Reviewer’s report

Title: Antibiotic prophylaxis for caesarean section at Mulago Hospital: a randomised clinical trial evaluating the effect of administration time on the incidence of post-operative infections

Version: 1 Date: 11 September 2014

Reviewer: Hassan Ba’aqeel

Reviewer’s report:

Title: Since the manuscript is being considered for publication in a none local journal, suggest to either delete the hospital name or add the name of the country (Uganda)

Abstract: Will need re-writing in line with the comments below.

Background: 1.1) The rate of C/S at Mulago needs to be reported in an explicit meaningful way. Without reporting the total number of deliveries per annum, fifteen thousand cases per year (Page 4, lines 12-14) is meaningless. Suggest reporting the C/S rate as percentage of total births.

1.2) Available evidence does not conclusively support the statement that pre-incision administration is not associated with negative effects on neonatal outcome (page 5, lines 3-4). The most recent systematic review and metaanalysis (2013) included six trials that met the criteria. It showed that there was no increased immediate neonatal risk but, the quality of the evidence (Grade) was downgraded to moderate because of the sample size. (1) The long term effects of in-utero exposure to antibiotics have not been studied in the context of C/S but there is some evidence linking such exposure to childhood allergies. (2, 3) This needs to be addressed in the manuscript.

1.3) In line with the above, the sample size calculation in the current study was based on maternal infectious outcome and not neonatal outcome, the last sentence (page 5, lines 16-
19) needs to be rephrased.

Methods: 2.1) Double blinding was very feasible yet was not adopted. This needs to be addressed and justified. The term “scheduled for C/S” (page 6, line 12) is vague. It would be better to state what type of C/S were included, elective versus emergency or both. The implication of this inclusion criterion on infectious morbidity is well established.

2.2) Block randomization deserve more description of how it was generated and what was the block size? The allocation method is not very clear. Was this through opening an opaque envelope that would indicate an “A” or a “B” allocation?

2.3) The timing of the intervention in the experimental group is clear (15-60 minutes before Skin incision). However for the control group the timing is inadequately described “after performing the skin incision”. How long after? Was it after clamping the cord? Please see comments under results.

2.4) Outcomes definitions (endometritis, wound infection and febrile morbidity) need to be revisited. For endometritis (page 6, lines 22-23) the criteria listed by the authors is missing a commonly used criterion i.e. fever. The definition of febrile morbidity is OK but the authors need to justify why it is only performed on the tenth postoperative day (after discharge from hospital). How this parameter was recorded. Was there actual home visit and actual thermometer measurement? This needs to be mentioned. Specific neonatal outcomes were neither listed nor defined. The study question posed by the authors included neonatal outcomes.

2.5) Primary outcome was stated to be “either the presence or absence of infection...” (page
7, lines 4-6) which is not among the three defined outcomes. Is this a composite outcome? If yes, this needs to be explicitly stated and the limitations of composite outcome need to be addressed.

Results: 3.1) The way that the timing of intervention in both groups is not informative, although numerically correct but, does not indicates how compliant the research team with the study protocol was. The SD values are large. The authors are advised to report what percentage of subjects in the experimental group received the intervention within 15-60 minutes before incision (point of reference). For the timing of the intervention in the control group, as mentioned earlier, the authors have to state the point of reference for the timing of intervention. In this type of trials it is usually after cord clamping.

3.2) The standard of reporting effect measure in clinical trials for dichotomous outcomes is the risk ratio. The authors are advised to use RR and their 95% CI.

Discussion: 4.1) Will need to be rewritten in line with the above.

References:

Level of interest: An article of importance in its field
Quality of written English: Needs some language corrections before being published

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.