Reviewer’s report

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

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Reviewer: Andrew Gray

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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.

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?). 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.?).

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.

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.

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.

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

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

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.

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.

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.

Prevalences 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.

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.

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.

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?

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.

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.

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?

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

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

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 targetting interventions, for example.

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.
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?

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.

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

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests