Author’s response to reviews

Title: Clinical Outcomes of a Combined Transcatheter and Minimally Invasive Atrial Septal Defect Repair Program using a 'Heart Team' Approach

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Author’s response to reviews:

AE: Please take the reviewers comments into consideration for the revision. I like the message of the paper since this or the other approach might be better for one or the other patient given different complications/shortcomings or strengths.

The authors would like to thank the Editor for kind consideration of this manuscript. We are highly encouraged that the emphasis on the ‘Heart Team’ approach was well-appreciated. Please find our detailed response to each Reviewer’s comments below. We have added changes to the manuscript as described using the ‘Track Changes’ feature.

Reviewer #1:

CONGRATULATIONS FOR THE MANUSCRIPT.
The authors would like to thank Reviewer 1 for the insightful comments and helpful suggestions in reviewing this manuscript.

THE CASES ARE NOT SIMILAR. THE AUTHORS SHOULD SPLIT THE GROUPS BETWEEN SECUNDUM ASD AND COMPARE THE RESULTS MAKING STATISTICAL ANALYSIS AND SEPARATE IN A 3RD GROUP OTHER TYPES OF ASD.

We initially chose to analyze all closed atrial septal defects (ASD) despite variability in cases because we sought to emphasize the results in the context of a minimally invasive ASD program.

We completely agree that there are important between-group differences since only certain cases will be amenable for transcatheter closure thus resulting in selection bias. Therefore, at the Reviewer’s insightful suggestion, we have added a subgroup analysis which includes the following 3 groups:

1) Secundum/PFO (percutaneous)
2) Secundum (surgical)
3) Complex (surgical)

This has been reported in Figure 4 using survival analysis with a Kaplan-Meier curve.

WAS THE PATIENT WHO HAD STROKE ON AF(COULD BE AN IMPORTANT ISSUE IN AVOIDING PERCUTANEOUS OVER SURGICAL APPROACH)?

We agree entirely with this astute comment. The patient was not in atrial fibrillation. Instead, the patient demonstrated a hypercoagulable state and was worked up further by a hematologist. This patient had thrombotic failure of an infrainguinal bypass as well as thrombosis of the ASD closure device. The increased risk of thrombosis was not fully appreciated at the time of ASD closure device placement.

COULD YOU MAKE A COST ANALYSIS? THIS COULD BE IMPORTANT TO CENTERS THAT HAVE FINANCIAL LIMITATION.
We agree completely that cost is an important consideration in the initiation or maintenance of a comprehensive structural heart disease program. Unfortunately, we lack the data required for thorough cost analysis. Qualitatively, the increased hospital and intensive care unit lengths of stay in the surgical group may suggest a higher upfront cost for the surgical approach and the impact of length of stay will naturally vary between healthcare systems. Additionally, the device costs of the percutaneous approach would have to be weighed in as well. We hope that this important consideration is analyzed in future research.

BEST REGARDS

Reviewer #2:

The article is interesting and represents a heart team approach to the treatment of ASDs. The introduction of heart teams is important and well suited for the purpose of utilizing various methods for ASD repairs. This has resulted in very good results in the experience of the authors.

The authors would like to thank Reviewer 2 for the insightful comments and helpful suggestions in reviewing this manuscript. We are encouraged that the emphasis on the ‘Heart Team’ approach was well-appreciated.

The paper is well written, the main concern is as the authors point out, the low number of patients included. I do think, however, that it is necessary to make a stronger statement in this regard. If we look at the incidence of residual shunt for example, where there is a much higher incidence in the transcatheter group, it certainly could be a good chance of a type 2 error or false negative if a larger number of patients were included. The same applies for several other factors when the study has so low power.

We completely agree with this astute observation and have amended the ‘Limitations’ section of the manuscript to further highlight this important point.

Questions to the authors
The inclusion of patients in the surgical group who had additional procedures, skews parameters such as bypass and crossclamp times. Did you do a comparison between the "straightforward" cases ie cases that had only ASD repair. Although the number of patients would be even lower, the additional patients included may make the comparison between groups less valid.

We agree that additional procedures add heterogeneity to the surgical group. Therefore, at Reviewer 2’s suggestion, we have included an additional subgroup analysis looking at secundum versus other defect type.

Notwithstanding the heterogeneity introduced by combining different defect types and isolated/combined procedures, our overall intent is to emphasize the outcomes of a comprehensive minimally-invasive structural heart disease program. Selection bias is inevitably introduced in a nonrandomized, unblinded study, as our program is collaborative, pursuing the most appropriate intervention after multidisciplinary discussion as well as discussion with the patient.

Did you look at predictors for residual leaks? Could there be other selection criterias developed from this study to prevent residual leaks?

We agree entirely that residual leak remains a clinically important problem. Therefore, at the Reviewer’s suggestion, we created a multivariable Cox proportional hazard model to determine adjusted hazard ratios for residual shunt. We report these individual parameter hazard ratios in Table 4.