

Peer Review File

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Reviewer A

This is an interesting topic, but the authors should review the physiological effects of massage and compare it with other types of therapies, such as active exercise, which have been shown to be essential in palliative care.

Author response: This is a great idea and we are considering this for a future narrative review. For this review, we have chosen to focus specifically on massage therapy as described by a (now included) published definition of this intervention.

I suggest not using the first person (we) but impersonal.

Author response: Thank you. We removed nearly all uses of the first person.

Scientific writing needs to be considerably improved throughout the manuscript.

Author response: Thank you for this observation. We have worked to improve the writing style accordingly.

The search equation is too short. You must include more synonyms

Author response:

Results could be explained in a better and easier way (outcomes in a table).

Author response: We appreciate this and have a Table 2 that includes Results.

Summary in the discussion section. That is not correct.

Author response: Thank you for this observation. We have modified accordingly, and hope to have focused the Discussion on reflections relevant to Results presented earlier.

Reviewer B

This manuscript is overall very well-written and on an important topic-- massage therapy in the palliative care setting. You make a compelling argument for improving the rigor in which massage therapy is tested, relating it to other, perhaps more mainstream/traditional symptom management approaches. I have a few recommendations for how this manuscript could be improved:

1. More detail is needed in the introduction to define and support what you are defining as massage therapy, as this itself is inconsistent in the literature. Are you including any touch-related therapy, like brief hand massages, or only more rigorous therapeutic massage techniques taught by a certified/licensed individual, or something else?

Author response: We appreciate this comment and have provided a published definition of massage relevant to this work.

2. I think it is a limitation that you only looked at massage therapy as the primary focus, and excluded articles that had acupuncture or aromatherapy included. Some research suggests these interventions are stronger when done in tandem, so by isolating just massage therapy you are severely limiting the scope of findings and potential impact.

Author response: We appreciate this observation, and it is true that some studies do show important outcomes when massage is combined with other interventions such as acupuncture or aromatherapy. However, we have chosen to focus on massage therapy alone for at least two reasons: first, including clinical outcomes from combined modalities might muddy the waters of what we understand of massage as an isolated intervention; second, while understanding the potential impact of combined therapies is important to the evolution of integrative health sciences, these combined therapies are very rarely offered at large, making their inclusion less relevant as well. Reviewing combined integrative therapies would be an interesting future review to consider.

Reviewer C

The review tried to reveal gaps between real world massage practice and literature outcomes. However, I have some concerns about this article as below. Please address them to clarify authors' conclusion of the narrative approach.

1. The major concern is what something new was. There have been some systematic reviews (e.g. ref 28) as to alternative medicines including massage and they have showed identical conclusions of the narrative review such as a wide variety of treatments, mixed evaluation methods, no concrete outcomes, and then needs of standardized methodology to reveal their effectiveness. It may be needless to say, but one of purposes of a narrative review is that unknown problems and actionable suggestions are provided by the approach despite of its weaker evidence level. If there were no newly added suggestions, what were cues to a next step derived from the narrative approach? The authors should clarify the core value of the review. For example, the authors mentioned that a possible assessment of symptom clusters (SC) like Miladinia et al, however SC was originally advocated by Kwekkeboom et al. in J Pain Symptom Manage of 2010. The authors did not provide any reason to add their validation as an indicator of assessment other than these original authors had asserted in palliative care settings. Please unveil

authors' unique perspective not mentioned in existing reviews.

Author response: *We appreciate that there are other reviews related to massage therapy. However, we believe there is a place for this review focused on massage therapy alone (without additional/other integrative therapies) and without a disease category. We have rewritten our justification for this narrative with specificity of our intentions.*

2. Second concern was that the authors intended that massage therapy is a low-cost maneuver in palliative care settings in the abstract section (line 46 to 47), and palliative-trained massage providers were important to participate in the result section (line 216 to 239). I cast doubt on the assertion because people under palliative care often do not massage by themselves, and therefore this means no small cost required to massage. Not only employment of therapists that the authors contended but labors and time to massage are also costs as the authors discussed in lines 437 to 439 and a concept of activity-based costing, for example. Please verify massage is low cost.

Author response: *We have modified this statement to say “can be accessible” and we believe this represents a more appropriate characterization of massage as an intervention. Evaluating cost is outside the scope of this review.*

3. There was a randomized trial in a relatively large size regarding determining massage doses (ref. 19). Did the authors have a question for the result or need more dosing studies? If so, the authors clarify how to do about dosing of interventions.

Author response: *We do not have a question about the results. Certainly, dosing studies can be important. But in massage therapy, so far, they are rare. In fact our team has tried to address this by completing and publishing a dosing trial earlier this year (outside the boundaries of our literature search for this review).*

4. I think that the reference to data collection points was an important viewpoint, however the authors did not comment at a palliative care-specific concern: health conditions among palliative care patients diverges significantly. Some can do anything independently, but others face last limited days. This means that critically-ill participants have no room being assessed weekly intervals. The authors selected excerpts which were ICU settings and studies during four to eight weeks. Please consider whether only these sources, whose almost all participants were better physical conditions, were appropriate to bring suggestions for future study in palliative care population.

Author response: *This is an interesting and important point. We believe we have clarified reasons for including palliative care populations across broad disease groups. This reflects a practical implementation of massage for patients receiving palliative care, because, like palliative care itself, massage should be delivered on the basis of need, not prognosis or disease group.*