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Exposure therapy for PTSD in military populations: A systematic review and meta-analysis of randomized clinical trials

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ABSTRACT

Military populations are disproportionally affected by posttraumatic stress disorder (PTSD) and may experience less benefit from first line psychotherapies for PTSD relative to civilians. We examined the efficacy of exposure therapy among Veterans and active duty military personnel across various control conditions and tested potential treatment-related, demographic, and clinical moderators. Randomized controlled trials of exposure-based therapies for PTSD in military populations were identified from a recent meta-analysis and through PsycINFO and Medline. Nineteen studies met inclusion criteria and were included in the meta-analysis (total $N = 2905$). Exposure therapy had medium to large effects compared to waitlist and treatment as usual, a small effect compared to non-trauma-focused therapy, and no effect relative to other trauma-focused therapy. The overall effect was similar at post-treatment and follow up. The effect size for exposure was larger in studies with younger participants, more women, fewer participants with comorbid major depression, and fewer participants taking psychiatric medication. Effect sizes were not impacted by treatment length or type, participant race or ethnicity, comorbid substance use, Veteran versus active duty status, or study risk of bias. Findings document the variable efficacy of exposure therapy in military populations across comparator types and point to several potentially important moderators of outcome that should be examined in future research.

1. Introduction

The rate of posttraumatic stress disorder (PTSD[5]) in Veterans and active duty military personnel is significantly elevated relative to civilians, with estimates as high as 14–20 % (IOM, 2014; Tanielian et al., 2008). The personal and societal cost of untreated PTSD is considerable; PTSD is linked with numerous mental and physical health comorbidities (Kessler et al., 2005; Sareen, 2005) impairments in functioning (Asnaani et al., 2014), and elevated health care utilization (Harper et al., 2022).

Clinical practice guidelines for treating PTSD in military populations recommend trauma-focused psychotherapy (VA/DoD, 2017). Leading trauma-focused psychotherapies such as prolonged exposure (PE; Foa et al., 2019) were initially developed and tested with civilians (i.e., women sexual assault survivors). Studies testing these treatments in Veterans, and more recently, active duty military personnel, have generally found less robust treatment effects than have prior studies of civilians. Indeed, several meta-analyses have found lower effect sizes for studies of Veterans than of civilians (e.g., Bradley et al., 2005; McLean et al., 2021a; Straud et al., 2019; Watts et al., 2013, although see Kline et al., 2018 for an exception). Reasons for the attenuated effects in

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[5] CI, Confidence Interval; CPT, Cognitive Processing Therapy; EMDR, Eye Movement Desensitization and Reprocessing Therapy; FSN, Fail-Safe N; ITT, Intent-to-treat; NET, Narrative Exposure Therapy; PE, Prolonged Exposure; PTSD, Posttraumatic Stress Disorder; RCT, Randomized Clinical Trial; TAU, Treatment as Usual; VRET, Virtual Reality Exposure Therapy; WET, Written Exposure Therapy

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military populations are unclear, but researchers speculate that they may relate to the unique characteristics of war-related PTSD. These characteristics include the nature of the traumatic events experienced in war zones (Litz et al., 2019; Pietrzak et al., 20187, 2011), the frequency and intensity of war-related trauma, and high rates of prior lifetime trauma exposure (e.g., Reger et al., 2019; Stretch et al., 1998). In addition, systemic factors such as service connection disability status are hypothesized to interfere with PTSD treatment outcomes in military populations, although the evidence supporting this concern is mixed (e.g., Belsher et al., 2012; Goodson et al., 2017; Schnurr & Lunney, 2016).

Recent reviews by Steenkamp et al. (2015, 2020) have highlighted the modest effects of trauma-focused psychotherapy in military samples. This review found that PE appeared superior to wait list and treatment as usual (TAU) but was not reliably better than non-trauma focused psychotherapies. In recent studies of PE with active duty military personnel, reductions in PTSD severity for PE were also comparable to those of non-trauma-focused treatments, including present-centered therapy (Foa et al., 2018) pharmacotherapy (sertraline; Rauch et al., 2019), and transcendental meditation (Nidich et al., 2018).

The efficacy of eye movement desensitization and reprocessing therapy (EMDR; Shapiro, 2001) another exposure-based therapy, also appears attenuated in military samples relative to civilians. In a 2013 meta-analysis, researchers concluded that there was no evidence for the effectiveness of EMDR in Veterans at that time (Verstrael et al., 2013). Notably, most studies of EMDR in Veterans are limited by small sample sizes (Carlson et al., 1998; Devilly et al., 1998; Jensen, 1994; Rogers et al., 1999, but see Boudewyns & Hyer, 1996 for an exception) and there are currently no published randomized clinical trials (RCTs) of EMDR with active duty military personnel.

