

Response to reviewers

Manuscript NO: 57053

Title: " Proximal tibial osteotomy for genu varum: Radiological evaluation of Deformity correction with plate versus external fixator."

Dear Editors,

Thank you very much for the comprehensive review of our manuscript.

We are grateful to you and the reviewers for the insightful comments. We have read these comments carefully and implemented changes in the manuscript, whenever possible. The manuscript has been revised according to these comments and detailed below.

Round #1

Reviewers' comments:

Reviewer 1:

I read with interest your article submitted to the WJO. This article elicited a number of comments. 1 the methodology is good, the objectives are clear and the scientific contribution is indisputable. However, the fundamental ethical question for me is to know what are the clinical criteria which make you decide on the surgical indication in these patients who certainly have a morphological deformation but very little radiological osteoarthritis as can be seen on the figures. The morphotype of the general population is distributed according to a Gauss curve and only the extremes are at risk without being able to define the borderline exactly. All your study is based on a radiological evaluation and not a clinical one which in my opinion minimizes the relevance of this manuscript. It would be advisable in the discussion chapter to specify what are for you the clinical and radiological symptoms that make you decide an indication for a tibial osteotomy.

Thank you very much for your thorough review. We have revised our manuscript in accordance with the reviewer's suggestions, as detailed in the responses below. We thank the reviewer for these comments and feel these amendments have strengthened the manuscript.

The reported indication for the HTO surgery is varus alignment of the knee combined with any of the situations such as medial compartment osteoarthritis, medial compartment overloading (like post medial meniscectomy), knee instability or osteochondral lesion. (Bonasia)
The indication for HTO surgery in our study was varus deformity greater than 5 degrees and knee pain.if there was radiographic medial joint space narrowing, then goal was overcorrection.if there was no radiographic medial joint space narrowing, then goal was neutral alignment.

Bonasia DE, Dettoni F, Sito G, Blonna D, Marmotti A, Bruzzone M, Castoldi F, Rossi R. Medial opening wedge high tibial osteotomy for medial compartment overload/arthritis in the varus knee: prognostic factors. Am J Sports Med. 2014 Mar;42(3):690-8. doi: 10.1177/0363546513516577. Epub 2014 Jan 21.

We added to the discussion part. We thank the reviewer for raising these points and feel the addition helps improve the manuscript and provide a more balanced piece for our readers.

2 For a clearer understanding you have to draw a synthetic figure with all the angles and deviations you measure as presented in fig1 for CD Index for example.

Per the reviewer's suggestion, we have added some figures regarding the measurements for more clarification.

3 please precise what are your routine recommendation after surgery in each group (partial or full weight bearing, day of discharge, who is in charge of the manipulation of the EF to modify distraction?)

thank the reviewer once again for a grateful comment. we added the post op plan in detail based on the precious reviewer's comment.

"In post-operative follow-up, the OWHTO patients were evaluated in clinic 2 weeks after surgery, 6 weeks, and then monthly until 6 months after surgery (Figure 2). The patients were non weight bearing for 6 weeks after surgery and then they were transferred to the Partial weight bearing. "

(In Gradual genu varum deformity correction with monolateral external fixator procedure)
"Adjustment was evaluated by x-ray 1 week after starting the distraction, and the distraction gap was reassessed by the senior authors."

"The patients were non weight bearing for 2 weeks after surgery and then transferred to the partial weight bearing. These patients were evaluated in clinic every month until frame removal"

4 who was responsible for the radiological evaluation in this study?

"all the measurements were done by limb reconstruction fellowship trained surgeons."

5 according to your results you assess that no differences are found between the 2 methods? Actually your series is not randomized and a selection bias could be argued.

We can observe for example that the BMI difference is statistically different between EF and IF. Is the patient status or any other criteria important to make your choice?

Actually as it was mentioned, our paper was a retrospective, nonrandomized study. In our past and current practice the BMI is not considered as a criteria to determine the surgical plan for HTO. Also, we mentioned there was not any significant difference in BMI between the two main groups IF and EF, (Table 1). However there were some differences in the subgroups (Table 2) we noticed the reader of this limitation of the study at the end of the paper and we suggested a study with a larger number of the patients with clinical evaluation would be helpful in clarification of the result of both techniques.

“As mentioned above, there were some demographic differences between some subgroups and also we evaluated the radiological outcome of two techniques . Future works would be helpful if they evaluate these results in a larger number of patients. Also, a comparison of the early and late clinical outcomes in both groups with a larger number of patients would be helpful.”

What are your current indications?

There is no change regarding the indication for HTO in our practice. The indication for HTO surgery is varus deformity greater than 5 degrees and knee pain. In the presence of radiographic medial joint space narrowing, overcorrection is the goal and if there is no radiographic medial joint space narrowing, then the goal would be neutral alignment.

thank the reviewer once again for grateful comments.

Reviewer 2:

Dear Authors, The report of your study is sound and meets the criteria for a scientific manuscript. I have no specific comments except the SF-36 analyses. For me, the very differing follow-up periods in both groups of your patients should be mentioned in the limitation section of your discussion. Average follow-up time says little and may be misleading when the range is 14-86 or 8-62 months. You clearly focused on the surgical technique side of your study, while the quality of life analysis was far more superficial. Some discussion of the SF-36 findings should also be included so that person centered outcomes are more pronounced in your report. There is no presentation of SF-36 in the methods section.

We are very grateful to the reviewer for insightful comments and excellent suggestions. We thank the reviewer for raising these points. We agree there were some limitations in our study. Actually the goal of this study was radiological evaluation of the two techniques retrospectively and we wrote the method for radiological evaluation in the method and material part. We mentioned in the part of the result about SF-36 scores and our limitation to evaluate

the clinical outcome however the radiological outcome was the goal of the study. We have added some details in limitation based on the valuable reviewer's comment for more clarification.

"As mentioned above, there were some demographic differences between some subgroups and also we evaluated the radiological outcome of two techniques . Future works would be helpful if they evaluate these results in a larger number of patients. Also, a comparison of the early and late clinical outcomes in both groups with a larger number of patients would be helpful"

thank the reviewer once again for grateful comments.

Reviewer 3:

Authors did not discuss about the indications of the medial open-wedge operation. The X-ray of two cases appeared to be of mild deformity. Symptoms and degree of osteoarthritis were not recorded. Other options of treatment e.g. supra patellar tendon lateral osteotomy were not discussed. The indications of surgery were not clear.

We are very grateful to the reviewer for insightful comments and excellent suggestions. We have added some sentences to the discussion to clarify the indication of the surgery. As mentioned in the title, reporting the radiological outcome of the two techniques of HTO was the goal of the study. So the clinical outcome of the study and changes in the symptoms of the patients were not evaluated in this study and we have added this limitation in detail at the end of the paper based on the precious reviewer's comment.

*"The reported indication for the HTO surgery is varus alignment of the knee combined with any of the situations such as medial compartment osteoarthritis, medial compartment overloading (like post medial meniscectomy), knee instability or osteochondral lesion. (Bonasia)
The indication for HTO surgery in our study was varus deformity greater than 5 degrees and knee pain.if there was radiographic medial joint space narrowing, then goal was overcorrection.if there was no radiographic medial joint space narrowing, then goal was neutral alignment."*

"As mentioned above, there were some demographic differences between some subgroups and also we evaluated the radiological outcome of two techniques . Future works would be helpful if they evaluate these results in a larger number of patients. Also, a comparison of the early and late clinical outcomes in both groups with a larger number of patients would be helpful"

We also added some X-rays for more clarification. thank the reviewer once again for grateful comments.

That said, if the reviewer could kindly suggest any specific statements that are unsupported, we would be more than happy to re-examine and re-address in accordance with their feedback.

Where specific areas for concern were highlighted, we have revised our manuscript in accordance with the reviewer's suggestions as detailed in the responses.

Round #2

Response to RE-REVIEW REPORT

Manuscript NO: 57053

Title:" Proximal tibial osteotomy for genu varum: Radiological evaluation of Deformity correction with plate versus external fixator."

Dear Editors,

Thank you very much for the comprehensive re-review of our manuscript.

We are grateful to you and the reviewer for the insightful comments. We have read these comments carefully and implemented changes in the manuscript, whenever possible. The manuscript has been revised according to these comments and detailed below.

RE-REVIEW REPORT OF REVISED MANUSCRIPT

Dear Authors, Thank you for your very kind reply. Nonetheless I find the content of your reply, as well as following amendemends to the paper, unsatisfactory. You argue that Actually the goal of this study was radiological evaluation of the two techniques retrospectively and we wrote the method for radiological evaluation in the method and material part. We mentioned in the part of the result about SF-36 scores and our limitation to evaluate the clinical outcome however the radiological outcome was the goal of the study. We have added some details in limitation based on the valuable reviewer's comment for more clarification. Once again, if your goal was radiological evaluation, why you presented SF-36 in the results section? Or, if you wish to present those findings, the tool should be described in the methods section, and the results need to be discussed. It cannot remain only as an accompaniment to the report. Otherwise those findings are uninformative and misleading. I cannot find any comment to the SF-36 findings in the 'limitations' part, as you have indicated in the reply to the peer-review.

Thank you very much for your thorough re-review. We are so sorry for misunderstanding and we did not address the comments well in previous revision. We have revised our manuscript in accordance with the reviewer's suggestions, as detailed in the responses below. We thank the reviewer for these comments and feel these amendments have strengthened the manuscript. We agree completely with the reviewer that the goal of the study was radiological evaluation of the two techniques and we wrote the method for radiological evaluation in the method and material part in detail. Also we agree there were some limitations in our study regarding clinical outcome however the radiological outcome was the goal of the study. We have removed the uninformative and misleading paragraph regarding the SF-36 and clinical outcome in the result part.

Per the reviewer's suggestion, we have added specific third part in the limitation regarding the clinical outcome for more clarification.

“Third, this study was a radiological comparison of the two techniques and a study for comparison of the early and late clinical outcomes in both groups with a larger number of patients would be helpful to determine the clinical performance of these techniques”

We thank the reviewer for raising these points and feel removing the misleading paragraph and mentioning the limitation help improve the manuscript and provide a more balanced piece for our readers.