Reviewer's report

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

Version: 1 Date: 15 November 2004

Reviewer: Alex Burdorf

Reviewer's report:

General

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Background:
3. The terminology is sometimes unclear, what is meant by development of LBP? Also, the introduction describes LBP prevalence, whereas in the first objective the term incidence is introduced (and given the tables I strongly suspect this to be wrong)

Material and methods
5. More information is needed on the contents of the questions on LBP and recall period, since it is unclear how these questions differed from baseline and follow-up, e.g. > 30 days LBP is something completely different for a one year or a 3 months recall. Also, present descriptions of those variables used in the tables, what is LLBP?, what is leg pain (radiating pain?), what is cut-off point for IQ, etc

7. It is not clear why certain variables were investigated, what is the rationale (why parents’ education and not the soldiers’ education?)

8. The analysis is not clear, differentiate between the cross-sectional and longitudinal analysis, present the endpoints used (developing LBP? is this incidence or merely prevalence (and hence maybe just recurrence)?)

9. I strongly doubt about the ROC approach, given my previous remarks. I would not apply this analysis on complaints characterised by their recurrent nature (and, thus, one does not predict the presence/occurrence of disease based on ‘objective’ measures of health).

Results
12. Table 1 should not include the complete definition of variables, this is something for the methods section.

13. Table 2 should present results from the cross-sectional and follow-up study, the results of the twin study can be presented in the section discussion.

14. I am really puzzled by the differences between tables 3 and 4. Essentially, both analyses are addressing determinants of prevalence of LBP, albeit with a different recall period. Hence, this analysis is in fact a repeated cross-sectional analysis and one should not expect these differences, unless some bias has occurred. Otherwise, I regard the results partly as random fluctuation without too much etiological consequences.

15. I would like to see information on recurrent and incidence in order to be able to interpret some rather peculiar findings (the sensitivity suggests a very low recurrence, which is rather surprising since most studies have shown recurrence rates of over 60% for 1 year LBP)

16. Overall, this section should be reduced strongly with more focus on the essential results.

Discussion
18. One would like to know why factors differed between cross-sectional and 3 months follow-up.
19. Some remarks are well off, such as “immediate protective effect of sitting™, since this study cannot be regarded as an etiological study (given the crudeness of the risk assessment, this could be discussed as well since sedentary occupation is a bad proxy for describing the differences in well-known risk factors such as awkward postures, MMH etc).
20. The remark that LBP does not predict LBP during follow-up seems to be the direct result of the chosen analysis. Problem is that table 5 includes baseline LBP and 1-7 days LBP previous year, how is this possible?

**Key points**

21. The first 3 key points are far too bold statements, given the design and material of the study.

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**Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)**

**Background:**
1. The consideration that many risk factors are interrelated is OK, but this manuscript suffers from exactly the same problem, hence, these remarks should be made in the perspective of the current study. Within this perspective I would expect some considerations (with references) to “early life™ risk factors, which gives the rationale for the selected factors in the analysis.
2. The Danish twin study is of interest to cite, but there are several studies among school children that have shown surprisingly high LBP prevalences.

**Material and methods**
4. Some information on confidentiality and procedures is required since one can easily expect bias both ways.
6. The section analysis starts with tests for interaction (sic !), I expect this to be association rather than interaction (this term has a very specific interpretation).

**Results**
10. Ahh, here we read about 4% females, whereas in the method section only reference was made to male conscripts.
11. The response rates make us wonder about remark 4 above, what are potential selection mechanisms?

**Discussion**
17. Since the study seems to lack a clear underlying model (see remark 1), some results seem rather haphazard and, thus, difficult to interpret.

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**Discretionary Revisions (which the author can choose to ignore)**

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable
Statistical review: No

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