Very little research has examined other exposure therapies for PTSD in military samples. There are also no published RCTs of narrative exposure therapy (NET; Schauer et al., 2011) in either Veterans or active duty personnel, although one study found large effects favoring NET over TAU among adolescents and adults who were former combatants in the Democratic Republic of the Congo (Koebach et al., 2021). Ex-combatants in this context include child soldiers and individuals who may have been conscripted therefore may not be comparable to Veterans. One study recently tested written exposure therapy (WET; Sloan & Marx, 2019) among active duty military personnel (Sloan et al., 2022), finding that WET was non-inferior to cognitive processing therapy (CPT) as hypothesized, but WET showed less robust effects than in two prior trials comprised predominately of civilians (Sloan et al., 2012; Sloan et al., 2018).

In contrast to the relatively limited research on EMDR, NET, and WET in military populations, virtual reality exposure therapy (VRET) for PTSD has been tested almost exclusively in military populations. This research suggests that VRET is superior to waitlist (Miyahira et al., 2021; Reger et al., 2016) and TAU (McLay et al., 2011), but not superior to standard exposure conditions (McLay et al., 2017; Reger et al., 2016). VRET uses a computer-generated virtual environment designed to match the individual's specific trauma memory. It was hoped that the use of VRET to facilitate imaginal exposure might improve outcomes relative to standard exposure protocols by allowing patients to more fully engage with their trauma memory. Although this hypothesis has not been tested directly, among Veterans with PTSD, Reger et al. (2019) found no overall differences between VRET and standard exposure in emotional activation during treatment.

Data on which exposure-based therapies work best for military populations are limited. One meta-analysis of Veterans with PTSD by Haagen et al. (2015) found that PE and other exposure therapy protocols using prolonged imaginal exposure to traumatic memories were superior to EMDR, which involves brief exposure to trauma memory details. To our knowledge, no other meta-analysis has examined the efficacy of various trauma-focused psychotherapies among military populations.

The study by Haagen et al. (2015) points to other potentially important moderators of treatment outcome for Veterans with PTSD. Specifically, the number of trauma-focused sessions, but not the number of treatment sessions overall, was positively associated with clinical outcomes. This contrasts with our prior meta-analysis of exposure therapy for PTSD across populations (i.e., not military specific) showing that treatment length was negatively associated with outcomes (McLean et al., 2021a). However, in the McLean et al. study, longer treatments included more non-exposure sessions, so both studies may suggest that a focus on trauma-focused content is important for good outcomes.

Haagen et al. (2015) also found that samples with high and low pre-treatment PTSD severity fared worse that samples with moderate PTSD severity. Similarly, in a study combining data from multiple PTSD treatment trials in active duty military personnel, Litz et al. (2019) found that those with higher severity of PTSD and depressive symptoms at baseline showed less improvement. The authors interpreted PTSD and depressive symptom severity as a proxy for clinical complexity and suggested that more clinically complex service members may benefit more from alternate treatments. Litz et al. also found that older age was negatively linked to clinical outcomes, a finding that we replicated in our meta-analysis of exposure therapy (McLean et al., 2021a). Other research has linked marginalized racial and ethnic identities with worse clinical outcomes among Veterans with PTSD (Maguen et al., 2014; Sripada et al., 2017) though not for exposure therapy specifically. Finally, some research has found greater PTSD treatment change among women Veterans than men Veterans (e.g., Maguen et al., 2014; Stefanovics & Rosenheck, 2020), although findings have been mixed (e.g., Tiet et al., 2015). In sum, treatment length, clinical complexity, and sociodemographic variables have all been identified as potential moderators of treatment outcomes in military samples and warrant additional research.

2. Current study

The goal of the current study was to examine the efficacy of exposure-based psychotherapy in military populations. Most prior meta-analyses on PTSD treatment have not focused specifically on military samples (Lewis et al., 2020; McLean et al., 2021a) or have focused on only one intervention (i.e., Verstrael et al., 2013 meta-analysis of four trials of EMDR with Veterans), limiting the conclusions that can be drawn about what works best for this unique population. The one meta-analysis that did focus on PTSD treatments for Veterans (Haagen et al., 2015) incorporated studies through 2013. Given the number of military PTSD trials conducted since 2013, an update is clearly warranted. Based on prior research, we predicted that exposure therapy would be superior to wait list and TAU, and comparable to non-trauma-focused treatment and other trauma-focused treatment.

In addition to examining the efficacy of exposure-based psychotherapies in military populations, we tested several potential moderators of treatment outcome based on prior research, including length of treatment, clinical complexity (comorbid major depressive disorder, comorbid substance use disorder, psychiatric medication), age, gender, and race and ethnicity, predicting that greater clinical complexity, older age, a lower proportion of women participants, and a greater proportion of participants with marginalized racial or ethnic identities would be associated with lower effects. We did not have a hypothesis about length of treatment, given prior mixed findings on the impact of this variable, or about exposure therapy type, given the lack of research comparing exposure-therapies in military samples. Given that many of the studies with active duty military personnel are more recent, and that active duty personnel may differ from Veterans in several important ways (e.g., time since trauma, ongoing exposure to potentially traumatic events, age), we also explored whether sample type (Veteran vs. active duty) moderated clinical outcomes.

2.1. Methods

This meta-analysis was conducted following the Preferred Reporting

Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines (Page et al., 2020) and a review protocol was made for this study (CRD42022315384) that can be accessed at https://www.crd.york.ac.uk/prospero/display_record.php?RecordID=315384.

2.2. Inclusion and Exclusion Criteria

Inclusion criteria were: 1) study was reported in English; 2) participants were age 18 years or older; 3) randomized controlled trial design; 4) at least one exposure-based therapy condition; and 5) at least one non-exposure control group, defined as any intervention that consisted of < 50 % of sessions devoted to exposure therapy; and 6) the sample was comprised of Veterans, and/or active duty military personnel. Exclusion criteria were: 1) combined or integrated treatments for PTSD and comorbid problems (e.g., testing concurrent PTSD and substance use treatment); 2) self-directed or self-help treatment; and 3) less than 10 participants per condition.

2.3. Data sources and literature search

The current study drew relevant data collected as part of a meta-analysis on exposure therapy for PTSD (McLean et al., 2021a). This parent meta-analysis included studies through September 2021 (search was performed on October 7, 2021 with no specified start date). A separate literature search for the current study was conducted using the PsycINFO and Medline electronic databases on February 22, 2022 that restricted the search to studies published in 2021 or later. This search had the following search terms: "Posttraumatic Stress Disorder" AND "Random*" AND "Therapy or Treatment" AND "Exposure or Flooding or Implosive or EMDR" AND "Veteran* or Military or Active Duty or Service Member". In addition, we reviewed published reviews and meta-analyses to identify any eligible studies that met our eligibility criteria.

2.4. Study selection

Eligible studies from the parent meta-analysis (i.e., military samples) were identified. Two authors (CPM and HCL) independently reviewed abstracts from the updated literature search results to assess initial eligibility. Any discrepancies were resolved through discussion until consensus was reached. Full-text articles were obtained for any eligible abstracts and raters either coded the study or provided a reason for exclusion.

2.5. Data coding

We extracted the following variables from each eligible study: *Study-related variables* included exposure and control group sample sizes; PTSD outcome means and standard deviations at pre-treatment, post-treatment, and follow-up; and study year. *Sample-related variables* included the mean age of participants; the percentage of participants who were women; the percentage of participants who were racial or ethnic minorities, taking psychiatric medications, diagnosed with comorbid depression, and diagnosed with comorbid substance use disorder; and sample type (Veteran vs. active duty). *Treatment-related variables* included the total number of sessions in the exposure condition; the type of exposure treatment package (PE, VRET, EMDR, or "other" exposure therapy [the only study of written exposure therapy was included in this category]); and control treatment (waitlist, treatment-as-usual [TAU], non-trauma-focused treatment, or other trauma-focused treatment).

2.6. Reliability

We completed inter-rater reliability in which we randomly selected 20 % of included articles and compared data between the original rater and one additional rater. Inter-rater reliability was strong across variables.

2.7. Risk of bias

We completed risk of bias coding using the Cochrane Risk of Bias tool 2.0 for randomized trials (Sterne et al., 2019). This tool assesses risk of bias across five domains, including the randomization process, deviations from the intended intervention, missing outcome data, outcome measurement, and selection of reported results. We focused on the primary PTSD outcome for risk of bias coding. Two independent raters (among CPM, HCL and MLM) assessed each study and any discrepancies were discussed until consensus was reached. This same protocol was used for the two additional studies identified in the new literature search. Risk of bias scores can be derived from the domain ratings, such that the overall study score is coded as "low risk" if all domains are scored as "low risk." The overall study score would be "high risk" if any domains are coded as "high risk". We also calculated a total risk score by adding up each domain score (low risk = 0, some concerns = 1, high risk = 2) to yield a total score ranging from 0 to 10 for each study.

