Reviewer’s report

Title: Ethnic disparities in children's oral health: findings from a population-based survey of grade 1 and 2 schoolchildren in Alberta, Canada

Version: 0 Date: 06 Oct 2016

Reviewer: Roger Keller Celeste

Reviewer's report:

Review of Manuscript CDOE-11-383

MAJOR COMMENTS

The manuscript presents results from a representative survey of Canadian schoolchildren on racial and ethnic disparities in oral health. The paper is well written and clear with interesting results for local and international audience. I would suggest some issues must be addressed before final decision.

SPECIFIC COMMENTS

Introduction:

1- The lack of data in Canada is an important issue to justify the study, but in addition to a descriptive aim, the study also wants to explain racial/ethnic differences and for this, very little has been said about what we already know.

2- The review of literature may include some more theoretical and empirical references if authors find it appropriate. On theoretical grounds, racial and ethnic inequalities have been explained by differences in individual behaviours, in socioeconomic factors and discrimination. In a very few and specific cases, racial inequalities may be accounted by genetic differences. I did not see any reference on this theoretical background.

3- On empirical grounds, there is plenty of studies trying to explain racial inequalities in oral health, mainly among adults and in USA (some references have already been presented) and also from countries with traditional ethnic diversity (Australia, Brazil, UK). Authors should include a bit of what we know and explain what differences may predict from other populations to Canadian schoolchildren. My guess is that, there should not be much difference. If any difference, please say why.

4- In paragraph 3, although I agree that most evidence points to the fact that minority groups have worse oral health, but differently from reference 11, recent evidence from UK has
shown that white adults did not have better oral health than other similar groups found in Canada (https://www.ncbi.nlm.nih.gov/pubmed/27412290). In Brazil, in 1960, Black schoolchildren had also lower DMFT (Revista de saude publica 1967; n 1: pag 38-43). This may mean that race is more a marker of risk factors than a direct putative cause. If this is culturally defined, what risk factors should be involved in this association for Canadian schoolchildren? Is it socioeconomic factors the major explanation, as we (myself included) think? Some references from the discussion could appear here, like refs 26 and 27.

Methods:

5- Reference 13 refers to a submitted paper, I recommend removing it if it has yet not been accepted, or include more details if otherwise.

6- why including in analysis only those children whose parents selected one single racial or ethnic group? I saw in the Results methods this accounted for 605 children, Couldn't those make another category? Further 443 were excluded because their racial/ethnic category was smaller than 250. Overall, it is 1048 children, and this is a lot. Another possible solution to deal with it is to create dichotomous dummy variables for each race/ethnic group instead of one variables with multiple categories. Including new categories may not change current results, but may give insights. Definitely, including more individuals to current categories (because they answered multiple categories) can change current results. Has any sensitivity analysis been done to see changes in associations? It will be interesting to see such results.

7- I assume that you have used the same variables in the counting and the inflated models in zinb, correct?

Results:

8- in Table 1 I suggest saying that the tables presents the "weighted percentages" (as you have used sampling weights) in the title, instead of "Distribution".

9- in Table 2, it think it is more natural to say in the title what the numbers in the body of the table represent (e.g. odds ratio) than explaining as a footnote.

10- could authors provide any fit indices for the regression model?
Discussion:

11-I see that one of the main findings is that authors explanatory variables did explain little of the association. Authors explanation, if I understood correctly, was that in Canada race/ethnicity is less associated with socioeconomic factor than in US. However, as I see table 1, there is an association between social factors and race/ethnicity. Could the fact that social factors (and behavioural factors) explain little be a problem related to the methods or selection bias? Could other methods, such as structural equations or path analysis help in assessing how much those variables mediate/confound the association?

12-Authors addressed limitations. Although I agree with most of them, the fact that data is cross-sectional would be a problem for temporalality between race and oral health, but we have no doubt, on theoretical grounds, that reverse causation is not at issue. Cross-sectional data may be a problem for other variables, and this may be clearer.

13-I would like to see some more words about the low response rate (49% in Calgary and 47% in Edmonton). This can introduce strong selection bias not only for descriptive figures but also for associations if the reason for non participation is a collider of race and oral health.

Are the methods appropriate and well described?  
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?  
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?  
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?  
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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