Prolonged Contact Does Not Improve Locals' Relations with Migrants in Wartime Settings*†

Yang-Yang Zhou‡ Jason Lyall§

June 29, 2021

Abstract

Can prolonged contact improve local residents' relations with 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 in conflict settings, and none with migrant outgroups. We evaluate a randomized controlled trial of a vocational skills training program in Kandahar, Afghanistan, that enrolled 2,597 locals and migrants in near equal numbers. Courses lasted three or six months and emphasized hands-on collaborative learning. We conducted an endline survey and followed up eight months later. Although the program met the optimal conditions for contact theory, we find no evidence of behavioral or attitudinal change by locals toward IDPs, regardless of classroom demographics or course duration. Our findings suggest that migrant status is a relevant identity cleavage, and in conflict settings, prolonged contact is insufficient to improve local-migrant relations.

Keywords: intergroup contact, 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, the UBC Centre for Migration Studies, Texas A&M Bush School of Government and Public Service, the Security Studies Seminar at the Watson Institute for International and Public Affairs at Brown University, and the University of Pennsylvania's Perry World House seminar series provided helpful feedback. Zhou acknowledges support from the National Science Foundation (SES.1148900). 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). Our Pre-Analysis Plan was archived in the Evidence in Governance and Politics (EGAP) repository: #20150928AA.

‡Assistant Professor, Department of Political Science, University of British Columbia, yangyang.zhou@ubc.ca, www.yangyangzhou.com

§James Wright Chair in Transnational Studies and Associate Professor, Department of Government, Dartmouth College, jason.lyall@dartmouth.edu, www.jasonlyall.com

By December 2020, a record 55 million people were living in internal displacement around the world; more than 85 percent 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 intergroup contact actually improves locals' relations with displaced persons.

Public opinion surveys, for example, have identified a positive association between self-reported contact and positive attitudes toward migrants (Getmansky, Sınmazdemir and Zeitzoff, 2018; Ghosn, Braithwaite and Chu, 2019; De Coninck, Rodríguez-de Dios and d'Haenens, 2020; Knappert et al., 2021). These claims are vulnerable, however, to selection effects and social desirability bias. Moreover, despite a voluminous intergroup contact literature (e.g. Allport, 1954; Pettigrew, 1998; Pettigrew and Tropp, 2006; Lemmer and Wagner, 2015), we have only two field experiments on prejudice reduction in (post-)conflict settings, and none with migrants as the outgroup category (Scacco and Warren, 2018; Mousa, 2020).[2] Perhaps most worrisome is the prospect that popular economic interventions such as vocational training and cash transfers might exacerbate local-migrant conflict by stoking intergroup competition over scarce jobs or reinforcing existing prejudices. Given the scale and urgency of this global problem, there is a pressing need to examine whether prolonged and collaborative contact can improve locals' relations with migrants in wartime settings.

In partnership with Mercy Corps, we conducted a field experiment in Kandahar, Afghanistan, that enrolled 2,597 locals and migrants in a vocational training program called Introducing New

[1] We define *IDPs* (which we use interchangeably with "migrants") as 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.

[2] On the need for more field experiments testing intergroup contact theory, especially in non-Western contexts, see Paluck, Green and Green (2019); Paluck et al. (2020). Some important exceptions include Ditlmann and Samii (2016); Gu et al. (2019); Lowe (2021); Weiss (2021).

Vocational Education and Skills Training (INVEST).[3] Over half of our participants were IDPs living in camps or informal housing arrangements on the outskirts of Kandahar City or three neighboring districts. Participants were randomly assigned to three or six month vocational courses or were waitlisted for eventual enrollment. Unlike existing studies, our sample not only explicitly draws on migrant populations but also includes women. Locals and migrants spent considerable time together, between 360 and 720 total hours 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 in (post-)conflict settings.

Yet despite these efforts, we find that prolonged contact did not affect general attitudes about migrants among our 1,276 locals. We also found no effect on their beliefs about whether migrants support political violence, represent economic competition for local jobs, and unfairly burdened public resources. Local graduates also did not report increased post-INVEST contact with migrants in their communities. These precisely-estimated null findings hold regardless of course duration, composition, and participant traits, including age, gender, ethnicity, and prior exposure to violence. These null effects persisted for at least eight months after the program's conclusion. In short, 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.

Taken together, our paper offers three implications across the diverse literatures on intergroup contact, migration, and political violence. First, we provide evidence that wartime attitudes toward migrants are especially persistent, able to withstand up to six months of collaborative and sustained contact in a well-designed and well-funded intervention.[4] Second, our research illustrates the need to consider migrants as a distinct identity category, one that intersects with other social identities (including ethnicity and tribe) but that likely has its own independent effects on intergroup relations. Third, our findings call attention to the need to consider the social ramifications of economic interventions and to the untapped potential of marrying these

[3]More details on the (INVEST) program can be found at Lyall, Zhou and Imai (2020).

[4]This mirrors observational research on the stickiness of ethnic identities in wartime. See Lyall (2020).

interventions with dedicated anti-prejudice treatments.

Theorical 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 (e.g. Haer and Hecker, 2019; Masterson and Yasenov, 2021); that migrants represent labor market competitors while straining welfare programs (e.g. Adida, 2014; Ceritoglu et al., 2017; Gaikwad and Nellis, 2017); and that perceived cultural or political differences will generate conflict (e.g. Alrababa'h et al., 2021).

Contact theory predicts that prolonged positive social contact would reduce locals' misperceptions about migrants and lead to more positive attitudes, particularly if the contact met the four optimal conditions set by Allport (1954): equal status among groups, intergroup cooperation, common goals, and support from authorities. Allport (1954) was originally writing about desegregation in U.S. institutions like classrooms. Prolonged contact within a naturalistic classroom setting is precisely our intervention. 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, such as the Ministry of Refugees and Repatriation (MoRR) and the UN High Commissioner for Refugees (UNHCR).

Furthermore, although prejudice against IDPs has been building since the early 2000s in this area of Afghanistan, the tensions between locals and IDPs are not so deep-seated and historic to make attitudinal change through months of contact an unrealistic goal (Pettigrew and Tropp, 2006). Yet recent work suggests that the positive effects of contact are generally modest and short-lived (Paluck, Green and Green, 2019; Paluck et al., 2020). There is also the possibility that wartime attitudes are so hardened that no intervention can sway them, and further, prolonged contact may even reinforce stereotypes and heighten a sense of competition between groups.

We hypothesize that positive and prolonged social contact with IDPs would positively shift

locals' attitudes toward IDPs in general:

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

We also explore heterogeneous effects by course duration, local-migrant classroom balance, and participant demographics.[5]

> **(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).

Research Design

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).[6] Kandahar, the site of our experiment, was home to about 125,000 IDPs, the fourth-highest total among Afghanistan's 34 provinces. It is important to clarify that while our study takes place in an active conflict, the main dividing lines of conflict are not between locals and migrants, but rather between the Taliban and Afghan government forces.

[5]To be transparent, these heterogeneous effects analyses were not pre-registered. While we have variation in these conditions, we also note that we did not randomize them.

[6]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 Internal Displacement Monitoring (2017, 2020).

	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	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.

Nevertheless, the setting of active conflict is contextually important to our study, as it is the major cause of displacement and heightens fears against migrants. For locals, migrants represent security threats, through perceived ties to non-state armed groups like the Taliban or criminal gangs, as well as economic competitors in a tight labor market, and a strain on resources such as schools and housing (Internal Displacement Monitoring, 2017; Lakhani and Amiri, 2020). For more discussion on the prejudice and discrimination experienced by migrants in Afghanistan, see Section S1 in the Supplementary Information (SI).

Active conflict settings also draw billions of dollars in aid programs, like INVEST, that provide opportunities for social contact. The INVEST program was designed with the primary goal of reducing unemployment of at-risk youth in Kandahar City and three surrounding districts through the provision of vocational training and a one-time unconditional cash transfer.[7] Prior to randomization, all participants chose to enroll in a particular vocational course. Blocking on course duration, location, and demographic characteristics, we randomized participants into either the training (treatment) or a waitlist (control). Table 1 shows the sample sizes, proportions, and compliance rates for the treatment and control groups by displacement status.

Demographically, locals and migrants were almost identical in terms of age (average 20 years old), high levels of unemployment, and ethnicity (80% Pashtun) (see SI Figure S1).[8] For the treatment group, local and migrant participants had a high degree of exposure to one another.

[7]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.

[8]Tribal affiliations likely differ between locals and migrants, but they correlate strongly with local or migrant status, and were a way for participants to identify whether a classmate was local or not.

They 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 (Scacco and Warren, 2018; Mousa, 2020), where participants spent 64 or 26 hours with members of the outgroup, respectively. Moreover, all courses used collaborative hands-on learning techniques; instructors confirmed that there was no self-segregation since students had to work together to learn skills through shared problem-solving.

All courses 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.[9] While participants were largely coethnic, locals and instructors were able to identify the migrants in the classroom through a number of cues, such as dialects and which neighborhoods / IDP camps (see SI Figure S2) the participant was coming from. We provide additional details about participant recruitment and demographics, course lists, program activities, classroom composition, displacement history of our migrant participants, and qualitative evidence on how well the program was received in SI Sections S2–S5.

Unlike most contact studies that measure outcomes the same day as the intervention, we conducted an endline survey after the courses ended and a second endline eight months later to measure longer-term effects. We asked the following questions to our local participants (N = 1276) about their 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, take employment away from locals, or be a burden on government resources. 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 through weighting. See SI Sections S6–S8 for more details on the blocked randomization procedure and estimation, exact wording of survey questions, study timeline, and

[9]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 (Paluck and Clark, 2020).

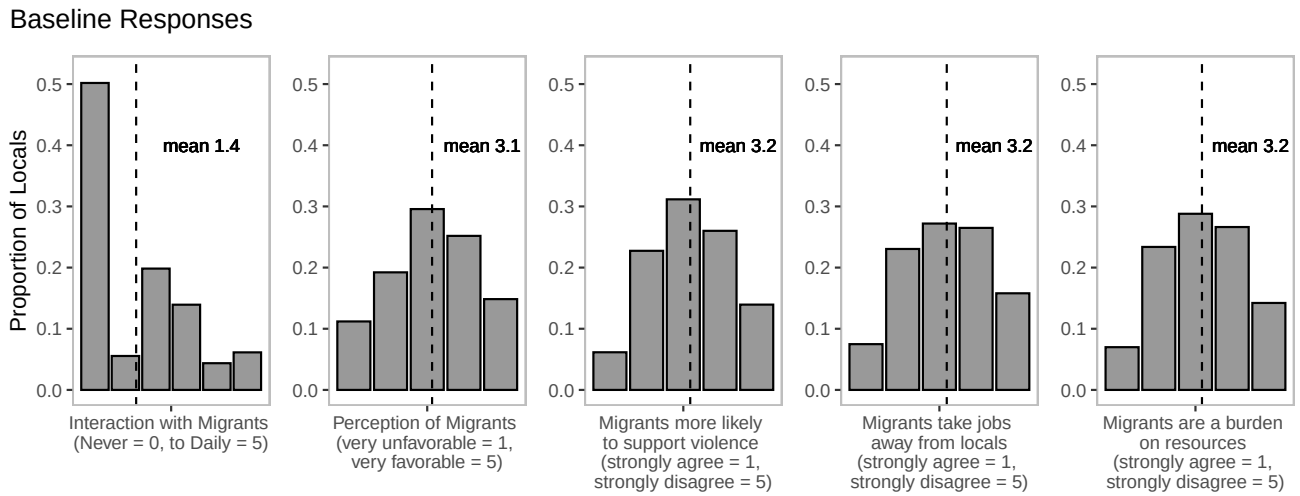


Figure 1: Histograms of locals' baseline (pre-intervention) responses to migrant questions. For each question, right-most (left-most) bar indicates more inclusive (more exclusionary) response.

balance tables.

Ethical Considerations

When designing and conducting this research, we were mindful that our participants (locals and migrants) were a vulnerable population. 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 1 shows that about half of our local participants reported never interacting with migrants. The average is several times a year (1.4 on the scale of Never (0) to Daily (5),

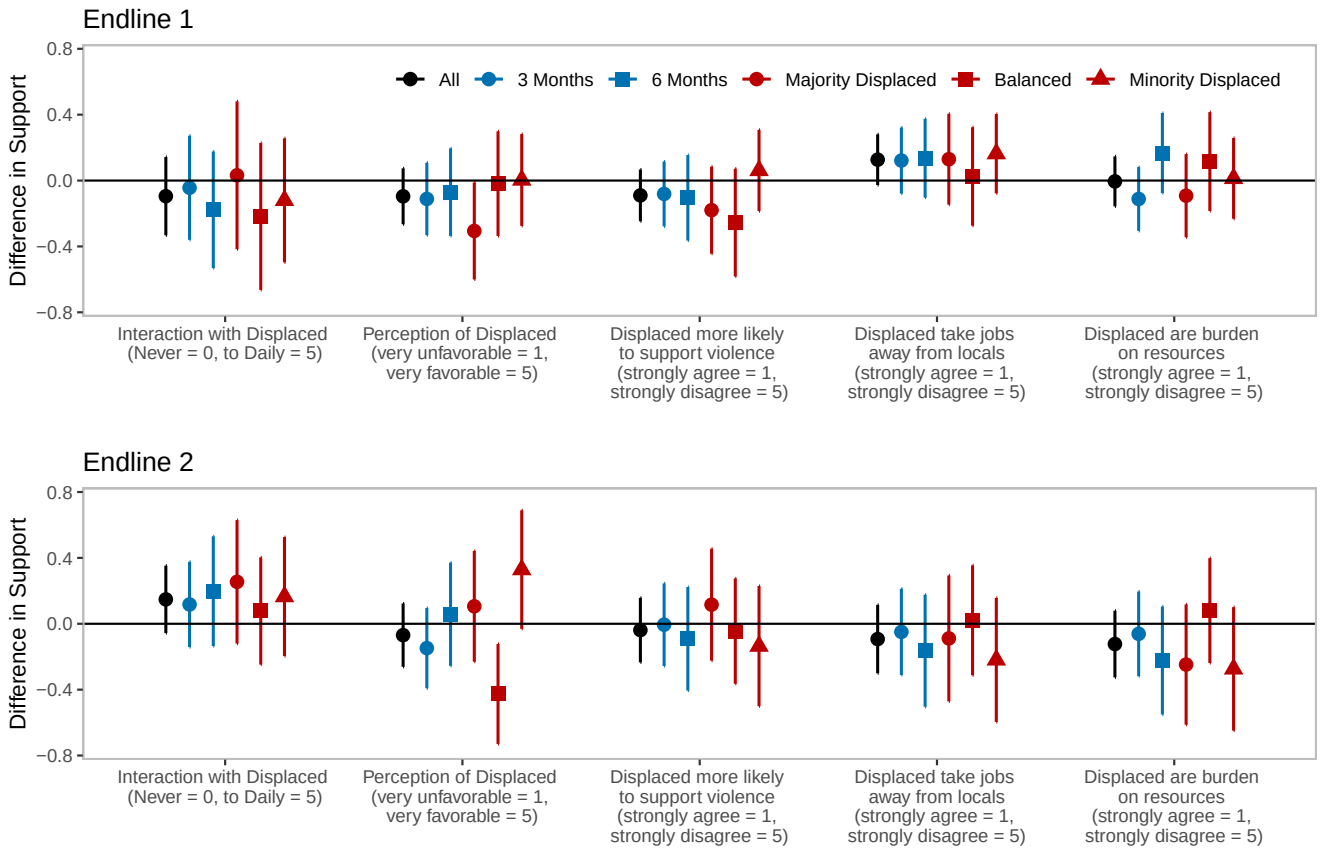


Figure 2: Intention-to-Treat effects of TVET at Endline 1 (top panel) and 2 (bottom panel) of prejudice outcomes by locals towards displaced, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

1.6 SD). Fig. S5 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 intergroup contact through the INVEST program affect locals' receptivity toward migrants? From Figure 2, 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. 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. These suggestive findings are consistent with recent literature. It is possible that prejudice is incredibly difficult to change, but regardless, behavioral changes may occur due to changed social norms and expectations (Paluck and Green, 2009; Adida, Lo and Platas, 2018; Scacco and Warren, 2018).

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%) 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).[10] 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 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).

[10]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.

Next, 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 these null effects as suggesting that even intensive intergroup contact interventions will struggle to change minds in active conflict settings. The incentives for motivated reasoning 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. Thus, barring a significant structural change, i.e. the end of conflict, preexisting biases are unlikely to attenuate. 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; and (4) it heightened economic competition.

First, surveys among 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). 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.[11] Thus, although our study does not examine historic and deep-rooted racial or religious

[11]Interview with VTC Director, December 2016

tensions between the two groups in contact, the local-migrant identity cleavage is highly relevant.

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 (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). In short, 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).

Third, there was substantial support from local authorities in addition to external actors like the UNHCR. The program design and participant recruitment involved several provincial ministries, village councils (*shuras*), local leaders, and tribal elders. Government officials also frequently visited the VTCs and were present at 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. Additionally, 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.

Conclusion

Given the massive scale of the global IDP population, identifying the best mix of programs to reduce prejudice and conflict toward vulnerable migrants is an urgent policy problem. Despite a well-designed and well-received program that offered locals prolonged and collaborative exposure to migrants, we find no change in locals' reception of migrants. Wartime migrants might prove a particularly unsympathetic category for prejudice reduction. 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 as well as explore the cross-cutting nature of migrant status and other identities, such as coethnicity. Second, INVEST did not have a module aimed at prejudice reduction and addressing the social cleavage. Given this context, it may be that in order to change beliefs, a similar intervention would need an explicit anti-prejudice programming component. Recent research shows the promising positive effects of more explicit empathy-inducing interventions (e.g. Broockman and Kalla, 2016; Adida, Lo and Platas, 2018; Kalla and Broockman, 2020). Future research should experiment with combining contact with these types of perspective-giving, -taking, and norm signalling interventions within (and outside) conflict settings. Researchers should directly compare these types of training programs with other interventions, such as playing a sport together (e.g. Mousa, 2020; Lowe, 2021), that remove the economic dimension. While our participants were largely coethnic, scholars should explore the cross-cutting nature of migrant status and coethnicity; perhaps social distance on one dimension mediates the effects of contact. Lastly, local-migrant relations are two-sided in nature. Future studies should incorporate the views and behaviors of migrants toward local populations. In doing so, this research can paint a more complete picture of intergroup relations in wartime and other fragile settings.

References

Adida, Claire, Adeline Lo and Melina Platas. 2018. "Perspective-taking can promote short-term inclusionary behavior toward Syrian refugees." *Proceedings of the National Academy of Sciences* 115(38):9521–9526.

Adida, Claire L. 2014. *Immigrant exclusion and insecurity in Africa*. Cambridge University Press.

Allport, Gordon. 1954. *The Nature of Prejudice*. Addison-Wesley: Reading, MA.

Alrababa'h, Ala', Andrea Dillon, Scott Williamson, Jens Hainmueller, Dominik Hangartner and Jeremy Weinstein. 2021. "Attitudes toward migrants in a highly impacted economy: Evidence from the Syrian refugee crisis in Jordan." *Comparative Political Studies* 54(1):33–76.

