CONTEMPLATIVE INTERVENTIONS AND EMPLOYEE DISTRESS: A META-ANALYSIS

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Abstract

Mindfulness, meditation, and other practices that form contemplative interventions are increasingly offered in workplaces to support employee mental health. Studies have reported benefits across various populations, yet researchers have expressed concerns that adoption of such interventions has outpaced scientific evidence. We reappraise the extant literature by meta-analytically testing the efficacy of contemplative interventions in reducing psychological distress in employees (meta-analyzed set: $k = 119$; $N = 6{,}044$). Complementing other reviews, we also examine a range of moderators and the impact of biases that could artificially inflate effect sizes. Results suggested interventions were generally effective in reducing employee distress, yielding small to moderate effects that were sustained at last follow-up. Effects were moderated by the type of contemplative intervention offered and the type of control group utilized. We also found evidence of publication bias, which is likely inflating estimated effects. Uncontrolled single sample studies were more affected by bias than large or randomized controlled trial studies. Adjustments for publication bias lowered overall effects. Overall, our review supports the effectiveness of contemplative interventions in reducing employee distress, but there is a need for proactive strategies to mitigate artificially inflated effect sizes and thus avoid the misapplication of contemplative interventions in work settings.

Keywords: Mindfulness, meditation, contemplative interventions, well-being, psychological distress, meta-analysis, publication bias

Contemplative Interventions and Employee Distress: A Meta-Analysis

Work in the 21st century is evolving rapidly, becoming increasingly volatile, unpredictable, complex, and ambiguous. Boundaries between work and personal life are increasingly blurred (Sonnentag & Fritz, 2015). Common employee issues include high levels of disengagement, unsafe workplace behaviors, stress-related mental and physical health issues, and burnout (APA, 2013; Casey & Liang, 2014; Johnson et al., 2005). As these issues create growing costs, many organizations are considering ways to sustain employee well-being. For organizations wanting to support their employees, a variety of options are available, including a diverse array of workplace well-being training programs.

Amongst such programs, contemplative interventions, including various forms of mindfulness, meditation, and other such variants, are becoming increasingly prominent. In 2016, for instance, mindfulness-related industries generated $984 million in revenue in the US (Gelles, 2016). Well-known organizations such as Ford, Google, Intel, Aetna, Target, and General Mills are incorporating mindfulness practices to better engage their staff (Gelles, 2015; Schaufenbuel, 2015; Sutcliffe, Vogus, & Dane, 2016; Talbot-Zorn & Edgette, 2016), and meditation is becoming a popular practice with leadership groups in organizations (Seppala, 2015; Talbot-Zorn & Edgette, 2016). Indeed, various forms of mindfulness and meditation are “close to taking on cult status in the business world” (Brendel, 2015, para. 1).

Despite their rapidly growing popularity, researchers have expressed concerns that the extant literature is lacking in methodological rigor, pointing to poorly designed studies, a variety of overlooked factors that may attenuate the efficacy of interventions, and threats to internal and external validity (e.g., Eby et al., 2018; Jamieson & Tuckey, 2017). Recent reviews have also pointed to factors that could artificially inflate effects, including the possibility of selective reporting (e.g., Coronado-Montoya et al. 2016; Jannsen et al., 2018). When strong claims are made based on weak studies, there is potential for misapplication of the research, resulting in

misuse of organizational time and resources, and overconfidence in the efficacy of programs. It is thus imperative that researchers and practitioners determine the relative and enduring effectiveness of such programs, consider possible biases that might falsely inflate reported effects, and identify factors that may moderate program efficacy. In the present study, we employ meta-analysis to address these issues, synthesizing the impact of various forms of contemplative interventions on employee mental health, with a focus on their effectiveness in relieving psychological distress.

Contemplative Interventions

Contemplative interventions are used by millions worldwide (Kemeny et al., 2012; Wallace, 2005). Such interventions stem from practices originally rooted in Buddhist traditions, and comprise a variety of cognitive-behavioral activities intended to produce sustained alterations in basic cognitive and affective processes, including the regulation of attention, affect, or distress, to support personal insight and well-being (Davidson et al., 2012). Secular uses of contemplative interventions focus on improved abilities such as self-awareness, attention, memory, and the resultant benefits for health and wellbeing (Creswall, 2017). A variety of programs are available, which typically consist of various forms of meditation, mindfulness, or combinations thereof (see Appendix 1 for a summary of interventions commonly used in workplaces).

While there are no universally agreed-upon definitions (Van Dam et al. 2017), “meditation” generally consists of a collection of introspective activities involving both concentration and analysis of a focal object (Davidson et al., 2012; Shonin et al., 2014). Sustained meditative practice intends to develop complex cognitive-behavioral abilities or traits, such as improved attentional processing and memory (Chambers, Lo, & Allen, 2008), compassion (Lim, Condon, & DeSteno, 2015), or adaptive and flexible processing of emotionally valanced information (Farb, Segal, & Anderson, 2012).

The term “mindfulness” has been characterized as a trait, state, and as a practice (see Jamieson & Tuckey, 2017). As a trait, it is broadly recognized as a dispositional tendency to notice and attend to present moment experiences, such as body sensations, breathing, thoughts, or environmental stimuli (Brown & Ryan, 2003). As a state, it is characterized by attention and awareness being grounded in the present moment and involves an open acceptance of one’s experience (Creswall, 2017). With regular practice, mindfulness is thought to become more easily accessible, less effortful, and more automatic (Chambers, Gullone, & Allen, 2009; Teper, Segal, & Inzlicht, 2013). As such, mindfulness is also a practice that guides a person toward becoming more mindful. Such practices involve present-moment volitional control of attention, by exercising deliberate and focused awareness on an attentional anchor (e.g., breathing), while taking active control of unregulated thoughts and mental habits such as rumination, mind-wandering, or distraction. Mindfulness-based practices (e.g., Creswall, 2017; Galantino et al., 2005), have been incorporated into a range of well-known and widely used therapeutic interventions, including mindfulness-based stress reduction (MBSR; Kabat-Zinn, 1990; Grossman, Niemann, Schmidt, & Walach, 2004), mindfulness-based cognitive therapy (MBCT; Segal, Williams, & Teasdale, 2002), and acceptance and commitment therapy (ACT; Bond & Hayes, 2002; Hayes, Strosahl, & Wilson, 1999).

Contemplative interventions vary in nature and duration. For instance, organizations might offer incentives for using a mindfulness “app” for a few minutes each day, whereas other workplace training programs can involve intensive daily practice lasting several months (Creswell, 2017; van Dam et al., 2017). In MBSR—one of the more widely used protocols in secular contexts (Van Dam et al., 2017)—participants typically attend eight weekly small-group sessions covering a variety of mindfulness-based meditative practices (e.g., awareness of breathing, scanning the body for physical sensations, yoga) and are encouraged to practice these exercises daily at home during the 8-week period. Other programs are less structured and time-

intensive than MBSR. This is particularly true of workplace adaptations, in which organizations may be hesitant to invest in the time and resources that MBSR requires.

Contemplative Interventions in Work Settings: Issues and Paths Forward

The growing uptake of contemplative interventions in recent decades has resulted in a growing scientific interest in understanding the efficacy of these interventions (Van dam et al., 2017). Numerous reviews of mindfulness and meditation studies have been conducted to assess their impact on physical and mental health across general populations, clinical and medical samples, and in students (e.g., Chiesa & Serretti, 2009; Grossman et al., 2004; Khoury et al., 2013a, 2015). Similar growth has occurred in organizational applications in both mixed employee samples (e.g., Lomas et al. 2017b; Virgili, 2015) and within specific occupational industries, including healthcare (e.g., Burton et al., 2017; Luken & Sammons, 2016; Lomas et al., 2018a, 2018b; Smith, 2014) and education (e.g., Hwang et al., 2017; Klingbeil & Renshaw, 2018; Lomas et al., 2017a).

To clarify the current state of existing literature, Table 1 provides a meta-summary of existing quantitative and qualitative reviews that have examined various forms of mindfulness or meditation in work settings. To align with the focus of our review, the table excludes reviews that focused more generally on healthy adults, which also include non-working participants such as community or student samples (e.g., Chiesa & Serretti, 2009; De Vibe et al. 2017; Gu et al., 2015; Khoury et al., 2015) and reviews that focused on clinical and medical populations (e.g., Khoury et al., 2013a, 2013b) or children (e.g., Zoogman et al., 2015). The table provides details on the interventions examined, study designs included, and the samples that were examined. It also details whether the review included quality appraisals, meta-analysis (including follow-ups), summary effects for distress, unpublished literature, corrections for publication bias, and moderators.

Insert Table 1 about here.

Across reviews, there is consensus that interventions have at least some degree of benefit, finding favorable effects across the outcomes studied and populations included. Still, reviews have identified several important issues that warrant further attention, several of which we address in the present study. First, reviews suggest a lack of methodological rigor (See Eby et al., 2018; Lomas et al., 2017b; Jamieson & Tuckey, 2017; Jannsen et al., 2018). Common issues include inattention to intervention fidelity, poorly controlled and maintained studies lacking in internal and external validity, absent manipulation checks, and inadequate reporting. Because insufficient study quality is a known source of bias and heterogeneity in research literatures (Higgins et al., 2003), its presence can lead to inaccurate conclusions about the efficacy of contemplative intervention programs. Meta-analysis can detect whether bias is present by evaluating whether observed dispersion in reported effects is systematically related to indices of study quality (Hattie & Hansford, 1983).

Second, reported effects may be artificially inflated by publication bias, evidence of which has been found in recent reviews. For example, in their systematic review, Jannsen et al. (2018) found statistically significant results in 22 of 23 studies reviewed, a proportion inconsistent with the level of power within those studies. Similar observations have been made across studies in the general population (e.g., Coronado-Montoya et al., 2016), suggesting that publication bias requires greater scrutiny. Publication bias is a critical factor to consider when evaluating the efficacy of contemplative interventions, as it may lead to over-estimates of program efficacy in workplaces. It can also reduce the variability across effect sizes within a literature due to the unavailability of small and low effect-size studies (Schmidt & Oh, 2016), making the detection of moderating variables more difficult. Compounding this issue further is the substantial proportion of pilot studies utilizing single-sample designs that lack comparison

groups (Lomas et al. 2017b; Jamieson & Tuckey, 2017). Given their smaller size, such designs are typically less resource-intensive and less onerous on the researcher than carefully controlled designs, which means they are in more danger of becoming "lost" to the proverbial file-drawer if small effects are observed (Borenstein et al., 2009). An examination of the extent to which publication bias is impacting effects, across different study designs, is thus needed to help to calibrate the size of effects, and which types of studies are particularly trustworthy or untrustworthy.

Third, a range of other study-related factors can yield an upward bias in estimated effects. For example, Kreplin, Farias, and Brazil (2018) meta-analyzed the prosocial effects of meditation interventions in general healthy adults. While they observed moderate increases in compassion at post-intervention, this effect was observed only when certain methodological factors were present, such as when one of the study authors facilitated the intervention, or when the study employed inactive (as opposed to active) control groups – suggesting that these factors can potentially yield inflated results. De Vibe et al. (2017) similarly showed elevated effects of MBSR with inactive control groups. It is thus important to consider the extent to which the strength of effects that can be expected after delivering various forms of contemplative interventions needs to be recalibrated.

Fourth, given the higher levels of stress in specific occupations, such as healthcare or education, it is also possible that the occupational industry in which the intervention is delivered moderates effects. On the one hand, populations in high stress industries may have more to gain from contemplative interventions, yet it is also possible that in such industries these interventions consume scant yet valuable time, energy, and resources, limiting their efficacy. Addressing this question will help contextualize intervention research and identify industries to target in future research and practice (Eby et al., 2018).

Finally, prior systematic reviews find considerable heterogeneity in the intervention protocols used across studies. In their inclusive qualitative review of the workplace literature, for instance, Lomas et al. (2017b) demonstrated that the range of intervention protocols were highly variable, with many studies making alterations to well-known and standardized treatment protocols such as MBSR and MBCT. Many others used lesser known variants of mindfulness-based interventions, with as many as 25 different variants of mindfulness-based training observed. Such heterogeneity makes the statistical aggregation of specific mindfulness programs in meta-analysis problematic, as it is difficult to develop eligibility criteria that reliably differentiates what is and what is not considered to be mindfulness. This lack of agreement has led to a number of idiosyncratic disparities in the studies that have been included in prior reviews. As examples, Jamieson and Tuckey (2017) included Loving Kindness Meditation (LKM; Fredrickson et al., 2008), while this study was excluded from other reviews (e.g., Eby et al., 2018; Lomas et al., 2017b). Some mindfulness reviews (e.g., Eby et al., 2018; Jamieson & Tuckey, 2014; Lomas et al., 2017b) include Meditation-Awareness Training (Shonin et al., 2014) and ACT (Flaxman & Bond, 2010), whereas other mindfulness reviews (e.g., Janssen et al., 2018; Virgili, 2015) excluded these interventions. Hence, reviews focused on specific programs may be dependent on the contemplative practices chosen for inclusion. Due to this, they are likely to be under-inclusive, and it is important for meta-analyses to be exhaustive if they are to resemble the available literature (Hattie & Hansford, 1983; Schmidt & Hunter, 2015). In this review, we thus include and evaluate a broad range of interventions that stem from the contemplative traditions, including various forms of both mindfulness- and meditation-based interventions, as well as combinations thereof, and test variations in specific treatment protocols as possible moderators of the efficacy in relieving psychological distress.

We focus on overall distress and the multiple ways distress has been operationalized, as prior reviews indicate that distress is the most commonly measured outcome in this literature

(Jannsen et al., 2018). Similarly, programs are typically offered with the intent to relieve the adverse impacts of workplace distress, including stress, anxiety and burnout (e.g., Lomas et al., 2017b). This is especially the case in work populations known to experience high levels of distress, such as healthcare or education (Burton et al., 2017; Hwang et al., 2017; Lomas et al., 2017a). By including a variety of distress-related outcomes, it is also possible to consider the extent to which effects diverge depending upon the distress outcomes. For example, more stable experiences such as burnout can be resistant to stress management and coping interventions (Maslach, Schaufeli, & Leiter, 2001), and may thus be more resistant to the therapeutic benefits of contemplative interventions.

The Present Study

There has been rapid growth in both research and practice involving contemplative interventions in work settings. This growth, combined with questions about the methodological quality of the available literature, warrants scrutiny, as evidenced by the growing number of systematic reviews published on such practices in recent years.

As seen in Table 1, five of the available reviews incorporated meta-analysis to synthesize findings, but those that did either focused on specific organizational contexts (e.g., healthcare or education, Burton et al. 2017; Klingbeil & Renshaw, 2018; Lomas et al., 2018b), or on a narrow intervention category, such as mindfulness (e.g., Virgili, 2015). Thus, prior meta-analyses have typically excluded forms of ACT or meditation (Lomas et al., 2018a, 2018b, 2018c), whereas these interventions are included in qualitative systematic reviews (e.g., Eby et al., 2018; Lomas et al., 2017b). With these imprecise boundaries for eligibility, we suggest prior mindfulness meta-analyses of the workplace literature are under-inclusive.

In addition, Virgili (2015) aggregated across different study designs which contained different effect-size calculation methods (e.g., single samples with randomized and non-randomized trials). Aggregating across these designs can create effect sizes that are difficult to

interpret due to these divergent points of comparison within each study (Borenstein et al., 2009; Eby et al., 2018). Similarly, combining randomized with non-randomized studies can artificially inflate results (Higgins & Green, 2011). While Lomas et al. (2018c) examined mindfulness interventions for general working samples in RCTs, they did not examine follow-up effects, nor how effects might differ across study designs. They also did not adjust effects for publication bias.

Hence, in conducting the present study, we had three primary aims. First, we aimed to systematically combine and meta-analytically aggregate the rapidly growing literature on contemplative interventions in work settings, considering their efficacy in relieving employee psychological distress immediately after training and at last follow-up. Second, we aimed to address questions about when and for whom workplace contemplative interventions are most useful by exploring moderators of the treatment effects, including the efficacy of different types of treatment protocols that stem from the contemplative traditions, and the dose of the interventions. Third, we aimed to evaluate the impact of several known biases that could yield upwardly biased estimates in the literature. This includes estimates of study quality, characteristics of the program facilitator, and the type of control group.

Our review extends the literature in three ways. First, we separate the meta-analytic aggregation procedure across the available study designs in the literature, allowing for summary effects that can be attributed to the efficacy of the interventions (or characteristics thereof) rather than study design. Second, our review has a broader focus, including all intervention protocols to stem from the contemplative traditions, rather than focusing specifically on mindfulness-based interventions. We also include a broader array of working populations, including general working adults, teachers, and health care workers. Third, we extensively test for publication bias. Although some studies (e.g., Lomas et al., 2018c; Virgili, 2015) examined publication bias, bias adjusted effect sizes were not reported, thus providing only limited indication of the extent to

which reported effects may be biased. We use bias-adjustments to statistically quantify how much of an impact publication bias is having on the literature and further consider whether bias is more or less evident across different study designs. This ensures our meta-analysis is more comprehensive than prior reviews in scope, testing of moderators, and sources of bias. Results should reflect a practice-friendly appraisal of the literature that practitioners and management can use to robustly evaluate the likely benefits of various contemplative interventions in their specific settings.

Method

Literature Search

Eleven electronic databases were systematically searched, initially from December 2015 to January 2016, and then again July to August 2017 to capture the most recently available studies: PsycINFO, Web of Science, PubMed, Scopus, Academic Search Complete, Business Source Complete, CINAHL, ERIC, MEDLINE, and ProQuest Dissertations and Theses Global.[1] No date restraints were imposed on the databases. We also combed the reference lists of existing mindfulness and meditation reviews to find any other potentially relevant articles not captured with the electronic searches.

The electronic searches were conducted using a combination of key words across three categories (see Appendix 2 for a complete list):

- **Set 1:** Contemplative practices (e.g., “mindfulness”, “MBSR”, “MBCT”, “meditation”, “ACT”)
- **Set 2:** Workplace (e.g., “work-based”, “organization”, “employee”)

[1] Further checks were completed on Google Scholar to locate any additional missing studies across the medicine, organizational behavior, management, education, and nursing subject areas.

- **Set 3:** Program design (e.g., “training”, “program”, “intervention”)

The database searches consisted of all keywords from sets 1 to 3, using the Boolean operator “OR” to separate words within each group, and the “AND” operator to combine each group. This ensured that any study with at least one word from each set would be captured. Truncation symbols were added to word stems to ensure all associated spellings were captured.

These procedures led to the identification of 8,016 records. Initial screening of titles and abstracts led to the exclusion of 7,557 articles due to study duplication, obvious irrelevancy, or clear failure to meet the inclusion criteria set out below (see Appendix 3 for a systematic search flow diagram). After applying the eligibility criteria, the resulting set of articles were screened following the procedure specified by Wood (2008) to eliminate bias created by duplicate studies. After removing outliers (discussed shortly), an overall database consisting of 116 sources (102 published), reporting data from 119 unique studies (N = 6,044) fit our eligibility criteria, yielding a mean sample of 50.78 (SD = 41.79). Studies consisted of 54 randomized controlled trials, 46 single sample studies, and 19 quasi-experimental studies. Of these, 55 studies identified as examining mindfulness interventions, 13 examined ACT-based interventions, 14 examined meditation interventions, and 37 used a combination of activities.[2]

Inclusion and Exclusion Criteria

We set six inclusion criteria *a priori*: (a) The study involved adult employee participants examined within an organizational setting. Studies that investigated clinical patients, students, unemployed or community samples were excluded. (b) The study was intervention-based.

[2] Year of publication showed that growth in mindfulness-based studies and combined intervention studies has accelerated more quickly in recent years than interventions focusing on meditation. The vast majority of studies focusing on mindfulness (78%) and combined interventions (86%) became available post-2010, compared to studies investigating meditation (56%). Similarly, a greater proportion of meditation-focused studies were available pre-2000 (36%), compared to mindfulness-based (3%) sand combined intervention (3%) studies.

Randomized-controlled trials (RCTs), quasi-experiments, and single sample (uncontrolled) pre-post interventions were included; correlational studies were excluded. (c) One or more forms of mindfulness-, meditation-, ACT-based, or combined therapies were a significant component of the delivered intervention or training program, and was mentioned in the title, abstract, or keywords of the study. While we initially considered yoga interventions, we later chose to exclude these, given their use as a control in some studies (e.g., Wolever et al., 2012). (d) The study reported sufficient data to extract an effect size or provided information that could be converted into an effect size (e.g., *t*, *F*, *p*). (e) Employee psychological distress (i.e., overall distress, depression, anxiety, burnout, stress, negative affect, somatic symptoms) was tested as a dependent variable. (f) The study was published in English.

Data Coding and Interrater Reliability

Using a systematic coding sheet, the coding of 50 studies was shared by three authors and then all 50 studies were independently recoded by a fourth author to test rating consistency. Percentage agreement was 95.18% across all coding categories. For nominal variables (e.g., type of intervention), Cohen's (1960) kappa was computed between the initial and secondary ratings. The resulting kappa suggested substantial agreement (κ = .64 to .83). For continuous variables (e.g., means and standard deviations for groups at each time point), a two-way mixed, absolute, single measures intra-class correlation coefficient (ICC; McGraw & Wong, 1996) was computed. The resulting ICCs were excellent (ICC = .95 to 1.00), indicating high agreement across coders. (See Appendix 4 for full range of inter-rater reliability statistics.) Interrater disagreements were primarily due to reporting inconsistencies or irregular reporting within primary studies and were resolved via discussion. An abridged summary of the coding for the most important variables and individual study-level effect sizes is included in Appendix 5.

Codes identified characteristics of the study: *N*, year of publication, study design, groups included (active control, wait-list control, no control group), whether randomization occurred,

type of intervention facilitator, length of follow-up (in months), and the outcome measures. For active control groups, a further distinction was made between education only comparisons (e.g., diversity training) with those that received an alternative form of therapy (e.g., cognitive behavioral therapy). We also coded characteristics of the participants including industry of employment. Finally, we coded for characteristics of the intervention: program duration (in weeks), number and duration of sessions (in hours), and intervention protocol (e.g., MBSR, MBCT, combination of treatments).

Quality Assessment of Individual Studies

To assess study quality, we adapted the Downs and Black (1998) criteria, which assesses adequacy of reporting, internal and external validity, and study power. We pilot-tested the quality assessment on a random set of five studies and made several refinements, including removing criteria that were specific to pharmaceutical trials and noting whether the study was pre-registered (see Appendix 6 for the full checklist with our modifications). We trialed the refined criteria with a few more studies, ensuring interrater consistency, and then full quality ratings were performed on the first 80 studies by the first two authors. Interrater agreement was $r = .91$. Disagreements were resolved via discussion. The remaining studies were then rated for quality solely by the second author.

Meta-Analytic Procedures

All analyses were conducted using Comprehensive Meta-Analysis version 3 (Borenstein, Hedges, Higgins, & Rothstein, 2014). We used the means, *SDs*, and *Ns* to compute effect sizes for each variable across each time-point for each sample within the 119 studies. The results of two samples included only follow-up effects, leaving 117 for post-treatment analysis. When only sub-scale scores were reported for a multi-dimensional construct (e.g., Maslach Burnout Inventory; Maslach, Jackson, & Leiter, 1996), the means and *SD*s were recorded for each facet and a total effect size was derived from the mean of these, assuming non-independence among

the facets (Borenstein et al., 2009). When these data were not available, we used other statistics (*F*, *p*, *t*) to estimate an effect size. In cases where these data were missing, we emailed the authors when possible to directly request the required information.

Individual effect sizes were aggregated using a random effects model, which allows parameters to vary across studies and provides an estimate of the variance in effect sizes. Random effects models lead to more accurate effect size estimates that are generalizable beyond the studies included in the meta-analysis, and lead to more plausible confidence intervals (Field, 2003; Hunter & Schmidt, 2000; Kisamore & Brannick, 2008; Schmidt, 2010). As no studies included a pre-post correlation, we used a relatively conservative imputed correlation of $r = .50$ between pre- and post-treatment to generate effect sizes.[3]

We calculated Cohen's *d* for each variable in each study, as well as a total Cohen's *d* across the studies for each variable. A 95% confidence interval (CI) was constructed around each effect to assess the precision of the estimate, and we also calculated the associated *p* and *z* values. Effect sizes were estimated at post-intervention and at last follow-up, separating the aggregation procedure across the different study designs (single sample, randomized controlled trial, and quasi-experimental). This procedure was necessary because combining studies that use randomization with those that do not may artificially inflate effect sizes (Higgins & Green, 2011). Similarly, combining different designs to establish summary effects may make effect sizes difficult to interpret due to divergent points of comparison across study designs (Borenstein et al., 2009; Eby et al., 2018). In single sample studies, *d* was derived from an analysis of change from pre to post-intervention. For RCT and quasi-experimental designs, *d* was derived from the

[3] As recommended by Higgins and Green (2011), a sensitivity analysis was performed using a varying estimate for the imputed pre-post correlation ($r = .70$) for overall distress across each design. Across designs, effects and corresponding CIs were almost identical at the varying strengths of pre-post correlation, indicating that results were robust at different strengths of imputed correlations.

difference in change between groups from pre to post-intervention. In interpreting our findings, we used Cohen's (1988) conventions for small (d = .20), moderate (d = .50), and large (d = .80) effects.

Heterogeneity was assessed with the Q and I^2 statistics, as well as a 95% prediction interval. The Q statistic, which is based on the chi-square distribution, is a measure of the weighted squared deviations and suggests whether heterogeneity in effect sizes is significant (Borenstein et al., 2009; Sagie & Koslowsky, 1993). Unlike Q, I^2 is not affected by power and measures the proportion of observed variance in effect sizes that indicates real differences in effects (i.e., possible moderators). Following guidelines by Higgins et al. (2003), we interpreted I^2 values of 25% as low, of 50% as moderate, and values of 75% as high. A prediction interval is analogous to a credibility interval (Borenstein et al., 2009) and quantifies how heterogeneity is distributed around the effect size, which is absent in Q and I^2.

For publication bias, we used Egger's test of funnel-plot asymmetry (Eggers, Smith, Schneider, & Minder, 1997) to examine the impact of excluding studies with weak effects. This test regresses the standard normal deviate for each effect size against the estimate's precision. The intercept of the regression line quantifies the asymmetry, with larger and significant deviations from zero suggestive of asymmetry and therefore missing literature. We first searched for bias across each variable from pre- to post-treatment as well as follow up across all study designs, and then used Duval and Tweedie's (2000) trim and fill technique to quantify the bias and estimate bias-adjusted effect sizes across all variables where bias was detected. The trim and fill technique removes the most extreme small studies from the positive side of the funnel plot and imputes them back into the analysis along with a mirror image of each effect on the inverse side of the plot. This process yields a bias-adjusted effect size while maintaining the same variance of effects (Borenstein et al., 2009).

We next explored potential moderators of the overall effects. To facilitate an "apples-with-

apples" comparison of effects across studies, we limited exploration of moderator sub-groups to RCTs, as they were highest in number. To maximize power (Borenstein et al. 2009), we also limited moderator analyses to general psychological distress (rather than specific distress outcomes such as depression or burnout). For categorical moderators, average effect sizes, CIs, and sub-group heterogeneity were estimated, separate for each categorical moderator. Categorical moderator analyses explored whether the effect sizes were moderated by the type of intervention protocol, type of comparison group (i.e., active control, education only, or no-intervention control), the type of intervention facilitator, and the industry in which the intervention was delivered. For numerical moderators, we used meta-regression (Borenstein et al., 2009) to explore whether the observed effects were related to the quality score of the studies, and indicators of dose, including the overall duration of programs or the number of sessions in the program.

Results

Data Inspections

To ensure that meta-analytic results are not driven by outlying, non-representative cases, we first inspected the forest plots and standardized residuals across each design for outliers, which showed one RCT (Zolnierczyk-Zreda et al., 2016) as a clear outlier (N = 144; d = 3.48 [2.90, 4.06], standardized residual = 5.90), and three RCTs (Franco et al., 2010; Klatt et al., 2009; Shonin et al., 2014) as possible outliers. A sensitivity analysis using the "leave-one-out" method (Borenstein et al. 2014) confirmed Zolnierczyk-Zreda et al. (2016) and Shonin et al. (2014) as having a dramatic positive influence on the results, given their larger size. These two studies were thus removed from our analysis. Given the difficulty in determining true outliers from legitimate extreme values in random effect models, particularly in small studies which have larger sampling errors, Schmidt and Hunter (2015) caution against over excluding extreme small studies, which can overcorrect for sampling error. Given their smaller size and less dramatic

impact on our results, we thus include Franco et al. (2010) and Klatt et al. (2009) in our RCT analysis. Using the same procedure, we did not identify any outliers amongst single-sample or quasi-experimental studies.

Effects on Psychological Distress at Post Intervention and Last Follow-Up

Table 2 summarizes meta-analytic estimates of the effect of contemplative interventions on employee psychological distress at post-intervention. Small improvements were observed across single samples (k = 44, N = 1,289, d = 0.48 [.41, .55]) and randomized controlled trials (RCTs; k = 54, N = 3,588, d = 0.39 [.30, .49]). Quasi-experimental study designs (k = 19, N = 1,090, d = 0.59 [CI = 0.40, 0.77]) showed moderate improvements. The weakest effects were observed for burnout related variables, across all study designs. Variability across all other effects can be observed between the different study designs, with effects in the small to moderate range across all variables.

Insert Tables 2 and 3 about here

Forty-eight studies were examined for follow-up effects (Table 3). The length of last follow-up varied considerably, ranging from one month to three years (sample size weighted mean = 4.73 months, SD = 6.08; median = 3 months). The effects on all other variables remained relatively stable to the point of last follow-up across single-sample and RCT designs. Few effects were estimated for quasi-experimental designs due to insufficient studies (k < 3). As the general distress Q and I^2 values in Table 3 suggested follow-up effects were heterogenous, we conducted meta-regressions to examine whether follow-up time-lag (in months) and time-lag squared were related to within-study effects. These tests examine whether effects sizes deteriorated over the reported duration of follow-up across studies. Results showed both time lag (k = 48, β = 0.009, SE = 0.009, [CI = -.001, .027]) and time-lag squared (k = 48, β = 0.0004, SE = 0.0003, [CI =

-.0002, .0010]) were unrelated to effect sizes.

Moderators of the Observed Effects

The *Q* and I^2 values in Tables 2 and 3 show that the effects of contemplative interventions on psychological distress were also heterogeneous immediately after the interventions, which suggests the presence of moderating variables. We report our analyses separately for categorical and numerical moderators, as these require separate analytic procedures.

Categorical moderators. As can be observed in Table 4, there was some evidence that intervention efficacy was moderated by the type of control group, as well as the type of intervention delivered, supported by significant heterogeneity between levels of the moderator for these variables. In particular, effects showed that general meditation-based interventions yielded the highest effects, followed by mindfulness-based interventions and ACT-based interventions showing the smallest effects. Despite the significant subgroup heterogeneity, however, it is important to acknowledge that amongst all of these analyses, there was still some overlap in the CIs across levels of the moderator, indicating that moderation was not substantial. While contemplative interventions performed better than no-intervention comparisons or comparisons that received education only, they were not substantively better than active control comparisons that received another type of therapeutic training – as evidenced by the CI encompassing zero.

It is possible that these effects are driven by interactions between the interventions used and the industry sector. While insufficient data were available to test intervention-sector interactions, a post hoc inductive examination suggests overlap between the two industries and the two treatment protocols that performed best and worst. Of the 10 ACT-based studies, which had the weakest effects, five were delivered in the healthcare sector and the remainder took place in the education (n = 1), government, (n = 2), social work (n = 1) and corporate (n = 1) settings. In contrast, of the eight meditation-based studies, none were delivered in the healthcare sector, four

took place in the education sector, with the remainder in corporate settings (n = 2), and mixed employee (n = 2) populations. The highest effects were observed in the corporate sector, then education, with the weakest effects observed in healthcare.

Insert Table 4 about here

Numerical moderators. Meta-regression results showed that study effect sizes were not substantively related to study quality ratings (k = 117, β = 0.005, SE = 0.012, [CI = -.019, .029]), overall duration of the programs in weeks (k = 108, β = 0.010, SE = 0.008, [CI = -.006, .023]), or the number of sessions included (k = 108, β = 0.002, SE = 0.004 [CI = -.005, .009]), with all CIs encompassing zero.

Publication Bias

We next examined the possibility that the literature included in our meta-analysis is a biased subset of available studies. We first ran Egger's test for the general distress variable from pre- to post-treatment, which contained the most studies (k = 117). Results suggested the presence of asymmetry (a = 0.98, p = .005; Figure 1), indicating a possible upward bias in the estimated effect-sizes at post-treatment. Less bias was evident in follow-up comparisons (k = 48; a = 0.61, p = .19).

Insert Figure 1 about here

Tables 2 and 3 (right side) provide adjusted estimates at post-treatment using Duval and Tweedie's trim and fill method. The highest number of corrections overall were for single sample studies, where studies were trimmed for general distress, stress, and most burnout variables. This led to a reduction in the effect size estimates for these variables, with the largest reduction for the stress, which dropped from d = 0.62 [0.48, 0.75] to d = 0.48 [0.33, 0.62] after 7 studies were

trimmed. RCTs also showed some evidence of bias, and corrections were made for burnout overall and the personal accomplishment facet of burnout. The largest reduction was for burnout overall (d = 0.20 [0.08, 0.33] to d = 0.07 [-0.06, 0.20]) after 9 studies were trimmed, with the adjusted CI encompassing zero.

Discussion

In the present study, we meta-analytically estimated the effect of contemplative interventions in reducing employee distress (e.g., anxiety, stress, burnout, depression). We also examined moderators of these effects, including factors that could falsely inflate estimations of effect sizes. Aligned with prior reviews (e.g., Glomb et al., 2011; Lomas et al., 2017a; 2017b), our results show promise for contemplative interventions as a means to reduce employee psychological distress, with participants showing improvements that were generally sustained at last follow-up. However, publication bias likely enhanced these effects, particularly in single-sample designs where bias was most evident. Below, we discuss our primary findings in more detail, including our main contributions and the practical implications of the meta-analysis. In doing so, we simultaneously raise various limitations with our approach and identify directions for future research.

Overall Effects and Possible Biases.

One of the strongest appeals of contemplative practices is for restorative purposes, with treatments generally showing efficacy in relieving psychological disorders and distress (e.g., Burton et al., 2017; Khoury et al., 2013a, 2013b; Lomas et al., 2017b; Luken & Sammons, 2016). Practices such as MBSR were originally developed to reduce distress with patient populations (Kabat-Zinn, 1990), a tradition that similar protocols including MBCT and ACT have since followed (Brown, Ryan, & Creswell, 2007). As such, there is benefit in considering the effectiveness of various contemplative interventions in everyday work settings, where employees are increasingly stressed, burned-out, and over-committed (Casey & Liang, 2014;

Johnson et al., 2005; Sonnentag & Fritz, 2015). Aligned with other reviews, our results show that there is some evidence to conclude contemplative interventions can be effective in helping employees relieve psychological distress in work settings, with programs yielding small to moderate improvements in distress at post-intervention, which were sustained at last follow-up.

Despite the positive effects, our review confirmed the conclusions of prior reviews that study quality is poor in the workplace literature (Jamieson & Tuckey, 2017; Eby et al., 2018). Adding to existing reviews, we sought to evaluate whether poor quality studies were systematically biasing the inferred conclusions of the literature. Our finding that study quality was unrelated to effect sizes suggests that this is not likely to be the case. We nevertheless implore researchers to employ greater rigor in study design and reporting so that treatment effects across industries and protocols can be more reliably established. We ultimately concur with the recommendations of other reviews (Eby et al., 2018; Jamieson & Tuckey, 2017) in ways to push this literature forward by enhancing methodological rigor. Our review also highlights the importance for future research to utilize active control comparisons in controlled experiments. Like other reviews (e.g., de Vibe et al., 2017) our study did not find contemplative interventions to be more effective than alternative therapies used as active controls (e.g., cognitive-behavioral therapy; relaxation therapy). Thus, whether contemplative interventions are more or less effective in terms of time and resources required presents an important avenue for future research.

Irrespective of study quality, our review demonstrated clear evidence of publication bias, which is likely biasing effects upwards. While the bias was most prevalent in single-sample designs, it was also evident in RCTs. Bias adjustments led to reductions in effects where bias was detected, and in some cases pushed effects initially in the moderate range into the small range (Cohen, 1988). Moreover, in some cases effects that were initially small, such as burnout in RCTs, became indistinguishable from zero. The greater bias observed in burnout related

variables may be attributable to the higher power available to detect bias in these variables, with these interventions commonly delivered in healthcare settings: the most common setting observed. While numerous reviews have supported contemplative practices, none have quantified the extent to which bias may be impacting the conclusions made. Despite the promise of contemplative interventions, our review shows that publication bias is inflating effects.

The detection of publication bias is important for two reasons. First, an upward bias in effects can lead to over-confidence in the efficacy of interventions, which has clear implications for management practice. If organizations are looking to adopt such trainings to benefit their staff, then a realistic appraisal of the likely benefits is important. We believe our adjusted effects more closely approximate the strength of effects that practitioners might expect if they are to roll-out such trainings in their contexts. While these effects are generally smaller than those published elsewhere (cf. Khoury et al., 2015; Virgili, 2015), it is important to note that programs that yield small-effects can still be worth-while pursuing if there are lasting improvements to employee mental health and if large numbers of employees benefit. An important follow-up question from this study is the cost-to-benefit return from such programs, which has received little research attention (cf. van Dongen et al., 2016).

Second, quantifying publication bias may identify directions for future research by clarifying why it is occurring. In particular, it may help to establish whether some study designs are particularly susceptible to over or underestimating effects. Our review suggests that single-sample studies, which already contain less internal validity than controlled designs (Jamieson & Tuckey, 2017), are more affected by bias. This conclusion makes more sense when one considers the assumptions that underpin models of publication bias: larger studies are likely to be published regardless of statistical significance because they require more time and resources (Borenstein et al., 2009). Smaller studies—most single-sample designs—are assumed to involve

less time and resources and are thus more likely to be "lost" when non-significant results are obtained. This conclusion is also reinforced by the noticeably lower bias evident at follow-up.

We suggest it is important for future research to adopt proactive strategies to ensure that the file drawer effect is minimized and future research outputs establish accurate cumulative knowledge. This might involve, for example, limiting the use of single-sample studies as a way to generate knowledge. Despite most of the single-sample studies in our database identifying as pilot or preliminary (n = 23; 52%), the literature has clearly moved beyond a need for pilot research, and if such designs are to be used, we suggest they are published with larger follow-up studies that employ proper controls, adequate statistical power, and randomization—characteristics of research that appears less affected by bias in similar reviews (e.g., de Vibe et al., 2017). Other useful strategies might involve the proactive pre-registration of confirmatory analysis plans and establishing required power *a priori*, which are known methods of proactively reducing publication bias (Button et al., 2013; Munafò et al., 2017).

Interestingly, across all designs our effects were smaller for burnout related variables than for other distress-related outcomes, particularly after publication bias corrections. This is contrary to the conclusions of some qualitative reviews (e.g., Lomas et al., 2017b; Jannsen et al., 2018) that are based on a count of statistically significant findings. Unfortunately this is a notoriously unreliable procedure (see Borenstein et al. 2009, Ch. 28 for a review), highlighting the importance of meta-analysis. While some meta-analyses (e.g., Lomas et al., 2018b, 2018c) show small to moderate effects for burnout, these findings are likely to be biased upwards given the omission of unpublished literature in these studies, as well as the smaller number of studies included. Our finding, based on a broader sample of literature, is consistent with the notion that burnout does not easily respond to interventions that target the individual, but rather perhaps need to be combined with structural interventions that concurrently address the work environment (see Maslach, Schaufeli, & Leiter, 2001), such as cultivating a climate that is

supportive of employee mindfulness (e.g., Slemp, Kern, Patrick, & Ryan., 2018). We suggest that future research should directly consider the efficacy of contemplative interventions for improving job burnout, as well as multi-level approaches that simultaneously target both the individual and their work context.

Moderation Analyses

Although our moderation analyses are limited insofar as they are exploratory rather than confirmatory, they raise a number of interesting directions for future research. Our results showed meditation-based interventions yielded the largest effects, whereas combined programs and particularly ACT yielded the smallest effects. A key distinction between ACT and alternative interventions is that both mindfulness and meditation-based interventions involve sustained contemplative practice as a key ingredient, while in ACT it is only one of several core features amongst many other non-meditative exercises (Brown, Ryan, & Creswell, 2007; McCracken & Vowles, 2014). This also typifies most combined programs.

We explored whether the smaller effect for ACT studies was related to the industry in which these studies took place, as well as the types of controls utilized. Half of the ACT studies were delivered in healthcare settings, which also showed smaller effects than other industries, whereas most meditation-based studies, which yielded the strongest effects, took place in the education or corporate sector, which showed stronger effects. Thus, there is overlap between the industries and the treatment protocols that showed the strongest and weakest effects. An interesting avenue for future research is to examine whether it is the industry, the protocol, or a combination of both that is the real causal determinant in effect-size variability. It is possible, for example, that interventions delivered in healthcare settings are less effective for a number of reasons. Participants within high-stress occupational settings, such as healthcare (Hakanen, Bakker, & Schaufeli, 2006; Kristensen et al., 2005; Travers & Cooper, 1996), might see these interventions as additional tasks to add to their already busy schedules, thus creating a source of

stress rather than providing benefit. Similarly, most healthcare studies utilized burnout as a dependent variable, which may not be sensitive to contemplative interventions. Nevertheless, the relatively small *k* among some levels of these moderator analyses still suggests that these results might be interpreted with caution and are an avenue for future investigation. Future studies might consider not only effects based on industry, but also what aspects of that occupation or protocols make contemplative interventions more or less successful.

We did not find effects to diminish as a function of follow-up time-lag. While this might suggest that effects are long-lasting, it is also possible that effects were maintained for reasons that were not reported within the studies. For example, it was often unclear whether followed-up participants continued to regularly engage in their contemplative practice or other variants of therapy after the conclusion of intervention-training, which might explain this finding. It will be important for future studies to report information that could allow this to be established.

Conclusion

The prevalence of psychological distress and mental illness continues to rise in workplaces, impacting personal and professional lives globally. Considering the time people spend working, both in offices and at home, employers play an important role in either contributing to or helping to prevent psychological distress from occurring. Contemplative training provides one approach to proactively support employee mental health. However, strategies are needed to limit the effect of publication bias when estimating program effectiveness, including ensuring greater research rigor, so that more reliable, cumulative knowledge can be established.

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TABLES

Table 1

Previous systematic reviews and meta-analyses examining various forms of contemplative interventions in work settings

Study	Interventions examined	Samples studied	Total *k*	Designs included	Quality rating of studies	Reported meta-analysed summary effect for distress [95% CI)	Published and unpublished	Publication bias corrections reported	Primary dependent variables covered	Moderators reported
Boellinghaus et al. 2014	Mindfulness & LKM	Health-care professionals	12	Single sample, RCT, qualitative, prospective cohort controlled	No	Meta-analysis not performed	No	--	Self-compassion and other-focused concern	--
Burton et al. 2017	Mindfulness	Health-care professionals	9	RCT, single sample, quasi-experimental	Yes	*(Stress only):* POST-INTERVENTION: $k = 7$, $N = 188$; $r = .34$ [.20, .47]; FOLLOW-UP: not examined	No	No	Stress	None
Donaldson-Fielder et al. 2018	Mindfulness & meditation	Leaders, manager, supervisors	19	RCT, single sample, quasi-experimental, mixed methods, qualitative	Yes	Meta-analysis not performed	Yes	--	Well-being, leadership, mindfulness, performance	--
Eby et al. 2018	Mindfulness	General working adults	63	RCT, single sample, quasi-experimental, multiple baseline,	No	Meta-analysis not performed	No	--	Well-being, distress, mindfulness performance,	--

				randomized switching replications design					physiological outcomes, behavior	
Escuriex & Labbé 2011	Mindfulness	Health-care professionals	20	RCT, single sample, quasi-experimental	No	Meta-analysis not performed	Yes	--	Well-being, distress, mindfulness	--
Hwang et al., 2017	Mindfulness, combination programs	Teachers	16	Quantitative (RCT, single sample, quasi-experimental), and qualitative	Yes	Meta-analysis not performed	No	--	Well-being, distress, mindfulness	--
Irving et al. 2009	MBSR	Health-care professionals and healthcare students	10	RCT, quasi-experimental, single sample	No	Meta-analysis not performed	No	--	Well-being, distress, physiological outcomes	--
Jannsen et al. 2018	Mindfulness	General working adults	23	RCT and quasi-experimental	Yes	Meta-analysis not performed	No	--	Well-being, distress, mindfulness, sleep	--
Jamieson & Tuckey 2017[a]	Mindfulness	General working adults	40	RCT, quasi-experimental, single sample	No	Meta-analysis not performed	No	--	Well-being, distress, performance, objective measures	--
Klingbeil & Renshaw 2018	Mindfulness	Teachers	29	RCT, quasi-experimental	No	POST-INTERVENTION: $k = 27$, $N = 1{,}469$; $g = .55$ [.37, .73] [a] FOLLOW-UP: not examined	Yes	Yes	Mindfulness, well-being, distress, physiological outcomes, classroom climate	Randomization, program developer, dose

Lomas et al. 2017a	Mindfulness	Employees in educational context	19	RCT, Single sample	Yes	Meta-analysis not performed	No	--	Well-being, distress, performance, mindfulness	--
Lomas et al. 2017b	Mindfulness	General working adults	153	RCT, single sample, quasi-experimental, correlational	Yes	Meta-analysis not performed	No	--	Well-being, distress, performance, mindfulness	--
Lomas et al. 2018a	Mindfulness	Healthcare professionals	81	RCT, single sample, quasi-experimental, correlational	Yes	Meta-analysis not performed	No	--	Well-being, distress, performance, mindfulness	--
Lomas et al. 2018b	Mindfulness	Healthcare professionals	41	RCT, single sample, quasi-experimental	Yes	POST-INTERVENTION: *Single sample* (*k* = 5, *N* = 169): *g* = -.54 [-.75, -.33] *RCT* (*k* = 7, *N* = 687): *g* = -.61 [-.79, -.44] FOLLOW-UP: not examined	No	No	Well-being, distress, performance, mindfulness	Design, publication year, gender, age, profession, dose, professional activity, quality
Lomas et al. 2018c	Mindfulness	General working adults	35	RCTs only	Yes	POST-INTERVENTION: *RCT* (*k* = 14): *g* = -.56 [-.72, -.41] [b] FOLLOW-UP: not examined	No	No	Well-being, distress, performance, mindfulness	Design, publication year, gender, age, profession, dose, professional activity, quality
Luken & Sammons 2016	Mindfulness	Healthcare professionals	8	RCTs only	Yes	Meta-analysis not performed	No	--	Burnout	--

Morgan et al. 2015	Mindfulness	Healthcare professionals	14	Qualitative only	Yes	Meta-analysis not performed	No	No	--	Experiences overcoming obstacles during training, and perceived benefits of training
Rudaz et al. 2017	MBCT, MBSR, MSC, ACT	Mental health professionals	24	RCT, single sample, quasi-experimental	No	Meta-analysis not performed	No	--	Well-being, distress	--
Smith 2014	MBSR	Nurses	13	RCT, single-sample, quasi-experimental	No	Meta-analysis not performed	No	--	Well-being, distress	--
Trowbridge & Lawson 2016	Mindfulness	Social workers	10	Mixed-method, general quantitative (e.g., correlational, single sample) and qualitative	Yes	Meta-analysis not performed	No	--	Well-being, distress	
Virgili 2015	Mindfulness	General working adults	19	RCT, single sample, quasi-experimental	Yes	POST-INTERVENTION: k = 19, N = 652; g = .68 [.58, .78] [a] FOLLOW-UP: k = 6, N = 193; g = .60 [.46, .75] [a]	Yes	No [c]	Distress	Intervention, industry, design (RCT and other), duration
The present study	Contemplative interventions	General working adults	119	RCT, single-sample, quasi-experimental	Yes	Post-intervention and follow-effect effects across all designs can be seen in Tables 2 and 3	Yes	Yes	Distress	Intervention, industry, type of control, type of facilitator, dose, follow-up lag, study quality

Note: Reviews are listed in alphabetical order and represent those reviews that were publicly available in December 2018. Contemplative Interventions = interventions that identify as some variant of mindfulness, meditation, ACT, or combination thereof; LKM = Loving Kindness Meditation; MBSR = mindfulness-based stress reduction; MBCT = mindfulness-based cognitive therapy; MSC = mindful self-compassion; ACT = acceptance and commitment therapy; RCT = randomized controlled trial; *k* = number of studies in the analysis; *N* = combined number of participants across studies; [a] = Reported summary effects were aggregated across study designs; [b] Total *Ns* for each summary effect were not reported in Lomas et al. (2018c). [c] = trim and fill analyses were conducted in Virgili (2015), but no effect size corrections were performed.
Only studies with working adult samples are shown. Only systematic reviews are shown, narrative and theoretical reviews are not shown. For space reasons, only the overall categories of dependent variables are listed, and in some cases, reviews examined narrower variants of dependent variables within these categories.

Table 2

Effect Size (Cohen's *d*) Estimates for the Effect of Contemplative Interventions on Psychological Distress Related Variables from Baseline to Post-Intervention.

Variable	Effect size data					Heterogeneity			Publication bias analyses		
	k	*N*	Cohen's *d* [95% CI]	*SE*	Z	*Q*	I^2	95% PI	Egger's test [c]	Adjusted *d* [95% CI]	Studies trimmed
Single Sample Designs											
General distress [a]	44	1,289	.48 [.41, .55]	.04	13.28***	58.15	26	[.23, .73]	1.64**	.40 [.33, .48]	10
Depression	13	329	.46 [.33, .58]	.06	7.12***	14.09	15	[.21, .70]	2.56	--	--
Anxiety	15	309	.45 [.29, .61]	.08	5.53***	24.44*	43	[-.03, .93]	2.96	--	--
Stress	29	822	.62 [.48, .75]	.07	9.05***	79.47***	65	[.01, 1.21]	2.11*	.48 [.33, .62]	7
Burnout [b]	25	816	.48 [.38, .58]	.05	9.48***	41.46*	42	[.15, .83]	2.59**	.37 [.26, .48]	7
Emotion exhaustion	18	564	.45 [.35, .55]	.05	8.43***	23.04	26	[.18, .72]	2.25*	.35 [.23, .47]	6
Depersonalization	13	426	.28 [.19, .38]	.05	5.69***	10.71	0	[.17, .39]	1.72*	.26 [.15, .36]	2
Personal Accompl.	13	445	.41 [.27, .55]	.07	5.75***	22.82*	47	[-.01, .82]	1.24	--	--
Negative affect	10	305	.51 [.31, .70]	.10	5.07***	21.78**	59	[-.09, 1.10]	2.58	--	--
Somatic symptoms	5	125	.56 [.27, .85]	.15	3.73***	9.38	57	[-.38, 1.50]	--	--	--
Randomized Controlled Trial Designs											
General distress [a]	54	3,588	.39 [.30, .49]	.05	8.14***	105.62***	49	[-.09, .87]	.65	--	--
Depression	13	727	.42 [.24, .59]	.09	4.73***	16.95	29	[.01, .83]	.61	--	--
Anxiety	11	570	.58 [.37, .79]	.11	5.44***	14.46	31	[.08, 1.07]	.01	--	--
Stress	32	2,260	.47 [.35, .58]	.06	7.60***	66.44***	53	[-.04, .97]	1.17	--	--
Burnout [b]	24	1,331	.20 [.08, .33]	.06	3.19***	31.29	26	[-.15, .56]	1.61*	.07 [-.06, .20]	9
Emotion exhaustion	17	792	.29 [.15, .42]	.07	4.10***	16.15	1	[.12, .45]	1.21	--	--
Depersonalization	13	563	.17 [.02, .33]	.08	2.14**	7.48	0	[-.01, .35]	.09	--	--
Personal Accompl.	13	527	.33 [.15, .50]	.09	3.64***	14.00	11	[.01, .64]	1.68*	.23 [.05, .42]	4

Variable	Effect size data					Heterogeneity			Publication bias analyses		
	k	*N*	Cohen's *d* [95% CI]	*SE*	Z	*Q*	I^2	95% PI	Egger's test [c]	Adjusted *d* [95% CI]	Studies trimmed
Negative affect	4	288	.50 [.26, .73]	.12	4.12***	2.77	0	[-.02, 1.01]	--	--	--
Somatic symptoms	4	353	.40 [.03, .77]	.19	2.13*	5.96	50	[-.97, 1.78]	--	--	--
Quasi-Experimental Designs											
General distress [a]	19	1,090	.59 [.40, .77]	.10	6.16***	50.95***	65	[-.12, 1.29]	1.58	--	--
Depression	5	243	.46 [.16, .76]	.15	3.00**	5.13	22	[-.25, 1.16]	--	--	--
Anxiety	6	312	.32 [.14, .49]	.09	3.53***	3.51	0	[.07, .56]	--	--	--
Stress	12	712	.59 [.34, .85]	.13	4.54***	37.15***	70	[-.28, 1.46]	1.02	--	--
Burnout [b]	8	394	.33 [.05, .60]	.14	2.35**	14.71	52	[-.44, 1.09]	--	--	--
Emotion exhaustion	4	196	.12 [-.16, .40]	.14	.84	2.65	0	[-.50, .74]	--	--	--
Depersonalization	4	196	.14 [-.14, .42]	.14	.97	1.97	0	[-.48, .76]	--	--	--
Personal Accompl.	4	196	.15 [-.15, .45]	.15	.95	3.40	12	[-.66, .95]	--	--	--
Negative affect	3	212	.56 [.26, .85]	.15	3.74***	3.18	37	[-.2.23, 3.34]	--	--	--
Somatic symptoms	3	175	.80 [-.14, 1.74]	.48	1.67	17.07***	88	[-10.81, 12.41]	--	--	--

Note: *k* = number of studies in the analysis; *N* = combined number of participants across studies; CI = confidence interval; *SE* = standard error; PI = prediction interval; Adjusted *d* = adjusted value of Cohen's *d* using Duval and Tweedie's (2000) trim and fill method; Effect sizes and CIs are derived from a random effect model; [a] Effect sizes include composites comprised of general psychological distress, and all other distress related variables; [b] Burnout includes composites that consist of general burnout, and all other burnout facets across studies. [c] Egger's test was performed when greater than 10 studies were available. Reported data includes intercept and significance (one-tailed). * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

Table 3

Effect Size (Cohen's *d*) Estimates for the Effect of Contemplative Interventions on Psychological Distress Related Variables from Baseline to Last Follow-Up

Variable	Effect size data					Heterogeneity			Publication bias analyses		
	k	*N*	Cohen's *d* [95% CI]	*SE*	Z	*Q*	I^2	95% PI	Egger's test [c]	Adjusted *d* [95% CI]	Studies trimmed
Single Sample Designs											
General distress [a]	22	638	.55 [.41, .69]	.07	7.53***	54.00***	61	[-.01, 1.11]	2.41*	.45 [.29, .61]	4
Depression	3	67	.61 [.34, .87]	.13	4.52***	1.91	0	[-1.09, 2.30]	--	--	--
Anxiety	5	95	.46 [.15, .77]	.16	2.91**	7.60	50	[-.47, 1.39]	--	--	--
Stress	17	482	.55 [.35, .74]	.10	5.56***	58.52***	72	[-.19, 1.29]	2.05	--	--
Burnout [b]	11	293	.58 [.35, .81]	.12	4.91***	30.54***	67	[-.18, 1.33]	2.28	--	--
Emotion exhaustion	8	213	.52 [.32, .72]	.10	5.17***	11.88	41	[.02, 1.02]	--	--	--
Depersonalization	3	93	.51 [.29, .73]	.11	4.62***	.56	0	[-.90, 1.92]	--	--	--
Personal Accompl.	4	129	.72 [.45, .98]	.13	5.35***	5.06	41	[-.22, 1.65]	--	--	--
Negative affect	6	124	.67 [.45, .89]	.11	6.01***	6.07	18	[.24, 1.15]	--	--	--
Randomized Controlled Trial Designs											
General distress [a]	22	1,815	.36 [.24, .47]	.06	6.24***	27.78	24	[.07, .64]	-.44	--	--
Depression	4	247	.78 [.31, 1.26]	.24	3.23***	8.15*	63	[1.13, 2.71]	--	--	--
Anxiety	5	338	.74 [.51, .96]	.11	6.53***	1.66	0	[.38, 1.09]	--	--	--
Stress	11	1,015	.44 [.27, .61]	.09	5.16***	17.99	44	[-.02, .91]	.46	--	--
Burnout [b]	10	689	.20 [.05, .35]	.08	2.57**	5.58	0	[.02, .38]	.15	--	--
Emotion exhaustion	5	304	.43 [.05, .81]	.19	2.19*	9.77*	59	[-.78, 1.63]	--	--	--
Depersonalization	5	304	.23 [-.09, .55]	.16	1.42	7.11	44	[-.68, 1.15]	--	--	--
Personal Accompl.	5	263	.01 [-.24, .25]	.12	.04	3.44	0	[-.39, .40]	--	--	--
Quasi-Experimental Designs											

Variable	Effect size data					Heterogeneity			Publication bias analyses		
	k	*N*	Cohen's *d* [95% CI]	*SE*	Z	*Q*	I^2	95% PI	Egger's test [c]	Adjusted *d* [95% CI]	Studies trimmed
General distress [a]	4	272	.52 [.11, .92]	.21	2.51*	8.77*	66	[-1.17, 2.20]	--	--	--
Stress	3	198	.49 [-.05, 1.02]	.27	1.79	8.46*	76	[-5.81, 6.80]	--	--	--

Note: Only variables for which $k > 3$ studies are shown; *k* = number of studies in the analysis; *N* = combined number of participants across studies; CI = confidence interval; *SE* = standard error; PI = prediction interval; Adjusted *d* = adjusted value of Cohen's *d* using Duval and Tweedie's (2000) trim and fill method; Effect sizes and CIs are derived from a random effect model; [a] Effect sizes include composites comprised of general psychological distress, and all other distress related variables. [b] Burnout includes composites comprised of general burnout, and all other burnout facets across studies; [c] Egger's test was performed when greater than 10 studies were available. Reported data includes intercept and significance (one-tailed). * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

Table 4

Categorical Moderator Analyses on Effect of Contemplative Interventions on General Psychological Distress

Moderator	General psychological distress					Subgroup heterogeneity		
	k	*N*	Cohen's *d* [95% CI]	*SE*	*Q*	*Q*	*df*	*Sig.*
Intervention [a]								
Mindfulness-based	21	1,161	.47 [.32, .63]	.08	46.40*			
Meditation-based	8	452	.67 [.43, .90]	.12	17.13*			
Combination	15	988	.33 [.17, .49]	.08	7.37			
ACT-based	10	987	.19 [.01, .37]	.09	15.52			
Random effect between groups						11.83	3	.008
Intervention facilitator								
Study authors	9	739	.27 [.09, .44]	.08	17.31*			
Mindfulness trained instructor [a]	12	564	.43 [.26, .60]	.09	11.81			
Health practitioner [b]	7	632	.11 [-.08, .30]	.10	6.29			
Self/online/audio training	7	400	.36 [.16, .60]	.10	3.58			
Random effect between groups						6.84	3	.077
Type of comparison group								
Active comparison	13	698	.17 [-.03, .37]	.10	42.14***			
Education only comparison	3	173	.44 [.07, .81]	.19	6.34			
No intervention comparison	38	2,717	.45 [.34, .55]	.05	45.82			
Random effect between groups						6.04	2	.049
Participant industry								
Corporate	8	511	.56 [.30, .82]	.13	1.94			
Education	16	1,161	.49 [.32, .66]	.09	50.06***			
Healthcare	18	849	.21 [.04, .38]	.09	29.31			
Mixed	6	419	.34 [.08, .61]	.14	4.60			
Random effect between groups						7.10	3	.069

Notes: To remove heterogeneity due to differences in study designs, only RCTs are included in the categorical moderator analyses; *k* = number of studies in the analysis; *N* = combined number of participants across studies; CI = confidence interval; *SE* = standard error; MBSR = mindfulness-based stress reduction; MBCT = mindfulness-based cognitive therapy; ACT = acceptance and commitment therapy; Combination = programs consisting of combinations of mindfulness-based therapies or other activities; [a] mindfulness trained instructors included facilitators who have undertaken training in mindfulness-specific intervention delivery; [b] Health practitioners comprised medical professionals, including psychologists and psychiatrists; * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

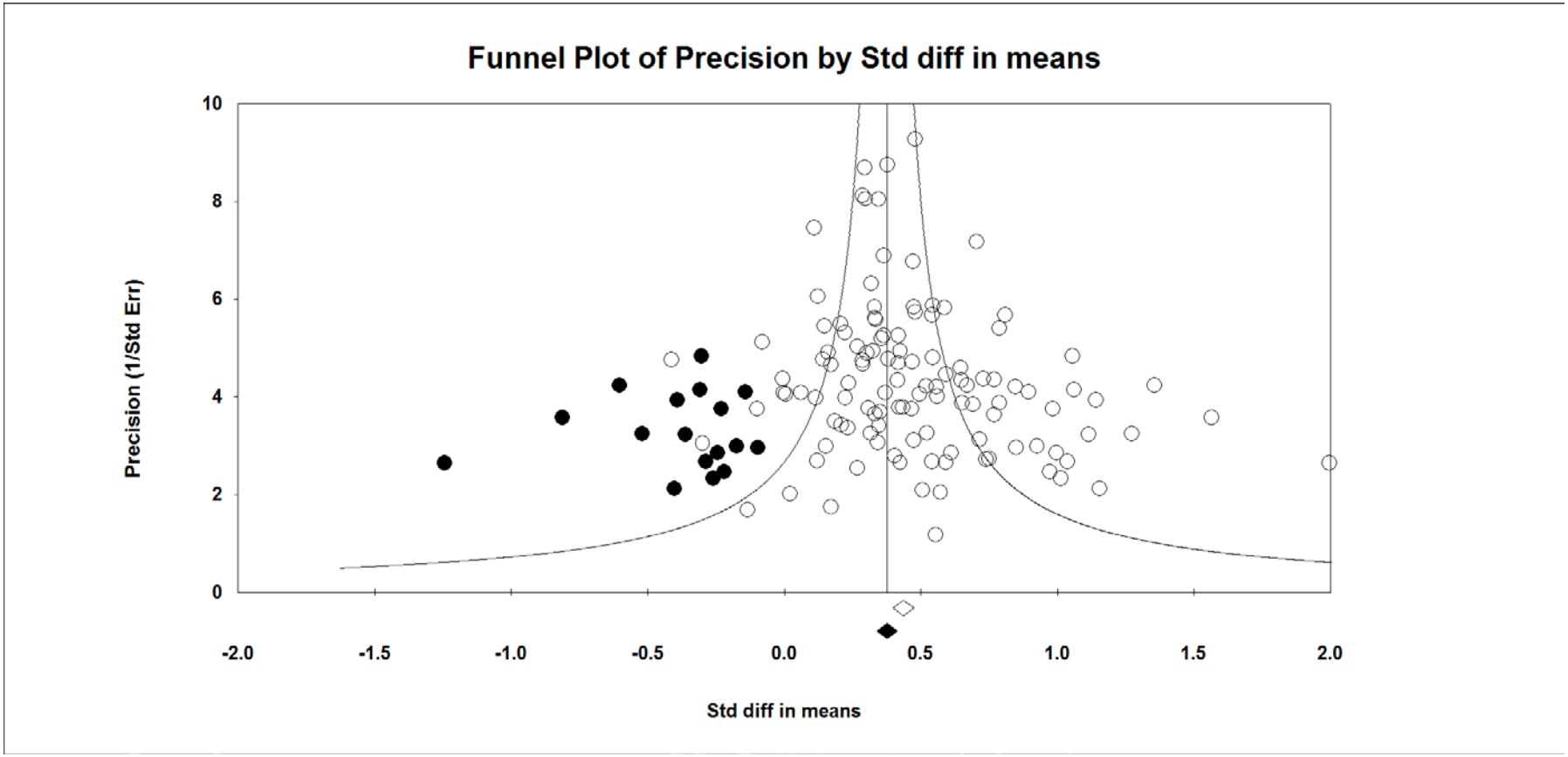

Figure 1. Funnel Plot of Precision by Standard Difference in Means with Imputed Studies Across all Psychological Distress Outcomes (k = 117). Note: Black circles represent imputed studies, white circles represent observed studies; k = 17 studies trimmed and imputed.

Appendix 1

Commonly Used Contemplative Interventions in Work Settings

Type of intervention	Description	Example study
	Mindfulness-based training	
Mindfulness-based stress reduction (MBSR)	A structured eight-week program consisting of guided meditation, formal lessons on stress, self-compassion and communication, and light yoga-based exercise. Classes last 2.5 hours and there is typically a full day silent meditation retreat after program completion.	Goodman & Schorling, 2012
Mindfulness-based cognitive therapy (MBCT)	An eight-week program originally tailored for individuals experiencing depression. Includes a substantial mindfulness component (e.g., breathing, body-scan, mindful movement), coupled with exercises drawn from cognitive-behavioural therapy (e.g., recognising unhelpful thoughts, reframing)	Ruths et al. 2012
Acceptance and commitment therapy (ACT)	A therapy fostering acceptance of thoughts and experiences, to gain psychological flexibility and minimise maladaptive avoidance behaviours, while also clarifying one's values and striving to live according to these. Mindfulness (e.g., focusing on breath and noticing thoughts without judgment) is typically a component, although not often the central component.	Bethay et al. 2013
	Meditation-based training	

Transcendental meditation (TM)	A concentration-based meditation practice rooted strongly in the "Samatha" Theravadin Buddhist meditation traditions. Individuals focus the whole of their attention on a specific object, commonly a mantra.	Punyaniyama, 1996
Insight Meditation	A "Vipassana" Theravadin Buddhist meditation that draws elements of concentration-based practice from TM, but additionally includes a close, non-judgemental observation of the outer world, with reflective, tolerant awareness.	Sheppard et al. 1997
General meditation awareness training	Typically, an eight-week program including mindfulness exercises, group discussion, and facilitator-led teachings. A secular analogue of traditional Buddhist meditation classes.	Shonin et al. 2014
	Combined training	
Cultivating Awareness and Resilience in Education (CARE)	A 30-hour program designed for teachers, designed to improve social-emotional understanding and improve classroom interactions with students. Includes mindfulness exercises (e.g., breath awareness, mindful movement), as well as lessons in compassion, and emotional regulation and awareness.	Jennings et al. 2017
REsilience and Activity for every DaY (READY)	An 11-week group resilience training program for stressed individuals. Involves didactic teaching, group discussions, and meditation exercises. Aims to improve positive emotions, increase cognitive flexibility and meaning in life, and encourage active stress coping strategies such as exercise.	Burton et al. 2010

Note: Combined trainings also include mixed delivery of programs (e.g., MBCT + MBSR), or programs that contain contemplative training with a separate form of therapy (e.g., psycho-education).

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Appendix 2

Search terms

Set 1
Mindful* OR MBSR OR MBCT OR Meditat* OR “Contemplative practice” OR “Cultivating Awareness and Resilience in Education” OR “REsilience and Activity for every DaY” OR “Acceptance and Commitment Therapy” OR “Learning to Breathe”

Set 2
Work-based OR Workplace OR Employee* OR Staff OR Organi?ation OR Healthcare OR Teachers OR Vocation* OR Occupation*

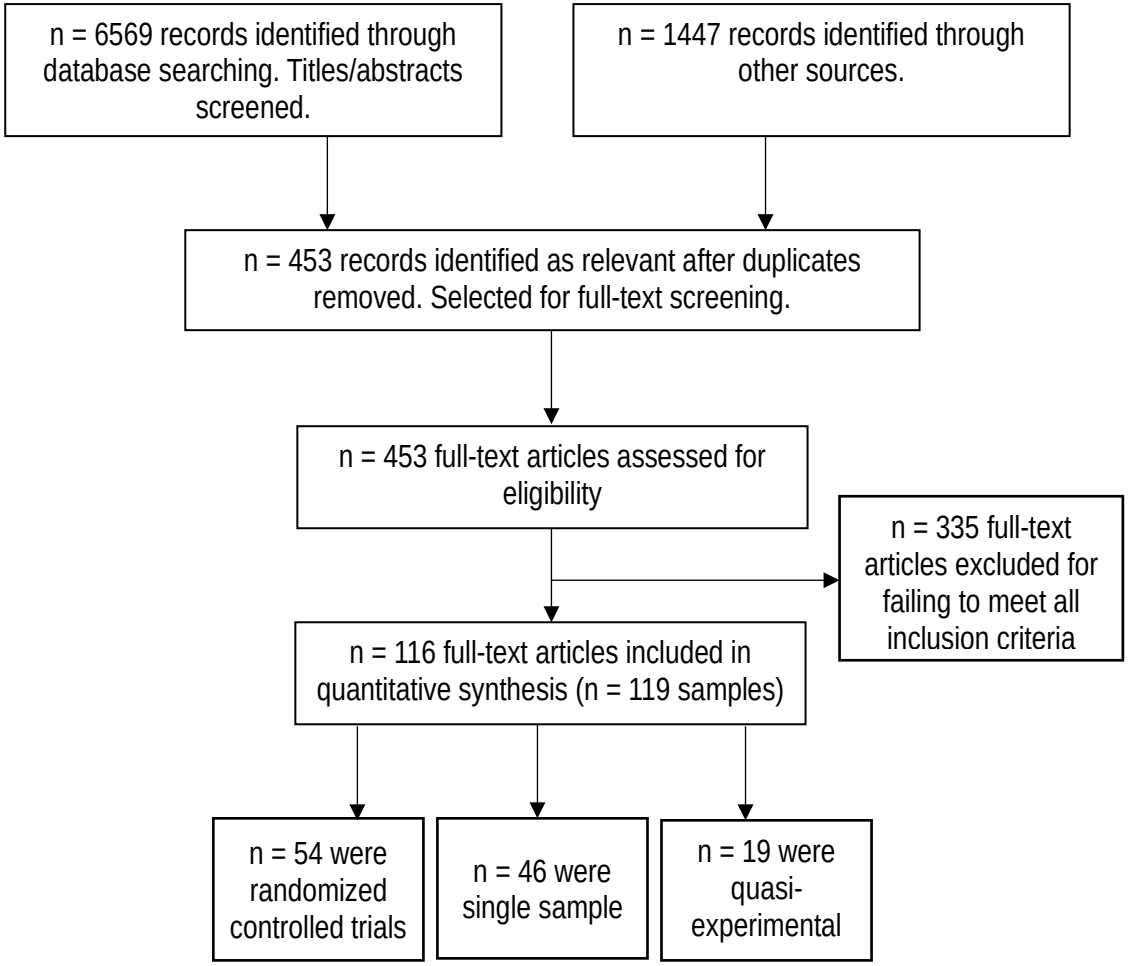
Set 3
Training OR Program* OR Intervention* OR pilot OR trial

Searched databases:

- PsycINFO
- Web of Science
- PubMed
- Scopus
- Academic Search Complete
- Business Source Complete
- CINAHL
- ERIC
- MEDLINE
- ProQuest Dissertations and Theses Global
- Google Scholar (supplementary checks)

Appendix 3

Flow Diagram Showing the Study Screening Process.



Appendix 4

Interrater Reliability Statistics

Variable	Kappa	ICC
Design	0.655	
Frequency	0.638	
Type of intervention	0.688	
Participant industry	0.826	
Type of facilitator	0.732	
Number of sessions		0.995
Duration (in weeks)		1.000
Length of follow up (in months)		0.993
M1 group 1		0.999
SD1 group 1		1.000
N1 group 1		0.984
M2 group 1		0.999
SD2 group 1		0.948
N2 group 1		0.999
M3 group 1		1.000
SD3 group 1		1.000
N3 group 1		0.998
M1 group 2		0.999
SD1 group 2		0.999
N1 group 2		1.000
M2 group 2		0.996
SD2 group 2		0.997
N2 group 2		1.000
M3 group 2		1.000
SD3 group 2		1.000
N3 group 2		1.000

Appendix 5

Effect Size (Cohen's *d*) and Study Details for Psychological Distress Variables for Studies Included in the Meta-Analysis

Study	Intervention	Outcome(s)	N_E	N_C	N_{SS}	Follow-up (months)	Effect size [95% CI]	
							Post	Follow-up
Acari 1996	Mod. MBSR	Anx., Dep., Str.	3	3	--	1	0.55 [-1.11, 2.22]	-0.10 [-0.98, 0.78]
Adams 2011	MBSR	NA, Str.	--	--	25	2	0.59 [0.15, 1.03]	0.51 [0.09, 0.94]
Aikens et al. 2014	Mod. MBSR	Str.	44	45	--	6	0.65 [0.22, 1.07]	0.10 [-0.38, 0.58]
Alexander et al., 1993	TM	NA, Anx.	30	25	--	--	0.29 [0.05, 0.53]	--
Allexandre et al.2016 sample 1	Mod. MBSR	Burn., Str.	--	--	69	12	0.33 [-0.20, 0.87]	0.28 [-0.35, 0.91]
Allexandre et al.2016 sample 2*	Combination	Burn., Str.	--	--	26	12	0.89 [0.42, 1.37]	0.36 [-0.17, 0.88]
Allexandre et al.2016 sample 3*	Combination	Burn., Str.	--	--	21	12	0.79 [0.28, 1.29]	0.40 [-0.12, 0.93]
Ancona & Mendelson 2014	Combination	Burn., Str.	21	22	--	--	0.31 [-0.29, 0.92]	--
Anderson et al. 1999	TM	Anx., Str., Burn.	45	46	--	1	0.42 [0.00, 0.83]	0.61 [0.19, 1.04]
Baccarani et al. 2013	Misc. Meditat.	Anx., Dep.	--	--	10	--	0.34 [-0.30, 0.98]	--
Bartlett et al. 2017	Mod. MBSR	Gen. Dist., Str.	20	66	--	--	0.98 [0.46, 1.50]	--
Bazarko et al. 2013	Combination	Burn., Str.	--	--	40	4	0.79 [0.43, 1.15]	0.90 [0.50, 1.29]
Beshai et al. 2016	Mod. MBSR	Str.	49	40	--	--	1.36 [0.89, 1.82]	--
Bethay et al. 2013	ACT	Gen. Dist., Burn.	14	16	--	3	0.12 [-0.61, 0.85]	0.01 [-0.71, 0.74]
Biglan et al. 2013	ACT	Str.	--	--	42	--	0.32 [0.01, 0.63]	--
Brady et al. 2012	Mod. MBSR	Burn., Str.	--	--	16	--	0.43 [-0.08, 0.95]	--
Brinkborg et al. 2011	ACT	Gen. Dist., Burn., Str.	38	36	--	--	0.52 [0.06, 0.98]	--
Burnett & Pettijohn 2015	Mod. MBSR	Str.	20	18	--	--	-0.30 [-0.94, 0.34]	--
Burton et al. 2010	ACT	Dep.	--	--	16	--	0.42 [-0.10, 0.94]	--
Carlisle 2005	TM	Somat., Str.	--	--	22	3	-0.01 [-0.45, 0.44]	-0.24 [-0.72, 0.24]
Christopher et al. 2016	Mod. MBSR	Burn., NA, Str.	--	--	43	--	0.81 [0.46, 1.15]	--
Clarke et al. 2015a	ACT	Burn., Gen. Dist.	57	49	--	6	-0.08 [-0.46, 0.30]	0.22 [-0.30, 0.74]
Clarke et al. 2015b	ACT	Burn., Gen. Dist.	48	46	--	6	-0.41 [-0.82, 0.00]	-0.19 [-0.73, 0.36]
Cohen-Katz et al. 2005	MBSR	Burn.	12	13	--	3	1.01 [0.17, 1.85]	0.70 [-0.11, 1.52]
Craigie et al. 2016	MBCT	Anx., Burn., Dep., Str.	--	--	21	1	0.42 [-0.04, 0.87]	0.42 [-0.04, 0.87]
Crain et al. 2017	Mod. MBSR	NA, Str.	54	59	--	3	0.42 [0.04, 0.79]	0.41 [0.04, 0.79]
Crowder et al. 2017	Mod. MBSR	Anx., Burn., Str.	6	7	--	--	0.17 [-0.95, 1.29]	--

Study	Intervention	Outcome(s)	N_E	N_C	N_{SS}	Follow-up (months)	Effect size [95% CI]	
							Post	Follow-up
Cutshall et al. 2011	Combination	Anx., Str.	--	--	11	--	1.04 [0.30, 1.77]	--
De Armond 1996	TM	Somat., Str.	38	37	--	--	1.14 [0.64, 1.64]	--
de Bruin et al. 2017	Mod. MBSR	Anx., Burn., Dep., Gen. Dist., NA, Somat., Str.	--	--	25	6	0.73 [0.28, 1.18]	1.10 [0.57, 1.62]
de Carvalho et al. 2017	Combination	Burn.	13	7	--	--	0.51 [-0.43, 1.44]	--
de Zoysa et al. 2014	MBCT	Anx., Gen. Dist., NA	--	--	10	18	0.75 [0.04, 1.47]	0.84 [0.12, 1.56]
Dobie et al. 2015	Mod. MBSR	Gen. Dist.	--	--	9	--	0.97 [0.18, 1.77]	--
Duarte et al. 2016	Mod. MBSR	Anx., Burn., Dep., Str.	29	19	--	--	0.23 [-0.35, 0.81]	--
Duchemin et al. 2015	Combination	Anx., Burn., Dep., Str.	16	16	--	--	0.40 [-0.30, 1.11]	--
Elder et al. 2014	TM	Burn., Dep., Str.	20	20	--	--	0.93 [0.27, 1.58]	--
Fiore 2015	Gen. Mindfulness	Str.	10	4	--	--	-0.13 [-1.29, 1.03]	--
Flaxman & Bond 2010	ACT	Gen. Dist.	177	134	--	6	0.29 [0.07, 0.52]	0.33 [0.10, 0.55]
Flook et al. 2013	Mod. MBSR	Burn., Gen Dist.	10	8	--	--	0.57 [-0.38, 1.53]	--
Fortney et al. 2013	Mod. MBSR	Anx., Burn., Dep., Str.	--	--	23	9	0.65 [0.20, 1.10]	0.70 [0.25, 1.16]
Foureur et al. 2013	Mod. MBSR	Anx., Dep., Gen. Dist., Str.	--	--	27	--	0.54 [0.14, 0.95]	--
Franco et al. 2010	Misc. Meditat.	Gen. Dist.	34	34	--	4	1.56 [1.02, 2.11]	1.73 [1.18, 2.29]
Frank et al. 2015	Mod. MBSR	Anx., Burn., Dep., Gen. Dist., Somat.	18	18	--	--	0.15 [-0.50, 0.81]	--
Galantino et al. 2005	Combination	Burn., NA	--	--	69	--	0.35 [0.10, 0.59]	--
Gauthier et al., 2015	Combination	Str.	--	--	38	1	0.47 [0.14, 0.81]	0.37 [0.06, 0.69]
Geary & Rosenthal, 2011	MBSR	Gen. Dist., Str.	59	49	--	12	1.05 [0.65, 1.46]	0.94 [0.50, 1.38]
Gerhart et al. 2016	Combination	Anx., Burn., Dep.	--	--	21	--	0.56 [0.09, 1.02]	--
Giluk 2010	MBSR	NA	22	53	--	1	0.69 [0.18, 1.20]	0.72 [0.21, 1.23]
Gold et al. 2010	MBSR	Anx., Dep., Str.	--	--	10	--	0.61 [-0.08, 1.30]	--

Study	Intervention	Outcome(s)	N_E	N_C	N_{SS}	Follow-up (months)	Effect size [95% CI]	
							Post	Follow-up
Goodman & Schorling, 2012 sample 1	MBSR	Burn.	--	--	40	--	0.59 [0.25, 0.92]	--
Goodman & Schorling, 2012 sample 2	MBSR	Burn.	--	--	33	--	0.34 [-0.02, 0.69]	--
Gouda et al. 2016	Mod. MBSR	Anx., Dep., Str.	--	--	14	4	0.42 [-0.31, 1.16]	0.31 [-0.24, 0.85]
Grégoire & Lachance, 2015	Gen. Meditat.	Gen. Dist., NA, Str.	18	25	--	--	0.71 [0.09, 1.34]	--
Hallman et al., 2014	Mod. MBSR	Str.	--	--	12	2	0.21 [-0.36, 0.78]	1.00 [0.31, 1.70]
Harris et al. 2016	Combination	Burn., Somat., Str.	34	30	--	--	0.22 [-0.27, 0.72]	--
Harrison 2014	Mod. MBSR	Burn., Str.	50	52	--	4	0.27 [-0.12, 0.66]	0.27 [-0.12, 0.66]
Hayes et al. 2004	ACT	Burn.	30	29	--	3	0.12 [-0.38, 0.61]	-0.06 [-0.58, 0.45]
Heeter et al. 2017	Combination	Burn.			36	--	0.33 [0.00, 0.67]	--
Hoge et al. 2017	Mod. MBSR	Symptoms	27	30	--	--	-0.10 [-0.62, 0.42]	--
Horner et al., 2014	Combination	Burn., Str.			31	--	0.21 [-0.15, 0.56]	--
Huang et al., 2015	Mod. MBSR	Gen. Dist., Str.	72	72	--	2	0.54 [0.21, 0.88]	0.38 [0.01, 0.76]
Hulsheger et al. 2013	Mod. MBSR	Burn.	22	42	--	--	0.47 [-0.06, 0.99]	--
Irving 2011	Mod. MBSR	Burn., Dep., Str.	--	--	51	--	0.36 [0.08, 0.65]	--
Jaltuch 1997	MBSR	Burn., Gen. Dist., Str.	--	--	11	--	0.47 [-0.15, 1.10]	--
Jennings et al. 2011	Combination	Dep., NA	--	--	29	--	0.22 [-0.15, 0.59]	--
Jennings et al. 2013	Combination	Burn., Dep., NA	23	27	--	--	0.19 [-0.37, 0.74]	--
Jennings et al. 2017	Combination	Gen. Dist., Somat., Str.	118	106	--	--	0.11 [-0.15, 0.37]	--
Johnson et al. 2015	Combination	Anx., Dep., Str.	18	19	--	--	1.00 [0.31, 1.68]	--
Josefsson et al., 2014	Mod. MBSR	Anx., Dep.	38	30	--	--	0.00 [-0.48, 0.48]	--
Kemeny et al. 2012	Gen. Meditat.	Anx., Dep., NA	41	41	--	5	0.77 [0.32, 1.22]	0.77 [0.32, 1.22]
Kinser et al. 2016	Mod. MBSR	Anx., Burn., Dep., Str.	--	--	27	--	0.42 [0.03, 0.82]	--
Klatt et al. 2009	Mod. MBSR	Str.	22	20	--	--	1.99 [1.25, 2.74]	--
Klatt et al. 2017	Mod. MBSR	Str.	27	30	--	2	0.77 [0.23, 1.31]	0.95 [0.48, 1.41]
Koncz et al. 2016	Mod. MBSR	Gen. Dist.	20	29	--	--	0.35 [-0.23, 0.92]	--
Krasner et al. 2009	Mod. MBSR	Burn.	--	--	68	15	0.30 [0.05, 0.54]	0.49 [0.20, 0.78]
Kuoppala & Kekoni 2013 sample 1	Combination	Anx., Burn., Dep., Somat.	--	--	25	--	0.47 [0.05, 0.88]	--

Study	Intervention	Outcome(s)	N_E	N_C	N_{SS}	Follow-up (months)	Effect size [95% CI]	
							Post	Follow-up
Kuoppala & Kekoni 2013 sample 2	Combination	Anx., Burn., Dep., Somat.	--	--	27	--	1.06 [0.59, 1.53]	--
Kwok 2010	Combination	Somat., Str.	6	13	--	--	0.02 [-0.95, 0.99]	--
Leary 2013	Gen. Meditat.	Burn., Str.	--	--	38	3	0.12 [-0.20, 0.45]	0.13 [-0.19, 0.45]
Lloyd et al. 2013	ACT	Burn., Gen. Dist.	43	57	--	6	0.30 [-0.10, 0.70]	0.34 [-0.06, 0.74]
Luberto et al. 2017	MBCT	Burn., Str.	--	--	65	--	0.70 [0.43, 0.98]	--
Ly et al. 2014	ACT	Gen. Dist., Str.	33	35	--	--	0.49 [0.01, 0.98]	--
Mackenzie et al. 2006	Mod. MBSR	Burn.	16	14	--	--	0.59 [-0.14, 1.33]	--
Manotas et al. 2014	Mod. MBSR	Anx., Dep., Somat., Str.	36	42	--	--	0.85 [0.38, 1.31]	--
Martín-Asuero et al. 2010	MBSR	Gen. Dist., NA, Rumin., Str.	--	--	29	3	0.36 [-0.02, 0.73]	0.54 [0.14, 0.95]
Martin-Asuero et al. 2014	Mod. MBSR	Burn., NA	43	25	--	--	0.65 [0.14, 1.16]	--
Marx et al. 2014	Combination	Str.	--	--	37	3	--	0.72 [0.36, 1.08]
McConachie et al. 2014	ACT	Gen. Dist., Str.	53	45	--	2	0.16 [-0.24, 0.56]	0.09 [-0.33, 0.51]
McGarrigle & Walsh 2011	Combination	Str.	--	--	12	--	0.85 [0.19, 1.51]	--
Michel et al., 2014	Combination	Str.	--	--	96	1	0.48 [0.27, 0.69]	0.43 [0.13, 0.73]
Moody et al. 2013	Combination	Burn., Dep.	--	--	23	--	0.14 [-0.27, 0.55]	--
Noone & Hastings 2010	ACT	Gen. Dist., Str.	--	--	34	--	0.33 [-0.02, 0.68]	--
Newman et al. 2014	ACT	Burn.	--	--	40	2	--	0.73 [0.38, 1.09]
Oman et al., 2006, Oman et al. 2008 (duplicate)	Combination	Burn., Str.	31	27	--	4	0.31 [-0.21, 0.83]	0.28 [-0.24, 0.80]
Orellana-Rios et al. 2017	Combination	Anx., Burn., Dep., Somat., Str.	--	--	26	--	0.32 [-0.07, 0.72]	--
Pflugeisen et al. 2017	Mod. MBSR	Burn., Str.	--	--	19	2	0.56 [0.07, 1.05]	0.71 [0.20, 1.23]
Pipe et al. 2009	Mod. MBSR	Anx., Dep., Gen. Dist., Somat., Hostility	15	17	--	--	0.74 [0.02, 1.46]	--
Poulin et al. 2008 Study 1	Mod. MBSR	Burn.	16	14	--	--	0.54 [-0.19, 1.27]	--
Prasad et al., 2011	Combination	Anx., Str.	--	--	17	--	1.11 [0.51, 1.72]	--
Punyaniyama 1996	In. Meditat.	Str.	38	36	--	--	0.24 [-0.22, 0.69]	--
Raab et al., 2015	MBSR	Burn.	--	--	22	--	0.17 [-0.25, 0.59]	--
Ramsey & Jones, 2015 Study 1	Combination	Str.	--	--	51	--	0.47 [0.18, 0.76]	--
Razzaque & Wood 2016	Mod. MBSR	Burn.	--	--	22	--	0.67 [0.21, 1.13]	--

Study	Intervention	Outcome(s)	N_E	N_C	N_{SS}	Follow-up (months)	Effect size [95% CI]	
							Post	Follow-up
Roeser et al., 2013	Combination	Anx., Burn., Dep., Str.	54	59	--	3	0.36 [-0.01, 0.74]	0.56 [0.18, 0.93]
Ruths et al., 2012	MBCT	Anx., Gen. Dist., NA	--	--	24	3	0.29 [-0.13, 0.70]	0.37 [-0.11, 0.86]
Shapiro et al., 2005	Combination	Burn., Gen. Dist.	--	--	18	--	0.37 [-0.11, 0.85]	--
Sheppard et al. 1997	TM	Anx., Dep.	22	22	--	36	0.52 [-0.08, 1.12]	0.91 [0.18, 1.65]
Suyi et al., 2017	Mod. MBSR	Str.	--	--	37	3	0.54 [0.19, 0.88]	0.30 [-0.03, 0.63]
Tagg 2015	Combination	Burn., Str.	33	33	--	--	0.00 [-0.48, 0.49]	--
Tarantino et al. 2013	Combination	Str.	--	--	82	12	0.38 [0.15, 0.60]	0.38 [0.15, 0.60]
Taylor et al., 2016	Combination	Str.	26	30	--	4	0.35 [-0.18, 0.88]	0.08 [-0.44, 0.61]
Tsai & Crockett 1993	Combination	Gen. Dist., Str.	--	--	132	--	0.29 [-0.13, 0.71]	--
Tsang et al. 2017	Combination	Anx., Dep., Str.	47	46	--	1	0.38 [-0.03, 0.79]	0.10 [-0.30, 0.51]
Walach et al., 2007	MBSR	Complaints	11	16	--	2	0.27 [-0.50, 1.04]	0.27 [-0.50, 1.04]
Wasylkiw et al. 2015	Mod. MBSR	Str.	11	10	--	--	1.15 [0.23, 2.08]	--
Wen et al. 2017	Gen. Meditat.	NA	--	--	30	--	0.15 [-0.21, 0.51]	--
Wolever et al., 2012	Combination	Dep., Str.	96	53	--	--	0.48 [0.14, 0.82]	--
Yang 2015	Mod. MBSR	Burn., Str.	35	32	--	--	0.06 [-0.42, 0.54]	--
Yong et al. 2011	Gen. Meditat.	Burn.	24	27	--	--	1.27 [0.67, 1.88]	--

Note: **Intervention:** ACT = acceptance and commitment therapy; MBSR = mindfulness-based stress reduction; Mod. MBSR = modified mindfulness-based stress reduction; MBCT = mindfulness-based cognitive therapy; combination = combination of activities (e.g., MBSR + MBCT); In. meditat. = insight meditation; TM = transcendental meditation; Gen. meditat. = general meditation exercise; Gen. mindfulness = General mindfulness exercise; **Outcome(s):** Anx. = anxiety; Burn. = burnout; Dep. = depression; Gen. Dist. = general distress; NA = negative affect; Somat. = somatic complaints; $\boldsymbol{N_E}$ = sample size for experimental/treatment group; $\boldsymbol{N_C}$ = sample size for control group; $\boldsymbol{N_{SS}}$ = sample size for single sample studies; CI = confidence interval. * = study coded as single sample to avoid duplication of control group data.

Appendix 6

Quality Ratings Checklists

<u>Original</u> Downs and Black (1998) Checklist

Reporting	
1 = yes, 0 = no	Is the hypothesis/aim/objective of the study clearly described?
1 = yes, 0 = no	Are the main outcomes to be measured clearly described in the Introduction or Methods section?
1 = yes, 0 = no	Are the characteristics of the patients included in the study clearly described?
1 = yes, 0 = no	Are the interventions of interest clearly described?
2 = yes, 1 = partially, 0 = no	Are the distributions of principal confounders in each group of subjects to be compared clearly described?
1 = yes, 0 = no	Are the main findings of the study clearly described?
1 = yes, 0 = no	Does the study provide estimates of the random variability in the data for the main outcomes?
1 = yes, 0 = no	Have all important adverse events that may be a consequence of the intervention been reported?
1 = yes, 0 = no	Have the characteristics of patients lost to follow-up been described?
1 = yes, 0 = no	Have actual p-values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?
External Validity	All the following criteria attempt to address the representativeness of the findings of the study and whether they may be generalised to the population from which the study subjects were derived.

1 = yes, 0 = no, 0 = unable to determine	Were the subjects asked to participate in the study representative of the entire (working?) population from which they were recruited?
1 = yes, 0 = no, 0 = unable to determine	Were those subjects who were prepared to participate representative of the entire population from which they were recruited?
1 = yes, 0 = no, 0 = unable to determine	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive?
Internal Validity - Bias	
1 = yes, 0 = no, 0 = unable to determine	Was an attempt made to blind study subjects to the intervention they have received?
1 = yes, 0 = no, 0 = unable to determine	Was an attempt made to blind those measuring the main outcomes of the intervention?
1 = yes, 0 = no, 0 = unable to determine	If any of the results of the study were based on “data dredging”, was this made clear?
1 = yes, 0 = no, 0 = unable to determine	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls ?
1 = yes, 0 = no, 0 =	Were the statistical tests used to assess the main outcomes appropriate?

unable to determine	
1 = yes, 0 = no, 0 = unable to determine	Was compliance with the intervention/s reliable?
1 = yes, 0 = no, 0 = unable to determine	Were the main outcome measures used accurate (valid and reliable)?
Internal Validity - Confounding (selection bias)	
1 = yes, 0 = no, 0 = unable to determine	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population?
1 = yes, 0 = no, 0 = unable to determine	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time?
1 = yes, 0 = no, 0 = unable to determine	Were study subjects randomised to intervention groups?
1 = yes, 0 = no, 0 = unable to determine	Was the randomised intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?
1 = yes, 0 = no, 0 =	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn?

unable to determine	
1 = yes, 0 = no, 0 = unable to determine	Were losses of patients to follow-up taken into account?
Power	
Scored 5 to 0	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%?

<u>Modified</u> Downs and Black (1998) Checklist

Reporting	
1 = yes, 0 = no	Is the hypothesis/aim/objective of the study clearly described?
1 = yes, 0 = no	Are the main outcomes to be measured clearly described in the Introduction or Methods section?
1 = yes, 0 = no	Has the study specified participant inclusion/exclusion criteria?
1 = yes, 0 = no	Are the interventions of interest clearly described?
2 = yes, 1 = partially, 0 = no	Are the distributions of principal confounders in each group of subjects to be compared clearly described?
1 = yes, 0 = no	Are the main findings of the study clearly described?
1 = yes, 0 = no	Does the study provide estimates of the random variability in the data for the main outcomes?
1 = yes, 0 = no	Has the study discussed potential adverse effects as a result of the intervention?
1 = yes, 0 = no	Have the characteristics of patients lost to follow-up been described?

1 = yes, 0 = no	Have actual p-values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?
External Validity	All the following criteria attempt to address the representativeness of the findings of the study and whether they may be generalised to the population from which the study subjects were derived.
1 = yes, 0 = no, 0 = unable to determine	Were the subjects asked to participate in the study representative of the entire (working?) population from which they were recruited?
1 = yes, 0 = no, 0 = unable to determine	Were those subjects who were prepared to participate representative of the entire population from which they were recruited?
1 = yes, 0 = no, 0 = unable to determine	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive?
Self-report only = 0, mixed measurement = 1	The study utilized mixed measures (Mixed measurement means self-report + one or more of biological data (e.g., cortisol), brain activity, behavioural or cognitive tasks, etc.)
Internal Validity - Bias	
1 = yes, 0 = no, 0 = unable to determine	Was the study pre-registered?
1 = yes, 0 = no, 0 = unable to determine	Was an attempt made to report the dose/practice, and did it approximate at least 50% of maximum?

1 = yes, 0 = no, 0 = unable to determine	Were the main outcome measures used accurate (valid and reliable)?
Internal Validity - Confounding (selection bias)	
1 = yes, 0 = no, 0 = unable to determine	Were study subjects randomised to intervention groups?
1 = yes, 0 = no, 0 = unable to determine	Was there an intention to treat design?
Power	
0.5 points = 51 people or more; 1 points; 64 people or more	Does the study have 80% power to detect a cohen's d of 0.5 (TWO GROUPS: at least 64 people per group. ONE GROUP: 34 participants)? OR does the study have a 70% power to detect a d of 0.5 (TWO GROUPS: at least 51 people per group. ONE GROUP: 27 per group)

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