Peer Review File

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Reviewer A

The authors submitted a well-motivated and written study on the impact of sagittal realignment on discharge disposition.

I would only ask the authors to present a logistic regression analysis of other confounding factors that could impact discharge disposition.

We are grateful for the reviewer's comments. The multivariable logistical analysis now controls for the American Society of Anesthesiology (ASA) Classification. Please see modified regression analysis in Table 3.

Table 3. Multivariable logistical regression analysis reporting adjusted odds ratios for predictors of discharge to rehab versus discharge to home. Statistically significant values in bold.

	OR_{adj} [95% Confidence Interval]	*P*
Modified Frailty Index	0.70 [0.30 – 1.66]	0.428
American Society of Anesthesia (ASA) Classification	0.53 [0.09 – 2.93]	0.475
Postoperative Sagittal Vertical Axis	1.27 [1.05 – 1.55]	**0.014**
Increasing Decade of Life	3.13 [1.05 – 9.36]	**0.041**
Male Gender	0.27 [0.03 – 2.01]	0.201

The appropriate modifications have included throughout the manuscript text as well. Lastly, in order to determine the most multivariable regression model, a sensitivity analysis has now been included in both the Methods and Results sections. After an initial univariable logistical regression, predictors of discharge disposition were calculated with multivariable logistical regression with a forward stepwise modeling, reporting ORadj. Each regression model tested was analyzed with a sensitivity analysis based on three statistical approaches: 1) Akaike's Information Criterion (AIC), 2) model discrimination: C-statistic corresponding to the area under the receiver-operating characteristic (ROC) curve; and 3) model calibration: p-value of the Hosmer-Lemeshow Goodness-of-Fit Test.

The authors should include some result reporting and discussion on the medical comorbidities they reported in detail in Table 1.

The reviewer raises an important consideration. Medical comorbidities are now detailed in the manuscript text under the Results section: "All other preoperative demographic data – male gender (*P*=0.215) and weight (*P*=0.067) – did not differ. Statistical significance was not reached with all measured comorbidities, including diabetes mellitus (*P*=0.710), congestive heart failure (*P*=0.367), hypertension (*P*=0.699), stroke with neurological deficit (*P*=0.320), functional dependency (*P*=0.490), myocardial infarction (*P*=0.367), peripheral vascular disease (*P*=0.064), chronic obstructive pulmonary disease (*P*=0.188), coronary artery disease (*P*=0.406), and altered sensorium (0%). As such, the discharge-to-home versus discharge-to-rehab cohorts did not differ with surrogate markers of comorbidity burden: mFI (*P*=0.328) and ASA Classification (*P*=0.658)."

The Discussion section does include a section on the statistically significant association between age and discharge disposition.

I also did not see an OR reported for the various spinal alignment parameters.

We thank the reviewer for the suggestion. The adjusted odds ratio for each of the various spinal alignment parameters has now been included in the sensitivity analysis in Table 4.

Table 4. Sensitivity analysis of 3 different multiple regression models. Model 1 is the most appropriate based on three criteria: 1) Akaike's Information Criteria (AIC), 2) C-statistic corresponding to the area under the receiver-operating characteristic (ROC) curve; and 3) P-value of the Hosmer-Lemeshow Goodness-of-Fit Test

Model		Akaike's Information Criteria (AIC)	C-statistic	Hosmer-Lemeshow Goodness-of-Fit Test, *P*
1	Modified Frailty Index, American Society of Anesthesia (ASA) Classification, Postoperative Sagittal Vertical Axis (OR_{adj}=1.27, *P*=0.014), Increasing Decade of Life, Male Gender	54.45	0.87	0.938
2	Modified Frailty Index, American Society of Anesthesia (ASA) Classification, Postoperative Lumbar Lordosis (OR_{adj}=0.97, *P*=0.344), Increasing Decade of Life, Male Gender	61.12	0.77	0.565
3	Modified Frailty Index, American Society of Anesthesia (ASA) Classification, Postoperative Pelvic Incidence-Lumbar Lordosis Mismatch (OR_{adj}=1.01, *P*=0.560), Increasing Decade of Life, Male Gender	56.15	0.75	0.546

The Results section now explains "Multiple logistical regression modeling was based on three parameters: 1) AIC; 2) model discrimination via the C-statistic; and 3) model calibration via the Hosmer-Lemeshow Goodness-of-Fit test. Of the three models enumerated in Table 4, a multiple logistical regression controlling for mFI, ASA Classification, postoperative SVA, increasing decade of life, and male gender (model 1) returned the lowest AIC of 54.45, suggesting the best model fit. With respect to model discrimination, the ROC curve for model 1 yielded a C-statistic of 0.87, which upholds excellent discrimination. Lastly, with respect to model calibration, the p-value of the Hosmer-Lemeshow Goodness-of-Fit test was not statistically significant, p=0.938. Adding covariates in multiple logistical regression modeling via a forward stepwise approach increased the AIC and/or decreased the C-statistic; therefore, a multiple regression model with the five covariates – mFI, ASA Classification, postoperative SVA, increasing decade of life, and male gender – was selected for the regression analysis in Table 3."

