

Strangers vs Neighbors: The Efficacy of Grassroots Voter Mobilization

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1 Abstract

Research on social capital and social networks suggests that collective activities have the potential to influence individual decisions to turnout to vote. This paper offers empirical evidence on the effectiveness of grassroots campaigning, focusing on the relationship between neighborhood-level social contacts and voter turnout. We analyze data from a get-out-the-vote (GOTV) campaign conducted by a local community outreach group during the 2006 General Elections. The campaign targeted low income and minority registrants in the South Los Angeles area and encouraged a random sample of them to vote via face-to-face canvassing. Within this experimental setting we are able generate a continuous measure of grassroots campaigning, defined as the share of contacts made by canvassers working in their own neighborhood. Our analysis focuses on both the effect of face-to-face campaign contact and the marginal effect of face-to-face neighborhood canvassing on voter turnout. While the base campaign increased turnout by six percentage points, similar to the power of previous door-to-door campaigns, we find striking results for the marginal effect of neighborhood-level social contacts on turnout. We estimate that the campaign increased turnout by an additional 3-4 percentage points in precincts where canvassers were working in their own neighborhood. Our results imply that increasing the share of contacts made by canvassers in their own neighborhood by 10 percentage points would increase turnout by an additional 3 percentage points. The implications of these results for marginal communities suggest that communities which have been historically disenfranchised can re-enfranchise with mobilization campaigns.

2 Introduction

Understanding what mobilizes individuals to vote has both practical and theoretical implications. On the practical side, political campaigns and political consultants continually work to find the most effective and inexpensive ways to turn out voters for their party or candidate. This need has created a multi-billion dollar industry of phonebanks, media consultants, mail houses, professional canvassing operations, and other services meant to address the needs of campaigns to identify and turn out their supporters on Election Day. Yet, the effectiveness of the services that these organizations are providing has rarely been subject to rigorous empirical tests. The consultants make claims and candidates believe those claims based on their previous electoral successes or anecdotal evidence of their effectiveness. Thus, this practical world of electoral campaigning has, by and large, not existed in conversation with the vast scholarly literature on voting and participation that has evolved over the past few decades in political science. Using a variety of methodological tools, this body of research has focused on issues such as socioeconomic status and civic resources, the importance of direct mobilization, and social context in order to explain what it is that motivates individuals to act politically (Campbell et al. 1960, Verba and Nie 1972, Verba, Schlozman and Brady 1995, Rosenstone and Hansen 1993, Leighley 2001, Uhlaner, Cain and Kiewiet 1989, Kenny 1992). At the heart of this debate is a tension between individual and contextual-level explanations. Much of the political behavior literature, building on the psycho-social model of participation that came out of *The American Voter*, has looked at individual-level characteristics, such as socioeconomic and civic resources, to explain differences in electoral turnout. Yet, even though this literature established that individuals with higher socioeconomic status are more likely to vote, this work cannot explain why, even though over the past three decades Americans have become more educated and wealthier, participation rates have gone down across the board. Similarly, rational choice theory, with its emphasis on individual-level calculations of self-interest and utility, has been unable to explain turnout in mass elections (Green and Shapiro 1994).

Stepping into this breach was Robert Putnam, with his highly influential book *Bowling Alone*. Putnam (2000) argued that it was the decline of community-based social capital, which he defines broadly as organizationally-based activity, that explains the decline of civic engagement in the United States. Putnam's approach is similar to that of Huckfeldt (1979: 579) who argues:

Political activity seldom occurs in individual isolation; as a result, the social context is an important determinant of the extent to which individuals participate in politics. Individual characteristics, attributes, and personality factors do not entirely determine the extent of individual political activity. People also respond to political events, cues, and opportunities which are specific to a given environment. External social factors, as well as individually intrinsic factors, provide powerful explanations for political participation.

Scholars also have emphasized the influence that political discussion within individuals' social networks has on their participation choices. Knoke (1990) finds that individuals with politicized social networks are more likely to engage politically. McClurg (2003: 450) finds the information provided by those networks to also be key:

Discussions with friends who are interested or active in politics can help people learn about the reasons for participating while reinforcing the idea that such behavior is desirable among ones peers. People also may be exposed to information about the mechanics of electoral politics and involvement.

Similarly, scholars have documented the correlation between individuals who are political active and the existence of relationships with other individuals who are politically active (Bolton 1972; Briet, Klandermans and Kroon 1987; Gerlach and Hine 1970; McAdam and Paulsen 1993; McAdam 1986) This work suggests that there are important collective activities, within social networks and local communities, which influence an individual's subsequent decision to turn out and vote.

Yet, despite the theoretical and practical importance of voters' community contacts, relatively little is written in the political science literature about neighborhood effects on voter turnout. Carlson (1999) finds that civic participation in communities is a strong predictor of voter turnout. In experimental studies in the laboratory, Großer and Schram (2006) have shown that providing information about turnout decisions of others significantly increases turnout while Schram and van Winden (1991) have shown that social pressure influences the decision to vote. Individuals are unlikely to make voting decisions in isolation at a minimum their turnout choices are likely to be observed by their immediate household, and potentially by others in their community. Individuals may also use information from their surroundings to decide whether or not to cast a vote (Lassen 2005). In particular, it may be that individuals are most likely to listen to election information from individuals whom they believe have their best interests at heart or have similar interests themselves (Downs 1957). The social interactions at work behind the turnout decision are therefore an essential component of theoretical and empirical work on voter turnout. If neighborhood effects are important for determining voter participation, grassroots political campaigns should be able to leverage neighborhood social contacts in order to increase turnout.

Not only is empirical evidence on neighborhood social networks relevant for developing a more complete theoretical model of voter turnout, it also has practical strategic importance for political campaigns. Statements made by political consultants and pundits in the wake of the 2004 Bush-Kerry presidential contest suggest that the extent to which the parties relied on local contacts may have affected their success in the election. The Bush-Cheney campaign strategy focused on encouraging volunteers to involve their friends and neighbors through each individual's "sphere of influence" (Bergan et al. 2004). On the other hand, the Kerry-Gore campaign relied upon paid professionals and imported volunteers (Fisher 2006). While the Kerry-Gore campaign organized higher numbers of individuals to help in their get-out-the-vote efforts, the Bush-Cheney campaign appeared to be more successful in turning out their base. According to Laurie

Moskowitz, who worked on the grassroots mobilization of progressive Americans during the 2004 campaign for the DNC, “The Republicans built a system that was based on personal connections over time...[they] had the time and energy invested in it, and the resources...[within the GOTV time frame] you had your ten people...based on that personal connection. At the end of the day, we just were trying to make contacts” (Fisher 2006).

The randomized experiment presented in this study provides a rigorous test of the effects of personal connections on turnout behavior. We empirically demonstrate that introducing individuals from a registrant’s “sphere of influence” into the campaign in this case from their zip code increases turnout. Previous field experiments have made great strides in recent years in bringing together the practical and theoretical sides of voter turnout by testing established methods and developing a list of “best practices” for mobilizing low-propensity voters (Druckman et al. 2006, Green and Gerber 2004). Dozens of experiments have shown that door-to-door canvassing is the most powerful method of turning out voters (Gerber and Green 2000, 2001; Green, Gerber and Nickerson 2003), phone calls from volunteer phone banks can also significantly increase turnout, and mailers, robocalls, and other impersonal methods tend to be ineffective. Experiments also suggest that the quality of a canvassing or phonebanking campaign (e.g. the sincerity and commitment of those who make contact with voters) is crucial to its success. Most of these experimental studies have been conducted by academics working with student volunteers (e.g. Matland and Murray 2005, Michelson 2003, 2005, 2006a, Trivedi 2005, Wong 2005). However, as knowledge of the scientific and practical benefits of field experimentation has spread, an increasing number of community organizations have collaborated with academics in designing their experiments, allowing researchers to examine the effectiveness of these “real world” community-based efforts (Green, Gerber, and Nickerson 2003; Green and Michelson 2007, Ramírez 2005, Michelson 2006b; Nickerson 2006). This work is the product of that kind of collaboration. As a result, we were able to construct an experiment that was implemented by a long-standing community organization in South Los Angeles. The organization itself is multi-racial and has a strong volunteer base in the

community. We are thus able to extend the existing research to examine the effects of mobilization using canvassers who are most likely to fit into the Downsian “trusted source” mold.

The individuals in this study are new or occasional voters (defined as having participated in fewer than four of the last ten general elections) who are all residents of a low-income neighborhood in Los Angeles. The majority of the individuals in this study are voters who historically have had a more difficult time gaining access to the ballot. According to the 2000 U.S. Census, the area where this organization was organizing is low income with a the median family income of \$29,718 and about 30 percent of the population living below the poverty line and racially mixed: the area is 55.9 percent Latino and 38.9 percent African-American. Of the occupied housing units in the area, 63.4 percent were renter-occupied. Additionally, 50.9 percent of the residents had less than a high school education and unemployment for most of the area hovers close to 11 percent.

It is unclear the extent to which the findings of previous studies apply to the activity and engagement of marginal communities. Although the literature on African American, Latino, and Asian American participation has grown significantly over the past decade, much remains unclear (Tate 1993, Dawson 1994, Leighley and Vedlitz 1999, Lien, Conway and Wong 2004, Fraga, García, Hero, Jones-Correa, Martínez-Ebers, and Segura 2006). Is the relationship between the individual and the collective different for members of these groups? And, how does that relationship affect these groups’ opportunity structure for engagement, and the effectiveness of particular mobilization tactics? Putnam’s work speaks to the first question. Although there have been a number of critiques of Putnam’s treatment of race in his analysis (Portes 1998, Hero 2003, Jennings and Withorn 2007), Putnam’s general approach has some affinity with works that look at the importance of “linked fate” on political engagement in marginal communities (Dawson 1994, 2001; Tate 1993, García Bedolla 2005). Building on social identity theory, this theoretical construct attempts to link the individual with the collective and discuss how group identities and attachments affect political attitudes and engagement within marginal communities (Tajfel

and Turner 1986). But, the bulk of this work has looked only at how issues of linked fate affect members of the same racial group and little of this work has looked directly at how these questions affect actual voter mobilization on the ground. Under what circumstances is the relevant community of interest the neighborhood, and to what extent does that relationship mediate individual-level choices to participate? In other words, what kinds of appeals are likely to motivate a voter? In order to answer these questions, we designed a unique field experiment. First, building on the literature on political mobilization in racial/ethnic communities, we tested the effectiveness of a multi-racial door-to-door canvassing campaign that was conducted within a multi-racial low-income community and which targeted low-propensity voters. Second, based on the social capital literature and findings about the importance of social networks in the politicization of individuals, we hypothesized that canvassers from the local neighborhood would be more effective at mobilizing voters than those coming from the outside. Thus, we directly test the proposition of whether or not it matters who it is that delivers the mobilization message. We find that, as expected, door-to-door canvassing is a very effective way to turn out low propensity voters, even when they reside in low-income multi-racial communities. In addition, the impact of that mobilization is significantly greater when the canvassers come from the voter's neighborhood, supporting the proposition that peers and social networks play an important role in individual political behavior.

3 The Campaign

The voter mobilization effort in this study was conducted by SCOPE (Strategic Concepts in Organizing and Policy Education), a grassroots political organization located in South Los Angeles. SCOPE has worked since 1993 to reduce structural barriers to social and economic opportunities for poor and working class communities and for recent years to increase civic engagement and voter turnout in disadvantaged communities. The November 2006 get-out-the-vote campaign was dedicated to mobilizing only new and occasional voters (defined as those voters who had participated in fewer than four of the last ten gen-

eral elections). SCOPE has spent years recruiting volunteers from the South Los Angeles area who are committed to canvass precincts prior to an election, and walkers are often residents of the areas slated for campaigning.

