Author's response to reviews

Title: Are lifestyle-factors in adolescence predictors for adult low back pain? A cross-sectional and prospective study of young twins.

Authors:

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Version: 2 Date: 22 November 2005

Author's response to reviews: see over
Response to review by Päivi Leino-Arjas.

Major compulsory revisions:

Clarifying information has been added to tables.

In response to both reviewers comments (in agreement with our own opinion) we have chosen to focus on persistent LBP. This is described in the Method-section (Outcome variable). 'Incidence' relates to those without LBP during the year prior to baseline. This is explained in the Discussion, 6th paragraph.

We did not intend the introduction to lead onto work-related issues. Rather, the data provided were meant to illustrate the extent of the problem and thus the importance of early intervention. The issue of workrelated factors in relation to our cohort has been briefly discussed in the 1st paragraph of the discussion.

Part of the conclusion did not arise directly from our findings. The last two sentences have therefore been moved to the discussion.

Minor essential revisions:

The obscure wording of the objective has been changed both places (thank you!). We are not very pleased with the term 'prevalent LBP’ either, but have not been able to find an appropriate synonym.

The ’validation’ paragraph has been moved to the material-section, since it has all been done in previous studies - not in this. The subtitle has been changed to 'validation and reliability’

We have checked for the existence of interactions between the three predictor variables (smoking, alcohol consumption and overweight) in three different ways, which seems to have confused both reviewers - and are therefore likely to confuse other readers as well! We have deleted the description of the first type of testing (comparing frequencies). Including interaction terms (multiplicative) in the multiple logistic regression analyses is the most correct and the simplest way to take interactions into account. Therefore this has been maintained. It could be argued, that stratification is a sort of interaction-analysis. However, after concluding that there was no statistically significant interactions-terms between the three potential predictor variables, we still wanted to further explore the distributions within different strata of BMI. The reasons for this are described in the method-section. Furthermore, we wanted to investigate the distribution in the various age- and sexgroups. Age and sex are not investigated predictor variables and have not been checked for interactions in any way.

All measures are self-reported. This has been added to the text and discussed in the discussion-section.

As mentioned previously, LBP-year is not part of the main analyses anymore. 'Alcohol’ has been changed to 'alcohol consumption’.

The twin-control analysis was only intended as an attempt to verify findings from the main study. To include categorical data would require a lot more explanations etc. which this manuscript does
not leave room for. Besides, there is insufficient power to obtain any significant findings if the data are categorized. For the information of the reviewer, we have afterwards performed categorical analyses, but did not have enough power to show anything in monozygotic twins. However, using all the twins, discordant for LBP, in a matched case-control design shows a significant dose-response relationship for smoking and also a negative association bt. alcohol consumption and later LBP. This is an issue, we consider to explore further in a future study.

We did not have any reliable data on leisure-time physical activity. This is mentioned in the discussion.

Possible causal pathways and possible origin of LBP are both interesting subjects, that are well worth dealing with. However, if this is to be done properly, it will require much more attention than is possible within the scope of this article. We have chosen to analyse our data and report our findings without dwelling too long on more theoretical issues.

The manuscript has now been reviewed by our language expert.

**Discretionary revisions:**

Title and subtitle have been changed.

Right - time has passed for primary intervention at the time of onset! The wording has been changed.

We agree, that pack-years is generally a better measure of exposure than intensity of current smoking. In this case we decided against it due to the young age of the subjects. The strategy for analysis was decided à priori. We decided on the four categories described and had to stick with them.

We do not wish to exchange the 'dose-response' for the suggested 'exposure-response'. 'Dose-response' indicates that the outcome is related to the dose of the exposure, while as 'exposure-response' could be a response to any type of exposure.

The other reviewer also meant that the manuscript contained too many analyses. Rather than omitting the dichotomous analyses, we omitted the analyses of 'LBP-year', since the interest of brief/transient LBP is limited, as described in the Methods-section (outcome variable).

The tables have been changed as requested.

The verbs in the results-section have been phrased in the past tense throughout.

The reviewer seems to think of the recall-period as 8 eight years (which is the follow-up period). This is not the case. The recall period is the one year prior to baseline. If they did not report any backpain during the year prior to baseline, but reported LBP at follow-up, they were categorized as incidence-cases. This has been better explained in the results-section (last part of the 6th paragraph).

We don’t want to carry the discussion further than the results can support. With a CI of 0.30-2.60, we cannot say, the relationship has been reversed, but rather that there is no significant relationship.
With regard to the association between alcohol consumption in 1994 and LBP in 2002, there is a negative relationship in all the analyses. A short discussion of this has been added (Discussion, 1st paragraph).
Response to review by Jens Ivar Brox

Major compulsory revisions:

We agree that long-lasting LBP is the most relevant outcome variable and have changed the manuscript accordingly.

We have tried to shorten the abstract and have also included the size of the cohort.

In the Material-section (variables) it is now mentioned that the data are self-reported and this has been discussed in the Discussion, 2nd paragraph.

The method used to obtain zygosity has been described in the Material-section (study subjects).

The transformation from continuous to categorical data has been described in the Method-section (Predictor variables).

We did not intend to present the risk factors as purely biological. We have included a little about the importance of psycho-social factors in the discussion (2nd paragraph).

The study by Rasmussen (I presume the reviewer refer to: Rasmussen C. Lumbar disk prolapse. Alcohol, tobacco and prognosis. Ugeskr Laeger. 1998;160(30):5189-92) is related to outcome after lumbar disc surgery rather than LBP and found intake of wine to be associated with a good prognosis - however no associations with other types of alcoholic beverages. Thus, the two studies can hardly be compared. There is one other study, though, demonstrating similar results to ours for developing chronic LBP in primary care (Thomas et al. BMJ 1999;318:1662-7), whereas a large study of adults found no associations between alcohol consumption and risk of early disability pension due to LBP (Hagen et al. Spine 2002;27:1790-1796). However, we agree with the reviewer that this finding should be discussed, and this has been addressed briefly in the first paragraph of the discussion.

Minor essential revisions:

The numbers of returned questionnaires have been included (Results, 1st sentence).

We have checked for the existence of interactions between the three predictor variables in three different ways, which seems to have confused both reviewers - and are therefore likely to confuse other readers as well! We have deleted the description of the first type of testing (comparing frequencies). Including interaction terms (multiplicative) in the multiple regression analyses is the most correct and the simplest way to take interactions into account. Therefore this has been maintained. It could be argued, that stratification is a sort of interaction-analysis. However, after concluding that there was no statistically significant interactions-terms between the three potential predictor variables, we still wanted to further explore the distributions within different strata of BMI. The reasons for this are described in the method-section. Furthermore, we wanted to investigate the distribution in the various age- and sexgroups. Age and sex are not investigated predictor variables and have not been checked for interactions in any way. Testing the stratified data by means of the Mantel-Haenszel method does not add information to what has been obtained through the multiple logistion regression analyses.
The printing/writing errors have been corrected and Table 1 changed as requested.

**Discretionary revisions:**

The last two sentences of the conclusion have been moved to Discussion.

Categories for smoking and alcohol have been added to Table 2.

The manuscript has now been reviewed by our language expert.