

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Risk factors of non-specific neck pain and low back pain in computer-using office workers in China: a cross-sectional study
AUTHORS	Ye, Sunyue; Jing, Qinglei; Wei, Chen; Lu, Jie

VERSION 1 - REVIEW

REVIEWER	Prawit Janwantanakul Department of Physical Therapy Faculty of Allied Health Sciences Chulalongkorn University Bangkok, Thailand
REVIEW RETURNED	11-Nov-2016

GENERAL COMMENTS	<p>General comments =====</p> <p>This cross-sectional study aimed to explore the associations of occupational risk factors in Chinese computer users. The main limitation of the study is the use of cross-section design. It is not possible to establish a causal relationship between exposure and outcome. Considering that several previous studies were conducted in this topic, the contribution of the present study to the current body of knowledge is limited. Methodology is also unclear and need more details.</p> <p>Specific comments =====</p> <p>1. INTRODUCTION</p> <ul style="list-style-type: none">• The content in this section should be limited to literature regarding NP and LBP in office workers or computer users as much as possible.• More previous studies of NP and LBP in office workers or computer users should be included.• Considering that several previous studies were conducted in this topic, please clarify the statement that “the current literature on modifiable determinants of NP/LBP among workers in modern workplace environments, where intensive computer use is common, is insufficient”.• The authors should be consistent with the writing format, either NP and LBP or LBP and NP. <p>2. METHODS</p> <ul style="list-style-type: none">• Inclusion and exclusion criteria of participants should be added.
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	<ul style="list-style-type: none"> • Page 5, Line 34: please clarify “a validated questionnaire”. • How was occupational risk factors collected, either subjectively or objectively? • Please provide justification for using NPQ or ODI, which are tools to assess disability related to NP and LBP, to identify those with non-specific NP or LBP? • How were subjects with missing data managed in the sensitivity analysis? <p>3. RESULTS</p> <ul style="list-style-type: none"> • Page 6, Line 28: 12-month prevalence? • It is unclear why participants were categorized into groups, according to their NPQ or ODI. <p>4. DISCUSSION</p> <ul style="list-style-type: none"> • Page 8, Line 18-25: Please explain how the authors reached this conclusion. • Page 8, Line 34: the definition of intensive computer users is needed. Were intensive computer users a target population of the present study? • Page 9, Line 28-30: I am confused by this statement “individual factors, including injuries of the neck/low back, married individuals, and female sex, were associated with non-specific NP/LBP, because, in the first paragraph of the discussion section, the report of findings is different.
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REVIEWER	Morten Wærsted National Institute of Occupational Health, Oslo, Norway
REVIEW RETURNED	21-Nov-2016

GENERAL COMMENTS	<p>This cross-sectional study gives data on risk factors and complaints in computer workers in Zhejiang, China, and thus supplements the knowledge on this subject from other parts of the world. The messages of the authors is clearly stated. The English language is relatively good, when considering that the authors do not have English as their main tongue. Some points for improvement</p> <ol style="list-style-type: none"> 1. Do not repeat in text results that are given in the Tables, but refer instead to the Table in question. The figures copied in the text are given with bold figures in the Table and is easy to find. With this in mind, the Results section can be much shorter. 2. The authors use the wording “.... limited studies have indicated", both in the Introduction (second paragraph) and the Discussion (second paragraph). I suppose that the studies in question are not limited in themselves, but that there is a limited number of studies. Please rephrase. 3. The wording “This result might have crucial implications and provide a direction of practice for future workstation designs” (Discussion, end of second paragraph) sounds a bit too dramatic to me. I suggest that the authors simply write, “This result might provide a direction of practice for future workstation designs”. 4. I find the reference to a hypothesis on bone mineral density in the
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	<p>Discussion rather speculative and not needed in this context. I advise the authors to delete the sentence "This might be due to their lower bone mineral density and specific anatomical structure".</p> <p>5. It is OK with several figures after the decimal point in the Tables. However, it gives a too precise impression to have this in the Abstract. I suggest that 2.6, 2.9, 3.2, 5.3 and 2.7 is used for the OR referred in the Abstract, instead of 2.590, 2.939,</p>
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REVIEWER	Martyn Lewis Keele University, UK
REVIEW RETURNED	06-Jan-2017

