Mental Health During Conflict? An RCT on Psychosocial Programming in Conflict-Affected Myanmar

Pre-analysis plan

Nicolas Cerkez *UCL*
Alexandra C. Hartman *UCL*
Jonathan Weigel *UC Berkeley*
Kyaw Zay Ya *Swiss TPH*

Abstract:

Introduction

In addition to its obvious direct effects on human welfare, conflict can cause a host of indirect effects—by fueling depression, PTSD, drug abuse, and domestic violence, and by disrupting communities' social cohesion. This project investigates how to improve mental health, resilience, and social cohesion in conflict-affected communities in rural Myanmar. One promising yet understudied approach is non-specialist, community-based mental health care delivery. We are conducting a randomized evaluation of one such community-based psychosocial program in war-torn villages in Kayin, Myanmar. The flagship Mental Health & Psychosocial Support (MHPSS) program of our implementation partner, Community Partners International, involves a simplified group-based curriculum focused on depression, post-traumatic stress disorder, and anxiety. In a second treatment arm, participants are invited to a "social capital" program—involving planting, basket-weaving, cooking, and other group activities—allowing us to examine if any causal effects of the MHPSS intervention are attributable to the curriculum or to the group-based nature of the program. We also cross-randomize a light-touch religious intervention—a non-sectarian prayer and meditation session delivered before each program session—to try to embed program content more closely in the local culture. The project will provide some of the first credible causal evidence on the clinical and cost effectiveness of non-specialist psychosocial first-aid programs in the midst of active conflict.

Hypotheses

We pre-register the following hypotheses regarding the impact of mental health interventions ("group counselling") and social group activities ("group solidarity") on participants' mental health and well being, their relationships with family and close members of their social networks, their social cohesion and sense of belonging, as well as their economic well-being.

Hypothesis 1 *People assigned to group counselling or group solidarity treatments have better mental health and well-being (including physical health and substance abuse).*

Hypothesis 2 *People assigned to group counselling or group solidarity treatments have improved relationships with family and close members of their social network.*

Hypothesis 3 *People assigned to group counselling or group solidarity treatments have improved social cohesion, and sense of belonging to their community.*

Hypothesis 4 *People assigned to the spirituality treatment experience an enhanced impact of group solidarity and group counselling on mental health, social relationships, and community belonging.*

Hypothesis 5 *People assigned to group counselling or group solidarity treatments have better economic well-being.*

For each of these five main hypotheses, we acknowledge the possibility of differential treatment effects between the two interventions. We remain agnostic about which of the interventions is more successful, leading to the following three "sub-hypotheses" for each of the primary hypotheses.

Hypothesis 6a *If people assigned to group counselling are better off in any of the outcome groups listed above than people assigned to group solidarity, this provides evidence that there is an additional benefit of the mental health curriculum (for said outcome).*

Hypothesis 6b *If people assigned to group counselling and group solidarity do not display any differential effects in any of the outcomes groups listed above, this provides evidence that the primary benefits of mental interventions come from social capital (for said outcome).*

Hypothesis 6c *If people assigned to group solidarity are better off in any of the outcome groups listed above, this provides evidence that the primary benefits of mental interventions come from social capital and work better in conflict contexts (for said outcome).*

Heterogeneity Analysis

We narrow our heterogeneity analysis down to four dimensions: gender, religion, educational attainment, and exposure to violence.

Research design

The set-up of the RCT is visualized in Figure 1. The study contains 90 communities, in each of which we interviewed 13 individuals in a baseline survey conducted at the end of 2022. These 90 villages were then randomly assigned to either treatment or control status, resulting in 30 group counselling, 30 group solidarity, and 30 control villages. Within the 30 group counselling and 30 solidarity villages, a second stage randomization assigned half of the units to the spiritual (religious) intervention. One to two months after the intervention, which is 10 weeks in duration, is over, an endline survey is conducted. The rest of this section explains each component of the research design in more detail.

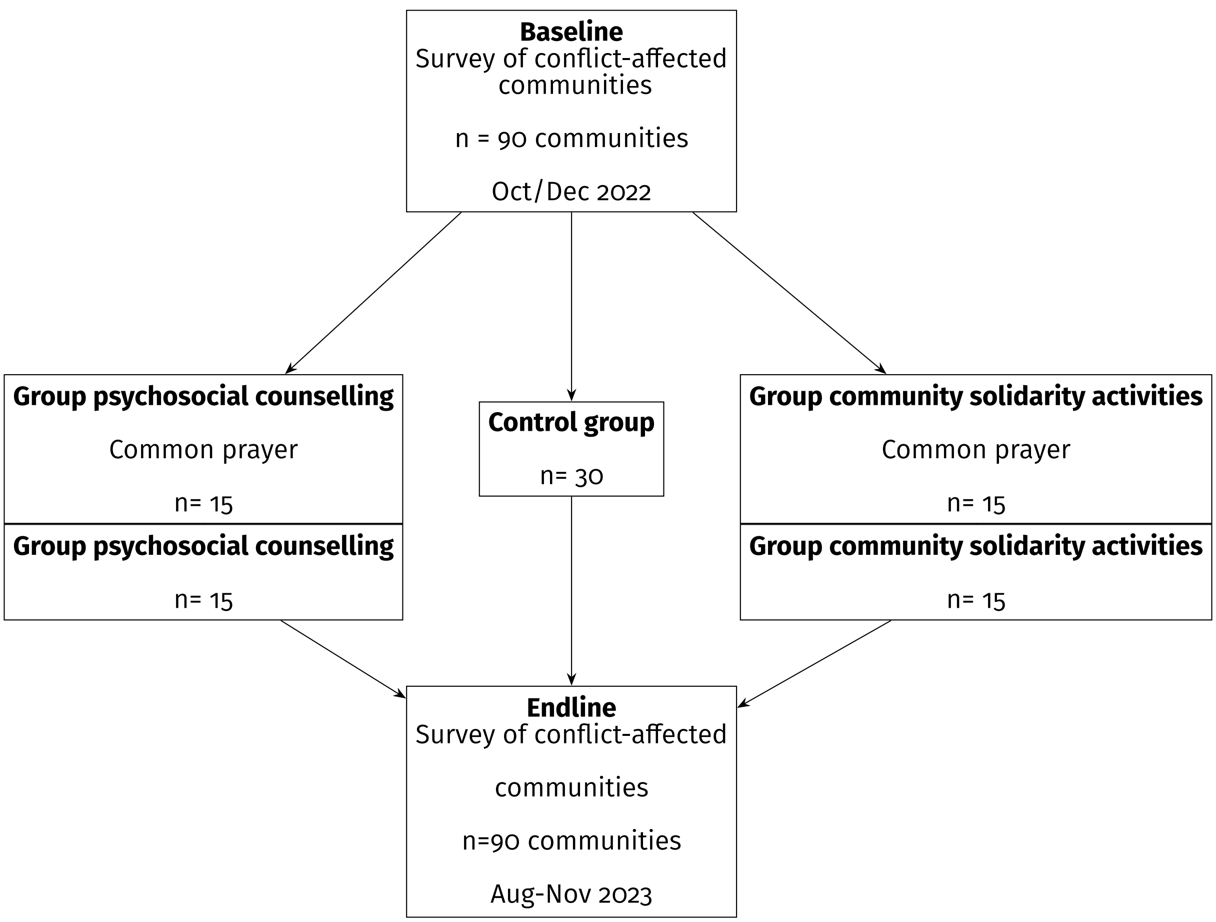

Figure 1: Visualisation of the RCT treatment arms

Study sample and panel survey

The study was conducted in Kayin State in Myanmar, a state neighboring Thailand. It takes place in three districts and five townships within Kayin, all of which are areas not controlled by the central government. In addition to experiencing the February 2021 coup, Kayin has a long history of conflict with the central government, long demanding Kayin to be an independent state and, since 1976, a federal system, thus lending itself well to a study of mental health in conflict affected areas. Figure 2 displays our study areas.

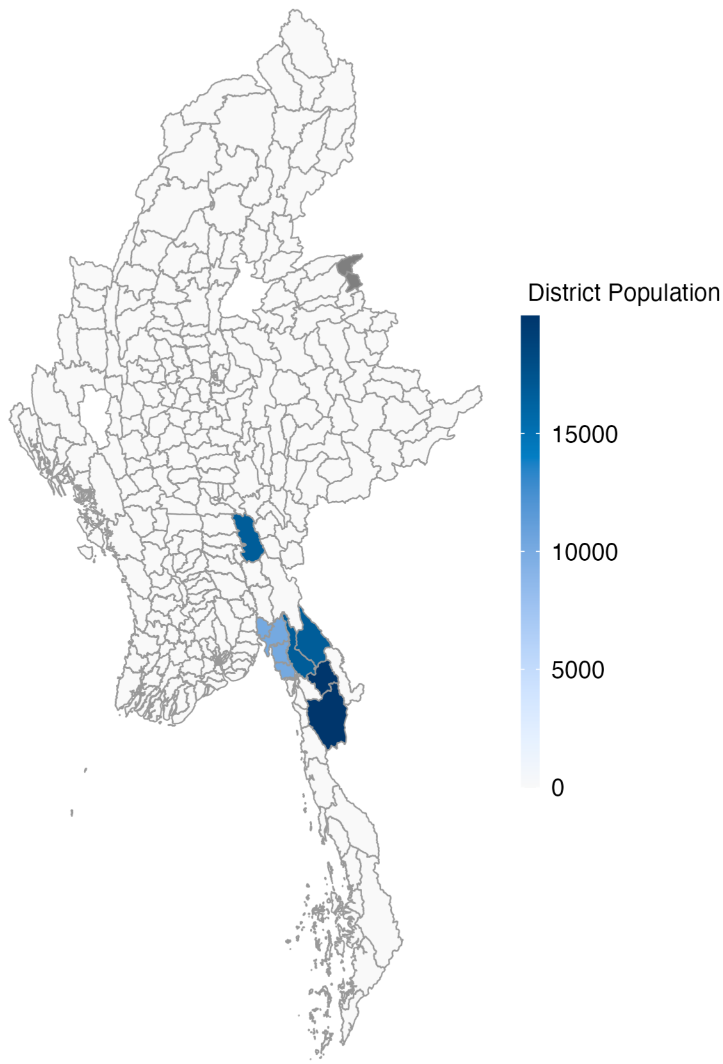

Figure 2: Map of study areas

The 90 communities within these areas were chosen based on accessibility. Given the ongoing conflict, our local implementing partner KEHOC selected villages that were deemed to be accessible and safe to enumerators and facilitators of the program. Within each community, a total of 13 individuals were selected to be part of the study, leading to a sample size of 1170 individuals. Recruitment of these individuals was also conducted by our local partner KEHOC. Specifically, KEHOC employees visited each local community and, with help from local leaders, advertised our study. Inhabitants were then allowed to sign up to the program, providing us with lists of interested individuals in each village. The 13 selected participants were the 13 individuals who first signed up, with the assumption that these would be individuals least likely to drop out of the program. Individuals not selected to be part of the program were kept on file for replacement purposes. Furthermore, they are tagged to receive potential future aid first should we be able to scale the program up after this initial RCT.

The baseline data collection was from Oct to Dec 2022 with no major interruptions, notwithstanding the conflicts. However, in January and February 2023, (very few) individuals who were interviewed in our baseline sample already started moving to Thailand in search for better jobs. Given that the intervention had not yet started, we replaced these individuals with the individuals on the "replacement list" described above. Once the intervention started, we did not replace individuals, unless they dropped out within the first three sessions of the program.

The endline data collection is nearing its end at the time of writing (Nov 2021). The precise duration of endline data collection is subject to the changing nature of the ongoing conflict.

Instrument and intervention

Group counselling. Group counseling programs have the potential to be more cost-effective and logistically feasible in our study environment. The program will include the services that can be delivered by lay trained providers/facilitators at the community level, primarily through a counseling approach to strengthen beneficiaries' problem-solving skills and overall psychosocial well-being. The program will cover the following ten topics over ten weeks:

1. Introduction and overview of mental health
2. Stress
3. Depression and anxiety
4. Resilience and self-care
5. Stress management

6. Hooking, unhooking and grounding
7. Alcohol/substance use problem
8. Problem solving technique
9. Strengthening social support
10. Practicing together

The group facilitators will be trained extensively prior to the implementation period and provided a detailed facilitation manual, created by doctors and other medical professionals. The intervention at large is a version of the well-known CETA program, modified to fit our context.

Solidarity group. Some communities in Kayin organize solidarity groups that resolve issues facing the group and/or wider community by discussing and sharing coping strategies. According to local experts, there is latent demand in many conflict-affected areas in Kayin for such groups. In other words, there is a collective action problem that prevents individuals with psychosocial needs from finding one another and forming potentially supportive bonds. The idea of the solidarity group treatment is to solve this problem.

The solidarity groups activities will be conducted, also over ten weekly sessions, in a safe place to foster discussion of issues participants raise without a specific curriculum. In the first session, the group will be introduced and told about the three activities they can choose to do in this program (cooking, handicraft, and planting or environmental cleaning). Participants then schedule the meeting times for the following nine sessions and decide what activity to conduct in what session.

Control group. There are no activities planned in control villages.

Spiritual intervention. The spiritual intervention will, in the selected groups, make up 15 minutes of each session. Specifically, each session will commence with 7-8 minutes of silence, followed by a "prayer for others" of about 7-8 minutes. The idea behind the spiritual intervention is that it, given the stigma associated with issues surrounding mental health in Myanmar, adding this "religious" component to the program can help legitimize it in the eys of the participants.

Data

We complement our collected baseline and endline data with data on violence external sources.

Self-collected data. The baseline and endline surveys - our self-collected data - are roughly one hour long surveys containing various modules for our outcomes (described in more details below) as well as for various control variables (e.g. demographics).

Violence data. While we directly ask respondents about their exposure to conflict in their life, we complement this information with data from the ACLED database as well as data from EXERA. The former of these is widely used in academia as a database describing "conflict events" all over the world, while the latter focuses only on Myanmar.

Compliance Measures

We gather variables describing individuals' compliance with the program, viewing compliance through a very broad lens. On the one hand, we get a straight forward measure of compliance by measuring attendance of each participant by session. This measure will be recorded by the facilitator of the program.

On the other hand, compliance can be viewed as a mechanism. Specifically, we aim to measure (i) engagement of the participant (by session) as measured by the facilitator, and (ii) self-reported engagement. In other words, the degree to to which an individual participates in the program may be correlated with character traits of said individual, something these "engagement-measures" allow us to explore.

Outcome variables

We will study the following sets of outcomes.

- Mental health and well-being. We will measure our primary outcomes in several ways. First, we will use standard survey-based diagnostics of mental illness. Our baseline survey, however, showed that these measures suggest that our population has almost no mental disorder. This is contrary to existing evidence which suggests that conflict settings amplify the occurrence of mental illnesses. Mental health in Myanmar, various mental health experts have told us, is highly stigmatized and we therefore posit that these standard survey measures may be susceptible to social desirability bias. Hence, we will supplement these standard tools as follows. We will make use of audio—instead of enumerators— to guide participants through the mental health questions in the survey, thus alleviating the possibility that respondents feel stigmatized or ashamed to answer honestly. We further make use of a list experiment. We will also look at effects on alcohol, tobacco, and drug consumption, as well as at a broad array of self-reported physical health indicators, which may be less subject to stigma and yet still potentially responsive to treatments through a mental health channel. Finally, we consider outcomes on self-efficacy (optimistic self-belief), life satisfaction, and locus of control. These variables are again correlated with mental illness, but likely less subject to stigma.

- Relationships with family and members of the network. We will measure the closeness and networks of individuals through surveys and social network modules. We intend to look at both dimensions as outcomes: the closeness of those relationships and the number of relationships respondents report. Furthermore, we explicitly look at intra-household relationships by analyzing the trust and communication style of the respondent and their spouse.

- Social cohesion and sense of belonging. We will measure these outcomes using survey questions as well as behavioral measures from an endline debriefing meeting called by the village head and facilitator. We will examine participation in this meeting as an indicator of the social cohesion and level of civic engagement in the village. We will also examine the inclusiveness of the meeting as measured by the number of different people who speak, especially among women and minority respondents.

- Economic effects. To measure potential economic impacts of the treatments, we will rely on survey-based proxies of labor supply (especially hours worked on the farm), consumption, and income. We will also look at impacts on cognitive skills, such as digit span.

Statistical analysis

To test the primary and secondary hypotheses mentioned above, we rely on various econometric methods that allow us to infer causal relationships. For all analyses, we focus on those respondents that participate in both the baseline and endline surveys.

Intention-to-treat analysis

For outcomes that we observe both at baseline and endline, we use intent-to-treat (ITT) analysis, estimating the following ANCOVA specification:

$$y_{i,EL} = \alpha + \beta_1 solidarity_i + \beta_2 psycho_i + y_{i,BL} + \delta_s + \varepsilon_i, \tag{1}$$

where $y_{i,EL}$ and $y_{i,BL}$ denote the outcomes at endline and baseline, respectively. The different outcomes of interest described in the previous section. $solidarity_i = 1$ if the respondent belongs to the static information group; $psycho_i = 1$ if the respondent belongs to the dynamic information group; and $solidarity_i = psycho_i = 0$ if the respondent belongs to the control group. δ_s are strata fixed effects and standard errors are clustered at the village level.

As mentioned, some outcomes are only observed at endline due to changes in the survey after baseline data collection. In that case, we slightly amend (1) by dropping $y_{i,BL}$ or, if possible, by constructing a "quasi-baseline" value of the outcome, $\tilde{y}_{i,BL}$. As an example, if the outcome is a variable only asked at endline, we may use the GAD-7 survey instrument – a widely used measure of anxiety consisting of seven questions which we ask at baseline and endline – as the "quasi-baseline" control (as we observe this measure also at baseline).

To maximize statistical power, we also use a simplified version of regression (1) where we code a single binary instrument indicator $instrument_i = 1$ if either $psycho_i = 1$ or $solidarity_i = 1$.

Instrumental-variables analysis

We complement the ITT analysis with an instrumental-variables (IV) analysis, where we instrument an individual's compliance with the intervention using the $instrument_i$ variable just defined above. The compliance measures we rely on are (i) attendance measured by the facilitator, (ii) engagement measured by the facilitator, and (iii) self-reported engagement. We denote compliance by the variable D_{ij}, where j represents one of the three different compliance measures.

The first stage therefore takes the following form:

$$D_{ij} = \alpha + \tau_1 instrument_i + \delta_s + \epsilon_i, \tag{2}$$

where all the variables were defined and standard errors are still clustered at the village level. The first stage allows us to calculate three $\widehat{D}_{ij}$ for $j = \{\text{attendance, engagement, self-reported engagement}\}$.

Note that we also estimate separate first stage regressions for the $psycho_i = 1$ and $solidarity_i = 1$ instruments, sub-setting the analysis on the dynamic instrument and control group, and the static instrument and control group, respectively.

In the second stage, we regress our outcome measure $y_{i,EL}$ from (1) on each of the three $\widehat{D}_{ij}$ separately. In the case of the pooled instrument $instrument_i$ the second stages is:

$$y_{i,EL} = \gamma + \tau_2 \widehat{D}_{ij} + y_{i,BL} + \delta_s + \varepsilon_i, \tag{3}$$

where all variables as well as the clustering are as defined above. If we don't observe $y_{i,BL}$, we either drop the variable completely or, where applicable, construct a "quasi-basline" value of the outcome.

We complement this pooled analysis with separate second-stage regressions for the dynamic information (and control) group and the static information (and control) group.

Heterogeneity Analysis

We conduct heterogeneity analyses for each dimension heterogeneity we investigate (gender, education, religion, and exposure to violence) for both ITT and IV. To do so, we either run the analysis as described above but for different sub-samples (e.g. for women only) or by interacting the hetergeneity dimension with the treatment dummies. To be clear, in the latter case, the ITT analysis (1) for the pooled treatment case would become

$$y_{i,EL} = \alpha + \xi_1 treat_i + \xi_2(heterog_i \times treat_i) + \xi_3 heterog_i + y_{i,BL} + \delta_s + \varepsilon_i, \tag{4}$$

where $heterog_i$ is any of the four dimensions of heterogeneity (e.g. gender) and $treat_i$ is a dummy that equals one if individual i lives in any treatment village.

Threats to inference

Individual drop out To address any issues that may arise from differential attrition rates, we first check whether we actually have differences in attrition between treatment and control groups. Furthermore, we plan on implementing the procedure proposed in Lee (2009) and calculate bounds on our treatment effects given the attrition rates. Lastly, given that the setting of our study is conflict ridden, we are documenting the reasons why individuals are moving meticulously. Given that we have their phone numbers we can – conditional on being able on reach them and/or them not changing their phone number – conduct a qualitative interview survey with them and gain insights into their decisions to leave. Overall we therefore address individual level attrition using both quantitative and qualitative methods.

Community drop out A concern specific to our setting is the possibility that full villages drop out of the study because they are no longer accessible by facilitators and/or enumerators due to the ongoing conflict. Recall that we have 90 villages in our study, eight of which have over 1000 inhabitants. Before we randomized villages into their treatment status, we identified these eight large villages as replacement villages. Simply put, the idea is that since our groups are relatively small with only 13 participants, we can easily have two groups of 13 participants within a large village without any concerns of spillover effects. Therefore, if a community drops out *prior* to the start of the intervention, it can be replaced. If a community drops out during of after the intervention, we drop the village from the sample.

Ethical considerations

This study is covered by IPA IRB number (15186) and a local IRB by Community Ethics Advisory Board (CEAB) based in Thailand (CEAB-2022-010). Furthermore, the study obtained IRB approval from UCL for secondary data analysis, as the raw data is handled by local partners and the PIs only see anononymized data.

References

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