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 1

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

TITLE: If this study is a gender study, the authors need to mention that in the title.

ABSTRACT:
Background needs some relevant information about why this study is important. Stating that “Shoulder pain is a frequent clinical problem” is not sufficient. The second sentence… We studied the associations of lifestyle and metabolic factors and carotid …” needs to be reworded as “the purpose of this study was to examine the association between …. The methods and results and conclusions need clarification and rewording.

We revised our aims.

Overall, the title and abstract do not sufficiently convey what is being investigated or found.

INTRODUCTION
The paragraphs are not linked and it is hard to establish a relationship between shoulder pain, metabolic syndrome and Intima-media thickness. I suggest you first describe “metabolic disorder”. For example: ...The metabolic syndrome is characterized by certain metabolic risk factors such as excessive fat tissue in and around the abdomen, high triglycerides, low HDL cholesterol and high LDL cholesterol, elevated blood pressure, insulin resistance or glucose intolerance and prothrombotic and proinflammatory states. Elevated C-reactive protein in the blood is a sign of proinflammatory. ….Certain diseases are associated with metabolic syndrome. These include heart conditions, aging, osteoarthritis, physical inactivity, and hormonal imbalance. Genetics appear to have a role in developing metabolic syndrome. ..... Then after explaining the prevalence of the shoulder pain and the cost associated with that, you mention some hypotheses about the link between metabolic syndrome and shoulder pain and the importance of investigating this link. Evidence shows that being overweight, diabetes,.. are related to higher prevalence of shoulder pain (reference). Ongoing inflammation and synovitis associated with osteoarthritis is associated with increased C-reactive protein and elevated levels of C-reactive protein appear to correlate with pain and stiffness .....or something like that. Therefore, the research questions and the justification for this study need to be defined better.
We have expanded the Introduction to better justify our study.

MATERIALS AND METHODS
The methods section is more organized and is described better. The term outcome is confusing here. I suggest: History and clinical examination.

In the analysis section you can mention that your dependent variable (outcome) was presence of Shoulder pain or diagnosis of tendonitis. The hypothesis should be after the purpose and not in the middle of the methods section.

The term “Determinants” is not very clear. Are these your independent factors? The authors need to mention how many factors they looked at and then talk about subcategories.

Intima-media thickness and US: Because the US was conducted on a subsample, it would be informative if you compare your full sample with the sample used for the US investigation and see if there are differences in the important factors such as age, sex, prevalence of shoulder pain, etc. (to see if the subsample is representative of your entire sample).

We used independent factors. However, we kept the "outcomes" to keep the structure of the methods clear. Clinical examination was used in the assessment of both outcomes and independent variables.
We have given information regarding the representativeness of subsample on page 5.

STATISTICS
Statistical analysis is based on the purpose of the study:
…The aim of this study was to assess the associations of lifestyle and metabolic factors and carotid IMT with shoulder joint pain and chronic rotator cuff tendinitis in the general Finnish adult population.

…..Statistical significance (p<0.05, two-tailed) was assessed by chi-square test for the categorical variables and by two-sample t-test for the continuous variables. What statistical significance? Who are in the two samples? Are you referring to men and women? If you are, this is not clear at all throughout the manuscript.

We revised this in the Methods, Statistical analysis section on page .

If this study is looking at gender differences in relationship between metabolic syndrome and shoulder pain, you need to clarify that in the title, purpose and the methods section.
Not clear what gender-specific multivariable models are?! It appears that a subgroup analysis was performed based on the gender. Please clarify.
It is not recommended to do subgroup analysis without first checking an interaction between the group and the outcome. This has potentially serious implications because erroneous identification of differential subgroup effects may lead to inappropriate provision or withholding of treatment in RCT studies. There
are clear guidelines on subgroup analysis for RCT studies. In observational studies, subgroup findings should be considered exploratory in nature. They usually do not affect the conclusions drawn from the trial. Exploratory subgroup analysis is helpful in exploring the strength and direction of the relationship between the independent factors and the outcome in each sex group. The authors need to clarify what factors went into the logistic regressions, they do mention age and sex but not other factors that they mentioned earlier (diabetes, IM thickness, smoking, …). Please clarify.

The first table seems to provide the demographic information on each sex which is fine. But I do not see any numbers related to statistically significant differences. I am not clear on how the logistic regressions were done. Was the shoulder pain and tendonitis (yes/no) the outcomes on separate analyses? Please clarify what is the dependent variable (binary?) and what are the independent factors for each analysis. You may want to add a table that includes sex as an independent factor and the rest of variables in relation to the outcome. Then if there is an interaction between sex and having a shoulder pain or tendonitis (which I expect), you progress to your subgroup analysis (men/women).

The manuscript therefore needs some clarifications and added tables to adhere to standards for clarity and proper data analysis.

We have tested for interactions (page 10). We have tested for differences in Table 1.

DISCUSSION
The discussion and conclusions need to be rewritten based on the changes made in the results section.

The limitations of the study need to be clearly stated.

We have modified our discussion and conclusion.

TABLES
My understanding is that the statistical significance of the odds ratio is related to the 95% confidence interval; a confidence interval that excludes an odds ratio of one will be statistically significant at p<0.05. Please indicate the level of significance in your tables.

For brevity we chose to present confidence intervals in the tables.
Reviewer 2

Lifestyle and metabolic factors in relation to shoulder pain and rotator cuff tendinitis: A population-based study
Martti Rechardt, Rahman Shiri, Jaro Karppinen, Antti Jula, Markku Heliövaara, Eira Viikari-Juntura

Conflict of interest: Jamie Gaida’s research topic of interest is the role that adiposity and metabolic factors play in the development of Achilles tendinopathy. This manuscript describes a population-based study of Finnish residents that examined whether adiposity and metabolic factors were related to shoulder pain or rotator cuff tendinopathy. The findings add important new information and will be of great interest to those working in this field. The quality of written English was in general outstanding, however, the last three paragraphs need improvement.

We have modified the last three paragraphs of the Discussion.

Major compulsory revisions:
1. Please add some detail on how blood was collected, processed, stored and analysed for triglycerides, glucose, insulin and hsCRP.

We added details on page 8.

2. Please add p-values to table 1.

We added p-values to table 1

3. Please justify why insulin resistance was analysed in tertiles while IMT was analysed per SD increase.

In the new analyses we use one SD increase for insulin resistance.

4. Limiting the analysis of IMT to men >60 has not been justified and detracts from the scientific rigour evident throughout the rest of the manuscript.

This was based on the median distribution of 45 to 74 years old subsample.

5. Please comment on whether validation of these findings is required in an independent sample.

Our study was a representative sample of the general population (page 5). However, prospective studies are needed (last sentence in Conclusions)

Minor essential revisions:
5. Please describe in more detail the distribution based cut-points for alcohol consumption.

We used three equal-sized groups: We made an addition in Methods on page 8.

6. Please describe (and reference) the method for measuring waist and hip circumference. Was it at the level of the umbilicus, at the narrowest point, halfway
between the lower costal margin and the iliac crest, etc?

We describe this on page 8.

7. How were the cut-points for waist circumference and WHR selected?

We added two references on page 8

8. “The diagnosis of diabetes was based on elevated fasting glucose..” What was the cut-point? Please add appropriate reference.

We added the cut-point value and the reference on page 8.


We added this reference on page 9.

10. Please add units for the calculation of HOMA-IR (e.g.. mmol/L and mU/L) and add reference

We added the units on page 9.

11. Please justify and reference the cut point for hsCRP.

High sensitive C-reactive protein was dichotomized into two groups, 3 mg/liter or >3 mg/liter, according to a recommended cutoff point for high risk of cardiovascular disease. We added a reference on page 9.

12. Please clarify in the section “Background characteristics” whether the findings mentioned were statistically significant between the men and women.

We added p-values on table 1.

13. Please avoid using the term “borderline significant”. It might to be more appropriate to state something like “although the results were non-significant, the 95% CI included odds ratios that would have clinical and public health significance”
We revised the text in para 3 on page 13.

14. Avoid using “obese” when describing waist circumference and WHR above the upper cut off. This is more commonly reserved to describe the higher levels of BMI.

We have modified the tables to avoid using "obese".

**Minor discretionary revisions:**
15. Consider using the term “tendinopathy” to describe clinically diagnosed rotator cuff pain without histopathological evidence of inflammation.

16. Consider using the terminology “action level 1” and “action level 2” for waist circumference

17. Add “HOMA-IR” in brackets after insulin resistance to table 1

Many thanks for the opportunity to review this excellent manuscript. I found it highly engaging and well written.
Jamie Gaida
Reviewer 3

Title: Lifestyle and metabolic factors in relation to shoulder pain and rotator cuff tendinitis: A population-based study
Version: 2 Date: 15 April 2010
Reviewer: Kurt T Hegmann

Reviewer’s report:
* Major Compulsory Revisions
1. The data need to be reported with univariate analyses. Without these, there cannot be reasonable assurances that the multivariate models were appropriately constructed from a statistical standpoint. If on the other hand, the variables were forced into the model without regard to statistical considerations, this is not wholly unsupportable, but needs to be stated as such and the univariate results should still be reported.

We reported univariable analyses for shoulder pain and chronic rotator cuff tendinitis. We added appendices 1 and 2.

2. There may be other issues that are major compulsory revisions depending on the univariate results.
3) Definitions of disease and exposures are not always as clear as they might be. This could impact the interpretation of the results and discussion.
Overall, the authors report on important issues in an under-researched area that has considerable morbidity. In this reviewer’s opinion, the research findings appear likely to merit publication after several issues are resolved.

We clarified our outcomes and determinants in the Methods section.

* Minor Essential Revisions
1. It is suggested the results and discussion need a bit better structuring. Until seeing the results of the univariate analyses, it is difficult to provide specific recommendations throughout, although some are able to be provided.
2. Abstract: The fact a diagnosis was made clinically is a bit worrisome to this reader if there was no structured approach to making the diagnosis, as a lot of subjectivity can be incorporated. As the methods in the text do not make this clear, it is difficult to suggest a solution. If the diagnosis was purely unstructured clinical examinations, then it is recommended this should be discussed in a new paragraph that either way it is suggested is needed in the discussion on ‘limitations.’

All subjects underwent a structured interview and a standardized clinical examination. This has been clarified in the Methods and the Abstract.

3. This reviewer believes these estimates of shoulder pain and RC tendinosis are low, thus I would seek additional language in the discussion for why that is the case. (FYI, our cross sectional analyses of approx 800 at baseline were 22-25% glenohumeral shoulder pain and 8.6-10.7% RC tendinosis at baseline depending on the side examined).
We defined shoulder joint pain as having pain in the preceding 30 days. Our estimate is similar to previous population-based studies. In addition, the criterion for chronic rotator cuff tendinitis was pain lasting for at least 3 months. We had strict criteria for chronic rotator cuff tendinitis.

4. Abstract: This reviewer’s reaction at this point is also that there are differences between the various purported associated factors, yet there is no cogent reason to expect this. (The later discussion section does not well address these concerns.)

5. Abstract: While I don’t dispute the last sentence as potentially true, the lack of positive findings throughout the results table for CV risks is a concerning. It would seem quite a bit more research is needed before being able to clearly support this view, the discussion section does not address this concern, and it is suggested this sentence be eliminated.

We revised the Abstract.

6. Methods. It is recommended the study design be specified. Presumably it is cross sectional (noted 1x in the discussion section).

We clarified the design of the study in the Methods section on page 4.

7. Methods: The methods for sampling are not very clear and another couple sentences are recommended to better explain the 2-stage sampling process so the reader knows how the population was constructed. Are people in Finland assigned to one hospital? If so, that also should be specified as it is important to understand the design and sampling.

We added additional information about sampling in the Methods section on page 4.

8. Methods: Figure 1. It is nice to have a flow chart. It is suggested there be side boxes to both account for the dropouts at each step, as well as to note the reasons for those dropouts.

We added side boxes for dropouts.

9. Methods: There are some questions that arise based on para 2. How can only 93 people have RA? This sounds like incomplete data to this reviewer (or are the Finns protected genetically from RA?). Missing shoulder disorder information is mildly concerning especially when added to the prior RA issue as presumably these were structured interviews (?) The term “qualified subjects” presumably means people included in the study (?)

Our exclusion criterion was based on the presence of rheumatoid factor and the clinical examination (N = 93). There were 257 subjects (3.5%) who reported that they had rheumatoid arthritis. However, self-reported rheumatoid arthritis is not reliable. Subjects may report osteoarthritis as having rheumatoid arthritis.
10. It is unclear throughout this document what is included in “shoulder” pain. Was a figure used with the people? If so, it is suggested it be included. There are many surveys that have aggregate upper arm, glenohumeral, scapular, trapzius, interscapular/paraspinal, nape of the neck and even neck pain in “shoulder” pain. What was included, as well as what was not included should be specified. (Text notes use of a manikin, yet it is unclear what the interviewer/examiner did with a person pointing to various locations…e.g., what did they do if someone pointed to the mid-upper trapezius?).

