

Peer Review File

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Reviewer A

In general, the article is well written. Nevertheless, reading the title "Overcoming Adversity in Thoracic Surgery in the Post COVID 19 Pandemic Era", and reading the article, I think that something is missing.

The Authors wrote: to note the impact on short and long term outcomes for those who had delayed care due to the back-log of cases.

Comment 1: my personal feeling is that as thoracic surgeons we need to look closely GGO, as they represent a sequela of COVID infection but also they could hide a cancer. This circumstance should be discussed to complete the editorial. Ground glass opacities of the lung before, during and post COVID-19 pandemic. *Annals of Translational Medicine*. 2021 Jul;9(13). PMID: 34422954

Reply 1: The reviewer recommends reporting additional information regarding short- and long-term outcomes, particularly with GGO as a sequela of COVID disease, to provide a more complete understanding of the manuscript.

We thank the reviewer for this recommendation and for providing literature on their recommendation. We agree that this will add complexity to future screening of GGOs that warrants mention in the manuscript.

Changes in the text: We have added additional commentary on GGO (lines 84-86)

Reviewer B

I just have a couple of comments on this paper, and I thank the authors for reading and considering them.

Comment 1: In lines 38 and 39 you include the following statement: "Currently, European countries have the highest rates of tobacco use as well as lung cancer (1)". I couldn't find data on the incidence of lung cancer in Europe in your reference 1. According to the International Agency for Research on Cancer, the estimated crude incidence rates of lung cancer in more than half of European countries in 2020 is comparable or lower than the ones in the US and Canada. Please consider rewording your sentence and including a different reference.

Reply 1: The reviewer reports a discrepancy in data regarding smoking incidence in European countries. We greatly appreciate you bringing this to light as it was misinterpreted during initial review.

Changes in the text: We have deleted this sentence (original manuscript lines 38-39)

Comment 2: The prevalence of tobacco use in Europe is not reported in reference 1. You can find that here: https://gateway.euro.who.int/en/indicators/epw_20-tobacco-use-prevalence/. According to the last, smoking prevalence in Europe has decreased almost 30% in the last 20 years.

Reply 2: The reviewer reports a discrepancy in data regarding smoking prevalence in European countries. We appreciate the reviewer providing additional and accurate information regarding smoking prevalence in Europe.

Changes in the text: Similar to "Comment 1," we have deleted this sentence regarding European smoking (original manuscript lines 38-39)

Comment 3: The trending of smoking rate in the US is not correctly quoted in your reference 1. A much more adequate reference is: <https://www.hhs.gov/sites/default/files/2020-cessation-sgr-full-report.pdf>

Reply 3: The reviewer reports a discrepancy in data regarding smoking trending rate in the US. We appreciate the reviewer providing additional and accurate information regarding smoking rate in the United States.

Changes in the text: We have added the information regarding US smoking prevalence (lines 38-40)

Reviewer C

I am well aware that you are quite limited in word numbers and therefore my feed-back might probably be difficult to compromise with your word count. However, I hope that my input could be integrated without major difficulties.

For reviewers who do not have access to the manuscript of Wang et al, it is difficult to judge in this editorial the content written about this paper. For readers who do not know the article, the editorial should briefly point out with a few more details the reasons why Wang et al obtained positive results.

In the following lines, I would like to point out some minor queries.

Comment 1: Line 33: "...which highlights positive findings in a tertiary-referral medical center."

When you talk about positive findings, could you mention them briefly in one or few keywords so that someone not knowing the article of Wang et al can get a glimpse of it? Do you mean by it "increasing surgical volume to decreasing average length of stay"? In this case, it would be helpful to briefly add with one word or a few more, if possible, by what means Wang et al achieve these positive results.

Reply 1: The reviewer recommends adding more specific information to the editorial manuscript that will allow readers to have more clarity and more context behind the motivations of the manuscript of Wang et al, "specifically line 33" in the original text. We thank the reviewer for this recommendation to add clarity to our editorial.

Changes in the text: We have made the recommended changes (lines 32-35)

Comment 2: lines 63 to 65: "the reported in-hospital mortality of 0.06% at Shanghai Chest Hospital is exceedingly much lower than the reported in-hospital mortality of lobectomies in the United States at 0.7% (3,4)."

Please point out in short, if possible, the reasons for the difference in mortalities. Is it because Shanghai Chest Hospital does more minimal thoracic surgery than the US or are there other reasons, too? In addition, is this the mortality for minimal thoracic surgery both in the US and Shanghai or does this count for thoracic surgery in general?

Reply 2: The reviewer recommends adding reasons for the difference in mortalities seen and if the data presented is for the mortality for minimal thoracic surgery both in the US and Shanghai or for thoracic surgery in general.

We thank the reviewer for this suggestion. It is difficult to exactly pinpoint differences in in-hospital mortality but may be due to more minimally invasive surgeries performed in Shanghai or patient selection (BMI, stage). Additionally, the mortality data is for minimally invasive lobectomies (VATS and robotic) in Shanghai and for all lobectomies (open, VATS, and robotic)

Changes in the text: We have added this additional commentary (lines 77-80)

Comment 3: lines 73 to 75: "In the midst of an uncertain future, Shanghai Chest Hospital has provided a potential envisioned framework to allow tertiary centers to continue to provide quality care to patients even in the most challenging circumstances."

As mentioned above, if not already clarified in the previous lines, could you please describe for

readers, who do not know the paper of Wang et al, the reasons by what means Shanghai Chest Hospital has provided its positive input? Is it because of a high case load of minimal thoracic surgery or is it because the surgical techniques of minimal thoracic surgery differ from the US?

Reply 3: The reviewer recommends adding more specific information to the editorial manuscript that will allow readers to have more clarity and more context behind the motivations of the manuscript of Wang et al, “specifically line 73-75”. We appreciate the reviewer for bringing this to our attention in order to provide a more meaningful editorial. Changes in the text: We have added these changes (lines 92-95)
