Author's response to reviews

Title: Risk factors for development of non-specific musculoskeletal pain in preteens and early adolescents: A prospective 1-year follow-up study.

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Author's response to reviews: see over
Re: MS: 1226373966132950 - Risk factors for development of non-specific musculoskeletal pain in preteens and early adolescents: A prospective 1-year follow-up study

Dear Editor,

I thank the reviewer for her helpful comments and queries and have responded to their specific points below. Reviewer comments are in bold.

1) This manuscript is now much improved. In particular, it was nice to learn about the high response rate which saved the authors from some extra work. It is also a great advancement that the pain is separated into traumatic and non-traumatic.

Glad to hear.

2) In the introduction, at the top of page 4, I read that refs support that psychological are stronger risk factors (for the development of neck and upper limb pain (refs 9 and 23) and for lower limb pain (ref 10) rather than physical factors. However, Ehrmann Feldman et al have shown in refs 9 and 10 (same cohort?) that work is a risk factor, and they also showed work was particularly detrimental in those with lower mental health scores. I do not feel that your interpretation gives a fair picture of their conclusions. In arguing the case for the set up of ones own study, it is of course always tempting to build on statements.

a) I reported that “A prospective 1-year follow-up study of high-school children showed that psychological rather than physical factors were associated with an increased risk of development of neck, upper limb pain (ref. 9,24) and lower limb pain (ref. 10)”. The reviewer read articles 9 and 10, which present results from data collected from the same cohort, the reviewer considered that work (which was a significant risk factor) is a physical factor, and based on that did not feel that I properly interpreted their results. Well in fact we think we did.

b) In these two reports (ref 9 and 10), physical activities did not predict the onset of neck/upper limb pain or predict the onset of lower limb pain. This was a consistent finding when physical activity was explored.
by using various definitions. On the hand, poor mental health (mood problems and anxiety) predicted both neck/upper limb pain and lower limb pain. Physical activity is a "physical factor" while poor mental stress is a "psychological factor".

c) These two reports (ref. 9 and 10) also studied other potential risk factors; one of them was adolescent "Work". Work significantly predicted both neck/upper limb pain and lower limb pain in these two reports, but work is not as a marker of only physical exposures, it can be a marker of physical and/or psychological exposures. In fact the observed significant relationship between work and pain was stronger in white-collar jobs than in blue-collar jobs in both reports. For example, in neck/upper limb pain report, figures were as following: 1-blue collar work odds ratio of 1.4 (95% CI 0.74-2.68) (i.e. insignificant association), white collar work was odds ratio of 2.88 (0, 97-8.29) and in jobs involving child care the OR was 2.25 (1.18-4.29). The same results were found for musculoskeletal pain in general. quote "Those who worked in white-collar jobs were at a higher risk of developing pain than those in blue-collar jobs or childcare". We would have expected a stronger (and significant) association with blue collar jobs if we assume that work is a marker of only physical exposures.

d) I am not sure what the reviewer meant by "they also showed that work was particularly detrimental in those with lower mental health scores". I read the articles several times and did not find such results. In fact the result section of the reference 9 article says that "poor mental health was a risk factor among the students who worked (OR 1.64; 95% 1.29-2.10), but not among those who were not working (OR 1.61; 95% CI 0.84-3.09)". Although I do not agree with the authors’ interpretation of this data (it was solely based on the width of confidence limits but ORs were almost identical!), but still it was poor mental health that was particularly detrimental in workers and not the opposite way around, and this goes well with my interpretation.

e) The fourth paragraph of the introduction, which includes the previously mentioned references (ref 9 and 10), is not my main justification for setting up the study. My main justification was in paragraph two and three, and in paragraph three I said clearly that there are inconsistent results concerning the predictive role of physical exercise (physical factor). In the 4th paragraph I was mentioning results of a study, which I will be comparing my results to later on (for physical and psychological potential risk factors), that’s why I did not mention work in this summary of results, simply because I have not measured work (I was studying younger children who do not even work). I also did not mention their results about other factors (e.g. height and weight) for the same reason.

f) The main purpose of this paragraph is indicated at its end (that most of the studies mentioned here were conducted in mid and/or late adolescents, while studying younger children might be also important
to know early contributing factors) i.e. I do not need to use “bits and pieces” of study result to convince the reader of the importance of my study.

g) I would like to thank the reviewer for asking me to check the references. Although I have not changed text words, I have deleted reference 24 because this is not part of the 1-year follow-up as mentioned at the beginning of the sentence (Introduction 4th paragraph).