Beelmann, Andreas and Kim Sarah Heinemann. 2014. "Preventing prejudice and improving intergroup attitudes: A meta-analysis of child and adolescent training programs." *Journal of applied developmental psychology* 35(1):10–24.

Broockman, David and Joshua Kalla. 2016. "Durably reducing transphobia: A field experiment on door-to-door canvassing." *Science* 352(6282):220–224.

Ceritoglu, Evren, H Burcu Gurcihan Yunculer, Huzeyfe Torun and Semih Tumen. 2017. "The impact of Syrian refugees on natives? labor market outcomes in Turkey: evidence from a quasi-experimental design." *IZA Journal of Labor Policy* 6(1):5.

De Coninck, David, Isabel Rodríguez-de Dios and Leen d'Haenens. 2020. "The contact hypothesis during the European refugee crisis: Relating quality and quantity of (in) direct intergroup contact to attitudes towards refugees." *Group Processes & Intergroup Relations* p. 1368430220929394.

Ditlmann, Ruth K and Cyrus Samii. 2016. "Can intergroup contact affect ingroup dynamics? Insights from a field study with Jewish and Arab-Palestinian youth in Israel." *Peace and Conflict: Journal of Peace Psychology* 22(4):380.

Gaikwad, Nikhar and Gareth Nellis. 2017. "The Majority-Minority Divide in Attitudes toward Internal Migration: Evidence from Mumbai." *American Journal of Political Science* 61(2):456–472.

Getmansky, Anna, Tolga Sınmazdemir and Thomas Zeitzoff. 2018. "Refugees, Xenophobia, and Domestic Conflict: Evidence from a Survey Experiment in Turkey." *Journal of Peace Research* 55(4):491–507.

Ghosn, Faten, Alex Braithwaite and Tiffany S Chu. 2019. "Violence, displacement, contact, and attitudes toward hosting refugees." *Journal of Peace Research* 56(1):118–133.

Gu, Jun, Annika Mueller, Ingrid Nielsen, Jason Shachat and Russell Smyth. 2019. "Improving Intergroup Relations through Actual and Imagined Contact: Field Experiments with Malawian Shopkeepers and Chinese Migrants." *Economic Development and Cultural Change* 68(1):273–303.

Haer, Roos and Tobias Hecker. 2019. "Recruiting refugees for militarization: The determinants of mobilization attempts." *Journal of Refugee Studies* 32(1):1–22.

IDMC and Norwegian Refugee Council. 2021. *GRID 2021: Internal Displacement in a Changing Climate*. Oslo and New York: Danish Refugee Council.

Internal Displacement Monitoring. 2017. Going "Home" to Displacement: Afghanistan's returnee-IDPs. Technical report. http://www.internal-displacement.org/sites/default/files/inline-files/20171214-idmc-afghanistan-case-study.pdf.

Internal Displacement Monitoring. 2019. Global report on internal displacement. Technical report. https://www.internal-displacement.org/global-report/grid2019/.

Internal Displacement Monitoring. 2020. A Different Kind of Pressure: The cumulative effects of displacement and return in Afghanistan. Technical report. https://www.internal-displacement.org/publications/a-different-kind-of-pressure-the-cumulative-effects-of-displacement-and-return-in.

Kalla, Joshua L and David E Broockman. 2020. "Reducing exclusionary attitudes through interpersonal conversation: evidence from three field experiments." *American Political Science Review* 114(2):410–425.

Knappert, Lena, Hans Van Dijk, Shuai Yuan, Yuval Engel, Jan-Willem van Prooijen and Andre Krouwel. 2021. "Personal contact with refugees is key to welcoming them: An analysis of politicians' and citizens' attitudes towards refugee integration." *Political Psychology* 42(3):423–442.

Lakhani, Sadaf and Rahmatullah Amiri. 2020. Displacement and the Vulnerability to Mobilize for Violence: Evidence from Afghanistan. Technical report. https://www.usip.org/sites/default/files/2020-01/pw_155-displacement_and_the_vulnerability_to_mobilize_for_violence_evidence_from_afghanistan.pdf.

Lemmer, Gunnar and Ulrich Wagner. 2015. "Can we really reduce ethnic prejudice outside the lab? A meta-analysis of direct and indirect contact interventions." *European Journal of Social Psychology* 45(2):152–168.

Lowe, Matt. 2021. "Types of contact: A field experiment on collaborative and adversarial caste integration." *American Economic Review* 111(6):1807–44.

Lyall, Jason. 2020. *Divided Armies: Inequality and Battlefield Performance in Modern War*. Princeton, NJ: Princeton University Press.

Lyall, Jason, Yang-Yang Zhou and Kosuke Imai. 2020. "Can economic assistance shape combatant support in wartime? Experimental evidence from Afghanistan." *American Political Science Review* 114(1):126–143.

Masterson, Daniel and Vasil Yasenov. 2021. "Does Halting Refugee Resettlement Reduce Crime? Evidence from the US Refugee Ban." *American Political Science Review* pp. 1–8.

Mousa, Salma. 2020. "Building social cohesion between Christians and Muslims through soccer in post-ISIS Iraq." *Science* 369(6505):866–870.

Norwegian Refugee Council. 2018. Escaping War: Where to Next? A Research Study on the Challenges of IDP Protection in Afghanistan. Technical report. https://www.nrc.no/resources/reports/escaping-war-where-to-next-the-challenges-of-idp-protection-in-Afghanistan/.

Paluck, Elizabeth Levy and Chelsey Clark. 2020. "Can Playing Together Help Us Live Together?" *Science* 369(6505):769–70.

Paluck, Elizabeth Levy and Donald P Green. 2009. "Prejudice reduction: What works? A review and assessment of research and practice." *Annual review of psychology* 60:339–367.

Paluck, Elizabeth Levy, Roni Porat, Chelsey S Clark and Donald P Green. 2020. "Prejudice reduction: Progress and challenges." *Annual Review of Psychology* 72.

Paluck, Elizabeth Levy, Seth A Green and Donald P Green. 2019. "The contact hypothesis re-evaluated." *Behavioural Public Policy* 3(2):129–158.

Pettigrew, Thomas. 1998. "Intergroup Contact Theory." *Journal of Personality and Social Psychology* 49(1):65–85.

Pettigrew, Thomas and Linda Tropp. 2006. "A Meta-Analytic Test of Intergroup Contact Theory." *Journal of Personality and Social Psychology* 90(5):751.

Saguy, Tamar, John F Dovidio and Felicia Pratto. 2008. "Beyond contact: Intergroup contact in the context of power relations." *Personality and Social Psychology Bulletin* 34(3):432–445.

Saguy, Tamar, Linda R Tropp and Diala Hawi. 2012. The role of group power in intergroup contact. In *Advances in intergroup contact*. Psychology Press pp. 127–146.

Scacco, Alexandra and Shana S Warren. 2018. "Can social contact reduce prejudice and discrimination? Evidence from a field experiment in Nigeria." *American Political Science Review* 112(3):654–677.

Tropp, Linda R and Mary A Prenovost. 2008. "The role of intergroup contact in predicting children's interethnic attitudes: Evidence from meta-analytic and field studies." *Intergroup attitudes and relations in childhood through adulthood* pp. 236–248.

Weiss, Chagai M. 2021. "Diversity in health care institutions reduces Israeli patients? prejudice toward Arabs." *Proceedings of the National Academy of Sciences* 118(14).

Prolonged Contact Does Not Improve Locals' Relations with Migrants in Wartime Settings*

Yang-Yang Zhou† Jason Lyall‡

For Online Publication: Supplementary Information

Contents

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

†Assistant Professor, Department of Political Science, University of British Columbia, yangyang.zhou@ubc.ca, www.yangyangzhou.com

‡James Wright Associate Professor in Transnational Studies, Department of Government, Dartmouth College, jason.lyall@dartmouth.edu, www.jasonlyall.com

S1 Additional Context: Displacement in Afghanistan

Afghanistan has one of largest IDP populations in the world, due to a legacy of nearly four decades of war, political instability, drought, and other natural disasters (Internal Displacement Monitoring, 2019). 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.[1]

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.[2]. 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 (Internal 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, 14).[3] 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

[1] IOM-DTM Baseline Mobility Assessment (Round 7, Oct-Dec 2018).
[2] Interview with VTC Director, December 2016
[3] A 27-year-old Pashtun IDP in Kunduz, Afghanistan.

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, 14).[4] Another survey conducted in 2018 found that 72 percent 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.

[4]A 35-year-old Pashtun IDP in Kandahar, Afghanistan.

S2 Additional Details on the INVEST Program

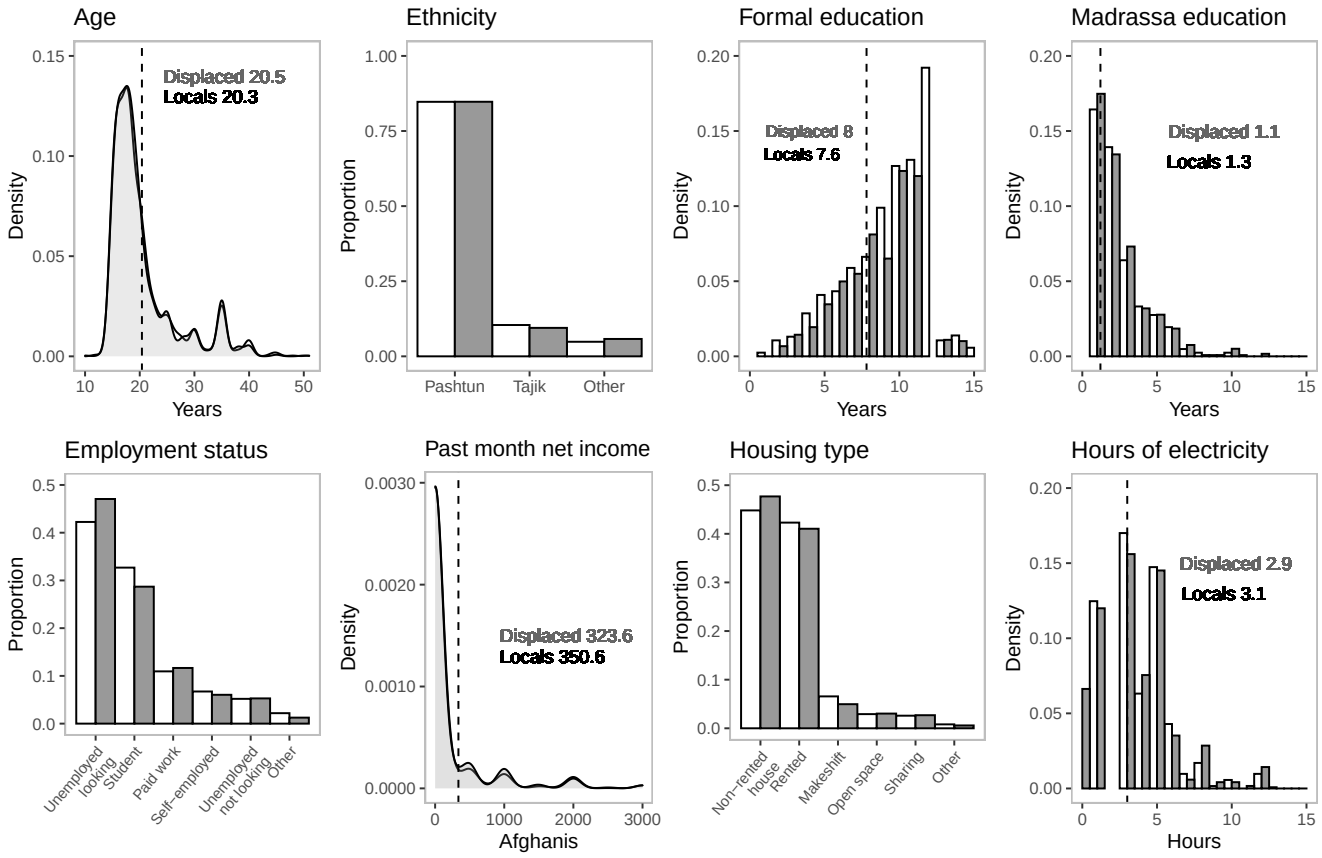


Figure S1: Participant demographics at baseline, grouped by displaced (white) and locals (gray).

INVEST provided a naturalistic test of intergroup contact theory. 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 a single VTC for the program's duration based on gender and proximity to their homes.[5] Figure S2 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.[6] 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 cost at about US$229 (15,600 Afghanis) per individual in 2016 (excluding fixed costs such as renting training

[5]Participants commuted up to 45 minutes daily between their homes and the assigned VTC.
[6]For the full list of courses, see Section S3.

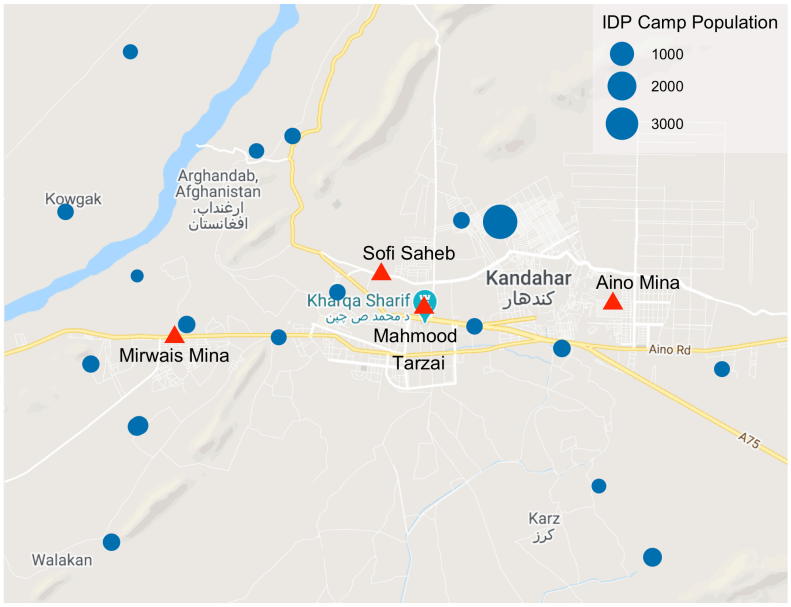


Figure S2: Map of Kandahar City showing the locations of the four VTCs in red, and surrounding IDP camp locations in blue, weighted by their population from January 2014 to March 2016. Source for IDP data: IDMC's Global Internal Displacement Database. Google basemap.

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 Ministry of Refugees and Repatriation (MoRR), 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 (49.13%) were Kandaharis while 1,321 (50.87%) 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 (Scacco and Warren, 2018) and Iraq (Mousa, 2020) 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 (1408.98 Afghanis).

Fig. S1 shows demographic summary statistics grouped by displacement status. Generally, migrant participants were similar to locals, particularly in terms of age and ethnic group. Section S5 of the SI gives an overview of the displacement history among our migrant 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. Across all the classes, they range from 34.27% 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 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). All participants were enrolled by lottery, with similar SES backgrounds, helping ensure formal equality in the classroom.

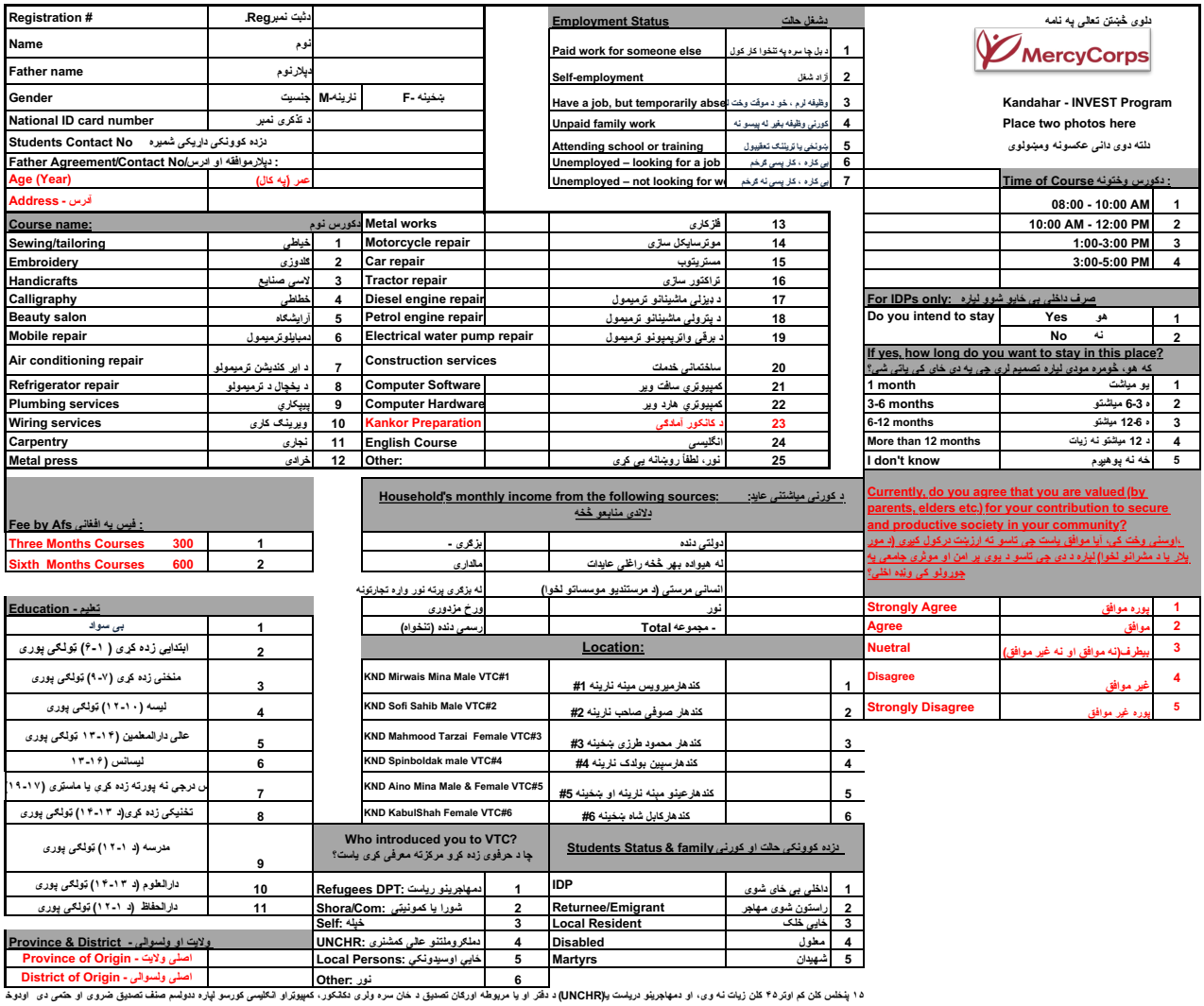
Locals and migrants also had a high degree of exposure to one another. Participants met for six hours per days, five days a week, for either three or six months, depending on their course. Participants therefore spent between 360 and 720 hours together. This compares favorably with existing studies in Nigeria and Iraq (Scacco and Warren, 2018; Mousa, 2020), where participants had only 64 or 26 hours with members of the outgroup, respectively. Moreover, all courses used collaborative learning techniques in which students worked together to learn new skills through shared problem-solving. Drawing on a practice-based curriculum, instructors had students collaborate on shared tasks — e.g., motorcycle or solar cell repairs — to learn new skills collectively. To receive accreditation, students had to complete a practical final exam that demonstrated their mastery of their new skillset. As a result, INVEST's certification process not only aided students in their search for jobs but also acted as a superordinate goal that facilitated cooperation between locals and IDPs. They also completed a soft-skills training component where they practices networking skills with each other.

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).

S3 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 #	د ثبت نمبر Reg.		
Name	نوم		
Father name	د پلار نوم		
Gender	جنسیت	نارینه M-	ښځینه F-
National ID card number	د تذکرې نمبر		
Students Contact No	د زده کوونکی داریکی شمیره		
Father Agreement/Contact No/د پلار موافقه او ادرس :			
Age (Year)	عمر (په کال)		
Address -	ادرس		

دلوی څښتن تعالی په نامه

MercyCorps

Kandahar - INVEST Program

Place two photos here

دلته دوی دانی عکسونه ومښلوی

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	وظیفه لرم ، خو د موقت وخت	3
Unpaid family work	کورنی وظیفه بغیر له پیسو نه	4
Attending school or training	ښوونځی یا تربیتی تعقیبول	5
Unemployed – looking for a job	بې کاره ، کار پسې ګرځم	6
Unemployed – not looking for w	بې کاره ، کار پسې نه ګرځم	7

Course name:	دکورس نوم		Metal works		فلزکاری	13
Sewing/tailoring	خیاطی	1	Motorcycle repair		موټرسایکل سازی	14
Embroidery	ګلدوزی	2	Car repair		مسترېتوب	15
Handicrafts	لاسی صنایع	3	Tractor repair		تراکتور سازی	16
Calligraphy	خطاطی	4	Diesel engine repair		د ډیزلی ماشینانو ترمیمول	17
Beauty salon	آرایشګاه	5	Petrol engine repair		د پترولی ماشینانو ترمیمول	18
Mobile repair	دموبایلوترمیمول	6	Electrical water pump repair		د برقی واټرپمپونو ترمیمول	19
Air conditioning repair	د اير کنديشن ترميمولو	7	Construction services		ساختمانی خدمت	20
Refrigerator repair	د یخچال د ترمیمونو	8	Computer Software		کمپیوټری سافټ ویر	21
Plumbing services	پیپکاري	9	Computer Hardware		کمپیوټري هارډ ویر	22
Wiring services	ویرینګ کاری	10	Kankor Preparation		د کانکور آمادګی	23
Carpentry	نجاری	11	English Course		انګلیسی	24
Metal press	خرادی	12	Other:		نور، لطفاً روښانه یې کړی	25

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 داخلی بې ځایو 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 تصمیم لری چې په دی ځای کی پاتې شې؟		
1 month	یو میاشت	1
3-6 months	3-6 د میاشتو	2
6-12 months	6-12 د میاشتو	3
More than 12 months	12 د میاشتو نه زیات	4
I don't know	خه نه پوهیږم	5

Fee by Afs فیس په افغانی :		
Three Months Courses 300		1
Sixth Months Courses 600		2

Household's monthly income from the following sources: د کورنی میاشتنی عاید: دلاندې منابعو څخه			
بزګری -		دولتی دنده	
مالداری		له هیواده بهر څخه راغلی عایدات	
له بزګری پرته نور واره تجارتونه		انسانی مرستی (د مرستندیو موسساتو لخوا)	
ورځ مزدوری		نور	
رسمی دنده (تنخواه)		Total - مجموعه	

Currently, do you agree that you are valued (by parents, elders etc) for your contribution to secure and productive society in your community? .اوسنۍ وخت کې، آیا موافق پاست چې تاسو ته ارزښت درکول کیږی (د مور پلار یا د مشرانو لخوا) لپاره د دی چې تاسو د یوی پر امن او موثری جامعی په جوړولو کی ونده اخلی؟		
Strongly Agree	پوره موافق	1
Agree	موافق	2
Nuetral	بیطرف(نه موافق او نه غیر موافق)	3
Disagree	غیر موافق	4
Strongly Disagree	پوره غیر موافق	5

Education - تعلیم		
بی سواد		1
ابتدایی زده کری (۱-۶) ټولګی پوری		2
منځنی زده کری (۷-۹) ټولګی پوری		3
لیسه (۱۰-۱۲) ټولګی پوری		4
عالی دارالمعلمین (۱۳-۱۴) ټولګی پوری		5
لیسانس (۱۶-۱۳)		6
ښ درجی نه پورته زده کری یا ماسټری (۱۷-۱۹)		7
تخنیکی زده کری(د ۱۳-۱۴) ټولګی پوری		8
مدرسه (د ۱-۱۲) ټولګی پوری		9
دارالعلوم (د ۱۳-۱۴) ټولګی پوری		10
دارالحفاظ (د ۱-۱۲) ټولګی پوری		11

Location:		
KND Mirwais Mina Male VTC#1	کندهارمیرویس مینه نارینه #1	1
KND Sofi Sahib Male VTC#2	کندهار صوفی صاحب نارینه #2	2
KND Mahmood Tarzai Female VTC#3	کندهار محمود طرزی ښځینه #3	3
KND Spinboldak male VTC#4	کندهارسپین بولدک نارینه #4	4
KND Aino Mina Male & Female VTC#5	کندهارعینو مینه نارینه او ښځینه #5	5
KND KabulShah Female VTC#6	کندهارکابل شاه ښځینه #6	6

Who introduced you to VTC? چا د حرفوی زده کړو مرکزته معرفی کری پاست؟		Students Status & family نزده کوونکی حالت او کورنی		
Refugees DPT: دمهاجرینو ریاست	1	IDP	داخلی بې خای شوی	1
Shora/Com: شورا یا کمونیټی	2	Returnee/Emigrant	راستون شوی مهاجر	2
Self: خپله	3	Local Resident	خایی خلک	3
UNCHR: دملګرملتنو عالی کمشنری	4	Disabled	معلول	4
Local Persons: خایی اوسیدونکی	5	Martyrs	شهیدان	5
Other: نور	6			

Province & District ولایت او ولسوالی -	
Province of Origin - اصلی ولایت	
District of Origin - اصلی ولسوالی	

۱۵ پنځلس کلن کم اوتر۴۵ کلن زیات نه وی، او دمهاجرینو دریاست یا(UNCHR) د دفتر او یا مربوطه اورګان تصدیق د خان سره ولری دکډکور، کمپیوټراو انګلیسی کورسو لپاره دوولسم صنف تصدیق ضروی او حتمی دی اودوهٔ

S4 Classroom Composition: Proportion of Migrant Participants by Course

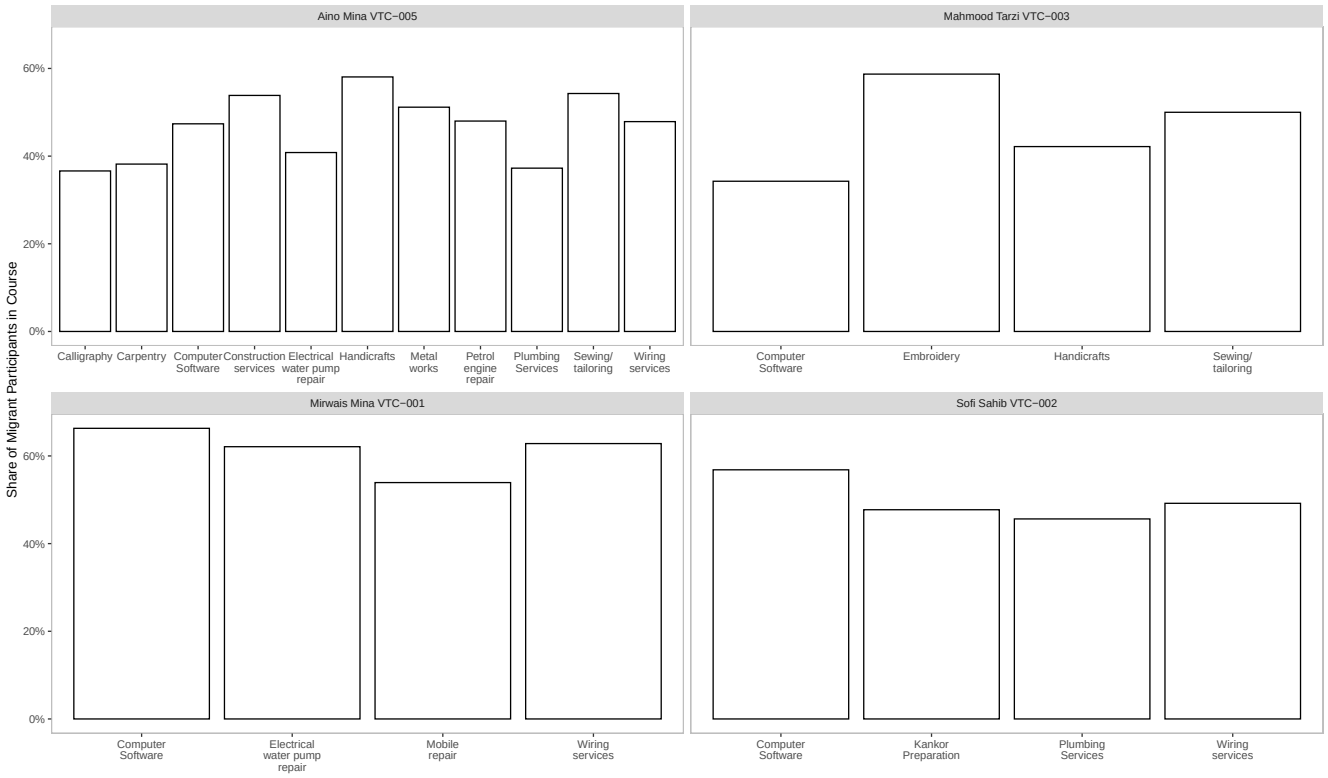


Figure S3: Proportion of migrant participants by course across the four VTCs.

This section shows the proportion of migrant 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 migrant participants, we examine subgroup treatment effects of TVET courses in which migrants were a minority (less than 45%), a majority (more than 55%), or balanced (between 45–55%) relative to locals.

S5 History of Displacement and Pre-Intervention Levels of Interaction

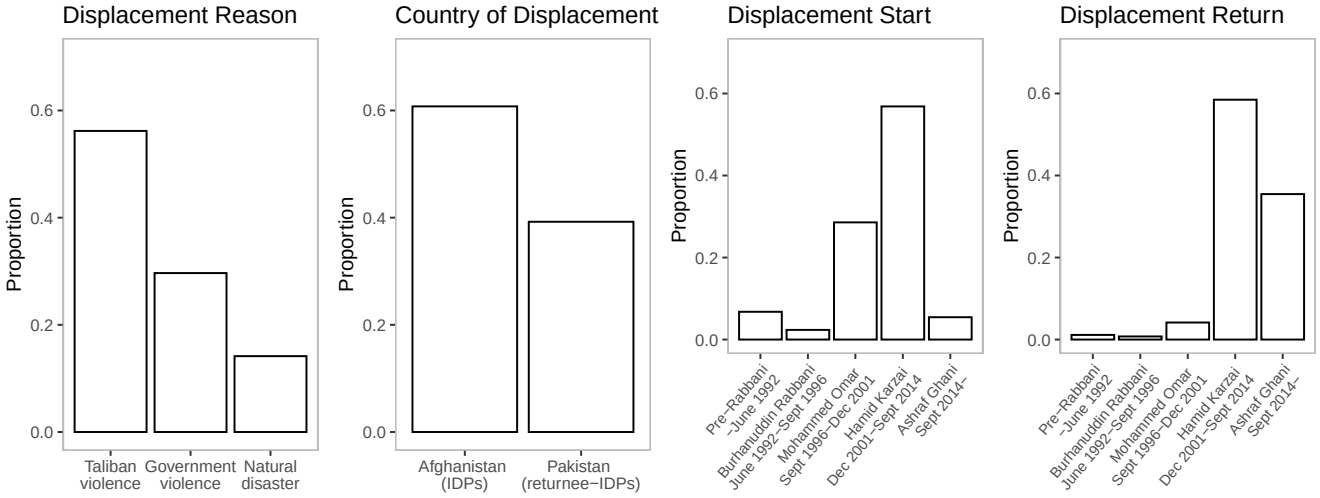


Figure S4: Displacement history among the migrant participants.

Figure S4 gives an overview of the displacement history among our migrant 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.

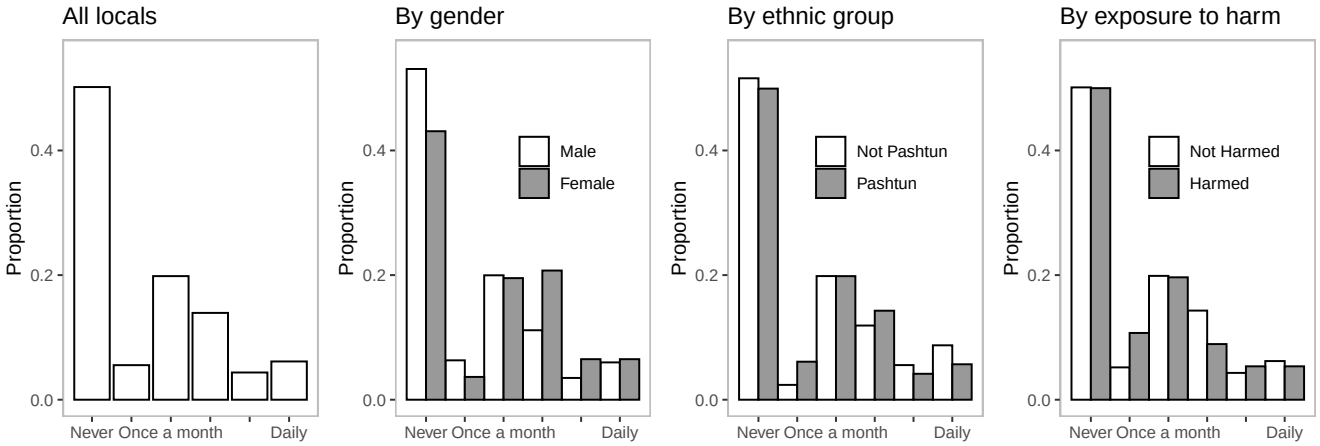


Figure S5: Among local participants, frequency of interaction with migrants at baseline.

Figure S5 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.

S6 Additional Details on Research Design

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 (Lyall, Zhou and Imai, 2020). Section S15 shows that neither UCT nor UCT-TVET treatments had any effects on attitudinal change at either endline.

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

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 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).

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

S6.1 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:

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

S7 Study Timeline

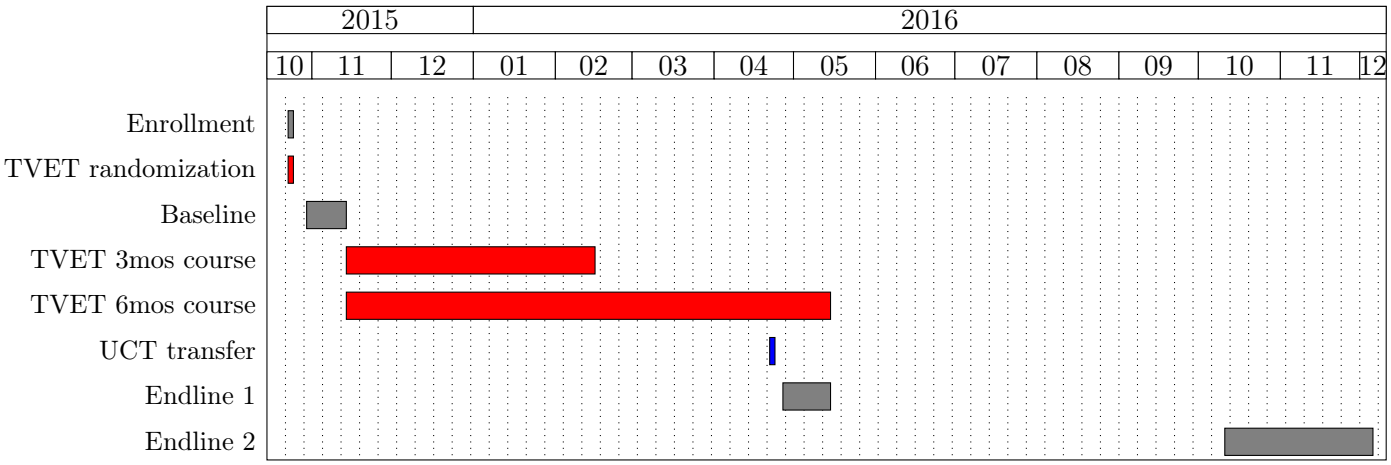


Figure S6: INVEST study timeline

S8 Balance By Treatment Group and Displacement Status

Baseline covariate	TVET Control-Migrants Mean	TVET Treatment-Migrants 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.

S9 Additional Research Ethics Considerations

When designing and conducting this research, we were mindful that our participants (locals and migrants) were a vulnerable population. We had an ethical responsibility to minimize harm, respect

autonomy, and make sure that benefits were fairly distributed.[7] 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. We properly de-identified, stored, and encrypted the data. Our study received institutional review board (IRB) approval from XXX and YYY .

[7]Please refer to the 2020 APSA Principles and Guidance for Human Subjects Research: `https://connect.apsanet.org/hsr/principles-and-guidance/`

S10 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 2 in the paper (Benjamini and Hochberg, 1995). The two estimates that were negative and statistically significant: perception of migrants for locals in majority migrant classes at Endline 1, and perception of migrants 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 Migrants	ref_interaction	0.032	0.229	0.445	0.490
Minority Migrants	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 Migrants	ref_perception	-0.306	0.150	0.021	0.356
Minority Migrants	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 Migrants	ref_violence	-0.180	0.135	0.091	0.356
Minority Migrants	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 Migrants	ref_dim_employment	0.130	0.141	0.178	0.356
Minority Migrants	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 Migrants	ref_dim_resources	-0.092	0.129	0.238	0.357
Minority Migrants	ref_dim_resources	0.013	0.125	0.458	0.490
Balanced	ref_dim_resources	0.115	0.153	0.226	0.356

Table S2: 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 Migrants	ref_interaction	0.255	0.191	0.092	0.346
Balanced	ref_interaction	0.078	0.166	0.320	0.398
Minority Migrants	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 Migrants	ref_perception	0.106	0.172	0.268	0.398
Balanced	ref_perception	-0.426	0.155	0.003	0.088
Minority Migrants	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 Migrants	ref_violence	0.116	0.173	0.251	0.398
Balanced	ref_violence	-0.044	0.163	0.393	0.421
Minority Migrants	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 Migrants	ref_dim_employment	-0.089	0.195	0.324	0.398
Balanced	ref_dim_employment	0.021	0.170	0.451	0.466
Minority Migrants	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 Migrants	ref_dim_resources	-0.248	0.186	0.092	0.346
Balanced	ref_dim_resources	0.080	0.162	0.311	0.398
Minority Migrants	ref_dim_resources	-0.274	0.191	0.076	0.346

Table S3: Adjusted p-values using Benjamini-Hochberg for ITT prejudice outcomes for Endline 2.

S11 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).

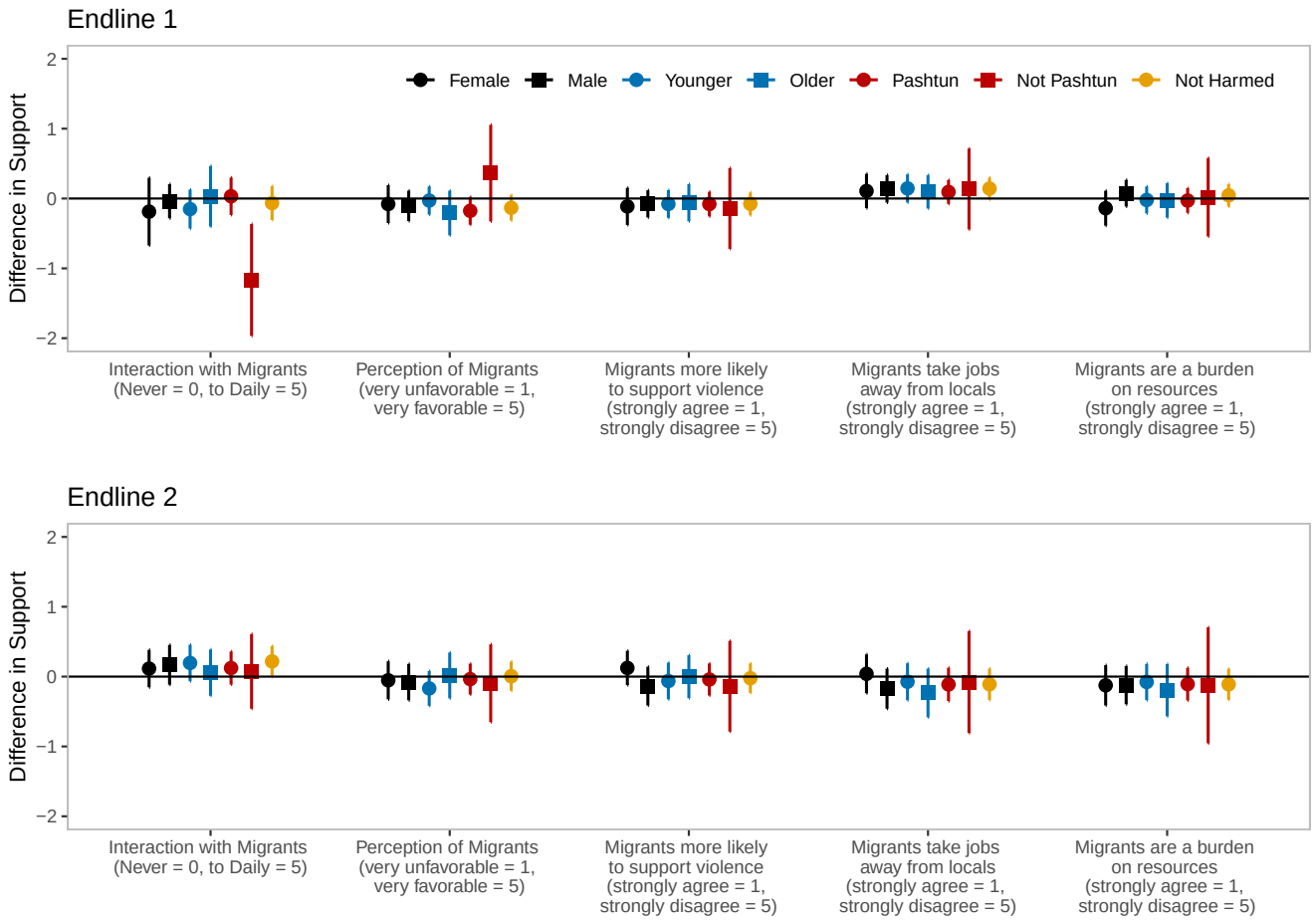


Figure S7: Intention-to-Treat effects of TVET at Endline 1 (top panel) and 2 (bottom panel) of migration attitude outcomes by locals towards migrants, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

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.

S12 Instrumental Variables Analysis

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) (Angrist, Imbens and Rubin, 1996). 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 S8 shows the the complier average treatment effects (CATE). Results are substantively similar to the main findings.

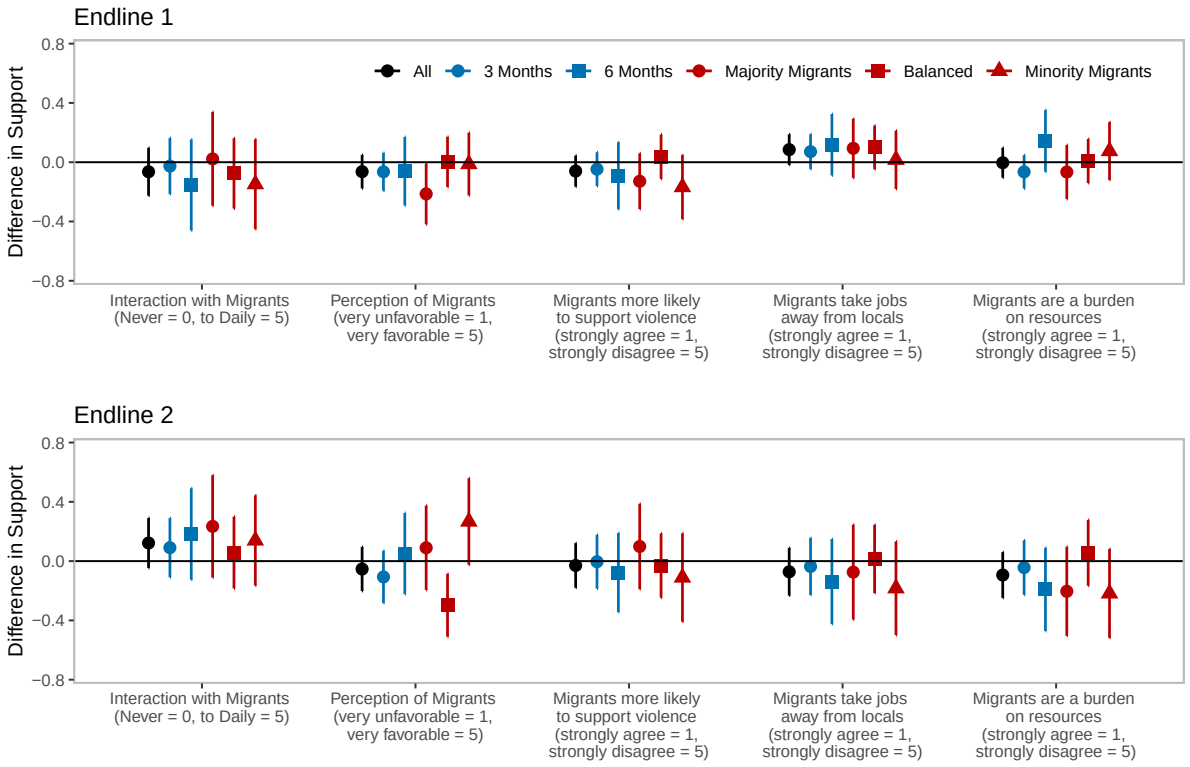


Figure S8: Instrumental Variables Endline 1 (top panel) and 2 (bottom panel) analysis of migration attitude outcomes by locals towards migrants, with 95% confidence intervals.

S13 Diagnosing Attrition: Comparing Endline 2 Participants and Endline 1 Participants who Attrited at Endline 2

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.

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. 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.

Baseline covariate	Endline 2 Migrants Mean	Attrition Group Migrants 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 S4: Balance Tests comparing Attrition and Endline 2 Participants, by Displacement Status

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 migrant 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. 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. 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, in the next section we use the R package *mi* (Gelman and Hill, 2011) for multiple imputation to address concerns regarding attrition and nonresponse at Endline 1 and/or 2 (Little and Rubin, 2002). We do not use Lee bounds analysis (Lee, 2009), because it requires the assumption of monotonicity: treatment assignment can only affect sample selection in one direction.

S14 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 attitudes 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 S9 shows the analysis using multiple imputation. Results are substantively similar to

the main findings.