We clarified shoulder pain in the Methods section, paragraph 1 on page 5.

11. What was used to assure the examiners or researchers all used the same definitions of “shoulder pain”? The para references “trained” and perhaps this is intended to indicate structuring in the research protocol. Another sentence could help make this clear.

We added a text on quality assessment in the Methods section on page 5.

12. Is the outcome being evaluated in the paper prevalence of shoulder pain of at least 30 days duration? If so, suggest that be made clear throughout. Since the article also primarily analyses chronic RC tendinosis of at least 3 months duration, presumably questions were asked about duration of the pain as the question included in the manuscript would not yield those relevant data.

The questionnaire included an item on shoulder pain in the preceding 30 days (at least a day). In addition, during the clinical examination the duration of the symptoms was inquired. Symptom duration for at least 3 months was considered as chronic.

13. Outcomes, Para 3. The case definition of rotator cuff tendinosis should be specified (e.g., specific pain location(s) at least 3 months plus XYZ). Examination components should be specified. Was the examination standardized. If not, this weakness should be noted both here and in the suggested limitations para.

Standardized clinical examination was used.

14. Were the examination maneuvers performed in all subjects? Only those with symptoms? Up to the examiner? (these issues may explain the low prevalence rates as noted above).

As noted before the examination was performed in all subjects.

15. What defined the differences between “possible” and “probable?”

We modified the text and use the clinical definition according to physician's assessment.
16. Determinants, para 1. ?Structured interviews? What is an “occasional” smoker? Is it possible to categorize the pack-years for all current and former smokers? If so, and if the data suggest a lack of consistent, dose-response relationship across the data, this would seem to raise questions about the conclusion in the abstract.

We estimated pack-years only for current smokers. For former-smokers there were missing data on number of cigarettes smoked or number of years smoked.

17. Determinants, para 2. Were the distribution based cutpoints quartiles? If so, suggest using that term. If not, suggest explaining how cutpoints were determined. Light, moderate, excessive should be defined.

We revised the text on classification of alcohol consumption in the Methods section on page 5.

18. Determinants, para 3. This para, as well as above/below, suggest it may be helpful to append the questionnaire, as there are many variables included and they are not apparent in the results.

The questionnaire is too long. It is about 100 pages. We cited the Health 2000 Website.

19. Determinants, para 4. This population may be relatively homogeneous, but it would seem the waist circumferences should be adjusted for body frame (e.g., height adjusted).

The association between waist circumference and shoulder pain did not change after further adjustment for height. We included this additional result in paragraph 2 on page 8.

20. What percentage of the study population was covered by these criteria? That should be specified. If it was 1867/6354 (29.4%), then the percentage is so small that a careful review of the variables in Table 1 differ is required as a selection bias could be present (e.g., more ill people locating closer to medical facilities). If they do differ, it is suggested the table will need additional columns and statistical testing to define the differences. If not, then it is suggested a sentence to that effect may suffice and help clarify that that issue was examined and presumably the results are then generalizable to the rest of the study population.

The population of IMT study was representative of the Health 2000 population. In our previous report (reference 12) we compared the characteristics of the study population with those of original Health 2000 sample. We added a text on the methods section, Carotid artery intima-media thickness, paragraph 1 on page 7.
21. The statistical analysis section does not note clearly how the overall approach was conducted. Were univariate analyses performed? How were the multivariate models constructed? Did the authors have preconceived ideas and force variables into the model to produce the final models?

The associations of lifestyle and metabolic factors and carotid IMT with shoulder pain and chronic rotator cuff tendinitis were controlled for age (continuous), years of education (continuous) and physical work load factors (categorical). These factors were associated with shoulder pain and chronic rotator cuff tendinitis in univariable analyses. We added univariable results in the Appendices and revised the Statistical analysis section on page 7.

22. It is unclear how the physical factors were incorporated in the models, as there are multiple different categories and domains.

We used binary variables (no vs. yes) for current exposure to physical work load factors.

