

Dear Editor,

We thank the reviewers and editorial team for taking their efforts to improve the article to increase its value for publication. Herewith we submit the revised version of the article addressing the reviewer's comments and the action taken for their valuable suggestions have been mentioned below.

Reviewer 1 Comments	Authors Reply	Action Taken
1. Thought the current paper focus on clinical efficacy of CT, it would be desirable if the authors can introduce what are i. the principles of MSC-based CT; ii. the proposed mechanisms of CT in repairing tendon injury; iii. The brief procedures of CT in RC tears and iv. Potential safety concerns of CT in the introduction. Also discussion of the mechanisms in the discussion part is welcome.	Thanks for the insightful comment. We have added the suggested content in the introduction of the revised manuscript.	Introduction
Are the stem cells used for the therapy autologous? When does CT starts in clinical trials?	Thanks for the comment. In table 2, it is mentioned that all the source of MSCs in the included studies are autologous. In 2014, P Hernigou et al. started clinical work on the use of autologous MSCs in augmentation of shoulder repair.	

Figure 1, flow diagram: Records excluded (n=212): should state i. reasons of being excluded in the figure. Is it based on the inclusion and exclusion criteria? And ii. is it screened by reading the abstract or whole text?	Thanks for the keen observation. The excluded articles were based on the title and abstract screening and the same has been mentioned as suggested in the revised Figure 1.	Figure 1
The final number of papers being screened is on the low side (only 6). The authors may need to comment on this and defend why they think this number is enough.	Thanks for the comment. We had our inclusion criteria to include studies with comparator groups to objectively analyse the efficacy of MSC-based biological therapy against controls population. Hence, the final studies with this inclusion criteria was only 6 but still it had valuable data to arrive at a meaningful conclusion.	
The current format of the tables is difficult to read. It should be formatted, e.g. in landscape orientation, and should have enough width for each column.	Thanks for the comment. The tables have been reformatted as suggested.	Tables
p. 11: '4/6 studies' should be 'Four out of six studies'. Same as '2/6 studies'.	Thanks for the comment. Revised as suggested.	
Studies from SJ Kim and JL Hurd recruited subjects with only partial thickness injury. Will it be	We considered that could be a confounding and made a sensitivity analysis	

a confounding factor for the analysis?	on the results obtained but we did not find any significant change in the final outcomes derived upon their inclusion into the analysis.	
Forest plot and result of VAS: improvement of reduction in pain was observed at 3 and 6 months after treatment, but not observed after 1 year. Is there any explanation on the observation? Does it suggest that CT may speed up the recovery?	Thanks for the comment. We also inferred the results to be due to the augmented healing of the injury in the short term with the help of MSCs in the vicinity.	
What is the possible explanation of no significant effect on Constant score?	The studies reporting constant scores were measured at 1 and 2 years which were not significantly different from the controls which is the case with the other outcomes also. Hence CT helps in the augmented healing in the short term with comparable results in the long term.	
Reviewer 2 Comments	Authors Reply	Action Taken
Evidence exists that mesenchymal stromal cells (MSC) from different origins might not have identical biological and	Thanks for the insightful comment. In order to explore into the heterogeneity of the	

physiological properties, 4 of 6 studies included in the paper utilized MSC from bone marrow, and the two remaining articles used adipose tissue-derived MSC. The complex sources of the MSC origins make it very difficult to interpret the results in the present study.	results based on the source of MSCs, we made a sensitivity analysis on this regard and we did not find a significant change in the results.	
Considerable heterogeneities also exist in cellular dosage, preparation method, and intervention in both treatment and control groups, making the results less robust.	Thanks for the valuable comment. We also note similar heterogeneity among the studies with respect to cellular dosage, and the preparation methods which are inherent in the comparative studies involving biological agents and we have duly mentioned them in the limitation section of the revised manuscript to make the point clear to the readers. Moreover, we made a sensitivity and subgroup analysis by grouping the studies of similar nature of variables assessed or leaving the odd type of study out of	

	the analysis and we did not find any significant change in the results.	
The heterogeneity of VAS and ASES is relatively high, although the authors stratified the analysis based on the duration of follow-up. However, the heterogeneity did not change significantly in every sub-analysis, especially in the ASES analysis, indicating that the duration of follow-up may not cause the heterogeneity. The authors need to figure out the causes of the high heterogeneity by meta-regression or other suitable statistical methods.	Thanks for the insightful comment. We do agree that the heterogeneity was not resolved by stratifying the studies based on follow-up but on close observation one could note that the one study by SJ Kim et al. reporting results at 3 weeks was the major outlier contributing to the heterogeneity and upon removal of the study we did not find any change in significance of results but only noted a further strengthening of the p value noted previously hence we did not find any reason to remove that from analysis despite the heterogeneity noted.	
Further editing work is recommended for table 1 and table 2 in order to improve readability.	Thanks for the valuable comment. Tables were reorganised to improve the readability as suggested.	

