Prolonged Social Contact With Internally Displaced Migrants Does Not Reduce Prejudice Among Locals in Wartime Settings

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Can prolonged social contact reduce local residents' prejudice toward internally displaced persons (IDPs) in fragile and violent settings? Despite record numbers of IDPs globally, there are few experimental tests of the causal effects of intergroup contact on views toward migrants, and almost none in countries experiencing active conflict. We conducted a randomized controlled trial of a vocational skills training program in Kandahar, Afghanistan, that enrolled 2,597 locals and migrants, in near equal numbers. The program offered prolonged and meaningful contact; courses lasted three or six months and emphasized soft-skills, interpersonal development, and learning a technical trade. Unlike most contact studies that measure outcomes the same day, we surveyed locals at the end of the program and again eight months later. Despite meeting the conditions for contact theory, we find no evidence of prejudice reduction toward IDPs regardless of classroom demographics or course duration.

Can interventions that promote positive social contact reduce prejudice of local residents toward internally displaced migrants in fragile and violent contexts? Some 45.7 million individuals, the highest total ever recorded, now claim status as internally displaced persons (IDPs), vastly surpassing the 26 million refugees globally [1, 2]. Given the size of this population, internal displacement is a pressing international humanitarian issue with broad consequences for local security and stability. The vast social contact literature expects that prolonged social contact between groups leads to reduced prejudice toward the outgroup. Yet randomized controlled trials testing the original contact hypothesis [3] remain rare, particularly outside of Western contexts [4–6]. Moreover, only two studies – one in Nigeria [7] and the other in Iraq [8] – have experimentally tested contact theory in violent settings. Both examined programs that aim to improve social cohesion between Christian and Muslim young men, and they found that in these high-stakes settings, intergroup contact does not reduce prejudice toward the out-group as a whole. Observational research also suggests that wartime conditions likely deter intergroup cooperation and reinforce, rather than reduce, prejudice [9].

To test contact theory with a migrant outgroup in a wartime setting, we conducted a field experiment in Kandahar, Afghanistan. We examine whether prolonged and positive social contact can lessen locals' prejudice toward migrants in a setting marked by high levels of violence, unemployment, and displacement. We define *prejudice* as a set of negative beliefs about or attitudes toward an individual based solely on membership in a particular social group or category [7]. Afghanistan offers an important example of war-induced population displacement and potential local-migrant antagonism. It has one of largest IDP populations in the world, a legacy of nearly four decades of war, political instability, drought, and other natural disasters. During late-2015 to 2016, when our study occurred, an estimated 1.35 million Afghans were internally displaced. By early 2020, that number had climbed to over 3 million. These 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 [10, 11]. Moving forward, we use the terms *IDPs*, "migrants," and "displaced persons" interchangeably to describe this population.

We evaluated the *Introducing New Vocational Education and Skills Training* (INVEST) program, run by Mercy Corps, which was designed to increase employability in Kandahar through the provision of vocational and soft-skills training among locals and IDPs [12]. We recruited 2,597 at-risk young women and men who were deemed vulnerable due to their youth, high unemployment, shared Pashtun ethnicity with the Taliban insurgency, and experience with forced displacement. Over half were IDPs living in camps or informal housing arrangements on the outskirts of Kandahar City and in three neighboring districts. We randomized participants either into taking vocational training courses that were all naturally mixed with locals and migrants, or to be put on a waitlist for these courses. Courses were run by Mercy Corps-trained staff, occurred in four Vocational Training Centers (VTCs) across Kandahar City, and took place for six hours a day, five days a week, over the duration of three or six months, depending on the course.

While much of the intergroup contact research has conceptually focused on ethnic, racial, or religious prejudices, our study highlights a largely overlooked basis of social conflict in this literature, migration status [13]. By centering migrants as a possible identity cleavage, we also engage larger literatures on the effects of forced displacement on the outbreak of new conflict [14–20] and citizen attitudes toward forced migrants settling in their country [21–23]. Hostility towards forced migrants by locals may be driven by several factors: fears that migrants are new sources of insecurity by being targeted for insurgent recruitment or committing crimes [24–28]; concerns over labor market competition [29–34]; concerns that migrants will strain government services and welfare programs [35]; and perceived cultural and social differences [36–39].

Drawing on this public opinion literature, we measured these dimensions of locals' attitudes

towards migrants as our outcomes of prejudice as well as their self-reported level of interaction with migrants outside of the course. Following contact theory [3], we hypothesized a reduction of prejudice toward migrants by locals who participated in INVEST. Specifically, among locals (N = 1,276):

> **(H1)** compared to the waitlisted (control) group, treated locals should (a) report more positive views of displaced persons at Endline 1 and Endline 2, which was conducted eight months later and (b) after the program's conclusion, report more interactions with displaced migrants in general outside of the program;
>
> **(H2)** the positive effects should be greater for locals enrolled in six month courses (N = 488), compared to those in three month courses (N = 788) given the larger dosage;
>
> **(H3)** those in more balanced classes (N = 559) in terms of the ratio of locals to migrants should see larger and longer effects, because the two groups are on equal footing;
>
> **(H4)** younger (less than age 20) participants (N = 816) will have larger and longer positive effects compared to older participants (N = 466) [5, 41, 42].

In many ways, INVEST presented an ideal test of contact theory. It met the conditions considered most amendable for prejudice reduction [3]. These are equal status in the classroom, cooperative learning with the common goal of gaining proficiency in the vocational trade, and institutional support by outside parties such as the United Nations High Commissioner for Refugees (UNHCR) and local authorities. This program was also naturalistic in two notable ways. First, participants were living in real conflict conditions, and although the main groups in conflict were not locals versus migrants, unlike lab-induced group identities, tensions between these groups existed in the real world. Thus, we can be fairly confident in the external validity of this study to similar training programs in wartime settings. Second, participants worked together in a natural classroom

setting. They were not engineered into inter-group pairings and there was no explicit programming around fostering cohesion between locals and migrants, which should therefore minimize reporting bias.

Ultimately, even given these promising conditions, we find null effects across all four hypotheses. Overall, participating in INVEST had no effects on prejudice among the local participants. These precisely-estimated null findings hold regardless of course duration, course balance, and participant characteristics like age, gender, ethnicity, and prior exposure to violence. Null effects persisted at least eight months after the program concluded. These null findings question the applicability of social contact theory and efficacy of related interventions in fragile and violent settings. Prejudice is, in a word, persistent, able to survive despite sustained intergroup contact in wartime settings.

The INVEST program

INVEST was designed to reduce unemployment in Kandahar City and three surrounding districts through the provision of vocational training and a one-time unconditional cash transfer. We 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 SI. INVEST was launched amid a backdrop of high violence, active Taliban recruitment, and a stagnant formal economy that heightened competition between locals and migrants for jobs. Kandahar was home to about 125,000 IDPs in 2015, the fourth-highest total among Afghanistan's 34 provinces. Kandahar City was by far the most popular destination within the province, reflecting a broader pattern of rural to urban forced migration due to wartime pressure [43].

Prior to randomization into TVET, all participants chose to enroll in either three or six month courses at one of the four local vocational training centers (VTCs): Mirwais Mina (male only), Sofi Saheb (male only), Mahmood Tarzai (female only), and Aino Mina (mixed). Participants

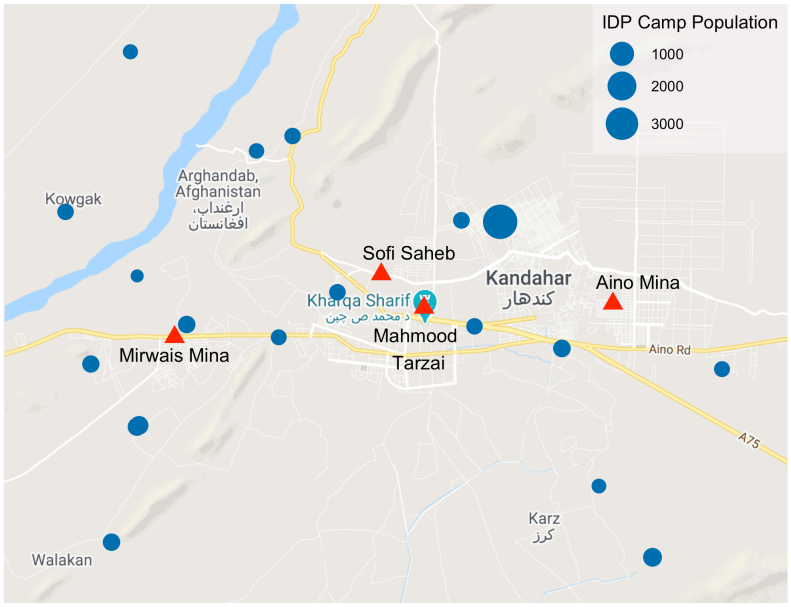

Figure 1: Map of Kandahar City showing the locations of the four VTCs in red, and surrounding IDP camp locations in blue, weighted by their population from January 2014 to March 2016. Source for IDP data: IDMC's Global Internal Displacement Database. Google basemap.

were assigned to a single VTC for the program's duration based on gender and proximity to their homes. Participants commuted up to 45 minutes daily between their homes and the assigned VTC. Fig. 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. For the full list of courses, see Section S2 in the SI. Students were also enrolled in a soft-skills course designed to bolster business skills such as time-management and networking. Finally, participants who successfully completed the courses were provided with a small start-up kit of trade-specific tools upon graduation. Mercy Corps estimated the total cost at about US$229 (15,600 Afghanis) per individual in 2016 (excluding fixed costs such as renting training facilities, main office expenses, and security).

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 Mercy Corps, three provincial Departments – the Department of

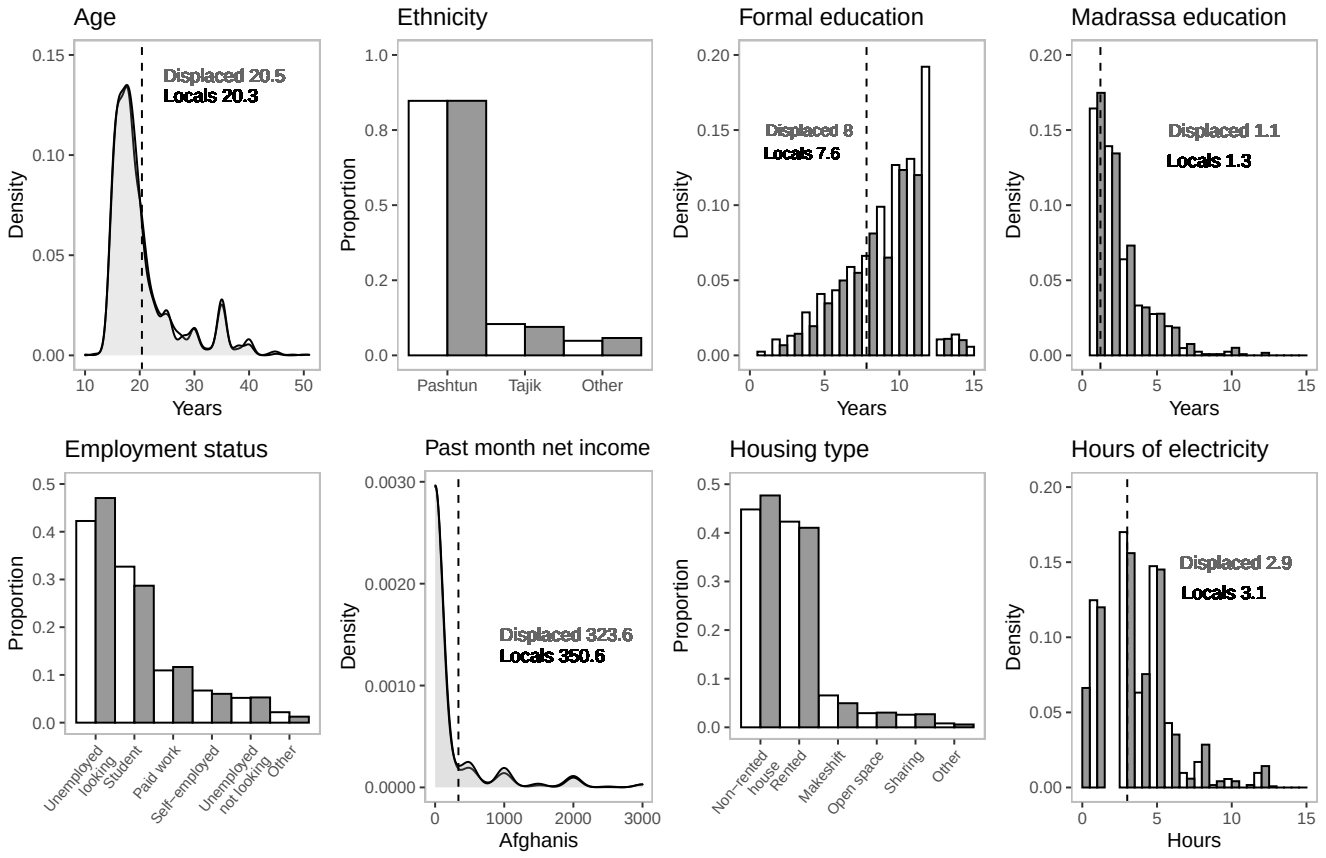

Figure 2: Participant demographics at baseline, grouped by displaced (white) and locals (gray).

Refugees and Repatriation (DoRR), the Education Department, and Ministry of Labour, Social Affairs, Martyrs and Disabled (MoLSAMD) — the UNHCR, village councils (*shuras*), local leaders, and tribal elders. 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 the Nigeria [7] and Iraq [8] studies which recruited men only, women comprise 36.2% of our sample (n=940). Only 28% were employed at baseline, 77.6% of the sample reported earning no income at all during the past month. And of those who did report a net income, the average amount was only about US$20 (1409 Afghanis).

Fig. 2 shows demographic summary statistics grouped by displacement status. Generally, displaced participants were similar to locals, particularly in terms of age and ethnic group. Section

S4 of the SI gives an overview of the displacement history among our displaced participants. The majority at 58% were displaced due to the Taliban, as opposed to government violence or natural disasters. Approximately two-thirds were displaced within Afghanistan, as opposed to being refugees in Pakistan and then returning.

All courses were naturally mixed in local-migrant composition. Section S3 in the SI shows the proportion of displaced participants within each VTC-classroom. Across all the classes, they range from 34.3% to 66.3%. Thus, there were no classes in which locals vastly outnumbered or were vastly outnumbered by displaced participants. Participants had formal equality in the classroom setting — all were enrolled by lottery, with similar SES backgrounds — and all courses used cooperative role-playing to transfer skills, thus increasing intergroup interaction. Similar to the vocational training intervention in the Nigeria study [7], since INVEST focused on skills training and not prejudice-reduction, this study avoids the problem of self-selection based on prior levels of prejudice. For the same reason, we are less concerned with social desirability and other reporting bias.

INVEST's design created a naturalistic setting ideal for studying prejudice reduction in three ways. First, locals and migrants had a high degree of exposure to one another while on an equal classroom footing. Participants met for six hours per days, five days a week, for either three or six months, depending on their course. Participants therefore spent between 360 and 720 hours together. This compares favorably with existing studies in Nigeria and Iraq [7, 8], where participants had only 64 or 26 hours with members of the outgroup, respectively. Second, our sample was far larger than either the Nigeria (N=849) and Iraq (N=677) studies. Finally, migrants represented a large share of participants in the classroom. Psychological theories anticipate that when outgroup members are viewed as not exceptional "tokens," then individuals are more likely to generalize their positive views to the entire group [44]. INVEST thus combined a high degree

of daily interaction with prolonged exposure in classroom settings marked by formal equality and a rough balance between migrants and locals.

Moreover, post-graduation focus groups revealed that participants were highly satisfied with INVEST's design and implementation. Indeed, participants frequently called for INVEST to be expanded to other regions of Afghanistan. None of our participants reported experiencing ethnic or status discrimination by teachers; the "environment was very good," noted one graduate (Male Participant #1, Red Focus Group, December 2016), while another noted that "the environment was also very friendly and alluring" (Male Participant #1, Orange FG, December 2016). Graduates were broadly enthusiastic about INVEST: "I was satisfied with everything," one participant stated, "there was nothing objectionable in this VTC" (Male Participant #6, Red FG, December 2016). 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" (Female Participant #3, Yellow FG, December 2016). 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" (Male Participant #7, Red FG, December 2016). INVEST was not perfect, of course, and male participants in particular argued that the courses should be even longer. Yet their enthusiasm is remarkable given how few participants actually found jobs or started businesses after they graduated (Male Participant #1, Orange FG, December 2016).

It is also worth emphasizing the real-world nature of anti-migrant sentiment outside the INVEST classroom. Surveys among IDPs in Kandahar reported widespread feelings of social exclusion, police harassment, and difficulty accessing public services like education and health care. Migrants were also victimized by locals. As one 35-year old Pashtun migrant recounted: "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

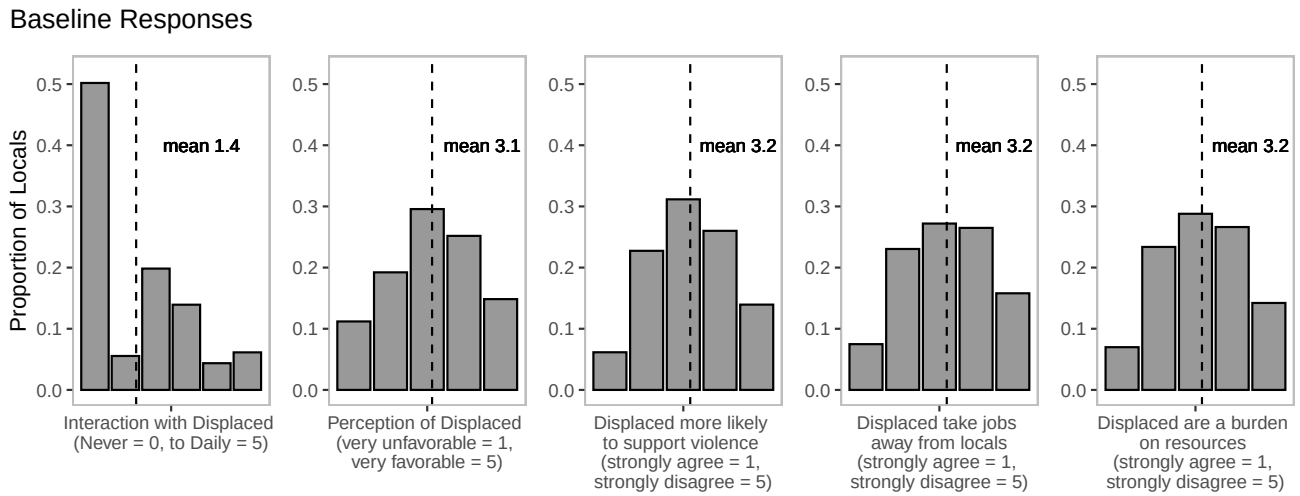

Figure 3: Histograms of locals' baseline (pre-intervention) responses to prejudice questions. For each question, right-most (left-most) bar indicates more inclusive (more exclusionary) response.

able to get help from the police to complain" [45, p. 14]. A 2018 survey found that 72 percent of migrants in Kandahar had been displaced from their homes twice; almost a third had been displaced three times or more [46]. 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" (VTC Director, December 2016).

Results

At Baseline, Fig. 3 shows that about half of our local participants reported never interacting with displaced persons. The average is several times a year (1.4 on the scale of Never (0) to Daily (5), 1.6 SD). Fig. S4 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 prejudicial attitudes, 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 displaced people on the community; 3.2 (1.1 SD) for displaced people supporting violence; 3.2 (1.2 SD) for employment competition;

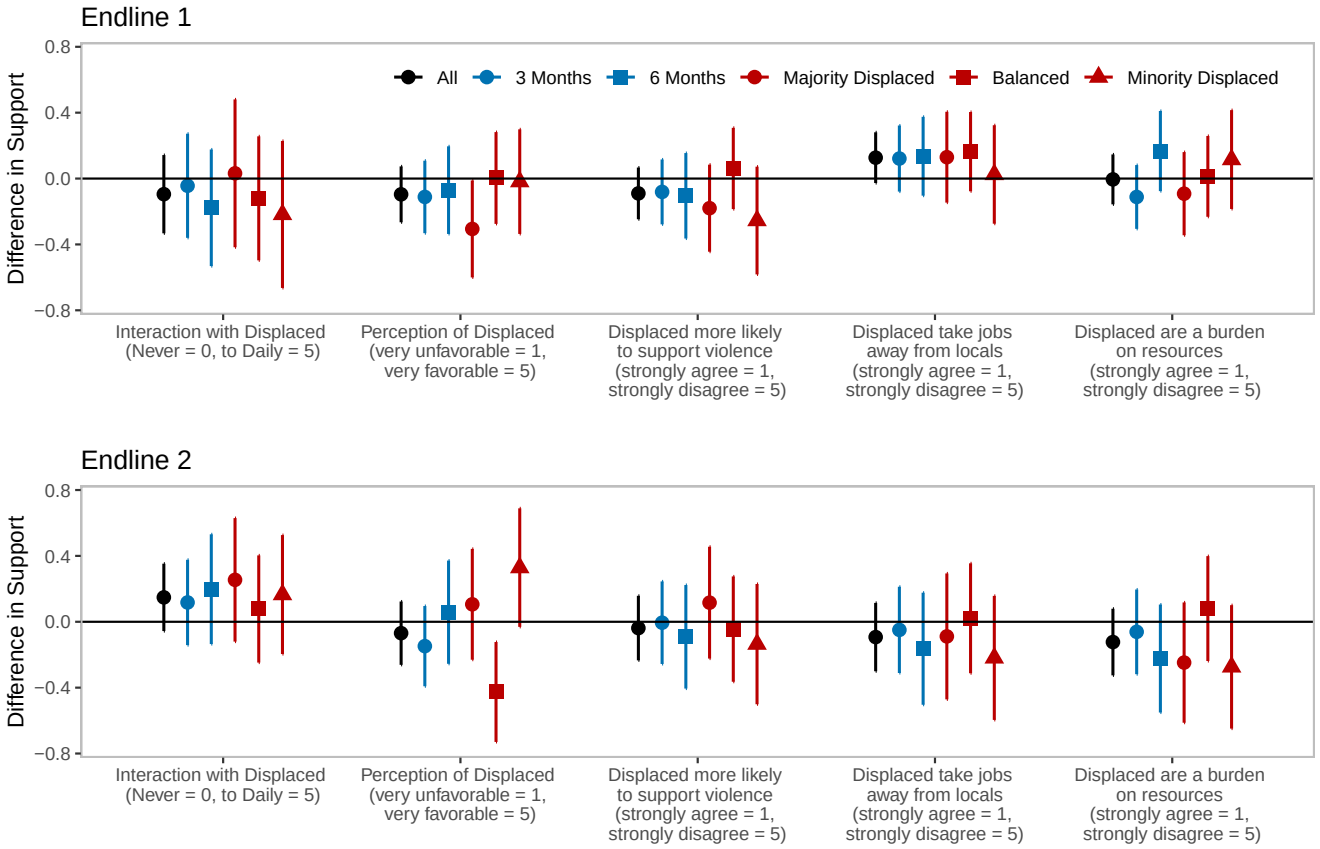

Figure 4: Intention-to-Treat effects of TVET at Endline 1 (top panel) and 2 (bottom panel) of prejudice outcomes by locals towards displaced, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

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 are prejudicial.

Did intergroup contact through the INVEST program affect prejudice toward IDPs? From Fig. 4, 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, 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. Next, taking into account course duration, when we examine three versus six month courses (blue), there are also no statistically significant effects.

We also hypothesized that locals (N = 559) who are in courses in which both groups are 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%) displaced classes (N = 375) or minority (less than 45%) displaced classes (N = 342) could create a power imbalance, thereby offsetting the initial condition of equal status [47]. To 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.

We do not find any significant effects of migrant-local balance in classrooms. Turning to the heterogeneous effects by course composition (red), again the effects were generally null. However, locals in majority (over 55%) displaced 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 negative effect of attending balanced TVET courses on perceptions of displaced people: 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, Section S7 in the SI 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. Shown in Section S8 of the SI, there are no statistically significant effects for female or male local participants. The literature suggests that prejudice reduction through contact programs is more likely for children and adolescents [5, 41, 42]. 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 displaced people in their community. 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) [48], 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.

Discussion

We find no effects of attitude change, despite a well-designed and well-liked program that offered locals prolonged and meaningful exposure to migrants. We note that while our study's design, population, and setting were notably different from the Nigeria and Iraq studies [7, 8], our findings are largely in line with theirs. This implies that in wartime conditions, prejudicial beliefs may be too difficult to budge. The presence of migrants is often associated with concerns of labor market competition and welfare burden, fear of crime or additional violence, and these concerns likely harden in insecure contexts. The incentives for motivated reasoning given the stakes of making mistakes in wartime setting, such as misplaced trust across group lines, means that prejudicial attitudes persist even with lengthy and prolonged classroom contact. Wartime migrants might prove a particularly unsympathetic category for prejudice reduction, suggesting that some types of prejudice are especially persistent. Additionally, our research suggests that migrant status, once established as a relevant identity category, may be just as powerful as ethnicity, religion, and other categories of social difference.

We acknowledge several limitations of our study that present avenues for future research. First, we did not randomize course duration or composition. Future studies should explicitly manipulate

these conditions. Second, INVEST did not have an explicit module aimed at prejudice reduction. Given this context, it is possible that in order to change beliefs, a similar intervention would need an explicit anti-prejudice programming component. For instance, recent research on interpersonal communication suggests that exchanging narratives in a non-judgmental environment, plus high-quality listening, can reduce prejudicial attitudes toward immigrants and transgender individuals with no or only minimal social contact [49–51]. A study on perspective-taking, which asked citizens to imagine themselves as a hypothetical refugee, also increased empathy and inclusion [21]. Future research should experiment with combining contact with these types of perspective-giving and -taking interventions within (and outside) conflict settings.

Our findings also suggest several avenues of research for interventions in conflict settings. Scholars could explore the cross-cutting nature of IDP status and coethnicity, for example. Perhaps social distance mediates the effects of group contact; coethnic migrants might be viewed more favorably than non-coethnics. Similarly, there is pressing need to develop behavioral measures for prejudice in these environments. It is possible that behavioral changes toward migrants might occur without corresponding changes in attitudes due to broader shifts in societal norms and expectations [7, 8, 21, 52]. We lacked the statistical power to examine how exposure to violence — as well as the perpetrator's identity — affected prejudice. It is plausible to expect that victimization might harden group boundaries, reinforcing prejudicial attitudes toward an out-group, especially if that out-group was blamed for the harm. Finally, local-migrant relations are two-sided in nature. Future studies should focus on the views of the powerless and marginalized toward local populations, not simply treat migrants as the target of prejudice. In doing so, this research can paint a more complete picture of the possibilities and limits of prejudice reduction in wartime and fragile settings.

	TVET treatment			TVET control			Totals	
	size (n)	size (%)	compliance (%)	size (n)	size (%)	compliance (%)	size (n)	size (%)
Displaced	667	25.7	80.2	654	25.2	100	1321	50.9
Locals	631	24.3	76.7	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 attending (not attending) at least one class of the program.

Methods

This research was approved by the Institutional Review Boards of XXX University and YYY University. Informed voluntary consent was obtained from all human participants. Participants were compensated for their travel costs to the VTCs. We used an α level of 0.05 for all statistical tests. All test statistics were two-sided.

Experimental procedure. 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. In addition, we blocked on gender for the Aino Mina VTC. This yielded 10 blocks in total. We then created four groups within each block matching on employment status, displacement status, and exposure to violence during the past year using Mahalanobis distances. These variables were collected on the pre-baseline 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 – TVET treatment-UCT treatment, TVET treatment-UCT control, TVET control-UCT treatment, and TVET control-UCT control – for a factorial research design [12]. Section S11 in the SI shows that neither UCT nor UCT-TVET treatments had any effects on prejudice at either endline. Table 1 shows the sample sizes and proportions for the TVET treatment and control groups by

displacement status, and Section S5 in the SI shows balance.

Measurement. We conducted two endline surveys. The first occurred shortly after the conclusion of the INVEST program. Then we returned at the eight month mark to measure longer-term effects. Data were collected via individual face-to-face interviews at the VTCs with enumerators using tablets and smartphones running Open Data Kit (ODK).

To measure prejudice, we asked the following survey questions to our local participants (N = 1276) about their levels of interaction with and attitudes toward displaced people in general:

1. Thinking about the past six months, approximately how much interaction did you have with refugees/displaced people 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/displaced people have on your community? Very unfavorable (1) to Very favorable (5).

3. Some people believe that these refugees/displaced people 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/displaced people will take employment away from native residents of Kandahar. Others disagree. Do you... Strongly agree (1) to Strongly disagree (5).

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

Note that the first outcome is self-reported behavioral, while the others are attitudinal.

Estimation strategy. We estimate the intention-to-treat (ITT) effects using a non-parametric analysis approach based on the difference-in-means estimator, while taking into account 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:

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

Treatment compliance. Noncompliance refers to participants assigned to receive TVET not attending a single course. From Table 1, 76.7% of locals assigned to TVET attended at least one class. We conduct an instrumental variables analysis using the Neyman stratification method by calculating each component of the standard Wald estimator as a weighted average across the blocks in order to identify average treatment effects for compliers (CATE) [53]. Section S9 in the SI shows that our results do not substantively change.

Attrition. Of the 1,276 local participants, 910 (71.3%) completed Endline 1, 837 (65.6%) completed Endline 2, and 652 (51.1%) completed both. To prevent attrition at both endlines, we sent frequent SMS messages and reimbursements of US$5 to offset the transportation costs of returning to the VTCs for each endline. Mercy Corps staff and recent INVEST graduates also reached out to encourage participants to return for the endline surveys. In Section S6 of the SI, we diagnose attrition by first comparing attrition patterns among local participants across treatment arms. Using a linear regression of an attrition indicator at Endline 2 on treatment, baseline covariates, and treatment-covariate interactions, we perform a heteroskedasticity-robust F-test of the hypothesis that all the interaction coefficients are zero. For this test, p-values below 0.05 are considered evidence of asymmetrical attrition. The p-value for our test is 0.16, thus we

cannot reject the null hypothesis that there was no uneven attrition by treatment arm. Second, we assess whether and how the participants at Endline 1 and 2 differ.we note that the balance between participants who came to Endline 2 and those who attrited remains high across the baseline covariates. Nevertheless, given slight differences between the two groups, we use the R package *mi* [54] for multiple imputation to address concerns regarding attrition and nonresponse at Endline 1 and/or 2 [55]. Simple two-sided t-tests show that we cannot reject the null hypotheses that locals assigned to TVET are no more or less likely to attrit than those assigned to control, with p-value 0.58. Section S10 in the SI shows that our results do not substantively change.

