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Text Messages as Mobilization Tools:

The Conditional Effect of Habitual Voting and Election Salience

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ABSTRACT

In their 2009 article published in the *American Journal of Political Science,* Dale and Strauss (DS) introduce the Noticeable Reminder Theory (NRT) of voter mobilization, which posits that mobilization efforts that are highly noticeable and salient to potential voters, even if impersonal, can be successful. In an innovative experimental design, DS show that text messages substantially boost turnout by levels comparable to personalized mobilization strategies, challenging previous field experimental research which argues that social connectedness is the key to increasing participation. This paper replicates DS's research design and extends it in two key ways. First, whereas the treatment in DS's experiment is a "warm" text message that was combined with some form of contact, we test NRT more cleanly by examining the effect of "cold" text messages that are completely devoid of auxiliary interaction. Second, because we have data on subjects' recent voting histories, we can test an implication of NRT that habitual voters should exhibit the largest treatment effects in lower-salience elections, whereas casual voters should exhibit the largest treatment effects in higher-salience elections. Via these two extensions, we find support for NRT.

In their 2009 article published in the *American Journal of Political Science*, Dale and Strauss (DS) introduce the Noticeable Reminder Theory (NRT) of voter mobilization, which posits that mobilization efforts that are highly noticeable and salient to potential voters, even if impersonal, can be successful. This is in contrast to Social Occasion Theory (SOT), which suggests that voting is a social occasion, and therefore explains why personal mobilization strategies such as in-person contact (Gerber and Green 2000) and volunteer telephone calls (Nickerson 2006) tend to be effective, while impersonal strategies such as direct mail (Gerber and Green 2000) and electronic mail (Nickerson 2007) are not. The main crux of DS's logic is that the weighing of costs and benefits is generally undertaken by citizens at the time of deciding whether to *register* to vote in an election, and that conditional on being registered, a voter simply needs to be reminded to vote in a salient manner (not personally convinced). Conversely, SOT contends that social contact is necessary to boost the perceived benefits of voting and consequently the decision to participate.

The bulk of the field experimental literature on voter mobilization has forwarded the importance of social connectedness (Green and Gerber 2008), which is challenged by DS. In an innovative experimental design, DS use text messages to distinguish between NRT and SOT. Like in-person contact and telephone calls, text messages are noticeable and salient. However, like direct and electronic mail, they are impersonal. Hence, if text messages significantly and substantially boost turnout at a level similar to that of personalized mobilization strategies, then it is the *noticeability* of the message (and not the *personalization* of the message) that promotes turnout. Conversely, if the effect of text messages is similar to the effect of direct and electronic mail, then SOT is supported.

DS find that the intent-to-treat effect of text messages on turnout is 3 percentage points,

similar to the average effect of volunteer phone calls and much higher than impersonal modes of communication such as direct mail, electronic mail, commercial phone calls, and robotic phone calls. Thus, DS find strong evidence for NRT. In other words, the reason why e-mails and pieces of direct mail do little to mobilize voters is not because they are impersonal, but rather because they are not noticeable.

DS also crucially distinguish between mobilization and reminding. Whereas the existing field experimental literature on turnout presumes that various modes of contact engage voters in the political process by increasing the perceived benefits of voting, DS argue that the mechanism is more about scheduling; campaign contact reminds people who are already generally inclined to vote that they should be doing so in the near future.[1]

This note highlights two potential limitations of DS's research design, which we address via our replication and extension. First, whereas the treatment in DS's experiment is a "warm" text message that was combined with some form of contact prior to the delivery of the text, we test NRT more cleanly by examining the effect of "cold" text messages that are completely devoid of auxiliary interaction. Second, because we have data on subjects' recent voting histories, we can test an implication of NRT that habitual voters should exhibit the largest treatment effects in lower- salience elections, whereas casual voters should exhibit the largest treatment effects in higher-salience elections.

Using "Cold" Text Messages to Eliminate Auxiliary Interaction

DS's treatment was not solely a text message sent to registered voters. Instead, participants were recruited through one of two mechanisms. Some citizens were registered in person by Student Public Interest Research Groups (PIRGs). During this registration process, cell

[1] Consistent with this argument, recent research shows that explicitly inducing people to develop a plan to vote increases the effectiveness of GOTV contact (Nickerson and Rogers 2010).

phone numbers were captured. Other citizens opted in to the experiment by registering to vote via a Working Assets website and specifically provided their cell phone number and permission for the company to contact them via text message in the future. The registration website was advertised on Google and was sent out by nonprofit organizations to their membership lists. In other words, part of the sample consisted of people who had previously been in contact with an organization and requested that they be text messaged in the future.

