# GUIDED REVIEW PROCESS: SPECIFIC COMMENTS

Wilfred CG Peh

Singapore Medical Journal,
National University of Singapore,
Khoo Teck Puat Hospital, Singapore

Khoo Teck Puat Hospital
Alexandra Health

# INTRODUCTION

- Structured reviews help authors
  - appreciate the major points regarding the submitted manuscript
  - address concerns raised by reviewers in a point-by-point manner

# INTRODUCTION

- Structured review: components
  - General comments
  - Specific comments

Khoo Teck Puat Hospital
Alexandra Health

[redacted] "Lesions of the biceps pulley: Diagnostic accuracy of MR arthrography of the shoulder and evaluation of established and new diagnostic criteria."

General comments:

Summary:
This retrospective study uses three reviewers to independently evaluate 28 patients with biceps pulley lesions, and 58 controls with normal biceps pulley, by assessing eight signs on MR arthrography at 1.5T and 3T, including a novel 'displacement sign', using an arthroscopic gold standard. The authors also analyze two sub-cohorts of the controls: pts with and without abnormalities of the rotator interval. They demonstrate good intra- and inter-rater agreement, and conclude that MR-A is accurate in diagnosing pulley lesions, with approximately 85% sensitivity and 94% specificity, and that the displacement sign, discontinuity of the SGHL, and tendinopathy of the LHBT are the most accurate signs. Presence of abnormalities in the rotator interval minimally decreased the specificity of MR-A in dx of pulley lesions.

Strengths:
1. Well-designed study with control arm and arthroscopic gold standard.
2. Independent observer image analysis with intra- and inter-rater assessment.
3. Presentation of a new sign to assist in dx of pulley lesions, which the authors prove to be of high sensitivity and specificity.

Weaknesses:
1. Retrospective design, meaning that surgeons were not blinded to MR findings.
2. Inclusion of unknown number of cases from two MR platforms (1.5 and 3T) which could make confirmation of authors' findings difficult, esp at 1.5T.
3. Uncertain role of the 'supplementary material' in the MS.

Implications for patient care:
These are, as written, very similar to Advances in Knowledge. Suggest that the authors re-read the 'Instructions to Authors' and re-address.

Specific comments:
Abstract:
1. Suggest including in the Abstract that both 1.5T and 3T platforms were used.

# SPECIFIC COMMENTS

Consists of

- Review of each section of manuscript
- List concerns or disagreements with statements made
  - must provide specific reasons

# SPECIFIC COMMENTS

## Headings

- Title
- Abstract and keywords
- Introduction
- Materials and methods
- Results

Khoo Teck Puat Hospital
Alexandra Health

# SPECIFIC COMMENTS

## Headings

- Discussion
- References
- Tables
- Illustrations (including legends)

Specific comments:

Abstract:

1. Suggest including in the Abstract that both 1.5T and 3T platforms were used.
2. The 'supplementary material' is not included in the Abstract (or Discussion). This data should either be part of the whole paper, or be deleted. I favour the latter.

Introduction:

3. The authors do not mention the transverse humeral ligament which admittedly many authors consider an extension of the subscapularis tendon. It may be worth clarifying that this structure, although not part of the intra-articular pulley, does serve as an extra-articular anchor to LHBT.
4. The authors should consider adding the CHL to figure 1 (an otherwise excellent figure), as it represents an important part of the pulley.
5. Wieshaput et al (authors' ref # 14) also refers to 'extra-articular contrast collection' as one of the MR findings of LHBT pulley lesions. Does this reflect a tear in the rotator interval? The current authors don't mention this in the Intro of Discussion as a useful sign.

Materials and Methods:

6. How many cases were performed at 1.5T and how many at 3T? Were any differences noted between the platforms?
7. The authors stated that they used the 'anterior approach' for MR-A, but should probably specify, given the nature of the study, that they did not use the common rotator interval approach for injection.
8. I am surprised that the spatial resolution was the same at 1.5 and 3T in this retrospective study. Why didn't the authors increase the through plane and in-plane resol'n at the higher field strength?
9. Need to record ETL for TSE sequences, and NSA for all.
10. The accepted terminology I believe would be 'oblique coronal' rather than para-coronal' if that is the authors' intended meaning. Same for 'para-sagittal'.
11. In terms of criteria for diagnosing LHBT tendinopathy, the authors state they made the dx '….if changes in diameter and/or increased signal were seen' (page 9, lines 32-4). Not only are these criteria somewhat arbitrary, but with the short TE sequences for MR-A, significant magic angle artifact would be expected to spuriously increase signal in the tendon, depending on plane of section and course of tendon. Could the authors please comment?

