Author's response to reviews

Title: Socio-economic differences in self-reported insomnia and stress in Finland from 1979 to 2002: a population-based repeated cross-sectional survey.

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Author's response to reviews: see over
Authors’ response to the reviewer, revision 2

Authors’ response: In order to make it more readable, those corrections that reviewers have previously accepted and found satisfactory are no longer seen as marked. Second round revisions, which are related to 2. reviewer comments 2 and 3, are shown in the manuscript. However, if reviewers prefer to see all corrections visible from the first and second revision, we are happy to provide one manuscript including all marks.

Reviewer’s report
Title: Socio-economic differences in self-reported insomnia and stress in Finland from 1979 to 2002: a population-based repeated cross-sectional survey.
Version: 2 Date: 16 January 2012
Reviewer: Jane Ferrie

Reviewer’s report:
Overall this paper documents enduring socio-economic inequalities in health in Finland between 1979 and 2002. In this respect the authors document a relatively well known phenomenon. My concern is that they have already published on this in relation to self-reported depression (Reference 6). In a number of their previous publications, as well as in this paper, they describe self-reported depression as one of their measures of psychological symptoms. All I want them to do in this paper is to show that their other psychological symptom measures are independent of self-reported depression and so have new information to add to the existing literature. This they have not done so far in my opinion. My more detailed comments are below.

2. I am concerned by the decision to dichotomise the self-reported stress variable into unbearable’/’bearable’, a split that results with 2.5% in the exposed and 97.5% in the unexposed groups. This dichotomy is justified by two references but neither reference provides evidence of a threshold effect. I would like to see these data reanalysed with response categories 1 and 2 combined to form the stress exposure group. At a minimum, these findings should be presented as sensitivity analyses in the text.

We acknowledge reviewer’s concern about stress cutpoint. We have used different cutpoint in our first article, where response categories 1 and 2 were combined as indicator for stress. We did additional analyses with wider stress outcome for present data, and we have added following texts on methods, results and discussion:

Methods, page 8, first paragraph: However, for additional analyses we expanded stress category to include “more stress than in people normally” group also to examine the robustness of the stress outcome.

Results, page 13, first paragraph: When wider stress category (including ‘more stress than normally in people’ and ‘unbearable stress’ combined) was used as an outcome measure in additional analyses, this resulted in reversed educational differences; highest educated had more stress compared to the lowest educated among both men and women in both models (data not shown in the tables).

page 15, last paragraph: Some other methodological issues need to be addressed concerning the variables used. In this study we used “nearly unbearable stress” as an indicator for stress. This results with 2,5% in the exposed and 97,5% in the unexposed groups. In our previous study with 2002-2003 data [36] and also in additional analyses with present data, we have
used other classification of stress, which included ‘more stress than normally in people’ and ‘nearly unbearable situation’ combined. In those analyses with stress prevalence of 16-20%, we found that the effect of education was reversed with stress; those in the highest or intermediate education were most stressed compared to the less educated. In our present study, unbearable stress was more common in the lower levels of education compared to the highest education. In general, our stress measure is exploring only perception of stress, with no further information about sources of stress, or possible coping resources or mechanisms. However, our present study and earlier results on mortality seem to indicate, that those in the lowest position are most affected by the extreme stressful situations.

I am afraid I am still not happy with the treatment of the stress variable. Firstly in English use of the description ‘unbearable’ is hyperbolic as it literally means stress that it is not possible to bear. Whatever the wording of the question in the questionnaire I would prefer to see this categorised as extremely high stress. However, my more important concern remains the dichotomisation of this variable. The authors report that when they dichotomise to include response categories 1 and 2 in their exposure measure that in men educational differences were reversed. They do not report what happens to income or household differences in either sex, nor do they report what happens to educational differences in women. Given that this extension of the exposure category makes such a fundamental change to their findings on educational differences in men the authors need to report these findings in full. Personally I would prefer to see this dichotomisation presented in the tables and the analyses of the more restricted dichotomisation of the variable reported in the text. Either way, the results reported in the text should include the actual results as ORs with confidence intervals for the whole period.

Authors’ response: We have changed the ‘unbearable stress’ for ‘extremely high stress’. For methods –section, starting page 7, last paragraph, we also tried to be even more precise with the option formulation: Stress was addressed in a separate four-point scale question: “Have you had symptoms of tension or been under great stress or considerable strain during the past 30 days?” (1= my life is nearly unbearable (2.5%), 2=more than people in general (15%), 3=somewhat but not usually so (60%), 4=not at all (23%)).

As suggested, we have done additional table to present results for extended stress category, for total study period. In text, we have explained following:

Methods, page 8, first paragraph: However, for additional analyses we expanded stress category also to include ‘more than people in general’ group (17%) in order to examine the robustness of the stress outcome.

Statistical analyses, page 10, last paragraph
Moreover, additional logistic regression analyses for expanded stress category (17%) by socio-economic indicators were conducted in order to examine the robustness of the stress outcome (Additional file 3).

Results, page 13, second paragraph: Complementary analysis with extended 17% stress category (including ‘more stress than people in general’ and ‘my life is nearly unbearable’ combined) as an outcome measure resulted in reversed educational differences; the highest educated had more stress compared to the lowest educated among both men and women in the both models (see Additional file 3). Employment
status and household income level gradients in the extended stress category were parallel to those associations of ‘my life is nearly unbearable’ category, in other words, extremely high stress.

