

Peer Review File

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Reviewer #A:

In this single-center observational study, the authors sought to compare the surgical outcomes between DeBakey type I and II acute aortic dissection. Overall, 395 patients were enrolled between 2001 and 2018. As results, there were no significant inter-group survival differences both in short- and long-term despite more aggressive baseline pathologies in type I aortic dissection.

The strengths of the paper include

- (1) fair number of patients, and
- (2) delayed information on baseline, surgical procedures and early outcome variables.

However, these strengths are hindered by several issues that are described below:

Major

(1) Most importantly, it is unclear why the authors sought to compare the outcomes between type I and II aortic dissection. The background of the study should vividly appear in the Introduction why this is important and how they came to aim this research, however, the first paragraph is mainly filled with too basic knowledge (definitions of types of aortic dissection, fatality of the disease). In addition, a sentence of “The ascending aortic dissections occur most commonly in patients in the 50- to 60-years age range, whereas descending aortic dissections occur frequently in older individuals” seems completely irrelevant with the study issue.

[We thank the reviewer for this comment. We edited the introduction in the revised manuscript on page 4.](#)

(2) Meanwhile, the majority of the Discussion is occupied by detailing the results of prior studies, but without integrating these findings into notions relevant to core study aims and messages.

We thank the reviewer for this valuable comment. We edited the discussion in the revised manuscript according to your comment on pages 11 and 12.

(3) In the comparisons of the 2 types of dissection, I would recommend performing any form of adjustments either by multivariable modeling or propensity score analysis to test the independent impact of the types of aortic dissection on outcomes, which I do not find from the present manuscript.

We thank the reviewer for this important advice, which helps us a lot to improve our manuscript. We have added a logistic regression analysis according to your recommendation and included the results into all sections of the manuscript on page 9.

(4) The paper present longitudinal data (survival). For this the quality of follow-up such as its completeness and duration (median with ranges) should be presented.

We thank the reviewer for this comment. We have added the follow up completeness and duration in the methods section on page 6.

Minor:

(5) The primary endpoints are defined as 30-day mortality and postoperative neurological events, so they should be presented and emphasized in the abstract and in main document.

We thank the reviewer for this important comment and carefully revised the abstract and main document and included the data of 30-day mortality and neurological events on pages 2 and 9.

(6) In the Abstract-Results, “Short- and long-term survival was satisfactory in both groups”, but ‘the term ‘satisfactory’ is subjective and is not adequate for Results section.

We thank the reviewer for this comment. We removed this expression and edited the results section on pages 2 and 9.

Reviewer #B:

This paper compares the outcomes of patients with DeBakey type I vs type II aortic dissection treated at a single center in Germany over the course of 18 years. They found that long-term mortality was similar despite more complicated operations for type I patients. My primary question is how you plan to use this data in practice.

1) Page 2, lines 35-38: Please state your primary outcome and hypothesis.

We thank the reviewer for helping us to improve our manuscript. We mentioned our primary outcome and hypothesis in the abstract on page 2.

2) Page 2, lines 40 & 46: The order of the percentages does not seem to correlate with the text. Please keep the same order for all comparisons.

We thank the reviewer for this comment. The order of percentages in the original version of the manuscript was correct. We have added the corresponding absolute values to the percentages in the revised manuscript in order to clearly present the differences between both groups for the reader on page 2.

3) Page 4, line 95: please spell out the acronym MHCA.

The MHCA was spelled out at the first mention on page 5 as moderate hypothermic circulatory arrest in the original version in the section “patients and study design”.

4) Page 5, line 123: it seems there was overall higher number of patients with coronary heart disease in group 1 but a much lower percentage. You should report this difference as actually fewer patients in group 1 had CAD.

We thank the reviewer for this helpful advice. We edited the results section according to this comment on page 8.

5) “DeBakey” is misspelled throughout the text.

We thank the reviewer for this comment and apologize for this error. We corrected the spelling of DeBakey in the entire text.

Reviewer #C:

This is a retrospective single-center study about the comparison of surgical outcomes of DeBakey type I and II aortic dissection. They collected 395 consecutive patients underwent surgical aortic repair. In this study, they concluded that there were no differences between the long-term survivals in both groups.

I have few comments and questions.

