Online Appendix

Refugee Networks and Cooperation

Evidence From a Social-Network Experiment with

Syrian Refugees in Lebanon and Jordan *

Daniel T. R. Masterson

November 7, 2017

*Daniel T. R. Masterson, Ph.D. Candidate, Yale University, Department of Political Science. daniel.masterson@yale.edu. This research is approved by the Yale Human Subjects Committee under protocols HSC #1603017430 and HSC # 1508016386

Contents

1 Research Design

Figure 1 presents a schematic of the research design.

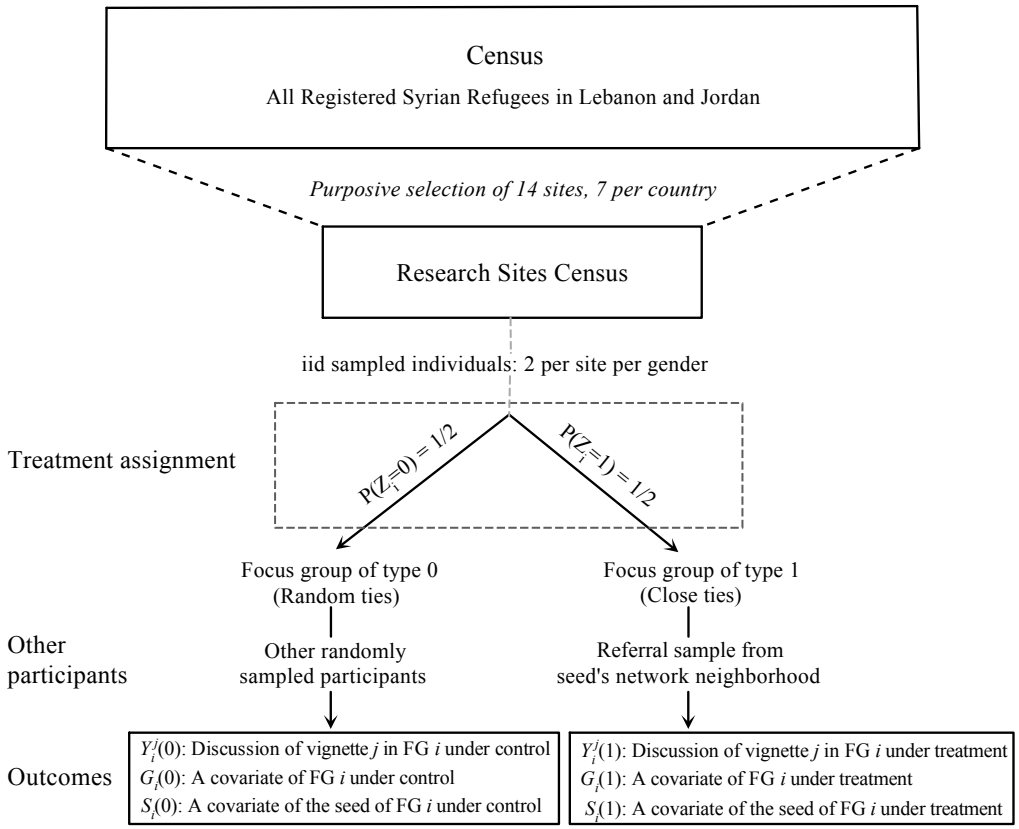

Figure 1: Research Design

1.1 Site Selection

In Lebanon and Jordan, I selected the three governorates in each country with the largest Syrian populations according to UNHCR records. Within each of the governorates, I selected a high-population site and an average-population site. To select an average-sized site I applied both quantitate and qualitative selection criteria. There was generally one clear high-population site per governorate. To select the average-population site, 'average' is defined as the 10% trimmed mean of Syrian town-level populations within that governorate – roughly that is the mean of the middle 80% of the data. I used this statistic because, in this context, both the mean and median have their respective problems. The mean might be too high an estimate of a 'typical' town for each region due to a small number of towns with very large Syrian populations in each region. The median might be too low as an estimate of a 'typical' town in each region due to the large number of towns with a single-digit registered Syrian population size. In short, within each governorate I sampled two sites: a major Syrian population center and a typically sized Syrian community for the region.

Even after applying this quantitative criterion, multiple sites in each governorate were feasible options for the typical town, and to further narrow the list of options I attempted to exclude areas with exceptional characteristics for that region (e.g.,

Christian town in Muslim region, rich town in a poor region). Selecting sites that process was based on my knowledge of the research sites and informal interviews with key informants from potential research sites the candidate areas who could comment on the characteristics of the potential research sites.

In addition to the six sites across three governorates, in each country I sought to include camp settings. In Jordan this was an official UNHCR-run camp, the Zaatari camp, in addition to the six other research sites. In Lebanon, because rural Bekaa is so populated by Syrian camps, in place of an average-sized town and a camp site, for the Bekaa region I included two towns with informal camps in the Bekaa: one with peri-urban, high-density camps, and a second with rural, low-density camps. In Lebanon, the sites are Burj-el-Barajneh in urban Beirut and Chhim outside Beirut; Baalbek, Khiara (rural camps site), and El-Marj (peri-urban camps site) in the Bekaa; and Tripoli (Tebbaneh) and Kouachra in the North. In Jordan research sites were Amman and Shafa Badran in the greater Amman area, Mafraq and Irhab in the greater Mafraq greater, Irbid and Huwwarah in the greater Irbid area, and Zaatari camp.

1.2 Pre-experimental Focus Group Recruitment

Recruitment for the focus groups was based on a random sample of UNHCR registration records, drawn from all registered refugees living in the research sites between the ages of 20–50. In defining the age range I sought to achieve a balance between breadth to capture variation and narrowness to avoid deferential youth feeling inhibited from speaking around much-older participants.

Inclusion criteria required that participants in both experimental arms be between the ages of 20 and 50, live in the study site, be the same gender as the seed, not be members of another participant's nuclear family or household. I deemed it sufficiently unlikely that randomly sampled participants would be drawn from the same family that I did not take any steps to modify random sampling to prevent, but I did monitor whether it occurred. Across the 28 control-group focus groups, moderators reported that one randomly sampled focus group had two members of the same household. In a parallel violation of inclusion criteria, one of the referral-sampled focus groups included four members of the same household. Results are robust to controlling for a dummy for focus groups that included members of the same household.

Focus groups were either all-male or all-female, with no mixed-gender focus groups. This represents a trade-off in the research design between attrition and the realism of the focus group setting. Some members of the Syrian population

would not be willing to sit in focus groups with members of the other sex, although in most Syrian communities the sexes frequently intermix in social settings outside the family.

Recruiters were provided with four lists of names from UNHCR data for each site, one per gender per experimental group. Within each gender, selection into one group or another was randomized and the order of names in each contact list was randomized. Recruiters were instructed to contact people by phone in the order listed until recruiting a sufficient number of participants.