GENERAL COMMENTS	<p>This is an interesting study that highlights some important workplace associations with neck and/or low back pain among computer-workers. The paper is quite well written and the detail and evaluation methods reasonably clear and concise. The discussion does cover the findings – though some important parts of the results are missed out (these can be easily incorporated). On the whole this would seem to be a reasonably valuable addition to the literature.</p> <p>More clarity required on the basis of the sample size – I think this was probably a fixed sample, in which case what justification would there have been in regards to power?</p> <p>On page 6 it was not too clear as to whether stratification by gender was carried out because of significant interactions observed in this study or whether there was apriori or external evidence to suggest different predictors of effect across the genders.</p> <p>On page 6 also, the authors mention that sensitivity analyses were carried out including subjects with missing values – how was this done?</p> <p>Some of the predictor variables are not very clearly defined e.g. work\geq5 years (does this mean work generally or work in the current job (with computers)), is previous neck injury general or related to injury at work?</p> <p>By tri-chotomising the outcome data there is a certain loss of information and power in testing. I would recommend carrying out as an additional (possibly sensitivity analysis of the main unstratified data at least) linear/polynomial regression on raw NP/ODI scores to check for linear/growth association with predictors (which is what I think is relevant here).</p> <p>The presentation of Table 1 is congested – I think 1 decimal place would suffice for the descriptive data. Similarly 2 decimal places may suffice for the ORs in Table 2.</p> <p>Do the statistical tests test for trend: trend-testing is not specified but perhaps may be preferable (in respect of Table 1)</p> <p>Prevalence of 'pain' is high – this is because the definition encompasses any level of pain/discomfort/inability according to the validated measures of the NPQ and ODI. I do not believe this is a common way of expressing 'prevalence of pain' (i.e. is not a well-used clinical threshold).</p> <p>There were significant associations with 'cold temperature' and location of computer 'not in front' – as well as female gender, unmarried status and a history of previous neck/back injury. Perhaps the authors could comment on the implication that increased computer use is not associated with increased neck/back pain.</p> <p>Though interaction and subgroup evaluations had been performed by gender, there was little discussion on the effect modifications achieved between men and women computer-workers. In particular,</p>
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	<p>the effect of monitor 'not in front' seemed to be exclusive to females – the discussion should address the possible reason and implication of this.</p> <p>Also, evaluations were carried out separately for neck and low back pain – the authors should focus some discussion around the comparable and contrasting modifier variables for these two clinical areas, which may further be helpful in guiding prevention/intervention strategies</p>
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VERSION 1 – AUTHOR RESPONSE

Response to Reviewer: 1

Thanks for your comments! We have revised the relative sentences depend on your comments.

INTRODUCTION-----Although several previous studies were conducted on the related topics, the exploration in population of computer based office workers is insufficient especially in China.

Nevertheless, we have revised and added little literature in manuscript.

METHODS----- The revisions were conducted depend on comments, such as inclusion criteria, deleted word of “validated”, measurement of occupational risk factors, and sensitivity analysis.

Besides, NPQ and ODI were tools to assess pain/discomfort/inability of NP and LBP, which were generally accepted by previous researchers. [1-3]

RESULTS-----The prevalence of our manuscript refers to point prevalence. And the method of TERTILE is commonly used in epidemiology, which allowed us to explore the relative risk level (odds ratio). The linear regression model requires normalization of the variable’s distribution. However, the distribution of age in our participants was not. Nevertheless, we have added the p value into table based on linear regression models, although it has similar results between categories and continuous variables of NP and LBP.

DISCUSSION----- In general, sitting time ≥ 6 hours/day was called prolonged sedentary behavior that was closely related to disease incidence and mortality.[4-5] Thus, we could define participants with the computer-using time ≥ 6 hours as intensive computer users. In our study, 74.6% of the participant used computers 6 hours or more per day.

Additionally, the discussion point of the first was very different from the one of the fourth paragraph. It was the main results (modifiable environmental factors) in the first paragraph of the discussion section. The sentence of “individual factors, including injuries of the neck/low back, married individuals, and female sex, were associated with non-specific NP/LBP” was a report of additional consequences.

Response to Reviewer: 2

Thanks for your comments! We have amended, deleted, or added some relative sentences based on your comments. We have deleted the figures of OR and 95% CI in text results for report concisely and smoothly. And, word of “limited” replaced by “a few”.

Response to Reviewer: 3

Thanks for your comments! We have revised, deleted, or added some relative sentences depend on your comments.

