Author's response to reviews

Title: Mental health of immigrants from the former Soviet Bloc: a future problem for primary health care in the enlarged European Union? A cross-sectional study

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

We are happy to re-submit the manuscript No. Ms 1139741791002230 Mental health of immigrants from the former Soviet Bloc: a future problem for primary health care in the enlarged European Union? A cross-sectional study

A month ago we received comments of the new reviewers (two out of three), who were assigned instead of the previous reviewers to whose questions we have answered in the previous re-review of the manuscript.

Two of three reviewers (Benjamin Vicente and Yaakov Lerner – a reviewer of the manuscript since earlier) have suggested to accept the manuscript without revision or after a minor revision respectively. However, the third reviewer (Kamaldeep Bhui) required major compulsory revisions.

This put us in a difficult situation of a necessity of changing the manuscript in response to the request of the third reviewer regardless the fact that the manuscript was approved by the other reviewers in its re-reviewed form. However, we chose to answer the requirements of the third reviewer and revised the manuscript by answering the questions posed by Dr. Bhui. We improved the flow of the manuscript by creating a theoretical frame of the study in the Introduction and presenting the Methods, Results and Discussion according to it. We modified the presentation of results in the tables making it clearer, and toned down the conclusion as requested. This caused re-writing of major parts of the manuscript and slightly increasing its size.

In the attached pages, we have addressed the reviewers’ comments point by point.
If you have any questions, please contact the first author:

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Yours sincerely,

Yulia Blomstedt,
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Reviewer: Benjamin Vicente

No comments were made or changes requested by this reviewer.

Reviewer: Yaakov Lerner

The authors reported in their revised manuscript (page 14 second paragraph) the results of an additional analysis on the hazard ratios for hospital admission due to mental disorders among immigrants from the former Soviet Union. Why didn’t they also perform such an analysis regarding the immigrants from the other Eastern Europe countries or from Poland, to see whether the results will be in line with their finding of an increased report of psychiatric illness among these immigrants.

We thank Dr. Lerner for a valuable comment. We have attached the results of the additional analysis for immigrants from Poland and other East European countries as well.

The attached text is presented on pp.15-16, and is as follows:

To test the main results of the study we conducted an analysis of hospital admission based on data for all hospitalizations of the entire nation. We used Cox regression to calculate the age- and sex-adjusted hazard ratios (HRs) for first hospital admissions due to mental disorders among immigrants from the former Soviet Bloc 25-84 years old, who arrived to Sweden after 1944 and lived in Sweden during 1994-2001. We found that both women and men born in Poland (HR 1.43; CI 1.32-1.56), as well as women born in other East European countries (HR 1.15, CI 1.01-1.31) had a higher hazard ratio of being hospitalized due to mental disorders than the Swedish-born women and men. However, persons born in the FSU, regardless of their sex, did not have a higher hazard ratio compared to Swedish-born persons (HR 1.09, CI 0.96-1.24).
1. This is an interesting paper which includes data which indicate a potentially higher risk of mental symptoms among migrants from Eastern Europe and Poland but not from the former Soviet Union compared with Swedish people.

We thank Dr. Bhui for this positive comment.

2. The outcome is overstated as ‘psychiatric illness’ and ‘psychosomatic illness’ The authors have modified their terminology to ‘illness’ rather than ‘disease’ or ‘disorder’, but they have actually measured symptoms. The process for assigning the diagnosis with clinical relevance is not clear from the methodology. It would be more accurate to simply report higher rate of self reported symptoms of mental distress rather than psychiatric illness and psychosomatic illness. Indeed for the latter physical illness seems not to have been excluded.

In response to this comment we have made the description of our variables more clear. The outcomes of the present study were “the self-reported psychiatric illness” and “psychosomatic complaints”. We agree with Dr. Bhui that the variable “psychosomatic complaints” represented the symptoms that the respondents reported. However, “the self-reported psychiatric illness” represented the illnesses or any disabilities which the respondents reported and which the coding group at Statistics Sweden have analyzed and assigned the diagnoses to in accordance with the ICD-9. We have slightly modified the description of this outcome for it to be clearer. It is presented on pp.10-11:

**Self-reported psychiatric illness was derived from two questions:**

1) “Have you any longstanding illness, after-effects of injury, disability or other ailment? Name and describe in detail every ailment/illness.” The coding group at Statistics Sweden assigned diagnoses according to ICD-9 (even after 1997) to everything the respondents had reported answering this question. For the present study all respondents with diagnoses 290–319 were defined as having a self-reported longstanding psychiatric illness.

2) “Do you have anxiety?” (Yes, severe; Yes, light; No). Those who answered “Yes, severe” and “Yes, light” were considered as having anxiety. All persons who reported having any longstanding psychiatric illness (1) and/or anxiety (2) were categorized as having a self-reported psychiatric illness.

The odds ratios of the “self-reported psychiatric illness” in immigrants in the present study were partly supported by the results of an additional analysis of the first hospital admissions of persons with the same age and other characteristics as the sample in the main study (described on pp.16-17). This is an additional argument that the assigned diagnoses represent mental disorders, rather than symptoms of distress.
3a. The conceptual basis of the study is not strong.

We thank Dr. Bhui for this constructive comment and have broaden the conceptual basis for the manuscript. We presented the literature review describing the major determinants of mental health reported by other studies applicable to our sample (please, see the Introduction under the subheading “Health determinants among immigrants from the countries of the former Soviet Bloc. A literature review” on pp.6-8). They were age, sex, marital status, social network, socioeconomic status, smoking and migration-related variables.

The presentation of these determinants in the Introduction explained our choice of independent variables used in the analysis of this study. The independent variables used in this study are presented in the Method section under subheading “Independent variables “ (pp.11-13)

We have subsequently modified the scope and the purposes of the study after presenting the health determinants in the Introduction on pp. 8-9:

There are various possible pathways of causal influence between these as well as other, not mentioned here, factors and mental health of immigrants from the former Soviet Bloc. The discussion of each of them deserves consideration in its own right. Unfortunately, this is not possible in an article. We therefore, limited the scope of this study to the health determinants which have been particularly known to influence the health of persons born in these countries. Our intention with this study was to find out whether these determinants could explain the poorer mental health among immigrants from the former Soviet Bloc, often reported by the other studies [9, 10, 38-43]. Thus, the purposes of this study were: 1) to study whether the self-reported mental health (measured here as the self-reported psychiatric illness and psychosomatic complaints) was poorer among persons born in the countries of the former Soviet Bloc currently living in Sweden than among the Swedish-born persons, and 2) to investigate whether the demographic, socio-economic, and immigration-related characteristics of respondents could explain this.

The Results and the Discussion sections were modified to follow the created conceptual frame. Please, see the modified Results section on pages 15-17 and the presentation of main findings in Discussion on page 18 (Paragraphs 1 and 2) and in the Conclusions on page 23, paragraph 1.

3b. There are many pre and post migration risk factors of importance. However, this study focuses only on pre-migration factors and gives an essentialist account of risk factors from the countries of origin, assuming these would be transported to Sweden. Yet, immigration policies and the host environment are known to influence risk of mental distress amongst immigrants particularly if there are differential degrees of receptivity or acceptance or prejudice. This has not been addressed.

We agree that immigration policies and the host environment are known to influence the risk of mental distress amongst immigrants but such factors do not explain differences between the studied groups. We have chosen a number of heath determinants that are important for the mental health among immigrants from the former Soviet Bloc (according to the previous studies).

However, we named the possible post-migration factors influencing health on page 8, paragraph 1:
Migration to the society with different patterns of health and health behaviours, with higher economic satisfaction among the inhabitants and different civil society might benefit immigrant’s health with time. On the opposite, stressors of immigration, such as novelty, not feeling at home in the host country, loss, and discrimination [10], in the host country might affect the health negatively.

Nevertheless, we pointed out that it was not in the scope of our study to present all the possible determinants (please, see the answer to the Comment 3a and pp. 18-19:

It is difficult to say whether the result depends on the pre-migration experience in the countries of birth which determined the health of immigrants from these countries or whether the post-migration experiences in Sweden, particularly, difficulties of acculturation to the new society usually experienced by immigrants in general, defined the result. Probably both. However, the aim of this study was not to clarify that and this question remains to be answered.

4a. It is difficult to interpret the findings with any confidence. Are there plausible reasons for why the former Soviet Union Block immigrants do not differ from Swedish people?

We thank Dr. Bhui for a constructive comment. It is interesting that the analysis of both sources of data, hospital admission register and the SALLS gave about the same results. We have changed the text of Discussion to include the plausible explanation of this finding (namely, why the odds of reporting poor mental health did not differ between immigrants from the former Soviet Union and the Swedish-born host population) on page 19:

We can only speculate why immigrants from the FSU did not have higher odds of poor self-reported mental health compared to the Swedish-born host population and had lower odds than immigrants from the other former Soviet Bloc countries (non-significant). It is possible that persons born in the FSU are afraid of stigmatization, have more negative attitude towards mental disorders and their treatments (as a legacy of the Soviet abuse of mental diagnoses and medical services for political purposes in the past) and are inclined not to report poor mental health or seek help for mental disorders.

4b. Could culture, nation, environment in Eastern Europe account for this higher risk of mental distress? Analysis of culture in these countries as the potential pathogenic effects is not reported, nor measured.

That is very interesting but we lack such data. As Dr. Bhui suggests, it is possible that culture, nation, environment in eastern Europe account for the higher odds of reporting poor mental health among immigrants from Poland and other East European countries than among the Swedish-born host population in Sweden. However, the design of this study (cross-sectional quantitative) did not allow to draw the causal pathways between the immigrants backgrounds and health status after migration or to “analyze the culture in these countries as the potential pathogenic effect”. A qualitative study with a specific aim of studying this could possibly answer this question. However, the purpose and the scope of this study did not cover this question.
5. Percentage responses by different national groups are important to report.

Unfortunately, Statistics Sweden do not report the non-response in these groups because they are not sampled after country of birth.

We included the following text to explain this on page 22, next to last paragraph:

Regretfully, it was impossible to estimate the particular non-response in the immigrants in this study, as the sample in SALLS is not chosen according to the country of birth.

6. The proposed implication is that immigration policy should be considerate of the burden of disability amongst immigrants, but I think this is overstated given the outcomes are symptom which may spontaneously fluctuate and not amount to illness or diagnosed conditions.

We thank Dr. Bhui for this comment and agree that the conclusion might have been overstated. However, we do find that a double higher odds ratios of reporting psychiatric illness and psychosomatic complaints among immigrants from Poland and other East European countries than the host population in Sweden should be acknowledged, as it is a considerable difference.

Yet, we re-wrote the conclusion on page 23 to sound as following:

These findings and prior awareness of health of immigrants from countries in the focus of the present study should be acknowledged, particularly with the continuing expansion of the European Union.

7. On page 5, the exact algorithm by which questions were transformed into diagnostic categories is unclear. I do not think the authors can report psychiatric illness and psychosomatic illness based on these questions.

Please, see the answer to Comment 2.

8. The authors have used the weighting mean and the relevance is not clear. They refer to by 80 post strata. It is not clear and maybe not relevant for the core analysis.

We thank Dr. Bhui for a constructive comment. The passage about weighting and weighted sample size was removed due to irrelevance.

9. Is there any validity work showing the classification of poor social network is valid.

We could not find the validation of exact the same classification of social network index as we have done in the present study. However, this variable has long been used in studies of health of persons born in the countries of the former Soviet Bloc and showed to be a significant health determinant.

We included the following to illustrate this (page 6):
Social capital is another widely-discussed health determinant. In Russia, and supposing in other former Soviet Bloc countries [14] social capital takes the form of informal social networks, such as the family, relatives and friends, rather than formal institutions [15]. Studies in the countries of the former Soviet Bloc and on immigrants from these countries show that the support received from friends [16, 17], emotional support in general [18] and social support [18-21] have profound effect on health. Thus those, without social networks are especially vulnerable [15]. Brown and Harris [22] underlined the importance of social network for mental health, suggesting that coping with stressful events depends on whether a person has access to a social network as a protective factor.

However, recognizing the possible limitation of this variable, we included the following on page 12:

The social network variable measured as the frequency of contacts does not reflect the ‘extensiveness’ and ‘resourcefulness’ of these contacts, nevertheless, it represents the potential possibility to receive support (emotional or material) by the respondents if in need.

10. On page 9 they report social network and occupational status as being acculturation variables but these would not ordinarily be considered so.

We agree with Dr. Bhui and have deleted this text.

11. Page 13, at one stage the analytic groups are proposed to have been formed on geographical and political considerations and, therefore, the interpretation of the findings should be placed in the same context.

We thank Dr. Bhui for this comment.
We have deleted the incorrect text and replaced it with the following on page 21:

Nevertheless, immigrants of differing backgrounds had to be combined in larger categories (“other East European countries” and “the former Soviet Union”) to achieve sufficient statistical power and to conduct the analyses in the present study. Poland was categorized separately due to a large number of Polish immigrants in Sweden. The FSU was categorized separately due to the differing economy and health among the inhabitants of the FSU compared to other East European countries [14].

The results of the study are presented accordingly, i.e. separately for immigrants born in Poland, other East European countries and the FSU.

12. Page 14, there are some analyses to support the findings of the main study; these were added in response to previous reviews, but seem misplaced in the discussion. They may be considered unpublished material and included in the main results section.

We agree and have moved this paragraph to the result section as a last paragraph.
13. The study refers most methodological issues of importance to reference number 38, which makes it difficult to judge the current paper on its merits.

We do not agree with this comment. The study with this reference number is referred to only for more detailed description of the SALLS database, from which we took the data for the present study. However, the SALLS database has also been described in the present study. For example:

(p.10) The information was collected by trained interviewers in face-to-face interviews as part of the Swedish Annual Level of Living Survey (SALLS) between 1994 and 2001.

The reliability of the survey questions was tested in other contexts in Swedish [44] and Latin American [45] subsamples and was found to be high. Moreover, the external validity of the self-reported severe or very severe longstanding illness was high. An over-reporting of 10% and under-reporting of 20% was found [45, 46].

Though ideally it would have been desirable to test the reliability of the questions in the immigrants from the former Soviet Bloc as well, this was not possible, as the current study was undertaken 5 to 12 years after the original surveys (1994 to 2001) were performed.

(page 22) The strengths of the SALLS extensive data balanced the limitations. SALLS is performed annually by Statistics Sweden and is designed to obtain information about the Swedish population’s living conditions. Statistics Sweden is a governmental statistical bureau with a long tradition of carrying out national surveys and analysing the collected data. The data collected by Statistics Sweden is used in multiple studies and for the national statistical reports. The reliability of survey questions is high and the non-response is rather low. The non-response rate in SALLS during 1994–2001 was about 20%, mostly due to refusal to take part. In the earlier samples it was found that those who refused to participate (2/3 of nonrespondents) had the same mortality rate as the respondents. Regrettfully, it was impossible to estimate the particular non-response in the immigrants in this study, as the sample in SALLS is not chosen according to the country of birth.

A more detailed description of the SALLS can be found elsewhere [66].

As one can see, other studies are referred to as references when presenting the methodology. Moreover, further studies are referenced in the description of the statistical methods applied in the present study (pp.13-14):

The data were analyzed by STATA software package [47]. Unconditional logistic regression [48] was applied to estimate odds ratios (OR) of poor self-reported health, and the results were presented with a 95% confidence interval (CI). First-order interactions between country of birth and independent variables (excluding migration-related) were tested. Full models included these independent variables analyzed together. An analysis including the migration-related variables was likewise performed on immigrant groups only (the FSU immigrants were used as a reference). The Hosmer and Lemeshow goodness-of-fit test [49] was used to assess the fit. All models showed satisfactory fit (p>0.05).

14. Overall, I think the current paper could be a brief communication and an exploratory paper suggesting higher rate of self reported mental and physical symptoms for some but not
other immigrant groups creating the need for more sophisticated and theorised research. The
current paper appears to over interpret the data which are limited and tries to draw too many
conclusions about culture and immigration without having the primary data available to
support the interpretations. I think drawing inferences about immigration policy from this
current paper is, therefore, not justified (for example in abstract).

We thank Dr. Bhui for the review of our article.
In response to the comments, we have improved the scope and the purposes of the present study,
clarifying some points that were not clear in the previous manuscript. We have also updated
Introduction, giving a more historical background to migration from the former Soviet Bloc to
Sweden and attached Tables 1 and 2 to illustrate it. We found this important because the
premigration experiences (which can be indirectly accounted for by including the year of
immigration to Sweden or years in Sweden) could possibly determine immigrants’ health and are
important to keep in mind.

We have modified the presentation of results to make it clearer and created Tables 3, 4 and 5.

It is important to mention, that a mistake made in the previous manuscript was discovered by re-
checking the analysis. It showed that the analysis in the previous manuscript was based on the years
1994-2000, although it said 1994-2001. We have corrected the mistake and conducted the analysis
including the data from eight years 1994-2001. That is why the results presented in the tables are
slightly different from the previous manuscript. However, the numbers changed marginally, which
did not influence the main results and findings of the study.

Due to modifying the scope and the purpose of the study, we were forced to reformulate the
presentation of results in the Result section, as well as to rearrange the Discussion.
We believe, that these measures improved the manuscript, making it more focused,
We have also toned down the conclusion (please, see the answer to Comment 6) to avoid over-
interpreting of the results. We are aware of the limitations of the data used in the present study and
are frank to discuss them at the end of Discussion (from page 20 “It is very difficult to compare…”
to the page 22, paragraph 2). Nevertheless, we find the results of the presented study to be
interesting. One of the advantages of the presented study was studying immigrants from the former
Soviet Bloc countries as a group, which has not been done before. There is a large body of studies
on health determinants of persons born and living in the countries of the former Soviet Bloc,
however it is seldom discussed whether the same determinants affect the health of persons born in
the former Soviet Bloc after migration or is it other factors. This was, however, done in the present
study.

We therefore, strongly feel that this manuscript should not be diminished to a short communication.