I recommend publishing the manuscript if the authors would consider these minor requested changes.

We appreciate the reviewer's time and effort to provide candid feedback that will strengthen the candidacy for our manuscript publication in the esteemed *Journal of Spine Surgery.*

Reviewer B

The authors present their findings on the association between spinopelvic sagittal alignment on risk for discharge to rehabilitation facilities following minimally invasive LLIF/TLIF. The current body of literature is limited on this topic, and the authors attempt to address this by providing analyses regarding postoperative radiographic parameters and their impact on discharge disposition. Publication of this manuscript is recommended, as it provides greater insight into the association between postoperative radiographic parameters and discharge disposition in minimally invasive surgical patients. While the authors should be commended for a well-written manuscript, I have a few suggestions, mostly stylistic, that may improve the manuscript prior to publication:

General Comments:

- Introduction: No major revisions necessary- please look at "Specific Comments" section below to address any comments regarding this section.
- Methods: The methods section needs some clarification, detailed below.
- Results: The Results section is well-written, with few minor points to be considered as detailed below.
- Discussion: No major revisions necessary

Specific Comments:

- Line 48- Incorrect idiom usage- "centered about" should be replaced with "centered around" or "centered on"

Corrected to "centered around"

- Line 68- Please rewrite this sentence after "however"

Corrected to "this potential relationship has been poorly established in the literature."

- Line 85-86- How were LLIF/TLIF patients identified? If CPT codes were used, please identify these CPT codes.

The reviewer raises an excellent question, which has now been clarified in the methods section: "Cases were manually abstracted from the daily operative schedule at our institution."

- Lines 85-86- Were these all single-level LLIF/TLIF? If not, the number of levels should be controlled for in the logistic regression analysis, given that multi-level fusions are more complex/invasive and may preclude a patient from being discharged to home.

We appreciate the reviewer's suggestion. The manuscript has now been corrected to "Cases were limited to 1- or 2-level operations."

- Line 102- presumes should be plural when referring to "higher scores" or make "higher scores" singular

Corrected to "wherein higher scores presume…"

- Lines 103- how many evaluators were there for each patient? And what was the ICC?

We value the reviewer's query. Radiographic parameters were only calculated once. Neither intraclass nor interclass correlations, or any other reliability assessments, were calculated. This has now been included under the Limitations heading within the Methods section.

- Line 118- Are these quantitative variables? It seems like this subparagraph on the cohort stratification into "discharge to home" or "discharge to rehabilitation facility" refers to categorical, non-quantitative variables…please edit

The subheading has been changed to "Categorical Variables".

- Line 126- Did the authors test for normality? Age, for example, is a continuous variable that seems unlikely to be normally distributed in a cohort of 83 patients, which would require Mann-Whitney-U-Tests for analyses rather than T-Tests

The reviewer raises a valid concern. Means of continuous variables were first calculated with a variance ratio test. Continuous data that followed a Gaussian distribution was compared using a Student's t-test; non-normally distributed numbers were calculated with Welch's test. This has been added to the Methods section.

Following a variance ratio test, we reject the null hypothesis that age is normally distributed between the discharge-to-home and discharge-to-rehab cohort. As such, Welch's t-test provided the appropriate P-value. In the Results section, the sentence comparing age has been clarified: "The mean age of 73.1 ± 1.6 years in the discharge-to-rehab cohort was statistically significantly older than the mean age of 63.9 ± 1.3 years in the discharge-to-home cohort (Welch's t-test, *P*=0.002)."

- Line 152- It would be helpful to provide comparisons to the control cohort (n (%)). I would suggest using the Bonferroni corrected tests to compare the proportions to assess whether there is a statistically significant difference in complication rates between control and rehab cohorts for each of the complications. As it is written now, there is one p-value for DVT, ileus, and hematoma listed in the "Results" section, so using Bonferroni corrected tests would certainly help differentiate which proportions are actually significantly different.