The four documents for each site included: a. Call list for female random-ties focus group b. Call list for female close-ties focus group c. Call list for male random-ties focus group d. Call list for male close-ties focus group

Recruiters read all people contacted a consent script approved by the Yale Human Subjects Committee under protocol HSC # 1603017430. Another consent script was read to participants before focus groups were conducted.

Participants were paid a $20 incentive for their participation, in consideration of the fact that the two-hour-long focus group and travel to and from the meeting site might keep someone from a normal workday. $20 is a typical daily wage for manual labor for Syrians in Lebanon and Jordan.

1.3 Seed Drop-out Backup Plan

For close-ties groups, 13 people were contacted to participate. And for random-ties groups 11 or 12 people were contacted to participate (11 in Lebanon, and I increased it to 12 for Jordan). More than 10 were recruited to account for no-shows on the day of focus-group conduct. In only a small number of focus groups did more than 10 people show up, and in that case the last people to show up were politely turned away. If they had travelled any significant distance to get to the meeting point, they were discreetly paid the participation incentive.

1.4 Treatment Randomization

I used block randomization at the design stage, creating treatment and controls groups that are balanced with respect to country, site, and gender, based on the expectation that these variables are likely highly predictive of cooperation. Block randomization can ensure that the treatment and control groups have equal proportions of participants in each of the notional cells in the $2 \times 2 \times 7$ research design (that is, two genders, two countries, and seven sites per country). When the blocking variables are predictive of outcomes, blocking improves precision by preventing chance correlations between treatment assignment and baseline covariates (Miratrix, Sekhon and Yu, 2013).

1.5 Experimental Recruitment Methods

From each study site, I randomly vary the network subsampling method used to recruit participants. Control-group focus groups were assigned to the low-density condition where I recruited participants through a random sample from the UNHCR registration database after filtering records to those that met the inclusion criteria (ages 20-50 and living in the study sites). The second experimental condition, the high-density groups, comprised one individual randomly selected from UNHCR records, who served as a seed for a two-wave, three-contact referral sample, where the seed and those in the first wave were asked to refer the three people outside their nuclear family who they interacted with most in the last two weeks — whether by phone, WhatsApp, or face-to-face. All participants in the high-density group needed to meet the same inclusion criteria as participants in the low-density groups — that is, they needed to be between the ages of 20 and 50, live in the study site, and be registered with UNHCR.

Random variation in the recruitment strategy used to construct focus groups allows us to study the impact of social-network structure on participants' ability to engage in collective problem solving. Four focus groups were conducted at each site: (i) Female close-ties, (ii) Female random-ties, (iii) Male close-ties, and (iv) Male random-ties.

Random ties To create a group of individuals with sparse social networks, representing the baseline of expected ties between people in the research sites, the 'random-ties' groups comprise ten individuals randomly selected from UNHCR data from each research site.

Close ties To create a group of individuals with dense social ties, approximating a displaced community where people settled in a location with numerous people they already know, the 'close-ties' groups comprise one 'seed' randomly selected from UNHCR data, and then 12 close acquaintances drawn from a systematic two-wave, three-referral snowball sample. Snowball referrals must meet a number of criteria, as discussed below.

In gathering referrals for the close-ties focus group, people were requested to give the names of the three Syrians who they had interacted with most over the past two weeks, where interaction was explained to include face-to-face communication and electronic communication via telephone, texting, WhatsApp, Facebook, etc. These three referrals needed to be between the ages of 20 and 50, live in the same town as the referrer, be the same gender as the referrer, not be members of the referrer's nuclear family, and not live in the referrer's household. Upon contacting these people, these new contacts were only included if they stated that they would be willing and able to refer three people themselves who they had interacted with frequently over

the past two weeks. This inclusion criterion was applied to potential participants in the random-ties group as well, establishing that both potential outcomes are defined for all units in the study.

The referral sampling method I designed and used is distinct from both standard respondent-driven sampling (RDS) and snowball sampling. Magnani et al. (2005) and Heckathorn (2011) describe in detail the specific meanings of snowball sampling and RDS. Snowball sampling is a nonprobability approach to sampling when the researcher does not have a list of population members (that is, a 'sampling frame'), and hence the seeds for the snowball sample are drawn from a convenience sample. In RDS, researchers can estimate selection probabilities by basic mapping of people's network (for example, asking them how many potential recruits they know). Potential recruits are commonly given a coupon by the referrer, and the referred respondent must present themselves at the study site. Researchers never need the names or contact information of potential participants. The referral sampling method in this study is not a convenience sample, as seeds were drawn at random from the UNHCR census, but unlike RDS research staff contacted referred potential participants rather than using a self-referral method.

Two finer points of the network intervention deserve mention. First, the density of social ties in each focus group is defined according to pre-existing ties. Once

everyone meets for the focus group, they all form ties so the network is a complete graph. Second, it is important to clarify that I do not assert causal identification based on intervening on participants' social networks. This is an experiment on measuring network characteristics, but not an experiment on network characteristics. I identify causal effects based on manipulation of the pre-existing network and not the changing participants' social networks.

1.6 Focus-Group Experiment Descriptives

Based on participant self-reports, the average participant in the random-ties groups had pre-existing weak ties with 11% of the other focus-group participants, compared with 66% in the close-ties groups. The average participant in the random-ties groups had pre-existing strong ties with 5% of other focus-group participants, compared with 42% in the close-ties groups. Weak ties were defined as people in the group who the participant knew by name before the day of the focus group. Strong ties were defined as people who the participant regularly exchanged visits with.

The average focus group had 8.7 participants, and focus groups ranged in size from 6 to 10 participants. The population is UNHCR-registered Syrians refugees in Lebanon and Jordan, which includes a vast majority of Syrians living in the two countries. Although precise numbers are lacking, roughly 75% of Syrians living in

Lebanon are registered, and Jordan more than 90% in Jordan are registered. UNHCR declared a (*de facto*) blanket refugee-status determination for all Syrians, rather than proceeding on a case-by-case basis as is done in many refugee registration processes, so any Syrian who seeks to register qualifies for refugee status. Based on my fieldwork, unregistered Syrians tend to be either upper- and middle-class Syrians who do not view themselves as 'refugees' in need of assistance from the UN or others who view the risks of legibility to the Jordanian and Lebanese governments as outweighing the benefits of aid provision. This latter class of unregistered often suspect, correctly in fact, that UNHCR shares its registration records with the host governments.

Attendance rates were balanced across treatment and control arms. Although attendance rates across treatment and control arms are balanced, due to an error in recruitment, low-density groups were one person smaller on average than high-density groups. One could argue that existing theory would predict that this difference would militate against the theoretical predictions and findings. I discuss this design flaw in depth in the appendix.

