Author's response to reviews

Title: Smoking differences between employees in faculties of the University of Tartu, Estonia, and changes during the country's transition

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Version: 2 Date: 27 October 2010

Author's response to reviews: see over
Dear Sir/Madam

Please find enclosed the revised version of the manuscript entitled “Smoking differences between employees in faculties of the University of Tartu, Estonia, and changes during the country’s transition”.

We found the referees’ comments very helpful and have edited the manuscript according to their advice. We hope it could now be accepted for publication in BMC Public Health.

The detailed responses to the referees are below.

Looking forward to your answer,

Yours sincerely,

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Responses to the referees (marked in blue)

Reviewer's report

Title: Smoking differences between employees in university faculties in Estonia and changes during the country's transition

Version: 1 Date: 5 September 2010

Reviewer: Anu Kasmel

Reviewer's report:

Review of the manuscript

“Smoking differences between employees in university faculties in Estonia and changes during the country’s transition”

submitted by Rauno Heikkinen, Jana Kivastik, Peet-Henn Kingisepp, SimoNäyhä

The text is well written and easy to follow. The study is relevant to the audience and it addresses important problem not only for the particular institution in particular country but also for the larger health community especially for health promotion professionals in the countries experienced rapid transition.

The strengths of the paper is the solid list of references and its focus to the important stakeholders group in health promotion point of view. The article adds to the existing literature on the subject in question.

Review questions:

1. Is the question posed by the authors new and well defined?

The research question posed is clear, concise and it is well articulated. The background chapter in the article builds a logical case and context for the problem statement.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

   The design of the study is mainly plausible and appropriate for the research question – it is well defined and clearly described. However, there are two questions.

   1) there is mentioned that questions for respondents in two studies were not completely identical – it would be clarifying if this differences would be described,

      Answer

      There is now a short description (in Questionnaires subsection) about how the questions differed – just a minor difference. This is also discussed (Discussion, 2nd paragraph)

   2) as there are more determinants of smoking behavior than just awareness and education, I would suggest authors to mention why these have left out in adjustment. Moreover, the authors could add the short description of the non-
response.

**Answer**

Our aim was to identify and quantify the smoking differences between faculties, independent of age and occupational structure which were considered as major potential confounders. The purpose was not to “explain” the differences, which would have required more data. We had data on marital status which could have been used as an explanatory factor, but stratification by marital status caused too much splitting of the data. This is shortly explained in Data analysis.

We added some details to the description of response rate (subsection Characteristics of subjects). Unfortunately, we had no opportunity to examine the non-respondents by a telephone interview or otherwise. This remains a weakness and is shortly commented upon in Discussion (2nd par).

3. Are the data sound and well controlled?

The sampling procedures are sufficiently described. However in some faculties the sample size is quite limited. I would suggest to make and describe the power control.

**Answer**

It is true that the numbers were small in some faculties, which sometimes led to inaccurate difference estimates. We think there is not very much we can do to amend this. The faculty of theology (the smallest faculty) was excluded for this reason but we did not want to exclude any other faculties.

We are not sure if we understood correctly what is meant by power control. We assume the issue is about power analysis. We could not consider any power analysis before the “sampling” was done, since no random sample was drawn, rather we surveyed the entire target population (all UT employees). It is true the power remained low in small strata; this is commented upon in Discussion (4th par).

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Yes it does. Results are organized in a way that is easy to understand. They are presented in context and effectively. Tables are used judiciously and agree with the text.

5. Are the discussions and conclusions well balanced and adequately supported by the data?

Discussions of the results are basically appropriate. The conclusions follow from the design, methods, and results; justification of conclusions is well articulated. I would suggest to add discussion on possible influence of the non-identical questions to the results.

**Answer**
The minor difference in smoking questions is shortly discussed (Discussion, 2\textsuperscript{nd} par).

6. Do the title and abstract accurately convey what has been found?

The title generally accurately convey the content of the study – however I would suggest to add to the title the name of study university as there are many universities in study country and data are not unconditionally generalizable to the other universities.

Answer

The name of the university is now included in the title, and the problem of generalizability is mentioned (Discussion, 1\textsuperscript{st} par.)

The abstract is structured well (background, methods, results, conclusions) and congruent with the text. The conclusions in the abstract are justified by the information in the abstract and the text. However the year of the study could be mentioned in the abstract for accurate understanding of the findings.

Answer

Years of the surveys are mentioned in the abstract

7. Is the writing acceptable?

Reviewer is not native English speaking person and will not take the authority to assess the quality of the language. However the reviewer would ask the authors to check the references as there are several defects.

Answer

We have added six references, one of which appeared this year (Barengo et al. 2004, Helasoja et al. 2002, Helasoja et al 2006, Lipand et al 1992, Vartiainen et al 2002, Weizer at al 2010). The reference list was checked against the original publications.

There is no apparent conflict of interest.

In conclusion:

Reviewer suggest editors to accept the manuscript after minor essential revisions, which the authors can be trusted to correct.

Sincerely

Dr. Anu Kasmel

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

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests
Reviewer's report

Title: Smoking differences between employees in university faculties in Estonia and changes during the country's transition

Version: 1 Date: 27 June 2010

Reviewer: Frida Eek

Reviewer's report:

The study aims to illustrate patterns of change in smoking habits during Estonia’s first transitional decade, by analysing two cross sectional surveys measuring smoking habits among university employees. It is an interesting topic with mostly relevant data sources. However, the methods need to be somewhat more closely described and some weaknesses should be discussed. The report, interpretation and discussion of results also has to be revised in order to clarify which differences that was tested and found to be significant and not.

Major compulsory revisions

Abstract

I do not agree that 1930/2940 persons were surveyed, since you do not have information from all of them. The actual number of participants, i.e. persons you have information about, was 1441/2117.

Answer

The numbers of respondents have been corrected. We did exclude respondents with missing data on faculty from the final analyses, which left 1390 respondents in 1992 and 1790 respondents in 2003 (mentioned in Abstract and Data sources). Out of these, 3-4% did not answer the smoking questions; this is mentioned in subsection Overall trends and is also readable from Table 2.

Methods

Considering the aim(s) of the study, why include two new faculties in the second survey? How is the group “others or missing” (faculty) treated in analyses? Are they even included? If not, number of participants should be adjusted.

Answer

The first aim of the study (mentioned in the Background section) was to identify any smoking differences between faculties in each year. It is therefore necessary to include all 12 units existing in 2003, even though they were only 10 units in 1992 (Figure 1, Table 2).

The second aim was to study the changes in smoking patterns. This has been done in Figures 2 and 3 which, of course, only include the 10 faculties/departments existing both in 1992 and 2003.
The numbers of respondents with missing data are in Table 1. Respondents with missing data on faculty and those working in separate institutions were excluded. This should be clear from the text (Data sources) and from Table 2. The numbers of respondents are now revised not to include persons with missing data (Abstract, Data sources).

All analyses appear to be gender stratified? Describe that in methods. What is actually measured in analyses, current smoking or ever smoking? How is the “quit ratio” treated or used in analyses?

Answer

The Data analysis section now states explicitly that all analyses were conducted separately for men and women.

We focused on daily smoking, but information on ever-smoking and quitting was also needed, since they affect current smoking rates. Therefore, all smoking measures are shown, first in form of crude empirical percentages (Table 1) some of which are also cited in the text. However, we have now added full adjusted analyses of all smoking measures as supplementary material (Supplementary Tables 1 and 2). The Results text has been modified accordingly.

Quit ratio was defined as proportion of ex-smokers out of all those who had ever smoked on a regular basis, multiplied by 100. In the multivariate analysis, quitting (1/0) was the response variate, and the analysis was restricted to those who had ever smoked regularly.

Results

Much of the results, such as quit rate, refer to plain descriptive percentage numbers (from table 2) without any analytical presentation. It is generally difficult to know which differences are significant and not, since no distinction is made in the text between significant and non-significant differences. It is not clear which differences are tested statistically and showing significance, and what refers to descriptive numbers. Quit ratio is referred to, without any analytical results presented.

Answer

We have added full adjusted analyses of ever-smoking and quitting (Supplementary Tables 1-2, see above). Crude percentages and quit ratios are referred to in subsection Overall trends. Analytical results are explained in subsequent sections, with reference to Tables 1-2 and Figures 1-3, and to Supplementary tables.

From figure 1, there appear to be no significant differences at all between women in different faculties (either survey 1 or 2), while you write “only weakly
associated”.

**Answer**

We were not looking for “significances”. We were looking for difference estimates accurate enough to differ from the baseline with a reasonable certainty. The description of the finding has been modified accordingly (subsection Smoking differences between workplaces / Women).

Regarding change in smoking prevalence between survey 1 and 2, few significant changes was found among women (only in four faculties/work places), judging from CI’s in figure. This is not discussed, you only write about “differences between -10% to +3%.

**Answer**

The finding is now stated in some more detail (subsection Changes in daily smoking…)

Beta coefficients from figure 3 are also non significant (not clearly stated when you interpret what the b indicate), etc.

**Answer**

The beta coefficients for consistency between genders (two lower panels in Figure 3) are too imprecise. This does not apply to beta coefficients in the two upper panels (differences between the surveys). This is now stated explicitly in subsection Consistency of smoking differences.

In summary: be clear about which differences that are tested significant and not!

**Answer**

We are reluctant to use significance testing, rather we use confidence intervals. This conforms to the current guidelines for reporting observational studies (International Committee of Medical Journal Editors 2010, Vandenbroucke et al. 2007). The growing criticism against “testing” since the 1980’s strongly discourages the use of significance tests (e.g. Langman 1986, Sterne 2001). We have therefore not reported p values. Especially regarding Table 1, p values would have been misleading anyway, since independently of “testing” results, the differences could have been biased by age and occupational class. It is the adjusted analyses which make allowance for these factors.

**Discussion**

Some faculties, e.g. law and sports faculties, includes very few persons, especially when gender stratified. This is not at all discussed.

**Answer**
This is now commented upon in Discussion (4th par)

The use of medical faculty as reference may be discussed and problematised. Since men in medical faculty smoke to a (relatively) higher extent than expected, it may indicate problems with representativeness. The gender comparison of faculty differences may also be confused by the fact that women in medical faculty smokes less than others, while men in medical faculty smoke to a higher extent than most others.

Answer

The choice of the reference faculty is arbitrary. The medical faculty is a good choice because of its largest size. Selecting any other faculty as reference would not change the results.

The issue of representativeness: Obviously, any smoking difference could be due to selection, but this is difficult to control. One could imagine that selection works differently in the medical faculty than elsewhere, but how exactly this would happen, remains conjectural. If the referee means that the relatively high prevalence of smoking among the medical faculty men could be due to selection (e.g. greater propensity to answer among smokers), one could equally well assume that the contrary is true. We therefore think any assumptions on how selection would work here would remain too speculative.

We did not conduct a formal analysis of gender differences, we just made separate analyses among men and women.

The possibility of selective participation is referred to in Discussion (2nd par). We think appropriate caveats regarding this are given to the reader.

Discretionary Revisions

Methods

P 6, data analyses: the use of term “change between surveys” indicates a follow up of a constant cohort. I would rather call it a “difference” between measurements/ time points.

Answer

We have changed the phrasing (subsection Data analysis), thank you for the advice.

Did you consider using logistic regression? Presenting (adjusted) OR with 95% CI would, at least to me, be a more pedagogical way to present results.

Answer
Yes, we did. This would be equivalent to using a generalized linear model with a logit link function. However, using identity link with binomial response (“binomial regression”) is better here, on the following grounds: (1) We viewed our study in the framework of the “smoking epidemic” (see e.g. Ferrence 1996) which is a theory on the rise and fall of smoking in the population. This theory describes smoking in terms prevalence figures (e.g. percentages of daily smokers). Hence, it is most appropriate to use a method which deals with smoking as absolute percentages and its changes, e.g. an increase of 10 percentage points (from 20% to 30%, for example). The method we use does this (this is explained in the Data analysis section). Logistic regression would result in odds ratios, which are relative measures. In this context, it is the absolute, not relative changes that matter. (2) Logistic regression is based on modelling of odds, not probabilities (percentages, prevalences), which alone make this approach less attractive here than the method we actually used. And we focused on smoking differences (not ratios); differences of odds would not make sense.

Discussion

The conclusion is too long.

Answer

We did our best to condense the conclusion.

Figures/tables

Figure legend: “Solid lines are linear regressions of the differences and dashed lines their 95% confidence bands” I see no dashed lines?

Answer

We are sorry if the dashed lines were not visible in referee’s copy of the manuscript. We revised Figure 3 to amend this; in the pdf file the lines are clearly visible.

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

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

References

