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Effects of meditation on pain intensity, physical function, quality of life and depression in adults with low back pain – A systematic review with meta-analysis☆

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ABSTRACT

Background: Low back pain (LBP) is a common biopsychosocial health problem. Meditation may provide a complementary treatment option for LBP patients.
Objectives: The aim of this systematic review with meta-analysis was to examine the effects of meditation on pain intensity, functional disability, quality of life, and depression in LBP populations.
Methods: This systematic review was conducted according to the PRISMA Guidelines. PubMed, Web of Science, CENTRAL, CamQuest and PubPsych were searched up to a publication date of June 2020. Inclusion criteria were RCTs or non-RCTs with LBP patients, aged at least 18 years, the application of a specific meditation technique, and pain intensity and/or functional disability as outcomes. Pooled SMDs were calculated at post-treatment and follow up. The Cochrane risk-of-bias tool was used to estimate risk of bias. The overall quality of evidence was assessed using the GRADE approach.
Results: 12 studies with a total of 1005 participants were included in this review. Compared to controls, meditation solely showed a significant positive effect on pain intensity (*SMD* = −0.27 [CI −0.43; −0.11]; $p = 0.001$; based on 10 studies with 934 participants) and physical quality of life (*SMD* = 0.21 [CI 0.07; 0.36]; $p = 0.005$; based on 5 studies with 756 participants) at post-treatment. At follow up (mean 20 weeks, range 4–52) there were no significant effects anymore. The quality of the evidence was moderate due to study limitations and imprecision.
Conclusions: Meditation seems to be promising with regard to reducing short-term pain intensity in patients with LBP. However, additional well-designed and large trials are required in order to draw more reliable conclusions.

1. Introduction

Low back pain (LBP) is defined as pain below the costal arch and above the gluteal crease, which eventually can radiate into the legs[1]. LBP is the worldwide leading cause of limitations in daily activities and is associated with a high economic burden[2–4]. The worldwide lifetime prevalence of LBP is between 50% and 85% in adults[5,6]. LBP is usually classified as specific or non-specific LBP. Specific LBP is characterized by an identifiable pathoanatomic correlate and is accompanied by warning symptoms, also called "red flags"[1]. Non-specific LBP, which occurs far more often than specific LBP, lacks this definable cause[7]. Psychosocial factors such as distress, anxiety, and depression – "yellow flags" – mainly contribute to the chronification of LBP and disability[8,9]. To address these, changes have been made to national clinical practice guidelines during the past decades and greater emphasis is now placed on active treatments and less on pharmacological and surgical treatments. In these guidelines the use of physical exercise, some forms of complementary medicine and psychological therapies such as progressive relaxation and others, are endorsed particularly for those with persistent low back pain[10].

Meditation is one of the most popular complementary health practices among US-Americans[11]. It is increasingly used especially by back- and neck pain patients[12]. However, the evidence for the effectiveness of so-called "mind-body-interventions" as a complementary therapy approach is still sparse, which underpins the need for high-quality research in this field. Meditation has already been practiced in

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religious and spiritual contexts over thousands of years[13]. The term "meditation" includes many different types of practices, that have the following aspects in common: 1) performed according to a specific technique, 2) leading to a muscular relaxation during the process, 3) including "logic relaxation", which are 4) inducible by the practitioner themselves and 5) based on a self-focussed skill[14]. Some meditation types are performed in a spiritual context, e.g. loving-kindness and Zen[15–17]. Others have been secularized over time, e.g. Jon Kabat-Zinn's Mindfulness-based Stress Reduction (MBSR)[11,18,19]. In previous systematic reviews the focus has been on MBSR for mixed medical conditions associated with chronic pain and they have included only a few studies with a LBP sample[20–22]. Despite this, there exist two systematic reviews from 2012 and 2017 in which MBSR and Mindfulness-based cognitive therapy (MBCT) were specifically investigated in LBP populations. These, however, only included a maximum of 7 studies with partially high risk of bias and did not contain a quantitative analysis or only included a maximum of 4 studies in the associated meta-analyses[23,24]. Furthermore, existing evidence is inconclusive with regard to the benefits of meditation for LBP and mainly shows small short-term and clinically non-relevant effects. The objective of this systematic review with meta-analysis was to synthesize the effects of various meditations besides MBSR, that have been disregarded in research so far, on LBP patients' pain intensity, physical function, quality of life (QOL), and depression in order to add to the latest evidence of meditation effects on LBP-related outcomes.

2. Methods

2.1. Study design and conception

This systematic review was conducted according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA)[25]. The research question was formulated using the PICO approach.

2.2. Inclusion and exclusion criteria

Eligibility criteria for studies were defined a priori, based on the PICO elements (Suppl. A). Studies had to fulfil the following criteria to be eligible for this review: 1) an experimental study design which could either be randomized controlled or non-randomized, 2) a sample of LBP patients with an age of at least 18 years, 3) a specific type of sitting meditation, walking meditation or a meditation program as intervention, 4) any passive or active comparator except for other types of meditation or relaxation techniques, and 5) at least pain intensity or functional disability as outcomes, which were defined as primary outcomes in this review. Studies were excluded for the following reasons: 1) observational or qualitative studies, study protocols or (systematic) reviews, 2) a sample including patients with LBP due to malignancy, infection or pregnancy, 3) Yoga, Tai Chi or Qi Gong as the main intervention, 4) meditation as part of psychotherapy such as Acceptance and Commitment Therapy, and 5) published in a journal listed on "Beall's List of Potential Predatory Journals and Publishers"[26]. Only publications written in English and German, and with a full text available were considered.

2.3. Data sources and search strategies

As meditation may be used in different health-related contexts, a systematic literature search was performed in five electronic databases with different main topics: PubMed with biomedical focus, PubPsych with psychological focus, CAM-Quest with complementary medical focus, CENTRAL for clinical trials and Web of Science with a broadly based subject focus. Additionally, Google Scholar and ResearchGate were searched unsystematically. ResearchGate was also used to contact authors if full-text articles were otherwise not available. Grey literature was not considered. The literature search was conducted independently by both authors between 13th and 16th of June 2020 without any publication date restriction. The search terms chosen for the intervention were "meditat* ", "mindfulness", "mindfulness meditation", and "MBSR", which were linked using the Boolean operator "OR". For the population "low* back pain", "back pain", "backache" and "lumbago" were used. Search terms for the intervention were combined with terms for the population by using the Boolean operator AND. The search strategy was adapted for each database. Detailed information on the search strategy in each database is provided in Supplement B. To avoid a lag time bias during the publication process we performed an updated search in September 2021 which did not reveal additional eligible articles.

2.4. Study selection and data extraction process

The results of the database search were transferred into a literature management program, followed by the removal of any duplicates. Subsequently, titles and abstracts were screened for relevance by one author (HS) and verified by another (CP). Any disagreements regarding inclusion and exclusion criteria were discussed and resolved by consensus. Abstracts which potentially seemed to match the inclusion and exclusion criteria were read in full text in order to decide on eligibility for the qualitative analysis. To be eligible for meta-analyses, studies needed to contain all required data for statistical analysis, these were 1) mean and standard deviation for intervention and control group each for pre- and post-intervention and 2) sample sizes of intervention and control group. Studies were only excluded from meta-analyses if the required data were missing and could not be calculated from other reported statistics or if their inclusion caused substantial heterogeneity. A predesigned and standardized sheet was used for data extraction by one author initially (HS). A second author (CP) reviewed the extracted data for its relevance, accuracy, and comprehensiveness. For the case of any disparities a final decision based on consensus building was made. Data were collected on the following variables: study characteristics (first author, year of publication, journal, and DOI), methods (study design, sample size, a priori sample size calculation, inclusion and exclusion criteria of subjects), subject characteristics (sex, age, ethnicity, pain characteristics, medication intake, number of participants in experimental and control group) and intervention characteristics (setting, type of intervention in experimental and control group, details on the intervention, length of treatment, duration and frequency of sessions, and attendance rate).

2.5. Study quality assessment

The methodological quality of each included study was independently assessed by two reviewers (HS and CP) using the Cochrane Collaboration's Risk of Bias Tool[27]. In this tool six different types of bias are considered: selection bias, performance bias, attrition bias, detection bias, reporting bias and confounding bias. Studies were included in the data synthesis regardless of their risk of bias.

2.6. Effect size measures and data synthesis

Effect sizes were calculated as standardised mean differences (SMD) for continuous data. In detail, d_{corr} according to Klauer[28] was calculated in order to take pretest and sample size differences between intervention and control groups into account. D_{corr} was determined by first calculating Hedges g for pre- and post-test separately and subsequently subtracting g_{pre} from g_{post}. The interpretation of d_{corr} is equivalent to the interpretation of Cohen's d: 0.2 is considered a small, 0.5 a moderate and 0.8 a large effect[29]. 95% confidence intervals were calculated for d_{corr}. The publication authors were contacted if the required data for effect size calculation were not reported. If the relevant data could not be obtained from the authors, any missing standard deviations were calculated from the standard error, confidence interval or t-statistics.

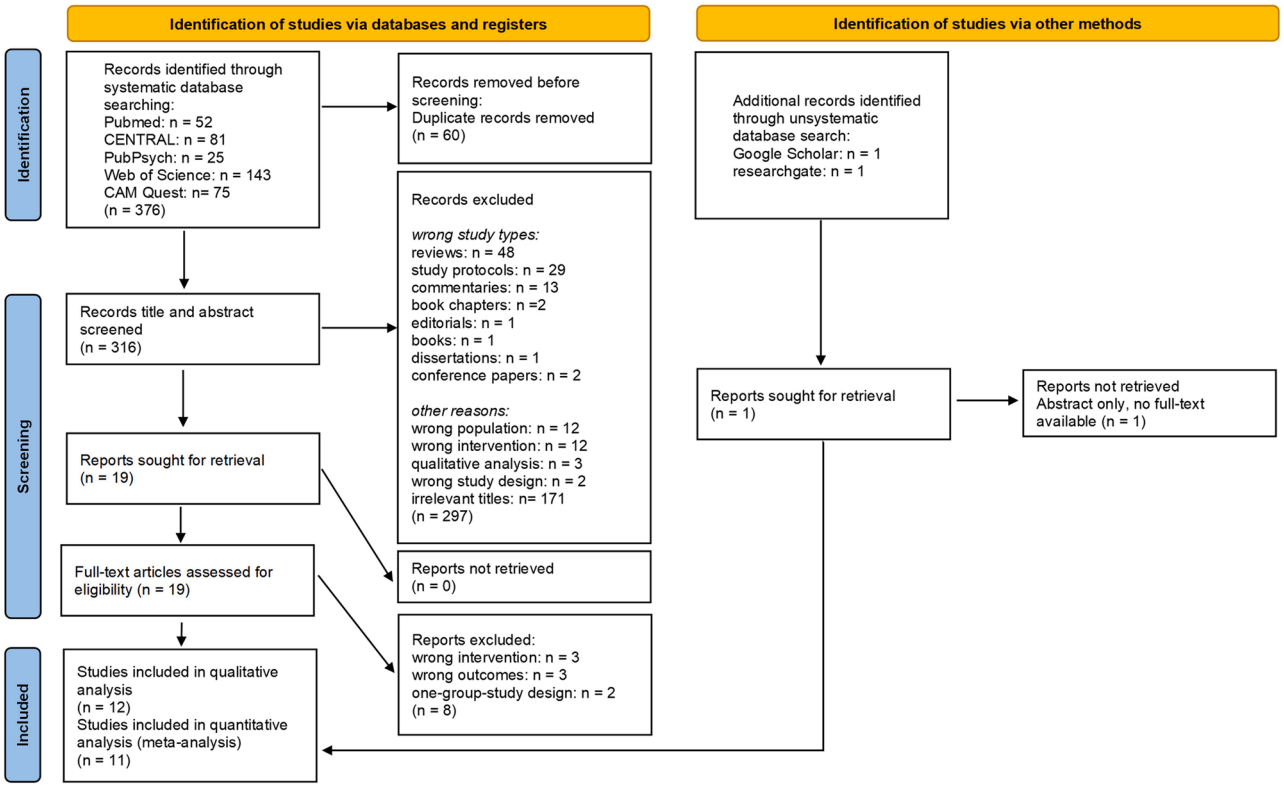

Fig. 1. Flowchart showing the study selection process.

For studies involving multiple control groups that equally fulfilled the inclusion criteria the sample sizes, SMDs and standard deviations of these groups were statistically combined in order to calculate an integrated effect size[30,31]. Meta-analyses were conducted if at least two studies were available for the respective outcome. Short- and long-term effects were analysed separately. Short-term effects are defined by the period of time from pre-treatment until post-treatment. Long-term effects refer to the period of time from pre-treatment until any follow-up, regardless of the number of weeks. Data syntheses were performed using Borenstein's software "Comprehensive Meta Analysis 3.0"[32]. A random effects model was applied for data pooling. Heterogeneity between studies was assessed by using the I^2 test with a significance level of 90% and by visually estimating the degree of overlap between confidence intervals in the forest plots. An I^2 result of 0–40% was considered as possibly insignificant, 30–60% as moderate, 50–90% as substantial and 75–100% as having considerable heterogeneity[30]. For meta-analyses consisting of at least 10 studies, a prediction interval was additionally calculated.

2.7. Overall evidence quality and clinical relevance assessment

Overall evidence quality for each outcome was assessed by two reviewers independently (HS and CP) using the "Grading of Recommendations Assessment, Development, and Evaluation" (GRADE) system[33]. GRADE allows authors to downgrade the evidence from "high" to "moderate" to "low" or "very low" based on five evaluation criteria: study limitations, inconsistency, indirectness, imprecision, and publication bias. Any inconsistencies were discussed until a consensus was obtained. The minimally clinically important difference (MCID) was used to decide on clinical relevance of each pooled effect for an outcome. If studies used different measurement tools for a specific outcome, the tool for which a MCID was published in the literature was chosen. To allow for a comparison between pooled SMDs from the meta-analyses in this work and MCIDs from the literature, the pooled effects were converted into absolute mean differences in the units of the respective measurement device. This was done by calculating the weighted average of pre standard deviations of all studies that had used the respective measurement device, which was then multiplied with the pooled SMDs and confidence intervals from the meta-analyses of this work[30]. An overview of the MCIDs for each outcome is provided in Supplement C.

2.8. Additional analyses

A subgroup analysis was performed in order to separately analyse pooled effects for meditation vs passive controls and meditation vs active controls if the number of studies in each subgroup was nearly balanced. Sensitivity analyses were made in order to explore the effect of highly biased studies on the pooled effects (Suppl. G). Studies were categorized as "highly biased" if less than 4 of 7 bias domains were estimated to be low risk. Given that at least 10 studies were available for an outcome[34], a check was made for publication bias by using Egger's test with a significance level of 90%[35] and the trim-and-fill method by Duval and Tweedie[36].

3. Results

3.1. Study selection

The study selection process is illustrated in Fig. 1. Initially 376 records were identified through database searching in PubMed, CENTRAL, PubPsych, Web of Science and CAM Quest. 2 additional records were identified by unsystematically searching Google Scholar and ResearchGate. After duplicates had been removed, 318 records remained and were title and abstract screened. Subsequently, publications were excluded with wrong study/publication types such as other (systematic) reviews, as well as article types other than RCTs or non-randomized controlled trials, e.g., book chapters, study protocols, commentaries or editorials and wrong study design. Finally, 20 full-text articles could be

Table 1
Summary of study characteristics.

Surname of first author & year	N	Age Ø [years] % female	Inclusion criteria	Intervention type Frequency per week Length	Type of control group (study type)	Follow Up [weeks] *	Outcomes (Assessment method)
Carson, 2005[46]	43 IG: 18 CG: 25	51 61%	cLBP for at least 6 months	Loving-Kindness-Meditation 1 x/w for 90 min 8 weeks	usual care(randomized)	12	pain intensity (MPQ)
Morone, 2008 [37]	37 IG: 19 CG: 18	75 57%	cLBP with at least moderate intensity	MBSR (without Yoga) 1 x/w for 90 min 8 weeks	wait list (randomized)	12 (IG only)	pain intensity (MPQ-SF) functional disability (RMDQ) physical QOL (SF-36) mental QOL (SF-36)
Morone, 2009 [38]	35 IG: 16 CG: 19	76 63%	cLBP for at least 3 months with at least moderate intensity	MBSR (without Yoga) 1 x/w for 90 min 8 weeks	health education program (randomized)	16	pain intensity (MPQ-SF) functional disability (RMDQ)
Banth, 2015[39]	48 IG: 24 CG: 24	40 100%	unspecific cLBP for at least 6 months	MBSR (without Yoga) 1 x/w for 90 min 8 weeks	usual care(randomized)	4	pain intensity (MPQ) physical QOL (SF-12) mental QOL (SF-12)
Braden, 2016[48]	23 IG: 12 CG: 11	46 66%	cLBP	MBSR (without Yoga) 1 x/w for 120 min 4 weeks	reading material on stress reduction techniques (non-randomized)	52	functional disability (ODI) depression (BDI-II)
Cherkin, 2016 [40]	341 IG_1: 116 IG_2: 112 CG: 113	49 66%	unspecific cLBP for at least 3 months	IG_1: MBSR IG_2: CBT both groups 1 x/w for 120 min 8 weeks	usual care(randomized)	18 44	pain intensity (NRS) functional disability (RMDQ) physical QOL (SF-12) mental QOL (SF-12) depression (PHQ-8)
Michalsen, 2016 [41]	68 IG: 32 CG: 36	55 75%	cLBP for at least 3 months (specific causes excluded), intensity of at least 40/100 on visual analogue scale	Jyoti-Meditation 1 x/w for 90 min 8 weeks	home training, 20 min per day (randomized)	-	pain intensity (VAS) functional disability (RMDQ) physical QOL (SF-36) mental QOL (SF-36) depression (HADS)
Morone, 2016 [32]	282 IG: 140 CG: 142	74 66%	cLBP for at least 3 months with at least moderate intensity	MBSR 1 x/w for 90 min 8 weeks	health education program (randomized)	24	pain intensity (NRS) functional disability (RMDQ) physical QOL (SF-36)
Ardito, 2017[47]	28 IG: 17 CG: 11	48 54%	cLBP for at least 3 months	MBSR 1 x/w for 120 min 8 weeks	wait list (non-randomized)	16–20	pain intensity (NRS) physical QOL (SF-36) mental QOL (SF-36) depression (BDI-II)
Reiner, 2018[43]	36 IG: 14 CG: 22	58 73%	cLBP for at least 3 months, intensity of at least 5/10 on numerical rating scale	mindfulness-based program 1 x/w for 90 min 8 weeks	wait list (randomized)	12 (IG only)	pain intensity (BPI)
Masumian, 2018 [44]	18 IG: 9 KG: 9	48 100%	cLBP	MBSR (without Yoga) 1 x/w for 120–150 min 8 weeks	no intervention (randomized)	-	pain intensity (MPI)

(*continued on next page*)

Table 1 (*continued*)

Surname of first author & year	N	Age Ø [years] % female	Inclusion criteria	Intervention type Frequency per week Length	Type of control group (study type)	Follow Up [weeks] *	Outcomes (Assessment method)
Day, 2019[45]	46 IG: 23 KG: 23	51 52%	cLBP for at least 3 months, intensity of at least 4/10 on numerical rating scale	IG: mindfulness meditation 1 x/w for 120 min 8 weeks	cognitive therapy (randomized)	12 24	pain intensity (NRS) functional disability (PROMIS) depression (PROMIS)

Studies are sorted by year of publication. The most recent study is listed at the very bottom of the table. * length of follow-up after post-measurement; IG = intervention group; CG = control group; QOL = quality of life; cLBP = chronic low back pain; N = total sample size; n = sample size for either IG or CG; FU = follow-up; w = week; MBSR = mindfulness-based stress reduction; CBT = cognitive behavioural therapy

retrieved and were assessed for eligibility of which 12 fulfilled the inclusion criteria and were therefore included in qualitative analysis. 11 of these studies were included in quantitative analysis. One study was excluded from meta-analyses because it caused high heterogeneity[39].

3.2. Study characteristics

Study characteristics are summarized in Table 1. 10 of 12 studies were RCTs[37–46], and two were non-randomized interventional studies[47,48]. 10 studies were conducted in a parallel group design[37–39,41–44,46–48], and two had three arms[40,45]. Most studies were conducted in the USA (n = 6), 2 in Europe, 3 in Asia and 1 in Australia. In total, 1005 participants were examined. Study sample size varied from 18 to 341. The age of participants varied from 40 to 76 years. In three studies only elderly subjects ≥ 70 years were examined[37,38,42]. All studies included at least 52% female participants, most of them between 60% and 75%[38,40–43,46,48]. Two studies had solely female participants[39,44]. All participants suffered from chronic LBP. Most of them had a pain duration of at least three months with a pain intensity from 4 to 5 on a 10-point pain scale. In three studies only subjects with non-specific LBP were examined[39–41]. In most studies the effects of mindfulness meditation on LBP were investigated, which also included the application of MBSR. In all studies intervention frequency was once per week. Session times varied from 90 to 150 min. All but one study (4 weeks)[48] had an intervention period of eight weeks. Studies used different control groups such as usual care[39,40,46], wait list control groups[37,43,47], health education programs[38,42], exercise[41], and psychotherapy[40,45]. Pain intensity was assessed in all but one study[48], functional disability in seven[37,38,40–42,45,48], quality of life in six[37,39–42,47] and depression in five studies[40,41,45,47,48]. The most used measurements for pain intensity were the McGill Pain Questionnaire (MPQ)[37–39,46] and a numerical rating scale[40,42,45,47]. Functional disability was primarily assessed by the Roland-Morris Disability (RMDQ) except of two studies which used Oswestry Disability Index (ODI)[48] and Patient Reported Outcomes Measurement Information System (PROMIS)[45]. A follow-up measurement was conducted in ten studies[37–40,42,43,45–48] and varied across studies (range: 4–52 weeks, mean: 20 weeks)[37,38,40,42,43,45–47].

3.3. Risk of bias within studies

Only one of 12 studies was judged with a low risk of bias in every domain (Fig. 2)[45]. Four studies were judged with a low risk of bias in only one domain (Fig. 2)[39,44,47,48]. In addition, four studies had a high risk of attrition bias since intention-to-treat-analysis was not observed and/or there was an unequal distribution of dropouts between the groups[38,39,44,46]. Bias from selective reporting could be excluded with certainty in only two studies (Fig. 2)[40,45] since most studies did not provide a priori study protocols. Risk of confounding was observed in two studies due to inadequate or missing randomization (Fig. 2)[47,48]. In summary, most studies used adequate methods of randomization and allocation concealment, and therefore had a low risk of selection bias. Performance bias was high in all but one study, due to a lack of blinding of participants and personnel. Detection bias was low in the majority of studies or not applicable due to missing information (Fig. 3)[44,47,48]. Supplement E includes a detailed Risk of Bias assessment for each study.

3.4. Results of data synthesis

Eleven of 12 studies were included in the meta-analyses. Cherkin et al.[40] used two different control groups which equally fulfilled the inclusion criteria of this work and were therefore statistically combined. A prediction interval could only be calculated for pain intensity (Fig. 4).

3.4.1. Effects of meditation on pain intensity

Ten studies with a total of 934 patients were included in the meta-analysis for short-term effects on pain intensity. From the pooled analysis (Fig. 4) a significant positive small effect of meditation on pain intensity was indicated ($SMD = -0.27$ [CI −0.43; −0.11]; $p = 0.001$; $I^2 = 20\%$; $p = 0.3$). The prediction interval ranged from − 0.05 to − 0.49. Sensitivity analysis showed that exclusion of highly biased studies[41,44] only marginally decreased the pooled effect ($SMD = -0.23$ [CI −0.36; −0.10], $p = 0.001$), but reduced I^2 to 0% (Fig. 14, Suppl. G). The effect remained similar in the long-term but was no longer significant ($SMD = -0.28$ [KI −0.64; 0.08]; $p = 0.12$; $I^2 = 75\%$, $p = 0.003$) (Fig. 5). A sensitivity analysis revealed a decrease in effect size, width of confidence interval and heterogeneity ($SMD = -0.17$ [CI −0.37; 0.03]; $p = 0.10$; $I^2 = 32\%$; $p = 0.3$) (Fig. 15, App G).

3.4.2. Effects of meditation on functional disability

Seven studies with a total of 831 participants were available to synthesize data for functional disability. Meditation wasn't superior, neither in the short-term ($SMD = -0.15$ [CI −0.31; −0.01]; $p = 0.07$; $I^2 = 12\%$, $p = 0.3$) (Fig. 6) nor in the long-term ($SMD = -0.01$ [CI −0.24; 0.23], $p = 0.95$; $I^2 = 45\%$, $p = 0.1$) (Fig. 7). A Sensitivity analysis showed that the exclusion of the highly biased study by Braden et al.[48] barely changed the pooled effect and heterogeneity even increased (Fig. 16, Suppl. G).

3.4.3. Effects of meditation on secondary outcomes: quality of life and depression

The pooled short-term effect for physical quality of life was calculated from 5 studies with 756 participants. The short-term effect was small and statistically significant ($SMD = 0.21$; [CI 0.07; 0.36]; $p = 0.01$; $I^2 = 0\%$, $p = 0.8$) (Fig. 9, Suppl. F). Only three studies were available for follow-up data synthesis. The results showed that the effect decreased in the long-term ($SMD = 0.05$; [CI −0.11; 0.20], $p = 0.57$; $I^2 = 0\%$, $p = 1.0$) (Fig. 10, Suppl. F). Sensitivity analysis neither showed a change in short- nor long-term effect (Fig. 17 & 18, Suppl. G). For mental quality of life three studies were available for data synthesis. The pooled short-term effect was not significant and of small size ($SMD = 0.18$; [CI −0.02;

