

Friends Don't Make Friends Vote: Selection and Reputation in Voter Mobilization

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Abstract: Intuition and theory argue that members of a social network should be more persuasive than strangers because of reputation effects. On the other hand, advocates are likely to reside in relatively homogeneous social circles where persuasion is not possible because of selection effects. This paper examines whether the reputation or selection effects predominate using voter mobilization as a test case. In two separate experiments, volunteers were asked to list friends with whom they would be comfortable talking about the importance of voting and encourage turnout. Each list was then randomly assigned to a treatment group that was exposed to voting discussions and a control group where no voting related discussion occurred. Voter turnout for the treatment and control groups was collected from official turnout records. Not only did the peer-to-peer mobilization strategy fall well short of the 7-10 percentage point boost in turnout generated by strangers, outreach from friends and neighbors failed to mobilize voters altogether. The conclusion is that selection effects outweigh reputation effects. To the extent that interpersonal influence encourages conformity within social networks, subtle cues rather than explicit advocacy cause homogeneity.

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Are friends, neighbors, and coworkers more persuasive than complete strangers? On the one hand, members of a social circle are known and presumably trusted. Trust and reputation effects should lead a person to be more responsive to specific requests from members of a social circle than requests from strangers. On the other hand, social circles are constructed via selection processes and tend towards homogeneity. When a person already intends to pursue a course of action, it may not be possible to *alter* the person's behavior in that direction. Thus, similarity within social circles may be due less to persuasion and more to selection. In order to determine whether friends and neighbors are more influential than strangers in a given context, the relative strength of the reputation and selection effects must be determined.

Voter turnout and mobilization is one context where this question has particular theoretical and practical importance. Social networks (Eulau and Rothenberg 1986), churches (Huckfeldt, Plutzer, and Sprague 1993), neighborhoods (Huckfeldt 1979, Giles and Dantico 1982, Gimpel, Dyck, and Shaw 2004, McClurg 2006), or social contexts more generally (Beck et al. 2004) are all found to effect political participation. Recognizing the practical value of this homogeneity, political campaigns utilize data about neighborhoods and membership in social organizations to predict supporters and target mobilization (Malchow 2003).

A common explanation for homogeneity within social networks is discussion. Political discussion is generally associated with greater participation (Lake and Huckfeldt 1998; Kloffstad 2007), but the relationship is complicated since discussion partners are selected and non-random. On the one hand, knowledge of the discussion participants should enhance reception and acceptance of messages (Mackie, Worth, and Asuncion

1990). On the other hand, homophily leads to homogeneous social circles where opposing viewpoints are unlikely to be encountered (Mutz 2006). Outside of the workplace, most of political conversation occurs among like-minded friends (Beck 1991). Discussion networks are also likely to be homogeneous with regards to important predictors of political participation such as education (Huckfeldt and Sprague 1993). Thus, when it is discovered that people with friends who participate are more likely to participate themselves (Kenny 1992), it is unclear whether discussion with friends or selection is behind the correlation.

In order to answer this question, two experiments were conducted that controlled communication within a defined social network. In both experiments, volunteers listed people with whom they would be willing to discuss the importance of voting. This list was then divided into a treatment group, whose members were encouraged by volunteers to vote, and a control group that received no requests to turn out. After the election, official turnout records were then checked to determine the percentage of the treatment and control groups that participated in the election. Since all subjects were drawn from the same social circle, the randomly constructed treatment and control groups have equal propensities to vote, and the inferences drawn are unbiased.

Voter mobilization is an excellent testing ground to compare selection and friendship effects because there is a well established literature on the effect of face-to-face canvassing. A brief conversation from a complete stranger will boost turnout by 7-10 percentage points in a wide range of settings (Gerber and Green 2000; Green, Gerber, and Nickerson 2003; Nickerson 2005; Matland and Murray 2006). If reputation effects are sufficiently strong, then the peer-to-peer mobilization strategy should boost

turnout by more than 10 percentage points. On the other hand, if selection effects predominate, then peer-to-peer mobilization campaigns may exhibit no effectiveness whatsoever. The two experiments presented in this paper strongly suggest that selection outweighs reputation for the purposes of voter mobilization.

Model and Hypotheses

Define people who actively discuss politics and encourage friends to turnout to vote as Activists denoted A_a , where $1 \leq a \leq M$. Each Activist has a set of friends and neighbors, $i \in F_a$, where $1 \leq i \leq N$. The Activist can either discuss the election with a friend, $D = 1$, or not, $D = 0$. The goal of our inquiry is to determine the extent to which discussion boosts turnout, α . Thus, the voter turnout of a friend, V_i , can be expressed through the following equation:

$$V_i = c + \alpha D_i + \mu_a + \varepsilon_i \tag{1}$$

where c dictates the average rate of turnout in the population, μ_a unobserved causes of voting within the social network, and ε_i represents individual-level idiosyncratic causes of voting (i.e., residual error).

Many scholars have used models similar to Equation 1 to estimate α , that is, the effect of discussion within a friendship network upon an outcome of interest. The empirical strategy most prominently employed within political science is the snowball survey (e.g., Huckfeldt and Sprague 1995). The outcomes of people who are mentioned as friends, $i \in F_a$ and $D_i = 1$, are compared to subjects in the sample who are not in the social network, $j \notin F_a$ and $D_j = 0$. The chief problem with this empirical strategy is all

primary survey respondents (i.e., Activists in this framework) are assumed to operate in social circles with similar patterns of behavior. That is, $\mu_a = \mu_b \forall a \neq b$. The assumption may fit better in some settings than others, but the assumption is unverifiable in the best of circumstances and could possibly bias results.

Another problem is that even within a friendship network, $i, j \in F_a$, Activists are more likely to discuss politics and encourage turnout with some friends more than others. That is, $\text{cov}(\varepsilon_i, D_i) \neq 0$. Thus, discussion is likely to be correlated with the idiosyncratic causes of voting and estimates of the effectiveness of discussion, α , are likely to be inflated.

To see how one might surmount these inferential problems, imagine that an Activist encouraged a random subset of her friends to vote. That is, the Activist behaves as if the friends are divided randomly into a treatment group, T , which receives the encouragement to vote ($D=1$), and a control group, C , which receives no encouragement to vote ($D=0$). Equation 1 could then be split into the following two equations:

$$V_{iT} = c_T + \alpha + \mu_{aT} + \varepsilon_{iT} \tag{2}.$$

$$V_{iC} = c_C + \mu_{aC} + \varepsilon_{iC}$$

The random assignment assures that $E[c_T - c_C] = 0$ and $E[\varepsilon_{iT} - \varepsilon_{iC}] = 0$. Moreover,

since the randomization occurred within the social network, $E[\mu_{aT} - \mu_{aC}] = 0$. Thus, the average effect of discussion on the likelihood friends vote can be expressed as:

$$\bar{V}_T - \bar{V}_C = \alpha \tag{3}.$$

Thus, subtracting the average rate of turnout in the control group from the average rate of turnout in the treatment group generates an unbiased estimate of α .¹

¹ While the inclusion of control variables in the analysis is not required to derive accurate estimates of α , they can be used to reduce the overall error variance and thereby tighten the standard errors associated with

From past experiments, it is known that a short face-to-face visit from a complete stranger can boost voter turnout by up to 10 percentage points (Gerber and Green 2000; Green, Gerber, and Nickerson 2003; Nickerson 2005; Matland and Murray 2006). If a brief discussion with a complete stranger can boost turnout by 10 percentage points, then what type of boost in turnout can be expected from a known and trusted friend?

Lab experiments have consistently shown that messengers with a connection to subjects are more persuasive than those without such attachments. Spouses are more apt to align their attitudes in response to one another than are dating couples or complete strangers (Davis and Rusbult 2001). Individuals view an issue as important and to retain the information provided more often when the messenger is viewed as “in-group” as opposed to “out-group” (Mackie, Worth, and Asuncion 1990). This research builds around the general assumption that subject attitudes are more likely to change when the person advocating the position is similar to the subject (Brock 1965).

Game theory provides a useful way of understanding why members of one’s own social network would be more persuasive than random strangers (see Hargreaves and Varoufakis 1995, chapter 6). When a stranger knocks on the door and provides information or requests participation in an election, the person answering the door knows nothing about the stranger except that they will probably never meet again. The interaction is a one-shot game, so there is no incentive for the participants not to play “defect” rather than “cooperate.” As a result, the person at the door should not be completely trusted.

By contrast, when a friend, neighbor, or coworker makes a request, the listener knows a great deal about the messenger and her trustworthiness. Furthermore, the

estimates of α .

listener knows that the messenger will be seen again and can be held accountable for bad information or improper requests. That is, the interaction is an iterated game where reputation effects encourage participants to play “cooperate” rather than “defect.” Thus, peers within a social network are trusted sources, whereas strangers will be viewed with suspicion.²

The literature on the two-step flow of communication finds a similar pattern in the real world (e.g. Lazarsfeld, Berelson and Gaudet 1948; Katz 1957, Robinson 1976; McClurg 2004). Individuals seek out like-minded peers as trusted sources of information for two reasons. First, the peer is a trustworthy source because she is known and has provided reliable information in the past. A stranger would not possess the same history, and the listener would attach more uncertainty to the information provided. Second, the peer is trustworthy because she possesses roughly the same material interests and ideological predispositions, enabling her to filter the news from a similar perspective. A stranger might possess very different interests and ideological beliefs, which might lead her to supply information slanted in an inappropriate manner. Again, the peer is deemed more persuasive than the stranger.

Some organizations apply the logic of trust and conduct peer-to-peer voter mobilization. Unions conduct “Labor-Neighbor” voter mobilization drives. A few churches encourage turnout among congregants and residents in the immediate neighborhood. MoveOn.org utilized friendship networks on the web to raise campaign donations, organize parties, populate rallies, educate voters, and increase turnout. The Republican Party pilot-tested peer-to-peer voter mobilization in the 2004 election. The

² Members of the social network who have proven to be unreliable in the past will obviously not be trusted.

idea that social networks can be harnessed to increase voter turnout is plausible, and civic organizations act upon it.

Returning to the model developed above, if friends and neighbors are more persuasive than strangers and better able to mobilize people within their social networks, then we should expect $\alpha > 0.1$. There is little theoretical reason to expect strangers to be equally credible to friends, $\alpha \approx 0.1$, and it would be genuinely surprising if friends were less credible than strangers, $\alpha < 0.1$. Thus, one should expect the effect of discussion by a friend to be larger than 10 percentage points and we formulate the following hypothesis:

Reputation Hypothesis: Members of social networks will be more effective than strangers at mobilizing voters ($\alpha > 0.1$).

A separate strand of the social networks literature suggests an alternative hypothesis. Homophily is a powerful force in human dynamics and many social networks do not contain a diverse array of voices (Mutz 2006). Even when presented with the opportunity to befriend different types of persons, most people will choose to form relationships with those most similar to themselves (Festinger, Schachter and Bach 1950). Selection into homogenous social networks helps to explain why members of the same social network exhibit highly correlated behaviors.

An individual's propensity to vote is highly correlated with the propensity to vote of her friends, neighbors, and coworkers. An Activist volunteering to discuss the importance of voting is likely to be the type of person who votes in every election and to reside in social networks where most people vote consistently. As a result, it may not be possible for the Activist to motivate friends, neighbors, and coworkers to vote for the

simple reason that most of them plan to vote already. Self-selection into social networks might thus render peer-to-peer voter mobilization an exercise in preaching to the converted.

This argument can be made formal by building the dichotomous nature of voting into equation 1. The actual behavior of voter turnout, V_i , is not a linear function of discussion, D_i . Instead, suppose discussion effects an individual's latent propensity to vote, V_i^* , and if that propensity is positive, the person votes. Equation 4 presents this relationship:

$$V_i^* = c + \alpha D_i + \mu_a + \varepsilon_i$$

$$\text{and } V_i = \begin{cases} 0 & \text{if } V_i^* \leq 0 \\ 1 & \text{if } V_i^* > 0 \end{cases} \quad (4).$$

If Activists operate in social circles where most people vote, then μ_a is large and positive. In such instances, the presence of absence of discussion is largely irrelevant because $V_i^* > 0$ in any case. If this condition holds broadly, then $\alpha \approx 0$.

However, social networks are rarely completely homogenous. Even in social networks where nearly everyone votes, there is likely to be the odd individual who habitually abstains. A person who is exposed to a social network where voting is commonplace (i.e., μ_a is large) and does not vote regularly, must possess individual characteristics that counter-balance the social pressure to vote. That is, ε_i must be small. If ε_i is sufficiently small, then the presence or absence of discussion explicitly encouraging turnout, D , is unlikely to push the individual over the threshold into voting. In other words, habitual abstainers residing in social networks with habitual voters are as

mobilized as possible and one more conversation about the subject will not make a difference. Since it is likely that people who explicitly discuss voting with friends (i.e., Activists) operate in high participation social networks, we formulate the following hypothesis:

Selection Hypothesis: Activists who discuss voting with friends operate in social networks where voting behavior is largely determined ($\alpha \approx 0$).

The Reputation and Selection hypotheses are not mutually exclusive and could both be true. A world where friends are more persuasive than strangers and the people who discuss voting with friends operate in social circles where mobilization is impossible is entirely plausible. However, it is of great interest as to which of these two forces predominates.

Data

Nearly all studies of interpersonal influence have difficulty disentangling the effects of interactions within the network, D , and selection into the network, μ_a . Without firm knowledge of the data generating process and good measures of all the relevant factors, providing an unbiased estimate of the effect of discussion, α , is not possible using observational data. The simple estimator for α presented in equation 3 was the result of assuming that discussion within an Activist's social network was randomized so that $\text{cov}(D_i, \mu_a) = 0$ and $\text{cov}(D_i, \varepsilon_i) = 0$. Two randomized field experiments were conducted during the 2002 Congressional elections meeting this precise requirement.

Vote for America (VFA) is a non-profit, 501(C)(3), voter mobilization organization whose goal is to engage citizens in politics through the use of peer-to-peer networking. Volunteers are contacted and asked to speak with friends, co-workers, neighbors, and other acquaintances about voting. The volunteers are also encouraged to recruit new volunteers to widen the scope of the mobilization effort. The idea is to harness the social networking of a pyramid scheme towards the civic-minded end of increasing voter turnout.

Training and oversight are important aspects of the Vote for America model. Volunteers were given extensive training regarding recruiting new volunteers, approaching voters, and filing paperwork requested by VFA. How to approach friends, neighbors, and co-workers about voting was discussed and practiced. The paperwork involved in getting pledge cards signed and returned to the main office and in documenting contacts was also explained. Volunteers were also given a toolkit that contained a wealth of information and resources. The aim of the toolkit was to ensure that volunteers would be able to answer most questions voters might ask, or at least to point the person towards the correct information.³ After reading through the material and making the presentation a few times, most volunteers were significantly better trained than the average non-partisan campaign worker. Thus, the face-to-face contact by Vote for America volunteers are “high quality” and should be maximally effective (Nickerson 2007).

³ The information provided included: a list of offices and candidates on the ballot; candidates’ issue positions; polling locations; registration deadlines and requirements; trends in voter turnout; examples of past elections decided by a few votes; contact information for the Secretary of State, the League of Women Voters, campaign headquarters for a wide variety of candidates and races, and Vote for America; synopses of research on civic participation and elections; and pledge to vote cards.

The instructions provided to volunteers on approaching people about voting were simple: be friendly; tell your own story about how and why you came to vote; ask the person to share her own thoughts about voting and participation; ask if the person has any questions and try to answer those questions; make sure the person is registered to vote; remain strictly non-partisan and never endorse a candidate or party; get the person to sign the pledge-to-vote card; be sure to follow up with the pledged voter; and send the signed pledge cards to the main VFA office. The goal of the instructions was for the encounter to be as personal and as responsive to the potential voter's needs as possible. The focus on signing the pledge card was useful from the standpoint of the experiment since it assured that every volunteer/voter relationship involved at least one face-to-face encounter. The pledge cards also generated a paper trail to ensure that contact occurred.

VFA recognized the problem of homogenous social circles and actively worked to counter act such effects. In order to increase the scope and range of the organization, volunteers were recruited from different geographic regions of the state and from a range of socio-economic strata. State organizers contacted a wide variety of civic organizations, groups, and employers to locate people with the organizational capacity, lengthy list of friends, and enthusiasm needed to serve as volunteers. At every step in the recruitment process, Vote for America sought to target people who did not regularly vote.

Most social networks do not exhibit any training and oversight of interactions between members, much less the rigor imposed by VFA. In order to measure the effect of less structured communication a parallel peer-to-peer network study was set up to run simultaneously. The parallel effort consisted of recruiting volunteers over the web to encourage a randomly selected set of friends to vote. The web-based mobilization effort

was broadly similar to VFA but differed in four key respects. First, the web-based volunteers were not trained. Volunteers were directed to a website where the purpose and requirements of the study were explained. The website offered a few links to outside voting resources, but no packets of information were directly provided. By and large, the web-based volunteers were left to their own devices and provided little guidance as to how to approach friends and neighbors about voting. Most of the instruction focused on how to follow the experimental protocol.

Second, there was no monitoring of the progress of the web-based volunteers. VFA featured an extensive oversight apparatus, whereas the web-based volunteers had no organizational structure to assist and encourage them and to assure that the mobilization occurred. Oversight was limited to a request that volunteers log back onto the website and record which friends and neighbors in the treatment group were contacted and in what manner. Of the 112 people who volunteered to participate in the study, 60% returned to report the manner in which subjects were contacted. The subjects assigned to the treatment group of volunteers that did not report contacts were probably not contacted. At the very least, there is no way of differentiating between non-reporting volunteers who did and did not execute the experimental protocol. Thus, the analysis will only include web-based volunteers who returned to the website and provided information on the nature of their contacts.

The third difference between the web-based study and VFA is that the web-based volunteers were not asked to have friends sign pledge-to-vote cards. Pledging to vote has a diminutive effect upon voter turnout (Smith, Gerber, and Orlich 2003), but does ensure that the volunteer met face-to-face with the voter at least once. In contrast, not all the

web-based volunteers met with members of their treatment group in person. Focusing on the subjects for whom contact information exists, 63% were contacted at least once in person, 26% received a phone call, and 11% received an email from the volunteer.⁴

These different forms of contact matter because the technologies employed have demonstrated unequal effectiveness in boosting voter turnout. Face-to-face blandishments have been shown to increase turnout by up to 10 percentage-points across a wide variety of political settings (Gerber and Green 2000; Green, Gerber, and Nickerson 2003). However, phone calls have been shown to be less effective, boosting turnout by only 3 to 5 percentage-points (Nickerson 2007a; Nickerson 2006; Nickerson, Friedrichs, and King 2006). Email messages have not demonstrated any ability to boost voter turnout (Nickerson 2007b). While every subject assigned to the treatment group in the VFA study should exhibit a boost of the order of magnitude associated with face-to-face turnout, slightly less than two-thirds of the web-based study treatment group received the same quality of contact. As a result, the two-stage estimator outlined in Angrist, Imbens, and Rubin 1996 will be employed to account for the different forms of contact making the two studies comparable.

