Journal of Development Economics

The Value of Landmine Awareness Programs in Conflict Settings: Evidence from Myanmar

--Manuscript Draft--

Manuscript Number:	DEVEC-D-25-02675R1
Article Type:	Registered Report Stage 1: Proposal
Section/Category:	Experimental Papers, credit, insurance
Keywords:	Landmines; conflict; Displacement
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Response to Reviewers:	

Cover page

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Registered Report Stage 1: Proposal

The Value of Landmine Awareness Programs in Conflict Settings: Evidence from Myanmar

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Date of latest draft: 1/23/2026

Abstract

Landmines threaten not only physical safety but also the livelihoods of communities in conflict-affected areas. Contamination limits mobility, agricultural activity, and access to essential services, especially for vulnerable populations. While landmine clearance is ideal, it is often infeasible amid ongoing conflict, leaving open the question of how to help residents cope with mine contamination. This study evaluates the Explosive Ordnance Risk Education (EORE) program in Myanmar through a randomized controlled trial. The intervention provides information on local mine risks to improve avoidance behavior and reduce injuries. Despite the program's intuitive appeal, rigorous evidence on its effectiveness remains scarce. Beyond direct safety benefits, improved information could enhance access to land and natural resources, bolster income and food security, and reduce psychological stress. However, gains may be limited if economic and infrastructural constraints prevent households from safely capitalizing on the knowledge provided.

Keywords: landmines, conflict, displacement

JEL codes: O1, D8, I1, I3, J6

Study pre-registration: https://www.socialscienceregistry.org/trials/17047

Proposed timeline *(required)*

December 2025: Interventions complete.

March 2026: Endline data collection complete.

March 2027: Anticipated Stage 2 submission.

1. Introduction

Research question: background, importance and relevance

- What is the main problem/question motivating the study? Why is this question important for the field of development economics?
- How has this question been addressed thus far in the relevant literature? What are the competing theories for explanation of this question? How is this study different from prior research on this problem/question?

Landmines pose a significant threat not only to physical safety but also to the socioeconomic well-being of communities in conflict-affected regions. Living near landmines may restrict agricultural activity, impede mobility, and diminish access to essential services such as schools and markets. Individuals living near landmines are often those with recent experiences of conflict and are typically among the most vulnerable in the country. Barring landmine clearance—which is not always possible or authorized, especially in settings of ongoing conflict—increasing awareness and salience of explosives contamination could help affected populations avoid and cope with landmines in their areas.

We study the economic and health impacts of the Explosive Ordnance Risk Education (EORE) program, the gold-standard landmine risk education program implemented by several mine action organizations throughout the world, in Myanmar.[1] Because of intense civil conflict ongoing since 2021, Myanmar is now ranked among the most heavily landmine-contaminated countries in the world. Understanding the impacts of EORE on beneficiaries is crucial for cost-benefit analysis and for effective targeting of the program, but to our knowledge no rigorous evaluations of EORE have been conducted.

There has been little economics research on landmines, and the extant literature has focused on the impacts of landmine exposure or clearance. Key findings are that landmine contamination is harmful to both health and economic outcomes (Merrouche, 2008; Arcand et al., 2015), and that landmine clearance spurs economic activity, largely by increasing market access (Chiovelli et al., 2025; Prem et al., 2025). In contrast, little is known about both the health and economic impacts of mine risk education: while several studies have demonstrated immediate impacts on knowledge, none has rigorously documented impacts on injuries or economic outcomes (Shabila and Saleh, 2024).

Our hypothesis that EORE programs may impact economic outcomes is based on the positive correlation between household exposure to EORE programs in landmine affected areas and labor market outcomes observed in the Multi Sectoral Needs Assessment (MSNA) surveys conducted by UNOCHA in Myanmar (see Table 1). The dependent variable in Columns 1 and 2 captures whether households reported experiencing mobility restrictions in the 2023 and 2024 MSNA rounds. Column 3 presents a household-level indicator equal to 1 when the household has at least one unemployed member actively seeking work;

[1] One major mines organization, Mines Advisory Group, delivered 32,000 risk education sessions in 2022, reaching almost half a million beneficiaries.

this unemployment measure is available only in the 2023 round.[2] All specifications are restricted to households that encountered mines, IEDs, or other explosive hazards at any point in the 12 months preceding the survey. Each model also includes fixed effects for displacement status to account for significant heterogeneities in mobility and employment outcomes across displaced and host populations.

While only suggestive, the table highlights a strong negative association between mobility challenges and receipt of EORE services among households affected by explosive contamination. The 2023 data further suggest a possible link between exposure to EORE and employment prospects in such areas. These correlations, however, do not indicate casual effects, as mobility and employment outcomes are likely influenced by non-random program placement of EORE programs, and confounding from other humanitarian or development interventions delivered alongside EORE.

Table 1: Correlation between EORE exposure, mobility, and unemployment

	(1)	(2)	(3)
	Mobility Restrictions 2023	Mobility Restrictions 2024	Unemployment 2023
EORE=1	-0.234***	-0.174***	-0.110***
	(0.069)	(0.044)	(0.042)
N	1380	4475	1382
Clusters	327	935	327
R^2	0.039	0.032	0.064

Notes: Standard errors clustered at admin 4 level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All specifications include displacement group fixed effects – accounting for whether a household is an internally displaced population (IDP), returnee, non-displaced stateless or a conflict affected non-displaced host.

2. Research Design

Hypotheses

- What are the main outcomes of interest? Which outcomes are primary to the analysis, which are secondary, and why?
- How will the main outcomes of interest be defined in your dataset? If applicable, how will they be aggregated?
- Please include all hypotheses which will be tested, linking each outcome specifically to how it will be measured. These should be reported as main results in the Stage 2 submission.

[2] Both variables are unavailable in the 2025 MSNA round.

We chose three main outcomes with clear theoretical links to improved knowledge about landmine contamination, which are welfare relevant, and which are relatively easy to measure:

M1. Employment status
M2. Land area cultivated
M3. Injury or death from landmines

Our secondary outcomes include intermediate outcomes (mechanisms) and harder-to-measure final outcomes like income and consumption:

S1. Exposure to the landmine awareness program
S2. Beliefs about landmine contamination and the risks of travel around their area.
S3. Plans to stay at the current location and migrate or commute for work
S4. Household income, including from wages (measured at the individual level), business profits (measured at the business level), and farming profits (measured at the household level)
S5. Household consumption (measured at the category level for the following categories: food, rent + transport, household items, healthcare, education).
S6. Psychological well-being (measured using PHQ-4, which has been used to measure mental health in Myanmar; see Saw et al., 2023).

We will test the following hypotheses:

- Treatment will increase labor force participation (M1)
- Treatment will increase M2
- Treatment will reduce M3
- Treatment will increase S1
- Treatment will increase confidence in and accuracy of knowledge of landmine contamination nearby (S2)
- Treatment will increase plans to migrate or commute for work (S3)
- Treatment will increase S4 and S5
- Treatment will create spillovers within villages (increase in S1 even for those not attending the session)

Basic methodological framework / identification strategy

- What is the basic methodological framework of the study (RCT, pre-post, simple comparison, difference-in-difference etc.)? Why is it suitable to address this research question?

We will use a cluster-randomized controlled trial, combined with pre- and post-intervention household surveys, to study impacts of the EORE program. This method is suitable to our question because it introduces random variation in exposure to EORE (which would be difficult to reconstruct in a quasi-random way as EORE delivery is strongly correlated with landmine exposure).

Treatment is assigned at the village tract (admin 4) level, stratified by state, availability of existing MSNA survey data, and average village consumption.

Intervention

- What type of an intervention does the study involve[3]? Elaborate in detail when, where and by whom it will be delivered. Please provide sufficient detail to allow for replication in line with this journal's Mandatory Replication Policy.
- How will individual observations be assigned to treatment and control conditions[4]?
- How is participation in the program defined for the purpose of your study?
- Are there multiple treatment arms involved and if so, are they exclusive or overlapping?
- What is the source of exogenous variation in your study?
- If applicable, what observations will be blinded (masked)[5] after assignment to interventions and how? If blinding is not possible, what measures will be taken to minimize the potential for performance and expectancy biases (e.g. keeping participants unaware of trial hypotheses, measuring participant and provider expectations of benefit at baseline, etc.)?
- The instructions and supporting materials for the administration of the intervention should be included as an appendix.

Our intervention is a landmine awareness program implemented by Community Safety Partnerships (CSP), an organization that helps coordinate the mine action activities of six international mine action organizations (MAG, Handicap International, Danish Refugee Council, Danish Church Aid, Norwegian People's Aid, and HALO Trust) as well as many local mine action organizations in Myanmar.

CSP's EORE toolkit was originally developed by the Myanmar Mine Risks Working Group under the erstwhile Ministry of Social Welfare, Relief and Resettlement. The development of the toolkit was underpinned by key findings from the 2013–2014 South-East Knowledge, Attitude and Practices (KAP) survey conducted by UNICEF and the 2014 Kachin/Northern Shan Rapid Assessment on the impact of landmines and other explosive remnants of war. The toolkit has been field-tested by local organization in 2015 across 7 states and regions of Myanmar.

EORE is primarily an awareness-raising and educational activity, designed to reduce injuries and fatalities from landmines and other explosive ordnance by improving people's understanding of risk, recognition of hazards, and adoption of safer behaviors. Its core function is educational and preventive, rather than technical or operational. The main components include risk awareness sessions, targeted messaging for high-risk behaviors like scrap metal handling or farming, distribution of materials and media, and

[3] For useful information on reporting standards for interventions, see Hoffmann et al. (2014).

[4] For useful information on what to report on randomization, see Bruhn and McKenzie (2009).

[5] Blinding or masking refers to methods of withholding information about assigned interventions post-randomization from those involved in the trial, when knowledge of this information could influence their behavior in a way that would later prove integral to interpreting the results.

coordination with actors who can respond where relevant. The intervention also includes a "community safety mapping," an innovative approach where communities discuss hazards in their vicinity and display maps, handouts, flyers and storyboards highlighting unsafe areas and safe routes to essential resources like schools, farmland, and water.[6] This intervention is designed to reduce landmine-related injuries while enabling safer access to economic opportunities, directly addressing the challenges faced by communities in mine-contaminated areas. CSP prioritized in-person EORE sessions in all the experimental locations (see Figure 1 for a map of sampled locations). As Myanmar remains an active conflict area, however, a sizeable number of the experimental locations subject to violent contestation (and significant landmine contamination) proved hard to reach. For those select villages, where physical access could not be achieved despite repeated attempts, CSP switched to delivering EORE through the phone. CSP has extensive prior experience with delivering EORE over the phone (in addition to delivering the program in-person). We anticipate that less than half of the locations who will receive EORE will receive it over the phone.

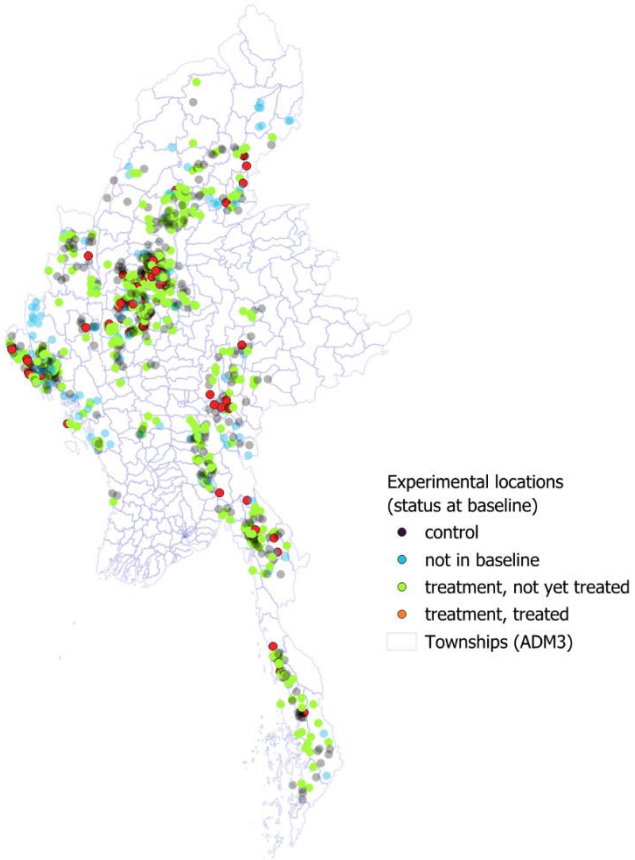
Treatment (i.e., delivery of EORE by CSP) began in June 2025 and ended by December 2025. The intervention is being delivered across Myanmar (see Figure 1). While we originally intended to engage 10 local partners to deliver the EORE program, challenges in mobility, conflict and disruptions caused by the recent earthquake, led us to engage with a larger number of local partners. Jointly with CSP, the team evaluated over 80 different partners and eventually worked with 15 partners to deliver the program across Myanmar.

Household-level participation will be measured at baseline and endline surveys. A direct measure of participation captures whether the household received an EORE program since June 2025 – the start of CSP's intervention in experimental locations. An indirect measure is a respondent household discussing the content of the CSP's EORE sessions with their neighbors. There are not multiple treatment arms. The source of exogenous variation is the randomized assignment of the EORE implementation.

Blinding is not possible as participants will be aware of the program session in their village. To reduce the risk of expectancy biases, data used for impact estimation will be collected by an independent organization separate from CSP, during a separate visit. Enumerators will emphasize that survey responses are for research only and will not affect respondents' eligibility for future services. Perhaps most importantly, we are primarily interested in downstream economic outcomes, not self-reported awareness. We expect demand effects to appear, if anywhere, in self-reported measures of attendance at the EORE session or in reported satisfaction with the session. This is because EORE is primarily framed to beneficiaries as a program to improve awareness and safety.

[6] Where group leaders express concerns that mapping activities could reveal mine locations to armed actors of the opposing faction, CSP adapts the EORE program by removing the ``community mapping" component while continuing to deliver other elements, such as risk awareness sessions, targeted messaging on high-risk behaviors, community reporting and coordination, and the distribution of educational materials

Figure 1: Locations of experimental sample



Sample and statistical power

- What is the unit of analysis for this sample (individuals, organizations, etc.)?
- What is the expected sample size? Please include statistical power calculations[7] to justify sample size. How does your statistical power compare to other contributions in the literature?
- What is the minimum effect size you will be able to detect?

We selected a sample of villages by combining MSNA and PMNT survey data. We restricted to the set of villages for which we had no records of prior EORE implementation and at least one record of landmine contamination in the area. Approximately 950 villages were randomized into a treatment and control group. Treatment is conducted at the village level through a group presentation by CSP. Within each village, we systematically sampled around 4--7 households for survey. The high number of villages relative to households in the sample reflects a moderate within-cluster correlation coefficient estimate on MSNA data.

[7] You can find useful information and software tools for power calculations here.

During roll-out it became clear that safety concerns would make implementation too risky in part of our experimental sample. Partway through roll-out of the intervention, we therefore replaced part of the experimental sample with new villages. This was done by dropping all villages in randomization strata where no visits could be made, and then adding new villages satisfying 1) our data collection partner could successfully complete a visit, 2) were not categorized as unreachable strata by our implementing partner, and 3) were located in states with lower safety concerns (Chin, Magway, or Sagaing) or were located in other states but phone numbers were available for remote implementation. Within the set of new villages added to the experimental sample, treatment was again randomized following the same stratification procedure described above.

The unit of analysis is a household or individual. The expected sample size at endline is 4,160 households. This sample offers a minimum detectable effect (MDE) of 4.7 percentage points in the employment rate (from a mean rate of 71%) at 80% power and 5% size. It is challenging to compare our statistical power to other, related studies given the lack of rigorous studies of landmine risk education programs. However, it may be helpful to note that our MDE of 4.7 pp (a change of 6.6%) is small compared to the estimated impact of landmine clearance from Chiovelli et al. (2025) of a 40% change in luminosity.

This calculation comes from the following Stata command:

power twomeans 0.71, sd(0.45) k1(419) k2(413) m1(5) m2(5) alpha(0.05) power(0.8) rho(0.106) cluster.

Avoiding operational distortions: Our implementing partner, CSP, targets areas for treatment largely by waiting for new reports of injuries. To avoid distorting operational efforts away from areas with the greatest need, we worked with CSP to exclude high priority areas with such reports from our randomization sample: our sample thus includes only medium-to-high, but not the most urgent, areas in terms of priority. Specifically, we shared a list of sample villages with CSP and asked them to identify any areas where they had planned visits, so that these areas could be excluded from randomization. No such villages appeared in our list, which is unsurprising given the large scale of contamination relative to EORE implementer capacity and that our sample already excluded locations with past EORE exposure, as these tend to be the highest-priority areas. For villages assigned to treatment, we fully funded CSP's treatment activities so that they could continue their work in the most urgent areas. Had any new reports of injuries from a control-group village emerged during our study period, CSP would have attempted to visit that area, but no such reports emerged.

3. Data

Please use this section to provide details on pilot data and *prospective* data that you will collect after Stage 1 of peer review is complete. Summarize the proposed procedures in the body of the paper, and include more details as an appendix.

Data collection and processing *(include in the appendix)*

- What are the key data sources? What data collection procedures and instruments will be used?
- What is the rule for terminating data collection (number of observations, available funds, available time, etc.)?

- How long will the data collection process take? If data will be collected at multiple points (longitudinal design), what is the proposed schedule (including enrollment, intervention delivery and outcome assessment)?

Our main data sources are a baseline and endline household survey we are collecting. We also have access to pre-intervention data through MSNA surveys in the same locations conducted between 2023 and 2024 as well as surveys conducted by International Organization for Migration (IOM) until January 2025. Our main instrument is attached in the appendix. Our data collection will be terminated according to the usual criteria of our data collection partner: namely, surveys will attempted with a sample of 943 villages (5 households per village) and in-person contact attempts will be terminated if the physical location is deemed inaccessible due to safety concerns AND (we have no phone contact for the household OR we have a phone contact but the household cannot be reached after three attempts).

Timeline: our baseline was conducted from September to October 2025. The EORE intervention began in June 2025 and is expected to be completed by December 2025. Our endline is anticipated to begin in February 2026 and end in March 2026.

While our baseline survey was conducted prior to treatment for the majority of locations assigned to treatment (349/413), a handful of locations were treated before the baseline survey, and we use this sample of 64 villages as a pilot. Given the short timing between treatment and survey in these pilot locations, we cannot use them to assess economic impacts, which we expect will take weeks or months to emerge. However, we can use them to check the quality of treatment implementation, as we discuss later on.

Safety and stopping criteria: The security situation in Myanmar evolves rapidly, and areas that are safe at one time may become unsafe in the future, and vice versa. Our implementation and data partners rely on extensive information from contacts on the ground to assess safety in real time, and we rely on their operational safety criteria to determine whether a location is safe to visit. This means that some locations were not reachable either by CSP or by the data firm based on their discretion. For all places excluded from field visits due to safety concerns, we attempted to reach participants by phone if possible.

Variations from the intended sample size

- Do you anticipate any challenges in collecting data (attrition, non-compliance with the assigned treatment, etc.) and what measures do you plan to take to address them?

