

Peer Review File

Article information: <https://dx.doi.org/10.21037/jtd-22-1373>

Reviewer A:

Comment: Well written paper detailing need for evidence-based approach to post-operative care in patients undergoing lung surgery

Reply: Thank you.

Reviewer B:

Comment 1: This is a very well written manuscript on the key elements of ERAS in lung cancer surgery and, in particular, chest drain management. It is an interesting read and, in my opinion, the specialist audience will benefit from the article calling in to earlier chest drain removal, in the absence of air leak and chylous drain output. This will be a good addition to the available guidelines.

Reply 1: Thank you.

Comment 2: I am wondering what the author's opinion on the ideal size of the chest drain - does it matter, should it be standardized in addition to your recommendation to use a single chest drain?

Reply 2: This is a good point. However, there are no good studies analyzing the impact of chest tube size after lobectomy. There have been trials on the treatment of thoracic empyema using small caliber drains and showing good efficacy, similar to that of a large bore chest tube. While it may be intuitive to recommend small caliber drains to reduce drain pain (and indeed many of us use them), it is difficult to recommend in the absence of evidence. An additional section has been included to address this.

Comment 3: In the Digital drainage systems part of the manuscript, I would mention about the ability to maintain a regulated suction pressure of such systems according to the fluctuating intrapleural pressure as opposed to the wall suction which may apply a fluctuation suction pressure itself.

Reply 3: Thank you. Changes made to the manuscript.

Comment 4: Also, I am wondering about the author's opinion and practice of prehabilitation before the lung resection and whether it is worthwhile to mention in the manuscript, among the other elements of ERAS.

Reply 4: I agree that prehabilitation is important, particularly in high risk individuals. However, it is not necessarily related to postoperative chest drain management and so I have chosen not to include it in the text. No changes made.

Comment 5: Once again, I commend on the well and clearly written manuscript dedicated to ERAS.

Reviewer C:

General Comment

Comment 1: This is an excellent paper dealing with the Enhanced recovery after surgery care elements, particularly chest tube management.

Reply 1: Thank you

Comment 2: My evaluation is that the paper is publishable, however, there are no figures or tables in this review, so it would be easier to read if there were a table summarizing the main points of the whole review.

Reply 2: Agreed. 2 tables inserted.

Comment 3: I hope that my comment is very useful for the improvement of the article.

Reviewer D:

Comment 1: Do not use abbreviations in the title

Reply 1: Agreed. This has been changed in the manuscript.

Comment 2: Consider to change the term ERAS to ERATS (Enhanced Recovery After Thoracic Surgery)

Reply 2: Thank you for the suggestion, but the correct term is ERAS although it is acknowledged that ERATS is becoming more popular and is interchangeable. See: www.erassociety.org. No changes made.

Comment 3: Is this article by any chance part of JTD's special series on air leakage? If so, please state so in abstract and Introduction and/or methods.

Reply 3: You are correct. This is part of the JTD series on air leakage. As I have had no instruction on format regarding abstract and methods sections, I will leave this to the series editors to advise and manage.

Abstract

Comment 4: From the abstract it does not become clear what the aim/goal of the present study is, along its design and conclusion.

Reply 4: An additional sentence has been inserted at the start of the abstract. Thank you for the suggestion.

Comment 5: The article seems to be a narrative review. This should also be mentioned.

Reply 5: This is part of the JTD series on air leakage. As I have had no instruction on format regarding abstract and methods sections, I will leave this to the series editors to advise and manage.

Introduction

Comment 6: The introduction is relatively lengthy; 3 pages. Generally, the introduction section is written funnel-like, ending with an aim or objective of the present paper/study. Since the above does not apply to the current paper, a clear red line is missing.

Reply 6: Agreed. The structure of the opening 6 paragraphs has been completely

changed to create a 2-paragraph introduction with an explicit aim in the last 2 sentences. A new section (ERAS) then follows.

Methods

Comment 7: Regardless of the type of article, even a small methods section is highly advised. Even in a narrative review.

Reply 7: This is part of the JTD series on air leakage. As I have had no instruction on format regarding abstract and methods sections, I will leave this to the series editors to advise and manage.

Main body

Comment 8: The abbreviation PONV (line 107) is introduced but not written in full first.

Reply 8: The abbreviation is introduced and written in full in the original introduction. It remains in the new introduction. No changes made.

Comment 9: It would be worthy to include two recent studies on the need for pleural drainage after VATS wedge resection:

- o Laven, I. E. et al., (2022). Risk of Pneumothorax Requiring Pleural Drainage after Drainless VATS Pulmonary Wedge Resection: A Systematic Review and Meta-Analysis. *Innovations*, 17(1), 14-24.

- o Laven, I. E. et al., (2022). A No-Chest-Drain Policy After Video-assisted Thoracoscopic Surgery Wedge Resection in Selected Patients: Our 12-Year Experience. *The Annals of Thoracic Surgery*.

Reply 9: Thank you for the references. I have included the meta-analysis and changed the text slightly. It appears to be similar to Huang L, Kehlet H, Holbek BL et al, 2021, JTD.

