Author’s response to reviews

Title: Lifestyle and metabolic factors in relation to shoulder pain and rotator cuff tendinitis: A population-based study

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Author’s response to reviews: see over
Response letter

We thank for the thoughtful comments of the reviewers and believe that they have improved the manuscript. We took into consideration all comments of the reviewers and revised the paper accordingly. Below we explain how we have dealt with each of the comments. All modifications are highlighted by red colour.

Reviewer comments

Reviewer 1:

One concern I have is the use of term tendinitis which has a specific definition, something that was not really investigated in this study. Considering the diagnosis was mostly based on the history of pain and an assessment of physician (not sure if this involved an MRI/US), I recommend changing the term Tendinitis to “tendinopathy” or “pathology”. This needs to be reflected in the title, page 6 and throughout the manuscript and tables.

In all of our previous publications we used tendinitis and we wanted to use the same definition. However, we added tendinopathy in parenthesis in the Method section on page 6 in order to address variations in the terminology.

Reviewer 2:

1. The case definition is still somewhat unclear. Was there a body diagram used, or was it merely a subjective interpretation of the region of the body? Also, this revision now uses terms of "shoulder joint pain" and "rotator cuff region", which makes this somewhat unclear (is there a difference? if not suggest one term)

The participants pointed out painful joint regions using a body diagram. However, there were also questions on pain perception in the neck, shoulder and neck-shoulder angle during the preceding 30 days. We have also revised the text on page 6. Moreover, the physician confirmed in his diagnosis on chronic rotator cuff tendinitis that pain was located in the deltoid or the epaulette region. We added a note in parenthesis on page 6.

2. The standardized protocol helped significantly in understanding the research for defining health outcomes. However, after having a standardized examination, the person is classified apparently by the physician (and not standardized case definitions?) into "possible or definite" (please see prior critique). The researchers do not state how that is done. Is this a gestalt step, or was there a protocol to take a certain combination of exam findings to reach those thresholds? If it was standarized, it should be spelled out. If it was not standardized, that weakness should be noted both in the methods and limitations para.

We modified the methods and discussed our study limitations.

3. Other weaknesses that should be added to the limitations paragraph in the discussion section include: 1) underpowered for some exposures (seems likely looking at some of the data that trend although probably not major impacts; please also see prior critique), 2) not all CV risks were positive and this is not well explained (please also see prior critique), and 3) the authors do not
have data to calculate pack-years for all subjects, rather only limited to those who are current smokers, thus this limits some of the strength of the conclusions on risk from tobacco and should be noted.

We have discussed the low power of the study in the Discussion on pages 16-17. We added to our conclusions that not all cardiovascular risk factors showed an association. We agree that we do not ideal data for a dose-response relationship between smoking and shoulder pain or disorders. However, our data allowed us to assess the effects of smoking on shoulder pain or disorders.

4. Abstract. There is not uniform support for the atherosclerotic theory in this study's data. Suggest some cautionary language, e.g., conclusions section could be reworded to: "Disturbed glucose metabolism and atherosclerosis may be partial underlying mechanisms, although this study did not find all atherosclerically-linked factors are apparent risks." (or similar)

We have modified the abstract.

Discretionary revisions:

1. The prevalence of chronic RC tendinosis is still low as previously noted, but the authors are correct that many publications have similarly low prevalence estimates (thus above re. case definition is somewhat important).
2. The other underlying mechanism for obesity may simply be weight of the arm lifted. The reason obesity may be prominent in this study could be the combination of systemic and biomechanical factors. The authors may wish to include this information (please see prior critique).