Author's response to reviews

Title: Marginalisation and cardiovascular disease among rural Sami in Northern Norway: a population-based cross-sectional study

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Version: 3 Date: 15 April 2013

Author's response to reviews: see over
COVER LETTER

Dear Editors

I hereby resubmit the manuscript entitled *Marginalisation and cardiovascular disease among rural Sami in Northern Norway: a population-based cross-sectional study*.

BMC Public Health has a special focus on the social determinants of health. The effects of marginalisation on health in indigenous peoples in the Arctic have received increasing attention among researchers and indigenous politicians. Research that specifically operationalizes marginalisation as an explanatory variable is however lacking.

I thank the referees for invaluable and helpful comments. Below is a step-wise response to the referees’ comments:

Author's response to reviews

Title: *Marginalisation and cardiovascular disease among rural Sami in Northern Norway: a population-based cross-sectional study*.

Authors:
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Version: 3 Date: 15th April 2013
REFEE 1

The introduction. Should be shortened, addressing more specific the research question (marginalization) and what has been found earlier regarding this topic in the Sami or other indigenous people.

The introduction is severely shortened.

The first paragraph on page 4 should be shortened and the number of references should be limited to only a few to support the much shorter description of the assimilations efforts historically undertaken.

Please refer to the revised introduction.

The second paragraph on page 4 should also shorten with a similar reduction of references.

Please refer to revised introduction.

The paragraph on page 5 starting with An extensive and growing body. could also be shortened.

Please refer to the revised introduction.

The discussion of stress as a risk factor for CVD could be moved to the discussion section and taken in to consideration only in relation to the discussion of the findings in the present paper.

Discussion of stress is shortened and moved to the discussion part as advised. See page 12 and paragraph 2 on page 13.

The last paragraph on page 5 could be omitted. This is not a paper about cardiovascular mortality could be omitted. This is not a paper about cardiovascular mortality and a summary of findings in the papers addressing incidence or prevalence of self-reported or registry based cardiovascular disease in Sami or other indigenous people would be of greater interest and paper.

The paragraph in question is omitted and the focus is on prevalence.

Methods

Sample

Mention that those born in 1973 and 1974 (30 years old) were excluded both due to both low response rate and no contribution to the research question. Cardiovascular disease is not very common in 30 years old.

We do not mention the 30 year olds in this version. See paragraph 3 on page 4.
Your models are difficult to understand, please provide a more detailed description. Did you choose a structured approach when including variables included the models? Please describe in more detail

The text describing the statistical methods used is revised. Page 9, paragraph 1.

**Statistical methods**
Describe the variables included in the full model in table 4, either in this paragraph or as a foot note in table 4.

Footnote included.

**Results**
Page 12, first paragraph, line 3: p=0.05 is a borderline finding, not a significant finding.

The reference to this finding is removed. See results.

**Discussion**
The discussion section should be shortened and discuss the findings in this paper in light of similar papers studying marginalization and cardiovascular disease or other outcomes.

Discussion is severely shortened and more focused on marginalisation.

**At present the discussion is too long and the authors provide data more like a review than like a discussion.**

Text is revised accordingly.

**Strengths and weaknesses should be addressed in a separate paragraph and the sections discussing confounding, information bias and selection bias could be shortened and included in the strength and weaknesses paragraph.**

This is now done.

**Page 13, second paragraph: The paragraph staring with: The effect of acculturation.**
This is the paragraph where you discuss your acculturation finding, but you should do it in more detail: You mention some earlier studies both in arctic indigenous populations and in immigrants groups, but you should mention the outcomes studied and the results and compare your finding with those findings.

