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Getting Out the Vote

Experiments in voter mobilization among
immigrants and natives in Norway

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Norwegian summary

Valgdeltakelsen blant innvandrere i Norge er betydelig lavere enn i befolkningen for øvrig. Den lave deltakelsen blant innvandrere blir ofte sett på som problematisk. Demokratiets legitimitet kan avhenge av et visst nivå av deltakelse i alle grupper, og for innvandrerne selv gir den lave deltakelsen en begrenset politisk innflytelse. En mulig løsning på problemet med lav deltakelse er å innføre tiltak som gir stemmeberettigede et incentiv til å stemme. Denne rapporten presenterer resultater av eksperimenter hvor man tester og måler effekten av slike tiltak.

I forbindelse med lokalvalget i 2015 ble det gjennomført to grupper av eksperimenter med tiltak for å øke valgdeltakelsen. Formålet med eksperimentene er å undersøke hvilke tiltak som vil være mest effektive for å mobilisere folk til å stemme. For å måle effekten av tiltakene, sammenliknes eksperimentgrupper som mottar tiltak, med kontrollgrupper som ikke mottar noen tiltak. Eksperiment- og kontrollgruppene er valgt ut på samme måte, slik at det eneste som skal skille dem er om de mottar tiltakene eller ikke.

Informasjon om valgdeltakelse hentes fra det elektroniske valgmanntallet som er innført i 27 av landets kommuner og som ble gjort tilgjengelig av Kommunal- og moderniseringsdepartementet (KMD). Eksperimentene ble bare gjennomført i de 27 kommunene med elektronisk manntall.¹

Eksperimenter av denne typen har aldri tidligere blitt gjennomført i Norge, og det er bare noen få eksempler på liknende forskningseksperimenter i andre europeiske land. Det aller meste av tidligere forskning på dette området er gjennomført i USA. Denne forskningen viser at enkle upersonlige tiltak har begrenset effekt, mens personlige henvendelser, og særlig dør-til-dør-aksjoner kan være virkningsfulle. Noe av denne

¹ Det gjelder Oslo, Bergen, Trondheim, Stavanger, Bærum, Fredrikstad, Drammen, Sandnes, Sarpsborg, Asker, Skien, Skedsmo, Bodø, Sandefjord, Larvik, Tønsberg, Karmøy, Porsgrunn, Haugesund, Ålesund, Mandal, Vefsn, Hammerfest, Re, Tynset, Radøy og Bremanger.

forskningen har likevel vist at en enkel påminnelse om det forestående valget i form av en SMS kan bidra til å mobilisere enkelte velgergrupper. De norske eksperimentene brukte brev og SMS som tiltak for å mobilisere velgere. Tidligere forskning gir grunn til å forvente signifikante, men begrensede effekter av slike tiltak.

Den første gruppen av eksperimenter ble gjennomført i samarbeid med Integrerings- og mangfoldsdirektoratet (IMDi) og åtte av landets fylkesmenn. Tiltakene gikk ut på å sende brev til stemmeberettigede innvandrere i forkant av valget. Det ble sendt ut tre typer brev. Alle de tre brevene har identiske første og siste avsnitt. Avsnittene inneholdt en generell oppfordring om å stemme og praktisk informasjon om stemmegivningen. I tillegg inneholdt brevene et varierende andre avsnitt.

I det første brevet ("Brev 1 Hemmelig valg") understrekes det at stemmegivning i Norge er hemmelig; at man som velger ikke risikerer at noen finner ut hva man har stemt på. Dette brevet ble bare sendt til utenlandske statsborgere som fikk stemmerett for første gang i 2015.

Det andre brevet (Brev 2 Positivt budskap) ble det lagt vekt på at valgdeltakelsen blant innvandrere er økende. Velgeren oppfordres til å stemme for å bidra til denne positive trenden. Brevet ble sendt til et generelt utvalg av stemmeberettigede innvandrere.

Til slutt sendte vi ut et brev (Brev 3 Negativt budskap) som beskrev valgdeltakelsen blant innvandrere som alt for lav, og mye lavere enn blant velgere for øvrig. Velgeren oppfordres til å stemme for å snu denne negative trenden. Dette brevet ble også sendt til et utvalg av stemmeberettigede innvandrere.

Avsender av disse tre brevene var fylkesmennene i det enkelte fylke. Brevene ble sendt ut slik at velgerne mottok dem på torsdag eller fredag (den 10 eller 11.). Med andre ord, 3–4 dager før valgdagen, mandag den 14. september.

Kampanjen med bruk av tekstmeldinger (SMS) ble gjennomført i samarbeid med KMD. Den rettet seg mot alle velgere – både de med og uten innvandrerbakgrunn. Meldingene ble sendt ut hver dag (til nye grupper av velgere hver gang) den siste uken før valget. Det ble i alt sendt ut 135 810 tekstmeldinger.

Meldingene som ble sendt ut før valgdagen inneholdt følgende tekst: ”Hei! Dette er en vennlig påminnelse om lokalvalget 14. september. Demokratiet har bruk for din stemme, så husk å delta i valget! Hilsen valg.no”.

På valgdagen ble følgende meldinger sendt ut (med riktige åpningstider for valglokalene i den enkelte kommune): ”Hei! Har du stemt? Hvis ikke, kan du ennå rekke det. Valglokalene er åpne fra kl. XX til kl. XX i dag. Delta i valget! Hilsen valg.no”

Resultatene av disse eksperimentene fremgår av tabellen under. Den nederste raden viser valgdeltakelsen i kontrollgruppene, altså blant de som ikke fikk et tiltak. De andre tallene i tabellen viser hvor mye høyere valgdeltakelsen er blant de som fikk tiltaket. For eksempel, blant norske velgere uten innvandrerbakgrunn økte valgdeltakelsen med 1,6 prosentpoeng for de som fikk en SMS. Valgdeltakelsen blant de som fikk SMS var altså (68,9 + 1,6=) 70,5 prosent.

Alle de tre brevene og SMSene viste seg å ha større effekt på valgdeltakelsen enn vi hadde grunn til å forvente. Når det gjelder SMS-kampanjen, så er effekten minst i den gruppen som har høyest valgdeltakelse: blant norske velgere uten innvandrerbakgrunn. Effekten på 1,6 prosentpoeng er likevel uttrykk for en reell og statistisk signifikant økning i valgdeltakelsen. Effekten øker betraktelig i to grupper av velgere som er kjennetegnet av å ha lav valgdeltakelse: unge velgere og innvandrere. I begge gruppene fører SMSene til en betydelig økning i valgdeltakelsen.

Tabell 1.1 Resultater av alle eksperimentene.
Prosentpoengs økning i valgdeltakelsen

	Velgere uten innvandrerbakgrunn		Innvandrere	
	Alle	Under 30 år	Fikk stemmerett i 2015	Andre
SMS	1,6	4,6	3,1	2,3
Brev 1 Hemmelig valg			5,6	
Brev 2 Positivt budskap			4,8	3,0
Brev 3 Negativt budskap			7,0	3,7
Prosent valgdeltakelse i kontrollgruppen	68,9	45,3	20,9	40,1

Forskningsprosjektet har hatt spesiell fokus på en velgergruppe med særlig lav valgdeltakelse: utenlandske statsborgere som får stemmerett for første gang. Valgdeltakelsen i denne gruppen (i de 27 kommunene hvor eksperimentene ble gjennomført) er 20,9 prosent. SMSene bidro til en økning på 3,1 prosentpoeng i denne gruppen (til 24,0 prosent). For disse velgerne er brevene sendt fra fylkesmennene mest effektive. De bidrar til en økning av valgdeltakelsen på mellom 5 og 7 prosentpoeng. Vi ser noe mindre effekter av brevene for innvandrere generelt.

Forskjellene i effekt mellom brevene er små, så vi kan ikke konkludere med at ett budskap er mer effektivt enn et annet.

Alt i alt har tiltakene som ble gjennomført for å øke valgdeltakelsen i forbindelse med lokalvalget 2015, vært effektive. Dette er tiltak som kan gjennomføres i stor skala. Resultatene fra 2015 tyder på at slike tiltak vil bidra til en økning i valgdeltakelsen ved norske valg.

