

Descriptive Social Norms and Motivation to Vote: Everybody's Voting and so Should You

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The fact that many citizens fail to vote is often cited to motivate others to vote. Psychological research on descriptive social norms suggests that emphasizing the opposite—that many do vote—would be a more effective message. In two get-out-the-vote field experiments, we find that messages emphasizing low expected turnout are less effective at motivating voters than messages emphasizing high expected turnout. The findings suggest that descriptive social norms affect vote intention only among citizens who vote infrequently or occasionally. Practically, the results suggest that voter mobilization efforts should emphasize high turnout, especially when targeting occasional and low rate of participation voters. More generally, our findings suggest that the common lamentation by the media and politicians regarding low participation may undermine turnout.

In the weeks leading up to the 2004 Presidential election, Women's Voices Women Vote (WVWV), a leading political organization, sent out approximately 1 million mailings aimed at increasing political participation by single women. The WVWV mailings presented only one fact about turnout: "4 years ago, 22 million single women did not vote." The fact that many citizens fail to vote is often cited to motivate others to vote. But does this common message actually increase participation? There is no evidence that it does, and recent work in social psychology suggests that, while an intuitively appealing attempt to tap feelings of civic responsibility, this persuasion strategy might be counterproductive. A growing literature on social norm perception shows that a person's behavior conforms to her beliefs about what people actually do in a given situation. These beliefs about how other people act, also known as descriptive social norms, have been shown to exert a powerful influence across a range of behaviors. Applying this logic to political participation yields the following proposition: to increase turnout, a persuasive message should employ a "high turnout" message that millions of people vote, rather than a "low turnout" message that millions of people stay home.

This prediction about the superior effectiveness of a "high turnout" message relative to a "low turnout" message is extrapolated from research on the

effects of descriptive social norms regarding prosocial behavior such as littering or recycling. There are several reasons why past findings might not apply to voting. First, economic reasoning suggests that anticipating higher turnout might cause a citizen to be less likely, not more likely, to participate. The probability that one's own vote will be pivotal in determining an election's outcome is reduced when many others plan to vote (Aldrich 1997; Downs 1957). Even if one's vote were not the pivotal vote, in a low turnout election each vote has a greater impact on the margin of victory than in a high turnout election. Thus, to the extent that margin of victory affects a political agent's power, or what Light calls political capital (1999), a vote cast in a low turnout election will be of greater political importance than a vote cast in a high turnout election. Second, political activists, who have real-world experience trying to affect voting behavior, are uncertain as to whether it is more effective to craft a voter mobilization message that creates an expectation of high turnout or an expectation of low turnout. A small survey performed at the 2004 Democratic National Convention asked 30 self-identified experts in voter mobilization which of two messages would be more effective at encouraging young citizens to vote, one stating that turnout among the young is relatively low and/or decreasing or another stating that turnout among the young is relatively high and/or increasing. Respondents were

closely divided over which approach was best, with 43% favoring the low turnout approach, and 57% favoring the high turnout approach (Rogers 2005). Whether a particular activist favors a high turnout or low turnout approach, it is clear that messages emphasizing low turnout are widespread and perceived by many to be an effective mobilization technique. Third, while there have been convincing demonstrations of the power of descriptive social norms in a variety of contexts, the mechanisms by which this effect occurs have not been well established. It is therefore unclear whether existing demonstrations of the impact social norms have in some contexts will necessarily apply to the voting context. In sum, if the theory of descriptive norms is found to apply to voting, this would demonstrate success in a new and difficult area for the theory.

We conduct two randomized field experiments to test the effect of descriptive social norms on turnout. Respondents were called on the phone and exposed to one of two naturalistic get-out-the-vote scripts in the days prior to the November 2005 general election in New Jersey and the June 2006 primary election in California. The scripts were designed to influence participants' perceptions of whether voter turnout in the upcoming election would be high (a high turnout script, which will be referred to as an "HTO" script) or low (a low turnout script, which will be referred to as an "LTO" script). That is to say that the scripts were intended to influence participants' perceptions of the descriptive social norm for voting in the upcoming election. After hearing either the LTO or HTO scripts, respondents were asked if they intended to vote. If descriptive social norms work in the voting domain as they do elsewhere, the HTO scripts should increase turnout intention relative to the LTO scripts. We find that, consistent with previous research on descriptive social norms, reported intention to vote is significantly higher in the treatment groups exposed to the HTO scripts than among those exposed to the LTO scripts.

This research extends the literature on descriptive social norms in two ways. First, the influence of descriptive social norms is tested and confirmed for a new type of behavior for which there is no strong presumption the theory will prove correct. We find that those who are exposed to the HTO script are much more likely to report that they intend to vote in the upcoming election.

Second, in addition to the attractive naturalistic context that field experiments provide, the field experiments were fairly large (the combined size of the two experiments reported here is more than

3,700) and diverse. This permits analysis of subgroup differences. Using data obtained from the respondents during the phone call and data from the voter registration records available prior to the treatment, we examined how the change in turnout intention varies across respondents. We find that the relative effectiveness of the HTO script relative to the LTO script was not uniform across respondents. Given the widespread use of the LTO appeal to mobilize those who generally fail to vote, it is ironic that citizens with a weak history of prior participation respond most strongly to the HTO appeal versus the LTO appeal. In contrast, for those with a history of regularly voting, the LTO script proves just as effective as the HTO script. Many efforts to boost turnout do so by increasing voting among those on the cusp of participation, while leaving those who rarely vote unaffected. Since moving from the LTO script to the HTO script increases participation intention among voters who often fail to vote while leaving the turnout levels of frequent voters unaffected, our results suggest that a shift from LTO to HTO messages may be an overlooked strategy for increasing participation among those groups that are the most unequally represented on the voter rolls.

The literature review briefly describes relevant theoretical and empirical work on descriptive social norms and behavior and also political science and sociological research on turnout. The experimental designs section describes the sample and design of the New Jersey and California experiments. We performed two field experiments to measure the effect of influencing eligible voter's perception of voting norms. Field experiments testing voter turnout strategies have seen a resurgence in recent years (Gerber and Green 2000), and the experimental methods developed in this recent research were used in the present two field experiments. The results section first reports results for the entire sample and then reports on differential effects across subgroups. We assess some explanations for the differential treatment effects.

We then discuss the theoretical implications of our findings in which we show that if turning out to vote is more attractive when other citizens participate, there is a possibility of multiple equilibrium turnout levels. One important feature of the current study is that the dependent variable is the respondent's intention to vote rather than actual turnout. While vote intention and similar attitudinal measures have been used in prominent research on the effects of political communication (e.g., Ansolabehere and Iyengar 1996; Mutz and Reeves 2005), it would have been preferable to have a study large enough to

reliably detect differences in actual turnout. For readers especially concerned about the validity of the turnout measure, our findings may provide guidance and significant justification for the large scale replication required to detect turnout effects on election day. We conclude with a general discussion of the findings and include additional discussion of the use of vote intention as a dependent variable.

Literature Review

Social psychologists have characterized two types of social norms: injunctive and descriptive. Injunctive norms describe what individuals perceive that others typically think they should do (e.g., “You should not litter”), and descriptive norms describe what individuals perceive that others typically actually do (e.g., “Most actually do litter”). Both types of norms, when primed, tend to encourage norm-consistent behavior (Reno, Cialdini, and Kallgren 1993). This implies that including descriptive norms in persuasive appeals can have perverse effects, since actual behavior often runs counter to a community’s desired behavior (Cialdini et al. 2006). If a descriptive norm does not reflect a desired behavior (e.g., “The park is actually full of litter...”) then highlighting the descriptive norm, even if to contrast it with the injunctive norm (e.g., “. . . so please do not litter”), can actually impair the effectiveness of the appeal (Cialdini, Reno, and Kallgren 1990, Experiment 1; Cialdini et al. 2006). This is because in addition to saying “one should not litter” the message also says “lots of people do litter.”

Studies have demonstrated that descriptive norms affect behavior in a variety of real-world situations. Researchers have found that emphasizing the pro-social descriptive norm that few people litter, or most people recycle, or most people reuse their towels when staying in a hotel room has the effect of reducing littering (Cialdini et al. 1990), increasing recycling (Cialdini 2003), and increasing towel reuse (Goldstein, Cialdini, and Griskevicius in press), respectively. Another recent study showed that emphasizing the descriptive norm that many people steal petrified wood from the forest floor increases an individual’s likelihood of conforming to that norm relative to emphasizing that the vast majority of people do not take the petrified wood (Cialdini et al. 2006). In summary, emphasizing a descriptive norm, regardless of its social desirability, can increase the likelihood that an individual will behave consistently with it.

Research on the determinants of voting yields findings consistent with the notion that descriptive norms might affect turnout, but this theme is not especially prominent in recent studies. The power of group membership to influence individual political attitudes and behavior is a central theme of early voting studies, which emphasized the role of social influences on political behavior.¹ The classic studies by the Columbia school (Berelson, Lazarsfeld, and McPhee 1954; Lazarsfeld, Berelson, and Gaudet 1944) explain individual voting behavior as the product of group affiliations such as religion, ethnicity, and occupation and argue that group pressure leads to conformity.²

More common in recent scholarship is an emphasis on the power of injunctive social norms. This work considers how individuals are trained to appreciate and endorse democratic values and recognize the responsibilities of citizenship. The role of education is a major focus of this research. There is a strong positive association between educational attainment and turnout across individuals (Wolfinger and Rosenstone 1980), and the effect of education on turnout appears to be causal (Milligan, Morretti, and Oreopoulos 2004). The importance of education in producing political participation has generated a variety of theoretical explanations. These include the notion that formal schooling produces enlightened citizens who recognize injunctive norms regarding how a good citizen ought to participate in political life, injunctive norms such as political participation and respect for collective interests (Nie, Junn, and Stehlik-Barry 1996).

Standard rational choice explanations of participation focus on the costs and benefits of participation to the individual. A person will decide to vote if the expected benefits from voting exceed the costs. In the standard account the benefit from voting equals the utility difference between the preferred candidate and the alternative multiplied by the likelihood of

¹This summary draws on an informative survey of the literature on voting behavior by Fiorina (1997).

²Deviation from group norms creates discomfort for the non-conforming person. More recent work emphasizes the role of information in producing similarity within groups. There is extensive evidence that people who belong to the same groups, workplaces, and neighborhoods are more likely to share political perspectives. Huckfeldt and Sprague (1995) argue that much of political behavior can be understood by observing the social context of an individual and the consequences this has for the information about politics the individual is exposed to. They argue that individuals who live and work together become more similar as what they learn and what information they share creates more similar informational endowments than non-connected individuals.

being the pivotal vote (Downs 1957; Tullock 1968). This basic model of participation has been expanded to incorporate “civic duty,” an additional benefit from voting beyond the effect of the vote on the election outcome (Riker and Ordeshook 1968). Recent research has found that reminders of civic duty can increase the attention voters pay to politics and to the extent and style of their political deliberation (Kam 2007). The “civic duty” component in the voting calculus is the psychological benefit derived from following the injunctive norm of voting. Recently, the basic rational actor models of participation have been expanded to include additional normative motivations. These include models of turnout by altruistic voters who incorporate the utility of general others into their calculations (Edlin, Gelman, and Kaplan 2006; Fowler 2006; Fowler and Kam 2007; Jankowski 2002), socially identified voters who incorporate the interests of others in their ingroup into their calculations (Fowler and Kam 2007) and ethical voters, who vote to maximize the interests of the greater society (e.g., Coate and Conlin 2004; Feddersen and Sandroni 2006). In addition to these theoretical arguments, empirical evidence links injunctive norms regarding participation to turnout. Survey evidence shows that individuals classified as having strong feelings of civic duty are much more likely to vote (Blais 2000; Riker and Ordeshook 1968). This finding may help account for the prominence of the messages that emphasize low levels of political participation in mobilization efforts as political agents may believe that calling attention to widespread failure would lead citizens to attend to their civic responsibilities more diligently.

