

06-Jun-2022
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Can female surgeons break the glass ceiling? A comparison of short-term surgical outcomes of male and female surgeons in Japan: a retrospective cohort study

Dear Dr. Nomura

Thank you for sending us your paper. We sent it for external peer review again.

We hope very much that you will be willing and able to revise your paper as explained below in the reviewer's comments so that we will be in a better position to understand your study and decide whether the BMJ is the right journal for it. We are looking forward to reading the revised version and, we hope, reaching a decision.

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Yours sincerely,

Tiago Villanueva
Associate Editor
tvillanueva@bmj.com

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Decision: Put points

First, please revise your paper to respond to all of the comments by the reviewers. Their reports are available at the end of this letter, below.

In your response please provide, point by point, your replies to the comments made by the reviewers, explaining how you have dealt with them in the paper.

** Comments from the external peer reviewers**

Reviewer: 1

Recommendation:

Comments:

The authors addressed all my queries accordingly.

Additional Questions:

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Comments:

I'd like to thanks the authors for addressing my comments but I still have some further queries.

- 1) In the revised text the authors mention the three surgeries were chosen because the number of female surgeons who performed these surgeries was sufficient for analysis – this is vague. What constitutes 'sufficient'? Was this some sort of sample size calculation required for the analysis?
- 2) In the main analysis the authors mention that continuous variable were categorised to account for non-linear relationships between the variable and outcome. I mentioned this in my previous comment – why did you not explore this and show the relationship – you could have used splines or polynomial regression to account for this non-linearity. I accept you did what planned to do and do later explore some of this in the newly added sensitivity analysis
- 3) However, later on the authors say non-linear relationship were assumed based on previous research on the volume-outcome relationship. This seems less satisfactory – you have data, why don't you explore the relationship in your data.
- 4) Your additional sensitivity analysis are post-hoc analyses which are not described in your protocol – you should describe them as such. These analyses become more exploratory.
- 5) In one of the tables you have a footnote to suggests that number of surgeons in each category does not add up to the total number of surgeons in the study population because some surgeons moved to a higher category (in terms of seniority) during the study period. So this is clearer why do you not have a rule and report the highest category achieved in the study timeframe.
- 6) I would check all tables and results for errors. E.g. table 2 – you need to specify the number of surgeries per year is reported as a median and IQR. I think the last category for the number of surgeries per year should also be ≥ 50 . Same applied for all other tables. Report all estimates to a consistent number of decimal places.
- 7) I think your Abstract needs to report some of the OR's and CI's you found from the main analysis. I wonder if all the OR's to the analyses performed would also be better in a Table as well as reported in the text.

I'

I'd like to thank the authors for addressing my comments.

Additional Questions:

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Job Title: Professor of Biostatistics

Institution: University of Manchester

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