Author's response to reviews

Title: High burden of hepatitis B infection in Northern Uganda: results of a population-based survey.

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Author's response to reviews: see over
April 11\textsuperscript{th}, 2013

The Editor,
BMC Public Health

Re: Response to reviewers’ comments on manuscript MS: 1152278792749821

We thank the editors and reviewers for their helpful comments on our manuscript entitled \textit{High burden of hepatitis B infection in Northern Uganda despite nine years of childhood hepatitis B vaccination: results from a population based survey}.

Please find below our responses in plain text and the reviewer’s comments in bold. The revised manuscript with revisions made highlighted for clarity is also attached.

We have also made the required changes from the editorial office namely;

- Changed the title 'Introduction' to 'Background'.
- Changed the title 'Patients and methods' to 'Methods'.
- Provided the ‘Conclusions’ heading before the Competing Interest.

**Referee #1**

**Reviewer’s report**

\textbf{Title}: High burden of Hepatitis B in Northern Uganda despite 9 years of childhood hepatitis B vaccination: results of a population based survey.

\textbf{Version}: 2 \textbf{Date}: 10 February 2013

\textbf{Reviewer}: Elizabetta Franco

\textbf{Reviewer’s report}: 
The Authors accepted almost all the suggestions of the reviewers, making the appropriate changes and now the paper can be published. I would suggest to change the title of the article, infact the paper describes the present situation of HBV in Uganda before an effect of the vaccination program can be seen.

Thank you for this comment. We have changed the title to 'High burden of hepatitis B infection in Northern Uganda: results of a population-based survey'

If data is available, it might be useful to show the vaccination coverage in Northern Uganda: from the figures of the present study, infact it seems to be not higher than 30%.

Unfortunately, there are no other data on vaccine coverage in the Northern Uganda. This, in effect, is another contribution of our publication

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable
Statistical review: No. the article does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests’ in relation to this paper

Referee #3

Reviewer’s report

Title: High burden of Hepatitis B in Northern Uganda despite 9 years of childhood hepatitis B vaccination: results of a population based survey.

Version: 2 Date: 26 February 2013

Reviewer: Paul Hewson

Reviewer’s report:

Major compulsory revisions

To my mind, various issues relating to the science of Hep B have been better dealt by the other referees who have more subject matter knowledge than I have. Therefore my two concerns are (a) the gender mismatch and (b) the small numbers in key groups on which prevalence estimates are based.

I appreciate that this is a report of a substantial piece of work and accept why we have a gender mismatch. However, I think this needs to be carefully acknowledged in the abstract-
in the patients and methods section. You do mention that your target population is the ‘at home during the day’ population- I think this should be clearly set out in the abstract for the benefit of systematic reviewers and so on.

We now better clarify this point as requested by including in the abstract and elsewhere that the sample included person at home, which produces a gender imbalance for inference.

Secondly, I think the actual numbers (numerator/denominator) need to be quoted in the paper in conjunction with the percentages.

The actual numbers have been included in the texts in conjunction with the percentages.

Thirdly, I do wonder whether we have clustered sampling here, or entirely random sampling.

Unfortunately, we do not have the data that would enable adjustment for clustering. We acknowledge this in the Discussion section limitations.

For what it’s worth, I entirely agree you should retain none-statistically significant predictors when calculating adjusted odd ratios- again the sample sizes are may be too small for statistical significance to be declared but the odds ratios still get adjusted for the partial effects of these theoretically important predictors. What I don’t understand is why you don’t use logistic regression here to produce adjusted O.R.s (although I appreciate you don’t have many positive responses on which to build these models)

We feel we are on the same page; we did in fact conduct logistic regression to produce ORs with the candidate variables. However, so few were significant (note that in 3 of the four models only a single variable is significant, in the 4th only 2 variables) and did not differ in the bivariate and multivariate (no real evidence of partial effects). Therefore, showing all the still non-significant adjusted ORs makes the tables very unwieldy (doubling the figures) without providing any further information than is already there.

Discretionary revisions

I think the English is acceptable, but could be improved slightly which could help other people read and refer to the paper.

Level of interest: An article whose findings are important to those with closely related research interests.

Quality of written English: Needs some language corrections before being published.

Document has been read and some editing done

Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I have no competing interest.

Thank you very much as we await your response

Yours sincerely;

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