Reviewer's report

Title: The association between physical fitness and depressive symptoms among young adults: results of the Northern Finland 1966 birth cohort study

Version: 1 Date: 26 March 2013

Reviewer: Bianca Lever- van Milligen

Reviewer's report:

Major Compulsory Revisions
A demographics description and/or table was missing.

Minor Essential Revisions
Depression and depressive symptoms are both used in this article. However, since no structured interview was used to confirm official depression diagnoses, only depressive symptoms should be used in this article.

Discretionary Revisions
Background
In this article cross-sectional results about physical fitness and depressive symptoms were examined. However, the background mainly showed studies about physical activity and depressive symptoms and not about physical fitness while physical fitness is the most important variable in this article. Adding a paragraph with previous literature about physical fitness measurements in relation with depressive symptoms would be needed. There are also articles about depressive symptoms and other physical fitness measurements available such as hand grip strength (for example Rantanen et al. and Van Milligen et al.) which you could mention in this additional paragraph.

In last paragraph of the background the suggestion was generated that physical fitness was an example of physical activity. My suggestion would be to split the information about this variables by adding a paragraph about physical fitness (see earlier comments) and to extend the last paragraph with concluded findings of the literature of both variables and to confirm two aims: 1) To determine the cross-sectional associations between physical activity and depressive symptoms and 2) to determined the cross-sectional associations between physical fitness (both cardiorespiratory and muscular fitness) and depressive symptoms. You could also writing about the differences between a self-reported questionnaire for determine physical activity level and a more objective measurement of the physical fitness measurements.

A beautiful very large cohort was used to answer the aims in this research. My suggestion would be to mention this important strength in last paragraph of the background. Further, it is a great plus point that physical fitness was measured with objective measurements which are in generally more objective than
self-reported questionnaires. My suggestion would be to mention this point in the last paragraph of the background.

Methods

In the study population section a short description of the cohort study was missing, such as description of the recruitment, percentage male/female etc. Further, a follow-up survey was conducted in 1997. Does this survey only included a self-report questionnaire with depressive symptoms and physical activity items or was the follow up more extensively? Could you add some information of the total follow up? 5497 persons completed the fitness test, do you mean both tests (cardiorespiratory and muscular tests)?

In the depressive symptoms section a mean score of 1.55 or 1.75 was used. In my opinion you should add that these points were a cut off point for important clinical depressive symptoms. A official depressive disorder diagnosis could not be reached by this measurement.

In the paragraph Leisure-time physical activity a description of the quintiles was missing. Further, I wondered which questionnaire you have used to measure physical activity, was it a standardized measurement (such as the physical activity questionnaire of Craig et al.)?

In the paragraph Physical fitness has been written that subjects were screened for cardiovascular diseases and orthopaedic problems. However, the results of this screening were not be given. Are there any drop outs due to this screening? Further, you could speculate about the fact whether heart rate is a good indicator of cardiorespiratory fitness. Why don’t you use maximal oxygen consumption (VO2max) which you do describe in this section? VO2max gives a more accurate and complete idea of cardiorespiratory fitness. However, if you choose to use heart rate is indicator of cardiorespiratory fitness, why do you describe maximal oxygen consumption in the methods although you do not use this variable in your analyses?

You do not adjust for somatic diseases since your sample was very young. However, even in a young sample somatic diseases could have large impact at physical and mental functioning. Are the results different when you will adjust all analyses for somatic diseases?

In the Statistical methods section in first sentence ‘prevalence of depression’ have to be replaced by ‘depressive symptoms’ since you do not use official depressive disorder diagnoses.

It would be most clearly when the sequence of paragraphs in the Methods is comparable to the sequence of the variables in table 1. Further, a description is the quintiles of each variable in the methods was missing.

Results

Why were men and women separated in the analyses? May you could add an interaction term to explain this separation?
In second paragraph important outcomes of the prevalence of depressive symptoms were described. However, numbers and important p-values were missing. Further, figures of the data of table 1 may give a better overview of the results than table 1.

In the tables ‘heart rate’ was mentioned although in the text ‘cardiorespiratory fitness’ was used. I would suggest that same indication will be used.

Discussion
The discussion section is not very extended. The author may give more information about the differences between men and women in the association between physical fitness and depressive symptoms. Further, the findings of this study could be compared to earlier research with for example self-reported questionnaires and the author may give their reaction on their earlier determined hypotheses. So, more detailed explanations of the results are needed.

First paragraph gives a good overview of the most important results of this study: poor muscular fitness was associated with more depressive symptoms which is following the author new information about this association. However, I don’t think that these results are new but the results are consistent with previous research of Rantanen et al., Van Milligen et al. and Goodwin et al., who all showed associations between hand grip strength/lung function and depressive symptoms.

Study limitations and strengths
In my opinion, third paragraph should be replaced to the background section because of the global information which was given between physical fitness/activity and depression.

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

Quality of written English: Acceptable

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

Declaration of competing interests:
I declare that I have no competing interests.