



Door-to-door canvassing in the European elections: Evidence from a Swedish field experiment[☆]



Pär Nyman¹

Department of Government, Uppsala University, Box 514, 751 20 Uppsala, Sweden

ARTICLE INFO

Article history:

Received 20 July 2016

Received in revised form

30 November 2016

Accepted 7 December 2016

Available online 13 December 2016

ABSTRACT

In this paper I report the results from a door-to-door canvassing experiment conducted in Sweden during the 2014 European elections. The canvassing was performed by members of the Social Democratic Party and the experiment closely resembles the partisan nature of most mobilizing campaigns in Europe. The paper is one of the first to provide causal evidence for the mobilizing effectiveness of canvassing outside the United States. Living in a household that was visited by canvassers increases the probability of voting by 3.6 percentage points. This effect is entirely driven by estimates twice as large for occasional and first-time voters. Compared to previous research, the high compliance rate gives precisely estimated effects that are closer to average treatment effects.

© 2016 Elsevier Ltd. All rights reserved.

1. Introduction

The rich world has long been plagued by a steady decline in voter turnout. This trend concerns the very core of political science, because lower turnout worsens the existing inequalities in political participation and – ultimately – threatens the legitimacy of our democratic institutions (Lijphart, 1997).

In recent research as well as contemporary political campaigns, different kinds of “get out the vote” efforts have been suggested as the cure to this problem. Following the seminal works of Gerber and Green (2012), a fast-growing literature has emerged that uses randomized experiments to test the mobilizing effectiveness of different campaign tactics. Door-to-door canvassing is generally proposed as the most cost-effective of these methods, trumping both phone calls and direct mail.

In addition to optimizing mobilization campaigns, such field experiments are also of scholarly interest because they deepen our knowledge about the reasons why people vote. Previous studies have provided insights into why couples living together share similar voting behaviors (Nickerson, 2008), why the level of

political participation differs between immigrants and native-born (Pons and Liegey, 2013) and whether our decision to vote is most affected by intrinsic satisfaction from norm-compliance or from extrinsic incentives and concern about what other people think of us (Gerber et al., 2008).

While field experiments are blessed with strong internal validity, they are also characterized by limited external validity. It is therefore unfortunate that with few exceptions, almost every experiment on canvassing effects has been conducted in the United States. While door-to-door canvassing is becoming increasingly popular in the rest of the world, many have questioned its effectiveness outside the US.

In this paper I report the results from a field experiment carried out in the Swedish county of Södermanland, where 11 640 citizens were randomly assigned to either the treatment or the control group. The experiment was conducted in cooperation with the Social Democratic Party, as a part of their campaign during the 2014 European elections. Approximately 60 per cent of the households in the treatment group were visited by party members, who asked them to vote in the election and suggested that they should vote for the Social Democrats.²

[☆] I am grateful to the two anonymous reviewers, Rafael Ahlskog, Elin Bjarnegård and John Ternovski for their helpful comments and to Peter Gerlach, Carl Kangas, Ingela Nilsson and Fredrik Olovsson at the Social Democratic party for their co-operation. I would also like to direct a special thanks to Nicklas Vinthagen for excellent research assistance and to all the canvassers for their hard work.

E-mail address: par.nyman@statsvet.uu.se.

¹ Contact details and replication files can be found at www.parnyman.com.

² Note that everyone living in a household where at least one person spoke to the canvassers is counted as contacted or treated. Approximately 40 per cent spoke directly to the canvassers and 20 per cent lived in a household where someone else participated in the conversation. Because randomization was conducted at the household level, the effect of living in a visited household is the only effect which can be estimated without making additional assumptions. In the supplementary material available online, I show the results from models where I try to separate the direct treatment from the spill-over effect.

This study differs from previous experiments in two important ways.

The first distinguishing feature of this study is the high rate of compliance. A contact rate of 60 per cent is twice as high as in most comparable studies and the number of treated households in the control group is negligible. There are two reasons why compliance matters. First, it is only possible to study how canvassing affects those voters that are actually contacted by the canvassers, but there are many occasions when we might be more interested in how the average voter would respond. The higher our contact rate, the less the effect on the treated differs from what the average treatment effect would have been. Second, the high rate of compliance in this study, in combination with a relatively large sample size, yields a precision in the estimated effects that few other field experiments can match.

The second distinguishing feature is that the European elections offer a setting very different from those of previous studies. With over 400 million eligible voters they are among the largest elections in the world. As such, it is unfortunate that we know so little about the effectiveness of canvassing in Europe (Nielsen, 2014). There are several reasons why we should expect the results of door-to-door canvassing to differ from the American experience. First, canvassing in Europe is typically performed by members of the political parties, but most American experiments have been conducted by non-partisan canvassers. Evidence from mailing campaigns suggest that non-partisan messages are more effective at mobilizing voters than mails that encourage the recipient to vote for a given issue or candidate (Green et al., 2013).

Second, turnout in most of Europe – and especially in Sweden – is much higher than in the United States. However, the elections for the European Parliament can be described as second-order elections, where turnout is lower and less is at stake than in the national elections. How do the relatively strong national voting tendencies, in combination with a low-salience election, affect the mobilizing effect of canvassing? If the voting habits mean that the norms about voting are more pronounced, I would expect the social pressure induced by mobilizing campaigns to be stronger, and thereby the effects of door-to-door canvassing to be larger. Similarly, the lower turnout in the elections to the European parliament means that there are more voters who can be mobilized.

Third, the European political context differs from the American in many other ways. Most electoral systems in Europe are based on proportional representation, but every successful canvassing experiment has been conducted in countries with majoritarian systems. Moreover, most European countries do not require the citizens to register before voting. As a result, it might be more difficult for parties to target likely voters and especially voters affiliated with their own party. On the other hand, there are no unregistered voters that by definition are out of reach for the canvassers. All these arguments are developed in more detail in the next section.

Compared to previous research in similar settings, the results of this experiment strengthen the case for door-to-door canvassing in Europe. The electoral rolls show that being visited by a canvasser increases the probability of voting with 3.6 percentage points. However, the effect differs substantially between subgroups. No effect was found among voters with a very high or very low propensity to vote. For citizens who voted in the last national election, but refrained from voting in the previous European election, the estimated effect was 6.0 percentage points. I also find larger effects for young people, those living in single-family dwellings and for people in large households, compared to old people, people living in multiple-family dwellings and single-person households.

Because the study is conducted in Sweden, it also fills an important gap in the comparative literature on electoral systems. It is widely acknowledged that electoral systems based on proportional representation increase voter turnout. One of the most

commonly proposed mechanisms for this regularity is that the voter mobilization achieved by parties and interest organisations is stronger in countries with PR systems (Cox, 2015). But to the best of my knowledge, this study is the first well-conducted canvassing experiment that takes place in a country with proportional representation.

The remainder of the paper proceeds as follows. In the following section I discuss the setting of this experiment in relation to previous research. I then proceed by describing the data analyzed in this paper before I eventually present the results. The last section concludes.

2. The context and previous research

This study takes place in Sweden during the 2014 elections to the European Parliament. The European elections are among the largest in the world, with over 400 million eligible voters. However, the turnout has been falling for almost every election, from 62 per cent in 1979 to 43 per cent in 2014,³ and the trend is likely to continue (Bhatti and Hansen, 2012). Despite its high turnout in national elections, also Sweden has difficulties mobilizing its voters in European elections, and when 51 per cent of its citizens voted in 2014 it was the highest turnout since the country joined the European union in 1995.

The experiment was conducted in 17 electoral districts, located in five municipalities in the county of Södermanland. The districts are characterized by a high share of Social Democratic voters and a low turnout. In the 2014 election for the European Parliament, the support for the Social Democrats amounted to 40.3 per cent of the votes in these districts, compared to 24.2 per cent in the rest of Sweden. The turnout of 41.4 per cent of the eligible voters was almost ten percentage points below the national average of 51.1 per cent.

The experiment was designed with the ambition to resemble ordinary partisan canvassing as closely as possible. The 155 canvassers were members of the Social Democratic Party and they used the same script as the party used in the rest of their national campaign. The age of the canvassers ranged from 16 to 80 with an average of 52, which is very different from the college students often employed in US experiments. Approximately 20 per cent of the canvassers reported that they hold a political position which makes them known to large parts of the electorate.

The canvassing took place during the three weeks that preceded the election and at least a few households were visited every day during this period. On average, there were 11 days between the visit and the election. Most of the canvassing (70 per cent of the successful contact attempts) was done in pairs. The canvassers knew that they participated in a scientific study, but the citizens they visited were not informed about the experiment.

Below I will go through the arguments for why we should expect different effects of this experiment compared to what previous research has found. First I argue why the mobilizing effect can differ between electoral systems and what consequences it may have. Then I discuss the aspect of partisan compared to non-partisan canvassing. Third, I provide three reasons for how strong voting habits, combined with the second-order nature of the European elections, can influence the effectiveness of mobilizing campaigns. Lastly I go through the empirical results in previous research and outline my expectations of this experiment.

2.1. Canvassing and proportional representation

That voter turnout is higher in countries with proportional

³ A large part of this decline is caused by the enlargement of the European Union.

representation than in countries with single-member districts is one of the most robust findings in the comparative literature on electoral systems (Lijphart, 1999), but little is known about the mechanisms behind this empirical regularity (Blais and Aarts, 2006). A commonly proposed explanation is that the incentives for mobilization efforts are stronger in PR systems (Rainey, 2015).

This discussion has been focused on how a proportional distribution of seats affects the incentives for mobilizing additional votes (Cox, 1999, 2015; Herrera et al., 2014; Karp and Banducci, 2008; Karp et al., 2003, 2008). However, the incentives for mobilization efforts are also dependent on how successful mobilization campaigns are in increasing voter turnout. Unfortunately, the literature is surprisingly scarce in this aspect.

There are theoretical arguments for both higher and lower effectiveness of mobilization campaigns in countries with proportional representation. On the one hand, Cox (1999) expects strong ties between parties and social groups to increase the effectiveness of mobilizing efforts. Because partisan links tend to be stronger in PR systems, Cox argues that mobilization efforts should translate into more votes in countries with proportional representation. Karp et al. (2003, 2008) hypothesize that mobilization will be more effective under proportional representation, because the absence of safe seats and "wasted votes" makes it easier for parties to persuade potential voters.

On the other hand, there are also arguments why mobilization should be less effective in PR systems. First, politics in majoritarian systems tend to be more focused on the candidates, with more personal campaigns, more contact between constituents and representatives and weaker party affiliation. It is probably easier to know what to expect from a party than from a specific candidate. Information about a candidate, and trust toward the same, can be obtained through contact with the candidate's campaign. Therefore, mobilization campaigns should be more effective when the need for trust and information is larger. In line with this argument, Górecki and Marsh (2012) find that the effect of canvassing is larger when the geographic distance between the home of the voter and the candidate is large. Second, Cox (1999, 2015) and Powell (1989) argue that mobilization in PR systems has relied on subcontracting to affiliated interest groups. The reason is that the larger number of parties typically found in PR systems give rise to more ideological parties with closer ties to labour unions, churches and other interest organisations. To explain the lower degree of partisan mobilization campaigns in PR systems, this argument implicitly assumes that the mobilization efforts conducted by affiliated groups have reduced the effectiveness of further partisan campaigns. Third, door-to-door canvassing and other forms of contacting are more common in countries with majoritarian electoral systems than in countries with proportional representation. Even in safe districts, a larger share of citizens report that they have been contacted than in countries with PR systems (Karp et al., 2008). This pattern is recurring in the elections to the European Parliament, where canvassing is most prominent in Ireland and the UK (Gagatek, 2010). If the effectiveness of different campaign methods influence how common they are, these differences could indicate that canvassing is more effective in majoritarian electoral systems. However, if the frequency of canvassing is determined by other factors, the relationship between frequency and effectiveness would instead depend on whether there are decreasing or increasing returns to scale, for which the evidence is lacking.

Unfortunately, we know very little about the effectiveness of door-to-door canvassing in countries with proportional representation. In an observational study on survey data, (Karp et al., 2008) find no clear patterns in the effectiveness of canvassing, but if anything, the results suggest that door-to-door canvassing might be more effective in PR systems.⁴ However, such studies are known for biased results because campaigns are more likely to target voters than non-voters and because contact with the campaign increases the propensity of non-voters to falsely report that they voted. Reliable estimation of canvassing effects therefore requires both random assignment and individual-level voter turnout records. To the best of my knowledge, the only canvassing experiments conducted in a PR system are the ones reported in Bhatti et al. (2014b). While they do not find any statistically significant effects, the small sample sizes and the unknown compliance rates makes it difficult to interpret their results. Consequently, this is the first study which convincingly identifies the effect of door-to-door canvassing in a country with proportional representation.

2.2. Partisan canvassing

Until very recently, the only solid evidence for the mobilizing effects of door-to-door canvassing came from non-partisan experiments where college students and community organisations asked voters to do their civic duty. While there have been a few partisan experiments conducted during the last few years, non-partisan canvassing still amounts for most of the published studies.

There are many reasons why we would expect the effectiveness to differ between partisan and non-partisan campaigns. First, non-partisan canvassers typically deliver a message that is purely about voting, while partisan canvassers also try to persuade the subject to vote for a certain candidate or party. Which kind of message that is most effective for mobilizing voters is essentially an open question. On the one hand, partisan messages can provide additional reasons for voting, like affecting policy or expressing support for a certain candidate. On the other hand, voters might respond negatively to a political message of which they disapprove. The latest meta-study of experiments using direct mailing concludes that only mailings with non-partisan messages boost turnout (Green et al., 2013), but it remains to be seen whether the same holds for door-to-door canvassing. Second, it is possible that partisan campaigns target different groups and have a more multifaceted purpose with their campaigns. For example, they might prioritize areas with many swing voters, which might not be the areas best suited for mobilization efforts.

It has proved difficult to conduct experiments with partisan canvassers and most attempts have been crippled by low contact rates or substantial contamination of the control group. One possible explanation could be that it is easier to supervise hired canvassers and university volunteers than it is to control party activists. While the latter are certainly motivated to contact as many voters as possible, they might be less concerned about whether the canvassed voters were assigned to the treatment group. The survey by Green and Gerber (2008) includes three partisan experiments with published results, in which the contact rate ranges from 8 (Nickerson et al., 2006) to 13 per cent (Nickerson, 2006) of the target population.⁵ On the other hand, through cooperation with party organisations, partisan campaigns also have the potential of reaching a much larger number of voters

⁴ They find statistically significant effects in a larger share of the countries with PR systems. Moreover, they only found a significant effect in New Zealand after the switch to proportional representation. Comparing the size of the effects is difficult because the baseline voting propensity differs between countries.

⁵ The third study is made by Arceneaux (2007) and includes two subsamples with a contact rate of 7 (canvassing conducted by candidates) and 11 per cent (partisan canvassing by volunteers). In the unpublished studies the contact rates can be even lower.

than what can be achieved with non-partisan canvassing. It is therefore not surprising that also some of the most high-powered experiments are conducted by partisan canvassers, like those analyzed by Pons (2014) and Pons and Liegey (2013). However, they find very limited mobilizing effects of partisan door-to-door canvassing, which only underscores the need for more experiments in a partisan context.

2.3. A second-order election in a country with high turnout

Following the seminal works of Reif and Schmitt (1980), it has become standard to describe elections for the European Parliament as second-order national contests (Hix and Marsh, 2007; Marsh, 1998). Among other things, the concept implies that voter turnout and intensity of election campaigns are lower than in national elections where executive power is at stake. Most countries in Europe – and Sweden in particular – have a higher turnout than the United States in their national elections. So while the elections for the European Parliament is the archetype of a second-order election, it takes place in an electorate with a strong habit of voting. Below I discuss how salience and voting habits might condition the effectiveness of door-to-door canvassing and other mobilizing campaigns.

First, in elections with very high turnout, the mobilizing effect is probably smaller. If almost everyone votes also in the absence of a treatment, there are much fewer citizens we have a theoretical chance of mobilizing. More generally, we can assume that the effect is largest for – or even limited to – people who teeter back and forth between voting and not voting. If the distribution of the voting propensity is symmetrical and unimodal, we would expect the share of people with a voting propensity close to 50 per cent to be the largest when also the turnout is close to 50 per cent.⁶ If that assumption is correct, the European elections are suitable for mobilizing campaigns.

Second, the high turnout in most national elections in Europe could indicate that there are strong norms about voting. Many scholars have argued that the reason why “get out the vote” campaigns work is that they induce social pressure and increase the cost for not complying with the norms. The effectiveness of canvassing is therefore dependent on the strength of these social norms. If the norms about voting are stronger in Europe, we would also expect larger mobilizing effects.

Third, some of the largest effects in previous research have been observed in non-competitive elections for school boards and city councils where the turnout has been less than 20 per cent. In such elections, direct contact with the campaign can sometimes be the only way of learning about the candidates, and the canvassing can then be something qualitatively different from the usual mobilizing campaigns. Such low-salience elections are uncommon in Europe and we should think twice before applying the results from those studies in a European context.

2.4. The size of the effects

Some of the most cited field experiments have estimated the effect of door-to-door canvassing to an increase in voter turnout by close to 10 percentage points (Gerber and Green, 2000; Green et al., 2003; Nickerson, 2008). However, they only represent a small share

of all canvassing experiments that have been conducted. A recent review by Green et al. (2013) includes 71 canvassing experiments. The authors calculated the average effect in those campaigns, weighted by precision, to be 2.5 percentage points. Because of publication bias and the small samples used in most experiments, this number is probably more accurate than the large effects found in the most cited articles. On the other hand, it is probably an underestimation of the true effects, because most things that can go wrong in an experiment would lead to downward bias in the estimated effect. Besides, many of the experiments study elections with so low turnout that an increase with one or two percentage points would count as quite substantial.

The experience from the few experiments that have been conducted in Europe is less inspiring. No mobilizing effect at all was found for the large-scale canvassing in the Hollande presidential campaign in France in 2012 (Pons, 2014). And during the French regional elections in 2010, the effects were limited to certain subgroups (Pons and Liegey, 2013). None of the experiments conducted in Denmark have found any statistically significant effects (Bhatti et al., 2014a). The only promising experiment carried out in Europe was conducted in England. In their study, John and Brannan (2008) reported an estimated effect of 6.7 percentage points, which also was statistically significant (although only with one-sided tests and unadjusted standard errors).

There are several reasons to be more optimistic about the expected effects than what previous studies would give reason to be. First, most of the non-existing effects in France and Denmark can be explained by the high turnout in the elections during which the canvassing was conducted. The level of turnout in the Swedish elections for the European Parliament are better suited for a mobilization campaign. Second, the same studies also report uncertainty about compliance. If the contact rate is lower than estimated, or canvassers decide to also do some campaigning in the control group, the true effects will be underestimated. When planning our experiment, we knew that we could not match the sheer volume of contacts reported in Pons (2014), and instead we took every effort to ensure a high rate of compliance.

In the initial planning phase, we wanted to limit the study only to those who voted in the national elections 2010 but stayed at home during the European elections of 2009. This was the group where we expected the largest effect. However, we were afraid that such a narrow sample would be too small, and widened it to all voters where we had information about whether they voted in the previous elections. As a result, we could now test whether our assumption about the heterogeneous effect was correct or not.

3. Data

In Swedish elections, the constituencies are divided into electoral districts with one polling station per district. The size of the district is usually between 1000 and 2000 citizens who are entitled to vote. A few weeks after each election, the electoral rolls are accessible for the public as printed books.

We have digitalized the electoral rolls for 17 electoral districts and three different elections: the 2009 election for the European Parliament, the 2010 national election for the Swedish parliament and – after the field work was finished – the 2014 election for the European Parliament (the outcome variable). The rolls include the person's name, his or her personal identification number and whether he or she voted in the election. To these records I have added street addresses from the preliminary electoral rolls for the 2014 election. The Swedish register of addresses includes apartment numbers, which makes it easy to identify which individuals live in the same household.

In total, 31 per cent of the citizens eligible to vote were excluded

⁶ Note that it is the voting propensity among the treated, and not in the electorate in general, that matters for the mobilizing effect of the campaign. Citizens with a high probability of being contacted by the canvassers tend to have a higher voting propensity than those who cannot be reached. On the other hand, mobilizing campaigns are often directed to areas with a lower turnout than average.

Table 1
Balance of the treatment and control groups.

	Treatment	Control	P-value
Voted 2009	41.4	40.8	0.554
Voted 2010	79.0	79.5	0.539
Age	56.7	56.7	0.948
Household size	1.9	1.9	0.493
Women	52.0	53.0	0.316
Voting propensity	45.4	45.1	0.517

Turnout in previous elections, the share of women and the voting propensity are all displayed as percentages. Voting propensity is calculated as predicted values from regressing turnout 2014 on a large set of predictors.

from the sampling frame, with a remaining 11 640 individuals distributed over 7579 households. Most of them were excluded because they lived somewhere else during the previous elections and I therefore lack information about whether they voted in 2009 and 2010. The households were then randomly allocated to equally sized treatment and control groups, using strata determined by electoral district and household size. That the randomization was successful is supported by the balance between the treatment and the control group that is shown in Table 1. The table presents summary statistics on the treatment and control group, together with the p-values from a *t*-test of the means. None of the differences are close to being statistically significant.

The personal identification numbers include information about a person's sex and age. I have also used them to separate the foreign-born from the Swedish-born. From the street addresses in the electoral rolls, I have been able to identify households, calculate the size of each household (this measure only pertains to people eligible to vote) and separate apartments from single-family dwellings. For each contact attempt, the canvassers reported the date and the outcome. If a contact attempt was successful, the canvassers specified who they had been talking to and which canvassers had participated. For each canvasser I have information about sex, age and whether they consider themselves to be "locally known". That makes it possible to examine possible identification effects, for which there is some suggestive evidence (Bennion, 2005).⁷

I rely on these contact forms to classify subjects as treated or not treated. Because spill-over effects within households can be large (Nickerson, 2008), I define treated individuals as everyone living in a household where a door-to-door canvasser has reported a successful contact attempt. It is thus only for single-person households that the treatment effect can be interpreted as the effect of direct contact with a canvasser. The procedures for excluding citizens from the sampling frame, assigning households to the treatment group and creating all the variables mentioned above are described in detail in the supplemental material on the author's homepage.

3.1. Identifying assumptions

All results in this paper rest on the assumption that the allocation to the treatment and the control group is random. Because the allocation process was entirely under my control, randomization was easily fulfilled. However, I also assume *one-sided noncompliance*, *non-interference* and *excludability*. If any of those assumptions are broken, the estimates will be biased. Below I describe those assumptions in more detail.

Throughout the paper I assume one-sided noncompliance,

which means that no subjects assigned to the control group have been visited by canvassers. If this assumption does not hold, all estimated effects are biased downwards. In a telephone survey conducted after the election, only 6 percent of the citizens in the control group responded that they had been visited by party workers, compared to 79 per cent in the treatment group.⁸ The survey not only shows that the experiment was conducted according to protocol, but also that control group contamination caused by door-to-door canvassers from other parties is negligible.⁹ The reason why the sample of survey respondents reported a higher contact rate (79 per cent) than the canvassers (60 per cent) is probably that those who were reached by the survey company also had a larger probability of being reached by the canvassers.

I also assume non-interference and excludability. Non-interference means that every subject's probability of being treated, and every subject's propensity to vote, is unaffected by other subjects' treatment assignment. Excludability means that the outcome is affected by the actual treatment and not by the treatment assignment. The largest threat to both these assumptions is probably that canvassed citizens might inform their friends about the upcoming election and even tell them to vote. Unless non-treated subjects in the treatment group are more likely than subjects in the control group to interact with treated individuals, all estimated effects are biased downwards if the assumptions about non-interference and excludability do not hold. Several measures have been taken to reduce the extent of excludability violations. Because people are likely to interact with other people in the same household, everyone in a household is classified as treated if at least one of the household members has talked to a canvasser. To leave non-contacted households without treatment, canvassers were instructed to not leave any leaflets or other information after unsuccessful contact attempts.

This study has a much higher contact rate than most similar studies, which affects the estimated effects in several ways that are worth pointing out.¹⁰ First, the large share of compliers substantially increases statistical power. While the loss in precision that results from low compliance can be compensated for through a large sample size, there are not many canvassing experiments that match the precision of this study.¹¹ Second, the high contact rate probably means that the estimated average treatment effect among compliers is closer to what the average treatment effect would be if everyone would have been treated. This is important, because most theories of political behaviour concern all citizens eligible to vote and not only those with a high probability of being canvassed. The data collected in this experiment also confirms that people with a

⁸ The survey included a total of 1000 randomly selected respondents, divided equally between the treatment and the control group. Approximately 75 per cent of the respondents claimed that they voted in the election (79 per cent in the treatment group and 70 per cent in the control group). The actual turnout among subjects in this experiment was 46.1 per cent, which shows why we cannot rely on self-reported turnout in this kind of study.

⁹ While it is well known that voters have poor memories (Michelson, 2014; Vavreck, 2007), an inability to remember cannot explain why so few in the control group state that they were visited by canvassers, at the same time as voters in the treatment group correctly recall having been canvassed. Because the assignment to the treatment group was random, systematic differences in political memory between treatment and controls is ruled out by design.

¹⁰ Most successful canvassing experiments have contact rates around 30 per cent. Melissa Michelson's study of a local school board election (Michelson, 2003) is the only similar study I know with a higher contact rate than mine, but the sample size of her study is too small to yield effects that are significant at conventional levels.

¹¹ To my knowledge, the only experiments with better precision are those conducted on the SCOPE campaign in the 2008 presidential election (Bedolla and Michelson, 2012) and the French Socialist Party's campaigns in the 2010 regional elections (Pons and Liegey, 2013) as well as in the 2012 presidential elections (Pons, 2014).

⁷ While Bennion (2005) finds that student canvassers only affected younger voters, we do not know if this was an effect of similar age or if we would have found the same impact if the canvassers were older.

high contact probability are very different from those that are harder to reach.¹² Third, whether the effect in high contact rate experiments provides a better estimate or not for what to expect in other canvassing campaigns is an open question and depends on the reasons why some people are contacted while others are not. On the one hand, one of the reasons for the high contact rate is that the canvassing has been conducted during different times and different days. That should make the results more generalizable than if the canvassing had only been conducted during either office hours or weekends. On the other hand, if the high contact rate primarily reflects a higher share of those subjects that are hard to reach, owing to the dedicated canvassers who visited the same address up to three times, this experiment might give too much weight to groups with a low probability of being contacted.

4. Results

If the assumptions discussed above hold, comparing the turnout between these groups is enough to identify the causal effect of canvassing. To improve precision and adjust for intra-household correlations, I employ an instrumental variable regression framework. In the next section I describe this approach in more detail and present the results for the full sample. I then proceed by examining how the effect differs depending on voting propensity, household size and other characteristics of the citizens.

4.1. Main effects

The most important finding of this paper is shown in Table 2. From left to right, the table displays the joint number of observations in the treatment and control group, the share of households in the treatment group that were reached by the canvassers and the voter turnout in first the control group and then in the treatment group. The last four columns show the estimated causal effects of door-to-door canvassing, first estimated in a bivariate model and then in a model which includes a set of covariates. The two effects can be interpreted as the expected change in the probability of voting that results from living in a household that was visited by a door-to-door canvasser.

Because the assignment to the treatment and control group is random, the difference between the two groups can be interpreted as the causal effect of being assigned to the treatment group, given that the assumptions about one-sided noncompliance and non-interference hold. This is usually called an intent-to-treat (ITT) effect and it is not shown in the table. To estimate the effect of actually being treated – that is, living in a household that was visited by a canvasser – I could divide the ITT with the treatment share in the subgroup. Compared to the ITT estimates, these estimates only add the assumption of excludability.¹³

The difference in turnout between the treatment group and the control group was in this experiment 2.32 percentage points (47.21 – 44.89). If I would divide 2.32 with the treatment share of 0.595 I would get the treatment effect of 3.9 percentage points that

Table 2
Full sample results.

Subgroup	Obs	Visited	Turnout		Bivariate		Covariates	
			Control	Treat	Effect	SE	Effect	SE
Full sample	11,475	59.5	44.9	47.2	3.9**	1.9	3.6**	1.6

Robust standard errors clustered on households. All tests are two-tailed.

* $p < 0.10$, ** $p < 0.01$, *** $p < 0.001$.

was reported as the bivariate effect. Because people living in the same household are not independent from each other, I have instead used an instrumental variable regression framework, which allows me to calculate robust standard errors that are clustered on the household level. All models are estimated using 2SLS.

As illustrated by equation (1), I regress the voting decision on the treatment outcome (living in a visited household) and instrument the treatment with the allocation to the treatment group. In the bivariate case, the two procedures yield identical estimates of the effect, but not correcting for the strong within-household correlations would deflate the standard errors substantially.

$$\begin{aligned} \widehat{Vote} &= a_1 + b_1 \widehat{Visited} + \mathbf{b}\chi + \varepsilon \\ \widehat{Visited} &= a_1 + b_1 \text{Assignment} \end{aligned} \quad (1)$$

The regression framework makes it easy to add more explanatory variables to the model, symbolized in the equation through the vector χ . If the variables are correlated with the outcome variable, and are unaffected by the treatment, they increase the precision of the estimates without inducing any measurable bias. The covariates I have included are earlier voting behaviour (two dummies for voting in the 2009 and the 2010 elections) and a dummy for whether the person is a first time voter), age, age squared and dummy variables for the 17 electoral districts.

The seventh column of Table 2 shows the estimated treatment effect from the multivariate model. When the results differ from the bivariate estimates, I am inclined to believe more in the regressions with covariates, but if the randomization was conducted properly the differences should be small. For the full sample, the estimated effect of living in a visited household is 3.6 percentage points and it is significant at the 95 per cent level. Another way of expressing this result is that for every 28 treated citizens, one more goes to the ballots. Because there are 1.9 persons living in each household on average, this effect amounts to one more vote for every 15 canvassed doors.

4.2. Subgroups

In addition to estimating average effects, it is important to analyse how the effects differ between subgroups. Such knowledge is valuable when planning a canvassing campaign, because it allows the canvassers to focus their efforts where they count. Such knowledge is also of scholarly interest because heterogeneous effects between groups of citizens can tell us something about the mechanisms behind successful canvassing and why some voters are more easily mobilized than others.

Before I proceed to how the effects differ between subgroups, there are three things I want to emphasize about how we should interpret the effects. First, among the people living in a contacted household, the share that actually talked to a canvasser can differ across subgroups. If this share is larger in one subgroup than in another, and the direct effect of contact with a canvasser is larger than the spill-over effect on the other members of the household, this will then appear in the table as a heterogeneous effect of canvassing. Second, belonging to a subgroup might be correlated

¹² Those living in households where the first contact attempt was successful had a turnout of 46 per cent in the 2009 election and 54 per cent 2014. By contrast, among citizens in households where the canvassers made at least three contact attempts without reaching anyone in the household, the average turnout was 30 and 32 per cent in the 2009 and 2014 elections, respectively.

¹³ These are sometimes called LATE (local average treatment effect, see Angrist et al. (1996) or CACE (complier average causal effects, see Gerber and Green, 2012) effects. Note that these estimates only tell how voters that live in contacted household are affected by canvassing. We cannot be sure that those in the treatment group who were not contacted would have been affected in the same way.

with also belonging to another subgroup or with unobserved covariates that affect the effectiveness of canvassing. For example, the differences between people living in single-family and multiple-family dwellings might reflect differences in wealth or income rather than housing per se. Third, the risk of falsely rejecting a true null hypothesis increases with the number of subgroups. When drawing conclusions, we must therefore assess the statistical results in comparison with our theoretical priors and with what has been found in previous research.

The results are presented in Table 3. It is clear that there are large differences between subgroups, not only in the level of turnout but also in how the turnout was affected by the treatment. Starting with the voting history, I can only find an effect among those who voted in the 2010 elections to the national parliament but refrained from voting in the 2009 elections to the European Parliament. The estimated effect in this group is 6.0 percentage points higher turnout for those living in a household that was visited by a canvasser. I had expected the effect in this group to be larger than for the others, because they are obviously prepared to vote when the circumstances are right but nonetheless refrained from voting in the last election. The idea that mobilization efforts are most effective among occasional voters is also supported by previous research (Arceneaux and Nickerson 2009; Niven 2001). Still, the difference between the groups is larger than I had expected. An equally large effect is found among first-time voters, but they are so few that the estimate is imprecise and not statistically significant.

Many have emphasized the social nature of voting and how social pressure is the mechanism behind successful mobilization (Ali and Lin, 2013; Gerber et al., 2008; Shachar and Nalebuff, 1999; Uhlaner, 1989). Besides, both simulations (Fowler, 2005) and experimental evidence (Bhatti et al., 2014a; Bond et al., 2012) suggest that effects of secondary mobilization are much larger than primary effects. I was still surprised to see larger effects for people living in houses and large households, compared to the non-existing effects for those living in apartments or single-person households.¹⁴ If anything, I would have expected the opposite. A large share of people living in large households did not talk to the canvasser themselves, but were only affected by so called spill-over effects.

Previous research offers little guidance about how the size of the household affects the effectiveness of canvassing. While Bhatti et al. (2014a) analyse the mobilizing effects of short text messages and find substantially larger *spill-over* effects in large households, Nickerson and Rogers (2010) found that phone calls were more effective in small households. Gerber et al. (2008) did not find any effect at all of household size. As one could suspect, there is a positive correlation between living in a house and living in a large household, but both categories have a positive effect when controlling for the other. In fact, both the effects are driven by a very large estimated effect for large households (>2 members) living in a house.

One interpretation is that in larger households, other household members might observe or even participate in the discussion, thereby increasing the social pressure of voting. There are indications of such a mechanism in the data. Controlling for household size, the turnout was 7 percentage points higher among citizens that participated in the conversation together with at least one other household member, compared to citizens that

Table 3

Results in different subgroups.

Subgroup	Obs	Visited	Turnout		Bivariate		Covariates	
			Control	Treat	Effect	SE	Effect	SE
Full sample	11475	59.5	44.9	47.2	3.9**	1.9	3.6**	1.6
<i>Voting history</i>								
Both elections	4659	63.7	75.3	75.9	1.0	2.3	0.8	2.3
Only 2010	4337	57.7	27.0	30.7	6.3**	2.8	6.0**	2.8
None of them	1408	48.6	7.5	6.8	−1.5	3.0	−1.4	3.0
First-time voters	760	60.6	36.2	40.2	6.5	6.2	6.4	6.0
<i>Dwelling</i>								
Single-family	4958	72.4	51.5	55.8	6.0**	2.6	5.1**	2.2
Multiple-family	6517	49.6	39.9	40.6	1.5	2.8	1.7	2.4
<i>Household size</i>								
Single person	4241	46.9	40.2	40.0	−0.5	3.2	−0.3	2.7
Two persons	5091	67.1	50.8	52.6	2.6	2.8	3.0	2.2
More than two	2143	66.0	40.1	48.7	13.0***	4.8	10.8**	4.3
<i>Gender</i>								
Male	5450	58.7	46.1	47.8	2.9	2.4	3.4*	2.0
Female	6025	60.1	43.8	46.6	4.7**	2.2	3.6*	1.9
<i>Age</i>								
18–29	1401	57.8	34.3	38.0	6.4	4.9	8.5*	4.7
30–59	4527	57.3	44.0	47.4	6.0*	3.1	4.5*	2.6
60–103	5349	61.5	48.2	49.8	2.5	2.6	2.6	2.1
<i>Land of birth</i>								
Native-born	10037	59.3	46.0	48.1	3.6*	2.0	3.3*	1.7
Foreign-born	474	59.9	44.4	39.6	−7.9	8.3	−5.1	6.4

The subgroup “none of them” does not include first-time voters. Robust standard errors clustered on households. All tests are two-tailed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

participated on their own. However, the evidence can at best be interpreted as suggestive. Because the number of discussants is not randomized the results might reflect that politically interested citizens are more willing to engage in a discussion with the canvassers.¹⁵

It should also be mentioned that most two-person households are couples, rather than single parents or friends living together. In almost 90 per cent of these households, the two household members are of different sex and have an age difference smaller than 15 years.¹⁶

In contrast to Pons and Liegey (2013), I do not find larger effects among immigrants. On the contrary, among foreign-born, the turnout is in fact smaller in the treatment group than in the control group. However, the number of observations is small and the difference is not significant. In contrast to studies that find large positive effects among foreign-born and second-generation immigrants, most canvassers in this study are native-born. I also find that the effect declines with age. There is a gender difference in the bivariate regression that disappears when the covariates are included. The reason for this is that there was a (not significantly) larger share of female 2009 voters in the treatment group than in the control group.

In theory it would be possible to differentiate between different kinds of heterogeneous effects by including them as interaction variables in a regression framework. However, most differences between subgroups are not significant at the conventional levels

¹⁴ The small effect in apartment buildings could theoretically reflect contamination of the control group, if there is a large amount of between-household interaction, but I find that unlikely in a Swedish context. Moreover, within the control group, the turnout was higher in single-family houses than in apartments, controlling for turnout in previous elections.

¹⁵ Indeed, running placebo tests on turnout in previous elections as well as controlling for earlier voting behaviour clearly shows that there are systematic differences between households depending on how many people participate in the discussions.

¹⁶ The age difference between the youngest and the oldest person in larger households is above 15 years in 95 per cent of the households.

(note the distinction between significant differences between subgroups and statistically significant in some subgroups but not others).

It has become popular to analyse heterogeneous effects of canvassing in terms of the citizens' underlying propensity to vote. In an attempt to escape the categorical nature of the voting history, I have estimated the probability of voting for individuals in the control group by regressing the decision to vote on a large number of predictors.¹⁷ These predictions form the horizontal axis of Fig. 1 and the histogram shows how they are distributed. In front of the histogram are two loess lines, showing the turnout in both the treatment and the control group as locally weighted functions of the predictions. The gap between the two lines makes up the estimated ITT effect. This effect is largest when voters in the control group have a predicted voting propensity between 20 and 35 per cent and remains positive up to approximately 70 per cent. But as the three peaks in the histogram make apparent, the variation in the voting propensity is not really continuous but focused around these three clusters. The clusters correspond to the voting propensity given by previous voting behaviour. The other predictors do not add much information and as a consequence there are few citizens with a predicted probability of voting between 40 and 60 per cent. We should therefore be careful in drawing other conclusions than that there does not seem to be a mobilizing effect among voters with a very low or very high propensity to vote.

4.3. Vote choice and other effects

Shortly after the election, a telephone survey was conducted with 1000 respondents. Of the 743 respondents who reported that they had voted, 47 per cent in the treatment group stated that they voted for the Social Democrats, compared to 39 per cent in the control group. That difference corresponds to an intent-to-treat effect of 8 percentage points, or a treatment effect of 10–13 percentage points, depending on what assumptions we make about the contact rate among these respondents. The effects are also statistically significant at the 95 per cent level.

However, I would advise against interpreting these as causal effects on vote choice. To begin with, the effect is both surprisingly large and imprecisely estimated, increasing the plausibility of serious sampling error. Moreover, it is possible that canvassing increases the probability of lying due to social desirability bias. According to the survey, the turnout was 9 percentage points higher in the treatment group than in the control group, which indicates a much larger effect on self-reported than actual turnout.

In the supplementary information on my homepage, I present results from models that were excluded from this paper. Those include probit models, estimations of spill-over effects within the household and models where I analyse whether the mobilizing effect is conditioned by the timing of the contact and the characteristics of the canvassers.

5. Conclusions

This field experiment has shown that door-to-door canvassing can increase voter turnout also in a European context. Living in a household that was visited by party workers is estimated to increase the probability of voting by 3.6 percentage points.

While the estimated effect is twice as large among occasional voters and first-time voters, I do not find any effect at all among

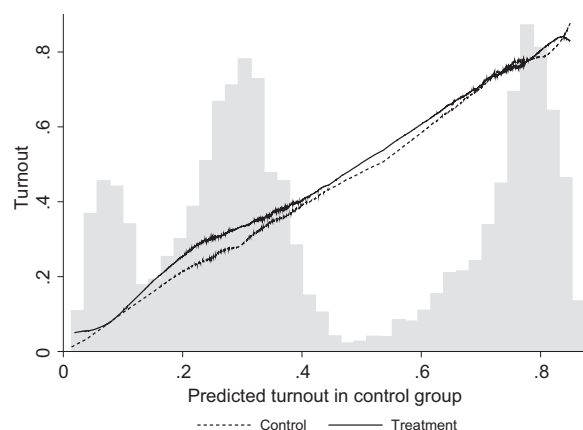


Fig. 1. Turnout over predicted turnout in control group.

chronic non-voters or voters with a very high vote propensity. This result is in line with previous research, which has shown that canvassing is most effective for citizens who are close to indifferent about voting.

I was surprised to find that the average effect was much greater in large households than in single-person households. If this finding can be replicated in other experiments, it has major implications both for how to conduct canvassing campaigns and for our understanding of voter behaviour. For campaign generals, it means that focusing the efforts on large households will not only increase the number of citizens who are reached by a single contact attempt, but also improve the average effect on every household member. For scholars of political behaviour, this finding underlines the social nature of voting. To learn more about why household size matters, we should design experiments with placebo messages where we can examine how the mobilizing effect is conditioned on the number of household members that are at home or participate in the conversation.¹⁸

Door-to-door canvassing is widely acknowledged as one of the most effective means for increasing voter turnout. It is therefore puzzling that such campaigns are much more common in countries with majoritarian systems than under proportional representation. The results in this paper confirm that door-to-door canvassing mobilize voters also in countries with PR systems and partisan canvassers. For scholars of comparative electoral research, the reasons why parties in many countries are so reluctant towards this form of campaigning remains to be explored. Are there perhaps institutional barriers that prevent our democracies from working as well as they could?

Appendix A. Supplementary data

Supplementary data related to this article can be found at <http://dx.doi.org/10.1016/j.electstud.2016.12.002>.

References

- Ali, Nageeb N., Lin, Charles, 2013. Why people vote: ethical motives and social incentives. *Am. Econ. J. Microeconomics* 5 (2), 73–98.
- Angrist, Joshua D., Imbens, Guido W., Rubin, Donald B., 1996. Identification of causal effects using instrumental variables. *J. Am. Stat. Assoc.* 91 (434), 444–455.
- Arceneaux, Kevin, 2007. I'm asking for your support: the effects of personally

¹⁷ The model is a logit with the following predictors: voting history, age, age squared, female, form of housing, household size and district dummies. Migrant status was excluded because of missing observations for first-time voters.

¹⁸ Delivering a placebo message to the control group, which could be any message that does not affect the probability of voting, would make it possible to estimate what the turnout would have been – conditioned on the number of conversation participants – in the absence of treatment.

