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Review article

Efficacy and acceptability of mindfulness-based interventions for military veterans: A systematic review and meta-analysis

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ABSTRACT

Background: Military veterans report high rates of psychiatric and physical health symptoms that may be amenable to mindfulness-based interventions (MBIs). Inconsistent prior findings and questions of fit between MBIs and military culture highlight the need for a systematic evaluation of this literature.
Objective: To quantify the efficacy and acceptability of MBIs for military veterans.
Data sources: We searched five databases (MEDLINE/PubMed, CINAHL, Scopus, Web of Science, PsycINFO) from inception to October 16th, 2019.
Study selection: Randomized controlled trials (RCTs) testing MBIs in military veterans.
Results: Twenty studies (k = 16 unique comparisons, N = 898) were included. At post-treatment, MBIs were superior to non-specific controls (e.g., waitlist, attentional placebos) on measures of posttraumatic stress disorder (PTSD), depression, general psychological symptoms (i.e., aggregated across symptom domains), quality of life / functioning, and mindfulness (Hedges' gs = 0.32 to 0.80), but not physical health. At follow-up (mean length = 3.19 months), MBIs continued to outperform non-specific controls on general psychological symptoms, but not PTSD. MBIs were superior to specific active controls (i.e., other therapies) at post-treatment on measures of PTSD and general psychological symptoms (gs = 0.19 to 0.25). Participants randomized to MBIs showed higher rates of attrition than those randomized to control interventions (odds ratio = 1.98). Several models were not robust to tests of publication bias. Study quality and risk of bias assessment indicated several areas of concern.
Conclusions: MBIs may improve psychological symptoms and quality of life / functioning in veterans. Questionable acceptability and few high-quality studies support the need for rigorous RCTs, potentially adapted to veterans.

Over the past two decades, accumulating evidence has demonstrated links between military service and health. Veterans, particularly those deployed to combat theaters, frequently show rates of psychiatric conditions including post-traumatic stress disorder (PTSD), depression, anxiety, and substance use above the civilian population and are more likely than civilians to die by suicide [1–10]. Veterans also have high rates of some physical health conditions, including chronic pain [11,12]. As in the general population, psychiatric and physical health conditions commonly co-occur among veterans [13–15]. The prevalence and comorbidity of psychiatric and physical health conditions among veterans has motivated the Veterans Health Administration (VHA) and other organizations that serve veterans to disseminate evidence-based treatments that target specific conditions [16], with a particular emphasis on mental health treatments (e.g., prolonged exposure and cognitive processing therapy for PTSD) [17,18]. Although those who complete evidence-based treatments may benefit, the impact of the available treatments on veteran health at the population level may be limited due to low rates of utilization and high rates of dropout [19]. While pharmacological treatment approaches are commonly used (e.g., antidepressants, benzodiazepines, opioids) [12,20,21], there is documented interest among veterans in non-pharmacological approaches to address common psychiatric and physical health concerns (e.g., chronic pain) [22].

Mindfulness-based interventions (MBIs) are a non-pharmacological treatment approach that has been used to address many of the psychiatric and physical health conditions experienced by veterans [23]. Standardized MBIs such as mindfulness-based stress reduction (MBSR) [24] and mindfulness-based cognitive therapy (MBCT) [25] emphasize training in mindfulness meditation techniques and have been used to treat specific health conditions including recurrent depression [26] and

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chronic pain [27]. Randomized controlled trials (RCTs) testing MBIs have shown promising effects on depression, anxiety, substance use, and chronic pain, typically outperforming waitlist controls and performing on par or better than other active therapies and evidence-based treatments [28–31]. MBIs have also shown promising effects on PTSD [32], although recent conceptual work has highlighted the need for trauma-sensitive mindfulness training [33].

Despite promising results in the general population, fewer RCTs have examined MBIs among veterans and the available studies have yielded mixed findings. For example, Polusny et al. [34] compared MBSR with an evidence-based treatment for PTSD (present-centered therapy) [35], with MBSR producing larger reductions in clinician-rated PTSD symptoms at two-month follow-up. In contrast, Kearney et al. [36] found no reliable differences in PTSD symptoms at four-month follow-up between combined MBSR and treatment-as-usual (TAU) with TAU alone. These discrepancies in the literature make it difficult for those serving veterans (e.g., VHA leadership and health care providers) to determine when, if ever, MBIs should be recommended.

In addition to mixed efficacy findings, there are also questions regarding the degree to which MBIs may be acceptable to military veterans. Certain aspects of military culture (e.g., emphasis on "toughness," self-reliance, and other traditional male gender norms such as avoiding expression of vulnerable emotions) [37–39] may, in theory, conflict with the attitudinal stance and group norms commonly adopted in MBIs (e.g., acceptance, non-striving, vulnerability, self-disclosure) [24]. Thus, in addition to evaluating efficacy, it would be valuable to examine the degree to which veterans find MBIs acceptable. Treatment acceptability is a multifaceted construct [40]. Treatment dropout is one objective indicator of acceptability that has been linked to poorer outcomes in psychotherapy [41,42]. Several systematic reviews and meta-analyses have examined attrition in MBI studies, reporting rates ranging from 15.5% to 29% [43–46], which is similar to rates found in psychotherapy generally and cognitive behavioral therapy specifically [47,48]. However, to our knowledge, no meta-analysis has quantified rates of attrition between MBIs and control conditions using data drawn from the same randomized controlled trial (i.e., likelihood of attrition from MBI versus alternative intervention arm), although such an analysis would provide a valuable indicator of acceptability.

Given mixed findings from RCTs and uncertain cultural fit, we conducted a meta-analysis to clarify the efficacy and acceptability of MBIs for military veterans. As MBIs have been applied to various psychiatric and physical health conditions common among veterans, we examined efficacy across a range of mental and physical health symptoms as well as non-symptom outcomes (e.g., quality of life, mindfulness). In addition, we assessed study characteristics that may account for discrepant findings (i.e., moderators). We restricted our analyses to RCTs and examined outcomes separately for comparisons with other therapies and with control conditions that were not intended to be therapeutic (e.g., waitlist, attentional placebo).

1. Method

1.1. Protocol and registration

Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines were followed [49]. This study was preregistered through the Open Science Framework (https://osf.io/e7w85/?view_only=ccda0960b784433f92d23caa9d9f4ea8). Four deviations were made from the protocol. First, we used meta-analysis to estimate the magnitude of differential attrition between MBIs and control conditions, rather than simply reporting attrition rates descriptively. We restricted this analysis to control conditions that involved receiving an active intervention. Second, we included a category of physical health symptoms. Third, we did not conduct an overall omnibus analysis with all outcomes included, given the heterogeneity in measure types. Fourth, we added a sensitivity analysis with outliers excluded.

1.2. Eligibility criteria

Eligible studies involved: [1] the delivery of an MBI [2] to military veterans [3] in a randomized controlled trial (RCT). To qualify as an MBI, an intervention had to include mindfulness meditation as a central treatment component and place an emphasis on home meditation practice [50]. Consistent with prior meta-analyses focused on MBIs [28,44], interventions that emphasized the attitudinal component only (e.g., Acceptance and Commitment Therapy) [51] or informal mindfulness practice (e.g., Dialectical Behavior Therapy) [52] were excluded. Interventions that involved non-mindfulness mind-body practices (e.g., mantram repetition, yoga) [53] were excluded. Samples focused on active duty military or veterans' family members were excluded to allow generalization specifically to veterans. No restrictions were placed on type of control condition (e.g., waitlist or active controls were both eligible), publication status (e.g., dissertations were eligible), or language.

1.3. Information sources

We searched five databases: MEDLINE/PubMed, CINAHL, PsycINFO, Web of Science, and Scopus. Databases were searched from inception to October 16th, 2019. In addition, recent reviews were hand searched [28,32,54].

1.4. Search

The following search terms were used for all five databases: mindful* AND (veteran* OR military) (see Supplemental Materials Table 1).

1.5. Study selections

Two authors independently reviewed each title and/or abstract based on inclusion/exclusion criteria. For studies that passed initial screening, full texts were reviewed. Coding disagreements were discussed with the first author until reaching consensus. Inter-rater reliability was high ($K = 0.76$) [55].

1.6. Data collection process

Standardized spreadsheets were created for coding study- and effect size-level data. Data were independently extracted by the first and second authors.

1.7. Data items

Data necessary for computing effect sizes (e.g., sample sizes, means, standard deviations) were extracted. We also extracted study inclusion criteria; sample age, gender composition, and percentage racial/ethnic minority; country of origin; type and length of MBI in weeks; type of control condition; post-treatment and follow-up timing; and intention-to-treat (ITT) and completer sample sizes.

Control conditions were coded on a two-tier system based on whether or not they were intended to be therapeutic [56,57]. Non-specific controls included no treatment conditions (i.e., waitlist), treatment-as-usual (TAU) conditions in which both the MBI and non-MBI arm received the TAU (e.g., [36]), and conditions which controlled only for non-specific factors and which lacked purported active ingredients (e.g., support group) [58]. Specific active controls were interventions that included specific treatment ingredients and specific mechanisms of change (i.e., cognitive behavioral therapy).

Outcomes were categorized into the following: PTSD, depression,

Table 1
Study-level characteristics.

Study	Inclusion	N_{mind}	N_{cont}	Age	Female	REM	Country	$Name_{mind}$	$Type_{mind}$	Weeks	$Name_{cont}$	$Type_{cont}$	FU
Arch 2013 [108]	anxiety	45	60	45.91	17	30	US	Modified MBSR	MBSR	10	CBT	specific	3
Arefnasab et al. (2013,2014) [109,114]	pulmonary injury	20	20	49.4	0	0	Iran	MBSR	MBSR	8	waitlist	non-specific	NA
Bein 2014 [83]	PTSD, SUD	4	4	50.13	0	37.5	US	Mindfulness for PTSD / GAD	None	8	TAU	non-specific	NA
Bremner 2017 [91]	PTSD	17	9	34.47	0	41.18	US	MBSR	MBSR	9	PCT	specific	6
Davis 2015 [110]	schizophrenia/ schizoaffective dx	18	16	51.74	3	61.76	US	MIRRORS	MBSR	16	Intensive Support	non-specific	2
Jasbi 2018 [111]	PTSD	24	24	52.97	0	0	Iran	MBCT	MBCT	8	Socio-therapeutic activities	non-specific	NA
Kearney 2013 [36]	PTSD	25	22	52	21.28	31.91	US	MBSR	MBSR	8	TAU	non-specific	4
Kearney 2016 [67]	Gulf War illness	26	29	49.88	14.55	38.18	US	MBSR	MBSR	8	TAU	non-specific	6
King 2016 [112]	PTSD	26	17	32.13	0	8.7	US	MB exposure therapy	MBCT	16	PCT	specific	NA
Mularski 2009 [58]	COPD	44	42	67.4	1.16	49	US	MB breathing therapy	MBSR	8	Support group	non-specific	NA
Niles et al. (2012,2013) [113,115]	PTSD	17	16	52	0	24	US	Mindfulness handbook	None	8	Psychoeducation	non-specific	1.5
Omidi et al. (2013,2018) [85,116]	PTSD, depression	31	31	41.11	0	0	Iran	MBSR/MBCT	MBSR/MBCT	8	TAU	non-specific	NA
Polusny 2015 [34]	PTSD	58	58	58.5	16	16	US	MBSR	MBSR	8	PCT	specific	2
Possemato 2016, Bergen-Cico et al., 2014 [84,117]	PTSD	36	26	46.4	12.9	17.7	US	Primary care brief mindfulness training	MBSR	4	TAU	non-specific	1
Wahbeh 2016a, Colgan et al., 2016 [87,118]	PTSD	28	28	51.1	6	12	US	Mindful breathing	MBSR	6	Biofeedback	specific	NA
Wahbeh 2016b, Colgan et al., 2016 [87,118]	PTSD	30	28	53.16	5.56	15.48	US	Mindful body scan	MBSR	6	Sitting quietly	non-specific	NA