The difference in the number of people contacted was a flaw in the execution of the research design. Recruiters in Lebanon contacted only 11 people to fill 10 spots in the low-density groups, and recruiters in Jordan contacted only 12 people to fill 10 spots in the low-density groups. This should have been held constant at 13 to match

the number of people contacted for the high-density group. The attendance rates across treatment control are balanced, but the number of participants is imbalanced due to differential recruiting targets. In both countries the high-density groups were larger by about one person on average. 8.2 vs. 9.3 overall, 8.7 vs. 9.6 in Lebanon, and 7.6 vs. 8.9 in Jordan. In both countries the minimum and maximum focus-group sizes were 6 and 10, with a standard deviation of the number of participants was 1.09 in Lebanon and 1.38 in Jordan. One could argue that existing theory suggests that the difference in group size would create a bias against both central findings. If Olson (1965) is correct that larger groups are more likely to atomize because cooperation is harder, we would be less likely to see the larger high-density group engaging in dialogue with each other – the opposite of what we observe. Second, a larger group should have more information about outside resources for responding to public-goods problems, and we would be less likely to see the smaller low-density group turning to outside solutions – again, the opposite of what we observe.

1.7 Data Collection and Focus Group Conduct

The focus groups were run in Lebanon in May and June 2016, and in Jordan in June and July 2016. Due to delays with obtaining permits for research in Zaʿatari camp, the focus groups there were run in September 2016. Pilot focus groups were

conducted in Lebanon in May 2016 to improve the public-goods vignettes, discussion guide, and framing of the study. Focus groups scheduled to be conducted in KRI, set to begin around October 20, 2016, but were cancelled because of security concerns surrounding the battle of Mosul, which began on October 16, 2016.

All aspects of the study were conducted in Levantine Arabic, including recruitment and data collection, and all documents read to or distributed to participants were in straightforward formal Arabic. I speak the dialect fluently and all recruiters and focus-group moderators were native speakers. I monitored all aspects of the study including recruitment, data collection, and focus group transcription. Two experienced female Lebanese focus group moderators conducted the focus groups.

During the focus groups, two recorders were used, one at each side of the group, to increase the transcribers' ability to hear all focus-group content.

1.8 Steps to Preserve Excludability

Research design and conduct were kept as similar as possible across experimental arms. Moderators were not told about the intention of the construction of close-ties and a random-ties groups, and were not told about the hypotheses under investigation. Although, in order to coordinate recruitment (using different strategies) and the logistics of getting the participants into the right room, moderators did know

that the groups were recruited using different strategies and had a different social network structure. Moderators were trained that they should do very little to guide discussion after the audio files were played. At most, if participants asked what they were supposed to do, the moderators were trained to say something minimal like, "What are you going to do?" or "Can you do anything in this situation?" but explain no more and never express expectations that people work together.

Random assignment of moderators to gender and experimental arms across focus groups was infeasible for multiple reasons, and was therefore manually balanced in designing the focus-group schedule. First, scheduling the focus groups across numerous areas in two countries for two moderators was already a formidable logistical effort. Second, out of respect for some participants' social conservatism, I constructed the schedule such that the staff did not transport both genders at the same time or conduct focus groups with both genders in the same building at the same time. To minimize the attrition of more socially conservative individuals from the sample, both moderators were female. In theory, one might be able to reduce attrition by having moderators of the same gender as focus-group participants, but this would introduce systematic imbalance between male and female focus groups, which is a more severe problem than attrition. Also, although male participants did not know in advance that their focus-group moderator would be a woman, no one

refused to participate upon arrival. Results are robust to controlling for moderator.

1.9 Content of the focus groups and audio vignettes

The paper examines outcomes drawn from the focus group discussion of four public goods vignettes, which lasted roughly 15 minutes in each focus group. The full focus groups lasted about two hours and covered location choice, living conditions, pre-migration ties, income generation, local law and order, and intra-household issues including intimate-partner violence.

The order of the audio vignettes was randomized to eliminate order effects.

The audio vignettes were intended to study how Syrians confront community problems, and the impact of social-network structure on the responses. Moderators played focus-group participants audio vignettes of two Syrian men discussing problems that Syrian communities commonly face. The vignettes discussed issues common in Syrian refugee communities including resource redistribution, public safety, access to labor and income, property rights, and the ability to run a shop. After playing each vignette the discussion was opened up for the participants. Moderators did little to shape participants' responses to the audio vignettes.

Although the community problems have possible collective solutions, the vignettes did not impose collective responses on participants. Indeed, we see varia-

tion in responses in the focus groups from disinterest to heated discussion, and from atomistic responses to communal responses.

One vignette describes a situation where an NGO delivers resources to the community without specific allocation criteria, and participants must distribute the resources. In the second vignette, a Syrian shopkeeper whose shop benefits local Syrians is being challenged and threatened by a local shopkeeper and participants are asked what they might do to respond. The third vignette presents a problem where checkpoints are preventing local Syrians from getting to work, leaving participants to discuss whether there is any response to increase access to work. In the fourth vignette, two young men get in a fight, which spills over into family conflict, and the participants are asked how they might respond. There was another level of experimental variation in this vignette, where half of focus groups were presented with a fight between two Syrian young men, and the other half of focus groups were presented with a fight between a Syrian young man and a young man from the host community. Beyond testing the general effect of social-network structure on problem solving in law-and-order scenarios, the second level of randomization allows us to study how responses vary between problems contained to the refugee community and those problems that spill into the host community.

in conjunction with NGO colleagues, some of whom are Syrian, who working with

Syrians.

Vignettes as must resonate with the participants to attain evidentiary validity, rather than being based on priori assumptions of the researchers. I designed the content of the audio vignettes after having spent more than one year conducting participant observation in Syrian communities in Beirut and the Bekaʿa valley. I also consulted Syrians, Lebanese, and Jordanian friends and colleagues in developing to audio vignettes.

The focus group transcripts demonstrate that the issues raised in the vignettes resonated in a vast majority of focus groups. I coded whether people made statements about the relevance and irrelevance of the vignettes during the focus-group discussions. In the majority of vignette discussions (79%) there was at least one explicit comment about the vignette's relevance (e.g., "This type of thing happens in our community"), and in only a minority of the vignette discussions (10%) did anyone say anything about its irrelevance (e.g., "This type of thing does *not* happen in our community").

1.10 Outcomes and Coding Guide

Outcomes in response to the vignettes are measured in a few different ways. In a survey after the focus group, participants reported whether they believed that they

would be able to work with the other people in the room to solve problems like those discussed in the vignettes. Second, I coded responses to the vignettes based on the transcripts. Coding was not automated or predictive; I read and hand-coded all focus group transcripts using the qualitative data analysis software Dedoose, which then output the results in a spreadsheet for statistical analysis.

I was blind to treatment status while developing the coding guide and during coding. Treatment status was only merged with the other covariates after coding was complete.

I developed the coding guide through a four-stage process with three other researchers, who generously shared their time to help develop the coding guide. First, a blocked sample of 14 focus group transcripts per researcher was drawn from the 56 focus groups, ensuring that each researcher read one focus group transcript per research site, read the same number of women's and men's transcripts, and read the same number of transcripts across experimental conditions. Based on this reading one report author wrote a first-draft of the coding guide, and the second author reviewed the coding guide assessing to improve the relevance of the themes and variables in the guide. Second, an outside researcher read a distinct blocked sample of 14 focus group transcripts, and then offered feedback to refine the themes and variables in the coding guide. Last, a second outside researcher reviewed the coding

guide without reading any transcripts and offered comments and suggestions on the salience and clarity of the variables.

Dialogue, example 1: Discussing fights, someone accused the host community of causing the fights

- No. Only some [Jordanians] are trouble-makers. Jordanian people are like any other people.
- I don't agree with him. We should worry from any Jordanian these days

Male focus group, Amman, July 2016

Dialogue, example 2

- I think that the NGO should check refugees' houses to select the neediest people.
- No. This won't change anything. I don't agree with you because when an NGO came to assess my need, they dismissed me from UNHCR assistance without even seeing my house.
- No. I would give aids to all refugees, because we all have the same situation.

Female focus group, Burj El-Barajneh, June 2016

Dialogue, example 3: Traveling through checkpoints is dangerous.

- I have to go myself to sort out my daughters' paperwork with the hospital [after her treatment], My son can't go due to his expired documents.
- Yes. I agree on what my friend said.
- We told your recruiter who visited us two days ago that we all don't have valid documents.
- Our children envied us when you came to take us from our homes to the Roucheh area. They don't go outside in Burj El-Barajneh area. They can only play in our dirty neighborhood that has the smell of sewage.
- Yes. I agree.
- Yes, for this reason, we inform each other about any activity regarding police and checkpoints.

Female focus group, Burj El-Barajneh, June 2016

Leaders

- A similar problem happened in our camp once. The leader (*shawish*) gathered parties and resolved their problem. Female FG, El Marj, Lebanon
- I am the leader (*shawish*) of the camp and that's why I will be able to do ease this problem (*żabaṫ al-hal*). Male FG, Khiyara, Lebanon

- The leader (*shawish*) will tell the organization about these families. Female FG, Khiyara, Lebanon

Brokers

- If you know someone who works with a specific aid organization, he helps you by registering your name and notifying you every time id comes Female FG, Chhim, Lebanon

- Those people who receive aids have connections and relations. ... if you know someone who works with a specific organization, he helps you by registering your name and notifying you every time aids come. Female FG, Chhim, Lebanon

- We talk to many people some accept to help and others don't Female FG, Bab Tebbeneh, Lebanon

Host

- When the owner of another nearby shop knew what happened, he made a work permit for the man and his son and he returned them to the shop. Female FG, Irbid, Jordan

- A Lebanese neighbor one filed a false police report that I paid money to terrorists but some other Lebanese I know in the neighborhood helped me to get over this problem. Male FG, Bab Tebbeneh, Lebanon

- If the Syrian wanted to open a business he should partner a Lebanese man. Male FG, Chhim, Lebanon

NGOs

- We should always go to the organization and ask [how to best distribute the aid] Female, FG, Shafa Badran, Jordan

- I think that NGOs should distribute aid themselves. My neighbor who has gold and a husband working in a foreign country is registered with UNHCR. Female FG, Zaʿatari, Jordan

- If an NGO asked us to do get aids we will put our phone numbers and let her choose the in need people. Female FG, Erhab, Jordan

National Government

- The solution is that Syrians complain to an authority that defends them in order to not letting this kind of problems increases. FG20 Lebanon

- You can resort to government and laws defend you rights, but the main problem is what will happen later. The tribal reaction. FG39 Jordan

- I have a Jordanian neighbor that complains about me everyday in different local offices since my supermarket is working better than his. But I don't have a license for my shop, but I know someone in the government who is helping me. FG36 Jordan

1.11 Considerations for a Two-country Study

I aimed to preserve as much similarity in research conduct across the two countries. Most importantly, I used the same two focus-group moderators in each country, which involved additional costs for moderator travel from Lebanon to Jordan.

Given differences in dialect or terminology between the two countries a few changes in the vignettes were necessary. These changes were necessary because some technical terms varying across the countries, and not because the Syrians in Lebanon and Jordan speak different dialects. All changes necessary for Jordan were inserted into the same audio files that were used in Lebanon. For example, any reference to a host community member was changed from "Lebanese" to "Jordanian," where the voice actors replaced just that one word in the existing audio files. Second, in Lebanon I used the term checkpoints (*hawajiz*) and in Jordan I used the term police patrols (*dawriat shourta*), which are the different terms used in the two countries to describe analogous phenomena.

The relevant legal residency document for Syrians in Lebanon is called a residency (*iqama*) and in Jordan it is a security card (*bataqa amnia*). In Lebanon is the *iqama* is sometimes also referred to as papers (*awraq*) and in Jordan alternatively as Services card (*bataqat khadimat*). Whereas Syrians in Lebanon lack legal residency because they never obtained the document or it expired, the residency document for Syrians in Jordan does not expire, but instead it was replaced by a new document that Syrians needed to go obtain. Therefore, whereas Syrians in Lebanon say their documents are expired, Syrians in Jordan do not say their "security cards are expired" (*bataqat amnia khalaseen*). Instead I modified the terminology to "I do not have a new security card" (*ma m3na bataqat amnia jadideh*). Also, whereas in Lebanon Syrians often speak about arrests, in Jordan people do not refer to police arrests so much as they refer to police detention. Therefore, moving from Lebanon to Jordan I changed the wording of the vignette about arrests from "he was arrested at a checkpoint," to "he was detained at a police search" (*jarna muhammad waqaffuu 3 dawriat shortah*).

These dialectical modifications ensure that the vignettes resonate with participants and therefore prompt substantive discussion of the public goods problems.

2 Quality Checks

I instituted a number of data quality checks. I was in Lebanon and Jordan during the recruitment for and conduct of focus groups to confirm the quality of data collection. I also validated data entry by reviewing samples of post-focus groups questionnaires to ensure for consistent answers. On data-collection days and in the follow-up to data collection a member of the research team further validated essential features of the recruitment strategy, and confirmed that they met inclusion criteria. I asked participants about the recruitment process, how they had been contacted, whether they had been asked if they would be able to refer to three people outside their nuclear family and household to participate.

I validated transcription quality by reading a sample of English-language transcripts while listening to the corresponding Arabic-language audio recordings. Last, a number of questions were included in the post-focus group questionnaire to test the successful randomization of experimental conditions. All data quality checks suggest that the design was successfully implemented.

3 Full Text of Vignettes

Below are English translations of the vignette scripts as they were played in Lebanon. The changes made for play in Jordan are discussed in section 1.11. In each audio vignette there were two Syrian male voice actors, who I simply label 1 and 2 below.

3.1 Checkpoints

1 – Hello Abu Mustafa, did you hear that the security forces are set up a checkpoint at the entrance to the area/neighborhood?

2 – It's big problem. Most of the Syrians here don't have up-to-date residency. And we can't pass out of the area, not to work, or for any other reason. I don't have my papers, so I cannot go to work today.

1 – Yeah, me too. There are many people who won't be able to go to work today, since they cannot go out because of the checkpoint

2 – Did you hear that our neighbor Mohammad was detained at a checkpoint a few days ago?

1 – Yeah, I know. It is not a good situation, they arrest us at the checkpoints. We cannot move. It's not a life.

2 – I know we cannot solve this problem, but isn't there some way to reduce the pressure for all these people?

1 – Yeah, how can we get to work today?

3.2 Fight, Law and Order

1 – Did you hear about the trouble that happened yesterday? Two young guys got into a fight. Two Syrians, one from our neighborhood and the other from the neighboring neighborhood. It's a serious problem. I'm afraid it'll get worse if we don't do anything. It's not just a problem for the young guys anymore, it's become a family problem.

2 – I don't know what we can do. There have been a lot of problems like this lately. The last fight was really serious. One of them was injured, hit by a rock, and the two were really hitting each other. The families were seeing red. A young guy from our neighborhood had to go to the hospital and needed to pay a lot of money. He might have broken a bone.

1 – I don't know who we can go to about this problem.

2 – Yeah, I know we cannot fix it, but I think we can do something to reduce the tension.

3.3 Aid Distribution

1 – How are you, Mohammad? Did you hear about the new NGO working in the area? They want the local residents to select the neediest families in the area to get some help

2 – Who do you mean, they want us to select?

1 – Us, the Syrians living here, they want us to choose which families will get aid and which will not.

2 – How are we supposed to do that? Everyone that you will ask will say that they need help more than the others.

1 – Yes, I know that we all need help, but we must know who really needs more help right now. We must find a way to deal with people who won't receive aid. How are going to work with this situation?

3.4 Local Syrian-run Shop

1 – Hello, Abu Ahmed, did you congratulate Moussa on his new mini-market? Good luck to him [may God help him]. His prices are good, his products are good, and he brings his bread to middle of the neighborhood so we don't have to go out when we don't have legal papers.

2 – Yes, I congratulated him but he didn't look happy.

1 – Why?

2 – Because Mohammad, the Lebanese owner of the super market, is not happy. He threatened Mousa, and said that he would force him to close his mini-market.

1 – And what he can do to Mousa?

2 – Mohammad threatened Mousa about his residency, since Mohammad has connections that could really give Mousa trouble.

1 – God protect him. What's some way to resolve this situation?

2 – We ought to do something together to mitigate the problem.

4 Transcription

Focus-group transcripts were transcribed and translated by 8 different Lebanese transcriptionists. I conducted quality checks every week during transcription by listening to the Arabic audio files while reading recent English-language transcripts. During these quality checks I deemed one transcriptionist's work to be inadequate, and she was fired, replaced, and all her transcripts were redone.

I could not randomly assign transcriptionists to transcripts given the dynamic nature of which transcripts needed to be transcribed and which transcriptionists were working. Instead I gave the transcription manager three covariates — focus group gender, treatment status, and moderator — and essentially had them use blocked non-random assignment of transcriptionists to focus groups. The transcription team did not retain data about which transcriptionist handled which documents, which means I cannot test whether results are robust to controlling for transcriptionist.

5 Differences Between Lebanon and Jordan

To test the effects of variation in the availability of outside resources, I leverage the fact that levels of resource availability is higher in Jordan than Lebanon and constraints on access are lower. I present data on aid availability and arrests in Figure 2. Although the phenomena resource availability and constraints for refugees are certainly broader than aid or arrests alone, these two metrics capture important dimensions of the broader concepts in Lebanon and Jordan. Aid constitutes a large share of the material resources that refugees can leverage to mitigate their problems, and my qualitative evidence suggests that network connections play an important role in helping people understand what aid resources exist and how to access them.

The threat of arrest is a dominant force in refugees' lives and an important con-

straint on refugees' ability to take actions that might be used to mobilize resources and mitigate their problems. The threat of arrest constrains refugees' ability to move freely, get to work, form and maintain social relations, and visit aid agencies and government offices. Even in Jordan where only 5.6% of Syrians in the study sites report ever having been arrested in the host country, the threat of arrest was a common theme of discussion in the focus groups. Focus group participants emphasized that their legal vulnerability and lack of meaningful legal protections mean that arrest comes with the risk of abuse and even forced return to Syria.

The results presented in Figure 2 are drawn from my survey data of focus group participants, subsetted to the control group, which comprises a random sample of registered Syrians in the study sites. We see that many more Syrians in the Jordan study sites received humanitarian aid in the last month than in Lebanon (Jordan: 62.5%. Lebanon: 32.5%). Second, we see that across a range of metrics of arrest, more Syrians living in the Lebanon study sites have been arrested. The first two questions about arrest asked whether the respondent or a member of their family had ever been arrested in Lebanon/Jordan. The third and fourth metrics present whether respondents reported that any Syrians in their community had been arrested in the last month, and whether more than five Syrians in their community had been arrested in the last month. I present these comparisons as evidence supporting

my broader claim that resource access is higher in Jordan than in Lebanon, and constraints on mobilizing those resources are lower in Jordan than in Lebanon.

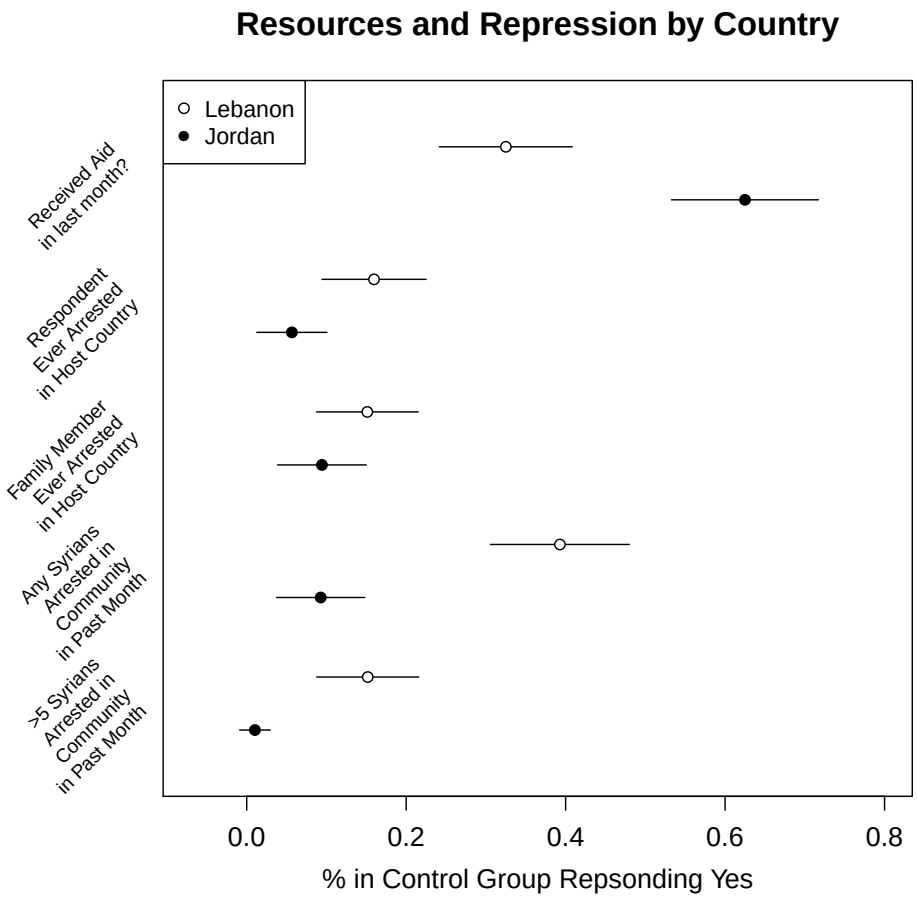

Figure 2: Variation in Country Descriptives ($n = 56$)

6 Alternative Theories That Do Not Rely on Information Flow

6.1 Psychological Mechanisms

People in high-density groups may cooperate more because they care about about each other's welfare, prefer working with each other, or share a sense of normative obligation. Putnam (2000) emphasizes that frequent interaction, even among diverse individuals, tends to produce norms of generalized reciprocity. Alesina and La Ferrara (2005) propose that people may be more altruistic towards 'in-group' members because they internalize the benefits to these people more than benefits to people outside their group.

People interacting with a group of close acquaintances may be more likely attend to the needs of others, thereby promoting cooperative behavior. If people have more altruistic or sociotropic preferences with their network neighborhood, participants in high-density groups will expresses concern or recognition of the issue as one that affects the well-being of other people in the community, not just themselves and their families, which leads to the fifth hypothesis:

H5: High-density groups will be more likely to expresses concern or recognition of problems as affecting the well-being of the commu-

nity or other people in the community.

Participants in high-density groups, sitting with people whom they know and relate with, may more easily think of problems and responses as collective issues, or they may more readily feel the trust that is necessary for people to contribute to public goods (Kahan, 2003). Similar to Tversky and Kahneman (1973)'s Availability Heuristic, high-density groups may be more likely to see the possibility or value of cooperation to mitigate community problems. These considerations give rise to a related hypotheses:

H6: High-density groups will be more likely to discuss collective action in response to the vignettes

6.2 Network Location

Even among similarly dense networks, the network location of key actors can have critical effects on outcomes. Recent work in political science argues that different node location will produce different outcomes even holding density constant, and that peripheral network locations may be more important for initiating and mobilizing high-risk collective action than central members (Larson and Lewis, 2016; Steinert-Threlkeld, 2017; Hassanpour, 2017; Larson, 2017). Peripheral actors may be more willing to adopt risky behaviors (Hassanpour, 2017) and peripheral network positions

may be unobservable in ways that create incentives for malfeasance (Larson, 2017). Such considerations motivate my final hypothesis:

H8: High-density groups will be more central in real-world networks than low-density groups.

7 Testing Theories That Do Not Rely on Information Flow

7.1 Normative Obligations and Preferences

Do people care more about members of their in-group in a way that facilitates cooperation? High-density groups may be more likely to cooperate because participants prefer cooperating with a tightly knit group of acquaintances. Just by being with people they know and trust, participants in high-density groups may talk more about collective action, and recognize collective needs of the community. High-density groups may be more likely to see the issues as collective-action problems because participants are surrounded by people whom they know.

To test Hypotheses 5 and 6, whether psychological mechanisms are at play in the observed differences between the high- and low-density groups, I proxy for psychological mechanisms using two metrics. First, if people have more sociotropic preferences

in high-density groups, participants in high-density groups will expresses concern or recognition of the issue as one that affects the well-being of other people in the community, not just themselves. Therefore, I coded whether a speaker expressed concern or recognition of the problem as one that affects the well-being people other than their own and that of their family, either self-inclusive (e.g., "this affects us") or self-exclusive (e.g., "this affects many other people in the community, although not me").

In a second coding, I applied a higher standard of sociotropic preferences. I coded whether statements discussed the impact of the problem in the vignette on people other than the speaker or their family members. Last, I coded whether comments explicitly acknowledged that people should work together to mitigate the community problem.

As Table 1 shows, I find no detectable treatment effect on whether groups discussed the need for collective action, and no detectable effect on whether groups viewed the problems from a more collective perspective. The lack of evidence of psychological mechanisms aligns with existing studies, most notably Habyarimana et al. (2009), who find that people do not exhibit greater concern for their in-group peers' welfare or prefer working with in-group members.

Table 1: Psychological Mechanisms

	Affects Collective	Affects Others	Mention Collective Action
Control mean	0.17	0.07	0.34
$\hat{\beta}$	0.02	-0.03	-0.07
	(0.05)	(0.03)	(0.06)
RI p-value	0.86	0.41	0.19

Notes: $n = 223$. $\hat{\beta}$ denotes difference-in-means estimate. HC2 robust standard errors, clustered at the focus-group level, are reported in parentheses. Asymptotic randomization inference performed with 10,000 simulated randomized treatment assignment vectors, clustered at the focus-group level and blocked by country, site, and gender. Results are generally robust to including covariates for country, country-treatment, and moderator.

7.2 Network Location

In addition to the experimental variation in network density, the high-density groups may include participants who have different network locations in their real-world community. Recent work in political science argues that node location affects cooperation even when holding density constant, and that peripheral network locations may be more important for initiating and mobilizing high-risk collective action than central members (Larson and Lewis, 2016; Steinert-Threlkeld, 2017; Hassanpour, 2017; Larson, 2017). Peripheral actors may be more willing to adopt risky behaviors (Hassanpour, 2017) and peripheral network positions may be unobservable in ways

that create incentives for malfeasance (Larson, 2017).

Hypothesis 8 asks whether high-density groups are more central in real-world networks than low-density groups. As we saw in the manipulation check, the density of pre-existing ties within the high-density group is 2.9 control-group standard deviations higher than the low-density group's density. However, the evidence does *not* indicate that the treatment groups are more central in the broader community. Instead, we need to look at the relationships of people outside the focus group. Measuring a complete social network of any reasonable size is extremely difficult and expensive. To proxy for a metric of network centrality, I study how high-density groups' network neighborhoods differ from the low-density groups. Below, I test treatment on two metrics of network centrality. First, I study how many times in the last week respondents visit someone or had someone visited them. Second, I study how many families in the respondent's current community, they knew in Syria before migrating.

