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Affect, Social Pressure and Prosocial Motivation: Field Experimental Evidence of the Mobilizing Effects of Pride, Shame and Publicizing Voting Behavior

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Abstract Citizens generally try to cooperate with social norms, especially when norm compliance is monitored and publicly disclosed. A recent field experimental study demonstrates that civic appeals that tap into social pressure motivate electoral participation appreciably (Gerber et al., Am Polit Sci Rev 102:33–48, 2008). Building on this work, I use field experimental techniques to examine further the socio-psychological mechanisms that underpin this effect. I report the results of three field experiments conducted in the November 2007 elections designed to test whether voters are more effectively mobilized by appeals that engender feelings of pride (for reinforcing or perpetuating social and cultural values or norms) or shame (for violating social and cultural values or norms). Voters in Monticello, Iowa and Holland, Michigan were randomly assigned to receive a mailing that indicated the names of all verified voters in the November 2007 election would be published in the local newspaper (pride treatment). In Ely, Iowa voters were randomly assigned to receive a mailing that indicated the names of all verified nonvoters would be published in the local newspaper (shame treatment). The experimental findings suggest shame may be more effective than pride on average, but this may depend on who the recipients are. Pride motivates compliance with voting norms only amongst high-propensity voters, while shame mobilizes both high- and low-propensity voters.

Keywords Social pressure · Voting · Field experiment · Pride · Shame · Emotions · Prosocial behavior · Public surveillance

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Participation in elections is an enduring political reality in the United States and in other democratic systems despite the fact that the costs associated with voting often outweigh the benefits because an individual vote has only an infinitesimal chance of influencing election outcomes (Downs 1957). One potential explanation for the seemingly irrational act of voting is that voters perceive abstention to violate social norms (Shachar and Nalebuff 1999; Knack 1992). Evidence that social norms influence behavior is abundant and suggests individuals are reluctant to appear to violate social protocols (Cialdini and Goldstein 2004; Posner and Rasmusen 1999). Studies demonstrate that voting is widely viewed as a civic duty (Blais 2000), and voters wish, at the very least, to appear compliant with this social norm. Vast overreporting of turnout in surveys would support this contention (Belli et al. 2001).

Scholars argue that the desire to adhere to social norms fosters electoral participation by activating voters' intrinsic motivation to engage in prosocial behavior.[1] But the extrinsic benefits voters arguably extract from being observed to participate are also potent. Social psychologists believe that people are generally sensitive to others' perceptions and that public visibility enhances the likelihood of norm-compliant behavior (Posner and Rasmusen 1999). The desire to develop favorable reputations (Buffacchi 2001) or fear of social sanctions may motivate voting, for example (Knack 1992; Opp 2001). Alternatively, the tendency to adherence to social norms, especially when behavior is readily observable, may reflect citizens' desire to avoid shame (Tadelis 2007). Because voting is, in most cases, a public act that can be monitored, verified and publicized, participation will partly be a function of the perceived probability that others learn whether one voted (Gerber et al. 2008).

Some scholars suggest norm-compliance may be linked to community size, arguing that pressure to adhere to social norms may be greater in small communities, where social networks and social interaction are presumably stronger and where cooperation (or defection) is easily monitored (Kropf and Knack 2003; Funk 2006; Ostrom 1990). Although the empirical evidence on this score is mixed (Kropf and Knack 2003), a recent study of the impact of the introduction of optional postal voting in Switzerland corroborates claims about surveillance effects (Funk 2006). The author finds that postal voting, introduced in Switzerland to curtail anemic electoral participation, raised turnout in Cantons where no citizens lived in small communities (less than 1,000 residents) by six percentage points on average, but turnout in Cantons with high concentrations of voters living in small communities dropped by seven percentage points on average, despite the fact that the innovation lowered voting costs. Funk (2006) argues that the introduction of postal voting transformed the social incentives to vote and emphasizes the role of community size in influencing norm-compliance. The external benefits of norm-adherence attenuated in small communities, and voting declined. By contrast, turnout strengthened in larger communities as voting costs were lowered in the absence of such social pressure.

