Reviewer’s report

Title: Lymphatic filariasis control in Tanzania: Effect of six rounds of mass drug administration with ivermectin and albendazole on infection and transmission

Version: 2 Date: 28 May 2013

Reviewer: Patricia Graves

Reviewer’s report:

The authors have responded well to the comments and revisions for this valuable and comprehensive paper. I just have a few minor remaining comments

Minor essential revisions

1. Please number the schools in text according to the map when you refer to them in Methods, Study Sites, page 5.

2. Thanks for including the map and new Table 1. These are very helpful. However Table 1 needs an additional column showing the village and hamlet names included in the community surveys. For example, Survey 1-6 included Kirare all 4 hamlets, whereas 7 and 8 included other villages and hamlets, and went down to 2 hamlets in Kirare. It would be helpful to also note in Table 1 when the new census was done and when scale up of net distribution occurred. These points may seem obvious to the authors who have worked in this area for so long, but the sampling frames and timelines need clarification for those new to this study area. You may be able to cut some of the text at bottom of page 5 and top of page 6, and refer to the Table 1 instead.

3. Re comment 2. I appreciate the sample sizes in Table 2, but you did not answer about the statistical significance, in Text Results Para 1. Instead of ‘no major differences’ please state ‘no statistical significance’ if that was the case and give p values. According to my estimate p= 0.068 for hydrocoele and p=0.226 for elephantiasis by one sided Fisher’s exact test; I did not check the others.

4. Re comment 3, thank you for including the sample size and more details about coverage and net surveys. The phrase “number of nets relative to individuals’ followed by a percentage is not clear and rather ungrammatical. If you cannot include information on who had access to nets in their households, this is a non-standard net coverage measure; please rephrase perhaps as a ‘ratio’.

5. The fact that there was no increase in net coverage until 2011 (and even then only just before the last survey) is useful for your study as it demonstrates the impact of MDA alone on both human and mosquito infection rates as well as perhaps on vector species composition. Please mention this important point in the discussion or conclusion.
6. In relation to the change in diagnostic tests over time mentioned by the other reviewer, I find one part of the discussion a little contradictory. You say on page 16, near top (discussion para 3) that there was a need to change to ‘more sensitive diagnostic techniques and procedures’ and reference Weil (ICT). I can understand that in relation to Mf; however you had been doing Og4C3, which is of equivalent (or perhaps greater) sensitivity compared to ICT. Then at bottom of same page, you say that Og4C3 was sensitive and specific, and that you had problem detecting a decrease in CFA for this reason, but this not very convincing as a reason to switch to ICT (should be easier to detect a difference when overall prevalence is higher, no?) Then you argue at top of page 2 that Og4C3 and ICT have been shown to be equivalent in sensitivity. Overall, I think you are trying too hard to argue why you changed tests over time. Tests evolve, Mf prevalence declines and becomes more difficult to detect, things change for reasons of convenience and efficiency, etc. I think the cautionary statement you have added in response to the other reviewer is fine, and you should cut out arguments based on sensitivity or at least make them consistent.

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:

I declare that I have no competing interests