Reviewer's report

Title: Identification of Congenital Rubella Syndrome in Sudan

Version: 1  Date: 26 January 2014

Reviewer: Pawel Stefanoff

Reviewer's report:

The article "Identification of Congenital Rubella Syndrome in Sudan" is a concise, well-written description of a survey of children with birth effects admitted to several hospitals in Sudan. The manuscript is suitable and interesting for the international audience because efforts to eliminate rubella and rubella syndrome need to be concerted across all continents. The main weakness of the manuscript is the methods section that do not contain several important details of the study. Also, the discussion section should be revised, as often it deviates from strict interpretation of obtained results, and sometimes embarks on unjustified generalizations. Below are some suggestions on possible improvements to the manuscript:

Major Compulsory Revisions:

1) The aim of the study is stated at the end of the Background. It is clear and straightforward. However, it is important to add the purpose of it, why the authors conducted the research; Was it done just to know how many cases occurred? Or was it done to inform policies, target interventions to prevent further CRS cases?

2) Methods: it is not clear how the authors selected the hospital units and how were suspected cases recruited; They should thoroughly describe the sampling procedure (the selection of hospitals, selection of children in the hospitals, was the sample size estimated for this study?)

3) Methods: the authors should justify the representativeness of the selected sample (what proportion of Sudanese hospitals was included in the sample frame and how well they represent the country? how the authors did assure that all children with birth defects were screened - did they systematically screen the hospital records? did they maintain logs of all children with birth defects that were subsequently ascertained for inclusion criteria?)

4) Methods: Who checked the inclusion criteria? Were there trained collaborators in each participating hospital who saw each suspect child and verified which infant meets the criteria for WHO suspect or clinically confirmed case?

5) Methods: The data collection procedures need to be described in more detail: Did the authors use a questionnaire or did they extract data from medical histories (using predefined data extraction form?). Did they collect any data from parents (for example on rash or vaccination status)? If yes, did they pilot test the questionnaire? Did they collect the data personally or using trained interviewers
(or maybe mail or telephone interviews)? How was data confidentiality assured (were the questionnaires anymized)?

6) Results: The authors should use more explicitly the WHO classification: which were laboratory confirmed cases, which clinically confirmed and which clinically suspected. It can be deduced from the description, but nevertheless the terms should not be used interchangeably with information such as "[...] were positive for IgM" (line 107). It would be useful to update the Table 1 with the WHO classification of each case. Ideally, particular columns of this table could refer to WHO criteria.

7) Discussion: The paragraph included in lines 116-126 is slightly confusing. It would be much clearer if the authors do not start from a general "encyclopaedic" statement about the need for careful evaluation of serologic results. This important interpretation should start from the results followed by sequential argumentation, for example: "XX children were diagnosed by detecting persisting IgG antibodies in absence of IgM antibodies. These antibodies could be persisting maternal antibodies. However, presence of symptoms compatible with CRS and the unvaccinated status of mothers lead to ascertaining these cases as probable CRS cases" or something like that.

8) Discussion line 134: The authors did not confirm the presence of CRS in Sudan, as it was already confirmed in the previous study. Furthermore, if there is massive circulation of rubella, it is not possible that there is no CRS. I would therefore suggest a subtle modification of the study interpretations (not to confirm the presence of disease, but rather document the burden of disease; This is one of the reasons why documentation of the study representativeness is so important!.

9) The discussion section should less concentrate on the justification of obtained numbers (why the frequency of CRS was low), and more on the public health aspect of this problem. CRS constitutes a huge burden to societies and is vaccine preventable. The socio-economic consequences of CRS should be emphasized!

10) Discussion lines 136-137: This does not have anything to do with this study results. It can be mentioned as the recommended next step, but cannot be used in the interpretation of the obtained results.

11) The discussion should end with a comprehensive paragraph listing the study limitations. The authors already described the limitations of the specimen collection and laboratory investigations. However, the limitations of the epidemiological study should be also listed and interpreted. Each epidemiological study has limitation, typically related to sampling, sensitivity and specificity of case definitions, data quality, etc.

12) The references list formatting is incorrect. For example, the journal names should comply with the authors guidelines (e.g. ref. 7 includes the name "Clinical microbiology and infection : the official publication of the European Society of
Clinical Microbiology and Infectious Diseases"; According to the journal style, the correct abbreviation is "Clin Microbiol Infect". All references should be double-checked.

Minor Essential Revisions:

13) Methods: It would be useful to summarize the case classification used in the study for example in a text box, referring to the WHO materials; Not all readers may be familiar with this classification.

14) Discussion lines 139-141: This statement is unjustified. The authors mention that the study was performed in many different provinces (possibly all country). Thus, the result of 2% laboratory confirmed clinical suspitions do not pertain to Khartoum region, but to the whole country.

15) Discussion: It would be useful if authors would discuss the potential impact of their results to the current situation in Sudan. For example if they knew that their sample represented 10% of hospitalized children during the study period, than the frequency of CRS could be such and such, leading to such and such socio-economic consequences...

Discretionary Revisions:

16) Style: the authors should avoid excessive use of the words that contain no information, like "moreover", "nevertheless" or "importantly". They are not needed if the scientific argumentation is provided in a clear and sequential manner.

Level of interest: An article of importance 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:

In 2008-2010 I was coordinating a research grant funded by GSK. Otherwise I do not have competing interests.