Prolonged Contact Does Not Reshape Locals' Attitudes toward Migrants in Wartime Settings ^{*†}

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January 28, 2023

Abstract: Despite record numbers of internally displaced persons (IDPs) globally, we know little about the causal effects of intergroup contact between migrants and locals in active conflict settings. We conduct a randomized controlled trial of a vocational skills-training program conducted by Mercy Corps that enrolled 2,597 locals and migrants in near equal numbers in Kandahar, Afghanistan, where IDPs face daily prejudice and discrimination. Courses lasting up to six months emphasized collaborative learning and soft skills development. We surveyed participants at end-line and followed up eight months later. While the program provided the most sustained duration of intergroup contact (360–720 hours) experimentally evaluated to date, we find no evidence of reported behavioral or attitudinal change by locals (N=1,276) toward migrants generally, regardless of classroom demographics or course duration. Our findings suggest that prolonged contact through vocational training programs is insufficient to improve relations in conflict settings.

Keywords: intergroup contact, conflict, migration, internal displacement, field experiment, Afghanistan

^{*}We thank Mercy Corps, and especially Ninette Adhikari, Naquib Ahmad, Keith Ives, Max Izenberg, Jon Kurtz, Beza Tesfaye, Muhammed Ashraf Wahidi, and Rebecca Wolfe for the opportunity to evaluate the INVEST-Kandahar initiative. We also thank colleagues at MRA Associates, Roshan Telecom Afghanistan, and Sayara International, including Abdul Rahim Israr, Haroon Rasheed, and Nama Vanier, and especially our 30 enumerators, for excellent work in challenging conditions. We are especially grateful to Prakhar Sharma for exemplary project management. We acknowledge the tragic death of Rahmatullah Nasiry, who was killed in the horrific suicide bombing in Kabul on 31 May 2017. Lee-Or Ankori-Karlinsky, Benjamin Fifield, Donald Green, Kosuke Imai, Rebecca Littman, Kristin Michelitch, Salma Mousa, Elizabeth Levy Paluck, Rebecca Wolfe and participants from APSA 2020, UBC Centre for Migration Studies, Texas A&M Bush School, Watson Institute for International and Public Affairs at Brown University, University of Pennsylvania's Perry World House, PRIO PREACH Seminar, the JDC Research Conference on Forced Displacement, University of Washington International Security Colloquium, and the EGAP Priority Themes Seminar Series provided helpful feedback. Zhou acknowledges financial support from the National Science Foundation (SES.1148900) and CIFAR (GS22-006-3369). Lyall acknowledges financial support from the Air Force Office of Scientific Research (#FA9550-14-1-0072) and the United States Institute for Peace (#SG-446-15). The views and findings expressed here do not reflect the official positions of AFOSR, Mercy Corps, USIP, or the U.S. Government. The authors declare they have no conflicts of interest.

[†]This research received institutional review board (IRB) approval from Princeton University (#7280) and Yale University (#1506016047).

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Introduction

By mid-2021, a record number of at least 55 million people were internally displaced around the world. More than 85% of them had fled conflict and violence (IDMC and Norwegian Refugee Council, 2021). The international community has spent billions of dollars on development aid and humanitarian assistance designed to assist internally displaced people (IDPs) in these fragile countries.¹ An important consequence of these programs has been the opportunity for locals and migrants to intermingle and work together, raising the possibility that sustained intergroup contact through these programs may reduce local-migrant tensions. Yet we have little evidence about whether contact actually improves locals' relations with displaced persons.

Perhaps most worrisome is the prospect that these widely-used economic interventions might actually worsen intergroup relations by stoking competition over scarce jobs or by reinforcing existing prejudices. Given the scale and urgency of global displacement coupled with the increasing popularity of such humanitarian aid programs that bring locals and migrants together, there is a pressing need to examine whether prolonged and collaborative contact can improve locals' relations with migrants in wartime settings.

This is especially the case in our setting, Afghanistan, where nearly six million persons — one in six Afghans — were displaced by war since 2001. In partnership with Mercy Corps, we conducted a field experiment in Kandahar, Afghanistan, in 2015-16 that enrolled 2,597 locals and migrants in a vocational training program called *Introducing New Vocational Education and Skills Training* (INVEST). Over half of our participants were IDPs living in camps and informal settlements in the outskirts of Kandahar City. Participants were randomly assigned to vocational courses or were

¹IDPs are individuals who have been forced to leave their home due to violent conflict but remain in their country of origin or return after being refugees in another country. Unlike refugees, IDPs share nationality with the local population. In this paper, we refer to IDPs interchangeably with “migrants,” because that is how locals in our context refer to them, and because the wider literature on anti-migrant attitudes is also relevant for our study on IDPs.

waitlisted for eventual enrollment.

Through INVEST, locals and migrants spent up to six months together in a classroom setting working collaboratively on collective tasks and skills acquisition. This dosage of intergroup exposure in a naturalistic setting dwarfs prior experimental work on intergroup contact. Furthermore, although prejudice against IDPs has been building since the early 2000s in Kandahar, the tensions between locals and IDPs are not so deep-seated and historic to make attitudinal change through months of daily contact an unrealistic goal.

Yet despite these efforts and modest economic gains from participating in the program, we find that prolonged contact did not affect general attitudes about migrants among the 1,276 local participants. We also found no effect on their beliefs about whether migrants support political violence, about competition for jobs with migrants, or whether migrants placed unfair burdens on government resources. Local graduates also did not report increased post-INVEST contact with migrants in their communities. These precisely-estimated null findings hold regardless of participant traits, including age, gender, ethnicity, and prior exposure to violence, as well as course type and duration. These null effects also persisted for at least eight months after the program's conclusion. At best, this prolonged contact intervention had a half-full quality: while it did not stoke further competition or conflict, it also failed to improve intergroup relations between locals and migrants.

We note that while our study was conducted during wartime, the main faultline in the conflict was between Taliban and government forces, not migrants and locals. Nonetheless, the combination of large-scale displacement, physical insecurity, and active Taliban recruitment among IDPs fueled local-migrant tension and, on occasion, violence. For locals, migrants represented possible security threats as potential (or actual) Taliban fighters and informants. They also raised fears of heightened economic competition in a tight labor market. Given the precarious state of Kandahar's economy,

migrants were also viewed by locals as a drain on resources like schools and housing (Internal Displacement Monitoring, 2017; Lakhani and Amiri, 2020). In short, IDP status was a salient axis that shaped patterns of local-migrant relations in wartime Kandahar.

Our findings contribute to three strands of literature. First, our null findings suggest important scope conditions for intergroup contact theory (e.g. Allport, 1954; Pettigrew, 1998; Pettigrew and Tropp, 2006; Lemmer and Wagner, 2015). Despite a well-designed intervention with sustained, positive, intergroup contact, we failed to shift short- or long-term attitudes toward migrants in wartime conditions. These null results echo a small but growing chorus of researchers casting doubt on the likelihood of meaningful prejudice reduction in (post)-conflict environments (Scacco and Warren, 2018; Mousa, 2020). Traditional “light touch” anti-prejudice programs are likely to fare poorly in these conditions. Second, our results reinforce earlier observational research within migration studies on the stickiness of anti-migrant prejudices (Kustov, Laaker and Reller, 2021; Hangartner et al., 2019). While contact theory typically defines groups in terms of ascriptive traits like race or ethnicity, our research highlights the need to consider migrant status as a societal cleavage around which intergroup prejudice and competition can form. Finally, we contribute to the growing literature on the effects of common economic interventions like vocational training in fragile or violent settings.² Unlike existing studies, we call attention to the need to consider the possible negative externalities that bundled programs, like INVEST, might have on local-migrant relations in locations marked by intergroup economic competition, mutual suspicion, and physical violence.³

²For a review, see Zürcher (2020).

³A search for “vocational training program” on *Open Science Framework* shows over 42,000 studies; very few even acknowledge possible local-migrant tensions.

Theoretical Framework

Migration scholars have identified multiple factors that drive hostility toward migrants. These include fears that migrants are criminals- or insurgents-in-waiting, and thus as a source of insecurity (Salehyan and Gleditsch, 2006; Haer and Hecker, 2019; Masterson and Yassenov, 2021; Zhou and Shaver, 2021); that migrants represent labor market competitors while straining welfare programs (Mayda, 2006; Hanson, Scheve and Slaughter, 2007; Adida, 2014; Ceritoglu et al., 2017; Gaikwad and Nellis, 2017); and that perceived cultural or political differences will generate conflict (Bhavnani and Lacina, 2018; Alrababa'h et al., 2021).

Since these anti-migrant concerns are often rooted in stereotypes and misperceptions, a promising remedy is increased intergroup contact between migrants and locals. Through social contact, locals would be able to learn more about migrants, build trust and friendships, and change their attitudes. Indeed, through public opinion surveys, migration research has identified a strong, positive association between observational and self-reported contact and more positive attitudes toward migrants (Getmansky, Sinmazdemir and Zeitzoff, 2018; Ghosn, Braithwaite and Chu, 2019; Andersson, Dehdari et al., 2020; De Coninck, Rodríguez-de Dios and d'Haenens, 2020; Knappert et al., 2021).

However, these claims are vulnerable to selection effects and social desirability bias; people who feel more positively towards migrants are more likely to welcome contact with them or feel comfortable in reporting contact on a survey. Moreover, despite a vast prejudice reduction and intergroup contact literature, the majority focuses on the short term effects of light touch interventions (Paluck et al., 2021). There are only a handful of studies that combine intensive and sustained contact with real-world naturalistic settings. There are even fewer experimental studies of contact in actual (post-)conflict settings, and only one other study with migrants as the

outgroup category (Finseraas and Kotsadam, 2017).⁴

Contact theory predicts that prolonged positive social contact would reduce locals’ misperceptions about migrants and lead to more positive attitudes if the contact met four optimal conditions: equal status among groups, intergroup cooperation, common goals, and an institutionally supportive context (Allport, 1954). More recently, scholars have relaxed these conditions somewhat, arguing that all four conditions need not be present for contact to be successful (Pettigrew and Tropp, 2006). The context for the original contact hypothesis was desegregation in U.S. institutions, including public school classrooms. We share the same classroom environment as these original studies. INVEST participants were young people who experienced equality within the classroom; collaborative classroom tasks and shared broader goals, including graduation; and substantial support from local and international authorities. Yet recent meta-analyses on experimental studies find that the positive effects of contact are generally modest and short-lived (Paluck, Green and Green, 2019; Paluck et al., 2021). There is also the possibility that wartime attitudes are so hardened that no intervention can sway them. Indeed, prolonged contact may even reinforce stereotypes and heighten a sense of intergroup competition.

How does the contact in our study compare to related studies? Contact within INVEST courses differed from contact examined in recent experimental interventions in several important ways. Local and migrant treatment participants had a much higher degree of task-oriented (rather than free-form) interaction with one another, which should lead to better outcomes (Toosi et al., 2012). Drawing on a practice-based curriculum, instructors had students collaborate on shared tasks to learn new skills collectively in order to achieve the common goal of passing the course for certification. As part of the curriculum, they also received soft skills training (called “Ready to

⁴On the need for more field experiments testing intergroup contact theory, especially in non-Western contexts, see Paluck et al. (2021).

Earn” clubs), in which they developed communication, teamwork, and goal-setting skills, largely by role-playing scenarios together. Thus, throughout the semester, participants interacted intensively and were able to build relationships (Page-Gould, Mendoza-Denton and Tropp, 2008).

Additionally, instructors confirmed to us that there was no self-segregation within the classrooms. Instead, students had to work together on hands-on projects (e.g. everyone gathering around an automotive engine) to learn practical skills through shared problem-solving. Participants met for 6 hours per day, five days a week, for either three or six months, depending on the course. They therefore spent between 360 and 720 classroom hours together. This compares favorably with similar experimental studies in Israel (Ditlmann and Samii, 2016), Nigeria (Scacco and Warren, 2018), Iraq (Mousa, 2020), and India (Lowe, 2021), where participants spent between approximately 25 to 60 hours with members of the outgroup, respectively, and much more than the brief Jewish patient–Arab doctor interaction in Israel (Weiss, 2021). But unlike the sports-based studies which were designed to improve intergroup relations (and participants likely understood that to be the goal), our study is similar to other recent studies (Finseraas and Kotsadam, 2017; Rao, 2019; Weiss, 2021) where contact occurred in a naturalistic setting without overt emphasis on prejudice reduction.

We emphasize that INVEST, like most economic interventions, was a bundled treatment, combining vocational training, a life-skills course, a small start-up kit of trade-specific tools for graduates and, for some, a randomly-assigned unconditional cash transfer. While we cannot isolate the effects of our high dosage of contact from the other components within INVEST, we believe these components collectively reinforce positive contact among locals and migrants. Mandatory enrollment in the life skills course, for example, provided another, albeit briefer, opportunity to amplify the positive effects created by the more sustained intergroup contact during vocational training. All graduates received start-up kits, dampening possible intergroup competition over

jobs. The cash transfers were randomly assigned, of equal amounts, and distributed privately via cellphone. Since recipient status was not public knowledge, it is doubtful that these transfers worked at cross-purposes to the other components of the intervention.

Thus, building on contact hypothesis and these related studies, we hypothesize that positive and prolonged social contact with IDPs will improve locals' attitudes toward IDPs in general:

(H1) treated locals will report (a) more positive views of migrants and (b) more interactions with migrants outside of the program after graduation compared to the waitlisted (control) group.

In a secondary analysis, we explore heterogeneous effects by course duration, local-migrant classroom balance, and participant demographics. These heterogeneous effects analyses were not pre-registered. While we have variation in these conditions, we also note that we did not randomize them.

(H2) the positive effects should be greater for locals enrolled in six month courses, compared to those in three month courses given the larger dosage (Pettigrew, 1998);

(H3) those in more balanced classes in terms of the ratio of locals to migrants should see larger and longer effects, because the two groups are on equal footing (Saguy, Dovidio and Pratto, 2008);

(H4) younger participants will have larger and longer positive effects compared to older participants (Tropp and Prenovost, 2008; Beelmann and Heinemann, 2014; Paluck, Green and Green, 2019; Brown et al., 2021).

Study Context: Prejudice and Discrimination Faced by IDPs in Kandahar

Afghanistan offers an important example of war-induced population displacement and anti-migrant prejudices. It has one of largest IDP populations in the world; during late-2015 to 2016, when our study occurred, an estimated 1.35 million Afghans were internally displaced (Internal Displacement Monitoring, 2019).⁵ Kandahar, the site of our experiment, was home to about 125,000 IDPs, the fourth-highest total among Afghanistan’s 34 provinces. Kandahar City, where the Vocational Training Centers (VTCs) were located and training conducted, was by far the most popular destination within Kandahar and one of the highest districts in all of Afghanistan, part of a broader pattern of rural to urban forced migration.⁶

What drives negative attitudes toward migrants in Afghanistan? Locals view migrants as security threats, economic competitors in a tight labor market, and a strain on resources such as schools and housing. Locals cite IDPs as the most likely group to sympathize with or join the Taliban. Survey evidence partly confirms this reality: 25% of IDP respondents in Kandahar reported being approached by a Taliban recruiter, compared with only 3% of Kandahari locals (Lakhani and Amiri, 2020). Furthermore, our interviews with INVEST program directors revealed their concern about the “miserable conditions” faced by migrants and the widespread belief among locals that migrants had “saturated the job market and taken the jobs of Kandaharis,” which creates another axis of conflict between the two groups.⁷ Additionally, from a 2017 study, those who remained during the country’s waves of conflict judge those who fled to Pakistan and later returned, saying they are undeserving of aid (Internal Displacement Monitoring, 2017).

