02-Apr-2022
BMJ-2022-070568 entitled "Can female surgeons break the glass ceiling? A retrospective comparison of short-term surgical outcomes of male and female surgeons in Japan"

Dear Dr. Nomura,

Thank you for sending us your paper. We sent it for external peer review and discussed it at our manuscript committee meeting. We recognise its potential importance and relevance to general medical readers, but I am afraid that we have not yet been able to reach a final decision on it because several important aspects of the work still need clarifying.

We hope very much that you will be willing and able to revise your paper as explained below in the report from the manuscript meeting, 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.

Please remember that the author list and order were finalised upon initial submission, and reviewers and editors judged the paper in light of this information, particularly regarding any competing interests. If authors are later added to a paper this process is subverted. In that case, we reserve the right to rescind any previous decision or return the paper to the review process. Please also remember that we reserve the right to require formation of an authorship group when there are a large number of authors.

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Tiago Villanueva
Associate Editor
tvillanueva@bmj.com

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Report from The BMJ's manuscript committee meeting

These comments are an attempt to summarise the discussions at the manuscript meeting. They are not an exact transcript.

Members of the committee were: Elizabeth Loder (chair), Jamie Kirkham (statistical editor), Nazrul Islam, Joseph Ross, Di Wang, Jessica Kimpton, Tiago Villanueva, John Fletcher

Decision: Put points

Detailed comments from the meeting:

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.

Please also respond to these additional comments by the committee:

- It is unclear why you have categorised many of the adjustment variables or hospital case numbers - why not treat these as continuous? Some adjustment has been made for cases from different hospitals but the same hasn't been done at the surgeon level, i.e. many of the surgeons in the study are undertaking many surgeries
- You have adjusted for a lot of covariates, many of which do not appear to be confounders. Therefore, the authors could expand why they adjusted for these factors. Should these confounders be adjusted for? 1) average annual surgery volume per surgeon (It's possible female surgeons generally have lower average volume). 2) SES of the patients. 3) Region of the hospital
- There are big differences between men and women in the types of surgeries they are performing, raising concerns about using MV models. Perhaps they should be stratified by surgical approach? Moreover, shouldn't hierarchical models be used (or at least GEE) to account for the clustering of patients by surgeon/hospital? Do surgeons operate at multiple hospitals?
- Competing risk was not discussed.
- Complete case analysis was done, but some ideas about the extent of missing data will be essential.
-More context is needed to understand how a surgeon is assigned to a procedure. Are there patients' preference? Some other factors? Or is it kind of random? This will also help us understand if simple adjustments are adequate in dealing with a potential selection bias.
- Findings for Female surgeons with <=5 years of experience are concerning. Will that affect their employability and trust/confidence among the patients?
- Could you include operations in other surgical fields (but maybe this is beyond the scope of this study?
- There is not a lot of detail on how the data were collected nor how complete they are. "Post operative" could cover quite a time period.
- It should be clarified in main text that only surgeries for gastric cancer/rectal cancer were included (flow diagram).
- Error of number: table 4, OR for Surgical mortality or severe complications of DG: 1.28 (0.93, 1.14).
- We don't see a role for PPI here as it would turn into another research question. You could declare this and how they plan to disseminate it.
- To make this paper better you could stress these 3 surgeries are only representative but that across medicine equity and in training, inclusion, mentoring and practice produce better medicine.
- Please submit the study protocol.

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

Comments from Reviewers

Reviewer: 1

Recommendation:

Comments:
Thank you for sending me this manuscript for review. This is an extremally important and relevant topic. Although, it lacks originality, it provides local evidence on this matter.

I have made some small comments to the authors:
1. Severe postoperative outcomes: define if this is in-hospital? Who classified the postoperative complications (e.g. CDC)?

2. Have the authors considered using any postoperative complication as an outcome of interest?

3. Statistical analysis section: It would be helpful to indicate what an OR >1 indicate.

4. Page 4 (Line 57). ‘A total of 14,0971 eligible DG surgeries…’ Please note typo on the total number of DG surgeries.

5. Adjusted variables. Its noticeable that all adjusted variables were dichotomised. It would be important to describe the method used for this? And also the reason for not using it as a continuous variable. Perhaps even an ad hoc analysis included as an online supplementary material would be helpful.

6. Tables 4-6. The non-adjusted ORs should be added. The footer should also include the variables that were used for the adjustment. Is it possible to include another column and have the analysis done for overall cohort (e.g. DG + TG + LAR as gastrointestinal surgeries)? This would address some of the limitations reported in the discussion section.

7. ‘Surgical mortality or complications with a CD classification of 3 or higher’. Could this be changed to severe postoperative complication? As mortality is classified as CDC V (i.e. redundant).

8. Page 5 (Line 18): ‘Female surgeons performed surgeries on relatively high-risk patients.’ It would be interesting to compare outcomes for high-risk patients / low risk patients only between female x male surgeons.

Best wishes!

Additional Questions:
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Please enter your name: Danie Steffens

Job Title: Director

Institution: Surgical Outcomes Research Centre

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Reviewer: 2

Recommendation:

Comments:
This study compared the overall 30-day mortality rate and complication rates between female and male surgeons in Japan for elective distal gastrectomy, total gastrectomy, and low-anterior colon resections between 2013-2017 in the Japanese National Clinical Database. There was no difference in mortality rates, complication rates, pancreatic fistulas, or length of hospital stay between female and male surgeons, even though the female surgeons performed more open surgical cases and had higher risk patients.
This study has the usual criticisms that can be leveled against any study that is examining outcomes from administrative data. The large number of cases studied should have resulted in some statistical differences which were not seen.
This is a valuable study for the gender comparison with these operations

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Please enter your name: Donald Edmund Fry

Job Title: Adjunct Professor of Surgery

Institution: Northwestern University Feinberg School of Medicine, Chicago

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Funds for research?: No

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Reviewer: 3

Recommendation:

Comments:
This is paper is based on an interesting topic but I have a number of recommendations for improvement, particularly with regards to the statistical modelling which I felt was a on the simplistic side given the data source.

1) What is the rationale for choosing just three surgeries – this could be seen as a highly selective cohort. Why did you not look at all surgeries – some rationale is needed. You could look at surgery type as a factor rather than looking at them separately.

2) Can you also justify the date range, 2013 to 2017 is a bit dated – is what is presented still reflective of current practice – again this could be seen as highly selective.

3) There is some missing data in this data set which is not discussed in much detail – a complete case analysis was in fact performed, can the missingness not be investigated further. Can you describe the case mix of those with missing data for example, are they reflective of those used in the full analysis.

4) The time point of analysis for all outcomes also needs justifying, this can be important here on outcome. Surgical mortality was defined as in-hospital deaths up to 90 days post-op but any death up to 30 days post-hoc – why the inconsistency? Which was used in the mortality analysis?

5) I am unclear why so many continuous variables were categorised – it makes more sense to treat these as continuous ad would make the paper easier to read.

6) The authors need to describe in full the statistical models that were used, how variables were selected for inclusion, the assumptions and the methods of estimation. I think hospital level characteristics are accounted for but what about the surgeon case mix. The surgeons in this data set are conducting many surgeries but how is this accounted for in the modelling? The correlation here may be important. Based on the final model, can we graphically see the profiles of the female vs male surgeons. We can also see from the univariate analyses that males are considerably more experienced and females are operating on higher risk, older and patients with more morbidity factors – how is this adjusted for in the model. All these things will affect outcome. This links to model selection. This needs describing better in the methods.

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Please enter your name: Jamie Kirkham

Job Title: Professor

Institution: The University of Manchester

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

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Reviewer: 4

Recommendation:

Comments:
Thank you for the opportunity to review this study which compared the short term surgical outcomes between male and female surgeons who performed DG, TG and LAR from the years 2013 - 2017. Overall, interesting question and I have a few comments, firstly relating to the manuscript itself then a few philosophical comments which perhaps may be more suitable for an editorial which I think is important that it accompanies the publication of studies like this for context and also to highlight the importance of the manuscript.
I also think that the authors need to be careful about the message that they are trying to deliver with this manuscript. There is the possibility of suggesting alot of stuff which I feel are not that clinically relevant eg point 2.
1. It is pretty difficult to read / understand the overall data in the first paragraph of the results. ie how many were eligible but then xx (%) opted out and of these, how many were performed by male and how many by female surgeons (and %). I would suggest a simple Table.
2. Because the dataset is large even small differences will be statistically significant. Eg the operating time differences between 259 and 261 minutes - a time difference of 2 minutes is really not clinically significant. You need to highlight this for all the operations. And everything else that does not make much clinical difference.
3. You need to run some multi-variate analyses. I think you need to highlight in the discussion that some of the outcome differences could be accounted by the fact that there are very small numbers of female surgeons, even one adverse event is likely to cause a large effect whereas the same does not apply to male surgeons. Additionally, the female surgeons are operating on sicker patients who are older, more likely to be anaemic, malnourished, and have comorbid conditions. Therefore, the seemingly higher morbidity rates, could be easily accounted for because of these reasons, rather than a genuinely clinically important difference for female surgeons < 5 years of experience. I think this needs to be flashed out in discussion more.
The message in Table 6 can be damaging when you are comparing with female surgeons who contribute to a very small workload. This really needs to be highlighted as a limitation.
4. I think that you need to clarify if these data are for public vs private hospitals and what proportion of females are in more regional vs major teaching hospitals. Eg private hospitals may have a greater proportion of male surgeons and they may have also have patients with earlier stage disease. Or major teaching hospitals may be better equipped for minimally invasive techniques compared to more regional centres where there may be more female surgeons. I think these are important aspects of the discussion which has not been mentioned at all in the discussion.
5. I think the authors need to also ask themselves why they feel the need to compare outcomes between male and female surgeons - both will have professional qualifications, both will be equally trained - and as we all know, there are training biases such that male trainees and surgeons are more likely to have more training, go away for fellowship (again - not discussed in manuscript), so, we need to ask ourselves, what is the reason for these fundamental differences. Is it not the doing of the society as a whole because of the glass ceiling that these female trainees / surgeons face? I agree that it is important to demonstrate that the female surgeons are not inferior, they are if anything, possibly better in view of the sicker patients they manage and this also needs stronger discussion in your manuscript.
6. The authors really need to make stronger statements in their discussion - eg surgeons performing LAR with > 20 years experience. Patients of female surgeons have a 11 times increased risk of dying.

The 95% CI however goes from 1.05 to 118 - this is a massive CI and suggests that the estimate is so imprecise and yet this is not mentioned in discussion. The small numbers of female surgeons needs to be discussed as a limitation.
7. Despite the comprehensive nature of NCD, the authors need to include in their discussion that they lose over 1/4 of their cases from exclusions. This a large number of cases that have been excluded. This needs to be acknowledged as a limitation. And please see point 1.
And I would dare say that this needs to be presented at a Japanese meeting and perhaps, published in some way in a Japanese journal.
If needed, I will be happy to be involved in an editorial for this paper if it gets published.

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Please enter your name: Cherry Koh

Job Title: Colorectal Surgeon

Institution: Royal Prince Alfred Hospital, Australia

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