1 Introduction

Voter turnout among immigrants lags behind that of the native populations in Western democracies (see e.g., Wüst et al. 2010). Given that political equality is the fundamental premise of democracy, the high number of non-voters among immigrants is a major concern in many countries. Inequalities in turnout may indicate a lack of inclusion of immigrants in society, and it probably means that the views and interests of immigrants are not brought into the political process to the extent that they could, and perhaps should, be.

In order to increase participation among underrepresented groups, such as immigrants, political scientists have noted the potential of Get Out The Vote (GOTV) strategies (Enos et al. 2014). Field experimentation has obvious advantages if we want to understand turnout (Green et al. 2013). Random allocation of treatment ensures that the potential turnout of individuals in the treatment group is identical to the potential turnout of those in the control group. Furthermore, field experiments measure treatment effects on “actual voters in the midst of an election” (Ibid: 30), voting is measured using public records (not self-reported), and individuals are usually not aware of the fact that they are part of an experiment. Hence, the method is unusually fruitful when it comes to shedding “light on the causes of political participation” (Ibid). Apart from a Danish SMS text message campaign aimed at first- and second-generation immigrants (Bhatti et al. 2014a), we know very little about the effectiveness of mobilization strategies in a European context (Green et al. 2013).

Research on the use of SMS text messages as voter mobilization tools in the U.S. has led to the formulation of the Noticeable Reminder Theory (NRT) (Dale & Strauss 2009; Malhotra et al 2011). Its premise is that registered voters generally have the intention to vote but frequently fail to do so because of time constraints and lack of planning. Normative arguments in favor of voting are not necessary to mobilize these voters; all they need is a noticeable reminder, such as one provided by a text message.

This report expands on this body of work by reporting the results of two sets of randomized field experiments, testing the effectiveness of mail mobilization appeals among immigrants as well as SMS text messages directed at both immigrants and native voters in Norway.

The purpose of this report is to present the results from these experiments. We begin with a look at previous research, and we outline expectations for our findings. We then go on to describe the setting of the experiment—the 2015 Norwegian local elections—and the data used to measure voter turnout. We continue with a description of the study population itself, and the sampling of experiment and control groups. Finally, we present the results and discuss the implications of our findings.

2 Previous research

Field experimentation has, until now, scrutinized the effectiveness of different ways of contacting voters (using tactics such as canvassing, direct mail, phone calls, and SMS text messages) and the responsiveness of voters to the content of the messages (Arceneaux & Nickerson 2009a; Michelson & Nickerson 2011; Matland & Murray 2012; Green et al. 2013). Previous studies find that the way voters are contacted matters. Phone calls seem to have no effect, direct mailing has limited positive effects, and canvassing has a large and substantial effect on turnout.

The general view in the GOTV literature is that face-to-face mobilization techniques get more voters to the polls compared to impersonal contact tactics such as phone calls, emails, and direct mail (Arceneaux & Nickerson 2009a; Michelson & Nickerson 2011; Matland & Murray 2012; Green et al. 2013). This view is challenged by the NRT, which holds that a simple nudge in the form of an SMS text message is enough to mobilize voters (Dale & Strauss 2009; Malhotra, Michelson & Rogers 2011). Dale and Strauss (2009), based on a sample of young voters, find that SMS reminders produce a statistically significant 3.0 percentage point increase in the likelihood of voting. These results are echoed by Malhotra et al. (2011) on a broader sample of voters. The authors conclude that social connectedness is not the only key to increased participation. Both studies were conducted in an American context, and Gerber et al. (2013: 34) describe the effectiveness of text messaging as “an intriguing anomaly.” The original Dale and Strauss study was later replicated in Denmark (Bhatti et al. 2014b). In the 2013 Danish local elections, they conducted three SMS text messaging campaigns. Two campaigns targeted young voters (one in the age group 18-29 (N=55,000), and one in the age group 22-29 (N=35,000)), and one was conducted on a random sample of 51,000 Danes (irrespective of age). As for the two campaigns aimed at young voters, the first produced a statistically insignificant 0.4 percentage point increase in turnout, while the second significantly raised turnout by 1.8 percentage points. The third SMS experiment resulted in an overall insignificant 0.3 percentage point increase in turnout but boosted turnout among immigrants and their descendants by 1.0 and 2.9 percentage points, respectively.

Although the GOTV body of literature is large and continuously expanding, studies focusing on minority populations are not plentiful. The experiments in this report speak to the GOTV literature that focuses on subpopulations with low average rates of voter turnout (see Trivedi 2005; Ramirez 2005; Michelson & Nickerson 2011; Matland & Murray 2010; Michelson, Garcia Bedolla & McConnel 2009, 2014; Nicherson 2006).

Bhatti et al. (2014a) targeted immigrants in an SMS text message campaign in Denmark. It increased turnout among immigrants by 0.96 percentage points and among descendants of immigrants (second-generation) by 2.93 percentage points compared to the control group. In a review of the U.S. literature on GOTV campaigns targeting minority populations, Chong and Junn (2011: 327–28) observe that “taken together – none of the field experiments shows strong or consistent positive effects from direct mailings, regardless of content, format.”

More recent U.S. research, however, finds substantively large and statistically significant effects of these types of GOTV campaigns directed at minority groups (see e.g. Matland & Murray 2012; Michelson & Bedolla 2014). Matland and Murray (2012), studying a Latino community in the 2004 U.S. presidential election using canvassing and direct mail, find substantial and statistically significant effects of their mailing campaign: a 2.95 percentage point rise in turnout. This is a larger effect than that of similar mailing campaigns directed at the majority population, or at voters generally. In a meta-analysis of 15 GOTV experiments directed at Latino and Asian-American voters in the U.S., Michelson and Bedolla (2014) find quite substantial effects overall, though with great variation from one experiment to the next. The experiments directed at people with immigrant backgrounds seemed to be most effective among Asian-American voters. For Latino voters, the experiments were most effective when directed at those born in the U.S.

In other words, the effectiveness of a GOTV campaign depends on the manner in which voters are contacted and the electoral context of the campaign. What works in one election in one country may not work in another election in a different country. The content of the messages to voters clearly also matters (see e.g., Green et al. 2013: 37). We address this in our discussion of the letter campaign below.

3 Experimental setting and data

The experiments were fielded prior to the September 14, 2015 Norwegian municipal elections. Before we present the data, a short note on the setting for the experiments may be useful. Norway is a two-tier system of local government consisting of 428 municipalities (*kommuner*) and 19 counties (*fylker*). The average-sized Norwegian municipality has about 11,000 inhabitants.² Elections are based on Proportional Representation, and they are held every second year, alternating between elections for the parliament (*Storting*) and local/county government.³ Turnout for parliamentary elections remains at a high level, comparatively speaking. Municipal- and county-level turnout, however, is lower and has decreased over time (Christensen & Arnesen 2013). In the September 2015 municipal elections, 60 percent of registered voters took part. Turnout in the 2013 Norwegian national parliamentary election was quite a bit higher: 78.2 percent.⁴

Eligibility rules state that all Norwegian citizens and citizens from other Nordic countries who are 18 years or older on Election Day and have permanent residence in the municipality have the right to vote. Immigrants from non-Nordic countries can vote after three years of continuous residence in Norway.

In order to design the experiment, we received access to an electronic version of the electoral roll for approximately 1.7 million Norwegian voters living in 27 municipalities⁵ (out of a total of 428 municipalities) that have adopted electronic registration of turnout.⁶ Since practically all of Norway's larger towns and cities have electronic registration of turnout, our datafile includes a large section of Norway's eligible voters: 42

2 Most municipalities are smaller; the median one has only 4.5 thousand inhabitants.

3 There is, in other words, a four year interval between each type of Norwegian election (parliamentary or local/county).

4 The government has created several initiatives to increase turnout, including direct elections of mayors in selected municipalities, Internet voting, and lowering the voting age from 18 to 16 in selected municipalities. The latter was tested out in the 2011 local elections, and again in 20 municipalities in 2015.

5 The 27 municipalities are (ordered by population size, from large to small): Oslo, Bergen, Trondheim, Stavanger, Bærum, Fredrikstad, Drammen, Sandnes, Sarpsborg, Asker, Skien, Skedsmo, Bodø, Sandefjord, Larvik, Tønsberg, Karmøy, Porsgrunn, Haugesund, Ålesund, Mandal, Vefsn, Hammerfest, Re, Tynset, Radøy, and Bremanger.

6 Turnout is registered electronically, but voters still vote using a paper ballot.

percent.⁷ To pull our samples, we received information from the National Population Register on every individual in our file with respect to birthdate, gender, country of origin, citizenship, and parents' country of origin and citizenship. For those who immigrated to Norway, we also gained access to the date of entry to Norway. This was used to pull immigrants without previous voting records (based on the requirement of three years of legal residence to get voting rights). Experiment and control groups were randomly sampled from this file. After the election, the Ministry of Local Affairs provided us with records for all 1.7 million citizens as to whether they voted or not.

⁷ All in all, about 4 million residents were entitled to vote in 2015.

4 Study population and experimental design

The populations studied in this report are first-generation immigrants in Norway and voters in general. First-generation immigrants constitute approximately 278,000 eligible voters in the 27 municipalities from which we have voting records. The immigrants come from every world region, though most are from Europe or Asia (see Table 4.1). Labour immigration from European Union countries has gone up in recent years, and has since 2007 overtaken the other two main causes for immigration to Norway: seeking refuge and family reunification. Sweden and Poland are the main countries of origin for labour immigrants to Norway. Those two countries also constitute the largest immigrant groups in our sample (see Table 4.2). The third largest group, Pakistanis, have a history in Norway that stretches back to labour immigration in the 1970s. A number of Pakistanis have arrived since then, through family reunification.

The records for these individuals were used to pull one individual name per address to generate samples for our field experiments. Since many immigrants live in apartment buildings, we resampled from addresses with more than six registered voters. For every additional 12 names per address above six, we sampled one more name. For example, if the address had 126 names, we randomly picked 11 to participate in the experiment.

Design of the letter campaign

The experiment group in the letter campaign consists of 19,500 individuals divided into three groups of 6,500; each group received a specific letter. The control group was made up of individuals selected for the sample but not pulled for treatments (141,625 individuals in all).⁸

⁸ This is less than the full 278,079–19,500 immigrants for two reasons. First, 48,500 immigrants were used in the text message experiment. Second, when pulling the samples for the households with more than one voter, only one person per household was identified to avoid sending multiple and different letters to the same household. After the election, we received data on turnout from 16,790 fewer individuals than those in the original file (used to draw samples). The main reason for this is probably that people have moved from one of the 27 municipalities to some other part of the country or abroad in the period from when our first sample was drawn (early July) until just after the election (mid-September). People who have passed away in the same period will also be removed from the electoral roll. Of these 16,790 individuals, 4,971 were in our control group, 131 received the privacy letter, 75 the positive letter, and 70 the negative treatment.

Table 4.1 Origin of immigrant voters in the 27 municipalities

World Region	Number of Voters
Asia	94,013
Eastern Europe	71,189
Western Europe	61,523
Africa	34,911
Latin America	10,307
North America	4,865
Oceania	900
Unknown	371

(N=278,079)

Table 4.2. Country of origin for the 20 largest immigrant groups in the 27 municipalities

Country	Number of Voters	Country	Number of Voters
Poland	28,575	Turkey	7,537
Sweden	22,061	Lithuania	7,066
Pakistan	13,785	The Philippines	6,658
Iraq	12,875	Great Britain	6,463
Somalia	12,481	Russia	6,221
Iran	9,027	Sri Lanka	6,115
Denmark	8,899	Bosnia and Herzegovina	5,955
Vietnam	8,101	Afghanistan	5,538
Serbia and Montenegro	7,913	Thailand	5,483
Germany	7,725	India	4,718

The three letters (treatments) have an introductory paragraph with a general appeal to participate in the election, followed by a second paragraph where the message is varied, and a final paragraph with identical information about how to participate (see Appendix A). The first and third paragraphs are exactly the same in all three letters and were included to add an extra reason why immigrants in all three groups should participate in the election. The second varying paragraphs start with a headline followed by a few lines of text. The three letters are worded as follows:

1. Your vote is private!

In the polling place, you enter into a booth where you are completely alone when voting. After deciding which party you will vote for, you put the ballot into a locked container with a lot of other ballots. Neither the election officials nor anyone else in the polling place are allowed to ask you who you voted for. You can be completely certain that your vote is private.

2. Participation among immigrants is increasing. Do your part and set a new voting record!

You do not have to be a Norwegian citizen to take part in the election. Anyone who has resided in Norway for three years or more has the right to vote. In the local election in 2011, more immigrants voted than ever before. Voter turnout in several immigrant groups was well above 50 percent. Do your part and contribute to an even greater participation among immigrants in this year's election!

3. Participation among immigrants is too low. Help turn this trend around!

You do not have to be a Norwegian citizen to take part in the election. Anyone who has resided in Norway for three years or more has the right to vote. Unfortunately, immigrants took part at a much lower rate than the rest of the population in the local election in 2011. Voter turnout in several immigrant groups was well below 50 percent. You can contribute to turning this trend around and to raising turnout levels among immigrants by voting on September 14!

All letters were written in Norwegian, which for most immigrants is a second language. It seems likely that at least some of the recipients will have trouble reading and understanding the content, though we believe most will have lived in Norway long enough to be able to make sense of these messages.⁹

The first varying paragraph (the privacy letter) has information about the measures taken to secure ballot secrecy in Norwegian elections. The Norwegian electoral system has strong formal rules to secure ballot secrecy. Some immigrants, however, come from countries with low-quality electoral institutions compared to that of Norway. Electoral fraud, ballot stuffing, and violence commonly occur with elections around the world, and survey data suggest that people's perceptions of such malpractices lead to lack of confidence in elected authorities and discourage voter turnout (Norris et al. 2014). A substantial portion of non-European immigrants have come to Norway as refugees, having fled oppression and persecution (quite a few have also come for family reunification with people who were refugees in the first place). It seems plausible that people with backgrounds from countries with faulty democratic procedures will be affected by information regarding ballot secrecy in Norwegian elections.

In the only relevant previous study, Gerber et al. (2013: 539) point out that formal rules regarding ballot secrecy may not be sufficient to secure participation. Voters may worry about direct sanctions for the choices they make when voting or whether they have to justify their choices when voting (Ibid). In an experimental setup, their study shows that assuring American citizens that their vote is private increased turnout among registered voters without a previous record of voting, but not among citizens who had previously voted. They report an increase in turnout of more than 3 percentage points for voters without records of previous turnout. If such an impact is found among U.S. citizens in general, we should expect to find a significant and possibly stronger effect among immigrants in Norway. In the empirical analysis, we test this assumption among immigrants registered to vote for the first time in the 2015 Norwegian municipal elections.

⁹ We did consider sending out messages in the native languages of the recipients, but we decided against this for two reasons. First, it would have been quite costly to have the letters translated into several languages, and second, people may have reacted negatively toward receiving letters that implied that their Norwegian skills were not adequate.

The other two letters suggest that the participation rate among immigrants had increased in the last election (the positive letter), or that immigrants participate at a lower rate compared to the native Norwegian population (the negative letter). Both messages are true. Thus, the two letters are meant to investigate whether feedback of past participation among immigrants affects turnout. Previous work suggests that it matters whether individual voters or subgroups of voters receive either positive or negative feedback about their participation rates (Arceneaux & Nickerson 2009b). Group identity theory, for instance, states that invoking consistent descriptive and prescriptive norms leads to stronger effects than invoking inconsistent descriptive and prescriptive norms (Gerber & Rogers 2009). These authors found that the message was significantly more effective when descriptive and prescriptive norms were consistent. However, this finding has been contradicted. Studies done by Panagopoulos et al. (2014) and Nickerson and White (2013) produce a fascinating finding: when minorities are seen as participating less than the majority Anglos, their participation is depressed; when they are seen as having an equal level of participation, their participation increases.

The letters used in our Norwegian experiments refer explicitly to turnout levels among immigrants. Even if some may see immigrants as a coherent group, we know less about whether immigrants see themselves that way.