Research Ethics. Our participants, both locals and migrants, represented a vulnerable population trapped amidst an ongoing conflict. 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 judged participation risks to be negligible. Informed consent was obtained for all surveys; individuals were given the option of ending the survey at any time. We explicitly stated in our consent form that individuals were not eligible for a second round of INVEST regardless of their answers. All surveys were conducted in private VTC classrooms. No deception was used. We organized group travel and paid transportation to ensure that participants could afford to return safely to the VTCs. We also fed our respondents as partial compensation for their time. We conducted focus groups with participants and interviews with INVEST directors and staff to probe for any negative experiences in the classroom and post-graduation. Individuals were de-identified to protect their anonymity; all data was encrypted and stored offsite. We tracked security incidents in Kandahar City and its environs; training was stopped twice during security operations by the government and after two additional Taliban attacks to minimize risk to participants as they traveled to the VTCs. Finally, given our waitlist design, individuals who

were randomly assigned to the control group were enrolled in the next round of INVEST.

Data Availability

De-identified data files necessary to replicate the results in this article, specifically Figs. 1–4 and Table 1 will be available at the following Dataverse repository: (link upon publication)

Code Availability

Code necessary to replicate the results in this article will be available at the following Dataverse repository: (link upon publication)

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Competing Interests

The authors declare no competing interests.

Additional Information

A pre-analysis plan for this study was archived in the Evidence in Governance and Politics (EGAP) repository.

Prolonged Social Contact With Internally Displaced Migrants Does Not Reduce Prejudice Among Locals in Wartime Settings*

For Online Publication: Supplementary Information

Contents

*All replication material, including R code and data, will be made available via Harvard University's Dataverse.

S1 Study Timeline

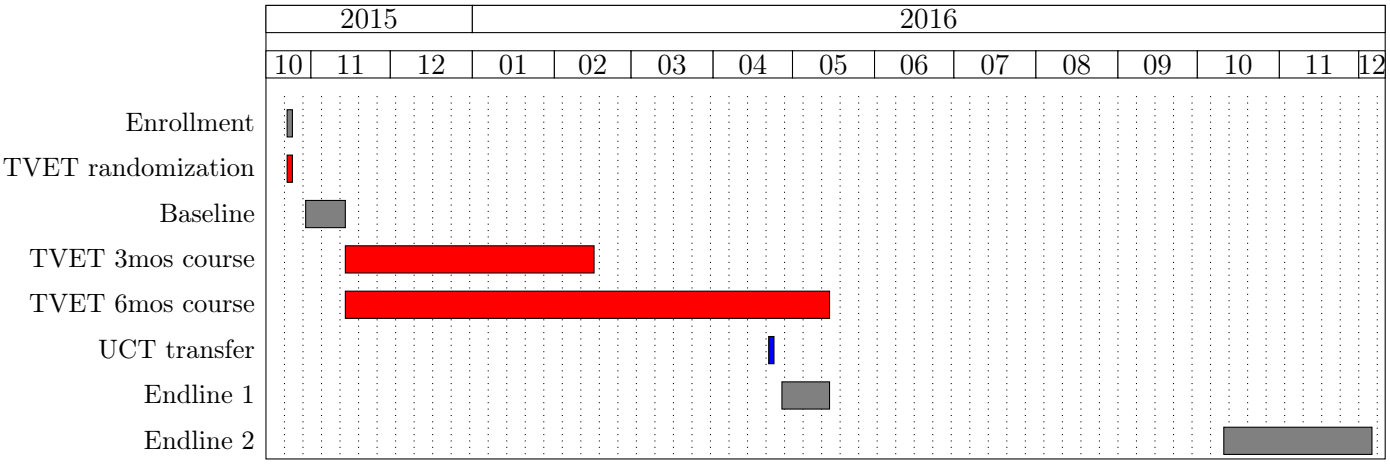
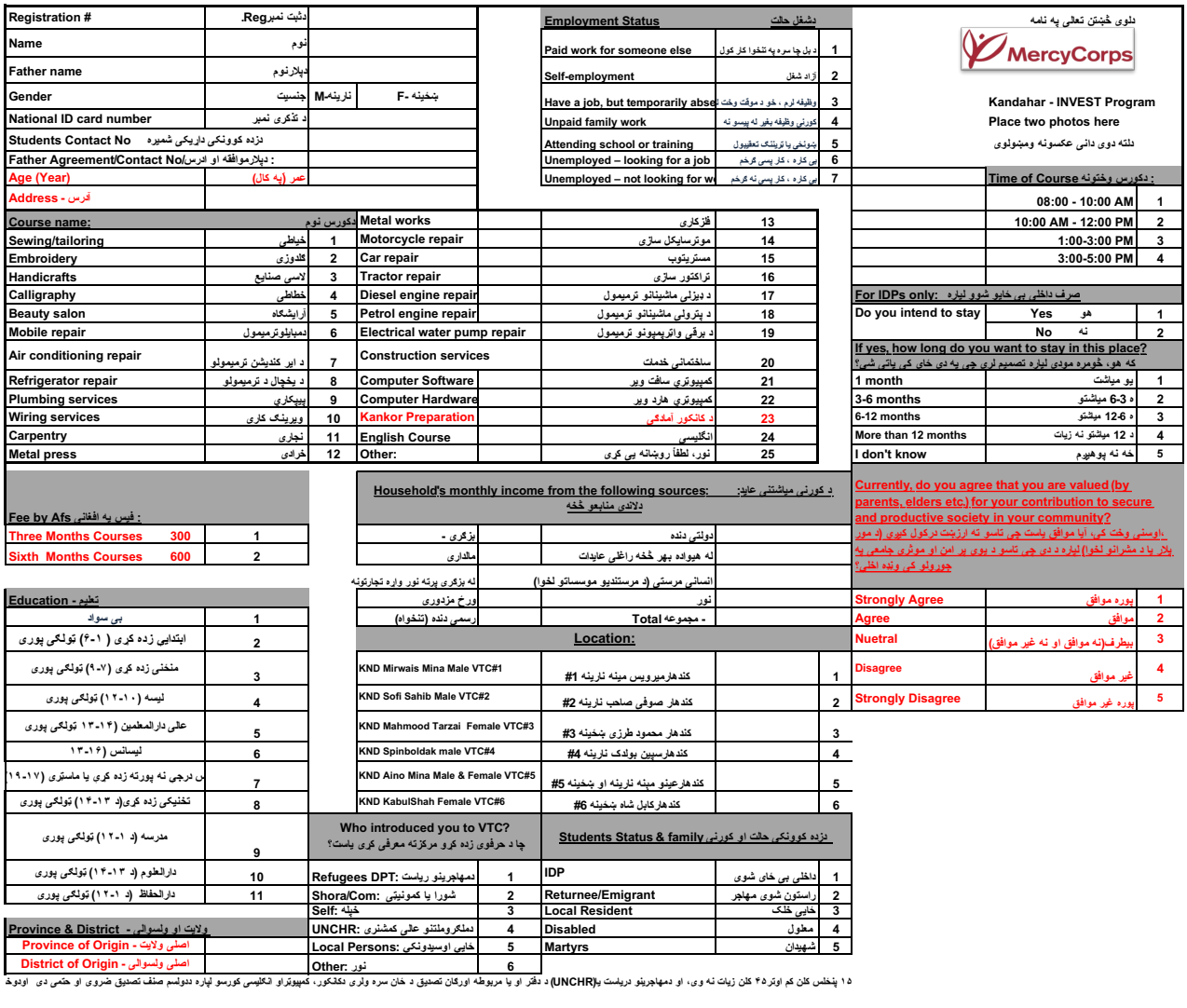


Figure S1: INVEST study timeline

S2 Program Enrollment Form and Course List

Below is the enrollment form for the INVEST program which all participants filled out prior to taking the baseline survey and treatment assignment. This form asks about pre-treatment employment, household net income, education, and displacement status. It also asks for the participant's VTC and preferred TVET course. All TVET courses are listed in this form.



Registration #	دثبت نمبرReg.	
Name	نوم	
Father name	دپلارنوم	
Gender	جنسیت	نارینه-M / بڅینه -F
National ID card number	د تذکرې نمبر	
Students Contact No	نزده کوونکی داریکی شمیره	
Father Agreement/Contact No	دپلارموافقه او ادرس :	
Age (Year)	عمر (په کال)	
Address -	آدرس	

Course name:	دکورس نوم	
Sewing/tailoring	خیاطی	1
Embroidery	گلدوزی	2
Handicrafts	لاسی صنایع	3
Calligraphy	خطاطی	4
Beauty salon	آرایشگاه	5
Mobile repair	دموبایلوترمیمول	6
Air conditioning repair	د اير کنديشن ترميمولو	7
Refrigerator repair	د يخچال د ترميمولو	8
Plumbing services	پيپکاري	9
Wiring services	ويرينگ کاري	10
Carpentry	نجاري	11
Metal press	خرادي	12

Fee by Afs : فيس په افغاني		
Three Months Courses	300	1
Sixth Months Courses	600	2

Education - تعليم	
بي سواد	1
ابتدايي زده کړی (۱-۶) ټولګی پوری	2
منځنی زده کړی (۷-۹) ټولګی پوری	3
ليسه (۱۰-۱۲) ټولګی پوری	4
عالی دارالمعلمين (۱۳-۱۴ ټولګی پوری	5
ليسانس (۱۳-۱۶	6
ماستری (۱۷-۱۹ درجی نه پورته زده کړی	7
تخنيکی زده کړی(۱۳-۱۴) ټولګی پوری	8
مدرسه (۱-۱۲ د) ټولګی پوری	9
دارالعلوم (۱۳-۱۴ د) ټولګی پوری	10
دارالحفاظ (۱-۱۲ د) ټولګی پوری	11

Province & District - ولايت او ولسوالي	
Province of Origin - اصلی ولايت	
District of Origin - اصلی ولسوالی	

Household's monthly income from the following sources: د کورنۍ میاشتنۍ عاید: دلاندې منابعو څخه			
بزګری -		دولتي دنده	
مالداری		له هيواده بهر څخه راغلی عايدات	
له بزګری پرته نور واره تجارتونه		انساني مرستې (د مرستندويو موسساتو لخوا)	
ورځ مزدوری		نور	
رسمي دنده (تنخواه)		Total - مجموعه	

Location:		
KND Mirwais Mina Male VTC#1	کندهارميرويس مينه نارينه #1	1
KND Sofi Sahib Male VTC#2	کندهار صوفي صاحب نارينه #2	2
KND Mahmood Tarzai Female VTC#3	کندهار محمود طرزی بڅينه #3	3
KND Spinboldak male VTC#4	کندهارسپين بولدک نارينه #4	4
KND Aino Mina Male & Female VTC#5	کندهارعينو مينه نارينه او بڅينه #5	5
KND KabulShah Female VTC#6	کندهارکابل شاه بڅينه #6	6

Who introduced you to VTC? چا د حرفوی زده کړو مرکزته معرفی کړی ياست؟	
Refugees DPT: دمهاجرينو رياست	1
Shora/Com: شورا يا کمونيټی	2
Self: خپله	3
UNCHR: دملګروملتنو عالی کمشنری	4
Local Persons: خاپی اوسيدونکی	5
Other: نور	6

Students Status & family نزده کوونکی حالت او کورنی		
IDP	داخلي بې خای شوی	1
Returnee/Emigrant	راستون شوی مهاجر	2
Local Resident	خايی خلک	3
Disabled	معلول	4
Martyrs	شهيدان	5

Employment Status دشغل حالت		
Paid work for someone else	د بل چا سره په تنخوا کار کول	1
Self-employment	آزاد شغل	2
Have a job, but temporarily absent	وظيفه لرم ، خو د موقت وخت	3
Unpaid family work	کورنی وظيفه بغير له پيسو نه	4
Attending school or training	بڅونځی يا تربيتی تعقيبول	5
Unemployed – looking for a job	بی کاره ، کار پسی گرځم	6
Unemployed – not looking for w	بی کاره ، کار پسی نه گرځم	7

Metal works	فلزکاری	13
Motorcycle repair	موترسايکل سازی	14
Car repair	مسترينوب	15
Tractor repair	تراکتور سازی	16
Diesel engine repair	د ډيزلی ماشينانو ترميمول	17
Petrol engine repair	د پترولی ماشينانو ترميمول	18
Electrical water pump repair	د برقی واټرپمپونو ترميمول	19
Construction services	ساختماني خدمات	20
Computer Software	کمپيوټری سافټ ويئر	21
Computer Hardware	کمپيوټری هارډ ويئر	22
Kankor Preparation	د کانکور آمادګی	23
English Course	انګليسی	24
Other:	نور، لطفاً روښانه يی کړی	25

دلوی څېښتن تعالی په نامه

MercyCorps

Kandahar - INVEST Program

Place two photos here

دلته دوی دائی عکسونه ومښلوی

Time of Course دکورس وختونه :	
08:00 - 10:00 AM	1
10:00 AM - 12:00 PM	2
1:00-3:00 PM	3
3:00-5:00 PM	4

For IDPs only: صرف داخلي بې خايو شوو لپاره		
Do you intend to stay	Yes هو	1
	No نه	2

If yes, how long do you want to stay in this place? که هو، څومره مودی لپاره تصميم لری چی په دی خای کی پاتی شی؟		
1 month	يو مياشت	1
3-6 months	3-6 مياشتو	2
6-12 months	6-12 مياشتو	3
More than 12 months	د 12 مياشتو نه زيات	4
I don't know	خه نه پوهيږم	5

Currently, do you agree that you are valued (by parents, elders etc) for your contribution to secure and productive society in your community? اوسنی وخت کی، آيا موافق ياست چی تاسو ته ارزښت درکول کيږی (د مور پلار يا د مشرانو لخوا) لپاره د دی چی تاسو د يوی پر امن او موثری جامعی په جوړولو کی ونده اخلئ؟		
Strongly Agree	پوره موافق	1
Agree	موافق	2
Nuetral	بيطرف(نه موافق او نه غير موافق)	3
Disagree	غير موافق	4
Strongly Disagree	پوره غير موافق	5

۱۵ پخلس کلن کم اوتر ۴۵ کلن زيات نه وی، او دمهاجرينو درياست (UNCHR) د دفتر او يا مربوطه اورګان تصديق د خان سره واری نکاتکور، کمپيوټراو انګليسی کورسو لپاره دتولسم صنف تصديق ضروی او حتمی دی اودوهـ

S3 Classroom Composition: Proportion of Displaced Participants by Course

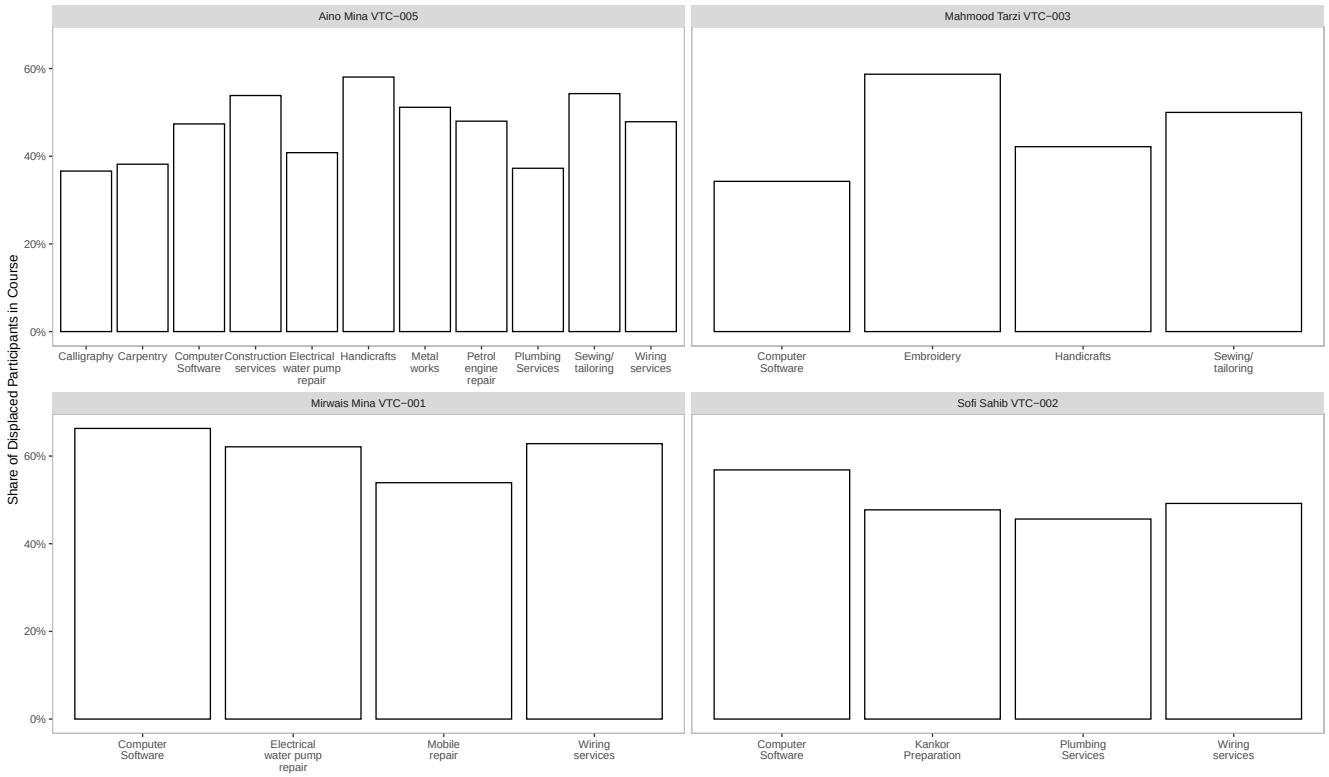


Figure S2: Proportion of displaced participants by course across the four VTCs.

This section shows the proportion of displaced participants within each VTC-course. Across all the courses, they range from 34.27% (Computer Software at Mahmood Tarzi VTC-003) to 66.3% (Computer Software at Mirwais Mina VTC-001). While there were no classes in which locals vastly outnumbered or were outnumbered by displaced participants, we examine subgroup treatment effects of TVET courses in which the displaced were a minority (less than 45%), a majority (more than 55%), or balanced (between 45–55%) relative to locals.

S4 History of Displacement and Pre-Intervention Levels of Interaction

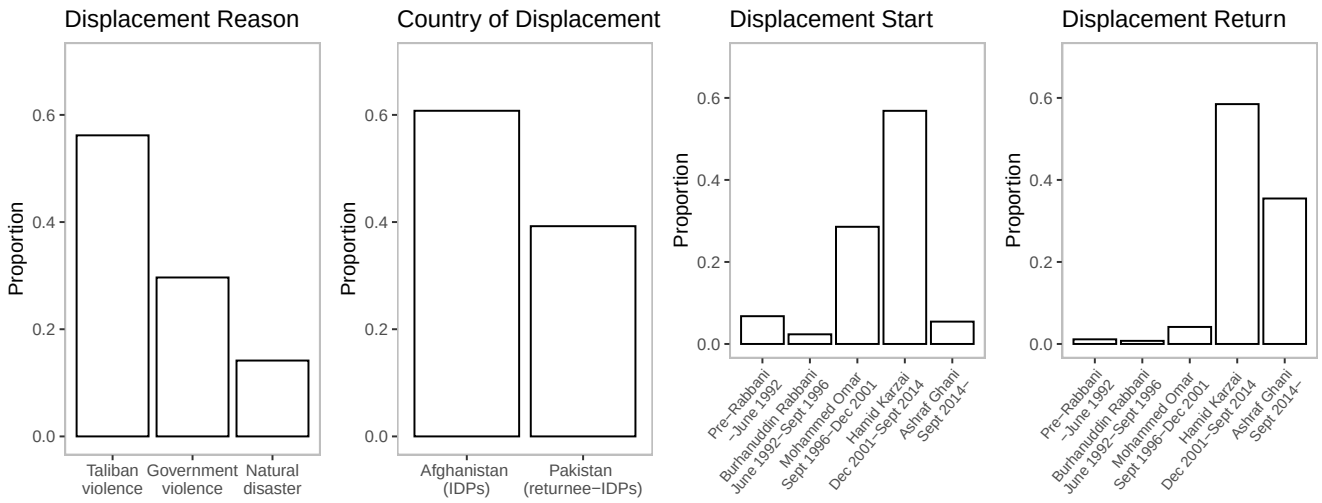


Figure S3: Displacement history among the displaced participants.

Figure S3 gives an overview of the displacement history among our displaced participants. The majority at 58% were displaced due to the Taliban. Approximately two-thirds were displaced within Afghanistan, as opposed to being refugees in Pakistan and then returning. Additionally, most at 56.8% were displaced sometime during Hamid Karzai's presidency from December 2001 to September 2014.

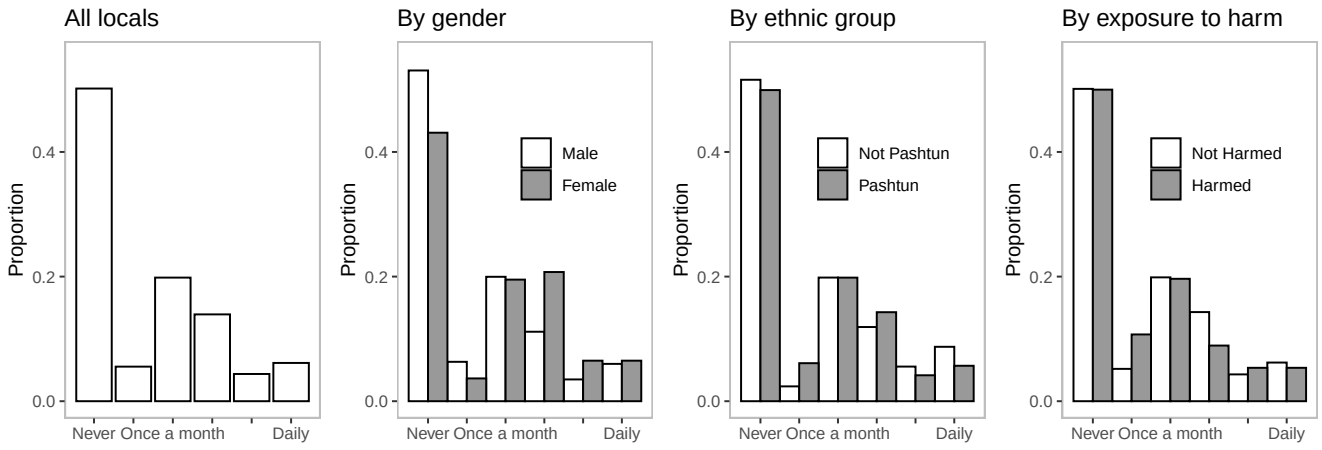


Figure S4: Among local participants, frequency of interaction with displaced people at baseline.

Figure S4 shows how frequently local participants of INVEST interacted with internally displaced people generally before the intervention (reported at baseline). Approximately half reported that they had never interacted with IDPs. While local women were slightly more likely than local men to report some interaction, there are no substantial differences among locals by ethnic group or prior exposure to harm.

S5 Balance By Treatment Group and Displacement Status

Baseline covariate	TVET Control-Displaced Mean	TVET Treatment-Displaced Difference	p-value	TVET Control-Local Mean	TVET Treatment-Local Difference	p-value
Age (years)	20.12	0.69	0.03	20.41	-0.15	0.63
Pashtun ethnicity	0.84	-0.00	0.95	0.84	0.02	0.29
Household Head	0.12	0.02	0.39	0.13	-0.02	0.25
Household Size	11.84	-0.07	0.83	11.37	-0.56	0.07
Married	0.25	0.01	0.58	0.27	-0.02	0.40
Formal Education (years)	7.94	-0.06	0.77	7.52	0.01	0.95
Madrassa Education (years)	1.09	0.08	0.44	1.29	0.00	0.98
Student	0.31	0.02	0.44	0.26	0.04	0.07
Paid Work	0.11	-0.01	0.71	0.13	-0.02	0.33
Employed	0.31	0.01	0.69	0.28	0.01	0.78
Not-rented House	0.44	0.01	0.83	0.46	0.02	0.47
Rented House	0.44	-0.04	0.20	0.43	-0.03	0.27
Electricity (hours)	2.87	0.11	0.37	3.10	-0.06	0.63
Past Month Profit (Afghanis)	319.86	1.90	0.98	322.66	52.58	0.46
No Land	0.52	0.02	0.53	0.60	-0.00	0.85
Personal Assets (5 items)	1.54	0.11	0.04	1.58	0.07	0.15
Livestock (6 items)	0.49	-0.02	0.72	0.38	0.01	0.75
Household Assets (12 items)	6.23	-0.02	0.86	6.49	0.10	0.47

Table S1: Baseline Covariate Summary Statistics and Balance Test by Treatment Group and Displacement Status

The control group means (columns 1 and 4) and the difference of the treatment group means from the control group means (columns 2 and 5) as well as the standard errors are calculated within the 10 blocks and then averaged across blocks, weighted by block size. We then obtain the t-statistic using the difference in means and standard errors and report the p-value (columns 3 and 6). Unless otherwise specified, covariates are binary. The results suggest that the treatment and control groups are balanced well. As expected, Kolmogorov-Smirnov tests (KS tests) shows that the distribution of p-values cannot be distinguished from the uniform distribution, which is what we would expect if the randomization is properly conducted: the p-value of the KS test for column 3 is 0.38, and for column 6 is 0.82.

S6 Diagnosing Attrition: Comparing Endline 2 Participants and Endline 1 Participants who Attrited at Endline 2

To diagnose attrition, we first compare attrition patterns among local participants across treatment arms. Using a linear regression of an attrition indicator at Endline 2 on treatment, baseline covariates, and treatment-covariate interactions, we perform a heteroskedasticity-robust F-test of the hypothesis that all the interaction coefficients are zero. For this test, p-values below 0.05 are considered evidence of asymmetrical attrition. The p-value for our test is 0.16, thus we cannot reject the null hypothesis that there was no uneven attrition by treatment arm.

Second, we assess whether and how the participants at Endline 1 and 2 differ. The means for the Endline 2 respondent group (column 1 for displaced participants and column 4 for local participants) and the difference of the attrition group means from the Endline 2 group means (columns 2 and 5) are calculated without respect to the blocks unlike in table S1, since only treatment was assigned with respect to the blocks. We run a simple two-sided t-test and report the p-values (columns 3 and 6). Unless otherwise specified, covariates are binary.

Baseline covariate	Endline 2 Displaced Mean	Attrition Group Displaced Difference	p-value	Endline 2 Local Mean	Attrition Group Local Difference	p-value
TVET treatment	0.52	-0.03	0.45	0.50	-0.02	0.58
Age (years)	20.55	-0.16	0.68	20.58	-0.78	0.04
Pashtun ethnicity	0.83	0.05	0.04	0.85	-0.01	0.78
Household Head	0.14	-0.01	0.76	0.12	-0.02	0.27
Household Size	11.81	0.34	0.41	11.17	-0.60	0.06
Married	0.26	-0.03	0.26	0.27	-0.04	0.18
Formal Education (years)	7.81	0.86	0.00	7.44	0.56	0.08
Madrassa Education (years)	1.13	0.02	0.85	1.27	0.01	0.93
Student	0.31	0.07	0.03	0.26	0.09	0.01
Paid Work	0.11	0.01	0.78	0.13	-0.03	0.19
Employed	0.28	0.07	0.08	0.26	0.02	0.60
Not-rented House	0.43	0.02	0.53	0.46	0.05	0.19
Rented House	0.43	-0.03	0.38	0.42	-0.03	0.44
Electricity (hours)	2.96	0.01	0.93	2.96	0.37	0.03
Past Month Profit (Afghanis)	270.90	178.92	0.10	345.35	-20.25	0.80
No Land	0.52	-0.01	0.77	0.61	-0.04	0.22
Personal Assets (5 items)	1.56	0.21	0.00	1.56	0.24	0.00
Livestock (6 items)	0.45	0.09	0.13	0.37	0.07	0.23
Household Assets (12 items)	6.20	0.15	0.33	6.57	-0.08	0.64

Table S2: Balance Tests comparing Attrition and Endline 2 Participants, by Displacement Status

We find that the attrition may not be completely at random. For local participants, the KS test rejects the null hypothesis that the p-values are uniformly distributed (with p-value 0.01). In addition, the mean differences for age, being a student, hours of electricity, and number of personal assets are statistically significant at the conventional level. Thus, in our analysis, we use multiple imputation in Section S10 to address this non-random attrition.

S7 Multiple Hypothesis Testing using Benjamini-Hochberg Procedure

In this section, we address concerns about multiple hypothesis testing by adjusting for the false discovery rate (FDR). We show the Benjamini-Hochberg (BH) adjusted p-values for each test of our main findings shown in Figure 4 in the paper (Benjamini and Hochberg, 1995). The two estimates that were negative and statistically significant: perception of displaced for locals in majority displaced classes at Endline 1, and perception of displaced for locals in balanced classes at Endline 2, both lose their statistical significance.

Population	Question	Mean	SE	p-value	adj p-value
All	ref_interaction	-0.095	0.121	0.217	0.356
3 Months	ref_interaction	-0.044	0.161	0.393	0.490
6 Months	ref_interaction	-0.178	0.181	0.163	0.356
Majority Displaced	ref_interaction	0.032	0.229	0.445	0.490
Minority Displaced	ref_interaction	-0.120	0.192	0.266	0.380
Balanced	ref_interaction	-0.218	0.228	0.169	0.356
All	ref_perception	-0.096	0.087	0.134	0.356
3 Months	ref_perception	-0.112	0.112	0.160	0.356
6 Months	ref_perception	-0.070	0.136	0.302	0.409
Majority Displaced	ref_perception	-0.306	0.150	0.021	0.356
Minority Displaced	ref_perception	0.003	0.142	0.490	0.490
Balanced	ref_perception	-0.019	0.162	0.454	0.490
All	ref_violence	-0.090	0.080	0.130	0.356
3 Months	ref_violence	-0.081	0.101	0.210	0.356
6 Months	ref_violence	-0.105	0.132	0.215	0.356
Majority Displaced	ref_violence	-0.180	0.135	0.091	0.356
Minority Displaced	ref_violence	0.061	0.126	0.313	0.409
Balanced	ref_violence	-0.255	0.167	0.063	0.356
All	ref_dim_employment	0.127	0.079	0.053	0.356
3 Months	ref_dim_employment	0.121	0.102	0.118	0.356
6 Months	ref_dim_employment	0.136	0.122	0.134	0.356
Majority Displaced	ref_dim_employment	0.130	0.141	0.178	0.356
Minority Displaced	ref_dim_employment	0.163	0.123	0.092	0.356
Balanced	ref_dim_employment	0.025	0.153	0.435	0.490
All	ref_dim_resources	-0.005	0.077	0.473	0.490
3 Months	ref_dim_resources	-0.112	0.099	0.129	0.356
6 Months	ref_dim_resources	0.167	0.124	0.089	0.356
Majority Displaced	ref_dim_resources	-0.092	0.129	0.238	0.357
Minority Displaced	ref_dim_resources	0.013	0.125	0.458	0.490
Balanced	ref_dim_resources	0.115	0.153	0.226	0.356

Table S3: Adjusted p-values using Benjamini-Hochberg for ITT prejudice outcomes for Endline 1.