We respect the reviewer's comments, but perhaps there is a misunderstanding. A p-value=0.010 applies to each complication. The sentence in the Results section has now been clarified: "The discharge-to-rehab versus discharge-to-home division saw one deep vein thrombosis (9% vs 0%, *P*=0.010), one ileus (9% vs 0%, *P=0.010*), and one wound hematoma (9% vs 0%, *P*=0.010). No other complications statistically differed." Because pairwise comparisons were not performed, a Bonferroni correction is not applicable.

- Lines 156, 189- I would refrain from saying "equivalent," since they aren't- I think saying "not statistically significant" would be better

The sentence has been corrected to "In spinal alignment parameters, the preoperative SVA of 5.4 cm in the discharge-to-rehabilitation cohort did not statistically significantly differ from 5.1 cm in the discharge-to-home cohort [*Table 2*; *Figure 2*]."

- Line 158- increased from what? I presume it's the preoperative, but this should be clarified since we're comparing preop to postop between two cohorts. This may become confusing for some readers, and the clarification that the increase in SVA is from preop values may help.

The sentence has been clarified to "As compared to preoperative measurements, the postoperative SVA increased to 8.0 cm in the discharge-to-rehab division versus decrease to 3.6 cm in the discharge-to-home division (*P*<0.001)."

- Lines 161-167- Did the authors conduct any post-regression diagnostics? This wasn't reported in the manuscript, and this is important as it will give readers a better understanding of how predictable the regression model is in predicting discharge to rehab. The HL goodness-of-fit test or reporting a C-statistic for the logistic regression models would help readers better understand how well the model is able to predict.

We thank the reviewer for the suggestion. The adjusted odds ratio for each of the various spinal alignment parameters has now been included in the sensitivity analysis in Table 4.

Table 4. Sensitivity analysis of 3 different multiple regression models. Model 1 is the most appropriate based on three criteria: 1) Akaike's Information Criteria (AIC), 2) C-statistic corresponding to the area under the receiver-operating characteristic (ROC) curve; and 3) P-value of the Hosmer-Lemeshow Goodness-of-Fit Test

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The Results section now explains "Multiple logistical regression modeling was based on three parameters: 1) AIC; 2) model discrimination via the C-statistic; and 3) model calibration via the Hosmer-Lemeshow Goodness-of-Fit test. Of the three models enumerated in Table 4, a multiple logistical regression controlling for mFI, ASA Classification, postoperative SVA, increasing decade of life, and male gender (model 1) returned the lowest AIC of 54.45, suggesting the best model fit. With respect to model discrimination, the ROC curve for model 1 yielded a C-statistic of 0.87, which upholds excellent discrimination. Lastly, with respect to model calibration, the p-value of the Hosmer-Lemeshow Goodness-of-Fit test was not statistically significant, p=0.938. Adding covariates in multiple logistical regression modeling via a forward stepwise approach increased the AIC and/or decreased the C-statistic; therefore, a multiple regression model with the five covariates – mFI, ASA Classification, postoperative SVA, increasing decade of life, and male gender – was selected for the regression analysis in Table 3."

- Line 184- how would this profoundly alter patient outcomes? This sentence needs clarification.

We are grateful for the reviewer's remarks. The sentence has been changed to "Because discharge disposition serves as a surrogate for favorable postoperative outcomes, our findings may profoundly alter patient outcomes." Please note that the following sentence explains how discharge-to-rehab serves as a marker of patient outcomes.

- Lines 212-223- Did the authors analyze predominant symptom/diagnosis in the current manuscript? Based on Cheng et al.'s study, it seems that there were differences in postoperative SVA between radiculopathy, low back pain, and neurogenic claudication groups, and so these should either be controlled for in the current manuscript or mentioned in the limitations.

We welcome the reviewer's feedback. Unfortunately, diagnosis was not available in the current study; the Limitations section now reflects this. "In terms of patient symptomatology, chief complaint – such as radiculopathy, neurogenic claudication, pure axial back pain – was not collected in the current study. Because these presenting symptoms have been included in the regression analyses of prior studies [Cheng et al, "Effect of Single-Level Transforaminal Lumbar Interbody Fusion on Segmental and Overall Lumbar Lordosis in Patients with Lumbar Degenerative Disease"], the

multivariable analysis presented in Table 3 is subject to an incomplete control of potentially confounding variables."

Overall, the manuscript provides clinically relevant insight on predictors for discharge disposition in minimally invasive LLIF/TLIF. The authors should be commended for a well-prepared manuscript and may consider the aforementioned suggestions to improve their manuscript for publication.

We appreciate the reviewer's time and effort to provide candid feedback that will strengthen the candidacy for our manuscript publication in the esteemed *Journal of Spine Surgery.*