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Do Community-Based Voter Mobilization Campaigns Work Even in Battleground States? Evaluating the Effectiveness of MoveOn's 2004 Outreach Campaign*

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ABSTRACT

One of the hallmarks of the 2004 presidential election was the unusual emphasis on face-to-face voter mobilization, particularly face-to-face mobilization conducted within neighborhoods or social networks. Unlike previous studies of face-to-face voter mobilization, which have focused largely on nonpartisan campaigns conducted during midterm or local elections, this study assesses the effects of a campaign organized by MoveOn.org, an organization that allied itself with the Democratic Party in 2004 to aid presidential candidate John Kerry. A regression discontinuity analysis of 46,277 voters from 13 swing states demonstrates that neighbor-to-neighbor mobilization substantially increased turnout among target voters during the 2004 presidential election. Contact with MoveOn volunteers increased turnout by approximately nine percentage-points. This finding corroborates experimental findings showing the effectiveness of door-to-door canvassing but contradicts results suggesting that such mobilization is ineffective in the context of high-salience elections.

One of the hallmarks of the 2004 presidential election was the extraordinary emphasis that campaigns across the political spectrum placed on voter mobilization. Hundreds of thousands, and possibly millions, of paid and volunteer canvassers conducted get-out-the-vote activities during the final weeks of the campaign. An unprecedented proportion

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of National Election Studies respondents (50.4%) reported having been contacted by a campaign, a number that far surpasses any election since the survey began in 1952 (Bergan *et al*. 2006, p. 764). In the end, both parties exceeded their target vote totals, as voter turnout rose from 107.4 million votes in 2000 to over 123.5 million in 2004 (McDonald 2007).

The 2004 election is remembered not only for the scale of its get-out-the-vote activities, but also for the manner in which these activities were organized. Republicans recruited conservative Christians, gun owners, and other pro-Bush social groups as canvassers of potential Republican voters. Democratic campaigns relied more heavily on paid canvassers, but large numbers of liberal activists migrated to battleground states as part of volunteer get-out-the-vote efforts. The net result was that although the campaign featured a heavy volume of impersonal communications via television and direct mail, voters in 2004 also received unusually personal and heartfelt appeals on behalf of the presidential candidates.

How effective were the GOTV campaigns conducted by core activists? Although political scientists from Rosenstone and Hansen (1993) to Putnam (2000) to Gerber and Green (2000) have long argued that face-to-face canvassing by committed volunteers constitutes an effective means for increasing voter turnout, a close look at the evidence reveals how little the discipline knows about the effectiveness of partisan canvassing campaigns during presidential elections. Since the 1990s, randomized experiments gauging the effectiveness of canvassing have primarily looked at nonpartisan campaigns conducted in low-salience elections. Of the experiments that have targeted populations of registered voters, Gerber and Green (2000) studied a nonpartisan campaign in an uncompetitive 1998 midterm election, and their follow-up studies focused on nonpartisan campaigns in municipal elections (Green *et al*. 2003; Nickerson 2005). Michelson (2003, 2005) studied Latino voters primarily in municipal elections (exception noted below), while Arceneaux (2005) studied African–American voters in municipal and federal midterm elections. Nickerson *et al*. (2006) examined a youth-targeted partisan campaign during the 2002 midterm election, but the canvassing effort was small. More conclusive is Nickerson's (2006) study of a large-scale campaign conducted by the Young Democrats of America during the 2005 Virginia gubernatorial election, but again the strong effects of canvassing emerge from a study of voters aged 18–35.

Although these studies are fairly uniform in their conclusion that canvassing increases the turnout of those who are reached at their doorsteps, it is not clear whether one can generalize their findings to partisan presidential campaigns, particularly in light of three studies: Bennion's (2005) study of nonpartisan canvassing in a hotly contested congressional district in 2002; Michelson's (2005) study of a partisan effort targeting Latinos in the California gubernatorial recall election of 2003; and Middleton's (2007) study in swing states during the 2004 presidential election. None of the studies that took place in contentious environments find significant positive canvassing effects, and the findings have been interpreted to mean that canvassing may be generally ineffective in high-salience elections. Given the small number of studies upon which this interpretation is based, the effectiveness of canvassing in high-salience elections remains an open question.

Addressing this question is no mean feat. For fear of losing votes, partisan campaigns in presidential elections are reluctant to randomize their canvassing efforts in battleground states. One therefore must rely on some type of non-experimental research design. One option is to analyze election survey data, but these have been roundly criticized on the grounds that campaign contact is endogenous — campaigns target and reach voters with unusually high voting propensities (Gerber and Green 2000, 2005). An alternative approach and one that has received increasing attention in political science is regression-discontinuity analysis (Thislethwaite and Campbell 1960), which takes advantage of situations in which geographical boundaries partition otherwise similar neighbors into treatment and control groups. This approach requires detailed information about the manner in which campaigns targeted and treated individuals living in different areas. Fortunately, we were able to obtain such data from a group that coordinated large-scale canvassing efforts in several battleground states, MoveOn.org. These data afford us the opportunity to estimate the effects of door-to-door canvassing by comparing across-the-street neighbors who, due to the arrangement of precinct boundaries, fell just inside of or just outside of areas that MoveOn.org precinct captains canvassed.

MOVEON

MoveOn.org was organized in 1998 during the Clinton impeachment hearings to collect signatures for an online petition asking congress to "immediately censure President Clinton and MoveOn to pressing issues facing the country (Hafner 1998)." MoveOn collected 500,000 online signatures (Evangelista 1998; Hafner 1999) and garnered national media attention as a result. MoveOn founders chose to continue their operation indefinitely, gathering petition signatures for a broad range of liberal and progressive causes (Schemo 2004). MoveOn expanded its repertoire of political activism to include political fundraising (Feifer 2004; Scott 2004) and the production of documentary films (Grynbaum 2004) and political advertisements (Rutenberg 2004; Hefling 2005). These activities were integrated into a set of community organizing tactics (Packer 2003), as MoveOn sponsored events such as house parties where volunteers hosted other local members at their homes and discussed political issues, made advocacy phone calls, or watched political documentaries.

