Reviewer's report

Title: Effort-reward imbalance and work ability: cross-sectional and longitudinal findings from the Second German Sociomedical Panel of Employees

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Reviewer: Hynek Pikhart

Reviewer's report:

This manuscript focuses on the association between work stress assessed by ERI and so-called Work ability index. There are some comments, both major and minor.

Major comments:

Probably the main comments related to representativeness of the data and response rate. I believe that 36.5% response to baseline data is very low. More, 1636 individuals after one year means that only about 22% of original sample responded. Seeing the final number of respondents included in the analysis, authors get below 20%. I think it is inappropriate to report any findings from such unrepresentative study. Additionally they rather dismiss this major weakness in the discussion. I think it is rather bad to have only 873 eligibles for prospective analysis 1 yr into cohort study from originally selected 7500 people.

Whole discussion should have much longer section on limitations as they are major. It is not just the response rate, it is also white collar selection of the sample that makes the study not representative for the population. Secondly, it is rather difficult to claim that it is longitudinal study. While the authors try to separate those free of WAI or ERI at the baseline, 1 yr follow is too short for such study, and it would be helpful to have longer gap between wave 1 and wave 2 of the study.

Use of ERI >1, <1 reduces substantially power of the study. There are not enough people with >1. maybe, upper quartile or upper tertile would improve analytical power.

Another important point is that WAI measure is too close to ERI. It is close to work demands and as such to effort, and it is not surprising that it is so well associated with ERI. I think the authors basically use two measures closely related to each other, so it is rather obvious that authors will find strong relationships.

Table 2 - probability can never be 0.000!! Authors should write <0.001. This is major misunderstanding of statistical inference

Minor essential comments:

In some places, both in Background and Discussion, authors use previous
evidence to support some arguments that come from the same study and from the same group of authors. As this study partly combines cross-sectional findings with longitudinal findings, use of these references is rather limited. I would suggest to list more external papers supporting authors' arguments.

When creating binary WAI measure, authors use cut-points based on Tuomi's paper which is in German, and I am unable to see. Are the cut-points validated? Is there any other work on validation of this instrument?

It seems to me that moving from continuous scale to binary data is also quite radical in terms of the loss of power. Using at least quartiles seems to be more effective use of collected data.

When describing ERI instrument authors stated they used 5+11 questions. There was originally 6 questions on effort - could they specify which question was omitted?

Similar to the point related to WAI, dichotomising ERI with cut-point 1.00 seems quite radical in terms of power reductions. It would be possible to use different cut-points, or to deal differently with ERI score.

Covariates - were any other SES measures collected? Could they be used?

Health-related behaviours - it seems again that authors simplified data too much using HB scores. It is not surprising that such simplified score did not play major role in the analysis. I suggest to use each HB variable as separate covariate in the analysis.

CFA: ERI is calculated just as ratio of E/R, so why to use 3 subcomponents of Reward in CFA if it is not used in the construction of the final measure?

Table 1 should be more detailed. It would be good to see whether WAI, ERI, Demands or age are normally distributed (maybe showing interquartile range and median). Also, to show more for categorical variables might help.

Discretionary comments:

I am not sure whether the authors need to present both directions of cross sectional analysis. The association is two-directional (and we can see that in univariate analysis results are indeed identical). Adjustment makes some very small differences but still it might not be essential to present two sets of cross-sectional results.

Table 3 - title - I think word "predicting" should be replaced as this is cross-sectional analysis, so we should talk about associations. It would also be good if authors add footnotes showing what adjustment is used in models 1.1., 1.2, 1.3

**Level of interest:** An article of limited interest
Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interest.