Author's response to reviews

Title: Effectiveness of an intervention at construction worksites on work engagement, social support, physical workload, and need for recovery: results from a cluster randomised controlled trial

Authors:

Karen M Oude Hengel (karen.oudehengel@tno.nl)
Birgitte M Blatter (birgitte.blatter@tno.nl)
Catelijne I Joling (catelijne.joling@365.nl)
Allard J Van der Beek (a.vanderbeek@vumc.nl)
Paulien M Bongers (paulien.bongers@tno.nl)

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Author's response to reviews: see over
Title: Effectiveness of an intervention at construction worksites on work engagement, social support, physical workload, and need for recovery: results from a cluster randomised controlled trial

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Response to reviewers: Revision 1

Dear editor and reviewers,

We would like to thank the reviewers for the time and effort they have put in reviewing our paper. The scientific and methodological remarks have improved the paper. The following table lists the comments and explains what actions were undertaken to improve the manuscript. We have numbered the comments in this table. We hope to have addressed the comments adequately, and that the paper will be accepted for publication in BMC Public Health.

Kind regards, also on behalf of the co-authors,

Karen Oude Hengel, MSc

Referee(s)' comments to the author:

<table>
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<tr>
<th>Comments of editor</th>
<th>Responses</th>
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<tr>
<td>E1. The list of authors in the manuscript should be written exactly as they are in the submission system, both in style and order. The preferred style is 'First name Initial Last name' (e.g. Joe F Bloggs).</td>
<td>We changed the style of the names of the authors in the manuscript, as they are now the same as in the submission system.</td>
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<td>E2. Please ensure that your Abstract contains the context information of the study in the Background section.</td>
<td>We added one sentence about the context information of the study.</td>
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<td>&quot;To prolong sustainable healthy working lives of construction workers, a worksite prevention program was developed which aimed to improve the health and work ability of construction workers.&quot;</td>
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<td>E3. The Trial Registration Number (TRN) needs adding at the end of Abstract in the submission system.</td>
<td>The Trial Register Number (NTR1278) was added at the end of the abstract in the submission system.</td>
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<tr>
<td>E4. Figure cropping: It is important for the final layout of the manuscript that the figures are cropped as closely as possible to minimise white space around the image. For more information, see the instructions for authors: <a href="http://www.biomedcentral.com/info/ifora/figures">http://www.biomedcentral.com/info/ifora/figures</a>.</td>
<td>The figure was cropped as closely as possible to minimise white spaces around the flow chart.</td>
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**Comments of reviewer #1 (Comments to Author):**

<table>
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<th>Comments of Reviewer 1</th>
<th>Responses</th>
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<td><strong>R1.1</strong> However, in its current form I think it will be of limited use and interest unless some additional aspects of the study are covered. The program was ultimately found to be ineffective and thus the usefulness of this article will be lessons learned from the study. I feel that more discussion of both Program Failure and Theory Failure is required to justify the publication of the article.</td>
<td>We agree that more information on the theory and program failure may be useful for researchers in the near future. We rewrote the two paragraphs of program failure and theory failure.</td>
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<td><strong>R1.2</strong> Program Failure – Did workers have an ability to feed back into the program regarding their experience of the program in which case it would be interesting to know what was said about the program from the participants. If workers did not find the tools provided useful in the first place, then it could go a long way towards explaining why the program did not have the expected outcomes and the lower than expected compliance.</td>
<td>We asked the construction workers and supervisors about their opinions towards the program and specific components. We described this in detail in the process evaluation. However, we agree with the review that some information from this process evaluation might be useful to explain the ineffectiveness of the intervention. Therefore, we added information about the feedback of the workers towards the program as part of program failure.</td>
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<td><strong>R1.3</strong> Research does not occur in a vacuum. What is lacking from the article is a description as to what industry based considerations was taken in the design of the study. Was employers, subcontractors, workers, unions and safety representatives consulted. If the intervention was based on an outsiders view of the industry this this could be a lesson on its own. It could be interested to know how industry culture, industrial circumstances, psychosocial conditions of the workforce and power relationships was considered in the study design.</td>
<td>Indeed, research does not occur in a vacuum. During the development of the intervention, the intervention mapping approach was used in which knowledge from the scientific literature was combined with experiences from practice. Therefore, we indeed consulted employers, workers and safety representatives in this sector. By doing so, we are convinced that the intervention is not only tailored to the needs of the target population but also to the abilities and opportunities of the implementers. We added information of the involvement of the stakeholders in the development of the study in the current study by the following sentences: “The intervention was developed using the Intervention Mapping approach, meaning that theoretical information from literature was combined with practical information from stakeholders (employers, supervisors, workers, health professionals, and providers).[7, 9] By applying the Intervention Mapping approach, the intervention is not only tailored to the construction workers but also to the abilities and opportunities of the implementers. Following from this, a prevention program was developed which consisted of a physical and a mental component.” Moreover, we also performed per-protocol analyses on contextual factors such as company size, and management engagement.</td>
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<th>R1.4</th>
<th>The intervention relied on workers taking particular actions based on training received to have a specific effect such as mini breaks. Part of the identified potential program failures was the empowerment training not always involved the supervisor in the training sessions. It is therefore not entirely clear from the paper whether the required actions from the workers as part of the program actually was within the workers power – i.e. would supervisors accept workers taking rest breaks when required and would workers be inclined to do this at a time when job security was threatened by the economic downturn.</th>
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<td>We agree that achieving a change in the topics discussed during the empowerment training sessions not only lies within the power of the workers. Therefore, we invited the supervisors for the empowerment training sessions. However, most of them did not attend the meeting. Moreover, we agree with your opinion that the economic recession might have hindered workers to change their behavior or activities such as additional rest breaks. In particular during the economic recession, supervisors would probably accept workers to take additional rest breaks. We rewrote the program and theory failure and incorporated this in these two paragraphs.</td>
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<td>R1.5</td>
<td>It appeared that the study relied on attitudes to fatigue and work environment as the responsibility of the individual and not at a systemic level as a collective attitude to these matters at a workplace level. Was measures taken to identify cultural changes in the workplace as a result of the intervention?</td>
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<td></td>
<td>Unfortunately, we did not specifically measure any cultural changes at company level. As described in the manuscript, we only measured social support from the supervisor and from colleagues at individual level. No changes were detected on these outcomes. We underestimated the collective attitude on some intervention outcomes such as mini rest breaks. We elaborated a little bit more on this in the discussion (see a R1.4).</td>
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<td>R1.6</td>
<td>Was any general psychosocial issues such as core needs, job satisfaction, work/life balance etc considered at any level in the study?</td>
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<td>After the development of the intervention, outcomes were chosen that were closely related to the intervention. Regarding psychosocial issues, we measured work engagement and social support from colleagues and supervisor. We expected a change on these outcomes as a result of the training sessions from the empowerment trainer. We measured need for recovery, which can be seen as the need for recovery from work in leisure time. We expected a change in the need for recovery as a result of the Rest-Break tool. We did not measure outcomes that the reviewer mentioned such as core need, job satisfaction and work/life balance. However, we agree that job satisfaction might have been interesting as an outcome in general.</td>
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**General**

**R2.1** Authors state in their Sample Size calculation that some effects may be expected and they have mentioned some prognosis in their former design study (medium effect), however, clear hypotheses are missing and could strengthen the introduction and consequently, their methodology.

The sample size calculation was shortly described in the manuscript because it was already described in the design paper. As the reviewer is right that clear hypotheses are still missing, we decided to add more information about the sample size calculation in the manuscript.

*The sample size of workers was calculated according to the number of cases needed to identify an effect on health status which was measured by the SF-12. Health status is one of the other outcome measures of the trial, and will be published in a separate paper. As the SF-12 has rarely been used in intervention studies among the general population, the sample size calculation was based on the SF-36.[12]. Based on means and standard deviations of the SF-36 from earlier studies among different groups of workers, we calculated the sample size needed to detect relevant changes in health, reflecting either “somewhat better (or worse)” or “much better (or worse)” health.[12, 13] Because of the cluster randomization design, a certain loss of efficiency associated with cluster randomization relative to individual randomization was taken into account.[14] An effect size of 0.40 was considered to be the lower boundary of a ‘medium’ effect size.[15] This effect size can be detected with a power (1-β) of 0.80 and a two-tailed alpha of 0.05 with two groups of 100. Taking a loss to follow-up of about 10% into account, 220 workers were required at baseline.*

**Introduction**

**R2.2** Page 3: A recent study added that psychosocial….Please refer.

Thanks for noticing, we added the reference².

**R2.3** Page 3: The Rest-Break Tool is insufficiently described on psychometric properties, are any references available or has this been constructed by the authors?

During the development of the intervention³, construction workers mentioned that they prefer more flexibility in rest breaks. This was especially true by heavy physical work tasks such as depositing concrete. Additionally, it appeared that they are not always aware of their fatigue in relation to their work tasks. It was therefore considered that a tool was needed which increased the awareness of the workers about their fatigue, and how they can reduce their fatigue by taking flexible rest breaks. As such a tool was not available, the Rest-Break tool was developed by the authors.

In the revised version of the paper, we mentioned in the methods that we constructed the Rest-Break tool.

*This tool was constructed by the researchers, and aimed to raise awareness about the importance of reducing fatigue by taking flexible rest breaks, and to stimulate to actually take rest breaks.*

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### Methods

R2.4 It is not stated how the randomization procedure was performed? Were the researchers blinded for the randomization?

The randomization procedure was performed by an independent researcher (i.e., research assistant) by using a computer-generated random-sequence table. However, it was impossible to blind the main investigator for group allocation as she visited the construction workers often. In the revision paper, we added some sentences about the randomization procedure and the blinding of the researchers.

“Cluster-randomization took place at the level of the department within each company, using a computer-generated random-sequence table. In order to avoid intervention group contamination, to accommodate the worksite intervention, and to enhance participants’ compliance, cluster randomization was considered the best randomization strategy for this study. The randomization procedure was performed by a research assistant, who had no prior information about the departments. Obviously, as the intervention took place at the worksite, it was impossible to blind the researchers, the construction workers, their supervisors and the trainers to the allocation.”

R2.5 Was the power analysis based on a one- or 2-tailed Alpha and which effect was expected to what direction? Please state.

The power analyses was based on a 2-tailed Alpha. The sample size calculation was performed on a 2-tailed Alpha because this is commonly accepted. We elaborated the paragraph about the sample size calculation including the reviewer’s remark.

See R.1.1

R2.6 Page 5: outcome measurements: „The present study investigated…..“ the sentence is incomplete.

The sentence is as followed:

“The present study investigated the effectiveness of social support at work, work engagement, physical workload, and need for recovery.”

R2.7 Page 6: need for recovery: “in the present study, the scale was highly skewed” to which side?

Most of the workers mentioned that they were not fatigued, meaning that the need for recovery was highly skewed to the right. We added this word to the manuscript.

### Results

R2.8 No recruitment and time details (date of baseline, FU) are provided.

Recruitment and time details were indeed not provided. Companies were recruited in 2008 and 2009, and workers were approached to participate in the intervention after the commitment of the companies.

The baseline measurement and follow-up measurement took place at different moments (depending on the company) in 2008, 2009 and 2010. Because of the different time moments, it is confusing for the reader to add these dates in the manuscript.

Because of the reviewers remark, we decided to mention the recruitment details of the companies but we also decided not to present the follow-up dates as this made the paper unreadable.

We added the following text in the manuscript:

“Those companies were recruited between March 2008 and December 2009. When a company agreed to participate in the program, construction workers of the company were approached to participate at the worksites, and they received the baseline questionnaire.”
**Discussion and conclusion**

R2.9 Although the effect of Workload is significant, the authors state that relevance of this effect is limited. Nevertheless, they present this finding too prominent in the second sentence of the discussion and in the results of the abstract. This result should be more in balance to the data found.

We agreed with the reviewer that results are presented in an unbalanced way. Although we think that mentioning the adverse significant results in the abstract and discussion is necessary, we present this finding less prominent in the revised manuscript as suggested by the reviewer.

We not only mentioned this small effect at the end of the summing-up, but also rewrote the sentence into: 

“At 6 months follow-up, the control group reported a small but significant reduce of physical workload.”

Page 10, in discussing their insufficient statistical power size, authors state that they feel confident that this would not change the intervention effect. Please state on which data they rely.

Indeed, we do not think that a larger sample of workers would have led to statistically significant intervention effects between the intervention and control group. If sufficient statistical power was provided, the confidence intervals were smaller. However, as the mean scores between the intervention and control group are quite similar for most outcome, the intervention effects were still not statistically significant. In the revised manuscript, we mentioned this argument:

“However, while providing sufficient statistical power would have diminished the confidence intervals, these smaller confidence intervals would still not have led to statistically significant intervention effects as the mean scores between the intervention and control group are quite similar for most outcomes (table 2 and table 3).”

Were sources of bias present? Please report on these.

In the limitation section of the discussion we added three examples that might occur in the current intervention.

“Third, data were obtained solely from questionnaires. As a result, all data were self-reported, inducing a potential risk of bias due to socially desirable answers. Fourth, participation in the program was voluntary, and bias due to non-response could therefore not be ruled out in intervention studies. However, the participation of workers was very high (84%), indicating that selection bias due to non-response was minimal in the current study. Fifth, the loss-to-follow-up was higher than expected due to the economic crisis and health-related absenteeism of the workers. As a consequence of the economic recession, one company was forced to lay-off workers, and to and to offer the remaining workers a temporary part-time job during the intervention program. Participants who were lost-to-follow up were higher educated. However, as no other differences between completers and non-completers were found, we assume the bias due to selective loss-to-follow-up was limited.”

Although in the conclusion, the authors state that engagement of the management did not influence effectiveness, a few sentences higher, the authors state that the workers mentioned that involvement of management could be valuable. Authors should explain this discrepancy.

The engagement of the top management towards the programme did not influence the effectiveness as the per-protocol analyses showed no differences. In other words, management that committed themselves to the project and facilitated the implement and stimulated the workers to participate in the project or not, did not change the results of the intervention.

However, engagement of the management towards the intervention program is not the same as involving managers in the intervention to achieve a change at worksite level. Achieve a change in several topics from the empowerment sessions rely also on the decision at management level. Therefore, we are convinced that involving supervisors and middle-management is needed to achieve a change in rest-breaks, communication or other topics discussed in the empowerment training sessions. In the manuscript, we distinguish management in the revised manuscript between the engagement of the top-management and involving supervisors and middle-management to achieve a change.