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The Efficacy of Mindfulness Based Interventions for Soldiers and Veterans

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The Efficacy of Mindfulness-Based Interventions for Soldiers and Veterans: A Meta-Analysis

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ABSTRACT

The psychological impacts of war can be devastating for soldiers and veterans. Even for those who have not seen combat, military service can be a stressful experience. Although posttraumatic stress disorder (PTSD) has been identified as the most common form of psychopathology in this population, military personnel are susceptible to a variety of other mental illnesses, including anxiety, depression, and substance use disorders (Prigerson, Maciejewski, & Rosenheck, 2002). Meta-analytic reviews demonstrate the efficacy of exposure-based interventions for soldiers and veterans with symptoms of posttraumatic stress (PTS), but results are inconclusive regarding the efficacy of other treatments for military servicemembers (Kitchiner, Roberts, Wilcox, & Bisson, 2012). Further, there appears to be a significant portion of soldiers and veterans with PTSD that does not respond to exposure-based treatments (Sher & Yehuda, 2011). Mindfulness-based interventions have shown to be effective in treating a breadth of psychopathology in civilian populations (Khoury et al., 2013). The purpose of the proposed study was to conduct a meta-analysis analyzing the efficacy of such interventions, specifically with soldiers and veterans. Three major literature databases, PsycINFO, Medline, and Cochrane were searched for randomized controlled studies that used mindfulness-based interventions with soldiers and veterans. It was hypothesized that mindfulness-based interventions would be shown to be efficacious for soldiers and veterans. Format and dosage of treatment, study quality, and diagnosis were coded and examined as potential moderator variables. General support was found for the efficacy of mindfulness-based interventions with soldiers in terms of reduction of symptoms of PTS and depression and positive changes in various “other” outcome areas. Future meta-analyses on this topic would benefit from incorporating biological and physical health outcome data (which were excluded from the present study).

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The threats to United States soldiers and veterans go far beyond threats to the physical self (hereafter, the term "soldier" will be used to refer to all active-duty military personnel as well as retired military servicemen and women, regardless of military branch). In fact, the emotional and psychological risks of serving in the military can be just as debilitating as any threat to the body (Shea, Vujanovic, Mansfield, Sevin, & Liu, 2010). The exposure and threat of exposure to horrific events, coupled with the soldiers' removal from their support systems, make war a breeding ground for chronic mental illness. Results of previous research suggest that approximately one quarter of active-duty soldiers meet criteria for at least one mental illness, while over 10% meet criteria for more than one mental illness (Ursano, Colpe, Heeringa, Kessler, Schoenbaum, & Stein, 2014). Some common mental health issues that have been identified in soldiers include major depressive disorder (MDD), substance abuse, and posttraumatic stress disorder (PTSD) (Hoge et al., 2004). The estimated rate of MDD in soldiers returning from Iraq and Afghanistan is up to five times higher than the rate in the civilian population, while the estimated rate of PTSD in soldiers returning from Iraq and Afghanistan is up to 15 times higher than in the civilian population (Ursano et al. 2014). Further, soldiers appear to be at significant risk of developing subthreshold post-traumatic stress (PTS) symptoms, which can increase risk of development of other psychiatric disorders and generally impair mental and physical functioning (Mota et al., 2016). Consequently, even soldiers who do not meet full diagnostic criteria for a mental health condition may experience marked repercussions.

Living with symptoms of mental illness after war can be perceived as worse than death for some, as evidenced by the high suicide rates of soldiers with mental illness (Kang & Bullman, 2008). A look at some of the features of the most common psychiatric issues faced by soldiers, PTS (Seal, Bertenthal, Miner, Sen, & Marmar, 2007), can shed light on what untreated

soldiers with this disorder must battle with on a daily basis. While soldiers living in wartime conditions need to be ever-alert to danger to stay alive, the persistence of these hyper-vigilant traits outside the context of war can be quite devastating (Rathus & Sanderson, 1998). Consequently, when soldiers' reliance on what can be seen as "survival instincts" bleeds into their post-war civilian life, they will suffer a host of negative consequences. According to Schnurr, Lunney, Bovin, and Marx (2009), soldiers with PTSD suffer from devastating quality of life impairments that are similar to impairments experienced by non-soldiers as well. Herman (1997) describes the hallmark symptoms of PTS. Avoidant symptoms include victims' minimization of interaction with trauma "cues" that may incite unpleasant memories or emotions, can severely limit individuals' social and occupational lives. These features are complemented by intrusive symptoms, which include persistent vexation by disturbing memories or nightmares about the traumatic events. These symptoms have the potential to leave individuals in states of terrific emotional turmoil, which may likely perpetuate the aforementioned avoidant features. Lastly, hyper-vigilant symptoms, which encompass victims' emotional overreactions to neutral stimuli and situations, can cause conflict between loved ones and impair relationships (Herman 1997). Soldiers with PTS may also experience dissociation symptoms, where they detach from reality as a means of protecting themselves from situations that may cause them undue stress (Bremner, Southwick, Brett, Fontana, Rosenheck, & Charney, 1992). Misuse of mind-altering substances, which may lead to substance use disorders, may also be a way for soldiers to cope with their debilitating symptoms. This is supported by the fact that soldiers with PTSD are three times more likely to have a comorbid substance use disorder than adults in the general population (Petrakis, Rosenheck, & Desai, 2011).

Not surprisingly, suicide in soldiers is becoming a major concern in contemporary society. Soldier mental illness has been identified as a key component of soldier suicidality (Nock et al., 2015). As many as 14% of soldiers have suicidal ideation, while 2.4% make one or more suicide attempts (Ursano et al. 2014). Other results from the national health and resilience study in soldiers, which gauged suicidal ideation at two time points two years apart, showed that 5% of soldiers had suicidal ideation onset, 4.9% had chronic suicidal ideation, and 3.8% had remitted suicidal ideation (Smith et al., 2016). Some researchers suggest that these statistics are under-representations of actual soldier suicide statistics, as soldiers are more prone to "accidental" deaths than the civilian population (Sher & Yehuda, 2011). Sher and Yehuda (2011) postulated that soldiers are at increased risk of suicide because of the stress induced by deployment. Once deployed, soldiers are physically separated from their lifelong support systems and often bear witness to unspeakable tragedies. Traumas experienced by soldiers appear to be qualitatively different from trauma experienced by civilians. Further, military traumas can have an immensely different impact because soldiers may initially have many positive experiences during a deployment. Soldiers form close ties with each other during deployment, which is a time for bonding and relationship-building (Sher & Yehuda, 2011). Exposure of harm to peers as well as to self may therefore be particularly devastating, as highlighted in Sebastian Junger's (2015) *Vanity Fair* piece on PTSD, which cites data from a survey of Operation Desert Storm combat veterans' greatest fears in battle (Gifford, Ursano, Stuart, & Engel, 2006).

For soldiers who have been exposed to combat trauma, they may experience a whole host of complicating issues to navigate, including having felt betrayed by leadership, blunted responsiveness to emotional, social, or ethical discourse with others who were not fellow combat

soldiers, grief and guilt for deaths of comrades, desire for revenge, renunciation of ever returning home, seeing oneself as already dead, dishonoring the enemy, and loss of humanity. As noted by Shay (1991), these factors can be identified in Vietnam veterans as well as in the characters portrayed in Homer's *Iliad*, a notion which suggests the universality of these themes. Soldiers may further struggle with the belief that they abandoned their fellow service-members by returning home, and the ensuing guilt may precipitate suicide. According to a study that assessed suicidality in veterans of Vietnam, guilt over actions in combat, especially when coupled with survivor's guilt, played a major role in veteran suicide risk (Hendin & Haas, 1991). The implications here are that suicide in soldiers may stem more from social factors, as guilt is an inherently social emotion. Therefore, it is of the utmost importance that the country to which soldiers return is one marked by social cohesion instead of division (Junger, 2016). A public health model, which fosters social outreach and engagement of returning soldiers, instead of the traditional medical model which treats PTSD as a purely biological, aberrant condition, may reduce the negative sequelae of trauma experiences with this population (Kudler, 2007).

With all of the debilitating psychological and emotional consequences soldiers face upon their return home, it is imperative that these individuals receive the best mental health treatments available. A substantial body of research has been conducted to ascertain the efficacy of various treatments in soldiers. Meta-analytic reviews provide evidence for trauma-focused interventions (Kitchiner, Roberts, Wilcox, & Bisson, 2012) as well as exposure-based interventions (Goodson, Helstrom, Halpern, Ferenschak, Gillihan, & Powers, 2011) for soldiers with symptoms of PTS. These findings mirror outcomes from civilian populations with symptoms of PTS (Powers, Halpern, Ferenschak, Gillihan, & Foa, 2010; Bisson, Ehlers, Matthews, Pilling, Richards, & Turner, 2007). However, Sher and Yehuda (2011) noted that soldiers' success rates in many

evidence-based treatments for PTS, such as exposure-based treatments, cognitive processing therapy (CPT), and psychiatric medication, are markedly lower than civilians' success rates in these treatments. The researchers attribute this outcome to the fact that the psychobiology of soldiers with symptoms of PTS is markedly different than the psychobiology of civilians with symptoms of PTS (Sher & Yehuda, 2011).

Kitchiner and colleagues (2012) conducted a meta-analysis to explore the efficacy of psychological treatments for soldiers with various psychopathologies. The meta-analysis included 29 randomized controlled trials. Sixteen of the included studies featured male soldiers of Korea or Vietnam who suffered from symptoms of combat-related PTS, while the remaining studies featured soldiers of Korea, Vietnam, or the Gulf War with borderline personality disorder (BPD), depression, Gulf War Illness, panic disorder, or insomnia. Participants of two studies were made up exclusively of women who had never been deployed. Results demonstrated efficacy for trauma-focused therapies for soldiers with PTSD, including exposure-based therapies, CPT, and eye movement desensitization and reprocessing (EMDR). The meta-analysis also cited individual studies that suggested that treatment of soldiers without PTSD was effective. Such treatments included dialectical behavior therapy (DBT) for women with BPD and cognitive behavioral therapy (CBT) for individuals with panic disorder. However, since evidence base for these treatments was limited to single studies, Kitchiner and colleagues (2012) concluded that more research needs to be done to determine the efficacy of psychotherapies for non-PTS mental illnesses in soldiers. This conclusion was further buttressed by the fact that the studies included were not the most methodologically sound or rigorous. Common methodological issues of these studies included incomplete outcome data, lack of researcher and

participant study blindness, and failure to report exclusion criteria. These results suggest that it is necessary to find out how effective non-exposure-based treatments are for soldiers.

The aforementioned empirically supported treatments for soldiers would classify as either first or second wave behavior therapies (Hayes, 2004). The third wave of behavior therapy, which includes mindfulness, takes a markedly different therapeutic approach than its predecessors. Allen, Blashki, and Gullone (2006) noted that mindfulness evolved from eastern philosophical traditions and the spirituality tenets of Buddhism. They further explained that mindfulness in the therapeutic setting teaches people how to exist nonjudgmentally in the moment. It requires individuals to intently focus their attention on an aspect of their present internal worlds or external environments and accept those aspects for what they are in that moment.

The application of mindfulness to the clinical setting is based on the premise that the mind wanders naturally, and when it does, it can trigger negative thoughts and moods (Segal, Williams, & Teasdale, 2002). People can come to an acceptance of their internal experiences by focusing their attention on those experiences and not imparting judgment to them (Allen et al. 2006). Incidentally, mindfulness can help reduce dysphoric emotions or troublesome thoughts by increasing awareness of all thoughts, emotions, and physical sensations, accepting them, and responding mindfully. Various techniques may be made use of in mindfulness-based therapies to facilitate awareness, including relaxation exercises, breathing exercises, and body awareness exercises (Segal et al. 2002). The cognitive component of mindfulness consists of teaching individuals to be aware of and accept negative mood states or thoughts (Hayes 2004). By doing so, people can prevent these disturbing internal states from spiraling out of control (Segal et al. 2002). To accomplish this, mindfulness participants are asked to explore their thoughts and

moods from a place of nonjudgmental awareness (Allen et al. 2006). They are also asked to identify warning signs of impending dysphoric mood states, and then implement specific adaptive techniques that have worked for them in the past to prevent the state from worsening. Such techniques can change the way individuals relate to their thoughts and feelings, and in turn can prevent future dysphoric episodes (Segal et al. 2002).

Mindfulness and acceptance-based therapies have been shown to be effective for a variety of disorders in *civilian* populations. A meta-analysis conducted by Khoury and colleagues (2013) produced promising findings for mindfulness therapies. The researchers included studies that examined the pre and post or controlled effects of mindfulness-based therapies for a wide range of individuals with physical and medical conditions, individuals with psychological conditions, and non-clinical individuals. They excluded studies if they did not include a mindfulness meditation-based intervention; did not aim to examine treatment effects; consisted merely of comparisons among meditators or among meditation styles; examined the non-direct effects of mindfulness; examined mindfulness as a component of another treatment; reported no clinical outcomes; reported insufficient information to compute an effect size; or reported data that overlapped with the data from other included studies. The most prominent conditions treated included symptoms of mood and anxiety disorders, cancer, chronic pain, and substance abuse. After a comprehensive review of over 200 studies, the researchers concluded that mindfulness-based therapies were moderately effective in terms of pre- and post-test analyses and control group comparison. Mindfulness-based therapies were further shown to yield effects comparable to other empirically supported treatments as well. Specifically, the results suggested that no significant differences in outcome existed between mindfulness-based therapies and cognitive behavioral, behavioral, or pharmacological treatments. The researchers

specified that mindfulness-based therapies were shown to be particularly effective in treating depression and anxiety symptoms.

Another meta-analysis conducted by Cavanagh, Strauss, Forder, and Jones (2014) explored the efficacy of mindfulness-based self-help interventions, such as books, computer programs, and audio-visual materials. The researchers focused on mindfulness self-help interventions because of the immensity of resources required for traditional mindfulness and acceptance-based therapies. Fifteen randomized controlled trials met the meta-analysis' inclusion and exclusion criteria. In general, these criteria required studies to be published reports of empirical studies that employed self-help mindfulness or acceptance-based interventions for adult populations. The populations featured in the meta-analysis varied widely in terms of participants and diagnoses. Five studies used non-clinical samples, which included college students, teachers, or community members; five studies used a sample with chronic pain; three studies used samples with symptoms of depression and/or anxiety, stress, exhaustion, or insomnia; one study used a sample with major depressive disorder; and one study used a sample of soldiers with combat related PTSD. About half of the studies included in the meta-analyses compared mindfulness-based self-help interventions to no treatment or wait-list control groups, while the other half compared mindfulness-based self-help interventions to more active control groups, like self-help CBT and psychoeducation. The researchers concluded that mindfulness self-help interventions were effective in reducing symptoms of both anxiety and depression compared to both types of control conditions.

The mechanisms by which mindfulness effects change are explored in an article by Shapiro, Carlson, Astin, and Freedman (2006). The authors propose that mindfulness practice yields positive outcomes through a global significant shift in perspective, termed "reperceiving."

Along with this overarching mechanism, several interrelating variables operate when mindfulness is practiced to achieve adaptive change. These variables include self-regulation, clarification of values, cognitive, emotional and behavioral flexibility, and exposure. Essentially, by purposefully attending to the environment without judgment, individuals develop the capacity to choose more adaptive methods of regulating dysphoric states; reexamine priorities and replace them with more valuable or meaningful ones; respond cognitively, emotionally, and behaviorally to the environment in more thoughtful and flexible ways; and withstand the intensity of internal states in a more objective and healthy way. While these variables often lead to beneficial outcomes such as mitigation of psychological symptoms, they can also be seen as ends in their own rights. Indeed, these less tangible outcomes might be accounted for by findings from a study by Hölzel and colleagues (2011), which demonstrated that participation in a brief mindfulness-based intervention led to changes in brain structures related to learning and memory, emotion regulation, self-referential processing, and perspective-taking.

The documented benefits of mindfulness-based interventions in civilian populations make its application to soldiers, a population highly susceptible to symptoms of mental illness, a priority. In another vein, while some have argued that exposure-based treatments are the only empirically supported treatments for soldiers with symptoms of PTS (Haagen, Smid, Knipscheer, & Kleber, 2015), Sher and Yehuda (2011) noted that such interventions have been shown to be less effective for soldiers than for civilians with symptoms of PTS. Further, it is imperative to demonstrate empirical support for psychological treatment of soldiers with other types of psychopathology, as Kitchiner and colleagues' (2012) meta-analysis concluded that there is insufficient evidence to support the use of psychological interventions for soldiers who have mental illnesses other than PTS. Given the fact that mindfulness-based interventions with

civilians have been shown to effectively treat conditions commonly found in military populations, it is conceivable that similar outcomes will be realized with soldiers.

Overall, mindfulness' emphasis on purposefully living in the present without judgment may provide soldiers with a new perspective on how they understand their thoughts and behaviors. The purpose of this study, therefore, will be to conduct a meta-analysis to determine the efficacy of mindfulness-based interventions for soldiers, regardless of presence or type of diagnosis. It was hypothesized that mindfulness-based interventions would yield significantly better outcomes than treatment-as-usual or no-treatment control groups for soldiers, because mindfulness-based interventions have shown to be effective for other populations (Cavanagh et. al 2014; Hofmann, Sawyer, Witt, & Oh, 2010; Khoury et al. 2013). Given the large effect sizes found for exposure-based treatments with soldiers (Haagen et al., 2015), this study hypothesized that mindfulness-based interventions would not outperform extant empirically-supported treatments.

Consistent with the extant literature of mindfulness-based interventions with civilians (Khoury et al. 2013), it was expected that outcomes would vary as a function of mental health condition. Specifically, soldiers with symptoms of anxiety and depression were expected to experience more gains than soldiers with other forms of psychopathology. Participants from the studies did not have to meet full criteria for a mental illness for a "primary diagnosis featured" code to be used. In addition, effect sizes calculated in these studies could have been taken from either categorical or continuous outcome measures. Treatment format was also anticipated to affect treatment outcome. While there is some evidence to suggest that mindfulness-based cognitive therapy (MBCT) may be equally effective in individual or group settings with civilians (Schroevers, Tovote, Snippe, & Fleer, 2016), individual therapy with soldiers has been shown to

yield greater outcomes than group therapy (Haagen et al., 2015). Therefore, it was anticipated that soldiers receiving mindfulness individually were expected to benefit more than soldiers receiving mindfulness in a group setting. Findings from Khoury et al. (2013) further suggested that longer duration of treatment led to enhanced treatment outcomes with civilians. It was therefore hypothesized that a longer duration of mindfulness treatment would be associated with more clinical benefits. Lastly, it was predicted that methodological study quality would be associated with the magnitude of the effect size, with higher quality studies having lower effect sizes (Goodson et al. 2011; Khoury et al. 2013). The information gleaned from these analyses will facilitate treatment planning for soldiers and ensure that they receive the most appropriate and efficacious treatments available.

Method

Eligibility Criteria

To be considered eligible for this meta-analytic review, studies had to be randomized controlled trials of mindfulness-based interventions for soldiers. Mindfulness-based intervention was defined for the purposes of this study as one in which participants were taught to focus their attention on internal and external stimuli—such as thoughts, feelings, and sensory experiences—occurring in the present moment. Participants were encouraged to do so in a nonjudgmental fashion and to accept the states for what they were (Hofmann et al., 2010). To meet eligibility criteria, studies must have described a treatment they delivered in a way that implicitly or explicitly reflected these aforementioned components, regardless of whether the study identified itself as a mindfulness-based intervention or not. Studies that utilized mindfulness techniques as either a primary *or* ancillary component of the treatment were considered eligible for this study. Some identified therapies with *primary* use of mindfulness techniques that were considered eligible for inclusion were MBCT and mindfulness-based stress reductions (MBSR). Some

identified therapies that used mindfulness as an *ancillary* component to the treatment included acceptance and commitment therapy (ACT) and dialectical behavior therapy (DBT). Less structured mindfulness programs, such as mindfulness self-help programs or mindfulness support groups, also qualified as eligible for inclusion.

To be eligible for inclusion, studies must also have used these treatments with samples comprised exclusively of soldiers or veterans. The present meta-analysis identified "soldiers" as any individuals who currently or previously served in a national military, regardless of country of origin, military branch, or occupational role. Studies were included regardless of whether soldiers had a formal diagnosis, regardless of what mental or physical condition was being treated, and regardless of comorbidity. In addition to treating soldiers with mindfulness, studies must have also provided data from at least one outcome measure, with the reported data being sufficient to calculate effect sizes. The studies were not required to treat a specific categorical diagnosis, *per se*, although some did. Both categorical diagnostic status and continuous symptom severity data met criteria as eligible outcome data, as did a variety of other psychosocial outcomes (see Appendix B).

Only experimental studies that used a randomized design, in which outcome data of soldiers receiving mindfulness was presented as well as outcome data for a control group of soldiers receiving an alternative intervention or no intervention. Naturalistic studies, in which mindfulness was provided for soldiers with only pre- and post-test data but no control group was included, were not eligible. Studies utilizing either individual or group formats of mindfulness therapy were included, but single or multiple case studies were excluded. Studies in which participants received mindfulness in addition to psychiatric medication were also included. In

addition, studies in which participants received mindfulness along with another psychosocial intervention were included.

Eligible studies included studies published in peer-reviewed journals, published or unpublished doctoral dissertations, as well as published or unpublished poster abstracts. Eligible studies must have been conducted after mindfulness was first introduced to the realm of clinical psychology, which was identified as 1979, when Jon Kabat-Zinn founded the first mindfulness psychological treatment program. Therefore, the time frame for eligible studies was from 1979 to the present.

Procedure

To obtain studies for the meta-analysis, three major scholarly search engines, PsycINFO, MEDLINE, and the Cochrane Central Register of Controlled Trials (CENTRAL) were used. Studies yielded from these searches included published articles from academic journals, published and unpublished doctoral dissertations, and published and unpublished poster abstracts. For PsycINFO and MEDLINE, search terms consisted of the following: (mindfulness* OR mindful* OR MBSR OR MBCT OR accept*) AND (meditation* OR program* OR therap* OR interven* OR treatment* OR psychotherap* OR counseling OR counselling) AND (combat* OR veteran OR veterans OR troop OR troops OR war OR wars OR military OR ex-military OR army OR soldier OR soldiers OR peacemaker OR peacemakers) AND (randomized controlled trial OR randomised controlled trial OR controlled clinical trial OR randomized OR randomised OR randomly OR RCT OR randomized clinical trial OR randomised clinical trial). The same search terms were used for the Cochrane search, except for the terms starting after the last "AND," i.e., the terms that reference RCT designs, since the Cochrane database includes only

controlled trials. In order to be more conservative and inclusive, the option “Search All Text” in the Cochrane database was selected instead of the default “Title, Abstracts, and Keywords.”

The next step entailed reviewing the abstracts of all identified studies to determine their potential relevance to the meta-analysis. All studies that clearly did not use a mindfulness-based intervention with soldiers in a controlled trial were automatically excluded. Studies that could not be automatically ruled out with information from the abstracts were retrieved in Portable Document Formats (PDFs). Then, these studies were examined in full text using the present meta-analysis’s eligibility criteria to make final eligibility determinations. Following the electronic searches, manual searches of five major academic journals were conducted. The journals were selected based on whether they yielded at least two eligible (or likely eligible) studies during the electronic database searches. The manually searched journals included *Journal of Alternative and Complementary Medicin*e; *Journal of Clinical Psychology*; *Psychological Trauma: Theory, Research, Practice and Policy*; *Medical Care*; and *Mindfulness*. The manual search was conducted using all issues of these journals published in 2015 and 2016.

Once all eligible studies were retrieved, they were coded for study level as well as effect size level data. Study level data consisted of information about the structural characteristics of the study and participant characteristics (see Appendix A). Examples of structural characteristics included: identifying the type of mindfulness-based intervention used; whether the mindfulness-based intervention was used in conjunction with pharmacological treatments or not; the type of control group used; whether participants were matched on some variable or not when being randomized; the length of the treatment; the sequence of treatment, including whether participants received some type of treatment before or after they received a mindfulness-based intervention; what the other treatments used were; attrition at posttest; therapist competency

level, as well as whether therapist competency checks were conducted; whether fidelity checks were conducted; the total sample size of the study after attrition; the year the study was conducted; where the study was conducted; and whether the study was published or not.

Examples of participant characteristics included the mean age of the sample at the time of treatment; the predominant race of the sample; the gender of the sample; participant diagnostic status; whether participants had secondary or multiple diagnoses or conditions; the presenting mental or physical health condition being treated; the soldiers' occupational roles in the military; disability status of participants; participant active duty or veteran status; whether the soldiers had been exposed to combat or not; the branch in which the soldiers served; and how long the soldiers had served.

Examples of codes for effect size characteristics (see Appendix B) included what outcome measures were administered; whether measures were self-report or informant report measures; when these measures were administered; whether follow-up measures were also administered and how long after treatment they were administered; how the data were reported statistically; the sample size of both the treatment and control groups; and whether the results for each outcome measure were consistent with the a priori hypothesis of the present meta-analysis, i.e., that mindfulness-based interventions will yield significantly better outcomes than control groups for soldiers.

The principal investigator coded each of the studies included in the meta-analysis. Three volunteers with bachelor's degrees in psychology coded a random subsample of the studies in order to determine inter-rater reliability of the coding process. The three coders underwent the same training, which consisted of an overview of the study level and effect size level coding forms, as well as practice coding on actual studies that were included in the meta-analysis.

After the coding process was completed, inter-rater reliability analyses were conducted. Discrepancies found between the coders were reviewed by the principal investigator and the alternative coder, and a decision as to the final code was made.

Data Analysis

All coded study level information was analyzed to provide descriptive information on included studies as well as to determine potential moderators of treatment outcome. Effect size data was standardized in order to determine the magnitude of the effects across all studies. Cohen's *d* was the statistic used to assess overall differences between the treatment groups and the control groups—specifically, to test the hypotheses that mindfulness-based interventions are as efficacious as other empirically-supported treatments and more efficacious than treatment-as-usual and no-treatment control groups with soldiers. The formula for this analysis is as follows: $d = \frac{\bar{x}_1 - \bar{x}_2}{s}$. where the mean post-test (or follow-up) outcome score of the control group is subtracted from the mean post-test (or follow-up) outcome score of the treatment group, and then divided by the pooled standard deviation (Borenstein, Hedges, Higgins, & Rothstein, 2009). All effect sizes were coded as positive when the data were consistent with the a priori hypothesis of the present meta-analysis (i.e., that mindfulness-based interventions will yield significantly better outcomes than treatment-as-usual or no-treatment control groups for soldiers) and negative when the data were *in*consistent with this hypothesis. Three sets of effect size calculations were conducted, i.e., between-groups comparisons for data gathered at (a) pre-test (in order to determine if groups differed on outcome variables prior to receiving the intervention despite the use of randomization), (b) post-test, and (c) follow-up.

Inter-rater reliability was calculated using intra-class correlation coefficients (ICCs) for continuous data, and Kappa (or percentage agreement when Kappa could not be calculated when

the data violated assumptions for its calculation) for categorical data. When studies had more than one effect size, the effect sizes for that study were averaged to obtain an average effect size for that study (Horvath & Symonds, 1991; Martin, Garske, & Davis, 2000), thus meeting the statistical assumption of independence. Effect sizes from each study were then combined to create a total effect size for all studies included in the analysis. Given the benefits of random effects methods, which include the argument that they are more representative (National Research Council, 1992) and generalizable to real world data than their fixed effect counterparts (Hedges & Vevea, 1998), effect sizes were aggregated across studies using this method (Hedges & Olkin, 1985; Hedges & Vevea, 1998). The second version of Comprehensive Meta-Analysis software (Borenstein, Hedges, Higgins, & Rothstein, 2005) was used to conduct all analyses.

To address the criticism that meta-analyses may be biased in favor of studies that demonstrate positive findings (Rosenthal, 1991), several analyses were conducted to assess for the possibility of publication bias. These analyses included Sterne's funnel plot display analysis (Sterne & Egger, 2001; Sterne & Harbord, 2004), Begg and Mazumdar's (1994) rank correlation, Egger's regression intercept analysis (Egger, Davey Smith, Schneider, & Minder, 1997), and Duval and Tweedie's (2000a, 2000b) trim and fill procedure. All four publication analyses were conducted to be conservative.

To examine categorical moderators, studies were divided into the relevant subgroups. Differences in effect sizes between the subgroups were analyzed for statistical significance using the $Q_{betweeen}$ statistic (Borenstein et al., 2009). Values for weights across all of the categorical subgroups were pooled when a subgroup for a categorical moderator included less than six studies (cf., Borenstein et al., 2009). This was done because this method will likely yield more accurate results than calculating separate weights for the different subgroups (Borenstein et al.,

2009). To be conservative, each level of the moderator variable was treated as independent of each other (M. Borenstein, personal communication to M. Diener, January 1, 2010).

Mixed effects (method of moments) meta-regression analyses that examined the relation between effect sizes for each study and continuous moderator variables were used for continuous moderator analyses (Borenstein et al., 2009). For all analyses conducted for the present meta-analysis, *p*-values are reported as two-tailed unless otherwise indicated.

Results

A total of 859 independent articles were identified through the electronic database search. An additional six independent, non-duplicate studies were identified through the manual search. Of these articles, a total of 63 were retained for closer analysis. Twenty-five of these studies met eligibility criteria for inclusion in the meta-analysis and were coded. These studies are marked with an asterisk in the reference section. A full description of the study procurement process is found in Figure 1.

Of the eligible studies, three were comprised of data from separate publications. In addition, one published article included four separate studies, each of which met eligibility criteria for inclusion in the meta-analysis (Heffner, Crean, & Kemp, 2016). Two studies were unpublished dissertations, one was an unpublished abstract from a poster presentation, one was a published abstract from a poster presentation, and the rest were published journal articles.
At pre-test (Table 1), the number of participants across all eligible studies was 1,041.92 (note that this number is not an integer, since studies included multiple effect sizes, some of which varied in terms of specific sample sizes due to missing data etc.; the sample sizes reported here are the sum of the sample sizes reported in each study, which were first averaged at the study level when sample sizes in a given study varied across outcome data). The mean number of

participants per study was 27.42 (SD = 20.07), with a range of 8 to 140.50 participants. When only the largest sample size of individual studies that had varying sample sizes were included in calculation of descriptive statistics for the present meta-analysis, the total number of participants across all eligible studies was 1,059.50 (note that this number, too, is not an integer; see note in Table 2). The mean number of such participants per study was 27.89 (SD = 20.21), with a range of 8 to 140.50.

At post-test (Table 1), the sum of the average number of participants across all eligible studies was 1,087.37. The mean number of participants per study was 24.72 (SD = 17.86) with a range of 8 to 114.25 participants. When only the largest sample size of individual studies that had varying sample sizes were included in calculation of descriptive statistics for the present meta-analysis, the total number of participants across all eligible studies was 1,111 (Table 2). The mean number of such participants per study was 25.25 (SD = 18.02), with a range of 8 to 139 participants. Attrition, defined as the number of participants who completed outcome measures at pre-test but not at post-test, in the mindfulness and control groups was also calculated. The mean attrition for the mindfulness-based intervention groups was 26.81% and the mean attrition for the control groups was 19.82%.

Finally, at follow-up (Table 1), the sum of the average number of participants across all eligible studies was 550.91. The mean number of participants per study was 27.55 (SD = 16.67) with a range of 9 to 112. When only the largest sample size of individual studies that had varying sample sizes were included in calculation of descriptive statistics for the present meta-analysis, the total number of participants across all eligible studies was 621 (Table 2). The mean number of such participants per study was 31.05 (SD = 18.33), with a range of 9 to 116. The skewness,

kurtosis, and medians at pre-test, post-test, and follow-up for both the average and largest sample sizes of the studies can be found in Tables 1 and 2, respectively.

Effect sizes for between-group differences were calculated for each of the 22 independent eligible studies at post-test, and when information was available at pre-test and follow-up. Weighted mean effect sizes were calculated, with 0.20 considered to be a small effect, 0.50 considered to be a medium effect, and 0.80 considered to be a large effect (Cohen, 1988). All *p* values for heterogeneity tests were one-tailed. Otherwise, all *p* values reported in the present meta-analysis were two-tailed.

Results indicated no demonstrable differences in pre-test outcome measures between participants randomized to the mindfulness-based groups or control groups (weighted mean d = -0.02, 95% CI [-0.15-0.10], Z = -0.37, p = .71, k = 19). The results did, however, indicate—as predicted—that participants who received a mindfulness-based intervention demonstrated better outcomes at posttest relative to participants in control groups (weighted mean d = 0.26, 95% CI [0.11-0.41], Z = 3.35, p = .001, k = 22). A similar pattern of results was found at follow-up (weighted mean d = 0.29, 95% CI [0.02-0.56], Z = 2.08, p = 0.04, k = 10).

Results demonstrated variation based on the type of outcome that was assessed. At post-test, significant effects were found for outcome measures of symptoms of depression (p = 0.02) and symptoms of PTS (p =.002), but not for symptoms of non-PTS anxiety (p =.36). Significant effects were also found for "other" outcomes (p <.001), which consisted of a wide variety of variables (including psychological health quality of life, social relationship quality of life, environmental quality of life, degree of internalized anger, dissociation, borderline personality disorder diagnosis, number of hospitalizations, mental quality of life, somatization symptoms, spiritual well-being, self-efficacy, impulsivity, emotion regulation, perceived stress, negative

affect, positive affect, sleep quality, symptom change, reported benefit from study, any anxiety disorder or civilian PTS, suicide attempts, fatigue, cognitive failures, variety of mental health symptoms, alcoholism, anger, insomnia, and pain). While the results overall remained clinically significant at follow-up, none of the results of the outcome measures individually were significant at follow-up, likely due to low power. Consequently, when the outcome measures were analyzed together, sufficient power was present to detect the true effect of the mindfulness-based intervention. The specific findings that were not significant at follow-up at the individual level were symptoms of depression and PTS (p = 0.29 and 0.27, respectively). Further, symptoms of non-PTS anxiety remained nonsignificant (p = 0.68). Moderator analyses for the outcome of overall well-being/quality of life were not calculated because only one study used that outcome. The results of the tests for heterogeneity at pre-test (Q[18] = 11.97, p = .85) and post-test (Q[21] = 28.62, p = .12) demonstrated statistically significant variation in effect sizes across studies. Effect size data at pre-, post-test, and follow-up can be found in Tables 3, 4, and 6, respectively. Forest plots of these results at post-test and follow-up can be found in Tables 5 and 7, respectively.

Inter-rater reliability analyses demonstrated results that ranged between excellent and poor reliability among the different codes. These benchmarks were determined based on commonly accepted guidelines (Cicchetti, 1994; Fleiss, 1981), with ICCs and kappas falling between 0.75 and 1.00 considered to be excellent, between 0.60 to 0.74 considered to be good, between 0.40 and 0.59 considered fair, and below 0.40 considered poor. The results in the present study indicated (see Table 8) that reliability for all effect size level codes was in the excellent range except for the equivalent outcome code, which determined whether the groups were equivalent at baseline on all outcome measures and which fell within the poor range of

reliability. In terms of the study level moderator codes, excellent reliability was found for primary diagnosis (k = 0.84). However, none of the studies actually targeted symptoms of depression or anxiety as the primary treated-condition, so the moderator analysis with this variable was not conducted. Good reliability was found for treatment format (k = 0.66). For the dosage of mindfulness intervention received variable and the study quality variable, reliability was too poor to consider running moderator analyses with these variables. Complete inter-rater reliability data for the study level and effect size level can be found in Table 8.

With regard to the only study level moderator analysis that could be completed as a result of sufficient inter-rater reliability—i.e., treatment format—only three studies were coded as individual treatment. Therefore, the within group estimates of tau-squared were used in this analysis, as explained in the Method section. While the results suggested that the aggregated effect size for group treatment at posttest was statistically significant (average weighted d = 0.23, 95% CI [0.07-0.39], $Z = 2.75$, $p = .006$), and the aggregated effect size for individual treatment was not (average weighted d = 0.25, 95% CI [-0.12-0.61], $Z = 1.33$, $p = .18$), the statistical contrast between these two aggregated effect sizes was not statistically significant (Q[1] = 0.009, $p = .93$).

While the overall findings for the efficacy of mindfulness-based treatment at post-test and follow-up were significant, caution should be used in interpreting these results given the results of the publication bias analyses. At post-test, Begg and Mazumdar's (1994) rank correlation method was significant, (Kendall's tau [with continuity correction, Borenstein et al., 2005] = 0.30, *p*[one-tailed] = 0.02 Borenstein et al., 2009)), as was Egger's (Egger et al., 1997) regression intercept method (intercept = 1.53, *p*[one-tailed] = 0.01). Additionally, results from Duval and Tweedie's (2000a, 2000b) trim and fill procedure suggested that publication bias may

have played a role in the magnitude of the effect sizes (six studies were trimmed, and the adjusted point estimate for the weighted mean d was 0.14, CI [-0.04-0.42]). A funnel plot of this data at post-test can be found in Figure 2. At follow-up, Begg and Mazumdar's (1994) rank correlation method was significant, (Kendall's tau [with continuity correction, Borenstein et al., 2005] = 0.49, *p*[one-tailed] = 0.02) as was Egger's (Egger et al., 1997) regression intercept method (intercept = 2.87, *p*[one-tailed] = 0.03). However, results from Duval and Tweedie's (2000a, 2000b) trim and fill procedure suggested that the impact of potential publication bias was likely minimal at this time point (zero studies were trimmed, and the adjusted and observed estimates of effect size were identical).

Discussion

Through compilation of results from very different individual studies of mindfulness-based interventions with soldiers, this meta-analysis was able to make a concise, comprehensive determination about the intervention's efficacy with this population. Overall, the results indicate that mindfulness-based interventions seem to be an efficacious treatment for soldiers, in particular with regard to reducing symptoms of PTS and depression. There are, however, a couple of important caveats. First, there was evidence of potential publication bias, suggesting that the efficacy of mindfulness-based interventions for soldiers may be lower than demonstrated in our primary analyses. Consequently, it appears as though randomized controlled studies of mindfulness-based interventions with soldiers were more likely to be accepted for publication if they yielded significant results. Studies that did not yield significant results, conversely, were seemingly more likely to be ignored and left unpublished. Therefore, the implications of this meta-analysis indicate that mindfulness-based interventions for soldiers may not be quite as

effective as the results may suggest. This is an important consideration for practitioners interested in using this treatment with soldiers.

Although not directly comparable, the findings of the present meta-analysis may suggest that mindfulness-based interventions for soldiers with various clinical presentations are not quite as effective as other treatments for combat soldiers with symptoms of PTS specifically (Goodson et al., 2011), since the effect sizes of exposure-based treatments for symptoms of PTS with soldiers has been shown to be between medium (Goodson et al, 2011) to large (Haagen et al., 2015). Given the fact that many first-line treatments have high nonresponse and drop-out rates (Steenkamp, Litz, Hoge, & Marmar, 2015), the fact that mindfulness-based interventions may be an effective alternative treatment for soldiers is an important finding. Consequently, mindfulness-based interventions may prove successful for soldiers who do not respond to the treatments that are already backed by a substantial body of empirical evidence. Some research suggests that many soldiers with symptoms of PTS do not respond or drop-out of mental health treatment due to feeling stigmatized, that the treatment was not working as intended, or that the treatment-deliverer was unattuned to the soldier (Hoge et al., 2014). Reading between the lines, it seems plausible that first-line treatments for many soldiers with mental illness may likely be perceived as too intense, as soldiers may not be ready to confront their most private issues face-to-face with a therapist. As mindfulness-based interventions are present-moment focused and do not explicitly require soldiers to divulge painful experiences before they are ready to do so, they may therefore be more appealing to soldiers.

Other reasons cited by soldiers for disengaging with traditional mental health treatment are soldiers feeling they could handle problems on their own, work interference, and insufficient time with the mental health professional (Hoge et al., 2014). One common theme among these

variables is that of the importance of soldier autonomy in overcoming mental health issues. Some strengths of mindfulness-based interventions can be emphasized to address these concerns, specifically mindfulness' emphasis on responding internally to one's own needs arising in the present moment. Indeed, once the principles of mindfulness are taught, the practice is intended to very much be a self-directed activity. The emphasis in mindfulness-based interventions is on the self—one's own body sensations, perceptions, thoughts, and feelings, and learning how to respond to those stimuli without judgment (Hofmann et al., 2010). Once the principles are learned, mindfulness is something soldiers can do and are expected to do on their own, which clearly addresses the treatment disengagement reasons of soldiers wanting to manage their problems on their own and work interference. It seems reasonable to maintain that individual practice should be emphasized throughout all stages of mindfulness-based intervention with soldiers, especially considering the drop-off rates post-treatment (Kabat-Zinn, Lipworth, Burney, & Sellers, 1987; Sephton et al. 2007). Another potential benefit of engagement in mindfulness-based interventions for soldiers with symptoms of PTS is that it can provide such individuals with a healthy means of regulating intense emotions. The importance of this factor is evidenced in the protocol of the empirically supported PTS treatment of prolonged exposure, which teaches clients "breathing retraining" in the first session as a means of facilitating adaptive management of distress and promoting relaxation before individuals begin to confront and process their traumas through exposure (Berenz, Rowe, Schumacher, Stasiewicz, & Coffey, 2012).

Another interesting finding from the meta-analysis was that significant results were found for symptoms of PTS, symptoms of depression, and a whole host of "other" variables, but not for symptoms of non-PTS anxiety. These findings appear to be inconsistent with the literature on the effect mindfulness-based interventions with civilian populations (Hofmann et al., 2010; Khoury

et al., 2013) that influenced this meta-analysis' hypothesis that soldiers with symptoms of general anxiety and depression would experience more treatment gains than soldiers with other forms of psychopathology. That literature suggested that mindfulness-based interventions have been shown to be primarily effective at reducing symptoms of non-PTS anxiety but did not identify such interventions as effective at reducing symptoms of PTS symptoms in civilian populations, findings which were reversed in this study. In the original meta-analyses of mindfulness-based interventions with civilians, little evidence was found in support of mindfulness-based interventions for symptoms of PTS. This was most likely due to the fact that there was a significant lack of research in this area. Indeed, of the two major meta-analyses of mindfulness-based interventions with civilians (Cavanagh et al., 2014; Hofmann et al., 2010), only one individual study targeted symptoms of PTS. Since those meta-analyses were conducted, a more recent meta-analysis of mindfulness-based interventions in civilian populations yielded results similar to those in the present meta-analysis (Hilton et al., 2016). Specifically, Hilton and colleagues (2016) demonstrated that mindfulness-based interventions were most effective at reducing symptoms of PTS and depression but not symptoms of non-PTS anxiety for civilians, which is what the present meta-analysis found for soldiers. These findings suggest that mindfulness-based interventions yield similar outcomes for both soldiers and civilians.

While differences in treatment efficacy based on clinical presentation are important to consider, conceptualizations of mental health conditions as discrete "diagnoses" such as PTS, depression, and non-PTS anxiety, are starting to change. Consequently, researchers are now beginning to understand that symptoms of anxiety and depression could be components of PTS rather than as distinct entities. As science evolves to develop enhanced understandings of mental health conditions, the way outcomes are assessed and treated will evolve as well (Byllesby,

Durham, Forbes, Armour, & Elhai, 2016). Many of the studies included in this meta-analysis appear to be based on older conceptions of psychopathology, as the mean study year of included studies was the year that the fifth edition of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5) was published, 2013 (American Psychiatric Association, 2013). Therefore, future research should address the degree to which newer views on psychopathology may moderate the role that mindfulness-based interventions have on clinical outcomes with soldiers.

While mindfulness-based interventions seem to be effective in similar domains for both civilians and soldiers, a significant discrepancy between the two populations exists. Consequently, mindfulness-based interventions have been shown to demonstrate a medium (Khoury et al., 2013) to large effect with civilians (Hofmann et al., 2010; Vøllestad, Birkeland Nielsen, & Høstmark Nielsen, 2011) but a small effect with soldiers in the present meta-analysis. This finding parallels research on first-line treatments for symptoms of depression (Hundt, Barrera, Robinson, & Cully, 2014) and PTS (Steenkamp et al., 2015), which also suggest that soldiers may experience less treatment gains than civilians. Multiple reasons, which may be applicable in the case of mindfulness-based interventions for soldiers as well, have been postulated to explain this outcome. First, the life of a soldier is a culturally unique experience, which may complicate the clinical treatment picture. Many soldiers engage in multiple deployments (Olenick, Flowers, & Diaz, 2015), which are stressful even if they do not lead to combat exposure. Of those soldiers who are exposed to combat, the traumas are often chronic and complicated by feelings of complex guilt revolving around morally compromising experiences (Steenkamp et al., 2015). Soldiers are also more likely to suffer from complex medical and mental comorbidities which can muffle treatment gains (Hundt et al., 2014; Steenkamp et al., 2015). Additional factors which can impact soldiers' response to mental health

treatment include the marked prevalence of homelessness, abuse histories, unemployment, suicide risk, and substance abuse among this population (Hundt et al., 2014). Given these factors, that soldiers receiving mental health care would experience less treatment gains than civilians is not surprising.

For many soldiers with such convoluted presentations, long-term psychodynamic therapy—with its intensive attention to personality functioning more broadly and in a transdiagnostic manner—may be warranted. Such interventions include a focus on cultivating a collaborative working alliance with patients, which may help soldiers to reintegrate back into civilian life and over time experience relief from their most distressing symptoms (Kudler, 2007). With civilians, the focus on brief psychotherapies and psychiatric medications does not appear to be reducing rates of depression (Clark, Goodman, & Petitti, 2017) or many other mental illnesses (Whitaker, 2012). Indeed, longer-term treatments such as psychoanalytic and psychodynamic therapies have been shown to produce longer lasting results than short-term approaches with civilians (Leichsenring & Rabung, 2011). These more intensive treatments have even been shown to be substantially more cost-effective than brief treatments in the long-term in civilian populations (de Maat, Philipszoon, Schoevers, Dekker, & De Jonghe, 2007). With soldiers, brief psychodynamic approaches have been proposed (Carr, 2011; Hendin, 2017). While few studies have explored the efficacy of brief psychodynamic interventions with soldiers, there is also no explicit evidence demonstrating that they are any less effective than the current empirically supported therapies for soldiers (Sharpless & Barber, 2011). More research is indeed needed to determine how longer-term psychodynamic treatments fare with soldiers.

In terms of the moderator analysis of treatment format that was conducted in the present meta-analysis, no significant differences existed between soldiers receiving mindfulness-based

therapy in group or individual modalities. This finding is at odds with the previous literature (Haagen et al. 2015) of soldiers with symptoms of PTS, but actually is in accordance with the literature on mindfulness-based interventions, which suggest that no difference in treatment efficacy exists between MBCT delivered in groups or individually (Schroevers et al., 2016). Still, the issue of what treatment format best suits soldiers requires further research, given the fact that only three studies provided individual mindfulness-based therapy and moderator analyses in meta-analysis are notoriously underpowered (Borenstein et al., 2009). If future research demonstrates it to be the case that soldiers respond equally as well in group or individual settings, the implications would be that mindfulness-based interventions may be more well-received by soldiers than the more traditional first-line therapies studied by Haagen and colleagues (2015). Specifically, soldiers may feel more comfortable in a group setting receiving a mindfulness-based intervention due to less of a fear of stigma or confidentiality issues, which have been cited as reasons for soldier disengagement from treatment (Hoge et al., 2014) and may have been more prevalent in a traditional type group treatment setting. Indeed, with traditional treatments for soldiers with symptoms of PTS, the emphasis on sharing traumas may be triggering and more difficult for soldiers (Haagen et al., 2015), a factor that would not be at play with mindfulness-based therapies in the group setting.

Despite the potential limitations of traditional group therapy with soldiers, the literature has also identified benefits of such therapy. For instance, there is some evidence that suggests that soldiers presenting with symptoms of PTS who initiated with group therapy on average received more therapy than soldiers who initiated with individual therapy. This suggests that soldiers in individual therapy are more likely to drop-out before they can experience treatment gains (Sripada, Bohnert, Ganoczy, Blow, Valenstein, & Pfeiffer, 2016). While soldiers receiving

group therapy for symptoms of PTS may face discomfort sharing difficult memories and feelings in the presence of others (Haagen et al., 2015), the group may also provide a source of support for soldiers, not to mention the exposure implications for soldiers with avoidance symptoms (Sripada et al., 2016).

Another implication of the potential finding that treatment format does not affect treatment outcome for soldiers receiving mindfulness-based interventions is that more soldiers may be able to benefit from treatment, as the group format is ostensibly more cost-effective than the individual modality. This is especially important in a Veteran's Affairs system which is increasingly concerned with improving access to mental health care for soldiers (Kehle, Greer, Rutks, & Wilt, 2011). Nonetheless, individual therapy for soldiers with PTS still appears to be disproportionately preferred and offered over group therapy at Veterans Affairs Medical Centers (VAMCs; Sripada et al., 2016), an outcome likely related to the personal issues soldiers may face in group settings (Hoge et al., 2014).