In an interesting study of group consciousness among Latinos in the U.S., Masuoka (2008) finds that “national origin consciousness” is the most prevalent. For our purposes, this means that group consciousness among Norwegian immigrants may be linked to more than 150 different countries of origin. Nevertheless, the type of group consciousness that affects political participation, according to Masuoka (2008), is not national origin but rather the most general category which she dubs “racial consciousness.” Identification with other racial or ethnic minority groups positively affects some forms of political participation, though not voting. Based on this, it may be that the most general type of minority group consciousness triggers political participation. Other studies have found similar effects, including effects on voting (Stokes 2003; Wright Austin et al. 2012). The only comparable study that we have found from a European country is that of Sanders et al. (2014). They find that perceptions of discrimination against ethnic minorities may trigger political participation among ethnic minority voters (see also Bergh & Bjørklund 2011).

This suggests that the term “immigrant” used in the Norwegian GOTV letter campaign may resonate with some voters’ sense of group identity. This may, in addition, be the most relevant category to use in an appeal for political participation. However, others may not identify with the term at all and may even find it offensive. We received feedback from some recipients that were offended by being (indirectly) identified as immigrants in these letters. These were generally people with a long period of residence in Norway.¹⁰

It is conceivable that our *positive* and *negative* letters are interpreted as more of an individual message to each person who receives them. The negative letter could be seen by some as a rebuke of previous abstention, while others could see the positive letter as an expression of appreciation for their previous vote. Panagopolous (2011) points out that thanking voters makes a difference. Gerber et al. (2013) test the effectiveness of mailing voters the record of their actual previous turnout. They find an overall effect of such messages, but the most effective message is the one that refers to previous abstention. Hence, a negative message that perhaps induces a feeling of shame in people for not having voted may also prove to be effective. Given this research, and considering the possibility that voters interpret the letters as a personal rebuke/recognition of past non-voting/voting activity, one may expect the *negative* letter to be the most effective.

The privacy letter was mailed to 6,367 immigrants who met the three year residence rule for the first time in the 2015 municipal election. The sample used for the second and third letters was pulled from the general immigration population, irrespective of their previous voting rights.

Table 4.3 displays the composition of the three treatment groups as well as the control group when it comes to immigrants voting for the first time, and the general immigrant population.

10 By a mistake, 134 letters were also sent to people who were descendants of immigrants; they were not immigrants themselves. Some of these individuals reacted negatively to receiving the letters. Data from the 134 individuals have been taken out of the analysis.

Table 4.3 Immigrant letter campaign: Control and treatment groups

	First-time voters	Other immigrants
Control group	49,881	72,711
Privacy letter	6,367	-
Positive letter	1,960	4,457
Negative letter	1,968	4,459
Total	60,176	81,627

The content of the letters (the treatment) was developed in cooperation with The Directorate of Integration and Diversity (IMDi), a government directorate with responsibilities for integration and diversity in Norway. IMDi sponsored the distribution of the letters and, during the election campaign, cooperated with the County Governors (*Fylkesmenn*), who mailed the letters to the subjects' homes. The letters were mailed so that they arrived at most households on Friday, September 11—three days prior to the day of the election (Monday, September 14).¹¹ IMDi and the County Governors are both relevant institutions when it comes to communicating with immigrants on issues related to elections. IMDi's main task is the settlement and general integration of immigrants who have been granted permanent residence in Norway. As part of its general work toward integration, the Directorate has for some time been actively involved in mobilizing the immigrant population to vote in elections.¹² The County Governor is the main representative of the central government at the county level. He or she gives advice to municipalities on how to hold elections. The County Governor is also in charge of Norwegian citizenship ceremonies for immigrants, so there is at least a possibility that immigrants have some knowledge of this office beforehand.

Hence, our experimental design should capture contact in a real world setting, something that is crucial to the external validity of field experiments such as this (see Michelson & Nickerson 2011: 235). The fact that the letter recipients had not given prior consent to receiving the letters adds to the strong external validity of the experiment.

11 To determine whether the letters actually arrived Thursday or Friday, we recruited 10 individuals to receive the letters (we also arranged for it to be sent to our home addresses). They all reported that the letters arrived either on Thursday or Friday.

12 It has always been a non-political effort, just like the letters used in our experiment. They generally encourage people to vote, but do not say anything about who, or which party, to vote for.

Design of the text message campaign

In designing the SMS text message experiment, we began with 1,263,667 people, both immigrants and native Norwegians, who had a registered cell phone number. We split this population into four groups: 1) foreign nationals who received voting rights for the first time in 2015; 2) other (first-generation) immigrants; 3) descendants of immigrants (second-generation); and 4) Norwegians with no immigrant background. From these four groups of voters, we randomly assigned individuals into treatment or control groups. Table 4.4 displays the composition of the treatment and control groups. The reason we included second-generation immigrants is that the Danish study found an especially strong effect in this group.

The large sample sizes in our experiment enable us to detect even small effects of the treatment. Note also that the number of individuals in the two control groups is less than the potential number of possible participants. This is due to two reasons. First, 19,500 immigrants were used in the letter-campaign experiment. Second, when pulling the samples for the households with more than one voter, only one person per household was identified to avoid sending multiple messages to the same household. Finally, we oversample native young voters (aged 18 – 29), to see if that low turnout group is affected by text messages.

Table 4.4. SMS text campaign: Control and treatment groups

	Treatment	Control
First-time voters (foreign nationals)	9,247	32,490
Other first-generation immigrants	38,996	45,375
Second-generation immigrants	7,513	7,515
Natives	75,559	379,634
Total	131,315	465,014

Text messages were sent in the last week before Election Day (From Monday, September 7 to Monday, September 14). Around 8,500 messages were sent at 7 p.m. for a full seven days prior to the election, for a total of 73,186 messages. The second round of messages was sent on Election Day. A total of 22,269 messages were sent to immigrants at 8 a.m. and every two hours onward until 6 p.m. 35,900 Norwegians also received SMS messages on that day.

Because we wanted to compare the results from our experiment with previous studies, we used similarly phrased text messages. The SMS messages included a reminder of the upcoming election with a short civic duty appeal. The two rounds of messages were worded as follows:

SMS#1: *Hi! This is a friendly reminder of the local election on September 14. Democracy needs your voice (vote)¹³, so remember to participate in the election! Regards valg.no.*

SMS#2: *Hi! Have you voted? If not, you can still make it. Polling stations are open from XX AM to XX PM today. Participate in the election! Regards valg.no*

The content of the messages was developed in agreement with the Ministry of Local Government and Modernization (KMD). KMD has the overall responsibility for all elections in Norway; we used the address of their election webpage, valg.no (election.no), as the sender of the messages. Thus, the experimental design captures contact in a real world setting, something that adds to the external validity of the field experiment (see Michelson & Nickerson 2011: 235). The receivers of the SMS messages had not given prior consent to receiving them, which further strengthens the external validity of the experiment. Hence, we studied the effect of “cold” text messaging (messages without prior consent), as opposed to the “warm” messages (messages with prior consent) used in the original Dale and Strauss study (2009). This means that the results from our experiment cannot be explained by recipients agreeing to receive the texts (Gerber et al. 2013: 34). The messages were sent out by the polling agency Respons. This agency also kept track of which message was actually delivered to the recipient. Thus, we are able to use the contact rate to calculate precise and different treatment effects (see below).

13

We used the Norwegian word *stemme*, which means both “voice” and “vote.”

5 Results – letter campaign

The empirical analysis is performed in three steps. We begin with a simple analysis comparing the three treatment groups (letters) with the control group. The analysis is done separately for first-time voting immigrants, and then for the general immigrant population. We do this not only because we expect the effects to differ between the two groups, but also to lay the foundation for the second step—testing whether there is variation in the effectiveness of the three letters. In the third step, we perform two regression specifications, testing whether the effects hold up to various types of tests.

Table 5.1 displays turnout rates, sorted from high to low, for each of the treatment groups compared to the control group, both for immigrants voting for the first time and the rest of the immigrant population. The table reports the “intention to treat effect” (ITT). ITT has become the standard for analyzing results from clinical trials, since it considers the individuals in the way they were originally randomized from the start of the experiment regardless of whether they received/completed the intervention or not (Michelson & Garcia Bedolla 2014). ITT analysis therefore maintains the benefits of randomization and gives an accurate answer to how effective a given treatment is in a real life situation. Obviously, we do not know if the letters were read or not, but we do have information about the number of letters that were returned to the sender. Thus, we are able to report the effect on individuals who actually received the letter. The “average treatment on the treated effect” (ATT) provides an idea of the maximum treatment effect, given that every single letter reaches the voter it is intended for.

Turning to the results, we begin with immigrants without previous voting records. Table 5.1 shows that this is a subgroup of voters with especially low turnout rates. Around 21 percent voted in the control group. On average, receiving a letter increased turnout by 4.9

percentage points. The associated standard error is 0.45 percentage points, so the effect is unlikely to be due to chance. Overall, the results for all treatments suggest strong evidence that the letters from the County Governors were read and acted on. First-time immigrant voters receiving the *privacy letter* voted at a rate of 26.5 percent, while the participation rate among those receiving the *positive letter* was 25.7 percent. Turnout climbs to 27.9 percent among first-time voting immigrants who received the *negative letter*. The latter led to a 7.0 percentage point increase in turnout compared to the control group. The effects are even more impressive in relative terms. Given the low base line among first-time voting immigrants, the rise in turnout in relative terms is around 33 percent. The effects among the group of first-time voting immigrants are sizeable compared to previous experiments, both in the U.S. and Denmark, targeting different subgroups of voters (Gerber et al. 2008; Bhatti et al. 2014b, 2015). The results are also a little surprising, given the modest findings from previous GOTV experiments aimed at minority populations (see Chong & Junn 2011), although there are examples of similar-sized effects in the literature (Matland & Murray 2012; Michelson & Bedolla 2014). Still, there have, as far as we know, been no previous field experiments studying immigrant first-time voters. Our results therefore suggest that direct mail may be a fruitful strategy to increase participation rates among immigrants without previous voting records.

Table 5.1 Experimental results. Intention to treat effects (ITT, percentage point increase in voter turnout in treatment group)

	Immigrants who could vote for the first time			All other immigrants		
	ITT	(s.e.)	N	ITT	(s.e.)	N
Letter – Privacy message	5.6	(.73)	6,367			
Letter – Positive message	4.8	(.93)	1,960	3.0	(.46)	4,457
Letter – Negative message	7.0	(1.15)	1,968	3.7	(.64)	4,459
Control group voter turnout	20.9		49,881	40.1		72,711

Continuing to the general immigrant population (excluding first-time voters), we find somewhat smaller effects. The table shows that the initial propensity to vote is substantially higher among this group of immigrants. Approximately 40 percent voted in the control group, which is nearly 20

percentage points higher than the control group of immigrants who voted for the first time. Hence, when evaluating the two treatment effects, we start out from a much higher base level. Still, both letters mailed to a randomly pulled group of immigrants increased turnout. Immigrants receiving the *positive letter* voted at a rate of 43.1 percent, compared to 43.8 among those receiving the *negative letter*. The results for this group suggest that receiving any of the two letters increased turnout by 3.4 percentage points compared to the control group. Looking only at these two letters, the increase in turnout among immigrants voting for the first time was higher than that of the general immigrant population. Receiving the negative letter increased turnout among first-time voters by 3.3 percentage points more than among the general immigrant population.

The results, so far, stand out in comparison to previous GOTV experiments, especially when it comes to the size of the effects on first-time voting immigrants. The increases in turnout in the treatment groups, as compared to the control groups, are large compared to most U.S. experiments on minority populations (Chong & Junn 2011; Matland & Murray 2012; Michelson & Bedolla 2014). However, our results square well with previous research finding that the treatment effects are stronger for individuals (groups) with the lowest propensity to vote in the first place (Bhatti et al. 2015). Mailing letters to immigrants who are voting for the first time will increase turnout more than mailing such letters to the general immigrant population. It is important to determine who the ideal recipients of such letters are, but we are also interested in whether the actual content of the letters matters or not.

Our initial results indicate that the negative letter is the most effective in raising voter turnout, particularly among immigrant first-time voters. Previous research, however, suggests that both positive and negative messages may influence political participation (Panagopoulos 2011). As we have discussed above, a possible explanation for our results that squares with previous research is that voters interpret the negative letters as a personal rebuke for their own lack of previous turnout. Gerber et al. (2013) find that those types of messages tend to raise election turnout.

However, is the policy implication from our findings that policy makers would maximize turnout by mailing the negative letter to immigrant voters? Before answering that question, we should test whether the differences in the effectiveness of our letters are statistically significant.

Table 5.2 tests the differences between all three letters to first-time voting immigrants, and between the two letters mailed to immigrants in general. The table reports differences in percentage points for each letter (group), and each difference is associated with a significance test (standard errors). One of these differences is statistically significant. The negative letter is (just barely) more effective than the positive letter. If any policy recommendation is to come from this, it would probably be that any of these letters could be used to raise turnout in the immigrant population, but the negative letter seems preferable to the positive one.¹⁴

Table 5.2 Experimental results: Test of differences between treatment groups

	First-time immigrant voters	Other immigrant voters
Positive vs. privacy letter	-.8 (.8)	
Negative vs. privacy letter	1.4 (1.2)	
Negative vs. positive letter	2.2 (1.0)	.7 (.8)
N	60,176	81,627

(Standard errors in parentheses)

Even if we can argue beyond a reasonable doubt that the letters caused an increase in turnout, it is important to check if the effect holds up in different types of analyses. We run two separate linear probability regression models. The first model includes only an indicator variable for the different letters, (the control group is the base category), while the second model includes control variables. If we measure a causal effect, including control variables should not change the treatment effects notably compared to the model without such controls. The approach corrects for imbalances between experimental groups due to chance.

In the analysis, we include the following social background variables, which are all associated with turnout: gender (1=Male), age (continuous), age squared, Norwegian citizenship (1=Norwegian and included only for the general immigrant population), and immigrant (1= Western (Europe,

¹⁴ In hindsight, we would have liked to have had a fourth letter in the experiment, one that only included the first and third paragraphs (i.e., without the varying second paragraph). This would have tested the possibility that it is the letter itself that raises turnout, and therefore that the positive-, negative-, and secrecy-messages are unimportant.

North America, or Oceania), 0=Non-Western (Africa, Asia, or Latin America)). In addition, we include robust standard errors, clustered by the 27 municipalities, to correct for non-independence across respondents within municipalities. Again, we run the regressions separately for first-time voters and other immigrants. The dependent variable—turnout—is coded 1 for individuals participating in the election and 0 for non-participants.

We have performed regressions and used the results to calculate the probabilities based on the results from the different models (using the margins command in Stata). The probabilities (in percentages) and their associated standard errors are shown in Table 5.3. The column with the ITT effect should be similar to the results presented in Table 5.1. The only difference is that Table 5.3 is based on a regression approach instead of a t-test of proportions, and the regression shows the effects based on municipal specific standard errors. As we see in Table 5.3, the results from the regression are identical to that of the t-tests. Adding the controls changes the results somewhat. The result for the negative letter is more or less identical if we compare the two columns among first-time immigrant voters and the rest of the immigrant population. For the positive letter, we see a decrease in ITT by .6 percentage points among first-time voters, but considerably less among the general immigrant population. Taking the two groups together, the results for the positive letter indicate that the ITT decreases by around 0.4 percentage points after including the control variables. The corresponding data for the privacy letter show a .3 percentage point decrease in the ITT. None of these differences, between the initial effects and the effects with controls, are statistically significant.

Table 5.3 Experimental ITT estimates (probabilities) without and with control variables, and average treatment on the treated effects (ATT)

	First-time immigrant voters (N=60,176)			Other immigrant voters (N=81,627)		
	ITT	ITT with controls	ATT	ITT	ITT with controls	ATT
Privacy letter	5.6 (.73)	5.2 (.73)	6.1 (.59)			
Positive letter	4.8 (.93)	4.2 (.85)	5.2 (1.02)	3.0 (.46)	2.7 (.48)	3.1 (.79)
Negative letter	7.0 (1.15)	6.8 (1.03)	7.6 (1.02)	3.7 (.64)	3.5 (.67)	3.8 (.79)

(Municipal specific standard errors in parentheses)

Table 5.3 also displays the average treatment-on-the-treated effects—the effects of the letters on those who actually received them. The ATT is calculated by dividing the intent-to-treat effect by the contact rate. Performing a two-stage least squares regression of vote on actual contact using randomization as an instrument variable will, however, produce exactly the same ATT (see Gerber & Green 2005). We use the latter approach because it provides us with the correct standard errors. Even if the ATT overestimates the treatment effects (and is more likely to reject the null hypothesis), it provides some idea of the maximum treatment effect on individuals actually receiving the letters. This calculation takes into account the 1,109 letters that were returned to the sender and thus could not have been received by the voters. The ATT effects are somewhat larger than the ITT effects, but the differences are within the margin of error. There is a greater preponderance of returned letters in the group of first-time immigrant voters, leading to the largest adjustment of the effects in that group. All in all, the largest effect that we have seen of the letter campaign is the 7.6 percentage point effect of the negative letter directed at first-time immigrant voters.

6 Results – text message campaign

Table 6.1 displays turnout rates for each of the four treatment groups compared to their respective control groups. The table reports both the ITT effect and the ATT effect. We also include ITT effects with control variables, controlling for age and gender.

We begin with immigrants who are first-time voters. Around 22 percent of those in the control group voted, compared to around 69 percent in the Norwegian control group. The magnitude of the effects of receiving the text messages also differs between the experimental groups. First-time immigrant voters who received the text messages voted at a rate of about 25 percent, suggesting that, on average, receiving the SMS increased turnout by 3.1 percentage points (SE=0.49 percentage points) in this group. The rest of the immigrant population voted at a rate of about twice that of first-time voters (41 percent), but the effect of the text messages is similar: 2.7 percentage points. Turnout among second generation immigrants was 50.6 percent in the control group, and receiving the SMS resulted in a 0.21 insignificant increase in turnout in this group (SE=0.81 percentage points).

The turnout rate among native Norwegians receiving the SMS was 68.9 percent, indicating that it increased turnout significantly (one-tailed) by 0.36 percentage points (SE=0.18 percentage points) compared to that of the control group. As expected, given the different contact rates, the ATT effect is larger for immigrants than for Norwegians. Overall, text messaging increased turnout by 4 to 5 percentage points among immigrants and by 0.40 percentage points (SE=0.21) among Norwegians.

Table 6.1 Experimental results: Percentage points

Control group turn-out		Intention to treat (ITT)		ITT with control variables		Treatment on the treated (ATT)		N
		Effect	s.e.	Effect	s.e.	Effect	s.e.	
First-time voters (foreign nationals)	21.7	3.13	.49	3.10	.49	5.16	.81	41,737
Other first-generation immigrants	41.3	2.67	.34	2.28	.33	4.27	.55	84,371
Second-generation immigrants	50.6	-.21	.81	-.04	.77	-.34	1.29	15,028
Natives	68.9	.36	.18	1.59	.18	.40	.21	455,169

*The following social background variables are included: gender, age, and “Western” (Europe, North America, Oceania) immigrant background.

Experiments, as research in general, are never completely perfect. Therefore, it is important to check if the treatments actually capture the average causal effects on turnout. In order to do that, we run linear probability models (the control group is the base category) with control variables. If we measure a causal effect, including control variables should not change the treatment effects notably compared to the model without controls. In either case, the approach will correct for possible imbalances between experimental groups. Table 6.1 shows the results from the regressions; we see that adding covariates does change the results. The ITT for immigrants is reduced somewhat; for Norwegians, it increases from 0.36 to 1.59 percentage points. This suggests that there are some imbalances in the control or experiment groups, probably with respect to age. We therefore believe that the best measures of the ITT effects of these text messages are those with control variables. In other words, the text messages did have significant effects on turnout, both among natives and in the immigrant population.

The effects among natives are more or less identical to the effects reported in Denmark targeting Danes in general (Bhatti et al. 2014b, 2015). The results among immigrants, on the other hand, are considerably larger. However, turnout in local elections is larger in Denmark than in Norway; this is especially the case for immigrants (Wüst et al. 2010). Thus, there are more immigrant voters to mobilize in Norway than in Denmark. This may explain the larger effects seen in our experiments.

The results so far indicate that the Noticeable Reminder Theory holds water in the Norwegian context. A simple reminder of the upcoming election is effective as a mobilization tool.

The effects are clearly stronger among immigrants than among Norwegians. This is suggestive of a pattern in which attentiveness to politics plays a role in mediating the effects of noticeable reminders. To explore this in greater detail, we look at immigrants' length of residence in Norway. We perform two regression models. The first model simply conditions the treatment on whether or not immigrants belong to one of the three types of voter groups, while the second model conditions treatment on the number of years each immigrant has lived in Norway. We do this because we have information about the date and year that each individual immigrant arrived in Norway; for obvious reasons, such information does not exist for second-generation immigrants who were born and raised in Norway.

Table 6.2 presents the results from the two models. In model I, the group of newly arrived immigrants is the base category. Turning to the coefficients of interest—the interactions—the table depicts that both of them are negative, indicating that receiving the SMS messages had less of an impact among both first- and second-generation immigrant voters, compared to the group of first-time voters.

Model II digs deeper into the relationship between receiving the text and the impact of the number of years immigrants have lived in Norway. It is reasonable to expect that immigrants become more like native Norwegians over time, when it comes to voting. That is, sooner or later they turn into more active voters. Hence, text messaging should be less effective among immigrants who are well established in Norwegian society.

Table 6.2 shows that receiving the SMS message is significantly negatively related to how long immigrants have lived in Norway suggesting that, at some point, receiving a text message did not help mobilize immigrants to vote compared to the control group. Again, we have calculated the predicted probabilities ranging from 0–90 years of residence in Norway with an interval of 2 years. Figure 6.1 displays the results. First, we see that the general effect of the number of years immigrants have lived in

Norway is huge. The propensity to vote increases sharply as immigrants become established in Norwegian society. The magnitude of the effect is large at the extremes. The participation rates are, as mentioned, very low among newly arrived immigrants (just over 21 percent), while the propensity to vote is over 60 percent when they have lived in Norway for 40 years. Second, the figure displays that the impact of receiving the text message decreases as immigrants become more integrated (have stayed longer) in Norway. The propensity to vote in the treatment group compared to that of the control group is significantly higher up to just above 30 years of residence in Norway. From then on, the overlapping confidence intervals show that the propensity to vote is the same in the treatment and the control groups.

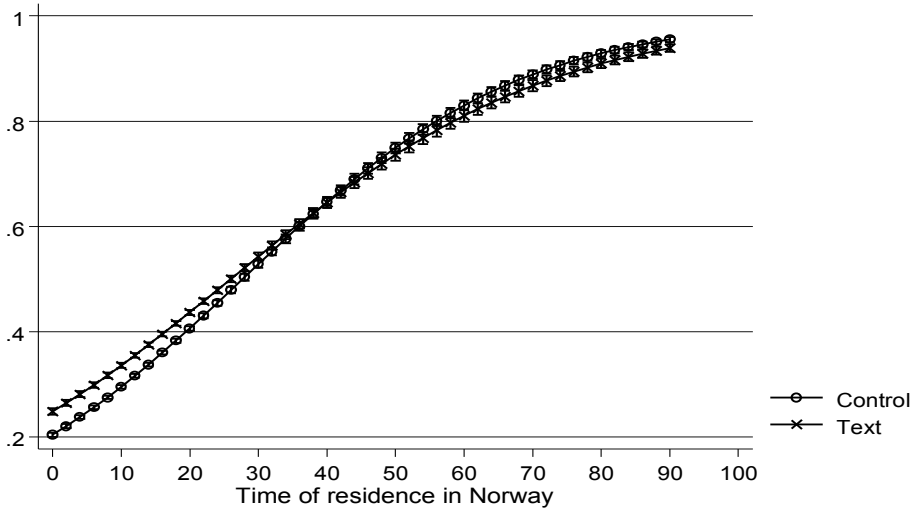
Table 6.2 Logistic regressions: Voting by type of immigrant and years lived in Norway

	Model I	Model II
Text	.175(.028)	.252(.01)
First-generation	.093(.016)	
Second-generation	1.308(.027)	
Text * First-generation	-.066(.0309)	
Text * Second-generation	-.184(.043)	
Years lived in Norway		.049(.000)
Text * Years lived in Norway		-.006(.001)
N	141,136	125,767

(Standard errors in parentheses)

Figure 6.1. Voting by years of residence in Norway

(Predicted probabilities with 95% confidence intervals - N=125,767)



Finally, we display the regression results for native Norwegians, testing whether there are differences by age. The results are shown in Table 6.3; the coefficient of interest—the interaction-term between treatment and being below 30—indicates a significant positive effect of receiving the text in the young age group. Again, we use the regression results to calculate the probabilities, and they show that the propensity to vote in the control group of youngsters below 30 was 45.33 percent, while the corresponding participation rate among youngsters receiving the text was 49.91 percent. Thus, even if the text experiment had a limited effect among Norwegians well into adulthood, it increased turnout among voters below the age of 30 by a substantial 4.58 percentage points.

