Reviewer's report

Title: Self-perceived stress reactivity is an indicator of psychosocial impairment at the workplace

Version: 1 Date: 5 March 2010

Reviewer: Sampsa Puttonen

Reviewer's report:

General comments
The study assessed cross-sectionally the convergent validity of self-perceived stress reactivity-scale and its bio-physiological correlates. The sample of middle-aged (male) employees with 174 participants is a relevant population to study these questions, although the variance of this selected population may be compromised and thus limit the possibility to establish significant differences between high and low reactive individuals. The manuscript is well-written and uses sound statistical methods and relevant references.

Minor Essential Revisions

1. The introduction provides a good basis for the study-hypothesis. What I would like to read more about is a short analysis of the concept of perceived stress reactivity. What is its relation to other stress concepts and what it does add to the existing stress formulations or coping styles?

2. In relation to 1st comment. It would be informative to get more information on the stress reactivity scale, also. The authors could consider following procedures; add sample questions in methods section or provide access to a whole (English) version by either adding items to appendix or providing a WWW-link to the scale.

3. As the scale is relatively new and its psychometric properties now widely known. It would be interesting see how the previous high internal consistencies were replicated in the present sample.

4. The authors write that theoretically SPSR is dispositional. Do the authors have good data on this? It appears that the heritability is low (which does not mean that it cannot be a stable propensity of an individual). I think it may be premature to state that is dispositional until the temporal or situational stability is proved.

5. Table 2 the upper panel. The variables and their values are not perfectly matching.

6. page 15 Prediction of self-perceived stress reactivity. The results have already been given in Table 1. Therefore I did not find this part of the results of much value. I suggest that authors remove table 5. If the authors think that the ORs add significantly to the results they may be put in the text where the other results of table 1 are presented.
7. Discussion. The first paragraph. I think that the general message of this might be included in the introduction. The difference from general population level is relevant for discussion and could perhaps be presented later after the main results.

8. The study has a high number of measures. Please, check if all of them have been used in the analyses e.g. waist circumference.

Discretionary Revisions

1. The results for allostatic load and its "components" were rather disappointing. As the authors write, self ratings of stress and psychophysiological responses or levels do not often match. The reasons for non-significant associations have been brought up in the discussion. I would like to add that in future the use of pre-selected high and low stress groups might be useful. The median split sample may not generate large group differences. Did the authors compare e.g. high and low tertiles or use the suggested scale cut-off point to dichotomise the sample?

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

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