Author’s response to reviews

Title: Physical inactivity is strongly associated with anxiety and depression in Iraqi immigrants to Sweden: A cross-sectional study

Authors:

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Version: 2 Date: 20 April 2014

Author’s response to reviews: see over
Dear Natalie Pafitis,

Thanks a lot for providing a review of our manuscript and for giving us the opportunity to submit a revised version. We would also like to thank our reviewers for their valuable comments. Their opinion has enabled us to review and improve our work. Our point-by-point response to the reviewer’s comments are presented below (in green color.)

We consider our manuscript being appropriate for publication in BMC Public Health and hope you agree with us.

Yours sincerely,

Louise Bennet (on behalf of my co-authors)
Center for Primary Health Care Research
Region Skåne and Lund University
Malmo, Sweden
Reviewer's report # 01

Title: Physical inactivity is strongly associated with anxiety and depression in Iraqi immigrants to Sweden: A cross-sectional study

Version: 1 Date: 6 February 2014

Reviewer: Anu Molarius

Reviewer's report:

This is a cross-sectional study about the prevalence of anxiety and depression among Iraqi immigrants and native Swedes in Malmö city in Sweden and factors related to these mental health conditions. Mental health among immigrants is an important issue and the authors do indeed find a considerably higher prevalence of anxiety/depression among Iraqis compared to native Swedes. Economic insecurity was found to be a risk factor among both Iraqis and Swedes and among Iraqis also reading skills in Swedish. The authors present as their main result that physical activity is more strongly associated with mental health among Iraqis than among native Swedes and recommend physical activity to prevent poor mental health in this group. Even though physical inactivity is known to be associated with poor mental health, this is quite a simplistic view. What is more surprising is that physical activity was not associated with anxiety/depression among the Swedes in this study. There are many possible explanations to this difference in the effect of physical activity. The measure was not very strong, only one dichotomised variable (<150 min per week), with no additional information of its distribution, was used. The representativeness of the study is also questionable. No response rates are given, but one can read from figure 1 that the response rate for Iraqis was 48% and for Swedes 32%. This is very low and differs between Iraqis and Swedes. The p-value given for the interaction term between physical activity and ethnic origin is not even statistically significant (although on the borderline). In that case even the finding that marital status was significant among Swedes but not among Iraqis would have been as important as the difference in physical activity. Also the cross-sectional design limits the interpretation considerably. And last, no possible mechanism for this difference is given in the discussion.

A more relevant question, than looking the Iraqis and Swedes separately, would have been to find out why Iraqis have poorer mental health. Table 1 gives some hint, in addition to lower physical activity, the Iraqis have more economic insecurity, higher unemployment and lower social participation. The authors state in the results section that Iraqis had three times higher odds of anxiety/depression even when adjusted for all the variables included in the model, so these variables do not explain the difference in prevalence (not even physical activity), but it would have been important to know to what extent they do and have variables that further contribute to these differences.

[Authors] The authors are thankful to the reviewer for the valuable comments. We believe that by addressing and incorporating the comments in to our work, the quality of our manuscript is improved.

We are in agreement with the reviewer that based on a cross-sectional study, the interpretation is limited as a causal association cannot be established between physical inactivity and mental health and we have now highlighted that in our limitations. Nevertheless, we wanted to bring physical inactivity among Iraqi immigrants into focus for two reasons. Firstly Iraqi immigrants are markedly inactive as compared to Swedes both in anxious/depressed as well as non-anxious/non-
depressed groups. Secondly, physical inactivity is a potentially modifiable variable in primary 
health care settings (although not easy to modify). Therefore, by reporting this association, we 
intend to draw attention to this important life-style related factor that might lead to improvement 
in mental health among Iraqi immigrants. This will also encourage further intervention studies 
focused on Iraqi immigrants. Moreover, previous intervention and longitudinal studies have 
reported therapeutic effect of physical activity on depression and anxiety, which provides support 
to the findings in this study. We have tried to capture all these aspects in our discussion section.

We agree that not finding physical activity as a factor associated with mental health among 
Swedes could be a result of small sample size. We have now discussed that in the manuscript.

The response rates are low and a discussion of this has now been included in the manuscript.

