Reviewer's report

Title: Integrated monitoring and evaluation and environmental risk factors for urinary schistosomiasis and active trachoma in Burkina Faso before preventative chemotherapy using sentinel sites

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Reviewer: Tony Fulford

Reviewer's report:

The statistics in this latest version of this paper are much better and more clearly described. I have no problem with this now.

I do still struggle with the point of the paper. It claims to be about assessing the feasibility of “integrated monitoring” of urinary schistosomiasis and trachoma but we little of the analysis seems to address this question. Is it enough to state that the two can be surveyed simultaneously? (Was that ever in question?) I would have thought it more pertinent to have assessed the efficiency of the method and to have compared it to others. There is no clear mention of the alternatives against which it is being judged.

Four benefits of the approach used are listed early in the Discussion, but none of these are based on the analysis of the data in this study. The Conclusion draws attention to the fact that few similar studies have been conducted before (is that really a conclusion?) and goes on to state that (i) co-infection is rare and (ii) the current methodology doesn’t access younger ages (wouldn’t this have been known before the study started). But we don’t seem to get any assessment of how these relate to integrated monitoring. There are other findings not mentioned in the conclusion, such as the observation that the infections are essentially independent (conditional on environmental factors) but, again, this is not interpreted in terms of the feasibility of the methodology. This finding is itself complicated by others relating each infection to different environmental factors. Many of these factors are associated with infections but only one or two picked up by the stepwise process used in the model building. This tells me that the environmental factors are probably themselves highly correlated so that (a) the infections may not be independent marginally, i.e. without conditioning on environmental factors; and (b) confounding between the environmental factors would mean that the stepwise procedure may not have resulted in a very meaningful model, i.e. that the data collected here were not particularly useful for identifying environment factors.

Although the authors seem to be telling us that they study confirms that utility of this methodology, they undermine this by telling us that (a) they would not want to replace existing trachoma surveillance systems and (b) the age range is not appropriate. I agree with the authors that more work would certainly be required in order to extrapolate these findings (such as they are) to other West African settings. But, if these results are not generalisable, what purpose do they serve?
A lot of effort has gone into the analysis of these data. If it is to be published I feel there needs to be a greater connection between the results and the policy recommendations. As things stands one seems to be just excuse for the other.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests