Reviewer's report

Title: Physiological responses to low-force work and psychosocial stress in women with chronic trapezius myalgia.

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Reviewer: Kristian Bernhard Nilsen

Reviewer's report:

Regarding the manuscript: Physiological responses to low-force work and psychosocial stress in women with chronic trapezius myalgia.

The authors investigated women with chronic trapezius myalgia and found increased trapezius muscle activity during a set of standardized work situations, mainly during a repetitive arm movement task.

The authors should have credit for the use of very standardized tests. The methods are well described and the results are clearly presented. However, the manuscript has considerable shortcomings.

Major Compulsory Revisions:

Background:

General comments:
1. The authors do not explain explicitly why the present study was performed. Why should another study on muscular activity and musculoskeletal pain be published? This should be clearly stated in the background together with a more detailed hypothesis.

More specific comments
2. Paragraph 4 must be rewritten. It does not make sense.
3. Paragraph 7 and first part of paragraph 8 does not contribute essentially.
4. The authors argue for the use of TSST. A similar argumentation for the other tests would be appropriate.

Methods:
1. Information on the number of invitations, number of responders, number of eligible patients, number of excluded subjects (specifically for each step in the inclusion and experimental process) should be given.
2. Information regarding the reference values should either be removed or a better explanation for why it is obtained (and why it is not reported in the results..).
3. Statistics:
   a. Differential responses are described here, but not reported in the Results
section?
b. A differential response variable on the effect of psychosocial stress on muscle activity is described relative to a reference relaxation, and not to the baseline value. Why?
c. A large number of correlations are performed. A discussion of the possibility of type I statistical error is justified.
d. The interaction between pain/stress, emg and group is not reported in the mixed model analysis. Why? Instead a correlation between pain and EMG with both groups merged (and not separately for each group) is reported. Why? The authors conclude later in the discussion that they have found a positive correlation between pain/stress and EMG. However, if this correlation is not stronger or specific for the patient group, the external validity of this finding is rather small. Furthermore, the correlation between pain and emg is not significant for the time periods which the patients have higher EMG compared to controls.

4. Have the authors performed any sample size calculations? This should be clearly stated in the text.

Results
1. Complete statistics should be reported.

Discussion:
General comments
1. Too much of the results are presented in the Discussion.
2. Other papers are referred and discussed very superficially. The number of citations could be reduced (but not necessarily), and those citations used should be discussed and compared to the present results more in detail.

Specific comments
3. Autonomic responses, section 5: “Pain and stress are complex phenomena with systemic effects and dysfunctions .. should be reflected distant from the painful region” This statement is not necessarily correct [1, 2]. Please consider another formulation.

Minor Essential Revisions
1. The first sentence in the in abstract is a bit too conclusive.
2. Paragraph 6 in the Introduction should be reformulated
3. If a written informed consent was obtained this should be stated.
4. The statement “The Stress-Energe questionnaire has proven its usefulness..” should be removed or documented.
5. The ECG channel input range and accuracy is reported in mVRMS. Please check if this is correct.
6. In the discussion the term “prime mover” is used. Please consider to use another term.
7. The reference to Nilsen et al is so superficial that the meaning is lost.

References


Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests