Author's response to reviews

Title: Household smoking and dental caries in schoolchildren: the Ryukyus Child Health Study

Authors:

Keiko Tanaka (k-tanaka@fukuoka-u.ac.jp)
Yoshihiro Miyake (miyake-y@fukuoka-u.ac.jp)
Masashi Arakawa (h069475@ll.u-ryukyu.ac.jp)
Satoshi Sasaki (stssasak@m.u-tokyo.ac.jp)
Yukihiro Ohya (ohya-y@ncchd.go.jp)

Version: 3 Date: 27 May 2010

Author's response to reviews: see over
March 27, 2010

The Editor

BMC Public Health

Manuscript ID: 1736701573172949
Title: Household smoking and dental caries in schoolchildren: the Ryukyus Child Health Study
First author: Keiko Tanaka

Dear Sir:

We appreciate your suggestions and the comments of the 3 reviewers regarding the above-referenced manuscript. We have revised the manuscript and are resubmitting it for publication in the BMC Public Health.

I am sending the revised manuscript with changes highlighted in red.

I am also sending the copies of original questionnaires (Japanese) and the English translation version.

Specific revisions and responses to the each reviewer and the Editor are described below.

We thank you in advance for your consideration.

Sincerely,

Keiko Tanaka, DDS, PhD
Referee 1 (Dr. Scott L. Tomar)

The authors have adequately addressed the reviewers’ comments. I note just a few suggested discretionary revisions.

Response:
Thank you very much for your appreciation of our manuscript.

1. p.10, para 1. While it is true that many of the studies on smoking and dental caries did not control for potential confounding, it is more likely in this case that uncontrolled confounding would lead to positive associations between smoking and caries than no association. Smoking is not uniformly distributed in most developed countries; it is generally associated with lower income, less education, lower levels of preventive behaviors, poorer diet, and higher levels of co-morbidity – the same factors generally associated with increased risk for caries.

Response:
We changed the sentences “Many of the published studies showing no association between smoking and dental caries did not control for potential confounders, such as socioeconomic status, diet, and oral health behaviors. Therefore, it is possible that the observed associations were influenced by confounding factors.” to “The heterogeneity of these results among studies might be explained by differences in characteristics, smoking habits, and lifestyle of the populations examined, the study design used, and potential confounders considered. In particular, few of the studies on the association between smoking and dental caries controlled for potential confounders, such as socioeconomic status, diet, and oral health behaviors.” (page 10, lines 6-10).

2. p.11. I know another reviewer suggest using the word “interproximal” for caries lesions, but it is actually incorrect. Caries occurs on tooth surfaces, not between adjacent teeth as the word “interproximal” suggests. The correct terminology is proximal caries or approximal caries.

Response:
As suggested, the term “interproximal” was changed back to “proximal” (page 11, line 16).
Referee 2 (Dr. Melbourne Hovell)

This is a large scale survey study of dental caries in children and their relationship to Second Hand Smoke exposure (SHSe). This study provides appropriate multiple variable analyses of the relationship between SHSe and caries after control for demographic and other possible confounding variables appropriate for a cross-sectional design. While there are minor methodological concerns and some important discussion issues that might be added to this manuscript, it meets my standard for methodological fidelity and significance to the health of children. I recommend that the editors accept this study for publication after minor refinements in methods and the preparation of the manuscript.

Response:
Thank you very much for your appreciation of our manuscript.

Purpose and significance. This study offers a remarkably large sample of Japanese children as participants of a study if dental carries and their relationship to Second Hand Smoke exposure (SHSe). As such this study offers findings for an Asian culture and for dental health uniquely that suggests that tobacco smoke exposure may have an etiological role in the development of carries. This adds to the cumulative evidence of the likely harm due to tobacco smoke and more importantly due to SHSe. As such the study offers a remarkably large epidemiological study of considerable significance for dental health and also contributes to the wide range of biological systems that appear to be damaged by tobacco smoke, including passive exposure.

Methods. This study employed a cross-sectional design to test hypotheses about SHSe and dental carries. The sampling procedures and size are major strengths of the study offering sufficient power to detect relatively small effects in children but effects that may have life-long impact on their dental health that might have been avoided had the children been protected from SHSe. Since children exposed as children are very likely to be exposed for much of their life, the cumulative exposure and the risks for premature morbidity and mortality are likely to be arithmetically greater than estimated by this study, making the estimates observed conservative.

Measures. In general, this study offers reasonable measures of reported SHSe and
dental carries, based on the observation of a clinical dentist. The only concerns raised about measures pertain to possible refinements in the quality assurance or fidelity of some of these measures. First, the measures of dental carries might be strengthened if a subsample of the children’s findings for carries could be compared to dental chart records or even a second dental examination by another dentist. These types of checks would provide evidence of reliability if not validity. While the dental examinations offer considerable face validity, absent X-ray confirmation there may be an under-estimate of carries and it is not clear how this might alter the association with SHSe. Use of dental charts also might offer the opportunity to confirm number of carries in a subset of children for whom X-ray information is the dental chart. If this sort of reliability check is possible, I suggest it be considered for a nominal subset of the children selected at random.

Response:
Thank you very much for your very comprehensive comments of our paper. As we indicated in our response to comments regarding the first revision, we think that a statement regarding the reliability and bias of the outcome measure, such as intra- and interexaminer consistency and absence of X-ray examination, had been given in the Discussion as follows: “Data on dental caries taken from school records are likely to avoid information bias. The school dentists were given detailed criteria for the examination, but calibration was not performed among school dentists. Therefore, it was unknown if intra- and interexaminer consistency was established. Additionally, radiographs were not taken. Oral examinations without radiographic information are likely to underestimate the prevalence of dental caries, especially interproximal caries [26, 27].” (page 11, lines 11-16).

Comment:
The variables employed included age and other factors that may be presumed to be count variables, if not continuous. However, most variables were converted to ordinal measures without clear rationale for doing so. If these measures were highly skewed and could not be adjusted to a normal distribution, the authors should explain this and how the final ordinal measure offered a more reliable measure than would be true as a count or continuous variable. This issue is of some concern due to the loss of precision and power true for continuous variables that are reduced to ordinal or dichotomous variables. This issue
pertains to both independent and Dependent variables.

Response:
When we analyzed our data using pack-years of passive smoking, age, and daily sugar intake as a continuous variable in the multivariate model, the log-binomial model failed to converge. In this case, Poisson regression capability with the robust variance would likely to be used. However, compared with log-binomial maximum likelihood estimators, the Poisson estimates are not fully efficient. Therefore, we decided to put those independent variables in the multivariate models as ordinal variables. In the current study, we used a log-binomial regression model in order to estimate the prevalence ratio for household smoking in relation to dental caries. With regard to a dependent variable, a binary variable is used to estimate a relative risk such as prevalence ratio and odds ratio.

Comment:
Results and discussion. The results are overall quite clear. However, in the context of a modest proportion of youth who did not participate, and in the context of education and other factors likely associated with failure to participate, the authors should explore the likelihood of under-estimates of carries (for their prevalence results) or at least add discussion of this issue. The loss of children at higher risk for carries and SHSe also raises a concern that the results may under-estimate the strength of the association reported. Thus, this too should be discussed.

Response:
In accordance with the comment, we have already added the following sentence to the Discussion in the first revision: “The loss of children who were at higher risk for caries and who had parents with a lower educational level might lead to underestimation of the strength of the association between household smoking and dental caries.”(page 12, lines 16-19).

Comment:
The Discussion section should make clear that there is now sufficient evidence and biological plausibility for concern that SHSe may contribute to carries in children.
They authors also should outline follow on studies that might confirm these findings in the context of methodological improvements in the research design and fidelity.

Response:
In accordance with the comment, we have already added the sentence “Although the biological and behavioral plausibility for an etiological relationship between SHSe and dental caries is likely to be high, epidemiological evidence is still insufficient.” in the first revision (page 13 lines 1-3).

Comment:
Larger issues for Discussion. Following traditionally conservative research culture, cross sectional and other non-experimental studies always are reported in tempered language and conclusions are restricted in scope and scale. However, this can be a risk to a special form of Type II error. The current results add to a growing list of biological and behavioral risks to children and/or adults with exposure to SHS. Thus, the biological and behavioral plausibility for an etiological relationship is quite high. In this context, the authors are encouraged to discuss the importance for dentists to be involved in routinely screening youth for SHSe and counseling avoidance for older children and counseling parents to establish no smoking bans in their home. Formal controlled trials of dental counseling of this nature might also be recommended as one of the strongest and quickest means of showing a “causal” relationship between SHSe and dental carries by demonstrating a protective effect when prevented. Thus, while all of the normal limitations of cross-sectional designs should be acknowledged, this study should also offer direction in the follow on science that might confirm causal relationships while also testing services to prevent the likely effects of SHSe.

