Author’s response to reviews

Title: Contribution of parental and school personnel smoking to health risk behaviours among Finnish adolescents

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

We thank You and the Reviewers for their very thorough assessment of our manuscript. Their very useful comments have greatly helped us to improve our paper. Our answers and the changes we have made are listed below, point by point.

Reviewer 1 report:

In this study, the authors investigated the role of parental and school personnel smoking on health risk behaviours among Finnish adolescents. This is a well-described descriptive epidemiologic study, based on a large number of participants. The manuscript is well-written and conclusions are interesting. However, below there are some comments that may help to further improve the manuscript.

Major points:

**Point 1.** The Abstract should be completely revised by authors: the second sentence of the Methods section of the Abstract should be re-written; in the Results section of the Abstract “95% CI” should be added to each occurrence of confidence intervals; OR, COR and CI should be spelled out at the first occurrence. Moreover, the Abstract should be modified according to all the following points.

**Our response:** The Abstract has now been corrected as recommended.

**Point 2.** Please add to the Results section findings on smoking prevalence of adolescents, parents and school personnel. Furthermore, please discuss these results in the Discussion section, comparing your findings on smoking prevalence (and for example binge-drinking) with those from previous studies.

**Our response:** In the revised Results section (p. 8) we have now reported all prevalence estimates as suggested and added a discussion on them comparing our results with those reported in previous studies (Discussion, page 11-12, new references 27-33).

**Point 3.** Please simplify the Measure section in favour of the Data Analysis section of Methods, specifying that the multilevel analysis was used in order to consider both individual and aggregate data.

**Our response:** This has now been done (p. 7, last paragraph).

**Point 4.** The use of prevalence rate ratio instead of odds ratio would be more suitable. Alternatively, the odds ratio can be used as an association estimate, but it should not be interpreted as a relative risk because of the high prevalence of the outcome. This should be at least discussed in the Discussion section.

**Our response:** We agree with the Reviewer that odds ratio can only be used as an association estimate when outcome prevalence is high. In our study, the prevalences varied between 9% and 40%. In the revision, we have revised all those sentences that interpreted odds ratios as relative
risks. In the Discussion, we have additionally noted that: ”....we used odds ratios as association estimates. Although they are relative measures, odds ratios should not be interpreted as risk ratios. Given the relatively high prevalence of our outcome measures (range from 9% to 40%), odds ratios tend to provide greater values than estimates of relative risk”. (p. 14, 2nd last paragraph)

Minor points:

**Point 1.** According to the question on “reporting adult smoking at school”, subjects answering “can not say” should be excluded from the analysis.  
**Our response:** This has now been done.

**Point 2.** Please consider to categorize the variable “proportion of pupils at school with low parental education” in four categories instead of five, joining the last two categories (#40%<50% and #50%).  
**Our response:** Combining Your and Reviewer #2 recommendations, we have now reformulated the variable into three groups of equal size. This is in accordance with the other school level variable (school personnel smoking) which is also a tertile.

**Point 3.** Please consider to delete the p-values in Table 1, since, given the large sample size, they are not informative.  
**Our response:** We have done as suggested. We have added a footnote informing that all differences were statistically significant except that for school personnel smoking prevalence.

**Point 4.** Please consider to replace CORs with ORs, considering dichotomous outcomes (for example, according to “smoking during school time” authors could consider ever vs never).  
**Our response:** The large size of study sample allows us to use more graded outcome measures than dichotomy in assessing smoking at school. We prefer this to a dichotomous variable because it taps the concept of smoking intensity. However, with regard to alcohol use, we have analysed and provided both ORs and CORs in the revised version.

**Point 5.** Please consider to delete the last paragraph of the Results section, since readers could have difficulties in understanding it.  
**Our response:** We think it is an important sensitivity analysis and are reluctant to fully delete the paragraph. However, we have now rewritten the paragraph hoping it to be more clear and understandable: "It is possible that boys more frequently report school personnel smoking because they themselves smoke more often during school time for example, on the school backyards. Therefore we ran a sensitivity analysis on non-smokers only. We found that among the non-smokers, the cumulative odds ratio of reporting that personnel smoke at school were 1.16 (95% CI 1.08 to 1.26) for boys when compared with girls. Among boys who were non-smokers, an association was also found between a higher percentage of school staff smokers and higher odds of boys seeing school personnel smoke (cumulative odds ratio 1.49, 95% CI 1.05 to 2.11).” (p. 10, 2nd paragraph).

**Point 6.** Please rephrase and simplify the 5th sentence of the 4th paragraph of the Discussion section: “Adults’ attitudes towards smoking ... green light for smoking”.  
**Our response:** We have now rephrased the sentence as follows: "School staff attitudes towards smoking may be less strict if they themselves smoke. We found that in schools where the proportion of staff smokers is higher, pupils actually see personnel smoking at school. This might be interpreted by the pupils as a "green light" for smoking." (p. 13, 2nd paragraph)

**Point 7.** In Table 2 and 3 please specify that the odds ratios were estimated by multilevel analyses.
Our response: This information has now been added to the footnotes of Tables 2 and 3.

Point 8. Please add to a footnote of Table 2 the list of covariates considered as adjustment in the multilevel analyses.
Our response: In Table 2, the variables were entered simultaneously into the model in order to see their associations with adolescent health behaviours when other covariates were adjusted for. This information has now been added to the footnote.

Point 9. Please carefully re-read the entire manuscript, for the presence of a few typos.
Our response: We have now checked the manuscript carefully and corrected the typos found.

Reviewer 2 report

Overall, the manuscript is well-written. The research question is well defined. The methods seem appropriate and are with some exceptions (as described below) adequately described. The authors link two different cross-sectional surveys of pupils as well as teachers in the same schools as the pupils and use multilevel analyses of the data. The manuscript adheres to the relevant standards for reporting and data deposition. The discussion and conclusions seem to be well balanced and adequately supported by the data.

Major Compulsory Revisions

Point 1. The response rate is much lower in the teachers’ survey compared to in the pupil survey. The authors have not discussed the possible bias due to different response rate. They state in the discussion that these non-response rates seem to be random. However, this statement is speculative and needs more elaboration.
Our response: We agree with the Reviewer that the statement was speculative. In the revised version, we have replaced the sentence with the following one: "One methodological consideration relates to selective non-response. For example, if substantially more smokers in the schools with high proportion of smokers than those in the schools with low proportion of smokers were non-respondents, our findings between school personnel high smoking prevalence and adolescent health behaviours would have been overestimated. However, we were not able to test such a bias with our data." (p. 14, last paragraph)

Point 2. Despite the cross-sectional design the authors partly treat the data as longitudinal, in e.g. writing about “effects” on health behaviour.
Our response: We have now replaced "effects" and other similar expressions with "associations".

Point 3. The authors comment on their significant findings in Table 1. However, a more relevant question is the implication of the differences. Almost all differences are significant due to a high number of pupils in the study.
Our response: According to both reviewers' recommendations, we have now revised the Discussion by commenting the findings related to prevalences and differences between boys and girls in light with other studies (p. 11-12).
Minor Essential Revisions

**Point 1.** Although the label of Table 2 indicates that information is given about perceptions of school personnel smoking, no such data can be found.  
**Our response:** The variable in question was adolescents' perceptions of school personnel smoking. The label has now been rewritten to be more understandable as follows: "Association of individual and school level characteristics with adolescents' health risk behaviours and their perceptions of school personnel smoking. The outcome variable has been renamed to be "reporting personnel smoking at school".

**Point 2.** What was the rationale for using different cut-off points in the categorisation of the school level characteristics?  
**Our response:** Combining Your and Reviewer #1 recommendations, we have now reformulated the variable into three groups of equal size. This is in accordance with the other school level variable (personnel smoking prevalence).