Reviewer’s report

Title: Endovascular repair combined with staged drainage for the treatment of infectious aortic aneurysm: a case report

Version: 0 Date: 25 May 2020

Reviewer: Jakub Sebastian Marczak

Reviewer's report:

Thank you for a chance to review. Here, I had been given the opportunity to assess scientific value of manuscript by Mr Zeng et al. who described quite peculiar approach to treatment of an infective aortic aneurysm.

I have several reservations to the above mentioned manuscript.

Firstly, case report must represent unique, rare or at least uncommon condition. In my experience infective vasculitis in the human aorta does not constitute neither rare nor unique condition and publications presenting case series are available on the major literature search engines: for example - Clin Cardiol. 2009Sep; 32(9); 488-490.

Secondly, In such case, as I understand, endovascular treatment may represent an unique approach to treatment of mycotic, pseudo - aneurysm of infectious origin. This, again has already been reported, for example in: Int J Cardiovasc Imaging 2013 Mar; 29(3):529-31.

Looking at this from a perspective of an active clinician, I also wish to comment on two aspects of presented paper. To start with, the Authors made an assumption: "Compared with open surgery, endovascular repair owe better early outcomes" - there is no reference backing that comment up. Currently, aortic infection represent one of the relative contraindications to stent implantation and in that case, in a relatively young patient without serious comorbidities (if we exclude DM) who may still be a good surgical candidate, using TEVAR is at least an off-label approach. Personally, I would not like to leave the audience with an impression that utilizing EVAR is a go to approach in patients with infected aneurysms. It has its place in treatment, but this needs further scientific clarification. Therefore, authors should comment on their rationale in choosing EVAR instead of conventional surgery, especially, when we may see a future need of re-intervention in this individual case.

To recapitulate, I congratulate Authors on successful treatment of this complex case but I am afraid that presented paper is repetitive and does not bring anything new to the cannon of knowledge.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No
Are the conclusions drawn adequately supported by the data shown?  
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?  
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

Not relevant to this manuscript

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Please indicate the quality of language in the manuscript:

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