

# Moving the campaign from the front door to the front pocket: field experimental evidence on the effect of phrasing and timing of text messages on voter turnout

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## ABSTRACT

Despite the widespread scholarly attention given to get-out-the-vote tactics the recent one and a half decade, few have studied the effect of short text messages (SMS) on voter turnout, and no previous such study has been conducted outside the US. We analyze four SMS experiments with more than 300,000 voters conducted in relation to two elections in Denmark and find intention-to-treat (ITT) effects between 0.33 and 1.82 percentage points with a pooled effect of 0.74 percentage points. Furthermore, we vary the timing and the content of the messages to test existing theories of text messages as mobilization tools. In one experiment, we find messages delivered before Election Day to have a higher effect than those delivered on Election Day, while we find no additional effect of delivering multiple messages. We also vary message content and in general find no significant differences from sending different messages.

Widespread voter abstention remains a challenge in most established democracies with an average abstention rate of as much as 40–50% not uncommon in the 2000s (Farrell 2011, 224). As a response to the challenge of low turnout over the past 15 years, interest organizations, authorities and scholars have issued campaigns especially with the purpose of increasing voter turnout rates (Gerber and Green 2000; Green, McGrath, and Aronow 2013; Bhatti et al. 2015). Get-out-the-vote (GOTV) campaigns have relied primarily on traditional means of campaign communication including door-to-door canvassing (e.g. Gerber and Green 2000; Nickerson 2008), mailings (e.g. Gerber and Green 2000; Gerber, Green, and Larimer 2008; Sinclair, McConnell, and Green 2012) and phone calls (e.g. Gerber and Green 2000; Nickerson

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2007a). With increasing internet and cell phone coverage (Pew Research Center 2015), voters increasingly use other platforms and the political campaigns have traveled with them (Farrell 2012, 37). Researchers have started to explore the mobilizing potential of campaigns via social media and emails (Nickerson 2007b; Bond et al. 2012; Malhotra, Michelson, and Valenzuela 2012), and short text messages (SMS) delivered to cell phones (Dale and Strauss 2009; Malhotra et al. 2011). However, the literature on these new campaign forms is still scarce, and we thus know little about the usefulness of text messaging in mobilizing voters (Green, McGrath, and Aronow 2013).

To expand the cumulative knowledge of new campaign forms where campaigns move from the voter's mailbox and front door to her inbox and front pocket, we present evidence from four randomized field experiments issuing different text messages to varying sub-populations of voters. Text messages sent directly to the voters' cell phones have several advantages. From a theoretical viewpoint they are noticeable reminders in the sense that voters are used to paying attention to messages they receive to their cell phones, whereas leaflets, mailing or phone calls might be more likely to be discounted or ignored (Dale and Strauss 2009). Second, they hold potential as a low cost mean of communicating directly to the voters. Third, and related, they do not contain the same risk of wasting resources trying to contact voters as phone calls or door-to-door canvassing do.

Studies using text messages have demonstrated remarkable effects, especially considering their low costs and impersonal nature (Dale and Strauss 2009; Malhotra et al. 2011). In a comprehensive meta-analysis Green, McGrath, and Aronow (2013) point to the apparent effectiveness of text messages as an intriguing anomaly when compared to other means of communication (Green, McGrath, and Aronow 2013, 34). In the same meta-analysis the authors identify one of the frontiers of research on voter mobilization to expand and replicate the overwhelmingly American research field in different settings (Green, McGrath, and Aronow 2013, 38). The studies presented here vastly expand the available knowledge on GOTV-text messages and expands the findings regarding GOTV campaign effects more generally from American settings. The four large experiments in our study conducted in two different elections on varying sub-populations of Danish voters, to the best of our knowledge, make our study the first to investigate the mobilizing effect of text messages on voters outside the US. In addition, we contribute by increasing the precision of the estimates of the effectiveness of text messages in general, which is currently only based on two studies (Dale and Strauss 2009; Malhotra et al. 2011).

Furthermore, we investigate two factors that might condition the impact of text messages. First, we look at the importance of timing. At the one hand, some argue that a noticeable reminder such as a text message must be sent close to the election (Dale and Strauss 2009, 792). Another view is that

information acquired early in a campaign could affect how later information is processed (primacy), which could also allow voters to plan their action on Election Day well in advance (Panagopoulos 2011; Murray and Matland 2014). We pit these two perspectives against each other, planned action versus a reminder, and provide an empirical test of timing using text messages.

Second, we vary message content by manipulating message wording and content. A large body of research has focused on this, especially by varying the content of mailings (Gerber, Green, and Larimer 2008; Bhatti et al. 2014; Panagopoulos 2014, 2016). Past research have found no variation in effects from different messages (Dale and Strauss 2009). We test the robustness of this finding with two new experiments manipulating the content of the message. We also vary message content by randomly varying if voters receive a text with or without a link to a voting advice application (VAA). VAAs have won increasing usage in many European countries including Denmark. By randomly assigning a link to the VAA, we can compare recipients of the link to voters who only receive the message without the link and isolate the effect of access to the VAA.

