Peer Review File

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Reviewer A

I was pleased to read your submitted paper for review. It was interesting and on-point.
I have some minor points in order of appearance:

Comment 1: Page 4 Line 124 - "conversative" - I believe you meant "conservative"?
Reply: Thank you for pointing out our mistake of typing error.
Changes in the text: We have corrected the word “conservative” as you commented. (Page 6, Line 100)

Comment 2: Page 4 Line 130 - who was the senior surgeon? Is it possible to write initials after the statement?
Reply: The senior surgeon was Prof. R.H. He had extensive experience in MIS TLIF operation.
Changes in the text: We have added this statement as you kindly suggested. (Page 6, Line 104-105)

Comment 3: Page 10 - 322-324 - could you give us some more specific details from previous studies performed, how and why your results would be different?
Reply: We observed a higher subsidence rate (25%) compared to earlier studies on expandable TLIF cages, which typically ranged from 15% to 20% in a short-term follow-up (1, 2). It is important to note that the methodology used to assess the subsidence significantly influences the reported rates. In general, subsidence was defined as a 2-mm loss in either disc height between postoperative and baseline measurements. Notably, we utilized CT scans for subsidence assessment in every patient, while many prior studies relied on standing lateral radiographs (3), and some studies used the cut-point of 4-mm loss of disc height as a definition (2), which are less sensitive in detecting subsidence. Furthermore, the expandable cages used in our study demonstrated the capability to increase both intervertebral height and disc lordosis. It is worth emphasizing that an overly aggressive attempt to expand these parameters may contribute to the observed higher rate of subsidence in our study. Thank you for your valuable suggestions.
Changes in the text: We have added these details from previous studies and explained why these would lead to difference in results among studies. (Page 11, Line 260-269)

Some general comments:
Comment 4: The limitations section mentions limited numbers in Group H, but it would be helpful to clarify if this impacted the statistical power of the study. Additionally, addressing the potential impact of incomplete data on the results would be beneficial.
Reply: The limited numbers of subjects in Group H were a result of a transition to utilize a new design of the expandable cage for MIS TLIF that expands both height and lordosis in Group HL. We acknowledge that unequal group sizes can potentially influence the apparent effect size and the precision of estimates, particularly if the smaller group exhibits a more

extreme value. However, it is crucial to note that the number of subjects in Group H exhibited a normal distribution, and we have carefully tested the assumption of equal variances between the two groups before employing the standard independent t-test. The decision not to equalize group sizes was made with the intention of avoiding the introduction of selection bias and ensuring the inclusion of all available data. We believe that discarding data merely to achieve equal group sizes would have led to the loss of valuable information and compromised the integrity of the study.
Changes in the text: We appreciate your consideration of these factors and additional clarifications were added as per your guidance. (Page 12, line 301-307)

Comment 5: Could you specify the rationale for selected statistical methods?
Reply: Statistical analysis was performed with SPSS 28.0.0 (IBM SPSS, Chicago, IL, USA). Normality was assessed via the Shapiro-Wilk test to determine the appropriate statistical test. In this study, the data followed the normal distribution so that parametric statistical techniques were employed. Standard descriptive statistics were reported for demographic data, perioperative data, functional and radiographic outcomes as means with standard deviations, frequencies and percentages, where applicable. Continuous variables between two groups were compared using 2-sample Student t-test and categorical variables were compared using Chi-square test. Any significant differences between demographic or baseline values were reported. For all analysis, the statistical significance was set at a threshold of p value less than 0.05. We believe that these choices align with the nature of our variables and the objectives of our investigation.
Changes in the text: Thanks to your consideration and valuable feedback, we have clarified the statistical methods in the statistical analysis part. (Page 8, line 166-173)

Reviewer B

I read with interest this paper about clinical results using an expandable cage that simultaneously improves disc height and lordosis. However, there were some major problems that I would like to point out.

Comment ①: Regarding the title, the author must clearly and concisely present his/her argument to the reader in it.
Reply: Thank you for your guidance in ensuring clarity and conciseness on the title of our manuscript. In response to your suggestion, we have revised the title to better convey our main argument.
Changes in the text: The title was changed to “Expandable Cages that Expand Both Height and Lordosis Provide Improved Immediate Effect on Sagittal Alignment and Short-term Clinical Outcomes Following Minimally Invasive Transforaminal Lumbar Interbody Fusion (MIS TLIF)”

Comment ②: In the introduction, the contents from line 106 onwards are difficult to understand. The main purpose of this study must be made clear.

Reply: We appreciate your suggestion in clarifying the main purpose of our study. To address your concerns and enhance clarity, we have revised the relevant section: Our study aimed to compare the outcomes of two different expandable cages in MIS TLIF: the conventional expandable cages that solely expand disc height, and the newer cages that additionally increase segmental lordosis. In this study, our primary focus was on analyzing the postoperative radiologic outcomes to demonstrate the true mechanical effect of the expandable cages on sagittal parameters without the effect of subsidence over time, and also suggest our technique to prevent early subsidence following the use of expandable cages for MIS TLIF. Our subsequent long-term study in the future will focus on the fusion rate, late subsidence, long-term maintenance of the lordotic alignment, and the effect on the adjacent level.
Changes in the text: We added these revisions to better depict the main purpose and the rationale of the study. (Page 5, line 86-93)

Comment ③: Statistical analysis requires demonstrating normality tests for continuous variables. If a normal distribution is not obtained, nonparametric testing techniques should be used. In this case, you need to indicate the median and interquartile range.
Reply: Thank you for your valuable feedback. We appreciate your attention to the statistical analysis in our study. In response to your comment, the normality of continuous variables was evaluated using the Shapiro-Wilk test. The results indicated that the data followed the assumption of normal distribution. Consequently, parametric statistical techniques were employed for the analysis. Descriptive statistics, including means with standard deviations for continuous variables and frequencies with percentages for categorical variables, were reported for demographic data, perioperative data, and functional and radiographic outcomes where applicable. To compare continuous variables between two groups, the 2-sample Student t-test was utilized, while Chi-square tests were employed for the comparison of categorical variables.
Changes in the text: We have added this clarification into the statistical analysis. (Page 8, line 166-173)

Comment ④: What is the basis for dividing the learning phase into 20 cases? Authors should also indicate where there have been technical changes. In particular, the results of a case series in which the "two-finger technique" was used should be presented. Additionally, there is a lack of objective data to show that this method is effective, and reproducibility must also be demonstrated.
Reply: Following the initial twenty cases, we observed a significant reduction in the rate of subsidence. This improvement coincided with the surgeon's increased proficiency in determining the optimal force required to tighten the cage expansion device, specifically aimed at increasing disc height and lordosis. These findings highlight the existence of a learning curve regarding optimal torque to be applied to prevent vertebral endplate injury from expandable cages.
Our proposed two-finger technique was introduced to reduce the risk of over-distraction force to the vertebral endplate when attempting to maximize the lordotic angle. This technique,

commonly referenced in orthopedic operations, particularly in screw insertion, serves as a precaution against excessive torque (4). We also followed this technique to avoid iatrogenic endplate injury during the expansion of the cages, given the absence of previous guidelines on optimal distraction force. We concurred with the reviewer's comment that, despite the recognized use of the two-finger technique, it is crucial to address the considerable variability in absolute torque values, percentage of stripping torque, and challenges in consistently reproducing specific torque levels (5, 6). Therefore, it is important to note that the two-finger technique is only a suggestion to avoid overtightening the cage inserter. This concept serves as a starting point, particularly crucial in osteoporotic bone, where surgeons may benefit from increased attention to tactile feedback from two fingers instead of palming the cage expander.
To further improve precision and prevent overexpansion leading to endplate fractures and early subsidence, the consideration of employing a torque limiting handle attached to the cage inserter may be beneficial. Thank you for your valuable comments, which have significantly contributed to the refinement of our discussion.
Changes in the text: These are added to the discussion. (Page 11-12, line 271-286)

Comment ⑤: In the discussion, the author must first fully explain the research results in relation to the research purpose presented in the introduction. In the present study, a significant difference was observed only in the local lordosis of the lumbar spine when evaluating the HL and H groups. The lack of significant differences in other clinical outcomes or radiological parameters should be discussed.
Reply: Thank you for your constructive feedback regarding the discussion. We acknowledge your guidance and agree that it is essential to provide a thorough explanation of the research results in relation to the initially stated research purpose. In our study, a significant difference was identified in the local lordosis when comparing the HL and H groups. However, other clinical outcomes and radiological parameters did not exhibit significant differences between the two groups. Furthermore, the extent of improvement in local lordosis did not have a substantial impact on clinical outcomes, which demonstrated no differences between the two types of cages. The meaningful clinical significance of this improvement in terms of overall balance restoration might be limited from one-level MIS TLIF with any type of expandable cage. Nevertheless, even a small increase in segmental lordotic angle could potentially lower the risk of adjacent segment degeneration (ASD) in long-term outcome (7, 8). In addition, multiple-level surgery may provide a substantial increase in segmental lordosis that could potentially contribute to the restoration of lumbar lordosis, as well as regional and global sagittal alignment.
Changes in the text: We have addressed your insightful comments that will undoubtedly contribute to the refinement of our discussion section. (Page 10-11, line 221-227 and line 247-259)

Comment ⑥: The HL group cage used this time has a structure with a surface treatment that is bone biocompatible. Therefore, it may influence long-term changes in the vertebral endplates.
Reply: Thank you for your insightful comment regarding the 3D-printed surface of the HL group cage and its potential long-term effects on vertebral endplates. Titanium, a well-studied

material for intervertebral cages, benefits from additive manufacturing, offering the creation of porous implants crucial for spinal fusion, while 3D printing has revolutionized the development of biomimetic, porous titanium implants, known for enhancing fusion through osseous integration within their porosity. Early interaction at the implant-bone interface with 3D-printed Ti cages may add stability and limit micromotion, promoting early fusion with stabilization (9). Laratta et al also found the ingrowth and formation of cartilage and bone to be significantly greater in the 3D-printed Ti cages, compared to PEEK cages (10). In our study, we utilized the 3D-printed Ti cages in Group HL, which may deliver an optimal environment for fusion and potentially exhibit significant osseous in-growth from vertebral endplates. To address this potential effect, we have included a discussion in the manuscript regarding the bone biocompatibility of the HL group cage and its potential implications for long-term changes in vertebral endplates. Additionally, we highlight the importance of investigating these long-term effects in our subsequent studies, which will specifically focus on aspects such as fusion rate or late subsidence. Thanks to your thoughtful suggestion, this added valuable perspective to our study.

Changes in the text: We have addressed the significance of the cage's surface treatment on potential long-term outcomes in the discussion. (Page 12, line 287-296)