Social Pressure, Descriptive Norms, and Voter Mobilization

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Abstract   Several recent field experimental studies show that social pressure raises the likelihood of turning out to vote in elections. Ratcheting up social pressure to show subjects their own as well as their neighbors' prior voting history significantly increases the effectiveness of direct mail messages. A key component in stimulating this effect seems to be the presence of individual vote history. When voters are presented with less specific turnout information, such as vote history for the community at-large, the effects on turnout often dissipate. Sensitizing voters to such descriptive norms appears to do little to stimulate participation. To address this contrast, this study presents results from a voter mobilization field experiment conducted in Hawthorne, CA prior to the November 2011 municipal elections. The experiment is a fully crossed 2 × 3 factorial study in which subjects were randomly assigned to one of six conditions, in which they receive no mailing, a mailing with individual vote history only, a mailing with individual vote history and a message emphasizing high (or low) community-level turnout from a previous election, and a mailing emphasizing high (or low) community-level turnout only. County voter files were used to randomly assign voters to treatment and control and to report the effects of each mailing on voter turnout. We find that only messages that included information about subjects' own voting histories effectively mobilized them to vote.

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After nearly 15 years of steady progress, beginning with the foundational work of scholars are beginning recognize clear patterns in voter mobilization research. Psychological and political studies demonstrate that social pressure strongly affects the degree to which individuals comply with social norms like the civic duty to vote in elections. Individuals whose social networks include voters are more likely to vote themselves (Huckfeldt and Sprague 1995), and attempts to mobilize others in informal conversations can successfully increase political participation (Klofstad 2007). Voting appears to be "contagious" among members of a household; Nickerson (2008) provides experimental evidence that when one household member is mobilized by a get-out-the-vote (GOTV) campaign, other household members also become more likely to vote. Indeed, since 2008, studies abound showing the powerful effect of social pressure on the likelihood of turning out to vote (see Gerber et al. 2010; Green et al. 2010; Panagopoulos 2010, 2011, 2013, forthcoming). Moreover, social pressure effects are not limited to interactions between people with strong relationships or to in-person conversations (e.g. Gerber et al. 2008) and can endure long-term, over multiple election cycles (Davenport et al. 2010).

Evidence from psychological studies however indicates the effect of social pressure is conditional on a number of factors: the salience of the social norm, the pre-existing propensity of the person being pressured to engage in the behavior, the degree to which the subject's norm-compliant behavior is observed by others, and the degree to which the subject believes others comply with the norm (Cialdini and Goldstein 2004). Voter turnout experiments have begun to shed light on how such factors can be leveraged to increase the effectiveness of mobilization efforts (e.g. Green and Gerber 2008; Nickerson 2008; Gerber et al. 2008; Larimer 2009; Mann 2010; Panagopoulos 2010, 2011, 2013, forthcoming). For example, Panagopoulos (2011) shows that a message expressing gratitude to voters for having voted in the past can significantly increase the effectiveness of a turnout message. Importantly, Panagopoulos observes significant positive effects without displaying one's own vote history. One intriguing possibility is raised by Gerber and Rogers (2009). In two sets of field experiments, the authors manipulated the degree to which subjects believed other members of their communities turned out to vote through a mobilization phone call. Registered voters in California and New Jersey who received calls emphasizing high turnout in the state were 3.5 % points more likely to report that they were certain they would vote compared to those who received a message emphasizing low state-level turnout. In other words, messages providing general information about a community's overall sense of civic duty produced significant positive effects on turnout.

Descriptive and Injunctive Norms, Social Pressure and Voting Behavior

Gerber and Rogers (2009) based their intervention on the psychological literature on descriptive norms. A social norm like civic duty can be communicated in two ways: as a descriptive norm, which communicates others' compliance (i.e., "most people vote") or as an injunctive norm, which communicates the social rule without reference to others' behavior (i.e., "you should vote"). In other domains of behavior beyond politics, both descriptive and injunctive norms encourage norm compliance (Reno et al. 1993), but descriptive norms are often found to be more powerful. Individuals' beliefs about descriptive norms are especially strong predictors of behavioral outcomes (Nolan et al. 2008). Studies that communicate descriptive norms in experimental settings have shown they increase compliant behavior in prosocial activities including littering (Cialdini et al. 1990), recycling (Cialdini 2003), hotel towel reuse (Goldstein et al. 2008), and removing natural artifacts from a forest (Cialdini et al. 2006). Furthermore, when both forms of the norm are simultaneously communicated, but the descriptive norm contradicts the injunctive norm (i.e., "you should not litter, but most of your neighbors do"), the effectiveness of the injunctive norm is decreased (Cialdini et al. 1990, 2006). Conversely, when a consistent descriptive norm is added to an injunctive norm, it can increase the effectiveness of the injunctive norm (Göckeritz et al. 2009).

