Author's response to reviews

Title: Prevalence and determinants of Campylobacter infection among under five children with acute watery diarrhea in Mwanza, North Tanzania

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Author's response to reviews: see over
Dear EIC Archive of Public Health

Response to reviewer comments

Title: Prevalence and predictors of Campylobacter infection among underfive children with acute watery diarrhea in Mwanza, North Tanzania
Version: 5 Date: 13 November 2013

Reviewer: Catharina Matheï
Reviewer's report:
Dear,
I thank the authors for appropriately addressing most of my comments. However there are still some pending issues.

Abstract
Reviewer: I missed this probably in my first review, but the authors describe their study design as “prospective cross sectional”, which is a contradiction in terminis. It should be a “cross sectional study”.
Authors: Amended as per reviewer suggestion.

Methods
Reviewer: I would describe the limitations of the study in the discussion section. The authors should also explicitly mention the fact that they do not know what is the proportion of eligible children that eventually participated in the study. Since they have no idea about the characteristics of these children and the reasons of non-participation this might be a source of bias. Figure 1 is in this respect misleading since it suggests that all eligible children participated in the study.
Authors: The limitation has been moved to discussion and modified accordingly to include suggested limitation page 11. Figure one has been deleted to avoid confusion most of the results have been given in the text and tables.

Results
Reviewer: Page 7 line 56: it should be: “a significant higher number of “
Authors: Amended accordingly

Discussion
Reviewer: Page 10 lines 217-22. I'm not convinced about this explanation. A bit earlier in the discussion the authors mention that their findings suggest that at least in the Mwanza region the prevalence hasn’t much changed. This doesn’t support an improved socio-economic situation. For the other region the lower prevalence is attributed to the higher use of antibiotics. However, this is not supported by the results of the multivariate analysis, which shows that antibiotic use is not independently associated with Campylobacter infection. How do the authors explain this?
Authors: Agree with comments, the reason has been deleted and the sentence modified accordingly. Antibiotic use was done later as sub-analysis in table 3 showing its influence in each hospital. As a single factor in the sub-analysis.
Reviewer: The study is a cross-sectional study. Therefore the authors shouldn’t use the term predictors. The terms risk factors or determinants are more appropriate.

Authors: Amended accordingly

Page 10-11, lines 233-41. The results of this study do not support any association between living in the proximity of cattle and/or consumption of unboiled water and Campylobacter infection. What about the relationship between Campylobacter infection and malaria?

Authors: Agree with the comment; the data does not significantly support but the findings show an association which is not statistically significant this has been commented in the discussion. Association between malaria and campylobacter infection has been commented page 10.

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

Authors: Corrected

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

Declaration of competing interests: I declare that I have no competing interests

Thanks

Prof Stephen Mshana

On behalf of all authors