

Peer Review File

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Reviewer A

Comment 1: The authors state in their conclusion that totally endoscopic robotic MV repair is associated with post-op stays of 2-3 days. Although this is possible even in the most experienced hands using this technique (Dr. Douglas Murphy) the average LOS was 4.9 ± 4.4 days with only 37% patients with a LOS <4 days according to their series with 1257 patients. I would make this point more clear to the reader.

Reply 1: Thank you for your comment. We have modified the abstract and text. We have quoted Dr Murphy's data with reference on p11 lines 11-13.

Comment 2: Dr. Modi's team performs this technique. Can they give us a quick summary of their experience with basic outcome results?

Reply 2: I did not give a summary of our experience because we have actually written this up for another publication and I wanted to avoid any potential for duplicate publication.

Comment 3: This manuscript mainly addresses MV repair as the preferred use of the robotic technology. The title of the manuscript states robotic MV surgery vs. transcatheter techniques. Current transcatheter techniques address both repair and replacement techniques. Please clarify. Are trying to create a comparison to MitraClip, Harpoon, Neo-chord, cardioband ... ect? My assessment is that the authors are trying to state that robotic MV repair surgery will be the ultimate challenger of transcatheter MV repair techniques. Please make this clear.

Reply 3: Thank you for your comment. What I was trying to do was make the point that robotic mitral valve repair is a procedure that is associated with a recovery time only slightly slower than transcatheter techniques, but with proven durability; whereas transcatheter techniques have uncertain and certainly inferior durability. I agree that I did not make this clear and have unintentionally created confusion. I have changed the title accordingly.

Reviewer B

Comment: I really enjoyed an excellent review article which overviewed the current situation regarding robotic assisted mitral valve surgery. I agree to the publication of the article. If possible, I would like to ask authors to add comment regarding knot tying for annuloplasty band or ring. The authors have already mentioned about the Core Knot system. However, it costs a lot, and it is unavailable in some countries. Please let us know the alternative way in secure ring fixation.

Reply: Thank you for your comment. Page 9 lines 4-14 now covers this.

Reviewer C: Congratulations for an exciting and comprehensive review of the topic.

The article is clear and concise and the discussion of the problem is supported by data.

Comment 1: Analysing the disadvantages of mini mitral surgery the authors conclude that many surgeons still working under direct vision. I do not agree, because: With the use of 3D HD camera systems like EINSTEIN Vision many experienced surgeons are able to perform a total endoscopic operation. Even in robotic cases there is a need of an incision - long enough to introduce an annuloplasty ring.

Response 1: Thank you and I agree with your point. I have modified page 5 lines 6-8 to reflect this. I have also modified the abstract and conclusions to reflect this. Flexible annuloplasty sizers and flexible bands allow some surgeons, both robotic and mini, to maintain ultra-small incisions.

Comment 2: Another little disadvantage in robotic surgery is the impossibility of concomitant procedures such as cryoablation for AF treatment.

Response 2: Thank you for your comment. We respectfully disagree because concomitant procedures like cryomaze, tricuspid valve surgery and ASD/PFO closure are routinely performed robotically.

Comment 3: Finally (in your conclusions) you compare robotic surgery and transcatheter procedures. This point could be problematic, because there is not

the same patient cohort. But I agree - cardiac surgeons should convince with excellent results in mitral valve repair.

Response 3: Thank you for your comment. What I was trying to do was make the point that robotic mitral valve repair is a procedure that is associated with a recovery time only slightly slower than transcatheter techniques, but with proven durability; whereas transcatheter techniques have uncertain and certainly inferior durability. I agree that I did not make this clear and have unintentionally created confusion. I have changed the title and conclusions accordingly.

Reviewer D: Thank you for the opportunity to review this paper. The paper, in some sections, nicely summarizes existing evidence from a balanced perspective, noting that many studies evaluating robotic vs. other approaches often provide extremely limited understanding of the true comparative effectiveness. There are several concerns regarding the significance and format of this review. Below are my comments.

Comment 1: In the presence of numerous existing reviews of robotic mitral valve operation, the novelty of this review is unclear. What novel angle or perspective does this review provide? Some existing reviews to consider are:
<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5135555/>
<https://www.ncbi.nlm.nih.gov/pubmed/24349987>
<https://www.ncbi.nlm.nih.gov/pubmed/24251030>
<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5872370/>

Response 1: Thank you for your comment. Our manuscript provides a contemporary review of the latest evidence compared to the articles the reviewer has cited, two of which were written in 2013. Another of the cited articles does not deal with robotic surgery, but rather mini mitral surgery. The novel perspective that our manuscript presents are our tips to traverse the learning curve.

Comment 2: It seems that there is a disconnect between the title, which implies the focus is somehow on comparison with the transcatheter approach, and the

actual content, which is a broad overview of minimally invasive mitral operations. The title should be revised for this reason.

