Reviewer’s report

Title: Arm rotated medially with supination: Description of its surgical correction

Version: 4 Date: 7 October 2008

Reviewer: scott H kozin

Reviewer’s report:

MANUSCRIPT REVIEW

“Arm Rotated Medially with Supination: Description of its Surgical Correction”

ABSTRACT:

1. The abstract is in structural form. The authors spend a great deal of time on background methods and conclusions. However, the results section very short. Additional information is necessary, especially since the major portion of the manuscript involves the forearm segment.

2. Throughout the manuscript the authors discuss medial rotation contracture of the arm. I think this term may be confusing to the readership. Most orthopaedic surgeons refer to this entity as an internal rotation contracture of the shoulder. This contracture positions the arm into internal rotation.

3. At the end of the background, the authors indicate that medial rotation contracture and supination will continue to worsen without surgical intervention. This is not true as the deformity may stagnate and not progress.

4. Within the first sentence, the authors discuss that there is a high incidence of musculoskeletal complications of initial nerve injury. I believe the authors mean after the initial nerve injury.

5. The conclusion section is entirely too long. Much of this material belongs in the discussion section and not in the conclusion section. Most of the material is conjecture and is not based on the results of this particular manuscript.

BACKGROUND:

6. The authors did delete the acronym ARMS deformity from the title. It appears the authors just attempted to placate the previous critique. I still believe the acronym ARMS should be removed. In fact, the authors are really describing an internal rotation contracture of the shoulder with a fixed supination deformity of forearm.

7. Within the background on page 4 the second sentence, discusses that the SHEAR is present in the majority of patients exhibiting intro rotation contracture of the arm. The authors further discuss the abnormal twisting of the clavicle and the tilting of the entire acromioclavicular plane. This needs additional referencing.
8. On page 4, the authors also discuss awkward lateral rotation of the arm. The authors need to consider whether they are referring to external rotation, and not really lateral rotation. I know this is being picky, but it’s important to stay consistent throughout the manuscript.

9. On page 5, the authors discuss recognition of the contracture about the shoulder and about the forearm segment. The second paragraph uses the term “quite” which needs to be removed. Again, the acronym ARMS deformity is used, which I don’t think is relevant to the manuscript. The authors make a point of combining these two entities into a single acronym, but I really don’t see the usefulness.

METHODS:

10. On page 5, the authors discuss the position of the volar of the forearm segment. They indicate that the forearm volar surface is usually perpendicular to or over-pronated in relation to the anterior arm surface. I am confused by this statement. I believe the authors discussing a supinated segment of the forearm and not an over-pronated position.

11. On page 6, the authors discuss their 8 of 14 patients at sufficient follow-up. This is an extremely small numbers of patients, especially since most of them did not have radiologic follow-up. Furthermore, there remains confounding factors including the previous surgeries that were listed. The authors also indicate that the patients were treated ethnically in compliance with the Helsinki declaration. Does this mean that there was no IRB?

12. On page 6, the authors indicate that all measurements were performed by a trained scientist. What does a trained scientist mean?

13. On page 8, the authors discuss the evaluation of active arm and shoulder movements. They indicate that the angle of form supination was recorded with zero being the neutral position. Was this a fixed deformity or was some passive motion?

14. On page 8, the authors discuss that the shoulder was corrected at the first stage and the supination correction at several months later to complete this surgical sequence. Several months were vague and the authors need to provide more information.

15. On page 9, the authors discuss their surgical procedure about the scapular. The authors indicate that there was “semi-rigid fixation” of the clavicular osteotomy segments to prevent nonunion. The term “semi-rigid” is confusing and were there any nonunions?

16. The authors also indicate on page 9 and in the discussion that 1 patient was too old to undergo triangular tilt, and underwent osteotomy. Within the discussion section, they indicate that the patient had a low SHEAR and age related increase ossification. This needs a much better explanation.
17. On page 9, the bottom paragraph, the term “metaphysis” is misspelled.

18. On page 10, the authors discuss the technique for radial osteotomy using a Steinmann pin. If the osteotomy was made in mid-forearm, then rotating the forearm from supination and pronation would prevent alignment of the bony ends. Furthermore, the authors need to explain their choice of a Steinmann pin, which provides little stability. Lastly, was there any consideration of osteotomy of both bones?

RESULTS:

19. The result section has a lot of pictures but still lacks substance. The important perimeters are the Mallet grading and the supination deformity.

20. On page 11, the authors indicate the preoperative Mallet evaluations were conducted before the first bony surgery (i.e., triangular tilt, humeral osteotomy, or forearm osteotomy). My understanding was that the shoulder was operated on first and the forearm second. If so, this contradicts previous statements.

21. On the bottom of page 12, the authors discuss the angles for supination. This section is extremely confusing. Previously, the authors indicate that the forearm segment was based on zero being neutral. From their numbers based on page 12, the authors indicate that the preoperative mean position was 5° and postoperative 34°. Does this mean, the only correction that they obtained was 29°? In addition, they started from 5° than they started from mid-position and not a supination posture.

22. On page 13, the authors discuss in their comments disregarding the radiologic grading based on the few postoperative values. I concur and believe this segment should be removed.

DISCUSSION:

23. The first paragraph discuss that the deformity impairs utility of extremity in significant ways. The term “significant” denotes statistical significance and this should be changed to “considerably”.

24. Throughout the discussion section, the authors use a variety acronyms and I think these should eliminated.

25. On page 14, the authors discuss the development of a supination posture, even in children with weak pronator teres and pronator quadratus. At the bottom line, they discuss excessive supinator and biceps muscle activity. Do the authors infer a passively correctable or fixed deformity of the forearm in the patients described?

26. On page 15 as discussed earlier, the authors discuss the single patient that was treated with the humeral osteotomy. I still don’t understand why this patient was chosen for this particular operation.

CONCLUSION:
27. The conclusion is still too far reaching. The acronym ARMS is also used. The last paragraph makes too gradiose conclusions. I think the authors should temper this last paragraph.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**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'