

Callers were again offered information about polling place locations and hours, when appropriate. In addition, Cal Dem volunteers entertained questions on issues surrounding the recall or the propositions when asked.

**Results**

In 2002, the Cal Dems’ registration drive captured nearly 3,000 names. However, since the Cal Dems’ mobilization efforts were centered on Berkeley elections, they tried to call within the city of Berkeley as much as possible. Many of the new registrants did not live in the city of Berkeley. Within Berkeley, 1,917 new registrants were identified, with 542 (28 percent) receiving the treatment. The results (summarized in Table 4) indicate that in 2002 there were some differences between the treatment and control groups in overall turnout and among Democrats in particular. However, the effect is in the wrong direction. For all voters, the control group voted more than the treatment group by 4 percentage points; for the major partisan affiliations, it was 6 points, 4 points, and 12 points, for Democrats, DTSs, and Republicans, respectively. Those in the treatment group seem less likely to vote than those not in the treatment group, although this result is likely to be due to chance; it is hard to imagine a theoretically plausible explanation why these calls would suppress turnout.

The Cal Dems made several efforts to improve their strategy in 2003. They kept better records of party affiliation and phone numbers, they used a more conversational phone script, they placed a greater emphasis on contact rates (they called a smaller group to which two phone calls at minimum were placed to everyone in the intent-to-treat group who was not reached initially), and they placed calls earlier in
## TABLE 4
COMPARISON OF TWO PARTISAN DEMOCRATIC GET-OUT-THE-VOTE (GOTV) PHONE DRIVES

<table>
<thead>
<tr>
<th></th>
<th>Turnout&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Contact Rate&lt;sup&gt;b&lt;/sup&gt;</th>
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<tbody>
<tr>
<td></td>
<td>N</td>
<td>All&lt;sup&gt;c&lt;/sup&gt;</td>
</tr>
<tr>
<td>2002</td>
<td></td>
<td>(1,917)</td>
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<tr>
<td>Control group</td>
<td>1,375</td>
<td>61.8%</td>
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<tr>
<td>Treatment group</td>
<td>542</td>
<td>58.3%</td>
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<tr>
<td>Intent-to-treat effect (SE)</td>
<td></td>
<td>-3.5% (2.5%)</td>
</tr>
<tr>
<td>2003</td>
<td></td>
<td>(329)</td>
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<tr>
<td>Control group</td>
<td>329</td>
<td>83.3%</td>
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<tr>
<td>Treatment group</td>
<td>553</td>
<td>81.0%</td>
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<tr>
<td>Intent-to-treat effect (SE)</td>
<td></td>
<td>-2.3% (2.7%)</td>
</tr>
</tbody>
</table>

<sup>a</sup> Party designations here come from the Board of Elections records and not from the database generated off of the registration cards. There are some discrepancies that are not material to the findings.

<sup>b</sup> Contact rate refers to the percentage of people assigned to the treatment group who received a successful treatment; not everyone who was supposed to get a phone call received one, for a variety of reasons.

<sup>c</sup> This partisan breakdown may appear unusual, but it is reflective of the leanings of the area on or about the UC Berkeley campus, where the registration drive took place. Also, the Cal Dems were the group doing much of the registering; people registering at their tables usually register Democratic.

<sup>d</sup> This number refers to the number of total registrants in the group in 2002. In 2003 the number of total registrants were 882; all but 14 were Democrats [see note 29].

<sup>e</sup> "Messages" include messages left on answering machines, voice mails, and live messages with people cohabiting with the intended recipient of the call.

<sup>*</sup> $p < .05$ (one-tailed test).
the evening. Despite all these efforts of the Cal Dems, the results of the 2003 GOTV drive remain discouraging for proponents of partisan phone drives. No statistically significant difference is present between the randomly selected treatment and control groups, and once again the difference that does exist is in the wrong direction. The contact rate was about the same among those assigned to be treated, although a higher percentage of the total group was contacted because the group was smaller.

It is striking that this pattern persists through two very different types of elections. The overall turnout was tremendously different, as one would expect. The 2002 race was a regular midterm election in which state offices were being contested. The governor’s race headling the ticket featured an ineffectual, unpopular incumbent (Gray Davis) and an inexperienced, gaffe-prone challenger (Bill Simon Jr.), in what was widely considered to be the least appealing choice for California voters in recent memory. Conversely, the recall election of 2003 drew perhaps more media attention than any nonpresidential race in American history. It involved the historic usage of a dusty constitutional mechanism, the recall, during a time of dramatic fiscal crisis for the state, and the leading challenger to the sitting governor was one of the world’s most famous and popular movie stars. The recall election was anticipated to have higher participation across the board. Statewide, voter turnout was up eleven points from the election eleven months prior. It is difficult to overstate how distinctive the 2003 gubernatorial recall was. It drew interest from people whose political interest is otherwise nonexistent through the sheer prominence and the epic scope of the event. Given how different the circumstances of these two elections were, taking place less than one year apart, that essentially the same results were generated in both field experiments is an indication of robustness in the finding of no effect.

Conclusions and Implications

Archetypal partisan GOTV phone drives, as conducted in these cases by the Cal-Berkeley Democratic Party organization, apparently do not work, at least in this environment. The GOTV drive that the Cal Dems conducted over the last two major elections is a fairly typical one. While the population is atypical, the method is standard. Furthermore, the partisan GOTV drive observed in San Francisco designed to encourage opposition to Proposition D also failed to result in any substantively or statistically significant increase in voter turnout. Even the nonpartisan campaign was only effective under limited circumstances.

The peculiar phenomenon of nonpartisan GOTV calls working best on a very specific and decidedly partisan subset of individuals—people who registered as Democrats roughly ninety days or less before the 2002 election—merits further discussion. It is noteworthy that these people were registered at an organized turnout drive located on and around the campus of UC Berkeley. The turnout drive was explicitly aimed at young people, as was the mobilization message in the phone call, and the aim was true—the average age of the new registrants was twenty-three for
Democrats and twenty-two and a half overall, while the median age overall (a better measure for these data) is about twenty-one. Furthermore, mobilization of this group of young Berkeley Democrats was much more effective when the caller explicitly made a claim of a community tie to Berkeley; the perception that the mobilization was a national effort rather than a local one stunted the impact of the treatment.

The preponderance of the evidence, ultimately, implies that GOTV [get-out-the-vote] phone calls are inefficient at increasing turnout. In particular, the evidence is very strong that these calls do not increase turnout among those who are already likely supporters of one's side.

This is a potentially important finding, but it seems exceedingly hard to generalize. An omnipresent threat to validity in field experiments is extrapolating findings outside of the setting and circumstances of the study. Berkeley is so heavily associated with progressive politics that it is virtually synonymous with such movements. Thus, it may act as shorthand to voters—anyone calling associated with Berkeley is assumed to be calling from the left side of the political spectrum. This treatment, therefore, perhaps made an ostensibly nonpartisan drive be perceived as implicitly partisan by recipients of the GOTV message.

Furthermore, I do not know if circumstances in the 2002 general election in California helped create this turnout effect, or if the phenomenon is particular to people who tend to be in the vicinity of the San Francisco Bay, or if the effect would exist in a group of Berkeley Democrats who had been registered for a longer period, or if it would exist among San Francisco Republicans. One might presume that the youth-centered appeal in the East Bay studies would lose some luster among the more mature, but in fact, when people over age twenty-eight (roughly 10 percent of the people in the study) were removed from those analyses, the efficacy of the phone calls decreases. (So perhaps flattery is helpful.)
I also do not know if the tie to the local community would translate outside of Berkeley. Berkeley is an extraordinarily highly educated community, with an intensely activist and contentious politics conducted on the (far) left side of the political spectrum. Its citizens are highly informed, and they are not tethered to political parties in typical ways. Many Berkeleyans are disaffected from the Democratic Party because they see it as too far to the right, and thus, while they tend to be supportive of Democrats and liberal policies in general, they are suspicious of the Democratic party as an institution. The Green Party is large and active in Berkeley, with 7 percent of the registrants, versus 2 percent in Alameda County and 1 percent in California, and they had an extraordinary rate of mobilization in the 2002 general election, turning out 80 percent of new registrants, versus 66 percent of new Democrats, 59 percent of new Republicans, and 56 percent of new nonaligned voters. The sui generis politics of Berkeley may well be responsible for the tepid, even negative reaction to direct partisan mobilization and an enthusiastic reaction to citizen activism because citizen activism is far more reflective of the political culture there. This is generally not the case most places in the United States, and only replication of these findings in future studies will determine whether this result is a robust one or whether it is a fluke of a highly particularized segment of the electorate.