23. The results later specify unilateral vs. bilateral outcomes. What is unclear is whether this was by history or simultaneously they were required to have shoulder pain in the structured interview of BOTH shoulders to be a bilateral case (e.g., rather than a history of right shoulder pain 5 years previously and currently have left shoulder pain to also be a bilateral case).

In the present study we defined unilateral shoulder joint pain as having pain either in right or left shoulder during the preceding 30 days, and bilateral shoulder joint pain as having pain in both shoulders simultaneously during the preceding 30 days. We clarified this in the Methods section on page 5.

24. Results. Para 1. A key issue is how well this population mirrors the Finnish population. If unknown or if different, this is a limitation. This references comparison with "reference values" but what the comparison(s) is(are) or what those values are is unclear. Suggest changing the sentence a bit “….Women reported being more active…”

Health interview data were obtained from almost 90% and the majority of the health examination data from nearly 85% of the persons in the sample. Participation in Health 2000 was exceptionally good: counted on the basis of all persons for whom at least part of the information was obtained, the rate of participation was 93%.

25. Results. This reviewer believes it would be best if there was a table with univariate analyses presented after the demographic table. This would help the reader better understand the data and the subsequent model building. Without the univariate analyses, subsequent analyses of the results and discussion is a bit impaired.

We included two tables of the univariable analyses in the Appendix.
26. The results could benefit from some structure. For example, as currently structured the results discuss smoking at 10-20 pack-years is a risk for women is a risk, but more than that is not. This is not logical. It is suggested this lack of dose-response be clearly pointed out. Later, this should be discussed in the limitations. As currently structured, the results ramble a bit with multiple subjects in one paragraph.

There was no dose-response relation between smoking and shoulder pain. We discussed it on page 11.

27. Table 2. The occupational physical factors adjusted are unclear. The table’s footnote could help clear that up. What is unilateral vs. bilateral is unclear as noted in the statistical analyses section above. Definitions of the various conditions are suggested to be footnoted, similar to table 1 as suggested above.

We added footnotes and definitions in tables 1, 2 and 3

28. Discussion. As noted above, until univariate analyses are presented, the discussion suggestions below are somewhat preliminary. The discussion section would be improved with additional work and structuring. Most paragraphs mix topics, including outcomes, literature review, and conjecture, making it a bit difficult to read.

29. It would seem the biggest finding of this study is a lack of consistent support for the metabolic/vascular supply theory of RC tendinosis and shoulder pain that the authors’ approached in their hypothesis. This is not made clear in the abstract, results or discussion. It is suggested that this be the first para of the discussion, then adjustments made to the other sections.

We revised the first paragraph of the Discussion.

30. The first para’s first sentence does convey some of the primary findings, although the results are not consistent across all measures. The next sentence is a non-seq. One possibility is for this para to solely deal with the obesity issues. The next could deal with the other issues.

We revised that part of the Discussion.

31. This reviewer noted the issue of increased weight lifting being the strongest limitation for job physical performance in a series of 4 experimental studies, 3 in the peer-reviewed literature and all presented at PREMUS or IEA (Garg et al. Short-Cycle Overhead Work and Shoulder Girdle Muscle Fatigue. International Journal of Industrial Ergonomics. 2006;36:581-597; Garg et al. Maximum One-Handed Shoulder Strength for Overhead Work as a Function of Shoulder Posture in Females. Occupational Ergonomics 2005;5:1-10; and Garg et al. The effect of maximum voluntary contraction on endurance times for the shoulder girdle. Int J Ind Ergonomics 30:103-113, 2002). From that, we hypothesized that obesity was a potential risk for RC tendinosis due to increasing weight lifted,
performed a study and appear to have supported that supposition (Wendelboe et al. Associations between Body Mass Indices and Surgeries for Rotator Cuff Tendinitis. J Bone Joint Surg 2004;86A(4):743-747.). A recent excellent review by these authors is included in the reference list. However, inferences that the mechanism is solely vascular including from obesity is heavily discounted in the orthopedic literature, is controversial, remains to be proven and this text does not fully note these issues. (This reviewer is not seeking to have the above included in the reference list, rather merely trying to highlight these issues.)

32. The results for diabetes seem to be problems of small sample size, as most results are trending, but statistically negative. The text as written does not make that clear. Additionally, discussion about unilateral outcome with a systemic disease is inadequate as it should affect both. This issue applies through most of the results section. It would seem if the issue is these are modest risks, but the study was underpowered, then stating it that way will make the results more clear. As written, this reader was initially confused how DM would be a risk for one gender and not the other.

We have modified the Discussion.

33. One succinct paragraph on mechanism(s) of action may be helpful.

We discuss the mechanisms on para 2 page 16.

34. Para 7, last sentence is a non-seq. The first clause is probably correct, whether there is linkage with the last clause is speculative and suggest deleting.

We deleted the last sentence in para 7.

35. Para 8 contains conjecture on gender differences. There are robust data from psychophysical experiments that men have far stronger shoulder joints than women (approx. 2.4-fold depending on the exact task), and those differences are more marked than other joints (see, e.g., Snook, Ciriello data on maximum acceptable weights and forces). The key for this conjectural statement would seem to be the percentage of maximum voluntary contraction at which the worker is operating. (Additionally, in our cohort study, women tend to do the more repetitive work in plants that involves less force and men tend to do the infrequent, but high force activities. This raises a lot of additional questions, as we simply do not understand which job physical factors, let alone combinations, are most relevant for producing increased risk of RC tendinosis.)

To discuss this, we added a new sentence in the end of para 8 on page16.

36. A para on limitations is very much needed. A para on study strengths may also be helpful.

We added a new sentence to discuss limitations on page 16.

* Discretionary Revisions
1. Title: consider “tendinosis.” Evidence of true classic inflammatory mechanisms is lacking. Suggest same throughout the manuscript.
In all of our previous publications we used tendinitis and we wanted to use the same definition.

2. Osteoarthritis. Suggest changing to osteoarthrosis for same reasons.
3. Abstract: it is recommended the study design be specified.

We modified the abstract

4. Throughout the document, it is suggested “diabetes mellitus” be substituted for “diabetes” to be more precise.

5. Abstract: It is suggested it would be helpful for readers scanning abstracts to be able to see quantification of the major results in the abstract (to decide if they want to read the full paper).

6. Abstract: It would be best to have chronic (>3months) defined in parentheses in the abstract as various definitions are used.

7. Introduction: Overall, the length and breadth of the introduction are reasonable. As a general principle, this reviewer is not particularly fond of using systematic reviews for evidence due to the number of mistakes incorporated in them and evidence and knowledge are primarily derived from original data. Line 4, suggest “the shoulder joint” as trapezius pain may be more common. The sentence should be referenced.

8. Intro: Last line of the first para, acromioclavicular OA is quite a bit more common than glenohumeral. Also, suggest osteoarthrosis, rather than osteoarthritis for same reasons as above. Reference(s) needed.

9. Intro: First sentence, 2nd para, suggest softening the language as physical factors-related research is rather primitive for the shoulder at this point. Consider “…may increase the risk of…” Para 3, first two sentences need at least one reference. Last para of the introduction is a nice description of the hypothesis.

10. Outcomes. Para 1. Presumably, the question was of pain with motion. Tenderness is a term generally reserved for pain that is elicited with palpation.
11. Determinants, para 5. First sentence should define level of fasting glucose used to diagnose (X). “…or use of glucose lowering medications.” It is recommended the mmol/l values be translated into mg/dL or similar in parentheses. The last sentence presumably means a highly sensitive CRP (?)

12. Carotid intimal thickness. First sentence, suggest “Ultrasound measurements of carotid intimal thickness have been previously described [12].” Presumably, these measurements were performed blinded to disease status and if so, should be noted.
13. Statistical Analyses. The term “determinants” is used here and a few other places in the manuscript. Realistically, this apparently is a cross sectional study, so whether the paper identifies true risk factors, or disease determinants, is unknown. Consequently, it is suggested that terms such as “associated factor” be used preferentially. This is another item suggested for the limitations paragraph I
recommend be included.
14. This reviewer believes first person is generally suboptimal for scientific publications, although occasionally reasonable for the hypothesis or a concluding sentence.
15. Table 1. If appropriate, the title may be more clear to state “….free of a diagnosis of rheumatoid arthritis.” The Confidence Interval may be a suboptimal method of defining the differences between the data, as standard deviations are more familiar for many. Suggest metabolic syndrome, shoulder joint pain, RC tendinosis all get footnoted definitions to help the table stand alone. Are the data presented those of a history of DM? if so, suggest noting that. If incorporating serological testing, suggest noting that.
16. Discussion. Para 2, last sentence, considering the substantial limitations, suggest changing to “….may be better…”