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Mindfulness-based therapy: A comprehensive meta-analysis

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HIGHLIGHTS

- We conducted a meta-analysis to provide a review of mindfulness-based therapy.
- It includes 209 studies enrolling 12,145 participants with a variety of disorders.
- We obtained Hedge's g=.53 in waitlist controlled studies, .55 in pre-post studies.
- When compared with other treatments, we obtained a smaller effect (Hedge's g=.33).
- The results obtained are robust and are maintained at follow-up.

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ABSTRACT

Background: Mindfulness-based therapy (MBT) has become a popular form of intervention. However, the existing reviews report inconsistent findings.
Objective: To clarify these inconsistencies in the literature, we conducted a comprehensive effect-size analysis to evaluate the efficacy of MBT.
Data sources: A systematic review of studies published in journals or in dissertations in PubMED or PsycINFO from the first available date until May 10, 2013.
Review methods: A total of 209 studies (n = 12,145) were included.
Results: Effect-size estimates suggested that MBT is moderately effective in pre-post comparisons (n = 72; Hedge's g = .55), in comparisons with waitlist controls (n = 67; Hedge's g = .53), and when compared with other active treatments (n = 68; Hedge's g = .33), including other psychological treatments (n = 35; Hedge's g = .22). MBT did not differ from traditional CBT or behavioral therapies (n = 9; Hedge's g = −.07) or pharmacological treatments (n = 3; Hedge's g = .13).
Conclusion: MBT is an effective treatment for a variety of psychological problems, and is especially effective for reducing anxiety, depression, and stress.

Contents

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1. Introduction

An increasing number of meta-analyses and systematic reviews have investigated the effectiveness of mindfulness-based therapy (MBT). These reviews reported inconsistent findings about the size of the treatment effect of MBT for reducing stress, anxiety, and depression associated with physical illness or psychological disorders (Baer, 2003; Bohlmeijer, Prenger, Taal, & Cuijpers, 2010; Chiesa & Serretti, 2010, 2011; Cramer, Lauche, Paul, & Dobos, 2012; de Vibe, Bjørndal, Tipton, Hammerstrøm, & Kowalski, 2012; Eberth & Sedlmeier, 2012; Fjorback, Arendt, Ørnbøl, Fink, & Walach, 2011; Grossman, Niemann, Schmidt, & Walach, 2004; Hofmann, Sawyer, Witt, & Oh, 2010; Klainin-Yobas, Cho, & Creedy, 2012; Ledesma & Kumano, 2009; Musial, Büssing, Heusser, Choi, & Ostermann, 2011; Piet & Hougaard, 2011; Sedlmeier et al., 2012; Zainal, Booth, & Huppert, 2012).

These inconsistencies may be due to a number of factors, including the choice of the MBT protocols, the restriction to specific research designs, and the inclusion of a particular group of patients. Moreover, little is known about the stability of treatment gains (Baer, 2003; Hofmann et al., 2010), about the active ingredients that may account for the efficacy of MBT (Chiesa & Serretti, 2011; Fjorback et al., 2011), and about the relevant moderator variables. It is assumed that mindfulness is a central mechanism of MBT (e.g., Bränström, Kvillemo, Brandberg, & Moskowitz, 2010; Greeson et al., 2011; Kuyken et al., 2010; Shahar, Britton, Sbarra, Figueredo, & Bootzin, 2010) that might enhance positive affect, decrease negative affect, and reduce maladaptive automatic emotional responses (Gross, 2007; Hofmann, Sawyer, Fang, & Asnaani, 2012; Koole, 2009; Thompson, 1991, 1994). Although this is consistent with the notion that mindfulness training is associated with changes in areas of the brain responsible for affect regulation, and stress impulse reaction (Davidson et al., 2003; Hölzel et al., 2011; Lazar et al., 2005) the empirical evidence for explaining the mechanisms of MBT remains sparse. Similarly, little is known about the potential moderators, including treatment duration (de Vibe et al., 2012; Hofmann et al., 2010; Klainin-Yobas et al., 2012; Sedlmeier et al., 2012), homework practice (e.g., Carmody & Baer, 2009; Fjorback et al., 2011; Toneatto & Nguyen, 2007), course attendance (de Vibe et al., 2012), and the clinical and mindfulness training and practical experience of the therapists delivering MBT (Carmody & Baer, 2009; Crane, Barnhofer, Hargus, Amarasinghe, & Winder, 2010; Davidson, 2010; Fjorback et al., 2011; Piron, 2001; Pradhan et al., 2007; Segal, Teasdale, Williams, & Gemar, 2002).

In order to address the weaknesses of the current literature, we conducted a comprehensive effect-size analysis with the following objectives: (1) to quantify the size of the treatment effect with the maximum available data; (2) to investigate and quantify the role of mindfulness in MBT; and (3) to explore moderator variables.

2. Methods

2.1. Eligibility criteria

Any study examining the pre-post or controlled effects of MBT for a wide range of physical and medical conditions, psychological disorders, and in non-clinical populations was considered in our analysis. Studies were excluded if they (1) did not include a mindfulness meditation-based intervention; (2) did not aim to examine treatment effects; (3) consisted of comparisons among meditators or among meditation styles; (4) examined the non-direct effects of mindfulness (i.e., mindfulness treatment administered to therapists and not directly to their clients); (5) examined mindfulness as a component of another treatment; (6) reported no clinical outcomes; (7) reported insufficient information to compute an effect size (e.g., only correlational data); or (8) reported data that overlapped with the data from other included studies.

The meta-analysis excluded studies that examined mindfulness as part of another treatment, such as cognitive behavior protocol, because it was difficult to dissociate the effect of mindfulness from other components. This led to the exclusion of Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 1999) and Dialectical Behavior Therapy (DBT; Linehan, 1993a, 1993b). Also, the meta-analysis excluded protocols using other forms of meditation (e.g., guided or concentration, or a combination of many meditation styles), excluding as result Loving-Kindness Meditation (LKM; Salzberg, 1995). A review of this specific meditation strategy can be found elsewhere (Hofmann, Grossman, & Hinton, 2011). Finally, studies based on meditation instruction, induction, or retreats were also excluded from this meta-analysis.

2.2. Information sources

Studies were identified by searching PubMed and PsycINFO from the first available date until May 10, 2013. No limits were applied for language and foreign papers were translated into English.

2.3. Search

We used the search term *mindfulness* alone or combined with the terms *MBSR* or *MBCT*.

2.4. Study selection

Eligibility assessment was performed in a non-blinded standardized manner by the first author and was revised by the second author. Disagreements between reviewers were resolved through discussions, and in a few instances the authors of the original studies were contacted for clarifications.

2.5. Data collection process

We developed an electronic data extraction sheet, pilot-tested it on five randomly-selected studies, and refined it accordingly. Data collection was conducted for the first time in April of 2010, was re-conducted and refined in April of 2011, and updated in May of 2013. When duplicate reports were identified for the same data, only the latest ones were included.

2.6. Data items

Information was extracted from each included trial based on (1) the characteristics of the trial (including the year of publication, design, randomization, blinding, therapist qualifications, number of participants, type of outcome measures, and follow-up time in weeks); (2) the characteristics of the intervention (including treatment protocol, target population, length of treatment in hours, attendance in number of sessions, length of assigned home practice in hours, quality of home practice as reported by participants, and treatment setting); (3) the characteristics of the control group, in controlled studies (including the number of participants, type of control, type of treatment, and length of treatment); and (4) the characteristics of participants (including mean age, percentage of males, attrition rate, and diagnosis). We made conservative assumptions for missing or unclear data. For example, if the report neglected to describe the qualifications of the therapists, we assumed that the therapists did not have appropriate clinical/mindfulness training.

2.7. Risk of bias in individual studies

To minimize the influence of data selection, we included data pertaining to all available outcomes, but we divided them into clinical and mindfulness, because effect sizes might vary between these two groups. The clinical outcomes included both physical measures (e.g., pain and blood pressure) and psychological measures (e.g., anxiety and depression). Mindfulness outcomes consisted only of measures of mindfulness. We included data from follow-ups, when such data were available.

We also included a study quality score, which was comprised of items based on Jadad's criteria (Jadad et al., 1996) and others pertaining to mindfulness. The included items are adherence of the treatment to an established protocol (MBSR, MBCT, MBRP, or MMRP); administration of measures at follow-up; use of validated mindfulness measures (i.e., MAAS, KIMS, FMI, FFMQ, SMQ, TMS, or CAMS-R); clinical training of therapists (i.e., clinical psychologists, trainees in clinical psychology, or social workers); and the mindfulness training of therapists (i.e., formal training in validated protocols, or mindfulness meditation training/experience). For controlled studies, the items included whether participants were randomized between MBT and control groups, whether participants in both groups spent an equal amount of time in treatment, and whether evaluators or experimenters were blind regarding the MBT/control conditions and/or participants were blind regarding the study's hypotheses. For all binary items (i.e., true or false), a value of 1 was assigned if the item was true and a value of 0 if it was false. For the study design, pre-post studies were assigned a value of 0; studies with a waitlist, no-treatment, or drop-outs control group were assigned a value of 1; studies with a TAU control group were assigned a value of 2; studies with a treatment control group (other than TAU) were assigned a value of 3. For blinding, non-blinded studies were assigned a value of 0; single-blind studies were assigned a value of 1; and double-blind studies were assigned a value of 2.

The inter-rater agreement was assessed by comparing the ratings of the first author (B.K.) to the ratings of each of the four co-authors (G.F., M.M., P.T. and V.B.). Each co-author received a set of articles to review, along with a written document including specific instructions on rating the studies. A one-hour training and discussion about the rating procedure was also provided.

2.8. Summary measures

The meta-analyses were performed by computing standardized differences in means. We completed all analyses using Microsoft Excel or Comprehensive Meta-Analysis, Version 2.2.057 (CMA; Borenstein, Hedges, Higgins, & Rothstein, 2005).

2.9. Synthesis of results

Effect sizes were computed using means and standard deviations (*SD*) when available. In the remaining studies, the effect sizes were computed using other statistics such as *F*, *p*, *t*, and χ^2. In within-group designs, when the correlations between the pre- and post-treatment measures were not available, we used a conservative estimate ($r = .7$) according to the recommendation by Rosenthal (1993). For all studies, Hedge's *g*, its 95% confidence interval (95% CI), and the associated *z* and *p* values were computed. To calculate the mean effect size for a group of studies, individual effect sizes were pooled using a random effect model rather than a fixed effect model, given that the selected studies were not identical (i.e., did not have either an identical design or target the same population).

For all study groups, the mean Hedge's *g*, the 95% confidence interval (95% CI), and the 95% prediction interval (95% PI) were computed. The prediction interval describes the distribution of true effects around the mean, whereas the confidence interval reflects the precision of the mean effect size. We systematically assessed the heterogeneity among studies in each group using I^2 and the chi-squared statistic (*Q*). I^2 measures the proportion of heterogeneity to the total observed dispersion, and is not affected by low statistical power. Higgins, Thompson, Deeks, and Altman (2003) suggested that an I^2 of 25% might be considered low, 50% considered moderate, and 75% considered high.

2.10. Risk of bias across studies

To assess publication bias, we computed the fail-safe *N* (Rosenthal, 1993) and constructed a funnel plot.

2.11. Additional analyses

According to the objectives of this meta-analysis, we conducted meta-regression and clinical significance analyses. The aim of meta-regression analysis is to assess the relationship between one or more variables (moderators) and the pooled effect size. Borenstein, Hedges, Higgins, and Rothstein (2009) suggested a ratio involving at least ten studies for each moderator. In this meta-analysis, we investigated eight moderators: the mean effect size of mindfulness outcomes (measuring the improvement in mindfulness among participants), treatment length, duration of home practice (as indicated in the mindfulness protocol), therapist clinical training, therapist mindfulness training, study quality score, the mean age of participants, and the year of publication. The study-to-moderator ratio was very high (26).

The aim of the clinical significance analysis is to assess the clinical implications of our findings. As physical symptoms were rarely assessed using similar measures, we only assessed the clinical significance of MBT for psychological measures, specifically measures regarding anxiety and depression. Therefore, we selected the Beck Depression Inventory

(BDI-I; Beck & Streer, 1987) (BDI-II; Beck, Steer, & Brown, 1996), the Beck Anxiety Inventory (BAI; Beck & Streer, 1993), the 20-items Center for Epidemiological Studies Depression Scale (CES-D), and the State-Trait Anxiety Inventory (STAI; Spielberger, 1983), because these were the most commonly used measures. Weighted average means were calculated at pre-treatment, post-treatment, and follow-up. The results were interpreted according to the corresponding instrument's manual. For BDI-I, raw scores of 0–9 were considered asymptomatic (or "minimal level of depression"), whereas scores of 10–16 indicated mild depression, 17–29 indicated moderate depression, and scores above 30 indicated severe depression. For BDI-II, raw scores of 0–13 indicated minimal depression, 14–19 indicated mild depression, 20–28 indicated moderate depression, and 29–63 indicated severe depression. For BAI, raw scores of 0–7 were considered asymptomatic ("minimal level of anxiety"), whereas scores of 8–15 indicated a mild level of anxiety, scores 16–25 indicated moderate anxiety, and scores above 26 indicated severe anxiety. For the CES-D, scores ranged from 0 to 60, with higher scores indicating increasing severity of depression. Scores of 16 or higher are considered indicative of depression. Finally, for the STAI, raw scores of 0–39 were considered non-clinically anxious, scores of 40–51 were considered moderately clinically anxious, and scores above 51 were considered highly clinically anxious.

3. Results

3.1. Study selection

PubMed searches produced 902 publications and PsycINFO searches yielded 1974 publications (including 278 dissertations). We carefully assessed the identified publications and applied the exclusion criteria, resulting in 209 studies (177 from journal articles and 32 from dissertations). Of the 209 studies, 207 reported post-treatment assessments, and two of them reported only follow-up data. The study selection process is illustrated in detail in Fig. 1.

3.2. Study characteristics

The effect size (Hedge's g) and other characteristics for each study are shown in Table A1. Studies were divided according to the methodological design. Then, within each of these groups, studies were sorted in an ascending manner: first, according to the target population (i.e., type of participants); second, according to the implemented intervention; third, according to the comparison group; fourth, according to the study's first author name; and finally, according to the year of publication. Seventy one studies were included in the 16 previously published meta-analyses, while 138 studies were not included in any of the previous meta-analyses. The total number of participants included in our meta-analysis was 12,145.

Pre-post design studies accounted for 72 studies, whereas the number of waitlist-controlled studies was 67. Treatment controlled studies accounted for 68 studies. The most common disorders were mood and cancer ($n = 25$), followed by anxiety ($n = 23$), pain ($n = 17$), alcohol/substance use ($n = 8$), and fibromyalgia ($n = 6$). Overweight/obesity and social anxiety/social phobia had a similar frequency ($n = 5$), followed by HIV and post-traumatic stress disorder ($n = 4$), and headache ($n = 3$). Attention deficit hyperactivity disorder, psychosis/schizophrenia, personality disorders, child sexual abuse, irritable bowel syndrome, brain injury, heart disease, tinnitus, multiple sclerosis, and rheumatoid arthritis were all with a similar frequency ($n = 2$). The rest

