Reviewer’s report

Title: WOMEN’S ECONOMIC EMPOWERMENT, PARTICIPATION IN DECISION-MAKING AND EXPOSURE TO VIOLENCE AS RISK INDICATORS FOR EARLY CHILDHOOD CARIES

Version: 0 Date: 30 Jan 2020

Reviewer: María del Carmen Villanueva Vilchis

Reviewer's report:

It is a very interesting and innovative paper. However I have some suggestions that maybe can improve it.

It is not clear if the data used for the study as predictors correspond to the same year of the variables of ECC used as a dependent variable. I think it would be good if the authors could emphasize this, especially if they try to establish a direct association between these two variables in the mother-son binomial. It is not mention neither which countries were included.

These aspects might be reflected in the fact that some results might be contradictory. For example:

"Countries with more females living under 50% of median income had a significantly higher prevalence of ECC for 3 to 5-year olds (B = 1.82, 95% CI =0.12, 3.52).

"Countries with higher percentages of educated women had higher ECC prevalence in both age groups, (η² = 0.27 and 0.05)"

"Higher percentages of females with food insecurity had lower ECC prevalence".

The same happens when women who make decisions are related to caries prevalence:

"higher percentage of women participating in their own health care decisions had higher prevalence of ECC for 0 to 2-year. Higher percentage of women participating in decisions related to visiting family, relatives and friends had higher prevalence of ECC for 3 to 5-year" In this last part, although it is an indicator of empowerment, how can it be biologically justified to decide to visit if a family member or friend increases or decreases the prevalence of caries?

Although the limits of the ecological studies are very well known, the discussion of the paper becomes very speculative and adventured.

The final sentence of the discussion is very exemplary of this:

ECC is a multifactorial disease and risk factors could differ between cultures with different practices, beliefs and traditions indicating the need for country specific studies prior to adoption of our study findings as evidence for country-level policy formulation and program development.

In my opinion the main problems of the manuscript are:
1.- The use of different databases, taken in different populations and at different times and trying to make a direct association (mother-child binomial). Probably the authors can clarify if the databases used for the analysis correspond to the same year. Probably they can add also a table with the countries that were taken for the analysis.

2.- The discussion is very speculative and it is risky, so it should be strengthened with more evidence and contrast in a clearer way with the hypothesis

**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.

Unable to assess

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No

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