Reviewer's report

Title: Impact of comorbidities on postoperative complications and overall survival in patients undergoing laparoscopy-assisted gastrectomy for gastric cancer

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Reviewer: Young-Woo Kim

Reviewer's report:

1. Is the question posed by the authors well defined?
Comorbidities could impact complications and survival we know all. Is it needed to study again? Probably, No!
And, in laparoscopic gastrectomy, is there any difference? In what basis and what clinical effect this study expect? For example, if it is related, surgeons will do something more or making another decision? Rather we need more sophisticated questions like nomogram or risk prediction model.

2. Are the methods appropriate and well described?
Methods itself internally OK. But we need external validity for this kind of issue. Very well established highly expertise surgeon’s data cannot be generalized. Even very ill patient can be OK in this kind of center. Authors should consider multicenter pooled data collection to improve power and generalizability.

3. Are the data sound?
Data are quite sound. But, retrospective data always have limitation in objectivity and selection of the patients included because data management is not independent.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
It is acceptable in terms of multivariate analysis. Though it does not comply CONSORT in prospective study. However, from the clinical questions to the relevant outcomes should have been in line. Is the effect is clearly comorbidities or age in stage I disease? Is there any interaction between two variables?

5. Are the discussion and conclusions well balanced and adequately supported by the data?
“less invasive procedures such as a reduced extent of lymphadenectomy should be considered in patients with multiple comorbidities” and “sufficient lymphadenectomy should be performed in such patients,” is quite contradictory phrases. What is the thing authors argue indeed?
Reader for this manuscript is hardly change any clinical practice with this conclusion. They need something clear and can be implemented readily.
6. Are limitations of the work clearly stated?
No. Not at all.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Surely No. References are quite lost balance. Almost all are from Japan and Korean publications in very limited institutions.

8. Do the title and abstract accurately convey what has been found?
Abstract is too long and redundant. Conclusion has lost clear direction because of very contradictory description.

9. Is the writing acceptable?
OK for this.

Reviewer's report

- Major Compulsory Revisions
Pooled analysis is recommended with strict inclusion criteria to see effect of comorbidities only and increase power if possible.

- Minor Essential Revisions
Overall relevance of context should be considered.

- Discretionary Revisions
Hypertension and heart disease is separated but it looks tricky.
Transfusion data could be interesting.

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
'I declare that I have no competing interests'