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How voter mobilization from short text messages travels within households and families: Evidence from two nationwide field experiments

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A B S T R A C T

Through two large GOTV field experiments in two different elections, we investigate the spillover effect to other household members and family members outside the household. We mobilized young voters with cell phone text messages, a campaign tactic unlikely to be observed by other persons than the treated. The direct effect varied but approximately 30 percent spilled over to other persons in the household, even parents. The effects are subtle and we cannot with certainty establish that a spillover effect exists. However, we demonstrate, using Bayesian updating, that even an initial skeptic becomes close to convinced that the effect spills over. Our study provides evidence by suggesting that young individuals' decision to vote affect other household members, including their parents, to do the same. When young voters live without their parents, we find no evidence of spillovers to parents, suggesting that households are more important than families ties for turnout contagion.

A large body of research has for decades suggested that citizens' decision to vote is influenced by their social environment (Campbell et al., 1960; Lazarsfeld et al., 1968; Fowler, 2005; Rolfe, 2012:4). Studies strongly indicate that individuals turnout decisions are influenced by their families (Wolfinger and Rosenstone, 1980; Stoker and Jennings, 1995; Bhatti and Hansen, 2012), members of their communities (Anderson, 2009), peers discussing politics with them (La Due Lake and Huckfeldt, 1998), people living in their neighborhood (Cho et al., 2006; Fieldhouse and Cutts, 2008) and individuals in their household (Fieldhouse and Cutts, 2012).

Though highly sophisticated, a sizeable proportion of these studies rely on observational data with its well-rehearsed limitations for causal inference. Recent research has focused on overcoming this problem and to provide better causal estimates of network effects. Particular convincing evidence of the spillover effects of turnout stems from quasi experiments (Klofstad, 2007; Hobbs et al., 2014) and field experiments. Nickerson's (2008) seminal study finds that the effects of door-to-door canvassing spread within a household, and Sinclair et al. (2012) find similar effects of mailings within households and no evidence of interference between households. Bond et al. (2012) find that effects travel within groups of friends.

In this study we built on the literature and conduct two large-scale field-experiments using short-text-messages in two different types of Danish elections to study transmission of treatment effects within households and within families. Our contribution is three-fold. First, we expand existing knowledge about within-household network effects with a new form of delivery. With our highly personal treatment, we minimize a potential problem stemming from the possibility that a change in behavior for a non-treated person in the household could be a consequence of the treatment itself having a direct influence on the (intentionally) untreated person. We argue that our experimental design

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comes closer than previous studies in isolating the contagious effect of voting.

Our second contribution is to further examine how spillover effects vary by the type of social network. The existing experimental literature tends to focus on spillover effects in general and mainly within households (though see Bond et al., 2012; Sinclair et al., 2012). With detailed knowledge of family relations and cohabitation we focus particularly on the importance of families relative to households and examine if spillover effects extend to close family ties outside the household. We find evidence suggesting that a behavioral change for a family member is reflected in own behavior only when the family member is a cohabitant.

The third contribution is to present the first experimental evidence proposing spillover effects outside the US. This is an important contribution as it informs us on how generalizable spillover effects are likely to be. Compared to most studied elections in a US context, turnout is much higher in the two elections studied here, 71.9 percent and 56.3 percent. This means that there are fewer citizens to mobilize directly and indirectly by applying GOTV-tactics. It also important to note that the media environment in Denmark with its broad reaching public service tradition increase the public awareness of any election compared to the US.

We find evidence suggesting that approximately 30 percent of the effects spill over. Since the direct effects are small and the spillovers even smaller, we are ultimately unable to unequivocally establish that the direct effects spill over. However, we show that even an initial skeptic based on our study updates her beliefs markedly in the direction of accepting spillover effects using Bayesian reasoning. On top of this, the design of our study is simple and can easily be replicated by other scholars.

1. The social act of turning out

In their classic *The American Voter*, Campbell et al. (1960) highlight interpersonal relationships as crucial for turnout behavior. Scholars have repeatedly found that people vote in groups (Lazarsfeld et al., 1968:137) and that turnout behavior is correlated within social networks (Stoker and Jennings, 1995; Fieldhouse and Cutts, 2008; Fieldhouse and Cutts, 2012). There are several sound theoretical arguments for why the empirical correlations at least partly reflect a causal relationship. These can be grouped in three types of social mechanisms: social norms, low information cost, and companion effect.

First of all, people evaluate their own behavior in relation to their social groups. The norms of what is seen as proper and correct behavior is in this way contingent on how it is perceived by the people around them (Zuckerman, 2005). Furthermore, individuals would according to theories of cognitive dissonance and social conformity comply to their social setting in order to avoid conflict (Mutz, 2006; Foos and Rooij, 2014). Thus social networks may be important for turnout because they contribute to spreading and enforcing norms (Sinclair, 2012). This logic is applied in several GOTV-studies using treatments with a social pressure appeal. For instance, letting people know that their turnout behavior will be exposed to their neighbors increase the turnout of the receivers, arguably because it highlights the social norm of voting and thereby creates social pressure (Gerber et al., 2008, 2010).

A second and related reason for networks to matter causally is that information from household members, family members, friends and so on is so-called 'low cost information' (Klofstad, 2007). Citizens receive large amounts of information and appeals every day, but might be too busy to cope with it all. A potential shortcut is to take more notice of information coming from one's social network, which people intuitively trust. When deciding whether to vote or not, citizens in this way will be particularly influenced by the arguments and behavior from their friends and family.

A third and more practical reason for the potential importance of social networks is that most voters choose to vote at a polling station on Election Day. If one lives with a voter, it may be difficult to abstain as one is confronted with the household members' decision to vote. In addition, it may be more convenient to vote when others go to the polls with you (e.g. sharing transportation costs). In fact, studies from Canada and Italy and our own survey evidence, see below, suggests that at least half of those attending the polling station do so with someone else, sometimes labeled the companion effect (Fieldhouse and Cutts, 2012; Hansen et al., 2017).

Accordingly, previous experimental studies find strong evidence of social effects within households. Nickerson (2008) devices a door-to-door campaign targeted to reach one member of a two-person household. For every 100 citizens mobilized to vote directly by the treatment, an estimated 60 extra citizens in these persons' households were indirectly mobilized, thereby providing strong evidence for intra-household spillovers. Sinclair et al. (2012) investigate the spillover effect of social pressure mailings within households and neighborhoods. They find some evidence of spillover effects within households though only slightly above half the magnitude of what Nickerson (2008) finds. Based on this, we hypothesize that there is a causal effect of social networks on turnout and expect to find spillover effects within households.

H1. Treatment effects travel among members of the same household.

1.1. Household vs. family spillover effects

As indicated above the existing experimental literature tends to focus on one type of network whereas differences in effects across different types of networks are less explored. Some exceptions are Sinclair et al. (2012) who, in addition to finding household spillovers, examine neighborhoods and find no spillover effects, while Bond et al. (2012) find that social effects in peer networks depend on closeness of a friendship tie. An important question is whether the household spillover effects found in the existing literature are conditioned on cohabitation or they extend to a broader range of close social relationships. To examine this further we look into the effects of family ties and sharing a household, both potential powerful mechanisms of inter-personal influence. To our knowledge no previous experimental work has looked at family ties even though this factor has often been emphasized in observational studies (e.g. Jennings et al., 2009).

We compare household and family spillovers and theorize that sharing a household is more important with respect to transmitting turnout than belonging to the same family. Our reasoning is founded in the three theoretical mechanisms outlined above. First, norms are more easily enforced within a household. Second, household members are more likely to interact on a daily basis and thus provide 'low-cost information'. Finally, household members are directly confronted with each other's decision to vote or abstain on Election Day and can accompany each other to the polling station. Survey evidence from the election backs this point (see supporting information and Hansen et al., 2017). Of more than 4000 surveyed voters, approximately 60 percent went to the polling station with a companion voter. The vast majority of the companions were voters themselves. Interestingly, of the 60 percent, 79 percent went with a cohabiting family member. Only 5 percent went with a non-cohabiting family member (Hansen et al., 2017).

We examine a key observable implication of the idea that sharing a household matters more than belonging to the same family by comparing child-parent relationships inside and outside households. If households are the key vehicles for social effects we expect treatment effects to travel among family members living together but not between family members who do not share a household.

H2. Treatment effects only travel to family members if they live in the same household.

2. High turnout elections and register data

The experiments were fielded prior to two Danish elections. The first experiment was conducted in relation to the November 19, 2013 municipal elections, while the second experiment was conducted prior to the May 25, 2014 European Parliament elections.[2] Both municipal and European Parliament elections can be considered as second order to national elections. Still, municipal elections are considered important. In 2013, 71.9 percent of the eligible population participated, which is slightly above the average over the last 30 years of local elections. European Parliament elections generate substantial less voter interest and there is also less campaign activity compared to municipality elections. Still, the European Elections enjoy much attention compared to many elections in other countries. The turnout in 2014 was 56.3 percent. Investigating spillover effects across two different types of elections allows us to examine the generalizability of our findings.

We gained access to the actual turnout from the two elections via the voter lists. The voter lists contain all eligible citizens with a code indicating whether they voted or not. In the 2013 municipal elections we managed to obtain voter lists from all 98 Danish municipalities. This means that we with few exceptions have access to the individual voter turnout of all eligible Danes. That is 4.36 million voters or 98.93 percent of the eligible citizens. In the 2014 European Parliament Elections we obtained voter files from 61 of the 98 municipalities with about 2.4 million voters (Bhatti et al., 2014b; Bhatti et al., 2014c). The municipalities absent in the 2014 election had voter lists that needed manual digitization in order to be included whereas the others had digital voter lists, which substantially lowered the burden of gathering the data. For the 2013 election we had resources to lift this extra burden. We did not have the same resources in 2014, and therefore 37 municipalities are absent in the follow up election. This bears no consequences for the causal inference, but is important to keep in mind when considering the generalizability of the second experiment. Generally, it is rare in a European context to have access to such data, as individual level turnout is seldom publically available.

All Danes have a unique personal identification number. Using this number, the voter files were merged in anonymous form with detailed and accurate socio-demographic register data from Statistics Denmark containing a long list of individual level information such as sex, age, education, residency and income. A special advantage of the register data is the possibility of linking individuals within households and families. All individuals have a household identifier allowing to connect cohabitants. In addition, it is possible to link parents and children even if they do not share a household. This leaves us with highly reliable data including validated turnout and the possibility of linking individuals within formal networks of cohabitation and family relations.[3]

3. Field experiments with treatments delivered as short text messages

In order to study spillover effects in social networks we analyze two experiments that were conducted as part of an investigation of the direct impact of short text messages (SMS) on turnout (see Bhatti et al., 2017a for a more detailed outline of the experiments). The idea behind the treatment is that the text messages function as noticeable reminders, which increase the likelihood that those who are already convinced about the value of voting remember to do so (Dale and Strauss, 2009). Both experiments applied cold text messages, meaning that the receivers had not given prior consent to get text messages from the messenger (cf. Malhotra et al., 2011).

The Danish law allows mass-distribution of short text messages to cell phones without consent from the receiver as long as it is not done for marketing purposes. We cooperated with The Danish Youth Council, an umbrella organization for roughly 70 Danish youth organizations. The council sponsored the distribution of the text messages. As a result from cooperating with the Danish Youth Council, the text messages were targeted citizens between 22 and 29 years (experiment 1) and 18–21 (experiment 2).[4]

To run the experiments we needed cell phone numbers that subsequently could be matched with data from the official voter records. From a list of the names and addresses of all eligible citizens in the relevant population a private company matched with cell phone numbers using public online phone registers. The cell phone number enriched data was then merged back on the official records.[5] The randomization into treatment and control groups was conducted in this enriched sample.

All text messages were in Danish and are described for the two studies below. To make the strongest possible treatment, we added the recipient's first name in the message based on the belief that this would help recipients perceive it as “warmer”. The sender of the messages was “stem.dk” (the direct English translation would be “vote.dk”) a webpage sponsored by several organizations including The Danish Youth Council in order to promote youth turnout and the organizations' mobilization campaigns.

3.1. Identifying voting contagion

Compared to previous studies, a considerable advantage of our design is that it gives a cleaner estimate of the contagious effect of voting. Previous studies used treatments that potentially could be directly observable to the untreated member of the household. Imagine that Mr. Jones opens the door. That does not rule out that Mrs. Jones is listening in the background or wonders who Mr. Jones was talking to. Correspondingly, a letter might be directed for Mrs.

[2] In municipal elections all Danish citizens and citizens from EU, Norway or Iceland who are 18 or older on Election Day and have permanent residence in the municipality are eligible to vote. Immigrants from non-EU countries are eligible to vote after three years permanent residency in the realm. In European Parliament Elections EU-citizens residing in Denmark are eligible to vote. Non-EU immigrants are only allowed to vote if they have obtained Danish citizenship. Registration is automatic in both types of elections and all eligible citizens automatically receive a polling card by mail.

[3] Data is stored on servers at Statistics Denmark, which is the central authority on Danish statistics. Due to security and privacy implications the data is not allowed to be made available on the internet. Researchers interested in replicating the findings are welcome to visit and work under supervision on Statistics Denmark's secure servers.

[4] A range of other campaigns targeted first-time voters (18–21 years old), while there was little attention paid to second and third time voters. To make up for this lack of attention, the Danish Youth Council wanted to target 22–29 years old in the municipality elections. In the European Parliament Elections (experiment 2), the Danish Youth Council was the only national organization with a GOTV-campaign targeting young individuals. Thus, they also wanted to contact the 18–29 year olds.

[5] Almost all Danes' uses cell phones. 93 percent of all Danes have used a cell phone within the last three months (Danmarks Statistik, 2013). For Danes under 35 years, this figure is 98 percent. So in theory, one could reach almost all Danes via short text messages. However, many phone numbers cannot be linked with sufficient information to merge them with the public records.

Jones but Mr. Jones empties the mailbox and sees the letter.

To see why the spillover effect might overestimate the contagious aspect of voting caused by a GOTV-treatment, we formalize how the spillover percentage is estimated and what assumptions are required before we can consider the spillover percentage to be caused by voting contagion. As Nickerson (2008) demonstrates, the contagion effect, α, is

$$\alpha = \frac{S}{T}$$

where T is the direct treatment effect and S is the indirect or spillover effect. We can expand this to a potential outcomes framework (Holland, 1986)[6]:

$$\alpha = \frac{E[Y(dt=1|type=cohabitant)] - E[Y(dt=0|type=cohabitant)]}{E[Y(dt=1|type=treated)] - E[Y(dt=0|type=treated)]} \quad (1)$$

where *dt* expresses the assignment of treatment to the household which affects both the treated and the cohabitant. While we assume that excludability holds for the treated, we can expand the potential outcomes for the cohabitants to depend on both the treatment status of the household and the behavior of the directly treated:

$$Y(\text{treated expresses } X(T=1); \text{house is treated}) \quad (2)$$

$$Y(\text{treated expresses } X(T=1); \text{house is not treated}) \quad (3)$$

$$Y(\text{treated expresses } X(T=0); \text{house is treated}) \quad (4)$$

$$Y(\text{treated expresses } X(T=0); \text{house is not treated}) \quad (5)$$

In each of the four cases the cohabitant can respond to two inputs: some behavior, *X*, expressed by the receiver of the treatment and the treatment itself. We denote the behavior of the treated this way to emphasize that the cohabitant responds to some social stimulus from the treated prior to the decision to vote, which we call the voting contagion. We discuss the content of this stimulus elsewhere.

With the aim being to estimate the contagious effect of voting, we want to know the difference between (2) and (4) and between (3) and (5). Unfortunately, the terms (3) and (4) are complex potential outcomes meaning that although we can imagine them we cannot empirically observe them (Gerber and Green, 2012, p. 329). Observing them would require the treated cohabitant to act as-if treated while the house is untreated or vice versa. We can only observe (2) and (5) and the numerator in (1) gives us exactly the difference between the two. Consequentially, any argumentation for voting contagion must rely on assuming that excludability holds.

Assuming excludability implies that (2) = (3) and (4) = (5) in which case the difference in outcomes between cohabitants of treated and untreated gives the contagious effect of voting. If that assumption does not hold, excludability breaks down, and one will get a biased estimate of the contagion effect.[7] Arguably, if the treatment has a positive effect on the directly treated it is most likely that any direct effect on the cohabitant is positive, too. If that is true, one overestimates the contagious effect of voting when excludability does not hold.

As we use a highly personal channel of communication, we argue that it is less likely that the cohabitant is directly aware of the treatment, especially in the case where the cohabitants are the parents of the treated voter. Hence, the bias in our estimate will be smaller than it has been in previous studies of the contagion effect. We see Nickerson (2008) and Sinclair (2012) as seminal studies, but we argue that if one takes interest in how voting is contagious, we offer an approach where a spillover effect is less likely caused by secondhand exposure to treatment, and more likely to actually capture the contagious effect of mobilizing voters.

4. Study 1: The Danish municipal elections in 2013

The first experiment was fielded in the days running up to the Danish municipal elections in November 2013. The target group for the campaign was Danes aged 22–29 years and 46.9 percent of the target population was successfully enriched with phone numbers. We restricted the sample to households with only one individual eligible for treatment.[8] Even though the treated were the only one in their age group residing in their household a considerable proportion still shared household with other voters in other age groups. We ended up with a sample of almost 48,917 voters from which we randomly sampled treatment and control groups (Bhatti et al., 2014a; Bhatti et al., 2017a). As is evident from the supporting information the sample for the experiment is very different from the overall population. This is an artifact of the non-random phone number enrichment and the fact that a large part of the young voters, including especially many immigrants and immigrant descendants, was set aside for another field experiment during the same campaign (Bhatti et al., 2015; Bhatti et al., 2017a; Bhatti et al., 2017b). This does not affect the causal inference for the study sample but is a consideration with respect to the generalizability of the findings.

The text messages were sent out over a timespan from seven days ahead of the elections until one hour before the polling stations closed. 17,500 were assigned to receive a text message in the days running up to the election and 7500 from this treatment group were assigned to receive an additional reminder on Election Day. 10,000 were assigned to receive a single message on Election Day and the remaining 21,417 voters were assigned to the control group. We leave the assessment of the timing effect (along with the direct effect) to be analyzed elsewhere and for the present purpose we dichotomize the experiment and pool all individuals who received a treatment in to one joint treatment group. We removed voters that between the time when we design the experiment and the election had moved to larger household and lost some as we could not match everyone to the voter lists (see appendix A for descriptive statistics for the sample). Therefore, the final size of the treatment group is 26,873 and the control group size is 20,973, which constitutes 97.7 percent of the original group for the treatment group and 97.9 percent of the original group for the control group.

The attrition is balanced across the groups and there is no reason to believe that any source of attrition, moving to a household shared with another voter in the experiment, moving very

[6] For simplicity we consider spillover to cohabitants. The framework is generalizable to others exposed to the spillover.

[7] Though it will still be an unbiased estimate of the total direct and indirect effect on cohabitants.

[8] We did this as the number of households with multiple members aged 22–29 years was quite small, and we wanted to utilize these households in another experiment. In the original experiment we also included a few individuals who lived in larger households, as it for political purposes was a requirement that all groups of young adults were treated. In all analyses in this paper we exclude all households where more than one individual was phone number enriched (345 individuals) to ensure that only one person from each household was included in the experiment. We furthermore exclude one person with no valid household ID.

close to election day, dying, or clerical errors are caused by or related to receiving a text message. In the appendix, we also show that the groups remain balanced on pretreatment covariates after attrition.

The main inspiration for the content of the text messages were drawn from Dale and Strauss' (2009) noticeable reminder theory. Small adjustments were made to the Dale and Strauss-message in order to fit it within a limit of 150 characters and to fit better with normal use of Danish language. A message sent out on Election Day to an imaginary voter named Alan would be:

> Hi Alan. This is a friendly reminder of the Election on Tuesday November 19. Democracy needs you so remember to vote!

> The message sent out on Election Day would be:

> Hi Alan. Thank you for voting in the municipality election. If you haven't voted yet, you can make it until 8 PM.

5. Study 2: The European Parliament election in Denmark in 2014

The second field experiment, conducted in connection with the European Parliament Elections, targeted eligible voters aged 18–29 years and 34.3 percent of this group was enriched with a mobile phone number. The primary reason for the lower enrichment rate was that it was very difficult to find the phone numbers of the youngest voters, which is likely due to their numbers being registered in their parents' name. This gave us a sample of 146,916 voters, from which 46,125 randomly were placed in the control group and the rest were randomly placed in one of the treatment groups (see appendix B for descriptive statistics for the sample). Contrary to the municipality elections only Danish citizens and EU citizens were eligible in this election. Immigrants and immigrant descendants with permanent residency but no citizenship were ineligible. This means that especially the proportion of immigrants and descendants is substantially lower in the overall population. But the rate in the treatment sample is not that different from above due to the sample restrictions discussed above for the first experiment.

We pool the analysis of two different text messages that were sent out in the four days running up to the election (including Election Day). One text message applied a ‘fresh tone’:

> “Dear Alan. Are you ready for the EP-election tomorrow? Because you are going to vote, right? For Democracy's sake. And your own. Regards stem.dk”

> The second text message was formulated in a formal tone:

> “Dear Alan. Tomorrow there will be elections to the European Parliament and a referendum. It is your choice. Vote for Democracy's sake. Regards stem.dk”

Like the first experiment, the second included young voters who did not share household with other voters in their age group eligible for treatment. This allows us to repeat the analysis from the first experiment in a different setting and reach more precise inferences about direct and spillover effects. On top of that, it also included young voters who shared a household with others eligible for treatment. This design allows us to estimate spillover effects within the intended population, thereby expanding on the findings from the first experiment.

There was also a little attrition between from treatment to data on validated turnout in study 2. In the treatment group, 99,145 of 100,731 subjects remained in the data, which corresponds to 98.4 percent. In the control group, 45,318 of 46,125 subjects remained in the data corresponding to 98.3 percent. In the supporting information, we show that the treatment and control groups are also balanced on pretreatment covariates after attrition in the second experiment.

After the experiments we performed power analyses of the spillover effects based on the previous results in the literature. These show that the studies combined are well-powered under the most optimistic assumptions based on the previous literature, slightly underpowered under more modest, and perhaps more realistic, assumptions and underpowered under pessimistic assumptions. The power analyses are described in further detail in the supporting information.

6. Results

6.1. Study 1: Municipal elections experiment

Direct treatment were assigned on the individual level with no clustering, while spillover treatment assignment was clustered on the household level. The confidence intervals for the spillover effect are clustered. In both studies about 85 percent of text messages were successfully delivered. As so few text messages failed to deliver, we choose to only estimate intent-to-treat effects (ITT). We keep those who we failed to reach in the treatment group as to not obliterate the randomization. Due to our focus on spillover effects, the direct effects are only of secondhand interest to us below and are analyzed in detail in Bhatti et al. (2017a).

Table 1 displays direct effects and spillover effects in households. The first column shows the direct average treatment effect of 1.82 percentage points for all directly treated in the experiment. This effect is in between the two intent-to-treat estimates from the existing literature (Dale and Strauss, 2009; Malhotra et al., 2011).

In columns two to four we direct our attention to the effect for those who reside in a household with others, and the spillover effect on these others. The spillover-treatment group consists of those citizens cohabitating with a receiver of the text message. The spillover-control group consists of cohabitants of those who were a part of the control group. Thus, the experiment was designed to make sure that we had households where no one received the text message. In column 2, we see that the estimate for the main effect is approximately the same at 1.87, though the sample size decreases somewhat reducing the precision. In the lower half of the table we see the estimated spillover effects. The turnout rate is 0.66 higher for cohabitants in the treatment group compared to cohabitants in the control group. Using two-stage least-squares to estimate the spillover proportion, this corresponds to a spillover percentage of around 27 percent. Though the effect has the expected sign and a reasonable order of magnitude the estimate falls short of conventional levels of statistical significance.

When we split the sample into two and three person households, we find a limited and statistically insignificant direct effect of 0.93 percentage points among voters sharing a household with one other person, while the effect estimate is 4.59 percentage points and statistically significant among voters who live with two other voters. We remain agnostic about this seemingly heterogeneous effect, and instead we turn our attention to the spillover effects. Thirty and 34 percent of the direct effects seem to spill over.[9] Thus,

[9] The pooled spillover effect surpasses each of the individual spillover effects. The fact that there are twice as many persons eligible for spillover in the large households, with the greater spillover effect explains this apparent paradox.

the first experiment shows that the treatment itself mobilized the intended receivers. In addition, it suggests that approximately a third of the effect spills over to other cohabitants though we must be careful with our conclusions as the confidence intervals are wide compared to the effect and spillover estimates. The spillover percentages are about half the size of the 60 percent reported by Nickerson (2008), but similar to those obtained by Sinclair et al. (2012).

Next, we turn to examine the importance of cohabitation. In Table 2, we narrow our focus to those who live with both of their parents or neither of their parents. First, we see that the estimated effect for young voters who live with both their parents is 2.32 percentage points, which due to a limited sample size falls short of statistical significance. The parents are almost one percentage point more likely to vote when their child is treated, which corresponds to an estimated spillover effect just above 40 percent. If we compare this spillover percentage to the spillover on other cohabitants (not reported) we find that they do not diverge markedly. To put it another way we find no evidence that the spillover on parents is greater or smaller than on other cohabitants. However, we find that the effect does not travel to their parents when they are living in separate households (cf. column two). When treated, young voters had an average turnout of 1.40 percentage points over the control group, the pooled estimate for the spillover effect on the parents was 0.02 percentage points.

In sum, the first experiment showed that approximately one third of the direct effect travelled to cohabitants. Furthermore, spillover to parents and other cohabitants were approximately similar while the spillover to parents was virtually absent when parents did not cohabitate with the young voter. This indicates that cohabitation trump family relations. Though the results are supportive of our hypotheses they do not constitute conclusive evidence. We therefore turn to experiment two for further investigation.

6.2. Study 2: European Parliament experiment

Table 3 displays the same results for study 2 as Table 1 did for study 1. The average direct treatment effect in the experiment is 0.63 percentage points in the group where only one voter was eligible for treatment. The effect is statistically significant, but substantially smaller than the direct effect found in study 1. Nevertheless, both point in the same direction and the direct effect allow us to examine if some of it spills over (see Bhatti et al., 2017a for further analysis and discussion).

Focusing on columns 2 to 4, we find that though the direct effect on voters in a shared household falls short of statistical significance its point estimate is virtually the same as the point estimate for all treated. This is similar to what we saw in study 1. Furthermore, the proportion that spills over is once again approximately one third, though the estimate is far from statistically significant. Column 3 and 4 reveals that in study 2, the direct effect is largest for young voters who live with one other person. Accordingly, the estimated spillover effect is 0.41 percentage points, which is not statistically significant but still corresponds to approximately 50 percent of the direct effect estimate of 0.80 percentage points. In the larger households the direct effect estimate is 0.39 percentage points while the spillover effect is virtually zero. That the effect is largest in the small households opposes the finding in study 1, which supports that the differences in effects between household sizes could be largely driven by random variation.

When we look at how the effect travels to parents residing with their child (Table 4), we also see a pattern similar to the one in study 1. For the young voters, we estimate a direct effect of 1.67. For their parents we estimate a spillover effect of 0.94 percentage point corresponding to 56 percent of the main effect. The experiment did not mobilize young voters who do not reside with their parents to the same extent as we saw in study 1, which makes the spillover calculations extremely fragile. The turnout rate in the treatment group was only marginally higher than in the control group, and the estimated spillover effects are in fact negative though small and statistically insignificant. It is therefore difficult to use this part to say more than we know from study 1 about the spillover to parents who do not cohabitate with their children.

In both experiments, the direct effect is stronger for young voters that live with their parents. Although we emphasize that the difference in effects are not causally identified, we might pause a moment to speculate about why we see such a pattern. One possible explanation has to do with the social context. Perhaps the young people living with their parents discuss the election with their parents after they receive the text message and the discussion leads them to vote. Such a mechanism would both drive the direct effect and the spillover effect. Young people who do not live with their parents are a mix of voters living alone and sharing households with others. Those living alone cannot engage with others in their household. Those living with others than their parents may live with people with whom they are less likely to discuss the election so a text message does not have the same impact on them. An alternative account for the difference in effects is that those living with their parents are younger and perhaps less set in their

Table 1
Direct treatment effects and spillover effects for household members in study 1.

	ALL	In shared household	1 other in household	2 others in household
Direct effect				
Treatment group turnout	61.18	63.40	65.85	55.03
	N = 26,873	N = 12,471	N = 9647	N = 2824
Control group turnout	59.36	61.53	64.91	50.43
	N = 20,973	N = 9859	N = 7555	N = 2304
Direct treatment effect	1.82* (0.45)	1.87* (0.65)	0.93 (0.73)	4.59* (1.40)
Spillover effect on other household members				
Treatment group turnout	–	70.02	65.95	76,97
		N = 15,295	N = 9647	N = 5648
Control group turnout	–	69.36	65.67	75.41
		N = 12,163	N = 7555	N = 4608
Spillover effect (percentage points)	–	0.66 (0.61)	0.28 (0.73)	1.55 (1.06)
Spillover percentage	–	27 (23)	30 (66)	34 (22)

Standard error in (). *$p < 0.05$ (one-sided test). The estimates for direct and treatment effects are difference in means and the standard errors are from linear regressions clustered by the household. A randomization inference based approach yield identical results. The spillover percentage is estimated using two-stage least-squares with the first stage being a regression turnout for the directly treated on assignment to treatment. See appendix A for descriptive statistics for the treatment and control groups.

Table 2
Direct treatment effects and spillovers in study 1 for parents conditional on whether parents are part of household.

