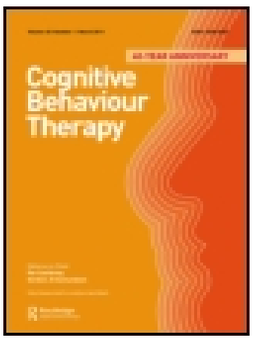

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Mindfulness-based cognitive therapy for the treatment of current depressive symptoms: a meta-analysis

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ABSTRACT

Mindfulness-based cognitive therapy (MBCT) appears to be a promising intervention for the prevention of relapse in major depressive disorder, but its efficacy in patients with current depressive symptoms is less clear. Randomized clinical trials of MBCT for adult patients with current depressive symptoms were included (k = 13, N = 1046). Comparison conditions were coded based on whether they were intended to be therapeutic (specific active controls) or not (non-specific controls). MBCT was superior to non-specific controls at post-treatment (k = 10, d = 0.71, 95% confidence interval [CI] [0.47, 0.96]), although not at longest follow-up (k = 2, d = 1.47, [–0.71, 3.65], mean follow-up = 5.70 months across all studies with follow-up). MBCT did not differ from other active therapies at post-treatment (k = 6, d = 0.002, [–0.43, 0.44]) and longest follow-up (k = 4, d = 0.26, [–0.24, 0.75]). There was some evidence that studies with higher methodological quality showed smaller effects at post-treatment, but no evidence that effects varied by inclusion criterion. The impact of publication bias appeared minimal. MBCT seems to be efficacious for samples with current depressive symptoms at post-treatment, although a limited number of studies tested the long-term effects of this therapy.

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KEYWORDS
Mindfulness-based cognitive therapy; depression; meta-analysis; relative efficacy

Of all mental health conditions, depression is one of the most common and is responsible for significant public health costs (Collins et al., 2011). A variety of evidence-based psychosocial interventions have been developed to treat the disease burden of depressive disorders (Cuijpers, van Straten, Andersson, & van Oppen, 2008). One such treatment with growing scientific, clinical, and popular interest is mindfulness-based cognitive therapy (MBCT; Segal, Williams, & Teasdale, 2002). MBCT was developed to function as prophylaxis against the relapsing course typical in major depressive disorder (MDD; Teasdale et al., 2000). The developers of MBCT posited that patients with a history of depression who were currently in remission could successfully learn cognitive and affective strategies that would better equip them for

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managing the patterns of thinking activated by dysphoria that unavoidably occurs in daily life that is magnified in the context of MDD. By learning these skills while in remission, patients could prevent the intensification of depressogenic patterns of cognition (e.g. rumination) that lead to relapse (Segal & Dinh-Williams, 2016; Teasdale, Segal, & Williams, 1995). Indeed, the initial formulation of MBCT highlighted the treatment being "acceptable in the euthymic state" (p. 38) and focused on preventing "the escalation of states of mild negative affect to more severe and persistent states" (p. 36; Teasdale et al., 1995). That is to say, the treatment was not initially intended for currently depressed patients whose degree of negative affect was more severe.

There has been a substantial increase in the number of studies published in the past several years examining the efficacy of MBCT among people who are currently experiencing depression or elevated symptoms of depression (see Table 1). While several meta-analyses have indicated that mindfulness-based interventions in general—including MBCT, mindfulness-based stress reduction (MBSR), and others—are effective at reducing depressive symptoms (see Goldberg et al., 2018; Goyal et al., 2014; Hedman-Lagerlöf, Hedman-Lagerlöf, & Öst, 2018; Khoury et al., 2013; Wang et al., 2018). To our knowledge, no meta-analysis to date has examined the efficacy of *MBCT specifically for*

Table 1. Study-level descriptive statistics.

Original study	Control group	Control type	Inclusion	ES	Var	ES FU	Var FU	Tx *n*	Cont *n*	Age	% Fem
Abolghasemi et al. (2015)	CT	Spec	Dep dx	0.61	1.87	1.06	1.68	15	15	29.00	60.00
Asl and Barahmand (2014)	Waitlist	Non	Elevated sx	0.81	0.14	NA	NA	18	17	29.50	0.00
Chiesa et al. (2015)	Psychoed	Spec	Elevated sx	0.54	0.08	0.81	0.09	26	24	48.95	72.09
Cladder-Micus et al. (2018)	Waitlist	Non	Dep dx	0.38	0.04	NA	NA	49	57	47.10	62.26
De Jong et al. (2016)	Waitlist	Non	Elevated sx	0.18	0.09	NA	NA	26	14	50.70	72.70
Eisendrath et al. (2016)	HEP	Spec	Dep dx	0.41	0.03	0.05	0.02	87	86	46.16	76.30
Kaviani, Hatami, and Javaheri (2012)	Waitlist	Non	Elevated sx	0.56	0.20	2.66	0.43	15	15	21.70	100.00
Leung (2015)	Waitlist	Non	Elevated sx	1.07	0.08	NA	NA	31	56	44.68	77.80
Michalak et al. (2015)	TAU	Non	Dep dx	0.43	0.06	0.43	0.09	36	35	50.84	62.26
Michalak et al. (2015)	CBASP	Spec	Dep dx	−0.34	0.07	−0.11	0.10	36	35	50.84	62.26
Omidi et al. (2013)	Waitlist	Non	Dep dx	1.40	0.11	NA	NA	30	30	28.00	66.33
Omidi et al. (2013)	CBT	Spec	Dep dx	−0.85	0.21	NA	NA	30	30	28.00	66.33
Panahi and Faramarzi (2016)	Waitlist	Non	Elevated sx	1.30	0.09	NA	NA	30	30	NA	100.00
Tovote et al. (2014)	Waitlist	Non	Elevated sx	0.74	0.06	NA	NA	31	31	53.10	49.00
Tovote et al. (2014)	CBT	Spec	Elevated sx	−0.19	0.07	NA	NA	31	32	53.10	49.00
van Aalderen et al. (2012)	TAU	Non	Elevated sx	0.57	0.05	NA	NA	74	72	47.50	71.00

Inclusion = study depression inclusion criterion (Dep dx = diagnosis of depression, Elevated sx = symptoms above clinical cut-off on standardized measure of depression); ES = standardized effect size in Cohen's *d* units; Var = variance; FU = longest follow-up; Tx *n* = mindfulness condition sample size; Cont *n* = control condition sample size; Non = non-specific control condition (i.e. not intended to be therapeutic); Spec = specific active control condition (i.e. intended to be therapeutic; Wampold & Imel, 2015); Age = average sample age; % Fem = percentage female; NA = not applicable; CT = cognitive therapy; TAU = treatment-as-usual; HEP = Health Enhancement Program; Psychoed = psychoeducation; CBT = cognitive behavioral therapy; CBASP = Cognitive Behavioral Analysis System of Psychotherapy.

patients with current depressive symptoms. However, such a meta-analysis is vital for establishing the efficacy of MBCT for this population (American Psychological Association, 2006). Given increasing efforts towards dissemination and implementation of MBCT (e.g. within the United Kingdom based on National Institute for Health and Care Excellence (NICE), 2009 guidelines; Crane & Kuyken, 2013), evaluation of the evidence for MBCT among individuals with current depressive symptoms may also have health service implications.

Methods

Eligibility criteria

We included randomized clinical trials (RCTs) of MBCT for adult patients with current depressive symptoms defined either via diagnostic interview (e.g. Structured Clinical Interview for DSM-IV diagnosis [SCID]) or scores above the clinical cut-off on a standardized, clinician-rated or self-report measure of depressive symptoms (e.g. Beck Depression Inventory [BDI]; Hamilton Rating Scale of Depression [HAM-D]). To be included, a trial had to specify that the mindfulness intervention offered was MBCT based on the work of Segal et al. (2002). Interventions had to be delivered in real time (i.e. provided synchronously, not simply through pre-recorded video instruction). Studies were excluded for the following reasons: (1) data unavailable to compute standardized effect sizes (even after contacting study authors); (2) no depression outcomes (e.g. BDI, HAM-D) reported; (3) data redundant with other included studies; (4) no non-mindfulness-based intervention or condition included (e.g. study compared two mindfulness-based interventions).

Information sources

We searched the following databases: PubMed, PsycInfo, Scopus, Web of Science. In addition, a publicly available comprehensive repository of mindfulness studies that is updated monthly was also searched (Black, 2012). Citations from recent meta-analyses and systematic reviews were also included (Goyal et al., 2014; Khoury et al., 2013; Kuyken et al., 2016). An initial search of citations included those from the first available date until January 2nd, 2017. In addition, we searched for unpublished trials by reviewing studies registered in ClinicalTrials.gov. The search was subsequently updated on 5 October 2018.

Search

The initial search was part of a larger, comprehensive meta-analysis examining the efficacy of mindfulness-based interventions for a range of psychiatric conditions (Goldberg et al., 2018). In contrast to the larger meta-analysis, which included 11 diagnostic categories (e.g. posttraumatic stress disorder, substance use disorders, sleep disorders) and various mindfulness-based interventions (e.g. MBSR, mindfulness-based relapse prevention; Bowen et al., 2014; Kabat-Zinn, 1990), the current study focuses exclusively on the evidence for MBCT in samples with current depressive symptoms. For the initial search, the terms

"mindfulness" and "random*" were used, with studies relevant to depression drawn from the larger sample. Dissertations and studies published in non-peer-reviewed journals were also included in the search. ClinicalTrials.gov was searched using the terms "depression" and "mindfulness-based cognitive therapy." For the updated search, we used the terms "mindfulness-based cognitive therapy" or "MBCT" paired with "random*."

Study selection

Titles and/or abstracts of potential studies were independently coded by the first author and a second co-author. Disagreements were discussed with the senior author.

Data collection process

Standardized spreadsheets were developed for coding both study-level and effect size-level data. Coders were trained by the first author through coding an initial sample of studies ($k = 10$) in order to achieve reliability. Data were extracted independently by the first author and a second co-author. Disagreements were discussed with the senior author. Inter-rater reliabilities were in the good to excellent range (Cicchetti, 1994): Ks > .60 and ICCs > 0.80. When sufficient data for computing standardized effect sizes were unavailable, study authors were contacted. One study was excluded due to data being unavailable from study authors (see Figure 1).

Data items

Along with data necessary for computing standardized effect sizes, the following data were extracted: (1) publication year; (2) intent-to-treat (ITT) sample size; (3) sample demographics[1] (mean age, percentage female); (4) country of origin; (5) quality of the control condition. Quality of the control condition was assessed based on a two-tier system: non-specific control conditions and specific active control conditions. Non-specific control conditions included waitlist controls as well as other comparison groups that were not intended to provide therapeutic benefits (e.g. attentional control groups). Treatment-as-usual (TAU) conditions were coded as non-specific controls if both the MBCT and non-MBCT arms received this treatment (i.e. there was no additional treatment provided to the TAU group that was not also provided to the MBCT group). The specific active control conditions (e.g. cognitive behavioural therapy) included comparisons that were based on actual therapies (i.e. not "placebo" conditions that were not intended to be therapeutic) and included specific treatment ingredients and mechanisms of change (Wampold & Imel, 2015; Wampold et al., 1997). The decision to code using this scheme was made to minimize the number of comparisons being tested, to increase the number of studies (and statistical power) available for a given comparison, and based on evidence that whether a comparison group represents a specific active control condition significantly influences the relative efficacy of mindfulness-based interventions (Goldberg et al., 2018; Goyal et al., 2014). The comparison with other active therapies has been consistently highlighted as a key test of the efficacy of mindfulness-based interventions (Davidson & Kaszniak, 2015; Goldberg et al., 2017).

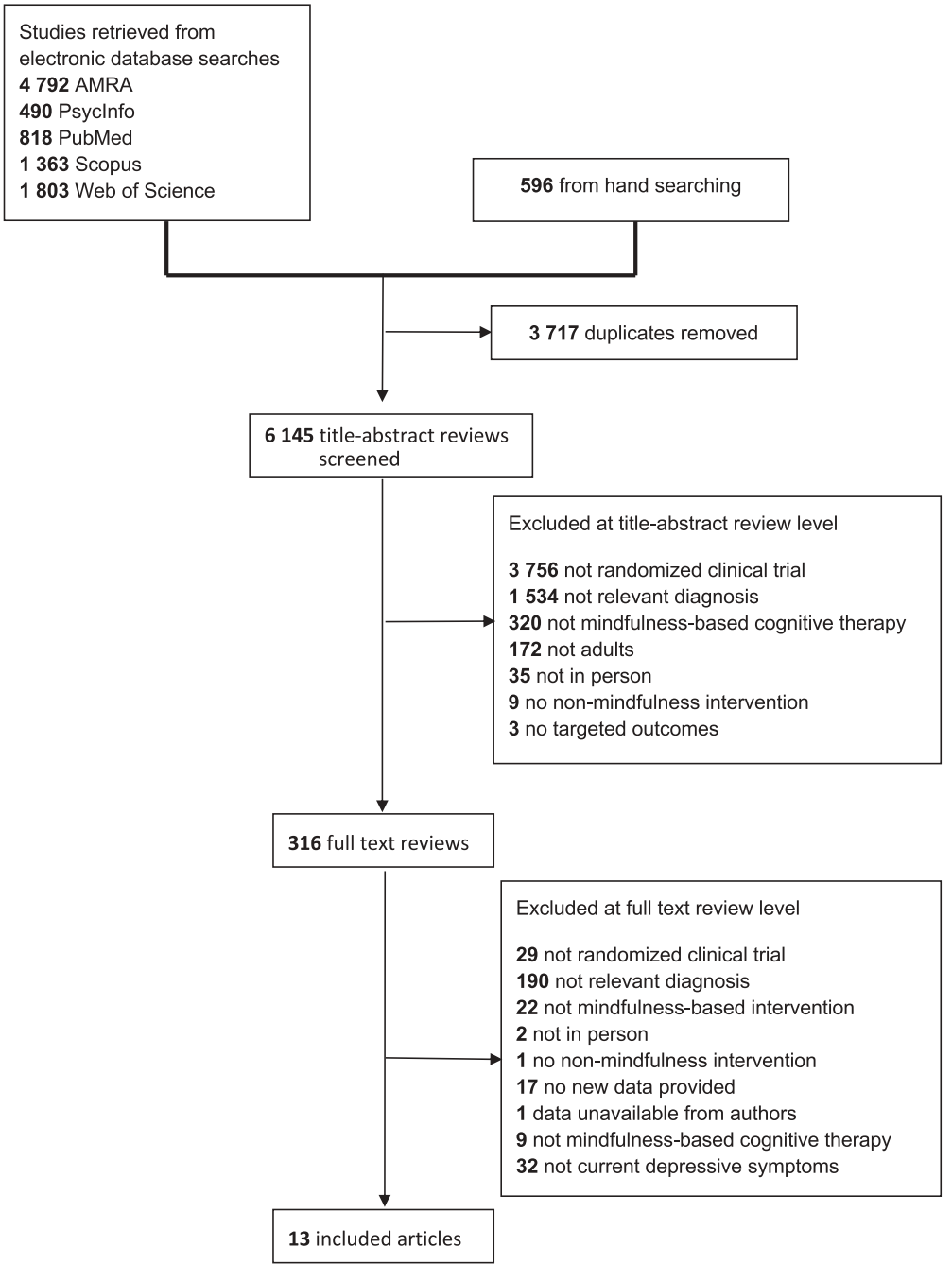

Figure 1. PRISMA flow diagram.

Risk of bias in individual studies

Considerations for minimizing bias and assessing methodological quality in individual studies were modelled after a previous meta-analysis of MBCT for depressive relapse (Piet & Hougaard, 2011). We used Piet and Hougaard (2011) modified Jadad et al. (1996)

criteria, coding (1) whether a trial was randomized, (2) whether the randomization procedure was described and appropriate (e.g. employed randomly generated group assignment), (3) whether outcome assessment was blind to group assignment, and (4) whether reasons were given for the withdrawal and dropouts in each group. Items were coded as "yes" if the criterion was present, "no" if not present, and "unclear" if the presence of this feature was ambiguous. To compute a Jadad score, we summed all four items, coding yes responses as 1 and no or unclear responses as 0. In order to assess the impact of these features on study results, Jadad scores were tested as moderators of treatment effects in primary analyses.

Five additional items were also coded based on Piet and Hougaard (2011). While these items did not contribute to the Jadad score, they provide information regarding the studies' risk for bias and methodological quality. Namely, we also assessed (1) whether treatment allocation was concealed (e.g. using an opaque envelope or an external allocator to provide group assignments), (2) whether groups were similar at baseline on prognostic indicators, (3) whether the number of withdrawals and dropouts in each group were mentioned, (4) whether analyses were conducted using the ITT sample, and (5) whether a power calculation was described. As mentioned earlier, these items were coded as yes, no, or unclear.

We considered using other methods for assessing risk for bias (e.g. Cochrane tool, GRADE; Atkins et al., 2003; Higgins & Green, 2011). Ultimately, we felt that providing information about specific study features and reporting the uncertainty of effect size estimates (using CIs and metrics of heterogeneity, i.e. I^2) was preferable to making at least partially subjective judgments about the strength of the body of evidence. Further, providing ratings consistent with previous meta-analyses of MBCT seemed ideal for allowing comparison between these related bodies of research. In addition, to reduce potential bias, we used data from ITT analyses when available.

Summary measures

Our primary effect size measure was the standardized mean difference (Cohen's *d*). As done by Goyal et al. (2014) and Goldberg et al. (2018), we first computed a pre-post (or pre-follow-up) effect size for both the MBCT and control groups alone. This method has the advantage of accounting for potential baseline differences (i.e. it does not rely exclusively on between-group differences at post-test; Becker, 1988). Formulas for these within-group effect sizes and their variance are as follows:

$$d_{within} = \frac{M_{post} - M_{pre}}{SD_{pooled}} \tag{1a}$$

$$var(d_{within}) = \left(\frac{1}{n} + \frac{d^2}{2n}\right) \cdot 2(1 - r) \tag{1b}$$

where r is the correlation between pre- and post-scores. As is often the case, the included studies did not report r, so we imputed a correlation of r_{XX} = .50 between time points (slightly lower than a typical test–retest correlation, to account for intervention effects; see Hoyt & Del Re, 2018). Within-group effect sizes were corrected for bias by converting to

Hedges' g_{within} as recommended by Borenstein, Hedges, Higgins, and Rothstein (2009). Effects were computed both from pre- to post-treatment (or time point closest to post-treatment) as well as from pre- to last available follow-up time point.

We then calculated the relative difference in the pre-post (or pre-follow-up) effects (i.e. change scores) between the MBCT and control conditions using standard methods (Becker, 1988).

$$\Delta = g^{M}_{within} - g^{C}_{within} \tag{2a}$$

$$var(\Delta) = var\left(g^{M}_{within}\right) + var\left(g^{C}_{within}\right) \tag{2b}$$

where the *M* and *C* superscripts refer to the MBCT and comparison conditions, respectively. Given the resultant effect size remains within standard deviation (SD) units, we have retained the notation of the more familiar *d* (rather than Becker's Δ) to refer to treatment effects.

Analyses were conducted using the R statistical software and the "metafor" and "MAd" packages (Del Re & Hoyt, 2010; Viechtbauer, 2010).

Synthesis of results

When available, effect sizes were computed using pre- and post-test means and SDs. Other reported statistics (e.g. *F, t, p*, odds ratios) were used when appropriate based on standard meta-analytic methods (Cooper, Hedges, & Valentine, 2009). When multiple measures of depression were reported (e.g. relapse rates based on clinical interview, scores on self-report measures of depression), data were aggregated first within-studies using the "MAd" package (and an assumed correlation between measures of .50, see Wampold et al., 1997 for a rationale) and then between studies, based on the comparison of interest (i.e. MBCT vs. non-specific control conditions). Summary statistics were computed in standardized units along with 95% CIs. Heterogeneity was systematically assessed using the I^2 (measuring the proportion of between-study heterogeneity) and interpreted based on Higgins, Thompson, Deeks, and Altman's (2003) guidelines. Random effects analyses were used with effect sizes weighted by the inverse of their variance (Cooper et al., 2009). Models were run separately for the specific active comparison conditions and the non-specific comparison conditions. In addition, we conducted analyses to assess the potential impact of variation in inclusion criterion related to depressive symptoms across the sample. Specifically, we conducted moderator analyses to assess whether effects differed for studies that required a formal diagnosis of depression (e.g. MDD diagnoses via SCID; Michalak, Schultze, Heidenreich, & Schramm, 2015) versus those that required only symptoms above a clinical cut-off (e.g. on the BDI-II; Tovote et al., 2014). Results are reported separately based on diagnostic inclusion criterion.

Three studies (Michalak et al., 2015; Omidi, Mohammadkhani, Mohammadi, & Zargar, 2013; Tovote et al., 2014) included both a specific active comparison group and a non-specific control group (i.e. waitlist). Given specific active comparisons and non-specific control groups were assessed in separate models, in order to more fully represent the available data, we allowed multiple comparison groups to be included from these three

studies. This required duplicating data from each study's MBCT condition to make the additional comparison. To assess potential bias introduced by duplicating MBCT condition data (Higgins & Green, 2011), we report sensitivity analyses where the comparisons with non-specific control conditions for these three studies are excluded.

Risk of bias across studies

We assessed publication bias by visually inspecting funnel plots for asymmetry within the comparison of interest. In addition, models were re-estimated using trim-and-fill methods that account for the asymmetric distribution of studies around an omnibus effect (Viechtbauer, 2010). Meta-regression models were also run modelling indicators of study quality using the modified Jadad score (Piet & Hougaard, 2011) as a moderator of treatment effects. A fail-safe N was computed based on Rosenthal's (1979) method in order to estimate the number of unpublished null finding studies that would need to exist to nullify the observed effect in the current sample of studies.

Results

Study selection

A total of 9862 citations were retrieved. After 3717 duplicates were removed, 6145 unique titles and/or abstracts were coded. Following the application of the exclusion criteria (see Figure 1 for Preferred Reporting Items for Systematic Reviews and Meta-analysis [PRISMA] flow diagram, see Supplemental Materials for full PRISMA Checklist; Moher, Liberati, Tetzlaff, & Altman, 2009), 13 studies were retained for analysis representing 1046 participants.

In addition, 61 trials were identified from ClinicalTrials.gov. Three of these trials were among the 12 located through the initial search of the published literature. The remaining 58 trials were excluded for the following reasons: trial not completed (k = 29), not relevant diagnosis (k = 16), not RCT (k = 6), not MBCT (k = 4), and no non-mindfulness condition (k = 3). One additional study (Cladder-Micus et al., 2018) was located during the course of updating the literature review.

Study characteristics

The sample was on average 41.44 years old (SD = 11.02) and 66.90% female. The largest percentage of trials was conducted in the Iran (38.46%) followed by the Netherlands (23.08%) and the United States (15.38%). A minority of studies (k = 5, 38.46%) included a follow-up time point. Among studies with follow-up, the average length to follow-up was 5.70 months (SD = 2.96, range = 2 to 10).

Risk of bias within studies

Table 2 presents scores on the four modified Jadad items along with five additional metrics of methodological quality and risk for bias. All included studies used randomized designs. Modified Jadad scores ranged from 1 to 4 with a mean rating of 2.46 (SD = 0.97), which is

Table 2. Methodological quality and modified Jadad scoring for included studies.

Study	Was the trial randomized?	Was the randomization procedure described and appropriate?	Was the treatment allocation concealed?	Were groups similar at baseline on prognostic indicators?	Was blind outcome assessments conducted?	Was the number of withdrawals/dropouts in each group mentioned?	In addition to stating the number of withdrawals/dropouts, were reasons given for each group?	Was an analysis conducted on the intent-to-treat sample?	Was a power calculation described?	Jadad score (revised maximum = 4)
Abolghasemi et al. (2015)	**Yes**	**No**	Unclear	Unclear	**No**	No	**No**	Unclear	No	1
Asl and Barahmand (2014)	**Yes**	**No**	Unclear	Yes	**No**	Yes	**Yes**	No	No	2
Chiesa et al. (2015)	**Yes**	**Yes**	Yes	Yes	**Yes**	Yes	**No**	Yes	Yes	3
Cladder-Micus et al. (2018)	**Yes**	**Yes**	Unclear	Yes	**No**	Yes	**Yes**	Yes	Yes	3
De Jong et al., (2016)	**Yes**	**Yes**	Unclear	Yes	**No**	Yes	**Yes**	Yes	Yes	3
Eisendrath et al. (2016)	**Yes**	**Yes**	Unclear	Yes	**Yes**	Yes	**Yes**	Yes	Unclear	4
Kaviani et al., (2012)	**Yes**	**No**	Unclear	Unclear	**No**	Yes	**No**	No	No	1
Leung (2015)	**Yes**	**Yes**	Unclear	Yes	**No**	Yes	**Yes**	No	Yes	3
Michalak et al. (2015)	**Yes**	**Yes**	Yes	Yes	**Yes**	Yes	**No**	Yes	Yes	3
Omidi et al., (2013)	**Yes**	**No**	Unclear	Unclear	**No**	No	**No**	Unclear	No	1
Panahi and Faramarzi (2016)	**Yes**	**Yes**	Unclear	Yes	**No**	No	**No**	Unclear	Yes	2
Tovote et al. (2014)	**Yes**	**Yes**	Unclear	Yes	**No**	Yes	**Yes**	Yes	Yes	3
van Aalderen et al. (2012)	**Yes**	**Yes**	Yes	Yes	**Unclear**	Yes	**Yes**	No	No	3

Bolded columns used for computation of revised Jadad score (see Piet & Hougaard, 2011).

slightly lower than the mean of 3.00 reported by Piet and Hougaard (2011). Only one study (Eisendrath et al., 2016) received a score of 1 for all four Jadad items, although seven studies received a score of 3. Blind outcome assessment was conducted in 23.08% of studies and ITT analyses were reported in 46.15% of studies.

Results of individual studies

For each included study, treatment effects on depressive outcomes in standardized units and other study characteristics are reported in Table 1.

Synthesis of results

Effects at post-treatment

MBCT was shown to be superior to non-specific control conditions at post-treatment across 10 studies (d = 0.71, [0.47, 0.96], Table 3, Figure 2), with a moderate amount of heterogeneity (I^2 = 50.40). In a sensitivity analysis that excluded three non-specific control conditions in studies that included multiple comparison groups, effects were similar (k = 7, d = 0.68 [0.38, 0.97]). Effects were not moderated by inclusion criterion (i.e. diagnosis of depression vs. symptoms above a clinical cut-off on a standardized measure of depression), ds = 0.69 and 0.74, for diagnosis of depression and elevated symptoms, respectively, Q[1] = 0.10, p = .754.

Across six studies, MBCT did not differ statistically from specific active control conditions at post-treatment (d = 0.002, [−0.43, 0.44], Figure 3). A moderate-to-large amount of heterogeneity was detected in this subsample (I^2 = 65.34). Effects were again not moderated by inclusion criterion (ds = −0.13 and 0.17, for diagnosis of depression and elevated symptoms, respectively, Q[1] = 0.32, p = .570). As inclusion criterion did not appear to influence post-treatment effects for studies using either non-specific or specific active control conditions, subsequent analyses collapsed across samples with and without a diagnosis of depression as inclusion criterion.

Table 3. Results of meta-analysis and trim-and-fill analysis.

		Meta-analysis					Trim-and-fill analysis	
Time point	Comparison type	N	k	ES [95% CI]	I^2 [95% CI]	Fail-safe N	k_{imp}	ES_{adj} [95% CI]
Post	Non-specific[a]	697	10	0.71 [0.47, 0.96]	50.40 [0.00, 86.13]	239	0	
Post	Specific[b]	447	6	0.002 [−0.43, 0.44]	65.34 [3.22, 94.97]	0	0	
Follow-up	Non-specific	101	2	1.47 [−0.71, 3.65]	89.40 [46.75, 99.74]	10	NA	
Follow-up	Specific	324	4	0.26 [−0.24, 0.75]	59.31 [0.00, 97.72]	1	1	0.23 [−0.24, 0.70]

N = sample size (note that total sample size is larger than the unique sample size due to some studies including multiple comparison groups); k = number of comparisons (note that total number of comparisons is larger than the unique sample size due to some studies including multiple comparison groups); *ES* = Cohen's d standardized effect size; I^2 = heterogeneity; k_{imp} = number of imputed comparisons based on trim-and-fill analyses; ES_{adj} = adjusted effect size based on trim-and-fill analyses; Dep diagnosis = studies requiring a diagnosis of depression for inclusion criterion; Elevated symptoms = studies requiring elevated symptoms of depression on a standardized measures of depression for inclusion criterion (i.e. depression diagnosis or elevated symptoms of depression). Superscripts a and b report results of testing inclusion criterion as moderator of treatment effects.

[a] Q[1] = 0.10, p = .754.

[b] Q[1] = 0.32, p = .570.

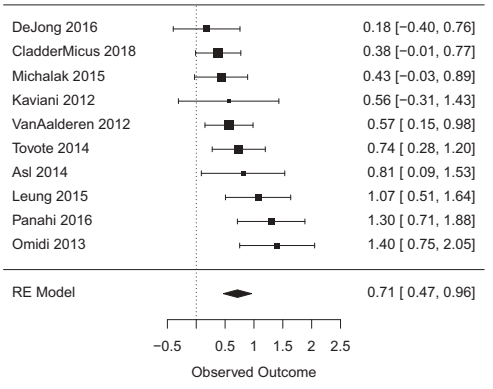

Figure 2. Effects of MBCT versus non-specific comparison conditions at post-treatment. The size of each point is relative to the given study's weight in the meta-analysis (i.e. inverse variance).

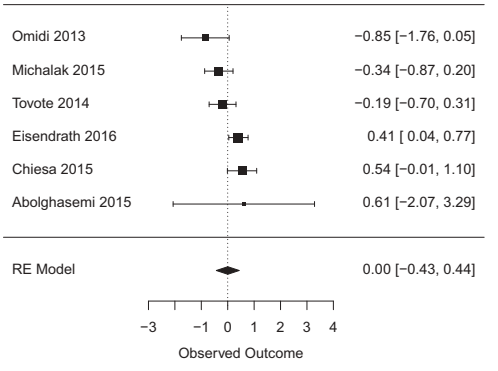

Figure 3. Effects of MBCT versus specific active comparison conditions at post-treatment.

Effects at longest follow-up

A subsample of studies (k = 5) included a follow-up assessment point. Two studies compared MBCT to a non-specific control at follow-up, showing a large but non-significant effect (d = 1.47, [−0.71, 3.65], Figure 4), with a large amount of heterogeneity (I^2 = 89.40). Although one of the two studies comparing MBCT to a non-specific control at follow-up included multiple comparison conditions, a sensitivity analysis excluding this study would have left only one study (an insufficient number for meta-analytic purposes; Fu et al., 2011), and is therefore not reported.