3) I would suggest that you either check your own references again with objectivity in mind, or that you do a proper literature review and report both those for and against your own ideas, OR that you discuss out of your own thoughts and opinions, but not using bits and pieces from references without really telling the whole truth.

I already did a proper literature review with objectivity in mind.

4) Also in the discussion section, I was intrigued by your use of references. You use the previous literature to support your own findings stating “it is unlikely that this association is only a chance finding, given the narrow confidence interval and the strong evidence in the majority of previous studies [18.27]". Majority of previous findings and 2 refs? One of these studies deals with persistent musculoskeletal pain, whereas yours deals with the incidence of pain = irrelevant ref. The other one is a non-systematic narrative review that although well written does not contain the full spectrum of the relevant literature = not acceptable ref.

a) I reported that it is unlikely that these association is only a chance finding, given the narrow confidence interval and the strong evidence in the majority of previous studies (18,27)". I have checked the occurrence estimates of musculoskeletal pain symptoms in previous studies and compared estimates in boys with that in girls; I found that in the majority of them the occurrence of pain in girls was higher than that in boys. For example for low back pain, the review article by Balague et al on its occurrence in children (1999 Eur Spine J), they reported results of 13 studies, 10 of them found that occurrence of LBP in females was significantly higher than boys, 1 found almost the same occurrence in both genders and 2 found it more in boys (only one with statically significant male predominance). I did not put a reference to all these articles; I just gave references from one of the earliest studies and a more recent study from Finland.
b) Reference 18 is not a non-systematic review article; it is an epidemiological study of 1715 schoolchildren, but I agree with the reviewer that the prospective study by Mikkelsson et al is not the best reference.

c) I have now changed the reference of this phrase to the systematic review by Balague (1999 Eur Spine J), kept the word “majority”, but added the phrase “but not all” to emphasise that some studies did not find gender differences with respect to musculoskeletal pain occurrence (discussion 4th paragraph).

5) I am sorry to spring this on you now, but I did not doubt your use of references during my previous read-through.

Its ok. I am glad that you have checked the references, because some mistakes can happen some times

6) These two ref-observations make me suspicious in relation to all the others, and I would therefore encourage you to make a check of their relevance and validity in relation to the statements you want them to support.

a) I believe that we have interpreted results of articles of reference 9 and 10 correctly. In fact if we have interpreted it as “both physical and psychological factors predicted musculoskeletal pain” we would have been biased.

b) As for the discussion references. The majority of previous studies reported that occurrence of musculoskeletal pain in females is more than boys. Maybe my use of references was not the best (but not wrong) in this one, but I believe that this should not raise suspicions on references: the first ref-observation, which we do not agree with reviewer on, has nothing to do with my main justification of the study as it was mentioned in another paragraph, and the second ref-observation is related to the selection of references for a finding that is well supported by the majority of previous research (that musculoskeletal pain in more common –in prevalence and new-onset – in females, both in adults and in pre-adults).

7) So, there is the accelerometer, which - although it does not measure cycling and swimming - is better than self-reported activities... It should perhaps be mentioned. Typical self-report physical activities and in activities have been validated against the accelerometer and found to be wanting. (Wedderkopp Spine 2003).
a) I added the main results of this study in the discussion. In addition I added that accelerometer is not the gold standard for measuring physical activities (end of second paragraph in discussion).

b) I already reported that our subjective assessment of physical activities is one of the limitations of this study. I here mean one of the limitations if the reader is interested in the aetiology of these symptoms, but it is not a limitation if the reader is interested in risk markers of these symptoms (to identify high risk groups for primary prevention purposes). If the reader aim is the second then, in fact, subjective measurements would be better than the advanced objective tools, because in order to identify high risk groups (in real-life) health care-providers or public health practitioners need to know simple methods to identify them (e.g. a questionnaire) regardless of whether the factor identified is etiologically related to the outcome or just a marker of another factor which is etiologically related to the outcome.

8) According to your statistical methods, sex was not included in the multivariate analysis, and I therefore cannot quite interpret your statement on sex in the result section ("borderline significance were found for female gender...")

Sex was included in all analyses (univariate and multivariate), as it appears in tables 2 and 3. The word "sex" was just missing in the independent variables’ list in the statistical methods; I added it to the list now.

9) Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

a) I corrected all these minor mistakes.

b) I asked an native English speaker and he told me that it sounds better if we say “data was extracted as to whether...”; “rather than “data was extracted as whether”. He also told me that “In a frequency of...” is ok, but “with a frequency of...” is better.

Sincerely,

Ashraf El-Metwally