

**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, relatively few have studied the effect of short text messages (SMS) on voter turnout, and no previous study has been conducted outside the U.S. We analyze four SMS experiments with more than 300,000 voters conducted in relation to two elections in Denmark and find 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. 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.

**Keywords:** Campaigns; SMS; mobilization; GOTV ;turnout; political participation

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Widespread voter abstention remains a challenge in most established democracies with an average abstention rate of as much as 40 to 50 percent not uncommon in the 2000s (Farrell 2011: 224). Scholars have raised concerns that prevalent abstention will lead to inequality in representation with voters having preferences that diverge from the non-voters (Lijphart 1997; Brady et al. 1995). As a response to the challenge of low turnout over the past fifteen years, interest organizations, authorities and scholars have issued campaigns especially with the purpose of increasing voter turnout rates (Gerber & Green 2000; Green et al. 2013; [reference omitted]). These Get-Out-The-Vote (GOTV) campaigns have relied primarily on traditional means of campaign communication including door-to-door canvassing (e.g. Gerber & Green 2000; Nickerson 2008), mailings (e.g. Gerber & Green 2000; Gerber et al. 2008; Sinclair et al. 2012), and phone calls (e.g. Gerber & Green 2000; Nickerson 2007a). With increasing internet and cell phone coverage, voters increasingly use other platforms and the political campaigns have travelled with them (Farrell 2012: 37). Consequently, researchers have started to explore the mobilizing potential of campaigns via social media and emails (Bond et al. 2012; Nickerson 2007b; Malhotra et al. 2012), and short text messages (SMS) delivered to cell phones (Dale & Strauss 2009; Malhotra et al. 2011). However, the literature on these new campaign forms is still scarce (Green et al. 2013).

A glance at recent figures on text message usage highlights the potential for using this technology in GOTV-campaigns. In September 2014, the number of unique cellphone users passed 50 percent of the global population (Kemp 2014), and this number is much larger in industrialized countries. For instance, 90 percent of adult Americans had a cell phone by the end of 2014 and more than 92 percent of American smartphone owners sent at least one text message a week, making it the most used feature of the cell phone (Pew Research Center 2015). While the reach and potential is large, we know little about the usefulness of text messaging in mobilizing voters.

In this article, we present evidence from four randomized field experiments issuing different text messages to varying subpopulations of voters. With this we expand on the knowledge of the potential of the new campaign forms that become available as the campaign moves from the voter's mailbox and front door to her inbox and front pocket. 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 & 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.

So far, studies using text messages have demonstrated remarkable effects (Dale & 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, which are typically much less efficient (ibid., p. 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 (ibid., p. 38). Thus, the four large experiments in our study contribute by increasing the precision of the estimates of the effectiveness of text messages in general, which is currently based on two studies (Dale & Strauss 2009; Malhotra et al. 2011). In addition, the experiments are conducted in two different elections on varying subpopulations of Danish voters, which, to the best of our knowledge, make our study the first to investigate the mobilizing effect of text messages on voters outside the US.

We also add new insights into the importance of timing. Theoretically, it has been argued that short text messages work because they are noticeable reminders to voters who by registering to vote already have demonstrated their interest in voting (Dale & Strauss 2009).

According to Dale & Strauss “*the timing of messages under the Noticeable Reminder theory (...) is crucial; the reminders must be close to the election to be relevant*” (Dale & Strauss 2009: 792). However they do not provide a test of this assumption. In a broader perspective, it is not that clear that messages delivered earlier should be less efficient than messages delivered closer to the election. Another perspective as pointed out by Panagopoulos (2011) as well as Murray & Matland (2014) 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. We pit these two perspectives against each other, planned action versus a reminder, and provide an empirical test of timing using text messages.

Finally, 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 et al. 2008; [reference omitted]; Panagopoulos 2015). Dale and Strauss (2009) did vary the text message by informing about the closeness of the election or emphasizing one's civic duty and find no clear difference between the effectiveness of the two. We test the robustness of this finding with two new experiments manipulating the content of the message. We 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. However, whether they have mobilizing effects remain elusive due to self-selected usage (Ladner & Pianzola 2010). 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.

Altogether, 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. As cell phones become more widespread and people across the world increasingly use text messaging, more knowledge about the mobilization potential of text

messaging is highly needed. We add to this pool of knowledge and hopefully inspire more research along these lines.

### **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 argue (p. 790) in line with most literature in political psychology that voters have limited capabilities for information processing (Zaller 1992; Lupia & McCubbins 1998; Lodge & 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 since text messages have yet to become a campaign tool. Compared to leaflets, election posters and other more conventional campaign communication, voters should be less inclined to automatically discount a text message as yet another impersonal campaign communication. Dale and Strauss find support for their theory with text messages leading to a three percent increase (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 & Straus 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 to them while they are at their campus. Empirically, an important caveat to Dale & 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 this 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 this way, the text message might remind people of a social connection they had earlier in relation to the election and the text message might not be impersonal in its nature.