SCOPE's get-out-the-vote campaign began on Saturday, October 7th, 2006 and organized volunteers to walk in targeted precincts each Saturday before the election as well as the Sunday, Monday and Tuesday immediately prior to the election (November 7, 2006).¹ A paid team (many of whom were recruited from the volunteer population) walked precincts to contact potential voters 6 days each week (never on Sundays). This process continued until the election.

Individuals in this experiment were registered voters, classified as new or occasional, who were randomly assigned to either be contacted by a canvasser or to receive no contact. Two different categories of canvassers were used in the experiment. The first type of canvasser was likely to live in South Los Angeles and was likely to match the subject on racial and language characteristics. The second type of canvasser was an identical individual but who was also a resident of the same zip code as the subject. Each type of canvasser went to individuals' homes and spoke for a few minutes about the upcoming election and the importance of issues on the ballot to "our neighborhoods" using the same script. The randomization process was repeated during the last week of the campaign, with some individuals then randomly assigned to receive a second treatment.² Voter turnout was directly observed in this election from data provided by the Los Angeles County Registrar-Recorder's office so that we could determine the effect of canvasser contact as well as to evaluate the effect of neighborhood canvassers.

The method by which walkers were assigned to precincts was not random. However, our analysis finds no observable differences between the characteristics of the precincts

¹Approximately 20 volunteers arrived each Saturday, participated in a brief training session, and spent approximately 2 to 3 hours canvassing in the local neighborhoods. They then returned for lunch and were debriefed.

²Analysis of the marginal effect of the second contact yields no statistically significant effect at traditional levels and thus all analyses which follow will simply consider the effect of any contact. The marginal second contact results are available from the authors on request.

which were canvassed by home-turf walkers versus the precincts which were not. The method by which canvassers were assigned to precincts was effectively haphazard upon arrival to SCOPE – at the beginning of each session, the staff would ask all canvassers to sign in with their addresses and to indicate whether or not they had a car. SCOPE had selected an order for precincts to be walked which appeared to be based loosely upon the precinct number. Canvassers were assigned simply to the next precinct folder from a box of precinct folders which had not yet been canvassed. The only variation was that canvassers were occasionally assigned based upon proximity to the organization’s office headquarters if they did not have access to a car. Thus an if an individual arrived and their home precinct had not yet been canvassed, there was no systematic mechanism to even assign them to walk in their own precinct. Since the walkers were assigned haphazardly and were not assigned based upon any precinct characteristics, the lack of random assignment in assigning walkers to precincts should present no difficulties in identifying the home-turf canvassing effect. We compare the differences in means of all observable characteristics between the precincts which were canvassed by home-turf walkers and those which were not. These results are presented in Table 1.

Table 1 Goes Here

We find no statistically significant differences, which supports our belief that the method of assignment for precinct walkers does not generate our results; we are certain that the home-turf precincts are not different in terms of their vote histories, for example, or other observable characteristics. Additionally in the our analysis below, we account for the fixed-effect of a particular canvasser (it might be possible, for example, that the canvassers drawn from the home-turf precincts were somehow better canvassers) and find the results are robust to this specification.

4 Experimental Setup and Data

Among the new and occasional registered voters in 50 precincts in South Los Angeles, we randomly assign new and occasional registered voters to control and treatment groups. The randomization was conducted by household, and households with more than 3 individuals were removed from the experiment. In our initial randomization, we assigned 3,578 individuals to the control group and 11,789 individuals to the treatment group. Thus we drew a control group consisting of approximately 20 percent of the eligible voters in our sample. The individuals in the control group were not assigned to be contacted in the experiment. Each individual assigned to the treatment group was targeted for a visit from a canvasser who may or may not have been a resident of their zip code area. Additionally, in the final week before the election SCOPE opted to re-contact some their earlier contacts. We randomized into new treatment and control groups the voters that they had determined would be advantageous to re-contact (those who they were able to successfully contact in the first wave), removing 360 individuals for the control group. These assignments are described in Table 2.³

Table 2 Goes Here

Based upon records kept by the canvassers, we are able to discern which of the individuals in the sample received either one or two contacts. The contacts are tabulated by the random assignments. Of the individuals assigned to receive the first contact, 3,907 were successfully contacted. Two individuals were inadvertently contacted from the control group. Of the individuals assigned to receive the second contact, 548 were successfully contacted, and 37 individuals were inadvertently contacted. Additionally, canvassers placed doorhangers on the doors of many of the individuals in this sample. The placement of doorhangers (where one doorhanger is placed for each household) is

³The data provided to us indicated contacts which SCOPE had scanned into their contact dataset using a bar scanner. However, our duplicate efforts discovered a total of 942 discrepancies. We include data for all individuals for whom there was not a worksheet or a walker code (for a total of 399) but when otherwise our scanned data disagrees with the data, we use ours instead.

documented as well. No individuals in the control group for the early campaign received a doorhanger, so this control group remains a pristine base-rate comparison category.⁴ Finally, it is possible to identify which of these individuals voted in the November 2006 election using turnout data provided by the Los Angeles Registrar Recorder.

Canvassers indicated their contacts by signing their initials. Consequently, it is possible to identify which canvasser contacted each individual in our sample. Each canvasser provided their street number and address on a sign-in sheet at the beginning of each GOTV walk session. Using these addresses, we locate each canvasser within a zip-code. Zipcodes serve as proxies for neighborhoods; the postal service designates zipcodes based upon geographically compact areas for mail delivery and zipcodes do not change each election cycle, unlike precinct boundaries, and thus may more closely resemble the geographic boundaries of a neighborhood. We then classify each individual who was contacted by a canvasser who shares their zipcode as contacted by a "home-turf" canvasser. We use this measurement to discern whether or not an individual was contacted by someone who lives in their neighborhood hereafter referred to as "home-turf canvassing".

