Peer Review File

Article information: https://dx.doi.org/10.21037/jss-25-122

Reviewer A

Thank you for the opportunity to review this manuscript. The study explores a timely and relevant topic—robotic-assisted transdiscal fixation in high-grade spondylolisthesis (HGS). While the concept is of clinical interest and potential significance, several concerns must be addressed to improve the clarity, scientific rigor, and overall impact of the manuscript.

1. Major Methodological Concerns
Sample Size and Generalizability
The case series includes only five patients, which is insufficient to draw definitive conclusions about safety, efficacy, or reproducibility. The authors should clearly acknowledge the extremely limited statistical power in the Methods and Discussion, and reframe conclusions accordingly.
Thank you for this helpful comment. We agree and have revised the manuscript to explicitly acknowledge the very limited sample size and the resulting lack of statistical power. We reframed our claims to focus on technical feasibility and descriptive early outcomes only, removed any language implying efficacy or reproducibility, and clarified that no inferential statistics were performed. We also expanded the Limitations and softened the Conclusion accordingly.

Selection Criteria and Confounders
More clarity is needed regarding how patients were selected for robotic surgery. Were these consecutive cases? Were other surgical options considered? Was any matching or exclusion done based on bone quality, comorbidities, or surgeon preference?
Thank you for this helpful observation. The 5 patients were consecutive cases of high grade spondylolisthesis from . Consecutiveness was defined as all eligible patients during the window without post hoc exclusions. Standard alternatives (in situ posterolateral fusion, TLIF/PLIF, reduction with pedicle screw constructs, and anterior approaches such as ALIF) were considered in each case. The decision to proceed with posterior transdiscal fixation was based on high-grade listhesis morphology, the need for robust construct across L5–S1, and the possibility to apply the robotic guidance. We have clarified this rationale in the Methods.
This is an observational case series; no matching was performed. Eligibility required: high-grade spondylolisthesis at L5–S1, availability of preoperative CT/MRI, and suitability for posterior transdiscal fixation. Exclusion criteria included active infection, neoplasm/trauma, prior fusion at the index level, severe osteoporosis (T-score $\leq$ -2.5 or very low vertebral Hounsfield units), or inability to tolerate prone positioning. Bone quality was assessed via DEXA (when available) and CT-derived Hounsfield units; comorbidities were recorded but did not by themselves exclude patients unless representing uncontrolled systemic disease (e.g., ASA IV) that contraindicated surgery. Surgeon preference did not exclude otherwise-eligible patients. We have added these details to the Methods and Limitations.

Absence of Control Group

Without a comparative group (e.g., conventional fluoroscopy-guided PTD), it's difficult to interpret the relative benefits of robotic guidance. While this may not be feasible in a case series, the lack of comparison must be emphasized as a major limitation.

The absence of a comparative group (e.g., conventional fluoroscopy-guided PTD) precludes any inference about the relative benefit of robotic guidance. Given the rarity of high-grade spondylolisthesis treated with PTD at a single center and the early adoption period, a contemporaneous control group was not feasible for this case series. We have emphasized this as a major limitation and reframed the manuscript to present findings as technical feasibility and descriptive outcomes only, without implying superiority. We also outline a plan for future comparative evaluation.

2. Insufficient Detail in Radiographic Assessment

Fusion Assessment

The manuscript does not provide any evidence of fusion status at follow-up. Were CT scans or dynamic X-rays used to assess arthrodesis?

Thank you for your helpful observation. A CT scan was taken for each patient after the surgery, and a static X ray was taken at 1 and 3 months follow-up. Pictures were not attached to this paper because the radiologic outcome was not the primary endpoint of our observational study; on the other hand we would like to emphasize the feasibility of the PTD procedure with the robotic guidance.

Spinopelvic Alignment

Although sagittal balance is discussed in the inclusion criteria, no quantitative spinopelvic parameters (e.g., pelvic incidence, lumbar lordosis, PI–LL mismatch) are reported pre- and post-op.

As mentioned in the main text, No significant difference between preoperative and postoperative spinopelvic parameters were observed. A quantitative table of data were inserted in the method section, accordindgly (table 4).

