Author's response to reviews

Title: Factors Associated With Commencing Smoking in 12-Year-Old Students in Catalonia (Spain): a cross-sectional population-based study

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

Please find enclosed the revised version of our manuscript entitled “Factors Associated With Commencing Smoking in 12-Year-Old Students in Catalonia (Spain): a cross-sectional population-based study” (Manuscript ID 1400509144369000).

In the present revised manuscript, as Reviewers and Editor required we have made all the revisions suggested by them.

In the new version of our manuscript we have highlighted in yellow the changes made in the text.

Point-by-point response to Reviewers' comments and changes introduced in the revised manuscript are provided below.

Yours sincerely,

Empar Valdivieso-López

(Corresponding author)
POINT-BY-POINT AUTHORS’ RESPONSE TO THE EDITOR’S COMMENTS.

1. Abstract, the first sentence of the conclusion seems to contradict itself.
We have modified the paragraph in the manuscript as follows:
Results indicate that the age of commencing smoking is maintained in spite of prevalence having decreased in the last few years

2. The term transversal doesn’t make sense.
We have modified the text of the paragraph as follows:
The study we report in this paper corresponds to the first transversal section of the 3-year follow-up community study TAB_ES recorded in Clinical Trials.gov under number NCT01048489.

3. Did 16% of subjects not answer the smoking question? Please explain.
The patients included in the analysis were the patients who recorded all the necessary questions to determine whether they belong to a smoker or non smoker cohort. For 15.7% of students the necessary information to determine whether or not they were smokers was not obtained.

4. What are centres?
Grammatical error. We refer to CENTERS and have corrected the word in the text.

5. Page 10. The sentence about the risk of commencing smoking in the future does not seem right, it seems it should be the risk of being a current smoker in the future.
At the editor’s suggestion we have modified the text.

6. Page 11 and page 14. You did not study peer pressure, you measured smoking by peers. There is no evidence that peers pressured the subjects to smoke.
Indeed, we did not study peer pressure but rather peer consumption. For this reason we have modified the following paragraphs in the manuscript as follows:
Page. 11: The behavior of friends has a major influence on adolescents for which reason in both sexes we detected a significant association with consumption in those students who declared that the majority of their friends smoked.
Page.14. We propose modifying the text as follows: For the multivariate analysis (Table 3) PEER TOBACCO CONSUMPTION PREDICTS smoking behavior in both sexes.

7. Page 11, you are studying the risk of being a smoker, not of becoming a smoker since this is a cross-sectional survey. This mistake is made in several places.
We have modified the manuscript. It is true that since this is a cross-sectional study, we are studying the risk of being a smoker. Corrected throughout the text

8.-The last paragraph on page 11 was very confusing.

We have modified the phrase in the paragraph:

A hierarchic logistics model was adjusted where students and schools defined the first and second levels respectively. The aim of this model was to study the determining factors of being a smoker based on personal characteristics but also from the school's characteristics

9.-Controversy is not the right word here in regard to paternal influence.

Certainly. We suggest modifying the manuscript with the following paragraph: Tobacco consumption of parents produces different effects in young people

10.- Page 12. The word consumption should be tobacco.

This has now been corrected

11.-Page 12. The sentence?the question whether?? Makes no sense.

We have modified the paragraph in the manuscript as follows: “If the student considered the possibility of smoking the next year plausible, this was associated with considering being a smoker”,.....

12.-Cooximetry is not useful because there was too much overlap between groups. Out of 33 subjects with high CO levels, 23 were nonsmokers.

Certainly, everything seems to indicate that co-oximetry in our study was not useful to detect child smokers probably because of the characteristics of the group studied as to the pattern of tobacco consumption. Children are experimenting, display irregular consumption, and inhale smoke unequally and these aspects are directly related to CO measurement.

According to Martín V et al, co-oximetry does not appear to be a suitable method to detect smokers in this population. They recommend that in future studies a questionnaire should collect data on passive exposure to tobacco smoke which could account for the false positives found in our study as our population could show low consumption and possible exposure to environmental tobacco smoke that we are unaware of.[*]

[*] Martín V., Fernández D., Ordóñez C., Molina AJ., Fernández H., De Luís JM., Valoración con tres métodos diferentes de la prevalencia de consumo de tabaco en
We also believe that, overall, co-oximetry discriminates smokers from non-smokers (this arises from logistic regression adjusting for other factors: age, perception of what is detrimental, what creates addiction, etc….). Nonetheless, if we look at the raw data (from the descriptive table) the trend is there but it is surprising that there are non-smoker students who have a positive registry of co-oximetry. Given this we can think in 2 different ways:

- They are not actually smokers and have produced a positive test because they are experimenting but we cannot yet consider them as smokers. To conclude, co-oximetry is not a reliable test to discriminate between smokers and non-smokers at these ages.
- These non-smokers are actually smokers. This means they lied to us. This could be a shortcoming of the study. It is reported that there are topics where it is likely that the interviewee lies (religion, drugs, sex, etc..). This would be biased information which is difficult to study.
- Another option is that we have not measured the effect of exposure to environmental tobacco smoke which could account for these differences.

We are studying this aspect in greater detail and depth. At the end of full follow-up we believe we will be able to provide more conclusive data.

We have added to the paragraph “Table 3 reveals…”

14. Table 4. Is offer from friends reversed?
This is an error in the table's label. We have modified this.

15.-Page 15, first paragraph is unclear.
We try to clarify the paragraph as follows:

Having a sibling who smokes duplicates the risk and this association arouses discrepancy between studies. The paper by Nebot et al., observed a relationship; conversely, those studies performed by Gómez-Cruz and Aburto Barrenechea et al. in the North of Spain showed a mild influence [23]

16.-Page 15, the study did not include all schools as 3 dropped out.
We have modified and clarified the text

17.-Did the CO confirm that subjects were telling the truth? You don't address the problems with the disconnection between CO and self-report with 23 self identified non smokers having high CO levels.

Measurement of CO in expired air is a very useful method in the smoker study and their weaning process. It is economic, simple, and innocuous and gives immediate results which can be used to gauge the abstinence phase, as a physiologic method to verify the verbal statement of abstinence and as a positive reinforcement mechanism.
The disadvantage of the method is that its elimination half-life is short for which reason its sensitivity is limited in mild and/or sporadic smokers which would include adolescents. 

We provide a reference bibliography


18.–Conclusion 2–4 in the discussion are not directly relevant to the data from this study.

We propose modifying the conclusions:

1. Early commencement of tobacco consumption is still maintained. In addition, this occurs at early ages in spite of observing a slight decline in the last few years for which reason multidisciplinary preventive programmes should be commenced during the final years of primary education. During the first year of compulsory secondary education (12 to 13 years) 27% have already experimented with smoking.

2. Lower prevalence of consumption in girls compared to boys at this age should be considered to better understand the factors and mechanisms which favour commencing smoking in young people and therefore be able to design better educational strategies.

3. The factors detected upon commencing consumption reveal that we should involve families and the educational community by offering them tobacco weaning programmes because of the exemplary role they play.

4. We should continue to investigate possible factors which in all likelihood are not fully known so as to be able to plan and develop specific programmes in schools and society in general.

18.–You should eliminate the figures as they are not mentioned in the text, explained of helpful.

We have added the graphical references to the text

We would be grateful if you could address the referee and editors comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns.

Please also highlight (with 'tracked changes'/coloured/underlines/highlighted text) all changes made when revising the manuscript to make it easier for the Editors to give you a prompt decision on your manuscript.

Please also ensure that your revised manuscript conforms to the journal style
(http://www.biomedcentral.com/info/ifora/medicine_journals). It is important that your files are correctly formatted.

19.–As this work relates to the trial NCT01048489, please can you add this to the abstract at first decision.

We have added this to the abstract
Parents and students respectively gave their consent formally in writing and verbally (already modified in the text at the suggestion of another reviser).

POINT-BY-POINT AUTHORS’ RESPONSE TO THE REVIEWERS’ COMMENTS.

Reviewer: Joy Johnson

Reviewer’s report:

Factors Associated with Commencing Smoking in 12-Year-Old Students in Catalonia (Spain): a cross-sectional population–based study.

The authors present the findings of an interesting study focused the prevalence of tobacco use in Catalonia. They specifically focus on predictors of smoking and attitudes toward smoking in the future. A unique aspect of the study is the focus on 12 year olds. Often studies such as this focus on older students. The sample is large and representative which is a strength. There are however a number of aspects of the study that require strengthening and I outline these below.

Major Compulsory Revisions

1. The theoretical basis for the study requires development. In particular there is no theoretical background provided in relation to the multilevel analysis.

Since this is a study on community citizens, the analysis units are grouped together in clusters. In our specific case students are grouped together in schools. It seems logical to postulate that students from a same school are more similar to students from the same school rather than other schools and therefore the hypothesis that analysis units arise from an independently distributed random sample is dismissed. Under these circumstances we should analyse the data considering the hierarchic structure. In this case we adjusted a logistic hierarchic model (J Epidemiol Community Health. 2006 Apr;60(4):290-7., J Epidemiol Community Health. 2005 Dec;59(12):1022-8. Review., J Epidemiol Community Health. 2005 Sep;59(9):729-36., J Epidemiol Community Health. 2005 Jun;59(6):443-9. Review.)


2. On page 6 there is reference is made to “the prior study” please clarify what is meant by this.

We agree with the reviser and we have modified this paragraph in the manuscript as follows:

“Parents were notified by letter and students gave their consent to participate. We included in this study all students who attended class the day the survey was performed.”

3. The notion of inter-centre variability in not defined conceptually or operationally.
In multilevel analysis overall variability is broken down into two sources of variability. The variability between the patients in the cluster (in our case schools) and intra-cluster variability (how different are the clusters).


4. The survey was based on a survey developed in Maastricht what steps were made to ensure the valid translation into Spanish? The measurements used in the study require more information – a description of the items used would be helpful. The Hollingshead measure was used to measure social economic status -what evidence is there that this is a valid measure for youth?

The survey was created and used in a European study in which Spain participated and was validated for the whole European population—the ESFA STUDY. See references 1, 6, 8, 9, 10 and 13. In addition, we have added other references which mention the questionnaire.

The Hollingshead index measures socio-economic status by means of weighting the following variables: mother's education, father's education, and each parent's job. It is classified into 3 socio-economic levels based on the following formula:

\[
\text{Hollingshead socio-economic status} = \frac{(\text{Mother's education} \times 3 + \text{Mother's job} \times 5) + (\text{Father's education} \times 3 + \text{Father's job} \times 5)}{2}.
\]

As we are dealing with variables directly related to the family setting in which students live we believe that this index was relevant to use in our population by and large, and considering the age of these children included in a family setting in which the parents' education and jobs have an effect on their socio-economic status.

5. Regarding data analysis it would be helpful to have more information about the model testing procedures. What procedures were used to find the “best model?”

Variables which in the descriptive study had a \( P \) value < 0.2 entered the model initially. Once the model was designed the final model was obtained by means of a backstep algorithm. Finally, the random effect of the school was introduced.

6. On page 11 a protective factor in relation girls’ smoking is discussed. It is not clear what the formula in the brackets represents – please clarify.

There is an interaction between biological sex and a smoker mother. This operation refers to the effect of the interaction: first, the fact the mother is a smoker is 1.76 (both for boys and girls). Independently, the effect of the interaction is 0.48. Therefore, the effect of the smoker mother in boys is an OR of 1.76 but the effect of having a smoker mother in a girl is an OR of 1.76 \times 0.48 = 0.85.

To clarify this we modify the paragraph as follows:

Paternal influence arouses controversy; the fact the father is a smoker does not imply any change in adolescents; conversely, the mother who smokes or who is a former smoker is a risk factor for boys OR 1.76 CI 95%: [1.10 2.84] and a factor which protects
girls from smoking \((1.76 \times 0.48 = 0.85)\); the interaction between a mother smoker and student's sex is significant.

7. **The tables would benefit from the inclusion of the statistical tests.** Following the proposed indications, we have added to the tables references

8. **Table 2 – the foot note suggests there are standard deviations – but none are reported.**

   Corrected in the footnote.
   This note only refers to number of cigarettes a day (which is a numeric variable). All other variables are categorical and we summarize these by means of number of students and the proportion of responses represented in brackets.

9. **Table 3 – please check the odds ratios and confidence intervals.** The confidence interval related to perception that tobacco is detrimental to health does not capture the odds ratio.

   We agree that the final category of variable does not have a statistically effect different to that of the reference category. Nonetheless, Wald statistical contrasts gives overall statistical significance to the variable, doubtless because of the fact that the second category has limits above 1. Therefore, we could add the two categories turning the initial variable of three categories into a two-category variable. Nonetheless, we decided to leave this in the initial categorization given that we observed a gradual estimated effect in the variable which was specifically an ordinal categorical variable. Therefore, we could consider that there really is an effect although this cannot be contrasted because of the low numbers in the superior category.

Minor comments

1. **The paper requires a careful edit.** There are a number of awkward phrases and incomplete sentences. For example, “collated” is used when likely the appropriate term is “collected.” It is probably best not to use the term “diagnosis” when referring to “smoking status.”

   See the modifications in the text

2. **I am not sure what is meant by “basal transversal section” I think this may be a translational issue and wonder if the authors are meaning baseline, cross-sectional data?**

   See the modifications in the text

3. **The abstract mentions the “controversy” related to parental smoking but provides no details.**

   See the modifications in the text.

4. **In the conclusion mention is made of “cognitive” variables – but it is not clear what variables are being referred to.**

   Perhaps the work "cognitive" is not the most suitable although we have modified this conclusion in the manuscript.
We refer to certain characteristics we should study in greater depth and which may be involved in commencing tobacco consumption such as intelligence, academic status, level of self-esteem, type and objective of the group the student belongs to, life goals, control locus.

Level of interest: An article whose findings are important to those with closely related research interests

Reviewer: Kimberly Horn

Reviewer's report:

1. Major Compulsory Revisions
None. I found this paper well written, clear, and to the point. The research question is timely and of great importance, especially in countries where tobacco marketing is increasing. The analysis was well done and novel. I believe this paper will make a valuable contribution to the field as our knowledge of smoking transition in this age group is quite limited. I especially enjoyed reading the discussion and was pleased to read the very complete and informative "applied" approach.

2. Minor Essential Revisions
A few minor suggestions:

1.-On p. 6, there is only mention of youth assent for participation. If parents provided written consent, please note. If not, please explain.
We certainly did not make it clear who gave consent. We have modified the text in the following way: “Parents and students respectively gave their consent in writing and verbally”.

2.-On p. 7, the instrument is described. However, there is no mention of psychometric properties, e.g., validity, reliability analyses. This information should be provided to increase support for the instrument.

Following the proposed indications, we have added to the text references from the study for which the questionnaire used in our study was designed and validated.

The questionnaire used in our study was prepared, validated and used in the ESFA study. Spain was one of the participant countries and the questionnaire was based on a review of the literature, 15 years of work on adolescent smoking behavior and revised according to pilot studies conducted by the NPMs in each country.
We have added bibliographical references regarding the validity and reliability of the questionnaire to the bibliography.

3.-On p. 9, the authors state that some schools dropped out or declined participation. It would be important to know if there were differences between those that dropped out and those that remained. Also, please indicate if there were important differences among those schools that remained. This is addressed briefly in the discussion section, but it would be useful to report any statistical examination of these differences in the results.
Schools did not decline to participate, the requirement to participate in the study was having a school nurse in the centre. Three schools did not have this professional available. In the text we have added that the requirement to be included in the study was having a "Health and school programme" nurse attached to the center. Only 3 of the 32 centers in the geographic area studied did not comply with this condition.

Regarding the inter-center variability a descriptive analysis with the aim of determining whether there were differences between centers could not be performed as we had no information on these schools. Nonetheless, since we are familiar with the characteristics of the country’s social and cultural setting, we assume that the results are not affected by any bias in selection.

4.- Also on p. 9, the study reveals some important gender differences in smokers and also among those who are at risk of becoming smokers. Please expand and speculate on the important gender differences (including those related to culture) in the discussion. Gender differences may have important implications for prevention and cessation programming.

This is the paragraph which refers to the discussion regarding sex. I don't know what else can be added.

In our study the prevalence of tobacco consumption was greater in males. This data is not unanimous, there are authors who did not find sex differences in smoking [8,26,28,29], compared to other researchers who observe more tobacco consumption in females [31]. Boys commence smoking earlier, girls start to smoke later but their tobacco consumption increases quickly between age 14 and 16 and as of this time they tend to stabilize to be equal or higher in males around age 18 [8]. Another factor which might account for this sex difference is the effect of immigration at the expense of males observed in the smoker cohort, immigrants from the Maghreb culture, and rural areas who disapprove of women smokers. This study did not detect any students of Maghreb origin who declared themselves to be a smoker for which reason we believe that the effect of immigration is directly related to a higher prevalence of smoking in males.

5.- Please clarify the use of CO verification among non smokers. Also, expand on why CO is positively correlated with risk of becoming a smoker.

Certainly, everything seems to indicate that co-oximetry in our study was not useful to detect child smokers probably because of the characteristics of the group studied as to the pattern of tobacco consumption. Children are experimenting, display irregular consumption, and inhale smoke unequally and these aspects are directly related to CO measurement.

According to Martín V et al, co-oximetry does not appear to be a suitable method to detect smokers in this population. They recommend that in future studies a questionnaire should collect data on passive exposure to tobacco smoke which could account for the false positives found in our study as our population could show low consumption and possible exposure to environmental tobacco smoke that we are unaware of.[*]

[*]Martín V., Fernández D., Ordóñez C., Molina AJ., Fernández H., De Luís JM., Valoración con tres métodos diferentes de la prevalencia de consumo de tabaco en...
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- They are not actually smokers and have produced a positive test because they are experimenting but we cannot yet consider them as smokers. To conclude, co-oximetry is not a reliable test to discriminate between smokers and non-smokers at these ages.
- These non-smokers are actually smokers. This means they lied to us. This could be a shortcoming of the study. It is reported that there are topics where it is likely that the interviewee lies (religion, drugs, sex, etc…). This would be biased information which is difficult to study.
- Another option is that we have not measured the effect of exposure to environmental tobacco smoke which could account for these differences.

We are studying this aspect in greater detail and depth. At the end of full follow-up we believe we will be able to provide more conclusive data.

3. Discretionary Revisions
None.