Response:
Thank you for your thorough consideration of the issues raised in our manuscript. Evidence on the association between passive smoking and dental caries is insufficient to infer a causal relationship, although the biological and behavioral plausibility for an etiological relationship is likely to be quite high. It is too early to make any conclusion about the relationship between passive smoking and dental caries right now. Therefore, in this part, we cannot infer clinical implications and recommendations based on our
results. We think that the issues that we addressed in the Conclusion section provided direction for future research.

Comments:
Minor issues. The term ETS is reportedly a product of the Tobacco Industry to move the attribution from cigarettes to a more general environmental source of smoke exposure and thereby protect the Industry. Many of the investigators involved in SHSe have adopted second hand smoke exposure (SHSe) as the preferential name. I recommend that be done for this paper too.

Response:
As suggested, we have already changed the term “environmental tobacco smoke (ETS) exposure” to “secondhand smoke exposure (SHSe)” in the first revision.

Comments:
While the rate or smoking among young children is likely quite low, the study should attempt to identify smokers and eliminate them from the analysis or discuss why this is not possible.

Response:
In accordance with the comment, we have already added the following sentence in the first revision: “When sensitivity analysis excluded these active smokers, the results were not changed, however (data not shown).” (page 12, lines 8-9).

Comment:
While region of residence was one of the control variables it is not clear how this coincides with city. Since two cities were employed it should be made clear how these were similar or different and then how the control for region of residence controls for city influences. This could be conceptualized as a cluster effect for which control for cluster effects would be important. However, this is not likely to provide a big influence given the likely similarity between city populations.

Response:
In accordance with the comment, we have already added information on the population density to the Methods (page 5, lines 4-6).

About clustering effects, we decided to control within schools, but not cities. We recalculated prevalence ratios by taking into consideration clustering within schools using via the PROC GENMOD procedure. This did not change our results. We have already added the sentence “We took into consideration clustering within schools via the PROC GENMOD procedure.” in the first revision (page 6, lines 3-4 from the bottom of the page).

Comment:
Regardless of how city may have been included in the current analyses, it might strengthen the results to replicate findings within each city independently. If patterns are the same, even if the power to detect significance is compromised especially in the smaller population from one city, this might offer an internal test of generalizability.

Response:
In accordance with the comment, we have already added the following sentence in the first revision: “When we analyzed study subjects separately according to city, the results for Naha city were similar to those in the overall analysis. Likewise, the results for Nago city were similar to those in the overall analysis.” (page 12, lines 20-22).
Referee 4 (Dr. Olalekan A Ayo-Yusuf)

The author(s) have addressed most of the concerns raised in the previous manuscript, but it was rather remarkable that the authors claim that their estimates did not change after adjusting for errors from cluster sampling.

Response:
Thank you very much for your appreciation of our manuscript.

It was also noted that the additional paragraph under results in the revised manuscript (page 7, lines 10-11) suggest a total over 100% (i.e. 42%+49.2%+9.1% = 100.3%). The authors should rectify this arithmetical error.

Response:
The words “About 42% of subjects” were changed to “A total of 8623 children (41.7%)” (page 7, line 8).

The sentence “this could have driven the results “(see page 12, line 2) is not clear, as it reads as an incomplete sentence. I assume the authors meant that the differences in responders (parents vs, child) may have influenced their results. If so, it should be state clearly in what direction this could have influenced their results.

Response:
We changed the sentence “This could have driven the results.” to the following: “Accuracy of the replies might differ according to whether parents or children completed the questionnaire. The misclassification of exposure would have given rise to an underestimation of our findings.” (page 12, lines 3-5).

Page 10 lines2-9: although the authors contend that not controlling for potential confounders may explain why some studies did not find an association between SHSe/active smoking and caries, one could argue that such situation may be more likely to support finding a spurious association between SHSe/active smoking and caries. Could the authors clarify.
Response:
We changed the sentences “Many of the published studies showing no association between smoking and dental caries did not control for potential confounders, such as socioeconomic status, diet, and oral health behaviors. Therefore, it is possible that the observed associations were influenced by confounding factors.” to “The heterogeneity of these results among studies might be explained by differences in characteristics, smoking habits, and lifestyle of the populations examined, the study design used, and potential confounders considered. In particular, few of the studies on the association between smoking and dental caries controlled for potential confounders, such as socioeconomic status, diet, and oral health behaviors.” (page 10, lines 6-10).