Reviewer 3 Comments	Authors Reply	Action Taken
The title of the article cannot summarize the main content. Such an expression is misleading and should be revised.	We have used the research question as the title to make it more clear to the readers and we do not impart any misleading impression with it.	
In the INTRODUCTION section, the logic of the article is very chaotic, and the theme it expresses cannot be well understood.	Thanks for the valuable comment. The introduction has been revised as suggested and has been organised for clarity of content.	
In the discussion section, the author only describes the data again, which is useless for in-depth analysis and discussion, and the logic is chaotic.	The discussion has been completely revised in the revised version of the manuscript with thorough analysis of the results obtained.	
The composition of the article is chaotic and difficult to read. It can be seen that the author's attitude is not rigorous.	Thanks for the valuable comment. We have revised the article composition and rigorously revised it to make it more organised to the readers.	
The language expression is very irregular and difficult to understand. Language needs polishing.	The manuscript has been completely revised with the help of a native language speaker and revised as suggested.	

Reviewer 4 Comments	Authors Reply	Action Taken
The authors need to update the protocol of their review to the PRISMA 2020 guidelines, instead of the 2009 version.	Thanks for the valuable comment. We have revised the protocol to PRISMA 2020 as suggested.	
Only 6 studies were included in the review, and some meta-analysis included as few as only two studies. And yet, the authors drew strongly conclusion about their results, which is completely inadequate. As a rule of thumb, meta-analyses should be performed only when there are at least 10 eligible studies, because when there are fewer studies the power of the tests is too low to distinguish chance from real effect (take a look at the book of systematic reviews from the Cochrane, which is available online with free access). The authors should be aware that and acknowledge their limitation.	Thanks for the insightful comment. We acknowledged our limitation on the number of studies and their quality of evidence in all the results and limitation of the revised manuscript as suggested.	
The first two paragraphs of the Discussion consist of a short literature review on some aspects of the subject being reviewed, but without an actual discussion of the findings of the study. This	Thanks for the valuable comment. The discussion has been totally revised as suggested by thoroughly analysing the results obtained with the	

means that there is no discussion at all in these two paragraphs. For the rest of the Discussion section, it consists of paragraphs beginning with a repetition of the results followed by the citation of the results of other studies, without an actual discussion of the findings of the study. This means that the discussion is very poor.	available evidence on the subject in the literature and the rationality of its utility.	
“We recommend a large-scale, multicentric trial analyzing autologous and allogeneic sources of MSCs with standardized dosage and intervention protocol, evaluated with established outcome measures both at short- and long-term follow-up to further confirm the results of our analysis.” This is not a conclusion, but a recommendation, which should stay at the end of the Discussion section.	Thanks for the valuable comment. The statement has been moved to the discussion as suggested and conclusion has been revised with due acknowledgement to the limitations in the current study and the power of the results obtained out of the analysis.	
Science Editor’s comments	Authors Reply	Action Taken
This article focuses on an analysis of published clinical data on the efficacy of mesenchymal stromal cell-based cellular therapy in the treatment of rotator cuff. The	Thanks for the valuable comment. As per the suggestion, the revised manuscript has been modified with the	

<p>authors mention that the study is a meta-analysis; however, the low number of studies included makes it difficult to consider this study as a meta-analysis. In addition, the introduction is too short and more information about the characteristics of mesenchymal stromal cell need to be added. Likewise, the authors should discuss the results and not list of the results and other findings without a thorough analysis of them.</p>	<p>introduction explaining more on the MSCs, their role in the management of rotator cuff tears.</p> <p>Moreover, in the revised manuscript, we have also thoroughly analysed the results obtained. In addition, due to the focussed research question to evaluate the role of cellular therapy in the rotator cuff tears we could find only 6 studies with comparator group involved in the study to arrive at a meaningful conclusion from the analysis. We have given our limitation on the number of studies included in the revised manuscript.</p>	
Editor-in-Chief's comments	Authors Reply	Action Taken
<p>I recommend the manuscript to be published in the World Journal of Meta-analysis</p>	<p>The manuscript has been transferred to World Journal of Meta-analysis as recommended.</p>	<p>Journal manuscript submission transferred to World Journal of Meta-analysis.</p>