2.8. Data synthesis

Outcomes for exposure and control samples were compared. The primary variable of interest was the mean score on a standardized measure of PTSD. Intent-to-treat (ITT) data were used to provide a more conservative estimate of treatment effects that more closely represents clinical practice (Abraha et al., 2015; McCoy, 2015). In rare cases when ITT data were unavailable, completer data were used. Data were analyzed using Comprehensive Meta-Analysis Version 2.2 (Borenstein et al., 2007) using strategies from Borenstein et al. (2009). Effect sizes (Hedges's *g*) were calculated using a random effects model and weighted for inverse variance. Hedges's *g* is a small-sample correction for Cohen's *d*, for which effect sizes of 0.2, 0.5 and 0.8 are traditionally interpreted as small, medium and large, respectively (Cohen, 1988). The 95 % confidence interval (CI) was calculated for each effect size estimate. Calculation of *g* for pre-post designs requires an estimate of the correlation (*r*) between the pre- and posttreatment scores; because this was not available in published reports, *r* was conservatively estimated at 0.7 according to the recommendation of Rosenthal (1991). Between-group effects, which assess the difference between exposure and control groups at posttreatment were calculated as $d = \frac{\overline{X}_1 - \overline{X}_2}{S_{within}}$, where $S_{within} = \sqrt{\frac{(n_1-1)S_1^2+(n_2-1)S_2^2}{n_1+n_2-2}}$, n_1 and n_2 are the sample sizes of the two groups, and S_1 and S_2 are the standard deviations of the two groups. All *d* scores were converted to *g* using the standard correction procedure that adjusts for degrees of freedom (Hedges, 1981).

Publication bias was assessed using funnel plots, examining asymmetry of effect size against standard error. Duval and Tweedie (2000) Trim and Fill was used; this method trims asymmetric studies from the right-hand side to locate the unbiased effect (in an iterative procedure), and then fills the plot by re-inserting the trimmed studies on the right as well as their imputed counterparts to the left of the mean effect. The I^2 statistic was used to assess heterogeneity. The I^2 statistic is expressed as the percentage of variation due to true heterogeneity rather than chance and is interpreted as follows: 25 % = little heterogeneity, 50 % = moderate heterogeneity, and 75 % = high heterogeneity (Higgins et al., 2003). To test the file drawer effect (the probability that unpublished null results would eliminate the obtained results), for each result the fail-safe N (FSN), or the number of null results that would be needed to overturn a significant result, was calculated. Generally, if the FSN is greater than or equal to 5 times the number of studies in the analysis plus 10, the obtained results are considered robust against the file drawer effect (Rosenthal, 1991).

Meta-regression was used to explore the relationship between continuous moderator variables (study year, number of sessions, and percentage of participants who were racial or ethnic minorities, taking

psychiatric medications, diagnosed with depression, or diagnosed with a substance use disorder) and effect size in the exposure therapy groups. Categorical moderator variables (exposure type and sample type) were explored by separating the samples by moderator variable and examining between-group heterogeneity using the *Q* statistic.

Additional statistics (risk of bias comparisons) were conducted using SPSS v. 26.

3. Results

3.1. Literature Search

A majority ($n = 16$) of the included studies were previously identified as part of our previous meta-analysis (McLean et al., 2021a). Our new searches yielded three studies. A total of 19 studies meeting our eligibility criteria were included in the analyses, representing a total of 2905 patients (1497 received exposure-based therapy and 1408 received a control intervention). See Fig. 1 for the PRISMA flow diagram of selected studies and reasons for study exclusion. See Appendix A for a list of the included studies. Coded characteristics of all included studies are available in Appendix B.

3.2. Primary Outcomes

When comparing conditions at posttreatment on PTSD measures, exposure therapy was superior to control conditions, with a small to medium effect size (k [number of comparisons]) = 22, $g = 0.442$, 95 % CI = 0.254–0.630 (see Fig. 2). Heterogeneity across studies was high ($I^2 = 77.251$). The Trim and Fill procedure (random effects) estimated 9 missing studies to the left of the mean; adding these resulted in an imputed effect size of 0.116 (−0.048 to 0.343). FSN was 352, suggesting that this finding is robust against the file drawer effect.

At follow-up, exposure was associated with a small effect size on PTSD measures ($k = 10$, $g = 0.350$, 95 % CI = 0.121–0580637, $p = 0.003$). Heterogeneity was high ($I^2 = 81.129$). The Trim and Fill procedure (random effects) identified no missing studies. FSN was 81, suggesting that this finding is robust against the file drawer effect.

Table 1 shows the effects of exposure therapy vs. various control conditions at posttreatment on PTSD measures. Note that because some studies included more than one control group, the total k for subgroup comparisons is more than 22 (for this, we used all comparisons, assuming independence). Heterogeneity across control conditions was significant, $Q(3) = 12.634$, $p = 0.005$. Exposure was superior to both wait list and TAU, with medium to large effect size estimates. The effect of exposure was small compared to non-trauma-focused therapy. There was no effect for exposure compared to other trauma-focused therapy; this was based on only two comparisons.

At follow-up, heterogeneity across control conditions was significant, $Q(3) = 40.492$, $p < 0.001$. However, the only control condition with more than 2 comparisons was for non-trauma-focused therapy ($g = 0.196$, CI = 0.030–0.362) and therefore no further analysis was conducted.

