Author’s response to reviews

Title: The impact of fellowship training on pathological outcomes following radical prostatectomy: a population based analysis

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Author’s response to reviews: see over
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Dear Editor,

We are pleased to re-submit for publication the revised version of MS# 1880857950129599, “The impact of fellowship training on pathological outcomes following radical prostatectomy: a population based analysis”. We would like to thank the reviewers for their insightful and helpful comments. We have used this constructive criticism to modify the manuscript and have addressed their individual comments in a point-by-point fashion below. Additionally, the manuscript has been modified with highlighting showing the changes, to facilitate the review process.

Referee 1 comments:

1. Surgeons’ experience should also be accounted for in analyses in this setting, long experience in a procedure will have an impact on the outcome, 1-2 years of fellowship training can make up some gap but eliminate the value of experience. The referred article in the discussion does not compare the outcomes head to head between new and experienced surgeons.
   
   We agree that surgical experience is an important variable to consider. We were not able to capture this variable objectively for our study; however, the fellowship-trained, academic surgeons (n=2) were relatively new to their practices, having completed their fellowship training 1-3 years prior to start of the study period. Both surgeons were the newest and youngest surgeons within the entire group.

2. The authors state that lower volume surgeons had improved rates for PSM. They discuss this very briefly in their paper. Given that the impact of fellowship training is being assessed here, and assuming that an academic fellowship trained urologist would have higher volume then this needs to be explored further, at least a breakdown of volume and training be shown.

   This is a difficult to directly explain; but may be due to the small number of surgeons practicing within the region (n=15). As a result, findings can be influenced by a limited number of individuals. The two FTA surgeons had moderate and high volume practices, despite being early in their respective careers. Yet over 70% of RP’s were performed in non-academic centers for the study period with a significant variation in average volume of practice (range from 0.2-51.7 RP’s per year). Thus, a high-volume non-fellowship trained surgeon who, in theory, has a high rate of PSM could influence the results. Further, 7 of 15 surgeons had annual volumes of <5 RP’s per year, highlighting the heterogeneous surgeon groups. Although less than ideal, we feel that this more truly represents the “real world” landscape of urologic practice. We have elaborated on this point within the discussion on page 13, lines 12-23.

3. The abstract states “the proportion of PSM between NFTA and NA urologists was not significant (p=0.815)” I do not see these results in the main part of the manuscript.

   This is an error. The p-value in the abstract should be p=0.492. This has been changed on page 4, line 23. This now corresponds to the text found in the body of the manuscript within the results section (page 11, lines 19-21).
4. Performing a nerve sparing procedure is an important factor that can affect rates of PSM. This has not been addressed at any stage in the article.

   We were unable to capture nerve-sparring rates in this population. A comment to this effect has been included on page 15, lines 2-7.

5. Are these all open retropubic radical prostatectomies? Any Robotic, laparoscopic or perineal cases?

   The vast majority of cases (>99%) were open RP with very few being performed laparoscopically (approximately 10 cases). This is now mentioned within the abstract (page 4, line 12) and the body of the text (page 8, lines 5-7). An additional comment stating that the results may not apply to minimally invasive techniques has been added to the discussion on page 15, lines 4-6. Robotic approach RP is not available in our region.

6. Why are the authors looking at 3 categories of surgeons? If the aim is urologic oncology fellowship and an oncologic parameter is being assessed there is no need to break the non-oncology trained group into further groups.

   The three categories of surgeon were based on the practice patterns within our populations. There were no fellowship-trained uro-oncologists within the non-academic sector. We agree that further de-identification of the surgeon groups into two groups (fellowship trained and non-fellowship trained) would also answer our question, however, we elected to sub-divide the surgeons into the third – non-fellowship trained, academic surgeons as a means to assess whether academia itself was a factor to consider. Ultimately, there was no difference in PSM rates between the NFTA and NA group.

Referee 2 comments:

Minor Essential Revisions:

7. In methods, the authors divide volume of cases in >10, 10-20 and >20. Why did they decide to divide it this way? Why is >20 cases a year a high volume? This would be less than 2 cases a month.

   Annual volume was initially assessed as a continuous variable but was not linearly related to PSM rates. As such, the annual volume was divided into tertiles, and assessed as a categorical variable based on where the frequencies were distributed among the surgeons.

8. The authors mention in results: there was less margins in the groups with low volume and mid volume. Is this correct or is it phrased wrong? Usually high volume surgeons have less margin positivity. Please clarify.

   This is a correct statement and has been addressed in point 2 of referee #1’s comments.

Discretionary Revisions:


   Done. Thank you.
Referee 3 comments:

Nayak et al. report data on a population-based analysis about the impact of fellowship training on the positive margin rates after radical prostatectomy. The paper correlates the level of oncological academic training to positive margin rates as surrogate marker for surgical quality. Overall, the paper is adequately written. Setting focus on training level rather than caseload per surgeon is not an entirely new aspect. However, given the growing importance of quality assessments in surgery it adds to the ongoing debate.

BACKGROUND:

10. P6 l 3: I would rephrase “However, neither individual surgeon data nor pathological outcomes were examined”.
   Please see page 7, lines 3-4. “Unfortunately, this study was limited by lack of individual surgeon data and information on clinic-pathological outcomes”

11. P6 ll5-8: I do not understand the point you are trying to convey with this sentence: please explain or rephrase as it might be redundant
   We were attempting to state that most urologic oncology fellows lead academic practices, thus if uro-oncologists truly had superior outcomes, this could contribute to the improve outcomes seen in academic institutions. However, we agree that there is redundancy to the sentence. The sentence has been re-structured on page 7, lines 4-6.

M&M:

12. Please state why laparoscopic or robotic approaches were not included in the study
   A minority (<1%) were performed laparoscopically. Robotic assisted RP was not available in our region at the time of data collection. This has been reflected in the manuscript on page 8, lines 5-7.

13. P 8 ll 10-12: Please explain why you chose your specific definitions of case load/surgeon (e.g. <10, 10-20 cases/year etc). Although this classification might reflect the situation in your population it should be commented on. Furthermore, it would be interesting, how many surgeons performed the procedures and also to include the range of procedures performed/surgeon.
   This has been partially responded to in point #7, above. Additionally, we have added a section within the results describing the surgeon population within the results on page 10, line 21 to page 11, line 2.

RESULTS:

14. Table 1: including p values and ranges – where applicable - would be nice to describe the homogeneity of characteristics in table 1. Please comment on this in the manuscript. Please also include depict the characteristics as whole numbers in addition to percentages as well.
   p-values and whole numbers have been added to table 1 with their corresponding frequencies. (page 22)
15. I do not understand why a p value of 0.2 was chosen in the univariate analysis for eligibility in multivariate analysis. Was this in order to include case volume?
   This point has been addressed in the statistical analysis part of the methods and can be found on page 10, lines 3-11.

16. Again, please include a table or graph with surgeon characteristics and range of cases/surgeon.
   We have described the surgeon groups in detail within the results section on page 10, line 21 to page 11, line 2.

DISCUSSION:

17. P 10 ll 11-14: The authors correctly cite the prospective study of Rosser 2006. However they remain vague about the conclusions of the cited study. It would be nice to comment a little further on the reasons why in this study less experienced fellows achieved almost the same results as experienced ones and how this relates to their own study.
   We have elaborated on the study by Rosser and colleagues on page 12, lines 6-14.

18. P 11 ll3-11: it is a good point to state that population based study may reflect every day practice better than tertiary care centers. However, the vague statement about “multi-factorial reasons should be made more specific.
   We have elaborated on this point on page 13, lines 12-23.

19. P 13 , l 3: rephrase to “...should be considered...”
   We have restructured the sentence to now read, “This finding highlights an important surgeon related factor that should be considered and requires further investigation in larger studies.” (page 15, lines 13-14)

Sincerely,

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