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Social pressure and voting: A field experiment conducted in a high-salience election

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ABSTRACT

A large-scale experiment assessed the turnout effects of the "Neighbors" mailer, which exerts social pressure to vote by disclosing the past turnout records of recipients and their neighbors. A prior large-scale experiment conducted in a low salience election found that this mailer increased turnout substantially. The experiment reported here gauges the effects of this mailer in the context of a hotly contested recall election. We find smaller but still sizable effects, especially for low-propensity voters. Turnout increases significantly in the presidential election several months later, and the immediate and downstream effects are similar regardless of whether the mailer is worded in partisan or nonpartisan terms. Using data furnished by the Obama campaign and several nonpartisan organizations, we find little evidence that receiving the Neighbors mailer caused people to become the targets of subsequent mobilizing activity, suggesting that the downstream effects of social pressure cannot be attributed to subsequent campaign contacts.

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A burgeoning academic literature investigates the extent to which voter turnout increases in the wake of *social pressure*, the forceful assertion of prescriptive norms. Social pressure can be exerted in many ways: by scolding citizens who do not uphold their civic duty, by monitoring compliance with civic norms, or by disclosing to others whether citizens actually fulfill their obligation (Green and Gerber, 2010). Field experiments testing the effects of social pressure date back to Gosnell's (1927) study of the 1924 election, in which he mailed Chicagoans political cartoons depicting non-voters as unpatriotic "slackers." Informing citizens that turnout will be monitored using public records dates back at least to Gross et al. (1974), who tested the effect of such warnings on voter turnout in college elections. Decades later, this line of inquiry was revived by Gerber et al. (2008), whose experimental study of social pressure sparked a series of follow-up experiments that shed light on the conditions under which social pressure's effects are large or small.

Set in the context of a largely uncompetitive 2006 primary election in Michigan, the Gerber et al. (2008) experiment involved five randomly assigned groups, a control group consisting of 100,000 households and four groups of 20,000 households apiece that received a single piece of mail. The first treatment group was reminded that voting is a civic duty. The second group was informed that they were part of a study that would monitor whether they voted. A third group received the "Self" mailer, which reported that "WHO VOTES IS PUBLIC INFORMATION!" and presented an official-looking chart indicating whether each member of the household voted in two recent elections. The fourth treatment group received the "Neighbors" mailer, which included not only the voting records of those in the household but also others living on the block. Both the Self and Neighbors mailers also promised a future mailer that would update the voter turnout log with information from the upcoming election.

The authors found a steady progression in treatment effects: the more social pressure a mailing exerted, the higher the turnout rate in the primary election. Two mailers in particular had enormous effects: the Self mailer increased turnout by 4.8 percentage points and the Neighbors mailer, by 8.1 percentage points. Both of these effects are more than an order of magnitude larger than the average piece of nonpartisan mail (Green et al., 2013). Indeed, years after the original Neighbors mailing, it continues to have a statistically significant effect on the voter turnout rates of those who received it (Davenport et al., 2010; Coppock and Green, 2016). This manuscript presents an experiment examining an intervention that is as similar to the Neighbors mailing as anything that researchers since Gerber et al. (2008) have evaluated. The experiment was conducted in the

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context of a major state-level election, the Wisconsin gubernatorial recall election of June 5, 2012.

The Gerber et al. (2008) study ranks among the most widely cited articles published in political science since 2008,[1] but its most arresting finding, the extraordinarily large effect of the Neighbors mailer, has not been the subject of academic research investigating its effects in other contexts. The lack of direct follow-up studies stands in contrast to research exploring other aspects of social pressure including the prospect that others may find out if one voted (Rogers et al., 2016; Panagopoulos, 2010, 2013; DellaVigna et al., 2014) and variants on the Self mailer, whose effects have been replicated in a variety of settings (Gerber et al., 2010; Sinclair et al., 2012) and formats (Mann, 2010; Abrajano and Panagopoulos, 2011; Matland and Murray, 2014; Panagopoulos et al., 2014). Our study replicates the key features of the Neighbors mailing in the context of a highly salient state-level election – the Wisconsin gubernatorial recall election of June 5, 2012.

Although the Neighbors mailer has been used widely by political campaigns – millions, for example, were sent out prior to the 2012 presidential election by the group Americans for Limited Government (WRAL, 2012) – to our knowledge, no experimental tests have been reported in the public domain. The lack of follow-up experiments by academic researchers may reflect the negative reaction that this mailer provokes. Green and Gerber (2008, p.72) counsel campaigns against using the Neighbors mailer[2] but offer no figures describing the extent of public outcry (no such data were collected during the 2006 study). One practical contribution of the present study, which was conducted by a campaign that shared the data with researchers, is that it measures responses to this type of mailer.

The principal substantive contribution of this study is that it assesses the effectiveness of the Neighbors mailer in the context of a high-salience election using the types of voter targeting strategies that large campaigns typically employ. Voter turnout among control group subjects in the Gerber et al. (2008) experiment was 29.7%. By contrast, the present study took place amid a widely publicized gubernatorial recall election that attracted national attention, lavish campaign spending, and a steady stream of campaign communication. Average turnout in our control group is close to 65.4%. How effective is the Neighbors mailer when it must compete against a barrage of other mailers for voters' attention and when the typical recipient is someone who already has a high likelihood of voting before receiving the social pressure message?

The experiment also speaks to three other theoretical questions. The first concerns persistence in voting behavior, specifically, the hypothesis that exogenous factors that induce voting in one election have repercussions for turnout in subsequent elections (Gerber et al., 2003; García Bedolla and Michelson, 2012). As noted above, a substantial body of evidence suggests that social pressure interventions have effects that last for several election cycles; less clear is whether persistence is attributable to the targeting strategies of campaigns. If campaigns focus their mobilization efforts on those who recently voted, apparent "habits" may merely reflect subsequent campaign attention. The process by which treatments cause participants to be exposed to behavior-reinforcing activities of other unrelated organizations has been described as "rip currents" (Frey and Rogers, 2014). This metaphor alludes to the oceanographic phenomenon of strong currents of water running perpendicular to beaches: swimmers can be unaffected when just a foot or two away from the currents, but if they enter the currents they can be carried out as much as a mile offshore. The present study offers a rare opportunity to test the rip currents hypothesis experimentally. Shortly after the June 5th election, the Obama For America (OFA) campaign ramped up its fall presidential bid in Wisconsin. OFA and other allied organizations provided us information summarizing the rates at which our experimental groups were contacted in the summer and fall of 2012.[3]

The second theoretical question concerns the motivational power of partisan versus nonpartisan appeals. The Neighbors mail urged recipients to do their civic duty; an alternative appeal framed the failure to vote as letting down one's partisan team and, in so doing, electing the Republican governor. Our experiment compares the relative effectiveness of normative appeals that focus on civic duty and partisan fealty. It is unclear, ex ante, which will carry more force. The social norm of civic duty is widely shared (Gerber et al. 2016), but the norm of supporting one's partisan team might resonate strongly in the context of a highly polarized campaign. The campaign deploying the mailers believed that both appeals would work and conducted the test because they were uncertain about which would be more effective.

Finally, the Neighbors message was framed in two different ways, one that urged the recipient to vote and another that further urged the recipient to mobilize his or her neighbors to vote. From a theoretical standpoint, the "mobilize others" message has two components. The first is a version of the door-in-the face phenomenon (Cialdini et al., 1975) whereby a large request (mobilize others) makes it more likely that recipients will accede to a smaller request (go vote). This psychological phenomenon has been widely confirmed in non-political domains but has rarely been tested in political settings (see McCabe and Michelson, 2015 for a recent exception showing no effect on turnout). The second mechanism, akin to the theories about visualizing the implementation of a proposed activity (Nickerson and Rogers, 2010), is to increase the salience of the message by encouraging the recipient to envision the process of mobilizing others. This hypothesis has rarely been tested experimentally. The one study of which we are aware is Ha and Karlan (2009), which found that phone calls from a commercial phone bank were no more effective at mobilizing voters when callers urged respondents to encourage others to vote. To our knowledge, the present study is the first to test the "mobilize others" message using direct mail.

The paper is organized as follows. We begin by providing some background on the unusual gubernatorial recall elections in which our study took place. We describe the experimental design, focusing in particular on the four different variants of the Neighbors mailer used in this study. Before presenting the turnout results, we first characterize the backlash that the Neighbors mailers provoked. The effects of the mailers on voter turnout in June and November are shown to be smaller than in Gerber et al. (2008) but substantial nevertheless, especially among those with lower-than-average turnout propensities. The turnout effects of the four mailers are found to be statistically indistinguishable from one another. Despite the intervention's significant effects on turnout in June, it has little effect on subsequent campaign contact by the Democratic presidential campaign or its nonpartisan allies, implying that the enduring effects of the mailers on turnout are due to psychological processes rather than the campaign environment. We conclude by discussing the implications of our findings for the theory and practice underlying social pressure tactics in voter mobilization campaigns.

[1] Web of Science citations accessed August 26, 2016, with search limited to journal articles in the Political Science category from 2008 to 2016.

[2] Green and Gerber (2008, p.72) advise "Although this type of mail is inexpensive and highly effective, we would warn readers against using this tactic… [M]any people who receive this mail become irritated at the prospect of a campaign snooping on them and scolding them in front of their neighbors. Your phone will ring off the hook with calls from people demanding an explanation."

[3] Only Democratic-leaning voters were part of the experiment, so our lack of contact data from right-leaning organizations should not affect our results.

1. Experimental setting and design

1.1. Setting

In 2011, Republican Governor Scott Walker's administration supported legislation that barred public sector unions in Wisconsin from bargaining collectively over pensions and healthcare, prompting a series of protests that led to the circulation of a recall petition. On January 17, 2012, organizers of the recall petition announced that they had secured nearly 1 million signatures, almost the same number of votes cast for Walker in 2010. Approximately 901,000 signatures were certified and, on March 30th, the Government Accountability Board of Wisconsin voted to order the recall. On May 8, 2012, Tom Barrett, Scott Walker's Democratic opponent in the 2010 gubernatorial elections, won the Democratic primary for the June 5, 2012 recall election. The ensuing election campaign featured the largest expenditures for a single election in Wisconsin history. Walker and supporters spent $58.7 million, compared to $22 million by Barrett and around $300,000 by independent candidate Hariprasad Trivedi (Bauter, 2012).[4] A total of 2.5 million votes were cast in this election, which means that total candidate spending amounted to $32 per vote cast. On June 5th, Scott Walker won the recall election by nearly 200,000 votes, or almost seven percentage points. The turnout rate was 57.8%, the highest rate for Wisconsin in a nonpresidential election since World War II (Gilbert, 2012).

1.2. Campaign

The Greater Wisconsin Committee (GWC) describes itself as a progressive issue advocacy organization (Greater Wisconsin Committee, 2014). It strives to inform the public about policy issues through grassroots and media campaigns. Established by the GWC, the Greater Wisconsin Political Fund is a 527 independent political organization that advocates on behalf of issues, public officials, and candidates for public office.

1.3. Subjects

Greater Wisconsin targeted 664,020 registered voters who had been identified as likely supporters of the Democratic candidate, Tom Barrett.[5] The Democratic partisans who comprise the subject pool include a disproportionate number of voters with high probability of casting a ballot (based on their prior vote history). This targeting strategy was thought to be optimal at the time given the information available. A source familiar with the Greater Wisconsin targeting criteria explained that

> [s]ince the recall election was so unusual and in a state without party registration, it was especially challenging for organizations running the program to find optimal targets. The targeting criteria selected for this experiment was not out of the ordinary. A proprietary turnout model built specifically for this election was used to target low to moderate propensity voters. But because there was additional budget remaining and it was not clear if even high turnout voters would end up turning out in such an odd election, high turnout voters were included as "third tier" targets.[6] (anonymous, personal communication, August 24, 2016).

While this targeting strategy would not be standard in all elections, it represents the type of targeting approach a typical campaign might have pursued under similar circumstances. Table 1 describes other known characteristics of the subject pool.[7]

1.4. Experimental design

Five percent of the subject pool was placed into an untreated control group, and the remainder was divided equally among the four versions of the Neighbors mailer. Individual voters were allocated using complete random assignment. We focus here on voters residing in households in which they were the only voter included in the experimental universe.[8] Restricting the subject pool in this way has no material effect on the results and simplifies the statistical analysis.

1.5. Treatments

The four mailers used in this study were delivered in the final week of the campaign. Examples of each mailer appear (with fictitious names) in the Appendix. For purposes of comparison, the Appendix also presents the original Neighbors mailer from Gerber et al. (2008). The mailers used in Wisconsin share several basic ingredients. Each presents voters with their own turnout record for the previous two federal elections (the name of the addressee appears at the top of the list), the turnout records for a dozen neighbors assigned to the treatment group, and a blank entry for the upcoming June 5th election. The accompanying text explains that "After the June 5th election, public records will tell everyone who voted and who didn't."

The experimental mailers differed along two dimensions. The first factor varied whether the wording was nonpartisan (as in the original Michigan mailing) or in opposition to Scott Walker. The nonpartisan mailers opened with:

> **Who votes is public record!**
>
> 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.

[4] It should be noted that there were other races on the ballot (including the office for Lt. Governor and four state senators). The presence of these additional races meant there was even more campaign outreach than indicated by candidate spending estimates, which would likely further dilute the marginal effect of any single piece of GOTV mail.

[5] Likely supporters are defined as all individuals with scores 50 or greater on GWC's proprietary Barrett support model, which was developed using commercial data and in-house surveys. See Appendix B for a distribution of Barrett support scores. Restricting the sample to Barrett supporters limits the generalizability of the results, although it should be noted that treatment effects in the Gerber et al. (2008) study did not vary appreciably by party support scores.

[6] There were three tiers of targets. Greater Wisconsin initially intended to mail only the first tier but included the other two tiers when its budget expanded. The first tier comprised 2008 General Election voters who had not voted in 2010 General Election. The second tier included voters predicted to have low to moderate likelihood of voting. The third tier was composed of voters who were modeled to have a high probability of voting.

[7] Besides the standard demographics, the table depicts the subject pool's turnout rate in a similar recall election, which gives the reader a sense of the subjects' underlying turnout propensity.

[8] Targeting one voter per household was the original experimental plan, but a programming error admitted a small percentage of larger households into the study population. As a result of this error, some voters who were randomized as if they lived at different addresses actually shared a household with another voter in the experimental universe. In total, 2.3% voters in the experimental universe were affected by this error. Including these additional voters (with the appropriate statistical adjustments to account for interference) has no effect on the results reported below.

Table 1
Background characteristics of the subject pool.

Mean Election Day Age[a]	Mean Barrett Support[b]	Turnout in 2011 Senate Recall Election	% Female
54.2	87	42.3%	61.4%

Notes:
[a] Age data available for 66.0% of the registered voters in the subject pool.
[b] Barrett support is derived from a proprietary model used by The Greater Wisconsin Committee. The model uses commercial analytics data and survey data to impute support for Barrett. The model is ordinal and does not necessarily translate to likelihood of supporting Barrett.

The anti-Walker variants instead led with:

Scott Walker won in 2010 because too many people stayed home!

Two years ago, more than half a million Wisconsinites who supported Obama failed to vote in the 2010 election. And that's how Governor Scott Walker got elected. 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 nonpartisan and advocacy mailings were each subdivided into two variants. After declaring, "We need to all pull together," the standard version proceeded to show the turnout chart. The "mobilize others" version instructed recipients to "Look at the list below: are there neighbors on this you know? Call them or knock on their doors before Election Day, and ask them to go vote on Tuesday, June 5th."

In sum, the four mailers form a 2 × 2 design. The first factor varies according to whether the content is nonpartisan or opposes Scott Walker. The second factor varies according to whether the recipient is encouraged to mobilize his or her neighbors. Variations in mailing content shed light on the relative effectiveness of couching the obligation to vote in partisan terms and of urging the recipient to take an active role in mobilizing others.

A close comparison of the mailers used in this experiment to the original Neighbors mailing reveals some subtle modifications. The mailings used here insert the phrase "We need to all pull together." Perhaps more importantly, the heading "Who votes is public information!" from the Gerber et al. (2008) "Self" mailing replaces "WHAT IF YOUR NEIGHBORS KNEW WHETHER YOU VOTED?" The latter change was made to maintain comparability across all four forms, two of which encourage recipients to mobilize *their* neighbors and would not have made sense with the original wording. Finally, the Wisconsin mailings do not promise to send an updated turnout chart but warn that "Public records will tell everyone who voted and who didn't." We cannot rule out the possibility that these wording changes undercut the effectiveness of the original mailer but would note that wording changes to the Self mailer (e.g., deleting the promise to send an updated vote history, cf. Sinclair et al., 2012) do not appear to undermine its effect so long as forceful assertion of voting norms is coupled with presentation of voting records.

1.6. Balance check

The number of subjects allocated to each experimental group is depicted in Table 2. For each experimental group, the table reports voting rates in three prior elections, the 2012 presidential primary, the 2011 Senate Recall election, and the 2010 November election. The groups, as expected, have similar voting profiles, and a randomization check confirms that treatment assignment is not significantly related to voting in previous elections. A multinomial logistic regression of the five assigned experimental conditions on voter turnout in 34 previous elections dating back to 2000 yields a log-likelihood statistic whose p-value is 0.60.

1.7. Outcome measurement

After the election, turnout records were updated to reflect voting in both the 2012 recall election and the 2012 November general election. We also measured the volume of communication associated with the four mailers, each of which provided a distinct email address.

1.8. Power

The large number of voters gives the study ample power to detect turnout effects. For a comparison of all mailings versus the control, the design has approximately 80% power to detect an effect of 0.7 percentage points using a one-tailed test. For any pairwise comparison of two mailings, the design has approximately 80% power to detect a turnout effect of 0.5 percentage points using a two-tailed test.[9]

2. Voter and media responses

As these mailers began to arrive in mailboxes on the Thursday and Friday before Election Day, June 5th, 2012, they immediately attracted the attention of journalists and bloggers. By late evening on Friday, discussions and accompanying photos of the mailers appeared on online political forums as well as conservative bloggers' websites. By Saturday, the *National Review* and other conservative media outlets picked up the story. On Monday, local newspapers, most prominently the *Milwaukee Journal Sentinel*, followed suit. Press coverage tended to be critical of the mailers.

The mailings also drew criticism from voters. Over the course of the next week, a total of 1524 emails were sent to the four addresses listed on the mailers, and another 638 messages were conveyed via Greater Wisconsin's website or left on its voicemail. Although we do not have access to the content of the emails sent to the four addresses, we do have access to the messages conveyed via the website. These comments are indignant and irate, in keeping with laboratory tests that examined reactions to the original Neighbors message (Matland and Murray, 2013). Interestingly, if the number of responses to the mailings is any guide to the degree of outrage, the mailer that most closely resembles the original Neighbors treatment was not more off-putting than the other variants, attracting 260 emails as compared to 457, 388, and 419 in the nonpartisan encouragement, partisan no-encouragement, and partisan encouragement conditions, respectively. Taken together, the mail sent to more than 600,000 households produced 2162 emails, which is roughly one for every 300 households that received a mailer.

3. Turnout results

How did the mailers affect voter turnout in the runoff election? Table 3 presents results for each experimental group and for all treatment groups combined. All four treatments elevated voter turnout to approximately the same extent. Pooling over treatments, the overall effect is 1.0 percentage point, and an F-test of the null hypothesis that all four treatments generated the same effect cannot be rejected ($p = 0.36$). In other words, in the context of a highly salient campaign that targets likely supporters, we find an estimated average treatment effect of 1.0 percentage point, which is far weaker than the 8.1 percentage point effect reported by Gerber

[9] For details about these calculations, see Appendix C.

Table 2
Covariate balance across assigned treatment groups.

	Control	Neighbors + Urge Others	Neighbors	Partisan + Urge Others	Partisan	chi-square test
2012 Presidential Primary Turnout	30.97%	31.24%	31.13%	31.08%	31.28%	p = .651
2011 Senate Recall Turnout	42.44%	42.26%	42.28%	42.42%	42.34%	p = .897
2010 General Turnout	59.56%	59.31%	59.13%	59.28%	59.51%	p = .243
N	32,609	153,963	153,952	154,216	154,010	

Table 3
Turnout rates in the June recall election, by experimental condition.

	Control	Neighbors + Urge	Neighbors	Partisan + Urge	Partisan	All Treatments
June Recall Election Turnout	65.38%	66.34%	66.37%	66.38%	66.61%	66.42%
N	32,609	153,963	153,952	154,216	154,010	616,141

Notes: Using regression to estimate the effect of receiving any treatment (the rightmost column) on turnout, we obtain an estimate of 0.0105 with a standard error of 0.0027. After controlling for voter turnout in all previous elections dating back to 2000, the estimate is 0.0107 with a standard error of 0.0019.

et al. (2008). The differences cannot plausibly be attributed to sampling variability given that the standard errors of the two studies' estimates are each 0.3 percentage points.

In part, the difference in effect sizes reflects differences in the subject pools; whereas the Michigan study restricted the subject pool to those with moderate probabilities of voting (see Gerber et al., 2008, pp.36–37), the Wisconsin subject pool contained a preponderance of high propensity voters. How do the apparent treatment effects in Wisconsin change when we restrict our attention to high, medium, or low propensity voters? The distribution of vote propensities in the Wisconsin subject pool can be approximated by estimating a logistic regression of voting in the recall election on indicators for turnout in each of the preceding 34 elections.[10] The coefficients from this regression were used to impute predicted voting probabilities for the entire subject pool.[11] The distribution of these predicted probabilities is depicted in Fig. 1, which reveals that 42% of the subject pool have more than a 90% chance of voting, and another 11.5% have less than a 10% chance of voting.

The effects of the Neighbors mailers become substantially larger when we focus attention on those with low to middling vote propensities. Table 4 presents the estimated treatment effects subdivided by vote propensity. As expected, the treatment has negligible effects on those whose vote propensities are above 80%. By contrast, those with vote propensities between 20% and 40% increase their turnout probability by an average of 3.3 percentage points (SE = 0.8) in response to the Neighbors mailers. Those with vote propensities between 40% and 60% show an even larger increase, 3.6 percentage points (SE = 1.0). An F-test decisively rejects the null hypothesis of equal treatment effects across propensity categories ($p < 0.001$), indicating that vote propensity significantly interacted with the treatment.[12]

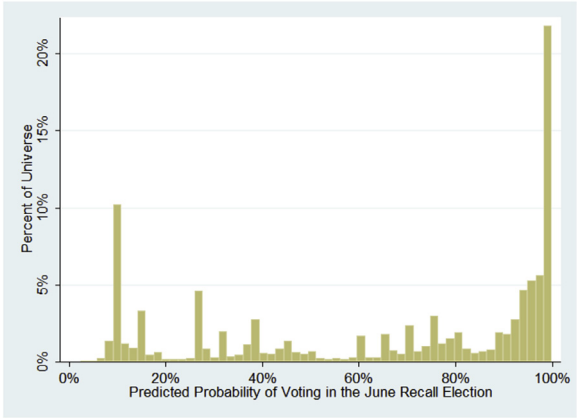


Fig. 1. Distribution of Baseline Propensities to Vote in the June Election. Notes: Estimation of vote propensities was based on a logistic regression of the turnout in the control group on turnout in 34 previous elections from 2000 to 2012. Coefficients from this regression were applied to the entire subject pool in order to construct the histogram. The histogram looks essentially the same if we were to plot only the control group.

Interestingly, even if the targeting of the mail had focused solely on those with vote propensities between 40% and 60%, the resulting 3.6 percentage point increase in turnout would have been smaller than the average effects of the Neighbors mailing in 2006. Indeed, the effect would have been smaller than the average effect of the Self mailer in 2006, despite the fact that the most noteworthy change in wording between 2006 and 2012 was the use of language taken from the 2006 Self mailer.

4. Enduring effects

The downstream effects of the mailers are presented in Table 5. The first column presents a simple regression of turnout in November on assignment to any one of the mailers; this regression is equivalent to a difference-in-means estimator of the intent-to-treat (ITT) effect. The second column presents the same regression, this time controlling for turnout in 34 previous elections. Both estimates are similar in magnitude (0.44 and 0.41 percentage points), but the latter specification more precisely estimates the ITT and generates a one-tailed p-value of less than 0.02. The third column presents an instrumental variables regression in which turnout in November is predicted by turnout in June, which in turn is instrumented by treatment assignment, following the procedure described in Gerber et al. (2003). The fourth specification presents the same model, with controls for past turnout. The two instrumental variables estimates imply that turnout in June raises turnout by an average of 42.2 or 38.6 percentage points among "compliers," those who would vote in June if and only if they receive a mailer (both are $p < 0.02$ using a one-tailed test). This strong effect is similar in magnitude to the estimates reported in other studies that gauge voting habits using experimental interventions (Gerber et al., 2003; García Bedolla and Michelson, 2012; Cutts et al., 2009). The final column presents a two-stage least squares regression model using as instruments four indicators for assignment to each type of mailer; this model is over-

[10] See Appendix C for further details. The regression model was estimated based on control group subjects only so that the experimental treatments would play no role in the analysis, but the estimated coefficients were used to calculate voting propensities for all subjects.

[11] Abadie et al. (2013) point out that this procedure can produce bias in small samples and recommend a repeated split sample (RSS) estimator. Applying the RSS estimator to our data produces results that fall within a half percentage point of our point estimates. These estimates are presented in the Appendix.

[12] For other work on treatment effect heterogeneity, especially in low-salience elections, see Arceneaux and Nickerson (2009) and Matland and Murray (2012). Like them, our results suggest the strongest treatment effects among those in the middle of the vote propensity spectrum. We investigated differential treatment effects across predicted Barrett support but found no evidence of an interaction.

Table 4
Turnout rates in the June recall election, by experimental condition and vote propensity.

	Control	Pooled Treatments	Treatment Effect	chi-square test
0%–20% vote propensity				
June Recall Election Turnout	7.08%	7.99%	0.91%	p = .011
N	5992	111,737		
20%–40% vote propensity				
June Recall Election Turnout	34.83%	38.11%	3.28%	p < .001
N	4203	79,373		
40%–60% vote propensity				
June Recall Election Turnout	55.09%	58.72%	3.63%	p = .001
N	2320	45,296		
60%–80% vote propensity				
June Recall Election Turnout	75.99%	76.40%	0.41%	p = .519
N	4664	88,520		
80%–100% vote propensity				
June Recall Election Turnout	94.68%	94.73%	0.05%	p = .790
N	15,430	291,215		

Table 5
Downstream effects of the June mailers on turnout in November.

	Intent-to-Treat Effects of Mailer On November Turnout		Complier Average Causal Effects of June Turnout on November Turnout		
Estimate	0.44	0.41	42.2	38.6	39.8
Standard Error	0.25	0.19	18.3	15.7	15.5
One-tailed *p*-value	0.041	0.015	0.011	0.007	0.005
Estimator	OLS	OLS	IV	IV	2SLS
Covariates	No	Yes	No	Yes	Yes
Instruments	None	None	Received Any Mailer	Received Any Mailer	Indicators for each Mailer Type
N	648,750	648,750	648,750	648,750	648,750

Notes: Intent-to-treat effects refer to the effect of assignment to any mailer condition in June on turnout in November. Complier average causal effect refers to the effect of June turnout on November turnout for the subset of the subject pool who would vote in June if and only if they received a June mailer. Effects are expressed in terms of percentage points. Hansen's J estimates were calculated using the IVREG2 procedure in Stata 12. Covariates are indicators for voter turnout in 34 elections from 2000 to 2012. OLS = ordinary least squares regression; IV = instrumental variable regression; 2SLS = two-stage least squares regression.

identified, allowing for a goodness of fit test. The Hansen's J test fails to reject the null hypothesis (p = 0.68), implying that each of the four mailings sets in motion similar voting habits.

One challenge to the interpretation that turnout is habit forming is the hypothesis that campaigns subsequently target those who vote (e.g., Frey and Rogers, 2014; Rogers and Frey, 2015). If true, some or all of the habit effect is attributable to the extra attention that June voters received in the run up to the fall presidential election – a backdoor path that is depicted in Fig. 2. To test this hypothesis, we obtained a special tabulation from two sources. The first is Obama For America, the political organization behind Barack Obama's 2008 and 2012 presidential campaigns. We provided them with the voter identification numbers for the subject pool, and they returned to us the rates at which they received contact by mail, phone, or door-to-door canvassing. These rates were disaggregated by month, allowing us to differentiate between early and late contacts. A second source is Catalist, which maintains data on contacts by several nonpartisan organizations allied with the Obama campaign (see Ansolabehere and Hersh, 2010; Nickerson and Rogers, 2014). Table 6 presents the rates of contact for the treatment and control groups.[13] The table shows negligible differences in contact rates for phone calls and doors knocked, but some differences in rate of mailers received. Members of the treatment group received a knock at the door at a rate of 13.5%, a phone call at a rate of 19.4%, and a mailer at a rate of 19.0%. Members of the control group received a knock at the door at a rate of 13.9%, a phone call at a rate of 18.8%, and a mailer at a rate of 10.9%. Since mailings rarely have an appreciable turnout effect in presidential elections, the downstream turnout effects presented in Table 5 cannot plausibly be attributed to the treatment and control groups' differential exposure to mobilization activity. A meta-analysis of 110 GOTV mail experiments found that direct mail's weighted average treatment effect is only 0.162 percentage points (Green et al., 2013). This figure implies that an 8.1 percentage point increase in mail contacts—or about 50,000 additional mail pieces—would generate only 81 extra votes. The apparent ITT of 0.41 percentage point implies that 2526 extra votes were produced by the June mailers, so even a mail effect many times larger than the meta-analysis average would fail to account for the apparent downstream effect.

[13] These figures were matched to the entire subject pool and therefore include the small proportion of individuals living in multi-voter households.

5. Conclusion

Scholars and campaign professionals have long suspected that the effectiveness of voter mobilization efforts diminishes as the salience of the election rises. Rarely, however, have the same tactics been evaluated in both high and low salience contexts. The Neighbors mailer provides an especially useful yardstick for this type of comparison; unlike most campaign mail, which has negligible effects on turnout even in low salience races, the Neighbors mailer is a powerful inducement to vote. When deployed in a Michigan primary in which less than one-third of the control group voted, the average treatment effect was 8.1 percentage points. When deployed in a high-salience recall election in which approximately two-thirds of the control group voted, the average treatment effect falls to 1.0 percentage point. Because both estimates come from very large experiments, the difference between 8.1 and 1.0 cannot be attributed to sampling error; the two estimates are roughly 15 standard errors away from one another. To some extent, the diminished effect of the Neighbors mailing in the Wisconsin recall election reflects the particular way in which voters were targeted, but even a targeting strategy that had focused solely on those whose vote propensities fall in the 40%–60% range would have boosted the effect of the mailings to 3.6 percentage-points – strong and statistically significant but much smaller than the effects generated in a low-salience election.