The final difference between the web-based and Vote for America studies is geography. The VFA experiment took place only in the state of Rhode Island, which maintains a statewide voter file that records voter turnout. The web-based volunteers were scattered across 18 states. The subjects in the experiment were even more widely dispersed, residing in 39 different states. Collecting voter-history information for all 39 states divided into many more jurisdictions was impractical. The results from the web-based experiment are restricted to voters from 20 states. This variety of states does not

⁴ Only 1 subject in the treatment group was reported as receiving no contact whatsoever.

pose a problem for inference since the subjects in each state were equally likely to be assigned to the treatment and control groups. As a result, the states are balanced between treatment and control, and the geographic diversity enhances the external validity of the findings.

Although the web-based experiment was not monitored as closely as the Vote for America experiment, it is nevertheless an informative experiment. The universe of subjects in the experiment is clearly defined (i.e., persons listed by the volunteers). The assignment to the treatment condition was fully random and provided to the correct subjects. Collection of data on the dependent variable (i.e., did the subject vote or not) utilized official records, thereby avoiding potential bias from self-reports of voting behavior. All of the theoretical requirements for a field experiment are satisfied. The lack of oversight was intended to serve as a counterpoint to and comparison of the VFA model. If Vote for America successfully mobilized voters but the parallel web-based effort did not, interested non-profit groups could surmise that training and monitoring of volunteers was important to success. On the other hand, if both efforts were equally successful at mobilizing voters, groups could conclude that people civic-minded enough to volunteer to discuss voting with friends and neighbors should require little supervision, enabling the model to be exported very easily. As it turns out, neither effort demonstrated an ability to mobilize voters, so the difference in supervision is largely moot.

Implementing the protocol for both experiments was simple. A volunteer logged onto a website that explained the purpose and requirements of the experiment. The site asked for the name and address of the volunteer entering the data and provide an ID

number that the volunteer could use when returning to the website. The next series of screens allowed the volunteer to enter the names and addresses of as many friends and neighbors as she felt comfortable including in the experiment. After submitting the information, the website would ask the volunteer to verify that the information entered was correct. After confirming the information, the website would list the names and addresses of the individuals assigned to the treatment group – those to be contacted by the volunteer. To ensure that contact information would be as accurate as possible, volunteers were allowed to return to the website and update contact information. Entering the data was somewhat tedious, but otherwise the process was straightforward.

A programming feature of the website deserves special mention. The website would not allow individuals to enter the name of a person who had been previously entered by another person. This simple programming feature prevented people from being assigned to both the treatment and control groups. Similarly, the website would not allow the same address to be listed twice. The goal was to avoid contamination of the control group that could occur if one member of the household assigned to the treatment group and another assigned to the control group.

The power of an experiment is optimized when dichotomous treatment and control groups are evenly balanced. However, the goal of Vote for America was to increase voter turnout; evaluation through the experiment was a secondary consideration for the activists themselves. To balance the competing claims of the research aims and of maximizing turnout, a compromise was struck whereby three-quarters of the subjects listed by volunteers would be assigned to the treatment group and only one-quarter to the control group. Since the web-based peer-to-peer mobilization effort was intended to

serve as a research endeavor from the start, no such compromise was necessary. Thus, the web-based study features a 50/50 split between treatment and control groups.

In the end, 31 volunteers from Vote for America logged onto the site and provided information for 481 subjects (see Table 1, column 2, rows 2-4). The Internet campaign had many more volunteers sign onto the website, but because not every volunteer reported contact information and voter history data was not available for all jurisdictions, the functional number of volunteers was 65. The web-based volunteers listed an average of six friends for a total of 374 subjects to be included in the experiment (see Table 1, column 3, rows 2-4).

While random assignment ensures that the treatment and control groups possess an equal propensity to vote on average, it is possible that any particular allocation into the treatment conditions may be an outlier. Before analyzing the results of the experiment, therefore it is important to double-check the balance between the treatment and control groups on observable characteristics. In Rhode Island, the treatment and control groups were equally likely to be registered and to have voted in the 2000 Presidential election (see Table 1, column 2, rows 5-8). In contrast, the treatment group in the web-based experiment appears to have contained a higher percentage of registered voters than the control group (see Table 1, column 3, rows 5-6). Members of the treatment group were also slightly more likely to have voted in local elections in 2001. However, neither the 8 percentage-point difference in voter registration nor the 3 percentage-point difference in voter turnout crosses traditional thresholds of statistical significance, so there is no reason to suspect a problem in the randomization process. However, the reader should keep in

mind this slight imbalance when interpreting raw the differences between the treatment and control groups presented in the next section.

Results

Analysis of voter mobilization experiments begins by comparing the rates of voter turnout for the treatment and control groups. In the Rhode Island Vote for America experiment, 31% of the subjects assigned to the treatment group voted in the 2002 Congressional election, compared to 34% of the subjects in the control group (see Table 2, column 2). Thus, the initial estimate is that the peer-to-peer campaign was not effective at mobilizing voters. Since the standard error for the -3 percentage-point estimate is 5 percentage-points, it is possible that the Rhode Island campaign did boost turnout and that the negative result is due to sampling variance. However, it appears extremely unlikely that the true effectiveness of peer-to-peer mobilization was equal to or greater than the 8 to 10 percentage-points demonstrated by stranger-to-stranger face-to-face mobilization (Gerber and Green 2000; Green, Gerber, and Nickerson 2003). If the two types of mobilization were equally effective, a result of -3.3 with a 5 point standard error would occur by chance only 1 time in 100.

The intent-to-treat results from the web-based peer-to-peer campaign are slightly more encouraging because 23% of the treatment group voted, compared to a 22% rate of turnout for the control group (see Table 2, column 3). However, the difference is less impressive when the reader remembers that the treatment group featured an 8 percentage-point advantage with regards to voter registration over the control group (see Table 1,

column 3). This observed imbalance can be corrected when the intent-to-treat effect is adjusted by the contact rate to estimate the efficacy of face-to-face contact.

Table 3 reports the results from a two-stage least squares estimation of the effect of the web-based experiment. Subjects' voting behavior is regressed upon actual face-to-face discussion, using the random assignment as an instrument for discussion. The two-stage least squares process also allows for control variables such as past voting history to be included in the analysis.⁵

The first model, which does not control for past voter history or party membership, estimates the effectiveness of face-to-face contact from a volunteer in the web-based experiment to be 4.8 percentage-points (see Table 3, column 2). That is, each person reached in person and encouraged to vote is 4.6 percentage-points more likely to vote as a result of the contact. Alternatively, if the experiment contacted 1,000 individuals, 46 new votes would be created. This effect remains well below the 8-10 percentage-points associated with a stranger knocking on the door, but it is certainly better than no votes created at all.

Once voter history is included in the model (see Table 3, column 3), however, the mobilization effect disappears. The second model estimates that face-to-face blandishments to vote among neighbors decrease turnout by 2 percentage-points. Moreover, past voter history is an excellent predictor of voting behavior, so there is less residual variance in the model and standard errors are tighter. The likelihood of the results from model 2 being randomly drawn from a distribution with a true effect size of 8 percentage-points is only 2 in 100. Controlling for registration in one of the two major

⁵ A two-stage bivariate probit model can also be used in this situation to capture the dichotomous nature of the dependent variable. The results obtained are substantively the same as the linear model presented. Two-stage least squares is presented purely for clarity of exposition.

political parties reduces the estimated effect on turnout even further to -4.6 percentage-points (see Table 3, column 4). That is, for every 100 face-to-face contacts made by volunteers in the web-based experiment, 4 or 5 *fewer* subjects voted. According to this third model, the probability of face-to-face contacts in the Internet campaign being as effective as stranger-to-stranger contacts are only 7 in 1,000. The web-based peer-to-peer campaign did not create any votes and was less effective than traditional canvassing techniques.

Similarly, this pessimistic conclusion does not change when covariates are added to the Rhode Island Vote for America experiment (see Table 4). Controlling for turnout in the 2000 Presidential election lowers the estimated effect of contact slightly from -3.0 percentage-points to -3.8 percentage-points (see Table 4, column 3). Adding two-party registration to the analysis raises the estimate back to -3.2 percentage-points – very near the initial difference between the treatment and control groups (see Table 4, column 4). The standard error associated the 3.2 percentage-point decline in turnout is 3.3 percentage-points. A positive mobilization effect from peer-to-peer encouragement to vote cannot be definitively ruled out in the Rhode Island experiment. However, it is almost certain that the Vote for America model tested in the experiment is less effective than traditional stranger-to-stranger canvassing efforts. An eight percentage-point treatment effect would randomly generate the results in Table 4 only 3 times in 10,000.

Neither the Vote for America nor the web-based peer-to-peer experiment demonstrated effectiveness at mobilizing voters. Pooling the results from the two experiments only reinforces that belief. Using precision-weighted averages, one would estimate that contact from peer-to-peer voter mobilization campaigns lowered rates of

turnout by 3.6 percentage-points. The standard error associated with the estimate is only 2.8 percentage-points. The upper bound of the 95% confidence interval for the estimate is just below 2 percentage-points. If peer-to-peer efforts were as successful as traditional canvassing campaigns, such a result would be observed only 1 in 100,000 times. The peer-to-peer voter outreach campaigns studied might be minimally effective, but it is clear that past stranger-to-stranger canvassing were superior at encouraging voter turnout. These findings provide strong evidence that any reputation effects in voter mobilization are outweighed by selection effects.

