Peer Review File

Article information: https://dx.doi.org/10.21037/ls-22-1

Reviewer A

Comment 1: The title is unnecessarily long and unclear. Please consider to shorten it.

Reply 1: First of all, thank you for your time and substantive criticisms. I tried to answer your objections and implemented many changes in the text, which definitely increased the quality of the work. Thank you!

To your first point: I don´t know which word to delete... I added even aspects of the methodology.

Changes in the text: Please see/correct line 2-3

Comment 2: The abstract needs further revisions. In the background, the authors did not describe the objectives, clinical significance, and knowledge gaps of this research topic.

Reply 2: Thank you! I rewrote the abstract many other passages of the methodic, results, discussion, and added various tables concerning frequencies and missing data to the Supplementary Appendix 5-7, as you recommended. Done.

For me, the article has really improved now. Thank you.

Changes in the text: Maybe, please see actually the whole article?

Comment 3: In the methods, please clearly indicate that this is cohort study of a group of patients.

Reply 3: Done

Changes in the text: Please see line 139.

Comment 4: Please briefly describe the inclusion of subjects, measurements of outcomes, and follow up procedures. Main statistical methods are also needed here. The Visick score, symptom score, and patient rating should also be briefly described.

Reply 4: These points are very important and we tried to put great emphasis on clear-cut definitions. I have incorporated your criticism and reformulated many part with regard to subjects, measurements of outcomes, and follow up procedures etc.. In addition, I included these points in the discussion and discussed them as critical weaknesses of the work. I hope I could meet your expectations.

Changes in the text: Please see line 138-143; 164-182; 318-361; Supplementary Appendix 4-7.

Comment 5: In the results, in addition to the rates of these outcomes, the T0, T1 and T5 scores of Visick score, symptom score, and patient rating should also be reported. The current conclusion should be made with cautions because of no control group. It is difficult to assess the efficacy and safety in the clinical methodology.

Reply 5: I revised these chapters, added data and tables in the Supplementary Appendix, and additionally extended the part of the methodological limitations in the discussion. I hope it is better now.

Changes in the text: Please see line 318-361; 385-388; Discussion 408-460; Supplementary Appendix 4–7.

Comment 6: In the introduction part, the authors should have comments on the clinical importance of the research topic, and limitations and knowledge gaps of previous studies, to support the needs for this research.

Reply 6: Thank you for your encouraging words. I have rewritten the introduction. It was indeed necessary. I feel happy about it now.

Changes in the text: Please see line 98-136

Comment 7: Fourth, the methodology part is too long and please consider to move some details to the Supplementary Appendix. Please describe the clinical research of this study and of patients should be reported.

Reply 7: I have shortened the methodology in particular the parts of the DeltaMesh and the surgical procedure. But it is still long. Maybe I should transfer the DeltaMesh and surgery etc. to the Supplementary Appendix. Actually I wrote a separate paper with many technical details and specialties of the new DeltaMesh. But I was asked better to integrate these data in this article. Therefore the chapters became long.

Changes in the text: Please see line 218-254

Comment 8: In statistics, please describe the test of normality of variables. Please explain why hierarchical ordered logistic regression models were used and the purposes of these analyses. Please describe the P value of statistical significance.

Reply 8: The analysed scores are measured on an ordinal scale, hence no normality tests were conducted. We have used the regression analysis in order to model the scores in relationship to the different measurement time-points at baseline and after surgery. Hierarchical ordered logistic regression model was chosen because of the ordered outcome variable and the repeated measurement design, whereby the scores were measured repeatedly in the study subjects. The model included parameters for the time points (measurement occasions), and we tested the null-hypothesis that the time effects are zero using a likelihood ratio test. This null-hypothesis would suggest that the distribution of scores are the same across all the time points.

I adopted your objections and rewrote many parts of the results and added data to the Supplementary Appendix.

Changes in the text: Please see line 260-284; 334-335; 343; 350; 357.

Comment 9: The authors must be aware of the methodology limitations of this study, which cannot assess the treatment efficacy and safety.
Reply 9: I expressed your objections at various parts of the discussion as weaknesses of the study.
Changes in the text: Please see line 408-460; 531-535; 572-574.

Reviewer B
Comment 1: How is this concept different from Allison who presented his life's work showing simple repair of the hiatal hernia leads to significant reflux? The Allison technique has received widespread application and is today the most popular of the transthoracic procedures used for the repair of hiatal hernia.
Reply 1: First of all, thank you for your time and substantive criticisms. I tried to answer your criticisms to your satisfaction and implemented appropriate changes in the text, which definitely increased the quality of the work. Thank you!
Please allow me to make the following comments on the Allison technique:
Allison was without question a great surgeon! But the invented procedure is an open transthoracic operation with all corresponding risks and complications of a thoracotomy. LOEHDE, on the other hand, is a laparoscopic minimally invasive procedure with almost exclusively 5mm incisions. This means the operation time can be limited to 1 hour and the patient is mobile a few hours after the procedure. Even a drainage is rarely necessary.
Conceptually, in Allison´s operation, there is the special surgical problem of guiding the stomach from intrathoracally back into the abdominal cavity and fixing it there. This obstacle is due to the fact, that Allison´s operation is basically performed from the wrong side of the diaphragm. Therefore, organ repositioning is of course no difficulty at all in all transabdominal procedures, as the stomach can thus simply be retracted back into the abdomen.
Allison tried to solve this problem by making the Allison-typical "counterincision" in the diaphragm. This additional incision in the diaphragm is necessary to place instruments and sutures in the abdominal cavity to achieve the caudal traction and organ fixation intrabdominally. Of course, these procedures are not simple, time-consuming, and traumatising.
In particular, this "counterincision" is a risk for long-term complications. Donald B Effler stated 1964 in his article "Allison´s repair of hiatal hernia: "Late complications of diaphragmatic counterincision and technique to avoid it". He wrote: "This seemingly innocuous step in a well-conceived operation has, on occasion, resulted in serious late complication. Direct herniation through the "counterincision" may occur early or late after operation. ... its effects may be devastating...".

Donald B Effler tried to overcome these disadvantages of a "counterincision" in the diaphragm by many diaphragm punctures instead, however "... employment of a series of punctures through the dome of the diaphragm might lead to laceration of the spleen or perforation of a viscus."
In addition Allison advises that "the proximal edge of the hiatus (should) be sutured directly to the undersurface of the left diaphragm" and thus to perform a skewed but not anatomically correct hiatal closure. And of course, any hiatal mesh enhancement was not part of the technique.
In summary, I think Allison approach of anti-reflux surgery is quite different in many aspects from laparoscopic anatomical hiatus reconstruction of LOEHDE.
Changes in the text: None

Comment 2: Please define the types of hernia, and the number of patients with each type. Also define the indication for surgery based on type. The indication for repair of type 1 hernia is confirmed GERD. How many patients with Type 1 hernia had abnormal pH test vs severe esophagitis?
Reply 2: Thank you for these questions. All types of hiatal hernias (types I–IV) were included but not differentiated. In practice a pre- and intra-operative definition and differentiation in particular between type I-III, II-III, and even I-II is hardly exact and is assessed differently by different examiners.
But this objection points to a very interesting unresolved question of how different hernia types can cause different symptoms. With regard to the pathophysiological concept of CODIS, axial displacement of the stomach in type I hernia can easily compromise the cardiooesophageal junction, resulting in loss of reflux control. However, when the stomach slides strictly para-oesophageal, in particular dorsal to the oesophagus, without pushing the oesophagus out of the cardiac pressure zone as in type II hiatal hernia, Patients have various symptoms such as incarceration, pain, dyspnoea, etc., but little or no symptoms related to CODIS function, e.g. reflux control. CODIS is still functional. However, if the cardio-oesophageal junction becomes increasingly compromised, as in a type III mixed hiatal hernia, there will inevitably be a loss of reflux control as well.
It seems that the different symptoms of the different types of hernia are like two sides of the same coin. Consequently, a correct anatomical reconstruction can apparently cure all these different types in the same way.
I added all your objections to the text. Thank you!
Changes in the text: Please see line 518-535

Comment 3: What was the outcome of esophagitis after surgery? How many patients had normalization of pH test after surgery?
Reply 3: Many obstacles turned out: the patients came from different parts of Germany and the EU, different health insurance companies, cost structures, and lack of interest or negative attitudes of the treating physicians towards surgical treatment and the refusal to order further examinations without medical reason

prevented a standardised direct follow-up, for example, by pH-metrics or endoscopy.
However, the exception was patients with Barrett's metaplasia. These patients were followed up endoscopically and histologically by 4-quadrant biopsies after 1, 2, and more years. The results were interesting: Barrett metaplasia histopathologically disappeared completely (roughly estimated and dependent on the size) and in about 40% of patients, improved increasingly with reduction in size in another 40%, and was stable in patients with extensive long-segment Barrett. This is why we actually recommend the operation specifically to patients suffering from Barrett metaplasia.
Interestingly, there were patients in whom Barrett's metaplasia disappeared postoperatively, but later, when a recurrence occurred, Barrett cells could be detected again.
I added all your follow-up objections as weaknesses of the study to the text.
Changes in the text: Please see line 408-460

Comment 4: Why did you choose to use an unvalidated symptom score?
Reply 4: The crucial aim of this study was not only to measure the outcome of well-being, QoL, reflux and recurrence frequency, but to identify in detail which single preoperative symptom of the patients would actually be influenced by the reconstruction of the oesophagohiatal unit alone. This should reveal the connection between a hiatal organ displacement and the many complaints such as cardiac arrhythmias, dyspnoea, detailed food intolerances and other reflected in the newly created symptom score.
This differentiation is not adequately covered by scores such as Visick score, Quality-of-life in Reflux and Dypepsia (QOLRAD), Gastrointestinal Symptom Rating scale (GSRS) and SF-36 Health Survey etc..
Thank you for these important objections. I added this to the text.
Changes in the text: Please see line 434-442

Comment 5: The follow up was very poor at 5 years with no 10-year data, so the **title is misleading.** Only 37.8% of the 14.8% with 5 year follow up completed the questionnaire, so these are very small numbers upon which to assess results. Please discuss.
Reply 5: Yes indeed, T5 follow up is very poor. I changed the wording for better transparency:
The follow-up rate of the 1351 operated patients within the 10-year period from January 2007 to December 2016 was 96% at T0 (1297/1351), 68.6% at T1 (927/1351), and 14.8% at T5 (200/1351).
Due to the end of the study after 10 years, observation point T1 could only be reached by 1287 patients and the questionnaire response rate was 72% (927/1287). Observation point T5 could be reached by 529 patients and the questionnaire response rate was 37.8% (200/529). The continuous follow-up of

the patients ended in December 2019.
In addition, I added tables of frequencies and missed data of the symptom score, Visick score, and patient rating score to the Supplementary Appendix 4-7.
Changes in the text: Please see line 311-317, Supplementary Appendix 4-7.

Comment 6: Recurrence rates are known to be higher for PEH compared to sliding (Type 1) hernia repairs. What was recurrence rate by type of hernia?
Reply 6: Please compare " Reply 2, number 2". We could definitely not confirm PEH as risk factor for recurrence. This may possibly reported because the diaphragmatic defect in PEH is often quite large, which is difficult to close with conventional measures. But with LOEHDE it is the identical surgical procedure focussing on the hiatal reconstruction and firm closure. There is no need for a special approach in PEH. All your objections were incorporated in the text.
Changes in the text: Please see line 518-535