One limitation of this meta-analysis was the poor inter-rater reliability for many of the study level codes, especially for two of the four codes that were proposed a priori for use in the moderator analyses. As a result of these findings, those two moderator analyses were not conducted, and thus the effect of study quality and dosage of mindfulness intervention on outcome could not be determined. Anecdotally, it was clear that the coders often struggled to differentiate between coding "cannot tell" or "no" for variables that asked whether a characteristic was present in the study or not, for example, whether all participants in either group were taking psychiatric medications or not. To continue with this example, many studies would neglect to explicitly mention whether participants were using prescribed psychiatric medications, in which case coders struggled to apply a code of "cannot tell," which would be the

correct code in this instance, or "no," in accordance with the logic that if psychiatric medications were not mentioned, no participant was taking them. Providing coders with more specific training around these intricacies would likely have yielded greater inter-rater reliability. Alternatively, many of the variables with poor inter-rater reliability would likely have realized enhanced reliability if they were coded continuously rather than categorically. For instance, in retrospect, most studies had significant variation in factors such as psychiatric medication usage among participants and reported such data in percentages. It is likely that reporting the percentages and defaulting to "cannot tell" when such percentages were not reported would have been much easier for coders than trying to decide between "no" and "cannot tell."

Interestingly, this latter solution may nonetheless not have always yielded the desired effect, as indicated by an observational analysis of the poor inter-rater reliability for the moderator variable of dosage of mindfulness-based intervention received. In each case of rater disagreement for this variable, which was coded continuously, one rater decided upon an actual "number of hours of intervention received" while the other coded "cannot tell." While there was indeed some ambiguity in the way researchers presented the data for this variable, enhanced inter-rater reliability would have likely been achieved with the creation of firmer coding guidelines for this variable and more time devoted to training on it. To make this variable more coder-friendly, the coding guidelines could have indicated that a code of "cannot tell" should only be applied in cases in which no numerical data on dosage of mindfulness-based intervention received was reported. This would encourage coders to make their best estimate with the data presented, which likely would likely result in greater congruence between raters.

As for the study quality variable, a couple of factors likely influenced the low reliability exhibited. First, the study quality variable was the last variable on the study level coding form,

which was likely the last variable coded for this reason as well as the fact that coders were trained to complete the effect size coding form first. Consequently, rater fatigue may have played a role in the poor reliability, especially since the study quality variable was comprised of multiple, somewhat convoluted coding steps. In retrospect, coders would have benefited from more time devoted to training for this variable and perhaps instruction to code it sooner rather than later in their coding.

Another important consideration of this meta-analysis is that moderator outcome analyses were not conducted for any of the codes in which moderator hypotheses were not explicitly identified a priori. This was the case regardless of whether the code demonstrated sufficient or poor inter-rater reliability. For example, the inter-rater reliability for attrition in the mindfulness group and control group was excellent and fair, respectively, but these analyses were not conducted. Therefore, no determination was able to be made about how an important potential moderating variable, attrition, may affect outcome. Future analyses would benefit from examining how all of the coded variables may influence the effects of the treatment.

Another limitation of the study is that outcome variables related to biological factors or physical health were excluded, since the presentation of such data in the literature was highly complex and difficult to determine if it could be converted into standardized effect sizes. Exclusion of this outcome data limits the scope of the understanding of the potential benefits of mindfulness-based interventions for soldiers. For instance, research has demonstrated that mindfulness-based interventions with civilians may improve immune functioning (Black & Slavich, 2016) and provide physical health benefits for a host of medical issues (Grossman, Niemann, Schmidt, & Walach, 2004). If research can support these same findings for soldiers, mindfulness-based interventions for soldiers will be that much more clinically indicated.

Future research would profit from adhering to the previously mentioned limitations: changing some categorically-coded variables to continuous, devoting more time to training raters on coding, analyzing the potential impact of many of the other coded variables on treatment outcome, and including physical health and biological outcome data in their analyses. Lastly, future research would benefit from identifying ways that mindfulness-based interventions address and correct for common barriers to mental health treatment for soldiers found in more traditional approaches.

In conclusion, mindfulness-based interventions show promise in helping soldiers reduce symptoms of PTS and depression as well as experience improvements in various psychosocial domains. While the effects of mindfulness-based interventions may not have been as robust as that of more traditional therapies (Goodson et al., 2011), the high nonresponse and drop-out rates in these treatments (Steenkamp et al., 2015) suggest that mindfulness-based interventions may be an important alternative treatment approach. Evidence of publication bias in the literature suggests that the effects of mindfulness-based interventions with soldiers may not be as robust as they initially appear. Another important consideration is that mindfulness-based interventions may not be as effective for soldiers as they are for civilians, a finding echoed in the literature of more traditional treatments for soldiers (Hundt et al., 2014; Steenkamp et al., 2015). Overall, this meta-analysis expands our understanding of the efficacy of an alternative mental health treatment intervention for soldiers, which can improve the quality of lives of countless individuals who sacrifice for our country.

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Appendix A

Study Level Coding Form

1. Study ID number [STUDYID]

2. Please indicate whether the study matched the treatment and control group on some characteristic(s) [DEPEND]
    a) No
    b) Yes

3. Type of publication [PUBTYPE]
    a) Book
    b) Book chapter
    c) Journal article
    d) MA thesis or doctoral dissertation
    e) Published or unpublished abstract from poster presentation

4. Publication year (999 if unknown)? [PUBYEAR]

Sample Descriptors

5. Mean Age [MEANAGE; code the mean age of both treatment groups; code "Cannot tell" if cannot tell]

6. Percentage white [RACE; code to two decimal places, e.g., code "50.24" if percentage white is 50.24%; code "Cannot tell" if cannot tell]

7. Percentage female [GENDER; code to two decimal places, e.g., code "50.24" if percentage female is 50.24%]; code "Cannot tell" if cannot tell)

8. Type of mindfulness-based intervention used [MBT]
    a) Mindfulness-Based Stress Reduction (MBSR)
    b) Mindfulness-Based Cognitive Therapy (MBCT)
    c) Mindfulness-Based Relapse Prevention (MBRP)
    d) Dialectical Behavior Therapy (DBT)
    e) Acceptance and Commitment Therapy (ACT)
    f) Mindful Meditation (MM)
    g) Mantram Repetition Program (MRP)
    h) Other (write in):_____

9. Format of the mindfulness-based treatment group [FORMAT_MIND]
    a) Individual intervention
    b) Group intervention
    c) Internet-based intervention

d) Individual & Group intervention
e) Other (write in):_____
f) Cannot tell

10. Format of the control group [FORMAT_CTRL]
    a) Individual intervention
    b) Group intervention
    c) Internet-based intervention
    d) Individual & Group intervention
    e) Other (write in):_____
    f) Cannot tell

11. Dosage or amount of formal intervention for experimental group, not including unsupervised home practice (number of hours): [DOSAGE_MIND] (Use "999" if cannot tell)

12. Dosage or amount of formal intervention for control group, not including unsupervised home practice (number of hours): [DOSAGE_CTRL] (Use "999" if cannot tell)

13. Was the intervention group required to practice mindfulness on their own outside of treatment for homework? [HMWK_MIND]
    a) Yes
    b) No
    c) Cannot tell

14. Was the control group required to engage in any extra-therapy activities on their own outside of treatment for homework? [HMWK_CTRL]
    a) Yes
    b) No
    c) Cannot tell

15. Were all participants in the experimental group treated with pharmacological medications in tandem with the mindfulness-based intervention? [MEDS_MIND]
    a) Yes
    b) No
    c) Cannot tell
    d) Mix

16. Were all participants in the control group treated with pharmacological medications? [MEDS_CTRL]
    a) Yes
    b) No
    c) Cannot tell
    d) Mix

17. Type of comparison group [CGTYPE] (Use "999" if cannot tell)
    a) No treatment control group

b) Waiting list
c) Active control group (write in): _____

18. Components of mindfulness-based intervention [COMPON]
    a) Full mindfulness-based intervention (i.e., MBSR, MBCT)
    b) Single component of a mindfulness-based intervention (e.g., body scan, mindful breathing)
    c) Other therapy with mindfulness as a smaller component (e.g., DBT, ACT)
    d) Other (write in): _____

19. Treatments received concurrently with the mindfulness-based intervention (do not include treatments like DBT or ACT in which mindfulness is a smaller component of a larger treatment modality) [CONTX_MIND]
    a) None
    b) Exposure therapy
    c) CBT
    d) CPT
    e) Other (write in): _____
    f) Cannot tell

20. Treatments received concurrently with the control group intervention (do not include treatments like DBT or ACT in which mindfulness is a smaller component of a larger treatment modality) [CONTX_CTRL]
    a) None
    b) Exposure therapy
    c) CBT
    d) CPT
    e) Other (write in): _____
    f) Cannot tell

21. Formal psychotherapy treatments received before the mindfulness-based intervention [PRIORTX_MIND]
    a) None
    b) Exposure therapy
    c) CBT
    d) CPT
    e) Other (write in): _____
    f) Cannot tell

22. Formal psychotherapy treatments received before the control group intervention [PRIORTX_CTRL]
    a) None
    b) Exposure therapy
    c) CBT
    d) CPT
    e) Other (write in): _____

f) Cannot tell

23. Formal psychotherapy treatments received after the mindfulness-based intervention [POSTTX_MIND]
    a) None
    b) Exposure therapy
    c) CBT
    d) CPT
    e) Other (write in): _____
    f) Cannot tell

24. Formal psychotherapy treatments received after the control group intervention [POSTTX_CTRL]
    a) None
    b) Exposure therapy
    c) CBT
    d) CPT
    e) Other (write in): _____
    f) Cannot tell

25. % Attrition for mindfulness-based intervention group (i.e., selected treatment group at post-test [ATTRIT_MIND; i.e., take the number of participants who completed the mindfulness-based intervention, divide by the number of participants who started the mindfulness-based intervention, and then multiply by 100, and code to 2 decimal places; Code "Cannot Tell" if cannot tell]

26. % Attrition for control group (i.e., selected treatment group at post-test [ATTRIT_CTRL; i.e., take the number of participants who completed the control group intervention, divide by the number of participants who started the control group intervention, and then multiply by 100, and code to 2 decimal places; Code "Cannot Tell" if cannot tell]

27. Therapist competency level in the experimental group [COMP_MIND]
    a) Mindfulness-expert Masters level therapist
    b) Non-mindfulness-expert Masters level therapist
    c) Mindfulness-expert doctoral level therapist
    d) Non-mindfulness-expert doctoral level therapist
    e) Other (write in):_____
    f) Cannot tell

28. Therapist competency level in the control group [COMP_CTRL]
    a) Masters level therapist
    b) Doctoral level therapist
    c) Other (write in):)_____
    d) Cannot tell

29. Was therapist competency to deliver the mindfulness-based intervention assessed? [COMPCHK_MIND]

a) No
b) Yes
c) Cannot tell

30. Was therapist competency to deliver the control group intervention assessed? [COMPCHK_CTRL]
    a) No
    b) Yes
    c) Cannot tell

31. Was therapist adherence in delivering the mindfulness-based intervention assessed? [ADHRCHK_MIND]
    a) No
    b) Yes
    c) Cannot tell

32. Was therapist adherence in delivering the control group intervention assessed? [ADHRCHK_CTRL]
    a) No
    b) Yes
    c) Cannot tell

33. Country of military enlistment [LOCATION]
    a) U.S.
    b) England
    c) Israel
    d) Canada
    e) Other (write in):_____
    f) Cannot tell

34. Did all participants have formal DSM diagnoses? [DXPRSNT]
    a) No
    b) Yes
    c) Mix
    d) Cannot tell

35. Primary diagnosis featured [DXFTRD]
    a) Depression
    b) Anxiety
    c) Personality Disorders
    d) Substance Abuse
    e) PTSD
    f) Mix
    g) None
    h) Other (write in): _____
    i) Cannot tell

36. Did all participants in the mindfulness-based intervention group have comorbid diagnoses? [COMORBID_MIND]
    a) No
    b) Yes
    c) Mix (i.e., some participants had comorbid diagnoses and some did not)
    d) Not applicable (no participants had even a primary diagnosis)
    e) Cannot tell

37. Did all participants in the control group have comorbid diagnoses? [COMORBID_CTRL]
    a) No
    b) Yes
    c) Mix (i.e., some participants had comorbid diagnoses and some did not)
    d) Not applicable (no participants had even a primary diagnosis)
    e) Cannot tell

38. Were all participants in the mindfulness group physically disabled? [DISABLED_MIND]
    a) No
    b) Yes
    c) Mix (i.e., some participants were disabled and some were not)
    d) Cannot tell

39. Were all participants in the control group physically disabled? [DISABLED_CTRL]
    a) No
    b) Yes
    c) Mix (i.e., some participants were disabled and some were not)
    d) Cannot tell

40. Military branch of participants in the mindfulness-based intervention group [BRANCH_MIND]
    a) Army
    b) Navy
    c) Air Force
    d) Coast Guard
    e) Marines
    f) Mix
    g) Other (write in):_____
    h) Cannot tell

41. Military branch of participants in the control group [BRANCH_CTRL]
    a) Army
    b) Navy
    c) Air Force
    d) Coast Guard
    e) Marines
    f) Mix
    g) Other (write in):_____

h) Cannot tell

42. Were all participants' in the mindfulness-based intervention group active duty or veterans? [ACTIVE_MIND]
    a) Active duty
    b) Veterans
    c) Mix
    d) Cannot tell

43. Were all participants' in the control group active duty or veterans? [ACTIVE_CTRL]
    a) Active duty
    b) Veterans
    c) Mix
    d) Cannot tell

44. Did participants in the mindfulness-based intervention group receive exposure to combat? [COMBTEX_MIND]
    a) No
    b) Yes
    c) Mix
    d) Cannot tell

45. Did participants in the control group receive exposure to combat? [COMBTEX_CTRL]
    a) No
    b) Yes
    c) Mix
    d) Cannot tell

46. Participant occupational role in military for those in the mindfulness-based intervention group [OCCROLE_MIND]
    a) Administrative
    b) Combat arms
    c) Healthcare
    d) Human Resources
    e) Media Relations
    f) Transportation
    g) Mix
    h) Other
    i) Cannot tell

47. Participant occupational role in military for those in the control group [OCCROLE_CTRL]
    a) Administrative
    b) Combat arms
    c) Healthcare
    d) Human Resources

e) Media Relations
f) Transportation
g) Mix
h) Other
i) Cannot tell

48. Mean time served in military in the mindfulness-based intervention group (number of years) [TIME_MIND] (Use "999" if cannot tell)

49. Mean time served in military in the control group (number of years) [TIME_CTRL] (Use "999" if cannot tell)

NOTE: The following items are adapted from the Jadad criteria (Jadad et al., 1996).

Instrument to Measure the Likelihood of Bias in Research Reports

This is not the same as being asked to review a paper. It should not take more than 10 minutes to score a report and there are no right or wrong answers.

Please read the article and try to answer the following questions (see attached instructions):

1. Was the study described as randomized (this includes the use of words such as randomly, random, and randomization)?
2. Was the study described as having blinded outcome assessment?
3. Was there a description of withdrawals and dropouts?

Scoring the items:
Either give a score of 1 point for each "yes" or 0 points for each "no." There are no in-between marks.

Give 1 additional point if: For question one, the method used to generate the sequence of randomization was described **and** it was **appropriate** (e.g., table of random numbers, computer generated, etc.)

Deduct 1 point if: For question one, the method used to generate the sequence of randomization was described **and** it was **inappropriate** (e.g., patients were allocated alternately, or according to date of birth or hospital number).

Guildelines for Assessment:

1. **Randomization**
   A method to generate the sequence of randomization will be regarded as appropriate if it allowed each study participant to have the same chance of receiving each intervention and the investigators could not predict which treatment was next (i.e., 2:1 randomization would **not** qualify as appropriate because each member does not have an equal chance of being in each group). Methods of allocation using date of birth, date of admission, hospital numbers, or alternation should be not regarded as appropriate.

2. **Blinding**
   A study is regarded as having blinded outcome assessment if the term "blind" or "blinded" is used to describe the assessment of the primary outcome measure(s). Alternatively, if the study stated that the person doing the assessment of the primary outcome measure(s) could not identify the intervention being assessed, that would be considered blinded.

3. **Withdrawals and Dropouts**
   Participants who were included in the study but did not complete the observation period or who were not included in the analysis must be described. The number and the reasons for withdrawal in each group must be stated. If there were no withdrawals, it should be stated in the article. If there is no statement on withdrawals, this item must be given no points.

50. Jadad Score [JADAD; code score based on scoring instructions above; score can range from 0 to 4]

Appendix B

Effect Size Level Coding Form

1. Study ID Number [STUDYID]

Dependent Measure Descriptors

2. Effect Size Type [ESTYPE]
    1) Comparison between experimental vs. control/comparison group at pretest
    2) Comparison between experimental vs. control/comparison group at posttest
    3) Comparison between experimental vs. control/comparison group at follow-up

3. Interval in months between completion of intervention and follow-up (if applicable) [FA_INT] [if applicable but cannot tell from the study, code "Cannot Tell"; if not applicable, code "N/A"]

4. Type of Outcome [TYPE of OUTCOME]
    a) PTSD symptoms or PTSD diagnosis (i.e., presence/absence of PTSD)
    b) Depression symptoms or depression diagnosis (i.e., presence/absence of depression)
    c) Non-PTSD Anxiety symptoms or Non-PTSD anxiety diagnosis (i.e., presence/absence of non-PTSD anxiety diagnosis)
    e) Overall well-being/quality of life
    f) Other: _____

5. Source of information for outcome measures [SOURCE]
    a) Self-report
    b) Clinician (e.g., diagnosis)
    c) Non-clinician informant (e.g., report of spouse)

6. Were the experimental and control/comparison groups in the study equivalent at baseline in terms of the outcome measures? (Note: If the groups differed **on at least one outcome** measure, code "No;" If cannot tell, select "No") [EQUIV_OUTCOME]?
    a) No
    b) Yes

Effect Size Data

7. Type of data effect size based on [Type of ES]

1. means and standard deviations:
2. *t*-value (from independent *t*-test)
3. *F*-value (from a one-way ANOVA with only two groups)
   F = _____ n_1= _____ n_2= _____

4. Frequencies, dichotomous

5. Proportions, dichotomous
6. Frequencies or proportions, polychotomous (contact Marc for how to code this type of situation, since it is complex)
7. Chi Square (with $df = 1$)

8a. Cohen's *d*

8b. Estimated Cohen's *d* from gain score data

8c. Estimated Cohen's *d* from means, sample sizes, and standard deviation of difference scores

9. Exact two-tailed *p* value (when all of the above are not available)
10. Other (Write in):_____

7a) Used WebPlotDigitizer to extract effect size data from graphs or figures?

(0) No
(1) Yes

Note: This section is used for when you are coding Ms, SDs, and N sizes. If you are coding different data, code all of these variables as "N/A".

8. Mean for mindfulness-based intervention group (MBIG) at pre-test [M_MIND_PRE; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

9. Standard deviation for MBIG at pre-test [SD_MIND_PRE; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

10. N size for MBIG at pre-test [N_MIND_PRE; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

11. Mean for control/comparison group at pre-test [M_CNTRL_PRE; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

12. Standard deviation for control/comparison group at pre-test [SD_CNTR_PRE; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

13. N size for control/comparison group at pre-test [N_CTRL_PRE; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

14. Mean for MBIG at post-test [M_MIND_POST; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

15. Standard deviation for MBIG at post-test [SD_MIND_POST; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

16. N size for MBIG at post-test [N_MIND_POST; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

17. Mean for control group at post-test [M_CTRL_POST; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"

18. Standard deviation for control group at post-test [SD_CTRL_POST; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

19. N size for control group at post-test [N_CTRL_POST; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

20. Mean for MBIG at follow-up [M_MIND_FU; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

21. Standard deviation for MBIG at follow-up [SD_MIND_FU; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

22. N size for MBIG at follow-up [N_MIND_FU; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

23. Mean for control group at follow-up [M_CTRL_FU; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"

24. Standard deviation for control group at follow-up [SD_CTRL_FU; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

25. N size for control group at follow-up [N_CTRL_FU; if not applicable (e.g., the only data reported is an independent t-test), code "N/A"]

Note: This section is used for when you are coding data from an independent t-test. If you are coding different data, code all of these variables as "N/A".

26. *t*-statistic from an independent *t*-test at post-test [T_POST; if not applicable (e.g., the only data reported is proportions), code "N/A"]

27. N size for MBIG at post-test [N_MIND_POST_T; if not applicable (e.g., the only data reported is proportions), code "N/A"]

28. N size for control group at post-test [N_CTRL_POST_T; if not applicable (e.g., the only data reported proportions), code "N/A"]

29. *t*-statistic from an independent *t*-test at follow-up-test [T_FU; if not applicable (e.g., the only data reported is proportions), code "N/A"]
30. N size for MBIG at follow-up [N_MIND_FU_T; if not applicable (e.g., the only data reported is proportions), code "N/A"]

31. N size for control group at follow-up [N_CTRL_FU_T; if not applicable (e.g., the only data reported proportions), code "N/A"]

Note: This section is used for when you are coding data from a one-way ANOVA with only two groups. If you are coding different data, code all of these variables as "N/A".

32. F-statistic from a one-way ANOVA with only two groups at post-test [F_POST; if not applicable (e.g., the only data reported is proportions), code "N/A"]

33. N size for MBIG at post-test [N_MIND_POST_F; if not applicable (e.g., the only data reported is proportions), code "N/A"]

34. N size for control group at post-test [N_CTRL_POST_F; if not applicable (e.g., the only data reported proportions), code "N/A"]

35. F-statistic from a one-way ANOVA with only two groups at follow-up [F_FU; if not applicable (e.g., the only data reported is proportions), code "N/A"]

36. N size for MBIG at follow-up [N_MIND_FU_F; if not applicable (e.g., the only data reported is proportions), code "N/A"]

37. N size for control group at follow-up [N_CTRL_FU_F; if not applicable (e.g., the only data reported proportions), code "N/A"]

Note: This section is used for when you are coding dichotomous frequencies. If you are coding different data, code all of these variables as "N/A".

38. Number of events in MBIG at pre-test [DIFREQ_MIND_PRE; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

39. N size for MBIG at pre-test [N_MIND_PRE_DIFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

40. Number of events in CTRL at pre-test [DIFREQ_CTRL_PRE; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

41. N size for CTRL at pre-test [N_CTRL_PRE_DIFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

42. Number of events in MBIG at post-test [DIFREQ_MIND_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

43. N size for MBIG at post-test [N_MIND_POST_DIFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

44. Number of events in CTRL at post-test [DIFREQ_CTRL_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

45. N size for CTRL at post-test [N_CTRL_POST_DIFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

46. Number of events in MBIG at follow-up [DIFREQ_MIND_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

47. N size for MBIG at follow-up [N_MIND_FU_DIFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

48. Number of events in CTRL at follow-up [DIFREQ_CTRL_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

49. N size for CTRL at follow-up [N_CTRL_FU_DIFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding polychotomous frequencies. If you are coding different data, code all of these variables as "N/A".

50. Number of events in MBIG at post-test [POLYFREQ_MIND_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

51. N size for MBIG at post-test [N_MIND_POST_POLYFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

52. Number of events in CTRL at post-test [POLYFREQ_CTRL_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

53. N size for CTRL at post-test [N_CTRL_POST_POLYFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

54. Number of events in MBIG at follow-up [POLYFREQ_MIND_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

55. N size for MBIG at follow-up [N_MIND_FU_POLYFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

56. Number of events in CTRL at follow-up [POLYFREQ_CTRL_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

57. N size for CTRL at follow-up [N_CTRL_FU_POLYFREQ if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding polychotomous proportions. If you are coding different data, code all of these variables as "N/A".

58. Proportion of events in MBIG at post-test [POLYPROP_MIND_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

59. N size for MBIG at post-test [N_MIND_POST_POLYPROP if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

60. Proportion of events in CTRL at post-test [POLYPROP_CTRL_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

61. N size for CTRL at post-test [N_CTRL_POST_POLYPROP if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

62. Proportion of events in MBIG at follow-up [POLYPROP_MIND_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

63. N size for MBIG at follow-up [N_MIND_FU_POLYPROP if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

64. Proportion of events in CTRL at follow-up [POLYPROP_CTRL_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

65. N size for CTRL at follow-up [N_CTRL_FU_POLYPROP if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding chi square. If you are coding different data, code all of these variables as "N/A".

66. Chi-square at post-test [CHISQU_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

67. N size at post-test [N_POST_CHISQU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

68. Chi-square at follow-up [CHISQU_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

69. N size at follow-up [N_FU_CHISQU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding Cohen's *d*. If you are coding different data, code all of these variables as "N/A".

70. Cohen's *d* at post-test [D_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

71. N size for MBIG post-test [N_MIND_D_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

72. N size for CTRL at post-test [N_CTRL_D_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

73. Standard error of Cohen's *d* at post-test [SE_D_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

74. Cohen's *d* at follow-up [D_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

75. N size for MBIG follow-up [N_MIND_D_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

76. N size for CTRL at follow-up [N_CTRL_D_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

77. Standard error of Cohen's *d* at follow-up [SE_D_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding estimated Cohen's *d* from gain score data. If you are coding different data, code all of these variables as "N/A".

78. Estimated Cohen's *d* at post-test [D_EST_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

79. N size for MBIG post-test [N_MIND_D_EST_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

80. N size for CTRL at post-test [N_CTRL_D_ EST_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]
81. Standard error of estimated Cohen's *d* at post-test [SE_D_ EST_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

82. Correlation of Pretest with Posttest Scores Across Both Groups on the specific outcome measure being coded [CORR_D_EST_PRE-POST; if this data is not

reported, code ".70"; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

83. Correlation of Pretest with Follow-Up Scores Across Both Groups on the specific outcome measure being coded [CORR_D_EST_PRE-FU; if this data is not reported, code ".70"; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

84. Correlation of Posttest with Follow-Up Scores Across Both Groups on the specific outcome measure being coded [CORR_D_EST_POST-FU; if this data is not reported, code ".70"; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

85. Estimated Cohen's *d* at follow-up [D_EST_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

86. N size for MBIG follow-up [N_MIND_D_EST_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

87. N size for CTRL at follow-up [N_CTRL_D_EST_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

88. Standard error of estimated Cohen's *d* at follow-up [SE_D_EST_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding estimated Cohen's *d* from means, sample sizes, and standard deviations of difference score data. If you are coding different data, code all of these variables as "N/A".

89. Estimated Cohen's *d* at post-test [D_EST_SDdiff_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

90. N size for MBIG post-test [N_MIND_D_EST_SDdiff_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

91. N size for CTRL at post-test [N_CTRL_D_ EST_SDdiff_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

92. Standard error of estimated Cohen's *d* at post-test [SE_D_ EST_SDdiff_POST; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

93. Correlation of Pretest with Posttest Scores Across Both Groups on the specific outcome measure being coded [CORR_D_EST_SDdiff_PRE-POST; if this data is

not reported, code ".70"; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

94. Correlation of Pretest with Follow-Up Scores Across Both Groups on the specific outcome measure being coded [CORR_D_EST_SDdiff_PRE-FU; if this data is not reported, code ".70"; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

95. Correlation of Posttest with Follow-Up Scores Across Both Groups on the specific outcome measure being coded [CORR_D_EST_SDdiff_POST-FU; if this data is not reported, code ".70"; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

96. Estimated Cohen's *d* at follow-up [D_EST_FU_SDdiff; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

97. N size for MBIG follow-up [N_MIND_D_EST_SDdiff_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

98. N size for CTRL at follow-up [N_CTRL_D_EST_SDdiff_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

99. Standard error of estimated Cohen's *d* at follow-up [SE_D_EST_SDdiff_FU; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

Note: This section is used for when you are coding exact two-tailed *p* values. If you are coding different data, code all of these variables as "N/A".

100. $d_{equivalent}$ at post-test [Deqv_POST_PVAL; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

101. N size at post-test [N_POST_PVAL; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

102. Standard error at post-test [SE_POST_PVAL]; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

103. $d_{equivalent}$ at follow-up [Deqv_FU_PVAL; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

104. N size at follow-up [N_FU_PVAL; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

105. Standard error at follow-up [SE_FU_PVAL]; if not applicable (e.g., the only data reported is means, standard deviations, and N sizes), code "N/A"]

106. Page number where effect size data found [PAGENUM]

107. Sign of the effect size at pre-test (assign a “1” if the MBIG had better outcome and a -1” if the MBIG had worse outcome; Code N/A if coding post or follow-up) [SIGN_PRE]

108. Sign of the effect size at post-test (assign a “1” if the MBIG had better outcome and a -1” if the MBIG had worse outcome; Code N/A if coding pre or follow-up) [SIGN_POST]

109. Sign of the effect size at follow-up (assign a “1” if the MBIG had better outcome and a -1” if the MBIG had worse outcome; Code “N/A” if there is no follow-up data) [SIGN_FU]

Appendix C

Table 1

Descriptive Statistics of Average Sample Sizes of Mindfulness and Control Groups in Studies Included in the Meta-Analysis

	Pre-test		Post-test		Follow-up	
	Mindfulness	Control	Mindfulness	Control	Mindfulness	Control
Sum	515.48	526.44	532.15	555.22	265.08	285.83
Mean	27.13	27.71	24.19	25.24	26.51	28.58
SD	18.89	21.25	16.73	18.98	15.35	17.99
Skewness	0.87	0.95	1.14	1.20	0.51	0.80
Kurtosis	-0.29	-0.40	0.90	0.72	-0.06	0.00
Median	20.80	22.75	20.40	23.00	25.17	27.50

Appendix D

Table 2.
Descriptive Statistics of Largest Sample Sizes of Mindfulness and Control Groups in Studies Included in Meta-Analysis

	Pre-test		Post-test		Follow-up	
	Mindfulness	Control	Mindfulness	Control	Mindfulness	Control
Sum	524.50	535.00	545.50	565.50	305.00	316.00
Mean	27.61	28.16	24.80	25.70	30.50	31.60
SD	19.07	21.34	16.94	19.10	17.24	19.42
Skewness	0.86	0.93	1.08	1.15	-0.04	0.25
Kurtosis	-0.35	-0.40	0.72	0.56	-0.97	-1.34
Median	20.80	22.75	20.40	23.00	25.17	27.50

Note. Not all of the data presented in this table are integers, despite the fact that they refer to the *largest* sample sizes for each of the groups from individual studies, since in one study, the separate samples sizes for the mindfulness and control groups were not provided. As a result, the overall sample size aggregated across both groups that was provided in the study was divided into two, in order to provide an estimate for the separate sample sizes for the mindfulness and control groups, which was needed for data analytic purposes, as explained in the text.

Appendix E

Table 3
Effect Size Data at Pre-test

Study Name	Standardized Difference in Means	Standard Error	95 % Confidence Interval Lower Limit	95 % Confidence Interval Upper Limit	Z-value	p-value
Arch et al. (2013)	-0.214	0.198	-0.603	0.174	-1.081	0.280
Bein (2014)	-0.442	0.718	-1.849	0.965	-0.615	0.538
Bormann et al. (2013)/Oman & Bormann (2015)	0.064	0.167	-0.262	0.391	0.387	0.699
Bremner et al. (2017)/Bremner et al. (2011)	-0.283	0.488	-1.240	0.674	-0.579	0.562
Goodman et al. (2016)	0.211	0.212	-0.205	0.626	0.993	0.321
Heffner et al. (2016a)	-0.255	0.312	-0.866	0.357	-0.817	0.414
Heffner et al. (2016b)	0.112	0.304	-0.483	0.707	0.368	0.713
Heffner et al. (2016c)	0.458	0.536	-0.592	1.507	0.854	0.393
Heffner et al. (2016d)	0.136	0.351	-0.552	0.823	0.387	0.699
Kearney et al. (2012a)	0.107	0.290	-0.463	0.676	0.367	0.713
Kearney et al. (2016)	-0.162	0.271	-0.692	0.369	-0.598	0.550
King et al. (2016)	0.106	0.428	-0.732	0.943	0.247	0.805
Koons et al. (2001)	0.243	0.471	-0.680	1.165	0.516	0.606
Lang et al. (2016)	0.000	0.187	-0.367	0.367	0.000	1.000
Marzabadi & Zadeh (2014)	-0.054	0.379	-0.797	0.689	-0.142	0.887
Niles et al. (2012)	0.743	0.411	-0.063	1.549	1.807	0.071
Polusney et al. (2015)	-0.317	0.187	-0.684	0.049	-1.696	0.090
Possemato et al. (2015)	-0.168	0.258	-0.673	0.338	-0.650	0.516
Wahbeh et al. (2016)/Wahbeh & Oken (2014)	0.013	0.278	-0.533	0.558	0.045	0.964

Appendix F

Table 4
Effect Size Data at Post-test

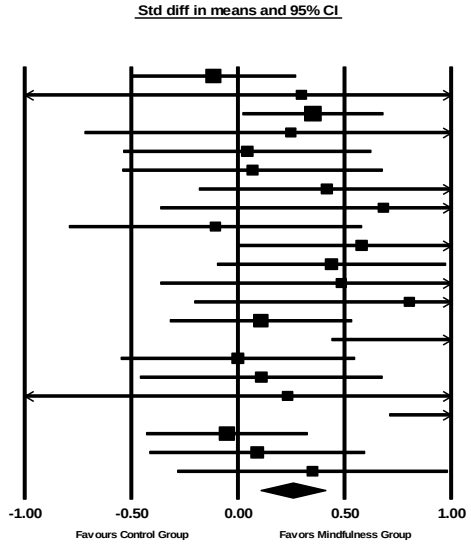
Study Name	Standardized Difference in Means	Standard Error	95 % Confidence Interval Lower Limit	95 % Confidence Interval Upper Limit	Z-value	p-value
Arch et al. (2013)	-0.115	0.198	-0.502	0.273	-0.581	0.561
Bein (2014)	-0.298	0.713	-1.099	1.696	0.418	0.676
Bormann et al. (2013)/Oman & Bormann (2015)	0.352	0.168	0.022	0.682	2.090	0.037
Bremner et al. (2017)/Bremner et al. (2011)	0.248	0.493	-0.719	1.215	0.503	0.615
Goodman et al. (2016)	0.044	0.297	-0.537	0.626	0.149	0.881
Heffner et al. (2016a)	0.069	0.312	-0.542	0.679	0.220	0.826
Heffner et al. (2016b)	0.418	0.306	-0.183	1.018	1.364	0.173
Heffner et al. (2016c)	0.683	0.534	-0.364	1.729	1.278	0.201
Heffner et al. (2016d)	-0.105	0.350	-0.792	0.582	-0.300	0.764
Kearney et al. (2012a)	0.581	0.299	-0.004	1.166	1.946	0.052
Kearney et al. (2016)	0.439	0.274	-0.097	0.976	1.604	0.109
King et al. (2016)	0.485	0.433	-0.364	1.334	1.120	0.263
Koons et al. (2001)	0.805	0.515	-0.204	1.813	1.563	0.118
Lang et al. (2016)	0.108	0.218	-0.319	0.535	0.496	0.620
Marzabadi & Zadeh (2014)	1.250	0.413	0.440	2.060	3.023	0.003
Mehta et al. (2012)	0.000	0.280	-0.549	0.549	0.000	1.000
Mularski et al. (2009)	0.110	0.290	-0.458	0.678	0.379	0.704
Nassif (2013)	0.233	0.697	-1.133	1.600	0.335	0.738
Niles et al. (2012)	1.613	0.461	0.710	2.516	3.501	0.000
Polusney et al. (2015)	-0.051	0.193	-0.430	0.327	-0.267	0.790
Possemato et al. (2015)	0.090	0.258	-0.415	0.596	0.351	0.726
Wahbeh et al. (2016)/Wahbeh & Oken (2014)	0.350	0.324	-0.284	0.985	1.082	0.279

Appendix G

Table 5
Forest Plot of Post-test Effect Sizes

Meta-Analysis for Posttest Effect Sizes

Study name	ES #	Std diff in means	Standard error	Lower limit	Upper limit	Z-Value	p-Value
Arch et al. (2013)	Combined	-0.115	0.198	-0.502	0.273	-0.581	0.561
Bein (2014)	Combined	0.298	0.713	-1.099	1.696	0.418	0.676
Bormann et al. (2013)/Oman & Bormann (2015)	Combined	0.352	0.168	0.022	0.682	2.090	0.037
Bremner et al. (2017)/Bremner et al. (2011)	Combined	0.248	0.493	-0.719	1.215	0.503	0.615
Goodman et al. (2016)	Combined	0.044	0.297	-0.537	0.626	0.149	0.881
Heffner et al. (2016a)	Combined	0.069	0.312	-0.542	0.679	0.220	0.826
Heffner et al. (2016b)	Combined	0.418	0.306	-0.183	1.018	1.364	0.173
Heffner et al. (2016c)	Combined	0.683	0.534	-0.364	1.729	1.278	0.201
Heffner et al. (2016d)	Combined	-0.105	0.350	-0.792	0.582	-0.300	0.764
Kearney et al. (2012) a.	Combined	0.581	0.299	-0.004	1.166	1.946	0.052
Kearney et al. (2016)	Combined	0.439	0.274	-0.097	0.976	1.604	0.109
King et al. (2016)	2.000	0.485	0.433	-0.364	1.334	1.120	0.263
Koons et al. (2001)	Combined	0.805	0.515	-0.204	1.813	1.563	0.118
Lang et al. (2016)	Combined	0.108	0.218	-0.319	0.535	0.496	0.620
Marzabadi & Zadeh (2014)	Combined	1.250	0.413	0.440	2.060	3.023	0.003
Mehta et al. (2012)	Combined	0.000	0.280	-0.549	0.549	0.000	1.000
Mularski et al. (2009)	Combined	0.110	0.290	-0.458	0.678	0.379	0.704
Nassif (2013)	Combined	0.233	0.697	-1.133	1.600	0.335	0.738
Niles et al. (2012)	Combined	1.613	0.461	0.710	2.516	3.501	0.000
Polusney et al. (2015)	Combined	-0.051	0.193	-0.430	0.327	-0.267	0.790
Possemato et al. (2015)	Combined	0.090	0.258	-0.415	0.596	0.351	0.726
Wahbeh et al. (2016)/Wahbeh & Oken (2014) b	Combined	0.350	0.324	-0.284	0.985	1.082	0.279
		0.258	0.077	0.107	0.409	3.346	0.001



Meta Analysis

Appendix H

Table 6
Effect Size Data at Follow-up

Study Name	Standardized Difference in Means	Standard Error	95 % Confidence Interval Lower Limit	95 % Confidence Interval Upper Limit	Z-value	p-value
Arch et al. (2013)	-0.118	0.198	-0.506	0.271	-0.594	0.552
Goodman et al. (2016)	0.213	0.294	-0.364	0.790	0.723	0.469
Kearney et al. (2012a)	0.518	0.297	-0.065	1.100	1.741	0.082
Kearney et al. (2016)	0.497	0.275	-0.042	1.036	1.807	0.071
Lang et al. (2016)	0.006	0.291	-0.565	0.576	0.019	0.985
Marzabadi & Zadeh (2014)	1.540	0.432	0.694	2.386	3.569	0.000
Nassif (2013)	0.223	0.683	-1.116	1.563	0.327	0.744
Niles et al. (2012)	0.894	0.428	0.055	1.733	2.087	0.037
Polusney et al. (2015)	0.096	0.195	-0.287	0.479	0.493	0.622
Possemato et al. (2015)	-0.126	0.258	-0.631	0.379	-0.488	0.625

Appendix I

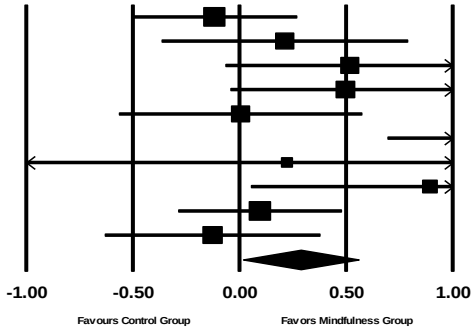
Table 7
Forest Plot of Follow-up Effect Sizes

Meta-Analysis for Follow-Up Effect Sizes

Study name	ES #	Std diff in means	Standard error	Lower limit	Upper limit	Z-Value	p-Value
Arch et al. (2013)	Combined	-0.118	0.198	-0.506	0.271	-0.594	0.552
Goodman et al. (2016)	Combined	0.213	0.294	-0.364	0.790	0.723	0.469
Kearney et al. (2012) a.	Combined	0.518	0.297	-0.065	1.100	1.741	0.082
Kearney et al. (2016)	Combined	0.497	0.275	-0.042	1.036	1.807	0.071
Lang et al. (2016)	Combined	0.006	0.291	-0.565	0.576	0.019	0.985
Marzabadi & Zadeh (2014)	Combined	1.540	0.432	0.694	2.386	3.569	0.000
Nassif (2013)	Combined	0.223	0.683	-1.116	1.563	0.327	0.744
Niles et al. (2012)	5.000	0.894	0.428	0.055	1.733	2.087	0.037
Polusney et al. (2015)	Combined	0.096	0.195	-0.287	0.479	0.493	0.622
Possemato et al. (2015)	Combined	-0.126	0.258	-0.631	0.379	-0.488	0.625
		0.288	0.138	0.017	0.558	2.083	0.037

Statistics for each study

Std diff in means and 95% CI



Meta Analysis

Appendix J

Table 8
Inter-Rater Reliability for Moderator Coding

Variable	Kappa	ICC (1, 1)	% Agreement
Effect Size Type	-	-	100
Follow-up Interval[a]	.815	-	-
Type of Outcome[a]	.817	-	-
Source[a]	.840	-	-
Equivalent Outcome[a]	-.200	-	-
Type of Effect Size Data Used[a]	.817	-	-
Sign at Pre-test[a]	.993	-	-
Sign at Post-test[a]	.892	-	-
Sign at Follow-up[a]	.875	-	-
Matching	.737	-	-
Publication Type	1.000	-	-
Publication Year	-	.990	-
Mean Age	-	1.000	-
Percentage White	-	1.000	-
Percentage Female	-	.998	-
Type of Mindfulness Intervention	.722	-	-
Format of Mindfulness Intervention	.655	-	-
Format of Control Group	.722	-	-

(*table continues*)

Inter-Rater Reliability for Moderator Coding (*continued*)

Variable	Kappa	ICC (1, 1)	% Agreement
Dosage of Mindfulness Group	-	-	50
Dosage of Control Group	-	-	90
Homework in Mindfulness Group	1.000	-	-
Homework in Control Group	.706	-	-
Medications in Mindfulness Group	.394	-	-
Medications in Control Group	.394	-	-
Type of Control Group	1.000	-	-
Components of Mindfulness Group	.333	-	-
Concurrent Treatments in Mindfulness Group	1.000	-	-
Concurrent Treatments in Control Group	.189	-	-
Treatments Before Mindfulness Intervention	.189	-	-
Treatments Before Control Intervention	.474	-	-
Treatments After Mindfulness Intervention	.474	-	-
Treatments After Control Intervention	.474	-	-
Attrition in Mindfulness Group	-	.858	90
Attrition in Control Group	-	.521	90
Therapist Competency in Mindfulness Group	.559	-	-

(*table continues*)

Inter-Rater Reliability for Moderator Coding (*continued*)

Variable	Kappa	ICC (1, 1)	% Agreement
Therapist Competency in Control Group	.333	-	-
Competency Check in Mindfulness Group	.268	-	-
Competency Check in Control Group	.412	-	-
Adherence Check in Mindfulness Group	.818	-	-
Adherence Check in Control Group	.796	-	-
Country of Enlistment	1.000	-	-
Diagnosis Presence	.310	-	-
Diagnosis Featured	.839	-	-
Comorbidity in Mindfulness Group	.111	-	-
Comorbidity in Control Group	.111	-	-
Physical Disability in Mindfulness Group	-.190	-	-
Physical Disability in Control Group	-.190	-	-
Military Branch in Mindfulness Group	1.000	-	-
Military Branch in Control Group	1.000	-	-
Active Duty or Veteran Status Mindfulness Group	1.000	-	-

(*table continues*)

Inter-Rater Reliability for Moderator Coding (*continued*)

Variable	Kappa	ICC (1, 1)	% Agreement
Active Duty or Veteran Status Control Group	1.000	-	-
Combat Exposure in Mindfulness Group	.643	-	-
Combat Exposure in Control Group	.643	-	-
Occupational Role in Mindfulness Group	.615	-	-
Occupational Role in Control Group	.615	-	-
Study Quality	-	-.324	-
Study Level Effect Size [+]	-	.968	-
Study Level Standard Error [+]	-	.992	-

Note. "-" indicates "not applicable; "ICC (1, 1)" = Model 1 (one-way random effects) of intraclass correlation coefficient

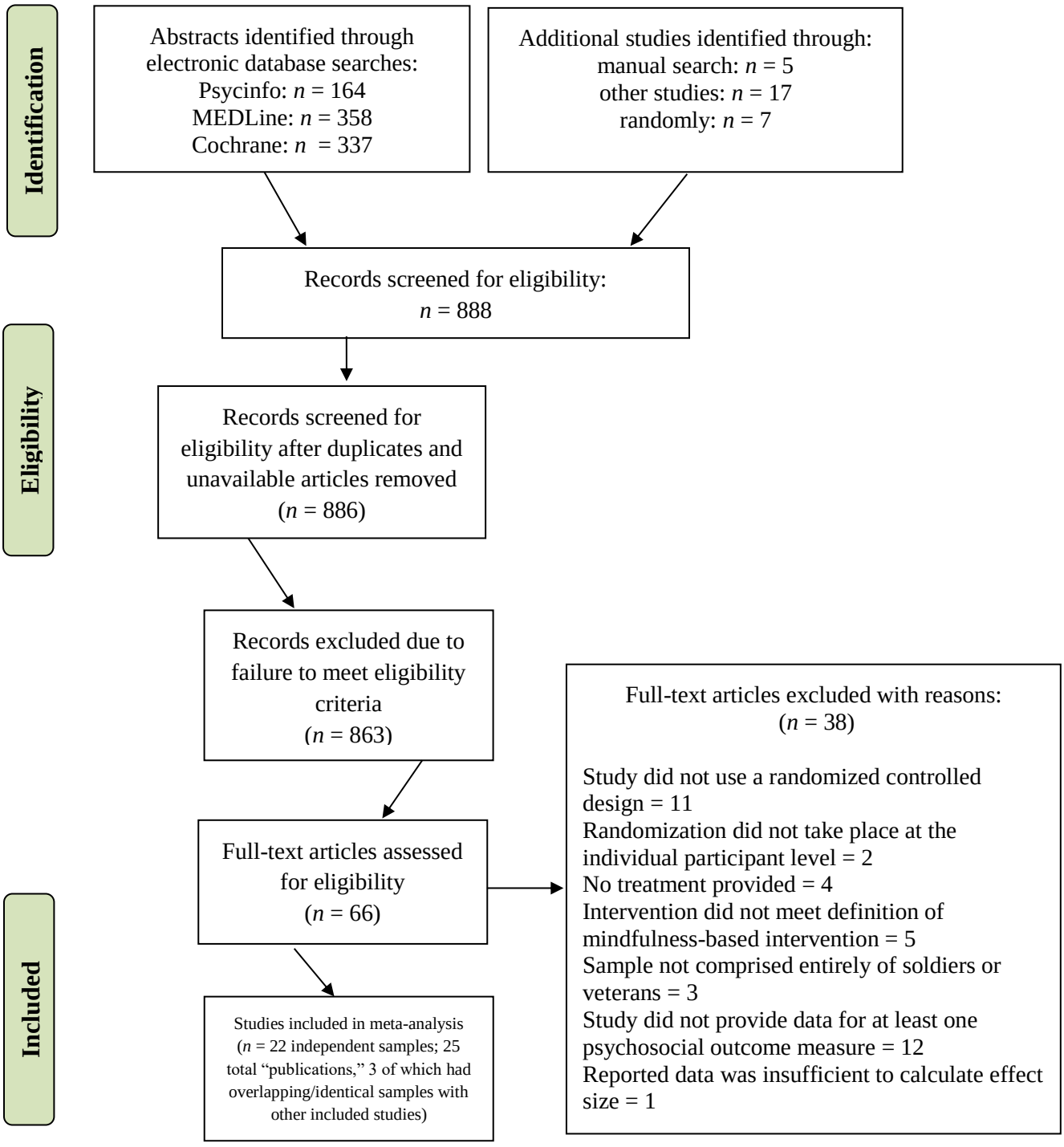
[a] This variable was coded at the effect size level rather than the study level; as a result, inter-rater reliability analyses for this variable were conducted utilizing only those effect sizes (N = 91) in which both raters agreed were present. The inter-rater reliability for the presence/absence of effect sizes was 90.10% (i.e., 91/101). For all remaining variables in this table, N = 10 for the inter-rater reliability analyses, since these variables were coded at the study level, with the exception of the Study Level Effect Size and Study Level Standard Error; for these latter two variables, N = 24, although there were only 10 studies coded by two raters, there were pretest, posttest, and follow-up effect sizes, all three of which were included as separate effect sizes, which yielded a total of 24 effect sizes.

[+] Although there were only 10 studies coded by two raters, there were pretest, posttest, and follow-up effect sizes, all three of which were included as separate effect sizes, which yielded a total of 24 effect sizes.

Appendix K

Figure 1

Flow Diagram for Search Process of Mindfulness for Soldiers Meta-Analysis



Note. Adapted from flow diagram in Moher D, Liberati A, Tetzlaff J, Altman DG, The PRISMA Group (2009). Preferred Reporting Items for Systematic Reviews and Meta-Analyses: The PRISMA Statement. PLoS Med 6(6): e1000097. doi:10.1371/journal.pmed100009.

Appendix L

Figure 2

Funnel Plot of Duval and Tweedie's Publication Bias Analysis at Post-test