Author's response to reviews

Title: Musculoskeletal disorders among construction workers: a one-year follow-up study

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Author's response to reviews: see over
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Dr William S Shaw  
Editor  
BMC Musculoskeletal Disorders

Date  
August 7th, 2012  
Enclosures  
Electronic manuscript  
Re  

Dear Miss Quiniquini, Dear Dr Shaw,

Herewith we resubmit the revised manuscript “Musculoskeletal disorders among construction workers: a one-year follow-up study”. This paper has been revised in line with the reviewers’ suggestions.

On the next pages are the reviewer’s remarks and suggestions stated in italics followed by our statements on how the manuscript has been changed accordingly. Changes in the manuscript are highlighted in the document “MSD construction workers_highlighted.doc”. The clean manuscript is entitled “MSD construction workers_clean.doc”. We hope this paper is now suitable for publication in BMC Musculoskeletal Disorders.

Looking forward to your reply,

Yours sincerely,
Julitta Boschman, Msc  
Henk van der Molen, PhD  
Judith Sluiter, PhD  
Prof. Monique Frings-Dresen, PhD
Responses to the comments from the reviewers on the paper:

MS: 7049594237027697
Musculoskeletal disorders among construction workers: a one-year follow-up study.

Below are the reviewer's remarks and suggestions stated in italics followed by our statements on how the manuscript has been changed accordingly (see ►). In the document entitled “MSD construction workers_highlighted.doc” the changed sentences and paragraphs are highlighted in yellow. The clean document is entitled “MSD construction workers_clean”.

Reviewers

Reviewer #1. Alberto J Caban-Martinez

Background section: In the last sentence of the abstract’s background section, the authors describe the study population to be bricklayers and supervisors. Are the authors referring to the bricklayer’s supervisors or some other independent supervisor?

► The study population consisted of two separate jobs, namely bricklayers and independent construction supervisors. We described this more clearly now in the last sentence of the background section.

What do the authors mean by “two time point”. Two time points of any on-going study; in the career of a bricklayer? It is not clear what two time points the authors are referring to.

► We were referring to the baseline measurement and the follow-up measurement one year later. We rephrased the sentence in the background section and described this issue in the methods section more clearly.

Methods Section: It would be helpful if the authors would describe to the reader the geographic locations from where they drew their sample. Furthermore the time period would be useful as well. For example, “We randomly selected 750 bricklayers and 750 supervisors from XYZ Area from June 2012 to June 2013.”

► We did not select specific Dutch geographic locations. All Dutch bricklayers and supervisors were eligible for participating in the study. Furthermore, we selected and included all participants at once in January 2011. We described this more clearly in the methods section of the abstract in line with the reviewers suggestion.

Results section: It is not clear why the sample size drastically dropped from 750 bricklayers and 750 supervisors down to 267 bricklayers and 232 supervisors. Was this attrition? If it is in fact the case that the final sample size (due to pre/post measurements) is 267 bricklayers
and 232 supervisors, it might be better to describe it that way in the methods section as to not confuse your reader.

► We selected a total of 1500 bricklayers and supervisors, of which 37% responded at baseline. Of these respondents, 80% responded at follow-up. We stated both response rates in the results section.

Regarding the sentence “Irrespective of the body region, more bricklayers (43-100%) reported that their complaints were work-related compared with supervisors (37-61%).” Perhaps the authors are referring to the workers “perception” about the relatedness of their workload to their MSDs. If so, it should be labeled as a perception rather than a causal statement.

► We rephrased the sentence according to the reviewers’ suggestion.

INTRODUCTION:
It is not clear what the authors are trying to say with this sentence “Furthermore, the strategy for addressing MSDs is also likely to be affected by the limitations that are experienced by workers or the problems that occur during work.”

► We agree that the sentence is unclear and we rephrased this sentence.

Is the first research question suppose explore “the change in self-reported MSDs from baseline to follow up”? If so it should, the sentence should be re-written to reflect this question.

► Our first research question focused on the six-month prevalence of self-reported long-lasting or regular musculoskeletal complaints at baseline. Next to that we questioned whether those self-reported MSD’s were recurrent over the course of one year. We understand that this question was not stated among the research questions and we have therefore rephrased the first research question for clarity. Furthermore, we rearranged the research questions in such a way that they are more clearly connected to the results section.

In question 2a, are the investigators seeking to document the perception of the MSDs perceived by the study participants as work-related at baseline or at the follow up mark? Is it the change of MSDs during the 6 month follow up period for which the investigators seek to document how much is perceived as work-related by the study participants?

► At both baseline and follow-up, we asked the respondents about MSDs and, if any, whether they perceived their complaints as work-related (yes/no). We explored only the
extent of experienced problems during work over the course of one year. We only reported the prevalence and perceived work-relatedness at baseline. We indicated this more clearly in the tables.

METHODS:
Sample and procedures section: Does the Dutch registry contain more than 750 bricklayer and 750 supervisors? If so, could the investigators elaborate as to why they randomly selected 750 individuals from each profession? Was there an apriori power analyses that indicated that given an X-factor of attrition, the study required a recruitment effort of at least 750 workers. Also how was the random selection conducted?
► A priori we aimed at estimating the prevalence with a precision of 5-6%, and estimated that the prevalence of MSDs would be around 40% (Holmstrom and Engholm, 2003). This leads to an estimated sample size of 257-369 among both occupations. Therefore, we aimed at including 300 participants. Based on previous results we expected the response rate to be around 40% (Boschman et al., 2012) and estimated that a total sample size 750 would likely to be sufficient. We added this rationale to the methods section.
The random selection was performed by the independent data manager of the registry, frequently assisting in selecting samples for research purposes. We added this information to the methods section.

Sample and procedures section: Could the authors explain what type of supervisors where interviewed? Presumably they are supervisors employed in the construction industry but it is not clear from the manuscript.
► Our study population consists only of workers employed in the construction industry. We explicitly mention the population in the title of the manuscript: “Musculoskeletal disorders among construction workers: a one-year follow-up study”. In first paragraph of the methods section we also refer to the supervisors as ‘construction supervisors’. Therefore, we think the reader will not be in doubt about the type of supervisors.

Sample and procedures section: Are the study participants provided any incentives for completing the baseline or follow up interview? Please describe this information in the manuscript. Was the lottery ticket the incentive? If so what was the prize provided to the winner(s). It appears that there were two lottery tickets available one at baseline and one at follow up.
► All participants received a lottery ticket of a national lottery as incentive, both at baseline and follow-up. Any prizes could be received by the standard procedure of the lottery involved. We described this more clearly in the methods section.

Work-related musculoskeletal complaints section: Did the authors use the Nordic instrument to assess musculoskeletal complaints? Or perhaps a modified version of the Nordic instrument? Could the authors also provide a citation/reference for the survey instrument they used to measure the MSK complaints?
► We used an adapted version of a questionnaire on MSDs, which has been used in previous research (van der Molen et al., 2009; van der Molen et al., 2010) and among other professions (Ruitenburg et al., 2012), aiming at screening of MSDs in an ergonomics context and providing epidemiological information on MSDs. This adapted version has also been used in previous studies among other professions (Ruitenburg et al., 2012). We described this more clearly in the methods section.

Tasks and activities section: The authors state “Additionally, the participants were asked to describe the cause or aggravator of their complaints if it was not represented in the list.” Could they indicate if this was an open-ended sentence? If so, could they explain how they handled these responses?
► This was indeed an open-ended question. We categorized the answers and described them qualitatively. We added information on how we handled this information in the methods section. We mention the responses on this open-ended question in the results section: “Most of these other causes were not work-related causes, including sports activity (n=12) or neural symptoms after a disc herniation (n=10). None of the bricklayers reported distress as a cause or aggravator of their MSD, whereas seven of the supervisors did.”

Discussion:
Did the investigators assessed what changed between baseline and follow up time period for those workers that reported at baseline long-lasting musculoskeletal pain yet none at follow up??
► No, we did not assess what changed between baseline and follow-up for those workers that reported at baseline long-lasting musculoskeletal pain but not at follow-up. We only verified that the respondents were still working in their occupation, and therefore change of occupation cannot be the cause. We agree with the reviewer that this might be an interesting research topic for further research.
Methodological considerations section: the wording of this sentence “Our study provides, based on a random, adequately sized sample of workers provides an improved understanding” is awkward. The word “provides is used twice. Fix this.
► We rephrased this sentence.

Could the authors speculate about the relationship between their older sample population and the prevalence of MSDs. It is well known that the prevalence of MSDs increases with age, therefore could the authors discuss the relationship between MSDs and Age in their study sample?
► We agree with the reviewer that there might be a relationship between the older age of our population and an increased prevalence of MSDs. It seems logical that the prevalence of MSDs is higher among older workers who are longer employed in a physically demanding job. Therefore, we verified whether age (years) was a significant risk factor for MSDs among both occupations using logistic regression analysis. We found a statistically significant association between age and having MSDs at baseline (OR 1.02, 95%CI 1.002-1.04). Furthermore, we found that the construction supervisors who responded, were statistically significant older (5.5 years, p=0.000) than the initial sample of supervisors. We discussed both findings in the discussion section. Although our results can not be generalized to younger populations construction workers, a high prevalence of musculoskeletal symptoms is also known among apprentice construction workers. Among these young construction workers 54.4% reports low back pain (Merlino et al., 2003). Furthermore, it could also be argued that the prevalence of MSD among our older population is not only biased by having more older respondents participating in the study, but also by the healthy worker effect. Younger workers may have changed their occupation due to MSDs. We can only guess about the size and scope of this health related occupational mobility in the present study population (Siebert et al., 2001).

Did the authors assess the study participants for existing health conditions such as arthritis that can be affecting the report of MSK?
► Although an interesting topic, we did not assess existing and diagnosed health conditions of the musculoskeletal system. We have chosen for an ergonomic approach instead of a medical approach. We aimed at identifying all workers with complaints and work limitations, regardless of the underlying medical diagnosis.
Tables

Table 1: The title of the table is poorly described. The table title should be standalone and describe the data in the table as well as study population and time period. For example, table 1's title could be written as "Demographic and job characteristics among bricklayers and supervisors participating in a baseline and follow up survey, December 2009 to January 2011" Same concern for Tables 2 to 4.

► We rephrased the titles of the tables in accordance with the reviewers suggestion.
Reviewer #2, Andrew Gray

This is a very interesting manuscript and one that I enjoyed reading. It was well written and thoughtful. I did, however, feel that the authors didn't make full use of the valuable data that they have collected and my main recommendations are based around this.

► Thank you for your positive feedback.

Major Compulsory Revisions

The response rate, while not surprising given the area of research and consistent with that from other studies, does lead to questions about how representative the baseline sample is and the possible effects of loss to follow-up at the one year stage. The former question could be in part addressed by giving demographics from the full registry to allow comparisons with the sample (this could be added to Table 1, along with a row for sex) and by discussing possible effects from more interested workers responding (e.g., could the prevalences be overestimates?).

► In order to provide information about the representativeness of the study sample, we compared the age of the baseline respondents and follow-up respondents with the age of the initially selected sample. We found no differences for the bricklayers, but we did find a statistically significant difference for the construction supervisors. At baseline, the responding construction supervisors were older (5.5 years, p=0.000) than the sample. This applies also for follow-up. We described these findings in the results section. Other demographic information then age is not available from the registry. From other studies it is known that response rates are particularly low among young men, but also among unmarried persons and among individuals with a different ethnic background (Christensen et al., 2012).

Whether the prevalence of MSDs found in our sample might be an overestimation of the true prevalence due to more interested workers responding, will be unknown. We acknowledge the added value of a survey among non-responders to gain insight in the non-response bias for future research.

The latter question is very important as those with severe MSD will be more likely to have left the occupation during the year and sensitivity analyses should be conducted to explore potential changes to the conclusions. At the very least, comparisons between those retained from baseline to one-year and those lost to follow-up should be conducted using the variables in Table 1 (plus sex) and also the baseline outcomes of interest (e.g., were those with MSDs more likely to be lost at one year? What about those older, etc.?).
We compared those retained from baseline to follow-up and those lost to follow-up using the variables in Table 1 and the presence of MSDs. We did not find differences in the proportion of MSDs between baseline and follow-up (Chi-Square 0.089, p=0.77), nor did we find an association between having or having not MSDs at baseline and response at follow-up (Odds Ratio (OR) bricklayers: 1.07 (95%CI: 0.57-1.92), OR supervisors: 0.84 (95% CI: 0.48-1.49).

However, we did find a statistically significant association between age (years) and response at follow-up for both bricklayers (OR 1.04, 95%CI: 1.02-1.06) and supervisors (OR 1.03, 95%CI: 1.002-1.05).

Furthermore, we found a statistically significant association between age and having MSDs at baseline for the bricklayers (OR 1.01, 95%CI: 1.001-1.04), but not the supervisors (OR 1.02, 95%CI: 0.997-1.05). We reported these findings in the results section and considered both issues with respect to generalization of the results in the discussion.

Proportions throughout the manuscript and in tables need to be accompanied by confidence intervals. Without these, the reader is left with point estimates and is unable to get a feel for the "plausible range" of values after allowing for sampling errors. This may make the manuscript rather dense in terms of numerals in places, and so perhaps this information could simply be provided in the tables, but it does need to be provided.

We provided the confidence intervals of the proportions. Furthermore, we presented the findings in Table 4 in another way, in order not to confuse the reader with too much point estimates with a large plausible range.

Tables 1, 2, 3, and 4 should include statistical comparisons between the two occupational groups. Otherwise the manuscript tells two almost completely separate stories about bricklayers and about supervisors. This would require additional columns for p-values. For Tables 2, 3, and 4 this may require rearranging the tables to have occupations nested within the other variable so that p-values can be adjacent to the values they are comparing. Without this, the comparisons between occupations are merely descriptive and the reader is unable to easily ascertain which differences are beyond chance levels. For example, the abstract states that "more bricklayers than supervisors are affected by MSDs". This statement claims differences between the two occupations and so a statistical test is required (the p-value of the test lies somewhere between 0.016 and 0.032 depending on exact counts that produced these percentages).
Once this is done, the statistical analysis paragraph (page 7) should state the actual statistical tests used and the level of statistical significance used (presumably all tests will be conducted at the two-sided 0.05 level). There should be enough information in this section to allow replication of the results.

► We unintentionally caused the impression that we were interested in differences in prevalence of MSD between the two occupations. We understand that this is confusing for the reader. However, we designed the study as a descriptive study and not as a comparative study. Therefore, we rephrased the inconsistent and confusing sections and sentences such as those in the abstract.

Minor Essential Revisions

Some of the parenthetical values in the abstract are not clear on first reading (e.g. "43-100%" or "54/111"). The meaning of these values should be clear from the abstract alone and not require reference to the body of the paper.

► We rephrased the sentences at issue.

Arbouw needs to be briefly explained in the sample and procedures for non-Dutch readers.

► We explained Arbouw in the methods section as suggested by the reviewer.

"bend back" (lines 3-4, page 7) should presumably be "bent back". The final item in this list should be proceeded by "and".

► We made these changes in line with the reviewers’ suggestion.

The statistical analysis paragraph should include the sample size calculation the design of the study was based on. This should include information such as the expected prevalences and desired precision as well as the participation rate. If no sample size calculation was performed, this must be explicitly stated here.

► A priori we aimed at estimating the prevalence with a precision of 5-6%, and estimated that the prevalence of MSDs would be around 40% (Holmstrom and Engholm, 2003). This leads to an estimated sample size of 257-369 among both occupations. Therefore, we aimed at including 300 participants. Based on previous results we expected the response rate to be around 40% (Boschman et al., 2012) and estimated that a total sample size 750 would likely to be sufficient. We added this rationale to the methods section.
I notice that there was no reporting of sex. I'm guessing that there are more men in the industry than women, but would be surprised if there were no women whatsoever. This needs to be clarified.

► There were no female respondents. We clarified this in the results section.

Table 1: some of these measures are clearly very skewed (e.g., years employed at current company) based on the ratio of SD to mean. Where the arithmetic mean differs considerably from the median, I'd prefer the latter to be reported along with the IQR. Alternatively, the skewed variables may be approximately log-normally distributed and geometric means and SDs would, in that case, be appropriate and more informative than medians and IQRs.

► In accordance with the reviewers suggestion, we reported medians and IQR.

Prevalances on page 9 are reported slightly inconsistently in that sometimes the actual % is given along with the numbers and sometimes not. This doesn't seem to depend on the percentage being described in the sentence itself. This comment applies throughout the manuscript and while a number of approaches would be fine, the presentation of counts and/or percentages should be consistent throughout the manuscript. I'd prefer both actual numbers and the percentage.

► We corrected the inconsistencies in the presentation of percentages and counts.

The second sentence on page 10 seems incomplete. Should be followed by "as being work-related". Again, there are inconsistencies with presenting percentages and n's.

► We corrected this sentence.

The sentence halfway down page 10, "On average these scores varied between five and seven..." doesn't quite make sense. Does this mean, most scores ranged between five and seven? In which case, I'd give the percentage in this range.

► We corrected this sentence.

The recurrent MSDs as described on page 10 are problematic due to the clustering of MSDs within workers. This should be accompanied by similar percentages for workers with MSDs to aid interpretation. Alternatively, total MSD statistics could be omitted to avoid this complication as I'm assuming each worker can only have one MSD per site?

► We rephrased this section and omitted total MSD statistics.
The triggers for MSD symptoms on page 11 are difficult to interpret due to the breakdown of tasks by the workers (if a worker is always carrying heavy objects, this will be the trigger; whereas if they were required to kneel all day, this would them be the trigger). Some discussion of the typical breakdown of activities would enable the reader to better interpret this data.

We discussed briefly the duration, frequency and intensity of the activities in both occupations, mainly based on the findings from a systematic review on occupational demands for both construction occupations (Boschman et al., 2011).

Discretionary Revisions

I wonder if the duties of the supervisors could be clarified earlier in the manuscript, although it is well covered in the discussion. This would be useful to have in mind while reading the results, especially for international readers where supervisors may have different mental/physical work splits in different countries.

We clarified the types of construction supervisors we selected more clearly in the methods section.

I wonder if the six month prevalence could be justified. While this would reduce potential information biases compared to 12 month recall, this does allow for issues due to seasonal differences in prevalence. Could the authors address this point?

We agree with the reviewer that there are likely to be seasonal differences in prevalence. Based on literature, it seems likely that musculoskeletal complaints will more prevalent during the winter than during the summer (Pienimaki, 2002), with probably larger differences for the bricklayers then the supervisors (Inaba and Mirbod, 2010). By asking the workers in December/January about their MSD during the past six months, we aimed at finding a good estimate of workers who might benefit from preventive actions without too much bias due to including only winter or only summer months. We have addressed this point in the discussion.

Recoding problems into three categories (analysis line 4 on page 7) will greatly reduce statistical power and seems rather arbitrary. This isn’t too much of a problem with the descriptive approach taken, but for inferential analysis, using the full range of values would be much more powerful statistically speaking than categories. I would strongly urge the
authors to reanalyse the data using the full information content of the data. I think that combining 0 (no problems) with 1, 2, and 3 is losing especially important information.

► We agree with the reviewer that recoding leads to loss of information. However, we have chosen for this approach to give the reader more insight in the size and scope of the problem for the individual workers. We feel that by presenting an average score for the whole group, the reader is unaware of the number of workers who are experiencing many problems. It is just this group of workers who are likely to be targeted first in selecting adequate (workplace) intervention measures. A hypothetical example to illustrate our approach:
When half of the workers with MSD does not report any problems (score 0), and the other half reports many problems (score 10), the whole group scores a 5 with a range from 0-10. This may raise the impression that the problems are not that large. On the other hand, when it is reported that 50% of the workers experiences many problems during their work, this may raise other conclusions among occupational health care providers and allied professions. We prefer the latter and have chosen a priori for this outcome to communicate.

This would also avoid the need for the seemingly arbitrary definition of a change in problems as a shift of 2 or more points.

► We agree with the reviewer that from a statistical point of view, it seems arbitrary to define a change in problems as a shift of 2 or more problems. However, as we know from other scales used to gain insight in change of a specific health status, the smallest detectable change should be taken into account. A statistically significant difference over time is therefore not by definition a real change, but might be due to the standard error of measurement. Based on previous experience with scales ranging from 0 to 10 (Boschman et al, submitted), we have a priori defined a shift of 2 or more problems as a smallest detectable change.

In the second line of the results, I'd prefer "...after excluding partially completed questionnaires..." rather than "[w]hen correcting...".

► We corrected this sentence in line with the reviewers’ suggestion.

It would be interesting to look at multivariate modelling for the prevalence of MSDs. I appreciate that this would change the focus of the manuscript somewhat, but I hope that the authors are able to at least look at this in a subsequent manuscript. Looking at whether
prevalences depend on age or years experience would be of interest and considerable value in targeting interventions, for example.

► We agree with the reviewer that multivariate modelling of the prevalence of MSDs might be interesting and of considerable value at group level. However, we feel that we should only report the research questions and resulting outcomes which we have chosen at the time of designing the study.

This is perhaps rather pedantic, but the use of "methodological" in a heading in the discussion may irk some readers as it means the study of methods.

► We changed the heading of the discussion.

This is not relevant to the current manuscript, but I wonder if the researchers had considered a more intensive process for improving the response rate. The response rate here is not atypical, but the findings would have been strengthened considerably by a higher rate. Perhaps Dillman's approach might be worth considering for future studies?

► We agree that it is worth considering how to increase the response rate. The combined use of multiple response-inducing techniques might be worth considering. We mentioned this aspect in the discussion, with special reference to enhancing the response rate among the younger population workers.

Table 4 gives "n.a."s for walking for bricklayers. Based on page 7, it appears the bricklayers were not given this option. Some comment on why this was the case would be useful for the reader on page 7.

► We made a comment on page 7 on why the bricklayers were not given this option.

The authors' contributions statement does not specify who performed the statistical analyses.

► We supplemented the authors’ contributions statement by specifying who performed the statistical analyses and who checked data entry, data analysis and statistical analysis.
References


