Author's response to reviews

Title: Gender and age differences among current smokers in a general population survey

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

thank you for giving us the opportunity to resubmit a revised version of the above mentioned paper. Please find attached the manuscript after our revision according to the comments of the reviewers. We considered each of the comments very carefully. Please find in the following text of this letter two lists of changes we made according to the comments of reviewer 1 and reviewer 2.

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With kind regards

Ulrich John
(on behalf of all authors)
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Title: Gender and age differences among current smokers in a general population sample

List of changes made

Comments of reviewer 1

The comments of the reviewer are numbered and written in italic. Our responses to each comment are written in plain font.

We are grateful to the reviewer for his very helpful comments. In our view, the revised version of the paper has become clearer and more informative.

Question 3
1. Is anything known about the persons with missing data?

Page 5: We analyzed whether the 30,769 individuals with missing data for smoking status differ from the 179,472 who provided data for smoking status with respect to gender, age, disease (last 4 weeks). Only a restricted subset of variables was available for this analysis, and we selected variables that we felt would provide valuable information: gender, age school education and whether the individual has been diseased four weeks prior to the interview. This analysis showed that there were no differences between subjects with and without information about the smoking status. The criterion for a difference was an effect size measure because this appears more informative than significance testing. Due to the large sample sizes very small differences were significant according to the \( \chi^2 \) tests used. These differences were not of practical importance and did not indicate sample selection bias.
2. There are doubts on the validity of answers on smoking status for the highest (age groups). E. g. Van de Mheen (1994) has suggested that elderly persons may misclassify their smoking status. Has this been checked?

Page 10, para 2: We now have included an additional paragraph on misclassification in the Discussion section, and we now cite Van de Mheen & Gunning-Schepers (1994). Indeed, there may be reporting bias of the smoking status among the elderly due to a long time since smoking cessation, especially when the individuals had smoked only a short period or a low number of cigarettes per day during their life. Furthermore, general memory deficits may play a role. Unfortunately we had no data to check the validity of the smoking status variables. We rely on the evidence revealing that the proportion of smokers who deny smoking in survey studies may be negligible and does not change the results according to smoking status (reference cited in the paper: Vartiainen et al., 2002). This has also been concluded by the SRNT subcommittee on biochemical verification of self-reported tobacco use and cessation (SRNT subcommittee on biochemical verification, Nic Tob Res, 2002, 4, 149-159). However, the general population study of Vartiainen et al. included only the age range of 25 to 64 years. They found that there were no differences by age according to self-reported non-smoking when serum cotinine was higher than 10 ng/ml.

Question 4
1. I miss the population size numbers. Thus, I know the number of current smokers as a proportion of the number of ever smokers and the number of ever smokers, but I do not now the latter number as a proportion of the total population number.

We had considered to include that information; however, we propose not to include these figures (n per age group, proportion of current smokers per age group) for two reasons: First, the proportion of current smokers per age group provides mixed information. It is influenced by the number of persons who had died from smoking or other causes and by smokers who have stopped smoking. We therefore hesitate to provide this biased information. Second, we wanted to refer concisely to the question of smoking cessation that is relevant from a public health point of view. A further minor argument was to keep the tables simple and readable. Altogether, we propose to focus on the public health aspects of not having stopped smoking, i. e. being a current smoker.
2. In Table 3 part 1 the choice of the age class 10-19 as the reference class does not work well, the absolute values become very small for the highest ages.

We chose the age group of 10-19 as the reference for two reasons: First, 76.5 % of the male and 68.4 % of the female ever smokers started smoking at the age of 10-19. Second, the odds ratios tend to decrease by age. Thus, in our view, it makes sense to take 10-19 years as the reference. Furthermore, we found it difficult to decide on criteria for an alternative reference group among the several age groups.

3. I was slightly confused, are these odds ratios women compared to men, or age classes compared within gender?

The odds ratios “Age by gender” show the interaction of gender and age group. It shows e. g. that 80 to 89 year old female ever smoker had an OR of 4.3 for being a current smoker compared to male ever smokers controlling for the other variables denoted in Table 3. We now provide a clearer description in the text on page 8, para 4.

Question 5
1. I miss some references to (hopefully available) older data on smoking prevalence rates in Germany.

Unfortunately, data older than 1989 were not available for Germany that are comparable in data gathering methods, i. e. gathered by the Mikrozensus. Comparable data from the Mikrozensus are available that were gathered in 1989; however, we did not present these data for two reasons: First, the 1989 data have not yet included the eastern part of Germany. Second, the time difference of 10 years between 1989 and 1999 does not seem sufficient to us to describe differences in smoking status by gender-specific age groups.

2. I thought there were relatively large smoking behavior differences between formerly Western and Eastern Germany women.

We tested this hypothesis with our data. The smoking status of residents aged 10 or older in East and West Germany did not different when effect sizes were considered. The proportion of current smokers among men was 34.0 % in Eastern Germany (the states of Brandenburg,
Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt, Thuringia) and was 32.3 % in West Germany (Likelihood Chi² 44.1; p <0.001; Cohen’s w .02, i. e. no effect). The proportion of current smokers among male ever smokers was 58.7 %, both in East Germany and in West Germany. Among East German women, 19.5 % were current smokers compared to 21.8 % among West German women (Likelihood Chi² 151.2; p <0.001; Cohen’s w .04, i. e. no effect). The proportion of current smokers among female East German ever smokers was 65.7 % and among female West German ever smokers 63.7 % (Likelihood Chi² 8.6, p < .01; Cohen’s w 0.02, i. e. no effect). We propose not to include these findings into the results section because we do not want to overload the paper with phenomena that are too specific for German history.

**Question 6**
1. I was slightly confused by the phrase “proportion of individuals who maintain smoking”. *The cross-sectional data set does not allow one to check this at the individual level.*

Page 2, Abstract: We changed the phrase to “current smokers”.

- End of list -
Comments of reviewer #2

The comments of the reviewer are numbered and written in italic. Our responses to each comment are written in plain font.

We are grateful to the reviewer for his very helpful comments. In our view, the revised version of the paper has become clearer and more informative.

General

1. Unfortunately the authors do not address the proportion of present smokers within age groups, which would give another perspective on the data and the preventable proportion of smokers.

We had considered to include that information; however, we propose not to include these figures (n per age group, proportion of current smokers per age group) for two reasons: First, the proportion of current smokers per age group provides mixed information. It is influenced by the number of persons who had died from smoking or other causes and by smokers who have stopped smoking. We therefore hesitate to provide biased information. Second, we wanted to refer concisely to the question of smoking cessation that is relevant from a public health point of view. A further minor argument was to keep the tables simple and readable. Altogether, we propose to focus on the public health aspects of not having stopped smoking, i.e. being a current smoker.

Major compulsory revisions

1. Although the authors mention the problem of cohort effects in the discussion section they do (not?) thoroughly deal with this problem. Female smoking histories in Germany follow different lines than males’. There are completely different social gradients for the different cohorts. Furthermore there is no mentioning of the prewar activities on smoking prevention especially for women during the Nazi regime. The authors should indicate how these factors may have influenced their results, discussing the problem of confounders (including social class) more intensively.

Page 9, para 3: We now discuss in more detail how this may have influenced our findings. Furthermore, we now discuss further confounders including social class.
2. 10 years age groups look rather large in light of the huge sample size.

We agree; however, first, among those aged 90 or older there are less than 100 male ever smokers. Second, the 10 years age groups show clear and consistent results. Third, the tables seem to be better readable when 10 years age groups are presented instead of 5 year age groups.

3. Although they mention the high proportion of missing values, there is no indication whether these occur differently with respect to gender.

Page 5: We analyzed whether the 30,769 individuals with missing data for smoking status differ from the 179,472 who provided data for smoking status with respect to gender, age, disease (last 4 weeks). Only a restricted subset of variables was available for this analysis, and we selected variables that we felt would provide valuable information: gender, age school education and whether the individual has been diseased four weeks prior to the interview. This analysis showed that there were no differences between subjects with and without information about the smoking status. The criterion for a difference was an effect size measure because this appears more informative than significance testing. Due to the large sample sizes very small differences were significant according to the chi² tests used. These differences were not of practical importance and did not indicate sample selection bias.

4. The same applies for the relatively low response rate.

Page 5: When looking at the participation rate, we have to consider that the Mikrozensus included a core part of mandatory questions and additional questions which were answered on a voluntary base. The Mikrozensus includes a 45 % random subsample of households in which the members receive voluntary questions. A limitation of the voluntary part is that exact information about the participation rate is not available. We only have the information that 45.8 % of the total sample of individuals answered voluntary questions. A foot-in-the-door effect may play a role, i. e. that a subject who already takes part in the mandatory survey may be more likely to also answer the additional voluntary questions. This may result in a higher response rate than in completely voluntary surveys. The proportion of 45.8 % has been
calculated by us from the scientific use file and refers to a 70 % random subsample of the total sample.

5. Another point that deserves some consideration is the type of sampling. This has been a household survey that overrepresents singles in the sample. Even if this has been corrected for (which is not mentioned), there must be a discussion on whether and how this may influence the results, given the fact that women become older than men.

Page 6, para 2, and page 11, para 2: Different kinds of bias may play a role in this respect. A household sample may overrepresent older women or individuals at young adult age. We compared the age distribution of men and women who had answered the smoking questions with the age distribution of the general population as provided by the official general population data of the Federal Statistical Office. Our final sample is representative for the general population of Germany aged 10 or older with respect to gender and age: In the 10 year age groups, the maximum deviance of the proportion of women in our final sample from the proportion of women in the same 10 year age group of the general population was 2.4 percentage points (mean deviance: 1.0 percentage points). The maximum deviance in the distribution of gender over the 10 year age groups was 1.3 percentage points (mean deviance: 0.6 percentage points) among women, among men the maximum deviance was 1.7 percentage points (mean deviance: 0.7 percentage points).

6. In the last para of the results section the authors deal with ORs as if they were relative risks. This is not correct and should be changed.

Page 8, para 4: This is rephrased now.

Minor essential revisions

1. It would be informative to see the differences between males and females (the deltas) in Tables 2 and 3.

Table 2: We have now included this information in Table 2. In Table 3 however we only show the results of the logistic regression. We will include mean differences if it is required.

- End of list -