Reviewer’s report

Title: Distribution and determinants of functioning and disability in aged adults - Results from the German KORA-Age study

Version: 4 Date: 11 December 2012

Reviewer: Bart Klijs

Reviewer’s report:

Dear Editor,

Thank you for inviting me to Re-review the manuscript by Strobl et al. The authors’ revisions substantially improved the original manuscript. However, there are two main points for which I specifically want to express my concern. The first is that, in my opinion, the part of the analysis for which the results are shown in table 2 does insufficiently account for differences between proximal and distal determinants of disability (see points 14 and 25). The second is that it is not sufficiently clear what this paper adds to existing literature. Furtermore, I think language may need improvement.

Below I give a point by point reply to the authors’ comments.

MINOR ESSENTIAL REVISIONS

Background

2. I agree that the (wording in the) background part has improved, but in my view, the importance of the research (what’s new) remains insufficiently explained (see also point 18).

3. Agree

4. Agree

5. Agree

6. ok, but it might be interesting to do a sensitivity analysis here?

7. Agree

8. I agree with the argument that the validity is under threat if the construct is underrepresented. However, repeated use of the variables in within the KORA studt may be a sufficient reason to believe that the bias remained limited.
9. Agree
10. Agree
11. Agree
12. Agree
13. Agree

14. I do not refer to reversed causation here. I mean that an interpretation in terms of causality is seriously hampered as the analysis does not account for differences in the position of the “causal chain”. For instance, low SES -> detrimental lifestyle -> increased disease risk -> presence of disability. When all these variables are put into one analyses, as the authors did, part of the effect of for instance SES is captured by lifestyle. In my opinion, this is a point that requires serious improvement in the analysis strategy. See also point 25.

15. Agree.

16. Yes, but what could be the rationale behind this? Why does the association between physical activity and disability remain after control for disease and why is this important. What does it mean?

17. I agree this is an improvement, but I am not sure whether non-participation associated with ill health inevitably affects the estimated impact of diseases on disability.

Methods

MAJOR COMPULSARY REVISIONS

Background

18. I agree that the background part has improved. A main issue that remains unresolved in my opinion is what exactly this study adds to existing (international) literature. If your main focus is on determinants of disability, I would expect a more thorough review of existing literature and a more thorough analysis of what is unknown. It may be that there are not so many German studies conducted, but, (why) would you expect determinants to different in Germany than in other Western countries?

19. The background part has improved, but still may need a more thorough embedding in existing literature.

Methods

20. agree (“Smoking habits were classified as …based on four questions” -> there are only three questions).

21. Thanks for the improvement. Do I understand you correctly that the current study population consists of a single follow-up survey for which the participants
were selected from S1-S4 of the KORA-Age study? I assume, but I am not sure. In which years were the various parts of the study conducted?

22. If you choose to include abstainers versus non-abstainers I think you require a more elaborate explanation on why you would, a priori, expect an effect on the presence of disability.

23. I can see that your study is bound to the data available, but this point may need some explanation as it may not be the most obvious choice. Also, given the small numbers for occasional and regular smoking (table1) it might be wise to combine these groups into current smokers to achieve more similar group sizes.

24. Agree, this is more clear. Why did you use a Wilcoxon test and not a t-test?

25. This point remains among my main criticisms and, in my opinion, together with the lack of framing of the paper in the introduction, may constraint publication of the manuscript. In my opinion, it is not satisfying to include all potential determinants in one model without any a priori assumptions on potential mechanisms through which the included factors might lead to disability. Socioeconomic factors, lifestyle factors and diseases are in a “chain of causality” leading to disability. Low SES, for instance, which is a distal determinant, is not likely to be directly associated with disability, but may “lead” to disability via clustering of suboptimal lifestyle. Lifestyle “causes” disability because persons with, for instance a high BMI, are at higher risk of developing diseases that are associated with disability. The way the analysis is performed in the paper (table 2) does not account for these differences in any way. As a result, the effect of the more distal factors may be severely underestimated. To illustrate, table 2 suggests that education is unrelated with the presence of disability. However, this is probably only because there is adjusted for diseases and lifestyle, which are in between education and disability. At least, results of univariate models including 1) diseases, 2) lifestyles, 3) SES etc. should be included.

26. Figure one is not self-explaining. Among others, the unit of measurement is lacking on the y-axis. To me, it would be clearer to simply show a mean score, together with a confidence band. Or, otherwise, please explain the relevance of showing the percentiles.

27. agree

28. agree

29. agree

30. This text part does mention potential reverse causation, but does not cover the implications for the results. An example of what I mean is that overweight may cause disability, but that disability may also cause obesity. The implication is that the association found overestimates the casual effect of overweight on disability.
31. agree

Further comments:

Table 1: the column “total” does not only show totals, but also mean values (e.g. for BMI). From the table, it is not clear what exactly the p-value refers to.

Table 2: In my opinion, one multivariate model including all variables at once does not give sufficient insight in associations and may be misleading as part of the effect of some variables is (falsely) captured by other variables.

Table 3: Does this table derived from the attribution results and do the results account for co-morbidity? If so, please mention this. If not, why not? Co-morbidity may be a confounder which needs adjustment.

Table 4: “Disease-specific attributable prevalence”? Better would be: “Contribution of diseases to prevalence of disability”. The part attributed to background is very large. In persons aged 80+, only 40 percent is caused by the diseases included. I think this is rather unexpected and needs explanation. Is this related with the disability measure used, or with the validity of diseases?

Furthermore, I wondered why disabling impacts of diseases available when running the attribution method are not shown.

P10. Attribution method: “This concept is similar to the epidemiological concept of the PAR.” Although both methods are developed to obtain an impression of the contribution of diseases to the burden of disability, they are substantial conceptual differences.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
'I declare that I have no competing interests'