1. Is nasopharyngeal temperature 18-22 °C moderate hypothermic circulatory arrest?

Yes, the mentioned nasopharyngeal temperature of 18-22 °C on page 6 is a moderate hypothermic circulatory arrest.

2. Since 2010, the cannulation of the left ventricle transatrial via the right upper pulmonary was established as the standard.

Why did you change standard cannulation strategy and what was advantage of this procedure? Did the outcomes improve?

Since 2010, we performed this technique as a standard procedure due to various advantages. Through this technique, we avoid cannulating the dissected aorta, as well as we avoid an extra incision to cannulate the subclavian artery or the femoral artery to reach the degree of temperature used to carry out the MHCA. Moreover, the blood flow remains antegrade when compared to the femoral artery cannulation (1).

3. During 17 years, how many independent surgeons involved? If there were 2 or more surgeons, were there any differences in outcomes according to surgeons?

We thank the reviewer for this important question. Yes, there were more than 2 surgeons involved. However, only senior surgeons with a longstanding surgical experience carry out those type of surgeries. Moreover, all surgeons followed the standard technique of our centre. We have added this explanation in the methods section “Patients and study design”, on page 5.

4. Primary endpoints in this study were 30-day mortality and postoperative neurological events. However, there were no mention about 30-day mortality in

manuscript nor table. And perioperative malperfusion or embolization were important cause of postoperative neurological events, but there was little mention of them.

We thank the reviewer for this valuable comment. We have added the results for 30-day mortality and postoperative neurological events in the results section on page 9.

5. There must have been many emergency surgeries due to the nature of the acute aortic dissection. How did you measure LV ejection fraction and pulmonary artery pressure? Conventional TTE or intraOp TEE? There were significant more patients with pulmonary hypertension in group 2, what effect do you think it had on outcome? And what was the cause of pulmonary hypertension?

We thank the reviewer for these important questions. All patients admitted to the hospital with AAAD were transferred directly to the operation theatre after confirming the diagnosis through computer tomography. An intraoperative TEE was performed in all patients as a standard technique to estimate the valve morphology as well pulmonary hypertension through measuring the maximal tricuspid regurgitation velocity with CW Doppler. The pulmonary hypertension is often detected prior to admission in a previous routine physical exam and reported in the patients' medical history, which could be collected after the emergency surgery.

Pulmonary hypertension showed no association to 30-day mortality ($p=1.000$, Fisher-test), therefore we did not include it into the multivariable analyses. We have added the description of the selection of variables in the methods section on page 9.

6. In table 1, how was AMI diagnosed? And in these cases, did PCI perform before the aortic surgery?

We thank the reviewer for this question. Acute myocardial infarction (AMI) was detected through ECG changes and specific laboratory parameters of heart infarction. We have added this explanation to the methods section on page 5. PCI was performed before the aortic surgery within the framework of patient anamneses, but not in the preoperative setting.

7. The number of pericardial tamponade was 64, but the number of cardiogenic shock was much less. Does tamponade show features of cardiogenic shock?

A larger number of patients have pericardial tamponade but still have a hemodynamically stable status. Therefore, tamponade does not necessarily show features of cardiogenic shock.

8. What does TEVAR(EVAR) mean in surgical procedure? I think it would be necessary to explain in which patient, where, which procedure was performed.

We thank the reviewer for this valuable question. Patients with a residual dissection or organ malperfusion were treated additionally interventionally using TEVAR (EVAR). We added this information into the material and methods section on page 5.

9. As a study on outcomes after aortic dissection surgery, shouldn't there be a reference to aortic death rather than cardiac death or cerebral death. What is cerebral death?

We thank the reviewer again for this interesting suggestion. Unfortunately, we did not collect information about the aortic death in our patients in our retrospective data, therefore we cannot supply these data.

10. Although it is a study that summarizes the results of 17 years of AD surgery with a relatively large sample size, it is difficult to follow overall, and the endpoints were not properly described.

We thank the reviewer for this valuable criticism. As we mentioned in the Material and method section of the original submitted manuscript on page 6, the primary endpoints were 30-day mortality and postoperative neurological events. Secondary endpoints were pre- and intraoperative variables, as well as the postoperative courses such as blood loss and transfusion of blood products. We have now defined the endpoints more clearly in the text and hope that these will now be more understandable for the reader