

Nag Him, Nag Her: Is Voter Mobilization within Households Gendered?

Eline A. de Rooij* Florian Foos† Vanessa Cheng-Matsuno‡

November 3, 2019

DRAFT

Do not quote without permission from the authors

Abstract

Observational studies show that women are more likely to list family members as political discussion partners. In this paper, we ask whether this greater, self-reported, role that family appears to play in the acquisition of political information and political decision-making among women, translates into gendered mobilization patterns within households. We combine data from two previously conducted GOTV experiments in the US and the UK that are uniquely suited to identify spillover effects within households because they randomly sampled either the male or female household member to be included in the experiment. We assess the extent to which within-household spillovers vary according to the gender of the experimental subject, and that of their household member. While we find no difference in the effectiveness of direct mobilization efforts on the propensity to vote between women and men, we find that women in the US are significantly more effective at indirectly mobilizing men in their households than the other way around.

Paper prepared for the 7th Annual Toronto Political Behaviour Workshop,
Munk School of Global Affairs & Public Policy, University of Toronto, November 8-9 2019

*Department of Political Science, Simon Fraser University, eline_de_rooij@sfu.ca

†Department of Government, London School of Economics, f.foos@lse.ac.uk

‡Department of Political Economy, King's College London vanessa.cheng_matsuno@kcl.ac.uk

1. Introduction

Observational studies show that women exhibit lower levels of political interest (Bennett and Bennett 1989), political knowledge (Carpini and Keeter 1996, 2000; Dow 2009) and political efficacy (Gidengil, Giles and Thomas 2008) than men. Women are also less likely than men to indicate that they discuss politics regularly (Verba, Burns and Schlozman 1997). Political interest, knowledge and efficacy, and regularly discussing politics, are seen as some of the key predictors of voter turnout. It is therefore puzzling that studies show that there is no longer a gender gap in electoral participation in advanced democracies, and that where it exists, women appear to be slightly more likely to vote than men (e.g., Coffé and Bolzendahl 2010).

One explanation offered in the literature is that differences between men and women in their actual levels of political knowledge and political discussion, in particular, are minor but that these differences are perceived as substantial. Research has shown that women perceive themselves and are perceived by others as less political knowledgeable than men (Banwart 2007; Morehouse Mendez and Osborn 2010), and that they are less likely to openly express political attitudes or answer political knowledge questions in the context of a survey (Atkeson and Rapoport 2003; Mondak and Anderson 2004). Thus, differences in reported political discussion and knowledge might not translate into actual turnout behavior.

A second explanation points to qualitative differences in the type of political knowledge and discussion. For instance, some contexts in which political discussion occurs might be more important than others. The household and/or family context might be one such context. Women are more likely than men to list members of their intimate network - i.e. family and close friends - as political discussion partners (Elder and Greene 2003; Huckfeldt and Sprague 1995, 198). Within the family though, men are at least equally, if not more, likely to discuss politics. In a study of adolescents in the US, Wolak and McDevitt (2011, 515) find no difference between young men and women in their tendency to challenge their parents on political matters or to discuss politics with their parents (*idem*, footnote 17).

A final explanation is that turnout differences might be better explained by differences in mobilization, rather than by pre-existing levels of engagement with and knowledge of politics. The relationship between political engagement and knowledge on the one hand, and turnout on the

other might be strong and positive, but it is not necessarily causal and one-directional. Instead, mobilization might be the key driver of changes in turnout. If women and men respond equally to mobilization efforts by campaigns, then this might explain why there is little evidence of a gender gap in turnout, at least in contexts where mobilization by political campaigns is commonplace. Mobilization might be even more important if we consider how direct mobilization attempts by political actors diffuse within social networks to indirectly mobilize individuals who were not the original target of the campaign (Rolfe 2012).

In this paper, we examine the role of gender in voter mobilization within the context of the household. We first ask whether men and women differ in their responsiveness to direct mobilization messages. Our main focus in this paper, however, is on whether men and women differ in the probability of discussing politics with household members of the opposite sex as a result of such messages, and to what extent men and women’s probability to turn out is subsequently differentially impacted as a result of the discussion initiated by their partner; that is, to what extent are men and women differentially susceptible to social influence exerted by each other?¹

To answer these questions we use data from two previously conducted field experiments aimed at mobilizing household members to vote (Foos and de Rooij 2017*b*; Sinclair, McConnell and Green 2012). Both field experiments are uniquely well suited for our purposes as households were randomly assigned to treatment and control, and then one person per household was sampled to receive a mobilization message, either by phone or in the form of a mailer. Both studies subsequently assessed the extent to which the effect of these direct mobilization messages on the randomly selected household member spilled over onto the second household member, i.e. whether the latter’s likelihood to vote also increased as a result of the selected household member being targeted. The advantage of our approach is threefold. First, by focusing on opposite-sex, two-person households we can test whether the causal effect of the mobilization messages on the likelihood to vote differs depending on whether the man or the woman in the household was randomly selected for treatment.² Second, we can likewise test whether GOTV spillover effects are larger when the spillover goes from men to women than from women to men, or vice versa. Third, we do not rely on any self-reported,

¹We recognize that we conflate gender and sex here. The literature we reference does mostly not distinguish the two and uses a binary conception of gender/sex (women/men). This is often a reflection of the data being used. The data we rely on here also restricts us to comparing men and women.

²Our analysis is limited to household members of the opposite sex due to reasons of statistical power. There are not enough same-sex households in our samples.

and potentially biased, measures of political discussion between household members. Instead, we rely on the assumption that any spillover of the treatment within households is likely a result of discussion between household members. We elaborate below on why we make this assumption.

2. Gender Differences in Mobilization

Mobilization is an intentional and non-coercive action by a political actor or ordinary citizen to cause, or with the intent to cause, another individual to participate in an election. Direct mobilization is when political actors attempt to mobilize voters, for instance by using typical Get-Out-The-Vote (GOTV) strategies such as door-to-door canvassing or phone calls by party volunteers. Mobilization can also be indirect when direct mobilization effects diffuse from citizen to citizen within social networks (Rolfe 2012, 122-123).

2.1 Direct Mobilization

The question as to whether women are more likely to be successfully mobilized to vote than men has not received much attention in the literature.³ Field experimental studies, aimed at testing the effectiveness at increasing turnout of different GOTV strategies in a variety of contexts, commonly include pre-treatment covariates such as age, gender and past turnout to enhance the precision of estimated average treatment effects. Although the data from these studies might be used to test whether the effectiveness of treatment varies by gender, scholars often refrain from testing such heterogeneous treatment effects because such tests are commonly underpowered and/or because such tests were not pre-registered as part of the original design. In a rare and well-powered study that does examine gender differences in the response to social pressure messages to turn out to vote Weinschenk, Panagopoulos, Drabot and van der Linden (2018, 53) find “little to no evidence of gender differences in receptivity to social pressure cues in the context of political participation”. Thus, we are not aware of any evidence of a difference between women and men in their likelihood to be responsive to direct mobilization efforts.

³A separate, but related, question is whether campaigns are more or less likely to *target* women than men in their mobilization attempts. Certainly, gender is one of many factors that is taken into account in data-driven campaigning by partisan groups; however, geographic and more proximate factors such as past turnout or likelihood to pick up the phone or open the door when contacted are most useful to campaigns in mobilizing the vote (Nickerson and Rogers 2014). We leave this question aside for now.

When discussing heterogeneous treatment effects such as those by gender, it is important to recognize that the effect of the moderator (e.g. gender) on the causal relationship between the treatment (mobilization message) and the outcome (turnout) is not in itself causal, as the moderator is not randomly assigned. As Nickerson and Rogers (2014, 59) state: “For instance, a campaign data analyst may discover that women are more responsive to a treatment than men, but since gender was not randomly manipulated by the campaign it is impossible to know that gender caused the differential response to treatment. The campaign data analyst only knows that gender is correlated with treatment responsiveness. Thus, even the search for moderators of the treatment effect in an experiment is essentially observational in nature.” In our study gender is also not randomly assigned; however, random assignment does determine whether a man or a woman in an opposite-sex, two-person household is selected for treatment.

2.2 Indirect Mobilization

While we have no reason to expect any gender differences in direct mobilization effects, we might expect gender differences in the transmission of these messages within social networks. A second question then is whether men and women differ in the extent to which they are responsive to social influence by household members. And relatedly, do women and men differ in their likelihood of passing on direct mobilization efforts through political discussion?

Indirect mobilization is a form of social influence, which occurs “when a change in an individual’s feelings, attitudes, or behaviors is caused by another individual in that network” (Foos and de Rooij 2019, 5). When it comes to turnout, Bhatti, Dahlggaard, Hansen and Hansen (2017) show how cohabitation increases the likelihood to vote, and suggest that this is a result of social influence. In examining whether men and women are differentially responsive to social influence from their partners (couples are by far the largest proportion of adult cohabitants), most of the literature has focused on political opinion formation and vote choice, rather than the turnout decision. There are reasons to think spouses may indeed influence each other differently, at least during certain phases of marriage or cohabitation. For instance, Stoker and Jennings (2005) argue that, in young marriages, the male spouse’s influence is dominant relative to that of the female’s over her male spouse. Though it is certain that the degree of consonance of spouses is in general higher than any other two people chosen at random to cohabit, evidence suggests that the male spouse

convinces the other party to move in their political direction. As the marriage ages, the influence gap between spouses diminishes, having female spouses take the lead in some respects, such as race policy and school prayer, and male spouses in others, such as party identification and vote choice. Osborn and Mendez (2011) also support political consonance among spouses, yet they find that spouses themselves misperceive the degree of political agreement with their partner, thinking they disagree more than they objectively do. Such biased perceptions suggest that social influence between spouses might not be significantly divergent, particularly as cohabitation matures over time.

However, identifying social influence, including indirect mobilization, within households as distinct from self-selection of similar individuals into the household and shared contextual experiences can be methodologically challenging (for a discussion see Foos and de Rooij (2019) and Fowler, Heaney, Nickerson, Padgett and Sinclair (2011)). By randomly assigning households to treatment and control conditions, spillover experiments allow indirect mobilization within households and shared contextual experiences resulting from the treatment to be empirically distinguished from pre-existing compositional and contextual differences. Such spillover experiments have shown that the effect of mobilization messages indeed spill over on household members who were not randomly selected to be part of the experiment (Foos and de Rooij 2017*b*; Nickerson 2008; Sinclair, McConnell and Green 2012).

Whether this spillover, also called contagion, results from actual discussion remains open for discussion. Social influence can also be exerted non-verbally, for instance, when one household member feels social pressure after observing another going out to vote. Or, household members might both be exposed by the treatment, for instance, when one household member reads a piece of direct mail addressed to the other household member. Here, we assume that spillover occurs through discussion – or at least because one household member has talked about the mobilization message and/or about voting to the other. Not only do we believe this to be the most likely mechanism, we also make this assumption based on prior work, in which we show how spillover effects differ depending on the actual content of a message, which varied in partisan intensity (Foos and de Rooij 2017*b*). So, an important question is whether men and women differ in their likelihood of discussing politics within the household.

Studies show that when asked who they discuss politics with, women are more likely than men

to list members of their intimate network - i.e. family and close friends (Elder and Greene 2003; Huckfeldt and Sprague 1995, 198). Two reasons have been suggested for women's preference for family members as discussion partners. First, women are said to value close relationships more than men and such relationships consequently play a greater role in the political decision-making process for women than for men (Elder and Greene 2003, 388). Second, (young) women are more likely to obtain political information through discussion that is characterized by consensus more than by conflict than (young) men and have been found to be more likely to find political discussion uncomfortable than men, and to be less likely to initiate political talk than men (Wolak and McDevitt 2011). As such, the family might particularly appeal to women as a safe context in which to exchange political information and practice political debating skills (Wolak and McDevitt 2011, 521). Although women might be more likely to list family members as discussion partners, other studies show that within the family men are at least equally, if not more, likely to discuss politics.

In sum, we would expect women to be at least equally, if not more, likely to discuss politics within the context of their household, than men. Thus, to the extent that indirect mobilization differs between men and women, we would expect women to be better at mobilizing men, than vice versa.

To test this hypothesis, we rely on two previously conducted field experiments in the United States and the United Kingdom aimed at mobilizing household members to vote. The first experiment was conducted by Foos and de Rooij (2017*a,b*) in Birmingham, United Kingdom and consisted in phone canvassing, whereas the second experiment was conducted by Sinclair, McConnell and Green (2012) in Chicago, United States using social-pressure mailings. Our analysis will focus on opposite-sex, two-person households, and we analyze whether GOTV spillover effects are larger when the spillover goes from men to women than from women to men. Both field experiments are uniquely well suited for this type of analysis because households were randomly assigned to treatment and control, and then one person per household was sampled to receive the phone-call or the mailer.

