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Review

The role of dosage and intensity in trauma-focused treatments for posttraumatic stress disorder: A meta-analysis

Cosima Leithner [a,*], Bianca Schreyer [b], Sara Däumling [a], Verena Semmlinger [a], Thomas Ehring [a,c]

[a] Department of Psychology, LMU Munich, Germany
[b] Department of Psychology, Catholic University Eichstaett-Ingolstadt, Germany
[c] German Center for Mental Health (DZPG), Munich, Germany

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ABSTRACT

Trauma-focused psychological interventions are widely recognized as the most efficacious treatments for posttraumatic stress disorder (PTSD); however, high rates of non-response and treatment discontinuation remain significant challenges. Modifying treatment dosage and intensity has been proposed as a promising strategy to enhance both efficacy and acceptability. This meta-analysis aimed to investigate whether factors related to treatment dosage and intensity moderate effects on PTSD and depression symptom severity, as well as treatment completion rates, in guideline-recommended PTSD treatments (PROSPERO: CRD42023485646). A systematic literature search was conducted in the PTSD Trials Standardized Data Repository, databases, and previous meta-analyses. Eligible trials were randomized controlled trials (RCTs) evaluating trauma-focused interventions delivered individually and in person to adults diagnosed with PTSD. Studies published until October 6, 2025, were considered for inclusion. Study quality was assessed using Cochrane's RoB 2 tool. Moderator analyses were conducted through meta-analyses of head-to-head trials comparing arms with deliberate variations in dosage or intensity, and meta-regression analyses. A total of 73 RCTs, comprising 5696 participants, were included. Interventions showed medium to large overall effects on PTSD and depression, with an average completion rate of 71.8%, 95% CI [68.3%, 75.0%]. Moderator analyses indicated weaker between-group effect sizes for PTSD with a greater number of trauma-focused sessions, stronger effect sizes with a higher number of completed sessions and personalized session numbers, and higher completion rates for highly intensive treatments. These findings suggest that PTSD treatment outcomes may be influenced by dosage and intensity, with personalizing session numbers and increasing treatment intensity appearing particularly beneficial.

1. Introduction

Posttraumatic stress disorder (PTSD) is a prevalent, debilitating mental health condition with significant individual and societal impact. Based on survey data from 24 countries, 70.4% of individuals experience traumatic events in their lifetime (Kessler et al., 2017), with PTSD affecting 5.6% of exposed individuals and 3.9% of the general population (Koenen et al., 2017). PTSD is associated with severe mental and physical health outcomes, including increased suicide risk (LeBouthillier, McMillan, Thibodeau, & Asmundson, 2015; Panagioti, Gooding, & Tarrier, 2012) and a higher prevalence of medical conditions (Pacella, Hruska, & Delahanty, 2013), resulting in substantial economic costs comparable to other high-burden mental disorders (Davis et al., 2022). Numerous PTSD-specific psychological interventions have been developed and their efficacy has been examined in a large number of randomized controlled trials (RCTs). Based on meta-analytic findings, several trauma-focused psychological interventions can be considered as first-line treatments for PTSD and are therefore recommended in current guidelines (see Martin et al., 2021, for an overview). However, significant treatment challenges remain. In their meta-analysis, Semmlinger et al. (2024) estimated a nonresponse rate of 39.2% for guideline-recommended psychological interventions, indicating a substantial subgroup of patients who continue to experience clinically significant symptoms posttreatment. Additionally, treatment dropout remains a concern, with an average rate of 20.9% found across studies implementing guideline-recommended treatments (Varker et al., 2021). Other meta-analyses examining a broader range of psychological PTSD interventions reported dropout rates of 18% (Imel, Laska,

* Corresponding author at: Department of Psychology, LMU Munich, Leopoldstr. 13, 80802 Munich, Germany.
E-mail address: cosima.leithner@psy.lmu.de (C. Leithner).

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Jakupcak, & Simpson, 2013) and 16% (Lewis, Roberts, Gibson, & Bisson, 2020), respectively. Treatment outcomes for PTSD, including efficacy (Morina, Hoppen, & Kip, 2021) and dropout rates (Varker et al., 2021), exhibit substantial heterogeneity across RCTs, suggesting the presence of moderating factors. Identifying these moderating factors is a crucial step toward optimizing treatment outcomes for individuals with PTSD.

Potential moderators may include variables related to patient, treatment, therapist, and study characteristics. In the context of treatment-related variables, the PTSD literature broadly distinguishes between trauma-focused and non-trauma-focused interventions, with the former consistently showing greater efficacy (e.g., Hoppen, Jehn, et al., 2023; McLean, Levy, Miller, & Tolin, 2022b). Building on this finding, recent literature has increasingly focused on comparing different trauma-focused treatment manuals to determine whether a particular approach is superior. Various primary studies and meta-analyses have investigated trauma-focused treatments through both direct and indirect comparisons among them (e.g., Hudays, Gallagher, Hazazi, Arishi, & Bahari, 2022; Jericho, Luo, & Berle, 2022). Results have not revealed any systematic differences between different manuals; instead, the efficacy of the main evidence-based psychological treatments for PTSD has to be considered as comparable (Kline, Cooper, Rytwinksi, & Feeny, 2018). In addition to manual-specific active treatment components or mechanisms, common factors shared across psychological treatments could also moderate treatment outcomes. For example, the mode of delivery (Scott et al., 2022) and therapeutic alliance (Howard, Berry, & Haddock, 2022) have been investigated as potential moderators of PTSD outcomes. Investigating such broader variables could reveal new opportunities to enhance psychotherapy outcomes.

Possible treatment-related moderators relevant to all psychological PTSD interventions include dosage and intensity of treatment. Previous primary studies have yielded conflicting findings on the necessary treatment dosage for clinical improvement. Several authors suggest that patients may either improve early on in PTSD therapy or not at all. For example, Holder et al. (2020) reported a median effective dose of only four sessions for prolonged exposure (PE), while findings from Ready, Lamp, Rauch, Astin, and Norrholm (2020) and Sripada, Ready, Ganoczy, Astin, and Rauch (2020) suggest that a lack of symptom reduction by session eight may predict poor treatment response for PE and Cognitive Processing Therapy (CPT), respectively. However, Galovski, Blain, Mott, Elwood, and Houle (2012) found that 58% of participants who received CPT reached end-state criteria before the 12th session, 8% reached it at session 12, and 34% between sessions 12–18, suggesting that some patients may require more sessions for clinical improvement. Therefore, the primary studies do not provide clear conclusions about whether higher treatment dosage moderates better average outcomes in psychological PTSD treatment, and this uncertainty is also reflected in aggregated analyses. While Asmundson et al. (2019), Kline et al. (2018), and McLean, Levy, Miller, and Tolin (2022a) all found that the number of sessions, duration of acute treatment, or both did not significantly moderate treatment outcomes, McLean et al. (2022b) reported that exposure treatments with fewer sessions were associated with larger effect sizes. According to the authors, conceivable explanations are higher dropout rates in longer treatments and a confound between symptom severity or complexity and number of sessions, in the sense that more complex cases are treated with protocols comprising more sessions. In contrast, Haagen, Smid, Knipscheer, and Kleber (2015) found that the number of trauma-focused sessions, rather than the total number of psychotherapy sessions, positively predicted treatment outcomes. Variables related to treatment dosage beyond the number of sessions have also received attention in primary studies on PTSD. Examples include session duration (Foa et al., 2022; Nacasch et al., 2015) and whether treatment length is tailored to patients' needs (Galovski et al., 2012; Resick et al., 2021), with the latter shown to increase the likelihood of patients achieving a good end-state. Understanding the optimal amount of psychotherapy is crucial not only for improving individual patient outcomes but also for improving treatment allocation. Specifically, optimizing access to treatment could enhance cost-effectiveness, help manage provider workload, balance patient demand with provider availability, and minimize the opportunity cost of missed appointments (Foa et al., 2022; Kazdin & Blase, 2011).

In contrast to the mixed findings on treatment dosage, evidence on how treatment intensity affects outcomes is more consistent. While Gutner, Suvak, Sloan, and Resick (2016) found that more frequent sessions led to greater PTSD symptom reduction, most evidence suggests that higher treatment intensity (i.e., multiple sessions per week) is just as efficacious as less intense treatment (i.e., once per week) in reducing PTSD symptoms (Hoppen, Kip, & Morina, 2023; Sciarrino, Warnecke, & Teng, 2020; Wachen, Dondanville, Evans, Morris, & Cole, 2019). Nevertheless, offering more intensive treatments may be beneficial because of the potential for faster improvement. In addition, higher-frequency treatment delivery has been associated with significantly lower dropout rates compared to standard-frequency treatment, particularly in trauma-focused interventions (Hoppen, Kip, et al., 2023; Levinson, Halverson, Wilson, & Fu, 2022; Sciarrino et al., 2020). However, it remains unclear whether this reduction in dropout also translates to higher treatment completion rates. Dropout is a broad concept that includes various aspects, including discontinuing treatment before reaching therapeutic goals, failing to complete the full course of therapy, or not attaining the full therapeutic benefit (Swift & Greenberg, 2012). Therefore, a lower dropout rate does not necessarily imply higher treatment completion, as individuals may remain in therapy longer without necessarily completing the full protocol. Previous meta-analyses on treatment intensity have operationalized dropout by using the number of participants missing post-treatment assessments as a proxy (Hoppen, Kip, et al., 2023) or by adopting individual studies' definitions without modification (Levinson et al., 2022). Focusing on treatment completion will provide evidence on whether more intensive treatment not only results in enhanced retention, but additionally in increased engagement to treatment. This seems crucial since sustained attendance in trauma-focused PTSD interventions has been shown to yield superior symptom improvement (Berke et al., 2019; Holmes et al., 2019). It is yet unclear whether treatment intensity has an impact on the reduction of depression symptom severity. In the depression literature, increasing the frequency of sessions has been strongly associated with larger treatment effects on depression symptoms (Cuijpers, 2024). Whether more frequent PTSD treatment may similarly lead to greater improvements in depression symptoms among PTSD patients remains to be tested. Since higher depression severity has been shown to predict smaller treatment effect sizes in PTSD treatment (Kline, Cooper, Rytwinski, & Feeny, 2021), understanding this relationship is essential for optimizing treatment outcomes.

