Author's response to reviews

Title: Child, Neglect and Oral Health

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Author's response to reviews: see over
Dear Editor,

We are pleased to submit the revised version of the manuscript “Child, Neglect and Oral Health” to your journal. The answers to the reviewers’ comments are addressed below and the changes made in the manuscript are marked in red.

Please, do not hesitate in contacting us if you need any further information.

We are looking forward to have this manuscript published in your journal.

Best regards,

Anyà Vieira, Maria Saintrain and Caroline Lourenço

**Answers to reviewers’ comments**

**Reviewer 1: Rahul Chopra**

*Reviewer's report:*

Minor essential revisions: reference for the highlighted in the introduction.

*Four references were added in the manuscript regarding low utilization of dental service by children in different parts of the world. It is important to say that there is no published data (article) regarding this on the municipality of Pacoti. This information was given (and confirmed) by the oral health coordinator (based on management data). Please let us know if you would like us to add this in the manuscript.*

Discretionary revisions: 3 issues raised in the discussion.

1. Consideration should also have been paid to the diet (its quality, frequency of intake of sugars etc.)

*Further discussion regarding this issue was inserted in the text in order to elucidate the important matter raised by the reviewer. These additions are highlighted in the manuscript.*

“Families living in disadvantaged circumstances may experience greater levels of stress, isolation and family conflict. Parents may have more feelings of powerlessness to achieve good oral health for their children. These factors may have an indirect effect on disease through their influence on diet and behaviour, for example, and must be taken into account if strategies for managing oral health care are to succeed. [Rayner, 2003] Three-day dietary diaries are commonly used by pediatric dentists to assess risk factors linked to diet. However, in community based studies, this diaries are difficult to be done in an accurate manner, specially because diet is a modifiable risk factor. According to
Chankanka, some dietary factors are associated with caries at one age only, while others are associated with caries across childhood. He concludes there is a complex relationship between diet and caries, and that more studies are needed in the field, including studies that place more emphasis on investigation of modifiable risk factors [Chankanka, 2010].

2. was the oral hygiene status of the siblings especially of those who had higher values of dmf-t reported/asked/questioned since a negative reply confirms that the caretaker is really callus and supports the neglecting attitude.

*Despite the importance of the fact pointed out by the reviewer, the oral hygiene status of the siblings was not evaluated, mainly due fact the majority of the siblings were not at home during the interview with the parents (which occurred during school hours).*

3. relation could also have included the criteria of a working/non-working mother.

Further to your suggestion, this relationship was evaluated. The findings reveal that there is no dmf-t difference in children with and without working caregivers/mothers (Mann Whitney test – p>0.05). However, there was a difference between the number of teeth indicated to extraction among the two groups (working and non-working caregivers), where the children from the working caregivers presented less teeth needing to be extracted than those of non-working caregivers (Mann Whitney test, p=0.047). Chi-square test also reviewed no relationship between working caregiver and dental care access (visit to the dentist) – p>0.05. These informations were added to the manuscript.

**Reviewer 2: GRAMMATI SARRI**

**Reviewer's report:**

Major compulsory revisions

1) Although the authors have attempted to investigate an important public health issue, child neglect, the aim of the paper was not met by the data collected in the study. The authors stated that they wanted to investigate the association between dental caries and neglect in children, however the factors selected in the paper as indicative of neglect do not correspond to any of the established definitions of child neglect. Is it dental neglect the sub type of child neglect that the authors refer to? One of the main improvements in the research of child neglect is the division of different types of neglect for example physical, emotional, health care neglect (World Health Organization, Dubowitz). In addition, it is well known the association of most of the selected factors of child neglect with dental caries experience. What the authors want to add to this topic with the data collected in the study? The authors would need to seriously revisit this part.

*Several changes regarding this issue were made in the manuscript and are highlighted. We used the Integrated Management of Childhood Illness (IMCI) to physically evaluate the children and asked questions to their caregivers regarding factors understood as sign of neglect (including dental neglect), as if they took their children to dental appointments,
if they perform health promotion actions with theirs children, the physical aspect of caregivers comparing to their children, etc. The main purpose of this article was to scientifically investigate the belief that neglect (including dental neglect) influences children oral health. According to the author best knowledge, this issue was properly investigated in this research and therefore needs to be shared to the scientific community, specially because it is the first time that such relationship is evaluated in a municipally where access to dental treatment is not an issue (all residents have quality health units close to home and free of charge). As mentioned in the text, Pacoti has a public (free of charge) dental health assistance program linked to the Brazilian Unified Health System (SUS). The quality of this service has been certified by national and international organizations (Brazil’s smiling prize and UNICEF seal).

2) The authors have focused solely on the parental responsibility of neglect and ignoring the well established social determinants of child neglect. Although the parents have primary responsibility on children’s health care, including oral care, the introduction gives only one sided part of the available evidence in the area. For example sentences such as the following "Thus, caregivers must get information and provide the child with preventive and restorative care" are very strong and in a way misleading as the provision of relevant information regarding child oral health prevention and dental treatment is not necessarily dependent on parents but also the availability and affordability of services. The introduction is lengthy and needs reorganizing.

In the case of Pacoti, the dental services are universal and free of charge for all patient. The public health system develops actions on public health promotion, prevention and rehabilitation (e.g., including individual’s dental services). The public health units are located in several points of the city and of easy access to the whole population. Therefore, despite the theoretical dependency on services’ availability and affordability, the parents/caregivers in Pacoti do not have such limitations. That is one of the main reasons that the city of Pacoti was chosen for this study. These information were added to the manuscript.

Introduction section was reorganized as suggested by the reviewer. The changes are highlighted in the text.

3) There were serious statistical misinterpretations. This is a descriptive study which explores associations but not proving causality. There are a couple of associations highlighted by the authors that were not statistically significant (p<0.05). The section of results needs to be rearranged and the language used needs to more methodologically appropriate. The tables are not very informative.

The results section was modified in order to be more informative and more suitable for the manuscript. The changes are highlighted in the text. Is was not clear for us which kind of changes the reviewer would like to see in the tables. We would appreciate if you could be more specific regarding this issue in order allow us to perform the necessary adjustments.
The expression ‘association trend’ was changed to ‘trend towards a significant association’, which better fits what the authors were trying to say in the manuscript.

Borrowing an explanation to this in ‘Chapter 2-13. Reporting Confidence Intervals vs P Values and ... humis.bmi.utah.edu/.../organization_1863_1321044...’, we would like to explain that:

“We can appreciate the reviewer’s viewpoint about p values and statistical significance. It is widely taught in introductory statistics courses the p < 0.05 indicates statistical significance, but rarely taught that a p value slightly larger than 0.05 offers suggestive evidence against the null hypothesis of no effect. Many prominent statisticians have been trying to correct this practice, as it is overly restrictive. The practice leads to more type II errors (failing to find significance) in favor of avoiding Type I errors (finding false significance), whereas the researcher should be concerned about both types of errors. Here is a short list of widely quoted works with references:

Fisher, himself, who introduced hypothesis testing in 1935, establishing the convention of p<0.05 to indicate statistical significance, later in 1956 retracted the idea as “absurdly academic”, advocating, instead, that actual values of the p values be shared with fellow researchers (Gigerenzer, Krauss, and Vitouch, 2004).

Decades ago, Burdette and Gehan (1970, p.9) advocated that 0.05< p < 0.10 provided “suggestive evidence” of statistical significance and should be reported as such.

Altman, Gore, Gardner, and Pocock (1983, section 4.3), in their seminal paper, “Statistical guidelines for contributors to medical journals” advise,

“Calling any value with p>0.05 “not significant” is not recommended, as it may obscure results that are not quite statistically significant but do suggest a real effect (see section 5.1).”

and clarify further (1983, section 5.1) with,

“Some flexibility is desirable in interpreting p values. The 0.05 level is a convenient cut off point, but p values of 0.04 and 0.06, which are not greatly different, ought to lead to similar interpretations, rather than radically different ones. The designation of any result with p > 0.05 as not significant may thus mislead the reader (and the authors); hence the suggestion in section 4.3 to quote actual p values.”

Altman (1991, pp.168-169), in this applied statistics textbook, Practical Statistics for Medical Research concurs,

“The cut-off level for statistical significance is usually taken at 0.05, but sometimes at 0.01. These cut-offs are arbitrary and have no specific importance. It is ridiculous to interpret the results of a study differently, according to whether the P value obtained was, say, 0.055 or 0.045. These P values should lead to very similar conclusions, not diametrically opposed ones. A minor change to the data can easily shift the P value by this amount or more….Quoting the actual P value allow the reader to make his or her own interpretation.”
Lang and Secic (2006, p. 58) in their book, How to Report Statistics in Medicine. Annotated Guidelines for Authors, Editors, and Reviewers, although less boldly than Altman, advocate for findings with p values close to 0.05, which the researchers feel are important,

“…report the observed difference and the (95%) confidence interval for the difference. …The point is that clinically important results should not be overlooked because they are not statistically significant.”

These arguments are obviously logical and are slowly gaining acceptance. You can find the practice of allowing the reporting of “marginally significant” or “trend towards significant” statements in top journals.

For example, Karunajeewa et al (N Engl J Med, 2008, p.2551) reported p values between 0.05 and 0.10 and referred to them as trends,

“Among children who had falciparum malaria, in univariate analyses, there was a trend toward a lower risk of any treatment failure (not corrected through PCR genotyping) at day 7 with a higher plasma piperaquine level in the dihydroartemisinin-piperaquine group (hazard ratio for each increase of 10 µg per liter, 0.86; 95% CI, 0.73 to 1.01; P=0.06) and with a higher plasma lumefantrine level in the artemether-lumefantrine group (hazard ratio for each increase of 100 µg per liter, 0.87; 95% CI, 0.74 to 1.02; P=0.09). According to the Cox model of treatment failure in the dihydroartemisinin-piperaquine group, after correction through PCR genotyping, there was a trend toward association in plasma piperaquine levels at day 7 (P=0.08), but the nutrition z score according to weight for age was no longer significantly associated (P=0.25).”

Another example is Chandrachud et al. (Br J Cancer, 1997) who reported,

“However, survival time was generally longer for patients with higher vascularity, reaching bordline significance (P = 0.06) for the average microvascular density values.”

A final example is Boyce et al. (Ann Surg, 1995) who simply avoided the use of “marginal”, “bordline” or “trend towards” significance, and reported such a result with the same force as a p<0.05 result.

“Sites treated with CSSs had increased incidence of excude (p = 0.06) and decreased percentage of engraftment (p < 0.05) compared with STAG.”

The reviewer was concerned about the few places in our manuscript where we reported p values that were between 0.05 and 0.10, and then suggested that these were clinically significant findings. We still hold to this, and we have left them this way in the paper. Above, we have provided many authoritative published works that supports the practice of doing this. In view of these works, we hope that the reviewer is now more comfortable with us doing this. The actual p values are reported, so the reader can assess the evidence and decide for him or herself, so we have not done anything misleading.

References


4) The discussion needs to focus on the main conclusions and its interpretations. More general conclusions were made than the authors could support with their data collection and analysis. The language used needs improvement in order to be more scientifically sound.

Changes were made in the text in order to focus on the main conclusions and its interpretations as requested by the reviewer.