Yes, attrition is a significant risk in this context: this is one of the reasons that research in Myanmar is especially scarce. We have two main strategies in place to mitigate this concern. The first is an intensive tracking process made possible by our data collection partner's deep expertise working in Myanmar. This is evident from their high rate of contact during our baseline survey, which was sampled from a broader set of villages available in MSNA data (the village-level contact rate was 88 percent reaching 832 out of a total 950 experimental village tracts). The second is a focus on village-level impacts through systematic household sampling. Namely, while we will first attempt to survey at endline those households that were surveyed at baseline, we will resample households within village when needed. Provided that resampling is orthogonal to treatment (which we expect, given that treatment should not significantly affect entire-household relocation), this procedure recovers unbiased ITT estimates. We will test this orthogonality assumption by evaluating randomization balance within the set of households surveyed at endline, and will report results estimated on the subset of non-replaced households, as well as estimates corrected for

attrition estimated through inverse probability weights, if significant imbalance (defined as an omnibus F-test significant at the 5% level) is detected.

Non-compliance, in the sense that a given household may not show up to the EORE education session, is expected as with all information-based programs, but does not complicate the interpretation of the ITT. We do not expect significant spillovers across villages (either information diffusion from treated to control units, or economic spillovers across units), as randomization was done at a high geographic level and mobility rates are low in this setting.

Pilot data

- Summarize any pilot data used in preparation for this submission. These can be included to establish effect size estimates, feasibility, or proof of concept.

Our experimental design originally aimed to assign 472 villages to treatment and 478 villages to control (for a total of 950 villages) before baseline data collection began. At baseline, we were able to survey 832 villages (413 treatment, 419 control), while 118 locations proved inaccessible. These 118 villages were evenly split between the original treatment and control assignments.

However, a sharp escalation in conflict, stricter checkpoint controls, and the lingering disruptions from the late-March earthquake in Myanmar significantly delayed the rollout of the intervention. As a result, only 64 of the 413 treatment villages had received CSP's EORE intervention between June 2025 (when implementation began) and October 2025 (the end of baseline data collection). Figure 2 shows a jump in village and household level treatment after the 4th week of the baseline survey in the field. But by that time about 80% of all survey interviews had already been completed, meaning that only a small share of villages in the survey sample had received the EORE program intervention at the time of survey data collection.

During the baseline, households were allowed to skip the EORE module—including questions about explosive contamination—if they felt distressed by the topic. Of the 3,912 households surveyed across the 832 experimental locations, 1,620 opted out of these questions. Module-level non-response was only 1.7 percentage points lower in the ITT group than in the control group, a statistically insignificant difference. We exclude these households from the analysis. Table 2 presents a summary of the resulting sample.

Figure 2: Treatment progression over the baseline survey period

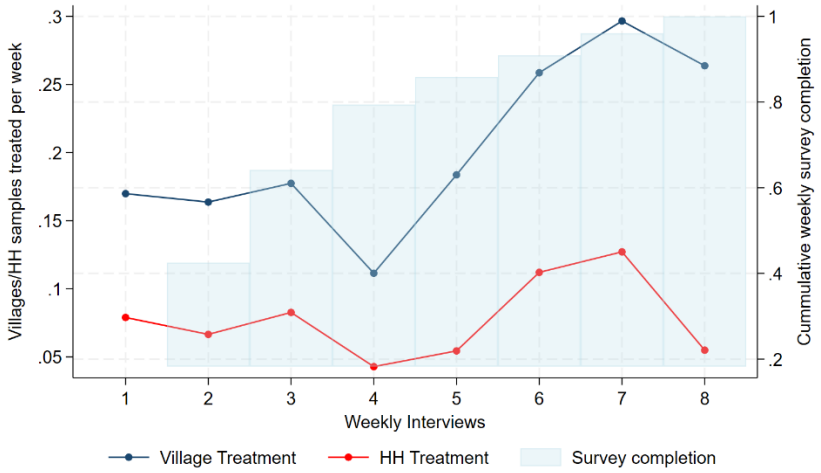


Table 2: Treated and ITT villages and households in the baseline round[8]

	Treated villages	Households in treated villages	Untreated villages	Household in untreated villages
ITT = Treated	64	257	252	909
ITT = Control	-		322	978

Balance tests presented in Appendix Table A1 confirm that the randomization protocol performed as intended, with only small differences in baseline household characteristics across treatment arms.

To verify whether the EORE program intervention reached the targeted population, we regress two dependent variables on the intention to treat (ITT) dummy variable: (1) whether at least one household in the village has received the program intervention (during the program delivery phase that started in June 2025), and (2) whether the household has received the EORE program. The unit of observation is the village level for the first choice of dependent variable and the household level for the second choice of dependent variable. We also consider a specification where the ITT dummy is replaced with two dummy variables that distinguish between intended to treat locations that had received the intervention at the time of the survey data collection and intended to treat locations that had yet to receive the intervention: while non-random treatment is a concern is this final regression, it may be helpful to assess the magnitude of treatment exposure within treated villages.

These regression results are presented in Table 3. Let us highlight three observations:

[8] 148 households from 40 control villages were treated by an EORE program that was delivered by a non-CSP organization outside of our experimental design

- The probability that at least one household in a village received CSP's EORE intervention is estimated to be 7.5 percentage points higher in ITT villages compared to control villages (see column 1).
- For villages that had received the intervention by the time of survey data collection, the probability that at least one sampled household indicated they received the program is estimated to be 86 percentage points higher compared to control villages (see column 2). For villages that had not yet received the program intervention by the time of survey data collection, the estimated treatment probability is 12 percent lower relative to control villages. This likely reflects the fact that these yet-to-be-treated locations are among the most difficult-to-access areas in Myanmar. As a result, CSP's local partners faced challenges delivering early treatment in these areas, while non-CSP actors—who non-systematically provide EORE across locations—may have reached a greater share of control villages. Consequently, the overall probability of receiving EORE after June 2025 (but not systematically through CSP) appears higher in control areas than in the yet-to-be-treated ITT locations. However, the high coefficient on villages listed as treated by CSP is consistent with successful program implementation in those villages.
- At the household level, the probability of receiving the EORE program is 2.2 percentage points higher in ITT villages, although the estimate is underpowered at baseline (t = 1.47). Disaggregating treated versus yet-to-be-treated households shows a 32.1 percentage point increase in treatment probability for the former group. For the yet-to-be-treated households, the probability of treatment is 6.1 percentage points lower than in control, consistent with the selection outlined above.

Table 3: Treatment probability in ITT, treated and yet-to-be-treated villages

	(1)	(2)	(3)	(4)
	Village	Village	Household	Household
ITT	0.075**		0.022	
	(0.029)		(0.015)	
Not yet treated		-0.121***		-0.061***
		(0.018)		(0.011)
Treated		0.859***		0.321***
		(0.023)		(0.032)
N	637	637	2292	2292
Clusters	637	637	638	638
R^2	0.026	0.601	0.032	0.211

Notes: Standard errors clustered at village tract level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

While the number of villages that received the treatment at the time of baseline survey data collection is comparatively small, we can still use the data to verify whether there is preliminary evidence of treatment effects on intermediate variables, like beliefs, that we expect to respond quickly to treatment. Specifically, we examine whether villages assigned to receive EORE are less likely to report difficulties with mobility, a key hypothesized channel through which EORE may improve economic outcomes in the longer run. The results are presented in Table 4. The outcome variables considered here are constructed from Likert-scale

questions (1–5), which measure increasing levels of agreement with statements indicating challenges in mobility, access, employment, or livelihoods. We create a binary variable equal to 1 for households that agreed—that is, those reporting more constraints, and 0 otherwise.[9] We aggregate this dummy to the village level by examining if any household in that village reported agreement with the prompt. The last variable in the table – "Challenges while traveling to place of work due to IEDs" is derived from the individual level employment module. It takes the value 1 if a household worker reported challenges in traveling to their place of work due to IED exposure, and 0 otherwise. We aggregate this variable at the household and village levels to obtain variables that equals 1 if any worker in the household or in the village reported such challenges, and 0 otherwise.

The regression results indicate some suggestive (though noisy) improvements in mobility in treatment locations. At the village level, the most pronounced effects relate to a reduction in ordnance-related constraints on mobility and market access. Specifically, ITT villages show an 8.3 percentage-point lower likelihood of experiencing challenges when traveling to markets or shops to trade goods (significant at 5 percent), and a 6.7 percentage-point reduction in challenges related to purchasing agricultural inputs such as fertilizers, seeds, and tools (significant at 10 percent). Similar results are observed for workers experiencing IED-related challenges in their commute to work: in ITT villages (and households) the incidence of encountering IED-related challenges during travel to workplaces is 3.7 (and 2.4) percentage points lower compared to control locations. Although noisy given the small pilot sample, these reported changes in mobility suggest that by endline—once a larger share of the ITT sample has been treated and several months have passed to allow effects to materialize—economic impacts are plausible.

Table 4: Potential consequences of EORE treatment at the baseline stage

Variable	Household level	Village level
Mobility challenges: School, hospital, religious place	–0.035 (0.029)	–0.038 (0.038)
Mobility challenges: Outside village visitors	–0.025 (0.032)	–0.034 (0.039)
Mobility challenges: Villagers Entry/Exit	–0.032 (0.032)	–0.041 (0.039)
Mobility challenges: to farm/fields	0.002 (0.030)	–0.021 (0.039)
Mobility challenges: Sell crops	–0.031 (0.029)	–0.046 (0.039)
Mobility challenges: Purchase ag. Inputs	–0.028 (0.029)	**–0.067*** **(0.039)**
Mobility challenges: Retail center	–0.030 (0.030)	**–0.083**** **(0.038)**
Challenges while traveling to place of work due to IEDs[10]	**-.024**** **(.010)**	**-.037**** **(.015)**
N	2292	637

[9] For instance, "Mobility challenges: Retail center" takes value 1 if households agree to the prompt: "Many households in this village/ward have **not** been able to go to a marketplace or a shop to buy or sell items because of explosive ordnances in the area."

[10] This is variable is constructed using the employment module. The household and village-level regression results are based on N=2174 households and N=619 villages that have at least 1 employed member per household.

Strata Fixed Effects	42	42

Notes: Standard errors clustered at admin 4 level in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01

4. Analysis

Please use this section to present your strategy for statistical analysis. In the appendices section of this submission, please also include any computer programs, configuration files or scripts which will be used to run the experiment and to analyze the data.

Statistical methods

- What statistical methods will be used to analyze the data and what are their underlying assumptions?
- How will the study deal with missing values?
- How do you define and handle outliers?

Statistical model

Provide the model in its functional form and submit math equations as text and not as images.

Our empirical model for measuring intent-to-treat effects will depend on the outcome variable being analyzed. For binary outcomes or outcomes containing negative values, we use the following linear specification:

$$y_{iv} = \beta T_v + \gamma y_{iv,pre} + \delta M_{iv,pre} + \theta_{ivt} + \alpha_v + \varepsilon_{iv}$$

where y_{iv} is an endline outcome for household i in village v , y_{iv,pre} is family i's mean pre-treatment value of y (when available in MSNA or our pre-treatment data), M_{iv,pre} is an indicator for a missing value of y_{iv,pre}, T_{v} is a treatment assignment dummy, Θ_{ivt} is a survey-month fixed effect which we interact with the survey date, α_{v} is a randomization stratum fixed effect, and ε_{iv} is an error term. We will cluster standard errors at the village level, matching the level of treatment assignment.

For non-negative, unbounded outcomes, we use an analogous Poisson specification.

We will impute missing values for y_{iv,pre} (and assign the dummy M_{iv,pre} appropriately) but will not impute outcome variables. We will consider standard approaches to outliers (such as winsorization) based on diagnostics such as Studentized residuals or Cook's distance to examine the sensitivity of main results.

Multiple outcome and multiple hypothesis testing

- How will the study address false positives from multiple hypothesis testing?
    - If you plan to adjust your standard errors, what adjustment procedure will you use? (e.g., Family Wise Error Rate, False Discovery Rates, etc.)
    - If you plan to aggregate multiple variables into an index, which variables will you aggregate and how?

In addition to “naïve” p-values, we will separately report sharpened q-values (Anderson, 2008) controlling the false discovery rate across our secondary economic outcomes (S4–S6).

Heterogeneous Effects

- Which groups do you anticipate will display heterogeneous effects? What leads you to anticipate these effects? Specify which baseline variable(s) will be used for heterogeneity analysis.

We will look at heterogeneity by:

- Displacement status (as displaced households likely have
- Gender of the household head (as this relates strongly to income earning opportunities)
- Baseline occupation (as this determines the value of travel through contaminated areas to markets or work opportunities)
- Baseline incidence of accident or death by landmine (as this may affect the salience of contamination risk as well as earnings potential)
- Baseline contamination (directly affects risk)
- Mode of program delivery (phone vs in person)
- Time since program delivery
- Prior EORE treatment by organizations other than CSP
- Incidence of ongoing conflict

5. Interpreting Results

- Depending on the outcome of the test(s), how will you interpret the results in the light of competing theories?
- How do they contribute to the development economics literature?
- What are the potential implications for policy?

If assignment to treatment is found not to affect reported participation in the EORE session (directly or indirectly), we would conclude that the implementation was poorly done or that people did not remember it. The results shown in Table 3 suggest that this is unlikely. If assignment is found to affect reported participation but not beliefs or knowledge of landmine risk, we would conclude that the information was already known to most people or that it was unknown but forgotten between the session and our endline survey; however, the results of Table 4 suggest that treatment did change beliefs. If assignment is found to affect beliefs but not economic outcomes, we would conclude that there was no scope in this setting for improved landmine knowledge (as delivered through the standard risk education program) to influence these economic outcomes. We believe such a finding would have strong internal validity; to assess the generalizability of that finding we would consider the following competing explanations:

- By focusing our study on locations assessed to be of moderate-to-high value for EORE by CSP (we avoided randomizing within locations CSP was already funded to work in because we did not want to withhold EORE from highly contaminated locations), it is possible that economic benefits could be muted. We will analyze heterogeneity by baseline contamination to speak to this possibility.

- It is possible that EORE could affect plans that take a long time to materialize, and thus that our endline survey did not pick up. We will include questions about planned migration, investment, crop cultivation, and business creation to test this.

The implications of these results for the economics literature and mine action policy are direct: if the benefits of EORE extend beyond mine knowledge and risk-taking behavior and into health or economic domains, then its benefits are being underestimated. We also expect our data and findings to relate to broader questions about how households cope in active conflict or post-conflict environments, and after displacement or when living nearby the displaced.

Interpreting impacts on injury and mortality from landmines: A major goal of the EORE program is to reduce landmine injuries and mortality by providing information and training on how to identify and avoid landmines. This is believed to be especially important for children, who often play in contaminated areas but may not recognize unexploded ordnance without training, and EORE sessions focus specifically on training sessions for children. We thus hypothesize that the EORE programs will measurably reduce landmine injuries and mortality. However, we cannot evaluate the impact of EORE in the highest-risk, most-contaminated regions, as these are excluded from our study by design. A null impact on mortality, if combined with a low mortality rate in the control group, could therefore indicate that there was insufficient scope to detect changes in mortality over our study period.