Table 6.3 Logistic regressions: Voting by age

	Model I
Text	.049 (SE=.009)
Below 30 years of age	-1.164 (SE=.009)
Text * Below 30 years of age	.134 (SE=.022)
N	455,193

Does timing matter?

A final issue is to test whether the timing of the text messages influenced turnout. In the literature, the jury is still out concerning the ideal time to deliver GOTV appeals. The argument according to the primacy hypothesis is that early delivery of GOTV appeals has a memory advantage, while the argument according to the recency hypothesis is that delivery close to the election puts such appeals in the forefront of recipients' short-term memory (Panagopoulos 2011). The results of these studies are that both messages (commercial phone calls) delivered during the last week of the campaign are effective (Nickerson 2007), and also that similar calls delivered early in campaigns could be just as effective (Panagopoulos 2011). The latter study found that appeals (to high-propensity voters) delivered four weeks prior to Election Day were more effective than appeals delivered two weeks prior. Thus, it is an open question whether "messages delivered early mobilize more (or less) effectively than appeals received closer to Election Day" (Ibid: 80). Still, based on the NRT, which argues that voters fail to vote because of time constraints and lack of planning, we should expect that SMS reminders sent close to the election would be more effective.

We designed our experiment so that we could test how close to Election Day text messages can be effective. Thus, in the last week of the campaign we sent out a batch of messages every day, and on Election Day we varied the time of day the messages were received. By doing this, we were able to study the timing effects through two experiments. In the first experiment, over 8,000 messages were sent at 7 p.m. for a full seven days, for a total of 73,146 text messages (39,659 to Norwegians and 33,487 to immigrants). In the second experiment, starting at 8 a.m. on Election Day and every hour (less frequently to immigrants) until 6 p.m., we sent text messages to a total of 58,169 potential voters (35,900 Norwegians and 22,269 immigrants).

The analysis is performed using (logistic) regression specifications. The results from these regressions are used to calculate the marginal probabilities in all groups (experimental and control groups), and the probabilities are presented graphically. Figures 6.2 and 6.3 display the results among immigrants and natives.

Figure 6.2. Probability of voting by day*

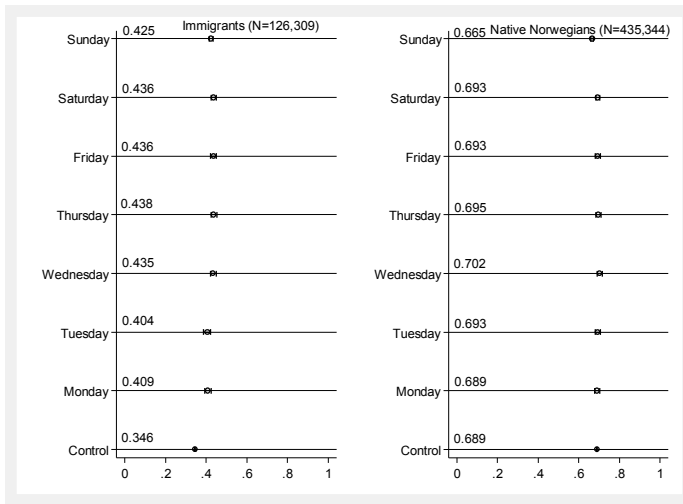
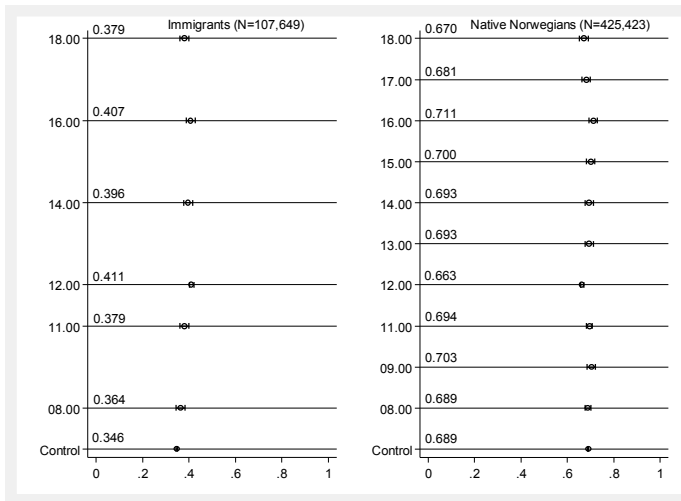


Figure 6.3. Probability of voting by the hour on Election Day*



*Probabilities with 95% confidence intervals.

Figure 6.2 shows that texting immigrants was effective in each of the seven days leading up to the election. The turnout rate is higher in all seven of those experiment groups compared to the control group. The results also indicate that texting immigrants is more effective the closer we get to Election Day. The propensity to vote is around 0.44 (or 44 percent) from Wednesday to Saturday, compared to around 0.41 (or 41 percent) for voters receiving the message six or seven days prior to the

election (Monday and Tuesday). The partly overlapping confidence intervals, however, indicate that we can't reject the null hypothesis that these differences are actually zero. Turning to native Norwegians, the results show that the propensity to vote is more or less identical, irrespective of when the text messages were received, as compared to the turnout rate in the control group. However, the results indicate that natives who received the text on Wednesday had a higher turnout rate than that of the control group (70 percent versus 68.9 percent in the control group).

The results of our by-the-hour experiment on Election Day are displayed in Figure 6.3. Starting with immigrants, we see that the propensity to vote is higher in the experimental groups than in the control group, irrespective of when they received the text. The only exception is immigrants receiving the text at 8 a.m. The results among immigrants indicate that the most effective time of the day to text voters is between 12 p.m. and 4 p.m. Again, the overlapping confidence intervals indicate that these differences may actually be indistinguishable. Turning to native Norwegians, the results once again show minor differences between the experimental groups and the control group. Comparing the control group with the experimental groups, the regressions indicate two significant differences—Norwegians who received the text at 16 a.m. turned out in higher numbers compared to the turnout rate in the control group (71.1 percent compared to 68.9 percent), while those who received the text at 6 p.m. (2–3 hours before the closing of the polling stations) turned out in lower numbers compared to the control group (67 percent compared to 68.9 in the control group).

The results from our timing experiment indicate that messages delivered close to the election do influence turnout rates, especially among immigrants. Elections in Norway are held on the second Monday in September in all municipalities, and our results indicate that messages sent before the last weekend (Wednesday to Friday) are most effective. The results from our by-the-hour experiment on Election Day also indicate that messages sent around noon are more effective than messages sent early in the morning and just hours before the polling stations close.

7 Conclusion

Few GOTV experiments have been conducted in a European context, and there is especially scarce knowledge on how specific campaigns can increase turnout among immigrants. Even if the general turnout levels in Europe—and especially in the Nordic countries—are fairly high, immigrant voters tend to lag behind the rest of the population in political participation (Wüst et al. 2010). Governments, political parties, and various interest groups in Europe may therefore be especially interested in GOTV drives that could potentially get more immigrants to the voting booth.

Our results show that GOTV mobilization drives among immigrant communities can be quite effective. The same is true for GOTV campaigns directed at another low turnout group: young voters. A summary of the results of all our experiments is shown in Table 7.1. The results are presented as ITT effects.