Similarly, the San Francisco experiment excluded Republicans, so no conclusions can be drawn about their behavior. More generally, San Francisco has rather parallel comparability problems to Berkeley in terms of political culture. While the proportion of activist citizens is not quite as high as in Berkeley, it is still substantial. Furthermore, the issue of homosexual rights plays a much bigger role in San Francisco than in any other major city in the country. Demographically, although the mean level of education is much closer to normal than Berkeley is, it is still above average. Also, housing costs compete with the borough of Manhattan and Honolulu as the highest in the country in an urban setting. Despite these caveats, I can say with a great deal of certainty that the “No on D” GOTV drive had no meaningful effect on voter turnout in 2002. This particular experiment is especially compelling because of the size and scope of the effort and because it was professionally administered and run. The experimental controls were perfect, and the experimental power was relatively strong.

The preponderance of the evidence, ultimately, implies that GOTV phone calls are inefficient at increasing turnout. In particular, the evidence is very strong that these calls do not increase turnout among those who are already likely supporters of one’s side. Contrary to expectations, adding a partisan cue (be it for a party, a candidate, or for or against a ballot proposition) to a GOTV message does not enhance the efficacy of a GOTV contact at all.
Appendix A

Ballot Digests—Prop. D
November 5, 2002 General Election

Ballot Simplification Committee
FINAL APPROVED DIGEST
August 7, 2002/ 11:39 a.m./ Odell, Packard, Ollis

(PROPOSITION D)
PUBLIC UTILITIES COMMISSION, ENERGY

The Way it is Now:

The City generates hydroelectric power at its Hetch Hetchy facility in Yosemite National Park. The City uses this power to meet its municipal power needs and those of other public entities such as the Unified School District. The City sells its excess power to the Modesto and Turlock irrigation districts. The City does not sell power to San Francisco residents and businesses.

The City’s Public Utilities Commission oversees the City’s water and power utilities. This Commission has five members appointed by the Mayor. By a two-thirds vote, the Board of Supervisors can reject any of these appointments.

In general, the Public Utilities Commission must get voter approval to issue revenue bonds.

Power for San Francisco residents and businesses is provided by a private company. The State sets the rates charged for that power. Some of that power comes from a power plant in the City’s Hunter’s Point neighborhood.

The Proposal:

Proposition D is a Charter amendment that would change the authority, duties and composition of the Public Utilities Commission in order to make it the primary provider of power to San Francisco residents and businesses. The Commission would be tasked with developing sufficient and reasonably priced power for the City. One goal of this would be to allow the Hunter’s Point power plant to be closed.

The Public Utilities Commission could:

• Buy and operate power plants and distribution facilities, and sell power;
• Enter into long-term contracts to buy power; and
• Issue revenue bonds to improve, buy or build energy facilities without voter approval. The bonds would have to be approved by the Board of Supervisors and meet certain other requirements.
The Public Utilities Commission would set rates for electric power with input from a new Rate Fairness Board. These rates would have to be approved by the Board of Supervisors.

Proposition D would increase the Public Utilities Commission to seven members. Three would be nominated by the Mayor and one by the Controller, all subject to Board of Supervisors approval. The Board would appoint the three remaining members.

A “Yes” Vote Means:
If you vote “Yes,” you want to change the authority, duties and composition of the Public Utilities Commission in order to make it the primary provider of power to San Francisco.

A “No” Vote Means:
If you vote “No,” you do not want to make these changes.

*Title is for identification purposes only. The Director of Elections determines the title after the Ballot Simplification Committee completes the digest of the measure.


Appendix B
Get-Out-the-Vote (GOTV) Phone Call Script,
“No on D” Campaign

Hello, [name of voter], I’m calling on behalf of the Coalition for San Francisco Neighborhoods reminding you to oppose Proposition D.

Proposition D is a risky scheme that allows the PUC\(^a\) to issue revenue bonds without voter approval. These bonds would be paid back through higher utility rates.

Can we count on you to join us in opposing Proposition D next Tuesday?\(^b\)

A. Yes
B. No
C. Undecided

\(^a\) Public Utilities Commission.
\(^b\) The answers here were recorded, but the main purpose of the question was to elicit a verbal commitment from the voter, hopefully increasing the propensity to vote even more.
Appendix C
Berkeley Youth Vote Phone Banking Script

LIVE
—Hi, may I speak to {full name}
—Hello, this is {your name} calling on behalf of the {National; Berkeley} Youth Vote Coalition. I am not calling to sell you anything or to ask for money. Youth Vote is a non-partisan coalition that encourages young people to vote.
—Can we count on you to vote on Tuesday, November 5th?