On the surface, the effectiveness of the Neighbors mailing even in a high-salience context makes it a tantalizing option for campaigns. Using a standard targeting strategy that focused on likely supporters, the campaign still generated one vote for every 95 mailers, which at 55 cents apiece implies that mobilizing voters cost $52 per additional vote cast. If one were to expand the calculation to include the significant number of “downstream” votes generated in the subsequent presidential election, the cost-per-vote declines to $37, which makes this type of mailer seem attractive vis-à-vis other voter mobilization tactics (Green and Gerber, 2008) and may explain why many political campaigns use the Neighbors mailer. The downside of the Neighbors mailing is the negative reaction it elicits from recipients, which in turn generates negative press coverage. A more complete cost-benefit analysis includes the staff time lost to handling press inquiries and angry emails. Although the Neighbors mailers elicited email from a small fraction of recipients, the sheer volume of resulting communication overwhelmed the staff of the sending organization.

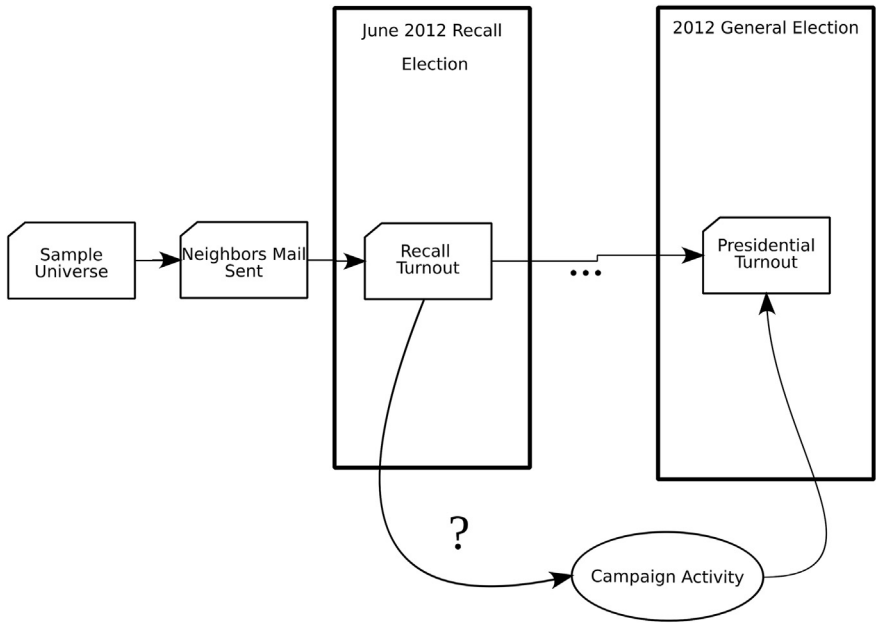


Fig. 2. Path diagram depicting a possible backdoor path from voting in June to voting in November via increased exposure to fall campaign Activity.

Table 6
Effects of the June mailers on campaign contact during the fall election season.

		Any Contact		Mail Sent		Phones Called		Doors Knocked		Other/Unknown Contact	
		Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Catalist data	% contacted	35.55%	42.72%	10.85%	19.03%	18.79%	19.38%	13.86%	13.48%	N/A	N/A
	Total N	32,609	616,141	32,609	616,141	32,609	616,141	32,609	616,141	N/A	N/A
	p-value	$p < 0.0001$		$p < 0.0001$		$p = 0.009$		$p = 0.050$		N/A	
OFA data	% contacted	33.18%	33.28%	N/A	N/A	16.55%	16.50%	9.94%	10.12%	8.19%	8.26%
	Total N	32,609	616,141	N/A	N/A	32,609	616,141	32,609	616,141	32,609	616,141
	p-value	$p = 0.709$		N/A		$p = 0.813$		$p = 0.293$		$p = 0.654$	

Notes: p-values are for two-tailed Z-tests for difference-in-proportions.
Catalist data includes all contacts of participating organizations from June 11, 2012 through Election Day. Obama For America(OFA) provided the frequency distribution of each type of contact by month and each type of contact by condition, respectively. We take the percentage of contacts of each type from July through November and use that as a multiplier for the totals of each type of contact in each treatment condition.

The rate of email complaints varied somewhat by the content of the mailers, but the various versions of the Neighbors treatment had similar effects on turnout. No significant differences emerge between advocacy and nonpartisan messages. No significant differences emerge between messages that do or do not encourage recipients to mobilize their neighbors. One interpretation of this pattern of null results is that recipients responded first and foremost to the overarching message: your turnout is a matter of public record that is now readily available to others living nearby. Given the forceful way in which this message is presented, other aspects of the Neighbors mailing that explained why the neighbors' turnout is relevant or even helpful may have receded in prominence. Ironically, the unusually strong effects of the Neighbors mailer are often used to refute the hypothesis that nuances of messaging tend to make little difference (for examples of such null findings, see Dale and Strauss, 2009 or Trivedi, 2005), yet messaging nuances seem to have little bearing on the effectiveness of the basic Neighbors mailer. Like Panagopoulos (2009), we find no special resonance of partisan appeals; like Ha and Karlan (2009), we find no increase in effectiveness when GOTV appeals urge people to mobilize others.

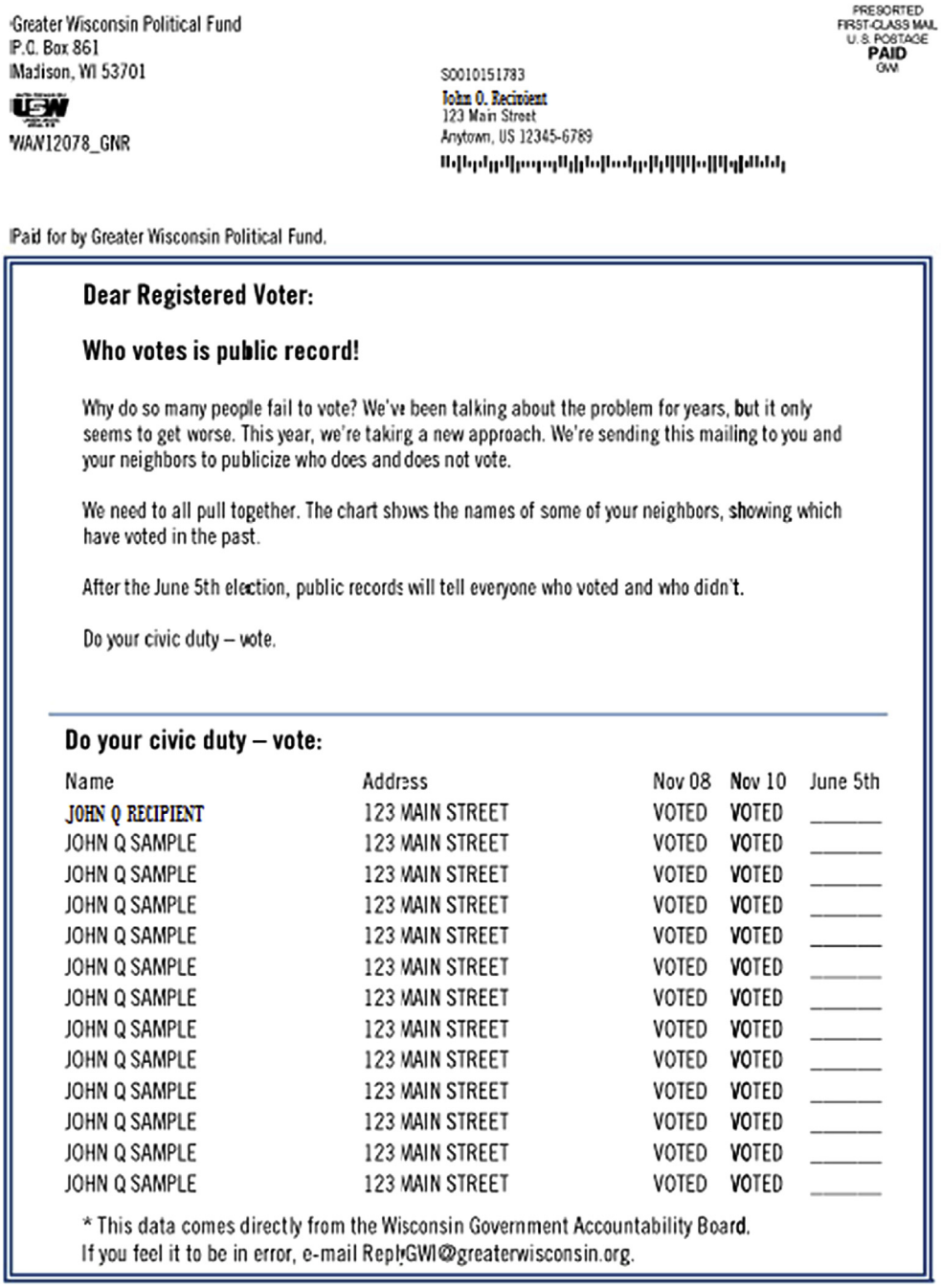
To our knowledge, this experiment is the first to examine the extent to which "rip currents" contribute to the downstream persistence of treatment effects (Frey and Rogers, 2014). We fail to find any evidence for the prediction that inducing people to vote in a June election changes whether they are targeted by a subsequent, analytically sophisticated campaign in October and November. This rules out one possibility for why treatment effects found in get-out-the-vote experiments tend to be persistent. There are a host of other possible mechanisms. These include the possibility that inducing targets to vote in the June 2012 election changed their self-perception after the election as they came to identify more strongly with being "voters." This changed identity might have resulted in increased likelihood of voting in November (Bryan et al., 2011; GarcíaBedolla and Michelson, 2012; Gerber et al., 2016). Another possible mechanism might be that the Neighbors treatment made voters aware that their vote histories are public record, creating the prospect of accountability for whether they vote or not in each subsequent election. Future research should examine these and other possible causes of downstream persistence in turnout.

Although the present experiment sheds light on the immediate and enduring effects of social pressure tactics in a high-salience election, future work should strive for even more exacting comparisons in order to isolate the moderating effects of electoral context. One limitation of this study is that in order to facilitate comparability across the four types of mailers, it did not use the precise wording of the original Neighbors mailer. Future work focusing exclusively on replication of the original study should include the exact wording as one of the treatment conditions to assess whether minor wording adjustments are consequential. Another limitation of this study is that it did not hold the subject

pool constant – by necessity, we compared a set of Michigan voters to a set of Wisconsin voters, each selected by different campaign consultants using different targeting criteria. A more systematic approach would define a target population ex ante and randomly allocate it into three groups: an untreated control, a group to be treated in a low-salience election, and a group to be treated in a high-salience election.[14] Although this approach seems intuitive, to our knowledge, no field experiments to date have used this design to isolate the extent to which treatment effects vary by context. In principle, this design could be applied to existing experiments by randomly sampling a new treatment group from the prior control group.

Appendix A. Examples of the Neighbors Mailers Used in Wisconsin (2012) and Michigan (2006)

Mailing 1: Nonpartisan Message without Encouragement to Mobilize Others.



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P.O. Box 861
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WAN12078_GNR

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Dear Registered Voter:

Who votes is public record!

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.

We need to all pull together. The chart shows the names of some of your neighbors, showing which have voted in the past.

After the June 5th election, public records will tell everyone who voted and who didn't.

Do your civic duty – vote.

Do your civic duty – vote:

Name	Address	Nov 08	Nov 10	June 5th
JOHN Q RECIPIENT	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____

* This data comes directly from the Wisconsin Government Accountability Board.
If you feel it to be in error, e-mail ReplyGWI@greaterwisconsin.org.

[14] An even stronger design would allocate subjects to treatment in a series of elections that alternate in salience, so that the timing and salience of the election are approximately orthogonal.

Mailing 2: Partisan Message without Encouragement to Mobilize Others.

Greater Wisconsin Political Fund
P.O. Box 861
Madison, WI 53701

WAW12078_BNR

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S0010151783
John Q. Recipient
123 Main Street
Anytown, US 12345-6789

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Dear Registered Voter:

Scott Walker won in 2010 because too many people stayed home!

Two years ago, more than half a million Wisconsinites who supported Obama failed to vote in the 2010 election. And that's how Governor Scott Walker got elected. 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.

We need to all pull together. The chart shows the names of some of your neighbors, showing which have voted in the past.

After the June 5th election, public records will tell everyone who voted and who didn't.

Do your civic duty – vote.

Do your civic duty – vote:

Name	Address	Nov 08	Nov 10	June 5th
JOHN Q RECIPIENT	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
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JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____

* This data comes directly from the Wisconsin Government Accountability Board.
If you feel it to be in error, e-mail ReplyGRW@greaterwisconsin.org.

Mailing 3: Nonpartisan Message and Encouragement to Mobilize Others.

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P.O. Box 861
Madison, WI 53701

WAW12078_GR

S0010151783
John Q. Recipient
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Anytown, US 12345-6789

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U.S. POSTAGE
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Paid for by Greater Wisconsin Political Fund.

Dear Registered Voter:

Who votes is public record!

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. Look at the list below: are there neighbors on this list you know? Call them or knock on their door before Election Day, and ask them to go vote on Tuesday, June 5th.

After the June 5th election, public records will tell everyone who voted and who didn't.

Do your civic duty – vote and remind your neighbors to vote.

Do your civic duty – vote:

Name	Address	Nov 08	Nov 10	June 5th
JOHN Q RECIPIENT	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____

* This data comes directly from the Wisconsin Government Accountability Board.
If you feel it to be in error, e-mail ReplyWI@greaterwisconsin.org.

Mailing 4: Partisan Message and Encouragement to Mobilize Others.

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Madison, WI 53701

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S0010151783
John Q. Recipient
123 Main Street
Anytown, US 12345-6789

PRESORTED
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Dear Registered Voter:

Scott Walker won in 2010 because too many people stayed home!

Two years ago, more than half a million Wisconsinites who supported Obama failed to vote in the 2010 election. And that's how Governor Scott Walker got elected. 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. Look at the list below: are there neighbors on this list you know? Call them or knock on their door before Election Day, and ask them to go vote on Tuesday, June 5th.

After the June 5th election, public records will tell everyone who voted and who didn't.

Do your civic duty – vote and remind your neighbors to vote.

Do your civic duty – vote:

Name	Address	Nov 08	Nov 10	June 5th
JOHN Q. RECIPIENT	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____
JOHN Q SAMPLE	123 MAIN STREET	VOTED	VOTED	_____

* This data comes directly from the Wisconsin Government Accountability Board.
If you feel it to be in error, e-mail ReplyGR@greaterwisconsin.org

Mailing 5: Original Neighbors Mailer from Gerber et al. (2008).

30423-3

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Dear Registered Voter:

WHAT IF YOUR NEIGHBORS KNEW WHETHER YOU VOTED?

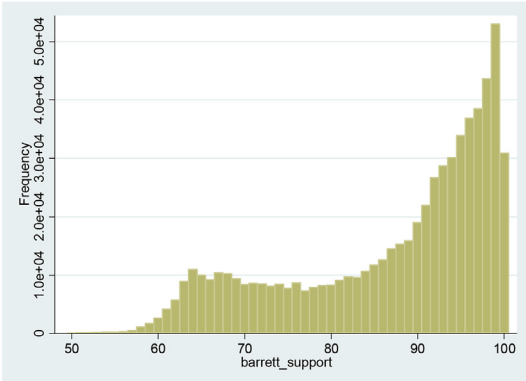
Why do so many people fail to vote? We've been talking about the problem for years, but it only seems to get worse. This year, we're taking a new approach. We're sending this mailing to you and your neighbors to publicize who does and does not vote.

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	_____

Appendix B. Distribution of Modeled Barrett Support in the Entire Experimental Universe



Appendix C. Technical Notes

Statistical Power

The power calculations were made based on the following Stata commands: sampsi 0.5 0.507, n1(32609) n2(616141) onesided and sampsi 0.5 0.505, n1(154035) n2(154035).

Vote Propensity

Restricting the sample to just those subjects in the control group, we used vote history data from 34 previous elections (i.e., every election for which there was information in the voter file) to predict the probability of voting in the Recall Election. Logistic regression was used to regress voter turnout on dummy variables marking turnout in each past election. Coefficients from this regression were then used to predict turnout for each subject in the treatment groups.

Admittedly, some voters may have been ineligible to vote in all 34 elections (e.g., they were too young to vote, were not residents, etc.). For these earlier elections, we are unable to distinguish between a lack of a vote from an individual who was eligible to vote at the time and a lack of a vote from an individual who, at the time, was *not* eligible to vote. While we can impute ineligibility in earlier elections based on age, we are unable to predict other eligibility criteria (e.g., residency, citizenship, etc.). Thus, even if there are indications that for an earlier election a given individual was not eligible to vote, we still code their lack of vote as a zero—just as we do with individuals who, at least age-wise, were eligible to vote at the time.

As a robustness check, we used the endogenous stratification procedure recommended by Abadie et al. (2013) to identify possible overfitting bias. Namely, we used repeated split sample estimation, which works as follows. First, the control group is split into a training set and a test set. The training set is used to generate the model, which is then applied to the control group test set and the whole treatment group. These predicted values are used to determine vote propensity subgroups. The treatment effect is the difference between the *actual* values in the treatment group and the *actual* values in the control group *test set only* for each predicted subgroup. This was repeated 100 times to generate 100 treatment effects, which were then averaged to produce the final treatment effect in each subgroup.

We used the **estrat** Stata package, which supports only equal-sized quintiles. To facilitate a direct comparison of the two approaches, we use the conventional method and the RSS method over the same equal-sized quintiles. We find trivial differences between the two approaches, so we conclude that, in this case, there is no evidence that the conventional approach produces overfitting bias. See Table A1 for the results.

Table A1
Comparing Endogenous Stratification Benchmarks with Conventional Estimates.

Quintiles	Conventional Method	Repeated Split Sample
1	0.0133203**	0.01156**
2	0.0366959**	0.036379**
3	0.0049309	0.010222
4	−0.0012511	0.000092
5	0.0012712	0.000173

Entries are estimated treatment effects within each vote propensity quintile. The conventional estimates differ somewhat from those presented in Table 4 because here the groups are defined by quintiles rather than vote propensity ranges (e.g., 20%–40%). ** $p < 0.01$.

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