Author's response to reviews

Title: Back and neck pain are related to mental health problems in adolescence

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Author's response to reviews: see over
Editorial Requests:

1) Please can you document the specific name of the ethical committee that approved your study.
   - We have edited the Methods to provide the specific name as requested.

Referee 1 Reviewer’s report
Title: Back and neck pain are related to mental health problems in adolescence
Version: 1 Date: 30 December 2010
Reviewer: Lars Lien
Reviewer’s report:
Major Compulsory Revisions
1. The article is of limited interest since there have been published hundreds of papers on the cross-sectional relation between pain and mental health problems and I just cannot see that this article really adds any new dimensions other than to utilize the CBCL questionnaire.
   - We are disappointed we were not able to communicate the contribution this paper makes clearly enough for this reviewer. In the Abstract and Discussion we have made it more explicit that the study is on adolescents, which together with the evidence already discussed in the Background and Discussion will hopefully make the contribution clearer.

2. In the aim it states that neck pain and mental health has been neglected in the literature. This is wrong (just from Norway; see literature by Lien, Bruusgaard and Haugland) and it is also wrong to isolate neck pain as most pain is widespread (see: Natvig B, Ihlebæk C, Grotle M, Brage S, Bruusgaard D. Neck pain is often a part of widespread pain and is associated with reduced functioning. Spine. 2010 Nov 1;35(23)).
   - The reviewer raises two linked points here: the relative neglect of research on adolescent neck pain and mental health and the validity of localised pain.
• We agree there has been extensive research on neck pain in adult populations. There has also been a reasonable amount of research on somatic complaints and psychological functioning in children and adolescents — including by the reviewer (Lien et al. 2005). The limitation of some of the somatic complaints literature is that it assumes there is only a shared mechanism for pain at various sites (and thus only widespread pain should be evaluated — see second point below). For example the Lien et al. paper analyses on number of sites. Whilst this is useful, it may mask important regional differences in pain. For example females usually have a higher prevalence of neck pain than males but back pain prevalence rates are more similar, suggesting pain mechanisms are not totally shared. We therefore consider it valuable to explore relationships with pain in specific sites. There is also limited research on neck pain in adolescents and even less on “the relationship between adolescent neck pain and psychological functioning”. The potential importance of sample age is exemplified in the point below. We have edited the manuscript to highlight adolescence more and that the contrast in research volume was between cervical and lumbar spinal regions.

• We are very cognisant of the issues related to widespread pain, comorbidity of pain, localised pain and sub-groups of pain. However we disagree that ‘most pain is widespread’. We note in the cited study by the reviewer’s research colleagues (Natvig et al. 2010) they found neck pain in adults was most commonly regional or widespread. However this was not the case with the adolescents in the Raine study. Whilst we did not categorise pain states in the same way as Natvig et al, Table 1 reported the percentage of adolescents reporting comorbid neck and back pain was less than the percentage reporting neck but not back pain. The paper did not report on widespread pain percentages, but we have another paper currently in review elsewhere examining chronic widespread pain and the percentage of adolescents reporting this was only 4%. Potential reasons for the substantially different percentages are multitude, but the age of the cohorts may be important. In other studies we have found it is not appropriate to assume that what happens in adulthood is the case in adolescence in regard to spinal pain (Aslfalck et al. 2010). However age is not the only reason as the Lien et al. (2005) study also reported a much higher prevalence rate of bodily pain in 3-5 areas (21-35%). We look forward to the ongoing discovery of knowledge in this area.


3. In the methods section there are no information about procedures for sampling and on how many non participants there were and their characteristics. There is no sex or age distribution presented. The word follow up is mentioned, but no statement that this is a cross-sectional study.
• We have provided more detail on the initial recruitment into the Raine study, the representativeness of the cohort at 14, and the number not participating in the Methods as suggested.
• The age distribution was reported in the Methods under Participants
• The gender breakdown was reported in Table 1.
• Whilst the cross-sectional nature of the study was previously mentioned, it was only in the Discussion so we have added this detail to the Abstract and Method.

4. One of the really major problems is the lack of possible confounding factors like socio-demographic and family economy. It is also a problem that the authors do not discuss the direction of causation especially in a possible prospective study.

• The reviewer raises two concerns: confounding factors and direction of causation.
• We agree that there are many possible confounders for the relationship between neck pain and/or back pain and mental health. In the Discussion we outlined a number of physical, pathophysiological and psychosocial factors which may be important to this relationship. However we chose not to include a group of multi-dimensional covariates in our analysis, as we will be exploring their role in the prospective analyses to follow. This will enable the exploration of their influence on pathways to spinal pain and psychological problems. We have clarified this in the final paragraph of the Discussion.
• We direct the reviewer to paragraphs 7, 8 and 9 of the Discussion where we discuss the direction of causality in terms of a potential reciprocal nature of the relationship between spinal pain and psychological factors and to the final paragraph in the Discussion were we explicitly acknowledge the limitation of this cross-sectional study and the need for longitudinal exploration. In response to the reviewer’s concern we have edited these sections to make it even more explicit.

5. It is also wrong spell out in the discussion that there are interesting gender differences. This has been found in several other studies. The same applies to internalizing and externalizing symptoms. There is a lot of boasting in the article using words like first large-scale, first to report etc. Some humbleness would have benefited the article.

• We are unable to identify anywhere in the Discussion where we stated that the gender differences were ‘interesting’, although we think that they are. We think the reviewer means that we are wrongly claiming they are novel. What we actually said about spinal pain prevalence gender differences was “This finding is consistent with other studies that have reported an increased prevalence of pain in general amongst females (10, 18). However, our finding of a gender difference in the prevalence of musculoskeletal pain is in contrast to some other studies (19, 20).” In terms of the relationship between spinal pain and internalising and externalising symptoms we stated “Importantly, our findings also support and extend those of Watson and colleagues (24) who found that the experience of low back pain was strongly related to a variety of emotional and behavioural problems in a similar sized cohort of 11-14
Thus believe we have appropriately indicated how the current findings build on the extant body of knowledge.

We are also saddened with the reviewer’s comment that we are not humble but rather boastful. We tried to explicitly and accurately state how our study contributed to the body of knowledge. What we actually said was “To our best knowledge this is the first study to report this finding in adolescents and there are a number of possible explanations.” and “This is the first large-scale study to show that the experience of mental health problems in adolescence is associated with increased risk for comorbid back and neck pain.” We are sorry if this was interpreted as boastful as it was not our intention.

Minor Essential Revisions

Discretionary Revisions

Level of interest: An article of limited interest
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'

Referee 2 Reviewer’s report
Title: Back and neck pain are related to mental health problems in adolescence
Version: 1 Date: 31 January 2011
Reviewer: Ola O Leijon
Reviewer's report:
Thank you for a very interesting and important study. I really enjoyed reading the clear and well-written manuscript, and I am very impressed by your study and work. However, I have some minor comment on the manuscript.

- We thank the reviewer for their kind comments.

Major Compulsory Revisions
1. None

Minor Essential Revisions
2. In the background (1st para) the authors write that the point-prevalence of mental health disorders in Australia and the US is 11-12 percent. Is it possible in this study to estimate the point-prevalence of mental health problems in this study population? Similar? Different?

- We have calculated the point prevalence as requested and added this to the first paragraph of the Discussion.

3. Throughout the manuscript the words gender and sex are used as synonyms. I do not agree that they are, and would prefer the the word sex. The study do not discuss gender differnces but sex differences.

- We agree that technically ‘sex’ is the correct term and have changed this throughout the manuscript. We used ‘gender’ to avoid the ambiguity of the widely used meaning of ‘sex’ (sexual intercourse).
Discretionary Revisions
4. heading of table 2: I suggest ... Multinominal Odds ratios (MOR) are presented...

• Thank you for picking up this error – we have amended as suggested.

Best Wishes,
Ola Leijon

Level of interest: An article of outstanding merit and interest in its field
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.