Author’s response to reviews

Title: The impact of subacromial impingement syndrome on muscle activity patterns of the shoulder complex: a systematic review of electromyographic studies

Authors:

Rachel Chester (r.chester@uea.ac.uk)
Toby O Smith (toby.smith@nnuh.nhs.uk)
Lee Hooper (l.hooper@uea.ac.uk)
John Dixon (John.Dixon@tees.ac.uk)

Version: 3 Date: 25 January 2010

Author’s response to reviews: see over
Title: The impact of subacromial impingement syndrome on muscle activity patterns of the shoulder complex: a systematic review of electromyographic studies

Response to Reviewer’s Comments

We thank the reviewers for their constructive comments which have enabled us improve the quality of this resubmitted article.

Reviewer: Rune Mygind Mieritz

1) Major Compulsory Revisions:

Comment: The definition of SIS is unclear. What characterizes the condition? Why the condition is important? Is there in the literature a uniform description and definition of the condition?

Response: The introductory paragraph has been revised to include this information.

Comment: In relation to the “Assessment of validity” section. How is validity defined in the context? Define validity or refer to a reference.

Response: This section has been more appropriately titled “Assessment of Trial Quality” and the absence of any literature on the validity of the tool stated. The strengths and weaknesses of using a modified CASP tool are described in this section and this is revisited in a new paragraph within the discussion section.

Comment: In the abstract it seems like there are some modifications of the Critical Appraisal Skills Programme appraisal tool. How was this done and by whom and why is it not mentioned? If it is your intention to use the modified appraisal tool to assess/score the included articles then this should be mentioned. The criteria causing the score and the modifications should be described. It should be possible and relatively easy to evaluate the method.

Response: These points have now been described under the revised title “Assessment of Trial Quality” as described above.

2) Minor Essential Revisions

Comment: In the “Background section” I would consider applying further information about the reliability of the EMG equipment. This review focuses on validity and potential clinical relevance but in my opinion it is of higher relevance to investigate if the equipment is estimated as being reliable and reproducible.

Response: We have not added this text in the introduction, as one cannot generalise too much about reliability as discussed in comments below, and the additions to the discussion. We would not wish to suggest in the introduction that all studies in this field have a particular level of reliability when that is not known.

Comment: In another section in relation to the included studies (line 139- 140) it is stated “Three studies indicated that reliability was assessed” – Was it estimated as a reliable instrument then and how? Can this be used as a reliable tool/instrument as indicated by this review?
Response: Thank you. These questions have now been addressed in lines 205-214 and revisited in the discussion.

Comment: The presents or absence of any research concerning reliability, reproducibility of the equipment tested on the different shoulder muscles should be mentioned.

Response: In the discussion at lines 407-412 we have added “Only three [17,18,20] of the included studies assessed reliability. Regarding reliability it is important to be aware that much depends on the equipment and techniques used (e.g. indwelling or surface electrodes, surface electrode size), the experience of the data collector, and the outcome measures (e.g. timing or intensity) and how they are determined. Because methodologies differ between studies, findings from studies that have not assessed the reliability of their specific protocol and outcome measures should be interpreted with caution”.

Comment: Clarification of concepts should be stated in the background or method section (line 144 and 232).

Response: Concepts have been clarified in both the background section and the method section.

3) Discretionary Revisions

Comment: Related to the Aim/purpose: “The purpose of this study was to systematically review the literature to determine whether a difference exists in activation of the shoulder complex of people with SIS compared to healthy controls.” I think it is more appropriate to use the word examine instead of determine.

Response: This has been revised so that “examine” has replaced “determine”.

Comment: I would state the aim in the last sentences of the background section.

Response: This section has been revised so that the aim is stated in the last sentence of the background section.

Comment: Subheadings in the discussion could be preferable especially in relation to limitations.

Response: Subheadings have been added to the discussion section.

Comment: The conclusion is a little long and should in my opinion only answer the aim and the most important limitations.

Response: The conclusion has been shortened to address the aim and most important points

Comment: In line 19 patients misspelled (patents).

Response: This has been corrected.

Reviewer: Eric J Hegedus

1) Major Compulsory Revisions:

Comment: The title needs to be changed because the authors elected to exclude studies that had subjects with large and massive rotator cuff tears, which, by definition, are subjects with stage 3 impingement. The title should reflect stage 1 and 2 impingement as the subject of this review
Response: The introductory paragraph has been revised to include this information. Given the multiple methods of classifying subacromial impingement, we would prefer the title to remain as it is, as the authors of the included studies have done. We feel that this reflects clinical practice in which distinction is not always clear between the different stages of Neer’s classification. We have stated that our criteria excludes subjects with large or massive rotator cuff tears but not those with partial tears.

Comment: Under your section entitled “assessment of validity”, you do not note any disagreement between 2 independent reviewers when using the CASP tool. Not only will the readers not believe that there was absolutely no disagreement, I find it hard to believe also. For disagreement, how was it solved and who adjudicated a tie?

Response: This section has been revised to more clearly outline the process. Disagreements per se did not arise but uncertainties as to whether a point had been covered due to perceived lack of clarity in the text did. This was then discussed by the three assessors who reached a consensus quickly. One area where further clarification was required was whether participants in the control and subject group were matched. This involved discussion between all the authors and the footnote was added to table one for clarity and reproducibility.

Comment: Please comment, in 1 or 2 sentences on the validity/reliability of the CASP tool if, indeed it has been found reliable and valid. If it has not, then state this lack of validity/reliability as a weakness in your discussion section.

Response: The section titled “Assessment of validity” has been more appropriately titled “Assessment of Trial Quality” and the developments of our modified scoring system described. The absence of any data stating the validity/reliability of the CASP tool has been stated. This has been revisited in a new paragraph within the discussion section.

Comment: There should be some discussion/analysis of the findings of higher quality studies vs. lower quality

Response: This has been addressed with greater clarity throughout the text.

Minor essential revisions:

Comment: In the “validity” section of your paper, you talk about examiner bias. Should it not be in the discussion section as a weakness of the literature that you reviewed.

Response: Trial quality (the term we have used to replace “validity”) has been addressed in the discussion and examiner bias highlighted as a potential weakness of the literature we reviewed. We have also recommended that future studies address this important issue in the discussion.

Comment: Define Type II error for the reader as missing a relationship that may be present or some other such wording.

Response: Type II errors have been defined in the discussion section.

Comment: There are numerous punctuation errors, please go back through the paper and edit. In addition, the use of “it” as the subject in a sentence is not acceptable in formal writing (It is not correct.) but happens on lines 304, 317, 325, 352, and 363 to name a few.

Response: Punctuation errors have been addressed.

Comment: This as a subject is also unacceptable when there is no noun following (This leaves the reader unsure what this refers to). (please correct this error in lines 74, 270, 293, 298, 307, 346, 351

Response: When used “This” is now followed by a noun.
Comment: There are many missing citations. Find and correct! Some areas are line 151, 185, 213, 215, 246, 248, 249

Response: Citations have been placed more fully in the text.

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

Response: Language corrections have been made

Reviewer: Karen Søgaard

Major Compulsory revision

Comment: In the method should be stated how decisions were made regarding which studies to contact authors for including individual data. And maybe this should also be in Figure 1. This part of the study is not mentioned at all in the method section.

Response: The methods section has been amended to include criteria for contacting authors for requests for more detailed results. Eight groups of authors were contacted.

Minor essential:

Comment: The limitation of the EMG variables that relate to the lack of standardized normalization should be outlined more clearly as a limitation. Further it would be a help if this could be discussed in relation to the findings in specific studies. Could this explained opposite findings or the general results of lower activation? If MVC is decreased in the patients this will bias the normalized data towards higher relative values even though the same absolute activation is required for an external load. Also the issue of calculating external load and how the weight of the arm is handled is essential; if MVC is low then just lifting the arm will give a higher relative load. This also relate to the timing of activation, how is onset defined? Is it based on a relative threshold or an absolute level of activation?

Response: We thank the reviewer for these very helpful and important comments. We had actually discussed the normalisation issues at line 315-328 of the original submission, and we are glad that the reviewer agrees with us on this. We have now further expanded this text and made it into a separate paragraph earlier in the

“There are major points to consider regarding the interpretation of EMG intensity. Normalisation contractions, usually maximal, must be carried out to allow comparisons between groups [24]. For normalisation contractions, some studies have carried out one reference task maximally for the all muscles investigated, rather than carrying out separate contractions for each muscle, which is probably impractical. However this may mean that activation is not maximal in all muscles during this one task. Also, importantly, in people with painful conditions the interpretation of EMG data normalised to %MVC(EMG) needs careful consideration. If participants with SIS cannot or do not fully activate their muscles during the normalisation contraction, whether because of pain, inhibitory mechanisms, or avoidance, then %MVC(EMG) values may be affected [10,24] as the 100% levels are not true maximal values. If this only occurs in the SIS group and not the control group (or contralateral limb) as is probable, then it will be a possible confounding factor, inflating the SIS normalised EMG levels during functional activity, when the true effect is on the normalisation contraction. The paper by Brox [10] used this very point as a rationale for also analysing non-normalised data. This issue is often not discussed in the literature, but may be one possible reason for differences between studies included here, for example the findings of reduced lower trapezius activation by Cools et al [12], compared to the higher activation reported by Ludewig and Cook [4]. Therefore, again, we recommend that large studies in future with carefully thought out methodologies.”
Subsequent to this excellent suggestion, we have also added a paragraph on onset determination methods and how different methods may have affected results.

“Similarly, regarding EMG timing, different methods of determining EMG onset are available, and were in fact used in the included studies. Wadsworth and Bullock-Saxton [16] determined onset as the point at which the EMG signal exceeded 5% of its maximum amplitude, whereas Cools et al [11] used 10% of maximum as the threshold value. Here the onset point is clearly relative to the maximum signal amplitude. In contrast, Moraes et al [17] identified onset as the point at which the EMG signal exceeded the resting baseline level by over 2 standard deviations, so that onset is relative to baseline levels. Therefore it is possible that the differences between study findings on muscle recruitment timing, with some delayed activation observed by Cools et al [11], and Wadsworth and Bullock-Saxton [16], but not by Moraes et al [17], could be due to methodological differences.”

**Comment:** Discussion should be shorter, more focused on the limitations of the EMG variables in the studies that may explain the findings or make interpretation impossible.

**Response:** We thank the reviewer for these helpful points. To address this, we have re-organised the discussion, bringing to the front the paragraphs that were previously near the end, and expanding them as suggested.

**Comment:** Parts that could be omitting are the pain adaptation model in the upper part of page 11 (since no longitudinal studies were found) and the pain inhibition only referencing low back studies (bottom page 11).

**Response:** We have deleted these paragraphs as suggested.

**Comment:** Be consistent with terminology, the result of reflex response mentioned in discussion on page 10 is not mentioned in the results but may relate to the response time?

**Response:** We have addressed these terms in the first paragraph of the discussion and throughout the text to provide greater clarity.

**Discretionary Revisions**

**Comment:** Part of the discussion would serve better as background for doing the literature search.

**Response:** We agree and amendments have been made. For example the paragraph providing an example of how aberrant muscle activity during scaption may contribute to SIS has been deleted from the discussion section and added to the background section.