

The Limited Effect of Mindfulness-Based Interventions on Anxiety in Children and
Adolescents: A Meta-Analysis

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Abstract

Anxiety disorders are common mental health problems among youth with harmful impacts often extending into adulthood. Mindfulness-based interventions (MBIs) have become increasingly popular for addressing mental health issues, particularly in schools, however, it remains unclear how effective they are for reducing youth anxiety. This meta-analysis aimed to evaluate the efficacy and effect moderators of MBIs on anxiety outcomes in children and adolescents. Eligible studies were published randomised controlled trials (RCTs) of MBIs conducted with participants aged 18 years or younger, investigating anxiety outcomes using a well-validated anxiety scale. A systematic search of RCTs published through to February 2019 identified 20 studies for inclusion ($n = 1582$). A random effects model was used to synthesise MBI effects. Stratified meta-analyses as well as individual, random effects meta-regressions were performed to examine how effects varied by age group, intervention setting, control type, research location, and intervention dosage. Although, across all studies, there was a small beneficial effect of MBIs on anxiety post treatment ($d = 0.26$), this was significantly moderated by research location, with RCTs conducted in Iran producing large effects ($d = 1.25$), and RCTs conducted in Western countries demonstrating no significant beneficial effect compared to controls (very small, $d = 0.05$). Effects were non-significant at follow-up assessment points. Post-treatment effects were significant for MBIs conducted with children ($d = 0.41$) and for MBIs when compared to passive controls ($d = 0.33$), but non-significant for adolescents ($d = 0.21$), for MBIs conducted in schools ($d = 0.30$) and in clinics ($d = 0.13$), and when MBIs were compared to active controls ($d = 0.12$). Results suggest that MBIs are likely to have a small to medium, yet temporary effect in reducing anxiety symptoms in children (not adolescents), but among Western youth populations the most likely outcome, from RCTs to date, is that MBIs produce no beneficial effect in anxiety

reduction. Results revealed a lack of evidence to support investment in school-based MBIs to address youth anxiety.

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Fears and anxiety are a frequent, developmentally normative occurrence, and are mostly short-lived during childhood (Beesdo-Baum & Knappe, 2012). However, for some children, symptoms develop into maladaptive anxiety disorders, characterised by excessive and persistent fears, avoidance due to anticipation of threat, and associated disruption in functioning (Beesdo-Baum & Knappe, 2012; Creswell et al., 2020). Anxiety disorders are the most common group of mental disorders affecting children and adolescents, with a worldwide prevalence estimate of 6.5% (Polanczyk et al., 2015), and many individuals have multiple anxiety disorders (Leyfer et al., 2013; Merikangas et al., 2009). There are concerns that youth anxiety is a growing problem, with recent data from Australia (Danchin et al. 2019) and England (Sadler et al. 2018) indicating an increase in anxiety disorder prevalence amongst children and young people. Youth anxiety disorders are associated with impairments in family, social, and academic functioning (de Lijster et al., 2018; Essau et al., 2000; Ezpeleta et al. 2001; Van Ameringen et al. 2003), and predict adult psychiatric conditions (Beesdo-Baum et al., 2013; Lieb et al., 2016) as well as poorer adult functioning across various life domains (Copeland et al., 2014). The wide-reaching negative impact of childhood anxiety, coupled with findings suggesting that most anxiety disorders have their first onset in childhood or adolescence (Kessler et al. 2005) highlight the need for youth-focused interventions, including both treatment and prevention programs.

Currently, cognitive-behavioural therapy (CBT)-based interventions are considered the best established approaches for the prevention and treatment of childhood anxiety disorders (James et al., 2015). However, there has been a growing interest in the use of mindfulness-based interventions (MBIs) to improve mental health outcomes in children and adolescents (Burke, 2010), subsequent to its rise in popularity with adults (Baer, 2003).

Mindfulness refers to a particular way of paying attention to present-moment experience that is intentional and without judgment, but with a sense of openness and acceptance (Bishop et al., 2004; Kabat-Zinn, 2003). Mindfulness practice has been formalized into various MBIs targeting a range of therapeutic benefits, including Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn, 1982), originally developed to help manage chronic pain, and Mindfulness-Based Cognitive Therapy (MBCT; Segal et al., 2002) to help prevent recurring depression. MBCT has been recommended as a psychological intervention for relapse prevention in adults who have experienced multiple episodes of depression (National Institute for Health and Care Excellence, 2009). It has been proposed that mindfulness practice fosters a de-centred, and non-reactive perspective that enables individuals to disengage from automatic, maladaptive response patterns (Shapiro et al., 2006). Therefore, for individuals with anxiety, the increased self-awareness may help by reducing the emotional, fearful reactions typically triggered by anxiety symptoms (Baer, 2003). Additionally, mindfulness practice is thought to be beneficial for anxiety by enhancing attentional self-regulation, which can help shift the attention away from anxiety-inducing thoughts (Semple & Lee, 2011), and by fostering greater tolerance of unpleasant internal states, which may help to reduce avoidance strategies (Bishop et al., 2004) that otherwise work to maintain anxiety.

Importantly, however, some treatment guidelines for anxiety disorders in both adults and children and adolescents, such as the Royal Australian and New Zealand College of Psychiatrists (RANZCP) for the treatment of panic disorder, social anxiety disorder and generalised anxiety disorder (Andrews et al., 2018), and the UK National Institute of Health and Care Excellence (NICE) guidelines for the treatment of social anxiety (NICE, 2013) either do not include mindfulness-based approaches or explicitly list MBI amongst treatments that are *not* recommended. These guidelines suggest there is still insufficient evidence to conclude that MBI is effective for the treatment of anxiety disorders and highlight the need

for further investigation. Notably, meta-analytic research subsequent to the publication of the above guidelines found that across RCTs conducted with adult psychiatric populations, for anxiety symptoms, mindfulness outperformed no treatment control conditions with a large effect size ($k = 8$; Cohen's $d = 0.89$), but was not significantly better than other active treatments (Goldberg et al., 2018).

In children and adolescents, MBIs have been used as school-based prevention programs for universal (e.g., Johnson et al., 2016) and selected students (e.g., Esmaeilian et al., 2018), and in treatments for medical or psychiatric populations (e.g., Biegel et al., 2009). However, the resulting empirical evidence for MBI efficacy in reducing anxiety symptoms among children and young people is inconsistent. For example, significant beneficial effects of MBIs on trait anxiety outcomes have been found in both universal (Ricarte et al., 2015) and selected (Esmaeilian et al., 2018) school-based prevention programs. However, two large-scale universal school-based MBIs (Johnson et al., 2016, 2017) observed increased anxiety symptom scores in the mindfulness group after intervention, which in one study (Johnson et al., 2016) reached statistical significance at follow-up for several participant subgroups. Similarly, among treatment-seeking psychiatric patients, some MBI trials have reported significant improvements in state and trait anxiety (Biegel et al., 2009) while others found significant reduction in state anxiety but no significant change in trait anxiety levels (Díaz-González et al., 2018).

To date there have been several meta-analyses examining MBI effects in children and adolescents. However, the majority have reported composite effect sizes that combined anxiety with other outcomes thereby failing to isolate anxiety-specific effects. For example, Zoogman et al. (2015) found a significant small-to-moderate effect size for 'psychological symptoms' (e.g., anxiety, depression) based on 15 studies that included both controlled and uncontrolled designs (Hedges' $g = 0.37$). Furthermore, Klingbeil et al. (2017) synthesized

treatment effects from studies published prior to January 2016, and found a significant small-to-moderate effect in the outcome domain of internalising problems (comprising both anxiety and depression measures) in controlled studies ($k = 29$; $g = 0.39$), but a smaller effect in pre-post designs ($k = 17$; $g = 0.26$). More recently, Dunning and colleagues (2019) found a small significant effect on ‘anxiety and stress’ (including stress, trait anxiety, state anxiety, and self-esteem), based on 20 RCTs (Cohen’s $d = 0.16$) including 9 studies with active controls ($d = 0.18$.) Several meta-analyses have specifically examined the implementation of MBIs in school environments, but these have also reported effect sizes on composite outcomes rather than anxiety-specific effects. Zenner et al. (2014) found a small non-significant effect in comparison to controls for ‘emotional problems’ (e.g., anxiety, depression, somatic reactions, emotion regulation difficulties etc; Hedges’ $g = 0.19$). Carsley et al. (2018) found a small significant effect compared to controls for all mental health and wellbeing outcomes ($g = 0.24$), but this became non-significant at follow up. Maynard et al. (2017) found a small significant effect of MBI on socio-emotional outcomes (e.g., anxiety, stress, engagement, social skills etc; $g = 0.22$) based on 35 controlled studies.