Response 2: Thank you for your comment. What I was trying to do was make the point that robotic mitral valve repair is a procedure that is associated with a recovery time only slightly slower than transcatheter techniques, but with proven durability; whereas transcatheter techniques have uncertain and certainly inferior durability. I agree that I did not make this clear and have unintentionally created confusion. I have changed the title accordingly.

Comment 3: Overall, there appears to be a bit of a narrative expression that either distorts or obscures the intent of the author. At many locations, this review does not read like a scientific article but rather like a perspective article.

Response 3: Thank you for your comment. I agree that in some places this may read like a perspective but would submit to you that this adds to its value. It is unavoidable that any article giving tips and tricks to shorten the learning curve would read like this.

Comment 4: There is quite a bit of speculation or authors' opinions that are presented without citations. For example, 'It is this, combined with the remote centering of the four arms of the Da Vinci ensuring that no pressure is placed on the intercostal bundles, that we believe leads to faster recovery with robotic surgery compared to mini mitral surgery, an observation that is entirely intuitive as speed of recovery is inversely related to surgical trauma.' (line 69-). These speculations do not add substance to the review and should be removed.

Response 4: The remote centering of the Da Vinci is not speculation, it is simply fact and as such should not be referenced. I believe that I have now made it clear on page 5 lines 3-6 that faster recovery with robotics rather than mini mitral is simply our observation from our institution.

Comment 5: There are several sections where reference to a specific surgeon by name seems to carry a weight, which makes the article reads like an informercial : 'Communication must be clear given there is reliance from the console surgeon on the patient-side team, and wireless headsets, as proposed by

Dr Sam Balkhy (Chicago).’ This sentence also lacks a reference and appears to be an anecdote, which again adds to my impression that this article in its current state has an unscientific tone.

Response 5: We disagree with the reviewer. When one surgeon has developed a technique, it would seem respectful and appropriate to acknowledge their contribution, e.g. Dr Chitwood, Dr Balkhy. A reference (ref 43) has been added for the headsets which are most certainly not an anecdote and are used by ourselves and Dr Sloane Guy, as well as Dr Balkhy. I have however removed Dr Balkhy’s name.

Comment 6: Reference to specific products (Intraclude or Chitwood clamp) as the only existing options for cross-clamp in this setting is probably not appropriate. I recommend the authors to use descriptive terms rather than the product name.

Response 6: Thank you. I have changed the one of the references to the Chitwood clamp to the Transthoracic clamp. As per the Editor, I have not changed the Intraclude nomenclature.

Comment 7: Line 405: term ‘Exclusion criteria’ is usually used in the context of research study, not patient selection in clinical practice. This should be referred in the context of indications/contraindications.

Response 7: Thank you. We have changed the section titled Exclusion criteria to Patient selection.

Comment 8: Figure 1: Were these images taken or produced by the authors? Please cite proper sources if it is not the authors’ production.

Response 8: Figure 1 has been reproduced with permission from Intuitive Surgical and this has been acknowledged in the figure. All other pictures are our own, except Fig 4 which has been removed (see below).

Comment 9: Figure 4: Same as the above. This COR-KNOT image appears to be taken straight out of the manufacture brochure.

Response 9: Again this has been reproduced by permission from LSI Solutions.

However, to avoid using too many figures from outside sources, I have removed this figure.

Comment 10: Unclear sentence

Line 212: Thus, on average, it would take only one surgeon in each unit a whole career to traverse the learning curve.

Response 10: We have clarified the meaning by adding to the sentence which now reads “Thus, at a rate of three cases per year, it would take only one surgeon in each unit a whole career to traverse the learning curve.” (line 19).

Comment 11: The following sentence reads narratively and is not well-supported by the reference: ‘What is evident is that departments that have invested in their teams as much as their equipment see consistent uptake in robotic cases expanding to fill up to 90% of operative activity (15).’ Please consider revising for clarity.

Response 11: Thank you for your comment. I have removed this sentence.

Guest Editors: Thank you for your submission on robotic mitral surgery. It is a very well written review of the topic. In addition to addressing the reviewers comments I would ask you to consider the following:

Comment 1: I agree with the reviewers to consider your title and if you want to include specific comment on this within the review, or to reword the title.

Response 1: Thank you, I have changed the title. I realise now that I inadvertently created confusion with the original wording.

Comment 2: Regarding reviewer Ds comments, you do not need to justify the review since it has been invited. Regarding point 6 I think it is reasonable to mention products by name, but perhaps allude to the fact that alternatives are available (±any examples if there are). I think it is also ok to offer speculation, if it is made clear that it is such.

Response 2: Thank you. I have answered these points in the responses to individual reviewers.