Reviewer's report

Title: A national survey of the prevalence of schistosomiasis and soil transmitted helminths in Malawi

Version: 1 Date: 4 June 2004

Reviewer: Dirk Engels

Reviewer's report:

General:

This article reports the results of a national survey to evaluate the current public health importance of both schistosomiasis and soil-transmitted helminths in Malawi. Compared to a previous version of this paper - which I reviewed earlier - this manuscript represents a major improvement. The results of the (low cost national) survey - with its unavoidable limitations - are now adequately compared with historical data and discussed in an operational perspective. I nevertheless recommend that a number of corrections, additions and clarifications be added to the manuscript in order to further improve it. These are all minor essential or discretionary revisions.

- In the M&M section, it is stated that infection intensity for STH's was calculated as the geometric mean. I could not spot geometric means in the results, only prevalences of heavy intensity STH. It should be defined in M&M section what exactly is meant by heavy intensity infection.
- The statement in the last paragraph of the "Results" section: "Only the Southern lowlands zone has a ...". First of all, according to the WHO Expert Committee report, Community category II for schistosomiasis is defined as "moderate prevalence" (between 10% and 50% infected according to TRS 912, page 34, or p.27 in "Helminth control in school-age children: a guide for managers of control programmes", which is the updated version of reference 10 - see www.who.int/wormcontrol). The statement sounds as if the authors are reassured by their statistics on the fact that in the other ecological zones no moderate or high prevalence schistosomiasis spots would exist. That can, of course, not be guaranteed by the use of upper confidence intervals. I would therefore strongly suggest to omit this paragraph. The authors draw the right conclusions subsequently in the discussion section, and therefore this statement is confusing.
- Discussion section - first paragraph: I agree with the authors that the higher overall prevalence estimated previously is probably due to the surveying of selected populations. However, if that assumption is made, one logically cannot state that "infections are not in general common any more". One can only say "the present survey has shown that they are not common in general". Similar to this - I would like to suggest to change the last sentence to: "S. mansoni does not seem to be a generalized problem in any particular ecological area in the country". It can indeed not be excluded that it is a problem in some sites (cfr. the 42% maximum prevalence reported by Randall et al. in his survey in Karonga).
- The structure of the discussion section merits to be (slightly) improved -
for instance the selection bias in historical studies (page 14) was already discussed in the beginning of the section (page 12).

- The argument of previous drug treatment would be much stronger if some figures would be included in the paper, both for schistosomiasis and STH. As a student's thesis seem to exist on this issue, there should be some figures available.

- Could the successive cycles of drought in Malawi have influenced transmission (of STH at least) ? This argument was not discussed.

- One of the main conclusion of this paper is that, particularly for schistosomiasis, the distribution is focal and the most appropriate way to proceed further is to through decentralised (district) surveys and management. The questionnaire approach has been specifically designed to identify hot spots and should indeed be used. However, this methodology has been extensively validated for specific purposes and therefore the calculation of the "local" sensitivity and specificity is not very relevant. Basing such a calculation on (a single) urine filtration result (as a gold standard) is incomplete as this latter method has also its limitations.

- As already stated previously, the implications for a control programme merit to be updated in view of more recent WHO documents than those mentioned in the bibliography - see www.who.int/wormcontrol.

- The value of the paper would further be enhanced if the authors would also make operational suggestions with regard to intestinal schistosomiasis.

- Lastly, it is a pity to use an S. japonicum paper as a reference with regard to variations in faecal egg counts, when a wealth of literature is available on this issue in S. mansoni infections (reference 15).

When assessing the work, I have considered the points hereafter and my answers are as follows:

1. Is the question posed by the authors new and well defined? Yes
2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? Yes (provided some of my remarks are dealt with)
3. Are the data sound and well controlled? Yes
4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes
5. Are the discussion and conclusions well balanced and adequately supported by the data? Yes (see my remarks)
6. Do the title and abstract accurately convey what has been found? Yes
7. Is the writing acceptable? Yes

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No
Declaration of competing interests:

None