

Appendix

Cooperation, Resources, and Network Structure

Theory and Evidence from the Syrian Refugee Crisis *

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1 Case Studies

To flesh out how networks facilitate responses to collective problems, I discuss comparative case studies of two Syrian camps (aka, informal settlements, IS's) in Lebanon's Beka'a valley, and how each responded to the 2015 trash collection crisis. I focus on the density and diversity of social ties in the camps, the nature of the camps' strategies in response to the crisis, and their effectiveness in finding ways to get trash collected.

1.1 Empirics

I conducted fifteen months of participant observation in Syrian communities in Lebanon from summer 2015 to autumn 2016, which followed fifteen months of exploratory research in Lebanon between fall 2013 and spring 2015. In the part of the project that I present in this essay, I use comparative case studies of two camps to study their response strategies and outcomes. The first case study, a camp that I call Helween, served as one of my two primary field sites during fieldwork. I spent 1-4 days per week in Helween over the course of one year. The second case study is a camp I call Sharqi. This site was not one of my primary field sites, and I spent about one day each month in Sharqi over the course of one year. I draw on multiple data sources for the comparative case studies, including my field notes; open-ended

interviews that I conducted with camp residents, camp leaders, Syrian NGO workers, and international NGO employees; and structured interviews that research assistants conducted with Syrian and Lebanese community leaders in the area of the camps.

In my fieldwork I sought to map the concepts of ‘public goods problems’ and ‘cooperation’ onto the grounded experience of Syrian communities. I positioned myself to observe the central collective problems that Syrians face and the patterns of responses. Syrians’ connections and access to resources regularly emerged as an essential feature of response strategies. In this sense, my fieldwork focused on observing and mapping processes and connections. It became clear that communities were continually attempting different processes for mitigating problems, and that people in the communities are acutely aware of the importance of their social relationships in trying to mitigate their problems. Connections matter because every relationship holds the possibility of useful information or assistance.

The comparative case studies of the garbage crisis in camps are useful for examining and validating mechanisms for several reasons. First, the comparative case studies allow me to relax the simplicity of the focus-group experiment, and highlight the complex ways that community structure shaped outcomes.

Second, case studies of the sudden nationwide public services crisis allows us to ‘hold constant’ the problem and focus on variation in the characteristics of the

cases. For most collective problems that Syrian communities face, the challenges themselves will be shaped by features of the community, rather than a national-level policy crisis, which means that not only the features of the communities vary but also the problems that they face.

Third, comparative case studies of refugee camps allows us a degree of control for alternative explanations and for a conceptual clarity in defining a group and the ties between members, given a camp's delimited geography. My broader theory and project pertain to urban, peri-urban, and camp settings, although the comparative case studies focus on camps alone. That being said, focusing on any single setting poses a constraint on evidentiary validity given the diversity of Syrians' experiences in Lebanon, and indeed the diversity of refugees' experiences worldwide. Only 15% of Syrians in Lebanon live in camps, and only about one third of refugees worldwide live in camps.

1.2 The Trash Crisis

Summer 2015 proved the perfect storm for Lebanon's solid waste management system. After years of strained makeshift solutions to trash collection, the region of Beirut and Mount Lebanon found itself with neither trash collection nor a place to deposit it. After years of protests by residents of the town of Na'ameh, the govern-

ment officially ended use of the town's landfill in July 2015. The Na'ameh landfill, which had received approximately half of the country's trash the previous year, closed without a replacement. The same month, in the midst of political gridlock, trash collection in Beirut and Mount Lebanon ended as the contract with the company Sukleen expired. Although these two problems were formally limited to Beirut and Mt. Lebanon, the failure of trash collection in the country's most populous region strained waste management nationwide.

As pressure on solid-waste management systems increased, Syrians struggled to find ways to avoid public health crises in their communities. Syrians in Lebanon generally need to pay for trash collection by household or by camp. When they cannot pay, Syrians need to find makeshift responses to manage solid waste. In 2015, many Syrian communities sought assistance from NGOs and municipalities. Without an external solution, some Syrian communities disposed of trash in unoccupied land or consolidated trash in places far from residences or drinking water supply. At worst, Syrian communities burned their trash or left it in streets, fields, and rivers.

1.3 Two Camps

Helween and Sharqi are located in the central Beka'a valley, about a 10-minute drive from each other. In 2015 the Helween camp comprised about 100 families. The

residents of Helween come from cities across western Syria, including Homs, Hama, Damascus, and smaller cities. Only small groups of Helween's residents knew each other before leaving Syria.

Some of Helween's residents are middle-class Syrians who were hit hard financially by the war and forced to live in a camp. Other residents worked in Syria's factories, construction industry, and ironwork. Some of the residents are longtime working class urban residents, and others are more recent migrants from the countryside, within the last generation or two.¹

After the trash crisis began more than a dozen Helween residents went to NGOs and the municipality to speak with people they knew in the organizations. Most of the connections led nowhere.² Other residents went to the municipality to speak with their social connections there. Many NGOs told them that were not financing trash collection and the municipal officials repeatedly said that the trash "was not the municipality's problem."³

Despite most connections proving unhelpful, one connection to a large INGO proved useful. One man from the camp had previously worked part-time for this particular NGO and still knew employees at the organization.⁴ More generally, a

¹Fieldnotes, Fall 2015

²Fieldnotes, Spring 2016

³Interview with Hassan, Syrian NGO worker, November 2017

⁴Interview with Hassan, November 2017

number of camp residents regularly interacted with the organization and repeatedly contacted the NGO to tell them about the conditions in their camp.⁵ Trash was piling up and a wave of rashes was affecting the camp's children, likely because of the garbage.⁶

After the Helween resident contact the INGO, it began to pay the municipality to collect trash in Helween and the surrounding camps. Although we cannot attribute the INGO response solely to this connection, camp residents believe that the INGO had not been aware of the severity of the problem, and the contact directly preceded the INGO's decision to pay the municipality to collect trash in the camps.

