Author's response to reviews

Title: Tobacco use and caries risk among adolescents - a longitudinal study

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Author's response to reviews: see over
Dear Dr. Wael Sabbah,

Thank you for the opportunity to revise our manuscript “Tobacco use and caries risk among adolescents – a longitudinal study” (title changed to “Tobacco use and caries risk among adolescents – a longitudinal study in Sweden”). The two referees gave several insightful comments and valuable suggestions for the revision. Below, we address the comments provided, point-by-point.

Thank you for considering our revised manuscript.

Best wishes,
Anders Holmén
Ulf Strömberg
Kerstin Magnusson
Svante Twetman

Comments from referee 1 (Sonica Singhal):
Role of tobacco as a risk factor/ risk indicator for dental caries is still not very clear and requires well designed studies. This study is a good attempt to assess the association of tobacco and dental caries among adolescents using a longitudinal study design.

Response: Thank you.

However, before accepting this manuscript for publication, I would recommend some revisions.

Major compulsory revisions:
1) In this longitudinal study, adolescents who started using tobacco at any time after the study begun were considered ever users. Increment of DMFS attributed to an ever user of long time vs. ever user of short time cannot be considered the same. This can be handled in two ways:
   a) Consider ever users and never users at baseline and then assess change in DMFS of both groups after four years. Do not consider adolescents, who started using tobacco anytime after the initiation of the study, in final assessment. This way the exposure will be of considerable time for dental caries to be attributed to it.
   b) If the number of adolescents who started using tobacco after being enrolled in the study is very small, then present those numbers. In table 1, construct four columns with absolute number of ever and never users for each year (baseline, 2nd year, 3rd year, and 4th year). Mention this as limitation and build an argument that such small numbers will not affect the outcomes.

Response: We acknowledged that our classification into “ever users” and “never users” implied misclassification of exposure, likely to have induced bias towards the null. Yet, our results demonstrated a clear impact of reported tobacco use on caries increment during adolescence.
Imposing a relevant time window of exposure, as suggested, can be expected to reduce bias. We now present the requested information in a new figure (Figure 2); dividing the “ever users” into two subgroups: tobacco users at baseline vs. the users commencing later during the follow-up. The discussion on timing of tobacco use in relation to caries development is now elaborated. However, we disagree with the suggestion of revising our presented results, excluding the considerable number of adolescents who started using tobacco at any time after the initiation of the study (according to reported data).

2) Previous studies suggest that the prevalence of snus is more common in the North as compared to the South of Sweden. Halland is on the western coast of Sweden. It will be great if authors can provide detail of ever users as smokers or snus users in Halland. This can provide information about the preference of tobacco usage among adolescents of different social strata in western areas of Sweden.
Response: Thank you for raising this aspect on type of tobacco use. Analyzing possible effect modification by type of tobacco use was not within the scope of the present study. We plan to address this question in future studies. In our project, among the tobacco users, around two thirds were smokers and one third snuff users. No changes made in the manuscript.

3) The authors have adjusted the results for gender; however, it will be interesting to see the change in DMFS, stratified by gender. Authors themselves have acknowledged that tobacco usage is different between boys and girls.
Response: Our primary focus was the co-variation of tobacco-use and socioeconomy. We further stratified on sex. However, reporting on the complex gender influence (which may be differential across socioeconomic strata) would require extensive result presentation. No changes made in the manuscript.

Minor revisions:
1) Please check table 1 for consistency in total never users for base line (8609) and change in dmfs (8610) columns. Check numbers at page 5 (n = 8609) as well.
Response: Table 1 has been corrected.

2) In the section of Statistical methods, line 4, and in the abstract, please make sure if the statement, “proportion with ∆DMFT >0” is correct or should it be ∆DMFT =0; because in table 1, % ∆0 indicates proportion with no new caries.
Response: The statement is correct, although it may appear confusing when referring to tables 1 and 2. In those table, we present the proportion of individuals with no new caries during the observation period (i.e., ∆DMFS = 0), denoted %∆0. We have made changes in the tables; presenting instead the proportion of individuals with caries increment during the observation period, denoted ∆DMFS>0 (٪).

3) In Discussion section, line 3, check “us” which probably should be “use”.
Response: We have made the correction.

Discretionary revision:
1) Use of the context in the title, for example, Swedish adolescents or study in
Comments from referee 2 (Elsa Karina Delgado):
This is an interesting paper which deserves to be published. The use of longitudinal data and a large sample makes it even better.
Response: Thank you.

However, there are a few issues that need to be addressed.
MINOR ESSENTIAL REVISIONS
Introduction paragraph 1
• Authors stated several studies world-wide have denominates tobacco as a risk factor for dental caries; however, only one reference is cited which is not even a review paper.
Response: The sentence is amended to clarify the statement above.
• Also mentioned increased caries rates in smoking young adults; the studies cited are for periodontal patients, that’s how it should be presented.
Response: We have added a new reference for this purpose.

Methods-Study population, paragraph 2
• Four year cohorts, observed in periods: 2006-09, 2007-12? (should it be 2007-10?), 2008-11 and 2009-12.
Response: We are sorry for this mistake. We have made the correction.

Discussion, paragraph 1
• The main finding ... self-reported use of tobacco ...
Response: We have made the correction.

Discussion, paragraph 4
• The description of the methodology must be within Methods, please move the sentence referring to Bitewing radiographs to that section.
Response: We added information on bitewing radiographs in the method section.
• Please expand the point given about the possible explanations for your results.
Response: We have added a possible explanation in the discussion.

MAJOR REVISIONS
Methods
• There is no description of the methodology for caries assessment.
Response: The caries data were scored according to the guidelines to the public dental service guidelines, which were in accordance with WHO. This scoring means that only manifest caries was taken into account. In fact, this information was already given in the manuscript (on page 5).

Methods-Statistical methods, paragraph 1
• Why did the authors summarise DMFS by SiC?
Response: Since caries data were highly skewed, SiC is a relevant measure of caries burden in the high-risk population. Therefore, both caries measures (mean DMFS and SiC) provided adequate information.

**Why did the baseline DMFS was stratified? What is the reference for the cut-off points used?**
Response: Because baseline DMFS was a likely confounder; related to tobacco use and caries development during adolescence. The cut-off values were based on the epidemiological caries data in the Halland region, reflecting no, low and high caries experience. We have motivated the cut-off values in the method section.

**Please provide a reference (or a reason) for using Wilcoxon-Mann-Whitney test instead of regression analysis.**
Response: Outcome data on ∆DMFS were highly skewed, with a high proportion of 0. Model conditions for ANOVA/linear regression analyses were not fulfilled. In the statistical method section, we have added this reason for using the non-parametric Wilcoxon-Mann-Whitney approach.

**Editorial requests:**
We have incorporated the requested information.

**Additional changes made by the authors:**
We clarified the definition of household purchasing power in the method section. Also, the legend to Figure 1 has been clarified.