Reviewer's report

Title: Genotyping of Streptococcus agalactiae (group B streptococci) isolated from vaginal and rectal swabs of women at 35-37 weeks of pregnancy

Version: 1 Date: 28 April 2009

Reviewer: Amos Adler

Reviewer's report:

Major Compulsory Revisions:
1. This manuscript is engaging in 2 separate questions- the performance of several culture methods and the heterogeneity of strains, as assessed by serotyping and RAPD. The first question is adequately approached, although it does not add any new data and I doubt that the differences are significant. The second question that the study aims to answer, engage with the strain heterogeneity within a single patient. This question is original, has potential clinical importance and the authors had adequately coped with it. My main concern is the adequacy of RAPD for that purpose. Although RAPD is relatively easy to use, it has some significant limitation compared with other methods (especially sequence-based, such as MLST) in regard with its reproducibility and portability. Hence, it is difficult to use it for understanding the population epidemiology of a species. Hence, I think that a method such as MLST could have been a better choice and would increase significantly the scientific value of the manuscript. However, in my mind it is still acceptable as it is. I would suggest that the authors will also report the susceptibility differences between the isolates, even only to a small number of antibacterial such as clindamycin and erythromycin, since that may have clinical importance.

2. ‘Sensitivity and specificity of different culture techniques for the detection of GBS'- since Granada was the most sensitive method, I wonder if they found also non-hemolytic strains on the CAN agar. If that is the case, it would be interesting to know whether these strains were also pigment-producing on the Granada agar, as these phenotypic traits (hemolysis and pigment production) are genetically linked. If non of the isolates were hemolytic, the authors should comment about the limitations of the Granada agar in detecting these strains.

3. The authors need to discuss the limitations of the study.

Minor Essential Revisions

Abstract:

1. The authors should detail that the indirect culture onto Granada agar was following overnight incubation in LIM.

2. Please fill in the number: “Of these, 19 harbored GBS in both rectum and vagina, ____only in the vagina and 8 exclusively in the rectum.”

Background:
1. Regarding the term ‘neonatal invasive disease’- since there is no neonatal non-invasive disease, I would just write either ‘neonatal infection’.

2. Delete: “vast majority of all cases”

3. The authors should elaborate on the rate of maternal carriage, screening practice and rate of neonatal disease in their own country.

Results, tables and figures:
1. The sentence ‘indirectly onto Columbia CNA agar resp. Granada agar after incubation overnight in…’ is not clear.

2. Table 1- please add statistics.

3. Table 2 needs a much more detailed legend, explaining the abbreviations.

Discussion:
1. The discussion regarding the different culture methods is too long and should be shorten.

2. ‘Epidemiology’- it would be interesting to know (either in the discussion or the background section) whether there is previous data from Belgium and what the screening practices in Belgium are.

3. ‘Serological and genotypic diversity among GBS isolates’- the sentence “Unfortunately, only one isolate was genotyped per visit and the observed genotypic diversity in our study indicates that the turnover of 8.3% might be an overestimation, caused by picking colonies belonging to different genotypes, both present at both sampling moments, but sampled only once, due to randomness of sampling.” Is too long and obscure and needs to be revised.

4. The paragraph ‘General serotype distribution’ is too long and should be minimized, as it adds no significant new knowledge.

5. The paragraphs ‘Serological and genotypic diversity among GBS isolates’ and ‘Correlation between serotyping and genotyping’ should be combined and altogether shorten.

6. The authors need to discuss the limitations of the study.

Methods:
1. There is no need for a detailed description of the sampling process.

2. I wonder how you can perform a CAMP test on non-hemolytic GBS colonies.

3. The paragraph ‘Identification of the isolates as Streptococcus agalactiae’ should come before ‘DNA extraction…’.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.
Declaration of competing interests:

I declare that I have no competing interests.