Experimental Design

We describe two between-subject election experiments.

New Jersey, 2005

Participants were randomly sampled from a New Jersey registered voters list which included phone numbers. Participants were called during Saturday-Monday prior to the Tuesday election in November 2005. Jon Corzine won this election by a margin of 10%. Turnout was 48%. The calls were performed by The Clinton Group, a well regarded firm specializing in preelection phone calls.

Participants heard one of two scripts written to convey that voter turnout in the upcoming election

was likely to be high (HTO) or low (LTO). Both scripts began with a short introduction. The HTO script continued with the following (120 words):

In last year’s election the vast majority of eligible New Jersey voters actually voted. It was the highest election turnout in decades. In fact, over three and a half million New Jersey citizens voted in last year’s election. That is the most ever. Many hope this trend will continue in the upcoming election. A promising sign of this is that in the primary election for New Jersey governor, just this past June, nearly 20% more New Jersey citizens voted than in the previous primary election for governor. Many political experts are encouraged by how many voters they expect to vote in the upcoming elections next week. We encourage you to join your fellow New Jersey citizens and vote this Tuesday!

The LTO script followed the introduction with this alternative appeal (110 words):

In the most recent election for New Jersey governor, voter turnout was the lowest it had been in over 30 years. Voter turnout in that election was down a full 7% from the previous Governor election. Many fear this trend will continue in next week’s election for Governor. A distressing sign of this is that in the primary election for governor, just this past June, less than 10% of New Jersey citizens actually voted. Many political experts are discouraged by how few voters they expect to vote in the upcoming elections next week. We encourage you to buck the trend among your fellow New Jersey citizens and vote this Tuesday!

After hearing the HTO and LTO scripts respondents were asked whether they intended to vote. Vote intention was measured using one of two questions. Each respondent was asked only one of the two questions. One of these measures asked how likely it was that the respondent would vote. This was modeled after a question used by the American National Election Studies to elicit less biased reports of past voting behavior (Question code: V980303). We refer to this structure of asking for self-reported intention to vote as the “standard” vote intention question:

In talking to people about elections, we often find that a lot of people are not able to vote because they were sick, they have important obligations, or they just don’t have time. How likely do you think you are to vote in the election for New Jersey Governor this coming Tuesday: Absolutely certain to vote, Extremely likely, Very likely, Somewhat likely, Not too likely, or Not at all likely?

The other measure asked respondents to provide an estimate of the probability that they would vote. This question was modeled after one developed by Dominitz and Manski (2006) for measuring expectations about pension benefits. We refer to this as the “probability” vote intention question:

Now I will ask you a question about something you might do in the future. I will ask you to say what is the PERCENT CHANCE of something happening. The percent chance must be a number from 0 – 100. Numbers like 2 or 5 percent may be “almost no chance,” 20 percent or so may mean “not much of a chance,” a 45 or 55 percent chance may be a “pretty even chance,” 80 percent or so may mean a “very good chance” and a 95 or 98 percent chance may be “almost certain.” The percent chance can also be thought of as the NUMBER OF CHANCES OUT OF 100.

In talking to people about elections, we often find that a lot of people are not able to vote because they were sick, they have important obligations, or they just don't have time. Now, what is the percent chance you will cast a vote in the election for New Jersey Governor this coming Tuesday?

California, 2006

Our second experiment was similar to the New Jersey experiment. A sample of registered California Democrats were randomly assigned to one of two scripts, to be administered by phone on the weekend prior to the Tuesday California Democratic Party Gubernatorial Primary election in June 2006. Phil Angelides won this election by a margin of less than 5%. Overall turnout was 39%. As in the New Jersey experiment, following a common introduction respondents heard either an LTO script or an HTO script. The HTO script had the following 144 words:

We would like to encourage you to vote. More and more California citizens are voting. In the last federal election the vast majority of eligible California citizens voted. It was the highest election turnout ever. More than 12.5 million Californians voted in that election. That was an increase of over 3 Million from the previous statewide election. In the last primary election fully 71% of registered California citizens voted. In fact, more California citizens voted in the last primary election than in the previous primary election, an increase in the number of voters by about 20%. In the upcoming primary election this Tuesday it is almost certain that many millions of California citizens will vote, just as millions have in the other recent elections.

We encourage you to join your fellow California citizens. Please get out and vote in the primary election this Tuesday!

The LTO script used the following alternative 137 words:

We would like to encourage you to vote. Voter turnout in California has been declining for decades. In fact, in the last 30 years turnout in primary elections has declined by nearly 40%. The last two primaries for Governor have been among the lowest turnout in California in modern times. In the most recent primary election for Governor, a meager 26% of eligible Cal-

ifornia citizens voted. That means that more than 15 Million California citizens did not vote in that election. In the upcoming primary election this Tuesday it is almost certain that many millions of California citizens will again fail to vote, just as millions have failed to vote in other recent elections.

We encourage you to buck this trend among your fellow California citizens. Please get out and vote in the primary election this Tuesday!

A survey followed the HTO and LTO scripts. Participants were randomly assigned to one of two different versions of the survey. In version A of the survey the respondent was first asked the standard turnout intention question. Following the turnout question, the respondent was then asked a battery of questions about the importance and difficulty of voting, her expectations regarding the closeness of the upcoming election, and a manipulation check about her expectations of voter turnout in the election. In version B of the survey the order of the vote intention question and the question battery was switched. Respondents were first asked the battery of questions and then asked the standard turnout question. With the exception of the vote intention question and the expected turnout question, there were no meaningful differences between conditions for the questions included in this question battery. For this reason we do not discuss the remaining questions further.

Sample Statistics

For both the New Jersey and California samples our initial records for each respondent included age, gender, and turnout history. Formal statistical tests show that the covariates are not significantly related to treatment group assignment. For New Jersey, a test of the joint significance of the covariates in a multinomial logit model (predicting treatment group as a function of all the covariates shown in Table 2) has a p-value of .17; for California the p-value is .80. However, the sample averages are not identical across groups. For example, turnout among those who heard the HTO script in the California experiment was about 3% higher in the 2005 General Election than turnout among those who heard the LTO script. Since prior turnout is an important predictor of both actual turnout and turnout intention, we present the results of the main statistical analysis including controls for past turnout. In addition, the share of female respondents in the LTO treatment conditions was generally lower than that in the HTO treatment conditions. As with prior turnout, the treatment effects

are only slightly affected by including a control variable for gender, which we demonstrate in the main results tables.

We randomly assigned scripts to participants prior to supplying the participant lists to the calling houses. This meant that any given participant had an equal chance of being assigned to each of the scripts used in New Jersey and in California. However, there are small but statistically significant differences in the number of respondents receiving the treatments in the California experiment. The California treatment groups are well-balanced with respect to the important covariates and attrition did not produce a statistically significant correlation between treatment assignment and the covariates. Nevertheless, there is a danger of bias if, after accounting for observable features of the respondents, there are systematic differences across LTO and HTO script groups in unobserved attributes that are correlated with turnout intention. There were fewer completed interviews using the LTO script than the HTO script. If those who dropped out of the California LTO script groups were especially likely to report intending to vote, a comparison of turnout intention between those in the LTO and HTO treatment groups who remain in the sample would exaggerate the increase in turnout induced by the HTO script. However, controlling for observed characteristics of the respondents does not change the results materially. Moreover, typically those who complete surveys are somewhat more likely to vote, which implies that any bias would tend to favor finding the LTO script as more effective at increasing turnout.

In both the New Jersey and California experiments we conducted a manipulation check to verify that, as we intended, the HTO script produced an expectation in respondents that turnout in the upcoming election would be greater than that expected after hearing the LTO script. In the New Jersey experiment we accomplished this by conducting a parallel experiment using respondents randomly drawn from the same respondent pool as those used in the turnout intention experiment. In this small experiment we asked participants to predict the percentage of registered New Jersey citizens that would vote. Consistent with our aims we found that participants who heard the HTO script expected turnout to be higher ($M = 60.2\%$, $S.E. = 2.54$) than participants who heard the LTO script ($M = 47.7\%$, $S.E. = 2.50$), $t(100) = 3.49$, $p = .001$. In the California experiment we asked participants about their turnout expectations in the question battery. Those who heard the HTO script thought turnout in the upcoming

election would be greater than those who heard the LTO script, $X^2(4, N = 1,715) = 80.3$, $p < .001$. Fully 59% of those who heard the HTO script responded that they thought almost everyone or most eligible people would vote, compared to 39% of those who heard the LTO script.

Results

Table 1A shows how the alternative scripts used in the New Jersey experiment affected the reported probability of voting. The responses are grouped into several categories. Despite the clustering of responses near 100%, the HTO clearly outperformed the LTO. The high turnout script was over 7 percentage points more likely to produce a response of 100% likely to vote. A Kruskal-Wallis rank-sum test shows that there is a statistically significant difference between the intention to vote of participants who heard the HTO script and those who heard the LTO script (for a one-sided test $p = .03$).³

Table 1B, columns 1 and 2, show the effect of the treatments on the standard vote intention question for the New Jersey experiment, and columns 3 through 8 show responses to the standard vote intention question for the California experiment. The results again show support for the relative effectiveness of the high turnout script. In both the New Jersey and the California samples, the HTO script was associated with higher turnout intention than the LTO script. While the difference between the HTO and LTO scripts was not statistically significant in the New Jersey experiment when the standard question was used (columns 1 and 2), the absolute pattern of responses is consistent with the prediction that the HTO would produce a greater probability that respondents would report intending to vote. Those receiving the HTO script were 5 percentage points more likely to report that they were absolutely certain to vote than those hearing the LTO script.

³Table 1A excludes those who refused to answer and those who reported having already voted. Substituting a 100% voting probability for those who said they had already voted increases the statistical significance of the difference between the HTO and the LTO ($p = .02$). The share of respondents who refused to provide a probability is *lower* in the HTO group. Since refusal to guess is reasonably assumed to be associated with an average or below average probability of voting, the results in Table 1A are probably a conservative estimate of the relative effectiveness of the HTO script.

TABLE 1A Effect of High and Low Turnout Scripts on Percent Chance of Voting, New Jersey

Reported % Chance of Voting	Treatment Script	
	High Turnout	Low Turnout
100	76.3%	68.9%
99–95	8.8%	9.1%
90–94	2.4%	5.4%
80–89	1.6%	4.6%
< 80	10.8%	12.0%
<i>N</i>	249	241

Note: Cell entries are the percentage of the respondents in each category, by treatment group. The difference in % chance of voting across the High Turnout and Low Turnout Scripts is statistically significant (Kruskal-Wallis rank sum test, $p = .03$ (one-sided)).

The results from the California experiment, columns 3–8, strongly support the hypothesis that HTO increases turnout intention relative to LTO. Recall that the intention to vote question in the California experiment came in two versions. In version A, after hearing either the HTO or LTO script, respondents were asked the vote intention prior to a battery of questions about the election. In version B, the question order was reversed, and the vote intention question followed the questions about the election. Columns 3 and 4 compare the turnout responses for those who heard the HTO and LTO script when the vote intention question was asked first (version A), while columns 5 and 6 compare the responses when the vote intention question was asked last (version B). For both versions A and B, the HTO was more effective at producing intention to vote. After merging the results from versions A and B, the California experiment shows a strongly significant increase in intention to vote among those who heard the HTO script relative to the LTO script. The hypothesis that the respondents in the California experiment who heard the HTO script and those who heard the LTO script were drawn from the same distribution is easily rejected at the .01 level.

Table 1B excludes the small proportion of participants who volunteered that they had already voted prior to the treatment call when answering the standard intention question (1.6% in New Jersey, 13.2% in California) or refused to answer the question (2.8% in NJ and 1.1% in CA). Including categories for these cases or using alternative coding does not have a material effect on size or statistical

significance of the comparisons reported in the following paragraphs.⁴

Reviewing the results from Tables 1A, and 1B we have very strong evidence that the HTO script was more effective than the LTO script. In all the tests the HTO outperformed the LTO; the difference was in most cases statistically significant beyond conventional standards by a substantial margin.

Robustness of Main Results

In randomized experiments without differential attrition, a comparison of group means will provide an unbiased estimate of the average treatment effect. Since there is some evidence of differences in completion rates across treatments, it is useful to examine the robustness of the results to the inclusion of control variables. Using all the cases where the respondents answered the standard vote intention question, we estimated an ordered probit model controlling for vote history, age, gender, state, and interview date. Including control variables is expected to increase the precision of the treatment effect estimates and may also help guard against bias due to differential attrition rates across treatment groups.

Table 2 reports the results for several alternative specifications. The dependent variable is respondent vote intention. As reported in Table 1B, vote intention is a categorical variable, ranging from category one (absolutely certain to vote) to six (not at all likely to vote). A negative coefficient on HTO implies that the HTO treatment moves the subject into a lower numbered category, and therefore into a category associated with a higher probability of voting. The key independent variable is the script type (HTO=1, LTO=0), and control variables include dummy variables for respondent turnout in past elections as reported on voter files (these are equal to 1 if the respondent voted in the general (the “gen” variables) or primary (the “prim” variables) election in each year), age in years, gender state, and date the interview was conducted.

The results are very robust to the inclusion of a variety of covariates. Across a range of model

⁴For the entire sample the percentage of participants who heard the LTO script who claim to have already voted is about 1% higher than the percentage in the HTO. This difference is not statistically significant. If these cases are classified as having voted with certainty, the statistical chi-square statistic falls slightly but the significance levels are not materially affected. For example, the effects on the p-values for the comparisons reported in the California experiment are: columns 3 and 4: no change; columns 5 and 6: increase from .26 to .33; and columns 7 and 8: no change.

TABLE 1B Effect of HTO and LTO Scripts on Turnout New Jersey Experiment and California Experiment

	New Jersey		California Experiment					
	HTO	LTO	HTO	LTO	HTO	LTO	HTO	LTO
	(1)	(2)	Version A	Version A	Version B	Version B	Combined	Combined
Absolutely Certain	76.8%	71.3%	68.5%	67.0%	62.7%	58.8%	65.8%	63.1%
Extremely Likely	7.2%	8.4%	10.8%	7.5%	13.6%	11.1%	12.1%	9.2%
Very Likely	9.6%	9.2%	10.0%	12.7%	12.2%	16.2%	11.0%	14.4%
Somewhat Likely	1.2%	3.5%	4.9%	8.9%	6.9%	9.5%	5.3%	9.2%
No too Likely	2.0%	2.3%	2.9%	1.6%	1.7%	2.3%	2.3%	2.0%
Not at all Likely	3.2%	5.4%	3.9%	2.3%	2.9%	2.1%	3.4%	2.2%
<i>N</i>	250	261	482	427	418	388	900	815

Note: Cell entries are the percentage of respondents in each category, by treatment, by treatment group. The difference in turnout intention for the New Jersey experiment (compare columns (1) and (2)) is in the direction predicted by the theory, but not statistically significant. For the California experiment (columns (3) through (8)) these differences are significant. The HTO and LTO are statistically significant for California Version A (compare (3) and (4)) and overall (compare (7) and (8)). The comparison of HTO and LTO combined (data columns (7) and (8)) slightly exaggerates the turnout effect of HTO in the California experiment since the relative contribution of Version A is slightly higher in the HTO versus the LTO treatments. Re-weighting the data does not have a material effect on the significance of the comparison.

specifications the effect of the HTO script is statistically significant or borderline significant, and the coefficient estimates are very stable. Combining these results with the results from Table 1A (in which the HTO was superior to the LTO, $p=.03$) suggests it is very unlikely the HTO’s superior performance is due to chance. To put the magnitude of the effects into perspective, using the results from column 1, we calculate the marginal effect of receiving the HTO rather than the LTO. Setting the independent variables at their mean values, the marginal increase in the probability a subject reports being absolutely certain to vote when receiving the HTO rather than the LTO is 3.5 percentage points (from a base of approximately 65%). This is a sizable change from a relatively modest treatment.

Subgroup Differences

We investigated whether the HTO advantage differed as a function of how often respondents voted in previous elections. While we found no statistically significant interaction effect, Table 3 shows that the HTO script advantage was concentrated in the portion of the electorate with occasional and infrequent prior turnout records. Panel B of Table 3 shows statistical results when the sample is partitioned into three approximately equal subsamples according to previous vote history: low rate of participation voters (voted in 0–2 elections), occasional voters (3–5), and frequent voters (6–10). The effect of the HTO versus LTO scripts are estimated separately for each of the three subsamples. The final column of Panel B in

TABLE 2 Robustness of Effect of HTO on Vote Intention, Alternative Statistical Ordered Probit

	(1)	(2)	(3)	(4)	(5)
HTO Treatment	−0.096	−0.095	−0.084	−0.085	−0.086
	[0.052]	[.053]	[.053]	[.053]	[.053]
<i>N</i>	2226	2226	2226	2226	2226
<i>p</i> -value, HTO Treatment	0.03	0.03	0.06	0.05	0.05
Vote history index?	NO	YES	YES	YES	NO
Age and Gender?	NO	NO	YES	YES	YES
Interview Date?	NO	NO	NO	YES	YES
Each Election Since 2000 separately?	NO	NO	NO	NO	YES

Note: Standard errors in brackets. To keep sample size constant across specifications for cases missing age and gender the variable is set to zero and a dummy variable for missing is included. Vote history index is equal to the number of times respondent voted in the 10 primary and general elections from 2000 through 2004. The *p* value is for test of null hypothesis that HTO Treatment has zero effect, one sided. All specifications include dummy variable for California and for Version A versus Version B of the California script.

Table 3 shows results when the infrequent and occasional voters are grouped together. The HTO script significantly increases vote intention relative to the LTO script among the infrequent and occasional voters, and it shows no boost in turnout among frequent voters.⁵

This finding is congruent with two lines of research on moderators of the effect of descriptive social norms on behavior. The first is a field experiment in which different descriptive social norm information was used to solicit donations for a once-per-semester fundraising effort at a university. Frey and Meier (2004) report finding that frequent-donators and never-donators were unaffected by the descriptive social norm information, but that occasional-donators were significantly affected by the different types of information. One could sensibly interpret our frequent voters as resembling the frequent-donators in the Frey and Meier study: both are on the extreme tails of the distribution of propensity to act, and both were unaffected by descriptive social norms. It might also make sense to interpret our occasional voters as resembling the occasional-donators: both seem to be near the middle of the distribution of propensity to act, and both were highly affected by social norm information.

How, then, can we make sense of the fact that our low rate of participation voters were also highly affected by descriptive social norm information, whereas Frey and Meier's never-donators were unaffected by it? We believe that this becomes clear when we consider the population of citizens who were included in our sample universe vis-à-vis the entire voting eligible population. Our sample universe excluded citizens who were not registered to vote. This means that our sample did not reflect the true distribution of propensity to vote in the voting eligible population. Interpreted this way, our low rate of participation voters might actually be considered as resembling our occasional voters. The extreme left-most tail of the distribution of propensity to vote was

⁵The vote history variable was coded based on the 10 most recent elections available on the voter file. This included both general and primary elections. In New Jersey all registered voters are eligible to participate in all general and primary elections, while in California all registered voters are eligible to participate in all general elections but the rules regarding unaffiliated voters in primary elections are more complex and vary over time and differ for the Democratic and Republican primaries. The results in Table 3 are for the entire sample. Restricting the sample to those old enough to be eligible for all the elections (those over 25 in 2006) has no important effect on the results for the occasional and frequent voter categories or the results when infrequent and occasional voters are grouped, but lowers the HTO effect slightly in the low rate of participation voter group (from $-.12$ to $-.09$) and raises the p value to .17).

TABLE 3 effect of HTO versus LTO, sample partitioned by vote history Ordered Probit

Panel A: Vote History: X/Last 10 elections										
	0	1	2	3	4	5	6	7	8	9 or 10
HTO	-0.21* [.15]	-0.02 [.13]	-.21* [.15]	-0.13 [.17]	-.28** [.16]	-.26** [.15]	0.08 [.14]	0.28 [.23]	-0.11 [.30]	0.22 [.42]
N	218	289	227	200	242	317	402	162	93	76
Panel B: Vote History: Categorized Into Three Vote History groups										
	Low rate of participation (< 3/10 of last 10 elections)			Occasional voters (3/10 - 5/10 of last 10 elections)			Frequent voters (> 5/10 of last 10 elections)			Low & Occasional
HTO	-.12* [.08]			-.20** [.09]			0.1 [.11]			-.15*** [.06]
P value	.734			.759			.733			1493

Note: Ordered probit coefficient from estimation of model including dummy variable for state and script version. Negative coefficient implies a stronger intention to vote. *p < .1, **p < .05, ***p < .01

not included in our sample universe. This convergence between our vote history finding and what Frey and Meier found regarding how past behavior affected susceptibility to the influence of descriptive social norms increases our confidence in the measures we used in these studies, and in our subgroup findings.

