Author's response to reviews

Title: Overweight, physical activity, tobacco and alcohol consumption in a cross-sectional random sample of German adults

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Version: 2 Date: 29 May 2006

Author's response to reviews: see over
Point-by-point response to the reviewers’ comments:

We are very thankful for the comments of all three reviewers and we have incorporated most of their suggestions. We also feel that the reviewers’ suggestions helped improving the manuscript and strengthening its meaning.

Reviewer 1:

General:

A large study with interesting results.

Major compulsory revisions:

There are no clear targets. There are many examinations but there has been no attempt to correlate them. For example, the liver ultrasound findings have not been correlated to alcohol consumption, obesity or hepatitis. Serum lipoproteins have not been correlated to physical activity, smoking, obesity or dietary intake.

Correlations of liver sonography to obesity, alcohol consumption, serum lipoproteins and other factors, correlations of physical activity to obesity, alcohol consumption, smoking, serum lipoproteins and other factors as well as correlations of serum lipoproteins to other factors have been added to the manuscript. A multiple logistic regression was performed and odds ratios were given were applicable.

The population sample (4000) seems to be very high in relation to the reference population (12475).

The initial aim of the study was the acquisition of data on the prevalence of E. multilocularis infection in southern Germany. E. multilocularis infection is a rare disease. In order to possibly find at least a few patients a large number of subjects was needed.

Did the authors perform any power calculations to estimate the sample size? It is also unclear what the terms “non-selected” and “collective” mean. There is no explanation in the Methods section.

A power estimation was performed. Calculations for sample size were made according to reference data on the prevalence of antibody titers for E. multilocularis in a rural population in southern Germany. The estimated prevalence of increased antibody titers for the study region was about 1.0%. The state health authority calculated a sample size of about 2600 persons to be able to show a difference
between the study population and the rural reference population taking a 5% sample error and a power of 80% into account.

The terms “non-selected” and “collective” have been changed to “randomly selected” and “study population” or other appropriate expressions.

*It is stated that the questionnaires included questions on nutrition but no data are presented. There has been no attempt to validate the data-questionnaires on physical activity and alcohol consumption. Alcohol consumption is usually under-reported.*

The questionnaires included data on vegetarian diet, vitamin supplementation, consumption of sweets, sugary drinks and coffee and tea intake. In the context of this manuscript this data didn’t seem to be of a very high relevancy. Apart from the consumption of sweets and sugary drinks (which we added to the manuscript) we therefore omitted the other nutritional parameters from the manuscript.

The questions referring to alcohol consumption were taken from the MONICA questionnaire. They are described in more details in the methods section and an additional reference has been added. The questions on physical activity were also described in more detail for better comparability.

*The liver enzyme γ-GT has not been correlated to alcohol consumption.*

This correlation was added to the manuscript (table 8).

*In the introduction almost a whole page is devoted to Echinococcus which is not related to the targets of this paper. (It is understandable however that it was the original aim of the study).*

We agree with the reviewer that the description of the study’s original aim forms a too lengthy paragraph. The introduction was shortened accordingly and the number of reference was reduced.

*The discussion in general tends to repeat results and there is limited literature comparisons from the country itself and abroad.*

We tried to improve the discussion by including the following international studies into the literature comparison:


The article needs more sophisticated statistical correlations.

Further correlations have been added to the manuscript (see also above).
Reviewer 2:

General:

*The paper brings forward important information for the local health authorities and tells us that the German population to a large extent behaves similar to the rest of the industrialised world*

Major compulsory revisions:

*There is no reference to the standardised questionnaire used, except that it includes instruments developed for the MONICA study. The standardised questionnaire should either be described in more detail, or if generally available (website?), referred to specifically.*

A more detailed description of the questionnaire was included into the manuscript text. One additional reference [12] describing the standardised questions referring to alcohol intake in more detail has been added to the manuscript text.

*Issues such as physical activity (as the authors correctly state) are difficult to compare between populations due to different methods. Still, one would like to know what the term standardisation implies, and whether the nutrition, alcohol and physical activity items are directly comparable to for instance MONICA data.*

The questions referring to alcohol a directly comparable to MONICA (this information was added to the manuscript). The other issues from the questionnaire are described in more detail in the manuscript text to allow better comparability to other studies,

*There are a few convoluted sentences under Discussion (p17) such as the one starting with "In our collective subjects in the 41-65 year age group..." there seems to be a word or two missing. The sentence should be rephrased.*

The sentence has been rephrased: In our population sample 59.2% of subjects in the 41-65 year age group reported 0-10 hours of physical activity per week.

*Also the term "collective" sounds outlandish, almost like something from the 1960-70s.*

The term “collective” has been changed throughout the manuscript. (See also reviewer 1).
Minor Essential Revisions:

Table headings: avoid the word Collective. An alternative heading in table 1 could be:
The gender and age distribution 2445 German adults aged 10-65 years in 2002.

The table heading has been changed and the word “collective” was omitted.

By the way table 1 includes also those aged 10-17 but the paper only deals with those beyond 17. The authors should either omit the youngsters from the table or do some changes in the article.

The age group of the 10-17 years old has been omitted from the table.

Table 3 is about physical activity both at work and in leisure time, if I understand the text correctly. This should be further clarified in the text, and probably indicated by a footnote in the table.

Following the reviewer’s suggestion the text has been clarified and the table heading of table 3 has been amended accordingly.
Reviewer 3:

General:

The paper reports original data on prevalence rates of several important health risk factors in a random sample from the adult general population residing in the south-west of Germany. A valuable database with a fairly large sample size seems to be a strength of the study. The cross-sectional survey comprises extensive examinations of the participants including laboratory analyses of blood samples, anthropometry, ultrasonography, and personal interviews.

However, in my opinion the research question of the paper is not very well defined, leading to unfocused descriptions of several data and study details, and to vague conclusions.

Regional prevalence data of health risk factors might be of some interest since regular surveillance measures in the field of health behaviours are not yet well developed in Germany. Unfortunately, due to the lack of information regarding methods and the absence of any statistics considering sampling error, interpretation of the results and comparison with other German epidemiological studies is not possible for the reader, so far.

Since the research question and data analyses are restricted to simple descriptive reporting of rates it is hard to generalise some of the findings beyond the study region, e.g. co-incidences of different risk factors or regional particularities were not mentioned. Therefore, the contribution to the scientific knowledge in the field of public health might be of limited interest.

Major compulsory revisions:

1. Page 5 para. 2 (and other teext passages): To me it is not clear how the issue of “Echinococcus multilocularis” relates to the aim of the paper. No findings or considerations regarding the findings later on in the paper refer to this issue. It should be considered to explain this background of the survey in the methods section in one or two sentences.

The acquisition on data on the prevalence of E. multilocularis infection, as the initial aim of the study, is explained in the “background section”. The presented manuscript however deals with other, more general, public health issues. In our opinion the presentation of data on the rather specific health issue (E. multilocularis infection) together with data on obesity, alcohol and tobacco consumption is not practical. We agree with the reviewers, that this matter wasn’t explained properly in the manuscript.

This information has been added to the “background section” and the lengthy explanations about E. multilocularis have been shortened.

2. The sampling region should be defined. Inclusion and exclusion criteria and reasons for non-response should be described more precisely. Reasons for non-response should be detailed.
The detailed reasons for non-response have been included into the manuscript. Inclusion criteria are already defined in the “method’s section”. A more precise definition of the sampling region has been added to the manuscript.

3. Page 9. Giving an example for sources of assessment instruments is insufficient. It is not exactly clear what the authors have measured with respect to the self-reported data. Criteria used to define health behaviours and wording of items could largely influence prevalence estimates (e.g. did the data refer to alcohol consumption in the past week, the past day, the past weekend or past 12 months; was a quantity frequency index calculated and if yes, how;…).

We agree with the reviewer that the given information on the used questionnaire is not sufficient. We included therefore a more detailed description of the used questions and added a reference [12] for directly comparable questions.

4. Page 10, last para.; In my view, information on the accuracy of the point estimates considering the given sampling design (e.g. SE or CI) is the minimum a paper on prevalence rates has to provide.

Following the reviewer’s suggestion we included CI values into the tables with the results of multiple logistic regression analysis.

5. Taking into account the conditions regarding survey research in the general population in Germany and the ambitious study design/assessments, the response rate of 62.8% (nevertheless I am not sure how it has been calculated) is respectable. However non-response could be a serious source of bias, obviously. The analysis of non-response is not adequate. I am not able to draw any conclusion from figure 1.

A detailed analysis of non-response has been added to the manuscript. As stated in the manuscript the number of 62.8% is calculated by taking the number of participants (2445 persons) and the randomly drawn population sample (3893 persons). Figure 1 is meant to give the reader an impression of the examined sample population in comparison with the total population of the city of Leutkirch.

6. Socio-economic characteristics of the sample (exceeding age and gender) should be described in more detail.

The characterization of the sample population was amended with information on nationality (ethnic background). The questions referring to the participants’ level of education have been omitted as they are not comparable to international studies and, as we have to admit, have not been addressed in great detail.
7. I am not sure if the findings from the ultrasound examination (gallbladder stones, polyps) match the purpose of the paper outlined in the introduction.

One of the established risk factors of gallstone disease is obesity. We agree though, that gallbladder polyps are not directly related to the manuscripts main theme. We therefore deleted the information on gallbladder polyps to focus the manuscript further.

8. Many conclusions in the discussion section regarding trends or regional variations are derived from comparisons with different studies, without sufficiently considering the differences in assessments, classification and sampling errors.

Although we believe that addressing different assessments and study designs in the comparison of our results with other studies in detail makes the discussion very long, we added some detail about the limitations of direct comparability between certain studies. We have to assume that the readers will be able to apply the generally mentioned limitations to the specific studies in those cases where further detailed descriptions would be to lengthy.

9. The discussion omitted a discussion of the limitations of the study

A paragraph discussing the study’s limitations has been included into the manuscript.

Minor essential revisions:

10. Page 4, line 5; No reference stated.

The first two sentences in his paragraph refer to reference [1]. We agree with the reviewer, that this fact wasn’t comprehensible from the manuscript text. The correct references have been added.

11. The use of the term “subject’s history” should be revised

Following the reviewer’s advice the term has been revised to “medical history”.

12. Page 8 para. 2; please revise phrasing (epidemiological studies apply methods developed in the field of e.g. clinical chemistry…)

The sentences have been rephrased: “Apart from the information gained by the use of questionnaires, the investigative techniques used in the study included methods
from the field of anthropometry and clinical chemistry. Abdominal ultrasonography was used as a further diagnostic tool.

Discretionary revisions:

13. Reference 1 focuses on diet and physical activity. GBD Study might be a more comprehensive source including a ranking and total disease burden of all risk factors considered in the paper.

The three most important risk factors for global burden of disease (childhood underweight, high blood pressure and unsafe sex) as stated in the Global Burden of Disease and Risk Factors study (Comparative Quantification of Mortality and Burden of Disease Attributable to Selected Risk Factors) do not apply to our study design (childhood underweight), sampling region and assessed diseases (unsafe sex) or frankly were not assessed in detail (blood pressure). As we didn’t see an important gain of information by adding the GBD study we did not include it as an additional reference,