Author’s response to reviews

Title: Utilisation of rehabilitation services for non-migrant and migrant groups of higher working age in Germany – results of the lidA cohort study

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Author’s response to reviews:

Dear Prof. Dias,

Thank you very much for your review and the kind offer to consider a revised version of our manuscript, especially after extending the revision period.

We are very grateful to the reviewers for their thorough and constructive comments and suggestions, which we have addressed carefully in revising the manuscript. Please see our detailed response to the reviewer’s comments, point by point on the next pages. The numbering of lines refers to the numbers in the newly revised manuscript where changes are marked in yellow.

Sincerely,

Chloé Charlotte Schröder (on behalf of all authors)
Reviewer reports:

Alexander Rommel (Reviewer 1): The numbering of lines refers to the numbers in the original manuscript.

I strongly recommend that the text be checked for correct English by a native speaker/proof reading service.

The revised manuscript was edited by an affiliated and reputable English language editing service, so that some phrasing in the text changed which is marked in yellow.

Line 44 „If the ability to work is at risk or impaired due to poor health or functioning, rehabilitation intends to improve, restore or inhibit deterioration to prevent premature work exits [4-6].” Please check sentence structure: improve, restore and inhibit all refer to deterioration what doesn't make sense.

Changed as suggested (page 2, line 44-46).

Line 44 et seq. Readers of international journals are usually not familiar with the German health system. Please describe in some sentences the role and legal foundation of medical rehabilitation in Germany and the access/entitlement to rehabilitative services.

We thank the reviewer for pointing out that this needs further clarification. Thus, we added more information about the system of rehabilitation in Germany to the manuscript (page 2-3, line 47-63).

Lines 50-51 MB is not an adequate abbreviation. Use PMB (people with…) and/or EMB (employees…).
The abbreviation has been changed as suggested in the whole manuscript in EMB or PMB where appropriate (page 3, line 66/68 f.).

Line 55 "a group exhibiting…” This description generalizes negative aspects of the health of people with a migration background. Studies show that the group is very heterogeneous and, in addition, is by no means consistently in poorer health than the German ethnic population. Please formulate more differentiated.

We thank the reviewer for the advice and reformulated the paragraph (page 3, line 70-75).

Lines 58-59 This is not mainly due to length of stay but due to the fact that many people with migration background are born in Germany.

Changed as suggested (page 3, line 78).

Line 95 et seq. Even if the sampling process can be found elsewhere a concise description should be given. Please describe more in detail the register that was used, the rationale behind the sampling (two age groups) and other information that is useful to comprehend the study approach.

We thank the reviewer for the advice and added further information about the study (page 5, lines 117-128).

Line 115 et seq. Both sentences say more or less the same (&gt;&gt; binary outcome).

Both sentences were combined (page 6, line 148-149).
Due to the statistical problem of unobserved heterogeneity, direct comparisons of effect estimators from different logistic regression models are not interpretable. Therefore, the results section must be revised. Basically, there are two options for this:

* All passages directly comparing ORs across models are omitted and cautious comparisons are made only with regard to persistent/non-persistent statistical significance (suboptimal solution).

* Or, according to the literature, alternative estimators can be used. For instance, average marginal effects that are robust against unobserved heterogeneity can be estimated in STATA using simple post-estimation commands. Since these effects are directly comparable across models the findings would become more informative.

We thank the reviewer for this critical appraisal of our analysis. We agree with the reviewer and have added analyses of average marginal effects to be able to compare effects across models. Therefore, several corresponding sentences were added within the abstract, the methods, the results section and the discussion.

Please consider to omit the interpretation of Pseudo-R-measures. These are no valid measures of model fit and the information that Nagelkerke increases with the inclusion of more covariates is rather trivial.

We considered the comment and have deleted the interpretation of R-measures. However, we have left the information in the tables as they could be of interest for the readers.

Title and research questions only partly fit the format in which the models are reported. If the article is to focus on the effect of migration, as the title and introduction imply, it would make sense not to report and interpret the effects of the covariates. If these effects are to be interpreted, the focus of the article changes. Then it is more about the analysis of a broader spectrum of determinants for the use of rehabilitation. Please consider to harmonize the objectives of the article and the reporting/interpretation of results.
We thank the reviewer for the advice. We focussed the results more on the effect of migration, suitable to our study objective. Though, to our knowledge, secondary findings should also be mentioned shortly in scientific articles as they could build the ground for further research and could be therefore informative for the readers. So, we permitted some short paragraph in the results and the discussion.

Tables 2 &; 4  Note that in table 1 there are gender differences in the sample composition between MB and non-MB and that gender is usually an important determinant for all kinds of health services utilization. This means that gender should be considered as a relevant covariate. In addition, existing evidence suggests that associations between migrant background and health outcomes differ considerably by gender. In order to ensure a gender-sensitive research approach I strongly recommend to calculate models either separately for women and men or to include gender into the models and to test for interactions between gender and MB.

We thank the reviewer for this critical appraisal of our analysis. We have tested interaction effects for gender and migration background, which were not statistical significant for any outcome of rehabilitation. Though we still adjusted for gender in the analysis. We have included a corresponding note in the methods section (page 6, lines 169-171).

Line 296 et seq. There are other findings indicating that utilization of rehabilitation due to mental disorders like depression and somatization is higher among migrants in Germany. The correct interpretation would be that there is a higher medical need for this kind of rehabilitation among migrants. The interpretation given in the text that psychosomatic rehabilitation resembles inpatient utilization in general is rather misleading.

We agree with the reviewer that this interpretation is misleading, so we changed the paragraph (page 15, lines 356-360)

Joana Joana alves (Reviewer 2): The article entitled "Utilization services for non-migrants and migrant groups of higher working age in Germany - results of the lidA-cohort study" aimed to analyze whether the use of rehabilitation services differed by migration status (non-migrants versus first- and second-generation migrants). The authors concluded that differences in use were not fully explained by the variables considered.

The findings are important to those authors with similar research interests.
However, I propose some changes to clarify the manuscript.

We thank the reviewer for the thorough review and the positive remarks regarding the importance of our findings for the research community.

Abstract:

1 The background and the objectives of the study are not very clear.

This part has been restructured so that it should be clearer (page 1, lines 8-14).

2 In the methods section is not possible to distinguish the dependent from the independent variables.

This information has been added (page 1, lines 16-18).

3 The word "chance" should be replaced by "likelihood.

Changed as suggested (page 2, line 22).

4 The conclusions are confusing and need some clarification.

This part has partly been restructured so that it should be clearer (page 2, lines 28-33).
Introduction:

Page 2, line 44: I could not understand the differences between DRV and DGUV. Why some people are entitled to one and not to the other? Are they both included in the lidA-cohort?

We thank the reviewer for pointing out that this needs further clarification. Thus, we added more information about the system of rehabilitation and it’s providers in Germany to the manuscript as already suggested from Reviewer 1 (page 2-3, line 46-63).

To answer your question: It is technically possible, that the participants in the lidA-cohort could have used rehabilitation from any provider. Though our analysis was restricted to still employed persons, who probably have used rehabilitation with the provider DRV or DGUV, which we don’t know.

Page 3, line 67: these results were found in which countries?

This information has been added (page 4, line 87).

Page 4, Line 87: differ in terms of what? The objectives need to become clearer.

The objectives have been rephrased (page 4-5, lines 108-112).

Methods

Page 5, line 64: I suggest a reformulation of the section study design and participants. First to describe the methods from lidA; then to describe the criteria for inclusion and exclusion of participants. Now they are somewhat mixed.

We thank the reviewer for this suggestion and changed this section, also fitting to what Reviewer 1 recommended. Under the heading ‘study design and participants’ first the sampling process and study design in general are described, followed by the inclusion and exclusion criteria (page 5, lines 116-143).
Page 4, line 107: exclude the sentence "note the paragraph below about migration background". It is clear from the objective that this study wants to analyze differences according to MB.

Deleted as suggested.

It is not clear to me what happened to those participants who were not born in Germany, but had German citizenship and both parents were born in Germany. Imagine that they were born in other country because parents were spending a period abroad… They were included in G2 MB?

These people were included in G1 EMB, as the criteria for this group was to be born abroad. The reason for this is the strictly defined reference group of non-EMB with place of birth in Germany, German nationality and both parents born in Germany. We have included a corresponding note in the methods section (page 6, lines 157-159).

Covariates section: The reason for including those covariates should be explained. E.g.: why should we expect that physical and psychosocial work exposure to be associated with the use of rehabilitation services? Also, the occupational status is very important per se, and not only because it represents education. It reflects social influences, networks, working conditions, etc.

We thank the reviewer for this comment. The German pension insurance is recommending and physicians are working with a checklist to assess need for rehabilitation. Within this checklist different physical and psychosocial work exposures are considered, so that we decided to include them as additional variables in terms of utilisation of rehabilitation. We have included a corresponding note and references in the methods section (page 7, lines 180-183). Besides, we also believe that the occupational status is important and reflecting several things, not only social status, so that it was already considered as a covariate.
Page 5, line 142: why including variables (as physical and psychosocial work exposure) that explain poor health, since the variable for poor health is also included? Also, why did the authors choose to include two psychosocial variables?

The intention was to see whether work-related factors could provide additional explanatory power for rehabilitation services beyond the health aspect. Several psychosocial factors were included because they have different explanatory roles and are also considered to assess the need for rehabilitation, as mentioned above.

Page 6, line 150: why the need for dichotomization of the variable for physical and psychosocial work exposure? The cut off values differ according to each variable: the authors chose tertiles for some variables, high vs low for other variables, and \( \frac{3}{4} \) for other. The reasoning should be explained.

We thank the reviewer for pointing this out. Nearly all variables were dichotomized in terms of interpretability.

Though, for the physical work exposure, meaning physical environmental and burdensome factors, we changed the cut off to ‘half of their working time’ as we evaluated this measure in the meantime by our data. When validating with the SF12 Single Item, people already working more than half of their working time had increased poor health.

In case of ERI, it can be transformed to a binary variable (values \( p 1.0 \) vs. \( \gt; 1.0 \)), which is highly controversial discussed among experts nowadays. We used tertiles as recommended by Siegrist et al., which is standardly used nowadays (Siegrist J, Starke D, Chandola T, Godin I, Marmot M, Niedhammer I, Peter R. The measurement of effort-reward imbalance at work: European comparisons. Social Science & Medicine. 2004;58:1483-99. doi:10.1016/S0277-9536(03)00351-4.)

Therefore, we changed the highlighted parts in the covariates section in order to make it clearer.

Page 6, line 153: The methods for the ERI questionnaire need to be clarified. These methods are taken from the ERI, or they were created by the authors?
The items from the ERI questionnaire were implemented in the lidA questionnaire and analysed with the same methods as Siegrist - who developed the ERI instrument, Siegrist (1996) - recommended in the publication mentioned above (Siegrist et al. 2004). We included some additional information in the methods section (page 7, lines 193-195).

15 Page 6, line 155: when the ERI ratio had up to two missing items, the missing observations were replaced by the individual mean. However, these missing values might not be missing at random… imagine that the participants with higher stress deliberately choose to omit some answers. It is not clear for me why using multiple imputation approaches for some variables and this particular method for the ERI variable.


16 Page 6, line 175: language spoken at home was included as a "non-work related factor". The authors justify this inclusion since lacking knowledge of German might be a barrier to access (page 16, line 371). But not using German as a first language at home does not mean that they do not understand the language… Since LidA questionnaire was applied in German (page 16, line 382), the participants had to have at least a minimum knowledge of the language. Furthermore, other studies use the language spoke at home as a definition for the migrant status.

We thank the reviewer for this notification. In our analyses, the language spoken at home serves mainly to take into account possible differences between migrants, knowing that the language spoken at home (or mother tongue) is otherwise used to identify third generation migrants. We have included a corresponding note in the methods section (page 8, Lines 219-221).
Page 7, line 182: "hierarchical logistic regression modelling" can be confused with "Hierarchical linear modelling", a regression-based analysis that takes the hierarchical structure of the data into account (e.g. students clustered within classrooms within school). Therefore, I suggest to call it only logistic regressions, mentioning that they were performed in a sequential way, adjusting block-wise for the group of variables.

We changed it to block-wise logistic regression to be consistent with the abstract (page 8, line 226/239).

Discussion:

Page 13, lines 298-301: it is not clear to me what the authors wanted to say.

We changed the paragraph as also suggested by Reviewer 1 (page 15, line 356-360).

Page 14, line 317: Can we observe a lower use of outpatient services among participants with MB because they do not have enough conditions at home to be treated in outpatient care?

We thank the reviewer for the comment. If ‘conditions at home’ imply resources at home meaning other people or family members who are supporting with e.g. care of children or parents in older age, it could possibly be an influential factor, we agree. Though in our analyses we have not analysed these factors and focussed more on work factors which can influence utilisation of rehabilitation. But we will consider for further analyses on barriers to entry the health care system in Germany.

Page 15, lines 346-354: this was not the focus of your study, and thus this is not relevant for the discussion. I suggest to comment only when the group of variables included decreased the coefficients for MB.

We agree with the reviewer and shortened this paragraph, also according to what Reviewer 1 said. We focussed the results more on the effect of migration, suitable to our study objective.
Though, to our knowledge, secondary findings are often mentioned as well, so we permitted some short paragraph in the results and the discussion.

Page 16, line 371: Again, I have some doubts about the language mainly spoken at home, since we are talking about migrants.

See comment above concerning the language.

Page 16, lines 377-379: this is not clear for a non-German reader.

We changed this sentence as suggested. It should be clearer in addition to the new section about study design in the methods section (page 17, line 430-431).