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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Version: 3 Date: 10 January 2006

Author's response to reviews: see over
10 January 2006

Dear Editor,

The revised manuscript entitled “Gender differentials in the evolution of cigarette smoking habits in a general European adult population from 1993-2003”, by myself, Mr. Julien Salamun, Prof. Alan D. Lopez, and Prof. Alfredo Morabia, is being resubmitted for publication as a Research Article in BMC Public Health.

Our point-by-point responses to the two reviewers’ comments begins on the following page.

In addition, we are sending (either in the online submission or in a separate email) a copy of the cited publication:


which describes the regression methodology that we used to investigate trends in more detail.

The revised manuscript is being submitted exclusively to BMC Public Health. The revised manuscript has been read and approved by all four authors, all of whom meet the requirements for authorship and have no conflicts of interest.

Sincerely,

Prof. Michael C. Costanza
Responses to 1st Reviewer’s report
Title: Gender differentials in the evolution of cigarette smoking habits in a general European adult population from 1993-2003
Version: 2  Date: 6 November 2005
Reviewer: Wasim Maziak
Reviewer’s report:

General
The paper has certainly some major strengths such as the large sample size, random sampling, length of followup, and elaborate procedures to ensure participation of subjects and their replacement. The information presented is interesting and important to public health.
Response:
Thank you.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Methods:

a. the age structure of the survey 35-74 years is a clear limitation since most of the interesting dynamics in the evolution of the tobacco epidemic are occurring among the younger groups. Actually the limitation section in the discussion does not include any limitation.
Response:
We agree. Accordingly, the last paragraph of the Discussion now begins: “On the other hand, the survey focus on adults 35-74 years of age is a clear limitation since other interesting dynamics in the evolution of the tobacco epidemic may be occurring among younger women and men.”

b. (i) Usually time trend analysis of prevalence rate is done on weighted estimates according to a reference population distribution. My understanding is that the authors try to compensate for that with their sampling strategy. However, the sentence on sampling “Stratified random sampling by gender within 10-year age strata is proportional to the corresponding population distributions” is framed in a statement-like manner and is not sufficient to understand the sampling procedure and how it corrects for change in distribution.
Response:
In each cross-sectional survey we employed self-weighted stratified random sampling within the four 10-year age strata 35-44, 45-54, and 65-74 years to ensure that the resulting annual independent samples are reasonably representative of the corresponding Geneva gender-specific population distributions. The population figures are monitored closely by the cantonal government and have been extremely stable throughout the study period. Thus, it is not necessary to correct the self-weighted annual estimates for (nonexistent) changes in the age distributions.
b. (ii) More so, the sampling frame was done for 10-year age categories while some of the comparisons were made on 5-year age categories. I recommend anyway consulting a statistician about this point and whether weighting of rates is needed.

Response:
Although it is true that the representativeness of the 10-year age categories-stratified random sampling does not necessarily guarantee the representativeness of the 5-year age categories used in the analyses shown in Figure 2, in fact the (fairly large) samples stratified by the 5-year age categories were not far off from the corresponding population figures. Hence, we have left Figure 2 intact.

c. The sentence about tobacco exposure questionnaire does not pertain to this study and should be removed.

Response:
The paragraph in which that sentence occurred has been rewritten as follows:
“Each participant receives several self-administered, standardized questionnaires covering exposure to risk factors for the major lifestyle chronic diseases, including cigarette smoking. During a scheduled appointment at a mobile epidemiology clinic (housed in a special bus) the questionnaires are checked for completion by trained health technicians. Self-reported current smoking had sensitivity 88.6% and specificity 87.2% when validated against concentrations of both salivary thiocyanate and expired carbon monoxide [18].”

d. Information was collected about socio-economic characteristics, yet they were not used in the study. Actually the change in the smoking habits among different socio-economic slices of the society is very relevant to the application of the tobacco epidemic model to the target population.

Response:
We agree that SES is certainly relevant, as evidenced by Table 2 in the cited publication:
In order to limit the present work to a manageable size we decided not to report on SES (or any other, except for age) subgroup analyses in this report. (Also, please see our response to the last (Minor) comment below.) Our objective here was to examine gender differences and to evaluate the smoking epidemic model.

e. Information about physical examination is not relevant to this report.

Response:
We agree. Please note that the new paragraph shown above in reply to Comment c has incorporated only what was needed from the previous paragraph which had referred to the physical examination.

f. (i) The definitions adopted are misleading. No need to mix never with ever smoking, just say never smokers are those who smoked less (no need to least) than 100 cig/lifetime.