	Lives with both parents	Lives without parents
Direct effect		
Treatment group turnout	58.87	64,66
	N = 1573	N = 17,308
Control group turnout	56.55	63.26
	N = 1229	N = 13,443
Direct treatment effect	2.32 (1.88)	1.40* (0.55)
Spillover effect on mother	0.97 (1.39)	0.27 (0.43)
Spillover effect on father	0.98 (1.42)	−0.22 (0.44)
Joint spillover over on parents	0.97 (1.27)	0.02 (0.36)
Spillover percentage on mother	42 (59)	19 (30)
Spillover percentage on father	42 (61)	−16 (33)
Joint spillover percentage on parents	42 (54)	2 (25)

Standard error in (). *p < 0.05 (one-sided test). The estimates for direct and treatment effects are difference in means and the standard errors are from linear regressions clustered by the household. A randomization inference based approach yield identical results. The spillover percentage is estimated using two-stage least-squares with the first stage being a regression turnout for the directly treated on assignment to treatment. Control group turnout for parents is approx. 80 percent.

habit of voting or abstaining. We reemphasize that we did not design our experiment to causally identify competing explanations for the pattern.

Finally, we also used study 2 to expand on our findings in study 1. In Table 5 we include voters in the age group who reside with another voter in the age group. This allows us to estimate the spillover effect on someone of similar age and to investigate if there is an additional effect of exposure to both treatment and spillover.

From the results, we see that turnout was 2.14 percentage points higher for those who received treatment and no exposure to spillover. This is voters who got a message and reside with a voter who could potentially receive a text message but did not. Moreover, those exposed to spillover but not treatment had a 0.67 percentage point higher turnout. Though the latter is far from reaching statistical significance, it is once again approximately one third of the effect that spills over to the cohabitants. Finally, we see that there is no evidence of an additional effect of being exposed to spillover once treated, quite the contrary. Voters in the treatment group who lived with another person from the treatment group and thus were exposed to both treatment and spillover turned out at a rate that was 1.26 percentage points higher than the control group. This is less than for those only exposed to treatment, though the wide confidence interval show that the difference could be due to sampling error.

Overall, the effect in study 2 was statistically weaker than in study 1. However, the findings in both studies point in the same direction and suggest that the treatment mobilized voters and approximately one third of the direct effect travelled in the household. There was no clear pattern of cohabiting parents being more receptive of spillover than other cohabitants are. Instead, we see that the effect is not transmitted to parents when they reside in another household. This is consistent with the expectations formulated above. When we mobilize voters, the change induced in the behavior mainly transmits to others in their household. Although our study is inconclusive, the results suggest that cohabitation matters more than the family relation.

6.3. Bayesian Integration of research findings

Both experiments offer evidence in the direction of spillover effects but neither have sufficient power to establish a definite finding. The power of the estimates can be increased by pooling them together using fixed-effects meta-analysis (Borenstein et al., 2009). From the two experiments we have three estimates that we can pool together by their precision using the formula:

$$u_{pooled} = \frac{\Sigma_1^J u_j * W_j}{\Sigma_1^J W_j}$$

where W_j are weights equivalent to the individual estimate's

Table 3
Direct treatment effects and spillover effects for household members in study 2.

	ALL	In shared household	1 other in household	2 others in household
Direct effect				
Treatment group turnout	44.41	46.64	47.81	43.86
	N = 77,050	N = 44,815	N = 31,603	N = 13,212
Control group turnout	43.78	45.97	47.00	43.48
	N = 35,181	N = 20,544	N = 14,520	N = 6,024
Direct treatment effect	0.63* (0.32)	0.67 (0.42)	0.80 (0.50)	0.39 (0.77)
Spillover effect on other house members				
Treatment group turnout	–	54.53	49.96	59.99
		N = 58,027	N = 31,603	N = 26,424
Control group turnout	–	54.29	49.55	60.01
		N = 26,568	N = 14,520	N = 12,048
Spillover effect	–	0.23 (0.42)	0.41 (0.50)	−0.02 (0.70)
Spillover percentage	–	39 (59)	51 (47)	−6 (187)

Standard error in (). *p < 0.05 (one-sided test). The estimates for direct and treatment effects are difference in means and the standard errors are from linear regressions clustered by the household. A randomization inference based approach yield identical results. The spillover percentage is estimated using two-stage least-squares with the first stage being a regression turnout for the directly treated on assignment to treatment. See appendix B for descriptive statistics for the treatment and control groups.

Table 4
Direct treatment effects and spillovers in study 2 for parents conditional on whether parents are part of household.

	Lives with both parents	Lives without parents
Direct effect		
Treatment group turnout	48.11	46.29
	N = 9300	N = 29,217
Control group turnout	46.44	46.13
	N = 4313	N = 13,361
Direct treatment effect	1.67* (0.92)	0.16 (0.52)
Spillover effect on mother	1.04 (0.86)	−0.32 (0.50)
Spillover effect on father	0.83 (0.86)	−0.10 (0.50)
Pooled spillover effect on parents	0.94 (0.81)	−0.21 (0.44)
Spillover percentage on mother	62 (46)	−201 (790)
Spillover percentage on father	50 (46)	−64 (410)
Pooled spillover percentage on parents	56 (42)	−133 (568)

Standard error in (). *$p < 0.05$ (one-sided test). The estimates for direct and treatment effects are difference in means and the standard errors are from linear regressions clustered by the household. A randomization inference based approach yield identical results. The spillover percentage is estimated using two-stage least-squares with the first stage being a regression turnout for the directly treated on assignment to treatment. NOTE: Control group turnout for parents is around 64–68 percent.

Table 5
Effects in households with two eligible for treatment in study 2.

	Turnout/effect	N
Control group turnout	48.5	3130
Treatment effect	2.14* (1.29)	7007
Spillover effect	0.67 (1.29)	7007
Treatment*spillover	−1.55 (1.67)	15,088

Standard error in (). *$p < 0.05$ (one-sided test). The estimates for direct and treatment effects are difference in means and the standard errors are from linear regressions clustered by the household. A randomization inference based approach yield identical result. See appendix B for descriptive statistics for the treatment and control groups.

precision given by one divided by its variance, that is $1/\hat{\sigma}_j$. For the households with two eligible for treatment, the spillover effect is estimated using two-stage least-squares where one subject from each household in the control group is picked at random to have their revealed outcome in the first stage and the other's revealed outcome is imputed as the second stage outcome. The median estimate from 5000 simulations over this approach is 31 percent spillover with a median standard error of 54 percent. Pooling together the estimates using the formula above, we get an estimated spillover effect of 29 percent with a standard error of 20 percent.

Pooling the results together we are still not able to estimate the effect with enough precision to establish that a positive spillover effect is present. However, we still learn substantially from our experiments. In order to quantify how one can learn from the experiment, Fig. 1 tracks how four types learn from each of the individual estimates in the order they were presented above.[10]

Each of the types are Bayesian learners who use the data to update their prior belief about the effect size in order to form a posterior belief (Gill, 2014). The first type is agnostic about the effect size. Before she sees any data, her prior is summarized by a normal distribution with an average of zero and standard deviation of 1, reflecting that she believes effects can be large in either direction. The second type is an “informed skeptic” who builds her beliefs on modest spillovers of around one third like those Sinclair et al. (2012) find. The mean of her prior is 0.30 with a standard deviation of 0.30, reflecting that she is quite certain that the effect is neither substantially larger nor smaller than her prior conviction.

[10] This section is inspired by (Green et al., 2016) and the figures are based on their publicly available replication code.

The third type is an “informed optimist” who has based her prior beliefs on the existing literature, too. However, she believes that around 60 percent of the effect spills over corresponding to spillovers like Nickerson (2008). She too is relatively certain about her beliefs reflected in the standard deviation of 0.30, similar to the standard deviation for the “informed skeptic”. Finally, the “skeptic” does not believe an effect exists and is quite certain that it is at best limited, summarized in a prior with mean zero and a relatively small standard deviation of 0.2.

Fig. 1 shows how different types update their beliefs in light of the research findings.

Though the priors of the four types differ, they arrive at quite similar conclusions. The “agnostic” (row 1) concludes that there is roughly a 92 percent chance that the effect spills over. The “informed skeptic” (row 2) put this probability at 96 percent and the “informed optimist” (row 3) concludes that there is a 99 percent chance. While the “Informed optimist” only changes her belief that there are spillovers from 0.977 to 0.990, she changes her belief about the spillover percent markedly from around 60 percent to around 38 percent. In fact, after updating her beliefs she puts the probability of the effect being equal to 0.60 or larger at just 0.097 (calculation not shown).

Finally, the skeptic (row 4) remains the most reluctant but even she puts the chance at 85 percent. Neither of the types is completely convinced but Fig. 1 does show how the new evidence almost uniformly pushes everyone in the direction of being more convinced. However, the optimist hardly changes her belief that the effect exists but changes what she believes to be a credible size of the effect. The priors are far from exhaustive of all priors, but regardless of which comes closest to one's own, it seems evident that all types that a priori have some doubt as to if the effects do exist are moved in a direction that spillover indeed take place.

7. Discussion

A central contribution of our study is to increase our confidence that spillover effects are due to exposure to the behavior of a treated person and not due to unintentionally being exposed directly to the treatment. The existing studies use treatment delivery methods which potentially can be observed by other individuals in the household, though this last concern is more pressing for Nickerson (2008)'s door-to-door treatment than Sinclair et al.'s (2012) direct mail treatment. This possibility declines substantially by using personal short text messages delivered directly to the cell phone of the treated person. Especially for young

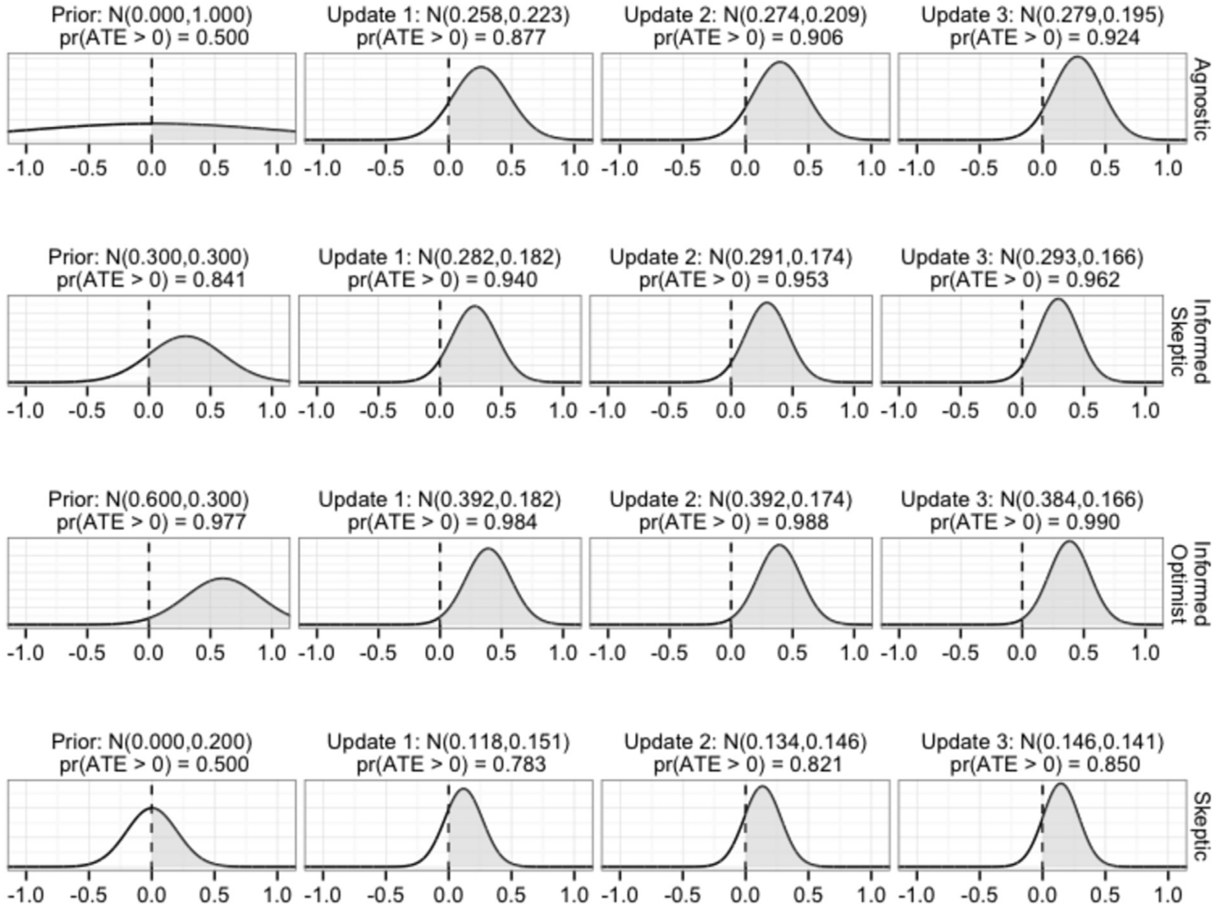

Fig. 1. Bayesian Integration of spillover effects from two SMS experiments.

voters who cohabitate with their parents it seems most likely that they keep the text messages for themselves. Even though we cannot completely rule out that some people might read messages on their household members' cell phones, our results does make it clearer that the contagious effect of turnout behavior happens because of the behavior of other household members and not because of direct exposure to a mobilization message. Ultimately, our findings are not unequivocal but as we demonstrated above, our findings pulls the skeptics towards accepting spillover effects, and drives the optimist towards more modest expectations about the spillover effect size without decreasing her confidence that a spillover effect exists.

