Author’s response to reviews

Title: Study approach and field work procedures of the MentDis_ICF65+ project on the prevalence of mental disorders in the older adult European population

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Author’s response to reviews:

Dear Prof. Ryan,

Thank you for giving us the opportunity to submit a revision of our manuscript entitled “Study approach and field work procedures of the MentDis_ICF65+ project on the prevalence of mental disorders in the elderly European population”. We have revised the manuscript according to the feedback from the reviewers, and herewith submit our revised manuscript to BMC Psychiatry on behalf of all authors.

We have carefully considered your comments and address these in the following. Some of the reviewer comments have been shortened to improve the readability of this response letter.

Reviewer 1:

1. This paper describes the study approach and the field work procedures of the MentDis_ICF65+ project on the prevalence of mental disorders in the elderly European population. The authors indicate that the manuscript contains detailed information on the adaptation, feasibility and psychometric properties of the instrument and detailed results on the sampling, final sample characteristics and representativeness as well as field procedures and that these procedures and findings have not been previously reported. However useful this paper may be regarding the description of the MentDis_ICF65+ study protocol (ref 17); it is unclear to me what else this article adds to the literature on its own. Perhaps the authors should make that clear in the text.

   Author reply: The study protocol described the planned methodology for carrying out the MentDis study. This paper now describes in detail how the MentDis project was actually carried out. Its main aim is to provide comprehensive and detailed information on the methodology and field work procedures, which are not described elsewhere. However, it is not the primary aim of this paper to make a novel contribution to the literature.

2. Furthermore, the authors should state clearly which mental disorders will be considered. For example, I thought that dementia would be considered in this population of 65+; however,
they describe later in the study how the participants with cognitive impairment and MMSE lower than 18 will be excluded. In a sense, it seems like a missed opportunity of investigating dementia prevalence too.

Author reply: The MentDis study focuses on DSM-IV axis I disorders. Dementia has not been included in this survey, as it has already been extensively investigated in other studies, whereas other mental disorders have not. Also age-appropriate diagnostic in-truments for dementia already exist, whereas for mental disorders these have been missing for older people up to now. We now clearly state this in the background section of this manuscript.

Page 5, line 92-94: “Because dementia has already been extensively assessed in previous studies and age-appropriate measures exist for this disorder, it is not included in this study.”

3. In terms of presentation, the writing style could be greatly improved.

Author reply: The manuscript has undergone another thorough proof check and corrections by a professional native English speaker.

4. Abstract/ Results: The only information I gather here is N=3,142 with a mean response rate of 20% which is very low.

Author reply: We have expanded the result section in the abstract to make it more comprehensive.

Abstract: “Sociodemographic differences between the study centres appeared for the place of birth, number of grandchildren, close signif-icants, retirement and self-rated financial situation. The comparison of the MentDis_ICF65+ sample with the catchment area and country population of the study centres revealed significant differences, although most of these were numerically small.”

5. Background: The authors talk about old age. However 65 is younger old age (lines 57-58). However, they further describe advanced age (ref 9 & 10) without providing the age brackets.

Author reply: We have rephrased the sentence to make it clearer that ageing in general is associated with increasing health issues (line 64). Furthermore, we have also modified the sentence explaining that Scott et al. 2008 and Kessler et al. 2010 report decreasing prevalence rates for people > 65 years, and Scott et al. 2008 also reports that rates de-crease even further for people > 80 years (lines 82-83).
6. Methods/ Survey procedure: I suggest renaming this subsection into "procedure". Move lines 215-218 after the next paragraph on data quality (lines 219-225).

Author reply: Thanks, we have renamed this section and moved the lines after the next paragraph accordingly.

7. Table 1: I suggest deleting the row indicating number of stages - it is evident from the next two rows below (avoid repetition).

Author reply: We have deleted the “number of stages” row.

8. Table 2: I suggest deleting the 2nd row indicating the contact to the sample - as a note or in text or as appendix.

Author reply: We have deleted the “contact to sample” row and instead added this in-formation as part of sampling description in the result section, page 13, lines 334-337.

9. Table 4: Remove shading from table

Author reply: Ok, the shading has been removed.

10. The authors should highlight and summarise the main findings in abstract and in conclu-sion before highlighting what the study could add in the future.

Author reply: We have now expanded on the main findings in the abstract and in the conclusion. The implications for future studies are described subsequently.

Reviewer 2:

1. I thank the authors for undertaking this significant effort to improve the methodology and empirical knowledge of mental disorders among older people and to describe their work thoroughly both in the present and previously published papers regarding the Mentdis_ICF65+ study. In general, the paper is well-written and gives a thorough descrip-tion of the study. I have a few comments.
Author reply: We thank the reviewer for this positive feedback.

2. In the original publication of the study protocol (Andreas et al, BMC Psychiatry 2013 13:62) the authors identify 4 phases of the study. The last of these, the longitudinal follow-up, is not mentioned in the present paper and only 3 research questions for MentDis_ICF65+ are stated. One wonders whether this is because this phase is not yet completed or whether such a follow-up is no longer planned. Can the authors comment on this?