The paper starts describing the experimental set-up for both interventions and replicates the analysis, differentiating between spillover effects in households where the male household member was randomly assigned to be contacted, and households where the female household member was assigned to be contacted. Since we are looking at two-voter households that means that the spillover

in the first instance occurs from men to women and in the second instance from women to men. Our well-powered results suggest that while there is no difference in the effectiveness of direct mobilization efforts on the propensity to vote between women and men, women are 2-3 percentage-points more effective at indirectly mobilizing men in their households than the other way around; however, only in the US experiment is this difference statistically significant. Thus, men are if not more, than at least equally susceptible to social influence within the household, and women can mobilize men to go out and vote at least as well as men can mobilize women. This is despite well-documented gender differences in political knowledge and interest.

3. Experimental Set-up

We rely on two previously conducted randomized campaign experiments to identify indirect mobilization effects between male and female household members. The first experiment was conducted by Foos and de Rooij (2017*a,b*) in several electoral wards in the British city of Birmingham in the context of the West Midlands Police and Crime Commissioner (PCC) election, which was held on November 15th 2012.⁴ The experiment consisted of telephone calls from Labour Party volunteers reminding citizens to vote and to encourage them to do so for a particular candidate. The objective was to identify mobilization effects conditional on voters' previously stated party preferences. Voter data came from the Labour Party's extensive targeting database that included not only voter contact information and party preference, but also information on gender, year of birth, whether individuals were registered as postal voters, the electoral ward in which they resided, and individuals' validated turnout histories (as available from public records).⁵ First, one voter in each of the 13,065 households in the database with landline numbers (of which 5,190 were two-voter households) was randomly selected to be in the experiment. Next, these experimental subjects were stratified into three blocks based on their latest recorded party preference. Then, within each group experimental subjects (and their household members) were randomly assigned to either one of two treatment groups or to the control group, which was assigned not to receive any form of contact from the campaign. The first treatment group was assigned to receive a 'high partisan

⁴For the replication data, see <http://dx.doi.org/10.7910/DVN/ZFLG25>.

⁵Data were anonymized before being shared with the researchers.

intensity' message, which emphasized the candidate's connection to the Labour Party. The second treatment group was assigned to receive a 'low partisan intensity' message, which avoided any mention of the Labour Party and simply focused on the candidate's credentials. For the purpose of this study, we pool both treatment groups. After the election, the local Labour Party updated the database with validated turnout data for the PCC election from the marked electoral register for both the experimental subjects and their household members.

The second experiment was conducted by Sinclair, McConnell and Green (2012) during a special election in 2009 in Chicago.⁶ The experiment consisted of sending social-pressure mailings to households in the treatment group. These mailings took the form of postcards reminding voters to do their civic duty and vote, and of whether they voted or not in two previous elections. The objective was to examine the extent to which the effect of these mailings on turnout was transmitted by experimental subjects to their household members and to their neighbors. The data used in the experiment was publicly-available voter registration data, and included information on gender, race and individuals' turnout histories in nine previous elections. Official turnout records were updated after the 2009 election. The experiment was designed as a large-scale voter mobilization multilevel experiment and included 71,127 individuals (of which 33,274 lived in two-voter households), 47,851 eligible households and 4,897 eligible zipcodes in the 5th Congressional District. Eligible individuals were those residents who had been eligible to vote during the 2006 and 2008 elections; eligible households were those with one and up to three registered voters. Finally, eligible zipcodes were 9-digit zipcodes with at least two households with two eligible individuals and containing between 3 and 15 households in total. Random assignment to treatment and control first occurred at the zip code level. To identify spillover effects within neighborhoods, households in 75% of zip code areas were assigned with different probabilities to treatment and control conditions. All households in the remaining 25% of zip code areas were assigned to control. Sinclair, McConnell and Green (2012) found no evidence of spillover effects between households and so, for the purposes of our study, we ignore this level of assignment. Then, within households assigned to treatment, one individual was randomly selected to be treated.

Key for the purpose of our study is that both experiments randomly sampled one individual per household to be part of the experimental sample, before assigning these experimental subjects

⁶For the replication data, see: <https://isps.yale.edu/research/publications/isps12-025>

to treatment and control groups. This was done to minimize violations of the non-interference assumption (SUTVA). The household members of these experimental subjects were excluded from the experiment and were not directly treated. By design, both experiments allow tests of the extent to which the treatment had an indirect effect on turnout of these excluded, non-experimental household members (i.e. whether treatment effects spilled over), and both Foos and de Rooij (2017*b*) and Sinclair, McConnell and Green (2012) indeed find evidence to support within household spillover. Here, we test whether spillover effects differ depending on whether the individual assigned to receive a social pressure leaflet or a partisan phone call was a man or a woman. We limit our study to two-voter households consisting of a man and a woman. The vast majority of these households likely consist of heterosexual couples.

4. Analysis and Results

We start by estimating the direct impact of treatment assignment in each of the two experiments on turnout of household members who were randomly selected to be part of the experiment (the experimental subjects), and the indirect impact of treatment assignment on their excluded household members (the non-experimental subjects). This first step is simply to replicate the significant direct and indirect effects found in both original experiments when both samples are restricted to two-voter households consisting of a man and a woman. We estimate these direct and indirect Intent-to-Treat (ITT) effects with the following two linear models:

$$Y_i = \alpha + \beta Z_{ij} + \epsilon_i \quad (1)$$

$$Y_j = \alpha + \beta Z_{ij} + \epsilon_j \quad (2)$$

where Y_i is validated individual-level turnout (1 or 0) for household members i who were randomly sampled to be included in the experiment (either in the treatment or control group), and Y_j is validated individual-level turnout for their household members j , who were randomly sampled to be excluded from the experiment. α is the baseline turnout rate, i.e. that of the control group. Z_{ij} is assignment of the household to either treatment (1) or control (0) by virtue of household member i being assigned to treatment or control, and ϵ_i , and ϵ_j are the error terms. We use ordinary least squares regressions with robust standard errors, clustered by household, to estimate these models. All models are estimated with (covariate-adjusted) and without (covariate-unadjusted)

pre-treatment covariates. The models using the data from Foos and de Rooij (2017*b*) also include block fixed effects for party preference to account for the block-randomized design of the experiment. The results are presented in Table 1.

Table 1 shows that the social pressure leaflets (Sinclair, McConnell and Green 2012) and the partisan phone calls (Foos and de Rooij 2017*b*) successfully mobilized voters to turn out. Both experimental and non-experimental subjects in households assigned to treatment were more likely to vote than control households. We estimate direct effects of 4.2 percentage-points for the Sinclair, McConnell and Green (2012) experiment and 3.0 percentage-points for the Foos and de Rooij (2017*b*) experiment (covariate-adjusted). Indirect effects are estimated to be larger in the Foos and de Rooij (2017*b*) experiment than in the Sinclair, McConnell and Green (2012) experiment: 5.1 percentage-points compared to 1.7 percentage-points.

Table 1: ITT of social pressure leaflet and partisan phone call on turnout of experimental and non-experimental subjects

	Sinclair et al. (2012)		Foos and de Rooij (2017 <i>b</i>)	
	Direct effects	Indirect effects	Direct effects	Indirect effects
Control mean	.237*** (.005)	.237*** (.005)	.240*** (.013)	.242*** (.013)
Treatment (covariate-unadjusted)	.043*** (.007)	.017* (.007)	.031* (.013)	.051*** (.013)
Treatment (covariate-adjusted)	.042*** (.007)	.017* (.007)	.030* (.013)	.051*** (.013)
Block fixed effects	No	No	Yes	Yes
Cluster standard errors	Yes	Yes	Yes	Yes
N individual	19,815	19,799	6,003	5,998
N cluster	13,424	13,408	3,855	3,850

Note: Standard errors clustered at household level. * <0.05 , ** <0.01 , *** <0.001

Next, we estimate the direct and indirect Intent-to-Treat (ITT) effects with models that include the gender composition of the household and the interactions between treatment assignment and gender composition. We estimate the following two linear models, in which X_{ij} indicates whether the woman (1) or the man (0) in the household was assigned to the experimental sample:

$$Y_i = \alpha + \beta_1 Z_{ij} + \beta_2 X_{ij} + \beta_3 X_{ij} * Z_{ij} + \epsilon_i \quad (3)$$

$$Y_j = \alpha + \beta_1 Z_{ij} + \beta_2 X_{ij} + \beta_3 X_{ij} * Z_{ij} + \epsilon_j \quad (4)$$

We again use ordinary least squares regressions with robust standard errors, clustered by house-

hold, to estimate these models, and provide the estimates both with and without pre-treatment covariates. Models I and III in Table 2 show the unadjusted estimates, and models II and IV show the covariate-adjusted estimates. The models using the data from Foos and de Rooij (2017b) also include block fixed effects for party preference.

Table 2: ITT of social pressure leaflet and partisan phone call on turnout of experimental and non-experimental subjects, conditional on whether the man or woman in the household was assigned to treatment

	Sinclair et al (2012)				Foos and de Rooij (2017)			
	Direct		Indirect		Direct		Indirect	
	I	II	III	IV	I	II	III	IV
Control mean	.239*** (.005)	-.028*** (.008)	.235*** (.005)	-.023** (.008)	.235*** (.014)	.193*** (.016)	.247*** (.014)	.192*** (.016)
Treatment	.052*** (.010)	.041*** (.009)	-.004 (.009)	.002 (.008)	.040* (.018)	.035* (.017)	.041* (.018)	.041* (.016)
Female assigned subject	-.004 (.005)	-.004 (.004)	.004 (.005)	.004 (.004)	.009 (.012)	.005 (.011)	-.009 (.012)	-.004 (.011)
Female assigned subject x treatment	-.019 (.012)	.001 (.011)	.041*** (.012)	.029** (.011)	-.019 (.024)	-.011 (.023)	.020 (.024)	.021 (.023)
Block fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Cluster standard errors	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates included	No	Yes	No	Yes	No	Yes	No	Yes
N individual	19,815		19,799		6,003		5,998	
N cluster	13,424		13,408		3,855		3,850	

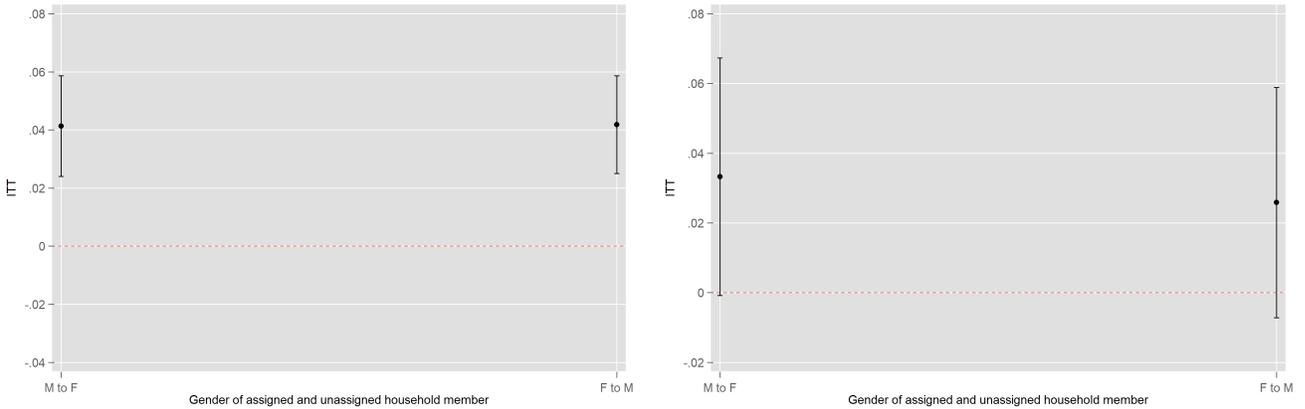
Note: Standard errors clustered at the household level. * <0.05 , ** <0.01 , *** <0.001

We focus our discussion on the covariate-adjusted estimates. In Figure 1 and 2 we graphically present the estimated conditional direct and indirect ITTs from Models II and IV in Table 2. Figure 1a shows that the direct treatment effect of being assigned to receive the social pressure leaflet on turnout of the assigned household members does not vary substantially or significantly depending on whether the contacted household member was a man or a woman; both effects are estimated to be just over 4 percentage-points. Likewise, Figure 1b shows that the direct effect of being assigned to receive a partisan mobilization phone call on turnout of the assigned household members also does not vary significantly depending on whether the contacted household member was a man or a woman, even though the direct effect for women is estimated to be 1 percentage-point smaller than that for men (approximately 2.5 versus 3.5 percentage-points). These results confirm previous findings of no significant differences between men and women in their propensity to respond to mobilization appeals (Weinschenk et al. 2018).

Figure 2 shows that spillover effects are 2-3 percentage-points greater from women to men

than vice versa. When a woman in a household is assigned to receive a social pressure leaflet in the US experiment, the probability that the uncontacted man in that household turns out to vote significantly increases by 3 percentage-points (Figure 2a). Strikingly, when a man is assigned to receive a social pressure leaflet, there is no significant spillover effect on the woman in the household. And when a woman in a household is assigned to receive a partisan phone call in the UK experiment, the probability that the uncontacted man in that household turns out to vote is about 6 percentage-points (Figure 2b). The probability of a woman turning out to vote when the man in the household was assigned to receive a call is 2 percentage-points lower; however, this difference is not statistically significant.

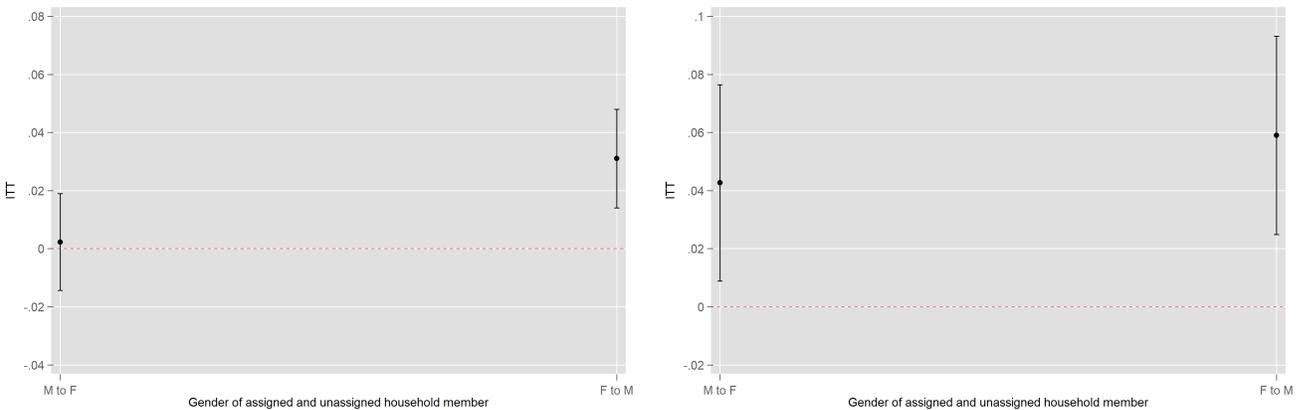
Figure 1: Direct mobilization effects conditional on gender, covariate-adjusted



a) Sinclair et al. (2012)

b) Foos and de Rooij (2017b)

Figure 2: Indirect mobilization effects conditional on gender, covariate-adjusted



a) Sinclair et al. (2012)

b) Foos and de Rooij (2017b)

In sum, while we find no evidence for a gender difference in turnout rates resulting from direct mobilization efforts, we find evidence suggesting that women are at least as, if not more, effective at indirectly mobilizing their male household members to go out and vote.

5. Conclusion and Discussion

Overall, and in line with previous research, we find no difference in the effectiveness of direct mobilization efforts on the propensity to vote between women and men. In the UK, women also appear at least as effective at mobilizing their male household members as men are at mobilizing women. These results are in line with the broader literature which finds that there is “little evidence of overall gender differences” (Carli 2017, 45) in influenceability between men and women outside the context of intimate networks.

Intriguingly, gender differences do materialise in the US-context, but men appear more susceptible to social influence than women. In fact, indirect mobilization effects for men in the US experiment are similar in size as in the UK experiment, while there are no indirect mobilization effects for women in the US.

Taken together then, the evidence seems to suggest that women are more likely than men to (indicate to) turn to family members as their main political discussion partners, but if political discussion within the family does occur, women are as effective at social influence as men. If at all, they are more effective, at least in the US context. Questions remain though, as to whether gender differences exist in indirect mobilization depending on the political issue under discussion, the nature of the discussion and the gender composition of the larger network.

References

- Atkeson, Lonna Rae and Ronald B Rapoport. 2003. “The more things change the more they stay the same: Examining gender differences in political attitude expression, 1952–2000.” *Public Opinion Quarterly* 67(4):495–521.
- Banwart, Mary Christine. 2007. “Gender and young voters in 2004: The influence of perceived knowledge and interest.” *American Behavioral Scientist* 50(9):1152–1168.
- Bennett, Linda LM and Stephen Earl Bennett. 1989. “Enduring gender differences in political interest: The impact of socialization and political dispositions.” *American Politics Quarterly* 17(1):105–122.
- Bhatti, Yosef, Jens Olav Dahlgaard, Jonas Hedegaard Hansen and Kasper M. Hansen. 2017. “Living Together, Voting Together: Cohabitation Causes Concordance in Turnout Behavior and Increases Turnout.” *Working paper* .
- Carli, Linda L. 2017. *The Oxford Handbook of Social Influence*. Oxford University Press chapter Social Influence and Gender, pp. 33–51.

