Reviewer’s report

Title: Surgical treatment of mild to moderately dilated ascending aorta in bicuspid aortic valve aortopathy: The art of safety and simplicity

Version: 0 Date: 28 Oct 2019

Reviewer: Mate Petricevic

Reviewer's report:

Dear authors,

Thank you for submitting this original scientific manuscript to the JCTS. I was pleased to receive it as a reviewer.

This is an interesting article addressing very important research question. Whether to wrap or simply replace ascending aorta remains to be the matter of debate among surgeons. The results you provide may add to the current knowledge by giving us an idea what would be the possible benefits of wrapping aorta.

This is observational comparative study which unfortunately does not provide the best level of evidence. It remains unclear how the patients were selected for each type of procedure. These results would even be much better if you would be able to prospectively randomize these patients. Propensity matching would also add additional value to this research, but I am afraid you are simply under powered for such an approach to the existing data.

In "Methods" section you declare that IRB approval was obtained and individual patient's consent was waived. Few sentences later, you write the following: "Following approval from the ethical committee and signing of written consent forms...". This does not make a sense to me. Please clarify. The study was retrospective observational, so I assume there was no signed written consent forms. The text throughout the document should be consistent and I ask you to clarify whether or not you used written consent forms.

All the "wrap" procedures were performed by one surgeon. Here the question is whether this surgeon is more experienced surgeon doing more cases per year for a many years more?? If so, this could be a significant bias to the results you report as the results may present single surgeon performance difference rather than advantages provided by the surgical technique as such. Only wrapping was present in 45% of patients and was clearly done by single surgeon which raises the question of the single surgeon caseload. This may be a significant bias to the results and indeed needs to be clarified.

Furthermore, patients undergoing concomitantly to mitral valve repair surgery should inevitably be excluded as this may significantly influence the reported results such as cross clamp time and reasonably chest tube output.
"Discussion"

The "Discussion" section is wide and lacking focus to the research question. Authors should be able to provide the answers to the questions like:

1) What are the results and what conclusions can we draw based on the results?

2) What are the study methodological flaws and do these methodological drawbacks allow for meaningful conclusions?

3) What is already known in this field?

4) Which patients may benefit from one type of procedure? Maybe older patients? If so, why "wrap" patients tend to be younger??

5) You had more mitral valve procedures in "wheat" subgroup. Even though the difference is not significant we may not say this is irrelevant as the study was underpowered to estimate significance of this difference. This is interesting and is in collision to the general philosophy we have for "wrapping" procedures. "Wrap" patients tend to be younger and to have less combined procedures involving mitral valve repair. Without addressing this in "discussion" and declaring this as a serious drawback we may easily conclude that herein you compare one "wrap" surgeon to the others. "Wpar" surgeon does majority of cases (45%) and tends to operate younger patients and less complex cases. Case load, patients' age and combination with mitral valve repair certainly makes the difference and could easily explain the observed differences. This needs to be addressed, otherwise the results could not be interpreted meaningfully.

**Level of interest**

Please indicate how interesting you found the manuscript:

An article of limited interest

**Quality of written English**

Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

**Declaration of competing interests**

Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?
3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal