Reviewer's report

Title: Possible increased malaria transmission and susceptibility to clinical malaria episodes following treatment of Plasmodium falciparum asymptomatic carriers: Results of a cluster-randomized study of community-wide screening and treatment

Version: 1 Date: 22 May 2013

Reviewer: Roly Gosling

Reviewer's report:

Overall comments

The authors present a well written and concise account of a secondary analysis of a trial published earlier this year (Tiono et al Mal J 2013). This report focuses on the follow up period post intervention (3 rounds of RDT mass screening and treatment with ALU for positives) observing for incident cases of symptomatic cases of malaria with parastemia > 5000/ microliter. In addition they report on the entomology of the study and gametocyte prevalence at time of diagnosis. It is unclear why these are grouped together. There is no rationale explaining why these disparate studies are grouped together. The methods section is weak with some clear omissions, for example, how gametocytes were measured and more majorly analytical methods. Data were explored very simply and need major input by a statistician. There seems little accounting for the cluster design. Results are sparse.

Major compulsory revisions:

1. Rationale for including incidence in the follow up post intervention (rebound) period being published with gametocyte and entomology data.
2. Details of how gametocyte density was measured in the Methods Section.
3. Detailed statistical methods in the Methods section.
4. There is a clear need for more data analysis. The trial was a cluster randomized trial. It is unclear if the survival analyses was adjusted for confounders, interactions and clustering. If not, they should be. It is possible to adjust for age, negating the need for 3 graphs looking at the same outcome. In the results we are given a P value but no hazards. It is also normal practice to include a table of the numbers at risk at various time points below the x axis. The effect of clustering may have major impact on the results.
5. Staying on the statistical analysis, I am unsure why the authors have just used 2 age categories, why not use age as a continuous variable. The authors lose statistical power by categorization. It may be that there is a justification for their categories but it is not stated. In addition we are given geometric parasite means but none of these look statistically different- yet there is no comment to this in the text and within the age groups there may be differences in ages- why not adjust
for age in the analysis.

6. The same can be said with gametocyte density/ prevalence at diagnosis- there are statements of the difference but no test to show that they are different and what is the effect of age and clustering.

7. Discussion – Cannot comment on the discussion until there has been a suitable analysis.

8. Conclusion seems dangerous- Screening and treatment increase malaria transmission. I don’t see the evidence for this statement. The authors show a weak statistical analysis that shows increase in high parasitemia associated with clinical symptoms, a reduced gametocyte burden (what ever that means) in clinical cases, and some entomology that does not really persuade me one way or the other.

Minor comments

1. Discussion: the authors spend much time in the second paragraph discussing rebound – but they only demonstrate a non-significant trend. They also allude to the fact that in the Mal J paper the authors show no difference between the groups over the whole intervention + follow up period. Does this mean that clinical episodes are just postponed?

2. Authors report “biting rates” – these are a result – why are they only in the discussion.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare I have no competing interests