The principle advantage of the randomization is to ensure that in fact the mobilization campaign is the cause of turnout.⁵ If the mobilization efforts were not randomized, several problems could emerge with the data. The ability to successfully administer treatment could be correlated with the probability that an individual turns out to vote (for example, being home during the day might correlate with age which may cause a spurious result since older voters are more likely to turn out to vote (Wolfinger and Rosenstone 1980) or worse, the organization could select voters to contact whom they believe are likely voters. We avoid both problems by randomly assigning treatment and control

⁴The existing literature on the effectiveness of doorhangers suggests that they have an extremely small effect on turnout (Nickerson 2005 and Nickerson, Friedrichs, and King 2005).

⁵Randomization also ensures that the covariates that are observed are almost balanced across treatment and control. For the first-wave of treatment assignments, only other-party-registration is observed to have any statistically significant difference between treatment and control, and for the second-wave of treatment assignments, only decline-to-state registration is observed to have any statistically significant difference between treatment and control.

groups.

5 Methods and Results

We begin by estimating the intent-to-treat effect (ITT) and the treatment-on-treated effect (TOT). The intent-to-treat effect is defined as the observed difference in turnout between those assigned to the treatment and control groups. If the contact rate is 100%, the intent-to-treat effect is identical to the treatment effect. Generally this is not the case, however, and to calculate the treatment effect we must adjust for the contact rate. The actual treatment effect is defined as the intent-to-treat effect divided by the contact rate. Treatment is defined as any campaign contact.⁶

Formally, let Z be an indicator which defines whether or not the individual is assigned to the treatment or control. Let x denote whether the individual actually receives the treatment. Let y indicate whether or not the individual voted. Then the intent-to-treat effect is defined as: $ITT = E(y|Z = 1) - E(y|Z = 0)$. Following from this, then $TOT = \frac{ITT}{E(x|Z = 1) - E(x|Z = 0)}$. Note again that the difference between the intent-to-treat and the actual-treatment-effect is that the TOT accounts for the contact rate. We can estimate TOT by regressing y on x and using Z as an instrument.

These results are presented in Table 3. Here we observe that of the 11,789 individuals who are eligible for treatment, only 5,341 are actually treated a contact rate of 45%. The canvassers did not attempt to contact every household in the treatment list, but many of the households which they did attempt to contact were not home or were unwilling to open their doors. Canvassers reported being unable to contact individuals for a variety of reasons, ranging from that household having a mean dog in the front yard to being unable to convince the individual to open their front door.

⁶The marginal benefit of a second contact is not statistically significant and thus the effect of any contact is most appropriate in analyzing the effect of the campaign. These results are available from the authors on request. We are unable to distinguish the effect of the doorhangers from the face-to-face canvassing as these were not randomly assigned; yet Nickerson (2005) finds that doorhangers increase turnout 1.2% and we observe significantly higher turnout effects, thus we have confidence that the face-to-face canvassing plays an instrumental role in increasing turnout. All additional results will simply evaluate the effect of any campaign contact, however.

One of the reasons it is essential to use the randomization assignments in the results is that there may be systematic differences between those individuals who are able to receive the mobilization "treatment" and those who are not. Given that we have so few covariates in the voter file, we are thus quite reliant upon the use of randomized experimentation. Looking at only the raw differences in the percent voting among treatment and control, it is clear that there is in fact a significant difference between the voting rates, 36.7 percent for individuals in the treatment group and 33.7 percent for individuals in the control group. The difference between these two numbers is the intent-to-treat effect. The observed difference between these two numbers is not, however, a precise measurement of the efficacy of the treatment as it fails to incorporate the contact rate for each of these groups if the contact rate is less than 100%, then this underestimates the actual treatment effect. Thus, controlling for the contact rate provides an estimated actual treatment effect of 6.6 percentage points. We estimate the standard errors for these estimates assuming that the errors will cluster by household (and thus produce robust cluster standard errors) both the ITT and the TOT are statistically significant at traditional levels.

We calculate the TOT using a two-stage-least-squares estimate (2SLS), where we want to estimate the effect of a canvass contact on whether or not an individual votes in the election where the application of the contact treatment may be endogenous. In order to implement this analysis we construct an instrumental variable for the contact using the randomization assignment this process assures us that the assignment is statistically independent of the observables and unobservables. Our instrument here is fairly good the correlation between the treatment assignment and any contact is .32. Note that we are assured that there is no correlation between the implementation of the campaign and the error terms, as the assignment was completed randomly prior to conducting the experiment.

Table 3 Goes Here

In the dataset we observe partisan registration, age, vote history, a surname classification for ethnicity, and gender. We calculate effects incorporating the covariates as well

as without them so as to ensure that the covariates are not themselves driving the results due to small variations in the observed covariates in the random assignment. Again in this context we evaluate the extent to which the door-to-door contact was able to successfully mobilize a particular individual. Additionally, so as to adjust for the potential of precinct-level variation, we run a different specification which incorporates fixed effects for each precinct. Our results are displayed in Table 4. There is a statistically significant and positive effect of receiving any contact across all methods of estimation whether we include the control variables or incorporate precinct fixed effects.⁷

Table 4 Goes Here

Our next analysis examines the relationship between the intensity of home-turf efforts within precincts and the success of the campaign. We analyze the effect of home turf canvassing in two ways. First, we consider treatment effects separately by precinct and determine whether the size of the treatment effect is related to the intensity of the home turf effort. Then we consider the marginal effect of a home-turf contact for the subset of voters who were successfully contacted.

An analysis of how differences in the success rates of precinct level campaigns is related to the intensity of home-turf effort relies on one crucial assumption. We must assume that these precincts do not differ in terms of unobservables that affect voting rates. We attempt to measure the effect of the intensity of home-turf efforts in precincts in a variety of ways that we hope can accommodate a variety of forms of heterogeneity across precincts.

If we consider each precinct separately, there are two possible ways in which home-turf canvassing could affect the success of the campaign. The first is that home-turf canvassers may be more likely to make contact with voters, whether because people are more likely to answer the door for someone who lives in the neighborhood or because

⁷We include indicators for whether or not the age or female control variables are missing and then fill in values for age and female to match those missing spaces. This allows us to directly compare the estimates of any contact across all models.

they make a larger effort when working in their own neighborhood. The second is that the same number of contacts would be made, but each contacts could be more persuasive and have a larger impact on the probability of voting. In order to examine the relationship between home-turf intensity and contact rates, we analyze the relationship between home-turf effort and the share of successful contacts contacts to show that home turf intensity does not affect turnout vis a vis more successful contacts.

Figure 1 shows the relationship between precinct level contact rates and the intensity of home-turf effort: each green dot represents a precinct's contact rate, with the yellow bars representing the 95% confidence interval for the precinct contact rate. The x-axis is the fraction of contacts within that precinct which were made by a home-turf canvasser. The blue line (the measurement of the contact rate given that amount of home-turf canvassing) is fit through the precinct estimates (the green dots), with the grey shading demonstrating the 95% confidence interval for the fitted line. The blue line is essentially flat; it slopes downward very slightly but is effectively pulled downwards by the one lone estimate with over 60% home-turf canvassing. We conclude that home-turf canvassers do not have higher contact rates.

Figure 1 Goes Here

If there is no relationship between contact rate and intensity of home-turf effort, home-turf canvassing would be expected to affect the effectiveness of each contact. We next examine the relationship between precinct level ITT and TOT estimates and the intensity of home-turf canvassing. Again the green dots demonstrate the precinct-level estimates of the home-turf treatment, with the yellow bars representing the 95% confidence interval for each precinct estimate. The x-axis demonstrates the intensity of the home-turf effort the fraction of individuals in that precinct who were contacted by a home-turf canvasser. The blue line (the measurement of the amount of turnout given that fraction of home-turf canvassing) is fit through the precinct estimates (the green dots), with the grey shading demonstrating the 95% confidence interval for the fitted line.

Figure 2 and Figure 3 Go Here

These figures demonstrate that as the intensity of the home-turf effort increases, the effectiveness of the campaign increases significantly. In fact, they suggest that a campaign that consists of only contacts by strangers would have little effect, while the effect of increasing the intensity of home-turf contact is significant. This provides powerful support for the hypothesis that home-turf canvassing is more effective at increasing turnout.

Our next sets of tables provide additional support for Figures 2 and 3. Table 5 breaks the data into two subsets the precincts that had home-turf canvassing and those had no home-turf canvassing and repeats the 2SLS estimates across the two models (both with and without the inclusion of control variables). The estimated treatment for the subset of home-turf precincts is positive and statistically significant in both models. However, the estimated treatment effect is smaller in the precinct where no home-turf contact occurred and is not different from zero when covariates are included. These regressions provide additional evidence that in fact the success of the campaign we observe is due primarily to the success of home-turf canvassing specifically and not face-to-face contact in general.

Table 5 Goes Here

We next explore the possibility that the home-turf effect is driven by certain particularly-effective walkers. Walkers could have made both home-turf and non home-turf contacts on different days of canvassing. Therefore we estimate another set of models, the first two models estimate the marginal effect of home turf contact on the subset of voters successfully contacted by the campaign. Another two models run on the subset of individuals successfully contacted by the campaign incorporate fixed-effects for walkers. These results are presented in Table 6. Across all models there is a positive and statistically significant effect of home-turf contact, regardless of the inclusion of walker fixed-effects or control variables. Controlling for walker fixed-effects increases the impact of the home-turf contact dramatically.⁸

⁸The results presented here are OLS coefficients. Probit coefficients produced similar substantive results,

Table 6 Goes Here

Finally we calculate a meta-analysis of precinct level 2SLS estimates in order to evaluate the effectiveness of the campaign and the effect of home-turf canvassing in Tables 5 and 6. Meta-analyses combine the results from different analyses on different groups. Here we consider precinct level campaigns that differ based on the intensity of their home-turf effort and attempt to determine the overall effect of home-turf canvassing was on turnout. When combining these precincts into a single analysis the meta-analysis, we incorporate each precincts' results without the assumption of homoskedasticity. The assumption of homoskedasticity is a concern since the implemented campaign resulted in different contact rates across precincts. With different contact rates by precinct, we must be careful in combining the effects of each precinct; if we were simply to average the precinct effects, we would fail to incorporate the variance of precinct size. On the other hand, if we were simply to look at the overall treatment effect, then we fail to acknowledge that the campaign was conducted on a precinct-by-precinct basis. The meta-analysis permits us to combine the effects while appropriately weighting each precinct by a measurement of its size; the meta-analysis incorporates the standard error of each individual precinct-level estimate. Thus the "average" treatment effect is in essence a weighted average.

Our model here examines the standardized mean difference between the treatment and control effects; we estimate the treatment effect, our quantity of interest, by incorporating the effect of each precinct and weighting that value by an estimate of the population variance. By pooling estimates and measurements of variance we will obtain the most precise estimate of the population variance (Hedges and Olkin 1985).

Formally, if Y_i^T is the treated turnout in precinct i , and Y_i^C is the control turnout in precinct i , then the treatment effect is $\beta_i = Y_i^T - Y_i^C$.⁹ We consider this effect on N precincts. Our quantity of interest is the weighted average of this difference: $\beta_{META} =$

but are not presented here simply for ease of interpretation when reading the table. The probit coefficients are available from the authors on request.

⁹If the contact rate is less than 100%, we would need to divide this quantity by the contact rate.

$\frac{\sum_{i=1}^N \beta_i * w_i}{\sum_{i=1}^N w_i}$ where $w_i = \frac{1}{v_i}$ where v_i is the variance of β_i . This assumes that the treatment effect is the same across units, but that treated individuals are also compared with others from their same precinct. It also ensures that larger precincts, or more precise estimates, are given more weight. As N , the number of precincts, becomes large, the estimate of β_{META} approaches normality; thus we can calculate confidence intervals (Hedges 1985).

We calculate a Q-statistic which measures the amount of heterogeneity across precincts. Formally, $Q = \sum_{i=1}^N \frac{\beta_i - \beta_{META}}{w_i}$. If all N precincts have the same population effect size, then the test statistic Q has an asymptotic chi-square distribution with $N - 1$ degrees of freedom; thus it is possible to obtain critical values to examine reject the hypothesis that each β_i is identical. This implies that there is more variation across precincts than would be expected from a sample which was drawn randomly.

We perform this analysis both on the 2SLS estimates of any contact and then we conduct a meta-regression that describes the fraction of contacts within the precinct that were made by home-turf canvassers. Meta-regression extends the above analysis by permitting us to incorporate a covariate which varies across precincts. Here we estimate the effect of the share of contacts which are home-turf by precinct.

Table 7 and Table 8 Go Here

The estimate for any contact is again statistically significant and is approximately 5.6 percentage points. The results in Table 8 provide us with a measurement of the effectiveness of home-turf canvassing. The coefficient here is positive and statistically significant. Note here that the Q-statistic is statistically significant, but that both fixed and random effects produce identical coefficients. We believe this is likely due to the fact that while there is statistically significant variation across precincts, the β_i estimates do not differ much in an absolute sense. The metaregression coefficients depict the same phenonema as the figures presented earlier; this technique allows us to take the random effect of each precinct into account and to model the heterogeneity of the treatment effect. This provides an extremely strong result in support of home-turf canvassing, with a positive and

statistically significant coefficient on the share of contacts that are home-turf. The metaregression states that if a campaign were to increase the intensity of home-turf canvassing by 10%, they would increase the fraction of individuals who voted by 3%. We find this statistically significant result regardless of whether we include additional covariates or not.

6 Conclusions

The analysis above has demonstrated that the campaign was able to mobilize a group of low-propensity voters to turnout to vote with very high rates of success. A six percentage point increase from a single contact demonstrates that it is indeed possible to mobilize low propensity voters to cast a ballot. The campaign contacted new and occasional voters by using individuals working in their own neighborhoods. Across all methods of estimation, there is a positive and statistically significant effect of canvasser contact. This campaign has demonstrated that, consistent with the rest of the voter mobilization literature, door-to-door canvassing is likely to have a significant effect on turnout even among low-propensity, low-income voters living in a multi-racial context. These kinds of voters are often "written off" by campaigns because mobilizing them is thought to be too costly. Our findings show that this is definitively not the case.

In addition, our findings also show the importance of who does the canvassing. SCOPE was very successful in this campaign partly because they are established in the community, have a well-developed get out the vote infrastructure within their organization, and years of experience doing this sort of work. In addition, their stature in the community and their use of local residents as canvassers also played an important role. Although grassroots organizers and campaign consultants often assert that local canvassers are important, our work provides empirical confirmation of it. We observe a significant increase in turnout as a result of home-turf canvassing. This increase (an additional 4 percentage points above the base increase) is dramatic. We are also able to identify a continuous measure of the intensity of the home-turf contact that translates into larger treatment ef-

fects. This presents compelling evidence for community-based mobilization campaigns. The fact that home-turf canvassers are more effective is consistent with the theoretical model that individuals are best able to communicate with individuals with whom they are connected socially.

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8 Tables and Figures

Table 1: Covariate Balance, Home-Turf Precincts vs Not

Variable	Difference in Means
Democratic Registration	-.032 (.0196)
Age	-13.65 (9.23)
Vote History	.039 (.049)
Percent Latino	.075* (.008)
* = $\alpha = .05$	

Each cell entry describes the difference in means between the home-turf precincts and those without home-turf canvassing. The standard errors for each difference are below in parenthesis.

Table 2: Treatment and Control Assignments and Outcomes

Assignment	Total Obs.	First Contact	Second Contact	Doorhanger	Voted
First Wave Treatment Only	9356	2949	32	1342	3288
First Wave Control Only	3578	2	0	0	1205
First and Second Wave Treatment	2073	2040	548	890	875
Second Wave Control and First Wave Treatment	360	352	5	37	161
Experiment Totals	15367	5343	585	2269	5529

Each cell entry describes the number of individuals who fall into the row assignment category.

Table 3: Effect of Any Contact (Using First Wave Assignment): Intent-to-Treat Effect and Treatment Effect

N in any first wave treatment	11789
N in control group	3578
N in the treatment group who are actually treated	5341
N in the control group who are inadvertently treated	2
N who voted in the treatment group	4324
N who voted in the control group	1205
Percent Voting — Treatment	36.68%
Percent Voting — Control	33.68%
Percent Contacted — Treatment	45.30%
Percent Contacted — Control	.06%
Contact Rate	.4524
Estimated ITT Effect	3.0002
SE of ITT Effect	.9479*
Estimated Treatment Effect	6.6305
SE of Treatment Effect	2.0912*
Household Clusters	13354

*Note that here standard errors are robust and clustered by household.

Table 4: Coefficients from 2SLS, Effect of Any Contact on Turnout

Model	2SLS	2SLS with Control Vars.	2SLS with Precinct Fixed Effects
Any Contact	.066* (.021)	.051* (.020)	.048* (.02)
Democratic Registration		.016** (.008)	
Age		.002* (.000)	
Vote History		.16* (.004)	
Latino		.096* (.014)	
Female		.002 (.010)	
Missing Age		-1.63* (.247)	
Missing Female		-.045 (.079)	
Constant	.340* (.008)	.017 (.012)	.580* (.033)
Precinct Fixed Effects			Included
Household Clusters	13354	13354	13354
N	15367	15367	15367
F	F(1,13353) = 10.05	F(8,13353) = 302.74	F(50,13353) = 3.94
* = $\alpha = .05$			
** = $\alpha = .10$			

Note: The random assignment for treatment in the first wave is used as an instrument and has correlation .4016 with the treatment variable.

Figure 1: Contact Rate by Percent Home-Turf Canvassing. Note that each dot represents one precinct.

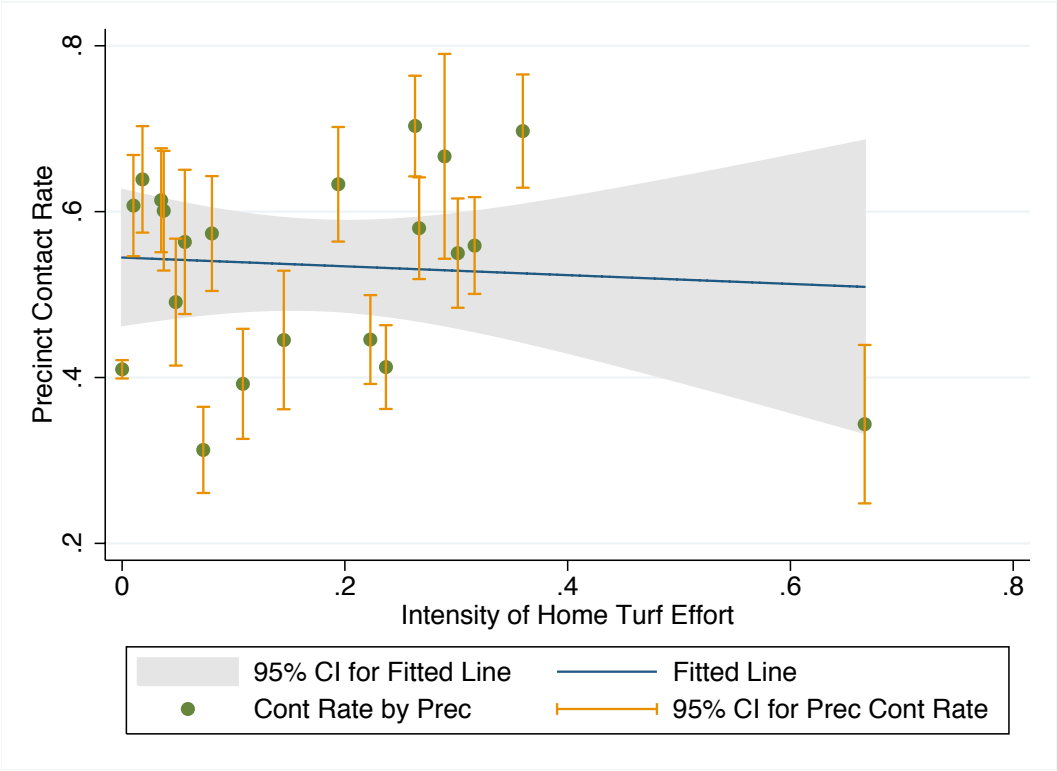


Figure 2: Intent-to-Treat Effect by Percent Home-Turf Canvassing. Note that each dot represents one precinct.

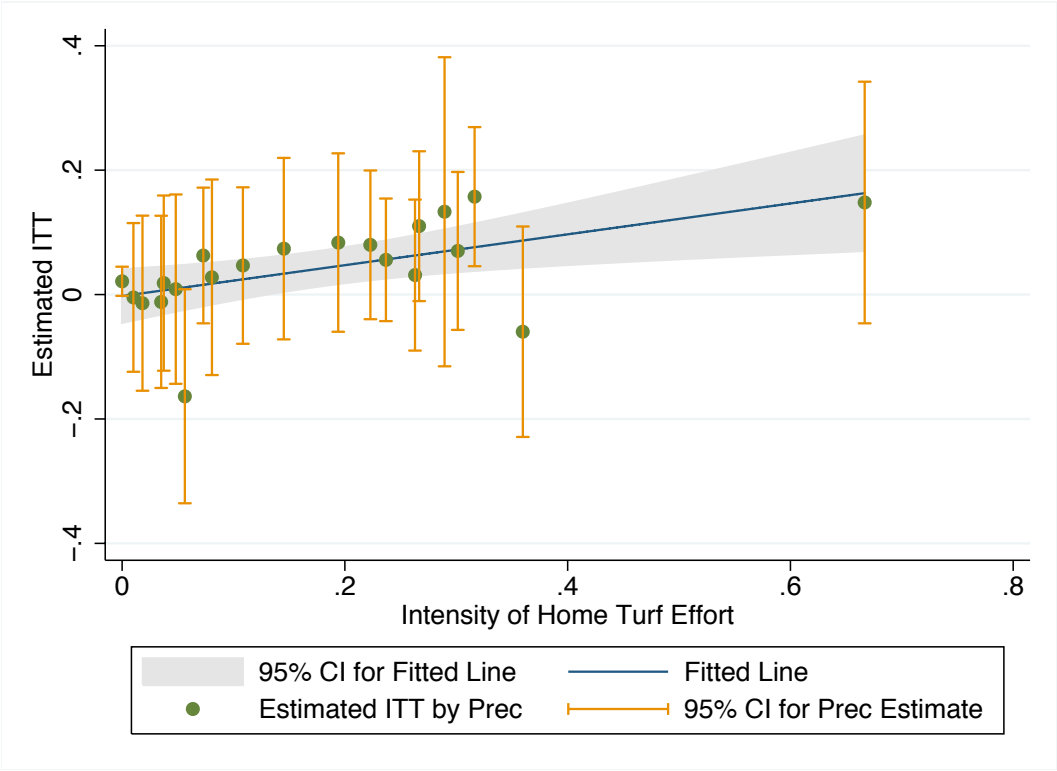


Figure 3: Treatment-on-Treated Effect by Percent Home-Turf Canvassing. Note that each dot represents one precinct.

