PEER REVIEW HISTORY

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ARTICLE DETAILS

| TITLE (PROVISIONAL) | Job strain among blue-collar and white-collar employees as a determinant of total mortality: A 28-year population-based follow-up |
|---------------------|-------------------------------------------------------------------------------------------------------------------------|
| AUTHORS             | Mikaela B. von Bonsdorff, Jorma Seitsamo, Monika E. von Bonsdorff, Juhani Ilmarinen, Claes-Håkan Nygård and Taina Rantanen |

VERSION 1 - REVIEW

| REVIEWER | Dr. Hans Bosma  
Maastricht University, CAPHRI, Social Medicine (The Netherlands)  
I have no competing interests |
|----------|---------------------------------------------------------------------------------------------------------------------------------|
| REVIEW RETURNED | 26/01/2012 |

GENERAL COMMENTS

1. The authors set out to examine the effects of job strain in the separate groups of blue and white-collar workers. The Introduction could have some hypotheses on why we could expect differences and in which direction.
2. There is no report on the non-response in the study. Could this be added and discussed regarding potential biases?
3. The job demands measure is uncommon and measures aspects of satisfaction, skill discretion, job control, and work support. This is too confusing.
4. Work support is not measured, neither discussed as a possible drawback. More generally, the study does not discuss the possible additional relevance of other job stress models, such as the effort-reward imbalance model.
5. The authors should briefly report on why these covariates (occupational group, lifestyle, and baseline diseases) were controlled for and why in the sequence that they control for them. Baseline disease needs further detail on how it was measured and why only cardiovascular diseases and metabolic disorders (and not cancer, for example).
6. Do the authors have information on cause of death?
7. Was the multiplicative interaction between control and demands statistically significant?
8. Was the multiplicative interaction between occupation and job strain statistically significant?
9. Page 9: last paragraph (on Table 4): in blue collar workers, it is passive jobs that heighten risks (not high job strain)!
10. Discussion, first paragraph: high demands and low control heighten risks of mortality. How does that compare with heightened risks for passive jobs in which demands are low and control is high? On page 12, the authors try to explain the heightened risks for passive jobs (in blue collar workers), but the paragraph, referring to reverse causation, is too unclear.
11. Page 11, last paragraph: effects are stronger in younger
employees. The next sentences are about the “...cumulative wear and tear...over time...”. I would then expect stronger effects in older employees, not younger.

12. Discussion, page 12: the authors explain the absence of effects in women by women having stayed at home for longer times (being exposed less to poor working conditions). Could this not be supported by the percentages (not) having a stable job (that they previously (page 12 on top) presented for the whole group)? It is also too easy to assume that in current times effects in women would probably be the same as in men, as women are more often working (page 13 on top).

REVIEWER
Töres Theorell
Professor emeritus and scientific consultant
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There are no competing interests

REVIEW RETURNED 14/02/2012

GENERAL COMMENTS
This is interesting work based on a large and well studied cohort. As the authors point out the long follow-up period is a potential problem but the fact that this cohort has a more stable work career than most studied cohort does counter-balance this problem.

The lack of effect of both job strain and socioeconomic status for women is in line with other research. However there is a possibility in this particular study - since the cohort is not a representative sample of the whole working population but representing the public sector - that there is less diversity among women than among men.

Another point of discussion is that cutting off at the median for both demands and decision latitude means that contrasts are weak, most people are actually in the middle of the distributions for both demands and decision latitude. The authors should at least discuss whether this may provide us with smaller odds ratios than would be the case had they used quartile or tertile divisions instead.

The authors also discuss the problem of "objective" versus "subjective" assessments. May I refer them to two epidemiological studies in which this has been examined? For decision latitude there is good agreement between objective and subjective ratings.

Theorell T, Tsutsumi A, Hallquist J, Reuterwall C, Hogstedt, C., Fredlund P, Emlund N, Johnson J., and the Stockholm Heart Epidemiology Program (SHEEP) 1998: Decision latitude, job strain, and myocardial infarction: a study of working men in Stockholm. Amer J of Public Health 88:382-88.