The second camp, Sharqi, comprised about 40 families in summer 2015. About half of the camp's residents come from the Damascus neighborhood of Saideh Zaineb and the other residents largely come from the periphery of Homs. Within each of these two groups, most people were family and friends before they arrived.⁷

Sharqi's residents knew fewer people in the municipality to contact and told me that they only knew one Syrian NGO, and no one at INGOs. The Syrian NGO contacted INGOs on Sharqi's behalf, but this indirect contact proved less effective than Helween's direct contact.⁸ Although the municipality occasionally came to

⁵Fieldnotes, Spring 2016

⁶Fieldnotes, Spring 2016

⁷Fieldnotes, Summer 2016

⁸Fieldnotes, Summer 2016

Sharqi to pick up trash after being paid by an INGO, these pickups were positive externalities from the efforts of other camps, and not the result of actions taken by Sharqi residents. Sharqi enjoyed much less frequent trash collection than Helween.⁹

The Helween camp was able to leverage its relationship with outside actors to get trash collected. Despite the fact that most of Helween's residents did not know each other a few years ago they asked searched across a diverse range of connections to external actors to find the resources they needed. Helween's residents had a range of diverse connections and were therefore more likely to have nonredundant information about and access to resources useful for mitigating their public goods problems. These diverse ties linked Helween's residents to people outside the camp, linkages that included relationships with NGOs, the UN, and local officials and elites. In contrast, Sharqi camp failed to get trash collected regularly. Despite the fact that Sharqi's residents knew each other, trusted each other, and wanted to help each other, they shared a narrow set of similar connections, and they did not have the right connections to get trash collected. In spite of Sharqi camp's dense networks of trust and strong reciprocity, the community's large proportion of overlapping network connections constrained the flow of diverse external information that could have given the residents access to relationships, public services, or NGO resources to

⁹Fieldnotes, Spring 2016

help resolve their community problems.

Despite the power of network diversity for drawing in external resources, dense networks still demonstrate their power to disincentivize free-riding. Despite Sharqi's inability to leverage relationships to get trash collected regularly, the camp was able to execute coordinated responses that kept trash off their streets. People did not burn their trash in the streets and did not generally throw trash in the streets. The residents of Sharqi disposed of trash outside of the camp in the nearby Litani river and agricultural fields and coordinated to consolidate trash just outside the entrance to the camp. Trash did not pile up in the streets, but instead piled up just outside the camp entrance and in the field and river next to the camp.¹⁰ Although the negative externalities of Sharqi's responses are obvious, the response demonstrates the capacity for coordination but an inability to access outside resources, which are both features we would expect given the camp's dense social network. In contrast, some of Helween's residents burned their own trash or tossed it in the street.¹¹ Some residents cooperated to consolidate trash by the entrance of the camp, but many residents did not participate.¹² Despite the failure to prevent free-riding inside the camp, outside actors more frequently picked up trash in Helween than in Sharqi,

¹⁰Fieldnotes, Spring 2016

¹¹Interview with Hassan, Helween resident, November 2017

¹²Interview with Muhammad, Helween resident, Spring 2016

in response to Helween residents leveraging their connections to outside actors and INGOs.

Another lesson from the case studies is that local public goods solutions will often leave underlying problems and downstream problems unresolved or even exacerbate broader problems. The fact that one camp found makeshift solutions for internal solid waste management does not resolve broader infrastructural problems. Even as individual Syrian communities and camps find methods for getting trash collected, Lebanon still suffers from its poor nationwide solid waste management. Helween is relatively better off than Sharqi, but public health problems, soil and groundwater pollution and chemical contamination, remain for both camps and Lebanon more broadly. Furthermore, although residents of Sharqi effectively avoided disposing of trash in their own streets, their actions have negative externalities for residents of the surrounding areas.

2 Research Design

Figure 1 presents a schematic of the research design.