Note: $N_{mind/cont}$ = intention-to-treat sample size for mindfulness and control conditions; Female = percentage female; REM = percentage racial/ethnic minority; Country = country of origin; $Name_{mind}$ = name of mindfulness condition; $Type_{mind}$ = standardized mindfulness-based intervention upon which mindfulness condition is based; $Weeks_{mind}$ = length of mindfulness intervention in weeks; $Name_{cont}$ = name of control condition; $Type_{cont}$ = control condition type; FU = length of follow-up in months; PTSD = posttraumatic stress disorder; COPD = chronic obstructive pulmonary disease; SUD = substance use disorder; US = United States; MBSR = mindfulness-based stress reduction; MBCT = mindfulness-based cognitive therapy; MB = mindfulness-based; GAD = generalized anxiety disorder; CBT = cognitive behavioral therapy; TAU = treatment-as-usual; PCT = present-centered therapy; non-specific = non-specific control condition not intended to be therapeutic; specific = specific active control condition.

anxiety, substance use, psychological symptoms, cognitive, mindfulness, quality of life / functioning, biological, and physical health outcomes. Measures of specific psychiatric symptoms (e.g., PTSD) were included in both the specific category (e.g., PTSD symptoms) as well as the more general psychological symptoms category. In other words, all measures of psychological symptoms contributed effect sizes to the broader psychological symptoms category. For studies that included multiple measures of psychological symptoms (e.g., PTSD and depression) [34], effect sizes were aggregated first within study as described below.

Data items were extracted for coding study quality based on modified Jadad [59](1996) criteria that have been used to evaluate MBIs previously [60,61]. A four-item study quality score was computed based on [1] whether a trial was randomized, [2] whether randomization was described and appropriate, [3] whether outcome assessment was blinded, and [4] whether reasons for withdrawal and dropouts were provided. Items coded as "yes" received a 1 and those coded as "no" or "unclear" received a 0, yielding a maximum total score of 4. Five additional aspects (e.g., use of ITT analysis) were coded but did not contribute to the total score (see Supplemental Materials Table 2).

1.8. Risk of bias of individual studies

We evaluated risk of bias of individual studies using the Cochrane tool [62]. We assessed bias in the domains of selection (random sequence generation, allocation concealment), performance (blinding of participants and personnel), detection (blinding of outcome assessors), attrition (incomplete outcome data), and reporting (selective reporting). Each study was assessed as low, high, or unclear risk of bias in each domain.

1.9. Summary measures

We calculated standardized effect sizes using recommended meta-analytic methods [63]. First, we computed within-group pre-post and pre-follow-up Cohen's [64] *d*s for the MBI and control conditions separately. For this computation, we assumed a correlation of $r_{xx} = 0.50$ between timepoints (lower than a typical test-retest correlation to account for potential intervention effects) [65]. Then, we computed a between-group effect by subtracting the within-group effect for the control conditions from that of the MBI conditions (i.e., Becker's del) [66]. In contrast to between-group effects based on post-treatment data alone, this effect size accounts for potential between-group differences at baseline. For outcomes that lacked baseline data (e.g., changes in diagnostic status) [34], post-treatment data were used [63].

To estimate differential attrition, we computed odds ratios representing the likelihood of dropout from the MBI conditions relative to the control group [63]. We calculated differential attrition only for studies in which the control group received an intervention, as some control conditions did not include an intervention from which one could dropout (e.g., treatment-as-usual) [67]. In this analysis we collapsed across control interventions that included specific ingredients (i.e., specific active controls) and those not intended to be therapeutic (i.e., non-specific controls).

1.10. Synthesis of results

In keeping with recommended methods [63], effects were first aggregated within measure (e.g., subscales of the PTSD Checklist) [68] and then within study using the 'MAd' package [69] in R [70]. In keeping with Fu et al.'s [71] recommendation, meta-analytic estimates were calculated when at least four studies were available for a particular outcome domain and control condition type (i.e., non-specific, specific active). Effects were converted from Cohen's *d* to Hedges' *g* to account for small sample bias [63]. When necessary, the sign for each effect was reversed so that a positive effect size always indicated

Table 2
Meta-analytic results across outcome domains.

Domain	Comparison	Timepoint	N	K	ES	I^2	k_{imp}	ES_{adj}	FSN
PTSD	non-specific	post	298	7	0.64 [0.16, 1.12]	76.93 [40.49, 95.67]	0	0.64 [0.16, 1.12]	61
PTSD	non-specific	fu	187	4	0.12 [−0.17, 0.41]	0.00 [0.00, 88.25]	0	0.12 [−0.17, 0.41]	0
PTSD	specific	post	206	4	0.25 [0.01, 0.50]	0.00 [0.00, 68.25]	2	0.22 [−0.01, 0.45]	3[a]
Depression	non-specific	post	333	7	0.80 [0.42, 1.19]	62.53 [0.00, 95.25]	0	0.80 [0.42, 1.19]	111
Psych Sx	non-specific	post	449	10	0.70 [0.38, 1.02]	72.68 [38.95, 92.75]	0	0.70 [0.38, 1.02]	222
Psych Sx	non-specific	fu	187	4	0.31 [0.04, 0.57]	10.89 [0.00, 92.30]	0	0.31 [0.04, 0.57]	5[a]
Psych Sx	specific	post	311	5	0.19 [0.00, 0.38]	0.00 [0.00, 62.30]	2	0.17 [−0.01, 0.35]	4
QOL/Function	non-specific	post	240	5	0.72 [0.47, 0.97]	0.00 [0.00, 82.25]	0	0.72 [0.47, 0.97]	57
Mindfulness	non-specific	post	281	7	0.32 [0.11, 0.54]	0.00 [0.00, 90.01]	1	0.30 [0.09, 0.51]	22[a]
Phys Health	non-specific	post	196	4	0.38 [−0.19, 0.95]	80.22 [34.10, 98.76]	0	0.38 [−0.19, 0.95]	6

Note: N = sample size, K = number of comparisons, ES = effect size in Hedges' *g* units; I^2 = heterogeneity; k_{imp} = number of imputed studies necessary for funnel plot symmetry; ES_{adj} = trim-and-fill adjusted effect size; FSN = fail-safe N; [a] = statistically significant effect that is not robust to FSN based on Rosenberg's (2005) guidelines; PTSD = posttraumatic stress disorder; Psych Sx = psychological symptoms; QOL/Function = quality of life or measures of functioning; Phys Health = physical health outcomes; non-specific = non-specific control conditions not intended to be therapeutic; specific = specific active control conditions; post = pre-post effect; fu = pre-follow-up effect. Values in brackets represent 95% confidence interval.

improvement (e.g., decreased PTSD symptoms, increased mindfulness). Separate estimates were computed for post-treatment and follow-up timepoints. Heterogeneity was characterized using I^2 (i.e., proportion of heterogeneity that is between-study heterogeneity; Higgins et al., 2003) [72]. Random effects models were used with weighting based on the inverse variance of each study's effect size through the 'metafor' package [73]. For attrition, we used Peto's method [74] recommended in the Cochrane Handbook [62] implemented in the 'metafor' package which conducts a fixed effects meta-analysis.

1.11. Risk of bias across studies

The potential impact of publication bias was assessed using trim-and-fill analyses and estimates of fail-safe N (FSN) in the 'metafor' package. Trim-and-fill analyses assessed funnel plot asymmetry to determine whether expected studies may be missing from the available literature (e.g., small studies with non-significant results). As trim-and-fill analyses can be underpowered, these tests were considered exploratory. FSN was calculated to estimate the number of non-significant results that would need to exist in order to nullify the observed effect [75]. FSN was interpreted based Rosenberg's [76] recommendation (i.e., FSN is robust if $> 5*n + 10$, where n is the number of available studies).

1.12. Additional analyses

We tested four study-level moderators, although, like trim-and-fill analyses, these were considered exploratory based on potentially low statistical power [77]. We selected moderators theoretically or previously linked to MBI efficacy [78–80]. These included study quality (based on modified Jadad criteria), PTSD inclusion criterion, gender (percentage female), and MBI treatment length in weeks.

Sensitivity analyses were conducted with outliers excluded. Several methods for identifying outliers in meta-analysis exist [81], and we used the 'find.outliers' function [82] which identifies outliers based on whether a study's confidence interval overlaps the omnibus effect confidence interval.

2. Results

2.1. Study selection

Our search produced 1484 citations. We removed 698 duplicates and evaluated 786 titles and/or abstracts for inclusion. After applying our inclusion/exclusion criteria (Fig. 1), 20 studies were retained representing 16 unique comparisons and 898 participants (see Supplemental Materials Table 3 for a list of the included studies).

2.2. Study characteristics

Study-level characteristics are reported in Table 1. All studies had a psychiatric or physical health-related inclusion criteria. The majority (68.8%) required a diagnosis of PTSD (or elevated PTSD symptoms) [83,84]. The remainder required a specific physical health condition (pulmonary injury, chronic obstructive pulmonary disease, Gulf War illness) or other psychiatric condition (anxiety, substance use disorder, schizophrenia or schizoaffective disorder). Two studies required PTSD symptoms along with either depression [85] or substance use disorder symptoms [83]. Participants were on average 49.27 years old (*SD* = 8.43), 6.1% female (*SD* = 7.57), and 24.0% racial/ethnic minorities (*SD* = 18.46). Most studies were conducted in the United States (81.3%), with the remainder occurring in Iran (18.8%).

MBIs were most commonly based on MBSR (68.8%), with 12.5% based on MBCT, 6.3% based on a combination of MBSR and MBCT, and 12.5% not explicitly based on MBSR or MBCT. MBIs lasted an average of 8.69 weeks (*SD* = 3.16, range = 4 to 16). The majority of the comparisons (68.8%) involved non-specific controls with the remainder (31.3%) involving specific active controls. Non-specific controls included no treatment, TAU, and attentional placebo control conditions that lacked active ingredients and were not intended to be therapeutic (e.g., support group, psychoeducation). Specific active controls included treatments found on the American Psychological Association's Division 12 list of evidence-based treatment (cognitive behavioral therapy for anxiety, present-centered therapy for PTSD) [86] or biofeedback (RESPeRATE) [87]. Half of the comparisons included a follow-up assessment which on average occurred 3.19 months post-treatment (*SD* = 1.96, range = 1 to 6). Average ITT sample size was 56.12 (*SD* = 30.54, range = 8 to 124). Average treatment completion rate was 76.4% (*SD* = 21.24) across all MBI conditions. For studies with control conditions that included an intervention (e.g., TAU controls were excluded), average MBI treatment completion was 69.3% (*SD* = 20.47) and control treatment completion was 80.5% (*SD* = 16.75).

Average Jadad study quality was 2.56 out of 4 (*SD* = 1.09). Only three studies received a 4 (Supplemental Materials Table 2). The area with the lowest average score was blind outcome assessment (mean = 0.44, *SD* = 0.51). Several studies did not report reasons for dropout (mean = 0.50, *SD* = 0.52).

2.3. Risk of bias within studies

Risk of bias varied across domains (Fig. 2; Supplemental Materials Table 4). Selective reporting bias was often high due to lack of pre-registration (e.g., through clinicaltrials.gov) or failing to clearly identify a pre-specified primary outcome. All studies were coded as high risk of bias for blinding of personnel and participants due to the nature of the

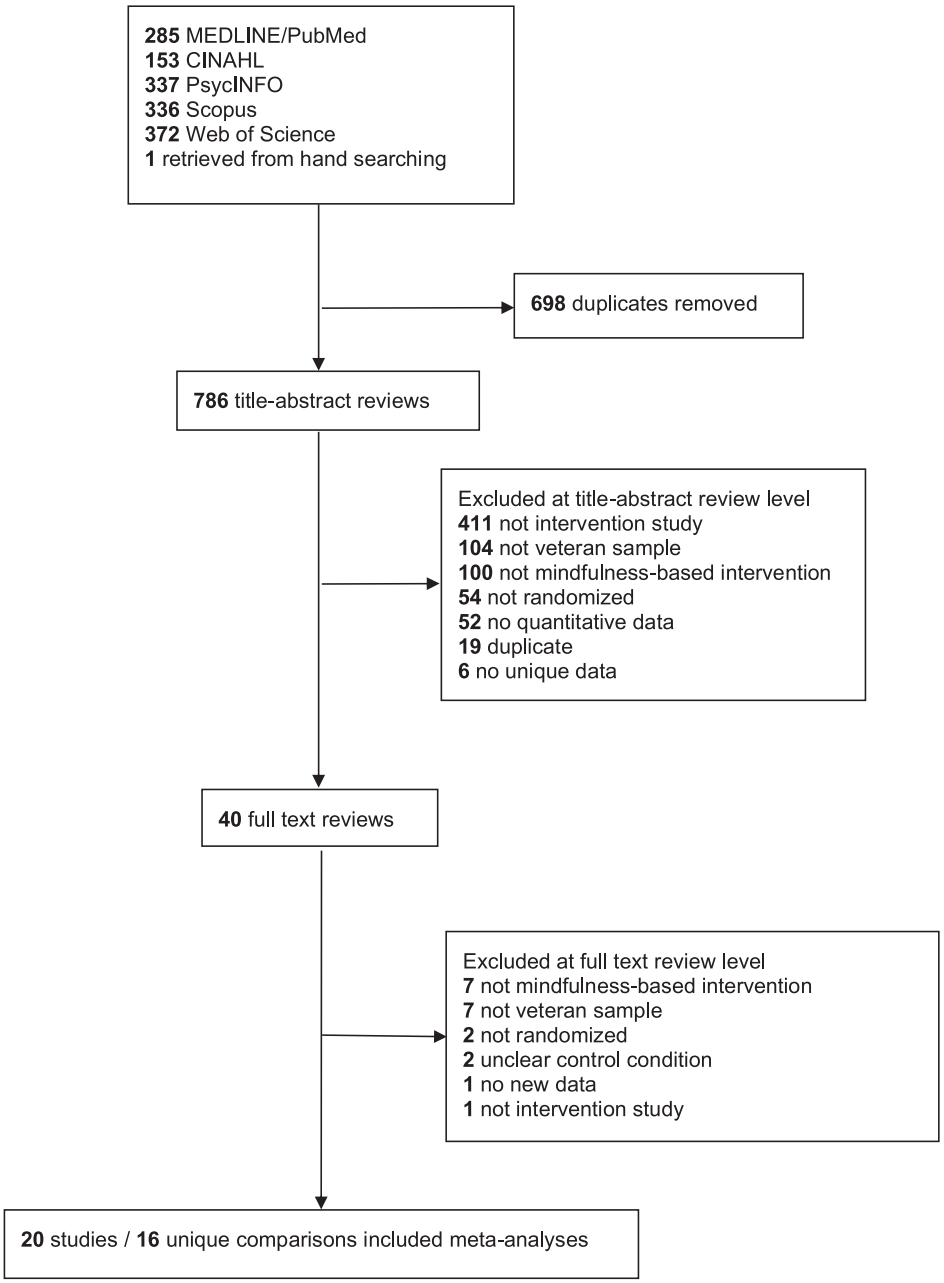

Fig. 1. PRISMA flow diagram.

intervention and lack of control conditions that obscured this fact (e.g., sham meditation) [88]. Lack of blinding of outcome assessor and attrition bias were also common potential sources of bias.

