Abstract

Objectives: Research with mindfulness-based programs (MBPs) has found participating in an MBP to predict beneficial outcomes, however, there is currently mixed research regarding the most helpful dose. This review aimed to determine whether different doses related to MBPs significantly predict outcomes.

Methods: Systematic literature searches of electronic databases and trial registration sites for all randomized controlled trials of MBPs identified 203 studies (N=15,971). Depression was the primary outcome at post-program and follow-up, with secondary outcomes being mindfulness, anxiety and stress. Doses examined related to session numbers, duration and length, facilitator contact and practice. Dose-response relationships were analyzed using meta-regression in R with separate analyses for inactive and active controls.

Results: Initial meta-analyses found significant between-group differences favoring MBPs for all outcomes. Meta-regression results suggested significant dose-response relationships for the mindfulness outcome for doses relating to face-to-face contact (d=0.211; C.I.[0.064,0.358]), program intensity (d=0.895; C.I.[0.315,1.474]) and actual program use (d=0.013; C.I.[0.001,0.024]). The majority of results for psychological outcomes, including depression, were not significant.

Conclusions: This meta-regression examines dose-response relationships for different types and doses relating to MBPs. Considered together, MBPs appeared helpful compared to controls, supporting previous research. Based on meta-regression results, there was no evidence that larger doses are more helpful than smaller doses for predicting psychological outcomes; a finding consistent with some previous research particularly with non-clinical populations. Additionally, greater contact, intensity and actual use of MBPs predicting increased mindfulness corresponds with previous research and theory. Potential limitations and recommendations for future research are explored.

Keywords*: mindfulness; mindfulness-based programs; dose-response; meta-regression; meta-analysis; RCT; depression.*

In recent years, research with mindfulness-based programs (MBPs) has grown exponentially (Goldberg et al. 2017). MBPs are programs which focus on the principles and practices of *mindfulness*, a term which originated from Buddhist traditions and has been defined as paying attention to the present moment non-judgmentally and on purpose (Kabat-Zinn 1990). Mindfulness has been associated with greater interpersonal abilities, intrapersonal awareness and effective emotion regulation (Davis and Hayes 2011) and previous research has found positive results of mindfulness for clinical (Keng et al. 2011), as well as non-clinical (e.g. Dane and Brummel 2014) populations. In particular, previous systematic reviews highlight the advantages of MBPs to treat depression in various populations (Hofmann et al. 2010) and to prevent relapse (Kuyken et al. 2016; Lu 2015). MBPs have been found to be equally as effective as evidence-based treatments for psychiatric disorders, such as Cognitive Behavioral Therapy (CBT) and antidepressants in a recent large review including 142 studies (Goldberg et al. 2018). A number of different MBPs with the aim of treating psychological problems such as depression, anxiety and stress exist.

In the western world, Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn 1982; 2013) and Mindfulness-Based Cognitive Therapy (MBCT; Segal et al. 2002) are among the most commonly employed MBPs with a standardized length of eight weekly 2-2.5-hour group sessions with one all-day retreat and daily homework practices of 40-60 minutes. Standardized MBSR and MBCT programs have generally found beneficial effects. For instance, in clinical populations, in recent reviews, significant improvements from participating in MBSR and MBCT programs have been identified for mental and physical health problems as well as quality of life (e.g. Gotink et al. 2015; Strauss et al. 2014). Furthermore, in non-clinical populations, mindfulness has been associated with positive changes in burnout, stress, anxiety, depression and empathy (e.g. Khoury et al. 2015; Lamothe et al. 2016). However, MBCT and MBSR, with their scheduled in-class as well as daily home practices, can be time-consuming programs and although they have been found to be beneficial for some populations, the question arises whether they may not be suitable for all and can at times even be counterproductive for some (Dobkin et al. 2012). This has for instance been suggested by the relatively high levels of attrition in these MBPs. For instance, Kabat-Zinn and Chapman-Waldrop (1988) observed a 24% dropout rate in MBSR programs and previous research mentioned participant attrition of up to 17% in MBCT programs (Kuyken et al. 2008; Ma and Teasdale 2004; Teasdale et al. 2000) with participants withdrawing from the program before having received what is considered an adequate dose of mindfulness (Crane and Williams 2010). Reasons for dropping out of MBPs are often cited as difficulties in adhering to the expected time involvement warranted in MBSR and MBCT not only during the sessions, but also when completing daily home

practices (e.g. Chang et al. 2004; Shapiro et al. 2005). In addition, recent qualitative studies have explored barriers associated with practicing mindfulness. For instance, in a recent qualitative study with healthcare staff who have engaged in a self-help MBP, the large time commitment of longer mindfulness practices as well as self-criticism associated with not being able to fully engage with the recommended mindfulness practices were identified as the key barriers of engaging with mindfulness (Banerjee et al. 2017). For example, a participant in Banerjee et al.'s (2017) research stated that "had there been less number of things (practices), I might have continued practice" (p.1658, lines 20-21). Additionally, challenging thoughts and feelings were often found difficult to be present with during mindfulness practice (Lomas et al. 2015). Therefore, participants often preferred shorter mindfulness practices (Boggs et al. 2014).

To address issues such as time constraints when participating in standardized MBPs, previous research has examined potential benefits of lower dose MBSR and MBCT programs. For instance, Klatt et al. (2009) discovered that a shortened MBSR program with reduced session time and abbreviated daily home practices, adapted to suit healthy working adults, reduced perceived stress and improved mindfulness with a relatively high rate of adherence. In the program evaluation, participants rated the reduced time commitment required, both in class as well as for recommended home practices, as the most useful aspects of the program (Klatt et al. 2009). Additionally, apart from standardized MBSR and MBCT, in recent years, different MBPs have been developed. These MBPs not only differ in program length or amount of home practices but also in mode of delivery, with an increasing number of MBPs being delivered by means of self-help materials (Hazlett-Stevens and Oren 2017) and online (e.g. Kvillemo et al. 2016). These are often delivered in lower doses than MBSR and MBCT. Previous reviews have found these low-dose, brief MBPs to be beneficial for a number of outcomes, including improving mental health (e.g. Schumer et al. 2018; Spijkerman et al. 2016) and self-regulation (e.g. Leyland et al. 2019). Even engaging with brief mindfulness exercises as stand-alone programs without introductory or discussion elements as part of a therapeutic framework have been found helpful in reducing anxiety and depression compared to controls in a recent meta-analysis (Blanck et al. 2018). The evidence for the effects for brief MBPs is strongest for the general population. For instance, in a recent meta-analysis of studies with working adult participants, findings suggested briefer versions of MBPs to be equally as effective as their higher dose counterparts for psychological distress (Virgili 2015). Nonetheless, brief mindfulness programs have also been found valuable for populations with low levels of mental wellbeing, for instance for individuals with acute depression (Costa and Barnhofer 2016). Additionally, in previous research of a brief MBP with university students, who were mostly novice mindfulness practitioners, it was discovered that participants

preferred engaging in shorter mindfulness practices since no difference in actual time spent practicing was found for participants who were asked to practice mindfulness at home for different lengths of time (Berghoff et al. 2017).

Given the variation in MBPs, there are a number of different ways in which the "dose" of program offered and/or received differs between MBPs. More specifically, MBPs vary in terms of how much mindfulness practice they recommend, the extent to which participants engage with this practice, how much face-to-face contact with a mindfulness teacher they provide, how many sessions participants receive, their proximity and duration as well as the total length of the MBP. In this paper, these will all be considered aspects of the dose of an MBP. In MBPs, a general consensus exists that more mindfulness instruction and mindfulness practice (i.e. a greater dose) are likely to be associated with better outcomes since these are generally viewed as significant components of MBPs (Beblo and Schulte 2017; Crane et al. 2014). According to the developer of MBSR, Kabat-Zinn (1990), mindfulness is like a muscle that needs to be exercised through continuous practice. Furthermore, one prediction theories of mindfulness and rumination generally share in common is the idea that greater dose relating to MBPs is associated with greater response in psychological outcomes. For instance, according to Modes of Mind theory, more practice of mindfulness should strengthen individuals' ability to disengage from the doing mode of mind and to switch into a being mode, thus reducing rumination and in turn depression (Williams 2008a). To examine the veracity of this prediction, empirical work on dose-response relationships in MBPs needs to be examined. It is also possible that such research might reveal optimal doses relating to MBPs for specific populations, for instance for individuals suffering from depression.

There may be discomfort for some in the community of mental health practitioners to refer to psychological programs in terms of dose. However, arguably, there is currently no better terminology to describe the amount of a program offered or received. Although dose does seem to be mostly a medical and pharmaceutical term, for instance to determine the specific quantity or combination of medications leading to a desired outcome (e.g. Dekker et al. 2005), perhaps it could be of value to extend the understanding of dose-response to psychological programs, a start of which has already been made for programs aside from MBPs. For instance, significant dose-response relationships were found between intensity (number of sessions a week) of psychotherapy interventions and depression outcomes (Cuijpers et al. 2013) as well as patient attendance and adherence to homework for CBT for anxiety (Glenn et al. 2013). In mindfulness research however, the above-mentioned difference in doses and delivery methods has led to considerable variations in MBPs offered finding

mixed or inconclusive results with regards to the relationship between dose and effectiveness (Niklicek and Kuijpers 2008).

On the one hand, in a study with chronic pain patients, greater levels of compliance to formal home meditation practices were shown to relate to improved levels of psychological and health-related symptoms (Rosenzweig et al. 2010). Similarly, in a study with adults with different mental health difficulties, time spent completing formal meditation exercises was significantly associated with increasing levels of mindfulness, which in turn predicted a decline in symptoms and enhanced wellbeing (Carmody and Baer 2008), and amount of home practice in MBSR and MBCT participants showed a significant, if small, association with program outcomes in a robust meta-analysis of 43 MBCT and MBSR trials (Parsons et al. 2017). In contrast, a review comparing traditional and adapted (shorter) MBSR programs found no significant relationship between length of sessions and mental health outcomes (Carmody and Baer 2009). Similarly, Jain et al. (2007) did not find a significant relationship between the total number of hours of mindfulness practiced and changes in psychological distress and rumination in university students, and in a previous randomized controlled trial examining the effectiveness of MBCT in preventing relapse of depression, although relapse occurred later in the MBCT group, this was not correlated with amount of practice (Bondolfi et al. 2010). Furthermore, in a review of home practices in MBPs, only about 50% of reviewed studies found a relationship between amount of home practice dose and outcomes with half of included studies finding no such association (Vettese et al. 2009). Reasons as to why previous research contains an inconsistent pattern of findings could be measurement difficulties, risk of bias, and low power in some studies given that some of the observed effects (e.g. Parsons et al. 2017) are small. Secondary research techniques such as meta-regression have the potential to overcome at least some of these limitations by aggregating data from across studies and examining whether factors such as the quality of measurement of dose moderate dose-response relationships in MBPs.

Although the above-mentioned meta-analysis by Parsons et al. (2017) has made a start in examining the quantitative synthesis of dose-response effects across MBP studies, only one aspect of dose relating to MBPs was examined, namely amount of home practice, but not other aspects of doses relating to MBPs, such as amount of contact with the mindfulness teacher, length of the program, intensity of the program, length of sessions, etc. Additionally, the meta-analysis by Parsons et al. (2017) only included MBCT and MBSR programs, but not further adaptations of MBPs such as online or self-help MBPs, which do not strictly follow the MBSR and MBCT guidelines. Therefore, while Parsons et al.'s (2017) findings provide valuable information about dose-response relationships with respect to the amount of mindfulness practiced in the

context of MBSR/MBCT, their findings are not generalizable across different MBPs and other doses relating to MBPs. Since there is currently no previous research in mindfulness which has comprehensively examined dose-response relationships for different types and doses of MBPs, this needs to be addressed (Creswell 2017; Thomas 2017).

Therefore, the current paper presents a comprehensive dose-response meta-regression of randomized controlled trials of MBPs that aimed to examine whether MBPs show dose-response relationships for doses including program length, number and duration of face-to-face contact, amount of recommended mindfulness practice, recommended and actual use of the program, and intensity of the MBP, and responses including the primary outcome of depression, and secondary outcomes of anxiety, stress and mindfulness. Depression was selected as the primary outcome due to the strong evidence-base of MBPs for depression (e.g. Lu 2015). It was hypothesized that greater doses would be predictive of better outcomes. Furthermore, if/when a dose-response relationship was found, it was planned to identify moderators of this relationship with regards to study characteristics including (1) the population receiving the program (clinical vs. non-clinical), (2) the type of program (MBCT/MBSR or close variant [as these are the MBPs with the strongest research evidence for effectiveness] vs. other MBPs) and (3) study quality (potential risk of bias).

Method

Definition of MBPs

Included programs were grounded in mindfulness principles and practices following the definition of MBPs outlined by Crane et al. (2017). In line with this definition, programs could include a range of different mindfulness practices or consist of a single practice only. Program structure, length and frequency of sessions and home practices could also be adapted to best suit the target population and context. Increasingly, solely digital and other self-help delivery methods of mindfulness practices are of interest. These are covered by Crane et al.'s (2017) definition and so are included here. Although programs such as Dialectical Behavior Therapy (DBT) and Compassion-Focused Therapy (CFT) may include some practices which are informed by mindfulness, these programs were not included, since mindfulness meditation is not considered as central and what these programs mainly focus on (Crane et al. 2017). Mindfulness practice was defined in this review as engaging with all aspects of a mindfulness-based program, including formal mindfulness practice completed in-sessions (where applicable) and at home, as well as engagement with related learning activities (e.g. noticing present moment experiences of pleasant and unpleasant events).

Eligibility Criteria

Included studies needed to (1) be randomized controlled trials (RCTs); (2) be in the English language; (3) be published in a peer-reviewed journal or registered on a clinical trials registration site; (4) contain a mindfulness-based program for adults with a majority of the program content focused on mindfulness principles and practice as detailed above; (5) be a program which involves more than one session; (6) include a quantitative measure of depression as an outcome measure; and (7) include either an inactive or active control group (with inactive controls being the primary comparison group) so long as the control group did not practice mindfulness. According to the Cochrane handbook for systematic reviews of interventions (Higgins and Green 2008) inactive control groups either receive no intervention, are waitlist controls or receive care as usual, whereas active control groups can include a variation of the intervention group or a different intervention. Papers were excluded if (1) they were a single-session laboratory experiment examining effects of mindfulness practice rather than testing a program; or (2) they included the same data as another included paper.

Search Strategy

The following sources were searched to identify studies: (1) the electronic databases PsycInfo, Web of Science, MEDLINE and CINAHL; (2) clinical trials registration sites, including clinicaltrials.gov (USA) and ISRCTN Registry (international); and (3) reference lists from included papers as well as current systematic reviews and meta-analyses. The terms [(mindful* OR MBCT OR MBSR) AND (random* OR RCT)] were searched for in titles, abstracts and keywords since the inception of the databases and registration sites up until and including June 19th, 2019.

Study Selection

At a first stage, titles, abstracts and full-texts were screened to determine whether the study involved an MBP and was an RCT. For the second stage, full-texts of retained papers were screened to determine whether a measure of depression was employed. Titles and abstracts of all identified studies were screened by SS with ineligible studies being excluded. As a reliability check, a sample of full-texts of 100 eligible papers were screened by FJ, CS and KC against inclusion/exclusion criteria. The level of agreement was sufficiently high (>90%) and remaining full-text papers were screened by SS with any areas of uncertainty resolved through discussion.

Data Extraction

Where available in the published articles, post-program and follow-up means and standard deviations were extracted for each outcome (depression, anxiety, stress, mindfulness) for each condition (MBP and control

group(s)) as well as numbers of participants in each group. Dose-response effects could be influenced by control condition type (active versus inactive), given that trials with active controls are likely to show smaller effects than comparable trials with inactive controls (Karlsson and Bergmark 2014). Therefore, if a study included more than one control group, both control groups were selected, and inactive and active control groups were examined in separate analyses. If a study included an inactive and active control group, each study was only added and counted once in each separate meta-regression.

Extraction and Calculation of Doses and Study Characteristics

In line with the pre-specified Prospero statement, information about the dose of the program was extracted and incorporated in the analysis as separate primary dose variables, which included "total number of face-to-face sessions," "duration of a face-to-face session (in hours)," "program length (in weeks,)" "frequency of recommended mindfulness practice" (number of practice sessions recommended per week) and "duration of a recommend practice (in minutes)" (where reported). Although it was originally planned to analyze different types of mindfulness practices, this proved difficult to extract reliably from published papers since this information was generally not clearly enough specified and was therefore not included as a dose variable. In addition to primary dose variables, separate theoretically derived composite dose variables were calculated; these being the "total amount of facilitator contact" (in hours; both including and excluding zero hours of contact for online and self-help programs), "total recommended use of the program" (in hours; based on attending all sessions plus completing all recommended home practices), "total actual use of the program" (in hours; based on actual session attendance plus actual home practice) and "program intensity", calculated as the number of sessions a week (both when excluding and including any all-day retreats). In addition, to ensure that intensity of programs was sufficiently measured, the above dose variables facilitator contact (both when including and excluding zero hours), recommended use of the program and actual use of the program were divided by weeks of the program and added as separate dose variables. An inclusive position was taken on practice since in-session work and home practice would frequently be a combination of formal mindfulness practice, related exercises and discussion about/reflection on practice. From the data available, it was not possible to separate out these different elements. Therefore, the recommended and actual use of the program doses considered these together. Strengths and limitations of the approach for calculation of dose variables are addressed in the discussion below.

In addition, study characteristics such as participant population and program type were extracted. Information on sample characteristics such as age, gender and country where the study was set in were also

extracted. Where there was lack of clarity, insufficient data or incomplete study and participant characteristics reported in the published article, authors were contacted via email to ask for the data and information.

Quality Assessment

The quality of all included studies was rated against the seven criteria of the Cochrane Collaboration risk of bias tool (Higgins et al. 2011) using the Review Manager (RevMan) software version 5.2 (The Cochrane Collaboration 2012). This tool assesses selection bias (random sequence generation and allocation concealment), performance bias, detection bias, attrition bias, reporting bias and other bias.

Memory and social desirability bias have previously been identified as problematic when recording home practice (Lacaille et al. 2018). Therefore, a quality rating tool was developed in similar style to the Cochrane Risk of Bias tool, to judge the risk of memory and social desirability bias in papers' reporting of actual practice. For memory bias, daily practice recording was taken to indicate low risk; weekly practice recording medium risk; and retrospective recording of mindfulness practice collected at the end of the program high risk. For social desirability, if practice amount was collected anonymously, this was judged as low risk; where practice amount was collected by a member of the research team who was not the instructor of the session, this was judged as medium risk; and where the instructor themselves collected practice records, this was judged as high risk. Where information on memory and social desirability was unclear, this was judged as high risk. Only studies where actual practice data were available, either through the published paper or from study authors, were assessed using the actual practice quality rating tool. To perform a reliability check, a random 20% of papers were rated independently on both tools by FJ and subject to inter-rater reliability analysis.

Meta-Analysis and Meta-Regression Analysis

Prior to dose-response meta-regression analyses, univariate meta-analyses were conducted for each of the outcomes (depression, anxiety, stress and mindfulness) at post-program and follow-up time points, compared to inactive and active control groups. For this, the packages "metafor," "meta" and "ggplot" (Viechtbauer 2010; Schwarzer 2007; Wickham 2009, respectively) of the R statistical software version 3.4.2. "Short Summer" (The R Foundation for Statistical Computing 2017) were employed. Standardized between group effect sizes (SMDs) were analyzed using a random effects model. Publication bias was assessed by checking funnel plots and employing Trim and Fill analysis to augment missing studies for a more symmetric funnel plot (Viechtbauer 2010).

To determine dose-response relationships, meta-regressions were conducted using between-group effect sizes employing the same packages as above. So long as there were sufficient numbers of studies, separate

meta-regression analyses were conducted for each combination of outcome (depression – the primary outcome, anxiety, stress and mindfulness), time-point (post-program, 1-4 months follow-up and 5-10 months follow-up), and control type (inactive and active). The Knapp and Hartung (2003) adjustment of standard errors of the estimated coefficients in meta-regression models was employed to account for the uncertainty of tau^2 (τ^2). The standardized mean difference was automatically corrected for its slight positive bias within the R functions used (Hedges and Olkin 1985; Viechtbauer 2010). Random effects models were used since populations differed and effect sizes were expected to vary across programs and across control groups (Borenstein et al. 2009). Heterogeneity statistics were also calculated to examine variability in effect sizes across included studies. To correct for inflation of family-wise alpha due to multiple comparisons, the Holm-Bonferroni sequential rejective multiple test procedure was employed (Holm 1979) with the multiple dose variables as the number of comparisons (n=15) at every time-point for every outcome separately. However, Bonferroni-type corrections have previously been criticized for being too stringent in particular where there is a large number of analyses (Diz et al. 2011), which is the case here due to the large number of dose variables assessed. Therefore, in addition to the Holm-Bonferroni correction, False Discovery Rate control was also employed using the Benjamini-Hochberg procedure (Benjamini and Hochberg 1997; Glickmann et al. 2014). Advantages and disadvantages of this approach are considered in the discussion. Both uncorrected as well as corrected results with both methods outlined above are reported, as is generally recommended (Clark-Carter 1997). According to the Cochrane handbook (Higgins and Green 2008), meta-regression analysis should only be undertaken if there are at least ten studies for each study-level variable. A smaller number of studies has only been viewed as feasible if there is a similar sample size in each included study (Fu et al. 2010), which was not the case here as sample sizes varied considerably. No significant dose-response relationships could be observed for any of the outcomes at five to ten months follow-up. For purposes of brevity, these data are not reported, however, these are available on request from the corresponding author.

For significant dose-response relationships, the meta-regression was repeated with baseline levels of the outcome (response) variable controlled for, to determine whether significant dose-response relationships were confounded by baseline differences. Baseline scores, collapsed across a studies' MBP and control group, were calculated and standardized in line with normative data published for each different measure employed (details on normative data used are available on request from the corresponding author).

Moderator/Subgroup Analyses

The metafor, meta and ggplot packages were also employed to examine moderator effects for significant dose-response relationships. Four moderators were planned to be examined; firstly, the categorical moderator population group with the categories "depression," "other mental health condition," "long-term physical health condition" and "general population"; secondly, the bivariate moderator of "MBCT/MBSR or close variant" vs. "other MBPs"; thirdly, a moderator of study quality based on risk of bias scores divided into low and high-risk; and fourth, a moderator of studies' actual practice quality rating scores for significant results with the actual use of the program doses. Significant interaction analyses were planned to be followed-up with subgroup analyses. Analyses using population group as a moderator were not possible to conduct due to insufficient data available (k<10). However, the meta-regression analyses were repeated only with studies that had included a depression population (study samples were defined as being from a depression population if participants had been selected by means of a diagnostic interview or scored above a certain threshold on depression measures indicating a level of depression) as well as with a general population sub-sample (defined as individuals who were not known to have a diagnosis of a mental or physical health condition) compared to inactive controls at post-program.

Additional Dose-Response Meta-Regression Analyses by Outcome Measures

There is a precedent in the literature for including different measures for depression, anxiety, stress and mindfulness in meta-analyses (e.g. Blanck et al. 2018; Gu et a. 2015; Khoury et al. 2015; Spijkerman et al. 2016). Therefore, the meta-analysis and meta-regression analyses pooled across different measures of the same constructs. However, one potential concern from previous research was that different measures may not necessarily be measuring exactly the same constructs (Fried 2017). Therefore, in order to determine whether the inclusion of a number of different measures had any influence on the results, in addition to the main analysis, separate dose-response meta-regression analyses were repeated on a measure-by-measure basis where a sufficient number of studies were available (k>10).

Results

Selection and Inclusion of Studies

After removing duplicates, 203 of the identified studies met the relevant criteria and were included in this dose-response meta-regression analysis. Figure 1 illustrates the stages of screening with numbers and reasons for exclusion of articles in the PRISMA flow diagram (Moher et al. 2009). No additional studies were identified from reference lists of included studies or published reviews. Four of the 203 studies incorporated two different participant groups, namely Kubo et al. (2019), who included cancer patients and caregivers; Schellekens et al.

(2009), who included patient and partner participants; Williams et al. (2008b), who included participant groups with unipolar and bipolar disorders; and Zautra et al. (2008), who included participants with and without depression. Rather than combining the data for these groups, participants with different conditions were included in the analyses separately, leading to a total of 207 separate participant groups to be analyzed (see Supplementary Material 1 in the Supplementary Materials for the reference list of included studies).

---**Fig. 1** Prisma flow diagram ---

Details and Doses of Included Studies

Please see the Supplementary Materials for full details of included studies (Table 2.1, Supplementary Material 2), their primary doses (Table 2.2, Supplementary Material 2) and their composite doses (Table 2.3, Supplementary Material 2). Overall, a total of 15,971 participants were included in the analyses with the number of participants randomized per study ranging from 16 to 476. There were *k*=30 studies where participants had no facilitator contact. These studies were delivered online or via other self-help formats, such as bibliotherapy.

Table 1 presents descriptive statistics for dose variables relating to MBPs taken across the included studies. As can be seen from Table 1, the majority of dose variables ranged noticeably with large differences between the minimum and maximum scores (also see Supplementary Material 3, Fig. 3.1 for histograms that illustrate the distribution for each dose). Variability of dose variables is considered further in the discussion below. For the majority of studies, sufficient information was available to calculate doses. A number of studies (*k*=20) did not report on recommended home practice. The actual use of the program and actual use of the program a week doses could only be calculated for *k*=56 studies. In correspondence with authors, the majority confirmed that information for these doses was not collected as part of their study.

--Table 1 Descriptive statistics of dose variables--

Quality Assessment

Figure 2 shows the judgements for each of the seven risk of bias domains across all studies. The quality of studies ranged considerably with only five studies meeting all the criteria for low risk of bias.

---**Fig. 2** Risk of bias graph ---

As described above, 20% of studies (*k*=41) were independently rated and subject to inter-rater reliability analysis with Cohen's kappa. Cohen's kappa was 0.92. According to McHugh (2012) a Cohen's kappa between .81 and 1 represents almost perfect agreement. Table 2.4 in Supplementary Material 2 shows the actual practice quality rating tool with scores for memory and social desirability bias as well as total tool rating for each study where these data were available. Again, 20% of studies (*k*=11) were independently rated with an

overall Cohen's kappa of .91 (memory bias: kappa=1; social desirability: kappa=.82). Scores ranged considerably with the majority of studies showing high risk of social desirability bias. Whether study quality and quality of practice reporting predicted results was addressed with moderator analyses, which is reported below.

Meta-Analysis Results

Meta-analyses prior to including dose variables showed significant medium to large between-group differences favoring the MBP group, for all outcomes compared to inactive controls at post-program and follow-up time points, where enough data were available. Compared to active controls, significant between-group differences were observed for all outcomes at post-program and for depression, anxiety and mindfulness at 1-4 months follow-up (where $k \geq 10$), with small to large effect sizes (see Table 2 for details). Asymmetric funnel plots suggested evidence of publication bias for depression, anxiety and stress outcomes, with trim-and-fill analysis resulting in slightly modified effect size estimates. However, the statistical significance and direction of effect size estimates remained unaltered (results of trim-and-fill analyses are included in Table 2, where applicable). Forrest as well as funnel plots based on the trim and fill method for meta-analysis are available on request from the corresponding author.

--Table 2 Meta-analysis results including trim-and-fill analyses—

Dose-Response Meta-Regression: Primary Outcome - Depression

For depression as the primary outcome at immediately post-program, neither primary nor composite dose variables significantly predicted effect sizes, for inactive or active controls (see Table 3 for results of the dose-response meta-regression analysis for depression compared to inactive results; see Supplementary Material 3, Fig. 3.2 for meta-regression plots and Table 2.5, Supplementary Material 2 for results compared to active controls). Plots at follow-up for the depression outcome as well as plots for non-significant results for secondary outcomes are available on request from the corresponding author.

--Table 3 Meta-regression results, depression (inactive controls), post-program--

At one to four months follow-up, there was a significant dose-response relationship between the dose duration of a recommended home practice and depression ($k = 38$; t =2.171; d =0.014; C.I.: [0.001, 0.027]; F(1, 36) =4.713; $p = 0.037$) indicating that being asked to practice mindfulness for longer predicted *increased* depression compared to inactive controls with a small effect size (see Table 2.6, Supplementary Material 2 for all depression results at 1-4 months follow-up). This finding is in the opposite direction to that hypothesized since this dose was associated with increased, as opposed to decreased, depression. Figure 3 shows the meta-regression plot for duration of recommended home practice and depression at one to four months follow up.

--**Fig. 3** Duration of a recommended home practice, depression (inactive controls), 1-4 months follow-up--

This finding did not remain significant when applying the two corrections for multiple-comparisons and when controlling for baseline depression. Therefore, this result does not appear particularly robust and needs to be interpreted with caution. No significant interaction effects were observed for either of the moderators type of program and study quality. To ascertain if the general lack of evidence for a dose response relationship for the depression outcome was due to different populations being included here, the analyses with both primary and composite dose variables were repeated with only the *k*=27 studies that had included individuals with depression as their participants. Secondly, the dose-response meta-regressions were also repeated with only the *k*=50 studies with participants from the general population. Both were compared to inactive controls. No significant dose-response relationships were observed for neither the depression population nor the general population (results from these meta-regression analyses are available on request from the corresponding author). These results also did not differ when controlling for the effects of different severities of depression at baseline (mild and severe).

Dose-Response Meta-Regression: Secondary Outcomes

Anxiety

For the anxiety outcome, similar to depression, neither primary nor composite dose variables significantly predicted effect sizes, for inactive or active controls at immediately post-program (see Table 2.7, Supplementary Material 2). At one to four months follow-up, a significant dose-response relationship between the dose program intensity (when including all-day retreats) (*k* =20; t =-3.768; *d* =-1.626; C.I.: [-2.533,-0.72]; F(1, 18) = 14.199; *p* =0.001) and the anxiety outcome compared to active controls was observed in the hypothesized direction (see Figure 4 for the meta-regression plot). This result remained significant when controlling for baseline anxiety and when applying the Holm-Bonferroni and False Discovery Rate corrections. However, this result was influenced by a single study with a substantially higher intensity score. When removing this study from the model, this finding was no longer significant.

--**Fig. 4** Program intensity (incl. retreats), anxiety (active controls), 1-4 months follow-up--

Where enough studies were available to conduct moderation analyses, no significant interaction effects for moderators were observed. For all the remaining combinations of dose, time-point and control group type, no significant dose-response relationships were found in the anxiety data (see Table 2.8, Supplementary Material 2 for results at 1-4 months follow-up).

Stress

Again, similarly to depression and anxiety outcomes, no significant dose-response relationships were observed for the stress outcome for either inactive or active controls at immediately post-program (see Table 2.9, Supplementary Material 2). At one to four months follow-up compared to inactive controls, a significant dose-response relationship between the dose duration of a face-to-face session (k =14; t = 2.334; d =0.823; C.I.: [0.055, 1.591]; F(1, 12) =5.45; p =0.038) and the stress outcome was observed; however, this was not in the hypothesized direction (see Figure 5 for the meta-regression plot).

---**Fig. 5** Duration of a face-to-face session, stress (inactive controls), 1-4 months follow-up---

Compared to active controls at one to four months follow-up, three doses significantly predicted effect sizes of stress. Specifically, there were significant dose-response relationships between the doses total amount of face-to-face facilitator contact (k =11; t = -2.743; d =-0.018; C.I.: [-0.033, -0.003]; F(1, 9) = 7.525; p =0.023) and total amount of face-to-face facilitator contact (excluding studies with no contact) (k =10; t = -2.547; d = -0.021; C.I.: [-0.04, -0.002]; F(1, 8) = 6.486; p =0.034) and the stress outcome, both in hypothesized directions (see Figures 6 and 7 for the meta-regression plots).

---**Fig. 6** Facilitator contact, stress (active controls), 1-4 months follow-up---

---**Fig. 7** Facilitator contact (excl. no contact), stress (active controls), 1-4 months follow-up---

Additionally, a significant dose-response relationship between the dose program intensity (including all-day retreats) (k =10; t = 4.031; d =0.208; C.I.: [0.089, 0.326]; F(1, 8) = 16.252; p =0.004) and stress in the opposite direction to that hypothesized was observed (see Figure 8 for meta-regression plot).

---**Fig. 8** Program intensity (incl. retreats), stress (active controls), 1-4 months follow-up---

However, this result needs to be interpreted with caution since this finding appears to be due to one study with higher intensity than other included studies and disappears when that study is excluded. All significant dose-response relationships found at one to four months follow-up for the stress outcome were no longer significant when controlling for baseline stress and when applying the Holm-Bonferroni and False Discovery Rate corrections for multiple comparisons. These results therefore do not appear particularly robust and need to be interpreted with caution. No significant interaction effects with any of the moderators were observed where enough studies were available. Please see Table 2.10 in the Supplementary Material 2 for the results of the stress outcome at one to four months follow-up.

Mindfulness

At immediately post-program, significant dose-response relationships between a number of different doses associated with actual program use, facilitator contact and program intensity and the response mindfulness were

found in the hypothesized direction (see Table 4 for results compared to inactive controls and Table 2.11 in Supplementary Material 2 compared to active controls).

---Table 4 Meta-regression results mindfulness (inactive controls), post-program---

Specifically, significant dose-response relationships in the hypothesized direction between the dose total actual use of the program and mindfulness compared to both inactive controls and active controls at post-program were found with small effect sizes. (inactive controls: see Table 4; active controls: k =11; t = 3.055; d =0.015; C.I.: [0.004, 0.026]; F(1, 9) = 9.33; p =0.014). Figures 9 and 10 show the meta-regression plots.

---**Fig. 9** Total actual use of program, mindfulness (inactive controls), post-program---

---**Fig. 10** Total actual use of program, mindfulness (active controls), post-program ---

Additionally, program intensity when excluding all day retreats as well as program intensity when including all-day retreats significantly predicted increased mindfulness at post-program with large effect sizes in the MBP compared to the inactive control group (see Table 4). Figures 11 and 12 show the meta-regression plots for both.

---**Fig. 11** Program intensity (excl. retreats), mindfulness (inactive controls) post-program ---

---**Fig. 12** Program intensity (incl. retreats), mindfulness (inactive controls) post-program ---

Next, a significant dose-response relationship between amount of face-to-face facilitator contact per week, both when including and excluding zero hours of contact, and the mindfulness outcome were observed in the expected direction at post-program compared to inactive controls, with small to medium effect sizes (see Table 4). Figures 13 and 14 show the meta-regression plots of both models.

---**Fig. 13** Facilitator contact, mindfulness (inactive controls), post-program ---

---**Fig. 14** Facilitator contact (excl. no contact), mindfulness (inactive controls), post-program ---

As can be seen from Table 4, dose-response effects were close to being significant (.1>p>.05) for other composite doses (facilitator contact (with and without the inclusion of no contact) and recommended use of the program a week) thus increasing the likelihood of genuine effects having occurred.

At one to four months follow-up compared to active controls, a significant dose-response relationship was observed between the dose program intensity when including all-day retreats and mindfulness with a small effect size (k = 12; t =; d =-0.219; C.I.: [-0.41, -0.027]; F(1, 10) =6.495; p = 0.029) indicating lower mindfulness after more intense MBPs at this timepoint (see Figure 15 for meta-regression plot). However, this result needs to be interpreted with caution since this finding appeared to be due to one study with higher intensity than other included studies with the effect disappearing when this study was removed. No further significant dose-response

relationships were found for the mindfulness outcome at one to four months follow-up (Table 2.12, Supplementary Material 2).

---**Fig. 15** Program intensity (incl retreats), mindfulness (active controls),1-4 months follow-up, ---

Baseline levels of mindfulness could not reliably be controlled for since baseline data were not available for over a third of studies. When applying the Holm-Bonferroni correction, results only remained significant for the program intensity (including retreats) dose at immediately post-program compared to inactive controls. However, when applying the False Discovery Rate control, all dose-response relationships aside from the actual use of the program dose remained significant.

For the dose amount of facilitator contact (excluding no contact), there was a significant interaction effect with program type (i.e. MBP type x amount of facilitator contact (excl. no contact)*;* $k = 50$; t = 2.549; $d = 0.358$; C.I.:[0.075, 0.64]; F(3, 46) = 5.15; $p = 0.014$). Subgroup analyses showed that the amount of facilitator contact (excl. no contact) dose only significantly predicted mindfulness for MBPs that were not traditional MBSR or MBCT programs (non-MBSR/CT: $k = 17$; t = 3.83; $d = 0.394$; C.I.: [0.175, 0.612]; F(1, 15) = 14.679; $p = 0.002$). However, this may be due to the fact that MBSR and MBCT programs generally do not differ in amount of facilitator contact due to following stricter guidelines (Kabat-Zinn 1982; Segal et al. 2002), whereas other types of MBPs can vary substantially in amount of facilitator contact. For the dose program intensity, a significant interaction effect was observed between the dose and study quality (i.e. study quality x program intensity; $k = 50$; t = 2.089; $d = 0.51$; C.I.:[0.019, 1]; F(3, 46) = 4.878; $p = 0.042$). When dividing included studies into two groups (higher and lower risk of bias), the program intensity dose only significantly predicted mindfulness in low quality (high risk of bias) studies ($k = 30$; t = 3.083; $d = 1.457$; C.I.: [0.489, 2.424]; F(1, 28) = 9.502; $p = 0.005$). The significant dose-response relationship with the program intensity dose therefore needs to be interpreted with caution since significant results are only found in low quality, high risk of bias studies. No significant moderator effects were found for the remaining significant dose predictors including for the moderator actual practice quality recording.

Dose-Response Meta-Regression Results by Outcome Measures

As outlined in the methods above, additional meta-regression analyses were conducted on a measure-by-measure basis. Findings did not contradict the results for the main analyses for anxiety, stress and mindfulness. For depression, the vast majority of additional analyses did not find different results to the main analysis; and while there were occasional significant findings (six out of 120 analyses) in the opposite direction to hypothesis, these should be treated with caution due to the substantially smaller sample sizes for separate measures ($k<1/3$

of the overall sample), the inconsistent pattern of findings and the likelihood of Type I errors. Specifically, if the null hypothesis is correct, at a 5% alpha level we would expect this number of spuriously significant findings (since 5% of 120 is 6), in the absence of any real dose-response relationships. Details of these analyses are available on request from the corresponding author.

Discussion

The aim of this paper was to determine whether a dose-response relationship exists between varying doses relating to MBPs and the outcomes depression, anxiety, stress and mindfulness. Results from meta-analyses suggest that MBPs are generally beneficial compared to controls, a finding which is consistent with previous research (Creswell 2017; Gotink et al. 2015; Keng et al. 2011). Dose-response meta-regressions were conducted to examine potential dose-response relationships.

Dose-Response Meta-Regression Mindfulness Outcome

Firstly, significant dose-response relationships in hypothesized directions were found for the mindfulness outcome. Specifically, larger effect sizes at immediately post-program, relative to inactive controls, were predicted by greater actual use of the program, amount of face-to-face contact and intensity of MBPs (i.e. sessions a week). These findings remained largely significant when applying the False Discovery Rate control but were no longer so when Holm-Bonferroni was applied, which may be due to the large number of tests conducted. Given that the Holm-Bonferroni method has been argued by some to be overly conservative (Diz et al. 2011; Glickman et al. 2014), it seems plausible that these findings reflect true underlying dose-response relationships. If that is indeed the case, then there are theoretical reasons as to why we might expect greater face-to-face contact to be more helpful in relation to learning mindfulness. One could be group processes being helpful for learning a skill such as mindfulness (Segal et al. 2002; Yalom 1983). The group process that is present in many MBPs is thought to be important since it gives a chance for people to ask questions and discuss problems with an experienced mindfulness practitioner and their peers (Kabat-Zinn 2003). According to Kabat-Zinn (2003), mindfulness should therefore not be something to be learned via books only, it needs to be taught by someone who has experienced it; all other materials used in teaching are considered as supplementary in nature (Bruce et al. 2010).

