Author's response to reviews

Title: Arm rotated medially with supination - the ARMS deformity: Description of its surgical correction

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Author's response to reviews: see over
Response to Reviewers:

Dr. Bahm:

The only remaining comment is about the relationship between SHEAR and MRC.
On page 2, Abstract- Background, line 5 and 6, the author states that Shear normally appears through MRC- it could be understood that SHEAR is the common term for the shoulder deformities, including all MRC features. This might be too general and dangerous, as the concepts are still under discussion. SHEAR concept is a novel and very valuable concept in some obpp shoulders, but I see a lot of MRC without SHEAR. This sentence in the abstract might be too dogmatic and does not add value to the present article.

We agree that MRC occurs in the absence of SHEAR and the relationship between the two is, as yet, undefined. This sentence has been changed.

Dr Kozin:

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.

Abstract was rebalanced.

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.

Medial and lateral rotation are equivalent terms to internal and external rotation. We understand that the former are more commonly used in British English while the latter are more commonly used in American English. We did use both in the manuscript, and have changed all occurrences of these terms to “medial” and “lateral” for consistency.

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.

Even a stagnation in progression of the deformity leads to functional consequences which continue to worsen. The condition never improves without intervention. We have changed the wording to remove ambiguity.

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.

“of” was changed to “stemming from”

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.

Although the conclusions section of the abstract did not contain conjecture and there is no discussion section to the abstract, the section has been shortened as part of the rebalancing addressed in query 1.

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.

We still believe that the implication of the interplay of these two deformities in the same patient is important enough to warrant a specific term, ARMS. The medial rotation contracture of the shoulder and fixed supination deformity of the forearm, together, lead to functional consequences which are more complicated than those from either alone. And the combination has never been described previously, leading to missed diagnosis and treatment possibilities by caregivers. We are hoping to draw attention to this with the publication of this manuscript.

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.

References were added.

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.

We have changed all instances of external to lateral to stay consistent. See also our answer to query 2.

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.

“quite” was removed.

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.

In someone unaffected by supination deformity, these two are perpendicular. In the patient affected by supination deformity, these two are parallel. In this section we are comparing what one would see in someone without SD to what one would see in someone with SD. This was further clarified with the addition of a few words.

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?

We believe that we are able to describe the ARMS deformity and our treatment approach with this small number of patients. It is not a common combination of deformities (14 out of 121 patients with MRC were also diagnosed with SD) and not all parents elect to have both surgeries performed. Because the sequelae of obstetric brachial plexus injury are complex and multiple, our patients were previously treated for various nerve and muscle conditions like adduction contractures and biceps contractures using established treatment methods. None had previous bony surgeries. These bony surgeries are the treatments which we are concerned with in this manuscript. We have based this study on chart studies, and have not sought IRB approval as we would with a prospective rather than retrospective study. Our institute treats our patients ethically (consistent with the tenets of the Helsinki declaration), and parental understanding and consent are attained before any treatment is undertaken. To maintain privacy, no identifying patient details are included.

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

Scientifically trained personnel (Bachelor or Doctoral degrees) who have been additionally trained to read and measure shoulder images from CT and MRI studies.

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?

As mentioned elsewhere in the paper, forearm supination was fixed due to the forearm contracture. The act of supination observed was a lateral rotation at the shoulder which leads to apparent supination when the position of the palm is observed. When we see a change in supination angle, this reflects changes in the lateral rotation at the shoulder and any active supination of the forearm together. Most of the observed changes are due to an improvement in rotational positioning (resulting from the surgeries) of the shoulder and forearm rather than any changes in the active range available.

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.

The dates are provided in Table 2 and range from 4 to 8 months.

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?

There were no nonunions of the clavicle in this study. We shortened the surgical methods, because they are published elsewhere. Details on the semi-rigid fixation are available in reference number 17.

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.

It is not clear what needs explaining. These are the two reasons the lead author decided on this treatment option.

17. On page 9, the bottom paragraph, the term “metaphysis” is misspelled.

Corrected.

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?
Alignment was not prevented, and the osteotomy was made distal to the mid-forearm (approximately the junction of the distal and middle thirds of the diaphysis). The Steinmann pin is used to maintain alignment of the bony ends. Rigid casting is important to maintain the chosen pronation position during bone healing. Osteotomy of both bones was not deemed necessary, since the desired rotational correction could be obtained with the radial osteotomy.

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.

The photos visually show the improvement in one key Mallet score movement, all of which are also provided in the table. The photos also show the changes in apparent supination in a way that we are unable to describe as succinctly. Please see Table 2 for the key results. We did not see a need to repeat them both in the text and table.

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.

This is not a contradiction. The first Mallet evaluation was performed before any bony surgery (“pre-operative” in Table 2). The final Mallet evaluation was performed after both bony surgeries in 5 cases and after the first (shoulder) bony surgery in 3 cases (“post-operative” in Table 2). All shoulder surgeries were performed before any forearm surgeries. The bony surgeries performed and their relative dates are also provided in Table 2.

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.

Yes. This is the point of the paper. These children with supination deformity have apparently neutral supination. The full extent of the supination deformity is masked by the medial/internal rotation of the shoulder. When first the shoulder rotation is corrected (the apparent supination shows the full extent of supination deformity now), and then the supination deformity is corrected, the end result is a small (not statistically significant and not intentional) increase in supination of only 29°. In order to clarify the crux of our paper, two sentences were added to the discussion and the term “apparent supination” was used in the table.

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.

These sections of the text and tables have now been 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”.

“Significant” was removed.

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

OBPI, MRC and SD are used throughout the obstetric brachial plexus injury literature. They have been introduced in this paper in their full form before using the abbreviations. ARMS is new terminology that is also well-explained in this paper before relying on the abbreviated version. If the journal style favors the use of full terms, we are happy to make this change if needed.

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?

It seems that the muscle imbalance will lead to a passively correctable deformity in some patients, and to a fixed deformity in others. We do not have enough information to speculate further.

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.

The reasons are provided. The humeral osteotomy is the current treatment used by most practitioners when addressing the medial rotation contracture. Both methods rotate the shoulder more laterally.

CONCLUSION:
27. The conclusion is still too far reaching. The acronym ARMS is also used. The last paragraph makes too grandiose conclusions. I think the authors should temper this last paragraph.
Our conclusion is not intended to be grandiose. We want mostly to state that awareness of the possible combination of supination deformity with medial rotation contracture is important to developing a treatment plan.