Author's response to reviews

Title: Associations between work-related stress in late midlife, educational attainment, 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
To: Editor, *BMC Public Health*
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Dear Editor:

Thank you for giving us the opportunity to revise the manuscript 5206452641322609 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 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 have now 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

Some of my concerns have been addressed, e.g. by using education as an indicator of SES, by clarifying or rather justifying a cutoff at 9 in the diseases/symptoms measure etc. However, some limitations still are not sufficiently discussed in the paper. Just stating that “our assessment of work stress is limited”, for example, without telling in what way and with what consequences is not enough. Not assessing lifetime or at least long-term exposure to work stress may lead to a non-differential misclassification bias and, in fact, result in an underestimation of the true effect, but nevertheless needs to be discussed more extensively in the Discussion section.

We had previously written about the limitation of the long gap between baseline and follow-up on p. 20, lines: 1-4. We have now expanded the discussion of this issue on p. 20, lines: 7-16:

“Third, our assessment of work stress is limited. Newer versions of measuring this construct include additional items, which were not available in our data. Similarly, we used only one assessment of occupation. Although there was very little work mobility in this cohort and we assessed individuals at the prime of their working careers, there is still a possibility that the data were subject to non-differential misclassification bias. As our measure is crude, a variance in work stress is unaccounted for. If these unaccounted differences in work stress are also associated with health, our models are likely to underestimate the associations. However, the fact that we found meaningful results with this relatively crude assessment of work stress and occupation may indicate that these results are robust.”
And the rationale for the chosen statistical approach (dichotomization of ordinal-scaled variables and stratification of association analyses by number of domains with serious health problems and additionally by health problems in each of specific domains as well as by high/low level of education) is either still missing or not really comprehensible and convincing.

Regarding dichotomization the different health domains: The cognition/communication domain is based on mmse and interview type (direct vs proxy interview). Interview type is by necessity dichotomous and therefore we had to dichotomize this domain. Both other two domains are ordinal and could have been analyzed as ordinal. We have still chosen to use dichotomous versions in the analyses of the separate domains. There are two reasons for this: 1) The most important reason is to make the analyses of the separate domains comparable; 2) We also thought that it was more didactic to have the domains dichotomized since they had to be dichotomized in the analyses of number of health domains with severe problems. This information is not included in the article, but we are willing to do this if the editor thinks it is a good idea.

Regarding dichotomization educational attainment: In Table 2 and Table 3 we have controlled for level of education as dummy-coded variable with four categories (compulsory, vocational, upper secondary, and university). In Table 4, level of education was dichotomized as low education (compulsory; 8 years for most of the study participants) and high education (vocational, upper secondary, and university). Education was dichotomized due to the low number of persons with education above the compulsory level (compulsory 816 persons, vocational 557 persons, secondary 102 persons, and university 70 persons). This information is now included in the article, p. 10-11, lines: 24-4.

In addition, the newly provided reason for performing (multinomial) logistic instead of linear regression analyses is not adequately positioned under ‘statistical analysis’ since subsequently presented study results are anticipated and taken as a justification for choosing this approach.

We have now re-written our statistical choices and hope this will clarify our approach, p. 11, lines: 9-19:

“Preliminary analyses showed that the variable number of health domains with serious health problems was a non-normally distributed, ordinal variable. Ordered logistic regression is suitable for ordinal variables like number of health domains with serious health problems if the assumption about proportional odds holds. Exploratory analyses with multinomial logistic regression showed that the assumption about proportional odds was violated, suggesting that using multinomial logistic regression was the better approach to analyze the available data. Therefore, multinomial logistic regressions were conducted to study the associations between work-related stress during midlife and number of health domains with serious health problems, analyzed as two separate outcomes in old age – serious problems in one health domain and serious problems in two or three health domains (what we call complex health problems).”
In sum, the paper even in its revised version cannot hide or blind us to the fact of somewhat disputable and poor measures as regards work exposures and health outcomes and the partly incomprehensible statistical approach. But I must admit and agree with the authors that the use of improved measures would presumably not change much of the overall pattern and probably strengthen rather than weaken the findings and associations between job strain in middle age and health outcomes in old age.

**Reviewer: 2**

*Comments to the Author*

I want to congratulate the authors for this well-improved second version of the manuscript. I think they overall addressed my comments in an appropriate way. I welcome their clarifications - and particularly the simplification of the SEP indicator seems to be a good choice. Moreover, I do see that these data have some limitations that cannot be dealt with by the authors. Having said that, I also see the high relevance of publishing this kind of research based on these valuable data. The most important message for the authors then is that they should go on with this work, while being constantly aware of these limitations and with a general attitude towards making them explicit in their manuscript and framing their results within the context of these limitations.

Thank you!

- **Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)**

Thank you for the suggested changes. We have tried our best to accommodate them (question 1-6).

- **Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)**

(7)

p.6. (and further). In the objective statement (III) it should be stated “measured with education at baseline” (or at the age of the sample xx-xx at baseline). Moreover, the authors should be aware that – although educational attainment is relatively stable over the life course – people’s SEP may change. In order to be accurate it is therefore advisable to consistently refer to “education” instead of “SEP” from the results section onwards. It may be more accurate to narrow the title down to “educational attainment” instead of “SEP”.

We have now decided to change from SEP to educational attainment. We agree that this makes it even clearer.
We have now expanded the limitation section of the discussion on p. 20, lines: 7-16, please see first response to Reviewer 1.

- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

Regarding the conceptual scheme of SEP. As the authors are now presenting the role of SEP, it is a common cause of both an adverse work environment (differential exposure) and adverse health. Moreover the rightly assume (see also their interactive effects) that SEP acts as an effect modifier (differential vulnerability). This role of SEP however does not mean that the relationship between work environment and health is "spurious". Statistically speaking this may be true, but from a theoretical sociological point of view it is not. I would like to ask the authors to reflect on this in their introduction, since it is essential for a correct understanding of the relationship between SEP and job characteristics.

We have now changed according to your comment and reflected on this in the introduction p. 4-5, lines: 24-5:

“But the relationship between socioeconomic position and work-related stress is far from straightforward. For example, lower socioeconomic position tends to be associated with poor psychosocial working conditions, such as low control [5, 17-19], whereas psychological job demands are often positively associated with higher socioeconomic position [3, 17, 20]. Finally, work-related stress has shown not correlate that strongly with socioeconomic position [21].”

Moreover, in my opinion, also the interpretation and discussion (see for example discussion, p.15; 17-18) of results of controlling for SEP should be framed in that kind of conceptual reasoning. Old, but still very useful in this regard are the concepts of proximal and fundamental causes of disease of Link and Phelan (Link & Phelan, 1995; Phelan, Link, Diez-Roux, Kawachi, & Levin, 2005). I would present it as SEP acting as a fundamental social determinant of health which "enacts" itself in the work environment. That is: via a complex set of formal and informal mechanisms, people with lower SEP have a higher probability to get allocated to less beneficial jobs in terms of health. So, the relationship between these jobs and health is not spurious in the sense that the real health effect comes from SEP. SEP in its own right is not to be expected to have any health effect whatsoever. SEP can be assumed neutral for health in this context, but it does act as an allocation mechanism putting people with a higher probability in does situations that are harmful for health (or beneficial).
We agree that socioeconomic position, and in this case level of education, may act as a social determinant of health that enacts in the work environment via different mechanisms. We have therefore added this in the discussion section on p. 18, lines: 12-15:

“Of note is that socioeconomic position may act as an “allocation mechanism”, hence reflecting individuals’ inherent risk for early onset of morbidity (Link & Phelan, 1995). Given the role of educational attainment the work stress-health links observed here, this inherent risk still interferes with the effectiveness of interventions concerning work-related stress.”

(10)
With their results, the authors are at least implicitly sending a strong message to the world: “in men (moderately) high job demands are protective for health in later life”. This message should be considered in a context of a consistent rise in quantitative job demands in Europe throughout the 1990’s and the 2000’s – see for example: (Greenan, Kalugina, & Walkowiak, 2013) – and as you stated yourself in the article. I recognise that the authors do their maximum in terms of controlling health at onset and they make the best out of older data with clear limitations. Moreover, these limitations may not be a reason not to use these valuable dataset. Nevertheless, it cannot be ruled out that numerous other effects are at play here.

Among these, I keep insisting that subtle selection effects of the most vulnerable workers into the lowest demanding jobs are a very plausible explanation as well. Keep in mind that the “low demand category” is the smallest one (33%), so the one where the “special cases” are in the majority. I agree that such selection effects are impossible to capture in the models with only two measuring points in time. They can also be very complex: minor health and cognitive problems, addictions, complex family situations, etc. My point is that this issue should be addressed in detail in the discussion in order to prevent un-nuanced recuperation of the findings of this article in the policy field.

We have added references (Greenan, Kalugina, & Walkowiak, 2013) as suggested with regard to increased job demands (work intensity) during 1995 – 2005. Moreover, the decreased odds of serious health problems among men with high job demands may partly be due to health selection. We have therefore added a sentence on p. 17, lines: 6-7.

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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