A comprehensive meta-analysis, similar to those conducted by Ciharova et al. (2024) and Cuijpers, Huibers, Ebert, Koole, and Andersson (2013) for psychological treatments of depression, has not yet been conducted for PTSD. Therefore, the current meta-analysis aimed to address the following question: To what extent do treatment dosage and intensity moderate the efficacy in terms of PTSD and depression, as well as the completion of guideline-recommended psychological interventions for adult patients with PTSD in RCTs?

2. Method

The aims and methods of this meta-analysis were pre-registered in PROSPERO (CRD42023485646), with minor protocol deviations described in Supplementary Material A. The meta-analysis is reported according to the PRISMA guidelines (Moher, Liberati, Tetzlaff, & Altman, 2009; see Supplementary Material B for the PRISMA checklist) and adheres to the AMSTAR criteria (Shea et al., 2017).

2.1. Identification and selection of studies

To be considered eligible, trials had to meet the following inclusion criteria: (a) published in English; (b) published in a peer-reviewed journal; (c) random group allocation of participants; (d) treatment under investigation was a psychological trauma-focused guideline-recommended intervention and PTSD was the primary treatment target in the study; (e) treatment delivery was individual and face-to-face[1]; (f) inclusion of either (i) a study arm that did not receive a guideline-recommended intervention and thus served as a control condition in the current meta-analysis, or (ii) a comparator arm that received the same guideline-recommended intervention as the experimental group, with an intentional variation in treatment dosage or intensity, thereby enabling a head-to-head comparison of different doses or intensities; (g) age of participants ≥16 years; (h) all participants met criteria for a PTSD diagnosis at study inclusion according to DSM-IV, DSM-5, ICD-10 or ICD-11 as established with a structured clinical interview; (i) the number of observations, mean, and standard deviation of PTSD measure at post-assessment and/or the completion rate were reported separately for each treatment condition; and (j) post-assessment was conducted no later than six weeks after the end of treatment. Whether an intervention was considered guideline-recommended was based on the APA Clinical Practice Guideline for PTSD in Adults (American Psychological Association, 2025), which recommends CPT, Cognitive Therapy (CT), Eye Movement Desensitization and Reprocessing (EMDR), Narrative Exposure Therapy (NET), PE, and Trauma-Focused Cognitive Behavioral Therapy (CBT) as first- and second-line psychological interventions. The strict inclusion criterion for PTSD diagnosis was chosen to limit heterogeneity and maximize comparability. Studies focusing on patients with severe cognitive impairment, combining psychological treatment with medication or placebo, or using virtual reality were excluded.

The literature search was conducted using the PTSD Trials Standardized Data Repository[2] (PTSD Repository), a publicly accessible database of RCTs on PTSD treatment (O'Neil et al., 2020; O'Neil et al., 2024). For our original search, the PTSD Repository was current through April 1, 2024. The Repository was updated again during the final phase of our work to include studies published up to April 17, 2025. To identify all studies published after April 1, 2024, and subsequently after April 17, 2025, a database search was conducted in Embase, Medline, PsycINFO, and PTSDpubs, using an adapted version of the search string from the PTSD Repository. As a result, the current meta-analysis includes studies published through October 6, 2025. Additionally, a backward search was conducted by screening studies included in 38 meta-analyses relevant to the topic to identify potentially eligible RCTs not included in the PTSD Repository. Details for each step of the search process are provided in Supplementary Material C.

The PTSD Repository was filtered to include only studies that utilized a structured clinical interview for diagnostic assessment and applied an intervention categorized as individually delivered, in-person psychological treatment. Titles and abstracts of the remaining studies, as well as those identified in the database and backward searches, were reviewed for eligibility by two independent researchers (S.D., C.L., and/or B.S.). If deemed eligible, full texts were independently reviewed by two of the named researchers. Any discrepancies were resolved through discussion and, if necessary, consultation with the remaining team members (T.E. and V.S.).

[1] Trials that specifically describe a transition from face-to-face therapy to telehealth therapy due to COVID-19 restrictions were included, as these interventions were initially designed for in-person implementation. This approach also helped prevent the systematic exclusion of trials conducted throughout the pandemic.

[2] The PTSD Repository is updated annually; therefore, the most recent available version was used.

2.2. Data extraction

Data extraction was conducted in two steps. First, relevant data available in the PTSD Repository was retrieved. Second, variables of interest not reported in the PTSD Repository or reported only at the study level but required at the treatment arm level were extracted directly from the studies by two independent researchers (S.D., C.L., and/or B.S.). For this latter step, a predetermined extraction manual and a form specifically designed for the current meta-analysis were used. If a study included more than one control group, only the least invasive one was considered. Any discrepancies were resolved through discussion and, if necessary, consultation with the remaining team members (T.E. and V.S.). Study authors were contacted to obtain missing data on primary outcomes, treatment dosage, and intensity. Studies were excluded if neither treatment completion rates nor the data necessary to calculate effect sizes could be retrieved.

Data related to the outcome variables, as well as treatment dosage and intensity were extracted for the moderator analyses, while study characteristics, sample characteristics, and other treatment variables were extracted for descriptive purposes or included in the analyses as control variables.

2.2.1. Outcome variables

The primary outcomes of the current meta-analysis are PTSD symptom severity at post-assessment and treatment completion rate. Secondary outcomes include PTSD symptom severity at follow-up assessment and depression symptom severity at post-assessment. For PTSD at both post- and follow-up assessment, data retrieved from the PTSD Repository included the PTSD instrument used, the type of analysis applied (intention-to-treat [ITT] or completer analysis [CA]), the length of the follow-up assessment from post-assessment (in weeks), the number of observations per study arm, and the mean and standard deviation of PTSD measures. If multiple PTSD outcome measures were assessed and reported, clinician-based PTSD measures were prioritized over self-report measures, and ITT data were prioritized over completer data when both were reported. In cases where multiple follow-up assessments were conducted, only the latest one available was extracted. Additionally, the definition of treatment completion, the number of treatment completers, the depression instrument used, and the mean and standard deviation of depression measures at post-assessment were directly extracted from the studies.

2.2.2. Variables related to treatment dosage and intensity

All variables related to dosage and intensity of treatment refer exclusively to the delivered, guideline-recommended intervention. Therefore, if a non-guideline-conforming treatment add-on was used, it was not included in the extraction of dosage and intensity variables. The following variables related to treatment dosage and intensity were extracted directly from the studies: intended number of sessions, mean number of completed sessions, number of trauma-focused sessions, whether the number of sessions was adapted to patients' needs (yes/no), intended treatment duration in weeks, session duration in minutes, and intended treatment frequency in minutes per week. Additionally, the total contact time for each active treatment condition was calculated by multiplying the intended number of sessions by the session duration. If the session duration or treatment frequency varied during the course of therapy, the average number of minutes per session and per week was calculated. If a range of intended sessions and treatment duration was reported instead of a fixed number, the mean value was used. For the adaptation of session numbers to patients' needs, "yes" was coded when the number of sessions within a study was flexible, determined based on factors such as reaching end-state criteria or agreement between the therapist and patient.

2.2.3. Other study, sample and treatment variables

The following variables were extracted for descriptive purposes or to

be included in the analyses as control variables (variables retrieved from the PTSD Repository are marked with an asterisk): (1) Study characteristics: publication year*, country of conduct*, control group type (waiting list/treatment as usual [TAU]/active control), structured clinical interview used for diagnostic assessment*, DSM version (DSM-IV/DSM-5), allowance of psychotropic medication* (yes/no); (2) Sample characteristics: race* (percentages of White, Black, American Indian/Alaska Native [AIAN], Asian, Native Hawaiian/Pacific Islander [NHPI], other), population type (veterans/active military/mixed veterans and active military/police officers/firefighters/healthcare professionals/refugees/civilians/mixed), trauma type* (accidents/child sexual abuse/combat-related/illness/military-related/political violence/sexual assault/mixed), age (mean), gender (percent female), marital status (percent married or in committed relationships), education (percent with high-school-level education, percent with college-level-education), employment status (percent in full-time or part-time employment or education), comorbid depression (percent); (3) Treatment variables: intervention type (CPT/CT/PE/NET/other CBT intervention/EMDR), treatment focus* (PTSD/PTSD + substance use disorder [SUD]), setting* (outpatient clinic/residential inpatient/mixed), whether providers had a graduate degree* (yes/no).

2.3. Quality assessment

The risk of bias (RoB) of included studies 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 studies not included in the PTSD Repository, a rating was performed by two independent researchers (C.L. and B.S.). The assessment evaluated different biases represented by five domains: randomization process, deviation from intended intervention, missing outcome data, measurement of outcome, and selection of the reported results. The overall RoB rating was derived from the ratings within these domains.

2.4. Statistical analysis

2.4.1. Overall estimates of efficacy and completion

The effect size Hedges' g was calculated for PTSD and depression symptom severity by subtracting the mean of the control group from the mean of the intervention group for the specified comparison at the respective assessment time point. The resulting difference was divided by the pooled standard deviation and adjusted using the sample size correction factor $J = 1 - \frac{3}{4df-1}$. Standardizing mean differences in this manner ensures that effect sizes are comparable across studies, regardless of the measures used. The completion rate was defined as the proportion of patients who completed treatment in a given condition relative to the total number of patients in that condition.

To establish overall estimates of efficacy and completion, independent of treatment dosage and intensity, overall analyses were conducted across all eligible trials. Multilevel models were used to estimate pooled effect sizes and the average weighted completion rates, with the latter being calculated as log-transformed proportions. Random effects models were chosen due to the anticipated substantial heterogeneity in outcomes, given the broad scope of the research question, which encompassed a variety of psychological interventions and diverse populations. While three-level models were conceptually suitable for the nested structure of the data (i.e., multiple active treatment conditions within a study), they were not statistically suitable for all outcomes. Accordingly, two-level or three-level models were employed based on which approach yielded lower AIC and BIC values.

Cochran's Q and I^2 statistics were used to assess heterogeneity in effect sizes and completion rates. I^2 between 0% and 40% was interpreted as potentially not important, 30%–60% as moderate, 50%–75% as substantial, 75%–90% as substantial to considerable, and > 90% as considerable (Deeks et al., 2019). Additionally, when between-study heterogeneity was present, prediction intervals (PIs) were calculated to provide the 95% range of true effects expected in similar studies, offering a more intuitive interpretation of heterogeneity than the I^2 statistics (IntHout, Ioannidis, Rovers, & Goeman, 2016). For the primary outcome PTSD symptom severity at post-assessment, publication bias was assessed for the overall efficacy estimate by inspecting the funnel plot and performing Egger's test of asymmetry (Egger, Smith, Schneider, & Minder, 1997). Since asymmetry was indicated, the trim-and-fill method was applied to calculate the asymmetry-adjusted estimate (Duval & Tweedie, 2000).

2.4.2. Moderator analyses

The analyses to investigate how dosage and intensity moderate treatment efficacy and completion involved two steps. First, head-to-head trials were analyzed, comparing two arms both providing guideline-recommended psychological interventions with deliberate variation in dosage or intensity between randomly allocated conditions. To conduct meta-analyses, a sufficient number of trials focusing on the same dosage or intensity variable was required ($k \geq 4$). For the meta-analysis of completion rates in head-to-head trials, the Odds Ratio (*OR*) was calculated to represent the relative completion rate of a treatment condition compared to an active comparator. Second, univariate meta-regression analyses were conducted to examine the relationship between each dosage and intensity variable and the following outcomes: (1) between-group effect sizes for PTSD at post-assessment and (2) at follow-up, (3) between-group effect size for depression at post-assessment, and (4) completion rates. If an indicator of dosage or intensity emerged as a significant predictor in a univariate meta-regression, a multiple meta-regression was subsequently performed to assess the extent to which the predictive effect remained significant after adjusting for patient attributes, study characteristics, and other treatment variables. A hierarchical, block-based approach was employed, where control variables were added to the model first, followed by the indicator of dosage or intensity. Control variables included z-standardized pre-treatment PTSD symptom severity, type of control group (waiting list/TAU/active control), type of assessment instrument (structured clinical interview/self-report), overall risk of bias rating (low/some concerns/high), and intervention type (CPT or CT/PE/NET or other CBT intervention/EMDR). To be included in the meta-regression analysis, each subgroup of a categorical variable had to include at least four comparisons.

2.4.3. Sensitivity analyses

Sensitivity analyses were conducted to assess the robustness of the findings under different conditions. First, analyses were performed including only studies with the same type of control condition. Trials comparing the guideline-recommended intervention with a waiting list or TAU were labeled as having an inactive control condition, while all other trials were categorized as having an active control condition. Second, sensitivity analyses for the primary outcome (PTSD symptom severity at post-assessment) were conducted, including only trials using PE as the intervention, as it was the most commonly provided treatment in the included studies. Within these sensitivity analyses, overall estimates of treatment efficacy were calculated. Furthermore, meta-regression analyses were conducted using variables related to dosage or intensity, as described above, provided that at least 10 comparisons ($k \geq 10$) were available for each analysis (Harrer, Cuijpers, Furukawa, & Ebert, 2021).

All analyses were conducted using the *metafor* package (version 4.6.0) in R (version 4.3.0) for Windows (Viechtbauer, 2010).

3. Results

3.1. Selection process and characteristics of included trials

After identifying 2983 records, 592 trials were excluded through

filtering of the PTSD Repository or duplicate removal following the database search. As a result, 2391 titles and abstracts were screened, and 175 proceeded to full-text review (see Supplementary Material D for a list of trials excluded after full-text screening). Ultimately, 73 RCTs (k_s) were included, reporting data on 92 active treatment conditions (k_t) and 5696 participants (see Supplementary Material E for references of trials included in the meta-analysis). Fig. 1 presents the PRISMA flowchart illustrating the study selection process.

The majority of included trials were conducted in the U.S. ($k_s = 39$) and involved participants diagnosed with PTSD according to DSM-IV ($k_s = 52$). Most trials focused on civilians ($k_s = 44$), followed by veterans ($k_s = 14$). While a variety of trauma types were commonly included ($k_s = 50$), some trials specifically focused on certain types, such as combat-related trauma ($k_s = 4$) or trauma resulting from an accident ($k_s = 3$). The weighted mean age of participants at baseline across trials was 39.8 years ($SD = 7.3$). Of the 38 trials that reported data on race, the majority of participants were white (59.1%) or black (28.9%). On average, trials included slightly more female participants (53.9%) than male participants, with a similar proportion of participants either married or in a committed relationship (50.8%) compared to those who were not. Slightly more than half of participants were either employed or in education (54.0%), and more than half had comorbid depression (56.3%). The types of trauma-focused interventions applied primarily belonged to the categories PE ($k_t = 44$), other CBT interventions ($k_t = 17$), CPT ($k_t = 12$), and EMDR ($k_t = 10$). Active treatment conditions were mostly compared with either a waiting list control ($k_t = 35$) or an active control condition ($k_t = 35$). The weighted averages of variables related to treatment dosage and intensity were as follows: (1) intended number of sessions: 11.0 ($SD = 2.9$); (2) mean number of completed sessions: 9.2 ($SD = 3.3$); (3) number of trauma-focused sessions: 8.2 ($SD = 2.9$); (4) intended treatment duration in weeks: 8.6 ($SD = 3.7$), (5) session duration in minutes: 83.9 ($SD = 15.4$); (6) intended treatment frequency in minutes per week: 141.0 ($SD = 127.4$); and (7) total contact time in minutes: 924.2 ($SD = 268.0$). In terms of adapting the number of sessions to patients' needs, 57 active treatment conditions had a fixed number of sessions, while 35 active treatment conditions allowed for session number adjustments. For an overview of the included trials, see Table 1, and for a detailed description, see Supplementary Material F.

3.2. Risk of bias assessment

The overall risk of bias was rated as low for 11 studies (15.1%), 21 studies showed some concerns (28.8%), and the rating was high for 41 studies (56.2%). For details on the risk of bias assessment results, see Supplementary Material G.

3.3. Results on PTSD symptom severity at post-assessment

3.3.1. Overall estimates of efficacy

Individually delivered, face-to-face trauma-focused psychological

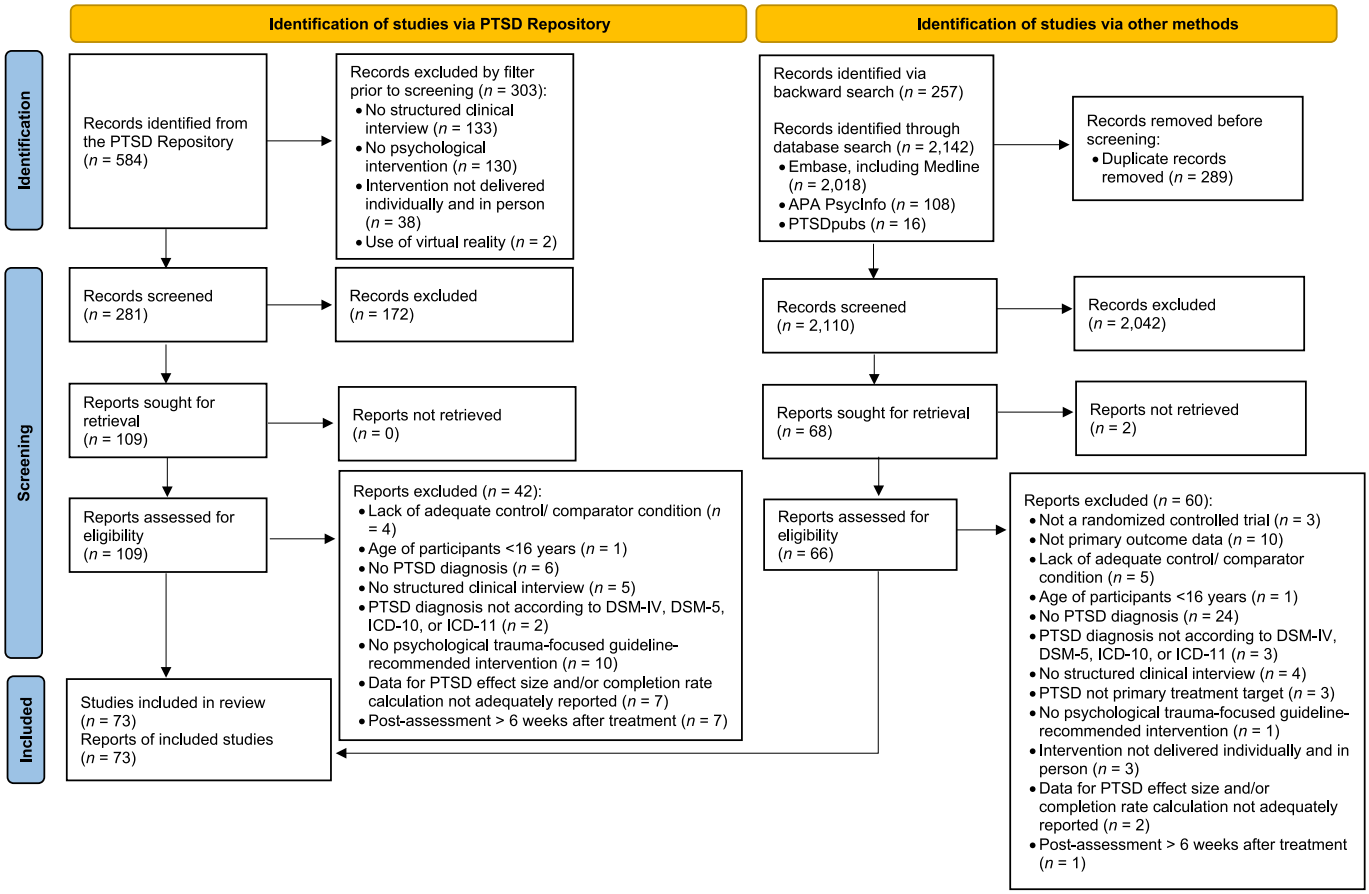

Fig. 1. PRISMA Flow Diagram.
Note. n = number of studies; PTSD = posttraumatic stress disorder; PTSD Repository = PTSD Trials Standardized Data Repository; DSM-IV = Diagnostic and Statistical Manual of Mental Disorders, fourth edition; DSM-5 = Diagnostic and Statistical Manual of Mental Disorders, fifth edition; ICD-10 = International Classification of Diseases, 10th revision; ICD-11 = International Classification of Diseases, 11th revision. PRISMA = Preferred Reporting Items for Systematic Reviews and Meta-Analysis. Adapted from "The PRISMA 2020 statement: an updated guideline for reporting systematic reviews" by M. J. Page, J. E. McKenzie, P. M. Bossuyt, I. Boutron, T. C. Hoffmann, C. D. Mulrow, L. Shamseer, J. M. Tetzlaff, E. A. Akl, S. E. Brennan, R. Chou, J. Glanville, J. M. Grimshaw, A. Hróbjartsson, M. M. Lalu, T. Li, E. W. Loder, E. Mayo-Wilson, S. McDonald, … D. Moher, 2021, *BMJ*, *372*, p. n71. CC BY-NC.

Table 1
Overview of included trials.

Author(s) (Year) Control Group Standardized Treatment Name	Study-Reported Treatment Name	*n*	Intended Number of Sessions	Frequency (Minutes per Week)	Hedges' *g* for PTSD Relative to Control at Post-Ass.	Hedges' *g* for PTSD Head-to-Head Comparison	Completion Rate (%)
Acarturk et al. (2016)							
Waitlist	Waitlist	49					
EMDR	EMDR	49	3.0	–	−1.88 [−2.35, −1.4]		75.5
Asukai et al. (2010)							
TAU	TAU	12					
PE	PE + TAU	12	11.5	90	−1.4 [−2.29, −0.50]		75.0
Back et al. (2019)							
Active control	Relapse Prevention	27					
PE	COPE	54	12.0	–	−1.04 [−1.52, −0.55]		53.7
Bohus et al. (2013)							
TAU	TAU	39					
Other CBT intervention	DBT-PTSD	43	23.0	90	−1.02 [−1.48, −0.56]		–
Bryant et al. (2003)							
Active control	Supportive Counseling	18					
Other CBT intervention	Imaginal Exposure/CR	20	8.0	90	−0.83 [−1.49, −0.16]		75.0
Other CBT intervention	Imaginal Exposure	20	8.0	90	−0.65 [−1.31, 0.00]		75.0
Bryant et al. (2011)							
TAU	TAU	12					
Other CBT intervention	CBT	16	8.0	60	−0.97 [−1.76, −0.18]		97.1
Cloitre et al. (2002)							
Waitlist	Minimal Attention	24					
PE	STAIR-modified PE	22	8.0	90	−1.27 [−1.91, −0.64]		71.0
Cloitre et al. (2010)							
Active control	STAIR/Support	38					
PE	STAIR/Exposure	33	8.0	–	0.02 [−0.45, 0.48]		84.9
PE	Support/Exposure	33	8.0	–	0.35 [−0.12, 0.82]		60.6
Coffey et al. (2006)							
Active control	Relaxation	9					
Other CBT intervention	Imaginal Exposure	8	6.0	–	−1.38 [−2.44, −0.32]		50.0
Coffey et al. (2016)							
Active control	HLS	41					
PE	mPE	45	10.5	120	−0.68 [−1.11, −0.24]		62.2
PE	mPE + MET-PTSD	40	10.5	120	−0.41 [−0.85, 0.03]		60.0
Cottraux et al. (2008)							
Active control	Supportive Therapy	15					
Other CBT intervention	CBT	27	16.0	90	−0.05 [−0.68, 0.58]		87.1
Dell et al. (2023)							
PE	Spaced PE	73	10.0	90		−0.11 [−0.45, 0.22]	74.7
PE	Massed PE	65	10.0	450			90.5
Dunne et al. (2012)							
Waitlist	Waitlist	13					
Other CBT intervention	Trauma-Focused CBT	13	10.0	60	−0.92 [−1.73, −0.11]		92.3
Ehlers et al. (2003)							
Waitlist	Repeated Assessments	27					
CT	CT	28	12.0	90	−1.12 [−1.69, −0.56]		98.3
Ehlers et al. (2005)							
Waitlist	Waitlist	14					
CT	CT	14	12.0	90	−2.19 [−3.12, −1.25]		96.7
Ehlers et al. (2014)							
Waitlist	Waitlist	30					
CT	Standard CT	31	12.0	90	−1.51 [−2.08, −0.94]	−0.19 [−0.69, 0.32]	96.8
CT	Intensive CT	30	12.0	1050	−1.35 [−1.91, −0.79]		96.7
Every-Palmer et al. (2024)							
TAU	TAU	12					

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Table 1 (*continued*)

Author(s) (Year) Control Group Standardized Treatment Name	Study-Reported Treatment Name	*n*	Intended Number of Sessions	Frequency (Minutes per Week)	Hedges' *g* for PTSD Relative to Control at Post-Ass.	Hedges' *g* for PTSD Head-to-Head Comparison	Completion Rate (%)
EMDR	EMDR	12	9.0	60	−0.64 [−1.46, 0.18]		83.3
Fecteau & Nicki (1999)							
Waitlist	Waitlist	10					
Other CBT intervention	CBT	10	4.0	120	−1.28 [−2.25, −0.32]		83.3
Feske (2008)							
TAU	TAU	12					
PE	PE	9	10.5	90	−1.14 [−2.07, −0.21]		95.0
Foa et al. (2005)							
Waitlist	Waitlist	26					
PE	PE	79	10.5	105	−0.80 [−1.26, −0.34]		59.5
Other CBT intervention	PE + Cognitive Restructuring	74	10.5	105	−0.66 [−1.11, −0.20]		65.8
Foa et al. (2018)							
Waitlist	Minimal contact	40					
PE	Spaced PE	110	10.0	113	−0.85 [−1.22, −0.47]	−0.14 [−0.4, 0.13]	75.5
PE	Massed PE	110	10.0	450	−0.69 [−1.06, −0.32]		86.4
Foa et al. (2022)							
PE	60-minute PE	80	11.5	90		−0.22 [−0.53, 0.10]	62.5
PE	90-minute PE	80	11.5	135			73.8
Fonzo et al. (2017)							
Waitlist	Waitlist	30					
PE	PE	36	10.5	125	−1.59 [−2.15, −1.04]		69.4
Forbes et al. (2012)							
TAU	TAU	29					
CPT	CPT	30	12.0	125	−0.39 [−0.91, 0.12]		70.0
Ford et al. (2018)							
Active control	TARGET	11					
PE	PE	5	10.0	83	0.02 [−1.03, 1.08]		35.7
Galovski et al. (2012)							
Waitlist	SMDT	47					
CPT	Modified CPT	53	11.0	–	−1.42 [−1.86, −0.98]		70.2
Hafkemeijer et al. (2025)							
Waitlist	Waitlist	30					
EMDR	EMDR	32	10.0	180	−1.05 [−1.58, −0.52]		–
Hensel-Dittmann et al. (2011)							
Active control	Stress Inoculation Training	13					
NET	NET	15	10.0	69	−0.25 [−0.99, 0.50]		80.0
Högberg et al. (2007)							
Waitlist	Waitlist	9					
EMDR	EMDR	12	5.0	–	−0.61 [−1.50, 0.27]		92.3
Ito et al. (2025)							
TAU	TAU	31					
CPT	CPT	29	14.0	–	−1.57 [−2.15, −1.00]		93.1
Kubany et al. (2004)							
Waitlist	Waitlist	62					
CT	CTT-BW	63	9.5	180	−1.45 [−1.85, −1.06]		78.0
Lely et al. (2019)							
Active control	Present-Centered Therapy	15					
NET	NET	18	11.0	68	0.58 [−0.12, 1.27]		–
Litz et al. (2021)							
Active control	Adaptive Disclosure	37					
CPT	CPT – Cognitive Version	33	12.0	60	−0.12 [−0.59, 0.35]		61.4
Lortye et al. (2025)							
Active control	SUD treatment only	51					
PE	SUD + PE	53	12.0	180	−0.40 [−0.79, −0.01]		47.2
EMDR	SUD + EMDR	50	12.0	180	−0.47 [−0.86, −0.07]		52.0