Theorell, T. and Hasselhorn, H.M. On cross-sectional questionnaire studies of relationships between psychosocial conditions at work and health – are they reliable? Arch Occup Environ Health 78; 517-522, 2005.
Reviewer 1

Reviewer: Dr. Hans Bosma
Maastricht University, CAPHRI, Social Medicine (The Netherlands)

1. The authors set out to examine the effects of job strain in the separate groups of blue and white-collar workers. The Introduction could have some hypotheses on why we could expect differences and in which direction.

[R] We agree with the reviewer and have added a hypothesis to the Introduction, p. 4. We expected that there would be differences in the associations between the four job strain categories and mortality and that among blue-collars high job strain would not correlate as strongly with increased mortality during the follow-up that among the white-collars.

2. There is no report on the non-response in the study. Could this be added and discussed regarding potential biases?

[R] We thank the reviewer for pointing out this important piece of missing information. We have added non-response to the Methods part to the study population description, p. 5. Non-response was rather small (8%) and the ones who did not respond were older. We added information on potential bias that this non-response might yield to the limitations part, p. 14.

3. The job demands measure is uncommon and measures aspects of satisfaction, skill discretion, job control, and work support. This is too confusing.

[R] We thank the reviewer for pointing this out. First of all we checked the wording of the question battery used to describe job demand and noticed that the translation from Finnish to English was not optimal. Secondly, we decided to drop three questions of the battery, which the reviewer was referring to, that were not as well suited for measuring job demand as the others. We are working with an old dataset that has extensive questions on several aspects of work. However, as always when using an old dataset, one needs to work with the data at hand. After these corrections the description of job demand on page 5, second paragraph reads: Job demand was assessed with 5 questions (Cronbach’s alpha =0.73) dealing with pressures related to the job: work pace fast and time schedule tight; responsibility; conflicting demands regarding work tasks and responsibility; pressure and interference with the job by the supervisor; and pressure of failing or doing errors on the job. Answering alternatives were ‘not at all=0’, ‘little=1’, ‘somewhat=2’ or ‘a lot=3’. A summary score ranging between 0 and 15 was calculated for job demand with high scores indicating high job demand.

Due to the change in the job demand measure, we reconstructed the four job strain categories and re-ran all analyses. The results for men did not change much but for women higher job demand decreased statistically significantly mortality during the follow-up. The trend was evident in the original analyses but now statistically significant and interesting. We feel that this change in the job demand measure is justified and more accurate than the original one we presented and thus feel that revising the results accordingly is appropriate. We have marked with red color the changes made throughout the text due to the change in the job demand measure.

4. Work support is not measured, neither discussed as a possible drawback. More generally, the study does not discuss the possible additional relevance of other job stress models, such as the effort-reward imbalance model.

[R] The reviewer is correct to point out the lack of discussion regarding other potential job stress
models, we have added this to the Discussion part, p. 14.

5. The authors should briefly report on why these covariates (occupational group, lifestyle, and baseline diseases) were controlled for and why in the sequence that they control for them. Baseline disease needs further detail on how it was measured and why only cardiovascular diseases and metabolic disorders (and not cancer, for example).

[R] The covariates were selected based on earlier literature on the potential modifiers related to job strain and mortality, we have specified this in the Methods part, p. 6.

Diseases were based on self-reported information of what a physician had diagnosed, which we added to the Methods part, p 6. In addition according to the reviewer’s suggestion we further adjusted the analyses with malignant tumors.

6. Do the authors have information on cause of death?

[R] At the moment we have data on cause of death for 1765 of the 1836 who died in the follow-up and who have data on job strain. According to the ICD coding from the follow-up period, 686 died of CVD causes, 643 of cancer and 436 of other causes. We did analyses on CVD mortality as an outcome and found in the age-adjusted Cox models that low job control increased the statistically significantly the risk of CVD mortality among both men and women. We also conducted analyses on cancer mortality as an outcome and found that in the age-adjusted models low job control increased the risk of cancer mortality among men and high job demand decreased that risk among women. However, in this study we aim to investigate total mortality during this long follow-up. By including all deaths we do not lose power in the gender and occupational class stratified analyses.

7. Was the multiplicative interaction between control and demands statistically significant?

[R] The interaction between job demand and control on total mortality was significant (p=0.005) and this information has been added to the Methods section, page 6.

8. Was the multiplicative interaction between occupation and job strain statistically significant?

[R] No the interactions between occupational group and job control/demand were not statistically significant, p > 0.281. This is why we stratified the analyses according to occupational group and did not combine these exposures. We included this information in the Statistical analyses part, p. 7.

9. Page 9: last paragraph (on Table 4): in blue collar workers, it is passive jobs that heighten risks (not high job strain)!

[R] We thank the reviewer for pointing out the typo and have corrected it, p. 11.

10. Discussion, first paragraph: high demands and low control heighten risks of mortality. How does that compare with heightened risks for passive jobs in which demands are low and control is high? On page 12, the authors try to explain the heightened risks for passive jobs (in blue collar workers), but the paragraph, referring to reverse causation, is too unclear.

[R] We have revised the first paragraph according to the changes in the results and also clarified the paragraph on passive job strain on page 13 and hope that it is now clearer.

11. Page 11, last paragraph: effects are stronger in younger employees. The next sentences are about the “...cumulative wear and tear....over time...”. I would then expect stronger effects in older
employees, not younger.

[R] We agree with the reviewer about what he rightfully points out, however, according to the literature (e.g. Kivimäki et al. 2008 J Epidemiol Community Health) the effects of job strain on e.g. cardiovascular outcomes are more evident in younger employees. We noticed similar trends in our data. We have clarified the Discussion, p. 12-13.

12. Discussion, page 12: the authors explain the absence of effects in women by women having stayed at home for longer times (being exposed less to poor working conditions). Could this not be supported by the percentages (not) having a stable job (that they previously (page 12 on top) presented for the whole group)? It is also too easy to assume that in current times effects in women would probably be the same as in men, as women are more often working (page 13 on top).

[R] Due to the changes in the results we have revised this paragraph and discuss now the potential causes of the protective effect of high job demand and active job among white-collar women on total mortality, p. 13-14.

Reviewer II

Reviewer: Tòres Theorell
Professor emeritus and scientific consultant Epidemiology Group Stress Research Institute Stockholm University Sweden

1. This is interesting work based on a large and well studied cohort. As the authors point out the long follow-up period is a potential problem but the fact that this cohort has a more stable work career than most studied cohort does counter-balance this problem.

[R] We agree with the reviewer and also believe that the stability of the work careers of the public sector employees allow us to think that the job strain situation in midlife is an accumulation of the work demands and control over a longer period of time.

2. The lack of effect of both job strain and socioeconomic status for women is in line with other research. However there is a possibility in this particular study - since the cohort is not a representative sample of the whole working population but representing the public sector - that there is less diversity among women than among men.

[R] As we now have re-analyzed the data according to the comments of the first reviewer we now see an effect of job strain on total mortality in women. We present and argue on these findings in the revised version of the paper.

3. Another point of discussion is that cutting off at the median for both demands and decision latitude means that contrasts are weak, most people are actually in the middle of the distributions for both demands and decision latitude. The authors should at least discuss whether this may provide us with smaller odds ratios than would be the case had they used quartile or tertile divisions instead.

[R] We agree with the reviewer about the cut-off at the median. However, in case we would use tertile cut-offs (two higher tertiles versus the lowest) we would end up with quite a small number of deaths in certain job strain categories because of stratification according to gender and occupational class.

4. The authors also discuss the problem of "objective" versus "subjective" assessments. May I refer
them to two epidemiological studies in which this has been examined? For decision latitude there is good agreement between objective and subjective ratings.

Theorell T, Tsutsumi A, Hallquist J, Reuterwall C, Hogstedt, C., Fredlund P, Emlund N, Johnson J., and the Stockholm Heart Epidemiology Program (SHEEP) 1998: Decision latitude, job strain, and myocardial infarction: a study of working men in Stockholm. Amer J of Public Health 88:382-88.

Theorell, T. and Hasselhorn, H.M. On cross-sectional questionnaire studies of relationships between psychosocial conditions at work and health – are they reliable? Arch Occup Environ Health 78; 517-522, 2005.

[R] We thank the reviewer for pointing out these good references and have cited one of them in the Discussion, p. 14.

VERSION 2 – REVIEW

| REVIEWER   | Tores Theorell  |
|------------|-----------------|
|            | Professor emeritus |
|            | Stress Research Institute |
|            | Stockholm University |
|            | 10691 Sweden |
| REVIEW RETURNED | 24/02/2012 |

The reviewer completed the checklist but made no further comment.