We agree with the reviewer that marital status is an equally important factor for Swedes; however, 
we could not include a detailed discussion of that because of space constraints. We have 
nevertheless now highlighted that in the abstract as well as in the results section.

As mentioned by the reviewer, Iraqis had higher odds for anxiety/depression even after adjusting 
for all the variables included in the analysis. Although we have attempted to capture most of the 
variables that were found relevant from literature search, including acculturation related variables 
for Iraqis, mental ill-health in Iraqis is a complex and multi-faceted phenomenon. It is difficult to 
account fully for this situation and it therefore needs further research. Other studies on mental 
health among immigrants conducted in Sweden have also indicated that the situation cannot be 
fully accounted for by socio-economic factors and ethnicity is a risk factor in itself. We have now 
mentioned that in the manuscript and also indicated the need to explore variables that can explain 
this association to a larger extent.

Specific comments:

The response rates should be given in the abstract and in the methods section and their possible 
effect on the reliability of the prevalence estimates and other results should be discussed

[Authors] The response rates have now been mentioned in abstract and method sections along 
with a discussion of their impact on prevalence estimates in the manuscript.

The background begins with a long description of the size of immigrant population in Europe and 
Sweden. This should be compressed and a description of what is known about the mental health of 
immigrants, in particular those from Middle East, should be given. Also, cardiovascular and 
metabolic diseases are brought up as the reason why to study mental health among immigrants, 
but the mental health of immigrants should be a focus on its own right.

[Authors] We have incorporated her suggestions in the Background section. The information on 
immigrant statistics is reduced with more focus on mental health and its value per se.

To calculate the standardised odds ratios standard deviations of categorical variables have been 
used. Either this is inappropriate or very poorly explained. Why would they be necessary?

[Authors] The analysis is now revised and standardized ratios are no longer used.

Only p-values for the interaction models are given, but the complete models should also be 
reported.
As per our understanding of this comment, complete models of multivariate logistic regression are now reported.

In the discussion the authors seem to argue that their study is the best in this area of research. A more neutral comparison with other similar studies and taking into consideration the limitations of the current study would be preferable.

In the light of this comment, the discussion section has been revised where we elaborated further on our limitations and attempted to present a more neutral comparison.

Backwards elimination was used in Table 2. In the note under the table it is said that the data were adjusted for age, male sex, physical activity etc. But if all these variables were not included in the model, they cannot have been adjusted for. In addition, instead of backwards elimination it would be better to include all the variables in the model to see the real differences in odds ratios e.g. for physical activity.

The analysis is now revised using multivariate logistic regression rather than backwards elimination. All the variables were included in the model and odds ratios for both significant and insignificant variables are now reported.

The figures are difficult to read due to quality problems.

The quality of figures has been improved. The figures now have following dimensions:

- Figure 1: 1050*950 pixels @300 DPI
- Figure 2: 1750*950 pixels @300 DPI
Reviewer's report # 02

Title: Physical inactivity is strongly associated with anxiety and depression in Iraqi immigrants to Sweden: A cross-sectional study

Version: 1 Date: 18 March 2014

Reviewer: Per Wandell

Reviewer's report:

This is an interesting and important study on mental health in a specific immigrant group. The study is well performed and described.

[Authors] The authors are pleased to know that the reviewer found this study interesting and important in its area.

Remarks:

Major compulsory revisions:

1. The authors have not discussed possible cultural aspects on mental health and expressing complaints about this. They should add this in the discussion.

[Authors] We have commented on this aspect in manuscript now.

2. One earlier study has looked at the Turkish-born group in Sweden with regard to mental disturbances. The authors should at least comment on this as both groups originate from the same region of the world. (Ethnicity & Health 2007;12:363-79).

[Authors] We have now referred to the above mentioned study in the discussion section.

Minor essential revisions:

3. The authors have used HADS. However, they have not mentioned the criticism from SBU, the Swedish Council on Health Technology Assessment, that HADS has a too indefinable sensitivity to be useful. They should at least discuss the implications of this in the section of limitations.

[Authors] This is now discussed in the limitations section.

Discretionary revisions:

4. Percentages are given with two decimals which is unnecessary; one (or none) is enough.

[Authors] Percentages are now reported to one decimal point only.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: I declare that I have no competing interests.