

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Reciprocal relationship between psychosocial work stress and quality of life: the role of gender and education from the longitudinal study of the Survey of Health, Ageing and Retirement in Europe
AUTHORS	Lu, Yake; Qiao, Ya-Mei; Liang, Xiao; Yao, Wu; Yan, Zhen; Wang, Hui-Xin; Pei, Jin-Jing

VERSION 1 - REVIEW

REVIEWER	Raphael Herr Heidelberg University, Germany
REVIEW RETURNED	07-Nov-2018

GENERAL COMMENTS	<p>Thank you for the opportunity to review the manuscript entitled "Reciprocal relationship between psychosocial work stress and quality of life: the role of gender and education" (bmjopen-2018-027051).</p> <p>The authors report longitudinal data from the SHARE study with a two year follow-up on the reciprocal association of quality of life (QoL) and psychosocial work stress. Low reward, low control and the combination of both were associated with incident poor QoL. This effect was most pronounced in persons with high education. Persons with poor QoL were on a higher risk for incident low reward but not for low control. The manuscript is clear and well written, interesting for the readership and I have only few comments for improvement.</p> <p>Psychosocial work stress was assessed as job rewards, job control and the combination of both. Rewards were measured by five items taken from the ERI questionnaire and control by two items from the JDC scale. I wonder why the authors constructed this new measurement of psychosocial work stress and did not use the established measurement of ERI and JDC?</p> <p>QoL was assessed in the four domains: control, autonomy, self-realization, and pleasure. Thus, there might be a conceptual and empirical overlap between the control measurement of QoL and job?</p> <p>I guess a "poor" is missing in the heading of Table 2: "Table 2. Characteristics of participants with good quality of life (QoL) at baseline (N =2006) and poor QoL status two years later, n (%)"</p> <p>In the first sentence of the abstract, I would suggest that it is "moderated by gender or education" instead of and?</p>
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	I hope my recommendations and comments help to improve the paper.
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REVIEWER	Jordi Fernández-Castro Universitat Autònoma de Barcelona Spain
REVIEW RETURNED	27-Nov-2018

GENERAL COMMENTS	<p>Reciprocal relationship between psychosocial work stress and quality of life: the role of gender and education bmjopen-2018-027051</p> <p>This paper investigates the reciprocal relationship between psychosocial work stress and quality of life. It consists in a statistical analysis of two waves of a wide European survey on health and ageing (SHARE). The topic of this paper is interesting and relevant, since there are still a lot to be learnt about the relationship between work stress and quality of life and the objectives are clear and well-grounded. I have read and followed with interest the discussion about the reciprocal relationship between psychological work stress and quality of life and the role played by education level.</p> <p>In general my impression is that this paper is suitable to be published but I have some concerns that, in my opinion, are needed to be clarified:</p> <ol style="list-style-type: none"> 1. - Introduction (page 4). SHARE survey is oriented to the study of ageing and the sample of this study is set up by people older than 50. Thus, ageing should be mentioned in the introduction section, at least to define the context in the life-span of the participants 2. - Methods (page 5, line 21). What does specifically mean "missing information on key variables"? 3. - Methods (page 5, line 34). Incidence of low reward is considered, why job control is mentioned? 4. - Methods (page 5, line 39). Incidence of low control is considered, why job reward is mentioned? 5. - Methods (page 5, lines 18-41). It would be preferable, in my opinion, to try that the explanation of the exclusion criteria were simpler and clearer. Criteria should be listed separately for wave one and two. 6. - Methods (page 6, lines 8-9). It is said: "In ERI model, the measurement was restricted to the reward dimension. We picked five items to measure "reward". Four Likert scale was used to calculate a sum score". I have some doubts: Five or four? Which items were picked up? Why? Has been tested the reliability of this mini scale? 7. - Methods (page 6, lines 16-17). I have the same doubts about the selection of the two items of control. 8. - Methods (page 6, lines 8-24). Could you provide specific information about the de definition of low control and low reward? Specially if the wave 1 or the whole data set was used, 9. - Methods (page 6, lines 34-37). I'm a bit confused, a variable could be treat as continuous or as a dichotomy, but it must be presented in the tables in the same way it has been considered in analysis. 10. - Methods (page 6, lines 51-52). It is said: "Poor QoL was defined as the country lowest tertile of the sum score"; so, what data set has been used, the baseline or the whole set?
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	<p>11. – Methods, Statistical analyses (page 7, line 32). Prevalence of poor QoL is mentioned. I don't understand the point, since participants with poor QoL in wave 1 are excluded, the quantity of people with poor QoL has to be necessarily the numbers of new cases, and thus this would be incidence, no prevalence.</p> <p>12. - Results (page 8, line 6). Which characteristics are these? Could you be more specific? Does it refer to list of covariates? In this case, please don't change the terms.</p> <p>13. - Results (page 8, line 10). Prevalence of QoL is mentioned, I assume that it refers to wave 1 but this detail is not mentioned. But, it seems that participants with poor QoL were excluded, they weren't?</p> <p>14. - Results (page 8, line 14). Is baseline set up with data from wave one? I'm afraid that this point is not clearly stated in methods. In this case, it is a starting point not a baseline (line: almost two points).</p> <p>15. - Results (Page 10, table 2). Please, do not wrote than the value of a p is 0, such thing do no exists. A probability could be extremely low but always different than zero. To avoid writing a huge number of 0, you could use the expression lower than .001. Moreover you can write the p-value without the 0 previous to the “.”</p> <p>16. - Results (Page 11, lines 30-41). In this paragraph, it this said than specific country values were used. Does that mean that the definition of low control and low reward has been made differently in the previous paragraph? I'm sorry but I do not know how this paragraph fits with the above.</p> <p>17. - Results (Page 11, line 43 and following). It seems that the rationale of this analysis is different than the previous analysis, because the focus is in the incidence of low control and low reward. And, in my understanding, that does not fit with the explanations in methods section. Clarify, please.</p> <p>18. - Discussion (Page 13, lines 25 and following). It is discussed that some differences there are between participants of the present study and younger people. Perhaps, it could be useful trying offering any explanation of these differences.</p> <p>19. - Discussion (Page 15, Limitations). It would be worth to suggest which variables no considered in this study that could influence the relationship between work stress and QoL and could be tested in futures studies.</p> <p>20. - Discussion (Page 15, Limitations).Also within the limitations, in my opinion, it should be stated that the result are limited to older workers.</p> <p>In summary, I think this study is valuable and interesting, but I also think that it is necessary to explain accurately various aspects of the methodology and the results. It could be that I had not interpreted the text correctly, but in any case I think it is better to make clear and precise explanations that avoid misunderstandings.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

1. Psychosocial work stress was assessed as job rewards, job control and the combination of both. Rewards were measured by five items taken from the ERI questionnaire and control by two items from the JDC scale. I wonder why the authors constructed this new measurement of psychosocial work stress and did not use the established measurement of ERI and JDC?

Response: Thanks for the reviewer's query. Given the constraints of multi-disciplinary collaborative research within large-scale epidemiological investigations, the inclusion of full original questionnaires was not possible. Thus, items were selected on the basis of factor loadings and item-total correlations on respective original scales, abbreviated versions of the original ERI and JDC questionnaires were used in SHARE. In the JDC model job control was selected based on evidence that the predictive power of 'control' exceeded the power of 'demand' in several studies (Marmot et al. 1997; Steenland et al. 1997). While in ERI model, due to effort had no relationship with quality of life in this study the measurement was restricted to the reward dimension. Besides, previous study also showed reward can predict the subsequent poor QoL but not low effort (Tobiasz-Adamczyk and Brzyski 2005; Teles et al. 2014). Because psychosocial stress from different dimensions may have an additive effect and previous study showed that the combined effects of the two models on cardiovascular health were considerably stronger than their separate effects (Peter et al. 2002), we combined those two scales to explore the relationship between work stress and QoL.

2. QoL was assessed in the four domains: control, autonomy, self-realization, and pleasure. Thus, there might be a conceptual and empirical overlap between the control measurement of QoL and job?