The selection argument modeled above makes a very clear prediction regarding types of voters. If selection is driving the null finding, one would expect the sample to be bifurcated. On one end, people who have voted in the past should continue to vote at very high levels. On the other end, people who have abstained in the past should continue to abstain from voting. The past voter history information collected in the Vote for America experiment and the web-based campaign can address this issue. Table 5 separates the subjects into abstainers and voters.

In both experiments, the expected pattern emerges. People who voted in the past were extremely likely to vote in the 2002 Congressional election, regardless of whether they were assigned to the treatment or the control group. In addition, people who abstained in past elections were unlikely to vote in 2002. The distribution of the data is thus consistent with the selection hypothesis.

Discussion

The finding that peers are less effective at mobilizing voters than strangers is surprising. Both laboratory experiments and observational studies strongly point towards the expectation that friends and neighbors would be far more influential than strangers. Given the excellent training and perfect contact rate in the Vote for America study (and the relatively high contact rate in the Internet study), the null finding is a surprise. The argument advanced in this paper is that selection effects outweigh any reputation effects for the purposes of voter turnout.

The model developed in Equation 4 suggests an explanation and a method of successfully harnessing the social networks to engage voters. Mobilization is not possible because most Activists reside in social circles with high propensities to vote. If activists were recruited from social circles where voting is uncommon (i.e., μ_a is small), then explicit discussion about voting among friends might boost turnout. Thus, if the strategy would succeed anywhere, the model predicts it would occur among low participation groups such as young people and minority populations.

An alternative explanation for the finding that friends and neighbors are less effective at mobilizing voters than strangers is that it is true: friends and neighbors *are* less effective. Perhaps neighbors resent the use of the personal relationship as an entry into a sales pitch. Since friendships are built upon trust, if asking someone to vote is viewed as a betrayal of that trust, then one would not expect familiarity to enhance the persuasiveness of contact.

If the betrayal argument is true, this would have important implications for theories of interpersonal influence. Attitudinal and behavioral contagion would not occur

through direct and open conversations. Lobbying on behalf of a position would be counter-productive. Any convergence that occurred would be the result of passive absorption rather than any explicit attempt at persuasion. The existence of such a passive mechanism for peer effects is highly plausible. However, the reverse claim that explicit endorsement of positions and attempts to persuade are ineffective defies too much of existing theory to be taken seriously without further evidence.

A skeptic could also argue that while the experiment offers internal validity, the treatment provided in this experiment did not resemble interactions that occur in daily life. That is, the prescribed treatment might have been awkward and artificial. In many cases, volunteers would have known subjects for many years and never once mentioned the importance of voting. The sudden initiation of a conversation about voting – particularly one that did not advocate a specific position or cause – might have struck the experimental subjects as unusual or even jarring. Three of the volunteers in the web-based experiment expressed discomfort about the process of discussing turnout with friends and neighbors. After establishing a lengthy history with a person, bringing up the patriotic duty to vote could be construed as insulting to many people, perhaps by implying that they are bad citizens. In other words, her reputation and relationship with the subject – the very thing one would expect to enhance persuasion -- may be working against the volunteer in the context of the experiment. In contrast, a stranger at the door is playing a role. The person answering the door expects nothing from the canvasser and is willing to believe the canvasser cares about voter turnout. No offense is taken at the encouragement to vote precisely because the canvasser is a complete stranger and knows nothing about the person answering the door. Friends and neighbors might still carry

more weight than strangers, but blandishments to vote may be more acceptable and effective coming from someone who does not know you personally.⁶

Three responses can be offered to this criticism. First, the web based experiment placed as few strictures on the interaction between volunteer and voter as possible. The goal was for the interaction to be organic and nature. Second, if awkwardness between volunteer and voter is a problem, then the entire peer-to-peer mobilization strategy is doomed to failure. The artificiality is inherent in the model itself and the two experiments present a discouraging evaluation of the mobilization strategy.

Finally, the conclusion that friends and neighbors cannot efficaciously encourage each other to vote has important implications for interpersonal influence. Explicit discussions and requests are not the driving mechanism behind some forms of behavioral contagion. Thus, peer pressure is most effective when the demands of conformity are subtle and unstated.⁷

The bottom line is that peer-to-peer efforts showed no success at motivating people to vote in these two experiments. In contrast, experiments involving contact by strangers have demonstrated a remarkable ability to move voters to the polls on Election Day. Both the selection and awkwardness hypotheses have the ring of plausibility and both cast serious doubt upon many findings in the contemporary literature on interpersonal influence. Some observational studies inquire as to whether the respondent

⁶ Perhaps relaxing the requirement that the contact be non-partisan could enhance the naturalness of the interaction. If so, the peer-to-peer voter mobilization model might be effective for political campaigns and ineffective for non-partisan get out the vote efforts. However, some evidence against this possibility surfaced in the web-based experiment. In the web-based experiment, volunteers were asked to report whether they encouraged voting for a particular candidate, party, or ballot initiative. 22% of the volunteers reported advocating support for at least one of the three. Since no mobilization effect was detected in the Internet campaign either, the partisan component is probably not sufficient to make the interaction comfortable. Limiting the sample to the 22% advocating a position does nothing to change this conclusion, but the sample size is so small as to be essentially uninformative.

⁷ Unless explicit endorsements only fail for voter turnout and work in other settings.

has discussed politics with friends. The selection explanation would suggest that respondents are discussing politics with likeminded individuals who have similar habits. The awkwardness hypothesis would suggest that discussions aimed at persuading someone are unlikely to be met with success. In either case, the homogeneity of opinion and behaviors found by observational surveys are unlikely to be due to discussion that occurred between friends. That is, the results from such studies are epiphenomenal. Future research should be conducted to adjudicate between these two hypotheses.

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Table 1 Description of Peer-to-Peer Voter Mobilization Campaigns

	Rhode Island Vote for America	Internet Campaign
Number of Volunteers	31	65
Number of Subjects in the Treatment Group	361	185
Number of Subjects in the Control Group	120	189
Percent of Treatment Subjects Registered at Address	61%	51%
Percent of Control Subjects Registered at Address	60%	43%
Percent of Treatment Subjects Voting in Prior Election	22%	8%
Percent of Control Subjects Voting in Prior Election	22%	5%

Prior election for Rhode Island was the 2000 Presidential election.

Prior election for the Internet campaign was 2001 local general elections.

Table 2 Summary of Peer-to-Peer Mobilization in Rhode Island and Internet

	Rhode Island Vote for America	Internet Campaign
Percent of Treatment Group Voting in 2002 General Election	30.7% [361]	23.3% [189]
Percent of Control Group Voting in 2002 General Election	34.2% [120]	21.6% [185]
Difference	-3.4% (4.9)	1.7% (4.3)
95% Confidence Interval	(-13.1 ↔ 6.2)	(-6.8 ↔ 10.2.)

Numbers in brackets report N's.

Numbers in parentheses report standard errors.

Table 3 Effect of Peer Face-to-Face Mobilization for Internet Campaign during 2002 using Two-Stage Least Squares

	Model 1	Model 2	Model 3
Discussion	0.048 (0.064)	-0.022 (0.051)	-0.046 (0.051)
Voted 2002 Primary		0.588** (0.063)	0.503** (0.065)
Voted 2001 General		0.481** (0.084)	0.427** (0.083)
Member of Major Party			0.190** (0.045)
Volunteer Dummies	Yes	Yes	Yes
Constant	0.412** (0.149)	0.268* (0.121)	0.219 (0.118)
N	374	374	374
Adj-Rsq	0.13	0.43	0.47

Dependent variable is voter turnout in the 2002 general election.

Coefficients are derived from two-stage least squares.

Treatment assignment was used as an instrument for face-to-face discussion.

Numbers in parentheses represent standard errors.

* implies $p < 0.05$ and ** implies $p < 0.01$

Table 4 Effect of Peer-to-Peer Mobilization during 2002 General in Rhode Island

	Model 1	Model 2	Model 3
Discussion	-0.030 (0.048)	-0.038 (0.035)	-0.032 (0.033)
Voted 2000 General		0.758** (0.039)	0.614** (0.041)
Member of Major Party			0.275** (0.033)
Volunteer Dummies	Yes	Yes	Yes
Constant	0.224 (0.205)	0.078 (0.152)	-0.062 (0.143)
N	481	481	481
Adj-Rsq	0.06	0.48	0.55

Dependent variable is voter turnout in the 2002 general election.

Coefficients are derived from ordinary least squares.

Numbers in parentheses represent standard errors.

* implies $p < 0.05$ and ** implies $p < 0.01$

Table 5 Comparing Mobilization for Past Abstainers and Past Voters.

	Rhode Island		Internet Campaign	
	Past Voters	Past Abstainers	Past Voters	Past Abstainers
Treatment Group	90.0%	13.9%	86.7%	11.3%
Turnout 2002	[80]	[281]	[30]	[159]
Control Group	88.5%	19.1%	81.8%	13.5%
Turnout 2002	[26]	[94]	[22]	[163]
Difference	1.5%	-5.3%	4.8%	-2.2%
	(6.9)	(4.3)	(10.3)	(3.7)

Numbers in brackets represent cell counts.

Numbers in parentheses represent standard errors.

Past voters in Rhode Island voted in the 2000 Presidential election.

Past Voters in the Internet campaign voted in either a 2001 local election or the 2002 primaries.