Beyond the simple fact that people in the high-density groups are more connected to each other, Table 2 shows that they are also more connected to their community, which aligns with intuition from Feld (1991). The people who get brought are recruited by referral high-density groups do not only know each other more, they also know more people in their community. In graph-theoretic terminology, sampling

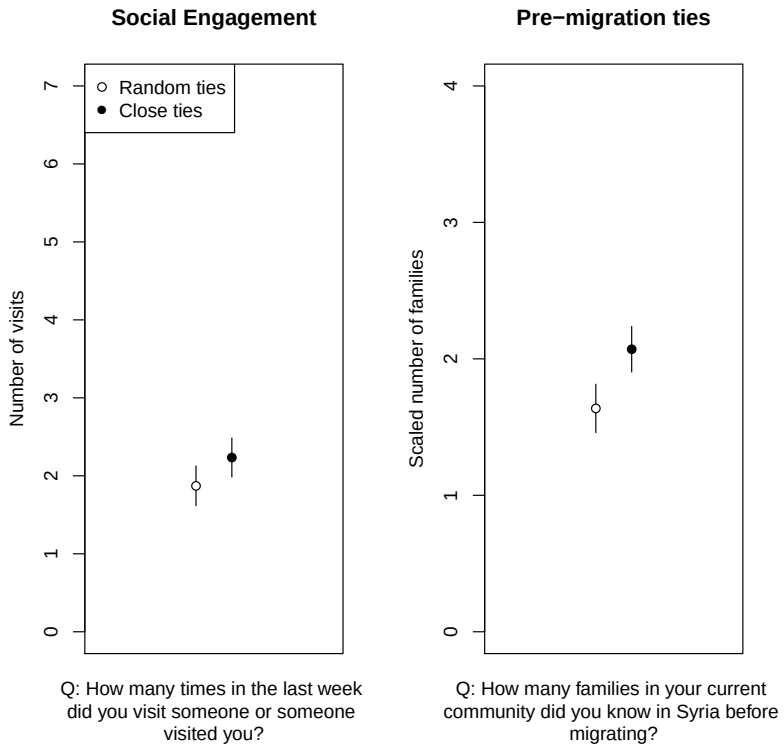

Figure 3: Community-Network Centrality ($n = 56$)

along edges brings in more connected individuals than recruiting nodes.

Even though the high-density groups know more people in the community, we saw in the main paper that they still talk less about external resources. Given that the high-density group is more connected with the community at-large, we might expect them to have access to a wider range of information and resources. If we thought that group members with higher network centrality in the group were more

Table 2: Results: Community Social Ties

	Social Engagement	Premigration Ties
Control mean	1.87	1.64
$\hat{\beta}$	0.36	0.43
	(0.28)	(0.19)
RI p-value	0.03	<0.01

Notes: $n = 56$. $\hat{\beta}$ denotes difference-in-means estimate. HC2 robust standard errors, clustered at the focus-group level, are reported in parentheses. Asymptotic randomization inference performed with 10,000 simulated randomized treatment assignment vectors, clustered at the focus-group level and blocked by country, site, and gender. Results are generally robust to including covariates for country, country-treatment, and moderator.

likely to have nonredundant resource access then we would predict that the effect of network centrality on the diversity of resources would be positive. The results in the main paper about deliberation and access to diverse resources, however, move in the opposite direction, with the treatment group turning to outside resources less in discussing their community problems. Although treatment does have an effect on network centrality, I do not interpret this as evidence that the treatment effect is driven by participants' locations in the broader community instead of the flow or information within between recruited participants.

8 Randomization Inference Procedures

I blocked treatment assignment on site and gender, with two focus groups in each block, and randomly assigned one focus group to control and one to treatment in each site-gender block. Therefore, I conduct randomization inference by simulating 10,000 treatment assignment vectors under the blocking scheme. For each vector of simulated treatment labels I calculate a test statistic according to the simulated treatment assignment vector. I use this approach to calculate RI p values for the treatment effect coefficient estimate in a difference in means calculation and in the heterogeneous treatment effects regression, and for the F-statistic from the Brown-Forsythe Tests for Equality of Variances.

9 Results are Robust to Covariate Adjustment

I test for the robustness of experimental results by estimating the treatment effect with covariate adjustment, across multiple covariate sets. The results shown in Figures 4 and 5, with point estimates and confidence intervals moving only slightly across specifications provides supportive evidence of the robustness of the experimental results. I calculate the results in Figures 4 and 5 using OLS regression with robust standard errors, clustered at the focus-group level. I present treatment ef-

fect point estimates and 95% cluster-robust confidence intervals for all the outcomes presented in the main paper. I present the coefficients estimates from regressions including covariates including an indicator variable for one of the two moderators, how many comments the focus group moderator made, how many suggestions the focus group moderator made about a response to the community problem, and in indicator variable for whether a focus group took place in Jordan. I also present results adjusting for covariates using Lin (2013)'s covariate adjustment method of including all covariates and the interaction of treatment with the demeaned covariates.

Both Figure 4 and Figure 5 show no meaningful variation in point estimates, confidence intervals, or statistical significance across multiple covariate sets. In the paper I proxy for information about and access to external resources with whether anyone in a focus group stated that Syrians could turn to a particular resource in response to a community problem. Figure 4 shows that treatment effect estimates and uncertainty vary only slightly across control sets. Just as with the basic difference in means estimates, the treatment effects on discussion about relying leaders, brokers, and sulha are robustly negative and significant at levels less than 5%. The treatment effects on discussion about relying on the host community and NGOs are robustly negative and significant at the 10% level. The treatment effects on discussion about relying on the national government or police are are robustly negative and positive,

respectively, and neither is significantly different from zero.. Similarly, the results presented in the paper for the treatment effect on statements about *not* turning to specific actors are robust across covariate sets.

In the paper I proxy for collaborative responses to public goods problems with the amount of dialogue between group members about their response. Figure 5 shows no meaningful variation in the treatment effect estimate and uncertainty across control sets.

References

Alesina, Alberto and Eliana La Ferrara. 2005. "Ethnic Diversity and Economic Performance." *Journal of Economic Literature* 43(3):762– 800.

Feld, Scott L. 1991. "Why Your Friends Have More Friends Than You Do." *American Journal of Sociology* 96(6):1464.

Habyarimana, James, Macartan Humphreys, Daniel N Posner and Jeremy M Weinstein. 2009. *Coethnicity: diversity and the dilemmas of collective action*. Russell Sage Foundation.

Hassanpour, Navid. 2017. *Leading from the Periphery and Network Collective Action*. Cambridge University Press.

Heckathorn, Douglas D. 2011. "Snowball versus respondent-driven sampling.".

Kahan, Dan M. 2003. "The Logic of Reciprocity: Trust, Collective Action, and Law." *Michigan Law Review* 102(71):71–103.

Larson, Jennifer M. 2017. "How the West Became Wild: Informal Governance with Incomplete Networks." *World Politics* .

Larson, Jennifer M and Janet I Lewis. 2016. "Ethnic networks." *American Journal of Political Science* .

Lin, Winston. 2013. "Agnostic notes on regression adjustments to experimental data: Reexamining Freedman's critique." *Annals of Applied Statistics* 7(1):295–318.

Magnani, Robert, Keith Sabin, Tobi Saidel and Douglas Heckathorn. 2005. "Review of Sampling Hard-to-Reach and Hidden Populations for HIV Surveillance." *Aids* 19(Suppl 2):S67–S72.

Miratrix, Luke W., Jasjeet S. Sekhon and Bin Yu. 2013. "Adjusting treatment effect estimates by post-stratification in randomized experiments." *Journal of the Royal Statistical Society. Series B: Statistical Methodology* 75(2):369–396.

Olson, Mancur. 1965. *The logic of collective action.* Harvard University Press.

Putnam, Robert D. 2000. *Bowling alone: The collapse and revival of American community.* Simon and Schuster.

Steinert-Threlkeld, Zachary C. 2017. "Spontaneous Collective Action: Peripheral Mobilization During the Arab Spring." *American Political Science Review* 111(02):379–403.

Tversky, Amos and Daniel Kahneman. 1973. "Availability: A heuristic for judging frequency and probability." *Cognitive Psychology* 5(2):207–232.

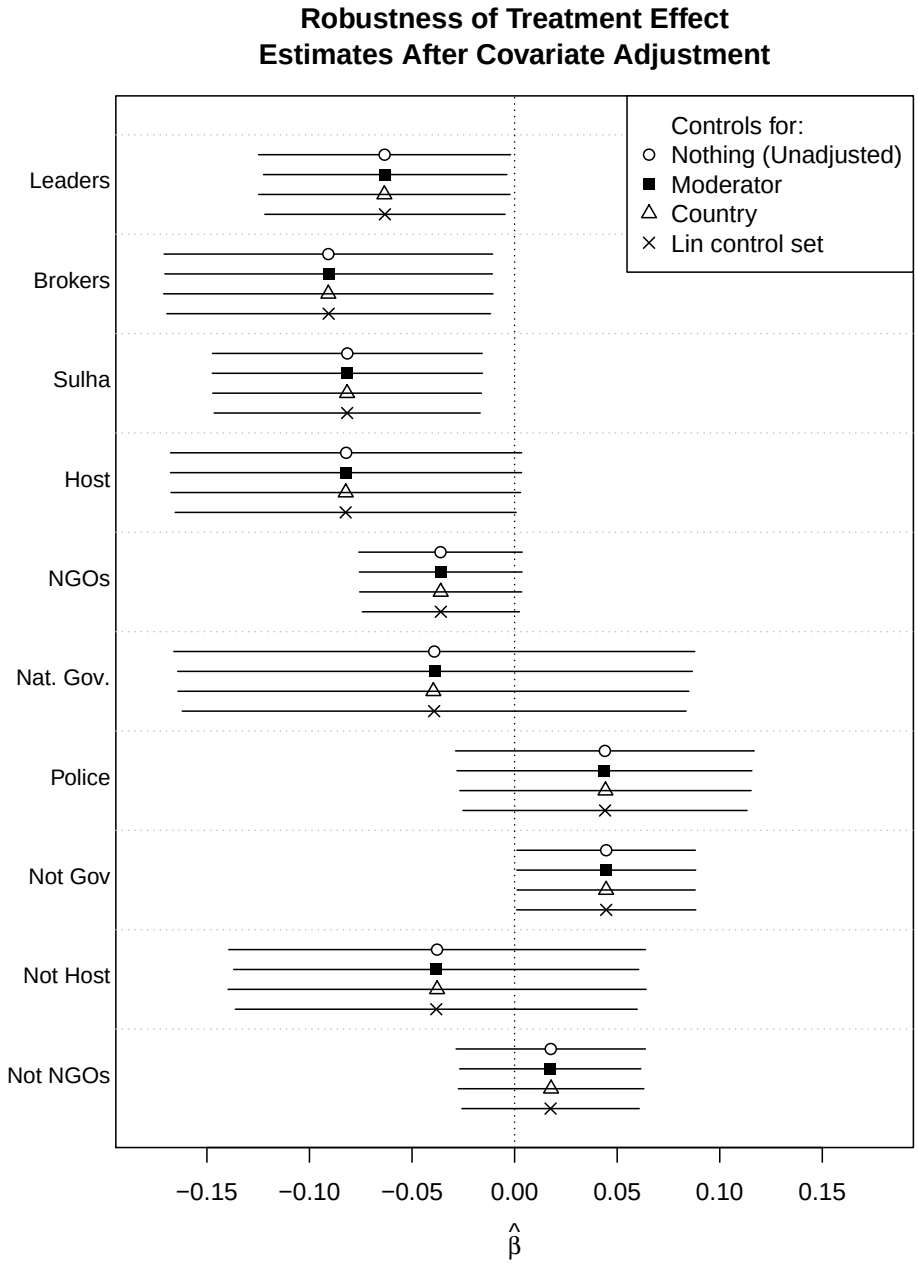

Figure 4: Robustness of External Resources Results Across Control Sets

Notes: $n = 223$. Confidence intervals are calculated with HC2 robust standard errors, clustered at the focus-group level.

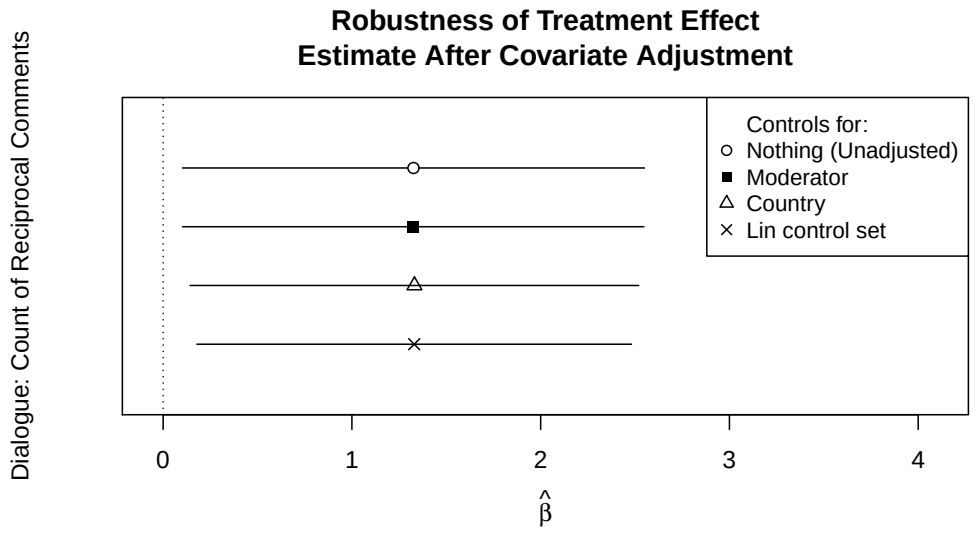

Figure 5: Robustness of Dialogue Results Across Control Sets

Notes: $n = 223$. Confidence intervals are calculated with HC2 robust standard errors, clustered at the focus-group level.