Periodic security crackdowns by local police on migrant communities also sowed tensions (Inter-

⁵These include “returnee-IDPs,” people who were refugees in Pakistan and Iran, but repatriated, often forcibly, back to Afghanistan and unable to return to their original homes (Internal Displacement Monitoring, 2017, 2020).

⁶IOM-DTM Baseline Mobility Assessment (Round 7, Oct-Dec 2018).

⁷Interview with VTC Director, December 2016.

nal Displacement Monitoring, 2019, 2020). Surveys among IDPs in Kandahar reported widespread feelings of social exclusion, police harassment, and difficulty accessing public services like education and health care. A 2018 study of returnees and IDPs in Afghanistan found high levels of reported social marginalization by host communities: “The host community has never considered us one of their own... They don’t allow their children to play with our children because they consider IDPs strangers... They [the nondisplaced] always argue with us over small conflicts. Clearly, they don’t want us to live beside them” (Lakhani and Amiri, 2020).⁸ This is a context where strong social networks are critical for local integration of migrants in wartime (Harpviken, 2009).

IDPs in Kandahar in particular cite social exclusion, harassment by police and local leaders, and greater difficulty accessing public services like education and health care: “A member of my extended family was a victim of a robbery—criminals attacked him, beating him, and robbed money from his small business. Without local connections (he is an IDP), he was not able to get help from the police to complain” (Lakhani and Amiri, 2020).⁹ Another survey conducted in 2018 found that 72% had been displaced twice; almost a third had been displaced three times. Insecurity and persecution by locals were the two most commonly cited drivers of secondary displacement (Norwegian Refugee Council, 2018). Thus, although our study does not examine historic and deep-rooted hatred between the two groups in contact, prejudice and discrimination are realities for migrants in Kandahar.

Study Intervention: the INVEST Program

Active conflict settings draw billions of dollars in aid programs that provide opportunities for social contact. The INVEST program was primarily designed with the main goal of reducing

⁸A 27-year-old Pashtun IDP in Kunduz, Afghanistan.

⁹A 35-year-old Pashtun IDP in Kandahar, Afghanistan.

Figure 1: Map of Kandahar City showing the locations of the four VTCs (red triangles), and surrounding IDP camp locations (blue points), weighted by their population from January 2014 to March 2016. Source for IDP data: IDMC’s Global Internal Displacement Database. Google basemap.

unemployment of at-risk youth through vocational training and an unconditional cash transfer.¹⁰

Local and migrant participants were recruited from Kandahar City and the neighboring districts of Dand, Daman, and Arghandab based their “at-risk” status as young, unemployed (or underemployed) individuals. Recruits were identified by the following actors: Mercy Corps, three provincial Departments – the Department of Refugees and Repatriation (DoRR), Ministry of Labour, Social Affairs, Martyrs and Disabled (MoLSAMD), and the Department of Education; as well as the UNHCR, village councils (*shuras*), local leaders, and tribal elders. This program received institutional support by local and international actors.

Prior to randomization, all participants chose to enroll in either three or six month courses at one of the four VTCs: Mirwais Mina (male only), Sufi Sahib (male only), Mahmood Tarzai (female only), and Aino Mina (mixed gender). Participants were assigned to the same VTC for

¹⁰We focus our attention here on the vocational training, known as the Technical Vocational Education and Training (TVET) arm, and report the effects of the cash transfer in the Supplementary Information.

the program’s duration based on proximity to their home and their gender.¹¹ Figure 1 shows the locations of the VTCs and their close proximity to IDP camps. There were 14 unique courses (examples include tailoring, carpentry, plumbing, and computer software skills), which across the four VTCs totaled 23 classes.¹² Mercy Corps estimated the cost at about US\$229 (15,600 Afghanis) per individual in 2016.¹³

Demographically, locals and migrants were almost identical in terms of age, high levels of unemployment, and ethnicity (see SI Figure S2). Among the 2,597 INVEST participants, 1,276 (49.1%) were Kandaharis while 1,321 (50.9%) were IDPs. On average, participants were 20.4 years old (median: 18 years), but their ages ranged from 10 to 51. 79.3% self-identify as Pashtun. Unlike Scacco and Warren (2018), Mousa (2020), Lowe (2021) which recruited men only, women comprise 36.2% of our sample (n=940). Thus, we are able to examine whether effects differ by gender. Only 28% were employed at baseline, 77.6% of the sample reported earning no income at all during the past month. For those who reported a net income, the average amount was only about US\$20 (1409 Afghanis).

All participants were enrolled by lottery, with similar SES backgrounds, helping ensure formal equality in the classroom. While participants were largely coethnic (Pashtun), both participants and instructors were easily able to identify who were migrants in the classroom through a number of cues, such as dialects and which neighborhoods, specifically IDP camps (see Figure 1), that migrant participants commuted from daily. All courses, which had an average of 40 students each, were naturally mixed in local-migrant composition; across the classrooms, the proportion of migrants ranged from 34.3% to 66.3%. Thus, there were no classes in which locals vastly outnumbered or were vastly outnumbered by migrant participants. Psychological theories anticipate that when

¹¹Participants commuted up to 45 minutes daily between their homes and the assigned VTC.

¹²For the full list of courses, see SI Section S1.

¹³This cost excludes fixed costs of renting the training facilities, main office expenses, and security.

	TVET treatment			TVET control			Totals	
	size (n)	size (%)	compliance (%)	size (n)	size (%)	compliance (%)	size (n)	size (%)
Migrants	667	25.7	80.2	654	25.2	100	1321	50.9
Locals	631	24.3	72.6	645	24.8	100	1276	49.1
Totals	1298	50	78.5	1299	50	100	2597	100

Table 1: This table shows the sample size and proportion for the TVET treatment and control groups, broken down by displacement status, as well as the compliance rate within each group. For the treatment (control) groups, compliance means (not) attending the courses.

outgroup members are viewed as not exceptional “tokens,” then individuals are more likely to generalize their positive views to the entire group (Paluck and Clark, 2020). Section S2 in the SI shows the proportion of migrant participants within each VTC-classroom.¹⁴

Research Design

Randomization

We used block randomization to assign treatment or waitlist (control) status. We first blocked on VTC site given their differences in gender composition and travel distance. Within each training site, we blocked on three versus six month vocational courses. Again, course duration was not randomized because they depended on the particular course selected by the respondent at baseline. In addition, we blocked on gender for the Aino Mina VTC. Thus, there are in total 10 blocks. Within each block, we created four groups by using Mahalanobis distances to match on displacement status, employment status, and whether the participant was exposed to violence in the prior year. We collected these variables prior to the baseline survey on an enrollment form. Although not the focus of this paper, INVEST also had a second intervention, a one-time unconditional cash transfer (UCT) of US\$75 (5,163 Afghanis). Across each group of four, we randomly assigned treatment to one of four types with equal probability – TVET treatment-UCT treatment, TVET treatment-UCT control, TVET control-UCT treatment, and TVET control-UCT control –

¹⁴For more details on the INVEST program, see SI Section S3.

for a factorial research design.^{15,16} Table 1 shows the sample sizes and proportions for the TVET treatment and control groups by displacement status. SI Section S4 shows the power analysis and that we are well-powered for the main hypothesis but not subgroup analyses, and SI Section S5 discusses balance.

Outcome Measures

Unlike most contact studies that measure outcomes the same day as the intervention, we conducted an endline survey after six month courses ended (2.5 months after the three month courses ended) and a second endline eight months later to measure longer-term effects.¹⁷ Following earlier studies on prejudice reduction, we only asked local participants questions on their self-reported attitudes and behavior towards the outgroup in general rather than their specific migrant classmates.

The questions, shown below, ask about locals' levels of interaction with migrants in their community outside of INVEST, general favorability towards migrants, and specific attitudes about whether migrants are more likely to support violence than locals, steal jobs from locals, or be a burden on government resources.

1. Thinking about the past six months, approximately how much interaction did you have with refugees/migrants outside of the INVEST program in your community? Never (0), A few times a year (1), Once a month (2), Once a week (3), Several times a week (4), Daily (5).

2. In general, what kind of effect do you feel that refugees/migrants have on your community? Very unfavorable (1) to Very favorable (5).

3. Some people believe that these refugees/migrants are more likely to support violence than local residents of Kandahar. Others disagree. Do you... Strongly agree (1) to Strongly disagree (5).

4. Some people believe that these refugees/migrants will take employment away

¹⁵Our Pre-Analysis Plan (PAP) was archived at (anonymized for this submission).

¹⁶SI Section S15 shows that neither UCT nor UCT-TVET treatments had any effects on attitudinal change at either endline.

¹⁷For the study timeline, see SI Section S6.

from native residents of Kandahar. Others disagree.
Do you... Strongly agree (1) to Strongly disagree (5).

5. Some people believe that these refugees/migrants will become a burden on government resources by requiring welfare assistance. Others disagree.
Do you... Strongly agree (1) to Strongly disagree (5).

In SI Section S7, we examine how well these measures correlate with each other and how stable they are across the survey waves. The first two questions – interaction and general perception – are correlated with each other, while the last three questions – violence, job competition, and resource burden – load onto the same dimension.

For the migrant participants, we did not ask about their attitudes towards locals in order to be able to ask about their displacement history and experiences, which we report in SI Section S8.

Estimation

We estimate the intention-to-treat (ITT) effects using a non-parametric analysis approach based on the difference-in-means estimator that accounts for the block randomization design. Specifically, we calculate difference-in-means estimates for a given outcome Y_i within each of the 10 blocks indexed by b , and then take their average across the blocks, weighted by block size N_b :

$$\hat{\tau} = \sum_{b \in B} \frac{N_b}{N} \left(\frac{1}{N_{T_b}} \sum_{i \in T_b} Y_i - \frac{1}{N_{C_b}} \sum_{i \in C_b} Y_i \right)$$

Similarly, standard errors are calculated as:

$$SE(\hat{\tau}) = \sqrt{\sum_{b \in B} \left(\frac{N_b}{N} \right)^2 \widehat{\text{Var}}(\hat{\tau}_b)}$$

where $\widehat{\text{Var}}(\hat{\tau}_b)$ is the estimated Neyman’s variance of the estimated treatment effect for block b , which are then summed across blocks using block-level weights $\left(\frac{N_b}{N} \right)^2$ (Gerber and Green, 2012).

Baseline Responses

Figure 2: Histograms of locals' baseline (pre-intervention) responses to migrant questions. For each question, right-most (left-most) bar indicates more inclusive (more exclusionary) response.

Ethical Considerations

Our participants, both locals and migrants, represented a vulnerable population living in wartime conditions. We therefore had an ethical responsibility to minimize any potential harm stemming from participation in INVEST while ensuring that benefits were fairly distributed. Together with Mercy Corps, which fielded INVEST in Kandahar for a year prior to our experiment, we worked to ensure that participation risks were minimized. For example, we obtained informed consent for all surveys, and individuals were given the option of ending the survey at any time. All surveys were conducted in private VTC classrooms. We did not use any deception. We organized group travel and paid transportation to ensure that participants could afford to return safely to the VTCs, and we tracked security incidents and stopped program activities in periods of heightened insecurity. We conducted interviews with participants and staff to probe for any negative experiences in the classroom and post-graduation, which we did not find. For additional details on ethical considerations, see SI section S9.

Results

At baseline, Figure 2 shows that about half of our local participants reported never interacting with migrants before INVEST. The average is several times a year (1.4 on the scale of Never (0) to Daily (5), 1.6 SD). Fig. S8 in the SI shows the distribution of baseline interaction for all local participants, by gender, ethnicity, and prior exposure to harm. There are no substantial differences across the groups. With respect to concerns about migrants, locals reported on a 5-point scale, in which 5 is most inclusive, on average 3.1 (1.2 SD) for the overall effect of migrants on the community; 3.2 (1.1 SD) for migrants supporting violence; 3.2 (1.2 SD) for employment competition; and 3.2 (1.1 SD) for resource burden. Although these averages are all at the center of the scale, the variation across these items show that locals did not simply choose the middle option because they all felt indifferent or neutral. While some hold inclusive attitudes, a substantial number hold negative baseline attitudes.

Did the INVEST vocational training program affect locals' receptivity toward migrants? From Figure 3, the effect of TVET for all local participants (black estimate), at the end of the intervention and eight months later, is null. Treated locals do not report greater interaction with migrants outside of the classroom after the program. Although not statistically significant, the estimate for disagreeing with the statement that migrants take jobs from locals is positive at Endline 1 at 0.1 (95% CI = [0, 0.3]) but it dissipates by Endline 2. At the very least, this null finding suggests that economic programs involving both locals and migrants do not increase animosity and feelings of labor market competition. In terms of the behavioral outcome of self-reported levels of interaction, there does seem to be a positive, but not statistically significant, shift for all groups by Endline 2. Across all outcomes, the effects are not only statistically null but substantively null, with point estimates near zero. Thus our findings suggest that in this context, prejudicial attitudes towards migrants are incredibly difficult to change.

We also hypothesized that locals (N = 559) who are in courses in which both groups are

Figure 3: Intention-to-Treat effects of TVET at Endline 1 (top panel, $N = 910$) and 2 (bottom panel, $N = 837$) of prejudice outcomes by locals towards migrants, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

approximately equal in number (meaning IDPs make up 45–55% of the class) will report more positive effects compared to courses in which one group outnumbers the other. Being in the majority (greater than 55%) migrant classes ($N = 375$) or minority (less than 45%) migrant classes ($N = 342$) could create a power imbalance, thereby offsetting the initial condition of equal status (Saguy, Tropp and Hawi, 2012).¹⁸ We do not find any significant effects of migrant-local balance in classrooms (red). However, locals in majority (over 55%) migrant courses reported a slightly *more unfavorable* perception of IDPs compared to the control group at Endline 1: the effect size was -0.3 (95% CI = $[-0.6, -0.01]$) on a 5-point scale. By Endline 2, there is a statistically significant

¹⁸To be clear, for these analyses, we know what course each control participant will eventually take, because they enrolled prior to randomization; thus, we are able to compare treated locals who *were* in balanced courses with control locals who *will be* in balanced courses, for example.

negative effect of attending balanced TVET courses on perceptions of migrants: a shift of -0.4 (95% CI = $[-0.7, -0.1]$) on the same 5-point scale. Nonetheless, when we adjust the p-values for the false discovery rate using the Benjamini-Hochberg procedure, SI Section S10 shows that these two results lose statistical significance.

Finally, when we explore group differences by gender, age, ethnicity, and prior exposure to harm, we also find largely null effects (SI Section S11). There are no statistically significant effects for female or male local participants. The contact literature suggests that prejudice reduction through contact programs is more likely for children and adolescents (Tropp and Prenovost, 2008; Beelmann and Heinemann, 2014; Paluck, Green and Green, 2019). Yet we do not detect effects for younger (less than 20 years old) or older (20 and greater) participants. At Endline 1, local participants who were not of Pashtun ethnicity reported much less interaction with migrants in their community outside of the program. But otherwise, there were no subgroup effects of TVET by participant characteristics at either endline. While we might expect greater empathy by those who had experienced violence prior to the intervention ($N = 193$) (Hartman and Morse, 2017), we unfortunately do not have enough power to analyze their outcomes. For unharmed individuals ($N = 1,169$), we also do not observe any statistically significant effects.

We address possible issues of noncompliance and attrition. Noncompliance refers to treatment participants not attending classes. From Table 1, 76.7% of locals assigned to TVET attended classes, so we consider noncompliance to be low. When we calculate the average treatment effects for compliers (CATE) in SI Section S12, results do not change. With respect to measurement attrition, of the local participants, 71.3% completed Endline 1, 65.6% completed Endline 2, and 51.1% completed both. In SI Section S13, we find no evidence of differential attrition by treatment status, and SI Section S14 shows that with multiple imputation, results do not change.

Understanding the Null Results

We interpret our null effects as suggesting that these economic interventions, which provide opportunities for intensive contact, are unable to change minds in active conflict settings. The incentives for holding onto prejudicial stereotypes given the stakes of making mistakes in wartime settings, such as misplaced trust across group lines, means that negative attitudes persist even with lengthy and prolonged classroom contact.

Here, we rule out several other possible explanations for our null results, including that (1) the local-IDP cleavage was not salient; (2) the program was poorly designed or poorly received by participants; (3) the program lacked institutional support from local and international stakeholders, undermining its effectiveness; and (4) it heightened economic competition between the two groups.

First, surveys of IDPs in Kandahar reported widespread feelings of social exclusion, police harassment, discrimination, and difficulty accessing public services like education and health care. These surveys also reported secondary displacement driven by persecution by locals (Internal Displacement Monitoring, 2017; Norwegian Refugee Council, 2018; Lakhani and Amiri, 2020). Figure 2 of our locals’ reported interaction levels and attitudes at baseline also confirm that many locals also view migrants as an outgroup. Our interviews with INVEST program directors revealed their concern about the “miserable conditions” faced by migrants and the widespread belief among locals that migrants had “saturated the job market and taken the jobs of Kandaharis.”¹⁹

Nor does the null finding hinge on group identifiability. While we did not directly survey them about class composition, local participants, along with VTC directors and staff, were clearly aware of migrants in the classroom. First, UNHCR and DoRR branding was highly visible at the VTCs, strongly signalling that migrants were participating. Second, local or migrant status would be

¹⁹Interview with VTC Director, December 2016.

clear through conversations among participants themselves. Given the large number of IDPs in Kandahar, along with continued dislocation from the ongoing war, the question of one's status and regional origins was a natural focal point for discussion. Unlike locals, migrants also arrived and departed daily from the VTCs in buses or minivans that shuttled them to camps and residences outside Kandahar City itself. This created a distinctive behavioral "tell" that separated migrants from locals. Finally, regional variation in dress and (Pashto) dialect offered additional clues of one's migrant status.

Second, Mercy Corps had already successfully implemented several rounds of INVEST in Kandahar and neighboring Helmand province, so staff and instructors were experienced with the programming. Post-graduation focus groups revealed that participants were highly satisfied with INVEST's design and implementation, which our low noncompliance rate supports. Indeed, participants frequently called for INVEST to be expanded to other regions of Afghanistan. None of our participants reported experiencing status discrimination by teachers, and many argued that the courses should have been longer. The "environment was very good," noted one graduate,²⁰ while another noted that "the environment was also very friendly."²¹ In short, graduates were broadly enthusiastic about INVEST: "I was satisfied with everything," one participant stated, "there was nothing objectionable in this VTC."²² Asked if she would enroll again, one graduate declared "of course, without a shadow of a doubt, I and every one of my classmates would like to take admission again."²³ Gratitude for INVEST was evident among participants: "If I hadn't learned this skill here," one individual argued, "I might be a drug addict, a hashish smoker wandering the streets aimlessly."²⁴ INVEST was not perfect, of course, and male participants in particular argued that

²⁰Male Participant #1, Red Focus Group, December 2016.

²¹Male Participant #1, Orange FG, December 2016.

²²Male Participant #6, Red FG, December 2016.

²³Female Participant #3, Yellow FG, December 2016.

²⁴Male Participant #7, Red FG, December 2016.

the courses should be even longer.

Third, there was substantial support from local authorities in addition to external actors like the UNHCR. The program design and participant recruitment involved many Afghan partners, including three provincial Departments — the DoRR, the Education Department, and MoLSAMD; village councils (shuras); local leaders; and tribal elders. Government officials also frequently visited the VTCs and attended graduation ceremonies.

