Major Compulsory Revisions

1. General Comment: The authors did not investigate other possible causes of febrile illness (such as a urinary tract infection, sexually transmitted infection, or other common viral or bacterial infections). Their finding that there was no statistically significant difference between symptomatic and asymptomatic women in terms of haemoglobin, WBC or geometric mean parasite load, represents to me a real possibility that at least some of those with symptoms may not in fact have had clinical malaria. Could the authors please elaborate on this in their manuscript? For example, has dengue been reported in this specific setting, as this would have similar risk factors to those as currently reported here, particularly water source proximity (e.g. as found in a recent study conducted in Cameroon; PLoS Negl Trop Dis 2014, 8(7):e2950). Other issues to consider include: did clinical disease vary by HIV status? What is the prevalence of tuberculosis (and HIV-TB co-infection) among pregnant women in this setting?

2. Lines 206-213: I found it very hard to follow parts of this section and suggest that it be re-worded for clarity e.g. what proportion of all those with a febrile illness were parasitaemic, and was parasitaemia significantly associated with clinical illness? We are told that there were 68 parasitaemic women (line 206), and then told of a sub-set of 51 women (lines 210-212) but it is unclear how this subset was derived e.g. are these 51/68 or a new sub-set of 51/303 who had clinical symptoms? In line 212 it appears that 41 of the 68 parasitaemic women were unwell and 16/68 were not, but I could not work out the proportion of women without detectable parasites who had a febrile illness. A comparison of these two proportions would clearly be very helpful.

3. Lines 269-271: please revise for clarity as per my comments above.

4. Discussion section: I found it difficult to comment on this section of the manuscript due to the unresolved issues above. Providing a brief summary of the relevant entomological literature specific to this setting would strengthen the authors’ assertion that water source and vegetation proximity to households are associated with increased risk of malaria e.g. is this plausible given local vector populations and has this been proven in earlier research? Have dengue / arbovirus vectors also been identified in such environmental niches and what does this mean in terms of the interpretation of study findings?

5. Lines 105-111: I suggest these should not come in the introductory section of
the paper as they are a summary of key findings.

6. Figure 1: I suggest ‘malaria clinical status’ in the title be changed to ‘febrile illness’

Minor Essential Revisions

1. Line 23: Please re-word – do you mean clinical illness is common among parasitaemic pregnant women? Strictly speaking they all have malaria infection if they have detectable Pf parasitaemia.

2. Line 28: ‘Parasitemia’ is used here but ‘parasitaemia’ used above and below (e.g. line 33)

3. Line 76: Specific adverse consequences should be briefly described here.

4. Line 91: do you mean haemoglobin variations or anaemia?

5. Line 94: unclear how this is relevant to pregnant women.

6. Line 102: ‘risk factors associated with’ might be a better term to use rather than ‘risk factors that predispose’. For example, lower socio-economic status per se may not directly predispose women to increased risk but may inhibit access to treated bed nets.

7. Lines 117-118: suggest re-word by removing ‘starts in’ and replace with ‘runs from’ or similar wording to indicate duration rather than the month in which the dry / wet seasons start.

8. Line 123: ‘infective bites’ rather than ‘infectivity bites’

9. Line 146: reference to axillary temperature is unclear.

10. Line 201: Please provide ‘FCFA’ in full. I also suggest that you indicate in the methods or background section of the paper any locally-derived indicators of income level and how these compare to measures based on US or international dollars.

Discretionary Revisions

None

Level of interest: An article whose findings are important to those with closely related research interests

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