Though many turnout experiments include language implying descriptive norms [e.g. "Why do so many people fail to vote?" (Mann 2010, p. 403)], few test the effect of such an intervention systematically. The Gerber and Rogers (2009) study gives us good reason to expect that including a descriptive norm element in a social pressure message delivered via direct mail should increase the effectiveness of GOTV messages. But, as the authors acknowledge, their analysis is limited to self-reported vote intentions collected at the end of the mobilization phone call. Scholars have shown that there is a tendency to over-report the act of voting when surveyed (Burden 2000; but see McDonald 2003). Furthermore, their study is limited to messages delivered by phone. Mass mailings are among the least expensive GOTV technique, and thus more common in the field, so it is of particular value to figure out how they can be made more effective.

The effect of descriptive norms on voter turnout is further complicated by findings reported by two recent studies using direct mail. The first, by Nickerson and White (2009), tests whether providing information about street-level turnout (either high or low) in a previous election increases turnout among low-propensity voters. Nickerson and White (2009) show these descriptive norms have no effect on voter turnout compared to a very basic message appealing to one's responsibility to vote. The effects of descriptive norm messages remain insignificant even when references to turnout rates are group-specific (e.g. black voters). This finding contradicts the significant findings reported by Gerber and Rogers (2009). It is worth noting that, unlike Gerber and Rogers (2009), Nickerson and White (2009) use direct mail to administer their treatments and measure turnout using the official state voter file, a point to which we return in the conclusion.

Similar to Nickerson and White, Matland and Murray (2011) are interested in whether information about community-level turnout affects the likelihood of voting.

Specifically, the authors test whether descriptive norm messages affect voter turnout when additional information, either consistent or inconsistent with the norm, as well as social pressure, is also presented. The results, taken from field experiments in two states, paint a complicated picture for the interaction between descriptive and injunctive norms. In one setting, Matland and Murray find that descriptive norms do significantly affect turnout, but only when high turnout is described and expected, the latter of which is operationalized by a statement about what experts are expecting for the upcoming election. The inclusion of social pressure (individual vote history) with descriptive norm messages produces mixed results. For mailings sent 8 days prior to the election, social pressure has no effect; mailings sent 4 days prior to the election show a significant positive effect for the inclusion of social pressure. In a second setting, however, Matland and Murray find no differences in the timing of the message and that descriptive norms emphasizing both low and high turnout are equally effective at boosting turnout. In contrast to previous studies, there is no added effect for the inclusion of individual vote history.

The summaries we present above imply the impact of descriptive norms on voter turnout remains in question. Gerber and Rogers (2009) and Nickerson and White (2009) yield competing answers about the effects of descriptive norm messages on voting behavior, while Matland and Murray (2011) find mixed results depending on location and the timing of the message. In the current study, we address this contrast with results from a randomized voter mobilization field experiment conducted in Hawthorne, CA prior to the November 2011 municipal elections in which voters received social pressure mobilization messages including negative, positive, or no descriptive norms about community-level turnout. Like all of the studies discussed above, we include the basic injunctive civic duty norm in all treatment conditions and observe whether adding consistent or contradictory descriptive norms bolsters or overpowers the injunctive norms as it does in other behavioral domains (Cialdini et al. 1990, 2006; Göckeritz et al. 2009).

In addition to providing conflicting findings about the main effects of descriptive norms on turnout, previous studies have not addressed whether the inclusion of descriptive norm elements improves the effectiveness of other types of effective mobilization messages. By applying an intensive form of social pressure through a mailer that shows individuals their own prior voting history, informs them that voting is a matter of public record, and shows the vote history for all registered voters in a household, Gerber et al. (2008) demonstrate that a direct mail message can effectively exert social pressure and increase turnout by 4.8 % points on average. The average effect of this treatment does not seem to vary based on the salience of the election context (Larimer 2009). Lacking from both the Gerber and Rogers (2009) and the Nickerson and White (2009) treatments is the inclusion of individual vote history social pressure. Matland and Murray (2011) include vote history, but the results across the two locations in the field experiment were inconsistent or inconclusive. We build on these studies, and on psychological work on descriptive and injunctive norms, by testing whether it is possible to increase the effectiveness of a social pressure mailing on voting behavior by adding descriptive and injunctive norm elements to a message that stresses voting is a matter of public

record. The second element we manipulate in our experiment is the inclusion of subjects' own, prior vote history.

The above-noted gaps in the extant research are more than just theoretical. There are also important practical implications. For political operatives, improving the cost-effectiveness of voter mobilization tactics is critical to mounting successful campaigns. More cost effective mobilization methods are used more widely in the field, and thus studying them produces more practically useful and externally valid knowledge. Previous studies have shown that ratcheting up social pressure to include neighbors' voting records significantly increases the effectiveness of direct mail messages (see Gerber et al. 2008).[1] This study extends previous research by examining whether adding a single piece of information about community-level voter turnout to information on individual vote history is a more effective method for getting people out to the polls.

Hypotheses

Our goals are to test (1) whether messages that impart information about descriptive norms increase turnout, and (2) whether descriptive norms increase the effectiveness of social pressure messages already known to improve turnout. Our fully-crossed, 2 × 3 factorial experimental design (summarized in Table 1), allows us to system-atically test hypotheses about both questions. First, we expect individual vote history, the intervention used by Gerber et al. (2008) to manipulate voters' perception of social pressure, to produce significant increases in voter turnout. A key component in the strong effects observed in Gerber et al. (2008) appears to be the presence of individual vote history. In both the "Self" and "Neighbors" mail treatments, individuals were shown their vote history for two previous elections. Importantly, this vote history was based on actual turnout from the statewide Qualified Voter File. This intervention provides voters with information about their voting history and lets them know that their turnout behavior is being observed. In these two treatment conditions, observed turnout was 4.8 and 8.1 % points higher, respectively, compared to those subjects who were not assigned to receive these postcards. Compared to typical nonpartisan direct mail treatments including only an injunctive norm message, for which effects are nonsignificant or marginal, such effects are quite remarkable (Green and Gerber 2008). Indeed, the effects for the two treatments in the Gerber et al. (2008) study that included individual vote history are significantly greater than for the two treatments in which subjects were not shown vote history (these elevated turnout by only about 1–3 % points on average compared to the control group). Therefore, we expect voters assigned to the three treatment groups for which the mailers include individual vote history to show significant increases in voter turnout as compared to the control group. We expect to see this effect regardless of whether the message included a descriptive norm element.

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[1] Gerber et al. (2008) report that mailings including individual vote history along with the vote history of all registered voters in a household as well as the vote history of neighbors, including a warning that voting behavior will be revealed publicly, increase turnout by 8.1 % points on average.

Table 1 Study design

	Individual vote history	No individual vote history
High community turnout	Self + high community turnout	High community turnout only
Low community turnout	Self + low community turnout	Low community turnout only
No community turnout information	Self only	Control

Second, though the literature is more divided here, we also expect descriptive norms, in the form of information about community-level turnout, to affect the likelihood of voting. As we discussed above, psychologists studying norm compliance outside of politics repeatedly find that descriptive norms affect behavior. And Gerber and Rogers (2009) offer evidence that the same is true for voting behavior. Therefore, we expect that individuals who receive the descriptive norm message in the high community turnout treatment condition, described below, will show increased turnout, compared to the control condition, regardless of whether the message includes their own voting history. We expect the highest turnout among those who receive their own voting history as well as the high community turnout message.

Finally, to be consistent with prior turnout experiments,[2] we include a basic injunctive civic duty norm message in all interventions. This decision allows us to examine a final question. The psychological literature has shown that when both forms of the norm are simultaneously communicated, but the descriptive norm contradicts the injunctive norm (e.g., "you should not litter, but most of your neighbors do"), the effectiveness of the injunctive norm decreases (Cialdini et al. 1990, 2006). Therefore, we expect that voters in the low community turnout condition should vote at a lower rate than their counterparts in the high community turnout condition. We test these hypotheses using a randomized field experiment described next.

Study Population

The complete experimental sample was comprised of 18,482 registered voters residing in single-voter households in Hawthorne, California. The municipal general election in Hawthorne on November 8, 2011 featured contests for mayor and members of the city council. The election was nonpartisan. In the mayoral race, Daniel Juarez, a Hawthorne city councilman since 2007 and finance manager, was elected with 50.5 % of the vote. His opponents, mayor pro-tem Alex Vargas and Dwan Fulwood, a local entrepreneur and federal civil servant retiree, received 41.3 and 8.2 % of the vote, respectively. There were also nine candidates competing for two seats on the city council. Ultimately, Nilo Michelin, a member of the school board and teacher, and Olivia Valentine, a retired federal prosecutor, were elected with 23.3 and 18.9 % of the vote, respectively. Overall turnout was approximately 16 % of registered voters. Voters were randomly assigned to either the control

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[2] See, for example, Gerber et al. (2008), Mann (2010), and Panagopoulos (2010).

Table 2 Relationship between treatment group assignment and covariates (mean levels)

Experimental conditions	N	Voted (Nov 2008)	Voted (Nov 2006)	Voted (Nov 2004)	Age (years)	Male	Partisan
Self + community high	1,000	64.4	29.2	43.1	27.5	39.9	80.4
Self + community low	1,000	66.8	30.2	44.4	28.0	38.4	83.2
Self only	1,000	64.0	26.9	40.0	28.0	40.1	78.9
Community high only	1,000	64.7	29.3	42.6	26.8	41.1	81.0
Community low only	1,000	64.7	27.8	41.3	27.0	41.9	80.3
Control	13,482	64.7	27.8	42.7	27.5	41.7	81.2
$p > F^a$		.83	.45	.43	.55	.31	.24

Figures in columns represent mean percentages unless otherwise indicated

[a] Test statistics generated using one way ANOVA to evaluate whether mean turnout levels differ across categories of random assignment. In all cases, we cannot reject the hypothesis of equal means at standard significance levels ($p < .05$), implying balance across groups

group or to one of five treatment groups described in the following section.[3] Voters assigned to the treatment groups were sent a postcard mailing within the week prior to the election.

We confirm that random assignment generated treatment and control groups that were balanced in terms of observable characteristics by conducting a series of randomization checks. The results provided in Table 2 present mean turnout levels for three prior general elections (in 2004, 2006 and 2008) as well as gender, age and partisan registration and confirm the randomization exercise yielded experimental groups that were balanced with respect to these attributes. Balance can also be confirmed statistically using multinomial logit to predict experimental assignment as a function of the six covariates. As expected, a likelihood ratio test with 30 degrees of freedom (6 covariates times 5 treatments) is nonsignificant (LR = 27.06, $p = .62$).

Treatments

Each individual assigned to one of the five treatment groups received one of five types of mailings shown in the Appendix. The mailings were mailed using first-class postage approximately 5 days prior to the November 2011 municipal elections in Hawthorne, CA. All mailings were nonpartisan, and each type of mailing was two-color, and printed on 4 × 6 postcard stock.

Voters assigned to the first treatment group, labeled "Self + High Community Turnout," received a mailing reminding people that voting is public record and implying that someone would be observing whether they vote in the upcoming election. The mailing included columns for their vote history for the 2006 and 2008 general elections with the word "Voted" next to their name for each election if they

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[3] Despite resource limitations that constrained the sizes of our experimental treatment samples, we note that our experiment was designed to be adequately powered (at the power = .80 level) to detect treatment effects in the 2-percentage point range.

voted, or "Did Not" if they abstained. A third column for the upcoming 2011 election was also included. In addition to individual vote history, voters were given information about turnout in their community. For the High Self Turnout treatment group, individuals were told, "THE MAJORITY OF YOUR NEIGHBORS DO THEIR CIVIC DUTY. DO YOURS TOO." Following this statement, individuals were told "TURNOUT IN YOUR COMMUNITY: 70 %."[4]

Individuals assigned to the second treatment group, labeled "Self + Low Community Turnout" received a similar mailing as those assigned to the High Self Turnout group. The exception was the information relayed about turnout in their community. Those assigned to the Low Self Turnout treatment group were told, "THE MAJORITY OF YOUR NEIGHBORS DO NOT DO THEIR CIVIC DUTY. BUT YOU SHOULD DO YOURS." Following this statement, individuals were told "TURNOUT IN YOUR COMMUNITY: 35 %."[5]

The third treatment group, labeled "Self Only" treatment group, received a standard mailing with individual vote history for the 2006 and 2008 general elections with the word "Voted" next to their name for each election if they voted, or "Did Not" if they abstained. This mailing was similar to the "Self" mailing used in a previous study (see Gerber et al. 2008). Voters were told, "We are reminding people that who votes is a matter of public record." A third column for the upcoming 2011 election was also included. Voters were told, "We intend to mail you an updated chart when we that information is available."

The final two treatment groups, "High Community Turnout" and "Low Community Turnout," only contained information about community turnout. No individual vote history was included. Voters in the "High Community Turnout" treatment group were told that the turnout in their community was high using the same script as for the first treatment group ("THE MAJORITY OF YOUR NEIGHBORS DO THEIR CIVIC DUTY. DO YOURS TOO. TURNOUT IN YOUR COMMUNITY: 70 %"). Those in the "Low Community Turnout" treatment group were told that turnout in their community for a previous election was low using the same script as for the second treatment group ("THE MAJORITY OF YOUR NEIGHBORS DO NOT DO THEIR CIVIC DUTY. BUT YOU SHOULD DO YOURS. TURNOUT IN YOUR COMMUNITY: 35 %"). The purpose of the last two treatment groups is to test whether group-level information, i.e. the descriptive norm, is enough to spur significant increases in voter turnout. The community-turnout statistic for these two groups is identical to that reported in the previous two treatment groups.

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[4] Though many studies of descriptive norms in the psychological literature employ stronger treatments, with perfect or nearly perfect compliance, we were concerned about compromising the credibility of the turnout message. Rather than deploy artificial or inflated turnout rates, we use actual turnout levels from two recent elections in Hawthorne, CA: 70 % of registered voters cast ballots in the November 2008 elections, and 35 % voted in November 2006, based on reported turnout in the voter file obtained for this study. We note further that the 70 % compliance figure is also comparable to the level (71 %) Gerber and Rogers (2009) use in their California study.

[5] As we note above, the actual turnout levels used in this study were selected and assigned by the researchers. Notwithstanding our efforts to randomly manipulate subjects' perceptions about turnout level norms in their community, we acknowledge that some subjects may have been more precisely aware of actual turnout patterns in recent or comparable election cycles. Such awareness would have been randomly distributed across conditions.