Comment 10: It should also be noted that the current chest tube strategy/management depends on the type of lung surgery performed. The same counts for the number of chest tubes. The present elaboration seems to be too generalized. This also comes forth from a missing specific aim/objective/guidance of the present study.

Reply 10: The vast majority of lung resection surgeries can be managed with a single tube, no suction etc as laid out in this review. The reviewer is maybe referring to more complex surgeries such as pneumonectomy or where there is management of a complex pleural space or massive air leak. The manuscript already contains the word “routine” at multiple points, but this has been re-emphasized and a new sentence included in the aims as the last sentence in the new introduction.

Comment 11: What about early chest tube suction for lung apposition, followed by a no suction policy in patients without air leakage. Versus suction in patients with air leakage.

Reply 11: This was addressed in the ERAS Society guidelines and the Society for Translational Medicine guidelines. Routine suction for any period does not confer an

advantage, so why use it?

Comment 12: Is anything known about the mechanism by which digital drainage systems reduce air leak duration as mentioned?

Reply 12: This is an interesting question. The short answer is no, but may be down to reduced inter-observer variability or an objective measure of when an air leak actually stopped. In other words, decisions can be made sooner. The manuscript has been altered to reflect this.

Comment 13: Line 220: in theory, a chest tube is no contra-indication for mobilization, but may even be advised to allow for intrathoracic air evacuation.

Reply 13: Apologies, but I do not have access to line numbers. I assume the reviewer is referring to the sentence: “After thoracic surgery, the presence of a chest tube is an important barrier to early mobilization”. The word barrier does not imply a contra-indication to mobilization, just that it may be difficult to achieve. However, the sentence has been changed for clarity.

Comment 14: What are your thoughts on extended (post discharge) thrombosis prophylaxis to prevent VTE?

Reply 14: An interesting point. Its role has yet to be firmly established in thoracic surgery, unlike pelvic surgery. However, current guidelines suggest it should be considered based on risk (e.g. using the Caprini system) or surgical type (e.g. pneumonectomy). However, inclusion in this manuscript would be beyond the remit of the review.

Comment 15: “Pain relief pathways should include multimodal enteral and parenteral analgesia with regional analgesia or local anesthetic techniques”. I would avoid such a statement given that the optimal pain treatment strategy is not found yet and subject of ongoing research alongside the given that is highly subjective to local preferences or historical use.

Reply 15: I strongly disagree with this assertion. The ERAS Society, and indeed all major peri-operative care associations, are clear that a standardized multimodal approach to pain relief, including good regional anesthesia, is recommended with the aim of reducing postoperative opioid use. The sentence the reviewer refers to is present in one form or another in all major international guidelines on peri-operative pain relief, irrespective of the type of surgery. The reviewer may wish to read the editorials in ATS and JTCVS:

Commentary: Enhanced recovery and postoperative opioid use: Good for the patient, good for society? Batchelor TJP. J Thorac Cardiovasc Surg. 2021 May;161(5):1703-1704.

Can Enhanced Recovery Pathways Prevent Opioid-Related Harm in Thoracic Surgical Patients? Batchelor TJP. Ann Thorac Surg. 2023 Jan;115(1):256.

Conclusion

Comment 16:

- The conclusion is not substantiated by the article's main body.
- Anxiety for whom?
- What can we learn from this article? What is its novelty?

Reply 16: The conclusion has been rewritten and I believe accurately summarizes the preceding discussion.

Reviewer E:

Comment 1: This is an excellent review article for ERAS procedures and specifically the management of chest tubes after thoracic surgical procedures. I thoroughly agree with the statements and conclusions of this article and believe that there are a lot of thoracic specific dogmatic principles that come into play with the management of chest drains, and bringing this issue back to the forefront of surgeons in the form of a review is an excellent idea.

Reply 1: Thank you

Comment 2: Some specific issues that I feel would improve the article.

There is no need for the bolding of catabolism and insulin resistance in the introduction, this process does not continue through the paper for emphasis and otherwise just comes across as somewhat awkward when only employed once.

Reply 2: Agreed. This has been changed in the manuscript.

Comment 3: The claim that through starling forces the amount of pleural fluid produced is increased when chest tubes are placed to suction needs to be supported with a reference.

Reply 3: Agreed. The reference is the ESTS textbook of thoracic surgery, chapter 11.

Comment 4: Don't quote me on the exact physics, but an 8cm column of water can only create a positive amount of pressure related to the height of the column of water, and the inference that water seal creates suction I think is simply false. This paragraph should be removed. The column of water is actually the amount of positive force the the air in the pleural cavity has to reach in order bubble through the water seal container.

Reply 4: I am afraid this is largely incorrect. I refer the reviewer to the ESTS textbook of thoracic surgery, chapter 11, section 3.3.2. Other references may include Refai M et al, Eur J Cardiothorac Surg 2012 Apr;41(4):831-3. Indeed, the "gravity mode" seen in digital chest drainage systems is an artificial construct designed to replicate the weight of the column of fluid (and the subsequent generation of negative pressure at the tip of the tube in the pleural space) in an underwater seal. I have now changed the manuscript to reflect this discussion.