Reviewer's report

**Title:** Association between early bacterial carriage and otitis media in Aboriginal and non-Aboriginal children in a semi-arid area of Western Australia: a cohort study

**Version:** 2 **Date:** 26 November 2012

**Reviewer:** Kimberly Shea

**Reviewer's report:**

Sun et al. have submitted a substantially improved revised manuscript. Upon review of the revised draft, there are several issues that remain to be addressed.

**Major Compulsory Revisions**

1. In their revision, the authors state “This study provides further evidence that early onset of bacterial carriage, and specifically NTHi carriage, increases risk of OM during the first two years of life but that the risk is attenuated by environmental factors in Aboriginal children.” Unfortunately, this wording has the potential to be misinterpreted: Stating that environmental factors attenuate the risk may be interpreted as suggesting that certain environmental factors are protective. I think the authors are trying to say that controlling for environmental factors produces an attenuated ratio measure because the unadjusted measure is confounded by these factors. As such, it would be better to eliminate the last part of the statement above.

2. In their response, the authors state “To our knowledge no data are available to explain in biological terms why one pathogen is more likely to predispose to OM than another.” This is reasonable, but the manuscript would be strengthened if the authors acknowledged this in their discussion – perhaps by simply acknowledging that there are no current data/hypotheses to explain this phenomenon in order to communicate to the reader that the authors did in fact consider this rather than just leaving it out. This might even be presented as an area for future research.

**Discretionary Revisions**

1. Although the authors included a long list of potential confounders in their analyses, they failed to acknowledge the possibility of residual confounding in their results. It should not be assumed that the authors have captured every possible confounder that exists for their specific exposure-disease relationship.

2. It should be reiterated that the authors' use of 2 significant digits to report ORs is not really appropriate for this study given the sample size. First, there is no such thing as a “standard for reporting point estimates.” Second, although it is true that confidence intervals provide some information about the statistical precision around a point estimate, the authors have failed to appreciate the fact that their data are too sparse to discern between an observed OR equal to 3.86
(as in Table 1) and an OR equal to 3.87, for example, especially given the
unavoidable potential for bias (all observational studies experience some bias,
even if it is only non-differential misclassification). Given the relatively small
sample size in this study, reporting ORs (and the 95% CI around those
estimates) using 2 significant figures misrepresents the level of accuracy of the
observed estimates. Because there is no standard for reporting OR, this is a
judgment call that is up to the authors.

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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a
statistician.

**Declaration of competing interests:**

I declare that I have no competing interests