Mindfulness (2019) 10:397–414
https://doi.org/10.1007/s12671-018-0979-z

REVIEW

Mindfulness for the Mental Health and Well-Being of Post-Secondary Students: A Systematic Review and Meta-Analysis

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Published online: 28 June 2018

Abstract

High levels of distress in post-secondary students, alongside the real or perceived barriers to accessing services, highlight the need for evidence-based, accessible, and brief interventions for students such as mindfulness-based interventions (MBIs). This systematic review and meta-analysis seeks to determine the effectiveness of MBIs for mental health outcomes in post-secondary students. We searched OVID MEDLINE In-Process, EMBASE, CENTRAL, CINAHL, PsychInfo, World Health Organization International Clinical Trials Registry Platform, ClinicalTrials.gov, Google Scholar, Proquest Dissertations, and OpenGrey. When possible, data were pooled using a random-effects model. Effect estimates were reported as standardized mean differences (SMDs) and then back-transformed into common scales of measurement. This review includes 41 randomized controlled trials reported in 49 studies. When comparing to a passive control, MBIs appear to reduce symptoms of depression [SMD − 0.49 (95% CI − 0.68, − 0.30)], anxiety [SMD − 0.53 (95% CI − 0.78, − 0.29)], and perceived stress [SMD -0.39 (95% CI -0.50, -0.27)] post-intervention (low-quality evidence). These findings were similar for shorter compared to longer interventions, although mindfulness-based cognitive therapy appeared to be the most effective for depression and anxiety. This review found no differential effects of MBIs compared to active comparators for depressive symptoms, anxiety symptoms, or perceived stress (low-quality evidence). Overall, MBIs of at least 2 weeks in duration appear to be a better alternative than no intervention for students with symptoms of depression, anxiety, and perceived stress.

Keywords Systematic review · Meta-analysis · Mindfulness · Post-secondary · Students · Mental health

Introduction

Most emerging adults in North America (~ 70%) attend post-secondary education with three-quarters of all lifetime mental illnesses occurring before the end of post-secondary schooling (Kessler et al. 2007; NCES 2014). A majority of university students report high levels of stress, anxiety, and feelings of sadness during the academic year with anxiety (~ 17%) and depression (~ 14%) being the most common disorders (American College Health Association (ACHA), 2016a, b).

Electronic supplementary material The online version of this article (https://doi.org/10.1007/s12671-018-0979-z) contains supplementary material, which is available to authorized users.

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Additionally, students report high rates of substance use, poor sleep quality, and emotion dysregulation; these are modifiable risk factors that can increase risk of psychopathology (ACHA 2016a, b; McLaughlin et al. 2011). Mental health challenges in students can lead to significant impairment in mental, social, and emotional functioning, heightening the risk of dropout, lower educational attainment, and suicidal behavior (Eisenberg et al. 2009; Keyes et al. 2012; Van Ameringen et al. 2003). Despite the high prevalence of mental health challenges in this population, less than half of students in need receive professional help (Hunt and Eisenberg 2010). Common student barriers to service utilization include stigma, embarrassment, feeling they do not have enough time, and post-secondary health and counseling centers being unable to meet the need of students (Eisenberg et al. 2007; Gulliver et al. 2010; Reetz et al. 2014). Mindfulness-based interventions (MBIs) offer an alternative and more accessible intervention aimed at improving student mental health.

Several reviews exist on mindfulness for stress, anxiety, and depression in broad clinical and non-clinical populations (Baer 2003; Carmody and Baer 2009; Chiesa and Serretti 2011, 2014; Eberth and Sedlmeier 2012; Fjorback et al. 2011; Grossman et al. 2004; Hofmann et al. 2010; Khoury et al. 2013; Klainin-Yobas et al. 2012; McConville et al. 2016; Piet and Hougaard 2011; Regehr et al. 2013; Sedlmeier et al. 2012; Shiralkar et al. 2013; Virgili 2015; Zainal et al. 2013). Although most studies report small to moderate effects of MBIs for anxiety symptoms and depressive symptoms, there is substantial variation in reported effect sizes. This variation may be due to broad definitions of what is considered an MBI, different population characteristics, and various types of study designs included in the analyses.

Only three systematic reviews discussing MBIs have focused on post-secondary students. Two of these reviews by Regehr et al. (2013) and Shiralkar et al. (2013) were on stress-reduction strategies for students, but neither identified mindfulness as a target intervention a priori nor included mindfulness terms in their search strategy. Regehr et al. (2013) included seven randomized controlled trials (RCTS) and two pre–post trials on mindfulness in their review. Shiralkar et al. (2013) included two RCTS and one non-controlled trial on mindfulness in their review. The most recent review, by McConville et al. (2016), focused on MBIs for health professional students. This review found 19 trials including a combination of RCTS and observational studies. They found MBIs compared to control or other interventions resulted in significantly lower anxiety (SMD = − 0.44; 95% CI − 0.59 to − 0.28; $p < 0.01$), lower depressive symptoms (SMD = − 0.54; 95% CI − 0.83 to − 0.26; p=0.01), and stress (SMD = − 0.44; 95% CI − 0.57 to − 0.31; $p < 0.01$) demonstrating medium magnitude effect sizes. This study found a trend toward maintenance of improvements in anxiety and stress at follow-up, but results were not significant (although limited by heterogeneity and few trials reporting follow-up; three for stress, two for anxiety). This study provided important information but is limited in generalizability as it restricted to health professional students.

Traditional MBIs require significant time and may not be practical or appealing to the student population. Some researchers believe the length of mindfulness interventions acts as a moderator; however, the current evidence of this moderating effect is limited and conflicting (Carmody and Baer 2009; Eberth and Sedlmeier 2012; Hofmann et al. 2010; Khoury et al. 2013; Klainin-Yobas et al. 2012; Virgili 2015). Thus, a systematic review and meta-analysis of MBIs for post-secondary students comparing brief non-traditional interventions (less than 8 weeks) to longer traditional interventions (8 weeks or longer) would be of value.

Mindfulness interventions in the post-secondary setting offer an opportunity to develop a skill or coping strategy which students with a broad range of difficulties or conditions can learn and practice. MBIs have no or minimal ongoing costs, can be practiced by the student in many settings and circumstances, and carries low risk of adverse effects or harm. Mindfulness may also be appropriate as an early stand-alone intervention (e.g., for stress or sleep issues) or as students wait for more comprehensive assessment or other services (e.g., for anxiety disorders). It teaches a lifelong skill which may reduce the need for ongoing and costly professional support and services.

The primary objective of this systematic review and meta-analysis is to address limitations of previous reviews and synthesize the current literature regarding the effectiveness of MBIs for all post-secondary students on (1) anxiety and (2) depression compared to passive control, active control, and broader psychotherapeutic techniques. Secondarily, this review explored if MBIs are effective at (3) reducing perceived stress, (4) improving sleep parameters, (5) reducing substance use, and (6) improving emotion regulation in post-secondary students. Results are presented meta-analytically when possible and narratively when unable to pool results. The results are summarized to indicate potential implications for practice and future research.

Method

Protocol and Registration

A review protocol was registered with PROSPERO found at https://www.crd.york.ac.uk/prospero/display_record.php?RecordID=56277.

Eligibility Criteria

Study Design This review includes RCTs only, as this is the best design for determining the effectiveness of an

intervention (Higgins and Green 2011). All RCT designs are included while quasi-randomized trials and other observational studies are excluded.

Population The population of interest is post-secondary students, including undergraduate, graduate, college, and health professional students. Many students are unaware they have a mental health concern or are unwilling to seek help, even though they experience high levels of stress, anxiety, and low mood (internalizing symptoms) (ACHA 2016a, 2016b; Hunt and Eisenberg 2010). Therefore, this review includes healthy participants and students with internalizing symptoms. We excluded studies that restricted to students with any physical neurological disorder, psychosis, ADHD, or other developmental disabilities due to different neurological processing associated with these conditions and the lack of precise understanding of the neurobiological mechanisms of mindfulness among these populations (American Psychiatric Association 2013; Tang et al. 2015). Studies with population samples comprised at least 75% students were included.

Intervention This review includes MBIs of at least 2 weeks in duration. We did not restrict MBIs to traditional MBSR or MBCT. Therefore, we assessed interventions for fidelity and integrity on the core components of mindfulness ensuring interventions involved: (1) grounding oneself in the present moment, and (2) being open and accepting of these experiences (Crane et al. 2016; Creswell 2016). There were no restrictions placed on methods of delivery (i.e., online, in person, guided, unguided, etc.) or length and frequency of practice periods within the study duration. The study selection criteria excluded other forms of meditation that did not include the two core components or broader psychotherapeutic approaches that include, but are not primarily focused on, mindfulness strategies. Combined approaches were selected only if both the intervention and control groups received the same co-intervention, but the intervention group additionally received mindfulness meditation.