Response: We understand the reviewer's concern. In CASP-12 questionnaire, the QoL control domain includes three questions, and they are: (1) my age prevents me from doing the things; (2) I feel that what happens to me is out of control, and (3) I feel left out of things. While job control was assessed by (1) I have very little freedom to decide how I do my work, and (2) I have an opportunity to develop new skills.

We conducted correlation analysis between each of the three items of QoL control domain with each of the two items of job control separately, and all of the correlation coefficients were less than 0.1. We added this part in the METHODS, RESULTS, and DISCUSSION and take it as one of the limitations.

METHODS (Lines 53-55, page 7), now it reads as: "Correlation analysis was conducted to examine whether there is an overlap between the QoL control dimension and job control".

RESULTS (Lines 21-25, page 8): "The correlation coefficients between each item of the QoL control domain and each item of job control were less than 0.1."

DISCUSSION (Lines 23-27, page 16): "Fifth, there might be a conceptual and empirical overlap between the control measurements of QoL and job, but their correlation coefficients were less than 0.1, the likelihood of overlapping is minimal".

3. I guess a "poor" is missing in the heading of Table 2: "Table 2. Characteristics of participants with good quality of life (QoL) at baseline (N =2006) and poor QoL status two years later, n (%)"

Response: Thanks for pointing out the mistake. "poor" was really missing in the heading of Table 2. We have now added it now in the revised manuscript. Referring to the changes on line 25 of the page 10.

4. In the first sentence of the abstract, I would suggest that it is "moderated by gender or education" instead of and?

Response: Thanks for the advice. We have now revised the sentence according to the suggestion in the new version of the manuscript. Referring to the changes on line 10 of the page 2.

Reviewer: 2

1. 1. - Introduction (page 4). SHARE survey is oriented to the study of ageing and the sample of this study is set up by people older than 50. Thus, ageing should be mentioned in the introduction section, at least to define the context in the life-span of the participants

Response: Thanks for the helpful advice. We have now added the important role of older workers in the end of paragraph 2, page 4. "However, relatively few studies addressed this issue in general population, especially in older adults. With an aging population, employment rates among older workers have been increasing,¹² QoL of older employees deserve more attention. Work-related stress has been associated with a range of adverse outcomes among older workers,^{3,4} which may increase their psycho-social work stress.^{13,14} Thus, understanding the relationship between psychosocial work stress and QoL among older employees is warranted for the development of preventive strategies for a longer and healthier working life."

2. Methods (page 5, line 21). What does specifically mean "missing information on key variables"?

Response: We have now clarified this by replacing "key variables" with "QoL, job control and job reward related questions". Now it reads: The following exclusion criteria were applied at wave 1: (1) participants below the age of 50 (n=3713), (2) those who were currently unemployed (n=19235), and (3) those with missing information on QoL, job control and job reward related questions (n=3114, 471 individuals missing work related factors and 2643 missing QoL related questions). In total 4754 of 30816 met the inclusion criteria, with an age range from 50 to 90 years old (line 25, page 5).

3. - Methods (page 5, line 34). Incidence of low reward is considered, why job control is mentioned?

Response: Thanks for the concern. We have now revised the excluding criteria as: "To study the influence of poor QoL on incidence of low reward, we excluded participants who had low reward at wave 1 (n=2818) and had missing information at wave 2 (n=864), 1072 subjects remained for the analysis" on page 5. All related analyses had been rerun and the corresponding texts have been revised (pages 12, 22).

4. - Methods (page 5, line 39). Incidence of low control is considered, why job reward is mentioned?

Response: Thanks for the comment. We have now revised the excluding criteria as: "To study the influence of baseline QoL on incidence of low job control, we excluded participants who had low

control at wave 1 (n=2931) and missing information on job control at wave 2 (n=714), leaving 1109 participants for this analysis” on page 5. All related analyses had been rerun and the corresponding texts have been revised (pages 12).

5. - Methods (page 5, lines 18-41). It would be preferable, in my opinion, to try that the explanation of the exclusion criteria were simpler and clearer. Criteria should be listed separately for wave one and two.

