Author's response to reviews

Title: Return to work after a workplace-oriented intervention for patients on sick-leave for burnout - a prospective controlled study

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Author's response to reviews: see over
Dear Editor,

Please find submitted a revised version of the manuscript:
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Björn Karlson, Peter Jönsson, Birgitta Pålsson, Gunnel Åbjörnsson, Birgitta Malmberg, Britt Larsson and Kai Österberg. Return to work after a workplace-oriented intervention for patients on sick-leave for burnout - a prospective controlled study

We have addressed the comments by the two reviewers by changes in the manuscript, and with responses to each of them point-by-point on how we have addressed their questions and comments. This will be found below.

Changes in the manuscript will be easily seen as tracked changes. However, tables and figures have been changed so substantially that they have been replaced in total.

Reviewer 1 found a need for some language corrections. The former version of the manuscript had in fact been professionally proofread by a native English speaking person. If you, however, find a need for an additional professional proofreading for language corrections of the revised version, we will of course do that.

We have to the best of our ability formatted the manuscript in accordance with the journal style.

Sincerely,

Björn Karlson
Response to reviewer’s reports

Reviewer 1 – Mirjam Ekstedt

Discretionary Revisions

1. The description of the recruitment process was somewhat improper, but is now clarified. As stated in the text: “To maximize the participation rate also persons with a not assured relation between sick leave and work stress were included in a list of possible participants being sent to the research group, who informed the identified persons by letter about the project. This was followed up with a phone call to invite them to participate.” The definite re-check of the relation between sick leave and work stress was carried out in the following interview. In contrast, the control group was selected among only those for whom the relation between sick leave and work stress was already assured from the files from the RSIO, why a check of this was not necessary. However, when preparing an answer the reviewer’s question the data were re-checked. We then found that a minority of the control group used in the former version of the manuscript had an uncertain relation between sick leave and work stress. These subjects have been changed into persons fulfilling the criteria properly, i.e. an assured relation to work stress, and the data have been re-analyzed. We had to drop two persons in the intervention group due to lack of matching control persons. Thus, the tables and figures are changed in accordance with the re-analysis of the data, which however only changed the results marginally, indicating a robustness of the findings.

2. Motivation could of course also be a factor contributing to decision to participate in the intervention. A section about this is now added under Limitations in the Discussion part. We are perfectly aware that this is the weakest part in the design and that it implies a potential selection bias not having been possible to fully control.

Minor essential revisions

1. The tables and figures are changed

2. The headings are changed and hopefully easier to understand.

3. This was a typing error which is corrected.
Reviewer 2 – Erik L. Werner

1. We agree that there is a possible bias with respect to the recruitment process, as discussed in the manuscript and further emphasised in the revised version.

2. We have a lot more information about the intervention group than presented in the manuscript. We have added a sentence commenting on education levels, type of jobs and employers under the description of the group. For the control group however, we only have information about gender and age, as we did not have any contact with them except for the invitation phone call. In addition, we have, for confidentiality reasons only anonymous sick-leave data for this group. The sick-leave data was acquired by an officer at the Regional Social Insurance who transferred the persons’ identities into a code number before returning the data to the research team. Thus, we could present more data about the intervention group, but that would increase the already great information imbalance between the groups, why we would prefer not to do that.

3. The first sentence on p 8 about ‘Theoretical base’ is now split into two sentences.

4. The reviewer asks: ‘On page 13 the last sentence in the second paragraph ”However, the rate of...” is confusing: was the only difference between the intervention and control groups the use of gradual RTW?’

We find the reviewer’s comment hard to grasp. Firstly, we fail to see in what way the last sentence in the second paragraph "However, the rate of..." is perceived as confusing by the reviewer. In the paragraph referred to, we try to discuss what possible reasons there might have been for the higher proportion of full-time RTW in the control group during the first year, for example, whether a premature full-time RTW might be one possible explanatory factor. Perhaps the reviewer has interpreted the concept of “rate” as meaning “pace” (and not, as intended, “proportion”)? For this reason we have now replaced “rate” with proportion throughout the manuscript.

Moreover, we identify two possible interpretations of the latter part of the reviewer’s comment:

a. That the reviewer presumes that gradual (partial) RTW was used only in the control group, which was certainly not the case, as shown in Table 2. Gradual RTW was actually not uncommon in the control group during the early stages of the study period; e.g. 26% on gradual RTW in week 10 and around 15-18% during weeks 30-60. This indicates that the strategy of gradual return to work also existed in the control group, although it did decrease towards the end of the study period.

b. That the reviewer interpret the occurrence of gradual (partial) RTW as the main difference between groups towards the end of the study period (Weeks 70 and 80). In a sense, this is true since the proportion having fully returned to work was equal in the intervention and control groups (64%), which means that the remaining parts of the groups having returned to work was on part-time RTW; 26% in the intervention group and 9% in the control group. This difference might be interpreted as one of the more prominent ones at the end of the study period. However, and from our perspective, we regard the considerably lower proportion of subjects on full-time sick-leave in the intervention group at the end of the study period (11% vs. 27%) as the more relevant finding, in order to prevent exclusion from the labour market.
5. The patient’s sick-listing doctor was informed about that the patient had accepted to participate in the program, and got a concluding summary of the results of the clinical examination and the CDM, but was not involved in the project. This is now clarified at the end of the second paragraph of the Method section.

6. In the Discussion section we now briefly comment on the temporary convergence of the sick-leave rates occurring at six months and after slightly more than a year.