DS's experimental treatment is therefore what we refer to as *warm texts*, or text messages combined with some sort of auxiliary contact. In the case of the PIRGs, an individual registered the voter in person. In the case of Working Assets, people did not receive text messages without warning, but rather asked an organization with which they had a personal connection (i.e. their nonprofit via Working Assets) to remind them to vote. In the case of individuals who joined the experiment via a Google advertisement, even these people opted in by responding to the initial advertisement and giving explicit permission to be contacted via text message in the future. An additional concern is that the text messages reminded people not only about the upcoming election, but also that they made an implicit commitment to vote when they registered with the organization. Consequently, DS cannot disentangle the effect of a noticeable reminder from social commitment effects.

Hence, DS's experiment may be unable to distinguish between the two theories of political participation described above. Proponents of SOT could respond by saying that the reason the warm texts were successful is not because of their noticeability, but because they reminded people of the prior contact (sometimes personal) at the time of registration, as well as the commitment they made to vote. In our extension, we address this criticism by eliminating all auxiliary contact from the text message treatment.

DS respond to this concern by pointing out that an interaction term between closeness of the registration date to the election (i.e. closeness to the prior contact) and the treatment was insignificant. However, this test has two main limitations. First, it assumes that the timing of auxiliary contact significantly influences its efficacy. Second, it assumes that the relationship between proximity of registration date and treatment efficacy is linear.

A much simpler and straightforward test of NRT is to use *cold texts* as the treatment. We define cold texts as text messages that have absolutely no prior or personal contact associated with them. In other words, people do not receive texts from an organization that registered them in person, and do not give permission to receive text messages prior to Election Day. Cold texts more closely approximate the impersonality of electronic and direct mail, but fulfill DS's criterion of being noticeable.

Using Voting Histories to Test for Heterogeneous Effects

One important implication of NRT is that the effect of noticeable reminders jointly depends on: (1) the salience of the election; and (2) an individual's voting history. DS's argument is similar to that of Arceneaux and Nickerson's (2009) theory of "contingent mobilization"—a noticeable reminder will only affect an individual's decision to vote if they are near their indifference threshold. In lower-salience elections, habitual voters (those who vote in almost every election) are near their indifference thresholds, whereas casual voters (those who only vote in major, higher-salience elections or those with spotty voting records) are uninterested in the contests and therefore well below the threshold. Conversely, in higher-salience elections, casual voters are near their indifference thresholds while habitual voters are more fully engaged in the contests and therefore far above the threshold and not susceptible to reminders. We describe each of these theoretical predictions in more detail below.

First, text messages should have a minimal impact for casual voters in lower-salience elections. These voters weigh costs and benefits at the time of registering for a higher-salience election, and therefore a more powerful blandishment to vote is needed to convince them that lower-salience elections are important. As DS note: "The first, and perhaps most prevalent, example of misprediction [of NRT] occurs when voters register for presidential elections and then fail to vote in succeeding non-presidential elections…The Social Occasion theory of mobilization is applicable in these cases" (789). Conversely, text messages should be highly effective for casual voters in higher-salience elections since they are more likely to be marginal with respect to their turnout decision given the broad level of interest in the election. As Arceneaux and Nickerson (2009) explain: "Because campaign coverage is intense, even [casual voters] have some interest in the election outcome, making them more receptive to entreats to vote than they are in less salient elections" (3).

NRT also implies that noticeable reminders should still increase voting among habitual voters even in lower-salience elections. These voters presumably perceive such high benefits from voting that registration implies a propensity to vote, and therefore a noticeable reminder in lower-salience elections should be effective. As DS write: "This scenario [lower-salience election] does not preclude a noticeable reminder from boosting turnout among a different set of individuals who intend, but forget, to vote in less-visible elections than the one for which they initially registered" (789). On the other hand, habitual voters should be unaffected by text messages in higher-salience elections, given than they have greatly exceeded their indifference threshold in these electoral contexts and "are aware and plan to vote" (Arceneaux and Nickerson 2009, p. 3).

Two aspects of DS's study precluded testing these hypotheses. First, most of the subjects

in DS's experimental design were new registrants, meaning that voting histories were unavailable. Consequently, it was not possible to identify habitual voters who participated in even the most low-salience elections. Second, DS studied the 2006 general election, which had a baseline turnout rate of over 50%. In addition to studying a higher-salience election, we replicate DS's study in the context of a low-turnout, off-cycle local election,[2] where registration *per se* may imply very different cost-benefit calculations between habitual and casual voters for the election at hand.[3]

Via these two extensions, we find support for NRT. Consistent with DS, we find that cold texts do indeed significantly increase turnout by approximately 0.7 to 0.9 percentage points, an effect size comparable to that found using warm texts. Second, we uncover an important source of heterogeneity in the treatment effect that is consistent with NRT and contingent mobilization theory. In a lower-salience election context, cold text messages increased turnout among habitual voters by a substantial amount—about 16 percentage points. Conversely, turnout gains among all other voter types were minimal. In a higher-salience election context, cold texts significantly increased turnout among casual voters but did not significantly affect participation among high-propensity voters. Below, we describe the experimental design, results, and implications for our understanding of political participation.