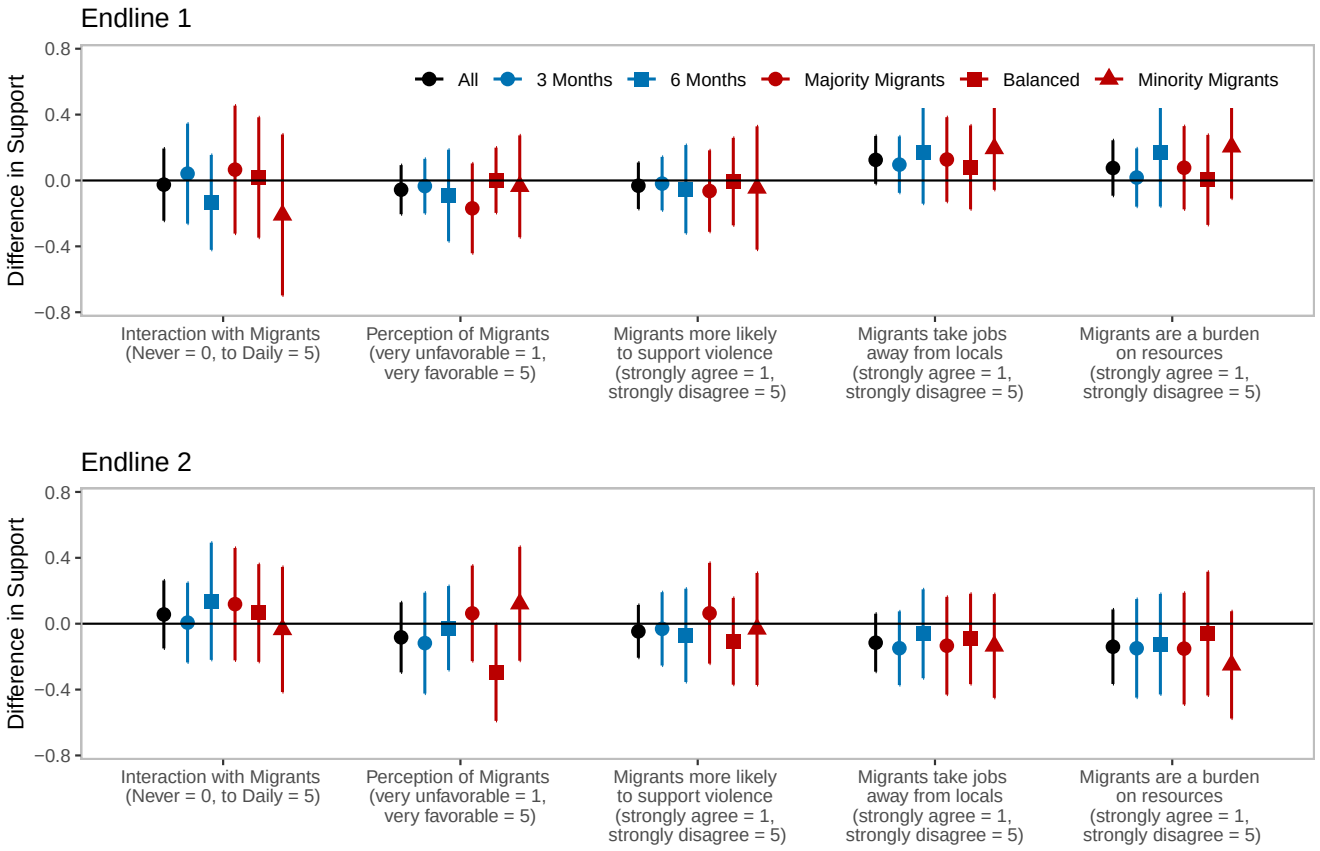


Figure S9: Intention-to-Treat Endline 1 (top panel) and 2 (bottom panel) analysis of migration attitude outcomes by locals towards migrants using multiple imputation, with 95% confidence intervals.

S15 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: the Unconditional Cash Transfer (blue) and the interaction of the two, UCT conditional on TVET (red). The other treatment groups similarly had no effects on migration attitude outcomes.

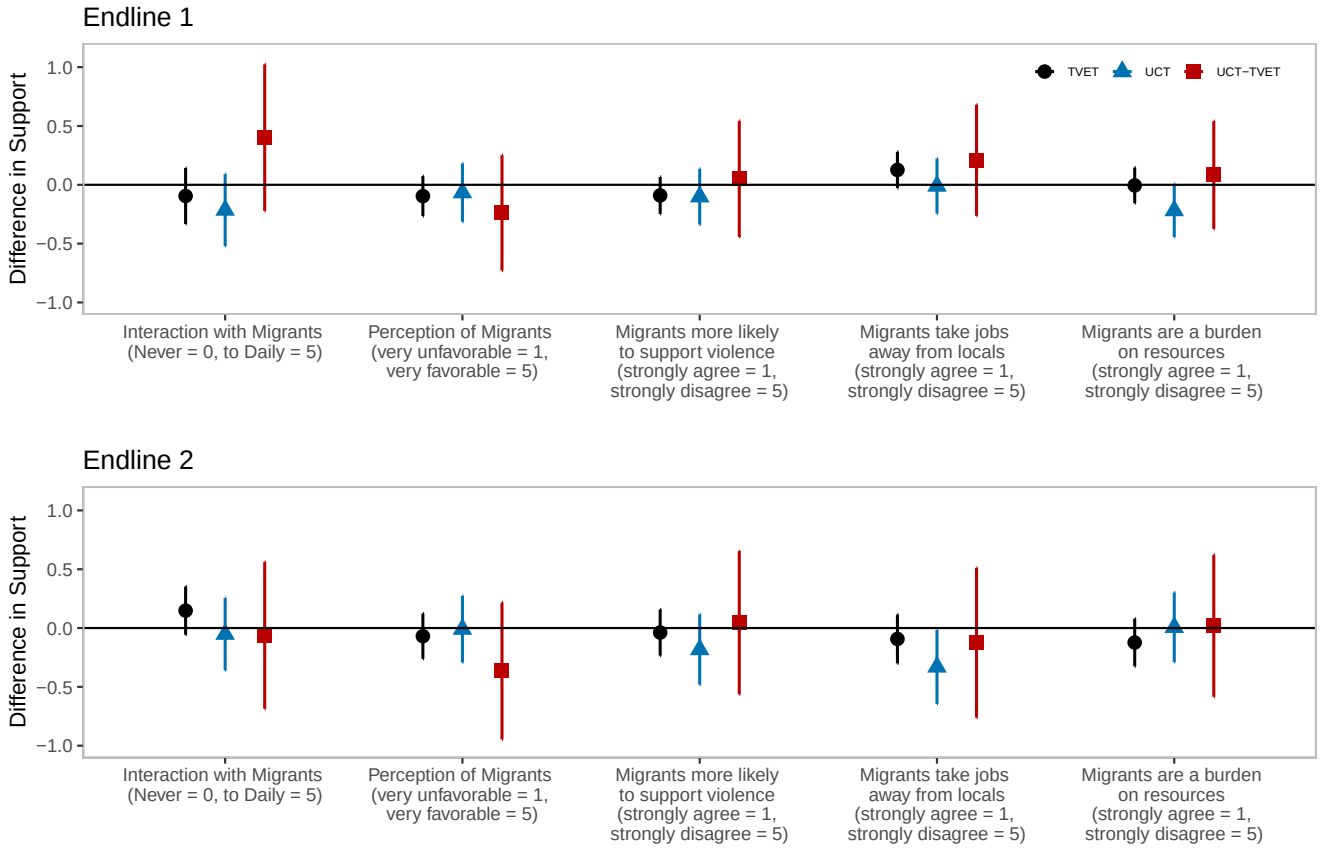


Figure S10: Intention-to-Treat Endline 1 (top panel) and 2 (bottom panel) analysis of migration attitude outcomes by locals towards migrants, with 95% confidence intervals. Positive (negative) values indicate more inclusive (more exclusionary) responses.

References

Angrist, Joshua, Guido Imbens and Donald Rubin. 1996. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 91(434):444–455.

Beelmann, Andreas and Kim Sarah Heinemann. 2014. "Preventing prejudice and improving intergroup attitudes: A meta-analysis of child and adolescent training programs." *Journal of applied developmental psychology* 35(1):10–24.

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.

Gelman, Andrew and Jennifer Hill. 2011. "Opening Windows to the Black Box." *Journal of Statistical Software* 40.

Harpviken, Kristian. 2009. *Social networks and migration in wartime Afghanistan*. Springer.

Hartman, Alexandra and Benjamin Morse. 2017. "Violence, Empathy, and Altruism: Evidence from the Ivoiran Refugee Crisis in Liberia.". `https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2769889`.

Internal Displacement Monitoring. 2017. Going "Home" to Displacement: Afghanistan's returnee-IDPs. Technical report. `http://www.internal-displacement.org/sites/default/files/inline-files/20171214-idmc-afghanistan-case-study.pdf`.

Internal Displacement Monitoring. 2019. Global report on internal displacement. Technical report. `https://www.internal-displacement.org/global-report/grid2019/`.

Internal Displacement Monitoring. 2020. A Different Kind of Pressure: The cumulative effects of displacement and return in Afghanistan. Technical report. `https://www.internal-displacement.org/publications/a-different-kind-of-pressure-the-cumulative-effects-of-displacement-and-return-in`.

Lakhani, Sadaf and Rahmatullah Amiri. 2020. Displacement and the Vulnerability to Mobilize for Violence: Evidence from Afghanistan. Technical report. `https://www.usip.org/sites/default/files/2020-01/pw_155-displacement_and_the_vulnerability_to_mobilize_for_violence_evidence_from_afghanistan.pdf`.

Lee, David S. 2009. "Training, wages, and sample selection: Estimating sharp bounds on treatment effects." *The Review of Economic Studies* 76(3):1071–1102.

Little, Roderick and Donald Rubin. 2002. *Statistical Analysis with Missing Data*. 2nd edition ed. New York: Wiley.

Lyall, Jason, Yang-Yang Zhou and Kosuke Imai. 2020. "Can economic assistance shape combatant support in wartime? Experimental evidence from Afghanistan." *American Political Science Review* 114(1):126–143.

Mousa, Salma. 2020. "Building social cohesion between Christians and Muslims through soccer in post-ISIS Iraq." *Science* 369(6505):866–870.

Norwegian Refugee Council. 2018. Escaping War: Where to Next? A Research Study on the Challenges of IDP Protection in Afghanistan. Technical report. https://www.nrc.no/resources/reports/escaping-war-where-to-next-the-challenges-of-idp-protection-in-Afghanistan/.

Paluck, Elizabeth Levy and Chelsey Clark. 2020. "Can Playing Together Help Us Live Together?" *Science* 369(6505):769–70.

Paluck, Elizabeth Levy, Seth A Green and Donald P Green. 2019. "The contact hypothesis re-evaluated." *Behavioural Public Policy* 3(2):129–158.

Scacco, Alexandra and Shana S Warren. 2018. "Can social contact reduce prejudice and discrimination? Evidence from a field experiment in Nigeria." *American Political Science Review* 112(3):654–677.

Tropp, Linda R and Mary A Prenovost. 2008. "The role of intergroup contact in predicting children's interethnic attitudes: Evidence from meta-analytic and field studies." *Intergroup attitudes and relations in childhood through adulthood* pp. 236–248.