Table 3 Experimental results

Experimental conditions	N	Turnout (%)	Intent-to-treat (ITT)
Self + community/high	1,000	12.6	+2.0 (1.0)**
Self + community/low	1,000	13.7	+3.1 (1.0)***
Self only	1,000	12.0	+1.4 (1.0)*
Community/high only	1,000	9.7	−.9 (1.0)
Community/low only	1,000	10.7	+.1 (1.0)
Control	13,482	10.6	

Standard errors in parentheses

*** Statistical significance at the $p < .01$ level, ** at the $p < .05$ level, and * at the $p < .10$ level, using one-tailed t tests

Results

Following the November 2011 general election, we obtained validated voter turnout data from the official Hawthorne, California voter file. Table 3 reports the basic turnout rates for the group of subjects assigned to each treatment condition.[6] The control group in the experiment voted at a rate of 10.6 %. Turnout amongst voters assigned to receive the postcard that included the voter's actual turnout in recent elections ("self") in addition to a descriptive norm that suggested high, community-level turnout in prior elections was significantly higher (12.6 %), suggesting a turnout boost of 2.0 % points (SE = 1.0).[7] The effect is significant at the $p < .05$ level using a one-tailed test. Subjects assigned to the "self" condition that included low, community-level turnout in previous elections treatment voted at a rate of 13.7 %, implying a statistically-significant (at the $p < .01$ level, one-tailed) intent-to-treat (ITT) effect of 3.1 % points (SE = 1.0) on average. Subjects assigned to receive the version of the mailing that included only the "self" component (with no mention of descriptive norms for community-level turnout) voted at a rate 12.0 %, suggesting a significant (at the $p < .10$ level, one-tailed) boost in turnout of 1.4 % points relative to the control conditions.

   The results presented in Table 3 also show that turnout among subjects assigned to receive postcards that included only descriptive norms about prior, community-

[6] We acknowledge that a substantial portion of California voters are signed up for permanent vote-by-mail (absentee) status. We did not exclude these voters from the experiment we conducted, but we expect, given random assignment, absentee voters to be evenly distributed across experimental conditions. As expected, we detect no differences in the rates of absentee voting across experimental conditions in our experiments (Scheffe multiple-comparison test is insignificant, $p > F = .35$; details available upon request). However, if subjects had voted by absentee ballot in advance of our treatments, they could not have been affected by the intervention. Such failure-to-treat would only magnify the estimated intent-to-treat effects we report.

[7] We acknowledge that some subjects assigned to be treated may not have been successfully contacted, but reliable estimates of contact rates for direct mailings are unavailable. Thus, we report intent-to-treat effects throughout, noting these are likely conservative estimates of the treatment effects. Taking contact rates into account would only magnify the treatment effects we report.
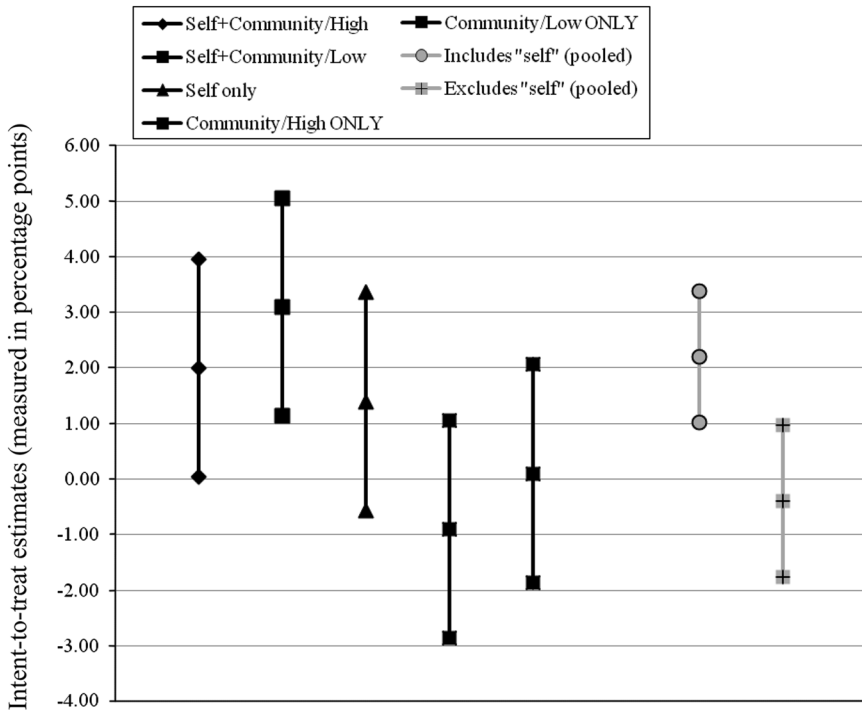
Fig. 1 Intent-to-treat estimates with 95 % confidence intervals for individual treatment conditions and pooled estimates for self and non-self conditions. 95 % confidence intervals are based on ITT estimates without covariates included in the model

level turnout voted at rates that were statistically indistinguishable from the control group. Subjects assigned to the high, community-level turnout condition with no inclusion of actual, "self" turnout in prior elections voted at a rate of 9.7 %, slightly lower than the control group, while those assigned to the low, community-level turnout only condition voted at a rate that was almost identical to the control group (10.7 %). In both cases, the intent-to-treat effects were not significantly different from the control group at conventional levels. The intent-to-treat estimates with bars for the 95 % confidence intervals are shown in Fig. 1. Treatment conditions which combine the "self" component with high and low descriptive norm messages (two left hand bars in Fig. 1), are statistically distinguishable from the other treatments.

For more rigorous analysis of the experimental results, we use multiple regression (OLS) to obtain estimates of the treatment effects. This approach permits the inclusion of control variables to correct for imbalances between experimental groups due to chance. We estimate six models: Eq. 1 expresses individual voter turnout as a linear function of the experimental treatment conditions. The results of a linear regression in which voter turnout (Y_i) for individual i is regressed on dummy variables $\{D_{1i}, D_{2i}...D_{ti}\}$ denoting each of the treatments (in our case, five

treatments; the reference category is the control group) are presented in the first column of Table 4. This model may be written simply as:

$$Y_i = \beta_0 + \beta_1 D_{1i} + \ldots + \beta_t D_{ti} + u_i, \tag{1}$$

where u_i represents an unobserved disturbance term.

Equation 2 is embellished to include the available covariates (prior voting, age, gender and partisan registration). The inclusion of covariates is optional, but it may reduce the disturbance variance and improve the statistical precision of the estimated treatment effects. The model may be written as:

$$Y_i = \beta_0 + \beta_1 D_{1i} + \cdots + \beta_t D_{ti} + \lambda_1 V_{1i} + \cdots + \lambda_t V_{ti} + u_i \tag{2}$$

where (λ)s represent parameters associated with each covariate (V), and u_i represents an unobserved disturbance term. The results are presented in the second column of Table 4.

The regression results parallel our initial findings. The estimations reveal that subjects assigned to the three experimental conditions that included a "self" component were effectively mobilized to vote in the election, relative to the control group, while the treatments that failed to include a "self" component exerted no appreciable impact. Estimates of the intent-to-treat effects across the two specifications are quite robust; the addition of covariates (Model 2) adjusts the estimates only modestly. The results suggest that the effects of each of the three "self" treatments do not differ statistically from each other ($p = .46$); the effects of the two non-"self" treatments are also statistically indistinguishable from each other ($p = .47$). Focusing on the effects of the two versions of the treatments that included injunctive norms that contradicted the descriptive norms (the two versions with low community-level turnout), the estimates also suggest these treatments exerted somewhat stronger effects than the corresponding two versions in which the injunctive norm did not contradict the descriptive norm (the two treatments with high community-level turnout information). Although these differences are not statistically significant, they suggest the inclusion of contradictory injunctive norms may actually stimulate (rather than depress) compliance. Such a finding, if confirmed, would stand in stark contrast to what has been reported in previous research (Cialdini et al. 1990, 2006).

For a more direct comparison of the effects of the experimental treatments that included a "self" component to those that did not, we pool treatments to estimate their effects. The results, both without and with covariates respectively, are presented in columns 3 and 4 in Table 4. The estimates show that treatments that included a "self" component effectively boosted turnout in the election, compared to the control group, while those that did not failed to do so. The results imply the "self" treatments elevated turnout by about 2.0 % points on average, an effect that is statistically significant at the $p < .01$ level. We underscore the fact that the magnitude of this effect is about four times stronger than what is typical for a nonpartisan mail intervention (Green and Gerber 2008). We also note that the effect of the treatments that included a "self" component differs significantly ($p < .01$, one-tailed) from the versions of the mailings that included no "self" element. These differences are displayed in the right-hand portion (shaded in gray) of Fig. 1

Table 4 Estimated treatment effects on turnout (Hawthorne, CA, November 2011)

Condition/variable	Model specification					
	(1)	(2)	(3)	(4)	(5)	(6)
Self + high	.020** (.010)	.018** (.010)				
Self + low	.031*** (.010)	.025*** (.010)				
Self only	.014* (.010)	.015* (.010)				
High only	−.009 (.010)	−.009 (.010)				
Low only	.001 (.010)	.003 (.010)				
Includes self (pooled)			.022*** (.006)	.019*** (.006)		
Excludes self (pooled)			−.004 (.007)	−.003 (.007)		
Self					.014* (.010)	.015* (.010)
High					−.009 (.010)	−.009 (.010)
Low					.001 (.010)	.003 (.010)
Self × high					.015 (.017)	.012 (.016)
Self × low					.016 (.017)	.008 (.016)
Covariates[a]	No	Yes	No	Yes	No	Yes
N	18,482	18,479	18,482	18,479	18,482	18,479
RMSE	.311	.292	.311	.292	.311	.292

Estimates represent intent-to-treat effects derived from OLS regression. Dependent variable is voter turnout in the November 8, 2011 general election in Hawthorne, CA. Numbers in parentheses represent standard errors

*** Statistical significance at the $p < .01$ level, ** at the $p < .05$ level, and * at the $p < .10$ level using one-tailed tests

[a] Covariates include: prior turnout in the 2008, 2006, and 2004 general elections (November), age, gender and partisan registration. See Table 1 for details

showing the 95 % confidence intervals for the pooled treatments with and without "self" in the treatment condition. Treatment conditions with the "self" element are statistically distinguishable from those without this element.

An alternative modeling approach facilitates a more direct test of the effectiveness of self conditions both with and without the inclusion of norm elements.[8] Instead of entering each of the five treatment conditions as dummy variables (as models 1 and 2 do), we leverage the 2 × 3 nature of the treatment variables in our experiment. We estimate a regression model that includes a dummy variable for the self-information variable and separate dummy variables for each of the two levels (high and low) of the norm variable. Accordingly, all subjects in a "self" condition were assigned to be exposed to their own prior voting history; all subjects in a "high" descriptive norm condition were assigned to be exposed to a community-level, prior turnout rate of 70 % condition; and all subjects in a "low" descriptive norm condition were assigned to be exposed to a community-level prior turnout rate of 35 %. We also include interactions of these variables. The results of these estimations (models 5 and 6) are presented in columns 5 and 6 of Table 4. Consistent with the findings reported above, we find no evidence that the inclusion of norm elements moderated the impact of the self component; neither of the two interactions are statistically significant at conventional levels, implying norm references exerted no magnifying effects. Only the dummy variable denoting a "self" element was included in the treatment is statistically significant at a conventional level. We interpret this result to reinforce our overall claim that systematic effects for norms are elusive in the current study.

Conclusion

The results of our study confirm our first expectation. As other scholars have found (Gerber et al. 2008), including individual vote history in a mailing effectively boosts turnout. The estimated effects we detect for the pooled "Self" treatments of about 2.0 % points represent an improvement over the baseline rate of turnout (10.6 %) of about 19 %. This is remarkably consistent with the estimated improvement in turnout, over the baseline rate of voting of 29.7 for the "self" treatment in Gerber et al. (2008), of about 16 %. When baseline differences in turnout rates are taken into account, our findings are nearly identical. These results differ from the mixed finding on social pressure shown in Matland and Murray (2011), where treatment effects vary by the locale and number of days prior to the election in which the mailings were delivered. We would suggest such differences may be a function of context as well as the nature of the treatments. Recall that the text of our treatments more closely resembles treatments administered in previous studies, and the sample size in our study is considerably larger.

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[8] We are indebted to an anonymous reviewer for this suggestion.

However, our results provide little evidence for the anticipated effects of descriptive norms on turnout. Here, our results are in line with the findings from the large-scale study by Nickerson and White (2009) and stand in contrast to Gerber and Rogers (2009) and Matland and Murray (2011). There are however, important caveats. Like Matland and Murray (2011) and Nickerson and White (2009), our treatments were delivered via direct mail, while Gerber and Rogers (2009) used live phone calls. Descriptive norms may take on more importance when delivered in a more personal manner. The effect of descriptive norms may also be sensitive to electoral context. The results from Gerber and Rogers (2009) were from a competitive gubernatorial election and a competitive gubernatorial primary. Turnout in each election was 48 and 39 %, respectively. The significant effects for positive descriptive norms without social pressure from Matland and Murray (2011) were taken from a statewide gubernatorial election. Our treatments were administered during a municipal general election in which turnout was 10.6 %. It may be that descriptive norms take on more importance during high-salience elections. This possibility raises speculation about the generalizability of the findings of the current study, but more research is necessary to explore this more fully.

Although not statistically indistinguishable, the impact of the self treatments which included information about community-level turnout was slightly higher than the self-only treatment version. Future studies may wish to examine whether this is a statistical artifact of our sample population or if a real difference exists between such treatments. An obvious extension of this work is to test what effects the inclusion of other types of information or messages have on the likelihood of voting when combined with individual vote history. For example, would positive (or negative) information about the electoral context (competitive or not competitive) or the physical environment (a lot of traffic and rainy compared to the ease of voting and a sunny forecast), when combined with individual vote history, affect the likelihood of voting?

From a theoretical vantage point, the findings we report do not conform to the expectations generated from psychology literature on norm compliance. While most of this literature shows that messages that others conform to a social norm increase compliant behavior, the same does not appear to be true for turnout. In this instance, political behavior does not conform to expectations generated from other domains of behavior. It is not clear why voting behavior is less responsive to descriptive norms compared to other behaviors like recycling (Cialdini 2003). Though there are numerous potential reasons for this null result (limited statistical power, the strength of the intervention), it is possible that voting, especially in low-salience elections, is simply less sensitive to normative communication. In other contexts, such as environmental conservation, informing individuals about descriptive norms is effective when the prosocial behavior is convenient and low-cost. Voting may be more demanding than other behaviors that have been studied, and thus more difficult to manipulate. Similar studies intended to reduce college students' binge drinking have produced mixed results, with some reporting null effects (Werch et al. 2000) potentially because modifying drinking behavior in college is more difficult or less desirable that other behaviors that have been studied. Alternatively, since the

salience of a social norm dictates the effectiveness of normative communication, there may be something unique about voters' understanding of social voting norms or their salience in certain election contexts, as we discuss above. Future studies should investigate these possibilities.

In short, this study demonstrates that evidence about others' compliance with a social norm, in this case the act of voting, only affects the likelihood of compliance when the information is personalized. What seems to matter to voters is an indication that someone or some entity is monitoring their voter record, and the thought of such observation increases their chances of going to the polls.

Appendix: Treatment Mailings

Treatment 1: Self + High Community-Level Turnout

Dear Registered Voter:

NOVEMBER 8, 2011 IS ELECTION DAY IN HAWTHORNE, CA!

Voters will vote for mayor and members of the city council.

DO YOUR CIVIC DUTY AND VOTE!

We are reminding people that who votes is a matter of public record. One the front side of this postcard, we show your name from the list of registered voters, showing past votes, as well as an empty box which we will fill in to show whether you vote in the November 8 election. We intend to mail you an updated chart when that information is available. Below we also show the percent of registered voters in your community who turned out to vote in a recent election.

THE MAJORITY OF YOUR NEIGHBORS DO THEIR CIVIC DUTY.

DO YOURS TOO.

	Nov 2006	Nov 2008	Nov 2011
[FIRSTNAME LASTNAME] [STREET ADDRESS]	[VOTED/DID NOT]	[VOTED/DID NOT]	_____

TURNOUT IN YOUR COMMUNITY: **70%**

Treatment 2: Self + Low Community-Level Turnout

Dear Registered Voter:

NOVEMBER 8, 2011 IS ELECTION DAY IN HAWTHORNE, CA!

Voters will vote for mayor and members of the city council.

DO YOUR CIVIC DUTY AND VOTE!

We are reminding people that who votes is a matter of public record. <u>On the front side of this postcard</u>, we show your name from the list of registered voters, showing past votes, as well as an empty box which we will fill in to show whether you vote in the November 8 election. We intend to mail you an updated chart when that information is available. <u>Below</u> we also show the percent of registered voters in your community who turned out to vote in a recent election.

THE MAJORITY OF YOUR NEIGHBORS DO NOT DO THEIR CIVIC DUTY.

BUT YOU SHOULD DO YOURS.

	Nov 2006	**Nov 2008**	**Nov 2011**
[FIRSTNAME LASTNAME] [STREET ADDRESS]	[VOTED/DID NOT]	[VOTED/DID NOT]	_____

TURNOUT IN YOUR COMMUNITY: **35%**

Treatment 3: Self Only

Dear Registered Voter:

NOVEMBER 8, 2011 IS ELECTION DAY IN HAWTHORNE, CA!

Voters will vote for mayor and members of the city council.

DO YOUR CIVIC DUTY AND VOTE!

We are reminding people that who votes is a matter of public record. <u>On the front side of this postcard</u>, we show your name from the list of registered voters, showing past votes, as well as an empty box which we will fill in to show whether you vote in the November 8 election. We intend to mail you an updated chart when that information is available.

DO YOUR CIVIC DUTY AND VOTE!

	Nov 2006	**Nov 2008**	**Nov 2011**
[FIRSTNAME LASTNAME] [STREET ADDRESS]	[VOTED/DID NOT]	[VOTED/DID NOT]	_____

Treatment 4: High Community-Level Turnout Only

Dear Registered Voter:

NOVEMBER 8, 2011 IS ELECTION DAY IN HAWTHORNE, CA!

Voters will vote for mayor and members of the city council.

DO YOUR CIVIC DUTY AND VOTE!

We are reminding people that who votes is a matter of public record. <u>Below</u> we show the percent of registered voters in your community who turned out to vote in a recent election.

THE MAJORITY OF YOUR NEIGHBORS DO THEIR CIVIC DUTY.

DO YOURS TOO.

TURNOUT IN YOUR COMMUNITY: 70%

Treatment 5: Low Community-Level Turnout Only

Dear Registered Voter:

NOVEMBER 8, 2011 IS ELECTION DAY IN HAWTHORNE, CA!

Voters will vote for mayor and members of the city council.

DO YOUR CIVIC DUTY AND VOTE!

We are reminding people that who votes is a matter of public record. <u>Below</u> we show the percent of registered voters in your community who turned out to vote in a recent election.

THE MAJORITY OF YOUR NEIGHBORS DO NOT DO THEIR CIVIC DUTY.

BUT YOU SHOULD DO YOURS.

TURNOUT IN YOUR COMMUNITY: 35%

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