Experimental Design

Study One: November 2009 Local Elections

The first field experiment was conducted in San Mateo County, California, during the

[2] We use the terms "lower" salience and "higher" salience to indicate the saliency of the two elections *relative* to one another. We do not make any claims as to the salience of either of the elections we study in any absolute sense. Accordingly, it would be instructive to replicate our findings in an extremely high salience context such as a general election.

[3] Of course, there are numerous differences between higher- and lower-salience elections that we cannot control for, such as the composition of the electorate. Accordingly, the second test of NRT should be viewed as more descriptive. Nonetheless, that the precise empirical patterns that we observe below—which is consistent with the specific predictions of NRT—would be driven by omitted variables seems implausible.

November 2009 local elections. This relatively sleepy, off-cycle election featured taxes and other ballot measures, as well as campaigns for city councils, local school boards, and other municipal or special district positions. There were 20 ballot measures in San Mateo County, California, on November 3, 2009, several of which were on similar topics including transient occupancy (hotel) and other tax increases, and turning various local appointed positions into elected ones.[4] Overall, there were 277,759 registered voters in the county who lived in a city in which an election was being held, and 77,340 cast a ballot (27.8% of registered). To provide some context, Figure 1 plots turnout rates for San Mateo County and three neighboring counties for seven recent elections. In the city of San Mateo, a relatively close election for a seat on the city council was decided by just 188 votes, with top vote-getter David Lim spending over $23,000, twice as much as any other candidate. Other races in the county were less close and some were uncontested; visibility of an ongoing election campaign was low, with little advertising or other electioneering activities. Countywide, most voters (65.6%) cast their voters via absentee ballot before Election Day.

[FIGURE 1 ABOUT HERE]

We began the experiment with a list of registered voters for whom a telephone number was provided by the voter at the time of registration. The list was prepared for randomization on October 7, 2009, approximately one month before Election Day (November 3, 2009). Of the 339,070 registered voters in the county, we culled names of individuals who had provided a valid

[4] Six cities voted on proposals to increase the transient occupancy (hotel) tax (Measures F, G, H, J, M, & O); each passed with over 64% support despite needing only majority approval. Five localities voted on proposals regarding other taxes, two involving the sales tax: a one-quarter cent increase in San Mateo (Measure L) passed with 61% support, but a one-half cent increase in San Carlos (Measure U) failed to pass with 43% support. Three cities passed measures to turn an elected position into an appointed position, with support ranging from 51% to 62%. Two of these were the measures with the closest vote margins. Measure K, regarding the City of Millbrae's treasurer, passed by just 67 votes (Yes: 1,693 vs. No: 1,626) with a 32% turnout rate (3,465 ballots / 10,815 registered voters). Measure I, regarding the City Clerk for Burlingame, passed 54% (2,570 votes) to 46% (2,214 votes) with a turnout rate of 34% (5,188 / 15,295). In both cases, the margin of victory was smaller than the number of voters who skipped that particular item (146 and 404).

seven-digit telephone number and lived in a local jurisdiction holding contested local elections. We eliminated individuals who did not provide a unique telephone number (i.e. if the number provided was listed for more than one individual). This left 128,465 voters. The firm Mobile Commons then determined which phone numbers belonged to cellular phones. This left a pool of 14,060 valid cell phone numbers,[5] which we randomly divided into two groups of 7,030 each: (1) a control group that received no text message prior to Election Day; and (2) a treatment group that received a message.

Following DS, text messages to the treatment group were sent on the Monday before Election Day (November 2, 2009). The text of the message was: "A friendly reminder that TOMORROW is Election Day. Democracy depends on citizens like you-so please vote!"[6] This is the exact same message used by DS that produced their largest treatment effect (intent-to-treat: 3.3%; treatment-on-treated: 4.5%). DS also found that including the number for the "National Voter Assistance Hotline" reduced the treatment effect, so we did not include it in our experimental condition.

