Mobilizing Activists through Intergroup Contact: 
Experimental Evidence from Israel *

Salma Mousa † Chagai M. Weiss‡
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Note to readers: Thank you for taking the time to read our pre-analysis plan. We recently began collaborating with an Israeli social movement to evaluate their on-line and off-line mobilization strategies. As you will see, the evaluation we are designing seeks to understand whether intergroup contact (virtual or in-person) can increase a host of outcomes relating to engagement with the social movement. We plan to implement our first study (study A- email study) by the end of the semester, and build on our findings from study a to refine study B (in-person canvassing). As you may notice, we are still in a rather preliminary stage and therefore any and all comments are most welcomed. Specifically, we would be grateful to receive feedback on study A, and the ways in which we can finalize the design in ways that will complement a future in-person canvassing study (which we hope to implement over the summer)

1 Overview

One of the most robust findings in social science is that diversity is negatively associated with collective action and public goods provision (Alesina, Baqir and Easterly, 1999; Miguel and Gugerty, 2005; Habyarimana et al., 2007; Fukuyama, 1995; Rothstein and Uslaner, 2005). Recent advances have explained why ethnic diversity limits collective action (Habyarimana et al., 2009, 2007). Nonetheless, we still know very little about how groups may overcome “diversity penalties” to promote collective action. This gap is concerning since many adverse social phenomena, not least of which is violent conflict, could plausibly be ameliorated if we could achieve two goals: (1) reducing prejudice toward out-groups, and (2) promoting collective action around public goods outcomes (Paluck, 2009; Green and Seher, 2003; Hewstone, 2015). This study aims to use inter-group contact to achieve both goals.

*This is a preliminary pre-analysis plan, please do not circulate.
†Stanford University
‡University of Wisconsin - Madison
Inter-group contact has long been thought to reduce prejudice by encouraging empathy and filling in information gaps about the “other” (Allport, 1954). Scholars have recently turned to field experiments to test the potential for intergroup contact to reduce prejudice (Enos, 2014; Burns, Corno and La Ferrara, 2015; Rao, 2014). These studies collectively suggest that extended, collegial contact is promising, while brief or negative contact can worsen prejudice (Sands, 2017; Enos, 2014).

Yet little is known about the impacts of intergroup contact in conflict settings – fewer than 3% of the 515 intergroup contact studies reviewed in Pettigrew and Tropp’s meta-analysis involved groups in conflict (Pettigrew and Tropp, 2006). These same societies arguably have the most to gain from successful intergroup contact interventions. Moreover, contact studies tend to focus only on prejudice as an outcome. Other policy-relevant outcomes that may be shaped by intergroup contact – like political mobilization – have yet to be systematically studied by contact interventions. Lastly, different forms of contact are rarely tested against each other despite calls to investigate the different conditions under which contact unlocks tolerance Paluck, Green and Green (2017).

Partnering with a Jewish-Arab social movement, we aim to overcome these gaps and test the efficacy of contact as an intergroup mobilization tool. To do so, we introduce two experiments that serve as a mini meta-keta, in that we coordinate treatment arms and outcomes as much as possible, while holding constant the conflict context. The first experiment targets members of the social movement’s listserv, and randomizes the ethnic identity of the mobilizer and the mobilization issue areas. The second experiment targets pedestrians in three Israeli cities, and randomizes the ethnic identity of the mobilizing canvassing teams and the mobilization issue areas. By varying the identity of mobilizers, as well as the issue area around which individuals are mobilized, we seek to learn whether intergroup contact can promote intergroup mobilization for politically salient and non-salient issues. We operationalize “mobilization” across both studies using three key outcomes:

1. Signing a petition
2. Donating to the movement
3. Attending volunteer events

We take prejudice as a secondary outcome, measured via a roster of survey questions in the canvassing experiment.
2 Filling Gaps in the Literature

2.1 Conditions of Contact

Recent studies have aimed to shed light on the “black box” of prejudice reduction by experimentally testing different conditions of contact. For instance, Lowe (2018) tests cooperative versus adversarial contact, while Scacco and Warren (2018) test how the specific environment in which contact occurs (i.e. mixed versus homogenous classrooms) affects prejudice outcomes. We build on this research agenda by testing two forms of contact across two related experiments: in-person versus virtual contact.