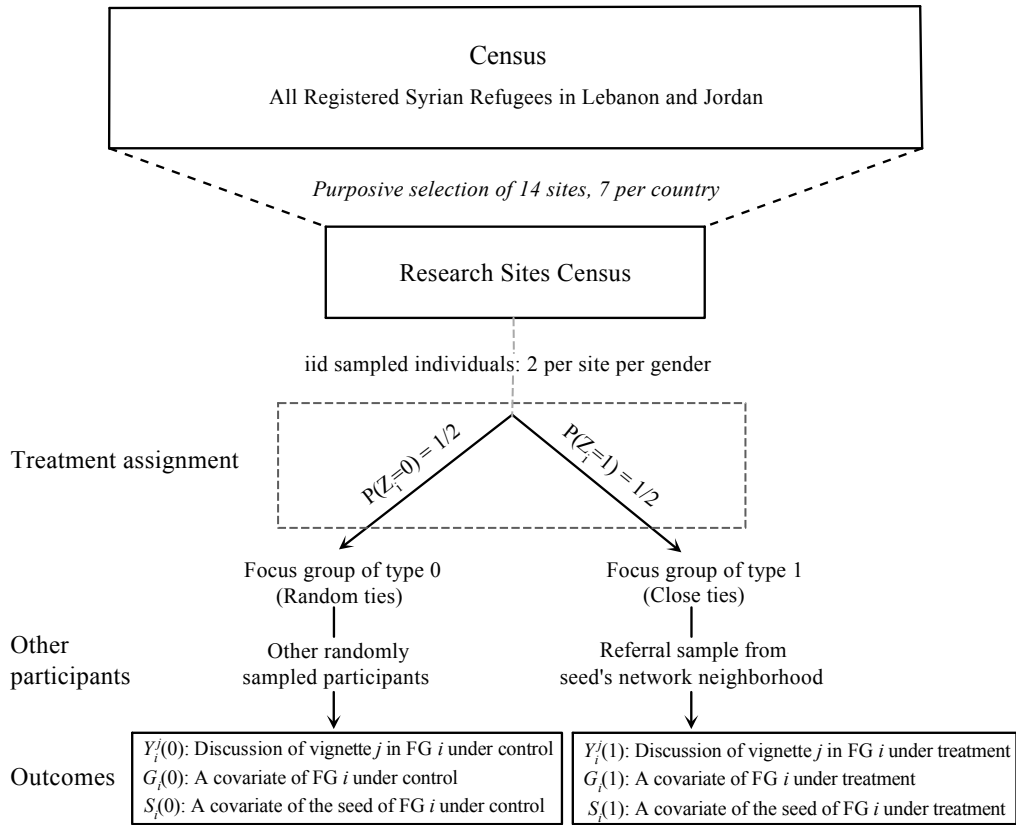


Figure 1: Research Design

2.1 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 quantitative 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.

2.2 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.

2.3 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.

2.4 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).

2.5 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.

2.6 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.

2.7 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 Za‘atari 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.

2.8 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 fo-

cus 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.

2.9 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 variation 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 Beka'a 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”).

2.10 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 (*zabat 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, Za'atari, 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

2.11 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 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.

3 Complete Coding Guide

Include complete coding guide.

4 Empirical Strategy

There are n focus groups, indexed $i = 1, 2, \dots, n$, and 4 vignettes in each focus group, indexed $j = 1, 2, 3, 4$. I define the focus group outcome as Y_i^j , for discussion

of vignette j in focus group i . I consider a binary treatment, denoted $Z_i = 1$ if focus group i is assigned to treatment. I define a covariate of focus group i as G_i , the population mean for covariate G as μ_G , and a covariate of the seed of group i as S_i .

I assume SUTVA at the level of the focus group, such that:

$$Y_i^j = Y_i^j(Z_i) \tag{1}$$

$$G_i = G_i(Z_i) \tag{2}$$

Equations 1 and 2 state that the observed values of Y_i^j and G_i are only a function of the treatment assigned to focus group i .

Random assignment implies the following independence relationships:

$$Y_i^j(z) \perp\!\!\!\perp Z_i, \text{ for } z \in \{0, 1\} \tag{3}$$

$$G_i(z) \perp\!\!\!\perp Z_i, \text{ for } z \in \{0, 1\} \tag{4}$$

Equations 3 and 4 imply Equations 5 and 6, below. I will use the plug-in principle to estimate the inferential targets, the left-hand side of Equations 5 and 6, with sample analogues of the right-hand side of Equations 5 and 6.

$$E[Y_i^j(z)] = E[Y_i^j | Z_i = z], \text{ for } z \in \{0, 1\} \tag{5}$$

$$E[G_i(z)] = E[G_i | Z_i = z], \text{ for } z \in \{0, 1\} \tag{6}$$

Although potential outcomes are independent of treatment assignment, referral recruited participants have different probabilities of recruitment than randomly

sampled participants. In traditional experiments, the research design implies both Equation 6 and the observable similarity of the pre-treatment covariate G across treatment arms, that is, $E[G_i|Z_i = 1] = E[G_i|Z_i = 0]$. My research design implies Equation 6 but does not imply that the observed covariate looks similar across treatment arms. That is, in this research design $E[G_i|Z_i = 1]$ does not necessarily equal $E[G_i|Z_i = 0]$.

S_i is a fixed covariate for group i and not a function of treatment, which implies that $E[S_i|Z_i = 1] = E[S_i|Z_i = 0]$. Furthermore, the characteristics of all randomly sampled units are equivalent in expectation. Therefore, when G and S denote the same covariate, $E[G_i(0)] = E[S_i|Z_i = 1] = E[S_i|Z_i = 0] = E[(G_i(0), (S_i|Z_i = 1))]$. In the special case where G_i is the group mean of a covariate, which I denote \bar{G}_i , the expectation for the control group will be the population mean for the covariate. That is,

$$E[\bar{G}_i|Z_i = 0] = \mu_G \tag{7}$$

5 Randomization Check

I check randomization by testing for the observable similarity of randomly sampled participants across treatment arms, which includes all control-group participants and treatment-group seeds, and excludes treatment-group referral recruits. I use the

plug-in principle to test Equation 8 with my sample.

$$E[G_i(0)] = E[S_i|Z_i = 1] \quad (8)$$

As we would expect under random assignment, data from a participant questionnaire shows that measured pre-treatment covariates of randomly sampled units are balanced across the two experimental conditions. I run a test of joint balance (aka, joint orthogonality), testing the joint hypothesis: $\beta_1 = \beta_2 = \dots = \beta_k = 0$, by running an F-test on a linear regression of treatment assignment on measured covariates, subsetting to randomly sampled observations, which includes the referral seeds and excludes referral-sampled observations. The test of joint orthogonality fails to reject the null hypothesis of equality (randomization-inference p value: 0.485). The variables for the randomization check are drawn from the post-focus group questionnaire, meaning they were measured post-treatment. The content of the questions, however, is plausibly fixed pre-treatment, including age, tribal identity, number of households members, and year of arrival in the host country.

6 Manipulation Check

6.1 Density Manipulation Check

As a basic test of design, I demonstrate that treatment groups had higher density and lower diversity. First, I test whether treatment groups had higher focus group density, defined as δ_i in Equation 9 as the share of realized ties to possible ties in focus group i . d_i denotes the total number of realized ties between the n_i participants in focus group i . $n_i(n_i - 1)$ denotes the total number of possible ties in the focus group. We divide the number of potential ties by 2 because we are interested in undirected ties.

$$\delta_i = \frac{2d_i}{n_i(n_i - 1)} \quad (9)$$

I present three metrics of participant connections. First, I measured simple ties, operationalized as other people in the focus group whom the respondent knew by name before the day of the focus group. Second, I asked respondents how many other participants they regularly exchange visits with, a metric I refer to as strong ties. Third, I calculate a metric of weak ties by subtracting the strong-ties metric from the simple-ties metric for each respondent, which captures the number of participants whom the respondent knows by name but does not regularly exchange visits with.

Figure 3 shows that the density of high-density groups was much higher than

that of low-density groups. Based on participant self-reports the low-density groups had 11.6% average density of pre-existing simple ties, compared with 64.6% for the high-density groups. Looking at panel 2, we see that the low-density groups had 5.4% average density of pre-existing strong ties, compared with 39.7% for the high-density groups. Looking at panel 3, we see that the low-density groups had 6.2% average density of pre-existing weak ties, compared with 24.3% for the high-density groups.

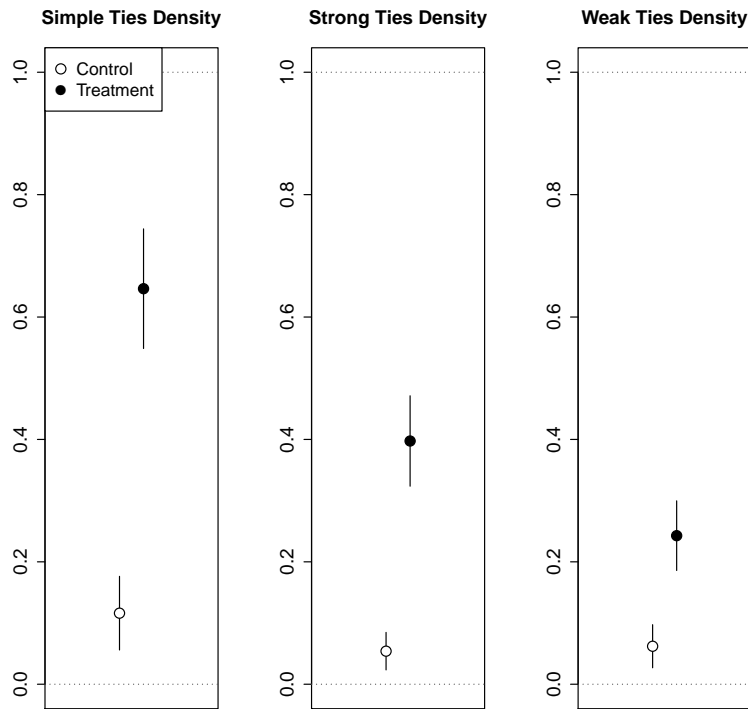


Figure 2: Density of Within-Focus-Group Social Ties, $n=56$

6.2 Diversity Manipulation Check

The assertion that the referral groups are denser than the randomly sampled groups is clearly supported by the data. The claim that the randomly sampled groups are more diverse than the referral groups is based primarily on a broad social network literature on homophily and the related empirical regularity found in many studies that people who are socially connected tend to be more similar than people who are not connected. There is widespread empirical and theoretical support for the idea that “birds of a feather flock together” (?). But in the current study my ability to provide definitive evidence that treatment manipulated diversity among study participants is constrained by issues of measurement and statistical power.

There are two measurement concerns around studying group diversity. First, whereas density has a single definitive metric, diversity does not have a single well-defined metric in the social network literature. Second, due to funding constraints I was not able to collect data on focus group participants’ diversity of connections.

My key claim is a group that is more densely connected will have a less diverse set of information about and connections to people outside of the group. That is, both the share of redundant information and redundant out-group connections will increase in in-group density. Because of funding constraints, I did not conduct extensive survey work about participants’ knowledge of the community to measure

diversity of information. I also could not conduct complete social network mapping within the focus groups, let alone network mapping for participants with people outside of the focus group. Ideally, we would have social network data for who focus group participants know in the complete community, and we could study whether the ties that participants have are more diverse.

One could present group-level variation in member characteristics as a metric of diversity, and assert that more diversity in individual-level characteristics (age, marital status, etc.) provides some evidence about diversity of ties. Indeed, compared to dense (referral sampled) groups, diverse (randomly sampled) groups exhibit higher variance in terms of a number of ascriptive and descriptive characteristics including age range, marital status, household size, and number of days worked in the past month. These differences are not statistically significant, possibly due to the small sample size.

In addition to from the questionable credibility of whether greater variation in a group's marital status teaches us about the variation in their out-group ties, these tests for treatment effects on the empirical variance may suffer from power issues. Once we define the unit of the analysis as the focus group, our sample size drops to 56, which may prevent us from identifying systematic differences between referral and randomly sampled groups that we would have detected with a larger number of

groups.

We can see the results presented in Table 1. I run a nonparametric permutation-based test. I define a scalar measure of homogeneity in groups, as in-group variance divided by sample variance for each outcome, as shown in equation 10. Then I compute its null distribution by randomly re-forming groups from all participants. Then I calculate the empirical probability (under this permutation distribution) of a measure as or more extreme than that actually observed.

$$\tilde{V}(X_k) = \frac{V(X_k)}{V(X)} \quad (10)$$

Where $V(X_k)$ is the variance of covariate X within group k and $V(X)$ is the sample variance of variable X .

Table 1: Differences between randomly and referral sampled units, first variable set

	Control (diverse) variance	$\hat{\beta}$	RI p value
Variance Pre-migration ties	0.84	-0.06 (0.14)	0.69
Var. Anticipated future ties	0.7	-0.03 (0.13)	0.595
Var. Tribal membership	0.62	-0.02 (0.11)	0.68
Var. How tribal is this town?	0.63	-0.05 (0.13)	0.715
Var. Birth year	0.89	-0.09 (0.13)	0.775
Var. Marital status	0.75	0.1 (0.22)	0.405
Var. Household size	0.89	0.15 (0.2)	0.24
Var. Work days, last month	0.6	0.01 (0.22)	0.45
Var. Year of arrival	0.68	0.43 (0.52)	0.275
Var. Humanitarian aid, last month	0.73	-0.06 (0.11)	0.905
Var. PCA dim. 1	0.84	-0.19 (0.21)	0.85

Notes: $n = 56$. $\hat{\beta}$ denotes difference-in-means estimate. Standard errors are reported in parentheses. Randomization inference performed with 10,000 simulated randomized treatment assignment vectors.

