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Reviewer A

1. A2 is NOT a burst fracture. We can't compare split fractures (A2) with burst fracures (A3 and A4).
We apologize for the lack of clarity. We agreed with reviewers that A2 and A3/A4 fractures are formally distinct fracture types making comparison difficult. However, as said in the paper, we still included A2 split fracture. We did it because intrasomatic devices plus cement augmentation use is still debated in such fractures. Several authors have already performed similar analyses and showed promising results in that kind of fracture (*Lofrese G, Ricciardi L, De Bonis P, et al. Use of the SpineJack direct reduction for treating type A2, A3 and A4 fractures of the thoracolumbar spine: a retrospective case series. J Neurointerv Surg. 2021:neurintsurg-2021-017682. doi:10.1136/neurintsurg-2021-017682*). Also, we did and showed a separate analysis for each fracture type, thus showing each relative outcome for each type. We reworded, when needed, the methods and results section to better clarify this point.
(Page 3, Paragraph 3, Line 27 to 29).

2. How long between fracture and surgical treatment in each group?
We apologize for the omission and the lack of clarity. We added a paragraph in the result sections to address this point **(Page 8, Paragraph 1, Line 3 to 4 and Page 8, Paragraph 4, Line 15 to 16)**.

3. Was there any type of randomization to choose each group?
We appreciate the reviewers' insight. We did not perform a randomization process. We carefully evaluated each case in our multidisciplinary spine group and decided which patients were suitable to undergo spine jack implant. We better clarify selection criteria in the methods section **(Page 13, Line 23 to 26)**.

4. What criteria to choose which patients would undergo arthrodesis and which would undergo the spinejack system?
We thank the reviewers for their insight. We added a paragraph and a new Figure (Figure 2) in the methods section to better elucidate this point **(Page 6, Line 1 to 7)**.

5. if the objective of the study is to compare 2 groups (arthrodesis x spinejack), why do you present cases where the 2 procedures were performed together (as shown in

table 1, with only few cases)?
We apologize for the lack of clarity. We included a small number of patients with spine jack and subsequent short arthrodesis to show any possible use of the device alone and in combination as did by other authors (*Kerschbaumer G, Gaulin B, Ruatti S, Tonetti J, Boudissa M. Clinical and radiological outcomes in thoracolumbar fractures using the SpineJack device. A prospective study of seventy-four patients with a two point three year mean of follow-up. Int Orthop. 2019 Dec;43(12):2773-2779. doi: 10.1007/s00264-019-04391-1. Epub 2019 Aug 15. PMID: 31418065)*. Again the results are not significantly different after performing a subanalysis of clinical and radiological outcomes. We better clarify this point in the methods section **(Page 6, Line 8 to 12)**.

6. Why do patients undergoing arthrodesis use 2 levels above and 2 below and some cases of short instrumentation? Doesn't this fact lead to bias in the results?
We appreciate the reviewers' insight and concern. We preferred to be inclusive of every choice of treatment to show. Therefore, we performed a sensitivity analysis between subgroups showing no statistical differences in clinical and radiological parameters outcomes. We reworded the methods section to be more explicit about this subject **(Page 6, Line 13 to 16)**.

Reviewer B

The topic that you deal with in your manuscript is extremely interesting, as it is a clear matter of debate.
Related to patient selection, I would like to understand why there is a limit of the spinal canal compromise and vertebral body spread only for the SJ group, as these could create a significant difference in between both groups.
We thank the reviewers for their precious insight and apologize for the lack of clarity. We reworded selection criteria and added a paragraph and a summary Figure (**Figure 2**) to explain better indications we used for SJ patients **(Page 6, Line 1 to 7)**.
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Data collection section
In the first paragraph it seems that you collect all that data only for the SJ group, please address it.
Thanks, we reworded and made the whole paragraph more clear.

Surgical technique: either you describe both techniques or no need to explain SJ technique nowadays.
Thanks, we reworded the paragraph accordingly **(Page 5, Line 27 to 30, Page 6, Line**

1 to 30).

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The follow up range 1-41 months does not make sense in terms of data. Please think about a more homogeneous group

We apologize for the lack of clarity, and it was a mistake in reporting data; we corrected it and updated interval range units to more appropriate statistical descriptors **(Median and interquartile range (IRQ))**.

In the statistical analysis you mention long or short arthrodesis. How many of each? Why?

We appreciate the reviewers' insight. We clarify this in the methods section **(Page 6, Line 8 to 12)**.

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Discussion: I think the initial part is not needed under this title. You have to focus on comparing your outcomes with literature of both techniques. Please rewrite.

We appreciate the reviewer's suggestions and reworded the discussion sections accordingly **(Page 10, Line 19 to 27)**.

Reviewer C

I read with great interest your submission entitled "Treatment of thoracolumbar burst fractures: SpineJack vs. posterior arthrodesis. Comparison of clinical and radiological outcomes". I found the paper well-written and generally of interest for the neurosurgical community. Having said that, there are some observations that should be taken into consideration by the Authors in order to improve their submission:

1. Please rephrase the following sentence "We retrospectively collected and analyzed prospectively collected patients diagnosed with types A2, A3, and A4 thoracolumbar burst fractures". the type of study is not clear

We thank the reviewer for their suggestion and apologize for the lack of clarity. We reworded the paragraph for a better clarity (Page 3, Line 27 to 29).

2. it could be useful to specify if the surgical groups were statistically comparables in terms of genders, age, fracture level and lenght of follow-up

We thank the reviewer for his suggestion. Both groups were homogenous in terms of clinico-demographic characteristics (no significant differences between parameters). We better elucidated it in the results section and made an adjunct in the limitation section. We also add p-values in Table 1 to better clarify the

readers (Page 8, Line 25 to 27).

3. in order to improve the statistical component, the authors could consider to use also some other clinical evaluation scale (such as ODI and SF-12) to allow a better statistical comparison.
We appreciate the reviewer's insight and suggestions. We agree with them; however, the paper is mainly focused on radiological outcomes between treatment groups. We tried to focus on what is missing in the literature. We will include a standard clinical evaluation scale (ODI, SF-12, etc.) in the progression of data collection.

3. the paragraph about the SJ technique is of interest; it could be improved if the authors decided to add the clinical algorithm used to divide the patients into 3 groups
Thank for the suggestion. We added a figure (Figure 2) to better explain the patients' selection process for SJ, and also we reworded the methods section (Page 6, Line 1 to 7)

4. a multivariate analysis should be taken into consideration in order to see if there is any significance with age, type of fracture, level, and type of fracture (with also A4)
We thank the reviewers' suggestion, and we agree with it. However, multivariate analysis with such a small number of patients is not feasible, and results may eventually be biased. In spine surgery, there are lots of factors to consider related to a specific outcome. Adjusting for more than one variable is problematic with those limited samples. We did not feel the sample adequate for the moment but will do in the future after collecting a sufficient number of data. We add a statement to account for this **(Page 14, Line 1 to 5)**.

5. a subanalysis of the follow-up should be considered in order to see if there is any difference at 3, 6, 12 or 24 (for those who have it).
Regarding radiological outcomes (LK and VK angles), we add a follow-up (6, 12, 24) subanalysis in the results section and add a paragraph in the discussion section **(Page 9, Line 17 to 23).**

it would be also interesting discussing these results with the present literature

-Muñoz Montoya JE, Torres C, Ferrer ER, Muñoz Rodríguez EE. A Colombian experience involving SpineJack®, a consecutive series of patients experiencing spinal fractures, percutaneous approach and anatomical restoration 2016-2017. J Spine Surg. 2018 Sep;4(3):624-629. doi: 10.21037/jss.2018.07.08. PMID: 30547128; PMCID: PMC6261771.

-Kerschbaumer G, Gaulin B, Ruatti S, Tonetti J, Boudissa M. Clinical and radiological outcomes in thoracolumbar fractures using the SpineJack device. A prospective study of seventy-four patients with a two point three year mean of follow-up. Int Orthop. 2019 Dec;43(12):2773-2779. doi: 10.1007/s00264-019-04391-1. Epub 2019 Aug 15. PMID: 31418065.

- Todeschi J, Ganau M, Zaed I, Bozzi MT, Mallereau CH, Gallinaro P, Cebula H, Ollivier I, Spatola G, Chaussemy D, Coca HA, Proust F, Chibbaro S. Managing Incomplete and Complete Thoracolumbar Burst Fractures (AO Spine A3 and A4). Results from a Prospective Single-Center Study Comparing Posterior Percutaneous Instrumentation plus Mini-Open Anterolateral Fusion versus Single-Stage Posterior Instrumented Fusion. World Neurosurg. 2021 Jun;150:e657-e667. doi: 10.1016/j.wneu.2021.03.069. Epub 2021 Mar 20. PMID: 33757885.

We really appreciate the reviewers' suggestions. We cited and discussed the following papers.

Reviewer D

The authors are to be congratulated on composing a well written manuscript on a topical area in spine surgery.

A few points to consider:

1. The authors have clearly defined the population in which the spine jack device is being studied in by excluding those with severe burst fractures and canal narrowing more than 50%, malignant lesion and where the posterior elements (pedicles) are damaged. The role of instrumented posterior fusion as the gold standard is acknowledged. Given the endplate is coronally disrupted in pincer A2 fractures, and at least in one or both endplates in A3 and A4 fractures what is the risk of cement extravasation and neurological compromise in other studies and in the authors' experience? Are there any intraoperative techniques to mitigate this risk?
We thanks the reviewers for their kind comments. We added a paragraph in the discussion section to cover the raised points **(Page 11, Line 30 to 32; Page 12, Line 1 to 11).**

2. The median follow-up time of 7 months appears to be relatively short. Do the authors themselves have any longer-term data on the outcomes (such as delayed kyphosis or failure) with respect to the spine jack device? Is there any other evidence

on the long-term efficacy of these devices?
We appreciate the reviewers' insight. After reviewing the cases, we updated the follow-up time, considering the time passed from submission, review process, and response. We also expanded the discussion section by adding existing literature on this topic (Page 12, Line 31 to 32 and Page 13, Line 1 to 3).

3. A tabulated summary on the benefits and risks of spine jack compared to posterior instrumented fusion may be useful. Additionally, other trials and their follow-up times as well as outcomes may give readers a deeper understanding on the role of this novel device which the authors are to be commended for bringing to the fore
We thanks the reviewers for the precious suggestion and added a Figure to summarize the pros and cons of SJ vs. posterior instrumentation (Figure 2)