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Table 1 (*continued*)

Author(s) (Year) Control Group Standardized Treatment Name	Study-Reported Treatment Name	*n*	Intended Number of Sessions	Frequency (Minutes per Week)	Hedges' *g* for PTSD Relative to Control at Post-Ass.	Hedges' *g* for PTSD Head-to-Head Comparison	Completion Rate (%)
Other CBT intervention	SUD + Imagery Rescripting	55	12.0	180	−0.67 [−1.06, −0.28]		72.7
Markowitz et al. (2015)							
Active control	Relaxation	32					
PE	PE	38	10.0	64	−0.30 [−0.77, 0.17]		75.0
Matthijssen et al. (2024)							
Waitlist	Waitlist	14					
EMDR	EMDR	17	7.0	90	−0.44 [−1.15, 0.28]		–
McDonagh et al. (2005)							
Waitlist	Waitlist	23					
Other CBT intervention	CBT	29	14.0	105	−0.49 [−1.05, 0.06]		58.6
Mills et al. (2012)							
TAU	TAU	48					
PE	COPE	55	13.0	90	−0.24 [−0.62, 0.15]		18.2
Monson et al. (2006)							
Waitlist	Waitlist	30					
CPT	CPT	30	12.0	–	–		80.0
Nacasch et al. (2011)							
TAU	TAU	15					
PE	PE	15	12.0	105	−1.74 [−2.58, −0.90]		86.7
Nacasch et al. (2015)							
PE	60-minute PE	20	12.5	–		0.12 [−0.51, 0.75]	97.6
PE	90-minute PE	19	12.5	–			94.7
Neuner et al. (2004)							
Active control	Psychoeducation	12					
NET	NET	17	4.0	180	−0.19 [−0.93, 0.55]		97.1
Nidich et al. (2018)							
Active control	PTSD Health Education	66					
PE	PE	68	12.0	90	−0.19 [−0.53, 0.15]		61.8
Oprel et al. (2021)							
PE	PE	48	16.0	90		−0.03 [−0.42, 0.36]	70.8
PE	Intensive PE	51	12.0	270			71.2
Peck et al. (2023)							
TAU	TAU	10					
PE	PE	10	12.0	60	−0.52 [−1.41, 0.37]		20.0
PE	PE + financial incentives	10	12.0	60	−0.45 [−1.34, 0.44]		90.0
Peck et al. (2025)							
TAU	TAU	17					
PE	PE	17	12.0	60	0.15 [−0.52, 0.83]		29.4
PE	PE + financial incentives	18	12.0	60	−0.47 [−1.14, 0.20]		88.9
Petrakis et al. (2024)							
Active control	Individual Drug Counselling	18					
CPT	CPT	20	12.0	–	0.16 [−0.48, 0.80]		50.0
Raabe et al. (2022)							
Waitlist	Waitlist	20					
Other CBT intervention	Imagery Rescripting	21	16.0	180	−0.62 [−1.25, 0.00]		76.2
Other CBT intervention	STAIR/Imagery Rescripting	20	16.0	180	0.02 [−0.60, 0.64]		80.0
Rauch et al. (2015)							
Active control	Present-Centered Therapy	15					
PE	PE	11	11.0	–	−0.92 [−1.73, −0.10]		61.1
Reger et al. (2016)							
Waitlist	Waitlist	54					
PE	PE	54	10.0	158	−0.80 [−1.20, −0.41]		59.3
Resick et al. (2002)							
Waitlist	Minimal Attention	47					
PE	PE	62	9.0	173	−1.13 [−1.54, −0.72]		66.1
CPT	CPT	62	12.0	130	−0.86 [−1.26, −0.47]		64.5
Rothbaum et al. (2005)							
Waitlist	Waitlist	20					

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Table 1 (*continued*)

Author(s) (Year) Control Group Standardized Treatment Name	Study-Reported Treatment Name	*n*	Intended Number of Sessions	Frequency (Minutes per Week)	Hedges' *g* for PTSD Relative to Control at Post-Ass.	Hedges' *g* for PTSD Head-to-Head Comparison	Completion Rate (%)
PE	PE	20	9.0	180	−1.42 [−2.11, −0.73]		80.0
EMDR	EMDR	20	9.0	180	−2.00 [−2.76, −1.24]		88.0
Schnurr et al. (2007)							
Active control	Present-Centered Therapy	143					
PE	PE	141	10.0	90	−0.24 [−0.47, −0.01]		62.4
Sele et al. (2023)							
Active control	STAIR	31					
PE	PE	37	12.0	108	0.34 [−0.16, 0.84]		–
Other CBT intervention	STAIR Narrative Therapy	31	8.0	80	−0.12 [−0.60, 0.36]		–
Shea et al. (2023)							
Active control	Interpersonal Therapy	50					
PE	PE	32	12.0	90	0.03 [−0.41, 0.48]		52.9
Shemesh et al. (2011)							
Active control	Educational Sessions	28					
Other CBT intervention	CBT	23	4.0	–	–		80.0
Simpson et al. (2022)							
Waitlist	Assessment Only	22					
CPT	CPT	41	12.0	90	−0.74 [−1.28, −0.21]		53.7
Sloan et al. (2018)							
Active control	Written Exposure Therapy	63					
CPT	CPT	63	12	60	−0.13 [−0.48, 0.21]		60.3
Sloan et al. (2022)							
Active control	Written Exposure Therapy	85					
CPT	CPT	84	12	120	−0.55 [−0.86, −0.24]		56.0
Sloan et al. (2023)							
Active control	Written Exposure Therapy	88					
PE	PE	90	11.5	90	−0.15 [−0.44, 0.15]		64.4
Smaik et al. (2023)							
Waitlist	Waitlist	20					
NET	NET	20	10.0	90	−3.15 [−4.08, −2.22]		97.6
Sturt et al. (2023)							
Active control	RTM	29					
Other CBT intervention	CBT	16	18.0	75	0.62 [0.00, 1.25]		–
Suris et al. (2013)							
Active control	Present-Centered Therapy	34					
CPT	CPT	52	12.0	–	−1.07 [−1.53, −0.61]		65.4
Ter Heide et al. (2016)							
Active control	Stabilization	36					
EMDR	EMDR	36	9.0	80	−0.06 [−0.52, 0.40]		83.3
Thorp et al. (2019)							
Active control	Relaxation Training	46					
PE	PE	41	12.0	90	−0.67 [−1.10, −0.23]		73.2
Van den Berg et al. (2015)							
Waitlist	Waitlist	47					
EMDR	EMDR	55	8.0	90	−0.66 [−1.05, −0.26]		80.0
PE	PE	53	8.0	90	−0.78 [−1.18, −0.37]		75.5
Van der Kolk et al. (2007)							
Active control	Pill Placebo	29					
EMDR	EMDR	29	8.0	90	−0.48 [−1.00, 0.04]		82.8
Vera et al. (2011)							
TAU	TAU	7					
PE	PE	5	15.0	105	−1.62 [−2.94, −0.30]		71.4
Vera et al. (2022)							
Active control	Applied Relaxation	49					

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Table 1 (*continued*)

Author(s) (Year) Control Group Standardized Treatment Name	Study-Reported Treatment Name	*n*	Intended Number of Sessions	Frequency (Minutes per Week)	Hedges' *g* for PTSD Relative to Control at Post-Ass.	Hedges' *g* for PTSD Head-to-Head Comparison	Completion Rate (%)
PE	PE	49	13.5	90	−0.30 [−0.69, 0.10]		77.6
Watkins et al. (2023)							
Waitlist	Waitlist	34					
CPT	CPT	78	12.0	120	−0.69 [−1.10, −0.27]		79.5
Wells et al. (2014)							
Waiting list	Waitlist	10					
PE	PE	10	8.0	60	−1.77 [−2.81, −0.74]		90.9
Yehuda et al. (2014)							
Waitlist	Minimal Attention	–					
PE	PE	–	12.0	90	–		65.4
Zemestani et al. (2022)							
Waitlist	Waitlist	24					
Other CBT intervention	TF-CBT	24	12.0	90	−3.57 [−4.48, −2.66]		91.7

Note. CBT = Cognitive Behavioral Therapy; COPE = Concurrent Treatment of PTSD and Substance Use Disorders Using Prolonged Exposure; CPT = Cognitive Processing Therapy; CR = Cognitive Restructuring; CT = Cognitive Therapy; CTT-BW = Cognitive Trauma Therapy for Battered Women with Posttraumatic Stress Disorder; DBT-PTSD = Dialectical Behavior Therapy for Post-Traumatic Stress Disorder; EMDR = Eye Movement Desensitization and Reprocessing; HLS = Health Information-Based Control Condition; MET-PTSD = Trauma-Focused Motivational Enhancement Session; mPE = Modified Version of Prolonged Exposure; NET = Narrative Exposure Therapy; PE = Prolonged Exposure; RTM = Reconsolidation of Traumatic Memories; SMDT = Symptom-Monitoring Delayed Treatment; STAIR = Skills Training in Affective and Interpersonal Regulation; SUD = Substance Use Disorder; TARGET = Trauma Affect Regulation: Guide for Education and Therapy; TAU = Treatment As Usual; TF-CBT = Trauma-Focused Cognitive Behavioral Therapy.

interventions showed a medium to large treatment effect compared to control conditions, $k_t = 81$, $g = -0.75$, 95% CI [−0.92, −0.58], $p < .001$, 95% PI [−2.01, 0.51] (see Fig. H1 in Supplementary Material H for the forest plot). Heterogeneity was rated as substantial to considerable, $Q(80) = 401.44$, $p < .001$, $I^2 = 85.8\%$, 95% CI [80.5%, 89.6%]. When interventions were compared exclusively to inactive control conditions, a large treatment effect was found, $k_t = 47$, $g = -1.11$, 95% CI [−1.33, −0.90], $p < .001$, 95% PI [−2.30, 0.07], whereas a small effect was observed when compared to active control conditions, $k_t = 34$, $g = -0.29$, 95% CI [−0.44, −0.13], $p < .001$, 95% PI [−0.94, 0.36]. As the most commonly delivered intervention was PE, a separate meta-analysis was conducted, indicating a medium-to-large treatment effect, $k_t = 35$, $g = -0.65$, 95% CI [−0.84, −0.45], $p < .001$, 95% PI [−1.59, 0.30]. For all three subgroup analyses, heterogeneity was significant and rated as at least substantial, with I^2 values ranging from 64.6% to 80.7%.

Inspection of the funnel plot (see Fig. I1 in Supplementary Material I) suggested potential publication bias, which was further supported by Eggers' test, indicating funnel plot asymmetry, intercept = −2.25, 95% CI [−3.68, −0.82], $t = -3.09$, $p = .003$. Consequently, the trim-and-fill method was applied, yielding an asymmetry-adjusted estimate of $k_t = 97$, $g = -0.49$, 95% CI [−0.66, −0.31], $p < .001$ (see Fig. I2 in Supplementary Material I for the funnel plot with imputed trials).

3.3.2. Moderator analyses

A total of six head-to-head trials were identified, including four trials comparing intensive versus standard treatment frequency and two trials comparing 60-min versus 90-min session lengths. Meta-analysis of the trials comparing intensive versus standard treatment frequency found no significant difference in symptom reduction, regardless of the treatment frequency, $k_t = 4$, $g = -0.12$, 95% CI [−0.29, 0.06], $p = .189$. No heterogeneity was observed; therefore, no prediction interval was calculated. An insufficient number of trials ($k = 2$) was available to conduct a meta-analysis of head-to-head trials comparing different session lengths. However, both trials indicated that 60-min and 90-min sessions of PE were similarly efficacious.

Meta-regression analyses revealed that a higher mean number of completed sessions was associated with a larger between-group effect size for PTSD at post-assessment ($p = .030$). However, this association did not persist in the sensitivity analyses. Additionally, a higher number of trauma-focused sessions was associated with a weaker between-group effect size for PTSD at post-assessment ($p = .029$). This moderation persisted in the sensitivity analysis with trials comparing interventions to inactive control conditions only, $k_t = 34$, $b = 0.09$, 95% CI [0.02, 0.16], $p = .014$, but was no longer significant when comparing interventions to active control conditions or when restricting to studies using PE. Lastly, studies in which the number of sessions was adapted to patients' needs yielded significantly stronger between-group effect sizes compared to those that did not ($p = .025$). This moderation was also maintained in the sensitivity analysis with trials comparing interventions to inactive control conditions only ($k_t = 47$, $b = 0.55$, 95% CI [0.16, 0.94], $p = .007$), but lost significance when comparing interventions to active control conditions or when restricting to studies using PE as the intervention. Furthermore, after adjusting for control variables in a multiple meta-regression analysis, neither the mean number of completed sessions, the number of trauma-focused sessions, nor the adaptation of session number to patients' needs remained significant as moderators. This pattern was consistent in the sensitivity analysis with inactive control conditions. The models with control variables alone demonstrated a better fit, as evidenced by lower AIC and BIC values, compared to the models that included dosage indicators. In the sensitivity analysis limited to trials with active control conditions, a longer intended treatment duration (in weeks) was associated with a weaker between-group effect size ($p = .023$), whereas a higher intended treatment frequency (in minutes per week) was associated with a stronger between-group effect size ($p = .018$). However, after adjusting for control variables in a multiple meta-regression analysis, neither intended treatment duration nor treatment frequency remained significant moderators, and the models including only control variables demonstrated a better overall fit. For detailed information on the results of the univariate meta-regression analyses and the sensitivity analyses, see Table 2 and Supplementary Material J, respectively.

3.4. Results on PTSD symptom severity at follow-up

3.4.1. Overall estimates of efficacy

The latest available follow-up assessment took place, on average,

29.2 weeks (SD = 16.9) after the end of treatment. Psychological interventions showed a medium treatment effect compared to control conditions, $k_t = 36$, $g = -0.58$, 95% CI [−0.79, −0.36], $p < .001$, 95% PI [−1.66, 0.51] (see Fig. H2 in Supplementary Material H for the forest plot). Heterogeneity was rated as substantial to considerable, Q (35) = 142.62, $p < .001$, I^2 = 82.6%, 95% CI [72.0%, 89.2%]. When interventions were compared exclusively to inactive control conditions, a large treatment effect was found, $k_t = 14$, $g = -0.96$, 95% CI [−1.36, −0.56], $p < .001$, 95% PI [−2.36, 0.44]. In contrast, when compared to active control conditions, a small effect was observed, $k_t = 22$, $g = -0.28$, 95% CI [−0.41, −0.14], $p < .001$, 95% PI [−0.65, 0.10]. Heterogeneity was significant for both subgroup analyses, with substantial to considerable heterogeneity in the comparison to inactive control conditions (I^2 = 83.3%, 95% CI [62.9%, 92.5%]) and moderate heterogeneity in the comparison to active control conditions (I^2 = 36.5%, 95% CI [0.0%, 65.8%]).

3.4.2. Moderator Analyses

Due to an insufficient number of trials focusing on the same dosage or intensity variables and reporting follow-up data ($k < 4$), no meta-analyses with head-to-head trials were conducted for PTSD symptom severity at follow-up.

Univariate meta-regression analyses revealed no significant moderators related to treatment dosage or intensity. For further details on the results of the univariate meta-regression analyses, refer to Table 2. No sensitivity analyses were conducted for this outcome, as not all univariate meta-regression analyses would have included at least ten comparisons due to missing data ($k < 10$).

3.5. Results on treatment completion

3.5.1. Overall estimates of completion

The weighted average completion rate across all studies and active treatment conditions ($k_t = 85$) was 71.8%, 95% CI [68.3%, 75.0%], 95% PI [43.2%, 89.5%] (see Fig. H3 in Supplementary Material H for the forest plot). The heterogeneity was rated as substantial, Q (84) = 274.0, $p < .001$, I^2 = 73.4%, 95% CI [64.0%, 80.4%].

3.5.2. Moderator analyses

The meta-analysis of trials comparing intensive versus standard treatment intensity ($k_t = 4$) revealed a pooled OR of 0.56, 95% CI [0.32, 0.97], 95% PI [0.26, 1.19], indicating that completion was significantly ($p = .039$) less frequent with standard intensity treatment compared to high intensity treatment. The heterogeneity was not significant, Q (3) = 3.33, $p = .343$, I^2 = 21.2%, 95% CI [0.0%, 88.5%].

Univariate meta-regression analyses indicated that a higher treatment frequency was significantly ($p = .004$) related to higher completion rates. This association did not remain significant in the multiple meta-regression analysis with control variables. While the AIC indicated that the model including treatment frequency provided a better fit than the model with control variables only, the BIC suggested that the model with control variables alone demonstrated a better fit. Additionally, an outlier analysis based on the interquartile range and visual inspection was conducted, which resulted in the moderating effect of treatment frequency on completion rates no longer being significant in the univariate meta-regression analysis. This suggests that the observed improvement in completion rates with higher treatment frequency was primarily driven by studies offering very intensive treatment. For further details on the results of the univariate meta-regression analyses, see Table 2.

3.6. Results on depression symptom severity at post-assessment

3.6.1. Overall estimates of efficacy

Psychological interventions showed a medium treatment effect compared to control conditions, $k_t = 60$, $g = -0.70$, 95% CI [−0.88, −0.52], $p < .001$, 95% PI [−1.86, 0.46] (see Fig. H4 in Supplementary Material H for the forest plot). Heterogeneity was rated as substantial to considerable, Q (59) = 299.26, $p < .001$, I^2 = 82.3%, 95% CI [74.5%, 87.7%]. A large treatment effect was found when interventions were compared exclusively to inactive control conditions, $k_t = 40$, $g = -0.90$, 95% CI [−1.12, −0.68], $p < .001$, 95% PI [−2.12, 0.32]. In contrast, a small effect was observed when compared to active control conditions, $k_t = 20$, $g = -0.28$, 95% CI [−0.45, −0.11], $p = .002$, 95% PI [−0.88, 0.31]. Heterogeneity was significant in both subgroup analyses, with substantial to considerable heterogeneity in comparison to inactive control conditions (I^2 = 80.5%, 95% CI [69.4%, 87.6%]) and moderate to substantial heterogeneity in comparison to active control conditions (I^2 = 58.3%, 95% CI [19.9%, 78.3%]).

3.6.2. Moderator analyses

Due to an insufficient number of trials focusing on the same dosage or intensity variables and reporting data on depression ($k < 4$), no meta-analyses with head-to-head trials were conducted for depression symptom severity at post-assessment.

The univariate meta-regression analyses showed no significant

Table 2
Results from moderator analyses of treatment dosage and intensity on outcome variables.