3.3. Moderator outcomes

Continuous moderators are shown in Table 2. Studies with older samples associated with a smaller effect size. The proportion of women in the sample was associated with a larger effect size. In addition, the proportion of participants with major depressive disorder and the proportion taking psychiatric medications were also associated with smaller effects. No other examined moderators impacted the effect of exposure.

For categorical moderators, we first examined the effects of various exposure therapy packages. Note that because some studies included more than one exposure group, the total k for subgroup comparisons is

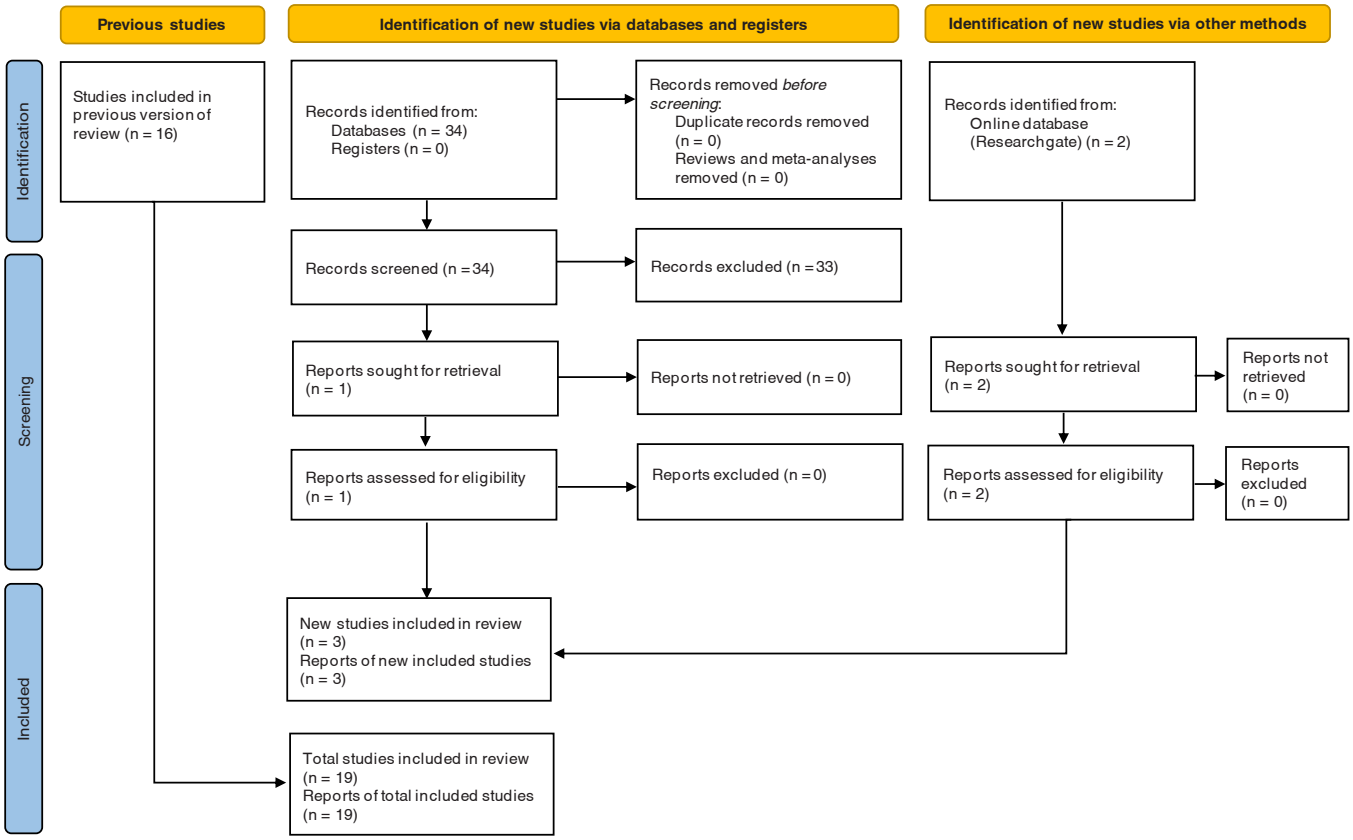

Fig. 1. prisma Flow Diagram of Articles selected for meta-Analysis *Note*. CI = confidence interval, TAU = treatment -as-usual. "Combined" indicates that there was more than one comparator examined.