From across these meta-analyses the specific effect of MBI on anxiety symptoms alone remains unclear. To date only two meta-analyses have separately analysed anxiety-specific outcomes in children and adolescents. Borquist-Conlon et al. (2019) reviewed English language controlled trials of MBI and associated interventions [e.g. Acceptance and Commitment Therapy (ACT; Hayes et al., 2011)] with youth meeting criteria for an anxiety disorder. On the basis of five studies ($n = 188$), they found a moderate and significant effect on trait-like anxiety symptoms (Hedges’ $g = 0.62$). The meta-analysis by Kallapiran et al. (2015) included RCTs published up to January 2014 and examined MBI effects on anxiety symptoms in both clinical and nonclinical samples of children and adolescents up to 20 years of age. The authors reported large significant effects on anxiety symptoms compared to

passive controls for both MBSR/MBCT (Hedges' $g = 0.96$) and other MBIs ($g = 0.87$).

However, the sample size in both arms was very small with three and two studies respectively. The review also extracted large effects from studies with multiple contradictory anxiety outcomes (Semple et al, 2010; Sibinga et al., 2013), thereby potentially over-estimating the overall effect. Additionally, inspection of study-level data suggested the authors included state anxiety measures (Liehr & Diaz, 2010). It can be argued that state anxiety, a situational measure of how the participant feels in the moment, does not accurately reflect stable anxiety levels and would rarely be considered to be a meaningful intervention outcome, hence should not be included when analysing effects on anxiety symptoms and disorders.

Given the recent growth in the publication rate of MBI-related studies (for example, see Dunning et al., 2019), the current meta-analysis aimed to advance our understanding of the efficacy of MBIs on anxiety symptoms and trait anxiety outcomes in children and adolescents by addressing the methodological limitations of previous meta-analyses in the following five ways. First, non anxiety-specific outcomes were excluded from the effect size estimates to isolate the effect on anxiety. Second, data synthesis used only well-validated measures of anxiety that reflected general anxiety symptoms or trait anxiety levels rather than transient, situational anxiety. While mindfulness may be used to target state anxiety, the purpose of the current study was to understand its effects on more general and persistent anxiety symptoms. Third, tightly defined criteria for what constituted an MBI were employed to minimise the impact of intervention components unrelated to mindfulness practice. Fourth, studies were limited to RCTs as the gold standard of intervention efficacy research (Hariton & Locascio, 2018). Finally, as MBIs have been conducted both with clinical and general (school) populations, the meta-analysis included both clinical and non-clinical samples to examine effects regardless of symptom severity.

Given the apparent inconsistency of results, the current meta-analysis sought to explore potential effect moderators. One such factor may be the developmental stage of the individual (Roeser & Pinela, 2014). Age has been explored as a moderating factor in past meta-analyses; older adolescents were found to benefit the most from school-based MBIs on a range of mental health outcomes (Carsley et al., 2018) and older age was found to have a significantly positive association with executive functioning improvements, although, notably, it was not a significant moderator in anxiety/stress outcomes (Dunning et al., 2019). Whether the participant age group moderates MBI effects on anxiety-specific outcomes has yet to be assessed, and an examination of this factor would be useful to determine whether children or adolescents benefit most from MBIs.

A second potential moderating factor is the intervention setting. A distinction can be drawn between MBIs that address nonclinical populations in schools, largely as preventative interventions, and MBIs targeting medical or psychiatric populations seeking treatment in clinical settings (e.g. Biegel et al., 2009; Chadi et al., 2016). While a previous meta-analysis (Klingbeil et al., 2017) found no moderating impact of setting, this related to overall treatment effects for all measured outcomes, not anxiety-specific outcomes. Given meta-analytic findings suggesting MBIs have relatively large effects on clinical populations, both in youth (Borquist-Conlon et al., 2019) and adults (Goldberg et al., 2018), it would be beneficial to examine whether MBI effects differ between school and clinic-based settings across our study sample.

The type of control group used may also potentially moderate MBI effects on anxiety. Indeed, control group characteristics have been shown to moderate results in the adult literature, with ‘no treatment’ controlled studies (e.g., waitlist) yielding larger benefits than studies comparing MBIs with active treatments (Goldberg et al., 2018). The majority of MBI studies with youth have used passive controls such as waitlist or no intervention. But some

studies (e.g., Sibinga et al., 2013) used active attention comparisons to disentangle benefits attributed to the mindfulness component of the intervention from non-specific components. Contrary to the adult literature, Dunning et al. (2019) found negligible MBI effect differences between studies using passive ($d = 0.16$) and active ($d = 0.18$) controls, however this was for anxiety and stress combined. Whether comparison group type moderates the MBI effects on anxiety-specific outcomes warrants examination.

The country where the MBI is held could also affect its efficacy. Many MBI studies conducted in Western countries found non-significant improvements in anxiety symptoms (e.g., Chadi et al., 2016; Johnson et al., 2016), however several studies conducted in non-Western countries found significant improvements with relatively large effects (e.g., Dehghani et al., 2014; Langer et al., 2017). Cultural factors are thought to influence anxiety expression (Hinton, 2012; Hofmann & Hinton, 2014) and there is some evidence for cultural differences in the conceptualisation of mindfulness (Christopher et al., 2009; Haas & Akamatsu, 2019). Hence cultural factors may influence the extent that MBIs are effective for anxiety reduction. Moderator analysis could help determine whether there is any systematic difference in MBI effects between countries.

Intervention duration and total time spent training were identified as further potential moderating factors, drawing on research suggesting the presence of a potential dose-response relationship in mindfulness interventions (see Creswell, 2017). Past youth-focused meta-analyses of MBIs found that intervention dosage was unrelated to overall treatment outcomes (Klingbeil et al., 2017) or to effects on anxiety/stress combined (Dunning et al., 2019), but this has yet to be explored for anxiety-specific outcomes. Finally, it was deemed important to assess whether the meta-analysis findings were impacted by study quality.

In summary, this meta-analysis was guided by the following research questions:

1. Across studies, what is the effect of MBIs on anxiety symptoms and trait anxiety in

children and adolescents, compared to controls?

2. Are any beneficial effects of MBIs on anxiety symptoms and trait anxiety evident beyond the end of the intervention?
3. How do MBI effects on anxiety symptoms differ according to (a) age (i.e., preadolescent children vs. adolescents), (b) setting (i.e., schools vs. clinics), (c) control condition (i.e., passive vs. active), (d) country/region; (e) dosage, (f) study quality?

Method

This meta-analysis followed the guidelines specified in the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) statement (Moher et al., 2009) and the protocol is registered in the international prospective register of systematic reviews (PROSPERO; 31/08/2019).

Inclusion Criteria and Search Strategy

To qualify for inclusion each study had to meet six criteria.

1. The study design had to be a randomised or clustered randomised controlled trial (RCT) such that participants or groups (e.g. school classes) were randomly allocated to either the MBI or a control condition. Studies were limited to RCTs as these represent the gold standard for evaluating the efficacy of an intervention (Hariton & Locascio, 2018) and are designed to control for potential confounding effect of maturational changes.
2. The participants had to be aged 18 years or younger.
3. The intervention had to be described explicitly by the study author(s) as a mindfulness-based intervention and administered over more than one session. Mindfulness practice had to be the core component of the intervention and make up more than half of the intervention sessions. Established interventions that

incorporate mindfulness as a sub-component, such as Acceptance Commitment Therapy (Hayes et al., 2011) and Dialectical Behaviour Therapy (Linehan, 1993), were excluded, as were other established forms of meditation, such as Transcendental Meditation (Travis & Shear, 2010). This definition of MBIs is consistent with the criteria used in previous meta-analyses performed by Dunning et al. (2019) and Klingbeil et al. (2017).

4. The study had to use validated, reliable measures of trait anxiety or anxiety symptoms. Any state-anxiety scales measuring how participants feel “right now” were excluded as the meta-analysis was interested in the effect of MBIs on reducing underlying anxiety symptoms and trait anxiety, not situational anxiety. The criteria for a validated measure of anxiety were based on the International Society for the Quality of Life Research (ISOQOL) recommended minimum standards for patient-reported outcome measures (Reeve et al., 2013), adapted for the purposes of the meta-analysis. If a scale did not meet the inclusion criteria based on available published evidence it was excluded from the meta-analysis.
5. The study had to report sufficient quantitative data to compute the effect size as the relative change in anxiety symptoms from pre- to post-intervention. If there were insufficient data, the authors were contacted for the required information and if they were unable to provide the data, the study was excluded.
6. To ensure a minimum data quality standard, only published peer-reviewed articles of any language were included. Dissertations and conference abstracts were excluded.

A systematic database search was performed in February 2019 to identify relevant studies, using a combination of search terms relating to the intervention (mindful* OR MBCT OR MBSR OR "learning to breathe" OR meditat*), anxiety (anxi* OR internali*) and

target sample (child* OR adolescen* OR youth* OR teen* OR classroom* OR school*). The search was limited to peer-review articles in databases that provided this filter. The final search date was February 13, 2019. The electronic databases used in the search were PsycINFO, Medline, Web of Science Core Collection, EMBASE, Cochrane, Academic Search Premier, Psychology and Behavioral Sciences Collection, CINAHL, Education Research Complete (ERC), and Education Resources Information Centre (ERIC). To verify the database search strategy a second independent coder used the same search terms in the Web of Science Core Collection and Cochrane databases in May 2019, and successfully retrieved the same articles as the first coder. Additional studies were found by inspecting the reference lists of eight previous meta-analyses in the field (Borquist-Conlon et al., 2019; Carsley et al., 2018; Dunning et al., 2019; Kallapiran et al., 2015; Klingbeil et al., 2017; Maynard et al., 2017; Zenner et al., 2014; Zoogman et al., 2015). After collating the search results and removing duplicates, a reviewer screened the titles and abstracts of identified articles ($n = 1932$) and excluded any articles that did not meet the inclusion criteria based on their title and abstract information. A second reviewer screened ten percent of the identified articles ($n = 194$). There was disagreement regarding inclusion or exclusion at the abstract screening for 5 (out of 182) studies. All of these 5 studies were subsequently excluded at the full text screening by the reviewers, indicating full agreement about inclusion in the final pool. A full-text screen was conducted for the remaining studies ($n = 252$), resulting in 20 studies qualifying for inclusion. A second reviewer reviewed 10% of studies included for full-text screening, with 100% agreement on study inclusion. See Figure 1 for the PRISMA flow diagram of the study selection process.

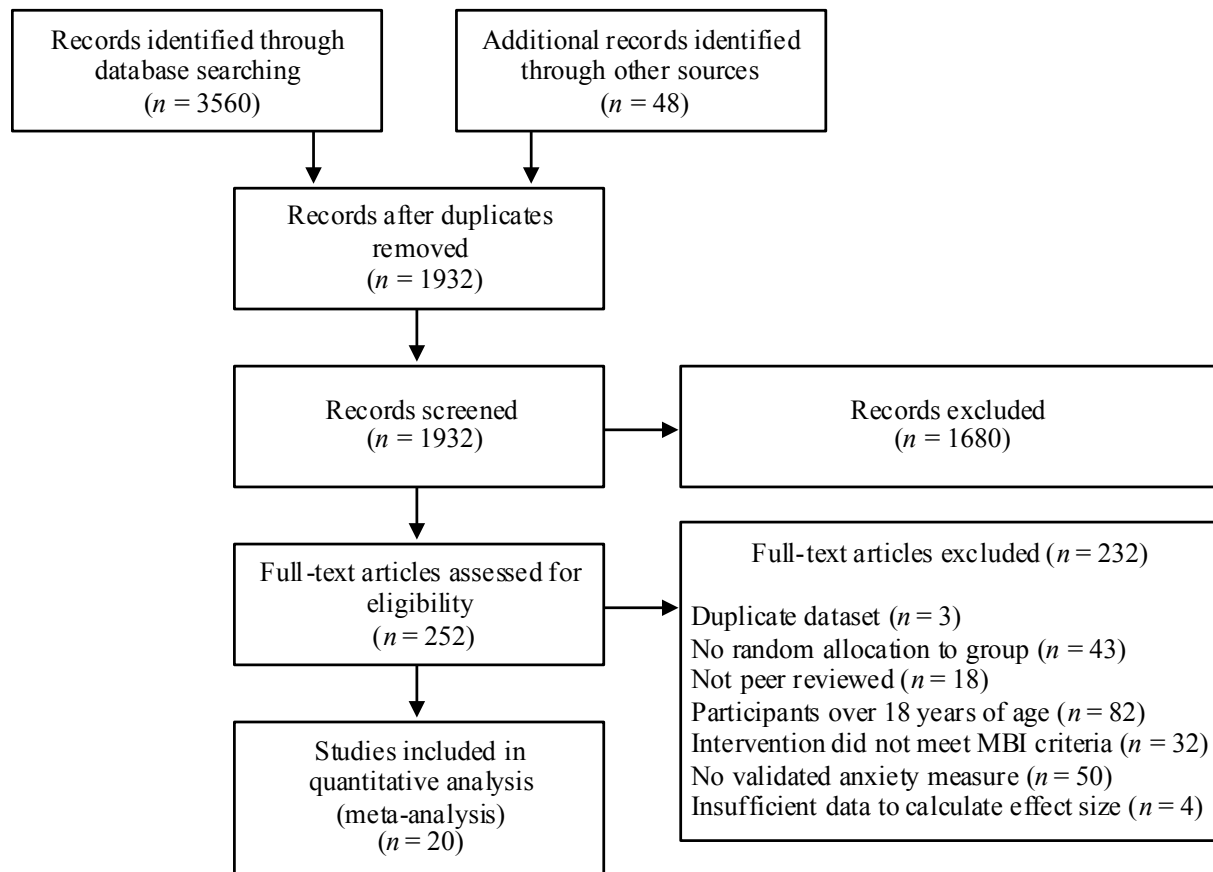


Figure 1. PRISMA flow diagram of study selection process (Moher et al., 2009)

Data Extraction

The data extracted from each selected study included items relating to publication (e.g. journal name, publication year, country), participant characteristics (e.g. number of participants, age range, clinical status), intervention characteristics (e.g. name of MBI program, setting, training period in weeks, total minutes of formal training), study design (e.g. type of control, outcome measure) as well as information required to calculate the standardised effect size (e.g. pre- and post-intervention means and standard deviations, F -, t -, or p -values). If information was missing the authors were contacted for clarification.

Each study was classified according to categorical variables that were identified as potential moderators of the effect size and used in subsequent stratification and meta-regression analyses. These were (a) age group (adolescent or child), (b) intervention setting

(school or clinic), (c) type of control (passive or active), and (d) country/region of research. School level was used as a proxy for age group, such that primary school samples were classified as children and middle or high school samples were classified as adolescents. If school information was not reported samples with participants between the ages of 12 and 18 years were classified as adolescents, and those with participants under 12 years of age as children. There were six studies in which the age range spanned these categories. Three of these studies were easily categorised into the children category because the children were all in primary school (Lam (2016) 9-13 yo; Andreotti et al (2017) 8-12 yo; Ricarte et al (2015) 6-13 yo). Two studies were categorised as ‘adolescents’ because all the participants were in middle school or above (Sibinga et al. (2013) 11-14 yo; Esmailian et al. (2010) 10-13 yo). Finally, one study (Semple et al., 2010) spanned the age range and included students from primary and middle school (i.e., grades 3-7). As most participants (18 out of 25) were in Grade 5 and below (i.e., primary school), this study was classified it as ‘children.’

Studies were categorised according to whether the intervention setting was a school or a clinic. Two studies were unable to be classified in this way as one study conducted treatment in the child’s home and the other in an unspecified setting, and were therefore excluded from the stratification by setting. Studies were classified according to whether the design included a passive control (waitlist control, no-treatment control) or an active control (treatment-as-usual, active intervention, attention placebo controls). Lastly, the country of research was used to classify the studies into the following regions: North America (containing USA and Canada), Europe (containing France and Spain), Australia, and Iran. A study from Hong Kong and a study from Chile could not be classified into a meaningful stratum of more than two studies; hence they were omitted from the stratification by region.

To assess the reliability of data extraction, a second coder independently coded five randomly selected studies, which represented 25% of the study sample. This was completed

for all variables included in the stratification analysis and meta-regression. Cohen's κ for agreement between coders for the categorical data were as follows: age group, $\kappa = 1.00$, $p = .025$; intervention setting, $\kappa = 1.00$, $p = .002$; type of control, $\kappa = 1.00$, $p = .025$; country/region, $\kappa = 1.00$, $p = .025$. Intra-class correlations (ICC) were obtained to evaluate the reliability of continuous data using an absolute agreement model. Because only the first coder's scores were used in the final analyses, the single measure ICC is reported. ICC were: number of participants, ICC = 1.00; training period, ICC = 1.00; total minutes of formal training, ICC = 1.00. Results suggested excellent reliability of data extraction. Details of included studies, including important variables and categories in which they were placed according to the above classification, are summarised in Table 1 for school-based trials and Table 2 for trials based in clinics and other settings.

Risk of Bias Assessment

Because all studies were RCTs, study quality was assessed using the Cochrane Collaboration's Risk of Bias (ROB) tool (Higgins & Green, 2011). Each study was rated for ROB across six factors: (a) method of random sequencing used in participant allocation to condition, (b) concealment of participant allocation to condition prior to assignment, (c) blinding of outcome assessment, (d) the extent and handling of missing outcome data, (e) selective reporting of outcomes, and (f) extent of researcher allegiance to the intervention. Each item for each study was assessed in one of three ways: low ROB, high ROB, or unclear ROB, the latter generally selected if there was insufficient information to permit judgment of low or high risk. These judgments were based on the criteria specified by the Cochrane ROB tool, with some modification. As per the recommendation of Corbett et al. (2014), selection bias due to randomisation and allocation concealment was judged to be low risk if there was evidence of baseline equivalence between groups on the outcome variables. Conversely, they were judged high risk if there was evidence of baseline imbalance. Additionally, blinding of

participants was excluded from the assessment so as not to penalise the majority of studies that were waitlist controlled, as participant blinding is generally not possible in these designs¹. Each low risk item was given a value of 0, unclear risk a value of 1, and high risk a value of 2. Individual items were summed for each study to give a total ROB score for that study, with high scores indicating a higher ROB, thus a greater threat to validity (see Tables 1 and 2). ROB scores independently assigned by a second coder for 25% of the study sample showed a high level of agreement with those of the original coder, $ICC = .80$, $p = .03$, based on a single-measures absolute agreement model. All analyses used assessments of the original coder. A meta-regression analysis was performed with the ROB score as a predictor to determine whether the effect size differed significantly by study quality.

Estimation of Effect Sizes

The effect size was based on the between-group comparison of the mean change in anxiety from pre- to post-intervention. This was consistent with the approach used in Dunning et al. (2019) and based on the recommendation of Morris (2008) and Carlson and Schmidt (1999). Effect sizes were interpreted using Cohen's (1988) suggested reference values of .20, .50, and .80 as small, medium, and large effect sizes, respectively. However while values of d in the range 0.2-0.5 are considered small in a generic sense, they are quite substantial in the context of a universal program, and as such this needs to be considered when interpreting effect sizes from universal studies. The calculation of Cohen's d was performed in several ways depending on the available data, using formulas specified by Lipsey and Wilson (2001). Where possible, the effect size was calculated from the reported mean change in anxiety scores from pre- to post-intervention, and their standard deviations. This was completed for 17 of the 20 studies. For one study (Keller et al., 2017), which reported only the pre-post means, the standard deviation was imputed from the published

¹ We also compared the effect sizes across studies that used different types of control groups

relevant population norms, consistent with the recommendations of the Cochrane Collaboration (Higgins & Green, 2011). Two studies did not provide relevant means or standard deviations. In the first instance (Chadi et al., 2016), Cohen's d was estimated from the p -value for the between-group comparison adjusted for baseline score. In the second instance (Sibinga et al., 2013), Cohen's d was based on the study's reported Cohen's d associated with the between-group comparison of change scores, consistent with the Dunning et al. (2019) meta-analysis approach. The Cohen's d for each study was adjusted to correct for a potential overestimation due to smaller sample sizes, using Hedges' formula (Cumming, 2012; Hedges, 1981). This unbiased estimate of Cohen's d was then used to calculate estimates of the standard error of the unbiased Cohen's d , based on formulas stipulated by Lipsey and Wilson (2001).

To ensure independence of effect sizes, only one effect size was extracted from each sample, based on the following decision rules. If a study reported two valid, reliable measures of general anxiety symptoms, the measure yielding the most conservative effect size was selected ($k = 4$). If a study reported both a general measure and a disorder-specific measure, the general measure of anxiety was selected for its similarity with other studies in the meta-analysis ($k = 2$). Lastly, if a study of pre-adolescent children included both parent-report and child-report outcomes of the same scale, the parent version was chosen based on recent findings that for pre-adolescent children, parent-report scores tend to be more consistent with diagnostic outcomes (Evans et al., 2017; $k = 1$). In instances where the study included more than one treatment or comparison group, the group most relevant or similar to other studies was selected for inclusion. For example, in one study that had MBI, waitlist control, and a yoga group, the waitlist control group comparison was selected over the yoga comparison as no other study had a yoga control. A subset of studies also reported outcomes after one or more follow-up periods, which varied from two to six months, and one study (Johnson et al.,

2017) reported results at six months and 12 months post intervention. Separate effect size calculations were conducted for results at the last follow-up, with the exception of the Johnson et al. (2017) study where the 6-month follow-up effect was selected over the 12-month effect due to its similarity with other follow-up periods. See Tables 1 and 2 for the outcome measures included in the meta-analysis and their follow-up periods where relevant.

Data Analysis

All statistical analyses were performed using version 15.1 of the Stata statistical program. Individual effect sizes were pooled using a random-effects model, based on the theoretical perspective that studies varied sufficiently in intervention, design and participant characteristics to assume that they may be estimating different true effect sizes (Borenstein, et al., 2009). A random-effects model allows for both random error and variation in true effect size across studies and is likely to be more appropriate than a fixed effects model when there is considerable between-study variance. The resulting pooled effect size represented an estimate of the mean of the distribution of true effects. The presence of heterogeneity was statistically tested using Cochran's Q -test (Cochran, 1954), and the I^2 statistic was used to estimate the proportion of total variance due to heterogeneity in our sample (Higgins & Thompson, 2002). I^2 values of 25%, 50% and 75% were interpreted as indicating a low, moderate, and large amount of heterogeneity, based on suggestions from Higgins et al. (2003). Potential sources of heterogeneity were investigated through a stratification analysis, which estimated the pooled effect size by age group, intervention setting, control condition, and country/region. Individual random-effects meta-regressions were run to test the statistical significance of the above moderators, and to explore if effect sizes were impacted by the ROB score, training period duration, and total minutes of training.

The risk of publication bias was investigated through a visual inspection of the funnel plot, and formally tested with a series of Egger's regressions for the entire study sample and

for the stratified meta-analyses (Egger et al., 1997). Finally, a trim-and-fill analysis was performed to estimate the number of missing studies in the meta-analysis and adjust the pooled effect size estimate accordingly (Duval & Tweedie, 2000).

Table 1. *Details of studies included in the meta-analysis: School-based trials*

Authors (year)	Country	Sample characteristics	N	Age group	Intervention	Control	Total training minutes	Training period weeks	Synthesised outcome measure (follow-up period)	Effect Size (95% CI)	ROB Score
Ebrahimejad et al. (2016)	Iran	Females with social anxiety disorder	25	Adolescent	MBCT	Passive: NTC	720	8	SPIN	0.59 (-0.21, 1.39)	8
Esmailian et al. (2018)	Iran	Living with divorced parent	83	Adolescent	MBCT-C with minor cultural adaptations	Passive: NTC	1080	12	STAI-C Trait (2 month)	1.72 (1.22, 2.22)	3
Franco et al. (2010)	Spain	All students	61	Adolescent	Meditacion Fluir: breathing meditation, body scan, ACT/acceptance exercises	Passive: WLC	900	10	STAI Trait	-0.23 (-0.74, 0.27)	4
Johnson et al. (2016)	Australia	All students	269	Adolescent	Dot be (Mindfulness in Schools Project): MBSR/MBCT elements adapted for adolescents	Passive: NTC	380	8	DASS-21 Anx (3 month)	-0.06 (-0.31, 0.18)	7
Johnson et al. (2017)	Australia	All students	364	Adolescent	Dot be (Mindfulness in Schools Project): MBSR/MBCT elements adapted for adolescents	Passive: NTC	450	9	DASS-21 Anx (6 month)	-0.14 (-0.35, 0.06)	7
Keller et al. (2017)	USA	Low SES, minority students	28	Child	Mindful breathing, guided mediation, mindful listening and eating	Passive: NTC	270	10	SCAS	0.12 (-0.63, 0.86)	7
Lam (2016)	Hong Kong	Subclinical internalising difficulties	20	Child	MBCT-C adapted to school setting, additional MBSR-based mindfulness exercises	Passive: WLC	720	9	RCADS TA	0.22 (-0.66, 1.10)	4
Langer et al. (2017)	Chile	All students	88	Adolescent	Mindfulness in Schools Project: MBSR/MBCT elements adapted for adolescents	Passive: WLC	360	8	DASS-21 Anx (6 months)	0.83 (0.39, 1.26)	3
Moreno-Gomez and Cejudo (2019)	Spain	All kindergarten students	74	Child	MindKinder program: mindfulness meditation, visualisation, mandalas, body awareness	Passive: WLC	2160	24	BASC-T1 Anx (6 months)	0.61 (0.12, 1.10)	4
Quach et al. (2016)	USA	All students (majority low SES)	101	Adolescent	MBSR modified for school setting	Passive: WLC	360	4	SCARED	-0.02 (-0.41, 0.37)	6
Ricarte et al. (2015)	Spain	All students (rural setting, majority low SES)	90	Child	Mindfulness Emotional Intelligence Training: breathing meditation, mindful observation and listening, body scan	Passive: WLC	450	6	STAI-C Trait	0.40 (-0.02, 0.82)	6
Sibinga et al. (2013)	USA	All male students (95% African-American)	41	Adolescent	MBSR, adapted for urban youth	Active: Health education	600	12	SCL-90-R Anx	-0.29 (-0.91, 0.32)	3

MBCT mindfulness-based cognitive therapy, *MBCT-C* mindfulness-based cognitive therapy for children, *MBSR* mindfulness-based stress reduction, *ROB* risk of bias. Risk of bias (ROB) score ranges from 0 to 12, with higher score indicating greater risk of bias. Outcome measures are as follows. *BASC-T1 Anx* Behavioral Assessment System for Children (2nd Ed.) Teachers' Questionnaire–Anxiety subscale, *DASS-21 Anx* Depression Anxiety Stress Scale Short Form–Anxiety subscale, *RCADS TA* Revised Children's Anxiety and Depression Scale–Total Anxiety subscale, *SCARED* The Screen for Child Anxiety and Related Emotional Disorders, *SCAS* The Spence Children's Anxiety Scale, *SPIN* Social Phobia Inventory, *STAI Trait* State Trait Anxiety Inventory–Trait scale, *STAI-C Trait* State Trait Anxiety Inventory for Children–Trait scale.

Table 2 *Details of studies included in the meta-analysis: Trials held in clinics and other (non-school) settings*

Authors (year)	Country	Sample description	N	Age group	Setting	Intervention description	Control	Total training minutes	Training period weeks	Synthesised outcome measure (Follow-up period)	Effect size (95% CI)	ROB Score
Psychiatric populations												
Biegel et al. (2009)	USA	Psychiatric facility outpatients	102	Adolescent	Clinic	MBSR adapted for adolescents	Active: TAU	960	8	SCL-90-R Anx (3 month)	0.23 (-0.16, 0.62)	1
Dehghani et al. (2014)	Iran	Females with generalized anxiety disorder	14	Child	Not stated	MBCT-C	Passive: NTC	1080	12	RCMAS	1.30 (0.15, 2.46)	5
Díaz-González et al. (2018)	Spain	Mental health disorder outpatients	80	Adolescent	Clinic	MBSR adapted for adolescents	Active: TAU	720	8	STAI Trait/ STAI-C Trait	0.19 (-0.25, 0.63)	6
Shomaker et al. (2017)	USA	Females at risk of Type II diabetes with elevated depressive symptoms	33	Adolescent	Clinic	Learning to Breathe: Based on MBSR, includes body scan, breath awareness, mindfulness meditation, yoga	Active: Cognitive-behavioural programme	360	6	STAI-C Trait (6 months)	0.20 (-0.49, 0.88)	4
Medical/other populations												
Andreotti et al. (2017)	France	Children born with oesophageal atresia	19	Child	Home	Audio-guided MBI: body scan, mindful meditation focused on thoughts, breath, walking, sound, and visualization	Passive: WLC	336	6	STAI-C Trait	0.53 (-0.39, 1.46)	6
Chadi et al. (2016)	Canada	Females with chronic pain	19	Adolescent	Clinic	MBSR/MBCT elements adapted for adolescents	Passive: WLC	720	8	BAI-Y 2 nd ed.	-0.40 (-1.31, 0.51)	5
Freedenberg et al. (2017)	USA	Adolescents with cardiac diagnoses	46	Adolescent	Clinic	MBSR	Active: Online support group	540	6	HADS Anx	0.06 (-0.52, 0.64)	9
Semple,et al. (2010)	USA	Children with reading difficulties	25	Child	Clinic	MBCT-C	Passive: WLC	1080	12	STAI-C Trait	-0.11 (-0.90, 0.67)	4

MBCT mindfulness-based cognitive therapy, *MBCT-C* mindfulness-based cognitive therapy for children, *MBSR* mindfulness-based stress reduction, *ROB* risk of bias. Risk of bias (ROB) score ranges from 0 to 12, with higher score indicating greater risk of bias. Outcome measures are as follows. *BAI-Y* Beck Youth Inventory for Adolescents (2nd ed)–Anxiety scale, *HADS Anx* Hospital Depression and Anxiety Scale–Anxiety subscale, *RCMAS* Revised Children’s Manifest Anxiety Scale, *SCL-90-R Anx* The Symptom Checklist 90 (Revised)–Anxiety subscale, *STAI Trait* State Trait Anxiety Inventory–Trait scale, *STAI-C Trait* State Trait Anxiety Inventory for Children–Trait scale

Results

Study Sample Characteristics

Characteristics of the included studies are shown in Tables 1 and 2. A total of 1,582 participants were represented in the meta-analysis; 793 received the MBI and 789 served as the control condition. Study sample sizes ranged from 14 to 364 participants ($M = 79.10$, $SD = 87.93$). Participant age ranged from 4 to 18 years and the sample mean age varied between 5.1 and 16.8 years. Two studies were conducted with participants meeting criteria for an anxiety disorder ($n = 39$), and a further two studies sampled outpatients with mental health disorders including anxiety (anxiety diagnosis $n = 48/182$), hence at minimum 5.5% of total participants represented in the meta-analysis met criteria for an anxiety disorder. Schools were the most common intervention setting ($k = 12$, $n = 1244$) accounting for 79% of total participants. The majority of interventions were either MBSR or MBCT, or programs adapted from these manualised interventions. Studies varied in the level of detail provided, however, most MBIs were delivered by trained instructors and one was delivered by a teacher. In at least 13 cases, the authors were involved in an aspect of intervention design, adaptation, or delivery. The intervention period lasted between 4 and 24 weeks ($M = 9.30$, $SD = 4.16$), and total minutes of formal practice ranged from 270 to 2160 min ($M = 712.30$, $SD = 435.18$).

Risk of Bias

Selection bias resulting from random sequencing and allocation concealment methods was rated as low risk in 60% of the studies, unclear risk in 28% of the studies, and high risk in 13% of the studies. The ROB from inadequate blinding was considered low in 15%, unclear in 45%, and high in 40% of the studies. ROB due to incomplete outcome data was judged to be low in 80% of studies, with the remaining 20% of studies split evenly between unclear risk and high risk. The majority of studies provided insufficient information to judge the likelihood of selective reporting; hence the ROB from selective reporting was considered

to be unclear in 85% of studies, with two studies deemed as low risk and one as high risk. The ROB due to researcher allegiance was considered to be high in 60%, and unclear in 35% of the studies, with one study rated as low risk. See Tables 1 and 2 for the total ROB score by study.

Effect Size Analyses

An alpha level of .05 was used for all statistical tests unless otherwise stated. The random effects model analysis revealed a small and significant weighted mean effect size, $d = 0.26$, 95% CI [0.04, 0.49], $p = .019$, indicating that across all 20 RCTs, MBI participants showed a significantly greater reduction in anxiety compared to participants in the control condition (See Appendix A for the forest plot). Cochran's Q -test showed a statistically significant level of heterogeneity between sample estimates ($Q = 75.07$, $p < .0005$). The I^2 statistic, $I^2 = 74.7$, 95% CI [61, 84], suggested that approximately 75% of between-study variance was due to systematic, not random, sampling error, confirming the need to investigate the sources of heterogeneity through stratification and moderator analysis.

Seven studies ($n = 978$) also reported outcomes at various follow-up periods, enabling a separate calculation of treatment effects at last follow-up, two to six months post intervention, albeit with reduced power. The follow-up mean effect size was small-to-moderate but no longer significant, $d = 0.35$, 95% CI [-0.03, 0.73], $p = .067$, indicating that across seven RCTs any beneficial effect of MBI was not statistically significant two to six months post intervention. Q -values showed a significant level of heterogeneity in follow-up effects, $Q = 43.25$, $p < .0005$, and I^2 estimates suggested it was large in magnitude, $I^2 = 86.1$, 95% CI [74, 93].

Four stratified meta-analyses were performed examining how the weighted mean effect of MBIs on anxiety varied by participant age group, intervention setting, control group-type, and region of research. Table 3 outlines the weighted mean effects obtained using a

random effects model, and results of the heterogeneity analysis by subgroup. Note, the Q -test is known to have poor power when there are few studies and the amount of information is limited (Hardy & Thompson, 1998). Additionally, The I^2 statistic is susceptible to bias when the number of studies is small (Von Hippel, 2015). Because the majority of subgroups contained seven or fewer studies, point estimates of I^2 were supplemented with 95% confidence interval estimates in line with the recommendation of Von Hippel (2015) and caution is required when interpreting heterogeneity for these particular subgroups.

