Pre-analysis plan

Intergroup interaction and attitudes to migrants

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1. Introduction

There are an estimated quarter of a billion international migrants in the world, and the number has been increasing over recent decades (IOM, 2018). While access to developed countries may become more restrictive, there is little reason to assume that migration pressures will become weaker in the decades to come. One reason is climate change, whose slow-onset effects have been predicted to substantially increase future displacement of people (Rigaud et al., 2018). Positive effects of migrants on receiving communities have been documented (Alix-Garcia et al. 2018; Tabellini, 2020). However, migration can also be associated with social tension and conflict (Burke et al., 2015; Hsiang et al., 2013). In politics, xenophobic populism feeds on and feeds these tensions. To address these types of negative dynamics related to migration, it is important to understand how attitudes towards migrants form and evolve, and what measures can be taken to reduce the risk of conflict between host communities and migrants. One way to influence attitudes towards migrants is through information; experimental studies by Hainmueller and Hopkins (2014) and Grigorieff et al. (2016) show that informing people about the extent of immigration and the characteristics of immigrants can have an impact on attitudes towards them. Another way to potentially improve attitudes is through increased interaction between host community residents and migrants, which is what we focus on in the study described here.

Inter-group contact theory posits that interaction between members of distinct groups can reduce inter-group bias and discrimination, and improve relations (Allport, 1954; Pettigrew, 1998). For this to happen, the interaction needs to have four key characteristics; the groups have i) equal status and ii) common goals in the interaction, the interaction promotes iii) cooperation rather than competition, and iv) enforcement by authority, law, or custom. Studies using observational data tend to find that contact with immigrants correlate positively with favourable attitudes to immigration (Jolly and DiGiusto, 2014; Heath et al., forthcoming). However, no causal conclusions can be drawn from these studies as the relation could

be driven by unobserved third variables, or reverse causality. Experimental studies exploiting randomized assignment into more or less ethnically heterogeneous groups such as army units suggest a positive causal effect of interacting with ethnic minorities on attitudes to immigrants (Finseraas and Kotsadam, 2017; Finseraas et al., 2019). Given the special nature of the groups studied in these experiments, whether results would replicate in other types of groups of a more civic cooperation type is an open question. Moreover, exactly what goes into the interaction is not completely clear, which raises the question of how sensitive the effects are to small changes in the contents of interaction, including whether subtle cues which trigger competitive associations or identity threats would undermine the effects. Furthermore, Scacco and Warren (2018) suggests that the interpretation of these types of studies may not be straightforward. Their experiment randomized students of different religious backgrounds into homogeneous and heterogeneous classes in a vocational training programme, as well as into a control group that did not participate in the programme. Their results show that while members of heterogeneous classes discriminate less against their religious out-group than members of homogeneous classes, comparison with the control group suggests that this difference is due to increased discrimination among homogeneous class members rather than reduced discrimination in the heterogeneous classes. It is possible, however, that the absence of an effect of inter-group interaction relative to the control group is due to the content of the interaction; a vocational training programme, while based on cooperation for mutual learning, could also naturally induce thoughts of later labour market competition.

We contribute to this literature by conducting a randomized field experiment which explicitly manipulates the content of interaction. We randomly assign 600 residents of the host community of a refugee camp in the Tigray region of Ethiopia, to four treatment and a control group. In the first three treatments, host community members are paired with a randomly selected refugee from the camp and the two then play a guessing game. The framing of the game is neutral in the first treatment, and our a priori expectation is that this form of active host-refugee interaction will improve host community residents' attitudes towards migrants relative to the control group. In the second treatment, we subtly introduce an economic framing to the game, to see if this triggers a focus on migrants as a source of competition in local markets. While our expectation is that the economic framing will reduce the impact of active interaction on attitudes towards migrants, it is in principle possible that economic cues may also highlight the economically difficult situation of migrants, leading to a more positive effect on attitudes. In the third treatments, we subtly introduce an identity framing to the game, to test whether attention to ethnic differences in the context of interaction induces identity threat responses in host community members. We expect this identify framing to reduce the impact of interaction on attitudes, but again it is in principle possible that the identity cues highlight the value of diversity in interaction, making the effect of interaction stronger. The fourth treatment pairs host community members with other host community members to play the neutral version of the game, in order to rule out the possibility that any improvement in attitudes to migrants is driven by the game in itself, rather than by host-migrant interaction.

In sum, our study addresses the following two research questions:

1. What is the effect of active interaction between host community residents and migrants on the residents' attitudes to migrants?
2. How does introducing economic and identity cues to the interaction change its effect on host community members' attitudes to migrants?

This pre-analysis plan presents the sampling procedure and the design of the experiment, and then details empirical strategies and the hypotheses to be tested. The plan was registered in the AEA RCT registry on 7 January 2020. The experiment will be starting no earlier than 13 January 2020.

2. Research strategy

The experiment will be conducted in Tigray region of Ethiopia, which borders on Eritrea. Host community participants will be recruited from the town of Mytsebri, which is close to the Mai Ani refugee camp, from which we will recruit the refugee participants. 600 host community participants will be recruited, by enumerators starting from the main intersection of the town, and fanning out in pre-specified directions, inviting an adult from every fifth household to participate in the experiment. In the refugee camp, a facilitator will register 1000 willing participants, from which 480 will be randomly selected to participate in the experiment. This will give us 120 host community participants for each treatment or control group, and the same number of refugees for the treatments in which they are involved (we also have a control group of refugees, but this is for a separate purpose not detailed in this pre-analysis plan). Participants are randomly assigned the day of participation ahead of time. At their assigned day of participation, host community and refugee participants are bussed to an experiment site in Mytsebri where the experiment will be conducted. The treatments and instrument were thoroughly piloted before the survey experiment started, in a different host community using migrants not living in a refugee camp, to avoid contamination.

3. Research design

Upon arrival at the site of the experiment, participants will be randomly assigned to one of the treatment groups or the control. Participants assigned to one of the four treatments groups are randomly paired with another participant; in treatments 1 through 3 host community participants are thus paired with a refugee participant, in treatment 4 host community participants are paired with another host community participant. In other words, the first three treatments entail host-refugee interaction of different forms, the fourth treatment entails host-host interaction. Participants in the treatment groups go through 15 minutes of paired interaction (detailed in the treatments section below), before responding individually and separately to a survey. Participants in the control group do not interact with another participant, and move straight to the survey. Survey interviews are conducted electronically using tablets running ODK (Open Data Kit). While host community and refugee participants respond to two different questionnaires, the analysis specified in this plan centres on the host community participants, using only information collected from their survey. The host community participant questionnaire has the following sections:

1. Treatment variables
2. Practicalities
3. Covariates 1
4. Outcome variables
5. Covariates 2
6. Heterogeneous effects variables
7. Mechanism variables
8. Additional variables (for descriptive analysis)

3.1 Treatments

Host community participants are randomly assigned to four treatments and a control group. The four treatments all begin with 10 minutes of informal interaction in the randomly formed pairs; host-migrant pairs in the first three treatments, host-host pairs in the fourth treatment. After the informal interaction, the pairs in the four treatment groups play variations on an incentivized guessing game, specified as follows:

- Treatment 1 (host-migrant interaction, neutral game):
  Participants are shown a sequence of five pictures of various items, and asked to guess physical properties of the items (such as the weight of a bag of teff, a staple grain in Ethiopia and Eritrea, or the age of a pictured waitress).

- Treatment 2 (host-migrant interaction, economic game):
  Participants are shown the sequence of the same five pictures as in Treatment 1, and asked to guess their price (such as the price of a bag of teff, or the wages of a pictured waitress).

- Treatment 3 (host-migrant interaction, identity game):
  Participants are shown the sequence of the same five pictures as in Treatment 1, and asked to guess their regional origin (such as how many regions of Ethiopia produce teff, or the regional origin of a pictured waitress).[1]

- Treatment 4 (host-host interaction, neutral game):
  Same game as treatment 1, but played by pairs of host community members.

Answers to the questions in the guessing game are discussed in the pairs, but their final answers are collected individually in the first section of the subsequent survey. Participant compensation depends on the number of correct answers. The control group do not interact with anyone (neither informally nor through a game), and receive a fixed participation fee. In all cases, compensation is made after completion of the survey.

3.2 Outcome variable

Our outcome variable is based on three survey questions:

- B1. "To what extent do you agree with the following statement: 'Refugees who are currently living in refugee camps in Ethiopia should be allowed to freely work and live outside of the camp.'"
- B2. "To what extent do you agree with the following statement: 'Refugees who are currently admitted to Ethiopia should be allowed to settle in my home community permanently if they are not able to return to their home country.'"
- B3. "To what extent do you agree with the following statement: 'If given a chance to settle, a refugee can be as good a citizen as someone who is born locally'"

All three questions are answered on the following scale: 5 – Agree very strongly, 4 – Agree, 3 – Neither agree nor disagree, 2 – Disagree, 1 – Disagree very strongly, 0 – Don't know.

For our dependent variable, Attitude to migrants, we create an index from responses to these questions. We sum the responses to the three questions (treating Don't know as missing), and rescale the sum to a number between zero and one by subtracting 3 and dividing the resulting number by 12.

3.3 Covariates

The following variables will be used to test for balance between the control and treatment groups, and as covariates in our analyses (with the addition of village fixed effects):

- Age (count variable, number of years)

[1] Regional origin is related to ethnicity in the Ethiopian case.

- Gender (dummy variable, 1 – male, 0 – female)

- Household head (dummy variable, 1 – respondent is household head, 0 - not head)

- Education (count variable, number of years of education)

- Occupation (6 dummy variables for i) Skilled employee, ii) Unskilled employee, iii) Day labourer, iv) Farmer, v) Self-employed (owns business with no non-family employees), vi) Self-employed (owns business with at least one non-family employee), with Other the excluded category)

- Asset index (based on factor analysis of the following asset variables: ownership of farm land, house, refrigerator, TV, bicycle, motorcycle, car/bajaj, mobile phone, computer, number of rooms the household occupies)

- Own migration history (dummy variable, 1 – respondent has lived outside of Tigray, 0 – otherwise)

4. Empirical strategy

Based on our research questions, we test three hypotheses as detailed below.

Hypothesis 1: Active interaction with a migrant through a neutral game has no effect on attitudes to migrants among host community members.

Hypothesis 1 will be tested through OLS estimation (with robust standard errors) of the following equation:

$$y_i = \alpha + \beta_{T1} T1_i + \beta_{T2} T2_i + \beta_{T3} T3_i + \beta_{T4} T4_i + \varepsilon_i \quad (1)$$

where y_i is the outcome for individual *i*, and Tt_i are indicator variables taking the value one if individual *i* is in treatment group *t*, and zero otherwise.

We will also estimate an equation which includes the covariates specified in section 3.4 and village fixed effects, captured by the vector X_i:

$$y_i = \alpha + \beta_{T1} T1_i + \beta_{T2} T2_i + \beta_{T3} T3_i + \beta_{T4} T4_i + \boldsymbol{X_i}\gamma + \varepsilon_i \quad (2)$$

Formally, hypothesis 1 is expressed as:

$$\beta_{T1} = 0 \quad (3)$$

Theoretically, our a priori expectation is that the effect of the neutral host-migrant interaction is positive. We test the following alternative hypothesis using a one-sided t test:

$$\beta_{T1} > 0 \quad (4)$$

To rule out the possibility that the effect is driven by the playing of a game, rather than the active interaction with a migrant, we also test the difference between the effects of treatments 1 and 4 using a one-sided t test:

$$\beta_{T1} > \beta_{T4} \quad (5)$$

Our second hypothesis is as follows:

Hypothesis 2: There is no difference in the effect of active interaction with a migrant through a neutral game and through an economically framed game.

Formally, hypothesis 2 can be expressed as:

$$\beta_{T1} = \beta_{T2} \quad (6)$$

A priori, it is not obvious which type of game will have a more positive effect on attitudes to migrants. We therefore test the following alternative hypothesis using a two-sided t test:

$$\beta_{T1} \neq \beta_{T2} \quad (7)$$

The third hypothesis can be phrased as:

Hypothesis 3: There is no difference in the effect of active interaction with a migrant through a neutral game and through an identity framed game.

Formally, hypothesis 3 can be expressed as:

$$\beta_{T1} = \beta_{T3} \quad (8)$$

Again, it is not obvious which type of game will have a more positive effect on attitudes to migrants. We therefore test the following alternative hypothesis using a two-sided t test:

$$\beta_{T1} \neq \beta_{T3} \quad (9)$$

We have designed our survey instrument to also analyze mechanisms behind our results and heterogeneities in effects across groups. We do not, however, pre-specify these analyses, noting the trade-off between the credibility that pre-specification generates and the potential costs in terms of developing highly complex pre-specification with limits on potential learning from the data (Olken, 2015).

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