Outcome / Moderator	k_t	b	95% CI	p
PTSD symptom severity at post-ass. (Hedges' g)				
Intended session number	81	0.01	−0.04, 0.06	.614
Completed session number (M)	47	−0.06	−0.11, −0.01	.030*
Trauma-focused session number	54	0.08	0.01, 0.15	.029*
Treatment duration (weeks)	73	0.01	−0.03, 0.05	.606
Session duration (minutes)	74	−0.01	−0.02, 0.00	.222
Treatment frequency (minutes/week)	70	0.00	0.00, 0.00	.917
Total contact time (minutes)	74	0.00	0.00, 0.00	.674
Session number adaptation[a] (yes vs. no)	81	0.36	0.05, 0.67	.025*
PTSD symptom severity at follow-up (Hedges' g)				
Intended session number	36	0.01	−0.05, 0.07	.710
Completed session number (M)	21	−0.01	−0.07, 0.05	.705
Trauma-focused session number	25	0.06	−0.04, 0.15	.215
Treatment duration (weeks)	34	0.01	−0.06, 0.08	.786
Session duration (minutes)	32	−0.01	−0.02, 0.00	.152
Treatment frequency (minutes/week)	32	−0.01	−0.01, 0.00	.104
Total contact time (minutes)	32	0.00	0.00, 0.00	.364
Session number adaptation[a] (yes vs. no)	36	0.14	−0.24, 0.53	.457
Treatment completion (completion rate)				
Intended session number	85	−0.06	−0.12, 0.01	.103
Completed session number (M)	51	0.17	0.07, 0.26	.001**
Trauma-focused session number	58	−0.05	−0.13, 0.02	.176
Treatment duration (weeks)	72	−0.04	−0.10, 0.01	.117
Session duration (minutes)	76	0.01	0.00, 0.02	.140
Treatment frequency (minutes/week)	70	0.00	0.00, 0.00	.004**
Total contact time (minutes)	76	0.00	0.00, 0.00	.749
Session number adaptation[a] (yes vs. no)	85	−0.15	−0.51, 0.21	.402
Depression symptom severity at post-ass. (Hedges' g)				
Intended session number	60	0.01	−0.05, 0.06	.769
Completed session number (M)	31	−0.04	−0.09, 0.01	.109
Trauma-focused session number	41	0.04	−0.02, 0.10	.141
Treatment duration (weeks)	54	0.03	−0.02, 0.08	.297
Session duration (minutes)	53	0.00	−0.01, 0.01	.856
Treatment frequency (minutes/week)	51	0.00	0.00, 0.00	.825
Total contact time (minutes)	53	0.00	0.00, 0.00	.648
Session number adaptation[a] (yes vs. no)	60	0.15	−0.20, 0.50	.394

Note. k_t = active treatment conditions; CI = confidence interval; regression models were estimated separately for each moderator.
[a] Session number adaptation was dummy-coded: "Yes" = 0 (reference category), "No" = 1.
* $p < .05$.
** $p < .01$.

moderating effects of treatment dosage or intensity on depression symptom severity, neither in the main analyses nor in the sensitivity analyses with inactive or active comparisons. For detailed information on the results of the univariate meta-regression analyses and the sensitivity analyses, see Table 2 and Supplementary Material J, respectively.

4. Discussion

This meta-analysis explored the potential moderating effects of treatment dosage and intensity on the efficacy and completion rates of trauma-focused, guideline-recommended psychological interventions for PTSD. The interventions demonstrated medium to large overall treatment effects on PTSD symptom severity at post-assessment ($g = -0.75$) and follow-up assessment ($g = -0.58$), as well as on depression symptom severity at post-assessment ($g = -0.70$). Across all efficacy outcomes, treatment effects were large relative to inactive control conditions but small when compared to active control conditions, most of which consisted of non-guideline-recommended psychological treatments for PTSD. The weighted average completion rate for the included interventions was 71.8%, 95% CI [68.3%, 75.0%].

Meta-analyses of head-to-head comparisons between high versus standard treatment intensity ($k_t = 4$) showed no significant difference in PTSD symptom severity at post-assessment, but completion was significantly less frequent for standard-intensity treatment compared to high-intensity treatment ($OR = 0.56$). Meta-regression analyses also revealed that high treatment intensity was positively associated with treatment completion; however, this effect was primarily driven by trials offering very intensive treatments ($\geq$ 270 min per week). Regarding PTSD symptom severity, a higher mean number of completed sessions, regardless of session type, was associated with stronger between-group effect sizes at post-assessment, whereas a higher number of trauma-focused sessions specifically was linked to weaker between-group effects. Trials where the number of sessions was adapted to patients' needs resulted in significantly stronger between-group effect sizes for PTSD compared to those that did not. When interventions were compared exclusively to active control conditions, higher treatment frequency was associated with a stronger between-group effect size for PTSD, whereas longer intended treatment duration (in weeks) was associated with a weaker effect size. Finally, two head-to-head trials comparing 60-min versus 90-min PE sessions found both session lengths to be equally efficacious, consistent with session duration not emerging as a significant moderator in any of the meta-regression analyses.

4.1. Overall estimates of efficacy and completion

Overall, individually delivered, face-to-face trauma-focused psychological interventions appear to be effective in reducing both PTSD and depression symptoms, with sustained benefits for PTSD in both the short and long term. The observed effect sizes align with those found in previous meta-analyses of trauma-focused PTSD interventions, demonstrating superiority over inactive control conditions with large effect size estimates, while the estimated effect size is small when compared to active control conditions (McLean et al., 2022b). In the meta-analytic literature, dropout rates are more commonly reported than completion rates. However, there is no consensus on the operationalization of dropout, resulting in several different definitions, one of which is failure to complete treatment. When comparing the 71.8% completion rate in the current meta-analysis with results from other meta-analyses examining studies using this definition of dropout, this completion rate appears relatively low. Across disorders, 18.4% of patients were defined as dropouts, operationalized as failure to complete treatment (Swift & Greenberg, 2012), with 16.6% specifically for PTSD (Semmlinger, Takano, Schumm, & Ehring, 2021). The lower completion rate observed in this analysis may be attributed to the exclusive focus on trauma-focused interventions, which a meta-analysis suggests are associated with higher drop-out rates (Lewis et al., 2020). Additionally, the inconsistency in findings may reflect the lack of a standardized definition of treatment completion across studies and, by extension, across meta-analyses. Lastly, the comparison with earlier meta-analyses on dropout should be interpreted with caution, as the differing focuses of the analyses may have led to non-opposite results between dropout and completion rates.

4.2. Number of completed sessions as a moderator

Meta-regression analyses revealed that a higher mean number of completed treatment sessions was significantly associated with greater reductions in PTSD symptom severity at post-assessment. This finding aligns with the results of a meta-analysis by Lambert and Alhassoon (2015), which demonstrated that, in trauma-focused therapy for refugees, symptom reduction increased with the average number of sessions completed by participants. When interpreting these findings, it is important to note the distinction between the intended number of sessions and the average number of completed sessions. Notably, this effect does not appear to extend to the number of intended sessions, either in earlier research (McLean et al., 2022b) or in the present analysis. Two additional meta-analyses reported no significant association between session number and PTSD symptoms, although it remains unclear whether the authors differentiated between intended and completed sessions (Asmundson et al., 2019; Haagen et al., 2015).

On average, participants completed nine treatment sessions across studies, aligning with commonly cited thresholds for an adequate PTSD treatment dose, typically defined as eight (Spoont, Murdoch, Hodges, & Nugent, 2010) to nine sessions (Seal et al., 2010). While some authors suggest that patients either improve early in PTSD therapy or not at all (Holder et al., 2020; Ready et al., 2020; Sripada et al., 2020), Galovski et al. (2012) reported that more than a third of participants receiving CPT reached end-state criteria only after session 12. The current findings support the idea that, for at least some patients, additional sessions yield greater symptom reduction. Consistent with this, a recent naturalistic study by Harper et al. (2025) found that the commonly suggested nine-session threshold leads to minimal symptom improvement. Together, these findings suggest that the concept of a fixed adequate dose may be overly simplistic, with the optimal number of treatment sessions likely varying between individuals.

4.3. Number of trauma-focused sessions as a moderator

Meta-regression analyses revealed that a higher number of trauma-focused sessions was associated with a weaker between-group effect size for PTSD at post-assessment. This finding stands in contrast to the meta-analysis by Haagen et al. (2015), which reported that a higher number of trauma-focused sessions was associated with better treatment outcomes. It is also surprising, given that the general literature suggests trauma-focused treatments lead to better outcomes compared to non-trauma-focused approaches (e.g., Hoppen, Jehn, et al., 2023; McLean et al., 2022b). Two possible explanations for this result merit consideration. First, although a recent meta-analysis found no overall PTSD symptom exacerbation at mid-treatment in trauma-focused interventions (Purnell, Graham, Chiu, Trickey, & Meiser-Stedman, 2024), evidence suggests that some patients experience a temporary worsening of symptoms during treatment. For instance, Larsen, Stirman, Smith, and Resick (2016) reported that between 14.7% and 28.6% of patients experienced symptom exacerbation, depending on the specific trauma-focused intervention applied. Similarly, Burger et al. (2023) reported that 46.5% of patients suffering from PTSD and psychosis experienced between-session exacerbation during EMDR and PE. Given that post-assessment in the included RCTs was conducted no later than six weeks after treatment completion, it is possible that this timing captured patients still experiencing temporary symptom exacerbation. Symptom relief may have occurred later, as symptom exacerbation has been shown to be unrelated to treatment response (Burger et al., 2023; Larsen

et al., 2016). This could also explain why the association between a higher number of trauma-focused sessions and weaker between-group effect sizes for PTSD was no longer present at follow-up. Consistent with this, exposure-based treatments often demonstrate stronger effects from post-treatment to long-term follow-up compared to other interventions (Kline et al., 2018). A second possible explanation, aside from the timing of measurement, is that the result may be confounded with other variables related to poorer treatment outcome. The fact that number of trauma-focused sessions was no longer a significant moderator in the multiple meta-regression with control variables supports this explanation. For instance, it is conceivable that individuals with more severe PTSD symptoms at baseline are more likely to receive a greater number of trauma-focused sessions because clinicians perceive a need for more intensive intervention. As a result, the association might reflect the severity of initial symptoms rather than the effect of the sessions themselves.

4.4. Adaptation of session number to patients' needs as a moderator

Interventions with a flexible number of sessions led to significantly stronger between-group effect sizes for PTSD at post-assessment compared to those with a fixed number of sessions. This aligns with a recent review by Galovski, Nixon, and Kehle-Forbes (2024), in which the authors suggest that flexible protocol delivery may enhance treatment effectiveness for PTSD. Based on the findings of Galovski et al. (2012) varying the length of the protocol was identified as one potential strategy for treatment improvement (Galovski et al., 2024). Personalizing PTSD treatment is further supported by Herzog and Kaiser (2022), who found heterogeneity in the effects of psychological PTSD treatments using Bayesian variance ratio meta-analysis. While the majority of the PTSD literature has focused on individual treatment selection (e.g., Deisenhofer et al., 2018; Keefe et al., 2018), the current findings suggest that even personalizing specific treatment components may already result in improved outcomes. One potential explanation for the greater symptom reduction associated with flexible session numbers is that allowing flexibility in the session numbers may be essential to account for the individual needs of patients to achieve optimal treatment improvement. In addition, increased patient involvement in determining the number of sessions may enhance the perceived self-efficacy and autonomy and thus improve treatment outcomes.

4.5. Treatment frequency as a moderator

The finding that treatment delivered at a higher frequency is equally efficacious as treatment delivered at a lower frequency is consistent with previous literature (Hoppen, Kip, et al., 2023; Sciarrino et al., 2020; Wachen et al., 2019). Furthermore, these previous reviews and meta-analyses have reported lower dropout rates with more frequent treatment sessions, which aligns with our finding of an association between higher treatment frequency and a greater likelihood of treatment completion. However, the current finding regarding the critical role of very high treatment frequency ($\geq$ 270 min per week) in improving treatment completion rates has not been reported in earlier studies.

In the depression literature, higher treatment frequency is associated with greater reductions in depressive symptoms, with two sessions per week proving more effective than one, and one session being more effective than biweekly sessions (Ciharova et al., 2024; Cuijpers, 2024; Cuijpers et al., 2013). In contrast, our findings suggest that treatment frequency does not significantly impact depression symptom reduction in PTSD patients. A conceivable explanation might be that PTSD treatment primarily targets trauma-related symptoms rather than depression. However, integrating depression-focused interventions could prove beneficial, as greater depression severity is linked to poorer PTSD treatment outcomes (Kline et al., 2021). For example, studies on behavioral activation in PTSD treatment suggest that incorporating such interventions can effectively alleviate depression symptoms (Etherton & Farley, 2022), potentially improving overall treatment outcomes.

4.6. Strengths and limitations

The current meta-analysis has several strengths. First, it is the first to examine how both dosage and intensity moderate treatment outcomes in PTSD. Although previous meta-analyses have considered these factors alongside other variables (e.g., McLean et al., 2022b) or focused exclusively on treatment intensity (e.g., Hoppen, Kip, et al., 2023), no comprehensive analysis has explored both dosage and intensity in PTSD treatment, similar to the work done for depression (Ciharova et al., 2024; Cuijpers et al., 2013). Second, best practices were followed by adhering to the PRISMA guidelines and AMSTAR criteria. Third, the use of strict inclusion and exclusion criteria strengthened the meta-analysis's internal validity. By focusing on evidence-based, guideline-recommended treatments, the analysis examined interventions widely recognized as effective for PTSD, enhancing the external validity of the findings and their relevance to clinical practice. Fourth, despite these selective eligibility criteria, this meta-analysis includes a large sample size, with 73 RCTs and 5696 participants across 92 active treatment conditions. Finally, the meta-analysis assessed not only PTSD symptoms but also treatment acceptability and depression, providing a more comprehensive understanding of treatment effects.

Despite its strengths, this meta-analysis also has several limitations. First, a significant proportion of the included studies were rated as having a high or moderate 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 nature of the control conditions across the included trials varied considerably. To address this, extensive sensitivity analyses were conducted, separately examining studies with active and passive control conditions. However, substantial variability persisted even among the active control groups. Fourth, the results of the meta-regression analyses are constrained by two key limitations: (i) moderate variability in treatment dosage and intensity variables, indicated by coefficients of variation ranging from 20% to 50% for most measures, which may reduce the sensitivity of the analyses; and (ii) the performance of several meta-regressions on each outcome, which increases the risk of inflated type I error rates. Fifth, the number of head-to-head trials with deliberate variation in dosage or intensity between randomly assigned groups was limited to six, even though these trials yield the most reliable insights. Sixth, patient-relevant outcomes, such as functioning and quality of life, were not considered, as only a few of the included RCTs reported these measures, limiting the clinical applicability of the findings. Seventh, the definition of treatment completion varied considerably across studies (see Supplementary Material F for the completion definition used in each study), potentially contributing to inconsistencies in completion rates and limiting the comparability of the findings. Eighth, nearly half of the included active treatment conditions (48.8%) applied PE, creating a strong predominance of this intervention relative to other guideline-recommended treatments. Sensitivity analyses restricted to conditions using PE partially differed from the results of the main analyses (see Supplementary Material J for detailed results of these sensitivity analyses), highlighting the potential influence of this imbalance on the pooled estimates. Finally, by including only treatments recommended by the APA Clinical Practice Guideline for PTSD in Adults (American Psychological Association, 2025), heterogeneity across interventions was reduced; however, due to the guideline's methodology, this approach may have excluded recently developed effective interventions not yet captured in systematic reviews.

4.7. Implications

4.7.1. Implications for future research

Given that meta-analytic research is inherently dependent on the

quality and comprehensiveness of the primary literature, the findings of this meta-analysis underscore several implications for future RCTs before such improvements can be reflected in subsequent meta-analytic work. Specifically, 40.0% of the included active treatment conditions did not report the mean number of completed sessions, and nearly one-third (32.6%) failed to report the number of trauma-focused sessions. To enable more rigorous investigation of treatment dosage and intensity as potential moderators in PTSD interventions, future RCTs should ensure systematic and transparent reporting of these variables. Furthermore, introducing greater variability in dosage and intensity across RCTs would facilitate a deeper understanding of how these factors influence therapeutic outcomes. Head-to-head trials, in particular, hold significant value for further evaluating and extending the findings of the present analysis. Regarding treatment intensity, a pre-registered, though not yet published, head-to-head trial comparing weekly versus intensive delivery of PE could offer valuable insights (Bragesjö, Fina, Ivanova, Ivanov, & Rück, 2024). Furthermore, as noted earlier, the current meta-analysis does not include outcomes related to functioning and quality of life, as these are rarely measured in RCTs of psychological interventions for PTSD. This is despite the fact that functional impairment can persist for many years following traumatic exposure (Goldberg et al., 2014), making it an important treatment target. Bonfils et al. (2022) found that PTSD patients experience moderate improvements in functioning through psychotherapy; however, these improvements are distinct from changes in symptoms, meaning the findings of the current meta-analysis cannot be applied to these outcomes. To assess the effects of treatment dosage and intensity on these patient-relevant outcomes, it is essential that future RCTs consistently measure and report this information.

4.7.2. Implications for clinical practice

Two key implications for clinical practice emerge from the results of the current meta-analysis. First, when resources permit, a more intensive delivery of trauma-focused interventions could be considered, as it appears to be equally efficacious as standard treatment intensity while offering the benefit of faster symptom relief for patients. Furthermore, delivering treatment in a highly intensive format appears to increase the likelihood of patients completing treatment. As highly intensive treatment delivery may be challenging to implement in typical clinical settings, the development of specialized treatment programs could help facilitate its feasibility. Second, the findings of the current meta-analysis support the flexible administration of manualized psychological interventions for PTSD, particularly regarding the number of sessions delivered to patients. While maintaining fidelity to evidence-based protocols is crucial when treating the complex nature of PTSD, as evidenced by the subpar outcomes associated with unstructured generic counseling for PTSD (Forbes et al., 2012), the data also highlight the potential drawbacks of excessively rigid adherence to therapy manuals. Clinicians may therefore consider determining the number of treatment sessions based not only on manual recommendations but also on ongoing assessments, patient preferences, and the clinician's professional judgment about the most appropriate course of treatment for the individual patient at any given time.

5. Conclusion

The findings of our comprehensive meta-analysis suggest that guideline-recommended PTSD interventions may benefit from both personalizing the number of sessions and delivering treatment intensively. These adjustments could improve PTSD symptom reduction and increase treatment completion rates. The results highlight the importance of exploring a variety of approaches to enhance PTSD treatment effectiveness and acceptance, moving beyond a sole focus on manual-specific factors. In this context, treatment personalization emerges as a key factor in further improving trauma-focused interventions, particularly through the flexible administration of evidence-based manuals. Future research should seek to identify the mechanisms underlying the observed positive effects on symptom reduction and treatment completion. Additionally, further studies are needed to gain a deeper understanding of how other factors related to treatment dosage and intensity, such as the number of trauma-focused sessions, influence treatment outcomes.

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Declaration of competing interest

Declaration of competing interest relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.cpr.2026.102720.

Data availability

I have shared the link to my data/code at the Attach File step. Dataset_Leithner_06.2025.xlsx (Reference data) (OSF)

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