EFFICACY OF MYOFASCIAL DECOMPRESSION FOR MUSCULOSKELETAL CONDITIONS

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Abstract

Musculoskeletal pain is highly prevalent and a significant contributor to global disability and disease, with most countries reporting neck and low back pain as a leading cause of disability. The use of the complementary therapy, dry cupping and its modified technique, myofascial decompression are becoming an increasingly popular modality utilised by manual and physical therapists in western clinical practice, aimed at reducing musculoskeletal pain and improving mobility. The aim of this thesis was to investigate the efficacy of dry cupping techniques in the treatment of musculoskeletal pain and improving range of motion.

Firstly, this research critically evaluated the evidence from randomised controlled trials through a systematic review and meta-analysis to determine the efficacy and safety of western dry cupping methods. A systematic literature search was performed from March until April 2018, for randomised controlled trials (RCT) pertaining to musculoskeletal pain or reduced range of motion, treated with dry cupping. Outcomes were pain, functional status, range of motion and adverse events. Risk of bias and quality of evidence was assessed using the modified Downs & Black checklist and GRADE. A total of 21 RCTs with 1049 participants were included. Overall, the quality of evidence was fair, with a mean Downs & Black score of 18/28. Low-quality evidence revealed that dry cupping had a significant effect on pain reduction for chronic neck pain (MD, -21.67; 95% CI, -36.55, to -6.80) and low back pain (MD, -19.38; 95%CI, -28.09, to -10.66). Moderate-quality evidence suggested that dry cupping improved functional status for chronic neck pain (MD, -4.65; 95%CI, -6.44, to -2.85). For range of motion, low quality evidence revealed a significant difference when compared to no treatment (SMD, -0.75; 95%CI, -0.75, to 0.32). Dry cupping was found to be effective for reducing pain in

patients with chronic neck pain and non-specific low back pain. However, definitive conclusions regarding the effectiveness and safety of dry cupping for musculoskeletal pain and range of motion were unable to be reached due to the low to moderate quality of evidence.

Secondly, this research aimed to test the feasibility of a randomised controlled crossover trial comparing dry cupping techniques to provide recommendations for future research. The study compared myofascial decompression (dry cupping with active movement) with static dry cupping (dry cupping with no movement) and an active movement protocol (control) and assessed pressure pain threshold and range of motion. This study's findings suggest that dry cupping techniques improve both pain threshold and range of motion; however, it is unknown whether myofascial decompression is superior to active movement only and could be investigated in future studies.

This thesis provides a critical review of the available evidence for the use of dry cupping therapy for musculoskeletal conditions. For definitive conclusions on the efficacy of dry cupping, further systematic reviews and meta-analyses are required as larger dry cupping randomised controlled trials are published. Furthermore, this thesis provides a basis for future research to be undertaken, specifically investigating the efficacy of myofascial decompression for the treatment of musculoskeletal pain and improving range of motion.

Declaration of Authenticity

"I, Sarah May Wood, declare that the Master of Applied Research thesis entitled **"Efficacy of Myofascial Decompression for Musculoskeletal Conditions"** is no more than 50,000 words in length including quotes and exclusive of tables, figures, appendices, bibliography, references and footnotes. This thesis contains no material that has been submitted previously, in whole or in part, for the award of any other academic degree or diploma. Except where otherwise indicated, this thesis is my own work". "I have conducted my research in alignment with the Australian Code for the Responsible Conduct of Research and Victoria University's Higher Degree by Research Policy and Procedures.

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Abbreviations

ACh	Acetylcholine
AChE	Acetylcholinesterase
AChR	Acetylcholine Receptor
AKE	Active Knee Extension
ANOVA	Analysis of Variance
ATP	Adenosine Triphosphate
BMI	Body Mass Index
CEBM	Centre for Evidence Based Medicine
CGRP	Calcitonin Gene Related Peptide
cLBP	Chronic Low Back Pain
CI	Confidence Interval
CNS	Central Nervous System
ECM	Extra Cellular Matrix
FAAM	Foot and Ankle Ability Measure
FEW-16	(German) Assessment of Physical Wellbeing
FIQ	Fibromyalgia Impact Questionnaire
GI	Gastrointestinal
GROC	Global Rating of Change Scale
GT	Graston Technique
GTO	Golgi Tendon Organs
HA	Hyaluronic Acid
HADS	Hospital Anxiety and Depression Scale
HIF-1α	Hypoxia Inducible Factor-1 Alpha
IASTM	Instrument-assisted Soft Tissue Mobilisation
IC	Ischemic Compression
ICC	Intraclass Correlation Coefficients
iNOS	Inducible Nitric Oxide Synthase
LBP	Low Back Pain
LEFS	Lower Extremity Functional Scale
MCID	Minimally Clinically Importance DiffeFrence
MD	Mean Difference

MDT	Mechanical Detection Threshold
MET	Muscle Energy Technique
MFD	Myofascial Decompression
MFR	Myofascial Release
MFI-20	The Multidimensional Fatigue Inventory
MTrP	Myofascial Trigger Point
NDI	Neck Disability Index
NMT	Neuromuscular Technique
NO	Nitric Oxide
NSAID's	Nonsteroidal Anti-inflammatory Drugs
OA	Osteoarthritis
O_2	Oxygen
PFAQ	Perceived Functional Ability Scale
PSFS	Patient Specific Functional Scale
PSQ-20	Perceived Stress Questionnaire
PPT	Pain Pressure Threshold
PRT	Positional Release Therapy
PSQI	Pittsburgh Sleep Quality Inventory
PVS	Primo Vascular System
RCT	Randomised Controlled Trial
RMDQ	Rowland Morris Disability Questionnaire
RNA	Ribonucleic Acid
ROM	Range of Motion
SCS	Strain Counter-Strain
SD	Standard Deviation
SE	Standard Error
SEA	Spontaneous Electrical Activity
SEM	Standard Error of the Mean
SF-36	36-Item Short Form Survey
SMD	Standard Mean Difference
SP	Substance P
SPSS	Statistical Package for the Social Sciences

SST	Skin Surface Temperature
TCM	Traditional Chinese Medicine
VAS	Visual Analogue Scale
VDT	Vibration Detection Threshold
VEGF	Vascular Endothelial Growth Factor
WOMAC	Western Ontario McMasters University Osteoarthritis Index

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CHAPTER 1

Introduction to this thesis 'Efficacy of Myofascial Decompression for Musculoskeletal Conditions'

1. Introduction to the Research Problem

Musculoskeletal pain is a highly prevalent and significant contributor to global disability and disease, with most countries reporting neck and low back pain as a leading cause of disability.[1] Improving function and controlling pain are key aims for the treatment of musculoskeletal pain, which typically consists of a combination of physical therapy, self-management and short-term analgesic medication.[2]

Complementary therapies are highly utilised, in conjunction with usual medical care for the management of musculoskeletal pain.[3] Researchers have observed higher use of complementary therapies in patients with neck and back pain.[4] Dry cupping, a complementary therapy is becoming an increasingly popular modality utilised by manual and physical therapists in western clinical practice, aimed at reducing musculoskeletal pain and improving mobility.[5] Due to the traditional applications of cupping and the treatment of a wide range of health conditions, there is an absence of evidence explicitly relating to dry cupping therapy for the treatment of musculoskeletal conditions. Recent systematic reviews[6,7] have investigated the use of all cupping methods for the treatment of musculoskeletal pain conditions; however, not dry cupping specifically. This research aimed to critically evaluate the efficacy of dry cupping therapy for musculoskeletal pain conditions.

1.1 The Research Intent

The underpinning goal of this research was to investigate the efficacy of dry cupping techniques in the treatment of musculoskeletal pain and improving range of motion (ROM). Firstly, this research critically evaluated the evidence from randomised controlled trials (RCTs) through a systematic review to determine the efficacy and safety of western dry cupping methods. Secondly, this research aimed to test the feasibility of a randomised controlled crossover trial comparing dry cupping techniques to provide recommendations for future research.

1.2 The Research Questions

The overarching research questions that underpin this project are:

(1) Is dry cupping an effective and safe modality in reducing musculoskeletal pain and increasing range of motion?

(2) Does myofascial decompression increase range of motion and improve pain pressure sensitivity?

1.3 Scope and Boundaries of the Research

This research aims to evaluate dry cupping methods that Western manual therapists commonly use in clinical practice. This research acknowledges the traditional origins of the technique; however, in the context of this project, only a focus on the western adaptations will be made. Providing evidence from a biomechanical model is the fundamental aim of this project.

1.4 The Structure of the Thesis

Having established the rationale for this research project, the following chapter provides a detailed review of relevant literature. Chapter 2 is divided into eight sections: [1] epidemiology of musculoskeletal conditions, [2] myofascial pain, [3] anatomy review, focusing on myofascial tissue, [4] myofascial release techniques commonly used to treat myofascial pain, [5] myofascial cupping therapy, including the history and modification of the method, and the use of cupping for pain conditions and mobility, [6] dry cupping methods for musculoskeletal pain conditions, [7] myofascial decompression and range of motion, [8] physiological and mechanical effects of dry cupping and current theories.

Chapter 3 examines the efficacy and safety of dry cupping methods for musculoskeletal conditions through a systematic review and meta-analysis. This chapter will discuss the findings of the comprehensive investigation.

Chapter 4 outlines the research methods employed for a reliability study of a pressure algometer device that is used as an outcome measure in the main clinical trial. The content is divided into sections that explain the conceptual framework, methodology, and results.

Chapter 5 provides an overview of the randomised crossover clinical trial undertaken to test the feasibility of dry cupping interventions on range of motion and pressure point thresholds. This chapter will discuss the methodology, analysis, and results of the investigation.

Chapter 6 discusses the purpose, frame and scope of the research undertaken and the final chapter concludes with a summary and critique of the research, implications of the research for manual therapists and recommendations for future research direction that could contribute to the current body of work.

CHAPTER 2

Literature Review

2. Introduction

The purpose of this review is to examine the literature pertaining to the efficacy of dry cupping techniques in the treatment of musculoskeletal pain and improving range of motion. This chapter will provide an overview of the epidemiology of musculoskeletal conditions and review the literature on myofascial pain and myofascial release techniques, as this underpins the theoretical basis of myofascial dry cupping. Additionally, this chapter will provide an overview of the proposed physiological mechanisms and mechanical effects that cupping has on fascial connective tissue.

2.1 Epidemiology of Musculoskeletal Conditions

Musculoskeletal conditions are a highly prevalent and significant contributor to pain, disability, and disease, affecting 30% of the Australian population.[8] Musculoskeletal conditions are the second largest contributor to disability worldwide, and in Australia is the fourth leading contributor for total disease burden, after cardiovascular diseases, cancer, and mental and substance abuse.[9,10] In 2011, the Australian Institute of Health and Welfare reported that 12% of the total burden of injury and disease resulted from a musculoskeletal condition, with back problems contributing to almost one-third of the overall reported burden and an estimated expenditure of $1.2 billion.[9] In 2008-09, 3.7 million Australians that reported a back problem, with approximately 17% of back pain resulting from occupational risks.[10] Globally, low back pain is ranked as the number one

cause of disability.[10] In 2012-13, an average of 2.9% of patients with low back pain visited primary care physicians for the management of their condition.

2.2 Myofascial Pain

Myofascial pain syndrome (MPS) is a common musculoskeletal condition characterised by non-articular localised musculoskeletal pain, and is often associated with various other pain conditions.[11] Research has found the prevalence of myofascial pain is higher in patients presenting with regional musculoskeletal pain complaints, particularly in the upper body.[12,13] Patients seeking primary care medical treatment for musculoskeletal pain conditions had a 30% incidence of a myofascial pain diagnosis.[13] Some studies observed an incidence of MPS ranging from 85% to 93% in patients presenting to specialist pain management clinics.[12] A recent cross-sectional study of 224 patients with non-specific neck pain seeking primary care, observed myofascial trigger points (MTrP) in all participants, which were thought to be contributing to their pain state. Over 93% of participants reported MTrPs in the trapezius muscle, 82% in the levator scapulae and 77% in multifidi muscles.[14]

Myofascial pain and the presence of MTrPs were first clinically defined by Travell and Simons[15] as:

> "A hyperirritable spot in skeletal muscle that is associated with a hypersensitive palpable nodule in a taut band. The spot is painful on compression and can give rise to characteristic referred pain, referred tenderness, motor dysfunction, and autonomic phenomena".

Pain referral patterns are observed for both muscle and the surrounding connective tissue (fascia), and it has been suggested that complex changes in the central and

peripheral nervous system contribute to sensory and motor abnormalities.[11] Myofascial trigger points can be classified as either active or latent. An active MTrP elicits a spontaneous pain response, whereas a latent MTrP will elicit a similar pain response upon palpation of the nodule.[16] Aetiology of MTrPs is unclear and the subject of debate and conjecture.[17] The most widely accepted theories are the energy crisis theory, motor endplate hypothesis, and the integrated trigger point hypothesis (capillary constriction and increased metabolic demand).[16] Current studies are now focusing on objective markers (biochemical, histologic, neurophysiologic, and sonographic) to explain the aetiology of MTrP.[11,18] Shah et al[11] found biochemical differences between active and latent MTrP with microdialysis muscle sampling, indicating biochemical changes in areas associated with pain.

An expanded theory that researchers have proposed is that the muscle contracture or 'taut band' associated with the MTrP may be a result of the excessive release of acetylcholine (ACh) in the synaptic cleft fluid at the postsynaptic membrane.[18] Electromyographic studies of MTrP loci have revealed spontaneous electrical activity (SEA), thought to be related to the fluctuations in ACh receptor (AChR) and acetylcholinesterase (AChE) activity.[16,19] The enzyme, AChE can terminate ACh action; however, acidic environments inhibits AChE activity, and increase delivery of Calcitonin Gene Related Peptide (CGRP), which further downregulates AChE activity.[20,21] It is proposed that these factors contribute to the activation of muscle contractures and the onset and perpetuation of myofascial pain.[18]

Researchers, Sikdar et al[22] and Ballyns et al[16] have used sonographic techniques to observe blood vessels and flow velocities of a healthy soft tissue environment, compared to MTrP sites. Sikdar et al[22] noted a unique blood flow waveform that was

observed in active MTrP when compared to latent MTrP and normal tissue. The authors suggest two contributing factors that may lead to variations in MTrPs waveforms. Firstly, an increase in outflow resistance from compression of capillaries, due to contracture of the muscle or inflammation, resulting in local vasoconstriction. Secondly, a volume increase of the vascular compartment. Expansion of blood vessels and angiogenesis is associated with tissue hypoxia, with the increase in the levels of vascular endothelial growth factor (VEGF), chemotactic agents, and chemokines.[16]

Reduced tissue oxygen levels result in a disruption of energy metabolism and mitochondrial dysfunction. Researchers have observed augmented mitochondrial disorganisation in individuals with trapezius myalgia, accompanied with low levels of adenosine triphosphate (ATP) and adenosine diphosphate, and a deficit of cytochrome C oxidase - indicative of muscle energy crisis.[18,23] Prolonged postural positions, such as sitting at a desk or operating machinery may be a contributing factor for MTrP development, due to sustained low-level muscle contractions.[17,24] Decreased intramuscular perfusion may be a result of the increase in calcium ions and acidity from insufficient ATP synthesis, thus further contributing to hypoxia, ischemia and sustained sarcomere contracture. The vicious cycle eventually releases sensitising substances that further contribute to muscle tenderness and pain.[19]

Lowered nociceptor threshold sensitivity, mechanical hyperalgesia and tissue inflammation are associated with low pH levels. Muscle pain following injury or trauma releases a biochemical milieu of substances, such as inflammatory mediators, neuropeptides, cytokines, and catecholamines, resulting in an increased acidity within the tissue. Acidic pH environments have been found to stimulate the production of the inflammatory mediator, bradykinin and further contribute to sensitisation and the

persistent pain state.[11] Prolonged tissue acidity contributes to muscle nociceptor activation, resulting in release of substance P (SP) and CGRP into the muscle. Shah et al[11] observed higher levels of SP and CGRP within active MTrP sites. The dorsal root ganglion produces SP and CGRP and antidromically transports the substances to the nociceptor terminals. The release of SP and CGRP into the muscle effects the biochemical milieu, stimulating neurogenic inflammation and the production of neuropeptides and inflammatory mediators - further perpetuating the abnormal ACh release. Neuroplastic changes in the dorsal horn neurons from the continual bombardment of up-regulated nociceptive input and its associated neurochemistry, may result in central nervous system (CNS) changes in pain perception and neuronal responsiveness.[11]

Peripheral and central sensitisation are sensory aspects attributed to myofascial pain conditions. Allodynia and hyperalgesia are signs of peripheral and central sensitisation. Reduced threshold, spontaneous activity and increased responsiveness of peripheral nociceptor ends can result in hypersensitivity to non-noxious stimuli.[24] An active MTrP induces peripheral nociception with sensitisation and enlargement of the receptive field.[11] Receptive field expansion results in the dorsal horn neurons inputting new information from surrounding areas.[24] Neuroplastic changes, due to the incessant nociceptive input from the MTrP may result in central sensitisation and spinal segmental sensitisation – hyperalgesia across numerous spinal segments.[26] Spontaneous pain at the spinal level activates dermatomes, myotomes and sclerotomes, and sensitisation increases to other dorsal horn levels and the contralateral side.[11]

Current treatment methods for MTrP include dry needling and injection, massage, stretching, ischemic compression (sustained pressure over the MTrP site), electrical stimulation,[27] relaxation techniques,[25] and self-myofascial release - with the use of a foam

roller or massage ball.[28] Dry cupping therapy has been used as a treatment for myofascial pain conditions and MTrP therapy,[29] based on the assumption it increases microcirculation and alters nociceptive signalling pathways.[30]

2.3 Myofascial Tissue

This next section will review the properties of myofascial tissue and the proposed aetiology, as western dry cupping therapy (*specifically, myofascial decompression*) is used as a myofascial release technique for improving flexibility and reducing myofascial pain.[5,29]

Fascia is defined as fibrous bands of connective tissue and plays an integral role in movement organisation and posture.[31] Facia provides a true continuity throughout the entirety of the human body, that gives form and function to every structure it surrounds.[31,32] Connective tissues can be found equally distributed throughout the body, enveloping and permeating blood vessels, viscera, meninges, nerves, bones and muscles - acting as a continuum that interacts and communicates between each structure. Willard et al[33] define fascia as irregular weaves of collagen fibres that can withstand stress in multiple directions; a tridimensional mechanical and metabolic matrix that consists of various layers.[33,34]

Fascia that surrounds muscle, is categorised as superficial and deep fascia. The membrane layers or superficial fascia are connected to the dermis and deep fascia by retinacula, constructing a three-dimensional network between adipose lobules.[34] The construct of superficial fascia is described by Stecco et al[35] as loosely packed interwoven collagen fibres, abundant with elastic fibres. The superficial fascia plays an important role in movement between the integument and underlying structures, and the conveyance of

blood vessels and nerves to and from the skin.[36] Numerous sheets of collagen fibres accompanied with elastin promote movement, with the dermis contributing to the mechanical properties of the skin.[36,37] Under stress or stretch of the skin, the collagen fibres align to the same direction of force and the elastic recoil returns the skin back to the original shape and position.[36,37] The superficial fascia is host to an abundant microvascular system.[38] A sophisticated vascular network operates independently within the superficial fascia, known as the primo vascular system (PVS), *previously described as the Bonghan channels*.[31,39] The PVS is semitransparent and includes subsystems such as the Bonghan ducts and Bonghan corpuscles.[40] The network has been found to exist on the surface of blood vessels, viscera, in adipose tissue, and in lymph vessels.[39] Researchers have observed PVS within the umbilical cord, specifically, the epithelial fascia and inside blood vessels.[40] Loose connective tissue consists of water, ions and is rich in Hyaluronic Acid (HA), proteoglycans and glycosaminoglycans.[41] The high proteoglycan content provides the tissue with viscoelastic properties that allows for sliding capabilities.[38]

The deep fascia is formed by two to three parallel layers of collagen fibre bundles that occur in a wave-like arrangement, with a combined mean thickness of 1mm.[35,41] The deep fascia can be found separating muscles with some consisting of fascial insertions, nerve and vessel sheaths that add reinforcement to ligaments and tendons.[35] Due to the high composition of HA, loose connective tissue can be found in between the deep fascia and epimysium, allowing the muscle to slide freely.

When a muscle contracts or external force is applied, the force is transmitted between the fascia to both synergistic and antagonist muscle groups via myofascial force transmission.[42,43] Due to the connection of fascia to the bones and soft tissue, innervation

is highly linked.[36] Oscillatory movements, such as walking, have been found to use fascial tissue as dynamic energy storage.[42,44]

The reorganisation of fascia throughout the body can be observed under a state of tension, as this can lead to an increase or decrease of fascial length at various joints.[42] Fascial contractility has been shown to occur due to the myofibroblasts and actin found in the connective tissue. Specifically, alpha-smooth muscle actin, similar to that of the digestive system.[31] Myofibroblasts are particularly important in wound contraction and scar formation. There is an increased density of myofibroblasts observed within the lumbar fascia, suggesting the ability to contract actively while playing a role in musculoskeletal dynamics.[42,45] Furthermore, studies of human fascia under electron photo microscope have exhibited smooth muscle cells embedded within the collagen fibres that enable the autonomic nervous system to regulate a fascial pre-tension autonomous from the muscular system.[42]

2.3.1 Fascial Alterations

It has been proposed that fascial alterations may contribute to myofascial pain syndromes.[41] Fascial densification and fibrosis are two separate alterations that affect the deep fascia. Fibrosis is an alteration that results in the deposition of excessive fibrous connective tissue, similar to the process of scaring.[41] Densification is the increased density of fascia, whereby the general structure is unaltered but the mechanical properties are modified.[41]

Disruptions to loose connective tissue's sliding capability through damage or injury may result in fascial alterations.[41] Decreased gliding between the layers of deep fascia and collagen fibres may be a result of increased viscosity of loose connective tissue,

and perceived as fascial stiffness and myofascial pain.[41,42] Schleip et al[32] hypothesised that fascial stiffness may influence muscle spindle sensitivity, thus contributing to myofascial pain syndromes.[45] The viscosity of loose connective tissue can be affected by temperature and changes in the pH environment.[41] Temperatures exceeding 40°C will cause HA chains to gradually break down and decrease in viscosity. Whereas accumulation of lactic acid and various waste products contributes to an increased viscosity due to changes in pH.[41]

The thixotropic nature of HA reduces viscosity with any loading conditions;[41] during periods of rest or immobility, the viscous state returns, resulting in a reduced range of motion due to decreased fascial gliding.[41,42] Prolonged pain states followed by fear avoidance behaviour and reduced movement may result in the remodelling of connective tissue, followed by inflammation, sensitisation of the nervous system and a further decrease in mobility.[46]

Interruption to remodelling and realignment of collagen during the healing process may result in alterations of the fibrous component, resulting in random deposition of collagen fibres. Other factors that alter collagen fibrous bundles include trauma, surgery, diabetes, hormones, and ageing.[41] Importantly, as discussed by Pavan et al[41] fascial densification is easy to reverse, as mechanical properties of the extracellular matrix (ECM) can be manipulated with temperature and an increase in local strain via controlled mechanical stimulus.[41] The proposed mechanisms that result in the reversal of fascial densification are outlined in the following sections.

2.3.2 Local Mechanoreceptors

Mechanoreceptors are stimulated during any mechanical manipulation of soft tissue structures, such as during dry cupping therapy. Golgi tendon organs (GTO) are sensory mechanoreceptors that send information to the CNS on muscle tension, sensed from muscle fascicles (located between muscle tissue and tendon aponeuroses) and connective tissue.[47] Slow stretch and active muscle contractions activate these receptors, albeit, Jami[48] demonstrated that passive stretch does not activate the GTO. Activation of GTO results in reflex inhibition of the muscle or a tonus decrease of muscle fibres through the inverse myotatic reflex mechanism.[32,49] Approximately 90% of GTO are found in myotendinous junctions, aponeurosis attachments, ligaments of peripheral joints and joint capsules close to the bone, and only 10% lie within the tendon.[32,50] Pratt[51] suggested that myofascial cupping techniques may activate GTO if the technique incorporates active movement.

Pacini corpuscles and the slightly smaller paciniform corpuscles are more frequently observed within the epimysium and are primarily involved in kinesthesia and proprioceptive feedback for movement control. The Pacini bodies respond to vibration and rapid pressure changes, and the paciniform corpuscles have a similar function and sensitivity.[32] Ruffini endings are activated by slow mechanical pressure and are extremely responsive to tangential forces such as lateral shear. A decrease in sympathetic activity is a result of the stimulation of ruffini corpuscles.[32] Myofascial cupping techniques are likely to activate pacini corpuscles and ruffini endings due to the sustained pressure and shear tension from the cup.[51]

2.3.3 Interstitial Muscle Receptors

Approximately 80% of sensory nerves belong to type III and IV nerve fibres.[32] A majority (90%) of these nerves are unmyelinated and belong to type IV, the remaining 10% are encased by a thin myelin sheath and belong to type III.[32] Most of these receptors are free nerve endings found abundantly within fascia, allowing their involvement in transmitting innocuous or nociceptive mechanical input to the CNS. These sensory neuron receptors are slower than types I and II, which originate in muscle spindles, pacini corpuscles, ruffini endings and golgi organs, making type III and IV pathways well placed to be utilised in manual therapy techniques involving mechanical input.[32]

The type III and IV interstitial mechanoreceptors consist of low and high-threshold pressure units. Low-threshold pressure units respond to light touch, and high-threshold pressure units are involved in pain sensitivity. Studies show that type III and IV receptors also respond to pressure and mechanical tension, ultimately functioning as mechanoreceptors. The chronic firing of these receptors has been observed within the presence of pain, altering sensitivity to normal physiological pressure changes.[32]

2.3.4 Fascia and Fluid Flow Dynamics

Fascia is comprised of approximately two-thirds water and when a mechanical load is applied, via local compression or stretching, fluid flow dynamics are altered.[52] Fascial layers require HA to slide over each other and the tissue can become compromised if the fluid flow decreases or is not regularly distributed.[31,42]

Dehydration or reduced fluid within the fascial layers alter the lines of force, as the HA becomes adhesive and it has been hypothesised that the change in tissue viscosity stimulates nociceptors.[31,42,45] The change in viscosity prevents catabolites of cellular

metabolism from being completely removed, resulting in an altered pH environment, thus leading to dysfunctional physiology, increased levels of circulating cytokines and stimulation of nociceptors.[31] Improvements in fascial sliding motion has been shown to improve pain patterns in patients with low back pain.[42]

2.4 Myofascial Therapy

Myofascial therapies are a form of manual therapy, intended to decrease musculoskeletal pain and improve function of impaired soft tissue structures, including muscular and connective tissues, such as fascial structures.[54] Myofascial techniques include a range of interventions to treat myofascial pain conditions and increase joint range of motion, such as hands-on techniques and instrumental interventions. Hands on techniques include myofascial release (MFR), muscle energy techniques (MET), positional release therapy (PRT), ischemic compression (IC), neuromuscular technique (NMT) and strain counter-strain (SCS). Interventions using instruments include wet and dry needling techniques the Graston technique and myofascial cupping therapy.[55] This section will review the literature pertaining to myofascial techniques to examine the clinical effectiveness for the treatment of musculoskeletal pain conditions.

