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FASCIA SCIENCE AND CLINICAL APPLICATIONS: Narrative Review

Improving the quality of myofascial release research – A critical appraisal of systematic reviews

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ABSTRACT

Introduction: Myofascial release (MFR) is a form of manual therapy that involves the application of a low load, long duration stretch to the myofascial complex intended to restore optimal length, decrease pain and improve function. MFR is being used to treat patients with a wide variety of conditions, with favourable evidence supporting its efficacy. Critical appraisal of the recent research trials or reviews aims to improve the quality and reliability of future works in this field.
Objective: This work attempts to examine and categorize the strength and limitations of current MFR research by critically appraising recent systematic reviews (SRs) to synthesise recommendations for improving quality and reliability of future clinical trials.
Methodology: SRs on MFR published until 2018 were selected for this analysis. The methodological qualities of the SRs were assessed by AMSTAR II tool.
Results: The SRs demonstrated moderate methodological quality. The overall confidence rating of the results of the review by AMSTAR II was low to moderate, mainly due to the omission of a risk of bias analysis in two of the reviews.
Conclusion: This review concludes that the SRs analysed were completed with moderate methodological quality, but with procedural weaknesses and interpretation biases. The most recent review was qualitatively superior due to the inclusion of risk of bias analysis and effect size calculation. This critical appraisal and the derived recommendations can act as 'stepping stones' on which high quality future MFR trials and evidence can be built.

1. Introduction

Over the past decade, manual therapy concepts for treating musculoskeletal disorders have gained popularity among an increasing number of researchers studying them and their effectiveness to aid in implementing evidence-based practice. One such concept is Myofascial Release (MFR), originally described in osteopathy but more recently used by manual therapists, as well to treat soft-tissue musculoskeletal disorders and pain. Although MFR encompasses several different treatment methods, it most commonly refers to a passive treatment method that requires participation from both the clinician and patient in terms of feedback. The feedback from the patient's body is used to determine stretch direction, force, and duration to address specific soft tissue restrictions. This is unlike other passive techniques such as massage; strain and counter-strain technique; or active methods such as muscle energy techniques and trigger-point therapy that are also considered a part of MFR by some practitioners (McKenney et al., 2013). In order to understand this concept further and also the development of dysfunction, it is important to understand the structure and function of fascia in general.

Fascia is a ubiquitous tissue permeating the entire body. It surrounds, supports, suspends, protects, connects and divides muscular, skeletal and visceral components of the organism (Kumka and Bonar, 2012). It not only lubricates the fibers, but gives nourishment to all parts of the body. The fascial system is considered a "tensegrity" or tensional integrity structure to manage the balance between tension and compression around the organs, joints and muscles (Chen et al., 2016). Its continuity aids in force transmission both longitudinally (for example, in myotendinous junctions) and epimuscularly (between adjacent muscles) through mechanotransduction (conversion of physical forces into intracellular biochemical responses). These forces may also be transmitted

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at a cellular level, altering gene expression of fibroblasts and thereby changing the extracellular matrix composition (Chaitow, 2016). Such repetitive mechanical stimulation of fibroblasts can result in secretion of inflammatory mediators (Dodd et al., 2006). All the above events can further affect the normal functions of force transmission or sliding in the musculoskeletal system, and lead to pain or proprioceptive issues, considering the fact that fascia has been shown to be innervated. Thus, assessment and treatment of disorders affecting the musculoskeletal system may need to be focused on together with the fascial network, in order to address the problem effectively (Kwong and Findley, 2014).

Recent studies have demonstrated the role of fascia in various musculoskeletal dysfunctions as the fascial tissues connect the skeletal muscles forming a body-wide web in a pattern called myofascial meridians. As fascia is able to modify its tensional state, strain transmission along the meridians might occur in response to changes in muscle activity (Wilke et al., 2016). Studying fascia objectively at a basic scientific and clinical level will provide important information that may change clinical practice.

Myofascial release (MFR) is a form of manual therapy which involves the application of a low load and long duration stretch to the myofascial complex intended to restore optimal length, decrease pain, and improve function (Ajimsha et al., 2015). The basic rationale for these techniques can be traced to various studies that investigated plastic, viscoelastic, and piezoelectric properties of connective tissue (Schleip, 2003; Greenman, 2003; Pischinger, 1991; Ajimsha, 2018).

The effects of such manual fascial interventions can be local (as tissue texture changes), segmental (as via neurological response), and global (as through hormonal effects) in extent, and may occur at different intervals—ranging from minutes to weeks—after a given input, with many interacting mechanisms influencing tissue properties and behaviours, including placebo. Some of these factors are strongly supported by the available evidence, whereas others need further investigation (Tozzi, 2015). MFR is being used to treat patients with a wide variety of conditions, mainly based on anecdotal evidence.

MFR is thought to bring sustained change in the architecture and/or hydration of the fascia being worked on. The fascial tissue may respond better to balanced and sustained stretch rather than intermittent and unequal loads (Meltzer et al., 2010). Fascia-oriented work may produce beneficial effects by activating various receptors in the connective tissues that elicit a series of neuromuscular reflexes (Ajimsha, 2018). These types of events occur together with concomitant autonomic and viscoelastic changes and are more likely to explain the fast tissue responses that a therapist perceives during fascial techniques (Tozzi, 2015). Fascial therapy theoretically restores mobility by re-optimizing the distribution of lines of force within fascia (Stecco et al., 2013; Day et al., 2009).

Despite its (MFR) wide clinical use there seems to be little evidence supporting its clinical effectiveness. Studies are emerging in this field with conflicting results and conclusions. Analysis of recent research trials and reviews will be a better way to appraise the quality and reliability of such work. We believe that this critical appraisal will help to summarize the strengths and weaknesses of existing research on this topic and help to fine-tune future studies. The present study attempts to critically appraise systematic reviews on MFR published between 2013 and 2018, with the aim of facilitating the creation of a more homogenous and reputable base for future trials in the field. This type of approach and its findings can be highly useful for a manual therapy technique that currently has many practice variations and applicability, to set more controlled and methodologically superior studies in the future.

2. Methodology

We searched the following electronic databases for systematic reviews (SRs) on MFR published until December 2018: MEDLINE, CINAHL, Academic Search Premier, Cochrane library, and PEDro databases. Key words used for the search were: systematic review; meta-analysis; myofascial release; myofascial release therapy; and, myofascial release treatment. Articles were identified as relevant based on the simultaneous use of the terms 'myofascial release/therapy' and 'systematic review/meta-analysis' in the abstract title. Those articles that mentioned self-myofascial release or myofascial trigger point therapy/treatment, or reporting MFR in combination with other manual methods were excluded. A total of 3 systematic reviews; McKenney et al. (2013); Ajimsha et al. (2015); Laimi et al. (2018); were considered as relevant subjects of interest and included in this critical appraisal.

The selected SRs were assessed with the AMSTAR II instrument for methodological quality. AMSTAR is a popular instrument for critically appraising SRs, especially those focusing on randomised controlled clinical trials (RCTs). The revised instrument (AMSTAR II) has 16 items in total (compared with 11 in the original) and has simpler response categories than the original AMSTAR (Shea et al., 2009). AMSTAR II allows users to adopt the rating process based on identification of critical domains or some variation based on these principles (Jüni et al., 1999). The inter-rater reliability of AMSTAR II is in an acceptable range, with 46 of the 50 κ scores falling in the range of moderate or better agreement and 39 displaying good or better agreement (Shea et al., 2017).

3. Results

After utilizing the AMSTAR II for rating the quality (Table 1), an attempt was made to categorize the reviews quantitatively (Table 2), qualitatively (Table 3) and based on potential biases in the reviews (Table 4).

4. Discussion

A systematic review (SR) is an exhaustive review of the literature addressing a clearly defined question, which uses a systematic and explicit methodology to identify, select, and critically evaluate all the relevant studies. It (SR) is also used to collect and analyse the data emerging from the studies included in it. SRs establish whether the scientific findings of research studies are consistent and whether these findings can be generalised to different populations, limiting the various possible forms of bias and increasing the reliability and precision of the estimates.

With healthcare literature being a large and heterogeneous resource, professionals must be able to obtain sufficient information to direct their practice. Professionals rely on higher levels of evidence to guide their practice, with SRs being at the higher end of evidence. Nevertheless, SRs are not free from methodological flaws and need critical analysis for interpretation of their findings.

A key consideration during any critical appraisal process is identifying the presence or potential for bias, which to a large extent can be qualitatively and quantitatively assessed in order to validate the conclusions drawn by the researchers. The strength of a well-conducted systematic review is the minimization of any bias, which then paves the way to more informed and objective decision making.

In our appraisal process we attempted to identify and assess key characteristics that make a good SR (Higgins, 2009); (i) a clearly stated set of objectives with pre-defined eligibility criteria for studies; (ii) an explicit, reproducible methodology; (iii) a systematic search that attempts to identify all studies that would meet the

Table 1
AMSTAR II rating scale.

No.	CRITERIA	McKenney et al. (2013)	Ajimsha et al. (2015)	Laimi et al. (2018)
1.	Did the research questions and inclusion criteria for the review include the components of PICO?	Y	Y	Y
2.	Did the report of the review contain an explicit statement that the review methods were established prior to the conduct of the review and did the report justify any significant deviations from the protocol?	N	PY	Y
3.	Did the review authors explain their selection of the study designs for inclusion in the review?	Y	Y	Y
4.	Did the review authors use a comprehensive literature search strategy?	N	PY	PY
5.	Did the review authors perform study selection in duplicate?	Y	Y	Y
6.	Did the review authors perform data extraction in duplicate?	Y	Y	Y
7.	Did the review authors provide a list of excluded studies and justify the exclusions?	N	N	N
8.	Did the review authors describe the included studies in adequate detail?	PY	Y	Y
9.	Did the review authors use a satisfactory technique for assessing the risk of bias (RoB) in individual studies that were included in the review?	N	N	PY
10.	Did the review authors report on the sources of funding for the studies included in the review?	N	N	N
11.	If meta-analysis was performed did the review authors use appropriate methods for statistical Combination of results?	NM	NM	NM
12.	If meta-analysis was performed, did the review authors assess the potential impact of RoB in individual studies on the results of the meta-analysis or other evidence synthesis?	NM	NM	NM
13.	Did the review authors account for RoB in individual studies when interpreting/discussing the results of the review?	N	N	Y
14.	Did the review authors provide a satisfactory explanation for, and discussion of, any heterogeneity observed in the results of the review?	Y	Y	Y
15.	If they performed quantitative synthesis did the review authors carry out an adequate investigation of publication bias (small study bias) and discuss its likely impact on the results of the review?	NM	NM	NM
16.	Did the review authors report any potential sources of conflict of interest, including any funding they received for conducting the review?	Y	Y	Y

(Y - Yes; N - No; PY – Partial Yes; NM – No Meta-analysis conducted).

Table 2
Summary of Quantitative comparison.

Author/Year	Total studies included	RCTs	non RCTs	Total Sample size (cumulative)	PEDro Score (range)	CEBM score (Mode)	Cochrane collaboration domain-based evaluation framework	Inclusion of published articles from-to (range)
McKenney et al. (2013)	10	4	6	321	6–8	4	Not defined	1994–2007
Ajimsha et al. (2015)	19	19	0	1228	5–8	2b	Not defined	1994–2014
Laimi et al. (2018)	8	8	0	457	Not defined	Not defined	Yes (Risk of bias assessment)	2013–2017

Table 3
Summary of Qualitative comparison. Analysis of potential biases.

Author/Year	Objective of the study	Prospero registration	heterogenous study population	Effect size measurement	Studied Minimal clinically important difference
McKenney et al. (2013)	Non-specific Musculoskeletal conditions	No	Yes	No	No
Ajimsha et al. (2015)	Non-specific	No	Yes	No	No
Laimi et al. (2018)	Chronic Musculoskeletal pain	Yes	Yes	Yes	Yes

eligibility criteria; (iv) an assessment of the validity of the findings of the included studies, for example through the assessment of risk of bias; and finally (v) a systematic presentation and synthesis of the characteristics and findings of the included studies.

4.1. Critical analysis

The SRs identified for this analysis were published in 2013 (McKenney et al.), 2015 (Ajimsha et al.) and in 2018 (Laimi et al.) Among the three SRs evaluated, we found more differences than similarities. The only similarities between the three reviews was their study objective; viz. the effectiveness of MFR in musculoskeletal conditions, and the inclusion of heterogenous study populations or clinical conditions. The study by Ajimsha et al. was the only one to include a trial on normal subjects besides those studying clinical conditions.

4.2. AMSTAR II analysis

Analysis of the SRs with AMSTAR-II tool revealed a low to moderate overall confidence in the results (Table 1). The studies by McKenney et al. (2013) and Ajimsha et al. (2015) had a critical flaw as there was no risk of bias analysis attempted, leading to a 'low' confidence level in the tool. The review by Laimi et al., (2018) was rated as 'moderate' in the overall confidence of results due to the presence of the risk of bias analysis. The review had positive findings regarding the inclusion criteria, method of selection of eligible studies, and consensus between the reviewers for all the three studies. There was 'partial yes' for the protocol as no meta-analysis or analysis of heterogeneity was conducted. All three studies justified this by mentioning that meta-analysis was not possible due to the heterogeneity of the study population. The comprehensive research strategy also met with 'partial yes' as the study did not attempt to find the grey literature and articles in press. No details of excluded studies were given in any of the reviews. In addition, reporting the procedure of 'blinding' was not comprehensive for details like sequence generation, allocation, and data reporting, none of which were clearly mentioned in these reviews.

Table 4
Summary comparison of potential biases identified in each review.

Bias type	McKenney et al 2013	Ajimsha et al., 2015	Laimi et al., 2018
Selection bias (occurs when a non-systematic approach to article inclusion is used, so that obtained studies do not accurately reflect the research literature)	+++	++	+
Publication bias (Focusing only on published literature ignores potentially useful negative or dated research)	++	+	+
Language bias (Limiting returned articles to English language has the potential to ignore useful research published in other languages, and skews the population to whom the research may be applied)	+++	+++	+++
Outcome bias (Within published studies, the included results may be limited to those with positive outcomes, limiting the reported data)	++	+	+
Study level bias (Biases associated with conduct of trials, such as sampling bias, allocation bias, placebo effect)	++	+	+

(+less likely, ++ likely, +++ very likely)

4.3. Quantitative vs qualitative analysis

The reviews by McKenney et al. and Ajimsha et al. used similar tools to grade the quality of individual studies, and Laimi et al. implemented the Cochrane recommended "risk of bias tool" for the analysis of included studies. While McKenney et al. included single case studies in their review, lowering the strength of generalizability of research findings, both Ajimsha et al. and Laimi et al. had included only RCTs in their reviews, making them superior to the one by McKenney et al. in this respect.

4.4. Quantitative analysis

Quantitative Research is used to quantify the problem by generating numerical data that can be transformed into usable statistics. An analysis of number of studies included, sample size, scoring system and year range of included studies was undertaken in this session. Table 2 summarises all the quantitative findings. It was noted that the review by McKenney et al. included limited articles in the review, the latest being from 2007 although their research was published in 2013. At this point it is difficult to evaluate if this was due to poor search strategy, selection bias, or other factors, due to the study search being non-reproducible. In contrast, Laimi et al. only presented research since 2013, although their search strategy stated that the search was performed with unrestricted dates. Although this only raises questions regarding a potential for selection bias, these reviewers were only concerned with researching MFR in chronic pain state alone as compared to the other two research groups. This may explain this trend. Moreover, we believe that their search strategy is highly reproducible as compared to the other two researchers, eliminating or minimizing the possibility for such a bias.

The review by Ajimsha et al. had the broadest study inclusion range from 1994 to 2014, which includes 2 decades of research work in the field of MFR. It therefore provides results based on a larger population, about 3 times the sample size (cumulative from all studies) reported by Laimi et al. Also worth noting is the fact that this review had more RCTs with a CEBM score of 2b (Individual cohort studies or low-quality randomised controlled trials), than that by McKenney et al., which had primarily included level 4 studies (Case series, poorly designed cohort or case-control studies). The reviews by both McKenney et al. and Ajimsha et al. included studies with a PEDro score up to 8 out of a maximum score of 10. The review by Ajimsha et al. appears more comprehensive in this regard, due to the inclusion of a larger number of high quality RCTs from the past 20 years.

4.5. Qualitative analysis

The qualitative analysis aims to increase the overall understanding of the quality, characteristics, and meaning of the research. It provides an understanding of the research objective by revealing patterns and themes in the data. We have included the objective of the study, registrations, heterogeneity/homogeneity, and the inclusion of meaningful statistical components under this category (Table 3). Analysing the inclusion or objective of the study, the review by McKenney et al. included all non-specific musculoskeletal conditions, while the study by Ajimsha et al. included all published MFR RCTs until 2014. The study by Laimi et al. was more specific and attempted to analyse the RCTs reflecting chronic musculoskeletal pain conditions. It is also worth noting that only the review by Laimi et al. was registered in the Prospero database or any other similar database, increasing the face validity and reliability of this review. In addition, only this review took into account the "effect size" and/or "minimal clinically important difference" while interpreting or inferring the results/outcome of the reviewed research. We consider this another strength of this review, making it stand ahead of the other two, helping clinicians towards

meaningful interpretations of the research findings.

Bias is defined as any tendency which prevents unprejudiced consideration of a question (Dictionary.com). In research, bias occurs when "systematic error [is] introduced into sampling or testing by selecting or encouraging one outcome or answer over others" (Merriam-Webster.com). Bias can happen at any stage of research, including study design or data collection, data analysis, interpretation and even with publication. Reviewers and readers of the study must know its influence on the study's outcome and the researcher's attempts to reduce the impacts of different biases in their studies. Some degree of bias is inevitable; readers must also consider how bias might influence a study's conclusions and generalizability (Gerhard, 2008). Table 4 provides a summary of potential biases found in these reviews. We have graded them as "very likely", "likely" to "less likely" for the ease of understanding.

Our evaluation of biases in these reviews revealed that the study by Laimi et al. has the least potential for bias, followed by Ajimsha et al., and finally McKenney et al. The only evident bias in the review by Laimi et al. is the language bias, which although made clear by the researchers in their selection criteria, only reduces generalizability of their findings. The study also demonstrated better adherence to PRISMA guidelines as compared to the other two. All researchers explained the limitations of their study correctly, and concluded the lack of RCTs on MFR being a reason for presenting with inconclusive evidence.

These SRs on MFR assessed the quality, results and limitations of RCTs and case studies found in a multi-database literature search of peer-reviewed articles in the English language. In many of these studies, the MFR treatment was adjunctive to other treatments and the potential specific MFR effect could not be determined. The reviews by McKenney et al. and Ajimsha et al. concluded that MFR was demonstrated to be equal to or more effective than sham, conventional, and no treatment for various musculoskeletal and painful conditions. Laimi et al. concluded that the current evidence is insufficient to warrant MFR effectiveness in chronic musculoskeletal pain based on the effect size calculation.

4.6. Evaluation of review validity

Even though the SRs clearly focused on MFR, the included studies were very heterogeneous in terms of population included, type of MFR administered, control groups, outcome measures, timing of follow-up and presentation of data. This might have affected the validity of the results and their generalization. Since MFR can be useful in a variety of conditions and relies on therapist-patient interaction, the inclusion criteria utilized across the reviews might not be sufficient enough to streamline the results of the reviews. Laimi et al. made it more acceptable by selecting the MFR RCTs of only chronic musculoskeletal pain. Although attempts were made by all researchers to find all published RCTs, some relevant trials might have been overlooked due to language constraints. The identified studies have been evaluated for methodological quality by various rating scales, but a risk of bias analysis was not done in the McKenney et al., and Ajimsha et al. reviews. This might have had an impact on the interpretation of results, considering the varying quality and standard of the RCTs reviewed. An analysis of the possible/potential biases or methodological flaws might be an ideal component for the limitations observed. The methodological quality evaluation was carried out by two reviewers and details regarding the degree of agreement between them were given in a non-conclusive way. The SR by Ajimsha et al. might be affected by reporting bias as four out of 19 RCTs included were by the principal author, but this was managed by selecting two independent evaluators with no direct knowledge of or expertise in MFR, once the author blinded the identified studies. The names of the authors, the journals, the country of origin and the results were concealed from the independent reviewers, as this information could theoretically have influenced the evaluation.

Restricting searches to databases of peer reviewed literature was another potential limitation. Including the so-called 'grey literature' would have reduced potential publication bias, but would also have been likely to increase data heterogeneity. Nonetheless, despite their heterogeneity and potential statistical validity issues, from the overall results it can be inferred that it is worth running large scale trials to explore the effect of MFR in managing various clinical conditions.

4.7. Interpretation of review results

It is not uncommon for individual studies from a SR to show different and even contradictory results. It is also relatively common to find that although the results of individual studies agree on the efficacy of a therapy, they may not necessarily agree on the magnitude of its effect. In their limitations sections, the respective authors of the three reviews mentioned issues such as heterogeneity of the population, treatments applied, outcome measure usage and assessment, and quality of studies included. The populations included in the studies differed with regard to certain characteristics that could have influenced the outcomes, such as the severity of the disease and population characteristics; including but not limited to: age, sex, health status, and so forth. The reviewers commented on the heterogeneity of the treatments administered, while justifying this as a common problem faced by every manual therapy intervention. It is a much-recognized limitation of manual therapy as there is great variation in the practice including, but not limited to the pressure, technique, treatment times, and engagement.

Since the diversity of the cases included in the reviews was vast, the outcomes measured in the RCTs were also different with regard to the technique, the frequency, or the criteria used which could jeopardize the comparability of results between studies. There is no doubt that the scientific methodological quality according to which a study is conducted could modify its results, and the differences between studies could be attributed to the differing qualities of the methods. None of the reviews carried out any homogeneity tests. It was possible to evaluate the probability that the differences between the results were exclusively due to chance and not to the factors mentioned above. Nevertheless, a clinical view of the differences may be more informative than the result of a hypothesis test as the differences may have no statistical significance but actually be of clinical importance, and vice versa. It should be noted at this point that the heterogeneity of the studies included was mainly due to the vast range of applicability of MFR. Also, the methodological quality rated by the scale did not plot any unusual findings of the included RCTs.

The two scales used for methodological quality of the included RCTs —the PEDro and CEBM by McKenney et al. and Ajimsha et al. —are known for their ability to measure methodological quality and clinical hierarchy. A risk of bias analysis as incorporated by Laimi et al., if performed by Ajimsha et al. could have improved the overall validity of his review. There were also issues with the protocol used in the studies reviewed by these authors as some studies used a fixed protocol, while a few others used a flexible one. However, in manual therapy clinical applications, both methods are acceptable and have to be considered as valid until the research has advanced towards dose-response levels.

Most of the studies included in the reviews lacked clear details about the blinding, and the majority of the studies reported the lack of blinding of the therapist and the patient as limitations. This can be justifiable considering the special characters that the manual

therapy generally and MFR specifically exhibit in the interaction and application part. According to Franke et al. (2014), therapist or patient blinding within manual therapy trials is impossible. There were also larger issues surrounding the generalizability of strictly controlled RCT-derived results into general manual therapy practice as required by an evidence-based model (Milanese, 2011). The reviews also highlight the fact that 'the magnitude of the effects were mostly retained' in studies with follow-ups even though such studies were fewer in number. Laimi et al. reported that five out of eight studies reviewed did not reach the minimal clinically important difference (MCID) to have a clinically meaningful conclusion. Usage of MCIDs in chronic pain conditions in manual therapy research will be a topic of debate in the future.

Though the results of the reviews were pointing towards some positive effects, there were several challenges to the statistical inferences underpinning these findings. Because of the high levels of data heterogeneity and underpowered sample size within trials, the results cannot be generalised into any population as a recommendation. It seems reasonable that in the authors' qualitative synthesis, the best evidence would be provided by the higher quality studies which are less likely to have biased results. It is also worth introducing the effect size calculations as a normal practice in forthcoming RCTs as introduced by Laimi et al. in his SR.

Another major issue affecting the interpretation of results was the higher possibility of type I error due to the underpowered samples tested. All reviewed RCTs analysed in the selected SRs set a prior significance level of $p \leq 0.05$; a level known to generate false positive findings in at least 30% of adequately powered studies and much higher percentages of false positive findings in underpowered studies (Colquhoun, 2014). There is a high chance of superrealisation bias in the studies as most of them used a relatively smaller sample size which in turn produces more tightly controlled delivery of interventions and the collection of outcome data in ways that are not logistically possible in larger ones (Crocker et al., 2013). This is not to say that the small trial results are therefore intrinsically biased, but pooling data from a number of these relatively well-controlled small trials may create a positive bias; pooling data from a series of small trials is not a substitute for a large scale trial (Slavin and Smith, 2009).

4.8. Applicability of review results in clinical practice

It is impossible to actively promote implementation of the results of all SRs because of the limited capacity of health systems to absorb the new evidence and implement the necessary measures to do away with the obstacles hampering translation of the results of theory into practice. With underpowered sample sizes and the heterogeneity of the studies, we cannot argue that MFR has a definitive role in the management of the conditions studied in this review. These reviews can only suggest to clinicians that there is 'statistical' evidence that MFR alone or as an adjuvant therapy, relieves pain and improves function as much as conventional therapies. The review by Laimi et al., reminds us of the importance of statistical and clinically meaningful interpretation of results and the difference it can draw.

If a therapist is looking at the results of a study, they must consider the feasibility of selecting and applying a particular MFR technique based on the environment, the options available, the usage of best practices, staff expertise in MFR, the type and stage of the condition, as well as factors such as staff training. The patient and the therapist should both feel at ease around one another. The experience and training of the myofascial therapists who performed the treatments were mentioned only in a few studies. One can say that MFR can be a non-invasive, non-pharmacological, and low cost alternative for various conditions with very minimal adverse events.

4.9. Recommendations for improved research

Recommendations for future randomised, effectiveness trials on MFRs include: a-priori calculated sample sizes; adequate power; reliable and validated outcome measures; limiting the sources of bias; follow-up time points beyond 24 h; appropriate levels of significance; assessing the threats to statistical validity; and, appropriate registration. Most importantly, RCTs should present between group differences (complete with confidence intervals and effect sizes) and desist from reporting within group differences as evidence of effectiveness (Matthews and Altman, 1996; Schulz et al., 2010).

Sample size calculation is one of the first and most essential parts of designing a good quality trial. To make a definitive conclusion about the findings of an RCT, it is essential to ensure at least 80% power throughout the trial. For ethical and methodological purposes, we strongly recommend involving an epidemiologist or a biostatistician at the planning stage of a study, because these calculations are prone to bias and because there are ethical and financial costs related to conducting a RCT. A well-planned and methodologically sound protocol will have a stronger chance of success.

These recommendations would support inferences of clinically meaningful results and facilitate future meta-analyses. We echo current calls for transparency within all clinical trials and endorse journal adoption of reporting guidelines (Hoffmann et al., 2014; Chan et al., 2014).

To attain the highest-quality evidence, RCT designs should be good quality, participants should be randomised, double-blinding should be adhered to, and the clinician performing MFR should use it regularly in clinical practice. The subjective component of MFR must be addressed in future study designs as it has a major relevance in this type of studies (Ajimsha et al., 2015). It is very clear that the effectiveness of MFR can vary with the comfort level of the patient, which is difficult, but not impossible to control.

4.10. Limitations of the study

Critical appraisal looks at the way a study is conducted and examines factors such as internal validity, generalizability and relevance (CEBM website, Aug 2019). Systematic reviews aim to collate and synthesise all studies that meet pre-specified eligibility criteria using methods that attempt to minimise bias. We believe that utilization of methodologically superior outcome measure (AMSTAR II), quantitative, qualitative and bias comparison analyses were justifiable to the objective of the study. The notion of study "quality" is not well defined but relates to the extent to which its design, conduct, analysis, and presentation were appropriate to answer its research question. We believe that the following limitations might have some effect on this appraisal.

Firstly, we had limited our search to SRs having specified keywords only, as stated in the methodology. This was inevitably necessary to set fixed key words for the literature search in order to remain focused. It may however be possible that other systematic reviews on myofascial release research would have become available with the input of a different set of keywords. Nevertheless, to our knowledge, there are no other SRs published to date on this topic. We encourage researchers to register their trials and/study protocols in order to allow for comprehensive assessment of published and unpublished research studies. Secondly, we may be also affected by language bias as the search and selection of systematic reviews in the English language alone was made. This was again inevitable as we did not have the resources to avail ourselves of

translation services. Thirdly, it would have been ideal if the AMSTAR II grading had been done by two blinded independent observers and verified for accuracy of grading. However no such blinded scoring was done, leading to potential sources of experimenter biases and/errors. Moreover, one of the present authors' own published SRs was also part of this critical appraisal, raising a possibility of bias in interpretation and scoring of this appraisal. Whether our approach to critical appraisal was appropriate and adequate is open to discussion. It is our opinion that a SR is not necessarily superior to a well-conducted RCT, and not all RCTs are necessarily superior to observational studies of good methodological quality. We encourage readers to critically appraise research evidence before applying it to clinical practice.

5. Conclusion

Critical appraisal is an important element of evidence-based medicine to carefully and systematically examine research to judge its trustworthiness, value, and relevance in a particular context. This review concludes that the systematic reviews analysed were completed with varying methodological quality, with observed procedural weaknesses, and interpretation biases. Rating overall confidence in the results of the review by AMSTAR II was low to moderate due to the omission of a risk of bias analysis in two of the reviews. The most recent review was qualitatively superior due to the inclusion of risk of bias analysis and attempted effect size calculation. This critical appraisal and the derived recommendations can act as 'stepping stones' on which high quality future MFR trials and evidence can be built.

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