

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Factors associated with quality of life and work ability among Finnish municipal employees: a cross-sectional study
AUTHORS	Bergman, Elina; Löyttyniemi, Eliisa; Myllyntausta, Saana; Rautava, Paivi; Korhonen, Päivi

VERSION 1 - REVIEW

REVIEWER	Nurka PRANJIC Medical Faculty University of Tuzla, Bosnia and Herzegovina
REVIEW RETURNED	04-Dec-2019

GENERAL COMMENTS	Research that supports the link between quality of sleep and work-related stress with the public sector work ability index is very welcome. It should be continued as a cohort study longitudinally.
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REVIEWER	Associate Professor Dr Marzuki Isahak University of Malaya, Malaysia
REVIEW RETURNED	12-Dec-2019

GENERAL COMMENTS	<p>This paper has great value in term the associated factors of Quality of Life (QOL) and work ability (WA) among workers in public sectors. The manuscript was well written, however, require a minor revision in certain areas.</p> <ol style="list-style-type: none">1. My main concern is the title of this paper. the term 'predictors' used in here will suggest temporality or causal relationship which was not achievable in this study. Suggest to change to 'The associated factors of...'2. Same goes to the study objective in page 4 line 1, change to 'the associated factors of...'3. Page 3 line 20, page 4 line 45 - remove the information on the 2014 data collection, and should not mention 2015 study as a follow up study. Explaining on data collection in 2015 is adequate.4. Introduction - Does not explain the research gap. Please add on this.5. Introduction - A lot of attention was given on sleep quality, which contradict with the study objective to look into the predictors of QOL and WA.6. Literature on association of QOL and WA is not relevant in page 3 line 50 as it was not studied here.7. Page 3 line 57 - '.... on self-reported sleep quality and its role'
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	<p>This sentence is not relevant here as it was not the main study objective.</p> <p>8. Page 4 line 1 - delete 'in a non-clinical sample of' and replace with 'among'</p> <p>9. Page 4 line 10 - the hypothesis did not reflect the study objective - to rephrase.</p> <p>10. Page 6 line 23 - change the term 'Psychosocial measures' to 'Psychological symptoms'. This is because none of the social measures were measured here and all are the psychological symptoms.</p> <p>11. Page 6 line 28 - explain why diagnostic tool was used to measure depression instead of screening tool.</p> <p>12. What was the follow up action done when the participants were detected to have severe psychological symptoms (depression and anxiety).</p> <p>13. Page 7 line 32 - Why the association between sleep quality and background variables was conducted? Sleep quality just one of the associated factors.</p> <p>14. Page 7 line 54 - Explain why 'Patient and public involvement' subheading is relevant.</p> <p>15. Page 8 line 19 - Why the analysis in Table 1 was conducted according to sleep quality?</p> <p>16. Table 1 - Any post-hoc test conducted for significant variables in ANOVA test?</p> <p>17. Page 17 line 18 - However, although both QOL and work ability were assessed, we could not enter'. Delete this sentence as it is not relevant.</p> <p>18. Page 19 line 1 - should not use the word 'prevalence' as the sampling technique used was not representative, Generalisability of the prevalence is limited.</p> <p>19. Page 20 line 43 - Explain the validity of one item sleep quality question used to measure sleep quality.</p> <p>20 Page 21 line 28 - Remove conclusion on '... one in four employees suffers from sleep quality.' as it is not part of the study objective.</p>
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REVIEWER	M Carmen Pérez University of Almería
REVIEW RETURNED	11-Jan-2020

GENERAL COMMENTS	<p>The work analyzes a topic of interest but presents great limitations for its publication:</p> <ol style="list-style-type: none"> 1. The theoretical foundation is null and does not justify the inclusion or analysis of the variables that have been analyzed. 2. In order to achieve the objective, they should carry out another type of study and data analysis. 3. The sample is mostly female. Such a sample should not be used. And less include sex as an explanatory variable. 4. The explanatory variables are included in the model one at a time, but in no case are they given in isolation in the subjects. Therefore, they should propose a previous model and check if that model is confirmed with the data analysis, for example, by performing a regression where all the variables are included to know which ones are explanatory. 5. Instruments are used without knowing their reliability and validity in the study sample.
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REVIEWER	Dr Kustaa Piha Department of Public Health University of Helsinki Finland
REVIEW RETURNED	20-Jan-2020

GENERAL COMMENTS	<p>Dear Sir,</p> <p>I'm pleased to receive an opportunity to review the original manuscript "Predictors of quality of life and work ability among Finnish municipal employees: a cross-sectional study". The manuscript aims to study how self-reported measures of sleep quality, quality of life, and work ability were associated with each other and with several confounding factors.</p> <p>The study data was received from questionnaires and health examinations that were carried out in 2015 to voluntary participants of the employees of the City of Pori, in Western part of Finland. Only 32.5% of those who were invited to take part in the study, joined. Further of these, 84.9% joined the second phase from which the data was collected. The measures were based in self-administered questionnaires, but information from height and weight were gathered in physical health examinations. The study is cross-sectional in character. Comprehensive set of possible confounding factors were taken into account.</p> <p>In this review, general interest and novelty of the topic in scientific or practical terms are not evaluated.</p> <p>In general, the manuscript is clearly written, and the internal logic is easy to follow, the analyses are carefully conducted, the results are presented in a clear manner with some notices, and the relevant topics are addressed in the discussion section. However, there are some limitations in the study that also brings out questions, comments and suggestions for further writing of the study. I have divided them in two categories as follows:</p> <p>Major compulsory revisions or questions</p> <ol style="list-style-type: none"> 1. The authors are well aware that the study is cross-sectional in character. That is, information about the interrelated factors are gathered from self-administered questionnaires in the same point of time. Therefore, as the authors have also themselves indicated in e.g. the section "Strengths and Limitations of This Study", causality of the associations cannot be evaluated. However, in many parts including the title the authors indicate that some factors, such as work stress or sleep quality would be "predictors" of end points such as quality of life or work ability. In the setting like this the authors cannot say that certain factors definitely predict another but could only indicate that there is an association or interrelationship between these. One could easily find that, for example, undiagnosed sleep apnea or chronic pain could result in both poor sleep quality and work ability. Or make a hypothesis where poor work ability due to e.g. poor health could result in sense of not being able to cope with work demands and increased work stress. I strongly suggest that the authors review the article and correct the writing in the title, abstract, introduction, and discussion section. 2. As there is a baseline and follow-up data available, why the authors did not use the baseline data for the study factors and
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	<p>evaluate the QoL and work ability in the follow-up time? This would give the possibility to evaluate possible causality. What data would not be available in the baseline?</p> <p>3. Only $32.5\% * 84.9\% = 27.6\%$ of the invited employees finally participated in the study. As the authors correctly indicate, this is a limitation of the study. One could easily make a hypothesis that employees who are willing to participate in the first place and after that willing to join the follow-up, are healthier or in other selected. Is there any information on the non-participants? Do they differ in any respects with the participants? The authors could possibly analyze characteristics or sickness absence rates of the participants and non-participants.</p> <p>4. Page 7 row 14: The authors indicate that some of the information was collected from medical records. What information? Were the medical records comprehensive including also diagnoses from primary or secondary healthcare or only from occupational health care providers own records or known for them? This is of importance because several medical conditions may affect QoL and work ability.</p> <p>Minor essential revisions or questions</p> <p>5. Page 3 row 6: In cross sectional study, the aspects are not affecting e.g. QoL but are associated with each other.</p> <p>6. Page 4 row 31: Are the participants employees of the health and welfare sectors? If yes or no, this should be clearly indicated, as the subsector differs in working conditions and gender distribution as compared with municipal sector or workforce in general.</p> <p>7. Page 4 row 56: The authors could consider listing the participants' occupations so that the largest group is first, and after that in diminishing order.</p> <p>8. In the conclusions, it is interesting that also short and self-administered questionnaires concerning work stress and sleeping quality may function this well in screening. The authors could describe maybe in more detail about what kind of practical suggestions do the results make the readers to think.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Nurka PRANJIC

Institution and Country: Medical Faculty University of Tuzla, Bosnia and Herzegovina

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

Research that supports the link between quality of sleep and work-related stress with the public sector work ability index is very welcome. It should be continued as a cohort study longitudinally.

Thank You for your support for our work and the interesting idea for future research.

Reviewer: 2

Reviewer Name: Associate Professor Dr Marzuki Isahak

Institution and Country: University of Malaya, Malaysia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

This paper has great value in terms of the associated factors of Quality of Life (QOL) and work ability (WA) among workers in public sectors. The manuscript was well written, however, requires a minor revision in certain areas.

Thank you for your valuable comments about our manuscript, which has been revised accordingly. Please find below our responses to your specific concerns:

1. My main concern is the title of this paper. The term 'predictors' used in here will suggest temporality or causal relationship which was not achievable in this study. Suggest to change to 'The associated factors of...'

You are right, that the term 'predictor' is not the best one to use in a cross-sectional study. We have now changed the title to "Factors associated with quality of life and work ability among Finnish municipal employees: a cross-sectional study" – page 1

2. Same goes to the study objective in page 4 line 1, change to 'the associated factors of...'

We have changed that one too, as well as all other similar expressions. –page 5

3. Page 3 line 20, page 4 line 45 - remove the information on the 2014 data collection, and should not mention 2015 study as a follow up study. Explaining on data collection in 2015 is adequate.

We have now rephrased the sentences. Unfortunately, we cannot totally delete the information about initial response rate, because it has to be considered as a limitation for the generalisation of the results. However, we have now tried to make this section more clear. –pages 3 and 5

4. Introduction - Does not explain the research gap. Please add on this.

5. Introduction - A lot of attention was given on sleep quality, which contradicts with the study objective to look into the predictors of QOL and WA.

6. Literature on association of QOL and WA is not relevant in page 3 line 50 as it was not studied here.

7. Page 3 line 57 - '... on self-reported sleep quality and its role ...' This sentence is not relevant here as it was not the main study objective.

8. Page 4 line 1 - delete 'in a non-clinical sample of' and replace with 'among'

Thank you for these comments. We have now rewritten the introduction section, and hope that you will find these changes to answer your concerns (4. – 8.) – pages 4-5

9. Page 4 line 10 - the hypothesis did not reflect the study objective - to rephrase.

We have now removed the hypothesis in that form and rephrased the objective. –page 5

10. Page 6 line 23 - change the term 'Psychosocial measures' to 'Psychological symptoms'. This is because none of the social measures were measured here and all are the psychological symptoms.

Corrected as suggested. –page 7

11. Page 6 line 28 - explain why diagnostic tool was used to measure depression instead of screening tool.

Since our study population was quite healthy and active work force, the mean MDI score was very low. Thus, we thought that using the diagnostic tool with cut-off would offer a well-validated and clinically meaningful option. –page 7

12. What was the follow up action done when the participants were detected to have severe psychological symptoms (depression and anxiety).

They were referred to the occupational health care. This information has now been added to the text. –page 8

13. Page 7 line 32 - Why the association between sleep quality and background variables was conducted? Sleep quality just one of the associated factors.

In our previous work about cardiovascular health and its associations with quality of life, self-reported sleep quality was observed to have a very strong association with quality of life. The association of good sleep quality with good quality of life was stronger than of for example favourable cardiovascular health profile or absence of chronic illnesses with quality of life. For that reason, we wanted to use the association of sleep quality and background variables to characterize the population. –page 8

14. Page 7 line 54 - Explain why 'Patient and public involvement' subheading is relevant.

This subheading is obligatory in the BMJOpen research articles. –page 9

15. Page 8 line 19 - Why the analysis in Table 1 was conducted according to sleep quality?

Please, see our response to the item 13. Table 1 displays the characteristics of the total study population and also divided according to sleep quality. –page 11

16. Table 1 - Any post-hoc test conducted for significant variables in ANOVA test?

No. As the sleep quality was used here only to characterize the study population, we did not perform any post-hoc tests. –page 11

17. Page 17 line 18 - However, although both QOL and work ability were assessed, we could not enter'. Delete this sentence as it is not relevant.

We have now removed the sentence. –page 19

18. Page 19 line 1 - should not use the word 'prevalence' as the sampling technique used was not representative, Generalisability of the prevalence is limited.

We have now rephrased the sentence. –page 21

19. Page 20 line 43 - Explain the validity of one item sleep quality question used to measure sleep quality.

This sleep quality question is an item from the well-validated Pittsburgh Sleep Quality Index (PSQI). [1]. The PSQI has good internal consistency ($\alpha = 0.83$) and test–retest reliability ($r = 0.82$; over an average of 19 days) [2,3]. The same one-item question was used also in the UK Household study to describe the subjective sleep quality of the participants [4]. This information has now been added also to the methods section, into the part “sleep related measures”. –page 7

20 Page 21 line 28 - Remove conclusion on '... one in four employees suffers from sleep quality.' as it is not part of the study objective.