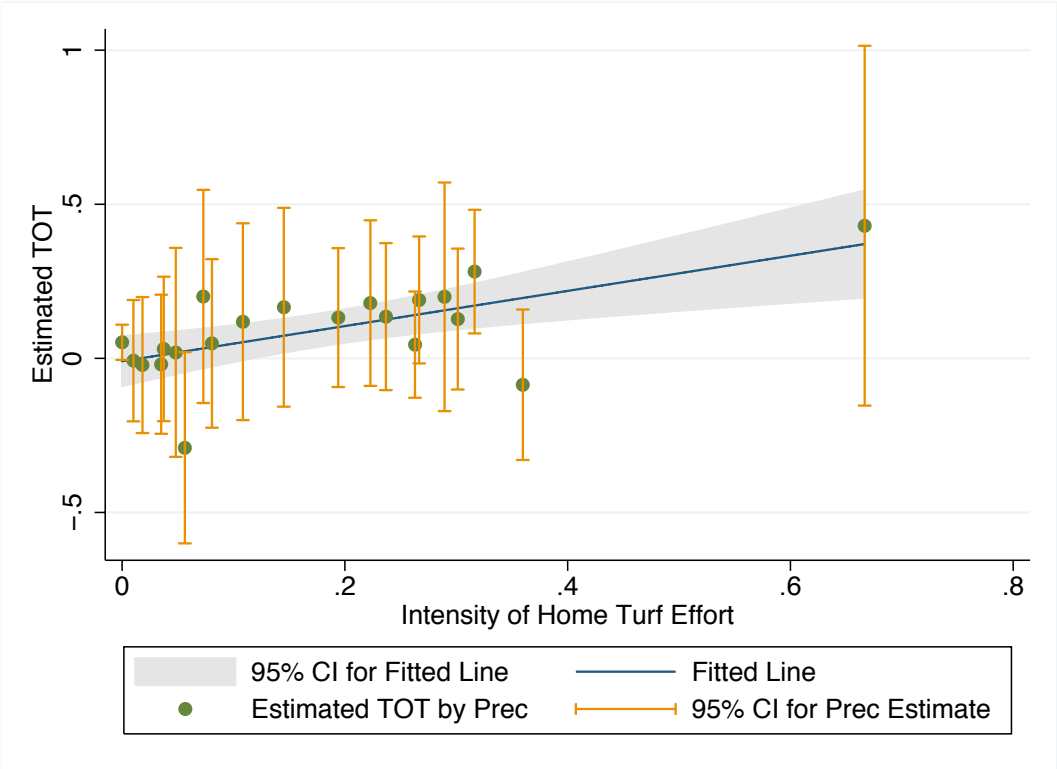


Table 5: 2SLS, Separated by Home-Turf Canvassed Precinct vs Not

	2SLS		2SLS with Control Vars.	
	Coefficients (Home-Turf)	Coefficients (Not)	Coefficients (Home-Turf)	Coefficients (Not)
Contact	.085* (.030)	.052** (.029)	.070* (.028)	.037 (.027)
Democratic Reg.			.001 (.014)	.024* (.010)
Age			.002* (.000)	.001* (.000)
Vote history			.155* (.007)	.163* (.005)
Latino			.117* (.030)	.090* (.016)
Female			-.007 (.016)	.007 (.011)
Missing Age			-2.14* (.395)	-1.37* (.319)
Missing Female			.030 (.136)	-.086 (.070)
Constant	.330* (.013)	.340* (.010)	.008 (.022)	.021 (.014)
F(Covariates, Clusters-1)	8.53	3.20	110.22	194.22
Household Clusters	4807	8547	4807	8547
N	5545	9822	5545	9822
* = $\alpha = .05$				
** = $\alpha = .10$				

The instrument used here is the treatment assignment from the first wave.

Table 6: OLS Coefficients: Effect of Home-Turf Contact on Turnout for Contacted Individuals

Variable	OLS	OLS with Control Vars.	OLS with Walker Fixed Effects	OLS with Walker Fixed Effects and Control Vars.
Percent Home-Turf Contact	0.056** (0.024)	0.043** (0.023)	0.113** (0.031)	0.091** (0.029)
Democratic Registration		0.007 (0.015)		0.011 (0.015)
Age		0.001 (0.000)		0.001 (0.000)
Missing Age		-0.017 (0.037)		-0.024 (0.017)
Female		-0.012 (0.017)		-0.015 (0.017)
Missing Female		0.060 (0.147)		0.085 (0.146)
Vote History		0.178** (0.007)		0.176** (0.007)
Latino		0.112** (0.026)		0.113** (0.025)
Walker Fixed Effects			Included	Included
Constant	0.434** (0.007)	0.108** (0.023)	0.500** (0.217)	0.212 (0.215)
F(Covariates,Clusters-1)	5.23	119.35		
* = $\alpha = .05$				
N	5343	5343	5343	5343

Note: There is a separate fixed effect coefficient for contacts made by unknown walkers (399 observations). Percent Home-Turf Contact indicates the number of walkers that made a contact who were from the same zip code as the voter. The dependent variable used here is an indicator for whether or not the contacted individual turned out to vote.

Table 7: Meta-Analysis

Method	Pooled Estimate: No Cov	p-val	Pooled Estimate: Cov	p-value	N
Fixed Effects	0.057	0.004	0.045	0.015	50
Random Effects	0.057	0.004	0.045	0.015	50
Q-statistic	46.293	0.584	47.480	0.535	49

The dependent variable here is the precinct-level treatment effect estimate. The Q-statistic is a statistical test for heterogeneity across precincts.

Table 8: Random Effects Meta-Analysis Regression

Variable	No Covariates	With Covariates
Share of Contacts that are Home-Turf	0.382** (.157)	0.321** (0.153)
Intercept	0.025 (0.023)	0.001 (0.022)
* = $\alpha = .05$		
N	50	50

The dependent variable used here is the estimate of the treatment effect by precinct. The share of home-turf contacts is calculated at the precinct level.