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INPLASY

Efficacy of Psychological Interventions for Posttraumatic Stress Disorder in Youth Exposed to Multiple, Repeated, or High-Adversity Trauma: A Meta-analysis of Randomized Controlled Trials

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ADMINISTRATIVE INFORMATION

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Review stage at the time of initial registration - Completed but not published.

Conflicts of interest - None declared.

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Amendments - This protocol was registered with the International Platform of Registered Systematic Review and Meta-Analysis Protocols (INPLASY) on 12 November 2026 and was last updated on 12 November 2026.

INTRODUCTION

Review question / Objective Youth exposed to repeated or sustained high-adversity trauma are at increased risk of persistent posttraumatic stress disorder (PTSD). This meta-analysis estimated the efficacy and certainty of psychological interventions for PTSD symptoms in this population.

Condition being studied On psychological interventions for PTSD or clinically significant posttraumatic stress symptoms among children and adolescents exposed to multiple, repeated, chronic, interpersonal, war-related, displacement-related, or other sustained high-adversity trauma.

METHODS

Participant or population Children, adolescents, or youth-dominant samples with a mean age below 21 years who had full PTSD, subthreshold PTSD, or clinically significant posttraumatic stress symptoms after multiple, repeated, chronic, interpersonal, war-related, displacement-related, abuse-related, community-violence-related, bereavement-related, or other sustained high-adversity trauma exposure.

Intervention A psychological or psychosocial treatment targeting PTSD symptoms.

Comparator Waitlist, minimal service, or usual care without structured trauma-focused psychotherapy.

Study designs to be included Individually or cluster-randomized controlled trials.

Eligibility criteria Eligibility was defined using the PICOS framework. Population: children, adolescents, or youth-dominant samples with a mean age below 21 years who had full PTSD, subthreshold PTSD, or clinically significant posttraumatic stress symptoms after multiple, repeated, chronic, interpersonal, war-related,

displacement-related, abuse-related, community-violence-related, bereavement-related, or other sustained high-adversity trauma exposure. Full PTSD, subthreshold PTSD, and clinically significant symptom samples were combined because pediatric treatment trials commonly use validated symptom thresholds when structured diagnosis is not feasible, and clinically significant subthreshold presentations can still involve substantial impairment [2]. A diagnostic-ascertainment sensitivity analysis was used to assess robustness to this eligibility breadth. Intervention: a psychological or psychosocial treatment targeting PTSD symptoms. Comparator: waitlist, minimal service, or usual care without structured trauma-focused psychotherapy. Outcome: post-treatment PTSD symptom severity reported with sufficient data to calculate a standardized effect. Study design: individually or cluster-randomized controlled trials. Eligibility was based on trial-level descriptions of repeated or multiple exposure, or exposure within a sustained high-adversity context; a single trauma-category label was not treated as evidence of a single event.

Information sources PubMed/MEDLINE, Embase, PsycINFO via Ovid, Web of Science Core Collection, Cochrane CENTRAL, and PTSDpubs were searched from database inception through June 30, 2026, without restrictions on publication year or language.

Main outcome(s) Post-treatment PTSD symptom severity reported with sufficient data to calculate a standardized effect.

Quality assessment / Risk of bias analysis Risk of bias for the primary post-treatment PTSD outcome was assessed using the Cochrane Risk of Bias 2 (RoB 2) tool and its cluster-randomized variant when applicable. The assessment covered the randomization process, deviations from intended interventions, missing outcome data, measurement of the outcome, and selection of the reported result, with the additional cluster-specific recruitment considerations where relevant. Domain-level and overall judgments were categorized as low risk, some concerns, or high risk. Measurement-of-outcome judgments reflected the specific outcome instrument, the masking of assessors and respondents, and the subjectivity of symptom reporting; selective-reporting judgments reflected the availability and detail of protocols, registrations, analysis plans, outcome multiplicity, and reported time points. Trial-level judgments are reported in Supplementary Table S1, with trial-specific rationales for Domains 4 and 5 in Supplementary Table S2.

Strategy of data synthesis The primary analysis pooled post-treatment Hedges g values using a random-effects model with restricted maximum likelihood estimation. Heterogeneity was summarized with I2, tau2, Cochran Q, and a 95% prediction interval. Interventions were classified as TF-CBT, TF-CBT-related, EMDR, multidisciplinary treatment (MDT), or other/non-trauma-focused. Developmentally adapted cognitive processing therapy (D-CPT) was classified as TF-CBT-related because cognitive processing therapy is a distinct trauma-focused cognitive protocol rather than standard TF-CBT. Subgroup analyses examined intervention family and control type. Sensitivity analyses excluded trials at high overall risk of bias, restricted the synthesis to trials using diagnostic or structured PTSD eligibility ascertainment, and removed each study in turn. Funnel plots and an Egger-type regression explored small-study effects, complemented by precision-effect testing and precision-effect estimate with standard error (PET-PEESE) . The 14 included trials are cited in Table 1 . Barron et al. randomized individual participants, whereas Santiago et al. was confirmed as a school-level cluster-randomized trial (eight schools; mean cluster size approximately 6.5). Because the Santiago trial did not report an extractable intracluster correlation coefficient (ICC), no unverifiable single ICC was imposed on the primary model; instead, its standard error was inflated in design-effect sensitivity analyses using DE = 1 + (m - 1)ICC with assumed ICCs of 0.01, 0.05, and 0.10. Exploratory univariable meta-regressions examined publication year, log-transformed sample size, and high overall risk of bias; multivariable models were not fitted because only 14 trials were available. Baujat plots and standard influence diagnostics identified studies contributing disproportionately to heterogeneity or the pooled estimate. Certainty of evidence for the primary outcome was rated using the GRADE domains of risk of bias, inconsistency, indirectness, imprecision, and publication bias . Analyses were conducted in R using meta and metafor.

Subgroup analysis Subgroup estimates differed by intervention family (chi-square = 10.57; df = 4; p = 0.0319). The pooled estimate was 0.42 (95% CI, 0.02-0.83) for EMDR, 0.69 (95% CI, 0.43-0.95; I2 = 0%) for TF-CBT-related interventions, 0.93 (95% CI, 0.63-1.22; I2 = 0%) for multidisciplinary treatments, and 1.52 (95% CI, 0.79-2.26; I2 = 92.9%) for TF-CBT; the single other/non-trauma-focused trial had an effect of 1.30 (95% CI,

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0.62-1.98). The TF-CBT subgroup remained highly heterogeneous, reflecting very large effects in smaller war-affected samples and a smaller, more precise effect in the usual-care trial (Figure 3).
The pooled estimate was larger for passive/waitlist controls (1.06; 95% CI, 0.72-1.40; I2 = 74.9%) than for usual care (0.52; 95% CI, 0.36-0.68). The subgroup difference was statistically significant (chi-square = 7.89; df = 1; p = 0.0050), but the usual-care subgroup contained only Dorsey et al. Although this trial contributed 640 of 1468 participants (43.6%), its approximate weight in the random-effects model was 9.1%, because between-study heterogeneity limited the dominance of large studies (Figure 4).

Sensitivity analysis Excluding the three trials at high overall risk of bias (Chen et al., Al-Hadethe et al., and Banoglu and Korkmazlar) produced a pooled effect of 1.07 (95% CI, 0.68-1.47; 11 RCTs; N = 1345; I2 = 84.0%; tau2 = 0.358), indicating that the primary conclusion was not driven by high-risk trials. Restricting the analysis to six trials in which PTSD eligibility was established using diagnostic criteria with structured clinical assessment, standardized diagnostic interviewing, or psychiatric confirmation yielded g = 0.85 (95% CI, 0.54-1.17; N = 336; I2 = 41.9%; tau2 = 0.061). Design-effect adjustment for the Santiago cluster trial using assumed ICCs of 0.01, 0.05, and 0.10 left the overall pooled effect essentially unchanged at approximately 1.01, with 95% CIs remaining about 0.69-1.34. Detailed sensitivity results are reported in Supplementary Table S4.

Country(ies) involved China.

Keywords Post-Traumatic Stress Disorders; Child; Adolescent; Psychotherapy; Randomized Controlled Trials as Topic.

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