Author's response to reviews

Title: Prevalence and predictors 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

Reviewer's report
Title: Prevalence and predictors of Campylobacter infection among under-five children with acute watery diarrhea in Mwanza, North Tanzania
Version: 1 Date: 25 September 2013
Reviewer: Catharina Matheï

Reviewer's report:
This paper describes a study investigating the prevalence of Campylobacter infection among under-five children presenting with acute watery diarrhea and its determinants. The study is relevant, though probably only for a limited public, and the results are presented in a clear and concise manner.

However, I have some issues that should be addressed before this paper can be considered for publication. Especially the discussion section needs an elaborate revision.

Abstract
Reviewer: In the results the association between age and campylobacter infection is not mentioned.
Author: Agreed with the comment and information has been provided in the abstract: line 49-50, page 2

Introduction
Reviewer: In the third paragraph the authors write “Poor hygiene, sanitation…, and this is responsible for sporadic cases in these countries” The “sporadic cases” seems contradictory to the high prevalence rates mentioned in the previous paragraphs.
Authors: Agree with the comment and the sentence has been rephrased line 74, page 3

Methods
Reviewer: The authors refer to the definition of acute diarrhea as defined by the WHO. I suggest including this definition in the methods section.
Authors: Agree with comment the definition has been provided and the link given page 4, line 89-91

Results
Reviewer: Did all children meeting the inclusion criteria participated in the study? This seems very unlikely to me. The authors should provide the non-response rate and should compare the non-responders with responders in terms of study population characteristics.
Authors: Agree with reviewer details has been given page 6 line 130-135
Authors: We agree with the reviewer comments some of children refused to participate but the recruitment was serially and those children who refused to participate their information were not asked. Page 4 line 94&95

Reviewer: Table 2 seems to contain some flaws:o 95% CI for association between malaria and campylobacter is incorrect o p-value for association between access to well water and campylobacter infection is also incorrect

Authors: Amended accordingly page 16

Discussion

Reviewer: In the section “The prevalence of Campylobacter infection” the authors report that “At the BMC significantly higher prevalence of Campylobacter infections were seen in those children who had not used antibiotics compared to those who had taken antibiotics.” Was this not the case in the Sekou Hospital and if not can the authors explain why?

Authors: Agree with the comment, this was not the case in Sekou Toure details have been given in result section page 8 and table 3. Discussion has been modified page 9 lines 169-175

Reviewer: I think the authors’ statement that the magnitude of Campylobacter infection has remained stable in their area based on the observed prevalence in the Sekou hospital, which is comparable with previous findings, is a bit to strong. It at the most suggests it.

Authors: The statement has been rephrased page 9 &10 line 212-214

Reviewer: The authors write “Higher prevalence rates of Campylobacter infection have been observed in children below the age the age of 24 months, contrary to the findings of our study which showed higher infection rates in children above 2 years of age.” I would change “children” by “children with acute watery diarrhea” since the absolute number of Campylobacter infections is still higher among children below the age of 2 compared to children above the age of two.

Authors: Amended as per reviewer recommendations page 10 line 215

Reviewer: The authors should explain a bit more into detail why the children in their study are less likely to be exposed to the source of infection before they are old enough to move around on their own than the children participating in previous studies performed in Tanzania.

Authors: Agree with reviewer comments and more details have been given page 10 217-224

Reviewer: Under the section “Predictors of Campylobacter infections” the authors state that found an association between living next to cows and Campylobacter infection. However, this association was only borderline significant in the bivariate analysis. The sentence “Several studies have shown that in developing countries household exposure to the feces of live chickens…” is superfluous because a rehearsal of the previous.

Authors: Agree with comments and sentences have been rephrased and some deleted
Reviewer: The sentence “in this study the use of unboiled water was 1.5 times more risky to acquire campylobacteriosis than the use of boiled water” may apply to another study but it certainly doesn’t fit in with the results of this study.
Authors: In table 2 the risk of acquiring campylobacter infection those using unboiled water is 1.5 times more though the difference is not statistically significant
Reviewer: What do the authors mean by the last sentence: “… by the endemicity of both disease in our setting so displaying co-founding effect”?
Authors: Agree and the sentence has been rephrased; this is not confounding effect because the confounder should be present in children with campylobacter and those with Malaria. These diseases are present with high prevalence in the study area
Authors recommend multicentre study to determine the association

Reviewer: The authors should include a section about the strengths and limits of their study.
Authors: The section has been included in page 6 line 140-144

Conclusion
Reviewer: I’m not sure whether the conclusion that further studies are needed to determine the species of Campylobacter and the susceptibility pattern of the isolate to guide appropriate antibiotic therapy, can be drawn from the results of this study.
Authors: With all due respect we agree with the comment but in this case we are recommending further studies in that area from limitation of this study; therefore it is not a conclusion of this study

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: The manuscript has been extensively edited
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests: I declare that I have no competing interests

Prof Stephen E. Mshana
On Behalf of Authors