Related studies have shown that online role-playing games can encourage perspective-taking (Simonovits, Kézdi and Kardos, 2018; Adida, Lo and Platas, 2018), although without a comparison group experiencing in-person role-playing games. Similarly, studies of virtual contact among children involving fictional out-group members have found positive effects on tolerance, but lack causal identification (Aboud, Friedmann and Smith, 2015; Beelmann and Heinemann, 2014; Yan and Bresnahan, 2018; Crisp and Turner, 2009). We aim to fill these gaps by leveraging two related field experiments to test the effects of in-person vs. virtual contact among a “hard test” sample: adults living in a conflict setting.1

2.2 Content of Contact

Previous research suggests that canvassing, which oftentimes involves some form of intergroup contact, can be effective for low-salience, but not high-salience issues (Broockman, Kalla and Sekhon, 2017). We systematically test this proposition by randomly varying the issue around which mobilization occurs. In doing so, we seek to determine whether brief intergroup interactions around issues with high (low) intergroup salience can ignite intergroup mobilization. Therefore, subjects in both experiments are randomly presented with one of two issue areas on which our partner organization is currently mobilizing citizens. In the in-person canvassing experiment, this prime takes the form of a script delivered by volunteers to pedestrians. In the virtual canvassing experiment, this script is adapted to text in the body of an email. For additional information about both issue areas (The nation state bill and minimum wage for senior citizens), see section A in our appendix).

1While we hold many factors constant across experiments, we are not randomizing subjects into virtual vs. in person contact.
3 Experimental Designs

3.1 Study A: Virtual Contact and Mobilization

Our social movement partner has a listserv of around 14,000 members. This organizational database also includes demographic information on most of these members. We sample a subset of respondents and send them an email on the movement’s behalf (For a sample email see figure A1 in the supplementary materials). We randomize the name of the email sender to reflect one of four options: (1) Jewish activist, (2) Arab activist, (3) an inter-ethnic pair of activists, (4) the organization (baseline).

Our collaborating social movement aims to mobilize Jews and Arabs on an array of issues areas relating to equality and social justice. Previous campaigns have addressed multiple topics including: universal minimum wage, anti-occupation rallies, domestic violence, zoning and planning amongst Bedouin communities, and social security for disabled citizens. We randomize the topic addressed in the email itself, adapting a script relating to either (1) minimum wage for senior citizens, or (2) the nation state bill. We select these two different campaigns in order to test whether online contact can result in mobilization over issues with low (high) intergroup salience.

3.1.1 Randomization

Blocking on ethnicity (proxied by name), and subsetting down to respondents according to geographical location, we randomly sample $n = 12,000$ members from the organizational listserv. We then randomly assign half of the sample ($n = 6,000$) to receive an email mobilizing around a campaign of minimum wage for senior citizens, while the other half is assigned to receive an email mobilizing against the nation state bill. The issue area is specified in the email subject heading.

Conditional on receiving either the minimum wage or nation state bill email, we then randomize the identity of the email senders. The movement currently sends emails from an “info@movement.org” account, which includes an employee name in the sender box written in both Arabic and Hebrew. We divide our sample to receive emails from one of four senders:

1. An easily identifiable Jew (Uri)
2. An easily identifiable Arab (Ahmed)
3. An easily identifiable Jew and Arab (Uri and Ahmed)
4. Organization name (control group)

$^2$Randomizing name order in the mixed sender condition
Table 1: Treatment Conditions (Email Experiment)

<table>
<thead>
<tr>
<th>Canvasser Identity</th>
<th>Topic</th>
</tr>
</thead>
<tbody>
<tr>
<td>Jewish</td>
<td>Minimum Wage</td>
</tr>
<tr>
<td>Jewish</td>
<td>Nation State</td>
</tr>
<tr>
<td>Arab</td>
<td>Minimum Wage</td>
</tr>
<tr>
<td>Arab</td>
<td>Nation State</td>
</tr>
<tr>
<td>Inter-ethnic</td>
<td>Minimum Wage</td>
</tr>
<tr>
<td>Inter-ethnic</td>
<td>Nation State</td>
</tr>
<tr>
<td>Organization</td>
<td>Minimum Wage</td>
</tr>
<tr>
<td>Organization</td>
<td>Nation State</td>
</tr>
</tbody>
</table>

In this factorial design ($2 \times 4$), roughly 12% of the sample is assigned to each treatment condition cell.

3.1.2 Procedure

The movement will send out eight distinct emails each to $n = 1500$ randomly selected members of their listserv. We randomize the time of day (afternoon at 3pm, or evening at 8pm) in which the email is sent, and whether the email is sent on a weekday or weekend. The movement typically includes both Arabic and Hebrew translations of any text included in the body of the email. As a robustness measure, we further randomize which language appears first in the email in order to avoid order or priming effects of language. We also randomize the framing of the subject header (personal vs. general). Contact theory predicts that personal information is more likely to induce empathy relative to a general framing. By exploring framing effects as an exploratory analysis, we learn about an additional dimension of contact – personal vs. impersonal contact.

3.1.3 Outcomes

The social movement team then records a set of behavioral outcomes capturing engagement and mobilization:

1. Whether the recipient opens the email
2. Signing an e-petition in support of the cause via a “click here to sign a petition” button in the email
3. Making monetary contributions to the social movement via a “click here to donate” button in the email
4. The level of the contribution conditional on donating (from a one-time 5 shekel donation to a recurring 150 shekel/month donation).

5. Attending social movements events, such as workshops and rallies

### 3.1.4 Covariates and Measurement

We control for the gender, ethnicity, and locality of subjects when estimating the average treatment effect. For a subset of roughly 4,000 recipients that joined the movement in person, we also have detailed information on additional covariates: recruitment method (i.e. during which campaign), recruiter ID, student, and membership type (paying/ newsletter/ participation in WhatsApp group). We run an additional analysis subsetting the data to these (presumably more committed) individuals, and adding these controls.

We estimate the data on an intent-to-treat basis. Average treatment effects are estimated at the treatment condition level. Standard errors are clustered at the time-date level (e.g. “afternoon-weekday” or “evening-weekend”).

### 3.2 Study B: Interpersonal Contact and Mobilization

Our second study, follows the main logic of the online email experiment. Thus we randomly assign pedestrians in Haifa, Jaffa and Jerusalem to be canvassed by (1) a Jewish activist, (2) an Arab activist, (3) an inter-ethnic pair of activists. A second treatment arm randomizes the topic addressed by the canvassers.

#### 3.2.1 Randomization

Study sites will include both predominantly Jewish and Arab, pedestrian-trafficked commercial areas in Haifa, Jaffa and Jerusalem. In coordination with our partners we will choose specific locations in order to limit variation within the subject pool and thus increase statistical power. On any given day, male canvassers of similar age, either one Jew, one Arab, or a team consisting of both one Jew and one Arab, approach passers at randomly scheduled times within a 15 day study period.

The canvassers rotate between two recruitment frames, one emphasizing the organization’s campaign relating to minimum wage for senior citizens, and another emphasizing the movement’s struggle against the nation state bill. The experiment is complete block-randomized by day, such that all six conditions occur once each day, rotating every 30 minutes (Sands, 2017).

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3In mixed groups, canvassers will randomize the identity of the “first approacher”.

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starting petition is randomized each day, and the canvassers rotate petitions based on these starting conditions. In total, there will be 60 date-time clusters across 15 days (Assuming 2 hours of petitioning every day).

3.2.2 Procedure

Canvassers approach every fifth adult, asking “Would you be interested in joining our Jewish-Arab movement which [works on increasing minimum wage for senior citizens/ fighting against the repercussions of the nation state bill]?” The canvassers are trained to paraphrase the petition language and respond to any questions with a brief, scripted response. This response includes facts and statistics curated by our partnering social movement. Canvassers are instructed not to deviate from this script in order to ensure consistency across canvassers and treatment conditions. If subjects ask to see the petition or wish to sign it, the canvasser then presents a clipboard with the petition sheet consisting of columns with Name, Signature, Zip Code, and Email (to join our mailing list). Zip codes allow us to estimate the socio-economic status and ethnic composition based on Israeli survey data.

3.2.3 Outcomes

In the experiment, the canvassers and later our partnering organization record a set of behavioral outcomes capturing mobilization:

1. Whether the pedestrian stopped to converse about the issue (Canvasser)
2. Whether the conversation was positive or negative (Canvasser)
3. Whether the pedestrian signs a petition in support of the cause (Canvasser)
4. Whether the pedestrian subscribed to the organization’s email list (Canvasser)
5. Attendance at the social movements events such as workshops and rallies (Social Movement)
6. Monetary contributions to the social movement via online fundraising campaigns (Social Movement)

Canvassers also measure prejudice reduction via two brief survey questions: “In general, do you think most Arabs/Jews can be trusted in business?”; and “Would you having an Arab/Jew as a neighbor?” These outcomes will be coded in a pro-tolerance direction, and collapsed into a tolerance index to increase statistical power. Note that all outcomes are conditional on outcome 1.

3.2.4 Covariates and Measurement

We will train the canvassers to quickly record the estimated age, gender, and ethnicity of the pedestrians approached. These variables will be used as covariates in estimating the average treatment effect, in addition to canvasser fixed effects. Average intent-to-treat effects are estimated at the cluster level, with standard errors estimated accordingly.

3.3 Non-Compliance and Attrition

To deal with non-compliance, we make the no-defiers assumption. We will adjust point estimates to account for non-compliance by dividing the point estimates by the proportion of treated compliers in the treatment group (contact with out-groups and/or exposed to a high-salience issue area) minus the proportion of treated compliers in the control group. If canvassers deviate from their assigned treatment and deliver the wrong script, we exclude these observations from the analysis. We test for differential attrition across the four treatment conditions in both experiments.

3.4 Missing Values

Canvassers directly collect data on subjects’ demographic traits, while our organizational partner systematically collects data on the same demographic traits when individuals join their listserv. We may nonetheless observe some random missingness if canvassers are not quick enough to record certain traits, or if listserv members fail to record certain traits (which may or may not be random). We address this issue by missing baseline values using the default predictive mean matching method in the mice package. We present the results with and without this imputation as a robustness check. All variables are taken as predictors for the imputation algorithm.

3.5 Treatment Effect Heterogeneity by Subject Attributes

We will analyze heterogenous treatment effects by subject ethnicity (Jewish or Arab) in both experiments. We will estimate this sub-group effect in two ways: (1) an interaction term in the ATE
model – the minimum sample size of $n = 10,000$ in the email campaign experiment is particularly well-powered to detect this interaction, and (2) via a Bayesian partial pooling model. The advantage of (2) is that treatment effect heterogeneity is often noisy. Pooling sub-group effects toward the ATE reduces this noise in a conservative and disciplined way. It is conservative because partial pooling biases against finding extreme sub-group effects and type $m$ errors. It is disciplined because we constrain our estimates using a prior with mean $\mu = \text{ATE}$ and $\sigma$ distributed gamma (fat tails).
References


Supplementary Information

A  Mobilization Issue Areas

A.1 Minimum Wage for Senior Citizens

Approximately 50% of Israeli senior citizens do not receive pensions or monthly salaries. For the most part, such senior citizens rely on government stipends which amount at less than 3,000 NIS (equivalent of approximately $860). Such stipends, which are substantially lower than the legal monthly minimum wage in Israel (5,300 NIS) are not sufficient to sustain a reasonable standard of living. Accordingly, one in four Israeli senior citizens lives in poverty. Our collaborating organization is currently promoting a campaign to increase senior citizens' stipends to a minimum of 5,000 NIS.

A.2 The Jewish Nation State Bill

After a long legislative process, the Israeli parliament has accepted the nation state bill in June 2018. This bill defines Israel as a Jewish nation state, raising many concerns regarding its exclusionary sentiment towards non-Jewish Israeli citizens. Critics of the nation state bill suggest that it will serve as a legal base for state-led discrimination against Palestinian, Druze and Bedouin Israeli citizens. Our collaborating organization has played a central role in combating the legislation of the nation state bill, and is currently working on alleviating its legal and social repercussions.

B  Email Details

We plan on following the organizations routine email format, as depicted in figure A1. To strengthen our treatment, we will include pictures of the mobilizing member above the email text. We will keep the format and background of pictures constant across different mobilizers.
Figure A1: Sample of email to be sent by the organization