Children and adolescents. For samples that comprised only children ($k = 7$), the mean effect size ($d = 0.41$) was small-to-moderate and significantly greater than zero, suggesting that children benefitted from MBIs relative to control. Notably, there was considerably more uncertainty in the amount of heterogeneity in child samples, with the point estimate equalling zero yet the 95% confidence interval around I^2 indicating it could be between 0% and 71%. This indicates there is a large amount of uncertainty about the heterogeneity in studies with children, most likely due to the small sample. For adolescent samples ($k = 13$) the relative benefit of receiving MBIs was small ($d = 0.21$) and non-statistically significant. There was significant heterogeneity in the adolescent subgroup and I^2 indicated that approximately 81% of the observed variance between these RCTs was due to systematic error, 95% CI [69, 89].

Schools and clinics. The mean effect size of studies conducted in clinics was small and non-significant ($d = .13$), showing that clinic-based participants receiving MBIs did not improve significantly more than those in the control condition. The wide confidence interval for I^2 implied that there was substantial uncertainty in the amount of heterogeneity across clinic-based studies. The mean effect of school-based trials was higher but marginally non-significant ($p = .057$), suggesting that MBIs conducted with school populations did not lead to significant improvements in anxiety symptoms relative to controls. School-based studies

appeared to have significant heterogeneity, and I^2 estimates indicated this was large in magnitude.

Passive and active controls. When separated by control group type, MBIs showed a significant, small-to-moderate beneficial effect on anxiety symptoms relative to passive controls ($d = 0.33$), but did not have a significantly greater impact compared to active controls. In studies with passive controls heterogeneity was large and statistically significant, while in the active control studies the amount of heterogeneity was potentially null to large.

Regions of research. The final stratification compared the weighted mean effects of RCTs by location across four countries/regions. Studies conducted in North America showed a mean effect that was negligible and not significantly different from zero. The mean effect of Europe-based studies was small and non-significant. However, the confidence interval was relatively wide, suggesting there was greater variability of results in this subgroup. The mean effect size of Australian studies was negative and non-significant, based on the weighted mean of two RCTs conducted by the same author with the same MBI program. Iran was the only region with a statistically significant effect, indicating that studies conducted in Iran found a large beneficial effect of MBIs on anxiety symptoms. The wide confidence intervals around I^2 indicated there was considerable uncertainty in the amount of heterogeneity for all region-based strata, possibly resulting from the small number of studies in each stratum. A post-hoc subgroup analysis conducted on the combined Western regions of North America, Europe, and Australia found a negligible and non-significant mean effect size, $d = 0.05$, 95% CI $[-0.08, 0.18]$, $p = .456$, suggesting that across 15 RCTs conducted in the West, MBIs had little to no beneficial effect on anxiety outcomes compared to controls. Heterogeneity analysis for this subgroup indicated a low amount of heterogeneity between sample estimates, $I^2 = 21.9$, 95% CI $[0, 58]$ that did not reach statistical significance, $Q = 17.93$, $p = .210$.

Table 3

Results of stratified meta-analyses examining MBI effects on anxiety symptoms

Sub-groups	<i>k</i>	Total <i>n</i>	Stratified MBI effects					Heterogeneity				
			Mean effect size (<i>d</i>)	95% CI		z- score	<i>p</i>	<i>Q</i>	<i>p</i>	<i>I</i> ² (%)	95% CI	
				<i>LL</i>	<i>UL</i>						<i>LL</i>	<i>UL</i>
Age group												
Children	7	270	0.41	0.16	0.66	3.27	.001	5.51	.480	0.0	0	71
Adolescent	13	1,312	0.21	-0.07	0.49	1.46	.144	64.52	<.0005	81.4	69	89
Overall	20	1,582	0.26	0.04	0.49	2.34	.019	75.07	<.0005	74.7	61	84
Setting												
School	12	1,244	0.30	-0.01	0.61	1.90	.057	68.41	<.0005	83.9	73	90
Clinic	6	305	0.13	-0.10	0.35	1.08	.279	2.11	.834	0.0	0	75
Overall	18	1,549	0.23	0.00	0.45	1.97	.049	70.54	<.0005	75.9	62	85
Control group												
Passive	15	1,280	0.33	0.04	0.62	2.25	.024	72.73	<.0005	80.8	69	88
Active	5	302	0.12	-0.11	0.35	1.04	.297	2.24	.691	0.0	0	79
Overall	20	1,582	0.26	0.04	0.49	2.34	.019	75.07	<.0005	74.7	61	84
Region												
NA	8	395	0.03	-0.17	0.23	0.29	.770	3.42	.834	0.0	0	68
Europe	5	324	0.28	-0.02	0.57	1.85	.064	6.53	.163	38.7	0	77
Australia	2	633	-0.11	-0.27	0.05	1.37	.169	0.23	.630	0.0	-	-
Iran	3	122	1.25	0.49	2.00	3.24	.001	5.50	.064	63.6	0	90
Overall	18	1,474	0.23	0.00	0.46	1.96	.050	65.31	<.0005	74.0	59	84

The overall mean effect sizes differ depending on the studies included in each stratification analysis. The *I*² 95% CI could not be estimated for the Australia sub-group, as its calculation requires at least 2 degrees of freedom.

k total number of studies in stratum, total *n* total number of participants in stratum, *CI* confidence interval, *LL* lower limit, *UL* upper limit, *NA* North America

Moderator Analyses

The previous analyses compared the size and significance of the effect for each subgroup. To test the significance of between-group differences in the effect size, a series of individual, random effects meta-regressions were performed for each categorical moderator. A further three meta-regressions examined whether MBI effects were significantly moderated by continuous study-level variables of intervention duration, total minutes of formal training,

and risk of bias score. Analysis results are summarised in Table 4. The MBI effect size was not significantly moderated by the participant age group, intervention setting or control group type. Region of research was a significant moderator of effect size, with studies conducted in Iran showing significantly larger effects relative to studies conducted in North America, Europe, and Australia. MBI effects were not significantly related to intervention duration, total minutes spent training or study quality expressed as a risk of bias score.

Table 4

Meta-regression results for potential effect moderators

Comparison	β	SE_{β}	p -value
Child vs. Adolescent ^a	0.20	0.26	.461
School vs. Clinic ^a	0.23	0.27	.389
Passive vs. Active ^a	0.24	0.27	.378
Region (overall effect)			< .0005*
North America vs. Iran ^a	-1.36	0.24	< .0005*
Europe vs. Iran ^a	-1.11	0.25	< .0005*
Australia vs. Iran ^a	-1.50	0.23	< .0005*
Total minutes for formal training	0.00	0.00	.184
Duration of intervention in weeks	0.03	0.03	.290
Risk of bias score	-0.07	0.06	.266

* Significant moderator at $p < .05$, or after Bonferroni correction

^a The reference category

Given that RCTs from Iran yielded significantly larger effects compared to other regions, a sensitivity analysis was conducted to investigate their impact on the overall mean effect size and the extent to which they accounted for the observed significance of sub-group effects. Removing the three RCTs conducted in Iran from the study sample resulted in a

small and non-significant overall mean effect of MBIs on anxiety symptoms, $d = 0.12$, 95% CI $[-0.04, 0.29]$, $p = .129$, and did not affect the significance of any subgroup effects, except for the passive control subgroup, which no longer showed a significant effect size, $d = 0.14$, 95% CI $[-0.07, 0.36]$, $p = .200$.

Publication Bias and Small-Study Effects

There was a small suggestion of asymmetry, with missing studies on the left side of the funnel plot (Figure 2). However, there were many studies published with non-significant effect estimates and the pattern of missing studies included areas of significance, suggesting publication bias was unlikely to be the sole cause of asymmetry. Egger's test result for the entire study sample was non-significant ($p = .118$), providing further support for the absence of small-study effects and associated publication bias. The trim-and-fill method did not identify any missing studies in our data and therefore no adjustment was made to the pooled effect estimate. Analyses were performed only for strata containing 10 or more studies (Sterne et al., 2000), namely the adolescent subgroup ($k = 13$, $p = .264$), the school setting subgroup ($k = 12$, $p = .148$), the passive control subgroup ($k = 15$, $p = .115$), and the Western countries subgroup ($k = 15$, $p = .258$). Non-significant findings suggested a lack of small-study effects, but these findings should be interpreted with caution given the modest number of studies within each analysed stratum.

