Author’s response to reviews

Title: The role of psychosocial stress in the development of chronic musculoskeletal pain disorders: protocol for a systematic review and meta-analysis

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Associate Editor comments:

Many thanks for your submission. In addition to the comments provided by reviewer 1, please also address the following comments:

1. I am not sure what this means “Secondary outcome measures such as disability, return to work and quality of life will be included if these are correlated with the primary outcome.” Do you mean that you will extract all secondary outcomes reported in all studies? It would be more usual to specify which secondary outcomes you are interested in. And by correlated, do you mean that some sort of correlation score is provided, and that this should be a positive correlation? This would introduce bias. What is the purpose of extracting this data? How will it help to answer your question? This needs to be clarified.

Response: We thank the Editor for this helpful comment. We agree with the Editor’s point of view and thus have removed the extraction of secondary outcomes (and any mention or reference to this) from the protocol.

2. The STROBE checklist is not a measure of methodological quality, and I have concerns that the list in Additional File 4 contains many repetitions (e.g. relating to loss to follow-up) and many questions that only ask if an item is reported, rather than if it is done well. Have you
considered other tools? I have seen the following tool used in some recent protocols, and wonder if this might be more appropriate (https://www.nhlbi.nih.gov/health-pro/guidelines/in-develop/cardiovascular-risk-reduction/tools/cohort)? Some of the tools relating to prognosis may also be worth a look, e.g. QUIPS.

Response: Thank you for this suggestion. A modified version of the Quality Assessment Tool for Observational Cohort studies and Cross-Sectional studies will be used to assess the risk of bias, as suggested. To ensure a rigorous assessment, we have included four additional items to this tool: two items are implemented from the QUIPS (Quality in Prognostic Studies) tool (Q6: Did the authors attempt to collect information on participants who dropped out?; Q7: Are there important differences between participants who completed the study and those who did not?) and two items considered important by the research team (1: whether losses of follow up were accounted for in the analysis, 2: whether the source of funding is provided) have also been added.

The relevant sections have been updated in the text and read:

‘The methodological quality of the included studies will be assessed using a customised version of the Quality Assessment Tool for Observational Cohort studies and Cross-Sectional studies [15]. Two additional items from the QUIPS (Quality in Prognostic Studies) tool (Q6: Did the authors attempt to collect information on participants who dropped out? Q7: Are there important differences between participants who completed the study and those who did not?) [16] and two items considered important by the research team (whether loss to follow up is accounted for in the analysis and whether the source of funding is provided) have been added to the risk of bias assessment tool [see Additional file 4]’. Page 8, paragraph 3.

3. To investigate the introduction of publication bias, you could plan a funnel plot analysis. See Cochrane handbook for details – this is most useful where there are 10 or more studies.

Response: This is a useful suggestion and we have now included funnel plots for publication bias in the review.

The text has been amended and reads:

‘In order to investigate the introduction of publication bias, a random effects version of Egger’s test will be utilised and visualised through a funnel plot [20]. The minimum number of articles suggested to examine publication bias is ten [21]’. Page 10, paragraph 2.
Reviewer #1:

This paper describes the protocol for a systematic review and meta-analysis of the role of psychosocial stress in the development of chronic musculoskeletal pain disorders. It is an interesting and important topic and the authors aim to assess the evidence for an aetiological role of psychological stress in chronic musculoskeletal pain disorders.

1. The background to the protocol makes sense and builds a rationale for the review, although it was not clear how the results of the review would inform clinical practice. This aspect needs to be explained in more detail.

Response: The following text has been added to the protocol – discussion section (page 10 – paragraph 3) to address this point:

‘Stress can be considered a modifiable factor that, if assessed and promptly recognised, can be addressed and potentially prevent the development of chronic pain. This study should help practitioners to become more aware of the effect of stress on pain, and, on the importance of working alongside mental health clinicians in order to minimize stress in people with acute musculoskeletal disorders. Finally, a better understanding of the role of psychosocial stress in the development of chronic MDs should facilitate further research in the area of prevention and early interventions, through stress management programs for those who present with high levels of stress at the onset of their MD’.

2. The review question is: Does non-work related psychosocial stress have an aetiological role in the development of chronic MDs in the general population? It is unclear how the answer to this question in terms of cause and effect will emerge from a review.

Response: Thank you for your comment. In order to clarify how we intend to answer the research question, a small amendment has been made in the abstract (page 2, paragraph 2) and a paragraph has been added in the text following the research question (page 5, paragraph 4) and reads:

‘In order to answer the review question, an association between exposure (psychosocial stress) and outcome (chronic musculoskeletal pain) will be sought. Specifically, a meta-analysis of the available scientific literature looking at the causal role (or aetiological role) of stress on pain outcome will be conducted. Only longitudinal prospective studies will be included, where stress is considered the exposure of interest, measured at baseline in an inception cohort, or a cohort that has not yet developed chronic pain. Adjustment for confounding factors (i.e. other psychological factors) will also be taken into account in the data extraction and analysis’.
3. The searches are clear and appear appropriate, although a date limit is not mentioned in this section. It is stated later stated that only evidence published before May 2017 would be included. What is the rationale for this?

Response: We thank the reviewer for this helpful comment. The initial search (already begun) is up to August 2017. We will also re-run the search immediately prior to publication to ensure all relevant literature is captured and included. The reference to the date has been removed in the text.

4. The section describing the inclusion and exclusion criteria for the primary studies was unclear and difficult to follow. Although the standard PICOS framework may not be appropriate for this particular review, some elements of it should have been applied, and would have given the section an appropriate structure. For example, the population, outcomes and study design could all have been described together with their respective inclusion and exclusion criteria. This has been attempted to some degree for population in the 'types of participants' section, although this is followed by a heading inclusion criteria but these do not all relate to the population. It would be easier to follow if all inclusion and exclusion criteria relating to the population were in the same paragraph, with a similar paragraph for study design, and outcomes.

Response: We have corrected the inclusion and exclusion criteria section according to the input given by the reviewer. Inclusion and exclusion criteria have been divided into Participants, Types of studies and Outcome sections, respectively. The following text has been added to the protocol – Methods section (page 6– paragraph 3, page 7 – paragraph 1-4) to address this point:

‘Participants

The study population will be adults (aged over 18 years) selected from the general population suffering from pain derived from any type of MD (e.g. back pain, neck pain, temporomandibular pain). Studies will be excluded if they investigate pain that is not of musculoskeletal origin, such as visceral or cancer pain or pain derived from central neurological conditions (e.g. stroke, spinal cord injury). No restriction will be placed on participants’ gender. Since the aim of this study is to evaluate whether previous high levels of stress, or high stress present at the time of pain onset, have an aetiological role in the development of chronic MDs, studies will be included if participants are pain free at baseline, or if their pain, deriving from a MD, is acute (between 0-6 weeks from pain onset) [13].
Types of studies

Studies will be accepted if: i) they are longitudinal observational studies measuring psychosocial stress at baseline; ii) they use one or more scales or questionnaires to assess non-work related psychosocial stress. Studies will be excluded if they: i) assess work related stress, or use occupational stress models such as the Karasek Demand-Control Model or the Siegrist Effort-Reward Imbalance model [13, 14], ii) assess distress and post-traumatic stress disorder (PTSD), as these are considered to relate to a specific psychological disorder, and iii) are based on serious childhood events, such as assaults or maltreatment, as these are considered traumatic life events and may lead to serious mental health problems (e.g. PTSD).

Outcomes

Eligible studies should report the development of chronic musculoskeletal pain, lasting three months or longer [13], as the primary outcome’.

5. Other aspects of the methods, data management, data extraction and quality assessment appear appropriate and it is clearly explained how they will be conducted.

6. The plan for analysis appears appropriate, although the authors should be careful not to confuse narrative analysis with qualitative synthesis. Qualitative synthesis refers to the synthesis of qualitative (e.g. interview) data. I assume the authors are not intending to do this, as it has not been mentioned previously, and are referring to narrative synthesis of the quantitative data should meta-analysis not be possible.

Response: The word “qualitative” has been changed to “narrative”.