DOI: 10.1017/S000305540808009X

Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment

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Voter turnout theories based on rational self-interested behavior generally fail to predict significant turnout unless they account for the utility that citizens receive from performing their civic duty. We distinguish between two aspects of this type of utility, intrinsic satisfaction from behaving in accordance with a norm and extrinsic incentives to comply, and test the effects of priming intrinsic motives and applying varying degrees of extrinsic pressure. A large-scale field experiment involving several hundred thousand registered voters used a series of mailings to gauge these effects. Substantially higher turnout was observed among those who received mailings promising to publicize their turnout to their household or their neighbors. These findings demonstrate the profound importance of social pressure as an inducement to political participation.

Among the most striking features of democratic political systems is the participation of millions of voters in elections. Why do large numbers of people vote, despite the fact that, as Hegel once observed, "the casting of a single vote is of no significance where there is a multitude of electors"? One hypothesis is adherence to social norms. Voting is widely regarded as a citizen duty (Blais 2000), and citizens worry that others will think less of them if they fail to participate in elections. Voters' sense of civic duty has long been a leading explanation of voter turnout among both behavioral (Campbell, Gurin, and Miller 1954) and formal (Downs 1957; Riker and Ordeshook 1968) theories of voter turnout.

Even those scholars who are sympathetic to this line of argument nonetheless concede that the theoretical and empirical basis for this hypothesis remains thin (Feddersen 2004). At a theoretical level, the notion that voters receive psychic utility from voting is murky, insofar as it fails to distinguish between the intrinsic rewards that voters obtain from performing this duty and the extrinsic rewards that voters receive when others observe them doing so. As Knack (1992) and Harbaugh (1996) point out, intrinsic and extrinsic incentives have very different empirical implications. Although most scholarly attention has focused on the "expressive benefits" of voting, Harbaugh argues that widespread overreporting of voting in surveys signals the potential importance of extrinsic incentives. This suspicion accords with a large literature in social psychology, which emphasizes the extent to which other-regarding behavior varies depending on whether people perceive their actions to be public (Cialdini and Goldstein 2004; Cialdini and Trost 1998; Lerner and Tetlock 1999).

The empirical literature on the effects of social norms on voting has not advanced much beyond the initial survey work on this topic during the 1950s. Researchers have frequently used cross-sectional survey data to show that people who report feeling a greater sense of civic duty are also more likely to report voting. However, such observational evidence is frequently a misleading guide to causality; it may be that espousing the virtue of voting is a symptom, not a cause, of being a voter. Similarly, it is sometimes pointed out that people whose friends and coworkers vote at high rates are themselves more likely to vote (Huckfeldt and Sprague 1995). Although this empirical regularity is consistent with the idea that voting is more likely when participatory norms prevail in one's social network, it is also consistent with other explanations, including affinity among people with similar political outlooks and group level differences in exposure to political campaigns. The ready availability of alternative explanations implies that, despite observed correlations, the key question remains unresolved: to what extent do social norms *cause* voter turnout?

This study departs from prior work on this subject by conducting an experiment designed to prime voters to think about civic duty while at the same time applying different amounts of social pressure in order to induce them to adhere to this norm. Unlike most previous experiments, which have taken place in laboratory settings, ours takes place in the context of an actual election. Prior to the August 2006 primary election in Michigan, approximately 80,000 households were sent one of four mailings encouraging them to vote. The content of these mailings was inspired by historical and cross-national examples of policies that publicized the names of voters and nonvoters (Lijphart 1997). One experimental group received a mailing that merely reminded them that voting is a civic duty; in a second group, they were told that researchers would

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The authors are grateful to the Institution for Social and Policy Studies at Yale University and to Practical Political Consulting, which funded components of this research but bear no responsibility for the content of this report. Special thanks go to Mark Grebner of Practical Political Consulting, who designed and administered the mail program studied here. This research was reviewed and approved by the Yale Human Subjects Committee.

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be studying their turnout based on public records; a third treatment group received mailings displaying the record of turnout among those in the household; a fourth mailing revealed both the household's voter turnout and their neighbors' turnout. The latter two treatments suggested that a follow-up mailing after the election would report to the household or the neighborhood the subject's turnout in the upcoming election.

Our study makes several important contributions. First, we provide strong statistical evidence that social pressure increases voter turnout. This finding is by no means obvious, for the literature in social psychology is divided between two sets of empirical findings, one emphasizing the tendency to comply with social norms (see Cialdini and Goldstein 2004 for a review; see Gerber and Rugers 2007 for an application to political behavior) and the other calling attention to "reactance" (Brehm and Brehm 1981) or "boomerang effects" (Ringold 2002) in which receivers reject heavy-handed demands. According to Ringold and Steward and Martin (1994), for example, public health messages concerning smoking, alcohol, drugs, and diet frequently lead to less compliance with the normative message.[1]

Notwithstanding the potential for reactance, the influence of a single piece of direct mail turns out to be formidable when (and only when) social pressure is exerted. Exposing a person's voting record to his or her neighbors turns out to be an order of magnitude more effective than conventional pieces of partisan or nonpartisan direct mail (cf. Cardy 2005; Gerber, Green, and Green 2003; Ramirez 2005). In fact, the turnout effect associated with this mailing is as strong as the effect of direct contact by door-to-door canvassers (Green, Gerber, and Nickerson 2003; Michelson 2005) and by far the most cost-effective voter mobilization tactic studied to date. Third, our experimental results also address the broad issue of how the content of a political communication affects political behavior. Previous experimental studies of voter mobilization have found that the way in which a turnout appeal is delivered has a critical effect on the effectiveness of the appeal. Personal, unhurried appeals are usually far superior to impersonal, mechanical and rushed communications (Gerber and Green 2000). The content of the message, however, consistently had little effect on whether the communication produced higher turnout, a fact that may reflect the similarity of the tested messages or the inattentive manner in which voters hear or read them (Gerber and Green 2000; Trivedi 2005). Here we test four alternative mailers with widely varying messages (but devoid of eye-catching graphics, colors, or format) and find sizeable and statistically significant differences in their relative effectiveness. Finally, by showing the extent to which voting rates change as a function of social pressure, our results speak to the enduring paradox of participation in large electorates while at the same time shedding light on the large dropoff in turnout that accompanied the transition from public balloting to secret balloting at the end of the nineteenth century. In sum, the evidence presented here casts new light on theories of collective action and on the role of institutions that affect the degree to which voting behavior is subject to surveillance and social sanction.

SOCIAL NORMS, THE CALCULUS OF VOTING, AND PRIOR RESEARCH

Social norms are rules of conduct that are socially enforced. The causal influence of norms may be divided conceptually into three categories. First is the awareness or recognition of norms. Some rules of conduct, such as removing one's glove before shaking hands, are relatively unknown, whereas others, such as holding the door for an elderly person, are widely recognized. The expectation that citizens should vote on Election Day is nowadays widely acknowledged, although one can think of periods in history when this was not the case. The second aspect of norms is internalization, the acceptance of particular norms as proper and applicable, even when they impose costs. Honoring the dead, for example, is a norm that is widely accepted, whereas bowing one's head before superiors is not, at least not in the West. Internalization of voting norms varies across individuals, and scholars have long observed that "the more strongly a person feels a sense of obligation to discharge his civic duties, the more likely he is to be politically active" (Campbell, Gurin, and Miller 1954, 199). Finally, enforcement of norms varies from disdain to ostracism to outright violence. Expectations of enforcement may induce compliance among those who recognize a norm but have not internalized it.

The extensive social psychological literature on the internalization and enforcement of norms attests to their powerful influence on behavior. People are attentive to the behavior modeled by others and internalize norms readily, especially when those around them provide clear signals about what types of conduct are considered appropriate (Scheff 2000). With regard to enforcement or anticipated enforcement, people are found to be highly sensitive to the perceptions of others, even strangers, and surveillance increases the likelihood of norm-compliant behavior (Rind and Benjamin 1994; Posner and Rasmusen 1999). Compliance with norms of reciprocity, for example, is powerfully shaped by whether one's behavior is publicly visible (Whatley et al. 1999).

These general psychological propositions about norms have been verified not only in the lab but also in field settings. Schultz (1999), for example, reports the results of a field experiment on the frequency and amount of recycling among families over a 17-week period. The manipulation in Schultz's experiment was the nature of social feedback that people receive concerning their behavior. When individuals received weekly updates on their own recycling behavior (individual feedback) or weekly updates about the average recycling behavior of those in their residential area (group feedback), participation in the recycling program and the amount of material recycled increased

[1] In an effort to avoid what are thought to be formidable problems of reactance, social psychologists have turned to foot-in-the-door techniques whereby compliance is achieved through a series of small requests (Fiske 2004).

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over time. By contrast, mere dissemination of information regarding the environmental benefits of recycling did not produce significant increases in recycling participation or the amount recycled. The results of this experiment and others (cf. Webster et al. 2003) suggest that public disclosure of norm-related behavior significantly increases compliance with norms.

The policy implications of norm-enforcement have not been lost on lawmakers. Consider, for example, compulsory voting laws that, until recently, operated in Italy. Although there is some debate as to the level of enforcement, the penalty for not voting in Italy was a form of social sanction whereby nonvoters had their names posted outside a town hall and a "certificate of good conduct" stamped with "Did not vote" (Jackman 1987, 409; Lijphart 1997, 9; Seton-Watson 1983, 111). These "innocuous sanctions" were designed to make it potentially more difficult to find adequate daycare or employment.[2] In other words, Italy had a "shaming" policy.

To what extent these shaming tactics in fact impel people to vote is an open question. As noted earlier, the literature in social psychology is replete with examples of norm-inducement campaigns that failed to increase compliance and sometimes decreased it. Telling people "don't litter" induced more littering than telling people nothing at all, and telling people "don't you dare litter" was worse still (Brehm and Brehm 1981, 333). Thus, the compliance-inducing effects of shaming and the compliance-reducing effects of heavy-handedness are in tension, which makes the net effect of a shaming campaign an open empirical question.

The Calculus of Voting

The tendency to conform to social norms in public has important implications for models of political participation. Voting, though far from universal, is widely considered a civic obligation. The "calculus of voting" model suggests that a potential benefit of voting is the satisfaction of performing one's civic duty (Downs 1957; Riker and Ordershook 1968; see also Aldrich 1993; Blais 2000, 93). Indeed, in the absence of such psychic benefits, the standard rational choice model of voting generally fails to predict significant positive turnout (Ledyard 1984; Palfrey and Rosenthal 1985). One hypothesis about why millions of citizens nonetheless vote is that they are willing to pay the slight costs in time and effort to avoid the feelings of shame associated with not voting or, conversely, to enjoy the satisfaction of voting.

When the formal model of voting is expanded to include these psychic benefits (Riker and Ordeshook 1968), a citizen votes if

$$pB + D > C. \tag{1}$$

Here p is the probability the vote is pivotal, B is the difference in utility from the candidates' attributes or policy stances, D is the direct benefit from voting and C is the cost of voting. Because the probability of casting the decisive vote in an election is typically infinitesimal, the calculus of voting boils down to the relative weight of C and D.

But what is D? The convention model in equation (1) can be extended by breaking the direct benefit term (D), into its components. Suppose that the utility from D were written as

$$D = U(D_I, D_E), \tag{2}$$

where U is the citizen's utility from voting given D_I and D_E, where D_I is the intrinsic benefits associated with voting, a term that captures the positive feeling the voter experiences from fulfilling a civic duty, regardless of any other consequences associated with the act, and where D_E is the extrinsic benefit from voting, a term which captures the social consequences of voting. Extrinsic consequences include feelings of shame or pride that accompany reflecting on the possibility that others might learn about your behavior. A linear approximation of the unknown function $U(D_I, D_E)$ is

$$U(D_I, D_E) \approx \beta_1 D_I + \beta_2 D_E, \tag{3}$$

where β_1 and β_2 are positive constants. The extrinsic benefits we consider here are by definition a function of the likelihood that the act of voting is observed by others. Assuming that these extrinsic benefits are proportional to the probability that others learn of one's behavior, D_E can be written as

$$D_E = \frac{\pi_r(\alpha + \beta_3 D_I)}{\beta_2}, \tag{4}$$

where π_r is the perceived probability others learn whether one voted, and α is a constant that indexes the importance of extrinsic consequences of voting to the citizen. The D_I term is included in (4) to capture the possibility of an interaction between the intrinsic and extrinsic components of civic duty. Subjects who obtain significant intrinsic benefits from voting, for example, may be especially concerned with whether others learn if they voted. All else equal, higher values of α imply that the extrinsic component of civic duty is more important. We will examine the sign of β_3 empirically later in the paper.

Combining (1)–(4), we can rewrite the traditional calculus of voting as vote if:

$$pB + \beta_1 D_I + \alpha\pi_r + \beta_3\pi_r D_I > C. \tag{5}$$

Our experimental treatments are intended to alter π_r and D_I. Under the stipulation that $\beta_3 \geq 0$, two conclusions follow immediately from equation (5). First, citizens become more likely to vote as the intrinsic return to voting (D_I) increases. Second, a citizen becomes more likely to vote with increases in the perceived probability that his or her participation will become known to others (π_r). On the other hand, if $\beta_3 < 0$, publicizing one's voting behavior will undercut a citizen's intrinsic motivation, possibly to the point where the citizen becomes even less likely to vote than in the absence of such publicity. This possibility is strongly

[2] See International Institute for Democracy and Electoral Assistance (http://www.idea.int/vt/compulsory_voting.cfm) for discussion of the administration of compulsory voting in Italy.

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suggested by literature in education showing that students' intrinsic motivation declines when extrinsic incentives are provided (Deci 1971; Lepper, Greene, and Nisbett 1973), and similar arguments have been advanced regarding pro-social behavior more generally (Bénabou and Tirole 2003). Thus, the direction and magnitude of effects associated with social disclosure are an open empirical question, motivating the experiment presented next.

Prior Experimental Research on Shaming and Voting

Prior experimental investigation of publicizing vote history to affect turnout is extremely limited. Our work builds on two pilot studies, which appear to be the only prior studies to examine the effect of providing subjects information on their own vote history and that of their neighbors (Gerber et al. 2006). These two recent experiments, which together had treatment groups approximately 10% of the size of the current study, found borderline statistically significant evidence that social pressure increases turnout. These pilot studies had important limitations. Although they suggest that social exposure has a causal effect on turnout, they share unusual features which make interpretation difficult and employ treatments that cannot isolate the effect of social pressure from the effects of other elements of the experimental treatments.[3]

EXPERIMENTAL DESIGN

Setting

Our field experiment was conducted in Michigan prior to the August 2006 primary election. The August 2006 primary was a statewide election with a wide range of offices and proposals on the ballot, most of which were limited to counties, cities, and local districts. In August 2006, the only statewide offices appearing on the ballot were Governor and U.S. Senate. There were no important contested statewide Democratic primary elections. The incumbent Democrats, Senator Stabenow and Governor Granholm, were unopposed on the ballot. The Republican candidate for Governor was also unopposed, but there was a moderately hard-fought primary contest for the Republican nomination for U.S. Senate. In addition, the election featured a scattering of primary contests in several of Michigan's 15 Congressional seats, but only one was likely to affect the ultimate winner of a seat: the Republican primary in the 7th Congressional District. Each voter was allowed to vote in either the Democratic primary or the Republican primary, but could not vote for a combination of parties in the primary. The voter's choice of party is secret under Michigan law, and there is no party registration. For those intending to vote as Democrats, there was little reason to vote in the 2006 primary apart from the occasional nonpartisan judicial race or contested local office. For Republicans, there was at least some interest throughout the state, since both U.S. Senate candidates spent significant funds and solicited votes wherever they could find them. Voter turnout in the August 2006 primary was 1,282,203, or 17.7% of registered voters.

Study Population

The sample for the experiment was 180,002 households in the state of Michigan. Households were defined to include everyone at the same address with the same last name. The 180,002 households represent a subset of all households that appear in the "Qualified Voter File" (QVF), the official state voter list. Prior to random assignment, we attempted to correct apparent errors in the official file (e.g., missing voter history, incorrect ZIP codes, typographical errors in names or addresses, or multiple listings on the QVF) using paper file records. Where we were unable to correct the file, voters were eliminated from the study. From the remaining file, we removed everyone for whom we could not assign a valid 9-digit ZIP because bulk rate requires a complete ZIP code. Next, we removed people who live on blocks where more than 10% of the addresses included apartment numbers.[4] We then removed people who live on streets with fewer than four addresses (or fewer than 10 voters).[5]

Prior to random assignment we also removed households with the following characteristics: all members of the household had over a 60% probability of voting by absentee ballot if they voted[6] or all household members had a greater than a 60% probability of choosing the Democratic primary rather than the Republican primary.[7] Absentees were removed because it was thought

[3] In both prior experiments, the identity of the sender of the mailing (a political consultant), an unfamiliar and unusual source of political information, was prominently displayed on the mailing. In addition, the mailings in the first pilot study announced to the subjects in large, bold type that they were part of an experiment, a feature that might be expected to induce a Hawthorne effect of its own. In the second pilot study, voters were told that the sender would be "looking for you at the polls," a suggestion with unclear and perhaps unsettling implications. Moreover, all treatments in the two pilot studies presented past turnout for both the voter's own household and their neighbors'. Thus it is impossible to parse the extent to which any observed change in turnout was due to social pressure from neighbors, knowledge that the sender knows whether the recipient voted, or the priming of the voter's sense of civic responsibility.

[4] The reasoning was that the study focused on typical neighborhoods of single-family homes. A study of apartment residents would need to consider the physical layout of apartment complexes in order to choose appropriate neighbors.

[5] We also removed addresses in the western Upper Peninsula because of concern that our mail would not be delivered in time for the primary.

[6] Those with a high probability of voting absentee voting were determined by: age (Michigan allows no-reason absentee voting for people over 60), previous use of absentee ballot, inclusion on a list of people to whom absentee applications are routinely sent, presence of another absentee voter in the household, and ease of using an absentee ballot in the voter's community.

[7] Those with a high probability of choosing the Democratic primary were determined by: living in a Democratic precinct, being African-American, being Hispanic, being a single female, being born between 1930 and 1959, voting in primary elections where most of the turnout was Democratic, expressing support for a Democratic candidate to a phone bank, contributing money to Democratic campaigns, signing

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TABLE 1. Relationship between Treatment Group Assignment and Covariates (Household-Level Data)

	Control	Civic Duty	Hawthorne	Self	Neighbors
	Mean	Mean	Mean	Mean	Mean
Household size	1.91	1.91	1.91	1.91	1.91
Nov 2002	.83	.84	.84	.84	.84
Nov 2000	.87	.87	.87	.86	.87
Aug 2004	.42	.42	.42	.42	.42
Aug 2002	.41	.41	.41	.41	.41
Aug 2000	.26	.27	.26	.26	.26
Female	.50	.50	.50	.50	.50
Age (in years)	51.98	51.85	51.87	51.91	52.01
N =	99,999	20,001	20,002	20,000	20,000

Note: Only registered voters who voted in November 2004 were selected for our sample. Although not included in the table, there were no significant differences between treatment group assignment and covariates measuring race and ethnicity.

that many would have decided to vote or not prior to receipt of the experimental mailings, which were sent to arrive just a few days before the election. Those considered overwhelmingly likely to favor the Democratic primary were excluded because it was thought that, given the lack of contested primaries, these citizens would tend to ignore preelection mailings. We removed everyone who lived in a route where fewer than 25 households remained, because the production process depended on using carrier-route-presort standard mail. To qualify for such treatment by the U.S. Postal Service requires that at least 10 pieces be mailed within each carrier route, which might not have been available after the control group was removed.[8] Finally, we removed all those who had abstained in the 2004 general election on the grounds that those not voting in this very high-turnout election were likely to be "deadwood"—those who had moved, died, or registered under more than one name.

Households assigned to treatment groups were sent one mailing 11 days prior to the primary election.[9] Households were randomly assigned to either the control group or one of four treatment groups described next. Each treatment group consisted of approximately 20,000 households, with 99,999 households in the control group. The 180,002 households were sorted exactly into the order required by the USPS for "ECR-LOT" eligibility (approximately: by ZIP, carrier route; then the order in which the carrier walks the route). The 180,002 households were then divided into 10,000 cells of 18 households each, with each cell consisting of households 1–18, 19–36, and so forth, of the sorted file. As a result, after sorting, each cell consisted entirely of either one or two carrier routes. A random number was generated and the entire 180,002 records were sorted by cell number and the random number. The effect was to leave all the cells together, but in a random order. Using this randomly sorted copy of the file, the records were assigned to treatments 1/1/2/2/3/3/4/4/c/c/c/c/c/c/c/c/c/c where "c" indicates "control group." The records were then resorted into carrier route order.

Table 1 shows sample statistics for subject households. The table divides the sample into treatment and control groups and shows the relationship between treatment group assignment and the covariates in the 180,002 households that form the sample for the experiment. The covariates include a set of known predictors of voting in primaries: turnout history in previous primary and general elections, gender, number of registered voters in the household, and age.

Since the randomization took place at the household level, we looked for suspicious household-level differences. Table 1 reports sample means for the households in the study and confirms that there is no relationship between a household's experimental assignment and its average level of past electoral participation. This point may be made statistically, using multinomial logit to predict experimental assignment as a function of all eight variables listed in Table 1. As expected, a likelihood ratio test with 32 degrees of freedom (8 covariates times 4 treatments) is nonsignificant ($LR = 18.6$, $p = .97$), reaffirming that the experimental groups are very closely balanced in terms of observable characteristics. Randomized assignment coupled with large sample size ensures that the unobservable characteristics are likely to be closely balanced as well.

Treatments

Each household in the treatment group received one of four mailings. The Appendix shows examples of each type. Priming voters to think about their civic duty is common to all of the treatment mailings. All four treatments carry the message "DO YOUR CIVIC DUTY—VOTE!" The first type of mailing ("Civic Duty")

Democratic nomination petitions, signing liberal initiative petitions, and living in a household with a Democrat. They were removed because of the extremely spotty pattern of contested Democratic primaries in the August 2006 election. Some people with a greater than 60% chance of voting Democratic were included, however, because they lived with another member of the household who qualified for inclusion. Such Democrats comprise 2.7% of our experimental sample. With regard to issues of external validity, we do not find any interactions between our treatments and the probability of voting Democratic.

[8] In order to achieve a universe of approximately 180,000 households, a small number of carrier routes were deleted which contained exactly 25 selected voters.

[9] These mailings are included in the Appendix.

TABLE 2. Effects of Four Mail Treatments on Voter Turnout in the August 2006 Primary Election

	Experimental Group				
	Control	Civic Duty	Hawthorne	Self	Neighbors
Percentage Voting	29.7%	31.5%	32.2%	34.5%	37.8%
N of Individuals	191,243	38,218	38,204	38,218	38,201

provides a baseline for comparison with the other treatments because it does little besides emphasize civic duty. Households receiving this type of mailing were told, "Remember your rights and responsibilities as a citizen. Remember to vote."

The second mailing adds to this civic duty baseline a mild form of social pressure, in this case, observation by researchers. Households receiving the "Hawthorne effect" mailing were told "YOU ARE BEING STUDIED!" and informed that their voting behavior would be examined by means of public records. The degree of social pressure in this mailing was, by design, limited by the promise that the researchers would neither contact the subject nor disclose whether the subject voted. Consistent with the notion of Hawthorne effects, the purpose of this mailing was to test whether mere observation influences voter turnout.

The "Self" mailing exerts more social pressure by informing recipients that who votes is public information and listing the recent voting record of each registered voter in the household. The word "Voted" appears by names of registered voters in the household who actually voted in the 2004 primary election and the 2004 general election, and a blank space appears if they did not vote. The purpose of this mailing was to test whether people are more likely to vote if others within their own household are able to observe their voting behavior. The mailing informed voters that after the primary election "we intend to mail an updated chart," filling in whether the recipient voted in the August 2006 primary. The "Self" condition thus combines the external monitoring of the Hawthorne condition with actual disclosure of voting records.

The fourth mailing, "Neighbors," ratchets up the social pressure even further by listing not only the household's voting records but also the voting records of those living nearby. Like the "Self" mailing, the "Neighbors" mailing informed the recipient that "we intend to mail an updated chart" after the primary, showing whether members of the household voted in the primary and who among their neighbors had actually voted in the primary. The implication is that members of the household would know their neighbors' voting records, and their neighbors would know theirs. By threatening to "publicize who does and does not vote," this treatment is designed to apply maximal social pressure.

RESULTS

Following the August 2006 election we obtained turnout data from public records. Table 2 reports basic turnout rates for each of the experimental groups. The control group in our study voted at a rate of 29.7%. By comparison, the "Civic Duty" treatment group voted at a rate of 31.5%, suggesting that appeals to civic duty alone raise turnout by 1.8 percentage points. Adding social pressure in the form of Hawthorne effects raises turnout to 32.2%, which implies a 2.5 percentage-point gain over the control group. The effect of showing households their own voting records is dramatic. Turnout climbs to 34.5%, a 4.9 percentage-point increase over the control group. Even more dramatic is the effect of showing households both their own voting records and the voting records of their neighbors. Turnout in this experimental group is 37.8%, which implies a remarkable 8.1 percentage-point treatment effect.

It is important to underscore the magnitude of these effects. The 8.1 percentage-point effect is not only bigger than any mail effect gauged by a randomized experiment; it exceeds the effect of live phone calls (Arceneaux, Gerber, and Green 2006; Nickerson 2006b) and rivals the effect of face-to-face contact with canvassers conducting get-out-the-vote campaigns (Arceneaux 2005; Gerber and Green 2000; Gerber, Green, and Green 2003). Even allowing for the fact that our experiment focused on registered voters, rather than voting-eligible citizens, the effect of the Neighbors treatment is impressive. An 8.1 percentage-point increase in turnout among registered voters in a state where registered voters comprise 75% of voting-eligible citizens translates into a 6.1 percentage-point increase in the overall turnout rate. By comparison, policy interventions such as Election Day registration or vote-by-mail, which seek to raise turnout by lowering the costs of voting, are thought to have effects on the order of 3 percentage-points or less (Knack 2001).

In terms of sheer cost efficiency, mailings that exert social pressure far outstrip door-to-door canvassing. The powder blue mailings used here were printed on one side and cost 30 cents apiece to print and mail. Treating each experimental group therefore cost approximately $6,000. The "Self" mailing generated 1,854 votes at a rate of $3.24 per vote. The "Neighbors" mailing generated 3,106 votes at $1.93 per vote. By comparison, a typical door-to-door canvassing campaign produces votes at a rate of roughly $20 per vote, while phone banks tend to come in at $35 or more per vote (Green and Gerber 2004).

The analysis thus far has ignored the issue of sampling variability. The main complication associated with individual-level analysis of data that were

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TABLE 3. OLS Regression Estimates of the Effects of Four Mail Treatments on Voter Turnout in the August 2006 Primary Election

	Model Specifications		
	(a)	(b)	(c)
Civic Duty Treatment (Robust cluster standard errors)	.018* (.003)	.018* (.003)	.018* (.003)
Hawthorne Treatment (Robust cluster standard errors)	.026* (.003)	.026* (.003)	.025* (.003)
Self-Treatment (Robust cluster standard errors)	.049* (.003)	.049* (.003)	.048* (.003)
Neighbors Treatment (Robust cluster standard errors)	.081* (.003)	.082* (.003)	.081* (.003)
N of individuals	344,084	344,084	344,084
Covariates**	No	No	Yes
Block-level fixed effects	No	Yes	Yes

Note: Blocks refer to clusters of neighboring voters within which random assignment occurred. Robust cluster standard errors account for the clustering of individuals within household, which was the unit of random assignment.
* $p < .001$.
** Covariates are dummy variables for voting in general elections in November 2002 and 2000, primary elections in August 2004, 2002, and 2000.

randomized at the household-level is that proper estimation of the standard errors requires a correction for the possibility that individuals within each household share unobserved characteristics (Arceneaux 2005). For this reason, Table 3 reports robust cluster standard errors, which take intrahousehold correlation into account. We also consider a range of different model specifications in order to gauge the robustness of the results.

The first column of Table 3 reports the results of a linear regression in which voter turnout (Y_i) for individual i is regressed on dummy variables $\{D_{1i}, D_{2i}, D_{3i}, D_{4i}\}$ marking each of the four treatments (the reference category is the control group). This model may be written simply as

$$Y_i = \beta_0 + \beta_1 D_{1i} + \beta_2 D_{2i} + \beta_3 D_{3i} + \beta_4 D_{4i} + u_i, \quad \textbf{(6)}$$

where u_i represents an unobserved disturbance term. The second column embellishes this model by including fixed effects $\{C_{1i}, C_{2i}, \ldots, C_{9999i}\}$ for all but one of the $K = 10{,}000$ geographic clusters within which randomization occurred:

$$Y_i = \beta_0 + \beta_1 D_{1i} + \beta_2 D_{2i} + \beta_3 D_{3i} + \beta_4 D_{4i} + \sum_{k=1}^{K-1} \gamma_k C_{ki} + u_i. \quad \textbf{(7)}$$

The parameters associated with these fixed effects are uninteresting for our purposes; we will focus on the treatment parameters β_1, β_2, β_3, and β_4. The advantage of including fixed effects is the potential to eliminate any observed imbalances within each geographic cluster, thereby improving the precision of the estimates. The final column of Table 3 controls further for voting in five recent elections:

$$Y_i = \beta_0 + \beta_1 D_{1i} + \beta_2 D_{2i} + \beta_3 D_{3i} + \beta_4 D_{4i} + \sum_{k=1}^{K-1} \gamma_k C_{ki} + \lambda_1 V_{1i} + \lambda_1 V_{1i} + \cdots + \lambda_5 V_{5i} + u_i. \quad \textbf{(8)}$$

Again, the point is to minimize disturbance variance and improve the precision of the treatment estimates.

The results are remarkably robust, with scarcely any movement even in the third decimal place. The average effect of the Civic Duty mailing is a 1.8 percentage-point increase in turnout, suggesting that priming civic duty has a measurable but not large effect on turnout. The Hawthorne mailing's effect is 2.5 percentage points. Mailings that list the household's own voting record increase turnout by 4.8 percentage points, and including the voting behavior of neighbors raises the effect to 8.1 percentage points. All effects are significant at $p < .0001$. Moreover, the Hawthorne mailing is significantly more effective than the Civic Duty mailing ($p < .05$, one-tailed); the Self mailing is significantly more effective than the Hawthorne mailing ($p < .001$); and the Neighbors mailing is significantly more effective than the Self mailing ($p < .001$).

Having established that turnout increases marginally when civic duty is primed and dramatically when social pressure is applied, the remaining question is whether the effects of social pressure interact with feelings of civic duty. Using an individual's voting propensity as a proxy for the extent to which he or she feels an obligation to vote, we divided the observations into six subsamples based on the number of votes cast in five prior elections; we further divided the subsamples according to the number of voters in each household, because household size and past voting are correlated. As noted earlier, one hypothesis is that social pressure is particularly effective because it reinforces existing motivation to participate. The contrary hypothesis is that extrinsic incentives extinguish intrinsic motivation, resulting in greater treatment effects among those with low voting propensities. To test these hypotheses while at the same time taking into account floor and ceiling effects, we conducted a series of logistic regressions and examined the treatment effects across subgroups.[10] This analysis revealed that the treatment effects on underlying voting propensities are more or

[10] This analysis (not shown, but available on request) divided the subjects according to past voting history and household size. We tested the interaction hypothesis by means of a likelihood-ratio test, which failed to reject the null hypothesis of equal treatment effects across these subgroups.

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less constant, regardless of whether the target group votes often or rarely.[11] We infer, therefore, that there are no appreciable interactions between social pressure and one's sense of civic duty. The enforcement of norms seems to have the same underlying effect on everyone, regardless of whether their past behavior bespeaks a high or low level of internalization of these norms. In terms of the model presented in equation (5), the interaction parameter β_3 appears to be zero.

The lack of interaction between intrinsic motivation and external pressure has important implications. From a theoretical standpoint, the results suggest that, subject to the usual caveats about ceiling effects, the influence of social pressure is additive: the more pressure, the more voting, regardless of whether the recipient is predisposed to vote in the first place. This finding may suggest that while people vary in terms of their willingness or eagerness to conform to norms of civic participation, the norm is widely accepted as an appropriate behavioral standard (see Blais 2000, ch. 5). The finding is also propitious from the standpoint of external validity. Voters with widely varying characteristics nonetheless respond similarly to this intervention, suggesting that the results obtained here may also apply to other demographic or electoral settings. Consistent with this argument, we find no evidence of interactions between the treatment and the voter's partisan orientations or levels of competitiveness within the voter's congressional district ($p > .25$).

DISCUSSION

The remarkable effectiveness of the social pressure appeals contrasts with the relatively modest effects observed in previous studies of the effectiveness of direct mail voter mobilization campaigns. Table 4 collects the results from previously published experimental studies of mailings involving at least 1,500 subjects. These studies cover a wide range of political contexts and a diverse set of experimental subjects. Consider the results for a single mailing. Treatment effects in the 1.8 percentage point range, similar to the effects we observe for the Civic Duty mailing, are rare but not unprecedented, though one mail piece more commonly produces a result less than 1 percentage point. Results in the 5% to 8% range have never before been observed, even for a nine-piece mail program. Given the large size of our experiment and those reported in Table 4, this startling result cannot be attributed to chance.

The difference between our intervention and mail used in previous experiments is that ours harnesses one of the most formidable forces in social psychology, pressure to conform to social norms. Prior experimental studies have often couched voter mobilization messages in terms of norms (e.g., "stand up for democracy"), but none have introduced social surveillance. Evidence from experiments in psychology and behavioral economics suggests that people are more likely to comply with social norms if they know their behavior will be made public (Cialdini and Goldstein 2004; Kallgren, Reno, and Cialdini 2000). Scholars in experimental economics, for example, consistently find that as social isolation is reduced, through either direct or indirect communication, individuals will tend to behave more generously in bargaining scenarios (Bohnet and Frey 1999; Burnham 2003; Hoffman, McCabe, and Smith 1996). Although heavy-handed requests sometimes encounter reactance, on balance people tend to comply with social norms when others are believed to be watching in order to avoid shame and social ostracism.

Decades of survey research have suggested that people implicitly defer to the norm of voting, insofar as they tend to exaggerate their past rates of voter turnout. The present study is the first since Gosnell (1927) to examine whether these social pressures to vote can be used to encourage people to cast ballots. Whereas Gosnell sent recipients mail deriding nonvoters as "slackers" (27), our interventions brought to bear the power of surveillance. The particular treatments used in this experiment were admittedly unconventional. It is therefore an open question as to whether the effects of appeals patterned on those employed here will have the same, smaller, or larger effects with repeated use, a subject of ongoing experimental investigation. By the same token, we can only speculate about whether the results obtained here will be replicated in the context of a high-salience election such as a presidential contest in a battleground state. In presidential elections, the supply of nonvoters available to be mobilized is reduced; on the other hand, shaming and surveillance might have greater force in the context of a presidential election, where abstention is more counternormative. Certainly, the results obtained here are sufficiently large and robust to warrant efforts to replicate and extend this research paradigm in other electoral settings.

From a theoretical vantage point, the sheer magnitude of the surveillance effect suggests an important new line of attack for scholars seeking to explain long-term declines in voter turnout or, more generally, to understand the components of the calculus of voting. The activation and enforcement of social norms, as many scholars have observed, is potentially subject to "tipping points" or "cascades" (Schelling 1978). When norm-compliant behavior drops off or norm-enforcing behavior dissipates, the equilibrium level of compliance may quickly deteriorate. Enforcement of norms is potentially costly—many recipients of the Neighbors message called the phone number provided on the mailing and demanded to be removed from future mailing lists—and arguably more so when the level of compliance is low.

From an historical vantage point, one could argue that the sharp declines in turnout rates that occurred

[11] The logistic function does imply, however, that the effects will generate the largest *percentage-point* movement among those whose baseline probability of voting is near 50%. As a practical matter, therefore, these treatments are most effective in terms of votes produced when directed at people with middling vote probabilities.

TABLE 4. Results from Previous Studies on the Effects of Pre-election Mailings on Voter Turnout

	Mailings Received								
Election	None	One	Two	Three	Four	Five	Six	Eight	Nine
1998 New Haven, nonpartisan[1a]									
Civic duty message									
Turnout ratev	42.2	41.7	46.3	44.8	...	...	...	...	
Number of observations	11,596	935	984	902	...	...	...	...	
Neighborhood solidarity message									
Turnout rate	42.2	42.0	44.4	43.9	...	...	...	...	
Number of observations	11,596	810	872	795	...	...	...		
Close turnout message									
Turnout rate	42.2	44.2	38.8	44.9	...	...	...	...	
Number of observations	11,596	805	843	830	...	...	...	...	
1999 New Haven, nonpartisan[1b]									
Civic duty message									
Turnout rate	38.9	...	39.2	...	44.6	...	45.8	42.2	
Number of observations	22,484	...	569	...	278	...	295	559	
Close turnout message									
Turnout rate	38.9	...	34.8	...	38.0	...	38.7	39.8	
Number of observations	22,484	...	569	...	271	...	302	545	
1999 New Jersey, partisan[1]									
Prime Democrats									
Turnout rate	63.7	...	...	...	65.6	...	...	...	
Number of observations	1,203	...	...	...	9,955	...	...	...	
Other Democrats and Independents									
Turnout rate	54.2	...	...	...	...	...	54.2	...	
Number of observations	1,925	...	...	...	...	...	17,816	...	
Republicans and low-turnout Independents									
Turnout rate	23.1	...	...	...	23.5	...	24.1	...	
Number of observations	1,863	...	...	...	990	...	1,447	...	
1999 Connecticut mayoral, partisan with negative tone[1]									
Turnout rate	56.1	...	...	...	...	...	...	...	55.0
Number of observations	2,155	...	...	...	...	...	...	...	17,693
2002 Pennsylvania, partisan[1]									
Prime Republicans									
Turnout rate	89.0	90.6	...	...	...	...	...	...	
Number of observations	819	7,224	...	...	...	...	...	...	
Other Republicans									
Turnout rate	63.6	...	65.1	...	...	...	...	...	
Number of observations	1,306	...	9,301	...	...	...	...	...	
Independents and Democrats									
Turnout rate	73.8	...	...	73.0	...	...	...	...	
Number of observations	4,606	...	...	30,727	...	...	...	...	
2002 gubernatorial primary, partisan[2]									
Democrats									
Turnout rate	78.1	...	77.4	...	...	79.3[c]	...	...	
Number of observations	1,569	...	1,527	...	...	1,614[c]	...	...	
2002 Los Angeles, nonpartisan[3]									
Asian Americans									
Turnout rate	35.4	37.3	...	...	...	...	...	...	
Number of observations	3,085	1,175	...	...	...	...	...	...	
2002 general election, nonpartisan[4]									
Latino voters									
Turnout rate									
Orange County, CA	18.1	...	18.9	...	...	...	...	...	
	4,775		767						
Orange County, CA	18.8	...	17.3	...	...	...	...	...	
	2,383	...	414						
Denver, CO A	16.8	...	...	17.7	...	...	...	...	
	21,486	...	...	20,707					

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TABLE 4. Continued

Election	Mailings Received								
	None	One	Two	Three	Four	Five	Six	Eight	Nine
Denver, CO B	3.2	. . .	. . .	3.2	. . .	. . .	. . .	. . .	
	8,020	. . .	. . .	17,426					
Harris County, TX	18.5	. . .	. . .	19.7	. . .	. . .	. . .	. . .	
	3,615	. . .	. . .	13,899					
New Mexico	40.8	. . .	. . .	40.5	. . .	. . .	. . .	. . .	
	10,002	. . .	. . .	25,769					
Los Angeles County	22.0	. . .	. . .	. . .	20.1	. . .	. . .	. . .	
	1,060	. . .	. . .	. . .	1,059				
Los Angeles County	22.6	. . .	. . .	. . .	21.9	. . .	. . .	. . .	
	1,438	. . .	. . .	. . .	1,472				
2004 general election, nonpartisan[5]									
Indian-American voters (Queens County, NY)									
Civic duty message									
Turnout rate	52.2	53.7	. . .	. . .	. . .	. . .	. . .	. . .	
Number of observations	1,561	1,561	. . .	. . .	. . .	. . .	. . .	. . .	
"People of color" message									
Turnout rate	52.2	54.2	. . .	. . .	. . .	. . .	. . .	. . .	
Number of observations	1,561	1,561	. . .	. . .	. . .	. . .	. . .	. . .	
"Indian-American" message									
Turnout rate	52.2	52.1	. . .	. . .	. . .	. . .	. . .	. . .	
Number of observations	1,561	1,561	. . .	. . .	. . .	. . .	. . .	. . .	

Note: . . . Not applicable.
[a] The control group is the same both New Haven experiments in 1998.
[b] The control group is the same both New Haven experiments in 1999.
[c] The treatment group received two phone calls and five pieces of mail.
[1] Reproduced from Green and Gerber (2004: 121); Results for 1(a) are from experiment reported in Gerber and Green 2000, 1 and 1(b) from experiments reported in Gerber, Green, and Green 2003; and Gerber 2004.
[2] Reproduced from Cardy 2005, 37.
[3] Reproduced from Wong 2005, 108.
[4] Reproduced from Ramirez 2005, 71, 76.
[5] Reproduced from Trivedi 2005, 120. The control group is the same for all three experiments.

in the United States after the 1880s reflect social forces, such as rapid population growth and mobility, coupled with institutional changes, such as the introduction of secret balloting and rules requiring that party officials remain a long distance away from where ballots are cast, that diminished both the surveillance of voters and their sense that their voting behavior was being monitored. Concomitant changes, such as the decline of party machines, membership organizations, and party-aligned newspapers that openly excoriated nonvoters, also may have contributed to the erosion of social pressure.

The question is whether these processes are reversible. To answer this question requires a research program of experimental interventions that replicate and extend the work presented here. We have seen the power of a single mailer disclosing the voting behavior of oneself and one's neighbors. Does this effect persist over time, in the form of newly created voting habits (Gerber, Green, and Shachar 2003; Plutzer 2002)? (Results from the November 2006 election suggest that it does, but 2008 presents a more stringent test.) Does the effect of the treatment spread to neighbors and others in the recipients' social networks (Huckfeldt and Sprague 1995; Nickerson forthcoming)? To what extent would the effect observed here be amplified if social pressure were conveyed in person, say, among members of the same social or political group (Schram and Van Winden 1991)? Does the effect grow when more than one person exerts social pressure?[12] Does it diminish, as in the famous Asch (1958) experiment, when other noncompliers openly challenge the pressure to conform?

In sum, the powerful effects of interventions like the ones described here present behavioral scientists with a new paradigm by which to study a wide array of different phenomena, ranging from habit-formation to interpersonal influence. Although we are not advocates of shaming tactics or policies, their cost-effectiveness makes them an inevitable development in political campaign craft, and social scientists have much to learn by studying the consequences of making public acts more public.

[12] The Massachusetts Affordable Housing Alliance presents each student attending its training classes with his or her vote history as a way of encouraging political involvement. Among fundraisers, publicizing whether and how much people contribute is a common tactic.

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APPENDIX A: MAILINGS

Civic Duty mailing

30426-2 ||| || || || | XXX

For more information: (517) 351-1975
email: etov@grebner.com
Practical Political Consulting
P. O. Box 6249
East Lansing, MI 48826

PRSRT STD
U.S. Postage
PAID
Lansing, MI
Permit # 444

ECRLOT **C002
THE JONES FAMILY
9999 WILLIAMS RD
FLINT MI 48507

Dear Registered Voter:

DO YOUR CIVIC DUTY AND VOTE!

Why do so many people fail to vote? We've been talking about this problem for years, but it only seems to get worse.

The whole point of democracy is that citizens are active participants in government; that we have a voice in government. Your voice starts with your vote. On August 8, remember your rights and responsibilities as a citizen. Remember to vote.

DO YOUR CIVIC DUTY — VOTE!

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Hawthorne mailing

30424-1 ||| || || ||

For more information: (517) 351-1975
email: etov@grebner.com
Practical Political Consulting
P. O. Box 6249
East Lansing, MI 48826

PRSRT STD
U.S. Postage
PAID
Lansing, MI
Permit # 444

ECRLOT **C001
THE SMITH FAMILY
9999 PARK LANE
FLINT MI 48507

Dear Registered Voter:

YOU ARE BEING STUDIED!

Why do so many people fail to vote? We've been talking about this problem for years, but it only seems to get worse.

This year, we're trying to figure out why people do or do not vote. We'll be studying voter turnout in the August 8 primary election.

Our analysis will be based on public records, so you will not be contacted again or disturbed in any way. Anything we learn about your voting or not voting will remain confidential and will not be disclosed to anyone else.

DO YOUR CIVIC DUTY — VOTE!

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Self mailing

30422-4 ||| || || | ||

For more information: (517) 351-1975
email: etov@grebner.com
Practical Political Consulting
P. O. Box 6249
East Lansing, MI 48826

PRSRT STD
U.S. Postage
PAID
Lansing, MI
Permit # 444

ECRLOT **C050
THE WAYNE FAMILY
9999 OAK ST
FLINT MI 48507

Dear Registered Voter:

WHO VOTES IS PUBLIC INFORMATION!

Why do so many people fail to vote? We've been talking about the problem for years, but it only seems to get worse.

This year, we're taking a different approach. We are reminding people that who votes is a matter of public record.

The chart shows 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 August 8 primary election. We intend to mail you an updated chart when we have that information.

We will leave the box blank if you do not vote.

DO YOUR CIVIC DUTY—VOTE!

	Aug 04	Nov 04	Aug 06
OAK ST			
9999 ROBERT WAYNE		Voted	_____
9999 LAURA WAYNE	Voted	Voted	_____

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Neighbors mailing

30423-3 ||| || || | |||

For more information: (517) 351-1975
email: etov@grebner.com
Practical Political Consulting
P. O. Box 6249
East Lansing, MI 48826

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U.S. Postage
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Lansing, MI
Permit # 444

ECRLOT **C050
THE JACKSON FAMILY
9999 MAPLE DR
FLINT MI 48507

Dear Registered Voter:

WHAT IF YOUR NEIGHBORS KNEW WHETHER YOU VOTED?

Why do so many people fail to vote? We've been talking about the problem for years, but it only seems to get worse. This year, we're taking a new approach. We're sending this mailing to you and your neighbors to publicize who does and does not vote.

The chart shows the names of some of your neighbors, showing which have voted in the past. After the August 8 election, we intend to mail an updated chart. You and your neighbors will all know who voted and who did not.

DO YOUR CIVIC DUTY — VOTE!

MAPLE DR	Aug 04	Nov 04	Aug 06
9995 JOSEPH JAMES SMITH	Voted	Voted	_____
9995 JENNIFER KAY SMITH		Voted	_____
9997 RICHARD B JACKSON		Voted	_____
9999 KATHY MARIE JACKSON		Voted	_____
9999 BRIAN JOSEPH JACKSON		Voted	_____
9991 JENNIFER KAY THOMPSON		Voted	_____
9991 BOB R THOMPSON		Voted	_____
9993 BILL S SMITH			_____
9989 WILLIAM LUKE CASPER		Voted	_____
9989 JENNIFER SUE CASPER		Voted	_____
9987 MARIA S JOHNSON	Voted	Voted	_____
9987 TOM JACK JOHNSON	Voted	Voted	_____
9987 RICHARD TOM JOHNSON		Voted	_____
9985 ROSEMARY S SUE		Voted	_____
9985 KATHRYN L SUE		Voted	_____
9985 HOWARD BEN SUE		Voted	_____
9983 NATHAN CHAD BERG		Voted	_____
9983 CARRIE ANN BERG		Voted	_____
9981 EARL JOEL SMITH			_____
9979 DEBORAH KAY WAYNE		Voted	_____
9979 JOEL R WAYNE		Voted	_____

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