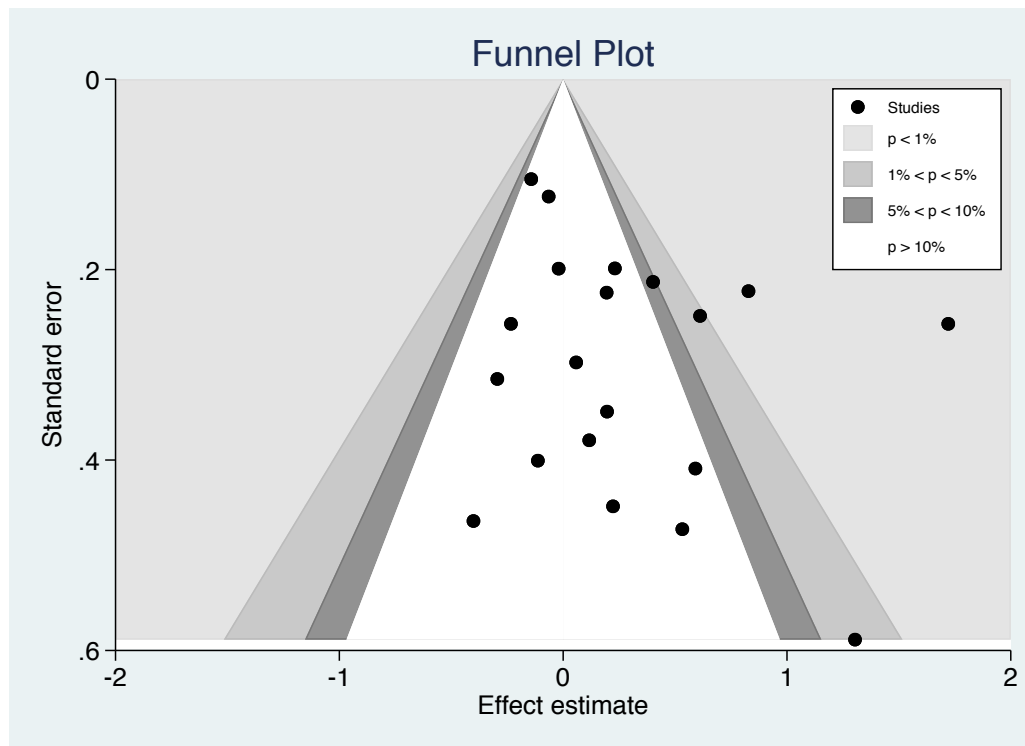


Figure 2. Contour-enhanced funnel plot of studies included in the meta-analysis

Discussion

Mindfulness has become increasingly popular as a mental health intervention for children and adolescents, particularly in school settings (Maynard et al., 2017). However, the evidence base for MBI efficacy is still developing (Dunning et al., 2019), and it is unclear how effective MBIs are for improving anxiety, one of the most prevalent and substantial mental health problems affecting youth (Beesdo-Baum et al., 2013; Polanczyk et al., 2015). The results of this meta-analysis showed a significant yet small overall mean effect of MBIs on anxiety ($d = 0.26$), but this effect size was statistically significantly moderated by the location of research and was substantially reduced to a small and non-significant mean effect when the three RCTs from Iran were excluded ($d = 0.12$). This non-significant mean effect size is less supportive of MBIs compared with previous meta-analytic findings, such as those of Dunning et al. (2019), which showed a significant positive effect of MBIs on anxiety/stress ($d = 0.16$) in a study sample that did not include Iran ($k = 20$, $n = 2319$). The

mean effect of MBIs on anxiety was further reduced and remained non-significant when only Western RCTs were included in the analysis ($d = 0.05$). Therefore, when generalising results to Western youth populations (from studies conducted in Europe, North America, and Australia) the null finding is likely to be a better indication of the true effect than the mean effect pooled from all 20 RCTs in the study sample.

The relatively superior results observed in Iran-based trials seem to exhibit a pattern consistent with findings in adult populations. Although no direct comparisons could be found in the literature, the mean effect size on anxiety reported in a meta-analysis of 38 Iranian studies with adults (Hoseyni et al., 2019: Hedges' $g = 1.51$) was substantially larger than the mean effect sizes reported in meta-analyses that included all locations not restricted to Iranian trials (Goldberg et al., 2018: $d = 0.89$; Khoury et al., 2013: $g = 0.96$). There may be factors unique to Iranian populations that promote higher intervention effects on anxiety. The three Iranian trials included in the current meta-analysis were examined for characteristics that could potentially explain their substantially favourable results. All three trials used similar intervention protocols based on MBCT. Notably two trials were conducted in female only populations with clinical levels of generalised anxiety (Dehghani et al., 2014) and social anxiety (Ebrahiminejad et al., 2016). These two were the only studies in the meta-analysis that were conducted with clinically anxious participants. Other studies either targeted general school populations or mixed psychiatric and medical populations. Previous meta-analyses have found large beneficial effects of MBIs for adults with anxiety disorders (Hofmann, et al., 2010; Khoury et al., 2013), and moderate effects have been reported in clinically anxious youth (Borquist-Conlon et al., 2019). Thus, the higher intervention effects observed in Iranian RCTs could, in part, reflect differences related to the clinical status of the participants, that is whether participants met the criteria for an anxiety disorder. Notably, we did not find a significant difference in effects of MBIs between clinic and school settings, but

this may have related to the fact that the clinic-based studies in this meta-analysis involved mixed psychiatric and medical populations, not clinically anxious populations. It was not possible to examine clinical status as a potential moderator in the current meta-analysis, because only two RCTs addressed populations with clinical levels of anxiety (Dehghani et al., 2014; Ebrahiminejad et al., 2016), precluding a meaningful stratification. Whether MBIs are relatively more beneficial for youth with clinical anxiety levels compared to general populations is an issue that could be addressed as more data from RCTs with clinical samples is accumulated.

Findings that MBI had significantly beneficial effects for children ($d = 0.41$), but not significant for adolescents suggest there may be early intervention advantages. The favourable results of MBIs conducted with children cannot be fully explained by the superiority of Iranian trials, because two of the three Iranian trials (representing 89% of Iranian trial participants) were conducted with adolescents, and removing Iranian RCTs from the analysis did not affect the significance or magnitude of the effect size for children. Moderator analysis failed to find a significant difference between effect sizes observed in children and those in adolescents, which is unsurprising given that statistical power for detecting differences between subgroups is often low in meta-regressions (Borenstein et al., 2009). Moreover, a large amount of heterogeneity remained after stratification, implying that other study-level factors may impact the intervention effect. Therefore, from the current evidence it is difficult to draw firm conclusions on the influence of developmental age on MBI effects.

We found significant positive effects of MBIs relative to passive controls ($d = 0.33$), but relative to active controls the mean effect was smaller and non-significant. The superior results of Iran-based studies could be driving these findings, as the sensitivity analysis showed that the mean effect of RCTs with passive controls was substantially smaller ($d =$

0.14) and no longer significant when the three Iranian trials were excluded. Despite the apparent difference in effect size between the two subgroups, control group type was not a significant moderator, suggesting that effects are not dependent on the comparison group employed by the study design. Interestingly, the mean effect size of MBIs compared to active controls ($d = 0.12$) was smaller than the comparable mean effect reported in the Dunning et al. (2019) meta-analysis on anxiety and stress combined ($k = 9$, $d = 0.18$), and those results reached statistical significance. Due to the paucity of studies with active controls in the current study sample ($k = 5$), active attention controls (e.g. health education) and active controls with an expected therapeutic benefit (e.g. CBT) were combined into one group for stratification. These controls are unlikely to be equivalent and therefore limit the interpretation of findings. Moreover, grouping all actively controlled studies together limited the ability to isolate the impact of the active mindfulness component from non-specific components of treatment, or to gauge how MBIs compare to other treatments in reducing anxiety symptoms. This is an area that may be reviewed as more trials with active comparisons are published.

Both clinic-based and school-based studies demonstrated small non-significant mean effects, indicating a lack of evidence to support the utility of both types of interventions in reducing anxiety severity. MBIs conducted in schools had somewhat larger effects than clinic-based trials, but the difference between them was not statistically significant. Findings showing relatively more positive effects in schools compared to clinics appear to be driven by the larger effects observed in Iran-based studies; removing Iran-based trials from the analysis resulted in equivalent mean effect sizes across clinics ($d = 0.12$) and schools ($d = 0.13$). Age group-related differences may also have influenced these results as clinic-based studies were heavily skewed towards adolescents (found to yield lower effects), with only one clinic-based trial conducted with children.

Results showing that MBI effects on anxiety were not significantly associated with intervention dosage (operationalised as both intervention duration in weeks and total minutes of formal training) are consistent with previous meta-analytic findings that time spent training did not significantly moderate effects on the outcome domain of anxiety/stress combined (Dunning et al., 2019). Notably, 80% of included studies specified homework practice in their protocol, indicating that this was a core component of the intervention. However, information on total minutes of personal mindfulness practice was generally not reported, hence could not be included in the analysis.