If YES: Great! Thank you very much for voting. Please remind your friends and loved ones to vote as well. Do you know how to find your polling place? (If yes) OK; goodbye. (If no) You can go to www.ss.ca.gov, or in Alameda County, call 1-510-663-VOTE (1-510-663-8683). Got that? OK; goodbye.

If NO: Well, we hope you will reconsider and vote on Election Day. Thank you; goodbye.

(If they ask) Would you like to know how to get nonpartisan voting information on the races in your area? (If yes) You can call the toll free number 1-888-Vote-Smart (1-888-868-3762) or visit www.youthvote.org.

If they ask about Youth Vote:
—We are a national nonprofit organization established to increase participation, build responsive government, and promote awareness of the power of young people voting.

REMEMBER: You can discuss issues with people generally, in terms of pointing them toward websites or local media that will tell them more about the election, but you must take a strictly “hands off” nonpartisan approach. DO NOT MENTION CANDIDATES. Try to listen to what the person is saying that is important to them, and link it to helping elect people who will make the decisions. If someone presses you on whom they should vote for or whom you are going to vote for, please explain that Youth Vote is completely nonpartisan, and you can’t talk about who to vote for, but the most important thing is that they vote.

ANSWERING MACHINE/VOICE MAIL

Hello this is {your name} calling on behalf of the {National; Berkeley} Youth Vote Coalition. Youth Vote is a non-partisan coalition that encourages young people to vote. We want to strongly encourage you to exercise your right to vote on Tuesday, November 5th, and also to remind others to vote. Thank you for getting out the youth vote. Good-bye.
Appendix D
Details on Random Assignment Protocols

1. The 2002 Youth Vote and 2003 Cal-Berkeley Democrats (Cal Dems) experiments assigned voters to treatment and control groups based on pure random sampling methods. Random numbers were generated in Microsoft Excel and assigned to each registrant in the sample population. Assignment to treatment or control groups was based on those random numbers.

2. For the San Francisco experiment, because of the format that the data were stored in, random assignment could not be done by the conventional method of random number selection. Instead, every $n^{th}$ name was flagged for inclusion into the control group so that $N$ would total to approximately 1,500. Since the data was ordered alphabetically by precinct, and $n = 30$, there is little reason to believe there is any periodicity in the data that would cause randomization problems. Posttests have confirmed they are comparable.

3. The 2002 Cal Dems experiment grouped registrants by party, when that information was known, and created an “unavailable” category for when it was not (about half the cases). Random draws similar to those were made from each party affiliation where we had information (party ID was not recorded by the coder in 52 percent of the cases), with the intention of weighting the sample so that most of those called would be Democrats or Greens. The assignments were as follows:

<table>
<thead>
<tr>
<th>Party ID</th>
<th>Treatment</th>
<th>Control</th>
<th>Party ID</th>
<th>Treatment</th>
<th>Control</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrats</td>
<td>80%</td>
<td>20%</td>
<td>Minor Right</td>
<td>50%</td>
<td>50%</td>
</tr>
<tr>
<td>Republicans</td>
<td>0%</td>
<td>100%</td>
<td>Green</td>
<td>65%</td>
<td>35%</td>
</tr>
<tr>
<td>Decline-to-state (DTS)</td>
<td>35%</td>
<td>65%</td>
<td>Unavailable</td>
<td>50%</td>
<td>50%</td>
</tr>
</tbody>
</table>

a. Libertarian, Natural Law, American Independent, and so on.

b. Checking this group against registrar records, it was 43 percent Democratic, 42 percent DTS, 6 percent Republican, 6 percent Green, and 2 percent other. This is consistent with the proportions in the group of voters I had party data for at the time.

Note that the unavailable group was just divided 50-50 between the treatment and control groups.

As a consequence of this, comparison of the entire treatment and control groups here must be taken with a grain of salt; the groups are not perfectly comparable, as they should be in an experimental setting. It is only experimentally appropriate to compare turnout rates within the above groups, although we can blend Democrats randomly selected as Democrats and Democrats randomly selected blindly.
The unavailable segment is a good proxy for the entire electorate, and they were effectively selected randomly without regard to party. Among the preidentified segment, the Democratic and decline-to-state (DTS) voters are sizable groups, and a low number of Republicans that were not identified as such slipped into the treatment group in the unidentified segment, fortuitously giving some basis for assessment of them as well. The smaller parties are mainly too small and particular to Bay Area politics to merit attention here. Party information from the registrar was used as the party identification variable.

Notes

1. See Appendix A for a digest of the ballot question.
2. The “Blank Check” refers to a part of the proposition that amends a provision in the city charter to permit revenue bonds to be issued without prior voter approval. The existing charter provision is an atypical one but is highly valued by San Franciscans who appreciate the check on their oft-profligate city fathers.
3. Indeed, because this issue recurs so often, polling never entirely ceases.
4. The consultant has a model to estimate in percentage terms the likelihood of a given voter voting in a future election, it is proprietary.
5. Power utilities are generally unpopular; however, in 2002 Pacific Gas & Electric (PG&E) had the extra burden of overcoming negative publicity related to both financial and environmental scandals, especially in northern California. These include well-publicized electrical outages and increasing utility rates throughout much of the previous two years.
6. That is, the predicted turnout of the group of targeted partisans as a whole; as will be discussed, it was decided that some of the people in this group who had the highest probability of voting would not receive get-out-the-vote (GOTV) calls since they would be likely unnecessary; thus, the turnout rate for those targeted people who were called actually turned out to be slightly lower than the city as a whole.
7. Decline-to-state, or DTS, is the designation California applies to those who do not register in a party; in practice, the label is analogous to Independent.
8. Indeed, because this issue recurs so often, polling never entirely ceases.
9. In the end, Proposition D was defeated by seven and a half points, a margin of about fifteen thousand votes.
10. See Appendix D, part 2, for details.
11. A subcontractor generated the Republican GOTV list out-of-house; the subcontractor did not maintain the file electronically but instead printed out hard copies and distributed these hard copies to the interested parties (call centers, mass mailers, etc.). Once this happened, there was no practical way to select or segregate a control group.
12. As one of several randomization checks, I regressed all possibly relevant variables on assignment to the treatment group. For the 2002 election, the $F$-statistic for that regression was 1.961 ($p = .097$). This is typically considered shy of statistical significance, but since it is close, I identified the driver of the relationship. It was major party ID—specifically, the 427 Republicans who ended up in the sample that was otherwise 98.1 percent Democratic and DTS. Via random chance, these Republicans ended up disproportionately in the control group; about a dozen more were assigned to the control group than should have been if assignment had been perfectly proportional. These Republicans are outliers—they vote more often than the Democrats by 10 percentage points and more often than the DTSs by 20 percentage points. To ensure that these outliers were not masking a true effect, I reran the regression shown in Table 1 without the Republicans. The new coefficient doubles in size to 0.47 percent but is still well short of statistical ($t = 0.356$) or substantive significance. Rerunning the $F$-statistic test with the Republicans out yields a new $F$-statistic of 0.279 ($p = .540$). For further confirmation of an effective random sample, I regressed assignment on variables from 2000, which I had for nearly 90 percent of the sample. This yielded an $F$-statistic of 0.996 ($p = .772$). I have 1998 data for two-thirds of the sample; that regression yields an $F$-statistic of 1.521 ($p = .217$). I choose to present the sample with the smattering of Republicans rather than a pure sample because I think the smattering is more realistic—it is
virtually impossible to obtain a sample of any size that will be precisely to specification, so the experiment should reflect that rather than artificially correct for it.

13. 0.23 percent \times 44,796 voters targeted for GOTV = 103 voters.

14. Total cost was $22,070, dividing by 103 voters yields $214.27.

15. Sometimes termed the “treatment effect among the treated.”

16. The dependent variable is categorical, so logistic regression is technically more appropriate. The results are essentially identical; the logistic function of turnout within these ranges is close to linear.

17. The Cal-Berkeley Republicans were also invited to join, but declined.

18. Not every single person who registered to vote through the Youth Vote drive was a brand new registrant to Alameda County. Approximately 30 percent of the database was previously registered but reregistered through the drive for any of a variety of reasons, which include changing addresses within the county, a name change following a marriage, or a change in party registration. Some may have reregistered without even needing to, out of ignorance, excessive caution, or unrelated motivations like wanting to help the volunteers or desiring the incentives offered (on different occasions candy bars, coupons for local merchants, or Krispy Kreme donuts were available).

19. The text provided is the message script left on answering machines or voice mail. Lengthy and detailed instructions were given for interacting directly with registrants; they are included in Appendix C.

20. Randomization checks: regressing party registration on assignment to the treatment group yields an F-statistic of 0.636 (p = .709). Regressing age on assignment yields an F-statistic of 0.748 (p = .387). For the roughly 20 percent of the sample who were registered in 2000, regressing their 2000 turnout on assignment yields an F-statistic of 1.457 (p = .228). Since the effect of the local treatment on Democrats is the most strongly significant, I look at that group alone; regressing the age of Democrats on whether they received the local treatment; the F-statistic was 3.135 (p = .077). The average age of Democrats assigned to receive the local “Berkeley” treatment was 1.1 year older than the average age of the rest of the sample. A multivariate regression was run to ensure that this age disparity was not driving the effect among these Democrats. It was not; the coefficient of the local treatment effect decreased only slightly with age as a covariate, from 0.095 to 0.096, and the treatment effect is still statistically significant (p = .029). The presence or absence of a curvilinear age-squared variable has no effect.

21. Minor because the sample size among Republicans is much smaller, so the statistics are more susceptible to random fluctuations.

22. Local elections in California are nonpartisan.

23. All statewide Democratic candidates and local Democratic legislative candidates won. Bates and Worthington won; Katz received enough votes to make a runoff election but eventually lost. The school bond passed; same-day registration did not.

24. Because I had to stratify my random sampling here, multiple randomization checks were necessary, and I have a number of F-scores to report. First, I report the F-statistics that correspond to actual party registration. The F-statistic for all Republicans in the sample using treatment as the dependent variable and age (no other covariates are available) as the independent variable was 0.000 (p = .986). The F-statistic for all Democrats in the sample is 0.509 (p = .476). The F-statistic for all DTS registrants is 0.306 (p = .580). The F-statistic for all Greens in the sample is 0.796 (p = .374). The F-statistic for all American Independents in the sample is 0.821 (p = .376). The F-statistic for all Libertarians in the sample is 0.573 (p = .459). Second, I report F-statistics of those party-registration data that were used to stratify the population of the experiment. Among identified Democrats, the F-statistic was 0.351 (p = .554). Among identified DTS, the F-statistic was 1.819 (p = .178). Among identified Greens, the F-statistic was 0.359 (p = .551). Among other minor parties, the F-statistic was 1.063 (p = .317). No identified Republicans were in the treatment group. Among registrants with unidentified party registration at the time of the experiment, the overall F-statistic was 1.357 (p = .239).


26. Proposition 53 also was defeated. Both proposition defeats were sizeable.
27. Fourteen non-Democrats slipped into the final data set of registrants. Nine declined to state a party affiliation (i.e., independent), four were Greens, and one was a Libertarian. This is a realistic outcome, in that most GOTV drives necessarily end up with some fraction of their lists composed of voters they are not necessarily targeting. It is probably attributable in most cases to late party switches. For the purposes of this analysis, I will ignore these minor discrepancies and treat the data as if it were all Democrats, as the Cal-Berkeley Democrats (Cal Dems) intended.

28. Regressing age on assignment to the treatment group as a randomization check yields an $F$-statistic of 0.027 ($p = .869$). About 20 percent of the sample were registered in 2002; regressing 2002 turnout on assignment yields a $F$-statistic of 1.456 ($p = .147$).

29. The decision was made to back off on Bustamante very shortly into the GOTV effort; feedback from callers had strongly indicated he was a drag on the ticket, and Cal Dems prioritized the defeat of the recall and of Proposition 54 much higher than Bustamante’s effort.

30. The city of Berkeley has the second highest proportion of Ph.Ds in the United States, behind only Ann Arbor, Michigan.

31. Another possibility that should be mentioned is that this could simply be a probabilistic fluke. All the statistical significance in this model is driven by 340 Democrats who received the “local” treatment and turned out at a higher rate than any other group, with 234 voting. It would only take about 10 fewer of these registrants balloting to fall short of even the most generous standard of statistical significance. The law of large numbers indicates that it is unlikely this happened, but it is also far from impossible.

References and Additional Resources


Green, Donald P., Alan S. Gerber, and David W. Nickerson. Forthcoming. Getting out the vote in local elections: Results from six door-to-door canvassing experiments. *Journal of Politics*.


