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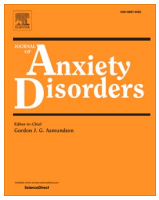

Review

Can mindfulness-based interventions reduce PTSD symptoms? An umbrella review

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ABSTRACT

Post-traumatic stress disorder (PTSD) is a debilitating, often chronic condition with substantial cross-national lifetime prevalence. Although mindfulness-based interventions (MBIs) may help reduce PTSD symptoms, efficacy results are inconsistent. Despite many systematic reviews (SRs) examining MBIs for PTSD, SR quality has been neither evaluated nor synthesized. We conducted an umbrella review to summarize and evaluate existing evidence regarding MBIs for PTSD, identifying 69 SRs (27 meta-analyses), consisting of 83 primary studies. Using AMSTAR2 (a valid SR quality assessment tool), we evaluated each SR on key domains relevant to methodological rigor and rated the confidence of inferences. Results found SRs were 65.2% non-rigorous, 27.5% likely rigorous, and 7.2% rigorous; common limitations included inadequate risk of bias assessment, extractions not completed in duplicate, and lack of pre-registration, highlighting the need for higher quality SRs. We then performed a meta-meta-analysis to estimate the efficacy of MBIs to reduce PTSD symptoms, yielding a medium effect size (SMD=0.41, $p < .001$), derived from 22 meta-analyses (with replicable data) and 35 unique articles. Analyses were consistent across control conditions and MBI type (first-generation/narrow [i.e., MBIs with well-established protocols]) versus broad (i.e., other MBI types), comparable with second-line treatments (e.g., pharmacotherapy). Findings were narratively synthesized; areas for methodological improvements in MBI research were identified.

1. Introduction

Trauma exposure is common; epidemiological estimates suggest lifetime trauma exposure in childhood and adulthood is 52% and 53%, respectively (Garfin et al., 2020). Resultingly, cross-national lifetime PTSD prevalence is estimated at 3.9%, with estimates in some countries as high as 8.8% (Koenen et al., 2017) and prevalence disproportionally higher in populations exposed to more traumas. For example, lifetime prevalence of male and female veterans was estimated at 7.7% and 13.4%, respectively (Schein et al., 2021). Four symptom clusters (re-experiencing, avoidance, negative cognitions/mood, and hyperarousal), experienced for at least one month following major traumatic event exposure, characterize PTSD (American Psychiatric Association, 2013). PTSD is often chronic (lasting more than 3 months), with devastating effects on individuals and families (Hilton et al., 2017), including impairments in social and occupational health (Ehlers & Clark, 2000), poor physical functioning, and decreased well-being (Kearney & Simpson, 2015). Feasible, evidence-based interventions to alleviate PTSD symptoms are critical to addressing this prevalent, multifaceted public health threat.

Numerous frameworks (e.g., cognitive model, emotional processing theory, social cognitive theory) describe PTSD etiology and inform treatment design and administration (Ehlers & Clark, 2000; Kangaslampi & Peltonen, 2022; Zalta, 2015). Corrections in erroneous and maladaptive trauma-related cognitions and appraisals appear to be core mechanisms guiding PTSD symptom improvement (Ito et al., 2021; Kangaslampi & Peltonen, 2022) and are targeted in cognitive-behavioral therapies (CBTs; Ehlers & Clark, 2000; Follette et al., 2006; Zalta, 2015). Significant evidence exists for short- and long-term efficacy of general and trauma-focused (TF-) CBTs, integrated as current first-line PTSD treatments [most commonly prolonged exposure (PE) and cognitive processing therapy (CPT)] (Bisson et al., 2013; Kearney & Simpson, 2015; Zalta, 2015). Nevertheless, important limitations exist, since PTSD symptoms are often chronic and recalcitrant. For example,

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randomized controlled trials (RCTs) of PE and CPT interventions found 56% to 72% of patients retained their PTSD diagnoses post-intervention, with approximately 25% dropping out of treatment (Banks et al., 2015; Steenkamp et al., 2015). Also problematic, PTSD treatment is often expensive, limiting accessibility, especially in underserved communities. For example, for one veteran, average annual treatment cost is $19,630 (Davis et al., 2022). Perhaps resultingly, 65% of individuals with PTSD report receiving no treatment in the past year (Wang, Lane et al., 2005); median time from onset of first symptoms to treatment initiation is 12 years (Wang, Berglund et al., 2005). Thus, there has been growing interest in integrative, complementary, and/or alternative tools for addressing PTSD symptoms. Increasingly, these include mindfulness-based interventions (MBIs).

1.1. Mindfulness-based interventions for PTSD

1.1.1. Mindfulness and PTSD

Mindfulness is often defined as "paying attention in a particular way: on purpose, in the present moment, and non-judgmentally" (Kabat-Zinn, 1994, p. 4). There have been attempts to conceptualize mindfulness using a two-component model: 1) self-regulation of attentional focus on present moment experience, and 2) a non-judgmental orientation toward that experience, characterized by openness, acceptance, and curiosity (Bishop et al., 2004). Yet, significant disagreements still exist about the definition of this complex construct (Van Dam et al., 2018).

Mindfulness is considered a state (Lau et al., 2006) and a trait (Brown & Ryan, 2003). Practicing mindfulness is a state-level experience, which can be invoked during meditation or other mindfulness practices. Evidence suggests repeated mindfulness practice may increase trait mindfulness (Kiken et al., 2015), defined as the intensity, frequency, and duration with which an individual is immersed in the present moment (Hülsheger et al., 2013). Increased trait mindfulness appears to correlate with physical and psychological health benefits including lower anxiety, depression, and chronic pain (Brown & Ryan, 2003; Hülsheger et al., 2013; Lang et al., 2012; Lorenzini et al., 2023; Tran et al., 2022). MBIs exhibit medium effect-sizes for increasing mindfulness, although other active treatments (e.g., CBT) may also increase it (Tran et al., 2022).

Theoretically, mindfulness may address key PTSD symptoms including avoidance, emotion regulation deficits, and distress intolerance through building non-reactionary and non-judgmental attitudes toward trauma-associated thoughts and feelings (Follette et al., 2006; Lang et al., 2012). Engaging with present moment experiences may reduce past-oriented (e.g., rumination) and future-oriented (e.g., worry) maladaptive cognitive styles, which are both associated with development and maintenance of mood and anxiety disorders, including PTSD (Ehlers & Clark, 2000; Lang et al., 2012). Fostering non-judgmental and non-reactive components of mindfulness may decrease anxiety surrounding exposure to fear-provoking stimuli, which independently, or in conjunction with TF-CBTs, may address avoidance symptoms (Lang et al., 2012). Moreover, the acceptance component of mindfulness may decrease distress responses related to trauma-relevant experiences of guilt and shame (Banks et al., 2015), hallmark components of PTSD. For example, a combat veteran could feel guilty for not saving a friend who did not survive active combat. Non-judgmental acceptance of this emotional state could help the individual acknowledge that the presence of guilt is also a signal of love or value associated with the previous relationship, resulting in a decrease in distress.

1.1.2. Mindfulness-based interventions

Mindfulness-based stress-reduction (MBSR) was the first MBI to demonstrate empirical evidence for improving psychological symptoms (Hofmann & Gómez, 2017). It is comprised of face-to-face didactic and experiential activities, administered as an 8-week group-based program, incorporating a focus on awareness of breath, body scans, walking meditation, gentle yoga, and related techniques (Esper & Gherardi-Donato, 2019). Relatedly, mindfulness-based cognitive therapy (MBCT) is an 8-week adaptation of MBSR that was developed for relapse prevention in patients with depression (Segal et al., 2002). MBCT has many practices that are identical to MBSR and incorporates principles of cognitive therapy within the context of mindfulness techniques. These two structured interventions typically exhibit the strongest evidence for addressing mental health concerns and are considered part of the first-generation mindfulness-based programs (MBPs; i.e., MBIs with a well-established protocol), characterized by a lesser emphasis on Buddhist aspects, and an increased accessibility to people from diverse religious backgrounds (Crane et al., 2017).

Other, less structured, MBIs include trainings that teach mindfulness in the context of various meditation practices, focusing on awareness of physical, emotional, or cognitive sensations, and physical activity and/or movement (e.g., yoga, tai chi, qigong, mindfulness-based stretching) (Esper & Gherardi-Donato, 2019). Interventions that focus on yoga (an ancient Hindu practice), combine physical postures, breathing exercises and meditation (Capon et al., 2019; Cramer et al., 2018). Additionally, many other MBIs (including MBSR) combine formal meditation with yoga practices. Consequently, definitional inconsistencies, overlaps in intervention composition, and varied protocols can muddy the delineation between types of MBIs and mechanisms of treatment-response (Van Dam et al., 2018). To understand these differences, it is imperative to delineate first-generation MBPs, which are the gold-standard of MBIs (Crane et al., 2017), from the broader category of MBIs that include other treatments such as mindful yoga, general mindfulness meditation practices, and related activities. Hereinafter, we refer to these two types of MBIs as narrow and broad MBIs, respectively. Further, additional types of MBIs have recently been developed including brief MBIs (Howarth et al., 2019) and online MBIs (Spijkerman et al., 2016). Brief MBIs are shorter in duration than standard, traditional MBIs; online MBIs may be either similar or modified versions of standard, traditional MBIs administered in a virtual or mobile application format. However, Van Dam et al. (2018) raised concerns about the efficacy of these minimally tested adaptations of standard, traditional MBIs for treating symptoms of mental health ailments.

1.1.3. Can mindfulness-based interventions help improve PTSD symptoms?

Since 2009, there has been a sharp increase in number of clinical trials of MBIs reported in the literature (Zhang et al., 2021). Yet despite the elevated traction of MBIs in clinical trials, many RCTs investigating mindfulness approaches for treating PTSD have suffered from methodological weaknesses; common limitations include lack of blinding of participants and research personnel and selection bias (Goldberg et al., 2020; Hilton et al., 2017). Perhaps accordingly, there has been mixed evidence supporting the efficacy of MBIs for reducing PTSD symptoms, raising questions if and when MBIs should be recommended for addressing PTSD symptoms (Goldberg et al., 2020). For example, significant improvements in PTSD symptoms were reported in an RCT comparing MBSR with present-centered group therapy (Polusny et al., 2015), while an RCT of veterans receiving MBSR or treatment-as-usual (TAU) found no statistical difference in PTSD symptoms between groups four months post-intervention (Kearney et al., 2013).

1.2. Are existing systematic reviews of MBIs for PTSD conclusive?

Systematic reviews (SRs) have sought to clarify the efficacy and appropriateness of MBIs for addressing PTSD. Yet similar to individual trials, SRs have reported conflicting results, with some demonstrating significant improvements in PTSD following MBIs (Hopwood & Schutte, 2017) and others finding no clinically meaningful change in symptoms (Hilton et al., 2017). A lack of clear consensus on the efficacy of MBIs for PTSD could be due to methodological weaknesses in research designs as well as variability in procedures (e.g., type of MBI, length of administration) and populations assessed (e.g., veterans, survivors of interpersonal violence) that exist across both SRs and the individual studies they evaluate.

1.2.1. Overproduction of systematic reviews

The number of SRs in the health sciences has exploded: for example, the field of epidemiology demonstrated a 20-fold increase in annual numbers of SRs published between 2000 and 2019, with 80 SRs published each day (Hoffmann et al., 2021). The MBI field is no exception to this trend (Baminiwatta & Solangaarachchi, 2021). Despite the potential utility of MBIs for ameliorating PTSD symptoms, distilling results and recommendations from the myriad of publications has proved difficult: there were 7430 publications on "mindfulness" published in psychology and psychiatry journals between 2016–2021 (Baminiwatta & Solangaarachchi, 2021). More generally, the mass production of SRs of varying quality has resulted in conflicting, confusing, or misleading information that could provide spurious recommendations rather than help guide evidence-based medicine and healthcare (Ioannidis, 2016).

1.2.2. Umbrella reviews: The highest level of evidence synthesis

Umbrella reviews can help address these challenges, as they can rigorously synthesize disparate MBI evidence across previously conducted SRs. Furthermore, umbrella reviews can replicate prior meta-analytic findings using a standardization protocol to allow key variables of interest to be compared across SRs; hence, umbrella reviews are considered one of the most rigorous and informative levels of evidence synthesis available (Fusar-Poli & Radua, 2018). Synthesizing the data from unique clinical trials included within SRs can provide useful information regarding the utility of MBIs for reducing PTSD symptoms and address contradictory inferences about the efficacy of MBIs for treating PTSD. Findings from an umbrella review could thus provide important information to guide clinical and policy recommendations regarding MBI implementation for individuals with PTSD.

To help ensure the rigor of umbrella reviews, A MeaSurement Tool to Assess systematic Reviews (AMSTAR) was created to provide guidance in summarizing and critiquing the prolific number of SRs that have emerged within the field of interventions and clinical care (Shea et al., 2009). AMSTAR was revised in 2017 into AMSTAR2 to incorporate criteria relevant to non-randomized designs and Cochrane Risk-of-Bias (RoB) instruments (Higgins et al., 2011) and to simplify response categories (Shea et al., 2017). AMSTAR2 has been validated and is widely considered a reliable tool for quality assessment of SRs (Luchini et al., 2021). Cochrane SRs, generally considered the methodological "gold--standard" for conducting SRs, tend to correlate with "high quality" AMSTAR2 ratings (de Santis et al., 2021). Hence, AMSTAR2 may be a useful tool for facilitating a review and critique of the extant literature on MBIs for PTSD, focusing on the SRs that have been conducted thus far.

1.3. Study aims

We sought to synthesize and evaluate the current literature on the efficacy of MBIs for addressing PTSD. We had several aims:

I. Conduct a comprehensive search of SRs of MBIs for PTSD and extract key descriptive data (i.e., population, intervention, control group, study design, RoB appraisal of included studies, overall inferences) from SRs meeting inclusion criteria.
II. Apply AMSTAR2 to conduct a critical appraisal of the rigor of SRs that evaluate the efficacy of MBIs for PTSD.
III. Describe the characteristics of the clinical trials contained within included SRs by extracting key data (i.e., population, intervention, control group, study design, secondary follow-up, and overall inferences) relevant to evaluating the efficacy of MBIs for PTSD.
IV. Assess the efficacy of MBIs for reducing PTSD symptoms by extracting data from identified meta-analyses and re-analyzing their results; the efficacy of narrow and broad MBIs will be compared in sub-analyses.
V. Summarize the evidence on adverse effects of MBIs for PTSD, potential moderators of efficacy, and the overall state of the field.

2. Method

2.1. Protocol and registration

Preferred Reporting Items for Systematic Review and Meta-Analyses (PRISMA) guidelines (Moher et al., 2009) were implemented. The protocol was pre-registered at PROSPERO (https://www.crd.york.ac.uk/prospero/display_record.php?RecordID=158470, CRD42020158470). Four registered deviations were made: first, a combination of EndNote, Microsoft Excel, and Rayyan [a software designed to help assist with SRs (Ouzzani et al., 2016)] was used to screen for inclusion and extract data. We added a manual inspection of search results to check for reporting errors (e.g., duplicates and other redundancies not captured with EndNote or Rayyan), to facilitate forward and backward citation searching, and to allow a more detailed and accurate study selection and extraction process. EndNote was used to identify duplicates; EndNote and Rayyan were used for title and abstract screening; and Microsoft Excel was used for note-taking during full text review and data extraction. Second, AMSTAR2 guidelines were followed for quality assessment (Shea et al., 2017). Third, one part of the extraction process was modified, namely data extraction for summary of results. Rather than four raters extracting the same data from all the SRs, AMSTAR2 protocol guidelines were followed (Shea et al., 2017): two reviewers extracted data from a subset of ten SRs meeting final inclusion criteria. Then, after achieving 86% agreement, one reviewer extracted the remaining data. Finally, we performed additional subgroup analyses contrasting the efficacy of narrow and broad MBIs, SR quality, risk of bias, and types of primary studies included, which were not originally planned.

2.2. Search strategy

2.2.1. Systematic review inclusion criteria

Inclusion criteria for SRs were: (1) peer-reviewed SRs including meta-analysis or meta-synthesis, (2) primary outcome was acute stress, posttraumatic stress (PTS), posttraumatic stress disorder or symptoms of PTSD (e.g., avoidance, intrusion, re-experiencing), and (3) included delivery of an MBI (including MBSR, MBCT, mindfulness meditation, mindful yoga, mindful breathing, body scan, mindful relaxation techniques, tai chi, qigong, mindfulness-oriented recovery enhancement, mindfulness-based relapse prevention). To delineate between well-established MBIs designed specifically to target mindfulness and other mindfulness treatments at earlier stages of development or that are not clinically standardized, two intervention categories were created: narrow MBIs, which incorporated only first-generation MBPs (i.e., MBSR, MBCT), and broad MBIs, which included other MBPs, mindful yoga and mindfulness meditation (if they explicitly stated integration of mindfulness training), mindful body-oriented therapies (MBOTs), tai chi, and qigong.

Meditation or physical movement interventions that did not specifically focus on mindfulness were excluded. Similarly, we excluded established therapies where mindfulness may have been incorporated into treatment, but was not the primary focus of the intervention [e.g., ACT (Hayes et al., 1999), DBT (Linehan, 1993), and combined therapies], in addition to somatic therapies or other contemplative and related practices that did not specifically incorporate mindfulness techniques (e.g., music therapy, art therapy, compassion-based treatments, and many forms of relaxation therapies). We also excluded minimally tested MBI adaptations, including brief training regimens and online MBIs. No restrictions were placed on population, setting, research design type, control condition, funding type, or publication status. Only articles available in English were included in this SR for consistency in evaluation from the research team.

2.2.2. Information sources

Comprehensive literature searches were performed in six bibliographic databases: PubMed, CINAHL Complete (EBSCOHost), APA PsycInfo® (ProQuest), Web of Science Core Collection, Scopus, and the Cochrane Library. An initial search was performed on 11/05/2019 and then updated on 10/12/2021, 05/18/2022, and 06/29/2023. The six databases were searched for all SRs with an MBI as the independent variable and PTSD and related outcomes as the dependent variable. See Supplemental Material 1 for full search strategy and detailed return by database. The reference lists of included studies were also searched for relevant SRs.

2.3. Study selection

Three reviewers independently performed title and abstract review using Rayyan (Ouzzani et al., 2016). Any article that passed initial screen by any reviewer was retained for full-text review by all reviewers; disagreements were resolved by re-reading these articles together with the senior author [DRG], followed by a discussion and detailed justification of the decision to include or exclude them. Two separate extraction processes were performed to obtain: 1) descriptive information and summary of results from the included SRs and key data from the unique studies included within the SRs, and 2) data for quality appraisal of SRs.

2.4. Data extraction

2.4.1. Data extraction for summary of results

Authors (BJ and DRG) achieved consensus on key data to extract for the summary of results, which included population type, intervention, control condition, research design, study setting, timeframe for secondary follow-up, and PTSD symptoms (Methley et al., 2014). Data were extracted for each SR and for each article within that SR that met our inclusion criteria. (Some studies within included SRs were not relevant to the present inquiry). Further, for each SR, we extracted the number of studies included and key findings; for each unique study included within each SR, we also extracted quality appraisal (e.g., RoB) if reported by SR authors.

2.4.2. Extraction process for critical appraisal of SR quality

We assessed the quality of included SRs through a multi-step process. First, we extracted relevant data pertaining to the rigor of the included SRs, as defined by AMSTAR2 items. Second, we slightly adjusted AMSTAR2 scoring guidelines to address their definitional gaps (e.g., a review with either 0 or 9 non-critical weaknesses would be appraised as equally rigorous), and consequently categorized included SRs as high, moderate, low, or very low quality. Third, we re-assessed the validity of each AMSTAR2 item and subitem pertaining to our research scope (Shea et al., 2017), thus balancing the stringency and relevance of the tool with its quality appraisal capabilities. Finally, SRs meeting neither the initial nor revised ASMTAR2 criterion were categorized as non-rigorous; SRs meeting both criteria were categorized as rigorous; and SRs meeting only the revised criterion were categorized as likely rigorous.

Data extraction for AMSTAR2-based critical appraisal was conducted by three reviewers, who independently coded all included SRs using AMSTAR2 guidelines (Shea et al., 2017). Disagreements were resolved through consensus and discussions with the senior author [DRG], who also evaluated these specific items. AMSTAR2 guidelines provide a scoring criterion to delineate quality appraisal according to *critical* and *non-critical domains* (Shea et al., 2017). AMSTAR2 has seven *critical domains*, defined as potentially affecting the "validity of a review and its conclusions," although the AMSTAR2 authors note the seven domains may not be critical in all circumstances (Shea et al., 2017). *Non-critical domains*, while important, may not be critical for the sum validity of a review.

AMSTAR2 *critical domain* categories include: established review methods, comprehensive literature search, excluded study list, RoB quality assessment, discussed potential impact of RoB, appropriate statistical analysis (meta-analysis only), and publication bias (meta-analysis only). *Non-critical domain* categories include the use of PICO (population, intervention, control, outcomes) for research question and inclusion criteria, explanation of study design selection, study selection process, data extraction process, adequate description of included studies, funding sources of included studies, discussed impact of RoB on results (meta-analysis only), discussed impact of heterogeneity, and disclosed conflict of interest(s). Of note, some domains can receive partial points allocated for meeting individual components of a given domain. For example, for the RoB critical domain, an SR might meet partial AMSTAR2 criteria if utilizing an RoB tool that was sufficiently rigorous, even if the SR did not meet the remaining sub-criteria for the RoB critical domain (e.g., attrition bias). Two composite scores were given to each SR: a *critical weaknesses* and *non-critical weaknesses* total score, calculated as a count of items in each AMSTAR2 domain *not* met by the SR.

We then used these *critical weakness* and *non-critical weakness* scores to categorize the overall confidence of inferences for each SR. First, we drew from prior recommendations where overall quality was ranked as *high* (*critical weaknesses*=0 and *non-critical weaknesses*≤1), *moderate* (*critical weaknesses*=0 and *non-critical weaknesses*>1), *low* (*critical weaknesses*=1 and *non-critical weaknesses* ≥0), and *very low* (*critical weaknesses*>1 and *non-critical weaknesses*≥0) (Shea et al., 2017). However, strict use of this general guidance created situations where an SR with *critical weaknesses*= 0 and *non-critical weaknesses*= 9 would be rated *moderate*, while another review with *critical weaknesses*= 1, and *non-critical weaknesses*= 0 would be rated *low*. Similarly, details regarding allocation of partial points were unclear, particularly problematic for reviews where *critical weaknesses*= 0.5, as that score would be equidistant from the *high* and *low-quality* categories. To increase consistency, categories were allocated a half point margin, leaving the final criteria for overall quality of each SR as: *high* (*critical weaknesses*≤0.5; *non-critical weaknesses*≤1.5), *moderate* (*critical weaknesses*≤0.5; 1.5<*non-critical weaknesses*≤2.5), *low* (0.5<*critical weaknesses*≤1.5; 1.5 <*non-critical weaknesses*≤2.5), and *very low* (*critical weaknesses*>1.5; *non-critical weaknesses*>2.5). Finally, we followed AMSTAR2 guidance that appraisers should decide the most critical items for their analyses based on the potential impact of ratings for a given subfield (Shea et al., 2017). For example, categories most critical for strong inferences may be different for a drug trial compared to a behavioral intervention. Hence, after data extraction and initial critical appraisal, we revisited each AMSTAR2 item to make final adjustments to the scoring guidelines, contextualized within the aims of the present analyses (i.e., to synthesize the extant literature on MBIs for PTSD). We then conducted a final critical appraisal of all SRs using the revised AMSTAR2 guidelines.

2.5. Synthesis of results

We categorized included SRs depending on the types of findings they provided: quantitative or qualitative. We used quantitative evidence from the SRs to provide a broad overview of the efficacy of MBIs for treating PTSD as reported by the included SRs. Qualitative and quantitative data were used to narratively synthesize any reports of moderation effects, safety assessment, and participants' experience with MBIs.

2.5.1. Quantitative synthesis

We re-analyzed all meta-analyses following the Cochrane Overview of Reviews methodology (see section V.4.12; Pollock, Fernandes, Becker, Pieper, & Hartling, 2023) to elucidate how MBI efficacy for PTSD may vary across our evidence base. For comparison, we standardized all effect sizes (Fusar-Poli & Radua, 2018), using standardized mean difference (SMD) estimated by Hedges' g to account for biases in small samples. We included only PTSD-related outcomes, and separated

evidence based on whether the intervention used a narrow MBI or a broad MBI and whether it used a passive or active control condition. This allowed for a more thorough comparison of summary effects and heterogeneity across SRs and per MBI inclusion criteria. Some SRs included primary studies that did not examine the relationship between MBIs and PTSD: these primary studies were excluded from analyses.

Data were analyzed using Stata 17. Analyses used a random effects model and the restricted-maximum-likelihood estimator (REML) procedure for estimating variance between studies (Langan et al., 2019). Each SR was re-analyzed as follows: (a) summary effects (summary effect sizes, heterogeneity, publication bias estimates) from meta-analyses were replicated, (b) analytic method and summary measures were standardized, (c) summary effect sizes (Hedges' g), significance levels, and 95% confidence intervals were calculated, (d) heterogeneity (I^2 parameter) was estimated, (e) and evidence for publication bias was evaluated by performing small-study effects and excess significance bias tests (J. P. Ioannidis & Trikalinos, 2007). Within each SR, items included in (c), (d) and (e), were calculated separately for broad and narrow MBI categories, and then combined to provide omnibus statistics for that SR.

After re-analyzing each meta-analysis as described, we aggregated the findings, performing a composite meta-analysis of all meta-analyses included in this report to derive a summary effect size across all findings (i.e., a "meta-meta-analysis"). To ensure results would not be confounded by the overlap of the primary studies within the SRs, we created weights to ensure primary studies were not double counted (Munder et al., 2013). For each meta-analysis with k relevant primary studies, each study was divided by the total number that it appeared in all meta-analyses, then summed to provide k_{adj} (adjusted primary study count). We then performed sub-group analyses to determine if results differed according to narrow and broad MBI categories.

SRs that did not provide sufficient data for replication (i.e., primary study effect sizes including standard errors, confidence intervals, population size, mean and standard deviation values for experimental and control groups, and pre- and post-treatment metrics) were not included. For several other studies, even after contacting the SR authors, replication could not be achieved: there was incongruency between reported trial data and summary effect size.

To estimate the overlap of primary studies included in the meta-analyses, for both broad and narrow MBI SR categories, we calculated corrected covered area (CCA), which is an adjusted overlap metric. A CCA of 100% would be interpreted as all SRs in that category used identical studies (Pieper et al., 2014), thus capturing redundancy in primary studies. More specifically, the CCA was calculated as:

$$CCA = \frac{N - U_p}{U_p S - U_p} \quad (1)$$

where N = total number of all primary studies across all SRs, U_p= total number of unique primary studies across all SRs, and S=the number of SRs that had at least 1 relevant primary study included (Pieper et al., 2014).

3. Results

3.1. Search results

Database searches (including updates) yielded 3538 results. After removing duplicates, 1685 articles were included in title and abstract screening, which resulted in 333 articles eligible for full text review. Of those, 69 were SRs that met inclusion criteria and were included in data extraction and AMSTAR2 critical appraisal. See Fig. 1 for PRISMA flow chart and Supplemental Material 2 for complete list of excluded articles with exclusion justification(s).

3.2. Characteristics of included systematic reviews and unique studies

The included 69 SRs represented a total of 83 unique articles and 80 unique samples (50% RCTs and 50% non-randomized studies of interventions [NRSIs]), comprised of 5066 participants. Of SRs, 24.6% ($n = 17$) had more than half of their included articles pertinent to our research question (i.e., the efficacy of MBIs for PTSD); almost 50% ($n = 33$) of SRs had only one or two relevant studies, with the remainder of other articles in that SR typically focused on interventions other than MBIs and/or outcomes other than PTSD.

Table 1 presents descriptive statistics of SRs (left side column) and the unique articles included within them (right side column). As indicated in the left column (bottom of Table 1), a notable increase in the number of relevant SRs published occurred in 2017. The vast majority of SRs (75.4%) were not pre-registered, although that trend improved over

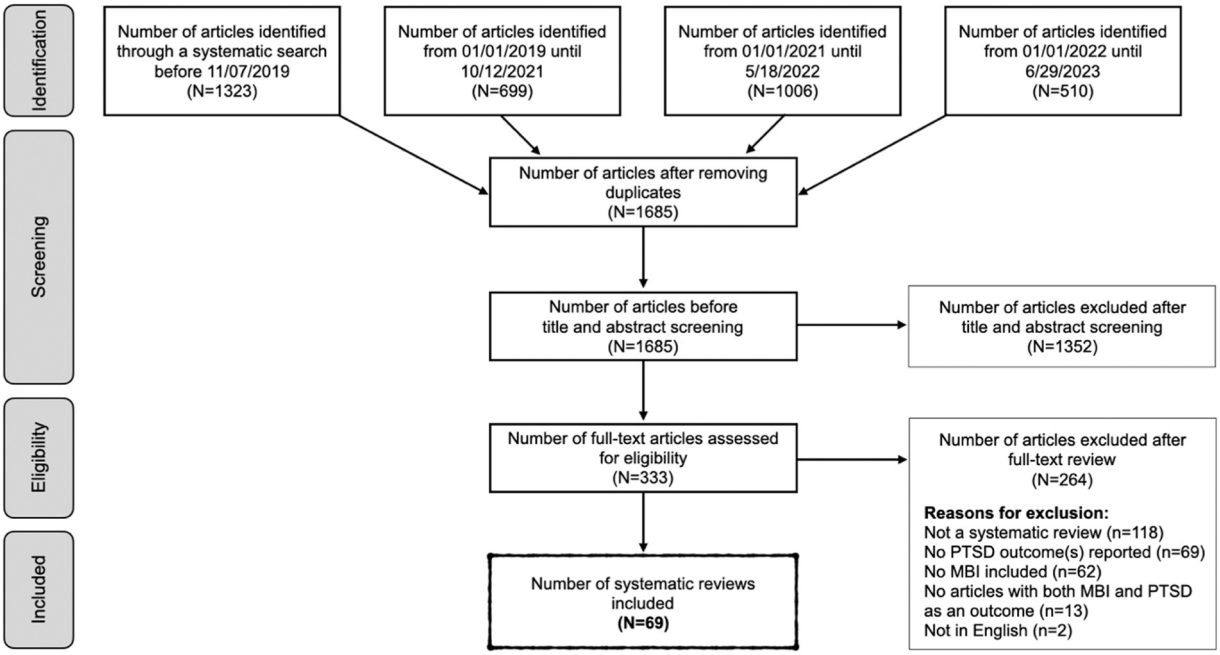

Fig. 1. PRISMA Flow Diagram.

Table 1
Characteristics of Systematic Reviews (N = 69) and Unique Included Studies (K=83[†]).

Characteristics of Systematic Reviews (SR), $N = 69$	n	%	Characteristics of Unique Studies Included within SRs, $K = 83$[†] (80 unique samples)	n	%
Study Design of Included SRs			**Study Design of Unique Articles**[a]		
RCT	28	40.6	RCT	40	50.0
NRSI	1	1.4	NRSI	40	50.0
RCT and NRSI	40	58.0	**Intervention Type**		
Meta-analysis			Mindful yoga	23	28.8
Yes	27	39.1	MBSR	22	27.5
No	42	60.9	Mindful body-oriented therapy	11	13.8
Was the SR Pre-registered?			Other MBPs (e.g., MBET, MORE)	11	13.8
Yes	17	24.6	Mindfulness meditation	5	6.3
No	52	75.4	MBCT	5	6.3
Year Published			Tai Chi + Qigong	3	3.8
2010	1	1.4	**Intervention Duration**		
2011	1	1.4	$L \leq 4$ weeks	4	5.0
2012	0	0.0	4 weeks $< L \leq 8$ weeks	14	17.5
2013	3	4.3	$L = 8$ weeks	41	51.3
2014	2	2.9	8 weeks $< L \leq 12$ weeks	13	16.3
2015	3	4.3	12 weeks $< L \leq 16$ weeks	5	6.3
2016	2	2.9	$L > 16$ weeks	3	3.8
2017	5	7.2	**Type of Comparator/Control Group**		
2018	8	11.6	Active control, PTSD treatments[b]	9	11.3
2019	10	14.5	Active control, non-PTSD targeted treatments[c]	14	17.6
2020	6	8.7	Passive	24	30.0
2021	12	17.4	No control group utilized	33	41.3
2022	12	17.4	**Sample Size**		5066[d]
2023	4	5.8	$N \leq 20$	21	26.3
Was SR Funded?			$20 < N \leq 50$	26	32.6
Yes	30	43.5	$50 < N \leq 100$	20	25.0
No	14	20.3	$100 < N$	13	16.3
Not reported	25	36.2	**Population**		
Was Risk of Bias (RoB) Assessment Performed?			Veterans	40	50.0
Yes	53	76.8	Victims of IPV	7	8.8
No	16	23.2	Adults with PTSD	5	6.3
Rating of RoB of Included Articles			Victims of childhood abuse	5	6.3
High	22	31.9	Minors with PTSD	3	3.8
Medium	4	5.8	Adults with comorbid SUD	3	3.8
Low	5	7.2	Police officers	3	3.8
No overall RoB	22	31.9	Other (e.g., nurses, refugees)	14	17.5
No RoB performed	16	23.2	**Secondary Follow-up Assessment, > 1 month**		
			Yes	37	46.3
			No	43	53.8

Abbreviations: IPV (intimate partner violence); MBCT (Mindfulness-based Cognitive Therapy); MBET (Mindfulness-Based Exposure Therapy); MBSR (Mindfulness-Based Stress Reduction); MORE (Mindfulness-oriented Recovery Enhancement); NRSI (non-randomized control trial); RCT (randomized controlled trial); SUD (substance use disorder)

[a] K includes unique articles across SRs (many articles were duplicated across SRs). Cumulative counts including duplicates were: total= 332, RCT= 224, NRSI= 102, SR= 6,

[b] includes common PTSD treatments (e.g., CBT),

[c] includes non-PTSD interventions (e.g., health promotion),

[d] total sample size from all unique articles,

[†] 3 articles were not unique samples as they presented follow-up analyses of samples presented in other reports. These data were excluded from descriptive statistics.

time: 8.6% of studies were pre-registered before 2020, compared to 44.1% after 2020. RoB analysis was not performed in 23.2% of SRs; one-third of SRs included an RoB that was not a rigorous evaluation on par with AMSTAR2 (Shea et al., 2017) or Cochrane Collaboration's tool (Higgins et al., 2011). Of SRs, 31.9% ($n = 22$), 5.8% ($n = 4$), and 7.2% ($n = 5$) rated their included studies as high RoB, medium RoB and low RoB, respectively. The remaining SRs did not report a cumulative RoB. See Supplemental Material 3 (Table S1) for all extracted information from unique articles.

As presented in Table 1 (right side column), of the 83 unique articles included within the 69 SRs, populations examined included veterans (50.0%), survivors of intimate partner violence (IPV) or childhood sexual abuse (15.0%), general population experiencing PTSD (10.0%) and "other" (25.0%). The most common MBI examined was mindful yoga (28.8%), followed by first-generation MBPs (e.g., MBSR, MBCT) (27.5%), mindful body-oriented therapies (13.8%), and other MBPs (e.g., mindfulness-oriented recovery enhancement, mindfulness-based exposure therapy, mission reconnect) (13.8%). The duration of MBIs was relatively consistent; 85% of all interventions lasted between 4 and 12 weeks, and more than half of MBIs were 8 weeks long. Comparator interventions were mostly passive (30.0%), while active/attention control groups included either general non-PTSD interventions (17.6%) (e.g., health education, biofeedback) or PTSD-specific, evidence-based treatments (e.g., CBT, PE, present-centered therapy) (11.3%). Finally, of unique articles, in addition to an immediate post-intervention assessment, 46.3% reported a secondary follow-up assessment (≥1-month post-MBI), with 12.5% including a secondary follow-up assessment ≥ 6 months post-MBI.

3.3. Results from quality appraisal using AMSTAR2 guidelines

Omnibus quality of included SRs is illustrated in Figure 2. Plots include each SR's non-critical weaknesses score (y-axis) by critical weaknesses score (x-axis), separated by meta-analysis (left side of panel, Figure 2A) and narrative (i.e., non-meta-analyses) SRs (right side of panel, Figure 2B).

High, moderate, low, and very low-quality SRs are denoted by shaded areas (see Figure 2 legend). Each circle on the plot represents the

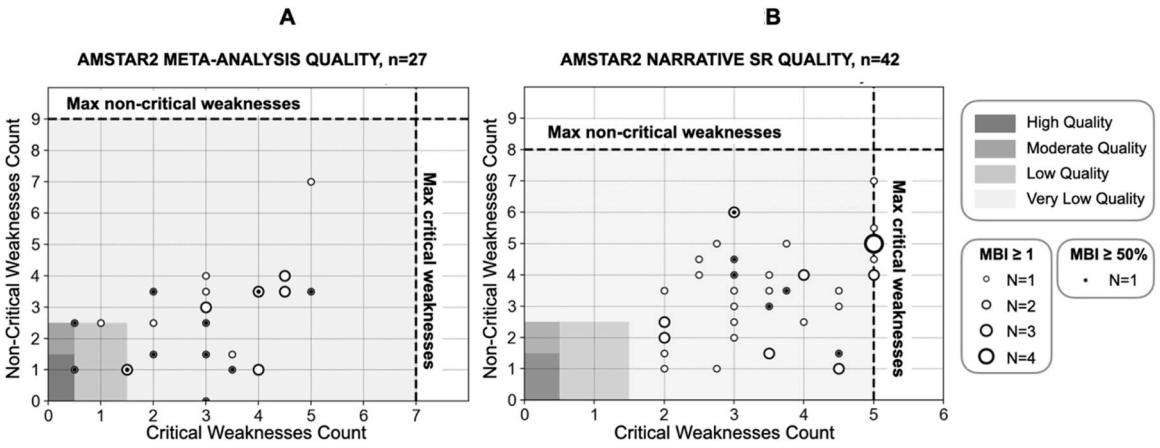

Fig. 2. Quality Assessment Results of Meta-analyses (Figure 2A) & Narrative SRs (Figure 2B).

Table 2
Initial and Final Critical Appraisal of Quality Systematic Reviews (N = 69).

	Initial Criteria				Revised Criteria			
	Meta-analysis		Narrative SRs		Meta-analysis		Narrative SRs	
	(*n* = 27)		(*n* = 42)		(*n* = 27)		(*n* = 42)	
AMSTAR2 Quality Appraisal	*n*	%	*n*	%	*n*	%	*n*	%
High	1	3.7	0	0.0	5	18.5	2	4.8
Moderate	1	3.7	0	0.0	2	7.4	0	0.0
Low	3	11.1	0	0.0	7	25.9	8	19.0
Very Low	22	81.5	42	100.0	13	48.1	32	76.2

score of an individual SR that had at least one MBI with PTSD as the outcome; thus, circles with larger radii denote more SRs with that score. Black dots indicate SRs that had ≥ 50% of included articles meeting our inclusion criteria ($n = 17$). "Max" range is the highest score possible according to AMSTAR2 scoring criteria. Figure 2A reveals that only 18.5% of meta-analyses were rated as better than very low quality, while 100% of narrative SRs were very low quality (see Figure 2B). In total 92.8% ($n = 65$) of all included SRs fell within the very low-quality region. Moreover, 20.3% ($n = 14$) of the included SRs did not fully meet any of the critical domains defined by AMSTAR2. SRs that focused primarily on MBIs for treating PTSD were of slightly better quality, although most of them were still categorized as very low-quality (82.4%). The four SRs plotted outside the very low-quality region represent the highest quality SRs (see Figure 2A). See Supplemental Material 4 for data file with full AMSTAR2 ratings for each SR.

3.4. Summary of quality assessment after revisions to AMSTAR2 criteria

Guided by theory and empirical data, we then conducted analyses to refine AMSTAR2 guidelines as relevant to the present study's aims. To this end, we evaluated the validity and reliability of each critical and non-critical weaknesses score in the context of the present inquiry (Shea et al., 2017) as items defined as critical or non-critical weaknesses may not all carry equivalent importance (Leclercq et al., 2020), nor have sufficient reliability when coded (Shea et al., 2017). For details of this re-analysis, please see Supplemental Material 5.

Table 2 presents results from quality assessment (high, moderate, low, and very low) using initial critical appraisal criteria and after AMSTAR2 item revisions. We synthesized these ratings and re-categorized SRs as non-rigorous (very low quality with initial and revised criteria), likely rigorous (very low quality with initial criteria but low quality or better with revised criteria), and rigorous (low quality or better with initial and revised criteria). Consequently, 65.2% of SRs were appraised as non-rigorous, 27.5% were appraised as likely rigorous, and 7.2% were appraised as rigorous. Post-revision, meta-analyses had significantly higher quality rating compared to narrative SRs ($\chi^2[3, N = 69]=8.52$, $p = 0.036$).

3.5. Synthesis of results and review of the evidence

3.5.1. Overview

Table 3 summarizes each SR with more than 50% of its primary articles relevant to our research question by rigor (non-rigorous, likely rigorous, and rigorous according to AMSTAR2 revised guidelines), number of unique studies relevant to our research question, population, intervention, AMSTAR2 quality score, key findings, and RoB of included articles in that SR as appraised by their authors. Supplemental Material 6 (Table S3) presents an expansion of Table 3 that also includes SRs with at least one relevant primary study.

3.5.2. Efficacy of MBIs for treating PTSD

We compared efficacy findings from all 22 included meta-analyses for which we could replicate results (see Table 4). Efficacy findings included summary effect sizes, heterogeneity, and evidence of publication bias for each MBI definition (narrow vs. broad). Additionally, number of participants (N), number of primary studies (k), and adjusted number of primary studies (k_{adj}) were included in the analysis to indicate proportion of evidence summarized in each SR. See Supplemental Material 7 for forest plots for both broad and narrow MBIs, active and passive control groups, and subgroup analyses for each SR that was replicated.

