

“Two Votes For One” *

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Abstract

Political scientists have demonstrated the effectiveness of get out the vote (GOTV) campaigns to mobilize voters. However, the effect that these strategies have on the network of the citizens targeted is far less explored. Is it possible to mobilize multiple individuals in a single household by contacting only one of them? We theorize that individuals are susceptible to pressure within their household but conditional on the civic and social distance between them. We test our hypothesis by conducting a randomized field experiment on early voting prior to the November 2012 General Election in Florida, where we assign households to a control condition (no mailer), or a treatment condition (mailer encouraging to vote early). In order to detect direct and spillover effects, we analyze the treatment effects based on whether subjects received it directly or indirectly. We not only find evidence of spillovers in households where the person contacted shows low levels of participation, but we also find support for our hypothesis that compliance of the influencer with the treatment is crucial for the spillover of the mobilization effect. Further, we exploit differences in social distance (age, last name, prior vote history), geographic distance (distance to the early-in-person-voting site), and timing (date the ballot was cast) to build a theory of group cues efficacy based upon network density.

Introduction

When you plan your day on Election Day, do you talk to anyone else in your household about when and where you will cast your ballot? Did you learn about how to vote early from a conversation you had with another voter in your household?¹ Do you typically carpool to your polling place when you go to vote? Did someone in particular ask you to vote? Is voting particularly important to someone in your family? After you cast a ballot, do you typically wear your “I voted” sticker for the rest of the day? The answers to these questions all point toward the fact that the act of voting is a social act. Indeed, voting is widely understood to be strongly influenced by social interactions, but the mechanisms that tie a person’s social environment to her voting behavior remain elusive.

Peers in a voter’s social environment are generally seen to provide influence via two channels. First, social pressure encourages voters to adhere to the norm of voting. Second, conversation with peers can reduce the costs of voting by providing trusted information about the logistics of voting itself as well as about candidates. Existing research on voting and the social environment cannot disentangle these mechanisms: the same voices that administer the social pressure are precisely those who are trusted to deliver political information. Are voters more likely to vote because of social pressure or because their social network delivers the relevant political information?

There is an established literature on the impact of social interactions and turnout but the mechanisms and magnitudes of the effects vary widely (Mann and Klofstad 2012; Sinclair 2010; Rolfe 2012; Verba, Schlozman and Brady 1994; Rosenstone and Hansen 1993). How are voting norms established within networks? Political scientists have varied experimental evidence that voting behavior is contagious within households, with estimates ranging from 20% - 60% (Sinclair, McConnell and Green 2012; Nickerson 2008), among friends, with estimates ranging from 1-3% (Bond et al 2012), and within a voters social network both online and offline, with estimates ranging from 1-15% (Apicella, Marlowe, Fowler and Christakis 2012; Centola 2011; Foos and de Rooij

¹In 34 states and the District of Columbia, voters can vote early in person without an excuse. Three states conduct their elections by mail (Colorado, Oregon and Washington). All remaining states have some system in place for voters to request a ballot by mail, albeit some require demonstrable need. This is to say that it wouldn’t be particularly unusual for a voter to be faced with the prospect of casting her ballot ahead of election day.

2016). One possibility to explain this variance in associations is the presence or absence of civic norms in these social ties (Sinclair 2012). This paper hypothesizes that if transmission is driven by a peer who holds the social norm of voting that the transmission will be more effective, largely due to the prioritization of personal relationships over political behavior. That is, family members who not only vote (and who care about voting) are likely to be able to successfully influence their family members to vote as well. Yet, this is a challenging problem to study, as observational studies have documented the high degree of political homophily within networks (Stoker and Jennings 2005; Cutts and Fieldhouse 2009; Fieldhouse and Cutts 2012; Hobbs, Christakis and Fowler 2014).

We design a randomized field experiment where we administer a mailer to a population of voters and monitor their turnout decisions. Unlike a conventional experiment, however, we are particularly interested in the spillover from a treated individual to another individual in the household. This gives us a chance to move beyond simply establishing the presence of spillover to better understanding how and when the desire to turn out to vote will be contagious to others. We accomplish this in two ways. First, we locate our experiment in the context of early voting. This, then, gives us the additional leverage of time. If we carpool to the polling place, for example, our ballots will be stamped with an identical time stamp and thus there is an empirical record of our shared behavior. Second, we randomly assign which individual in a household receives our treatment mailer and thus vary distance between a voter and her network, in terms of social, geographical, and civic distance. Voters who are more proximate, in each of these categories, should exert more influence on each other.

This project speaks to the literature on political engagement and electoral mobilization. If mobilization happens because of a reduction in cost versus if mobilization happens because of social pressure, we gain new insights into basic questions of political participation. We also gain new insights into how to best mobilize future voters. Perhaps most importantly, we learn about the potential impacts of convenience voting in a participatory democracy.

Theory

There are two principle theories regarding how individuals might adopt the behaviors of their political network. First, individuals may rely on their social networks for information, in particular from trusted sources. Social networks may provide arguments to use for deliberation, may provide information about candidate platforms or places where donations are needed, or may simply be able to relay how and when to cast a ballot. Information has been typically considered contagious across political networks; this is the most common explanation for the presence of social network effects. Second, individuals may face social pressure from their political network. Individuals rely on their social networks to maintain their social identity and may base their preferences on social comparison with their network. In this case, the network exerts social pressure on each member, particularly if a majority of network members have homogeneous preferences regarding a political or social norm. There are strong, separate arguments underpinning each of these theories, and each theory generates a distinct set of hypotheses regarding a set of empirical regularities. For example, if social pressure dominates a citizen's experience of democracy, then across behaviors there should be a network effect. The effect should be determined more by intimate network ties and less by a broader social context or other indirect influences. This effect should be larger for relationships in which there is a tighter social connection. In contrast, network influence generated by shared information should not vary based on the social proximity of the relationship.

Recent field experiments have consistently noted the efficacy of publicly revealing whether or not a particular voter has adhered to the social norm of voting. These experiments, with stimuli ranging from mailing postcards listing a voter's publicly-available history of electoral participation to threatening to publish non-voter names in the local newspaper, have generated large mobilization effects comparable with the best door-to-door canvassing campaigns (Abrajano and Panagopoulos 2009; Gerber, Green and Larimer 2008, 2010; Grose and Russell 2008; Larimer 2009; Mann 2010; Panagopoulos 2009, 2010; Sinclair, McConnell and Green 2010). Dozens of other experiments have demonstrated that more personal mobilization stimuli (door-to-door conversations between a voter and a canvasser, for example, or a lengthy unscripted phone call) generate larger effects

than more indirect methods, such as robocalls (Gerber and Green 2000, 2001; Green and Gerber 2008; Green, Gerber and Nickerson 2003; Michelson, García Bedolla and McConnell 2009). More recent studies have documented that closer personal connections – either closer friendship (Bond et al. 2012) or closer geographic proximity (Sinclair, McConnell and Michelson 2013) generate larger effects. The success of these experiments suggest that there are important social interactions at work behind the turnout decision. Voters fear they will lose some component of their social relationships — either status or respect — for failing to take part in the social norm of participating in democracy, that is, the civic duty of casting a ballot (Blais 2000).

However, much of this literature is limited in that it cannot fundamentally disentangle whether these effects are associated with shared norms or shared costs. Here we design an experiment to begin to provide limited evidence to disentangle these factors. In this project, we randomly assign a population of individuals to receive a mobilization appeal to vote early. We randomly assign this stimuli to households with varied propensities to participate in politics. That is to say, imagine two very different three-person households. In one case, a high-propensity voter, who lives with two low-propensity voters, receives a piece of political mail asking her to vote early. Even if she complies with the mailer and changes her participation from habits, from voting in a precinct to voting early, is that shift likely to affect her housemates? Now consider the opposite household: we send a piece of mail to one of the low propensity voters, asking her to vote early. The mail adheres to all the features known to increase turnout, with a little twist – it is a nudge to vote early. It just so happens this nudge gets the low-propensity voter to the polls. The other person in her household who is also a low-propensity voter is shocked. She observes a credible signal about the importance of voting in the election – and this doesn't have to do with simply an observation about the election logistics. She receives a credible cue about the social norm of voting from her housemate. Then, and only then, is she likely to turn out to vote.

Theories of political contagion vary as to which actors are most likely to transmit voting behavior changes. In this paper we argue that this design will allow us to evaluate whether information (which can easily be communicated from the high propensity voter to the low propensity

voter) or social norms (which is harder to communicate because it requires the low propensity voter send a credible signal about the importance of the norm of voting) is more likely to drive political participation. That is to say, we anticipate that only by shifting the behavior of low propensity voters will we be able to establish changing the norms of voting – and we need to do this exogenously to see if we can generate spillover because a new norm. Beyond vote history, we will also establish evidence whether shared traits (shared last name, difference in age) can provide additional boosts to generate spillover.

Research Design

To test and explore the expectations and hypotheses regarding spillover effects and distance, we conducted a field experiment during the 2012 presidential general election in Florida. The experiment was conducted in partnership with a non-partisan public charity organization with a mission to increase participation in the electoral process. Our experiment was embedded in the organization’s regular voter mobilization program, so the it has a high degree of realism, and the arrangement with the partner organization granted unrestricted publication rights, thus avoiding potential selection bias in reported results (Gerber 2011; Nickerson 2011).²

The 2012 Presidential general election was a very difficult context to identify mobilization effects, especially in Florida as it was a critical state for both presidential nominees’ Electoral College strategies. Very high natural turnout and extensive voter mobilization activity by other organizations was likely to attenuate treatment effects. If it were a low-salience off-year election, our treatment might have been the only voter contact, but in the Florida’s 2012 electoral environment, our treatment was more likely to be part of a great flood of voter contact. This context makes it far more difficult to generate a marginal change in voting behavior. However, in an attempt to account for this situation and in order to “isolate” the effect of our treatment, our design focus on

²The name of the partner organization is withheld in accordance with the partnership agreement. The organization was disclosed to the Institutional Review Board and (could be, upon request) disclosed to the journal editors upon submission.

the mobilization for *early voting* and not only on *voting* as most of the studies in this field. This novel features not only allows us to explore a new dimension of political participation and electoral behavior, but also permits us to improve the identification of treatment effects.

Experimental population

Our partner organization sought to mobilize voters with a low to moderate probability of voting in the 2012 General Election, as recommended from past field experiment research (Arceneaux & Nickerson 2009), and common practice among political professionals (Issenberg 2012; Malchow 2008). The organization targeted registered voters in Miami-Dade and Broward, the two largest counties in Florida, with one or more of the following characteristics: 1) a 10 to 75 percent of probability of voting in the upcoming General Election,³ 2) of Latino ethnicity,⁴ or 3) interested in the organization's issue-based mission.⁵

We assume that co-habitants at each address are part of the targeted voter's *closest* social network as family members or close friends. The assumption is not much of a stretch, but it means we are only measuring spillover within a specific segment of the generally complex social networks. A household of co-habitants is the portion of a social network that is physically closest and with whom interaction is likely to be most frequent, so it is a critical starting point for understanding the transmission of voting behavior. Therefore, our experimental population is comprised of all registered voters residing at the same address as a registered voter targeted by our partner organization's.⁶ Since we were investigating intra-household dispersion of treatment effects, addresses with only a single registered voter were excluded from the experimental population.

³The predictive model to identify this pool of voters was provided by Catalist LLC.

⁴As coded by Catalist LLC

⁵Additional information is available upon request.

⁶The rest of the members of a household where a targeted voter is identified are not required to have any of the criteria to be selected enumerated above.

Mobilization treatment: description and assignment

The mobilization treatment consisted on sending letters addressed to a specific voter through post-mail with a message exerting positive social pressure, and encouraging her to vote early. We identify the person to whom the mailer is directed as a *contact*. The letters were mailed on October 23, 2012 so they would arrive just before the eight days period of early in-person voting prior to Election Day. The text of the mailer derives from an amalgamation of previously successful mailers that have used a social pressure mechanism (Gerber et al. 2008, 2010; Mann 2010; Matland & Murray 2013; Panagopoulos 2010, 2011). Social pressure treatments communicate that voting behavior is being monitored, and they are thought to be effective because they increase extrinsic direct benefits from voting in the form of social rewards (or avoiding social sanction for failing to vote). Our mailer included a version of Panagopoulos “gratitude” tactic: thanking the voter for past participation implies voting behavior is being monitored but disguises the social pressure with positive social reward rather than a heavy-handed social threat. The mailer also provided information about opportunities to vote early, including the hours and locations for EIPV for the county in which the recipient resides. A series of field experiments demonstrated that the version of the gratitude treatment we use is effective for mobilizing voters for EIPV (Mann 2013).⁷ In sum, our study comprises two groups: a control group of households that did not receive any type of mailer, and a treatment group of households that receive a mailer with a mobilizing message.

The assignment of households to treatment and control groups proceeded in multiple stages illustrated in Figure 1. First we classify the members of each household into two groups according to their levels of participation: low and high. A subject labeled as Low has voted in less than 33% of the elections to which she was registered since 2004. In contrast, a High participation subject has participated in 33% or more of the elections to which she has been registered. Given that we are interested in the effect that political and civic norms like participation have on networking dynamics, we excluded homogeneous households whose members were all type Low or all type

⁷A second version of the treatment mailer appended a message urging the voter to tell her family and friends to vote early but was otherwise identical. Further details are presented in the Supplemental Materials.

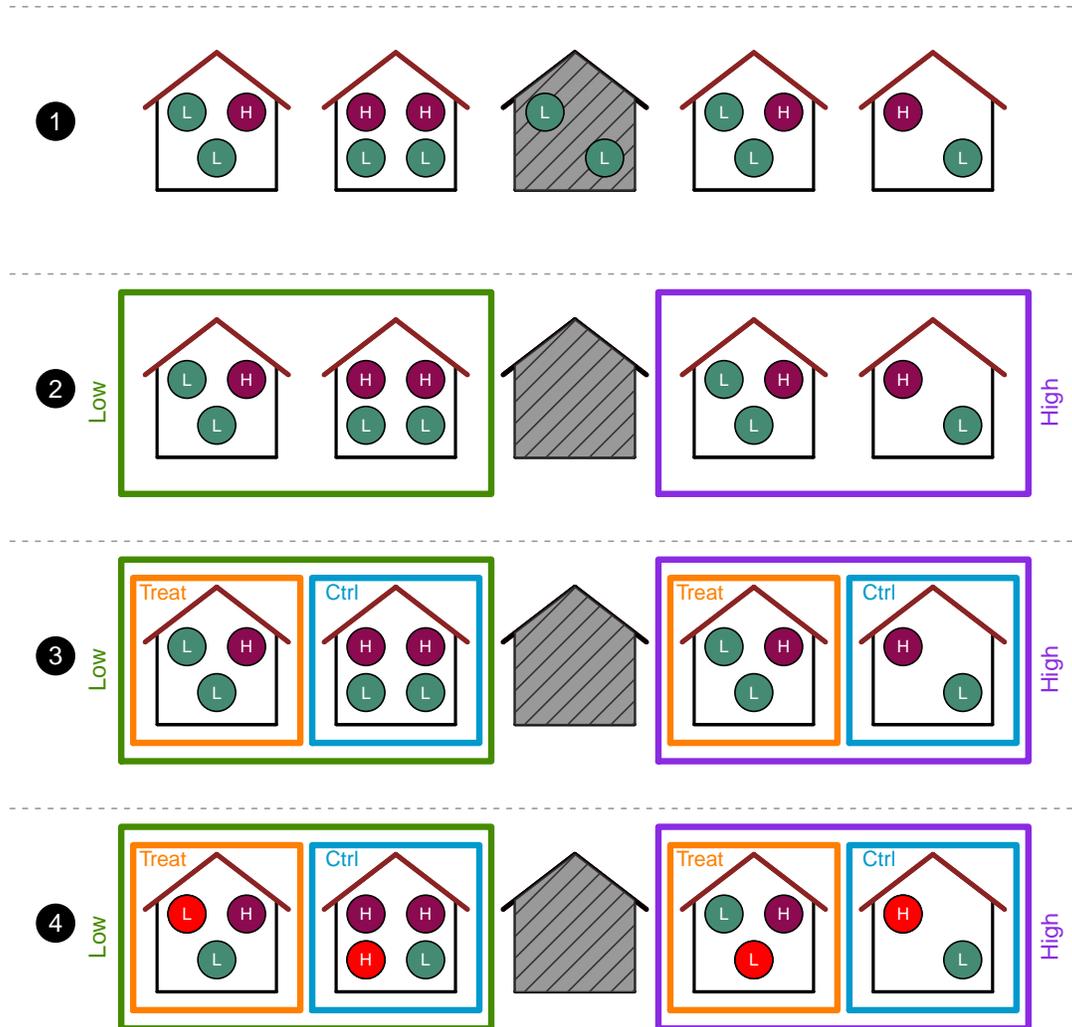
High.

Second, we randomize whether a household will have a Low or a High type *contact*. Each household has a contact member that, in case of belonging to the treatment group, receives a mailer directed to her. If the contact is in the control group, she does not receive a mailer but is still identified in the data with that label. We refer to the rest of the members of the household as *non-contacts*. In Figure 1, the green rectangle indicates that the first two households will have a Low contact, while those contained in the purple one will have a High contact. This classification allows us to analyze the directions of the treatment spillovers: Low to High, High to Low, Low to Low, and High to High.

The third stage includes randomizing within participation groups whether a household will be under treatment or under control. The orange rectangle in the third row of Figure 1 indicates that the contact in those household receive a mailer, while the blue rectangles are grouping the houses that remain under control. Finally, within each household, a specific person is selected to be the contact among those eligible according to the participation label of the household. In other words, if a household is assigned to be Low, a specific contact is selected from its subjects that are Low type. This implies that if there is only one Low type person in a household that is assigned to be Low, then she will be automatically selected to be the contact. We account for these unequal probabilities of being selected in all of our analyses.

In sum, our sample is comprised by 180,854 registered voters within 62,446 households. From this pool of households, 31,298 were assigned a Low participation contact, and within this group 10,298 remained under control while 21,000 were assigned to treatment. On the other hand, the High participation group has 31,148 households from which 10,148 belong to the control group, and 21,000 to the treatment group.

Figure 1: Illustration of the treatment assignment stages



Analysis and results

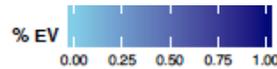
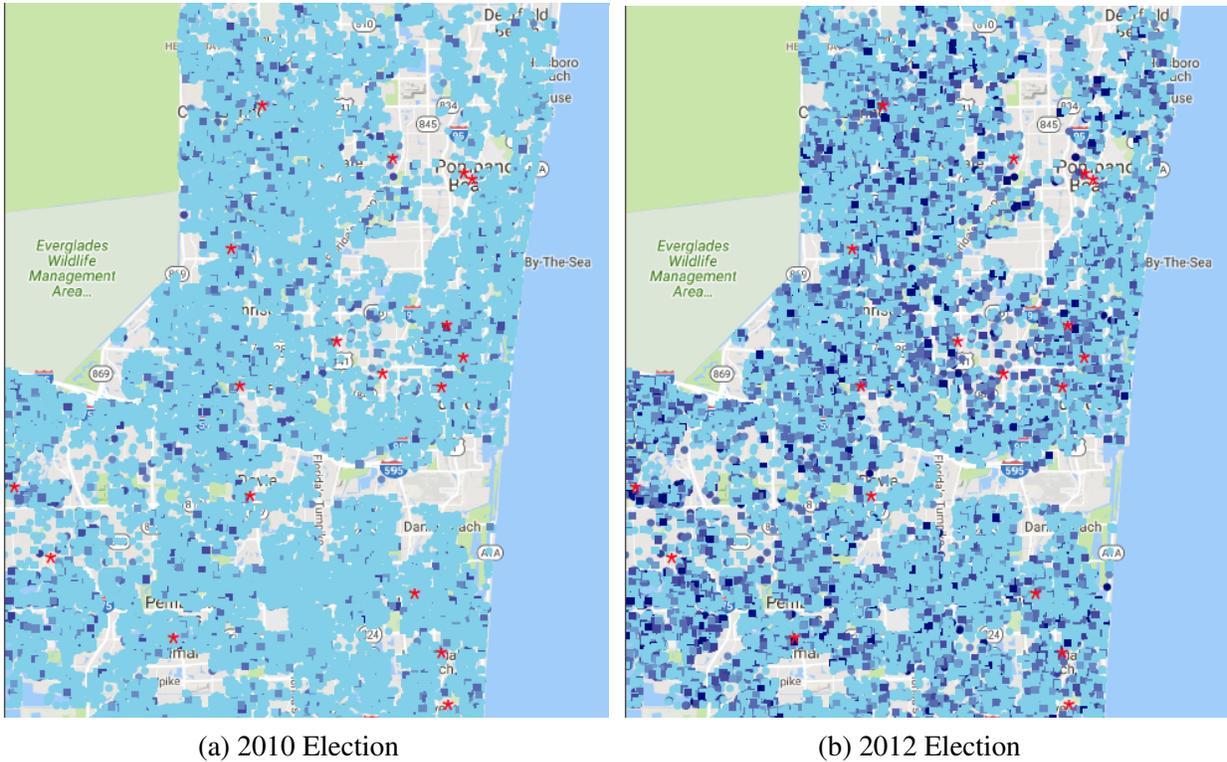
We conduct a series of analyses to test the presence of two types of treatment effects: direct effects and spillover effects. Further, we also explored and analyzed the role of distance, conceptualized in multiple ways, on the effectiveness of our attempts of mobilization. The models that we specify to reach these objectives have as outcome variable a binary indicator of whether a particular set of subjects voted early in the 2012 presidential election. While some of the models of early vote that we conduct are merely a function of the treatment, in certain specifications we also included the following covariates: size of the household, gender, age, ethnicity and vote history of the voters, and county.

The first thing that we consider worth highlighting is that there is a substantive change in the amount of people that voted early in the 2012 election in comparison to the 2010 election. Figure 2 illustrates these differences. Each panel shows a map of Broward county. Each point in the map represents a household and the color of them indicates the proportion of members in the household that voted early: the darker the blue, the highest the proportion of the members that attend the polling stations before Election Day. A simple visual inspection evidences that the number of members per household that voted early increased from 2010 to 2012. Of course this does not offer conclusive evidence that our mobilization attempts were successful. Other factors such as the saliency of the election (presidential vs. congressional), alternative mobilization efforts, campaigns, etc. could have played a role. Therefore, the analyses presented below aim to dissect the effects that the mobilization treatment has on voting behavior.

Direct effects

Does social pressure and early vote mobilization efforts increase the likelihood of voters of attending the polls before the Election Day? We first tested whether receiving a mailer urging to vote early increases the likelihood of doing so. To do this, we restricted our sample to only the *contacts* in each household (62,246). Recall that we selected one individual per household to be

Figure 2: Early vote patterns in Broward County, Miami: 2010 and 2012 elections



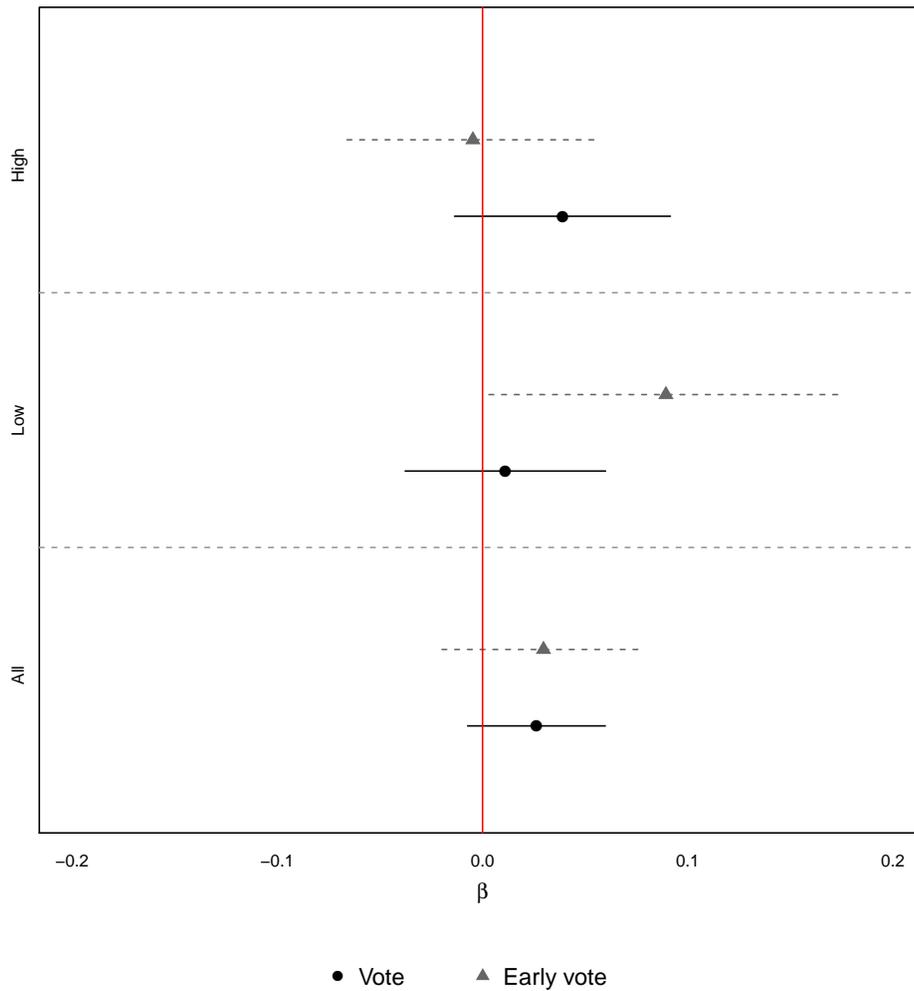
Note: Each square represents a household. % EV = proportion of members of a household that voted early. Red stars indicate the location of polling stations.

the contact to whom a mailer could be potentially directed. However, only contacts in households under treatment actually received it.

We inspect whether the mailer increased vote and early vote by comparing the rates of these two events between control and treatment groups and Figure ?? shows the results. Under this setting, we find that the effect of the treatment is not reliable: the effects of the mailer on vote and early vote are not distinguishable from zero. However, when we repeat the exercise among low and high participation individuals, we find that the effect of the treatment on early vote among the former is positive and statistically significant at conventional levels (middle row). In other words, the mailer increased early voting rates among individuals with a low turnout history, offering evidence that the mailer acted as an effective mobilization strategy. In contrast, we find that this effect is not

reliable among subjects with high participation levels (top row). These findings are in line with our theoretical expectations. On the one hand, we expect “vulnerable” individuals to be affected by the our external stimuli which provides information and exerts social pressure. On the other hand, we identify a “ceiling effect” among high participation subjects: the treatment is unlikely to change the behavior of people that a) generally comply with a variation of the activity encouraged by the treatment, or that b) already tend to vote early.

Figure 3: Direct effect of mailer on early vote, by participation levels



Spillover effects

Now we proceed to test whether the treatment has spillover effects on early vote. In other words, we are interested in analyzing whether the mailer urging to vote early mobilized subjects that were not directly contacted by our partner organization. For this purpose, we restrict the analysis to those subjects labeled as *non-contacts*. Recall that our sample includes households of sizes ranging from 2 to 4 individuals. Therefore, after excluding the *contacts*, the number of individuals per unit ranges from 1 to 3.

This characteristic of the our experimental sample poses some challenges for the analysis: our subjects are clustered within households, and the treatment is not applied individually but at the household level. Further, there are multiple directions in which the spillover effect can happen: from low participation contacts to high participation non-contacts ($L \rightarrow H$), low to low ($L \rightarrow L$), high to low ($H \rightarrow L$), and high to high ($H \rightarrow H$). To account for these issues in our analyses, we analyze the treatment effects among each spillover directionality group and include a random intercept per household. This approach implies identifying “twins” in our data (i.e. non-contacts with the same type of contact, living in households with the same number of people), that only differ in the treatment group to which they were assigned. We include the random intercept per household to account for the similarities between the subjects within each of them. With this approach we guarantee a proper identification of the causal effect of the mailer on voting behavior.⁸

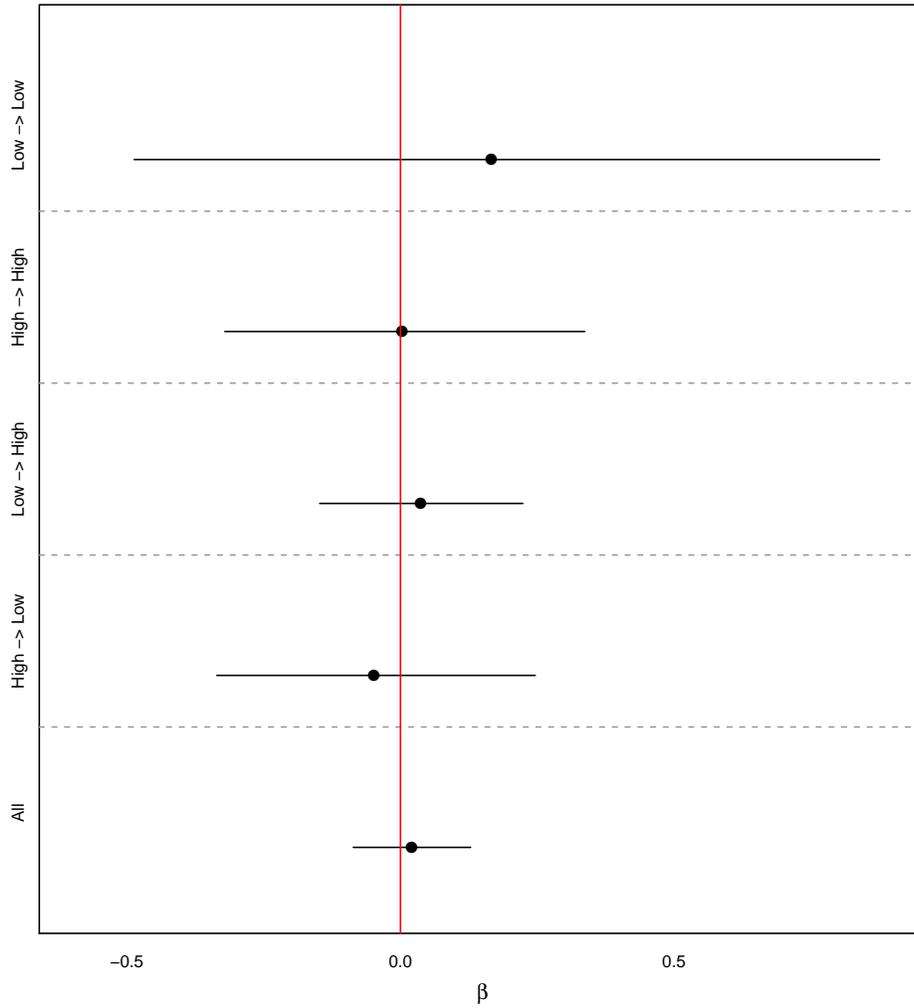
The results from these analyses are presented in Figure 4. Each row of the figure shows a direction of the dispersion effect. The points illustrate the coefficients of the treatment in the models described above.⁹

We do not find support to reject the hypothesis of null treatment effects in any of the models,

⁸Further, given the clustered nature of the data we also conduct a Bayesian Hierarchical Model with intercepts and covariates at the household level (including the treatment). The results from this analysis are presented in the Appendix. These are in line with the main findings, offer information about the effect of the covariates at the group level, and also provide another strategy for the analysis of multilevel designs as the one in this article.

⁹We conduct three sets of models: a basic model with just the treatment, a second one that includes size of the household as a covariate, and a third one that includes subject’s characteristics (age, ethnicity/race, gender, vote history), county, whether the subject shares a last name with the contact, and age difference between the two as covariates. Figure 4 shows the coefficients from the basic + household size models.

Figure 4: Dispersion effects of mailer on early vote, by directionality



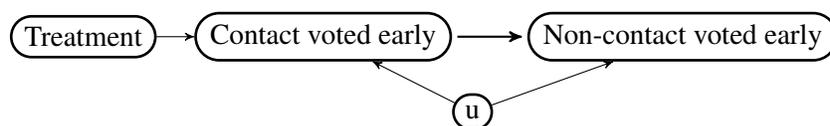
suggesting that there are no spillover effects. The result might be surprising but raises a crucial aspect in the analysis of network dynamics and spillover effects: is it possible for a subject to “spread” a behavior to which she does not comply? In simplified terms, we suggest that a subject cannot be “contagious” if she is not “sick”, and in our particular research context, a subject cannot drive her network members to vote early if she does not vote early in the first place. This is in line with our theoretical expectations regarding the relationship between credibility of signals and capacity to influence others.

For our analysis, we define “contagious” subjects as those contacts who actually voted early

(13% of the contacts, 4% belong to the control group and 9% to the treatment group). This can also be understood as an analysis of compliance and it poses two major challenges: one, compliance issues are hard to address in field experiments, and two, as Figure 5 shows, the indicator of whether the contact in a particular household voted early is clearly a post-treatment variable and controlling for it might lead to biased results. First, the design of the experiment allows us to partially deal with identification of compliance. While we cannot be fully sure that a particular contact received and read the mailer, we have information on whether he complied with the message. It is fair to assume that the probability of receiving and spreading the message is higher if he evidences the behavior that the treatment aimed to activate. Second, we use an instrumental variables approach to explore the dispersion effects of a contact who voted early on whether her household mates also voted early. In terms of descriptive statistics, among the voters assigned to control, 87% “complied” and did not vote early. On the other hand, among the treatment group 13% attended the voting centers before election day.

Figure 5 shows the endogenous relationship between the electoral behavior of contacts and non-contacts (as noted by the common confounder u). Therefore, in order to identify a dispersion effect (i.e. non-contacts voting early as a result of a contact voting early) we need a variable that predicts whether the contact voted early, but uncorrelated with the error term u . The randomization of the treatment assignment acts as a good candidate for such instrument. It is uncorrelated with the error term given that it was randomly assigned, and as it was shown in the Direct effects section, it increases the early vote rates of contacts with low levels of participation. Therefore, we restrict the analysis to those households with a Low type contact.

Figure 5: The contagious effects of voting early



A challenge of this approach is the multi-member nature of households. Therefore, we implement a multilevel instrumental variable approach. In the first stage we model whether a

household contact voted early as a function of the treatment (direct mailer), age, and gender. Recall that the number of observations in this first stage is equal to the number of households with a low type contact.¹⁰ Further, the second stage models whether the non-contacts voted early and takes the predicted values from the first stage as a covariate, indicators of social distance, their interactions with the predicted values, and gender and age as exogenous covariates. The reasoning for including social distance and an interaction with the treatment is that we expect the dispersion effects, if any, to differ depending on the distance between the contact and the non-contact. The question is how to conceptualize and measure distance in network terms? We propose two ways of measuring social distance between the contact and the non-contacts in each household: first, we include an indicator of whether the contact and non-contact share a last name to capture family ties, and a second variable capturing the absolute difference in the ages between those subjects. For this second stage, we implement a linear mixed effects model with random intercepts per household. Formally, we define the stages as follows:

First stage:

$$y = \text{Contact voted early}_j = \beta_0 + \beta_1 \text{Treatment}_j + \beta_2 \text{Sex}_j + \beta_3 \text{Age} + \epsilon_i$$

Second stage:

$$\begin{aligned} \text{Non-contact voted early}_i &= \alpha_{j[i]} + \gamma_1 \hat{y}_{j[i]} \\ &+ \gamma_2 \text{Shared name}_i + \gamma_3 [\hat{y}_{j[i]} \times \text{Shared name}_i] \\ &+ \gamma_4 |\text{Age diff.}_i| + \gamma_5 [\hat{y}_{j[i]} \times |\text{Age diff.}_i|] \\ &+ \gamma_6 \text{Gender}_i + \gamma_7 \text{Age} \\ \alpha_j &\sim \mathcal{N}(\mu_\alpha, \sigma_\alpha^2) \\ \hat{y}_j &= \hat{\beta}_0 + \hat{\beta}_1 \text{Treatment}_j + \hat{\beta}_2 \text{Sex}_j + \hat{\beta}_3 \text{Age} \end{aligned}$$

¹⁰Results from the first stage are presented in the Appendix.

The results are presented Table 1. We conduct each of the models in the full sample of households with a low type contact based on results presented above that indicated that treatment assignment is an adequate and strong instrument.¹¹ We find that that the effect of a contact voting early on the non-contact is not distinguishable from 0. However, we find that this factor has a reliable effect on non-contact early vote under certain conditions. More specifically, we find that such effect is conditional on sharing a name with the contact, and on age difference between the contact and non-contact. In other words there are spillover effects of mobilization when the person directly contacted shares a name with the non-contact and complies with the mailer message and votes early. Similarly, social distance as measured by age difference also moderates the effect of the contact behavior: the effect of a contact voting early is greater when the difference in ages between the contact and the non-contact increases. This is, the greater the social distance the stronger the spillover effect and influence of the contact on the non-contact. These results shed some light on the way in which social distance shapes mobilization effects, but also call for a more accurate and deeper conceptualization and measurement of social distance. Further, as we theorize, a “car pooling” effect might be behind these findings: a contact sends a credible and unusual sign of active participation and convinces the rest of her household members to join her and attend the polling station early, when the social distance is short.

Mechanisms of early voting

Throughout this article we have presented several findings that support the existence of direct and spillover effects of mobilization strategies. However, in this section we are interested in analyzing the mechanisms through which this occurs. In order to achieve this, we investigate the factors associated with early voting. The approach does not rely on the experimental design of our study but takes advantage of our full dataset of voters which includes demographic characteristics, information on the households in which they live, and their voting behavior (up to the 2012 election). We

¹¹The F-statistic is 20.85, and the treatment is reliably associated with contact vote early at conventional levels. Detailed results are presented in the Appendix.

Table 1: Dispersion effects when the contact voted early (second stage of 2SLS)

	Non-contact early vote
Contact voted early	−0.213 (0.268)
Match name	−0.062* (0.022)
Age difference	−0.002* (0.001)
Female	0.015* (0.003)
Age	0.002* (0.0001)
Contact voted early × Match name	0.748* (0.260)
Contact voted early × Age difference	0.031* (0.008)
Constant	0.104* (0.024)
Observations	59,374
<i>Note:</i>	*p<0.05

Table 2: Factors associated with early voting

	Days before election day
Contact voted early	0.199* (0.042)
Age difference	0.001 (0.001)
Shared last name	-0.039 (0.035)
Distance to closest center	-0.00001 (0.00001)
High participation contact	0.161* (0.038)
N	17,035

focus on the sample of non-contacts, and the variable “Day of vote” to construct an indicator of “Time of vote” in number of days before the election (ranging from 0=vote the day of the election, to 8=vote the first day of the early vote period).

The model includes three dimensions of distance: social, using shared name and age difference as components; civic, whether the contact in the household voted, and geographic, the distance in miles to the closest voting center from a subject’s household. Our model takes as outcome days before the election (when the vote was submitted) and the measures of distance explained above. The results are presented in Table 2.

The results indicate that only the civic distance is reliably associates with early votes: a high participation contact, and a contact that voted early are correlated with earlier votes of non-contacts. While this approach does not allow us to properly test the mechanisms through which contagion occurs, it offer hints on how distance, as interpreted in a multidimensional matter, affects network dynamics. In this case, voters in households that maintain a norm of voting are more likely to cast an earlier ballot. This accords with what we see in the experimental results as well: voters are likely to respond to cues from their housemates to vote early (if we can successfully modify their behavior) and voters are most likely to respond to cues if they share a last name from the cue-giver. This is to say, we observe significant evidence that shared voting patterns are more

attributable to shared norms than to shared costs.

Conclusion

In this project, we evaluate the impact of a mailer on early voting decisions for subjects who are contacted directly or indirectly. We have three main findings. First, we observe an increase in the rate of early voting only among those who had been less likely to cast a ballot (the low propensity voters). To the extent to which voting is habitual, this is not overly surprising – the individuals we contact who have a long history of participation are perhaps less willing or interested in changing their participatory habits to vote early. Yet, it is somewhat remarkable that a low propensity voter can be mobilized to vote early, in person, as a consequence of a single piece of mail. Second, we observe a limited amount of contagion. Early voting is contagious if the direct contact is voting early. That is, there are spillover effects of mobilization when the person directly contacted complies with the mailer and votes early. Third, we observe this effect is most frequently present when the spillover relationship shares a last name but not when the spillover relationship has a large age discrepancy. We then guess, but cannot know, that these spillovers are more likely to occur among subjects who are married than they are among subjects who have a parent-child relationship.

When we look at the patterns of early voting in our observational data, we do see earlier voting based upon whether a housemate voted early or whether a housemate is a high propensity voter. That is to say, we do observe patterns suggesting shared norms within households. That households have shared norms surrounding political participation is surely not surprising; but again, patterns of shared behavior could also be attributed to shared costs.

We find the most powerful component of our findings is that contagion is limited by whether the contact has "contracted the disease" of early voting – that voting is only contagious if the direct contact has voted early. That the exposure needs to be sincere is in line with our theoretical expectations of contagion if the social norm of voting is driving the results more than simply cost

sharing political participation. This result is supported by the finding that spillover is more likely among individuals who share last names.

That spillover is driven by social norms speaks volumes to our understanding of political participation. We often think of participatory choices as highly rational and individually-based; these are supposedly calculations that derive from an individual's best political interests. But what if, instead, most individuals are simply trying to maintain the political norms of their social environments? To this end, then, the most persuasive act is indeed getting a low propensity voter to switch their behavior; in early voting, this behavior can be noticed and the behavioral change itself can persuade others.

The push towards increasing rates of convenience voting has largely been driven by conversations about the desire to decrease the cost of voting. Indeed, this is a lofty aim. One concern about increasing convenience voting, however, has been that it could undermine the social environment that supports voting – the flurry of activity on of Election Day, with its new media coverage and patriotic stickers and polling place American flags might in itself build social infrastructure to support a social norm of voting. Our paper suggests otherwise. Early voting might indeed make voting more convenience and less costly for voters who would otherwise be unlikely to participate. Moreover, mobilizing those voters has the capacity to generate more than one vote in the ballot box.

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Supplemental Materials

Treatment Mailer

Below is the text for the treatment mailer. The "friends and family" addition is labeled at the beginning of the paragraph. The text enclosed in brackets (< >) was completed for each individual voter, such as name, address, county election official, and EIPV locations in each county.

The mailing was printed on heavy paper, then mailed by folding the paper in thirds and closing the letter with a tab. The address of the recipient, return address for our partner organization, and the bulk mail permit were printed on the outside of this self-mailing letter.

Text

<Full Name>

<Address>

<City>, <State> <Zip>

<Mailing Date>

Dear <First Name>,

We want to thank you for <Recent Voting Action>.

Official public records from the <Local Election Official> show that you <Recent Voting Action> and we want to thank you. Our democracy relies on exercising the right to vote in this election.

The <Local Election Official> official voting records are public information that show whether you cast a ballot, but not who you voted for. We appreciate your commitment to voting. We hope that you will continue your record of exercising your civic duty by voting in the important election on Tuesday, November 6th.

In this election, you can vote early between October <Day1> and November <Day2>. Many <Descriptive Group> will be voting early. I encourage you to join them. Voting on your own time makes casting your ballot in this election easy and convenient.

[Friends & Family treatment addition] The <Local Election Official> official voting records also show other people are registered to vote at your address. Please encourage them to join you in voting early. By getting your friends and family to vote early you can make a difference in this election.

As a registered voter in <County Name>, you can vote at the <County Name> early voting location between October <Day1> and November <Day2>. You can find information about the early voting location and hours, including any last minute changes, at <Local Election Official URL>. The early voting location and hours of operation are listed below.

Thank you in advance for voting in this important election. I look forward to thanking you after the election for voting and making our democracy work.

Sincerely,
<Signature>
Name of Signer

Early Voting Locations and Hours:
<EIPV Location>
<EIPV Address>
<EIPV Date & Hours>

The <Name of Partner Organization> is a nonprofit, nonpartisan organization that encourages citizens to vote and does not endorse any candidate or political party. If you have questions or comments about our work, you can contact us at earlyvoting@<partner organization>.org.

Balance tests: demographics by treatment assignment

Table 3: Descriptive statistics of contacts by treatment group

Category	Control	Treatment
Mean age	45.670	45.783
Female	0.572	0.571
Male	0.427	0.428
Unknown gender	0.001	0.002
White	0.266	0.265
Black	0.066	0.067
Hispanic	0.643	0.643
Democrat	0.451	0.454
Republican	0.251	0.253
Other	0.298	0.293

Note: None of the differences are distinguishable from 0.

Heterogeneous treatment effects: mailer versions

Table 4: Effects of treatment versions

	Vote		Early vote	
	L→H	H→L	L→H	H→L
Basic Mailer	0.006 (0.028)	0.047 (0.031)	0.073 (0.050)	-0.013 (0.036)
Friends & Family Mailer	0.017 (0.029)	0.030 (0.031)	0.108* (0.051)	0.005 (0.037)
Constant	-0.534* (0.020)	0.916* (0.022)	-2.463* (0.037)	-1.505* (0.026)
N	31,298	31,148	31,298	31,148

Note: *p < .05

Instrumental variables approach: Two-stage Linear Mixed Effect Model

Table 5: First stage results: effect of treatment on contact's early vote

	Contact early vote
Treatment	0.007* (0.003)
Female	0.018* (0.003)
Age	-0.0005* (0.0001)
Constant	0.089* (0.005)
N	31,298
F Statistic	20.825*** (df = 3; 31294)

*p < 0.05