The messages were sent at 1:00 P.M. PST by the firm MessageMedia. The firm also recorded which messages were not successfully delivered. The contact rate in the treatment group was 99.8%; only 13 of the 7,030 messages bounced back as undelivered. This means that, for all practical purposes, the intent-to-treat effect is the same as the treatment-on-treated effect. Accordingly, we only present intent-to-treat effects below. Moreover, because San Mateo County records the method of voting for each voter, we know if voters participated in early

[5] Comparing our pool of 14,060 voters with those in the county that did not provide a valid cell phone number, we find that individuals included in the experiment were younger, more likely to be born in the U.S. than abroad, less likely to affiliate with one of the two major political parties, and less likely to have voted in any of the previous four statewide elections.

[6] We randomly assigned half of the treatment group to receive a personalized message (i.e. the message began "Dear [NAME OF VOTER]"). We found no difference in turnout rates between these two groups, and therefore pool them for the remainder of the analyses.

voting and excluded those who did from the analysis (n = 1,217), therefore reducing measurement error. Because DS did not have this information, they had to estimate the number of in-person voters using a post-treatment survey. Hence, a methodological advantage of our study is that we are able to more accurately measure the contact rate.

The resulting sample size is 12,843 (control: n=6,409; treatment: n=6,434). Of the 7,017 delivered texts, 172 individuals sent replies: 41 were negative in nature (e.g. "Please do not ever text again."), 106 were neutral (e.g. "Hi, who is this?"), and 25 were positive (e.g. "Got it, thanks"). As shown in Appendix A, randomization was successful. Differences between control and treatment across a host of demographic and political variables (age, party registration, voting history, nativity) were both substantively small and statistically insignificant.

Study Two: June 2010 Statewide Elections

The second field experiment was also conducted in San Mateo County, California, this time during the June 2010 statewide primary elections. It included contested primary elections for the Republican nominee for U.S. Senate, the Republican and Democratic nominees for California Governor, and seven other statewide offices. It also included five statewide ballot measures. There were 16,977,031 total registered voters eligible to vote in the election, and 5,654,813 cast a ballot (33% of registered). Ballot Measure 17—which dealt with auto insurance regulation—was the closest race on the ballot. It lost by 1.9 percentage points (48.1% to 51.9%), a margin of 202,940 votes. Although the final outcomes were not particularly close, there was considerable interest in the gubernatorial contests. On the Democratic side, California Attorney General Jerry Brown, well known from his days as former governor and former mayor of Oakland, easily defeated a field that offered only token opposition. The Republican contest, in contrast, saw ugly exchanges between the top two candidates, and set a record for the most

expensive campaign in California history, evidenced by the flooding of airwaves with competing television advertisements. In the end, former EBay Chief Executive Meg Whitman defeated state Insurance Commissioner Steve Poizner by double digits.[7]

We began the experiment with a list of registered voters for whom a telephone number was provided by the voter at the time of registration. The list was prepared for randomization on May 17, 2010, approximately three weeks before Election Day (June 8, 2010). Of the 338,378 registered voters in the county, we culled names of individuals who had provided a valid seven-digit telephone number (128,471). We eliminated individuals who did not provide a unique telephone number or who had sent an unsubscribe request in response to the November 2009 text message. The firm Mobile Commons then determined which phone numbers belonged to cellular phones. This left a pool of 34,281 valid cell phone numbers which we randomly divided into two groups: (1) a control group (n = 17,150) which received no text message prior to Election Day; and (2) a treatment group (n = 17,131) which received a message.

Paralleling the November 2009 experiment, text messages to the treatment groups were sent on the Monday before Election Day (June 7, 2010). The text of the message was the same as in the lower-salience election study, and we also randomized whether the message was personalized. The messages were sent at 1:00 P.M. PST by the firm MessageMedia. The firm also recorded which messages were not successfully delivered. The contact rate in the treatment group was 99.99%; only 65 of the 17,131 messages bounced back as undelivered. Moreover, because San Mateo County records the method of voting for each voter, we know if voters participated in early voting and excluded those who did from the analysis (n=4,608), therefore

[7] Whitman spent more than $71 million of her own money on the campaign and more than $88 million overall. Poizner's campaign was also mostly self-financed, spending more than $25 million. The record-level spending continued in the gubernatorial general election for November 2010, with Whitman self-funding her campaign with a record $140 million as of this writing.

reducing measurement error.

The resulting sample size is 29,673 (control: n=14,829; treatment: n=14,844). Of the 17,066 delivered texts, 452 individuals (less than 3%) sent replies: 123 were unsubscribe requests, 41 were negative in nature, 185 were neutral, and 103 were positive. As shown in Appendix A, randomization was successful.

