Reviewer’s report

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

Version: 1 Date: 29 Aug 2017

Reviewer: Robert Sigstrom

Reviewer's report:

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 description of the study. I have a few comments.

Comment #1

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 planed. Can the authors comment on this?

Comment #2

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.
Comment #3

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).

Comment #4

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.

Comment #5

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?
Comment #6

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).

In the discussion, Line 435-436: the authors state that a mean response rate of 20% is "comparable to that of previous studies with similar recruitment procedures."

The authors support this statement with a reference to the Pew Research Center (PRC), which conducts public opinion polling in the United States. While the PRC may have used similar sampling recruitment procedures, it does seem more appropriate to compare the response rate to other epidemiological studies of mental disorders among older people (in which the response rate is typically no less than 40% and often 60% or higher). Many studies use a similar sample recruitment process as the present study (a letter followed by a telephone call). In comparison, it seems to me that the response rate is remarkably low, even in the study centres with the highest response rate.

While it is fortunate that the authors have been able to conduct a comprehensive analysis of sociodemographic representativeness of their samples, data availability varies between study centres, and the authors are only able to compare the age and gender of participants and those who refused. It therefore seems to be unknown how they might have differed regarding some of the most important variables of interest, i.e. mental and physical health. Even if the consequences of a low response rate can be discussed, which the authors do, these consequences seem to be largely unknown in this study. A priori, a higher response rate should decrease the risk of bias with respect to these unmeasured variables.

My questions are:

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

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?

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.
Comment #7

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 important 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 necessary.

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.

Comment #8

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 understand 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 differences 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 difference 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).

Minor comments

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.

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

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

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

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