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Psychosocial interventions for posttraumatic stress disorder in low- and middle-income countries: a Bayesian meta-analysis

Cosima Leithner[1], Smilla Gerst[1], Rebeca Niyaz[1], Luzia Heusler[1], Yannik Terhorst[1,2], Thomas Ehring[1,2] and Verena Semmlinger[1]

[1]Department of Psychology, LMU Munich, Munich, Germany and [2]German Center for Mental Health (DZPG), Partner-Site Munich-Augsburg, Munich, Germany

Review Article

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Corresponding author:
Cosima Leithner;
Email: cosima.leithner@psy.lmu.de

Abstract

Posttraumatic stress disorder (PTSD) is prevalent and debilitating in low- and middle-income countries (LMICs), yet the effectiveness of psychosocial interventions remains poorly synthesized. This study presents the first comprehensive meta-analysis of their effectiveness for adults with PTSD symptoms in LMICs (PROSPERO CRD42024584333). A literature search was conducted using the PTSD Trials Standardized Data Repository. Additional searches were performed through backward citation tracking and in Embase, Medline, PsycINFO, and PTSDpubs using terms for PTSD, nonpharmacological interventions, and randomized controlled trial (RCT) methodology. Trials published from 1 January 1980 to 23 June 2025 were included. Eligible studies were RCTs evaluating psychosocial interventions in LMICs, with participants exhibiting PTSD symptoms. Effect sizes were aggregated using three-level Bayesian random-effects meta-regressions. Study quality was assessed using Cochrane's RoB 2 tool. Fifty-five RCTs, comprising 5,041 participants, were included. At post-assessment, psychosocial interventions yielded large within-group ($g = -1.88$; 95% credible interval [CrI], −2.25 to −1.51) and between-group ($g = -1.31$; 95% CrI, −1.66 to −0.95) effects for PTSD symptoms, both sustained at follow-up. Sensitivity analyses confirmed robustness of results across varying prior specifications. For the secondary outcomes of depression and anxiety, medium to large, sustained effects emerged. Treatments were particularly effective when trauma-focused, delivered face-to-face, and administered to older participants. This meta-analysis supports the use of psychosocial interventions for adults with PTSD symptoms in LMICs. Consistent with guidelines from high-income countries, trauma-focused interventions should be prioritized as first-line treatments. More high-quality research on PTSD treatment in LMICs is urgently needed, particularly in low- and lower-middle-income countries.

Introduction

Posttraumatic stress disorder (PTSD) is a prevalent mental health condition in low- and middle-income countries (LMICs; e.g. Koenen et al., 2017), with significant individual (e.g. Pacella, Hruska, & Delahanty, 2013) and societal impacts (e.g. Davis et al., 2022). According to the World Mental Health Surveys, 69.1% of individuals in low- and lower-middle-income countries and 63.2% in upper-middle-income countries experience traumatic events in their lifetime, and the PTSD lifetime prevalence is 2.1% and 2.3%, respectively (Koenen et al., 2017). More recent data show that, particularly for civilians residing in armed conflict-affected regions, a higher PTSD prevalence was identified among LMICs at 25.40%, relative to 11.40% in high-income countries (Ahmed et al., 2024). PTSD is associated with severe mental and physical health outcomes, including an increased risk of suicide (LeBouthillier, McMillan, Thibodeau, & Asmundson, 2015; Panagioti, Gooding, & Tarrier, 2012) and a higher prevalence of medical conditions (Pacella et al., 2013), as well as substantial economic costs comparable to those of other high-burden mental disorders (Davis et al., 2022). Despite these severe consequences, treatment coverage for PTSD remains markedly limited in LMICs: among individuals meeting diagnostic criteria for PTSD within the past 12 months, only 19.8% had any contact with mental health services, compared with 50.6% in high-income countries. This gap is proportionally even more pronounced for effective treatment coverage, that is, receipt of adequate, severity-specific pharmacotherapy and/or psychotherapy, which was received by only 4.1% of individuals with PTSD in LMICs, versus 17.8% in high-income countries (Stein et al., 2023). Delayed access to adequate treatment is associated with lower remission rates, emphasizing the chronic progression of the disorder when left untreated (Morina, Wicherts, Lobbrecht, & Priebe, 2014). Therefore, research on accessible and effective treatment options for PTSD in LMICs is urgently required.

Psychological interventions are widely recommended as the first-line treatment for PTSD (American Psychological Association, 2025), given their demonstrated efficacy in numerous meta-analyses (Kip et al., 2025). However, the majority of meta-analytic research on PTSD treatment is based on randomized controlled trials (RCTs) conducted in high-income countries (Harrer et al., 2025), limiting the generalizability of the findings to LMICs. Factors such as insufficient mental health services, a lack of qualified practitioners, and difficulties in adapting treatments to local contexts may impact intervention effectiveness (Kaminer et al., 2024). While reviews focusing broadly on psychosocial interventions in LMICs include PTSD outcomes (Purgato et al., 2018; van Ginneken et al., 2013), only two meta-analyses have specifically investigated nonpharmacological treatments for adults with PTSD. One focused on refugees and asylum seekers resettled in LMICs (Dowllah & Melville, 2024), while the other examined survivors of mass violence (Morina, Malek, Nickerson, & Bryant, 2017). These meta-analyses found that the examined interventions were effective in reducing PTSD and depression symptoms in LMICs at post-assessment compared to control conditions. However, a comprehensive meta-analysis assessing the generalizability of these findings from specific populations to the broader population of trauma survivors with PTSD symptoms in LMICs has yet to be conducted. Thus, the current meta-analysis aims to evaluate the effectiveness of psychosocial interventions in reducing PTSD symptoms and improving depression and anxiety in adult patients with PTSD symptoms in LMICs, as assessed in RCTs, without restrictions on the control condition.

Methods

This meta-analysis followed the PRISMA reporting guideline (see Supplementary Table S1; Moher et al., 2009) and was registered in PROSPERO (CRD42024584333) on 8 September 2024.

Identification of Trials

The literature search was conducted using the PTSD Trials Standardized Data Repository (PTSD Repository), a publicly accessible database of RCTs on PTSD treatment (O'Neil et al., 2020, 2024). The PTSD Repository covers trials published from 1980 onward, coinciding with the introduction of PTSD as a diagnosis in the DSM-III. It is updated annually and was current up to 1 April 2024 when this meta-analysis was conducted. To identify trials published after this date, an additional database search was performed in Embase, Medline, PsycINFO, and PTSDpubs, based on the PTSD Repository's search strategy for nonpharmacological interventions. This search strategy combined terms related to PTSD (e.g. posttraumatic stress disorder, PTSD), a broad range of terms related to nonpharmacological interventions (e.g. psychotherapy, complementary therapies, self-help, telemedicine, cognitive behavioral therapy, eye movement desensitization and reprocessing, mindfulness, and exercise), and terms related to RCT methodology (e.g. random*, trial, and blind*), together with corresponding MeSH terms (O'Neil et al., 2019). This search was first performed on 6 January 2025 and repeated during the final phase of the study. Therefore, this meta-analysis included trials published up to 23 June 2025. Furthermore, a backward search was conducted by screening trials included in 48 relevant meta-analyses and the reference lists of eligible trials to identify any additional RCTs not present in the PTSD Repository or the database search. Details on each step of the search process are available in Supplementary Methods S1.

Selection of trials

To be eligible, trials had to meet the following inclusion criteria: (a) published in English; (b) appeared in a peer-reviewed journal; (c) used random group allocation; (d) included participants aged ≥16 years; (e) confirmed that participants had PTSD symptoms at study inclusion, diagnosed according to DSM-IV, DSM-5, ICD-10, or ICD-11; (f) evaluated a psychosocial intervention (i.e. any psychological, social, or rehabilitative effort) primarily aimed at improving PTSD symptoms; (g) conducted in a country classified by the World Bank as an LMIC in the corresponding year of the trial's publication (The World Bank, 2024); and (h) reported data from a validated PTSD measure sufficient to calculate either a within- or between-group effect size at post-assessment (i.e. number of observations, mean, and standard deviation reported separately for each group). The chosen age threshold was applied to avoid excluding otherwise eligible trials that recruited predominantly adult samples but included participants from age 16 years onward. Trials were excluded if they enrolled participants based solely on trauma exposure without confirmed PTSD symptoms at study entry, or if they focused exclusively on psychopharmacological treatments or neuromodulation without incorporating a psychosocial intervention.

The PTSD Repository was filtered to include only trials conducted in countries that had ever been classified as an LMIC by the World Bank, and that evaluated psychosocial interventions (i.e. trials exclusively focusing on pharmacotherapy were excluded). Titles and abstracts of the remaining studies, along with those identified through database and backward searches, were independently screened for eligibility by two researchers (S.G., L.H., and/or R.N.). Full texts of potentially eligible studies were then assessed independently by two of the same researchers. Any discrepancies were resolved through discussion and, when necessary, consultation with additional team members (T.E., C.L., and/or V.S.).

Data extraction

Data extraction was initiated on 4 October 2024. For trials included in the PTSD Repository, data extraction was performed in two steps. First, relevant data available in the PTSD Repository were retrieved. Second, variables of interest that were either not reported in the PTSD Repository or were available only at the study level but required at the treatment arm level were extracted directly from the publications by two independent researchers (S.G. and R.N.). For trials not listed in the PTSD Repository, all variables were extracted from the publications by the same researchers. Any discrepancies were resolved through discussion and, if necessary, in consultation with additional team members (T.E., C.L., and/or V.S.).

Data were extracted to enable the calculation of effect sizes for the primary and secondary outcomes. With PTSD symptoms as the primary outcome and depression and anxiety symptoms as secondary outcomes, sample sizes, along with mean values and standard deviations for the corresponding validated instruments, were extracted separately for each group at baseline, post-assessment, and follow-up. When studies reported multiple follow-up assessments, only data of the latest available time point were extracted. For descriptive purposes and moderator analyses, data on study characteristics (e.g. country of implementation, type of control condition, and allowance of psychotropic medication), patient characteristics (e.g. age, sex, employment status, and population type), and treatment characteristics (e.g. delivery method, number of sessions, and cultural adaptation) were extracted. Following the definition of the American Psychological Association (2025), interventions were classified as trauma-focused if they directly

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addressed the traumatic event and aimed to help patients process associated cognitions, emotions, somatic reactions, or memories (e.g. Cognitive Processing Therapy, Prolonged Exposure, Eye Movement Desensitization and Reprocessing, and Narrative Exposure Therapy), and as non-trauma-focused if they did not involve such direct processing of the trauma (e.g. supportive counseling, applied muscle relaxation, and resource activation). Details on the data extraction process are available in Supplementary Methods S2, and an overview of the extracted variables is presented in Supplementary Table S2.

Quality assessment

The risk of bias (RoB) of included trials was assessed using Cochrane's RoB 2 tool (Sterne et al., 2019). The RoB rating was based on the rating provided in the PTSD Repository, or, for trials not included in the PTSD Repository, a rating was performed. The overall certainty of evidence for the primary outcome was evaluated using the GRADE approach (Schünemann, Brożek, Guyatt, & Oxman, 2013).

Statistical analysis

As the measure of effect size, Hedges' *g* (Borenstein, Hedges, Higgins, & Rothstein, 2009) was calculated for the primary outcome (PTSD severity) and the secondary outcomes (depression and anxiety severity) at both post-assessment and follow-up, using between- and within-group comparisons. A negative Hedges' *g* value indicates a reduction in symptom severity, either over time within a group (within-group effect) or in the intervention group compared to the control group (between-group effect). Pooled effect sizes were estimated using three-level Bayesian random-effects meta-regressions employing weakly informative priors. A Bayesian meta-analytical approach was chosen over frequentist methods because it better accommodates the uncertainty inherent in small sample sizes (Borenstein et al., 2009). Random-effects models were selected due to the anticipated substantial heterogeneity across trials, reflecting the broad scope of the research question, which encompassed diverse psychosocial interventions and populations. Three-level models best represented the nested data structure by modeling Hedges' *g* for each comparison between groups or time points at Level 1, accounting for differences between outcomes within trials (i.e. multiple intervention groups) at Level 2, and differences between trials at Level 3.

Sensitivity analyses were conducted to assess the robustness of the findings for PTSD symptoms at post-assessment under varying prior specifications. Publication bias was examined through visual inspection of funnel plots and Egger's test for asymmetry (Egger, Smith, Schneider, & Minder, 1997). To explore the influence of study-, patient-, and treatment-level characteristics on within- and between-group effect sizes for PTSD symptoms at post-assessment, moderator analyses were conducted.

All analyses were conducted using R version 4.3.0. Bayesian modeling was performed using the R packages rstan (Stan Development Team, 2025) and brms (Bürkner, 2017). Details of the statistical methods are provided in Supplementary Methods S3 and Supplementary Table S3. The analysis code and underlying data are openly available on OSF (Leithner et al., 2025).

Results

Characteristics of included trials

The study selection process is illustrated in the PRISMA flowchart (see Figure 1). A list of trials excluded after full-text screening is provided in Supplementary Table S4. Ultimately, 55 RCTs (k_s) were included, comprising 66 active treatment conditions (k_t) and 5,041 participants. For a complete list of references of the included trials, see Supplementary Results S1.

Most of the included trials were conducted in countries classified as upper-middle-income ($k_s = 38$), while a similar number were conducted in low-income ($k_s = 9$) and lower-middle-income countries ($k_s = 8$). Specifically, the majority of trials were implemented in China ($k_s = 8$), Iran ($k_s = 8$), and Turkey ($k_s = 6$). Most trials focused on civilians ($k_s = 34$), followed by refugees ($k_s = 10$). The majority of trials included participants with either a variety of trauma types ($k_s = 15$) or those who had experienced terrorism, political violence, or forced displacement ($k_s = 15$). The mean age of participants at baseline across trials was 35.91 years (SD = 8.49). Only four trials reported data on race (Bröcker et al., 2024; Bryant et al., 2011; Moradi et al., 2021; Proenca et al., 2022), with most participants identified as Asian (51.0%) or Black (31.0%). On average, trials included more female participants (65.0%) than male participants, and slightly more participants were married or in a committed relationship (55.0%) compared to those who were not. Most participants received trauma-focused interventions ($k_t = 46$, 69.7%) that were delivered individually ($k_t = 45$) and face-to-face ($k_t = 57$), with an average of 7.73 intended treatment sessions (SD = 5.09), each lasting ~72.51 minutes (SD = 33.87), distributed over an average of 6.64 weeks (SD = 5.57). Treatment was predominantly delivered by specialized practitioners ($k_t = 46$), with a smaller subset of active treatment conditions delivered by basic practitioners ($k_t = 8$). Wait-list controls were the most commonly used comparison condition ($k_s = 29$), although active control conditions were represented in a substantial number of trials as well ($k_s = 23$), comprising treatment-as-usual ($k_s = 8$) and other active controls ($k_s = 15$). Follow-up assessments were conducted on average 16.37 weeks ($SD = 13.12$) after treatment completion. For a detailed description of the included trials, see Supplementary Table S5.

Risk of bias assessment

The overall risk of bias was rated as low for three studies (5.5%), 22 studies showed some concerns (40.0%), and the rating was high for 30 studies (54.5%). For details on the risk of bias assessment results, see Supplementary Figures S1 and S2.

Primary outcome: PTSD symptoms

Psychosocial interventions conducted in LMICs demonstrated large and sustained effects on PTSD symptoms, both within intervention groups across assessments ($g \geq -2.21$) and relative to control groups ($g \geq -1.31$). Detailed estimates of Hedges' *g* are presented in the Level 1 columns of Table 1. Forest plots illustrating between- and within-group effect sizes at post-assessment are displayed in Supplementary Figures S3 and S4. The strength of the evidence was rated as very low according to GRADE (see Supplementary Table S6).

The three-level analytic model was supported, as indicated by 95% credible intervals (CrIs) for Level 2 and Level 3 variance estimates excluding zero. Sensitivity analyses confirmed the robustness of the results across varying prior specifications. Visual inspection of funnel plots suggested potential publication bias, which was further supported by Egger's test results (see Supplementary Figures S5 and S6).

Results of moderator analyses are presented in Table 2, with findings from dummy-coded meta-regressions for direct subgroup comparisons detailed in Supplementary Table S7. In terms of

