Author's response to reviews

Title: Gender differentials in the evolution of cigarette smoking habits in a general European adult population from 1993-2003

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Author's response to reviews: see over
RESPONSES TO

Reviewer's report
Title: Gender differentials in the evolution of cigarette smoking habits in a general European adult population from 1993-2003
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Reviewer: Paul Hewson
Reviewer's report:

General Point: I don't see that many of the earlier points have been covered in the paper.

RESPONSE: We apologize. We hope that we have better-addressed your points now.

Point 1. So the paper is an INFORMAL comparison of gender differences. It seems strange that the comparison referred to in the title (gender differences) is only compared informally when other comparisons are made formally. I don't feel that some of these other formal comparisons are sensible (e.g. point 7). Either the analysis or the title / aims etc. of the paper should be adjusted.

RESPONSE: We decided to present a more formal analysis which is now described in detail in the new Statistical analyses subsections “Trends by calendar year” and “Age effects”.

Point 2. I don't see why these adjustments can't be made to the model - I appreciate it may not be sensible or interesting to make them the prime focus of the analysis. Is 11 years really too short to age adjust the sampled population? I think there should be some justification that age adjustment isn't necessary.

RESPONSE: We have now provided a series of supplemental graphs to address your point. These are essentially Figure 2 stratified by the years of survey subgroups [1993-1994], [1995-1996], [1997-1998], [1999-2000], and [2001-2003]. These stratified graphs show that there are no consistent differences in the age effect results by period beyond those expected from random sampling fluctuations.

Point 3a. I'm still nervous about the simple linear trends applied to these data. The trends really do not appear to be simple lines.

RESPONSE: We agree that, in principle, more complex (e.g., polynomial or other) models could be fitted to the data. But we are also aware that most of the background fluctuations are due to random sampling variations. In other words, they cannot necessarily be treated as true prevalence changes in the population. We therefore believe that it is inappropriate to explicitly model the inherent background survey sampling fluctuations observed in the prevalence estimates with more complexity. Assuming a linear trend is a conservative approximation, but it is also the most plausible one given what is known about the slow modification of population habits or risk factor exposures.

Point 3b. Why do you think multiple comparisons are not a problem. If you fit a lot models to your data, you are more likely to find a spurious significant parameter than if you have one model defined a priori.

RESPONSE: There was a misunderstanding of your original point on our part, for which we apologize. Our previous response was more concerned with the niceties of comparing many means vs. summarizing them all with a single linear slope. Regardless, we now present simultaneous (Working-Hotelling type)
95% confidence bands (CB) adjusted for the number of models (6 for prevalences, 4 for age at initiation) for all the estimated regression lines shown in the figures. We hope that this addresses your point better.

Point 4a. I am not happy with the reply to this question. The data might be "just as reliable" as other similar studies, but this paper has been submitted to BMC Public Health, a fairly general journal. Some indication should be given as to the limitations of the data being used. I can't see how this concept ISN'T subject to recall bias, and this should be briefly acknowledged, with reference to other work where appropriate.

RESPONSE: At the beginning of the Discussion subsection, “Study limitations and strengths” we have added:

“Recall bias in retrospectively reported age at smoking initiation is a potential study limitation. However, it has been found that adult self-reported age at first substance use, as routinely collected via survey questionnaires, is sufficiently reliable for most epidemiological applications [21, 22].”

The 2 new references are:


Point 4b. Your study is also subject to differential recall bias in that you define ex-smokers differently to some other studies. Do you think your definition might be subject to more bias (because people are trying to think whether they stopped 11 or 13 months ago) than other definitions.

RESPONSE: At the beginning of the Methods subsection, “Cigarette smoking outcomes”, we now say:

“Survey participants were post-classified into cigarette smoking outcome subgroups based on their questionnaire responses.”

In addition, in the Discussion subsection “Study limitations and strengths” we have reiterated and added:

“The questions on cigarette smoking habits were included in a larger standardized questionnaire requesting information on a variety of risk factors in addition to smoking, and the study participants were post-classified into the three smoker subgroups solely on the basis of their questionnaire responses. For example, individuals who reported having quit smoking less than one year before their interview were classified as current smokers in the analyses.”

That is to say, the hypothetical situation you posed of a study participant possibly having to decide between reporting having quit at 11 vs. 13 months in order to classify themselves as a former or current smoker was never an issue. In other words, the 12 month cessation definition of former smoker employed in the analyses was never mentioned to the study participant during their interview.

Point 5. There is still no clear explanation of all the models you have fitted in your analysis; it was much easier to follow the models used in your supplemental paper.
RESPONSE: We think that the new Statistical analyses subsections, “Trends by calendar year” and “Age effects”, as well as the new description of the preliminary results that now appears at the beginning of the Results section, provide sufficient enough detail for the reader to understand the various multiple and simple linear regression models that were fitted.

Point 6. I am still interested in CIs, partly because it is good practice, partly because it would be interesting to see whether your model CIs match the data or not.

RESPONSE: Please see our response to Point 3b above about simulataneous 95% confidence bands (CB) which are now shown in Figures 1 and 2.

Point 7. If the linear trends are simple descriptives they should not be given model status with parameter significance stated. I still feel these should be density plots - why would we expect anything resembling a simple linear trend with different ages? You have not given me any reason to believe that it was sensible to fit these models in the first place.

RESPONSE: As indicated above in our response to Point 1, we now present more formalized analyses of age effects, and in our response to Point 2 we have redrawn the age effects graphs stratified by period subgroups. We still do not agree that density plots are needed. As far as why one might reasonably consider examining linearity with age, we still think it is a priori reasonable to ask (e.g.) if men smoke less the older they are, or quit smoking more the older they are, etc. In other words, we think there is as much justification to look at linear trends as to exclude them from consideration. Finally, the supplemental age effect graphs stratified by period show that it is not unreasonable to collapse the results over all survey years combined, as done in Figure 2.

Point 8. I still feel the description in the paper is inadequate.

RESPONSE: To buttress our previous response as well as what we said about the analyses of CPD and pack-years for current and former smokers in the Results (where we say “Data not shown otherwise”), we have now provided supplementary graphical results of the trend and age effects analyses of CPD and pack-years (both log transformed). (Please note: these graphs differ from Figures 1 and 2 and the other supplemental graphs stratified by period in that both the trend and age effects results are shown on the same graph.)

Point 9. Dealt with earlier.

RESPONSE: None.

Point 10. Can you provide any justification for not needing age adjustment?

RESPONSE: We have explained our rationale for checking whether age-adjustment was needed in the new Statistical analyses subsections “Trends by calendar year” and “Age effects”. Correspondingly, in the beginning of the Results, mid-paragraph, we now say:

“Moreover, in the ensuing analyses that were performed after stratification by gender, there was little difference between the crude and the age-adjusted year of survey trend effects (all $p>0.38$) (data not shown otherwise). By design, the gender-specific age distributions of the sample participants were virtually identical across survey years. Thus, the final trend results described below were based on gender-specific simple linear regression models without age-adjustment.”