Thank you for this notion, we have now removed the sentence. –page 23

Reviewer: 3

Reviewer Name: M Carmen Pérez

Institution and Country: University of Almería

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

The work analyzes a topic of interest but presents great limitations for its publication:

Thank you for your constructive criticism. We have now made a major revision for the article and we hope you will find it more satisfactory.

1. The theoretical foundation is null and does not justify the inclusion or analysis of the variables that have been analyzed.

We have now rewritten the introduction section and also rephrased the aim of the study. We hope that this will now better clarify the theoretical basis and the objective of the study. –pages 4-5

2. In order to achieve the objective, they should carry out another type of study and data analysis.

We admit, that the objective “to demonstrate the predictors of QoL and work ability...” was not fully acceptable in a cross-sectional study and have now changed it to “demonstrate factors associated with QoL and work ability...” –page 5

3. The sample is mostly female. Such a sample should not be used. And less include sex as an explanatory variable.

The aim of this study is to demonstrate the factors associated with QoL and work ability especially among public sector employees. Public sector is a large employer in the Scandinavian countries and most of the employees are females. The gender distribution in this study corresponds to the standard gender distribution of the employees of the city of Pori and to the distribution among Finnish public sector employees (81.3% women in 2015 (Statistics Finland 2019). Also in the large Finnish prospective study of the public sector employees “The Public Sector Study” about 80% the participants are female [5]. –page 22

4. The explanatory variables are included in the model one at a time, but in no case are they given in isolation in the subjects. Therefore, they should propose a previous model and check if that model is confirmed with the data analysis, for example, by performing a regression where all the variables are included to know which ones are explanatory.

All the background variables seen in the tables 2 and 3 were first examined one by one with the outcome variable EuroHIS or WAS (univariate in the tables). Along with age and gender, all the background variables that were significant in the univariate approaches, were then entered in the multivariable models. –pages 8-9, 11-12 and 14-15

5. Instruments are used without knowing their reliability and validity in the study sample.

All the instruments used in the study are internationally well-known and validated as described in the methods section. –pages 6-8

Reviewer: 4

Reviewer Name: Dr Kustaa Piha

Institution and Country:

Department of Public Health

University of Helsinki

Finland

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

We want to thank you for your valuable comments about our manuscript, which has now been revised accordingly. Please find below our responses to your specific concerns:

Dear Sir,

I'm pleased to receive an opportunity to review the original manuscript "Predictors of quality of life and work ability among Finnish municipal employees: a cross-sectional study". The manuscript aims to study how self-reported measures of sleep quality, quality of life, and work ability were associated with each other and with several confounding factors.

The study data was received from questionnaires and health examinations that were carried out in 2015 to voluntary participants of the employees of the City of Pori, in Western part of Finland. Only 32.5% of those who were invited to take part in the study, joined. Further of these, 84.9% joined the second phase from which the data was collected. The measures were based in self-administered questionnaires, but information from height and weight were gathered in physical health examinations. The study is cross-sectional in character. Comprehensive set of possible confounding factors were taken into account.

In this review, general interest and novelty of the topic in scientific or practical terms are not evaluated.

In general, the manuscript is clearly written, and the internal logic is easy to follow, the analyses are carefully conducted, the results are presented in a clear manner with some notices, and the relevant topics are addressed in the discussion section. However, there are some limitations in the study that also brings out questions, comments and suggestions for further writing of the study. I have divided them in two categories as follows:

Major compulsory revisions or questions

1. The authors are well aware that the study is cross-sectional in character. That is, information about the interrelated factors are gathered from self-administered questionnaires in the same point of time. Therefore, as the authors have also themselves indicated in e.g. the section "Strengths and Limitations of This Study", causality of the associations cannot be evaluated. However, in many parts including the title the authors indicate that some factors, such as work stress or sleep quality would be "predictors" of end points such as quality of life or work ability. In the setting like this the authors cannot say that certain factors definitely predict another but could only indicate that there is an association or interrelationship between these. One could easily find that, for example, undiagnosed sleep apnea or chronic pain could result in both poor sleep quality and work ability. Or make a hypothesis where poor work ability due to e.g. poor health could result in sense of not being able to

cope with work demands and increased work stress. I strongly suggest that the authors review the article and correct the writing in the title, abstract, introduction, and discussion section.

We admit that the term “predictor” is not fully acceptable to use in describing the associations between variables in a cross-sectional study. We have now corrected the term throughout the article, including the title.

2. As there is a baseline and follow-up data available, why the authors did not use the baseline data for the study factors and evaluate the QoL and work ability in the follow-up time? This would give the possibility to evaluate possible causality. What data would not be available in the baseline?

In the present work, we used the data from the year 2015 because complete information about psychosocial risk factors was available only from that year. In the baseline data in 2014, all the participants had only answered to so called “core questions” which are recommended by the European Society of Cardiology for the initial screening of psychosocial risk factors for cardiovascular diseases[6]. Because depression and anxiety are conditions known to have a significant impact on a person’s work ability and quality of life, we wanted to use the data with more detailed information about these factors. Complete information about data collection from the year 2014 has been described earlier [7]. The information about data collection has now been more clearly written in the methods section, part “participants”. –page 5

3. Only $32.5\% \times 84.9\% = 27.6\%$ of the invited employees finally participated in the study. As the authors correctly indicate, this is a limitation of the study. One could easily make a hypothesis that employees who are willing to participate in the first place and after that willing to join the follow-up, are healthier or in other selected. Is there any information on the non-participants? Do they differ in any respects with the participants? The authors could possibly analyze characteristics or sickness absence rates of the participants and non-participants.

The participation rate is a clear limitation of our study and may have led to selection bias. As you suggested, we compared the sickness absence rates of the participants and non-participants. The mean annual rate of sickness absence days among the study participants during the two-year follow-up (2014 -2015) was 11 days [8]. The mean annual rate of sickness absence among all employees (n=5736) in the sectors that included the invited work units in 2015 was 11.7 days (data not shown). – page 22

4. Page 7 row 14: The authors indicate that some of the information was collected from medical records. What information? Were the medical records comprehensive including also diagnoses from primary or secondary healthcare or only from occupational health care providers own records or known for them? This is of importance because several medical conditions may affect QoL and work ability.

Information about diagnosed diseases and medications was gathered from primary and occupational health care medical records. We could also use the information from the Doctor’s statements for sick leave, rehabilitation, special reimbursement for medications or for disability pension (So-called Statements A and B) for the Social Insurance Institution of Finland (Kela).

Minor essential revisions or questions

5. Page 3 row 6: In cross sectional study, the aspects are not affecting e.g. QoL but are associated with each other.

Quite true, thank you. Corrected as suggested. –page 3

6. Page 4 row 31: Are the participants employees of the health and welfare sectors? If yes or no, this should be clearly indicated, as the subsector differs in working conditions and gender distribution as compared with municipal sector or workforce in general.

Yes, employees of the health and welfare sectors were well represented in our study. The following sentence has now been added to the methods section: The involved employment sectors, reported according to the number of employees participating in the study in 2015 were Health and Welfare (275 employees), Social Work (198), Technical Services (143), Education and Culture (62) and Administration (32). –page 6

7. Page 4 row 56: The authors could consider listing the participants' occupations so that the largest group is first, and after that in diminishing order.

Please, see our response to the previous item.

8. In the conclusions, it is interesting that also short and self-administered questionnaires concerning work stress and sleeping quality may function this well in screening. The authors could describe maybe in more detail about what kind of practical suggestions do the results make the readers to think.

Thank you for this clinically meaningful notion. The following sentence has been added to the conclusions:

We suggests that short, self-reported assessment tools could be used for this purpose. – page 23

VERSION 2 – REVIEW

REVIEWER	Associate Prof Dr Marzuki Isahak University of Malaya, Malaysia
REVIEW RETURNED	20-Mar-2020

GENERAL COMMENTS	The author has addressed all my concern.
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REVIEWER	MARIA DEL CARMEN PEREZ FUENTES Spain
REVIEW RETURNED	22-Feb-2020

GENERAL COMMENTS	<p>The paper analyzes a topic of great interest, but presents important limitations for its publication.</p> <ol style="list-style-type: none"> 1. The theoretical foundation is null. Relevant studies that analyze these same variables in other countries and contexts are not reviewed or included. 2. The objective is very basic and does not delve into the variables and the possibilities they offer. 3. The data is currently somewhat obsolete, the professional reality sure has had important changes since the data was collected. Social changes make five years too long for the results to be of interest now. 4. The sample selection criteria is not specified. It is a clearly feminine sample. 5. The data analyzes are based on the description of the sample,
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	<p>and on the realization of nonsense tests, because they must decide between the tests they perform and analyze the different dependent variables without apparent criteria.</p> <p>6. The discussion includes multiple works that have not been taken into account for the theoretical foundation of the study.</p> <p>7. The conclusions are limited to a paragraph, which shows that the work done by the authors does not have the level or quality sufficient.</p> <p>8. Should improve the ethical treatment, has not gone through a commission in this regard.</p>
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REVIEWER	<p>Kustaa Piha University of Helsinki Department of Public Health Finland</p>
REVIEW RETURNED	24-Feb-2020