Response:
This sentence now reads:
“Never cigarette smokers reported having smoked less than 100 cigarettes in their lifetime.”
f. (ii) Usually current smoking is defined as past month smoking, and ex smoking is smoking in the past but not at the time of survey (month of the survey). With the adopted definitions, if I was a smoker who quit 6 months prior to the survey, I will be a smoker not an ex smoker, which is inappropriate, I think. The question about former smoking as it is stated “quite smoking at least one year before the interview” does not allow to calculate the duration of smoking given the age of initiation.

Response:
We understand your point of view and appreciate your example, but we (as well as many others) have opted for a more stable definition of former smokers. Insisting that they have not smoked for at least a year before their interview provides this. Accordingly, the calculated (approximate) duration of smoking (a rough estimate in any case) is then accurate to within a year or so, which is sufficient for most comparative purposes.

g. Lung cancer incidence should be given while discussing the results not in the results, since they did not arise from this study

Response:
We agree. This has been rewritten as follows and now appears as a paragraph 4 in the Discussion:
“The observed smoking decrease among men and the smoking increase among women are in general accord with lung cancer surveillance data collected by the Geneva Cancer Registry. Specifically, lung cancer incidence (per 100,000/yr, age-standardized to the EU population) for the years 1983-1986, 1995-1998, and 1999-2002, respectively, declined from 80.7 to 75.2 to 67.1 in men, and increased from 14.8 to 24.3 to 27.8 in women (http://www.asrt.ch/rgt/ and personal communication).”

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Background: the first statement is erroneous as it is. This statement applied to industrialized nations only.

Response:
We agree. The sentence now begins:
“A review of the literature on changes in cigarette smoking by gender in different populations of the developed world…”

Discussion:
First sentence needs re-writing.

Response:
We agree. The sentence now reads:
“The present data show that current cigarette smoking remained stable in Geneva men and women from 1993 through 2003. This may superficially suggest that cigarette smoking patterns by gender are the same. However, there appear to be fundamentally different smoking behavior processes for men vs. women.

First sentence in para 5 needs re-writing.

Response:
This sentence now reads:
“Different countries vary in terms of where they are with respect to the above four developmental stages.”

As mentioned above the limitations should be stated clearly, such as the age range, response rate, and socioeconomic analysis of trends in smoking.

Response:
We agree, although we view the participation rates as a strength rather than a weakness for this type of survey. Accordingly, the last paragraph of the Discussion now reads:

"On the other hand, the survey focus on adults 35-74 years of age is a clear limitation since other interesting dynamics in the evolution of the tobacco epidemic may be occurring among younger women and men."

We explained in our response to Comment d above that subgroup analyses by SES was not our objective here.

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes

Declaration of competing interests:
I declare that I have no competing interests

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Responses to 2nd Reviewer’s report

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

Version: 2 Date: 5 December 2005

Reviewer: Paul Hewson

Reviewer’s report:

General

The analysis is based on an impressive and thoroughly collected dataset.
Response: Thank you.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. Given your title ("gender differences ") why have you analysed data for each gender separately (your first statement in Statistical Analysis section). Why haven’t you included gender as a term (with interactions where necessary) in your model so that you can make a formal assessment of the role of gender?
Response: Given the large sample sizes by gender and the nature and magnitude of the gender differentials we do not see that there is much to be gained (other than for claiming that men and women are statistically significantly different) by formally modelling the gender differences as suggested. Hence we have not done so.

2. When considering age groups (35-39) at 11 different sampling points, what allowance have you made for the different grouping structure. For example, if something dramatic happened in 1940 that had an effect on age of onset of smokers who were 60 in 2000, but that factor would be absent in smokers who started in 1930 and were 60 when surveyed in 1990. Equally, this factor would be present in
smokers age 50 when surveyed in 1990. I would therefore like to see much more
details of the models used, and an explanation of how these possible confounding
factors have been avoided. I am nervous of graphs (figure 1 and figure 2) of crude
data which have not been produced in a way that is sensitive to these possibilities. In
particular, I am confused by the statement “young men started to smoke earlier than
older men”, as the younger men become older men (agreed, only by 11 years in this
study, but unless you block out the year of the survey in some way you will tend to
mask this effect). I would prefer that statement to be derived from a model parameter
estimated from the data.

Response:
Unfortunately, the 11 years of cross-sectional data for the age range 35-74 years that
we have collected do not provide sufficient overlap of the various birth cohorts to do
any meaningful analyses such as those suggested.

