Author's response to reviews

Title: The influence of self-reported leisure time physical activity and the body mass index on recovery from persistent back pain among men and women: A population-based cohort study

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Author's response to reviews: see over
Dear Editor-in-Chief,

Thank you for the revision on our manuscript “The influence of self-reported leisure time physical activity and the body mass index on recovery from persistent back pain among men and women: A population-based cohort study.” by Tony Bohman, Lars Alfredsson, Johan Hallqvist, Eva Vingård and Eva Skillgate.

The reviewer’s comments have been very helpful to revise and improve the manuscript. We have considered their comments and hopefully made necessary changes and clarifications. All changes, except for changed estimates in the tables, are highlighted with red font in the revised manuscript.

Reviewer: Zumin Shi

Based on the Stockholm Public Health Cohort, the authors assessed the association between leisure time physical activity and BMI and recovery from persistent back pain. The study found that regular leisure time physical activity improves recovery from persistent back pain in women not in men. The strength of the study is its large sample size and detailed information on potential confounders. The statistical analysis strategy is appropriate. The presentation of the results is good.

Major concern

1. There is no mention of health service use. The association between leisure time physical activity and recovery from back pain among women could be confounded by medication or health service use.
Response: Thank you for the comment. This is an important issue and would have been very interesting to study. Unfortunately we do not have information on health service.

If health care positively affects recovery and women that are physically active use more health care than sedentary women our result may be overestimated. On the contrary, if sedentary women use more health care than physically active women this may underestimate our results. We believe the latter scenario is the most probable.

The gender difference of the association may also be due to the difference in health service use.

Response: To explain the sex difference in our result, first of all, health care utilization must positively affect recovery from back pain. Secondly, health care utilization must have different effect on men and women and/or be used in different amounts between the sexes. We believe that this possible bias is unlikely to explain the differences found.

Moreover, we have stratified our analyses by self-rated health (additional analyses). Maybe persons with poor self-rated health seek health care more often than persons with good self-rated health? Then, if health care positively affects recovery, there should be differences in the results between the groups. As there are no major differences, neither among women nor among men, this further supports our beliefs that health care utilization is unlikely to explain our results.

To address the issue we have added a comment on the lack of information about health service and a possible bias due to confounding from differences in health care utilization in the manuscript (page 12, line 5-6).
Minor

1. Change in lifestyle factors may contribute to the recovery from back pain. The finding will be robust if the authors can address this issue.

Response: In studies with a long term follow-up there is always a possibility that studied baseline variables, e.g. life style, change over time. To study this you need repeated measures which we did not have. Nevertheless, given physical activity positively affects the prognosis of back pain, changes of physical activity between baseline and follow up would dilute our results, why we believe that our conclusion for women is robust.

The questions regarding physical activity 2002 and 2007 are different why we could not check if participants changed their activity though we have checked smoking habits. 89% of non-smokers 2002 still were non-smokers 2007 and 86% of the smokers 2002 still were smokers 2007, no matter if the participants were sedentary or physically active 2002. In summary, our belief is that possible changes in life style did not exaggerate our findings.

Moreover, Dr. Shi’s comment made us aware of that if men tend to misclassify their physical activity non-differentially or if men change their physical activity more than women, this could partly explain the different findings between men and women. We have added a comment on this in the discussion section of the manuscript (page 12, line 13-14).

2. As more than 20% of the participants recovered from back pain, odds ratio will overestimate the risk. I would suggest use other measurements, e.g. incident rate ratio by Poisson regression.

Response: Thank you for the advice. We agree and choosing between Poisson regression and Log-binomial regression we decided to use Log-binomial regression when re-analysing the data [1, 2]. As a consequence all estimates in the abstract and result section are changed
in the revised manuscript. The results of the new analyses did not change the conclusion of the study.

Reviewer: Wei Bao

In a Swedish population-based cohort, Bohman and colleagues evaluated the associations of recovery from persistent back pain (PBP) with leisure time physical activity and body mass index, with an emphasis on the sex difference. The study in general was well designed, but several major points should be addressed.

1. As the recovery from PBP is a common outcome (> 10%) in this study, the use of odds ratio estimated by logistic regression is misleading. The pitfalls and alternative approaches have been discussed in previous publications, such as McNutt et al. Am J Epidemiol. 2003;157(10):940-3; or Knol et al. CMAJ. 2012;184(8):895-9.

Response: We have considered the advice and re-analysed the data using Log-binomial regression to be able to present the result as Risk Ratios. As the readers may find Risk Ratio a more intuitive estimate than Odds Ratios this has improved our manuscript. As a consequence all estimates in the abstract and result section are changed in the revised manuscript. The results of the new analyses did not change the conclusion of the study.

2. PBP could be a primary condition or a secondary complication as a result of other diseases. Is the information about some relevant diseases or illness (e.g. rheumatoid arthritis) collected in this study? If so, a sensitivity analysis after excluding participants with these known diseases or illness will provide better understanding on the underlying mechanism.

Response: We agree. Unfortunately there was no information about relevant diseases or illness in the data but we believe that self-rated health may incorporate influence from such
problems. If that is true, the analysis stratified by self-rated health as presented in the
manuscript (page 9 and table 4), provides some information on this issue. Our conclusion
seems to be confirmed when excluding persons with poor self-rated health.

In addition, the sex difference of the association between physical activity and recovery from
PBP may at least partly reflect the benefits of physical activity on the PBP-related underlying
diseases or illnesses that are more common among women.

Response: We fully agree and have discussed this issue in the former manuscript. Please see
page 11, line 7-9 in the revised manuscript.

3. It seems that the definition of recovery from PBP in this study is different from other
studies. The authors need justification for this discrepancy.

Response: One main problem with studies concerning back pain, recognised by several
authors, is the lack of consensus for the definition of back pain episodes as well as definition
for recovery from back pain [3-5]. In 82 studies included in a systematic review of definitions
for recovery from back pain Kamper et al. found 66 different definitions used. In a 2011
Delphi study aiming to standardize the definition of recurrent back pain the included
definition for recovery was “At least 30 days pain-free”, a definition also suggested by de Vet
in 2002 [6, 7]. The definition did not include activity limitations. To our knowledge this is the
closed a consensus for the definition of recovery from PBP that there is today.

Our recovery outcome (no periods of disturbing back pain periods longer than 7 days during
the latest 5 years) was a combination of the only two questions regarding back pain in the
follow up survey in 2007. This definition has several advantages; 1.It incorporates not only
pain but also disability which is shown to be important when persons with back pain define
recovery [3], 2. It covers free of “sub-acute” as well as “chronic back pain”, definitions used
by other authors. 3. It also account for the fact that back pain is a recurrent disorder and being free of disabling pain for 5 years is a very stringent definition. 4. Furthermore if factors studied as e.g physical activity, affects recovery defined in this stringent way they have the potential to have even greater influence on recovery defined according to de Vet. We believe using our stringent definition of recovery supports the importance of physical activity as a prognostic factor something we have discussed in the manuscript (page 11, line 1-2).

The five year recall could be a limitation as we have discussed this in the manuscript (page 12, second last paragraph).

In addition, a sensitivity analysis using the definition used in previous study is necessary.

Response: As mentioned above, we have only one possible definition for recovery from PBP, why we have no possibility to perform a sensitivity analysis using other definitions.

Additional editorial requirement:

“Please include the aims of your study in the Background section of the Abstract”

Response: The requested change is done.

Thank you for considering our revised manuscript for publication in BMC Public Health.

Sincerely

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