Author's response to reviews

Title: Pilot survey of oral health related quality of life: A cross-sectional study of adults in Benin City, Edo State Nigeria

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Version: 2 Date: 17 March 2005

Author’s response to reviews: see over
March 16, 2005

Dear Editor:

Thanks for forwarding the comments by the reviewers, which we found very helpful, and it contributed to improve the quality of the manuscript. We have addressed all the concerns regardless of whether they were classified as compulsory or optional. We have reproduced the reviewers’ comments below and our responses are in the section following the series of asterisks “****” after the comments.

Reviewer # 1
Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

COMMENTS 1) - The study presents results about oral health-related quality of life (OHRQoL) from a developing country. This is interesting and new. However, it is not clear how far the study results from “two large outpatient medical care facilities all in Benin City, Edo State” can be generalized. Study subjects seem to be well-educated and socioeconomic status is an important factor for OHRQoL. This may limit the generalizability and implications of the subject selection process should be addressed in the discussion.

**** We had mentioned the limitations to generalization earlier, and have re-emphasized the issue in our discussion as suggested by the reviewer. Data on socioeconomic status is hard to collect in Nigeria and when collected it can sometimes be misleading, so our study did not collect that information.

COMMENT 2) - An earlier version of the OHQOL-UK instrument was used. It needs to be explained in the methods section what the differences are compared to the current version of the instrument. Are the present study results using the earlier instrument version generalizable to investigations using the current version of the instrument (discussion)?

**** A detail explanation on the study instrument used compared to the final version of OHQoL-UK© is included in the methods section and discussed as suggested by the reviewer. We also included the extent to which our findings can be generalizable to the current version of the instrument we used.

COMMENT 3) - The precision of the study estimates is important. For key results (e.g., the proportion of participants perceiving their oral health as affecting their quality of life, or the instrument’s median/mean summary score in the sample) confidence intervals need to be reported.
**** Because this is a small study from convenience sample (as pointed out by the reviewer also), provision of confidence intervals and testing estimate precisions will convey a false impression about generalizability. We have refrained from this potential misunderstanding of the readers. However, testing of differences involves the measure of variability, and those tests have been performed as needed. Furthermore, the aim of this study was not to provide national estimates, but analyze the data more as a descriptive epidemiology and not provide national estimates.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

**** We have modified the tables to incorporate the reviewer's suggestions. However, we disagree about the precision of estimates.

COMMENT 4) - “oral health related quality of life factors” (abstract) is ambiguous. Please describe in detail.

**** We have modified this as suggested by the reviewer.

COMMENT 5) - The asterisk in table 2 should be explained (probably similar as in table 3).

**** We have added a foot note as suggested by the reviewer.

Discretionary Revisions (which the author can choose to ignore)

COMMENT 6) - Although the used oral health-related quality of life instrument is widely used, a paragraph in the methods section should give more details for the reader not familiar with the instrument.

**** We have provided a description of the theoretical and conceptual background for development of OHQOL instruments or socio-dental indicators and further explanation was given on the study instrument used compared to the final version of OHQol-UK© in the methods and discussed as suggested by the reviewer and we also included relevant references regarding the application of OHQol instruments in developing countries.

COMMENT 7) - Table 3 uses two asterisks to mark results close to statistical significance. This may be misleading because often a system with one, two, and three asterisks is used to characterize different levels of statistical significance (in this system two asterisks mean “highly” statistically significant results).

**** We have addressed this and changed the asterisks to superscript alphabets for easy reading.
Reviewer # 2
Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

COMMENT 1) This paper addresses an important issue in dental research, to assess self reported oral health and oral health related quality of life in general and disease specific populations. Truly, this is a relatively new area, particularly in the context of developing countries. In the introduction, the authors refer, rather briefly to oral quality of life instruments (OHQoL) developed and validated in various parts of the world. The paper would benefit from providing a more explicit description of the most common instruments available together with the actual references. Also needed is a more careful description of the theoretical and conceptual background for development of OHQoL instruments or socio-dental indicators.

**** We have modified the background and methods section with explicit description of the most common instruments available together with actual references; a description of the theoretical and conceptual background for development of OHQoL instruments or socio-dental indicators; relevant references regarding the application of OHQoL instruments in developing countries as suggested by the reviewer.

COMMENT 2) There is a lack of relevant references regarding the application of OHQoL instruments, particularly in developing countries. The authors give the impression that the present approach is a completely new one in sub-Saharan Africa â€“ which is not the case (e.g. Å°strÅ„m AN, Okullo I. Validity and reliability of the Oral Impacts on Daily Performances (OIDP) frequency scale: a cross sectional study of adolescents in Uganda. http://www.biomedcentral.com/1472-6831/3/5; Masalu JR, Å°strÅ„m AN. Applicability of an abbreviated version of the oral impacts on daily performances (OIDP) scale for use among Tanzanian students. Community dent Oral Epidemiol 2003; 31: 7-14; Soe KK et al. reliability and validity of two oral health related quality of life measures in Myanmar adolescents. Community Dental health 2004; 21: 306-311).