The acculturation research is overall undefined which was reflected in my previous discussion. The acculturation research provided in the revised manuscript is specified to marginalisation; marginalisation is in the theory conceptualised as one out of four possible modes of acculturation, all of which are assumed to be present to a varying degree during acculturation. Acculturation theory is very much debated and many contributors in the field apply it carelessly with little concern for the complexity of culture and social change as phenomena. The previous discussion was meant to clarify and underline these points. However, due to the given page limit I have structured the discussion and left out the acculturation argument.
There are many competing factors not measured in your study of relevance for your CVD, and you should mention the most important in this paragraph. In this paragraph you also mention results from former Norwegian studies on cancer incidence and mortality. From the mortality study you mention the results, but again the focus could be is this a confounder for your finding of a potential effect of marginalization on CDV in Sami minority areas.

We have included metabolic syndrome in the analysis and performed sensitivity analyses by including total cholesterol. By doing this we address the referee’s comments concerning “many competing factors not measured in your study”.

The article referred to herein concerns CVD mortality and the relevant contribution from this study in my research is included to highlight possible confounding. Se paragraph 1 one page 13.

Your defensive statement: We are unable to comment on any effect should be rephrased to a more careful consideration of these findings in relation to your findings. For following paragraphs the discussion is more like a review than a discussion of your own findings up against other authors finding, try to restructure in order to find similarities and differences between your findings and other authors findings addressing the same question. Try also to shorten the discussion and be more focused on the question addressed in your article.

Due to lack of research within the field specifically operationalizing marginalisation as a independent variable, it is difficult to compare my results. But we have re-structured this part of the discussion.

There are some problems with self report of CVD. Both for stroke and for Rose angina the reporting may cover more diagnosis than only CVD, for stroke TIA as you mention, and for Rose angina questions more stress related musculo-sceletal symptoms.

We believe that this section is covered well enough, and a description of the musculoskeletal symptoms bias with regard to Rose angina is perhaps more appropriate in an article focusing on Rose Angina. We have only used Rose angina in the described sensitivity analysis.

In your conclusion you say may be related and that your cross-sectional study design does not however allow any conclusion with regard to causality and risk. I agree and I would say that there are many other risk factors for CVD not measured which might vary between majority and minority municipalities in North Norway, e.g. smoking, diet, etc.

We agree and have included MetS in the model and performed sensitivity analyses which included total cholesterol and intensity and duration of smoking.

References

Number of references reduced to 37. We hope this suffices.
I find the article very interesting and it definitely has a novelty. It is difficult to collect this amount of data that this study comprises and I need to acknowledge that. The authors address their limitations well; nevertheless, I find it problematic that the authors do not include diet as a confounder when they refer to an article from the same study dealing with diet. This means that the data are available so I do not understand why it is not included.

As described in paragraph 1 on page 13: We have not included any information on diet as the questionnaires were not designed to include total diet and total energy intake.

Furthermore, the manuscript has to be shortened substantially; especially the discussion Part is very long.

Discussion part is severely shortened as described above.

I have one minor –hopefully helpful comment – which is regarding the punctuation: When it comes to writing the age span e.g. in the abstract in method part. It is not correct to write 25-79 years with a single hyphen. What you mean is that participants are from 35 to 79 years old. To write this you need the – (the dash, it is a bit longer than the hyphen): 35–79. Press Ctrl and minus at the same time and no space in between. This is the same as writing from 35 to 79 years old. The hyphen is commonly used to combine words, as you do correctly in your title (population-based, cross-sectional).

This is corrected.

You write in method part in the abstract ‘questionnaire’, but you mean questionnaires? In the article you mention three questionnaires.

This is corrected.

Methods
It is not clear to me what you have adjusted your analyses for. Please be more detailed in your description.

Footnote included in Table 4.

Sample: What do you mean with family background (ethnicity)? Is this based on parents or grandparents ethnicity?

Specified in paragraph 2 on page 5.
Questionnaires: Please describe if your questionnaires were validated.

First sentence, paragraph 2 on page 5: The questionnaires were not validated in this population but most of the questions have been used in previous population studies in Norway.

Page 9 regarding smoking. Was it only cigarette smoking? What kind of tobacco was/is used?

The type of tobacco was not specified in this question as specified in the question formulation. See paragraph 3 on page 6.

