Reviewer’s report

Title: Oxidative and pre-inflammatory stress in wedge resection of pulmonary parenchyma using the radiofrequency ablation technique in a swine model.

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Reviewer: PETROS SOUTOULIDES

Reviewer’s report:

I would like to thank the editors for inviting to review the article under the title:

“Oxidative and pre-inflammatory stress in wedge resection of pulmonary parenchyma using the radiofrequency ablation technique in a swine model.”

I hope that my remarks will improve the quality and strength of the scientific merit of the paper.

It is a fact that the last decades, there is a tremendous interest about ablating techniques concerning metastatic non-operative nodules in the liver, kidney and lungs. These techniques are mostly used by the interventional radiologist through transdermal access, under CT guidance. The last decade radiofrequency ablation has expanded its indications for partial hepectectomy offering a bloodless plane of resection and also for kidney partial resection. From my search in the literature through pubmed, using the words radiofrequency ablation and lung resection or lobectomy, I didn’t find any article except of Dr. Tomos’ article that is mentioned in Mr. Karaiskos’ article and as I understand Dr. Tomos is also a participant in this study. So answering in the first question I would say that it is really a new idea and the question posed has not been posed before concerning lung resection using the radiofrequency ablation technique.

By reading the methods, I think that are detailed described, especially the surgical part, but also the methodology of tissue analysis and the statistical analysis.

Concerning the data provided by the authors, I could not compare their findings with any other relevant study, having in mind that there are no specific normal values for the markers tested in the porcine lung tissue and are also depending values from the kit used for measurement. So we should accept them as they are. Their statistical analysis is based on a simple paired student t-test, not a complicated one. I would only comment that the results are not so strong, perhaps because of the small number of the animals tested.

Concerning manuscript’s adherence to the relevant standards I would say that their article possess the basic standards of report with tables of their measurements and statistical diagrams analyzing their findings, having no much difficulty in understanding them.

In their discussion, the authors try to compare their findings with other relevant
studies being conducted in liver, since as they mention there is still no other equivalent study in the lung. Having read the discussion, my opinion is that oxidative stress and pre-inflammatory burden have not been fully verified as there are missing data concerning the clinical impact on the animals after the radiofrequency ablation assisted lung resection and the fact that there are no peripheral blood measurements of the markers tested before and after their resection, that would also add more strength in their conclusions. I agree with the authors, concerning the disadvantages-limitations of their study.

Concerning the abstract and the data supported, I think that is well balanced and adequate.

I have no special comments, just two observations. In the discussion, in the first paragraph, 5th line, 6th word it is written “thawig”. Do they mean “charring”, and not thawing? In their references I observed that the 11th and 17th references are the same. This error should be corrected.

In conclusion, my opinion is that this is an interesting article studying a new indication of radiofrequency ablation, and besides its aforementioned disadvantages, I would strongly recommend it for being published, as I think that could act as an incentive for further studies.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I have no competing interests