Our meta-meta-analysis included 22 meta-analyses, comprising of 35 unique primary studies and 2803 study participants representing 55.3% of the entire identified sample of MBIs for treating PTSD. Results yielded an overall effect size of $SMD_{adj}= 0.41$, $p < .001$, 95% CI [0.30, 0.52] of MBIs when compared to combined active and passive control groups. Moderator analyses revealed no group differences across MBI definition (narrow vs. broad) or control group condition (active vs. passive). The degree of overlap for narrow and broad MBIs was CCA= 23.5% and CCA= 16.4%, respectively.

3.5.3. Narrative synthesis

To summarize evidence on MBI moderators, safety/adverse effects, and participants' perceived experience with MBIs, we synthesized key findings from meta-analyses that performed moderator analysis ($n = 13$) and SRs that included at least three articles relevant to our research question ($n = 32$).

Meta-analyses investigated a wide variety of moderators. No notable differences were observed according to study populations and trauma

Table 3
Characteristics of Included Systematic Reviews, Organized by Revised AMSTAR2 Ratings.

Citation	Relevant Studies		Population	Intervention	Quality Rating (Number of Weaknesses)		Risk of Bias[†]	Key Findings
	RCT	NRSI			Critical Weaknesses	Non-Critical Weaknesses		
RIGOROUS EVALUATION								
Goldberg et al. (2020)[a]	10	0	Veterans	MBSR, MBCT, Other MBPs	1.5	1	No overall RoB	Significant PTSD symptom reduction ($p < 0.05$) for both narrow ($k = 5$) and broad MBIs ($k = 5$) despite large heterogeneity ($I^2 > 50\%$). MBI participants were significantly more likely to drop out (OR=1.98).
Hilton et al. (2017)[a]	6	0	Veterans, Survivors of IPV, Survivors of abuse	MBSR, Yoga	0.5	1	High	Significant PTSD symptom reduction ($p < 0.05$) for only broad MBIs ($k = 6$) despite large heterogeneity ($I^2 > 50\%$). Four studies assessed safety; none found adverse events for MBI groups.
Reis et al. (2022)[a]	2	5	Veterans	Yoga	0.5	2.5	High	Significant PTSD symptom reduction ($p < 0.05$) for broad MBIs ($k = 2$).
LIKELY RIGOROUS EVALUATION								
Björkman & Ekblom, 2021[a]	6	0	Veterans, Mothers of stillbirth babies	Yoga, MBOT	2	3.5	No overall RoB	Significant PTSD symptom reduction ($p < 0.05$) for broad MBIs ($k = 6$) despite large heterogeneity ($I^2 > 50\%$). Higher intensity treatment (>20 h/treatment) was more beneficial for decreasing PTSD.
Esper and Gherardi-Donato (2019)[b]	2	4	Survivors of IPV	MBSR, Mindfulness meditation	3	4	High	Four relevant studies reported decreases in PTSD symptoms.
Gallegos et al. (2017)[a]	13	0	Veterans	MBSR, Yoga, MBOT, Mindfulness meditation	2	1.5	High	Significant PTSD symptom reduction ($p < 0.05$) for both narrow ($k = 4$) and broad MBIs ($k = 9$) despite large heterogeneity ($I^2 > 50\%$).
L. N.Sun et al. (2021)[a]	13	0	Veterans	MBSR, Yoga, Mindfulness meditation, Other MBPs	3	1.5	No overall RoB	Significant PTSD symptom reduction ($p < 0.05$) for both narrow ($k = 5$) and broad MBIs ($k = 8$).
Taylor et al. (2020)[a]	18	15	Veterans, Nurses, General population with PTSD	MBSR, MBCT, Yoga, Mindfulness meditation, Other MBPs	3	0	Low	Significant PTSD symptom reduction ($p < 0.05$) for both narrow ($k = 7$) and broad ($k = 10$) MBIs, despite large heterogeneity ($I^2 > 50\%$). Subgroup analyses revealed significantly higher effects for non-funded vs. funded studies, and optimal intervention length ≥ 8 and < 10 weeks. Four studies reported mild adverse effects.
van de Kamp et al. (2019)[a]	7	4	Veterans, Nurses, Survivors of sexual abuse	Yoga, MBOT	3	2.5	No overall RoB	Significant PTSD symptom reduction ($p < 0.05$) for broad ($k = 9$) MBIs, despite large heterogeneity ($I^2 > 50\%$).
NON-RIGOROUS EVALUATION								
Banks et al. (2015)[b]	4	9	Veterans, Survivors of IPV	MBSR, MBCT, Mindfulness Meditation	3	4.5	No overall RoB	Twelve studies showed PTSD symptoms improved; one study found no improvements.
Cramer et al. (2018)[a]	5	0	Veterans, Chronic PTSD	Yoga	3.5	1	High	Significant PTSD symptom reduction ($p < 0.05$) for broad MBIs ($k = 5$).
Cushing and Braun (2018)[b]	5	5	Veterans	MBSR, Yoga	3.75	3.5	No overall RoB	Five studies showed that PTSD symptoms improved significantly at post-test and follow-up; two studies found no improvements.
Hopwood and Schutte (2017)[a]	13	0	Veterans, Survivors of IPV, Chronic PTSD	MBSR, Yoga, MBOT, Mindfulness meditation, Other MBPs	5	3.5	Not reported	Significant PTSD symptom reduction ($p < 0.05$) for both narrow ($k = 5$) and broad ($k = 8$) MBIs. Longer interventions were associated with lower levels of PTSD.
Liu, Zhu, & Zhang, 2022[a]	9	0	Veterans, Survivors of IPV, Traffic accident survivors	MBSR	4	3.5	No overall RoB	Significant PTSD symptom reduction ($p < 0.05$) for narrow MBIs ($k = 9$).
Niles et al. (2018)[b]	12	0	Veterans	Yoga, MBSR, Other MBPs	3.5	3	High	Nine studies showed improved PTSD symptoms post-MBI; 1 showed no improvements; 2 did not quantify them.
Sciarrino et al. (2017)[b]	5	1	Disaster survivors	Yoga	4.5	1.5	Not reported	Average weighted effect size from RCT studies was 0.48. NRSI study reported no improvement.
Wagner and Cáceres-Melillo (2023)[b]	1	3	Veterans, Adolescents, Black women	MBSR, MBCT	3	6	No overall RoB	Two studies reported significant improvements in PTSD after participation in MBI. The rest showed mixed results.

Abbreviations: NRSI (non-randomized studies of interventions); MBCT (mindfulness-based cognitive therapy); MBI (mindfulness-based intervention); MBOT (mindful body-oriented therapy); MBP (mindfulness-based programs); MBSR (mindfulness-based stress reduction); RCT (randomized controlled trial); SR (systematic review);
[a] meta-analysis,
[b] narrative SR,
[†] Overall RoB of included articles as appraised by SR authors.

Table 4
Efficacy of mindfulness-based intervention for reducing PTSD symptoms for each replicated meta-analysis (N = 22), and results of meta-meta-analysis.