The second line of research on moderators of descriptive social norms looks at how the difficulty of a decision affects social conformity. Baron, Vandello and Brunzman (1996) find that people are more prone to social conformity when a decision is experienced as relatively difficult than when it is experienced as relatively easy. This is relevant to our vote history finding if we interpret the decision of whether or not to vote as being relatively easy for frequent voters (e.g., “Of course I will vote, I always do”), but as being relatively difficult for occasional voters (e.g., “I don’t know, sometimes I vote, sometimes I do not”). If we accept the interpretation described above regarding the censored distribution of citizens included in our sample universe, then even our low rate of participation voters might experience some difficulty deciding whether or not to vote. Viewed this way, our finding that frequent voters are unaffected by social norm information is consistent with this research on decision difficulty and social conformity.⁶

This subgroup finding has clear implications for voter mobilization efforts. When attempting to increase turnout among those lacking a record of prior voting, it is much more useful to emphasize how common voting is rather than how few Americans make the effort to vote. The results also suggest how the LTO message might have become so popular among political communicators. Those crafting the voter mobilization messages are political operatives who are likely very frequent voters. If communicators generate get out the vote (GOTV) messages initially through introspection, then the common practice of LTO GOTV messages is consistent with our findings that an LTO message will be deemed no different than an HTO message among frequent voters. This could then result in a belief among experts that descriptive social norm information is inconsequential in terms of mobilization impact.

Theoretical Implications

If a citizen is encouraged to vote when other citizens participate, this adds an interesting wrinkle to our understanding of how the overall level of participa-

tion in a society is formed and responds to changes in the political environment. To explore the theoretical implications of descriptive social norms, consider a simple model of the probability each voter i votes:

$$P(i) = a + bf(P), \quad (1)$$

where a and b are constants, P is overall turnout, and $f()$ is some increasing function. If $b > 0$, this equation captures the idea that i is affected by the overall turnout levels and is more likely to vote when P is increased. In equilibrium, $P(i) = P$ for all i .

Equation (1) can be used to show two effects of descriptive social norms. First, voter response to descriptive social norms amplifies the effect of other factors on voter turnout. Anything that affects turnout directly (through “ a ”), also has a secondary effect through the induced change in P . Therefore if there is a strong union or party organization in a town, turnout will increase directly as well as indirectly. Further, the returns to mobilization efforts are larger than their direct effects. For example, when f is linear (say $f(x)=x$), there is a unique solution for the turnout level: $P=a/(1-b)$. The “multiplier effect” is equal to $1/(1-b)$, which is greater than 1 as long as $b > 0$. This multiplier effect is consistent with evidence reported by Fowler (2005) using social network data to show what he terms “turnout cascades.”

Second, there may be multiple equilibrium turnout levels. While this is not true for the linear case, if f is nonlinear for a given population there may be several sustainable turnout levels. When turnout is low, people may be less inclined to vote due to the low turnout descriptive social norm. In contrast, when turnout is high, people may feel a greater compulsion to participate. For some functions f both of these situations are stable and either can be the observed equilibrium. As usual, the presence of multiple equilibria implies the possibility of dramatic changes in turnout levels in response to small changes in the process governing turnout (equation 1). For example, small changes in a , b or f in equation (1) can cause what was a stable equilibrium to vanish.

General Discussion

The New Jersey and California experiments suggest that a citizen’s intention to vote in a given election is directly affected by her perception of whether others are going to vote in the election. This is contrary to the intuitions of many voter mobilization experts (Rogers 2005), is inconsistent with the content of

⁶We thank a reviewer for directing us to this research.

many voter turnout messages delivered during election time, and runs contrary to the rational inference that the more people turning out, the less impact any one vote can have. But the descriptive social norm effect shown in these two experiments does not hold for all potential voters. Only citizens who are infrequent and occasional voters appear to be affected by this descriptive norm information, while frequent voters show no differences in their intention to vote based on whether they expect turnout to be relatively high or low. The findings of these experiments have practical implications for GOTV strategies, and theoretical implications for political scientists.

Future research should address some remaining issues. The outcome measure used in the two experiments reported in this article is a participant's stated intention to vote, and not whether the participant actually voted. Previous surveys have established that a person's stated intention to vote is strongly correlated with actual turnout (see Fishbein and Ajzen 1981). In the 1988 ANES study, the most recent version of the ANES with a preelection vote intention question and validated vote, of respondents who said they planned to vote over 75% were found to have voted. In contrast, only 13% of those who said they would not vote were found to have participated according to voter records.⁷

While vote intention correlates strongly with actual voting, a limitation of research that relies on vote intention is that a change in vote intention suggests the direction of the treatment effect, but provides little guidance as to its magnitude. In experimental studies of voting, with the exception of the recent papers in which the explicit goal has been to measure the real-world magnitudes of alternative campaign activities (e.g., Gerber and Green 2000), self-reports about voting are employed far more commonly than actual recorded votes.⁸ This common practice can be defended both conceptually and practically. Conceptually, as touched upon before, this proxy for real voting can be used to identify relationships between variables, even without knowing effect magnitudes. Let us consider the current study. Our conclusion is that a citizen's expectation of voter turnout causally affects her likelihood of voting. We established this causal relationship by experimentally administering

scripts conveying turnout information that implied either low expected voter turnout or high expected voter turnout. The turnout intention changes observed as a consequence of the experimental phone call cannot be expected to have endured at the same strength through to the moment the respondent entered the voting booth the following Tuesday. This is because from the moment the respondent hung up the phone to that moment the following Tuesday, she likely encountered many other messages about voter turnout. And, given that political professionals use both low and high turnout messages, it is likely that she encountered other messages that ran counter to our single experimental HTO or LTO script.

Practically, given that it is difficult to change turnout behavior, dilution of the effects of any intervention is a significant impediment to using validated vote to test a theory about the role of descriptive social norms in voting. Previous work on the behavioral effects of voter mobilization phone calls, in which effects of .5% or less are most commonly observed (Gerber and Green 2005), suggests that even a study with several thousand respondents is far short of what is needed to have the power to reliably detect behavioral differences caused by alternative scripts. The central argument of this paper is not that a single GOTV message emphasizing high turnout will increase voter turnout (though it might), but rather that a citizen's expectation of turnout has a causal impact on her likelihood of voting. Given this relationship, we believe that a political and media environment that consistently emphasizes high expected turnout could result in greater participation in elections. Moreover, this increase in turnout would likely be driven by greater turnout among those who are currently least likely to vote.

One other concern is worth addressing. It has been widely shown that the more strongly a respondent feels that there is a specific socially desired response to a question, the more likely she will be to respond in that way (for review see Tourangeau, Rips, and Rasinski 2000). It is conceivable that the HTO script induced a perception that it was more socially desirable to say that one intends to vote than the LTO script (without a genuine increase in vote intention). This would constrain how we could interpret our results. This social desirability pressure could be experienced as a heightened feeling of civic duty to vote. We addressed this question in the California experiment in which we found that participants did not feel greater civic responsibility to vote with the HTO script than with the LTO script. Had we found civic duty was heightened in

⁷Based on a tabulation of v880396 by v881148. We are grateful to Daniel Bergan for assistance with this calculation.

⁸A selection of recent studies using self-reported vote as the dependent variable includes: Wolfinger and Rosenstone (1980); Iyengar and Ansolobehere (1996); and Fowler and Kam (2006). Examples of recent experimental studies using vote intention include: Fowler (2006) and Grober and Schram (2006).

the HTO conditions we would have interpreted this as a potential mechanism for how descriptive social norms influence intention to vote, rather than as a confound.⁹

Further support for our conclusion is offered by our subgroup findings. We found that frequent voters were unaffected by the norm information, whereas occasional and low rate of participation voters were highly affected. This finding is consistent with past research on who are most vulnerable to the influence of descriptive norms, as discussed above in subgroup differences (Ansolobehere and Iyengar 1996; Baron et al. 1996; Frey and Meier 2004). Past research on overreporting of voting behavior after an election offers divergent evidence that is also consistent with our conclusion that social desirability does not account for our results. This research finds that those who feel most strongly that they should vote (e.g., endorse the injunctive norm) are the most likely to misreport that they did vote when they actually did not (for review see Tourangeau et al. 2000, 274; Silver, Anderson, and Abramson 1986). If we construe susceptibility to vote overreporting as closely related to susceptibility to intention overreporting, than this research suggests that frequent voters (who endorse the injunctive norm most strongly) would be more vulnerable to changing their vote intention based on the different scripts than occasional and low rate of participation voters. This is exactly opposite of what we found.

It is worth noting that the present two experiments took place in relatively moderate salience elections. In the 2005 New Jersey Gubernatorial election 48% of registered citizens voted (versus

73% in the 2004 Presidential election), and in the California Democratic primary only 39% of registered Democrats voted (versus 47% in the 2004 Presidential primary). One might ask how the salience of an election would affect the impact of descriptive social norms on turnout. The present data suggests that these norms would have their greatest effect on turnout in high salience elections. This is because high salience elections induce most frequent voters to actually vote, making less frequent voters the citizens on the margin of whether or not to vote. Since less frequent voters are the most affected by social norm messages, this scenario creates the conditions for expectations about turnout to have a meaningful effect on actual turnout. At the same time, though, the message environment of high salience elections is highly crowded. This crowded message environment reduces the effectiveness of any single communication. The combination of the increased importance of descriptive social norms and the decreased impact of any single communication reinforces our argument that for descriptive social norms to have their largest effect on turnout, a wholesale, coordinated emphasis on high expected turnout would be necessary. On the other hand, the opposite would also be true: low salience elections would be the least likely to be effected by descriptive social norms. This is because the citizens on the margin of whether or not to vote in low salience elections are likely frequent voters, and the present studies show that frequent voters are unaffected by descriptive social norms.

The present paper focuses on what has been termed “psychological political science” (Krosnick and McGraw 2002). That is to say that by borrowing research on descriptive social norms from social psychology we expanded our theoretical understandings of the factors that affect voting. In the process, we also gain insight into the original psychological theory in three ways. We demonstrated the robustness of descriptive social norms by demonstrating their effect in a new domain. Second, we identified a promising new moderator of the effect of descriptive social norms on voting: a person’s vote history. This finding is consistent with past research, but its general meaning with regards to descriptive social norms is not very clear. Our best guess is that the moderator is the regularity of a person’s previous actions. For example, in terms of recycling this vote history finding might suggest that those who always and never recycle are least susceptible to the influence of descriptive social norms, whereas occasional recyclers are most susceptible. More research needs to

⁹We conducted a pilot study investigating whether social desirability bias differed across treatment scripts. If respondents who heard the HTO script felt that the interviewer intended to put more pressure on them to vote than respondents who heard the LTO script, than our findings might be, in part, explained by characteristics of the social dynamic of the phone call. To test this, we asked a convenience sample of adults through an online survey to read a description of the CA study, and then to read both the LTO and HTO scripts, in counterbalanced order. When asked in which script “the caller [intended] to put the greatest degree of pressure to vote on the potential voter” the majority of participants believed that the LTO script involved more pressure to vote (67%, $N = 238$), $X^2(N = 354) = 42.0, p < .001$. Moreover, when asked in which script “the potential voter listening [would actually feel] the greatest degree of pressure to vote from the caller” the majority of participants, again, believed that the LTO script involved more pressure to vote (58%, $N = 204$), $X^2(N = 354) = 8.2, p = .004$. These results suggest that, if there was a social pressure from the interviewer experienced by the respondent, it favored the LTO script over the HTO script, which is the opposite of our findings.

be done. Future research will extend the present findings with regards to turnout, as well as explore this new moderator of descriptive social norms.

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