Reviewer's report

Title: Prospective association between self-reported life satisfaction and mortality: Results from the MONICA/KORA Augsburg survey 3 Cohort Study.

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Reviewer: Gunnar Einvik

Reviewer's report:

In a large, well-described population-based cohort, the authors assess parameters determining life satisfaction (LS), and assess the prospective influence of high LS on long-term mortality.

At baseline, psychological factors were the most important determinants of LS. Furthermore, in men, but not in women, the hazard ratios for mortality were significantly lowered in persons with high LS, adjusted for somatic and socio-demographic variables, but the associations were weakened and not significant when adjusted for psychological factors.

The authors investigate well-defined research questions, mainly with appropriate methods. The statistical analyses are sound with strict levels of significance. All data collection and statistical analyses are well described and sound. Relevant references are given, and the authors have discussed limitations properly.

Still, there are some concerns commented on below: The authors need to clarify the place for their method of measuring LS relative to other self-reported health measures. The present abstract does not accurately convey the main findings. The language needs to be improved.

Major comments:

1. Background, paragraph 1: In the introduction, LS is defined as “trait levels of positive affect as well as cognitive assessments of the extent to which a person’s life matches his or her expectations”. Furthermore, the authors will “focus on life satisfaction because it reflects subjective perception ... and may be more stable than measures of positive affect”. This is an interesting approach; this study seems to contribute with important knowledge on how each individual’s subjective perception of their health, regardless of somatic and psychological health, affect mortality. However, in the rest of the manuscript this aspect is not emphasized. The results are rather discussed in relation to previous studies of positive well-being, which, according to the introduction, is less stable measure.

2. I have some concerns regarding the relation between life satisfaction and other psychological measures. The concept of a single-item self-reported variable having significant influence on future life-length expectancy is tempting. Still, as expected, the analyses show that life satisfaction is closely associated with other psychological variables. Some of these variables are for this reader very similar to LS (e.g. self-rated health, health status), and the authors need to
clarify why these variables need to be evaluated in the same Cox regression analysis. For me, it would be more clinical useful to evaluate the prospective influence by high LS adjusted by somatic, demographic and social factors, and psychological factors not very similar to LS. Indeed, the authors have done this (discussion, paragraph 3), when reporting high LS is independently associated with lower mortality if self-rated health is not added. In conclusion, the possible clinical value of asking for subjective perceptions is preferable evaluated without adjusting for other measures of subjective perception. Alternatively, these two measures may be compared.

3. Methods, paragraph 3: Two of the references regarding previous use of this item in Norwegian studies are somewhat misleading. Reference 10 does not present validated data, while reference no 12 regards a selected sample of medical doctors, and is less relevant for a population-based study. Furthermore, in the referred HUNT-study this question was one of three items in a subjective well-being scale, in which the English translation is “When you think about the way your life is going at present, would you say that you are by and large satisfied with life or are you mostly dissatisfied?” Thus, the authors should clarify that this item, at least in the cited study, is one of several items in a well-being scale, and also consider referring to one of the following references:

- Moum, T., Næss, W., Sørensen, T., Tambs, K., & Holmen, J. Hypertension labeling, life events, and psychological well-being. Psychological Medicine, 1990; 20: 635–646.

Minor comments:

Generally:

4. The authors should consider presenting the two aims in the same order throughout all parts of the manuscript - i.e. 1) determinants of LS 2) prognostic influence by LS on mortality.

Abstract:

5. The authors analyze the data with respect to how LS in the highest tertile compared to the lower tertiles influence on mortality, and which factors determinate high LS. This should be reflected in the aims (i.e. which variables determine high LS et.c.)

6. Absolute risks are asked for in aims, but not mentioned in the results section.

7. “Confirmed” is perhaps not the correct statement when it was not hypothesized in the aim. Furthermore, the second sentence in the conclusion does not state in which direction high LS influence on long-term survival.

Background:
8. The abbreviation for LS should be given when first mentioned.

9. In line with comment 1; After contrasting the term LS from the terms well-being and positive affect in paragraph 1, paragraph 2 mainly comment on well-being or positive affect, and not on LS, which is somewhat confusing.

10. The first sentence in paragraph 3 imply that there is a a-priori hypothesis of the effect by LS on mortality, which is lacking in the last sentence in the same paragraph (page 4). Clear hypotheses or statements in the introduction would improve the readability.

Methods:

11. The authors have used C-statistics to explain the determinants of LS. They may also consider using C-statistics comparing regression models including somatic and demographic variables with and without various psychological variables when evaluating the mortality risk. In my opinion such comparisons of the additional mortality risk contributed by LS (or similar measures as mentioned in comment 2) would improve the clinical significance of including self-reported measures in prognostic evaluations.


Results:

13. Paragraph 6 (and table 2): The text literary says that the gender-specific analyses was adjusted for gender.

14. Paragraph 7: I am not convinced that all CVD risk factors and health factors are confounders, as several of these are not associated with LS in crude analyses (suppl. table 1). At least, the authors should consider provide hazard ratios for all variables in the regression analyses, in order to ascertain which factors are possible mediators (e.g. as a supplementary table).

15. Paragraph 8: Supplementary table 1 mentions that LS is associated with angina pectoris, which most often reflect coronary heart disease.

Conclusions

16. The conclusion should reflect the aims, and state the direction and magnitude of effect by high LS on mortality.

Discretionary revisions

17. Methods, paragraph 2: Is the question asked by a researcher in an interview or answered by the participants in a questionnaire? The abstract states that there was an interview, it is mentioned as a questionnaire.

18. Methods, paragraph 8: The number of end-points registered is also stated in the results section.

19. Results, subtitle and first sentence paragraph 2: Consider revise the language.

20. Table 2: Please consider to revise the caption to reflect that LS is
dichotomized in high vs. medium/low.

Minor issues not for publication

21. There are several misprints and grammatical errors throughout the manuscript (e.g. “interviews were conducted on..” (abstract), “..but generally, they indentify.. (background) “the association of all previously mentioned factors on…” (methods), “…more capable of coping with psychic distress” (discussion)

22. Abstract: The first sentence is aims rather than background, and should be a full sentence.

23. Abstract: Considering the third sentence in the result section, the second sentence is redundant.

24. Methods, paragraph 5: The order could preferably be changed so that blood sampling is mentioned first. Additionally, spell out CVD when first mentioned.

25. Methods, paragraph 7: I believe the common term is food frequency questionnaire rather than food frequency test.

26. Results, paragraph 7: The first part of the second sentence repeat the first sentence in the same paragraph, and may be omitted.

27. Discussion, paragraph 7: The term DEEX-scale is not mentioned in the methods.

28. References: Please check the references 5, 23, 30.

Level of interest: An article of importance in its field

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

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.