Author's response to reviews

Title: Developmental delays and dental caries in low-income children in the USA: a pilot cross-sectional study and preliminary explanatory model

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Author's response to reviews: see over
September 30, 2013

We are pleased to submit our revised manuscript entitled “Developmental delays and dental caries in Head Start children in Washington State: a pilot cross-sectional study and preliminary explanatory model”. All responses to reviewers’ comments and major revisions are noted in fuchsia colored font.

Thank you very much for the thoughtful review. We look forward to a final decision.

Sincerely,

Donald L. Chi, DDS, PhD
Corresponding Author
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

Reviewer's report:
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.

Thank you for providing an international perspective. We revised the manuscript to minimize confusion about these terms.

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.

This has been corrected.

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.

These points are very well taken. The manuscript has been revised to clarify any potential misunderstanding.

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.
We provided additional text in the Methods section on the Patrick model.

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.

In the Methods section, we provide additional details on our definition of dental home.

6. Where were any sealed surfaces in preschoolers’ teeth?

Yes, some children had sealed surfaces. However, these surfaces were coded as sound. De-aggregated data are no longer available.

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

Sentences starting with the word “about” have been edited.

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 (i.e. that the care-giver has a dentist) is an indicator of other good oral health-related behaviours. I.e., if the care-giver has a dentist they may be more careful with the child’s diet and tooth brushing.

We agree with your first point (because dmfs includes both decayed and restored/treated tooth surfaces). This explanation has been removed. While residual confounding is a possibility, we do not believe it explains why we found differences in dmfs prevalence for children by developmental delay status. We have included a sentence in the Discussion section to provide additional explanation of why dental homes are associated with lower prevalence of dmfs (caregiver behaviors).

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.
These are excellent points that have been included in the Discussion section. Thank you for pointing this out.

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.

We agree that our model excludes important cultural, social, and environmental factors. We included this as an important study limitation. We believe that including the original whole model has the potential to mislead readers. Given the incremental nature of oral health disparities research, we hope to address this limitation with future investigations.

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.

We agree that insufficient power is a potential explanation. However, we believe it is important to explain negative findings so that future investigators do not overlook these potentially important variables from their analyses. Thus, we retained the paragraph.

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.

The chi square tests and p-values are retained in Table 1 to give readers an idea of how model covariates are related to our main predictor variable. This will also help readers identify potential confounders in future studies. Thus, we retained the tests and p-values in Table 1.

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

This part of the column header has been removed.

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.
We retained beta coefficients and corresponding standard errors to provide readers with information on the level of sample fluctuation for each model covariate. This information, which is often times omitted from studies, is important in planning for future studies.

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.

Thank you very much for the thoughtful review and suggestions.

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.
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: 25 September 2013
Reviewer: Tegwyn Brickhouse

Reviewer’s report:
Major Compulsory Revisions
1. It seems that the p-values for several covariates in the model such as: communication difficulty, being 4 or 5 years old, having fluoride in the water, caregiver education, etc. are more significant than the p value for having developmental delay. Was the sample size large enough to have 10 covariates in the model? It states that that there was not collinearity between developmental delay and communication difficulties but may be helpful to include Goodness of fit tests for the model. The bivariate analysis showed little no significance.

Some of the model covariates had smaller p-values, but our main predictor variable was developmental delays. To acknowledge the importance of these covariates, we provide explanations for each model covariate in the Discussion section. The rule of thumb for multiple variable regression models is 1 covariate for every 10 subjects. Therefore, we have a large enough sample size for the number of covariates analyzed. We used variance inflation factors (VIFs) to evaluate collinearity, but as you mention the bivariate analyses provide additional evidence of low collinearity between developmental delays and communication difficulties. Collinearity statistics are standard and it is customary not to report findings unless they influence findings. Therefore, we have not included VIFs in our revised manuscript.

2. Developmental delay group was rather small N=20, and how do you know that kids with developmental delay have caries because of their developmental delay and not because their parents are not well educated or unemployed, etc. It may be helpful to emphasize where the significant findings and covariates fall under the theoretical model of Ascribed, Proximal, Immediate, and Distal factors in the text to supplement the tables.

Those are potential explanation. However, as you mention, our population of children with developmental delays was too small for subgroup analyses. We hope to address these questions in the future with a larger study population. We agree that it would be helpful to include appropriate domain labels in our Results section. This part of the Results section has been revised.

3. Under “Methods”-“dental home was assessed by asking caregiver whether they needed assistance finding a dentist", I feel that this question adequately represent the existence of a dental home.

Did you mean inadequate? If so, we agree and this has been more explicitly included as a limitation in the revised manuscript.
4. Under “Results” In regards to “caregivers” being employed/educated, is that one or both parents?

Both variables are for the primary caregiver of the child. This has been clarified in the revised manuscript.

Minor Essential Revisions/Minor issues not for publication
1. Under “Introduction”. First paragraph, sentence reads, particularly is regards to the U.S…Replace is with in.

Thank you for catching this typo.

2. Under “Methods”. First paragraph, tense discrepancy. “The median household income is $42,769 and 22.3% of individuals were below the Federal Poverty Level.” Either use past or present tense is/are or was/were?

Done.

3. Under “Conclusions”. #2. I believe authors meant to say “living in a community with fluoridated water were associated with significantly lower dental caries prevalence.” Not non-fluoridated water, as the manuscript reads.

Yes, you are correct. Thank you for catching this error.

4. Under “Discussion”. Third paragraph, second sentence, “There is an extensive literature…Take out the word an.

Done.

5. Under “Results” last sentence “children with a caregiver in school had significantly lower caries prevalence”. Should “in school” be “employed”?

Thank you for pointing this out. We inadvertently repeated our finding on caregiver’s education. This has been deleted in the noted sentence.

6. Under “Discussion”, last paragraph “Second the data were cross sectional and there is no….” should the sentence read, “Second the data is cross sectional

Data is plural of datum. Therefore, we retained the word “are”.

7. Under “Discussion”, last paragraph, “Longitudinal studies are needed to identify to better understand how risk factors...Take out to identify.

Done.
8. Under “Introduction” last paragraph, “We adapted….”, perhaps use a word other than we, the investigators, for example.

Done.

Discretionary Revisions
1. Under “Abstract”, results section, consider taking out the word about in “About, 17.4%....

Done.

2. Under “Introduction” “12% of Headstart enrollees had an IEP [16]”, then under “Methods” under predictor variable, the same study is referenced [16] and you state “which approximates the 15% of Headstart with an IEP [16]. Is it 12% or 15%?

The correct number is 12%. Thank you for pointing out this discrepancy.

3. Under “Introduction” second to last paragraph, second to last sentence, “enabling services to the caregivers of Headstart enrollees improved dental use for children but did not improve oral health status.” Not sure what this means.

This is a technical term for care coordination. We replaced the word “enabling” with the term “care coordination” to avoid confusion. Thank you for pointing this out.

4. Under “Discussion”, third paragraph, the “potential explanation” for why a child, whose caregiver is in school, has caries seems weak

This is an explanation noted anecdotally from parents in school whose child may be cared for by other family members. We agree there is no empirical evidence for it, but wanted to include it in the Discussion section.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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

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