Reviewer’s report

Title: The contribution from psychological, social, and organizational work factors to risk of disability retirement: a systematic review with meta-analyses

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Reviewer: Jan Fekke Ybema

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Review for BMC Public Health
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The contribution from psychological, social, and organizational work factors to risk of disability retirement: a systematic review with meta-analyses

This is a highly relevant and interesting review and meta-analysis of the contribution of work factors to disability retirement. I believe the used methods are adequate, the results are interesting, and the conclusions are generally supported by the findings. However, I believe both the Introduction and the Discussion could benefit from a more rigorous theoretical framework to structure the description and outcomes of included work related factors. I also have a few other suggestions to improve the manuscript. In my view, a revised version of this manuscript would be a valuable contribution to the literature.

Abstract

The "moderate evidence for the combination of high demands and low control" has a confidence interval that includes RR=1. This means that it does not reach common levels of statistical significance (p < .05). It seems strange to label this as moderate evidence. I would rather label this as limited evidence.

Background

The Introduction (Background section) of this study is relatively short, lacks a clear theoretical focus, and is somewhat disorderly. Moreover, many statements are not substantiated with references.

Example: "While there are several facets of disability, work ability … holding a job in general" (p 3-4). Why are work ability and competence introduced? How are these terms related to disability? References are missing for the definitions of work ability and competence, etc.

I was somewhat surprised that the dominant model of psychosocial work factors of the last decade, i.e., the Job Demands-Resources (JD-R) model (Demerouti, et al., 2001; Schaufeli &
Bakker, 2004; see Schaufeli & Taris, 2013 for an overview) is not mentioned in the Introduction. This model has a much broader view of job demands and job resources than either the (older) Demand-Control Model and Effort Reward Imbalance model, and could easily be used to include all work related factors that are studied in this manuscript.

The work ability framework by Ilmarinen could be an alternative theoretical framework for this manuscript.

The organizational factors that are mentioned on page 4 - downsizing and work schedules - seem to be only a small subset of possibly relevant organizational factors. I would also mention factors like organizational policy and organizational culture, including workplace health promotion, occupational safety and health policy, absence policy and culture, and organizational justice (which is mentioned briefly).

The last sentence of this section "Retirement may promote health if …" (p 6) does not seem relevant for this manuscript.

I would like a summary of the Research Questions that are addressed in this manuscript at the end of the Introduction.

Methods

The time frame of included studies ends at April 23, 2015. By the time of publication of this study, the most recent study will be about 2 years old. I realize that you cannot keep on refreshing your search with new studies. But it may be worthwhile to add the more recent studies to this review.

I think it strange to take as a criterion that "no studies showing nonsignificant … effects" (p 12-13). Nonsignificant effects can - among many other things - be caused by lack of power of the concerning study. Therefore, nonsignificant effects cannot be interpreted as evidence for no effect (i.e., you cannot confirm the null hypothesis).

Results

According to the Methods section (p 12), the highest score for quality was 77%, but in the Results section (p 15), it says 81%. Which one is correct?

Are the reported RR, OR, HR in Table 1 the crude relative risks or the risks adjusted for the 'confounders' in the final column.

Did you use the crude or the adjusted RR in the meta-analyses in Table 2 (p 37)? I suppose these are the crude RR as each individual study uses other confounders?
I strongly believe you should not adjust for health status, psychological strain, etc. in the meta-
analysis. These health related variables are likely mediators of the effects of work related factors 
on disability. This means that correcting for these mediators will lead to underestimation of the 
effects of the work related factors. These work related factors will probably have had their effect - at least in part - on health already by the time of measurement of these work related factors.

"Analyses stratified analyses" (p 37) please correct.

Does the NMS of 623 in Table 2 (p 37) mean that 623 studies (of average sample size) with no 
relationship between control and disability retirement (i.e., RR = 1) are needed to reach nonsignificant findings? This seems a high number.

Also the NMS = 21 for the sub group seems rather high as the lower threshold of the 95% CI = 1.04, which is very close to 1.

Both passive jobs and high strain jobs seem predictive of disability retirement (p 41). I think it 
should be mentioned that both kinds of jobs are characterized as jobs with low control, which is 
dealt with elsewhere in the manuscript.

Under the heading "organizational change", it says "of the 32 studies". This should be 39 studies.

Discussion

In the Discussion many individual effects of work related factors are mentioned. A clear 
theoretical framework to structure this discussion could help the reader to understand their 
relationships with each other and the assumed pathway through which these work factors 
influence disability retirement.

In the first sentence (p 46), 12 high quality studies (out of a total of 32) are mentioned. However, 
according to the Results section (p 15) this should be 19 (out of 39).

As in the Abstract, the "moderate evidence for the combination of high quantitative demands and 
low control" (p 46) has a confidence interval that includes RR=1. This means that it does not 
reach common levels of statistical significance (p < .05). I would rather label this as limited than 
as moderate evidence.

What is meant by "negative findings" (p 46): nonsignificant findings or significant opposite 
findings?

I believe you cannot conclude that there is "… evidence that general job demands does not 
predict disability …" as you cannot confirm the null hypothesis that there is no relationship.

The finding that high support predicted disability may be explained by strain or health problems 
as a third variable (cf. your alternative explanation of reduced workload at p 53).
"highest score was 77%" (p 55). Or was it 81% (p 15)?

"The fail-safe N statistic showed that 45 studies …" (p 56). How does this relate to the NMS=623 studies in Table 2 (p 37)?

References


Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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