Shame-avoidance should impel people to vote, and voters are presumed to be responsive to social pressure exerted by efforts that publicize whether or not they

[1] Coleman (1988, 1990) argues it is rational for individuals to consider civic norms when contemplating contributing to public goods. Knack (1992) builds on this work, focusing on applications to voting.

voted. One hypothesis is that efforts designed to raise the perceived probability that citizens' participation in elections will be publicly disclosed should, all else equal, elevate turnout. A recent field experimental study conducted by Gerber et al. (2008) reveals strong empirical support for this hypothesis and concludes that that social pressure effectively motivates voting. The authors' experimental manipulations determined that treatments that revealed individuals' own voting history, as well as that of their neighbors, boosted subjects' turnout considerably (Gerber et al. 2008). The results imply that the threat of being exposed as norm-rejecting effectively counteracts inclinations to abstain.

These findings suggest powerful implications about the psychological underpinnings of voting behavior. In particular, the activation of emotional mechanisms—like shame or pride—has the capacity to influence behavior and to induce norm-compliant behavior. Citizens will engage in costly prosocial behavior to avoid feeling ashamed, for example, or, conversely, to feel proud. Benabou and Tirole (2006, p. 1663) argue that, "[p]ublic authorities and private sponsors aiming to foster prosocial behavior make heavy use of both public displays and private mementos conveying honor and shame." For instance, nations commonly award medals and honorary titles to inspire pride, but authorities also televise arrests, post the names of sexual offenders and parents who are delinquent on child support, and publish the license plate numbers of vehicles photographed in areas known for drug trafficking or prostitution in the local newspapers to deter irresponsible behavior through shame (Benabou and Tirole 2006, p. 1663). Scholars have recently investigated the impact sentiments of shame on electoral participation, but studies that examine the effects of activating prideful emotions remain surprisingly rare. Moreover, studies that permit us to draw conclusions about the impact of shame, relative to pride, on voting are also elusive. This paper seeks to fill that void by presenting field experimental evidence that investigates the impact of activating "negative" emotions (shame) or "positive" emotions (pride) by threatening to publicize individuals' voting behavior in the local newspaper. Admittedly uncommon, contemporary examples of interventions akin to electoral pillory exist in the real world. Such methods have been used in recent elections to motivate participation in traditionally low-turnout communities, like African-American populations. Prior to the 2006 and 2008 federal elections, for example, the *Tennessee Tribune* published the names of nonvoters in select, predominantly African-American city council districts in Nashville in an attempt to induce these voters to participate in the upcoming elections. Publisher Rosetta Miller-Perry believes the 2006 intervention boosted turnout a whopping 28 percentage points, to 65% from 37% in a previous election (Fitzgerald 2008). *Dallas Examiner* publisher Mollie Belt, who has publicized voting history in the newspaper (or made it available on the newspaper's website) since 1994, believes similarly that voting in targeted, predominantly-black precincts tripled after the initial intervention in 1994 (Curry 2004). Though they have prompted advocates to promote vote-publicizing schemes vocally as a way of counteracting anemic electoral participation (Curry 2004), these claims may be exaggerated and underscore the need for reliable, scholarly estimates of the impact of such initiatives. The results of three randomized field experiments conducted in November 2007 suggest both

pride and shame, inspired by publicizing individuals' abstention or participation in elections, can effectively mobilize prosocial behavior but that shame is a more effective motivator.

This essay proceeds as follows: The following section provides a more detailed theoretical context for a series of hypotheses that are developed and that will be empirically tested using field experimental techniques. The next sections describe the experimental setting and procedures and present details about the experimental treatments. I also describe a criterion validation exercise I conducted. The experimental results are presented and analyzed in the following section. Reflections about the implications of the experimental findings are advanced in the final section.

Social Norms, Emotions and Prosocial Behavior

Theorists have long established that psychological mechanisms shape effort and behavior (Gollwitzer and Moskowitz 1996). There is an emerging consensus that emotions exert powerful influence over individuals' political attitudes and behavior (Marcus et al. 2000; Neuman et al. 2007; Brader 2005). A primary function of emotions is to initiate and guide goal-oriented, including prosocial, behavior (Barrett and Campos 1987; Cosmides and Tooby 2000; Frijda 1986; LeDoux 1996). Manipulations that activate shameful feelings in particular appear to stimulate prosocial behavior considerably (Bear et al. 2003; Gerber et al. 2008). Psychologists view shame as a painful feeling of having lost the respect of others because of improper behavior (Smith et al. 2002) and generally consider shame to be a negative emotion. People will generally try to avoid feeling shameful by complying with social norms, especially when surveillance is incorporated.

There is also evidence that positive emotions, such as pride, have the capacity to motivate prosocial behavior (Williams and DeSteno 2008). Most psychologists view pride as a "positive, self-conscious emotion arising from achievements that can be attributed to one's abilities or efforts" (cf. Williams and DeSteno 2008), Mascolo and Fischer (1995, p. 66) define pride as "[an emotion] generated by appraisals that one is responsible for a socially-valued outcome or for being a socially-valued person." This view reflects the connection between pride and public evaluations of the self, emphasizing that one is conscious and that one cares about one's evaluation by others. Not unlike the claim that shame motivates prosocial activity, Williams and DeSteno (2008) hypothesize that pride has the capacity to stimulate prosocial behavior and find empirical support for this claim in a series of laboratory experiments.

My hypothesis is that voters are likely to be responsive to efforts that publicize either adherence to or violations of social norms. The activation of shameful feelings when norm violations are publicized or prideful feelings when norm compliance is publicized should both promote electoral participation. The extent to which the impact of triggering negative emotions (like shame) compares to activating positive emotions (like pride) with respect to motivating prosocial behavior in terms of voting remains an open question, however.

Studies in psychology offer some insight on this score. Researchers consistently find evidence that negative feedback enhances performance (Brehm and Brehm 1981; Kluger and DeNisi 1996), especially if the intent is to be remedial or corrective (Murphy and Harris 2007), while positive feedback exerts less uniform effects. Notwithstanding the possibility that extremely negative feedback can result in "reactance" or cause a "boomerang effect" that impedes performance (see Ringold (2002) and Kluger and DeNisi (1996) for a discussion), critical feedback appears to enhance performance more effectively than positive feedback (Waldersee and Luthans 1994).

Affective Intelligence theory provides additional theoretical leverage on this question (Marcus et al. 2000). Marcus and his colleagues suggest emotions can be compartmentalized into two, discrete dimensions—enthusiasm and anxiety—that compare roughly to the positive/negative dichotomy psychologists have traditionally adopted. Affective Intelligence theory suggests both dimensions may influence political attitudes and behavior, but that anxiety fuels political reactions more readily. Once an individual's surveillance system has been triggered to be anxious, one will typically become more attentive to political information and developments, consider political attitudes and actions more carefully, or engage more actively in internal deliberations about political activity. Ultimately, changes in political behavior are more likely to be stimulated by anxious (negative) stimuli than by enthusiastic (positive) stimuli (Marcus et al. 2000). Taken in whole, the discussion above implies the hypothesis that shame will mobilize prosocial behavior more effectively than pride.

Experimental Setting

To explore the motivational effects of pride and shame on turnout, I conducted three randomized field experiments in the November 2007 general elections. Three cities holding municipal elections in November 2007 were selected for the studies (Ely, Iowa, Monticello, Iowa and Holland, Michigan). The selected locations were similar in terms of institutional features (municipal elections in all are nonpartisan) and electoral context (mayoral incumbents were seeking reelection against at least one challenger).[2]

Study Population

Households were randomly assigned to treatment conditions within each location. In total, 15,584 households in the three cities were included in the experiments. In Holland, Michigan, 1,850 households were assigned to be treated, while 10,551 households were assigned to the control group; in Monticello, IA, 716 households were assigned to be treated and 1,661 were in the control group; and in Ely, IA, 395

[2] I note that early voting, which is permitted in Iowa but not in Michigan, would only make the estimated treatment effects I report below more conservative.

households were assigned to be treated and 411[3] were assigned in the control group.[4] Prior to randomization, permanent absentee voters as well as those who had moved or died were removed from the sample. To ensure that random assignment generated treatment and control groups that were balanced in terms of observable characteristics, I conducted a series of randomization checks at the household level. Table 1 displays mean levels of household size, gender, age and prior voting (in the 2000 primary and 2000 and 2004 general elections) for treatment and control households in each city and reveals no differences at conventional levels of statistical significance ($p < .05$) across experimental groups within cities. Successful randomization can be confirmed using regression analysis that predicts household experimental assignment as a function of these covariates for each city. The results of F-tests presented in Table 1 indicate no significant imbalances exist in any city. Similarly, balance across experimental groups is affirmed when households across treatment locations are pooled and the regression is expanded to include indicators for treatment cities: F (6, 11995) = .78, p = .59.

The balance checks presented in Table 1 confirm observable characteristics are similarly distributed in households across treatment and control conditions within cities, but I acknowledge differences in overall distribution of some attributes across experimental locations. Even as I set out to select towns that were, on the whole, similar in terms of population size and socioeconomic character,[5] as well as political features, households in Holland, MI, exhibit lower, overall propensity to vote on average, compared to the other two locations.[6] While this heterogeneity does not compromise our ability to estimate treatment effects accurately within sites, comparisons across sites must take these differences into account. I elaborate below.

Treatments

Households randomly selected to be treated in each jurisdiction were assigned to receive postcards with a nonpartisan message that encouraged voters to participate in the local election and reminded voters that voting is a matter of public record. Additionally, postcards received by subjects in the treatment conditions in Holland, MI and Monticello, IA informed recipients that the names of voters who

[3] The ratios of subjects assigned to treatment and control conditions vary across towns due to practical considerations, primarily resource constraints. Allocations were made to maximize power given these constraints.

[4] Notwithstanding my best efforts to avoid interference, I cannot rule out the possibility that treatment subjects interacted with subjects assigned to the control conditions, thus violating the basic assumption of no interference in causal inference. I acknowledge that such violations can potentially add bias to the estimated treatment effects, but the direction of the bias can be positive or negative.

[5] According to the 2000 U.S. Census, the population in Holland, MI was 35,048, 3,607 in Monticello, IA and 1,149 in Ely, IA. Per capita incomes in 2000 were $20,857, $16,699, and $20,936 for locations respectively.

[6] As noted above, all three towns included in the experiments are small (in terms of population) and have similar socioeconomic (per capita income) characteristics, but there are some differences. Households in Holland, MI contain more women, and voters in Monticello, IA households are generally older, than in the other sites.

Table 1 Household-level relationships between treatment group assignment and covariates (mean levels)

Characteristic	Treatment	Control	Prob > F^a
Monticello, IA (Pride)			
Household size (# voters)	1.59	1.63	.28
Voted 2004	70.1	69.8	.87
Voted 2000	55.8	57.5	.39
Voted 2000 (primary)	4.9	4.5	.66
Male	44.2	46.2	.24
Age (years)	51.3	51.7	.63
N	716	1,661	
F-test: F (6, 2366) = .74, p = .62			
Holland, MI (Pride)			
Household size (# voters)	1.40	1.42	.20
Voted 2004	46.7	48.4	.16
Voted 2000	37.7	38.8	.37
Voted 2000 (primary)	14.0	14.3	.67
Male	44.0	43.5	.62
Age (years)	45.6	46.0	.43
N	1,850	10,551	
F-test: F (6, 8818) = 1.03, p = .40			
Ely, IA (Shame)			
Household size (# voters)	1.71	1.67	.44
Voted 2004	75.7	77.2	.59
Voted 2000	56.2	55.0	.73
Voted 2000 (primary)	5.2	3.4	.15
Male	49.1	48.0	.65
Age	47.2	46.7	.62
N	395	411	
F-test: F (6, 799) = .59, p = .74			
Pooled			
F-test: F (6, 11995) = .78, p = .59			

Notes: Figures in columns represent mean percentages unless otherwise indicated

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

participated in the election would be published in the local newspaper, while subjects in Ely, Iowa were told that the names of voters who failed to vote would be published.[7] This was the key manipulation in the experiments, but both treatments

[7] Ideally, subjects in each experimental location (context) would have been randomly exposed to *both* treatment conditions (pride and shame), but this was avoided, primarily for practical reasons, in order to prevent a violation of the stable unit treatment value assumption (SUTVA) that could occur as subjects in the same community interact. Given the choice to focus on relatively small towns, there would have been

conditions clearly incorporated a public disclosure component about subjects' participation in the elections.[8] The former treatment was designed to stimulate positive sentiments of pride, while the latter was intended to inspire shame (see Appendix 1 for examples of each postcard). Subjects assigned to the control condition in each jurisdiction did not receive a mailing. Postcards were mailed via first class mail 10 days prior to Election Day, with the expectation, based on information provided by the postal service, that they would be delivered 3–5 days prior to the election.