Results and Implications

Main Effect of Cold Texts

November 2009. We first describe the main effects of the cold text messages for the experiment conducted in the lower-salience election. After Election Day, we obtained an updated voter file from the San Mateo County Registrar of Voters, which provided validated turnout information for individuals in our treatment and control groups. As shown in the top row of the top panel of Table 1, we observed a statistically significant treatment effect of the cold text messages. In the control group, 253 of 6,156 individuals voted (3.95%). In the treatment group, 300 of 6,434 individuals voted (4.66%). This generates an intent-to-treat effect of 0.72 percentage points (s.e. = 0.36 percentage points), a statistically significant difference (p = 0.02, one-tailed). We also conducted regression analysis via a linear probability model.[8] We predict voting with a treatment dummy and a host of demographic controls available in the voter file (previous voting history in four elections, age, age squared, party identification).[9] As shown in

[8] We obtained similar results estimating a logistic regression model. The coefficient associated with the cold text treatment is statistically different from zero at p=.01. We present estimates from the linear probability model for ease of interpretation and because logistic regression may not be appropriate for analyzing randomized experiments (Freedman 2008) and makes unneeded functional form assumptions (Angrist and Pischke 2009). Moreover, interpreting interaction terms in limited dependent variable models introduces unneeded complexities (Ai and Norton 2003).

[9] A valid measure of age was not available for 56 registered voters in the November 2009 experiment and 56 voters in the June 2010 experiment. We include age squared because the effect of age in this election was non-linear. Previous research suggests that party identification explains variance in the dependent variable as Democrats are less likely to vote than Republicans (e.g. Radcliff 1994; Citrin et al. 2003) and unaffiliated voters are less likely to vote than partisans (Gerber and Green 2000).

the first column of Table 2, we obtain a similar intent-to-treat effect in terms of both size and statistical significance (0.79 percentage points, s.e. = 0.34, $p = 0.01$).

[TABLE 1 ABOUT HERE]

[TABLE 2 ABOUT HERE]

How big is this effect in substantive terms? As a percentage of the baseline turnout rate in the control group, cold texts increased turnout by 18.2%. In DS's experiment, they found that text messages increased turnout by 7.3% over their baseline rate. Running a simple probit regression predicting turnout with the treatment dummy, we find the following coefficient estimates: a = -1.76 (s.e. = 0.029) and b = 0.078 (s.e. = 0.039), producing a linear effect equivalent to the approximately 0.7 percentage point effect noted above. However, assuming a baseline turnout rate equivalent to DS's 56.4% (a = 0.16), we find that the linear effect of the treatment is 3 percentage points, identical to that observed by DS.

June 2010. We found an effect of similar size in the higher-salience election. As shown in the top row of the bottom panel of Table 1, 8.9% (1,319/14,829) of individuals in the control group voted compared to 9.8% (1,448/14,844) in the treatment group, producing an intent-to-treat effect of 0.86 percentage points (s.e. = 0.34, $p = 0.005$). As we might expect, turnout rates were over twice as large in the June 2010 election compared to the November 2009 election. As shown in the third column of Table 2, the regression estimate is nearly the same (0.88 percentage points, s.e. = 0.32, $p = 0.003$). This represents a 9.7% increase over the baseline turnout rate, similar to the effect size observed by DS. Assuming a baseline turnout rate of 56.4%, we find the linear effect of the treatment is 2 percentage points, slightly smaller than that observed by DS. Thus, in two different electoral environments, we replicate DS's main finding that noticeable reminders are sufficient for increasing turnout. However, we strengthen the original results by

showing that they are robust to the elimination of auxiliary contact from the treatment messages. We next explore whether different types of voters are contributing to the treatment effect in different electoral contexts.

Heterogeneity by Voting History

November 2009. Voting history was available in the voter file for the four previous elections: the May 2009 statewide special election, the November 2008 general election, the June 2008 primary election, and the February 2008 presidential primary. The November and February[10] elections had extremely high statewide turnout (79.4% and 57.7% of registered voters, respectively) while the May and June elections had lower turnout levels (28.4% and 28.2%, respectively). We divided the sample into five natural subgroups:[11] (1) habitual voters who voted in each of the four elections (3.0% of the sample); (2) voters who only voted in the November general presidential election (40.6%); (3) voters who only voted in the two major, higher-turnout elections (21.0%); (4) voters who did not vote in any of the previous elections (19.3%); and (5) all other voters (16.0%).[12]

As shown in the top panel of Table 1, we find an extremely large treatment effect among habitual voters who voted in all of the prior elections, including those which featured low turnout. Cold texts boosted turnout by 16.2 percentage points ($p < 0.001$) for these voters. For the other subgroups examined, the estimated effects are substantively small and fail to reach statistical significance. As shown in the second column of Table 2, compared to the baseline group of "never voted," the treatment effect was significantly greater for habitual voters ($p <$

[10] The February 2008 presidential primary featured highly competitive contests for both major political parties.
[11] As with our notion of "higher" and "lower" election salience, habitualness of voting is a relative construct. However, for simplicity of interpretation, we separate voters into distinct categories and refer to them as "habitual voters," "major election voters," and so forth.
[12] The voter file provided by San Mateo County only included the voting history from the past four elections. On the one hand, having data from earlier elections would better identify the various groups of voters. On the other hand, given mobility of residents in the county, we would have substantial missing data for individuals who recently moved into the county.

0.001). This is the case no matter which subgroup is selected as the baseline. Moreover, the treatment effects among the other four subgroups are not significantly different from one another. Noticeable reminders in this low-salience election were effective among voters who had a high latent propensity to turnout—those individuals who seek to vote in every election, no matter how minor. However, they had little effect on more casual voters that may be more persuaded by social contact. These results are consistent with those of Arceneaux and Nickerson (2009), who find that face-to-face efforts in low-salience elections have larger effects among high-propensity voters than among low-propensity voters.

June 2010. We added turnout in November 2009 to our analysis, using five previous elections to define the voting history of individuals in the higher salience experiment. We again divided the sample into the five groups mentioned above. As shown in the bottom panel of Table 1, the effect of cold texts in the higher salience election was only positive and statistically significant among casual voters—those who only voted in the presidential election or major elections with high overall turnout. Among presidential election voters, the treatment effect was 1.0 percentage point (s.e. = 0.4, $p = 0.005$). Among major election voters, the treatment effect was 2.3 percentage points (s.e. = 0.8, $p = 0.002$). Conversely, we did not observe a statistically significant treatment effect among habitual voters ($p = 0.81$) and the effect size was negatively signed (although the estimate cannot be distinguished from zero). Further, based on the coefficient estimates in the fourth column of Table 2, the treatment effect among habitual voters is significantly smaller than the treatment effects for both presidential election voters (5.4 percentage points, $p = 0.03$) and major election voters (6.8 percentage points, $p = 0.01$). Hence, consistent with both NRT and contingent mobilization theory, casual voters are most affected by reminders in higher-salience electoral environments, presumably because these voters are

marginal with respect to their turnout decisions. Habitual voters, on the other hand, do not drive the treatment effect in this case because they have far exceeded their indifference threshold for voting in higher-salience election contexts.

Implications

In this research note we have sought to refine DS's original experimental design via two extensions, testing the robustness of NRT. First, after isolating the noticeability component by cleaning out auxiliary contact, we find that cold text messages represent an effective mobilization strategy, replicating DS's original experiment both in terms of statistical significance and effect size. Second, leveraging data on subjects' voting histories, we find that non-personal, salient reminders appear to be extremely effective among habitual voters in lower-salience election contests, but ineffective among casual voters. Consistent with an implication of NRT, registration in a previous election implies a net benefit to voting among habitual voters (therefore making a noticeable reminder sufficient), but not so among casual voters (perhaps necessitating social contact). The opposite effect is found in higher-salience environments, where habitual voters have far exceeded their indifference threshold while casual voters are near theirs.

There are several potential extensions to this line of research. NRT and SOT are, of course, not mutually exclusive theories and it would be instructive to explore the conditions under which each mechanism operates. One limitation of the current analysis is that we did not replicate DS's "warm texts" treatment. One could conceive of a four-way design in which both warm and cold texts are provided to isolate their individual effects and assess whether personal contact and impersonal reminding individually affect turnout above and beyond each in isolation. Additionally, a future research design could vary the content of the message. Although both DS and this analysis found little effect of the text of the message itself, a very limited number of

messages have thus far been tested. SMS/MMS technology provides numerous opportunities to provide more complex treatments (e.g. graphics and multimedia). For instance, one could imagine showing voters their polling place with a Google map, thereby further reducing the transaction costs involved in voting. Via further experimentation, scholars can gain a better understanding of the mechanisms underlying political participation.