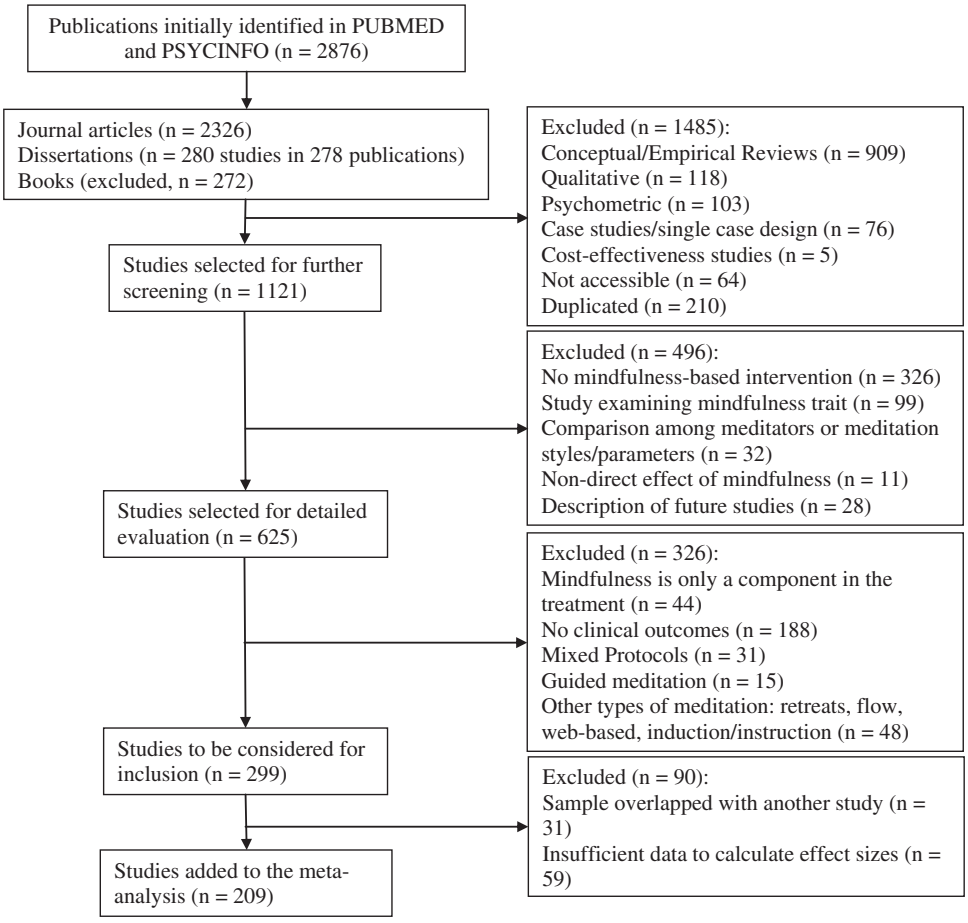

Fig. 1. Flow diagram of the study selection process.

of the disorders or conditions accounted for a single study each. Many studies targeted more than one disorder.

3.3. Risk of bias within studies

Table A1 presents the included studies and their quality scores. One hundred and nine studies were randomized, 93 used at least one validated mindfulness measure, 35 assured an equal time between treatment and control groups, and 28 used blind evaluators, including four that were double-blinded. For controlled studies, the total score varied from a minimum of 1 (lowest quality) to a maximum of 11 (highest quality) with a mean of 4.84 ($SD = 2.19$) and a median of 5. For pre-post studies, the total score varied from a minimum of 0 to a maximum of 5, with a mean of 2.93 ($SD = 1.19$) and a median of 3. Inter-rater agreement was high (kappa = .94).

3.4. Results of individual studies

Hedge's g values for both clinical and mindfulness outcome measures, and at both post treatment and last follow-up, are presented in Table A1.

3.5. Synthesis of results

3.5.1. Effect on clinical outcomes at the end of the treatment

The results of the main groups are represented in Fig. 2. Thirty-five studies compared MBT with other psychological treatments. MBT was more effective than psychoeducational interventions ($n = 9$; Hedge's $g = .61$; 95% CI [.27, .96], $p < .001$), supportive therapies ($n = 7$; Hedge's $g = .37$; 95% CI [.17, .57], $p < .001$), relaxation procedures ($n = 8$; Hedge's $g = .19$; 95% CI [.03, .35], $p < .05$), and imagery/suppression techniques ($n = 2$; Hedge's $g = .26$; 95% CI [.10, .53], $p < .005$). However, the heterogeneity of effect sizes was high among studies comparing MBT with psychoeducation ($I^2 = 82.72\%$, $Q = 46.29$), moderate to high among studies comparing MBT to supportive therapies ($I^2 = 64.30\%$, $Q = 16.81$), moderate among those comparing MBT to relaxation procedures ($I^2 = 59.11\%$, $Q = 17.12$), but low among those comparing MBT to imagery/suppression techniques ($I^2 = 0.00\%$, $Q = 0.12$). MBT did not differ from traditional CBT or behavioral therapies ($n = 9$; Hedge's $g = -.07$; 95% CI [−.26, .16], $p = .60$, ns) or pharmacological treatments ($n = 3$; Hedge's $g = .13$; 95% CI [−.11, .37], $p = .27$, ns).

As Fig. 2 shows, when investigating pre-post and waitlist controlled studies separately, effect sizes associated with MBT were larger when treating psychological disorders, and smaller when treating physical or medical conditions. Among psychological disorders, anxiety disorders showed the largest effect sizes, followed by depression. These effects were even larger when only measures corresponding to the target disorder were included (e.g., only anxiety measures when the treatment targeted an anxiety disorder). The mean effect size on anxiety was large for ten pre-post studies, Hedge's $g = .89$ (95% CI [.71, 1.08], $p < .001$) with low heterogeneity ($I^2 = 13.90\%$, $Q = 10.45$), and in four waitlist controlled studies, Hedge's $g = .96$ (95% CI [.67, 1.24], $p < .001$). The mean effect size on depression was moderately strong for five pre-post studies, Hedge's $g = .69$ (95% CI [.52, .86], $p < .001$) and moderate for eight waitlist controlled studies, Hedge's $g = .53$ (95% CI [.32, .73], $p < .001$). Studies targeting non-clinical populations showed a moderate to high mean effect size in 18 pre-post studies, Hedge's $g = .65$ (95% CI [.51, .80], $p < .001$) and in 16 waitlist controlled studies, Hedge's $g = .62$ (95% CI [.42, .82], $p < .001$). However, heterogeneity was high in both groups. No differences in the mean of clinical outcomes were found among groups based upon gender.

3.5.2. Effect on clinical outcomes at the last follow-up

Follow-up periods varied across studies from three weeks to three years with a weighted mean of 28.92 weeks. Results at follow-up were largely similar to those at the end of treatment. The follow-up effect sizes of pre-post studies ($n = 24$) showed an average effect size (Hedge's g) of .57 (95% CI [.44, .69], <.001), waitlist controlled studies ($n = 17$) showed a Hedge's $g = .43$ (95% CI [.31, .55], $p < .001$), and treatment controlled studies ($n = 30$) showed a Hedge's $g = .24$

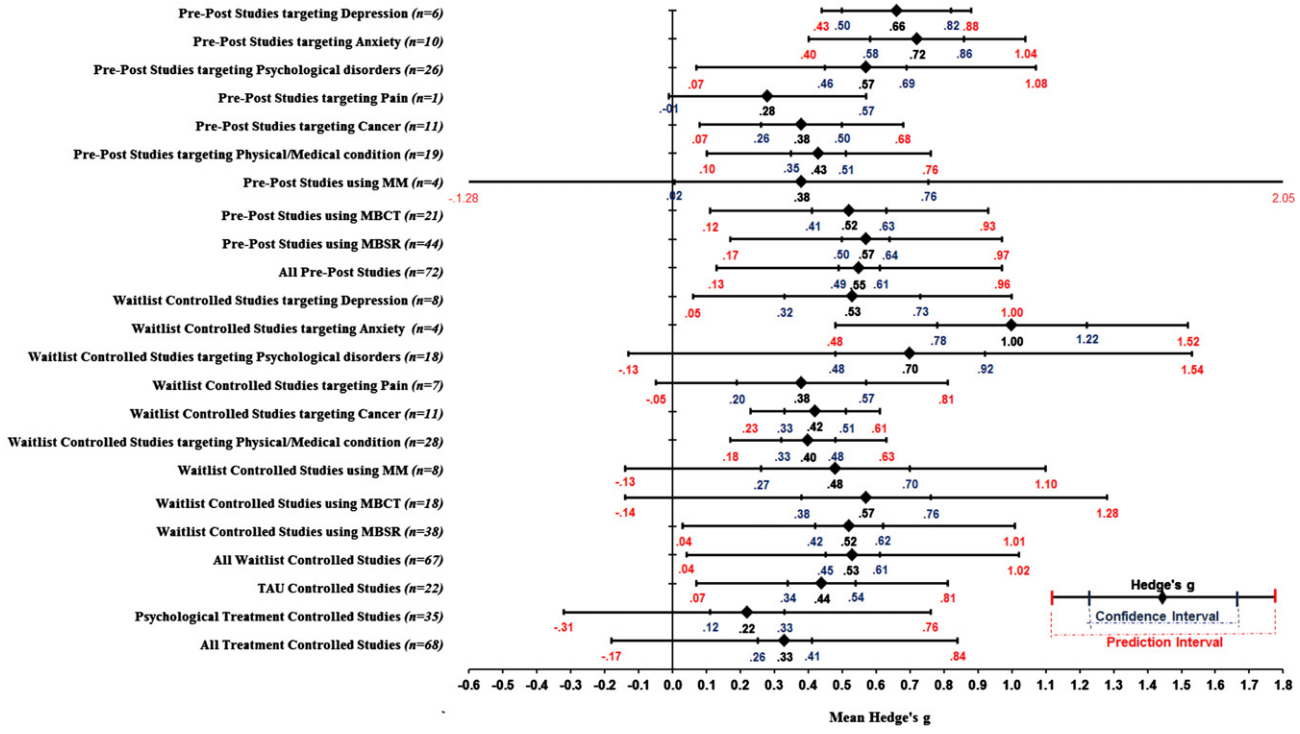

Fig. 2. Mean Hedge's g, 95% confidence interval, and 95% prediction interval of main study groups. Note that the effect sizes were calculated at the end of the treatment and solely based on the clinical outcomes. Note also that MM = mindfulness meditation (meaning mindfulness protocols other than MBSR or MBCT).

(95% CI [.12, .35], $p < .001$), heterogeneity was high among the three groups.

Seventeen studies compared MBT with other psychological treatments at follow-up. MBT was more effective than supportive therapies ($n = 3$; Hedge's $g = .34$; 95% CI [.11, .56], $p < .005$). The heterogeneity of effect sizes was moderate ($I^2 = 48.78\%$, $Q = 3.90$). The effect size was small and not significant of studies comparing MBT with relaxation ($n = 5$), psychoeducation ($n = 3$), and traditional CBT or behavioral therapy ($n = 6$; Hedge's $g = .04$; 95% CI [−.22, .29], $p = .78$, *ns*).

Treatments targeting psychological disorders showed larger effect sizes compared with physical/medical conditions in both pre-post and waitlist controlled studies. In addition, MBT was associated with the largest mean effect sizes for anxiety and depression and the smallest effect sizes for cancer and pain. Six pre-post studies targeting anxiety showed a mean effect size of Hedge's $g = .91$ (95% CI [.69, 1.14], $p < .001$) at follow-up when only including anxiety measures; two pre-post studies targeting depression showed a mean effect size of Hedge's $g = .75$ (95% CI [.38, 1.12], $p < .001$) when only including mood measures.

3.5.3. Effect on mindfulness at the end of the treatment

A total of 93 studies included measures of mindfulness. Mean effect sizes of MBT on mindfulness at the end of the treatment were lower for treatment controlled-studies ($n = 23$; Hedge's $g = .42$; 95% CI [.27, .57], $p < .001$) than for waitlist controlled-studies ($n = 28$; Hedge's $g = .53$; 95% CI [.42, .65], $p < .001$), and pre-post studies ($n = 42$; Hedge's $g = .69$; 95% CI [.59, .80], $p < .001$), heterogeneity was moderate in the three groups. Mean effect size of mindfulness outcomes was also higher in studies targeting psychological disorders compared to studies targeting physical or medical conditions. Five studies comparing MBT with relaxation showed the superiority of MBT on mindfulness ($n = 5$; Hedge's $g = .37$; 95% CI [.04, .69], $p < .05$), heterogeneity was moderate ($I^2 = 49.35\%$, $Q = 7.90$). Studies comparing MBT with other treatments (e.g., support, CBT, and imagery) did not reach statistical significance.

3.5.4. Effect of mindfulness at the last follow-up

Only 31 studies reported measures of mindfulness at follow-up. Results indicated that mindfulness was maintained with similar effect sizes. Treatment-controlled studies showed the smallest effect size ($n = 9$), Hedge's $g = .30$ (95% CI [.13, .47], $p < .005$), heterogeneity was low ($I^2 = 22.71\%$, $Q = 10.35$), followed by waitlist-controlled studies ($n = 8$), Hedge's $g = .56$ (95% CI [.34, .78], $p < .001$), heterogeneity was moderate ($I^2 = 47.71\%$, $Q = 13.39$), and pre-post studies ($n = 14$), Hedge's $g = .66$ (95% CI [.41, .92], $p < .001$), however, heterogeneity was high ($I^2 = 79.58\%$, $Q = 63.67$).

3.5.5. Prediction intervals

We computed the prediction intervals (95% PI) for different groups of studies; results are presented in Fig. 2 along with the 95% CI. In all groups, the prediction interval was wider than the confidence interval, a predictable result.

3.6. Risk of bias across studies

The effect size of all pre-post studies corresponded to a z value of 37.35 ($p < .00001$) indicating that 26,078 studies with a null effect size would be needed to nullify our results (i.e., for the two-tailed p value to exceed .05). Using the Trim and Fill method, 19 studies would need to fall on the left of the mean effect size to make the plot symmetric (Fig. 3). Assuming a random effects model, the new imputed mean effect size was Hedge's $g = .44$ (95% CI [.42, .46]). Similar results were obtained for waitlist controlled studies, with a z value of 21.06 ($p < .00001$) and a corresponding fail-safe N of 7675. No studies were trimmed. For treatment-controlled studies, z value was 15.95 ($p < .00001$) and fail-safe $N = 4434$. When 12 studies were trimmed, the new imputed mean effect size was Hedge's $g = .26$ (95% CI [.23, .30]). These analyses suggest that the effect-size estimates were unbiased and robust.

3.7. Additional analyses

3.7.1. Meta-regression results

The effect size of MBT on clinical outcomes was positively moderated by the effect size on mindfulness outcomes ($n = 91$; $\beta = .41$, $SE = .04$, $p < .00001$) (Fig. 4), the duration of treatment ($n = 182$; $\beta = .01$, $SE = .0015$, $p < .00001$), the mindfulness training of the therapist(s) ($n = 154$; $\beta = .13$, $SE = .04$, $p < .0005$), negatively moderated by the study quality score ($n = 207$; $\beta = -.05$, $SE = .004$, $p < .00001$), and the year of publication ($n = 207$; $\beta = -.01$, $SE = .003$, $p < .0005$). The effect of MBT on clinical outcomes was not moderated by the duration of home practice ($p = .09$, ns), the clinical training of therapists ($p = .07$, ns), or by the age of participants ($p = .78$, ns).

At follow-up, the effect size of MBT on clinical outcomes was positively moderated by the effect size on mindfulness outcomes ($n = 28$; $\beta = .58$, $SE = .08$, $p < .00001$), and negatively moderated by the study quality score ($n = 65$; $\beta = -.029$, $SE = .006$, $p < .00005$). The remaining moderators did not reach statistical significance level.

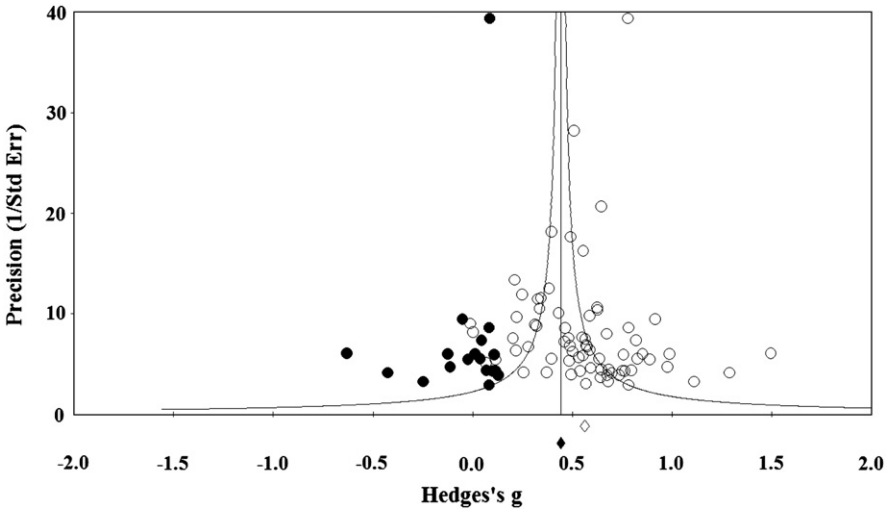

Fig. 3. Funnel plot of precision by Hedge's g of pre-post studies including only clinical outcomes.

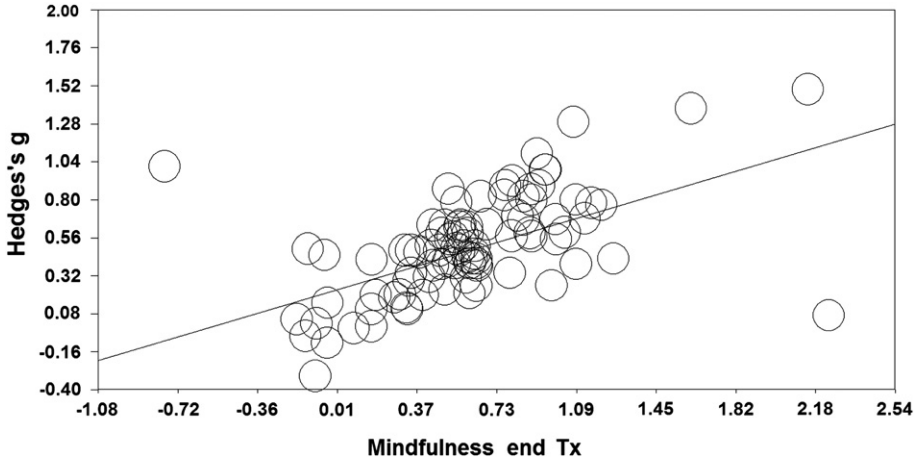

Fig. 4. Relationship between mindfulness effect size and clinical effect size at the end of treatment for all studies. Each circle represents a specific study; its diameter is proportional to the study weight (i.e. to the ratio of the number of participants of the study to the total number of participants for the present meta-analysis). Note that Tx = treatment.

3.7.2. Clinical significance

Pre-treatment, post-treatment, and follow-up outcomes using BAI showed that a mild level of anxiety ($n = 9$) at pre-treatment ($M = 12.17$) was further reduced at both post-treatment ($M = 7.51$) and follow-up ($M = 8.14$). A moderate level of anxiety ($n = 12$) at pre-treatment ($M = 19.34$) was decreased to a mild level at both post-treatment ($M = 11.79$) and follow-up ($M = 11.38$). A severe level of anxiety ($n = 1$) at pre-treatment ($M = 31.32$) was decreased to a mild level at post-treatment ($M = 12.93$), no data were available at follow-up. On both BDI-I and BDI-II, a mild level of depression ($n = 24$ for BDI-I and $n = 16$ for BDI-II) at pre-treatment ($M = 14.08$ for BDI-I and 16.19 for BDI-II) was decreased to a mild level of depression at post-treatment ($M = 8.77$ for BDI-I and 8.64 for BDI-II), and to a mild or minimal level at follow-up ($M = 10.48$ for BDI-I and 9.70 for BDI-II). A moderate level of depression ($n = 6$ for BDI-I and $n = 5$ for BDI-II) at pre-treatment ($M = 22.13$ for BDI-I and 23.27 for BDI-II) was reduced to a mild level at both post-treatment ($M = 13.43$ for BDI-I and 14.12 for BDI-II) and follow-up ($M = 13.93$ for BDI-I and 14.97 for BDI-II). A severe level of depression ($n = 1$ for BDI-I and $n = 4$ for BDI-II) at pre-treatment ($M = 30.33$ for BDI-I and 32.29 for BDI-II) was reduced to a moderate to mild level at post-treatment ($M = 12.33$ for BDI-I and 21.13 for BDI-II) and to a mild level at follow-up ($M = 18.56$ for BDI-II).

