**Reviewer’s report**

**Title:** Spontaneous First Trimester Miscarriage Rates Per Woman Among Parous Women with 1 or more pregnancies of 24 weeks or more

**Version:** 0  **Date:** 09 Mar 2017

**Reviewer:** James Scott

**Reviewer’s report:**

This is a revised manuscript that has been resubmitted. My comments and questions are as follows:

1. The author implies in the cover letter that the reviewers only wanted wording changed and that there were no problems with the methods or results. That is not the case.

2. The purpose of the study "was to identify the prevalence of first trimester miscarriage per woman in parous women." This is stated clearly in lines 103-104, but there should also be a clear-cut statement at the end of the Background section of the Abstract. In addition, it should be clarified that this is a study on "spontaneous" miscarriages.

3. Lines 56-104 - Much of this long Introduction is a critique of other studies and would be better placed in the Discussion section. There is still unusual wording (lines 62-63) and some unreferenced opinions (lines 72-74, 92-93). The statement that D&C may increase subsequent miscarriage rates is unproven and needs a reference (line 93).

4. Methods: Line 113 - "whenever possible" - How often was it not possible? Line 116 & 136 - What is the definition of "practitioners"? Do you mean midwives, physicians or both? Line 142 - What is the "standard equation"?

5. Results: The study relies on patient recall rather than documentation of spontaneous miscarriages. Since 18% of patients were excluded because of a blank field, there is still a question about whether that group is the same as those included and whether that would affect the results. Moreover, these are all inpatients admitted for delivery. Isn't it possible that there are a significant number of patients who had miscarriages and who have not had a successful pregnancy and would not be included because they would not be admitted for delivery?

6. Discussion: Rather than attempting to embellish the results by stating "almost half" (line 177), it is better to stick to the facts (43%) about the number of women with one or more miscarriage. Line 193 - The statement that recurrent miscarriage is associated with high rates of infertility is questionable and needs a reference. Statements about future studies (lines 221) add little to the paper. There are a significant number of limitations listed that limit the accuracy and validity of the results. The statement that "all large databases suffer from data entries and missing data" is an opinion and is incorrect.
7. Conclusions: For the reader, what do you consider "young"? How would your counselling differ with a 24 year old woman vs a 39 year old?

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.

No

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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Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

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