



Letter to the Editor

Letter to the editor concerning “Intramedullary nailing versus plating fixation for the treatment of midshaft clavicular fractures: A meta-analysis of randomized controlled trials”


Dear Editor

We recently read an article written by Jing-Xin Zhao [1], entitled “Intramedullary nailing versus plating fixation for the treatment of midshaft clavicular fractures: A meta-analysis of randomized controlled trials”.

We appreciate the authors' contribution, as they compared intramedullary nailing and plating fixation for the treatment of midshaft clavicular fractures with 13 researches from different countries. We believe INJURY is an influential academic magazine in the orthopedic field and regarded as orthopedists' bible worldwide. Owing to this, we have to point out some defects in this article.

First, Lee published 2 articles in 2007 [2] and 2008 [3]. Both researches had an overlapping study time in 2007, given the inclusion criteria and exclusion criteria were the same. Some cases could be recruited in both trials, so some data could be analyzed repeatedly in this meta-analysis.

Second, the authors claimed a random-effects model would be applied when facing substantial heterogeneity ($I^2 > 50\%$). However, they used a fixed-effects model when analyzing post-operative DASH scores. This might be a careless mistake. When we made a forest plot using random-effects model which provided a different result in DASH scores comparison (Fig. 1), we found, apparently, the result of 12 months post-operative DASH scores was wrong, where they thought there was a significant consequence favoring the intramedullary nailing. Certainly, some of their conclusions were not changed even if they used the wrong model. We noticed that no matter a fixed-effect model or a random-effect model was applied in 6 months post-operative DASH scores, the result was the same, and there was no statistical difference between intramedullary nailing and plating fixation. However, even if an incorrect analytical method has no negative effect on their results, the incorrect analytical process should be modified.

Third, we found that there were different plating methods which could greatly increase the heterogeneity and defect the reliability of the conclusion. For instance, some patients were performed with plating on the superior surface [4] while others received reductions with anterosuperior [2,5] or anteroinferior [6,7] approaches. According to Coner G Ziegler's study [8], different plating leads to distinctive bending stiffness. The authors might ignore the heterogeneity caused by different plating methods. How could the authors compare intramedullary nailing with different plating fixations unless there was proof that different plating methods had led to biomechanical stability and other clinical outcomes that were not statistically different? It is advisable for

authors to first analyze the data of the different plating methods in subgroups and then to compare them with the intramedullary nailing group if there is no statistical difference.

Also, only 3 or fewer studies were analyzed in some subgroup analysis, which can affect the reliability of this meta-analysis.

In conclusion, these controversies should be clarified.

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Author contribution

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NA

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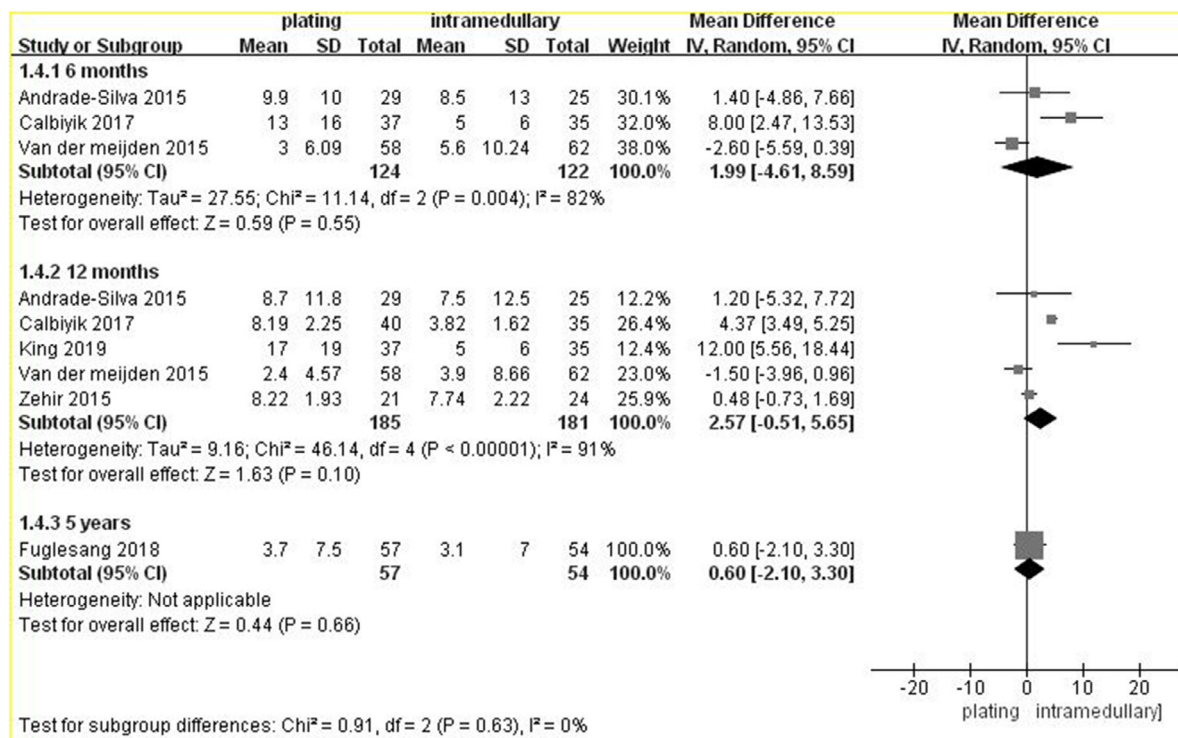


Fig. 1. Forest plot for the post-operative DASH scores between two groups based on different follow-up intervals using a random-effect model.

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