Peer Review File

Review Comments-Reviewer A

1) First, the title needs to indicate the RACS vs. TOHS and the clinical research design of this study, i.e., a retrospective cohort study.

Reply: Thanks for your suggestion. According to your comments, We have changed the title to"*Efficacy and safety of robotic cardiac surgery: a retrospective cohort study- RACS vs. TOHS"*.

Changes in the text: We have modified our text as advised (see Page 1, line 3).

2) Second, the abstract needs some revisions. The objective did not indicate the knowledge gaps on the efficacy and safety of RACS and the clinical significance of this research focus. The methods did not describe the inclusion of subjects of the two groups, assessment of baseline clinical factors, and efficacy and safety outcome measurements. The results need to first report the clinical characteristics of the two groups and their baseline comparability. Please use detailed figures and accurate P values to quantify the results. Since this is not a RCT, the authors need to tone down the current conclusion and have more detailed comments for the clinical implications of the findings.

Reply: Thanks for your suggestion. According to your comments, we have changed the abstract. In our center, robotic-assisted surgery is all used for adult selective surgery. Specifically, the inclusion criterion of RACS group include: 1) age > 18 years; 2) selective surgery; 3) non-aortic surgery; 4) no serious pleural adhesion; 5) good lung function which can tolerate one-lung ventilation; 6) suitable femoral artery diameter. In relation to TOHS group, patients in the same period were included by reviewing the hospital electronic medical record information system.

Changes in the text: We have modified our text as advised (see Page 1, line 23-27 and Page 4, line 91-95).

3) Third, the introduction is not adequate and not informative. The authors did not review what has been known on the efficacy and safety of RACS, analyze the limitations and knowledge gaps of prior studies, analyze the limitations of TOHS, and explain the clinical needs for comparing the two treatments. Please also clarify the clinical significance of this study.

Reply: Thanks for your suggestion. According to your comments, we have changed the introduction.

Changes in the text: We have modified our text as advised (see Page 3, line 58-78).

4) Fourth, in the methodology of the main text, the authors need to describe the inclusion of subjects, how the two groups were selected, clinical research design, sample size estimation, and assessment of baseline clinical factors. Importantly, please describe how one of the two treatments was selected for an individual patient in the authors' real-world clinical practice, which would indicate the systemic bias between the two groups. In statistics, please describe the test of baseline comparability of the two groups and how to analyze the independent effects of RACS with multiple adjustment analysis.

Question 1: In this study, the inclusion criterion of the both groups.

Response: In our center, robotic-assisted surgery is all used for adult selective surgery. Specifically, the inclusion criterion of RACS group include: 1) age > 18 years; 2) selective surgery; 3) non-aortic surgery; 4) no serious pleural adhesion; 5) good lung function which can tolerate one-lung ventilation; 6) suitable femoral artery diameter. In relation to TOHS group, patients in the same period were included by reviewing the hospital electronic medical record information system.
Changes in the text: We have modified our text as advised (see Page 4, line 91-95).

Question 2: What is the design type of this study.

Response: This study is a clinical retrospective cohort study.
Changes in the text: We have modified our text as advised (see Page 3, line 84).

Question 3: How to estimate sample size of this study.

Response: Due to the design of this study is retrospective; thus, we did not estimate the sample size.

Question 4: The assessment of baseline clinical factors.

Response: The selection of baseline clinical factors was on the basis of previous published articles and our personal experience.

Question 5: RACS versus TOHS, which should be selected for an individual patient?

Response: In recent decade, the concept of minimally invasive surgery has gradually taken root. Similarly, if a patient was definitely diagnosed and need a cardiac surgery in our center. We will firstly evaluate whether this patient is suitable for performing RACS, if yes, we will then communicate as well as inform him the advantages and shortcomings of RACS, and the choice is made by the patient. Of course, RACS is our first recommendation.

Question 6: The test of baseline comparability of the two groups and how to analyze the independent effects of RACS with multiple adjustment analysis.

Response: We have corrected our paper and revised the statistics. The comparability of baseline data between groups is extremely important for a rigorous paper. But unfortunately, although propensity score matching method may be a good choice, it is hard for us to do this because limited sample size and different disease types. According to our knowledge, IPTW method is another alternative as it can do post-randomization; however, because of the virtual sample size, its result may cause serious bias. As for the revised manuscript, we directly compare the clinical results between the two groups, and reasons are as follows. First, for eligible patients, the advantages of RACS are self-evident compared with TOHS, furthermore, a large number of published articles have also proved these. Second, there is no significant difference when comparing all baseline variables that we collected, this may be a natural randomization of the real world, and indicated that our results are credible. In relation to Cox analysis, the selection of variables has described in the main text. Briefly, we select by referring to the previous literatures and clinical practice.

Changes in the text: we have modified our text as advised (see Page 6, line 177-194).

5) Finally, please cite one important related paper: Chitwood WR Jr. Historical evolution of robot-assisted cardiac surgery: a 25-year journey. Ann Cardiothorac Surg 2022;11(6):564-582. doi: 10.21037/acs-2022-rmvs-26.

Reply: we have modified our text as advised (see Page 3, line 69).

Review Comments-Reviewer B

Thank you for the opportunity to review this manuscript reporting on a series of robotic cases and comparing to some standard cardiac cases at a single institution. My comments follow:
There are some grammatical errors and I would suggest a native English speaker reviewing the paper.

Methods
1. You have described briefly several procedures and how they are performed robotically. Is this really necessary? Do you need to describe this in the paper - perhaps this would be better suited in supplementary information, or just leave references as presumably they are described techniques? In any case they are presented in only limited detail anyhow. Additionally, there are numerous figures which similarly don't really need to be included in this manuscript.

Reply: Thanks for your advice, we have placed the descriptions of robotic procedures into the supplementary files and deleted some un-necessary figures. (page 4, line 98-100)

2. You have not described in your methods about where the control groups came from? What was the selection process to decide who has robotic surgery? surely there is bias here in that easier cases would have been selected for robotic.

Reply: We have described where the control groups came from in the revised manuscript. In brief, patients with the same disease types who underwent median sternotomy and had complete data in the same period were selected as the TOHS group, and the selection of patients was by reviewing the hospital electronic medical record information system. (page 4, line 85-88)

3. Does this period cover the learning curve? Was there a proctor? Is it select surgeons performing robotic; what was their training path etc.?

Reply: In our department, after training, robotic surgery began in 2015, and all operations were performed by the same person.

Changes in the text: None.

4. Why did you not match patients formally, e.g. propensity matching to make the analysis fairer?

Reply: On this issue, we have described in the discussion section of the revised manuscript. Now, we quote it in here. The main limitation of this study is the comparability of baseline data between both groups. Comparability is extremely important for a rigorous paper, but unfortunately, although propensity score matching method may be a good choice, it is hard for us to do this because limited sample size and different disease types. According to our knowledge, IPTW method is another alternative as it can do post-randomization; however, because of the virtual sample size, its result may cause serious bias. In this study, we directly compared the clinical results between the two groups, and reasons are as follows. First, for eligible patients, the advantages of RACS are self-evident compared with TOHS, furthermore, a large number of published articles have also proved these. Second, there is no significant difference when comparing all baseline variables that we collected, this may be a natural randomization of the real world, and indicated that our results are credible. (page 10, line 289-300)

Results

1. You are sometimes comparing outcomes for individual procedures with small numbers of patients. I feel that this is a little artificial since we have no information on the decision making and selection for robotic vs open and so I am unsure it is a fair comparison

Reply: As we described in the manuscript, RACS is the first choice in our department as long as the patient's condition is suitable. However, RACS is just the first choice, not just the only

one. After adequate communication, the final procedure plan was determined by the patient.
Changes in the text: None.

2. Line 251 - you say shorter time to return to normal activities - how is this defined? you have not described on what basis this is determined in the methods?

Reply: There is no quantitative evaluation method for this indicator, mainly based on patients' subjective feelings for self-reporting.
Changes in the text: None.

Discussion

1. Line 341 - you say longer perfusion (bypass) time does not lead to increased morbidity - are you sure that this is not correlated?

Reply: Thanks for your comment, we have corrected it.

2. Line 344 - you say that increased stroke can be mitigated by preoperative screening etc. - what do you do at your center to screen?

Reply: Routine head CT scan and carotid artery ultrasonography.
Changes in the text: None.

3. You have not included any limitations? This is an important section of any paper

Reply: We have described in the revised manuscript. (page 10, line 289-300)

4. There are a large number of tables and I am not sure if these are all required.

Reply: We have deleted some unnecessary tables. (page 15, line 410)

5. Overall, you have described use of robot for a range of techniques but I am not convinced by your data as you have not really explained the origin of the control data. Is the postoperative ICU stay etc. shorted simply because they are simpler cases and the more complex cases went for open surgery? I would say that you need to match the groups to make this analysis valid.

Reply: The main limitation of this study is the comparability of baseline data between both groups. Comparability is extremely important for a rigorous paper, but unfortunately, although propensity score matching method may be a good choice, it is hard for us to do this because limited sample size and different disease types. According to our knowledge, IPTW method is another alternative as it can do post-randomization; however, because of the virtual sample size, its result may cause serious bias. In this study, we directly compared the clinical results between the two groups, and reasons are as follows. First, for eligible patients, the advantages of RACS are self-evident compared with TOHS, furthermore, a large number of published articles have also proved these. Second, there is no significant difference when comparing all baseline

variables that we collected, this may be a natural randomization of the real world, and indicated that our results are credible.

Changes in the text: page 10, line 289-300.

Liu et al present a single center robotic experience and compare against a traditional open heart surgery cohort.

This publication is interesting since the array of procedures and techniques are listed out and include some not often performed robotic, such as VSD repair and HOCM repair. The technique specifics are explained well. Although the statistics and comparison group are likely confounded by numerous other variables, they at least show a degree of safety which is required for implementing a new platform. A few minor corrections I would request:

1. Line 94 includes technique of robotic ASD, but in the results PAPVC is also shown with robotic n=3. Please add a section to the techniques with your robotic PAPVC technique.

   Reply: Thank you greatly for your suggestion about add a section about PAPVC technique. In accordance with your suggestion we have revised the manuscript and added a description about it in the supplementary file.

   Changes in the text: Supplementary file.

2. Several techniques described include ascending aortic occlusion, please clarify if this is performed with endoclamp or a cardioplegia system and clamp via robotic or thoracoscopic method.

   Reply: As to your suggestion 2, all the ascending aortic occlusions were performed with endo-clamp via robotic and we also added it to the supplementary file.

   Changes in the text: Supplementary file.

3. Table 5 should clarify it is unadjusted raw data.

   Reply: According to your suggestion 3, the data shown in the table 5 is unadjusted raw data and we have added a clarification under the table.

   Changes in the text: page 18, line 425.

4. Was there significant selection bias? If the controls were all timed historic than unlikely, but if the robotic patients were chosen as healthier than the results could be susceptible to selection bias. Please address in a limitations section of your discussion.

   Reply: According to your suggestion 4, we added the content about the limitations of this study in discussion part in the revised manuscript.

   Changes in the text: page 10, line 289-300.

Thanks for the reviewer's good advice, which will make our work more rigorous and the results more reliable.

1. The authors compare 255 RACS patients with 736 patients who were operated with standard median sternotomy. Did they use propensity score matching to match robotic-assisted to nonrobotic cases?

Reply: On this issue, we have described in the discussion section of the revised manuscript. Now, we quote it in here. The main limitation of this study is the comparability of baseline data between both groups. Comparability is extremely important for a rigorous paper, but unfortunately, although propensity score matching method may be a good choice, it is hard for us to do this because limited sample size and different disease types. According to our knowledge, IPTW method is another alternative as it can do post-randomization; however, because of the virtual sample size, its result may cause serious bias. In this study, we directly compared the clinical results between the two groups, and reasons are as follows. First, for eligible patients, the advantages of RACS are self-evident compared with TOHS, furthermore, a large number of published articles have also proved these. Second, there is no significant difference when comparing all baseline variables that we collected, this may be a natural randomization of the real world, and indicated that our results are credible.
Changes in the text: page 10, line 289-300.

2. I believe EUROSCORE is an important tool to predict mortality after conventional cardiac surgery. Reporting EUROSCORE would give a better understanding in the comparison between patient demographics.
Reply: Sorry, EUROSCORE was not routinely assessed before surgery in our department.
Changes in the text: None.

3. Please explain further the term "rate of patients who left hospital without cure".
Reply: Firstly, thanks a lot for your precious recommendation. About this confusing expression "rate of patients who left hospital without cure", it means some patients withdrew the treatment for many reasons such as family chooses. We have revised this expression to "withdrew treatments".
Changes in the text: page 6, line 156.

4. Please elaborate on the conversion rates to open surgery (median sternotomy or larger thoracic incisions) according to the learning curves. i.e. how many patients, if any, were converted in the beginning of the RACS implementation vs in the latest stages.

Reply: Since we conducted RACS in 2015, so far, there has been no conversion to median sternotomy or larger thoracic incisions, which may be related to the relatively simple surgery (ASD repair) that we chose in the early stage.

Changes in the text: None.

5. The author mention that postoperative complications between the RACS group and the TOHS group were not significantly different. Could they further explain what sort of complications they encountered? Was there only 1 vascular complication (abdominal aorta)? Any associated lung injuries?

Reply: In our department, patients who underwent RACS were always associated younger age and better body status compared with TOHS, this mainly attributed that they wanted to recover quickly and have a small incision. To be honest, we encountered only two serious complications up to now, one was the abdominal aortic rupture and another one was the liver bleeding caused by the mechanical arm. As for lung injuries, patients may experience delayed detubation (time > 48h), but all of them survived after appropriate treatment.

Changes in the text: None.

6. Finally, it would significantly add to the manuscript the number of grafts on CABG patients. Multivessel operations have significantly higher mortality and cardiovascular complications than LIMA-LAD for example, regardless of whether a robotic or conventional approach is utilized.

Reply: Of course, it is undoubted that multivessel bypass have a higher mortality rate and complications no matter whether a robotic or conventional approach is utilized. In our department, if patient with robotic assisted CABG surgery, only 1 bypass constructed during operation, which was LIMA to LAD.

Changes in the text: None.

7. Please state for the record which Da Vinci robotic system you currently use i.e. Si, Xi.

Reply: The Da Vinci robotic system we used is from Intuitive Surgical, Inc, Sunnyvale, CA.

Changes in the text: page 4, line 98-100.

I enjoyed reading this manuscript and congratulate the authors for their results using robotic cardiac surgery. I do have some comments.

1. Please refer to the robotic console as: Da Vinci® (Intuitive, Sunnyvale, California).

   Reply: Thank you for you remind, The Da Vinci robotic system we used is from Intuitive

Surgical, Inc, Sunnyvale, CA and we have mentioned it in the revised manuscript.

Changes in the text: page 4, line 98-100.

2. The authors present their results over 5 years of RACS in their program, this article is well written and describe results accurately. However, I would like to see more information, for instance; how many surgeons performed these procedures?

Reply: Thank you for your suggestion, we now have only one surgeon perform these procedures.

Changes in the text: none.

3. What was their learning curve before including these patients? Did they have formal training or previous experience?

Reply: Firstly, thank you for your questions. The surgeon who performs robotic surgery and the entire robotic surgery team of our institution are all trained in Hong Kong and have gained certification.

Changes in the text: none.

4. Were these the same surgeons performing the open surgeries?

Reply: Yes.

Changes in the text: none.

5. Additionally, in the discussion the authors talk about the benefits of RACS, this is not necessary, the advantages of robotic cardiac surgery are well-detailed in the literature, I would rather see information on other programs in China; how many RACS programs, when did they start?

Reply: Thanks a lot for you recommends. We have revised the discussion part.

Changes in the text: page 10, line 289-300.

6. Also, the everlasting topic of costs need to be discussed, how much does a RACS procedure cost in comparison to open surgery? This information is crucial to the topic in general.

Reply: The mean cost of RACS procedure is about 30000 CNY.

Changes in the text: none.

Responses to Reviewer F

In this study entitled "Efficacy and safety of robotic cardiac surgery: a single-center clinical study", the authors evaluated the efficacy and safety of the Da Vinci robotic surgical system in

cardiac surgery by comparing it with traditional open-heart surgery.

The study found that the Da Vinci robotic surgery system was safe and effective, with no significant differences in reoperation rate, patients who left hospital without cure, and mortality rate between both groups. Additionally, the postoperative ICU stay and 24-hour drainage volume in the RACS group were lower, and the time required to recover to normal daily activities of patients in the RACS group was shorter than that in the TOHS group. Altogether, this is an interesting study with sound clinical suggestions that could be considered and good clinical impact.

Please view my comments below that could be considered to potentially improve this work:

1. Abstract:

a. Objectives: Please provide a brief introduction in the abstract objectives.

Reply: Thanks, we have made some changes in the abstract's part.

Changes in the text: page1, line22-25.

b. Methods: Usually, the number of patients and age should be mentioned in the Results section. Also, the Methods section should be summarized as there are too much redundant information for an Abstract.

Reply: Thanks, we have made some changes according to your suggestions.

Changes in the text: page1, line 26-37.

c. Results: Please provide more data with corresponding P values to really show the significances of the comparisons performed.

Reply: Thanks, we have added corresponding P values in the table.

Changes in the text: page2, line 38-48.

d. Conclusions: should be brief and straightforward.

Reply: Thanks, we have made some changes to make the conclusions brief and straightforward.

Changes in the text: page2, line 49-50.

2. Introduction: The introduction provides a good overview of the topic, but it can be improved by:

a. adding more specific information on the aims and objectives of the study.

Reply: This has been mentioned in the introduction.

Changes in the text: None.

b. highlighting the research gap that the study aims to address.

Reply: We aimed to report the experience of robotic-assisted cardiac surgery (RACS)

using the Da Vinci robotic surgical system, meanwhile its efficacy and safety was also evaluated by comparing with traditional open-heart surgery (TOHS), thus to provide evidence for a broader applications of RACS in clinical practice.

Changes in the text: None.

c. providing a more precise definition of key terms and concepts related to robotic cardiac surgery, which could help readers better understand the context and significance of the study.

Reply: This has been mentioned in the introduction.

Changes in the text: None.

3. Methods:

a. Provide a more detailed description of the patient selection criteria for each surgery type. Were patients with similar characteristics and disease severity included in each group? How were they matched?

Reply: By searching the hospital's electronic medical record information system, 736 patients with the same disease types who underwent median sternotomy and had complete data in the same period were selected as the TOHS group.

Changes in the text: page 4, line 86-88.

b. Clarify the steps taken to ensure the quality and safety of the robotic-assisted surgeries. Were the surgeries performed by experienced surgeons? Did the surgeons receive training on the use of the Da Vinci robotic surgery system? How were any complications or adverse events during or after the surgeries handled?

Reply: In our department, after training, robotic surgery began in 2015, and all operations were performed by the same person. Furthermore, the surgeon who performs robotic surgery and the entire robotic surgery team of our institution are all trained in Hong Kong and have gained certification.

Changes in the text: None.

c. Provide more information on the outcomes measured and the statistical analyses conducted. What were the primary and secondary outcomes measured in this study?

Reply: Thanks, we have described in the manuscript.

Changes in the text: None.

4. Results and Discussion:

a. Please consider providing more detailed information about the study population, such as their demographic characteristics, comorbidities, and other relevant clinical data. This would help readers better understand the results and their generalizability to other patient populations.

Reply: Thanks, we have described in the revised manuscript.

b. Kindly include a discussion of the limitations of the study, particularly regarding the small sample sizes for certain disease types in the RACS group. Additionally, the authors should acknowledge the potential for selection bias, as patients were not randomized to treatment groups.

Reply: Thanks, we have described in the revised manuscript.

c. The Discussion seems to lengthy, should be shortened and revised based on the context of the Results obtained. Please avoid out-of-context descriptions as this is not a Review.

Reply: Thanks, we have described in the revised manuscript.

5. Figures and Tables:

a. Table 1: consider providing the P values.

Reply: Thanks, we have added the P values.

Changes in the text: Table 1.

b. Figures: I would suggest merging the Figure 1-8 into 2 groups rather than separating into 8 different figures. E.g.: Start till middle of surgery (e.g.: merging Figure 1-4 to Figure 1A-D), then most important section of the surgery till end (e.g.: merging Figure 5-8 to Figure 2A-D) or simply merger all to 1 figure (e.g.: Figure 1A-G).

Reply: Thanks, we have deleted some figures according to your suggestions.

Response to Reviewer G:

1. **Table 1 and Table 2**

Please indicate the unit of "BMI".

Index	RACS gro (N=255)
Age (year)	52.2±6.1
Gender (female/male)	134/121
BMI	20.8±2.4
LVEF	62.3±2.1

Table 1 Comparison of baseline dat

Reply: Thanks, we have added the explanation of BMI under the table.

2. Table 2

Please add the description to the table footnote that how the data are presented in table.

Reply: We have modified this part.

3. Table 3

    a) Please explain all the abbreviations in the table footnote.

    b) Please add the description to the table footnote that how the data are presented in table.

    c) Please explain the meaning of the * in the table footnote.

Reply: We have revised, and added all the abbreviation meanings in the table footnote.

4. Table 4

    Please add the description to the table footnote that how the data are presented in table.

Reply: We have added all the abbreviation meanings in the table footnote.

5. Table 5

    Please add the description to the table footnote that how the data are presented in table.

Reply: We have modified this part.

6. Table 6：

    a) Please explain all the abbreviations in the table footnote.

    b) Please add the description to the table footnote that how the data are presented in table.

    c) Please explain the meaning of the * and # in the table footnote.

Reply:

    a) We have added the abbreviation meanings in the table footnote;

    b) We have modified this part;

    c) We have added all the abbreviation meanings in the table footnote.

7. Table 7

Please add the description to the table footnote that how the data are presented in table.

Reply: We have modified this part.

8. Please double-check if more studies should be cited as you mentioned "studies".

Reply: Thank you for your reminder! This part does should be "study" rather than "studies" and we have corrected it in the revised manuscript.