Response: Thanks for the helpful advice. We have now revised the expression of the criteria for wave 2, thus, the second paragraph of Method-population listed the criteria for wave 1, and the third paragraph listed the exclusion criteria for wave 2 on page 5. It reads as:

“In the longitudinal study, three data sets were acquired from 4754 participants: (1) To study the influence of baseline psychosocial work stress on incidence of poor QoL, we excluded participants who had poor QoL at wave 1 (n=1828) and missing information on QoL at wave 2 (n=920), leaving 2006 subjects for the analysis. (2) To study the influence of baseline QoL on incidence of low job control, we excluded participants who had low control at wave 1 (n=2931) and missing information on job control at wave 2 (n=714), leaving 1109 participants for this analysis (3) To study the influence of poor QoL on incidence of low reward, we excluded participants who had low reward at wave 1 (n=2818) and had missing information at wave 2 (n=864), 1072 subjects remained for the analysis”.

6. - Methods (page 6, lines 8-9). It is said: "In ERI model, the measurement was re-stricted to the reward dimension. We picked five items to measure "reward". Four Likert scale was used to calculate a sum score". I have some doubts: Five or four? Which items were picked up? Why? Has been tested the reliability of this mini scale?

Response: Thanks for the query. In effort reward imbalance model, we picked up five of reward items (received adequate support, recognition from work, adequate salary, poor job promotion and poor job security) to measure "reward" with response options including "strongly disagree, disagree, agree, strongly agree" scored on four-Likert scale. The scores range from 5 to 20. Items were selected on the basis of factor load-ings and item-total correlations on respective original scales as the constraints of multi-disciplinary collaborative research within large-scale epidemiological investigations, the inclusion of full original questionnaires was not possible. The changes on measure "re-ward" was on lines 15-17 of the page 6. The internal consistency of the reward was not very high ($\alpha=0.542$), we discussed it as a limitation (lines 14-17, page 16).

7. - Methods (page 6, lines 16-17). I have the same doubts about the selection of the two items of control.

Response: The abbreviated versions of the original ERI and JDC questionnaires were used in SHARE as the constraints of multi-disciplinary collaborative research within large-scale epidemiological investigations, the inclusion of full original questionnaires was not possible. The underlying dimensions of the JCQ were based on a sum score of the two job control items (I have little freedom to decide how I do my work in job, I have an opportunity to develop new skills) with response options ranging from "strongly dis-agree" to "strongly agree" scored on four-Likert scale. Besides the explanatory contri-bution of 'control' exceeded the contribution of 'demand' in several landmark studies (Marmot et al. 1997, Steenland et al. 1997), due to these reasons, JDC model was re-stricted to the control dimension. Referring to the changes on lines 26-28 of the page 6.

8. - Methods (page 6, lines 8-24). Could you provide specific information about the definition of low control and low reward? Specially if the wave 1 or the whole data set was used,

Response: Thanks for your advice. We have now added the definition of low control and low reward. "At wave 1 among 4754 participants those with job control score or job reward score lower than the country median score was defined as low control or low reward, respectively. The same definitions were applied at wave 2." was added on lines 29-33 of the page 6.

9. - Methods (page 6, lines 34-37). I'm a bit confused, a variable could be treated as continuous or as a dichotomy, but it must be presented in the tables in the same way it has been considered in analysis.

Response: Thanks for the reviewer's suggestion. We have reanalyzed all the results. age was taken as continuous variable. The mean and SD of continuous variable was reported in table 1, table2 and table3. Figures 1 and 2 have been revised in the new version of manuscript. Now it read as "Age was treated as a continuous variable, and all others variables were treated as dichotomized variables in the analysis" (lines 49-50 on page 6).

And we also revised the age as continuous variable in table 1 (line 46, page 8), table 2 (line 29, page 10), table 3 (line 35, page 12).

10. - Methods (page 6, lines 51-52). It is said: "Poor QoL was defined as the country lowest tertile of the sum score"; so, what data set has been used, the baseline or the whole set?

Response: At both wave 1 and wave 2 analyses, country-specific median score were used to define poor QoL. As described in the definition of quality of life "Poor QoL was defined as the sum score lower than the country lowest tertile in both wave 1 (n=4754) and wave 2 (n=2006)". See changes on lines 3-5, page 6.

11. - Methods, Statistical analyses (page 7, line 32). Prevalence of poor QoL is mentioned. I don't understand the point, since participants with poor QoL in wave 1 are excluded, the quantity of people with poor QoL has to be necessarily the numbers of new cases, and thus this would be incidence, not prevalence.

Response: Thanks for the query. Prevalence of poor QoL is cross-sectional analysis based on wave 1 information only, thus participants with poor QoL were included.

12. - Results (page 8, line 6). Which characteristics are these? Could you be more specific? Does it refer to list of covariates? In this case, please don't change the terms.

Response: Thanks for the helpful advice. "Characteristics" does refer to list of covariates. According to the advice we revised "characteristics" as "Demographic and health variables". Now it reads: "There were no significant differences between participants and nonparticipants in all demographic and health variables except that nonparticipants had higher prevalence of depression than participants (9.67% vs. 7.17%)". See changes on line 19, page 8.

13. - Results (page 8, line 10). Prevalence of QoL is mentioned, I assume that it refers to wave 1 but this detail is not mentioned. But, it seems that participants with poor QoL were excluded, they weren't?

Response: Table 1 shows the results of cross-sectional analysis. Participants in wave 1 who had information on QoL, job reward and job control (n=4754), including those with poor QoL, were included.

14. - Results (page 8, line 14). Is baseline set up with data from wave one? I'm afraid that this point is not clearly stated in methods. In this case, it is a starting point not a baseline (line: almost two points).

Response: The reviewer is right, baseline is wave 1. We have now replaced "baseline" with "wave 1" to clarify it.

The sentence now reads: "People with low reward+low control, low reward+high control, and high reward+low control at wave 1 were significantly associated with poor QoL as compared with those with both high reward and high control". Referring to the changes on line 31 of the page 8.

15. - Results (Page 10, table 2). Please, do not write that the value of a p is 0, such thing does not exist. A probability could be extremely low but always different than zero. To avoid writing a huge number of 0, you could use the expression lower than .001. Moreover you can write the p-value without the 0 previous to the "."

Response: We appreciate the recommendation from the reviewer. We have now replaced "0.00" with "<.001" now.

16. - Results (Page 11, lines 30-41). In this paragraph, it is said that specific country values were used. Does that mean that the definition of low control and low reward has been made differently in the previous paragraph? I'm sorry but I do not know how this paragraph fits with the above.

Response: The reviewer is right. In the previous paragraph, specific country values were used in the definition of low control and low reward. While in this paragraph population median was used to define low control and low reward. Similar results from both specific country median and population median increased our confidence in the results. This point was added in the RESULT (lines 59-60, page 11) and DISCUSSION (lines 47-50, page 16). We add a sentence now in the RESULTS: "We used both country specific median and population median to define high and low levels of job reward and job control, similar results were observed" (lines 59-60, page 11).

"Finally, similar results from both specific country median and population median increased our confidence in the results" was added as the fourth strength in the DISCUSSION (lines 47-49, page 16).

17. - Results (Page 11, line 43 and following). It seems that the rationale of this analysis is different than the previous analysis, because the focus is in the incidence of low control and low reward. And, in my understanding, that does not fit with the explanations in methods section. Clarify, please.

Response: Thanks for the query. The main aim of this study is to investigate the reciprocal relationship between psychosocial work stress and quality of life (QoL). The first part we examined whether participants with low reward and low control at baseline predict higher risk of poor QoL over the 2-year follow-up. The second part, as the reviewer noticed, focuses on the incidence of low control and low reward occurred during the 2-year follow-up in relation to baseline poor QoL. To clarify this, we have now changed the text to "Demographic and health variables of participants with low levels of psychosocial work stress at wave 1 and incidence of high psychosocial work stress occurred during the two years follow-up (wave 2) were presented in table 3". The title of table 3 was also revised to "Demographic and health variables of participants with low levels of psychosocial work stress at wave

1 and incidence of high psychosocial work stress occurred during the two years follow-up (wave 2), n (%)” (lines 5-9, 26-30, page 12).

18. - Discussion (Page 13, lines 25 and following). It is discussed that some differences there are between participants of the present study and younger people. Perhaps, it could be useful trying offering any explanation of these differences.

Response: Thanks for the helpful advice. We have now added an explanation for the age differences on lines 4-10, page 14. It reads: “The different finding may be that older workers have more experience to cope with stressful situations compared with the younger, thus the differences between high and low efforts may be smaller in older workers than in younger workers.”

19. - Discussion (Page 15, Limitations). It would be worth to suggest which variables no considered in this study that could influence the relationship between work stress and QoL and could be tested in futures studies.

Response: Thanks for the advice. We have now added “Fourth, we cannot exclude the possibility that residual confounding, such as social relations, work conflicts, might have biased the true associations, futures studies should take into account these potential confounders.” (line 20, page 16).

20. - Discussion (Page 15, Limitations). Also within the limitations, in my opinion, it should be stated that the result are limited to older workers.

Response: Thanks for the advice. We have now added the limitation on lines 28-31 of page 16: “Finally, the results of the current study were based on participants aged 50+, thus, these results cannot be generalized to younger workers.”

In summary, I think this study is valuable and interesting, but I also think that it is necessary to explain accurately various aspects of the methodology and the results. It could be that I had not interpreted the text correctly, but in any case I think it is better to make clear and precise explanations that avoid misunderstandings.

Response: We are very grateful to the reviewer’s encouragement. Your suggestions/comments have helped us a lot in improving the Ms, and our endeavor can be seen in the revised version of manuscript.

VERSION 2 – REVIEW

REVIEWER	Raphael Herr Heidelberg University, Germany
REVIEW RETURNED	08-Mar-2019

GENERAL COMMENTS	I thank the authors for their efforts to address my comments. While they generally did a good job, one concern however remains. I asked the authors to use the original conceptualisation of the work stress models; i.e., job strain as the combination of demands and control, and effort reward imbalance ratio as the deviation of efforts from rewards. I understand that job strain is not possible to calculate, because job demands are not measured in SHARE. However, even not the original, long questionnaires are used in
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	SHARE, effort and reward were measured and it is possible to calculate the theoretical funded ratio between efforts and rewards. Even if effort is not related with QoL, the combination of efforts and rewards is from a theoretical basis more relevant than the combination of reward and control. Other studies using the SHARE data also defined the effort-reward imbalance ratio, e.g., Hoven et al. (2015. Occupational position, work stress and depressive symptoms- a pathway analysis of longitudinal SHARE data). In my opinion, additional testing of the relationship of the effort-reward imbalance ratio with QoL improves the quality of the manuscript.
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REVIEWER	Jordi Fernández-Castro Universitat Autònoma de Barcelona Spain
REVIEW RETURNED	12-Mar-2019

GENERAL COMMENTS	The explanations of the authors and the changes introduced in the article are satisfactory. I recommend its publication.
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VERSION 2 – AUTHOR RESPONSE

Question: I asked the authors to use the original conceptualisation of the work stress models; i.e., job strain as the combination of demands and control, and effort reward imbalance ratio as the deviation of efforts from rewards. I understand that job strain is not possible to calculate, because job demands are not measured in SHARE. However, even not the original, long questionnaires are used in SHARE, effort and reward were measured and it is possible to calculate the theoretical funded ratio between efforts and rewards. Even if effort is not related with QoL, the combination of efforts and rewards is from a theoretical basis more relevant than the combination of reward and control. Other studies using the SHARE data also defined the effort-reward imbalance ratio, e.g., Hoven et al. (2015. Occupational position, work stress and depressive symptoms- a pathway analysis of longitudinal SHARE data). In my opinion, additional testing of the relationship of the effort-reward imbalance ratio with QoL improves the quality of the manuscript.

