Reviewer's report

Title: The Effect of Massage on Muscle Fatigue and Recovery

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Reviewer: Dr Michael Callaghan

Level of interest: A paper whose findings are important to those with closely related research interests

Advice on publication: Unable to decide on acceptance or rejection until the authors have responded to the compulsory revisions

This seems to be the first paper I have reviewed that uses the technique of EMG MNF, MDF & RMS to measure fatigue pre and post massage. As such it is unique, and the authors should be commended for this innovation.

However, there are some concerns about aspects of this paper which should be addressed.

COMPULSORY REVISIONS
Methods
There is no power size calculation, so how did the authors arrive at n = 29?
There has been no effort to check reliability of their procedures for the EMG and MF.

Procedure
I am not sure what happened to the electrodes after the 1st test and during the massage. If they were removed, then there has been no attempt to ensure precise replacement. If they were not removed, then I cannot see how the massage was performed.

What was the rationale behind 5 minutes of massage?

There should be a name for the software used. If they used the Biopac was it AcKnowledge software?

Other EMG parameters have been omitted such as the CMRR, the Filters, differential input impedance etc.

Why was the sampling rate set at 1000 Hz, when it should processed at 1024 hz for FFT.

EMG Analysis
EMG analysis is difficult to interpret. Examples of the types of decline for MDF and MNF and increase for the RMS should be given in figures.
Results
I am surprised the authors do not relate their results more to other massage studies that have shown essentially the same results (i.e. the subjects record subjective improvement, in this case a decrease in VAS, but there is no objective improvement). See Hemmings et al. Brit.J.Sports Med. 2000; 34(2): 109-114).

Table 2 is very cumbersome and off-putting. It is difficult to understand what the authors want to relay. This should be simplified somewhat without being superficial. In table 5 the authors should consider the clinical meaning of their VAS scores. For example, the 'Comparison measure' means would be 6,6,1 and the 'Compared measure' 6,7,0 if rounded up or down to the nearest whole integer. This should be highlighted in the discussion.

Discussion
Paragraph 3 of the discussion should be an ideal section for the authors to describe their hypothesis as to how massage would affect the physiology of fatigued muscle, but they fail to do so. The essential question is: 'How would massage ease muscle fatigue physiologically.'
The authors get confused over this as in the Background section they cite Goats (ref 3) saying it has not been fully elucidated and then cite the 2nd paper by Goats (ref 13) stating that decreased fatigue is a main expected outcome. Which is it?

There should be a paragraph citing the limitations of the study. An important limitation is that of an 'in-laboratory' experiment rather than an 'in-the-field' study. Some massage therapists will certainly look at this study and state that they perform massage for longer than 5 minutes on athletes who exercise for longer than 90 seconds and that the study has no relevance. I think it would be wise for the authors to be prepared for this by accepting these limitations.

References
Reference 3 and 13 are the same. Clarify please.

Competing interests:
None declared.