Criterion Validity

Throughout this study, I argue that publicizing citizens' participation in elections evokes feelings of pride, while publicizing abstention evokes shame. To what extent do the experimental treatments successfully induce these emotional reactions? I conduct a series of manipulation checks to explore this further. A convenience sample of 57 graduate and undergraduate students at Fordham University were randomly assigned to participate in one of two versions of a brief survey.[9] They key manipulation in the survey was to randomly vary the prime about vote publicizing. Consistent with the two versions of the experimental treatment conditions, respondents in one version of the survey were informed about a proposal to publish the names of nonvoters in the local newspaper, while respondents in the alternative version were told the names of voters would be publicized. All respondents were then asked to report the degree to which having their names included on the list would elicit feelings of pride or shame using a nine-point scale on which 0 indicates respondents "would not feel this emotion at all" and 8 signified they "would feel this emotion very strongly."[10] All surveys included standard demographic questions (age, race, gender, party identification, and an item used to probed political knowledge).[11]

A randomization test confirms background characteristics between respondents in the two versions of the survey are balanced. An *F*-test of significance for a regression in which assignment to the survey version is regressed on gender, age,

Footnote 7 continued
such a danger in introducing both treatments in any one setting. Such contamination could have compromised the reliability of the experimental results.

[8] The original intent was to publish voters' names as indicated. After receiving several calls from local elections officials expressing concerns about publishing names in the newspapers, I decided to bypass this step. This decision followed the election, however, and there is no reason to suspect subjects would have anticipated the names would not ultimately to be published. I expect the decision not to publish the names exerted no influence on subjects' reaction to the initial intervention.

[9] The post-experimental criterion validity check was conducted between October 30 and November 2, 2008. Ideally, the manipulation check would have been conducted in advance, but the results should be consistent. Students completed the survey online and data was collected using www.surveymonkey.com.

[10] This approach is adopted from work in social psychology. See Lerner and Keltner (2001) and Lerner et al. (2004) for details.

[11] Details about the manipulation check and a complete questionnaire are available upon request.

race, party identification and political knowledge are independent variables is, as expected, insignificant F (8, 47) = .37, p = .93.

Respondents in the version of the survey that indicated abstainers' names would be publicized reported they would feel significantly more shameful ($M_{\text{shame}} = 4.78$) than proud ($M_{\text{pride}} = 1.48$; $p < .01$) if their name appeared on the newspaper list. By contrast, the expectation of prideful feelings was significantly greater than shameful sentiments for respondents who were told the names of voters would be publicized ($M_{\text{pride}} = 5.03$ vs. $M_{\text{shame}} = .31$, respectively, $p < .01$). Moreover, differences in mean levels of pride and shame between the two survey versions are significant at the $p < .01$ levels.

These results suggest the manipulations employed in these experiments should create an expectation or anticipation of a future sense of pride or shame, accordingly, upon publication of their names. Subjects told compliance with social norms (voting) would be publicized would feel prideful, while subjects told violators (abstention) would be publicized would feel shame.

Experimental Results

Following the November 2007 elections, I obtained validated voter turnout data from the official voter files in each locality. Table 2 reports the overall, individual-level turnout rates for subjects assigned to each treatment condition in each city. The intent-to treat effects (ITT) reported are based on assignment to treatment condition. Notwithstanding my efforts to expose all subjects assigned to the treatment conditions to the stimulus, some subjects were not successfully contacted (the postcards cards were returned as undeliverable).[12] In the three experiments I conducted, 88.0% of subjects assigned to be treated in Holland, MI, 94.7% in Monticello, IA and 92.3% of subjects in Ely, IA were successfully contacted. The actual, average treatment-on-treated effects (ATT) reported in the final column of Table 2 take these contact rates into account. To obtain an accurate estimate of the direct effect of contact, it is necessary to divide the intent-to-treat effects described above by the contact rate. This is functionally equivalent to performing a two-stage least squares regression of vote on actual contact using randomization as an instrumental variable (Angrist et al. 1996; Gerber and Green 2000, 2005), an exercise I conduct below.

The average treatment-on-treated effects (ATT) reported in Table 2 indicate receiving the pride treatment in Holland, MI boosted turnout modestly (by 0.9 percentage points), although this effect was not significant at conventional levels.

[12] I note the criterion I adopt to determine "successful contact" (postcards were not returned) is quite conservative. It is conceivable that failure to treat subjects could have resulted from other impediments (cards were delivered late, discarded without being seen by the intended household members, or not looked at until after the election). I acknowledge that non-treatment of cases assigned to be treated may include observable (returned postcards) as well as unobservable non-treatment. Such measurement error in a known direction would plausibly increase the estimated treatment effects, however. A narrower definition of "successful contact" to take such circumstances into account would likely only enhance the number of unsuccessfully treated subjects, thereby increasing the magnitude of the observed treatment effects.

Table 2 Estimated effects of two mail treatments on voter turnout in the November 2007 elections (by treatment location)

Location	Turnout rates (percent voting)				
	Control	Treatment	Intent-on-treated (ITT) (%)	Contact rate (%)	Treatment-to-treat (ATT) (%)
Pride treatments					
Holland, MI	24.4%	25.2%	+0.8	88.0	0.9
N	(13,620)	(2,298)			
Monticello, IA	30.9%	35.4%	+4.5**	94.7	4.7**
N	(1,470)	(653)			
Shame treatment					
Ely, IA	15.6%	21.9%	+6.3***	92.3	6.9***
N	(685)	(674)			

Notes: Subjects assigned to be treated in Holland, MI and Monticello, IA were mailed the "pride" treatment, while subjects assigned to be treated in Ely, IA were mailed the "shame" treatment. Figures in parentheses represent total number of individuals in each experimental condition accordingly. Contact rate represents the percentage of unreturned postcards. *** Indicates effect is statistically significant at $p < .01$ level, and ** at the $p < .05$ level, using one-tailed tests

Receipt of the pride treatment in Monticello, IA elevated the electoral participation rate by 4.7 percentage points, however, an effect that is statistically significant at the $p < .05$ level. Turnout among subjects who received the shame treatment in Ely, IA, was 6.9 percentage points higher than among voters in the control group, a sizable and statistically-reliable (at $p < .01$) effect.

As I note above, average treatment-on-treated effects can also be estimated using two-stage least squares regression analysis (2SLS) of vote on actual contact using randomization as an instrumental variable. Assignment to treatment condition is a perfect instrument for contact because assignment causes contact but is statistically independent of other causes of voting behavior (Gerber and Green 2000, 2005). This estimator also permits the additional inclusion of control variables to correct for imbalances between experimental groups due to chance.

Estimates of the effects reported in Table 2 suggest that both the pride and shame treatments elevated voter turnout, but that the shame treatment did so more effectively. To explore these findings further, I conduct a series of additional, individual-level analyses to take into account both the failure to treat as well as the issue of sampling variability. Proper estimation of standard errors for individual-level analyses that were randomized at the household level requires a correction to account for the possibility that members of households share unobservable characteristics (Gerber et al. 2008). Robust cluster standard errors take intrahousehold correlation into account (Arceneaux 2005).

Table 3 reports the results of a series of two-stage least squares regression analyses in which individual turnout is regressed on actual contact and random assignment is the instrumental variable. I estimate average treatment-on-treated effects separately for each experimental location; I also pool the samples to compare

Table 3 Estimates of the effects of two mail treatments on voter turnout in November 2007 (by treatment location and pooled)

Independent variables	Holland, MI (Pride)	Holland, MI (Pride)	Monticello, IA (Pride)	Monticello, IA (Pride)	Ely, IA (Shame)	Ely, IA (Shame)	Pooled	Pooled
Pride treatment	.009 (.013)	.015* (.011)	.047** (.028)	.045** (.026)			.016* (.012)	.020** (.010)
Shame treatment					.069*** (.029)	.069*** (.028)	.069*** (.029)	.069*** (.028)
N	15,918	15,918	2,123	2,123	1,359	1,359	19,400	19,400
Covariates[a]	No	Yes	No	Yes	No	Yes	No	Yes
R^2	.00	.26	.00	.14	.01	.05	.01	.24
RMSE	.430	.369	.468	.433	.39	.382	.431	.377

[a] Covariates: Voting in the 2000 primary and the 2000 and 2004 general elections. Estimates derived from 2SLS using treatment assignment as an instrument for successful treatment. Dependent variable is voter turnout in the 2007 general election. Robust cluster standard errors (in parentheses) account for clustering of individuals within household, the unit of random assignment. Using one-tailed tests, * signifies $p < .10$, ** signifies $p < .05$ and *** signifies $p < .01$. Pooled models include dummy indicators for location strata and interactions with covariates

pride and shame effects directly. In each case, I estimate two models: one model expresses individual voter turnout as a linear function of the experimental treatment conditions, and the second model includes prior voting (in the 2000 primary election and the November 2000 and 2004 general elections), age, and gender as covariates to potentially improve the statistical precision of the estimated treatment effects (the pooled models also include controls for the experimental locations and interactions between sites and covariates).[13] The standard errors I report are clustered at the household level, the unit of randomization.

The results displayed in Table 3 suggest that both the positive and negative stimuli boosted turnout levels, but that the shame treatment was considerably more effective on average. Overall, the pooled model (with covariates) suggests the average effect of the pride treatment is 2.0 percentage points (standard error = 1.0), while the estimated effect of the shame stimulus is 6.9 percentage points (standard error = 2.8). The shame treatment is significantly more effective than the pride treatment ($p < .05$, one-tailed test).

There are two possible interpretations of the overall patterns I detect. One possibility is that there is a meaningful pride versus shame effect. Alternatively, the results may be a function of sampling variability. I note there seems to be heterogeneity in the estimated effects of the pride interventions across the two experimental locations. The impact of the pride treatment in Monticello, IA outstrips the effect of the same treatment in Holland, MI considerably. By contrast, the magnitude of the shaming effect I uncover is quite strong and remarkably consistent with the estimated impact of similar field experimental treatments designed to exert social pressure to vote reported by Gerber et al. (2008). The question, thus, is not whether or not the shame treatment mobilized voters effectively, but whether or not the pride treatment did so as well.[14] These possibilities encouraged me to look for individual-level factors that may have conditioned the effects of the treatments. I consider these in the following section.

A Closer Look at Treatment Effect Heterogeneity

Given the infeasibility of administering both the shame and pride treatments in the same experimental location (see discussion above), and the fact that some background characteristics, including baseline voting propensity or community size, differ somewhat across the three experimental locations, readers may be concerned about whether or not the heterogeneity that exists affects our ability to compare treatment effects across towns. With just three towns in the experimental sample, we cannot know with certainty if contextual differences or sampling variability account for the differences we observe across sites. We can, however,

[13] I also include a dummy variable to account for cases of missing or unavailable covariate data.

[14] I note that restricting the analysis to comparisons between the two Iowa towns included in the study, which exhibit greater similarities (in terms of geographic location (Cedar Rapids metropolitan area), demographic composition, and baseline voting propensity), suggests the pride and shame treatments were roughly equally effective.

Table 4 Estimates of the effects of two mail treatments on voter turnout in November 2007 by vote propensity[a]

Independent variables	Holland, MI (Pride)		Monticello, IA (Pride)		Ely, IA (Shame)		Pooled	
	Low-propensity	High-propensity	Low-propensity	High-propensity	Low-propensity	High-propensity	Low-propensity	High-propensity
Pride treatment	−.001 (.010)	.033** (.019)	.033 (.031)	.049* (.037)			.004 (.010)	.037** (.017)
Shame treatment					.075** (.035)	.064** (.040)	.075** (.035)	.064** (.040)
N	8,789	7,129	893	1,230	613	746	10,295	9,105
R^2	.03	.13	.08	.02	.05	.01	.04	.12
RMSE	.264	.464	.328	.494	.315	.430	.273	.465

[a] Voters who voted in fewer than two of the three prior elections (2000 primary, 2000 and 2004 general elections) are classified as "low propensity" voters, and voters who voted in two or more of these elections are considered "high propensity" voters. All models include the following covariates: Voting in the 2000 primary and the 2000 and 2004 general elections, age, gender. Estimates derived from 2SLS using treatment assignment as an instrument for successful treatment. Dependent variable is voter turnout in the 2007 general election. Robust cluster standard errors (in parentheses) account for clustering of individuals within household, the unit of random assignment. Using one-tailed tests, * signifies statistical significance at the $p < .10$ level and ** at the $p < .05$ level. Pooled models include dummy indicators for location strata and interactions with covariates

look at individual-level heterogeneity within towns to examine this more closely. I am mainly concerned about any potential interaction between voting propensity and the treatments. As I note above, subjects in Holland, MI, the largest of the three sites, exhibit substantially lower propensity to vote compared to both Monticello, IA and Ely, IA. Recent scholarship suggests voting propensity conditions the effects of efforts designed to mobilize voters (Arceneaux and Nickerson 2009; Parry et al. 2008; Niven 2004). Niven's (2004) study of a 2001 municipal election in Boynton Beach, Florida finds that voters across the board were mobilized by face-to-face contact, but that intermittent voters (those with mid-level propensities to vote) were most responsive to the intervention. Parry et al. (2008) use observational data to show that seldom voters can be mobilized by targeted campaign communications in high-salience contests. More recently, Arceneaux and Nickerson (2009) re-analyze 11 field experiments designed to mobilize voters in various electoral settings to show that high-propensity voters will be most responsive to mobilization efforts in low-salience elections, such as the ones in which our interventions were executed. With respect to the current study, the pride treatment itself is likely to induce an interaction with vote propensity because it promises to reward those who vote. Table 4 presents comparisons of the treatment effects in each experimental location (as well as a pooled analysis) for subgroups of voters based on propensity to vote. I classify voters who had voted in fewer than two of the three previous elections I examined (the 2000 primary and 2000 and 2004 general elections) as low-propensity voters, and subjects who voted in two or more of these elections as high-propensity voters. The results suggest the pride treatment operated similarly in both Holland, MI and Monticello, IA, effectively mobilizing only high-propensity voters, despite the differences between these two sites in terms of population and baseline vote propensity. These results bolster claims that comparisons across the experimental sites are appropriate. By contrast, the shame treatment motivated turnout amongst both low- and high-propensity voters roughly evenly. It appears that dangling the prospect of publicly extolling voters for norm compliance (in this case, voting) only mobilizes those who are likely to vote, while the prospect of public shaming for failure to comply motivates voters across the board. This result may account for the stronger average treatment effects associated with the shaming intervention overall. But among high-propensity voters, both pride and shame appear to motivate compliance with voting norms.