The goal of MoveOn's 2004 effort was to increase turnout among supporters of Democratic presidential candidate John Kerry. MoveOn worked with a political firm to identify precinct leaders from membership rolls. Once the precinct leaders were in place, MoveOn's web interface provided these precinct leaders with voter contact information and gathered information about attempted contacts. The targeting strategy provides the analytic foundation for our regression discontinuity analysis, while the data gathered from field workers enables us to estimate the effects of actual contact with intended targets. This section therefore lays out in detail the steps by which MoveOn targeted and contacted voters.

MoveOn selected target precincts in 17 swing states. Voters considered most likely to vote for Kerry were identified by combining voter files with information from commercial data aggregators. Although the precise targeting criteria varied somewhat across states, the following factors were considered when selecting precincts:

- Number of voters who would presumably lean toward or consider Kerry according to information provided by The National Committee for an Effective Congress.
- Number of MoveOn members in the precinct. To mount a successful mobilization effort, sufficient numbers of MoveOn members had to be present to run the operation.
- Proportion of voters registered as Democrats. Precincts with the highest proportion of Democrats were ruled out based on the assumption that these were likely targets for other organizations.

Ultimately MoveOn identified precincts containing a total of 10,798,949 target voters. This pool of precincts represents the initial population from which our subset of treatment and control precincts is drawn.

Next, MoveOn recruited precinct leaders from among their membership within target precincts. Initial contact was made through email or telephone. The precinct leaders in turn recruited and organized local volunteers for local canvassing. As they canvassed potential Kerry supporters, volunteers in turn recruited additional canvassers.

The voter outreach component of the MoveOn canvassing campaign took place in three phases. During each phase an internet interface was the backbone of managing the efforts of local volunteers and recording information on canvassing outcomes.

In the first phase, targeted voters were contacted to identify individuals intending to vote for Democrat John Kerry. When contacted, individuals were asked their vote preference. Individuals who indicated Kerry were classified by the campaign as likely Kerry voters and encouraged to vote on Election Day. If the individual favored another candidate, they were simply thanked for their time. Volunteers recorded the candidate preference of each contacted voter using MoveOn's online interface.

In the second phase, during the five days prior to Election Day, MoveOn volunteers attempted to re-contact those who indicated they would vote for Democrat John Kerry and once again asked them to vote. In the second phase volunteers also attempted to contact voters who could not be reached during the first phase to determine if the voter supported John Kerry and if so, to encourage them to vote. The third phase took place on Election Day, as volunteers attempted to re-contact voters who indicated support for Kerry in either of the first two phases.

In the analysis that follows, the term *contact* refers to encouragement to vote by a canvasser. As the canvassing scripts in the appendix indicate, the first round of communication was essentially a brief opinion survey; once the respondent expressed a preference for Bush, the survey ended. Given that recent large-scale experiments show that pre-election surveys have no effect on voter turnout (Smith *et al*. 2003; Mann 2005), it seems quite reasonable to assume that this initial conversation has no effect. Upon expressing some type of pro-Kerry preference, respondents were encouraged to vote and slated for a possible follow-up visit. Overall, 24.7% of the initial target group participated in the voter identification phase of the campaign, and 15.4% of those targeted were, by dint of their expressed preference, encouraged to vote. Below we develop a statistical model to estimate the average effect of contact among the 15.4% who were contacted.

Although a few contacts were made over the phone, the MoveOn effort was primarily a face-to-face campaign. Overall, 91% of successful contacts were made via door-to-door canvassing.[1] Given the preponderance of face-to-face contact and the research literature's conclusion that face-to-face contact is more effective than phone contact (Green and Gerber 2004), we interpret the effects estimated below as the effects of door-to-door visits. Prior experimental research on campaigns that employed an initial wave of voter identification canvassing followed by voter mobilization canvassing during the last week of the campaign found this strategy to be highly effective (Arceneaux 2005; Michelson 2005). It is worth emphasizing that estimates represent the effect of the entire treatment regimen, which in some cases included multiple contacts.

METHOD AND ANALYSIS

To measure the effectiveness of MoveOn's mobilization campaign we use regression discontinuity analysis.[2] This type of analysis is valuable in situations where individuals were selected for treatment based on a cutoff value along a continuum. As illustrated in Figure 1, we consider cases along the discontinuity boundary which is the geographic location dividing (1) a precinct where MoveOn found a precinct leader and attempted to mobilize voters from (2) a precinct initially identified for mobilization efforts but where MoveOn was unable to find a precinct leader. It should be stressed that this comparison *focuses solely on adjacent precincts that MoveOn originally sought to mobilize.* The difference is that in some precincts, leaders were found and therefore GOTV efforts went forward. No canvassing occurred in leaderless precincts.

Our analytic focus is the narrow set of voters who live on the border between adjacent leader-led and leaderless precincts. While one approach might be to include the entirety of adjacent precincts in such a discontinuity analysis, we limit the data set to just those individuals who live on *streets bisected by such a boundary*. The critical assumption in our analysis is that across-the-street neighbors have the same expected turnout rates in

1 When a canvasser successfully made contact with a registered voter, MoveOn recorded (1) that the contact occurred and (2) the mode of contact (i.e., phone vs. door-to-door). However, when a canvasser made an *unsuccessful attempt* at contact, the attempted contact was recorded *but not the mode of the attempt*. This was an unfortunate gap in MoveOn's record keeping. One consequence of the gap is that independent effects of phone and door-to-door are slightly confounded for 2SLS analysis. One can look at this as a bounds problem. Our estimates represent one end of the continuum, where phone and in-person contacts are assumed to be equally effective. At the opposite end of the continuum, phone contacts are assumed to have no effect. Because phone contact occurs so rarely, the difference in coefficients is less than 10%, and our assumption errs on the side of producing more conservative point estimates.