**page 15, last paragraph:** Some other methodological issues need to be addressed concerning the variables used. In this study we used ‘my life is nearly unbearable’ category as an indicator for extremely high stress. This results with 2,5% in the exposed and 97,5% in the unexposed groups. In our previous study with 2002-2003 data [36] and also in the additional analyses with present data, we have used extended classification of stress, which included ‘more stress than people in general’ and ‘my life is nearly unbearable’ combined. In those analyses with stress prevalence of 17-20%, we found that the effect of education was reversed with stress; those in the highest or intermediate education were most stressed compared to the less educated. However, extremely high stress was more common in the lower levels of education compared to the highest education. In general, our stress measure is exploring only perception of stress, with no further information about sources of stress, or possible coping resources or mechanisms. However, our present study seems to indicate, that those in the lowest socio-economic position are most affected by extreme stressful situations.

3. The authors have published previously on the psychological symptom data in this study. In their previous paper they look at self-reported depression (reference 6). This paper similarly found that there had been little change in socio-economic inequalities in self-reported depression over the 24 years reported. With insomnia included in the diagnostic criteria for depression the assumption in the past always tended to be that insomnia was a symptom of depression. However, studies over the last decade show insomnia to be a separate condition; albeit one that has high co-morbidity with depression. The present study is limited if findings for insomnia are presented without adjustment for self-reported depression. A similar argument can be made for self-reported stress.

This is a justified comment. However, next concern is why not to adjust for physical symptoms, as these may also be associated with insomnia (pain, chronic diseases, musculoskeletal diseases)? Furthermore, adjustment for stress with depression does not seem necessary as stress is not a diagnostic criteria for depression. We had both insomnia and depression questioned in a list of 14 symptoms and health problems (including physical symptoms). People were asked if respondents experienced any of the following symptoms or problems during the past 30 days. Based on that question, our focus is on the self-reported psychological distress symptoms, not diagnosed depression or insomnia. Regardless, we conducted all analyses for insomnia with adjustment for depression. We have not shown any additional data, but we added text on methods, results and discussion about the effect of adjustment, and we hope these will be satisfactory and clear the issue.

The authors’ question in relation to physical symptoms seems to imply a level of unreasonableness in my request that they adjust their analyses of insomnia and stress for self-reported depression. They ask why shouldn’t they adjust for physical symptoms? The answer is that according to their own description, the three variables; insomnia, stress and self-reported depression, are all psychological symptoms. Self-reports of physical symptoms may contain a subjective component and may well cause insomnia and stress but they are not potentially the same as insomnia or stress. So far the authors have adjusted their findings for insomnia for self-reported depression. They only report on the result
of this adjustment in women and only for education and income. In both cases there is no evidence of differences after the adjustment; a finding that would indicate that their measure of insomnia and depression are not independent. I would like to see the results of these adjustments reported fully with ORs and confidence intervals either in the tables or in the text. Similarly for stress as I am not convinced that stress as measured here is a measure of psychological distress that is independent of depression.

I don't understand the significance of the authors’ claim above that their “focus is on the self-reported psychological distress symptoms, not diagnosed depression or insomnia”, as in their previous paper (Soc Psychiat Epidemiol (2009) 44:871–879) they report that there was a high correlation between their single-question depression measure and the general mental health inventory (MHI-5), which is a validated measure of depression and anxiety.

Authors’ response: We tried to be more precise and accurate with this issue. As was suggested, we have conducted additional tables for models of both insomnia (additional file 1) and stress (additional file 2), adjusted for depression. These adjustments are explained in text as following:

Page 10, last paragraph: Finally, as insomnia is included in the diagnostic criteria for depression with well-known co-morbidity [26], we used self-reported depression (measured by a single-item question) as a covariate in additional analyses for total study-period 1979-2002 for insomnia and extremely high stress (see Additional files 1 and 2).

Page 12, first paragraph: Additional adjustment for self-reported depression was conducted in both age and fully adjusted models for total study period 1979-2002. Following this adjustment, socio-economic differences attenuated but existed statistically significant among men, however, the lowest educational and household income levels were no longer associated with higher odds for insomnia among women (see Additional file 1).

Page 13, first paragraph Additional adjustment for self-reported depression as a covariate in the models slightly attenuated educational, employment status and household income level differences in extremely high stress among both sexes. However, statistically significant associations vanished only for the lowest income level in the fully adjusted model among men (see Additional file 2).

Page 16, second paragraph: Mental symptoms are known to be associated with each other with complex interrelations and causality. In particular, insomnia is included in the diagnostic criteria for depression [26]. In our study, Pearson's correlations (p<0.001) between insomnia and depression were r=0.37 in males and r=0.34 in females; and between extremely high stress and depression r=0.24 in males and r=0.21 in females. After additional analyses with adjustment for self-reported depression, statistically significantly higher insomnia no longer existed by the lowest educational and household income levels among women. Among men, the independent effect of the lowest household income on extremely high stress was either statistically significant. Therefore, some of the educational and income level differences in insomnia and stress may be explained by depression.

We hope these additional analyses have shown the association of insomnia and stress with depression in detail, and although some of the SES differences in insomnia and stress may be explained by depression, trends in the prevalence for different indicators were not identical, as we have previously written on page 19, second paragraph: Trends in
the prevalence of insomnia and stress were not identical to either each other or to what we previously found using the same data with self-reported depression, which for example showed a decreasing trend contrary to the increasing trend in insomnia [6].

Additional comment:
Page 19, first paragraph: we omitted following sentence due to the fact that those associations we were referring to were not statistically significant: This finding was similar to what we have already demonstrated with self-reported depression [6].
We replaced the sentence with: Similarly, Valkonen et al. [56] found that economic recession slowed down rather than sped up the growth of relative inequalities in mental health related mortality (such as alcohol-related causes, accidents and suicide) in Finland.