Discussion

Field experimental studies of voter mobilization routinely prime social norm considerations by incorporating appeals to civic duty to motivate participation, but the introduction of social surveillance is relatively new (Gerber et al. 2008). The field experiments I conducted extend this line of research and reinforce the notion that social surveillance is an active ingredient in norm compliance. More generally, this study bolsters the emerging consensus that activating powerful, underlying psychological mechanisms can prompt citizens to political action and that the inclusion of psychological triggers exerts more potent effects than generic appeals.

The experimental results I report in this paper provide empirical support for the hypothesis that interventions designed to publicize individuals' voting behavior (or lack thereof) will effectively promote participation. Social pressure, triggered by emotional reactions to stimuli, appears to motivate prosocial behavior. While the experimental evidence I present suggests that activating positive emotions (pride) may promote socially-responsible activity, I find, consistent with the hypothesis developed above, stronger evidence that stimuli that activate negative feelings of anxiety or shame do so more effectively on average. That said, the heterogeneity in treatment effects I report above suggests the picture may be more nuanced: shame seems to motivate both low- and high-propensity voters, while the pride effects appear limited to high-propensity voters. These findings are theoretically compelling and open up new possibilities for subsequent inquiry and experimental exploration. Replication is necessary to sharpen and confirm these initial conclusions.

An experimental research agenda that focuses further on the mobilizing capacity of shame or pride seems especially promising. In some places, such as North Carolina, fore example, voting records are public and readily available online. Can similar effects be generated by informing subjects that their voting records are (or will be) made public without any editorial remarks that shame or praise them? Alternatively, it would be interesting to examine the effects of giving voters the *option* of having their own (of their friends' or neighbors') voting records publicized. One could also investigate whether or not shame and pride operate differently depending on the overall visibility of the medium used to publicize subjects' voting behavior, employing television, the Internet, or radio, for instance, as opposed to newspapers. Such extensions could yield further insights about the relationship between shame and pride and prosocial activity, and, more generally, about how emotions like these interact with social pressure, social norms, public surveillance and contextual factors to foster (or hinder) political behavior.

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Appendix 1: Treatments Details

[Pride Treatment Example: Holland, MI]

WHO VOTES IS PUBLIC INFORMATION!

Dear registered voter:

On November 6, 2007, an election to select local leaders will be held in the city of Holland, MI.

As a registered voter, you are eligible to vote in this election. We urge you to exercise your civic duty and vote on November 6th.

We also remind you that who votes is a matter of public record.

To honor those who take time to vote in the upcoming election, we will obtain a complete list of registered voters who cast ballots on Election Day from local election officials in Holland and publish their names in the local newspaper.

The names of voters who did not vote will *not* be published because only voters deserve special recognition.

DO YOUR CIVIC DUTY! VOTE ON ELECTION DAY!

[Shame Treatment Example: Ely, IA]

WHO VOTES IS PUBLIC INFORMATION!

Dear registered voter:

On November 6, 2007, an election to select local leaders will be held in Ely, IA.

As a registered voter, you are eligible to vote in this election. We urge you to exercise your civic duty and vote on November 6th.

We also remind you that who votes is a matter of public record.

To promote participation in the election, we will obtain a complete list of registered voters who cast ballots on Election Day from local election officials. Shortly after the November 2007 election, we will publish in the local newspaper a complete list of all Ely registered voters who did not vote.

The names of those who took the time to vote will not appear on this list.

DO YOUR CIVIC DUTY! VOTE ON ELECTION DAY!

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