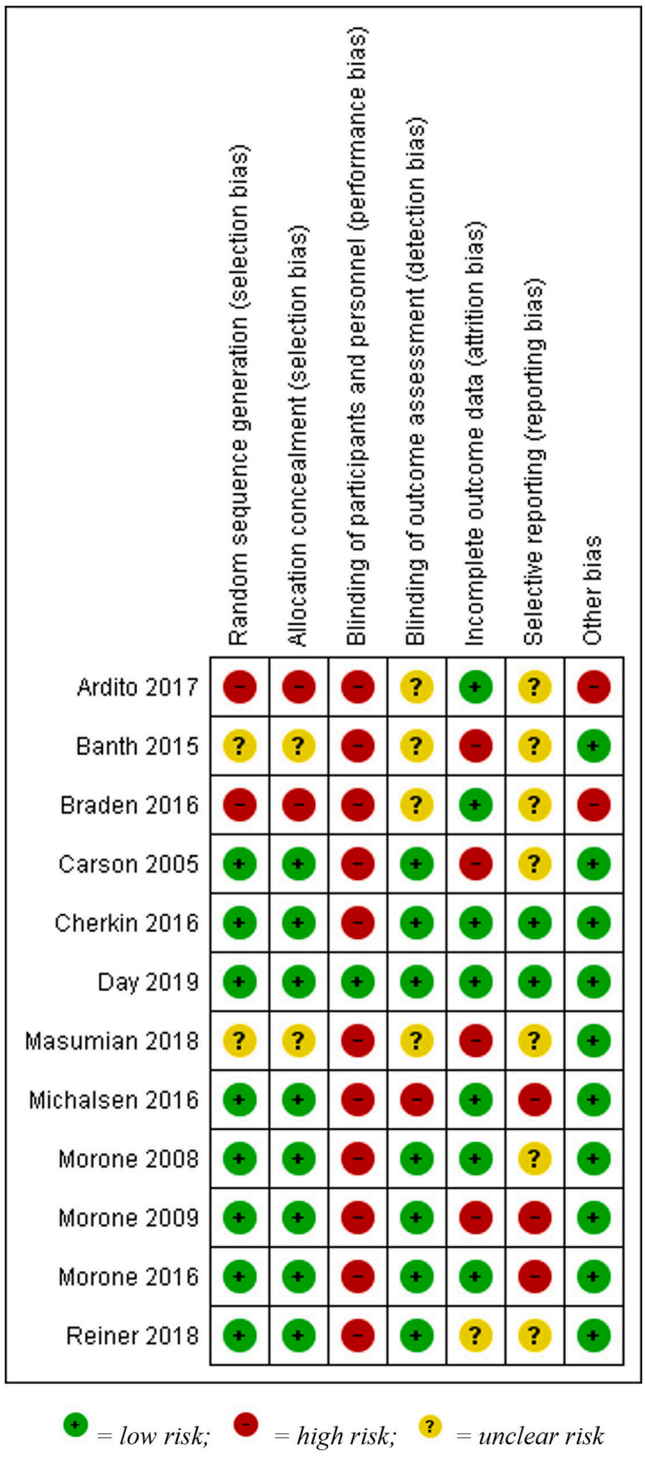

Fig. 2. Risk of bias summary: review of authors' judgements about each risk of bias item for each included study.

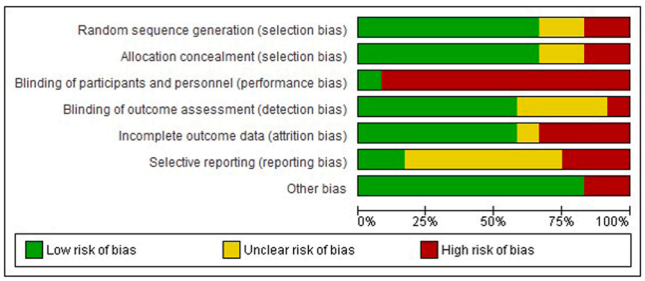

Fig. 3. Risk of bias graph: review of authors' judgements about each risk of bias item presented as percentages across all included studies.

0.37], $p = 0.07$) (Fig. 11, Suppl. F), but heterogeneity was substantial ($I^2 = 65\%$, $p = 0.04$). Follow-up data was not pooled since there were only two studies available, of which one was highly biased[47]. The only evidence for long-term effects comes from Cherkin et al.[40] who could not show any benefit of meditation (*SMD* = −0.04; [CI −0.26; 0.18]; $p = 0.72$). Short-term data synthesis for depression was based on five studies. The effect was small, not statistically significant (*SMD* = −0.15; [CI −0.33; 0.03]; $p = 0.10$, $I^2 = 0\%$, $p = 1.0$) (Fig.12, Suppl. F), and decreased in the long-term (*SMD* = −0.05; [CI −0.25; 0.15], $p = 0.65$; $I^2 = 0\%$, $p = 0.5$) (Fig. 13, Suppl. F). Sensitivity analysis neither showed a difference in short- nor in long-term effect size (Fig. 19 and 20, Suppl. G).

3.5. Results of subgroup analysis

In eight studies the comparison between meditation and a passive control group were investigated, five with an active control group. For short-term effects there were enough studies to conduct meta-analyses for every outcome and control group. However, this was not possible for long-term data synthesis of pain intensity, functional disability, and mental quality of life. The results showed that effect sizes for the comparison between meditation and passive controls were constantly larger in favour of meditation compared to active controls (Suppl. H) (e.g. short-term pain intensity meditation vs passive control *SMD* = −0.49 [CI −0.73; −0.26]; $p < 0.001$; $I^2 = 12\%$; $p = 0.34$, Fig. 21, compared to short-term pain intensity meditation vs active control *SMD* = −0.15 [CI −0.30; 0.00]; $p = 0.056$; $I^2 = 0\%$; $p = 0.82$, Fig. 22, and short-term physical quality of life meditation vs. passive control *SMD* = 0.26 [CI 0.03; 0.48]; $p = 0.03$; $I^2 = 0\%$; $p = 0.67$, Fig. 27, compared to short-term physical quality of life meditation vs. active control *SMD* = 0.15 [CI −0.01; 0.31]; $p = 0.07$; $I^2 = 0\%$; $p = 0.86$, Fig. 29). The separate analysis of active and passive control groups mainly led to a reduction of heterogeneity among studies with passive control groups (not shown).

3.6. Publication Bias

Only for pain intensity were there enough studies to explore publication bias. Visually, the funnel plot was slightly asymmetrical since small studies with negative effects are missing. Egger's test, however, did not reveal a significant result ($p = 0.17$). The trim-and-fill method revealed that one study was missing in order to create a symmetrical picture (Fig. 8). The imputed effect, however, was only slightly different from the effect without imputation, which means that statistically only a minor publication bias was indicated. It has to be considered, however, that the negative intercept of − 0.72 indicates that small-study-effects could have biased the pooled effect[35].

3.7. Risk of bias across studies – Grade evidence profiles

The quality of evidence for each outcome is displayed in Table 2. Evidence profiles for subgroup analyses are shown in Supplement I. Overall, the quality of evidence was consistently low to moderate for both short- and long-term effects of meditation, particularly due to different study limitations and imprecision. Study limitations were mainly caused by performance bias due to a lack of blinding. The quality of evidence was downgraded for imprecision when confidence intervals included both the non-effect and at least a small effect regardless of which direction. Additionally, in subgroup analyses, the quality of evidence was reduced due to imprecision since there were less studies per outcome and the recommended optimal information size not reached[49].

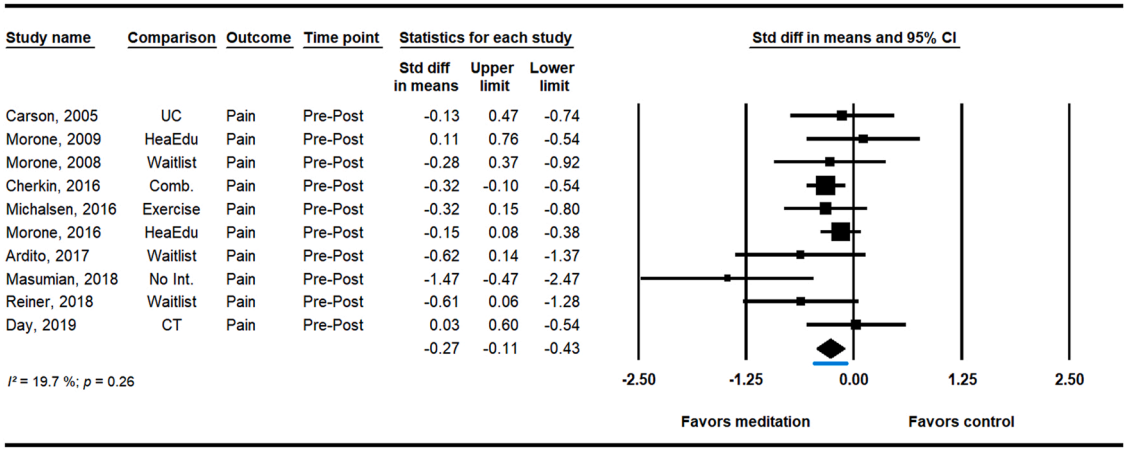

Fig. 4. Meta-analysis of short-term effects on pain intensity (time period 0–8 weeks). UC: usual care, HeaEdu: health education program, Comb.: Combination of usual care and cognitive therapy, No Int.: no intervention, CT: cognitive therapy.

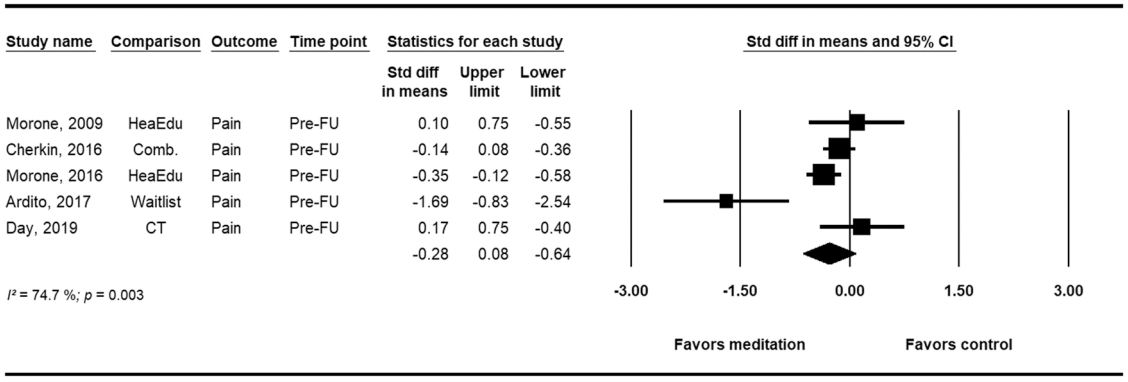

Fig. 5. Meta-analysis of long-term effects on pain intensity (time period 0–32 weeks). HeaEdu: health education program, Comb.: Combination of usual care and cognitive therapy, CT: cognitive therapy.

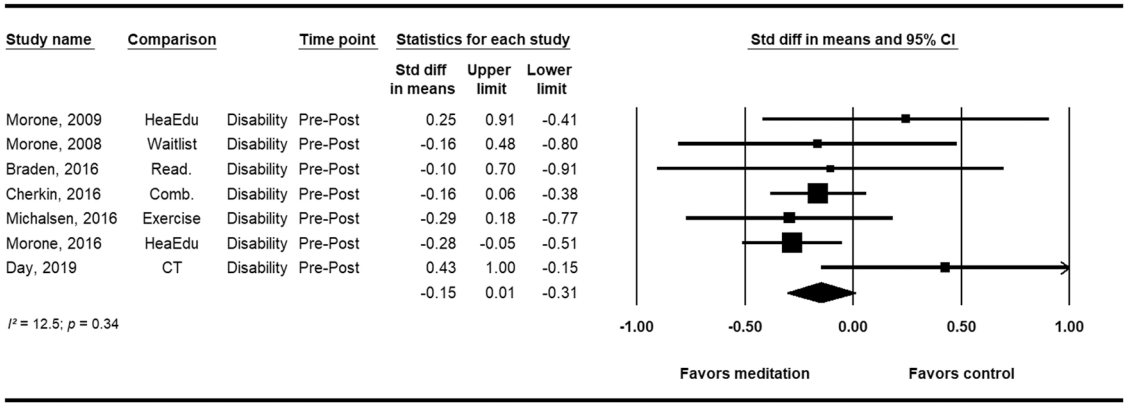

Fig. 6. Meta-analysis of short-term effects on functional disability (time period 0–8 weeks). HeaEdu: health education program, Read.: reading material on stress reduction techniques, Comb.: Combination of usual care and cognitive therapy, CT: cognitive therapy.

4. Discussion

4.1. Summary of evidence

The aim underpinning this systematic review with meta-analysis was to explore the effects of any meditation technique on LBP-related outcomes. The meta-analyses showed that meditation has significant positive, small short-term effects on pain intensity and physical quality of life in LBP patients, which could be confirmed by sensitivity analyses in both cases. These effects were not clinically relevant (−0.36 points on a NRS [95% CI, −0.15 to −0.57] for pain; 1.47 units [95% CI, 0.49–2.52] for physical quality of life respectively) (Summary-of-Findings table 21, Suppl. J), and were non-significant in the long-term. Evidence quality for both short-term effects was moderate due to study limitations, especially due to performance bias. For the reduction of functional disability there was a low quality of evidence for a marginal short-term effect that barely missed being significant. In the long-term there was no longer an observable effect. There was low quality of evidence for no

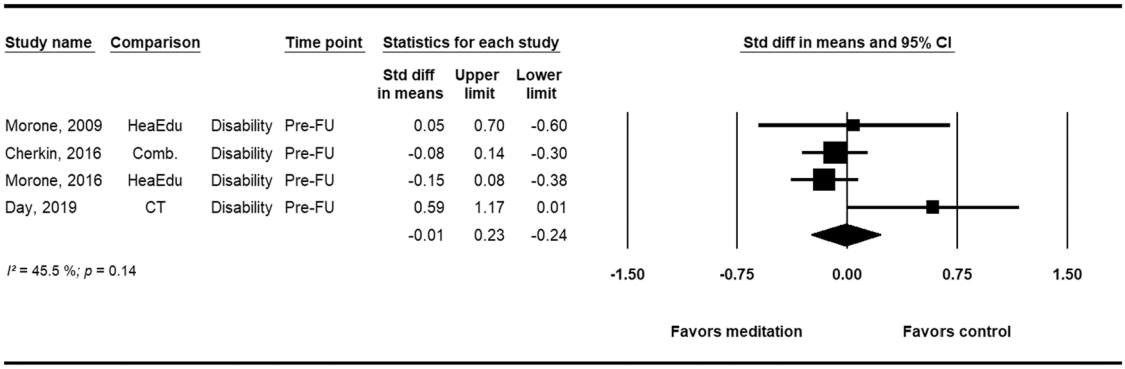

Fig. 7. Meta-analysis of long-term effects on functional disability (time period 0–32 weeks). HeaEdu: health education program, Comb.: Combination of usual care and cognitive therapy, CT: cognitive therapy.

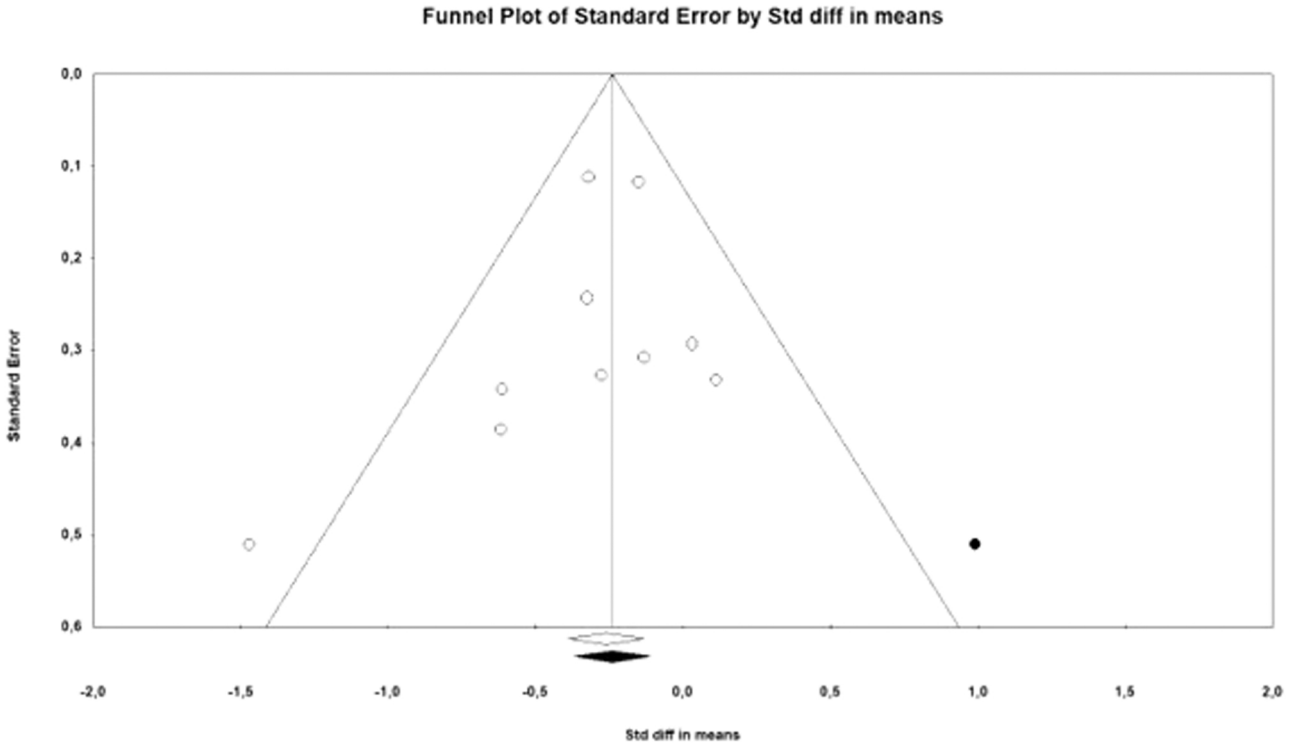

Fig. 8. Funnel plot for pain intensity after application of the trim-and-fill method.

effect on both the short- and long-term reduction of the mental quality of life and depression. Nevertheless, the study by Banth and Ardebil[39], which was excluded from the quantitative analyses since it caused substantial heterogeneity, showed large, significant and clinically relevant effects. It could be assumed that this study created heterogeneity due to sociocultural reasons. They had a female only sample which stemmed from Southeast Asia (Iran) where meditation is more common than in other parts of the world, especially within religious traditions. It could be speculated that people from this region have more trust in, and by this are more sensitive to, meditation. This idea is strengthened by the high effect estimates from the study of Masumian et al.[44] which also took place in Iran. However, the confidence of the authors of the work reported here in these effects was low due to unclear or high risk of bias in all but one domain.

Subgroup analyses showed that the short-term pain reduction was higher when meditation was compared to passive control groups only (Fig. 21, Suppl. H). Furthermore, for meditation vs. passive control groups short-term and small benefits were demonstrated in terms of reducing functional disability (Fig. 24, Suppl. H), increasing physical quality of life (Fig. 27, Suppl. H), and reducing depression (Fig. 33, Suppl. H). Sensitivity analyses showed that studies with a high risk of bias altered the pooled effects in most cases only marginally. In three subgroup meta-analyses, however, the study by Ardito et al.[47] caused substantial heterogeneity and was consequently excluded. The findings of this work are mainly in line with those of previous reviews with similar research questions. Anheyer et al.[23] found positive short-term effects of MBSR on pain intensity and functional disability in LBP patients. However, they reported that these effects were not sustained in the long-term, and that meditation demonstrated the highest effects compared to passive control groups, which is in agreement with the results obtained in this work. Cramer et al.[24] focussed on MBSR and MBC, and found only inconclusive evidence of the short-term effectiveness of MBSR in improving pain intensity and disability. However, the latter review included only a total of 3 studies. When looking at a wider set of chronic pain conditions such as migraine, rheumatoid arthritis, fibromyalgia, and other chronic musculoskeletal pain, reviews

Table 2
GRADE evidence profile.

	Quality assessment						Overview of results	
Time point	Number of participants (studies)	Study limitations (risk of bias)	Inconsistency	Indirectness	Imprecision	Publication bias	SMD (95% CI)	Overall evidence quality
Pain intensity								
Short-term	934[10] IG: 404 CG: 530	Serious study limitations[a](−1)	No serious inconsistency[b]	No serious indirectness[c]	No serious imprecision[d]	No risk of publication bias[e]	-0.27 (−0.43; −0.11); s. $p = 0.001I^2 = 20\%$; $p = 0.3$Small effect, favours meditation	⊕⊕⊕Moderate
Long-term	704[4] IG: 295CG: 409	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	-0.17 (−0.37; 0.03); n.s.$I^2 = 32\%$; $p = 0.2$Small effect, favours meditation	⊕⊕Low
Functional disability								
Short-term	832[7] IG: 358CG: 474	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	-0.15 (−0.31; 0.01); n.s.$I^2 = 12\%$, $p = 0.3$Small effect, favours meditation	⊕⊕Low
Long-term	704[4] IG: 295CG: 409	Serious study limitations[a](−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	-0.01 (−0.23; 0.24); n.s.$I^2 = 45\%$, $p = 0.1$No effect	⊕⊕Low
Physical quality of life								
Short-term	756[5] IG: 324CG: 432	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	No serious imprecision[d]	No risk of publication bias[e]	0.21 (0.07; 0.36); s. $p = 0.004I^2 = 0\%$; $p = 0.8$Small effect, favours meditation	⊕⊕⊕Moderate
Long-term	651[3] IG: 273CG: 378	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	0.05 (−0.11; 0.20), n.s.$I^2 = 0\%$; $p = 0.9$No effect	⊕⊕ Low
Mental quality of life								
Short-term	446[3] IG: 167 CG: 279	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	0.18 (−0.02; 0.37); n. s.$I^2 = 0\%$; $p = 0.8$ Small effect, favours meditation	⊕⊕ Low
Long-term	341[1] IG: 116 CG: 225	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	-0.04 (−0.26; 0.18); n.s. No effect	⊕⊕ Low
Depression								
Short-term	506[5] IG: 200CG: 306	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	-0.15 (−0.33; 0.03); n. s.$I^2 = 0\%$; $p = 0.9$ Small effect, favours meditation	⊕⊕ Low
Long-term	410[3] IG: 151CG: 259	Serious study limitations[a] (−1)	No serious inconsistency[b]	No serious indirectness[c]	Serious imprecision[f] (−1)	No risk of publication bias[e]	-0.05 (−0.25; 0.15); n. s.$I^2 = 0\%$; $p = 0.5$ No effect	⊕⊕ Low

