Supplementary Material to "Voting early means voting alone"

For Online Publication

S1 Data and empirical specifications

We use individual-level data sourced from Swedish registers. This data repository is securely stored on an encrypted server, and access is exclusively granted through a remote desktop application for all our data analyses. Given the confidential nature of this information, we are bound by contractual and ethical obligations, prohibiting the distribution of this data to external parties. Consequently, we sought an exception from the journal's data and replication policy during our initial submission.

Researchers aiming to replicate our findings have two avenues for accessing the administrative data. The first method involves directly ordering the data from Statistics Sweden (SCB), necessitating approval from The Swedish Ethical Review Authority. Alternatively, to reproduce our analyses, researchers can visit Sweden and analyze the data through the same remote server system we employed. Those interested in this option should contact us in advance so that we can initiate the process of obtaining approval from the Ethical Review Board. This step is mandatory to temporarily include the researcher in our research team, facilitating access to the remote server system.

Our dependent variable, a binary indicator for voting in 2019, was provided by Statistics Sweden, which has recently commenced the production of population-wide data on individual-level turnout. To facilitate the spillover analysis, we initially generate variables to identify household members and colleagues. Household measurement relies on the household ID variable from the 2018 wave of the ´´Longitudinal integrated database for health insurance and labour market studies" (LISA), where a household is defined as individuals living in the same place, related either as partners (through marriage, partnership, or joint children—adopted or biological) or as child and parent (with the "parent" encompassing biological, adoptive, another guardian, or foster parent).

Statistics Sweden supplies a workplace indicator grouping individuals working at the same address for the same employer. Recognizing that these workplaces can be sizeable, we refined this indicator to create groups more likely to interact frequently. No adjustments are made for workplaces with ten or fewer employees. For larger workplaces, we create sub-units based on individuals' three-digit occupational codes, excluding individuals from workplaces where more than 50 employees share the same occupational code (thus treating the workplace variable as missing data). Data on workplace (variable KU1CFARNr) and occupational category (three-digit SSYK, the Swedish equivalent of ISCO) are also sourced from the LISA database.

Thanks to the extensive dataset at our disposal, we can include a comprehensive set of covariates into the regression models, enhancing the precision of our estimates. Statistics Sweden has supplied data on turnout in 2018. Prior to this, population-wide data on turnout was not available. In a recent initiative, we compiled such data for the 2009 European Parliament election and the 2010 general election by scanning and digitizing the electoral rolls (see the appendix of Lindgren et al. (2019) for a description of this procedure). Using these turnout data, coupled with the multi-generation registry, we construct variables for the individual's, the father's, and the mother's turnout in 2009, 2010, and 2018. Additionally, we create a variable measuring the average 2019 turnout in the electoral district.

Information regarding birth year, gender, and foreign background (foreign-born or having both parents foreign-born) is sourced from the Swedish Population Register. Leveraging the LISA indicator mentioned earlier, we compute household size. Moreover, using the same database, we construct income deciles based on disposable income per consumption unit. Additionally, we derive variables measuring field of education (SUN 2000, 1 digit) and years of education (SUN 2000 with levels 1/2/3/4/5/6/7 corresponding to 7.5/9.4/11.2/12.4/14.2/17.0/20.4 years).

H1

To estimate the *direct* effect, we regress the binary voting indicator (v_i) on an intercept and an indicator for belonging to the treatment group (T_i), and a vector of control variables ($\boldsymbol{\chi}_i$). These control variables include birth year, income decile, district turnout, household size, gender, field and length of education, foreign background, as well as binary indicators for whether the individual and their parents voted in 2009, 2010, and 2018. The model is depicted in Equation 1, and H1 is tested through β to ascertain whether it is significantly different from zero.

$$v_i = \alpha + \beta T_i + \boldsymbol{\beta \chi_i} + \epsilon_i \tag{1}$$

H1 implies that $\beta > 0$. For transparency, we will also present results from a specification without this vector of control variables. However, we expect the control variables to reduce the standard errors with approximately 15–20 percent, and therefore we prefer to include them in the model.

H2a and H2b

When estimating the direct effect, we know that everyone in our sample had the same (pre-randomization) probability of being treated. However, for estimating the spillover effect, we must consider that the probability of being a colleague or household member to an individual in the treatment group depends on the number of colleagues or household members one had in the sampling frame used during the randomization into the treatment and control groups. To account for this, we regress individual-level turnout on the number of treated colleagues (T_i^W) and household members (T_i^F), as well as the number of household members or colleagues in the sampling frame (SF_i^W and SF_i^F). We will include T_i^W and T_i^F linearly in the regression models, while SF_i^W and SF_i^F will be controlled for using fixed effects for each category. Similar to the direct effect, our preferred specification also incorporates the same vector of control variables used previously ($\boldsymbol{\chi}_i$).

As household and workplace contexts may be correlated — for instance, studies have indicated that many individuals meet their potential partners in the workplace (Kalmijn and Flap 2001; Pinder 2008) — we will estimate common models for co-workers and household members when examining spillover effects. The sample will include all individuals eligible to vote who had at least one colleague or at least one household member in the sampling frame. As a robustness check, we will also estimate separate models for co-workers and household members.

$$v_i = \alpha + \beta_1 T_i^F + \beta_2 SF_i^F + \beta_3 T_i^W + \beta_4 SF_i^W + \boldsymbol{\beta \chi_i} + \epsilon_i \tag{2}$$

Hypotheses H2a and H2b imply that $\beta_1 > 0$ (household) and $\beta_3 > 0$ (workplace).

H3a and H3b

Our next hypotheses focus on the impact of gender on voter mobilization within households and workplaces. Within households, we expect stronger spouse-to-spouse mobilization effects when the treated individual is a woman, given prior research indicating that women are more inclined to engage in political discussions with family members (Elder and Greene 2003). In workplaces, we anticipate that interactions will be more pronounced among colleagues of the same sex due to shared task involvement and homosocial preferences.

H3a Spouse-to-spouse spillovers are larger when the treated person is a woman.

H3b Spillovers between co-workers are larger when the treated and the person of interest are of the same sex.

When examining spillover effects with binary data on biological sex, there are four categories of interest: men-to-men, men-to-women, women-to-men, and women-to-women. To obtain parameters directly testing hypotheses H3a and H3b, we will include three treatment variables and three sampling frame variables per context (workplace or household): one where everyone is included, one where only women are included, and one where only individuals of the same sex as the outcome person are included.[1]

$$
\begin{aligned}
v_i = \alpha + \beta_1 T_i^F + \beta_2 T_i^{F,fem} + \beta_3 T_i^{F,same} + \beta_4 SF_i^F + \beta_5 SF_i^{F,fem} + \beta_6 SF_i^{F,same} + \\
\beta_7 T_i^W + \beta_8 T_i^{W,fem} + \beta_9 T_i^{W,same} + \beta_{10} SF_i^W + \beta_{11} SF_i^{W,fem} + \beta_{12} SF_i^{W,same} + \\
\boldsymbol{\beta \chi_i} + \epsilon_i
\end{aligned} \tag{3}
$$

Our hypotheses H3a and H3b imply that $\beta_2 > 0$ (household) and $\beta_9 > 0$ (workplace) in Equation 3. We can also calculate the estimated effects for men-to-men ($\beta_1 + \beta_3$ (household) and $\beta_7 + \beta_9$ (workplace)), men-to-women (β_1 (household) and β_7 (workplace)), women-to-men ($\beta_1 + \beta_2$ (household) and $\beta_7 + \beta_8$ (workplace)) and women-to-women ($\beta_1 + \beta_2 + \beta_3$ (household) and $\beta_7 + \beta_8 + \beta_9$ (workplace)).

We do not see a need for interacting the control variables with any of the treatment variables. If we find that men and women are affected in different ways, or that the effect is stronger when both persons are of the same sex, it is, of course, possible that those differences are caused by observable characteristics that differ between men and women, such as their income or level of education. However, those characteristics would then act as mediators rather than confounders for the gender differences.

H4a and H4b

Previous spillover studies have been limited in determining whether effects on indirectly affected individuals are due to direct interaction with treated individuals or because these non-treated individuals are also exposed to the treatment, for example by the child's phone number being registered on the parent or two partners reading their mail together. To address this, our study delivers text messages at two distinct times: noon (when most are at work with colleagues) and in the evening (when they are likely with family). This approach aims to discern if spillover effects are caused by interactions between the treated person and their peers, which would suggest a positive spillover effect even when the second person is less likely to be directly exposed to the treatment. This forms the basis of our hypotheses.

[1] An alternative strategy would be to interact the sex of the outcome person with the number of treated male and female peers, respectively. However, this would have led to the moderating effect of the recipient being of the same sex as the outcome person being estimated separately for men and women, which would no longer correspond to the more general hypothesis that spillovers are larger when both respondents are of the same sex.

Prior spillover studies have faced challenges in differentiating whether effects on indirectly affected individuals result from direct interaction with treated individuals or if these non-treated individuals are also exposed to the treatment through alternative means, such as the child's phone number being registered on the parent's device or two partners reading their messages together. To overcome this limitation, our study deploys text messages at two distinct times: noon (when most individuals are at work with colleagues) and in the evening (when they are likely to be with family). This approach aims to discern if spillover effects arise from interactions between the treated person and their peers, suggesting a positive spillover effect even when the second person is less likely to be directly exposed to the treatment. This constitutes the foundation of our hypotheses.

H4a Receiving the text message has a positive effect on the turnout among household members also if the message was delivered at noon.

H4b Receiving the text message has a positive effect on the turnout among colleagues also if the message was delivered in the evening.

To test Hypotheses H4a and H4b we want to parameterize the regression models with one coefficient for the difference between the two time-points (β_2 for familes and β_6 for workplaces) and one coefficient for the effect when the second person is less likely to be exposed to the treatment (β_1 for familes and β_5 for workplaces). Because the latter corresponds to noon for household members and the evening for colleagues, we will run the following two regressions:

To examine Hypotheses H4a and H4b, we parameterize the regression models with one coefficient representing the difference between the two time points (β_2 for families and β_6 for workplaces) and another coefficient for the effect when the second person is less likely to be exposed to the treatment (β_1 for families and β_5 for workplaces). As the latter corresponds to noon for household members and the evening for colleagues, we will run the following two regressions:

$$\begin{aligned} v_i = \alpha + \beta_1 T_i^F + \beta_2 T_i^{F,evening} + \beta_3 SF_i^F + \beta_4 SF_i^{F,evening} + \\ \beta_5 T_i^W + \beta_6 T_i^{W,noon} + \beta_7 SF_i^W + \beta_8 SF_i^{W,noon} + \boldsymbol{\beta\chi_i} + \epsilon_i \end{aligned} \quad (4)$$

We can then map the tests of our hypotheses to the corresponding tests of regression coefficients, with H4a and H4b tested by β_1 (household) and β_5 (workplace) in Equation 4. We expect $\beta_1 > 0$ and $\beta_5 > 0$.

S2 Pre-registered analyses

In this section, we present the results from analyses specified in our pre-analysis plan. These include a table with balance tests, the regression table for the main results, as well as the same regression table without covariates other than those required for identification. Due to space restrictions, we have excluded the second balance test (p. 8 in the PAP) and most of the robustness tests (p. 13–14 in the PAP) from the pre-analysis plan.

Our balance tests indicate that the allocation to treatment and control worked well. None of the tested variables show a statistically significant difference between the treatment and control groups. When regressing the treatment indicator on the same variables used in our main specification, an F-test cannot reject the null hypothesis of joint orthogonality ($p = 0.59$, $R^2 = 0.0000$).

In Table S2, we present the regression table behind the coefficient plot presented in the main paper, including results for the excluded hypotheses. The first two columns repeat the findings regarding hypotheses 1, 2a, and 2b. In the first column, we observe that receiving the text message increased turnout by an estimated 0.3 percentage points (CI 0.1–0.5). As noted in the paper, this effect is smaller than what has usually been found in previous research. In the second column, turnout is estimated to increase by 0.15 percentage points for each household member that receives a text message. However, the effect is not statistically significant. There do not appear to be any spillover effects between colleagues, and the narrow confidence interval even allows us to reject effects larger than 0.1 percentage points.

The other two columns present the results for hypotheses 3a–4b. According to hypotheses 3a and 3b, spillovers are expected to be larger when the treated household member is a woman and when colleagues are of the same sex. However, as we can see in the third column, these hypotheses are not supported in our analysis. In the fourth column, we test whether spillovers only take place when the person affected by the spillover is likely to be present when the text message was delivered (at lunch for colleagues and in the evening for household members). The results indicate that this may be the case because the estimated lunch effect for household members is close to zero, and the evening effect for colleagues is even negative, but the small baseline effect means that we cannot say for sure.

Table S3 presents the results for the pre-registered hypotheses but without the vector of covariates we use in our main specification. The results are quite similar to the main results, which is to be expected given our sample size and that the balance tests went well. Both the direct treatment effect and the household spillover are 12–13 percent larger than what was estimated in the specification with covariates, and the standard errors increase by 17 and 14 percent, respectively. The most significant difference is found in the fourth column. When not including any covariates in the model, we no longer find that messages sent in the evening are substantially more likely to mobilize other household members.

Table S1: Balance test

Variable	Control	Treat	Dif	p
Voted 2009	37.26	37.35	0.09	0.36
Mother voted 2009	32.58	32.66	0.08	0.45
Father voted 2009	28.39	28.30	−0.10	0.32
Voted 2010	72.56	72.65	0.10	0.31
Mother voted 2010	59.34	59.39	0.05	0.65
Father voted 2010	49.99	49.88	−0.11	0.31
Voted 2018	90.75	90.75	−0.00	0.99
Mother voted 2018	54.11	54.04	−0.06	0.55
Father voted 2018	45.11	45.04	−0.06	0.55
District turnout	56.46	56.47	0.01	0.81
Female	0.52	0.53	0.00	0.86
Income decile	6.13	6.13	−0.00	0.93
Family size	1.83	1.83	0.00	0.80
Years of education	13.14	13.13	−0.00	0.51
Birthyear	1971.01	1970.95	−0.06	0.12

Note: The table shows the mean value for the treatment and control group, the difference between the two and the p-value for the null-hypothesis of no difference.

Table S2: Main results

	(1)	(2)	(3)	(4)
Treated directly	0.302^{***}			
	(0.090)			
Family members		0.154	0.188	0.044
		(0.096)	(0.145)	(0.126)
Colleagues		−0.003	−0.072	−0.065
		(0.042)	(0.086)	(0.059)
Female family members			−0.111	
			(0.209)	
Same-sex family members			0.112	
			(0.246)	
Female colleagues			0.063	
			(0.085)	
Same-sex colleagues			0.036	
			(0.085)	
Family members (evening)				0.220
				(0.198)
Colleagues (lunch)				0.123
				(0.081)
Observations	3,006,062	5,035,713	5,035,713	5,035,713

Note: These regression results correspond to the coefficient plot in the main paper (Figure 1). The stars correspond to hypothesis-tests without adjustment for multiple comparisons. Our hypotheses are tested through the regression coefficients for being treated directly (H_1), and for being indirectly treated through household members and colleagues (H_2 and H_4) and by female household-members and same-sex colleagues (H_3). In the fourth column, the coefficients for household members and colleagues correspond to the treatment effect at the time-point when they were the least likely to be around (at lunch for household members and in the evening for colleagues).

Table S3: Without vector of covariates

	(1)	(2)	(3)	(4)
Treated directly	0.341^{***}			
	(0.105)			
Family members		0.189^{*}	0.231	0.166
		(0.109)	(0.156)	(0.159)
Colleagues		-0.038	-0.267^{*}	-0.025
		(0.084)	(0.152)	(0.117)
Female family members			-0.194	
			(0.221)	
Same-sex family members			0.314	
			(0.281)	
Female colleagues			0.285^{*}	
			(0.159)	
Same-sex colleagues			0.085	
			(0.127)	
Family members (evening)				0.045
				(0.251)
Colleagues (lunch)				-0.025
				(0.161)
Constant	59.186^{***}	58.397^{***}	58.401^{***}	58.397^{***}
	(0.033)	(0.039)	(0.038)	(0.039)
Observations	3,006,062	5,035,718	5,035,718	5,035,718

Note: These are our main results but without the vector of controls. Asterisks denote unadjusted p-values.

S3 Additional results

The analyses presented in this section were not specified in our pre-analysis plan, but are included because they are valuable when interpreting the main results. These results consist of a subgroup analysis to identify heterogeneity in the direct treatment effect (Table S4), an analysis of whether the direct effects differed between those who received the message at lunch and those who did so in the evening (Table S5), an analysis of how the spillover effects differed between lunch and evening, across groups with different vote propensity (Table S6), and similar rolling regression graphs for the workplace spillovers as we presented for the household spillovers in the main text (Figure S2).

Starting with the heterogeneous treatment effects presented in Table S4, we see that the impact of receiving a text message was larger among those with a low vote propensity and those who voted in only one of the three preceding elections. This result is discussed in more detail in the main paper. Surprisingly, we find smaller effects among young people (aged 18–30) than others, but the difference is not statistically significant. The largest effect, using these categories, is found in the group with a foreign background, where the text message was estimated to increase turnout by 0.84 percentage points.

In Table S5, we present how the direct effects differ depending on whether the message was sent at lunch or in the evening. The first column displays the main effect, while the second column shows the estimated effects separately for lunch and evening. The table also includes the difference between the two effects and a test of whether we can reject the null hypothesis of no difference. As we can see, the estimated effect is approximately three times as large when the messages were sent out in the evening, but the difference is only bordering on being statistically significant.

Similar to the rolling regressions presented in Figure 2, Figure S6 shows how the spillover effects differ depending on a person's vote propensity (low in Column 1 and 4, medium in Column 2 and 5, and high in Column 3 and 6) and the timing of the message (Column 4–6). What we observe is a negative spillover effect among those with a vote propensity below 40 percent (Column 1), which is driven by a negative effect twice as large when the message was sent out at lunch (Column 4). This supports the idea that some people who receive the text message while at work cast an early vote, instead of voting with their family on Election day, as we speculate in the main paper.

Related to this, and as noted in the main text, one potential explanation for the negative spillover effect among partners with low vote propensity could be that a voting partner's decision to vote early, rather than on Election day, has a greater impact on their turnout. This hypothesis is supported by the results presented in Figure S1, which utilize data from the Swedish election to the European Parliament in 2009, a context where we have access to information on early voting. The solid line indicates the difference in voter turnout between individuals who have a partner who voted on Election day and those with a partner who voted early. As can be seen, the impact of having a partner who votes on Election day is largest for individuals with low vote propensity. For instance, for individuals with a vote propensity below 40 percent, the average difference is about 6 percentage points, whereas the corresponding figure is about 4 percentage points for those with a vote propensity above 60 percent.

Table S4: Sub-group analysis of the direct effect

Subgroup	Obs	Turnout	Bivariate		Covariates	
			Effect	SE	Effect	SE
Full sample	3,006,062	59.19	0.34	0.10	0.30	0.09
Vote propensity						
Below 40 percent	531,971	22.09	0.73	0.21	0.59	0.20
40–60 percent	998,500	47.94	0.29	0.19	0.31	0.18
Above 60 percent	1,475,591	80.17	0.20	0.12	0.21	0.12
District turnout						
Below 40 percent	164,193	39.27	−0.12	0.45	−0.21	0.39
40–60 percent	1,788,822	54.59	0.36	0.14	0.28	0.12
Above 60 percent	1,053,047	70.09	0.40	0.16	0.44	0.14
Voting history						
None	85,259	2.77	0.13	0.21	0.12	0.21
One election	205,291	23.60	0.76	0.35	0.84	0.34
Two elections	957,175	51.79	0.38	0.19	0.40	0.18
All three elections	1,042,557	84.95	0.12	0.13	0.12	0.12
Sex						
Female	1,577,850	61.14	0.38	0.14	0.34	0.12
Male	1,428,212	57.03	0.30	0.15	0.26	0.13
Age						
18–30 years old	593,216	46.45	0.01	0.24	0.07	0.21
30–50 years old	1,041,742	57.43	0.49	0.18	0.45	0.15
Older than 50	1,371,104	66.03	0.34	0.15	0.29	0.13
Immigrant background						
Foreign background	403,790	43.38	0.84	0.29	0.69	0.25
Swedish background	2,602,272	61.64	0.26	0.11	0.24	0.10
Education						
SUN 1–3	998,568	50.30	0.25	0.19	0.23	0.16
SUN 4–6	1,964,196	63.40	0.43	0.13	0.35	0.11
SUN 7	32,044	86.85	−0.82	0.72	–	–

Note: The table shows the estimated direct treatment effects across different samples.

Table S5: Direct treatment effect divided by lunch and evening

	(1)	(2)
Treated directly	0.302***	
	(0.090)	
Treated directly at lunch		0.144
		(0.125)
Treated directly in evening		0.460***
		(0.125)
Difference		0.316
Standard error		0.173
p-value		0.068
Observations	3,006,062	3,006,062

Direct treatment effect divided by lunch and evening.

Figure 2 showed the household spillover effects across different vote propensity intervals. For completeness, Figure S2 shows the same estimates for workplace spillovers. The estimated spillover effects remains close to zero for all levels of vote propensity.

Table S7 shows the results from alternative ways of estimating the spillover effects for households and workplaces. The first column is identical to the results presented in the main paper, using continuous measures for the number of treated peers, including both indicators in the same model. The primary reason we prefer this specification is that if there is only a small probability that people who receive our message will let other people know about it, then the probability that a person has actually been reminded about the election by at least one person can reasonably well be approximated as a linear function of the number of peers who received the message. In the second column, we have dichotomized these variables to indicate whether there was at least one household member or colleague that received the text message, and in the last two columns, we use the continuous variable but run separate models for households and workplaces. The results are relatively stable across the specifications, although the marginally larger effect for the dichotomous indicator for household spillovers (the effect of having at least one treated household member) is, of course, not directly comparable to that of the continuous variable (the effect per additional treated household member).

The text message was delivered at six different time points: at noon or in the evening, with four, three, or two days remaining until the election. After the first day, or the two first batches, the University's name was removed from the message. Figure S3 shows the estimated effects for the six different batches. Both the direct effect and the household spillover appear to be larger for messages sent in the evening; for both treatments and for all delivery days, we estimate a slightly larger effect for these batches compared to the messages sent at noon the same days. There are no clear signs that the time to the election or the alteration of the message impacted the mobilizing effect. The results for household spillovers are in line with a

Table S6: Sub-group analysis of the spillover effect

	(1)	(2)	(3)	(4)	(5)	(6)
Family members	−0.453**	0.238	0.258**			
	(0.181)	(0.173)	(0.120)			
Colleagues	0.047	−0.034	−0.003			
	(0.082)	(0.075)	(0.053)			
Family members (lunch)				−0.900***	0.025	0.320**
				(0.255)	(0.249)	(0.148)
Family members (evening)				−0.012	0.450*	0.196
				(0.253)	(0.241)	(0.194)
Colleagues (lunch)				0.144	−0.026	0.080
				(0.113)	(0.103)	(0.073)
Colleagues (evening)				−0.051	−0.041	−0.087
				(0.114)	(0.104)	(0.074)
Observations	903,277	1,704,225	2,428,208	903,277	1,704,225	2,428,208
Vote propensity	<40	40−60	>60	<40	40−60	>60

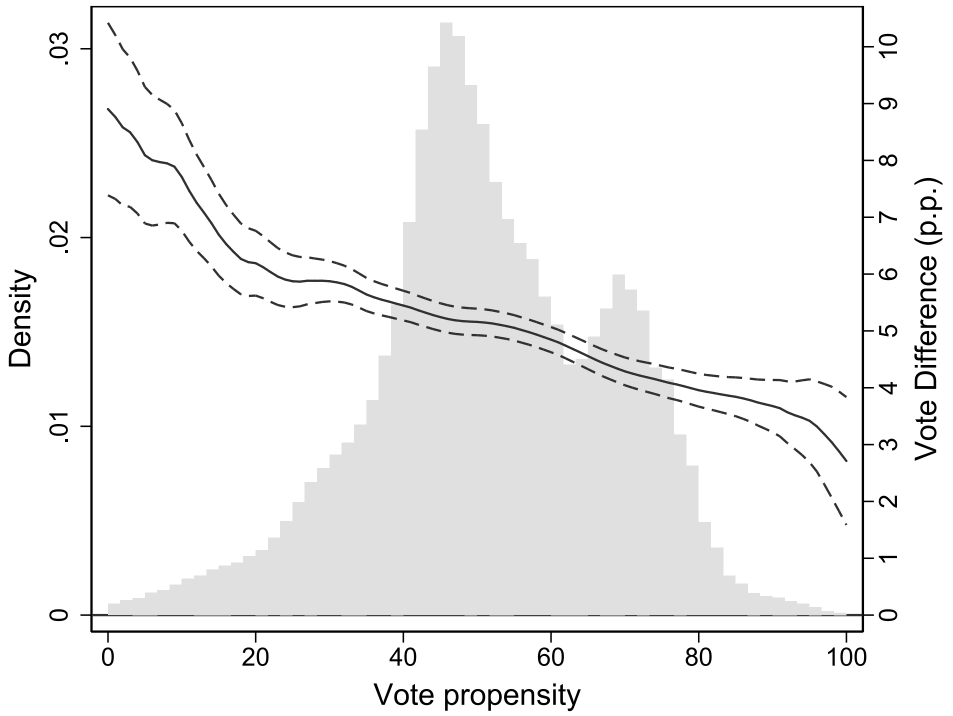
Note: The table shows how the estimated spillover effects within household and workplace differ across vote propensity and timing of the message.

Table S7: Other definitions of the spillover effect

	(1)	(2)	(3)	(4)
Family members	0.154		0.155	
	(0.096)		(0.096)	
Colleagues	−0.003			−0.003
	(0.042)			(0.042)
At least one family member		0.179*		
		(0.098)		
At least one colleague		−0.093		
		(0.070)		
Observations	5,035,713	5,035,713	5,035,713	5,035,713
Model	Main	Binary	Separate	Separate

Note: The table shows how the estimated spillover effects within household and workplace differ across definitions.

Figure S1: Effect of the partner voting on election day over different vote propensities



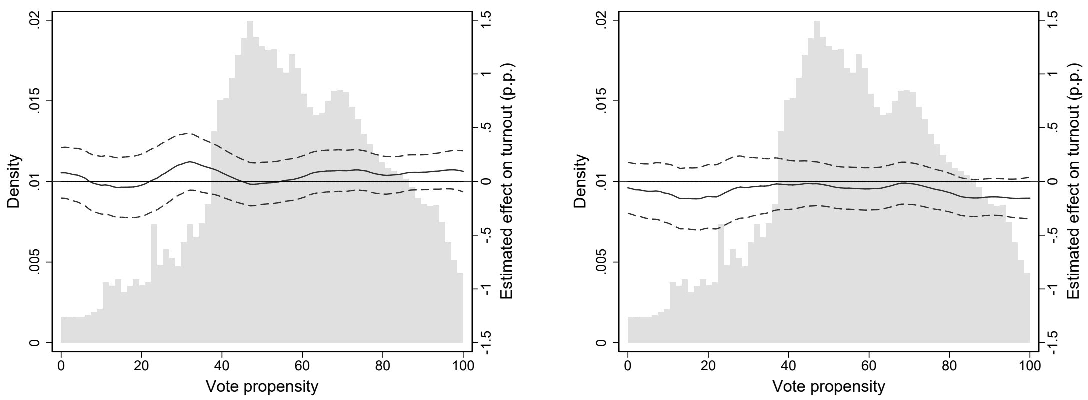
Note: The graph displays the difference in voter turnout between individuals who have a partner who votes on Election Day and those who have a partner who vote early, using rolling regressions for different vote propensity intervals, with a window of ± 15 percentage points and triangular weights (decreasing linearly with the distance from the middle of the window). The histograms show the distribution of vote propensities for the respective samples. The vote propensities are estimated using information on: birthyear, years and type of education, sex, immigrant background, household size, income, parental voting in 1994, and district-level turnout.

negative effect of the text alteration in combination with a positive time trend. However, we see no similar pattern for the other effects, and it is unlikely that this – and only this – effect would be affected by both the sender of the text message and the time to the election.

Figure S4 illustrates how the estimated direct effect and firm spillovers vary with two workplace characteristics: the number of colleagues, as per our workplace definition, and the average vote propensity among these colleagues. The size of the spillover effect is close to zero, irrespective of the workplace size. The estimated direct effects are considerably larger for individuals working at small firms. However, we find it more plausible that this is due to confounding individual characteristics rather than the work context itself. Regarding vote propensity, we observe a similar asymmetry as in the main results, with positive direct effects and negative spillovers for firms with low vote propensity. Nevertheless, the standard errors are substantial, the difference is less pronounced than in the main paper, and it may be reflec-

Figure S2: Spillover effects over different vote propensities

(a) Workplace spillover at lunch (b) Workplace spillover in the evening

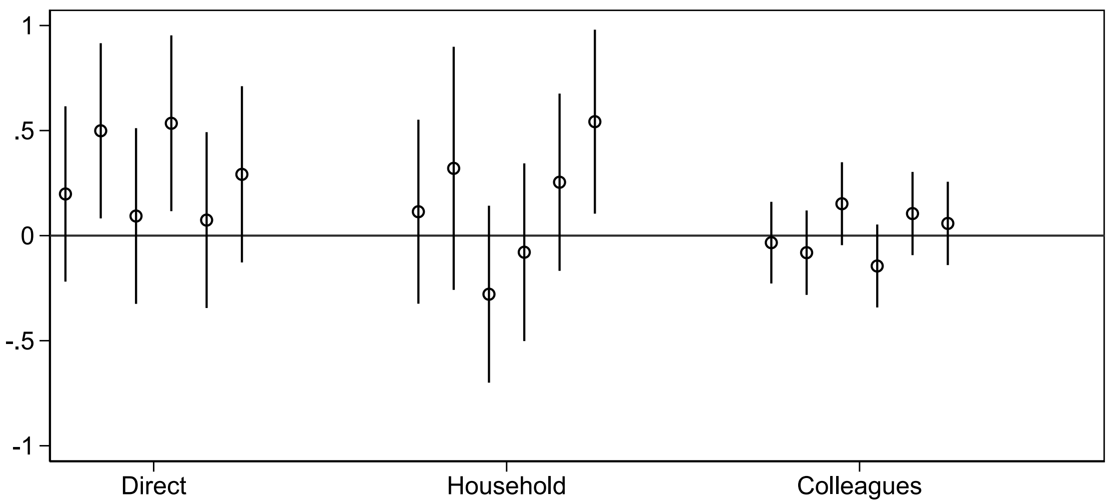


Note: The graphs display the workplace spillover effects, divided by the lunch delivery (left) and the messages delivered in the evening (right), using rolling regressions for different vote propensity intervals with a window of ± 15 percentage points and triangular weights. The histograms show the distribution of vote propensities for the respective samples.

tive of the positive correlation between the vote propensity of individuals and that of their colleagues ($r = 0.47$).

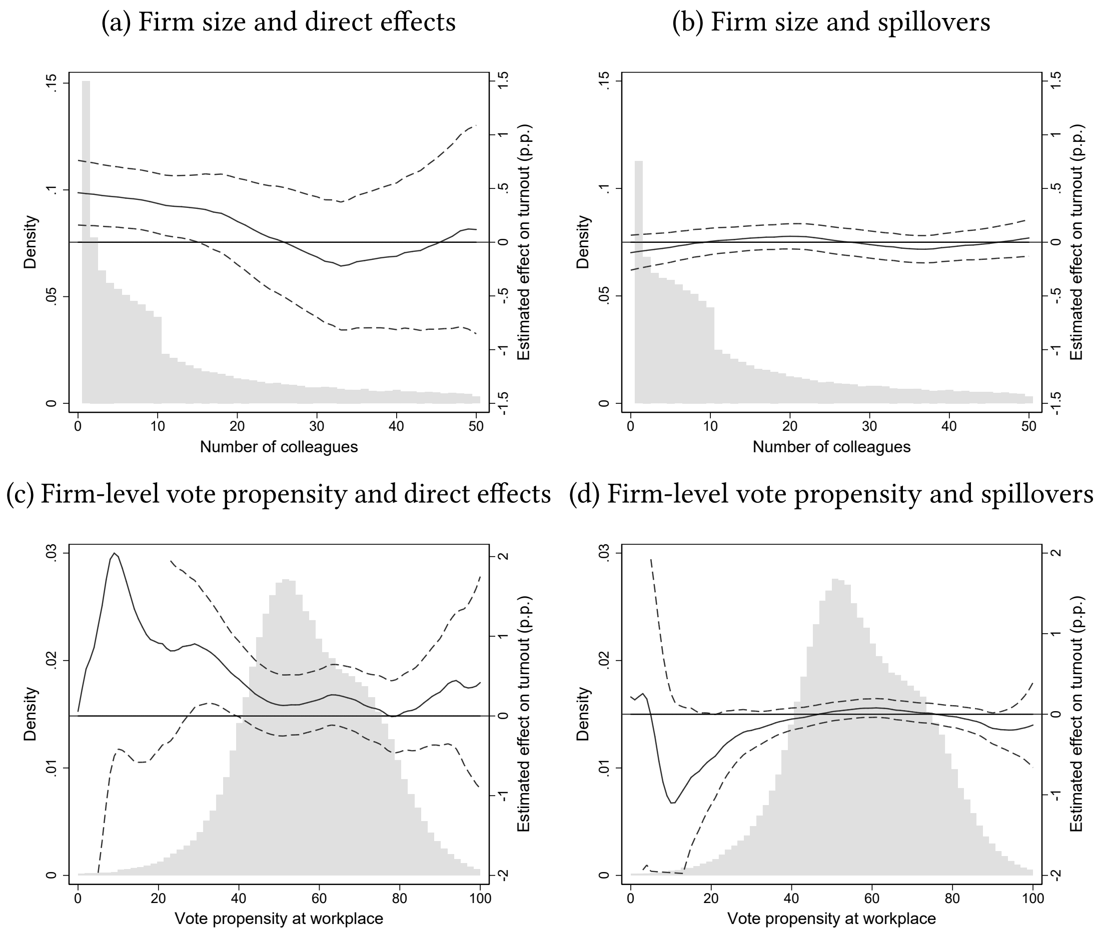
In Figure S5, we examine the impact on our results of varying the thresholds used in our workplace definitions. We modified our analysis by implementing different firm size thresholds (0, 5, 10, 15, and 20 employees) for when a firm is partitioned into smaller groups based on the three-digit occupational codes of their employees. Additionally, we applied thresholds of 25, 50, 75 and 100 employees, for when a workplace is too large to be included in the analysis, and one last specification where no workplaces were excluded. The left panel of Figure S5 reveals that altering firm size thresholds does not significantly affect the results, indicating a lack of sensitivity to these particular thresholds. In contrast, the right panel shows that adjusting the thresholds for workplace exclusion yields slightly more noticeable variations in the results. Although none of the estimated spillover effects are statistically significant, larger thresholds generate higher point estimates, with exception for when all workplaces are included regardless of size.

Figure S3: Treatment effects over time



Note: For each kind of treatment effect, the estimated effects are from noon four days before the election (the leftmost estimate in each cluster) to the evening two days before the election (the rightmost estimate in each cluster).

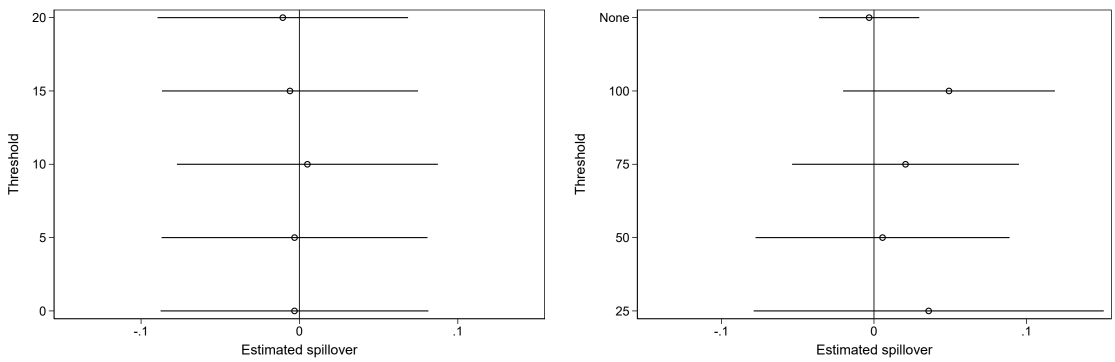
Figure S4: Treatment effects over different firm characteristics



Note: The graphs display the direct effect from receiving the text message (left panels) and the indirect effect from when a colleague member receives a text message (right panels) on voter turnout, using rolling regressions for different firm size and firm-level vote propensity intervals, with a window of ± 15 employees/percentage points and triangular weights (decreasing linearly with the distance from the middle of the window). The histograms show the distribution of firm size and firm-level vote propensity for the respective samples. The sharp decline at ten colleagues occurs because we partition workplaces with more than ten employees into smaller groups consisting of colleagues within the same occupation.

Figure S5: Workplace spillovers with different thresholds

(a) Occupation threshold (b) Exclusion threshold



Note: The graphs display the estimated spillover effects in the workplace across varying thresholds for our workplace definitions. In the left panel we have altered the firm size thresholds for when firms are partitioned into smaller groups based on occupational codes. In the right panel we have altered how large these groups can be before we exclude them from the analysis.

S4 Previous GOTV studies using text-message reminders

Previous GOTV experiments using text messages have been carried out in the US (Dale and Strauss 2009; Malhotra et al. 2011), Denmark (Bhatti et al. 2017), Norway (Bergh and Christensen 2022), and Moçambique (Grácio and Vicente 2021). As shown in Table S8, they have estimated treatment effects between 0.3 and 7.1 percentage points (intent-to-treat). A very simple meta-analysis would reveal that the inverse-variance weighted average effect is approximately 0.8 percentage points. However, those experiments that have been directed towards large proportions of the electorate – and not specifically aimed towards immigrants or young voters – have tended to result in smaller treatment effects. Besides, the main estimates chosen by the authors are sometimes larger than other estimates reported in the paper. For example, if we would have presented the estimates with covariates from Bergh and Christensen (2022) instead of the bivariate ones they report in the abstract, the ITT effect for the largest sample would have been 0.38 instead of 0.96. So while we would expect that also our experiment has a mobilizing effect on voter turnout, the average effect is likely to be smaller than the average effect reported in Table S8.

Table S8: Previous GOTV studies using text-message reminders

Study	Population	Observations	Base	ITT	SE	Pre-reg
Dale and Strauss (2009)*	Prior consent	8,053	55.9	1.1	3.0	No
Malhotra et al. (2011)*	November 2009	12,843	4.0	0.7	0.4	No
Malhotra et al. (2011)*	June 2010	29,673	8.9	0.9	0.3	No
Bhatti et al. (2017)*	Young	47,846	59.4	0.5	1.8	No
Bhatti et al. (2017)*	Almost representative	92,089	65.3	0.3	0.3	No
Bhatti et al. (2017)*	Young	54,694	62.7	0.4	0.7	No
Bhatti et al. (2017)*	Young	112,231	43.8	0.3	0.6	No
Bergh et al. (2021)*	Immig. first-timers	41,400	21.7	2.9	0.5	Yes
Bergh et al. (2021)*	Other immigrants	83,988	41.3	2.7	0.3	Yes
Bergh et al. (2021)*	Natives below 30	66,086	45.3	4.6	0.5	Yes
Bergh et al. (2021)*	Natives 30 or older	389,107	72.7	1.0	0.2	Yes
Grácio and Vicente (2021)	Moçambique survey	925	83.1	7.1	4.1	Yes
Bergh and Christensen (2022)	Almost representative	2,254,829	82.6	0.4	0.1	Yes
Shaw et al. (2022)	Evangelical Christians	97,023	61.2	2.9	1.2	No
Cheng-Matsuno et al. (2023)	UK non-registrants	493	0.6	2.7	1.2	No
Cheng-Matsuno et al. (2023)	UK prior consent	1,975	42.2	-0.2	2.8	Yes

Note: The table enumerates previous Get Out the Vote (GOTV) studies that utilized text-message reminders. We exclude studies in which the text-message treatment was combined with other interventions (e.g., LeRoux et al. (2023)), or non-randomized GOTV studies (e.g., Elvik Næss (2022)). Asterisks indicate studies that were known to us at the time of posting the pre-analysis plan.

S5 Ethical approval and justification for lack of informed consent

The APSA Principles on Human Subjects Research stipulate that "political science researchers should generally seek informed consent from individuals who are directly engaged by the research process, especially if research involves more than minimal risk of harm or if it is plausible to expect that engaged individuals would withhold consent if consent were sought". This aligns with the requirements for documented, voluntary, and informed consent that exist in the Swedish law pertaining to research ethics (Law 2003:460 on ethical review of research involving humans).

In general, for most types of research, obtaining informed consent is not only a necessary but also a highly significant requirement. However, in the context of studies based on population-wide registry data, it is unfeasible. This is partially because it's practically impossible to inform millions of individuals about the research, especially when most data were collected by Swedish agencies well before the research commenced. It's also due to the possibility that providing information about the study's objectives to subjects would likely influence their behavior, potentially creating bias and compromising the intent of the experiment.

On the other hand, it's worth noting that a text message intervention, such as used in our study, does not harm subjects. Furthermore, our utilization of Swedish registry data presents only a minimal invasion of privacy, given that these data already exist, and we do not have the ability to identify the individuals.

Given these considerations, we applied for and were granted ethical approval for this study, which includes an exemption from the informed consent requirement. The approval was granted by the Regional Ethics Review Board in Uppsala (ref. 2016/164 and 2016/164/2).

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