Population	Question	Mean	SE	p-value	adj p-value
All	ref_interaction	0.148	0.104	0.078	0.346
3 Months	ref_interaction	0.117	0.132	0.187	0.380
6 Months	ref_interaction	0.198	0.170	0.122	0.346
Majority Displaced	ref_interaction	0.255	0.191	0.092	0.346
Balanced	ref_interaction	0.078	0.166	0.320	0.398
Minority Displaced	ref_interaction	0.165	0.184	0.185	0.380
All	ref_perception	-0.069	0.098	0.241	0.398
3 Months	ref_perception	-0.148	0.124	0.117	0.346
6 Months	ref_perception	0.058	0.160	0.358	0.398
Majority Displaced	ref_perception	0.106	0.172	0.268	0.398
Balanced	ref_perception	-0.426	0.155	0.003	0.088
Minority Displaced	ref_perception	0.329	0.184	0.037	0.346
All	ref_violence	-0.038	0.100	0.351	0.398
3 Months	ref_violence	-0.006	0.128	0.482	0.482
6 Months	ref_violence	-0.091	0.160	0.285	0.398
Majority Displaced	ref_violence	0.116	0.173	0.251	0.398
Balanced	ref_violence	-0.044	0.163	0.393	0.421
Minority Displaced	ref_violence	-0.136	0.186	0.233	0.398
All	ref_dim_employment	-0.093	0.106	0.190	0.380
3 Months	ref_dim_employment	-0.049	0.134	0.356	0.398
6 Months	ref_dim_employment	-0.164	0.174	0.173	0.380
Majority Displaced	ref_dim_employment	-0.089	0.195	0.324	0.398
Balanced	ref_dim_employment	0.021	0.170	0.451	0.466
Minority Displaced	ref_dim_employment	-0.219	0.192	0.127	0.346
All	ref_dim_resources	-0.123	0.103	0.117	0.346
3 Months	ref_dim_resources	-0.061	0.131	0.321	0.398
6 Months	ref_dim_resources	-0.223	0.167	0.091	0.346
Majority Displaced	ref_dim_resources	-0.248	0.186	0.092	0.346
Balanced	ref_dim_resources	0.080	0.162	0.311	0.398
Minority Displaced	ref_dim_resources	-0.274	0.191	0.076	0.346

Table S4: Adjusted p-values using Benjamini-Hochberg for ITT prejudice outcomes for Endline 2.

S8 Additional Subgroup Effects by Participant Characteristics

This section examines additional subgroup results based on local participants' own characteristics. We compare women (N = 480) versus men (N = 796), those younger than age 20 (N = 816) versus 20 or older (N = 466), participants of Pashtun ethnicity (N = 1094) versus not (N = 265), and those who had no prior exposure to harm (N = 1169). We do not have enough power to analyze those who had prior harm exposure (N = 193).

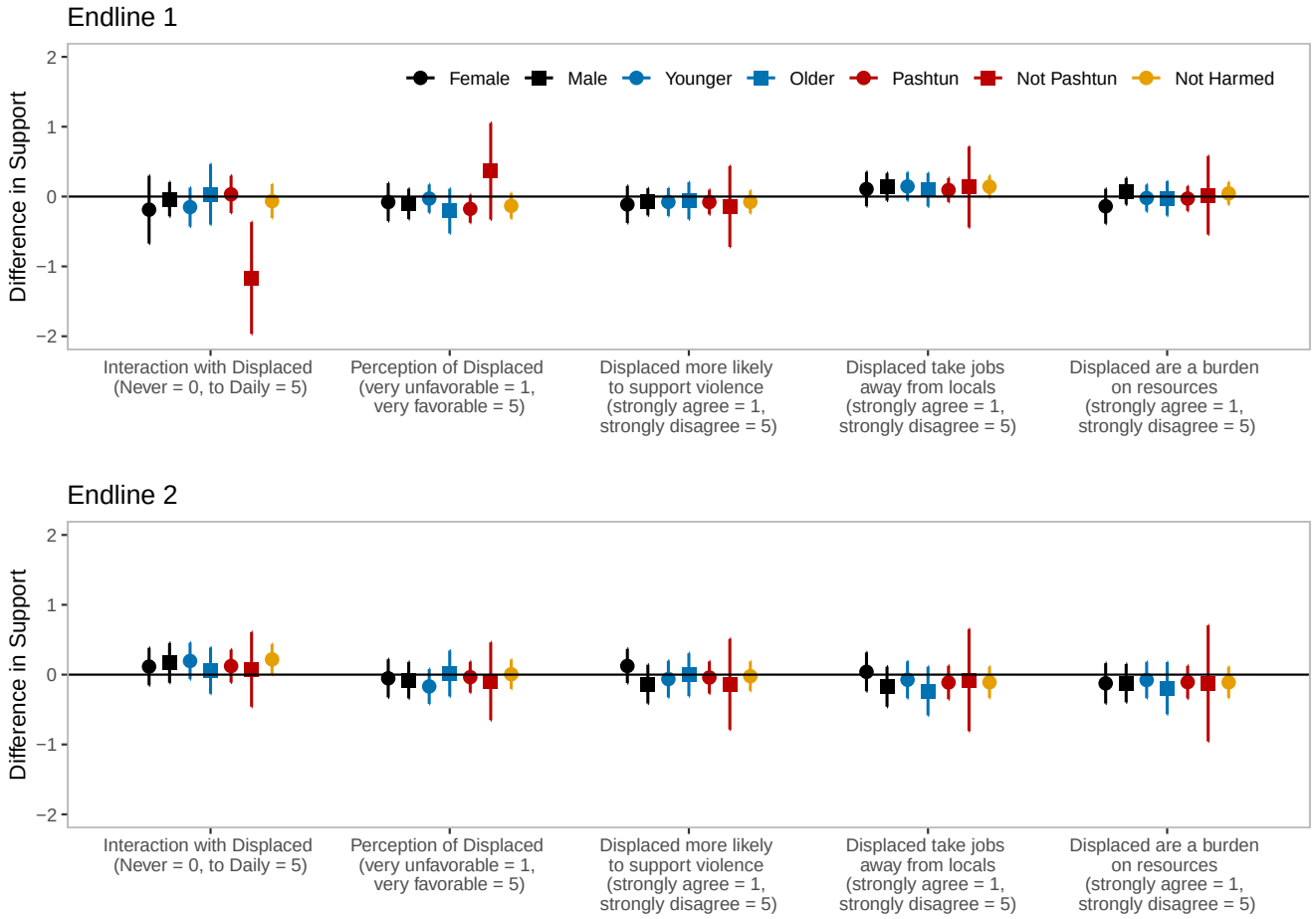


Figure S5: Intention-to-Treat effects of TVET at Endline 1 (top panel) and 2 (bottom panel) of prejudice outcomes by locals towards displaced, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

At Endline 1, local participants who were not of Pashtun ethnicity reported much less interaction with displaced people in their community. But otherwise, there were no subgroup effects of TVET by participant characteristics at either endline.

S9 Instrumental Variables Analysis

This section shows the main effects for only those who attended at least one class of the TVET program, the complier average treatment effects. To identify the average treatment effects for compliers, we use the Neyman stratification method, namely we calculate each component of the standard Wald estimator as a weighted average across each of the 10 blocks

$$\widehat{IV}_W = \frac{\widehat{ITT}_Y}{\widehat{ITT}_T} = \frac{\sum_b w_b \widehat{ITT}_{Yb}}{\sum_b w_b \widehat{ITT}_{Tb}} \tag{S1}$$

$\widehat{\text{ITT}}_{T_b}$ is the difference in R_i, which is actual treatment uptake for individual i (i.e. taking at least one TVET class) between T_b those assigned to treatment and C_b those assigned to control within block b.

$$\widehat{ITT}_{T_b} = \frac{1}{N_{T_b}} \sum_{i \in T_b} R_i - \frac{1}{N_{C_b}} \sum_{i \in C_b} R_i \tag{S2}$$

Figure S6 shows the the complier average treatment effects (CATE). Results are substantively similar to the main findings.

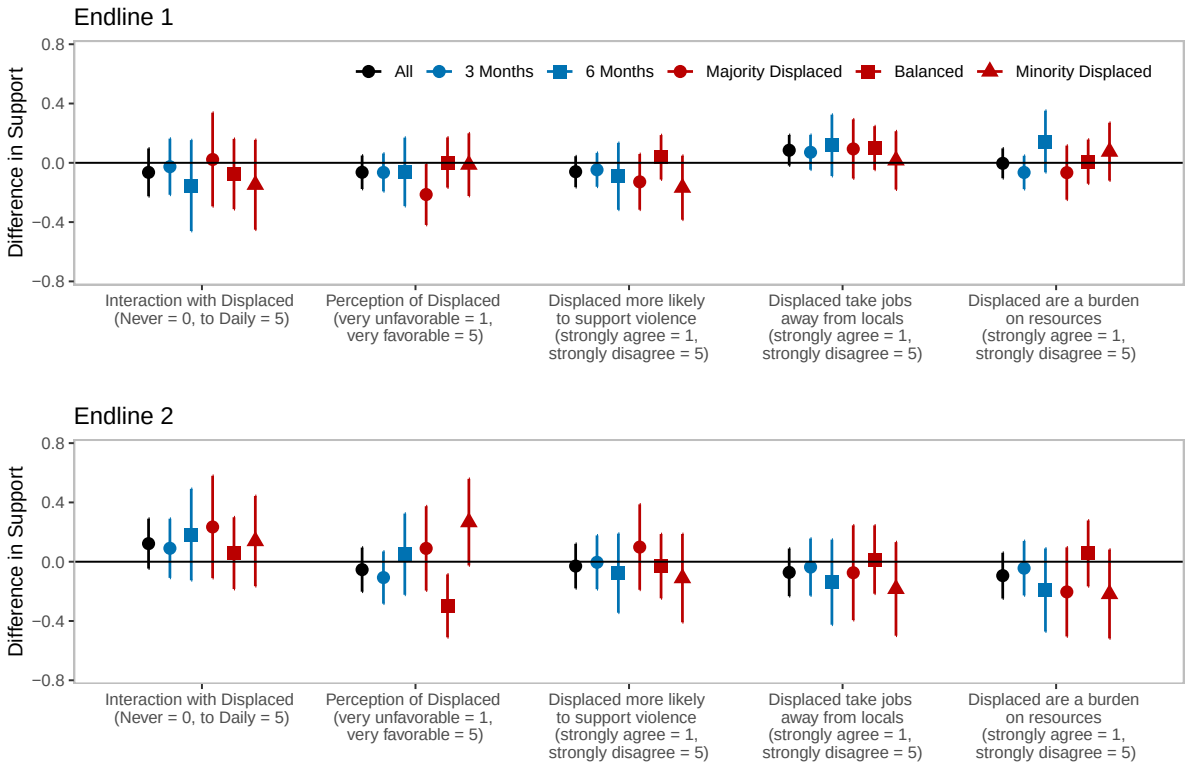


Figure S6: Instrumental Variables Endline 1 (top panel) and 2 (bottom panel) analysis of prejudice outcomes by locals towards displaced, with 95% confidence intervals.

S10 Analysis using Multiple Imputation

This section shows the main effects using multiple imputation to address attrition concerns. We use the R package *mi* to multiply impute the data with four chains. The variables we include are `block`, `displacement status`, `female` and program treatment assignment for which there is no missingness; baseline covariates `employment`, `age`, `prior exposure to harm`, `Pashtun ethnicity`, `household head`, `household size`, `married`, `formal education years`, `madrassa years`, `electricity hours`, `landownership` and `monthly net income`; and main outcomes of interest on prejudice toward IDPs measured at Baseline, Endline 1 and Endline 2.

Once we obtain the estimates for all four chains, we simply take the mean and use the standard variance formula:

$$\text{Var}(\hat{\phi}) = \frac{1}{M}\sum_{i=1}^{M}\text{Var}(\hat{\phi}_i) + \left(1+\frac{1}{M}\right)\frac{1}{M-1}\sum_{i=1}^{M}(\hat{\phi}_i - \hat{\phi})^2 \tag{S3}$$

where M indicates the number of chains and $\hat{\phi}_i$ is the point estimate from the i'th chain.

Figure S7 shows the analysis using multiple imputation. Results are substantively similar to the main findings.

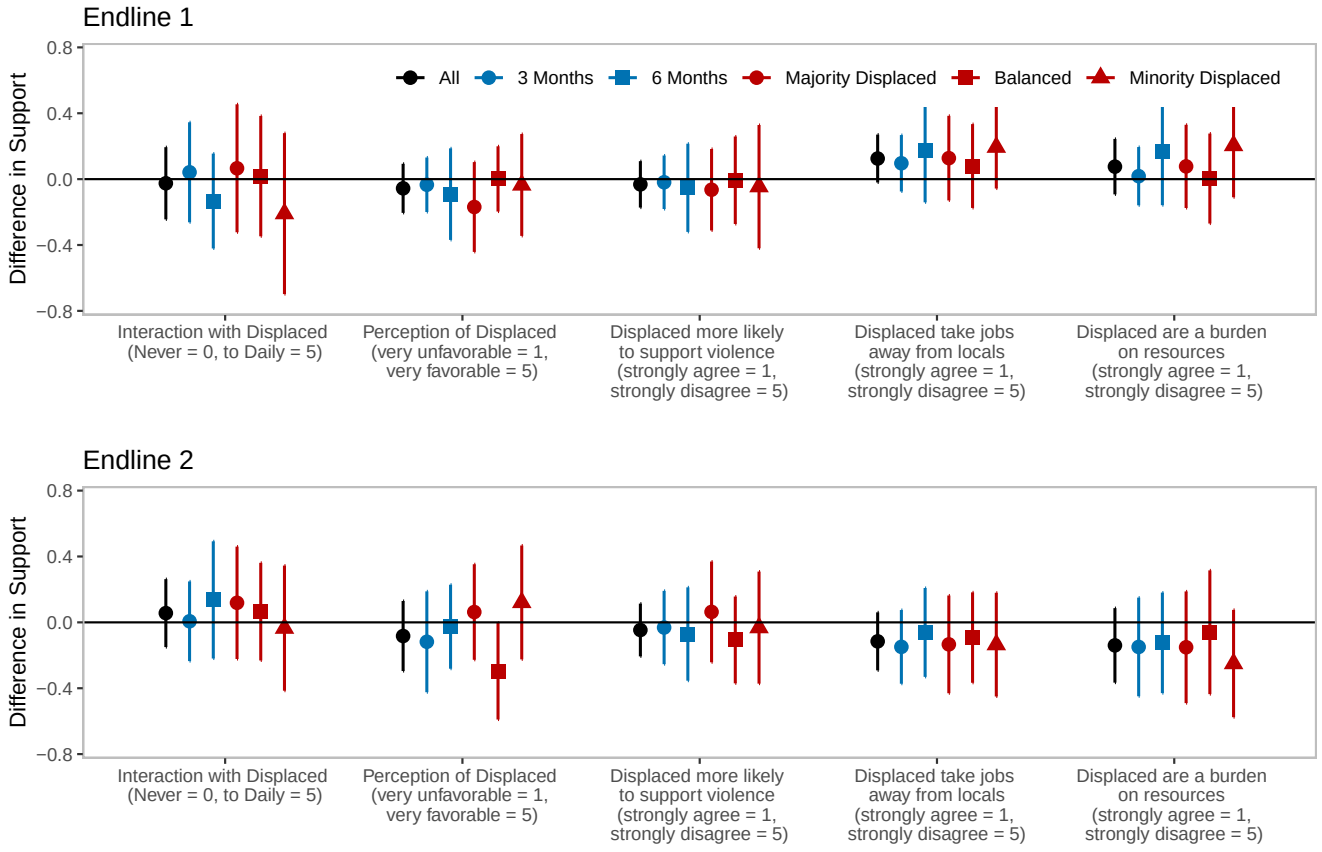


Figure S7: Intention-to-Treat Endline 1 (top panel) and 2 (bottom panel) analysis of prejudice outcomes by locals towards displaced using multiple imputation, with 95% confidence intervals.

S11 Comparing Effects of TVET to other treatment groups, UCT and UCT-TVET

This section shows compares the effects of TVET (black) to the those of the other treatment groups in INVEST: UCT (blue) and the interaction of the two, UCT conditional on TVET (red). The other treatment groups similarly had no effects on prejudice outcomes.

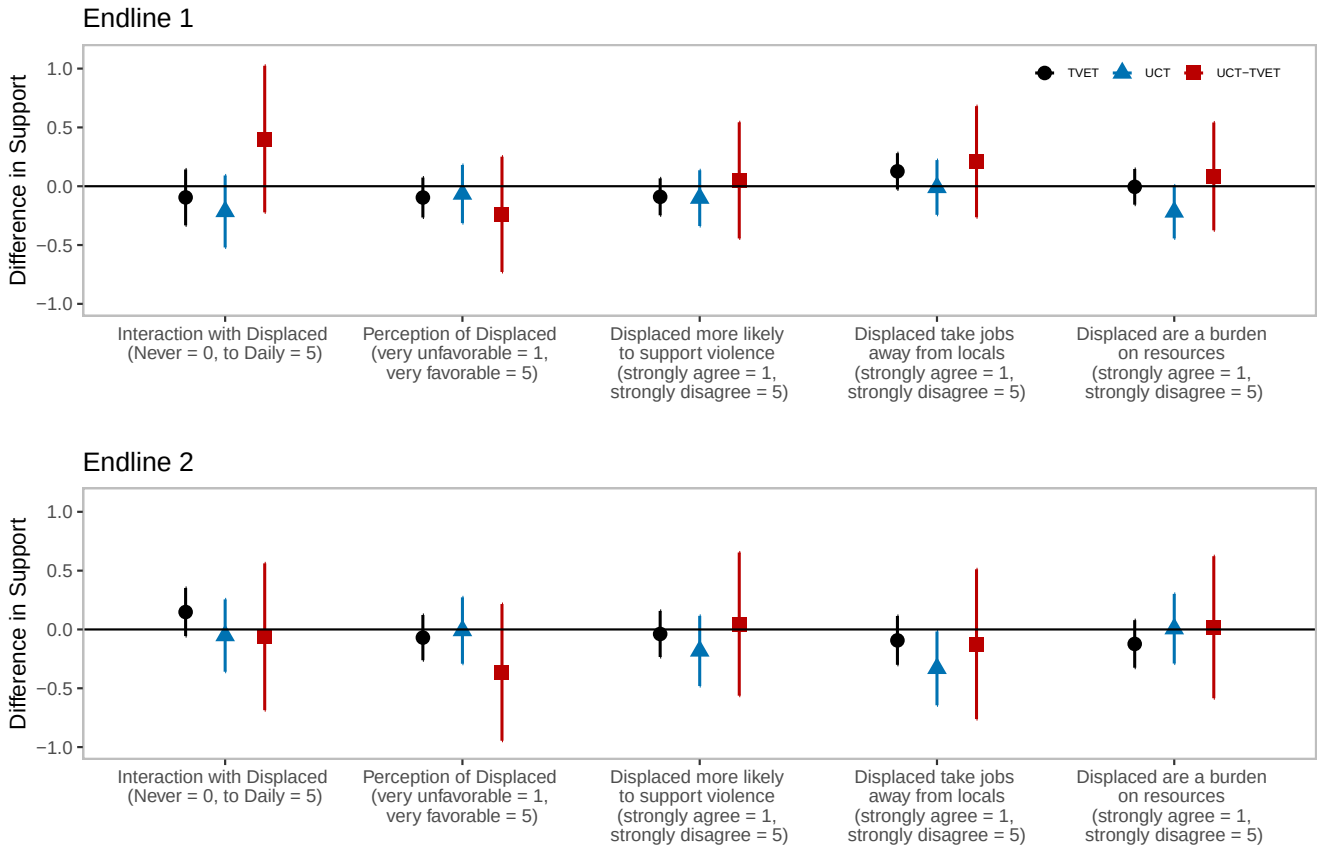


Figure S8: Intention-to-Treat Endline 1 (top panel) and 2 (bottom panel) analysis of prejudice outcomes by locals towards displaced, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

References

Benjamini, Yoav and Yosef Hochberg. 1995. "Controlling the false discovery rate: a practical and powerful approach to multiple testing." *Journal of the Royal statistical society: series B (Methodological)* 57(1):289–300.