Author's response to reviews

Title: Computer work and musculoskeletal disorders of the neck and upper extremity: A systematic review

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Author's response to reviews: see over
Dear Sir

We do appreciate the thorough comments of the referees and have revised the manuscript accordingly, as pointed out in our point-by-point response below. We do hope that our revision of the manuscript is satisfactory and that the paper may be published in the BMC Musculoskeletal Disorders.

Response to Referee 1:

The referee has a general comment that the paper is lengthy and has extensive tables. Our aim has been to give the interested reader a possibility to have comprehensive information on the contents of the single papers, without having to read the actual paper. Our thought has been that only the very interested reader will look into the details of Tables 1, 2 and 3, and we therefore have made Tables 2 and 3 rather comprehensive with respect to design, methods and results. We have intended to balance the text and these tables, so that reading the text only will give a general and easy accessible overview of the included papers, while further details may be found in Tables 1, 2 and 3. This implies that there are a few instances were a result is mentioned in the text and then given in more details in Table 3. At the same time we bear in mind the principle commented by the referee that data shall only be given once and have checked this when revising the manuscript. We have tried to reduce the contents of Table 3 as suggested by the referee. However, other comments by the referee have made us add information (see minor essential revision point 4), so that the revised Table 3 is not reduced in size. We have considered several different ways of presenting the data given in Tables 2 and 3 in one or more tables. We think that merging these two tables would result in a too extensive table and that the information given in Table 2 may well stand alone.

Major compulsory revisions

1. In the choice of search terms we only used the more general terms for musculoskeletal disease, as we did not expect that the inclusion of specific terms as indicated by the referee would identify additional relevant papers, but would complicate the search process. We will argue against performing an extra literature search with disease-specific terms at present, as that would cause a substantial delay in the publication of the review.

2. The comments by the referee have shown us that the paragraph on quality assessment in the Methods section should be rewritten to make our reasoning clearer, see the new version of this paragraph in the revised manuscript. This new version of the paragraph answers the questions raised by the referee.

3. The original data base search in April 2005 produced 13410 citations and the update search for the years 2005-2009 in February 2009 produced in the order of 2500 extra citations. After excluding duplicates and papers with non-relevant topics, approximately a thousand abstracts were read, resulting in the retrieval of nearly 200 epidemiological papers that were read in full. Of the final 20 studies that were included, 19 were identified through the computerized search, while one study was identified by the additional search in the reference lists of the included studies and selected reviews. We have included the following sentence at the start of the Results chapter: ‘After exclusion of duplicates and papers that covered non-relevant topics from the ca 16,000 titles retrieved through
the computerized data base search, approximately a thousand abstracts were evaluated for relevance and nearly two hundred epidemiological papers were read in full.

4. We do agree that there is a considerable variation between the included studies with respect to the evidence they give to the question raised in this review. However, we have experienced that studies may shed light on our research question, even if the main purpose of the actual study was not in line with the purpose of our review. As the amount of research literature in this field is sparse (and especially so when we only include papers with a physical examination), we are reluctant to impose criteria that would possibly exclude papers that give some, however limited, insight into our research question. In our discussion of the individual studies and in the weight they have on our final conclusions, the arguments put forward by the referee are considered. We do agree that randomized controlled trials may give the best evidence, and this quality of a study has been given due attention in our evaluation of the importance of the individual study.

5. The first three pages of the Discussion section in the manuscripts discuss strengths and limitations of the review and this part has been strengthened in the revision with regard to the questions raised by referee. We did not observe specific signs of publication bias. However, it is a general knowledge that positive findings more easily get published and some of the papers of lesser methodological quality would probably not have been published if they had not shown associations between computer work and musculoskeletal conditions. As the objective of this review was to examine a possible relationship between computer work and musculoskeletal disorders that could be diagnosed by a physical examination, our inclusion criteria had to be strict on this point. Otherwise our review would to a great extent be a replication of other recent informative reviews (cited in our manuscript) and not important to publish. However, the knowledge from other well-conducted studies has been included as contributing evidence in the Discussion.

Minor essential revisions

1. See the argument under major essential revisions, point 5. The previous reviews did not explicitly focus on the musculoskeletal disorders, but studied a broader category including subjective musculoskeletal complaints. This is pointed out in the first paragraph of the Introduction, which in the revision has been modified to make this point clearer by using the term ‘musculoskeletal complaints’ (instead of ‘musculoskeletal outcomes’), as distinct from ‘musculoskeletal disorders’, in the second sentence of this paragraph.