Additionally, it was found that greater actual engagement with the program (both in-sessions and at home) predicted increased mindfulness compared to controls; however, this finding needs to be interpreted with caution since it was no longer significant when controlling for the inflation of family-wise alpha levels, either by

Holm-Bonferroni or False Discovery Rate. However, it is worth noting that the failure to survive these corrections could be a Type II error particularly given that there was a substantially smaller sample size for this dose. Other researchers have found evidence for a relationship between amount of home practice and beneficial outcomes (e.g. Parsons et al. 2017) and if future dose-response meta-regressions with larger sample sizes were to replicate this analysis and finding, this corresponds with previous research and theory advocating for the notion that mindfulness as a skill takes time and commitment to learn (Kabat-Zinn 1990) since greater engagement with the MBP and its practices has been found to strengthen individuals' ability to switch from a doing to a being mode of mind (Williams 2008a).

Regarding significant dose-response relationships found between intensity of MBPs and mindfulness, this finding remained robust when controlling for both multiple comparison methods and therefore does not appear to be due to the inflation of family-wise alpha. However, this finding does need to be interpreted with some caution since it was found to be moderated by study quality within lower quality studies. If however this finding would be substantiated in future research, an explanation of this finding could be that when first designing MBSR, Kabat-Zinn (1982) argued that duration of the mindfulness program and thus the amount of facilitator contact and the intensity of sessions were some of its key elements to be helpful for individuals with chronic pain. Furthermore, a parallel can be drawn to previous dose-response research by Cuijpers et al. (2013), where greater intensity of psychotherapy interventions significantly predicted outcomes with the explanation that the relationship between facilitator and participants, which is necessary for learning to occur, may develop more quickly when delivery of psychotherapy is more intense (i.e. closer proximity between sessions to aid experiential learning), which could also apply to MBPs.

However, despite significant dose-response relationships for contact and intensity doses, (although these may need to be treated with some caution) this was not found for primary doses such as program and session length and frequency and duration of recommended practice. This could be due to the composite variables of amount of contact and intensity being more nuanced to the learning processes mentioned above. The hypothesized dose-response relationships found at post-program did not remain significant at follow-up. On the contrary, greater MBP intensity (including all-day retreats) predicted *decreased* mindfulness compared to active controls at one to four months follow-up, although this effect disappeared when removing a higher-dose study. A possible reason for this finding may be that very intense MBPs may have potentially discouraged individuals, especially those new to mindfulness, to continue practicing after the end of the program.

Dose-Response Meta-Regression Psychological Outcomes

Despite finding significant dose-response relationships for mindfulness, these did not appear to have translated to psychological outcomes, including the primary outcome depression, where no significant dose-response relationships in expected directions were found for doses at post-program. Participants in the included RCTs were mostly novice practitioners with the MBP often being the first time participants had become aware of mindfulness. For instance, participants were often excluded from studies if they had previously practiced mindfulness meditation (e.g. Gambrel and Piercy 2015; Godfrin and van Heeringen 2010). It could well be that different results would be found for a participant group of more experienced mindfulness practitioners. A dose-response effect being found for mindfulness could be due to mindfulness being something that can be increased and learned continuously (Kabat-Zinn 1990), in particular for those new to it. Contrastingly, the majority of participants did not experience clinical levels of ill mental health at baseline which may therefore have resulted in a floor effect since there was not as much variability for improvement for psychological outcomes as there was for mindfulness for those currently not suffering from severe mental health difficulties. A floor effect for psychological outcomes could therefore have made it more difficult to draw out dose effects since changes in depression, anxiety and stress may not have been significantly different enough to baseline levels to highlight dose effects. However, when repeating the analyses only with studies who included participants from a depression population and again from the general population, no significant dose-response effects were observed for either. Additionally, for novice practitioners even smaller doses could have been beneficial in decreasing depression, anxiety and stress with larger doses not being significantly more helpful. This corresponds with previous meta-analyses advocating for the benefits of brief mindfulness programs on outcomes introduced above (Blanck et al. 2018; Schumer et al. 2018). Furthermore, the limited evidence of dose-response relationships found coincides to an extent with Cuijpers' et al. (2013) research also not having found a dose effect between doses such as contact time and duration of psychotherapy interventions and the outcome depression. Contrarily, this result does not correspond with findings of the previous meta-analysis by Parsons et al. (2017) on MBPs introduced above. However, Parsons' meta-analysis only included specific types of MBPs, namely MBSR and MBCT, but not others such as self-help MBPs, which generally employ lower doses. Additionally, an association between amount of practice and outcomes may have been found in this previous review since participant-level variables were examined but not study-level variables (i.e. dose) in different MBPs. It feels important for reviews to be inclusive of different types and dose variations of MBPs due to the recent increase in self-help MBPs finding positive effects (Spijkerman et al. 2016).

One potential explanation for a lack of dose-response relationships involving psychological outcomes found might be the restricted range and variability for some of the dose variables. This does however seem an unlikely explanation since, as could be seen from descriptive statistics of the doses, there was a reasonable range on the majority of dose variables and indeed enough variability on doses with a more restricted range (in particular doses examining program intensity) such that dose-response effects were found for the mindfulness outcome. Another possible reason for no significant dose-response relationships found for psychological outcomes could be the pooling across different outcome measures (Fried 2017). However, this does not appear to be the case here since when separate analyses were conducted, findings of different measures did not greatly contradict pooled findings and any significant results were likely Type I errors.

Although no significant dose-response relationships were observed for dose variables at post-program, doses associated with recommended practice, program intensity and face-to-face contact significantly predicted effect sizes in depression, anxiety and stress at follow-up, albeit at times in unexpected directions. Most notably, larger effect sizes in depression were predicted by longer duration of recommended home practices, which was not in the hypothesized direction. However, none of these findings were particularly robust, as was established through Holm-Bonferroni and False Discovery Rate corrections and controlling for baseline levels. If however future research were to substantiate high durations of recommended practice predicting worse outcomes, one possible explanation might be that if homework demands are too high, there may be a paradoxical effect of participants practicing less, in particular at follow-up. Vettese et al. (2009) found that recommending a certain amount of practice does not necessarily translate to participants engaging in recommended practices, something which is also often not tracked accurately enough. Additionally, difficulties related to the often-large time commitments for practice have previously been associated with the high level of attrition in MBPs (Shapiro et al. 2005) and with compliance with home practice at follow-up (e.g. Dimidjian et al. 2016). This is also supported by previous qualitative research that suggests that lengthy mindfulness practices are perceived as barriers to engagement (Banerjee et al. 2017) and that individuals often prefer shorter practices (Boggs et al. 2014). Furthermore, this finding corresponds to an extent with some previous research finding no association between amount of practice and psychological outcomes (e.g. Bondolfi et al. 2010; Jain et al. 2007; Ribeiro et al. 2018). Next, although a small number of significant dose-response effects were also found for stress and anxiety at follow-up, these were again not robust, were frequently based on small sample sizes and often due to single studies without which the effect disappeared. Substantial caution therefore needs to be exercised with

regards to these particular findings given that they may well be spurious and until they are replicated, these are arguably not worthy of further consideration.

Limitations and Future Research

Small sample sizes were not only an issue at follow-up time points, but also particularly for the actual use of the program dose. Additionally, the high risk of social desirability and memory bias were problematic for this dose, limiting what can be concluded from findings. Future RCTs should therefore aim to reduce risk of potential memory and social desirability bias by adopting low risk methods for actual practice reporting, for instance ensuring regular and anonymous (e.g. computerized) practice recording. Another limitation of this review was that the composite dose variables recommended and actual use of the program were calculated as a combination of in-session and home practice and learning activities. The strength associated with this way of calculating engagement with MBPs is that these doses represented the entirety of different aspects associated with mindfulness practice. However, having employed an inclusive position regarding mindfulness practice has the disadvantage of not knowing how much of this dose was exclusively recommended or actual formal practice as opposed to engagement in other exercises or discussion. For recommended use of the program, this issue was addressed to an extent by having included primary-level doses examining number and duration of recommended home practices, thus assessing practice recommended to be completed outside of sessions only. However again, this still also included exercises other than practice. On the other hand, for actual use of the program, from the data that was available either from published papers or through communication with authors, it was not possible to distinguish the different elements of in-session and home work. Additionally, previous research has emphasized the importance of informal mindfulness practice in daily life (e.g. Langer 2014). However, data on informal mindfulness practice was generally not reported in included studies and dose-effects of amount of informal mindfulness practice could therefore not be examined. In future RCTs, it could be helpful to collect separate data on different types of home practice (formal, informal) and other work completed as well as specifying how much of this work was completed during and outside of sessions. Nevertheless, the aim of this dose was not to examine actual engagement in formal mindfulness practice only, but to ascertain the overall actual use of MBPs. Arguably, all the different practices and exercises connected with MBPs are of potential importance, as is generally the view of experienced mindfulness teachers (e.g. Kabat-Zinn 1990; Segal et al. 2002).

Regarding further limitations with regards to the explorations of dose-response in MBPs possible in this review, although the potential effects of confounding variables have been controlled for as moderators, a

couple of confounding factors could not be examined. For instance, the incidence of adverse events could not be assessed since, as has been acknowledged recently by van Dam et al. (2018), this is currently not measured and reported in studies. Additionally, mindfulness teachers' experience in facilitating face-to-face MBPs was not reported in sufficient detail in included studies in order for this to be assessed. It may therefore be prudent for future research to examine the effects of adverse events as well as mindfulness teachers' previous experience.

Another caution with regards to the interpretation of results is the possibility of Type I and II errors. Type I errors could have occurred for significant effects due to statistical multiple comparisons arising from testing multiple hypotheses for each outcome (Abdi 2010). To address this possibility, both the Holm-Bonferroni (Holm 1979) procedure and the False Discovery Rate control (Benjamini and Hochberg 1997; Glickmann et al. 2014) were employed. As has been outlined in the methods above, the Holm-Bonferroni has been argued to be too stringent (Diz et al. 2011), therefore both corrections were applied and presented here to assess robustness of findings. However, a disadvantage associated with the above family-wise alpha correction procedures is that although the chance of a Type I error reduces, power is reduced further when adopting a correction thus increasing the chance of a Type II error (Nakagawa 2004). To reduce the likelihood of a Type II error, analyses undertaken were only completed where $k \geq 10$ studies per study level variable (Fu et al. 2010) were available. However, analyses containing smaller sample sizes ($k > 9$), still need to be interpreted with caution (Christley 2010). For some analyses with larger sample sizes however, when inspecting meta-regression plots (see Supplementary Material 3, Fig. 3.2), there do not appear to be trends for a relationship; these therefore appear unlikely due to Type II errors. Furthermore, particularly in meta-analytic investigations, the chance of Type I and II errors can be increased if individual studies already contain errors (Kempton et al. 2008). This is a possibility here due to the large number of analyses conducted with multiple doses and moderators with often small sample sizes (Berlin et al. 2002).

Another limitation is that the current meta-regressions included participants from a large number of different populations with different histories of psychological and physical health conditions, which were not possible to identify fully for all included participants. Additionally, for four studies, separate participant groups derived from the same study were included, which risks violating the assumption of equal independence. However, the number of studies to which this applied ($k=4$) was very small compared to the overall number of studies, therefore, this did not seem likely to have a material impact on the results. This was confirmed by repeating the primary meta-analyses with only one sample from each of these four studies included and this made no material difference to the findings. Furthermore, due to the nature of meta-regressions, only average

baseline scores across participants in a study were available (not individual participant data). This limitation has to an extent been addressed by controlling for average baseline levels, comparing studies with participants with mild and severe baseline depression and completing separate analyses including only participants with depression and only participants from the general population. Due to the heterogeneous sample of studies with participants suffering from various long-term physical health conditions, it was not possible to analyze each homogenous population group separately in a reliable meta-regression because of the very small sample sizes. However, according to Kabat-Zinn (2005), there may be a universality to human suffering regardless of its causes with individuals experiencing similar psychological processes such as worry and rumination. Through mindfulness, attention can therefore be brought away from focusing on adversity and support individuals in being in the present moment with a sense of compassion thus providing a universal aid across individuals with different roots for their suffering (Feldman and Kuyken 2011). Nevertheless, it is entirely possible and indeed seems probable that similar processes may result in dissimilar outcomes across different medical and psychological groups and therefore this inability to examine effects across different groups still needs to be noted as a limitation on what can reliably be concluded.

Not only different population groups but also different outcome measures were included in this review. One possible concern is that effects might be obscured if measures assess slightly different constructs (Fried 2017). However, as has been mentioned above, the approach taken here to combine across a range of different measures is typical of meta-analyses in the field (e.g. Blanck et al. 2018; Gu et al. 2015; Khoury et al. 2015; Spijkerman et al. 2016) and the pattern of findings did not materially differ when analyses were repeated on a measure-by-measure basis.

In the future, perhaps a dose-response meta-regression could be repeated once there is more literature, particularly for dose variables where sample sizes were small, and when actual use of the MBP is more reliably reported. Additionally, a dose-response meta-regression analysis for different population groups could be completed once this data is available to support more viable moderator analyses. Exploring common predictors for success in MBPs for individuals of different backgrounds/personalities would also be an interesting area to explore in future meta-analyses to further understand the type and dose of MBP that works best for certain individuals. Furthermore, although possible reasons for the lack of dose-response effects found for psychological outcomes here have been outlined, this is an area that would benefit from further research and possibly theoretical analysis. Finally, since the different doses relating to MBPs were not randomly assigned to

studies, only predictability but not causation can be inferred from significant effects. This needs to be examined in future experimental studies that manipulate dose, which would allow for causal conclusions to be drawn.

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Table 1

Descriptive statistics of dose variables

Dose	*k*	Mean	SD	Min.	Max.
Total no. of face-to-face sessions	203	6.75	3.95	0	30
Duration of a face-to-face session (in hours)	173	2.06	0.89	0.25	8
Program length (in weeks)	203	7.52	2.35	0.36	19
Frequency recommended practice (no. recommended practices/week)	183	6.54	1.06	1	8
Duration of recommended home practice (one practice in minutes)	183	35.92	14.83	6	60
Total amount of face-to-face facilitator contact (in hours)*	203	14.92	10.01	0	60
Total recommended use of the program (in hours)	183	39.2	21.18	1.2	81
Total actual use of the program (in hours)	56	25.68	16.73	1.67	69.96
Program intensity (sessions a week)	173	1.089	0.43	0.25	4
Program intensity (sessions a week) incl. retreats	173	1.12	0.43	0.25	4
Amount of face-to-face facilitator contact a week (in hours)*	203	2.05	1.8	0	18
Recommended use of the program a week (in hours)	183	5.09	2.5	0.23	10.13
Actual use of the program a week (in hours)	56	3.34	2.04	0.7	8.75

Note: k= Number of studies where data was available to calculate this dose; SD=Standard Deviation; Min.=Minimum; Max.=Maximum; *for the contact hours and contact hours/week doses, the analyses were repeated with studies that had zero hours of contact removed.

Table 2

Meta-analysis results including trim-and-fill analyses

Compared to inactive control groups			
Outcome	**Post-program**	**1-4 months follow-up**	**5-10 months follow-up**
Depression	**k=149; d= -0.601; [-0.697, -0.504]*** k_{imp}=23; d_{adj}= -0.733; $_{adj}$[-0.833, -0.633]*****	**k=45; d= -0.824; [-1.095, -0.554]*** k_{imp}=10; d_{adj}= -1.066; $_{adj}$[-1.329, -0.802]*****	**k = 21; d = -0.338; [-0.503, -0.173] *** k_{imp}=4; d_{adj}= -0.448; $_{adj}$[-0.62, -0.275]*****
Anxiety	**k=100; d= - 0.485; [-0.592, -0.379]*** k_{imp}=17; d_{adj}= -0.609; $_{adj}$[-0.717, -0.5]*****	**k=29; d= - 0.621; [-0.936, -0.307]*** k_{imp}=7; d_{adj}= -0.903; $_{adj}$[-1.226, -0.581]*****	**k = 12; d = -0.356; [-0.553, -0.159] *** k_{imp}=2; d_{adj}= -0.412; $_{adj}$[-0.596, -0.228]*****
Stress	**k=51; d= - 0.73; [-0.997, -0.464]*** k_{imp}=16; d_{adj}= -1.041; $_{adj}$[-1.285, -0.797]*****	**k=17; d= - 0.983; [-1.444, -0.522]*****	k<10
Mindfulness	**k = 61; d = 0.511; [0.371, 0.651] *****	**k = 19; d = 0.863; [0.374, 1.353] *****	**k = 10; d = 0.374; [0.138, 0.609] ** k_{imp}=1; d_{adj}= 0.314; $_{adj}$[0.068, 0.56]***
Compared to active control groups			
Outcome	**Post-program**	**1-4 months follow-up**	**5-10 months follow-up**
Depression	**k=84; d= -0.203; [-0.298, -0.107]*** k_{imp}=11; d_{adj}= -0.294; $_{adj}$[-0.394, -0.194]*****	**k=30; d= -0.206; [-0.367, -0.044]*** k_{imp}=9; d_{adj}= -0.362; $_{adj}$[-0.518, -0.206]*****	k=13; d= -0.03; [-0.161, 0.101]
Anxiety	**k=50; d= - 0.155; [-0.258, -0.053]** k_{imp}=8; d_{adj}= -0.247; $_{adj}$[-0.357, -0.137]*****	**k=22; d= - 0.165; [-0.287, -0.043]** k_{imp}=3; d_{adj}= -0.207; $_{adj}$[-0.333, -0.081]****	k<10
Stress	**k=26; d= - 0.325; [-0.612, -0.038]* k_{imp}=7; d_{adj}= -0.561; $_{adj}$[-0.848, -0.274]*****	k=11; d= - 0.019; [-0.175, 0.137] k_{imp}=3; d_{adj}= -0.076; $_{adj}$[-0.222, 0.069]	k<10
Mindfulness	**k = 34; d = 0.214; [0.091, 0.338] *****	**k = 13; d = 0.232; [0.076, 0.387] ****	k<10

Note: k=number of included studies; d=effect size; [] =95% confidence intervals; k_{imp}=number of imputed studies based on trim & fill; d_{adj}=adjusted effect size based on trim & fill; $_{adj}$[]=adjusted 95% confidence intervals based on trim & fill; *** = p < .0001; ** = p < .01; *=p <.05; significant results in bold; k<10 = not enough studies to complete analysis; results of trim-&-fill analysis only added where evidence of publication bias found.