Results

13. In the 'Arthroscopy' section, the authors refer to the Habermeyer classification, without referring to Table 1 where it is explained.

14. Why did the authors not exclude the 2/80 cases in which the image quality was rated as 'poor'?

Discussion:

15. Wrong syntax for page 15, line 8: 'disclose' should be 'exclude'.

16. Same for 'Conclusion': MR arthrography is 'objective'??

17. Given point #6 above, do the authors have any advice about whether 3T is a better platform for MR-A in diagnosing these pulley lesions? Could readers with only 1.5T systems expect similar diagnostic performance, as that reported in this MS, using the authors signs?

18. If tendinopathy is as highly sensitive and specific as the authors indicate, and doesn't require MR-A to diagnose, does this, in the authors' opinion, diminish the need for MR-A?

References:

19. The authors might want to consider adding Gaskill et al Arthroscopy 2011; 27: 556-67, an excellent review of rotator interval pathology.

Figures:

20. The authors indicate in M and M that fat-suppression was used for the T1 weighted images, however none of the figures appear to show fat suppression.`

# SPECIFIC COMMENTS

## Identify

- Important but missing items
- Contradictory statements
- Reviewer’s own limitations
  - i.e. be honest!

Khoo Teck Puat Hospital
Alexandra Health

"Magnetic Resonance Imaging Findings and Clinical Features in Localized Scleroderma."

General

1. The manuscript is not formatted in accordance with Radiology Publication Information for Authors.

Introduction

2. Most radiologists think of scleroderma in terms of systemic scleroderma (systemic sclerosis). They are aware of the musculoskeletal characteristics of cutaneous calcification, bone resorption, contractures, etc., seen in patients with systemic disease; however, they are not aware of the localized forms of the disease. The educational value of the manuscript would be improved if the Introduction included a brief explanation of the rheumatological classification of the systemic scleroderma, defining both the diffuse and localized forms. It would be important to use an accepted classification and that used by the American College of Rheumatology is recommended.

3. It is also important to explain to the reader in the Introduction why the MR imaging of localized scleroderma is important. Without this, why would one think the manuscript is worth reading?

4. The specific purpose of the manuscript needs to be definitively stated in the Introduction. Radiology Publication Information for Authors specifically states that the final paragraph of the Introduction should clearly state the hypothesis and purpose of the study in a fashion similar to the Purpose statement in the abstract.

Methods

5. Should this section be titled Methods and Patients or Methods and Materials?

6. Page 4, line 14: The term "deep LS" is not defined and the reader does not know what this refers to.

7. Of the five types of juvenile localized scleroderma described in the authors' reference 11 which follows the use of the term "deep LS," none are designated as "deep LS." Terminology needs to be precise and consistent.

Methods

5. Should this section be titled Methods and Patients or Methods and Materials?

6. Page 4, line 14: The term "deep LS" is not defined and the reader does not know what this refers to.

7. Of the five types of juvenile localized scleroderma described in the authors' reference 11 which follows the use of the term "deep LS," none are designated as "deep LS." Terminology needs to be precise and consistent.

8. Page 4, line 26: Competency is assumed on the part of the dermatologists participating in the study. The credentials are not required.

9. Page 4, line 54: Eosinophilic fasciitis is not listed in the classification scheme of the authors reference 11. I think a discussion of eosinophic fasciitis as a variant of localized scleroderma is beyond the scope of the current manuscript and should be deleted. While the two entities have some similar imaging features, the American College of Rheumatology classifies eosinophic fasciitis as a scleroderma-like disorder. Certainly, the rheumatologists I work with would agree with this and would note that there are several distinctions between the two entities, not the least of which is the peripheral eosinophilia. Eosinophilic fasciitis has been addressed by a number of prior publications in imaging journals. It would seen to confuse many to include it here as part of localized scleroderma.

10. MR imaging: How many diagnostic MR imaging sequences were acquired for each patient? It seems there were only three: axial STIR, axial T1 or GRE and axial fat-suppressed enhanced T1. Is that correct?

11. Page 5, paragraph 1: Joint contracture is noted as one of the features assessed clinically. The assessment of joint contracture is not included in the imaging protocol or in the Results. It should be excluded from the Methods section unless completely evaluated.

12. Page 5, paragraph 1: Greater detail on the determination of the LoSSI score should be included.

13. Page 6, line 9: As with the dermatologists, competence is assumed on the part of the radiologists and their experience need not be included.

[redacted] entitled "Magnetic Resonance Imaging of Osteoradionecrosis of the Upper Cervical Spine Following Radiotherapy for Head and Neck Cancer: Differentiation from Recurrent or Metastatic Disease

General Comments

(1) P-values of "0.000" should be listed as "< 0.001". P-values between 0.01 and 0.99 can be rounded to two digits after the decimal place.
(2) Given the sample size, percentages presented in the Results section (e.g. 10.26%) can be rounded to the nearest whole percentage point (e.g. 10%).

Specific Comments

Abstract:
(3) It would be reasonable to state that 35 patients (not 39) were included in this study as having met the study eligibility criteria with a diagnosis of ORN or recurrence via follow-up studies or biopsy. The 4 patients who were excluded do not contribute much information to this study or manuscript.

Materials and Methods:

Patient Enrollment and Grouping:
(4) Second sentence: List how many patients had undergone irradiation for head and neck cancer. Specifically of interest is the number of such patients who had a follow-up image, but did not have evidence of recurrent tumor or ORN. If the number of patients were substantial, it would be worth considering presenting some basic descriptive information about this third group.

# Statistical reviewer

Statistical Analysis:

(5) 'Independent t-test' would be more accurately described as a two-sample t-test (or Student's t-test). Were equal or unequal variances assumed? Several factors in Table 1 appear to differ in the variability by group.

(6) In the results section, the authors summarize the survival characteristics of patients in this study. It would be useful to define the period of time for which survival was considered, as some survival data were censored and/or the patients may have died elsewhere. For example, the authors might consider providing a 5-year survival rate post-RT treatment. Alternatively, the survival characteristics may be more usefully described with Kaplan-Meier survival curves (with time-since-RT as the starting point).

Results:

(7) The authors present the survival rate in the ORN group as 17/20. This is incorrect as they do not have survival information on 2 patients (e.g. due to censoring). Also, it's often conventional to present follow-up times as medians in lieu of means.

Discussion:

(8) In the limitations paragraph: it is sufficient to state that because the sample size was small, the authors were not able to adjust the comparisons for any confounding factors. Discussion of logistic regression or multicollinearity is not necessary.

Table and Figures:

(9) Table 1: Please provide ranges for the follow-up period and total RT dose.

(10) Table 1: Under symptoms it is noted that 5 patients in each group were asymptomatic. How/when were these patients identified? As a part of routine imaging surveillance?

(11) Table 2: Please provide column-based percentages for each category.

(12) Table 2: An asterisk indicating the footnote should be added to the 'P value' column.

(13) Table 2: The footnote indicates that a Pearson Chi-Squared test was utilized, but the Statistical Methods section notes that in some cases a Fisher's Exact test was used. Was Fisher's Exact test used for any factors in this table?

# EDITOR & REVIEWERS

## Editorial decisions

- Selection of reviewers'

  comments are sent to author

  – numbered & edited

Aim is to help author improve manuscript - regardless of outcome

## RADIOLOGY Decision Notice

A decision of "Accept with Revisions." has been rendered on manuscript

RAD-11- entitled "Position, Shape, and Orientation of the Long Biceps Tendon of the Shoulder in the Bicipital Groove in Neutral Position, External and Internal Rotation in Asymptomatic Volunteers."

Please find the Reviewer comments below.

Sincerely,

Herbert Y. Kressel, M.D.
Editor

Reviewer comments:

Reviewer: 1
Comments to Author
With regard to the previous review, the authors have addressed the preceding comments, although not in an optimal fashion.

1. Concerning the intricate system of ligaments and pulleys that stabilize the biceps tendon, the authors have inferred "normalcy" of all of these structures and their relationship with the LBT with the following statements on page 6:
"All MR examinations were evulauated by a musculoskeletal radiologist prior to the measurements. Volunteers with lesions of any kind...would have been excluded from the study. However, no volunteers had to be excluded."
This implies that these structures were not part of a systematic review, which is not ideal if the authors are attempting to distinguish normal features from those that contribute to instability of the LBT.
In fact, were the images of these 53 voluteers part of a prospective study of the LBT or were they culled retrospectively from "normal examinations"? If this large number of volunteer images was obtained prospectively, what was the purpose of obtaining so many sequences if the sole focus was on the axial images?