6.3 Manipulation Check

I present the formalization of the manipulation checks and the detailed results in the appendix. First, as a basic test of design, I test whether treatment groups had higher focus group density, defined as the share of realized ties to possible ties in focus group i , for example, if everyone knows everyone else in a group, the density is 1. If half of the possible dyadic relationships in a group are realized, the density of the group is 0.5

I present three metrics of participant connections. First, I show simple ties, operationalized as other people in the focus group whom the respondent knew by name before the day of the focus group. Second, I present a metric of strong, from data where I asked respondents how many other participants they regularly exchange visits with. Third, I calculate a metric of weak ties by subtracting the strong-ties metric from the simple-ties metric for each respondent, which captures the number of participants whom the respondent knows by name but does not regularly exchange visits with.

Figure 3 shows that the density of dense groups was much higher than that of diverse groups. Based on participant self-reports the diverse groups had 11.6% average density of pre-existing simple ties, compared with 64.6% for the dense groups. Looking at panel 2, we see that the diverse groups had 5.4% average density of pre-

existing strong ties, compared with 39.7% for the dense groups. Looking at panel 3, we see that the diverse groups had 6.2% average density of pre-existing weak ties, compared with 24.3% for the dense groups.

Although Figure 3 clearly demonstrates both the statistical and substantive significance of treatment effect, I also present regression results in the appendix.

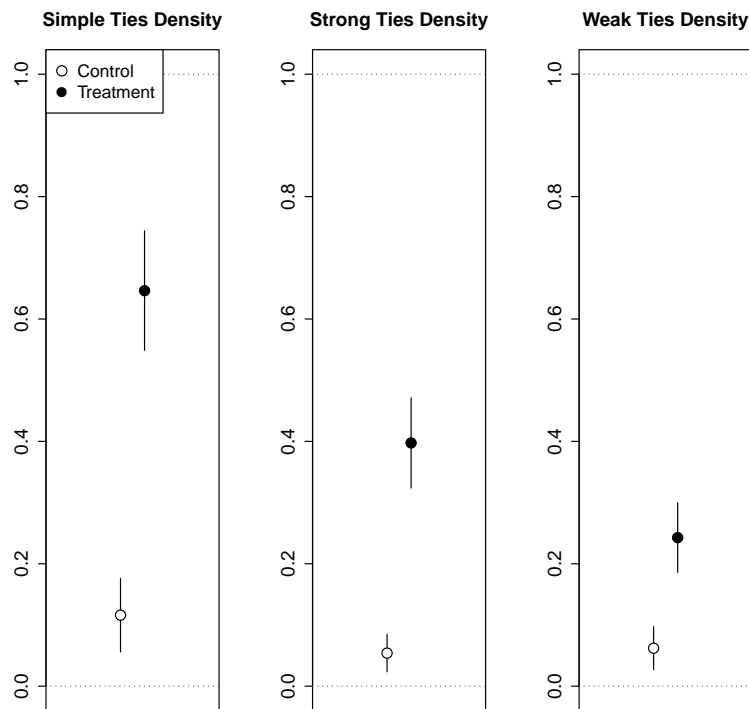


Figure 3: Density of Within-Focus-Group Social Ties, $n=56$

The data clearly demonstrates that the groups assigned to the ‘dense’ condition are in fact more dense. Next, I examine whether the dense groups had lower average

diversity than control groups. The claim that the dense groups will be less diverse is based primarily on a broad social network literature on homophily and the related empirical regularity that people who are connected are more similar than people who are not connected. There is widespread empirical and theoretical support for the idea that “birds of a feather flock together” (?). In the current study my ability to provide definitive evidence that treatment manipulated diversity among study participants is constrained by issues of measurement and statistical power.

Empirical measurement of diversity is more nuanced than density because direct measurement of the network characteristics we care about to study information diversity is difficult. My key claim is that a densely connected group will have less diverse information and a less diverse set of connections to people outside of the group. That is, the share of redundant information and redundant out-group connections is increasing in group density. The best metric of information diversity that I have – the diversity of comments in the focus group – is itself a main outcome in this essay, and therefore cannot be used as a manipulation check.

Measuring diversity of out-group social ties requires extensive and costly network mapping. Such measurement is more complicated than asking about the number of people participants know due to the redundancy of out-group connections. Because of funding constraints, I did not conduct complete social network mapping within

the focus groups, let alone attempting network mapping for participants with people outside of the focus group. Ideally, we would have social network data for who focus group participants know in the complete community, and we could study whether the ties that participants have are more diverse.

One could present group-level variance in member characteristics as a metric of diversity, and assert that more diversity in individual-level characteristics (age, marital status, etc.) provides some evidence about diversity of information and ties. As I present in the appendix, compared to dense (referral sampled) groups, diverse (randomly sampled) groups exhibit higher variance in terms of a number of ascriptive and descriptive characteristics including age range, marital status, household size, and number of days worked in the past month. These differences are not statistically significant, possibly due to the small sample size (that is, 56 focus groups).

7 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.

8 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 2.11. In each audio vignette there were two Syrian male voice actors, who I simply label 1 and 2 below.

8.1 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?

8.2 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.

8.3 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?

8.4 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.

9 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.

10 Heterogeneous Effects Analysis

10.1 Heterogeneous Effects by Gender

First, I find no strong evidence that the treatment effect on dialogue varies in gender.

