Author's response to reviews

Title: Closing-wedge high tibial osteotomy: survival and risk factor analysis at long-term follow up

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Author's response to reviews: see over
“Closing-wedge high tibial osteotomy: survival and risk factor analysis at long-term follow up”

Dear Editor-in-Chief:

Thank you very much for your valuable comments and critiques. They helped us to substantially improve our manuscript. We have explained our revisions below. Our changes are shown in **bold type** in the manuscript.

**Reviewer 1:**

**Major:** None  
**Minor:** None

**Discretionary Revisions:**

1. A narrower age range would have been nice. Since as stated in the introduction, the best candidate for a HTO would be a young active patient. Perhaps the inclusion of the 70 year old patients was attempt to increase the numbers?

   Only three patients were older than 70 years. We agree that the best candidate for HTO appears to be the younger, active patient. However, there is no age limit for osteotomy and therefore patients should not be excluded based on chronological age alone (Trousdale RT 2002; Marti RK et al. 2001, Dowd GSE, 2006).

2. You describe the complications, but fail to attribute them to a specific patient...Ie. were the non-unions in the smokers, older patients? It would be nice to clarify.

   Please see last paragraph of the results section:  
   “9 patients had non-union of the osteotomized tibia and 8 of the fibula which required reostheosyntheses with bone grafting. Comparing obese and non-obese patients, no significant difference in complications was noted (p=0.37). 6 of the 21 smokers had postoperative complications (3 non-union of the tibia, 2 non-union of the fibula, and 1
deep vein thrombosis). There was no significant difference in complications between smokers and non-smokers (p=0.54)."

3. A power analysis would have been nice. I believe the lack of finding significance in your pre-operative risk factors is due to the study being under-powered.

This may be possible. Group size is often an Nevertheless, there are few studies that include long-term data of 199 patients, which makes our study one of the largest on patients after HTO. Moreover, in retrospective studies, power analysis is unusual.

Level of interest: An article of limited interest
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no competing interests.

Reviewer 2:

I would like to compliment the authors of the above manuscript on the beginnings of a good article. They present a large cohort of subjects who have undergone an orthopaedic procedure which has been gaining much needed attention. High tibial osteotomy has a growing role as an alternative treatment for young middle-aged adults with localized osteoarthritis of the knee. With the population living longer and maintaining an active, athletic lifestyle, joint-preserving procedures are needed in order to reduce the overall rate of early total knee arthroplasty which will also reduce the rate of early revision total knee arthroplasty. The authors present data to further promote the use of high tibial osteotomy which will help to accomplish this goal. With that said, there are several issues currently holding this article back from its full potential.

Major Compulsory Revision
1. First, and foremost, there are many grammatical errors throughout the paper. This can probably be attributed to the article originally being written in German then translated into English for review. Be that as it may, the whole article needs a thorough proofreading prior to publication. Two examples of this are immediately found in the Background section. In the first sentence, “…high tibial osteotomy (HTO) has become an accepted procedure…” should
read, “…high tibial osteotomy (HTO) is an accepted procedure…” The second to last sentence where the authors try to state the aim, or purpose, of the study does not make sense when compared to the overall read of the paper. Is the paper about failure rates or survival rates of HTO?

Thank you very much. The manuscript has been revised by a native English speaker.

Minor Essential Revisions

2. Maintain consistency in reporting throughout the paper. In the Abstract the mean follow up time is reported to be 9.6 years however, in the body of the manuscript the Methods section reports mean follow up to be 10 years. Which one is it?

The mean follow up time is 9.6 years. We revised this figure throughout the manuscript.

3. Consider removing the text referring to 15-year survival rate in the Results section. Given that the mean follow up of this cohort is 10 years, the addition of this data clouds the conclusions the authors are trying to reach. Or, preferably, expand upon this by providing more data. Specifically, provide the numbers of patients who had a mean 15 year follow up. Lastly, consider expanding upon what exactly is meant by “survival rate”. To some survival may mean how long a patient reports a good to excellent clinical result over conversion to another procedure.

Survival rates were determined applying the Kaplan-Meier test with its known limitations, especially since only a fraction of the patients reached >15 years of follow-up.

We added:
“The 5-year, 9.6 year, and 15-year HTO survival rates as determined by Kaplan-Meier analysis were 93%, 84%, and 68%.”

4. The strengths and weaknesses of this study are not clearly discussed at the end of the manuscript. The authors mention they feel the patients they could not report on due to loss to follow up was a weakness. Though that is true, they still had 83% of their cohort to report on which is better than many articles which have been published in “higher tier” journals. This is
an obvious strength of their study and should be emphasized. There are other weaknesses such as this being a retrospective study and there being no control group to compare to which can be discussed.

We changed the respective paragraph as follows:
There were several limitations to our study, including its retrospective design and lack of control group. There were a significant number of patients who were lost to follow-up, although over 80% of patients could be included, which is satisfactory when compared with other studies in this area [18, 24].

5. Again, the Discussion section is fraught with grammatical errors which need to be addressed as the strengths and weaknesses are further delineated.

Revised

Discretionary Revisions
6. If it is possible, it would further strengthen this article if there were a cohort of medial opening-wedge HTOs or unicompartmental knee arthroplasty patients to compare to. This is not a requirement but could further strengthen the case for more surgeons to utilize a lateral closing wedge HTO for medial gonarthrosis of the knee. Also, if there is arthroscopic data on the condition of the articular cartilage at the time of surgery and, whether the grade of articular cartilage wear correlated with survival or patient outcome.

We discussed the alternatives to lateral closing wedge HTO in more detail. The condition of the articular cartilage was assessed in preoperative x-rays as described by Kellgren-Lawrence. A Kellgren-Lawrence grade >2 correlated with a higher rate of conversion to TKA.

Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I have no competing interests

Thank you very much for your comments, editing, and proofreading of our manuscript. We hope that we answered your questions satisfactorily and look forward to your response.

Sincerely,

Turgay Efe, MD