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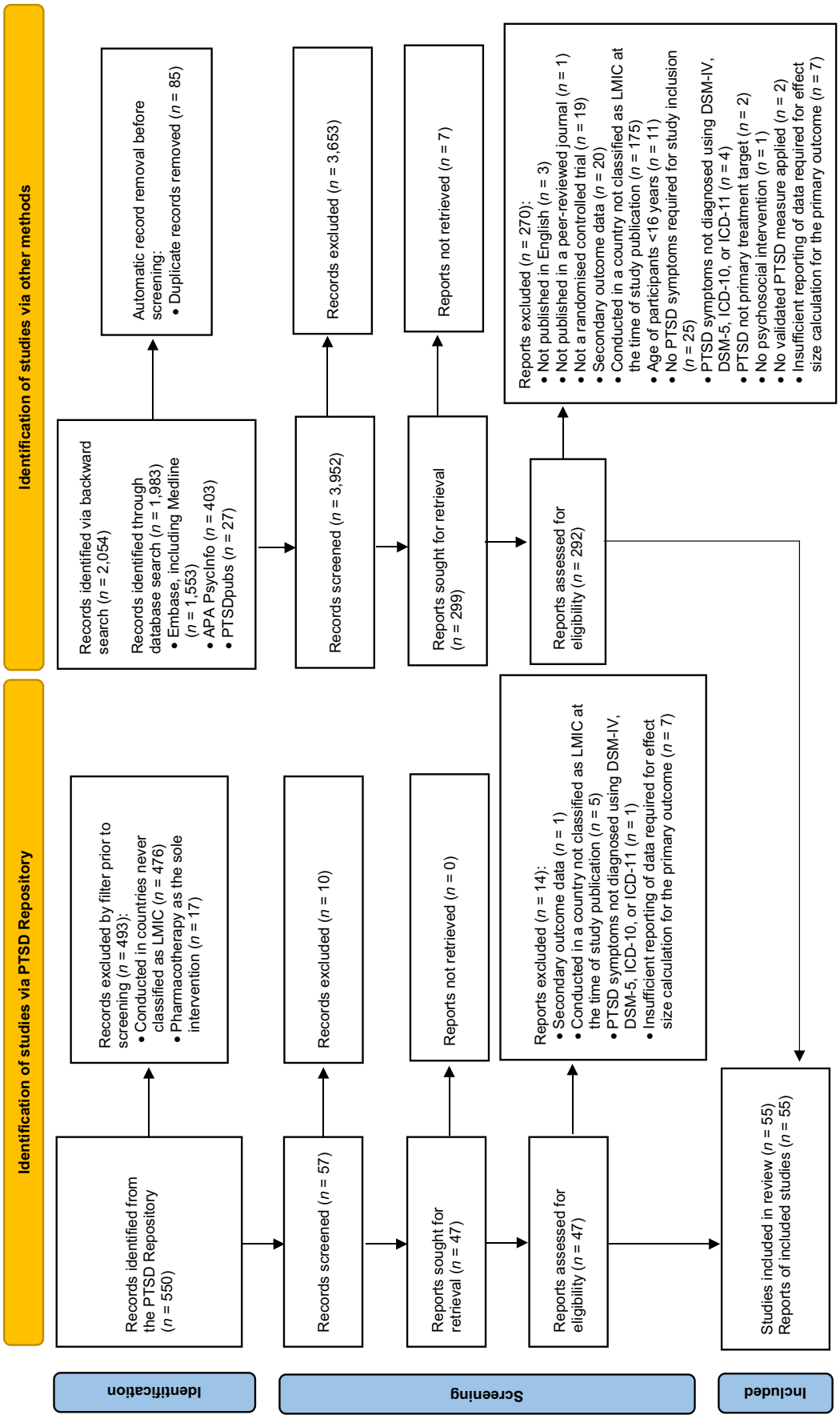

Figure 1. PRISMA flowchart.

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Table 1. Bayesian three-level meta-regression results for effect sizes (Hedges' *g*) of psychosocial PTSD interventions in LMICs[a]

Outcome	Within-group effect sizes				Between-group effect sizes			
Time point	k_t	Level 1 (95% CrI)	Level 2 (95% CrI)	Level 3 (95% CrI)	k_t	Level 1 (95% CrI)	Level 2 (95% CrI)	Level 3 (95% CrI)
PTSD								
Post-Ass.	NA	NA	NA	NA	58	−1.31 (−1.66 to −0.95)	0.59 (0.23 to 1.26)	1.06 (0.17 to 1.50)
Pre- to Post-Ass.	66	−1.88 (−2.25 to −1.51)	0.67 (0.30 to 1.34)	1.15 (0.19 to 1.61)	NA	NA	NA	NA
Follow-up	NA	NA	NA	NA	33	−1.15 (−1.64 to −0.67)	0.74 (0.28 to 1.40)	0.88 (0.05 to 1.57)
Pre to follow-up	37	−2.21 (−2.70 to −1.74)	0.69 (0.26 to 1.40)	1.02 (0.11 to 1.62)	NA	NA	NA	NA
Post to follow-up	37	−0.18 (−0.44 to 0.08)	0.19 (0.01 to 0.58)	0.63 (0.26 to 0.94)	NA	NA	NA	NA
Depression								
Post-Ass.	NA	NA	NA	NA	36	−0.86 (−1.22 to −0.51)	0.57 (0.11 to 1.12)	0.69 (0.04 to 1.23)
Pre- to Post-Ass.	42	−1.32 (−1.69 to −0.96)	0.80 (0.27 to 1.27)	0.60 (0.02 to 1.26)	NA	NA	NA	NA
Follow-up	NA	NA	NA	NA	21	−0.75 (−1.28 to −0.24)	0.86 (0.39 to 1.40)	0.56 (0.02 to 1.31)
Pre to follow-up	24	−1.59 (−2.10 to −1.05)	0.72 (0.33 to 1.28)	0.77 (0.11 to 1.33)	NA	NA	NA	NA
Post to follow-up	24	−0.30 (−0.59 to −0.01)	0.29 (0.06 to 0.57)	0.50 (0.29 to 0.77)	NA	NA	NA	NA
Anxiety								
Post-Ass.	NA	NA	NA	NA	22	−0.74 (−1.11 to −0.38)	0.56 (0.06 to 1.00)	0.45 (0.02 to 0.98)
Pre- to Post-Ass.	27	−0.99 (−1.38 to −0.59)	0.52 (0.03 to 1.08)	0.66 (0.04 to 1.23)	NA	NA	NA	NA
Follow-up	NA	NA	NA	NA	11	−0.44 (−0.83 to −0.04)	0.29 (0.01 to 0.77)	0.38 (0.02 to 0.89)
Pre to follow-up	13	−1.03 (−1.35 to −0.72)	0.21 (0.01 to 0.58)	0.28 (0.02 to 0.69)	NA	NA	NA	NA
Post to follow-up	13	−0.23 (−0.53 to 0.06)	0.26 (0.01 to 0.60)	0.27 (0.02 to 0.63)	NA	NA	NA	NA

Note: CrI, credible interval; k_t, number of comparisons; LMICs, low- and middle-income countries; NA, not applicable; Post-Ass., post-assessment; PTSD, post-traumatic stress disorder.
[a]Level 1 estimates quantify the effect size Hedges' *g* (where zero indicates no difference in symptom severity between time points or groups). Level 2 estimates quantify how much estimates vary within studies. Level 3 estimates quantify the extent to which estimates vary between studies. 95% CrIs quantify the 95% interval in which the true estimate lies given the provided data.

Table 2. Bayesian three-level meta-regression results for potential moderators of effect sizes (Hedges' *g*) in psychosocial PTSD interventions in LMICs[a]

Domain	Within-group effect sizes				Between-group effect sizes			
Moderator variable	k_t	Level 1 (95% CrI)	Level 2 (95% CrI)	Level 3 (95% CrI)	k_t	Level 1 (95% CrI)	Level 2 (95% CrI)	Level 3 (95% CrI)
Study characteristics								
Publication year	66	−0.03 (−0.11 to 0.04)	0.66 (0.29 to 1.34)	1.16 (0.20 to 1.63)	58	−0.05 (−0.11 to 0.03)	0.58 (0.23 to 1.23)	1.06 (0.20 to 1.50)
Type of control								
Active control	NA	NA	NA	NA	27	−1.11 (−1.57 to −0.65)	0.77 (0.28 to 1.32)	0.63 (0.03 to 1.38)
Passive control	NA	NA	NA	NA	31	−1.42 (−1.97 to −0.88)	0.50 (0.02 to 1.52)	1.21 (0.15 to 1.83)
First author's location								
Local	38	−1.97 (−2.53 to −1.41)	0.96 (0.28 to 1.83)	1.09 (0.04 to 1.98)	36	−1.37 (−1.86 to −0.88)	0.88 (0.26 to 1.61)	0.90 (0.04 to 1.68)
Not local	28	−1.68 (−2.11 to −1.26)	0.55 (0.09 to 1.07)	0.71 (0.08 to 1.23)	22	−1.11 (−1.62 to −0.60)	0.37 (0.01 to 1.06)	0.91 (0.21 to 1.48)
GNI per capita	65	0.00 (0.00 to 0.00)	0.66 (0.29 to 1.36)	1.18 (0.19 to 1.65)	57	0.00 (0.00 to 0.00)	0.56 (0.23 to 1.20)	1.12 (0.33 to 1.54)
Country classification								
Low/lower middle income	20	−1.91 (−2.56 to −1.26)	0.55 (0.02 to 1.46)	1.03 (0.08 to 1.79)	18	−1.22 (−1.90 to −0.52)	0.71 (0.03 to 1.65)	0.95 (0.05 to 1.86)
Upper middle income	46	−1.83 (−2.29 to −1.35)	0.80 (0.31 to 1.54)	1.08 (0.08 to 1.74)	40	−1.29 (−1.73 to −0.86)	0.63 (0.23 to 1.33)	1.02 (0.12 to 1.57)
Risk of bias								
Low/some concerns	29	−1.85 (−2.41 to −1.29)	0.75 (0.24 to 1.55)	1.01 (0.07 to 1.75)	29	−0.97 (−1.48 to −0.48)	0.80 (0.27 to 1.45)	0.79 (0.03 to 1.57)
High	37	−1.84 (−2.36 to −1.33)	0.79 (0.20 to 1.57)	1.04 (0.07 to 1.78)	29	−1.57 (−2.06 to −1.06)	0.51 (0.02 to 1.37)	1.02 (0.11 to 1.63)
Psychotropic medication								
Allowed	18	−1.10 (−1.75 to −0.39)	0.69 (0.03 to 1.60)	0.93 (0.35 to 1.60)	14	−0.76 (−1.29 to −0.21)	0.44 (0.02 to 1.15)	0.75 (0.31 to 1.30)
Not allowed	7	−1.92 (−3.19 to −0.42)	0.97 (0.03 to 2.58)	0.95 (0.03 to 2.57)	7	−1.26 (−2.48 to 0.03)	0.97 (0.03 to 2.50)	0.96 (0.03 to 2.54)
Patient characteristics								
Population type								
Civilians	40	−1.83 (−2.24 to −1.42)	0.87 (0.38 to 1.39)	0.70 (0.04 to 1.37)	38	−1.12 (−1.55 to −0.69)	0.61 (0.23 to 1.25)	0.95 (0.14 to 1.48)
Refugees	14	−1.67 (−2.28 to −1.08)	0.38 (0.02 to 1.02)	0.72 (0.08 to 1.44)	10	−1.28 (−1.97 to −0.57)	0.51 (0.02 to 1.29)	0.64 (0.04 to 1.45)
Other	12	−1.80 (−3.24 to −0.29)	0.97 (0.02 to 3.01)	1.78 (0.06 to 3.62)	10	−1.57 (−2.80 to −0.26)	1.13 (0.04 to 2.71)	1.11 (0.04 to 2.65)
Age	48	−0.04 (−0.09 to 0.01)	0.68 (0.23 to 1.37)	1.00 (0.08 to 1.57)	42	−0.05 (−0.10 to −0.01)	0.69 (0.24 to 1.25)	0.77 (0.05 to 1.33)
Percent female	51	0.22 (−0.88 to 1.37)	0.67 (0.27 to 1.22)	0.85 (0.08 to 1.35)	47	0.33 (−0.76 to 1.45)	0.64 (0.25 to 1.20)	0.87 (0.09 to 1.35)
Percent in relationship	35	1.84 (−0.29 to 3.95)	0.85 (0.28 to 1.67)	1.10 (0.07 to 1.87)	33	−0.02 (−2.03 to 1.97)	0.83 (0.30 to 1.58)	0.98 (0.07 to 1.70)
Percent employed	22	0.46 (−2.16 to 2.89)	0.93 (0.13 to 1.89)	1.08 (0.04 to 2.21)	20	1.33 (−0.39 to 3.01)	0.67 (0.05 to 1.32)	0.66 (0.03 to 1.42)
Baseline PTSD severity	58	0.00 (−0.02 to 0.03)	0.63 (0.25 to 1.37)	1.19 (0.19 to 1.68)	50	0.02 (0.00 to 0.04)	0.57 (0.21 to 1.23)	1.04 (0.19 to 1.48)

(*Continued*)

Table 2. (*Continued*)

Domain	Within-group effect sizes				Between-group effect sizes			
Moderator variable	k_t	Level 1 (95% CrI)	Level 2 (95% CrI)	Level 3 (95% CrI)	k_t	Level 1 (95% CrI)	Level 2 (95% CrI)	Level 3 (95% CrI)
Treatment characteristics								
Delivery method								
Face-to-face	57	−2.04 (−2.41 to −1.68)	0.66 (0.30 to 1.24)	0.99 (0.16 to 1.46)	49	−1.47 (−1.89 to −1.05)	0.59 (0.24 to 1.29)	1.18 (0.29 to 1.65)
Non-face-to-face	9	−0.74 (−1.88 to 0.38)	1.13 (0.04 to 2.71)	1.11 (0.03 to 2.74)	9	−0.60 (−0.89 to −0.26)	0.23 (0.01 to 0.59)	0.22 (0.01 to 0.59)
Delivery format								
Individual	45	−2.21 (−2.65 to −1.78)	0.82 (0.35 to 1.42)	0.90 (0.06 to 1.54)	39	−1.50 (−2.01 to −1.00)	0.68 (0.26 to 1.47)	1.17 (0.14 to 1.78)
Group/mixed	14	−1.60 (−2.27 to −0.93)	0.37 (0.01 to 1.19)	0.89 (0.10 to 1.67)	12	−1.13 (−1.79 to −0.46)	0.46 (0.01 to 1.34)	0.78 (0.05 to 1.60)
Trauma focus								
Trauma-focused	46	−2.15 (−2.57 to −1.74)	0.63 (0.23 to 1.31)	1.02 (0.12 to 1.56)	41	−1.39 (−1.85 to −0.94)	0.63 (0.23 to 1.40)	1.11 (0.14 to 1.66)
Non-trauma-focused	20	−1.12 (−1.76 to −0.44)	1.12 (0.28 to 1.84)	0.61 (0.02 to 1.59)	17	−0.97 (−1.52 to −0.43)	0.68 (0.03 to 1.43)	0.66 (0.03 to 1.41)
Cultural adaptation								
Culturally adapted	11	−1.43 (−1.95 to −0.91)	0.48 (0.03 to 1.10)	0.48 (0.03 to 1.10)	9	−0.83 (−1.58 to −0.07)	0.67 (0.03 to 1.60)	0.67 (0.03 to 1.61)
Not culturally adapted	54	−1.97 (−2.43 to −1.52)	0.60 (0.25 to 1.37)	1.33 (0.35 to 1.82)	48	−1.40 (−1.83 to −0.98)	0.58 (0.22 to 1.30)	1.15 (0.22 to 1.63)
Session number	61	0.01 (−0.06 to 0.08)	0.65 (0.30 to 1.26)	1.04 (0.17 to 1.50)	53	0.00 (−0.08 to 0.08)	0.58 (0.23 to 1.30)	1.17 (0.25 to 1.64)
Session duration	52	0.00 (−0.01 to 0.01)	0.73 (0.26 to 1.40)	0.99 (0.09 to 1.56)	44	0.00 (−0.01 to 0.01)	0.65 (0.12 to 1.47)	1.11 (0.12 to 1.68)
Treatment duration	59	0.06 (−0.01 to 0.14)	0.72 (0.31 to 1.43)	1.14 (0.14 to 1.66)	53	0.01 (−0.07 to 0.08)	0.59 (0.23 to 1.33)	1.16 (0.22 to 1.64)

Note: CrI, credible interval; k_t, number of comparisons; LMICs, low- and middle-income countries; NA, not applicable; PTSD, post-traumatic stress disorder.
[a]Level 1 estimates in subgroup analyses quantify the effect size Hedges' *g* (where zero indicates no difference in symptom severity between time points or groups). Moderation analysis of continuous variables presents the association of the investigated variables with the effect size Hedges' *g* (zero indicates no effect on Hedges' *g*). Level 2 estimates quantify how much estimates vary within studies. Level 3 estimates quantify the extent to which estimates vary between studies. 95% CrIs quantify the 95% interval in which the true estimate lies given the provided data. For results of dummy-coded meta-regressions for direct subgroup comparisons, see Supplementary Table S7.

between-group effect sizes, age was a significant moderator, indicating stronger effects in older populations. Regarding within-group effect sizes, trauma focus had a strong impact; trauma-focused interventions demonstrated stronger effect sizes than non-trauma-focused interventions, with Δg = 0.81, 95% CrI [0.11, 1.53]. Additionally, delivery method was an important factor: face-to-face delivery yielded a stronger effect size compared to non-face-to-face delivery, with Δg = 1.08, 95% CrI [0.08, 2.09].

Secondary outcomes: depression and anxiety

Psychosocial interventions for PTSD conducted in LMICs resulted in sustained, medium-to-large effects on depression and anxiety symptoms, as reflected in both within- and between-group effect sizes. Estimates of Hedges' *g* are presented in the Level 1 columns of Table 1.

Discussion

This meta-analysis examined the effectiveness of psychosocial interventions for PTSD among adult patients in LMICs. Large effects on PTSD and medium-to-large effects on depression and anxiety were observed and sustained both within intervention groups across assessments and between intervention and control conditions. Age, trauma-focus, and delivery method emerged as moderators of treatment effects.

The results demonstrate the effectiveness of psychosocial interventions for PTSD in LMICs. While this finding is consistent with previous meta-analyses on nonpharmacological PTSD interventions in these countries (Dowllah & Melville, 2024; Morina et al., 2017), the effect sizes observed in the current analysis are notably larger. Given that the effectiveness of PTSD interventions can vary across populations (Straud, Siev, Messer, & Zalta, 2019; Turrini et al., 2025), this difference seems plausible, as prior analyses focused on specific populations, namely refugees, asylum seekers, and victims of mass violence, whereas the present meta-analysis included a broader PTSD-affected population without such restrictions. Although population type (civilians vs. refugees vs. other) did not emerge as a relevant moderator within our sample (Table 2 and Supplementary Table S7), differences in trauma chronicity or multiplicity (e.g. single-incident vs. prolonged or repeated exposure) between our sample and those of prior meta-analyses, not captured by this categorical distinction, may still contribute to the observed discrepancy in effect sizes. Moreover, our findings indicate that the observed effect sizes are comparable to, and in some cases exceed, those reported in previous meta-analyses conducted in high-income countries (Kip et al., 2025). This suggests that psychosocial interventions for PTSD, which are typically developed and evaluated in Western contexts (Kaminer et al., 2024), can also be effectively implemented in LMICs. This is consistent with recent evidence on the effectiveness of psychotherapy across cultural regions and income levels (Harrer et al., 2025). Notably, only 17% of active treatment conditions in the current meta-analysis underwent cultural adaptation. The comparatively high effect sizes observed in our meta-analysis should be interpreted with caution, however. One plausible contributor is the relatively low methodological quality of trials conducted in LMICs, most of which were rated as having moderate to high risk of bias. A second contributor may lie in the content of control conditions. Notably, the discrepancy between our findings and those reported in meta-analyses including trials conducted in high-income countries appears most pronounced for comparison with active control conditions rather than with passive control conditions. For example, whereas Morina, Hoppen, & Kip (2021) reported a pooled effect size of $g = -0.47$ for comparison with active control conditions only, our corresponding estimate was $g = -1.11$ (see Table 2). This pattern may suggest that treatment-as-usual and other active control conditions are not directly comparable across settings, as routine or comparator care in LMICs may differ substantially in availability, content, and intensity from corresponding care in high-income countries. Disentangling these methodological and contextual explanations from genuine differences in intervention effectiveness will require future research. In addition to demonstrating substantial effects on PTSD symptoms, the interventions examined also led to sustained improvements in comorbid depression and anxiety, aligning with outcomes commonly reported in high-income countries (Ronconi, Shiner, & Watts, 2015; Yunitri et al., 2023). A plausible explanation for this effect is the symptom overlap between PTSD and depression, such as sleep difficulties and avoidance of thoughts, as shown in network analyses (Lazarov et al., 2020). Furthermore, shared transdiagnostic processes such as emotion dysregulation and rumination are associated with both PTSD and depression symptoms (Hernández-Posadas et al., 2024). These findings collectively support the successful implementation of psychosocial PTSD interventions in LMICs.

Several moderators of treatment effect were identified. First, stronger treatment effects were observed in older populations. Given that the study samples were relatively young overall (M = 35.91 years, SD = 8.49), treatment outcomes appeared to be less favorable among younger adults. This may be due to several factors; for instance, younger individuals have a greater likelihood of premature therapy discontinuation (Swift & Greenberg, 2012). This can contribute to poorer outcomes, as a higher number of completed treatment sessions has been associated with greater reductions in PTSD symptom severity (Lambert & Alhassoon, 2015; Leithner et al., 2026). Additionally, younger adults may have reduced autonomy in deciding when and how to engage in therapy, a factor that has been linked to increased side effects (Lorenz, 2021). This explanation may be especially pertinent in LMICs, where respect for parental authority is deeply embedded in cultural norms and may continue to influence young adults' autonomy in healthcare decisions (Salaam & Kyere, 2025). Evidence on the moderating role of age in high-income countries is scarce and mixed: one study found no significant age-related differences in treatment outcome following intensive trauma-focused treatment (Gielkens et al., 2021), whereas a meta-analysis of exposure-based therapies found worse outcomes among older participants (McLean, Levy, Miller, & Tolin, 2022). Hence, whether the age effect observed in the present meta-analysis generalizes to high-income settings remains unclear. Second, trauma-focused interventions demonstrated greater effectiveness compared to non-trauma-focused approaches. This finding aligns with clinical guidelines in high-income countries, which recommend trauma-focused psychological interventions as the first-line treatment for adults with PTSD symptoms (American Psychological Association, 2025; Martin et al., 2021). Despite various uncertainties and concerns raised in the literature regarding their implementation in LMICs (Booysen & Kagee, 2020; Chen, Olin, Stirman, & Kaysen, 2017; Kaminer et al., 2024), the results of the current meta-analysis suggest that this recommendation may also be applicable in these countries. Notably, trauma-focused interventions in the included trials were predominantly delivered by specialized practitioners (k_t = 39), that is, professionals with formal training in mental health or psychology (e.g. psychologists and licensed therapists), rather than practitioners with more basic

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training (k_t = 4; e.g. community volunteers and informal helpers). This is consistent with the majority of high-income country trials, where trauma-focused treatment is similarly delivered by trained specialists. Lastly, face-to-face interventions were found to be more effective than those delivered via non-face-to-face modalities. Of note, most non-face-to-face interventions in the included trials did not involve real-time telehealth (e.g. via video or phone), but instead relied on asynchronous formats such as websites (Miller-Graff, Ellis, & Hosny, 2021; Wang, Wang, & Maercker, 2013; Xu et al., 2016), apps (Bröcker et al., 2024), or text messaging-based programs (Darvish, Khodadady, Abdoosti, & Ghapandar Kashani, 2019). Furthermore, most of these asynchronous interventions were self-guided (Bröcker et al., 2024; Miller-Graff et al., 2021; Wang et al., 2013; Xu et al., 2016), which are known to be less effective than therapist-guided formats (Lewis et al., 2019).

