Author's response to reviews

Title: Psychosocial work environment factors and weight change: a prospective study among Danish health care workers

Authors:

Helle Gram Quist (hgg@nrcwe.dk)
Ulla Christensen (ulch@sund.ku.dk)
Karl B Christensen (k.christensen@biostat.ku.dk)
Birgit Aust (bma@nrcwe.dk)
Vilhelm Borg (vbo@nrcwe.dk)
Jakob B Bjørner (ibj@nrcwe.dk)

Version: 3 Date: 18 December 2012

Author's response to reviews: see over
Thank you so much for the general comments about the manuscript. These are greatly appreciated.

Major Compulsory Revisions:

1) In the last paragraph of the chapter “Results” terms “total model”, “adjusted model” and “final model” are used, but it is unclear what models they are referring to. Please specify what these models include.

Response: Thanks for pointing this out. We have now dropped this section in response to other comments. Please see below under “discretionary revisions”.

2) More information is needed regarding the statistical methods and the interpretation of odd ratios. Now it is not clear how the odd ratios were obtained. Which procedure was used? Only when considering BMI as continuous variable it is mentioned that SAS Proc Mixed was used. For the psychosocial work factors such information is not available. Were the psychosocial work factors treated as continuous variables, or were they dichotomized? If the variable were treated as continuous, some of the achieved odd ratios per unit increase of the psychosocial seem disproportionally large considering that the scales of the work variables is said to be 0-100. Additionally, I suggest sensitivity analyses: are the associations between work factors and bmi change similar if the work factors are treated as categorical variables or as continuous variables.

Response 1: Yes, this was indeed a bit vague in the manuscript. We have now clarified that we conducted the analyses using Proc Logistics (SAS, version 9.2). Furthermore, we now also mention
that the odds ratio represents 10-points increases and not 1-point increases, as it could have been interpreted in the original manuscript. Please see page 9 in the revised manuscript.

Response 2: On your suggestion, we have also conducted sensitivity analyses. We checked whether the associations were similar regardless of the predictor variables being treated as categorical or continuous. We only tested the significant factors from the regression analyses. We found that when treated as a categorical variable (three levels) quality of leadership was no longer a significant predictor for weight change among men. However, among women, role clarity and role conflicts remained significant predictors of weight change, as did age and cohabitation. Please see page 9 and 11 in the revised manuscript.

Discretionary Revisions:

1) Why was +/- 2 kg/m² chosen as a cut-off? What is the authors’ interpretation of the different outcome when using weight as a continuous variable?

Response 1: We have addressed this more explicitly in the revised manuscript. Based on the existing literature, there is to our knowledge no commonly accepted standard as to what constitutes a meaningful weight change. Thus, we have not been able to find a common cut-off point. Therefore we looked at the distribution of weight change in our population. We considered a +/- 2 and 5 kg/m² change, but the number of respondents who had gained or lost more than 5 kg/m² was too small. Only 10% had either gained or lost more than 5 kg/m², while 28% of the respondents had gained/lost more than 2 kg/m². Due to the low number of men represented, this was especially difficult among men. Only 7 male respondents had either lost or gained more than 5 kg/m². So based on these distributions and the lack of a common standard, we chose to use +/- 2kg/m² as a cut-off. However, we argue this is appropriate as previous research has indicated that – almost - any level of weight change (both loss and gain) is associated with increased risk of mortality, regardless of the initial weight level (Mikkelsen et al., 1999). This finding would justify the use of the chosen cut-off point. Please see page 8 in the revised manuscript.

Response 2: As mentioned in the manuscript we found that the associations between psychosocial work factors and weight change were weaker when weight change was addressed as a continuous variable (LogBMI). Age and cohabitation remained significant among women, but none of the psychosocial variables did (for either men or women). As we found in this study, the same work
factor can increase the risk of both weight gain and a weight loss. In other words, an unsuitable weight change can occur in both directions depending on the individual. This goes well with findings from Epel and colleagues (2004) who found that some people react to stress by eating more, while others eat less and Kivimäki et al. (2006) who found evidence for the bi-directional effects of work stress on BMI. Thus, using BMI as a continuous variable could limit the possibility of finding these bi-directional effects. We argue that the weaker results we found when using LogBMI could be due to this. It is different to analyze changes away from the normal range (unchanged weight) than changes on a continuous scale, thus different results could be anticipated. Please see page 11.

Finally, it should be noted that we mistakenly had written in the original manuscript that meaning of work and workload were significant predictors when addressing BMI as a continuous variable among men. This mistake has been corrected. Please see last sentence on page 10.

2) When considering the original study population (n=12746) the attrition rate was quite high (participants n=4135). Is there any knowledge how well the participants represent the original study population? What kind of bias could high attrition cause?

Response: We have conducted analyses of difference (Kruskal-Wallis test, SAS, version 9.2.). We compared the study population (those responding at both baseline and follow-up) with those who only responded at baseline. Based on the analyses of difference, we found that the study population did not differ from the baseline respondents with regards to BMI, age or leisure-time physical activity. However, the study population were less often smokers, had longer tenure and lower physical demands at work. Furthermore, they report a better psychosocial work environment than the baseline population on most measures. However, these differences in work environment are small. Please see page 5 and 6 in the manuscript.

Response: High attrition can cause loss of power by the loss of participants and the reduction in persons with poor work environment. However, since we found no difference on BMI, we would argue that bias from attrition is not a major concern in this study. Please see page 14.

3) Men and women were analysed separately, but the amount of men was very low. It could be informative to report the amount of participants for regression analyses, as it is said that 20 men were excluded additionally because of missing information. For example, now the
amount of men who lost weight seems to be 8, but is it even lower? I would be quite cautious to draw any conclusions from such a small group.

Response: Thank you for pointing this out. Two typos were reported regarding the number of men and women included in the analyses. A total of 136 men were included in the regression, as 17 (not 20) were excluded due to missing values on the response or explanatory variables. Out of the 136 men, 112 belonged to the reference category, while 14 and 10 men had, respectively, gained or lost more than 2 kg/m². Caution must be taken when drawing conclusions regarding the men. A total of 3647 women were included in the analyses after 335 (not 597) women were excluded due to missing values. Please see page 10 in the revised manuscript.

4) Concerning the issue of multicollinearity: In the chapter “Statistical analyses” it is reported that “we checked for multicollinearity…”, however, in the chapter “Results” only pair-wise correlations are reported. It is therefore unclear if you have used explicit tests for multicollinearity (such as proc REG with the /Collin and /vif options) or not.

Response: Yes, it was indeed a bit unclear as to how we had checked for multicollinearity. We have now explicitly stated how we checked for multicollinearity in the revised manuscript. We have tested for multicollinearity by using the VIF option in a regression analyses (Proc Reg, SAS, version 9.2). For simplicity of the presentation we have dropped the correlation analyses. Please see page 11 in the revised manuscript.

5) Do you think that multiple testing affects the results?

Response: This is indeed a valid question and one we did not address in the original manuscript. We are now addressing this in the discussion section of the manuscript as limitations of the study. It is clear that multiple testing can affect the results, as it increases the risk of mass significance. Thus, some caution must be taken when interpreting the significant results. Please see page 14 in the revised manuscript.
Minor essential Revisions not for publication:

1) Material and Methods: “Table 1 present descriptive characteristic of participants..” ->

presents descriptive characteristics of the participants

Response: This has been changed accordingly (page 5).

References:

Mikkelsen KL, Heitmann BL, Keiding N, Sorensen TIA. Independent effects of stable and changing body

Epel E, Jimenez S, Brownell K, Stroud L, Stoney C, Niaura R: Are stress eaters at risk for the metabolic

and weight loss: evidence for bidirectional effects of job strain on body mass index in the Whitehall II