Reviewer's report

Title: Socio-behavioural factors and early childhood caries in Trinidad: A cross-sectional study

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Reviewer: Kimon Divaris

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The present manuscript presents results of a cross-sectional study of caries prevalence among preschool children in 9 preschools of one region of Trinidad. The authors report their estimated prevalence and severity of caries and associate the prevalence of (binary) caries with several socio-demographic and parental behavior factors. This reviewer appreciates a report on children's oral health from an under-studied region, and finds that these data are of potential interest. It is important to acknowledge that due to sample characteristics (sampling methods and size) the data that were collected cannot adequately reflect the entire region of Trinidad, whereas several conceptual and mainly methodological issues limit the scientific validity of the current report. I elaborate below for the authors' consideration in future revisions of their work.

1. Is the question posed by the authors well defined?

Not entirely. The authors aim to “describe the prevalence and severity of ECC in preschool children”, but it is implied that this refers to Trinidad in its entirety. Moreover, the aim “to analyse the effect of social and behavioral factors” is not well-defined in my opinion. There are several well-developed models regarding children’s oral health (i.e. the Fisher-Owens model) which place these social and behavioral factors in hierarchical context. The authors appear not to use any conceptual model to guide their hypothesis, and thus ignore the inherent hierarchy and nested nature of these factors.

2. Are the methods appropriate and well described?

In its current form, the manuscript is not adequate methodologically in my opinion. First, in their analyses the authors did not account for a) the urban/rural character of the 9 preschools and b) the clustering of observations (children) within preschools.

Second, logistic regression was used instead of a more appropriate log binomial regression that would help derive prevalence ratios (due to the prevalence of outcome exceeding 20%-- see Barros et al., BMC Medical research methodology, 2003).

Third, the nature of the (dmft) data would ask for a more appropriate analytical approach compared to the non-parametric Wilcoxon and Kruskal-Wallis. Studies examining dmfs or dmfs indices typically employ models of the poisson (P) or negative binomial (NB) regression families, or their zero-inflated counterparts.
Fourth, the scarcity of data and the analytical strategy based on hypothesis testing that is followed result to a failure to recognize important associations that do not reach the levels of statistical significance, but have large effect estimates. The examples of parental occupation and education level in Table 1 are striking.

Finally, the model building approach that the authors employ does not consider the nature and contextual significance of the variables that are entered in the analysis; instead, statistical criteria are used. This results in paradoxes, such as theoretical antecedents (sweet snacks consumptions), concurrent ones (parental assessment of child’s oral health) and downstream event (seeking dental care) appear in the final “predictive” model. Noteworthy, the impressive odds ratio of 46 (95% CI: 4.5-469) for poor parental oral health rating and caries prevalence is based on apparently a single observation which may lead to model non-convergence.

3. Are the data sound?

The clinical information that was obtained appears to be sound, within the limitations of all field examinations. The intra-examiner agreement (reliability) statistic that is reported however (kappa = 1.0) refers to the diagnosis of caries vs. no caries, and not to the actual index (dmft) that was used. A kappa for the latter (a more sensitive measure of caries experience) should be reported as well.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Yes.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

In my opinion the manuscript will require a reframing (considering the limited potential of these data for inference) and a different analytical approach (statistical methods and model building).

6. Are limitations of the work clearly stated?

No. The major limitations regarding the sample characteristics (selection and size) and analytical methods are not recognized in the discussion.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

Yes.

8. Do the title and abstract accurately convey what has been found?

My assessment is that the title and the abstract of the manuscript extend beyond “what the data can say”. The study included 250 children from 9 preschools in one specific region (Caroni Education District) of Trinidad. This sample cannot reflect the oral health of all children of the same ages in Trinidad; due to manner
the sample was recruited (non-representative, non-probability) and due its small size. Therefore, the title and abstract would better represent the study if they reflected the exact sample characteristics without making interpretational inferences about S-ECC in the entire Trinidad.

9. Is the writing acceptable?
I find in the present form of the manuscript the writing quality borderline unacceptable. The manuscript requires substantial editing for English language and scientific writing.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

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