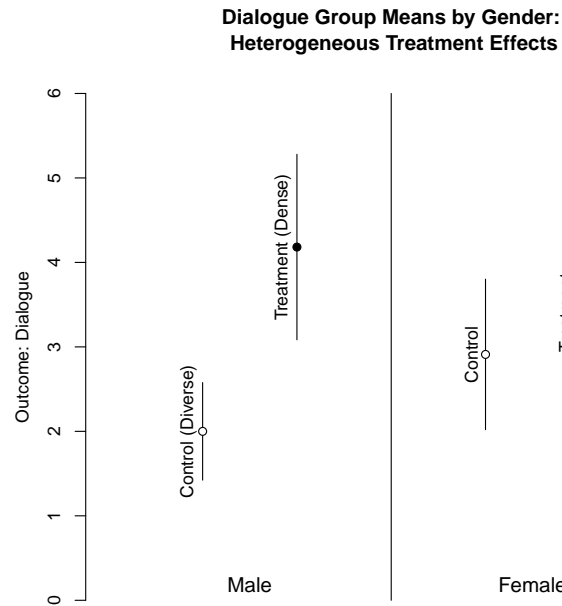
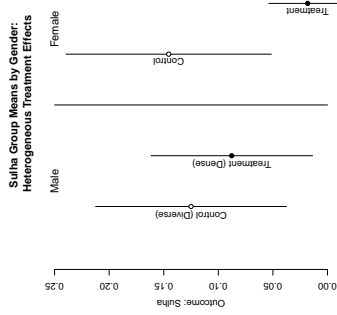
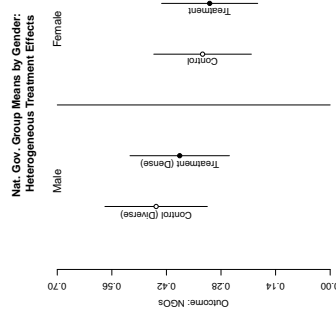


Figure 4: Outcome – Dialogue

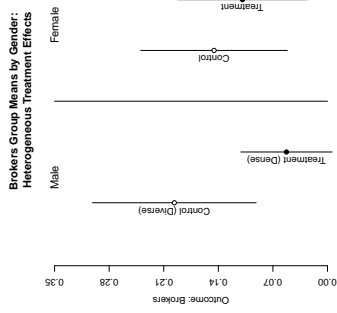
Second, I find no strong evidence that the treatment effects on resource access varies in gender.



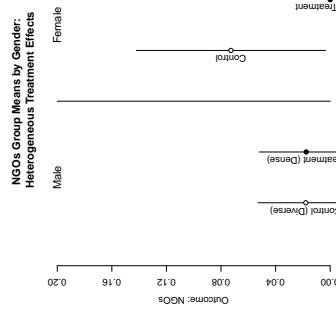
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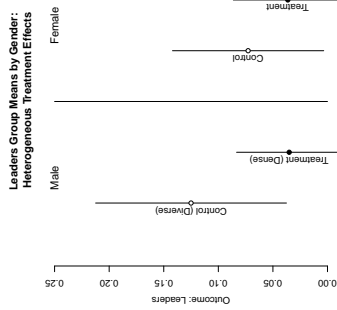
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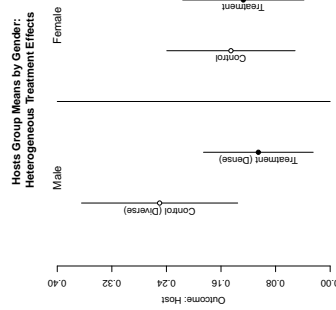
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(b) Subcaption B



(a) Subcaption A



(a) Subcaption A

Figure 6: Caption

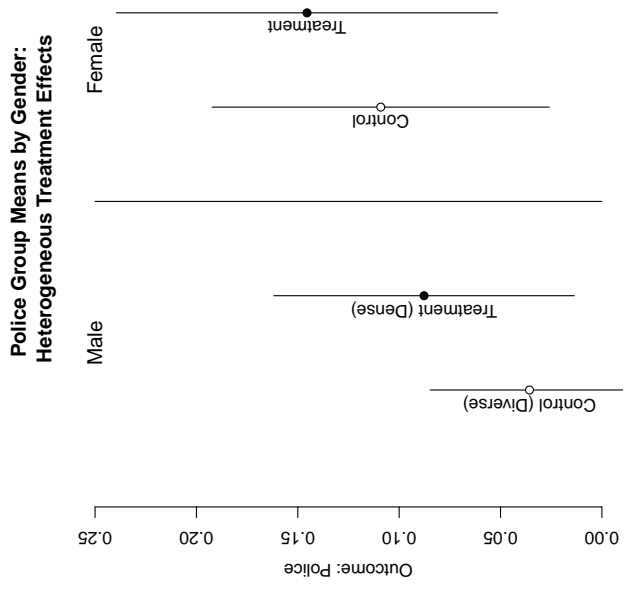


Figure 7: Outcome – Police

10.2 Heterogeneous Effects by Country

Next, I find no strong evidence that the treatment effect on dialogue varies across the two countries.

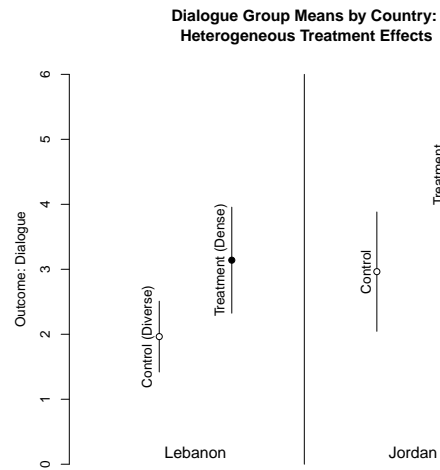
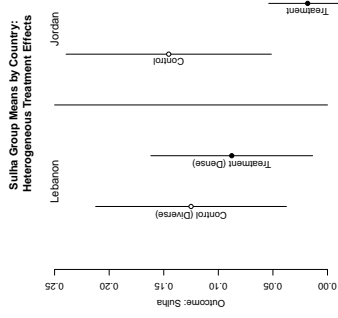
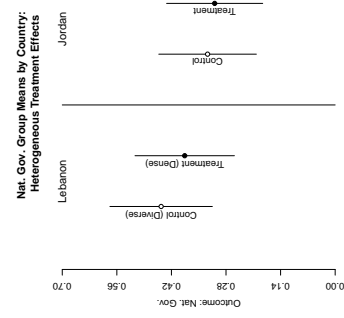


Figure 8: Outcome – Dialogue

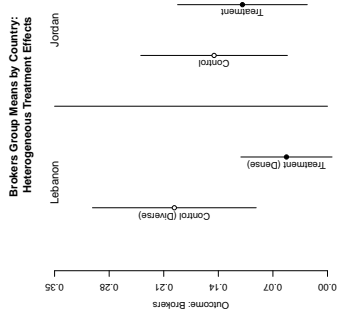
Last I see no strong evidence that that treatment effects on resources access vary across the two countries.



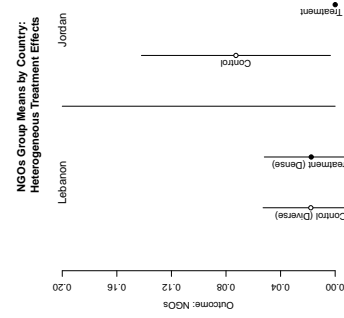
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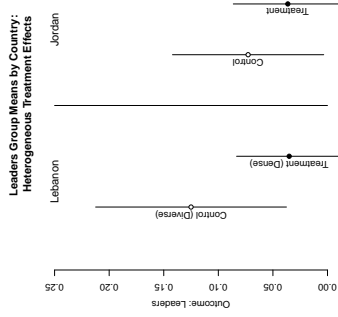
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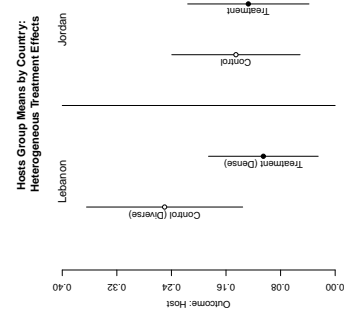
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(a) Subcaption A



(a) Subcaption A

Figure 10: Caption

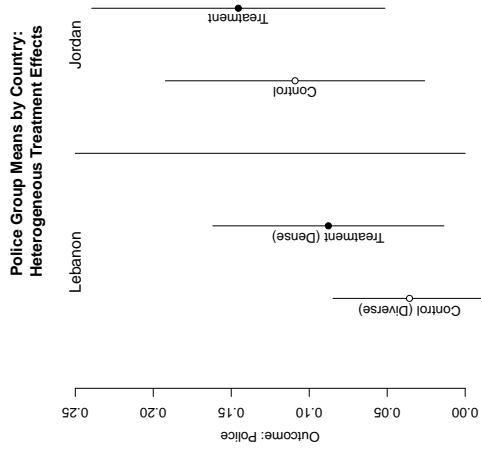


Figure 11: Outcome – Police

11 Alternative Theories That Do Not Rely on Information Flow

11.1 Psychological Mechanisms

People in high-density groups may cooperate more because they care 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 express 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 express 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

11.2 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.

12 Testing Theories That Do Not Rely on Information Flow

12.1 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 express 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 of 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 2 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 2: 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	0.87	0.4	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. 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.

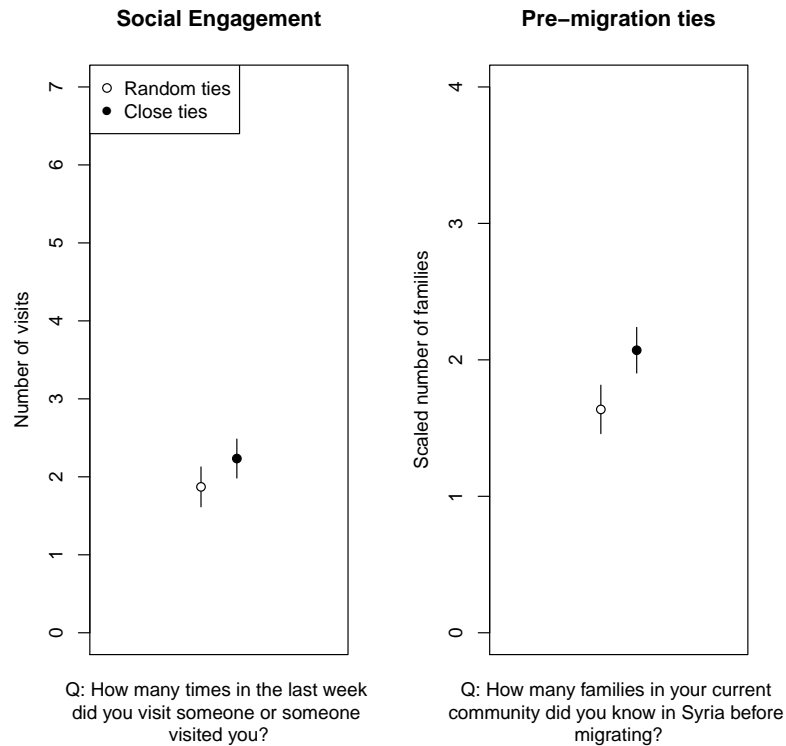
12.2 Network Location

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 2.87 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 3 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 12: 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 3: 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. 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 see the evidence as pointing toward the role of information flow between participants and not as evidence that the treatment effect is driven by participants' locations in the broader community.

13 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.

14 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 13 and 14, 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 13 and 14 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 13 and Figure 14 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 13 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 14 shows no meaningful variation in the treatment effect estimate and uncertainty across control sets.

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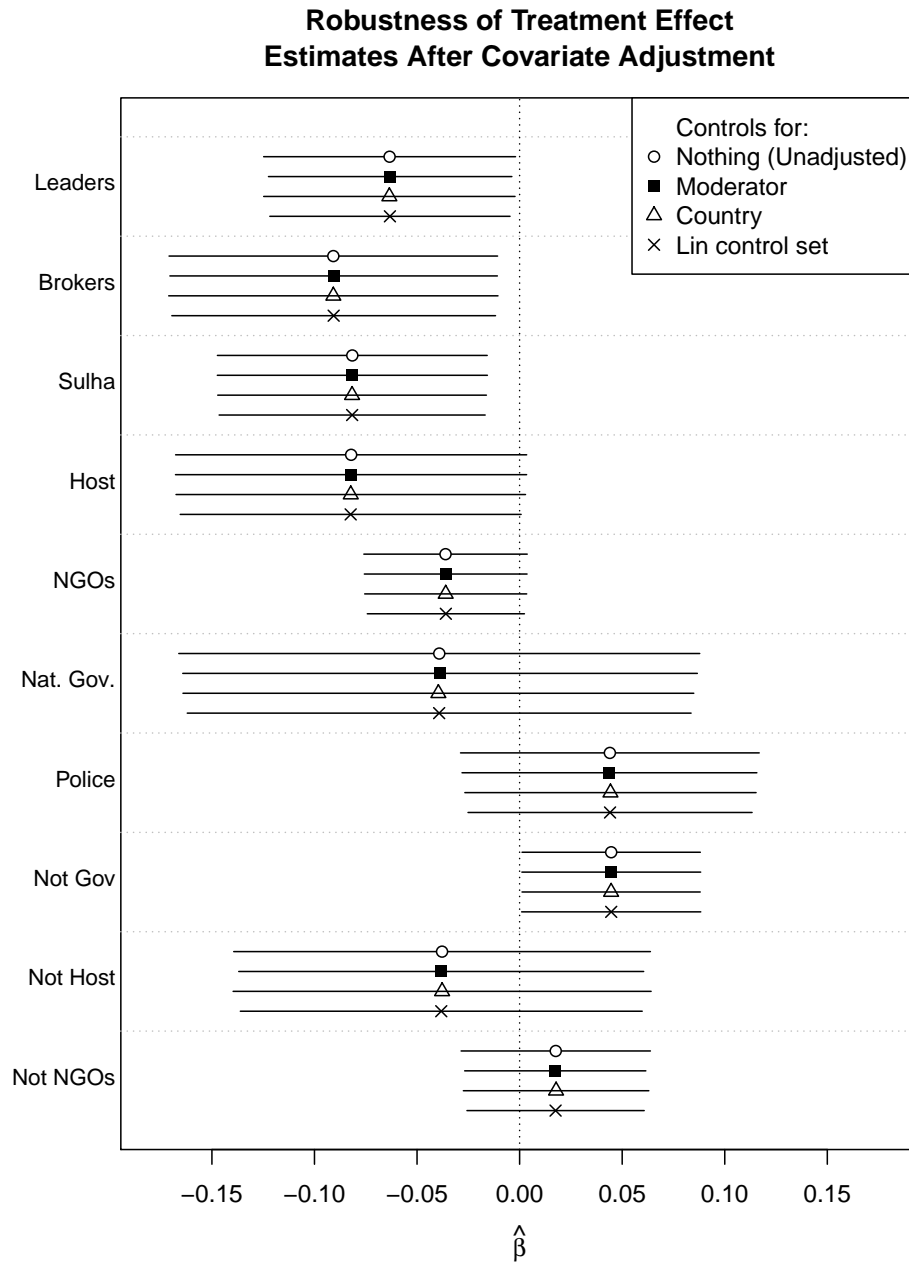


Figure 13: Robustness of External Resources Results Across Control Sets

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

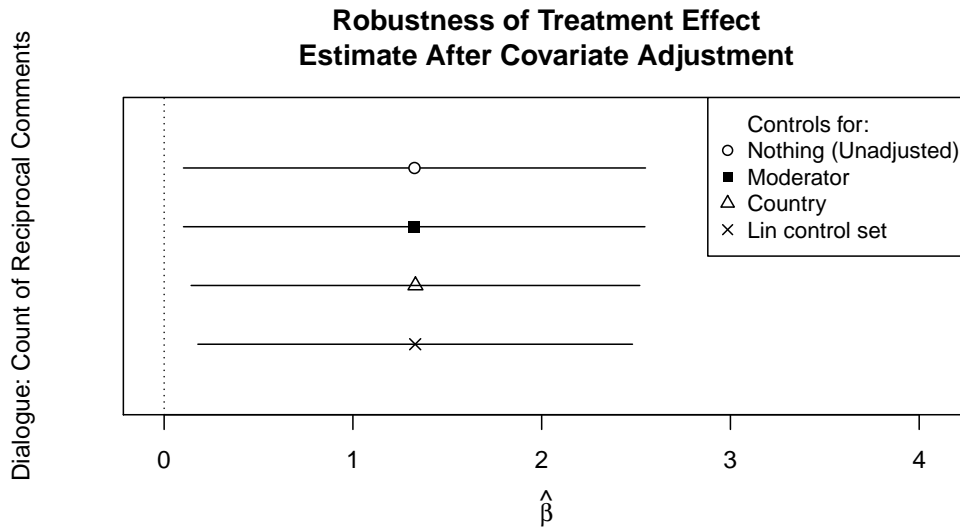


Figure 14: Robustness of Dialogue Results Across Control Sets

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