Reviewer's report

Title: Analysis of pregnancy outcome after anastomosis of oviduct and its influencing factors

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Reviewer: Yvonne Louwers

Reviewer's report:

The authors Feng en Zhao and colleagues reported on pregnancy rates after laparoscopic oviduct anastomosis. They identified factors increasing the pregnancy rate after oviduct anastomosis, namely oviduct length and bilateral recanalization and duration of sterilization. Obviously this issue is actual in China since the new reform the two-child policy in 2016. Patients were included from 2014 till 2018, which is even before this new law was passed. I do have several issues comments on the manuscript.

Page 6 line 5. Do the authors mean that the surgery was performed on the 3-10th cycle dag, or do they mean the surgery was performed on the 3-10th day after the menstruation stopped?

Did all included subject had a regular menstrual cycle?

156 patients were included and the authors describe a patency rate of 100% of at least van oviduct. The authors checked the success of the surgery with methylene blue during surgery. Did they also investigate the accessibility of the oviduct 6 weeks after surgery using HSG for example?

Were these surgeries performed by the same surgeon?

Why do the authors conclude in the abstract that 'the pregnancy rate after laparoscopic tubal ligation and anastomosis was higher than that of open tubal ligation and anastomosis' since all subjects underwent laparoscopic oviduct anastomosis?

The authors do not report data on complication rate (including blood loss, (urinary tract) infection, ectopic pregnancies).

How was pregnancy-rate defined? Were these pregnancies ungoing pregnancies? Inclusion period is until October 2018 so I assume these were not.

Obviously, fertility rates decrease with age because a decrease in ovarian reserve. How do the authors think that this would have influenced their results?

Did the authors correct for confounding in their analyses, for example BMI, smoking, adhesions found during surgery?
The first alineas of the discussion are a description of the results, instead of a discussion putting the result in a broader perspective.

The authors do not consider IVF for women who have a renewed wish to conceive. Do the authors have information on the cost-effectiveness of this procedure compared with IVF?

For me the conclusion (in the abstract) is not clear. The authors state that the pregnancy rate after oviduct anastomosis can be ‘improved’ in patients. How do they think that this can be improved? Do they mean that the counseling in these patients can be improved to a more patient-tailored approach?

Some recent international literature on this subject is missing (for example an extensive systematic review by van Seeters et al Human reproduction Update 2017).

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I recommend additional statistical review

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