Comparisons Comparisons included (1) passive controls (no intervention, waitlist control), (2) active controls that control for non-mindfulness specific effects of an MBI intervention (i.e., health education, relaxation groups, and physical activity), and (3) broader psychotherapeutic approaches (e.g., cognitive-behavioral therapy, CBT).

Outcome A search of the COMET database (2017) for "mindfulness" or "meditation" yields no results for standardized outcomes. Consultations with student mental health experts at McMaster University helped determine clinically meaningful outcomes based on the 9-point rating scale suggested by the Grading of Recommendations Assessment, Development and Evaluation (GRADE) Working Group (Guyatt et al. 2011a). Primary outcomes include anxiety and depressive symptoms; the focus was on symptoms rather than diagnosis as anxiety and depression are under-diagnosed in this population. Secondary outcomes include perceived stress, sleep parameters, substance use frequency, and emotion regulation. Physiological measurements of stress (i.e., salivary cortisol) were not analyzed. There were no restrictions on psychological measures for outcomes.

Search Strategy

We searched OVID MEDLINE In-Process (1946 to Week 6 2017), EMBASE (1974 to 2017 Week 4), the Cochrane Central Register of Controlled Trials (CENTRAL) (The Cochrane Library 2016, Issue 11), CINAHL (1981 to January 2017), and PsychInfo (1987 to February Week 1 2017). Ongoing trials were identified using the World Health Organization (WHO) International Clinical Trials Registry Platform, ClinicalTrials.gov, and Google Scholar (first 100 hits) (up to February 2017). Unpublished trials were identified using Proquest Dissertations and OpenGrey (up to February 2017). No restrictions were placed on language or publication status. We checked abstracts and reference lists of included articles and existing systematic reviews and contacted authors for further information and data when appropriate. We used search terms related to post-secondary students, MBIs, and RCTs to find relevant articles. Subject-specific terms were initially identified in MEDLINE and then modified for other databases. We ensured subject headings were combined with key words to maximize a search for relevant studies on mindfulness. To identify RCTs, database-specific sensitivity-maximizing search terms were used (Higgins and Green 2011; HIRU 2017, b; Wong et al. 2006a, b).

Data Collection and Analysis

Selecting, Extracting, and Managing Studies Two review authors (J.E.H. and J.L.D.) screened titles and abstracts of identified studies for possible inclusion based on piloted screening forms. After title and abstract screening, all studies that were selected for full-text review by either author were aggregated for full-text screening. The same reviewers independently assessed each full text for inclusion; disagreements were resolved through discussion or consultation with a third reviewer (N.M. or C.M.). For included full texts, one review author extracted data to a Microsoft Excel spreadsheet (J.E.H., J.L.D., I.F.M., A.J.C., I.V.), which were then verified by a second review author (J.E.H., J.L.D, I.F.M., A.J.C., I.V.). Discrepancies were resolved through discussion or consultation with a third reviewer. Trial authors were contacted for additional information when necessary.

Assessment of Risk of Bias in Included Studies To assess the overall methodological rigor of the included studies, studies were assessed for risk of bias using the Cochrane Risk of Bias (RoB) tool at the study level (Higgins and Green 2011). One review author (J.E.H., I.F.M., A.J.C., I.V.) conducted RoB assessments and a second reviewer (J.E.H., J.L.D., I.F.M., A.J.C., I.V.) verified the assessments. Discrepancies in judgments were discussed and, if a consensus was not met, a third reviewer resolved the remaining discrepancies. Each study was assessed on sequence generation, allocation concealment, blinding of participants and personnel, blinding of outcome assessment, attrition, whether there was selective outcome reporting, and other biases (including non-compliance bias) (Higgins and Green 2011). For cluster-randomized studies, the RoB assessment also included recruitment bias, baseline imbalances, loss of clusters, incorrect analysis, and comparability with individually randomized trials (Higgins and Green 2011). Each domain was judged as "low" or "high" RoB (when possible, "unclear" was avoided to ensure more meaningful findings).

Measures of Treatment Effect Since studies measured outcomes using different measures, standardized mean differences (SMDs) were calculated to create a uniform measurement scale and allow pooling of effects across studies. Independent outcome measures were sought when available (e.g., Beck Depression Inventory (BDI), Beck Anxiety Inventory (BAI), Perceived Stress Scale (PSS)) as opposed to subscales on a larger measure (e.g., Depression Anxiety Stress Scales (DASS) subscales). If independent measures were not available, the subscales were used. Review Manager 5 (RevMan) software was used to calculate SMDs, which uses the Hedges adjusted g formula (Deeks & Higgins, 201a; The Cochrane Collaboration 2014). SMDs were based on post-intervention means and standard deviations (SDs) in both the MBI and comparison groups. Change from baseline scores were not used due to inconsistent reporting and missing information. Substance use frequency and sleep parameters can be measured continuously (i.e., number of drinks, hours of sleep) or categorically (i.e., abstinence, quality of sleep), and therefore data extracted for these outcomes included means and SDs or odds or risk ratios.

Unit of Analysis Issues Some data had to be adjusted due to (1) cluster randomization and (2) factorial designs. Firstly, when an included study was a cluster-randomized study, the data had to be adjusted for the clustering effect by either using the reported adjusted post-score means or the intra-class correlation coefficient (ICC) to calculate the associated design effect (DE) and effective sample size. If adjusted post-score means, an ICC, or the residuals of the hierarchical linear model were not reported, a conservative estimated ICC of 0.05 was used to calculate the effective sample size. This estimate was selected based on two previous studies: (1) Kuyken et al. (2013), a study on mindfulness in secondary schools found a between-school ICC of 0.007 for depression, and (2) van Dijk et al. (2017), another study on MBIs for medical students, randomized based on clinical clerkships, found a negligible ICC (0.00) for general psychological distress. Secondly, in three-armed designs where the control group needs to be entered into the same meta-analysis twice, the sample size of the duplicate group was divided in half with means and SDs left unchanged (Higgins and Green 2011).

Assessment of Heterogeneity

Heterogeneity between trials pooled for meta-analyses was assessed by (1) visual inspection of the forest plot, (2) χ^2 test for statistical significance ($p < 0.1$), and (3) I^2 statistic to examine the proportion of between-trial differences not due to chance (Higgins and Green 2011). A moderate I^2 statistic was operationalized as 40%. All pre-specified subgroup analyses were carried out regardless of heterogeneity.

Data Synthesis The inverse variance method was used to present pooled SMDs using a random-effects model. RevMan 5.3 software was used to calculate an SMD and a 95% confidence interval (CI) for each analysis (Deeks and Higgins 2010). The pooled effect estimate was back-transformed to the most validated and common scale of measurement for that outcome (e.g., BDI, BAI, PSS). If there were not enough studies to pool results for a meta-analysis (i.e., less than 2), outcomes were reported narratively. For the continuous outcomes where only one study was available, a fixed-effect model in RevMan was used to present the MD using post-scores or change scores.

Subgroup Analyses and Sensitivity Analysis Subgroup analyses determine a priori included (1) length of the intervention (i.e. brief (< 8 weeks) versus longer interventions (≥ 8 weeks)) and (2) study quality (low and high RoB). Post hoc subgroup analyses included (3) MBI type (i.e., MBSR, MBCT, other MBI) and (4) effect of expected daily practice. Significant sub-group differences were identified using a p value of < 0.1.

Presentation of the Results SMDs of 0.2, 0.5, and 0.8 are interpreted as small, medium, and large effects, respectively (Cohen 1988; Higgins and Green 2011). When effects were significant, the pooled SMD was back-transformed to the most clinically applicable measure by multiplying the pooled SMD by the SD of the scale of interest (Higgins and Green 2011). The GRADE approach was used to assess the quality of the evidence for each outcome to help infer confidence in the review findings and guide future research (Schünemann et al. 2013). Optimal information sizes were used to help guide judgments around imprecision (Guyatt et al. 2011b). Overall, results are presented as an interpretation of the pooled SMD estimate along with the GRADE assessment.

Results

Description of Studies

Results of the Search The search was conducted during the fourth week of January 2017. J.E.H. and J.L.D. independently screened a total of 4674 records (after 1838 duplicates were removed in Endnote). The search identified a total of 221 studies for full-text screening. Two independent reviewers (J.E.H. and J.L.D.) conducted full-text screening, agreeing on 92% of the texts; discrepancies were resolved through discussion or consultation with a third reviewer (C.M.). The inter-rater reliability was "good" according to a kappa of 0.8 (Altman 1990). There were two protocols that met inclusion criteria to which a publication was not found and authors did not return inquiry emails and four studies that met criteria but are currently ongoing. There was one additional study abstract that met criteria, but the full publication was not found; results from the abstract are included but reported narratively (Burger 2015). Ultimately, 41 RCTs reported in 49 studies are included in this review. See Fig. 1 for the flow diagram.

Included Studies The 41 included studies (see Table 1) involve 4211 post-secondary students including undergraduate, graduate, and health professional students. The mean age of participants in the individual studies ranged from approximately 18 to 29 years. Twenty-four studies did not have any mental health inclusion criteria, nine studies recruited students with at least moderate levels of internalizing mental health or substance use concerns, three studies excluded participants if they were currently engaging in psychotherapy, and five studies actively excluded students with a diagnosis of depression or any mental illness. Most studies conducted randomization at the participant level, apart from one study that randomized classrooms (Leggett 2010). Twenty-seven studies compared MBIs to passive control, ten studies compared to active control (typically involved an intervention mirroring the same length and delivery method of the MBI, but with different or no specific content), three studies included a three-arm study comparing to both passive and active controls, and one study was three-armed with passive control and CBT.

The most commonly reported outcome was perceived stress (27 studies) and depressive symptoms (27 studies), followed by anxiety symptoms (24 studies) whereas only 2 studies included substance use outcomes, 2 studies reported emotion regulation outcomes, and 2 studies reported sleep outcomes. Where only cumulative scores were reported, all trialists provided the subscale scores for inclusion in this meta-analysis. Similarly to cumulative scores, when post-scores were not available, trialists were contacted requesting post-scores and we heard from all but one of these trialists (Oman et al. 2008). We were unable to obtain the follow-up control group sample size for one study (Gallego et al. 2014) and another trialist was unable to provide us with post-scores since they no longer have access to the data (Shapiro and Schwartz 1998).

There were three studies presenting unit of analysis issues. One included study randomized by class, and the clustering effect was accounted for using our pre-defined conservative estimated ICC 0.05 (DE = 1.3) (Leggett 2010). Also, there were 2 three-armed designs where the sample size of the duplicate group (control group) was divided in half with means and SDs left unchanged (Call et al. 2014; Delgado-Pastor et al. 2015).

Risk of Bias of Included Studies

Most included studies presented moderate to high risk of bias based on sequence generation, allocation concealment, performance bias, detection bias, attrition, selective outcome reporting, and other biases. Only one study failed to use appropriate random sequence generation: Oman et al. (2008) allowed four participants to switch intervention groups due to scheduling conflicts. Studies varied in their use and reporting of allocation concealment. Regarding performance bias, high RoB is inevitable across all studies where mindfulness was compared to passive control due to the inability to blind participants and instructors to group assignment. That being said, blinding of participants might have been possible if the comparator was another intervention (i.e., active control). Although four included studies that compared to active control blinded participants to group assignment, in most studies, there was a risk that knowledge of assignment, as opposed to the intervention itself, may be affecting outcomes (Higgins and Green 2011). Additionally, since the outcome(s) of interest were all measured through self-report questionnaires (apart from one study that used biomedical testing to confirm self-reported smoking abstinence), all studies that did not blind participants to group assignment present a high risk of detection bias, i.e., knowledge of assignment may impact the way a participant reports outcomes (Higgins and Green 2011). Additionally, about half of the included studies had significant attrition (> 20%) with limited insights into the cause of attrition. Limited availability of study protocols and a lack of standardized outcome measures for MBIs make the assessment of reporting bias difficult; therefore, unless there were explicit discrepancies within the article, reporting bias was judged as low. Only one included study was considered "low RoB," and therefore, the subgroup analyses on study quality were altered to compare studies that had low RoB in all domains except for performance and detection bias (as these biases were present among almost all included studies) to those with higher risk of bias. Figure 2 summarizes the RoB authors' judgments about each risk of bias item presented as percentages across all included studies.

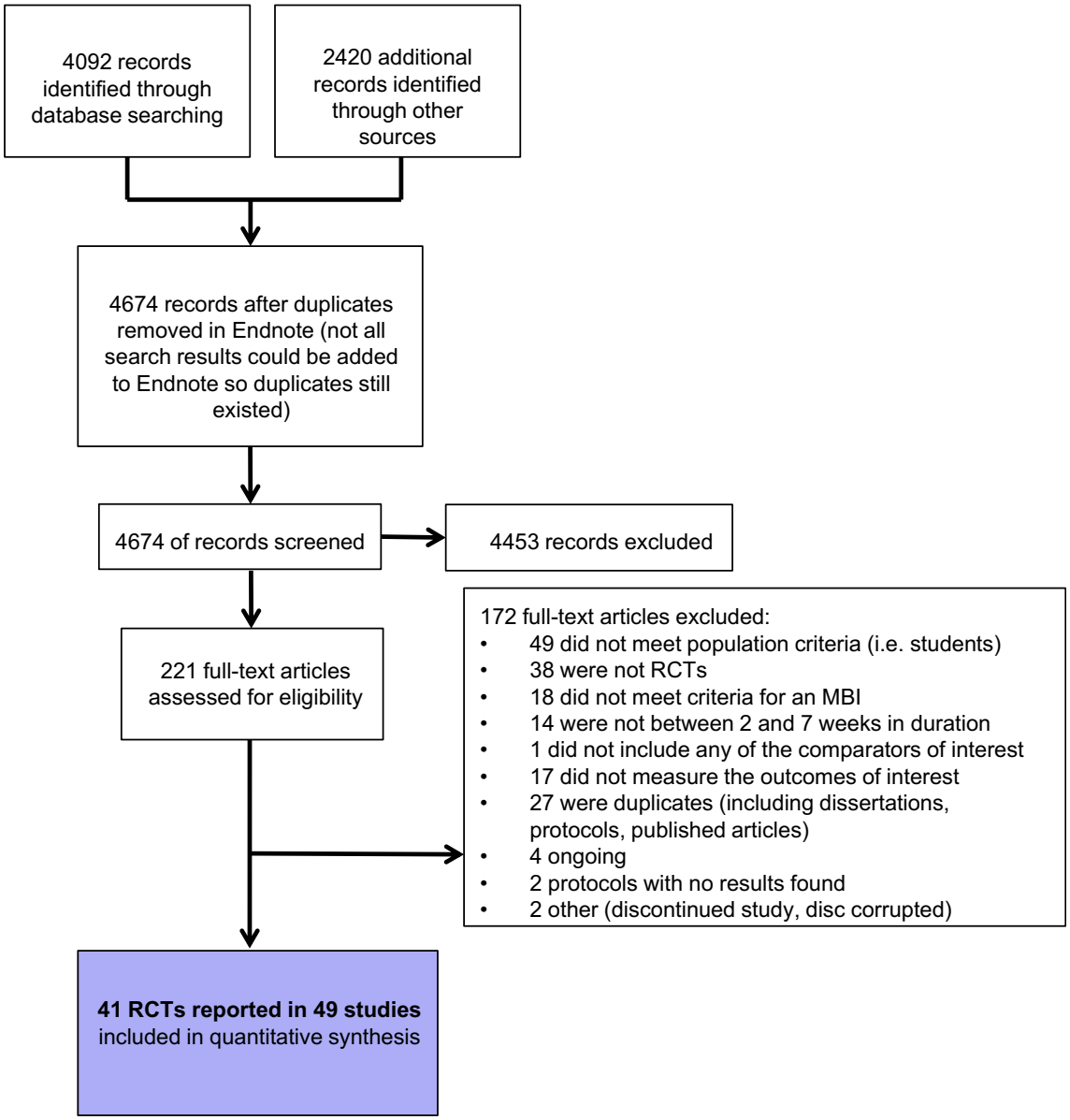

Fig. 1 A flow diagram following the PRISMA template of included studies

Effects of the Intervention

Depressive Symptoms

Pooling data from 20 studies (1266 participants) comparing MBIs to passive control, we found a moderate significant reduction in depressive symptoms (SMD −0.49 (95% CI −0.68, −0.30); BDI −4.1 (95% CI −5.7, −2.5)) (see Fig. 3). An I^2 of 59% ($p < 0.05$) reflects substantial heterogeneity in the pooled analysis. The sub-group analysis comparing length of interventions revealed no significant difference in effect between brief (SMD = −0.32 (95% CI −0.55, −0.09)) and long (SMD = -0.59 (95% CI −0.86, −0.31)) interventions for depressive symptoms ($p = 0.15$). However, this analysis revealed most heterogeneity was present in the longer interventions (31% in brief, 68% in long). Since most of the brief interventions are adaptations of specific types of mindfulness interventions, a post hoc subgroup analysis was conducted to determine the impact of traditional compared to adapted interventions (i.e., MBSR versus MBCT versus other or adapted MBIs). This post hoc analysis revealed that MBCT (SMD = −1.21 (95% CI −1.76, −0.66)) was producing significantly larger effects than MBSR (SMD = −0.44 (95% CI −0.72, −0.16), $p = 0.01$) and Other MBIs (SMD = −0.29 (95% CI −0.45, −0.12), $p < 0.01$), but MBSR and Other MBIs were similar ($p = 0.35$). On removal of the MBCT studies from the overall analysis, the I^2 dropped to 23% and pooled effect size reduced but remained significant (SMD = −0.29 (95% CI −0.45, −0.12); BDI = −2.4 (95% CI −3.7, −1.0)). There was no subgroup effect for

Table 1 Comparison of types of interventions, comparators, lengths of interventions, and outcomes from each study included in the meta-analyses

	Mental Health Selection Criteria	Intervention	Comparator	Length	Outcomes Assessed	Time(s) of Assessment
Aeamla-Or (2015)	Excluded participants with any mental illness.	MBSR	No Intervention	8 weeks	• Depression • Stress	Post, 8 weeks, 16 weeks
Blevins (2008)	None	Standard Treatment + Mindfulness (MBT)	Standard Behavioral Treatment (SBT)	8 weeks	• Depression • Anxiety	Post, 3 months
Bohecker and Doughty Horn (2016)	None	Mindfulness Experiential Small Group curriculum	Active Control (met in group each week)	8 weeks	• Stress	Post
Call et al. (2014)	Required at least moderate levels of worry.	1) MBSR based hatha yoga 2) MBSR based body scan	Wait-list Control	3 weeks	• Anxiety • Stress	Post
Cavanagh et al. (2013)	None	Learning Mindfulness Online	Wait-list Control	2 weeks	• Stress	Post
Danilewitz et al. (2016).	None	Mindfulness meditation program	Wait-list control group	8 weeks	• Depression • Anxiety • Stress	Post
Davis et al. (2013)	Participants needed to report smoking 10 or more cigarettes per day and engaging in 5 or more 'binges' per month	Mindfulness Training for Smoking	Interactive Learning for Smokers	6 weeks	Smoking cessation	Post
de Vibe et al. (2013)	None	MBSR (adapted)	No Intervention	7 weeks	• Stress	Post
Delgado-Pastor et al. (2015)	Required high levels of worry without Generalized Anxiety Disorder or treatment.	1) Mindfulness Cognitive Training Group 2) Mindfulness Interoceptive Training Group	No Intervention	3 weeks	• Depression • Anxiety • Stress	Post
Dvorakova et al. (2017)	None	L2B Program	Wait List Control	6 weeks	• Depression • Anxiety • Sleep • Alcohol Outcomes	Post
Erogul et al. (2014)	None	MBSR intervention	Wait List Control	8 weeks	• Stress	Post, 6 months
Falsafi (2016)		Mindfulness	No Intervention	8 weeks	• Depression • Anxiety • Stress	Post, 1 month
Gallego et al. (2014)	None	MBCT-based Mindfulness	1) Physical Education 2) No Intervention Control Group	8 weeks	• Depression • Anxiety •Stress	Post
Grandpierre (2013)	Excluded students with a diagnosis of depression.	Mindfulness for Academic Success (MAS) program	Wait-list control	6 weeks	• Depression • Anxiety • Stress • Emotion Regulation	Post
Greer (2015)	None	Mindfulness Intervention	Stress Management Comparison Group	4 weeks	• Depression • Anxiety • Stress	Post, 1 month
Greeson et al. (2014)	None	Koru	Wait-list Control	4 weeks	• Sleep • Stress	Post

Table 1 (continued)

	Mental Health Selection Criteria	Intervention	Comparator	Length	Outcomes Assessed	Time(s) of Assessment
Hazlett-Stevens and Oren (2017)	Excluded participants if they were currently engaging in psychotherapy.	Mindfulness-Based Stress Reduction Bibliotherapy	No Intervention	10 weeks	• Depression • Anxiety • Stress	Post
Jimenez (2009)	Excluded participants if they were currently engaging in psychotherapy.	Mindfulness CD	Relaxation CD	4 weeks	• Depression	Post, 8 weeks, 20 weeks
Kang et al. (2009)	Excluded participants with any mental illness.	Meditation-based stress management	No Intervention	8 weeks	• Depression • Anxiety • Stress	Post
Kaviani et al (2011)	None	MBCT	Wait List Control	8 weeks	• Depression • Anxiety	Post, 1 month, 6 months
Kaviani et al. (2012)	Required participants to score high on a depression symptom inventory.	Group based mindfulness cognitive therapy	Wait List Control	8 weeks	• Depression • Anxiety	Post, 1 month, 6 months
Kuhlmann et al. (2016)	None	MediMind	1) Autogenic Training 2) Wait-List control	5 weeks	• Stress	Post, 1 year
Kvillemo et al. (2016)	None	Internet-based mindfulness program	Internet-based expressive writing program	8 weeks	• Depression	Post
Leggett (2010)	None	Brief Mindfulness Breathing Intervention	No Intervention	5 weeks	• Depression	Post, 2 months, 4 months
Mak et al. (2015)	None	Basic Mindfulness Group	Waitlist control	8 weeks	• Depression • Anxiety • Stress	Post, 3 months
McIndoo et al. (2016)	Required participants to be experiencing mild to severe depression.	Mindfulness-Based Stress Reduction	Behavioural activation	4 weeks	• Depression	Post, 1 month
Nguyen-Feng et al. (2016)	None	Present Control + Mindfulness	Present Control	3 weeks	• Depression • Anxiety • Stress	Post
Omidi et al. 2013)	Participants needed to meet criteria for Major Depressive Disorder.	MBCT	1. CBT 2. Treatment as usual	8 weeks	• Depression • Anxiety	Post
Paholpak et al. (2012)	None	Breathing Meditation	Group contact control	4 weeks	• Depression • Anxiety	Post
Phang et al. (2015a).	None	Mindful-gym	Wait-list Control	5 weeks	• Stress	Post, 6 months
Phang et al. (2015b)	None	Mindful-gym (DVD)	Wait-list Control	5 weeks	• Stress	Post
Ratanasiripong et al. (2015)	None	Mindfulness Meditation	1) Biofeedback Group 2) No Intervention Group	4 weeks	• Anxiety • Stress	Post
Rowland et al. (2016)	Excluded participants with any mental illness.	Mindfulness Intervention	No Intervention	6 weeks	• Depression • Anxiety	Post
Shapiro et al. (2011)	None	MBSR	Wait List Control	8 weeks	• Stress	Post, 2 months, 12 months
Song and Lindquist (2015)	Excluded participants with any mental illness.	MBSR	Wait List Control	8 weeks	• Depression • Anxiety • Stress	Post
Taylor et al. (2014)		MBCT-Self Help	Wait List Control	8 weeks	• Depression	Post

Table 1 (continued)

	Mental Health Selection Criteria	Intervention	Comparator	Length	Outcomes Assessed	Time(s) of Assessment
	Excluded participants if they were currently engaging in psychotherapy.				• Anxiety • Stress	
Walsh (2014)	Required participants to be experiencing moderate depressive symptoms.	Mindfulness Training	Contact-Control	5 weeks	• Depression • Anxiety • Emotion Regulation	Post
Warnecke et al. (2011).	Required participants to be experiencing moderate to high levels of psychological distress.	Mindfulness Intervention CD	Wait List Control	8 weeks	• Depression • Anxiety • Stress	Post

study quality. Two relevant articles could not be included in the meta-analysis due to unavailable post-score data, but results are consistent with pooled effects. Gallego et al. (2014) compared an 8-week MBCT intervention to no intervention control group and found the MBCT group had a significant reduction in depressive symptoms compared to control ($n = 125$). Shapiro and Schwartz (1998) found an 8-week MBSR intervention resulted in reduced depressive symptoms in comparison to control ($n = 76$).

Given the substantial differences in depression scores in the MBCT intervention studies, follow-up studies were split by type for further analysis of prolonged effects. MBCT interventions demonstrated lasting effects at 1 month post-intervention (two studies, 64 participants; MD of the Beck Depression Inventory (BDI) − 5.68 (95% CI − 7.15, − 4.21)) and at 6 months (two studies, 65 participants; MD on BDI − 5.06 (95% CI − 6.52, − 3.59)). In Other MBIs, effects did not remain at 1 month (two studies, 188 participants) or 2–3 months (three studies, 374 participants), but were present at 4–5 months post-intervention (two studies, 191 participants; SMD − 0.43 (95% CI − 0.72, − 0.14)).

Pooling data from nine studies (830 participants) comparing MBIs to active control, we found no statistically significant difference between intervention groups (SMD 0.04 (95% CI − 0.13, 0.22)) (see Fig. 4). Heterogeneity was below the a priori cut off with an I^2 29% ($p = 0.18$), and therefore we did not explore past our pre-specified subgroup analyses, which were not significant. There were no effects found at 1 month (two studies, 188 participants), 2–3 months (two studies, 102 participants), or 20 weeks (one study, 85 participants) post-intervention.

Omidi et al. (2013) was the only study to compare MBIs to psychotherapy. This study compared an 8-week traditional MBCT intervention to CBT and found no differences between groups on depressive symptoms.

Anxiety Symptoms

Pooling data from 20 studies (1185 participants) comparing MBIs to passive control, a moderate significant reduction was found in anxiety symptoms (SMD − 0.53 (95% CI − 0.78, − 0.29); BAI − 3.8 (95% CI − 5.6, − 2.1)) (see Fig. 5). There was high heterogeneity among the studies (I^2 74%, $p < 0.05$). There was no significant difference between brief (SMD = − 0.37 (95% CI − 0.59, − 0.15)) and long (SMD = − 0.66 (95% CI − 1.09, − 0.23)) interventions ($p = 0.24$). Most of the heterogeneity was in the longer interventions (i.e., 31% brief and 84% long); therefore, a post hoc sensitivity analysis on type of intervention was conducted. Results mirrored what was found with depressive symptoms, where MBCT contributed the most heterogeneity and MBCT (SMD = − 0.91 (95% CI − 0.96, − 0.04)) was significantly different from MBSR (SMD = − 0.53 (95% CI − 0.96, − 0.04))

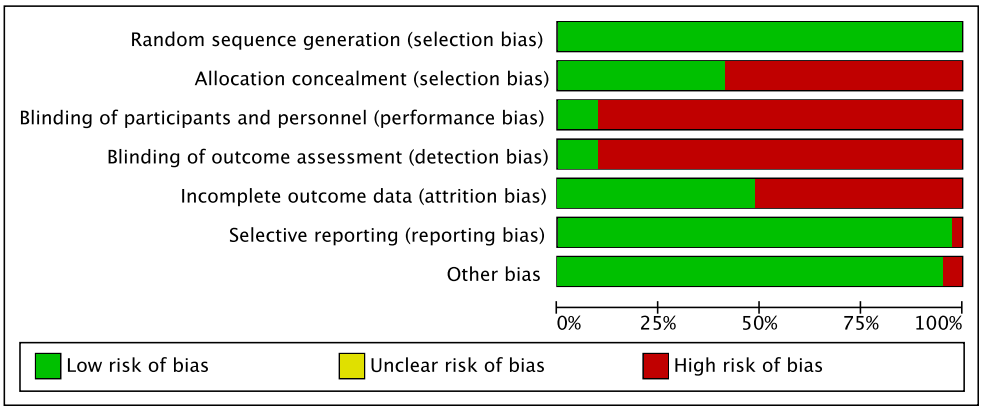

Fig. 2 Risk of bias graph: review authors' judgments about each risk of bias item presented as percentages across all included studies

($p = 0.04$) and Other MBIs (SMD = − 0.31 (95% CI − 0.49, − 0.12)) ($p < 0.01$), but MBSR and Other MBIs were not significantly different from one another ($p = 0.44$). On removal of the MBCT studies, the I^2 dropped to 41%; results remained statistically significant, however the effect size decreased (SMD = − 0.32 (95% CI − 0.49, − 0.12); BAI − 2.3 (95% CI − 3.5, − 0.9)). There was no subgroup effect of lower versus high risk of bias studies ($p = 0.30$). Similarly to the depression meta-analysis, we were unable to include Gallego et al. (2014) and Shapiro and Schwartz (1998) in the anxiety meta-analysis. Both of these studies also found significant reductions in anxiety symptoms in the MBI groups compared to the control groups. These results are consistent with the pooled effect size found in this meta-analysis.

Follow-ups were again split by type. MBCT demonstrated sustained reductions in anxiety symptoms at 1 month (two studies, 66 participants; MD on BAI − 7.12 (95% CI − 8.23, − 5.97)) and 6 months (two studies, 65 participants; MD on BAI − 5.95 (95% CI − 10.78, − 1.13)). Other MBIs did demonstrate significant reductions at 1 month (one study, 33 participants; MD Hamilton Anxiety Scale − 9.50 (− 17.27, − 1.73)) and no significant differences than passive control at 2–3 months (one study, 183 participants).

Pooling data from seven studies (663 participants) comparing MBIs to active control, we found a non-significant effect favoring active control for anxiety symptoms (SMD 0.13 (95% CI − 0.08, 0.34)) (see Fig. 6). Heterogeneity was just below the cut-off (I^2 39%; $p = 0.14$). There was no difference

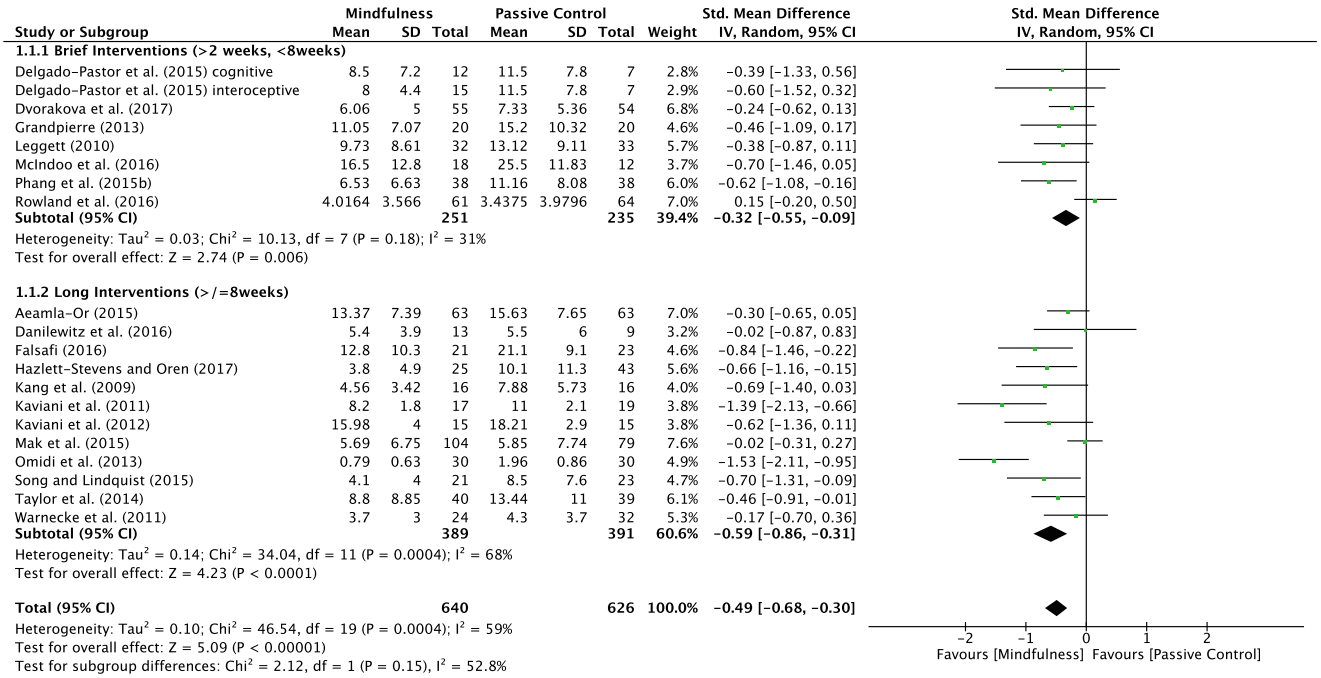

Fig. 3 Forest plot for the meta-analysis of MBIs compared to passive control for depressive symptoms at post-intervention, including the study length subgroup analysis

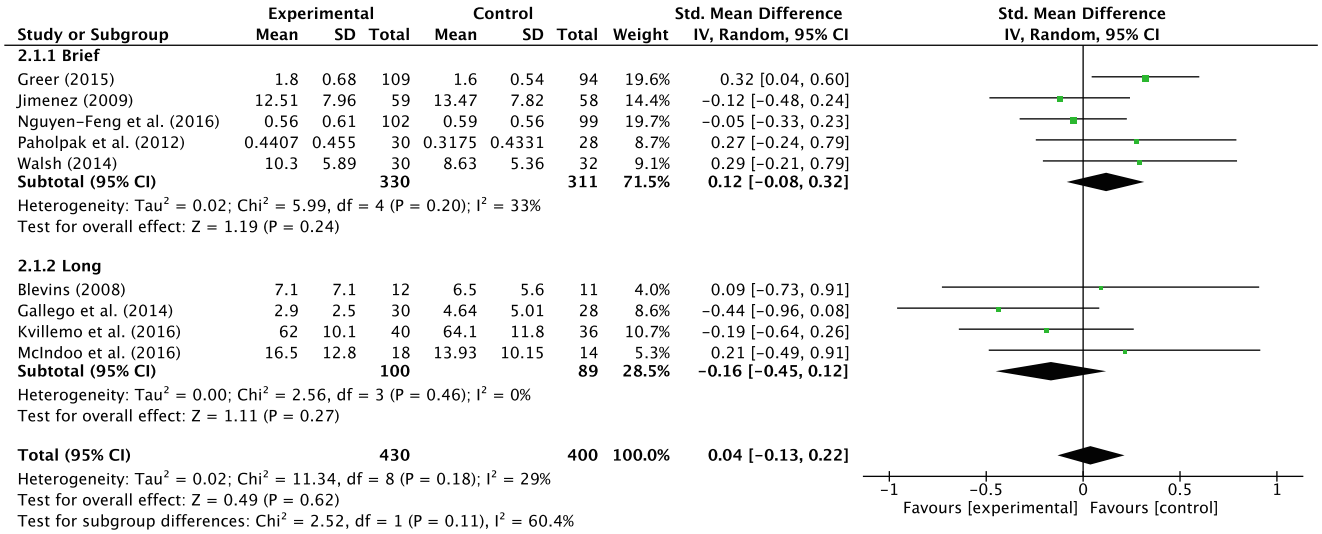

Fig. 4 Forest plot for the meta-analysis of MBIs compared to active control for depressive symptoms at post-intervention, including the study length subgroup analysis

between lower and high risk of bias studies ($p = 0.77$). There was a significant subgroup difference between brief and longer interventions ($p = 0.03$), with longer interventions favoring MBIs and brief interventions favoring active control although both estimates were not significant. There were no significant differences at follow-up for brief interventions (1 month post; one study, 156 participants) or longer interventions (2–3 months post; one study, 23 participants).

One study compared MBI to psychotherapy. Omidi et al. (2013) compared an 8-week traditional MBCT intervention to regular CBT and found no significant differences between groups on anxiety symptoms.

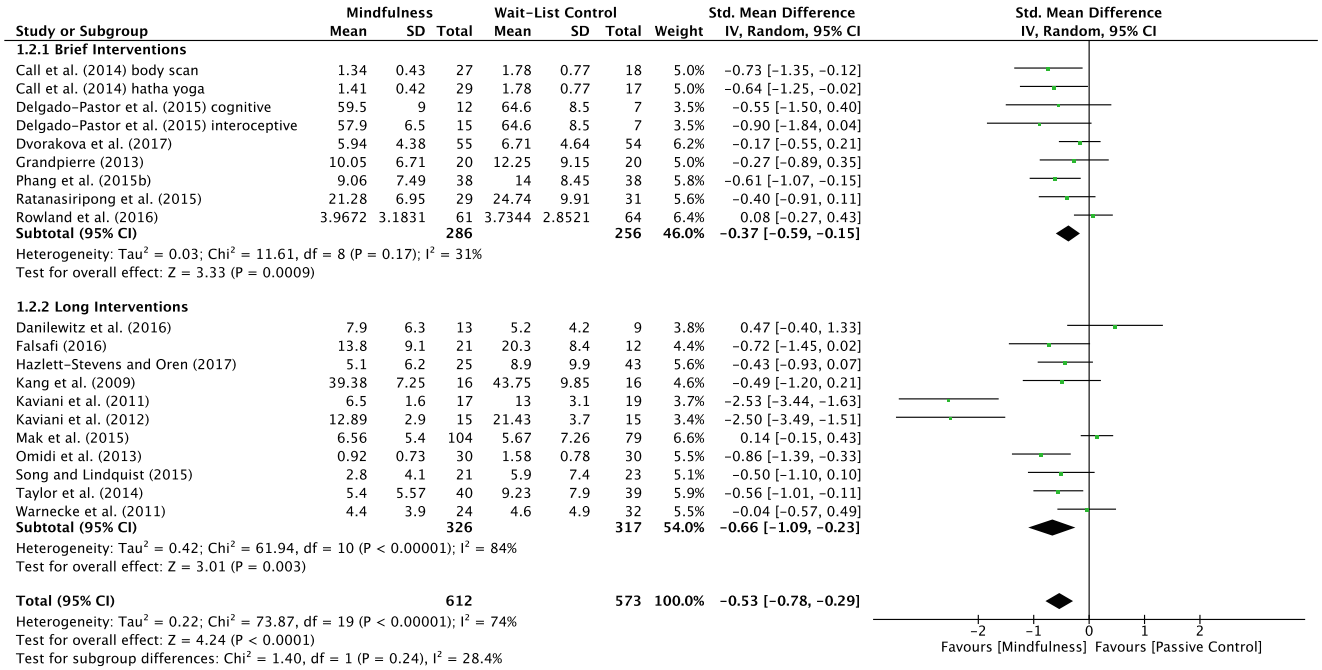

Fig. 5 Forest plot for the meta-analysis of MBIs compared to passive control for anxiety symptoms at post-intervention, including the study length subgroup analysis

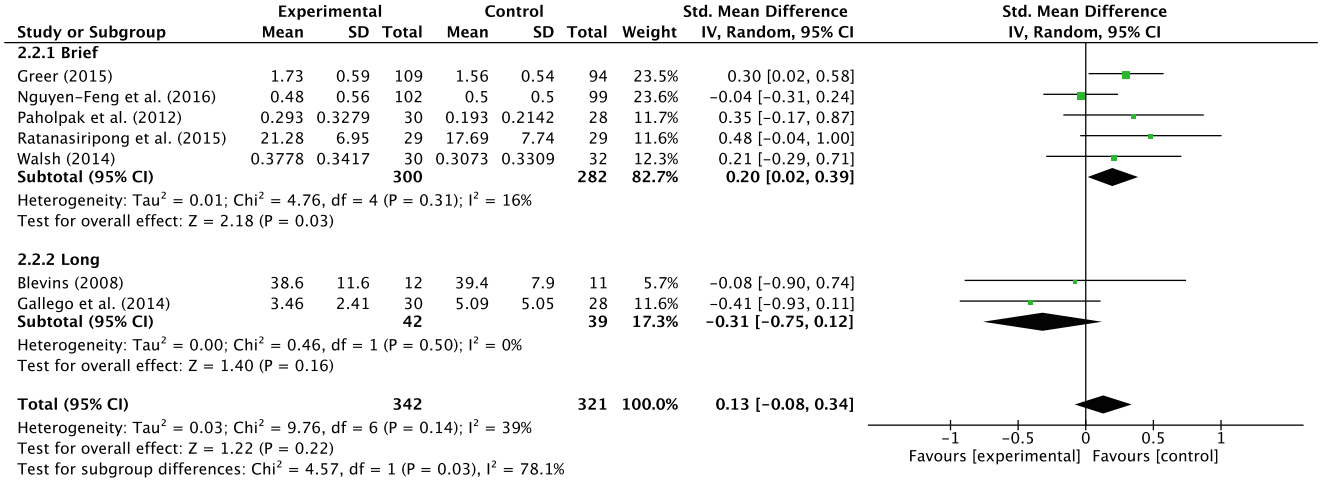

Fig. 6 Forest plot for the meta-analysis of MBIs compared to active control for anxiety symptoms at post-intervention, including the study length subgroup analysis

Perceived Stress

Pooling data from 23 studies (1643 participants) comparing MBIs to passive control, we found a small significant reduction in perceived stress (SMD − 0.39 (95% CI − 0.50, − 0.27; PSS − 2.4 (95% CI − 3.1, − 1.7)) (see Fig. 7). Heterogeneity was negligible according to an I^2 of 14% ($p = 0.27$). This meta-analysis did not include any studies utilizing MBCT. There was no significant difference between brief (SMD = − 0.42 (95% CI − 0.55, − 0.29)) and long (− 0.36 (95% CI − 0.56, −0.17)) interventions ($p = 0.64$). We were unable to include three studies in the meta-analysis due to lack of access to scores, but single-study results were consistent with the pooled effects. Burger (2015), who conducted a 4-week MBI, found the MBI group had significantly lower scores post-intervention in comparison to the control group ($n = 52$). Gallego et al.

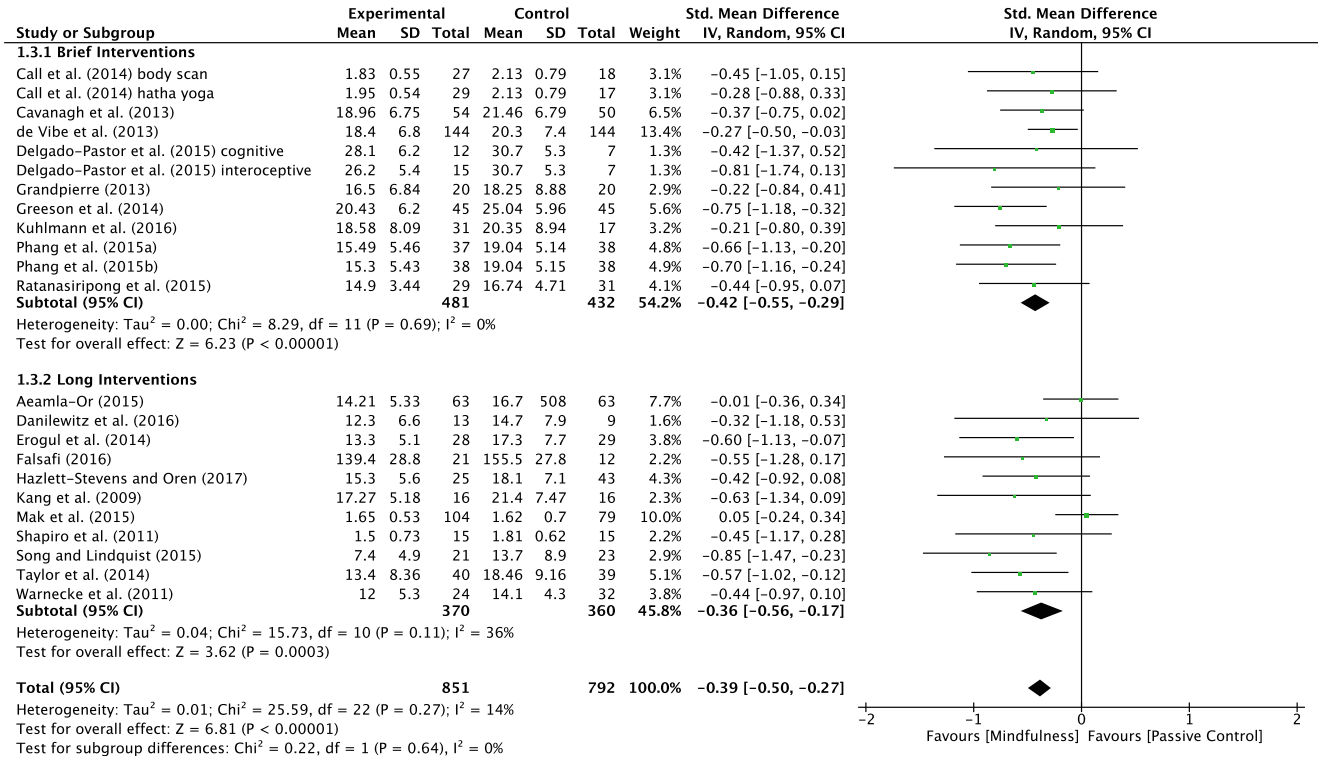

Fig. 7 Forest plot for the meta-analysis of MBIs compared to passive control for perceived stress at post-intervention, including the study length subgroup analysis

(2014), in an 8-week MBCT intervention, also demonstrated reductions in stress post-intervention in comparison to control. Oman et al. (2008) conducted an 8-week MBSR intervention and found significant reductions in the MBSR group in comparison to waitlist control ($n = 47$).

Significant, but imprecise, results from a single study depicted sustained reductions in stress 1 month post-intervention (one study, 33 participants; MD Student Life Stress Inventory −23.10 (95% CI −41.57, −4.63)). Significant and meaningful effects did not remain at 2–3 months post-intervention (three studies, 339 participants), at 4–5 months (one study, 126 participants), 6 months (two studies, 132 participants), or at 1 year (two studies, 78 participants).

Pooling data from six studies (605 participants) comparing MBIs to active control, no difference in effect was found for perceived stress (SMD −0.08 (95% CI −0.32, 0.16)). The I^2 was 47% ($p = 0.09$). Length of interventions appears to account for heterogeneity between studies (32% brief, 19% long), but there was no significant subgroup effect ($p = 0.13$) (see Fig. 8). There was no significant difference found due to study quality ($p = 0.92$). Effects did not persist at 1 month (one study, 63 participants) or 1 year (one study, 63 participants).

Sleep Parameters

Pooling data from two studies (199 participants) comparing MBIs to passive control, we found no evidence of effect on sleep (SMD −0.35 (95% CI −0.78, 0.08)). Heterogeneity was moderate to high (I^2 56%), but this was not explored due to there only being two studies for this analysis and both studies were brief MBIs and high risk of bias. There were no follow-up measures and no studies comparing MBIs to active controls or psychotherapy.

Substance Use Frequency Two studies assessed substance use frequency. Dvorakova et al. (2017) ($n = 109$) compared MBI to passive control and found a trend toward a reduction in the number of drinks per week in the MBI group (MD −2.61 (95% CI −9.06, 4.14)), a reduction in the number of drinks consumed when most drunk (MD −2.53 (95% CI −4.89, −0.17)), and a reduction in alcohol-related problems (MD = −1.72 (95% CI −2.93, −0.51)). Davis et al. (2013) ($n = 55$) found the MBI compared to active control was significantly associated with a greater number of days abstinent from smoking tobacco (MD 3.06 (95% CI 0.41, 5.71)) and higher odds, although not statistically significant, of being abstinent for 2 weeks (OR 6.00 (0.67, 53.68)). Any reduction in substance use is considered clinically relevant when taking a harm reduction approach (Erickson et al. 2002).

Emotion Regulation

Two studies addressed emotion regulation, although with different comparators inhibiting pooled effects. Neither Grandpierre (2013) ($n = 40$) nor Walsh (2014) ($n = 61$) found evidence of differential effects between MBIs and active or passive controls.

Daily Practice, Continued Practice, and Booster Sessions

MBIs typically aim to build mindfulness practice as a habit through the course of the intervention. Most interventions indicated encouragement of home practice. Twenty-four

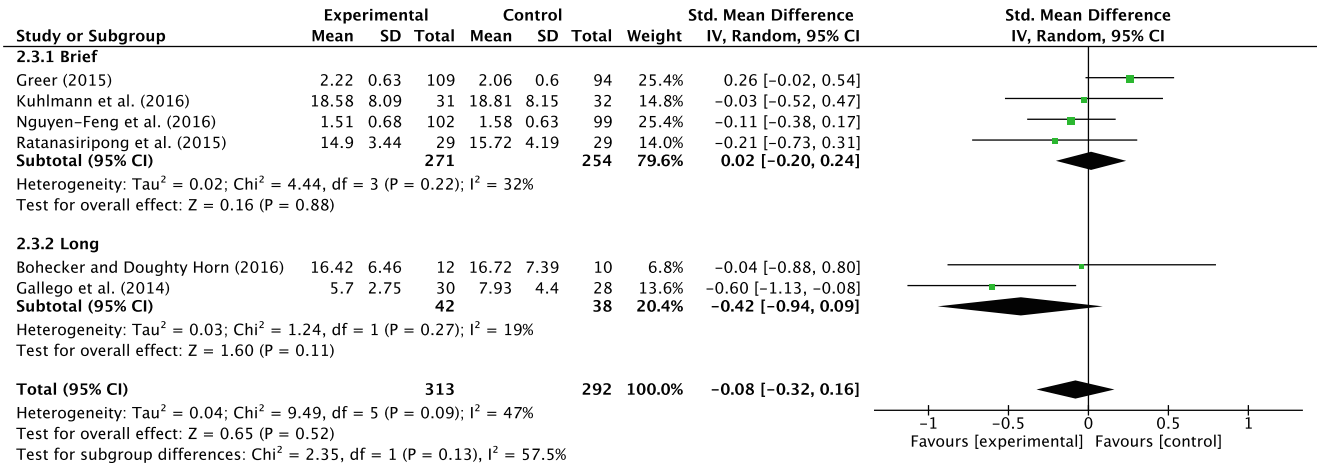

Fig. 8 Forest plot for the meta-analysis of MBIs compared to active control for perceived stress at post-intervention, including the study length subgroup analysis

interventions (59%) had expectations of home practice ranging from 3 to 5 min to 1 h per day. However, compliance was inconsistently measured and reported, and even if compliance was reported, there was rarely a pre-determined compliance requirement. Only one study had excellent daily compliance (Paholpak et al. 2012) as the intervention was done in-person before class. There were no subgroup differences for interventions with daily practice compared to no daily practice for outcomes of anxiety, depression, or stress compared to passive or active control post-intervention ($p > 0.1$), although these results should be interpreted with caution due to poor reporting and compliance. Most studies did not measure continued practice post-intervention; however, in one study, 80% of participants reported they were still engaging in mindfulness practice at 10 weeks post-intervention with 57.5% reporting practicing at least once a week (Taylor et al. 2014). This being said, the relatively poor homework compliance during the intervention may be indicative of failure to form habitual practices. Additionally, included studies did not provide booster or drop in sessions following the interventions. However, in studies that measured mindfulness at follow-up, it appears mindfulness traits remain elevated in MBI groups at 4 weeks (Falsafi 2016; Greer 2016) and 2 months (Shapiro et al. 2011) post-intervention.

Quality of the Evidence

The evidence included in these meta-analyses are of very low to low quality. For MBIs compared to passive or active controls on post-intervention outcomes of depression, anxiety, and perceived stress, the pooled effects are of low quality. For these studies, quality was consistently downgraded for risk of bias and publication bias. Risk of bias was primarily high due to (1) performance bias and detection bias, from a lack of ability to blind the intervention and self-report nature of the outcomes; (2) lack of clarity around allocation concealment procedures; and (3) high attrition. As per publication bias, it appears that smaller studies with results that favor the control condition are not being published. For all other outcomes and comparators, including follow-up results, comparisons to psychotherapy, and other outcomes, the evidence is of very low quality. In addition to risk of study bias and risk of publication bias, these effects were based on small sample sizes and at times single studies (below optimal information sizes) that contributed to inconsistency in results.

Discussion

There has been a recent interest in the effect of MBIs on mental health outcomes, particularly in post-secondary students due to the high need and lack of availability of appropriate resources. The results of this meta-analysis indicate that, in post-secondary students, MBIs appear to produce small to moderate reductions in symptoms of depression, anxiety, and perceived stress post-intervention when compared to passive control. Results were similar for shorter versus longer interventions; however, studies using MBCT appeared to produce larger effect sizes for depression and anxiety symptoms when comparing to passive control. The results do not suggest differential effects of MBIs compared to active comparators for these outcomes. These non-differential effects may reflect a shared effect between MBIs and other active comparators. For example, any intervention (MBI or active comparator) involves some degree of behavioral activation through the act of scheduling and participating in some form of activity, which is known to be effective for improving mental health (Ekers et al. 2014). Importantly, back-transformations revealed that although the effect estimates may appear to be beneficial when using the SMD based on low (0.2), medium (0.5), and high (0.8) cut-points (Higgins and Green 2011), these SMDs may not reflect the same magnitude in potentially clinically relevant changes and, therefore, most effect estimates may actually reflect clinical uncertainty. Interpretations of findings for other outcomes, follow-up points, and comparators are limited due to few existing studies and, therefore, small sample sizes.

This study presents a current summary of the mental health effects of short and long MBIs for post-secondary students. We included a broader student population than the most recent review (McConville et al. 2016) and included a number of new studies that have not been published in previous reviews on mindfulness for post-secondary students. This review additionally included a subgroup analysis on the length of the intervention, which is critical due to students' perception of limited time to access resources and short duration of school terms or semesters. Comparing to MBI reviews in general, the effect estimates in this review were typically smaller than previous systematic reviews comparing MBIs to passive control for depressive symptoms (slightly smaller (Hofmann et al. 2010; Klainin-Yobas et al. 2012; Khoury et al. 2013; McConville et al. 2016; Zainal et al. 2013); much smaller (Khoury et al. 2013; Regehr et al. 2013)), anxiety symptoms (slightly smaller (Hofmann et al. 2010); much smaller (Hofmann et al. 2010; Khoury et al. 2013; Regehr et al. 2013; Zainal et al. 2013)), and perceived stress (Zainal et al. 2013). This attenuation of effect was expected due to the heterogeneity in the existing literature around inclusion criteria, specifically regarding the inclusion of observational studies. McConville et al.'s (2016) review is the most similar to this review in terms of population criteria; McConville's review focuses solely on health professional students and found a similar effect for depression (SMD = − 0.54; 95% CI − 0.83, − 0.26), anxiety symptoms (SMD = − 0.44; 95% CI − 0.59, − 0.28), and stress (SMD = −0.44; 95% CI − 0.57 to − 0.31). When comparing MBIs to active control, our results were much lower than existing effects for both depression and anxiety (this study included different active controls) (Hofmann et

al. 2010). Lastly, these results substantiate the hypotheses from Carmody and Baer (2009) that shorter interventions may be just as effective as longer traditional interventions. Importantly, previous reviews have not back-transformed SMDs to facilitate interpretation of clinically meaningful differences. Therefore, the current presentation of effect sizes in the existing literature may result in misinterpretation of the clinical relevance of effects.

Overall, our findings suggest that, in general, MBIs of at least 2 weeks in duration appear to be a better alternative than no intervention for students, particularly for reducing symptoms of depression, anxiety, and perceived stress. When comparing to no intervention, traditional MBCT appears to be the most effective for symptoms of depression and anxiety compared to other MBIs. It is important to note that this review found no significant difference between shorter and longer interventions (apart from MBCT), and therefore shorter interventions may provide feasible, brief, and effective strategies for reducing student anxiety, depression, and perceived stress. These findings suggest that MBIs may be an appropriate intervention for students who are waiting for counseling services for depression, anxiety, and stress. There is insufficient evidence at this time to evaluate the effectiveness of MBIs in students presenting to health and counseling services for sleep difficulties, substance use problems, or emotion dysregulation or to make recommendations on mindfulness compared to other psychotherapeutic interventions in reducing common mental health concerns among students.

Limitations

It is important to note that the individual studies included in these meta-analyses are of very low to low quality due to risk of bias, publication bias, and at times, inconsistency. As per GRADE, low quality means, "our confidence in the effect estimate is limited: the true effect may be substantially different from the estimate of the effect," and for very low quality, "We have very little confidence in the effect estimate" (Schünemann et al. 2013). Although it may not be feasible to eliminate performance bias, given the nature of the intervention (unless comparing MBIs to active control or another intervention), trialists can seek to reduce bias in future studies in other domains by diligently reporting concealment methods, using a form of deception so participants are unaware of primary outcomes or using diagnostic interviews to reduce detection bias, and making attempts to minimize and adequately explain attrition. Additionally, researchers and journals need to be aware of the risk of publication bias and to address this by publishing all high-quality studies, including negative trials. Also, small pooled sample sizes contribute to inconsistency in results, highlighting the need for more trials performing such analyses (e.g. MBIs compared to active controls, MBIs compared to psychotherapy, and MBIs for substance use, sleep, and emotion regulation).

Additional limitations in the current review include missing studies, inability to determine clinical relevance, limited number of subgroup analyses, and no measurement of adverse events. This review did not completely retrieve all identified studies due to inability to access articles and inability to extract subscale scores resulting in missing studies and outcome data. Another significant limitation, outside the control of this review, was the lack of established MIDs for clinical interpretation of effects. There are also many other potential contributors to effective mindfulness interventions that this review did not capture, including severity of symptoms at baseline, concurrent psychotherapy/pharmacotherapy, and if the intervention was manualized or guided. Homework and daily practice compliance may also be a potential contributor to the effectiveness of an MBI. Although this review attempted to address compliance, and many interventions expected and encouraged home and/or daily practice, compliance was rarely measured or reported. As many interventions involve in-person sessions only once per week, much of the mindfulness practice may be expected outside of these sessions. This limits our ability to determine what components of MBI protocols are helpful and effective for post-secondary students. Therefore, appropriate measurement and reporting of compliance with personal practice outside of structured sessions is critical for future studies. We also did not measure any adverse effects in our review, as this is not typically considered a concern with MBIs; however, high attrition in included studies points to a need to explore the reasons for not completing the intervention, specifically examining student preference for and engagement in MBIs as compared to other pharmacological and non-pharmacological interventions.

Acknowledgments We would like to Thank Dr. Jan Young (McMaster Student Wellness Centre Medical Director), Dr. Mary Fletcher (Family physician at McMaster), and Nikki Carter (Social Worker at McMaster) for providing additional insight into the primary and secondary outcomes for the study.

Author Contributions J.E.H. conceived of and designed the study, screened for included studies, engaged in extraction and verification, interpreted the data, and drafted and revised the manuscript. J.L.D. screened for included studies and engaged in extraction and verification. I.F.M., A.J.C., and I.V. engaged in extraction and verification. N.M. and C.M. are both senior authors on this manuscript and contributed equally. Senior authors listed alphabetically. N.M. provided methods support during study design and analysis. C.M. provided content support in the conception, design, and interpretation of the study. All authors approved the final version of the manuscript for publication and agree to be accountable for all aspects of the work.

Compliance with Ethical Standards

Disclosures J.E.H. and C.M. have participated in a paid research project (grant—Ontario Mental Health Innovation Fund) implementing, analyzing, and disseminating the effects of a brief MBI for post-secondary

students (KORU). Time spent on this review was not compensated. N.M. has received grant support from AstraZeneca, Merck, and Sanofi outside of the submitted work. Other reviewers do not have any conflicts of interest.

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