External validity: While our study is anchored in Myanmar's current context, it evaluates intervention components that are common to EORE in many settings. To speak more directly to how impacts may differ across operational environments, we will examine heterogeneity by local conflict exposure using geographic measures (e.g., recent incident intensity, access disruptions, and displacement) and test whether effects are attenuated or amplified in areas experiencing less active conflict. Conceptually, this variation can partially proxy for conditions closer to post-conflict settings, where mobility constraints, information channels, and day-to-day risk differ but contamination risk remains. This exercise will help us assess how results might translate to (i) other ongoing-conflict contexts where implementation constraints and elevated perceived risk are similar, and (ii) post-conflict environments where the same pedagogical content and behavior-change mechanisms may operate, potentially with different magnitudes due to changed exposure and opportunity costs of avoidance.

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7. Appendices

If there is more than one appendix, they should be identified as A, B, etc. Formulae and equations in appendices should be given separate numbering: Eq. (A.1), Eq. (A.2), etc.; in a subsequent appendix, Eq. (B.1) and so on. Similarly for tables and figures: Table A.1; Fig. A.1, etc.

8. Administrative information *(required)*

Funding: This work was supported by The World Bank, UNHCR, and J-PAL’s Humanitarian Protection Initiative (grant # HPI-24-03001).

Institutional Review Board (ethics approval): Ethics approval was obtained from University of Rochester’s Research Subjects Review Board (study #10416).

Declaration of interest: The authors have no competing interests to declare.

Acknowledgments: We are grateful to Community Safety Partnerships, and especially Bekim Shala, for implementing the EORE program. We thank our partners at IMPACT, especially Patrick Sayers, Aletta Buehler, and Cecile Van Maanen for their data collection efforts. We thank Natsuko Nozaki for her outstanding work managing project implementation, and Jingle Liu for excellent research assistance.

Appendix

Figure A1: Ongoing EORE sessions by CSP partners in Tanintharyi Region

Figure A2: A typical EORE handout

Translation: ***Title: Danger***

Children who are out collecting firewood (or walking in the forest) may come across explosive remnants of war.

Do not touch and report immediately.

*If you find strange or dangerous-looking objects while walking outside or in the forest, **do not touch them**. Tell adults or the village authorities immediately.*

Figure A3: Samples from EORE training toolkit

Translations:

Center Panel (Title) : *"Landmines and Explosive Remnants of War Can Cause Severe Injury or Death."*

Red sign: "DANGER – MINES"

Panel 1 (Top left, child herding cattle): *"Children who herd animals or walk in forested or unfamiliar areas may encounter landmines."*

Panel 2 (Explosion with cow): *"Stepping on or disturbing a mine can cause serious injury or death."*

(The red text near the child indicates a scream: "Ahh!")

Panel 3 (Man lighting something in bushes): *"Touching strange objects or explosive remnants can be extremely dangerous."*

Panel 4 (Explosion): *"If you touch or try to move these objects, they can explode instantly."*

Panel 5 (Man digging with stick): *"Landmines are hidden beneath soil and grass; you cannot know where they are by looking."*

Panel 6 (Explosion while digging): *"Digging or farming in mined areas is very dangerous."*

(The Burmese text in red shows a scream: "Ahh!")

Table A1: Randomization Balance

Variable	Control Mean/(SE)	ITT Mean/(SE)	Pairwise t-test Mean difference
Share of 18 to 65 years	0.627	0.617	0.009
	(0.008)	(0.009)	
Share of above 65 years	0.064	0.069	-0.005
	(0.005)	(0.005)	
Share of below 18 years	0.309	0.314	-0.005
	(0.009)	(0.010)	
Household size	4.639	4.574	0.066
	(0.062)	(0.057)	
Age of the head of household	4.365	4.296	0.070
	(0.046)	(0.045)	
Log(time to health facility)	3.198	3.192	0.005
	(0.039)	(0.039)	
Log consumption	7.459	7.467	-0.008
	(0.012)	(0.012)	
Poverty status	0.582	0.577	0.005
	(0.021)	(0.021)	
Savings: Cash at home	0.363	0.268	0.096***
	(0.023)	(0.018)	
Savings: Livestock	0.089	0.077	0.012
	(0.012)	(0.009)	
Savings: Jewelry	0.175	0.173	0.002
	(0.017)	(0.016)	
Savings: Other	0.072	0.054	0.018
	(0.012)	(0.008)	
Displaced Household	0.544	0.569	0.025
	(0.013)	(0.015)	
Owns a formal saving account	0.045	0.045	-0.000
	(0.007)	(0.008)	
Owns a formal mobile money account	0.264	0.252	0.013
	(0.019)	(0.017)	
Use mobile money	0.394	0.403	-0.009
	(0.025)	(0.025)	