This article estimates the extent to which nonpartisan phone calls from commercial phone banks increase voter turnout. Prior to the 1998 and 2002 elections, randomized field experiments were conducted in which more than 1 million subjects were randomly assigned to treatment and control conditions. The results indicate that this type of phone calling campaign is ineffective.

Keywords: voter turnout; voter mobilization; field experiments

Do Phone Calls Increase Voter Turnout? An Update

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Political campaigns in the United States have grown increasingly reliant on mass marketing techniques to mobilize voters. One of the most important developments has been the advent of inexpensive telemarketing services. Large telemarketing firms have the capacity to call hundreds of thousands of voters in a single day. Coupled with increasingly detailed databases about the political and demographic profile of each voter, phone banks represent a potentially attractive means by which to conduct large scale get-out-the-vote (GOTV) campaigns.

The question is whether such efforts do in fact raise voter turnout. Nonexperimental studies have noted the positive correlation between "party contact" and voting (e.g., Kramer 1970; Rosenstone and Hansen 1993). Unfortunately, since "party contact" is a broad category, these studies do not isolate the effects of phone contact. More important, nonexperimental studies of voter mobilization may produce misleading

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results if parties and campaigns target active voters. A correlation between voter turnout and phone contact is open to two opposing interpretations: the phone call increases turnout or likely voters tend to receive disproportionate attention from campaigns.

Experimental studies of phone mobilization (Eldersveld 1956; Adams and Smith 1980; Miller, Bositis, and Baer 1981; Gerber and Green 2000, 2001; Green 2004; Green and Gerber 2004; McNulty 2005 [this volume]; Cardy 2005 [this volume]; Nickerson 2004) try to overcome these problems by randomly assigning phone calls to treatment and control groups. The present study reports the results of two such experiments. The West Haven experiment was conducted in 1998 and reported in Gerber and Green (2001). The published results of that study contained some errors, which we correct here. The substantive conclusions, however, are unaffected by the corrections. The second study, which was designed to replicate and extend the West Haven experiment, was conducted in 2002 in Iowa and Michigan. Both studies use similar nonpartisan scripts and very large samples of registered voters. The West Haven study randomly assigned 12,348 households containing 17,866 voters to treatment and control conditions. The Iowa and Michigan experiment comprises two subexperiments, depending on whether voters resided in a competitive or uncompetitive congressional district. Within each stratum, we randomly assigned one registered voter from each household to an experimental group. A total of 59,972 registered voters were assigned to receive phone calls, and 1,845,348 were assigned to a control group.

Consistent with the results of other large experiments using brief phone calls made by telemarketing firms delivering partisan messages (McNulty 2005; Green 2004; Cardy 2005), we find little evidence that telemarketing firms increase turnout by reading brief nonpartisan scripts. Although telemarketing calls are relatively inexpensive, they are not necessarily a bargain. If one were to pay 50 cents per completed call, the cost per vote associated with this type of voter mobilization effort falls in the neighborhood of $250, which is not competitive with other voter mobilization tactics (Green and Gerber 2004).

The West Haven Experiment

This experiment was performed during the 1998 general election in West Haven, Connecticut, a town of fifty-four thousand people. Connecticut holds state elections during federal midterm elections, so in addition to a congressional race, the ballot included races for several statewide offices (including governor), as well as races for state representative and state senator. From public records, we obtained a list of all registered voters, which we sorted by household address. Excluded from the sample were all voters with post office box addresses and all addresses where more than two registered voters resided. We then randomly assigned households on this list to the experiment and treatment groups. We provided the list of names in the treatment group to Survey Sampling, Inc., which performed a telephone match and located phone numbers for 69 percent of the sub-

jects who were selected to be called. After the election, we determined from the voter cross-off sheets which registered voters had actually cast ballots.[1]

The basic experimental treatment was a phone call reminding the voter of the upcoming election. All calls were made between November 1 and Election Day, November 3. The phone calls were executed by a Washington, D.C., area political consulting firm that specializes in political phone calls. The firm has done extensive work in U.S. Senate races and House races, as well as state and local elections. We hired this firm in the spirit of building realism into our experiment: with the capacity to conduct more than one hundred thousand calls in a single day, this phone bank is typical of the large-scale firms that campaigns turn to for voter mobilization campaigns that involve statewide or national target groups.

Although telemarketing calls are relatively inexpensive, they are not necessarily a bargain.

It should be noted, however, that the quantity of calls that a telemarketing firm can generate is more impressive than the quality of those calls. Having monitored several hours of phone calls, it is our impression that the telephone scripts were generally delivered competently, but sometimes hastily or mechanically. In other words, the calls sounded as though they were made by a commercial firm rather than local volunteers or neighbors.

Callers read scripts that were developed in collaboration with professional political consultants. The themes were selected and the wording crafted with the goal of stimulating turnout as much as possible. The experimental GOTV phone calls each began with a reminder about the upcoming election. The calls continued with one of three different short appeals designed to stimulate voting. All three versions of the phone scripts (the scripts are provided in the appendix) began with

> Hi. This is (caller's name) calling from Vote '98, a nonpartisan group working with the League of Women Voters. We just wanted to remind you that elections are being held this Tuesday.

We also sought to estimate the effects of getting citizens to pledge to go to the polls, and so we prepared two versions of each of the three treatments. In the first version, the phone callers closed their appeal by saying, "We hope you'll come out and vote." For a random subset of each treatment group, this closing was replaced with the question, "Can we count on you to vote this Tuesday?" The callers then

waited for the respondent to answer before concluding the call. Previous research suggests that asking the respondent to affirm a voting intention might boost the effectiveness of the appeal (Greenwald et al. 1987; Morwitz, Johnson, and Schmittlein 1993). As we are asking for a pledge of participation rather than simply soliciting a prediction about the respondent's intentions, our work finds a close analogy in Reams and Ray's (1993) recycling experiment, which demonstrates the importance of wringing a commitment out of would-be participants. Although a verbal promise to a stranger is a long way from signing an enforceable contract, these previous findings imply that making a pledge might create or reinforce feelings of obligation.

A final treatment group received a phone call asking the respondent to donate blood to an upcoming Red Cross blood drive. (The script is provided in the appendix). As described below, the purpose of this placebo appeal was to provide another benchmark for experimental comparison. The blood drive "treatment" also serves to illustrate the fallacy of comparing the voting rates of those who are contacted by phone banks to the voting rates of those who are not contacted. Even if the message has no effect on turnout, those who are easier to reach by phone tend to vote at higher rates.

Random assignment was performed at the household level, and each household was placed into a control group, a placebo group, or a GOTV group.[2] This procedure placed 7,137 individuals in the control group, 3,005 in the placebo group, and 7,724 in the group receiving one of three GOTV messages. Of those encouraged to vote, approximately 60 percent were assigned to be asked the follow-up question, "Can we count on you to vote?"

Statistical Model

To gauge the effect of receiving a GOTV phone call, we have to allow for the fact that the phone bank will be unable to reach some of the people assigned to receive GOTV calls. In the West Haven experiment, some people could not be reached because we did not know their phone numbers; others, because they did not answer the phone when we called; and still others, because they refused to listen to our GOTV message after they answered our call. We want to estimate the effects of the GOTV treatment on those who actually received our calls, while allowing for the possibility that people who can be reached by phone may be more likely to vote than those who cannot be reached.

For analytic purposes, let us divide our population of voters into two groups, those who are reachable and those who are not. Let p_r denote the probability that a reachable person votes, and let p_{nr} denote the probability that a nonreachable person votes. Thus, the expected voting rate in the control group is a weighted average of these two probabilities. Let α represent the share of the population that is reachable. The expected voting rate in the control group (V_c) is

$$V_c = \alpha p_r + (1 - \alpha)p_{nr}. \quad (1)$$

In the treatment group, the reachable people, having been exposed to a GOTV message, have a probability of voting of $p_r + t$, where t is the treatment effect. Random assignment of subjects into experimental groups means that the treatment and control groups have the same expected proportion of reachable people. Thus, the expected voting rate in the treatment group is

$$V_t = \alpha(p_r + t) + (1 - \alpha)p_{nr}. \quad (2)$$

These two equations suggest a consistent estimator of the treatment effect. We have sample estimates of the theoretical quantities V_c (the observed voting rate among those assigned to the control group), V_t (the observed voting rate among those assigned to the treatment group), and α (the observed proportion of contacted people among those who were assigned to the treatment group). Substituting these sample values into these equations and then subtracting equation (1) from equation (2), we obtain

$$\frac{\hat{V}_t - \hat{V}_c}{\hat{\alpha}} = \hat{t}. \quad (3)$$

This estimator allows us to generate consistent estimates of the treatment effect even though reachable people and nonreachable people may vote at very different rates. This estimator is equivalent to an instrumental variables regression of vote on actual treatment, using assigned treatment as an instrument (Angrist, Imbens, and Rubin 1996; Gerber and Green 2000).

This estimation approach can be extended to cover the case in which some people are called with a placebo treatment urging blood donation. The same algebra applies here, but now we define the population as the group of people whose phone numbers are known. Some are reachable by a GOTV message and others not. The proportion of reachable people is assumed to be the same in the GOTV and placebo groups. We can estimate this proportion using the observed proportion of reachable people among those called by the phone bank. The estimator in equation (3) is used, this time using the voting rates among those called (but not necessarily reached) in the treatment (GOTV) and control (placebo) groups. The key assumption in the use of a placebo control is that the placebo treatment has no effect on voter turnout.

Both the placebo-controlled design and the untreated-control design estimate the same quantity, t. The potential advantage of the placebo-controlled design is that α is higher in this design, which reduces the standard error associated with the estimated treatment effect. On the other hand, the untreated-control design involves a larger number of observations than the placebo-control design, which offsets the lower contact rate. As we will see below, both estimation approaches produce similar results.

A final statistical issue concerns the proper calculation of the standard errors. The unit of assignment in the West Haven experiment was the household. Since members of two-voter households have correlated characteristics—they are often

TABLE 1
VOTER TURNOUT RATES BY EXPERIMENTAL GROUP

	Entire Sample		Those Called by Phone Bank	
Experimental Group	Turnout Rate	*n*	Turnout Rate	*n*
Control	51.5%	7,137		
Voter mobilization	51.1%	7,724	57.0%	5,271
Requested blood donation	50.9%	3,005	57.3%	2,089

similar in age and are exposed to similar campaign activity—they cannot be treated as independent observations. Proper calculation of the standard errors requires that we account for this intrahousehold correlation, which is easily done using Stata 8.

Data Analysis

Table 1 reports the key findings of the West Haven experiment. The leftmost column of numbers suggests that *assignment* to the GOTV group did not increase turnout. Counter to our expectations, those assigned to the control group voted at a slightly higher rate (51.5 percent) than those who were assigned to the treatment group (51.1 percent). The same pattern holds if we combine the control group with the placebo group and compare their joint turnout rate (51.3 percent) with that of the GOTV group. Since the contact rate (α) must fall between 0 and 1, it follows that the estimated treatment effect derived from equation (3) is negative.

A similar assessment may be derived from the second column of numbers in Table 1, which compares the turnout rates of those whom the phone bank *attempted* to call using either a GOTV or blood donation script. This column of numbers excludes subjects with unknown telephone numbers; because we did not attempt to locate phone numbers for the control group, these subjects do not appear in this part of the table. Again, we find that those called with a GOTV message were no more likely to vote (57.0 percent) than those called with a placebo message (57.3 percent). Thus, before we attempt to calculate the contact rate (α), we know something important about the findings of this study. Since the numerator of equation (3) is negative, and since the contact rate must lie between 0 and 1, the estimated treatment effect must also be negative.