Across four studies, MBCT did not differ statistically from specific active control conditions at follow-up (d = 0.26, [−0.24, 0.75], Figure 5). Heterogeneity in this subsample was moderate to large in magnitude (I^2 = 59.31).

Risk of bias across studies

Bias across studies was assessed through funnel plots, trim-and-fill analyses, an additional moderator analysis using modified Jadad scores, and estimation of the fail-safe N (Rosenthal, 1979). Asymmetric funnel plots suggested evidence for publication bias in one model (comparisons with specific active controls at follow-up; Figure 6 and Supplemental Materials). Modified effect sizes from trim-and-fill analyses are reported in Table 3. The significance test for the model with asymmetry did not change with this adjustment and the effect size remained largely unchanged. Study methodological quality was a significant moderator of treatment effects for comparisons with non-specific controls at post-treatment. In particular, higher quality studies showed smaller effects (B = −0.33, Q[1] = 5.66, p = .017, see Supplemental Materials). Study quality did not moderate treatment effects in any other models, although insufficient studies precluded testing moderation for comparisons with non-specific controls at follow-up. The fail-safe N was small for all models with the exception of comparisons with non-specific controls at post-treatment (fail-safe N = 239). Rosenberg (2005) provides

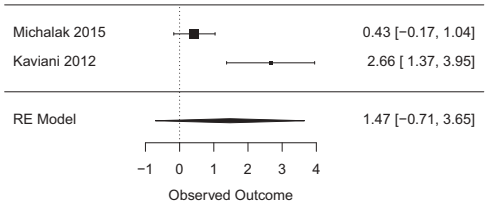

Figure 4. Effects of MBCT versus non-specific comparison conditions at follow-up.

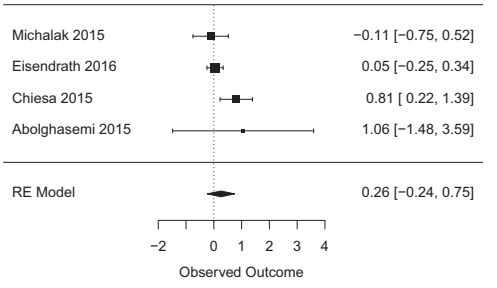

Figure 5. Effects of MBCT versus specific active comparison conditions at follow-up.

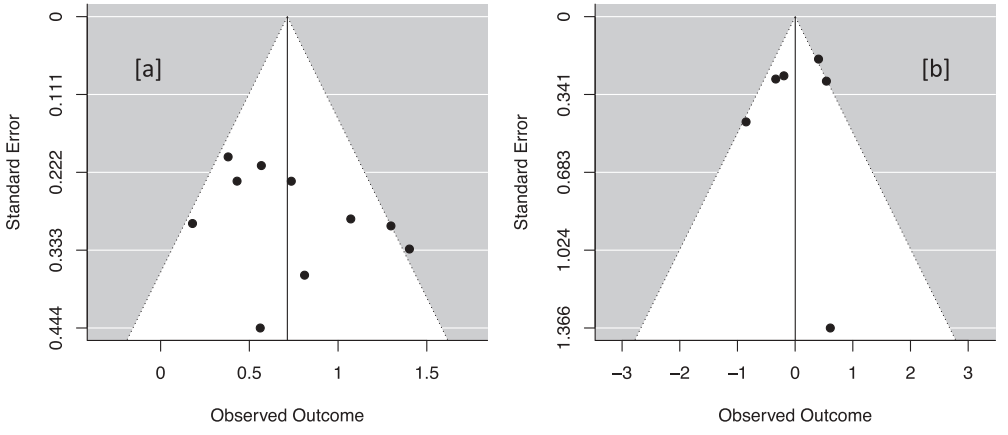

Figure 6. Funnel plots of post-treatment effects. (a) Non-specific; (b) specific active comparisons.

guidelines for determining whether a given fail-safe N can be considered robust to publication bias based on the number of published studies (i.e. fail-safe N > $5n$ + 10, where n = the number of published studies). Based on these guidelines, the fail-safe N for non-specific controls at post-treatment is robust against publication bias (239 > 5 × 10 + 10).

Discussion

To our knowledge, this is the first meta-analysis to examine the efficacy of MBCT specifically for patients who are currently experiencing depressive symptoms. In general, results at post-treatment (ds = 0.71 and 0.002 for non-specific and specific active controls, respectively) mirror other recent meta-analyses of MBCT (Chiesa & Serretti, 2011; Kuyken et al., 2016) such that effects for patients who are currently experiencing depressive symptoms were similar to effects for those who were in remission (Piet & Hougaard, 2011). This finding is consistent with the Kuyken et al. (2016) meta-analysis that found patients in remission but with higher baseline residual depressive severity benefit from MBCT, perhaps even more so than patients with lower severity at baseline. Results are also consistent with moderate effect sizes found in recent reviews of mindfulness-based therapies (not exclusively MBCT) for

depressive symptoms (Hedman-Lagerlöf et al., 2018; Wang et al., 2018) as well as meta-analyses of cognitive behavioural therapy for the treatment of current depression (e.g. Cuijpers et al., 2013). Importantly, effects at post-treatment were robust to trim-and-fill analyses accounting for publication bias (an area of concern for mindfulness research; Coronado-Montoya et al., 2016), did not differ between studies requiring a diagnosis of depression vs. elevated symptoms for inclusion, and were consistent whether or not multiple comparison groups were allowed from the same study. However, it did appear that study quality impacted results in one sub-analysis: higher quality studies using a non-specific comparison group showed smaller effects at post-treatment. A high degree of heterogeneity within subgroups (I^2 in the moderate-to-large range) was also evident, supporting the notion that further systematic differences may exist between studies.

Results at follow-up are less conclusive. MBCT was not superior to non-specific controls. Although effects were large (d = 1.47), only two small studies (combined n = 101) were available for this comparison, which raises concerns regarding the reliability of this estimate and statistical power to detect an effect. Across four studies, effects of MBCT were comparable to other active therapies at follow-up (d = 0.26). This suggests that MBCT may be of similar efficacy to other therapies that are routinely offered (e.g. cognitive therapy; Abolghasemi, Gholami, Narimani, & Gamji, 2015). However, given the small number of studies including follow-up time points, these findings should be interpreted cautiously. Follow-up effects did not appear meaningfully influenced by publication bias nor did methodological quality moderate observed effects for comparisons with specific active controls (these analyses were not possible for comparisons with non-specific controls due to an insufficient number of studies). Heterogeneity within subgroups was also in the moderate-to-large range, again implying that further systematic differences may exist between studies.

Despite uncertainty regarding the long-term effects of MBCT, primarily due to the small number of studies, effect size estimates from this meta-analysis support future research on MBCT for patients who are actively depressed. It will be crucial that future studies more clearly investigate potential mechanisms of MBCT, and the possibility that MBCT functions differently for patients who are currently depressed and those in remission (and even, perhaps, differently among those in remission with varying numbers of prior episodes, e.g. two versus three or more; Teasdale et al., 2000). Such investigations may involve exploring the neural bases of changes in depressive symptoms over the course of MBCT (Davidson, 2016). If evidence of overall equivalence of MBCT to other therapeutic interventions is confirmed in future studies, a key issue will be to determine whether individuals with specific affective or cognitive styles benefit more from MBCT compared with other interventions so that prescriptive assignment to intervention might occur. Of course, patient preferences for MBCT or other evidence-based psychotherapies may also be important to consider in clinical implementation and future research efforts (Swift & Callahan, 2009). Studies comparing MBCT (without antidepressant medications) to antidepressant medications for currently depressed individuals could also be valuable (and were surprisingly not represented in the included studies, although several studies allowed continuing medications as usual; e.g. De Jong et al., 2016).

Given that MBCT may be of similar efficacy to other active therapies for currently depressed individuals, further research could continue to examine the

efficacy of MBCT in other psychiatric conditions (e.g. anxiety disorders). While a significant number of RCTs have explored MBCT for depression, far fewer RCTs have been conducted on other psychiatric conditions potentially amenable to the active treatment ingredients offered in MBCT (Goldberg et al., 2018). Numerous other psychiatric conditions share core cognitive and affective features with depression (e.g. rumination in posttraumatic stress disorder), and may therefore be positively impacted by MBCT.

Several limitations of the current study are worth noting. The primary limitations are related to the meta-analytic sample itself. There was evidence of bias in one of our analyses, with trim-and-fill results suggesting effects may have been overestimated (although the trim-and-fill adjusted effect sizes did not change the significance tests). In addition, some analyses (e.g. follow-up effects for non-specific control conditions) included very few studies, limiting statistical power and the reliability of effect size estimates. Tests of publication bias (e.g. trim-and-fill analyses) were also likely underpowered which may have limited our ability to detect bias (Sterne, Gavaghan, & Egger, 2000). There was considerable heterogeneity across studies (i.e. large I^2 values), which brings into question the aggregate effect sizes as a reliable metric of treatment effects. Study quality was another concern in the meta-analytic sample. Effects at post-treatment for non-specific controls were moderated by study quality, no study included all quality features assessed, and several included only a few features, reducing confidence in the available evidence. Increasing methodological quality (see Goldberg et al., 2017 for a discussion of this issue within mindfulness research generally) will be vital in future studies of MBCT. Further, our results are only as valid as the reporting conducted in the original studies, which may be at risk for "p-hacking" and other selective reporting biases (Simmons, Nelson, & Simonsohn, 2011). Of course, our focus on depressive symptoms (presumably a primary outcome in all of the studies, given their focus on MBCT for depressive symptoms) may have reduced risk of some reporting biases.

Nonetheless, it is unlikely that these threats to validity alone account for the beneficial effects of MBCT observed in this meta-analysis and the apparent equivalence of MBCT with other active therapies. Future research is warranted to establish the long-term effects of MBCT for patients with current depressive symptoms which may help determine whether MBCT can be recommended for current as well as remitted depression (NICE, 2009).

Note

1. Additional demographic feature were considered (e.g. sample average education, race/ethnicity) but were found to be inconsistently reported.

Disclosure of Interest

RJD is the founder, president, and serves on the board of directors for the non-profit organization, Healthy Minds Innovations, Inc. The remaining authors report no conflict of interest. Any views, findings, conclusions, or recommendations expressed in this publication do not necessarily reflect those of the Mind and Life Institute [Varela Award].

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