3. Do you really think that a "trend analysis", consisting of a line fitted to 11 years
data is a reasonable way of modelling time series data? Whilst there may be
insufficient data for formal time series methods I did wonder if a simple linear trend
was just too simple for 11 years data. I also felt that these linear fits had been mis-
applied in a few places. (It's not by biggest worry here, but are you running the risk of
multiple comparison problems?)

Response:
We have used a reasonable way to model independent cross-sectional (not time
series) data for each year. The type of linear model, which (despite appearances)
was applied to the individual-level (not aggregated) data, is described in detail in the
cited publication:
We will send the pdf of this article together with our revision.
Multiple comparison problems are not so much of an issue because we are only
evaluating single slopes for determining the statistical significance of corresponding
trends, and then just informally describing the aggregated data to “fill in the picture” a
bit when discussing the results.

4. How reliable is your age of "starting smoking" data. How much might differences in
this figure be due to recall effects- for example if you asked 20 year olds when they
started might you get a
different figure from 60 year olds not because they figures are different but because
memories are different?

Response:
To our knowledge, the type of data that we collected and employed for determining
age at smoking initiation is similar to data that many others have used for the same
purpose, so are just as reliable. A previous publication by our group in the area of
smoking initiation,
Morabia A, Costanza MC, Bernstein MS, Rielee JC. Ages at initiation of cigarette
smoking and quit attempts among women: A generation effect. Am J Public Health
2002;92:71-4,
shows that we have are well aware of this issue.

5. It is not clear to me what analyses have been done; the graphical presentation of
results is poor.

Response:
Please see our response to your Comment 3 above which addresses these issues.

6. It is becoming established practice in many medical journals to quote confidence
intervals. On the lower panel of figure 2 it would have been most useful to have
confidence intervals plotted. I was curious as to what you make of the 60-64 smoking initiation age?

Response:
We did not plot the (very narrow due to large sample sizes) confidence intervals in the figure for visual clarity. We are not sure if there is any meaning to the “60-64 smoking initiation age” you pointed out beyond background random sampling fluctuations. Also, as indicated in our response to your Comment 2 above, our cross-sectional data do not permit much disentanglement of the results by birth cohorts to illuminate this any further.

7. The upper panel of Figure 2 should be some kind of density plot and I fail to see the point of the linear model fits. I can't read the key (legend) properly, but the group represented by grey bars (Former smoking???) look like they have a peak at 50-54 (a bit like a very spread out and slightly assymetric bell curve). So why is it sensible to fit a linear to these data. Given the title of the paper (Gender differentials), where can I find a model that provides a formal comparion between the two groups?

Response:
We disagree that a density plot is needed here, although one could be produced. What is shown are grouped bar-charts, and the linear fits to the prevalences of current, former (yes, the grey bars), and never smoking were meant only as simple descriptive approximations, not as final model “solutions”. We agree that more complex methods could be used to model any nonlinear trends, but we chose not to do so more for clarity than for any formal statistical reason.

8. The details given on your analysis of Cigarettes Smoked per day are inadequate. What models have you fitted, what are the parameter estimates, do you have any graphical or tabular summaries of the results.

Response:
We performed the same types of analyses for these data as described in our response to your Comment 3, but we analyzed the log data and translated the results into geometric means as described in the last sentence of the Methods, which was and remains: “For both CPD and pack-years, log-transformed data were analyzed and the results were expressed as the geometric mean weighted over all smoking episodes.”

9. What is the point of the lung cancer incidence data?????????? What's the point of your comments on socio-economic status - might that be another confounding factor in this study????

Response:
Please see our responses to the 1st reviewer’s Comments g (lung cancer incidence) and d (socio-economic status), where both these issues were addressed.

10. I wondered whether (however formally) you should carry out some age-adjustment. In particular, might you have lost more heavy smokers in your older age groups because of smoking related illness which would bias the results slightly (I appreciate you might not be able to correct this formally, but you could acknowledge the potential problems).

Response:
We did not perform any age-adjustment because the annual age distributions remained virtually identical for all 11 survey years. Also please revisit our response to your Comment 2 regarding the limitations of our cross-sectional data.
Legend missing on figure 2.

Response:
This was included previously together with the legend for Figure 1 on a separate page. We repeat it below for clarity:

“Figure 2. Cigarette smoking habits across five-year age subgroups by gender (6,164 men, 6,107 women) in Geneva, Switzerland, 1993-2003. Top panels: prevalence of current, former, and never cigarette smoking. Bottom panels: age at smoking initiation for current and former cigarette smokers.”

Discretionary Revisions (which the author can choose to ignore)
What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No
Declaration of competing interests:
I declare that I have no competing interests