Finally, it may be that the program’s focus on increasing employable skills and improving economic livelihoods elevated feelings of economic *competition* rather than cooperation in locals, offsetting any gains in positive attitudes. We do not believe this was the case. Individuals from both groups had to work together in order to complete projects and accomplish the common goal of completing the final practical exam and graduating. Indeed, we do not observe a negative effect on the belief that migrants will take jobs away from locals, nor do we see any movement in outcomes unrelated to economic concerns. Additionally, SI Section S16 tests for treatment effect heterogeneity, i.e. treated locals who experienced backlash cancelled out the positive effects of those who experienced prejudice reduction. However, we do not find evidence for such heterogeneity.

Nevertheless, we also note that the program led to modest economic gains for participants. In another study of INVEST, treated locals and IDPs were more likely to report some economic activity and cash earned in the past month by Endline 2 (Lyall, Zhou and Imai, 2020). However, they were not ultimately successful in securing jobs, given the shortage of jobs during this time in Kandahar. It is unclear how the effects of contact would be different if this program was implemented in an environment with a greater job supply.

Conclusion

Identifying the best mix of programs to reduce prejudice and conflict toward vulnerable, and growing, migrant populations around the world is a critical research question and an urgent policy problem. We draw on the example of INVEST to evaluate whether sustained intergroup contact through a widely implemented bundled economic intervention can reduce anti-migrant prejudice among locals in Kandahar, Afghanistan. Our results are sobering. Despite being a well-designed and -received program with up to six months of close local-migrant contact, INVEST did not change locals' short or long-term attitudes toward migrants. These findings echo a small set of related field experiments that find modest, if any, prejudice reduction toward out-groups despite purpose-built prejudice reduction programming (Scacco and Warren, 2018; Mousa, 2020). Taken together, evidence appears to be mounting that programmatic interventions which rely on contact alone are insufficient to budge intergroup attitudes in fragile or violent settings.

Our study does have limitations, however. We did not randomize course duration or the local-migrant balance inside the classroom. Future studies should explicitly randomize these conditions to further examine how the length of intergroup contact and the local-to-migrant distribution might affect attitudes. Since we lack behavioral measures, we also cannot rule out the possibility that locals' conduct toward migrants changed even without corresponding changes in their attitudes toward them (Paluck and Green, 2009; Mousa, 2020). Like nearly all existing studies, we only measured local attitudes, leaving us blind to whether migrant attitudes also changed. Yet local-migrant relations are two-sided in nature; future research should include the views of the powerless and marginalized to paint a more complete picture of the possibilities and limits of attitudinal change.

Our findings also raise implications for research on the contact hypothesis. Migrant status has

received far less attention in this literature than ethnicity or race. Yet it is likely that migrant status and these ascriptive identities have cross-cutting effects that might amplify, or mitigate, prejudice. Exploring attitudes and behavior toward both coethnic and non-coethnic migrants, to take one such example, remains an important task for future research. Similarly, there is pressing need to assess how direct exposure to violence, along with the perpetrator’s identity, affects intergroup relations. It is plausible that victimization hardens group boundaries, reinforcing prejudicial attitudes toward an outgroup, especially if that outgroup was blamed for the harm (Lyll, 2020). Factorial experiments could be fielded to tease apart the effects of each component of a bundled treatment on prejudice reduction. Such efforts might improve policy by steering practitioners away from individual components that are working at cross purposes to the intervention’s overall intended effects.

While these findings are discouraging, it is possible that pairing vocational training with more explicit prejudice reduction programming could yield more positive results. In this sense, INVEST was not bundled enough. Recent research on empathy-inducing interventions and interpersonal communication suggests that exchanging narratives in a non-judgmental environment can reduce prejudicial attitudes with minimal social contact (Broockman and Kalla, 2016; Kalla and Broockman, 2020). Perspective-taking, where individuals are asked to imagine themselves as refugees, can also increase empathy and inclusive attitudes (Adida, Lo and Platas, 2018). Interventions that promote shared social norms across groups can also reduce bias toward migrants (Choi, Poertner and Sambanis, 2019). Economic interventions could also be paired with collective activities, including playing team-based sports together, to break down stereotypes and reduce prejudice (Mousa, 2020; Lowe, 2021).

We conclude on a cautionary note. These light-touch interventions, however well-meaning, may prove insufficient to improve intergroup attitudes and behavior in wartime. Instead, it is likely

that sweeping structural changes are necessary to increase opportunities for meaningful contact while reducing incentives for intergroup competition and violence. In many contexts, including Afghanistan, migrants and refugees are confined to specific locations that restrict, or eliminate, opportunities for direct contact with locals. By geographically segregating them in camps or settlements outside of the city, they are also psychologically segregated from the local population, which can heighten distrust and animosity (Enos, 2017). Segregated housing, schooling, and places of worship all create barriers to positive intergroup contact that can be difficult to dismantle without wrenching structural changes (Bhavnani et al., 2014; Corno, La Ferrara and Burns, 2022). Recasting settlement patterns to encourage, or enforce, contact across group lines would be politically and economically costly. Economic reforms that boost job creation and thus dampen locals' fears of replacement by migrants might also be a bridge too far for many governments in weak states, especially those enmeshed in war. Yet without these structural changes, meaningful reduction in prejudice toward migrants and other vulnerable populations may remain out of reach in many fragile and violent settings.

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