Author's response to reviews

Title: Effects of organisational-level interventions at work on employees' health: A systematic review

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Author's response to reviews:

Responses to the remarks of reviewer 1

Major compulsory revisions

1. Link between three stated objectives and discussion/conclusion.

We thank the reviewer for pointing us to an urgent issue of clarification, namely the focus on one main research question. In re-drafting the manuscript we now clearly emphasize the core research question, i.e. “What is the link between different types of work-related interventions and their effects on employees' health?” We addressed this point in the title, the Abstract and throughout the manuscript. Moreover, in drawing the conclusion we are more cautious about our recommendations, taking into account the challenges of implementing organisational-level interventions (see Discussion and Conclusions, p. 16).

2. Lack of precision of organisational-level interventions and health outcomes definitions.

In view of the large heterogeneity of organisational-level intervention studies we were urged to classify them into relatively broad categories in order to be able to compare them and to analyse our research question mentioned above. This broad classification concerns both, the types of work-related interventions, and the scope of different health outcomes. Focusing on more specific health outcomes would have resulted in too few studies for a systematic review. We justified this decision in the Methods section (p. 5) and mentioned it as a limitation (p. 15).

3. Selection criteria.

Following the reviewer’s critique we restricted all outcomes to clearly defined health measures. We therefore excluded 2 studies with “psychosocial outcomes”. Concerning the term “strong organizational approach” we deleted this term and replaced it throughout the paper by “organisational-level interventions”.

With the harmonisation of the working conditions we intend to synthesise the results of very heterogeneous interventions as stated in the Introduction (p. 3-4). In the Methods section we give an example how the categorisation of the implementations was conducted (p. 7-8). We do not argue that interventions should be standardised, but we maintain that the instruments used in the design, reporting and evaluation of interventions should be harmonised in order to enhance the comparability of studies. In fact, our results demonstrate that different types of interventions matter for their effects on employees’ health. By reducing the research questions to one core question (see above answer 1), we clarified our argumentation throughout the manuscript.

5. RCTs.

Maybe in the previous version of the manuscript we did not clearly explain that the quality criteria of an intervention study (see Table 1) and the level of evidence as defined by the study design (see Table 2) are two distinct parameters of evaluation. By combining these parameters we defined the three categories mentioned in Table 2. This does not mean, as the reviewer assumes, that we consider quasi-experimental studies as being generally of lower quality. In fact, they are often the best choice of tackling the challenges of intervention studies. We mentioned the limitations of RCTs in the Introduction (see p. 5 f).

6. Classification of working conditions.

Importantly, as evidenced from Table 4, several studies include more than one type of intervention when implementing their modification. As our main objective was the analysis of the link between different types of interventions and their health effects, we applied the proposed classification taking into account its non-exhaustive character. Moreover, we reduced the number of categories to three in order to avoid potential misclassifications between psychosocial and organisational conditions of work (see p. 7 f).

We apologise for a misunderstanding concerning orthogonality in our previous version of the manuscript. We did not intend to define statistically valid constructs of different types of working conditions. Rather, we mentioned the potential gain of such a procedure. We have deleted this paragraph as it is not relevant in our context. We recognise the limitations of our classification scheme in the Discussion section (p. 14-15).

Minor revisions

1. Quality criteria.

We agree with the reviewer: the quality criteria are indeed not exhaustive. Nevertheless, they are not a mere “quality criteria for quantitative data”. They cover relevant aspects related to the conduction and evaluation of the intervention, and are embedded in a quantitative transformation scheme. Given that we are interested in the effectiveness of interventions, we focused the criteria on confounders that may influence the estimation of intervention effects.
For instance, the criteria “Is the follow-up response greater than 50%?” and “Have the authors adjusted for non-response and drop-out?” may account (as proxies) for personnel turnover or similar more qualitative aspects associated with re-structuring, as discussed e.g. in Olsen et al. 2008. In the revised manuscript we expanded our argumentation on the quality appraisal scheme (p. 7-9), and provided the rationale for each criterion in a new column in Table 1. This weighting scheme might be questionable, of course, but we think it is appropriate in the context of our systematic review. Concerning the higher weight given to the number of subjects, our argument is as follows: “The reason of emphasizing sample size within the evaluation of study quality is the lack of information on statistical power within the reviewed studies. We therefore used sample size at baseline and at follow-up as proxies of potential statistical power estimates of meeting significant intervention effects” (p. 6)

2. Studies of high evidence level.

We agree with the reviewer and therefore discussed potential explanations extensively in the revised Discussion section (see p. 13 f).

3. Publication bias.

The assumption that some studies might omit factors that were initially thought to be targets cannot be substantiated with the information supplied by the studies considered in our review. As a matter of fact, as it can be seen in our Table 4, only 19 out of 39 intervention studies reported positive results. This means that the distribution of statistically significant results is rather symmetrical around zero. Therefore, we do not see any evidence of publication bias. Thus, the circularity argument may not apply.

Discretionary revisons

1. We thank the reviewer for the excellent bibliographical remarks. We have commented and incorporated some important ideas of these studies in the Introduction and Discussion sections.

Responses to the remarks of reviewer 2

Major compulsory revisions

1. We fully agree with the reviewer that the broad classification of intervention studies may be questionable. However, as stated in the Introduction, we intend to compare and synthesise the results of very heterogeneous interventions in order to answer our main research question: “What is the link between different types of work-related interventions and their effects on employees' health?” We justify our approach in the Introduction (p. 4) and discuss its limitations in the Discussion section (p. 15).

2. The reviewer is right in proposing that interventions should be classified according to the underlying theoretical model, e.g. job-demand-control model. However, in an additional re-analysis of our data we observed that even this classification did not improve the successful rate of interventions (7 out of 15
studies based on the job-demand-control model report statistically significant effects on health). This rather disappointing result is probably due to the difficulties of implementation discussed in the manuscript.

3. Although we agree with the reviewer that the perception of changes of the work characteristics following the implementation of an intervention is an important mediator, most of the studies included in this review did not provide appropriate data to analyse this aspect. It is a general shortcoming in this field of research that the role of the workers and (line) managers in dealing with the intervention has not received appropriate and systematic attention (see p.3f and p. 13f. for discussion).