To estimate how negative the treatment effect is, we must first define "contact." In previous work (cf. Gerber and Green 2000, 2001), we defined contact generously so as not to overstate the magnitude of the treatment effects. A voter was considered contacted if the phone message was delivered in its entirety to someone in the treatment household who identified himself or herself as a voter listed by the caller. The meaning of a contact is therefore somewhat different in one- versus two-person households. For households with only one registered voter, a contact means that this registered voter was identified and received the treatment. In the

TABLE 2
ESTIMATED EFFECTS OF TREATMENT, ACCOUNTING FOR CONTACT RATES

1. Calculation based on the original treatment assignments, using a broad definition of contact

{Voting rate in the assigned get-out-the-vote [GOTV] group (51.10%) – Voting rate in control or placebo groups (51.34%)} divided by the contact rate in the GOTV group (44.48%) = –0.54% (robust *SE* = 1.97%)

2. Calculation based on the original treatment assignments, using a narrow definition of contact

{Voting rate in the assigned GOTV group (51.10%) – Voting rate in control or placebo groups (51.34%)} divided by the contact rate in the GOTV group (32.61%) = –0.74% (robust *SE* = 2.70%)

3. Calculation based on the subjects who were called by the phone bank, using a broad definition of contact

{Voting rate in the assigned GOTV group (57.03%) – Voting rate in control or placebo groups (57.30%)} divided by the contact rate in the GOTV group (65.19%) = –0.42% (robust *SE* = 2.30%)

4. Calculation based on the subjects who were called by the phone bank, using a narrow definition of contact

{Voting rate in the assigned GOTV group (57.03%) – Voting rate in control or placebo groups (57.30%)} divided by the contact rate in the GOTV group (47.79%) = –0.56% (robust *SE* = 3.07%)

case of two-voter households, often just one of the two registered voters in the household was identified and received the treatment.

An alternative approach is to count the total number of GOTV contacts that the phone banks made with voters on their target lists. Of the 2,960 GOTV treatment households containing one voter, 1,028 were contacted. Of the 2,382 households containing two voters, 1,204 were contacted (in 287 of these cases, the caller spoke to two people in the household). Thus, out of the pool of 7,724 individuals who were assigned to GOTV calls, a total of 2,519 actually listened to the GOTV message, which implies a contact rate of 32.6 percent. Using the more generous definition of contact above, this rate climbs to 44.5 percent.

Table 2 calculates the estimated effects of the experimental treatments based on the turnout and contact rates presented in Table 1. We find phone calls to have weak effects that cannot be distinguished statistically from zero. The turnout rate for the GOTV group is 51.1 percent. The turnout rate for the control group and the placebo group is 51.3 percent. The "generous" contact rate is 44.5 percent. The estimated effect of actually receiving a GOTV phone call is therefore –0.5 percentage points with a robust standard error[3] of 2.0.

An alternative approach to analyzing the effect of receiving a political message is to compare the turnout rates among those who were called in an effort to deliver a GOTV message and those who were called with a placebo message.[4] Any difference in observed turnout rates would then be ascribed to the political message. The advantage of this approach is that 65.2 percent of those who were called with a GOTV message were actually contacted, as opposed to a 44.5 percent contact rate among all of those assigned to the GOTV group (some of whom did not have known phone numbers). Again, however, we find a weakly negative treatment effect. Recall from Table 1 that the voting rates for those called with GOTV and blood donation scripts are 57.0 and 57.3 percent, respectively. Dividing by the contact rate of 65.2 percent produces an estimated treatment effect of –0.4 with a robust standard error of 2.3.

Looking more closely at the variants of the GOTV message, we find that none of them works especially well. Among those called by the phone bank, turnout was actually slightly lower among those who were assigned to the question prompting them to commit to voting than those not prompted. The prompted (n = 3,132) turned out at a rate of 56.6 percent, whereas the unprompted (n = 2,139) voted at a rate of 57.6 percent. And whereas those called with a blood donation script turned out at a rate of 57.3 percent, those called with one of three GOTV scripts turned out at rates of 56.5 percent (n = 2,085), 57.3 percent (n = 2,163), and 57.4 percent (n = 1,023). None of these contrasts remotely approach statistical significance. This pattern of findings suggests that brief nonpartisan phone calls are ineffective at raising turnout.

Notice that the conclusion would be quite different if we were to analyze these data as though they were derived from a nonexperimental study. The turnout rate among those whose households were actually contacted by a GOTV message was 64.5 percent, as compared to a rate of 48.1 percent for all of the other subjects in the study ($p < .001$). However, this apparent "effect" is belied by the fact that the same conclusion can be drawn from the blood donation appeal. Those residing in households contacted by a blood donation appeal voted at rate of 67.2 percent, as compared to 50.1 percent for everyone else ($p < .001$). The problem with the nonexperimental approach is that it assumes (incorrectly) that reachable and nonreachable people have the same propensity to vote in the absence of a treatment. The experimental analysis presented here imposes no such assumption.

Iowa and Michigan Experiments

We conducted a replication of the West Haven phone experiment in the federal midterm elections held in November 2002. Two national phone banks were hired to read a script very much like the ones used in the 1998 studies:

> Hello, may I speak with (name of person) please? Hi. This is (caller's name) calling from Vote 2002, a nonpartisan effort working to encourage citizens to vote. We just wanted to remind you that elections are being held this Tuesday. The success of our democracy

TABLE 3
VOTER TURNOUT BY EXPERIMENTAL GROUP, STATE, AND LEVEL OF COMPETITIVENESS

	Competitive Congressional Race			Uncompetitive Congressional Race		
	Control	Phone Bank No. 1	Phone Bank No. 2	Control	Phone Bank No. 1	Phone Bank No. 2
Michigan						
Voter turnout	55.9	55.9	56.5	51.0	50.6	50.9
Contact rate	0.0	30.0	47.3	0.0	31.6	41.9
n	317,182	7,500	7,500	1,153,072	7,490	7,482
Iowa						
Voter turnout	64.1	63.9	63.3	55.5	57.0	56.2
Contact rate	0.0	36.8	53.4	0.0	38.6	53.3
n	289,163	7,500	7,500	85,931	7,500	7,500

depends on whether we exercise our right to vote or not, so we hope you'll come out and vote this Tuesday. Can I count on you to vote next Tuesday?

The population for this study was registered voters in Iowa and Michigan. These states were chosen because they maintain high-quality computerized voter files with vote history for each registered voter. The congressional districts of each state were divided into "competitive" and "uncompetitive" strata. Within each stratum, households containing one or two registered voters were randomly assigned to treatment and control groups. Only one type of treatment was used: a GOTV phone call. Just one representative from each household was assigned to treatment or control; the other voter was ignored for purposes of calling and statistical analysis. Since only one member of each household was treated, no complications arise due to correlation within households. A total of 60,000 individuals were assigned to be called; the corresponding control group contains 1,846,885 individuals. At the time of this writing, voter turnout results for the November 2002 elections remain unavailable in two small Michigan counties. Eliminating those observations reduces the treatment group to 59,972 and the control group to 1,845,348.[5]

Table 3 shows that the 2002 results closely resemble the 1998 findings. Of the eight possible experimental comparisons (2 Phone Banks × 2 Levels of Competitiveness × 2 States), four show higher turnout in the treatment group than the control group, and four show lower turnout in the treatment group. The pattern of results shows no clear relationship between the size of the treatment effect and the level of competitiveness in the congressional district. In Michigan, the treatment effects seem stronger in the competitive districts; in Iowa, the reverse is true. Neither phone bank seems particularly effective; they each generate positive treatment estimates in half of the comparisons. Only one of the eight comparisons has a *t*-ratio of more than 1.65, and even here the unexpectedly large effect produced by

the first phone bank in the uncompetitive Iowa stratum falls short of statistical significance once a Bonferroni correction is made for multiple comparisons.

These patterns can be established more precisely using instrumental variables regression. The effect of the treatment-on-the-treated, controlling for the design strata (competitiveness levels within each state), is 0.4. Due to the massive *N*, the standard error of this estimate is just 0.5, which means that the 99 percent confidence region extends from –0.9 to 1.7. The effects of phone banks are similar across states (Iowa: $b = 0.6$, $SE = 0.6$; Michigan: $b = 0.1$, $SE = 0.8$), competitiveness strata (uncompetitive districts $b = 1.0$, $SE = 0.7$; competitive districts $b = –0.3$, $SE = 0.7$), and commercial phone bank firms (first phone bank $b = 0.6$, $SE = 0.9$; second phone bank $b = 0.2$, $SE = 0.6$). The results remain unchanged when controls are introduced for past voting behavior and age ($b = 0.4$, $SE = 0.4$). Again, we cannot reject the null hypothesis that brief nonpartisan calls do nothing to increase voter turnout.

Discussion

The early literature on phone canvassing indicated that it could have profound effects on voter turnout. The 15 to 26 percentage point increases in turnout reported in Eldersveld (1956); Miller, Bositis, and Baer (1981); and Greenwald et al. (1987), however, were based on very small samples. As Gerber, Green, and Nickerson (2001) pointed out, the fact that large effects emerge from small samples may be more than coincidence. If journals are reluctant to publish statistically insignificant findings, then smaller studies must render large estimated effects to find their way into print. Their review of the published literature on phone calls reveals a powerful inverse relationship between sample size and effect size.

The one early study of appreciable sample size (Adams and Smith 1980) found that calls on behalf of a candidate running in a special Washington, D.C., election produced a large increase in turnout, but recent experimental work based on much larger samples has tempered the conclusion that calls from commercial phone banks stimulate voter turnout. The West Haven experiment, which is more than twice the size of the Adams and Smith study, finds that several different variants on a nonpartisan script fail to increase voter turnout. The Iowa/Michigan experiments, which are much larger than the West Haven experiment, suggest that calls from commercial phone banks have at most a minimal positive effect. One hypothesis is that Adams and Smith studied the effects of calls on behalf of a candidate, whereas the experiments reported here use nonpartisan scripts. This hypothesis is called into question by the recent experiments of McNulty (2005) and Cardy (2005). McNulty's study examined the effects of calls on behalf of a ballot proposition; Cardy's examined the effects of two phone calls on behalf of a gubernatorial primary candidate, one designed to persuade voters and the other to mobilize them. Neither study found phone calls to be effective. There remains the possibility that phone calls of the sort deployed by Adams and Smith are effective only in

very low-salience elections, but that hypothesis has yet to be tested in recent research.

The West Haven experiment, which is more than twice the size of the [largest previous] study, finds that several different variants on a nonpartisan script fail to increase voter turnout.

This is not to say that phone calls are inherently incapable of motivating voters in competitive electoral settings. During the 2000 presidential elections, local nonpartisan phone banks staffed by volunteer callers increased turnout among the young voters they targeted (Gerber and Green 2001). Volunteer phone banks also increased turnout in the 2002 when targeting low-propensity Latino voters with nonpartisan appeals (Ramírez 2005) or targeting young voters with pro-Democratic appeals (Nickerson, Friedrichs, and King forthcoming). Even commercial phone banks proved successful in 2002 when a special premium was paid to train and supervise callers reading longer, more interactive scripts (see Green and Gerber 2004). Apparently, the problem is not that phone calls are ineffective. The problem seems to be that mechanically delivered phone scripts are ineffective. The task for future research is to more systematically assess the extent to which a phone call's effectiveness is determined by the manner in which the script is delivered.

Appendix

Scripts used in the West Haven experiment

1. Red Cross

After identifying someone on the list, the caller proceeds with

Hi. This is (caller's name) calling on behalf of the American Red Cross to invite you to donate blood at an upcoming blood drive in your community. Each day volunteer blood donors are needed to support patients in Connecticut's hospitals. Your blood donation

could save someone's life. Can a representative from your local blood drive call you to schedule an appointment?

2. Three Versions of the Get-Out-the-Vote (GOTV) Message

After the text reported in the article, the phone call proceeded with one of three appeals. In the *close election* condition, the script continued with, "Each November, significant elections are decided by a small number of votes. The races this year are very close, so please vote on Tuesday." In the *civic duty* appeal, the phone call instead went on to say, "The success of our democracy depends on whether or not we exercise our right to vote, so we hope you'll come out and vote this Tuesday." In the *neighborhood solidarity* condition, callers stated that "Politicians sometimes ignore issues in a neighborhood when its people don't vote, so we hope you'll come out and vote this Tuesday."

Corrections to Version Published in Gerber and Green (2001)

The five experimental groups (control, blood, and the three GOTV treatments) were mislabeled, causing errors in the tables and obscuring the fact that the contact rates varied across experimental conditions. As a result, the 2001 article mistakenly compared those contacted in the placebo/GOTV conditions. Also, the text in the 2001 essay reversed the sequence in which random assignment occurred. We first assigned the experimental groups and then obtained phone numbers for names in the phone treatment groups. Some of the estimation nuances are different in this version. The 2001 essay did not take clustering within households into account when computing standard errors. In this version, we do not exclude missing data; those not found on the voter rolls are considered nonvoters. For corrections to the tables reported in Gerber and Green (2001), see "Erratum," *Public Opinion Quarterly* 68 (3): 489.

Notes

1. The outcome measure used here is scored 1 if one's name was found on the registrar's cross-off list and 0 otherwise. In Gerber and Green (2001), we eliminated observations where names could not be found on the registrar's Election Day voter list. The results reported here change trivially when we eliminate missing data. The proportion of voters with valid vote data is 96.5 percent for the control group, 96.1% for the get-out-the-vote (GOTV) group, and 96.5 percent for the blood donation group.

2. To verify the random assignment was done properly, we used multinomial regression to predict the assignment of households to a GOTV treatment, placebo treatment, or control group. The predictors were the numbers of voters in the household, district of residence, and whether phone numbers are listed on the voter file. As expected, this regression proves statistically insignificant (chi-square with 22 df of 19.4, $p = .62$).

3. The robust standard error takes into account clustering within two-person households.

4. In recording the disposition of their calls, callers indicated whether they had completed their scripts. (The phone bank was paid on the basis of the number of completed scripts.) One unfortunate consequence of

this coding scheme is that the contact rate dropped when callers prompt respondents to commit to voting because some respondents hung up or refused to answer. Thus, we cannot make a direct comparison between alternative scripts conditional on contact because contact means something different for different experimental groups. Instead, we must limit our comparison to those we attempted to call. The same point holds for the blood donation script. This script enlisted slightly lower rates of cooperation than the GOTV script. Of those called for a blood donation, 58.8 percent completed the call, as compared to 65.2 percent among those called with a GOTV script.

5. As a randomization check, we used logistic regression to predict treatment based on vote in 2000, age, the number of registered voters in a household, and the state house district. As expected, the chi-squares for each stratum are nonsignificant: Iowa noncompetitive ($df = 24$, $p = .49$); Iowa competitive ($df = 63$, $p = .72$); Michigan noncompetitive ($df = 95$, $p = .60$); Michigan competitive ($df = 31$, $p = .23$).

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