Reviewer’s report

Title: Arm Rotated Medially with Supination (the ARMS Deformity): Description of a New Clinical Entity and its Surgical Correction

Version: 2 Date: 16 June 2008

Reviewer: scott H kozin

Reviewer’s report:

MANUSCRIPT REVIEW

“Arm Rotated Medially with Supination (the ARMS deformity): Description of a New Clinical Entity and Its Surgical Correction”

SUMMARY:

The article entitled “Arm Rotated Medially with Supination (the ARMS deformity): Description of a New Clinical Entity and Its Surgical Correction has been reviewed for publication in BMC Musculoskeletal Disorders. In addition, I reviewed a previous article entitled “Surgical Correction of the Medial Rotation Contracture in Obstetric Brachial Plexus Palsy published in the British Journal of Bone and Joint Surgery in 2007. After reviewing both these articles, my concerns regarding this article are expressed in the point by point basis. In its current form, the article is not acceptable for publication.

In summary, the article attempts to define a new clinical entity and its surgical correction. However, this is not really a new clinical entity and the patient numbers are quite small. The authors make a point that it is uncommon to have this combination of internal rotation of the shoulder and supination of the forearm. The authors may be right, but give very little evidence to support this position. Furthermore, the number of patients is quite small and the follow-up is even less. My particular concerns are expressed below:

ABSTRACT:

1. The abstract is written in structural form. The authors have previously described SHEAR deformity in previous publication. The authors now attempt to add an additional acronym for arm rotated medially with supination (a.k.a. the ARMS deformity). I think this acronym represents jargon and its better just to describe the deformity as a fixed supination deformity of the forearm. This would comply with previous publications regarding supination posture of the forearm.

2. The methods section in the abstract should include more detail regarding the number of patients. This is listed in the results section but the demographics should be moved from the result section up to the methods section.

3. The results section in the abstract indicates that glenohumeral anatomy was also improved, but not statistically significant. I not sure this belongs in the results
section, but better left in the manuscript as it is non-significant.

BACKGROUND:

4. On page 3, the authors indicate there are two common secondary deformities are the medial rotation contracture of the arm, and supination deformity of the forearm. The authors have previously stated that the supination deformity of the forearm is uncommon and here they stated it as common. This needs to be clarified.

5. The SHEAR deformity has recently been described by these authors and should be better delineated in the background. Most readers will be unfamiliar with the SHEAR deformity and a better definition describing this finding is necessary.

6. On page 4 the authors write that it is important to know that apparent loss of supination in patients with MRC is actually a manifestation of a misaligned humeral head related to the glenoid fossa, which rotates the wrist and hand into a resting position that is medial of neutral. This sentence is awkward and needs to be better written. The authors should state that the internal rotation contracture of forearm masks the inability to supinate.

7. On page 4 the last paragraph uses the term “disability”. This should be changed to impairment.

8. On the bottom of page 4 and the top of page 5, the authors indicate that triangle tilt operation has been shown to be anatomically and functionally superior to traditional techniques. This sentence has very little evidence and is based on a single article in 2007 that was reviewed. The article makes no comparison to other techniques and therefore this statement is too strong.

9. On page 5 the authors attempt to define the imbalance across the forearm resulting in a fixed supination posture. The standard anatomy needs to be re-reviewed. The pronator teres and pronator quadratus are not solely innervated by C7. Certainly the pronator teres is the C7 muscle, but the pronator quadratus is innervated by the anterior interosseous nerve, which has substantial lower C8-T1 innervation. Furthermore, the authors needs to be better explain why the patients listed in Table 1 had primarily upper trunk involvement, especially patients 1-5.

METHODS:

10. The methods includes a series of patients from 2005-2006. In a previous publication the authors had patients between February and August of 2005 that underwent triangular tilt operation. Were any of these patients included in the first publication? If so, this needs to be mentioned.

11. On page 6, the authors indicate that the sequence is recognized clinically by the parallel positioning of the volar surface of the forearm to the anterior surface of the arm. I’m assuming that they are talking about the supination posture of the forearm as it relates to the arm. This needs to be clarified.
12. The authors indicate on page 6, that to the untrained eye the hand is a neutral position and symmetric with the uninjured side. This is condescending and needs to be toned down.

13. On eight of the fourteen patients had sufficient follow-up data for the study. In addition, when you peruse Table I there is even less follow-up available. Patients 3, 5, 7, and 8 were all missing radiographic data that would be important to substantiate the authors’ conclusions.

14. A variety of patients had previous surgeries including mod quad, nerve grafting, posterior glenohumeral capsulorrhaphy and biceps tendon lengthening. The authors relate the improvement in glenohumeral alignment in their small number of patients to the triangular tilt. However, the posterior glenohumeral capsulorrhaphy and modified quad may have changed glenohumeral alignment. This needs to be at least discussed in more convincing terms.

15. On page 7 the authors discuss measurements of glenohumeral joint and SHEAR deformity. The authors spent a great deal of time on how to measure subluxation and how to measure glenoid version. However, no time was spent on how to measure SHEAR deformity. When one reads the 2007 article, there is a similar critique. The authors at least need to have a diagram demonstrating the measurement technique for SHEAR deformity.

16. The authors indicate on page 8 and 9 that they use a variety of scales and surgical technique designed by the senior authors. I don’t think that the senior authors should be listed as Nath’s modified Mallet scale, but rather just modified Mallet scale. In addition, the adding of a variety of subcategories may affect the reliability of this measurement. Have the authors done any reliability measurements on these additional subscales?

17. Similarly, on page 9 the authors indicate that the lead author has developed a novel and effective osseous procedure, named the triangle tilt. I agree this is a novel procedure with single publication has been noted. I think the authors should simply state that a novel osseous procedure called the triangular tilt has been developed, which has shown an earlier promising results.

18. On page 10, the authors discuss the second stage surgical correction was a derotational osteotomy of the radius and intramedullary pinning. There are no details concerning where the osteotomy was performed or radiographs. If the authors main point is to describe the combination of internal rotation contracture of the shoulder and fixed supination deformity, than this would be valuable information to the readership.

19. The authors show a variety of pictures of patients before and after surgery. The authors did include one patient humeral osteotomy based upon her age. The authors comment on page 6 that the osteotomy was done because of her advanced age. If the authors should define why they have an age cut-off? This would be useful information.
20. On page 12 the authors have a footnote that needs to be referenced. It is in the second paragraph, and needs to be taking out of ( ).

21. On page 12 the authors indicate the angles of supination deformity significantly increased, as did the Mallet scales for supination also increased. This is confusing and I may have a misunderstanding. My understanding is once the arm was placed into external rotation, the forearm was corrected into pronation. If one accesses the Mallet scales for supination, I don’t see how these can improvement following a rotational osteotomy into pronation. Please explain.

22. Radiological criteria – Table 1 show the results of CT and MRI evaluation. Table 1 also shows considerable missing data. There so little data within Table 1, that I’m not sure any conclusion can be made. In fact, patient 1 showed a percentage increased in SHEAR following surgery. Patients 2, 3, 4 and 6 decreased and the remaining patients have no values at all. I think the authors need to temper their radiologic results.

23. Both the discussion and introduction are too long. Both sections need to remove some extraneous material and get to the point. The introduction section should simply introduce the problem and the discussion section should compare the previous published results.

24. On page 14, the authors indicate that they described for the first time the simultaneous occurrence of the medial rotation contracture and supination deformity of a group of patients with a history of brachial plexus birth palsy. I don’t think that this is the case. The authors would better indicate that they make note of bringing attention to their simultaneous occurrence. Certainly, we have all seen this combination of deformities in our clinical population.

25. Again, I think the acronym ARMS needs to be removed. It does not provide any additional information, but rather is just a jargon.

26. On page 15, the authors indicate that there are variety studies that correct supination posture of forearm. If one of the points of the article is that rotational osteotomy allows for correction of this deformity, then this needs to be expanded. The authors need to provide more information as to the following:
   a. Radial osteotomy was performed
   b. Techniques of fixation
   c. Amount of correction obtained.
   d. Information regarding how they determined how much correction was necessary

27. On page 15, the authors indicate that they have successfully used a triangular tilt procedure to correct the shoulder in hundreds of patients over the last four years. The previous report in 2007 only includes forty-four patients and the others have not undergone peer review. Therefore, this statement needs reference only those patients that have undergone adequate peer review or
follow-up. If the authors truly have hundreds of patients that have undergone the triangular tilt, then those numbers should be reported in a separate manuscript.

CONCLUSION:
28. The conclusion section is fairly well written. I agree that the shoulder should be corrected prior to the forearm. This section is the best written section of the entire manuscript and should be used as a platform to rewrite the introduction and the discussion sections in a shorten fashion.

Level of interest: An article of limited interest

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
no