Author’s response to reviews

Title: Associations between work-related stress in late midlife, socioeconomic position, and serious health problems in old age: A longitudinal study with over 20 years of follow-up

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Author’s response to reviews: see over
Author's covering letter for initial submission

Title: Associations between work-related stress in late midlife, socioeconomic position, and serious health problems in old age: A longitudinal study with over 20 years of follow-up

Authors:

Version: 1 Date: 10 June 2014

Comments: see over
Dear Editor:

Thank you for giving us the opportunity to revise the manuscript 1677057343115315 entitled “Associations between work-related stress in late midlife, socioeconomic position, and serious health problems in old age: A longitudinal study with over 20 years of follow-up” submitted for publication in the BMC Public Health. We would also like to thank you and the reviewers for the careful study of the article and thoughtful comments.

The enclosed manuscript aims to explore work-related stress self-reported between 46-67 years of age in relation to health between 69-91 years of age in a nationally representative sample of Swedish adults. The results showed that particularly job strain (measured as low job control and high job strain) was associated with serious health problems in old age. Most of the associations between work-related stress and health in old age diminished after controlling for education (our measure of socioeconomic position). However, the dimension measuring job demands indicated (even after adjustments of socioeconomic position) that high job demands were associated with higher risk of serious health problems among women and lower risk of serious health problems among men.

In this study, we had the rare opportunity to explore the potential effects of work-related stress in midlife on health in older age using data with over twenty years of follow-up. Late life health reflects the cumulative effects of different environmental influences experienced over the life course where psychosocial work environment plays an important role due to the considerable amount of time we spend at work. However, few studies with sufficient information about occupation and health exist where such a long follow-up is possible.

There are no commercial sponsors, and no conflicts of interest.

The manuscript has been submitted in accordance with the instructions. I hope this has been done in a satisfactory manner, otherwise please let me know so we can make the necessary corrections.

We revised the manuscript according the comments and suggestions. Below please find a detailed description of the changes we have made in response to the comments and suggestions.

Reviewer: 1

Comments to the Author

1. First of all since the workers included in the survey at baseline were already between 46 and 67 years old, I am not sure whether health selection (reverse causation from prior mental/physical health problems to jobs with lower work
stress and/or socioeconomic position) can be disregarded. Since evaluations of job demands, control, job strain are of course perceptual and highly influenced by prior health problems, mental impairments, prior stressful experiences, etc. The authors should at least discuss the possibility of reverse causation or a common cause behind the exposure and the outcome. The controls for more serious health problems at baseline might not be sufficient to deal with this issue, since there may be a time lag before severe health problems emerge. If the data permit additional controls the authors should do so. If not they have to address this issue with more attention in the discussion.

We agree that selections of occupations and perceived work stress may be affected by the individual's physical and mental state. However, we hope that our control for the related variables at baseline rectifies this issue at least to some extent. We already controlled for all available physical health covariates in the original submission. In response to the comment, we included additional controls for mental health (anxiety, depression, and general fatigue) at baseline in the analysis (see Tables 2-4). These additions did not change the results. Unfortunately, we cannot know how the participant's health and/or life situations were before participating in the baseline survey and we can therefore not rule out possible reverse causation (selection effects) above and beyond controlling for variables at baseline. However, if there would be an undetected reverse causation, the results in our study would most probably be an underestimation of the correlation of work-related stress and health in old age. We discuss the issue of reverse causation in the Discussion section on p 19.

"Fourth, we cannot control for previous health and/or life situations before participating in the baseline survey. We did, however, control for health status (including mental state) at baseline to minimize the effect of a direct association between the psychosocial work environment and health. However, if there were an undetected reverse causation in regard of health status and work condition, the results in this study would most probably be an underestimation of the association between work-related stress and health problems in old age."

It may also be true that it may take years before problems associated with work emerge, and this is the essence of our study. We tried to make this clearer in the Introduction p 5.

"Most previous research on associations between work-related stress and health is cross-sectional, however, it may take years before physical consequences of work stress emerge, making it important that a long follow-up period be allowed to track the link between work stress and health."

2. Second, the authors have chosen to use the Karasek model. Of course this is probably the occupational stress model with the most solid basis in empirical evidence. Although I see two problems: (1) the items of both autonomy and skill discretion (a part of control) and job demands are quite thin and distant from the recommended JCQ items. See further for methodological remarks on this fact.; (2.)The Karasek model, although very valuable, also provides a reductionist picture of the (psychosocial) work environment. Were there other occupational stressors that could be included...
in the models? ERI, job insecurity, social support, physical exposures, ...? If this was the case, the authors should argue why they did not include additional occupational exposures. In general, I recommend the authors at least to discuss these two issues in the discussion section.

1) It is true that the Karasek model was expanded subsequent to our baseline data collection. Unfortunately, the expanded version is not available to us. In our study, we used the questionnaire from Karasek's original job strain model (Job Demand-Control model) which was actually constructed with data from the Level of Living Survey (LNU) 1968 and 1974. All baseline data in this study (from LNU 1968, 1974, 1981, and 1991) are based on this original version of the model. There are some additional items, but these items were never considered part of any validated version of the Karasek model. Therefore, to maintain good internal validity, these items were not added. We are aware of that the measures constructed at that time (late 70s) were perhaps not as developed as they are today. But to be able to explore how work-related stress in midlife is associated with late life health, we depend on this data. We added a sentence in the manuscript to confirm that we use a validated version of the job strain model p 7.

“…which have been described and validated by Karasek and colleagues [2],…”

2) While we agree that the measure may be simplistic, it has also been found to correlate highly with other known work stressors e.g., job insecurity, lack of rest breaks, and piece rate work (Karasek 1979). Still, we added text to the Discussion section on p 19 discussing the issue of measuring only one aspect of work, and added control variables for the physical work environment in our analysis to make our focus on the psychosocial dimension of the work environment clearer. It was not possible to measure ERI and social support with our data as these variables were not available in the dataset.

“Third, our assessment of work stress is limited. However, the fact that we found meaningful results with a crude assessment of work stress may indicate that these results are robust”

3. The authors provide a concise and to-the-point overview of the relevant literature. Their threefold objective is clearly stated. The main objective is to describe relations between work-related stress at midlife and a series of more or less severe health problems. They also want to look at relations with SEP. They should address better the conceptual relations between SEP-Risk factors-Health (see further).

We have now tried to explicitly spell out our hypotheses in the manuscript p 5.

“Education is a commonly used indicator of socioeconomic position and a major determinant of important socioeconomic conditions such as occupation and income. It aims to reflect intellectual and material resources derived from the possibilities that an education can give you in life [15]. For example, a person with high education may have her health less affected by a stressful work environment as she may possess more resources (material, cognitive, and psychosocial) that can be used to cope with the stressful conditions.”
In order to further clarify the conceptual relations we have reanalyzed the data using education as the indicator of SEP rather than the previously used index.

The rationale for including a measure of socioeconomic position in the study is twofold:

i) As SEP is associated with both work environment and health it is a potential confounder. That is, any crude relationship found between work environment and health may be entirely spurious. Thus, that is why all analyses are adjusted for SEP.

ii) SEP may moderate the association between work environment and health. For example, a person with high education may have her health less affected by a stressful work environment as she may possess more resources (material, cognitive, and psychosocial) that can be used to cope with the stressful conditions.

4; The authors dispose of a truly interesting dataset. However they should do an effort to describe the links between the different initial datasets in a more straightforward way. Especially the second paragraph of “data and study population” is very confusing: please make an effort explaining the links between the databases in a way that makes it more clear to the readers. Maybe a scheme can be helpful?

In the second paragraph of the Data and study population we have tried to clarify these linkages and hopefully made it less confusing p 6.

“The information used in this study consisted of four linked sets of data combined into one longitudinal dataset. These linked sets of data included 1) baseline data from LNU 1968 plus re-interviews of the same people in SWEOLD 1992, 2) baseline data from LNU 1981 plus re-interviews of the same people in SWEOLD 2002, 3) baseline data from LNU 1981 plus re-interviews of the same people in SWEOLD 2004, and 4) baseline data from LNU 1991 plus re-interviews of the same people in 2011. “

5. As regards the measures, in my opinion there are some problems with the Karasek-components: First of all the authors should provide correlations for the relations between the separate demand and control components: are they (especially demands) representing one empirical dimension? Next, I am not sure about the indicator for job demands: 1050 people have high demands, compared to 452 with low demands; By no means "high demands" can be perceived as an "acute group" and this might explain the counter-intuitive protective effects emerging from high job demands. Maybe the "low demand" group is the most problematic group: as suggested higher, this group might be composed of people that were downwardly mobile during their career (something which is further suggested by the even stronger protective effect in lower SEP men (table 4)). In my opinion the authors should try (and at least
demonstrate the results to the reviewers) other cut offs regarding job demands, including the effects of the single items separately.

With respect to the correlations, (Hectic jobs and psychologically taxing jobs: \( \rho = .31 \ p < .001 \)); (a) the correlations are not that high, but we should keep in mind that these two items measure different aspects of demands. They do not intend to represent one empirical dimension, but to measure two different aspects of work demands. Some jobs may "only" be taxing and others "only" hectic and still be considered as a stressful work environment. (b) Dichotomous variables have notoriously low correlation coefficients, (c) a percent agreement in answers may be a better indicator of consistency. The agreement is 67% (Yes on both questions = 33%), (d) these are based on a validated model, and (e) internal validity is aided by the fact that we found meaningful results using these measures. Among those with the lowest skill level (normally required education) 74% also had a monotonous work. However, looking at the association between skill level and monotonous work does not give an adequate picture since it is the combination of having a monotonous job and low normally required education that gives a sense of low control. Moreover, the lowest group of control includes all with monotonous jobs regardless of level of skill. With respect to the cut-off for high job demands, we agree that the cut-off might be considered quite low (you had to have answered ‘Yes’ on only one of the items) so we re-run our analysis with a higher cut-off (you have to have answered ‘Yes’ on both the items). The results from these analyses are presented in Table A below. They are not presented in the article. We also ran models including job demands across all three groups. The results from these analyses showed that lower cut-off fitted the data better – there was a bigger difference in the studied aspects of health between zero and one ‘yes’ answers compared to between one and two ‘yes’ answers. We also ran separate analysis for the two different job demand items (hectic and taxing). The results are not presented in the article, but in Table B, below. From the results we could see that there were separate effects from each of them. We have decided to keep the low cut-off.

Table A. Modified version of Table 2 Associations between work-related factors and the number of domains with serious health problems

<table>
<thead>
<tr>
<th>WORK-RELATED FACTORS</th>
<th>Model I</th>
<th>Model II</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1 domain</td>
<td>2-3 domains Complex health problems</td>
</tr>
<tr>
<td>Job Demands</td>
<td>OR (CI 95%) (^a)</td>
<td>OR (CI 95%) (^b)</td>
</tr>
<tr>
<td>High demands</td>
<td>Total</td>
<td>1.00 (0.75-1.33)</td>
</tr>
<tr>
<td>(ref. low &amp; med demands) (^b)</td>
<td>Women</td>
<td>1.20 (0.79-1.82)</td>
</tr>
<tr>
<td></td>
<td>Men</td>
<td>0.77 (0.51-1.17)</td>
</tr>
<tr>
<td></td>
<td>Difference women/men (^d)</td>
<td>ns</td>
</tr>
<tr>
<td>High demands</td>
<td>Total</td>
<td>0.76 (0.58-1.01)</td>
</tr>
<tr>
<td>(ref. low demands) (^b)</td>
<td>Women</td>
<td>1.02 (0.69-1.51)</td>
</tr>
<tr>
<td></td>
<td>Men</td>
<td>0.54 (0.36-0.81)</td>
</tr>
<tr>
<td></td>
<td>Difference women/men (^d)</td>
<td>*</td>
</tr>
</tbody>
</table>

Results of multinomial logistic regressions. All analyses were adjusted for follow-up year and baseline characteristics: age, sex, physical work environment, hours worked during previous year, mental health, mobility, and an index based on all diseases and symptoms that were used to create the outcome. Model II was adjusted for all these factors and for level of education. The analyses were done separately for women and men. Results in bold: \( p \) value <0.05. a. Those with serious problems in two or three health domains were classified as having complex health problems. b. Abbreviations: OR = odds ratio, CI = confidence interval, ns = nonsignificant, \( p \) = \( p \) value. c. For women, the reference category was women with low and medium job demands. For men, the reference category was men with low and medium job demands. d. \( p \) value for how the association between hectic/taxing and health problems differs between men and women; i.e., the interaction between sex and job demands; \( ^a p < 0.05 \), \( ^b p < 0.10 \), ns = \( p \geq 0.10 \). e. For women, the reference category was women with low job demands. For men, the reference category was men with low job demands.
Table B. Associations between the separate items of job demands and the number of domains with serious health problems

<table>
<thead>
<tr>
<th></th>
<th>Number of domains with serious health problems (ref. no serious problems)</th>
<th>Model I</th>
<th>Model II</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1 domain</td>
<td>2-3 domains</td>
<td>Complex health problemsa</td>
</tr>
<tr>
<td>WORK-RELATED FACTORS</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Job Demands</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hectic (ref. not hectic)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>OR (CI 95%)b</td>
<td>0.87 (0.66-1.13)</td>
<td>1.08 (0.72-1.63)</td>
</tr>
<tr>
<td>Women</td>
<td>1.04 (0.72-1.52)</td>
<td>1.53 (0.88-2.65)</td>
<td>1.07 (0.74-1.56)</td>
</tr>
<tr>
<td>Men</td>
<td>0.67 (0.46-1.00)</td>
<td>0.68 (0.36-1.29)</td>
<td>0.73 (0.49-1.08)</td>
</tr>
<tr>
<td>Difference women/men</td>
<td></td>
<td>*</td>
<td>†</td>
</tr>
<tr>
<td>Taxing (ref. not taxing)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>OR (CI 95%)b</td>
<td>0.91 (0.69-1.19)</td>
<td>0.92 (0.61-1.38)</td>
</tr>
<tr>
<td>Women</td>
<td>1.14 (0.78-1.66)</td>
<td>1.02 (0.60-1.72)</td>
<td>1.20 (0.81-1.78)</td>
</tr>
<tr>
<td>Men</td>
<td>0.66 (0.44-0.99)</td>
<td>0.88 (0.45-1.71)</td>
<td>0.74 (0.49-1.11)</td>
</tr>
<tr>
<td>Difference women/men</td>
<td></td>
<td>ns</td>
<td>ns</td>
</tr>
</tbody>
</table>

Results of multinomial logistic regressions. All analyses were adjusted for follow-up year and baseline characteristics: age, sex, physical work environment, hours worked during previous year, mental health, mobility, and an index based on all diseases and symptoms that were used to create the outcome. Model II was adjusted for all these factors and for education. The analyses were done separately for women and men. Results in bold: p value <0.05. a. Those with serious problems in two or three health domains were classified as having complex health problems. b. Abbreviations: OR = odds ratio, CI = confidence interval, ns = nonsignificant, p = p value. c. For women, the reference category was women with a not hectic/taxing job situation. d. P value for how the association between hectic/taxing and health problems differs between men and women; i.e., the interaction between sex and job demands: *p<0.05, †p<0.10, ns = p≥0.10.

Regarding the paragraph on job strain: include an additional sentence: "Job strain: Job strain was based on the cross-classification of job demands and job control".

This has been done. See p 8.

6. Health measures: a number of decisions are not well explained for a larger audience. Why is low BMI relevant (frailty?) - This should be explained briefly. Also the grounds for deciding cut-off points seem arbitrary without explanation. Also the information on exclusion of proxy-interviewed persons is not in place here - this is something to describe in the section on study population.

We added a sentence on why low BMI is relevant and how we have chosen our cut-offs p 9.

“Underweight is considered to be a good predictor of mortality among old people”

Low BMI is relevant because underweight is associated to several different health problems, and is independently associated to higher mortality among old people. The summed index ranged from 0 to 42 and a cut-off for serious problems for this domain (Diseases and symptoms) was determined at the highest quintile for the 1992 sample (corresponding to 9 on the summed scale). Accordingly, the persons belonging to the highest quintile had, e.g., at least three severe symptoms/diseases or two severe and three mild. The same cut-off was used for the later samples. The health measures used in this study are necessarily crude as they were directed to nationally representative samples of the population aged 77 years and older including cognitively impaired and institutionalized persons. In an effort to exclude
minor health problems, we made our definition of CHP (complex health problems) rather restrictive, setting high thresholds for serious problems within each domain: the highest quintile of symptoms/diseases, inability to perform three of four mobility tasks and having at least mild dementia or being unable to communicate with the interviewer. As a consequence a very vulnerable subset of the elderly population with a high mortality risk (Meinow et al., 2010) has been identified. Limitation: However, people with serious problems in only one domain may also have a high mortality risk and complex care needs from a variety of providers. The resulting rates most likely represent an underestimation of actual prevalence rates of serious health problems and needs for care.

We added a sentence about proxy interviews in the section on study population. Proxy-interviewed persons were included, not excluded in our study p 7.

“Data were mainly collected through face-to-face interviews, with the exception of SWEOLD 2004 which was based on telephone interviews. Proxy interviews (e.g., a caregiver or family member) were conducted if direct interviews were not possible because of the individual's health.”

7. Measures of SEP. The authors should explain why they use this composite indicator. The added value over a well chosen single indicator is not well explained. It could be suitable to include evolutions in SEP over time, but this is not mentioned... is that the reason? If so, it needs to be described better. It should also be clearly mentioned for each SEP-indicator whether it is collected at baseline or later or at both times. This is not completely clear now. Also it is surprising that entrepreneurs with up to 19 employees are classified as lower white collar: on what basis is this justified? Finally, the first line of this section contains a mistake: "an indicator" --> "one indicator"? The second line of p.11 too: "socioeconomic position on A number of health domains..."

The rationale for using the SEP-index, rather than a specific single indicator of SEP was that we wanted to capture as much socioeconomic variation as possible with a single measure. However, we do realize that in doing so we did, to some extent, create a ‘black box’ that may obscure more than it clarifies. Thus, we have now reanalyzed the data using education as the indicator of SEP instead of the SEP-index. The rationale for using education as the indicator is that it is stable over time (important as we only assess SEP at baseline), as well as highly correlated with both working conditions and health.

Results

8. Provided of the potential problems due to choices made in the operationalization of the indicators, the results are well-described. Referring to further remarks, the counter-intuitive results (high demands --> less odds of health problems in men) should be discussed in detail in the discussion section, in the case that these results remain unchanged.

As described under item number 5 above, the overall results remained unchanged, they became weaker (but still significant) after the revision. We added to the
manuscript a more detailed discussion of these results in the discussion section p 16, as well as further down in this document, under item number 11.

“In addition, as shown in Table 3, the protective effect found among men in regard of the association between job demands and serious health problems is notable in all the three health domains, although it is only significant for cognition. It may be that any effects of stress as a function of job demands in men is offset by intellectual engagement reflected in job demands. Alternatively, men may consider psychologically demanding work from a different perspective than women. This possibility may deserve further study.”

Tables
9. The tables are in general well designed and include most necessary information, some remarks however:
* Since relations with SEP are an important focus, I think it is necessary to show a table of the SEP-distribution of the occupational risk factors studied.
* In table 3, I am not sure whether P-values need to be mentioned: maybe indicating the level of significance with *, **, *** is more common - This is a minor remark of course.

In table 1, the descriptive statistics, we have added a column on Education to clarify the educational distribution of sex and the occupational risk factors that we study. We have now changed table 3 completely and the p-values are not included in the table.

Discussion
10. The authors should make explicit how they link SEP, the work-related risk factors they are studying and the health outcomes to each other. What is the status of SEP in their conceptual scheme: a mediator? A moderator? A fundamental cause? It is important to make this explicit - for example: when they interpret the effect of controlling for SEP on the relation between the occupational risk-factors and health. Is it not logical that the effects of demand and control diminish since probably both these risk factors and health are unequally distributed according to SEP? The authors could discuss their findings in the light of "differential exposure"/"differential vulnerability". These remarks can also be linked to the part on 'selectivity due to mortality' in the discussion.

As stated above, and clarified in the manuscript, we view SEP as both a potential confounder and moderator of the association between work environment and health.

11. As already indicated above, the authors should discuss the possibility of downward occupational/social mobility during the working career, because of selection effects, affecting the distribution in the sample of their baseline measures of occupational risk. In general, the finding of high job demands related with better health at later age - if confirmed - should be discussed more. Now the authors concentrate on the stronger effects found in women, but they do not refer to evidence from other studies for their finding of a protective effect of high job demands in men.
Unfortunately we cannot look at social/occupational mobility prior to our baseline data. A possible explanation why it might be considered a risk among women and not among men could be a possible double-burden among women (as referred to under item number 13). In addition, we discuss the possibility of selection “bias” which we addressed in the article (p 17) indicating that our results may be due to unmeasured heterogeneity (e.g., that women and men tend to work in different types of occupations). However, we discuss these results in more detail and added references p 16.

12. Regarding their "policy justification", the authors stress the importance of working conditions for life after retirement or for policies of keeping workers longer in the labour force. I agree with them. But how do they see their specific results in that light? The link with policy is too general now, it should be focused more on their specific findings.

In the Discussion section (p 18) we have now clarified the policy implications more in detail (with focus on our specific findings).

"From the perspective of our findings, paying special attention to the health in occupations characterized by high job strain, via policies targeting these workers with screenings and health promotion outreach may be particularly useful. Additionally, considering options to ease work stress on these workers may also be helpful."

13. When discussing the differences between men and women, the authors also need to address the issue of double work burden (occupational and household demands) which affects women the most.

We agree that this is an important drawback. In response, we added a limitation to the Discussion section that while we accounted for the overall life stress at least partially via control over depressive symptoms, we were unable to account for the work-life balance specifically p 19.

“Finally, we were unable to account specifically for the work-life balance. Although controlling for mental health is helpful in this respect, a specific consideration of the work-life balance should be the focus of future studies.”

Reviewer: 2

Comments to the Author

The study is designed as longitudinal and addresses basically an important and understudied issue – long-term health effects of work stress. The manuscript is clearly and understandably written. However, I have some major concerns, particularly as regards the measures used or rather how work stress, socioeconomic status and (complexity of) health problems were operationalised and constructed. In addition, a time span of more than 20 years between baseline and follow-up without knowing anything about (constant or changing) occupational exposures and other life events during
this long period seems unsuitable to study and find causal relationships. Unfortunately, such concerns and limitations of the study are not discussed in the Discussion section.

In response, we added this as a limitation to the Discussion section on p 19.

“A related issue is that the long gap between baseline and follow-up presents some advantages with respect to the assessment of long-term effects of work on health, it also has limitations, particularly as it offers only a snapshot of health in older age and does not allow for continuous tracking of health across the life course.”

In the Discussion section authors state that one of the strengths of their study would be the inclusion of a combination of a broad range of serious health problems, but they did not take full advantage of these variety of included health problems since they classified a number of health problems somewhat discretionary into three domains and then simply added up the number of domains affected by such health problems. To end up, finally, with two dichotomized health outcomes (1 domain, 2-3 domains with serious health problems) out of such a variety of health problems is not comprehensible for me and appears to me as being an over-simplification and a too big loss of information.

We understand your concerns and have tried to discuss this in detail under item number 6.

Major Compulsory Revisions

1. Measures used for job demands and job control were severely limited and fairly inadequate. Although monotony at work usually is associated with low job control this does not mean that the two job characteristics are identical. The same applies to ‘hectic work’ which can not be simply equated with a psychosocial demanding job. Moreover, job demands can not be reduced to just psychosocial demands. Are there no other indicators available as measures of such constructs? Since validity of measures is shortened, there is a high risk for a misclassification bias.

Unfortunately, no other measures of work-related stress were available. However, the measures that we use were the original measures that Karaseks (Karasek, 1979) job strain model was based on. We understand that this model was subsequently expanded. However, given that the same associations with health outcomes continued to be reported with the expanded measure give some validity to the original Karasek items. We added the limited measurement of job strain as a limitation to the Discussion section (p 19). Finally, a detailed answer on other indicators available as measures of work-related stress and the validity of the chosen variables has been discussed under item number 2, Reviewer 1.
2. When studying longitudinal and substantially delayed effects of work stress in midlife on health status in later life, one should consider also the duration of exposure to specific work stressors or working conditions rather than just the current job situation. In cross-sectional studies the current work environment may influence the actual health status but in longitudinal studies with a follow-up time of more than 20 years only long lasting occupational exposures are expected to have a long-term health effect. A better exposure assessment is needed in a study with such a long follow-up.

We agree that our assessment of work stress is limited. However, we believe that this limitation results in underestimation of the true effect of work-related stress on health in old age. Therefore, the fact that we found statistically significant results with a relatively crude measure may indicate that these effects may be robust. Still, we discuss this as a limitation on p 19.

“Third, our assessment of work stress is limited. However, the fact that we found meaningful results with a crude assessment of work stress may indicate that these results are robust”

3. Some measures of serious health problems’ again were strongly limited. For example, a summary score for disease symptoms was used with a score range from 0 to 42 and a cut-off at 9. However, a total score of 9 could mean mild problems regarding nine diseases/symptoms just as well as severe problems in three of the fourteen diseases/symptoms. Why didn’t the authors not just count and sum up those symptoms with ‘severe problems’. And although self-reported weight and height were used to measure BMI, a score more than 0 was just given in case of underweight and not for overweight and/or obesity. This is an incomprehensible categorization.

The cutoff was determined, comprising the highest quintile for the 1992 SWEOLD survey (see a more detailed answer under item number 6, Reviewer 1). Having nine mild diseases/symptoms is in itself not mild. The importance of deficit accumulation in clinical research is well established (Guralnik 1996; Fillenbaum et al. 2000; Rockwood and Mitnitski 2007). Previous studies (Parker et al. 2013; Kåreholt 2001), found the strongest association to mortality with this coding. Since mortality can be considered as an objective health measure we considered this coding as the most relevant.

Low BMI is relevant because underweight is associated to several different health problems, and is independently associated to higher mortality among old people. Severe obesity (BMI ≥ 35) is also associated to mortality and health problems in old age. There are very few persons with severe obesity in these cohorts. However, expanding the category to those who were overweight may not be reasonable given that overweight older adults often have better health outcomes than normal weight ones.

4. Socioeconomic position (SEP) was measured using combinations of occupational class, highest level of education achieved, cash margin and individual income. Why using individual income (not a valid measure as is known) rather than equivalent household income which is the much better
measure for income? Individual income is definitely not a valid indicator for socioeconomic status, even though adjusted for purchasing power. And why using both measures cash margin and individual income that gives the income an inadequate weight within the SEP-index?

For this reason (and other) we have now reanalyzed the data using education as indicator of SEP instead of the SEP-index.

5. In addition, authors have put much effort in constructing the SEP index out of four different variables but then differentiated only between low and high SEP (see Table 4), which is an inadequate simplification of the analysis. Using a more differentiated measure for SEP would have allowed for better testing dose-response relationships which are a good indication of causality.

While we have changed the SEP indicator from a composite measure to a measure of education, we still use it dichotomously in the analyses presented in Table 4. We realize that this analytic strategy is limited as it does not allow us to test for dose-response relationships. However, our primary aim with these analyses was to assess whether the associations between working conditions and health were moderated by education. Thus, we prioritized maximizing statistical power in order to increase the possibility to detect significant differences in the associations by SEP.

6. Authors used two health outcomes: serious problems in one health domain and serious problems in two or three health domains. They first created an own ordinal scaled outcome variable (number of domains with serious health problems) but then dichotomised this variable in two different ways indicating the two mentioned outcomes in order to perform logistic regression analyses. This statistical approach is not understandable and, moreover, also not justified or explained in the manuscript. What do these health outcomes really measure? Comorbidity? Complexity of health problems? Accumulation of serious health problems? Why didn’t they use the three health domains as separate individual outcome variables or the number of health domains as the dependent variable in a linear instead of a logistic regression analysis? Either the accumulation or the complexity of health problems or some specific health problems are of relevance and under study but the used outcomes were somewhat in between... It is also not obvious why the different diseases and symptoms, mental health limitations (cognition and communication problems) and physical health impairments (mobility problems) were classified like this into the three health domains. Musculoskeletal disorders for example may be directly associated with mobility problems and not indicating two different health dimensions or domains.

The results from the multinomial logistic regressions show that it is not reasonable to use a linear or even ordinal regression. We have now tried to explain this better in the Method section on p 10-11.
“The results from the multinomial logistic regressions show that it is not reasonable to use a linear or even ordinal logistic regression. In ordinal logistic regressions it is assumed that the independent variables have similar association to the different steps in the dependent variable. In this case ordinal logistic regression would have been reasonable if the odds for serious health problems in 2-3 domains had been proportionally larger than the odds for serious health problems in one domain – and they are not.”

We understand that our chosen outcome variables at first may seem like an oversimplification. The main focus of this article is not only to study if work-related stress in midlife is associated with individual health problems in old age, but rather whether it is associated with serious health problems in old age that may substantially limit the ability of older adults to live independently. Having problems in 1, 2, or 3 health domains can all be considered as serious health problems due to high thresholds for serious problems. In addition, having serious problems in 2-3 health domains (complex health problems) has a value of its own. The complex health problems measure was originally constructed to measure care needs from several providers of medical care (due to cognitive problems, and/or diseases/symptoms) and social services (due to mobility- and/or cognitive problems). Beyond complexity of care needs, the measure reflects a broader impact of health problems that affect an individual in everyday life than single items or disease-based measures of multimorbidity or comorbidity. These three health domains capture different dimensions of health. This complexity is not only directly linked with health (via the health care system in Sweden) but also indirectly in the extent to which older adults must cope with having particular comorbidity. (Additional information is presented under item number 6 in the answers of Reviewer 1.)

We do agree that there was loss of information when not including information describing associations between work-related stress and the three separate health domains (i.e., diseases/symptoms, mobility, cognition). Originally we felt that this was beyond the scope of this article. In response to reviewer comments, we now include this information (see Table 3) to give more detailed information about the association beyond what is in Table 2 regarding problems in 1 health domain. We have now tried to explain this more clearly on p 8-9.

The concept of complex health problems follows work by Meinow and colleagues, and was originally constructed to capture the simultaneous presence of serious medical and functional problems indicating care needs from several different providers of both medical care and social services.”

Regarding musculoskeletal disorders and a possible direct association with mobility: The correlation between mobility and musculoskeletal pain was 0.26. There is an association between pain and mobility problems and pain can cause mobility problems. Even so, the need for care and help, and the problems caused to the persons subjected, differ a lot between having pain and no mobility problems and having pain and mobility problems – the mobility problems per se cause other kind of problems and the need of different caregivers than if “just” having pain.

Minor Essential Revisions

7. Gender differences were analyzed (?) and illustrated in different ways (see Tables 2, 3 and 4). Although all analyses were stratified by sex, i.e. were done
separately for men and women, gender-specific results were illustrated differently in Tables 2 and 4 compared to Table 3. Why? This is confusing and not intuitively understandable.

We changed our tables to improve clarity of presentation.

References

Thank you again to your attention to this manuscript and thoughtful review comments. We hope the Editor and the Reviewers find our revisions responsive to the comments.

Sincerely,

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