Table 7.1 Results of two sets of experiments in voter mobilization in the Norwegian Local Election of 2015. Intention to treat effects (percentage point increase in voter turnout in treatment group)

	Natives		Immigrants	
	All	Below 30 years of age	First-time voters	Other immigrants
SMS text messages	1.6	4.6	3.1	2.3
Letter – Privacy message			5.6	
Letter – Positive message			4.8	3.0
Letter – Negative message			7.0	3.7
Control group voter turnout	68.9	45.3	20.9	40.1

Sending an SMS text message reminder of the upcoming election is quite an effective tool in mobilizing low turnout groups in Norway. Young Norwegians and immigrants are mobilized by these messages. The text messages work, but the effects are not as strong in high turnout groups.

Text messaging is also effective in the group that has the lowest level of turnout: foreign nationals who received voting rights for the first time in 2015. However, the best tool to mobilize this group seems to be the more informative and extensive letters used in the other experiments. In addition to being a reminder of the upcoming election, the letters provided practical information about the voting process and normative arguments for why one should vote.

Looking at the rest of the immigrant population, the SMS and letter campaigns have about the same effect on turnout. When trying to mobilize immigrants who have had voting rights in Norway for at least one previous election, text messaging and letters in the mail seem to produce similar results.

There are no significant differences in the effectiveness of the three letters. This suggests that the letters serve mostly to remind people of the election (with some useful information about how to vote) and that the varying normative messages are fairly unimportant. Future research in this area should delve deeper into the effectiveness of different types of messages. Is a reminder enough to mobilize immigrants, or are there some messages that could be more effective than others? Furthermore, the literature seems to assume that GOTV contact will have a similar impact on voters regardless of who mails the message to the subjects. Our results may indicate that having government- (and election-) related institutions mailing the message matters. Do identical messages mailed by different institutions produce identical results? Finally, an especially fruitful line of inquiry would be comparative research on the effect of identical treatments in different countries. Our results indicate that a treatment that isn't very effective in the U.S. can be much more effective in other electoral settings and possibly vice-versa.

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Appendix Letters



Fylkesmannen
i Buskerud



Bruk stemmeretten den 14. september !

14. september er det valg til kommunestyre og fylkesting i Norge. Ved å delta i valget støtter du demokratiet i Norge. Hvilket politisk parti og hvilke kandidater du stemmer på er selvsagt opp til deg. Det viktige er at du deltar.

Din stemme er hemmelig!

I valglokalet går du inn i et eget avlukke der du er helt alene når du stemmer. Etter at du har besluttet deg for hvilket politisk parti du vil stemme på, legger du stemmeseddelen i en låst beholder med mange andre stemmesedler. Hverken valgmedarbeidere eller andre i valglokalet har lov til å spørre deg hvem du har stemt på. Du kan være helt trygg på at din stemme er hemmelig.

Praktisk informasjon om valget

Valgdagen er 14. september 2015, men mange kommuner holder også valg søndag 13. september. Du har tidligere fått tilsendt et valgkort i posten, med informasjon om ditt valglokale og åpringssteder på valgdagen. Husk å ta med legitimasjon (ID-kort). Legitimasjonen (pass, førerkort, bankkort) må inneholde ditt navn, fødselsdato og bilde. Har du praktiske spørsmål, kontakt kommunen du bor i.

Benytt sjansen til å bestemme hvordan politikken skal utformes i din kommune.

Bruk stemmeretten!

Med vennlig hilsen


Helen Bjørnøy

Appendix 51



**Fylkesmannen
i Buskerud**



Bruk stemmeretten den 14. september!

14. september er det valg til kommunestyre og fylkesting i Norge. Ved å delta i valget støtter du demokratiet i Norge. Hvilket politisk parti og hvilke kandidater du stemmer på er selvsagt opp til deg. Det viktige er at du deltar.

Deltakelsen blant innvandrere er økende. Bidra til å sette ny rekord!

Du trenger ikke være norsk statsborger for å delta i valget. Alle som har bodd tre år eller mer i Norge har stemmerett. Ved lokalvalget i 2011 deltok flere innvandrere enn noen gang tidligere. I flere innvandrergupper var deltagelsen godt over 50 prosent. Bidra til enda større deltakelse blant innvandrere ved å stemme ved årets valg!

Praktisk informasjon om valget

Valgdagen er 14. september 2015, men mange kommuner holder også valg søndag 13. september. Du har tidligere fått tilsendt et valgkort i posten, med informasjon om ditt valglokale og åpningstider på valgdagen. Husk å ta med legitimasjon (ID-kort). Legitimasjonen (pass, førerkort, bankkort) må inneholde ditt navn, fødselsdato og bilde. Har du praktiske spørsmål, kontakt kommunen du bor i.

Benytt sjansen til å bestemme hvordan politikken skal utformes i din kommune.

Bruk stemmeretten!

Med vennlig hilsen


Hellen Bjørnøy

52 *Getting Out the Vote*



Fylkesmannen i Buskerud

Bruk stemmeretten den 14. september !

14. september er det valg til kommunestyre og fylkesting i Norge. Ved å delta i valget støtter du demokratiet i Norge. Hvilket politisk parti og hvilke kandidater du stemmer på er selvsagt opp til deg. Det viktige er at du deltar.

Deltakelsen blant innvandrere er for lav. Bidra til å snu trenden!

Du trenger ikke være norsk statsborger for å delta i valget. Alle som har bodd tre år eller mer i Norge har stemmerett. Dessverre deltok innvandrere i langt mindre grad enn resten av befolkningen ved lokalvalget i 2011. I flere innvandrergupper var deltagelsen godt under 50 prosent. Du kan bidra til å snu trenden og øke deltagelsen blant innvandrere ved å stemme den 14. september!

Praktisk informasjon om valget

Valgdagen er 14. september 2015, men mange kommuner holder også valg søndag 13. september. Du har tidligere fått tilsendt et valgkort i posten, med informasjon om ditt valglokale og åpningstider på valgdagen. Husk å ta med legitimasjon (ID-kort). Legitimasjonen (pass, fererkort, bankkort) må inneholde ditt navn, fødselsdato og bilde. Har du praktiske spørsmål, kontakt kommunen du bor i.

Benytt sjansen til å bestemme hvordan politikken skal utformes i din kommune.

Bruk stemmeretten!

Med vennlig hilsen


Helsen Bjørnøy

Appendix 53

Institute for Social Research

2016:12

Authors	Johannes Bergh, Dag Arne Christensen and Richard E. Matland
Title	Getting Out the Vote Experiments in voter mobilization among immigrants and natives in Norway
Summary	This report describes the results of two sets of randomized Get-Out-the-Vote experiments, testing the effectiveness of mail mobilization appeals directed at immigrants as well as SMS text messages directed at both immigrants and native voters in Norway. The results show that GOTV mobilization drives among immigrant communities can be quite effective. The same is true for GOTV campaigns directed at another low turnout group: young voters. These results indicate that the mobilization tools used in the experiments could be employed to raise turnout in future Norwegian elections.
Index terms	Voter turnout, experiments, Get-Out-the-Vote, immigrants
Forfattere	Johannes Bergh, Dag Arne Christensen og Richard E. Matland
Tittel	Velgermobilisering Eksperimenter med tiltak for å øke valgdeltakelsen ved lokalvalget i 2015
Sammendrag	I forbindelse med lokalvalget i 2015 ble det gjennomført to grupper av eksperimenter med tiltak for å øke valgdeltakelsen. Formålet med eksperimentene var å undersøke hvilke tiltak som vil være mest effektive for å mobilisere folk til å stemme. I den første gruppen av eksperimenter ble det sendt brev til stemmeberettigede innvandrere i forkant av valget. Det andre eksperimentet var en kampanje med bruk av tekstmeldinger (SMS) som rettet seg mot alle velgere; både de med og uten innvandrerbakgrunn. Alle tre brevene og SMSene viste seg å ha større effekt på valgdeltakelsen enn det var grunn til å forvente. Effekten var størst i to grupper av velgere som kjennetegnes av å ha lav valgdeltakelse: unge velgere og velgere med innvandrerbakgrunn. Resultatene tyder på at de tiltakene som her er prøvd ut, vil være effektive virkemidler for å øke valgdeltakelsen ved fremtidige norske valg
Emneord	Valgdeltakelse, eksperimenter, innvandrere, politisk mobilisering