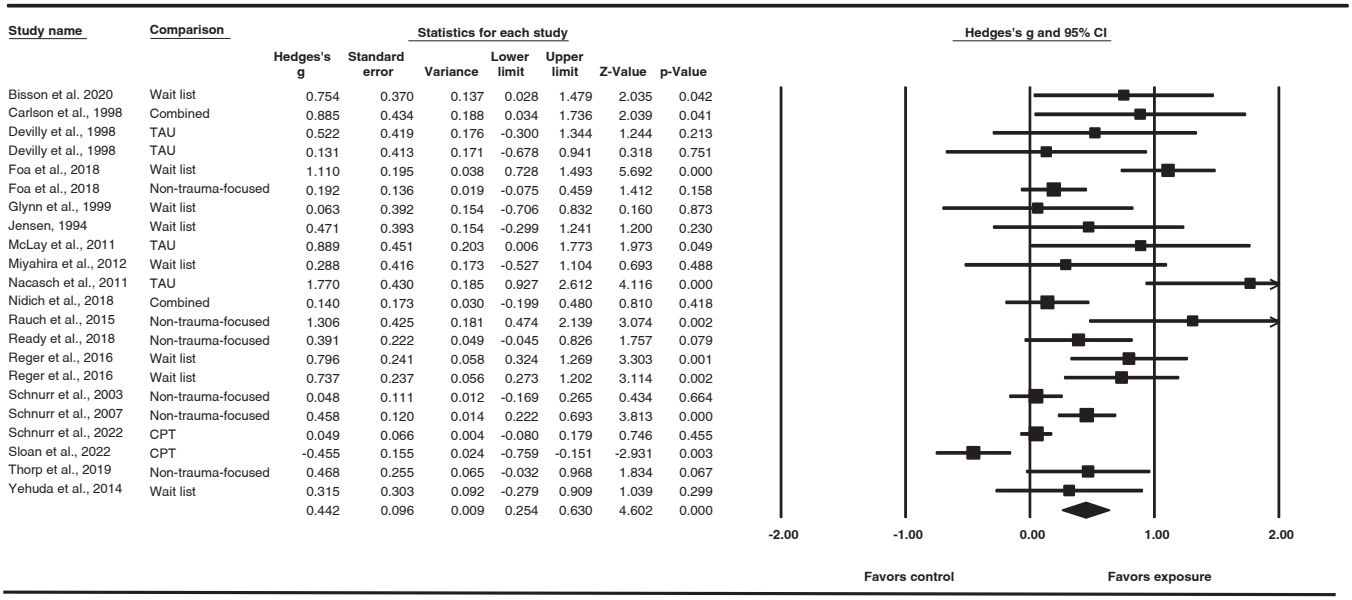

Fig. 2. Forest plot of PTSD measures at post-treatment. *Note.* CI = confidence interval, TAU = treatment as usual, CPT = Cognitive Processing Therapy. Studies are listed more than once if they included multiple exposure therapy conditions.

Table 1
Efficacy of exposure therapy vs. various control conditions on PTSD measures at posttreatment.

Comparison	*K*	*g*	95 % CI	FSN
Wait list	9	0.688	0.460–0.915	89
TAU	4	0.821	0.128–1.514	12
Non-trauma-focused	9	0.335	0.110–0.559	57
Other trauma-focused	2	-0.183	-0.676 to 0.309	–

*Note. CI, confidence interval; FSN, fail-safe N; TAU, Treatment as Usual.

Table 2
Meta-regression of continuous moderator variables predicting PTSD outcomes from exposure therapy at posttreatment.

Moderator	*Z*	*P*
Number of sessions in protocol	-0.508	0.612
Mean age	-3.722	< 0.001
%Women	2.072	0.038
% Racial minority	-1.531	0.126
% Hispanic/Latinx	-1.858	0.063
% Diagnosed with major depression	-3.550	< 0.001
% Diagnosed with a substance use disorder	0.637	0.524
% On medications	-3.890	< 0.001

more than 22 (again, we used all comparisons, assuming independence). Heterogeneity across treatment packages was not significant, $Q(3) = 7.213$, $p = 0.065$.

At follow-up, heterogeneity across treatment packages was significant, $Q(2) = 8.144$, $p = 0.017$, indicating significant differences across exposure treatments. There was a small effect for PE ($k = 6$, $g = 0.477$, CI = 0.123–0.832), and no effect for "other" exposure therapies ($k = 3$, $g = 0.069$, CI = −0.095 to 0.233). There was only 1 study of EMDR, which showed a large effect ($g = 1.014$, CI = 0.166–1.863),

Active duty vs. Veteran status was not associated with significant heterogeneity, $Q(1) = 0.121$, $p = 0.728$.

3.4. Risk of bias

Risk of bias assessments for the included studies is summarized in Appendix A. The median risk of bias score was 1 (range = 0–8) and 5 studies were rated as high risk. High risk of bias was most frequently due to nonadherence to the intervention (i.e., lack of fidelity assessment or a high proportion of participants not completing the full treatment protocol plus failure to use an ITT analytic approach). Meta-regression indicated that there was no significant relationship between risk of bias score and effect size, $Z = 1.536$, $p = 0.124$.

4. Discussion

This study documents the variable efficacy of exposure therapy for PTSD among military populations. Across comparator types, the overall effect of exposure therapy was small to medium, and results of the trim and fill procedure suggested that the included studies may overestimate the true effect. As expected, there were medium to large effects favoring exposure over waitlist and treatment as usual and no effect relative to other trauma-focused treatment (i.e., CPT). There was a small effect favoring exposure over non-trauma-focused treatment. This finding contrasts slightly with conclusions drawn by (Steenkamp et al., 2020) that trauma-focused therapy does not outperform non-trauma-focused therapy. This discrepancy is likely because Steenkamp and colleagues focused on a smaller sample of studies that enrolled active duty personnel only. Our finding is consistent with prior meta-analyses of broader populations showing a small effect for exposure (McLean et al., 2021a) and trauma-focused therapy in general (e.g., Bisson, 2013) relative to non-trauma-focused treatment.

At follow-up, exposure therapy showed a small effect compared to all control types, suggesting that effects are generally stable throughout study follow-up periods (range: 3–12 months). This is the first meta-analysis, to our knowledge, to document the effects of exposure therapy at follow up among military populations and is consistent with prior research showing stable effects for exposure (McLean et al., 2021a) and trauma-focused therapy (Ehring et al., 2014) across follow up.

The average age of the sample moderated the effect of exposure, such that studies with older samples showed smaller effects than those with younger samples. It's possible that age is a proxy for chronicity of PTSD, which we did not examine in this study as this variable is rarely reported in clinical trials. However, several prior studies have found PTSD chronicity to be unrelated to treatment outcomes (e.g., Ehlers et al., 2005). Litz et al. (2019) hypothesized that therapeutic alliance may be limited when predominately young civilian therapists deliver treatment

to older service members.

Studies with a greater proportion of women were associated with a larger effect size. This finding differs from our prior meta-analysis across populations (McLean et al., 2021b), but is consistent with the meta-analytic results of Watts et al. (2013), who also found that across populations, studies with more women had larger effects. Findings from individual studies on the effect of gender on PTSD treatment outcomes have been somewhat mixed, even within studies of exposure therapy for Veterans (Khan et al., 2020; Mouilso et al., 2015). Maguen et al. (2012) found that women Veterans received minimally adequate mental health care approximately two years sooner than men Veterans and hypothesized that this might in turn impact treatment benefit (Maguen et al., 2014). Gender differences in military roles and type of traumatic event exposure (Lehavot et al., 2018) complicate interpretation of this finding.

Effect sizes for exposure therapy were also lower among studies with a larger proportion of individuals diagnosed with major depression and taking psychiatric medication. These findings are broadly consistent with the conclusions of Litz et al. (2019) that trauma-focused treatments may be less beneficial among more clinical "complex" military samples. Whereas Litz et al. examined PTSD and depressive symptom severity, this is the first meta-analysis, to our knowledge, to identify comorbid depression and psychiatric medication status as potential negative prognostic factors for exposure therapy. It may be that individuals with comorbid major depression benefit more from integrated or alternate treatment. Psychiatric medications, namely benzodiazepines, can impede the efficacy (Guina et al., 2015) and maintenance (Rosen et al., 2013) of exposure therapy. Alternately, or in addition, psychiatric medication status may reflect greater clinical complexity and/or treatment resistance. Future research is needed to replicate these findings. The proportion of individuals with comorbid substance use disorder did not moderate the effect of exposure. For most studies, substance dependence was an exclusion criterion, which may have limited the range of substance use represented in the samples. It is worth noting that these findings are consistent with a study showing that depression, but not substance use, predicted worse treatment outcomes among Veterans (Richardson et al., 2014).

In contrast to our prior meta-analysis across populations, we did not find that the number of sessions in the exposure protocol moderated the effects of exposure. In Veterans, Haagen et al. (2015) found that studies with more trauma-focused sessions, but not more sessions overall, showed larger effects. Future research may want to examine the number of sessions overall and the number of trauma-focused or specifically exposure-based sessions to better understand how treatment length and focus impacts outcomes. We also found that Veteran vs. active duty status did not moderate outcomes, suggesting that the effect of exposure therapy is comparable across these groups, despite probable contextual differences (e.g., ongoing training, deployment).

Neither ethnicity nor race moderated the effect of exposure. This is consistent with our prior meta-analysis across populations (McLean et al., 2021a), although other research has found that Veterans who identify as racial or ethnic minorities have worse clinical outcomes following routine outpatient PTSD care through the Veterans Affairs health care system than Veterans who do not identify as racial or ethnic minorities (Maguen et al., 2014; Sripada et al., 2017). Because we could only examine these variables in very gross categories due to the limited information most studies provided, future research examining racial and ethnic identities as potential moderators of PTSD treatment outcomes is warranted.

Effects sizes did not differ across leading exposure-based psychotherapy protocols at post-treatment. This suggests that the effect of exposure therapy is robust when delivered in different ways (e.g., prolonged exposure vs. brief exposure, traditional imaginal exposure vs. VR exposure). At follow up, the effect sizes did differ, with no effect for "other" exposure (including a large non-inferiority trial comparing WET with CPT), a small effect for PE (including an unprecedentedly large trial of PE and CPT), and a large effect for EMDR (based on one study). These findings are based on a subset of studies and different exposure therapies were compared to different comparators, which limits conclusions about the relative effects of different exposure therapies at follow up.

Most of the included studies had at some methodological limitations that introduced some risk of bias and a significant minority of studies ($n = 5$) were rated as high risk of bias, consistent with previous meta-analyses (Cusack et al., 2016). However, risk of bias was not related to the effect size for exposure therapy in this sample of studies.

4.1. Strengths and limitations

Strengths of this study include adherence to PRISMA guidelines (Page et al., 2020) and pre-registration in PROSPERO. We also used a validated tool for evaluating risk of bias of the included studies (Sterne et al., 2019) covering a wide range of potential sources of bias. Our study represents an important update to Haagen et al.'s (2015) meta-analysis of recommended treatments in Veterans. Unlike Haagen et al. we focused specifically on exposure therapies, expanded to include active duty personnel, examined change at follow-up, and were able to include ten more recently published studies.

There are several important limitations worth noting. First, the trim and fill procedure results indicated that 9 studies were missing from the analysis examining the overall effect of exposure. This indicates that the true effect of exposure may be lower than estimated in the current analyses. However, it should be noted that we included a very large study ($N = 916$) that found a small, not clinically meaningful advantage for exposure relative to another trauma-focused therapy (Schnurr et al., 2022). Schnurr et al. (2022) compared PE to CPT, which is another trauma-focused therapy; therefore, superiority of PE was not hypothesized. Given the sample size, this study would have had a disproportionate negative impact on the estimated true effect of PE. Second, our examination of moderators was limited by what was reported in the studies. For example, only a minority of the studies reported the ethnicity of participants, and ethnicity focused on Hispanic (and more rarely, Latinx) identity. Race was also not always reported and when it was, racial categories were not reported consistently. Most studies reported only the proportion of participants who were Black, White, or "other," and many conflated race and ethnicity. Therefore, our analysis merged racial groups that were not listed as White, which could obscure important differences across specific racial groups. More consistent reporting of participant race and ethnicity, following recommended guidelines (Flanagin et al., 2021) is needed to permit analyses of the effect of exposure across specific racial and ethnic groups. In addition, we had intended to examine the type of trauma reported by participants given that previous work (e.g., Straud et al., 2019), including our own (McLean et al., 2021a) found effect size differences across trauma types. However, a closer examination of this variable among the included studies revealed inconsistencies in how trauma type is reported that would render an analysis of this variable, in our view, unhelpful. Many studies provide no trauma type information at all, and most that did report only broad trauma type categories such as "combat" or "military" which could include a range of traumatic events (e.g., "military-related" might include being shot, loss of a close friend, and sexual assault) may be too imprecise to be clinically meaningful. Greater consistency in reporting more detailed index-trauma type information is needed. For example, adopting the trauma categorization scheme proposed by the South Texas Research Organizational Network Guiding Studies on Trauma and Resilience Consortium (Stein et al., 2012) could facilitate a closer examination of the impact of trauma type in military populations. Third, several of the comparison conditions (e.g., TAU, $k = 4$; trauma-focused, $k = 2$) had low sample sizes, therefore the findings from these comparisons should be interpreted with caution. Fourth, as noted above, the methodological quality of the studies included in our meta-analysis was mixed, with few studies being rated as "low" risk of bias. However, risk of bias did not affect the effect size estimates. Finally, although recent studies suggest that exposure is efficacious

when delivered through telehealth (e.g., Acierno et al., 2017), web-based programs (e.g., McLean et al., 2021b), and intensive delivery formats (e.g., Dell et al., 2022; Rauch et al., 2021), these studies did not meet our study inclusion criteria, therefore, we were not able to examine delivery format or tempo of exposure delivery as moderators in this study.

5. Conclusion

Results of this study provide evidence that exposure therapy is effective for treating PTSD in military populations, with benefits that, on average, exceed waitlist, treatment as usual, and, to a lesser degree, non-trauma-focused therapy. Importantly, the effects of exposure were larger for studies with younger participants, more women, fewer participants with comorbid major depression, and fewer participants taking psychiatric medication. Further research to replicate these findings, test these variables as individual-level moderators of outcome, and examine potential pathways through which these factors may impact clinical outcomes is needed. Such work may inform treatment planning and tailoring that further improves PTSD outcomes for veterans and service members.

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Conflicts of interest

We have no conflicts of interest to disclose.

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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.2022.102607.

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