PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Muscle Strength in Adolescent Men and Future Musculoskeletal Pain: A Cohort Study with 17 Years of Follow-up
AUTHORS	Timpka, Simon; Petersson, Ingemar; Zhou, Caddie; Englund, Martin

VERSION 1 - REVIEW

REVIEWER	Urho Kujala, MD, PhD Professor of Sports & Exercise Medicine Department of Health Sciences University of Jyväskylä Finland No conflicts of interest.
REVIEW RETURNED	06-Feb-2013

GENERAL COMMENTS	The focus/design of this study includes novelty. Although the study includes some limitations related to its retrospective nature etc., this reviewer's opinion is that the results are important and the limitations have been properly discussed. Also the statistics is appropriate although there may be other possible alternatives/additional ways to analyze the data, such as using strength values per body weight as a predictor. The paper is mostly well written except for some very minor technical problems such as 'two main two main' in the beginning of Methods.

REVIEWER	HSJ Picavet senior researcher Centre for Nutrition, Prevention and Health Services National Institute of Public Health and the Environment The Netherlands
REVIEW RETURNED	19-Feb-2013

THE STUDY	Though the research question is clear, it is not clear why it is interesting or relevant
RESULTS & CONCLUSIONS	The given explanation for the unexpected finding is probably just one of many explanations.
GENERAL COMMENTS	The study presents a nice example of using existing data by linking health survey data to data from the physical examination for military service. My major comment is that the apriori hypothesis and the arguments for studying this are not very strong. In general, the relation between muscle strength and musculoskeletal pain is not strong, although there is also much unclear and more studies are necessary. With the data available the associations may be explored but the current hypothesis is weak, so the unexpected results are difficult to

	interpret and the major conclusion, with just one possible explanation, is highly speculative. In addition, it is not clear what it would tell us, when there was an association according to the hypothesis. In my opinion the current paper is not complete convincing and balanced, though they have potentially very interesting data. Other comments: - Why are those ,17 yr and >19 yr excluded? - To correct for possible differences in testing of the different periods an adjustment was made for a cohort effect. How large were these cohort effects? Probably there were some, but it is unclear that it may be due to testing instructions. They even may be due to real cohort effects, because there are also cohort effects in height, weight (bot affecting strength!) and probably smoking. - Why were 11% with musculoskeletal diagnosis at baseline excluded. This is a very large number! - Why was age at follow-up (or years of follow-up) not part of the analyses? - What were the results of the separate strength tests? - Grip strength: which arm was measured? - The given explanation for the unexpected finding is probably just one of many explanations. - In the section of 'statistical analyses' complete new measurements are introduced; they should be described in earlier sections or be left out.

VERSION 1 – AUTHOR RESPONSE

Professor Urho Kujala
Comment 1
'two main two main' in the beginning of Methods.
Measure 1
Corrected, we have also edited a few other minor typos.
Dr HSJ Picavet
Comment 2
"Why are those ,17 yr and >19 yr excluded?"
Measure 2
We have added information regarding the typical age of conscripts in the method section (please see page 6).
The conscription testing is in general performed at the age of 18. Those who are younger than 17 or older than 19 years are likely different from the rest of the cohort. Furthermore, adolescence is defined as ages 10-19 by WHO.
Comment 3
"To correct for possible differences in testing of the different periods an adjustment was made for a cohort effect. How large were these cohort effects? Probably there were some, but it is unclear that it may be due to testing instructions. They even may be due to real cohort effects, because there are also cohort effects in height, weight (both affecting strength!) and probably smoking."
Measure 3
We have made adjustments to improve the clarity of the method section in the manuscript.
In absolute terms, the conscripts seem to get stronger in the late 70s. Due to this sudden change, this is probably not due to a specific cohort effect but the change of testing instruments. However, by dividing the cohort into subgroups, we not only take change of testing machinery into account but also potential cohort effects, as the strength of all men is compared to the strength of men conscripted at about the same time period.

Comment 4
"Why were 11% with musculoskeletal diagnosis at baseline excluded. This is a very large number!"
Measure 4
Our intention was to create a cohort free of musculoskeletal disease at baseline. A musculoskeletal disorder might potentially influence baseline testing and also be a risk factor for future disease.
Comment 5
"Why was age at follow-up (or years of follow-up) not part of the analyses?"
Measure 5
As the time to follow-up should not be associated with muscle strength test result, we do not regard age at follow-up as a confounder. The mean time to follow-up is also very similar in the three strength groups. (Low: 17.05 years, Average: 17.26 years, High 17.36 years.). Furthermore, when we performed a sensitivity analysis and included the variable in the model, the results remained essentially the same.
Comment 6
"What were the results of the separate strength tests?"
Measure 5
The result of the muscle strength tests have been added to Table 1 (page 20).
Comment 6
"Grip strength: which arm was measured?"
Measure 6
In general, the conscripts had their preferred arm tested. Information added to the method section (page 7)
Comment 7
"The given explanation for the unexpected finding is probably just one of many explanations."
Measure 7
We agree and we have made some minor corrections to make the text more balanced. For example, please see page 12 as well as the separate "Key messages".
Specific comment 8
"In the section of 'statistical analyses' complete new measurements are introduced; they should be described in earlier sections or be left out."
Measure 8
We have now added "Sensitivity analyses" as a separate heading.
Other minor changes to the manuscript
Throughout manuscript: 3 subjects had erroneously been included in the study cohort, although they were not included in the regression models. This has been corrected throughout the manuscript text, tables, and figures.
Page 8: We have corrected a typo and now describe the participation rate instead of drop-outs.
Figure 1: Please, see first measure above.
Table 1: Several errors of rounding off have been corrected.

VERSION 2 – REVIEW

REVIEWER	HSJ Picavet, PhD Centre for Nutrition, Prevention and Health Services National Institute of Public Health and the Environment
REVIEW RETURNED	25-Mar-2013

THE STUDY	In the revised version of the paper no attention has been paid to my main comment, i.e.: My major comment is that the (apriori) hypothesis and the arguments for studying this hypothesis are not very strong. In general, the relation between muscle strength and musculoskeletal pain is not strong, although there is also much unclear, and more

	studies are necessary. With the data available the associations may be explored but the current hypothesis is weak, so the unexpected results are difficult to interpret and the major conclusion, with just one possible explanation, is highly speculative. In addition, it is not clear what it would tell us, when there was an association according to the hypothesis. In my opinion the current paper is not complete convincing and balanced, though they have potentially very interesting data.

VERSION 2 – AUTHOR RESPONSE

In the revised version of the paper no attention has been paid to my main comment.

Authors' response: We regret that the reviewer got such impression after reading the revised version of our manuscript. As the editorial decision was a minor revision, our intention was to keep the changes in the manuscript to a minimum without neglecting any reviewer comments. Furthermore, we misinterpreted the first and major comment and regarded it to be on a meta level (i.e. a general comment about the area of study and a foundation for the more specific comments). Therefore, we did unfortunately not address it properly as a comment on the manuscript as such. However, we do particularly acknowledge that further clarification of the background to the study is possible. Please see our detailed response below and also the marked-up changes we have made to improve the clarity of the manuscript.

My major comment is that the (apriori) hypothesis and the arguments for studying this hypothesis are not very strong.

Authors' response: We agree that the prior evidence for our hypothesis is limited, this is also part of the rationale for the study. However, it is important to point out that there are previous studies in the area that has investigated similar research questions and also a reasonably sound theoretical model for a possible association. Furthermore, there is support that establishing a healthy life style, strong body (and muscles etc) in youth might have long-lasting or carry-over effects in adult life (also including cross sectional work by the reviewer).1 However, there is paucity in the literature of the potential mechanisms as it is challenging to study these associations over several decades. We suggest that the study fills a current gap of knowledge.

Authors' action: We have revised parts of the introduction to better clarify the rationale for the study (p. 5):
"As physical work load is a risk factor for many MSDs,16 a model in which the muscle strength in the loaded parts of the body are protective for future disorders is appealing. This is also the main rationale of studies in the area; does general or demarcated muscle strength have a protective effect on future complaints in the adjacent structures? It is also known that physical exercise with focus on muscle strength is an important secondary and tertiary prevention of MSDs.17 18 A handful of studies have hitherto investigated the strength of isolated muscle groups as a determinant of later MSDs in adulthood.19-22 However, there is conflicting evidence of the value of muscle strength as a protective factor of musculoskeletal pain, such as neck/shoulder pain and low back pain in adult subjects.19 When considering muscle strength in youth as a potential risk factor in the longer perspective, its association with later disease of any kind is relatively unknown, including musculoskeletal pain.23 At least two studies have investigated the result of single muscle strength tests as determinants of musculoskeletal complaints decades later.24 25 The first study, using number of sit-ups during 30 seconds as a strength measure, found no association with later low back pain or tension neck in men. In women however, the high strength group had a decreased odds ratio (OR) of tension neck.24 The second study found a decreased OR for MSDs in men who either had a strong performance in isotonic bench press or in a isometric two hand lift test.25 Neither of the two studies includes a

measure of overall muscular capacity. Hence, although there is some evidence of an association between low muscle strength in youth and later risk of MSDs, the association between overall muscle strength in adolescence and later musculoskeletal pain has never been studied. Furthermore, with a larger sample size, data on common risk factors, testing of three different muscle groups, and data on physical work capacity, we also address some of the limitations of earlier studies."

In general, the relation between muscle strength and musculoskeletal pain is not strong, although there is also much unclear, and more studies are necessary. With the data available the associations may be explored but the current hypothesis is weak, so the unexpected results are difficult to interpret and the major conclusion, with just one possible explanation, is highly speculative.

Authors' response: We agree that more studies are needed in this field. This is also the reason why we think that this kind of cohort study adds to the current literature. Importantly, the overall conclusion of our study is that adolescent men with low strength seem not to have an increased risk. As we do not have access to data to further explain why so, we can only speculate of its causes in the discussion (and we also think that a reader would have some interest in us doing so). We offer two possible explanations; one primary biological and one primary social. Due to the structure and coherence of the manuscript, we refrain from giving more than these two examples. It was by no means our intention to give the impression that the two mentioned examples are the only possible (or plausible) explanations.

Authors' action: We have revised the text to limit our speculations. Instead, we suggest possible research questions for future investigations in the field (p. 14):
"In future studies, it would be of interest to investigate if low muscle strength serves as a deselection criterion for professions or types of leisure time physical activity with higher risk of acute injuries or chronic physical overload, factors with negative impact on musculoskeletal health."

Also, on page 13 regarding possible causes:
"Primarily, we do not suggest that low muscle strength in youth is a protective factor for later musculoskeletal pain. However, our observations could potentially be explained by both social and biological factors. First, as former occupational exposure16 and certain sport participation37 are established risk factors for future MSDs, it lends some evidence for a general model in which certain forms of physical activity is negative for the musculoskeletal health. It has also previously been suggested that there is a U-shaped association between physical activity and later back pain,38 39 i.e that subjects with low and high levels of physical activity has an increased risk compared to the group with average activity. First, we speculate that our results may be explained by muscle strength in youth being one selection criterion for future high risk activities with a negative influence on the musculoskeletal health, e.g. higher risk of joint injury due to sports participation or manual repetitive work load. This would also include more immediate exposure in youth such as more physically demanding military service.40 Although we have controlled for level of education, which we regard as a proxy for occupational exposure, there is potential for residual confounding. In other words, individuals with low general muscle strength might to a certain degree be deselected for high risk activities compared to stronger men.. A second potential explanation for our observation can be based on that, the strength of an individual is associated with the muscle fiber type distribution, which have a large genetic component.41 Whereas type I fibers are more common in endurance athletes42, a high type II percentage have been reported to be associated with both isometric muscle strength43 as well as low back pain.44"

The last section of the manuscript now reads (p. 15):
"In conclusion, we observed no increased risk of self-reported musculoskeletal pain in adult men with low overall isometric muscle strength in youth. Thus, this study add no support to a model in which muscle strength is a risk factor for future musculoskeletal pain in men."

In addition, it is not clear what it would tell us, when there was an association according to the hypothesis. In my opinion the current paper is not complete convincing and balanced, though they have potentially very interesting data.

Authors’ response: If the hypothesis was verified it would have provided evidence in support of favorable long term effects of high muscle strength on musculoskeletal health e.g., potentially by physical education in school and be a yet another incentive to establish an active lifestyle in adolescence. We do however take into consideration the valuable input and have also made the additional changes to the manuscript as described below to make it more appealing. For more minor changes, please see the revised text.

Authors' action:
1. In "Key Messages" (p. 4), we have removed the sentence in which we speculate of the causes of the observed decreased risk in men with low muscle strength. Instead we have added the following sentence:

"The study does not provide evidence in support of a theoretical model in which low muscle strength in young men is associated with an increased risk of musculoskeletal pain later in life."

2. Regarding the main result of the study (p. 14):

"It is important to note that our main result is not the significantly decreased risk of later musculoskeletal pain observed in men with low strength but the nonexistent risk increase in the same group."

References
1. Heneweer H, Picavet HS, Staes F, Kiers H, Vanhees L. Physical fitness, rather than self-reported physical activities, is more strongly associated with low back pain: Evidence from a working population. Eur Spine J. 2012;21:1265-1272