Regarding the evidence for the longer-term impact of MBIs on anxiety outcomes, although the mean effect was non-significant two to six months post intervention, it is somewhat difficult to draw conclusions due to the wide confidence interval (-0.03 to 0.73) and the small number of studies providing follow-up data ($k = 7$), possibly resulting in an underpowered analysis. The seven studies included in the analysis were from varying regions; hence the imprecision in the follow-up effect estimate may be partly related to the previously discussed region-based differences in outcomes. The non-significant follow-up effects are inconsistent with previous meta-analytic findings in adult populations, which largely support the maintenance of MBI effects on anxiety (Hofmann et al., 2010; Khoury et al., 2013). They also contradict previous meta-analytic findings in youth populations indicating that average MBI effects across combined outcome domains increase slightly from post intervention to follow-up (Klingbeil et al., 2017). The small number of trials measuring anxiety-specific outcomes at follow-up precluded a better assessment of the longer-term therapeutic effect of MBIs on anxiety. Based on currently available data however, there remains a lack of compelling evidence that, in general, MBIs are likely to have an important and lasting effect on youth anxiety.

Strengths and Limitations

The current meta-analysis aimed to determine firstly, whether across controlled studies MBIs had beneficial effects on anxiety outcomes in young people, and secondly, through stratification and moderator analysis, which population or study-level factors yielded the greatest effects. Novel aspects of the study include the use of RCT data obtained from both clinical and non-clinical samples, and isolating MBI effects on general anxiety symptoms or trait anxiety, by including only well-validated anxiety scales and excluding state anxiety measures. We make no conclusions regarding MBI effects on state-anxiety, as this was not a focus of our methodology.

Findings must be interpreted in the context of several limitations. A key limitation is that across all planned stratifications, the amount of unexplained heterogeneity was high or uninterpretable, suggesting that studies within subgroups may be estimating different underlying population effects, thus caution is required in the interpretation of findings. The subgroup of Western-based studies was the exception; the low and non-significant heterogeneity in this subgroup suggests that region-based differences could, in part, explain the inconsistency in overall effect estimates. The decision to pool the effects of different types of MBI protocols and approaches may have contributed to heterogeneity.

The decision not to impose exclusion criteria on the studied population (apart from age) resulted in a wide range of non-clinical, medical, and psychiatric populations included in the meta-analysis. While the inclusion of varied populations may have increased the applicability of findings, there may also have been some disguising of effects for specific populations. Due to the limited number of trials sampling populations meeting diagnostic criteria for anxiety, we were unable to draw conclusions on MBI effects for individuals with clinical levels of anxiety compared to non-clinical populations. The majority of studies in our sample were universal prevention programs, in which symptom change may be harder to detect.

Methodological shortcomings of included studies are a potential limitation for consideration when drawing causal conclusions. Included studies had variable quality and many studies provided insufficient information to determine the risk of bias. Researcher allegiance was the most common type of risk of bias, possibly leading to an overestimation of the overall effect size (Berman & Reich, 2010). Nonetheless, moderator analysis showed that effect sizes were unrelated to study quality.

The variation in the outcome measures used across trials presents a further limitation. Only data based on well-validated anxiety scales were used, but the resulting mean effect size estimate was based on 12 different anxiety scales, which likely contributed to heterogeneity. Four studies used more than one anxiety scale, but in two such studies (Semple et al., 2010; Sibinga et al., 2013) reported scales yielded directionally inconsistent outcomes, raising concerns about the reliability of findings. While these different outcomes could not be averaged meaningfully into one study-level effect, the decision to select the measure that produced the most conservative effect size may have introduced bias into the analysis.

Implications for Future Research and Policy

The above discussion highlights gaps in existing research, such as the need for better follow-up data and more actively controlled trials. Additionally, it is still unclear how MBI effects differ for clinical and non-clinical youth populations. Past meta-analytic findings suggesting positive effects of MBIs for adults with anxiety disorders (Hofmann et al., 2010; Khoury et al., 2013), raise the question of whether and when this is replicated in youth populations.

It is not clear why MBIs have such a limited impact on anxiety symptoms, particularly among Western youth. Trials included in the meta-analysis did not attempt to explain empirically the relationship between the intervention and the processes thought to maintain anxiety, such as avoidance and safety behaviours, threat appraisals, and attentional

deployment (Clark, 1999). It could be that MBIs are not particularly effective in addressing important anxiety maintenance factors. For example, while mindfulness practice may facilitate exposure to unpleasant or feared internal sensations, it may offer less opportunity for proactive exposure to feared external stimuli that can facilitate learning and subsequent threat reappraisal (Waters & Craske, 2016). Including measures of anxiety maintenance processes in trials with anxious youth would help to illuminate the mechanisms by which interventions are purported to reduce anxiety.

From a policy perspective, the non-significant results for school-based MBIs raise questions about the utility of promoting MBIs for general school populations, specifically as a means of addressing youth anxiety. Further trials may be needed, but on the basis of existing research, insufficient evidence exists to confidently support investment in such programs for anxiety reduction, particularly in Western countries.

Conclusions

In conclusion, although overall pooled results suggest that MBIs might reduce anxiety symptoms, the estimated effect is likely to be small and temporary. However, these results need to be interpreted in the context of significant heterogeneity. Importantly, indications that region of study is a significant moderator suggest that across Western youth populations, the most likely outcome is that MBIs have no beneficial effect in anxiety symptom reduction. Results point to a lack of compelling evidence to support school-based MBIs as an effective means of tackling youth anxiety.

References

References marked with an asterisk indicate studies included in the meta-analysis

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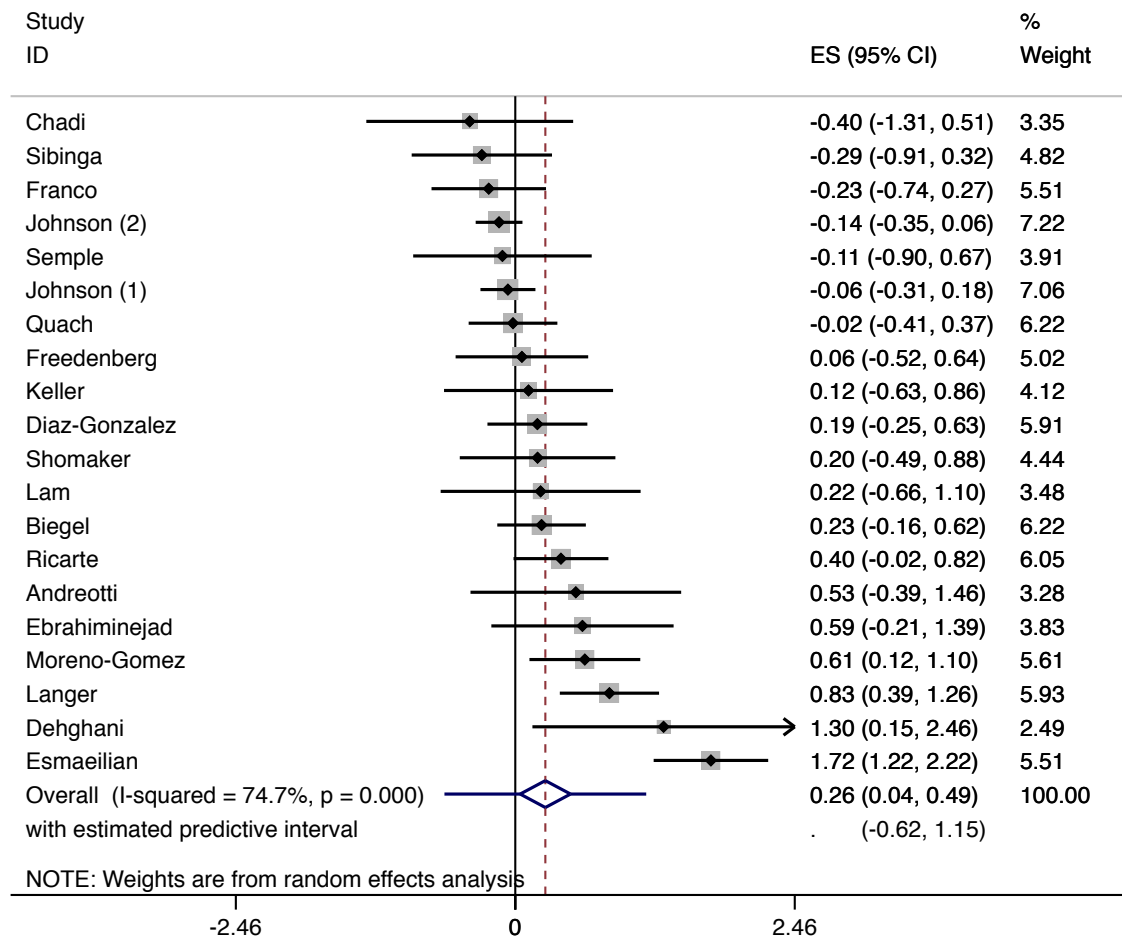
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Appendix A

Forest Plot of Effect Sizes of MBIs on Anxiety Outcomes



Note. Johnson (1) refers to Johnson, Burke, Brinkman, and Wade (2016); Johnson (2) refers to Johnson, Burke, Brinkman, and Wade (2017).