Author’s response to reviews

Title: Effects of self-rated health on sick leave, disability pension, hospital admissions and mortality A population-based longitudinal study of nearly 15,000 observations among Swedish women and men followed 1973-2003

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Author’s response to reviews: see over
Dear Sir,

Thank you for yours and the reviewers’ comments on the manuscript. These have now been carefully considered and the following actions have been taken.

**Editor’s comments**
1. A Competing interest section has been inserted in the manuscript.
2. There is no Figure 6 but a Figure 5a and 5b. The Figure 5 legend covers these two parts.
3. The Figures have been cropped as requested.

**Reviewer #1**

**Major Compulsory Revisions**
1. **Comment:** Some numerical results could be presented in the abstract.
   **Response:** Done.

2. **Comment:** Introduction. The "gap" in knowledge is missing, that is, the reasoning for why this study needed to be done? Information on SRH and various outcomes are presented, but what more is needed to be known? Please clarify why this study is important and be more concise. Structure- What is known? What is missing ("However,..."), but need to be known? and What we did and how? -could be useful.
   **Response:** The text has been changed in accordance with the comment (page 3-4).

3. **Comment:** Sick-leaves and disability pensions are not mentioned in the introduction. These must be presented here and some background should be given for why these outcomes need to be studied. There are several studies on disability pension that could be referred here, e.g.: Pietiläinen et al. Plos ONE 2011;6(9), Krokstad et al. IJE 2002, Månsson et al. Eur J Epi 2001;7(1) Månsson et al. Scand J Public Health 2001;29(2) Karpansalo et al. Scand J Public Health 2001.
   **Response:** The text has been changed in accordance with the comment (page 3-4).

4. **Comment:** Page 3, 2nd paragraph: Has SRH "become a recommended part of standard health surveys" or is it "becoming a recommended part of standard health surveys"? Expression: "come to be recommended as a..." is vague.
   **Response:** The text has been re-worded (page 3, paragraph 4).

5. **Comment:** Page 3, para 3: "...poorer SRH in later cohorts..." This is also vague, later than what? Do you mean that in recent studies decreasing/increasing trends of SRH have been reported?
   **Response:** The text has been changed.

6. **Comment:** Methods. Page 4, study population: The used study population needs to be characterized in more detail. Reference to a doctoral thesis is not sufficient if there is no web page where the description can be found in English. If this work is available online in English, please add an http-address to the reference list.
Response: All theses at Uppsala University are available on the university website. However a more detailed description of the study population has been inserted in the Methods section and new references have been inserted (page 5, paragraph 4).

7. Comment: Page 4, data collection: The sentence: "The data used in this report was obtained... by questionnaire..." is misleading as most data were not from questionnaires. Were the individual level data from baseline questionnaires? Please clarify.
Response: Most data were obtained by questionnaire at baseline or follow-up investigations, only outcome data were not. The Data collection section text has been modified (page 5, paragraph 4).

8. Comment: Page 7, statistical considerations: The authors mention that not all variables were measured in all sub-populations, but this should be mentioned and more precisely described earlier in the methods sections were the variables are presented. The reference to missing data could be mentioned in the results.
Response: The text has been clarified (page 8, paragraph 2).

9. Comment: Page 7, last para: The last two paragraphs on page 7 seem to talk about the same analysis? If not, please clarify what the hazard functions are, and for what they are used for.
Response: No, there were two types of analyses. The text has been clarified (page 8, paragraph 4).

10. Comment: Results. Figure 2: Men and women have the same colours in the Figure, so it is hard to interpret. The Figure legend also says that these are admission rates, but the y-axis suggests that these are percentages. Please clarify.
Response: The text has been clarified and the colours have been changed.

11. Comment: Figure 2 and 4: Please explain why women (also in Figure 2?) have so much shorter follow-up.
Response: All cohorts were followed up until 2003. Since the female cohorts on average were investigated later than the male cohorts they have a shorter follow up.

12. Comment: Discussion. The discussion needs major revision, it is very short and mainly repeats what was done and some phrases from the introduction (some of which can be presented here, if omitted from the introduction).
Response: The text has been changed in accordance with the comment (pages 11-14).

13. Comment: There is no comparison of these results to those from prior studies (size of the effects, each outcome separately handled). I'm also missing discussion about what is the meaning of these results. There is no discussion about the limitations of this study (what were the technical reasons for shorter duration of effects in women, is the data valid -possibility of errors?), or how generalisable these results are to other populations.
Response: The text has been changed in accordance with the comment (pages 1-14).

14. Comment: Page 11, 1st para: If the follow-up time for women (for technical reasons?) was much shorter than for men, it is not relevant to compare the duration of the effects among men and women.
Response: As long as there is proportionality in the hazards functions the effect duration may be estimated even when the follow-up time differs between the sexes.
Minor Essential Revisions

15. Comment: Introduction. Page 3-4: References to differences by SES or age groups are unnecessary since these have not been studied in this work. These could be mentioned in the methods as justifications for adjustments for these covariates.  
Response: The text has been changed in accordance with the comment (page 4).

16. Comment: Page 4, study population: Cohort characteristics can be presented in the results section.  
Response: Yes, it may, but we usually present it in the Methods section because we regard it as a Methods item.

17. Comment: Page 7: Please provide the SAS procedures for each analysis, and give the formats in which the results for each analysis are presented (e.g. hazard ratios with 95% CIs).  
Response: The requested information has been provided (pages 8-9).

18. Comment: Results. Page 8, 1st para: It is unnecessary to repeat the results that are presented in Table 2, same for the last paragraph on this page and Table 3.  
Response: The text has been modified (page 10).

19. Comment: Page 9, 1st para: Please provide the 95% confidence intervals for the numbers of sick leave days. P-values are not needed when 95% CIs are provided (hospital admissions, disability pension, mortality).  
Response: The text has been shortened.

20. Comment: Figure 5: Figure 5 has parts a and b, but it is not clear from the figures, which part refers to men and which to women.  
Response: The figure labelling has been clarified.

21. Comment: Discussion. Page 10, 2nd para: Submitted work cannot be referred to as: "was recently reported".  
Response: The article referred to is now in press.

Reviewer #2

Major Compulsory Revisions

1. Comment: Page 4, it is stated that “3,590 subjects were part of more than one subpopulation”. This means that one subject may be included several times in the analyses. How many subjects actually are included more than once? Could this be a problem? Could this affect the results?  
Response: The study population text has been clarified on this issue (page 5, paragraph 3 and page 13, paragraph 2).

2. Comment: Page 4, the use of the term/concept “occupational status” needs more explanation, justification, or at least a few valid references for the readers’ guidance as this is not a standard way of classifying occupational position, even less so of occupational status, from a theoretical (sociological) point of view, which has implications for the way of operationalizing occupational position (social class based on occupation). For more information, see e.g. Bihagen, E and Nermo, M. Social stratifiering och social klass. In Rostila, M and Toivanen, S (Eds.) Den orättvisa hälsan – om socioekonomiska skillnader

Response: We agree that researchers in sociology define occupational status in a different way than researchers in medicine do. We choose the latter definition, that is whether the subjects has a job or not, in analogy with marital status (in medicine usually defined as never married, married/cohabiting, divorced or widowed).

3. Comment: Page 4, regarding the operationalization of occupational status, was the variable constructed from several items, or only from one question? How was the questions stated?
Response: The question had six response possibilities, gainfully employed, unemployed, student, on sick leave or disability pension, old age retirement, and other (specify). We choose to have four groups, by amalgamating gainfully employed and students and reclassify other to one of the four groups.

4. Comment: Page 5, regarding SRH. It is stated that SRH is measured with a well-established instrument using a seven-point Likert scale. In the analyses a three category SRH is used. How are these categories constructed and why? There is no information what so ever about the operationalizing of SRH which is quite alarming considering that the main focus of the manuscript is in fact SRH. This is not so convincing. Please clarify.
Response: All analyses were performed on the seven-point scale. The three-point scale was used only for illustration purposes (a high level group, an intermediate group and a low level group).

5. Comment: Page 6. The section “Ethical consideration” would be better placed on page 4 after the section of “Study population”. It is not in a relevant place now, between the “Data collection” and “Statistical considerations”.
Response: This is the position the section on ethical considerations usually has in medical writing. However, it has been moved to the position suggested (page 6, paragraph 1).

6. Comment: Page 7. The authors state that “Missing data were not replaced”. Were subjects with missing data removed from the analyses? It is unclear on how many observations the linear and cox regressions were based on. This could preferably be stated in the Table headings or in the Tables somewhere. The reader needs this information in order to see whether the number of observations (=n) varies in the models and therefore make the models incomparable.
Response: The SAS software automatically removes subjects with missing data from the analyses. The requested numbers have been given in Tables 3 and 4.

7a. Comment: Page 7 and Table 3, regarding linear regression analyses. The text that introduces the linear regression of number of days of sick leave on SRH, adjusted for covariates, do not refer to Table 3, and thus the reader do not know where to look for the results presented in a table. Please include Table 3 in parenthesis in an appropriate place in the text on page 7 as well.
Response: The reference to the Table is given in the Results section (page 10, paragraph 2).

7b. Comment: It is unclear in the text and in Table 3 which reference categories are used for the categorical independent variables and how the results should be interpreted. This needs to be explained for the reader. (The reader understands that the independent variables are categorical from the way the variables are presented earlier in the methods section).
Response: In the SAS version of proportional hazards regression, categorical variables are treated in the same way as continuous variables and therefore there is no need to use reference
levels in categorical variables. The hazards ratio describes the odds of reaching outcome from one step in the variable to the next. The alternative, dividing variables into strata means loss of statistical power and was therefore avoided.

7c. **Comment:** The outcome variable is “number of days on sick leave” and one the independent variables, ”occupational status”, includes a category labeled “on sick leave or disability pension”. Does this mean that sick leave is captured both in the outcome (number of days on sick leave) as well as in the exposure variable (being on sick leave)? What kind of problems/bias could this maneuver introduce?

**Response:** Since this is a longitudinal study covering a long time period those on sick leave or on disability pension have in the vast majority of cases not been so during the complete study period, and therefore the sick leave variable is not necessarily “captured”. In Table 3 being on sick leave has a rather strong relation to being granted a disability pension, for obvious reasons. The sick leave variable was introduced the “balance out” sick leave, since a number of people had been on sick leave for a long time without being granted a disability pension. Being on sick leave was therefore regarded as a potential confounder, or possible a variance reducer, and for this reason introduced as a covariate.

7d. **Comment:** A modeling strategy for the linear regression is missing. Such a strategy would explain if there is a theoretical base for the order in which the covariates are entered in the analysis, or why are all the covariates thrown in simultaneously? Is the aim to look at the association between SRH and number of days on sick leave? Or is the aim to establish the “determinants of sick leave in multivariate analysis” as stated in the heading of Table 3? Then any of the covariates is as important as SRH? Well, this is not what the aim of the study stated. This should be clarified and adjusted accordingly.

**Response:** The independent variables were entered simultaneously. The main interest of course was to assess the importance of SRH for the number of sick leave days. The other variables are determinants, but since the analysis is multivariate they may all be regarded as determinants. regarding choice of variables see comment 8a.

7e. **Comment:** What does “backward elimination of non-significant variables” mean? Which variables were eliminated? Did the statistical program decide which variables to enter or is there a theoretically based modeling strategy somewhere?

**Response:** Backward elimination means that all variables were initially entered. The program then eliminated the variable with the weakest marginal impact on outcome. Then the program made a new run and eliminated the variable that now had the weakest impact, and so on, until all non-significant variables were eliminated. Those eliminated are shown in Table 3 as non-significant (no beta, t- or p-value.

7f. **Comment:** Is number of days on sick leave relevant, or is the sick leave spell more relevant, i.e. whether short or long term sick leave? Previous studies have tended to focus on long term sick leave as this form of sick leave is most harmful for the individual’s working life and health as well as for the workplace and the society as a whole. Could the analyses be run for short versus long term sick leave separately? SHR might have a different predictive value for long term sick leave?

**Response:** Separate analyses for short-term and long-term sick leave may be done but imposes several problems. First, there is no natural cut-off point separating the two. The cut-off proposed by the Swedish National Social Insurance Board, 90 days, is based on administrative grounds. Second, any dichotomisation of a continuous variable means loss of power, since there is a considerable residual variation in the two resulting responses. Third,
we have been active in sick leave and disability research for more than 20 years, and we have repeatedly found that the variable 'number of days of sick leave' has a higher precision than the variable 'number of sick spells', whatever the outcome. We therefore chose 'number of days of sick leave' as the sick leave variable.

7g. **Comment:** As the association between SRH and number of days on sick leave was investigated in terms of linear regression then the time dimension of the data was not taken into account. Could this be done using a different statistical method of analysis? As it is now, the analyses are interpreted as cross-sectional associations? Are there possibilities and potential for more refined and sophisticated data analyses which are not taken into account in the present manuscript?

**Response:** Linear regression does not necessarily mean that the regression line is straight. We tested also curvilinear relationships by introducing squared and cubic terms in addition to potential interaction terms, but found that the straight regression line had the best fit. A note on this issue has been introduced in the Statistical considerations section (page 9, paragraph 1).

8a. **Comment:** Page 7, paragraph 4, and Table 4, regarding Cox regression analyses. A modeling strategy for the Cox regressions are missing, see point 7d above. Here it is unclear on what bases the covariates were chosen for the three outcomes: disability pension, hospital discharge, and mortality.

**Response:** The same type of analysis strategy was chosen for the linear and the proportional hazards regressions with entrance of all potential determinants, confounders, and variance reducers with backward elimination of non-significant independent variables. The choice of these potential variables was based on what variables were available and which could be potentially related to outcome. A note on this issue has been inserted in the Statistical considerations section (page 9, paragraph 3).

8b. **Comment:** “with the outcome entered as the dependent variable”. This sentence should be omitted or rewritten as the dependent variable is the outcome (and the independent variable is the same as covariate or exposure).

**Response:** The wording we used is the standard in medical writing. We therefore prefer to keep it.

8c. **Comment:** “and being on sick leave or disability pension (regarding the outcomes hospital admission and mortality)”. Does this sentence refer to the covariate “occupational status” described earlier in the methods section? Why is it now labeled using one of its’ categories? See also point 7c above regarding inclusion of this variable as a covariate.

**Response:** Occupational status was meant as a description of whether the subjects were in gainful work, or if not, why. The variable occupational status was not used in the analyses, but rather its categories.

8d. **Comment:** Table 4, regarding the heading. See point 7d. What is the aim of the analysis? Establish an association between SRH and the three outcomes, adjusted for covariates, or to investigate a set of determinants for the outcomes, including SRH.

**Response:** Actually both. The prime purpose was to establish an association between SRH and the three outcomes, but because of the methodology used we also arrived at (some of the) determinants.

8e. **Comment:** Table 4, the heading needs information on number of observations included in
the models, years of baseline and follow-up.

Response: Done.

8f. Comment: Table 4, in addition to confidence intervals, is it necessary to report the p-value and chi², as these are not commented in the results? P-value is explained in the text already.

Response: In the old days the test parameter was usually obtained from mean and standard deviation. Since the 1990s Wald’s chi-square is used as the test parameter. The chi-square measure reported in the Table is Wald’s chi-square. The reason why it is included in the Table is that it is one of the best measures of relative variable impact on outcome. We did not include it in the text since HR, 95%CI and p-values are sufficient for the illustration of association.

9a. Comment: Page 8, Results. Paragraph 3. Regarding amount of days of sick leave per year among women and men, this number is less interesting when we do not have information whether this was a long term sick leave spell or several short spells during a year. This makes big difference. Again, could the analyses be conducted separately for long resp short spells of sick leave?

Response: See comment 7f.

9b. Comment: Paragraph 4. Regarding the results of SRH and sick leave, authors should explain how the results from linear regression should be expressed, what does it mean? One unit decrease of SRH increases the probability of sick leave by how much? It is important to explain for the reader how the regression analyses are interpreted, this increases the pedagogical value of the manuscript. As it is now, there is very limited information and the reader is asked to look at Figure 1. And all of a sudden the SRH is a categorical variable instead of a continuous as was described earlier in the methods section. No information is given on why and how SR was categorized. Lots of information is lost by categorizing this 7-point Likert scale variable. It would be much more interesting to plot the continuous SRH in Figure 1.

Response: The requested information has been added (pages 10-11).

10a. Comment: Page 9, Results. Paragraph 1. Start a new paragraph after line 4, sentence that ends with “…, and 121 days.” when the analyses go from SRH and sick leave to deal with SRH and hospital admission. This is another outcome and another analyses and should not be mixed as it is now, it is confusing for the reader.

Response: The first paragraph of the Results has been changed (see Reviewer 1 comment 18)

10b. Comment: Paragraph 2. Authors state “Self-rated health was also significantly associated with being granted a disability pension after adjustment for the influence of the covariates.” Does this mean that a weak SRF increased the risk of disability pension or what? Or that a strong SRH decreased the risk of disability pension, and how much? Please explain properly so the reader can understand and follow.

Response: Both interpretations are correct. The text has been clarified (page 10, paragraph 3).

10c. Comment: Paragraph 3. Same questions as in point above 10b.

Response: The text has been clarified.

11. Comment: Page 16. Authors state for Figure 2 “Cumulative first hospital admission rate among women and men in groups according to self-rated health”. Here it is important to
indicate that is the level of SRH that defines the groups. This applies for all the headings. Ant the grouping of SRH needs to be explained in the methods section as well. Why and how? Why three levels? Why not four? Why not use the SRH in continuous form?

Response: We could have used the seven-point SHR grading but that might have been confusing for the reader (14 lines to keep track of). We therefore decided to merge the SRH groups into three. The requested text has been added (page 6, paragraph 4).

12. Comment: Page 19, Table 3. See point 7b above. Table 3 is not easy to interpret, the reader needs more information on reference categories, how to interpret the estimates and the results from the linear regression analyses.

Response: See comments 7b and 9b.

13. Comment: Page 20, Table 4. See point 8d above. How many observations are included in the models? Do we need all these statistics for significance in the table?

Response: Regarding numbers see comments Discrete Revision comment 1, Major Compulsory Revision comments 6, and 8e. Not all, but some of these statistics are necessary to interpret the results, such as effect size (HR), its significance (confidence interval), significance again (p-value, not necessary, in medical epidemiology the use of p-values are discarded), and Wald’s chi-square (variable impact on outcome). The latter cannot be determined from HR, since HR depends heavily on how the variable is graded.

14. Comment: Pages 25 and 26, regarding Figures 5 and 6. The rule of thumb is that figures should display very important findings. If not, they can be added in an appendix. There are 4 tables and 6 figures in this relatively short manuscript which is somewhat unbalanced. The authors are recommended to “kill their darlings” and choose those tables and figures that are essential for making a clear story of their results. Everything else can be omitted or maybe added in an appendix.

Response: The reviewer’s point is well taken for printed journals. For web based journals it does not fully apply. First of all, in web journals there is no lack of space. Second, for the general reader it is much more troublesome to go to several websites (the site where the article is stored and the website where an appendix is stored). Going to more than one web site is discouraging. Third, this matter is not with the reviewers but with the Editor, so we leave it to the Editorial Office.

Minor Essential Revisions


Response: The spelling has been corrected (now reference no. 19).

2. Comment: Page 17, Table 1. The table is squeezed and difficult to read, could it be placed horizontally instead?

Response: Yes, it might. However, we have published a lot in this web-based journal and the guys who do the editing always ask us to raise horizontally oriented tables (landscape) to an upright orientation (portrait). So we prefer to leave it as it is.

Discretionary Revisions

1. Comment: One easy way of improving the readability of a paper is to keep the same order of the main terms (e.g. women and men, or sickness absence, disability pensions, hospital
discharge and mortality) through the whole manuscript text as well as in the figures and tables. In the preset manuscript there is some jumping back and forth in the use of the main terms/concepts. For instance, look at the order of terms in the title of the manuscript and then in Table 4.

Response: The text has been changed in accordance with the comment.

2. Comment: Relevant references of self-rated health are missing in terms of the work by Manderbacka and Lundberg:

Response: The text has been changed in accordance with the comment (page 3, paragraph 4).

On behalf of the author group

Christina Halford