2 Regression discontinuity analysis was conceived by Thislethwaite and Campbell (1960) to examine the effects of merit based scholarships on academic outcomes (see also Visser and De Leeuw 1984; Angrist and Lavy 1999). This methodology is used to study topics in political science as well (Lee 2008; Butler and Butler 2005).

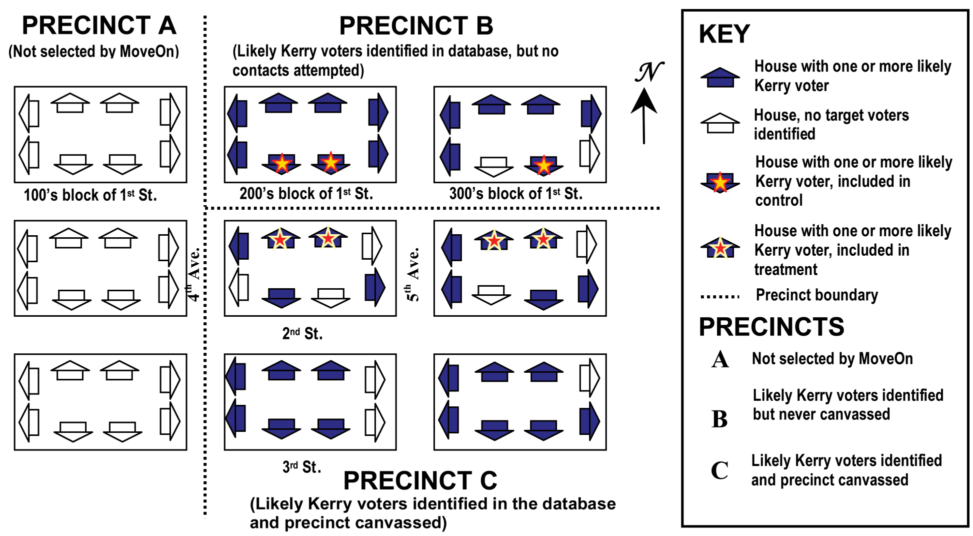

Figure 1. Illustration of how voters were selected for inclusion in the regression discontinuity analysis. Represented is the intersection of three fictional precincts. Precinct A was not chosen by MoveOn for canvassing. Precinct B and C were selected by MoveOn, and target voters (those likely to vote for Democrat John Kerry) were identified. Suitable precinct leaders were sought for precincts B and C, but a precinct leader was found for precinct C only. As a result, voters in precinct B were never canvassed. Target voters along the precinct boundary between precincts B and C were included in the study. Target voters on the south side of the 200's and 300's blocks of 1st Street are in precinct C and are in the treatment group. Voters on the north side are in precinct B and are in the control group.

the absence of contact from canvassers. On the basis of this assumption, voters on the non-targeted side of the street are considered a control group for the individuals targeted across the street.

We attempted to verify our assumption that voters on opposite sides of a precinct boundary have similar voting propensities. Under the null hypothesis, variables such as past voting and age should fail to predict the quasi-random assignment of some voters to the treated side of the block and others to the untreated side. Visual inspection of Table 1 does not reveal any striking differences. The treatment and control groups have voting rates within 2 percentage-points of one another in 41 of 52 pairwise comparisons. However, an F-test based on individual-level data and allowing for clustering by street block shows a statistically significant deviation from the null model of no relationship between the covariates listed in Table 1 and whether an individual was "assigned" to treatment or control blocks ($F = 2.27$, $df_1 = 107$, $df_2 = 7641$, $p < 0.001$).

Table 1. Summary statistics for available covariates by state

State	Side of Block	N	# Blocks	Contact Rate (%)	Avg. Age	% Reg. Dem.	% Male	% Gender Missing	General Election Turnout (%)				Primary Election Turnout (%)					% Vote History Missing
									1996	1998	2000	2002	1996	1998	2000	2002	2004	
AZ	Treatment	513	144	23.6	40.7	74.9	43.7	5.7			32.2	34.3			3.9	10.3	23.6	0.0
	Comparison	435			39.3	76.1	45.7	4.6			34.0	32.2			6.4	10.6	24.4	0.0
FL	Treatment	2322			47.5	70.0	39.1											
	Comparison	2279	403	10.2	51.7	66.6	38.8											
IA	Treatment	1113				39.7	42.6	3.1		9.5	22.5	11.8		1.4	1.0	3.1		13.5
	Comparison	939	276	7.0		47.2	42.7	4.2		8.8	21.0	11.1		1.3	0.9	3.5		19.8
MI	Treatment	1920			32.6		49.4	1.6										
	Comparison	1923	59	13.8	31.4		48.6	1.3										
MN	Treatment	688					45.5			19.0	24.4	46.4		5.7	5.2	7.1		5.2
	Comparison	732	268	12.1			48.2			18.4	19.1	47.8		5.2	3.1	5.6		7.5
MO	Treatment	2525			37.6		44.7	10.7	8.2	7.7	30.7	26.3	2.5	2.5	7.3	7.6	8.9	8.3
	Comparison	2495	394	11.5	36.6		44.2	11.3	8.8	9.5	30.6	22.4	2.2	2.5	8.3	7.9	8.5	7.7
NC	Treatment	529			43.9	94.7	43.1		17.0	18.1	44.8	31.8	5.7	4.9	8.5	12.7		0.0
	Comparison	588	186	15.1	42.9	94.2	40.3		13.4	17.3	42.5	37.8	3.4	5.1	7.1	12.4		0.0
NM	Treatment	415			37.9	57.3	40.7		14.2	15.4	27.2	12.0						11.1
	Comparison	325	102	13.0	30.2	57.2	44.0		10.2	11.4	25.2	12.9						24.3
NV	Treatment	380			32.9	58.4	38.2				11.8	9.7						32.4
	Comparison	342	43	12.1	33.4	65.2	37.7				13.7	8.2						32.5
OH	Treatment	1185			33.1		8.7	86.2										
	Comparison	1267	414	7.4	32.5		7.7	86.1										
OR	Treatment	176			44.4	51.1	30.1			10.1	5.7	16.3						0.0
	Comparison	215	51	5.1	35.5	52.1	47.0			6.6	14.0	19.8						0.0
PA	Treatment	11512			40.9	78.3	43.1	5.6	8.2	4.7	22.3	17.3	1.5	1.7	3.5	12.0	9.8	0.0
	Comparison	11182	840	19.1	40.7	78.7	45.5	6.6	8.0	4.4	21.7	16.9	1.7	1.7	3.1	11.7	9.2	0.0
WI	Treatment	106			30.0		50.9											
	Comparison	171	41	6.6	24.6		36.3											

Table 2. State-by-state randomization checks

State	N	Blocks	p
AZ	948	144	0.021
FL	4601	403	0.008
IA	2052	276	0.032
MI	3843	659	0.002
MN	1420	268	0.005
MO	5020	394	0.001
NC	1117	186	0.010
NM	740	102	<0.001
NV	722	43	0.096
OH	2452	414	0.839
OR	391	51	0.033
PA	22694	840	0.758
WI	277	41	0.100

Because targeted voters were identified by MoveOn *in advance* of the volunteer recruitment phase it is not clear why this difference occurs. During the campaign, to the best of our knowledge, the list of target voters was not modified as new voters were registered or existing voters were found to have moved. Our data base consists of only those individuals who were *originally targeted by MoveOn*. Apparently, precinct boundaries differ in a subtle fashion that a large-N statistical analysis is able to detect.

In Table 2 a state-by-state examination of covariate balance reveals that certain states make for an especially apt natural experiment. In particular, Pennsylvania shows outstanding balance between treatment and control groups; when the F-test presented above is conducted solely on observations from Pennsylvania, the test statistics is nonsignificant ($F = 0.72$, $df_1 = 14$, $df_2 = 1679$, $p = 0.758$). Because Pennsylvania is the only state with both a full set of covariates and excellent covariate balance, we present overall results and also analyze the cases from Pennsylvania separately. Reassuringly, results from Pennsylvania are not substantively different from the results obtained using the full sample.[3]

To estimate the intent-to-treat of treatment assignment (as opposed to actual contact by a MoveOn canvasser), we begin by using a linear probability model with and without covariates. The model without covariates is

$$Y_i = \alpha_0 + \alpha_1 X_i + \lambda_1 D_{1i} + \lambda_2 D_{2i} + \cdots + \lambda_{\mathcal{J}-1} D_{\mathcal{J}-1,i} + U_i, \tag{1}$$

[3] We do not include estimates for each state because the precision of state-level estimates for states other than Pennsylvania is too low for meaningful interpretation. Exploratory analysis using the `metareg` procedure in STATA did not reveal any significant predictors of state-level effects. Covariates in this analysis included baseline turnout rate, closeness of the Presidential race in state, 2000 presidential vote in state and party-registration status of the state. The null result is interesting in light of Goldstein and Ridout's (2002) observational study that finds an interaction between the effectiveness of mobilization efforts and target voters' propensity to vote.

where Y_i is a dummy variable scored 1 if person i voted, X_i is a dummy variable scored 1 if person i lives on the treated side of a precinct-boundary street, D_{ji} are dummy variables scored 1 if person i lives on block $j \in \{1, 2, \ldots, \mathcal{J}\}$, and U_i is a disturbance term. The inclusion of block-level fixed effects means that cross-street comparisons are made within each block; the intent-to-treat effect is essentially a weighted average of all within-block comparisons. An analogous probit model was also used to corroborate the linear regression analysis. The findings, presented in the Appendix, show that the linear and nonlinear regression models produce substantively identical results.

Equation (1) can be augmented with a set of covariates: dummy variables indicating whether an individual voted in the 1996, 1998, 2000, and 2002 *general* elections, dummy variables for voting in the 1996, 1998, 2000, 2002 and 2004 *primary* elections, the voter's age and the square of age, an indicator for whether the voter is a registered Democrat, and an indicator for whether the voter is male.[4] We do not ascribe a causal interpretation to the coefficients obtained for these variables, as they are collinear and redundant indicators of baseline voting propensities. The reason to include covariates is that doing so allows for a more precise estimation of the treatment effect.

The focal parameter in Equation (1) is α_1, the average effect of being assigned to a treatment block, or the intent-to-treat effect. The parameter of interest, however, is the average effect of actual (as opposed to attempted) contact. Although it is tempting to replace attempted with actual contact in the above equations, applying OLS regression to such a model would produce biased estimates, because the kinds of individuals who are reachable by canvassers may have higher unobserved voting propensities (Gerber and Green 2000). The proper estimation approach is instrumental variables regression, which takes these unobserved differences into account (Angrist *et al*. 1996). This method, which is now widely used in the experimental voting literature, is based on a two equation system. The first equation expresses contact by canvassers as a function of the quasi-random variable, *attempted* contact. When the matrix of covariates (W) listed above is included as control variables, the equation predicting *actual* contact (C_i) is

$$C_i = \gamma_0 + \gamma_1 X_i + \lambda_1' D_{1i} + \lambda_2' D_{2i} + \cdots + \lambda_{\mathcal{J}-1}' D_{\mathcal{J}-1,i} + W\Omega + U_i^C. \qquad (2)$$

The second equation in this system models vote as function of actual contact, covariates, and block level dummies.

$$Y_i = \beta_0 + \beta_1 C_i + \lambda_1'' D_{1i} + \lambda_2'' D_{2i} + \cdots + \lambda_{\mathcal{J}-1}'' D_{\mathcal{J}-1,i} + W\Delta + U_i^Y. \qquad (3)$$

The key parameter in this system is β_1, the average effect of actual contact with canvassers.[5]

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4 In some states one or more of the above-listed covariates were unavailable, in which case dummy variables were added to specify missing data as distinct from a value of zero. No observations were dropped on account of missing data.

5 Although techniques such as bivariate probit regression may be used to model a binary dependent variable embedded in a two-equation system such as ours, 2SLS accurately estimates the average treatment effect among the treated. Because no one in the control group received the treatment, the average treatment effect in this application is the same as what Angrist *et al*. (1996) refer to as the complier average causal effect.

Five key assumptions ensure that the treatment effect may be estimated consistently by two stage least squares (2SLS), using treatment group *assignment* as an instrument for actual contact. Angrist *et al*. (1996) describe these assumptions in detail, and we briefly comment on their suitability in our application. The first assumption is the *stable unit treatment value assumption*, which states that the outcome for each unit is unrelated to the treatment status of other units. This assumption could be violated, for example, if voters were to mobilize their across-the-street neighbors. We expect that this is a relatively uncommon problem and a relatively weak influence on voter turnout rates in the control group. To the extent that this assumption is violated, the voting rates of treatment and control groups will be more similar, causing us to underestimate the effects of the treatment.

The second assumption is that of *random assignment*. While random assignment was not used in this study, the core idea behind the regression discontinuity method is that the process that determines the treatment cut-off is nearly random, in the sense that it is arbitrary and unrelated to factors that might correlate with outcomes, at least at the point of discontinuity (Rubin 1977).

The third assumption is the *exclusion restriction* which says that the outcome can only be affected if treatment is *delivered* — meaning that contact with the voter is made — and no other process related to (near) random assignment has a causal effect on these individuals.

The last two assumptions concern the relationship between assignment to treatment group and the probability of receiving treatment. The fourth assumption is that there is at least some relationship between treatment assignment and the receipt of the treatment, which is certainly true in this study. The fifth assumption is that of *monotonicity*, which states that for every unit the likelihood of *receiving* treatment is not lower when assigned to the treatment group compared to the control group. Additionally for at least one case the likelihood of receiving treatment must be *greater* when assigned to the treatment group compared to the control group. Since the control group was not canvassed, this assumption is satisfied by design.

It should be stressed that 2SLS generates consistent estimates even though contact by canvassers is a function of unobservables (U_i^C) that may be correlated with U_i^Y.

The models above estimate the effect of the MoveOn campaign by applying 2SLS to pooled data from all blocks. These models treat the individual as the unit of analysis, but our quasi-experiment assigned sides of street-blocks to treatment or control conditions. In order to take account of the possibility that those living on the same side of a street-block share unobserved characteristics, we estimate robust cluster standard errors (Arceneaux 2005).

Only 13 out of 17 states where canvassing took place could be included in the analysis. Four states — Colorado, Maine, New Hampshire, and Washington — had no cases represented in the analysis because there were no blocks where: (1) MoveOn identified target voters in adjacent precincts; (2) MoveOn found a precinct leader in only one of the two precincts and therefore volunteers canvassed only one of the two precincts; and (3) the boundary between this pair of precincts runs down the middle of a street containing targeted voters on both sides of the street (as in Figure 1). Narrowing our focus to the set of cases subject to this natural experiment, we analyze the 23,384 individuals in the treated precincts and 22,893 in the adjacent untreated precincts. Nearly half of these cases are from Pennsylvania: 11,512 in the treatment group, and 11,182 in the control.

RESULTS

Our estimation results are presented in Table 3. Without covariates, 2SLS produces an estimated treatment effect of 8.7 percentage-points, with a standard error of 2.9. When we control for covariates, this estimate rises to 10.5, and the standard error declines to 2.7. This is the *complier average causal effect*, also known as the *treatment-on-treated* effect.[6] Substantively, the results suggest that the turnout rate among those encouraged to vote by MoveOn canvassers was approximately 9 percentage-points higher than it would have been in the absence of this voter mobilization effort.

Based on a reported 570,004 Kerry supporters contacted by MoveOn nationwide,[7] an 8.7 percentage point effect would correspond to 49,590 additional votes. A 10.5 percentage point effect corresponds to an estimated 60,420 additional votes.

Restricting our attention to Pennsylvania, we obtain very similar results. Table 4 reports that voter mobilization contacts increased turnout by 7.5 percentage points with a standard error of 3.4. Controlling for covariates, the estimate rises to 8.8 with a standard error of 3.0.[8] Based on a total of 120,978 contacts in Pennsylvania, a 7.5 percentage point effect corresponds to 9,073 additional votes due to MoveOn's efforts. For an 8.8 percentage point effect we would estimate an additional 10,646 votes in Pennsylvania.

DISCUSSION AND CONCLUSIONS

Our findings indicate that MoveOn's 2004 get-out-the-vote efforts raised turnout rates substantially among targeted voters. Contact with MoveOn canvassers increased voter

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6 The intent-to-treat estimates — which estimate the effect of assignment to treatment group rather than the effect of actual contact tell a similar story. Table A.1 shows the overall intent-to-treat effects. Without covariates the intent-to-treat effect is 1.4 percentage points with a standard error of 0.5. When we control for covariates the estimate rises to 1.7 percentage points with a standard error of 0.4. Dividing by the contact rate of 15.2% we arrive at approximate complier average causal effect estimates of 9.2 percentage points and 11.2 percentage points for without and with covariates specifications, respectively.

7 Note that this estimate is based on *all* MoveOn contacts nationwide, not only the contacts that occurred within the subset of the data that we analyze. It seems reasonable to generalize to the population of MoveOn targets based on estimates derived from the subset of across-the-street neighbors.

