Author’s response to reviews

Title: The influence of working conditions on health satisfaction, physical and mental health – Testing the effort-reward imbalance (ERI) model and its moderation with over-commitment using a representative sample of German employees (GSOEP)

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Author’s response to reviews:

I presented the responses in the attached Excel document. Just in case, I also copied the comments and my changes here:

Reviewer 1:

Thank you for your comments and criticism on my resubmission. I worked on the development of my arguments and clarified the discussion. The paper was language proofed once again. Besides a very few typos, I changed only the following points listed in the tables.

- Also, why is it important to study these outcomes? Why is health satisfaction an important outcome?

--> P. 2: "In addition, this paper draws a direct comparison between different health indicators for health and examines whether health satisfaction, physical health or mental health was predicted the best by the effort–reward imbalance” (ERI) model. In this way, it might be examined if health satisfaction – measured by only one question – might also be an appropriate substitute for extensively collected health items." was added.

- P. 6: "whereas over-commitment was often neglected [4]. Why is it a bad thing that overcommitment was neglected?

--> P. 6: "This paper aims to close the research gap and includes over–commitment in the empirical analysis as it is actually outlined in the third hypothesis." was added.

- P. 6: To the best of my knowledge, it still lacks a methodological debate on, e.g., distributions of the variables or using different model estimators which will be taken into account and discussed in this paper." Why is the methodological debate needed?
P. 6: This sentence was deleted because the methodological discussion constitutes only a small part in the revised paper.

- P. 7: What is the final sample size? You just state "more than 7300 respondents" on page 7.

--> P. 7: You were right: This is really confusing. I changed it to": the majority of the respondents were male (60%)." And the sample size is shown on page 8.

- P. 8: "whereas women were less satisfied" Less satisfied than what?

--> P. 8: "than men" was added.

- I am wondering why you chose to center the variables instead of standardizing them?

--> P. 9f.: "The sum scores were mean–centered in order to be able to interpret the constant properly because the value “0” does actually exist on the scale representing the mean value. Centering is a linear transformation of metric variables, which does not influence the interpretation of the regression coefficients." P.10: "In order to avoid multicollinearity in the regression models with interaction effects [28], the ERI ratio was also mean–centered and ranged from –0.77 to 2.98." For further information see: Aiken,L.S. & S.G.West, 1991: Multiple Regression: Testing and Interpreting Interactions. Newbury Park: Sage

- P. 13f. Table 1 and 2: I would advise to only use the standardized coefficients and reporting the standard error in the tables.

--> Some prefer the not standardized regression coefficients for the interpretation of the results and I also refer to them in the text. I left out the standard errors purposely because they are really low in my model with 7000 cases and do not differ only little. Besides, the standard errors would make the table too complex.

- What are the actual implications of these results if you look at the standardized estimate? It is quite small and your sample is quite large so what conclusions do you draw based on that? This needs to be addressed in the discussion at the latest. Contemplate on the practical implications of the small effect sizes you obtained.

--> I only looked at a specific part of health determinants. Therefore, it should be clear that I cannot explain 90% of the variance of the health indicators. Besides, social scientists know that it is always a set of variables which affects social phenomena. This interplay is what we create in complex models where the whole R² has to be looked at. The standardized regression coefficients should therefore only be used in order to compare the effects of the different variables.
The discussion still needs some work as there were some unresolved issues and undiscussed results. Overall, I was sometimes left with more questions than when I started, so the author should try to argue as thoroughly as possible, while at the same time keeping it concise (difficult, I know, but necessary for scientific literature).

--> I added a few sentences in order to clarify the text and restructured it.

- "Therefore, Siegrist first hypothesis was not supported and the use of the ERI ratio should no longer be recommended" Why is this?

--> P. 15: I softened the sentence: "In these models, Siegrist first hypothesis was not supported by representative data of more than 7,000 respondents."

- On page 16 you discuss the results relating to the second hypothesis but the paragraph steers into a discussion about the ERI ratio and at the end you make suggestions for practice based on rewards and effort - where is overcommitment?

--> P. 16: I expanded the discussion on overcommitment.

- Where was the discussion on the third hypothesis?

--> P. 17: Added.

Reviewer 2:

Thank you for your comments and criticism on my resubmission. I clarified the discussion. Besides a very few typos, I changed only the following points listed in the tables.

- P. 6: It is said that: "In the here used data from 2016…" Could be revised for clarity as: "In this study, I used data from 2016 where respondents…"

--> Changed.

- There could be a reference to the statistical modelling used.

--> Added.

- P. 13: In both Tables 1 and 2 it would be good to have a footnote for the meaning of "Coef." and "Stand. Coef."
- Table 2. Why was education not included in the interaction models?

---> P. 14: Since the results did not differ so much, I decided to leave out the control variables for reasons of simplicity. In my last version of the paper, I gave the variables sex and age the value 0, which meant that the graph was valid for men with the average age. The reviewers did not understand this; that is why I kept it simple. Yet, I added this sentence to my actual manuscript: "Figure 3 illustrates the interplay of the variables in the model, where the control variables were left out for reasons of simplicity because they only exerted a very small influence on mental health."

- P.15: It is rather a strong argument (based on results of one study from one country) to say: "…ERI ratio should no longer be recommended…” I suggest the author revises this sentence.

---> P. 15: I softened the sentence: "In these models, Siegrist first hypothesis was not supported by representative data of more than 7,000 respondents."

- P. 15: It is said that: "…it might be easier to develop programs in order to preserve employee health and create healthy working conditions." Perhaps this could be further elaborated to include "health promotion" in addition to preserving health, and "positive working environments/ positive leadership" in addition to healthy working conditions. There are references on these topics.

---> P. 15: Thank you for your advice. I added: "Workplace health promotion could also include leadership development towards an honoring working environment."

- P.: 16. The author discusses that the weak association with physical health might be due to cross-sectional study design. This can be true, but also the type of the measure may affect this. For example, we have observed in longitudinal setting that ERI is associated with physical health measure (pain) (Halonen et al. Associations between onset of effort-reward imbalance at work and onset of musculoskeletal pain: analyzing observational longitudinal data as pseudo-trials, Pain 2018)

---> P. 16: I added a discussion on the indicator.