- delivered campaign messages on voting decisions and opinion formation. *Q. J. Political Sci.* (Oct 2005), 43–65.
- Arceneaux, Kevin, Nickerson, David W., 2009. Who is mobilized to vote? A re-analysis of 11 field experiments. *Am. J. Political Sci.* 53 (1), 1–16.
- Bedolla, Lisa García, Michelson, Melissa R., 2012. Mobilizing Inclusion: Transforming the Electorate through Get-out-the-vote Campaigns. The Institution for Social and Policy St. Yale University Press.
- Bennion, Elizabeth A., 2005. Caught in the ground wars: mobilizing voters during a competitive congressional campaign. *Ann. Am. Acad. Political Soc. Sci.* 601 (1), 123–141.
- Bhatti, Yosef, Hansen, Kasper M., 2012. The effect of generation and age on turnout to the European Parliament – how turnout will continue to decline in the future. *Elect. Stud.* 31 (2), 262–272.
- Bhatti, Yosef, Dahlgaard, Jens Olav, Hansen, Jonas H., Hansen, Kasper M., 2014a. How Voter Mobilization Spread in Households and Families - the Use of Short Text Messages on Cell Phones to Boost Turnout.
- Bhatti, Yosef, Dahlgaard, Jens Olav, Hansen, Jonas Hedegaard, 2014b. Kan Man Øge Valgdeltagelsen? CVAP Working Paper Series University of Copenhagen.
- Blais, André, Aarts, Kees, 2006. Electoral systems and turnout. *Acta Polit.* 41 (2), 180–196.
- Bond, Robert M., Fariss, Christopher J., Jones, Jason J., Kramer, Adam D.I., Marlow, Cameron, Settle, Jaime E., Fowler, James H., 2012. A 61- million-person experiment in social influence and political mobilization. *Nature* 489 (7415), 295–298.
- Cox, Gary W., 1999. Electoral rules and the calculus of mobilization. *Legis. Stud. Q.* 24 (3), 387–419.
- Cox, Gary W., 2015. Electoral rules, mobilization, and turnout. *Annu. Rev. Political Sci.* 18 (1), 49–68.
- Fowler, James H., 2005. Turnout in a small world. In: Zuckerman, Alan (Ed.), *Social Logic of Politics*. Temple University Press, pp. 269–287.
- Gagatek, Wojciech, 2010. Campaigning in the european parliament elections. In: Gagateg, Wojciech (Ed.), *The 2009 Elections to the European Parliament*. European University Institute, pp. 13–19. Chap. 2.
- Gerber, Alan S., Green, Donald P., 2000. The effects of canvassing, telephone calls, and direct mail on voter turnout: a field experiment. *Am. Political Sci. Rev.* 94 (3).
- Gerber, Alan S., Green, Donald P., 2012. *Field Experiments : Design, Analysis, and Interpretation*. W. W. Norton, New York.
- Gerber, Alan S., Green, Donald P., Larimer, Christopher W., 2008. Social pressure and voter turnout: evidence from a large-scale field experiment. *Am. Political Sci. Rev.* 102 (1), 33–48.
- Górecki, Maciej a., Marsh, Michael, 2012. Not just 'friends and neighbours': canvassing, geographic proximity and voter choice. *Eur. J. Political Res.* 51 (5), 563–582.
- Green, Donald P., Gerber, Alan S., 2008. *Get Out the Vote : How to Increase Voter Turnout*, second ed. Brookings Institution Press, Washington, D.C.
- Green, Donald P., Gerber, Alan S., Nickerson, David W., 2003. Getting out the vote in local elections: results from six door-to-door canvassing experiments. *J. Polit.* 65 (04), 1083–1096.
- Green, Donald P., McGrath, Mary C., Aronow, Peter M., 2013. Field experiments and the study of voter turnout. *J. Elections, Public Opin. Parties* 23 (1), 27–48.
- Herrera, Helios, Morelli, Massimo, Palfrey, Thomas, 2014. Turnout and power sharing. *Econ. J.* 124 (574).
- Hix, Simon, Marsh, Michael, 2007. Punishment or protest? Understanding European parliament elections. *J. Polit.* 69 (2), 495–510.
- John, Peter, Brannan, Tessa, 2008. How different are telephoning and Canvassing? Results from a 'get out the vote' field experiment in the British 2005 general election. *Br. J. Political Sci.* 38 (03), 565–574.
- Karp, Jeffrey A., Banducci, Susan A., 2008. Political efficacy and participation in twenty-seven democracies: how electoral systems shape political behaviour. *Br. J. Political Sci.* 38 (2), 311–334.
- Karp, Jeffrey A., Bowler, Shaun, Banducci, Susan A., 2003. Electoral systems, party mobilization and turnout: evidence from the European parliamentary elections. *J. Elections, Public Opin. Parties* 13 (1), 210–225.
- Karp, Jeffrey A., Banducci, Susan A., Bowler, Shaun, 2008. Getting out the vote: party mobilization in a comparative perspective. *Br. J. Political Sci.* 38 (01), 91–112.
- Lijphart, Arend, 1997. Unequal participation: democracy's unresolved dilemma. *Am. Political Sci. Rev.* 91 (1), 1–14.
- Lijphart, Arend, 1999. *Patterns of Democracy, Government Forms and Performance in Thirty Six Countries*. Yale University Press, New Haven.
- Marsh, Michael, 1998. Testing the second-order election model after four European elections. *Br. J. Political Sci.* 31 (1997), 591–607.
- Michelson, Melissa R., 2003. Getting out the Latino vote: how door-to-door canvassing influences voter turnout in rural central California. *Polit. Behav.* 25 (3), 247–263.
- Michelson, Melissa R., 2014. Memory and voter mobilization. *Polity* 46 (4), 591–611.
- Nickerson, David W., 2006. Forget Me Not? the Importance of Timing in Voter Mobilization.
- Nickerson, David W., 2008. Is voting contagious? Evidence from two field experiments. *Am. Political Sci. Rev.* 102 (1), 49–57.
- Nickerson, David W., Rogers, Todd, 2010. Do you have a voting plan? Implementation intentions, voter turnout, and organic plan making. *Psychol. Sci.* 21 (2).
- Nickerson, David W., Friedrichs, Ryan D., King, David C., 2006. Partisan mobilization campaigns in the field: results from a statewide turnout experiment in Michigan. *Political Res. Q.* 59 (1), 85–97.
- Nielsen, Julie Hassing, 2014. Why use experiments in EU studies? *Comp. Eur. Polit.* 1–19.
- Niven, David, 2001. The limits of mobilization: turnout evidence from state house primaries. *Polit. Behav.* 23 (4), 335–350.
- Pons, Vincent, 2014. Does Door-to-door Canvassing Affect Vote Shares? Evidence from a Countrywide Field Experiment in France (July).
- Pons, Vincent, Liegey, Guillaume, 2013. Increasing the Electoral Participation of Immigrants – Experimental Evidence from France.
- Powell, G. Bingham, 1989. Voting turnout in thirty democracies: partisan, legal and socio-economic influences. In: Rose, Richard (Ed.), *Electoral Participation: a Comparative Analysis*. Sage, Beverly Hills, CA, pp. 396–412.
- Rainey, Carlisle, 2015. Strategic mobilization: why proportional representation decreases voter mobilization. *Elect. Stud.* 37, 86–98.
- Reif, Karlheinz, Schmitt, Hermann, 1980. Nine second-order national elections – a conceptual framework for the analysis of european election results. *Eur. J. political Res.* 8, 3–44.
- Shachar, Ron, Nalebuff, Barry, 1999. Follow the leader: theory and evidence on political participation. *Am. Econ. Rev.* 89 (3), 525–547.
- Uhlauer, Carole J., 1989. Rational turnout: the neglected role of groups. *Am. J. Political Sci.* 33 (2), 390–422.
- Vavreck, Lynn, 2007. The exaggerated effects of advertising on turnout: the dangers of self-reports. *Q. J. Political Sci.* 2 (4), 287–305.

Supplementary information

This is an appendix to the paper *Door-to-door canvassing in the European elections: Evidence from a Swedish field experiment*. It consists of four parts. First I describe the data and definitions in more detail. Then I provide the results from my main models, but on different samples and with alternate functional forms. Third, I examine how the mobilizing effect differs depending on the time of the canvassing, the canvassers' characteristics and whether the treated person spoke directly to the canvassers. Fourth, I provide a copy of the script, or conversation guidelines, which the Social Democrats used in their campaign.

Details about data

Matching the preliminary electoral rolls for the upcoming European Parliament election to the rolls from previous elections was a tricky process. While the old rolls include 10-digit personal identification numbers, the preliminary rolls only include date of birth, names and addresses. To match the new electoral rolls with the ones from previous elections, I had to use two different methods.

- If the date of birth was unique within the electoral district for both 2010 and 2014, and the gender was the same for both entries, I assumed that it was the same person and matched the entries.
- To match entries with non-unique dates of birth, I wrote a script that searched for these entries at upplysning.se (a Swedish site that provides credit records) and gathered the complete identification number from there.
- I then cross-validated the two methods by using the script to find the personal identification numbers for 533 people whose dates of birth *were* unique in the electoral district. For all these entries, both methods gave the same result.

Households were then excluded from the sampling frame if at least one third of the household members met any of these exclusion criteria: i) I do not have information about whether they voted in 2009 and 2010 (first time voters are not excluded), ii) they voted in 2009 but not 2010 (very small group), or iii) they have temporarily changed their address to an address outside the 17 districts or to a postal box. Households were also excluded if there were more than 3 people older than 30 living on the address (typically retirement homes). In total, 31 per cent of the citizens eligible to vote were excluded from the sampling frame, with a remaining 11 640 individuals distributed over 7579 households. Most of them were excluded because of

the first criteria, which could mean that they lived somewhere else during the previous elections.

I used strata determined by electoral district and household size to randomly allocate the households to equally sized treatment and control groups. That the randomization was successful is supported by the balance between the treatment and the control group that is shown in Table 1. The table presents summary statistics of the treatment and control group, together with the p-values from a t-test of the means. None of the differences are close to being statistically significant.

165 individuals changed address between the creation of the preliminary and the final electoral rolls. While they were included in the sampling frame, and thus allocated to either the treatment or the control group, they have been excluded from the analysis because I have no information about whether they voted. Because the decision to change address is unrelated to the treatment status, this does not affect the validity of the experiment.

The personal identification numbers include information about a person's sex and age. In addition, numbers given before 1990 contain information about where a person was born. Any person that migrated to Sweden after 1946 was until 1990 given a certain code for birth locations outside Sweden. The same code was to a lesser extent also used when the common region codes were all used up. Consequently, this method of identifying foreign-born will also include some native-born. Using this measure of birth location will therefore underestimate any difference between native- and foreign-born.

The size of the household is calculated from the number of people living at the same address. Because the estimation is based on voting registers, only people older than 17 are included. The address, or more precisely whether it includes an apartment number, is also used to separate people living in flats from those living in single-family houses.

For each contact attempt, the canvassers reported the date and the outcome. In short, they could chose between a successful contact, that the address was unaccessible, a failed contact with no new attempts and a failed contact but that new attempts should be made. If a contact attempt was successful, the canvassers specified who they had been talking to. The canvassers also identified themselves using a four-digit code, which I match to canvasser lists with information about their sex, their age and whether they are local celebrities. That makes it possible to examine possible identification effects, for which there is some suggestive evidence (Bennion 2005).¹⁸

¹⁸While Bennion finds that student canvassers only affected younger voters, we do not know if it was an effect of similar age or if we would have found the same difference if the canvassers were older.

Table A1: Geographical differences

Subgroup	Obs	Visited	Turnout		Bivariate		Covariates	
			Control	Treat	Effect	SE	Effect	SE
<i>Municipality</i>								
Flen	1643	72.6	49.7	50.6	1.3	4.3	3.9	3.5
Katrineholm	4725	60.6	41.7	45.2	5.8**	2.9	4.7*	2.5
Nyköping	2597	45.4	45.4	45.0	−1.0	5.2	2.3	4.4
Strängnäs	1497	59.4	49.9	52.5	4.4	5.6	0.2	4.5
Vingåker	1013	69.6	43.3	49.0	8.1	5.6	3.4	4.8
<i>Electoral district</i>								
Flens södra	744	58.0	50.4	49.3	−1.9	7.6	5.1	6.2
Hälleforsnäs	899	84.7	49.1	51.7	3.0	5.1	3.5	4.3
Nävertorp	644	66.7	43.3	47.8	6.8	7.2	11.6*	6.2
Linnéan	644	52.8	35.9	39.2	6.2	8.5	0.4	7.2
Nyhem	684	68.5	40.2	45.7	7.9	7.1	5.7	6.3
Gamla Vattent.	584	59.5	38.6	45.8	12.1	8.0	9.5	7.2
Norr City	642	45.5	41.8	45.1	7.4	10.1	−2.1	9.2
Järven	737	46.0	42.7	46.3	7.7	9.4	5.7	7.9
Lövåsen	790	81.6	47.2	46.2	−1.3	5.9	0.8	5.0
V. Brandkärr	406	37.3	32.7	31.3	−3.6	14.4	−10.7	13.1
Oppeby gård	408	47.5	39.7	35.3	−9.3	11.8	1.9	9.6
Oppeby	910	56.8	52.0	54.8	5.0	7.2	8.2	6.0
Stenkulla	873	36.1	47.3	45.5	−4.8	11.5	−0.1	9.6
Stadsskogen	686	65.8	49.0	57.1	12.4*	7.1	7.5	5.8
Åker N	811	54.1	50.7	48.6	−3.9	8.7	−7.4	6.8
Sävsta	544	61.6	39.9	47.6	12.5	8.4	6.8	7.3
Högsjö	469	79.1	47.1	50.7	4.5	7.6	1.3	6.3

Robust standard errors clustered on households. All tests are two-tailed.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Alternative specifications

Table A1 displays the main results divided on geographic location. The top part shows the results when the sample divided on the 5 municipalities and the bottom part shows separate results for the 17 electoral districts. The estimated effects differs substantially, but as shown by the standard errors, most samples are too small to give precise estimates of the effects.

Despite using a binary dependent variable, I only use linear models in the paper. The main reasons for doing so is to facilitate interpretation and make the results comparable to previous research; most similar experiments use either linear regression models or simple comparisons of proportions to describe their results. To test whether the results are dependent on the choice of model, I here complement my linear models with probit regressions. The estimated models are identical to the linear model except for the functional form.