Table 3

Meta-regression analysis results by MBP dose for between-group depression effect sizes at immediately post-program compared to inactive controls

Dose	Meta-regression model								Heterogeneity statistics			
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	***SE tau²***	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.583	0.014	[-0.031, 0.026]	-0.191	0.849	149	0.037	0.00%	0.281	0.042	676.191	<0.001
Duration of a face-to-face session	0.067	0.082	[-0.095, 0.23]	0.819	0.414	126	0.671	0.00%	0.309	0.05	574.858	<0.001
Program length	0.005	0.023	[-0.041, 0.05]	0.198	0.843	149	0.039	0.00%	0.28	0.042	669.856	<0.001
Frequency of recommended practice	0.015	0.046	[-0.076, 0.107]	0.327	0.744	137	0.107	0.00%	0.204	0.034	531.905	<0.001
Duration of a recommended practice	-0.001	0.003	[-0.007, 0.006]	-0.209	0.835	137	0.044	0.00%	0.204	0.034	525.116	<0.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	***SE tau²***	Q_E	$p\ (Q_E)$
Total amount of contact	-0.003	0.005	[-0.013, 0.008]	-0.526	0.6	149	0.277	0.00%	0.28	0.042	672.621	<0.001
Total amount of contact (excl. 0 hours)	0.003	0.007	[-0.011, 0.018]	0.435	0.664	126	0.189	0.00%	0.31	0.05	575.411	<0.001
Total recommended use of program	<-.001	0.002	[-0.005, 0.004]	-0.09	0.928	137	0.008	0.00%	0.204	0.034	530.524	<0.001
Total actual use of program	0.002	0.006	[-0.011, 0.014]	0.291	0.773	32	0.085	0.00%	0.111	0.046	88.666	<0.001
Program intensity excl. retreats	0.066	0.173	[-0.276, 0.408]	0.384	0.701	126	0.148	0.00%	0.311	0.05	572.715	<0.001
Program intensity incl. retreats	0.105	0.169	[-0.228, 0.439]	0.625	0.533	126	0.391	0.00%	0.311	0.05	573.573	<0.001
Amount of contact/week	-0.036	0.035	[-0.106, 0.034]	-1.018	0.31	149	1.036	0.5%	0.276	0.041	661.997	<0.001
Amount of contact (excl. 0 hours)/week	-0.013	0.046	[-0.103, 0.077]	-0.283	0.777	126	0.08	0.00%	0.31	0.05	572.677	<0.001
Recommended use of program/week	-0.005	0.02	[-0.044, 0.034]	-0.248	0.804	137	0.062	0.00%	0.204	0.034	526.608	<0.001
Actual use of program/week	0.02	0.051	[-0.084, 0.124]	0.396	0.695	32	0.157	0.00%	0.111	0.046	89.729	<0.001

Note: d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$) Q_E significance level.

Table 4

Meta-regression analysis results by MBP dose for between-group mindfulness effect sizes at immediately post-program compared to inactive controls

Dose	**Meta-regression model**								**Heterogeneity statistics**			
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	***SE tau²***	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	0.028	0.023	[-0.018, 0.073]	1.213	0.23	61	1.471	0.00%	0.227	0.057	220.258	<0.001
Duration of a face-to-face session	-0.052	0.102	[-0.257, 0.152]	-0.516	0.609	50	0.266	0.00%	0.277	0.074	187.442	<0.001
Program length	0.014	0.033	[-0.052, 0.08]	0.418	0.678	61	0.175	0.00%	0.232	0.058	223.039	<0.001
Frequency of recommended practice	-0.06	0.077	[-0.214, 0.094]	-0.782	0.437	58	0.612	0.00%	0.177	0.048	190.645	<0.001
Duration of a recommended practice	0.002	0.004	[-0.007, 0.011]	0.488	0.628	58	0.238	0.00%	0.178	0.048	189.082	<0.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	***SE tau²***	Q_E	$p\ (Q_E)$
Total amount of contact	0.012	0.007	[-0.001, 0.026]	1.796	0.078	61	3.225	3.45%	0.219	0.055	217.432	<0.001
Total amount of contact (excl. 0 hours)	0.018	0.01	[-0.002, 0.038]	1.839	0.072	50	3.38	5.99%	0.252	0.069	179.463	<0.001
Total recommended use of program	0.004	0.003	[-0.002, 0.011]	1.364	0.178	58	1.859	2.99%	0.17	0.047	184.366	<0.001
Total actual use of program	**0.013**	**0.006**	**[0.001, 0.024]**	**2.25**	**0.04**	**17**	**5.064**	**60.73%**	**0.014**	**0.027**	**18.571**	**0.234**
Program intensity excl. retreats	**1.069**	**0.36**	**[0.346, 1.792]**	**2.972**	**0.005**	**50**	**8.833**	**16.68%**	**0.224**	**0.063**	**172.476**	**<0.001**
Program intensity incl. retreats	**0.895**	**0.288**	**[0.315, 1.474]**	**3.105**	**0.003**	**50**	**9.64**	**20.89%**	**0.212**	**0.06**	**167.508**	**<0.001**
Amount of contact/week	**0.133**	**0.052**	**[0.03, 0.236]**	**2.575**	**0.013**	**61**	**6.63**	**9.21%**	**0.206**	**0.053**	**211.116**	**<0.001**
Amount of contact (excl. 0 hours)/week	**0.211**	**0.073**	**[0.064, 0.358]**	**2.888**	**0.006**	**50**	**8.343**	**16.93%**	**0.226**	**0.063**	**173.109**	**<0.001**
Recommended use of program/week	0.048	0.027	[-0.005, 0.102]	1.803	0.077	58	3.251	5.16%	0.166	0.046	182.671	<0.001
Actual use of program/week	0.061	0.052	[-0.049, 0.172]	1.185	0.255	17	1.404	17.44%	0.03	0.034	22.176	0.103

Note: significant effects in bold; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$) Q_E significance level; significant results in bold.

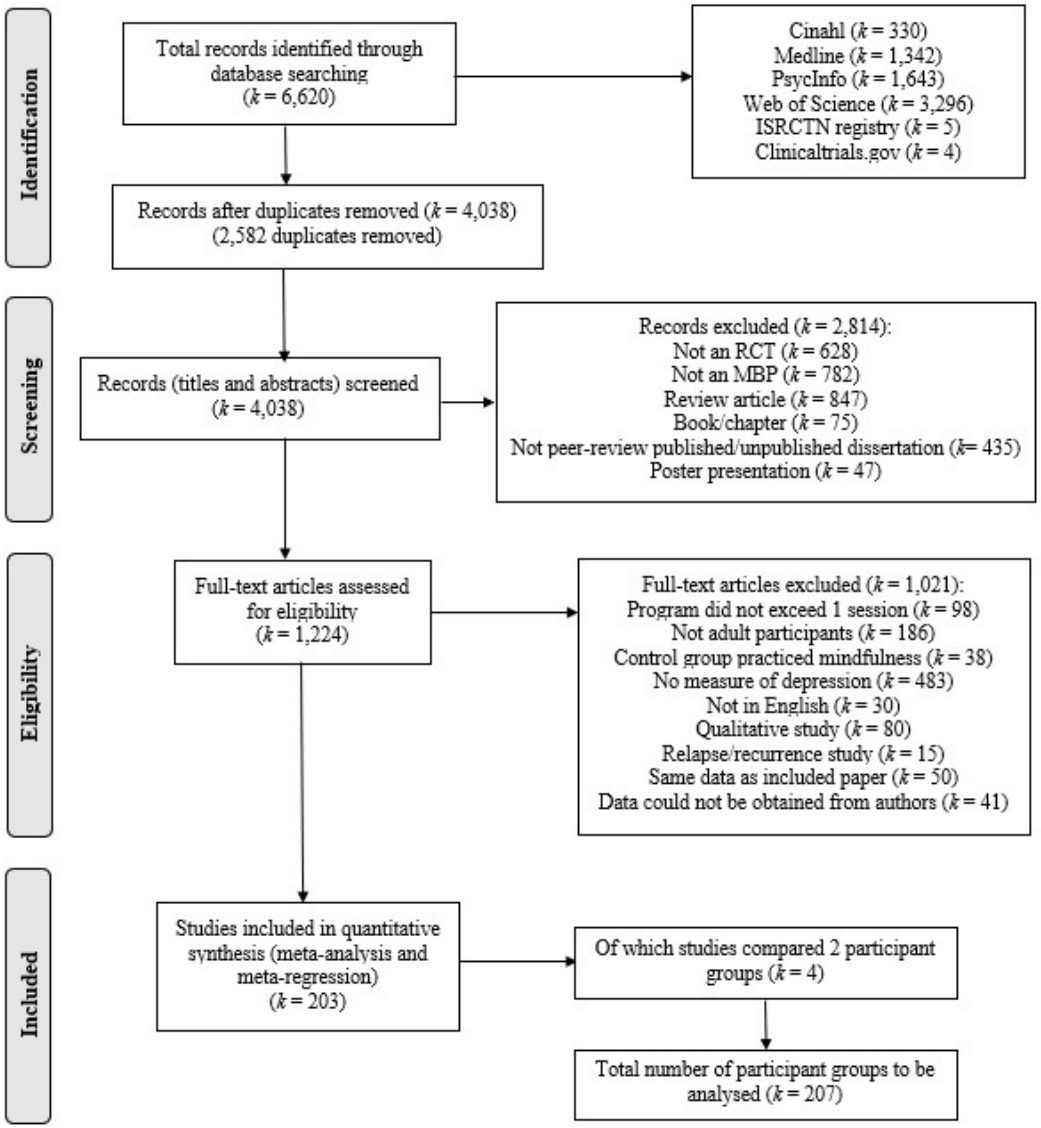
Identification
Total records identified through database searching (k = 6,620)
Cinahl (k = 330)
Medline (k = 1,342)
PsycInfo (k = 1,643)
Web of Science (k = 3,296)
ISRCTN registry (k = 5)
Clinicaltrials.gov (k = 4)
Records after duplicates removed (k = 4,038) (2,582 duplicates removed)
Screening
Records (titles and abstracts) screened (k = 4,038)
Records excluded (k = 2,814):
Not an RCT (k = 628)
Not an MBP (k = 782)
Review article (k = 847)
Book/chapter (k = 75)
Not peer-review published/unpublished dissertation (k= 435)
Poster presentation (k = 47)
Eligibility
Full-text articles assessed for eligibility (k = 1,224)
Full-text articles excluded (k = 1,021):
Program did not exceed 1 session (k = 98)
Not adult participants (k = 186)
Control group practiced mindfulness (k = 38)
No measure of depression (k = 483)
Not in English (k = 30)
Qualitative study (k = 80)
Relapse/recurrence study (k = 15)
Same data as included paper (k = 50)
Data could not be obtained from authors (k = 41)
Included
Studies included in quantitative synthesis (meta-analysis and meta-regression) (k = 203)
Of which studies compared 2 participant groups (k = 4)
Total number of participant groups to be analysed (k = 207)

Fig. 1 Prisma flow diagram (Moher et al. 2009) outlining identification, screening, eligibility and included stages of study selection (*k* = number of studies)

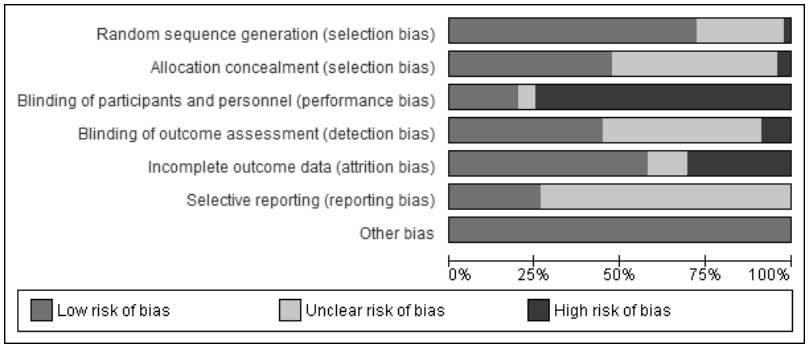

Fig. 2 Risk of bias graph showing ratings for each domain presented in percentages of *k*=203 (more detailed representation of judgements for each risk of bias domain for each included study is available on request from the corresponding author)

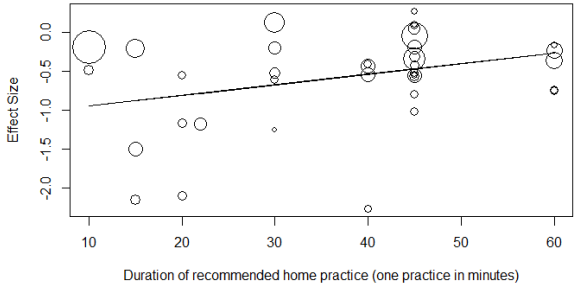

Fig. 3 Meta-regression plot for duration of recommended home practice (one practice in minutes) predicting depression at 1-4 months follow-up compared to inactive controls

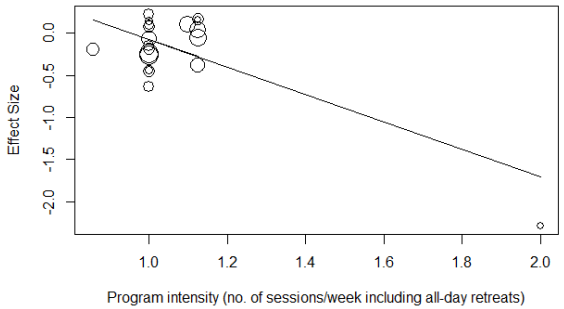

Fig. 4 Meta-regression plot for program intensity (incl. all-day retreats) predicting anxiety at 1-4 months follow-up compared to active controls

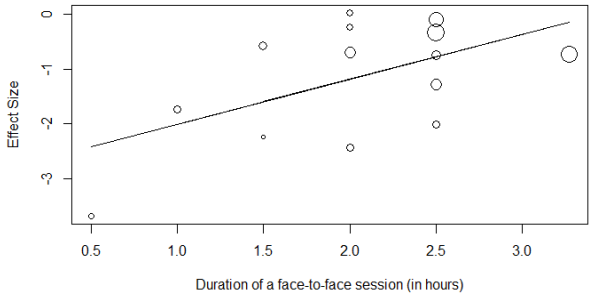

Fig. 5 Meta-regression plot for duration of a face-to-face session predicting stress at 1-4 months follow-up compared to inactive controls

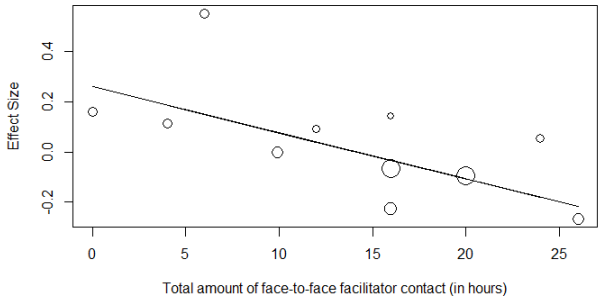

Fig. 6 Meta-regression plot for total amount of face-to-face facilitator contact predicting stress at 1-4 months follow-up compared to active controls

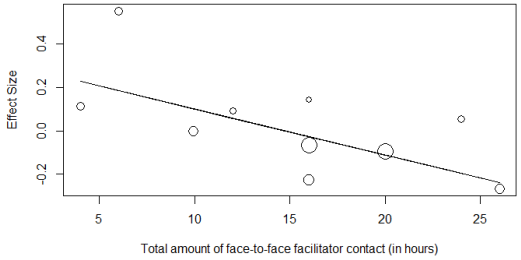

Fig. 7 Meta-regression plot for total amount of face-to-face facilitator contact (excl. studies with no face-to-face contact) predicting stress at 1-4 months follow-up compared to active controls

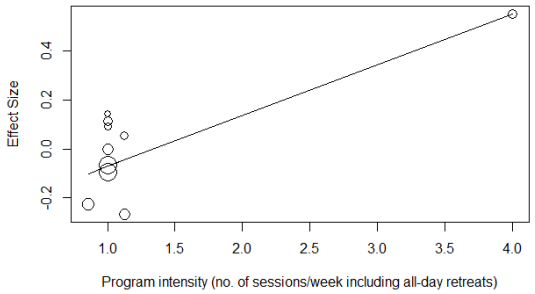

Fig. 8 Meta-regression plot for program intensity (incl. retreats) predicting stress at 1-4 months follow-up compared to active controls

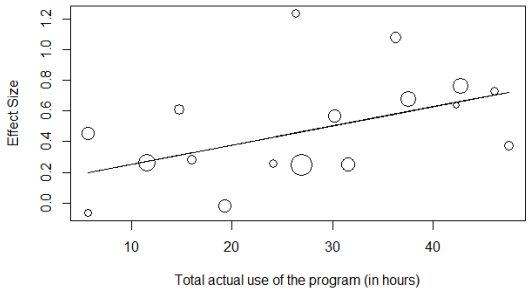

Fig. 9 Meta-regression plot for total actual use of the program dose predicting mindfulness at post-program compared to inactive controls

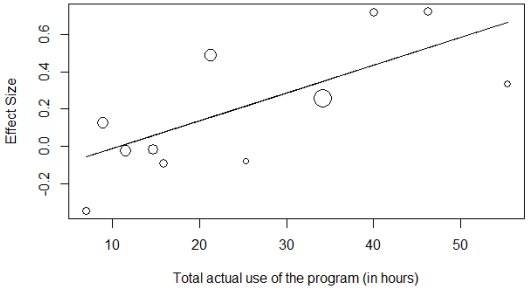

Fig. 10 Meta-regression plot for total actual use of the program dose predicting mindfulness at post-program compared to active controls

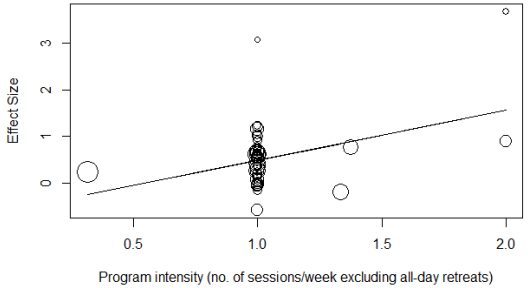

Fig. 11 Meta-regression plot for program intensity (excl. retreats) predicting mindfulness at post-program compared to inactive controls

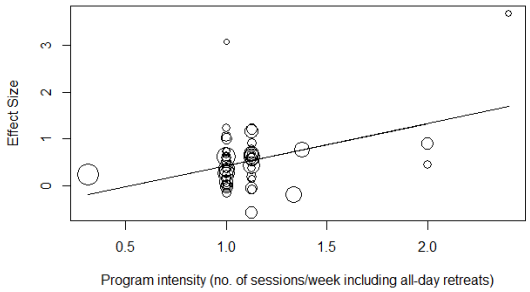

Fig. 12 Meta-regression plot for program intensity (incl. retreats) predicting mindfulness at post-program compared to inactive controls

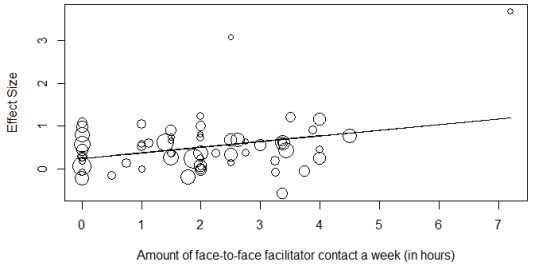

Fig. 13 Meta-regression plot for amount of facilitator contact predicting mindfulness at post-program compared to inactive controls

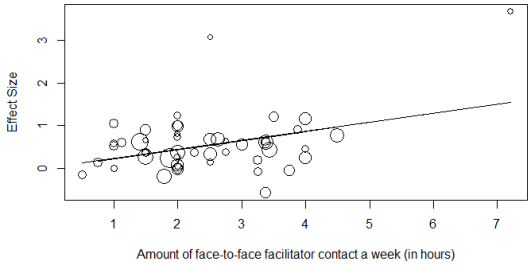

Fig. 14 Meta-regression plot for amount of facilitator contact (excl. no contact) predicting mindfulness at post-program compared to inactive controls

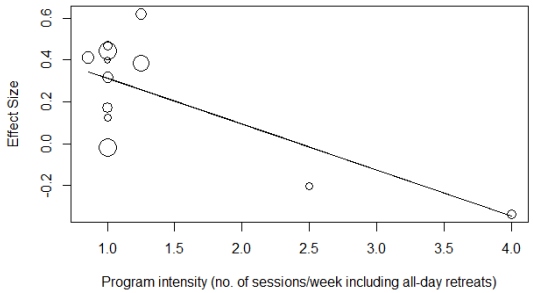

Fig. 15 Meta-regression plot for program intensity (incl retreats) predicting mindfulness at 1-4 months follow-up compared to active controls