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Table 1: The Effect of Cold Text Messages on Turnout

November 2009 Election

Sample	Control (No Texts)	Treatment (Cold Texts)	Intent-to-Treat Effect
All Voters (100.0%)	3.95% (253/6,409)	4.66% (300/6,434)	.72% (s.e. = .36)
Habitual Voters (3.0%)	25.9% (52/201)	42.0% (79/188)	16.2% (s.e. = 4.7)
Pres. Election Voters (40.6%)	1.4% (36/2,573)	1.4% (36/2,643)	.0% (s.e. = .3)
Major Election Voters (21.0%)	3.6% (49/1,371)	4.1% (55/1,328)	-.6% (s.e. = .7)
Never Voted (19.3%)	.9% (11/1,232)	1.8% (22/1,251)	.9% (s.e. = .5)
"Other" Voters (16.0%)	10.2% (105/1,032)	10.5% (108/1,024)	.4% (s.e. = 1.3)

June 2010 Election

Sample	Control (No Texts)	Treatment (Cold Texts)	Intent-to-Treat Effect
All Voters (100.0%)	8.89% (1,319/14,829)	9.75% (1,448/14,844)	.86% (s.e. = .34)
Habitual Voters (1.1%)	60.1% (104/173)	87.0% (91/164)	-4.6% (s.e. = 5.4)
Pres. Election Voters (36.8%)	3.4% (184/5,440)	4.3% (238/5,492)	1.0% (s.e. = .4)
Major Election Voters (19.0%)	8.7% (246/2,830)	11.0% (310/2,816)	2.3% (s.e. = .8)
Never Voted (23.3%)	3.9% (136/3,467)	4.0% (137/3,441)	.1% (s.e. = .5)
"Other" Voters (19.7%)	22.2% (649/2,919)	22.9% (672/2,931)	.7% (s.e. = 1.1)

Table 2: The Conditional Effect of Habitual Voting and Election Salience

	Nov. 2009 Election		June 2010 Election	
Treatment (Cold Text Message)	.0079*	.0075	.0088**	.0005
	(.0034)	(.0078)	(.0032)	(.0066)
Habitual Voter	.31***	.23***	.51***	.53***
	(.011)	(.015)	(.015)	(.022)
Presidential Election Voter	-.0015	.0024	-.0006	-.0106*
	(.0048)	(.0067)	(.0043)	(.0060)
Major Election Voter	.02***	.02**	.05***	.04***
	(.0055)	(.0077)	(.0051)	(.0071)
"Other" Voter	.08***	.08***	.17***	.17***
	(.006)	(.008)	(.005)	(.007)
Treatment x Habitual Voter	——	.16***	——	-.05
		(.02)		(.03)
Treatment x Presidential Election Voter	——	-.0079	——	.0086
		(.0095)		(.0085)
Treatment x Major Election Voter	——	-.0027	——	.023*
		(.011)		(.010)
Treatment x "Other" Voter	——	-.004	——	.007
		(.012)		(.010)
Age	.0027***	.0028***	.0021**	.0020**
	(.00068)	(.00067)	(.00068)	(.00068)
Age Squared	-.000022**	-.000022**	-.000004	-.000004
	(.0000077)	(.0000077)	(.000008)	(.000008)
Republican	.017**	.017**	.035***	.035***
	(.005)	(.005)	(.005)	(.005)
Decline to State/Other	.004	.004	-.016***	-.016***
	(.004)	(.004)	(.004)	(.004)
Constant	-.059***	-.060***	-.023*	-.019
	(.014)	(.014)	(.013)	(.013)
N	12787	12787	29617	29617
Adjusted R^2	.09	.10	.10	.10

Note: ***p<.001; **p<.01; *p<.05 (two-tailed). Estimates from linear probability models. Baseline category is "never voted."

