Reviewer's report

Title: Developmental delays and dental caries in Head Start children in Washington State: a pilot cross-sectional study and preliminary explanatory model

Version: 1 Date: 16 September 2013

Reviewer: Peter Robinson

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This is a pilot cross-sectional clinical study comparing dental caries and treatment experience amongst preschool children attending a programme for disadvantaged children. The primary hypothesis is that children with developmental delays experience a higher prevalence of caries. The conduct of the study is essentially sound, but the report could be written for an international readership. The results need more careful interpretation.

1. The abstract assumes readers will be familiar with colloquial phrases such as “Headstart”, “Individualized Education Program” and “Dental Home”. BMC Oral Health is an international journal so the manuscript needs to be revised accordingly.

2. dmfs is not a measure of caries prevalence, but of caries and treatment experience. The prevalence of caries and treatment experience is measured by the proportion with dmfs > 0.

3. There is an assumption throughout the manuscript that dental attendance will effectively prevent caries in preschool children. There are references in the discussion to evidence-based strategies, although the citations are to guidance rather than the evidence-base. The authors claim that targeted interventions improve the oral health of children at greatest risk, but their reference is for fissure sealants, and presumably not applicable to preschool children. What is the evidence that dental attendance reduces caries in preschoolers? What interventions do US dentists have to prevent caries in this age group? Only dietary advice and tooth brushing instruction (with fluoride toothpaste) I think, and there is no evidence that either of these things is effective. I am not arguing against dental attendance. However, pinning hopes in something that will not work means that we will be less likely to look for and find something effective. For instance, in other parts of the world, programmes where children have been mailed fluoride toothpaste on their birthday have been effective.

4. The description of the conceptual model in the methods section could be expanded slightly to provide more information and written in text rather than a list.

5. The phrase “dental home” is used three times before it is defined. Even after I had read it in the methods section I wasn’t sure what it meant, although the explanation in the discussion is clear.
6. Where there any sealed surfaces in preschoolers’ teeth?

7. Perhaps I am being picky, but starting sentences with “About” looks a bit relaxed in a the results section of a scientific journal.

8. Given my comments above about the effectiveness of dental attendance to prevent caries in this age group, the discussion needs careful consideration. The first explanation regarding finding a dentist is unlikely to account for differences in caries levels. Another explanation is that there is residual confounding as many of the covariates were only analysed as binary variables. It may also be that dental homes (ie that the care-giver has a dentist) is an indicator of other good oral health-related behaviours. Ie, if the care-giver has a dentist they may be more careful with the child’s diet and tooth brushing.

9. The explanation of the links between socio-economic status (as indicated by education and employment status of the parent) really could do with development as these links go way beyond oral health. The WHO report on social determinants of health would be a good place to start. Perhaps this reading will help the authors consider whether caregiver education level and employment are really immutable. These things are determined by social policy and dental health professionals have a health promoting role as advocates for their patients at this level. The WHO report could reframe this section.

10. I did wonder if this gave the authors an opportunity to critique their conceptual model, which doesn’t seem to consider social and environmental factors sufficiently. This may be just because of the way they have presented it. As space is not a problem in an online journal the authors could have a figure outlining the original whole model in the introduction, how they populated it with variables in the methods section and perhaps even summarising their final model in the results. However, as presented, the model does not seem to focus sufficiently on the broader factors.

11. The authors need not discuss their negative findings so much as their study is a pilot and may simply be insufficiently powered to detect some associations.

12. The chi squared tests and p values are not necessary in Table 1 (and the related results section). The comparisons in Table 1 are to compare the SAMPLES of children with and without developmental delays rather than to make generalisations about the prevalence of developmental delays in (for instance) all three year olds.

13. Would it change the meaning if the authors removed “for Study Population by Model Covariate Levels” from the column header in Table 2?

14. Table 3 could be simplified by removing several columns. Only the prevalence ratios and their 95% confidence intervals are needed as these values essentially duplicate the betas, standard errors and p values. The ratios and their confidence intervals are the most informative.

In summary, this is an interesting and well conducted study. I think it needs placed into a broader context that recognises the effectiveness of dental services and the broader social influences on oral health.
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

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