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 effects they find are smaller than the three percent reported by Dale and Strauss (ITT effects, 0.8-0.9 percentage points in two different experiments). 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 arguably only become a more integrated part of most voters' life. Furthermore, in the country under study, Denmark, political campaigns like in the U.S. have yet to

move to text 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, Dale and Strauss 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 & 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 United States 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 & Gratschew 2002). Yet, in the published work, NRT is explicitly targeted volunteer registrants. 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 seems 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.

## *Timing*

In their article, Dale & 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*” (Dale & Strauss 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 & 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). Altogether, their results mostly points in the direction of a recency effect, but they are also highlight that the results are quite fragile to contextual variation and stress the need for further experimentation.

Since most of both the empirical and theoretical work point to encouragements late in the campaign as most efficient, we focused on the delivering the treatments late in the campaign, specifically 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. Dale & Strauss (2009) does highlight timing as being important, but does not give any precise advice regarding what to expect other than it should be close to the Election Day, but not too close. Without empirical backing of this claim, we choose to vary the timing in one of the experiments by sending out messages on a daily basis up until seven days prior to the election. We followed up on the findings in the next election, where we sent out texts up until four days prior to the election. 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.

Regardless the theoretical underpinnings, our approach offers valuable insights to campaigners and policy makers who just want to increase turnout and know how to time their effort. Text messages have desirable properties in this respect, as we have extraordinarily precise control over the timing. Many campaigns on the door-to-door canvassing or phone calls rely on voters being accessible. Letters can also be a little tricky since letters might become delayed or some voters do not check their mail every day. With text messages voters can be targeted directly on a device they are likely to use often. For instance, actual cell phone usage data show that Danish university students on average check their phone 431 seconds after receiving a text message.<sup>5</sup>

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<sup>5</sup> These figures stems from the Copenhagen Networks Study (Stopczynski et al. 2014). We thank Radu Gatej for providing the figures.

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 & 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 & Green 2000: 660; Michelson et al. 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 treatment content in GOTV campaigns (Gerber & Green 2000; Green et al. 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 Voting Advice Application (VAA) in one of them. There are several ways, the inclusion of the VAA link could increase turnout. In the classical model of voting, voters have an expected benefit from their preferred alternative,  $B$ , a perceived probability of casting the pivotal vote,  $p$ , a sense of civic duty,  $D$ , and costs associated with forming an opinion and turning out to vote,  $C$  (Riker & Ordeshook 1968). Voters' turnout only if:

$$p * B + D > C$$

Gemenis & 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.

With regard to message content we test in one experiment if it is less effective to emphasize the conflict element in the election compared to highlighting the civic duty. Emphasizing the conflict element of the election serves to emphasize the B-term in the calculus of voting while the duty element impacts the D-term. Therefore, we expect each treatment to be effective. However, some research indicate that civic duty plays a particular important role in the calculus of voting (Blais et al. 2000; Gerber et al. 2008). Therefore, we have a weak theoretical prior that text messages emphasizing the 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 et al. 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 & Rogers 2009 for a similar approach) or a somewhat fresh and negative tone. We expect the negative tone to apply a form of negative social pressure that is more efficient in mobilizing voters than the positive.

## **Data and context**

We fielded four experiments in Denmark, with three experiments conducted in relation to the municipal elections on November 19, 2013 and one experiment in relation to the European

Parliament election on May 25, 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 percent voting in the 2013-elections. The European Parliament elections generate less interest and 56.3 percent 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 percent used early voting in the 2013-elections and 5.8 percent voted early in 2014.

The turnout data stem from the actual voter lists, 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 percent 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 Appendix), 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 individuals in each of the experiments that we were interested in contacting. This list included names and addresses for the citizens. 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 percent. 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. The treatment status of each individual was merged onto the public registers together with the validated turnout data.

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

\*\*\*INSERT TABLE 1\*\*\*

## Analysis

We start out with a short description of the four experiments, which 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 a well-respected private company (Epinion A/S) was in charge of distributing the text messages under our supervision. We checked the implementation of each experiment by including our own phone numbers in the treatment groups, and all messages were delivered as expected. All text messages were sent as cold messages. We included the name of the recipient in all messages. The first three experiments were conducted in relation to the 2013 municipal elections and have the following characteristics:

*Experiment 1 - Danish Youth Council:* This is an umbrella organization for a number of Danish youth groups (e.g. political parties and scouts organizations). The organization has an aim of increasing young citizens' political participation. The text messages were sent to 2nd and 3th time voters (22-29 years old). The experiment was designed to test the effect of timing, as each person in the treatment group received one text message at a random day between seven days ahead of the Election Day and on Election Day. The text messages sent out before Election Day were delivered at 3 PM and read: "*Hi [name]. Just a friendly reminder of the election on Tuesday the 19<sup>th</sup> of November. The democracy needs you, so remember to vote!*". The text message sent out on Election Day were delivered at a random hour mark between 10 AM and 7 PM and read: "*Hi [name]. Thank you for voting in the municipal election. If you haven't voted yet, you can make it until 8 PM.*" The sender of the message shown on the recipients' phone was *stem.dk* (*vote.dk* in English). Stem.dk was the name of a large-scale campaign conducted by a range of large organization, including the Danish Youth Council and Local Government Denmark. A random part

of the treatment group receiving a message before Election Day was also assigned to get the Election Day message, which thereby makes it possible to test the effect of repeated messages. In sum, of the 26,873 individuals in the treatment group, 9,770 only received a message before Election Day, 9,764 only received a message on Election Day, while 7,339 received two messages.

*Experiment 2 - Everybody's election:* A campaign financed by the Ministry of Integration and Social Affairs with the aim of increasing turnout for non-Western immigrants<sup>6</sup> of all ages. Both ethnic Danes and non-western immigrants were included in the experiment in order to evaluate if the text messages were more effective in getting the target group to vote compared to the rest of the population. The experiment was designed to test the effect of phrasing with two different messages being tested. The first message emphasized social norms and read: "*Hi [name]. Your friends are voting at the election tomorrow. They are counting on you to do the same. Don't fail them. VOTE!*". The second message emphasized political conflict and read: "*Hi [name]. The politicians disagree about what should happen in your municipality. Vote for the ones you agree with at the election tomorrow.*" Furthermore, half of each of the messages included a link to a mobile friendly voting advice application from the Danish Broadcasting Corporation. In these messages, "*Find your candidate: dr.dk/l/Yr*" was added in the end. The 47,915 individuals in the treatment group was assigned equally to the four treatments, meaning about 12,000 individuals received each type of message. All messages were sent out the day before the election between 3-4 PM and the sender displayed on the phones was *Alles valg (Everybody's election* in English).

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<sup>6</sup> Non-Western Immigrant is an official category in the Danish public registers. It is defined as immigrants not from a list of Western countries. Western countries include all EU countries, Andorra, Iceland, Liechtenstein, Monaco, Norway, San Marino, Switzerland, the Vatican, the United States, Canada, Australia, and New Zealand.

*Experiment 3 - The Parliament:* The Danish Parliament sent text messages to young voters (18-29 years). The experiment did not test variations in content; meaning that all 29,649 individuals in the treatment group were assigned to the same treatment. However, it can be used to inform us about the general effect of receiving a text message with a link to a campaign video compared to not getting a text message. The messages were phrased in the same tone as the Parliament's general campaign and read: “[Name]! Election day is tomorrow! You're voting! How? Like this: <http://www.ft.dk/stem>. Press the link! Watch the movie! Vote!” All messages were sent out the day before the election between 10 and 11 AM, and the sender displayed on the phone was stem.dk<sup>7</sup>. To maximize power and coordinate with other experiments two blocks were applied with different probabilities of assignment to the treatment group. We take this into account when analyzing the results (see Gerber & Green 2012: 73-74).

Based on the experiences from the first three experiments, a follow-up study was conducted.

*Experiment 4 - European Parliament:* The experiment was financed by 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: A traditional and positively toned message read: “Dear [name]. Tomorrow there is election for the European Parliament and a referendum. It's your choice. Vote for the sake of democracy. Regards Vote.dk.”. A fresh and more negative tone was applied in the second message: “Dear [name]. Are you ready

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<sup>7</sup> The text messages in all three experiments are translated from Danish. The Danish version of the text messages can be found in ([AUTHOR CITATION]).

*for the European Parliament Election tomorrow? You are going to vote, right? For the sake of democracy. And yourself. Regards Vote.dk.”* More than 77,000 individuals received one of the two messages.

### *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 during at the European Parliament election in 2014. The results are shown in table 2.

**Table 2: Effect of receiving any text message in across four experiments**

\*\*\*INSERT TABLE 2\*\*\*

As expected, the point estimates are positive for all of the experiments, with point estimates ranging from 0.33 percentage points<sup>8</sup> to 1.82 percentage points, although only two out of four reach statistical significance at conventional levels. The effect sizes in percentage points are roughly the same as seen in earlier studies using cold text messages though smaller if measured in

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<sup>8</sup> The unique link in the Parliament’s video was opened 7,223 times between the delivery of the message and the closing of the polling stations. We cannot detect whether the same people opened the link multiple times and it is also possible that people forwarded the message to other people who opened the link. If we assume that it was opened once by people in the treatment group, approximately 57 percent of the receivers opened the link. 49 percent of the openings happened within the first five hours of the text message campaign.

probit space (Malhotra et al. 2011). 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. Altogether, we find that there is a positive effect on turnout from receiving a GOTV-text message compared to not getting such text message. If we assume that the treatments are comparable we can pool them together using fixed effects meta-analysis (Gerber & Green 2012: 361). We do this in figure 1 to get a pooled estimate of 0.74 percentage point with a 95 percent confidence interval from 0.38 to 1.09 percentage point. 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 U.S.

**\*\*\*INSERT FIGURE 1\*\*\***

### *Timing of receiving a text message*

The next step is to analyze the experiments to learn about how variation 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 conducted with the Danish Youth Council explored this question with 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 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 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 (two-sided p-value of difference of effect is 0.03). 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 2 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 2: The effect of receiving a text message conditional on timing, municipal elections**

**\*\*\*INSERT FIGURE 2\*\*\***

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 2, right). Most of the effect estimates are insignificantly 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, 7,339 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 the follow-up experiment, the treatment was sent out starting four days before Election Day and with the last messages sent out on Election Day, which was Sunday May 25, 2014. As described above, two different text messages were used each day, but for the present purpose we pool these two text messages together for each day in figure 3.<sup>9</sup>

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<sup>9</sup> The results split on text messages are shown in the Appendix, table 5.

### **Figure 3: The effect of receiving a text message conditional on timing, European elections**

\*\*\*INSERT FIGURE 3\*\*\*

The effect was largest for the text messages sent out on Thursday before the election, though the confidence intervals are slightly overlapping with the other days. We note that Thursday also was the most effective day in the first experiment, though the Election Day was on a Tuesday last time and on a Sunday in the European Parliament experiment. Why Thursday stands out is difficult to say and it might be a prospect to further investigation if this is a meaningful variation or just a chance variation. One potential explanation could be that the effect is larger if you receive the message when you are part of a social setting (e.g. at work or with your classmates) as is the case with the Thursday message. Being part of a social setting when receiving the message could generate some discussion and increase the social pressure to go vote.

Overall, there seems to be a tendency that messages delivered prior to the election are more efficient. This speaks to planned action triumphing the last minute reminder. To emphasize the decreasing effect as the election approaches we fit a straight line to the daily estimates in figure 4. The estimates are weighted by their precision. In both elections we see the same pattern of a decreasing tendency as the election approaches. On Election Day the trend is that messages delivered later in the day carry more weight. This is evident from figure 5. This runs somewhat counter to what we have otherwise seen, as treatments delivered earlier in the day should give voters more time to plan their day around voting. However, both figure 4 and 5 are based on relatively few data points and all conclusions should be taken with caution.

### **Figure 4: Linear fit to daily estimates (precision weighted)**

\*\*\*INSERT FIGURE 4\*\*\*

### **Figure 5: Linear fit to estimates over time on Election Day (precision weighted)**

\*\*\*INSERT FIGURE 5\*\*\*

*Effect of differently phrased messages*

Having evaluated the general effect of receiving any text message as well as the relevance of timing, we now turn to the content of the messages. First, we analyze the effect of a social norms message and a message emphasizing political conflict. Second, we include a link to further information on a voting advice application before we end with evaluating a fresh and a traditionally phrased message. Analyzing the experiment conducted with the Everybody's Election campaign will do the first two tasks and the results are shown in table 3.

**Table 3: Effect of receiving text message in the 2013 municipal elections conditional on phrasing and inclusion of link**

\*\*\*INSERT TABLE 3\*\*\*

As expected, the effect estimate for the messages emphasizing social norms of voting is larger than those with a focus on political conflict. However, the difference between the pooled effect of the two norm (ITT=0.49, CI [-0.26 ; 1.24]) and the two conflict messages (ITT=0.16, CI [-0.59 ; 0.91]) is small and far from statistically significant, so the interpretation of these results must be careful.

Within the same experiment, two of the groups received the exact same message with a link to a mobile version of a VAA from the Danish Broadcasting Corporation. The expectation was that it would help voters get easier access to information about the election and the text messages would thereby have a positive effect on turnout. As it shows in table 3, there is no evidence backing such expectation. None of the messages including links 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 (though the difference is not statistically significant,  $p=0.11$ )<sup>10</sup>. This is the opposite of expected. We will discuss some potential explanations for this in the end of the paper.

The final test of the relevance of phrasing is conducted using the experiment from the European elections in 2014. A message using a traditional and positive tone was used for one group and a fresh tone inspired by a direct mailing campaign from 2013 ([AUTHOR CITATION]) was used for the other group. The results are shown in table 4.

**Table 4: Effect of receiving text message ahead of EP-elections 2014 conditional on phrasing**  
\*\*\*INSERT TABLE 4\*\*\*

The effect estimates for both messages are positive, but it is only the fresh and negatively toned message that gives a statistically significant effect on turnout. We did expect that the fresh message would have the largest impact on the target group of young people, but we must again be careful when drawing conclusions as the effect from the two messages are not statistically significant different from each other (two-tailed,  $p=0.34$ ). In conclusion, like Dale and Strauss we do not find clear evidence of message effects although we have some suggestive findings, especially with respect to the use of links.

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<sup>10</sup> We cannot directly detect whether the receivers open the link to the VAA. Traffic data from the Danish Broadcasting Corporation shows that there is a small uptake of users from 4-5 PM, which is just after the text message was sent out. There is a dramatic increase from 19,005 to 45,113 users between 5-6 PM. This increase might be partly due to the text messages, but it could also be explained by the mentioning of the page in TV and Radio shows or due to people simply searching for information.

## Discussion

In this article we have investigated four large field experiments using SMS as noticeable reminders, thereby expanding the scarce literature, which previously only included two articles based on studies from the U.S. Over four experiments we obtain a precision weighted-average ITT of 0.74 percentage points (CI = [0.38 ; 1.09]). This finding substantially improves the precision of the collective knowledge about SMS effects on voter mobilization and extends the previous results to a new context and a new institutional setup.

The similarity of the effects between the U.S. and Denmark are somewhat remarkable for two reasons. First, as the turnout rate in Denmark is substantially higher than in the elections studied by Malhotra et al. (2011), there are simply fewer voters to mobilize. Second, Danish voters do not need to register and therefore we attempted to treat all types of individuals, including those with less desire to vote, who would likely not benefit from a noticeable reminder. One explanation could be that unsolicited commercial activities on cell phones are prohibited in Denmark, and therefore a SMS encouraging voter participation could stand out very clearly, possibly making the reminder more effective.

A further contribution was to examine message timing and content. We found evidence that timing matters, even though we focused on variations within the last week, which is deemed the most effective in the existing literature. More specifically, messages sent before Election Day were significantly more effective than messages sent on Election Day. This finding supports that voters need to get the participation encouragement in time to plan turning out. In relation to this it might also be relevant to consider the individual's social setting when they receive the message. For instance, people are more likely to be together with classmates or colleagues on

weekdays, and this social setting might lead to a different processing of the GOTV message compared to being at home and receiving a message.

We found no evidence of positive effects of repeated messages even though we theoretically could expect this. Some experimental evidence suggest that repeating a message to citizens who have committed to vote can deliver an extra effect (Michelson et al. 2009). The low costs and easy administration of text messages does make such messages an attractive tool for sending reminders. Going forward it would therefore be useful to test whether there is an effect of getting a text message for citizens who have pledged to the campaign that they will vote.

Message content caused some variations in treatment effects, though in most cases not sufficiently to be detected with sufficient statistical certainty. One surprising finding was the inferior effect of including links to VAAs in the messages compared to excluding the links. We interpret this finding as voters being skeptic 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 her vote. Thereby, it will be perceived to be more demanding to vote when receiving a link compared to not receiving a link.

The use of text messaging has expanded tremendously since the first message was sent in 1992 (Snowden 2006: 107). More than 50 percent of the global population use a cellphone and in industrialized countries this figure is much higher (Kemp 2014). Furthermore, the low costs and scalability of text messaging campaigns makes the technology highly attractive if it actually is useful for increasing turnout. 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. As campaigns are likely to 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.

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## Tables and figures

**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 (N)	47,846	92,089	54,694	112,231
Control group (N/group pct. of sample)	20,973 43.83	44,174 52.03	25,045 45.79	35,181 31.35
Treatment groups (N/group pct. of sample)	26,873 56.17	47,915 47.97	29,649 54.21	77,050 68.65

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

**Table 2: Effect of receiving any text message in across four experiments**

	Danish Youth Council (2013)	Everybody's election (2013)	Parliament (2013)	European Parliament (2014)
Turnout in control group	59.36	65.27	62.71	43.78
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]
Total N	47,846	92,089	54,694	112,231

\*  $p < 0.05$ , 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 & Green 2012: 73-74 for relevant formulas).

**Table 3: 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

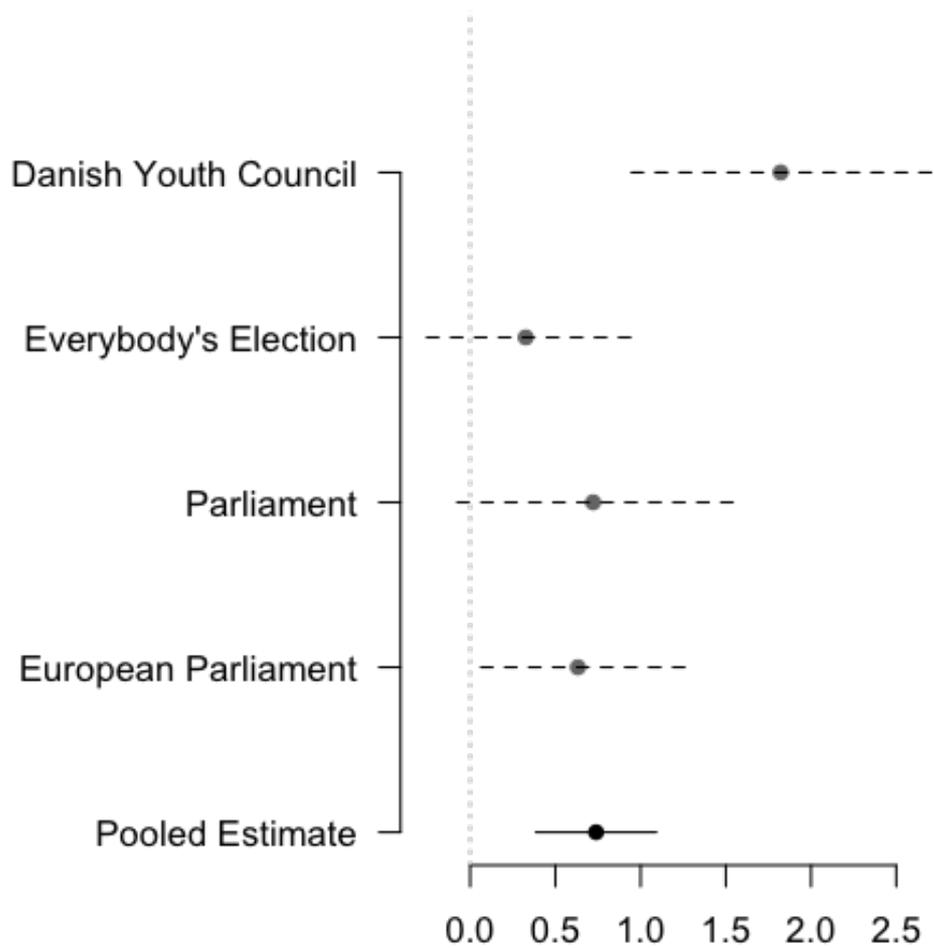
\*  $p < 0.05$ , one-sided test. 95% CIs in brackets.

**Table 4: Effect of receiving text message ahead of EP-elections 2014 conditional on phrasing**

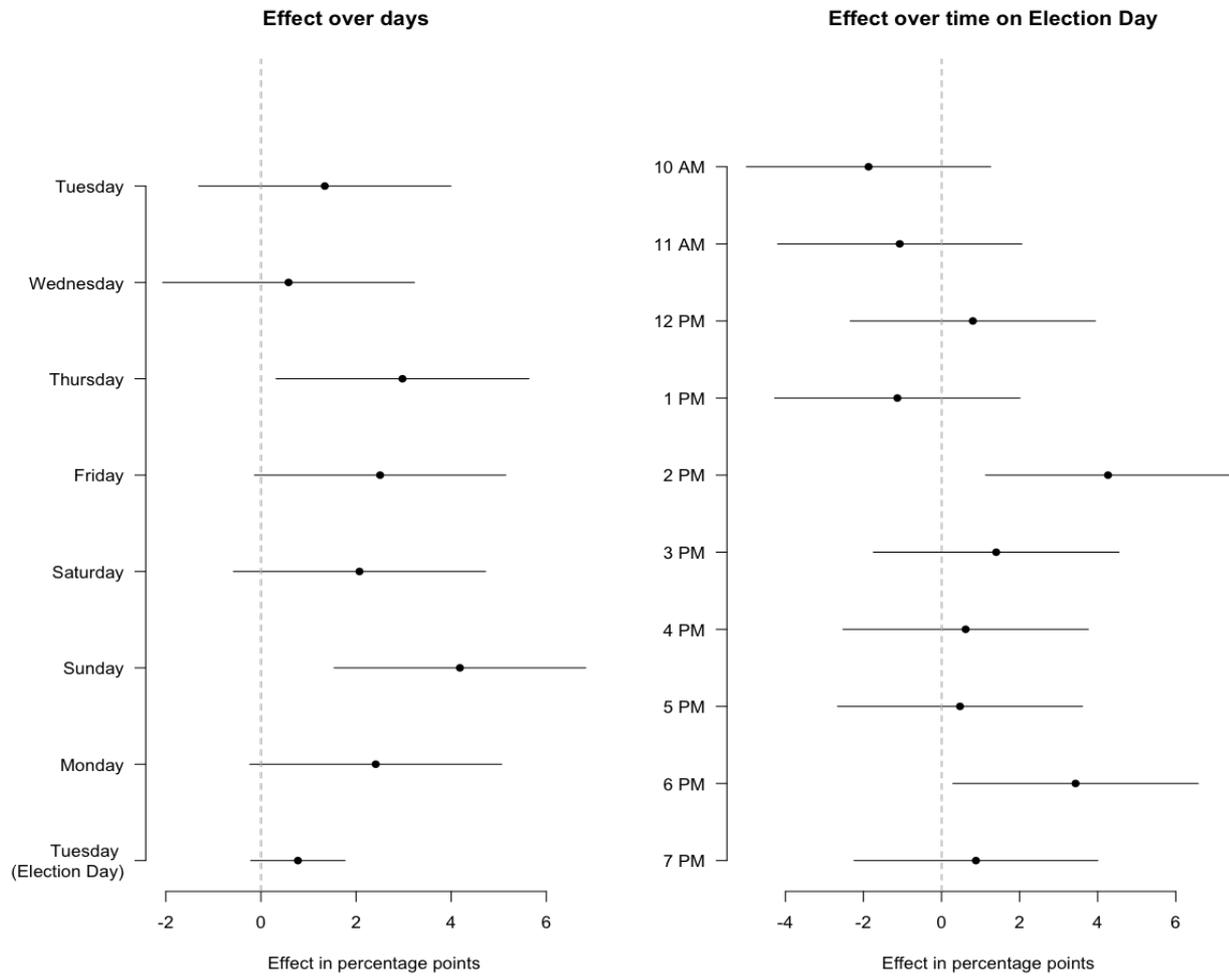
	European Parliament 22-29 years	N
Turnout in control group	43.78	35,181
Traditional tone	0.46 [-0.25 ; 1.18]	38,635
Fresh tone	0.80* [0.08 ; 1.52]	38,415
Observations		112,231

\*  $p < 0.05$ , one-sided test. 95% Cis in brackets.

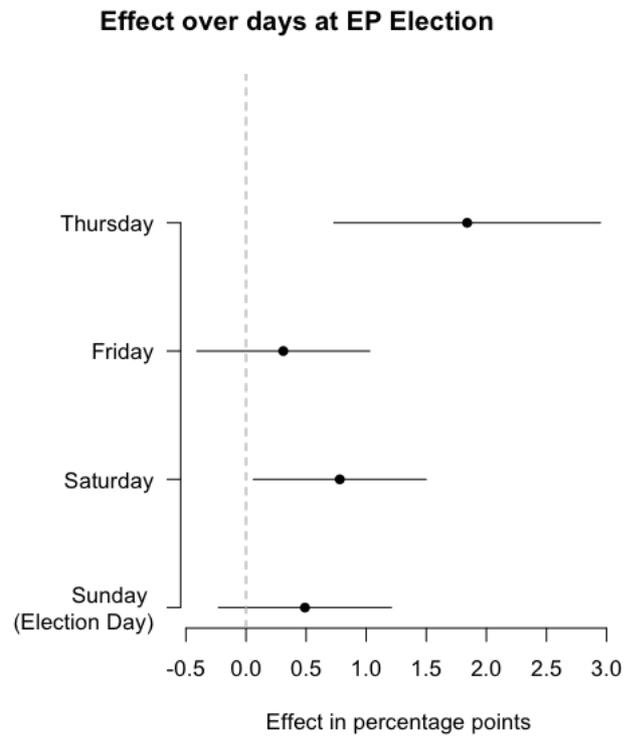
**Figure 1: Effect estimates and pooled estimate from four text message experiments**



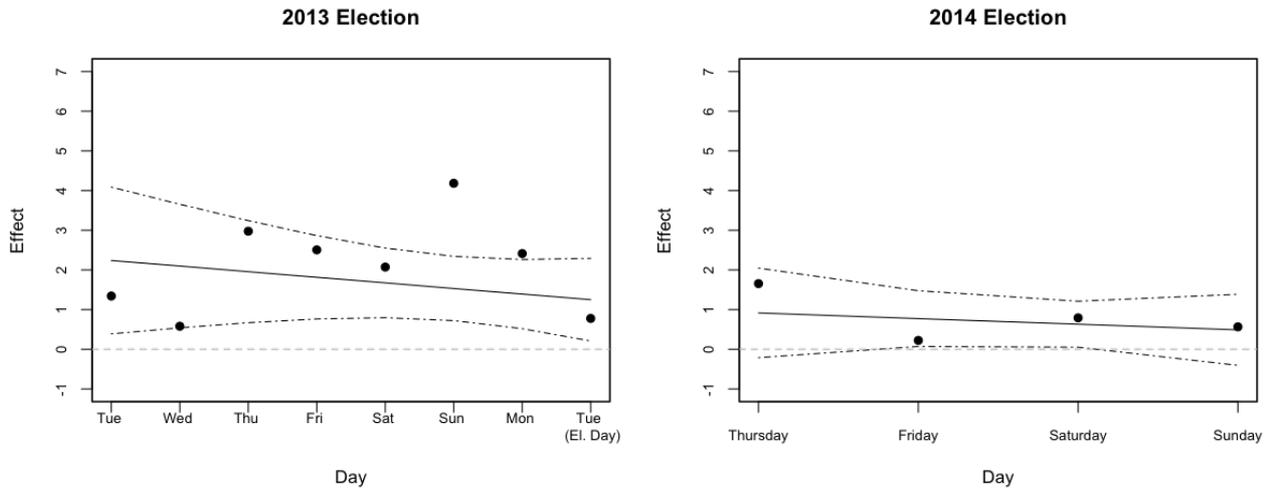
**Figure 2: The effect of receiving a text message conditional on timing, municipal elections**



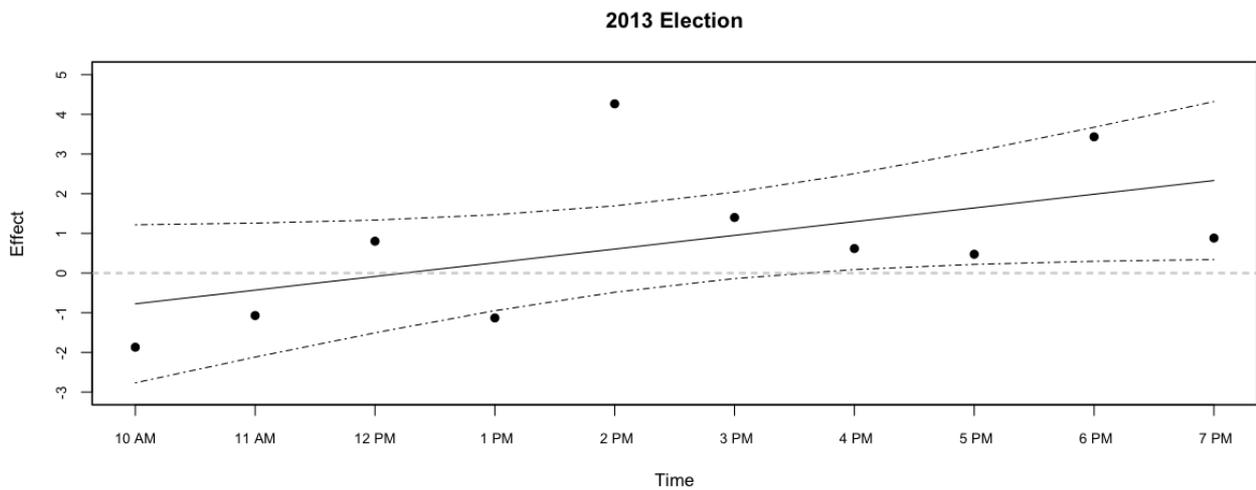
**Figure 3: The effect of receiving a text message conditional on timing, European elections**



**Figure 4: Linear fit to daily estimates (precision weighted)**



**Figure 5: Linear fit to estimates over time on Election Day (precision weighted)**



## Appendix

**Table 5: Effect of receiving a text message ahead of European Elections conditional on timing and phrasing**

	Treatment effect	N
Thursday, traditional	1.55* (0.87)	3,572
Thursday, fresh	1.76* (0.87)	3,564
Friday, traditional	-0.03 (0.53)	11,655
Friday, fresh	0.48 (0.53)	11,606
Saturday, traditional	0.39 (0.53)	11,677
Saturday, fresh	1.20* (0.53)	11,707
Sunday (Election Day), traditional	0.70 (0.53)	11,731
Sunday (Election Day), traditional	0.43 (0.53)	11,538
Turnout in control group	43.78* (0.26)	35,181
		112,231

\*  $p < 0.05$ , one-sided test. Standard error in parenthesis.

## Covariate balance across the four experiments

Table 6: Experiment 1: Danish Youth Council

	Age	Percentage females	Education*	Percent with Danish ethnicity
Control Group	25.68	50.05	2.74	94.05
Treatment Group	25.70	50.23	2.74	94.01
N	47,790	47,790	46,360	47,790

\* Education is a scale ranging from 1 to 5, with 1 representing high school as the highest completed education and 5 for those who have completed a at least five years of university.

Table 7: Experiment 2: Everybody's Election

	Age	Percentage females	Education*	Percent with Danish ethnicity
Control Group	46.55	47.49	2.85	61.31
Treatment Group	46.70	47.84	2.85	61.34
N	91,872	91,872	84,619	91,872

\* Education is a scale ranging from 1 to 5, with 1 representing high school as the highest completed education and 5 for those who have completed a at least five years of university.

Table 8: Experiment 3: Danish Parliament

	Age	Percentage females	Education*	Percent with Danish ethnicity
Control Group	22.72	45.55	2.24	91.74
Treatment Group	22.73	45.38	2.22	91.69
N	54,625	54,625	53,177	54,625

\* Education is a scale ranging from 1 to 5, with 1 representing high school as the highest completed education and 5 for those who have completed a at least five years of university. Note the descriptive statistics are based on the weighted average of the two blocks.

Table 9: Experiment 4: European Parliament Elections Experiment

	Age	Percentage females	Education*	Percent with Danish ethnicity
Control Group	23.79	47.87	2.38	94.74
Treatment Group	23.82	48.04	2.40	94.79
N	110,962	112,231	111,111	110,962

\* Education is a scale ranging from 1 to 5, with 1 representing high school as the highest completed education and 5 for those who have completed a at least five years of university.