The Effectiveness of a Neighbor-to-Neighbor Get-Out-the-Vote Program: Evidence from the 2017 Virginia State Elections

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Abstract

We analyze the results of a neighbor-to-neighbor, grassroots get-out-the-vote (GOTV) drive in Virginia, in which unpaid volunteers were encouraged to contact at least three nearby registered voters who were likely co-partisans yet relatively unlikely to vote in the 2017 state election. To measure the campaign's effectiveness, we used a pairwise randomization design whereby each volunteer was assigned to one randomly-selected member of the most geographically proximate pair of voters. Because some volunteers unexpectedly signed up to participate outside their home districts, we analyze the volunteers that adhered to the original hyper-local program design separately from those that did not. We find that the volunteers in the original program design drove a statistically significant 2.3% increase in turnout, which was concentrated in the first voter pair assigned to each volunteer. We discuss implications for the study and design of future GOTV efforts.

In many representative democracies, increasing voter turnout is viewed as a desirable public policy goal. If voter turnout is low, politicians may fail to adequately represent the interests of the electorate. The problem of chronically low voter turnout is particularly acute in the USA, where only 55.7% of the voting-age population cast ballots in the 2016 presidential election (DeSilver, 2017). Turnout in off-year and local elections is typically well under 40% (DeSilver, 2014).

A widely investigated aspect of get-out-the-vote (GOTV) campaigns has been the method of contact (Gerber and Green, 2017; Green and Gerber, 2015; Bedolla and Michelson, 2012). A consistent finding of this literature is that "the more personal the interaction between [the] campaign and [the] potential voter, the more it raises a person's chances of voting" (Green and Gerber, 2015, p. 10). Meta-analyses have consistently shown that in-person canvassing is the most robust and efficacious method of increasing turnout. With a few noteworthy exceptions (Gerber et al., 2008), campaigns that rely on other forms of contact—such as phone, direct mail, e-mail, texts, and social media—have had much smaller effects.

Less investigated has been the relationship between the volunteers who participate in these campaigns and the voters they target – the "who" rather than the "what" of GOTV. Yet a related strand of research on social pressure in voting suggests that the relationship between the campaign representative and the voter may matter a great deal. This literature has shown that many voters are sensitive to social pressure exerted—even if only tacitly or implicitly—by peers, family and neighbors. For example, mailings promising to publicize to a voter's neighbors whether (s)he casts a ballot in an upcoming election are remarkably effective in stimulating turnout (Gerber et al., 2008), and several studies suggest that increasing a voter's likelihood of voting also increases the odds that his/her close friends and family will cast ballots (Bond et al., 2012; Nickerson, 2008; Bhatti et al., 2017). More tellingly, canvassers seem to be more effective when they interact with voters who reside in the same zip code (Sinclair et al., 2013).

In addition to this GOTV literature, there is a substantial literature in political science concerning interpersonal discussion of politics within close networks such as with family, close friends and neighbors. Lake and Huckfeldt (1998) and McClurg (2003) analyzed survey data and found that having a large and politicized social network was correlated with higher levels of political participation. Klofstad (2009, 2010, 2015) used random assignment of students to college dormitories to show that part of this correlation in fact reflects a causal pathway from engagement in civic talk to civic participation, and that political discussion with college neighbors increases an individual's level of political participation even several years after graduation.

We analyze data from a novel GOTV campaign uniquely positioned to test the efficacy of a design based on volunteer-voter relationships. The Plus3 campaign was conducted in Virginia during the three months leading up to the 2017 state election, in which the governor, lieutenant governor, attorney general, and all

100 House of Delegates seats were on the ballot (see Appendix 1 for more details on the campaign). The program combined three unique elements: (1) volunteers were responsible for turning out three specific voters (with the option of requesting more later on); (2) volunteers could make varied and repeated attempts over time to reach each voter, instead of just a single attempt at contact; and (3) voters were selected for close geographic proximity to volunteers, and were therefore more likely to be perceived as "neighbors."

Only a subset of districts in Virginia contained enough interested local activists to field the program. Volunteers who resided in these districts were assigned to contact a minimum of three voters who were on average 0.32 miles away from their homes, in keeping with the original design of the program. Our primary analysis focuses on these volunteers, since they participated in the program as originally designed. Due to the unexpectedly high interest generated by this particular election, however, Plus3 made an accommodation shortly before the election to assign volunteers residing outside of participating districts (and at times outside of the state) to the nearest available voters in participating districts. We analyze the efforts of these volunteers separately as an alternate GOTV program that engaged different types of volunteers, targeted different voters, and involved different methods of contact. Because participation in these two programs was not randomly assigned, we cannot interpret differences between the two programs in a causal fashion, but we report both results for completeness.

We find that the volunteers who participated in the original Plus3 design ("original") drove a statistically significant 2.3% increase in voter turnout. The volunteers in the alternate design ("alternate") had no substantive or significant impact on voter turnout. To analyze the most novel aspect of the Plus3 design, which was providing volunteers with a shortlist of three geographically proximate voters, we decompose the treatment effect for original volunteers by the order in which the voters were assigned to them. Even though the original volunteers' (self-reported) behavior toward the first three voters was largely uniform, the volunteers were able to drive the largest increase in turnout among their first assigned voter. Volunteers may have been most successful with their first assigned voter for a variety of reasons, including that the voter was on average the most geographically proximate to the volunteer and also the most salient (listed first on the assignment sheet). We discuss implications for the design of future GOTV programs that seek to leverage hyper-localism with a small number of assigned voters.

Our paper proceeds as follows. Section 1 discusses the data, randomization procedure, and empirical strategy used to assess the efficacy of each program design. Section 2 presents the empirical results. Section 3 discusses implications for the study and design of future GOTV efforts.

Figure 1: Participating districts in Virginia.
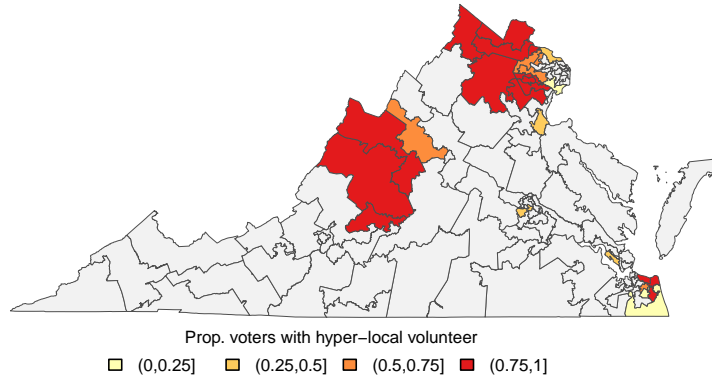
Notes for Figure 1: This figure shows participating districts in Virginia shaded in color by the proportion of voters with a home-district volunteer. Districts not participating are shown in grey.

1 Design, Data, and Methods

Data

Our primary data come from Catalist, a purveyor of data on voting-age individuals in the US. The Catalist data include information on each voter's name; street and mailing address; phone number (populated in about 65% of cases); e-mail address (populated in about 20% of cases); gender; race; age; voting "propensity" score (ranging from 0 to 100) indicating the probability that the voter will turn out in an election given his/her past voting history and demographic characteristics; and "partisanship" score (also ranging from 0 to 100) indicating the probability that the individual is a Democrat.[1] Due to concerns about the quality of the data available for unregistered individuals, only registered voters were included in the program. For each of the 27 House of Delegates districts with participating volunteer groups (shaded in color in Figure 1), data were obtained on all registered voters who were likely Democrats (with partisanship scores ranging from about 60-100) yet relatively unlikely to vote in the November election (with propensity scores typically ranging from 11-60).[2]

The volunteer program was run entirely by the partner organization Plus3, which designed and presented the program at monthly meetings held by various progressive and Democratic party organizations in Virginia. During each meeting, Plus3 leaders explained that after the election, academic researchers would use the data collected on volunteers' outreach efforts and voter turnout to analyze the effectiveness of the program. The

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[1]The final field is important because Virginia is one of several states in which voters do not register party affiliation.

[2]These ranges varied slightly between districts. More details on the inclusion criteria for each district are presented in Appendix 1.

PIs were not involved in running the volunteer program, and volunteers were given no financial incentives for their participation. Volunteers were offered an opportunity to register for the program in one of two ways: either by writing their name, address, e-mail and phone number by hand on a sheet of paper, or by signing up online through the Plus3 website. In both scenarios, consent to the use of volunteer information for the running of the program and its evaluation was obtained.

In total, 1,009 volunteers participated in the campaign. Each volunteer signed up to participate in at least one of the 27 districts.[3] Because of the large amount of interest generated by the election, Plus3 adjusted at the last minute to field two separate GOTV programs to accommodate the participation of as many volunteers as possible. 72% of volunteers signed up at least once within their district of residence in keeping with the original design of Plus3,[4] while the remaining volunteers participated in a district not their own as an alternate accommodation. Most (87%) of the alternate volunteers were forced to participate outside their home district because they lived within a district in which not enough local activists were available to field the program. The remaining alternate volunteers lived either within the region but outside of the state of Virginia (7%), or within a participating district but voluntarily chose a different district (6%). Because district participation depended on the level of *ex ante* interest in the program among grassroots activists, the demographic composition of participating districts was different from that of non-participating districts. For example, participating districts tended to have higher population densities than non-participating districts. Figure 1 shows the proportion of voters that were assigned to a volunteer in the original program by district.

Beginning in mid-September of 2017 until the election in early November, each Plus3 volunteer was sent the names and contact information for the three closest voters meeting the inclusion criteria for the relevant district through an online system. Contact information included street addresses and, where available, phone numbers and email addresses. The volunteer was then encouraged to contact these voters in whatever way(s) he or she preferred—such as by mail, by phone, through social media, and/or in person—to highlight the importance of the upcoming election and encourage the voter to cast a ballot. Any volunteer who wished to contact more than three voters was permitted to do so through the online system, but first had to fill out a Voter Contact Summary Form (VCSF) summarizing the progress (s)he had made in contacting the voters to whom (s)he had previously been assigned. Appendix 2 provides the survey instrument in detail. Volunteers who did not wish to contact more than three voters were also encouraged to fill out the VCSF summarizing their progress. 76% of volunteers did not request more than their three initially assigned pairs. The median number of voters requested by both sets of volunteers was 3, and the average was 4.84 voters

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[3]Most volunteers signed up for only one district—only six volunteers signed up for multiple districts. A small percentage of volunteers (4%) signed up for the program more than once through multiple accounts. We merged their accounts based on matching first name, last name, and home address.

[4]Two of these volunteers participated both in their home district and in another district.

for original-design volunteers and 5.46 voters for alternate-design volunteers.

The VCSF was comprehensive, reflecting all voters to whom a volunteer had been assigned, and dynamic in the sense that it could also be updated at any time. The descriptive information on methods of contact used by volunteers were gleaned from the final version of the VCSF submitted by each active Plus3 volunteer.[5] Volunteer attrition was relatively high: Roughly one-third (34%) of volunteers never opened their voter assignments. Volunteer attrition also varied significantly by program: 31% of volunteers in the original design never opened their voter assignments, compared to 44% of volunteers in the alternate design. While we did not collect individual-level demographics on volunteers, a comparison of imputed gender and race using volunteer names and counties suggests that volunteers who did not open their assignments were not statistically different on these demographics from volunteers that did.[6] As discussed in the analysis section, this high rate of attrition decreases statistical power but does not bias the intent-to-treat effect, since we include all randomized voter pairs in the analysis regardless of whether their assigned volunteers made contact with the treated voter.

Randomization Scheme

We conducted a pairwise randomization of eligible voters into our treatment and control groups. In light of research suggesting that voting behavior is subject to within-household spillover effects, which can be as large as 30–60% (Nickerson, 2008; Bhatti et al., 2017), we included only one eligible voter from each household in the sample frame (Rubin, 1980).

To implement the pairwise randomization design, we first geocoded the household of each volunteer and voter, and then used the Blossom V algorithm (Kolmogorov, 2009) to calculate the optimal set of voter pairs (from among all qualifying voters in each district) that minimized the average within-pair geographic distance. Figure 2 illustrates the results of this process by displaying the set of paired voters for McLean, Virginia. For each pair, we then: (1) calculated the latitude and longitude of the midpoint between the two voters' street addresses; (2) randomly assigned one voter to the treatment condition, rendering him/her eligible for assignment to a volunteer, and (3) assigned the other voter to the control condition, precluding him/her from assignment to any Plus3 volunteer. Finally, each volunteer was allocated the treated member of a physically proximate voter pair within their chosen House of Delegates district, identified by minimizing the global geographic distance between all volunteers' home addresses and the midpoint of the voter pairs' respective addresses within each district. We used the midpoint to preserve balance on distance to the volunteer between treated and control voters.

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[5]Volunteers had until the end of November to finish documenting their efforts during the pre-election period.

[6]We used the Bayesian method proposed in Imai and Khanna (2016) to infer ethnicity, and the method proposed in Blevins and Mullen (2015) to infer gender. 1.7% of volunteers could not be assigned a gender through this method.

Figure 2: Example of Treatment/Control pairs of voters.
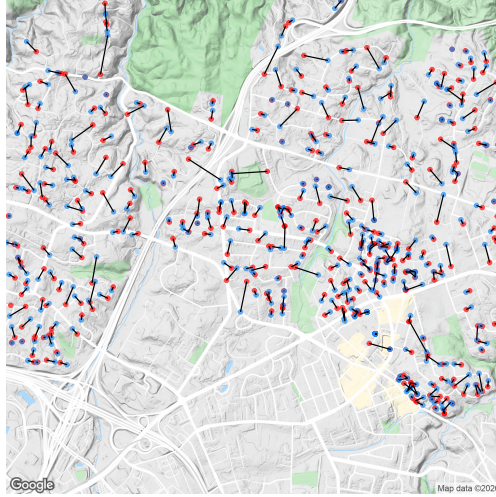
Notes for Figure 2: In this figure, red dots indicate voters in the treatment group, blue dots indicate voters in the control group, and the lines between dots designate a treatment/control pair of voters.

This assignment process was carried out using the Hungarian Algorithm (Kuhn, 1955). Since the optimization problem was global within each district, taking distances between all volunteer-voter pairs into account, the first voter assigned to a volunteer was not always strictly closer than the second voter assigned (though this was the case for 70% of original-design volunteers). We explore this aspect of the optimization in more detail in Appendix 6. Volunteers could ask for more pairs dynamically throughout the course of the program, in which case they would receive their closest geographic match out of the remaining unassigned pairs. In this way, the volunteers were assigned 5,068 pairs of voters throughout the course of the program.

After randomization and assignment, while we had excluded the same household from appearing as a treatment-control pair, we discovered that 3% of volunteer-pair matches assigned a volunteer to a household member (or in rare instances themselves). To simplify interpretation of treatment effects, especially with regard to spillovers, we drop all volunteer-voter pairs in which the volunteers were assigned to a pair including themselves or a household member. This left us with 4,914 voter pairs. In Appendix 4, we report weaker results using all voter pairs, likely due to spillover effects.

Hypotheses

Our primary goal was to evaluate the Plus3 program as originally designed. Because the alternate design was a last-minute addition to the program, we also evaluate this effort separately, but we did not form hypotheses ahead of time for this group. We conducted our evaluation of the program based on the most novel feature of the original design, which was the shortlist of proximal voters. In particular, we investigated

Table 1: Covariate balance between voters in the treatment and control conditions.

Covariate	Original Design			Alternate Design		
	Control	Treatment	p	Control	Treatment	p
Asian	0.04	0.04	0.95	0.06	0.06	0.72
	(0.00)	(0.00)		(0.01)	(0.01)	
Black	0.17	0.18	0.75	0.36	0.35	0.72
	(0.01)	(0.01)		(0.01)	(0.01)	
Hispanic	0.07	0.07	0.75	0.07	0.07	0.72
	(0.00)	(0.00)		(0.01)	(0.01)	
Male	0.37	0.36	0.75	0.37	0.37	1.00
	(0.01)	(0.01)		(0.01)	(0.01)	
Partisanship Score	75.94	76.33	0.75	78.47	78.85	0.72
	(0.20)	(0.20)		(0.29)	(0.29)	
Under 30	0.23	0.24	0.75	0.25	0.25	0.72
	(0.01)	(0.01)		(0.01)	(0.01)	
Vote Propensity Score	29.96	29.78	0.75	27.89	28.90	0.44
	(0.26)	(0.26)		(0.38)	(0.38)	

Notes for Table 1: In this table, the means of each variable are shown for voters assigned to each program, with standard errors given in parentheses. *p*-values are adjusted for multiple testing using Benjamini and Hochberg (1995).

two specific quantities:

1. The overall effect on voter turnout. We expect this effect to be positive.

2. The effect on voter turnout for each voter pair on the shortlist. We expect the effect to be decreasing in the order in which the voters were assigned, in keeping with the design principle that volunteers would be most effective with a smaller number of voters in close proximity to them.

Empirical Strategy

Due to the fundamental differences in volunteers, targeted voters, and tactics between the original design and the alternate design, we analyzed voter pairs in two mutually exclusive groups – those assigned to the original program, in which volunteers engaged primarily with their neighbors, and those assigned to the alternate program, in which out-of-district volunteers (residing within Virginia, Maryland, West Virginia, or DC) contacted voters in particpating districts often far from their own. Table 1 shows that, by virtue of the randomization, voter demographics were well-balanced within each program. It is worth noting that the communities - and in turn, the characteristics of voters - targeted by the Plus3 intervention differed between the two program designs. Overall, the original design targeted significantly lower proportions of Black (18% vs. 35%) and Asian voters (4% vs. 6%) than the alternate design. The original design also targeted voters with slightly higher average vote propensity scores (29.87 vs. 28.39) and slightly lower partisanship scores (76.14 vs. 78.66) than the alternate design. However, a comparison of imputed gender and race using volunteer names and counties suggests that the volunteers themselves were not statistically different between programs on gender and race: about 75% were predicted to be female based on first name, and about 91%

were predicted to be white based on last name and county. Such differences affect the comparability (and generalizability) of findings from the two program designs, but not their internal validity given the uniformly excellent balance between the treatment and control groups.

Our quantity of interest is the intention-to-treat (ITT) effect of a voter being assigned to be contacted by a volunteer. This quantity is unbiased because it preserves balance between paired voters, yet is likely under-powered due to high volunteer attrition. Because the treatment was randomized with equal probability within pairs, we estimate the ITT effect as an average over within-pair differences in turnout as shown in equation 1.

$$\hat{\tau}_{pair} = \frac{1}{J}\sum_{j=1}^{J}(Y_{j,t} - Y_{j,c}) = \frac{1}{J}\sum_{j=1}^{J}\Delta Y_j \tag{1}$$

where $Y_{j,t}$ is the observed binary turnout indicator for the voter assigned to treatment in pair j, and $Y_{j,c}$ is turnout for the voter assigned to control in pair j. This quantity and the conservative within-pair standard error can be obtained through an OLS regression of the within-pair differences in turnout on a single intercept term (Athey and Imbens, 2017; Imbens and Rubin, 2015). To increase the precision of our estimates, we also present OLS estimates of the ITT effect controlling for within-pair differences in the voter demographic controls as shown in Equation 2, where ΔY_j is the within-pair difference in turnout, $\boldsymbol{\Delta X_j}$ represents a vector of within-pair differences in the control variables, and the intercept term, τ is our parameter of interest for the ITT.[7] The model in equation 2 assumes an additive and linear function for the conditional expectation of within-pair differences in turnout given within-pair differences in covariates (Imbens and Rubin, 2015). We present the ITT estimates separately for the voter pairs assigned to volunteers under the original design and the voter pairs assigned to volunteers under the alternate design.

$$\Delta Y_j = \tau + \boldsymbol{\Delta X_j'}\boldsymbol{\beta} + \epsilon_j \tag{2}$$

To investigate the most novel feature of the original design, the shortlist of geographically proximate pairs, we also estimate conditional average intent-to-treat effects by the order in which the pair was assigned to the volunteer. All volunteers were initially assigned three voter pairs as part of the program, but volunteers were free to request as many voter pairs as they liked throughout the program. Pairs were assigned in an order that minimized the global distance between all volunteers and their voter pairs, such that lower assignment orders generally reflected closer distance to the volunteer.[8] We first estimate conditional average treatment

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[7]Demographic controls include race, gender, age, and Catalist's proprietary estimated partisanship and vote propensity scores as shown in Table 1.

[8]As mentioned before, distance was not always monotonically related to assignment order for volunteers because of the global nature of the optimization. We also report results in Appendix 6 specifically for volunteers for whom monotonicity in their assignments held.

Table 2: OLS intention-to-treat estimates.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Intercept	0.022	0.023*	0.005	−0.004
	(0.012)	(0.011)	(0.018)	(0.017)
Voter Covariates	N	Y	N	Y
Pairs	3,364	3,364	1,550	1,550
Adjusted R^2	0.000	0.075	0.000	0.066

Notes for Table 2: This table presents the within-pair OLS intention-to-treat estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). Robust standard errors in parentheses. *p*-values are two-tailed. $^* = p < 0.05$; $^{**} = p < 0.01$; $^{***} = p < 0.001$

effects as the within-pair average difference in turnout for each assignment order group $k \in \{1, 2, 3, 4+\}$ by regressing the within-pair differences in turnout on indicators for each assignment order group. To improve precision, we also estimate the model including the vector $\mathbf{\Delta X_j}$ of within-pair differences in the control variables as shown in equation 3.

$$\Delta Y_j = \sum_{k \in \{1,2,3,4+\}} \tau_k + \mathbf{\Delta X_j'}\boldsymbol{\beta} + \epsilon_j \qquad (3)$$

Appendix 4 presents an alternative conditional logit specification, which provides directionally similar (though noisier) results.

2 Results

Table 2 presents our main findings. Model (1) shows that the unconditional ITT estimate for the original design is positive at 2.2% but lacking in precision to reach conventional levels of statistical significance ($p = 0.065$). After adding in controls to improve precision, we find that the original design increased turnout by 2.3% ($p < 0.05$). The alternate design saw neither substantively nor statistically significant differences in turnout, with point estimates close to zero. We note, however, that a causal comparison between the two programs' effectiveness cannot be made due to the wide range of aforementioned demographic differences between the voters involved in each program.

Table 3 shows that, in addition to the fact that voter demographics differed across the two programs, volunteers in the original design engaged in different GOTV tactics than the volunteers in the alternate design.[9] In particular, original-design volunteers were less likely to send postcards and more likely to contact

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[9] We learned of these tactical differences through the volunteers' VCSF submissions. 62% of treated voters had a VCSF submitted ($n = 3,047$). There was no significant difference in submission rates between volunteers in each design ($p = 0.82$).

Table 3: Comparison of volunteer behavior in original and alternate designs.

	Original Design (Treated Voter $n = 2090$)	Alternate Design (Treated Voter $n = 957$)
Contact methods (prop. of voters)		
Postcard	0.66	0.84**
Social Media	0.02	0.02
Email	0.07	0.06
Phone	0.12	0.17
In Person	0.16	0.03***
Text	0.09	0.07
Other Method	0.24	0.10**
Number of Unique Contact Methods	1.36	1.29
Status Updates		
Days Until First Status Update	29.84	33.53*
Total Number of Status Updates	4.90	6.26
Prop. Last Status In Progress	0.41	0.63*
Prop. Last Status Success	0.12	0.03***

Notes for Table 3: This table presents descriptive comparisons of volunteer behavior in the two programs towards treated voters, based on submissions of the VCSF (response rate $= 62\%$). Appendix 5 shows that there was demographic balance between voters with updates and those without. Statistical significance is for a test of (unadjusted) differences in means, with standard errors clustered on volunteer. p-values are adjusted for multiple testing using Benjamini and Hochberg (1995). $^* = p < 0.05$; $^{**} = p < 0.01$; $^{***} = p < 0.001$

their voters in person or by methods other than postcards, social media, email, phone, or text. Alternate-design volunteers waited nearly 4 days longer on average to give status updates. They were also more likely to report their contact efforts as "in progress," and less likely to confirm that they had success in interactions with their voters, by the end of the program. These tactical differences evidence the difficulties of fostering more personal methods of contact in the alternate design.

Table 4 shows that the conditional intent-to-treat effect for voters in the original design was the largest and statistically significant only for the first assigned pair ($p < 0.01$). By virtue of the assignment order, the first pair was the most salient, and usually the closest, pair to the volunteer. In particular, Table 4 shows that the ITT point estimate for the first pair was about three times the overall average ITT point estimate reported in Table 2. Though we cannot reject a Wald Test for the joint equality of all conditional intent-to-treat effects ($p = 0.09$), the effect for the first pair is significantly different from the effect for the second pair ($p = 0.01$), and significantly different from the pooled effect for pairs 2–4+ ($p = 0.04$). Figure 3 presents the covariate-adjusted conditional average treatment effects graphically to facilitate inspection. We do not see a monotonic relationship between the conditional intent-to-treat point estimates and assignment order, as we had initially expected. We caution as a matter of interpretation that, while the population of volunteers remains relatively constant for the pairs in groups 1, 2, and 3,[10] the 4+ group contains pairs

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[10]The only exception is that pairs assigned to members of their own households are removed from the analysis, so affected volunteers would only appear in groups 2, 3, and 4+.

Table 4: OLS conditional average intent-to-treat estimates.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Order 1	0.075**	0.071**	0.021	0.024
	(0.026)	(0.026)	(0.042)	(0.041)
Order 2	−0.019	−0.021	−0.004	−0.022
	(0.026)	(0.024)	(0.043)	(0.042)
Order 3	0.025	0.030	0.050	0.026
	(0.026)	(0.026)	(0.042)	(0.040)
Order 4+	0.016	0.020	−0.016	−0.019
	(0.019)	(0.018)	(0.026)	(0.025)
Voter Covariates	N	Y	N	Y
Pairs	3,364	3,364	1,550	1,550
Adjusted R^2	0.002	0.076	−0.001	0.065

Notes for Table 4: This table presents OLS conditional average treatment effect estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). Robust standard errors in parentheses. *p*-values are two-tailed. $^* = p < 0.05$; $^{**} = p < 0.01$; $^{***} = p < 0.001$

assigned only to the volunteers who chose to receive more than the minimum number of voter assignments.

Once more, we turned to the VCSF responses to see whether this pattern might arise from volunteers exerting the most effort for the first pair and perhaps less effort for subsequent pairs, or even due to demographic imbalances across pairs, since pairs were assigned in an order that minimized global geographic distance. For this analysis, we estimated the model in Equation 4:

$$\text{Covariate}_j = \alpha + \gamma_2 \text{Order } 2_j + \gamma_3 \text{Order } 3_j + \gamma_{4+} \text{Order } 4^+{}_j + \varepsilon_j \tag{4}$$

where Covariate_j is the covariate for pair j shown in the rows of Table 5 and "Order n" is a set of dummy variables taking on the value of 1 if pair j was assigned in order $n \in \{2, 3, 4+\}$, and 0 otherwise. For covariates that were measured using the VCSF, the sample is limited to treated voters from pair j for whom

Figure 3: Conditional average treatment effects with 95% confidence intervals.

Table 5: Balance in volunteer behavior and pair demographics by assignment order.

	Original			Alternate		
	Order 2	Order 3	Order 4$^+$	Order 2	Order 3	Order 4$^+$
Contact methods (prop. of voters)						
Postcard	0.02	0.06	0.10	0.02	0.01	0.01
Social Media	0.00	0.00	-0.02	-0.02	-0.02	-0.01
Email	-0.02	-0.02	-0.02	-0.01	0.02	-0.00
Phone	0.00	-0.00	-0.04	-0.00	0.00	-0.06
In Person	0.03	0.00	-0.07	-0.02	-0.02	-0.03
Text	-0.01	-0.00	0.08	-0.02	-0.00	-0.00
Other Method	-0.05	-0.04	-0.08	-0.03	-0.00	-0.13
Method Count	-0.02	-0.00	-0.05	-0.07	-0.01	-0.22
Status Updates						
Days Until First Status Update	0.67	0.74	11.08***	0.77	0.58	12.89***
Total Number of Status Updates	-0.03	0.00	3.70*	-0.04	-0.07	4.64
Prop. Last Status In Progress	0.02	0.09**	0.27***	0.04	-0.01	0.27
Prop. Last Status Success	0.00	-0.05	-0.08^*	-0.00	-0.00	0.01
Pair distance from volunteer (mi)	0.04***	0.09***	0.30***	0.37	0.38	3.87
Demographics (pair average)						
Black	0.01	0.02	0.08***	0.03	0.01	0.17***
Hispanic	0.01	0.00	0.01	0.00	-0.00	-0.01
Asian	0.01	0.01	0.01	-0.02	-0.01	-0.02
Male	0.00	0.01	-0.00	-0.03	-0.03	-0.07
Under 30	-0.01	0.02	0.00	0.03	0.01	0.04
Vote Propensity	-0.87	-0.97	-1.38	2.07	1.71	0.11
Partisanship	-0.34	0.08	0.32	-0.45	0.72	2.19*

Notes for Table 5: This table presents balance for pairs on volunteer behavior and voter demographics by assignment order. The sample for the volunteer behavior covariates is limited to treated voters for whom the volunteer filled out a VCSF, while the distance and demographic comparisons use all pairs. Values in the table are the coefficients $\gamma_{2,3,4+}$ in Equation 4, which represent differences in means between the assigned order and order 1. Standard errors for VCSF responses and volunteer-voter distance are clustered on volunteer. p-values are two-tailed and adjusted for multiple testing using Benjamini and Hochberg (1995). $^* = p < 0.05$; $^{**} = p < 0.01$; $^{***} = p < 0.001$

a volunteer submitted a VCSF entry. Otherwise, the sample includes all pairs and the covariate value is the average value across voters in each pair. Table 5 displays the coefficients $\gamma_{2,3,4+}$ in the columns, which show the difference in means from the first assigned pair. Note that, in contrast to Table 4, the population of volunteers does not remain largely constant across orders 1-3 for the contact method and status update covariates, since 19% of the volunteers who submitted updates did not submit updates for all three of their assigned voters.

Table 5 suggests that volunteers behaved largely similarly towards their first three assigned voters. However, the volunteers that requested more than the obligatory three pairs may have exerted less effort for their subsequent pairs. It took original volunteers an average of 11 days longer to give status updates on their progress for voters in pairs 4$^+$ compared to the first pair ($p < 0.001$), and they gave slightly over three fewer status updates on average for these voters compared to the first pair ($p < 0.05$). Similar patterns exist

for the alternate volunteers. Original volunteers were also significantly more likely to report that they were still working on contacting their voters assigned third and later (last status "in progress") compared to their first assigned voter ($p < 0.01$), and less likely to report success for their fourth and later pairs ($p < 0.05$). However, there were no significant differences in reported tactics between the first and second voter pairs. We note that these are self-reported methods of contact, but there is no obvious reason to infer that self-reporting should differ between assigned pairs. Demographic covariates were also relatively well-balanced across assignment order, though pairs assigned fourth and later were more likely to include black voters than the first assigned pair for both original and alternate design volunteers ($p < 0.001$).

As expected for the original-design volunteers, the strongest differentiation between the assigned pairs was distance from the volunteer to the pair. On average, pair 2 was 0.04 miles farther from the volunteer compared to pair 1 ($p < 0.001$), pair 3 was 0.09 miles farther than pair 1 ($p < 0.001$), and all subsequent pairs were on average 0.30 miles farther than pair 1 ($p < 0.001$). Though this strong monotonicity did not hold for each individual volunteer, the aggregate patterns are suggestive of a relationship between proximity and effectiveness. In particular, the fact that behavior choices in the aggregate remained largely consistent across the first three voters, but that distance varied significantly, suggests that the effectiveness of the same GOTV tactics can change substantially alongside small changes in distance from the volunteer to the voter. However, it is important to note that the assignment order mechanism was more than simply a function of volunteer-voter distance. Volunteers were assigned their first three pairs simultaneously with other volunteers in their district who signed up for the program at the same time, and given the most proximate voter pair in a way that minimized the distance across all concurrent voter-volunteer matches. Consequently, assignment order is not solely a reflection of volunteer-voter distance. Other factors, such as the salience of the first position on the assignment list, may have contributed to the stronger effect observed within the first assigned pair.

3 Discussion

The GOTV program analyzed in this study was just one of several grassroots organizations conducting numerous GOTV activities alongside campaign workers and party operatives. What distinguished the program from its peers, and from programs analyzed in prior literature, was the confluence of three factors: the fact that volunteers were assigned no more than three voters at a time; that they were encouraged to contact each voter repeatedly (and, if they wished, in different ways) over a multi-week time frame; and that the volunteers and voters were so geographically proximate that they would likely perceive one another as "neighbors." We find that the volunteers who adhered to the original program design were able to generate

a 2.3% increase in turnout, and volunteers in the alternate design were not able to meaningfully influence turnout. The fact that the original program effect was concentrated in the first, and usually most proximate, assigned voter pair may suggest that the shortlist of a small number of proximate voters was an important design feature shaping the effectiveness of the volunteers' efforts.

There are some limitations to this study which could be improved upon by subsequent work. The Virginia 2017 election was an off-year election, with unusually high turnout, so external validity cannot be assumed. It is difficult to interpret the difference in effectiveness between the original and alternate arms of the program for several reasons. First, since the participating districts were not randomly assigned, original-design volunteers may have had characteristics that made them better canvassers *independent* of the effect of being perceived as a "neighbor." Because over 1,000 volunteers had to be coordinated in a highly decentralized fashion, we could not collect detailed information on the volunteers themselves to assess this possibility. Second, the characteristics of the voters canvassed differed between the original and alternate treatment arms. Different groups of voters will vary on how responsive they are to partisan persuasion to cast a ballot. Third, as shown in Table 3, the canvassing methods used differed between original and alternate-design volunteers, with original-design volunteers far more likely to try to contact their assigned voters in person. Prior evidence shows that personal contact is more effective (Green and Gerber, 2015), so the difference between the two arms may stem from the different methods of voter contact. As was the theory by the Plus3 group, localism may also facilitate more personal methods of contact. Canvassers who are neighbors of potential voters may have more background knowledge engage with voters, which may constitute one of the advantages of hyper-local canvassing. Finally, our survey instrument did not measure frequency of contact beyond whether or not contact was made at all and which type of contact the volunteer chose. It is possible that localism also fostered more frequent opportunities to contact voters, especially given the extended length of time volunteers were given to engage with their assigned voters. Future studies may investigate this further by measuring frequency as well as the personal intensity of contact.

Our results suggest that, in the aggregate, small increases in volunteer-voter distance (0.04 miles on average between the first and second assigned voter) can correlate with less observed effectiveness in the same GOTV tactics. Future randomized studies of GOTV efforts can more precisely measure the impact of volunteer-voter distance as a mediating variable by randomizing the distance between volunteers and voters. Yet the primary implication for the design of GOTV programs is that a trade-off exists between high-impact activities and geographic reach. Volunteer efforts may be best leveraged on only one voter who is their closest neighbor rather than across several voters who are within their neighborhood or local area. Furthermore, the geographic composition of the volunteer base might determine which voters can be mobilized to the greatest extent, which has implications for the equity impacts and targeting capabilities of GOTV programs.

15

It would seem fitting that, true to the grassroots nature of these campaigns, their impact stays close to the locales of volunteers.

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Appendix

1 Summary Statistics By District

The GOTV program was conceived and executed by a newly-formed political organization called Plus3 in collaboration with a number of progressive, grassroots political organizations. These groups included various local Indivisible chapters and Democratic organizations, Network NOVA, Our Revolution, The Liberal Women of Chesterfield County, and Thunderdome Politics. Since this particular election cycle received a great deal of national attention (Schwartzman, 2017; Schneider et al., 2017), Plus3 was just one of several grassroots organizations engaging in GOTV efforts during this period. Other grassroots organizations engaging in GOTV efforts during this period include Let America Vote, New Virginia Majority, and Liberal Women of Chesterfield County.

To determine participation in the study, we obtained information on all registered voters who were likely Democrats (with partisanship scores ranging from about 60-100) yet relatively unlikely to vote in the November election (with propensity scores typically ranging from 11-60). Table A1 shows the specific partisanship and propensity score ranges for each participating district in the campaign that defined the inclusion criteria. Table A1 also presents summary statistics on vote propensity, partisanship, race, and turnout by district.

Table A1: District Characteristics

	Voter Selection Criteria		Voters Assigned to Volunteers (V) and their Paired Controls (C)				
District Number	Range Voting Propensity	Range Democratic Partisanship Score	Number Assigned	Mean Voting Propensity (V+C)	Mean Democratic Partisanship Score (V+C)	Percentage White (V+C)	Mean Turnout (C only)
10	11–60	60–100	211	31.261	75.914	73.934	38.389
13	11–60	60–100	272	28.741	76.561	56.985	37.132
18	6–60	60–100	200	28.823	75.138	86.750	30.000
20	11–60	60–100	149	31.450	75.584	81.544	48.322
21	11–60	60–100	144	28.764	78.824	43.750	40.278
23	11–60	60–100	143	31.913	75.824	65.734	38.462
24	10–60	50–100	334	30.445	68.358	90.569	32.934
25	11–60	60–100	78	31.564	76.862	90.385	57.692
28	11–60	60–100	136	26.143	77.742	45.956	36.029
29	6–60	60–100	249	27.460	76.007	86.747	35.341
31	11–60	60–100	136	30.603	76.364	59.926	39.706
32	11–60	60–100	192	29.427	76.057	60.417	42.188
33	11–60	60–100	273	30.372	76.160	82.967	36.264
34	11–60	60–100	205	31.398	78.415	77.317	45.854
40	11–60	60–100	277	31.209	76.615	69.675	46.570
42	11–60	60–100	69	28.790	78.254	66.667	44.928
50	11–60	60–100	134	29.743	76.660	52.612	41.045
51	11–60	60–100	121	31.711	77.169	67.769	45.455
68	11–60	60–100	251	32.530	77.325	70.319	53.785
81	6–60	60–100	235	25.255	80.323	35.319	32.340
82	11–60	60–100	188	29.460	77.760	68.351	43.085
83	11–60	60–100	87	28.718	79.934	54.598	47.126
84	11–60	60–100	39	30.500	77.540	64.103	38.462
85	11–60	60–100	48	28.167	77.004	55.208	47.917
86	11–60	60–100	139	28.978	75.871	65.108	40.288
87	11–60	60–100	137	29.354	75.405	59.854	44.526
94	11–60	60–100	621	27.320	81.277	32.206	38.003
All	6–60	50–100	5,068	29.456	76.887	64.088	40.272

2 VCSF Form

[NAME], Thank you for volunteering with Plus3!

 Below is your voter contact information. Please also use this form to:

1. Regularly update us on the status of your voters. You can update the form as often as you would like, but, if possible, please do so at least once per week. (Note: you do not need to update the status of all your voters to submit this form.)

2. Request additional voters to contact. (Scroll down to bottom of this form to go directly to this.) Please do not request additional voters until after you have at least begun contacting your current voters.

3. Check out the Quick Start Guide and other regularly updated volunteer resources we have created for you on the Plus3 website: `http://thunderdomepolitics.com/plus-3-initiative/quickstart/`

NOTE: Each time you submit this form, a new link with your updated voter information will be emailed to you. This will replace your existing link and your existing link will no longer function, but don't worry, you will not lose any existing voter information when you press submit.

If you have any questions, please email us at [program contact email].

1. Name: [VOTER 1 NAME]
   Address: [VOTER 1 STREET ADDRESS]
   Age: [VOTER 1 AGE]
   Gender: [VOTER 1 GENDER]
   Phone number: [VOTER 1 PHONE]
   Email: [VOTER 1 EMAIL ADDRESS]

   (A) If you have contacted [VOTER 1 NAME], please let us know how. (Check all boxes that apply. If "Other", please give a brief description).

   ☐ Postcard/Mail

   ☐ Phone

   ☐ Email

   ☐ Social Media

   ☐ Text

   ☐ In-person

   ☐ Other; please describe (open text)

   (B) Based on what you know, please provide us with your best guess right now of [VOTER 1 NAME]'s voting status. (If "Other", please give a brief description).

   ◯ Success! Plan made to vote/already voted absentee

   ◯ Still working on it!

   ◯ Not interested/refused contact/not responsive

   ◯ Incorrect contact information/moved out of district

   ◯ Republican or other party

   ◯ Deceased

   ◯ Other; please describe (open text)

2. Name: [VOTER 2 NAME]
   Address: [VOTER 2 STREET ADDRESS]
   Age: [VOTER 2 AGE]
   Gender: [VOTER 2 GENDER]
   Phone number: [VOTER 2 PHONE]
   Email: [VOTER 2 EMAIL ADDRESS]

   (A) If you have contacted [VOTER 2 NAME], please let us know how. (Check all boxes that apply. If "Other", please give a brief description).

   ☐ Postcard/Mail

   ☐ Phone

   ☐ Email

   ☐ Social Media

   ☐ Text

   ☐ In-person

   ☐ Other; please describe (open text)

   (B) Based on what you know, please provide us with your best guess right now of [VOTER 2 NAME]'s voting status. (If "Other", please give a brief description).

   ◯ Success! Plan made to vote/already voted absentee

   ◯ Still working on it!

   ◯ Not interested/refused contact/not responsive

   ◯ Incorrect contact information/moved out of district

   ◯ Republican or other party

   ◯ Deceased

   ◯ Other; please describe (open text)

3. Name: [VOTER 3 NAME]
   Address: [VOTER 3 STREET ADDRESS]
   Age: [VOTER 3 AGE]
   Gender: [VOTER 3 GENDER]
   Phone number: [VOTER 3 PHONE]
   Email: [VOTER 3 EMAIL ADDRESS]

   (A) If you have contacted [VOTER 3 NAME], please let us know how. (Check all boxes that apply. If "Other", please give a brief description).

   ☐ Postcard/Mail

   ☐ Phone

   ☐ Email

   ☐ Social Media

   ☐ Text

   ☐ In-person

   ☐ Other; please describe (open text)

   (B) Based on what you know, please provide us with your best guess right now of [VOTER 3 NAME]'s voting status. (If "Other", please give a brief description).

○ Success! Plan made to vote/already voted absentee

○ Still working on it!

○ Not interested/refused contact/not responsive

○ Incorrect contact information/moved out of district

○ Republican or other party

○ Deceased

○ Other; please describe (open text)

If you would like to contact additional voters, please select how many. You will receive an email with your new assignments soon. If you would not like to contact additional voters at the current time, please select "No Additional Voters Required". Please do not request additional voters until after you have at least begun contacting your current voters.

○ No Additional Voters Required

○ 1 Additional Voter

○ 2 Additional Voters

○ 3 Additional Voters

Submit

3 Alternate Specifications

Tables A6 and A7 report the main ITT results including the volunteer-voter pairs with at least one voter who resided in the same household as the volunteer ($n = 5,068$ pairs). We find weaker effects when including these households, likely due to spillover effects. Tables A8 and A9 reports the main ITT results using a conditional logit regression specification rather than OLS. Patterns are directionally similar in the conditional logit but the standard errors are slightly too large to reject the null hypothesis for the overall intent-to-treat effect at $\alpha = 0.05$ ($p = 0.066$).

Table A2: OLS intention-to-treat estimates including voters matched to volunteers in the same household.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Intercept	0.020	0.021	0.005	−0.004
	(0.012)	(0.011)	(0.018)	(0.017)
Voter Covariates	N	Y	N	Y
Pairs	3,518	3,518	1,550	1,550
Adjusted R^2	0.000	0.076	0.000	0.066

Notes for Table 2: This table presents the within-pair OLS intention-to-treat estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). Robust standard errors in parentheses. *p*-values are two-tailed. * $= p < 0.05$; ** $= p < 0.01$; *** $= p < 0.001$

Table A3: OLS conditional average intent-to-treat estimates including voters matched to volunteers in the same household.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Order 1	0.058*	0.053*	0.021	0.024
	(0.025)	(0.024)	(0.042)	(0.041)
Order 2	−0.012	−0.016	−0.004	−0.022
	(0.025)	(0.024)	(0.043)	(0.042)
Order 3	0.023	0.031	0.050	0.026
	(0.026)	(0.025)	(0.042)	(0.040)
Order 4+	0.015	0.019	−0.016	−0.019
	(0.019)	(0.018)	(0.026)	(0.025)
Voter Covariates	N	Y	N	Y
Pairs	3,518	3,518	1,550	1,550
Adjusted R^2	0.001	0.077	−0.001	0.065

Notes for Table 4: This table presents OLS conditional average treatment effect estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). Robust standard errors in parentheses. *p*-values are two-tailed. * $= p < 0.05$; ** $= p < 0.01$; *** $= p < 0.001$

Table A4: Conditional Logit intention-to-treat estimates.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Treatment	0.093	0.100	0.021	−0.048
	(0.051)	(0.055)	(0.073)	(0.078)
Voter Covariates	N	Y	N	Y
Pairs	3,364	3,364	1,550	1,550

Notes for Table 2: This table presents the (within-pair) conditional logit intention-to-treat estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). *p*-values are two-tailed. * = $p < 0.05$; ** = $p < 0.01$; *** = $p < 0.001$

Table A5: Conditional Logit conditional average intent-to-treat estimates.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Order 1	0.345**	0.312*	0.086	0.024
	(0.122)	(0.131)	(0.169)	(0.185)
Order 2	−0.081	−0.054	−0.014	−0.079
	(0.111)	(0.129)	(0.167)	(0.179)
Order 3	0.103	0.093	0.204	0.159
	(0.107)	(0.115)	(0.171)	(0.193)
Order 4+	0.067	0.087	−0.065	−0.143
	(0.080)	(0.087)	(0.109)	(0.120)
Voter Covariates	N	Y	N	Y
Pairs	3,364	3,364	1,550	1,550

Notes for Table 4: This table presents the (within-pair) conditional logit conditional average treatment effect estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design") by assignment order. Each coefficient and standard error is from estimating a separate conditional logit model for pairs assigned in order $n \in \{1, 2, 3, 4+\}$. *p*-values are two-tailed. * = $p < 0.05$; ** = $p < 0.01$; *** = $p < 0.001$

4 Alternate Specifications

Tables A6 and A7 report the main ITT results including the volunteer-voter pairs with at least one voter who resided in the same household as the volunteer ($n = 5,068$ pairs). We find weaker effects when including these households, likely due to spillover effects. Tables A8 and A9 reports the main ITT results using a conditional logit regression specification rather than OLS. Patterns are directionally similar in the conditional logit but the standard errors are slightly too large to reject the null hypothesis for the overall intent-to-treat effect at $\alpha = 0.05$ ($p = 0.066$).

Table A6: OLS intention-to-treat estimates including voters matched to volunteers in the same household.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Intercept	0.020	0.021	0.005	−0.004
	(0.012)	(0.011)	(0.018)	(0.017)
Voter Covariates	N	Y	N	Y
Pairs	3,518	3,518	1,550	1,550
Adjusted R^2	0.000	0.076	0.000	0.066

Notes for Table 2: This table presents the within-pair OLS intention-to-treat estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). Robust standard errors in parentheses. *p*-values are two-tailed. * $= p < 0.05$; ** $= p < 0.01$; *** $= p < 0.001$

Table A7: OLS conditional average intent-to-treat estimates including voters matched to volunteers in the same household.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Order 1	0.058*	0.053*	0.021	0.024
	(0.025)	(0.024)	(0.042)	(0.041)
Order 2	−0.012	−0.016	−0.004	−0.022
	(0.025)	(0.024)	(0.043)	(0.042)
Order 3	0.023	0.031	0.050	0.026
	(0.026)	(0.025)	(0.042)	(0.040)
Order 4+	0.015	0.019	−0.016	−0.019
	(0.019)	(0.018)	(0.026)	(0.025)
Voter Covariates	N	Y	N	Y
Pairs	3,518	3,518	1,550	1,550
Adjusted R^2	0.001	0.077	−0.001	0.065

Notes for Table 4: This table presents OLS conditional average treatment effect estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). Robust standard errors in parentheses. *p*-values are two-tailed. * $= p < 0.05$; ** $= p < 0.01$; *** $= p < 0.001$

Table A8: Conditional Logit intention-to-treat estimates.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Treatment	0.093	0.100	0.021	−0.048
	(0.051)	(0.055)	(0.073)	(0.078)
Voter Covariates	N	Y	N	Y
Pairs	3,364	3,364	1,550	1,550

Notes for Table 2: This table presents the (within-pair) conditional logit intention-to-treat estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design"). *p*-values are two-tailed. * = $p < 0.05$; ** = $p < 0.01$; *** = $p < 0.001$

Table A9: Conditional Logit conditional average intent-to-treat estimates.

	Original Design		Alternate Design	
	(1)	(2)	(3)	(4)
Order 1	0.345**	0.312*	0.086	0.024
	(0.122)	(0.131)	(0.169)	(0.185)
Order 2	−0.081	−0.054	−0.014	−0.079
	(0.111)	(0.129)	(0.167)	(0.179)
Order 3	0.103	0.093	0.204	0.159
	(0.107)	(0.115)	(0.171)	(0.193)
Order 4+	0.067	0.087	−0.065	−0.143
	(0.080)	(0.087)	(0.109)	(0.120)
Voter Covariates	N	Y	N	Y
Pairs	3,364	3,364	1,550	1,550

Notes for Table 4: This table presents the (within-pair) conditional logit conditional average treatment effect estimates for voter pairs assigned to home-district volunteers only ("original design"), and voters assigned to out-of-district volunteers only ("alternate design") by assignment order. Each coefficient and standard error is from estimating a separate conditional logit model for pairs assigned in order $n \in \{1, 2, 3, 4+\}$. *p*-values are two-tailed. * = $p < 0.05$; ** = $p < 0.01$; *** = $p < 0.001$

5   VCSF Response Bias

Table A10 shows the differences in means across voter covariates between treated voters for whom volunteers submitted at least one VCSF update, and treated voters for whom volunteers never submitted VCSF updates. The largest substantive differences in the alternate sample were for black voters (about 4% more likely to receive an update), voters under 30 (about 3% more likely to receive an update), and male voters (about 4% less likely to receive an update). After adjusting for multiple testing, these differences are not statistically significant for the alternate volunteers. Before and after adjusting for multiple testing, no p-value is significant for differences between original volunteers' treated voters with and without updates. There were no significant difference in submission rates between original and alternate volunteers ($p = 0.82$).

Table A10: VCSF Response Bias

Covariate	Original			Alternate		
	No Update	Update	p	No Update	Update	p
Asian	0.04	0.04	0.61	0.06	0.05	0.25
	(0.00)	(0.00)		(0.01)	(0.01)	
Black	0.17	0.19	0.61	0.34	0.38	0.11
	(0.01)	(0.01)		(0.01)	(0.02)	
Hispanic	0.07	0.07	0.95	0.07	0.07	0.98
	(0.00)	(0.01)		(0.01)	(0.01)	
Male	0.37	0.37	0.95	0.38	0.34	0.11
	(0.01)	(0.01)		(0.01)	(0.02)	
Partisanship	76.02	76.39	0.61	78.47	79.07	0.25
	(0.17)	(0.25)		(0.24)	(0.37)	
Under 30	0.23	0.24	0.61	0.24	0.27	0.11
	(0.01)	(0.01)		(0.01)	(0.01)	
Vote Propensity	29.98	29.63	0.61	28.66	27.79	0.24
	(0.22)	(0.33)		(0.33)	(0.48)	

Notes for Table A10: This table shows the degree of covariate balance between treated voters for whom volunteers submitted at least one VCSF update (Update) and for whom volunteers never submitted VCSF updates (No Update). Means are shown with standard errors in parentheses. p-values are adjusted for multiple testing using Benjamini and Hochberg (1995).

6   Monotonicity of Voter Assignment Process

Table A11 compares the covariate-adjusted conditional intent-to-treat results reported in the main body of the paper ("All pairs") to the results on subsamples of pairs assigned to volunteers for whom distance tracked assignment order. We first estimate the conditional average intent-to-treat effects on the sample of pairs assigned to volunteers for whom the first assigned pair was the closest to their home address ("first pair closest"). We then estimate the conditional average intent-to-treat effects on the sample of pairs assigned to volunteers for whom assignment order increased monotonically with the pair's distance from their home address between pairs 1, 2, 3, and 4+. We find that the effect for the first pair grows in magnitude for both subsamples of pairs, though a lack of precision makes the effect statistically indistinguishable from the effect using the whole sample. These results suggest that, while related to the strong effect for the first pair, volunteer-voter distance was not the sole mechanism driving the assignment order effect.

Table A11: OLS conditional average intent-to-treat estimates by degree of correlation between assignment order and distance.

		Original Design	
	All Pairs	First Pair Closest	Strong Monotonicity
Order 1	0.071**	0.080**	0.088**
	(0.026)	(0.029)	(0.032)
Order 2	−0.021	−0.026	0.020
	(0.024)	(0.029)	(0.032)
Order 3	0.030	0.044	0.036
	(0.026)	(0.031)	(0.035)
Order 4+	0.020	0.019	−0.003
	(0.018)	(0.021)	(0.025)
Voter Covariates	Y	Y	Y
Pairs	3,364	2,401	1,820
Adjusted R^2	0.076	0.077	0.079

Notes for Table A11: This table presents OLS conditional average treatment effect estimates for all voter pairs assigned to the original design ("All Pairs"), voter pairs assigned to volunteers for whom the first pair was geographically the closest ("First Pair Closest"), and voter pairs assigned to volunteers for whom volunteer-pair distance increased monotonically with assignment order ("Strong Monotonicity"). Pairs assigned to volunteers in their own households are excluded. Robust standard errors in parentheses. p-values are two-tailed. * = $p < 0.05$; ** = $p < 0.01$; *** = $p < 0.001$