- Carpini, Michael X Delli and Scott Keeter. 1996. *What Americans know about politics and why it matters*. New Haven, CT: Yale University Press.
- Carpini, Michael X Delli and Scott Keeter. 2000. *Gender and American Politics: Women, Men and the Political Process*. M.E. Sharpe chapter Gender and political knowledge, pp. 186–204.
- Coffé, Hilde and Catherine Bolzendahl. 2010. “Same game, different rules? Gender differences in political participation.” *Sex roles* 62(5-6):318–333.
- Dow, Jay K. 2009. “Gender differences in political knowledge: Distinguishing characteristics-based and returns-based differences.” *Political Behavior* 31(1):117–136.
- Elder, Laurel and Steven Greene. 2003. “Political information, gender and the vote: the differential impact of organizations, personal discussion, and the media on the electoral decisions of women and men.” *The Social Science Journal* 40(3):385–399.
- Foos, Florian and Eline A. de Rooij. 2017a. “The Role of Partisan Cues in Voter Mobilization Campaigns: Evidence from a Randomized Field Experiment.” *Electoral Studies* 45(1):63–74.
- Foos, Florian and Eline A. de Rooij. 2019. *The Oxford Handbook of Electoral Persuasion*. chapter Voter mobilization in intimate networks, pp. 1–24.
- Foos, Florian and Eline de Rooij. 2017b. “All in the Family: Partisan Disagreement and Electoral Mobilization in Intimate Networks - a Spillover Experiment.” *American Journal of Political Science* 61(2):289–304.
- Fowler, James, Michael Heaney, David Nickerson, John Padgett and Betsy Sinclair. 2011. “Causality in Political Networks.” *American Politics Research* 39(2):437–480.
- Gidengil, Elisabeth, Janine Giles and Melanee Thomas. 2008. “The gender gap in self-perceived understanding of politics in Canada and the United States.” *Politics & Gender* 4(4):535–561.
- Huckfeldt, Robert R. and John D. Sprague. 1995. *Citizens, Politics and Social Communication: Information and Influence in an Election Campaign*. Cambridge: Cambridge University Press.
- Matland, Richard E. and Gregg R. Murray. 2012. “An Experimental Test of Mobilization Effects in a Latino Community.” *Political Research Quarterly* 65(1):192–205.
URL: <http://journals.sagepub.com/doi/10.1177/1065912910388192>
- Mondak, Jeffery J and Mary R Anderson. 2004. “The knowledge gap: A reexamination of gender-based differences in political knowledge.” *The Journal of Politics* 66(2):492–512.
- Morehouse Mendez, Jeanette and Tracy Osborn. 2010. “Gender and the perception of knowledge in political discussion.” *Political Research Quarterly* 63(2):269–279.
- Nickerson, David W. 2008. “Is Voting Contagious? Evidence from Two Field Experiments.” *American Political Science Review* 102(1):49–57.
- Nickerson, David W., Ryan D. Friedrichs and David C. King. 2006. “Partisan Mobilization Campaigns in the Field: Results from a Statewide Turnout Experiment in Michigan.” *Political Research Quarterly* 59(1):85–97.
- Nickerson, David W. and Todd Rogers. 2014. “Political campaigns and big data.” *Journal of Economic Perspectives* 28(2):51–74.
- Osborn, Tracy and Jeanette Morehouse Mendez. 2011. “Two Become One? Spouses and Agreement in Political Opinions.” *American Politics Research* 39(5):783–803.
- Rolfe, Meredith. 2012. *Voter Turnout: A Social Theory of Political Participation*. Cambridge: Cambridge University Press.
- Sinclair, Betsy, Margaret McConnell and Donald P. Green. 2012. “Detecting Spillover Effects: Design and Analysis of Multi-level Experiments.” *American Journal of Political Science* 56(4):1055–1069.

- Stoker, Laura and M. Kent Jennings. 2005. Political Similarity and Influence between Husbands and Wives. In *The Social Logic of Politics: Personal Networks as Contexts for Political Behavior*, ed. Alan S Zuckerman. Philadelphia: Temple University Press pp. 51–74.
- Verba, Sidney, Nancy Burns and Kay Lehman Schlozman. 1997. “Knowing and caring about politics: Gender and political engagement.” *The Journal of Politics* 59(4):1051–1072.
- Weinschenk, Aaron C., Costas Panagopoulos, Karly Drabot and Sander van der Linden. 2018. “Gender and social conformity: Do men and women respond differently to social pressure to vote?” *Social Influence* 13(2):53–64.
- Wolak, Jennifer and Michael McDevitt. 2011. “The Roots of the Gender Gap in Political Knowledge in Adolescence.” *Political Behavior* 33(3):505–533.