Reviewer's report

Title: Prospective association between self-reported life satisfaction and mortality: Results from the MONICA/KORA Augsburg S3 Survey Cohort Study.

Version: 2 Date: 18 June 2011

Reviewer: Torbjørn Åge Moum

Reviewer's report:

Major compulsory revisions:
None

Minor essential revisions:
- Is the question posed by the authors well defined?
  - yes

2. Are the methods appropriate and well described?
  - yes

3. Are the data sound?
  - yes

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
  - yes

5. Are the discussion and conclusions well balanced and adequately supported by the data?
  - Yes, but see below

6. Are limitations of the work clearly stated?
  - yes

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
  - yes

8. Do the title and abstract accurately convey what has been found?
  - Not entirely, see below

9. Is the writing acceptable?
  - Yes, but see below

Regarding response under #1:
Ok, this is fine.

Regarding response under #2:
The authors suggest changing to “A different effect of psychological factors on the relationship between LS and mortality was observed, whereby the strength, significance and in the case of women, the direction of the effect of LS on mortality was changed when psychological factors were accounted for in the model. This effect modification suggests a confounding role of psychological variables in the effect of LS on mortality.”, I would suggest changing to “Controlling for psychological factors affected the relationship between LS and mortality as the strength, significance and, in the case of women, the direction of the effect of LS on mortality was changed when psychological factors were included in the model. This pattern suggests a confounding role of psychological variables in the association between LS and mortality.” [since effect modification is something different].

In the next paragraph the following sentence needs language modification as indicated “Indeed, only when either self-rated health or social network index were excluded from the psychological model, was the LS association with mortality restored (data not shown). The exclusion of none any of the other variables from the "psychological model" (including depressed mood) did not modified the association between LS and mortality."[assuming this is indeed what the authors wanted to express].

Regarding responses under #3 through 7:
This is fine, thank you.

Remaining issue:
Although the authors now acknowledge that the association between LS and mortality may be due to common underlying causes such as social network integration and self-rated health, the abstract and the concluding sentences of the “conclusion” paragraph apparently continue to imply a direct causal impact of LS on mortality. Adding moderating clauses to the effect that “the observed association between LS and mortality wholly or in part may be attributed to common underlying causes such as social network integration and/or self-rated health” would strengthen this paper.

Discretionary revisions:
A few specific suggestions regarding language:
- first line p. 4: “age spane” rather than “life span”.
- The sentence starting on p. 8 and ending on p. is incomplete (words lacking)
- Second and third line on p. 14 should be changed to “…. Suggests that neither CVD risk factors nor DVD risk factors are confounders.”
- Line #9 on p. 16: “one’s”

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.