

Peer review file

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Reviewer A

This is a review of minimally invasive approach for some surgical procedures in congenital heart disease. The first author has published this work as individual procedures in the past, so is nice to see a summary of all the patients in one document.

Comment 1: There are major document changes needed. The methods reflect a lot of information that has to go in the introduction or the discussion (i.e. the first couple of paragraphs in methods).

Reply 1: We have moved the first paragraph of the Methods into our Introduction.

Changes in the text: On pages 5 and 6, lines 115-132, we moved up a paragraph from the Methods section. This section of the Introduction now includes, “All of our approaches started with the patient. During initial consultations and postoperative visits, patients often expressed concern about having surgery. For patients with a history of prior cardiac surgery, the concerns were centered around the recovery process, healing, and potential complications. Conversations with patients who had never had cardiac surgery often focused on the size of the incision, what to expect postoperatively, and the recovery process. After discussing these concerns with our adult thoracic and cardiac colleagues, we agreed to attempt a series of minimally invasive adult techniques for pulmonary insufficiency, anomalous aortic origin of coronary artery, atrial septal defects, partial anomalous pulmonary venous return, and a number of other congenital cardiac defects.”

Comment 2: Needs to actually describe methods: retrospective/prospective, database review? chart review?, etc.

Reply 2: In our abstract (page 3, line 78) and our methods section (page 8, lines 204-207) we describe the study as a retrospective chart review and cite the associated IRB number. We believe this accurately describes our study design.

Changes in the text: No changes were made.

Comment 3: The discussion should be labeled as such and not as "Comment."

Reply 3: Wording of the final section has been changed from "Comment" to "Discussion."

Changes in the text: On page 11, line 285, the section heading "Comment" was changed to "Discussion."

Comment 4: The discussion should be expanded to understand the clinical relevance of this approach and more the challenges than only on the learning curve.

Reply 4: The clinical relevance of minimally invasive approaches are referenced on page 5 line 100. The literature highlights that minimally invasive approaches may decrease intraoperative transfusion requirement, postoperative ICU and total hospital length of stay, postoperative pain, and postoperative surgical site infection rates, in addition to the cosmetic and psychological benefits of smaller incisions. The goal of this paper was to evaluate outcomes of the approach, not necessarily the individual procedures. We didn't think the small sample size and case heterogeneity would provide a fair assessment on the individual procedure basis. The challenges represent the learning curve. Throughout our results section, we highlight and describe our complications. On pages 12 and 13, lines 324-342, we discuss our impression with each approach for the major operations. We have also added a limitations section, highlighting the fact that surgeon familiarity and comfort with operating in limited fields may also bias our results and influence the likelihood of success for those attempting similar approaches based on our experience.

Changes in the text: On page 14, lines 362-371, "A single institution, retrospective review with low sample size and high operative heterogeneity carries a number of limitations. First and foremost, patient selection was key, particularly early on for each individual approach. The study was retrospective and did not randomize patients to an approach. The small sample size also limited our ability to make meaningful conclusions at the individual procedure level. While previous reports have described the technical challenges with each minimally invasive operation, we focused on the different exposures and our results may not be generalizable. Surgeons at our institution were very comfortable operating through a limited field, which may also bias our results and the likelihood of success for those attempting similar approaches based on our experience."

Reviewer B

In this manuscript, Drs. Nellis & Turek describe their institutional experience using minimally invasive techniques for congenital cardiothoracic surgery.

The manuscript is well written and clearly presented. Most importantly, I believe the topic to be very timely. The use of minimally invasive techniques in adult cardiac surgery has been perfected over the last decade and it makes sense that this technology be transmitted to congenital surgery- especially as we begin to treat more adult patients with congenital disease. The authors make many salient points about transparency among providers and with patients. They appropriately point out the need for multi-institutional collaboration to ensure that outcomes remain patient-centered.

Comment 1: There are obvious limitations to this type of study and all data must be taken within the context. These limitations (size, retrospective outcomes review, heterogeneity of patient population, selection bias) should be better outlined in the discussion.

Reply 1: We agree. To better capture the points you've mentioned, we've added a paragraph to the discussion.

Changes in the text: On page 14, lines 362-371, we added the following paragraph, "A single institution, retrospective review with low sample size and high operative heterogeneity carries a number of limitations. First and foremost, patient selection was key, particularly early on for each individual approach. The study was retrospective and did not randomize patients to an approach. The small sample size also limited our ability to make meaningful conclusions at the individual procedure level. While previous reports have described the technical challenges with each minimally invasive operation, we focused on the different exposures and our results may not be generalizable. Surgeons at our institution were very comfortable operating through a limited field, which may also bias our results and the likelihood of success for those attempting similar approaches based on our experience."

Comment 2: Another suggestion would be the use of some sort of image to show the different types of incisions and techniques used to really give readers an appreciation

for the difference this can make.

Reply 2: A figure was added to demonstrate the different locations utilized to perform minimally invasive repairs in our series.

Changes in the text: Figure 1 was switched to Figure 2. A new Figure 1 was introduced on page 9, line 226. The figure legend is located on page 19 and reads, "Figure 1. Minimally invasive approach by procedure. Graphical representation of various incisions including pulmonary valve replacement (PVR), pulmonary artery (PA) banding, PA translocation, anomalous aortic origin of a coronary artery (AAOCA), atrial septal defect (ASD), partial anomalous pulmonary venous return (PAPVR), and Scimitar syndrome."

Comment 3: As a relatively young surgeon I trained extensively in minimally invasive techniques- both in general and adult cardiothoracic surgery. Since transitioning to a congenital practice, I have been shocked by the resistance to using minimally invasive techniques on many of our common pathologies- especially in teens and adults. In order to push the field further and potentially limit the morbidity of the operations we perform with minimally invasive techniques, it is imperative that we study these techniques in our patients and then share that information so as to increase confidence and limit adverse outcomes.

Reply 3: We agree.

Changes in the text: No changes were made.

Reviewer C

The authors of this single-center retrospective review, report on their experience in minimally invasive pediatric cardiac surgery. They include 49 patients who were submitted to minimally invasive cardiac procedures over a period of 3 years.

The authors are to be commended for their program and their outcomes despite the quoted 14 % conversion rate. Overall, I enjoyed reading this paper through. It is well written and informative, with an in-depth analysis on the risks and pitfalls that can be encountered should any surgeon engage in the referenced minimally invasive procedures.

I agree with most of the points made in this report. It is true that a true learning curve exists especially in pediatric cases. But minimally invasive surgery in the pediatric population should advance not only for the cosmetic benefits per se, but also for the psychological impact that sternotomy incisions may induce...

I just have a few minor comments for the authors.

Comment 1: What sort of cardioplegia do they use for their cases (Custodiol/ Del-nido)? This needs to be involved.

Reply 1: We included a reference to our use of del Nido solution on pages 8 and 9.

Changes in the text: On pages 7-8, lines 197-200, we added, "Fibrillation or cardiac arrest using del Nido cardioplegia solution were utilized, if repairs could not be accomplished with a beating heart."

Comment 2: Do they routinely establish CPB through peripheral cannulation? I understand this in case of ASDs but is this the standard procedure even in a fibroelastoma resection?

Reply 2: Yes with the exception of AAOCA repairs which we are no longer performing central cannulation. On page 14, lines 350-353, we previously described how, "When peripheral cannulation is used, we always establish our arterial connection using a 6mm Dacron graft in an end-to-side fashion. This is a technique that is critical for smaller pediatric patients, but one we've carried over for all of our cases, including adults." We believe this appropriately addresses your concern.

Changes in the text: No changes were made.

Comment 3: Finally, I would appreciate a few comments on a comparison between the minimally invasive approaches and the conventional ones according to their experience. In other words have they seen any benefit in terms of LOS, pain, transfusion requirements etc.. in the minimally invasive group? This can not necessarily derive from a direct comparison with a patient population submitted to the conventional procedures (even with unmatched patients).

Reply 3: To the Reviewer's point, the sample size is too low and heterogeneous to

make substantiated claims, therefore as we mention on page 15 lines 377-379, “performance should be evaluated on a case-by-case basis...[and we should] share our collective experience in a public forum, to get honest feedback.” We also included a limitations paragraph within the Discussion. In a separate publication, currently under review, we have matched patients undergoing minimally invasive pulmonary valve replacement through a left anterior mini-incision to those receiving median sternotomy. We did not appreciate a difference in operative times, ICU LOS, total LOS, transfusion requirement, postoperative pain, or surgical site infections. With time though, we are confident that we will be able to appreciate a shorter hospital LOS as members of the care team become more familiar with the approach.

Changes in the text: In the Discussion on page 14, lines 362-371, we describe the limitations of the study, “A single institution, retrospective review with low sample size and high operative heterogeneity carries a number of limitations. First and foremost, patient selection was key, particularly early on for each individual approach. The study was retrospective and did not randomize patients to an approach. The small sample size also limited our ability to make meaningful conclusions at the individual procedure level. While previous reports have described the technical challenges with each minimally invasive operation, we focused on the different exposures and our results may not be generalizable. Surgeons at our institution were very comfortable operating through a limited field, which may also bias our results and the likelihood of success for those attempting similar approaches based on our experience.”

Comment 4: Overall, an interesting article, worth reading and publishable material. Congratulations to the authors.

Reply 4: Thank you for your comment.

Changes in the text: No changes were made.

Reviewer D

Comment: Nellis and colleagues should be commended for their efforts to popularize minimally invasive approaches in congenital cardiac surgery. The introduction is useful and interesting. The methods are very clear, and the study was thoughtfully designed. The results are clearly important. This includes the information on the learning curve

and cardiopulmonary bypass time, the conversion rate to larger incisions, risk factors for conversion to a larger incision, which included an increased body mass index. This is an important study because minimally invasive approaches are used far less in congenital cardiac surgery than adult cardiac surgery. It adds important details to our understanding of the role of minimally invasive approaches. In conclusion, I would like to add that the commonest procedure was PVR, which is an operation that may be mostly replaced with an even more minimally invasive approach namely transcatheter Harmony valve replacement. Again, I commend the authors on an excellent and timely paper and recommend acceptance without delay.

Reply: None.