Reviewer's report

Title: Risk factors for secondary transmission of Shigella infection within households: implications for current prevention policy

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Reviewer: Isabel Oliver

Reviewer's report:

This is a clear paper which answers a well defined question. The authors summarise the policy context well. All comments fall into teh category of minor essential revisions.

The methods are appropriate and well described but I have a number of comments:

The use of the term non-single in the definition of a high risk household is confusing. 'A single person' would usually mean someone not in a relationship in this case it presumably means someone who does not leave alone. I would suggest rephrasing to say 'a high risk household was defined as any household with more than one inhabitant including at least one child of <16 years..'

The authors do not explain the rationale for selecting a date of onset of symptoms of >1 day after the date of onset of symptoms of teh primary case, although they explain that using a period of >3 days would not change the results. It would be helpful to understand the rationale.

The data are sound and the response is very good. The statistical methods need further clarification. There is no power calculation presented, presumably this is because the sample was identified through routine surveillance and it included all cases in the relevant period, however , the number of secondary infections is fairly low and it is possible that the study has insufficient power to detect some differences. In page 7 the authors describe taht 13 cases were associated with one transmission and 6 cases with two. They do not describe if this was taken into account in the analysis or if they are any factors that may have been associated with more extensive transmission.

Was duration of symptoms and severity of symptoms in the primary cases considered as factors that may be associated with transmission?

Shigella secondary attack rates are usually considered to be high. Is it possible that the ascertainment of positive contacts may have been reduced due to the delay in notofocation of cases and that cultures may have been negative by then?

Although the losses to follow up are very small (6%) considering the need to submit a stool sample, it would be helpful to udnerstand from the authors if they
think that any bias may have arisen from differences between the contacts screened and those who were not.

The conclusions seem reasonable although it would be useful to get the comments from the authors about the questions posed above and further discussion is needed regarding the limitations of the study.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests