

## **Heeding the Call: The Effect of Targeted Two-Round Phonebanks on Voter Turnout**

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**ABSTRACT**

Field experiments in voter mobilization have indicated that personal contact is most effective, but that multiple contacts have no apparent additional impact on voter turnout. Yet, a number of theories from social psychology – cognitive dissonance theory, the theory of reasoned action, and the theory of the self-erasing nature of errors of prediction – would lead us to expect that a targeted follow-up contact, one that cues social norms, should have a greater impact on turnout. We test these theories using four phonebanking field experiments that utilized follow-up calls to committed voters. Contrary to previous studies, we find that this kind of targeted follow up greatly increases the effectiveness of phonebank campaigns, in some cases almost tripling their effect on voter turnout.

Political scientists long have argued that mobilization is a key variable in voter turnout (Rosenstone and Hansen 1993, Leighley 2001). Recent studies using field experiments have shown the power of personal tactics in mobilizing individuals to vote. In a variety of political and social environments, and targeting a variety of populations, the face-to-face interactions of door-to-door canvassing have provided the strongest boosts to turnout, usually by 7-12 percentage points (Green and Gerber 2004, Green, Gerber and Nickerson 2003a, Michelson 2003, 2006, 2006-7). Live, conversational phonebanks can also boost turnout significantly, generally by 3-5 percentage points (McNulty 2005, Wong 2005, Nickerson, Friedrichs and King 2006, Nickerson 2006a, Ramírez 2005, 2007). Indirect methods, including mailings and robocalls, consistently have failed to produce a significant mobilization effect (Green and Gerber 2004, Michelson, García Bedolla and Green 2007, García Bedolla and Michelson, forthcoming, but see Gerber, Green and Larimer 2008).

Field experiments also have explored whether there are ways to add components to this personal contact in order to make it even more effective. For example, door-to-door efforts are enhanced when drawing on a volunteer pool of neighbors or co-ethnics (Sinclair et al. 2007, Michelson 2006). Phonebank scripts that consist only of a brief reminder to participate are less effective than longer scripts, but the content of the script (the message) has been found to have no measurable effect (Michelson, García Bedolla, and Green 2007, Green and Gerber 2008). Calls have been found to be most effective during the last week prior to the election, and contacting people more than once, either by phone or in person, does not seem to increase turnout significantly more than a single phone contact (Green and Gerber 2001, 2004; Green, Gerber and Nickerson 2003b, Arceneaux, Gerber and Green 2003, Friedrichs 2002, Nickerson 2003, Nickerson 2007). On this last point, Green and Gerber (2004: 78) conclude: “Contacting

voters more than once prior to an election is a waste of resources. Multiple contacts have no apparent benefits.”

Yet, findings from social psychological research suggest that targeting a follow-up contact in a particular way could make a mobilization campaign more effective. Specifically, given that in U.S. society there are positive social norms attached to voting, contacts that elicit a commitment to vote from those targeted, and that therefore cue those social norms, may be more effective than a single contact alone. Psychologists long have recognized that the act of making a written or verbal commitment significantly increases the likelihood that an individual will engage in the pledged behavior (Cialdini 1984, Kiesler 1971), and there are three particular psychological theories that we believe are especially applicable to the question of voter mobilization: cognitive dissonance theory, the theory of rational action, and the theory of the self-erasing nature of errors of prediction. All of these theories provide a basis for hypothesizing that pledges to vote will make mobilization efforts more effective.

Cognitive dissonance theory (Festinger 1957) posits that individuals who have made an oral or written pledge to vote will be motivated to follow through on that commitment in order to avoid holding dissonant or inconsistent cognitions. In other words, asking individuals to commit to voting will increase their likelihood of doing so because they do not want to suffer the negative feelings that can result from behaving inconsistently with their initial answer. This means that promising to vote will increase the likelihood of the individual actually voting only if the idea of being inconsistent with one’s prior commitment to vote creates enough internal conflict (or potential internal conflict) to necessitate some resolution.

A second applicable theory is Fishbein and Azjen’s (1975) theory of reasoned action. Fishbein and Azjen claim that behavior is a function of intent, which is determined by individual

attitudes and perceived social norms. In the case of voter turnout, the intent to vote is thus influenced by whether other people value voting and the degree to which an individual cares what others think. In other words, the voting pledge works because it signals to the voter that voting is valued by society, and thus the promise to vote creates an intention to vote. The degree to which an individual cares about social norms and what other people think will influence whether or not they follow through and act as promised. Consistent with this understanding of the power of commitments to vote, Funk (2006) proposes that voter turnout may be partly driven by a desire to signal to others that one is a good citizen, citing evidence of decreases in voting rates after the introduction of vote by mail. This proposition is supported by evidence from comparisons of survey responses to election rolls, which have shown that voters are more likely to misreport that they did vote when they did not than the reverse (see e.g. Traugott and Katosh 1979).

A final theoretical frame is provided by Sherman's (1980) theory of the self-erasing nature of errors of prediction. The theory posits that asking people to predict their future behavior increases the likelihood of them engaging in the predicted behavior. Sherman contends that making such predictions causes individuals to imagine themselves performing the said action, and even associating the action with supporting reasons for doing so. To test this theory, small convenience-sample experiment was conducted by a team of researchers at Ohio State University prior to the 1984 presidential election (Greenwald et al. 1987). One group of students was asked to predict whether or not they would register; a second group was asked to predict whether or not they would vote. Although the experiments were small and the dependent variables measured with self-reported data, results from the second experiment were statistically significant.<sup>1</sup> Those who predicted that they would vote were much more likely to do so. An

attempt to replicate these findings with a larger and more representative sample, however, found no evidence that the self-prophecy effect generates increased turnout (Smith, Gerber and Orlich 2003).

Given these contradictory findings, we hypothesize that whether or not Sherman's theory can be applied effectively to voter turnout will depend on how it is operationalized. Both of these previous experiments merely asked individuals to predict their future behavior, but made no attempt to persuade them to behave in a particular way. We posit that a campaign that clearly intended to persuade an individual to vote, and therefore cued the positive social norms associated with voting, could reasonably be expected to have a different psychological effect on targeted voters. This is because, in that scenario, the pledge to participate causes voters to imagine themselves voting and to associate voting with arguments in favor of doing so, thus reducing the likelihood that they would allow themselves to have made an error in predicting (or promising) that they would vote.<sup>2</sup>

These theories and experiments all are possible ways of understanding why the pledges to vote used by so many mobilization campaigns might work to influence voting behavior. Extending the argument of these theories suggests that following up on those initial pledges to participate – with, for example, a second round of phone calls – should have an even larger influence on turnout. In other words, reminding voters of their commitment may alter the behavior of even more individuals than a campaign that simply collects the initial commitment. Therefore, we hypothesize that a reminder call to committed voters will increase the need for the voter to follow through on the initial commitment to vote, and increased turnout will result as individuals act to avoid cognitive dissonance, or to behave in a way that is consistent with social norms or with their previous prediction that they will turn out to vote.

That for some voters a single telephone call is insufficient, and that two telephone calls are potentially decisive, is consistent with Arceneaux and Nickerson's (2006) view of how mobilization efforts affect individual voters' calculations about whether or not to vote. In their meta-analysis of door-to-door canvassing experiments, Arceneaux and Nickerson find that voter mobilization campaigns have the strongest influence on registered voters with an intermediate voting propensity: neither die-hard nor occasional voters, but rather those on the cusp of voting. Die-hard non-voters are logically less likely to say that they plan to vote. Those on the cusp of voting are more likely to be those that say they plan to vote, and therefore are more likely to be motivated to vote by the social norms cued by second phone call.

This brings us to the hypothesis we test in the four field experiments described in this paper:

*H<sub>1</sub>: Including a second phone call in a voter mobilization campaign will increase turnout significantly, particularly among individuals previously self-identified as likely voters.*

In order to test our hypothesis, we use the results from four randomized field experiments, all of which targeted groups (youth, Latinos, and Asian Americans) not generally considered to be likely voters (see Table 1). The first targeted young voters in New Jersey in 2003, the second Latino voters in Los Angeles in 2006. Two more experiments were conducted in 2008 among Asian and Pacific Islander Americans, one in Orange County and one in Los Angeles County. All test the utility of targeted follow-up calls. Additionally, the 2008 Los Angeles County experiment takes this line of inquiry to the next level, randomly assigning some "yes" voters to receive a second call while leaving other self-identified likely voters in the control group in order to allow for precise estimation of the effect of each round of calls.

[Table 1 about here]

All of the field experiments analyzed here utilize randomly assigned treatment and control groups, allowing for robust statistical evaluation of their impact. After each election, validated voter turnout information was obtained from the relevant county registrars. Turnout rates for the treatment and control groups were then compared, using two-stage least squares (2SLS) regression with “contact” as an explanatory variable and “assignment to the treatment group” as an instrumental variable. Because exposure to the mobilization effort is unrelated to all observed and unobserved causes of voter turnout, this generates unbiased estimates of the effects of each campaign. This estimator is the multivariate generalization of the instrumental variables estimator proposed by Angrist, Imbens, and Rubin (1996), and is employed in most published research on field experiments on voter mobilization (see Green and Gerber 2008).

### New Jersey Youth, 2003

New Jersey is among a handful of states that conducts its state-level elections during odd-numbered years. These elections generally attract fairly low levels of voter turnout, particularly when there is no gubernatorial contest at the top of the ticket, as was the case in 2003. The election was still significant, in that it represented a fight for control of the state’s closely divided legislature, but most voters were generally disinterested and others were turned off by the increasingly negative tone of the campaign; as a result, overall turnout was less than 30 percent (Kocieniewski 2003, Kocieniewski and Mansnerus 2003). In an effort to increase youth participation in the November 2003 election, the Public Interest Research Group (PIRG) targeted approximately 60 precincts across the state with large concentrations of young voters, defined as registered voters aged 18-24, including precincts near college campuses and non-campus precincts. The campaign was conducted using a decentralized network of organizers and

recruiters. Four organizers recruited volunteer precinct captains for each target precinct, and these volunteers were in turn responsible for contacting young voters residing in their precincts.

During a pre-election phase, volunteer canvassers working by telephone and door-to-door contacted as many young people on their lists as possible, inquiring whether or not those individuals intended to vote. This resulted in recorded outcomes for a final pool of 2,817 young registered voters. Most of the outreach during this phase was by phone; contact with about 11 percent of the pool (N=313) was made via door-to-door visits. Overall, messages were left for 54.8 percent of those in the pool (N=1,543). Canvassers were able to speak to 45.2 percent of voters in the pool (N=1274). Of those with whom contact was made, 68 percent (N=871) said that they planned to vote, 18 percent (N=227) said that they did not plan to vote or refused to indicate whether or not they planned to vote, and 14 percent (N=176) said that they were not yet sure whether or not they would participate. Sixty-eight percent is clearly a much higher rate of turnout than would be expected for any population, particularly a low-propensity group in a low-salience election, as is the case here; however, this large number is not surprising given that most people know that expressing an intention to vote is the socially-desirable response.

In preparation for the get-out-the-vote (GOTV) phase of the experiment, the entire pool of voters was randomly divided into treatment and control groups, with 1,418 assigned to the treatment group and 1,399 to the control group.<sup>3</sup> Assignment to treatment and control for this phase of the mobilization campaign was made without regard to the outcome of the initial phase of contact attempts. In other words, while information about vote intention was collected during the first phase of the experiment, results from that phase were not used to determine whether or not someone was targeted for a follow-up phone call, or to alter in any way what was done to try to get individuals in the treatment group to turn out.

The GOTV phase involved Election Day telephone calls to individuals assigned to the treatment group. Precinct captains and volunteers made calls throughout the day to targeted voters, reminding them of the election, noting the general tendency of 18-24 year olds not to participate, and asking for an oral pledge to turn out. Voters who indicated that they would vote were also informed of their polling place location.<sup>4</sup> The script did not include any reminder about the previous contact, if any, that canvassers had had with targeted voters. Direct conversations with targeted voters occurred in 37.7 percent of the calls (N=534); messages were left with another 39.2 percent (N=556) of targeted individuals.

[Tables 2-3 about here]

Turnout in the control group was 13.2 percent, compared to 17.0 percent for the treatment group (Table 2). This 3.8 percentage-point intent-to-treat (ITT) effect is significant at the .01 level (Fisher's exact test, one tailed) and falls within the range of effects found in previous phonebank experiments. Looking at intent-to-treat effects for individuals based upon the disposition of the pre-election contact, turnout was increased significantly among those who self-identified as likely voters (ITT = 10.6 percentage points). There is no statistically significant ITT observed for those who said that they did not plan to vote or were unsure about voting, or among those for whom no direct contact was made and a message was left during the first round of contacts. Another approach to analyzing these results is to treat the two rounds of contact as part of an overall mobilization package, with the first call asking for self-predictions and the second call asking for pledges. In other words, rather than a pure control group, this experiment compares the effect of a single call to the effect of the pair of calls.

A simple 2SLS model without covariates generates a treatment-on-treated effect of 10.2 percentage points and a standard error of 3.6, which is statistically significant at the  $p < .01$  level

(see Table 3). Adding controls for mode of pre-election contact (by phone or face-to-face), disposition of pre-election contact, and voter history (2000, 2001, 2002) generates a treatment-on-treated estimate of 7.6 percentage points (s.e. = 3.5, sig. at  $p < .01$ ).<sup>5</sup> These estimates are stronger than the usual high end of the range of effects found for well-conducted phone campaigns, indicating that this method proved quite effective for young voters in a low-salience election.<sup>6</sup>

A more nuanced understanding of the power of the second round of phone calls emerges when taking into consideration the mode (by phone or face-to-face) and outcome of the pre-election phase of canvassing (whether or not the individual self-identified as a likely voter). While the mode of the first contact is not a statistically significant predictor of turnout, there are very different effects among subgroups of the pool based on the outcome of the first phase of the experiment. As shown in Table 2, among those contacted in the initial phase of the experiment only through messages, the Election Day contact is ineffective, as are contacts to those who in the initial stage either refused to state or indicated that they did not plan to vote in the election. By contrast, those who in the initial stage of the experiment stated an intention to vote were significantly influenced by the Election Day GOTV call, with turnout increasing from 16.9 percent to 27.5 percent, an intent-to-treat effect of 10.6 percentage points. 2SLS analysis (see Table 3) shows that individuals who indicated that they were likely to vote are 7.8 percentage points (SE = 2.3, sig. at  $p < .01$ ) more likely to have voted than are those for whom no direct contact was made (those for whom messages were left). Dummy variables for “no” and “undecided” voters indicate that the GOTV contact actually decreased their likelihood to vote, compared to those for whom messages were left; combining these coefficients with the “contact” coefficient estimate indicates that the GOTV calls failed to move these voters to the polls. The

sample sizes for these categories of voters are quite small, and thus the results must be interpreted with caution. Yet, if the psychological explanations we advance here are correct, follow-up calls to those who have said they do not plan to vote may have the perverse effect of encouraging them to hold firm to that earlier commitment and stay home on Election Day.

The New Jersey experiment thus offers an important insight into the potential effectiveness of follow-up telephone calls when they are made to voters that have already committed to vote. While previous work had indicated that multiple calls were not a good use of resources, the PIRG experiment suggests that targeted follow-up calls can in fact substantially increase the effectiveness of a phonebank campaign. However, rather than trying to make a second contact with all voters in the target pool, the results indicate that such follow-ups should be restricted to those who previously identified themselves as likely to participate.

#### Los Angeles County Latinos, 2006

Results from the New Jersey experiment, although intriguing, were based on post-hoc analysis of subgroups and a relatively small sample. In order to better test the power of targeted follow-up telephone calls, we conducted a second experiment in Los Angeles in the weeks prior to the 2006 general election. In contrast to the New Jersey experiment which contacted all voters regardless of their previously stated likelihood of voting, in the second experiment callers re-contacted only those voters who reported an intention to vote.

For the November 2006 general election, the Southwest Voter Registration Education Project (SVREP) targeted low-propensity Latino voters in Los Angeles for a two-stage get-out-the-vote campaign. SVREP's target pool included registered voters with Latino surnames who had registered after August 1, 2004 or who had not voted in any primary or general election since 1998. The effort focused on five city council districts in Los Angeles where 48 percent or more

of the registered voters were Latino. The congressional races in these areas were not competitive; all seven Democratic House incumbents whose districts overlap the included city council districts were reelected with comfortable margins, two without major party opposition. Turnout was relatively low despite the high-profile contest on the ballot between incumbent Republican Governor Arnold Schwarzenegger and Democratic challenger Phil Angelides. Overall, turnout was 39 percent, and in Los Angeles County fewer than 36 percent of eligible voters went to the polls.<sup>7</sup>

SVREP conducted a variety of activities in the weeks and months prior to the November 2006 election aimed at increasing Latino voter turnout in Los Angeles, including two mailings, live phonebanking, and a small door-to-door canvassing effort. This analysis focuses only on those precincts where no door-to-door canvassing occurred. This allows us to isolate the effects of the phonebanking and mailing efforts.<sup>8</sup> Given that previous experiments have found mailings to have a negligible effect on turnout, we include a description of the mailing campaign but assume that most of the observed effect, if not all, is due to the phone calls (Green and Gerber 2008, García Bedolla and Michelson, forthcoming). The results are best understood as the result of the combination of tactics used to encourage turnout, particularly as the phone scripts made reference to the mailings and therefore may have created a synergistic effect. The first mailing, sent at the beginning of the campaign (in early October), included an absentee voter application and a message about the importance of the November election. The second mailing, sent approximately three weeks before Election Day, included an inspirational message from SVREP President Antonio González about the salience of the issues on the ballot for Latino voters and urged the voter to follow through on the immigration reform marches of early 2006 by turning out to vote.<sup>9</sup>

Phone canvassing was conducted in the three weeks prior to the election, with up to six attempts made to contact each individual on the treatment list. Individuals working the phonebanks were both volunteers and paid workers, with the majority being paid workers. They were all fluent in Spanish and many of the phone contacts were made in Spanish. Phonebank callers were recruited using a network of contacts maintained by SVREP that consisted of local activists and organizations. Callers were primarily Latinos and the majority of callers were female. Approximately half of phonebank callers had experience working with SVREP in previous campaigns. The calling occurred between 4 and 8 pm on weekdays and throughout the day on weekends. Callers asked contacted voters whether they intended to vote; those who responded affirmatively were contacted a second time the day of or the day before the election and reminded to vote.<sup>10</sup> As often as possible, voters were contacted both times by the same canvasser, and the script was altered informally to remind voters that they had spoken with them earlier and promised to turn out. The phonebank script changed slightly several times over the course of the campaign, including at various times encouragements to complete the previously-mailed absentee voter application, to mail in completed absentee ballots, to volunteer to work at their local polling place, and to vote at the polls on Election Day. All scripts urged “yes” voters to volunteer with SVREP to help mobilize their neighbors; canvasser trainings emphasized the importance of asking every “yes” voter to help. In addition, canvassers were asked to use a conversational tone rather than to appear to be reading the script.<sup>11</sup>

The experimental design consisted of two rounds of random assignment. First, 478 precincts were randomly assigned to treatment and control groups. The 432 treatment precincts included 29,316 individuals, while the 46 control precincts included 3,071 individuals. The treatment precincts were then further divided by a second round of randomization into treatment

and control individuals, clustered by household, with 25,000 voters assigned to the treatment group and the remaining 4,316 individuals assigned to the control group.<sup>12</sup> Only individuals in the treatment group as determined by this second round of randomization were targeted to receive phone calls from SVREP. As noted above, we exclude from our analysis here the small set of voters (about 4 percent of the sample) who were also targeted for door-to-door canvassing by SVREP. The final pool of 25,862 individuals included a treatment group of 19,512 and a control group of 6,350. Within the treatment group, 23.5 percent (N=4,579) of targeted individuals were successfully contacted.

[Tables 4-5 about here]

The treatment group voted at a rate of 36.6 percent, compared to 34.3 percent in the control group (Table 4). This 2.3 percentage-point intent-to-treat effect is statistically significant ( $p < .01$ , Fisher's exact test). 2SLS generates a treatment-on-treated effect of 9.3 percentage points, with a robust cluster standard error of just 3.2 percentage points.<sup>13</sup> Controlling for voter history increases the coefficient estimate for the phone calls to 10.3 percentage points (SE = 3.0), as shown in Table 5.<sup>14</sup>

This 10.3 percent treatment effect was the strongest effect for live phone calls that had been observed in a large-N study, leading us to design two additional experiments in order to determine whether such large effects could be achieved in other circumstances using the same tactics.<sup>15</sup> These experiments were conducted in 2008, again using the strategy of follow-up calls to self-identified likely voters. One of these duplicated the tactics used by SVREP, targeting all "yes" voters for a reminder call; the second included a second round of randomization, targeting only a random sample of "yes" voters for a reminder call and thus allowing us to generate separate estimates for the effect of each round of contacts.

In order to implement follow-up phonebank experiments during the June 2008 California primary election, we worked with two community-based organizations targeting Asian and Pacific Islander American voters. One group mobilized voters in Orange County, the other in Los Angeles County. Turnout in this election was historically low: about 28 percent statewide, but only 20 percent in Los Angeles County and 21.5 percent in Orange County, the lowest turnout ever recorded for a regularly scheduled election in California.<sup>16</sup> The lack of interest in the election was likely due to the split primary; the more salient presidential primary election had been held earlier, in February. Most of the primary election contests on the June ballot were not particularly competitive; statewide, not a single member of Congress and only one member of the California state legislature lost in the June 2008 primary election.

Both June 2008 mobilization efforts began with selection from the relevant county voter file of individuals identified as (1) Asian, (2) low-propensity (those who voted in 1, 2 or 3 of the last 5 major elections, were younger than 25 years of age, or had newly registered), and (3) residing in geographic areas with large numbers of Asian voters. The files were then sent to MDM DATA Solutions, which used commercially-available data to identify those with valid mailing addresses and phone numbers. This resulted in pools of 33,405 voters in Orange County and 28,801 voters in Los Angeles County.

#### Orange County Asian Americans, 2008

The Orange County effort for the June 2008 election was conducted by the Orange County Asian and Pacific Islander Community Alliance (OCAPICA). Although OCAPICA has long been active in political issues of concern to the Asian Pacific Islander community, the organization directly mobilized voters for the first time in fall 2006. Their phonebank for the November 2006 election used a single-call strategy, resulting in an effect of 2.9 percentage

points among those contacted, or 4.2 percentage points when taking voter history into consideration (García Bedolla and Michelson, forthcoming). For the June 2008 election, OCAPICA adopted the two-call strategy used by SVREP, targeting voters who self-identified as likely voters during the first call to receive a follow-up call closer to Election Day.

The pool of 33,405 low-propensity Asian and Pacific Islander American voters was randomized into a treatment group of 6,285 individuals, all of whom also received mailed *Easy Voter Guides*.<sup>17</sup> As noted above, given robust findings that mailers do not increase turnout, we assume that increases in turnout as a result of the campaign are due mostly, if not completely, to the phonebank campaign (García Bedolla and Michelson, forthcoming). In addition, mailers were sent to 3,000 individuals not targeted to receive phone calls; these individuals were dropped from the analysis in order to avoid contamination of the control group. Later analysis resulted in an additional 363 individuals being dropped from the file due to out-of-state addresses or for whom a telephone number was not available. This resulted in a final total pool of 33,042 individuals. Members of five national-origin groups, including Asian Indians, Chinese, Filipino/as, Koreans, and Vietnamese were randomized (separately for each national-origin group) into treatment and control groups; members of smaller Pacific Islander groups (e.g. Guamanians, Samoans) were not randomized and are not included in the analysis below.

A first round of calls, including up to three attempts to contact each targeted individual, resulted in 1,633 contacts, a contact rate of 26.3 percent. Individuals who were reached in the first round and who told canvassers that they were planning to vote (“yes” voters) were targeted for reminder calls just before Election Day. Of those contacted in the first round, 70 percent (N = 1,149) said that they planned to vote, and an additional four percent said that they had already voted. Only seven percent of individuals contacted said that they did not plan to vote. Dropping

20 individuals from the “yes” group who were determined to either not be citizens or not Asian/Pacific Islander, a second round of calls was made to the remaining 1,129 self-identified likely voters. Overall, 479 secondary contacts were made with “yes” voters, for a contact rate of 42.4 percent (see Table 6).

[Tables 6-7 about here]

Comparison of the voting rates for treatment and control is done separately for each national-origin group, as shown in Table 6. Across all the national-origin groups, turnout was higher in the treatment group than in the control group, ranging from an Intent-to-treat (ITT) effect low of 1.9 percentage points for Asian Indians to a high of 4.5 percentage points for Korean Americans. Using 2SLS analysis to compute treatment-on-treated effects, pooling across ethnic groups and using robust cluster standard errors to cluster by household, the effect of treatment was a statistically significant and substantively large 11.1 percentage points ( $SE = 2.1$ ). Adding controls for voter history reduces the treatment effect slightly, to 10.3 percentage points (Table 7). That this estimate is identical to that obtained in the earlier SVREP experiment increases our confidence in those earlier results, despite their being so much larger than those observed in previous large-N phonebank studies.

This pooled result also is much stronger than that previously achieved by OCAPICA using a one-call strategy. That their effectiveness jumped from a treatment effect of 4.2 percentage points in November 2006 to 10.3 percentage points in June 2008 strongly suggests that follow-up calls to self-identified likely voters significantly increases the power of a phonebank campaign. Another possibility is that OCAPICA’s phonebank was qualitatively stronger this election as compared to 2006, the result perhaps of increased experience, although, again, given the earlier SVREP results we believe this alternative explanation to be unlikely.

However, both the SVREP and OCAPICA treatment effect estimates are for the overall campaigns, best understood as “the effect of one or more contacts.” Estimating the power of the follow-up calls, specifically, requires a second round of randomization, with only some self-identified likely voters targeted to receive a reminder call. This is precisely the strategy we adopted in the fourth experiment.

#### Los Angeles County Asian Americans, 2008

The Los Angeles County effort in June 2008 was conducted by the Asian Pacific American Legal Center (APALC), which has conducted several get-out-the-vote experiments targeting Asian and Pacific Islander Americans in the county. Like OCAPICA, APALC’s efforts were quite successful in those previous studies. For example, in November 2006 their mobilization campaign had a treatment effect of 3.7 percentage points. The June 2008 experiments included six national-origin groups: Asian Indians, Chinese, Filipinos, Japanese, Koreans, and Vietnamese. Of the pool of 28,801 individuals, 12,168 were randomly assigned to receive a phone call and a mailed English-language *Easy Voter Guide*. Chinese, Korean and Vietnamese American voters also received *Easy Voter Guides* in their native languages.

As shown in Table 8, individuals targeted to receive a single phone call were more likely to vote than were individuals assigned to the control group. Table 8 also reports contact rates for each national-origin group, which ranged from 17.6 percent to 36.2 percent, and each group’s respective intent-to-treat effects.

[Tables 8-10 about here]

When contacted during the first round, individuals were asked whether or not they intended to vote. “Yes” voters were randomly divided into treatment and control groups to be targeted for a second call. Of the 3,758 individuals contacted in the first round, 74 percent (N = 2,777)

indicated that they planned to vote, and an additional 2.6 percent said that they had already voted. Only 5.6 percent of individuals said that they did not plan to vote. For the second round of calls, APALC conducted an experiment focused on the 1,901 self-identified likely voters who had indicated that they planned to vote at the polls, and not by absentee ballot. The second randomization assigned 1,501 “yes” voters to receive a follow-up call, while 400 “yes” voters were assigned to the control group. Of those assigned to receive a reminder call, 44.3 percent were successfully contacted. As shown in Table 10, the follow-up calls had a powerful intent-to-treat effect, of 5.5 percentage points. 2SLS analysis generates an estimated treatment-on-treated effect of 13.0 percentage points, or 13.2 percentage points when controlling for voter history (Table 10).

In order to more accurately parse out which new votes were generated by the first call and which by the second call, we make the following calculations: first, calculating the net vote gain for the entirety of the calling campaign suggests an overall marginal vote gain of 235 votes, with 152 created during the first round and 83 created during the second round.<sup>18</sup> Second, we divide 152 by the number of total contacts in the first round (3,758), generating a first-round effect on those contacted of 4.0 percentage points. This figure is in line with APALC’s one-call treatment-on-treated effects from previous experiments and well within the expected range for a well-conducted phonebank campaign. The second-round treatment effect of 13.2 percentage points, by comparison, is extremely large and explains why the other two-round calling campaigns described here (by SVREP and OCAPICA) were so successful.

## Discussion

As mentioned above, Green and Gerber (2004: 78) argue that “Contacting voters more than once prior to an election is a waste of resources. Multiple contacts have no apparent

benefits.” However, the conclusion is made equivocally, and is noted as being supported by only a few experiments. In contrast, we find that phonebank canvassing which includes a second round of calls, particularly when those follow-up calls focus on self-identified likely voters, results in increases in turnout far beyond the 3-5 percentage-points that would be expected from an otherwise well-conducted phonebanking campaign.

The 2003 PIRG experiment attempted to contact all targeted voters a second time, regardless of their initial self-prediction. Those individuals who had initially indicated that they were going to vote were very receptive to the GOTV message, and the campaign had a very large and statistically significant influence on their propensity to vote. Those less willing to commit to voting, or who indicated that they did not intend to vote, were not moved by the Election Day follow-up calls. This pattern of results indicates that making a blanket round of secondary calls could indeed be a waste of resources. As noted by Arceneaux and Nickerson (2006), some individuals may be simply die-hard non-voters. In other words, while making multiple calls to all registered voters may not significantly improve turnout, focused follow-up calls to self-identified likely voters may be an efficient and cost-effective method of turning out more voters with a phonebank campaign.

This finding is further supported by the much larger SVREP and OCAPICA experiments, which targeted only those voters who had initially indicated an intention to vote with a follow-up telephone call on the day of or day prior to the election. SVREP was able to increase turnout by 10.3 percentage points, much larger than the 4.6 percentage-point effect found for the other published study of a phonebank campaign targeting Latinos, the single-call NALEO effort in 2002 (Ramírez 2005). OCAPICA also increased turnout by 10.3 percentage points, a much larger effect than the 4.2 percentage-point impact of their effort during the November 2006

election and by far the largest effect found for a large-N phonebank campaign targeting Asian American voters.

However, because these two campaigns employed a blanket strategy of targeting all “yes” voters for a second call, the results could not be used to calculate the actual power of each round of calls, and only produced estimates of the power of a phonebank campaign that results in “one or more” contacts. In order to more precisely estimate the impact of each round of calls, it was necessary to conduct two rounds of randomization. It is here that the June 2008 APALC campaign provides the crucial next piece of evidence. APALC’s first round of calls produced a treatment effect of 4.0 percentage points, and the second round of calls increased turnout an additional 13.2 percentage points. This shows that the second round of calls is what is responsible for the large treatment effects observed in these experiments. While self-identified likely voters are more likely to vote than others who receive a single telephone call, our findings show that they are even more likely to vote when contacted again.

This raises the theoretical question as to why this works. The scripts used by the community organizations that conducted these campaigns are instructive in helping us to ascertain which psychological mechanism is likely at work in the minds of these voters. The PIRG, OCAPICA and APALC second-call scripts did not refer to the earlier round of calls; the SVREP calls did so, but only informally and with unknown levels of consistency. OCAPICA and APALC leaders observed that some canvassers occasionally went off-script to remind voters of the earlier contact; this was done informally and infrequently by OCAPICA and more frequently during APALC’s follow-up round of calls. This leads us to believe that it is not the theory of the self-erasing nature of errors of prediction that is at work, as voters are not consistently being asked in the second call to promise to vote. It also does not appear to be the

result of voters aiming to avoid cognitive dissonance, since three of the four groups did not mention the earlier commitment in most of their follow-up calls. It is possible that those contacted nevertheless did recall the earlier call and pledge to vote; therefore, these theories should not be dismissed completely until further research is conducted. But, we would argue that the strongest support is found for the theory of reasoned action, as the second call scripts included a reminder to vote and mentioned that the election will make a difference “for our community.” In other words, what is key is that the scripts cued the social norm of voting, which makes a difference among those voters for whom conforming to that norm matters.

The mechanism at work here may be a relatively simple one: self-selection. In the PIRG, OCAPIA, and APALC experiments, around 70 percent of those contacted in the first round indicated an intention to vote. Clearly, this is a much higher percentage than we could logically expect to turn out, given these are low-propensity voters and low-salience elections. Yet, what this first call may be doing is culling out those voters who are most open to a mobilization message, and therefore most responsive to the positive social norm attached to voting. It is these norms that are being cued by the follow-up call, regardless of whether or not the follow-up references the initial call or the voter remembers the initial call. Of course, one potential drawback to this strategy is that habitual non-voters, those not self-identifying as likely voters during an initial contact, will be further marginalized within the political system because campaigns will focus their subsequent efforts only on those voters who indicate that they plan to vote. But, we must keep in mind that even those not responding “yes I will vote” to the initial call are receiving at least one encouragement to vote. Findings from previous phonebank experiments, as well as the APALC experiment detailed here, indicate that initial call would likely increase their likelihood of voting by 3-5 percentage points. These habitual non-voters are

perhaps not as responsive to mobilization efforts as “yes” voters, but they are still responding. Thus, the focus on self-identified likely voters recommended by these results allows campaigns with limited resources to maximize their effectiveness while still reaching out to all voters.

Whether or not the follow-up to commitments to vote must be made through a live phone call and not another method remains an open question. For example, it is possible that the same psychological mechanism triggered by the follow-up calls could be brought into play with the use of mailers or pre-recorded telephone calls (robocalls). Many organizations currently use vote pledge postcards that are signed by contacted individuals and mailed back just before the election, often with polling place information appended. However, no field experiments have yet examined the power of the use of these reminder postcards. Additional research is necessary to determine how important the live component is to the efficacy of these calls, and whether less personal and more cost effective methods also can be a valuable tool for increasing turnout.

### Conclusions

Political campaigns often fail to target low-propensity voters based on the assumption that it would be an inefficient use of resources. The argument is that it is much cheaper, and more effective, to focus on individuals who vote regularly. Previous experimental research has found that using phonebanking to turn out those voters results in a 3 to 5 percent increase in their voting propensity. The findings from these studies, which focused on various low-propensity groups, show that this cost/benefit analysis on the part of campaigns may be based on flawed assumptions. Not only were these campaigns able to use phonebanking to turn out these voters very effectively, but our findings demonstrate that a round of reminder telephone calls to previously committed voters resulted in a significant increase in turnout – an increase much greater than had been found in previous phonebanking experiments. The experiments detailed

here provide evidence that such tactics can provide a significant boost to the overall impact of a GOTV phone campaign, making them potentially as effective as door-to-door canvassing.

Although the psychological theories discussed here ought to apply to all voters, we should not automatically make that assumption. That these studies are limited to a small study of youth and large campaigns targeting Latinos and Asian Americans limits to some extent the confidence with which we can assert that follow-up calls will have similar effects among other groups of voters. Our findings are also limited in that they all occurred in relatively low-salience elections. We therefore look forward to experiments that test the power of targeted follow-up calls in high turnout elections and with different target populations. On the other hand, that these findings are consistent across a variety of populations and geographic areas, and with large-N studies, makes us confident that they will hold in other circumstances. This is important from a resource-allocation standpoint because phonebanks are significantly less expensive to organize and run than are door-to-door canvassing campaigns. In addition, these findings demonstrate that phonebanks may be used to effectively target groups of voters that are not geographically concentrated, such as Asian Americans, and who are therefore difficult to reach using a door-to-door campaign.

An increasing body of research demonstrates that even a brief get-out-the-vote contact, when made via door-to-door visit or with a live telephone call, can have a significant effect on voter turnout. Yet, scholars have yet to explain why this should be the case. The psychological theories explored in these experiments provide some guidance on this point. When an individual is asked to vote, this cues awareness of the social norm of voting, and may trigger a desire to comply with that norm among intermediate-propensity voters. The campaign by SVREP extracted a commitment to vote from contacted individuals, and then referred to this earlier

commitment when making a second round of calls. The OCAPICA and APALC campaigns asked for self-predictions of voting behavior and then followed up on those predictions with a reminder call. Those reminder calls did not consistently refer to the earlier conversation; nevertheless, individuals were clearly influenced, and quite strongly. Given the strong effects found by OCAPICA and APALC, we lean towards the theory of reasoned action as an explanation for why these campaigns work. However, theories of cognitive dissonance and the self-erasing nature of errors of prediction are also consistent with these results, assuming that individuals contacted a second time remembered the earlier call. Different psychological pressures may be at work in different campaigns, depending upon which approach was employed. Further research is necessary to clarify this point. More importantly, this work enhances our understanding of why brief, albeit personal, GOTV appeals work at all.

It also is important that our analysis tests these psychological theories regarding the power of social norms in the “real world.” Much social psychology research is conducted in college laboratories, with college students as subjects, and with the very obvious limitations that the population is not representative of society at large and participants know that their behavior is being monitored. In the real world, there is very little expectation that stated commitments to vote will be subjected to post-election review, and that strangers will keep track of one’s future behavior. Nevertheless, our findings show these commitments can have a very real effect on individual voting behavior.<sup>19</sup>

Political campaigns often ignore the types of low-propensity voters targeted by these mobilization campaigns – youth and people of color. Our findings show not only that personal tactics such as phonebanking are highly effective with these voters, but also that it is possible, through targeted follow-up calls, for a phonebank to be as effective in mobilizing voters as a

door-to-door canvassing effort. For organizations targeting voters that are not geographically concentrated, or who do not have the volunteer infrastructure and/or resources necessary to conduct a door-to-door campaign, this strategy has the potential to be highly effective. Our findings also speak to the high value Americans of all backgrounds place on the social norm of voting. An overwhelming number of the respondents, through their commitment to vote, expressed their belief that they should vote. Clearly, even members of marginal communities who are low-propensity voters believe voting is important, and are highly responsive if they are asked to participate. Ideally political campaigns and community organizations will use this strategy in order reach out in a more systematic and effective way to these groups of voters.

**Appendix 1: PIRG GOTV (Second Call) Script:**

Hi, can I speak to [voter's full name]?

Hello, this is [caller's name] and I'm a volunteer with the New Voters Project. We're a local campaign working to get 18-24 year olds voting this Election Day. I'm just calling to see if you've voted yet today.

[Wait for answer, if yes]: That's great to hear! Remind your friends and thanks for helping to get out the youth vote!

[If no]: OK, well we're just calling because right now, less than half of all eligible 18-24 year olds are voting. We're trying to turn that trend around, getting young people voting and making the youth vote the most powerful in America. The polls are only open for [time left] more hours!!!

Can we count on you to vote today?

[If no]: OK, well we'll still hope to see you down at the polls, and remind your friends to vote!

[If yes]: Great, your polling place is \_\_\_\_\_.

Have a good day!

## **Appendix 2: OCAPICA and APALC Second Call Script**

Good evening, may I please speak with \_\_\_\_\_? My name is \_\_\_\_\_. I am calling on behalf of the Asian American Voter Project.

I'm calling to remind you to vote on Tuesday, June 3rd. Your vote in this election will make a difference for our community. Do you know where your polling place is located?

[provide polling place if voter answers no]

The polls will be open from 7:00 am to 8:00 pm on Tuesday, June 3rd. If you need translated materials, you can get them at your polling place.

Thank you and have a good evening/afternoon.

NOTE: Additional scripts are available in the Web appendix to this article.

## Endnotes

<sup>1</sup> Results from the first experiment were not statistically significant.

<sup>2</sup> While Festinger's theory of cognitive dissonance and Sherman's theory of the self-erasing nature of errors of prediction are similar in that they both predict that individuals will act to be consistent with prior commitments, the underlying mechanisms are quite different. In the case of cognitive dissonance, the key is avoidance of negative feelings (dissonance); in the case of Sherman's theory, the key to self-fulfilling prophecies is that individuals imagine themselves performing the promised action and come up with positive reasons for doing so. In other words, Festinger's theory is about the avoidance of negative feelings, while Sherman's is about the creation of positive feelings.

<sup>3</sup> A randomization check found no significant differences between individuals assigned to the two groups ( $df = 14, p = .81$ ).

<sup>4</sup> During the second round of calls, individuals who indicated that they planned to vote were given polling place information, while those who said that they did not plan to vote were simply thanked for their time. Previous research has found that providing polling place information can increase turnout, but this finding is limited to mail campaigns. Experiments for various messages using live phonebanks or door-to-door visits have failed to find any message effects (Green and Gerber 2008). Comparing participation across states with different election procedures, Wolfinger, Highton and Mullin (2005) find that mailed polling place information increases turnout by 2.5 percentage points, with even stronger effects among younger citizens. Thus while it is possible that the polling place information provided to these voters during the second round of calls increased turnout, it is unlikely to account for much, if any, of the observed power of the overall campaign.

<sup>5</sup> Defining contact more broadly, to include those for whom a message was left (and thus doubling the contact rate), generates a TOT effect of 5.0 (s.e. = 1.7) without covariates and 3.8 (1.7) with covariates.

<sup>6</sup> Although the usual range of effects for a well-conducted phonebank is 3-5 percentage points, recent experiments looking at the effectiveness of such campaigns among youth of color (Asian Americans and Latinos) find stronger effects, of 13.4 percentage points (s.e.=5.0) for Asians in Los Angeles and 4.4 percentage points (s.e.=3.0) among Latino youth in Central and Southern California in, both in November 2006 (Michelson, García Bedolla and Green 2007).

<sup>7</sup> [http://www.sos.ca.gov/elections/sov/2006\\_general/reg.pdf](http://www.sos.ca.gov/elections/sov/2006_general/reg.pdf).

<sup>8</sup> The results for the canvassing effort are available from the authors upon request.

<sup>9</sup> The slogan, “today we march, tomorrow we vote” was a notable component of the spring 2006 marches.

<sup>10</sup> Unfortunately we are unable to determine from SVREP’s records what proportion of individuals in the original treatment group self-identified as likely voters.

<sup>11</sup> While we provide several SVREP scripts in the Web appendix to illustrate the nature of those conversations, we are unable to provide a copy of the reminder script as this language was added informally by individual phonebank volunteers relying on their own paper notes from earlier rounds of calls.

<sup>12</sup> This split control group design was implemented in order to test for spillover effects (see McConnell, Sinclair and Green 2008).

<sup>13</sup> Traditional standard error estimates using independent observations is not appropriate for datasets with clustered structure because observations in the same clusters tend to have similar characteristics and are more likely correlated each other. In this case we cluster at the level of

assignment to treatment (household or precinct). Robust standard error estimates are thus used to take into account the intra-cluster correlation.

<sup>14</sup> We should note that voter history does not have a significant effect on SVREP's effects, likely because of the nature of their voter pool. Overall, 82.5 percent of individuals in the SVREP pool had never voted before. The pool included new registrants as of August 2004 as well as individuals who had never voted. This means that some voters could potentially have voted in November 2004, November 2005, or June 2006; 16.5 percent had participated in one of these elections.

<sup>15</sup> This figure may be inflated slightly by the effects of mailings, but judging from the weak results from other mailing experiments this bias is likely to be quite small.

<sup>16</sup> [http://www.sos.ca.gov/elections/sov/2008\\_primary\\_june/sdp08SOV.pdf](http://www.sos.ca.gov/elections/sov/2008_primary_june/sdp08SOV.pdf)

<sup>17</sup> *Easy Voter Guides* are summaries of election information that are provided free to organizations and voters in California by a consortium of non-profit organizations. They are meant to provide voters with simple and accessible non-partisan voting information they can use to make better informed electoral choices.

<sup>18</sup> To obtain these estimates, we first calculated the number of votes produced in the second round, using the turnout rate for the control group (14.0 percent) to determine how many individuals in the pool would have voted in the absence of a second round of calls:  $.140 \times 1501 = 210$ . We subtract this from the number of individuals in the pool who did actually vote to obtain the vote gain:  $293 - 210 = 83$ . Similar calculations for each national-origin group in the first round generate an estimate of 235 overall votes gained:  $[(117 - (.084 \times 1200)) + (469 - (.098 \times 4000)) + (227 - (.105 \times 1900)) + (125 - (.121 \times 800)) + (498 - (.171 \times 2500)) + (153 - (.085 \times$

1600))] = 235. We subtract from this estimate the votes generated by the second call:  $235 - 83 = 152$  votes generated during the first round.

<sup>19</sup> Note that in a recent field experiment that did suggest individuals would be watched, and their behavior reported to their neighbors, mailed requests to vote had a significant effect (Gerber, Green and Larimer 2008).

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Table 1. Overview of Experiments

	<b>Location/Population Targeted</b>	<b>Outreach Strategy</b>
PIRG 2003	New Jersey Youth	Follow-up calls to entire treatment group
SVREP 2006	Los Angeles Latinos	Follow-up calls to self-identified likely voters (“yes” voters)
OCAPICA 2008	Orange County Asian-Americans	Follow-up calls to “yes” voters
APALC 2008	Los Angeles Asian-Americans	Follow-up calls to random sample of “yes” voters

Table 2. Contact Rates and Intent-to-Treat Effects, PIRG 2003 Campaign

	N	% Voting (control group)	% Voting (treatment group)	Intent-to- treat effect	Contact Rate
ALL VOTERS	2,817	13.2% (184/1,399)	17.0% (241/1,418)	<b>3.8*</b>	37.7% (534/1,418)
<i>Outcome of first contact</i>					
Message left	1,543	13.0 (98/755)	14.2 (112/788)	2.2	30.7 (242/788)
Not voting or refused	227	7.3 (8/110)	3.4 (4/117)	-3.9	36.8 (43/117)
Uncertain about voting	176	4.2 (4/96)	7.5 (6/80)	3.3	50.0 (40/80)
Will vote	871	16.9 (74/438)	27.5 (119/433)	<b>10.6*</b>	48.3 (209/433)

\* p < .01, one-tailed.

Table 3. 2SLS Regression Results, PIRG 2003 campaign (clustered standard errors in parentheses)

	Model 1 (no voter history)	Model 2 (with voter history)
Contact	10.1* (3.5)	7.6* (3.5)
Initial contact face-to-face	-2.1 (2.5)	-3.4 (2.3)
“Yes” voter	7.8* (2.3)	6.6* (2.0)
“No” voter	-8.7* (2.1)	-5.8* (1.9)
“Undecided” voter	-8.2* (1.9)	-5.2* (1.6)
Voted 2000		2.6 (1.4)
Voted 2001		12.7* (2.4)
Voted 2002		22.8* (2.4)

N = 2,817. \* p < .01, one-tailed. Standard errors are clustered by the unit of assignment to treatment or control (precinct). “Initial contact face-to-face” indicates that the initial contact was made via door-to-door visit rather than by telephone. Dummies for “yes” voter, “no” voter and “undecided” voter indicate responses to the initial visit; the excluded category is those for whom a message was left.

Table 4. Contact Rate and Intent-to-Treat Effect, 2006 SVREP Campaign

N	% Voting (control group)	% Voting (treatment group)	Intent-to-treat effect	Contact Rate
25,862	34.3%	36.6%	<b>2.3*</b>	23.0%

\*  $p < .01$ , one-tailed.

Table 5. Treatment-on-Treated Effects, 2006 SVREP Campaign, 2SLS (clustered standard errors in parentheses)

	Model 1 (no voter history)	Model 2 (with voter history)
Contacted	9.3* (3.2)	10.3* (3.0)
Voted June 2006		45.3* (0.8)
Voted 2005		18.9* (3.3)
Voted 2004		-2.2 (2.4)

N = 25,862. \* p < .01, one-tailed. Standard errors are clustered by the unit of assignment to treatment or control (precinct or individual).

Table 6. Contact Rates and Intent-to-treat Effects, 2008 OCAPICA Campaign

	% voting, control group	% voting, treatment group	Intent-to-treat Effect (ITT)	Contact Rate
Asian Indians N=3,292	7.0	8.9	1.9	34.6
Chinese N=3,207	8.7	12.5	3.8*	30.6
Filipino/a N=3,656	9.1	12.5	3.4	21.6
Korean N=4,746	11.4	15.8	4.4*	33.5
Vietnamese N=14,862	22.4	24.7	2.3*	20.7

\*  $p < .01$ , one-tailed. Control group includes only individuals not targeted for mailings or phone calls.

Table 7. Treatment-on-Treated Effects, 2008 OCAPICA Campaign

Model 1 (no voter history)						
	Asian Indians N = 3,292	Chinese N = 3,207	Filipino/a N = 3,656	Korean N = 4,746	Vietnamese N = 14,862	POOLED
Contacted	5.5 (4.5)	12.6* (4.3)	16.1 (8.4)	13.2* (3.9)	10.9 (5.1)	11.1* (2.1)
Model 2 (with voter history)						
Contacted	4.4* (4.5)	12.0* (4.1)	16.1 (8.0)	12.3* (3.7)	9.1 (4.6)	10.3* (2.0)
Voted Feb 2008	8.4* (1.2)	14.0* (1.4)	12.3* (1.2)	20.9* (1.4)	3.5* (1.0)	
Voted Nov 2006	2.1* (1.2)	3.3* (1.2)	2.5* (1.1)	3.4* (1.3)	12.4* (0.8)	
Voted June 2006	10.3* (2.9)	12.5* (2.6)	12.8* (2.4)	10.7* (2.4)	6.5* (1.2)	

\*  $p < .01$ , one-tailed. Robust cluster standard errors by unit of assignment to treatment and control (household). Control group includes only individuals not targeted for mailings or phone calls.

Table 8. Contact Rate and Intent-to-Treat Effects for First Call, APALC June 2008

	% voting, control group	% voting, treatment group	Intent-to-treat Effect	Contact Rate
Asian Indian N = 1,473	8.4 (23/273)	9.8 (117/1,200)	1.4	35.5
Chinese N = 12,216	9.8 (804/8,216)	11.7 (469/4,000)	1.9*	36.2
Filipino/a N = 4,043	10.5 (224/2,143)	11.9 (227/1,900)	1.4	27.4
Japanese N = 2,153	12.1 (164/1,353)	15.6 (125/800)	3.5	17.6
Korean N = 5,336	17.1 (486/2,836)	19.9 (498/2,500)	2.8*	28.0
Vietnamese N = 3,167	8.5 (133/1,567)	9.6 (153/1,600)	1.1	32.6

\*  $p < .01$ .

Table 9. Contact Rate and Intent-to-Treat Effect for Second Call, APALC June 2008

	% voting, control group	% voting, treatment group	Intent-to-treat Effect	Contact rate
ALL	14.0 (56/400)	19.5 (293/1,501)	5.5*	44.3

N = 1,901. \* p < .01. The campaign also contacted eight individuals in the control group during the second-round phonebank.

Table 10. Treatment-on-Treated Effect for Second Call, APALC June 2008 (standard errors in parentheses)

	Model 1 (no voter history)	Model 2 (with pooled voter history)
Contacted	13.0* (4.9)	13.2* (4.7)
Voted Feb 2008		15.1* (2.0)
Voted Nov 2006		6.6* (2.1)
Voted June 2006		15.6* (4.0)

N = 1,901. \*  $p < .01$ . Robust cluster standard errors by unit of assignment to treatment group.