**** We have added the relevant references suggested by the reviewer and other references regarding the application of OHQoL instruments in developing countries.

COMMENT 3) The writing is not satisfactory and there is a need for language corrections throughout the manuscript.

**** We have re-written substantial parts of the manuscript to address this comment.
COMMENT 3-1) The purposes of the study should be described more clearly. Ad 1): without providing a description of the OHQoL instruments (see below) utilized throughout the introduction the first purpose in terms of describing the effect and impact is less meaningful. What is the difference between effect and impact of the oral health related quality of life factors presented?

**** We have explained the purpose of the study with clear aims as suggested by the reviewer.

COMMENT 3-2) The purpose should be rephrased in terms of exploring the association between oral quality of life scores (effects or impacts?) and oral health care seeking behaviour.

**** We have re-written substantial parts of the manuscript and explained the purpose of the study with clear aims as suggested by the reviewer. We have also added a section of analysis as suggested by the reviewer.

COMMENT 3-3) The authors claim to use oral quality of life instrument similar to what has being used in the OH-Qol UK.-without further explanation. For readers to understand what is the instrument applied, itâ€™s a prerequisite that the authors describe the instrument carefully, its single items used, the justification for arriving at a number of dimensions, the scoring system applied and eventually how the current application deviate from the reference instrument of OH-QoL UK. The items included under attributes of effect can be derived from tables 1-3. It appears that three dimensions in terms of physical, psychological and social are derived from 16 single items. The question is: whatâ€™s the rationale for the current division of domains?? For the attributes of impact, three dimensions in terms of daily activities, social activities and talking to people are mentioned (table 4). From what single items are those dimensions derived and what is the correspondence between attributes of effects and attributes of impacts? How did the authors arrive at the summary score of oral health effects on quality of life described on page 8 line 3-5?

**** We have provided a description of the theoretical and conceptual background for development of OHQOL instruments or socio-dental indicators and further explanation was given on the study instrument used compared to the final version of OHQol-UK© in the methods and discussed as suggested by the reviewer. Furthermore, we have explained the current division of the domains and described how the summary score was derived and Table 3 was slightly revised following the reviewer's suggestion.

COMMENT 4) The method in general seems appropriate but important details are lacking. The authors need to justify their choice of two large outpatient medical care facilities, in light of having a study group consisting of mainly young and well educated attendees. A more careful description of the interview situation is needed as well, in terms of place, time, etc.
**** We have re-written and described the issues with justifications for use of study sites and the interview situation as suggested by the reviewer.

COMMENT 5) Recognising the wide age range (18-64) of the study group and the fact that OHQoL often vary with age and gender it seems reasonable to provide the age adjusted prevalence scores (possibly also separately for each gender) with 95% CI rather than reporting the prevalence rates for the whole group. To avoid small figures in the cells, categories should be reduced (e.g. from five to three). Table 3 shows the effect responses according to previous dental visits. My main objection is that conclusions are based on a bivariable analyses without even mentioning the possibility of confounding. Some kind of multivariable analyses should be provided to adjust for possible confounding effects of for instance socio-demographic factors (i.e. factors that associate with both quality of life and dental visiting). Moreover, it is confusing that the discussion is supported by provision of new results, i.e. findings that are not reported in the result section.

**** We agree with the potential for confounding, and have carried out a multivariable analysis using ANOVA for the summary scores. As will be noticed, the final model included only education in it. We have provided a discussion of this issue also. However, as we mentioned earlier, because this is a small study from convenience sample, provision of confidence intervals around estimates and testing estimate precisions will convey a false impression about prevalence and generalizability. This study was not done on a nationally representative probability sample, and therefore we refrained from providing any "prevalence" estimates or discuss such prevalence. Because of the sampling limitation, we have restricted our analyses to evaluating differences between groups rather making prevalence estimate claims.

COMMENT 6) Tables-both percentages and numbers should be presented. The headings of the tables should be more informative. Table 3 is difficult to grasp and should be revised.

**** We have modified tables for easy reading. However, because the total number of participants is explicitly mentioned in the methods, we have persisted with providing only percentages, with the understanding that anyone can easily compute the number in the cell instantly. We believe that providing numbers along with the percentages will lead to cluttering of the tables, and will not add any extra information.

We look forward to your early favorable response to this re-submission.

Thanks,
Christopher okunseri