8 For Pennsylvania, we obtain similar intent-to-treat effects. Table A.2 shows the specification without covariates generates an estimate of 1.6 percentage points with a standard error of 0.7. With covariates the estimate increases to 1.9 with a standard error of 0.7. Dividing by the contact rate in Pennsylvania of 19.1% we achieve complier average causal effects of 8.4 percentage points and 9.9 percentage points for without and with covariates specifications, respectively.

Table 3. 2SLS estimates of complier average causal effect

	2SLS (Complier Average Causal Effect)												
	No Covariates		With Covariates										
Treatment	8.7 (2.9)		10.5 (2.7)										
Covariate Estimates	AZ	FL	IA	MI	MN	MO	NC	NM	NV	OH	OR	PA	WI
Election '02	8.4 (3.6)		33.5 (3.7)		13.4 (3.1)	16.9 (2.1)	26.8 (3.8)	29.7 (4.8)	21.3 (6.2)		11.6 (6.9)	23.6 (1.0)	
Election '00	4.6 (3.4)		20.1 (3.3)		20.1 (3.2)	6.1 (1.5)	17.0 (3.0)	8.5 (5.3)	22.6 (5.3)		0.8 (9.2)	13.0 (1.0)	
Election '98			1.6 (4.3)		7.6 (3.2)	−1.8 (2.2)	−5.5 (4.0)	−11.4 (7.8)			−28.6 (8.4)	−5.2 (1.6)	
Election '96						−1.6 (2.5)	−2.8 (4.4)	−9.2 (7.6)				5.4 (1.4)	
Primary '04	17.5 (3.1)					13.5 (2.4)						23.6 (1.1)	
Primary '02	6.0 (4.4)		3.6 (6.8)		3.9 (4.2)	−4.2 (2.1)	10.3 (3.9)					1.7 (1.1)	
Primary '00	−3.5 (5.7)		−4.6 (9.4)		10.0 (5.4)	−1.2 (2.5)	6.9 (5.2)					−5.1 (1.6)	
Primary '98					−4.2 (5.5)	0.2 (4.0)	−7.1 (7.0)					2.9 (2.5)	
Primary '96						0.2 (3.2)	6.9 (7.1)					−10.9 (2.7)	
Age	0.7 (0.5)	2.0 (0.2)		0.2 (0.3)		0.1 (0.2)	0.7 (0.4)	−0.5 (0.5)	0.3 (0.3)	0.1 (0.3)	1.5 (0.4)	0.3 (0.1)	−3.6 (0.5)
Age2	0.0 (0.0)	0.0 (0.0)		0.0 (0.0)		0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)
Democrat	2.6 (3.9)	4.7 (1.7)	12.5 (2.4)				−3.1 (8.2)	0.0 (4.4)	3.3 (4.2)		14.4 (4.3)	15.9 (1.2)	
Male	1.2 (8.6)	−3.8 (1.4)	11.6 (6.2)	−10.8 (7.9)	8.3 (5.2)	−0.1 (1.6)	−3.8 (2.5)	−4.0 (3.6)	−10.0 (3.0)	−7.9 (5.0)	−2.9 (5.2)	−3.0 (1.6)	2.6 (4.9)

Note: Robust clustered standard errors are presented in parentheses. Complier average causal effect refers to the average effect of contact with a canvasser who encourages the respondent to vote. All specifications include block-level fixed effects (not reported). We present the coefficients for the covariates for completeness but do not ascribe a causal interpretation to them.

Table 4. 2SLS estimates of complier average causal effect for Pennsylvania

	2SLS (Complier Average Causal Effect)	
	Without Covariates	With Covariates
Treatment	7.5 (3.4)	8.8 (3.0)
Election '02		23.7 (1.0)
Election '00		13.1 (0.9)
Election '98		−5.2 (1.5)
Election '96		−5.5 (1.4)
Primary '04		23.7 (1.0)
Primary '02		1.8 (1.1)
Primary '00		−5.2 (1.5)
Primary '98		2.9 (2.4)
Primary '96		−10.9 (2.6)
Age		0.3 (0.1)
Age^2		0.0 (0.0)
Democrat		15.9 (1.2)
Male		−3.0 (1.5)

Note: In parentheses are robust cluster standard errors, which account for clustering of individuals on treatment and control sides of a street. All regressions include fixed effects for each block, as shown in Equations (1–3), but the intercepts for each cluster are not listed here. Complier average causal effect refers to the average effect of contact with a canvasser who encourages the respondent to vote. We present covariates for completeness but do not ascribe a causal interpretation to their coefficients.

turnout by approximately nine percentage-points, a result that was robust across alternative model specifications and highly statistically significant. This pattern of results has several important substantive implications.

First, the strong effects found here run counter to the usual hypothesis that get-out-the-vote activities are less effective when conducted in high-salience elections. This hypothesis, which dates back to Kramer (1970), rests on the notion that canvassers have difficulty boosting turnout in electoral settings where a relatively small fraction of the electorate abstains. Prior experimental research has been unable to speak to this hypothesis because studies focused either on low-salience elections or uncompetitive enclaves in presidential elections.

The similarity of the estimated effect of MoveOn's canvassing campaign in battleground states to previous estimates obtained in less competitive contexts provides new clues about the reason why canvassing works. Given the extraordinary resources that campaigns pump into battleground states in presidential election years, it seems unlikely that canvassing works because it reminds people of the upcoming election. Rather, like other highly effective community-based canvassing campaigns (see Arceneaux 2005; Michelson 2005), this one appears to work because it conveys social norms about participation in the upcoming election as modeled by the behavior of a local volunteer. Communicating the same partisan or nonpartisan appeals by phone has consistently proven to be less influential than face-to-face communication (see experiments summarized in Green and Gerber 2004), arguably because the appearance of a canvasser is a stronger social cue.