Will you please justify why you choose to use waist circumference and not BMI? It is fine to use waist circumference but you have to provide some kind of argument. Is it because of SAMI anthropometry? When it comes to Inuit populations I would agree that waist circumference is the best measurement if your outcome is clinical. But this is due to shorter stature compared to other populations that BMI are normally used on. But I have no knowledge on Sami anthropometry.

Paragraph 2 on page 7: As we have now included MetS in the analyses we must use WC. Originally we used WC due to Sami anthropometry.

Statistical analyses: Needs a further elaboration and description. E.g. what covariates did you remove, what adjustments were done?

Improved description in “Statistical analyses”. Footnote added in Table 4.

Results
See my comments in the ‘table’ section.

Page 12. For description of table 4. As written in the beneath part under table 4, it is not clear to me what confounders were finally included in the model. See above.

You should include dietary factors if you have the data. Since you refer to a paper from the SAMINOR study, the data are available…. You write that you did not adjust for diet but you did not give a good reason for why not. Check dietary intake for confounding.

See above.

Discussion
Your discussion part is 9 pages! It is too long and has to be shortened substantially. I have therefore not commented specifically on this part since I assume the content will change after a shortening. Nevertheless, I have a few points for your consideration.

Discussion part is shortened.

On page 18 you write that diet and other lifestyle factors might change as a result of acculturation. Could it be the other way around? Please, at least provide a reference for this statement.
Discussion rewritten.

You need to carefully revise the discussion so there is consensus e.g. you write that there was a substantial confounding with physical activity, but in the abstract and results you use the word ‘light’ and you do not show the data!

Leisure-time light physical activity (LTLPA) is now specified. As the only substantial intermediate effect detected was for LTLPA we wish not to expand Table 4 additionally to accommodate this data. It is described in detail in the text which should suffice.

Tables.
For all tables Please provide N in all groups.

N included.

Table 1–3. Please justify why you split analyses on men and women. It is fine by me if you find sex differences, but you write under table 4 that you do not. So why are these tables stratified?

It is to my knowledge customary to present the characteristics for men and women separately. If I do not do this, the reader is unable to assess whether the joint analyses in Table 4 is appropriate given the sex-specific characteristics presented in Tables 1 and 2.

Could you insert a row in the tables where you write N for each group.

See above.

The p-value is for the difference between the four groups? If yes, I do not find this p-value very informative. I would recommend p-values for the difference between unexposed and exposed in the majority group and a p-value for the difference between unexposed and exposed in the minority group.

This is now provided in Tables 1-3.

Table 4.
‘selected risk factors’ – you need to specify in the table (foot note) how these risk factors were selected).

It is not clear to us what the referee means by “how these risk factors were selected”. In accordance with the requests of referee 1, we have described in “statistical analyses” how the covariates were included.

Discretionary Revisions
Only for table 4.
You can exclude the p-values when you give the 95% CI. It is clear from the 95% CI if the point estimate is significant. You do not need the foot note c. the 95% CI is given in the header in the table. please provide the reference category with OR=1.
These changes are made.

- Remember that tables have to be self-explanatory – In my opinion your tables cannot be interpreted alone without reading the text.

Tables are revised.

- Make a foot note describing adjustments for the ‘full model’. It is not clear from the statistical part or the results describing table 4 what was included in the fully adjusted model.

See above

REFEREE 3

1) The words “areas” and “arenas” are used interchangeably.
   This is corrected.

2) Page 20 (7th last line) “the” is used two times.
   Corrected.

Additional changes made

We have decided to present the response rate in the age group 36-79 which was 60.9%. The previous one included the 30 year olds (60.3%).

Sensitivity analyses made in terms of the presented prevalence rates of CVD are described in “Statistical analyses”.

During the revision of the manuscript and the analyses, we discovered an error in the recoding of family history of CVD. In the first version the prevalence of family history of MI and stroke were too low due to this. This did however not affect the final results (see Table 4).

We sincerely apologize for the delay.

I hope the study is considered relevant for publication in your journal, and I look forward to hearing from you.

Yours Sincerely,

Bent-Martin Eliassen
PhD student