2.4.1 Myofascial Release Techniques

MFR are hands-on techniques that involves the application of low load, long duration stretch to the myofascial complex to reduce pain and improve mobility.[56] Slow, sustained pressure is applied to target tissue in the restricted motion or restricted fascial layers either directly or indirectly. Direct MFR technique involves the application of force, tension or stretch applied to the target tissue in the restricted motion. Indirect MFR

takes the path of least resistance, whereby the practitioner palpates a sense of ease or relaxation, usually by shortening the tissue.[56] Several systematic reviews[55,56] have examined both direct and indirect MFR techniques as a treatment option for musculoskeletal pain conditions.

In a systematic review on the effectiveness of MFR techniques on numerous musculoskeletal conditions, Ajimsha et al[56] analysed 19 RCTs, involving 1228 participants, and found the studies varied greatly, in both the results and the quality of evidence. A more recent review by Laimi et al[55] specifically looked at chronic pain conditions and included eight RCTs with 457 participants. Although PEDro scores of the included studies ranged from five to eight out of ten, indicating moderate to high-quality study design, a high risk of bias was observed in five out of eight trials. Only three low-quality studies reported clinically important improvements in pain or function. Due to small sample sizes or no long-term follow up in some of the studies, a CEBM rating of 2b was given to 14 studies, while five studies rated 1b - indicating relatively high-quality study design. As it stands, there are limited available high-quality, long-term studies that achieve clinically important improvements.

There have been several RCTs that have examined MFR techniques for musculoskeletal pain conditions, including Hsieh et al,[57] a high-quality RCTs included in the Ajimsha et al[56] systematic review. The study was well designed, with sufficient sample size (200 participants). Participants with sub-acute low back pain were randomly assigned to a treatment protocol, including either myofascial release, back school program, joint manipulation or combined myofascial release and joint manipulation. All groups improved in pain and activity scores; however, there were no significant differences between the individual treatment groups. Systematic review, Laimi et al[55]

included two high-quality studies[58,59] that used MFR in participants with fibromyalgia (chronic widespread pain). One of the trials utilised 90-minute massage-myofascial release therapy once per week and the other included twice weekly 60-minute MFR sessions. Both trials observed significant results between the treatment and placebo group and concluded that MFR is beneficial for reducing symptoms; however, neither reached clinical significant when analysed in the Laimi et al[55] review. The evidence to support MFR for musculoskeletal pain conditions remains mixed, as the quality of the trials differ greatly. Moreover, due to the variability of the technique and lack of underpinning mechanisms of action, optimal intervention timing and frequency has not yet been established.

2.4.3 Myofascial Release and Range of Motion

MFR has typically been used to treat myofascial pain conditions; however, it can also be used to improve flexibility and range of motion. Webb and Rajendran[55] conducted a systematic review and meta-analysis to determine the efficacy of MFR techniques on joint range of motion. The review examined nine RCTs, which included 534 symptomatic participants. PEDro scores ranged from four to seven out of ten, with four RCT's scoring a moderate to high quality rating. None of the trials received a score for 'blinding of all subjects' or 'blinding of therapist administering therapy'. Similarly, the Cochrane Risk of Bias also observed a 'high' risk of bias for blinding of participants. Sample sizes ranged from 20–117 and studies used a variety of techniques including MET, SCS, IC, MFR, NMT and PRT. The review found that all included trials reported positive outcomes, with statistically significant results. Meta-analysis of two RCTs found a moderate effect for

the use of MFR for increasing joint ROM; however, the results were highly heterogeneous and numerous threats to statistical validity of the trials were present.

2.5 Myofascial Cupping Therapy

Myofascial cupping therapy is a modern version of traditional dry cupping, used as an instrument-assisted myofascial release technique. The next section will discuss the history of the traditional cupping methods and the contemporary adaptions.

Historically, cupping has been used to treat a myriad of health conditions, with majority of the published literature reflecting traditional applications. Evidence of cupping can be found in the medical records of various cultures, dating as far back as 1536 BC[60] and since the 1950s, cupping has been formally used in hospitals practicing traditional medicine throughout the world.[61] Traditionally, cupping has been used to promote and regulate *qi*, blood stagnation, and reduce swelling.[62]

Cupping involves the use of glass, plastic or bamboo cups that are placed over localised areas of skin. A vacuum suction is achieved using either heat from a flame, a manual handheld pump, or electrical pumping device to create a negative pressure within the cup, drawing localised skin and soft tissue structures into the cup.[5,63] Depending on geographical regions and culture, there may be variations in the use of terms to describe the numerous cupping approaches.[60]

The most documented application of cupping methods described in Traditional Chinese Medical (TCM) literature is wet cupping: superficial skin incisions are made using a surgical instrument and the cup is then placed over the incisions to promote bloodletting.[60,64] In contrast, dry cupping does not involve incisions or penetrate the skin barrier. Dry cupping methods involve cups being placed on the skin following application

of a barrier cream or oil. The cups are can be left for a period of time (retained and static cupping) or passively moved around by the practitioner (gliding cupping, dynamic cupping and myofascial cupping) with the primary goal of reducing musculoskeletal pain.[60,64]

2.6 Dry Cupping and Musculoskeletal Pain Conditions

Five systematic reviews[7,61,65-67] have examined the efficacy of cupping therapy for pain conditions; however, all methods of cupping are included in the reviews (*wet, dry and moving cupping therapy*). The following section will provide an overview of dry cupping evidence pertinent to musculoskeletal pain conditions.

2.6.1 Dry Cupping for Low Back Pain

Low back pain is a significant global economic burden and cause of disability.[10] Researchers from several countries have investigated the use of dry cupping in the treatment of both acute and chronic low back pain. An Iranian RCT[68] investigated the effects of dry cupping on low back pain post labour. El Rahim et al,[69] an RCT from Egypt, examined the effects of combining dry cupping with physical therapy in the treatment of mechanical low back pain. A large RCT from Germany[70] used dry pulsatile cupping in their trial, with the aim of reducing pain and improving back function and quality of life in patients with nonspecific chronic low back pain. A small pilot study from the United States[71] examined the effectiveness of dry cupping therapy on subacute or chronic low back pain. Additionally, a placebo-controlled study from Brazil[72] conducted a single session of dry cupping study to examine the immediate effects on nonspecific chronic low back pain.

All trials used pain intensity and disability as their primary outcome measures, and several used pain pressure thresholds,[72] physical function and range of motion as secondary outcome measures.[69,71] RCTs investigating dry cupping for low back pain have larger sample sizes than any other musculoskeletal condition, and both Akbarzadeh et al[68] and Teut et al[70] recruited more than 100 participants in their trials.

Markowski et al[71] observed a strong relationship between higher levels of disability (Oswestry Disability Index) and improvements in lumbar flexion. Improvements in lumbar flexion strongly correlated with an increase in pain pressure thresholds at the lumbar paraspinal muscles. El Rahim et al[69] also observed an increase in lumbar flexion after dry cupping therapy; however, when dry cupping was combined with interferential therapy and physical therapy a larger effect was detected. Participants in the dry cupping and physical therapy group from the El Rahim et al[69] study experienced a pain reduction of 46.65mm on a 100mm scale and a 54% reduction in disability. When combined with interferential therapy, there was a pain reduction of 55.12mm on a 100mm scale and 79.63% reduction in disability. Participants from the Teut et al[70] study didn't observe as large pain reduction than the El Rahim et al[69] study, which may be contributed to lower baseline pain levels. Participants from the cupping group had an 18.3mm (100mm Visual Analogue Scale (VAS)) reduction and 19.9mm reduction in the minimal cupping group. A large pain reduction was observed in the Azbarzadeh et al[68] study, participants from the dry cupping group had a 64mm reduction in pain severity at the two-week follow-up. Participants from this study began treatment for postpartum low back pain eight hours post labour and had a baseline pain severity of 78mm on a 100mm scale. It is difficult to determine the true effect of the cupping intervention, as the high baseline pain levels may be due to the labour itself and potentially be self-limiting. Volpato et al[72]

observed significant differences between the dry cupping and placebo groups for the brief pain inventory and items such as 'pain now' and 'sleep'. Moreover, a recent protocol[73] was published for a placebo controlled double-blind RCT, investigating dry cupping therapy for chronic non-specific low back pain, and plans to recruit 90 participants. The study has had ethics approved and is currently in the recruitment phase.

2.6.2 Dry Cupping for Chronic Neck Pain

Non-specific and chronic neck pain yields the highest number of trials of all musculoskeletal conditions treated with dry cupping therapy. A total of seven RCTs investigated dry cupping therapy for non-specific neck pain compared with no intervention,[62,74] standard medical care,[75] wait-list,[29] usual care,[76] progressive muscle relaxation,[77] and ischemic compression.[78] The trial durations ranged from a single session,[62] two weeks,[29,75,76,79] four weeks,[78] five weeks,[74] and twelve weeks.[77] The majority of the RCTs investigated the effects of dry cupping on pain severity and functional status, measured by the visual analogue scale and neck disability index, respectively. Chi et al [62] observed the greatest pain reduction of all trials, with a decrease of 61mm (100mm VAS) for the dry cupping group. Lauche et al[29] observed a difference of 22.5mm (95%CI, -31.9, -13.1) between the dry cupping and control group. Cramer et al[75] and Saha et al[76] observed similar group differences of 11.22mm (95%CI, -16.24, -6.20) and 10.8mm (-21.5, -0.1), respectively. Saha et al[76] analysed patient expectations measured on a 100mm VAS, with 0mm indicating 'not successful at all' and 100mm indicating 'as successful as possible', the reported average benefit for cupping massage was 67.0mm (Standard deviation (SD) 23.8mm).

Pressure pain sensitivity was included as a secondary outcome measure for five trials,[29,75-79] measured by pressure pain threshold (PPT) using pressure algometry. Saha et al[76] only found significant differences between the groups at the site of maximal pain, with a mean difference (MD) of -0.019 (p=0.022); however, the reported 95%CI were -0.069, 0.032 indicating no significant effect was present. Cramer et al[75] observed significant group differences in PPT for the site of maximal pain, with a MD of 0.08 (95%CI, 0.03, 0.13; p=0.022). Similarly, Lauche et al[29] reported a MD of 0.08 (95%CI, 0.01,0.16; p=0.026). Lauche et al[77] observed a difference of 63.95 (95%CI, 6.33, 121.56; p=0.03) between the cupping massage group and progressive muscle relaxation, favouring the cupping massage group. Nasb et al[78] observed the greatest increase in PPT when dry cupping was combined with ischemic compression, compared to dry cupping or ischemic compression alone. From the results, it appears that dry cupping decreases pressure pain sensitivity by increasing the mechanical pressure pain threshold, as well as patient-reported pain severity.

Neck disability, functional status and quality of life have been of interest in the non-specific neck pain studies. For disability, measured by the neck disability index, five trials[75-79] reported a statistically significant difference between the dry cupping and control groups; however, the largest group difference was -6.3 (95%CI, -10.2, -2.4) and only the intervention groups from Lauche et al[79] and Cramer et al[75] studies exceeded the minimal clinically important difference (MCID) of >5.0 points.[80] Kim et al[7] conducted a meta-analysis on cupping therapy for neck pain and the effects on the level of disability. Compared with no treatment, the meta-analysis revealed a significant reduction in pain (MD -4.34; 95%CI, -6.77, -1.91; p=0.0005). Compared with the active control group, the meta-analysis also revealed a significant reduction in pain (MD -4.36; 95%CI, -8.67, -

0.04; p=0.05); despite the combination of wet and dry cupping, the results failed to observe a MCID.

2.6.3 Dry Cupping for Knee Osteoarthritis

Dry cupping therapy has been used in two RCTs[81,82] to relieve knee osteoarthritis symptoms. Both trials were reasonably small, with samples of 62 and 40, respectively. Teut et al[81] included a total of eight cupping sessions, conducted over four weeks, with a twelve-week follow-up. Outcome measures included the Western Ontario McMasters University Osteoarthritis Index (WOMAC) and pain intensity. Khan et al[81] used their own assessment scale that included a range of questions regarding pain, stiffness, tenderness, movement disability, oedema, crepitation and nocturnal pain. Li et at[83] caution against using generic instruments, such as the visual analogue scale to measure the complexities of knee osteoarthritis, and state that it may not be appropriate to use on their own, as it can affect the validity and reliability of the results.

The Khan et al[81] trial duration was 15 days with a 15-day follow-up. Teut et al[81] observed a significant improvement in pain intensity and WOMAC scores for the cupping group, when compared with the control group, at both four weeks and twelve weeks. The cupping group had a mean baseline pain intensity of 60.2mm (SD 12.2) out of 100mm, and after four weeks reduced to 38.4mm (95%CI, 30.5, 46.2). Out of the WOMAC subscales, physical function exceeded the MCID at four weeks, with a mean change of 12.1. For a clinically relevant change, a score exceeding 9.1 is required. The other WOMAC subscales reported a statistically significant difference between the cupping and control groups; however, failed to surpass the MCID. Khan et al[81,82] reported statistically significant changes between the cupping and control group for all items; however, due to the scale not being validated, it is difficult to determine whether the

results are clinically significant. Li et al[83] conducted a meta-analysis on western medicine vs western medicine and dry cupping for knee osteoarthritis. The results of two trials displayed a significant effect for all WOMAC subscores; however, no subscores exceeded the MCID. Meta-analysis for VAS of three trials did not observe any significant effect between the intervention and control groups.

2.6.4 Dry Cupping for Chronic Widespread Pain (Fibromyalgia Syndrome)

There have been two trials[84,85] that have investigated dry cupping for the treatment of fibromyalgia symptoms. Lauche et al[84] conducted the largest RCT, including 141 participants that were randomised to receive either dry cupping therapy, sham cupping or usual care. The trial observed a statistically significant difference between cupping therapy and usual care for pain intensity; however, no difference was found between the dry cupping and sham cupping groups. Significant differences were observed for pressure pain sensitivity in favour of the dry cupping group. The authors concluded that no recommendations could be made for dry cupping in the treatment of fibromyalgia due to the small treatment effect. An earlier medicinal cupping study for fibromyalgia by Cao et al[84] observed a greater pain reduction than Lauche et al,[85] with participants reporting an average pain reduction of 12.6mm after 15 days, compared to 4.4mm after 18 days as stated in Lauche et al.[85] Variances in pain response may be attributed to differences in fibromyalgia diagnosis criteria, resulting in a heterogeneous sample. The application of herbal medicine used in the Cao et al[84] study may have added a therapeutic component to the cupping treatment, resulting in a greater pain reduction. Cupping therapy was performed daily in the Cao et al[84] trial, compared to two treatments per week as described in Lauche et al.[85] It is unclear whether daily treatments provide an added benefit.

2.6.5 Dry Cupping for Plantar Heel Pain (Plantar Fasciitis)

Several studies have examined the use of dry cupping for plantar heel pain.[86,87] AlKadhrawi et al[86] conducted an RCT including 71 participants that received a single session of either dry cupping and an active movement protocol or an active movement protocol without cupping. Dry cupping was performed for 5-minutes over a MTrP that was identified in the gastrocnemius or soleus muscle before completing a series of exercises for 3-minutes. All participants had a covering placed over the calf area to blind the outcome assessors to the group allocation. Ge et al[86] randomised 29 participants to either dry cupping or electrical stimulation therapy at the painful site for 10-minutes, over a duration of four weeks.

Both studies included pain intensity, functional status and PPT as the primary outcome measures. Whereas, only AlKadhrawi et al[86] used secondary outcome measures of ankle ROM and strength. Ge et al[85] found no significant differences between the groups and concluded that both dry cupping and electrical stimulation were effective in reducing pain and improving function. AlKadhrawi et al[86] found that both groups significantly improved on the patient specific functional scale (PSFS), ROM, and pain with morning first steps; however, only the dry cupping group significantly improved with current VAS, PPT and strength. The study concluded that the addition of dry cupping with an exercise program was more beneficial to self-stretching and exercise alone.

2.7 Myofascial Decompression (MFD)

Myofascial decompression (MFD) is a contemporary myofascial dry cupping technique, predominantly used as a ‘decompressive’ myofascial release technique for soft

tissue mobilisation and fascial gliding. MFD is used for the treatment of musculoskeletal pathologies, sporting injuries, and rehabilitative purposes.[60] MFD employs both static and dynamic myofascial cupping approaches, as well as incorporating functional movement patterns to increase flexibility for improved range of motion.[89,90] Combining MFD with movement has been proposed to be more efficacious than traditional dry cupping methods, because it combines the effects of sustained controlled stretch and tissue changes associated with dry cupping.

Many of the early MFD cupping trials are published in grey literature, from theses.[51,90-92] Lacross[90] was the first study to introduce the term MFD by incorporating dry cupping with active movement. The trial compared MFD with active hamstring movement to foam rolling for participants with a hamstring injury, statistically significant differences were observed in both groups for range of motion and hamstring flexibility. Participant's perception of the treatment effect measured by the Global Rating of Change scale was found to favour the MFD group significantly. An additional two trials investigated the effects of MFD on hamstring flexibility[92] and strength[51] in a healthy population. Both Xie[92] and Pratt[51] compared static and dynamic myofascial cupping therapy. The results revealed a significant increase in ROM for both cupping groups[92]; however, no significant effect was observed for changes in strength.[51] Pratt[51] concluded that MFD can be used without a detrimental effect on strength, which would be of importance in the athletic population.

Smith[91] investigated the effects of MFD on shoulder range of motion. The trial compared MFD cupping with active shoulder movements to a rest control group. Minimal statistical significance was found, with only external shoulder rotation improving significantly, with an increase of 7.4°. MFD was only applied to the posterior rotator cuff

muscles; therefore, a limitation may be due to the anterior muscles not being treated with MFD. All MFD trials included a convenient sample of participants that were either collegiate athletes or had a mean age of 20-25 years; therefore, it is difficult to generalise to the population until further studies have been completed.

2.7.1 Myofascial Cupping and Range of Motion

Various myofascial cupping trials have used ROM as a primary outcome measure, with a focus on the athletic population,[88,90-95] chronic LBP[69,71] and neck pain.[78,89] Lumbar ROM was investigated by Markowski et al[71] and El Rahim et al,[69] whereby both studies observed a significant increase in lumbar flexion. Barger,[93] Lacross,[90] Kim et al[88] and Xie[92] examined the effects of myofascial cupping on hamstring ROM in an athletic population. Xie[92] observed a 7.13° increase immediately after cupping treatment, measured by active knee extension (AKE) with a goniometer. The results were statistically significant; though, the results may not be clinically meaningful. The literature varies when determining MCID values for the AKE test, Hamid et al[96] found that a change between 9.7 and 10.5° would be required to detect a MCID; however, Neto et al[97] found a difference of at least 7° is needed for a MCID. Barger[93] and Lacross[90] measured hamstring flexibility with a passive straight leg raise and a digital protractor and an overall statistically significant improvement of 5.41° and 4.07° was observed, respectively. Both results may not have reached clinical significance, as Neto et al[97] state that a change of at least 6° with a passive straight leg raise is required for a MCID. Conversely, Kim et al[88] conducted a passive straight leg raise in their trial, using dry cupping to increase hamstring ROM and observed an 11.4° difference, which exceeds the MCID. Yim et al[89] and Nasb et al[78] investigated dry cupping on cervical ROM. In the

Yim et al[89] crossover trial, dry cupping was compared to a McKenzie cervical stretching protocol. Significant improvements were observed in all ROM directions for the dry cupping group. When compared to the McKenzie's stretching group, cupping therapy was significantly better for extension and lateral flexion. Nasb et al[78] investigated four weeks of a combination of dry cupping and ischemic compression, compared to ischemic compression and dry cupping as standalone treatments for MTrP associated with chronic neck pain. All cervical range of motion measures were significantly improved in all treatment groups.

2.8 Physiological Mechanisms of Dry Cupping Therapy (Current Theories)

There are several proposed theories addressing the physiological mechanisms underpinning dry cupping therapy. The most cited theories include mechanotherapy, microcirculation, immunomodulation, genetic, and neural mechanisms. This section will review the evidence that support the current proposed theories.

2.8.1 Mechanotherapy

Thompson et al[98] define mechanotherapy as "any intervention that introduces mechanical forces with the goal of altering molecular pathways and inducing a cellular response that enhances tissue growth, modelling, remodelling, or repair". Soft tissue expansion therapies are types of mechanotherapy involving overstretching of the skin in a controlled mechanical manner to exploit the viscoelastic properties of the skin.[99]

External mechanical forces or mechanical stimuli include compression, tension, shear and fluid shear, vibration, and hydrostatic pressure.[98] Tham et al[63] have shown that

soft tissue enclosed in a cupping device is exposed to both a state of tension and compression. Alford et al[100] suggest that cellular activity may be altered by inducing changes in the cell shape, through mechanical cellular distortion from external mechanical forces.[100] However, the location of the cell, mechanical cellular properties, and interactions between the ECM and incoming mechanical stimulus dictate the precise cellular response.[98] Tissue regeneration and wound healing have been the primary focus of mechanotherapies.[99]

The target of mechanotherapy interventions is the ECM-integrin-cytoskeleton signalling axis, as the cytoskeleton represents the tensegrity architecture of ECM.[100] Tensegrity describes a structural-relationship principle, whereby, the structures employ continuous tension and discontinuous compression.[100,115] The cytoskeleton creates a dynamic mechanical equilibrium between the ECM and cellular traction forces, through a state of mechano-reciprocal isometric tension.[101] The elasticity and rigidity of the ECM microenvironment is altered by the cells that produce a traction force.[102] The transfer of mechanical force from the cytoskeleton to the ECM is mediated by transmembrane integrins. Integrin-mediated mechanisms are of interest, particularly the ones altering cellular functions, such as cytoskeleton-related tensegrity, cell shape-dependent function, and cell-matrix interactions.[99] Integrins have been shown to aid mechanotherapy, acting as a mechanosensor.[103] Another notable mechanosensor that mediate mechanical stimuli is transient receptor protein (TRP) channels, a group of ion channels located on plasma membranes. Several of the TRP subfamilies have been found to be associated with mechanical hyperalgesia and neuropathic pain.[104,105]

Mechanotransduction is the process whereby cells identify an external mechanical stimulus and transduce the signal into changes in intracellular biochemistry and gene

expression.[106] Mechanotherapies target and modulate one of four intracellular mechanotransduction signalling pathways; (1) mechanocoupling phase: the external mechanical signal is sensed inside the cell and is transformed into a mechanical signal; (2) biochemical coupling: mechanical signal is transduced into a biochemical signal resulting in protein or genetic modifications; (3) signal transmission: the sensor cell passes the biochemical signal to the effector cell; and (4) effector cell response.[99,106,107]

2.8.2 Microcirculation theory

2.8.2.1 Blood flow and fluid flow dynamics

Oxygen (O_2) is vital in supporting oxidative phosphorylation for proficient adenosine triphosphate (ATP) production. Tissue oxygenation is managed by the balance between the supply of oxygen from the vasculature and demand of tissue metabolic output. Changes in the supply or demand of oxygen results in hypoxia.[108]

In the presence of O_2, a primary cellular response is mediated by the hypoxia inducible factor (HIF) pathway, promoting short-and long-term adaptation. An upregulation of the vasodilatory enzyme inducible nitric oxide synthase (iNOS) rapidly increases O_2 supply. Short term or acute adaptation is achieved by vascular smooth muscle cells becoming relaxed, providing an increase in blood flow in response to nitric oxide (NO).[108]

Cupping therapy causes a temporary disruption to microcirculation and reduction of blood flow to the skin for several minutes, due to the compressive force directly under the rim of the cup (Figure 2.1). During the disruption, an accumulation of vasodilator chemicals and metabolites occurs in the tissues. Upon the removal of vascular compression, there is a dramatic increase in blood flow (reactive hyperaemia).[23,63,109,110]

Li et al[111] used near-infrared spectroscopy during a single cupping session and observed concentration changes in oxy-haemoglobin, deoxy-haemoglobin and blood volume. The study observed an increase in oxygenated and a decrease in deoxygenated haemoglobin during and post cupping treatment, changes in blood volume were also observed. In conditions where blood flow is impaired, such as chronic muscle myalgia, haemoglobin concentration changes in the microcirculation may give rise to therapeutic benefits, such as local tissue repair.[23,110,111] However, Li et al[111] used a strong vacuum pressure (-562.54mmHg) which is not typically used in clinical practice and further studies are required to observe haemodynamic effects of typically used pressures.

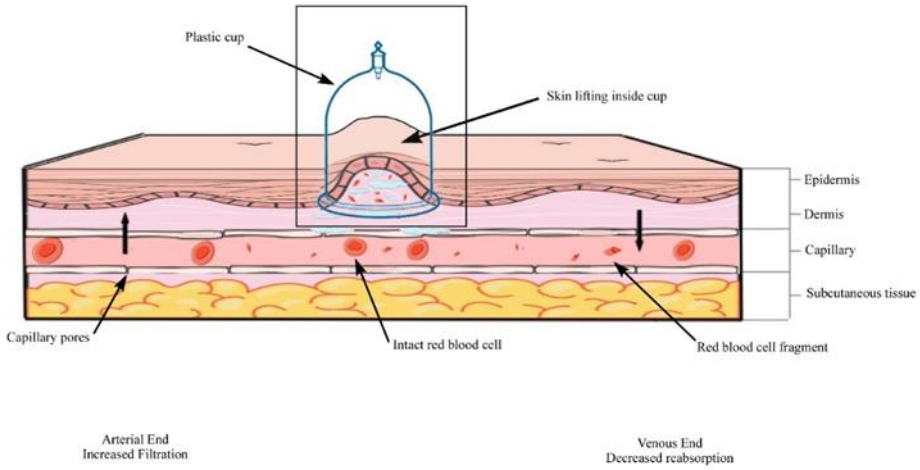

Figure 2.1 Compressive and decompressive force of vacuum cup (Adapted from El Sayed[109])

2.8.2.2 Fluid Exchange

Dry cupping has been proposed to influence tissue fluid exchange and facilitate the transport of fluids into the extracellular space. Filtration of interstitial fluid occurs at the arterial end of the capillaries. A net filtration pressure of -13 mmHg shifts tissue fluids to the outside of the capillaries and is absorbed at the venous end. A net absorption pressure of +7mmHg shifts tissue fluid to the inside of the capillaries. Normal capillary

pressure ranges from 10.5 to 22.5 mmHg.[112] There is a continuous exchange that occurs within the interstitial fluid and intravascular fluid compartments. The capillary walls filter tissue fluids across the interstitial spaces between parenchymal cells, before accessing lymphatic capillaries on venous return.[109] The volume of blood passing through the capillaries affects oxyhaemoglobin concentration and deoxyhaemoglobin concentration is affected by local tissue metabolism and the timing of erythrocytes passing through the capillaries for oxygen exchange.[113] The factors that determine interstitial fluid composition and pressures include equilibrium of Starling forces across capillary wall (plasma protein osmotic pressure and capillary hydrostatic pressure), plasma composition, surface area of capillary endothelium, lymph transport, capillary permeability and physicochemical properties of the interstitial space matrix.

During cupping therapy, the transient decrease in interstitial pressure due to the vacuum force (sub-atmospheric pressure) and mechanical stretch on the tissue may facilitate the transport of fluids into the extracellular space.[109,113,114] Emerich et al[30] observed local oedema and an increase in subcutaneous tissue under ultrasound, immediately after a session of dry cupping therapy and found local oedema was not immediately reabsorbed. Removal of negative pressure causes oedema to be a relative compressive force on blood vessels and individual cells[113] thus increasing hydrostatic pressure, resulting in increased capillary filtration.[109]

The increase of local oedema may temporarily provide lubricant to areas of connective tissue with limited sliding mobility. Stecco et al[35] state that muscle overuse or injury, increases the amount of HA, resulting in reduced lubricant from the change in viscosity. The changed environment increases resistance and friction of the sliding capability of the fascial layers, resulting in fascial densification. It has been suggested by

Findley[115] that modalities that increase oedema, such as cupping may also increase fluid flow adaptability. The increase in fluid may be beneficial in increasing fluid flow dynamics within the fascial layers and sliding capability.

2.8.3 Gene Expression Theory

During a hypoxic state, a systemic process occurs to maintain homeostasis, resulting in the regulation of numerous cellular activities, such as angiogenesis to increase oxygen delivery.[116] When oxygen is limited, mitochondrial respiration decreases, and cells are energetically challenged. Hypoxic conditions cause protein synthesis to be inhibited, as protein translation requires high energy consumption. Ribonucleic acid (RNA) transcription and splicing are activated under a hypoxic state to induce gene expression for hypoxic adaptation. Hypoxia-inducible factor (HIF) is a transcription factor that is activated to regulate hypoxia. There are over 100 genes that HIF-1 targets, including genes related to energy metabolism, anti-apoptosis, angiogenesis, and cell motility. Oxygen regulates the α subunit of HIF and the β subunit is constitutively expressed.[116]

The partial deprivation of O_2 during a cupping session leads to anaerobic metabolism, demonstrated by an increase in lactate concentrations and the ratio of lactate/pyruvate.[30] Shaban and Ravalia[117] propose that some therapeutic effects associated with cupping therapy may be due to gene expression being activated or inhibited in response to the anaerobic metabolism; however, this hypothesis is yet to be tested in experimental studies.

2.8.3.1 Capillary Shear Stress and Transient Hypoxia

Under normal conditions, blood vessels maintain homeostasis with a dynamic balance between mechanical forces, chemical stimulus, physiological stresses, and strains via biological responses and vasoprotective roles.[118] Shear stress induced by blood flow on the epithelial surface of blood vessels affect the release and signalling pathways of several factors, including angiogenesis-related membrane receptors, hypoxia inducible factor-1 alpha (HIF-1α) pathways, VEGF and endothelial nitric oxide synthase (producing NO). The release of VEGF stimulates endothelial cell proliferation and increases vessel networks.[113] The subdermal plexus is stimulated by vacuum pressure, increasing blood perfusion from vasodilation and angiogenesis. The subdermal plexus is a crucial component in skin blood perfusion remodelling under ischemic conditions.[116]

Stretching of tissues can produce blood flow changes, including transient ischemia, resulting in activation of the HIF-1α pathway. Chin et al[119] observed an increase in HIF-1α with cyclical stretching, accompanied by an increase in oxygenated tissue post stretch, suggesting transient hypoxia occurs during repetitive stretching. Additionally, cyclical-intermittent force application has shown higher rates of angiogenesis when compared to static applications.[120] During dry cupping, it has been shown that soft tissue enclosed within the cup is under stretch.[63]

Lactate, for much of the 20th century has been considered a waste product of glycolysis due to hypoxia, playing a major role of acidosis-induced tissue damage and muscle fatigue. More recently, lactate has been re-evaluated and has an important role in wound healing, as an intermediate in repair and regeneration.[121] High lactate levels and hypoxia are well documented characteristics of wound healing, due to the disruption of microcirculatory and increase of oxygen consumption; during this state, several cells

produce energy via anaerobic glycolysis.[122] Local hypoxia resulting from specific stimuli and a response to increased metabolic demand has shown an increase of endothelial proliferation.[123]

Microdialysis studies[30] conducted during a single cupping session found an increase in both lactate and pyruvate, confirming tissue hypoxia occurs during cupping. The study observed significant long-lasting elevated levels of lactate in the subcutaneous tissue, resulting from hypoxia; although, subcutaneous tissue has limited capacity to produce lactate. Emerich et al[30] suggest that the source of lactate may be a result of destroyed erythrocytes or from the underlying muscle, as 75% of lactate comes from blood flow or diffusion from the underlying muscle. Kairinos et al[124] state that transient tissue hypoxia would only be beneficial after the tissue pressure has been restored, when vasodilation and reactive hyperaemia have occurred.

2.8.4 Neural mechanism theory

An analgesic effect, similar to that of acupuncture analgesia, has been proposed as the underpinning mechanism of the pain abating effects of dry cupping.[125] Stimulation of the skin surface through external factors (pressure, heat or trauma) or internal factors (O_2, pH, hormones, cytokine levels or neurotransmitters) excites peripheral nerve endings and leads to CNS input.[126] The stimulation of local tissue from cupping activates nociceptors and stimulates Aδ and C-fibres through the spino-thalamo-cortical pain pathways. Musial et al[127] have suggested that the tissue trauma associated with cupping therapy leads to local vasodilatation and increased blood flow to the nociceptive environment. Increased blood flow also increases the concentration of factors that

sensitise peripheral nociceptors, including bradykinin, prostaglandins, histamine, potassium ions, serotonin, tumour necrosis factor and interleukins.

2.8.5 Immunomodulation theory

Cupping causes changes in the microenvironment and biological chemical signals, due to local tissue stimulation and damage, resulting in a release of signalling molecules that activate the neuroendocrine-immune system.[126] Following neuromodulation of the CNS, endocrine modulation is instigated. As a result, the hypothalamus releases neurotransmitters, including corticotropin-releasing hormone that stimulate the adrenal glands to release cortisol and noradrenaline. Once immune cells are stimulated, immunomodulation occurs, resulting in the release of neuropeptides, chemokines, and cytokines. Guo et al.[126] propose that the regulatory effects of cupping are a result of the neuroendocrine-immunomodulatory network being initiated by neuromodulation, endocrine modulation, and immunomodulation.

The proposed theories (mechanotherapy, microcirculation, immunomodulation, genetic, and neural mechanisms) for explaining the physiological mechanisms of dry cupping remain hypotheses. Many of the experimental studies that have been conducted used animal subjects, computer generated models, or small samples of human participants. However, it is still unclear on the specific physiological mechanisms underpinning dry cupping therapy, and future studies should continue to investigate the current theories and replicate the findings.

2.9 Conclusion

This literature review has presented the epidemiology of musculoskeletal conditions and the associated burden. The review presented an overview of myofascial tissue, the characteristics of myofascial pain syndrome and discussed myofascial trigger point theories. Pertinent to myofascial cupping therapy is myofascial release, as it underpins the theoretical basis – this chapter discusses the evidence associated with myofascial release therapies before moving on to present the dry cupping literature and the current evidence for myofascial decompression – the contemporary dry cupping technique that incorporates movement into treatment. Lastly, this literature review has attempted to provide a thorough overview of the proposed theories of the physiological mechanisms that underpin cupping therapy.

CHAPTER 3

Dry cupping for Musculoskeletal Pain and Range of Motion: a systematic review and meta-analysis

3.1 Introduction

Recent systematic reviews have investigated the use of cupping in musculoskeletal pain conditions.[6,7] However, there remains an absence of reviews specifically examining the use of *dry cupping* for the treatment of musculoskeletal pain and range of motion. Therefore, this study aimed to critically evaluate the evidence from randomised controlled trials (RCTs) to determine the efficacy and safety of western dry cupping methods for the treatment of musculoskeletal pain and range of motion.

The objective of this study was to evaluate the efficacy and safety of western dry cupping methods for the treatment of musculoskeletal pain and range of motion to answer the following research questions:

Is dry cupping an effective modality in reducing musculoskeletal pain?

Is dry cupping an effective modality for increasing range of motion?

Is dry cupping a safe modality for musculoskeletal complaints?

3.2 Subjects and Methodology

The protocol for this systematic review was registered on PROSPERO (Registration Number: CRD42018088855). The protocol for this review followed the Preferred Reporting Items for Systematic Reviews and Meta-analyses (PRISMA).

3.2.1 PICO Criteria

Criteria for considering studies for the current review

3.2.2 Types of Trials

Randomised controlled trial (RCTs) pertaining to the effects of dry cupping therapy for musculoskeletal pain and range of motion were included.

3.2.3 Types of Participants

Participants were limited to adults (18 years and older) that received dry cupping treatment for musculoskeletal pain or restriction in range of motion. There were no restrictions to the duration of pain period.

3.2.4 Types of Intervention

Treatment was limited to dry cupping therapy, including myofascial decompression for a musculoskeletal condition as the sole intervention or as an adjunct therapy, and based on a western biomedical treatment model. Any other form of cupping (e.g., wet cupping) was excluded. There were no limitations on frequency or duration of dry cupping.

3.2.5 Types of Comparison

Western medicine, sham cupping treatment, wait list control groups, usual care, or cross over intervention used as a control were included. Trials with an active

comparison group were also included (stretching, exercise, foam rolling). Trials were excluded if the comparison was not relevant to Western Medicine.

3.3 Included Outcome Measures

3.3.1 Primary Outcomes

The primary outcomes were pain and functional status. Pain was measured by visual analogue scale (VAS), numerical rating scale (NRS), Short Form McGill Pain Questionnaire (SMPQ) and pain pressure thresholds (PPT). Functional status was measured by validated self-reported functional ability questionnaires, SF-36, neck disability index, Roland-Morris Disability Questionnaire, Oswestry Disability Index and WOMAC.

3.3.2 Secondary Outcomes

Secondary outcomes were range of motion and adverse events. Range of motion was measured by a goniometer, inclinometer, or digital software. Any safety concerns, including adverse events, side effects, drop-outs, injury, or illness reported for each trial were recorded.

3.4 Search Protocol

3.4.1 Literature Searches

Authors SW and GF searched the following databases from their inception until April 2018: Cochrane Central Register of Controlled Trials, EBSCO Host, PEDro, ProQuest, PubMed, Science Direct and Scopus. In addition, a manual search for citation references in relevant literature was applied. Search strategies are outlined in Appendix

A and B. Titles and abstracts were screened by both authors for assessment against the inclusion criteria.

3.4.2 Trial Selection

Only RCTs relating to the effects of dry cupping therapy and published in the English language were included in the systematic review. Trials published in the form of dissertations or grey literature were included. A flowchart depicting the trial selection process using the preferred reporting items for systematic review and meta-analysis (PRISMA) is shown in Figure 3.1.

3.5 Data Extraction and Quality Assessment

Author SW extracted data from the included studies independently using a Cochrane Collaboration standardised data extraction form. Extracted information included study methodology (design, randomisation, blinding method), characteristics of participants (inclusion/exclusion criteria, sample size, age, gender, condition, pain duration), details of intervention and control (dry cupping method, location, frequency, vacuum pressure, duration, type of control, details of cointerventions), follow-up data (duration of follow-up, withdrawal rates and reasons), outcome data, data analysis (method of analysis, baseline comparability of groups, statistical techniques) and reported adverse events. LT verified the extracted data. Quality assessment was undertaken by authors SW and CC (Appendix C).

3.5.1 Assessment of Heterogeneity

Assessment of heterogeneity was based on the calculation of I^2. The Cochrane Collaboration provides the following interpretation of I^2: 0% to 30%, might not be important; 30% to 60%, may represent moderate heterogeneity; 50% to 75%, may represent substantial heterogeneity; and 75% to 100%, considerable heterogeneity.[128]

3.5.2 Assessment of Risk of Bias and Methodological Quality

The Downs & Black (D&B) quality assessment scale was applied to evaluate the methodological quality of the included trials.[129] SW and CC completed an independent evaluation of the included trials; disagreements and discrepancies between author evaluations were resolved through discussion or by consulting a third review author, GF. The D&B assessment scale is a validated risk of bias tool and has been found to have good inter-rater reliability.[129] The modified D&B assessment scale provides individual scores for each section and an overall numeric rating, out of a possible 28-points. The following score ranges were given to the corresponding quality levels: excellent (26–28), good (20–25), fair (15–19), and poor (≤14).

Reporting: the first subscale consists of ten items, totalling a possible eleven points. The reporting items address any potential bias through concise reporting on various areas of the study, including objectivity, hypothesis and aim of the study, outcomes and inclusion/exclusion criteria addressed, clear description of interventions, provision of principle cofounders, denominators and numerators reported, adverse events measured, participant attrition accounted for, and precise probability values reported.

External validity: the second subscale consists of three items, totalling a possible three points. This item assesses the ability to generalise the outcomes of the study.

Internal validity bias: the third subscale consists of seven items, totalling a possible seven points. This item measures the potential bias within the research design and includes factors relating to blinding, compliance, selective reporting and pertinence of outcomes and statistical measures.

Internal validity confounding bias: the fourth subscale consists of six items, totalling a possible six points. This item determines the potential bias within participant selection, recruitment, and randomisation. Additionally, concealment and participant follow-up are addressed.

Power: the last subscale consisting of one item, totalling a possible one point. This item has been modified to address the use of a power statement within the study, to detect a difference - if a difference really exists.

3.6 Statistical Analysis

3.6.1 Measures of treatment Effect

A quantitative meta-analysis of the included trials was conducted using RevMan 5.3 software (RevMan, Version 5.3., Cochrane Centre). For binary outcomes, data were summarised using risk ratios (RR) with 95% CI, and for continuous outcomes, data were summarised using mean difference (MD) or standard mean difference (SMD) with 95% CI. When heterogeneity I^2 statistic was less than 25%, a fixed-effects model was applied, greater than 25%, a random-effects model was applied.[128] Scores for outcome measures, such as visual analogue scale were converted to a 100-point scale.

3.6.2 Assessment of Clinical Relevance

Assessment of clinical relevance was made using the recommendations of the Cochrane Back Review Group,[130] defined as follows: small effect as MD less than 10% of the scale (e.g., 10 mm on a 100 mm VAS) and SMD or '*d'* scores less than 0.5. Medium effect as MD 10% to 20% of the scale and SMD or '*d*' scores from 0.5 to 0.8. Large effect as MD greater than 20% of the scale and SMD or '*d'* scores greater than 0.8.

3.6.3 Quality of Evidence

The overall quality of evidence for each outcome was assessed using the Grading of Recommendations, Assessment, Development and Evaluation (GRADE) approach. Quality of evidence is specified as high, moderate, low, and very low. Key factors are (1) limitations in study design; (2) inconsistency of results; (3) indirectness or generalisation of findings; (4) imprecision; and (5) others (such as publication bias). Depending on the quality of evidence evaluated, levels of evidence can be downgraded to moderate, low, or even very low.[131]

3.7 Results

3.7.1 Included Trials

The literature search identified 262 trials (Figure 1). Duplicates were removed, and 232 trials remained for title and abstract screening. A total of 21 RCTs including 1049 participants met the inclusion criteria. Significant characteristics of the included trials are summarised in Table 1. Sample sizes varied from 14 to 141, with a median of 40 participants. Of the 21 included trials, seven trials originated from Germany, six from the

United States, two from both India and Korea, and one each from Turkey, Taiwan, Iran, and Egypt. The treated musculoskeletal conditions were neck pain,[29,62,74-77,89] back pain,[68,69,131] knee osteoarthritis,[81,82] and fibromyalgia.[85] Numerous trials investigated the use of dry cupping for increasing range of motion for the hamstring muscle,[88,90,93] iliotibial band[94,95] and shoulder.[91] The number of treatment sessions ranged from 1 to 24, with a duration of 4 to 30-minutes per session. There was variation in dry cupping methods implemented, including vacuum cupping, fire cupping, stationary or static cupping, moving cupping, and myofascial decompression with active movements.

3.7.2 Excluded Trials

A total of 232 titles and abstracts were screened for eligibility (Figure 3.1). Seventy-eight trials did not include RCT methodology; 21 used wet cupping application; 30 were not related to a musculoskeletal condition; 24 trials were not relevant to manual therapy; 33 trials pertained to Traditional Chinese Medicine (TCM) theory; 19 were not published in the English language, and one trial was unable to be accessed. Twenty-six articles were accessed in full, with a further five trials excluded; three did not include RCT methodology, one was unable to be accessed in full, and one used wet cupping application.

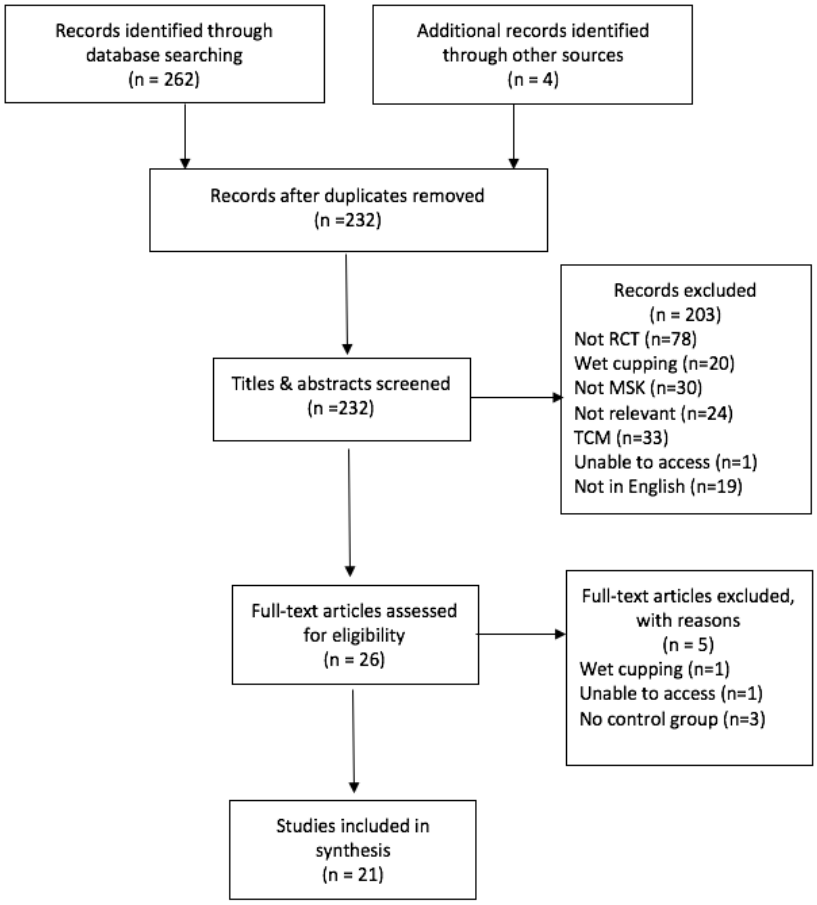

Figure 3.1. Flowchart of study selection depicted by preferred reporting items for systematic review and meta-analysis (PRISMA). Abbreviations: RCT, randomised controlled trial; MSK, musculoskeletal; TCM, Traditional Chinese Medicine.

3.7.3 Risk of Bias and Methodological Quality

Methodological quality of the trials ranged from excellent to fair. Overall, the quality of evidence was found to be fair, with a mean D&B score of 18/28. Two trials from Germany[69,84] were rated excellent quality with a D&B score of 26/28 and were found to have low internal bias. Seven trials[29,62,68,75-77,82] were rated as being good, with scores ranging from 20-25. Six trials[69,81,87,88,90,94] were rated fair, with a score ranging from 15-19. Six trials[74,89,91,93,95,131] were rated poor quality, with high internal bias and scores ranging from 8-14.

Blinding of both the participant and outcome assessors was only carried out by two trials,[85,94] two trials[70,89]attempted to blind the participants only, and two trials[77,88]attempted to blind the outcome assessors. All 21 trials were RCTs; however, only eleven of the trials described adequate randomisation techniques and only eight of the 21 trials sufficiently concealed randomised intervention assignment. Inadequate reporting of random variability in the data were observed in ten of the trials, with failure to report confidence intervals for normally distributed data or interquartile ranges for non-normally distributed data.

Table 3.1. Overview of the included clinical trials

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Akbarzadeh 2014 Iran	To investigate the effect of dry cupping therapy at BL23 point on the intensity of low back pain in primiparous women	100/0	Condition: low back pain post labour Gender (female): a: 50; b: 50 Mean age (SD): a: 25.0 (4.2) b: 27.0 (3.8)	Dry cupping (*fire cupping)*: Performed on the lumbar erector spinae muscle (BL23) for 15-20 min every day	Routine care and referral to specialist in case of severe pain	i) 4-days ii) 4 sessions iii) 2-weeks	VAS (cm), SMPQ	Baseline: before intervention Post: after intervention, at 2-week follow up	"The study results showed cupping therapy to be effective in sedation of pain. Thus, it can be used as an effective treatment for reducing the low back pain"	Good quality 21/28
Arslan 2015 Turkey	To investigate the effectiveness of active dry cupping of the upper shoulder and neck to alleviate pain	40/0	Condition: upper shoulder and neck pain Gender (female) a: 20; b: 20 Mean age (SD): Not reported	Dry moving cupping (*vacuum cupping*): Performed twice weekly 30 min cupping session to upper shoulders and neck	Unclear	i) 5-weeks ii) 10 sessions iii) nil	VAS	Baseline: before intervention Post: after intervention	"Cupping therapy is a non-invasive and harmless therapeutic application, and it can be confidently used to reduce upper shoulder and neck pain…"	Poor quality 11/28
Barger 2016 United States (thesis publication)	To examine the effects of compressive or decompressive soft tissue techniques on hamstring flexibility, strength, and perceived function	20/0	Condition: hamstring flexibility Gender (male): a: 10; b: 10 Mean age (SD) 21.35 (1.76)	Dry cupping (*vacuum cupping*): 6 stationary cups over the hamstrings for 3 mins, 1 min of moving cupping	Graston Technique for 4 min	i) 1-day ii) 1 session iii) nil	ROM, Muscle Strength, PFAQ, GROC	Baseline: before intervention Post: after intervention	"The results of this study suggest that both GT and MFD are effective therapies for improving hamstring flexibility and strength and decreasing pain immediately following the therapy"	Poor quality 14/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Biehl 2017 United States (thesis publication)	To determine if dry cupping is an effective treatment intervention in releasing ITB tightness and increasing hip and knee range of motion in a physically active population.	40/0	Condition: ITB flexibility Gender (male/female): 17/23 Mean age (SD): 21 (1.8)	Dry cupping (*vacuum cupping*): 4 stationary cups placed along the ITB for 7 min	Sham cupping	i) 1-day ii) 1 session iii) 24-hours	AROM, PROM	Baseline: before intervention Post: after intervention, at 24 hours follow up	"… This study supports the assumption that dry cupping may be a safe and effective treatment option to combat ITB tightness seen by clinicians."	Fair quality 15/28
Chi 2016 Taiwan	To investigate the effectiveness of cupping therapy in changes of skin surface temperature for relieving chronic neck and shoulder pain	60/0	Condition: chronic neck and shoulder pain Gender (male/female) a: 3/27; b: 2/28 Mean age (SD): a: 43.6 (8.0) b: 42.5 (7.4)	Dry cupping *(fire cupping)*: Cups placed on 3 acupoints for 10 min before repeating on opposite side	Rest for 20 mins	i) 1-day ii) 1 session iii) nil	VAS, SST, BP	Baseline: before intervention Post: after intervention	"One treatment of cupping therapy is shown to increase SST and reduce systemic blood pressure. Cupping therapy mimics an analgesic effect which has no known negative side effects and may be considered safe"	Good quality 23/28
Cramer 2011 Germany	To investigate the effect of pneumatic pulsation therapy on chronic neck pain compared to standard medical care	50/2	Condition: chronic neck pain Gender (male/female) a: 4/20; b: 6/18 Mean age (SD): a: 44.46 (10.79) b: 47.88 (13.50)	Dry cupping (*pneumatic pulsatile cupping*): Glass cupping massage over neck and shoulders for 10-15min with a mechanical device, followed with 4 stationary cups applied to the trapezius muscle for 5-10min	Usual care	i) 14-days ii) 5 sessions iii) 18-days	VAS, NDI, PPT, MDT, VDT, SF-36	Baseline: before intervention Post: after intervention	"Pneumatic pulsation therapy appears to be a safe and effective method to relieve pain and to improve function and quality of life in patients with chronic neck pain"	Good quality 24/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Doozan 2015 United States (thesis publication)	To evaluate the effectiveness of Chinese cupping in increasing iliotibial band range of motion	32/3	Condition: ITB flexibility Gender (male) a1: 10; a2: 9; a3: 10 Mean age (SD): a1: 20.6 (1.3) a2: 19.9 (1.2) a3: 20.3 (1.2)	Dry cupping (*vacuum cupping*): **a1**: 8 cups over ITB left for 5-10min + stationary bike for 10 min. **a2:** Cupping for 10 min + stretch protocol. **a3**: 10 min stationary bike + stretch protocol	Non-treatment leg used as control	i) 6-weeks ii) 6 sessions iii) 2-weeks	ROM	Baseline: before intervention During: 2 weeks, 4 weeks Post: after intervention, at 2 weeks follow up	"The study results showed Chinese cupping is beneficial as a short term and a possible long-term therapeutic technique that can be used to increase ROM in athletes"	Poor quality 11/28
El Rahim 2017 Egypt	To investigate the effect of cupping therapy with inferential therapy on mechanical low back pain	60/0	Condition: mechanical LBP Gender (male/female) a1: 10/10 a2: 10/10 b: 10/10 Mean age (SD): a1: 27.35 (4.23) a2: 28.8 (4.57) b: 27.3 (4.32)	Dry cupping *(fire cupping)*: a1: Cupping therapy + physical therapy a2: Cupping therapy + interferential therapy for 30min + physical therapy	30 min of traditional physical therapy	i) 4-weeks ii) 12 sessions iii) nil	ROM, SMPQ, RMDQ	Baseline: before intervention Post: after intervention	"Cupping therapy and interferential therapy in addition to traditional physical therapy can be used as an effective treatment in patients with mechanical low back pain"	Good quality 20/28
Ge 2017 United States	To determine the effects of dry cupping on pain and function with plantar fasciitis	29/0	Condition: plantar fasciitis Gender (male/female) a: 4/10; b: 10/5 Mean age (SD): a: 40.1 (14.6) b: 39.3 (13.5)	Dry cupping (*vacuum cupping)*: Cupping applied to the most painful site for 10 min	10 min of interferential therapy	i) 4-weeks ii) 8 sessions iii) nil	VAS, FAAM, LEFS, PPT	Baseline: before intervention During: at each session Post: after intervention	"The results support that both dry cupping therapy and electrical stimulation therapy could reduce pain and increase function in the population tested"	Fair quality 16/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Khan 2013 India	To evaluate the effect of cupping therapy in a clinical setting for knee osteoarthritis	62/22	Condition: knee OA Gender (male/female) a: 6/14; b: 8/12 Mean age (SD) Unable to determine	Dry cupping *(fire cupping)*: Stationary cups applied around the knee for 15 min	Medication Acetaminophen	i) 15-days ii) 11 iii) nil	Grading scales (pain, stiffness, crepitus, oedema, movement, tenderness)	Baseline: before intervention Post: after intervention	"The study proved cupping to be a good analgesic and anti-inflammatory with efficacy better than acetaminophen. Thus, cupping can be recommended for other painful conditions besides being a line of treatment for osteoarthritis"	Fair quality 17/28
Kim 2017 Korea	To measure the effects of cupping on flexibility, muscle activity, and pain threshold of the hamstring muscle compared to passive stretching in healthy subjects	30/0	Condition: hamstring flexibility Gender (male/female) a:12/3 Mean age (SD) 30.10 (5.52)	Dry cupping *(fire cupping)*: Cups applied to the hamstring muscle for 5 min	Passive stretch to the hamstring muscle and held for 10 sec x 9 reps	i) 1-day ii) 1 session iii) nil	ROM, PPT, EMG	Baseline: before intervention Post: after intervention	"It was evident from findings of this study that cupping therapy has as much positive effect on flexibility, pain threshold, and muscle contraction as passive stretching"	Fair quality 19/28
Lacross 2014 United States (thesis publication)	To examine the effectiveness of MFD and moist heat pack with foam roller on hamstring pathology	17/0	Condition: hamstring pathology Gender (male/female) a: 8/1; b:5/3 Mean age (SD) Unable to determine	Dry cupping (*vacuum cupping)*: Cups applied to hamstrings + active movement protocol with cups in place	Heat pack for 10min + foam roll	i) 1-day ii) 1 session iii) nil	ROM, PFAQ, GROC	Baseline: before intervention Post: after intervention	"Results of this study suggest that either treatment may be beneficial for ROM increases in patients with hamstring injuries"	Fair quality 17/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Lauche 2011 Germany	To determine whether a series of cupping treatments effectively relieves chronic non-specific neck pain	50/4	Condition: non-specific neck pain Gender (male/female) a: 7/15; b: 4/20 Mean age (SD) a: 48.6 (1.2) b: 53.0 (11.4)	Dry cupping *(fire cupping)*: Cups retained on affected areas for 10-20 min every 3-4 days	Wait list	i) 2-weeks ii) 5 sessions iii) 18-days	VAS, NDI, PD, SF-36, PPT, MDT, VDT	Baseline: before intervention During: at each session Post: after intervention	"A series of five dry cupping treatments appeared to be effective in relieving chronic non-specific neck pain. Not only subjective measures improved, but also mechanical pain sensitivity differed significantly between the two groups, suggesting that cupping has an influence on functional pain processing"	Good quality 24/28
Lauche 2013 Germany	This study aimed to test the efficacy of 12 weeks of a partner-delivered home-based cupping massage, compared to the same period of progressive muscle relaxation in patients with chronic non-specific neck pain.	61/7	Condition: non-specific neck pain Gender (male/female) a: 16/45; b: 6/24 Mean age (SD) a: 54.16 (12.7) b: 54.5 (12.3)	Dry cupping: Cupping massage twice weekly for 10-15 min	Progressive muscle relaxation	i) 12-weeks ii) 24 sessions iii) nil	VAS, NDI, PD, SF-36, PPT, HADS, FEW-16, PSQ-20, patient expectations	Baseline: before intervention Post: after intervention	"… cupping massage is no more effective than progressive muscle [relaxation] in reducing chronic non-specific neck pain. Both therapies can be easily used at home and can reduce pain to a minimal clinically relevant extent."	Good quality 23/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Lauche 2016 Germany	This study aimed to investigate the efficacy of cupping therapy compared to usual care and a sham procedure to improve symptoms and quality of life in patients diagnosed with fibromyalgia syndrome.	141/48	Condition: fibromyalgia Gender (male/female) a: 1/46 b: 1/47 b2: 0/46 Mean age (SD) a:54.35 (10.6) b1: 56.3 (8.7) b2: 56.8 (7.7)	Dry cupping (pneumatic *pulsatile cupping*): 4-8 cups retained on trapezius, levator scapula, latissimus dorsi, or gluteus maximus for 30 min, twice weekly	b1: Sham cupping b2: usual care	i) 18-days ii) 5 sessions iii) 6-months	VAS, FIQ, SF-36, PPT, MFI-20, PSQI, PPT, Blinding success, satisfaction, safety	Baseline: before intervention Post: after intervention, at 6-month follow up	"Five cupping treatments were more effective than usual care to improve pain intensity and quality of life in patients diagnosed with the fibromyalgia syndrome. Given that effects were small, and cupping was not superior to sham cupping treatments currently no recommendation for cupping in the treatment of fibromyalgia can be made."	Excellent quality 26/28
Saha 2017 Germany	To test the efficacy of cupping massage in patients with chronic non-specific neck pain	50/5	Condition: chronic neck pain Gender (male/female) a: 4/21; b 0/25 Mean age (SD) a: 54.3 (8.6) b: 53.3 (11.1)	Dry cupping (*vacuum cupping)*: Cupping massage twice weekly for 10 min	Usual care	i) 2-weeks ii) 5 sessions iii) 3-weeks	VAS, NDI, SF-36, PPT, MVD, VDT	Baseline: before intervention Post: at 3-week follow up	"Cupping massage appears to be effective in reducing pain and increasing function and quality of life in patients with chronic non-specific neck pain"	Good quality 25/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Singh 2016 India	Clinical evaluation of *Hijamat Bila Shurt* (Dry cupping) in the management of *Waja ul zahar* (Low Back Pain)	40/12	Condition: low back pain Gender (male/female) 4:1 ratio Mean age (SD) 20 (60)	Dry cupping (*vacuum cupping)*: Cupping over lumbosacral region for 20min	Diclofenac sodium 50 mg orally twice a day	i) 15-days ii) 6 sessions iii) nil	VAS	Baseline: before intervention During: at each session Post: after intervention	"The result was clinically significant in both therapies, but *hijamat bila shurt* was more effective in comparison to diclofenac sodium."	Poor quality 8/28
Smith 2015 United States (thesis publication)	To assess the effectiveness of MFD on shoulder ROM and strength in healthy overhead athletes	30/0	Condition: shoulder ROM Gender (male/female) 15/15 Mean age (SD) 22.5 (2.21)	Dry cupping (*vacuum cupping)*: IASTM + cupping to rotator cuff + active movement protocol with cups in place for a total of 10-15 min	Rest for 7 min	i) 1-day ii) 1 session iii) nil	ROM, strength	Baseline: before intervention Post: after intervention	"Due to lack of statistical significance in all variables except [external rotation] ROM, this study demonstrates little to no clinical relevance to the use of MFD for the purpose of increase immediate shoulder ROM and strength"	Poor quality 13/28
Teut 2012 Germany	To investigate the effectiveness of cupping in relieving the symptoms of knee osteoarthritis	40/0	**C**ondition: knee OA Gender (male/female) a: 5/16; b 8/11 Mean age (SD) a: 68.1 (7.2) b: 69.3 (6.8)	Dry cupping (pneumatic *pulsatile cupping*): Silicone dry cupping via mechanical cupping device over low back and knee joint for 10 min	No intervention	i) 4-weeks ii) 8 sessions iii) 12-weeks	WOMAC, VAS, SF-36	Baseline: before intervention Post: after intervention, at 12-week follow up	"Dry cupping with a pulsatile cupping device relieved symptoms of knee OA compared to no intervention"	Good quality 24/28

Table 3.1. Overview of the included clinical trials *(continued)*

Study ID, year and country	Aim	Sample size/ drop outs	Participants a) Intervention; b) Control	a) Intervention	b) Control	i) Treatment duration ii) Total No. of sessions iii) Follow up	Outcome measures	Measurement times	Authors conclusions	D&B score
Teut 2018 Germany	"The aim of our study was to investigate the effectiveness of dry pulsatile cupping in reducing pain and improving back function and quality of life in patients with nonspecific cLBP"	110/0	Condition: chronic LBP Gender (male/female) a1: 43.2/16 a2: 36.1/13 b: 32.4/12 Mean age (SD) a1: 49.0 (3.7) a2: 47.5 (13.8) b: 50.7 **(**10.7)	Dry cupping (pneumatic *pulsatile cupping*): Silicone dry cupping via mechanical cupping device over low back for 8 min, twice weekly a: 150-350 mbar a2: 70 mbar (minimal cupping)	Medication paracetamol 4x500mg/day	i) 4-weeks ii) 8 sessions iii) 12-weeks	VAS, SF-36	Baseline: before intervention Post: after intervention, at 12-week follow up	"Both forms of cupping were effective in cLBP without showing significant differences in direct comparison after four weeks, only pulsatile cupping showed effects compared to control after 12 weeks."	Excellent quality 26/28
Yim 2017 Korea	"…to investigate the differences in the angle of the cervical spine and the pain thresholds around the cervical vertebrae by applying the McKenzie exercise and the cupping therapy."	18/0	Condition: Neck ROM Gender (male/female) 12/6 Mean age (SD) 22.66 (2.98)	Dry cupping (*vacuum cupping)*: Cupping applied to upper trapezius & levator scapulae muscle to 8 min	McKenzie's stretch protocol for 8 min	i) 1-day ii) 1 session iii) nil	ROM, PPT	Baseline: before intervention Post: after intervention	"…Cupping treatment is more effective in improving the ROM of the cervical spine and pain thresholds compared to the McKenzie stretching method…"	Poor quality 14/28

Abbreviations: AKE Active Knee Extension Test; cLBP Chronic Low Back Pain; FAAM Foot and Ankle Ability Measure; FEW-16 (German) Assessment of Physical Wellbeing; FIQ Fibromyalgia Impact Questionnaire; GROC Global rating of change scale; GT Graston Technique; HADS Hospital Anxiety and Depression Scale, IASTM Instrument-assisted soft tissue mobilisation; LBP Low back pain; LEFS Lower Extremity Functional Scale; MDT Mechanical detection threshold; MFD Myofascial Decompression; MFI-20 The Multidimensional Fatigue Inventory; NDI Neck Disability Index; OA Osteoarthritis; PFAQ Perceived functional ability scale; PSQ-20 Perceived Stress Questionnaire; Pain pressure threshold; PSQI Pittsburgh Sleep Quality Inventory; RMDQ Rowland Morris Disability Questionnaire; ROM Range of motion; SD Standard Deviation; SF-36 36-Item Short Form Survey; SST Skin surface temperature; VAS Visual analogue scale; VDT Vibration detection threshold; WOMAC Western Ontario McMasters University Osteoarthritis Index

3.7.4 Effect of Interventions

Meta-analysis results are depicted in the forest plots (Figures 3.2-10) and GRADE summaries are presented in Tables 2-7. All results are based on the short-term effects of dry cupping (< 3 months).

3.7.4.1 Dry Cupping Therapy for Chronic Non-Specific Neck Pain

Effects of dry cupping vs. no treatment on pain

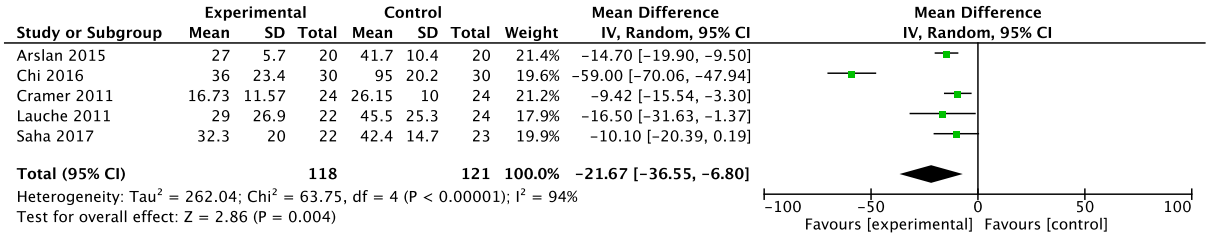

Figure 3.2. Effects of dry cupping vs. no treatment on pain (Outcome Measure: VAS 100mm)

Five trials including 239 participants were analysed for the effect of dry cupping for pain relief in chronic non-specific neck pain.[29,62,74-76] The five trials compared cupping therapy to no intervention,[62,74] standard medical care,[75] wait-list,[77] and usual care.[76] All five trials reported a statistically significant effect on reducing pain, in favour of dry cupping. Meta-analysis of the five trials (Figure 3.2) revealed a statistically significant effect on pain relief in favour of dry cupping, with a MD of -21.67 (95% CI, -36.55, -6.80; I^2 = 94%) and a large effect was observed when compared to the control group, with a SMD of -1.04 (95% CI, -1.79, -0.28). Heterogeneity was considerable between the five trials; omission of the outlying trial by Chi et al[62] reduced heterogeneity and resulted in a MD of -12.40 (95% CI, -15.99, -8.81; I^2 = 0%). For chronic neck pain, there was low-quality evidence (downgraded due to inconsistency and imprecision) that dry cupping had a significant effect on pain relief (Table 3.2).

Effects of dry cupping on neck functional status

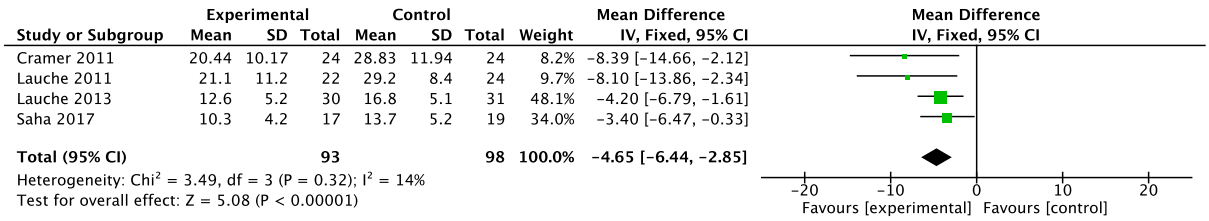

Figure 3.3. Effects of dry cupping on function (Outcome Measure: NDI)

Four trials including 191 participants were analysed for the effect of dry cupping on functional status in non-specific neck pain, measured by the neck disability index. All four trials[29,75-77] reported a significant effect on disability in favour of dry cupping. Meta-analysis of the four trials (Figure 3.3) displayed a statistically significant effect on functional status in favour of dry cupping, with a MD of -4.65 (95%CI, -6.44, -2.85; I^2 = 14%); although only a medium effect was observed when compared to the control group (SMD, -0.77; 95%CI, -1.07, -0.48). For functional status in chronic neck pain, there was moderate-quality evidence (downgraded due to imprecision) that dry cupping had a significant effect (Table 3.2).

Effects of dry cupping on pressure pain sensitivity

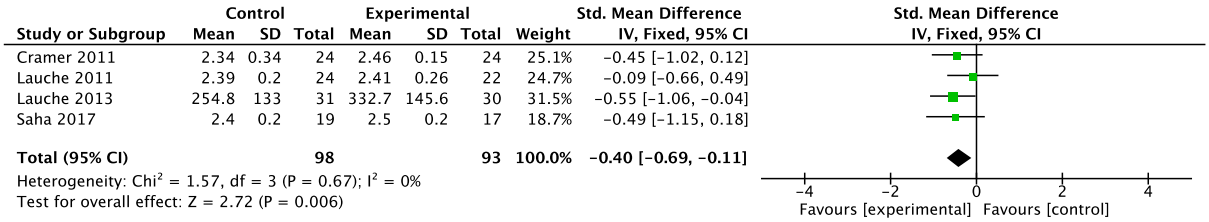

Figure 3.4. Effects of dry cupping on pressure pain sensitivity (Outcome measure: PPT)

Four trials including 191 participants were analysed for the effect of dry cupping on pressure pain sensitivity in non-specific neck pain, measured by pressure algometry. All four[29,75-77] trials reported a significant effect on pressure pain thresholds, in favour of

dry cupping. Meta-analysis of the four trials (Figure 3.4) displayed a statistically significant effect on pressure pain thresholds in favour of dry cupping, with a SMD of -0.40 (95%CI, -0.69, -0.11; I^2 = 0%); however, only a small effect was observed when compared to the control group. For pressure pain sensitivity in chronic neck pain, there was moderate-quality evidence (downgraded due to imprecision) that dry cupping had a significant effect (Table 3.2).

3.7.4.2 Dry Cupping Therapy for Low Back Pain

Effects of dry cupping on low back pain

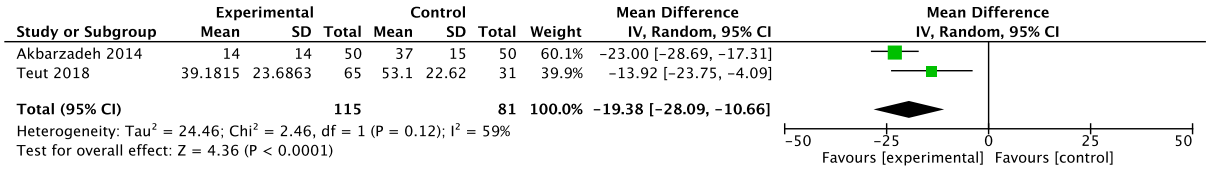

Figure 3.5. Effects of dry cupping on low back pain (Outcome Measure: VAS 100mm)

Two trials including 196 participants were analysed for the effect of dry cupping for pain relief in non-specific low back pain.[68,70] The trials compared cupping therapy to routine care,[68] minimal cupping and medication.[70] Both trials reported a significant effect on pain, in favour of dry cupping. Meta-analysis of the trials (Figure 3.5) displayed a statistically significant effect on pain relief in favour of dry cupping, with a MD of -19.38 (95%CI, -28.09, -10.66) and a large effect was found when compared to the control group, with a SMD of -1.08 (95%CI, -2.04, -0.12). Moderate heterogeneity was observed between the two trials, I^2 = 59%. The mean difference of -19.38mm on the VAS was found to exceed the clinically important range of ≥15mm.[132] For low back pain, there was low-quality evidence (downgraded due to inconsistency and imprecision) that dry cupping had a significant effect on pain relief (Table 3.3).

Effects of dry cupping on low back pain

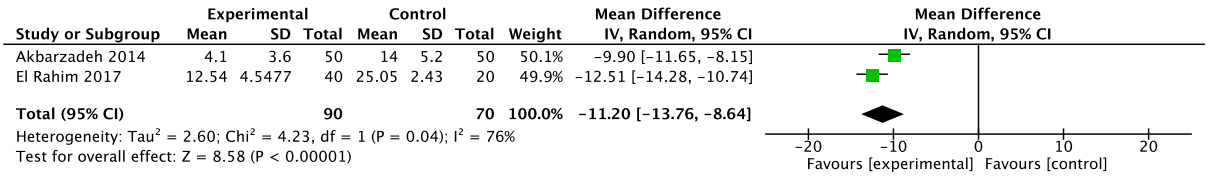

Figure 3.6. Effects of dry cupping on low back pain (Outcome Measure: Short Form McGill Pain Questionnaire)

Two trials including 160 participants were analysed for the effect of dry cupping for pain relief in non-specific low back pain.[68,69] The trials compared cupping therapy to routine care,[68] interferential therapy and traditional physical therapy.[69] Both trials reported a significant effect on pain, in favour of dry cupping. Meta-analysis of the trials (Figure 3.6) displayed a statistically significant effect on pain relief in favour of dry cupping, with a MD of -11.20 (95%CI, -13.76, -8.64) and a large effect was found when compared to the control group, with a SMD of -2.60 (95%CI, -3.48, -1.72); however, considerable heterogeneity was observed between the two trials, $I^2 = 76\%$. For low back pain, there was low-quality evidence (downgraded due to inconsistency and imprecision) that dry cupping had a significant effect on pain relief (Table 3.3).

3.7.4.3 Effects of Dry Cupping Therapy on Pressure Pain Sensitivity

Effects of dry cupping on pressure pain sensitivity in symptomatic participants

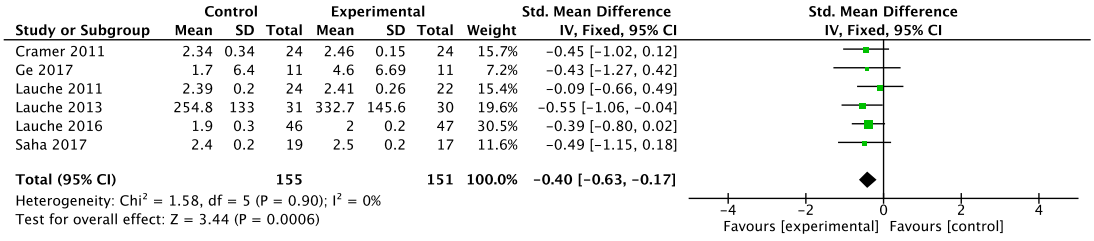

Figure 3.7. Effects of dry cupping on pressure pain sensitivity in symptomatic participants (Outcome Measure: PPT)

Six trials including 306 participants were analysed for the effect of dry cupping on pressure pain sensitivity in symptomatic participants, measured by pressure algometry. The trials used dry cupping therapy for the treatment of chronic neck pain,[29,75-77] plantar fasciitis[87] and fibromyalgia.[85] Meta-analysis of the trials (Figure 3.7) displayed a statistically significant effect on pressure pain thresholds in favour of dry cupping, with a SMD of -0.40 (95%CI, -0.63, -0.17; $I^2 = 0\%$); however, only a small effect was observed when compared to the control group. For pressure pain sensitivity in symptomatic participants, there was moderate-quality evidence (downgraded due to imprecision) that dry cupping had a significant effect (Table 3.4).

Effects of dry cupping on pressure pain sensitivity in asymptomatic participants

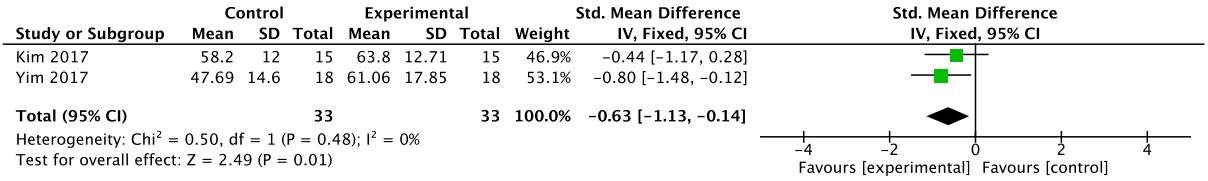

Figure 3.8. Effects of dry cupping on pressure pain sensitivity in asymptomatic participants (Outcome Measure: PPT)

Two trials including 66 participants were analysed for the effect of dry cupping on pressure pain sensitivity in asymptomatic participants, measured by pressure algometry. The two trials compared dry cupping therapy to passive stretching[88] and active stretching.[89] Meta-analysis of the two trials (Figure 3.8) displayed a statistically significant effect on pressure pain thresholds in favour of dry cupping, with a SMD of -0.63 (95%CI, -1.13, -0.14; $I^2 = 0\%$); however, only a medium effect was observed when compared to the control group. For pressure pain sensitivity in asymptomatic participants,

there was low-quality evidence (downgraded due to limitations and imprecision) that dry cupping had a significant effect (Table 3.5).