[a] All but one study showed study limitations due to performance bias. Since the outcome is a subjective one, lack of blinding leads to an even higher risk of performance bias [54]. Additionally, most studies showed high risk for selective reporting, which was considered to be part of a study limitation but not as a publication bias.

[b] Heterogeneity < 50% and homogenous confidence intervals

[c] Patients and interventions of single studies did not differ significantly from patients and interventions of interest. Research questions of single studies matched the research question of this systematic review.

[d] The OIS-criterion of at least 400 participants was fulfilled. Since the effect was also significant, the estimated effect was considered to be precise.

[e] The applied tests in this work either did not demonstrate a distinct publication bias or were not applicable.

[f] The OIS-criterion of at least 400 participants was fulfilled. However, the confidence interval included both the non-effect and at least a small effect (regardless of which direction).

have been mainly focussed on mindfulness-based meditation programmes and show heterogenous results. Hilton et al.[22] demonstrated that there was a low quality of evidence for pain and physical health-related quality of life improvements, and a high quality of evidence for improvements in depression, whereas Ball et al.[20] only detected minor effects on pain intensity and Bawa et al.[21] could not find evidence for benefits in terms of pain intensity or depression. Nevertheless, there are several neuroscientific studies which provide an evidence base for potential mechanisms of meditation effects. It has been demonstrated that the anterior cingulate cortex (ACC) structurally and functionally adapts to regular meditation practice[50,51]. As the ACC is generally believed to be part of a neural network engaged in interoceptive awareness and more specifically in the affective sensation of pain[52,53], it is proposed in this work that this could be a potential neurophysiological link on how meditation ameliorates pain in chronic LBP patients. Moreover, regular meditation increases body awareness and by this might be helpful in facilitating a person's emotion regulation[51]. Therefore, as several emotional factors such as anxiety and fear go along with characteristic pain cognitions and are associated with the development and maintenance of LBP[9], meditation might have a positive impact on theses adverse psychological constructs and by this support beneficial changes in maladaptive beliefs and behaviours of LBP patients.

4.2. Limitations at study and outcome level

In general, poor reporting quality of the included studies impeded the risk of bias assessment. The included studies were slightly inclined to a high risk of bias, especially performance bias due to lack of personnel and participant blinding, which is even more problematic in terms of the present subjective outcomes[54]. Almost in half of all studies performing and/or reporting on an intention-to-treat-analysis was omitted.

Selective reporting bias was present or at least remained unclear in most cases since the availability of study protocols were very rare and four studies contained incomplete outcome data. In addition, the confidence levels in the estimates of the effect suffered from inadequate control groups, such as wait list control groups, which could have led to nonspecific effects that are not truly caused by the intervention itself but e.g., by social contact with other patients or the supervisor in a group setting. Follow-up-data were not available for every study and sometimes were not delivered on request either[46]. Long-term effects are, therefore, based on data from a few studies only. The GRADE quality of evidence was low to moderate. The main reasons for downgrading evidence were study limitations, publication bias and in some cases an insufficient number of participants.

4.3. Limitations at review level

In this systematic review several limitations have been revealed which must be addressed. Firstly, the included studies varied in terms of age and gender of the participants, causes and intensity of LBP, applied meditation technique, length of meditation sessions, control group and used measurement device, which hampers the external validity of our review. Furthermore, the study sample included two non-randomized studies which could impact the findings and robustness of the results. However, the present sensitivity-analyses excluded highly biased studies and didn't alter the main findings. Secondly, grey literature as well as records in languages other than English or German were neglected in the literature search. Thirdly, in the sensitivity analyses differences in the risk of bias were only considered although a differentiation for attendance rate or meditation practicing time (per session or in total) probably would have provided additional useful information. Fourthly, to some extent it was hard to classify the control groups as "active" or "passive" for subgroup analysis (e.g. 48). Fifthly, research article authors were asked for statistical data but not for other missing information such as study protocols or methodical aspects, which are intervention or participant characteristics that could have been useful to better estimate the risk of bias. In addition, the Cochrane RoB tool was used to judge the risk of bias for every study. However, since two studies were not randomized, another tool such as the ROBINS-I tool would have been more adequate for these. Sixthly, long-term data were pooled, although follow up time varied between studies substantially (4–52 weeks). Seventhly, the pre-registering of a review protocol was not done. However, a PRISMA reporting form can be found in Supplement K. Lastly, since previous systematic reviews about meditation in pain patients reported on rare and only mild adverse events, no adverse events in this systematic review were considered.

4.4. Conclusions

In conclusion, this systematic review reveals favourable short-term effects of meditation on pain intensity and physical quality of life compared to passive control groups. Compared to active control groups meditation was not superior. The effects were neither clinically relevant nor sustained in the long-term. The findings should be interpreted with caution since the quality of evidence was low to moderate. Therefore, meditation seems to be promising in reducing pain-related complaints in patients with LBP, but no explicit implications for clinical practice can be given at this point of time. Consequently, larger RCTs with higher methodological quality are needed. The following recommendations can be made for future studies: a priori sample size calculations should be performed to reach sufficient statistical power; study protocols should be registered and registration should be quoted to facilitate judgement of reporting bias; by using sham meditation as a comparison[55] and by concealing research hypotheses performance bias could be reduced; when using other kinds of active control groups the setting, length and frequency should be comparable to the meditation group; future studies should ensure a higher attendance rate and evaluate adverse events; in order to increase external validity more attention should be paid in studies to a balanced percentage of male and female participants, higher representation of ethnic minorities and patients with lower level of education; and lastly, key variables of meditation application such as total length of intervention and frequency of weekly sessions should either adopt the values of previous studies, to be more comparable, or clearly be varied in order to titrate dose-response relationships.

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Author contributions

All authors contributed to the study. Conception and design of the study were performed by Helena Schmidt (HS) and Christian Pilat (CP). Search in electronic databases was performed by HS and CP, data collection and data analysis were performed by HS. Interpretation of data was performed by both authors. All authors read and approved the final manuscript.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.ctim.2023.102924.

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