### **Theoretical departure: text messages as noticeable reminders**

In the first study issuing text messages to mobilize voters, Dale and Strauss (2009) suggested that text messages might serve as noticeable reminders. The Noticeable Reminder theory (NRT) posits that voters do not need to be persuaded to vote as much as they need to be reminded about their innate intention to vote in due time to make plans to go to the polling station on Election Day or vote early. Dale and Strauss (2009, 790) argue in line with most literature in political psychology that voters have limited capabilities for information processing (Zaller 1992; Lupia and McCubbins 1998; Lodge and Taber 2013). Hence, during a campaign they automatically and likely subconsciously filter out bulks of information and campaign communications. For a communication to have any effect whatsoever it must therefore pass some threshold of attention. Dale and Strauss suggest that text messages are more likely to surpass this threshold, since voters in general pay attention to their phones and messages are difficult to ignore due to the nature of the display. Dale and Strauss find support for their theory with text messages leading to a three percentage point increase (intention-to-treat (ITT) effect) in turnout among registered voters.

Dale and Strauss contrasts NRT with Social Occasion theory (SOT). According to SOT, personal communication works because it establishes a social connection between the campaign and the citizen and makes the citizen feel more wanted at the polling stations (Dale and Strauss 2009, 788). As text messages are impersonal, they further argue that NRT is in contrast with

SOT. There is both a theoretical and empirical caveat with arguing that text messages *per se* are an impersonal campaign form. Theoretically, if the text message initiates a discussion between the receiver and her friends, family or colleagues, it can be argued that the presumably non-social campaign communication creates a social occasion. An example of this could be if a campaign targets young voters and send a message to several of them while they are at their campus. Empirically, an important caveat to Dale and Strauss' findings is that both of their experiments are conducted on a sample of citizens who have registered with an organization, either in person or online, and at that time accepted future communications from the organization. Thus, we might wonder whether there have already been established a social connection through personal contact at the time of registering with the organization.

In another study using text messages, Malhotra et al. (2011) underlines this empirical limitation and expand on NRT in two important ways. First, they replicate the findings, which is a valuable contribution to the cumulative science. Second, they apply "cold" messages, which are text messages issued by an organization that voters did not opt into. This is in contrast to Dale and Strauss who apply "warm" text messages. Overall, the article documents that text messages are efficient though the ITT effects on 0.8–0.9 percentage points are smaller than the three percentage points reported by Dale and Strauss. By showing that text messages even work in a situation without prior contact between the citizen and the campaign, NRT gets a stronger empirical foundation.

In the years that have passed since Dale and Strauss outlined NRT, with the success of smartphones, cell phones have only become a more integrated part of most voters' life. Furthermore, in most countries, political campaigns rarely send text messages to individuals who have not signed up for receiving such messages, so voters should not automatically discount political information they receive on their phone. Contrary, it should be something voters notice. In the experiments presented here, we therefore expect the general terms of NRT to hold and text messages to effectively mobilize voters.

However, there are important reasons to expect the effects to be smaller than what Dale and Strauss find. First of all, like Malhotra et al. (2011) we use cold text messages. Second, and importantly, previous studies target only voters who had previously demonstrated an interest in voting by registering to do so. Using an approach inspired by the classic calculus of voting (Riker and Ordeshook 1968), Dale and Strauss argue, that since registering to vote is instrumental for voting, choosing to register should signal that one's perceived gain of voting outweighs the cost, including the cost to register (Erikson 1981; Nickerson 2015). Therefore noticeable reminders should be particularly effective for this group. With the few studies implemented so far

carried out in the US without compulsory or automatic registration, it leaves open the question how effective text messages are when they target voters who are either compelled to register or registered automatically. Compulsory or automatic registration is the norm in most other Western democracies (Pintor and Gratschew 2002). It seems likely that institutional arrangements might dampen the effect if they cause the potential voters to consist of all eligible voters and not just those who have already indicated their interest in voting. With this limitation of the existing literature it is more necessary than usual to expand the established findings and replicate them under different institutional arrangements. In all, we still expect text messages to mobilize voters but to a smaller extent than Dale and Strauss find.

## **What conditions the effect of text messages?**

### ***Timing***

In their article, Dale and Strauss assume that the timing of the message is “crucial” in the NRT framework, as “the reminders must be close to the election to be relevant” (2009, 792). This goes along the line of their argument that voters need the reminder in order to plan for voting. If the reminder is sent too soon voters are likely to forget it before Election Day approaches. If it comes too late, they will not have time to change their plans. Even though they present this as a central argument for the theory, they do not vary the timing of the messages in their experiments and no study of text messages until now have tested the relevance of timing. To make up for this empirical deficit, we randomly vary when voters receive text messages to isolate the effect of timing.

Looking at the broader GOTV-literature, there are a few studies that have investigated potential timing effects. Most notably Panagopoulos (2011) puts forward a hypothesis of prevalence of recency against one of primacy. In a somewhat low-powered study, he does not find clear-cut patterns when varying the timing of a phone effort over the weeks prior to the election. His results do, however, point to encouragements administered in the week before the election as most efficient, though the differences to the effects in other time periods fall short of conventional levels of statistical significance. In the same vein, Nickerson (2007a) only finds robust effects when voters are also encouraged to vote in the week before the election. Finally, a recent paper from Murray and Matland (2014) tests the effect of a direct mailer sent either eight days or four days before the election in two US states. They find support for the recency effect in one state (Wisconsin), while the timing does not matter significantly in another state (Texas). Their results mostly points in the direction of a recency effect, but they also highlight that the results are quite fragile to contextual variation and stress the need for further experimentation.

Since most empirical and theoretical work point to encouragements late in the campaign as most efficient, we focused on delivering the treatments in the week running up to the election. Within this relatively narrow timeframe we did not have strong theoretical priors for one certain timing over the other. Thus, we vary the timing in one of the experiments by sending out messages on a daily basis in the week running up to the election as well as on Election Day. We followed up on the findings in the next election, where we sent out texts at a daily basis on the three days running up to the election as well as on Election Day. Following the prediction made from NRT, we would expect the effect to fade within a few days prior to the election. On the other hand, if the reminder facilitates planned action, we might expect it to be more efficient some days prior to the election, where voters have time to change their plans.

On top of the recency–primacy debate, we might consider the potential of repeating the message. One line of reasoning is that the loudest message will have the largest influence (Zaller 1992). Repetition is one way of being loud, as citizens’ attention gets drawn towards one’s message. Thus, delivering the message early might increase the availability and repeating it might make it easier accessible on Election Day (Higgins 1996). However, survey experimental evidence suggests that repetition in itself does not convert to strong influence (Chong and Druckman 2007). Existing evidence on the effect of repeated messages is scarce though some studies suggest that the marginal effect of a second or a third message may exist (Gerber and Green 2000, 660; Michelson, Bedolla, and McConnell 2009). To investigate further, we sent a random subsample two messages in one of the experiments.

### ***Message content***

Researchers have devoted much attention to varying content in GOTV campaigns (Gerber and Green 2000; Green, McGrath, and Aronow 2013). Dale and Strauss (2009) argue that treatment content should not matter for the NRT and investigates this idea by varying both message and whether or not they provide information about a hotline. Along these lines we vary the message content in two of our experiments and randomly provide a link to a VAA in one of them. There are several ways the inclusion of the VAA-link could increase turnout.

Gemenis and Rosema (2014) formalize three potential mechanisms for how VAAs could increase voter turnout. (1) VAAs provide easy access to information, thus reducing the cost from information-seeking. (2) The information provided through VAAs may increase the perceived benefit by helping clarify differences between alternatives and perhaps enhancing preexisting preferences. (3) VAAs may cause voters to think about politics and discuss politics in general, which could lead to an increase in the sense of civic duty.

We also test in one experiment if it is less effective to emphasize the conflict element in the election compared to highlighting the civic duty. While we expect both treatments will have a positive effect on turnout, we also note that some research indicate that civic duty plays a particular important role in the calculus of voting (Blais, Young, and Lapp 2000; Gerber, Green, and Larimer 2008). Therefore, we have a weak theoretical prior that text messages emphasizing civic duty will prove more efficient.

In a second experiment, we vary the tone of a civic duty message. Previous research shows that applying social pressure multiplies the effect of a more traditional civic duty message (Gerber, Green, and Larimer 2008). Along those lines we tried to frame the civic duty message in either a relatively traditional, positive and prosocial tone (see e.g. Gerber and Rogers 2009 for a similar approach) or a somewhat informal and negative tone. We expect the informal message to apply a form of negative social pressure that is more efficient in mobilizing voters than the traditional message.

## Data and context

We fielded four experiments in Denmark, with three experiments conducted in relation to the municipal elections on 19 November 2013 and one experiment in relation to the European Parliament election on 25 May 2014. Voters are automatically registered to vote in both elections and receive a polling card by mail approximately 10 days before Election Day.

While national elections take up most interest in Denmark, municipal elections also draw much attention, and campaigns are highly visible on the streets and in the media. The elections are considered as important and enjoy relatively high participation with 71.9% voting in the 2013-elections. The European Parliament elections generate less interest and 56.3% turned out to vote in the 2014-election. Early voting is available in both elections, but is not used by many in Denmark. 5.3% used early voting in the 2013-elections and 5.8% voted early in 2014.

The turnout data stem from the voter lists (see Elklit et al. 2000 for a similar study), which include a code for each citizen indicating whether they voted or not. In the 2013-elections we have access to validated turnout for 4.36 million voters or 98.93% of the eligible citizens. In the 2014 European Parliament Elections we have access to 2.4 million voters. The citizens absent in the 2014 election had voted at polling stations using manual lists for registering turnout, which needed to be digitalized manually. We had resources to lift this burden in the 2013-elections, but not in the 2014-election. While it would have been desirable to have data for all voters in both elections, the absence of some voters does not cause serious concern regarding the field experimental evidence presented in this article as we just restrict the experiment to polling stations with digital lists.

The voter files were merged in anonymous form with detailed and accurate socio-demographic register data from Statistics Denmark containing a long list of individual level, pretreatment information such as sex, age, education, ethnicity and much more. The data are useful in checking the balances across experimental groups with high validity (see the supporting information, Tables A2–A5), but are not used otherwise in the analysis.

The final information needed for the experiments were phone numbers to deliver the text messages to. All the experiments use cold text messages, meaning that the receivers did not opt-in to get text messages from the sender beforehand. Consequentially, there was no organization from which we could get phone numbers and information on the receivers to use in the experiment.

We went through three steps to go from a list of names to being ready to deliver the treatments. First, we obtained a list from the public registers containing the names and addresses of the individuals we were interested in contacting. Second, we hired a well-respected market research company (Epinion A/S) to search publically available, online resources (e.g. online phone books) for cell phone numbers. The numbers that could be connected with the given individual with high certainty were collected. Across the experiments the enrichment rate was between 34% and 49%.<sup>1</sup> Third, we randomized the individuals in each of the experiments into control and treatment groups based on the list of individuals that was enriched with a phone number (cf. Table 1). While the sample for the study is not a random draw of the eligible voters, the fact that we randomize after the enrichment process ensures that the control and treatment group are alike except from whether they received a text message or not. In the supporting information, we show descriptive statistics for the groups in each study. The treatment status of each individual was merged onto the public registers together with the validated turnout data.<sup>2</sup>

## Experimental design

The four experiments share a number of common features. The text messages were sent on behalf of well-known and trustworthy organizations though it was typically the name of their election campaign that appeared in the receiver's inbox. We administered all the experiments and performed the randomizations while Epinion A/S was in charge of distributing the text messages under our supervision. We included the name of the recipient in

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<sup>1</sup>In comparison, the enrichment rates in Malhotra et al.'s (2011) two experiments are 5% (experiment 1) and 10% (experiment 2) (own calculations – number of citizens sample with valid cell phone number/ registered voters). Dale and Strauss (2009) rely on opt-ins, which, if we consider all voters in some given area as potential subjects, probably implies an even lower enrichment rate.

<sup>2</sup>The data for this study is stored on servers at *Statistics Denmark*. Due to security and privacy implications the data cannot be made available on the Internet. Researchers interested in replicating the findings are welcome to visit and work under supervision.



**Table 1.** Experiment name, phone number enrichment, and control and treatment group sizes.

Experiment name	Danish Youth Council	Everybody's election	Parliament	European Parliament
Citizens successfully enriched with phone numbers ( <i>N</i> )	47,846	92,089	54,694	112,231
Control group ( <i>N</i> /group pct. of sample)	20,973 43.83	44,174 52.03	25,045 45.79	35,181 31.35
Treatment groups ( <i>N</i> /group pct. of sample)	26,873 56.17	47,915 47.97	29,649 54.21	77,050 68.65

Note: To avoid contamination from intra-household spillovers we exclude households with more than one phone number enriched. Table 1 only includes the relevant cases.

all messages. We give an overview of the experiments in Table 2 and describe them shortly below.

The first three experiments were conducted in relation to the 2013 municipal elections. Experiment 1 was conducted with the *Danish Youth Council*, an umbrella organization for a number of Danish youth groups (e.g. political parties and scouts organizations). The experiment was designed to test the effect of timing, as 9770 individuals received one text message at a random day up to seven days ahead of Election Day and 9764 received a message on Election Day. The messages sent out before Election Day were delivered at 3 PM on the assigned day. Individuals assigned to a text message on Election Day received it at a randomly assigned hour mark between 10 AM and 7 PM. Finally, 7339 received a message before Election Day and on Election Day, which allows us to test the effect of repeated messages. In sum, 26,873 received at least one text message.

Experiment 2 was conducted with *Everybody's election*, a campaign financed by the Ministry of Integration and Social Affairs. While the campaign's aim in general was to increase turnout for non-Western immigrants of all ages, 61% of the sample were ethnic Danes. The experiment was designed to test the effect of phrasing with two different messages being tested, one focusing on social norms and one on political conflict. Furthermore, half of each of the message types included a link to a mobile friendly VAA from the Danish Broadcasting Corporation. In these messages, "Find your candidate: [dr.dk/l/Yr](http://dr.dk/l/Yr)" was added in the end. 47,915 individuals received one message the day before Election Day between 3 and 4 PM.

Experiment 3 was conducted with The Danish Parliament and targeted young voters, including first-time voters whom were not targeted in experiment 1. The experiment informs us about the general effect of receiving a text message with a link to a campaign video compared to not getting a text message. 29,649 individuals received a message the day before the election between 10 and 11 AM phrased in the same tone as the Parliament's general campaign. To maximize power and coordinate with other experiments two blocks were applied with different probabilities of assignment

**Table 2.** Overview of the four experiments.

	Election	Study population	Focus on	Phone Enrichment rate	Text message(s) sent
Experiment 1: Danish Youth Council	Municipal Elections 2013	2nd and 3rd time local elections voters. Mean age: 25.7	Timing + repetition	46.9	Before Election Day text message: "Hi [name]. Just a friendly reminder of the election on Tuesday the 19th of November. The democracy needs you, so remember to vote!" Election Day text message: "Hi [name]. Thank you for voting in the municipal election. If you haven't voted yet, you can make it until 8 PM." Social norms message: "Hi [name]. Your friends are voting at the election tomorrow. They are counting on you to do the same. Don't fail them. VOTE!" Political conflict message: "Hi [name]. The politicians disagree about what should happen in your municipality. Vote for the ones you agree with at the election tomorrow." + the following in half of all messages: "Find your candidate: dr.dk/1/Yr."
Experiment 2: Everybody's election	Municipal Elections 2013	All ages. Mean age: 46.7. Oversampling of non-Western ethnic background (38.7 pct.)	Phrasing: Social norms vs. political conflict + link to VAA	49.0	
Experiment 3: Parliament	Municipal Elections 2013	1st, 2nd and 3rd time voters. Mean age: 22.7	General effect	40.2	"[Name]! Election day is tomorrow! You're voting! How? Like this: <a href="http://www.ft.dk/stem">http://www.ft.dk/stem</a> . Press the link! Watch the movie! Vote!" Traditional message: "Dear [name]. On Sunday there is election for the European Parliament and a referendum. It's your choice. Vote for the sake of democracy. Regards Vote.dk."
Experiment 4: European Parliament	European Parliament Elections 2014	1st, 2nd and 3rd time voters. Mean age: 23.8	Timing + phrasing: Traditional vs. informal message	33.8	Informal message: "Dear [name]. Are you ready for the European Parliament Election On Sunday? You are going to vote, right? For the sake of democracy. And yourself. Regards Vote.dk."

Notes: In all experiments, the study population is restricted to those who have a public phone number. In experiments 1, 3 and 4, the sender of the message appeared as vote.dk on the receiver's display in case she had a smartphone. In experiment 2, the sender read "Everybody's election". In experiment 4, "On Sunday" was exchanged with "tomorrow" the day before Election Day and "today" on Election Day. The untranslated messages are shown in the supporting information, Table A6.

**Table 3.** Effect of receiving any text message across four experiments.

	Danish Youth Council (2013)	Everybody's election (2013)	Parliament (2013)	European Parliament (2014)	Pooled estimate
Turnout in control group	59.36	65.27	62.71	43.78	57.74
Effect of any text message	1.82* [0.94; 2.71]	0.33 [-0.29; 0.94]	0.72 [-0.10; 1.54]	0.63* [0.01; 1.26]	0.74* [0.38; 1.09]
Total N	47,846	92,089	54,694	112,231	306,860

Notes: \* $p < .05$ , one-sided test. 95% CIs in brackets. The result for the parliament is based on a weighted average of the effects in the two blocks: Block 1: ITT = 0.19, SE = 0.59,  $N = 27,603$ . Block 2: ITT = 1.26, SE = 0.59,  $N = 27,091$  (see Gerber and Green 2012, 73–74 for relevant formulas).

to the treatment group. We take this into account when analyzing the results (see Gerber and Green 2012, 73–74).

Experiment 4 was also conducted with the Danish Youth Council and targeted young voters (18–29 years). The aim of the experiment was to test the robustness of the findings regarding the general effects of text messages and the relevance of timing for the effect. Thus, the text messages were randomly distributed over four days ahead of the election. Finally, the relevance of phrasing was tested with two different messages, one with a traditional tone and one with an informal tone. More than 77,000 individuals received one of the two messages.

Before we move on to the analysis, we briefly compare the four experiments. First, the sample for experiments 1, 3 and 4 are rather similar as they target citizens under 30 years old. These three experiments are useful for comparing different content and timing, although we note that experiment 1 does not include first-time voters. The sample in experiment 2 differs substantially from the rest of the experiments as it is older and more ethnically diverse. The sample enables a broader test of the effect of text messages on turnout and helps us learn more as to how generalizable the results are. Reflecting upon the context, experiments 1, 2 and 3 are all conducted in relation to the municipal elections in 2013, while experiment 4 was conducted in relation to the European elections 2014. As experiments 1 and 4 has a quite similar research design, the relevance of timing can be tested in two elections with different saliency (medium-high vs. medium saliency), which ought to make it somewhat harder to mobilize low propensity voters in the latter experiment (Arceneaux and Nickerson 2009).

## Analysis

### *The general effect of receiving a text message*

We start out by analyzing the general effect of receiving any text message for each of the experiments in the 2013-elections and the experiment conducted at the European Parliament election in 2014. The results are shown in Table 3.

As expected, the point estimates are positive for all of the experiments, with point estimates ranging from 0.33 to 1.82 percentage points, although only two out of four reach statistical significance at conventional levels.<sup>3</sup> The effect sizes in percentage points are roughly the same as seen in earlier studies using cold text messages (Malhotra et al. 2011).<sup>4</sup> The results from the European Parliament experiment are in line with the findings from the three municipal election experiments and thus add to the robustness of our findings.

As one might note, the point estimates differ. Such differences could for instance be explained by differences in content, context or sub-population treated. It could also be due to chance, and we note that the confidence intervals are overlapping or tangent for all of the experiments (see Figure A1 for a visual illustration). In particular experiment 1 delivers a large point estimate, while experiment 2 has the smallest point estimate. The two experiments are from the same election but with quite different sub-populations, which may be one reason for the variation (cf. Table 2). Another plausible explanation is that the second experiment had a link in the text message. Below, we show evidence indicating that the effect may be smaller when including a link. However, we once again also emphasize that the differences could be chance variation.

If we assume that the treatment effects are comparable we can pool them together using fixed effects meta-analysis (Gerber and Green 2012, 361–365).<sup>5</sup> We do this in the last column of Table 2 to get a pooled estimate of 0.74 percentage points with a standard error of 0.18 and a 95% confidence interval from 0.38 to 1.09 percentage points (cf. Figure A1). This is remarkably close to the 0.8–0.9 percentage points effect reported by Malhotra et al. (2011). Although the effect sizes are moderate, it is noteworthy that such a short and limited treatment as a text message does seem to mobilize voters across elections and that the size of the effect in percentage points is consistent with that from cold messages in the US. To get a better idea of the substantial effect size, we can compare the findings to a well-known GOTV-example in a high-salience context. In a recent paper, Gerber et al. (2016) tests the effect of social pressure mailings in high-salience elections and find an effect on 0.7 percentage points, which is comparable to the pooled effect estimate presented here.

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<sup>3</sup>In the statistical tests throughout the paper, we use a 0.05 alpha level. When we compare treatment groups to the control groups, we use one-sided tests as we have a clear one-sided hypothesis regarding the positive general effect of receiving a text message on turnout, backed by prior evidence. We use two-sided tests when comparing different treatment groups. While we do have hypothesis regarding the relative strength of some of these, it is in most cases possible to make a hypothesis pointing in the other direction. We do not take into account in the tests that we make multiple inferences.

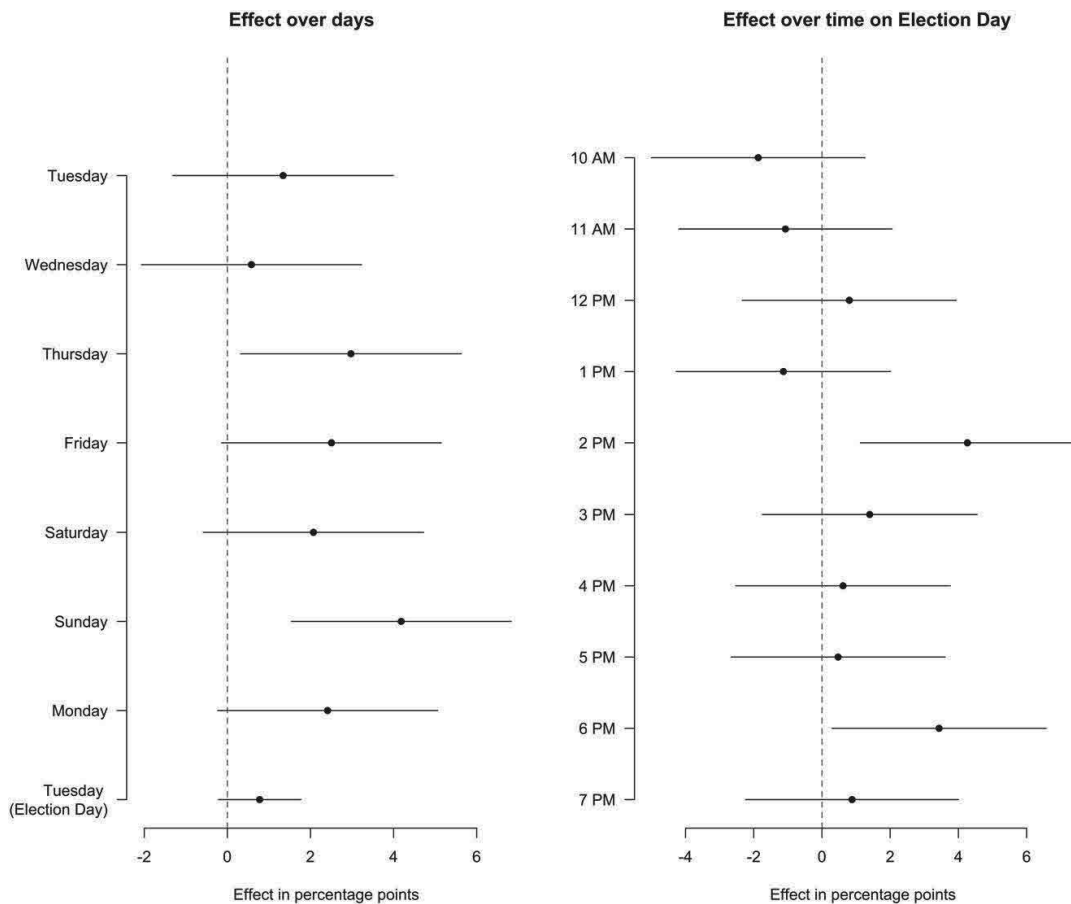
<sup>4</sup>If we convert the effects to probits where we allow the probability of voting to be a non-linear function of some underlying function, they are slightly smaller than in Malhotra et al. (2011).

<sup>5</sup>One might question the appropriateness of including experiment 2 in the meta-analysis due to its different study population, that is, an oversampling of non-Western immigrants and also with no age limit. However, since the electoral context, treatment and dependable variable still are quite similar (see also Gerber and Green 2012, 361–365), we find it acceptable to pool the experiments together.

### Timing of receiving a text message

The next step is to analyze the experiments to learn about how variation within each experiment matters for the effect of text messages. We start out by analyzing how the timing of receiving a GOTV-text message matters for the effect on turnout. Experiment 1 explored this question by sending out text messages starting seven days before the day of election and ending on Election Day.

Analyzing the group receiving text messages any day before Election Day together, we find a statistically significant increase in turnout of 2.30 percentage point (CI [1.12; 3.47]). For those receiving a text message on Election Day, the effect estimate is 0.78 and statistically insignificant (CI [-0.40; 1.95]). The immediate take away-point is that it is better to send text messages out before Election Day than on Election Day. The difference in effects is 1.52 percentage points (CI [0.15; 2.89]). This may be because voters need the reminder in sufficient time before the election to be able to make voting plans. Splitting the two groups up, Figure 1 shows the effect on turnout for each of the groups. The left panel shows the effect over days prior to the election; the right shows the effect over time of day on Election Day.

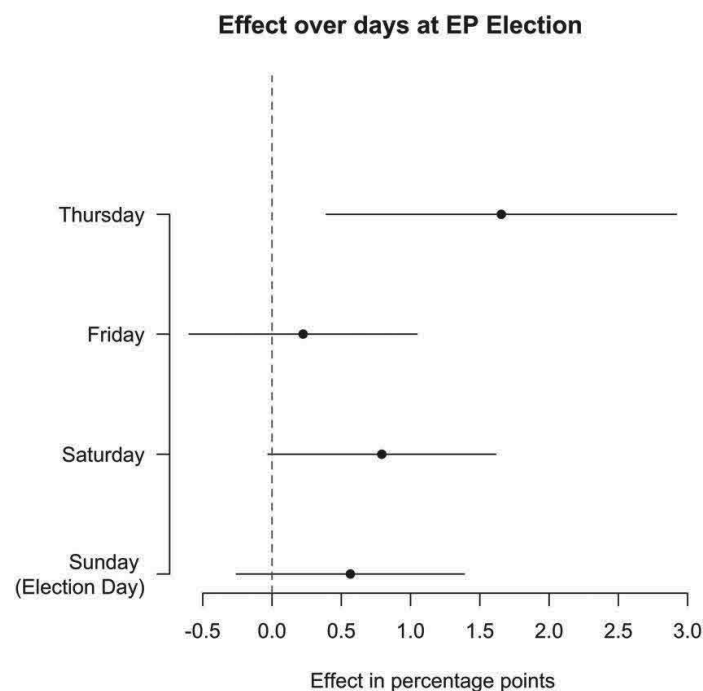


**Figure 1.** The effect of receiving a text message conditional on timing, municipal elections.

From the figure's left panel it seems that texts sent earlier are more efficient, though the estimates are uncertain, and there might be a drop six to seven days before the election. Analyzing the text messages sent out during Election Day, we find no clear pattern regarding the treatment effect (Figure 1, right). Most of the effect estimates are not statistically significantly different from 0, though the text messages sent at 2 and 6 PM had quite large effects on turnout. The variation in effect sizes mostly look like random noise around a small and statistically insignificant effect.

Finally, the experiment was designed to test if repeating the message could give an additional effect. To test this, 7339 of the text message receivers before Election Day received an extra text message on Election Day. The results cannot confirm the repeated messages hypothesis: The effect estimate of receiving an additional text message on Election Day for individuals who received another message before is 0.28 (CI [-1.20; 1.77]) and statistically insignificant. The best estimate implies a declining marginal effect of more than one treatment.

With the lessons learned from the municipal elections in 2013, we did a follow-up experiment with the Danish Youth Council at the European Elections 2014, in which the treatment was sent out starting four days before Election Day and with the last messages sent out on Election Day. As described above, two different text messages were used each day, but for the present purpose we pool these two text messages together in Figure 2. Below we return to the effects of the different messages.



**Figure 2.** The effect of receiving a text message conditional on timing, European elections.

In the second experiment there is no real evidence that the effect is stronger when the message is sent out prior to Election Day. The estimate for messages sent out before the Election Day is slightly larger at 0.66 compared to 0.57 on Election Day (two-tailed  $p < .81$ ). Why the possible Election Day effect is small and insignificant in the second election is worth speculating about. Perhaps the fact that the election was on a Sunday mattered, or maybe the difference stems from the texts being sent out earlier during the day. We note that as in the first experiment the text messages sent out on Thursday were the most effective.

Across the experiments, there is a tendency that messages delivered prior to the election are more efficient. This speaks to planned action trumping the last minute reminder. In further analyses, we fitted a straight line to the daily and hourly estimates, weighted by their precision (see Figures A2 and A3 of the supporting information). In both elections we see the same pattern of a decreasing tendency as the election approaches.

***Effect of differently phrased messages***

We now turn to the content of the messages. In Table 4, we compare the effects of a social norms message and a message emphasizing political conflict with and without including a link to a VAA.

While the point estimate is larger for the social norms message compared to the political conflict message, the difference between the effect of the messages is small and far from statistically significant, and this remains the case if we pool the two norms and the two conflict messages. The expectation was that including the VAA-link would help voters get easier access to information about the election and the text messages would thereby have a positive effect on turnout. However, the messages including links do not have a positive impact on turnout. Pooling the messages with links (ITT = -0.02, CI [-0.77; 0.73]) and without links (ITT = 0.67, CI [-0.07; 1.42]) into two groups shows that the turnout is higher for the messages without links. The difference is not statistically significant, though it comes close (two-tailed  $p < .11$ ).

**Table 4.** Effect of receiving text message in the 2013 municipal elections conditional on phrasing and inclusion of link.

	Everybody's election	N
Turnout in control group	65.27	44,174
Social norms	0.77 [-0.19; 1.73]	11,982
Conflict	0.58 [-0.38; 1.54]	11,969
Social norms with link	0.21 [-0.75; 1.17]	11,979
Conflict with link	-0.25 [-1.21; 0.71]	11,985
N		92,089

Note: 95% CIs in brackets.

**Table 5.** Effect of receiving text message ahead of EP-elections 2014 conditional on phrasing.

	European Parliament 22–29 years	<i>N</i>
Turnout in control group	43.78	35,181
Traditional tone	0.46 [–0.25; 1.18]	38,635
Informal tone	0.80* [0.08; 1.52]	38,415
Observations		112,231

Note: \* $p < .05$ , one-sided test. 95% CIs in brackets.

The final test of the relevance of phrasing is presented in Table 5. We used a traditionally toned message for one group and a more informal tone for the other group in the European elections in 2014.

The effect estimates for both messages are positive, but it is only the informally toned message that gives a statistically significant effect on turnout. The effects from the two messages are not statistically significant different from each other (two-tailed  $p < .35$ ). In conclusion, like Dale and Strauss, we do not find clear evidence of content effects although we have some suggestive findings with respect to the use of links.

## Discussion

In this article we have investigated four large field experiments using SMS as noticeable reminders. We have expanded on a scarce literature that previously only included two articles based on studies from the US. We obtain a precision weighted-average ITT of 0.74 percentage points (CI = [0.38; 1.09]). Three of the four experiments targeted voters under 30 years old. We might speculate that the effect would be larger because older people do not receive as many text messages as young people and a GOTV-text message therefore would be more notable. It may also be that the effect is smaller, for example, due to a ceiling effect since older voters are more likely to vote or have more established voting habits.<sup>6</sup> Nevertheless, our findings substantially improve the precision of the cumulative knowledge about SMS effects on voter mobilization and extend the previous results to a new context.

The similarity of the effects between the US and Denmark are somewhat remarkable for two reasons. First, as the turnout rate in Denmark is substantially higher there are simply fewer voters to mobilize. Second, Danish voters do not need to register and we treat all voters and not just those who already demonstrated some interest in voting through registering. One explanation could be that using text messages for unsolicited commercial activities are prohibited

<sup>6</sup>We have examined heterogeneity based on age in our experiments by interacting a continuous age variable with the treatment indicators. We found three negative and one positive interaction coefficient and no significant effects. However, these tests are of course limited by the age restrictions in our samples, and there could be heterogeneity when looking at broader age spans.



and extremely rare in Denmark. A text message encouraging voter participation could stand out, possibly making the reminder more effective.

A further contribution was to examine message timing and content. We found some evidence that timing matters, even though we focused on variations within the last week. More specifically, messages sent before Election Day were significantly more effective than messages sent on Election Day in one of our experiments though insignificantly so in another. This finding provides some, though not conclusive evidence that voters need to get the participation encouragement in time to plan turning out. We found no evidence of positive effects of repeated messages even though we theoretically could expect this.

Across two experiments, message content did not matter much for the treatment effects. One surprising finding was the inferior effect of including a VAA-link in the messages, a difference that was borderline statistically significant. This could be because voters are skeptical towards messages that include links, which can be seen as spam-like or possible viruses. Another explanation is that receiving the link to further information introduces a displacement effect: While the link to the VAA decreases the costs of obtaining information, it might also increase the level of information that the receiver expects to be appropriate to have in order to cast his or her vote.

Our results show that text messages can deliver moderate, positive effects on turnout, but text messages are not likely to be a panacea to the participation challenge that many countries are struggling with. Even though text messages are clearly cheaper and may seem less bureaucratic than for instance letters there are also practical obstacles. For example, low to moderate phone number enrichment rates makes it difficult for campaigners to target specific groups, especially the youngest voters who may have their phones registered in their parents' names. As campaigns may continue using and developing their text message campaigns, there is a great need for more studies in order to learn more about the pitfalls and opportunities by this communication medium.

The existing literature has shown that personal contacts produce larger effects than impersonal types of contact (Green, McGrath, and Aronow 2013). The text message findings are in that light quite remarkable considering the impersonal nature of the communication. Looking ahead, one might wonder whether adding a more personal element to text message campaigns will produce larger effects. For instance, one might wonder whether text message reminders can be a useful supplement to other types of GOTV contacts like, for example, a door-to-door effort or a personal phone bank. Another question is whether text messages sent from people within individuals' social network would produce larger effects than those sent from organizations. One could imagine that it is possible to activate a "send it forward" wave of text messages and thereby get a more personal touch to the text message campaign.

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No potential conflict of interest was reported by the authors.

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