Table A2 displays the results from four different regressions. The first four columns show the regression coefficients and the standard errors of

Table A2: Comparing linear and probit regressions

Subgroup	Linear				Probit			
	Bivariate Effect	SE	Covariates Effect	SE	Bivariate Effect	SE	Covariates Effect	SE
Full sample	3.9**	1.9	3.6**	1.6	9.6*	4.9	11.0**	5.3
<i>Voting history</i>								
Both elections	1.0	2.3	0.8	2.3	3.8	7.4	3.2	7.5
Only 2010	6.3**	2.8	6.0**	2.8	17.5**	8.3	16.8**	8.3
None of them	-1.5	3.0	-1.4	3.0	-12.5	22.0	-10.4	22.3
First-time voters	6.5	6.2	6.4	6.0	17.1	16.3	17.1	16.1
<i>Housing</i>								
House	6.0**	2.6	5.1**	2.2	15.2**	6.6	16.1**	7.1
Flat	1.5	2.8	1.7	2.4	3.7	7.4	5.4	8.0
<i>Household size</i>								
Single person	-0.5	3.2	-0.3	2.7	-2.0	8.3	-2.3	9.3
Two persons	2.6	2.8	3.0	2.2	6.6	6.9	9.8	7.5
More than two	13.0***	4.8	10.8**	4.3	32.8***	12.1	30.7**	12.4
<i>Gender</i>								
Male	2.9	2.4	3.4*	2.0	7.3	6.1	10.4	6.7
Female	4.7**	2.2	3.6*	1.9	11.5**	5.7	11.2*	6.2
<i>Age</i>								
18-29	6.4	4.9	8.5*	4.7	16.8	13.1	23.5*	13.2
30-59	6.0*	3.1	4.5*	2.6	15.0*	7.7	13.3	8.3
60-103	2.5	2.6	2.6	2.1	6.2	6.6	9.1	7.4
<i>Land of birth</i>								
Native-born	3.6*	2.0	3.3*	1.7	9.0*	5.1	10.2*	5.5
Foreign-born	-7.9	8.3	-5.1	6.4	-20.4	20.9	-17.2	23.5

The subgroup "none of them" does not include first-time voters. Robust standard errors clustered on households. All tests are two-tailed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the linear model used in the paper, expressed in percentage points (scaled up 100 times). The last four columns show the coefficients and standard errors of a probit model. In all models, the treatment is instrumented on the assignment. The relative size of effects between subgroups, as well as the levels of statistical significance, are very similar between the two kinds of models.

Models without random assignment

The canvassers reported the date and outcome for each contact attempt. If the attempt was successful, they also reported the name of the voters and the id of the canvassers that participated in the conversation. It is therefore

possible to examine whether the mobilizing effect differs depending on the timing of the canvassing and the characteristics of the canvassers. However, these factors were not randomized, so the results must be interpreted with care.

Table A3 presents the results from regressing the dependent variable (voting 2014) on different canvasser characteristics in addition to the standard set of predictors. The sample is restricted to people who talked to the canvassers. The first column shows the effect of talking to a canvasser that is considered to be "locally known", compared to talking to a not known canvasser. The second column shows the effect of gender and the third column shows the effect of age and age similarity. None of the effects are close to being statistically significant. A canvasser's age or gender does not appear to affect its effectiveness. Neither is canvassing more effective when the canvasser is of the same age or gender as the voter or when the canvasser is locally known.

Most of the canvassing was done in pairs, which means that usually two canvassers were involved in the contact. The models presented in Table A3 are based on the characteristics of the canvasser that led the conversation. I have also run models that include the characteristics of the second canvasser – and different joint characteristics – but it does not change the results.

In Table A4 I present the regression results from models that examine whether the timing of canvassing affects its efficiency. The sample used for the first three models was restricted to citizens who live in a household that was visited by a canvasser. The regression coefficient for "Days left" tells us whether people who were visited close to the election had a higher turnout than those who were visited much earlier. Many of the people who were visited close to the election had not been at home when the canvassers made their first contact attempts. Because the reasons for being difficult to reach could be associated with vote propensity, the second column controls for the number of contacts that the canvassers attempted before they eventually succeeded. In the third column, I have instead instrumented the day of the contact with the day that the first contact attempt was made. The effect of time has the wrong sign in all of the three models (households that were canvassed early have a larger probability of voting) and is never close to being statistically significant.

The model presented in the fourth column better resembles the identification strategy used in the rest of the paper. Based on the addresses, I have imputed when the first contact attempt would have been made if the households in the control group had instead been assigned to the treatment group. I then estimate an interaction effect between the treatment (living in a household that was visited by a canvasser) and the date for when the first contact attempt was made. The treatment and the interaction of time and treatment are instrumented on the assignment to the treatment group and an interaction of assignment and time. While the interaction effect now has

Table A3: Canvasser effects

	(1)	(2)	(3)
<i>Voter</i>			
Voted 2009	0.454*** (0.023)	0.449*** (0.022)	0.454*** (0.023)
Voted 2010	0.223*** (0.027)	0.229*** (0.026)	0.223*** (0.027)
First-time voter	0.260*** (0.077)	0.298*** (0.073)	0.265*** (0.077)
Age	0.004 (0.004)	0.005 (0.004)	0.003 (0.005)
Age squared	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
Female	0.001 (0.018)	0.001 (0.017)	0.001 (0.018)
Locally known	−0.024 (0.023)		
Female		0.002 (0.020)	
Same sex		−0.022 (0.017)	
Age			0.001 (0.001)
Age difference			−0.001 (0.001)
Constant	0.020 (0.131)	−0.105 (0.136)	0.019 (0.170)
Observations	2098	2229	2089
Adjusted R^2	0.273	0.272	0.273

District dummies are included in all regressions. Robust standard errors clustered on households. All tests are two-tailed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the right sign, it is still far from being statistically significant.

The treatment effect analysed in this paper is the effect of living in a household that was visited by a canvasser. It is likely that the people who talk directly to the canvassers are more strongly affected than other household members, but we cannot assume that the latter are unaffected. Not only are people living in the same household likely to discuss the canvasser's visit with each other, but if one household member decides to vote, chances are that he (she) will bring the others with him (her).

It is difficult to identify the direct effect and the "spill over"-effect separately, because we cannot assume that those who participate in the conversation would have had the same probability of voting as the other household members in the absence of treatment. To create a control group each for those who participate and for the rest of the household, Nickerson (2008) alters the "get out the vote"-message with a placebo message about recycling, and estimates the spill-over effect to be 40 per cent smaller than the direct effect.

Table A4: A diminishing effect?

	(1)	(2)	(3)	(4)
Days to election	0.002 (0.002)	0.001 (0.002)	0.001 (0.003)	
Contact attempts		-0.012 (0.017)		
Visited				0.069 (0.042)
Days left, first attempt				0.002 (0.001)
Visited $\times$ Days left				-0.003 (0.003)
Voted 2009	0.437*** (0.020)	0.437*** (0.021)	0.442*** (0.020)	0.467*** (0.011)
Voted 2010	0.202*** (0.024)	0.202*** (0.024)	0.201*** (0.023)	0.170*** (0.012)
First-time voter	0.279*** (0.056)	0.279*** (0.056)	0.281*** (0.055)	0.332*** (0.027)
Age	0.006* (0.003)	0.006* (0.003)	0.006* (0.003)	0.011*** (0.002)
Age squared	-0.000** (0.000)	-0.000** (0.000)	-0.000** (0.000)	-0.000*** (0.000)
Female	-0.006 (0.012)	-0.006 (0.012)	-0.009 (0.012)	-0.014** (0.007)
Constant	-0.127 (0.119)	-0.120 (0.120)	0.039 (0.104)	-0.114** (0.051)
Observations	3337	3337	3441	11475
Adjusted R^2	0.239	0.239	0.243	0.269

District dummies are included in all regressions. Robust standard errors clustered on households. All tests are two-tailed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

There are two assumptions that on their own – if they were true – would make it possible to identify the separate effects also within the design of this paper. First, if the effect of canvassing would be the same regardless of household size, I could use the household size as an instrument for whether a person talks to the canvasser (the probability is 1 in a treated single-person household and much lower in larger households). However, as shown in Table 3, that assumption is clearly not true. Second, if those who participate directly were similar to those who are only affected indirectly, the difference in turnout between the groups would measure the difference between the direct effect and the spill over effect. Unfortunately, the two groups are quite different. In two-person households, the difference in turnout 2009 was over five percentage points.

The only remaining approach is therefore to control for the pre-treatment vote propensity using voting history and other predictors. It is difficult to estimate the direct and the indirect effect in the same model.¹⁹ I have

¹⁹It is not random which households in the treatment group that are treated, and the

Table A5: Estimating spill-over effects

	(1)	(2)	(3)
Average effect	3.0	9.8	5.1
Participating share	61.1	44.9	55.8
Effect of participating	4.0	8.9	3.7
Direct effect	4.6	14.7	6.7
Indirect effect	0.6	5.8	3.0
Observations	1709	486	2419
Household members	2	3	>1

The table shows the average effect of living in a visited household (being treated), the share of the treated who talked to the canvassers, the difference in turnout between participants and other treated and calculations of the direct and indirect treatment effect.

therefore estimated the effect of participating in the discussion, compared to only being affected by spill-over effects, on a sample restricted to the canvassed households. The direct and indirect effects were then derived from the average treatment effect, the share of participators and the effect of participating (which is synonymous with the difference between the direct and the indirect effect).²⁰ An obvious problem is that household size appears to affect both the effectiveness of canvassing (positively) and the probability of speaking directly to a canvasser (negatively). If we do not take household size into account, we will therefore underestimate the direct effect and overestimate the spill over. Table A5 therefore present separate calculations for two-person households, three-person households and households with at least two members. The estimated spill-over effects are 13, 39 and 45 per cent of the direct effect, depending on the size of the household. However, if the control variables are insufficient measures of the pre-treatment vote propensity, so that those who participated in the canvassing talk had a higher pre-treatment vote propensity also when we control for the 2009 and 2010 elections, that would bias the direct effect upwards and the spill-over effect downwards. Such a bias could explain why these estimates are smaller than those found by Nickerson (2008).

assignment instrument is correlated with participation, so we cannot simply add a variable for being treated or being assigned to the treatment group.

²⁰The average effect is calculated with the multivariate IV regression framework used in the paper. The share of the treated individuals that participated in the conversation is known and the effect of participating – compared to living in the same household and not participate – is estimated in a regression on treated individuals. The direct and indirect effects are then calculated using the identity $average = direct \times share + indirect \times (1 - share)$.

Assuming a size of the spill-over effects

Because randomization was conducted at the household level, the effect of living in a visited household is the only treatment effect which can be estimated without making additional assumptions. This is why I throughout the main part of the paper define everyone who live in a household which was visited by canvassers as 'contacted' or 'treated'.

But as pointed out by one of the anonymous reviewers, it is increasingly common that only those who speak directly to the canvassers are counted as 'contacted'. However, because one of the identification assumptions is that any systematic difference between the treatment and the control group is caused by the treatment, defining the treatment as only direct contact will induce a bias if there are spill-over effects (those directly contacted mobilize other household members). When the randomization is done at the household level, so that spill-over effects within the household are only raising the turnout rate in the treatment group, the bias will be upwards.

With all that in mind, Table A6 shows what Table 3 would have looked like, if only direct contacts were counted as treatment. Because the contact ratio is reduced from 60 to 40 per cent, the estimated effects increase with approximately 50 per cent ($60/40 = 1.5$). For the full sample, that means that the treatment effect increases from 3.6 to 5.3 percentage points. This is partly due to a new treatment definition (direct contact) and partly because of a positive estimation bias (if there are positive spill-over effects). For subgroups with a large share of indirect contacts (people who live in treated households but that did not personally talk to the canvassers), the increase is larger. It is therefore not surprising that the most dramatic change compared to the main result is found for people living in large households. If the spill-over effect is positive and independent of household size, the estimation bias will be larger in large households.

In principle, the contact rate for single-person households should not be affected by the new definition; if there is only one person living there, he or she should be the one who talked to the canvassers. Yet, the ratio decreases from 46.9 to 44.9 per cent. This difference corresponds to instances where the canvassers have stated that they spoke to someone at the address, but without checking the box for the only person registered as living there. Possible explanations are that they simply forgot to check the box, that they talked to a friend or relative of the person living there, that there are people in the household who are registered on another address or that someone else moved in during the time between we received our register data and the canvassing took place.

Instead of assuming that there are no spill-over effects, we can set them to a pre-defined percentage of the direct effect. If this percentage is wrong, the estimations will be biased, but this bias will hopefully be smaller than when we assumed that the spill-over effect was zero. The most well-known

Table A6: Alternative treatment: speaking to the canvasser (no spill-over)

Subgroup	Obs	Visited	Turnout		Bivariate		Covariates	
			Control	Treat	Effect	SE	Effect	SE
Full sample	11475	40.1	44.9	47.2	5.8**	2.9	5.3**	2.4
<i>Voting history</i>								
Both elections	4659	45.2	75.3	75.9	1.4	3.3	1.1	3.2
Only 2010	4337	40.1	27.0	30.7	9.1**	4.0	8.6**	4.0
None of them	1408	35.8	7.5	6.8	-2.0	4.1	-1.9	4.1
First-time voters	760	25.3	36.2	40.2	15.7	14.9	15.4	14.5
<i>Dwelling</i>								
Single-family	4958	43.6	51.5	55.8	9.9**	4.3	8.4**	3.7
Multiple-family	6517	37.5	39.9	40.6	2.0	3.8	2.3	3.2
<i>Household size</i>								
Single person	4241	44.9	40.2	40.0	-0.5	3.4	-0.3	2.8
Two persons	5091	41.0	50.8	52.6	4.3	4.5	4.9	3.7
More than two	2143	28.5	40.1	48.7	29.9***	11.0	24.9**	9.9
<i>Gender</i>								
Male	5450	38.2	46.1	47.8	4.5	3.7	5.2*	3.1
Female	6025	42.0	43.8	46.6	6.7**	3.2	5.2*	2.7
<i>Age</i>								
18-29	1401	24.7	34.3	38.0	15.0	11.4	20.0*	11.0
30-59	4527	37.5	44.0	47.4	9.1*	4.7	6.8*	4.0
60-103	5349	46.4	48.2	49.8	3.4	3.5	3.4	2.8
<i>Land of birth</i>								
Native-born	10037	41.5	46.0	48.1	5.2*	2.9	4.8**	2.4
Foreign-born	474	44.7	44.4	39.6	-10.6	11.2	-6.8	8.6

The table shows the average effect of speaking directly to the canvassers, estimated under the assumption that there are no spill-over effects. Only those who spoke directly to the canvassers are counted as treated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

study of spill-over effects found that 60 per cent of the direct effect is passed onto the other household members (Nickerson 2008). In all previous analyses, the treatment variable is a binary indicator for whether a person was treated (1) or not (0). The average of this variable has been referred to as the contact ratio. For the analysis presented in Table A7, the treatment variable can also take the value 0.6 for indirect contacts (60 per cent of 1). Note that the mean of this variable (0.517) can no longer be interpreted as a proportion, because it is now a weighted average of the share of directly and indirectly treated ($0.401 + 0.194 * 0.6 = 0.517$).

For the full sample, the change in the mean of the treatment variable means that the estimated effect increases by approximately 15 per cent ($59.5/51.7=1.15$), from 3.6 to 4.1. As in the previous table, this increase

Table A7: Alternative treatment: speaking to the canvasser (60% spill-over)

Subgroup	Obs	Visited	Turnout		Bivariate		Covariates	
			Control	Treat	Effect	SE	Effect	SE
Full sample	11475	51.7	44.9	47.2	4.5**	2.2	4.1**	1.9
<i>Voting history</i>								
Both elections	4659	56.3	75.3	75.9	1.1	2.6	0.9	2.6
Only 2010	4337	50.6	27.0	30.7	7.2**	3.2	6.8**	3.2
None of them	1408	43.5	7.5	6.8	-1.6	3.4	-1.6	3.4
First-time voters	760	46.5	36.2	40.2	8.5	8.1	8.3	7.8
<i>Dwelling</i>								
Single-family	4958	60.9	51.5	55.8	7.1**	3.1	6.0**	2.6
Multiple-family	6517	44.7	39.9	40.6	1.7	3.2	1.9	2.7
<i>Household size</i>								
Single person	4241	46.1	40.2	40.0	-0.5	3.3	-0.3	2.7
Two persons	5091	56.7	50.8	52.6	3.1	3.3	3.6	2.7
More than two	2143	51.0	40.1	48.7	16.8***	6.2	14.0**	5.6
<i>Gender</i>								
Male	5450	50.5	46.1	47.8	3.4	2.8	3.9	2.4
Female	6025	52.9	43.8	46.6	5.3**	2.5	4.1*	2.2
<i>Age</i>								
18-29	1401	44.6	34.3	38.0	8.3	6.3	11.0*	6.0
30-59	4527	49.4	44.0	47.4	7.0**	3.5	5.2*	3.0
60-103	5349	55.5	48.2	49.8	2.8	2.9	2.9	2.3
<i>Land of birth</i>								
Native-born	10037	52.2	46.0	48.1	4.1*	2.3	3.8**	1.9
Foreign-born	474	53.8	44.4	39.6	-8.8	9.2	-5.7	7.2

The table shows the average effect of speaking directly to the canvassers, estimated under the assumption that the size of the spill-over effects are 60 per cent of the direct effects. In this table, the share of visited households cannot be interpreted as a percentage, because those who were only treated indirectly are here counted as 0.6 visits. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

reflects a change in the definition of treatment (only direct contact, instead of everyone in the visited household), but we can no longer be sure if our results are biased upwards or downwards. If the true spill-over effect is larger than 60 per cent, the bias is positive, but if the effect is smaller, the bias will be negative. Just like in the previous table, the largest differences compared to the main analyses are found for large households, where the directly treated compose a smaller share of those who live in a visited household.

Instead of setting a pre-defined spill-over effect, we can estimate the treatment effect for a range of different assumptions and use a graph to show how the effect changes depending on what assumptions we make. Figure A1 shows the estimated effect for the full sample, with (black) and without (grey)

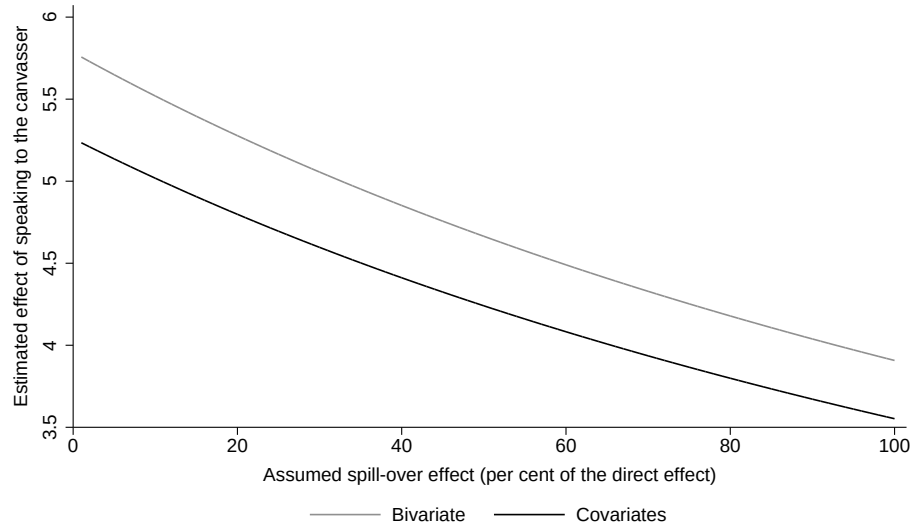


Figure A1: The effect of direct contact under different spill-over assumptions

covariates, for assumptions from that the spill-over effect is zero (identical to Table A6) to that it is 100 per cent. Note that while an assumed spill-over effect of 100 percent leads to identical estimates as in the main part of this paper, this assumption is only needed when the treatment is defined as talking to the canvassers. When the treatment is defined as living in a visited household, it cannot be biased by spill-over within the household.

Conversation guidelines

At the end of this appendix, you find the script or conversation guidelines that were given to the canvassers. The same document that was used for this experiment was also used by the Social Democrats in the rest of their election campaign. Because the material is in Swedish, I have provided a brief summary below.

The conversation was divided into four parts with different goals. The goal in the first part was for the canvasser to establish a contact with the potential voter, by presenting him- or herself and in a personal tone explain the purpose of canvassing. The second part of the conversation was focused on asking questions about the European union and the election, with the purpose of making the canvassed person think about him- or herself as a voter. This is where they typically would discuss societal problems and what political solutions that the Social Democrats could offer.

In the third part, the canvasser tried to reach an agreement where the other person promised to go and vote. The canvasser would also inform about where the polling station was located. The fourth part was about

saying goodbye in a friendly manner and reminding the potential voter about the agreement.

The political message was focused on three topics: the fight against unemployment, fair working conditions for everyone working in Sweden, and investment in renewable energy. For each topic, the guidelines provided three political proposals as well as some one-liners that had been proven to work well.

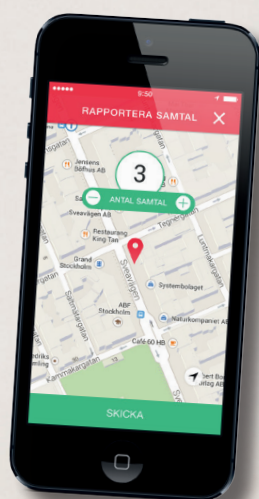
The script was clearly inspired by experiences from other field experiments, and many of the tips and tricks can also be found in recommendations published by campaign consultants such as the Analyst Institute.

While the script is quite similar to those that have been used in other get-out-the-vote campaigns, there are also some notable differences. For example, it did not emphasize recruitment to the same extent as many of the canvassing campaigns in the US, as it did not include any questions about raising money or joining the campaign as a volunteer. The only element of recruitment was that sympathisers were asked for their e-mail address.

EU-VALET 2014

SAMTALS- KAMPANJEN

SAMTALET VID DÖRREN
FÖR DIG SOM VALARBETARE



Socialdemokraterna
FRAMTIDSPARTIET

HUVUDBUDSKAP

Rättvisa villkor och fler jobb

Det är ett historiskt misslyckande att 26 miljoner människor i dag saknar arbete i Europa. Klyftorna mellan människor och länder vidgas, framtidstron urholkas, hopplösheten breder ut sig. Det drabbar också Sverige.

EU skulle kunna vara annorlunda. De senaste femton årens högerpolitik kan bytas ut.

I EU-VALET PRIORITERAR VI

Fler jobb genom investeringar i forskning och aktiv näringspolitik. Det ger även fler jobb i Sverige.

Rättvisa arbetsvillkor så att svenska avtal och villkor ska gälla för alla som arbetar i vårt land. Oseriösa företag ska inte kunna konkurrera med sänkta löner eller dålig arbetsmiljö.

Investeringar i förnybara alternativ och klimatomställning för att skapa fler jobb och ta ansvar för framtiden.

Tillsammans kan vi i EU säkra en hållbar tillväxt och full sysselsättning. Det vore bra för Europa, och för Sverige.

Din röst behövs - för rättvisa villkor och fler jobb!

NÅGRA SAKER ATT TÄNKA PÅ VID DÖRRKNACKNING

Placering; Stå i linje med gångjärnen på dörren, så personen som du ringer på hos öppnar dörren helt och inte bara öppnar en liten glugg.

Avstånd; Stå på lagom avstånd. Står du för nära kommer personen sätta upp garden, bli skygg, orolig eller nervös. Står du för långt ifrån kommer det också att bli konstigt. Två-tre meters avstånd är lagom.

Presentation; Presentera dig klart och tydligt så det inte råder någon tvekan om vem du är och i vilket syfte du är där.

Lyssna; För att skapa kemi krävs det att du lyssnar.

FYRA DELAR I ETT SAMTAL FÖR ATT MOBILISERA VÄLJARE

Gör som de flesta - gå och rösta

Vi vet att människor gör det de uppfattar att de flesta gör, inte som de uppfattar att man borde göra. Därför är det mer effektivt och mobiliserande med ett budskap som tydligt sätter väljaren i rollen att vara en väljare istället för en direkt uppmaning till väljaren att gå och rösta. Budskapet under samtalet blir då att väljaren ska gå och rösta eftersom alla andra gör det. Det har också visat sig att människor är mer benägna att gå och rösta om man lovar att göra det. Samtalet bör därför innehålla en del som handlar om att planera röstandet. En bra metod för att få en väljare att börja tänka på sig själv som en väljare och planera sitt röstande är att ställa frågor i en muntlig enkät. Därför kommer samtalsstödet vara i enkätform.

1. Att inleda ett samtal - kontakt

Syftet med inledningen är att få möjlighet att inleda ett samtal och på ett naturligt sätt få igång samtalet. De första sekunderna är viktigast för att ge ett bra intryck och skapa en positiv inställning. Presentera dig och berätta syftet med varför du knackar dörr med en personlig ton.

2. Att tänka på i samtalet - behov

Behovsfasen handlar framför allt om att ställa frågor och ta reda på information om personens inställning till EU valet. Här ska vi ställa frågor som innebär att personen får tänka på sig själv som väljare och reflektera över hur viktigt de tycker att valet är.

3. Att tänka på i samtalet - lösning

Nu handlar det om att få en överenskommelse kring att personen går och röstar. Vi ska också såklart berätta om vår politik och fånga in de människor som är potentiella röstare för fortsatt bearbetning och påverkan. Be om e- postadressen! Här ska vi också föreslå medlemskap och möjligheten att engagera sig i valrörelsen för de som är intresserade av det. Se det som en självklarhet att personen ska gå med om den är uppenbart Socialdemokrat.

Om någon undrar vad vi ska ha e postadressen till, berätta att vi bara använder den till att skicka information och att uppgifterna sparas enligt vår integritetspolicy

www.socialdemokraterna.se/Om-webbplatsen/Integritetspolicy

4. Att tänka på när du avslutar

Avsluta samtalet på ett trevligt sätt och glöm inte bort överenskommelsen kring att personen ska gå och rösta.

EXEMPEL PÅ ETT SAMTAL

1. Kontakt

Valarbetare: Hej! Jag heter X och kommer från Socialdemokraterna i X. Som du säkert vet är det EU-val 25 maj. Eftersom valet kommer handla om två tydliga vägval så kommer många ta chansen att gå och rösta. Jag har några frågor kring hur du ser på EU valet.

Väljare: Okej!

2. Behov

Valarbetare: Hur viktigt är EU-valet för dig?

Viktigt Varken eller Oviktigt

Väljare: Varken eller

Valarbetare: Vilken fråga tycker du är viktigast i EU-valet?

Väljare: Jag tycker att man borde göra något åt miljön och arbetslösheten.

Valarbetare: Jag håller med dig om att det är viktiga frågor. Vi Socialdemokrater tycker att det behövs en annan politik i Europa för att minska arbetslösheten och ta ansvar för miljön. Våra prioriteringar i EU-valet kommer vara fler jobb, bra villkor på arbetsmarknaden och miljön. Hur tänker du kring det?

Väljare: Det låter ju bra.

Valarbetare: Om jag får din epost adress kan jag skicka mer information om vad vi vill arbeta för i EU.

Väljare: Okej.

Valarbetare: När planerar du att gå och rösta?

Förtidsrösta På valdagen Vet inte

Väljare: Jag vet faktiskt inte, jag har inte riktigt funderat på det.

3. Lösning

Valarbetare: Har du all information du behöver för att förtidsrösta/rösta på valdagen?

Väljare: Nej, jag vet faktiskt ingenting.

Valarbetare: Okej, men då ska vi ta reda på hur det ser ut för din del. (Gå in på SAPPEN och leta efter närmaste plats där röstning kan ske) .I EU-valet är det en lista i hela landet. Det är möjligt att både rösta och kryssa en kandidat på alla röstningslokaler.

Väljare: Tack!

Valarbetare: Okej, då kan jag lita på att du går och röstar i valet nu? (Ta gärna i hand)

Väljare: Absolut!

4. Avslut

Valarbetare: Då får jag tacka för ett väldigt trevligt samtal, ha en fortsatt trevlig kväll!

Glöm inte att rapportera in samtalet i SAPPEN!

VÅRA POLITISKA BUDSKAP

Vår viktigaste fråga

Kort: Jobben. Med 26 miljoner människor utan jobb i Europa är det dags att sätta rättvisa villkor och fler jobb först.

Mellan: Vi sätter jobben först. Arbetslösheten har brett ut sig både i Europa och i Sverige. Med fler utan jobb pressas löner och villkor också för de som jobbar. Vi vill ha ett EU som fokuserar på rätt saker – rättvisa villkor och fler jobb.

Lång: Vi sätter jobben först – i Sverige och i EU. Det är ett historiskt misslyckande att 26 miljoner människor, av vilka nästan 6 miljoner är ungdomar, idag saknar jobb. Även de utan jobb drabbas när löner pressas ner och arbetsvillkor försämrats. Vi vill se gemensamma investeringar i forskning och klimatsmart teknik istället för ensidiga åtstramningar som inte fungerar och vi vill sätta stopp för lönedumping och sämre villkor. Vi vill ha ett EU som fokuserar på rätt saker – rättvisa villkor, fler jobb och ansvar för miljön.

Vilka är våra tre viktigaste frågor?

Vi vill ha ett EU som fokuserar på rätt saker – rättvisa villkor, fler jobb och ansvar för miljön. Arbetslösheten har brett ut sig både i Europa och i Sverige. Med fler människor som saknar jobb pressas löner och villkor också för de som jobbar. Vi vill se gemensamma investeringar i forskning och grön teknik istället för ensidiga åtstramningar som inte fungerar och vi vill sätta stopp för lönedumping och sämre villkor. Svenska avtal och villkor ska gälla alla som jobbar i Sverige, oavsett varifrån man kommer. Att investera i ny miljövänlig teknik är att investera i framtidens jobb och välfärd samtidigt som vi tar ansvar för framtida generationer.

Budskap rättvisa villkor

När arbetslösheten breder ut sig både i Europa och i Sverige drabbas även de med jobb. Löner dumpas och anställningsvillkor försämrats. Människor utnyttjas. Högern i EU använder krisen och den fria rörligheten i EU som förevändning för att luckra upp tryggheten på arbetsmarknaden. Det vill vi sätta stopp för. Vi vill förhindra att seriösa företag slås ut av andra som kringgår lagar och regler. Svenska avtal och villkor ska gälla alla som jobbar i Sverige, oavsett varifrån man kommer.

RÄTTVISA VILLKOR – VI VILL:

Skydda anställdas fackliga fri- och rättigheter genom att skriva in ett socialt protokoll i EU:s grundlag.

Skapa en strategi för bättre arbetsmiljö i hela EU. Företag ska inte konkurrera med dålig arbetsmiljö.

Den som är ansvarig för ett projekt, till exempel ett bygge, ska genom att införa huvudentreprenörsansvar göras ansvarig för arbetsmiljön och villkoren för alla som jobbar där.

Budskap Fler jobb

Det finns 26 miljoner arbetslösa i EU, varav nästan 6 miljoner är ungdomar. Det är ett historiskt misslyckande. Vi måste sätta jobben högst på dagordningen i EU igen – där är de inte idag.

FLER JOBB – VI VILL:

Se till att unga får jobb, utbildning eller praktik inom fyra månader genom att genomföra den gemensamma ungdomsgarantin.

Investera i forskning och innovationer för ökad tillväxt.

Bygg ut och framtidssäkra det europeiska järnvägsnätet.

Budskap Ansvar för miljön

Klimatomställningen är en av vår tids största utmaningar. EU måste leda arbetet, även globalt. Men samarbetet haltar i miljöfrågan, både mellan länderna i Europa liksom mellan EU och övriga världen.

ANSVAR FÖR MILJÖN – DÄRFÖR VILL VI:

Investera i miljöteknik som ger nya jobb samtidigt som vi tar ansvar för kommande generationer.

Få EU att gå i spetsen för ett nytt internationellt klimatavtal genom egna bindande mål för halverade utsläpp, ökad andel förnybar energi och energieffektivisering.

Förbjuda farliga kemikalier som skadar människor och miljö.

SÄGNINGAR SOM FUNGERAR VÄL

EU måste fokusera på rätt saker, mindre byråkrati och mer jobb.

I Sverige ska svenska kollektivavtal gälla – oavsett var du kommer ifrån.

Ett stopp för lönedumpning och usla arbetsvillkor i EU är inte bara viktigt för några få – utan en fråga om anständighet för oss alla.

EU kan bara konkurrera med andra regioner genom höjd kunskap och kompetens, inte lägre löner och sämre arbetsvillkor.

Vi behöver investera i forskning, utbildning och infrastruktur för att bryta massarbetslösheten i Europa. Det skapar exportmöjligheter och jobb här hemma i Sverige.

Klimatomställningen är vår tids största utmaning. Vår generation och EU måste ta sitt ansvar. Det skapar jobb och ett bättre liv för kommande generationer

MER INFORMATION OM SAMTALSKAMPANJEN

För mer information, samt för tillgång till manualer och samtalsstöd, gå in på **www.socialdemokraterna.se/valarbetare**

För erfarenhetsutbyte mellan arbetarekommuner och partidistrikt finns en särskild facebookgrupp för valledare och ansvariga för samtalskampanjen. Gruppen är tillsvidare sluten. Inbjudningar kommer att skickas ut per e-post, men det går också bra att ansöka om att gå med i gruppen via **www.facebook.com/groups/samtalskampanj/**

Om ni har frågor eller funderingar tveka inte att kontakta partistyrelsens kansli och de personer som arbetar med samtalskampanjen.

KONTAKT SAMTALSKAMPANJEN

Johan Öhrn

johan.ohrn@socialdemokraterna.se

Åsa Olsson

asa.olsson@socialdemokraterna.se

Peter Åhlberg,

peter.ahlberg@socialdemokraterna.se

Toomas Almqvist

toomas.almqvist@socialdemokraterna.se



Socialdemokraterna
FRAMTIDSPARTIET