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The efficacy of dry cupping compared to placebo cupping for people with musculoskeletal complaints: a systematic review with meta-analysis

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Abstract

Objective: To compare the effects of dry cupping and placebo cupping in adults with musculoskeletal pain, in the short- (1-week), medium- (4-weeks) and long-term (6-months).

Design: Intervention systematic review with meta-analysis

Literature search: Medline, EMBASE, CENTRAL, AMED, CINAHL, PEDro and Web of Science were searched until June 2025.

Study selection criteria: Design: Randomised clinical trials; Participants: People with musculoskeletal complaints; intervention: dry cupping; control: placebo cupping; primary outcome: pain intensity.

Data synthesis: Risk of bias was assessed using the Cochrane Risk of Bias 2.0 tool. Pain scales were converted to a 0 to 100 scale, with a meaningful difference threshold of 15 points. Random effects meta-analyses were conducted. We judged the certainty of evidence using the Grading of Recommendations Assessment, Development and Evaluation (GRADE) framework.

Results: 3330 records were screened; 5 trials (n=281) were included. There was very-low-certainty evidence that dry cupping was trivially harmful to meaningful beneficial when compared to placebo cupping in the short-term (MD: -9.9, 95%CI: -30.5 to 10.7, 4 trials, n=243) and long-term (MD: -2.2, 95%CI: -11.8 to 7.4, 1 trial, n=52). There was very-low-certainty evidence that dry cupping was effective compared to placebo cupping in the medium-term; the effect may be clinically meaningful (MD: -17.2, 95%CI: -33.0 to -1.4, 1 trial). Four trials were at high overall risk of bias.

Conclusion: The effects of dry cupping on pain was very uncertain at all timepoints. Treatment alternatives with higher certainty evidence of effectiveness should be trialled before cupping.

Open Science Framework registration: https://osf.io/3fa9m/

Keywords: dry cupping; sham; function; musculoskeletal pain, hijama

Background

Dry cupping is a non-invasive technique purported to treat musculoskeletal pain, improve blood circulation, improve immune responses, detoxify blood and provide anti-inflammatory effects[3]. Dry cupping usually involves placing cups over the primary area of pain. Negative pressure is created by a suction device, with the cup remaining *in situ* for several minutes[2]. Frequently, multiple cups are placed around the painful areas and sometimes cups are moved over the painful areas or acupoints during treatments[2]. The mechanisms of dry cupping for reducing pain are currently unknown and could be due to improved circulation, muscle relaxation and reduced inflammation, with multiple theories being proposed[3].

Given that the mechanisms for pain relief are still unknown, it is possible that dry cupping is effective due to the placebo effect—outcomes improve (e.g., reduced pain) because patients believe the treatment is effective, not because of a mechanism of action of the treatment itself[7]. Well-performed placebo-controlled trials can differentiate the placebo effect from a true treatment effect. Such trials can be hard to perform for some physiotherapy interventions (e.g., exercise) but can be performed for passive modalities such as dry cupping[20]. For new interventions, like dry cupping, ideally its efficacy (when compared to placebo interventions) should be evaluated before making strong clinical recommendations on the use of dry cupping in clinical practice. The next step would be to evaluate its effectiveness when compared to other interventions. Some reviews have included randomised trials investigating the effectiveness of dry cupping for musculoskeletal pain, against another intervention, combined with another active intervention or compared to no-treatment controls[42,43]. Although these reviews are valuable, they do not provide evidence that the treatment effect of dry cupping is greater than the effect of a placebo.

To date, no systematic review has investigated dry cupping compared to placebo cupping. The true treatment effect of dry cupping interventions is unknown. Therefore, the primary aim of this systematic review was to estimate the efficacy of dry cupping for reducing musculoskeletal pain in the short-, medium- and long-term. Secondary aims were to estimate the efficacy of dry cupping for improving function, and describe the success of blinding of the placebo cupping intervention and reporting of adverse events/harms.

Methods

This is a systematic review with meta-analysis. The reporting follows the guidelines proposed by Preferred Reporting Items for Systematic Reviews and Meta-Analyses 2020 (PRISMA 2020) guidelines[31]. We conducted the systematic review using the recommendations from the Cochrane Handbook for Systematic Reviews of Interventions[15]. The protocol was pre-registered on the Open Science Framework on December 6, 2023 (https://osf.io/3fa9m/).

Eligibility criteria

Design: We included randomized controlled trials that used parallel or cross-over designs, published in peer-reviewed journals. Quasi-randomized and non-randomized trials were excluded. There were no language restrictions, but trials in languages other than English, Dutch and German were classified as 'awaiting assessment'.

Population: People who were ≥18 years old and sought treatment for a musculoskeletal complaint in any area of the body for any duration. We defined 'musculoskeletal pain' as "*pain related to known pathological conditions affecting the muscles, bones, or joints, such as osteoarthritis, inflammatory arthritis, and diseases of connective tissue, or due to unknown pathology in a particular body region such as "non-specific low back pain*."[33] Pain caused by specific pathology (e.g., pulmonary, cardiac, neurological, oncological) or trials including participants within 12 months of a surgical intervention (e.g. spinal surgery) were excluded.

Intervention and Comparator: The interventions included used dry cupping only. Dry cupping was defined as the placing of cups on a patient's skin over the area of pain (or related acupoints) where a suction device creates a negative pressure within the cup with the cup remaining on the skin for several minutes [2]. We compared dry cupping to placebo cupping. Placebo cupping was defined as an intervention that intended to mimic the dry cupping intervention but missed components such as creating negative pressure[28]. Trials that combined dry cupping with a co-intervention, and the same co-intervention was combined with placebo cupping were included. Trials that combined dry cupping with adjunct treatments, such as acupuncture, that were not present in intervention and comparator groups were excluded. Wet cupping and bloodletting (with cupping) interventions were also excluded.

Outcome: Trials must have measured pain after the intervention to be included. Secondary outcomes were function and adverse events, if presented in the included trials.

Timepoints: Trials were included if they measured pain intensity or function up to 6 months post-intervention. We accepted pain however it was measured in trials (e.g., average 24-hour pain or worst pain). If trials reported multiple pain measures, we used the measure that most likely reflected their average daily pain. We defined the timepoints as short-term (immediately post intervention to 1-week post-intervention), medium-term (1-week post-intervention to 4-weeks post-intervention) and long-term (4-weeks post-intervention to 6 months post-intervention). When there was more than one measurement within a timepoint, the measurement closest to the end of the intervention was chosen for the short-term timepoint, the measurement closest to 4 weeks was chosen for the medium-term timepoint and the measurement closest to 6 months was chosen for the long-term timepoint.

Information Sources and Search strategy

We searched Medline (Ovid), EMBASE (Ovid), CENTRAL (Wiley), AMED (Ovid), CINAHL (EBSCO), Physiotherapy Evidence Database (PEDro) and Web of Science Core Collection on June 24, 2025. We combined search terms related to 'cupping' with randomized trial filters provided by Cochrane[29] for Medline, EMBASE and CINAHL and a placebo filter[9] adapted for Web of Science, to limit the number of records. The search strategies for each database are provided in Appendix 1. The search strategies were developed in consultation with an information specialist with experience in systematic reviews.

We searched ClinicalTrials.gov and the World Health Organization (WHO) International Clinical Trials Registry Platform (ICTRP: apps.who.int/trialsearch) on July 14, 2025, for protocols related to our research question. All ongoing clinical trials that were potentially eligible and did not have an associated publication (with results) were listed. We did not search grey literature.

We performed backwards and forwards citation tracking in Web of Science on June 24, 2025 and checked the references or any relevant reviews[35,37,42–45] to identify potentially eligible trials.

Selection Process

Search results were deduplicated in EndNote (version 20) and uploaded to Covidence for screening. We automatically excluded non-randomized trials using the Covidence automation tool that identifies and removes non-randomized trials[10]. Following this, 2 review authors independently assessed the title and abstract of each report and removed ineligible reports. Two review authors independently assessed full texts of potentially eligible reports. Reasons for exclusion were provided at the full-text stage. In both screening phases, disagreements were resolved by discussion or with consultation with a third review author if an agreement could not be reached.

Data Collection Process

Data extraction for each trial was independently conducted by 2 reviewers. Any disagreement was resolved by a third independent reviewer. Data were extracted and compiled into a custom-made data extraction form in Microsoft Excel (Version 2408 Build 16.0.17928.20114). We extracted the following information: author names, year of publication, country/region, study design, conflict of interest, funding source, sample size of the intervention and control groups, age/gender of participants, details on patient diagnosis (such as pain location duration and intensity), details of the intervention and placebo (details of the cupping procedure [cups used, location of cupping], time of session, number of sessions per week, number of weeks of treatment, administering practitioner)), duration of follow up, relevant outcome measures and assessment of the success of blinding with the placebo cupping group.

TIDieR-Placebo: We also reported intervention details in line with the TIDieR-Placebo[17]. The TIDieR-Placebo[17], an extension to the TIDieR scale[16], is used to assess the completeness of reporting of trials that use placebo interventions. It contains 13 items, with each item being rated "Yes" or "No". For "Yes", items were clearly and fully reported. For 'No", items were either not reported or not fully reported. Each trial was rated independently by 2 trained raters, with disagreements being resolved by discussion.

Adverse events: We noted if trials reported adverse events/harms or not. When trials reported adverse events, these were classified as minor (such as bruising or skin reactions, syncope or pre-syncope, increased pain or stiffness that resolves, spread of pain that resolves), moderate (such as brief hospitalization, fractures, a medical procedure that required admission) and major (e.g. prolonged hospitalization, significant morbidity or death). Where available, we used the explanation from each trial to determine likely causality and severity.

Risk of Bias (RoB) assessment

Two review authors assessed risk of bias using the Cochrane Risk of Bias 2.0 tool for randomized trials[39]. We used the Excel Workbook and input algorithm provided by Cochrane to perform the assessments[39]. Disagreements between raters were resolved by discussion or by consultation with a third review author if there was no agreement. Risk of bias was assessed for pain and function variables for short-, medium- and long-term timepoints. The domains investigated were bias arising from the randomization process (domain 1), bias due to deviations from intended interventions (domain 2), bias due to missing outcome data (domain 3), bias in measurement of the outcome (domain 4), bias in selection of the reported result. (domain 5). Signaling questions for each domain determined the domain level bias. Domain level bias was classified as 'low RoB', 'some concerns', or 'high RoB'. We defined the overall (trial level) RoB as 'low' when an outcome was judged low RoB on all domains. We defined the trial as having 'some concerns', if there were some concerns in at least one domain, but not high RoB for any domain. Trial level RoB was defined as 'high' when an outcome was high RoB in at least one domain or some concerns for multiple domains, in a way that substantially lowers confidence in the result. Risk of bias results were plotted using the robvis visualization tool (https://www.riskofbias.info/welcome/robvis-visualization-tool).

Data synthesis and statistical analysis

Effect Measures: Continuous data were analysed and presented using mean differences (MDs) and 95% confidence intervals (CIs). For the primary outcome (pain), the Numerical Rating Scale (NRS) and Visual

Analogue Scale (VAS) were standardised onto a 0–100-point scale for the quantitative syntheses, with a 15-point difference considered clinically meaningful [12,13]. When trials used scales other than the NRS or VAS scales, these were reported but not aggregated. For function, if outcomes measures were similar, these were aggregated using the common scale. If outcome measures were dissimilar between trials (e.g., used the Oswestry Disability Index (ODI) and Roland Morris Disability Questionnaire[21]), data were analysed and reported using standardised mean differences and 95% CI's, with a 0.35-point difference between groups considered clinically meaningful as this falls between a small and moderate effect [8] and has been used previously[26]. If another outcome measure was presented, an arbitrary value of 15% of the scale range was deemed clinically meaningful. We used the recommendations of Kamper[22] to summarise the range of estimates from the lower- to upper-bounds of the 95%CIs. Comparisons with 95%CIs below the clinically meaningful effect were deemed trivial and comparisons with 95%CIs that exceeded the minimally important effect were deemed meaningful. These were deemed beneficial or harmful depending on if they favoured the intervention or control, respectively.

Dealing with missing data: If data were collected but not reported or were unclear, we contacted the corresponding author for the necessary values. When no measure of variability was presented (and authors did not respond), we estimated the standard deviation from baseline standard deviation or the most similar trial in the review.

Heterogeneity: I^2 statistic was provided but not used to measure heterogeneity. Instead, we calculated the prediction intervals to measure the range in which the true effect of the intervention is expected to fall as recommended by others [5,6,30]. We determined considerable heterogeneity when prediction intervals crossed the smallest clinically worthwhile effect (in a positive or negative direction) and no change.

Meta-analyses: Quantitative data from individual trials were combined using a random effects meta-analysis for each outcome (pain and function) and each timepoint (short-, medium- and long-term) using RevManWeb (Version 9.6.0). Intention-to-treat data were used over per-protocol or as-treated data.

Assessing the certainty of the evidence. Grading of Recommendation, Assessment, Development, and Evaluation (GRADE) was used to assess the overall certainty of evidence for the primary outcome (pain)[36]. We classified evidence as high certainty, moderate certainty, low certainty and very low certainty. The certainty of evidence started at high certainty and was downgraded for risk of bias (downgraded by one level if greater than 25% of participants were from trials with high risk of bias and by two levels if more than 50% of participants were from trials with high risk of bias); inconsistency and

imprecision (downgraded by one level when the prediction interval included the smallest clinically worthwhile effect or 'no effect', by two levels when the prediction interval included the smallest clinically worthwhile effect and 'no effect' and by 3 levels when the prediction interval were wide and included the smallest clinically worthwhile effect (favouring the intervention), 'no effect' and the smallest clinically worthwhile effect (favouring the control))[30]. Indirectness (downgraded by one level if more than 50% of the participants were not from the target population); Small study bias (downgraded by 1 level if selective reporting was observed through visual inspection of funnel plots. This domain was only analysed if more than ten trials were included). For inconsistency and imprecision single trials with fewer than 400 participants (on continuous outcomes) started at low-certainty evidence and the confidence interval was used instead of the prediction interval.

