Reviewer’s report

Title: Parasitological impact of two-year preventive chemotherapy on schistosomiasis and soil-transmitted helminthiasis in Uganda

Version: 1 Date: 2 July 2007

Reviewer: Andrew Roddam

Reviewer’s report:

General

This is a very nice manuscript providing some convincing epidemiological data on the short-term impact of control programmes for schistosomiasis and helminthiasis. I have only a few minor comments on the analysis/presentation of results.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The authors use arithmetic mean egg counts and perform their analyses on the original scales. However it is clear that the egg distribution is clearly non-normal with a large peak at 0 and a very skew distribution for positive values. The authors need to therefore justify their use of techniques such as analysis of variance which assume normality of the response or alternatively to use an appropriate transformation/alternative technique which can be justified.

2. As stated there are 4351 schoolchildren recruited at baseline but only 1704 have complete follow-up data which is used in the analysis. Whilst I appreciate the difficulties of following up children and adults in resource-limited environments I think it would be helpful to readers and provide more support for the results if there is some comparison between those who are followed-up and those who are not. For example, I was left wondering if there were differences in some key variables – like infection intensity – between those followed-up and those lost.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. At a number of points in the manuscript it is stated that results were significantly different for a number of groups but there is no further details – please give details of the tests used in the methods section and provide p-values in the results section. Similarly for Figure 3.

2. In all the results tables there are no p-values for tests of reduction/significance nor indications of whether the 1 year follow up is different form the 2 year follow
up. Although I appreciate that there are some clear and large reductions I still think it would be helpful to the reader if relevant p-values were also presented.

3. The authors regularly say “unadjusted reduction of …” which had me waiting to see what the “adjusted reduction…” was. However this was never presented. I think the authors need to choose a different description and justify why no adjustments were presented.

4. I found the continual repetition of large portions of the results tables in the text unnecessary and distracting. The results section should be used to bring out the key highlights from the results, not to repeat what is presented in a table – readers should just be referred there.

Discretionary Revisions (which the author can choose to ignore)

Which journal?: Appropriate or potentially appropriate for BMC Medicine: an article of importance in its field

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests