Author's response to reviews

Title: A cross sectional study of Schistosoma japonicum prevalence in 50 villages of Samar Province, the Philippines.

Authors:

Mushfiqur R Tarafder (mushfiq-tarafder@ouhsc.edu)
Ernesto Balolong Jr (earnestdon@yahoo.com)
Helene Carabin (helene-carabin@ouhsc.edu)
Patrick Belisle (pbelisle@epimgh.mcgill.ca)
Veronica Tallo (vltallo@pacific.net.ph)
Lawrence Joseph (lawrence.joseph@mcgill.ca)
Portia Alday (palday@ritm.gov.ph)
Ryan O’NEIL Gonzales (contagion34@yahoo.com)
Steven Riley (steven.riley@hku.hk)
Remigio Olveda (rolveda@ritm.gov.ph)
Stephen T McGarvey (stephen_mcgarvey@brown.edu)

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Author's response to reviews:

Dear Editorial Team,

Thank you for the thorough review of our paper. We have carefully considered the comments of the reviewers in your email of December 20, 2005 and have revised the manuscript accordingly. We have been very careful in answering the major comments of Dr Spear and have made sure to cover all minor comments for the 3 reviewers. We think that our description of the variability in the prevalence of intensity of infection from village to village is of interest and that the description of the association between ecological factors and infection should not be included here as it was not the focus of this manuscript.

The material in this manuscript has not and will not be offered elsewhere for possible publication, as long as it is under the BMC Public Health consideration. All authors have seen and approved the content of the revised manuscript and have contributed significantly to the work.

We hope that the above improvements to the manuscript will be satisfactory. We look forward to hearing from you.

Yours sincerely,
Helene Carabin, DVM MSc PhD

Manuscript submitted to BMC Public Health: A cross sectional study of Schistosoma japonicum prevalence in 50 villages of Samar Province, the Philippines.

Specific answers to the reviewers' comments:

Responses to reviewer: Juerg Utzinger

Major compulsory revisions: None required

Minor essential revisions

(1) Title: We have modified the title to: "A cross-sectional study of the prevalence of intensity of infection with Schistosoma japonicum in 50 villages with variable types of water management systems in Samar Province, the Philippines".

(2) Gender vs sex: Thank you to the reviewer for this point. We have modified the manuscript to use the term sex throughout.

(3) Study area: we have added a map depicting the location of all study villages with their type of irrigation.

(4) Description of population sample in tabular form: We have added a descriptive Table of our study population. We do not provide p-values for their differences as the goal of this manuscript is to describe the village-to-village variation in prevalence of intensity of infection and not to test the effect of irrigation on the prevalence.

(5) Percentages: we have changed all percentages to having only one decimal.

(6) Random selection of households: we actually used this cluster sampling method because we were expecting individual prevalences to cluster within households and household prevalences to cluster by village. We originally wanted to sample 6 people per household but had to reduce it to 5 for eligibility reasons (too few households would have had 6 people eligible for sampling). We used this cluster sampling approach because 1) we wanted to be able to control for clustering and 2) we wanted to be certain that there was at least one rice farmer in each HH representing the irrigation status of the village (i.e. rice farmer working in an irrigated farm irrigated villages) in addition to a representation of all other age groups. The clustering by household was very likely since we were told that wives and children would often help their husbands/father in the rice fields. This fact would also help our contrast in exposure between the irrigated and rain-fed villages. Another reason that we felt that there would be clustering by household is because it is likely that members from the same households would bath and wash themselves in similar area and would have the same source of drinking water. Taking a completely random sample would have run into the risk of having some villages with people from numerous households and some others with several people from the same household, all of this dependent on the village size. Some of these villages have no more than about 40 or 50 households total and some others have up to 400. Therefore, in some villages, some individuals could have been more independent from one another than in some others, leading to even more complex statistical challenges.

(7) Number of people examined per household: we thank the reviewer for having noticed this mistake. The number was at least five individuals per household. We have corrected the text accordingly.

(8) Occupation: The category "working on a farm but never on a rice farm" includes individuals who had
clearly declared non-rice farming as their usual occupation and had declared not having worked in a rice farm in the past 9 months. The category "not working on a rice farm but may be working on another type of farm" were individuals going to school (more than 10 years old) who said that they did any chores their parents asked them to perform or who did part time jobs when not at school and who also declared not having worked on a rice farm in the past 9 months. It also included non students who did not mention their usual occupation and declared that they had not worked on a rice farm in the past 9 months. This later group of people are hence individuals for whom it was uncertain if they worked on non rice farms but for whom we knew that they did not work on rice farms. We have clarified this in the methods section.

Discretionary revisions

= Page 1, line 1: DONE
= Page 1, lines 1-2: DONE
= Page 1, lines 2 and 5: DONE

= Page 3, line 4: We replaced it with "prevalence of two levels of infection intensity" which better reflects what we have actually done
= Page 3, line 12: DONE
= Page 3, line 16: DONE
= Page 3, line 19: DONE
= Page 4, last line: DONE

= Page 5, line 2: Thank you for the comment and for the correction! DONE
= Page 5, line 12: This format is the default format that we get in reference manager using the default format required by BMC PH.
= Page 5, line 17:

= Page 5, last line: DONE

= Page 6, lines 1-2: DONE

= Page 7, line 10: We replaced pvc by Poly(vinyl chloride)
= Page 8, line 12: Children less than 18 but old enough to be able to judge if they want to participate in research projects are usually asked for their "assent" to participate. Their parents also need to sign an informed consent for their participation. This is common jargon for ethical considerations in the United States.
= Page 8, line 17: DONE
= Page 8, line 18: It was called "Danish Bilharziasis Laboratory" at the time of ethical approval and we have therefore decided to leave it as is.
= Page 8, line 20: DONE
= Page 9, line 4: DONE
= Page 9, lines 9-10: We define our classification here and do not feel the need to specify a reference.
= Page 10, lines 5-6: DONE
= Page 10, lines 8-9: We decided to keep the name of our research group in the text.
Responses to reviewer: Charles King

To address the general report of the reviewer, we have added a paragraph at the end of the discussion summarising the impact of our findings. This also addresses the general comments from the previous reviewer.

Major compulsory revisions: None required

Minor essential revisions

Page 4: We agree that this sentence was not clear and we have rewritten it.
Selection of study subjects: we agree with Dr King that the purpose of the study should be more clearly stated. We have added at the beginning of the methods section a short paragraph explaining the overall objective of the study. We have also provided explanations to Dr Utzinger's questions on why we chose 35 households per village and 5 individuals per households.

Page 9: Our analysis was unweighted, but we used a hierarchical model, so, each village estimate did indeed take into account the sample size from each village. So, while similar in concept to weighting, the estimates would be a bit different, since in a hierarchical model some of the between village variance is attributed to random error (especially in small villages), and so these estimates are pulled in towards to overall mean. We did not make any changes in the paper regarding this.

Responses to reviewer: Robert Spear

Comments in paragraph 1

The first comment of Dr Spear is that we had left out the variable irrigation from our analysis. The variable irrigation was and is still provided in Table 3 (now table 4). We have added a paragraph at the beginning of the methods section to emphasize the fact that this paper is a sub-study within a larger project that will look into the variables mentioned by the reviewer. We are currently analysing the GIS data but this was not the objective of this particular paper. We are also now analysing factors that influence the presence of the intermediate hosts. Talking about the irrigation beyond what we have already mentioned and the additions made following the comments of the other two reviewers is not relevant for this particular paper, as it was not the primary scope. We had already mentioned that in our manuscript in the discussion. We have nonetheless added a comment in the last paragraph on the snails.

The reviewer is asking for more description of the agriculture in these villages. All these villages are rice farming villages and are very rural. We have added a sentence in the methods sentence to clarify. Table 1 and its description in the results section, which was added following the recommendations of Dr Utzinger, should also help better understanding these communities. We would nonetheless want to add that we have adjusted for the major occupation of the participants which is likely to adjust for at least part of the water contact behaviors.

Comments in paragraph 2

We would like to emphasize again that the objective of this paper was to present the village-to-village variability in the prevalence of 2 categories of infection. The reviewer will have noticed that even with the adjusted results, the prevalence of at least moderate infection was zero in 14 villages. This means that in 14 villages, no "tail" existed. As for the other villages, there were a small number of individuals (68) with heavy infection (more than 400 epg) and even fewer individuals had 800 epg or more (30). This is why we have combined the categories of those with heavy and moderate infections. We have added some details about this in the methods section. As for defining lightly and at least moderately infected individuals, we have use the standard WHO classification of intensity of infection (Montresor et al., 1998).

Comments in paragraph 3

1. We do not agree that we should write out the formulae for our entire model. This would take much space and add little, as we already have a much more user friendly (for this audience) text version that fully describes each level of our model. We have nonetheless added a note in the methods section stating that we will make the WinBUGS code available to any interested researchers.

2. Only through a Bayesian approach could we simultaneously adjust for the imperfect testing results (epg categories) and run a random effects model. In other words, our model CANNOT be represented as a
"straightforward random effects approach" as the reviewer suggests.

3. We have removed the word "true".

4. The reviewer writes: "4. Take out the part on post-hoc adjusting for misclassification (MIN). Why assume that here - can't one assume the eggs are measured without error? The model already accounts for the fact that the true mean egg count (for a infinite stool sample) is not known"

We do not understand the reviewer's comment here. We think that it may be relating to comments in paragraph 3, point 2, that we address above.

We have conducted a very thorough pilot study (manuscript under review) on evaluating the best method to measure water contact. In that study, we realized that water contact measures were extremely variable from day to day and highly unreliable. In a study of the scope of ours, we would have had to use a diary or questionnaire approach to measure water contact. Our pilot study clearly shows that asking people about their water contacts in the past day or 2 weeks is not valid. This is why it was not included. In addition, our measure of occupation clearly shows that the rough irrigation classification does not impact what people do in villages.

Reference