2.4. Results of individual studies

All study-level effect size aggregates are reported in Supplemental Materials Table 5, separated by outcome domain and timepoint. A list of outcome measures associated with each domain is provided in Supplemental Materials Table 6.

2.5. Synthesis of results

2.5.1. Efficacy

Meta-analytic estimates separated by comparison type (non-specific controls, specific active controls), timepoint (pre-post, pre-follow-up), and domains are reported in Table 2 and displayed in Fig. 3.

2.5.2. Non-specific controls

At post-treatment, MBIs compared favorably with non-specific controls in the domains of PTSD (g = 0.64), depression (g = 0.80), psychological symptoms (g = 0.70), quality of life / functioning (g = 0.72), and mindfulness (g = 0.32). MBIs did not differ from non-specific controls at post-treatment on measures of physical health (g = 0.38, [−0.19, 0.95]). Heterogeneity for post-treatment comparisons was generally high and effect size estimates with low heterogeneity had wide confidence intervals.

At follow-up, MBIs continued to show superiority to non-specific controls on measures of psychological symptoms (g = 0.31) but no longer differed from non-specific controls for PTSD (g = 0.12). Heterogeneity was low, but again with wide confidence intervals.

2.5.3. Specific active controls

At post-treatment, MBIs compared favorably with specific active controls on measures of PTSD (g = 0.25) and psychological symptoms

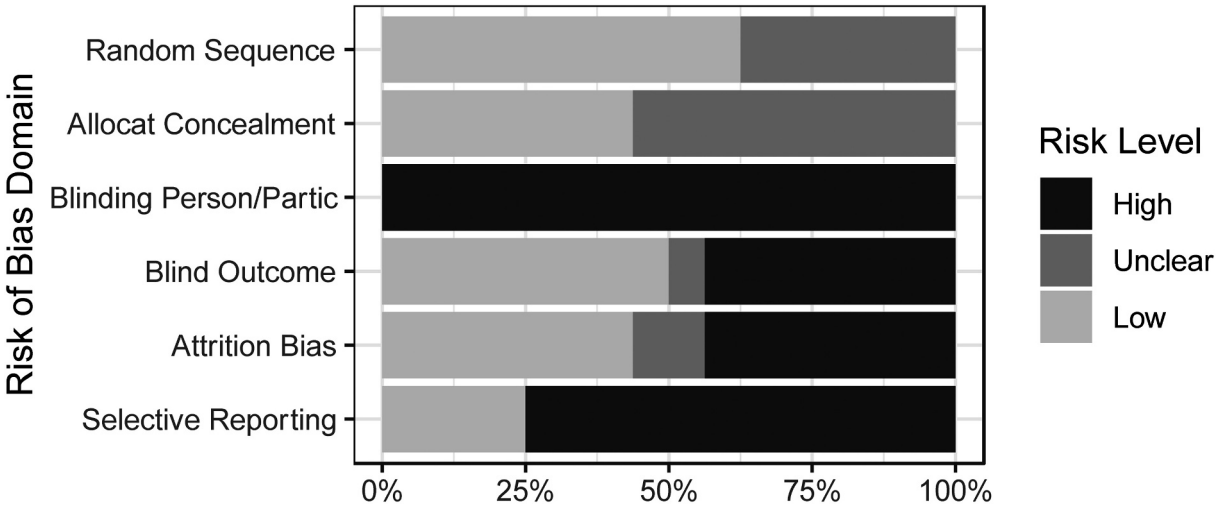

Fig. 2. Cochrane risk of bias coding. Random sequence = random sequence generation; Allocat Concealment = allocation concealment; Blinding Person/Partic = blinding of personnel and participants; Blind Outcome = blinding of outcome assessor.

(g = 0.19). Heterogeneity for these comparisons was low. Insufficient studies were available for estimating effects at follow-up.

2.5.4. Attrition

Estimates of differential attrition were based on nine studies that included a control group that received an intervention (see Supplemental Materials Table 7). Participants randomized to the MBI condition were significantly more likely to drop out than those randomized to the control group (log OR = 0.68, [0.27, 1.09]; Fig. 4). Converting to odds ratio, this indicates that MBI participants were 98% more likely to drop out relative to participants in active control conditions (OR = 1.98).

2.6. Risk of bias across studies

Asymmetric funnel plots were detected for three models (Table 2). In two cases, trim-and-fill adjusted effect sizes no longer differed from zero (the two post-treatment comparisons with specific active controls). Fail-safe Ns ranged from 0 to 222. Based on Rosenberg's [76](2005) guidelines, three originally significant effects were not robust to

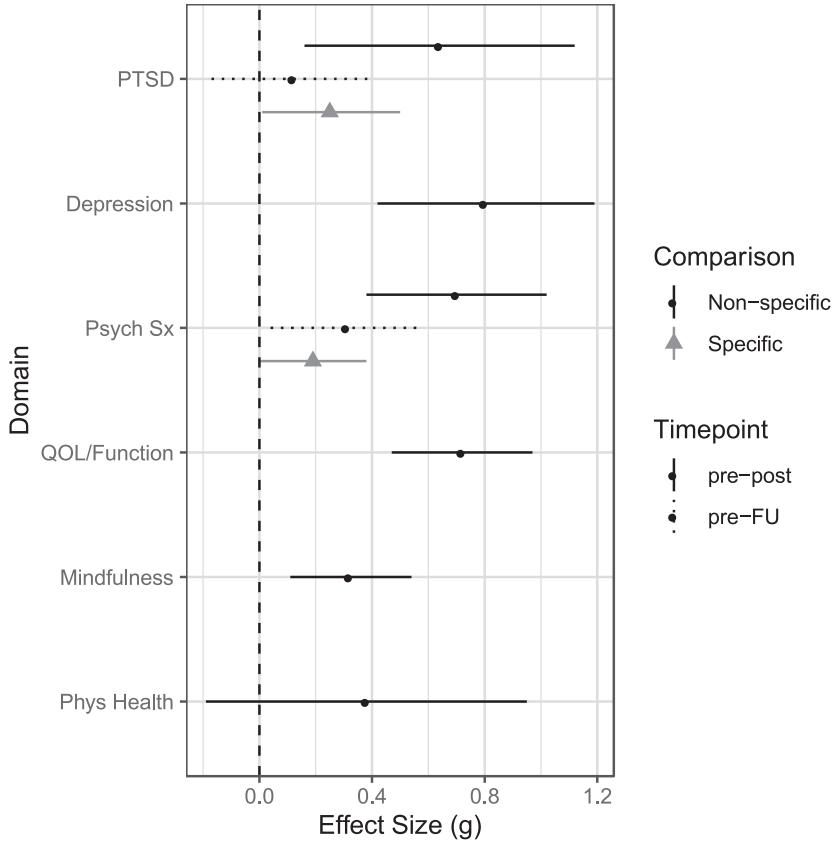

Fig. 3. Forest plot displaying meta-analytic estimates in Hedges' g units when four or more studies were available for a given comparison. Error bars represent 95% confidence intervals.

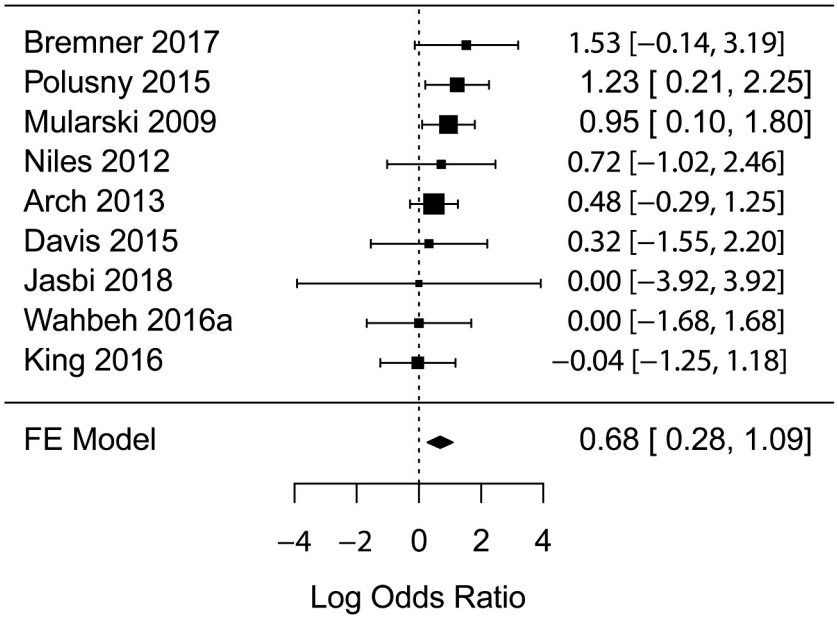

Fig. 4. Forest plot depicting differential attrition for mindfulness-based intervention conditions relative to control intervention conditions. Effect sizes are displayed in log odds ratio units, with larger effect sizes indicating higher attrition in the mindfulness arm.

publication bias (the two comparisons with specific active controls at post-treatment, comparison with non-specific active controls at post-treatment on mindfulness).

2.7. Additional analyses

Although four study characteristics were tested as moderators (study quality, PTSD inclusion criterion, gender, MBI treatment length), no significant moderator effects were detected (Supplemental Materials Table 8). Models with outliers removed yielded results similar to the primary models, but with reduced effect in three models (change in *g*s = 0.18 to 0.27). Statistical significance tests did not change as a result of removing outliers (Supplemental Materials Table 9).

3. Discussion

Despite growing interest in the potential application of MBIs for the treatment of psychiatric and physical health conditions among veterans, no meta-analysis has examined the efficacy of this approach. This review, based on 16 comparisons and 898 participants, provides some support for this treatment approach within veteran populations, while also highlighting important limitations of the available literature. At post-treatment, promising effects of MBIs were seen relative to non-specific controls (waitlist, attentional placebo) on measures of psychological symptoms and on quality of life / functioning. At follow-up, however, sustained effects were only seen on psychological symptoms with a small effect size. Nonetheless, the magnitude of the observed effects relative to non-specific controls are similar to those found in the broader MBI literature (e.g., *g*s = 0.55 to 0.59 for effects on PTSD, psychological symptoms, and quality of life versus waitlist controls) [28,32,89]. MBIs compared favorably to specific active controls at post-treatment on measures of PTSD ($g = 0.25$) and psychological symptoms ($g = 0.19$). While small in magnitude, these effects also mirror prior reviews of the MBI literature which have found that MBIs yield larger reductions in psychological symptoms than specific active controls (e.g., $d = 0.26$, $g = 0.23$) [28,78]. However, comparisons with specific active controls were not robust to tests of publication bias and a lack of follow-up assessment weakens the strength of this evidence. In order to clarify whether MBIs should be recommended for veterans, it is crucial that future RCTs compare MBIs with available therapies and assess outcomes at follow-up.

To our knowledge, no previous meta-analysis has quantified the acceptability of MBIs relative to other interventions. Dropout is an important objective metric of acceptability and high rates of attrition from psychotherapy have been reported for veterans [19] and from RCTs of PTSD treatments generally [90]. Results indicated that participants assigned to MBI conditions were 98% more likely to dropout than those assigned to a control intervention. This suggests that MBIs may be perceived as less acceptable than attentional placebos or alternative treatments. Examination of the rates of differential attrition across studies indicated rates were highest in two studies comparing MBIs to present-centered therapy for PTSD [34,91]. We were underpowered to properly test whether this particular comparison condition and/or a PTSD inclusion criterion was associated with higher retention. However, this pattern is consistent with a previous meta-analysis indicating that present-centered therapy (a “trauma-avoidant” treatment that proscribes trauma-related discussion) showed lower dropout than trauma-focused treatments for PTSD [90]. While MBIs are not explicitly trauma-focused, it has long been theorized that exposure and desensitization may be one the underlying mechanisms [92,93]. Recent theoretical and qualitative research has also highlighted the need for trauma sensitivity in meditation training [33,94]. A lack of trauma sensitivity may contribute to higher attrition in MBIs.

Regardless of the specific cause, higher attrition within MBIs treatment arms raises questions regarding the degree to which veterans find these treatment approaches acceptable. Prior work among veterans has highlighted difficulties in MBI treatment initiation and difficulties understanding and engaging with mindfulness practices [95,96], while other work indicates high self-reported interest in mindfulness meditation [97]. Future research should continue to explicitly examine MBI acceptability among veterans and consider the possibility of adaptations for this population that are culturally relevant and diagnostically appropriate (as has been done to beneficial effect for racial/ethnic minorities) [98,99].

This study has important limitations, several of which are related to the meta-analytic sample itself. The relatively small number of comparisons reduced statistical power for testing efficacy. The relatively small number of participants per study puts effect size estimates at risk for small sample bias [100]. There were insufficient studies to estimate effects in some important outcome domains (e.g., anxiety, substance

use). Our choice of requiring four studies per estimate [71], while increasing confidence in the reported effects, reduced the number of domains covered. We chose to combine passive controls and active controls that were not intended to be therapeutic (i.e., attentional placebos) into a single category (non-specific controls), anticipating insufficient studies to examine these separately and not wanting to combine attentional placebos with actual therapies. While justified on theoretical grounds [56,57], this may have produced an overly conservative estimate of MBIs' efficacy relative to passive controls. The number of studies likely limited our ability to properly test moderators. The very low number of female participants reduces generalizability, which is a particularly important limitation as gender diversity in the military grows [101].

Additionally, several aspects of our results make conclusions tenuous, including indications of publication bias, high or unclear risk of bias in several domains, and moderate to high heterogeneity within some analyses. This degree of heterogeneity suggests that meaningful differences between studies may exist, although we were unable to determine the cause of these differences. To reduce risk of bias, it is essential that future studies properly account for attrition, particularly given evidence of higher dropout within MBIs. Including non-self-report outcome measures and pre-specifying primary outcome measures (e.g., through Open Science Framework) [102] will also increase confidence in this literature. One important potential source of bias is researcher allegiance, which has been defined as a researcher's belief in the superiority of a particular treatment approach [103]. Research allegiance has been shown to predict treatment differences in psychotherapy generally [104] and MBIs specifically [105], but has rarely been explicitly discussed in the MBI literature. Unfortunately, we were unable to test effects associated with researcher allegiance in the current study due to the small number of studies with specific active controls. Future meta-analyses should assess the impact of this potentially important source of bias.

These limitations notwithstanding, the overall pattern of findings suggests that MBIs may be a promising treatment option for reducing psychological symptoms and increasing quality of life / functioning in veterans. Benefits of MBIs beyond other therapies, at follow-up, and on physical health outcomes are less clear. The possibility that MBIs may results in higher rates of attrition is an important limitation to address in future studies and could support the adaptation of MBIs for veterans specifically. Efforts to match veterans with their preferred treatment approach is a promising route for decreasing attrition [106]. Evidence that MBIs effectively reduce common psychiatric symptoms and chronic pain in the general population [28] coupled with promising effects of MBIs in several domains in the current meta-analysis supports future RCTs testing this approach for military veterans. As there are at least four million veterans from the recent wars in Afghanistan and Iraq and the VHA remains one of the largest healthcare providers in the world [107], clarifying the potential of MBIs for this population is warranted.

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Declaration of Competing Interest

The authors declare no conflicts of interest.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpsychores.2020.110232.

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