The difference in delivery may explain why our estimate of the spillover percentage of about 30 is approximately half of the effect found by Nickerson (2008) and on par with the effect in Sinclair et al. (2012). Other potential explanations are the overall high turnout, which makes a potential ceiling effect more likely, and the difference in political culture and context might also explain the difference. The exact reasons for these explanations should be subject to future research, e.g. by employing the same research designs across countries or testing different means of treatment within the same experiments.

We also provide suggestive evidence indicating that sharing a household is important for the transmission of turnout behavior. Being related seems not to be sufficient. Physical interaction on a regular basis with the newly mobilized voter seems to be central for the spillover effect to work. This can be because norms are easier enforced within households, because household members are a useful source of low-cost information or because household members accompany each other to the polling station.

By examining spillover effects with departure in young individuals, a by-product of our study is that we simultaneously offered evidence suggesting that adult children can affect their parents' turnout. As noted previously, observational research has observed a correlation between parents and their adult children's political attitudes and behavior. In a general sense the literature argues that while growing up children learn certain participation norms from their surroundings and especially from their parents (Plutzer, 2002). However, not much attention have been given to the possibility that the effect might also work the other way around (though see Washington, 2008; Glynn and Sen, 2015 among others), although the political socialization approach indeed is open to this possibility (cf. Jennings et al., 2009). Our experimental approach indicates that children in fact can affect their parents if they share a household. Of course, our findings far from rules out the relevance of the other perspectives on the parent-child transmission of political behavior.

As a final contribution, we show that findings of spillover effects in get-out-the-vote experiments from American contexts are consistent with findings in a European context. The existing studies rely on US elections with turnout levels between 25 and 40 percent. Our studies are conducted across two elections in Denmark, where turnout is substantially higher and the political system and context varies from the US system. Showing that turnout behavior travels within households in different contexts is in itself an important addition to the scientific accumulation of knowledge as it implies that it is not specific to one context. We suggest that the social transmission of turnout behavior might be a general principle, though more research on spillover effects in different contexts and with other types of participation are encouraged in order to investigate how far the findings generalize. For such purpose, our research design lends itself nicely to reproduction by researchers in different elections and countries. A particular strength of the design is that the identified spillover effect is more likely caused by voting contagion and not indirect treatment. Hopefully, future research can use our design as a template to estimate spillover effects and update our beliefs about the spillover effects of GOTV-treatments as well as other types of studies where social contagion might take place.

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Appendix A. Descriptive statistics for population, sample, and groups in study 1

Table A1
Study 1: Descriptive statistics for population and experimental sample.

	Population of 22–29 year olds			Experimental sample		
	Mean	St.dev.	N	Mean	St.dev.	N
Voted	54.86	49.76	520,509	60.39	48.91	47,846
Age	25.37	2.29	520,509	25.59	2.31	47,790
Female	0.49	0.50	520,509	0.50	0.50	47,790
Immigrant or descendent	0.18	0.38	520,509	0.06	0.24	47,790
Lives with mother	0.09	0.29	517,049	0.10	0.30	47,523
Lives with father	0.08	0.26	513,270	0.08	0.28	47,214
Student	0.40	0.49	520,509	0.41	0.49	47,846

Table A2
Study 1: Descriptive statistics for treatment and control group.

	Treatment group			Control group		
	Mean	St.dev.	N	Mean	St.dev.	N
Voted	61.18	48.73	26,873	59.36	49.12	20,973
Age	25.60	2.31	26,842	25.57	2.31	20,948
Female	0.50	0.50	26,842	0.50	0.50	20,948
Immigrant or descendent	0.06	0.24	26,842	0.06	0.24	20,948
Lives with mother	0.10	0.30	26,698	0.10	0.31	20,825
Lives with father	0.08	0.28	26,506	0.08	0.28	20,708
Student	0.41	0.49	26,873	0.41	0.49	20,973

Appendix B. Descriptive statistics for population, sample, and groups in study 2

Table B1
Study 2: Descriptive statistics for population and experimental sample.

	Population of 18–29 year olds			Experimental sample		
	Mean	St.dev.	N	Mean	St.dev.	N
Voted	43.27	49.55	444,091	45.43	49.80	144,463
Age	23.33	3.39	444,091	24.24	3.11	144,174
Female	0.49	0.50	444,091	0.48	0.50	144,174
Immigrant or descendent	0.07	0.25	444,091	0.05	0.22	144,174
Lives with mother	0.21	0.41	408,093	0.18	0.35	138,663
Lives with father	0.26	0.44	428,774	0.15	0.38	132,008
Student	0.52	0.50	444,091	0.48	0.50	144,463

Table B2
Study 2: Descriptive statistics for treatment and control group.

	Treatment group			Control group		
	Mean	St.dev.	N	Mean	St.dev.	N
Voted	45.66	49.81	99,145	44.94	49.75	45,138
Age	24.25	3.12	98,947	24.22	3.12	45,227
Female	0.48	0.50	98,947	0.48	0.50	45,227
Immigrant or descendent	0.05	0.22	98,947	0.05	0.22	45,227
Lives with mother	0.18	0.38	95,212	0.18	0.38	43,451
Lives with father	0.15	0.35	90,606	0.14	0.35	41,402
Student	0.48	0.50	99,145	0.48	0.50	45,318

Appendix C. Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.electstud.2017.09.003.

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