Sensitivity and subgroup analysis: This was planned for assessing trials with high placebo fidelity or low placebo fidelity only if >10 trials could be included.

Deviations from protocol

We intended to include quasi-randomised trials. We changed the eligibility criteria to only included randomized trials to ensure the included trials were as robust as possible. We had also only included a risk or bias tool appropriate for randomised trials (Cochrane ROB-2.0) and not the recommended tool for quasi randomised trial (Risk Of Bias In Non-randomised Studies - of Interventions) (Note: No trials were only excluded for having quasi randomisation). We intended to use the recommendations of Cochrane for measuring heterogeneity (using I^2). Due to limitations with using I^2, we provided prediction intervals for each meta-analysis with more than one trial[5,18,41]. To ensure consistency with these methods, we used prediction intervals instead of I^2 for our GRADE interpretation. We changed and combined our GRADE assessment of inconsistency and imprecision to enable using prediction intervals, following the recommendations of Murad et al.[30].

Results

The database search returned 3,330 records (Figure 1). Following removal of duplicates and removal of records marked ineligible by automation tools, 1,525 unique records remained. After title and abstract screening, 96 full texts were screened with five trials[4,27,34,38,40] included in the qualitative synthesis and four trials[4,27,34,38] included in the quantitative synthesis. Authors were contacted for additional details on randomization for two trials[38,40], and provided responses allowing the trials to be included. Two trials[27,38] were 3-arm RCTs so only details from the dry cupping and placebo cupping groups were extracted. Protocols of likely relevant ongoing trials are shown in Appendix 2.

<Insert figure 1 about here>

Figure 1: PRISMA flow chart[31] for selection of included trials and protocols

Characteristics of included trials

Participants. The trial characteristics and intervention details are presented in Table 1. The trials included participants with chronic nonspecific low back pain[4,34,40], fibromyalgia[27] and non-specific neck pain[38] (Table 1). Total sample sizes ranged from 21 to 95 participants (ranging from 11 to 47 in the dry cupping group and 10 to 48 in the placebo cupping group) with an approximate 1:1 allocation between groups. There was a total of 281 participants, with 140 in the dry cupping group and 141 in the placebo cupping group. Four trials[4,27,34,38] provided information on age and sex for both groups. For these trials, 55 males and 199 females were included with ages ranging from 23 to 56 years.

Intervention. Two trials[38,40] had dry cupping performed for 1 session, two trials[27,34] had dry cupping performed for 5 sessions over 2.5 weeks and one trial[4] had dry cupping performed for 8 sessions over 8 weeks. The duration of the sessions ranged from 8 min to 30 min. The number of cups ranged from 1 to 17 with four trials using a pressure of 300 millibars and one using 2 'suction pumps'.

Placebo. For placebo cupping, all trials[4,27,34,38,40] used cups with a hole in the cup to release the pressure. Four trials[4,27,34,38,40] used tape to affix the placebo cup. One trial[27] also affixed tape in the dry cupping group and for three trials[4,34,38] this was unclear.

<Insert table 1 about here>

Table 1: Trial characteristics and intervention details

Risk of Bias

All included trials reported pain as an outcome in the short-term[4,27,34,38,40], one trial in the medium-term[34] and one in the long-term[27] (Figure 2). For the trials that reported medium- and long-term pain outcomes, the RoB was the same as for the short-term pain outcomes. For short-term pain, the overall RoB was low for one trial[4], unclear for another[34] and high for three trials[27,38,40]. When trials were at high RoB, this arose from bias due to deviations of the intended intervention[38,40], bias in the measurement of the outcome[38] and bias in the selection of the reported result[27,40].

<Insert figure 2 about here>

Figure 2: Risk of bias summary for pain

Four trials reported function as an outcome in the short term[4,27,34,40], one trial in the medium-term[34] and one trial in the long-term[27] (figure 3). For function, the RoB was the same as for pain except for one trial[27] that had high risk of bias for bias in selection of the reported result for pain and some concerns for function.

<Insert figure 3 about here>

Figure 3: Risk of bias summary for function

Pain

Short-term: Five trials[4,27,34,38,40] (n=283) measured pain in the short-term; 1 trial[40] did not provide outcome data for the placebo cupping group. There was very low certainty evidence (downgraded due to risk of bias (1 level), inconsistency + imprecision (3 levels)) that dry cupping was trivially harmful, had no effect or had a trivial or large benefit when compared to placebo cupping (MD: -9.9, 95%CI: -30.5 to 10.7, 4 trials, n=243, I^2=85%, 95% prediction interval: -52.8 to 33.0) (Figure 4, table 2).

<Insert figure 4 about here>

Figure 4: Forest plot of dry cupping versus placebo cupping for pain in the short-term

Medium-term: One trial[34] (n=37) measured pain in the medium-term. There was very low certainty evidence (single trial, downgraded due to inconsistency + imprecision (2 levels)) that dry cupping was effective, and the effect maybe meaningful compared to placebo cupping (MD: -17.2, 95%CI: -33.0 to -1.4, 1 trial, n = 37) (table 2).

Long-term: One trial[27] (n=52) measured pain in the long-term. There was very low certainty evidence (single trial, downgrade due to RoB (2 levels)) that dry cupping had no effect or was trivially effective or trivially harmful compared to placebo cupping (MD: -2.2, 95%CI: -11.8 to 7.4, n=52, 1 trial) (table 2).

Function

Short-term: Four trials[4,27,34,40] (n=262) measured function in the short-term however one trial[40] did not provide outcome data for the placebo cupping group. Two trials[4,34] used the Oswestry Disability Index and 1 trial[27] used the Fibromyalgia Impact Questionnaire. There was very low-certainty evidence (downgraded due to imprecision + inconsistency (3 levels)) that dry cupping was meaningfully harmful, trivially harmful, had no effect, was trivially beneficial or meaningfully beneficial when compared to placebo cupping (SMD: -0.32, 95%CI: -1.27 to 0.63, n=222, 3 trials, I^2=54%, prediction interval: -1.80 to 1.16) (Figure 5, table 2).

<Insert figure 5 about here>

Figure 5. Forest plot of dry cupping versus placebo cupping for function in the short-term

Medium-term: One trial[34] (n=37) measured function in the medium-term using the Oswestry Disability Index (range: 0 (no disability) – 100 (maximum disability)). There was very low certainty evidence (single trial (downgraded due to imprecision + inconsistency (1 level)) that dry cupping had no effect or was trivially effective or trivially harmful compared to placebo cupping (MD: -4.2, 95%CI: -9.9 to 1.6, n=37, 1 trial) (table 2).

Long-term: One trial[27] (n=52) measured function in the long-term using the Fibromyalgia Impact Questionnaire (range: 0 (no impairments) – 100 (severe impairments)). There was very low certainty evidence (single trial (downgraded due to imprecision + inconsistency (1 level)) that dry cupping had no effect or was trivially effective or trivially harmful compared to placebo cupping (MD: -4.3, 95%CI: -12.2 to 3.6, n = 52, 1 trial, table 2).

<Insert table 2 about here>

Table 2: Summary of findings table for the outcomes pain and function in the short-, medium- and long-term.

Reporting of adverse events

Three out of 5 trials[4,27,34] reported on adverse events. Two trials[4,34] reported minor adverse events only and 1 trial[27] reported minor and moderate adverse events. In 1 trial[34], the (minor) adverse event was discoloration of the skin in 19 people (it did not state if this was in the intervention group only or in the intervention and placebo cupping groups). One trial[4] reported 3 (minor) adverse events in the dry cupping group and no adverse events in the placebo cupping group. One patient had increased back pain at the beginning of the treatment (and was referred to a General Practitioner after treatment was immediately stopped), and 2 patients developed flu-like symptoms. One trial[27] reported 5 (minor) adverse events in the dry cupping group and 3 (1 minor, 2 moderate) adverse events in the placebo cupping group. In the dry cupping group, 2 patients had increased pain after dry cupping, 1 had bruised ribs, 1 developed flu-like symptoms and 1 patient developed acute torticollis with pain radiating into the arm which resolved without further treatment within days. In the placebo cupping group, 1 patient had a torn meniscus, 1 patient had persistent pain after a spinal operation and 1 patient developed flu-like symptoms. The timing of the moderate adverse events in relation to the placebo cupping intervention was not reported and the causality was unclear.

TIDieR-Placebo

The median TIDieR Placebo score was 7 (IQR: 7-9) for dry cupping and 7 (IQR (6-9) for placebo cupping (table 3). In one trial[40] a TIDieR item differed between the dry cupping and placebo cupping groups. All trials[4,27,34,38,40] reported item 1 (Brief name describing the intervention), 3 (Describing physical or information materials used in in the intervention) and 4 (Describing the procedure, activities and processes in the intervention), one trial[38] reported item 9 (how the intervention was personalized, titrated or adapted) and no trials reported item 10 (modifications to the intervention during the course of the trial). Three trials[4,27,34] reported item 12 (whether trials reported adherence or placebo fidelity) in both groups, while 1 trial[40] reported this only in the dry cupping group.

<Insert table 3 about here>

Table 3: Completeness of reporting according to the TIDieR placebo

Success of placebo blinding

Two out of 5 trials[4,27] assessed the success of blinding of the placebo. One trial[4] reported that the placebo cupping intervention was effective at mimicking dry cupping, as 60% of patients in the placebo cupping group believed they had received real dry cupping (compared to 49% in the dry cupping group) and 40% believed they had received placebo cupping (compared to 51% in the dry cupping group). The other trial[27] reported that placebo cupping was ineffective at mimicking dry cupping, as 80% of patients in the dry cupping group and 73% of patients in the placebo cupping group successfully identified their group allocation.

Discussion

There was very low certainty evidence for pain and function in the short-, medium- and long-term timepoints meaning we are very uncertain about the likely effects of dry cupping against placebo cupping. Three trials reported minor adverse events. One trial reported moderate adverse events, although it was unlikely that dry cupping or placebo cupping caused these events. Only 2 trials[4,27] measured the effectiveness of blinding of placebo cupping, and of those, only 1 effectively blinded participants. The only trial of low risk of bias had an effect estimate for pain in the short-term favouring placebo cupping. The reporting of interventions was variable between trials and similar between dry cupping and placebo cupping groups.

Comparison with existing literature

We could not find any reviews focused on the efficacy of dry cupping versus placebo cupping. One review published in 2017 on dry and wet cupping compared to medical management or usual care in people with non-specific low-back pain showed statistically significant benefits favouring (dry or wet) cupping for pain and function in the short-term[43]. Like us, they found very high heterogeneity in their included trials, but did not perform a GRADE analysis. We suggest it is likely that the certainty of evidence would be low or very low. This means there are very few high-quality randomised trials investigating cupping against placebo cupping or other therapies, meaning low or very low certainty of any treatment effect. Given this, clinicians should be cautious recommending or applying dry cupping in clinical practice. The only trial of low risk of bias, that successfully blinded patients and was the most completely reported, showed no difference between placebo cupping and dry cupping for pain or function[4]. That trial only reported outcomes in the short-term, so more high-quality trials are required to estimate the effects in the medium- or long-term.

Dry cupping interventions were highly variable, ranging from 1 to 8 sessions, provided once or over 2.5 to 8 weeks. Interventions used 1 to 17 cups placed on acupoints or areas of pain. This wide variability in dry cupping protocols could be a reason for the variations of treatment effects between trials. We are unaware of standardised dry cupping interventions that have been proposed for patients with pain conditions (such as standardised cup locations or number of cups). Although this will likely vary based on area and musculoskeletal complaint, more high-quality trials comparing different dry cupping protocols with appropriate placebos are required to determine if and how intervention parameters matter in the delivery of dry cupping interventions. In the current review, we were unable to perform subgroup analysis comparing factors within dry cupping interventions as only 5 trials were included.

Previous reviews have included comparisons of dry cupping with usual care[43,44] and in healthy people[44]. Usual care included medications (diclofenac and acetaminophen), interferential, Graston massage, passive stretch and muscle relaxation[43,44]. These reviews found that the use of dry cupping may be beneficial compared to alternative interventions. The effect of placebo interventions can be large when assessing within group changes in 2-arm trials, using interventions such as placebo acupuncture[25] and placebo ultrasound[24]. However, a study that assessed pain in sixteen 3-arm trials with a conservative intervention, placebo, and no treatment group showed that the placebo effect accounted for only 18% of the improvement in pain [32]. Although that study had low or very low certainty recommendations, and did not include dry cupping trials, we are uncertain of the size of the placebo effect for dry cupping interventions.

Despite this, it is important to take care when comparing treatments when the efficacy of the treatment (e.g., dry cupping) has not been established. There may be no or some difference between two ineffective or partially effective interventions. Without initially establishing the efficacy of dry cupping, comparing dry cupping to other (in)effective interventions does not provide information on whether dry cupping is effective. Numerus trials compared dry cupping to a waitlist or no intervention[44]. This likely overestimates the effects of dry cupping as it includes the placebo effects and other non-specific effects. There is ongoing debate about whether treatments that are no better than mainly/solely placebo, should be used in clinical practice, especially when effective treatments are available[11]. However, this debate is premature given the high risk of bias in placebo-controlled trials on dry cupping.

Three trials reported mild adverse events linked to dry cupping and/or placebo cupping. One trial reported moderate adverse events in the placebo cupping group although the trials did not provide details on the likely causality to the treatment session. In that trial, 1 patient had a torn meniscus, and 1 patient had persistent pain after a spinal operation. Although these are unlikely related the intervention, it was not explicitly stated in the trials. Adverse events are frequently reported inadequately, including over or under reporting of adverse events in clinical trials[1]. When reporting adverse events, it is important to provide an indication of the likelihood of causal relationship of the adverse event and the treatment (or placebo treatment) provided so readers can determine if these are plausibly related[1]. The trials that reported adverse events could have reported the likely causality of the relationship to the adverse event, which would have aided our interpretation of the adverse event reported. It is recommended that trials consult recommendations to discuss causality[1,46] and use guidance from the CONSORT-harms checklist[19]

Strengths and limitations

This is the first systematic review that was pre-registered, assessed the risk of bias and provided a GRADE assessment for pain and function. Further, our search was comprehensive and included MEDLINE, EMBASE and CENTRAL, as recommended by the MECIR guidelines as well as searching for ongoing trials in the WHO clinical trials database[14].

The smallest clinically worthwhile effect was based on previous studies which compared the intervention against a no intervention comparator. Ideally, the smallest worthwhile effect would be specific to the context (e.g., the intervention performed against the appropriate (placebo) comparator) [23]. There is currently no literature that provides estimates of clinically meaningful effects based on the contextual factors used in this review. Given this, recommendations could change if a lower or higher clinically meaningful effect is determined in the future.

Another limitation is that only 5 (small) trials were included, there was high statistical heterogeneity and only one trial that was low risk of bias. Reasons for high statistical heterogeneity could be the clinical diversity of the trials included. These trials included people with specific diagnoses (fibromyalgia, non-specific low back pain) and had diverse dry cupping interventions. This affects the certainty of the recommendations from our review. Due to the low sample size, we were unable to pool similarly-delivered dry cupping interventions in a subgroup analysis. Only one trial (of low risk of bias) effectively blinded placebo cupping. In all other trials we do not know if placebo cupping was delivered as intended. This hampers the credibility of the findings and may mean that treatment estimates are overestimated. Another limitation is that the TIDieR placebo is supposed to be used as a reporting guideline and not an evaluative tool, as we have done in the current study.

Recommendations/implications

We cannot recommend dry cupping as a sole intervention for musculoskeletal pain or function in the short-, medium or long-term. The evidence is very uncertain about the effect of dry cupping on pain although moderate/severe adverse events are unlikely. Treatments for musculoskeletal pain such as exercise, education and advice or cognitive functional therapy should be first-line treatment with dry cupping used as an adjunct therapy, if used at all. More trials with low risk of bias and larger sample sizes should be performed to increase the certainty of the recommendation. In addition, interventions should be reported more completely so the treatments can be replicated by trialists or clinicians. Adverse events should be reported more transparently and with more detail so readers can determine causality of these to the treatment.

Conclusion

There was very low certainty evidence that dry cupping was not effective compared to placebo cupping in the short or long-term for improving pain. There was low certainty evidence that dry cupping was effective in the medium-term, and this effect may be clinically worthwhile. The completeness of reporting of dry cupping and placebo cupping interventions was variable.

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Declaration of Interest statement

We report no competing interests.

Funding statement

This work is unfunded.

Key points

Findings: Dry cupping was no better than placebo cupping for reducing pain in the short-term or long-term. In the medium-term, based on 1 trial, dry cupping was better than placebo cupping for pain.

Implications: As there are higher quality interventions for musculoskeletal pain, dry cupping should be used cautiously and only as an adjunct therapy.

Caution: It is uncertain if dry cupping is effective for reducing pain at any timepoint post-intervention. Effects could be due to the placebo effect and/or non-specific treatment effects.

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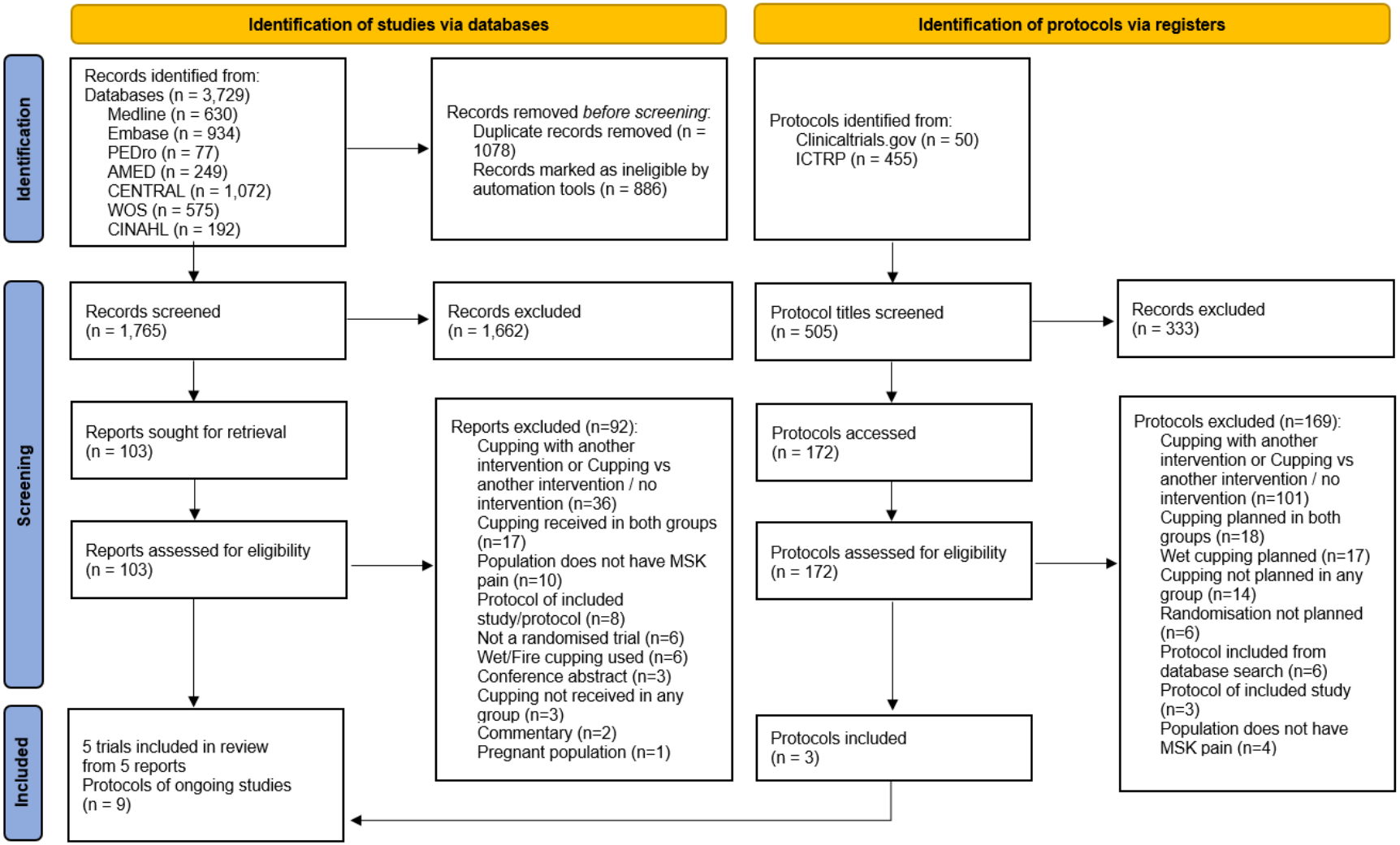


FIGURE 1. PRISMA flow chart[31] for selection of included trials and protocols.

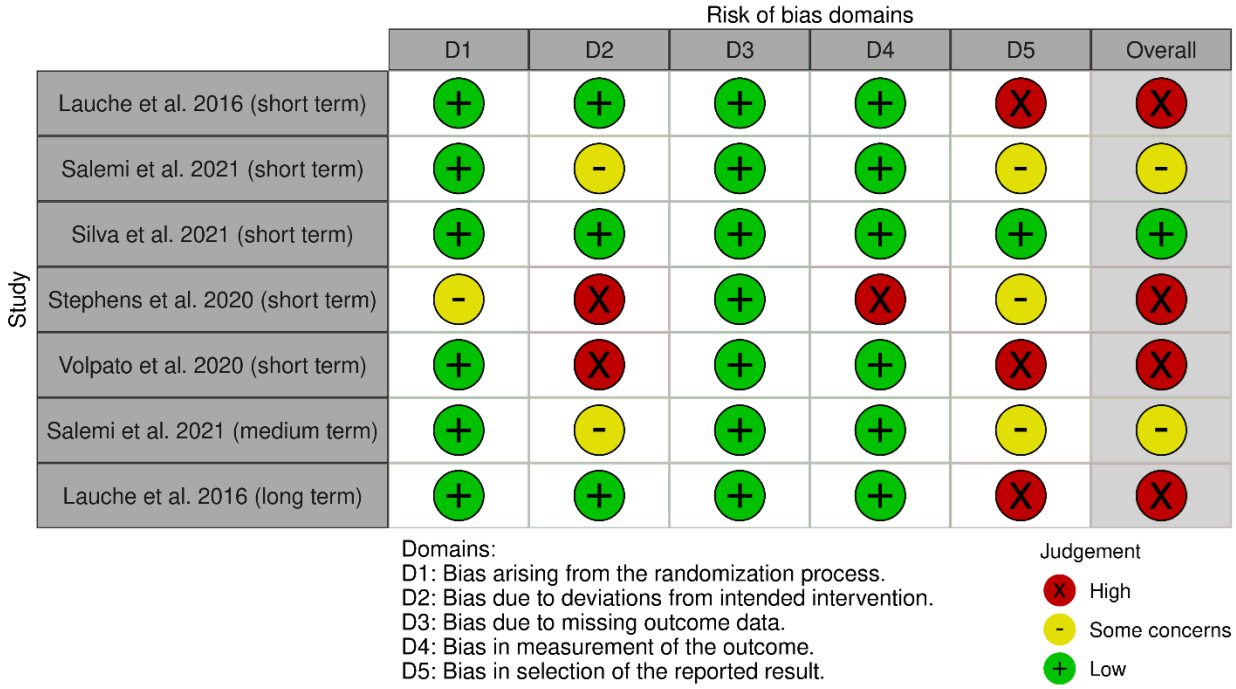


FIGURE 2. Risk of bias summary for pain.

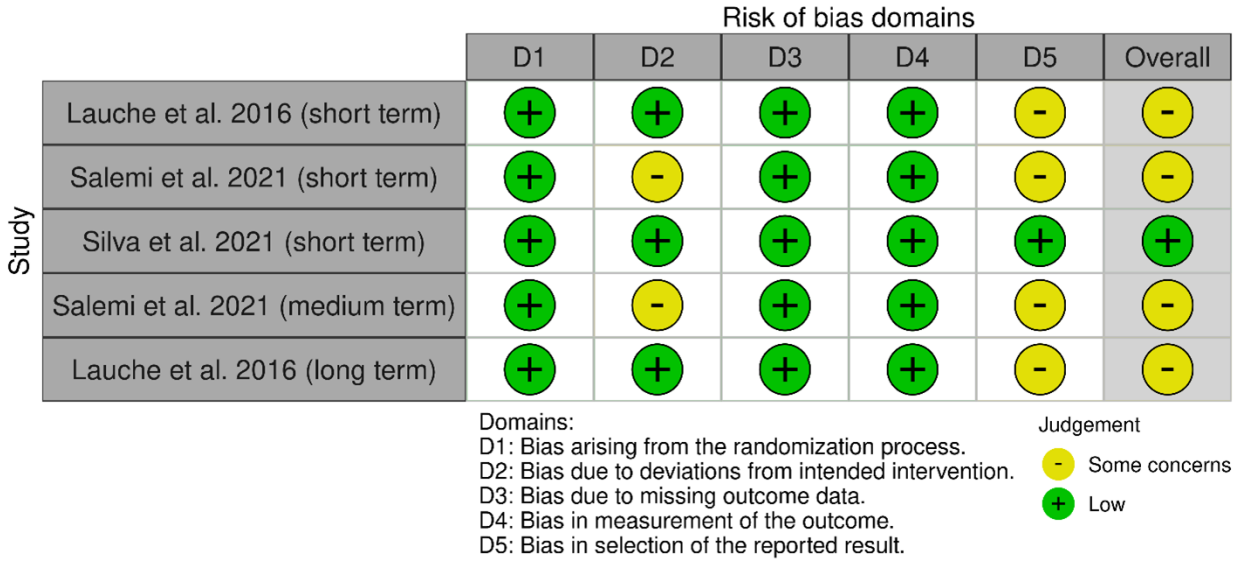
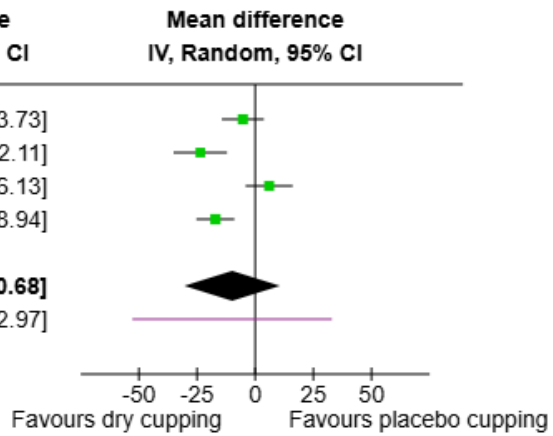


FIGURE 3. Risk of bias summary for function.

Study or Subgroup	Dry Cupping Mean	SD	Total	Placebo cupping Mean	SD	Total	Weight	Mean difference IV, Random, 95% CI
Lauche et al. (2016)	51.1	20.9	47	56.4	23.91	48	25.5%	-5.30 [-14.33 , 3.73]
Salemi et al. (2021)	23.9	17.84	19	47.5	17.81	18	23.6%	-23.60 [-35.09 , -12.11]
Silva et al. (2021)	33	29	45	27	19	45	24.7%	6.00 [-4.13 , 16.13]
Stephens et al. (2020)	-20	12.62	11	-2.9	5.36	10	26.2%	-17.10 [-25.26 , -8.94]
Total (HKSJ[a])			**122**			**121**	**100.0%**	**-9.92 [-30.52 , 10.68]**
95% prediction interval								[-52.80 , 32.97]



Test for overall effect: T = 1.53, df = 3 (P = 0.22)

Heterogeneity: Tau² (REML[b]) = 139.69; Chi² = 18.91, df = 3 (P = 0.0003); I² = 85%

Footnotes

[a]CI calculated by Hartung-Knapp-Sidik-Jonkman method.

[b]Tau² calculated by Restricted Maximum-Likelihood method.

FIGURE 4. Forest plot of dry cupping versus placebo cupping for pain in the short-term.

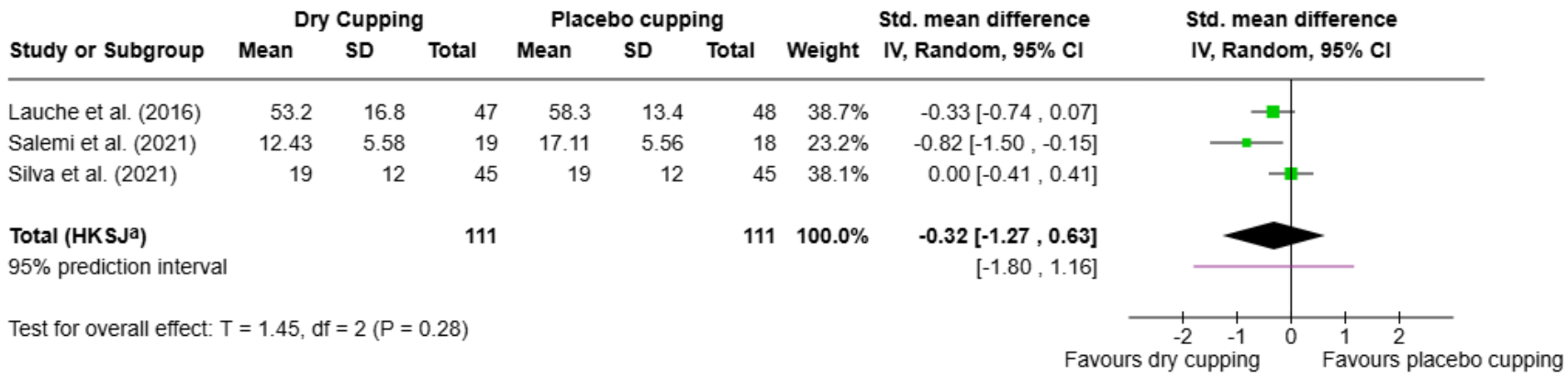


FIGURE 5. Forest plot of dry cupping versus placebo cupping for function in the short-term.

Table 1: Trial characteristics and intervention details

Author / Year Country	**Participant characteristics**	**Intervention**	**Difference between dry cupping and placebo cupping**	**Outcomes & timing (post-randomization)**	**Conflict of interest (COI) Funding**	**Notes**
Lauche et al. 2016 [27] Germany	Fibromyalgia syndrome (chronicity NR): n=95* (Dry cupping = 47, Placebo cupping = 48) Mean age (SD): 55 (10) y/o Sex (% F): 98% Pain on inclusion: > 45mm in the VAS	5 sessions of 30 min, 2×/week for 2.5 weeks Cups: 4-8 acrylic cups (diameter = 5-10 cm) Pressure: NR Cup location: Upper and lower back in semi-standardized manner	Cups with small holes <1mm in diameter Double sided adhesive tape applied to keep cups in contact with skin (applied in all groups)	Pain (VAS, 0-100) and function (Fibromyalgia Impact Questionnaire, 0-100) Short-term (2.5 weeks) and long-term (6 months)	COI: Reported and none declared Funding: Reported and none declared	*Pneumed GmbH* provided cupping equipment and prepared the placebo cupping devices for the trial)
Salemi et al. 2021 [34] Brazil	Non-specific low back pain (> 3 months duration): n=37 (Dry cupping = 19, Placebo cupping = 18) Mean age (SD): 25 (6) y/o Sex (% F): 59% Pain on inclusion: NR	5 sessions of 20 min, 2×/week for 2.5 weeks Cups: 17 acrylic cups (diameter = 3.5 cm) Pressure: Approx. 300millibars Cup location: bilaterally on HT3 and ST36 (10 min) then bilaterally on GV4, BL23, BL24, BL25, BL30, B40 and BL58 (10 min).	A 1.9 mm diameter hook released the pressure Double sided adhesive tape applied to the cup edge in the placebo cupping group (unclear if applied in both groups)	Pain (VAS, 0-10) and function (Oswestry Disability Index, 0-100) Short-term (2.5weeks) and medium-term (6.5 weeks).	COI: Reported and none declared Funding: Not reported	None
Silva et al. 2021 [4] Brazil	Non-specific low back pain (> 3 months duration): n=90 (Dry cupping = 45, Placebo cupping = 45) Mean age (SD): 31 (11) y/o Sex (% F): 74% Pain on inclusion: 3 to 8 on the NRS	8 sessions of 10 min, 1×/week for 8 weeks Cups: 4 acrylic cups (internal diameter = 4.5cm) Pressure: Negative pressure of 300millibars Cup location: Used parallel to L1 to L5 vertebrae, with 3cm distance between them bilaterally	Cups with small holes <2mm in diameter Double sided adhesive tape applied to the cup edge in the placebo cupping group (unclear if applied in both groups)	Pain (NRS, 0-10) and function (Oswestry Disability Index, 0-100) Short-term (8 weeks)	COI: Reported and none declared Funding: Reported and declared	Funding provided by CAPES Master's degree scholarship and Sao Paulo Research Foundation
Stephens et al. 2020 [38] USA	Unilateral or bilateral non-specific neck-pain (<2 weeks): n=21* (Dry cupping = 11, Placebo cupping = 10) Mean age (SD): 23 (3) y/o[+] Sex (% F): 53%[+] Pain on inclusion: ≥ 3 in the VAS	1 session of 8 min Cups: 1 Biomagnetic Chinese cupping therapy cup (internal diameter = 3.56 to 4.57 cm depending on size of treatment location Pressure: 3 suction pumps Cup location: On the location of pain (varied between participants)	A 0.4-mm hole was created with a heated needle. Tape ensured that the cup remained in contact with the skin in the placebo cupping group (unclear if applied in both groups)	Pain (VAS, 0-100) Short-term (1 session)	COI: Not reported Funding: Not reported	None
Volpato et al. 2020 [40]	Non-specific low back pain (> 3	1 session of 15 min	A small hole was present in the	Pain (Brief Pain	COI: Reported	Outcomes were

Author / Year Country	Participant characteristics	Intervention	Difference between dry cupping and placebo cupping	Outcomes & timing (post-randomization)	Conflict of interest (COI) Funding	Notes
Brazil	months duration): n=38 (Dry cupping = 18, Placebo cupping = 20) Mean age (SD): NR[#] Sex (% F): NR[#] Pain on inclusion: ≥ 4 in the VAS.	Cups: 6-8 acrylic cups (diameter = 5 cm) Pressure: Approx. 300 millibars Cup location: Bilaterally on BL23, BL24 and BL25	cupping glass	Inventory) and function (Roland Morris disability questionnaire) Short-term (1 session)	and none declared Funding: Reported and none declared	only reported for the dry cupping group

* Only participants in the dry cupping and placebo cupping groups were included. The other group was excluded from this review.

[+] Average only provided for the 3-arms.

[#] Reported for the dry cupping group only

Table 2. Summary of findings table for the outcomes pain and function in the short-, medium- and long-term.

People: People with musculoskeletal disorders **Intervention:** Dry cupping **Comparison:** Placebo cupping			
Outcomes (timepoint)	**Mean difference (95% CI's)** Interpretation based on SCWE	**Number of trials (participants)**	**Certainty of evidence (GRADE)** [†]
Pain (short-term) 100-point VAS (lower score reflects better outcome), SCWE = 15 points	**-9.9 (-30.5 to 10.7)** Dry cupping was trivially harmful to meaningfully beneficial when compared to placebo cupping.	4 (n=243)	⊕⊖⊖⊖ Very low [a,b]
Pain (medium-term) 100-point VAS (lower score reflects better outcome), SCWE = 15 points	**17.2 (-33.0 to -1.4)** Dry cupping was effective, and the effect maybe meaningful compared to placebo cupping	1 (n=37)	⊕⊖⊖⊖ Very low [c,d]
Pain (long-term) 100-point VAS (lower score reflects better outcome), SCWE = 15 points	**-2.2 (-11.8 to 7.4)** Dry cupping was trivially effective to trivially harmful when compared to placebo cupping.	1 (n=52)	⊕⊖⊖⊖ Very low [c,e]
Function (short-term) Standardised Mean Difference (negative scores favour the interention, positive scores faour the control), SCWE = 0.35	**-0.32 (-1.27 to 0.63)** Dry cupping was meaningfully harmful to meaningfully beneficial when compared to placebo cupping	3 (n=222)	⊕⊖⊖⊖ Very low [b]
Function (medium-term) 100 point Owestry Disability index ((lower score reflects better outcome), SCWE = 10 points	**-0.42 (-9.9 to 1.6)** Dry cupping was trivially effective to trivially harmful when compared to placebo cupping.	1 (n=37)	⊕⊖⊖⊖ Very low [c,f]
Function (long-term) 100 point Fibromyalgia Impact Questionnaire (lower score reflects better outcome), SCWE = 14 points	**-4.3 (-12.2 to 3.6)** Dry cupping was trivially effective to trivially harmful when compared to placebo cupping.	1 (n=52)	⊕⊖⊖⊖ Very low [c,f]
[a] downgraded 1 level for risk of bias, [b] downgraded 3 levels for inconsistency + imprecision, [c] single trial (start at low certainty, [d] downgraded 2 levels for inconsistency + imprecision, [e] downgraded 2 levels for risk of bias, [f] downgraded 1 level for inconsistency + imprecision [†] GRADE Working Group grades of evidence **High** = This research provides a very good indication of the likely effect. The likelihood that the effect will be substantially different[‡] is low. **Moderate** = This research provides a good indication of the likely effect. The likelihood that the effect will be substantially different[‡] is moderate. **Low** = This research provides some indication of the likely effect. However, the likelihood that it will be substantially different[‡] is high. **Very low** = This research does not provide a reliable indication of the likely effect. The likelihood that the effect will be substantially different[‡] is very high. [‡] Substantially different = a large enough difference that it might affect a decision			

SCWE = Smallest clinically worthwhile effect.

Table 3: Completeness of reporting according to the TIDieR placebo

Study	Group	1	2	3	4	5	6	7	8	9	10	11	12	13	Total
Lauche et al. 2016 [27]	Dry cupping	Y	N	Y	Y	N	Y	Y	Y	N	N	Y	Y	Y	9

Study	Group	1	2	3	4	5	6	7	8	9	10	11	12	13	Total
	Placebo cupping	Y	N	Y	Y	N	Y	Y	Y	N	N	Y	Y	Y	9
Salemi et al. 2021 [34]	Dry cupping	Y	Y	Y	Y	Y	N	N	Y	N	N	N	Y	N	7
	Placebo cupping	Y	Y	Y	Y	Y	N	N	Y	N	N	N	Y	N	7
Silva et al. 2021 [4]	Dry cupping	Y	Y	Y	Y	Y	Y	Y	Y	N	N	Y	Y	Y	11
	Placebo cupping	Y	Y	Y	Y	Y	Y	Y	Y	N	N	Y	Y	Y	11
Stephens et al. 2020 [38]	Dry cupping	Y	Y	Y	Y	N	Y	N	N	Y	N	N	N	N	6
	Placebo cupping	Y	Y	Y	Y	N	Y	N	N	Y	N	N	N	N	6
Volpato et al. 2020 [40]	Dry cupping	Y	N	Y	Y	Y	Y	N	Y	N	N	N	Y	N	7
	Placebo cupping	Y	N	Y	Y	Y	Y	N	Y	N	N	N	N	N	6

Y: Yes (reported), N: No (not fully reported)

TIDieR-Placebo items[17]

1) Brief name describing the intervention; 2) Rationale, theory or goal of the intervention; 3) Describing physical or information materials used in in the intervention; 4) Describing the procedure, activities and processes in the intervention; 5) Profession of the provider including expertise, background or specific training; 6) Describing modes of delivery and whether this was individual or group based; 7) Describing the types of locations, settings and necessary infrastructure; 8) Describing the number of times the intervention was delivered including sessions, schedule, duration, intensity and/or dose; 9) How the intervention was personalized, titrated or adapted; 10) Modifications to the intervention during the course of the study; 11) whether studies planned to assess adherence or placebo fidelity; 12) Whether studies reported adherence or placebo fidelity; 13) Whether the success of blinding was measured.