Reviewer’s report

Title: Active Trachoma Two Years After Three Rounds of Azithromycin Mass Treatment in Cheha District, Gurage Zone, Southern Ethiopia

Version: 3 Date: 17 October 2013

Reviewer: Jonathan King

Reviewer’s report:

Major comments:
1. The authors should remove the word “infection” from the first sentence of the discussion as they have not assessed infection, only clinical signs as is recommended by WHO to measure program impact.

2. The authors should clarify the result “children of age 3 years being most affected (50.0%). The data in Figure 1 do not support this result, nor in table 1. It could be interpreted as the prevalence of trachoma but this is not shown in any table or figure.

3. Table 1 – please clarify what versus means and what the chi-square is testing. If the authors are trying to determine associations between active trachoma and the variables listed in the first column then the analysis should be expanded to include non-cases and also report that the analysis is unadjusted for potential confounders or effect modifiers. What is currently presented includes only active trachoma cases (n=175).

4. The authors should provide a measure of association to support figure 2 and thus the authors’ interpretation “The chance of getting active trachoma was lower for children who had received azithromycin three times than twice or once.” The chi-square in table 1 does not test such association (only among TF cases).

5. Similarly, the p-value 0.002 does not measure the statistical difference between TF prevalence among different ages. The closest evidence to support the claim would be the age and sex specific prevalence plot of Figure 1 where it seems that children 3-6 had the highest TF prevalence. The authors should reword their interpretation in the discussion (avoid using the p-value in table 1) unless they adjust their analysis appropriately.

6. On what basis can the authors conclude that azithromycin distributions have reduced active trachoma? What are the alternative hypotheses? Could the observed minimal decline be due to the joint impact of the SAFE strategy?

Minor revisions:
1. The authors should consider removing the word “Trend” in all figures unless there was a trend analysis performed providing appropriate statistics. The authors could simply state – “Age and sex specific prevalence of active trachoma in children aged 1-9…” for figure 1 and likewise for figure 2 “Age-specific prevalence of active trachoma by azithromycin treatment history…” or similar
2. In the methods section of the abstract, the authors could simplify the description and state “A multistage stratified cluster random survey was employed to determine prevalence of active trachoma among children aged 1 to 9.”

3. The data suggest that uptake of AFE activities should be improved. In the conclusions of the abstract, the authors may want to state that all those improvements mentioned are the other components of the SAFE strategy.

4. The authors should consider referencing the methodology of compact segment sampling (perhaps Turner et al or other)

5. The authors should clarify what is meant by p-value of <0.5 being considered as statistically significant association. I believe the authors mean that statistical significance was determined by p-values of <0.5.

6. The limitations of the study should be included in the discussion rather than in the methods section.

7. The authors may want to define safe and clean water, simply as “improved water source” consistent with WASH core variables.

8. The authors should avoid interpretation in the results (i.e. section starting with “scary for use by children” and continuing to the end of the paragraph). If included these observations should be mentioned in the discussion.