GENERAL COMMENTS	<p>This is a revision of a manuscript I have earlier reviewed. The authors have sufficiently changed the manuscript in most important issues as follows:</p> <p>1) As the setting is cross-sectional in character, only associations between variables can be analyzed.</p> <p>2) Main characteristics of the non-respondents vs. respondents is analyzed.</p> <p>3) Description about the sectors where the participants worked was opened in more detail.</p> <p>4) Practical applications are added to the discussion.</p> <p>In addition, minor adjustments have been made.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 3

Reviewer Name: MARIA DEL CARMEN PEREZ FUENTES Institution and Country: Spain Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below The paper analyzes a topic of great interest, but presents important limitations for its publication.

1. The theoretical foundation is null. Relevant studies that analyze these same variables in other countries and contexts are not reviewed or included.
2. The objective is very basic and does not delve into the variables and the possibilities they offer.

We think that the theoretical foundation of our work is firm, and the research question is relevant to employees, employers, and public health. In the first revision, we rewrote the introduction, to better clarify the theoretical basis of the study. We have now performed a careful literature search again and found no new relevant studies to be referred.

We did also rewrite and clarify the objective in the revised version of the manuscript, since the reviewer had had concerns about the carried-out analyses not achieving the aim of the study we introduced in the first version of the manuscript. We are quite surprised that after these clarifications the reviewer now wishes the objective to be more complex. We don't find it relevant to further adjust the objective.

3. The data is currently somewhat obsolete, the professional reality sure has had important changes since the data was collected. Social changes make five years too long for the results to be of interest now.

The variables examined tend to change very slowly in the population and it is not likely that any significant changes would have occurred in the past five years.

4. The sample selection criteria is not specified. It is a clearly feminine sample.

The complaint about “a feminine sample” is inappropriate and even offensive, since the gender distribution in our study corresponds well to that of the Finnish and Scandinavian public sector. We have clearly explained this in the manuscript as well as in our response to the reviewers of the first revision.

5. The data analyzes are based on the description of the sample, and on the realization of nonsense tests, because they must decide between the tests they perform and analyze the different dependent variables without apparent criteria.

We have explained the statistical procedure, the reviewer found improper, in detail in the first response letter. We find these new arguments about “nonsense tests” inappropriate and not relevant.

6. The discussion includes multiple works that have not been taken into account for the theoretical foundation of the study.

We find that according to the results of a study it is often essential to find new references for the discussion section. These studies may not have been relevant to discuss in the introduction.

7. The conclusions are limited to a paragraph, which shows that the work done by the authors does not have the level or quality sufficient.

We find it very strange to estimate the quality of a scientific work by the length of the conclusion section.

8. Should improve the ethical treatment, has not gone through a commission in this regard.

We cannot understand the claim about the lack of ethical treatment, since our work has gone through all the normal ethical procedures, as clearly stated in the section “Ethical approval”.

No changes have been made to the manuscript according to these concerns.

Reviewer: 4

Reviewer Name: Kustaa Piha

Institution and Country:

University of Helsinki

Department of Public Health

Finland

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below This is a revision of a manuscript I have earlier reviewed. The authors have sufficiently changed the manuscript in most important issues as follows:

- 1) As the setting is cross-sectional in character, only associations between variables can be analyzed.
- 2) Main characteristics of the non-respondents vs. respondents is analyzed.
- 3) Description about the sectors where the participants worked was opened in more detail.
- 4) Practical applications are added to the discussion.

In addition, minor adjustments have been made.

Thank you for your valuable comments! We are pleased to hear that you are satisfied with the changes and adjustments we have made according to your concerns.

Reviewer: 2

Reviewer Name: Associate Prof Dr Marzuki Isahak Institution and Country: University of Malaya, Malaysia Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below The author has addressed all my concern.

We are pleased to hear that you are satisfied with the changes and clarifications we have made on our manuscript according to your concerns.