2. Concerning the criteria for distinguishing normal from LBT subluxation, the only statement the authors have made is on page 12:
"Because of this big variability, a far medial LBT position of 8 mm medial to the deepest point of the bicipital groove would be required if the diagnosis of LBT subluxation was being considered."

3. On page 14, the authors refer to the study by Spritzer et al, which stated that "...a flat, degenerated biceps tendon perched on the lesser tubercle with an obtusely angled bicipital groove should raise the suspicion of instability..." of the LBT. The authors indicate that their data is "...partly in agreement with these findings," but that these findings can also be physiologic. Specifically, what is there in the authors' data that is "partly in agreement" with Spritzer's findings?

4. The authors define the superior level as "the most superior image that included the subscapularis tendon..." Concerning Fig. 5, please label the subscapularis tendon in each image. Compared to Fig. 1, there is a definite difference in the appearances of the subscapularis. Specifically, is the subscapularis tendon normal in Fig. 5?

5. To be commended, the authors have tackled a controversial issue in terms of defining what constitutes features of a normal LBT from those associated with LBT instability and subluxation, but they have left the reader with more questions and little clarity on these issues. For example, if there is a degenerated LBT perched on an obtusely angled lesser tubercle in the absence of a subscapularis tear, is this the same as one associated with a tear of the subscapularis? Would the authors not acknowledge that the position, shape and orientation of the LBT should be interpreted in the context of the other findings that are believed to contribute to LBT stability?

Reviewer: 2
Comments to Author
General comments
The authors prospectively studied MR images of the shoulders of 53 volunteers with respect to the position, shape and orientation of the long head of biceps tendon (LBT) in neutral, external and internal rotation positions. They found that the LBT was only slightly dependent on shoulder rotation, that LBT eccentricity is maximal in the neutral position, and that rotation does not increase LBT eccentricity.
This study is generally well performed and documented. The scope of this study is rather narrow but it does add incremental contribution to what is currently known about the LBT.

Specific comments
Advances in knowledge: okay
Implications for patient care: okay
Results:
Suggest that the authors show images of change in shape of the LBT at different levels and with different positions in the same patient.
References:
These are not in Radiology format and are inconsistent.

Reviewer: 3
Comments to Author
The authors used MR imaging to evaluate long biceps tendon (LBT) with respect to position, shape and orientation in 53 asymptomatic volunteers (21-58 years of age). They assessed inter-observer reliability and used Wilcoxon signed-ranks tests to determine changes in LBT for external and internal rotation compared to the neutral position at three measurement levels (superior, middle, inferior) and concluded that position of the LBT is not highly dependent on shoulder rotation and that rotational misplacement during image aquisition should not lead to incorrect diagnoses of LBT subluxation or displacement.

Comments

1. In general it appears that interobserver agreement is good regarding the position and orientation of the LBT.

2. Tables 3 and 4: Evaluating the percentage of tendons changing their shape (round, oval, flat, comma) between different external rotation or internal rotation versus neutral is difficult in terms of choosing the most appropriate statistical test. Wilcoxon signed-ranks tests were used however this is probably not great for paired categorical data. Nevertheless, despite the large amount of information in this table, it is clear that changes in shape were most frequently observed at the inferior level of measurement, and curiously in Table 4 it was the inferior level where the worst interreader agreement was found.

3. Table 5: Why do the authors use ICC values for assessing agreement in LBT orientation and weighted kappa for shape? It seems that this is because orientation is more of a continuous variable and shape is obviously categorical. This should be made clear to the reader.

4. In general the authors have tried to summarize their data as best as possible, although have only used simple statistical tests for their comparisons rather than a multivariable approach to determine which variables influence position of the LBT (i.e., shoulder rotation, level, age, gender). This could be done using ANOVA or a multivariable regression model although my suspicion is that, while a preferred statistical strategy, it might be difficult to interpret and summarize any better than has already been done.

# Decision notice

Khoo Teck Puat Hospital
Alexandra Health

# SUMMARY

- Reviews are ideally structured
- Specific comments
  – list each area of concern precisely
    - with supporting reasons
  – identify missing items
  – point out contradictions
- Aim to improve manuscript

Khoo Teck Puat Hospital
Alexandra Health