Author reply: This manuscript focuses on the methodology and field work procedures of the cross-sectional study (phase 3), and includes a description of the two preparatory phases (1 and 2). As data analysis of the longitudinal follow-up is not yet completed, the description is not included in this manuscript.

3. Regarding Research Question 1: In the Results section (Lines 303-308), a brief overview of the field work is presented. However, nothing is mentioned on the actual problems that were detected, how they were handled and in what way this changed the content of the interview. In its current form, I think this description is too vague to be placed in the Results section of the paper. What did this work contain and what did it lead to? I suggest that the authors give some examples of what kind of problems that were detected and how they were managed. Another possibility is to categorize the problems into different types to give some overview.

Author reply: Thank you for this comment. We have added more details on the actual problems that were detected and include examples. All problems that were identified in the pre-test phase were solved, e.g. typos corrected, unclear questions were rephrased, programming issues resolved.

Method, Page 13, lines 311-316: Most frequent problems were usability problems (120), followed by acceptability (63) and programming (34) problems. Most of these problems were mild (146). Usability problems were for example missing words, typos or inconsistent format, acceptability problems included unclear or complicated questions, and programming problems were for example an inconsistency between the respondent booklet and the interview.

Method, Page 17, lines 428-431: In the pre-test phase the applied multi-method approach proved as an indispensable step, which identified problems with the interview’s acceptability, usability and programming and allowed to solve these problems prior to the field survey.

4. Regarding Research question 2: In the Statistical analysis (lines 235-242) several statistical analyses (calculation of specificity, sensitivity, NPV, PPV and Yules Y) are described. The
results of these analyses are not presented in the present paper. Instead, the authors refer to another publication (reference 18) in the Results section. The results of these statistical analyses are thus already published. To me, it seems unnecessary to report details regarding these statistical methods when the results from these analyses are not present in this paper. The authors could even consider omitting the results regarding this research question from the Results section and perhaps just give a very brief summary in the Methods section instead (with reference to the original publication). This would shorten the paper or leave room for more information regarding RQ1 (see Comment #x).

Author reply: We have shortened the description of the statistical analyses of research question 2 and omitted description of analyses, which are not reported in the results. Furthermore, we have shortened the results of research question 2, but still mention them briefly in order to maintain the comprehensibility and structure of the study phases.

5. In the publication of the study protocol (referenced above), the authors state that MMSE >27 points was an inclusion criterion for the study. In the present paper, the cut-off seems to have been changed to >18 points. Could the authors comment on this discrepancy? The reasons for this change could also be mentioned in the present paper.

Author reply: We thank the reviewer for his detailed feedback. The MMSE cut-off point in the study protocol was unfortunately a typo, the cut-off >18 points is correct. We apologize for this mistake and explain this change in the present paper (footnote, page 7)

6. Regarding presence of significant cognitive problems: Table 2: In all study centers except for Jerusalem, the only indicated reason for exclusion is cognitive problems. Thus, the number of excluded participants may be used to calculate prevalence of significant cognitive problems among responders. It seems that the proportion with such cognitive problems (MMSE < 19) varies and is very low in some centers (e.g. 0.5% in Madrid, 0.7% in Geneva compared to 4.2% in Ferrara). Do the authors have an explanation to the low rates of significant cognitive problems in some centers? These rates should arguably be higher considering that the participants were up to 84 years old and that institutionalized persons were not excluded. These rates could indicate that significant cognitive problems were more common among non-responders, which puts the representativeness of the responders into question. For example, was it specified in the invitation letter, that major cognitive problems were an exclusion criterion for participation?

Author reply: We think that the recruitment procedure, having to reply to a written invitation letter and contact via phone may have led to people with cognitive problems to refrain from participation. The invitation letter described that participants will take part in a 1 ½ hour
interview, it did not explicitly state that cognitive problems were an exclusion criteria. We think that this had already had the effect that older people with cognitive problems decided not to participate. Moreover, due to this recruitment procedure, we do not think that calculation of a prevalence rate of cognitive problems is justifiable. Furthermore, completing a standardized comprehensive diagnostic interview with a duration of over 90 minutes on average does require fair cognitive fitness. Hence there may be a response bias in our sample. We have added this as a limitation in the Discussion section.

7. Regarding the response rate: The response rate was about 20%, which was within the range expected according to the publication of the Study Protocol (referenced above). My questions are:

1. Why did the authors expect a response rate of only 20-25%?

Author reply: The expected conservative response rate was based on advice from previous experience of the company, which conducted the sampling and contact to sample.

2. Prior to the recruitment process, did the authors take any measures to increase response rate, for example by spreading information about the study through media and service providers for older people, or getting endorsement of the importance of the study from local authorities?

Author reply: A number of strategies were used in each county to spread information about the study and engage potential participants. The following measures were taken: publication of information about the importance of mental health in old age and our specific study through local newspapers and local TV networks, engagement of a patron (celebrity known to our target sample). Informing participants about the study results to make them interested in research.

3. Could the authors extend the discussion regarding factors contributing to the low response rate? This might be helpful for the conductors of future studies. For example, could the fact that the study was focused on mental disorders contribute? Many epidemiological studies of older people are multidisciplinary, covering also aspects of physical health, blood sampling, imaging etc, which may increase the value for participation for many people, especially those without mental health problems and those with a negative attitude towards ventilating such problems.

Author reply: We tried to address potential fear of stigma and discrimination issues by using a more open wording for advertising and informing about the study, i.e. the study was named
“well-being in older adults” (instead of mental health). We now also address this point in the discussion.

Page 18, lines 453- 456: “It may be possible that the low response rate was also as-sociated with the fact that the study focused only mental disorders. However, we tried to address the potential effect of negative attitudes towards mental health issues by labelling the study “well-being in older adults”.

8. Regarding the discussion on representativeness: In the discussion (Line 458-469) the authors discuss discrepancies in the representativeness of the samples as a limitation. At the end, although the authors don’t explicitly state so, their writing seems to invoke the argument (Line 468-469) that representativeness is not necessary for scientific studies. They reference a paper (reference no. 45) that criticises the view that scientific inference requires representative samples. Reference 45 clearly makes a case for that many im-portant scientific discoveries have been made without representative samples. However, these discoveries are emanating from hypothesis testing research. In fact, the authors of ref. 45 explicitly state that in descriptive studies aiming to examine the health status of a population (of which the present study is clearly an example), representativeness is nec-essary. It seems to me that representativeness is of high importance to a study like the present. I suggest that the authors reconsider their discussion on this topic. In my opinion, it should also be added as a limitation that since the response rate was only 20%, there is a significant risk of bias regarding unmeasured variables.

Author reply: We thank the reviewer for his critical discussion about the representativeness and have revised the discussion of this issue according to the recommendation:

Discussion, limitations, Page 19, lines 479- 481: “The needs for representativeness have been critically discussed [43, 44] and the unknown added risk of bias of unmeasured var-iables due to the low response rate needs to be critically kept in mind, when interpreting the findings.”

9. Regarding the use of Somers' d and its interpretation. In the Statistical analysis section, the meaning of the Somers’ d statistic is explained. However, I have difficulties to under-stand what additional interpretable information that is given from the Somers’ d in Table 3, 4 and 5. For example, in the discussion, the authors state (line 452-454) that most dif-ferences between sample and catchment area or country were "numerically small" and with "very small associations". I accept this interpretation of the general picture. However, looking into Table 4, it can be seen for example that the proportion of retired persons in Madrid was 72.1% for the sample and 52.4% for the Catchment area, an absolute differ-ence of 20%. This particular difference seems quite large and could be of importance to the results of the
study. However, Somers' d has a value that does not seem meaningfully different from the Somers' d for the London centre (0.001 vs. 0.000), where the absolute difference in retirement rate between sample and catchment area is only about 4%. In fact, Somers' d in most cases throughout Table 4 and 5 is just about zero, although it can theoretically take a value from -1 to 1. I find a value of about zero to be difficult to interpret, especially when similar values are given to associations that seem appreciably different. The authors also present the absolute proportions and a Chi-square test for differences in proportions, which is probably much more meaningful to most readers. I suggest that the authors either omit their analyses of Somers' d, present a stronger case for using it, or utilize a different test of heterogeneity between study sites for Table 3 (eg. Higgins I²).

Author reply: We agree with the reviewer that the interpretable information provided by Somers’d is not entirely meaningful and we have therefore followed the reviewer’s recommendation and have omitted Somers’d from the analysis. We now only present results on absolute proportions and mean differences (Chi-Square, ANOVA) in Table 3.

We have adapted the results and discussion section accordingly and discuss differences between the sample and catchment area and country with regard to numerical differences (Table 4 and 5).

10. In Table 3, last column, the heading is "p-value (Somers' d/n2)" What does n2 mean? This term is not explained in Statistical analysis or in the Table.

Author reply: We are sorry for this typo. It should have stated $\eta^2$ partial as a measure of effect size for the Analysis of Variance (ANOVA). However, we have now followed the reviewer’s recommendation and omitted effect size measures, according to the recommendation of the reviewer (point 10).

11. Line: 327-328: This sentence gives the impression that Table 1 includes information given in Table 2. Please rewrite.

Author reply: Sorry, we have corrected this. The information is provided in Table 2.

12. Line 441-442: "While Alonso…." is an incomplete sentence.

Author reply: Thank you, we have corrected this sentence.
The comments from the reviewers have been very valuable and helped to improve the clarity of the manuscript. We hope that it is now acceptable for publication in BMC Psychiatry and look forward to hearing from you.

Sincerely,

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