On the CES-D, results showed that non-clinical depression in five studies at pre-treatment ($M = 11.03$) was further reduced at both post-treatment ($M = 6.76$) and follow-up ($M = 8.44$). Clinical depression ($n = 9$) at pre-treatment ($M = 18.31$) became non-clinical at both post-treatment ($M = 13.48$) and follow-up ($M = 15.49$). Finally, on the STAI, non-clinical anxiety in 13 studies at pre-treatment ($M = 35.91$) was further reduced at both post-treatment ($M = 31.25$) and follow-up ($M = 29.35$). A moderate clinical anxiety ($n = 22$) at pre-treatment ($M = 42.94$) was reduced to non-clinical level of anxiety at post-treatment ($M = 39.73$) and to a mild level at follow-up ($M = 40.33$). A high clinical anxiety ($n = 8$) at pre-treatment ($M = 52.87$) was reduced to moderate levels at both post-treatment ($M = 47.20$) and follow-up ($M = 46.54$).

4. Discussion

This meta-analysis examined 209 studies with a combined total of 12,145 participants of diverse ages, genders, and clinical profiles. The wide variety of studies, the variety of participants, and the use of meta-analytic validity measures allowed us to clarify some inconsistencies concerning the therapeutic value of MBT. The results showed that MBT is moderately effective in pre-post studies. When compared to some other active treatments (including psychoeducation, supportive therapy, relaxation, imagery, and art-therapy), the effect sizes were small to moderate, suggesting the superiority of MBT. However, MBT was not more effective than traditional CBT.

MBT was more effective in treating psychological disorders than it was in treating physical or medical conditions. More specifically, MBT showed large and clinically significant effects in treating anxiety and depression, and the gains were maintained at follow-up. These findings were similar to those obtained in previous meta-analyses (e.g., Hofmann et al., 2010). In addition, the average attrition among participants in the selected studies (16.25%) was smaller than the attrition rate usually obtained in cognitive and behavioral studies (e.g., 22.5% of 1646 patients offered CBT in an National Health Service clinic in the UK; Westbrook & Kirk, 2005). These results suggest a high commitment among participants to MBT.

One obvious question is whether MBT also changes measures of mindfulness. Surprisingly, mindfulness was measured in only 45% of all studies. The results showed that participants in MBT were more mindful at the end of the treatment, and that gains were maintained at the last follow-up. In addition, there was a strong positive correlation between the mindfulness levels of the participants and the clinical outcomes. These results provide preliminary support for the role of mindfulness in the effectiveness of MBT. Future studies will need to explore the mechanism of action for MBT. Similarly, little is known about treatment moderators, such as therapists' training. We observed that therapists' experience with mindfulness, but not their general clinical training, moderated clinical outcomes at the end of the treatment, which was consistent with earlier reports (Pradhan et al., 2007), suggesting that therapists' experience with mindfulness might have a direct or an indirect effect on the clinical outcomes of the participants (Grepmair et al., 2007). Unfortunately, however, very few studies have quantified the therapists' training and experience. Future studies should explicitly report this information.

In contrast with previous meta-analyses of MBT (Hofmann et al., 2010; Klainin-Yobas et al., 2012; Piet & Hougaard, 2011), our results showed that the study quality score negatively moderated the efficacy of MBT, pointing to expectancy and other biases. Similar results were obtained in other meta-analyses (e.g., Wykes, Steel, Everitt, & Tarrier, 2008). However, the duration of treatment and the assigned homework practice time did not consistently moderate the efficacy of MBT. These results are consistent with the contradictory outcomes found in the published literature. Better efficacy predictors could be attendance and the actual duration of home meditation practice, because they measure motivation and might indicate whether participants find the intervention useful (Carmody & Baer, 2008; de Vibe

et al., 2012; Toneatto & Nguyen, 2007). Other possible moderators include meditation depth (Piron, 2001) and group cohesion (Imel, Baldwin, Bonus, & Maccoon, 2008).

In order to conduct a comprehensive review of the literature, we inevitably included studies with different levels of quality, which we quantified and included in the analyses. Our meta-analysis only included mindfulness meditation protocols, limiting the scope of the results to this particular practice. To address our own expectancy bias, we implemented liberal selection criteria and included a large variety of studies. Despite these limitations, our results showed that MBT is moderately to largely effective. Furthermore, the findings suggest that mindfulness is a central component of the treatment effectiveness, and that the mindfulness of participants and of therapists is a strong predictor of effective MBT. We recommend conducting more methodologically rigorous studies to establish the efficacy of MBT in comparison with, or in addition to, other standard treatments (especially to CBT) and in order to thoroughly examine and quantify moderators and mediators of effective MBT.

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Note. A complete list of references for publications included in the meta-analysis is available in Appendix B.

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