Author's response to reviews

Title: Low back pain in military recruits in relation to social background and previous low back pain. A cross-sectional and prospective observational survey.

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Author's response to reviews: see over
Re: Low back pain in military recruits in relation to social background and previous low back pain. 
A cross-sectional and prospective observational survey.

Thank you for an insightful and thorough review of our paper. On basis of the review, we have restructured the paper, and changed part of the analysis-strategy, which has resulted in a clearer paper. We will try to answer your queries below; however, some figures in your letter are missing. Hopefully, we have not missed any of your comments due to a downloading error. The following will address the numbers as they appear in our version of the review.

**Major compulsory revisions:**

3. This is true. We are at no point referring to incidence of LBP. In objectives, “incidence” is changed to “reporting” and in the background section “development” has been changed “occurrence”.

5,7,12. A section has been added to the method-section describing the variables and Table 1 has been changed to only include the responses.

8. We have changed the structure of the article. Both in the method- and the results-sections the cross-sectional and the longitudinal study have been separated. Hopefully, this improves the clarity of the whole paper.

9. Right. We agree that the ROC-approach was not appropriate. Actually, the whole predictive section could be questioned and has been deleted. In stead, some simple consequence calculations have been added, which are of greater value in the “real world”.

13. We do not agree on this point. The comparison with the twins is for external validation and should be presented in the validation-section.

14. Table 3 presents results from the cross-sectional study and Table 4 from the longitudinal study. Differences between the two probably reflect the fact that the effects of background variables differ as the subjects are moved from their usual environment to be exposed to new kind of stressors. In other words, the IQ might be of less importance when hiking with a back pack than in the home environment. Anyway, this should be clearer with the new structure of the manuscript.

15. The analysis of sensitivity and specificity was not correct and has been deleted – with regard to incidence/recurrence: this is implied in the odds ratios – higher OR indicate higher rate of recurrence/continuation as compared to incidence.

16. The results-section has been drastically reduced: only descriptions of the final models are kept in the manuscript.

18. Only the variables demonstrating significant associations with the outcome in the multivariate analyses are included in the final models. Therefore, it is not necessarily the same variables included for all outcome variables. The text-part of the result-section describes this.

19. We have gone through the text carefully with the intent of being as sober as possible.
20. This was a serious mistake and the analyses have been redone, using only one LBP-variable.

21. With an odds ratio of 0.55 (0.33-0.90), we do not think it is “far too bold” to call sedentary occupation a significant protective factor. However, we have changed the wording to “statistically significant”. As for the next two key points: with wordings like “..education may increase the risk...” and “..IQ seems to protect...” we can hardly be more cautious when formulating key points based on our findings. For more detailed information, the article must be read in full.

**Minor essential revisions:**

1. By moving persons away from their usual environment and placing them under different stressors than usually, the effect of the investigated social background variables, such as schooling and parents’ education, are separated from their usual external environment factors, i.e. workload, housing, diet etc. The variables have been better described in the method-section.

2. Indeed, several references could be added. We chose this one because the effect measure is the same, and the population relatively similar to our study population.

4. More information has been added (Methods, subjects, sentence 7-8). However, as described in the discussion, we do not find reason to suspect bias.

6. We actually did test specifically for interactions.

10. Nowhere in the method-section do we refer to the sample as purely male. In the background we wrote “young men” – this has been changed to “young persons (primarily males)” as we do not wish to imply, that the sample is representative of the general population.

11. The low response rate is a great cause of concern. However, as discussed in length in the first paragraph of the discussion, poor design of the questionnaire and probably procedure-failure at regimental level caused most of the selection. This is unfortunate, but does not seem to be biased. See analysis of non-responders.

17. We are not sure about the implication of this remark. The biopsychosocial model, as mentioned in the background-section, attaches some relevance to social factors. This study attempts to estimate the influence of some of these factors. Furthermore, it has previously been shown that LBP predicts future LBP. We therefore try to determine, whether these associations persist, when environment, occupation etc. changes.