Author's response to reviews

Title: The prevalence and pattern of Deciduous Molar Hypomineralization and Molar-Incisor Hypomineralization in children from a suburban population in Nigeria.

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Author's response to reviews: see over
Letter to the editor

We authors would like to thank the peer reviewers for the thorough review provided for the manuscript. We have highlighted our responses to the points raised by the reviewers in the attached document.

Yours sincerely
Dr Morenike Folayan

Corresponding author
I appreciate the opportunity to review the manuscript. The study is interesting and well done. However some aspects could be improved:

Abstract: Add the indices used, method of data collection (interview + clinical examination), statistical tests used. **We the authors have included this information in the abstract. See lines 54, 56 and 58.**

There must be careful about the use of the word risk, since the risk transversal study but does not check associations. **We do agree with this and have revised the conclusion to reflect how the study had addressed the study objectives. See lines 66 to 68.**

Introduction: the text needs to be improved. I suggest the removal of the second paragraph. **This has been done**

Prevalence measures would be better in the 3rd paragraph. **This has been done**

Improve the justification of the study: what the study brings new to the literature? Explain the relationship with caries, impact on quality of life, the population has some characteristic that could predispose to the HMI? **This has been done. The suggestions made by the peer reviewer were found very useful. See lines 103 to 112.**

Statistical analysis suggest that regression is performed. **We have corrected this to reflect that we only conducted tests of association. Reference to bivariate analysis has been deleted.**

Discussion better discuss the results compared with the literature. Explain the absence of association with the factors studied. What are the limitations of the study? Suggest new studies.
We have addressed this suggested edits in the last two paragraphs of the manuscript. See lines 257 to 264 and lines 268 to 274.

Reviewer: Joana Ramos-Jorge

Reviewer’s report:

Introduction The second paragraph should be restructured. The problem addressed in the study is the hypomineralization. So, should start the paragraph with the second sentence:

"Hypomineralization is an important risk factor for caries in the primary dentition ..." The second paragraph has been deleted as suggested by the peer reviewer and the sentence referred too has been merged with the first paragraph.

Studies also show the association between enamel defects and quality of life. Authors can cite these studies in that paragraph. We have deleted the discussion on enamel defects in the introduction and focused on MIH which is the focus of the paper.

Authors should engage in justification of the study. Why is it important that the international scientific community is aware of the prevalence of hypomineralization in a community of Nigeria? The authors have written up a justification for the study as highlighted in lines 103 to 112.

Methods The sample definition is the first problem of this study. To calculate the sample size, the authors used a prevalence of 40% found in previous studies. However, the studies cited by the authors analyzed children in mixed dentition. But in this study, children in the primary dentition were also included. MIH is diagnosed in the mixed dentition stage. We the authors think the justification for using this prevalence for determining the sample size is appropriate. The study included children aged 3 to 5 years and 8 to 10 years. We are not clear what the peer reviewer expects of us with respect to this comment.

The second mistake is to include more children than is necessary. If the sample calculation resulted in 405 children, why the authors included 563? First, the sample size determined is the minimum sample size. Second, as the study highlighted that study was part of a larger study. We
were therefore able to access the data of 563 children – a sample that was larger than the minimum sample for the study. The study is therefore not underpowered.

Another question is whether to evaluate the associations between DMH and MIH and socioeconomic status, this was an adequate sample size calculation. There was no previous study on MIH that evaluated the relationship between MIH and SES. The study therefore utilized the largest prevalence on MIH in the literature to ensure we obtained adequate sample size. We think we have used the evidence available in the most appropriate way to determine the sample size for this study.

Results The authors report: "Three hundred twenty-seven 184 children (58.1%) between the ages of 3 and 5 years were examined for DMH, and 236,185 (41.9%) children between the ages of 8 and 10 years were examined for MIH". Since the authors evaluated DMH in primary second molars (as cited in methodology), why they included children 3-5 years? This evaluation could be performed in children 8-10 years still have the primary second molars. The appropriate age for diagnosis of DMH is 3-5 years just as MIH diagnosis for prevalence studies is made at 8-10 years. At age 8-10 years, teeth affected by DMH may have been extracted because of post-eruptive breakdown.

In the objective, the authors state, "The study also examined the association between the presence of MIH and DMH and their relationship with the sex and socioeconomic status of the children." In the introduction, the authors devote almost a paragraph to the association between DMH and MIH (paragraph 4, line 86). Thus, expected results a analysis of the association between MIH and DMH, which was not performed. This study was a cross-sectional study and not a prospective study. The current design of the study makes it difficult to analyze the association between MIH and DMH. The best we could do to show the relationship between MIH and DMH in this study was to report on the co-relationship. Since we did not collect a longitudinal data on the same patient, we could not have conducted an analysis showing an association between MIH and DMH in this study.
Discussion I believe that the authors should be more modest in relation to this study. Again, at the beginning of the discussion the authors mention the relationship between MIH and DMH, but not investigated this relationship. We did conduct an analysis on DMH/MIH co-morbidity. That was the best we could have done with a cross sectional study. An association between MIH and DMH would not be appropriate in this study as the sample for the 3-5 years were not the same as the 8-10 year old study participants.

The authors really discuss their findings in the second paragraph. Thank you. We have expanded the discussion in line with suggestions from the peer reviewer.

Conclusion The conclusion is not appropriate to the objectives and results of the study. We have revised the conclusion to reflect the objective of the study.

Tables The tables are poorly organized. The sum of percentages cannot exceed 100%. All the sums in the tables have been revised to ensure they all sum up to 100%.

In Table 2, when the authors report the distribution of MIH, the table is contained DMH. Thanks for picking this up. We have corrected this.

What is the need of Table 3? It is a summary for the lines 208 to 212 reported in the manuscript. We think reduces the need for the narrative.