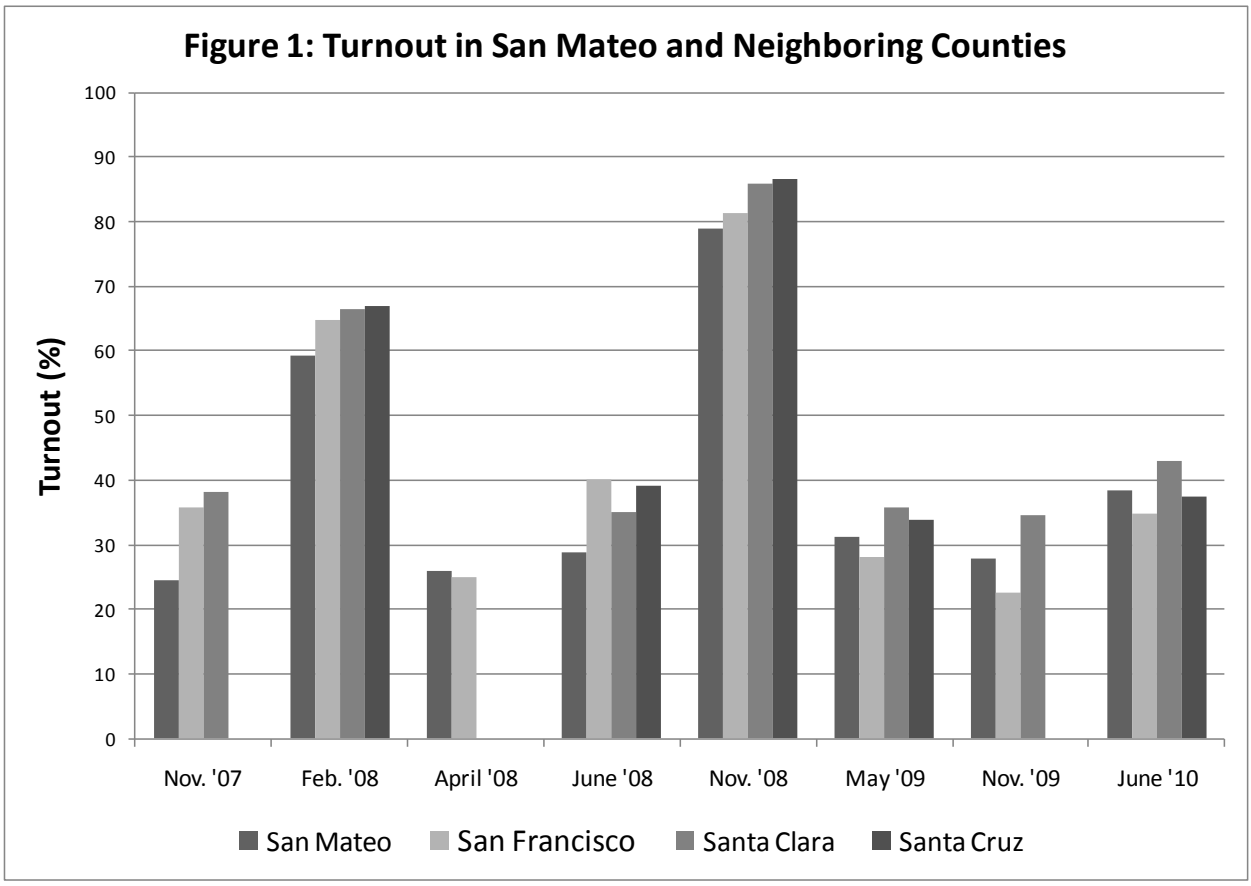
Figure 1: Turnout in San Mateo and Neighboring Counties
Turnout (%)
100
90
80
70
60
50
40
30
20
10
0
Nov. '07
Feb. '08
April '08
June '08
Nov. '08
May '09
Nov. '09
June '10
San Mateo
San Francisco
Santa Clara
Santa Cruz

Appendix A: Balance Statistics

	November 2009 Election				June 2010 Election			
	Control	Treatment	dif.	p-value	Control	Treatment	dif.	p-value
Age (years)	38.0	38.0	.0	.99	35.8	35.7	.1	.33
% Foreign Born	21.7	21.7	.0	.97	22.9	22.9	.0	.96
% Democrat	51.2	51.0	.2	.75	50.5	50.5	.0	.94
% Republican	15.0	15.7	-.7	.29	12.8	13.5	-.6	.12
% Decline to State/Other	33.8	33.4	.4	.64	36.6	36.1	.6	.31
% Habitual Voters	3.1	2.9	.2	.48	1.2	1.1	.1	.62
% Presidential Election Voters	40.1	41.1	.9	.28	36.7	37.0	-.3	.58
% Major Election Voters	21.4	20.6	.8	.30	19.1	19.0	.1	.80
% Never Voted	19.2	19.4	-.2	.75	23.4	23.2	.2	.69
% "Other" Voters	16.1	15.9	.2	.77	19.7	19.7	-.1	.90
% Voted in November '09 Election	———	———	———	———	6.3	6.3	.0	.79
% Voted in May '09 Election	12.5	12.0	.5	.40	11.6	11.5	.1	.86
% Voted in November '08 Election	77.2	77.1	.1	.86	72.6	72.7	-.1	.78
% Voted in June '08 Election	8.4	8.3	.1	.80	8.2	7.9	.4	.25
% Voted in February '08 Election	34.0	33.2	.8	.33	31.5	31.5	.0	.96

Note: November 2009 Election: For age, n= 6,381 for control and n=6,406 for treatment. For % foreign born, n = 6,287 for control and n = 6.317 for treatment. For all other variables, n = 6,409 for control and n= 6,434 for treatment. June 2010 Election: For age, n= 14,805 for control and n=14,812 for treatment. For % foreign born, n = 14,680 for control and n = 14,700 for treatment. For all other variables, n = 14,829 for control and n= 14,844 for treatment.