Author's response to reviews

Title: The contribution of psychological distress to socio-economic differences in cause-specific mortality: a population-based follow-up of 28 years.

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Reviewer 1: Laura A Pratt
Discretionary revisions:
1. I believe this paper would be strengthened considerably if the authors included all-cause mortality and did all the same analyses using all-cause mortality as a third mortality outcome. If they choose not to add all-cause mortality, then there are a number of places (see major compulsory revisions below) that need to be re-worded.

We have analysed all-cause mortality in the preliminary analyses, but in order to focus on evidence based causes of death relevant to socio-economic inequality, we decided not to report all-cause mortality. Therefore, we are pleased to do the compulsory revisions as was suggested below by first reviewer.

Major compulsory revisions:
1. The title, the last sentence of the background section of the abstract, the last sentence in the discussion section of the paper, and the second sentence of the conclusions section all go beyond the data. Either the authors need to include all-cause mortality in their analyses, as suggested above, or in all these sentences the word “mortality” needs to be modified. It needs to say either cause-specific mortality or unnatural causes of death or something similar.

We agree with this comment, and have changed suggested sentences appropriate way using ‘cause-specific’ mortality. We also added ‘cause-specific’ to page 5, second sentence. On page 14, last sentence, we changed the following line “The present study supports previous findings that psychological factors only weakly or moderately mediate the relationship between SES and mortality.” for “In the previous studies psychological factors only weakly or moderately mediate the relationship between SES and all-cause mortality”

2. The discussion section should include some discussion of why psychological factors may explain some part of the socio-economic differentials in unnatural mortality but not in CHD mortality.

We edited discussion section on page 14, last sentence, and on page 15, first sentence. We added to discussion about why psychological factors may explain some part of the socio-economic differentials in unnatural mortality, on page 15. However, psychological distress explaining some of the inequalities in suicide, accidents and violence, and alcohol-related mortality indicates that in these specific causes of death, poor mental
health is related to more severe consequences in the lower socio-economic status groups than in the higher SES groups. It is possibly due to poor coping strategies of psychological distress in the lower SES. Obviously, that includes risky behaviour and, above all, heavy alcohol consumption which may be aimed at relieving psychological symptoms.

3. The unemployed vs. employed measure for socio-economic differences in employment is a limitation (most studies have information on class of employment) and should be mentioned in the limitation section.

We mentioned unemployed vs. employed measure in the limitation section as following, on page 14: Another limitation concerning measures used in this study is the unemployed versus employed classification, which is a crude measure of employment status.

Reviewer 2: Mark Hammer
I do not have any major revisions, only suggestions for discretionary changes;
1. Please clarify how the random sample was selected - ie, was it by household address, etc?

The random sample was selected using the simple random sample method. The total Finnish population was a basic population, and the sample covered individuals, not for example households.

We were more specific about the selection process by adding the following text to the manuscript on page 6, first sentence: The simple random sample was conducted by The Finnish Population Information System which is a computerized national register that contains basic on-line information about all Finnish citizens residing permanently in Finland.

2. Please present more detailed information about the participants excluded from the present sample - ie, did they differ in key characteristics such as SES markers

Reviewer suggested that we present more detailed information, such as SES, about the participants excluded from the present sample. We excluded year 1985 due to missing personal identification codes, so unfortunately for those respondents, we do not have adequate information. We excluded respondents under 25 years of age because their socio-economic status is not yet established, and therefore not relevant. We also excluded those persons who had missing data on psychological distress variables, total N=1129 (1.6%), and in order to clarify this exclusion, we added the total number of missing respondents on page 6, second sentence, and added the following text on page 13, last sentence; Additional analyses for respondents with missing data on psychological distress variables (N=1129, 1.6%), although containing relatively small number, showed
that those with missing data on psychological distress measures were also more likely to be in the lower SES groups.

For non-respondents, we have reported socio-economic characteristics from our previous study, page 13, last sentence.

3. The paper does acknowledge that the distress measures are not validated and this is a limitation. Another issue to consider is the possibility that the measures reflect transient but not chronic stress since the question enquires about the last 30 days. One would only expect chronic stress to impact on health thus there may be some misclassification that might lead to effect dilution.

We agree with the reviewer. We do not know if the symptoms are transient or chronic. However, as according to our study, these self-reported psychological distress symptoms are associated with mortality, so we assume that these measures are indicators that have impact on health. We modified our text as following on page 13: These measures may cover a variety of transient or chronic psychological symptoms, a wide range of meanings from the temporary decrease of psychological well-being to deeply impaired, even life-threatening disorders.

4. It would be interesting to add health behaviours into the models. Psychological distress is often highly correlated to smoking. In addition there is a higher rate of smoking in lower SES groups.

We agree with this comment, and we have considered this issue in our primary analyses. However, trying to avoid over-adjustment, and in order to focus our study to specific research questions, we chose not to add health behaviours into the models. For future research, it would be interesting to analyse health behavioural pathways related to this study question.

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Additionally we added within our manuscript, as requested by editorial, that our study is reviewed and approved by The Institutional Review Board of National Institute for Health and Welfare, IRB 00007085, FWA 00014588.