Reviewer's report

Title: Effort, reward and self-reported mental health: A simulation study on negative affectivity bias

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Reviewer: Johannes Siegrist

Reviewer's report:

This paper analyses the potential impact of bias due to negative affectivity (NA) in epidemiological studies testing associations between (self-reported) psychosocial adversity at work and (self-reported) mental health, using statistical simulation techniques. Data from a survey of paramedics and emergency medical technicians in Switzerland served as empirical basis where items of the effort-reward imbalance questionnaire (measuring an adverse psychosocial work environment) and items of the General Health Questionnaire (measuring mental health) were analysed in multiple simulated populations. It is the aim of this study to estimate to which extent NA can induce spurious associations between stressors at work and mental health. Authors conclude from their simulation analyses that substantial correlations with an explained variance beyond 10 per cent are unlikely to be the result of bias due to respondents’ negative affectivity.

When reviewing this paper I should declare that I have no personal experience with the simulation procedure applied in this study. Thus, I restrict my evaluation to the theoretical and methodological aspects of the research topic.

Overall, the subject of this paper is relevant because adverse influences of stressful work on mental health are a growing concern in occupational public health research, with far reaching policy implications. At the same time, given the restrictions of objective measurements in this area of scientific analysis, methodological critiques of research findings are wide spread and need to be tackled. The authors’ methodological approach towards this problem is innovative and deserves careful consideration.

Despite these merits the manuscript in its current version suffers from several shortcomings.

Major Compulsory Revisions:

1. In the Introduction, authors should explain to the readers the effort-reward imbalance model by describing its basic ideas and the main components of its operationalisation in terms of self-reported Likert-scaled items. This explanation should also clarify why the different variables measuring the model are used separately, i.e. the log-transformed and the non-transformed ratio between the scale ‘effort’ and the scale ‘reward’ as well as the separate scores of these two scales. Moreover, the choice of the General Health Questionnaire as a valid measure of mental health needs to be justified more clearly.
2. In reference 14 authors point to the empirical study (meanwhile published) which serves as an empirical basis. The readers of this paper would like to know more details about this study, e.g. sample size and composition, the strengths of observed associations, the confounders included in their multivariate analyses.

3. I am not sure whether enough information is provided in the Methods section to fully understand the authors’ argument of how they proceeded to test the influence of NA. For instance, what were the reasons to define the proportion of NA subjects in a range between 5 per cent and 20 per cent of the population and to define the number of items affected by NA in a range between 10 per cent and 40 per cent?

Minor Essential Revisions:

1. The results of table 1 and 2 should be described in more detail in the text. When presenting mean scores of scales in table 1, the range of the scores for each scale should be given.

2. When discussing the results authors should refer more explicitly to the rationale of their study as explained in the Introduction. How do their results answer the critical question of potentially biased correlations between two self-reported measures due to NA? Does the fact that inter-population variations of the scores of predicting variables are much larger than variations attributed to extent of NA invalidate the methodological critique? Why is the evidence produced by the current study stronger than the evidence produced by the alternative strategy of adjusting for NA in multivariate regression models? Answers to these questions might strengthen the receptivity of the innovative message of this study among readers.

3. To adequately frame the significance of their results in the Discussion section authors might stress the fact that evidence on associations between effort-reward imbalance at work and mental (as well as physical!) health outcomes has already been provided using prospective epidemiological study designs (see e.g. Ref. 4, 7, 9 and J. Siegrist 2005: Social reciprocity and health: New scientific evidence and policy implications. Psychoneuroendocrinology 30:1033-1038). Thus, prospective evidence indirectly supports the authors’ main study findings that NA is unlikely to invalidate substantial associations between stressful work and mental health.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I have no conflict of interest in reviewing this manuscript.