2. The reason for this restriction is pointed out in the first paragraph in the introduction. This point is made clearer in the revised version of the manuscript, by modifying the second last sentence of this paragraph: ‘.... when evaluating a possible causal relationship between computer work and musculoskeletal disorders, such as when handling insurance claims, it is necessary with a more objective measure of sustained effect on the musculoskeletal system and this is the basis for the present review’.

3. ‘Acceptable quality’ is included in three items (7, 9 and 16) in the form of ‘..... data .... were collected using standardized methods of acceptable quality’. The definition is given in the foot-note (b) stating that ‘this item was scored positive if the quality of the methods used was tested and
documented by the authors or the authors used (and made reference to) well established and documented methods in the literature’.

As pointed out in the revised paragraph on quality assessment (see major essential revisions, point 2), the use of quality assessment check-lists has several shortcomings and the assessment list used may be improved. Actually a work on revising the assessment list is presently under way at our institute. However, at the time the assessment for this review was performed, we chose to use an established assessment list that had proven its convenience in a similar review setting. We added one item to make the list more appropriate in our setting with several diagnostic entities. Possibly we could have added one or two more items, i.e. as proposed by the referee an item for blinding with respect to exposure status and an item for randomized controlled study design. However, we think that instead of introducing several changes in a published assessment list, one should perform a total and thorough revision of the quality assessment list and that was not within the scope of the project of performing this review. As mentioned in the revised paragraph on quality assessment in the Methods section (see major essential revisions, point 2), our non-systematic evaluation of the quality of the studies did not give a substantially different result for the grouping of the studies as having low, moderate or high quality. The same is true if we had included extra quality scores items as suggested by the referee. We therefore prefer to keep the quality assessment list as it is, and thus not change Table 1.

4. We have revised Table 3 in order to be more consistent in the selection of information and in the way this information is presented, e.g. in the reporting of confounders included. However, there are limitations for our reporting posed by the presentation given in the included papers themselves. We have chosen not to try to go beyond the results presented in the papers by e.g. estimating odds ratios or risk ratios when these are not given in the paper. In the paper by Dainoff which to referee mention as an example, no such estimates are given. The authors only give the statistical test of significance for the changes observed. In the revised table we now refer these test statistics and not only the p-value.

We do agree that the content of the interventions is very important. We have thus secured that they are clearly stated in the revised table. However, we are reluctant to subdivide the table, as that might make it more difficult for the reader to find a specific paper. We have also concluded that the overall most practical way is to present the studies in the same order in the text and in the two tables, and we have chosen to present them in alphabetical order as that probably will give the easiest way of finding a specific study, without having to first find the relevant category. We therefore prefer not to single out the intervention studies in a separate table. In addition most of the intervention studies present both cross-sectional and longitudinal data and for some of them the cross-sectional data are the main results as the longitudinal part of the study was inconclusive.

Discretionary revisions

1. The column headings of Table 3 have been changed in order to be consistent, as pointed out by the referee.
Response to Referee 2:

The referee’s major comment concerns the number of researchers involved in assessing the scientific papers reviewed. We have here chosen a common standard procedure of having two researchers independently evaluating the quality of each paper, and then deciding on the final score in a meeting with all authors of the review (which in our case was three researchers). Possibly the quality of the assessment could be improved by involving more researchers, but it is impractical. Besides, we do not agree that the nature of our review is so special that the standard procedure could not be followed. It may be mentioned, however, that in the process of making the report that initiated our review, the report was reviewed by two external reviewers and the final draft then discussed in a one day meeting with the Scientific Committee of the Danish Society for Occupational and Environmental Medicine. Among other aspects of the review, the quality assessment was discussed thoroughly in this meeting.

Minor essential revisions

1. Possibly more search terms could have been included. However, we considered that our search string was sufficient to capture the papers we wanted to include. See our reply to referee 1, major essential revisions, point 1, for further arguments on this point.

2. We are very happy to be made aware that there still are misspellings in our manuscript and we have done a thorough work to read through and eliminate the remaining misspellings. We hope this has been successful (i.e. we have discovered the misspelling ‘idsorders’ for ‘disorders’ in the submission of the title of our manuscript (not in the manuscript itself)). However, of the three examples given by the referee, we only agree on one of them. The first example given, is from the search string where we have used the truncation ‘extremit*’ in order to capture both ‘extremity’ and ‘extremities’. In the last example we think the meaning of the sentence will be changed by including an ‘a’. In our opinion the meaning of ‘… these conclusions are based on few included studies …’ is different from ‘… these conclusions are based on a few included studies …’.