Reviewer's report

Title: The epidemiology of bacterial vaginosis in relation to sexual behaviour

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Reviewer: Per Gunnar Larsson

Reviewer's report:

I have reviewed the article by Verstralen, Verhelst Vaneechoutte and Temmerman. The epidemiology of bacterial vaginosis in relation to sexual behaviour.

I find this a very interesting review and I am very impressed by the very fine and well done work. I think the authors make a very good presentation of the literature but I do not come to the same conclusion as they do. This difference is however not wrong as it reflects that we do have different opinion in this matter. I think therefore that the article shall be published as it is without any changes.

I could however suggest some modifications that could improve the article but these suggestions are not necessary if the authors choose to ignore these I still think the article should be published.

1. I would prefer STIs rather then STDs.

2. The author presents very much data that BV is not exclusive a result from a penis vaginal penetration. But the data that WSW has nearly always the same vaginal flora implies to me that BV is a contagious disease. And that BV can be transmitted both by fingers, tongue, sex toys or penis.

3. Your conclusion that BV is a sexually enhanced disease with frequency of intercourse being the critical factor is not supported by very much epidemiological data. I would therefore suggest a slight modification of that conclusion.

4. About partner treatment you make an excellent resume of the literature that has been published. And you say that the studies are not as good and maybe one should be careful about conclusions but then in the end you still use this as an argument. The treatment regimen in Moi (42) and Veitorg (41) study is 2 gram of metronidazole day 1 and 3. This treatment has only been evaluated in an open label study (Jerve) and not in a placebo study. I would say that the one month cure rate with this treatment is not better than 50%. With that low cure rate a partner treatment would not make any difference. But in Colli study (45) he has a 10% improvement (not significant) and he uses clindamycin orally to the male. What we should do is to suggest a better and well designed study.

5. In you summery you have as the first section that BV are present among sexually non-experienced girls. But in the next section you modify this. I would suggest that you delete the first and only discuss the second paragraph. You discuss the excellent study by Fethers (76) very well but then you modify this
with the Bump study. The Bump study (83) should you be more aware of. First the girls were recruited by three adolescents classmates (!) then subjects received monetary compensation. Then the diagnosis of BV is “Discharge was labelled as abnormal and consistent with BV if it was of abnormal consistency (homogenous or frothy) and excessive volume and/or if it had a marked fishy malodor. Then one girl of 8 who had BV on the initial visit had still BV at 3 month. A recovery rate of 87% for girls that have not got any treatment against BV!! As you know that the placebo cure rate after oral treatment are not more than 5%!! Well I guess that this study would never have been published today. Do then not draw any conclusions from it. You make one reference to a study of Swidsinski (26) that are in press. I am sorry but have no access to that study.

I congratulate you to a very nice article and I suggest it to be published.

**Level of interest:** An article of outstanding merit and interest in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

'I declare that I have no competing interests’