3.7.4.4 Effects of Dry Cupping on Range of Motion

Dry cupping vs. active control group

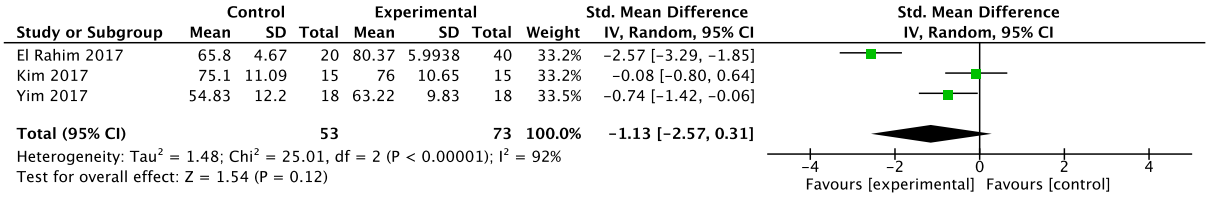

Figure 3.9. Dry cupping vs. active control (Outcome Measure: ROM)

Three trials including 126 participants were analysed for the effect of dry cupping on range of motion, measured by active range of motion using a goniometer. The trials compared dry cupping therapy to an active control group, including interferential therapy and traditional physical therapy,[69] passive stretching[88] and active stretching.[89] Meta-analysis of the three trials (Figure 3.9) displayed no significant effect on range of motion with a SMD of -1.13 (95%CI, -2.57, +0.31), with considerable heterogeneity observed between the two trials, $I^2 = 92\%$.

Dry cupping vs. no treatment

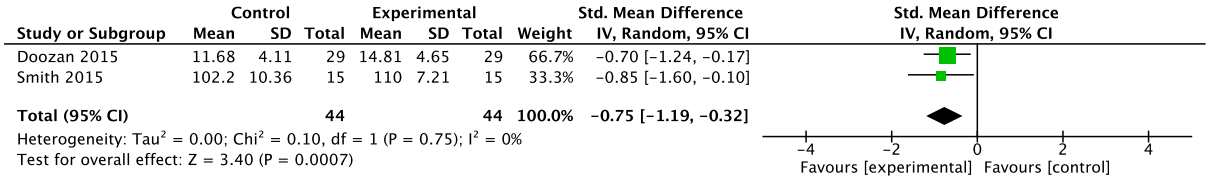

Figure 3.10. Dry cupping vs. no treatment (Outcome Measure: ROM)

Two trials including 88 participants were analysed for the effect of dry cupping on range of motion, measured by active range of motion using a goniometer. The two

trials[91,95] compared dry cupping therapy to no treatment. Meta-analysis of the two trials (Figure 3.10) displayed a statistically significant effect on range of motion with a SMD of -0.75 (95%CI, -1.19, 0.32; $I^2 = 0\%$; however, only a medium effect was observed when compared to the control group. For range of motion, there was low-quality evidence (downgraded due to limitations and imprecision) that dry cupping had a significant effect versus no treatment (Table 3.6).

Table 3.2. Dry cupping therapy in comparison to no intervention, standard care, wait-list and usual care in chronic non-specific neck pain

Quality assessment						No. of patients		Treatment Effect (95%CI)	Quality of the evidence (GRADE)
No. of studies	Limitations	Inconsistency	Indirectness	Imprecision	Other considerations	Intervention	Control		
Pain measured with: VAS from 0-100 (worse pain)									
5	no serious limitations	serious[1]	no serious indirectness	serious[2]	none	118	121	MD, -21.67 [-36.55, -6.80]	⊕⊕OO LOW
Functional status measured with: neck disability questionnaire									
4	no serious limitations	no serious limitations	no serious limitations	serious[2]	none	93	98	MD -4.65 [-6.44, -2.85]	⊕⊕⊕O MODERATE
Pressure pain sensitivity measured with: pressure algometry									
4	no serious limitations	no serious limitations	no serious limitations	serious[2]	none	93	98	SMD -0.40 [-0.69, -0.11]	⊕⊕⊕O MODERATE

[1]I^2=94%; [2]Sample size<400; Abbreviations: CI Confidence Interval, MD Mean Difference, SMD Standard Mean Difference, VAS Visual Analogue Scale

Table 3.3. Dry cupping therapy in comparison to routine care, minimal cupping and medication for non-specific low back pain

Quality assessment						No. of patients		Treatment Effect (95%CI)	Quality of the evidence (GRADE)
No. of studies	Limitations	Inconsistency	Indirectness	Imprecision	Other considerations	Intervention	Control		
Pain measured with: VAS from 0-100 (worse pain)									
2	no serious limitations	serious[1]	no serious limitations	serious[2]	none	115	81	MD -19.38 [-28.09, -10.66]	⊕⊕OO LOW
Pain measured with: Short Form McGill Pain Questionnaire									
4	no serious limitations	serious[3]	no serious limitations	serious[2]	none	90	70	MD -11.20 [-13.76, -8.64]	⊕⊕OO LOW

[1]I^2=59%; [2]Sample size<400; [3]I^2=76%; Abbreviations: CI Confidence Interval, MD Mean Difference, SMD Standard Mean Difference, VAS Visual Analogue Scale

Table 3.4. Effects of dry cupping therapy on pressure pain sensitivity in symptomatic participants

Quality assessment						No. of patients		Treatment Effect (95%CI)	Quality of the evidence (GRADE)
No. of studies	Limitations	Inconsistency	Indirectness	Imprecision	Other considerations	Intervention	Control		
Pressure pain sensitivity measured with: pressure algometry									
6	no serious limitations	no serious limitations	no serious limitations	serious[1]	none	155	151	SMD -0.40 [-0.63, -0.17]	⊕⊕⊕O MODERATE

[1]Sample size<400; Abbreviations: CI Confidence Interval, MD Mean Difference, SMD Standard Mean Difference, VAS Visual Analogue Scale

Table 3.5. Effects of dry cupping therapy on pressure pain sensitivity in asymptomatic participants

Quality assessment						No. of patients		Treatment Effect (95%CI)	Quality of the evidence (GRADE)
No. of studies	Limitations	Inconsistency	Indirectness	Imprecision	Other considerations	Intervention	Control		
Pressure pain sensitivity measured with: pressure algometry									
2	serious[1]	no serious limitations	no serious limitations	serious[2]	none	33	33	SMD -0.63 [-1.13, -0.14]	⊕⊕OO LOW

[1]Serious risk of bias; [2]Sample size<400; Abbreviations: CI Confidence Interval, MD Mean Difference, SMD Standard Mean Difference, VAS Visual Analogue Scale

Table 3.6. Effects of dry cupping therapy in comparison to no treatment on range of motion

Quality assessment						No. of patients		Treatment Effect (95%CI)	Quality of the evidence (GRADE)
No. of studies	Limitations	Inconsistency	Indirectness	Imprecision	Other considerations	Intervention	Control		
Range of motion measured with: goniometer									
2	serious[1]	no serious limitations	no serious limitations	serious[2]	none	44	44	SMD -0.75 [-1.19, -0.32]	⊕⊕OO LOW

[1]Serious risk of bias; [2]Sample size<400; Abbreviations: CI Confidence Interval, MD Mean Difference, SMD Standard Mean Difference, VAS Visual Analogue Scale

3.7.5 Adverse Events

Of the 21 RCTs included in this review, an adverse event statement was reported in 11 trials; the remaining 10 trials failed to mention adverse events. Of the 11 trials that reported on adverse events, two trials reported that no adverse events occurred during the trial duration and nine trials reported a total of 47 adverse events occurred in the dry cupping group. All adverse events are summarised in Table 8. Most symptoms were of mild to moderate severity, resolving within 48-hours, and included mild muscular soreness (18.9%), increase in pain (13.79%), and an onset of a headache (3.45%). Mild hematomas were also reported, as was blister formation – often associated with fire cupping.

There were two serious adverse events reported in the dry cupping group; however, both authors concluded was not a consequence of the intervention.[75,76] Firstly, Lauche et al[77] reported a participant was diagnosed with a prolapsed intervertebral disc in the home-based cupping massage group. Secondly, a participant from the Saha et al[76] trial was diagnosed with a lipoma after the first cupping session that required surgical removal. The authors concluded that it was unlikely that the cupping massage caused the lipoma, although, it may have elicited the visibility. A case-study report is available; however, it is only available in German.[134] Overall, the adverse events were mild to moderate, with two serious events – not directly resulting from the dry cupping treatment; the overall relative risk ratio from dry cupping therapy was 1.88.

Table 3.8. Reported adverse events for dry cupping in musculoskeletal conditions

Study ID	Sample Size	No. of events T = Treatment group C = Control group	Details of the reported adverse events
Chi 2016	(n=60)	**T:** (n=2)	**T:** mild low back pain related to the seated position (n=2)
Cramer 2011	(n=50)	**T:** (n=5)	**T:** muscle soreness for 1–2 days (n=2); minor hematoma at the treated site for 2 days (n=1); increased neck pain for 1–5 h (n=2)
Khan 2013	(n=62)	**T:** (n=11) **C:** (n=8)	**T:** blister formation (n=5); ecchymosis (n=6) **C:** GI symptoms (n=8)
Lauche 2011	(n=50)	**T:** (n=1)	**T:** symptoms temporarily worsened (n=1)
Lauche 2013	(n=61)	**T:** (n=3)	**T:** increased muscular tension and pain (n=1); pain in shoulder (n=1); prolapsed intervertebral disc (n=1)
Lauche 2016	(n=141)	**T:** (n=4) **C:** (n=3)	**T:** severely increased pain (n=1); bruised ribs (n=1); flu (n=1); acute torticollis (n=1) **C:** torn meniscus (n=1); persistent pain after spinal operation (n=1); flu (n=1)
Saha 2017	(n=50)	**T:** (n=5)	**T:** headache <1hr (n=2); upper back pain <48hrs (n=1); vertigo <48hrs (n=1); lipoma (n=1)
Teut 2012	(n=40)	**T:** (n=6)	**T:** mild hematoma (n=3); self-limiting light tingling sensations for a few minutes in the legs after cupping the knee (n=2); increased LBP <24hrs (n=1)
Teut 2018	(n=110)	**T:** (n=10)	**T:** aggravation of LBP <24hrs (n=2); light muscular backache (n=8)

***Abbreviations: GI* Gastrointestinal; *LBP* Low back Pain**

3.8 Discussion

The purpose of this study was to evaluate the efficacy and safety of dry cupping therapy for the treatment of musculoskeletal pain and range of motion. To our knowledge, this review is the first systematic review and meta-analysis specifically examining the effects of dry cupping therapy for musculoskeletal pain and range of motion.

Meta-analyses were conducted for 15 trials. When the included trials were pooled and analysed, a significant large effect was observed for dry cupping therapy on pain intensity in chronic neck pain and non-specific low back pain when compared to control groups. A significant medium effect was found for dry cupping therapy on neck function when compared to the control group. Despite the significant effect, the quality of evidence to support the use of dry cupping for chronic neck pain and low back pain was low-quality, due to high heterogeneity and small sample size (<400 according to the GRADE recommendations).[131] The analysis for chronic neck pain revealed high heterogeneity; when an outlying trial[62] was omitted from the analysis, heterogeneity was low. This trial reported high baseline VAS scores with little to no improvement in the control group, resulting in a large effect and variability in the meta-analysis data.[62] For changes in pressure pain sensitivity and functional status, the quality of evidence was moderate, due to serious limitations associated with small sample sizes. For pressure pain sensitivity, there were different effects between symptomatic and asymptomatic participants, with moderate-level evidence of a small effect in symptomatic patients and low-level evidence of a medium effect in asymptomatic participants. Most of the symptomatic participants were diagnosed with chronic neck pain; when a separate analysis was conducted for chronic neck pain, a similar small effect was observed when compared to the control group. Moreover, a study by Lauche et al[135] found that pressure pain thresholds are not a

reliable measure of pain intensity in chronic neck pain and observed a lack of association between pressure pain sensitivity and pain intensity.

The reported minimal important change for low back pain on a visual analogue scale (VAS) is 15mm on a 100mm scale.[133] A change of over 20mm, as seen in the results for low back pain in this review, exceeds the clinically important range. This suggests that the changes in pain from the dry cupping treatment were clinically meaningful; however, these results do not take into consideration patient preferences, risks, and costs; therefore, it cannot be deemed clinically important until further research with high-quality trials has been undertaken. A previous review by Lauche et al[136] reanalysed four cupping trials to assess the minimal clinical important differences (MCID) for chronic neck pain patients. Lauche et al[136] observed comparable results in pain reduction to other conventional therapies. Results revealed a MCID of -8mm (-21%) on the VAS and substantial clinical benefit (SCB) of -26.5mm (-66.8%). For a clinical benefit, a percentage change of over 50% for SCB is recommended.[136]

This current systematic review assessed the risks and safety of dry cupping therapy for the treatment of musculoskeletal pain, thus providing important information when judging an intervention as clinically meaningful. A total of 11 trials included an adverse event statement, with a total of 47 adverse events reported for the dry cupping group. Most of the events were mild to moderate and abated within 48-hours. Two serious events were reported but were not associated with the treatment.[76,77] The most frequent events reported in the dry cupping group were mild muscular soreness, increased pain, headache, and blister formation. Dry cupping can be performed with either a manual handheld pump (or mechanical device) or heat from an ignited cotton ball and glass cups (fire cupping). Blister formation and burns have mostly been associated with fire cupping,

and numerous severe adverse events have been documented through case reports.[137] This current review found no reports of burns in the included trials that used fire cupping[29,62,68,69,81,88]; however, blister formation was reported by Khan et al.[81] Vacuum cupping using a manual handheld pump has far less risks than the use of fire cupping; however, it is unknown whether the benefits of heat outweigh the risks associated with fire cupping. Caution is warranted in the use of fire cupping and it is recommended that only trained practitioners perform this technique.

Previous systematic reviews and meta-analysis have examined the efficacy of all cupping methods, rather than dry cupping alone. Cao et al[65] conducted a meta-analysis on two dry cupping trials that produced a significant effect for reducing pain and improving quality of life. The analysis combined data from two separate musculoskeletal conditions, knee osteoarthritis and chronic neck pain, and therefore may not be generalisable to a specific condition. Recent systematic reviews have investigated cupping therapy on chronic neck pain[6,7] and chronic back pain[67]; however, the recent reviews included all types of cupping therapy. Nonetheless, Kim et al[7] reported similar results in their meta-analysis to this current review for the effects of cupping on chronic neck pain and function. Kim et al[7] observed a significant reduction in pain and improved function in patients treated with cupping compared with no treatment. Additionally, when compared to active treatment, there was also a significant reduction in pain and improved quality of life. The results reported by Kim et al[7] were not limited to dry cupping therapy; however, they do concur with the results produced from this current review.

From this review, eight trials[69,88-91,93-95] investigated the use of dry cupping as a form of myofascial release to increase range of motion. Meta-analysis of dry cupping compared to an active control group did not display significant differences. Dry cupping

was found to have a medium effect when compared to no treatment; however, the quality of evidence was low. The individual trials found dry cupping to be just as effective as passive stretching[88] and self-myofascial release with the use of a foam roller,[90] with both trials reporting no significant differences between the interventions. Furthermore, dry cupping was found to be more effective than the Mc Kenzie's cervical stretch protocol[89] for increasing cervical range of motion and traditional physical therapy[67] for increasing lumbar range of motion.

D&B scores varied greatly between 21 RCTs, ranging from excellent to poor quality, with almost half the trials found to be good quality. For internal validity, many trials suffered due to lack of blinding of participants and outcome assessors. Most trials did not perform any blinding of the intervention to participants; however, a sham cupping device was used in two trials.[85,94] The reliability of the sham device was tested in a pilot study[138] and it was reported that the device was valid; however, in contrast, Lauche et al[85] observed a lack of success in their trial, with 73.2% of patients correctly identifying the sham treatment, resulting in questionable validity of the sham device for blinding participants.

External validity was weak for more than half of the trials examined, with most of the trials using convenient samples or healthy college athletes that are not generalisable to the population at large. Treatment representation was weak for many studies, as dry cupping is not representative of a usual intervention to treat musculoskeletal conditions in the facilities where participants were treated. Other domains of limitation included statistical power, with less than half of the included trials including a power analysis in the methodology. Small sample sizes due to underpowered trials can lead to an overestimation of the treatment effects or fail to detect a clinically important effect.[139]

There were several limitations associated with this current review. A language restriction of English resulted in the exclusion of 19 trials and may have resulted in a possible selection bias. Many of the included trials had small sample sizes which may lead to statistical heterogeneity and overestimation of the effect size. Additional sources of heterogeneity may have been from multiple interventions performed, variation in comparison groups between trials, and patient characteristics, including pain duration. This study only investigated the short-term (less than 3 months) effects of cupping, and the long-term effects remain unknown.

The specific physiological mechanisms underpinning dry cupping remain mostly unclear; however, the proposed theories prevail within the literature.[23,30,63,125,140] Future trials should continue to investigate the mechanisms of dry cupping therapy and how the application of cupping may benefit musculoskeletal pain conditions, additional to the clinical importance of dry cupping therapy from the perspectives of patients to assess the MCID. Cost analysis should be conducted to determine the benefits of dry cupping treatment compared to other interventions currently used for the treatment of musculoskeletal conditions. Adverse event statements should continue to be reported to monitor the safety and risks of dry cupping therapy. Furthermore, future trials should examine the long-term effects of dry cupping and ensure the sample size is appropriate, and the trial is considerably powered.

3.9 Conclusion

To our knowledge, this current systematic review is the first to analyse western dry cupping methods in the treatment of musculoskeletal pain and range of motion. The results suggest that dry cupping may be effective in reducing pain and improving

functional status in patients with chronic neck pain when compared with no intervention. A significant reduction in pain for non-specific low back pain was observed, although the quality of evidence was found to be moderate to low. The adverse events associated with dry cupping were typically mild to moderate and resolved within 48-hours. Considering the low quality of evidence, further higher-quality RCTs with larger sample sizes and long-term outcomes are warranted to provide definitive conclusions regarding the effectiveness and safety of dry cupping for the treatment of musculoskeletal pain and range of motion.

CHAPTER 4

Reliability and Validity of Pressure Algometry for Measuring Pressure Pain Thresholds of the Hamstring Muscle

4.1 Introduction

There are numerous subjective methods of measuring the perception of pain, including unidimensional and multidimensional self-reported pain intensity scales.[141] One method of quantifying subjective pain and tissue sensitivity is pressure algometry.[142] A pressure algometer measures a controlled mechanical force on the soft tissue and the minimum pressure that induces patient reported discomfort, tenderness or pain is defined as the pressure pain threshold (PPT).[142] Pressure algometry has been widely used to measure pressure pain sensitivity, hyperalgesia and trigger points.[143] Pressure algometry has been shown to be a stable and reliable outcome measure for assessing change in pain sensitivity[144,145] and is used in clinical settings for the diagnosis of myofascial pain conditions.[143] Due to the high individual variability of an examiner, it is important to establish intrarater reliability of the instrument for reproducibility of consistent clinical measurements. Reproducibility of clinical measurements has been highlighted as an important aspect of clinical research to confidently detect treatment effects.[146]

Little available literature exists pertaining to the reliability and validity of pressure algometry for the hamstring muscle. Previous lower limb studies have examined intra- and inter-examiner reliability of pressure algometry for lower limb nerves,[147] medial knee pain,[148] knee osteoarthritis,[149-151] patellar tendinopathy,[152] calf [153] and foot musculature.[154]

This study aimed to establish intrarater reliability of the pressure algometer for measuring hamstring pressure pain thresholds.

4.2 Methodology

4.2.1 Participants

A total of 15 participants were recruited from Victoria University (mean age = 28.93 years; range = 21-56 years). There were two participants lost to follow-up and thirteen were included in the final analysis. The inclusion criteria were healthy participants that were available to attend three separate testing sessions, all completed on the same day. Exclusion criteria included history of lumbar spine or lower extremity pathology, current low back pain, systemic disease, diabetes mellitus, rheumatic disease and pregnancy. All participants provided written informed consent approved by Victoria University Human Research Ethics Committee (VUHREC), which also approved the study.

4.2.2 Procedure

Participant characteristics, such as age, height, and weight were obtained during the first session. PPTs were measured using an electronic handheld pressure algometer (Somedic Algometer, Sweden) for all test sites. The algometer was calibrated before any testing commenced. The algometer consists of a 1cm circular rubber tip that connects to a pressure transducer within the handle of the unit, and the rate of applied pressure is displayed on the output screen. Pressure algometry was undertaken by a registered physiotherapist with 10 years of experience. The procedure was based on the method used by Fryer et al.[155]

Three sites were measured on the dominant leg of each participant using algometry:

1. Mid muscle belly of the medial hamstring muscle, determined by measuring halfway between the ischial tuberosity and popliteal fossa.
2. Mid muscle belly of the lateral hamstring muscle, determined by measuring halfway between the ischial tuberosity and popliteal fossa.
3. A point of marked tenderness to palpation within the hamstring muscle.

Research has shown that there are no significant differences in PPTs between dominant and non-dominant measurement sites.[156] The sites were marked with a permanent marker during the first session to ensure subsequent measurements were consistent. Before commencing measurement, a demonstration of PPT was performed at the participant's wrist to explain the testing protocol. Participants were instructed to wear shorts and to lay prone on a plinth for testing on the posterior thigh. The algometer was positioned perpendicular to the body surface and a constant pressure of 30 kPa/second was applied until the participant started to feel discomfort, tenderness, or pain. Once the participant indicated a change in sensation, the applied pressure ceased, and the maximum pressure used was recorded. For each site, three PPT readings were performed, with a 20-second break between each reading. The mean of three consecutive readings was used as the PPT measurement for the session. The participants were then asked to attend two subsequent measurement sessions on the same day with at least 30-minutes between each session.

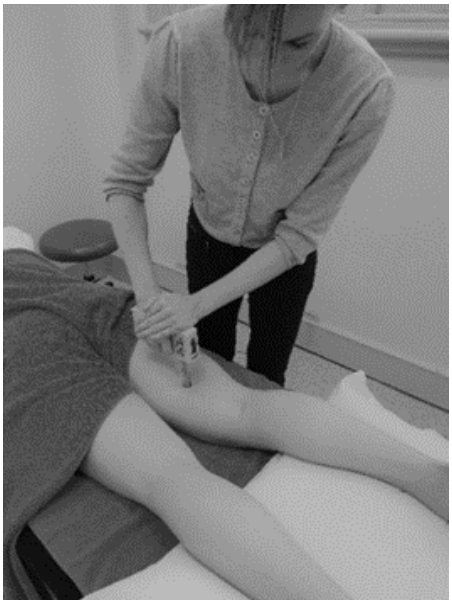

Figure 4.1. PPT measurement

4.2.3 Statistical Analysis

Data analysis was conducted to assess the intrarater reliability of the PPT measurements. All data was entered into SPSS statistical package version 25 (SPSS Inc, Chicago, IL) and descriptive statistics were collated for participants' age, height, weight and BMI (body mass index). The Intraclass Correlation Coefficient (ICC) average estimates and their 95% confidence intervals was calculated on the PPT measurements, based on a mean-rating (k=3), absolute-agreement, 2-way mixed model. The precision of the PPT measurement procedure was estimated by the standard error of measurement (SEM) and an ANOVA was conducted to assess any systematic variability in the data. The SEM was calculated using the formula ($SEM = S \times \sqrt{1 - ICC}$), where S is the pooled standard deviation and ICC is the reliability coefficient. Statistical significance was set at

the alpha 0.05 level. Reliability can be determined based on the work by Shrout and Fleiss[157] and the guidelines provided by Koo & Li;[158] values less than 0.5 are indicative of poor reliability, values between 0.5 and 0.75 indicate moderate reliability, values between 0.75 and 0.9 indicate good reliability, and values greater than 0.90 indicate excellent reliability.

4.3. Results

A total of six males and seven females were included in the final analysis (Table 4.1). The mean PPT for medial hamstring was 543.60 kPa (SD 207.27), lateral hamstring 492.26 kPa (SD 170.10) and tender point 451.46 kPa (SD 157.88). The PPT readings ranged from 185.00 to 1193.00 kPa (Table 4.2). Tests of normality (Shapiro-Wilk) revealed significance for one of the measurement sessions that was further explained by an outlier. Log transformation resulted in normally distributed data for all measurements.

Table 4.1. Participant characteristics for algometry testing.

	Male (n=6)	**Female (n=9)**
Age (years	30.50 ± 14.49	27.89 ± 6.07
Height (cm)	180.53 ± 8.32	164.72 ± 9.99
Weight (kgs)	79.37 ± 7.65	70.31 ± 8.43
BMI	24.35 ± 1.63	26.08 ± 4.17

Data are mean ± Standard deviation; BMI: body mass index

Table 4.2. Mean pressure (kPa) for each PPT measurement (sessions 1,2,3) per testing site.

	Measurement	**Mean (SD, SE)**	**Range**	**Median**
Medial Hamstring	Session 1	566.85 (234.60, 37.57)	230.00-1188.00	518.00
	Session 2	529.31 (197.45, 31.62)	284.00-1188.00	512.00
	Session 3	534.64 (190.46, 30.50)	262.00-1193.00	490.00
Lateral Hamstring	Session 1	487.44 (166.54, 26.67)	201.00-811.00	482.00
	Session 2	484.41 (174.77, 27.99)	230.00-843.00	444.00
	Session 3	504.92 (172.57, 27.63)	235.00-856.00	518.00
Tender Point	Session 1	440.59 (167.30, 26.79)	185.00-811.00	405.00
	Session 2	459.23 (162.65, 26.04)	202.00-861.00	435.00
	Session 3	454.56 (146.48, 23.46)	239.00-880.00	440.00

The average measure intrarater reliability between the three PPT measurement sessions for medial hamstring measurements were excellent (ICC=0.91; 95%CI 0.78-0.97) (Table 4.3). The average measure intrarater reliability were good for both lateral hamstring (ICC=0.83; 95%CI 0.58-0.95) and tender point measurements (ICC=0.88; 95%CI 0.67-0.96). The SEM values from Table 4.3 varied slightly between testing sites, with a total error of 21.78 kPa.

Table 4.3. Intraclass Correlation Coefficients (ICC) with 95% Confidence Intervals for repeated PPT measurements per testing site.

	Mean PPT (SD)	**Median PPT**	**ICC (95%CI)**	**SEM (kPa)**
Medial Hamstring	543.60 (207.27)	512.00	0.72 (0.55, 0.88)	109.68
Lateral Hamstring	492.26 (170.10)	482.00	0.71 (0.53, 0.87)	91.60
Tender Point	451.46 (157.88)	429.00	0.69 (0.50, 0.87)	87.90

The intrarater reliability was higher among male participants than female participants for medial hamstring and tender point measurements (Table 4.4). Male participants had larger SEM values compared to females for both medial and lateral hamstring measurements; however, females had a larger SEM for tender points. The overall total error for females and males was 12.93 kPa and 27.33 kPa, respectively. Repeated measures tests revealed no significant differences between sexes.

Table 4.4. Intraclass Correlation Coefficients (ICC) and Standard Error of Measure (SEM) for repeated PPT measurements per testing site for female and male participants.

		Mean PPT (SD)	**ICC (95%CI)**	**SEM (kPa)**
Medial Hamstring	Female	527.25 (146.89)	0.59	94.06
	Male	562.67 (260.97)	0.78	122.41
Lateral Hamstring	Female	537.90 (156.11)	0.65	92.36
	Male	439.00 (171.60)	0.68	97.07
Tender Point	Female	497.24 (166.50)	0.58	107.90
	Male	398.06 (129.36)	0.64	77.62

Log transformation of the data revealed slightly lower intrarater reliability values (Table 4.5); however, still provided good reliability. Medial hamstring measurements were 0.87, lateral hamstring measurements were 0.89 and tender point measurements were 0.88.

Table 4.5. Log transformed Intraclass Correlation Coefficients (ICC) and Standard Error of Measure (SEM) for repeated PPT measurements per testing site.

	Mean PPT (SD)	**Log transformed ICC**	**SEM (kPa)**
Medial Hamstring	2.71 (0.15)	0.63	0.15
Lateral Hamstring	2.67 (0.16)	0.69	0.16
Tender Point	2.63 (0.15)	0.62	0.15

4.4. Discussion

This study aimed to establish the intrarater reliability of the pressure algometer for measuring hamstring PPT. To the best of our knowledge, the reliability of PPT of the hamstring muscle has not been previously tested. The moderate level ICC values and no significant differences between measurement sessions indicate that PPT of the hamstring muscle can be measured with moderate reliability.

Studies have reported ICC values for PPT reliability within healthy subjects between 0.57-0.91.[159] For pain conditions, such as knee osteoarthritis, ICC values have been reported to be between 0.61-0.91;[159] however, more recent studies have observed excellent reliability, with values between 0.93-0.97.[149] Pressure algometry of the knee in healthy subjects has shown to have good to excellent reliability, with intrarater ICC values between 0.73-0.98 and inter-rater agreement values between 0.73-0.95.[148] Fingleton et al[147] found palpation of lower limb nerves to have good to excellent reliability for the sciatic, femoral and common peroneal nerve (ICC, 0.69-0.90) and moderate to good for the tibial nerve (ICC, 0.56-0.64). Heterogeneity can occur depending on the structural

properties of the respective tissue being measured.[160] Both superficial bony landmarks, such as the knee and nerves are easier to locate and measure PPT. Research has shown that muscle/nerve sites have a lower PPT than pure muscle sites.[161] Sites that require smaller probes effect cutaneous afferents and require lower forces to achieve the PPT, as the perceived pain is usually sharp.[160] Deep somatic tissue, such as muscle, require larger probes and the perceived pain is typically a dull sensation, often requiring a higher force to achieve the PPT.[160] Moreover, conducting consecutive PPT on deep larger muscles requires accuracy, as it can be difficult to locate the exact same position as previous measurements.

To estimate precision between measurement intervals, SEM should be considered with ICC values, as SEM is a measure of absolute reliability.[147,162] Smaller SEM values equate to greater reliability given they represent the measurement error in the units being measured (e.g., kPa). In this study, the SEM values varied between sites (medial, lateral hamstrings and tender points), with medial hamstring values having the highest reliability but lowest precision. It is difficult to compare these results with other studies, as no other study has conducted PPT on hamstring muscles. However, previous lower limb reliability studies reported ICC values between 0.75-0.79 for calf PPT measurements[153] and 0.94 (95% CI, 0.61-0.90) for quadriceps PPT measurements,[163] which are comparable to the results from this study.

This study observed higher PPT values for females compared to males for lateral hamstring and tender point sites and for medial PPT sites, males had higher values; however, the results were not significant. Interestingly, other studies[148,164]observed significantly lower PPT values and higher sensitivity to pressure pain in females when

compared to males. Due to the small sample size and nonsignificant results of this study, conclusions are unable to be drawn regarding sex differences.

4.5. Conclusion

This study found moderate intrarater reliability for measuring hamstring PPTs for the medial and lateral hamstring muscles, as well as for a palpated tender point, in asymptomatic individuals. Only intrarater reliability of pressure algometry was investigated in this study; therefore, inter-rater reliability should be examined in future studies. Additionally, this study aimed to only investigate reliability over one day; therefore, the long-term stability of the measures is unknown. Further larger studies should be conducted to confirm the reliability of pressure algometry of the hamstring muscle in healthy populations.

CHAPTER 5

Efficacy of Myofascial Decompression for Muscle Extensibility: a feasibility study

5.1 Introduction

This study aimed to test the feasibility and acceptability of the intervention, measurement and trial procedures comparing dry cupping techniques for musculoskeletal tissue pain sensitivity and range of motion in a randomised crossover trial design. Dry cupping involves placing cups on the skin, creating a suction on the skin and underlying tissue. Myofascial Decompression (MFD) is a modified cupping technique that combines dry cupping with active movement. Few researchers have investigated these effects or compared MFD to dry cupping methods. This study aimed to test the feasibility of comparing dry cupping techniques using the outcome measures of pressure pain threshold (PPT) and range of motion (ROM).

5.2. Methodology

5.2.1 Study Design

The feasibility study was conducted between October and December 2019 in the Osteopathy Clinic, Victoria University, Melbourne, Australia, and approved by Victoria University Human Research Ethics Committee (VUHREC). The study design was a randomised crossover trial consisting of two experimental interventions and one control intervention, investigating the immediate effects of a single dry cupping session (Figure 5.1). The objectives of the feasibility study were to: [1] test the recruitment capabilities,

including the recruitment rates and eligibility criteria; [2] test the acceptability and suitability of the interventions and study procedures; and [3] test the data collection procedures and outcome measures. Participants completed all three interventions with a one-week washout period between each intervention. Interventions were not blinded; however, participants were blinded to the experimental intervention being investigated. The outcome measures included in this study were PPT and ROM, taken immediately before and after the intervention. All outcome measures were collected by a registered physiotherapist with ten years of experience. The outcome assessor was blinded to treatment allocation.

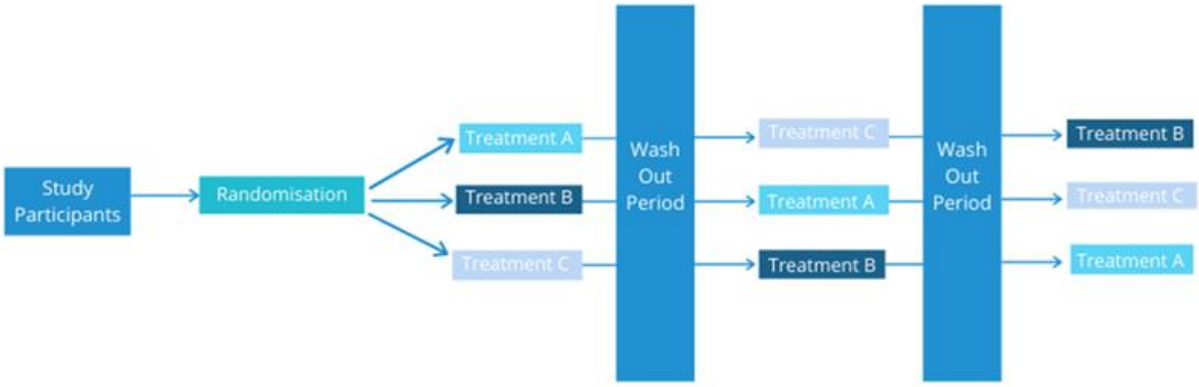

Figure 5.1. Flow chart of crossover study design

5.2.2 Participants

A convenient sample of five healthy participants were recruited from Victoria University to complete all three interventions. The study aimed to recruit twenty participants; however, recruitment was ceased due to the COVID-19 pandemic. Inclusion criteria were male and female participants between the ages of 18-65 with limited hamstring flexibility and reduced ROM as determined by a minimum of 20-degrees loss

of passive knee extension, measured in the supine position with the femur held at 90 degrees of hip flexion. Exclusion criteria included history of lumbar spine or lower extremity pathology, current low back pain, systemic disease, diabetes mellitus, rheumatic disease, and pregnancy. All participants provided written informed consent.

5.2.3 Measures

5.2.3.1 Range of motion

Before baseline outcome measurements were obtained, reflective stickers were placed on the participants key bony landmarks, including the central greater trochanter, lateral femoral condyle, and lateral malleolus. A digital camera (Nikon B500) was set perpendicular to the plane of the treatment table to photograph the participants thigh and reflective stickers when performing ROM. The joint angles were later calculated using Digimizer Version 4.6.1. A digital handheld dynamometer (microFET®2) was used to measure the passive torque applied to the limb during ROM.

5.2.3.2 Pressure pain threshold

PPT were measured using an electronic handheld pressure algometer (Somedic Algometer, Sweden) for all test sites. The algometer consists of a 1cm circular rubber tip that connects to a pressure transducer within the handle of the unit, and the rate of applied pressure is displayed on the output screen. The algometer was calibrated before any testing commenced and an intrarater reliability study was conducted to determine the reliability of this procedure (Chapter 4). The reliability study found moderate intrarater reliability for measuring hamstring PPT. The PPT procedure was based on the methods used by Fryer et al.[155]. The sites for algometry testing were mid muscle belly of the medial

and lateral hamstring muscles, determined by measuring halfway between the ischial tuberosity and popliteal fossa. Before commencing any measurements, a demonstration of PPT was performed at the participant's wrist to explain the testing protocol. The algometer was positioned perpendicular to the body surface and a constant pressure of 30 kPa/second was applied until the participant first started to feel discomfort, tenderness, or pain (Figure 5.2). Once the participant indicated a change in sensation, the applied pressure ceased. For each site, three PPT measurements were taken, with a 20-second break between each reading.

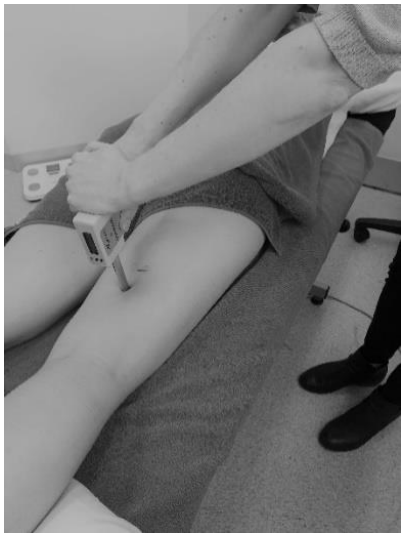

Figure 5.2. PPT measurement

5.2.4. Procedure

When each participant arrived, they were asked to change into shorts that could expose the entire length of the posterior thigh. The protocol was explained, and participants signed a consent form (Appendix H). Participant characteristics, including age, height, and weight were obtained during the first session. To determine participant

eligibility, a 90/90 passive knee extension test was conducted. After the initial screening, participants were instructed to draw a card that contained a randomised sequence of interventions. The participant was positioned in the supine position with the right hip in 90 degrees of flexion, the left leg stayed flat on the plinth (Figure 5.3). The outcome assessor extended the knee until maximal tolerable stretch of the hamstring muscle was achieved and the knee angle were measured.[165] For the participant to be included in the study, a minimum 20-degree loss of knee range needed to be observed. Next, participants commenced their first randomly assigned intervention of either myofascial decompression (MFD), static dry cupping without movement or an active movement protocol (control).

5.2.5 Interventions

For the two cupping interventions, three plastic cups (Hansol professional cupping therapy set, Model CU-30, Hansol Medical, South Korea) of 45 mm size were placed on the hamstring muscle: mid belly of the medial and lateral hamstring muscle and the musculotendinous junction of the hamstring tendon. Initially, three pumps were delivered to the cups with the handheld vacuum device and was adjusted as tolerated. A sorbolene cream (Redwin) was used as a barrier between the cup and skin for the intervention. The interventions were as follow:

a) Myofascial decompression (MFD): this intervention consisted of the cups being applied as above and the participant completed the following active movements: side lying passive hip flexion, knee extension x 10 repetitions, side lying active hip flexion, knee extension x 10 repetitions, standing hip hinge with a towel under the foot x 10 repetitions each side.

b) Static dry cupping: this intervention consisted of the cups being applied as above and the participant resting for 5-minutes.

c) Control: the control group completed the same active movement as MFD but without any cups applied. This intervention controlled for the effect of the active movement alone.

During the intervention the outcome assessor left the room to maintain blinding. Immediately after the intervention, the outcome assessor re-entered the room and completed ROM and PPT measurements as previously described. Participants were asked to attend another session in one week after the washout period and again the week after, to ensure all the interventions were completed.

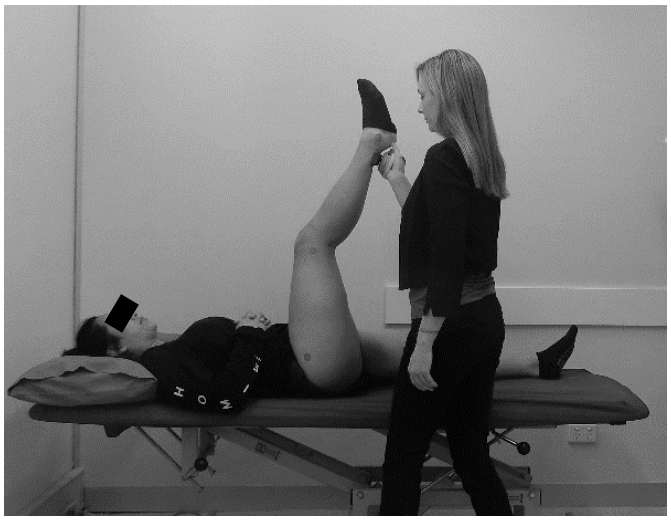

Figure 5.3. Passive knee extension test

5.2.6 Data Analysis

All data was collated in Microsoft Excel and analysed descriptively for baseline characteristics (age, height, weight, and BMI) and outcome measures (ROM and PPT), using means, standard deviations, and effect size (Cohens *d*).

5.3 Results

A total of three males and two females were included in the study (mean age of 30.6 years; range, 20-49 years). Participant characteristics are shown in Table 5.1 and descriptive statistics of the measures, including mean and standard deviation are presented in Table 5.2.

5.3.1 Feasibility

5.3.1.1 Recruitment capabilities, recruitment rates, and eligibility criteria

This study showed that participants were able to be recruited into the randomised crossover trial design and were able to commit to the required intervention sessions. Of the participants that were enrolled, 93% of the intervention sessions were attended. The study intended to recruit twenty participants; however, recruitment was ceased due to the COVID-19 pandemic. Therefore, it is unknown whether full enrolment would be successful during the study duration.

During the recruitment period, the study had sixteen enquiries; however, only five met the inclusion criteria of limited hamstring flexibility and reduced ROM, as determined by a minimum of 20-degrees loss of passive knee extension. Recruitment of participants were from the health sciences department, which may include fewer participants that met the inclusion criteria because this group was young and likely to be

more active than the general population. A larger trial could recruit a more sedentary population, such as office workers that may potentially have limited hamstring flexibility.

5.3.1.2 Acceptability and suitability of the interventions and study procedures

All participants completed the MFD and active movement interventions; however, one participant did not undergo the static cupping intervention. All participants were able to follow the movement protocols successfully. Blinding of the groups was maintained by the outcome assessor leaving the room. Due to the cupping marks that are left on the participants thigh, blinding of a cupping group was unable to be maintained. Future studies could use a tubular bandage over the participant's thigh to improve blinding. There were no serious adverse events in the dry cupping groups. One participant experienced a self-limiting cramp of the adductor muscle during the active movement protocol that relieved after four days.

The crossover design was implemented to utilise the small enrolment into the study, by having all participants complete each intervention. This design has successfully been used in previous cupping studies.[89,89] Changes to the study design to utilise a 2x2 factorial design could be considered with a larger sample. A 2x2 factorial design could enable investigation into two independent variables (movement; dry cupping) and analyse the effects on the dependant variable over time, with the inclusion of a no-treatment group.

5.3.1.3 Data collection procedures and outcome measures

Data was successfully collected from all participants that completed the interventions, including digital photographs that were analysed for ROM using the Digimizer software. The validity of pressure algometry for hamstring PPT was tested in a reliability study prior to this feasibility study. Due to a faulty algometer tip attachment, the 1cm tip was replaced after the first intervention session; it is unknown whether this had any effect on the results.

5.3.2 Outcomes

The MFD group mean PPT for medial hamstring were pre 520.33 kPa (SD ±137.21) and post 570.87 kPa (SD ±157.80), with a mean difference of 50.54 kPa (SD ±66.93), and small effect (*d*= 0.36). The MFD group mean PPT for lateral hamstring were pre 471.60 kPa (SD ±134.12) and post 518.33 kPa (SD ±149.91), with a mean difference of 46.73 kPa (SD ±40.28), and small effect (*d*= 0.34). The MFD group mean ROM were pre 158.20 degrees (SD ±4.10) and post 166.13 degrees (SD ±7.38), with a mean difference of 7.93 degrees (SD ±6.55), and large effect (*d*= 1.13).

The static cupping group mean PPT for medial hamstring were pre 536.42 kPa (SD ± 115.78) and post 489.08 kPa (SD ± 125.90), with a mean difference of 47.33 kPa (SD ±41.92), and small effect (*d*= 0.41). The static cupping group mean PPT for lateral hamstring were pre 466.83 kPa (SD ±78.95) and post 522.58 kPa (SD ±138.38), with a mean difference of 55.75 kPa (SD ±69.02) and medium effect (*d*= 0.51). The static cupping group mean ROM were pre 154.87 degrees (SD ±10.20) and post 163.68 degrees (SD ±5.89), with a mean difference of 8.81 degrees (SD ±6.34), and large effect (*d*= 0.98).

Finally, the active movement group mean PPT for medial hamstring were pre 489.93 kPa (SD ±110.42) and post 515.07 kPa (SD ±137.81), with a mean difference of 25.13 kPa (SD ±59.18) and small effect (d= 0.21). The active movement group mean PPT for lateral hamstring were pre 345.20 kPa (SD ±115.22) and post 370.93 kPa (SD ±120.96), with a mean difference of 25.73 kPa (SD ±68.64) and small effect (d= 0.23). The active movement groups mean ROM were pre 151.50 degrees (SD ±5.65) and post 162.33 degrees (SD ±5.61), with a mean difference of 10.83 degrees (SD ±4.78), and large effect (d= 1.39).

Table 5.1. Participant characteristics

	Male (n=3)	**Female (n=2)**
Age (years)	28.00 ± 2.65	34.50 ± 20.51
Height (cm)	179.00 ± 10.54	164.75 ± 4.60
Weight (kgs)	81.80 ± 22.14	70.20 ± 8.49
BMI	25.23 ± 3.85	25.85 ± 4.45

Data are mean $\pm$ Standard deviation; BMI: body mass index

Table 5.2. Changes in Range of Motion and PPT

		MFD group (n=5)	**Effect (*d*)**	**Static cupping group (n=4)**	**Effect (*d*)**	**Movement only group (n=5)**	**Effect (*d*)**	**Difference (post-pre)**		
								MFD group	Static cupping	Movement only
Medial Hamstring	Pre Post	520.33 (137.21) 570.87 (157.80)	0.36	536.42 (115.78) 489.09 (125.90)	0.41	489.93 (110.42) 515.07 (137.81)	0.21	50.54 (66.93)	47.33 (41.92)	25.13 (59.18)
Lateral Hamstring	Pre Post	471.60 (134.12) 518.33 (149.91)	0.34	466.83 (78.95) 522.58 (138.38)	0.51	345.20 (115.22) 370.93 (120.96)	0.23	46.73 (40.28)	55.75 (69.02)	25.73 (68.64)
Knee angle (°) 90/90	Pre Post	158.20 (4.10) 166.13 (7.38)	1.13	154.87 (10.20) 163.68 (5.89)	0.98	151.50 (5.65) 162.33 (5.61)	1.39	7.93 (6.55)	8.81 (6.34)	10.83 (4.78)

Data are mean (Standard deviation)

5.4 Discussion

This study aimed to test the feasibility and acceptability of the intervention, measurement and trial procedures for a randomised controlled crossover trial comparing dry cupping techniques. This study shows that participants were able to be recruited and were able to return in the required time frame, the outcome measures were successful in obtaining measurement data, and the intervention was able to be applied successfully and was acceptable to all participants (comfort, timeframe, etc). Data was successfully collected from all participants that completed the interventions, including digital photographs that were analysed for ROM using the Digimizer software and pressure algometry for hamstring PPT.

For the ROM and PPT outcome measures, this study observed increases in all groups. Our recent systematic review[166] found dry cupping improved pressure pain sensitivity in asymptomatic participants and was more effective than passive and active stretching, though only low-quality evidence was available. Similarly, the two cupping interventions in this study observed greater increases with PPT when compared to the active movement protocol; however, the results only showed a small to medium effect. In the review, Wood et al[166] no significant differences were found between dry cupping and an active control intervention. Other studies have found dry cupping to be just as effective as passive stretching[88] and self-myofascial release[90] for hamstring ROM. The current study observed increases in all groups for ROM, with the active movement group observing the largest difference (10.8±4.78 degrees), followed by static cupping (8.81±6.34 degrees) and MFD group (7.93±6.55). All groups observed a large effect for ROM.

Given the small participant numbers and that this study was testing feasibility, no interferential statistical analysis was performed, and the treatment results cannot be generalised to the population. This study does demonstrate that the methods are feasible, and we recommend that a larger treatment study be conducted to determine which of the cupping approaches is the most efficacious treatment method.

5.5 Conclusion

This study demonstrated the feasibility of implementing a randomised controlled crossover trial to compare dry cupping techniques. Despite the disruptions to recruitment, the interventions used in this study were successfully able to be applied to participants and all data was collected for the outcome measures. This study observed increases in both PPT and ROM immediately after all intervention; further studies are required to investigate whether using dry cupping with movement is superior to active movement. This study aims to provide evidence to inform future myofascial decompression study designs.

CHAPTER 6

Summary and Conclusions

6.1 Summary of Findings

The intent of this research was to investigate the efficacy and safety of dry cupping techniques. This chapter will discuss the findings that address the underpinning research question of this thesis.

(1) is dry cupping an effective and safe modality in reducing musculoskeletal pain and increasing range of motion?

A systematic review and meta-analysis (Chapter 3) were conducted to assess the effectiveness of western dry cupping methods. To date, there are no systematic reviews specifically examining the use of dry cupping for the treatment of musculoskeletal pain or range of motion; therefore, the aim of the review was to critically evaluate the evidence from randomised controlled trials (RCTs) to determine the efficacy of western dry cupping methods. A significant large effect was observed for dry cupping therapy on pain intensity in chronic neck pain and non-specific low back pain (LBP) when compared to control groups. A significant medium effect was found for dry cupping therapy on neck function when compared to the control group. Despite the significant effect, the quality of evidence to support the use of dry cupping for chronic neck pain and LBP was low-quality, due to high heterogeneity and small sample sizes of the reviewed studies.

The results from this thesis suggest dry cupping may be effective for reducing musculoskeletal pain in patients with chronic neck pain and non-specific low back pain; however, when implementing treatment interventions into clinical practice, the minimal

important difference or change of outcome needs to be considered. This thesis found changes in visual analogue pain scores that exceed the clinically important range for LBP, suggesting that dry cupping treatment is clinically meaningful; however, the results do not consider patient preferences, or a cost-benefit or risk-benefit analysis.

There have been multiple RCTs[69,88-91,93-95] that have investigated the use of dry cupping as a form of myofascial release to increase range of motion (ROM). The trials found dry cupping to be just as effective as passive stretching[88], and self-myofascial release with the use of a foam roller,[90] with both trials reporting no significant differences between the interventions. Dry cupping was found to be more effective than the Mc Kenzie's cervical stretch protocol[89] for increasing cervical ROM, and traditional physical therapy[69] for increasing lumbar range of motion. Despite the significant results reported within each trial, meta-analyses found that dry cupping compared to the active control group did not display significant differences. Dry cupping was found to have a medium effect when compared to no treatment, and the quality of evidence was low.

The safety of dry cupping is an important consideration for practice and was examined through the analysis of reported adverse events in the systematic review, and from a systematic search of all literature on adverse events associated with cupping. Most of the events were mild to moderate and abated within 48-hours. Two serious events were reported but were not associated with the treatment. The most frequently reported events were mild muscular soreness, increased pain, headache, and blister formation. Blister formation and burns have been mostly associated with traditional fire cupping. This thesis found no reports of burns in the included trials of the systematic review that used fire cupping[29,62,68,69,81,88]; however, blister formation was reported.[81] A 2017, case report[137] was published documenting burn injuries associated with fire cupping, warranting caution

with the use of fire cupping. From 2009-16, there were twenty patients that were treated for cupping-related burns, according to the Burns Registry of Australia and New Zealand.[137] Vacuum cupping using a manual handheld pump has far less risk than the use of fire cupping; however, there have been case reports of blister formation associated with strong suction and prolonged treatment duration.[167] It is recommended that manual therapists using cupping techniques are appropriately trained, and additional considerations may be required when using fire cupping.

Definitive conclusions regarding the effectiveness and safety of dry cupping for musculoskeletal pain and ROM were unable to be made due to the low to moderate quality of evidence found in the systematic review.

(2) does myofascial decompression increase range of motion and improve pain pressure sensitivity?

In the past ten years, there have been multiple studies specifically investigating myofascial decompression (MFD).[90-95] MFD is a modified cupping technique that combines dry cupping with active movement. Since most of the MFD studies are published within the grey literature, there are limitations with internal and external validity, including blinding, small and convenient sampling, and lack of long-term follow-up. To address this research question, a feasibility study (Chapter 5) was conducted, that aimed to test the feasibility and acceptability of the intervention, measurement and trial procedures for a randomised controlled crossover trial that compared dry cupping techniques on ROM and pain pressure thresholds (PPT). The interventions investigated were: 1) myofascial decompression; 2) static dry cupping (no movement); and 3) active movement protocol (control intervention), investigating the

immediate effects after a single dry cupping session. Five healthy participants were recruited from the university and completed all three interventions, with a one-week washout period in between each intervention. The outcome measures included in this study were ROM and PPT, taken immediately before and after the intervention. This study observed increases in all groups for ROM and PPT. The results from the systematic review found that dry cupping improved PPT in asymptomatic participants and was more effective than passive and active stretching, though only low-quality evidence was available.

6.2 Limitations

There were several limitations that should be recognised in this study, so that further research can benefit. Firstly, the inability to recruit enough participants that met the inclusion criteria for the feasibility study. The study required a minimum of 20-degrees loss of passive knee extension as an inclusion criterion, in order to measure a clinically meaningful difference in ROM after the intervention. Recruitment of participants were from the health sciences department, which may have limited the number of eligible participants. A larger trial could recruit a more sedentary population, such as office workers that may potentially have limited hamstring flexibility. A second round of recruitment was planned for the feasibility study to meet the planned sample size of 20 participants; however, due to the COVID-19 pandemic, recruitment was ceased.

Secondly, blinding was a limitation and is a frequently reported limitation among manual therapy interventions, especially with an intervention such as dry cupping where marks are left on the skin. For the feasibility study conducted in Chapter 5, the outcome assessor left the room during the procedure to maintain blinding of the groups; however,

due to the cupping marks that are left on the participants thigh, blinding of a cupping group was unable to be maintained. Future studies could use a tubular bandage over all participant's thighs to maintain blinding. A similar method was implemented by AlKadhrawi et al[86], whereby a tape covering was applied to every participant to maintain blinding of the outcome assessor to group allocation. Sham cupping has been utilised in two trials[85,94]; albeit the reliability of the device is questionable. The reliability of the sham device was tested and validated in a pilot study;[138] however, studies that have since applied the device have observed a lack of success.[85]

6.3 Summary of Recommendations

Findings from the feasibility study suggest that dry cupping improve both PPT and ROM; however, it is unknown whether using dry cupping with movement is superior to active movement only; therefore, this could be investigated with future studies. The feasibility protocol was successful in recruiting participants, applying the interventions, and obtaining measurements. A future RCT with a large sample size could conduct inferential statistics to investigate whether dry cupping with movement is superior to active movement only.

For definitive conclusions regarding the effectiveness and safety of dry cupping for musculoskeletal pain and ROM to be made, further high-quality trials with larger sample sizes, long-term follow up, and reporting of adverse events is required. Additionally, future studies could determine whether dry cupping treatment is clinically meaningful by investigating participant preferences for the treatment, analysing the cost-benefit or risk-benefit.

6.4 Conclusion

This thesis examined the evidence of western dry cupping methods in the treatment of musculoskeletal pain and improving ROM, in addition to providing comparative evidence of dry cupping techniques, such as MFD. The results from the systematic review suggest that dry cupping may be effective in reducing pain and improving functional status in patients with chronic neck pain when compared with no intervention. A significant reduction in pain for non-specific low back pain was observed, although the quality of evidence was found to be moderate to low. Dry cupping was found to be just as effective as other modalities used for increasing ROM. The review found no serious adverse events associated with cupping; however, there are additional consideration required when using fire cupping methods, due to the risk of burns and blister formation. The results from the feasibility study suggest that MFD improves both ROM and pain pressure sensitivity immediately after the intervention; however, it is unclear whether these results are clinically meaningful or whether MFD is superior to static dry cupping or active movement only.

The research within this thesis provides a critical review of the available evidence for the use of dry cupping therapy for musculoskeletal conditions. For definitive conclusions on the efficacy of dry cupping, further systematic reviews and meta-analyses are required as larger dry cupping trials are published. Furthermore, this thesis provides a basis for future research to be undertaken, specifically investigating the efficacy of MFD for the treatment of musculoskeletal pain and improving ROM.

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APPENDICES

APPENDIX A: Systematic Review Search Strategy (Chapter 3)

1. musculoskeletal pain
2. musculoskeletal diseases
3. muscular disease
4. myopathy
5. myopathies
6. myopathic condition
7. muscle disorder
8. myalgia
9. myofascial pain
10. myofascial pain syndrome
11. pain
12. acute pain
13. chronic pain
14. pain management
15. or/1-14
16. dry cupping
17. myofascial cupping
18. myofascial decompression
19. negative pressure cupping
20. cupping therapy
21. or/16-20
22. 15 and 21
23. range of motion
24. flexibility
25. moblity
26. movement
27. Visual Analog Scale
28. Numeric Rating Scale
29. McGill Pain questionnaire
30. brief pain inventory
31. Pain Pressure Threshold
32. pressure algometry
33. pain measurement
34. pain threshold
35. quality of life
36. adverse events
37. adverse reactions
38. safety
39. adverse healthcare event
40. health care errors
41. or/23-40
42. 23 and 42

APPENDIX B. PUBMED Search Strategy (Chapter 3)

1. musculoskeletal pain (MeSH)
2. musculoskeletal diseases (MeSH)
3. muscular disease (MeSH)
4. myopathy (KW)
5. myopathies (KW)
6. myopathic condition (KW)
7. muscle disorder (KW)
8. myalgia (KW)
9. myofascial pain (KW)
10. myofascial pain syndrome (KW)
11. pain (KW)
12. acute pain (KW)
13. chronic pain (KW)
14. pain management (KW)
15. or/1-14
16. dry cupping (KW)
17. myofascial cupping (KW)
18. myofascial decompression (KW)
19. negative pressure cupping (KW)
20. cupping therapy (KW)
21. or/16-20
22. 15 and 21
23. range of motion (MeSH)
24. flexibility (KW)
25. moblity (KW)
26. movement (KW)
27. Visual Analog Scale (MeSH)
28. Numeric Rating Scale (KW)
29. McGill Pain questionnaire (KW)
30. brief pain inventory (KW)
31. Pain Pressure Threshold (KW)
32. pressure algometry (KW)
33. pain measurement (KW)
34. pain threshold
35. quality of Life (MeSH)
36. adverse events (KW)
37. adverse reactions (KW)
38. safety (KW)
39. adverse healthcare event
40. health care errors
41. or/23-40
42. 23 and 42

APPENDIX C: Downs & Black Assessment (Chapter 3)

Dry Cupping for Musculoskeletal Conditions (n=21)					
Akbarzadeh et al. 2012					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		"This study aimed to investigate the effect of dry cupping therapy at BL23 point on the intensity of low back pain in primiparous women." – pg. 112
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes are described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		An inclusion criterion was outlined on pg. 14-115. An exclusion criterion was not described
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Confounders described – no significance between groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		Written and outlined in Table 2
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Normally distributed data with SD reported but does not reported confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			Adverse events were not addressed
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		No drop-outs
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	

11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			Cupping is not representative of an intervention majority of patients would receive
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			No mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			Not mentioned
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		No retrospective unplanned analysis – all outcomes reported are listed in methods
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		

Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		All participants recruited from Hafez hospital - District 1, Shiraz, Fars Province, Iran
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		"Randomization was performed using the table of random numbers" – pg. 114
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			No mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		None lost to follow-up
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		"According to the statistical consultation and using the sample size formula n= [(σ12+σ22)(zα/2+zβ)2]/D2 a 100-subject sample size (50 subjects in each group) was selected for the study." – pg. 114. There was adequate power to detect a difference pre/post treatment
Total Score: 20/28					

Arslan et al. 2015					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		Clearly describes the aims of the study is to investigate the possible effects of dry cupping therapy on the degree of pain in office workers

2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes are described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.	0			"Inclusion criterion of the study was clinically diagnosed minimum 3 neck pain" – no mention of what a minimum 3 neck pain is or how it was clinically diagnosed
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.	0			No mention of what the control participants did
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			No description or list of principal confounders - only a comment in results that there was no statistical significance in age and hours worked
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).	0			Mean scores were not reported for control group. Only study group
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Standard deviations were only reported for the intervention group. Does not reported confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			Adverse events were not addressed
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			Study does not report losses or drop outs
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was	0			

	representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.				
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			No mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		Trial didn't include a follow-up. Participants finished at the same time.
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		

23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			States that participants were 'divided' into two groups but doesn't describe randomisation process.
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			No mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.	0			Not mentioned
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			No statistical analysis to justify sample size – unlikely to have adequate power to detect a difference
Total Score: 11/28					

Barger 2016					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim of the study is to examine the effects of a single session of GT or MFD therapy on hamstring flexibility in patients with perceived hamstring tightness.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		Yes – all outcomes are described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.	0			The only inclusion criteria were perceived hamstring tightness, pain, lack of flexibility and/or decreased strength
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			Principle confounders were described together – there was no comparison between the groups

6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Normally distributed data with SD reported but does not reported confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			Adverse events were not addressed
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			No mention of loss to follow-up
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			No mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on “data dredging”, was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		No unplanned retrospective analysis undertaken

17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			Unable to determine – not enough information of participant positioning to ensure accuracy
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		All participants were male collegiate athletes from Oklahoma State University
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Unable to determine – the study states that subjects were randomly assigned but not how
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			No mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		No mention of loss to follow-up
Power		**0**	**1**	**2**	

27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			No statistical analysis to justify sample size – unlikely to have adequate power to detect a difference
Total Score: 13/28					

Biehl 2017					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim is to determine if dry cupping is an effective treatment intervention in releasing ITB tightness and increasing hip and knee range of motion in a physically active population.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		Main outcomes are described clearly in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		An inclusion/exclusion was outlined on page 16
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			Principle confounders were described together – there was no comparison between the groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data - SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			Adverse events were not addressed
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			No mention of loss to follow up

10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.		1		Page 1 states there was clinician and participant blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?		1		
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		The study was a single session with a 24-hour follow up for all participants
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		

20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			Obers test is not a reliable outcome measure for ITB specifically
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.	0			Doesn't explicitly state where the participants were recruited from – if they were all athletes, staff or students.
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Unable to determine. A randomisation system was used to randomise participants – pg. 10. Doesn't state what system was used.
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			No mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.	0			No mention of loss to follow-up
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			The study observed strong effect sizes; however, the study did not provide sample size justification. The small sample size was unlikely to have adequate power to detect a difference
Total Score: 15/28					

Chi et al. 2016					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states that the aim to investigate the effectiveness of cupping therapy in changes on skin surface temperature for relieving chronic neck and shoulder pain

2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		A detailed inclusion/exclusion was described on page 2
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Listed in Table 1
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Non-parametric tests used for non-normally distributed data, does not report interquartile ranges
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All adverse events recorded
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if,	0			

	for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.				
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Allocation was blinded but there was no mention of intervention blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			Unable to be determined
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		The study was a single session
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		Participants were recruited in Hualien City, Taiwan, via advertising and e-mail – pg. 2
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		Participants were recruited from October 2012 to February 2013
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Sequence coded random selection – pg. 2
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		Concealed with sealed envelopes – pg. 2

25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		The study included a power analysis "Employing the Wilcoxon Mann-Whitney test (G power v 3.1.3) to achieve a power of 0.8, with Cronbach's α value = 0.05 and an effect size of 0.80, the required size for each group is minimum of 27 subjects." – pg. 2
Total Score: 22/28					

Cramer et al. 2011					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim was to investigate the effect of pneumatic pulsation therapy on chronic neck pain compared to standard medical care.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		An extensive inclusion/exclusion criteria was described
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard		1		Normally distributed data, SD and confidence intervals reported

	error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.				
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All minor adverse events were recorded
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		Participants lost to follow-up were adequately reported
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.		1		
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			The study was a randomised unblinded clinical trial
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on “data dredging”, was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		No retrospective unplanned analysis reported
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		

18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.	0			Recruited by press release but unable to determine where the recruitment advertisement was released.
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		Recruitment period was Aug 2009 – July 2010
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Software was used to create random numbers.
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		"Randomization was carried out by means of sequentially numbered, sealed opaque envelopes, prepared by the study coordinator, who was neither involved in treatment nor evaluation."
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		Lost to follow-up was reported
Power		**0**	**1**	**2**	

27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		Power analysis was completed "The study was powered to detect an effect size of the primary outcome measure of 0.87, which was estimated based on the findings of a pilot study on dry cupping in chronic neck pain. To detect this effect with 80% power and a 2-sided α of 0.05, a sample of 44 patients was needed. To account for possible dropouts, a sample of 50 patients was chosen."
Total Score: 24/28					
Doozan 2015					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states the aim is to determine the ability of cupping to act as a myofascial release technique to increase range of motion of the iliotibial band.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		Main outcomes are clearly described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			The study did not provide principle confounders for each group separately.
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).	0			
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Normally distributed data, SD reported, does not report confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			No mention of adverse events

9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?	0			Actual probability values only described for some interactions, not all
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine – no mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		No retrospective unplanned subgroup analysis reported
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		

19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.	0			
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Subjects were randomised but doesn't state how randomisation occurred
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			No mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.	0			There were three withdrawals but there is no information stating how many participants data was analysed and reported in the results
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.	0			No mention of when subjects withdrew (only states during the study) - pg. 9
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			No
Total Score: 10/28					

l Rahim et al. 2017					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states the aim is to investigate the effect of cupping

					therapy with inferential therapy on mechanical low back pain.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes are described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Detailed list of principle confounders described with no significance between the groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Normally distributed data with SD reported but does not reported confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		The study stated that no adverse events occurred
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			No mention of lost to follow-ups
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			

13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			No mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			No mention of concealment
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes	0			Unable to determine. No clear outline of planned statistical analysis in methods, first described in results section
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		

23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Randomisation occurred but doesn't state how
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			No mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		
Total Score: 19/28					

Ge et al. 2017					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		Clearly states the purpose of the study was to determine the effects of dry cupping on pain and function of patients with plantar fasciitis
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.		1		No significance between groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		

7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – standard deviations and confidence intervals are reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			No mention of adverse events
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			No mention of lost to follow-up
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			Convenient sample from the university
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine – no mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		No retrospective unplanned analysis reported
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up		1		

	were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.				
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		Outcome measures are all reliable and valid
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		Convenient sample from the university
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.	0			
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			States that the participants were randomised but doesn't state how
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			Unable to determine – no mention of concealment
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.	0			
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		A power analysis was completed using G*Power 3.1 (version 3.1.9.2) with the

					following parameters: effective size of 0.5, alpha of 0.05, and power of 0.8
Total Score: 16/28					

Khan et al. 2013					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim is to evaluate the effect of cupping therapy at a clinical setting for knee osteoarthritis
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes are clearly described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		An inclusion/exclusion criterion is described
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	No significance between groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Normally distributed data – reports SEM but does not report confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All adverse events are reported
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-up addressed
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample	0			The broad inclusion age range of 30-60 is not entirely representative of adults diagnosed with knee OA

	of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.				
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to be determined - No mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.	0			Unable to determine – the study states two participants withdrew as the treatment was too expensive
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			Unable to determine - No references for outcome scale used. There are other reliable and validated outcome measures for KOA
Internal validity - confounding (selection bias)		**0**	**1**	**2**	

21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Unable to determine - States randomisation occurred but doesn't explain methods used
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.	0			Unable to determine - No intent-to-treat analysis was undertaken to account for drop-outs
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		Participants lost to follow-up were addressed and described
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			
Total Score: 16/28					

Kim et al. 2017					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim is to measure and compare the change in flexibility, muscle activity, and pain threshold in hamstring muscle with application of cupping therapy and static stretching
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		Outcomes are described in the results
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		Inclusion/exclusion criterion is provided

4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			Unable to determine – Unclear. Subject numbers from Table. 1 doesn't match description
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.	0			Normally distributed data with SD reported but does not reported confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			No mention of adverse events
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	

14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			No blinding of participants
15	Was an attempt made to blind those measuring the main outcomes of the intervention?		1		Testers were blinded
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		Convenient sample of students from the University of Seoul
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Participants were randomised by coin toss
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups		1		

	was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.				
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			
Total Score: 18/28					

Lacross 2011					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim is to examine effectiveness of two different hamstring soft tissue treatments; myofascial decompression and a moist heat pack with self-myofascial release using a foam roller.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The outcomes are clearly described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.	0			No exclusion criterion
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			Principle confounders were not clearly described for each group
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			Unable to determine – adverse events were not mentioned

9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine – no mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		

19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		The intervention was a single session
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Participants were randomised by the means of a coin toss
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			Unable to determine – concealment was not mentioned
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			
Total Score: 17/28					

Lauche et al. 2011					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim was to determine whether a series of

					cupping treatments effectively relieves chronic non-specific neck pain
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes are clearly described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		A detailed inclusion/exclusion criterion was described
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Principle confounders were clearly described for both groups – no statistical significance between groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All adverse events addressed
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-ups addressed
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.		1		Depicted in Figure. 2

13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine – no mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			Assessors were not blinded
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		All patients were recruited by notices printed in their local newspapers.
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		Recruitment was from July – November 2009

23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Sequential numbered envelopes
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		Sealed opaque envelopes
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		All lost to follow up was taken into account
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			
Total Score: 24/28					

Lauche et al. 2013					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aim was to test the efficacy of 12 weeks of a partner-delivered home-based cupping massage, compared to the same period of progressive muscle relaxation in patients with chronic non-specific neck pain
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All main outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		A detailed inclusion/exclusion criterion was given
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.	0			Unable to determine - The intervention wasn't clearly described, there was no inclusion of where the cupping massage was going to be applied

5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Principle confounders were clearly described for both groups – no statistical significance between groups
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All adverse events were reported
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-up has been described
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.		1		Outlined in Figure. 1
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine

	Was an attempt made to blind those measuring the main outcomes of the intervention?		1		"During the active treatment phase, only the trial coordinator had contact with patients and knew of their group allocation. The trial coordinator was not involved in patients' outcome assessments and the outcome assessor remained blind to patients' group allocation throughout." – pg. 2
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.	0			Unable to determine - Both groups participated in a 12-week home program – compliance could be an issue when unsupervised
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			The outcome for pain on motion had not been validated at the time of the study
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		Recruitment occurred December 2011 – May 2012
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Patients were randomly assigned to one treatment group using a non-stratified block-randomization approach with

					randomly varying block lengths. The ‘‘ranuni’’ random number generator of the SAS/STAT H software (SAS Inc., Cary NC, US) was used to generate random numbers. – pg. 2
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		Sequentially numbered sealed envelopes containing patients’ treatment assignments were prepared by a statistician who was not involved in conducting the study. Following each baseline assessment, the trial coordinator opened the next lowest numbered envelope to reveal patient’s treatment assignment. – pg. 2
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		All lost to follow-up were addessed
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		Power analysis was completed “A previous study of cupping massage for chronic non-specific neck pain led current researchers to expect a statistically significant between group difference of 214.3 mm (Cohen’s d=0.66) on the VAS. Given an effect size of d=0.66, and a two-sided level 5% t-test, 76 patients would be needed to detect such a group difference with a statistical power of 80%.” – pg. 3
Total Score: 23/28					

Lauche et al. 2016					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states the aim was to investigate the efficacy of cupping

					therapy compared to usual care and a sham procedure to improve symptoms and quality of life in patients diagnosed with the fibromyalgia syndrome.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All outcomes were clearly described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		A detailed inclusion/exclusion criterion was described
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	A detailed table and description of principle confounders was described
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All adverse events were described
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-up were reported
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was		1		

	representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.				
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.		1		The study was double-blinded
15	Was an attempt made to blind those measuring the main outcomes of the intervention?		1		Outcome assessors were blinded to the patients' group allocation (all groups) at outcome assessment.
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.	0			Unable to determine – no mention of who administered the intervention
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		Local newspaper advertisement

22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		"Patients were allocated to one of three groups in sequential order adopting a computer-generated (Random Allocation Software, version 1.0.0) stratified block randomization with randomly varying block sizes." – pg. 2
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		Opaque envelopes
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		All lost to follow-up are reported
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		Power analysis completed "Given the effect size of Cohen's d = 0.615, and a two-sided 5% level t-test with a statistical power of 1-β = 80%, 43 patients would be needed to detect this group difference. We planned to include 141 patients in this trial (n = 47 per group); recognizing a potential loss of analytical power due to patient withdrawal from 10%." – pg. 3
Total Score: 26/28					

Saha et al. 2017					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states the aim was to test the efficacy of 5 cupping massage treatments, compared to a wait list control group in patients with chronic non-specific neck pain

2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All the main outcomes are clearly described in the methods
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		A detailed inclusion/exclusion criterion was described
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	A detailed table and description of principle confounders was described
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		All adverse events were described
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-up were reported
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.		1		
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the	0			

	intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.				
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine – doesn't state clearly if there was an attempt to blind
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		Newspaper and website advertising
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		Non-stratified randomisation approach. The randomisation sequence was generated using the random number

					function of Microsoft Excel software – pg. 2
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		Sequentially numbered sealed envelopes containing the patients' treatment assignments were prepared by a research coordinator who was not otherwise involved in the study. After inclusion into the study, the research fellow opened the next lowest numbered envelope to reveal the patient's treatment assignment – pg. 2
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		All lost to follow-up were taken into account and reported
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		Power analysis completed "When this study was planned, no data on the efficacy of cupping massage for chronic neck pain were available. As a consequence, a convenient sample of N = 50 participants was planned. This sample size was considered to be sufficient to detect a group difference of Cohen's d = 0.8 given a 2-sided level 5% t-test and a statistical power of 80% even after a loss of 20% of participants during the trial." – pg. 3
Total Score: 25/28					

Singh et al. 2016					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?	0			Unable to determine - The aim is not clearly described in abstract nor the intro or methods

2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.	0			
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		An inclusion/exclusion was given
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.		1		
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).	0			Unable to determine - unclear
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Non-normally distributed data - Reports interquartile ranges but doesn't describe it
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			12 participants were lost to follow-up, but the characteristics were not described
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?	0			Actual probability values not reported
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if,	0			

	for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.				
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine - unclear
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			Unable to determine - unclear
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes	0			Unable to determine
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.	0			
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.	0			Non-compliance could be a possibility with the control group with self-managing medication
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			The outcome measure is not validated
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.	0			Unable to determine - unspecified
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Random allocation occurred but the process was not described
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			Unable to determine – not mentioned

25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.	0			
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.	0			Lost to follow-up was not taken into account
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			
Total Score: 8/28					

Smith 2015					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states the purpose was to understand the immediate effects of myofascial decompression therapy (MFD) has on range of motion (ROM) strength of internal (IROT) and external rotation (EROT) of the shoulder, and to explore MFD as an effective treatment for overhead athletes
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		The main outcomes were clearly described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.	0			No inclusion criteria given. Only an exclusion criteria
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.	0			
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.	0			Principle confounders were not provided for each group
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard	0			Normally distributed data – reports SD but does not report confidence intervals

	error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.				
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			No adverse events were mentioned
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			Unable to determine – no mention of blinding
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on “data dredging”, was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		

18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			Unable to determine –no mention of where the dynonamometer was placed when taking the measurement and no mention of goniometer positioning
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.	0			Unable to determine – a convenient sample was used but doesn't state where the sample was recruited from
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Unable to determine – no mention of how randomisation occurred
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			

Total Score: 12/28

Teut et al. 2012					**Notes/Justification**
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly described the aim was to investigate the effectiveness of pulsatile cupping in relieving pain and stiffness and improving quality of life in patients with osteoarthritis of the knee compared to no intervention
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All main outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		Detailed inclusion/exclusion criterion provided
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Principle confounders for each group were described – no statistical significance between the groups were observed
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-up characteristics were described
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	

11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.		1		
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.		1		
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.	0			No – the study states "…due to the nature of the intervention, a blinding of patients and study physicians was not possible."
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		

20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		Participants were recruited from Berlin newspapers
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		January – July 2010
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		"Patients were allocated to treatments groups by simple randomization with a 1:1 ratio via a central telephone randomization process. The random allocation sequence was generated by our statistician using SAS 9.2 software (SAS Institute Inc. Cary, NC, USA)." – pg. 2
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		An independent study nurse on the telephone line centrally assigned patients to intervention or control according to the randomization list, allocation was concealed. – pg. 2
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			No power analysis of sample size calculation was conducted
Total Score: 24/28					

Teut et al. 2018					Notes/Justification
Reporting		**0**	**1**	**2**	
1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly states the aim was to investigate the effectiveness of dry pulsatile cupping in reducing pain and improving back function and quality of life in patients with nonspecific cLBP
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All main outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		Detailed inclusion/exclusion criterion provided
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.			2	Principle confounders for each group were described – no statistical significance between the groups were observed
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – SD and confidence intervals reported
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).		1		
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.		1		All lost to follow-up characteristics were described
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of		1		

	the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.				
12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.		1		
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.		1		"Patients of both cupping groups were blinded to their study intervention." Control group wasn't blinded – pg. 3, 8
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			Unable to determine – not mentioned
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.		1		
Internal validity - confounding (selection bias)		**0**	**1**	**2**	
21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups		1		

	should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.				
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.		1		
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.		1		"The randomization sequence was generated by SAS 9.2 Software (SAS Institute Inc. Cary. NC, USA)" – pg. 3
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?		1		
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.		1		
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.		1		
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.		1		A power analysis was completed "The sample size calculation was performed for the primary comparison between the cupping and the control group. An adjusted difference of 15 mm on the VAS after 4 weeks with a common standard deviation of 20 mm, given a significance level of $\alpha = 0.05$, was assumed for a two-sided t-test. Based on these assumptions and a power of 85%, 33 patients per group were needed. To compensate for drop-outs, a total of 36 patients per group were included and randomized. Sample size calculation was done with nQuery Advisor 6.02."
Total Score: 26/28					

Yim et al. 2017				**Notes/Justification**
Reporting	**0**	**1**	**2**	

1	Is the hypothesis/aim/objective of the study clearly described?		1		The study clearly describes the aims were to investigate the differences in the angle of the cervical spine and the pain thresholds around the cervical vertebrae by applying the McKenzie exercise and the cupping therapy.
2	Are the main outcomes to be measured clearly described in the Introduction or Methods section? If the main outcomes are first mentioned in the Results section, the question should be answered no.		1		All main outcomes are described in the methods section
3	Are the characteristics of the patients included in the study clearly described? In cohort studies and trials, inclusion and/or exclusion criteria should be given. In case-control studies, a case-definition and the source for controls should be given.		1		Detailed inclusion/exclusion criterion provided
4	Are the interventions of interest clearly described? Treatments and placebo (where relevant) that are to be compared should be clearly described.		1		
5	Are the distributions of principal confounders in each group of subjects to be compared clearly described? A list of principal confounders is provided.		1		Principle confounders for each group were described
6	Are the main findings of the study clearly described? Simple outcome data (including denominators and numerators) should be reported for all major findings so that the reader can check the major analyses and conclusions. (This question does not cover statistical tests which are considered below).		1		
7	Does the study provide estimates of the random variability in the data for the main outcomes? In non-normally distributed data the interquartile range of results should be reported. In normally distributed data the standard error, standard deviation or confidence intervals should be reported. If the distribution of the data is not described, it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		Normally distributed data – reports SD but does not report confidence intervals
8	Have all important adverse events that may be a consequence of the intervention been reported? This should be answered yes if the study demonstrates that there was a comprehensive attempt to measure adverse events. (A list of possible adverse events is provided).	0			Unable to determine – no mention of adverse events
9	Have the characteristics of patients lost to follow-up been described? This should be answered yes where there were no losses to follow-up or where losses to follow-up were so small that findings would be unaffected by their inclusion. This should be answered no where a study does not report the number of patients lost to follow-up.	0			All lost to follow-up characteristics were not mentioned
10	Have actual probability values been reported (e.g. 0.035 rather than <0.05) for the main outcomes except where the probability value is less than 0.001?		1		
External Validity		**0**	**1**	**2**	
11	Were the subjects asked to participate in the study representative of the entire population from which they were recruited? The study must identify the source population for patients and describe how the patients were selected. Patients would be representative if they comprised the entire source population, an unselected sample of consecutive patients, or a random sample. Random sampling is only feasible where a list of all members of the relevant population exists. Where a study does not report the proportion of the source population from which the patients are derived, the question should be answered as unable to determine.	0			

12	Were those subjects who were prepared to participate representative of the entire population from which they were recruited? The proportion of those asked who agreed should be stated. Validation that the sample was representative would include demonstrating that the distribution of the main confounding factors was the same in the study sample and the source population.	0			
13	Were the staff, places, and facilities where the patients were treated, representative of the treatment the majority of patients receive? For the question to be answered yes the study should demonstrate that the intervention was representative of that in use in the source population. The question should be answered no if, for example, the intervention was undertaken in a specialist centre unrepresentative of the hospitals most of the source population would attend.	0			
Internal Validity - bias		**0**	**1**	**2**	
14	Was an attempt made to blind study subjects to the intervention they have received? For studies where the patients would have no way of knowing which intervention they received, this should be answered yes.		1		"In order to minimize the learning effect of the subjects, the single blind method was applied which does not allow the subjects to be aware of the purpose of the study" – pg. 84
15	Was an attempt made to blind those measuring the main outcomes of the intervention?	0			Unable to determine – not mentioned
16	If any of the results of the study were based on "data dredging", was this made clear? Any analyses that had not been planned at the outset of the study should be clearly indicated. If no retrospective unplanned subgroup analyses were reported, then answer yes		1		
17	In trials and cohort studies, do the analyses adjust for different lengths of follow-up of patients, or in case-control studies, is the time period between the intervention and outcome the same for cases and controls? Where follow-up was the same for all study patients the answer should be yes. If different lengths of follow-up were adjusted for by, for example, survival analysis the answer should be yes. Studies where differences in follow-up are ignored should be answered no.		1		
18	Were the statistical tests used to assess the main outcomes appropriate? The statistical techniques used must be appropriate to the data. For example nonparametric methods should be used for small sample sizes. Where little statistical analysis has been undertaken but where there is no evidence of bias, the question should be answered yes. If the distribution of the data (normal or not) is not described it must be assumed that the estimates used were appropriate and the question should be answered yes.		1		
19	Was compliance with the intervention/s reliable? Where there was noncompliance with the allocated treatment or where there was contamination of one group, the question should be answered no. For studies where the effect of any misclassification was likely to bias any association to the null, the question should be answered yes.		1		
20	Were the main outcome measures used accurate (valid and reliable)? For studies where the outcome measures are clearly described, the question should be answered yes. For studies which refer to other work or that demonstrates the outcome measures are accurate, the question should be answered as yes.	0			Smart phone goniometer has not been validated as an reliable outcome measure
Internal validity - confounding (selection bias)		**0**	**1**	**2**	

21	Were the patients in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited from the same population? For example, patients for all comparison groups should be selected from the same hospital. The question should be answered unable to determine for cohort and case-control studies where there is no information concerning the source of patients included in the study.		1		All recruited from the Sahmyook University
22	Were study subjects in different intervention groups (trials and cohort studies) or were the cases and controls (case-control studies) recruited over the same period of time? For a study which does not specify the time period over which patients were recruited, the question should be answered as unable to determine.	0			No reference date for recruitment
23	Were study subjects randomized to intervention groups? Studies which state that subjects were randomized should be answered yes except where method of randomization would not ensure random allocation. For example alternate allocation would score no because it is predictable.	0			Unable to be determined – not mentioned
24	Was the randomized intervention assignment concealed from both patients and health care staff until recruitment was complete and irrevocable?	0			Unable to be determined – not mentioned
25	Was there adequate adjustment for confounding in the analyses from which the main findings were drawn? This question should be answered no for trials if: the main conclusions of the study were based on analyses of treatment rather than intention to treat; the distribution of known confounders in the different treatment groups was not described; or the distribution of known confounders differed between the treatment groups but was not taken into account in the analyses.	0			Unable to be determined
26	Were losses of patients to follow-up taken into account? If the numbers of patients lost to follow-up are not reported, the question should be answered as unable to determine. If the proportion lost to follow-up was too small to affect the main findings, the question should be answered yes.	0			Lost to follow-up was not mentioned
Power		**0**	**1**	**2**	
27	Did the study have sufficient power to detect a clinically important effect where the probability value for a difference being due to chance is less than 5%? Sample sizes have been calculated to detect a difference of x% and y%.	0			
Total Score: 14/28					

APPENDIX D: Information to Participant Form (Chapter 4)

INFORMATION TO PARTICIPANTS INVOLVED IN RESEARCH

VICTORIA UNIVERSITY
MELBOURNE AUSTRALIA

You are invited to participate

You are invited to participate in a research project entitled:

'Reliability and Validity of Pressure Algometry for Measuring Pressure Pain Thresholds of the Hamstring Muscle'

A pilot reliability study for validating the use of pressure algometry for measuring pressure pain thresholds of the hamstring muscle

This project is being conducted by research student Sarah Wood BHSc (MST), supervisor Associate Professor Gary Fryer PhD, BSc (Osteo), College of Health & Biomedicine, Victoria University, Dr Karen Lucas PhD, and Caroline Cleary BHSc (MST), MPhySt (Physio), MSportsPhysio

Project explanation

This study aims to validate the intra-rater reliability of pressure algometry for measuring pressure pain thresholds of the hamstring muscle.

Am I eligible to participate in this study?

You are eligible to be included in the study if you are between the ages of 18-65 years with no major health concerns. You may not be eligible to participate if you currently have the following (please tick as appropriate; the researcher will discuss these with you):

Condition	No	Yes
Current lower back pain		
Numbness, tingling or altered sensation in the legs		
Any condition or disease of the lumbar spine (e.g., disc prolapse, spondylolisthesis, degeneration, tumour)		
Systemic disease or active oncologic disease		
Insulin-dependent diabetes mellitus		
Haemorrhagic disease		
Currently taking blood thinning medication (Warfarin)		
Inflammatory rheumatic disease		
Currently pregnant		

What will I be asked to do?

If you decide to take part in this study, you will be invited to attend three measurement sessions occurring 30 minutes apart. During the session, we will ask to conduct pressure algometry measurements at several locations on the hamstring muscle. The handheld algometer consists of a 1cm circular rubber tip that connects to the device and the researcher will apply a constant rate of pressure onto the hamstring muscle. We will ask you to verbalise the word 'now' when you start to feel discomfort, tenderness or pain. The researcher will perform a demonstration on your forearm to explain the process before any testing commences. You will be asked to attend two more measurement sessions, at least 30 minutes apart and the same measurement procedure will be applied. Your total time commitment will be less than 2 hours.

You will be requested to:

- Complete an informed consent form.
- Answer questions regarding any previous history of back or hip conditions (see above).
- Bring some tight-fitting shorts to wear during intervention (a changing room will be provided).
- The measurements will be performed as you lie on your stomach.

What are the interventions?

The intervention consists of the researcher taking pressure pain threshold measurements using a handheld pressure algometer.

If at any stage of the study you feel any pain, sensations of discomfort or symptoms, please inform the researchers immediately.

What will I gain from participating?

There are no direct benefits to you, but your participation will allow for further knowledge regarding the intra-rater reliability of pressure algometry to the hamstring muscle. This outcome measure will be used in a future research project.

How will the information I give be used?

The personal information that you provide researchers will remain strictly confidential. The researcher affiliated with the study will be the only person to have access to your private information and the data obtained during the study, which will be stored at Victoria University. You will be provided with your own personal data for your interest. Further, any reporting of data (such as thesis publication, conferences or seminars) will be done so in a summarised form without personal information that may identify you. Other participants in the study will not have access to the results that you have received throughout the study.

What are the potential risks of participating in this project?

There a minor risk associated with this project, that may include discomfort, tenderness or pain. We will ask you inform the researchers immediately if you experience any of these symptoms.

How will this project be conducted?

Testing will be conducted at Victoria University City Campus, Level 4, 301 Flinders Lane, Melbourne. You will be screened to determine your suitability to participate, involving identifying any history of spinal injury, pain, pathology or other potential contra-indications.

Who is conducting the study?

Associate Professor Gary Fryer PhD, BSc (Osteo), Phone: 99191065 & 0402 029 098
Sarah Wood BHSc (MST) Phone: 0421 728 689

Any queries about your participation in this project may be directed to the Chief Investigator listed above. If you have any queries or complaints about the way you have been treated, you may contact the Research Ethics and Biosafety Manager, Victoria University Human Research Ethics Committee, Victoria University, PO Box 14428, Melbourne, VIC, 3001 or phone (03) 9919 4148.

APPENDIX E: Consent Form (Chapter 4)

CONSENT FORM FOR PARTICIPANTS INVOLVED IN RESEARCH

VICTORIA UNIVERSITY
MELBOURNE AUSTRALIA

'Reliability and Validity of Pressure Algometry for Measuring Pressure Pain Thresholds of the Hamstring Muscle'

A pilot reliability study for validating the use of pressure algometry for measuring pressure pain thresholds of the hamstring muscle

This study aims to validate the intra-rater reliability of pressure algometry for measuring pressure pain thresholds of the hamstring muscle.

CERTIFICATION BY SUBJECT

I, _____

Of _____

certify that I am at least 18 years old* and that I am voluntarily giving my consent to participate in the study:

''Reliability and Validity of Pressure Algometry for Measuring Pressure Pain Thresholds of the Hamstring Muscle'

being conducted at Victoria University by Sarah Wood and supervisor *Associate Professor Gary Fryer.*

I certify that the objectives of the study, together with any risks and safeguards associated with the procedures listed here under to be carried out in the research, have been fully explained to me by:

Masters student researcher, Sarah Wood and that I freely consent to participation involving the below mentioned procedures:

- Lying on your back on a treatment table in the test position
- The use of a pressure algometer on the hamstring muscle to conduct 3 separate measurements

I certify that I have had the opportunity to have any questions answered and that I understand that I can withdraw from this study at any time and that this withdrawal will not jeopardise me in any way.

I have been informed that the information I provide will be kept confidential.

Signed:

Date:

Any queries about your participation in this project may be directed to the researcher
Associate Professor Gary Fryer PhD, BSc(Osteo), ND Tel: 03 99191065.
Sarah Wood BHSc (MST) Tel: 0421 728 689

If you have any queries or complaints about the way you have been treated, you may contact the Research Ethics and Biosafety Manager, Victoria University Human Research Ethics Committee, Victoria University, PO Box 14428, Melbourne, VIC, 8001 or phone (03) 9919 4148.

APPENDIX F: Brief Medical Questionnaire (Chapter 4)

BREIF MEDICAL QUESTIONNAIRE

VICTORIA UNIVERSITY
MELBOURNE AUSTRALIA

You are invited to participate in a research project entitled:

'Reliability and Validity of Pressure Algometry for Measuring Pressure Pain Thresholds of the Hamstring Muscle'

A pilot reliability study for validating the use of pressure algometry for measuring pressure pain thresholds of the hamstring muscle

Am I eligible to participate in this study?

You are eligible to be included in the study if you are between the ages of 18-65 years with no major health concerns. You may not be eligible to participate if you currently have the following (please tick as appropriate; the researcher will discuss these with you):

Condition	No	Yes
Current lower back pain		
Numbness, tingling or altered sensation in the legs		
Any condition or disease of the lumbar spine (e.g., disc prolapse, spondylolisthesis, degeneration, tumour)		
Systemic disease or active oncologic disease		
Insulin-dependent diabetes mellitus		
Haemorrhagic disease		
Currently taking blood thinning medication (Warfarin)		
Inflammatory rheumatic disease		
Currently pregnant		

APPENDIX G: Information to Participant Form (Chapter 5)

INFORMATION TO PARTICIPANTS INVOLVED IN RESEARCH

VICTORIA UNIVERSITY
MELBOURNE AUSTRALIA

You are invited to participate

You are invited to participate in a research project entitled:

'Efficacy of Myofascial Decompression for Muscle Extensibility'

A cross-over trial comparing the effect of two dry cupping techniques for the hamstring muscle.

This project is being conducted by research student Sarah Wood BHSc (MST), supervisor Associate Professor Gary Fryer PhD, BSc (Osteo), College of Health & Biomedicine, Victoria University, Dr Karen Lucas PhD, and Caroline Cleary BHSc (MST), MPhySt (Physio), MSportsPhysio

Project explanation

This project aims to examine if different dry cupping techniques improve musculoskeletal tissue pain sensitivity and range of motion. Dry cupping is a Traditional Chinese Medicine therapy that involves placing cups on the skin, creating a suction on the skin and underlying tissue. Myofascial Decompression (MFD) is a modified cupping technique used by some Western manual therapists and combines dry cupping with active motion. This study aims to determine if this modified application of cupping is more effective for reducing musculoskeletal pressure pain sensitivity and increasing range of motion than traditional dry cupping.

Am I eligible to participate in this study?

You are eligible to be included in the study if you have limited hamstring flexibility and between the ages of 18-65 years. You may not be eligible to participate if you currently have the following (please tick as appropriate; the researcher will discuss these with you):

Condition	No	Yes
Current lower back pain		
Numbness, tingling or altered sensation in the legs		
Any condition or disease of the lumbar spine (e.g., disc prolapse, spondylolisthesis, degeneration, tumour)		
Systemic disease or active oncologic disease		
Insulin-dependent diabetes mellitus		
Haemorrhagic disease		
Currently taking blood thinning medication (Warfarin)		
Inflammatory rheumatic disease		
Currently pregnant		

An eligibility test will be applied to determine if you have limited flexibility of the hamstring muscles. This will be measured by laying on your back with your hip flexed to 90 degrees, and slowly straightening your knee. If you are very flexible, you will not be able to join the study.

What will I be asked to do?

If you decide to take part in this study, you will be invited to attend a one intervention session per week, for three weeks. During the intervention, we will ask to conduct two sessions of measurements, one before the dry cupping technique is applied and one after.

You will be requested to:

- Complete an informed consent form.
- Answer questions regarding any previous history of back or hip conditions (see above).
- Bring some tight-fitting shorts to wear during intervention (a changing room will be provided).
- Undergo measurement of your hamstring muscle flexibility via a photograph (these will be of the hip and leg, not of the face). You will be asked to lie on your back and the researcher will bend your knee and lift your leg to stretch your hamstring (back of your thigh). You should indicate when the stretch becomes uncomfortable and the researcher will stop. The researcher will have a device that measures the pressure used to stretch your leg.
- You will receive an envelope with a random sequence of interventions, which may consist of dry cupping, active movement of your legs, or both. These interventions will be performed as you lie on your stomach.
- Undergo a measurement of your hamstring muscle flexibility as before.
- Return one week later to undertake second intervention.
- Return the third week to undertake the final intervention.

What are the interventions?

The intervention may include a dry cupping technique. Dry cupping involves the use of plastic cups that are placed on the back of your thigh and a manual handheld pump is used to create a vacuum suction on the skin and underlying tissue; these will be left on for a 15-minute duration. You may be asked to rest or undertake a series of movements while the cups are in place. The movements may include a range of hamstring stretches lying on the treatment table or in the standing position.

If at any stage of the study you feel any pain, sensations of discomfort or symptoms, please inform the researchers immediately.

What will I gain from participating?

There are no direct benefits to you, but your participation will allow for further knowledge regarding the effectiveness of two different dry cupping techniques on the treatment of hamstring tightness. These results can then be applied to a clinical setting, aiding effective treatment in the future. Participants will receive a $10 Coles Myer gift card after the 24-hour follow up measurements.

How will the information I give be used?

The personal information that you provide researchers will remain strictly confidential. The photos will be of the hip and leg and not include your face. The researcher affiliated with the study will be the only person to have access to your private information and the data obtained during the study, which will be stored at Victoria University. You will be provided with your own personal data for your interest. Further, any reporting of data (such as thesis publication, conferences or seminars) will be done so in a summarised form without personal information that may identify you. Other participants in the study will not have access to the results that you have received throughout the study.

What are the potential risks of participating in this project?

There is some risk with participating in this study, whereby you may experience some discolouration of the skin under the site of cupping (called ecchymosis) due to the pressure suction of the cups. These marks are not painful and may last from 2-10 days. The risks associated with vacuum cupping are far less than traditional methods that use open flames and skin incisions. The method used in this project does not break the skin barrier and does not use an open flame. There are some risks of infection and skin irritation if the cups are applied over broken skin, however, this is unlikely as the area will be thoroughly inspected prior to the application.

How will this project be conducted?

Testing will be conducted at Victoria University City Campus, Level 4, 301 Flinders Lane, Melbourne. You will be screened to determine your suitability to participate, involving identifying any history of spinal injury, pain, pathology or other potential contra-indications.

Who is conducting the study?

Associate Professor Gary Fryer PhD, BSc (Osteo), Phone: 99191065 & 0402 029 098
Sarah Wood BHSc (MST) Phone: 0421 728 689

Any queries about your participation in this project may be directed to the Chief Investigator listed above. If you have any queries or complaints about the way you have been treated, you may contact the Research Ethics and Biosafety Manager, Victoria University Human Research Ethics Committee, Victoria University, PO Box 14428, Melbourne, VIC, 3001 or phone (03) 9919 4148.

APPENDIX H: Consent Form (Chapter 5)

CONSENT FORM FOR PARTICIPANTS INVOLVED IN RESEARCH

VICTORIA UNIVERSITY
MELBOURNE AUSTRALIA

'Efficacy of Functional Release Cupping for Muscle Extensibility'

A cross-over trial comparing the effect of two dry cupping techniques for the hamstring muscle.

This project aims to examine if different dry cupping techniques improve musculoskeletal pain and range of motion. Dry cupping is a Traditional Chinese Medicine therapy that involves placing cups on the skin, creating a suction on the skin and underlying tissue. Functional release cupping (FRC) is a modified cupping technique used by some Western manual therapists and combines dry cupping with active motion. This study aims to determine if this modified application of cupping is more effective for reducing musculoskeletal pressure pain sensitivity and increasing range of motion than traditional dry cupping.

CERTIFICATION BY SUBJECT

I, _____

of _____

certify that I am at least 18 years old* and that I am voluntarily giving my consent to participate in the study:

'Efficacy of Functional Release Cupping for Muscle Extensibility'

being conducted at Victoria University by: Sarah Wood and supervisor *Associate Professor Gary Fryer.*

I certify that the objectives of the study, together with any risks and safeguards associated with the procedures listed here under to be carried out in the research, have been fully explained to me by:

Masters student researcher, Sarah Wood and that I freely consent to participation involving the below mentioned procedures:

- Lying on your back on a treatment table in the test position
- Receiving dry cupping therapy and may be asked to perform an active movement protocol, including hamstring stretching

I certify that I have had the opportunity to have any questions answered and that I understand that I can withdraw from this study at any time and that this withdrawal will not jeopardise me in any way.

I have been informed that the information I provide will be kept confidential.

Signed:

Date:

Any queries about your participation in this project may be directed to the researcher
Associate Professor Gary Fryer PhD, BSc(Osteo), ND Tel: 03 99191065.
Sarah Wood BHSc (MST) Tel: 0421 728 689

If you have any queries or complaints about the way you have been treated, you may contact the Research Ethics and Biosafety Manager, Victoria University Human Research Ethics Committee, Victoria University, PO Box 14428, Melbourne, VIC, 8001 or phone (03) 9919 4148.

APPENDIX I. Brief Medical Questionnaire (Chapter 5)

BREIF MEDICAL QUESTIONNAIRE

VICTORIA UNIVERSITY
MELBOURNE AUSTRALIA

You are invited to participate in a research project entitled:

'Efficacy of Functional Release Cupping for Muscle Extensibility'

A cross-over trial comparing the effect of two dry cupping techniques for the hamstring muscle.

Am I eligible to participate in this study?

You are eligible to be included in the study if you have limited hamstring flexibility and between the ages of 18-65 years. You may not be eligible to participate if you currently have the following (please tick as appropriate; the researcher will discuss these with you):

Condition	No	Yes
Current lower back pain		
Numbness, tingling or altered sensation in the legs		
Any condition or disease of the lumbar spine (e.g., disc prolapse, spondylolisthesis, degeneration, tumour)		
Systemic disease or active oncologic disease		
Insulin-dependent diabetes mellitus		
Haemorrhagic disease		
Currently taking blood thinning medication (Warfarin)		
Inflammatory rheumatic disease		
Currently pregnant		

An eligibility test will be applied to determine if you have limited flexibility of the hamstring muscles. This will be measured by laying on your back with your hip flexed to 90 degrees, and slowly straightening your knee. If you are very flexible, you will not be able to join the study.

APPENDIX J: Victoria University Human Research Ethics

Application Approval

RE: Quest Ethics Notification - Application Process Finalised - Application Approved

-----Original Message-----
From: Quest NoReply
Sent: Wednesday, 22 August 2018 3:03 PM
To: Gary Fryer <Gary.Fryer@vu.edu.au>
Cc: Sarah Wood <sarah.wood@live.vu.edu.au>; shane.sweeney@mh.org.au
Subject: Quest Ethics Notification - Application Process Finalised - Application Approved

Dear ASPR GARY FRYER,

Your ethics application has been formally reviewed and finalised.

» Application ID: HRE18-114
» Chief Investigator: ASPR GARY FRYER
» Other Investigators: MS Sarah Wood, DR KAREN LUCAS, MS CAROLINE CLEARY » Application Title: Efficacy of Functional Release Cupping for Muscle Extensibility » Form Version: 13-07

The application has been accepted and deemed to meet the requirements of the National Health and Medical Research Council (NHMRC) 'National Statement on Ethical Conduct in Human Research (2007)' by the Victoria University Human Research Ethics Committee. Approval has been granted for two (2) years from the approval date; 22/08/2018.

Continued approval of this research project by the Victoria University Human Research Ethics Committee (VUHREC) is conditional upon the provision of a report within 12 months of the above approval date or upon the completion of the project (if earlier). A report proforma may be downloaded from the Office for Research website at: http://research.vu.edu.au/hrec.php

Office for Research - Ethics & Biosafety

Human Research Ethics Ethical Conduct in Human Research. It is required that all research involving or impacting on humans is performed in an ethical manner.

research.vu.edu.au

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Please note that the Human Research Ethics Committee must be informed of the following: any changes to the approved research protocol, project timelines, any serious events or adverse and/or unforeseen events that may affect continued ethical acceptability of the project. In these unlikely events, researchers must immediately cease all data collection until the Committee has approved the changes. Researchers are also reminded of the need to notify the approving HREC of changes to personnel in research projects via a request for a minor amendment. It should also be noted that it is the Chief Investigators' responsibility to ensure the research project is conducted in line with the recommendations outlined in the National Health and Medical Research Council (NHMRC) 'National Statement on Ethical Conduct in Human Research (2007).'

On behalf of the Committee, I wish you all the best for the conduct of the project.

Secretary, Human Research Ethics Committee
Phone: 9919 4781 or 9919 4461
Email: researchethics@vu.edu.au