Reviewer’s report

Title: The reliability of three-dimensional scapular attitudes in healthy people and people with shoulder impingement syndrome.

Version: 1 Date: 25 January 2007

Reviewer: Paula M Ludewig

Reviewer’s report:

General

This manuscript addresses intersession reliability of scapular angular measures in healthy subjects and those with shoulder impingement. There is limited data in the current literature on this topic. I have identified areas of the manuscript that might benefit from clarification, as well as concerns regarding the axes definitions and generalizability of the results in specific comments below. Thank you for the opportunity to review this manuscript.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1) To understand the definition of the coordinate systems, readers need to refer back to a previous article. I believe a brief description of the axis definitions needs to be provided in this manuscript. From the picture I would have presumed x is perpendicular to the scapular plane, however from the previous article, I believe z is? I would also encourage the authors to reconsider if they want to retain these axes alignments. I realize this is a measurement method they have used in previous work. However, I do not believe it is accurate to state in the figure legend that the “rotations are defined in accordance with the ISB recommendations” when neither the axis alignments nor the rotation sequences are the same as the ISB recommendation, and this changes the numerical values. You have the same underlying scapular points digitized, so to the best of my knowledge you would be able to recalculate the data using ISB recommended axes and rotation sequences. Coordinating the terms is only one small piece of the issue. To the best of my knowledge, the axes alignments you use have not been adopted by other researchers outside of your group, so retaining the current alignments, the manuscript has uncertain generalizability to other more commonly used measurement methods. If wishing to retain these alignments, perhaps presenting the data both ways would allow these results to be of use to other shoulder researchers? Although the authors state on pg. 9 the sequence was the “optimal sequence” this is contradictory to the rotation sequence study by Karduna et al. As a shoulder researcher with a background as a clinical physical therapist, I must admit I have difficulty interpreting the values the authors axis alignments produce (Table 3). A 1st axis aligned between the posterior lateral acromion and inferior angle is obliquely placed on the scapula without obvious anatomical or clinical meaning to me? This oblique placement skews the remainder of the axes as well. Without generalizability beyond your group’s specific measurement approach, I am uncertain of benefit of the manuscript in the broader literature?

2) It is my understanding that it is generally not acceptable to consider sides as independent observations on subjects without statistically demonstrating their independence (pg. 10)? In particular, both sides of the impingement subjects analyzed together when one side is impaired is confusing and difficult to interpret. It seems each side needs to be handled seperately, or only one side chosen. Also, what was the rationale for these sample sizes?

3) In the results the authors begin to refer to reliability as being higher or lower under different conditions or between groups. In order to determine if these point estimates are actually different, 95% confidence intervals should be provided for all ICC and SEM values, and these “difference” characterizations only made where confidence intervals do not overlap. Also both ICCs and SEMs should be provide for all conditions, and both would have to agree to indicate reliability is higher or lower for a condition or group. Why are ICCs provided for both the trunk and rest scapula method but not SEMS? If ICCs are lower but SEMS equivalent, this would suggest that the between subject variation was what changed between groups or conditions and not the repeatability of the measurement. This issue needs to be considered and discussed in interpreting the data.

4) Some of the very low ICC values in the figures should be highlighted in the results and or discussion text. I believe the second sentence of the discussion and other summary statements in the manuscript and abstract are overstatements considering some ICCs in the .4 range.

5) The SEMs in Table 2 are not consistently better for the 8 symptomatic shoulders, so I don’t see how it can be concluded that this group is different or has a more restricted choice in motor strategy? That may be
one possibility if in fact a consistently higher reliability can be supported after confidence intervals and SEMs are considered. However, additional issues such as the limited symptomatic sample, gender differences, age differences, etc need to be considered and discussed with equal consideration.

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

1) In the abstract, please quantify “good to very good between sessions” with the ICC ranges as you did in other areas.
2) Given the similarity of your SEMs and lack of confidence intervals for the ICCs, I do not believe the authors can yet conclude your one group reliability was superior to the other in the abstract. These apparent ICC differences are possibly a function of less between subject variation in the healthy group. See previous comments above.
3) Given the various use of terminology in the literature, I think the authors should define terms for scapular rotations early in the manuscript (pg. 4), as well as alternate terms. For example, lateral rotation in US terminology is an axial rotation, and upward rotation is the alternative standard term. I do not think you should use US terminology, just clarify how these terms relate, as well as your previous terms that you identify later of pg. 9.
4) Pg. 4, 1st line of last paragraph refers to differences across studies but includes only one reference?
5) On the top of pg. 5, I believe it would be helpful to indicate and refer to the ISG/ISB standardization recommendations here.
6) Pg. 5: It would be helpful to briefly clarify what are the fixed marker comparisons referred to.
7) Pg. 6. The reason given that the researchers chose to measure 70° flexion and 90° abduction was because those positions “could explain as much as 91% of the variance of the pain and disability level experienced by people with SIS” in reduced posterior tilting along with five other variables. The reference cited for this is a non-published Ph.D. thesis written in French. The selection of these positions for measurement should be further clarified/justified. Additionally, the lack of known generalizability of this data to other test positions or motions needs to be clearly identified rather than just the very brief mention at the end of the discussion.
8) Please clarify if the examiner was blinded to group status or past values when retesting?
9) Please clarify on pg. 7 what point was digitized as the “mid-upper arm” and how bony landmark measures were used to verify posture?
10) Please clarify on pg. 8, once the scapular landmarks are identified in one position are they re-palpated in the other positions, or are skin markers used? Also please clarify if the flexible template is “locked” into position once the landmarks are found?
11) I am confused about the 2 versus 3 trials in the results for the healthy subjects. The methods refers to the results on this yet it is not further identified in the ICC results. Are these results of 2 or 3 trials? What is the “same” level of reliability?
12) The figures should state 70° flexion and 90° abduction rather than just flexion and abduction.
13) The short list of limitations in the discussion needs to be expanded to discuss and explain these various issues

Discretionary Revisions (which the author can choose to ignore)

1) Given different terminology used in various literature, it would be helpful to clarify for the reader early in the manuscript that scapular “attitudes” are equivalent to angular positioning of the scapula.
2) Pg. 4 “three planes of movement” seems redundant with “around three different axes”. As these motions are not really planar, I would recommend just including the second part of that sentence about different axes. In that same sentence, do you mean optimizing length tension of glenohumeral rather than scapulothoracic muscles? Statements such as this generally refer to deltoid length?

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

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

Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:

I declare that I have no competing interests.