The current meta-analysis has several strengths, including its novelty as the first comprehensive investigation of psychosocial interventions for PTSD in LMICs, a large sample size (n = 5,041), and adherence to best practices, such as compliance with PRISMA guidelines (Moher et al., 2009) and the use of Bayesian statistics appropriate for small sample sizes within trials (Borenstein et al., 2009). Nevertheless, several limitations should be acknowledged. First, a significant proportion of the included studies were rated as having a moderate or high risk of bias, which may affect the reliability of the findings. Second, both visual inspection of the funnel plot and results from Egger's test suggested the presence of potential publication bias, raising the possibility that studies with less favorable results were underrepresented. Third, the broad scope of the analysis introduced substantial heterogeneity due to the inclusion of diverse interventions, populations, and control conditions. While extensive moderator analyses sought to explain at least some of this heterogeneity, one plausible source could not be fully accounted for: the type of trauma experienced by the study population. Subgroups of most single trauma types were too small to permit meaningful analyses (e.g. accidents, k_t = 2; combat-related, k_t = 1; intimate partner violence, k_t = 1). A considerable number of studies, moreover, did not involve a single, specific trauma type, either because no trauma type was reported (k_t = 9) or because the population experienced mixed trauma types (k_t = 18). As an illustrative exception, a subgroup analysis was conducted for terrorism/political violence/forced displacement, the only specific trauma type with a sufficient number of comparisons, yielding effect sizes closely comparable to the overall pooled estimates (between-group analysis [k_t = 16]: g = −1.20; 95% CrI, −1.71 to −0.68; within-group analysis [k_t = 18]: g = −1.92; 95% CrI, −2.47 to −1.39). Fourth, only 60% of included trials reported that all participants had a formal PTSD diagnosis at study inclusion; furthermore, trauma exposure was not applied as a separate eligibility criterion, meaning that not all included trials necessarily verified trauma exposure. As such, the findings may not fully generalize to populations with diagnosed PTSD and confirmed trauma exposure. Fifth, although analyses accounted for income level, the predominance of trials from upper-middle-income countries limits the extent to which the findings can be confidently applied to low- and lower-middle-income settings, where contextual factors may differ substantially and relevant data remain sparse. Sixth, some moderator analyses relied on a limited number of comparisons in one subgroup (e.g. psychotropic medication not allowed, k_t = 7; non-face-to-face delivery, k_t = 9), reducing the precision of these estimates and the ability to detect credible differences between subgroups, warranting cautious interpretation of these specific results. Finally, patient-relevant outcomes, such as functioning and quality of life, could not be considered, as only a few of the included RCTs (< 10) reported these measures, limiting the clinical applicability of the findings.

The key clinical implication emerging from this meta-analysis is that individuals with PTSD symptoms in LMICs should be offered trauma-focused psychosocial treatment, as also recommended by current guidelines from high-income countries (American Psychological Association, 2025; Martin et al., 2021). As trauma-focused treatment in the included trials was predominantly delivered by specialized practitioners, this recommendation applies most directly where such treatment can be delivered by, or under the supervision of, appropriately trained providers. Preferably, this treatment should be delivered face-to-face, as there is limited evidence on the effectiveness of real-time telehealth interventions in LMICs, and asynchronous non-face-to-face treatments appear less effective. Since PTSD treatment also reduced comorbid depression in the present meta-analysis, and as depression-focused treatment has not been shown to improve PTSD, PTSD treatment should be applied first when PTSD and depression co-occur, with residual depressive symptoms addressed thereafter (Rosen, Ortiz, & Nemeroff, 2020), including in LMICs. This asymmetry may be explained by the fact that depressive symptoms frequently develop secondary to PTSD. The present investigation also highlights several implications for future research. There is a need for more high-quality RCTs on PTSD treatment in LMICs, particularly in low- and lower-middle-income countries. The applicability of the current findings to populations with a formal PTSD diagnosis also remains to be clarified. In addition, the weaker treatment effects observed in younger patients warrant further investigation, alongside the development of suitable treatment alternatives. Furthermore, given the clinical complexity of delivering trauma-focused interventions, future research should examine whether structured training and supervision models could enable nonspecialist providers to safely and effectively deliver (components of) trauma-focused interventions, in light of ongoing efforts to expand access to mental health care in LMICs through task-shifting (Faregh et al., 2019). Finally, future research would benefit from investigating additional treatment outcomes and moderating factors beyond those considered in the present analysis.

Conclusion

The current meta-analysis indicates that psychosocial interventions for PTSD in LMICs result in meaningful reductions in PTSD, depression, and anxiety symptoms both in the short and long term. These treatments appear particularly effective when trauma-focused, delivered face-to-face, and administered to older participants. Moving forward, there is a clear need for more high-quality research on PTSD treatment in LMICs, especially in low- and lower-middle-income countries.

Supplementary material. The supplementary material for this article can be found at http://doi.org/10.1017/S0033291726105807.

Data availability statement. The data and analytical code supporting the findings are openly available on OSF: https://osf.io/kzf74/. This step was taken to maximize the transparency and reproducibility of results. All correspondence should be addressed to the corresponding author; without permission of the corresponding author, data may not be used for purposes other than reproduction of the present results.

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Supervision: Y.T., T.E., V.S.; Validation: C.L., Y.T.; Visualization: C.L., L.H.; Writing – original draft: C.L.; Writing – review and editing: C.L., S.G., R.N., L.H., Y.T., T.E., V.S.

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