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]

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[18]While 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
Municipality								
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
Electoral district								
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

| | Linear | | | | Probit | | | |
| | Bivariate | | Covariates | | Bivariate | | Covariates | |
Subgroup	Effect	SE	Effect	SE	Effect	SE	Effect	SE
Full sample	3.9**	1.9	3.6**	1.6	9.6*	4.9	11.0**	5.3
Voting history								
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
Housing								
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
Household size								
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
Gender								
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
Age								
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
Land of birth								
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 effects 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)
Voter			
Voted 2009	0.454***	0.449***	0.454***
	(0.023)	(0.022)	(0.023)
Voted 2010	0.223***	0.229***	0.223***
	(0.027)	(0.026)	(0.027)
First-time voter	0.260***	0.298***	0.265***
	(0.077)	(0.073)	(0.077)
Age	0.004	0.005	0.003
	(0.004)	(0.004)	(0.005)
Age squared	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Female	0.001	0.001	0.001
	(0.018)	(0.017)	(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	-0.105	0.019
	(0.131)	(0.136)	(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	0.001	0.001	
	(0.002)	(0.002)	(0.003)	
Contact attempts		−0.012		
		(0.017)		
Visited				0.069
				(0.042)
Days left, first attempt				0.002
				(0.001)
Visited × Days left				−0.003
				(0.003)
Voted 2009	0.437***	0.437***	0.442***	0.467***
	(0.020)	(0.021)	(0.020)	(0.011)
Voted 2010	0.202***	0.202***	0.201***	0.170***
	(0.024)	(0.024)	(0.023)	(0.012)
First-time voter	0.279***	0.279***	0.281***	0.332***
	(0.056)	(0.056)	(0.055)	(0.027)
Age	0.006*	0.006*	0.006*	0.011***
	(0.003)	(0.003)	(0.003)	(0.002)
Age squared	−0.000**	−0.000**	−0.000**	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Female	−0.006	−0.006	−0.009	−0.014**
	(0.012)	(0.012)	(0.012)	(0.007)
Constant	−0.127	−0.120	0.039	−0.114**
	(0.119)	(0.120)	(0.104)	(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

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[19]It 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).

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

[20]The 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 systematical 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 Control	Turnout Treat	Bivariate Effect	SE	Covariates Effect	SE
Full sample	11475	40.1	44.9	47.2	5.8**	2.9	5.3**	2.4
Voting history								
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
Dwelling								
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
Household size								
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
Gender								
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
Age								
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
Land of birth								
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
Voting history								
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
Dwelling								
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
Household size								
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
Gender								
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
Age								
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
Land of birth								
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)
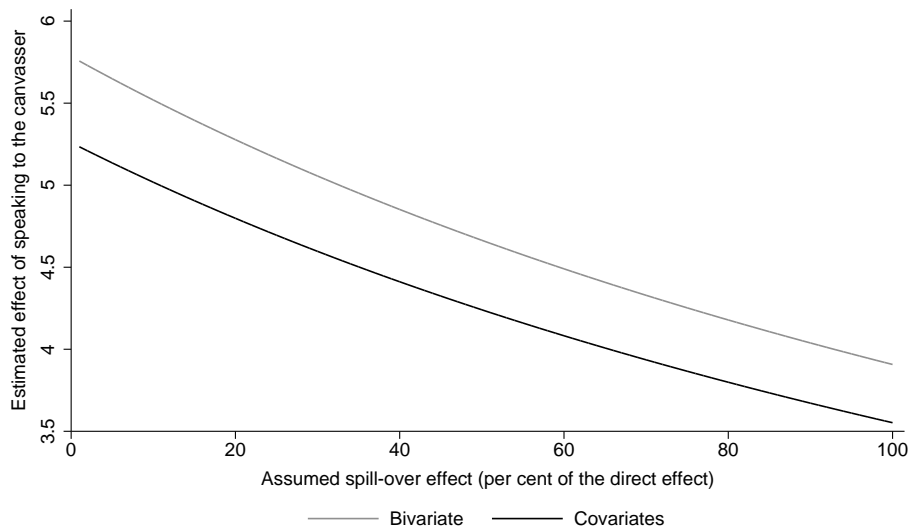
Figure A1: The effect of direct contact under different spill-over assumptions

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.

SAMTALS-KAMPANJEN

SAMTALET VID DÖRREN
FÖR DIG SOM VALARBETARE
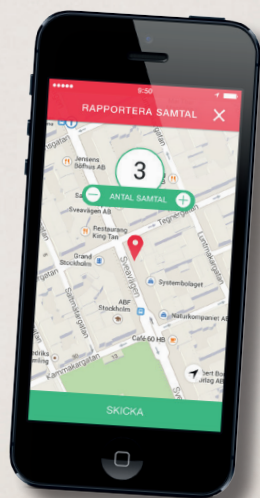
Socialdemokraterna
FRAMTIDSPARTIET

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 intres-serade av det. Se det som en självklarhet att personen ska gå med om den är uppen-bart 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 försämras. 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 försämras. 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.

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