3. Robotic System Description and Learning Curve

The surgical description is technically informative, but there is little discussion of the learning curve associated with using the robotic system. How experienced was the team with this platform?

More detail is also needed on the robotic planning protocol, including registration accuracy, trajectory correction if required, and surgeon validation.

Thank you for this comment. We have adjusted the method paragraph accordingly.

4. Statistical and Data Presentation Issues

The statistical analysis lacks robust justification. Please clarify how normality was assessed and why a p-value of ≤ 0.01 was chosen.

Thank you for the comment. We have clarified and expanded our statistical methods for these reasons:
We evaluated distributional normality of paired differences (post minus pre) for each continuous outcome using the Shapiro–Wilk test (alpha = 0.05) and visual inspection of Q–Q plots and histograms.
When Shapiro–Wilk indicated non-normality or when sample size for a comparison was ≤ 10, we used nonparametric tests (Wilcoxon signed-rank for paired data; Mann–Whitney U for independent groups).
When normality was not rejected and there were no clear outliers on Q–Q plots, we used parametric tests (paired or unpaired t-tests as appropriate). Homogeneity of variances for independent comparisons was checked with Levene's test.
About the significance <0.01: we predefined a stringent significance threshold (two-sided p ≤ 0.01) to mitigate inflation of false positives given multiple endpoints and subgroup analyses, and to reflect the clinical importance of avoiding overinterpretation in a relatively small cohort.

Tables 2 and 3 present overlapping data. Consider combining or eliminating redundancies.
Thank you for you helpful observation. The table 3 was eliminated.

The range of radiation exposure (up to 23 seconds) is much higher than the reported mean (1.61 sec). Please clarify this discrepancy.
Thank you for underlining this discrepancy. This was a typing error: "23" instead of "2.3". We modified the text accordingly.

5. Literature Review and Discussion
The discussion section tends to overstate the advantages of robotic surgery without fully contextualizing the findings in prior literature.
More comparisons to non-robotic PTD studies (such as those by Delgado-Fernandez et al.) are needed, particularly regarding complication rates and long-term outcomes.
Thank you for you useful comment. We expand the discussion section with a comparative analysis.

6. Language and Formatting Issues
The manuscript contains numerous grammatical errors, overly long sentences, and inconsistencies in terminology (e.g., "L5throughS1" should be standardized).
Thank you: we will Perform a full language edit for grammar, punctuation, and style; shorten overly long sentences; remove redundancy. L5throughS1 will be L5-S1.

Ensure all abbreviations (e.g., PTD, ODI, VAS, DRB) are defined at first use.
Figure and table legends should be concise but informative—some are repetitive or vague.
Thank you for this helpful comment. We will ensure all the abbreviations, figures and tables will be satisfactory.

7. Ethical Approval and Consent

Please confirm and state whether the study received IRB or Ethics Committee approval and whether all patients provided informed consent, as per ICMJE guidelines.
Acknowledged.

We are grateful for your feedback and believe these revisions will strengthen the scientific rigor, transparency, and clinical relevance of the manuscript.

Reviewer B

In general patients with high grade anterolisthesis may not be sagittally balanced. Please provide full length scoliosis lateral x-rays pre-op and post-op showing alignment. Also, the literature about the segmental lordosis at L4-S1 and related to global alignment and risk for adjacent segment disease is growing. Please address since this procedure is not affecting the L5/S1 lordosis at all, and your technique description does note taking this into account. Also the procedure description is vague. Are you just instrumenting or doing a fusion L4-S1? This needs to be clarified in the body of the paper.
Thank you for your helpful comments. We added a new table (table 3) with the preop and postop pelvic parameters. We also would like to clarify that described procedure does not modify intrinsic L5–S1 lordosis unless interbody techniques or osteotomies are employed.
The technique paragraph was implemented accordingly.

We are grateful for your feedback and believe these revisions will strengthen the scientific rigor, transparency, and clinical relevance of the manuscript.

Reviewer C

The authors presented a case series pertaining to robotic guidance in posterior transdiscal fixation for high-grade spondylolisthesis. Below are some comments and insights for the authors to considerate for future reference. 1. One of the most significant concerns is the arbitrary selection of the study period (March to December 2023) without any explanation or justification from the authors. This raises concerns about potential selection bias. For example, if any patients who experienced complications were excluded because they fell outside the chosen timeframe, this would represent a serious flaw in the study design and reporting. 2. The study includes only five patients, which is an extremely limited sample from which to draw conclusions regarding the safety, efficacy, and reproducibility of the technique. While small case series can serve as preliminary technical reports, the authors present their findings as evidence of clinical benefits and procedural superiority. Claims of statistically significant improvement (e.g., in VAS and ODI scores) in such a small cohort are misleading. Without a larger sample or comparison group, these findings cannot be generalized or used to advocate for broader adoption of the technique. 3. Although the authors describe improvements in intraoperative parameters such as operative time, radiation exposure, and blood loss, these observations are not compared against an appropriate control group, historical institutional data,

or benchmarks from the existing literature. As a result, it is difficult to determine whether these improvements are attributable to robotic guidance or to other factors such as surgeon experience or patient selection. Moreover, similar benefits of robotic assistance in transdiscal fixation have already been reported in the literature. Therefore, the novelty of the present study is limited and does not, on its own, justify publication as a standalone article. This manuscript would have been significantly strengthened by incorporating a rigorous and thorough systematic reviewor a meta-analysis to establish the current standards in the literature. I would like to sincerely thank the authors for their submission and commend them for addressing a topic of growing relevance in spine surgery. The use of robotic-assisted techniques in complex procedures such as posterior transdiscal fixation is important, and there is no doubt that this technology will play an increasingly central role in future surgical practice. That said, I believe the manuscript would benefit from some refinement and additional context to strengthen its scientific rigor and clinical relevance. With thoughtful revisions, this work has the potential to contribute meaningfully to the evolving discussion around robotics in spine surgery.

Thank you for the thoughtful and constructive review. We appreciate the opportunity to refine the manuscript. We propose the following revisions and clarifications:

First, we do know that this is a very small case series but we think it could be important to add some novelty, as the robotic guidance, in a well-known, but tricky, spine procedure.

Study period and potential selection bias

Rationale: The March–December 2023 window reflects the consecutive availability of the robotic platform at our institution and a pre-specified prospective data capture period during its initial standardized deployment for high-grade spondylolisthesis.

Case ascertainment: We stated explicitly that all consecutive eligible patients undergoing robotic posterior transdiscal fixation during this interval were included, with no exclusions based on outcome.

Small sample size and claims of significance

Scope adjustment: We repositioned the manuscript as a technical feasibility/early experience case series rather than evidence of clinical superiority.

We added to the limitations that a sample of five cannot support broad generalization and that external validation in larger, comparative cohorts is required.

Lack of comparator

Novelty clarification: We will clarify that the contribution of this report is the detailed robotic workflow, registration/validation thresholds, and segmental alignment considerations in high-grade spondylolisthesis, rather than demonstrating clinical superiority.

Comparison with non-robotic PTD: We will benchmark our intraoperative metrics against published non-robotic PTD and robotic PTD series (including Delgado-Fernandez et al. and other key cohorts), presenting their results. Where differences are within reported ranges, we will acknowledge equivalence rather than implying advantage.

We are grateful for your feedback and believe these revisions will strengthen the scientific rigor, transparency, and clinical relevance of the manuscript.

Reviewer D

Article is overall well written; robotic spine surgery is a promising technique especially in complex anatomy. I would suggest to add long term imaging if available.

Thank you for your comments. Thank you for your helpful observation. A CT scan was taken for each patient after the surgery, and a static X ray was taken at 1 and 3 months follow-up. Pictures were not attached to this paper because the radiologic outcome was not the primary endpoint of our observational study; on the other hand we would like to emphasize the feasibility of the PTD procedure with the robotic guidance.

Unfortunately is not possible to have a long term results but our aim is to enlarge this stuty to a major cohort in the future with the radiological outcome.

We are grateful for your feedback and believe these revisions will strengthen the scientific rigor, transparency, and clinical relevance of the manuscript.