Reviewer’s report

Title: Preoperative radiographic and clinical factors associated with postoperative floating of the lesser toes after resection arthroplasty for rheumatoid forefoot deformity

Version: 1 Date: 08 Aug 2018

Reviewer: Kade Paterson

Reviewer’s report:

**Please note that I am submitting this reviewer's report on behalf of an external reviewer who requested to remain anonymous**

Comment to authors

BMSD-D-17-01347R1

The preoperative first ray condition is a risk factor for recurrent dislocation of the lesser toes in both columns after resection arthroplasty for rheumatoid forefoot deformity.

Recommendation: Reject

The paper provides a lot of tables and statistics, but I found very little useful or novel information. Moreover, I found some leaps of logic. This made me to consider the manuscript unacceptable, and I will recommend to resubmit it after addressing the suggestions below.

The critical points to address are:

1) The authors concluded that pain is a risk factor for recurrent dislocation of the lesser toes. How could they derive this conclusion without providing any information about pain in results? The results they got was just the significant difference of JSSF hallux scale, which is not equal to the significant difference of pain scale.

2) The authors used the term "recurrent dislocation of the lesser toes"; however, I think it as misuse of words. As the authors referred in methods section, we cannot judge dislocation in the resected joints. The authors defined the dislocation of MTP joint as non-grounded toes while weight-bearing. This condition is a "floating toe" but not a dislocation. I will recommend to choose the right word.

3) Line 183-184: How did the authors can lead the conclusion that the recurrent dislocation group had more severe "deformity" preoperatively than non-dislocated group? The significant difference in JSSF hallux scale is not equal to the significant difference of "deformity", isn't it?
4) Line 189-190: In this part, the authors lead the conclusion of the significant difference of "pain" from the significant difference in JSSF hallux scale in contradiction to Line 183-184. I will suggest to provide the breakdown of the each subscale of the JSSF scale.

5) Line 210-211: How could the authors conclude that adequate bone resection is required to avoid recurrent MTP joint dislocation from the result of no significant correlation between bone resection length and recurrent MTP dislocation? More careful logic flow is required.

6) Line 223-224: The authors did not provide any information about the range of motion of first MTP joint before surgery in the result section; therefore, this part is not acceptable as discussion.

7) Line 228-231: Why is it reasonable to suppose that preoperative hallux pain result in shifting the center of pressure laterally in the postoperative status? Did not pain go away after arthrodesis? It is partially acceptable if you could provide the correlation between postoperative hallux pain scale and recurrent dislocation of the lateral lesser toes, but too much gap in logic to accept as it now stands.

Additional Questions or comments:

1) The authors should add the information of MTP arthrodesis of the great toe in the title of the manuscript and abstract, otherwise the readers will mistakenly perceive this manuscript as one regarding resection arthroplasty of all 5 metatarsal heads.

2) Statistics: Which statistical method was used for analysis of patients' demographics? Mann-Whitney U test or unpaired t-test?

3) Statistics: Unpaired t-test is not appropriate for analysis of ordered scale such as preoperative dislocation grade.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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Please indicate the quality of language in the manuscript:

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No