Thanks for the reviewer's query. Following the reviewers suggestion, we calculate the theoretical funded ratio between efforts and rewards and defined ERI as participants with a higher value than country specific upper tertile. The results showed that ERI can predict subsequent poor QoL and QoL can predict subsequent ERI, but the ORs are consistently smaller than the combination of low reward and low control. It has been argued that the JDC model and ERI model primarily emphasize different elements in the psychosocial working environment and there is empirical evidence showing that these models, at least dimensions of them have separate effects. It might be fruitful to study their combined effects¹⁻³. Though previous study combining components of the JDC and ERI (including effort, reward, demand, control, support) did not find greater explanatory power than either one of them separately in relation to mental distress 1, it provided possibility to combine different dimensions of JDC and ERI. Besides, the internal reliability of reward and control combination was higher (Cronbach $\alpha = 0.616$) than that of the effort and reward combination ($\alpha = 0.536$). The correlation between job reward and job control is higher ($r = 0.366$) than the correlation between effort and reward ($r = -0.186$). Similarly, the correlations of QoL with the combination of control and reward was higher ($r = 0.330$) than that of QoL with the combination of effort and reward ($r = -0.233$). Thus, we decided to study the association of QoL with the combination of job control and reward.

We now added the results in page 13 lines 23-33 as: "We also calculated the ERI ratio to quantify the imbalance between high cost and low gain as: effort/reward (adjusted for the numbers of items). ERI was defined as the score higher than the country specific upper tertile. The results showed that ERI

can predict subsequent poor QoL (OR=1.37 95% CI: 1.102-1.706) while no significant results were found when stratified by gender and education. Poor QoL can also predict subsequent ERI (OR=1.28 95% CI: 1.03-1.60), especially among participants with high education (OR=1.58 95% CI: 1.10-2.29)."

In page 14 lines 25-27 we added "In addition, our result that ERI was related to poor QoL incident is in line with several previous studies.^{4,5}"

We also added in the discussion the reason we use the combination of job reward and job control in page 14 lines 41-59, " Apart from different adverse health outcomes, a possible explanation for the difference maybe that advanced technologies have considerably changed the occupational exposures, the traditional models may not match the modern psychosocial work environment very well. An experimental study showed that manipulation rewards may substantially decrease the physiological responses.⁶ It is interesting to note that both loss of reward and loss of control have been used as theoretical approaches for psychosocial stress in studying adverse health outcomes,² thus the combination of reward and control may provide a new direction to study psychosocial stress and QoL."

1. Calnan M, Wadsworth E, May M, et al. Job strain, effort--reward imbalance, and stress at work: competing or complementary models? *Scand. J. Public Health.* 2004;32:84-93.
2. Siegrist J. The Effort–Reward Imbalance Model. In: Siegrist J, ed. *The Handbook of Stress and Health: A Guide to Research and Practice.* first ed. UK: John Wiley & Sons, Ltd; 2017:12.
3. de Jonge J, Bosma H, Peter R, et al. Job strain, effort-reward imbalance and employee well-being: a large-scale cross-sectional study. *Soc. Sci. Med.* 2000;50:1317-1327.
4. Krause N, Rugulies R, Maslach C. Effort-reward imbalance at work and self-rated health of Las Vegas hotel room cleaners. *Am. J. Ind. Med.* 2010;53:372-386.
5. Kudielka BM, Hanebuth D, von Kanel R. Health-related quality of life measured by the SF12 in working populations: associations with psychosocial work characteristics. 2005;10:429-440.
6. Landolt K, O'Donnell E, Hazi A, et al. An experimental examination of the effort-reward imbalance model of occupational stress: Increased financial reward is related to reduced stress physiology. *Biol. Psychol.* 2017;125:121-129.

VERSION 3 - REVIEW

REVIEWER	Raphael Herr Heidelberg University, Germany
REVIEW RETURNED	26-Apr-2019
GENERAL COMMENTS	All my comments were addressed well. I recommend the manuscript for publication.

Correction: *Reciprocal relationship between psychosocial work stress and quality of life: the role of gender and education from the longitudinal study of the Survey of Health, Ageing and Retirement in Europe*

Lu Y, Qiao Y, Liang X, *et al.* Reciprocal relationship between psychosocial work stress and quality of life: the role of gender and education from the longitudinal study of the Survey of Health, Ageing and Retirement in Europe. *BMJ Open* 2019;9:e027051. doi: 10.1136/bmjopen-2018-027051

This article was previously published with an error.

Earlier the corresponding author is only listed as Hui-Xin Wang.

The correct corresponding authors are Hui-Xin Wang and Zhen Yan.

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