Author’s response to reviews

Title: Low level of alcohol drinking among two generations of non-Western immigrants in Oslo: a multi-ethnic comparison

Authors:

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Author’s response to reviews:

Dear editor

Here is my response to reviewers’ comments.

Reviewer Carolin Donath

Major compulsory revisions

1 I agree that the data are old. It took a while before I got aware of the study among the five largest groups of immigrant in Oslo. Then, it took a while before I got permission to use the data and then it took a while before I got access to the data on my computer. Then, after I presented the first results at a conference a couple of years ago, it took time to learn how to apply SEM and, then finally, select and focus on the issues I wanted to address – in competition with all my other projects. This study illustrates drinking frequency after 30 years of continuous immigration (1970 to 2000) from non-western countries to a western country/city with a much higher level of drinking than in their country of emigration. Another decade has gone by, and I would like to follow up and see whether the third generation, now into their teens, deviate from the drinking pattern of the second generation – and if ethnic Norwegians become influenced by low level of drinking in the non-western population. But it is difficult to establish datasets with as many persons as necessary from each country background and their offspring. I have incorporates the 30 year perspective in the discussion, last paragraph under weaknesses, page 25.

2 I have rewritten the aim to clarify it. The sentence addressed is no longer part of the background. Age groups are presented in the method part of the abstract.

3 The aim/research question is rewritten, focusing more on the actual research questions asked and answered.

4 I have added that the survey among immigrants was carried out among the five largest immigrant groups in Oslo. I find no space in the abstract for the names of the two groups that were not included (Sri Lanka and Viet Nam), but this is mentioned in the method section.

5 The outcome variable “Frequency of drinking last year with four categories” is now specified in the method part of the abstract.

6 I agree that some numbers should be present in the result part of the abstract. I
find no space in the abstract, however, for a simple presentation of differences in the outcome variable “distribution of drinking frequency” for each country background within the 350 words available. Table 3 and 4 with Chi2 and p-values for testing differences in the distribution of drinking frequency will have to be the source for actual numbers.

7 The conclusion is clarified and based on the findings and the discussion. It is an interesting exercise to widen the perspective and look forward, however, so this is part of the conclusion.

8 As requested, information on the Norwegian drinking culture, alcohol policy and how drinking is established in the Norwegian population are now included in the background section. I have not included beverage types, however, since this study is about alcohol drinking in general. Information on immigration in general and with non-western background is also extended, see page 4-7.

9 The aim is rewritten to be more precise.

10 I have included information on alcohol use from other studies in Europe/US where country background of the immigrants is reported, see 2nd paragraph page 4 + page 5. I have in general not included the actual measurements. I think this will exceed the degree of detail that the reader is comfortable with in the background section. I also find it difficult to compare reported figures for alcohol use in the discussion, mainly based on that there are few measurements for the same countries. For Iranians I have discussed the similarity of results with the Donath et al. study from Germany. For others immigrants with a given country background I have made a general comparison, naming the country background studied, mainly in Europe, but also some in US and Australia. I have made a rather thorough new search regarding alcohol drinking (not alcohol problems) and have reported all I found. Multi ethnic comparisons are discussed on pages 20-21.

11. The surveys included in this study were carried out by the Norwegian Institute of Public Health. I therefore have to rely on this institute’s documentation of the surveys and methods and I have referenced this documentation available on their web page www.fhi.no. Web addresses are added on reports, even though this is not standard, to make information easily available. I agree that a CONSORT type description would be appropriate. Unhappily, there is only a CONSORT type figure for one of the surveys in the documentation (the HUBRO study). I have added some CONSORT type information regarding the youth survey since this cannot be found in any documentation in English. More information could be included in the present manuscript, but is easily available electronically in the references given. I have tried to include enough information on the surveys for most readers and then given references for those who want more details. The electronic references as found for the BMC Public Health in my Endnote version did not include the necessary web pages addresses to easy access the information, and I have corrected that.

Response rates among immigrants have been included under the description of each survey. Also item non response is reported.

12. I do not compare sample 2 with sample 3 in the analysis. Within sample 2 I
compare persons born in Norway with persons born in Iran, Pakistan and Turkey, so there is no ‘pooling’. I have added a sentence at the start of the paragraph and in the description of sample 3. The only place I compare the total population (including immigrants) with immigrant groups is in Table 1 where the data come from Statistics Norway where there are no results for ethnic Norwegians only.

13. I have added Chi square tests for males and females for all interesting hypotheses regarding differences in alcohol frequency distributions at the end of table 3 and 4. I did not find a sensible way of making an extra column in the tables, and so the text is placed under the tables as endnotes. I see that this text is very long. It can be incorporated in the result section of the text also, but will not be easily readable. The second paragraph in the result section is slightly adjusted after the repeated significant calculations.

14. I agree that results regarding colinearity in the regression model should have been reported. I have now reported the maximum correlation numerical value for all and for each country separately in the text, se page 16, paragraph 2. This does not, however, enlighten why the latent ‘Social interaction’ variable was not correlated to the other latent variables or not ‘influenced’ by the exogenous variables. Potential correlation between all the latent variables was part of the SEM model before the process of fitting the ‘best’ model. Only the correlation between own and host culture competence was significant and thus included in the final model. A paragraph on the process of reducing the SEM model has been added,, see last paragraph page 13 and last paragraph page 16.

15. A description of the fit measures applied, with rules for a good fit, is now included in the statistical methods paragraph, see bottom of page 13, top of page 14.

16. In the previously submitted manuscript, paragraph 5 and 6 in the result section reference regression parameters and their products in Table 5. The full association of an exogenous variable on frequency of drinking is the sum of the direct effect (regression coefficient) on alcohol frequency and the indirect effect through one of the latent variables (product of two coefficients). This is common practice from path analysis. I have tried to explain this in the 3rd paragraph page 17 for the first example and also included a couple of sentences in the statistical analysis paragraph in the method section, see page 14, 2nd paragraph.

17. In Table 5 all significant associations are flagged with a star. I have added a *** footnote indicating this, I had forgotten to do that. So the phrase “fewer significant results” is just on the basis of counting the stars. I have rewritten the sentence to clarify this, see top of page 18.

18. I agree; the sentence is removed. In the discussion I have commented on the role of low n regarding lack of significant results, see page 25, bottom of 3rd paragraph.

19. I have extended and refined both the result and the discussion section regarding country wise comparisons (see Multi ethnic comparisons pages 20-22, and including the relevant study by Donath et al.

20. I have included comparisons of the studies referenced in the background section ++, but not on a detailed level. Such comparisons may be confounded in
so many ways and the actual alcohol measures were partly different.

21. The conclusion is modified.

Minor essential revisions
1. I have clarified in the method chapter that the original survey included 20 to 60 year olds, while I only analysed the 30-60 year olds. The reason was that I wanted to analyses the parent generation of the 15-16 year olds, and those grownups who socialized with them bottom of page 8.

2. The exact phrasing of the question was “How often have you consumed alcohol in the course of the past year?”. This is now written in the text (top page 11).

3. This comment on ethnic background is only relevant for 15-16 year olds. The categories by country background were based on the mother’s country of birth, while the father could be from any other country than Norway. All Iranians had both mother and father from Iran, among Pakistani mono-ethnic parents constituted 98 percent, and 97 percent for Turks. This information is now included in the paragraph on Country of birth/ethnic background, see page 11.

4. I agree that missing values for alcohol use were central in this study and such are now included in the method section in the description of each original survey and discussed. Dataset 1, 2 and 3 were established based on a selection of those who had reported alcohol frequency. For dataset 1 and 2 the only other variables employed than alcohol use were gender and country of birth/ethnicity and missing for these variables are now reported. Another important set of item missing was for acculturation and other variables in dataset 3. Since there are many variables, only the range of missing is reported, see page 10, no. 3.

Discretionary revisions and minor issues not for publication:
Most of these comments are followed and the English is corrected by a professional translator. Regarding the correlation value between own and host competence, this is not a common Pearson’s r since it is the correlation between two latent variables that are not measured directly. I have tried to check in the literature, but within the time limits I had no success.

Reviewer John E. Berg

Major compulsory revisions
a) I have extended and refined both the result and the discussion section regarding country wise comparisons, and including the relevant study by Donath et al.

b) I agree that a response rate of 39.7 percent in the immigrant population may seem low. This is a percentage of the total immigrant population of Iranians and Turks, however, not the response rate of a sample drawn from the total population. For Pakistanis this is a percentage of 30 percent of the immigrant population of Pakistanis. But anyway, a question of representativeness should be raised since we do not know the selection mechanism into the sample. This has already been done on page 25, 2nd paragraph.

c) The result of the factor analysis was placed in the method section, but I agree
that it could rather be in the result section. Psychologists usually put it as part of the method section, and I followed that practice. I have now moved it to the result section.

d) This is now better discussed, see Multi-ethnic comparisons pages 20-21.

Minor essential revisions

1. I agree, ‘drier’ drinking cultures is now exchanged with non-western countries

2. In the method section I have first described the three original surveys and how they were conducted, their response rates etc., Then I have described the three samples which have been the basis for the article: 15-16 year old ethnic Norwegians and youth from the three non-western countries + 30-60 year old adults in the same group. I have adjusted the description in the method section such that this is clearer, see page 10.

3. The statement on the Iranians in the last paragraph is found in Table 5, and presented at the bottom of the last paragraph in the result section on page 12. The result can be better presented, however, and I have enlarged the result section to ease the reading.

Discretionary revisions:

I. I have given a better description of the fit measures CFI and RMSEA in the method section/statistical analysis and also added a note on the * which means statistical significance at the 5 percent level. In ‘Non variance between groups’ the ‘group’ is here country background. I have exchanged this and also rephrased the sentence to ease understanding. The term is standard, however, in reporting from multi group analyses using SEM.

II. I can see that understanding figure 1 requires knowledge of SEM modelling. I had split the total presentation of the model in two, since it was too complicated to put the factor model on top of the structural model. In addition some terms (measurement errors, some residual terms and correlations) have not been drawn. I am sorry to say that I do not know how to improve figure 1; this is a standard way of reporting such models. The heading (legend) is on page 29.

III. The sentence is corrected

IV. The conclusion is now rephrased

V. Corrected

VI. What AMOS stands for is now in the statistical method section page 13, bottom of page.

VII. The fit measures CFI and RMSEA are now better explained and a reference to a book section on fit measures in SEM in the book by Byrne is given.

VIII. I have enlarged the statistical analysis section, added a paragraph in the result section on how the SEM modelling was carried out and included some more results, also actual test results. The value of the correlation between host and own culture competence is corrected in the text.

IX. I have explained better the direct and indirect effects, see page 14, 2nd paragraph.