Citation	SR Quality	Risk of Bias[†]	N	k	k_{adj}	SMD (g)	I^2	SSE/ ESB	N	k	k_{adj}	SMD (g)	I^2	SSE/ ESB
			Narrow MBIs						Broad MBIs					
			Combined active and passive control groups											
Bisson et al. (2013)	Rigorous	High	47	1	0.11	0.49	NA	NA	No broad MBIs included					
Goldberg et al. (2020)	Rigorous	No overall RoB	292	5	1.15	0.70*	85%	no/no	220	5	1.83	0.33**	0%	no/no
Hilton et al. (2017)	Rigorous	High	163	2	0.24	0.09	76%	NA	215	4	0.48	0.68**	63%	no/no
Montero-Marin et al. (2019)	Rigorous	High	No narrow MBIs included						102	1	0.13	0.19	NA	NA
Reis et al. (2022)	Rigorous	High	No narrow MBIs included						256	2	1.25	0.46***	0%	NA
Björkman & Ekblom, 2021	Likely rigorous	No overall RoB	No narrow MBIs included						370	6	1.78	0.57**	63%	no/no
Hedman-Lagerlöf et al. (2018)	Likely rigorous	No overall RoB	163	2	0.24	0.15	66%	NA	164	2	0.29	0.02	0%	no/no
Gallegos et al. (2017)	Likely rigorous	High	448	4	1.07	0.32**	0%	no/no	862	9	1.81	0.39**	63%	no/no
Maher et al. (2021)	Likely rigorous	No overall RoB	No narrow MBIs included						211	3	0.44	0.78*	72%	no/no
L. N.Sun et al. (2021)	Likely rigorous	No overall RoB	458	5	0.99	0.23**	0%	no/no	441	8	2.16	0.27**	0%	no/no
Taylor et al. (2020)	Likely rigorous	Low	374	7	1.90	0.56***	0%	no/no	605	11	2.85	0.51***	63%	no/yes
van de Kamp et al. (2019)	Likely rigorous	No overall RoB	No narrow MBIs included						584	9	3.15	0.58**	75%	no/no
Yu et al. (2022)	Likely rigorous	High	No narrow MBIs included						348	5	0.86	0.80	91%	no/no
Benfer et al. (2021)	Non-rigorous	Medium	37	1	0.50	0.55	NA	NA	No broad MBIs included					
Cramer et al. (2018)	Non-rigorous	High	No narrow MBIs included						333	5	0.78	0.61***	32%	no/no
Hopwood and Schutte (2017)	Non-rigorous	Not reported	334	5	1.15	0.58***	47%	no/no	899	8	1.77	0.32***	0%	no/no
Goldstein et al. (2019)	Non-rigorous	Medium	55	1	0.17	0.29	NA	NA	160	1	0.50	0.62**	NA	NA
Kysar-Moon, Vasquez, & Luppen, 2021	Non-rigorous	Not reported	No narrow MBIs included						102	2	0.17	0.31	43%	NA
Liu, Zhu, & Zhang, 2022	Non-rigorous	No overall RoB	682	9	4.24	0.46***	0%	no/yes	No broad MBIs included					
Mahoney et al. (2019)	Non-rigorous	No overall RoB	45	1	1.00	0.39	NA	NA	282	3	0.67	-0.02	98%	no/yes
Nguyen-Feng et al. (2019)	Non-rigorous	High	No narrow MBIs included						424	6	1.86	0.39*	58%	no/no
Ramachandran et al. (2022)	Non-rigorous	No overall RoB	No narrow MBIs included						102	2	0.17	0.31	45%	NA
Meta-meta-analysis (narrow vs. broad MBIs)			838	12	NA	0.43***	0%	no/no	1965	23	NA	0.40***	0%	no/no
			Combined narrow and broad MBIs											
			Active						Passive					
Meta-meta-analysis (active vs. passive control groups)			1530	18	NA	0.42***	0%	no/no	1273	17	NA	0.40***	0%	no/no

Abbreviations: SR (systematic review); RoB (risk of bias); N (sample size); k (number of SRs analyzed); k_{adj} (adjusted number of SRs analyzed); I^2 (measure of heterogeneity); SSE (small-study effect); ESB (excess significance bias); MBI (mindfulness-based intervention); NA (not applicable).
[*] $p < 0.05$,
[**] $p < 0.01$,
[***] $p < 0.001$,
[†] Overall RoB of included articles as appraised by SR authors.

types, gender, age, pre-intervention diagnosis of PTSD, sample size, types of MBIs, group vs. individual setting, trauma-focused vs. non-trauma focused treatment, or assessment tools. Differences were found between types of control groups, timeline of the follow-up assessment, duration and length of MBI administration, types of funding, and quality of included clinical trials. Seven SRs evaluated potential differences between active and passive control conditions, but only Goldberg and colleagues (2020) found evidence of such differences (efficacy decreased from SMD=0.64 to SMD=0.25 in passive compared to active control groups, respectively). PTSD improvements were not sustained at 3-month post-intervention in either active or passive controls (Goldberg et al., 2020). Optimal MBI treatment duration was between 8–9 weeks, and publicly funded trials reported lower effect sizes compared to either non-funded or non-governmental organization funded studies (Taylor et al., 2020). Despite the abundance of high RoB primary studies, only Hedman-Lagerlöf et al. (2018) found evidence that lower quality MBI trials showed higher treatment effects.

Five out of 32 SRs assessed safety, mostly reporting no or mild evidence of adverse effects. However, Goldberg and colleagues (2020) reported a significantly higher drop-out rate for MBI participants compared to those in an active or passive control condition (OR=1.98, 95% CI [1.31, 2.97]), and highlighted some difficulties among veterans in understanding and engaging in mindfulness practices. Relatedly, common factors reported by veterans in a narrative SR that hindered MBSR enrollment included lack of time, scheduling difficulties, group aversion, and insufficient or inaccurate information about MBSR (Marchand et al., 2021). Full results of the narrative synthesis can be found in the Supplemental Material 8.

4. Discussion

4.1. Overview

By presenting data from a comprehensive search of SRs of MBIs for treating PTSD, this umbrella review aimed to address important challenges in summarizing the existing MBI literature. These challenges include inconsistencies in defining MBI interventions (i.e., broad compared to narrow MBIs), the overproduction of SRs with methodological concerns, and mixed results regarding the efficacy findings of MBIs for reducing PTSD symptoms. Using an empirically rigorous assessment tool (AMSTAR2), which we systematically refined and tailored to our research question, we conducted a critical appraisal of SRs of MBIs for PTSD, finding that many did not meet the recommended standards for empirical rigor. Further, we synthesized the existing evidence of MBIs for reducing PTSD symptoms, providing an improved understanding of the rigor of the field, a comprehensive description of relevant SRs and their primary studies, a refined estimate of the overall efficacy of MBIs for addressing PTSD symptoms, and a summary of

reported adverse effects and potential moderators of efficacy.

To our knowledge, this is the first inquiry that compared the efficacy of narrow and broad MBIs for reducing PTSD symptoms, finding comparable effects across narrow and broad MBIs, with no significant between group differences. Additionally, we compared the efficacy of MBIs for PTSD across clinical trials that used active versus passive control groups; we found no differences. This data corroborates with the majority of the included SRs that performed this type of moderator analysis. Importantly, these estimates were derived using at least twice as many primary studies and study participants as any of other meta-analyses included in our umbrella review. The majority of SRs concluded that MBIs had a positive effect on ameliorating PTSD and related symptomatology; analyses of the articles included within those SRs found a medium effect size of that relationship, consistent with other second-line treatments for PTSD (Gallegos et al., 2017).

4.2. Summary of AMSTAR2 quality appraisal and rigor of unique articles

Quality of the included 69 SRs varied widely, with the vast majority (65.2%) categorized as non-rigorous in our final quality appraisal based on revised AMSTAR2 guidelines. The rigor of narrative SRs compared to meta-analyses was significantly lower, with 76.2% appraised as non-rigorous. One third of narrative SRs did not fully meet any critical domains defined by AMSTAR2 (established review methods, comprehensive literature search, excluded study list, RoB assessment quality, and discussed potential impact of RoB). Notably, two of the five rigorous SRs in our review did not focus on MBIs, each including only one relevant article. In other words, only three SRs focusing on MBIs specifically were potentially rigorous enough to make strong inferences or guide clinical and policy decisions related to the use of MBIs for PTSD. The other SRs that we included in our review had common flaws that may reduce reproducibility of the findings including absent or non-rigorous RoB assessments, study selection and extraction not done in duplicate, and impact of RoB or heterogeneity on findings not discussed, and lack of pre-registration. These methodological concerns of SRs may have significant impact on the validity of results (Møller et al., 2018).

Our findings demonstrate that while many SRs examining the potential benefit of MBIs on PTSD and related symptoms have been conducted, most were overlapping in nature, utilizing similar data sets to guide their inferences. Indeed, we found only 25% of articles across SRs were unique. Similarly, meta-analyses demonstrated an overlap of more than 15% across narrow or broad MBIs. Such overlap is considered very high (Pieper et al., 2014), and resulted in 22 meta-analyses yielding only 35 unique studies from which inferences were drawn. These problems highlight a broader issue in the field of SRs: as Ioannidis (2016) and Møller and colleagues (2018) note, there is an overproduction of relatively low-quality SRs, where many reviews suffer from excessive redundancy, providing little additional value, further diluting the field, and potentially causing more confusion than clarity with respect to clinical recommendations.

We also investigated the rigor of the articles included within SRs as appraised by SR authors. While more than half of the unique articles were RCTs, considered the "gold-standard" of research design, there were a number of common methodological weaknesses (Gallegos et al., 2017). Only 7.2% of SRs appraised their articles as low RoB; common weaknesses included lack of blinding of participants and personnel (performance bias) and lack of random sequence generation (selection bias). Lack of blinding is especially concerning due to its association with the reproducibility crisis (Begley & Ioannidis, 2015). Future scholars should consider incorporating and following rigorous instruments into their design of RCTs, such as the Cochrane Risk of Bias instrument (Higgins et al., 2011), to bolster inferences.

4.3. MBIs are likely beneficial for reducing PTSD symptoms

We conducted a meta-meta-analysis, incorporating all replicated SRs data, which maximized the number of primary studies (k = 35) and study participants (N = 2803) in our analysis. We address methodological concerns of prior research by only including data from meta-analyses that were reproducible, improving the confidence of our findings. Our analysis demonstrated a robust medium effect size (SMD_{adj}=0.41) across MBI interventions on PTSD symptoms. This included both narrow MBIs such as MBSR and MBCT, which are considered the gold-standard, and broad MBIs including mindful yoga, mindfulness-body oriented therapies, other mindfulness-based programs, and mindfulness meditations. Surprisingly, no difference was found between narrow and broad MBIs from subgroup analyses, even though the MBSR and MBCT are more structured and standardized. This finding, however, corroborates evidence presented in our narrative summary, which also found similar effects between different MBI intervention types. Similarly, no difference was indicated when comparing MBIs with the active versus passive control conditions, suggesting these results were not inflated by the type of comparator intervention.

This general salutary effect of MBI on PTSD is encouraging and robust as it represents data analyzed from 22 meta-analyses that included narrow and/or broad MBIs. However, this effect size is lower than other first line treatments, including TF-CBT (SMD=1.62), non-trauma-focused CBT (SMD=1.22) and eye movement and desensitization and reprocessing (SMD=1.17), which demonstrate large effects (Bisson et al., 2013). Importantly, the efficacy of MBIs is comparable to other second-line treatments such as medication management (SMD=0.42) (Gallegos et al., 2017) with perhaps fewer side effects. While MBIs report smaller effect sizes compared to first-line treatments, they are shorter in duration (8 weeks is typical) and often delivered in a group format, thus requiring less resources, and decreasing overall cost. MBIs are also associated with decreases in shame and self-criticism (Westerman et al., 2020), common emotional responses in PTSD that can hinder cognitive restructuring (Ehlers & Clark, 2000) and potentially trauma disclosure. Estimates suggest up to 4.9% of trauma survivors are not comfortable sharing details of their trauma (Kessler et al., 2017). As such, MBIs might be an alternative treatment for such individuals who are reticent to disclose details as is required by other first-line treatments such as CPT or PE.

4.4. Constraints of the current evidence and suggestions for future research

The present SR highlighted several important limitations of the extant research on MBIs for PTSD. Although many population-related factors (such as age, gender, study population and trauma type, and prediagnosis of PTSD) did not moderate the efficacy of MBIs, samples still tended to be homogeneous. For example, more than half of unique articles examined samples of veterans. Kessler et al. (2017) differentiated seven categories of traumatic experiences: war-related trauma, physical violence, intimate partner or sexual violence, accident, unexpected death of a loved one, traumas of loved ones or witnessing traumas of loved ones, and other traumas. The last four of these categories have either not been represented in the literature on MBIs for PTSD or represented very minimally (i.e., one unique article studied survivors of an accident), despite unexpected death of a loved one being the most common trauma type at 39% (Kessler et al., 2017). It is important to study MBIs for PTSD across a range of trauma survivors as findings suggest critical caveats across populations. For example, despite some compelling evidence regarding the effectiveness of MBIs with veterans (Polusny et al., 2015), others have raised concerns about the fit of mindfulness within military culture and its aspects (e.g., self-reliance, emphasis on "toughness") more broadly (Goldberg et al., 2020). Finally, key trauma exposure characteristics (e.g. type, frequency, age of event) are rarely performed in diagnostic or clinical interviews during MBI study protocols (Taylor et al., 2020). Including a comprehensive trauma inventory in studies could help clarify trauma survivors who

may benefit the most from participating in MBIs.

Another key limitation for making strong recommendations on the efficacy of MBIs for PTSD is the lack of standardization in intervention procedures and some heterogeneity between types of MBIs (e.g., MBSR compared to mindful yoga). While MBSR has a standard protocol for intervention administration, over 59% of unique articles in this SR utilized interventions (e.g., mindful yoga, mindful-body oriented therapies, mindful meditation techniques) without standard cross-study protocols. We partially addressed this limitation by separating efficacy findings into structured interventions compared to all MBIs, and while no difference in PTSD outcomes were observed, there are other practical limitations to be considered. Increased standardization (e.g., published manuals with clear guidelines for mindfulness activities such as asanas or postures, number of sessions, weeks practiced, total intervention hours) could help future research with consistency and guide evidence-based implementation.

Adverse effects were rarely investigated in the included SRs (15.6% of SRs that included 3 or more MBIs for treating PTSD). While these six reviews reported no or mild adverse effects, Goldberg et al. (2020) found that MBI participants were 98% more likely to drop out relative to participants from active controls. However, the reasons participants drop out (e.g., distress triggered from MBI activities) are unknown. Future clinical trials should investigate adverse effects of MBIs and their implications on treatment of trauma-exposed populations.

Future research on MBIs for PTSD should include more long-term follow-up data, active control conditions, and integration with other treatments. Although almost one half of unique studies included secondary follow-up assessments, only one meta-analysis appraised as either rigorous or likely rigorous assessed long-term MBI effectiveness and did not find support for sustained PTSD improvements after 3-month follow-up (Goldberg et al., 2020). This suggests modifications to existing MBIs that help sustain improvements in symptoms over time could be an area of fruitful research. Similarly, a little over 10% of unique articles used PTSD-specific, evidence-based treatments as control groups. Additional active control groups are needed not only for direct comparison of potential and established PTSD interventions, but also to explore the potential benefit of MBIs to address non-response to first-line treatments. For example, it would be beneficial to explore if MBIs could be effective for patients not responding to CPT or PE (Steenkamp et al., 2015) or if there are additive effects for engaging in MBIs for those with PTSD who are responding to first-line treatments. Finally, to expand and broaden the MBI scope to clinical settings, other factors, such as cost, burden, and accessibility of MBIs need to be investigated and compared to other treatments (Bower, 2016).

4.5. Limitations

We categorized rigorous and non-rigorous systematic reviews, detected common weaknesses, and scored SRs accordingly to examine the efficacy of MBIs for PTSD. However, our study is not without limitations. We note several caveats of AMSTAR2 use and of our SR in general. AMSTAR2 focuses solely on the methodological quality of SRs, but not the studies included in them. To address this limitation, we sought to extract key data from included reviews and include in our synthesis. While the stringent criteria of AMSTAR2 allows for an identification of the highest quality SRs, its conservative guidelines may be highly sensitive to poor reporting (de Santis et al., 2021), thus some SRs may have been categorized consistent with their reporting, rather than the actual methodology they employed. Relatedly, AMSTAR2 evaluates the rigor of the SRs themselves, rather than the quality of articles included. Thus, methodologically rigorous SRs could include many unique articles with high RoB. However, SRs that meet the most stringent criteria for AMSTAR2 would likely have evaluated those weaknesses in the primary articles (e.g., through RoB evaluation). While our title and abstract screening, full text review, and extraction process for quality appraisal were conducted in triplicate, the extraction process and the analyses for the summary of the results were predominantly completed by the first author [BJ]. Similarly, we did not compute inter-rater reliability parameters during the SR quality assessment.

Our research scope also has several limitations, providing suggestions for future research. First, we did not include compassion-based treatments, which are both related to MBIs and may be helpful for reducing PTSD symptoms (Westerman et al., 2020). Second, no analysis was conducted on working mechanisms of MBIs. Third, for greater standardization and consistency in our research question, we did not include brief or online interventions.

4.6. Conclusions

We present a broad overview of the existing literature on the efficacy of MBIs for PTSD, focusing on SRs that sought to synthesize that information. Research on MBIs for PTSD has blossomed over the last decade (Zhang et al., 2021); this umbrella review provides evidence that MBIs are efficacious for treating PTSD and may be a useful second-line treatment for trauma survivors. While a number of high-quality SRs have sought to summarize and evaluate the strength of those inferences, the vast majority of included SRs were low in quality, redundant with other more high-quality reviews, and thus provided little additional information to guide clinical and policy decisions. Improving the methodological rigor of these SRs can bolster their validity and the development of the MBI field – and the SR field - more generally. Individual studies of MBIs also varied in quality; future intervention studies should consider the importance of appropriate comparator groups, improved standardization of intervention protocols for wider and more consistent dissemination, more long-term MBI follow-ups, more adverse effects analyses, and application of MBIs across a wider range of trauma survivors and demographic groups. These recommendations could help expand knowledge and establish MBIs as efficacious low-cost, evidence-based interventions for PTSD, ameliorate some of the suffering associated with trauma exposure, and improve both individual and public health.

CRediT authorship contribution statement

Dana Rose Garfin: Writing – review & editing, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Data curation, Conceptualization. **Branislav Jovanovic:** Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

All data is included in Supplementary Materials. Any additional information will be shared upon request.

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Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.janxdis.2024.102859.

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