Second, the findings suggest the political relevance and technical feasibility of a national voter mobilization campaign. Scholars in recent years have expressed increasing concern about diminished membership in civic and political organizations and corresponding declines in social capital (Putnam 2000). MoveOn came to life in an environment where members of civic organizations are relied upon less for the delivery of votes (Mayhew 1992) and more as a source of funding for lobbying efforts in Washington or paid advertising (Skocpol 2003; Fisher 2006). Using the internet to recruit and coordinate a vast network of local activists, MoveOn conducted a campaign that was simultaneously broad in geographic scope yet ideologically focused. MoveOn's capacity to put tens of thousands of unpaid activists on the streets shows that membership organizations still have low-cost opportunities to act as vote-brokers in the political system.

Finally, our results demonstrate the feasibility of quasi-experimental approaches to the assessment of campaign effects. In closely contested races, experimental research is often infeasible, because campaigns resist the extraction of random control groups. Researchers are therefore forced to come up with second-best approaches. This study demonstrates the scientific value of having extremely detailed information about targeting, because it enables the researcher to take advantage of discontinuities in the application of treatments. Our study seized upon geographic discontinuities, but depending on the targeting criteria, other designs may focus on discontinuities associated with voters' age or date of registration. The broader point is that campaigns' targeting criteria, far from creating an impenetrable endogeneity problem, present research opportunities that may significantly advance our understanding of elections.

APPENDIX

Phase 1 Canvass Script

Hi, how are you. My name is ______. Is _______[name] in?
[If the person you are seeking is not in, ask when they might be in, and then thank the person for their time]
[Otherwise continue] I live here in ______ [name of neighborhood or section of town] and I'm working on the upcoming election. Have you decided who you are going to vote for?

Table A.1. OLS estimates of intent-to-treat effect

	Linear Regression (Intent-to-treat effect)	
	No Covariates	With Covariates
Treatment	1.4 (0.5)	1.7 (0.4)

Covariate Estimates	AZ	FL	IA	MI	MN	MO	NC	NM	NV	OH	OR	PA	WI
Election '02	9.3 (3.6)		32.7 (3.5)		13.4 (3.0)	17.2 (2.0)	27.1 (3.7)	29.5 (4.6)	23.8 (5.8)		11.4 (6.6)	24.0 (1.0)	
Election '00	4.2 (3.3)		20.3 (3.2)		20.1 (3.1)	6.1 (1.4)	16.9 (2.9)	8.4 (5.0)	22.7 (5.1)		1.1 (8.8)	13.2 (0.9)	
Election '98			1.5 (4.1)		7.9 (3.1)	−1.8 (2.1)	−5.7 (3.8)	−11.9 (7.6)			−28.7 (8.0)	−5.1 (1.5)	
Election '96						−1.5 (2.4)	−2.6 (4.2)	−8.9 (7.4)				−5.8 (1.3)	
Primary '04	18.5 (3.1)					14.1 (2.3)						24.0 (1.0)	
Primary '02	5.2 (4.3)		3.9 (6.5)		3.0 (3.9)	−4.3 (2.0)	10.5 (3.8)					2.0 (1.1)	
Primary '00	−4.0 (5.4)		−4.6 (9.0)		10.8 (5.2)	−0.9 (2.3)	6.9 (5.0)					−5.3 (1.5)	
Primary '98			−0.5 (8.9)		−4.3 (5.3)	0.2 (3.9)	−7.0 (6.7)					2.9 (2.4)	
Primary '96						−0.2 (3.1)	6.3 (6.8)					−10.8 (2.6)	
Age	0.7 (0.4)	2.0 (0.2)		0.2 (0.3)		0.1 (0.2)	0.7 (0.4)	−0.5 (0.5)	0.2 (0.3)	0.1 (0.3)	1.5 (0.4)	0.3 (0.1)	−3.6 (0.5)
Age2	0.0 (0.0)	0.0 (0.0)		0.0 (0.0)		0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)	0.0 (0.0)
Democrat	3.1 (3.9)	4.8 (1.6)	12.9 (2.3)				−3.2 (7.8)	−0.2 (4.3)	3.8 (3.9)		14.7 (4.1)	16.2 (1.2)	
Male	0.7 (8.4)	−3.8 (1.4)	11.5 (6.0)	−10.7 (7.9)	9.5 (4.8)	−0.1 (1.5)	−3.8 (2.4)	−4.0 (3.4)	−10.1 (2.9)	−7.9 (4.8)	−2.8 (5.0)	−3.0 (1.5)	2.5 (4.7)

Note: Robust clustered standard errors are presented in parentheses.

Table A.2. OLS estimates of intent-to-treats for Pennsylvania

	Linear Regression (*intent-to-treat* effects)	
	Without Covariates	With Covariates
Treatment	1.6 (0.7)	1.9 (0.7)
Election '02		24.0 (1.0)
Election '00		13.2 (1.0)
Election '98		−5.1 (1.6)
Election '96		−5.8 (1.4)
Primary '04		24.0 (1.0)
Primary '02		2.0 (1.1)
Primary '00		−5.3 (1.5)
Primary '98		2.9 (2.5)
Primary '96		−10.8 (2.6)
Age		0.3 (0.1)
Age^2		0.0 (0.0)
Democrat		16.2 (1.2)
Male		−3.0 (1.5)

Note: In parentheses are robust cluster standard errors, which account for cluastering of individuals on treatment and control sides of a street. All regressions include fixed effects for each block, as shown in Equations (1–3), but intercepts for each cluster are not listed here. Intent-to-treat effect refers to the average effect associated with assignment to the treatment group, ignoring actual contact. We include covariates for completeness but do not ascribe a causal interpretation to their coefficients.

[If Kerry]: Great! I'm a volunteer with MoveOn PAC and we're working to defeat George Bush and elect John Kerry. For our get-out-the-vote efforts, we're compiling a list of Kerry supporters, can I get a contact phone # and e mail address ______ [name]? (turn over clipboard)

[If Yes]: Great, thanks.
[If No]: Ok, just one more question. What issue is most important to you in making your decision for president?

OK, thank you for your time _______ [name], and thank you for your support. Be sure to vote on November 2 — this race is incredibly close, but if we all get out and vote, we can definitely defeat George Bush. Here's a fact sheet from MoveOn PAC about this election. And if you want to volunteer to help us out here in our neighborhood, we'd

Table A.3. Probit estimates of intent-to-treat effects

	Probit Regression (Intent-to-treat effect)												
	No Covariates		With Covariates										
Treatment	0.044 (0.014)		0.058 (0.015)										
Covariate Estimates	AZ	FL	IA	MI	MN	MO	NC	NM	NV	OH	OR	PA	WI
Election '02	0.405		1.114		0.599	0.901	1.087	0.988	0.676		0.393	0.891	
	(0.172)		(0.149)		(0.123)	(0.084)	(0.174)	(0.200)	(0.210)		(0.221)	(0.040)	
Election '00	0.205		0.610		1.071	0.352	0.605	0.256	0.700		0.066	0.435	
	(0.145)		(0.114)		(0.177)	(0.070)	(0.119)	(0.163)	(0.156)		(0.283)	(0.032)	
Election '98			0.125		0.544	−0.140	−0.307	−0.372			−0.942	−0.194	
			(0.162)		(0.188)	(0.106)	(0.165)	(0.233)			(0.263)	(0.065)	
Election '96						0.012	−0.162	−0.249				−0.207	
						(0.110)	(0.151)	(0.254)				(0.043)	
Primary '04	1.014					0.878						1.157	
	(0.191)					(0.123)						(0.063)	
Primary '02	0.827		0.130		0.225	−0.076	0.775					0.143	
	(0.319)		(0.162)		(0.302)	(0.150)	(0.252)					(0.050)	
Primary '00	0.095		−0.030		1.028	−0.138	0.419					−0.152	
	(0.403)		(0.342)		(0.464)	(0.134)	(0.267)					(0.084)	
Primary '98			0.064		−0.249	−0.054	−0.403					0.123	
			(0.352)		(0.360)	(0.190)	(0.302)					(0.106)	
Primary '96						−0.002	0.197					0.426	
						(0.177)	(0.372)					(0.097)	
Age	0.022	0.062		0.006		0.013	0.031	−0.016	0.007	0.003	0.054	0.009	−0.150
	(0.019)	(0.006)		(0.009)		(0.008)	(0.015)	(0.015)	(0.156)	(0.010)	(0.017)	(0.003)	(0.027)
Age2	−0.000	−0.000		−0.000		−0.000	−0.000	0.000	0.000	0.000	−0.000	−0.000	0.002
	(0.000)	(0.000)		(0.000)		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democrat	0.103	0.147	0.434				−0.135	−0.056	0.127		0.445	0.505	
	(0.150)	(0.050)	(0.080)				(0.266)	(0.132)	(0.118)		(0.128)	(0.031)	
Male	0.012	−0.120	0.303	−0.367	0.328	0.065	−0.168	−0.130	−0.327	−0.260	−0.103	−0.119	0.071
	(0.271)	(0.042)	(0.180)	(0.269)	(0.203)	(0.109)	(0.095)	(0.105)	(0.090)	(0.166)	(0.162)	(0.050)	(0.195)

Note: Clustered standard errors are presented below the estimates in parentheses. Covariates are presented in matrix form below the treatment estimates due to the sheer number. Across the top of the matrix the states are listed. Along the side the covariate is listed. Intent-to-treat refers to the average effect of being assigned to the treatment group. We include covariates for completeness but do not ascribe a causal interpretation to their coefficients.

love to have you come to our next meeting and get involved — just give me a call or go to our website, which is on the fact sheet.
[If Undecided] OK, which of these best describes your position? (Show ID options.) OK, and which issue will be most important to you in making your decision for president?
Ok, thank you for your time _______ [name]. I'm a volunteer with MoveOn PAC and we believe George Bush has taken the country in the wrong direction on Iraq, the economy, and other issues, so we're working to elect John Kerry. I'd like to leave you with this fact sheet, and I hope you'll decide to support John Kerry on November 2nd.
[If Nader] Would you say you're leaning toward Nader or definitely going to vote for Nader?
[If Leaning Nader] Ok, thank you for your time _______ [name]. I'm a volunteer with MoveOn PAC and we believe George Bush has taken the country in the wrong direction on Iraq, the economy, and other issues, and the best way to defeat Bush is to vote for John Kerry. I'd like to leave you with this fact sheet, and I hope you'll consider supporting John Kerry on November 2nd.
[If Strong Nader] Ok, thanks for participating in our survey, and have a nice day.
[If Bush] OK, thanks for participating in our survey, and have a nice day.

Phase 2 Canvass Script — For Voters Identified as Kerry Voters in Phase 1

Hi, how are you. My name is _____. Is ______ [name] in?
I am here with MoveOn PAC and I want to make sure you are all set to vote to defeat George Bush Tuesday. The race is really close, but we have an upsurge of momentum and we're all going to get out and vote and put our country on a new and better course.
What time are you planning to vote? [Write down time]
Do you need a ride to the polls [*or any other assistance to allow you to vote*]? I can send someone over to pick you up. [Mark that they need a ride, and if they need other help]
Here's a fact sheet from MoveOn PAC about this election and directions to our polling place. Please share it with like-minded friends and make sure they get out to vote! [if you don't have these new materials, just use your existing ones and you'll get the new ones in by the weekend] Can you volunteer to help us out here in our neighborhood? We'd love to have your help even for 2 hours.

> [If Yes] Great! Here's a sign up sheet, you can pick any day and time and give 2 hours; it's fun. Thanks for your time _______ [name], and thank you for your support. When you go vote, will you check in with one of our volunteers at the polls so we know you've voted? Look for the volunteer in the red hat and with the red and silver MoveOn armband. See you on Tuesday!

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