1. The present study was conducted in a relatively small sample size, which might limit the statistical power. We have added this point to the limitations section.
2. We have compared the results of the descriptive analysis between included and excluded the participants, which were encoded as mean for continuous variables and mode for categorical variables.
3. “Working years” means office work years in current job. And “related injuries” mean general neck/low back injuries. We have revised the related sentences in the METHODS part.
4. We have assessed linear regression models on raw NP/ODI scores. The results have been included into tables.

5. The 'prevalence of pain' of the present study refers to point prevalence. This prevalence included levels of neck/low back pain from mild to severe, which may over estimate the rate compared with the approach of clinical examination or define it by moderate or severe pain, despite the fact that the similar method was used in the previous studies. [6-9]
6. We have added a sentence of potential implication in the conclusion part.

Reference:

[1] Sim J, Jordan K, Lewis M, Hill J, Hay EM, Dziedzic K. Sensitivity to change and internal consistency of the Northwick Park Neck Pain Questionnaire and derivation of a minimal clinically important difference. Clin J Pain 2006;22(9):820-6 doi: 10.1097/01.ajp.0000210937.58439.39[published Online First: Epub Date]].

[2] Sheahan PJ, Nelson-Wong EJ, Fischer SL. A review of culturally adapted versions of the Oswestry Disability Index: the adaptation process, construct validity, test-retest reliability and internal consistency. Disabil Rehabil 2015;1-8 doi: 10.3109/09638288.2015.1019647[published Online First: Epub Date]].

[3] Murphy DR, Lopez M. Neck and back pain specific outcome assessment questionnaires in the Spanish language: a systematic literature review. Spine J 2013;13(11):1667-74 doi: 10.1016/j.spinee.2013.08.046[published Online First: Epub Date]].

[4] BISWAS A, OH PI, FAULKNER GE, et al. Sedentary Time and Its Association With Risk for Disease Incidence, Mortality, and Hospitalization in Adults: A Systematic Review and Meta-analysis [J]. Ann Intern Med., 2015, 162(2): 123.

[5] PATEL AV, BERNSTEIN L, DEKA A, et al. Leisure Time Spent Sitting in Relation to Total Mortality in a Prospective Cohort of US Adults [J]. Am J Epidemiol, 2010, 172(4): 419.

[6] Chiu TT, Lam PK. The prevalence of and risk factors for neck pain and upper limb pain among secondary school teachers in Hong Kong. Journal of occupational rehabilitation 2007;17(1):19-32 doi: 10.1007/s10926-006-9046-z[published Online First: Epub Date]].

[7] De Loose V, Burnotte F, Cagnie B, Stevens V, Van Tiggelen D. Prevalence and risk factors of neck pain in military office workers. Mil Med 2008;173(5):474-9.

[8] Sim J, Jordan K, Lewis M, Hill J, Hay EM, Dziedzic K. Sensitivity to change and internal consistency of the Northwick Park Neck Pain Questionnaire and derivation of a minimal clinically important difference. The Clinical journal of pain 2006;22(9):820-6 doi: 10.1097/01.ajp.0000210937.58439.39[published Online First: Epub Date]].

[9] Yue P, Liu F, Li L. Neck/shoulder pain and low back pain among school teachers in China, prevalence and risk factors. BMC Public Health 2012;12:789.

VERSION 2 – REVIEW

REVIEWER	Morten Wærsted Given in my first review
REVIEW RETURNED	03-Feb-2017

GENERAL COMMENTS	<p>It seems to me that the authors have corrected according to my comments. However, their specific reply to me is not easy to read due to a not to good English language. This point to my main comment this time: The English language has to be improved, as there are several examples of bad English or not comprehensible sentences. These are not all over the text, but far too many to be acceptable. I advise the use of a person clever in English copy editing.</p> <p>Minor comment: The sentence “For LBP, high-level pain was associated with an OR of 3.2 compared with low-level pain in females” in the Abstract does not make sense. It seems to lack the name of the risk factor with this increased OR.</p>
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REVIEWER	Martyn Lewis Keele University UK
REVIEW RETURNED	07-Feb-2017

GENERAL COMMENTS	Thank you for your updates and for responding to my earlier comments - which have been largely addressed - and to which have improved the paper.
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VERSION 2 – AUTHOR RESPONSE

We have revised the abstract according to the reviewer comments, and the full text has been proofread by a native English scholar from Cactus Communications Inc. (<https://www.editage.com/>).